



# Evaluating the effects of a massive rural school expansion in pre-reform China

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## Abstract

On the eve of its economic reforms, China achieved a much higher secondary school enrollment rate than other countries with a similar per capita income at the time. This study investigates the source of this high enrollment rate by examining a massive expansion of rural schools during the Cultural Revolution that increased the number of secondary schools more than tenfold. We estimate the impact of the expansion by compiling a new county-level dataset from local gazetteers and exploiting the county-level variation in the speed of expansion for identification purposes. We provide strong evidence that the program significantly increased rural children's years of schooling and suggestive evidence that teachers contributed more to this improvement than schools. By building a pool of middle-skilled labor years later, the expansion program boosted local agricultural yields and increased the productivity of the township and village enterprises that emerged after the reform. Finally, we find some evidence that this rapid expansion was associated with a deterioration in the quality of schooling.

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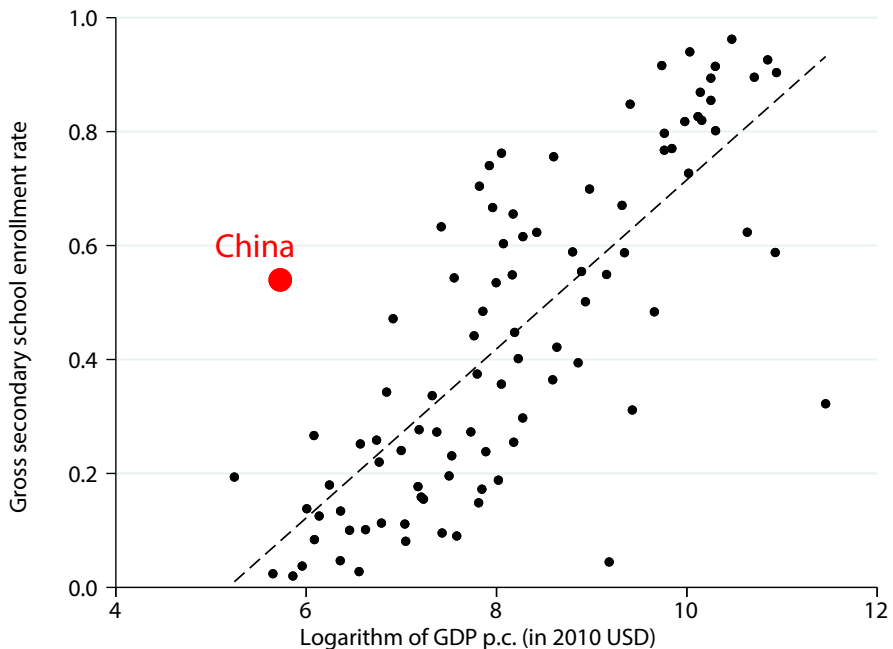
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## 1 Introduction

China had achieved a high level of secondary school enrollment relative to its per capita income even before it started to move towards market-oriented reforms. In 1978, China's per capita GDP was merely \$307, only 5% of the world average and much lower than that of India and sub-Saharan Africa. However, its secondary school enrollment was 54%, higher than the world average (49%), and much higher than in India, sub-Saharan Africa, or middle-income countries (World Bank 2020). If we regress the secondary school enrollment rate on the GDP per capita of 95 countries in 1978 (Fig. 1), China is a stark outlier far above the regression line. Considering that the country's population was predominantly rural in 1978 (82.1% according to the National Bureau of Statistics of China, or NBS), such an achievement was even more extraordinary since it enrolled tens of millions of rural children in secondary school at a time when most Chinese peasants were struggling to feed themselves. Evaluating the consequences of a campaign that involved such a large population is important for understanding both human capital formation in general and the origins of China's economic development in particular.



**Fig. 1** GDP per capita and the secondary school enrollment rate in 1978: an international comparison. Data Source: World Bank (2020)

Economists have regarded China's massive stock of medium-skilled labor before the economic reforms as a critical initial condition for its rapid post-reform growth (Dreze and Sen 1999; Heckman and Yi 2014; Yao 2014). Yet little is known about how it managed to achieve such a high level of educational attainment at an extremely low income. This paper explores how China managed to enroll a large number of secondary school students through a massive rural school expansion program launched during the Cultural Revolution, which contributed to a substantial pool of labor with some secondary school education before the economic reform. This program increased the number of secondary schools tenfold, from 18,102 in 1965 to 201,268 in 1977 (National Bureau of Statistics of China 2010). The number of secondary school teachers rose by a similar magnitude, from 0.46 million in 1965 to 3.2 million in 1977. The goals of the expansion were to achieve universal education through junior secondary school and to increase the rate of progression to senior secondary school for rural children (Pepper 1990). The expansion supported Mao's ideology during the Cultural Revolution to reduce class differences and close the education gap between rural and urban areas (Hannum 1999).

In this study, we seek to quantify the impact of this massive rural school expansion program on the educational attainment of rural children and rural development. We digitized over 3000 book-length local gazetteers and collected county-level information on the number of secondary schools and teachers in over 1000 counties. We then constructed an index to capture the differential expansion processes across the nation. We use a cohort difference-in-differences (DID) strategy to estimate the causal effect that relies on two types of variation. First, the speed of expansion was different across counties. Second, cohorts within the same county were differently exposed to the expansion because they entered secondary schools in different years. We provide strong evidence that the increase in the number of schools or teachers during this period contributed to a significant increase in the pool of medium-skilled labor with some secondary school education. Additionally, the policy effect of more teachers is about triple that of more schools. Our conservative estimates suggest that the campaign nurtured at least 3.21 (1.67) million junior (senior) high graduates from 1966 to 1977, accounting for 33% (83%) of total junior (senior) high graduates during the previous decade (1956–1965).<sup>1</sup> Therefore, the expansion of rural schools made a huge contribution to the formation of a large stock of medium-skilled labor at a time when China was extremely poor by international standards.

Despite a rollback of the school expansion shortly after the end of the Cultural Revolution, we estimate that the program benefited at least 136.8 million rural children. Rural children who were of school age during the expansion period gradually entered the rural workforce after their graduation from junior or senior high school. Most of

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<sup>1</sup> We estimate that there were 9.76 (2.02) million rural junior (senior) high graduates from 1956 to 1965. We infer those numbers in two steps. First, the official statistics show that there were a total of 14.05 (3.11) million junior (senior) high graduates during this period without further distinguishing the rural and urban students (National Bureau of Statistics of China 2010). In the second step, we use data from the 2010 wave of the China Family Panel Studies (CFPS) to estimate the share of rural graduates. We estimate that of those who graduated from junior (senior) high schools between 1956 and 1965, 69.5% (65.1%) held a rural *hukou* at the age of 12. Multiplying the total number of graduates by the share of rural *hukou* yields the estimated number of rural graduates.

them were young adults when the economic reforms were launched in the late 1970s. Therefore, it is important to investigate whether a greater pool of educated workforce in rural areas contributed to rural development. One reason that the expansion benefited rural regions is that the curriculum in rural schools during that period was more practically oriented and emphasized agricultural and industrial knowledge. Additionally, the strict migration control during the period prevented the better-educated rural population from leaving the countryside, and their human capital was bound up with the rural land. Here, we focus on two critical aspects of rural development: agricultural productivity and the development of the township and village enterprises (TVEs for short). Exploiting county-level data on annual grain yields and the number of tractors, we find that the expansion program contributed to an increased usage of tractors and higher agricultural productivity. TVEs, which were collectively owned, rurally located, and managed by township and village leaders (Weitzman and Xu 1994), served as a key driver of the Chinese economy during the 1980s and 1990s. They accounted for 38.1% of the country's gross industrial output value in 1993 (Lin and Wang 2012) and 28.9% of aggregate exports in 1995 (Brandt et al. 2017). Our own calculation using a TVE sample of the 1995 China Industrial Census indicates that the labor force in TVEs was surprisingly well educated: 80.0% completed junior high schools and 26.4% received at least a senior high school education. Using firm-level data on TVEs, we show evidence that the program increased the workers' average years of schooling in TVEs and boosted the TVEs' total factor productivity (TFP).

However, such a rapid expansion of rural schools did not occur without costs. We find that more schools did not lead to better individual labor market outcomes. We claim that the null effect on labor market outcomes is likely to originate from the deteriorated schooling quality associated with the school expansion. We reach this claim by comparing the school expansion with a contemporaneous event—the send-down movement, which forced about 16 million urban youths (known as sent-down youths, or SDYs) to relocate to rural areas from 1968 to 1977. The movement unintentionally benefited rural education by providing a pool of natural candidates for qualified rural teachers. Different from the school expansion, the send-down movement improved *both* educational achievement and labor market outcomes. We further provide suggestive evidence that receiving more SDYs during the movement helped alleviate the decline in school quality. Lastly, by dividing secondary schools into higher-quality government-run ones and lower-quality people-run ones, we find that higher-quality schools yield more positive outcomes.

Our study contributes to two strands of the literature. First, we provide concrete evidence of the effect of a massive rural school expansion on educational attainment in China's pre-reform era. Dreze and Sen (1999) and Heckman and Yi (2014) highlighted the importance of this topic, but it has not yet been well studied. A large body of literature documents the Cultural Revolution's catastrophic effects on the country's education system, particularly elite education in urban areas (Deng and Treiman 1997; Meng and Gregory 2002; Giles et al. 2019). However, it is less well-known that rural China experienced a swift school expansion during the same period. Hannum (1999, pp. 202) commented that "*the Cultural Revolution, condemned on the most counts as an educational disaster, successfully raised educational levels in rural areas and narrowed urban-rural education differentials.*" Some prior studies provide descriptive

or anecdotal evidence on the rural school expansion campaign.<sup>2</sup> Our study is the first to quantitatively estimate the impact of the school expansion by using the county-level variation in the process of expansion. It complements our previous study (Chen et al. 2020a), which examined the send-down movement during the same period that benefited rural children's education as an *unintended* consequence. This study, in contrast, investigates the *intended* effect of a massive school expansion campaign initiated by the central government.

Zhu and Sicular (2022), the study most related to ours, also exploits rural school expansion during the Cultural Revolution. They use the average high school attainment rate at the cohort-by-county level as an instrumental variable to estimate the returns to schooling. Our study differs from theirs in two important ways. First, the validity of the instrumental variable (i.e., county-by-cohort high school attainment rate) adopted by Zhu and Sicular (2022) relies upon an implicit assumption that the school expansion during the Cultural Revolution successfully raised years of schooling. By contrast, our study tries to provide *direct* evidence on the consequences of the school expansion on rural education by compiling comprehensive county-level data on the numbers of schools and teachers during the Cultural Revolution from numerous local gazetteers. Second, Zhu and Sicular (2022) focus on the labor market consequences of the school expansion and estimate the returns to schooling. Our study additionally looks at the effect on rural development including agricultural production and the productivity of TVEs, which is a new perspective.

As a second contribution, our paper advances the literature on improving access to education in developing countries. Previous studies have highlighted the role of school infrastructure (Duflo 2001; Handa 2002; Alderman et al. 2003; Burde and Linden 2013; Khanna 2023), the availability of teachers (Andrabi et al. 2013; Duflo et al. 2015; Chen et al. 2020a), and teachers' incentives (Muralidharan and Sundararaman 2011; Duflo et al. 2012; Barrera-Osorio and Raju 2017). Our study contributes to this literature in three important ways. First, the size and scope of China's school expansion we examine were unprecedented in human history. In roughly one decade, China set up over 180,000 secondary schools across the country, and secondary school enrollment rose by about 60 million (National Bureau of Statistics of China 2010).<sup>3</sup> Second, a unique feature of our data is the availability of information on the expansion of both schools *and* teachers, which allows us to evaluate their relative importance in improving rural education. We find that teachers were much harder to supply than schools in a given period of time, but played a much more important role than school infrastructure. Third, we show that faster expansion may lead to lower schooling quality, which raises potential concerns about the downsides of the rapid school expansion.

<sup>2</sup> Han (2001) and Andreas (2004) presented two detailed case studies of Jimo county (in Shandong Province) and Laishui county (in Hebei Province), respectively. Hannum (1999) documented a temporary shrinkage in the urban-rural education gap during the Cultural Revolution. Zhang (2018) adopted a regression discontinuity design that exploits the massive *closure* of rural high schools after 1978, which happened after the school expansion our paper investigates, to study the consequences of high school closures on labor market outcomes.

<sup>3</sup> To the best of our knowledge, the only other campaign of comparable size is India's District Primary Education Project (DPEP), which created about 160,000 new schools (Khanna 2023). However, DPEP was introduced in 1994 while the school expansion in our paper was initiated in the mid-1960s when China's per capita GDP remained at an extremely low level.

We wish to emphasize that our paper evaluates the impact of the rapid rural school expansion on the educational attainment of rural children and other related outcomes. Analyzing the catastrophic consequences of the Cultural Revolution and performing a cost-benefit analysis of the school expansion program are beyond the scope of the current study. We seek to deepen the understanding of an understudied event (the rural school expansion) that had influenced millions of rural children and may have had lasting and important implications for China's subsequent economic development.

The remainder of the paper is organized as follows. Section 2 briefly reviews the institutional background related to the school expansion program. Section 3 introduces our dataset and discusses the econometric setups. Two subsequent sections estimate the program's effect on rural children's educational attainment (Sect. 4) and rural development beyond schooling (Sect. 5). Section 6 investigates the individual labor market outcomes, which provides suggestive evidence of the possible deterioration in schooling quality as a result of the expansion. Section 7 concludes.

## 2 Institutional background

### 2.1 A history of China's pre-reform school expansion

When the People's Republic of China was founded in 1949, the entire nation was poorly educated. The vast majority (70–80%) of the population was illiterate, and only about 25% of school-age children attended primary school (Pepper 1978). The central government sought to expand primary school education during the 1950s and 1960s by attempting to build a primary school in every village (Hannum 1999). This campaign quickly increased the primary school enrollment rate to 84.7% by 1965.

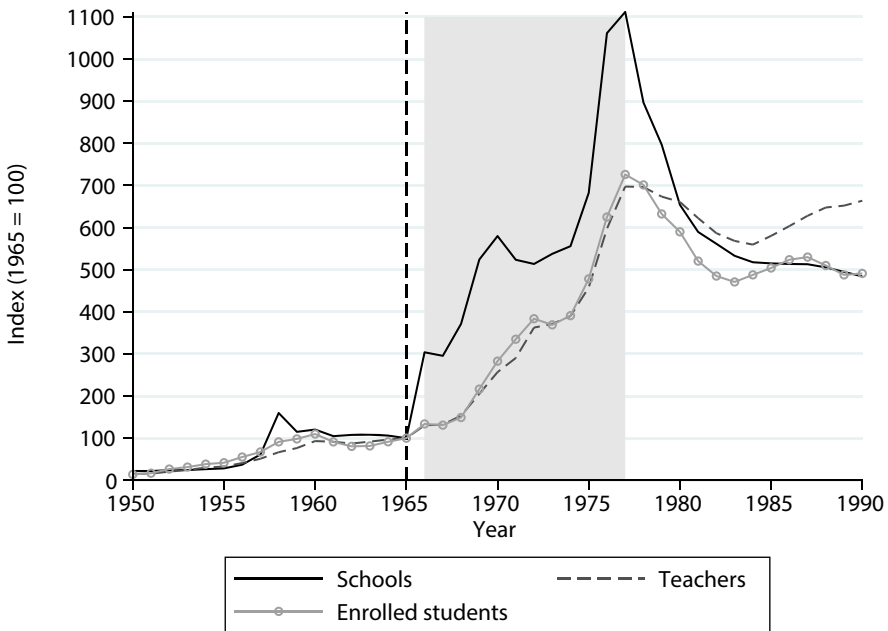
The government faced a dilemma about how to concentrate its limited resources on promoting secondary education: should it focus on a small group of elites to produce talent (liberal competitive model) or popularize basic education to reduce class differences (socialist egalitarian model) (Hannum 1999)? To balance the competing perspectives of top party leaders, China's education system before the Cultural Revolution adopted different school systems in urban and rural areas. Urban schools played the role of quality maintenance. There were key point schools for the elite few targeting tertiary education. Rural schools served the purpose of popularization and eliminating illiteracy. The quality of schooling was not a major focus. Most rural schools at the time were people-run (*minban*) (as opposed to government-run, or *gongban*) and could take flexible forms such as half-work/half-study.

The Cultural Revolution triggered a radical shift in ideology beginning in 1966. From 1966 to 1976, the revolution sought to destroy the old elite education system and construct a new mass education system (Pepper 1996). The central goal of the reform during the Cultural Revolution was to reduce class differences between urban and rural areas and between the elite and the masses. The government suspended college entrance examinations until the end of the revolution and replaced the admission policy with individual recommendations. Exam-based progression at lower levels of schools (such as primary school to junior high school) was also prohibited, and there was no grade repetition. Key schools were abolished. The government also gradually

shortened the curriculum from a 6-3-3 system (6 years of primary school, 3 years of junior high, and 3 years of senior high) to a 5-2-2 one (Chen et al. 2020b) and introduced factory and farm work into the curriculum (Pepper 1990; Meng and Gregory 2002). Separate academic and vocational streams were merged into a single stream for secondary education.

Together with its efforts to suppress elite education, the government initiated a massive secondary school expansion program in rural areas. The goal was to achieve universal education through junior secondary school and to increase the rate of progression to senior secondary school for rural children (Pepper 1990)—what the government called “*building schools near the front doors of poor peasants.*” The program target was to construct at least one secondary school in every People’s Commune (the basic unit of organization in rural areas, comprising several thousand households). Figure 2 displays the remarkable increase in the number of secondary schools, together with an almost parallel rise in the number of secondary school students and teachers. There were only 18102 secondary schools, 0.46 million secondary school teachers, and 9.3 million registered secondary school students at the dawn of the Cultural Revolution in 1965. These numbers rose to 201,268, 3.19 million, and 67.8 million, respectively, by 1977 (National Bureau of Statistics of China 2010). We explain in detail in the next subsection how this expansion was achieved.

The conventional wisdom is that the quality of schooling was relatively low during the Cultural Revolution, due to the shortened curriculum and the modified content of



**Fig. 2** Expansion index (year 1965 = 100) in secondary schools, teachers, and students. Note: The data source is NBS (2010). In 1965, China had 18102 secondary schools, 0.46 million secondary teachers, and 9.34 million enrolled secondary school students

education.<sup>4</sup> Students were required to spend a considerable amount of time in political courses, farming, and factory work. Yet the evidence indicates that the schooling during this period was meaningful,<sup>5</sup> particularly after the standard school curriculum gradually resumed in about 1972 (Meng and Gregory 2002).<sup>6</sup> The emphasis on education also shifted from scientific knowledge to practical experiences during the Cultural Revolution. For example, science courses (such as physics and chemistry) were consolidated into “agricultural knowledge” and “industrial knowledge”. Students needed to learn skills such as repairing electric motors, developing new crop seeds, and treating common animal diseases (Bo 1982). While those skills were generally regarded as useless for urban students, they were quite helpful in rural settings. Rural secondary education was also more vocationally oriented. Andreas (2004) documents that in Laishui county, senior high students were divided into small groups, and each group received training for a specific occupation, such as tractor operation and repair, orchard care, or medicine (to become “barefoot doctors” (BFDs)).<sup>7</sup>

## 2.2 How was the expansion achieved?

It was undoubtedly challenging to achieve a tenfold increase in the number of secondary schools within only one decade. There were three major ways to promote secondary schools in rural China during the Cultural Revolution.<sup>8</sup> The first and most important way was to build new secondary schools. While county governments provided some funding, local villages paid most of the costs of school construction. Using locally produced construction materials, several villages would pool their resources to build a secondary school (*lianban zhongxue*). The second way was to request urban schools to establish branch schools in rural areas and to reallocate teaching

<sup>4</sup> Since this change of content was *universal* across the country, it does not necessarily threaten our identification strategy. We not only exploit a before/after expansion difference but also use cross-county variations in the speed of secondary school expansion.

<sup>5</sup> If the schooling during the Cultural Revolution was genuinely ineffective, we would anticipate no changes or even some improvements in the labor market performance of rural youths following the extensive school closures that occurred after the Cultural Revolution, which effectively reversed the rural school expansion we are examining. However, Zhang (2018) discovered adverse labor market outcomes for rural high school closures after the Cultural Revolution.

<sup>6</sup> For example, Gansu Province specified five courses for secondary education in 1969: Mao Zedong thought, revolutionary literature, basic agricultural knowledge, basic industrial knowledge, and military sports (Committee of Compiling Gansu Education Gazetteers 1991 pp. 212). Zhejiang Province taught these courses in addition to Chinese, mathematics, and foreign language (Yu and Committee of Compiling Zhejiang Education Gazetteers, 2004 pp. 304). In 1973, both provinces resumed their standard curriculum, which covered politics, Chinese, mathematics, English, history, geography, physics, chemistry, etc.

<sup>7</sup> BFDs were community health workers who provided basic healthcare services in rural China in the 1960s and 1970s. They received basic medical training, and their main duties were to treat common illnesses and promote basic hygiene, preventive healthcare, and family planning. Most BFDs continued to work on the farmland and often walked without shoes to visit patients, which is how they got their name. BFDs became an important part of China’s rural public health system in 1965 following Chairman Mao’s instruction to consolidate the rural healthcare system. The BFD system gradually ended after the collapse of the People’s Commune system starting in the late 1970s.

<sup>8</sup> We summarize the three approaches based on our reading of the related literature and historical documents (Pepper 1990; Han 2001; Andreas 2004; Yu and Committee of Compiling Zhejiang Education Gazetteers 2004).



resources (including teachers, books, equipment, and teaching aids) to rural schools. This method was in line with the central ideology of the education reform during the revolution—to reduce class differences and the urban-rural gap in schooling. The last way to promote secondary schools in rural areas was called “hat wearing” (*ceng-ceng daimao*)—teaching older students in lower-level schools (e.g., adding senior high classes to junior high schools). The shortage of qualified teachers presented a great obstacle to the expansion. Anecdotal evidence indicates that villages adopted three methods to alleviate the shortage (Andreas 2004). First, some teachers who originated from rural villages but resided in urban areas were required to return to their home villages. Second, village leaders selected some better-educated young villagers who had finished middle school to become teachers. Third, the arrival of SDYs, who came from urban areas and were generally better educated, were natural candidates for rural teachers (Chen et al. 2020a).

What determined the number of new schools to be built in a county? Based on our readings of historical materials, the primary factor determining the number of new schools built was the local economic condition.<sup>9</sup> Communes and county governments bore most of the costs of establishing new schools, which included procuring building materials and hiring teachers, primarily because rural schools were mainly people-run during the expansion period, although with some public assistance (Pepper 1996). For example, as documented by Pepper (1996, pp. 429), prosperous communes in coastal provinces like Guangdong and Jiangsu were encouraged to establish their own senior high schools to meet growing demand and save state funds. In contrast, counties in less affluent regions sometimes limited growth to the junior high level, even when commune authorities requested further expansion. Additionally, more developed counties were better equipped to find qualified teachers, a lack of whom was a significant obstacle to the school expansion (Andreas 2004). We will empirically examine the association between the speed of school expansion and some county characteristics in the next section.

After Mao’s death in September 1976 and the end of the Cultural Revolution, the number of secondary schools decreased by about 50% from 201,000 in 1977 to 102,000 in 1982 (Fig. 2). Multiple driving forces led to this rollback. The rapid expansion during the Cultural Revolution was the joint result of the government’s ideology to prioritize schooling *equality* over *quality*, communes’ grassroots support, and the arrival of the SDYs. All of those supporting factors gradually diminished after 1977. A big reversal of education policy occurred after 1978 when the model shifted from socialist egalitarian back to liberal competitive. The new policy reemphasized schooling quality, restored the 6-3-3 system, and reintroduced exam-based progression.<sup>10</sup> Many rural schools were closed because they were considered of low quality. The collapse of the People’s Commune system in the early 1980s, which provided administrative and financial support to most rural schools, accelerated the decline. Most of the SDYs,

<sup>9</sup> While interviewees in Pepper (1996) also suggested other factors, such as popular traditions, leadership initiative, and managerial skills, quantifying these variables proved challenging, so we cannot delve into them extensively here.

<sup>10</sup> A test-based hierarchical school system was gradually reinstated in 1977 with the restoration of the national college entrance examination after 11 years of suspension.

many of whom became an important force of rural teachers, returned to their urban homes after the end of the send-down movement in the late 1970s.

### 3 Data and empirical strategy

#### 3.1 Data, sample, and variables

##### County-level data

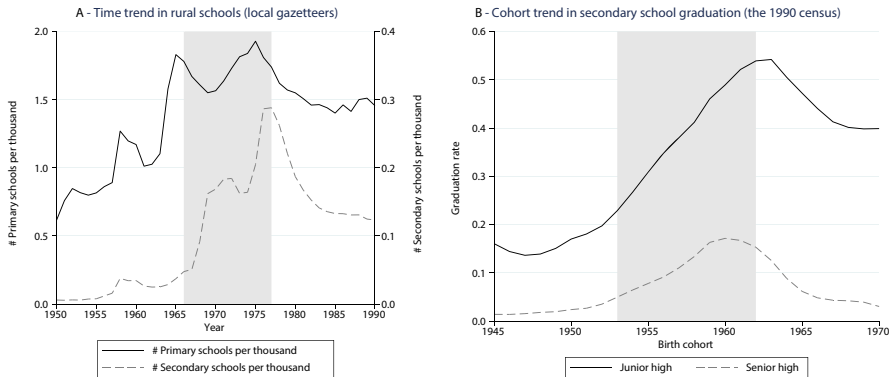
Our study combines data at three levels: county, individual, and firm. The county-level information mainly comes from local gazetteers, which are comprehensive collections of a county's major historical events; they are regarded as a generally trustworthy information source for Chinese counties.<sup>11</sup> Our final sample covers over 1000 counties, each of which compiled its own gazetteer. The key county-level variation for our identification purposes is that counties built secondary schools and recruited teachers at different speeds during the expansion. We therefore define the school construction rate as the annual increase in the number of secondary schools<sup>12</sup> from 1965 to 1977 per 1000 county population in 1964.<sup>13, 14</sup> The left panel of Fig. 3 plots the times series of the average number of primary and secondary schools per thousand residents in our sample counties. The secondary school expansion progress in local gazetteers roughly matches that using data from the NBS (Fig. 2), which provides suggestive evidence of the precision of the data we collected. Figure 3 also verifies that the period 1966–1977

<sup>11</sup> Almond et al. (2019) and Chen et al. (2020a) provide evidence of the reliability of local gazetteers by cross-validating the information using other sources and employing statistical methods to detect data anomalies (such as Benford's Law).

<sup>12</sup> By default, "secondary schools" in our paper refer to regular academic secondary schools. There are two other types of secondary schools in China's education system. The first is specialized secondary schools, *zhongzhuan* (senior high level). The second is vocational schools, *zhixiao* (either junior high or senior high level). We use only regular secondary schools for three reasons. First, county gazetteers usually refer to regular secondary schools when presenting the historical number of secondary schools. Second, normal secondary schools account for the vast majority of China's secondary schools. In 1980, the number of regular secondary schools dropped to 118,377—a 41% fall from its peak in 1977. But this number is still 18 times the two other types of schools combined (3069 + 3314) (National Bureau of Statistics of China 2010). Third, we estimate that over 90% of our sample entered regular secondary schools. Because the population census does not distinguish between vocational and regular schools, we analyze data from the 2010 wave of the CFPS to understand the composition of different types of secondary schools. We first keep the CFPS sample as close as possible to our final census sample by dropping three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. We further focus on the 1943–1962 cohorts who had a rural registration status at age 12. Among the final sample of 6977 observations, 1991 of them reported entering regular junior high school; only 10 entered vocational junior high school. At the senior high level, 602 students entered regular academic schools, and only 53 enrolled in other types of schools (*zhongzhuan* or *zhixiao*).

<sup>13</sup> We chose 1964 because China's second population census was conducted that year, and it is the only year in the 1960s for which we know the precise county-level population.

<sup>14</sup> One complication is that local gazetteers did not record information in the same format. While some counties recorded a complete time series of the number of secondary schools every year, others may have only reported one number every few years. We discuss in detail how we handle this complicated issue in Appendix B1 and perform a robustness check if we focus on a subset of counties with a complete time series of information.



**Fig. 3** The expansion of secondary education in our data

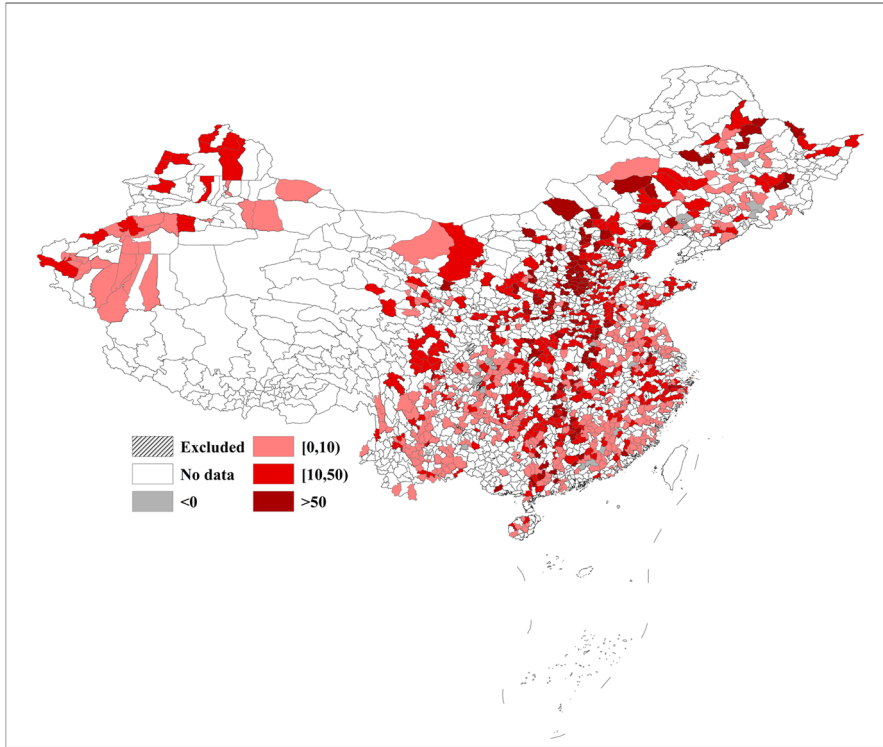
mainly exhibited school expansion at the secondary level, while the growth at the primary level mainly took place before 1965.

Because we focus on the school expansion in less developed rural areas, we exclude more-developed county-level areas that were predominately urban, including three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. There are 2039 remaining counties in the 1990 census. Our core sample consists of 1113 counties with information on the school construction rate. Fewer counties (471) contain information on the number of secondary school teachers. We also use other information from local gazetteers in our supplementary analysis, including the number of SDYs received, annual grain output, and the number of tractors.<sup>15</sup> Figure 4 visualizes the distribution of the school construction rate across the nation.

Although our cohort DID strategy does not require the school construction rate to be exogenously determined, Appendix Table A1 presents its cross-sectional association with some county-level characteristics. The first two columns examine the empirical association between the availability of school construction rates and various county characteristics. We find no significant evidence for such an association.<sup>16</sup> No coefficient estimate is statistically significant, and the R-squared is small ( $\leq 0.004$ ). Columns (3) and (4) provide supporting evidence for our earlier discussion on the determinants of the speed of school expansion. Using the average years of schooling in the control group as a proxy for county-level economic development, we observe a

<sup>15</sup> Strictly speaking, all those numbers (including the number of secondary schools) are aggregated at the county level without being broken down into urban and rural areas. However, because we already excluded more-developed county-level units from our sample, we believe rural areas account for the vast majority part of our sample counties. To ensure that the inability to separate urban and rural schools has limited impacts on our estimation, we perform a heterogeneity analysis in Table 4 regarding the local share of the urban population. We assume that the urban schools account for even smaller proportions in less-urbanized schools, and we find that the per-school effect is larger in those counties.

<sup>16</sup> We face a tradeoff when introducing additional county characteristics. The inclusion of a new variable can provide new information but can also impose sample restrictions since we can only run a regression if the new variable is not missing. Consequently, in the odd columns of Appendix Table A1, we only include the most important variable, which is the baseline schooling level of each county. We introduce more variables that measure the county-level exposure to contemporaneous events in the even columns (see Appendix B for further details).



**Fig. 4** Geographic variations in secondary school construction rate from local gazetteers (unit: annual increase in secondary schools per million inhabitants)

robust positive association between economic factors and the speed of school expansion. Additionally, local densities of SDYs were strongly correlated with an increase in the number of teachers (columns (8)) but not with more schools (columns (4)), underscoring the importance of SDYs as a source of rural teachers during the expansion period. Columns (4) and (8) also explore other proxies for economic development, such as grain production, and historical events intertwined with the school expansion, including the Cultural Revolution and the Great Famine. Given the potential impact of these factors on the school expansion, we allow for the correlation of county-specific cohort trends with the base schooling level (details in Sect. 3.2) and discuss the robustness concerning contemporaneous events (see Appendix Tables B3 and B4).

An alternative measure of school expansion is the increase rate of secondary school teachers (which we refer to as “teacher increase rate” as opposed to “school construction rate”).<sup>17</sup> There are pros and cons associated with using this alternative expansion measure. On the one hand, teachers could be a more decisive factor in promoting education than schools. Therefore, evaluating the policy effect with two different indices allows us to understand the relative importance of teachers versus schools. On the other

<sup>17</sup> Appendix Fig. A1 presents the raw distribution of the school construction rate and the teacher increase rate.

hand, we have information on the number of teachers for far fewer counties, and the pattern of missing information seems to be non-random.<sup>18</sup> Column (8) of Appendix Table A1 indicates that the teacher increase rate exhibits stronger correlations with certain county characteristics in comparison to the school construction rate, although these correlations do not necessarily pose a threat to our identification strategy that relies on the parallel-trend assumption (we will discuss this assumption in Sect. 3.3). Therefore, we adopt the school construction rate as our main measurement of school expansion and use the teacher increase rate mainly for comparison purposes. Finally, we exclude the counties that fall into the top or bottom 1% in school construction rate or teacher increase rate to alleviate the influence of outlier counties; our results are not sensitive to this exclusion.

### Individual-level data

Our main source of individual-level information is the 1% sample from China's 1990 Census. We consider the 1953 cohort to be the first affected by the school expansion program (who reached junior high school entry age in 1966) and the 1962 cohort (who would have entered senior high school in 1977) as the last. Earlier-born cohorts (1943–1952) serve as the control group. We acknowledge that internal migration is a potential concern: by linking the population census with data from the local gazetteers, we implicitly assume that people's residence in 1990 is the same as that during the expansion period.<sup>19</sup> However, we believe this is a reasonable assumption for China before 1990. The household registration system (known as the *hukou* system) was highly restrictive during the 1960s and 1970s, and all internal migration was subject to approval by the local government. China's massive internal migration began in the mid-1990s when the government loosened its control over internal migration, and the rise of private firms and foreign direct investment created a huge demand for cheap labor from rural areas (Meng 2014). Nevertheless, we exclude migrants whose *hukou* county does not match their residence county (0.87% of the original sample) and focus on the sample with rural *hukou*. Our main sample contains 1,115,230 individual observations.

We are mainly interested in students' years of schooling and labor market performance after graduation. Since the population census does not directly ask for years of schooling, we coded this variable according to the individual's highest level of education and the corresponding completion status.<sup>20,21</sup> We also use a set of dummy

<sup>18</sup> Column (6) of Appendix Table A1 indicates that a set of historical events including the send-down movement and the Great Famine can predict the availability of teacher information, but none of those factors can predict the availability of school information (column (2)).

<sup>19</sup> People may also worry about the mortality selection in addition to the migration selection. According to World Bank (2020), the health status of the Chinese population improved greatly during the 1960s and 1970s, and the life expectancy at birth reached 67 years in 1980. The average age of our sample cohorts is about 37 in the 1990 census. Therefore, the mortality selection is unlikely to bias our results.

<sup>20</sup> We assume that people received 6 years of schooling if they graduated from primary school and 3 if they dropped out of primary school. Schooling years for higher-level education are coded similarly.

<sup>21</sup> China's education system switched from 6-3-3 (6 years of primary school, 3 years of junior high school, and 3 years of senior high school) to 5-2-2 in some counties during the Cultural Revolution. We retain

variables (such as junior high graduation) to measure educational attainment. The right panel of Fig. 3 presents the cohort trend of junior and senior high graduation rates. Our treated cohorts (1953–1962) received much more secondary school education than the earlier cohorts. The junior high graduation rate rose to 54.0% for the 1962 cohort, compared to only 19.8% for the 1952 cohort. There is limited information on labor market outcomes in the 1990 census. Therefore, we only focus on labor force participation and occupation. Because most people in our sample were farmers, we assign a dummy variable indicating whether an occupation is agricultural or not.

We further supplement our analysis with a 20% sample of the 2005 1% inter-decennial population census (the 2005 mini-census), which is the only wave of the census we can use to construct the hourly wage rate.<sup>22</sup> We exclude the top and bottom 1% of the wage rate to alleviate the influence of outliers. The 2005 mini-census asks for both residence county and *hukou* county, and we define migrants as those who are not currently residing in their *hukou* county. For the migrant sample, we use *hukou* county to match their indices of school expansion in the empirical analysis because changing *hukou* remains difficult in 2005 although migration was becoming much easier.

Table 1 presents the summary statistics for the control and treatment cohorts. One profound difference between the two groups is that the treatment cohorts are much better educated. Their imputed years of schooling are 1.67 years (or 37.1%) higher than the control group. The increase in secondary schooling mainly accounts for this improvement. Note that the chances of primary school graduation only modestly improved (71.1% versus 57.8%). By contrast, the probability of junior high graduation more than doubled (38.8% versus 16.2%), and the likelihood of senior high graduation is five times higher (11.3% versus 2.1%).

## Firm-level data

We use the 1995 China Industrial Census for the firm-level analysis. Our data covers the full sample of 510,218 industrial enterprises that are affiliated with a township unit (the next level down from a county) or above. There are 189,951 enterprises in our core counties that contain information on school expansion. We are mainly interested in TVEs, which we define as an enterprise that satisfies three conditions: (1) it was affiliated with a township unit, (2) it was collectively owned, and (3) the rural

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the 6-3-3 coding because it was a gradual transition process and not all counties adopted this change or did so in the same way. For example, Bernstein (1977, pp. 21) noted that “since the Cultural Revolution, the length of schooling has been cut, but the system is described as in a state of experimentation and much variation obtains. In some places, a unified four-year middle school has been established. In others, secondary education takes five years and in some even six, the old junior and senior divisions having been retained...” Incorporating the transition into the coding requires strong and somewhat arbitrary assumptions about the switching process. By comparing the imputed years of schooling to a survey that contains precise data on the number of years of schooling at each level (CFPS), Chen et al. (2021) show that our way of coding years of schooling is reasonably close to reality. Nevertheless, most of our empirical analysis focuses on secondary school graduation, which is immune to coding years of schooling.

<sup>22</sup> This mini-census asks about income in the previous month and working hours the previous week. We impute the hourly wage rate as  $\log \frac{\text{monthly income}}{\text{weekly working hours} \times 4}$ .

**Table 1** Summary statistics of the 1990 census data

Sample Cohort	All sample 1943–1962		Control group 1943–1952		Treated group 1953–1962	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Birth cohort	1953.223	5.447	1948.037	2.852	1957.309	2.958
Cohort treatment status	0.297	0.344	0.000	0.000	0.531	0.296
Male=1	0.508	0.500	0.507	0.500	0.509	0.500
Han=1	0.912	0.283	0.912	0.283	0.912	0.283
Labor force participation=1	0.956	0.206	0.950	0.218	0.960	0.195
Non-agricultural industry=1	0.087	0.282	0.075	0.263	0.097	0.296
Imputed years of schooling	5.433	3.631	4.496	3.305	6.171	3.706
Primary school graduate=1	0.652	0.476	0.578	0.494	0.711	0.453
Junior high graduate=1	0.288	0.453	0.162	0.368	0.388	0.487
Senior high graduate=1	0.072	0.259	0.021	0.142	0.113	0.317
Secondary school construction rate	0.021	0.021	0.021	0.021	0.022	0.022
Secondary teacher increase rate	0.294	0.139	0.290	0.137	0.297	0.139
Local density of SDYs	0.017	0.018	0.017	0.017	0.017	0.018
Observations	1,115,230		491,482		623,748	

Note: This table uses the rural sample and excludes three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. We define the cohort treatment status as  $\frac{1}{10}(g - 1952)$  for the treated cohorts (1953–1962) and as zero for the control cohorts (1943–1952). We define the secondary school construction rate (or secondary teacher increase rate) as the annual increase in secondary schools (or teachers) from 1965 to 1977 per 1000 county population in 1964

workforce accounted for more than half of its employment. Our final TVE sample contains 90,027 enterprises.

Appendix Table A2 compares the summary statistics of TVEs to those of other firms, which are either state/collectively owned or privately owned enterprises that were mostly located in urban areas. TVEs are, on average, much smaller than other firms. The average of four production output and input measures (gross industrial output, intermediate industrial input, number of laborers, and stock of capital) of TVEs are all less than half of other firms. TVEs also employed a much larger proportion of rural workers than other firms (93.7% versus 24.0%). One important fact is that most workers in TVEs have a secondary education: 79.9% completed junior high school and 26.6% received a senior high education or higher. The proportion of workers who receive some secondary education in TVEs is quite close to that in urban firms (79.9% versus 86.3%).

### 3.2 Empirical strategy

Our study exploits a cohort DID strategy following Duflo (2001) and Chen et al. (2020a). We rely on two types of variations. First, counties built secondary schools and hired secondary school teachers at different rates during the expansion period. Second, different cohorts within counties were exposed differently to the program. If a rural child was beyond secondary school entry age before the expansion began,

we do not expect her/him to benefit from the school expansion. We set the empirical specification as follows,

$$Y\_Edu_{i,g,c,p} = \beta_0 + \beta_1 \text{Expansion}_{c,p} \times I_g + \beta_2 \mathbf{X}_{i,g,c,p} + \lambda_c + \mu_{g,p} + \mathbf{\Lambda}_c \times \mu_g + \varepsilon_{i,g,c,p}, \quad (1)$$

$$I_g = \begin{cases} \frac{1}{10}(g - 1952), & \text{if } 1953 \leq g \leq 1962 \\ 0, & \text{if } 1943 \leq g \leq 1952 \end{cases}$$

$Y\_Edu_{i,g,c,p}$  refers to the schooling status of individual  $i$  of cohort  $g$  in county  $c$  of province  $p$ , which could either be years of schooling or dummy variables indicating school attendance or graduation.  $\text{Expansion}_{c,p}$  is the index of the school expansion of county  $c$  in province  $p$ , measured as the annual increase in the number of secondary schools (or teachers) from 1965 to 1977 per 1000 county population in 1964.<sup>23</sup>  $I_g$  measures the cohort's exposure to the expansion program. As explained in the previous subsection, we define cohorts 1953–1962 as the treatment group, and earlier-born cohorts (1943–1952) as the control group. Because the expansion process was progressive and spanned over a decade, later-born cohorts would have more schools available when they were supposed to enter junior high school (Fig. 3). This is why we set  $I_g = \frac{1}{10}(g - 1952)$  for cohorts born between 1953 and 1962.<sup>24</sup>

We control for individual characteristics including gender and ethnicity ( $\mathbf{X}_{i,g,c,p}$ ), county fixed effects ( $\lambda_c$ ), and province-cohort fixed effects ( $\mu_{g,p}$ ). The introduction of  $\mu_{g,p}$  allows the cohort effects to be flexible and different across provinces. To further alleviate concerns of possible heterogeneous trends, we introduce the interaction terms between cohort dummies and baseline county characteristics ( $\mathbf{\Lambda}_c \times \mu_g$ ). Because baseline schooling level is a strong predictor of the county expansion process (Appendix Table A1), we include in  $\mathbf{\Lambda}_c$  the reference cohorts' graduation rates from primary school, junior high school, and senior high school. We cluster the standard errors at the county level.

### 3.3 Identification requirements

Because we adopt a cohort DID strategy, which does not require the school expansion rate to be randomly assigned, the central identification requirement is the parallel-trend assumption: in the absence of the program, the cohort trends in schooling should not be related to the intensity of the treatment. Although this assumption is not directly testable because it requires knowledge of the counterfactual scenarios, we provide evidence in favor of the parallel-trend assumption. First, we show in Appendix B2

<sup>23</sup> One advantage of our progressive approach is that it only requires county-level data on secondary schools and teachers at the beginning and end of the expansion, thus reducing data requirements. Since each county maintained its own local gazetteers, the record formats vary, and many counties lack a complete time series. Out of the 1113 counties capable of computing school construction, only about half (571) possess a complete time series. In this sub-sample, as detailed in Appendix B1, we assess the robustness of our progressive approach in comparison to an alternative approach utilizing complete time series.

<sup>24</sup> We show in Appendix Tables B5 and B6 that our results are robust to using  $I_g$  as a dummy variable instead of a progressive treatment.

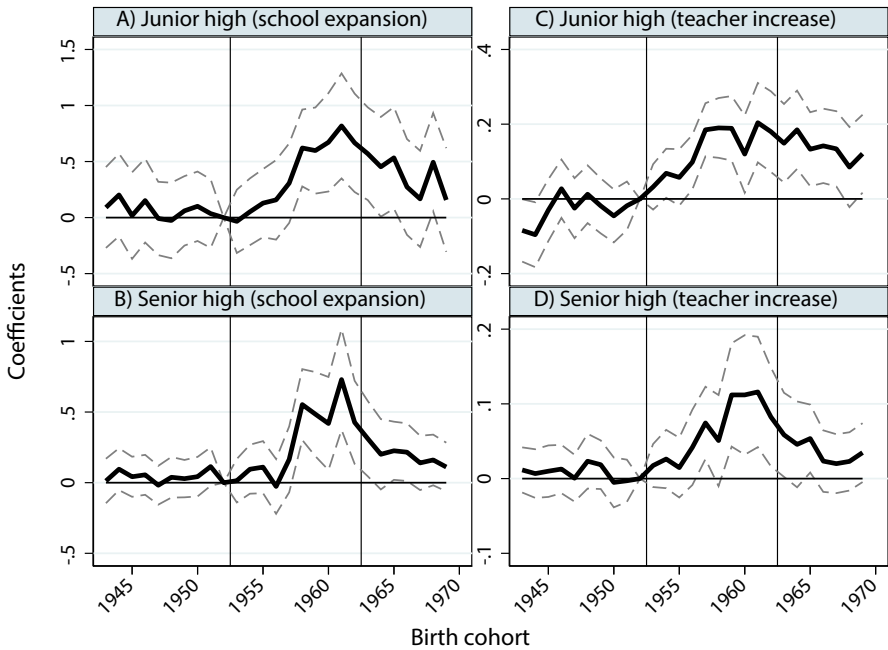


that no other confounding events that took place around the same period (such as the send-down movement or the Cultural Revolution) are driving our main results. Second, we examine the pre-existing trends by breaking down the cohort treatment  $I_g$  into a set of cohort dummies  $\{\mathbb{1}[g = \gamma]\}$ . The equation becomes

$$Y\_Edu_{i,g,c,p} = \beta_0 + \sum_{\gamma=1943:\gamma \neq 1952}^{1970} \beta_{1,\gamma} \text{Expansion}_{c,p} \times \mathbb{1}[g = \gamma] + \beta_2 X_{i,g,c,p} + \lambda_c + \mu_{g,p} + \Lambda_c \times \mu_g + \varepsilon_{i,g,c,p}. \tag{2}$$

Figure 5 plots the coefficients  $\beta_{1,\gamma}$  for each cohort and provides strong evidence that there is no pre-existing trend regardless of how we choose the outcome variable and the indices of school expansion. Before the massive school expansion, the coefficients  $\beta_{1,\gamma}$  fluctuate around zero and are statistically indistinguishable from zero. Starting with the first treatment cohort (1953), the coefficient estimates increase roughly linearly until the last treatment cohort (1962), which strongly supports our progressive treatment specification. The coefficients exhibit a sharp decline after the end of the school expansion (although do not drop to zero), which corresponds to its rollback.

Our main empirical specification Eq. 1 is a two-way fixed effect (TWFE) model. A burgeoning econometric literature points out that even if the parallel-trend assumption holds, the TWFE estimator may not correctly identify the policy effect of interest



**Fig. 5** The effects of school expansion on the educational achievement of each rural cohort. Note: The coefficients are from estimates of Eq. 2 using the 1952 cohort as the baseline, which captures the effect of the school expansion on different cohorts

in the presence of heterogeneous treatment effects because of a negative weighting problem (de Chaisemartin and d'Haultfoeuille 2020; Callaway and Sant'Anna 2021; Goodman-Bacon 2021). To resolve this issue, we adopt a heterogeneity-robust DID estimator that is applicable to a continuous treatment proposed in de Chaisemartin and D'Haultfoeuille (2022) using the `did_multiplegt` package in Stata. We discuss the technical details in Appendix B4. The key takeaway, which we show in Appendix Fig. B1, is that the treatment effects derived from the heterogeneity-robust DID model display similar patterns as those from the TWFE model: the treatment effects fluctuate around zero in the placebo period before the school expansion and rise steadily after the expansion began. If anything, the TWFE estimators underestimate the actual treatment effects.

## 4 The effect of the expansion on the educational achievement of rural children

### 4.1 Main results and the cohort DID specification

Table 2 illustrates how the school expansion campaign promoted rural education based on the 1990 census data. Column (1) shows a statistically significant coefficient of 5.116.<sup>25</sup> The average exposure status ( $I_g$ ) for the treated cohorts is 0.531, and the mean secondary school construction rate was 0.022 schools per thousand inhabitants every year. Combining those numbers indicates a 0.0598 increase per person in schooling years—a number comparable to the effects of compulsory education in the United States during the first half of the twentieth century (Angrist and Krueger 1991; Acemoglu and Angrist 2001; Lleras-Muney 2005), which range from 0.025 to 0.05 years. Regarding the magnitude of the per-school effect, our estimate indicates 0.87 extra years of schooling for each new secondary school per 1000 children aged 5–14<sup>26</sup>—a number about five times the effects of new primary schools in Indonesia (Duflo 2001), which range from 0.12 to 0.19. The low initial enrollment rates in our study can partly explain our relatively large magnitude.<sup>27</sup>

Columns (2)–(7) break down the years of schooling into a set of attendance or graduation dummies. Not surprisingly, the secondary school expansion program mainly improved secondary school graduation rates (columns (4)–(7)); it had a much smaller effect on primary schooling (columns (2)–(3)). The effects on school attendance and

<sup>25</sup> The coefficient estimate is 3.706 and remains statistically significant if we use the 5-2-2 coding of schooling years in Chen et al. (2021) instead of 6-3-3 during the Cultural Revolution. As explained in footnote 21, we prefer the 6-3-3 coding since the shift to 5-2-2 was not uniform, and the 6-3-3 coding was a better fit with the data pattern. Nevertheless, our follow-up analysis mainly focuses on secondary school graduation and is immune to the coding issue.

<sup>26</sup> One new secondary school per 1000 inhabitants implies a value of 1/12 for our defined school construction rate that looks at annual school increases during 1965–1977. The average treatment status is 0.531, and children aged 5–14 accounted for 26% of the total population in the 1964 census.  $0.87 = 5.116 \times \frac{1}{12} \times 0.531 \times \frac{1}{0.26}$

<sup>27</sup> Only about 20% of our sample received some secondary education prior to the school expansion (Table 1). In comparison, about 70% of Indonesian children enrolled in primary schools prior to the school construction program in Duflo's study.

**Table 2** The effect of rural school expansion on completed education (rural sample)

Dependent variable	Years of schooling		Primary school		Junior high school		Senior high school	
	(1)	(2)	Attend (2)	Graduate (3)	Attend (4)	Graduate (5)	Attend (6)	Graduate (7)
Secondary school construction rate	5.116 *** (1.405)	0.185 (0.114)	0.185 (0.114)	0.277 ** (0.136)	0.653 *** (0.202)	0.722 *** (0.205)	0.553 *** (0.140)	0.531 *** (0.136)
× Cohort treatment status	2.339 *** (0.036)	0.224 *** (0.005)	0.224 *** (0.005)	0.259 *** (0.005)	0.248 *** (0.003)	0.222 *** (0.004)	0.068 *** (0.001)	0.064 *** (0.001)
Male=1	0.126 (0.088)	0.022 ** (0.009)	0.022 ** (0.009)	0.018 * (0.011)	0.004 (0.009)	0.006 (0.009)	-0.003 (0.004)	-0.003 (0.003)
Han=1	1,115,229	1,115,229	1,115,229	1,115,229	1,115,229	1,115,229	1,115,229	1,115,229
Observations	0.326	0.249	0.249	0.268	0.233	0.231	0.093	0.090
R-squared	4.496	0.729	0.729	0.578	0.189	0.162	0.023	0.021

Note: All specifications include county FEs, province-cohort FEs, and base schooling×cohort FEs. This table uses the rural sample born between 1943 and 1962 and excludes three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. We define the cohort treatment status as  $\frac{1}{10}(g - 1952)$  for the treated cohorts (1953–1962) and as zero for the control cohorts (1943–1952). We define the secondary school construction rate as the annual increase in the number of secondary schools from 1965 to 1977 per 1000 county population in 1964

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ . We cluster the standard errors at the county level. Base schooling is calculated as the primary/junior high/senior high graduation rates of the control group

graduation are similar because exam-based progression was prohibited during the Cultural Revolution.<sup>28</sup> Therefore, our following analysis focuses on junior high and senior high graduation.

We can measure the school expansion process as either the increase in the number of schools or the number of teachers. Table 3 explores which factor is more relevant to rural children's schooling. It focuses on a sub-sample of 392 counties for which we have data on both schools and teachers. For the treatment group in this sub-sample, the average treatment status ( $I_g$  in Eq. 1 is 0.535. The number of secondary schools (teachers) per 1000 inhabitants grew by 0.021 (0.297) per year during the period 1965–1977. While school construction contributed only 0.112 ( $= 9.981 \times 0.535 \times 0.021$ ) years of schooling, the teacher increase explained 0.291 ( $= 1.834 \times 0.535 \times 0.297$ ) years. The policy effect of teachers on years of schooling is thus 2.60 times that of schools. The ratios for junior high and senior high graduation are 2.78 and 1.97, respectively.

The estimate of 5.116 in column (1) of Table 2 corresponds to 8.18 million more years of schooling with a population of beneficiaries (school-age rural children) of about 136.8 million.<sup>29</sup> The magnitude of the increase rises to 21.26 million if we believe the number of teachers is a more relevant proxy for school expansion, as shown in Table 3. This number is still conservative and is severely underestimated for at least three reasons. First and most importantly, it is hard to precisely count the number of rural schools and teachers every year 50 years ago. The measurement errors in the school construction rate ( $\text{Expansion}_{c,p}$ ) therefore bias our estimates toward zero. The second reason is that we use the 1990 census and implicitly target those who were still in rural areas in 1990. It is well known that education facilitates rural-urban migration and increases the number of educated rural children who leave the countryside (Foster 1965; Zhao 1997). Although this is not a serious issue in our setting because China's massive rural-urban migration had not started in 1990 (Meng 2014), we still miss some rural-born people who gained the most from the school expansion and went to college after the Cultural Revolution. Finally, the control group could have also benefited from new schools if they decided to go back to school.

Using a similar calculation procedure, we infer that the expansion program nurtured an additional 3.21 (1.67) million rural junior (senior) high graduates—again, a very conservative estimate.<sup>30</sup> As a comparison, there were a total of 9.76 million rural junior high graduates and 2.02 million senior high graduates from 1956 to 1965 (see footnote 1). Therefore, the school expansion contributed significantly to a pool of medium-skilled laborers with secondary education in rural China, and the increase in senior high graduates was particularly noteworthy.

<sup>28</sup> For our treated cohorts (1953–1962), 42.3% attended junior high school, and 38.8% reported “graduating,” suggesting a graduation rate of 91.7%. The graduation rate is even higher for senior high school (95.0%).

<sup>29</sup> We infer this number using the 1982 China Population Census, which is very close to the school expansion period. This census indicates that 136.8 million rural children were born between 1953 and 1962 (the treatment group in our subsequent analysis).

<sup>30</sup> The estimated effects come from multiplying the treated population (136.8 million), mean policy exposure (0.531), mean school construction rate (0.022), and the corresponding coefficient (0.722 or 0.531). We further adjust the multiplication by a factor of 2.78 or 1.97 due to the finding that the number of teachers contributes more to this improvement in years of schooling than the number of schools (Table 3).

**Table 3** The effect of school expansion on education—schools versus teachers

Dependent variable	Schools as measurement for expansion Average intensity = 0.021			Teachers as measurement for expansion Average intensity = 0.297		
	Years of schooling (1)	Junior high graduate (2)	Senior high graduate (3)	Years of schooling (4)	Junior high graduate (5)	Senior high graduate (6)
Secondary school construction rate	9.981*** (2.335)	1.380*** (0.389)	0.788*** (0.241)			
× Cohort treatment status				1.834*** (0.326)	0.271*** (0.054)	0.110*** (0.033)
Secondary teacher increase rate						
× Cohort treatment status				374,583	374,583	374,583
Observations	374,583	374,583	374,583	374,583	374,583	374,583
R-squared	0.337	0.240	0.098	0.338	0.240	0.098
Mean of control group	4.347	0.159	0.020	4.347	0.159	0.020
Info on Schools & Teachers	YES	YES	YES	YES	YES	YES

Note: All specifications include county FEs, province-cohort FEs, base schooling × cohort FEs, and individual controls (gender and ethnicity). This table uses the rural sample born between 1943 and 1962 and excludes three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. We define the cohort treatment status as  $\frac{1}{10}(g - 1952)$  for the treated cohorts (1953–1962) and as zero for the control cohorts (1943–1952). We define the secondary school construction rate (or secondary teacher increase rate) as the annual increase in secondary schools (or teachers) from 1965 to 1977 per 1000 county population in 1964

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ . We cluster the standard errors at the county level. All regressions are conditional on containing information on the number of schools and teachers. Therefore, the coefficients in columns (1)–(3) are different from those in Table 2. Base schooling is calculated as the primary/junior high/senior high graduation rates of the control group

Appendices B2 and B3 report the results of a wide range of robustness checks. The first category (Appendix Tables B3 and B4) evaluates the robustness of our findings to a set of contemporaneous historical events, including the send-down movement, the Cultural Revolution, and the Great Famine. The second category (Appendix Tables B5 and B6) examines the robustness of the econometric specification. For example, we set a progressive treatment ( $I_g = \frac{1}{10}(g - 1952)$ ) as our main specification because of the gradual expansion process. An alternative approach is to define a simple dummy treatment status.

Table 4 reports the heterogeneity of our main results to evaluate how the school expansion affected schooling inequality. Specifically, we estimate the following equation:

$$\begin{aligned} Y\_Edu_{i,g,c,p} = & \beta_{0,h}\mathbb{1}_h + \beta_{0,l}\mathbb{1}_l + \beta_{1,h}\text{Expansion}_{c,p} \cdot I_g \cdot \mathbb{1}_h + \beta_{1,l}\text{Expansion}_{c,p} \cdot I_g \cdot \mathbb{1}_l \\ & + \beta_{2,h}\mathbf{X}_{i,g,c,p} \cdot \mathbb{1}_h + \beta_{2,l}\mathbf{X}_{i,g,c,p} \cdot \mathbb{1}_l \\ & + \beta_{3,h}\text{Expansion}_{c,p} \cdot \mathbb{1}_h + \beta_{3,l}\text{Expansion}_{c,p} \cdot \mathbb{1}_l \\ & + \beta_{4,h}I_g \cdot \mathbb{1}_h + \beta_{4,l}I_g \cdot \mathbb{1}_l + \lambda_c + \mu_{g,p} + \mathbf{\Lambda}_c \times \mu_g + \varepsilon_{i,g,c,p}. \end{aligned} \quad (3)$$

In this context, indicator variables  $\mathbb{1}_h/\mathbb{1}_l$  indicate whether a county's baseline schooling level exceeds the average or not. Coefficients  $\beta_{1,h}$  and  $\beta_{1,l}$  represent the per-dose effect of more secondary schools, approximated by  $\text{Expansion}_{c,p}$ , on the outcome variables for different subsamples. Note that we do not need to add the term  $\text{Expansion}_{c,p} \cdot I_g$  because it is broken down to  $\text{Expansion}_{c,p} \cdot I_g \cdot \mathbb{1}_h + \text{Expansion}_{c,p} \cdot I_g \cdot \mathbb{1}_l$ . We performed the heterogeneity analysis on alternative segments of the sample in a similar fashion.

Table 4 presents the heterogeneity of our main results, assessing the impact of the school expansion on schooling inequality. We specifically analyze gender and regional disparities. Our findings indicate that the expansion reduced inequality at the junior high level but increased it at the senior high level. Notably, at the junior high level, females benefited significantly more from the construction of new schools compared to males (column (1)). Prior to the expansion, females had only one-third the likelihood of graduating from junior high schools compared to males (7.9% versus 24.2% for the control group). However, at the senior high level, the expansion had a slightly larger effect on males than on females (column (4)), likely due to the initially low graduation rates for both genders (0.7% for females and 3.3% for males). A similar pattern emerges in regional disparities. At the junior high level, the per-school effect is more pronounced in less-educated and less-urbanized counties (columns (2) and (3)).<sup>31</sup> The gap in coefficients diminishes or even slightly reverses at the senior high level. The findings from Table 4 align with the Maximally Maintained Inequality (MMI) hypothesis (Raftery and Hout 1993), which predicts that school expansion reduces inequality only when enrollment of the most advantaged socioeconomic groups approaches saturation. In our case, when aggregate enrollment rates

<sup>31</sup> We define less-educated counties based on whether the average schooling year of control cohorts is below 4.65, which divides the sample. Similarly, more urbanized counties are defined by a share of urban population below 5.44%, bisecting the sample.

**Table 4** The heterogeneity effect of rural school expansion

Dependent variable	Junior high graduate		Senior high graduate	
	(1)	(2)	(3)	(4)
Secondary school construction rate	-0.214			0.555***
× Treatment × Male	(0.234)			(0.184)
Secondary school construction rate	1.414***			0.300***
× Treatment × Female	(0.275)			(0.141)
Secondary school construction rate		0.540**		0.452***
× Treatment × More-educated counties		(0.243)		(0.164)
Secondary school construction rate		0.810**		0.421**
× Treatment × Less-educated counties		(0.354)		(0.202)
Secondary school construction rate			0.185	0.377**
× Treatment × More-urbanized counties			(0.277)	(0.166)
Secondary school construction rate			1.030***	0.555***
× Treatment × Less-urbanized counties			(0.252)	(0.177)
Observations	198,412	198,412	198,350	198,412
R-squared	0.255	0.253	0.253	0.081
Coefficients equality	0.000***	0.514	0.010**	0.153
Mean of control group	0.185	0.185	0.185	0.020

Note: All specifications include county FEs, province-cohort FEs, base schooling × cohort FEs, and individual controls (gender and ethnicity). We also control for the necessary double interactions such as the interaction between individual controls and *hukou* status. Please refer to Eq. 3 for details. This table uses the rural sample born between 1943 and 1962 and excludes three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. We define the cohort treatment status as  $\frac{1}{10}(g - 1952)$  for the treated cohorts (1953–1962) and as zero for the control cohorts (1943–1952). We define the secondary school construction rate as the annual increase in the number of secondary schools from 1965 to 1977 per 1000 county population in 1964. We report in each column the *p*-value that tests whether the coefficients are the same for each sample. We define more-educated counties according to whether the average schooling year of control cohorts exceeds 4.65, which bisects the sample. We define more urbanized counties according to whether the share of urban population exceeds 5.44%, which also bisects the sample

\* *p* < 0.1; \*\* *p* < 0.05; \*\*\* *p* < 0.01. We cluster the standard errors at the county level. Base schooling is calculated as the primary/junior high/senior high graduation rates of the control group

were low, the expansion exacerbated inequality by prioritizing high socioeconomic status (SES) groups. As enrollment rates increased, the system began admitting low-SES groups, ultimately reducing schooling. The goal of the expansion was to achieve universal junior high education and enable lower-SES groups to catch up. However, senior high schooling was not prevalent, even among high-SES groups at the time.

#### 4.2 Did rural school expansion harm the urban population?

We explained in the background section that the advancement of rural education during the Cultural Revolution was partly through reallocating education resources from urban to rural areas. A natural question is thus how the rural school expansion affected urban schooling during that period. We would like to make two claims before proceeding with our analysis. First, we aim to estimate the effects of the rural school expansion rather than those of the entire Cultural Revolution, which occurred in a much broader context. They were closely related but *different* events. Because we exploit the variation from different counties building different numbers of new secondary schools in rural areas during the Cultural Revolution, we are effectively estimating the impact of new schools. Second, a cost-benefit analysis of the rural school expansion is beyond the scope of this study because (1) there were no estimates of the economic costs of school construction, (2) the costs and benefits of the expansion program fell on different sets of people in rural and urban areas, and it is hard to make interpersonal comparisons, and (3) even if the quantity of schooling is comparable, it is hard to incorporate the quality of schooling (e.g., one year of urban schooling is generally better than that of rural schooling).

Appendix [Table A3](#) estimates and compares the effects of the school expansion on their educational attainment for the rural and urban population in our sampled counties using a similar specification as the heterogeneity analysis [Eq. 3](#). The coefficients for the urban sample are all statistically insignificant and are much smaller than their counterparts using the rural sample. These results confirm that the school expansion mainly took place in rural areas and did not significantly damage urban schooling in the same counties. This analysis also increases the credibility of our empirical design: if the results in [Table 2](#) are driven by some unobserved county-wide heterogeneous trends, the effects are likely to show up in the urban sample as well. We find no significant negative effect of rural school expansion on urban students, probably because the People's Communes were the main forces constructing new schools during the Cultural Revolution (and this is the reason why the number of secondary schools dropped so dramatically after the collapse of the commune system in the early 1980s). Furthermore, the transfer of educational resources from urban schools may have played a limited role, given the rapid expansion of rural schools (a tenfold increase in one decade).

The analysis in Appendix [Tables A3](#) suffers from two limitations, however. First, it does not involve more developed county-level units such as city-governed districts. To partially address this issue, Appendix [Fig. A2](#) presents the cohort trends in secondary school graduation by *hukou* using the unrestricted sample of the 1990 census. We find



that the treated cohorts outside our main sample (including the urban populations of our sample counties and all residents of city-governed districts or three municipalities) also experienced significant increases in secondary school attainment during the expansion period. As a second limitation, the census data only ask for the current *hukou* status, but some urban residents in 1990 might have rural *hukou* during the Cultural Revolution. As a result, the rural school expansion could have also affected some individuals who were living in cities at the time of the 1990 census. To resolve this issue, Appendix Fig. A3 uses the 2010 wave of the CFPS, which contains respondents' *hukou* status at age 3, but the empirical pattern is the same as when we use the census data. To summarize, we find no evidence that the rural school expansion caused severe negative consequences on the urban population, even though urban education might have been damaged by other events (e.g., the suspension of national college entrance examinations).

## 5 The effect of the expansion on rural development

Above, we evaluated the *individual* impact of the school expansion campaign: rural children who were more exposed to the program achieved more years of schooling. In this section, we highlight that the expansion's influence could go beyond the *individual* level to the *county* or *firm* level and benefit rural development in terms of agricultural and industrial production. One reason is that the education content during the expansion period was more practically oriented (such as “agricultural knowledge” and “industrial knowledge”). The courses covered not only important agricultural skills such as developing new crop seeds but also how to operate and repair machines including internal combustion engines, electric motors, radios, and amplifiers (Bo 1982). According to an interview reported in Han (2001), farming equipment (including tractors) was mostly operated by secondary school students trained during the school expansion period. The expansion period also focused on vocational education. The target was not to pursue tertiary education (the college entrance exam was temporarily suspended), but to train specialists for a particular occupation. These specialists could help boost the productivity of TVEs. In addition, the traditional view that promoting education may be not effective in boosting rural development because villagers view schools as a means to escape rural life (Foster 1965) did not work in China during the Cultural Revolution period. The strict migration restriction accounts for this inapplicability. Therefore, the better-educated rural villagers were bound up with the rural land.

### 5.1 Agricultural production

To understand the influence of the school expansion on local agricultural productivity, we collect data on annual grain output and the number of small and large tractors at the county-year level for the period 1965–1985 from local gazetteers and provincial agricultural statistics. All variables were scaled by county population in 1964. Appendix Fig. A4 plots the time-series index (the year 1985 = 100) of those variables. The new data structure (county-year level data instead of individual-level data) requires a

new empirical specification and a new method of defining exposure to the expansion program in each year  $t$ . We calculate the new exposure  $I_t$  in three steps.

$$I_t = \frac{\sum_i I_g \times \mathbb{1}[20 \leq t - g \leq 50]}{\sum_i \mathbb{1}[20 \leq t - g \leq 50]}. \quad (4)$$

First, we assign individual exposure as our main specification Eq. 1 for individual  $i$  of cohort  $g$  in the 1990 census:  $I_g = \frac{1}{10}(g - 1952)$ , if  $1953 \leq g \leq 1962$ , and zero otherwise. Second, we infer whether individual  $i$  of cohort  $g$  belongs to the peak workforce age period (age 20–50) in year  $t$  using the 1990 census.<sup>32</sup> For example, an individual aged 60 in 1990 was probably no longer working in the census year. But back in 1975, he/she was aged 45 and was still at the peak of labor force participation. Third, we combine the previous two steps to compute a mean policy exposure  $I_t$  for a pseudo sample of individuals who were aged 20–50 in year  $t$ . Appendix Fig. A6 plots the time series of this variable. The key implication here is that the school expansion campaign gradually started to affect the rural labor force in 1972, and its influence increased steadily until the mid-1980s.

Therefore, we can evaluate the county-level impact of the school expansion by estimating the following DID equation:

$$Y_{t,c,p} = \beta_0 + \beta_1 \text{Expansion}_{c,p} \times I_t + \lambda_c + \mu_{t,p} + \varepsilon_{t,c,p}, \quad (5)$$

where  $Y_{t,c,p}$  represents the outcome variable of county  $c$  in province  $p$  in year  $t$ . The following equation examines the pre-existing trend and looks into the dynamic policy effects:

$$Y_{t,c,p} = \beta_0 + \sum_{\gamma=1965:\gamma \neq 1971}^{1985} \beta_{1,\gamma} \text{Expansion}_{c,p} \times \mathbb{1}[t = \gamma] + \lambda_c + \mu_{t,p} + \varepsilon_{t,c,p}. \quad (6)$$

Table 5 reports the regression outcomes of Eq. 5, and Fig. 6 and Appendix Fig. A7 plot the time series of  $\beta_{1,\gamma}$  in Eq. 6 for a set of county-level outcomes. Panel A of Fig. 6, which charts how the school expansion affected grain productivity, deserves special attention. The association between grain output and school construction fluctuates around zero prior to 1972 and rises steadily afterward. Note that annual grain output increased roughly linearly from 1965 to 1985 (Appendix Fig. A4). Grain output only became closely related to the secondary school construction rate after 1971.<sup>33</sup>

<sup>32</sup> Appendix Fig. A5 supports our definition of age range 20–50 as the “peak workforce age” by presenting the age-specific rate of labor force participation using the 1990 census.

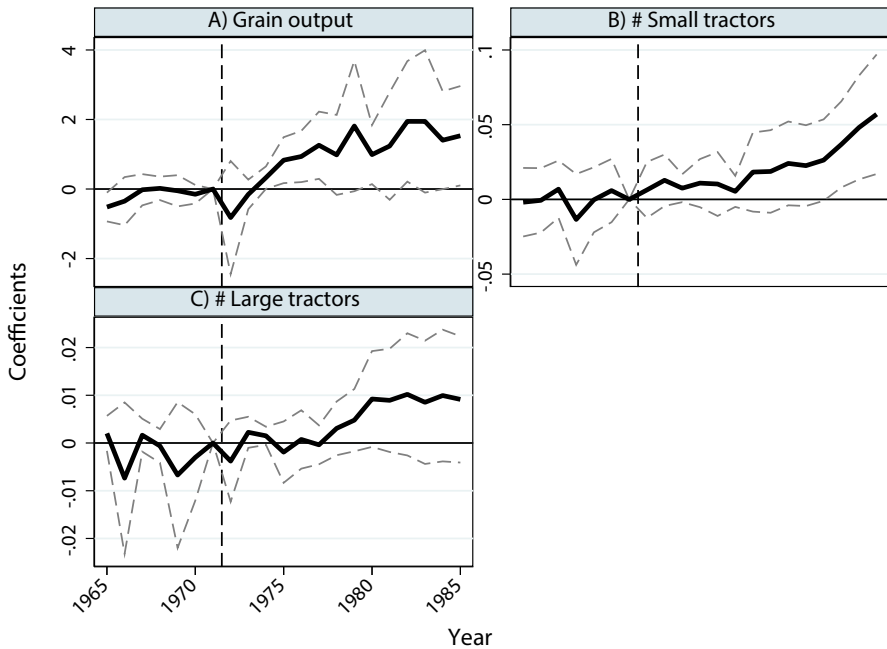
<sup>33</sup> Because our county-level analysis adopts a DID strategy, a possible concern is that other contemporaneous events might also affect agricultural productivity. One such event is the gradual adoption of the household responsibility system across the country since 1979, which spurred a remarkable growth in agricultural output (McMillan et al. 1989; Lin 1992). We address this concern by controlling for a variable that indicates whether county  $c$  had initiated the household responsibility system in year  $t$  using the county-level rollout data from Almond et al. (2019). We confirm that controlling for this rural reform has little impact on our estimates. The logic is simple: the two events affected agricultural production at different times. Most counties experienced the rural reform from 1979 to 1981, but the school expansion program gradually benefited agricultural productivity after 1972 (Fig. 6).

**Table 5** How did the school expansion program benefit county-level agricultural production?

Dependent variable	Schools as measurement for expansion Average intensity = 0.022		Teachers as measurement for expansion Average intensity = 0.297	
	Grain output (1)	Number of small tractors (2)	Grain output (4)	Number of small tractors (5)
Secondary school construction rate	8.985** (4.222)	0.173 *** (0.057)		
× Year mean exposure		0.057 (0.044)		
Secondary teacher increase rate			0.643** (0.317)	0.026 *** (0.009)
× Year mean exposure				0.011 *** (0.002)
Observations	14,958	9204	6034	3693
R-squared	0.810	0.803	0.815	0.814
Mean of reference years	0.355	0.000	0.349	0.000

Note: All specifications include county FEs and province-year FEs. This table uses county-year level data for the period 1965–1985 and excludes three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. Section 5.1 describes how we constructed annual policy exposure. We define the secondary school construction rate (or secondary teacher increase rate) as the annual increase in secondary schools (or teachers) from 1965 to 1977 per 1000 county population in 1964

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ . We cluster the standard errors at the county level



**Fig. 6** The effects of school expansion (schools as measurement) on local agricultural production. Note: The coefficients are from estimates of Eq. 6 using 1971 as the baseline, which captures the effect of the school expansion on a set of county-level agricultural outcomes

This coincidence provides suggestive evidence that recent school graduates from the new wave of secondary school expansion contributed to agricultural productivity. Our estimates, reported in Table 5, suggest that the school expansion can account for 14% of the increase in agricultural productivity from 1965 to 1985.<sup>34</sup> One potential channel is that some of the secondary school students were trained to operate and repair tractors during the expansion period. Panels B and C of Fig. 6 support this channel by demonstrating that the school construction rate was related to the greater usage of small or large tractors since the mid-1970s. Using the rate of the increase in the number of teachers as the alternative measure of school expansion (columns (4)–(6) of Table 5 and Appendix Fig. A7) yields similar findings. Interestingly, while more teachers are much more effective at boosting years of schooling than more schools, both have similar effects on grain output ( $8.985 \times 0.022$  versus  $0.643 \times 0.297$ ).

## 5.2 Productivity of TVEs

The TVEs played an important role in the Chinese economy in the first two decades of economic reforms and opening (Lin and Wang 2012; Brandt et al. 2017). These enter-

<sup>34</sup> The coefficient estimate for grain output per capita in Table 5 is 8.985. Multiplying the coefficient by the mean policy exposure in 1985 (0.198) and the average school construction rate (0.022 per thousand inhabitants per year) generates the estimated policy effect. The average grain output per thousand inhabitants for counties in the regressions is 0.60 (0.32) tons in 1985 (1965).  $\frac{8.985 \times 0.198 \times 0.022}{0.60 - 0.32} \approx 0.140$

prises were located in rural areas, run by township and village leaders, and employed rural workers; they contributed over one-third of China’s industrial output in the mid-1990s. Therefore, it would be important to examine the extent to which the school expansion program provided medium-skilled labor and affected the productivity of TVEs.

We use the TVE sample of the third National Industrial Census in 1995 and focus on the same set of counties in our sample that excludes all municipalities and city-governed districts. The National Industrial Census of 1995 only provides a cross-sectional dataset of TVEs and the age and educational structure of their workers. As a result, we cannot perform a standard DID estimation over time. Instead, we exploit variations in worker age structure across firms and compute firm-level policy exposure. Due to data limitations, this new strategy must rely on some assumptions (e.g., the uniform distribution of age among TVE workers for each age group) and is thus less rigorous than the cohort DID approach that uses individual-level data.

We perform the empirical analysis in the following steps. First, we compute the average treatment status for the  $k$ th cohort group  $I_{G_k}$  using the 1990 census:

$$I_{G_k} = I_{[k_a, k_b]} = \frac{\sum_{g=k_a}^{k_b} N_g \times I_g}{\sum_{g=k_a}^{k_b} N_g}, \tag{7}$$

where  $N_g$  represents the cohort size and  $I_g = \frac{1}{10} (g - 1952)$ , if  $1953 \leq g \leq 1962$ . In a second step, we define firm-level exposure to the program as  $EXPO_i = \sum_k s_{i,k} \cdot I_{G_k}$ , where  $s_{i,k}$  represents the proportion of firm  $i$ ’s workers from cohort group  $k$ .<sup>35</sup> Lastly, we use econometric models to evaluate the influence of a firm’s exposure to the rural school expansion program. The first outcome variable of interest is a worker’s average years of schooling ( $Y\_Edu_i$ ) for firm  $i$ .<sup>36</sup> We estimate the following equation:

$$Y\_Edu_i = \beta_0 + \beta_1 \text{Expansion}_c \times EXPO_i + \sum_k \beta_{2,k} s_{i,k} + \mu_d + \lambda_c + \varepsilon_i, \tag{8}$$

where  $\mu_d$  and  $\lambda_c$  represent the four-digit industry fixed effects and county fixed effects, respectively. Because  $EXPO_i$  can be expressed as a linear combination of workers’ age structure  $\{s_{i,k}\}$ , the coefficient  $\beta_1$  is a DID estimator when we control for county fixed effects ( $\lambda_c$ ) and firms’ age structure  $\{s_{i,k}\}$ . Columns (1) and (4) of Table 6 report the estimates. The magnitude of the estimated effects on firms’ average years of education is identical to the effect on individuals.<sup>37</sup> This finding bolsters our firm-level

<sup>35</sup> The 1995 industrial census reports the proportion of TVE workers in four age ranges: below 20, 21–35, 36–50, and over 50. The average treatment status for those four groups is 0, 0.1165, 0.1991, and 0, respectively. Therefore,  $EXPO_i = 0.1165 \cdot s_{i,2} + 0.1991 \cdot s_{i,3}$ .

<sup>36</sup> The 1995 industrial census contains information on the education structure of the workforce (e.g., the share of workers that have junior high as the highest level of education).

<sup>37</sup> The average policy exposure of the TVE sample is 0.122. If we measure rural school expansion as more schools (column (1)), the coefficient estimate (23.717) multiplied by the school construction rate (0.021) and firm policy exposure (0.122) yields an estimated magnitude of 0.061 years. At the individual level, the school construction contributed 0.0598 years of schooling based on the estimates from Table 2.

**Table 6** How did the school expansion program benefit township and village enterprises?

Dependent variable	Schools as measurement for expansion Average intensity = 0.021		Teachers as measurement for expansion Average intensity = 0.297	
	Firm's mean years of schooling (1)	log(value added) (2)	Firm's Mean Years of Schooling (4)	log(value added) (5)
Secondary school construction rate	23.717 ** (9.237)	11.991* (6.614)	11.891* (6.591)	
× Firm policy exposure			4.183 * (2.385)	3.340 ** (1.638)
Secondary teacher increase rate				
× Firm policy exposure		0.753*** (0.009)		0.762 *** (0.014)
log(labor)		0.225*** (0.005)		0.228 *** (0.008)
log(capital)				0.050*** (0.019)
log(labor) <sup>2</sup>				0.033*** (0.006)
log(capital) <sup>2</sup>				0.026*** (0.003)
log(labor) × log(capital)				-0.026*** (0.007)
Observations	84,976	77,174	37,608	34,332
R-squared	0.271	0.666	0.279	0.674
Mean of control group	9.197	5.583	9.150	5.626

Note: Note: All specifications include county FEs, province-year FEs, and firms' age structure (the share of the workforce aged below 20, 21–35, 36–50, and over 50). This table uses a TVE sample of the 1995 China Industrial Census and excludes three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. Section 5.2 describes how we constructed firm-level policy exposure. We define the secondary school construction rate (or secondary teacher increase rate) as the annual increase in secondary schools (or teachers) from 1965 to 1977 per 1000 county population in 1964

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ . We cluster the standard errors at the county level

specification that exploits a different approach to constructing the policy. We further find that the policy effect measured by more teachers is about triple that measured by more schools at the firm level,<sup>38</sup> which again corresponds to our individual-level findings (Table 3). To summarize, the policy effect of rural school expansion at the individual level largely translates to that at the firm level for TVEs, which lays the foundation for our following analysis.

To estimate whether the school expansion program boosted the TFP of TVEs by providing more medium-skilled labor, we use the following Cobb-Douglas production function:

$$\log(va_i) = \gamma_0 + \gamma_1 \text{Expansion}_c \times \text{EXPO}_i + \sum_k \gamma_{2,k} s_{i,k} + \gamma_3 \log(k_i) + \gamma_4 \log(l_i) + \mu_d + \lambda_c + \varepsilon_i, \quad (9)$$

where  $va_i$ ,  $k_i$ , and  $l_i$  represent the added value (gross industrial output minus intermediate input), the net value of fixed assets for production, and the total workforce of firm  $i$ . We also estimate the translog production specification that allows for interaction and square terms:

$$\log(va_i) = \gamma_0 + \gamma_1 \text{Expansion}_c \times \text{EXPO}_i + \sum_k \gamma_{2,k} s_{i,k} + \gamma_3 \log(k_i) + \gamma_4 \log(l_i) + \gamma_5 \log(k_i)^2 + \gamma_6 \log(l_i)^2 + \gamma_7 \log(k_i) \cdot \log(l_i) + \mu_d + \lambda_c + \varepsilon_i. \quad (10)$$

The remaining four columns of Table 6 present the estimation of the production functions. The estimated capital and labor elasticities are 0.75 and 0.23, which are comparable to those for other developing countries (De Loecker et al. 2016; Gandhi et al. 2020). Our estimates suggest that the school expansion program increased TVEs' productivity by 3.2% (if measured by more schools) or 12.9% (if measured by more teachers).<sup>39</sup> Although the result is preliminary due to data limitations, Table 6 provides interesting evidence of the positive effect of rural school expansion on the subsequent development of TVEs. The gain in TFP from the school expansion comes from at least two sources. First, better-educated workers usually had higher labor productivity. Fleisher et al. (2011) estimated that an additional year of schooling raises workers' marginal productivity by 30.1% using firm-level data from China. This rate is much larger than the returns to schooling on individual earnings in China: previous instrumental variable estimates indicate a range of 10–20% (Fang et al. 2012; Giles et al. 2019; Chen et al. 2020b). Second, because the school expansion boosted each affected region's human capital, the spillover benefits to a firm could go beyond improving the schooling level of its own workers (Moretti 2004).

<sup>38</sup> Following the calculation in footnote 37, the policy effect measured by more teachers is 0.152 ( $4.183 \times 0.297 \times 0.122$ ).

<sup>39</sup> Following the calculation in footnote 37, the school expansion increased the TFP by 0.121 log points if we take the estimate from column (5) of Table 6, which could be translated into a 12.9% increase.

## 6 Labor market outcomes and possible deterioration in education quality

The previous sections show that the secondary school expansion program increased individual years of schooling and contributed to rural agricultural and industrial development. A natural follow-up concern is about the potential costs of such a rapid school expansion. In this section, we provide suggestive evidence that counties that expanded secondary schools more rapidly may have experienced a faster deterioration in the quality of schooling. We reach this conclusion in two steps. First, we show that the school expansion *did not* improve individual labor market outcomes. Second, we provide three pieces of indirect evidence on the deterioration in schooling quality. Those pieces together, albeit none of them is perfect, can offer some insights into the quantity-quality tradeoff in education during the Cultural Revolution.

Panel A of Table 7 finds no evidence that school expansion during the Cultural Revolution created better individual labor market outcomes. In 1990, people who were more exposed to the expansion did not participate more in the labor force (column (1)) and were not more likely to acquire more skilled non-agricultural jobs (column (2)). The 2005 mini-census provides more information on labor market outcomes, including the wage rate last month and the migration status, but still shows no sign of statistical significance (columns (3) and (4)).<sup>40</sup> What accounts for this null effect? If we presume that both the *quantity* and *quality* of schooling benefit the labor market, and the previous section confirms that there was an increase in the quantity of schooling, one reasonable conjecture is that the quality of schooling decreased during the expansion period. As Panel B shows, once we control for schooling *quantity*, faster school expansion drove people away from non-agricultural occupations.

At first glance, the insignificant improvement in labor market performance at the individual level seems odd given our previous findings of the positive effects of the reform on agricultural production and the productivity of TVEs. We wish to highlight that the expansion's null effect on *individual* labor market outcomes does not necessarily contradict the positive impacts on the more aggregated county- or firm-level for two reasons. First, the analysis of individual labor market outcomes primarily focuses on the extensive margin of whether an individual takes a non-agricultural job or not. Census data provide limited information on labor market outcomes, including only one's occupation and industry. Given that most rural areas of China remained underdeveloped in the 1990 census year, and more than 90% of our sample worked in the agricultural sector, the individual-level analysis of labor market outcomes mainly pertains to the extensive margin, i.e., whether an individual takes a non-agricultural job. Therefore, the null effect at the extensive margin does not necessarily imply that the expansion has no impact at the intensive margin. For instance, because the education content during the expansion period was more practically oriented (the curriculum covered courses like "agricultural knowledge" and "industrial knowledge"), people could become more knowledgeable farmers and enhance agricultural productivity,

<sup>40</sup> The 1990 census only covers the extensive margin of labor force participation. In contrast, the 2005 mini-census also inquires about the intensive margin, specifically the number of hours worked last week. Appendix Table A4 displays the corresponding results with working hours as the outcome variable. Columns (1)–(2) show no evidence that school expansion increases the intensive margin of the labor supply.



**Table 7** Suggestive evidence on the deterioration of education quality

Data Dependent variable	Census 1990		Census 2005	
	Labor force participation (1)	Non-agricultural occupation (2)	Wage rate (log) (3)	Migration (4)
<b>Panel A: Effect on labor market outcomes</b>				
Secondary school construction rate	-0.064	-0.073	0.035	0.049
× Cohort treatment status	(0.066)	(0.090)	(0.282)	(0.077)
Observations	1,115,229	1,065,823	169,740	205,491
R-squared	0.193	0.138	0.347	0.426
<b>Panel B: Control for schooling</b>				
Secondary school construction rate	-0.066	-0.188 **	-0.041	0.044
× Cohort treatment status	(0.067)	(0.088)	(0.276)	(0.077)
Observations	1,115,229	1,065,823	169,740	205,491
R-squared	0.195	0.172	0.354	0.426
Schooling FE	YES	YES	YES	YES
<b>Panel C: Heterogeneous effects w.r.t. SDY densities</b>				
Secondary school construction rate	-4.026	6.272*	9.014	4.652**
× Cohort treatment status	(3.837)	(3.692)	(10.883)	(2.346)
× Local density of SDYs				
Observations	1,048,150	1,000,783	157,963	191,235
R-squared	0.199	0.174	0.352	0.434
Schooling FE	YES	YES	YES	YES
Double interactions	YES	YES	YES	YES
Mean of control group	0.949	0.077	0.380	0.016

Note: All specifications include county FEs, province-cohort FEs, base schooling×cohort FEs, and individual controls (gender and ethnicity). This table uses the rural sample born between 1943 and 1962 and excludes three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. We define the cohort treatment status as  $\frac{1}{10}(g - 1952)$  for the treated cohorts (1953–1962) and as zero for the control cohorts (1943–1952). We define the secondary school construction rate as the annual increase in the number of secondary schools from 1965 to 1977 per 1000 county population in 1964. The density of SDYs is calculated as the number of SDYs a county received during the period 1968–1977 as a share of the county population in 1964. “Double interactions” refer to that we also control for the necessary double interactions such as the interaction between individual controls and the local density of SDYs. Please refer to Eq. 11 for details

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ . We cluster the standard errors at the county level. Base schooling is calculated as the primary/junior high/senior high graduation rates of the control group

even if the school expansion campaign did not help them secure non-agricultural jobs. Second, individual labor market returns to schooling may underestimate the social returns at a more aggregated level due to the widely documented positive externalities of education (Acemoglu and Angrist 2001; Moretti 2004; Lange and Topel 2006). Furthermore, the general equilibrium effect of a large-scale school expansion can dampen the returns to schooling at the individual level (Khanna 2023). For example, the school expansion increased the supply of secondary school graduates and lowered their equilibrium wages. However, from firms’ perspectives, they could benefit from both having more educated workers and lower equilibrium wages.

What explains the lack of improvement in the labor market? While we don't want to disregard the possibility of a general equilibrium effect,<sup>41</sup> we want to emphasize the potential decline in schooling quality by presenting three pieces of evidence.

First, we compare the outcomes of this study with the impact of SDYs on rural education, as demonstrated in Chen et al. (2020a). They faced a similar situation where a group of rural children were exposed to SDYs who had received higher-quality education in urban areas and possessed more modern knowledge. In contrast, Chen et al. (2020a) illustrated that the arrival of SDYs not only increased the *quantity* of rural education but also enhanced its *quality*, measured by the likelihood of pursuing high-skilled occupations. We hypothesize that a shortage of qualified teachers is a key factor explaining the decline in schooling quality. Several case studies have identified teacher shortages as a significant obstacle to school expansion.<sup>42</sup> During this period, there was a tenfold increase in secondary schools, but it was a challenging time for intellectuals who were frequently attacked and labeled "bad" classes (Bernstein 1977; Walder 1989), leading to a reduced supply of qualified teachers. This teacher shortage explanation aligns with the findings of Alderman et al. (2003) who studied a school expansion program in Pakistan. They found that the expansion succeeded in urban areas but was relatively unsuccessful in rural areas, with the availability of teachers identified as a contributing factor to the disparate outcomes.

Second, we test the teacher shortage hypothesis by showing that this shortage can be partly alleviated with the arrival of SDYs. One possible reason that the exposure to SDYs improved labor market outcomes but the exposure to the school expansion did not is that the SDYs could be an important source of qualified teachers and alleviate the teacher shortage problem. Specifically, we examine whether SDYs were more effective in enhancing the quality of rural education or mitigating its decline using the following triple-difference specification:

$$\begin{aligned}
 Y_{i,g,c,p} = & \beta_0 + \beta_1 \text{Expansion}_{c,p} \times I_g \times \%SDY_{c,p} + \beta_2 \text{Expansion}_{c,p} \times I_g \\
 & + \beta_3 \%SDY_{c,p} \times I_g + \beta_4 \mathbf{X}_{i,g,c,p} + \beta_5 \mathbf{X}_{i,g,c,p} \times \%SDY_{c,p} \\
 & + \text{Edu}_i + \lambda_c + \mu_{g,p} + \mathbf{\Lambda}_c \times \mu_g + \varepsilon_{i,g,c,p}.
 \end{aligned} \tag{11}$$

The above regression aims to utilize labor market outcomes ( $Y_{i,g,c,p}$ ), conditional on the fixed effects of the "quantity" of schooling ( $\text{Edu}_i$ ), as a proxy for the "quality" of schooling. If an increase in SDYs indeed contributed to improving schooling quality or slowing down its deterioration, we would anticipate a positive estimate of  $\beta_1$ . We present the results in Panel C of Table 7, and the estimates align with our expectations.<sup>43</sup>

<sup>41</sup> The logic of the general equilibrium effect suggests that the sudden increase in secondary school graduates might diminish their relative advantage in the labor market.

<sup>42</sup> In a study of Jimo county, Han (2001) noted that locally built schools were inferior to those in towns due to the inability to offer certain courses caused by a shortage of teachers, a common issue in rural areas. Another case study of Laishui county, Andreas (2004) listed the shortage of qualified teachers as the principal practical obstacle to expanding village education, particularly during the ambitious expansion of the Cultural Revolution decade.

<sup>43</sup> Column (3) in Appendix Table A4 applies the same triple-difference specification, substituting the outcome variable with the hours worked last week. In line with our results in Panel C of Table 7, we observe that an increase in SDYs correlates with more positive effects of the school expansion program.

Third, we provide more direct evidence on schooling quality by showing the differential effects of government-run (*gongban*) schools and people-run (*minban*) schools. One common limitation of the previous two pieces of evidence is that they cannot rule out the direct channel of SDYs other than through education, such as peer effects and role modeling. From local gazetteers, we identified a potential quality indicator for schools: whether they are government-run (*gongban*) or people-run (*minban*). Generally, government-run schools are considered to be of higher quality. However, we can only make this distinction for a small subset of counties (38 counties). In this subset, we can categorize the increase in total secondary schools into government-run (29.5%) and people-run (70.5%). Panel A of Table 8 presents the effects of new government-run and people-run secondary schools separately.<sup>44</sup> It's important to note that this table contains considerably fewer observations than our other tables. The first two columns report the effects of these two types of secondary schools on junior high and senior high graduates, confirming that government-run schools have a more substantial positive impact on years of schooling. This finding supports the hypothesis that government-run schools are generally of higher quality. The remaining four columns present the effects on labor market outcomes. Although the coefficients for government-run schools are quantitatively larger than those for people-run schools, they are statistically insignificant due to the smaller sample size. In summary, Table 8 provides suggestive evidence that higher-quality schools yield more positive outcomes.

## 7 Conclusions

Education is undoubtedly an important driving force of economic development. On the eve of its market-oriented economic reforms, China achieved a higher-than-world-average secondary school enrollment rate with a per capita GDP of about 5% of the world average. Our study is the first to exploit a unique dataset to explore how it accomplished such educational attainment unparalleled by its level of economic development. We focus on an important historical event that previous research has largely overlooked: a massive rural school expansion program during the Cultural Revolution. The number of secondary schools grew more than tenfold from 18,102 in 1965 to 201,268 in 1977.

Analyzing hand-collected data from thousands of book-length local gazetteers, we systematically and quantitatively evaluated the program's effects on rural education. While the construction of more schools and the recruitment of more teachers both significantly increased rural children's enrollment in secondary education, we find that the latter had a much larger effect than the former. Our conservative estimates suggest that the campaign nurtured at least 3.21 (1.67) million junior (senior) high graduates. We find no evidence that the rural school expansion caused a significant decline in urban education. The curriculum, although shortened and more practically oriented, better fits the needs of rural production settings (e.g., how to operate and repair farm machines). Our empirical analysis shows that agricultural productivity

<sup>44</sup> Panel B of Table 8 presents the results using total schools without further distinguishing between government-run and people-run schools in this subsample of 38 counties. Despite the reduced sample size, we can still observe a strong positive effect on junior high graduation.

**Table 8** The differential effects of government-run and people-run secondary schools

Data Dependent variable	Census 1990		Census 2005			
	Junior high graduate (1)	Senior high graduate (2)	Labor force participation (3)	Non-agricultural occupation (4)	Wage rate (log) (5)	Migration (6)
<b>Panel A</b>						
Government-run secondary school construction rate	7.749*** (2.307)	2.222** (0.930)	-0.079 (0.278)	-0.801 (1.262)	2.480 (2.190)	2.041* (1.204)
× Cohort treatment status						
People-run secondary school construction rate	2.227* (1.312)	-0.537 (0.664)	-0.236 (0.243)	-0.778 (0.540)	-4.676 (4.038)	1.080 (0.878)
× Cohort treatment status						
Observations	61,377	61,377	61,377	60,151	8,285	9,475
R-squared	0.191	0.094	0.116	0.086	0.265	0.739
<b>Panel B</b>						
Secondary school construction rate	4.304*** (1.049)	0.561 (0.597)	-0.135 (0.177)	-0.785 (0.472)	-1.892 (2.921)	1.468* (0.840)
× Cohort treatment status						
Observations	61,377	61,377	61,377	60,151	8,285	9,475
R-squared	0.191	0.094	0.116	0.086	0.265	0.739
Info on government/people-run schools	YES	YES	YES	YES	YES	YES
Mean of control group	0.126	0.012	0.972	0.059	0.287	0.019

Note: All specifications include county FEs, province-cohort FEs, base schooling × cohort FEs, and individual controls (gender and ethnicity). This table uses the rural sample born between 1943 and 1962 and excludes three municipalities (Beijing, Tianjin, and Shanghai) and all city-governed districts. We restrict the analysis to counties that can divide secondary schools into government-run and people-run. We define the cohort treatment status as  $\frac{g - 1952}{10}$  ( $g - 1952$ ) for the treated cohorts (1953–1962) and as zero for the control cohorts (1943–1952). We define the government/people-run secondary school construction rate as the annual increase in the number of government/people-run secondary schools from 1965 to 1977 per 1000 county population in 1964

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ . We cluster the standard errors at the county level. Base schooling is calculated as the primary/junior high/senior high graduation rates of the control group

gradually increased as the rural school expansion program introduced more secondary school graduates into the labor force. We estimate that the school expansion accounted for 14.0% of the increase in agricultural productivity from 1965 to 1985. We further find suggestive evidence that the program also improved TVEs' productivity by contributing to a more educated rural workforce. However, creating a tenfold increase in secondary schools in just one decade made it difficult to maintain the quality of schooling. We provide suggestive evidence that counties that built new schools more rapidly experienced larger declines in schooling quality and that the decline is likely to originate from a lack of qualified teachers.

As mentioned above, our study does not seek to undertake a cost-benefit analysis of the rural school expansion campaign during the Cultural Revolution, as it is tremendously difficult to quantify the cost of this massive campaign for both rural and urban areas for two reasons. First, there is no historical documentation that can precisely estimate the economic cost of labor and physical inputs to build a new school and to recruit or train new teachers. Second, although we find no evidence that the program negatively affected urban education attainment, the reallocation of some teachers from urban to rural areas may have jeopardized the quality of urban education in ways that we do not observe here. Nevertheless, our study highlights the effect of an understudied historical event on education in rural areas that could play a vital role in understanding China's post-reform economic miracle.

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