Long-Run Impacts of Public Investments in Children's Nutrition on Education: Evidence from Rural China*

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Abstract

There have been many discussions about the effects of school meal programs in the literature. But these programs' long-term impacts on educational attainment and intergenerational mobility in education, especially for developing countries, are still under-investigated. To fill this gap, this paper explores a nutrition improvements program implemented in rural China using China Family Panel Study data and a Difference-in-Differences strategy. The results show that this policy improved educational attainment for rural students: One additional year of policy exposure led to a 0.14-year increase in schooling, a 9.3 percent increase in compulsory education completion, and a 37 percent decrease in illiteracy rate. Girls and students from poverty-stricken counties and low-income families, benefited more. The improvements in educational attainment were mainly driven by better health status, fewer absence days, a lower likelihood of absence for illness, and better academic performance. Further analyses show that this policy also promoted intergenerational mobility in education in rural China, and this effect was more pronounced for aforementioned socioeconomically disadvantaged groups. The findings suggest that increasing public investments in children's nutrition in less developed areas can be an effective way to reduce educational inequality and improve intergenerational mobility in education.

Keywords: School meal program; Educational attainment; Intergenerational mobility in education; Educational inequality

JEL Codes: H52; I24; I28

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1 Introduction

Government-sponsored school meal programs have been introduced in some developing countries to fight the pressing malnutrition issue for children. The short-run impacts of these programs, such as on nutritional status, school participation, test scores and cognitive abilities, have received much attention from researchers (e.g., Alderman and Bundy (2012), McEwan (2013), Chakraborty and Jayaraman (2019)). But their long-run impacts on human capital accumulation are still under-investigated. At the same time, another thread of literature shows that childhood nutrition and health status are important factors that determine the transmission of socioeconomic status between parents and offspring (Case et al., 2005; Currie, 2009; Carvalho, 2012). Nevertheless, we know little about how policy interventions on childhood nutrition could affect the intergenerational transmission. Understanding the role of school meal programs from these two perspectives is especially important for developing countries, because human capital accumulation in these countries is typically at a low level and highly persistent across generations (Torche, 2019).

To fill the gap in the literature and link the two threads of literature together, this paper provides the first empirical evidence on the long-run impacts of school meal programs on educational attainment and intergenerational persistence in education in the context of developing countries. Specifically, I ask: Can school meal programs affect educational attainments in the long run? If they are effective, can they help improve intergenerational mobility in education?

To answer these questions, this paper explores a free lunch program in rural China. As the largest developing country in the world, China launched its Nutrition Improvements Program (NIP) in the fall of 2011, which provided free nutritious lunch to rural students in compulsory education stage (grades 1 to 9). The program was initially introduced in poverty-stricken counties in contiguous poor areas, then expanded to other rural areas. I estimate the effects of the NIP using a sample of rural individuals aged 16–30 in 2018 from the China Family Panel Studies (CFPS). The staggered roll-out of the NIP at county level and the variation in the exposure years to the program of different birth cohorts allow me to use a cohort Difference-in-Differences (DiD) method as the identification strategy.

The analysis of this paper yields several findings. First, exposure to the NIP had an overall positive effect on educational attainment. One additional year of NIP exposure increased years of schooling by about 0.14 years (1.3 percent relative to the control mean). While no impacts are found on primary school and high school and above education, exposure to the program one more year increased middle school completion, which marks the completion of compulsory education, by 2.3 percentage points (9.3 percent relative to the control mean). In addition, one more year exposure to the program reduced illiteracy rate by 1 percentage point (37 percent relative to the control mean). The magnitude of these coefficients are comparable to another large-scale education subsidy program in rural China, the free compulsory education reform, where one additional semester of reform exposure was estimated to increase the probability of being enrolled in school by 8.5 percentage points and years of schooling at ages 17–22 by 0.17 years (Xiao et al., 2017).

Second, I also find heterogeneity in the above effects, where socioeconomically disadvantaged groups, such as girls and students from poor counties and low-income families benefited more from the program. In general, the NIP's effect on a summary index of educational attainment was 1.7 percent of a standard deviation higher for girls, and 7.8 percent of a standard deviation higher for students from poor counties. The NIP's effect decreased by 0.7 percent of a standard deviation as the net family income per capita increased by one quartile. Further explorations in the potential mechanism using data from 2010–2018 waves of the CFPS show that the NIP had short-run positive impacts on health status and academic performance measured by grade rankings. In addition, exposure to the NIP also reduced the number of days absent from school, and the likelihood of absence for illness.

Third, the NIP significantly promoted intergenerational mobility in education in rural China. I obtain this finding by first showing the general trends in intergenerational educational persistence for treated and control cohorts using a transition matrix, and then estimating the causal effects of the NIP on intergenerational educational mobility using a DiD design. The causal analysis shows that holding parents' years of schooling as constant, one additional year of NIP exposure decreased father-children educational persistence and mother-children educational persistence by 0.024 and 0.031, respectively. Again, these effects were more pronounced for girls and students from poor counties and low-income families.

This paper mainly speaks to three strands of literature. First, there is a growing literature evaluating the effects of school meal programs in developing countries. Many studies have discussed the short-run impacts of such programs on students' nutrient intakes, health outcomes and school participation (see Alderman and Bundy (2012) for a review). Some other literature also investigates the impacts of school meals on students' test scores in the short-run (McEwan, 2013; Chakraborty and Jayaraman, 2019), and cognitive abilities in the long-run (Fang and Zhu, 2022). Although existing literature has fruitful discussion on the effects of school meal programs, we know little about whether and how these programs affects educational attainment, which is an important measure of human capital. This paper fills the gap in the literature by providing the first empirical evidence on these questions. Specially, this paper analyzes the effects of a school meal program in China on years of schooling and the completion of different levels of education. Moreover, this paper also goes one step further by analyzing whether the program affects intergenerational mobility in education, which is an even more important long-term outcome but has not been covered by the literature.

Second, this paper contributes to the literature on the long-term effects of policy interventions for children with disadvantaged backgrounds. Existing literature mainly documents the effects of policies that aim to increase education accessibility or relieve household financial constraints, such as public preschool programs (Deming, 2009; Carneiro and Ginja, 2014; Bailey et al., 2021), subsidized housing vouchers (Chetty et al., 2016; Chyn, 2018; Pollakowski et al., 2022), and school subsidy programs (Xiao et al., 2017; Tang et al., 2020; Baez and Camacho, 2011; Behrman et al., 2011). But as another important form of childhood policy intervention, the effects of public investments in children's nutrition remain under-investigated. Moreover, the empirical evidence found by these researches has demonstrated that policy inventions on children, especially those at early ages, can have significant effects on their academic and labor market performance as adults (Barr and Gibbs, 2018). Despite this, there is little evidence about whether and the extent public interventions in children's nutrition can affect the intergenerational transmission of disadvantaged socioeconomic backgrounds. As a complement to this strand of literature, this paper studies a free lunch program in China, which not only targeted at rural students, but also covered all poverty-stricken counties. In addition to examining its long-run effects on educational attainment, this paper also shows that such nutrition supplement policy can effectively decrease the intergenerational persistence of education, which helps to break the transmission of disadvantaged backgrounds across generations.

Lastly, in a broader sense, this paper also adds to the understanding of intergenerational mobility in China. Existing literature has documented the patterns of the persistence of socioeconomic status across generations in China in different aspects, such as education (Golley and Kong, 2013; Chen et al., 2015), income (Gong et al., 2012; Fan et al., 2021), and occupation (Li and Goetz, 2019; Jia et al., 2021). Yet, little is known about how government policies, especially educational policies, affect intergenerational mobility. Some recent studies have tried to reveal this, but they mainly focus on the correlation between government educational expenditures and intergenerational mobility at aggregate level (Guo et al., 2019; Tang et al., 2020; Huang et al., 2021). Different from existing work, this paper analyzes a particular school meal policy, and relies on a quasi-experimental approach and nationally representative survey data to estimate its effects on intergenerational mobility in education. The finding of this paper provides new causal evidence at individual level.

The rest of the paper is organized as follows. Section 2 introduces the background of the NIP. Section 3 describes the empirical strategy and data used in this paper. Section 4 and Section 5 present the impacts of the NIP on educational attainment and intergenerational mobility in education, respectively. Section 6 concludes.

2 Background of the NIP

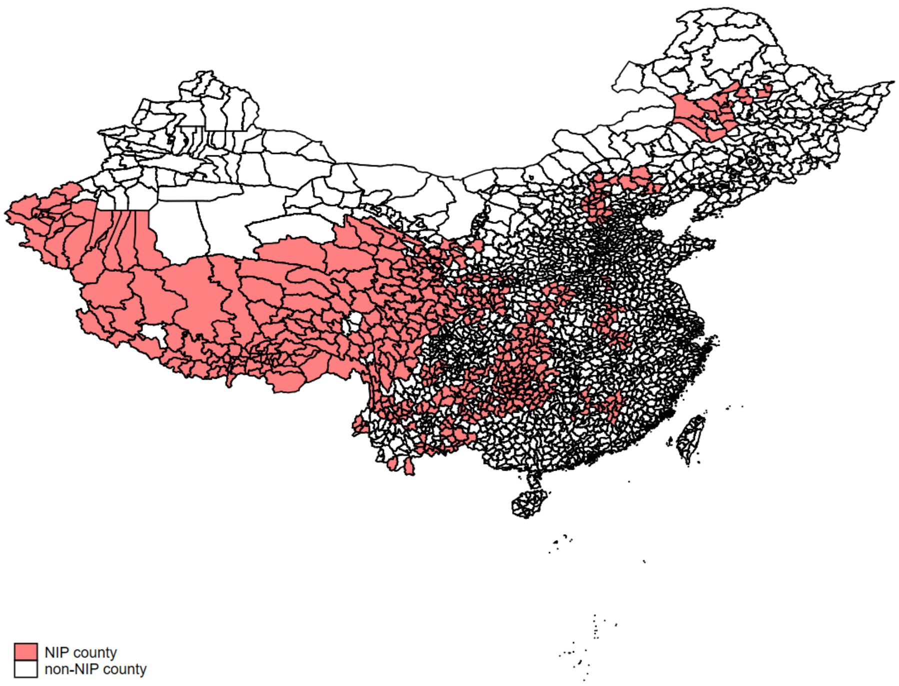
The predecessor of the NIP was a non-governmental charity project called the Free Lunch Program (FLP). The FLP was initiated by some teaching volunteers in 2011 when they noticed that many left-behind children, whose parents worked away from home, in rural areas of southwestern China could not have lunch at school, and therefore suffered from hunger and malnutrition. This phenomenon was not uncommon in rural China at that time. Due to the long-lasting unbalanced socioeconomic development between urban and rural areas, the malnutrition problem among rural students was still challenging, especially in poor areas. According to a survey conducted in western provinces of China, the stunting rate and underweight rate of rural students were 12% and 9%, respectively, and their height and weight were significantly lower than the normal values for children of the same ages (China Development Research Foundation, 2012). The malnutrition problem was more severe among left-behind children, who received less attention on diets due to the absence of parents. By the end of 2010, left-behind children were estimated to be 58 million. Among them, the stunting rate and underweight rate of children under five were 2.5 times and 2.2 times higher than those in urban areas, respectively, and these rates were even higher in poor rural areas (Feng, 2015).

Although the Chinese government had already offered living subsidies for boarding students from rural families with financial difficulties in mid-western regions, this program covered only a small proportion of rural students.[1] Motivated by the FLP, the central government took a quick and significant action. A nationwide policy supported by central government funding, namely, the Nutrition Improvements Program for Rural Compulsory Education Students (NIP) was implemented since the fall semester of 2011. It was initially

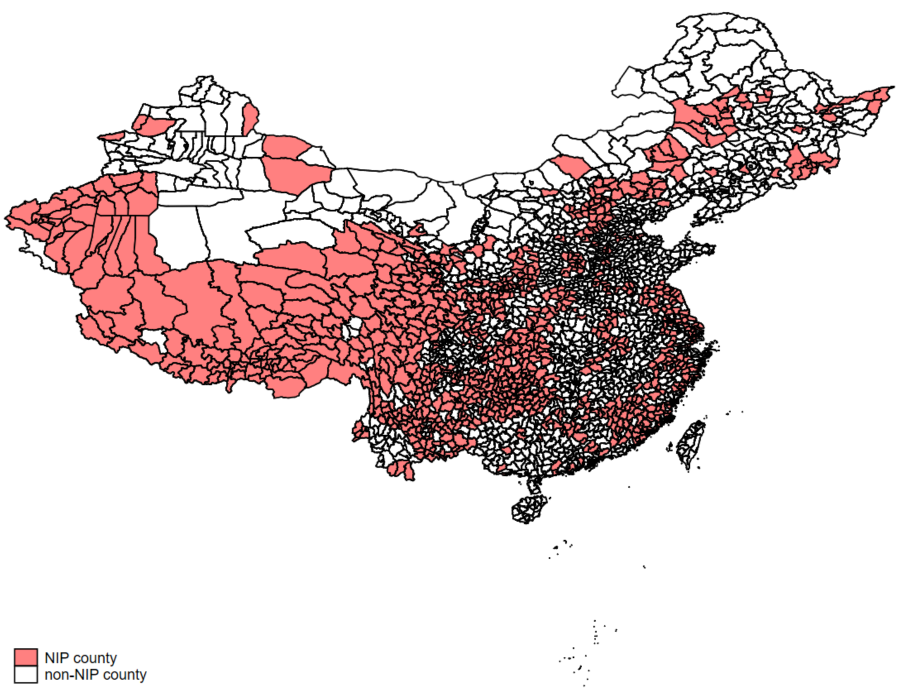
[1]Interpretation of the Nutrition Improvement Program for Rural Compulsory Education Students by the Finance Department of Ministry of Education. http://jyt.hunan.gov.cn/sjyt/xxgk/zcfg/zcjd/201701/t20170119_3906804.html

implemented in the 680 poverty-stricken counties *in contiguous poor areas*, which were the poorest counties, covering 26 million students, and then expanded to the remaining poverty-stricken counties and other non-poverty rural areas across the country. By the end of 2018, 1,642 counties including 726 national pilot counties and 916 local pilot counties have adopted the NIP. Figure 1 shows the spatial distribution of the NIP. The NIP aimed to provide subsidies for nutritious lunch to compulsory education students in rural areas. The daily subsidy was 3 CNY per student before 2012 and then increased to 4 CNY since October 2014, which could guarantee the basic nutritional needs of most students. The central government has allocated 159.1 billion CNY to the NIP since its launch, covering 134,000 schools and benefiting more than 36 million rural students (China Development Research Foundation, 2018). In addition, with the support of special funds from the central government, 98% of rural schools in poverty-stricken areas have completed the construction and renovation of school canteens and put them into use (China Development Research Foundation, 2017).

The effectiveness of the NIP in improving students' nutrition and health status has been proved by several evaluations. For example, the monitoring data of 1.92 million students aged 7–15 from 62 pilot counties shows that the malnutrition rate has decreased from 18.5% to 15.4% in 2016 (China Development Research Foundation, 2017). A periodic evaluation conducted by the Chinese CDC shows that in pilot counties, the average height of boys and girls in 2015 was 1.2–1.4 cm higher than that of 2012, and the average weight increased by 0.7–0.8 kg, which were higher than the average growth rates of rural students in China . In addition, the anemia rate among students in pilot counties has decreased from 17.0% in 2012 to 7.8% in 2015 (Chinese Center for Disease Control and Prevention, 2016).



(a) 2012 (initial pilot counties)



(b) 2018 (expanded pilot counties)

Figure 1: Spatial distribution of NIP counties.

3 Empirical strategy and data

3.1 Identification strategies

Since different counties adopted the NIP in different years and the length of policy exposure of different birth cohorts varies, I employ a cohort DiD method following Duflo (2001) to estimate the average effects of the policy. The empirical specification is as follows:

$$y_{itc} = \beta_1 exposure\_year_{itc} + X_{itc}\gamma + \alpha_c + \lambda_t + \epsilon_{itc} \tag{1}$$

where i, t and c denote individual, birth cohort and county of residence at age 12, respectively; y represents the outcomes of interest, i.e., educational attainment measured by a summary index, years of schooling and educational levels; *exposure_year* is the core explanatory variable, which measures years of NIP exposure; X is a vector of control variables, including gender (=1 if male), ethnicity (=1 if Han), years of exposure to the free compulsory education policy, net family income per capita and parents' educational levels; α_c is county fix effects and λ_t denotes birth cohort fixed effects. To address the potential serial correlation and heteroskadesticity, standard errors are clustered at county level.

For the heterogeneous analysis, I estimate the following specification:

$$y_{itc} = \beta_1 exposure\_year_{itc} + \beta_2 exposure\_year_{itc} * subgroup_{itc} + X_{itc}\gamma + \alpha_c + \lambda_t + \epsilon_{itc} \tag{2}$$

where *subgroup* refers to indicators for male, richer counties, and poverty-stricken counties, as well as a continuous variable for the quartiles of net family income per captia. Other variables are defined as in equation (1).

The impact of the NIP may vary with the length of exposure. In the context of this policy, longer exposure means being affected by the NIP at a younger age. For example, students who were exposed to the policy at the age 6 may benefit more from the NIP than those exposed at age 14. To clearly capture such different effects, I further conduct an event study analysis using the following specification:

$$y_{itc} = \sum_{k=9}^{23} \beta_k exposure\_year_k + X_{itc}\gamma + \alpha_c + \lambda_t + \epsilon_{itc} \tag{3}$$

where k is a dummy variable indicating whether individual i was k years old when the NIP first launched in county c. In the sample, when the NIP was launched in 2011 in the first batch of pilot counties, the youngest cohort was 9 years old and the oldest was 23 years old. So, k ranges from 9 to 23. The omitted baseline cohort is individuals who were 16 years old when the NIP was launched in the counties they lived, so the estimated coefficients capture the difference in outcomes of the group exposed to the policy at age k relative to the baseline cohort. Other variables are the same as in equation (1).

For the mechanism analysis, following Fang and Zhu (2022), I explore a series of short-run impacts of the NIP across several waves of the survey, namely, 2010, 2012, 2014, 2016 and 2018. The empirical specification is as follows:

$$y_{isc} = \beta_1 exposure\_year_{isc} + X_{isc}\gamma + \alpha_c + \lambda_s + \epsilon_{isc} \tag{4}$$

where i represents individual, s the survey year, and c the county of residence; y is the outcomes of interest, including health status, the number of days absent from school, if the absence was caused by illness, and the grade ranking in a most recent major exam; α_c is county fixed effects and λ_s is survey year fixed effects. Standard errors are clustered at county level. Other variables are defined in the same way as in equation (1).

Lastly, to identify the impact of the NIP on intergenerational mobility in education, following existing literature (Solon, 2002; Corak, 2013; Fan, 2020), the specification takes the following form:

$$education_{itc} = \beta_1 education^p_{itc} + \beta_2 education^p_{itc} * exposure\_year_{itc} + X_{itc}\gamma + \alpha_c + \lambda_t + \epsilon_{itc} \tag{5}$$

where *education* is educational attainment measured by years of schooling of a sample individual, and $education^p$ is father's or mother's years of schooling. Since we are concerned about if exposing to the NIP improved intergenerational educational mobility, β_2, the coefficient of the interaction term between parents' education and NIP exposure ($education^p * exposure\_year$), is the coefficient of interest. If the NIP decreased the intergenerational persistence in education (increased intergenerational mobility in education, in other words), β_2 would be negative. Other variables are the same as in equation (1), with X excluding parents' educational levels.

3.2 Data and variables

Data source. The individual-level data used in this paper are from the China Family Panel Studies (CFPS). The CFPS is a nationally representative, biennial longitudinal survey of Chinese communities, families, and individuals launched in 2010 by the Institute of Social Science Survey (ISSS) of Peking University, China. The survey covers a wide range of topics, including economic activities, educational outcomes, family dynamics and relationships, migration, health, etc. The CFPS sample covers 25 province-level regions (excluding Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia, and Hainan), representing 95% of the Chinese population. The baseline survey was conducted in 2010, covering 42,590 individuals from 14,960 households. These respondents were surveyed in the following waves, and the follow-up rate reached about 80%. County-level characteristics used in robustness checks are collected from county statistical yearbooks.

Sample selection. To analyze the long-run effects of the NIP on educational attainment, I use the 2018 wave of the survey, because it is the latest fully publicized data that includes information on parents and household. Based on the nature of the policy and the outcome variables of interest, I restrict the sample to individuals who were 16 to 30 years old in 2018 and lived in rural areas when they were 12 years old. The reason I set this age range is that individuals between this age range should at least have finished compulsory education in 2018. Since 16 is the minimum legal age for work in China, some of the sample individuals might have already left school. Their education levels therefore are final. Individuals aged above 30 could be too old to be comparable with the treated cohorts, so the upper bound of the age range is set at 30. In the mechanism analysis, I match the respondents in the baseline estimation who were in compulsory education stage between 2010 and 2018 with their data from all previous waves, namely, 2010, 2012, 2014, 2016 and 2018, to track the short-run impacts of the NIP on the outcomes of interest (see below).

NIP exposure. There are 78 out of 162 counties in the sample having adopted the NIP when the 2018 survey was conducted. Among these NIP counties, 57 of them are initial pilot counties, which implemented the NIP in late 2011 and 2012. The rest 21 of them participated in the NIP during its expansion period. The treatment variable, $exposure\_year$, measures the number of years that an individual was supposed to expose to the NIP during their compulsory education studies. Longer exposure to the policy could lead to more pronounced changes, so a continuous measure of the NIP exposure could give more information than a binary treatment variable.

Outcome variables for the main analysis. The CFPS collected information on several educational outcomes of a respondent, including years of schooling up to 2018 and educational level in 2018. To show general impact of the NIP on educational attainment, I first create a summary index aggregating information over multiple treatment effect estimates following Kling et al. (2007). This summary measure can improve statistical power to detect effects that go in the same direction within a domain (Kling et al., 2007). To be specific, the educational attainment index includes years of schooling and five dummy variables for given levels of education (i.e., illiterate, primary school graduation, middle school graduation, high school graduation, and college and above) as subcomponents. Each subcomponent is standardized by subtracting the control group mean and dividing by the control group standard deviation. The summary index is the equally weighted average of these standardized subcomponents, with negative value for being illiterate and positive values for the others. To gain a more detailed understanding of the policy effects, I also use each of the subcomponents as outcome variables and examine the treatment effects separately. The CFPS also provides the educational attainment of a respondent's parents, which allows to investigate the effect of the NIP on intergenerational mobility in education. I use both parents' years of schooling as the outcome variable to show how the NIP affects educational persistence across generations.

Outcome variables for the mechanism analysis. Motivated by the existing evidence (Hinrichs, 2010; Lundborg et al., 2022; Fang and Zhu, 2022), I investigate the potential mechanisms of the NIP's effects on educational attainment from three perspectives, namely, health status, absence from school, and academic performance. In the questionnaire of each wave of the survey, respondents were asked to self-rate their health status as "excellent, very good, good, fair, or poor". Based on this classification, I create a dummy variable which equals 1 if an individual has excellent, very good or good health status. In addition, respondents who were still at school were asked the days of absence in the past month. If they were absent, then they were further asked the reason for the absence. Based on this information, I construct a dummy variable to indicate if the absence was caused by illness. As for academic performance, I use a question from the survey that asked the grade rankings on a most recent major exam (midterm or final) for individuals still at school at the time of the survey. Respondents were asked to select from several options: first 10%, 11%–25%, 26%–50%, 51%–75%, or last 24%. I re-code these options so that one indicates the lowest ranking and five the highest.

Table 1 presents the summary statistics for the variables mentioned above. I divide the full sample into two categories base on the treatment status. On average, the treated cohorts exposed to the NIP for about 2.7 years. In terms of control variables, the average age and gender composition are very close for the treated and control groups. Individuals from the treated cohorts have slightly longer exposure to the free compulsory education reform, but their parents are less educated and earn less than the control cohorts. The difference in the control variables suggest the necessity to control for them to rule out the influence from other reform and family background on the outcome variables.

Table 1: Summary statistics.

Variable	Treated sample N	Treated sample Mean	Treated sample S.D.	Control sample N	Control sample Mean	Control sample S.D.
Treatment variable						
exposure_year	3,483	2.738	1.497	1,360	0.000	0.000
Control variables						
age	3,483	23.340	4.369	1,360	23.592	4.365
male	3,483	0.533	0.499	1,360	0.506	0.500
compulsory education	3,483	2.660	4.369	1,360	2.408	4.365
ethnicity	3,483	0.809	0.393	1,360	0.972	0.164
father's years of schooling	3,483	7.123	3.934	1,360	7.882	3.169
mother's years of schooling	3,483	4.824	4.379	1,360	6.425	3.746
net family income per capita	3,483	24643.260	38132.610	1,360	31729.520	65701.950
Outcome variables for the main analysis						
educational attainment index	3,483	0.089	0.303	1,360	0.070	0.390
years of schooling	3,483	11.872	3.622	1,360	11.182	3.191
illterarcy	3,483	0.027	0.162	1,360	0.007	0.084
primary school graduation	3,483	0.055	0.229	1,360	0.036	0.185
middle school graduation	3,483	0.269	0.444	1,360	0.246	0.431
high school graduation	3,483	0.273	0.445	1,360	0.284	0.451
college and above	3,483	0.376	0.484	1,360	0.427	0.495
Outcome variables for the mechanism analysis						
absence days	6,841	0.206	0.928	3,102	0.240	0.774
absence for illness	6,841	0.093	0.306	3,102	0.105	0.290
healthy	14,690	0.455	0.498	6,510	0.437	0.496
ranking	8,715	2.572	1.178	3,756	2.644	1.197

4 The effects of the NIP on educational attainment

4.1 Baseline results

Table 2 shows the effects of the NIP on educational attainment. As presented in column 1, in general, the NIP had a positive effect on the treated cohorts and this effect is statistically significant at 5 percent level. The coefficient means that one additional year exposure to the NIP increased the summary index of educational attainment by 1 percent of a standard deviation. Columns 2 to 7 then show the results of each subcomponent of the summary index, respectively.

Column 2 focuses on the effect on years of schooling. The estimated coefficient suggests that one additional year of NIP exposure increased years of schooling by about 0.14 years, which is significant at 10 percent level. This coefficient can be translated into a 1.3 percent increase relative to the control mean (11.182).

Regarding the effects on different levels of education, the result shown in column 3 suggests that receiving one additional year of NIP could decrease the probability of illiteracy by 1 percentage point, corresponding to a 37 percent decrease relative to the control mean (2.7 percent). This result implies that the NIP contributed to eliminating absolute poverty in rural China caused by illiteracy. Columns 4 and 5 show the estimated effects on primary school graduation and middle school graduation. The effect on primary school graduation is positive but not significant. This result is consistent with the fact that finishing primary school is common in China in recent years, even in rural areas, so introducing the NIP may not directly affect primary school graduation. In contrast, the estimated effect on middle school graduation is significantly positive at 5 percent level, and is greater in magnitude than that of primary school graduation. It means that one additional year of exposure to the NIP significantly increased the probability of completing middle school education by 2.3 percentage points, or 9.3 percent relative to the control mean (0.246). This result is more meaningful than the effect on primary school graduation. In China, the compulsory education stage covers primary education and middle school education, namely, grades 1 to 9. Therefore, the result on middle school graduation suggests that the NIP contributed to the completion of compulsory education, which marks the formation of basic human capital. The effects of the NIP on high school and higher education, as shown in columns 6 and 7, respectively, are positive but not statistically significant. The results for separate educational outcomes indicate that the positive effect of the NIP on educational attainment is mainly driven by the increased likelihood of completing compulsory education.

Although there is no comparable findings from previous research about the NIP, a comparison with another large-scale education subsidy program in rural China, which is known as the free compulsory education reform, can provide a better sense of the results. The free compulsory education reform was launched in rural areas of China in 2006. Under the reform, all rural students enrolled in primary and middle schools are exempted from paying tuition and miscellaneous fees. One additional semester exposure to the reform was estimated to increase the probability of being enrolled in school by 8.5 percentage points and years of schooling at ages 17–22 by 0.17 years (Xiao et al., 2017). It is reasonable that the free compulsory education policy had larger effects than the NIP, because it directly reduced private costs of education. But the comparability of the estimated effects of these two policies in magnitude verifies the effectiveness of the NIP.

In addition, comparing the coefficients with those found for school meal programs in developed countries can also help understand the significance of the effects to some extent. A recent study about the school lunch program in Sweden shows that one additional year of school lunches increases years of schooling by 0.03 (Lundborg et al., 2022). We can find that the return to public investments in children's nutrition in developing countries is higher than that in developed countries. This may because educational attainment in developed countries is already at high level. Overall, the comparisons with other school subsidy program in rural China and similar programs in developed countries further supports the socioeconomic significance of the effects of the NIP.

4.2 Event study estimates

The validity of the DiD estimation relies on the parallel trends assumption that in the absence of the policy, the outcomes of interest would follow similar trends for different cohorts. In addition, the estimates in the previous subsection measure the average treatment effects of the NIP on educational attainment. However, different initial age receiving the NIP means different length of policy exposure. Therefore, we may see the policy effect varies across different initial exposure ages. To test the parallel trends assumption and estimate the dynamic effects of the policy, I conduct an event study analysis for the educational

Table 2: Effects of the NIP on educational attainment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	educational attainment index	years of schooling	illiteracy	primary school graduation	middle school graduation	high school graduation	college and above
exposure_year	0.010**	0.137*	-0.010*	0.006	0.023**	0.011	0.007
	(0.004)	(0.069)	(0.006)	(0.004)	(0.011)	(0.009)	(0.008)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.061	0.043	0.026	0.013	0.045	0.150	0.142
Observations	4,843	4,843	4,843	4,843	4,843	4,843	4,843

Notes: This table shows DiD estimates of the effects of the NIP on educational attainment measured by educational attainment index, years of schooling and different levels of education. The educational attainment index is the equally weighted average of standardized subcomponets including years of schooling and different levels of education. All regressions controls for gender (=1 if male), ethnicity (=1 if Han), years of exposure to the free compulsory education policy, net family income per capita and parents' educational levels. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

attainment index and three subcomponents that are significantly affected by the policy according to the baseline estimates, namely, years of schooling, illiteracy and middle school graduation.

Figure 2 plots the estimated coefficients of β_k from equation (3) and the 95% confidence interval for the four outcomes. The horizontal axis of the four graphs displays the starting age receiving the NIP. Respondents who were 16 years old at the implementation of the NIP are set as the baseline group. As showed in the graphs, most of the estimated coefficients for pre-treatment cohorts (i.e., aged above 16 when the policy was implemented) are not statistically different from zero, implying that there is no significant difference in terms of the outcome variables for older cohorts who did not benefit from the policy. Note that for years of schooling, the coefficients for ages 18 and 19 are still significantly positive. A plausible explanation is that in rural areas, delayed school starting age and grade retention are not uncommon (Fang and Zhu, 2022). Therefore, individuals who were 18 or 19 years old when the NIP was implemented could still be affected. Despite this, the four graphs still support the validity of the parallel trends assumption.

The post-treatment cohort estimates in Figure 2 show the effects for different length of exposure. As initial exposure age increases, there exists downward trends of the estimated policy effect for the educational attainment index as well as years of schooling, and an upward trend for illiteracy, for the post-treatment cohorts. There is no noticeable trend for middle school graduation, but the coefficients are quite significant for younger cohorts. Overall, the graphs suggest that the estimated policy effects are more pronounced if an individual exposed to the NIP at a younger age.

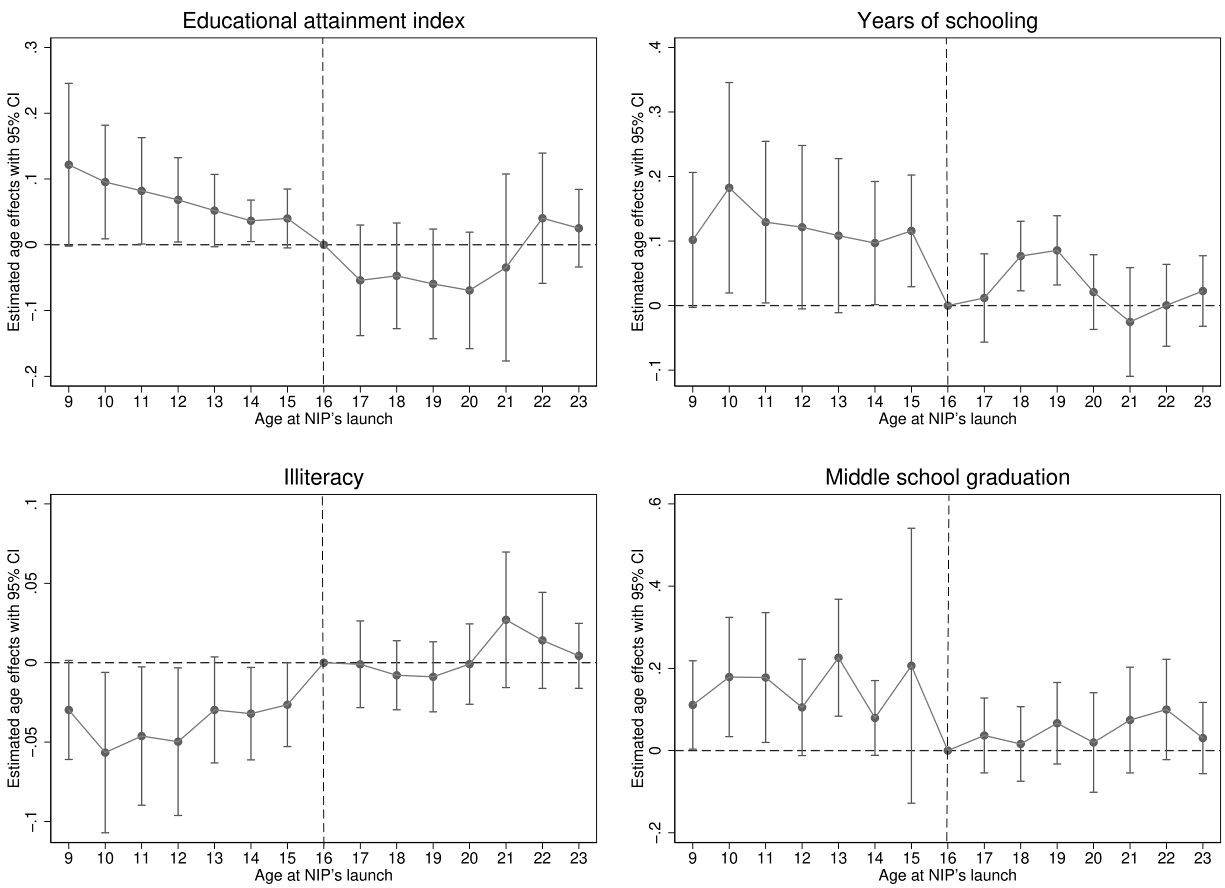


Figure 2: Event study estimates.

Notes: The figures plot the event-study estimates of β_k and their 95% confidence intervals for different outcomes using the specification in equation (3). The horizontal axis denotes the age when an individual started to be enrolled in the NIP, where age 16 is set as the benchmark.

4.3 Additional tests on the identifying assumption

I further assess the plausibility of the identifying assumption in several ways. To begin with, I examine observed trends in the number of students in primary school and the number of students in middle and high schools for NIP and non-NIP counties. The selection of these two variables is primarily limited by the availability of county-level education data in public data sources. Due to this data limitation, one cannot directly examine the trends in the outcome variables in the baseline estimation at county-level. As an alternative, these two indicators measure the number of students at county level and can be viewed as proxy for the systematic difference in county-level education to some extent. Therefore, the changes in these two variables can partially reflect the general trends in the outcome variables analyzed above at county level.

The left panel of Figure 3 plots the average number of students in primary school for NIP and non-NIP counties in the period before the NIP was introduced, while the right panel plots the average number of students in middle and high schools for these two types of counties over the same period. These two figures illustrate that although NIP counties on average had more students relative to non-NIP counties prior to policy implementation, the trends followed a parallel pattern over time. This pattern provides support for the common trends for the pre-policy period between NIP and non-NIP counties.

In addition, to check if parallel trends were likely to continue in the absence of the NIP, I generate predicted outcomes based on individuals' characteristics during the pre-policy period and then estimate the impacts of the NIP on these predicted educational outcomes. Table 3 reports the results. The estimated coefficients of $exposure\_year$ are very small in magnitude and all of them are statistically insignificant. These results suggest that conditional on birth year and county fixed effects, there is little association between the NIP and these predicted educational outcomes. Therefore, the parallel pre-trends assumption can be further supported.

Lastly, to further verify the assumption that the NIP are not correlated with a unobserved

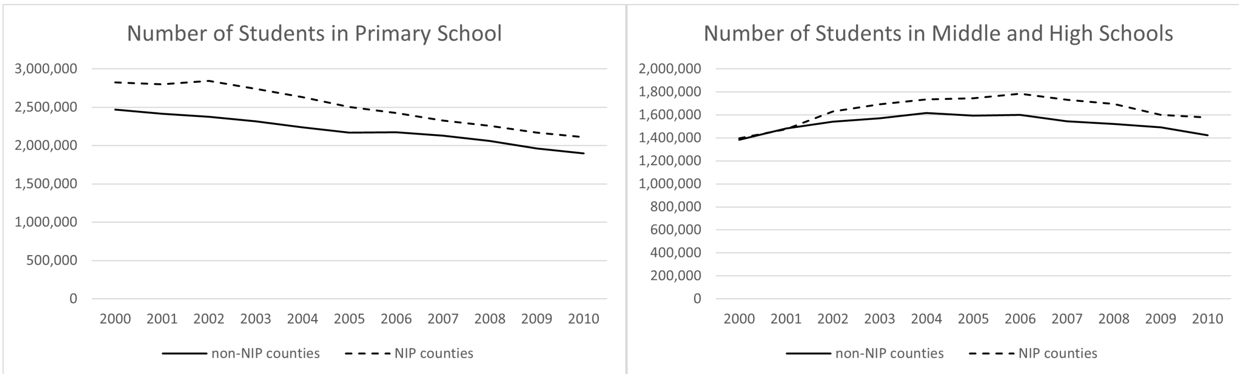


Figure 3: Number of students in different counties.

Notes: The figures show the average number of students at county level from 2000 to 2010, before the NIP was implemented. The left figure plots the average number of students enrolled in primary schools, while the right figure plots the average number of students enrolled in middle and high schools. The solid lines represent non-NIP counties, and the dashed lines represent NIP counties. The data is collected from county-level statistical yearbooks.

Table 3: The impact of the NIP on counterfactual educational attainment.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Predicted outcomes	educational attainment index	years of schooling	illiteracy	primary school graduation	middle school graduation	high school graduation	college and above
exposure_year	-0.001	-0.002	0.001	-0.002	-0.002	0.001	0.000
	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.852	0.718	0.796	0.943	0.989	0.939	0.563
Observations	4,843	4,843	4,843	4,843	4,843	4,843	4,843

Notes: This table shows DiD estimates of the impact of the NIP on counterfactual educational attainment using the specification in equation (1). The predicted outcomes are obtained based on individuals' characteristics during the pre-policy period. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients.

trend in education outcome, I test whether the NIP was preceded by a systematic change in any of a number of county-level socioeconomic characteristics. To detect the preexisting trend, following Kose et al. (2021), I estimate a modified event-study specification where a liner trend is used to replace the pre-NIP indicators:

$$y_{ct} = \alpha_0 + \alpha_1 year\_from\_nip + \sum_{k=1}^{10} \beta_k (year\_from\_nip = k) + Z_{ct}\gamma + \delta_c + \theta_t + \epsilon_{ct} \tag{6}$$

where c and t denotes county and year, respectively; y is county-level characteristics and $year\_from\_nip$ is a linear trend in years since the county c implemented the NIP. Because $\sum_{k=1}^{10} \beta_k (year\_from\_nip = k)$ are indicators for post-NIP periods, α_1 captures the slope of the outcome variables over time before the NIP and it is the coefficient of interest. County-level per capita GDP, fiscal expenditure and revenue, and the number of students enrolled in primary, middle and high schools are controlled. δ_c and θ_t are county fixed effects and year fixed effects, respectively.

Table 4 shows the estimates of α_1. Among fourteen county-level characteristics, only two are significant, namely, the added value of the secondary industry ($\alpha_1 = 0.030$) and the number of industrial enterprises ($\alpha_1 = 0.048$). The direction of bias from these trends is not obvious. To be specific, the effect of an increase in the added value of secondary industry or number of industrial enterprises on education might be negative due to higher demand for workers, but the effect might also be positive through improvements in family income.

4.4 Placebo test and robustness checks

In this subsection, I test the validity of the baseline results. To check if the baseline results are driven by unobserved factors that affected both urban and rural residents in the same county, I conduct a placebo test with a sample of urban residents aged 16 to 30. The treatment status are assigned using the policy roll-out in rural areas in the same county. Because urban residents are not eligible for the NIP, the policy should have no effect on

Table 4: Trend in county characteristics before the NIP.

	Trend coefficient	Standard error	p-value	Observations
Variable (in logarithm)	(1)	(2)	(3)	(4)
Total power of agricultural machinery	-0.041	0.032	0.205	786
Number of fixed phone users	-0.002	0.023	0.942	912
Number of employees in the secondary industry	-0.026	0.017	0.119	913
The added value of the primary industry	-0.014	0.02	0.482	967
The added value of the secondary industry	0.030	0.011	0.005	967
Total food production	0.017	0.032	0.603	709
Total cotton production	0.113	0.109	0.307	355
Total oil production	0.054	0.041	0.199	960
Total meat production	0.061	0.052	0.245	711
Number of industrial enterprises	0.048	0.018	0.009	902
Gross industrial output value	0.032	0.027	0.237	707
Fixed asset investment	-0.022	0.027	0.419	451
Number of beds in medical and health institutions	-0.012	0.011	0.250	967
Number of social welfare and adoption institutions	0.048	0.222	0.831	900

Notes: This table summarizes the estimates of α_1 from equation (6) for 14 regressions. Column 1 shows the trend coefficient, which is α_1, and the p-value in column 3 tests whether there is a significant pre-trend for each outcome variable. The data of these outcome variables are collected from county statistical yearbooks. For each regression, standard error are clustered at the county level.

them. As shown in Panel A of Table 5, the coefficients of $exposure\_year$ are insignificant for all outcome variables, suggesting that the baseline results are not driven by county-level shocks that affected both urban and rural residents.

For robustness checks, I first add the interactions between province dummies and a second-order polynomial function of birth year trend to the baseline specification to control for province-specific linear and nonlinear cohort trends in the outcomes. The reason for controlling province-specific cohort trends is that in China, provincial governments are responsible for the allocation of educational funds and making educational policies for the jurisdiction. Due to the difference in the level of economic and social development among provinces, the cohort trends in the outcome variables for different provinces may vary. The results are presented in Panel B of Table 5. The estimated policy effects on the educational attainment index, years of schooling, the probability of illiteracy and middle school graduation are still significant and close to the baseline estimates in magnitude.

Then, to address the concern that cohort trends in outcome variables may vary with predetermined county characteristics, I interact county characteristics in 2010 with birth year dummies, and add the interaction terms to the baseline specification following Xiao et al. (2017). These predetermined county characteristics include GDP per capita, government revenue and expenditure per capita, and the number of students enrolled in primary, middle and high schools. The estimates shown in Panel C of Table 5 change little compared with the baseline results.

Moreover, I consider the potential sample selection issue caused by migration. Specifically, the availability of free lunch in some particular counties might affect the school choice of some parents for their children. Therefore, such self-selection into the treatment group may bias the estimates. An ideal way to deal with this issue is to exclude these migrate individuals from the sample. In the CFPS, respondents were asked if they changed *hukou* address or residential address in order to attend school. The answers to these questions can be a good indicator to identify the migrate sample. Based on their answers, 27 respondents are dropped from the full sample. As we can see from Panel D of Table 5, this slight adjustment to the sample does not affect the baseline results.

Lastly, recent literature on econometric theory has shown that DiD estimates could be biased in staggered treatment settings if already treated units are used as controls for newly treated units, or there is heterogeneity in the treatment effects over time (de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway et al., 2021; Borusyak et al., 2021). To test if the baseline results are robust to these concerns, I adopt the approach developed by de Chaisemartin and D'Haultfoeuille (2020) to re-estimate the specifications in Table 2.[2] They show that linear regressions with two-way fixed effects estimate weighted sums of the treatment effect in each group and period, and the weights may be negative. The DiD_M estimator they proposed estimates the treatment effect in the groups that switch treatment at the time when they switch, and it does not rely on any treatment effect homo-

[2]Borusyak et al. (2021) also propose an imputation approach to address these concerns. But the treatment variable in their setting is binary. Since the treatment variable of this paper is continuous, I use the method proposed by de Chaisemartin and D'Haultfoeuille (2020) which allows continuous treatment.

geneity condition (de Chaisemartin and D'Haultfoeuille, 2020). Panel E of Table 5 shows the results. The estimates are close to those in Table 2, suggesting that there is little bias induced by using already-treated units as controls and the heterogeneity in the treatment effects.

4.5 Heterogeneity analysis

The previous analysis has shown the positive effects of the NIP on educational attainment. In this subsection, I further investigate the heterogeneity of policy effects across different groups.

In rural China, girls are educationally disadvantaged group because they usually receive fewer household educational investments compared with boys (Song and Zhou, 2019). To see if public investments in education can complement the deficiency in household investments for rural girls, I check the gender difference in the effects of the NIP on educational attainment in Panel A of Table 6. The variable of interest is the interaction term between the length of NIP exposure (*exposure_year*) and the indicator for male (*male*). The estimated coefficients of the interaction term for the educational attainment index, years of schooling, middle school graduation and high school graduation are significantly negative, while that for illiteracy is significantly positive. These results indicate that compared with boys, girls enjoyed more improvements in human capital with the help of the NIP (1.7 percent of a standard deviation higher in educational attainment index), as reflected in relatively longer education (0.021 years longer), higher probability of middle school and high school graduation (1.4 percentage points and 2.2 percentage points higher, respectively), as well as a lower probability of being illiterate (1.2 percentage points lower). Therefore, this policy helps to combat educational inequality across genders in rural China.

The effect of the NIP may also vary across regions with different levels of development. For relatively more developed counties, providing students with free lunch may not have much impact on their educational outcomes, since they already have adequate nutrient intakes.

Table 5: Placebo test and robustness checks.

	(1) educational attainment index	(2) years of schooling	(3) illiteracy	(4) primary school graduation	(5) middle school graduation	(6) high school graduation	(7) college and above
Panel A: placebo test (urban sample)							
exposure_year	-0.004	-0.041	-0.002	-0.008	0.020	-0.013	-0.016
	(0.037)	(0.100)	(0.002)	(0.005)	(0.022)	(0.026)	(0.017)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,098	1,098	1,098	1,098	1,098	1,098	1,098
R-squared	0.032	0.266	0.023	0.035	0.097	0.229	0.266
Panel B: controlling for provincial trends							
exposure_year	0.010**	0.131*	-0.008*	0.006	0.021**	0.011	0.002
	(0.004)	(0.083)	(0.005)	(0.005)	(0.010)	(0.011)	(0.008)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,817	4,817	4,817	4,817	4,817	4,817	4,817
R-squared	0.064	0.085	0.027	0.014	0.045	0.152	0.144
Panel C: controlling for predetermined county characteristics							
exposure_year	0.005**	0.120*	-0.006*	0.003	0.021*	0.015	0.010
	(0.002)	(0.070)	(0.004)	(0.013)	(0.011)	(0.013)	(0.018)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,288	4,288	4,288	4,288	4,288	4,288	4,288
R-squared	0.070	0.061	0.039	0.019	0.047	0.147	0.143
Panel D: excluding migration sample							
exposure_year	0.009**	0.130*	-0.010*	0.006	0.023**	0.010	0.008
	(0.004)	(0.064)	(0.005)	(0.004)	(0.011)	(0.009)	(0.008)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,816	4,816	4,816	4,816	4,816	4,816	4,816
R-squared	0.061	0.084	0.026	0.013	0.044	0.148	0.140
Panel E: DiD_M estimator proposed by de Chaisemartin and D'Haultfoeuille (2020)							
exposure_year	0.009**	0.122*	-0.016*	0.018	0.073*	0.014	0.003
	(0.004)	(0.070)	(0.010)	(0.023)	(0.039)	(0.014)	(0.069)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,843	4,843	4,843	4,843	4,843	4,843	4,843

Notes: This table summarizes the results of a placebo test (Panel A) and several robustness checks (Panels B to E). Panel A shows the DiD estimates of the NIP's effects on educational attainment using the specification in equation (1) and urban sample aged 16 to 30. Panel B adds the interactions between province dummies and a second-order polynomial function of birth year trend to the specification in equation (1) to control for provincial trends. Panel C adds the interactions between county characteristics in 2010 with birth year dummies to the specification in equation (1) to control for predetermined county characteristics. Panel D excludes migration samples. Panel E uses the approach developed by de Chaisemartin and D'Haultfoeuille (2020). The DiD_M estimator they proposed estimates the treatment effect in the groups that switch treatment, at the time when they switch, so it is robust to treatment heterogeneity across groups and time. All regressions controls for gender (=1 if male), ethnicity (=1 if Han), years of exposure to the free compulsory education policy, net family income per capita and parents' educational levels. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

However, for less developed counties, students are more likely to suffer from malnutrition and food insecurity, so such supplementary nutritious lunch could make a big difference. To test this hypothesis, I first investigate the effects of the NIP for richer counties. I divide counties in the sample into five groups based on the quintiles of their GDP per capita in 2010, the year before the NIP started. Then, I create an indicator for richer counties ($richer$) if a county's GDP per capita is at the highest two quintiles. The interaction term between the indicator and the treatment variable captures the heterogeneous impact for richer counties. As shown in Panel B of Table 6, all the estimated coefficients of the interaction term are insignificant, which indicates no significant effects of the NIP on students from richer counties. To check the heterogeneity for poor counties, I create another dummy variable PSC, which equals one if a county is a national poverty-stricken county in 2010, and interact it with $exposure\_year$. The estimated coefficients of the interaction term are presented in Panel C of Table 6. The standardized index is 7.8 percent of a standard deviation higher for students from poor counties (column 1). These students have a 0.022-year longer education (column 2), and they are 2.7 percentage points more likely to complete middle school (column 5) and 3.8 percentage points less likely to be illiterate (column 3). These results suggest that the policy had a more pronounced positive effect on the human capital accumulation for students from poor counties. Taken together, the heterogeneous policy effects across development levels suggest that public investments in nutritious in less developed areas are more productive.

In addition to county-level development disparity, the difference in household-level income maybe another important source for the heterogeneity of the policy effect. Students from low-income families typically face binding financial constraints and suffer from nutritional deficiencies. Moreover, since many parents from low-income families have to spend more time on making a living, their children may receive less attention on diet. This is especially true for left-behind children, who have to live on their own or with their elderly grandparents because their parents have to leave hometown for work. To see if students from low-income families benefit more from the NIP, I interact the quartile of a respondent's net family income per capita in 2010 provided by the CFPS with $exposure\_year$ and add the interaction term to the baseline estimation. The results are shown in Panel D of Table 6. In general, as net family income per capita decreases by one quartile, the summary index is 0.7 percent of a standard deviation higher. In addition, the estimated coefficients for years of schooling, middle school graduation and college degree and above are significantly negative, and that for being illiterate is significantly positive. Since larger quartile represents higher income, these results indicate that the effects of the NIP on educational attainments were more positive for students from low-income families. Also note that the estimated effect of the NIP on obtaining college degree and above is statistically significant here when considering the heterogeneity in family's financial situation. This finding supports the long-run positive effect of the NIP, especially on economically disadvantaged students.

4.6 Potential mechanisms

In this subsection, I analyze how this policy contributes to the improvements in educational attainment from three aspects, namely, health status, school attendance, and academic performance. The way each outcome variable is constructed has been detailed in Section 3.2. The results for the full sample are presented in Panel A of Table 7.

I begin by analyzing the short-run impact of the NIP on students' health status. Existing literature has shown that the NIP improved students' nutrient intakes, such as protein, zinc, calcium, and other micronutrients (Fang and Zhu, 2022). Therefore, better health status may be one possible channel to explain the improvements in educational attainment. Column 1 of Panel A shows the result using an indicator for being healthy as the outcome variable. As we can see, one additional year of NIP exposure increased the probability of being healthy by 2.5 percentage points, suggesting that the NIP significantly improved students' health status, which laid a physical foundation for receiving education.

The improvements in educational attainment maybe also caused by higher attendance rate, because if students can closely follow the curriculum, they may have better academic

Table 6: Heterogeneous effects of the NIP.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	educational attainment index	years of schooling	illiteracy	primary school graduation	middle school graduation	high school graduation	college and above
Panel A: gender							
exposure_year	0.026*	0.030	0.004	0.010	0.015	0.001	-0.013
	(0.015)	(0.026)	(0.003)	(0.006)	(0.012)	(0.011)	(0.009)
exposure_year×male	-0.017**	-0.021***	0.012*	-0.007	-0.014***	-0.022*	0.011
	(0.008)	(0.006)	(0.007)	(0.007)	(0.003)	(0.011)	(0.007)
R-squared	0.060	0.043	0.027	0.013	0.046	0.151	0.142
Panel B: richer counties							
exposure_year	0.020	0.024	-0.011*	0.006	0.022*	-0.009	-0.008
	(0.014)	(0.017)	(0.006)	(0.005)	(0.012)	(0.010)	(0.008)
exposure_year×richer	-0.026	-0.024	0.009	-0.003	0.002	-0.014	0.006
	(0.017)	(0.020)	(0.007)	(0.005)	(0.011)	(0.018)	(0.016)
R-squared	0.060	0.043	0.027	0.013	0.045	0.150	0.142
Panel C: poverty-stricken counties (PSC)							
exposure_year	0.009	0.009	-0.006	0.005	0.019	-0.006	-0.012
	(0.020)	(0.015)	(0.004)	(0.005)	(0.015)	(0.012)	(0.011)
exposure_year×PSC	0.078**	0.022**	-0.038***	0.001	0.027***	-0.009	0.008
	(0.031)	(0.008)	(0.011)	(0.006)	(0.007)	(0.015)	(0.012)
R-squared	0.062	0.044	0.028	0.013	0.045	0.150	0.142
Panel D: family income							
exposure_year	0.031*	0.047**	-0.016**	0.004	0.015	-0.018	0.015
	(0.018)	(0.020)	(0.008)	(0.006)	(0.015)	(0.012)	(0.010)
exposure_year×family_income	-0.007**	-0.013***	0.003**	0.001	-0.014**	0.004	-0.011***
	(0.003)	(0.004)	(0.001)	(0.002)	(0.005)	(0.004)	(0.003)
R-squared	0.060	0.044	0.028	0.013	0.045	0.150	0.143
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,843	4,843	4,843	4,843	4,843	4,843	4,843

Notes: This table summarizes the heterogeneous effects of the NIP for different groups. In Panel A, $male$ is an indicator for males. In Panel B, $richer$ equals 1 if a county's GDP per capita in 2010 is at the highest two quintiles. In Panel C, PSC equals 1 if a county is a national poverty-stricken county in 2010. In Panel D, $family\_income$ measures the quartile of an individual's net family income per capita in 2010. All regressions controls for gender (=1 if male), ethnicity (=1 if Han), years of exposure to the free compulsory education policy, net family income per capita and parents' educational levels. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

performance. In column 2 of Panel A, the outcome variable is the number of absence days. The significantly negative coefficient suggests that exposing to the NIP reduced the incidence of absence from school. Column 3 further investigates the reason for absence. The outcome variable is a dummy variable which equals 1 if the absence was caused by illness, and 0 otherwise. The result indicates that the NIP significantly reduced the probability of absence due to illness. This result echoes the positive effect of the NIP on health status.

Lastly, more nutrient intakes contributes to better cognitive abilities (Fang and Zhu, 2022). Therefore, we may expect to see improvements in students' academic performance after the implementation of the NIP. Such progress is another possible channel that enables students to achieve higher level of education. The result on the effect of the NIP on rankings is presented in column 4 of Panel A. The estimated coefficient suggests that exposure to the NIP promoted ranking significantly.

In additional to the full sample analysis, I also investigate the heterogeneity of the mechanisms across different subgroups. As shown in Panel B of Table 7, for rural girls, they had better educational outcomes relative to boys because of the relatively better self-reported health status (see column 1) and relatively better academic performance (see column 4). As for students from poverty-stricken counties (see Panel C of Table 7), the effects of the NIP were realized through more improvements in health status (see column 1) and fewer absence days from school (see column 2). The result on absence is interesting, because it implies that for poor areas, free nutritious lunch can serve as an effective incentive to keep students stick to school. Panel D of Table 7 shows the heterogeneous mechanisms for students with different family incomes. The coefficient of the interaction term in columns 1 and 4 are significantly negative, indicating that students from low-income families improved more in health status and exam rankings relative to students from high-income families. In sum, these results suggest that the NIP played a significant role in improving health status, school participation and academic performance for socioeconomically disadvantaged groups. However, the insignificant improvement in exam rankings for students from poor counties implies that other investments in education besides the NIP, such as improving the quality of rural teachers, are still needed to narrow the gap in education between developed and less developed areas.

5 The effect of the NIP on intergenerational mobility in education

Education policy is an important mediating factor in the process of mobility across generations (Torche, 2019). As we have seen in previous sections, public investments in children's nutrition can improve educational attainment. Then, a further question is how this policy affects intergenerational mobility in education in the long-run? To answer this question, I provide statistical evidence followed by causal analysis.

5.1 General impact on intergenerational mobility in education

In this subsection, I show the general trends in relative intergenerational mobility in education before and after the implementation of the NIP with a transition matrix. This matrix displays the percentage of children in each level of education, conditional on their parents' educational level. I focus on individuals who have already finished school, so the comparison between their educational level and that of their parents can depict the intergenerational mobility in education. Parents' educational level is measured by the highest degree of parents. In Table 8, Panels A and B show the distribution of children's and their parents' educational levels for the control cohorts (born in 1988-1995, aged 16 and above at the implementation of the NIP in 2011) and the treated cohorts (born in 1996-2002, 9-15 years old in 2011), respectively. To make the values in Panels A and B more comparable, I row-normalize each value and report it in the parentheses below each cell. Panel C shows the difference between Panel A and Panel B using row-normalized values.

We can get the following information from this transition matrix. First, the educational

Table 7: Mechanism analysis.

	(1)	(2)	(3)	(4)
	healthy	absence day	absence for illness	ranking
Panel A: full sample				
exposure_year	0.025***	-0.076***	-0.034***	0.399***
	(0.004)	(0.008)	(0.003)	(0.055)
R-squared	0.091	0.056	0.086	0.484
Observations	21,200	9,943	9,943	12,471
Panel B: gender				
exposure_year	0.024***	-0.024***	-0.010***	0.400***
	(0.004)	(0.007)	(0.003)	(0.056)
expousureyear×male	-0.004**	-0.001	0.003	-0.007**
	(0.002)	(0.007)	(0.003)	(0.003)
R-squared	0.091	0.167	0.281	0.484
Observations	21,200	9,943	9,943	12,471
Panel C: poverty-stricken counties (PSC)				
exposure_year	0.025***	-0.027***	-0.009***	0.393***
	(0.005)	(0.008)	(0.003)	(0.060)
expousureyear×PSC	0.008***	-0.004***	0.001	0.011
	(0.002)	(0.001)	(0.006)	(0.069)
R-squared	0.091	0.167	0.281	0.484
Observations	21,200	9,943	9,943	12,471
Panel D: family income				
exposure_year	0.010**	-0.067***	-0.028***	0.699***
	(0.004)	(0.018)	(0.007)	(0.069)
expousureyear×family income	-0.002*	0.011	0.002	-0.173***
	(0.001)	(0.007)	(0.002)	(0.026)
R-squared	0.331	0.197	0.379	0.605
Observations	21,200	9,943	9,943	12,471
County FE	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes

Notes: This table summarizes the results of mechanism analysis for the full sample (Panel A) and other subgroups (Panels B to D). In Panel B, *male* is an indicator for males. In Panel C, *PSC* equals 1 if a county is a national poverty-stricken county in 2010. In Panel D, *family_income* measures an individual's net family income per capita in 2010. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

level witnesses an improvement for the treated cohorts. To be specific, comparing the second last rows in Panels A and B, which show the distribution of children's educational level in the sample, we can find that the percentage of illiteracy drops from 1.8 percent for the control cohorts to 0.6 percent for the treated cohorts. The treated cohorts also have higher percentage of receiving high school education and above (about 70.7 percent), compared with that of the control cohorts (about 65.5 percent).

Second, relative to their parents' educational level, the treated cohorts experience upward mobility when their parents are not well educated. Consider the children of illiterate parents. The proportion of children who are still illiterate drops from 1.4 percent (7 percent after row-normalized) for the control cohorts to 0.3 percent (1.7 percent after row-normalized) for the treated cohorts. On the other hand, the proportion of children who grew up with illiterate parents but end up with high school and above education increases from 9.3 percent (46.7 percent after row-normalized) for the control cohorts to 10.3 percent (51.6 percent after row-normalized) for the treated cohorts. For children whose parents' educational level is primary school, the proportion of receiving middle school and above education is 28.3 percent (93.5 percent after row-normalized) for the control cohorts and 28.5 percent (93.9 percent after row-normalized) for the treated cohort. When parents' educational level is middle school, the proportion of children receiving high school and above education is 24.5 percent (70.7 percent after row-normalized) for the control cohorts, while this proportion increases to 28.2 percent (77 percent after row-normalized) for the treated cohorts.

Third, the NIP contributes to the upward intergenerational mobility in education in rural China. The upper-right triangle of each transition matrix represents higher educational level than parents, while the lower-left one represents the opposite. As we can see from Panel C of Table 8, many of the cells in the upper-right triangle are negative, implying that individuals from the treated cohorts are more likely than the control cohorts to have higher educational level than their parents. In addition, all the cells in the third-to-last column are negative, suggesting that no matter what their parents' educational level is, children from the treated cohorts are more likely to receive high school education.

Table 8: Transition matrix of intergenerational education persistence.

parents' education level	children's education level					
	illeteracy	primary school	middle school	high school	college and above	Total
Panel A: 1988-1995 cohorts (control group)						
illiteracy	0.014	0.023	0.070	0.036	0.058	0.200
	(0.070)	(0.113)	(0.351)	(0.178)	(0.289)	(1.000)
primary school	0.002	0.018	0.092	0.068	0.123	0.303
	(0.007)	(0.058)	(0.305)	(0.225)	(0.405)	(1.000)
middle school	0.002	0.010	0.090	0.074	0.171	0.346
	(0.004)	(0.030)	(0.259)	(0.213)	(0.494)	(1.000)
high school	0.000	0.002	0.022	0.025	0.076	0.124
	(0.000)	(0.017)	(0.174)	(0.199)	(0.610)	(1.000)
college and above	0.000	0.000	0.002	0.002	0.024	0.027
	(0.000)	(0.000)	(0.058)	(0.058)	(0.885)	(1.000)
Total	0.018	0.053	0.275	0.204	0.451	1.000
	(0.081)	(0.218)	(1.146)	(0.872)	(2.683)	(5.000)
Panel B: 1996-2002 cohorts (treated group)						
illiteracy	0.003	0.019	0.074	0.079	0.023	0.199
	(0.017)	(0.096)	(0.370)	(0.399)	(0.117)	(1.000)
primary school	0.001	0.017	0.078	0.121	0.086	0.304
	(0.004)	(0.057)	(0.258)	(0.397)	(0.284)	(1.000)
middle school	0.001	0.007	0.076	0.155	0.127	0.366
	(0.003)	(0.019)	(0.208)	(0.423)	(0.347)	(1.000)
high school	0.000	0.001	0.012	0.044	0.033	0.090
	(0.000)	(0.013)	(0.129)	(0.490)	(0.368)	(1.000)
college and above	0.000	0.000	0.003	0.018	0.020	0.042
	(0.000)	(0.000)	(0.083)	(0.431)	(0.486)	(1.000)
Total	0.006	0.045	0.243	0.417	0.290	1.000
	(0.024)	(0.185)	(1.048)	(2.140)	(1.602)	(5.000)
Panel C: Difference between control and treated cohorts (using row-normalized values)						
illiteracy	0.052	0.017	-0.020	-0.222	0.172	0.000
primary school	0.003	0.001	0.047	-0.172	0.121	0.000
middle school	0.001	0.011	0.051	-0.210	0.147	0.000
high school	0.000	0.004	0.045	-0.291	0.242	0.000
college and above	0.000	0.000	-0.026	-0.373	0.399	0.000
Total	0.056	0.032	0.098	-1.268	1.081	0.000

Notes: Each cell in Panels A and B reports the percentage of children in each educational level (as given by the column) conditional on parents' educational level (as given by the row). Row-normalized values are reported in the parentheses below each cell in Panels A and B. Panel C displays the difference between Panel A and Panel B using row-normalized values.

5.2 Causal analysis on intergenerational mobility in education

To provide causal evidence on the impact of the NIP on intergenerational mobility in education, I further conduct DiD estimation in this subsection. Table 9 presents the results. Columns 1 to 3 use father's years of schooling as independent variable, while columns 4 to

6 use mother's years of schooling as independent variable. All the specifications also control for the interactions between birth cohort dummies and county characteristics in 2010. In columns 1 and 4, I first regress children's years of schooling on that of their parents, respectively. Then, I include the interaction terms between parent's education and NIP exposure in columns 2 and 5. As we can see, holding parents' years of schooling as constant, one additional year of NIP exposure decreased father-children educational persistence and mother-children educational persistence by 0.024 and 0.031, respectively. The results suggest that the NIP significantly contributed to the improvements in intergenerational mobility in education. As a robustness check, I use a binary measure for NIP exposure ($exposure\_dummy$, which equals 1 if exposure year is greater than 0, and 0 otherwise) to replace the original continuous variable $exposure\_year$ based on the specifications in columns 2 and 5. The estimated coefficients of the interaction terms are still significantly negative as shown in columns 3 and 6.

5.3 Heterogeneous impacts on intergenerational mobility in education

Next, to further investigate the effects of the NIP on intergenerational mobility in education, I analyze the heterogeneous patterns across genders, regions and family income in this subsection. Again, like the analysis in the previous subsection, I look into the heterogeneity in father-children educational persistence (in column 1 of Table 10) and mother-children educational persistence (in column 2 of Table 10) separately.

Panel A of Table 10 exhibits the difference in the NIP effects for different genders. The estimated coefficients for the triple interaction between parent's education, policy exposure and the indicator for male are positive in both columns, but the coefficient is only significant in mother-children educational persistence. The coefficients mean that for girls, everything being equal, father-children educational persistence and mother-children educational persistence are 0.010 and 0.012 lower than boys, given one additional year of NIP exposure. The

Table 9: The effects of the NIP on intergenerational educational mobility.

	children's years of schooling					
	(1)	(2)	(3)	(4)	(5)	(6)
father's years of schooling	0.181***	0.198***	0.200***			
	(0.017)	(0.019)	(0.019)			
father's years of schooling×exposure_year		-0.024***				
		(0.006)				
father's years of schooling×exposure_dummy			-0.088***			
			(0.026)			
mother's years of schooling				0.183***	0.203***	0.204***
				(0.017)	(0.019)	(0.020)
mother's years of schooling×exposure_year					-0.031***	
					(0.007)	
mother's years of schooling×exposure_dummy						-0.111***
						(0.026)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
County characteristics×Birth year dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.171	0.174	0.174	0.175	0.179	0.179
Observations	3,005	3,005	3,005	3,169	3,169	3,169

Notes: This table summarizes the effects of the NIP on intergenerational mobility in education. Columns 1 and 4 show the educational persistence between parents and children without considering the effects of the NIP. Columns 2 and 5 show the effects of the NIP on intergenerational educational persistence using a continuous measure of policy exposure, while columns 3 and 6 use a dummy variable for NIP exposure as a robustness check. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

results suggest that the NIP reduced the intergenerational persistence of education more for girls, which is especially true for the association between mothers and daughters.

As for different regions, I examine the heterogeneous effects for poverty-stricken counties (PSC) and non-PSCs in Panel B of Table 10. The estimated coefficients of the triple interaction between parent's education, policy exposure and the indicator for PSC are all significantly negative for both father-children and mother-children educational persistence (-0.038 and -0.026, respectively). These results indicate that students from poor counties witnessed more improvements in intergenerational mobility in education after the NIP was implemented.

Lastly, Panel C of Table 10 displays the heterogeneity pattern across family income, which is measured by the quartile of net family income per capita. Since larger quartile corresponds to higher income, the significantly positive sign of the estimated coefficients of the triple interaction between parent's education, policy exposure and family income indicates lower intergenerational persistence of education for individuals from low-income families. Everything being equal, father-children educational persistence and mother-children educational persistence are 0.016 and 0.010 lower, respectively, if net family income per capita decreases by one quartile.

Table 10: Heterogeneous impacts on intergenerational educational mobility.

	father-children	mother-children
	(1)	(2)
Panel A: gender		
parent's years of schooling	0.243***	0.252***
	(0.020)	(0.021)
parent's years of schooling×exposure_year	-0.027***	-0.032***
	(0.006)	(0.008)
exposure_year×male	0.204**	0.093

Table 10 continued from previous page

	father-children	mother-children
	(0.085)	(0.084)
parent's years of schooling×male	-0.069***	-0.084***
	(0.014)	(0.016)
parent's years of schooling×exposure_year×male	0.010	0.012**
	(0.010)	(0.005)
R-squared	0.175	0.181
Panel B: poverty-stricken counties		
parent's years of schooling	0.187***	0.179***
	(0.024)	(0.022)
parent's years of schooling×exposure_year	-0.015*	-0.025***
	(0.008)	(0.007)
exposure_year×PSC	0.319**	0.159
	(0.148)	(0.141)
parent's years of schooling×PSC	0.053	0.092**
	(0.042)	(0.044)
parent's years of schooling×exposure_year×PSC	-0.038**	-0.026**
	(0.015)	(0.010)
R-squared	0.170	0.176
Panel C: family income		
parent's years of schooling	0.183***	0.198***
	(0.047)	(0.040)
parent's years of schooling×exposure_year	0.017	0.002

Table 10 continued from previous page

	father-children	mother-children
	(0.012)	(0.014)
exposure_year×income	-0.022	-0.055
	(0.041)	(0.040)
parent's years of schooling×income	0.005	0.001
	(0.016)	(0.013)
parent's years of schooling×exposure_year×income	0.016***	0.010*
	(0.005)	(0.005)
R-squared	0.172	0.176
Birth year FE	Yes	Yes
County FE	Yes	Yes
County characteristics×Birth year dummies	Yes	Yes
Observations	3,005	3,169

Notes: This table summarizes the heterogeneous impacts of the NIP on intergenerational mobility in education for different groups. In Panel A, *male* is an indicator for males. In Panel B, PSC equals 1 if a county is a national poverty-stricken county in 2010. In Panel C, $family\_income$ measures an individual's net family income per capita in 2010. All regressions control for gender (=1 if male), ethnicity (=1 if Han), years of exposure to the free compulsory education policy, and net family income per capita. Interactions between county characteristics in 2010 and birth year dummies are also controlled. The predetermined county characteristics include GDP per capita, government revenue and expenditure per capita, and the number of students enrolled in primary, middle and high schools. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

This is paper provides the first empirical evidence on the effects of a free lunch program in rural China on individuals' educational attainment and intergenerational mobility in education. Based on the gradual roll-out of the program, I employ a cohort DiD method and find that exposure to the NIP improved educational attainment for rural students. Specifically, the program increased years of schooling as well as compulsory education completion, and decreased the probability of illiteracy, which contributed to the formation of basic human capital. The NIP helped to reduce educational inequality since socioeconomically disadvantaged groups, such as rural girls and students from poverty-stricken counties and low-income families, benefited more from the policy. The improvements in educational attainment were mainly driven by better health status, fewer absence days, a lower likelihood of absence for illness, and better academic performance. Further analysis shows that the NIP also promoted intergenerational mobility in education in rural China. This effect was more pronounced for rural girls and people from poor counties and low-income families. Overall, the findings of this paper suggest that increasing public investments in children's nutrition in less developed areas can be an effective way to reduce educational inequality and improve intergenerational mobility in education.

Admittedly, due to the short history of the NIP and the availability of micro-survey data, this paper only discusses NIP's effects on educational outcomes. Other socioeconomic outcomes, such as life-time income and occupational status, as well as their intergenerational transmissions, are also very important for understanding the long-run effects of school meal programs. Based on the results of this paper, it is reasonable to believe that the improvements in education resulted from better nutrition may be translated into labor market success. Formal analysis about this is guaranteed in future work.

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