Weathering the Storm: How do Free Compulsory Education and Early-life Shocks Shape Long-Term Education Attainment?

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Abstract

We explore how early-life shocks interact with subsequent human capital investments to impact individuals' long-term education attainment. Leveraging the rollout of free compulsory education reform in rural China and early-life rainfall shocks as two sources of exogenous variation, we employ cohort difference-in-differences and triple-differences methods to identify the causal effects on educational outcomes. We show that exposure to the reform significantly improves grade completion during compulsory schooling. Conversely, early-life adverse rainfall substantially decreases educational attainment, leading to an average decrease in schooling of 0.2-0.4 years. However, longer exposure to the reform mitigates these adverse effects, with approximately three additional semesters of enrolment compensating for a one-standard-deviation increase in early-life extreme rainfall exposure. Our findings suggest a dynamic substitutability rather than complementarity between family and government human capital investments.

Keywords: Human capital investment, Education attainment, Dynamic substitutability, Free compulsory education reform, Early-life shocks

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1 Introduction

The influence of early-life events on adult outcomes has been extensively documented (Almond et al., 2018; Currie, 2009). Substantial evidence indicates that early-life experiences of poverty, malnutrition, and poor health can result in enduring deficits in cognitive development and skills (Case & Paxson, 2008; Duque & Schmitz, 2023; Spears & Lamba, 2016; Wu et al., 2023), with far-reaching implications for lifelong health, human capital, and labor market outcomes (Gertler et al., 2014; Shah & Steinberg, 2017). Concurrently, the enactment of various public policies and social protection programs aims to serve as safety nets to support the development of children's human capital (Aizer et al., 2016; Bitler et al., 2014; Chetty et al., 2016). A critical challenge for both researchers and policymakers is to determine the role public policy can play in alleviating the long-term and persistent effects of adverse early-life conditions on children.

This paper focuses on exploring how early-life shocks interact with subsequent human capital investments to impact individuals' long-term education attainment. This is of particular interest given that dynamic complementarities may characterize these interactions. Specifically, Cunha and Heckman (2007) suggest that human capital investments may be more productive when an individual's baseline stock of skills is higher. However, empirically answering this question poses a considerable challenge, akin to hoping for "lightning to strike" twice -- two identification strategies influencing the same cohort but at different developmental stages, as highlighted by Almond & Mazumder (2013) in their literature review. Firstly, a causal estimate of the effect of an early-life shock on later-life outcomes is necessary. This requires isolating variation in exposure to early-life disadvantage that is independent of other determinants of long-term outcomes. Secondly, to investigate the effectiveness of a policy in mitigating or exacerbating the effects of early-life disadvantages, we must isolate exogenous variation in this policy. For example, exposure to public programs like conditional cash transfers (CCTs), a prevalent anti-poverty policy implemented worldwide, primarily aimed at low-income populations, could be influenced by household income, parental preferences, and local resource accessibility. These factors may also impact long-term outcomes, complicating comparisons between individuals who experienced the same shock but were differentially exposed to public policies, potentially leading to biased estimates of the remedial efficacy of these programs.

We attempt to overcome this challenge by leveraging the combination of a natural experiment inducing variation in early disadvantage alongside a large-scale free compulsory education (FCE) reform in rural China, covering all rural children and documented to increase educational attainment. We primarily utilize three datasets: (i) individual data primarily sourced from the 2010 Population Census and the 2015 1% National Population Sample Survey, conducted by the National Bureau of Statistics (NBS) of China; (ii) rainfall data derived from the Global Surface Summary of the Day (GSOD); and (iii) regional covariates sourced from various yearbooks. Subsequently, we merge individual-level microdata with regional rainfall data and covariates by matching individuals' birth dates and residence locations. The sample is restricted to rural residents born between September 1984 and August 1994, aged 16-25 in 2010 and 21-30 in 2015. Our outcomes of interest include both years of schooling and dummy variables denoting enrollment or completion of specific education stages.

Our empirical analysis consists of three steps, and a concise research framework is illustrated in Figure 1. We initially examine the effects of the FCE reform on individuals' educational outcomes. The reform was initially introduced in rural areas across 16 provinces in the spring semester of 2006, gradually expanding to achieve full coverage in rural areas by September 2007, to alleviate the financial burden on rural households and improve compulsory education completion rates. Leveraging the cross-province variation in the phased implementation of the reform, we employ a difference-in-differences (DID) framework where the treatment varies by province of residence and birth date. Following Xiao et al. (2017) and Tang et al. (2020), we define our treatment variable for the FCE reform exposure as the number of semesters during which an individual is supposed to be eligible for free compulsory education. We find that the FCE reform significantly improves rural children's enrollment and completion of junior high school, as well as years of schooling, with a slightly positive effect on high school completion. These effects of the FCE remain robust even after considering various confounding factors and conducting robustness checks.

Subsequently, we investigate the impact of weather shocks during early life. In our study's agrarian setting and sample period, agriculture played a vital role in household income. From 1985 to 2000, agricultural income constituted over 60% of total household income in rural China (NBS, 2001), highlighting the significant dependence on agriculture. This reliance underscores the pivotal role of weather conditions in determining household income and, consequently, the resources available for children. We show that the exposure to extreme rainfall (defined as the number of months that rainfall deviates by one standard deviation from the 10-year historical county-specific mean) during their early years leads to worse educational attainment than those born in relatively normal rainfall periods. An increase equivalent to the sample mean of early-life extreme rainfall exposure leads to a notable decline in years of schooling, ranging from 0.2 to 0.4. These results do not appear to be driven by specific variable definitions or potential sources of selection bias, such as migration patterns.

Finally, we investigate the interaction effects of the FCE and the weather shock. To achieve this, we combine both sources of variation into a cohort triple differences design. Our findings reveal that the interaction coefficients are significantly positive for years of schooling and high school completion. The point estimates suggest that the FCE reform had a more pronounced impact on disadvantaged individuals who experienced extreme rainfall shocks during early life. Specifically, nearly two additional exposures to the FCE could counteract the negative effects of one standard deviation of extreme rainfall, while nearly three additional exposures to the FCE could further narrow the gap between the weather-affected group and the unaffected group, enabling disadvantaged children affected by rainfall to catch up with their unaffected peers. Our main results are robust to a wide range of alternative specifications and robustness checks.

Our study is mainly related to three strands of literature. The first literature estimates the interaction effects of human capital investments. Some early studies have examined whether the effects of human capital intervention policies vary depending on the pre-intervention level of skills (Aizer & Cunha, 2012; Garcıa & Gallegos, 2017). However, as mentioned previously, empirically testing these interactions is challenging because it requires exogenous variation in multiple investments over time. Since these studies do not utilize exogenous variation in prior skills, they do not directly address whether early and late human capital policies exhibit dynamic complementarity. Recently, an emerging literature attempts to address this issue. Goff et al. (2023), for example, utilize the repeal of Romania’s decades-long ban on abortions as a positive shock to family environments and early investments, and the cutoff transition score as a positive shock to school quality, and find that access to abortion and access to better schools each have positive impacts, but find no consistent evidence of interactions between these impacts; if anything, there is an indication of a negative interaction between the impact of better family and school environments, suggesting the substitutability rather than complementarity. A similar pattern is found by Adhvaryu et al. (2024), who examine the interaction between parental resources and later educational investments using variation in local rainfall shocks and the PROGRESA program, and find that educational investments mitigate the negative effects of adverse rainfall shocks. However, there is also literature supporting dynamic complementarity. For instance, Duque et al. (2023) use exogenous shocks from early extreme weather exposure and a CCT program (Familias en Acción) in Colombia, finding that the CCT program had a smaller effect on children who experienced extreme weather events early in life. Johnson & Jackson (2019) find that the benefits of Head Start were larger when followed by access to better-funded schools, and increases in K–12 spending were more efficacious when preceded by Head Start exposure, suggesting dynamic complementarities.

Our study contributes to this literature by leveraging a universal and unconditional public education policy to reduce potential estimation biases in causal identification and enhance external validity. Previous research has primarily explored dynamic complementarity through targeted programs such as CCTs, preschool programs, and infant care programs, which often focus solely on low-income families and have income thresholds, posing challenges for identification. For example, income thresholds of CCTs may introduce endogeneity in the interaction term between early-life household income shocks and subsequent human capital investments. In other words, parents may respond to these early-life income shocks in ways that could, in turn, influence the effectiveness of the CCT program (Adhvaryu et al., 2024). Furthermore, additional requirements of CCTs, such as PROGRESA's mandate for students to attend at least 85% of regular school days, may bias estimates of long-term effects. Moreover, government subsidies provided through CCT programs may be subject to endogenous determinants such as specific uses and timing by each family. In contrast, our study of the 2006 rural free compulsory education policy better meets these criteria because it covers all rural children without additional restrictions and directly reduces education expenses for families rather than providing subsidies.

The second strand of literature investigates the heterogeneous effects of large-scale public education subsidy policies. Existing literature has primarily focused on programs in developed countries, such as the "Head Start" preschool education program in the United States (Bailey et al., 2021; Barr & Gibbs, 2022; Bitler et al., 2014; Kose, 2021) and the infant health intervention program in Denmark (Rossin-Slater & Wüst, 2020). We contribute to this literature by providing evidence for developing countries, where insufficient early human capital investment is more prevalent. Therefore, the importance of using public policies to compensate for missed opportunities in early human capital accumulation is heightened.

The third strand of literature investigates the impact of climate change on human capital. Existing empirical research has demonstrated that early-life exposure to extreme environmental conditions, such as extreme temperatures, rainfall, or climate disasters, can have detrimental effects on health, education, and labor market outcomes (Dinkelman, 2017; Shah and Steinberg, 2017; Duque, Rosales-Rueda, and Sanchez, 2023). Substantial evidence also indicates that, in an agrarian setting, adverse rainfall is often associated with a reduction in agricultural wages

(Dell et al., 2014; Miguel et al., 2004; Munshi, 2003). We contribute to this line of research by showing that subsequent policy-induced human capital investments have the potential to help mitigate the effects of these shocks.

The remainder of this paper is organized as follows. Section 2 reviews the relevant reform background and describes our data. Section 3 discusses our empirical strategy. Section 4 presents the main empirical results as well as the validity of identification. We discuss our results in section 5 and section 6 concludes.

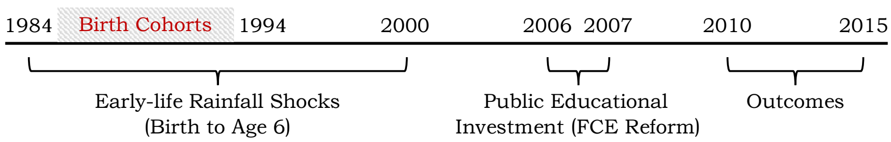

Figure 1. Research Design

2 Background and Data

2.1 The Free Compulsory Education Reform in rural China

The implementation of the Compulsory Education Law of the People's Republic of China on July 1, 1986, granted school-aged children the entitlement to a minimum of nine years of compulsory schooling, comprising six years of primary education and three years of junior high school. However, despite authorizing tuition-free education for the nine-year compulsory period,[1] the law was inadequately enforced (Xiao et al., 2017). Compulsory education funding heavily relied on local government financing and lacked national public finance protection. This reliance was exacerbated following the 1985 basic education financial reform, which delegated full responsibility for basic education provision and financing to local governments.[2]

[1] Article 10 of the Compulsory Education Law of the People's Republic of China stipulates that the state shall not charge tuition for students receiving compulsory education.
[2] Please refer to the document titled "Resolution on Educational System Reform," which was issued by the Central Committee of the Chinese Communist Party in May 1985.

Consequently, local governments and schools were incentivized to shift the financial burden of education from public entities to private households through fees, imposing a heavy financial burden[3]. This burden placed on households potentially rendered basic education financially unattainable for children, thus serving as a major deterrent to enrollment and completion of compulsory education, particularly for children from low-income rural households (Connelly & Zheng, 2003; Yi et al., 2012). Moreover, in the central and western regions of China, counties with limited financial capacities struggled to sustain the funding required for rural compulsory education, thereby widening the regional education gap and exacerbating inequality.[4]

Before the nationwide introduction of the Free Compulsory Education (FCE) reform in 2006, the Chinese government implemented two educational initiatives aimed at alleviating the financial burdens of rural education. The first, known as the "Tuition Control" reform in 2001, stipulated that tuition fees for rural public primary schools should not exceed 160 yuan per student annually, and for junior high schools, the limit was set at 260 yuan (Chyi & Zhou, 2014). This reform was extended nationwide by 2004. The second, the "Two Exemption and One Subsidy" (TEOS) reform in 2003, provided tuition fee exemptions, free textbooks, and living subsidies for extremely poor households. By the spring of 2005, the TEOS had been expanded to cover all primary and junior high school students from nationally designated poor counties.[5]

To further alleviate the financial burden on rural households and improve primary and junior high school completion rates in rural China, the Chinese government initiated the

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[3] Guo et al. (2021) use data from the National Fixed Point Survey (NFPS) in 2004 and observe that primary and junior high school expenditures per child in rural China constituted 8% and 20% of total household income, respectively.

[4] Please refer to the Ministry of Education's report for more details at http://wap.moe.gov.cn/jyb_xwfb/s5147/201909/t20190926_401046.html.

[5] In 2005, there were a total of 592 nationally designated poor counties, constituting approximately 30% of all counties in China.

nationwide Free Compulsory Education reform in 2006.[6] This reform exempted all rural primary and junior high school students from tuition and miscellaneous fees, while also providing free textbooks and living subsidies to students from poor households. Additionally, this reform transitioned from the sole responsibility of local governments to a joint responsibility shared between central and local governments in financing compulsory education. Under this new system, the central government contributes a larger share of funding in less developed regions, covering eighty percent of the costs in the western provinces and sixty percent in the central provinces. In municipalities and the eastern provinces, the proportion of compulsory education funding covered by the central government is determined based on the fiscal capacity of the local government.

Following the State Council's rollout plan, all western provinces were required to implement the reform no later than the spring of 2006, with the remaining provinces to follow no later than the spring of 2007. The implementation of the FCE reform commenced gradually from the spring semester of 2006 and achieved full coverage in rural areas by September 2007. See online Appendix Table A1 for the timeline of FCE reform implementation across provinces.

The FCE reform holds significant importance. According to statistics from the Ministry of Finance, approximately 130 million rural students were exempted from paying tuition and miscellaneous fees between 2006 and 2010, resulting in an average reduction of 250 yuan per primary school student household and 390 yuan per junior high school student household annually. While the total cost of these policies is not explicitly provided, it's noteworthy that

[6] Please refer to the State Council document [2005] No.43, titled "Notice of the State Council on Deepening the Reform of the Rural Educational Expenditure Assurance Mechanism" at https://www.gov.cn/zhengce/zhengceku/2008-03/28/content_5545.htm.

the national government allocated a substantial amount, totaling 458.8 billion yuan (approximately US$69.28 billion based on the exchange rate in 2010), for the FCE reform.[7]

2.2 Data

We rely on three types of data: (i) census data, (ii) county-level monthly rainfall data, (iii) regional statistical yearbooks, and manually collected data.[8]

First, our data on individual characteristics and outcomes mainly come from the 2010 Population Census and the 2015 1% National Population Sample Survey (also known as the mini-census), conducted by the NBS of China.[9] The census data have larger sample sizes and better representativeness of the national population, enabling more accurate estimations of policy and weather effects. These data encompass individuals' basic demographic information as well as details on education, employment, and fertility.

Second, the rainfall data is derived from the Global Surface Summary of the Day (GSOD), available through the NOAA of the United States. This dataset includes meteorological observations from over 9,000 stations worldwide since 1929, covering rainfall, temperature, wind speed, other weather parameters. Recent studies have used this data to investigate the impact of climate on economic growth (Felbermayr & Gröschl, 2014), mental health (Mullins & White, 2019), and mortality rates (Barreca, 2012; Fan et al., 2020). Following the procedures in existing literature, we initially interpolated the station-level data into 0.1-degree latitude and

[7] Please see the Ministry of Finance's report on the FCE reform in 2010 for more details at http://www.mof.gov.cn/zhengwuxinxi/caijingshidian/zgcjb/201012/t20101223_384395.htm.
[8] The detailed data matching process is discussed in online Appendix B.
[9] In accordance with the Regulations on National Population Census promulgated by the State Council in 2010, China conducts decennial population census in years ending with 0, and the 1% population sample survey during the inter-censual years ending with 5. To date, seven censuses have been conducted, in 1953, 1964, 1982, 1990, 2000, 2010, and 2020. The most recent inter-census survey was conducted in 2015.

longitude grid nodes by using observations from the five nearest stations, employing inverse-distance weights. Subsequently, this grid-day data was aggregated to the county level through GIS software, matching rainfall observations per node to the corresponding county and averaging across nodes to derive a monthly rainfall estimate per county. Our analysis focuses on the period after 1973, given the limited number of stations available before this year, which compromised the accuracy of aggregated data (Barreca, 2012; Egan & Mullin, 2016). Finally, we constructed the monthly rainfall data for each county between 1973 and 2001.

Third, regional pre-determined covariates are sourced from the *China Statistics Yearbooks*, *China City Statistical Yearbooks*, and *China Statistical Yearbooks for Regional Economy* in 2005. Additionally, the number of nationally designated poor counties in each province in 2005 is obtained from the Summary of Poverty Alleviation and Development in Rural China (The Central People's Government of China, 2006).[10] The effective date of the FCE reform in each province is manually collected from a variety of central and local government documents.

2.3 Variable Construction

Education Attainment. We initially code years of education based on the highest level of education attained by individuals and whether they completed each educational tier (Chen et al., 2020). For example, individuals who completed primary school are assigned 6 years of education, while those who dropped out of primary school are coded with 3 years. This coding pattern extends to higher-level education: 3 years for junior high school, 3 years for high school, 3 (or 4) years for college (or undergraduate), and 3 years for postgraduate education. However,

[10] Please refer to the full context of the Summary of Poverty Alleviation and Development in Rural China for more details at https://www.gov.cn/zwhd/ft2/20061117/content_447141.htm?eqid=851269d80003386400000002647e82aa.

potential confounding factors might be introduced since years of schooling mechanically incorporate potential factors from all educational stages. To mitigate this concern, we include dummy variables indicating enrollment or completion of specific education stages alongside years of schooling as measures of educational outcomes.

Reform exposure. Following Xiao et al. (2017) and Tang et al. (2020), we measure the duration of reform exposure by the number of semesters that an individual is supposed to be exposed to free compulsory education. In China, the academic year typically begins on September 1st and ends in July of the next year, comprising a fall semester from September to January and a spring semester from March to July. According to the school enrollment policy, children are required to enroll in primary school upon celebrating their 6th birthday, with the expectation of completing the nine-year compulsory education by the age of 15. Consequently, the number of semesters exposed to free compulsory education is jointly determined by the reform's effective date in the province of residence and their date of birth. For example, a child born between September 1990 and August 1991 would enroll in primary school in September 1997 and complete junior high school in July 2006. If the FCE reform took effect in the province of residence in March 2006, the individual's FCE exposure would be one semester.

Rainfall shocks. Following Shah & Steinberg (2017), Duque et al. (2023), and Adhvaryu et al. (2024), we use rainfall as an exogenous shock to household income during the early life stages of a child. Specifically, we define rainfall shocks as the number of extreme rainfall months from birth to age 6.[11] An extreme rainfall month is indicated by a dummy variable

[11] We explore alternative definitions of weather shocks, such as different S.D. cutoffs, alternative durations for defining early life, and separate analyses of droughts and floods. Our robustness checks, discussed in online Appendix D, confirm the consistency of our main findings across these alternative specifications.

denoting whether the month's rainfall deviates by one standard deviation from the 10-year historical county-specific mean, calculated as the average monthly rainfall of the same months over the preceding 10 years before birth (Bobonis, 2009). We use this relative measure to capture the fact that the same amount of rainfall may have different consequences for different regions with different average rainfall levels. Adhvaryu et al. (2024) demonstrate that defining the shock variable in this way captures the contemporaneous relationship between rainfall and agricultural wages: normal rainfall is associated with better outcomes than extreme rainfall. Furthermore, by using monthly records instead of annual measures, we aim to comprehensively capture extreme rainfall events and mitigate the risk of being obscured by mean values. Rainfall shocks are trimmed at the 5th and 95th percentiles to guard against outliers.

The selection of the early life period, spanning from birth to age 6, is motivated by two key considerations. Firstly, this timeframe represents a critical developmental stage in an individual's life, characterized by significant investments in human capital that have enduring effects on intellectual, health, and cognitive development, thereby shaping long-term educational and health outcomes (Vikram & Chindarkar, 2020). Secondly, as children typically commence formal education at age 6 in China, transitioning into compulsory schooling and becoming subject to the influence of school environments, extreme rainfall shocks during this pre-school period serve as indicators of adverse impacts on family human capital investments.

2.4 Summary Statistics

Online Appendix Table A2 presents summary statistics for individual-level variables from the 2010 census data and 2015 mini-census data, focusing on rural residents born between

September 1984 and August 1994 (aged 16-25 in 2010 and aged 21-30 in 2015), with non-missing location of residence information. Individuals born between 1984 and 1989 were not affected by the FCE reform, and therefore are the control group in our quasi-experimental setting. Furthermore, to explore the longer-term impact of the free compulsory education reform, our analysis is limited to individuals who should have completed junior high school education by 2010, i.e., those born in August 1994 or earlier.

In our main analysis, our sample comprises 622,860 individuals from the 2010 census data and 200,053 from the 2015 mini-census data.[12] On average, individuals in our sample received 9.9 years of schooling in 2010 and 10.3 years in 2015. Nearly all individuals (99%) completed primary school, with approximately 90% completing junior high school. However, the percentage of individuals who enrolled in high school was less than 40%, and the proportion who completed high school was even lower (25.6% in 2010 due to some individuals still being in high school at the time of the survey, increasing to 37% in 2015). On average, individuals were exposed to the FCE reform for 0.99 semesters in 2010 and 0.94 semesters in 2015. Regarding exposure to rainfall shocks, individuals experienced an average of 22.7 months of extreme rainfall during ages 0 to 6.

3 Empirical Strategy

To explore the interactions between early-life shocks and subsequent human capital investments, we conduct our empirical analysis in two steps. Initially, we examine the individual effects of early-life exposure to rainfall shocks and the free compulsory education

[12] Due to the integration of geographic administrative divisions, there are missing county-level rainfall observations. As a robustness check, we analyze the effects of the FCE reform using a sample with non-missing rainfall information. The results are presented in online Appendix Table D6. We find that the main coefficients remain largely unchanged, indicating the robustness of our results even when considering the data missing pattern.

reform on children's education. Subsequently, we combine the two sources of variation to estimate the interactive effects between these two treatments.

3.1 The Effects of FCE Reform

We estimate the effects of the FCE reform on children's educational outcomes by using a cohort difference-in-differences specification:

$$Y_{icptm} = \beta_1 Semester_{ptm} + \boldsymbol{X}_{icptm}\alpha + \delta_c + \gamma_{tm} + \varphi_{pt} + \theta_t \times \boldsymbol{W}_c + \epsilon_{icptm}, \quad (1)$$

where Y_{icptm} represents the education attainment of individual *i* residing in city *c* of province *p*, born in month *m* of year *t*; $Semester_{ptm}$ indicates the number of semesters that an individual is supposed to be eligible for free compulsory education; $\boldsymbol{X}_{icptm}$ is a vector of individual-level controls, including gender and ethnicity; δ_c and γ_{tm} represent city fixed effects and birth year-month fixed effects, respectively. Unobservable heterogeneous cohort trends that may be correlated with the reform exposure are of crucial concern to the cohort DID strategy. To alleviate this concern, we introduce province-birth year fixed effects (φ_{pt}) and the interaction terms between pre-determined city characteristics and birth year dummies ($\theta_t \times \boldsymbol{W}_c$). Pre-determined city characteristics include the number of secondary schools (logged) and the number of students enrolled in secondary schools (logged) in each city in 2005. By introducing φ_{pt} and $\theta_t \times \boldsymbol{W}_c$, we not only allow the trends to differ across provinces but also allow the trends to be related to the city's initial educational-related characteristics. To account for potential spatial correlation, we cluster standard errors at the city level. Our coefficient of interest in equation (1) is β_1, which provides the marginal effect per semester of free compulsory education on individuals' long-term education attainment.

After presenting a standard cohort DID specification, we introduce a by-cohort specification to estimate the FCE effects on each cohort separately:

$$Y_{icptm} = \beta_{1,k} \times \sum_{k=12}^{23} I_{pk} + \boldsymbol{X}_{icptm}\alpha + \delta_c + \gamma_{tm} + \theta_t \times \boldsymbol{W}_c + \pi_t \times \boldsymbol{W}_p + \epsilon_{icptm}, \quad (2)$$

where I_{pk} is set of indicator variables that take value one if, for individuals in province p, the age at the time of policy implementation was k. Due to the complete collinearity between the province-birth year dummy variables and the age indicator I_{pk}, we are not able to control for province-birth year fixed effects. To allow the cohort trends in outcome variables to vary with provincial characteristics in the initial period, we follow Xiao et al. (2017) and include the interactions of provincial characteristics in 2005 denoted by $\boldsymbol{W}_p$ and a vector of birth-year-specific coefficients π_t. The vector of provincial characteristics includes the natural log of gross domestic product (GDP), the proportion of the population with high school education or above, the proportion of rural household education expenditure to total expenditure, and government expenditure on education as a share of GDP in 2005. Considering that the Two Exemptions and One Subsidy (TEOS) policy was initially introduced for students from extremely poor households and later expanded to cover all primary and junior high school students from nationally designated poor counties in 2005, we also control for the interaction between the proportion of nationally designated poor counties in each province in 2005 and the birth year dummy indicators.

Our identification strategy does not require the rollout plan in each province to be exogenous. The central requirement for identification in our cohort DID strategy is the parallel-trend assumption: in the absence of the FCE reform, the cohort trends in education should not be related to exposure to the treatment. To validate our identification strategy, we not only account for the possibility that unobservable cohort trends could vary by province and be associated with pre-determined characteristics of city education but also employ a by-cohort specification as laid out in equation (2). Furthermore, we carefully consider the potential impact of other significant historical events occurring around the same time, such as the widespread closures and mergers of rural schools since 2001, the introduction of the New Cooperative Medical Scheme in rural China starting in 2003, and the Tax-for-Fee reform beginning in 2001. These results are presented in Section 4.2.

We argue that our estimation should be interpreted as a conservative lower-bound estimate of the FCE effect on rural education. Our baseline specification defines individuals above 16 years old when their province initiated the FCE reform as the control group. However, some among this group may still have been influenced by the FCE reform. Even if they had passed the age of compulsory education, they could have potentially benefitted from the free compulsory education system by, for instance, returning to school if they had previously dropped out.[13] Additionally, they could have indirectly benefited from the reform through interaction with their younger rural peers who were directly impacted. Thus, our estimation may underestimate the true effect of the FCE reform on the treated group under this scenario.

3.2 The Effects of Early-life Rainfall Shocks

We turn to estimate the effects of early-life rainfall shocks on individuals' education attainment using the following equation:

[13] We can observe the similar pattern in online Appendix Figure A2, which shows the slightly positive effects for individuals who just passed the age of corresponding education stages.

$$Y_{icptm} = \beta_2 RainfallShock_{jtm} + \boldsymbol{X}_{icptm}\alpha + \delta_c + \gamma_{tm} + \varphi_{pt} + \theta_t \times \boldsymbol{W}_c + \epsilon_{icptm}, \quad (3)$$

where $RainfallShock_{jtm}$ represents the number of extreme rainfall months from birth to age 6 for individuals born in month m of year t and residing in county j. An extreme rainfall month is indicated by a dummy variable denoting whether the month's rainfall was above or below one standard deviation from the 10-year historical county-specific mean. Our coefficient of interest in equation (3) is β_2, which measures the marginal effect per month of adverse rainfall shocks on these long-term outcomes. It provides the reduced-form effect of an early-life income shock on individuals' long-term education outcomes. The scope of this effect is comprehensive, including not only the direct, biological implications that such a shock might have on a child's health and human capital but also any subsequent adjustments in the form of compensating or reinforcing investments that parents might undertake in reaction to the shock.

3.3 Interaction between Early-life Shocks and FCE

To estimate the interactive effects between early-life shocks and subsequent human capital investments, we combine both sources of variation in a cohort triple differences framework:

$$\begin{aligned} Y_{icptm} = \beta_1 Semester_{ptm} + \beta_2 RainfallShock_{jtm} + \beta_3 Semester_{ptm} \times RainfallShock_{jtm} \\ + \boldsymbol{X}_{icptm}\alpha + \delta_c + \gamma_{tm} + \varphi_{pt} + \theta_t \times \boldsymbol{W}_c + \epsilon_{icptm}, \quad (4) \end{aligned}$$

where β_1 captures the causal effect of FCE reform for individuals who did not experience extreme rainfall shocks in early life, β_2 captures the causal effect of exposure to rainfall shocks in the early stages for children who did not receive the FCE, and β_3 captures the differential effect of FCE reform for disadvantaged individuals (who experienced extreme rainfall shocks). A positive β_3 would indicate a "catch-up" effect, suggesting larger effects of the FCE reform for more disadvantaged individuals. Conversely, a negative β_3 would imply that FCE reform widens the gap between disadvantaged and non-disadvantaged children.

4 Results

4.1 Baseline Results

4.1.1 The Effects of FCE Reform

We begin our empirical analysis by examining the effects of exposure to free compulsory education on individuals' education outcomes based on the cohort DID specification in equation (1). The coefficient on Semester, denoted as β_1, represents the effect of an additional semester of exposure on the outcomes. Tables 1 and 2 show the results for compulsory education in 2010, as well as high school completion and years of schooling in 2014, respectively.

In Table 1, all regressions include exogenous individual characteristics, city fixed effects, and birth year fixed effects. In columns (2), (5), and (8), we further control for the interaction terms between pre-determined city characteristics and birth year dummies ($\theta_t \times \boldsymbol{W}_c$) to allow for potential unobservable heterogeneous cohort trends related to the city's initial educational-related characteristics. Columns (3), (6), and (9) additionally include the birth year-month fixed effects (γ_{tm}) and province-birth year fixed effects (φ_{pt}), which is our preferred specification in line with equation (1). In all three specifications, the FCE reform exposure is positively associated with individuals' educational attainment. Estimates from our preferred specification suggest that one additional semester of reform exposure increases the probability of completing primary school by 0.16 percentage points, being enrolled in junior high school by 0.96 percentage points, and completing junior high school by 1.06 percentage points. However, the marginal effect on primary education completion is insignificant and small, likely due to the fact that 99% of individuals in our sample had already completed primary school by 2010.

Table 1. Effects of FCE on Compulsory Education Attainment (2010 Census)

	Completed Primary Sch			Enrolled in Junior High Sch			Completed Junior High Sch		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Semester	0.0014***	0.0012***	0.0016	0.0089***	0.0083***	0.0096***	0.0043**	0.0038*	0.0106***
	(0.0003)	(0.0003)	(0.0010)	(0.0016)	(0.0015)	(0.0028)	(0.0020)	(0.0022)	(0.0034)
City FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Birth year FE	YES	YES		YES	YES		YES	YES	
2005 city char.× birth year FE		YES	YES		YES	YES		YES	YES
Province-birth year FE			YES			YES			YES
Birth year-month FE			YES			YES			YES
Observations	622859	622859	622858	622859	622859	622858	622859	622859	622858
R^2	0.0793	0.0799	0.0834	0.0938	0.0938	0.0978	0.1060	0.1069	0.1134
Sample ages	16-25	16-25	16-25	16-25	16-25	16-25	16-25	16-25	16-25
Mean of Y	0.9909	0.9909	0.9909	0.9336	0.9336	0.9336	0.8836	0.8836	0.8836

Notes: The dependent variables in columns (1)-(3), (4)-(6), and (7)-(9) are the dummy indicators for whether the individual completed primary school, enrolled in junior high school, and completed junior high school in 2010, respectively. The Semester variable represents the number of semesters that an individual is supposed to be eligible for free compulsory education. All regressions include the individual-level control variables. * denotes for significance at 10%, ** at 5% and *** at 1%. Standard errors in parentheses are clustered at the city level.

Considering that one-fourth of our sample had not completed their schooling in 2010, with more than half of those individuals being in high school at the time, the estimated effect for high school completion and years of schooling in 2010 may not fully reflect the reform's impact on completed schooling. Educational outcomes in 2015, when 96% of individuals had completed their schooling and 85% of those who were in school had completed high school, can better capture the effect of the reform on lifetime schooling.[14] The estimates in Table 2 suggest that one additional semester of exposure increases years of schooling by 0.045 years and the probability of completing high school by 0.16 percentage points in 2015. However, those effects are statistically insignificant in some specifications, implying that the overall reform effects are mainly targeted at compulsory education, with a relatively smaller impact on

[14] We also exclude individuals who had not completed schooling and present the results of completed years of schooling in online Appendix Table D4.

higher-stage education attainment margins. It's worth noting that the years of schooling measure encompasses both compulsory education and post-junior high school education. With the average years of schooling in our sample exceeding the compulsory education period of 9 years, even if the FCE reform significantly improves junior high school completion, it may not necessarily translate into a significant overall increase in years of schooling on average.

Regarding the results of high school completion, our findings differ somewhat from those of Xiao et al. (2017). While both studies find a positive effect of the FCE on high school graduation rates, their results show significance whereas ours do not. This discrepancy could be due to two main reasons. First, there might be differences in the samples used. Xiao et al. (2017) used data from the 2014 China Family Panel Studies (CFPS), with fewer than 1400 samples in their final regression, possibly leading to sample selection bias. We compared the high school graduation rate in their sample with that in the rural samples from the NBS for the same birth cohorts and found theirs (45%) to be substantially higher than the national value reported by the NBS. Second, there might be differences in the reference groups chosen. Xiao et al. (2017) only controlled for individual birth year and birth month fixed effects, while our analysis additionally controlled for birth year-month fixed effects. If there are seasonal shocks specific to certain birth years or if seasonal effects change over time and are correlated with the dependent variable, this could lead to biased coefficient estimates. For instance, the age threshold for school enrollment (children must be six years old by September 1st to start primary school) began to be strictly enforced gradually after the enactment of the Compulsory Education Law, but enforcement varied by region. Family decisions to enroll children early or delay enrollment are often related to educational beliefs and attitudes, which could influence children's education attainment through parental investment in education.

Table 2. Effects of FCE on HS Completion and Years of Schooling (2015 Mini-Census)

	Completed High Sch			Years of Schooling		
	(1)	(2)	(3)	(4)	(5)	(6)
Semester	0.0023	0.0022	0.0052	0.0308**	0.0260**	0.0452
	(0.0020)	(0.0021)	(0.0098)	(0.0129)	(0.0128)	(0.0530)
City FE	YES	YES	YES	YES	YES	YES
Birth year FE	YES	YES		YES	YES	
2005 city char.× birth year FE		YES	YES		YES	YES
Province-birth year FE			YES			YES
Birth year-month FE			YES			YES
Observations	200053	200053	200053	200053	200053	200053
R^2	0.0986	0.0980	0.1011	0.1352	0.1355	0.1388
Sample ages	21-30	21-30	21-30	21-30	21-30	21-30
Mean of Y	0.3721	0.3721	0.3721	10.3299	10.3299	10.3299

Notes: The dependent variables in columns (1)-(3) and (4)-(6) are the dummy indicators for whether the individual completed high school, and the continuous variable representing years of schooling in 2015, respectively. All regressions include the individual-level control variables. * denotes for significance at 10%, ** at 5% and *** at 1%. Standard errors in parentheses are clustered at the city level.

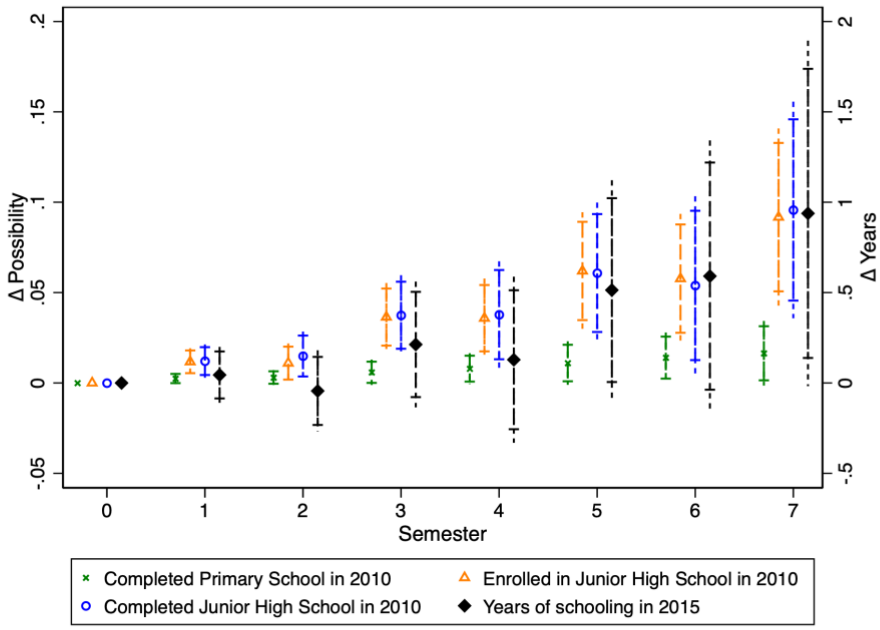

Figure 2. Effects of FCE on Education Attainment by Different Exposures to the Reform

Notes: This figure illustrates the effects of the FCE reform on individuals' education outcomes for each duration (i.e., 1-7 semesters) exposed to the reform, with the never-treated individuals serving as the control group. The coefficient estimates of each semester dummy are plotted using different symbol dots along with the 90% and 95% confidence intervals. The left y-axis represents the coefficients on the dummy indicators, including indicators for whether the individual completed primary school, enrolled in junior high school, and completed junior high school in 2010, while the right y-axis represents the coefficients on the continuous variable representing years of schooling in 2015. All regressions include the individual-level control variables, city fixed effects, birth year-month fixed effects, province-birth year fixed effects, and the interaction terms between pre-determined city characteristics and birth year dummies. Standard errors are clustered at the city level.

Additionally, we present the results of the by-exposure specification in Figure 2, which translates the continuous semester variable into eight semester dummy indicators. The results indicate that, compared to those not affected by the reform, individuals exposed to the policy for a longer duration have higher probabilities of completing primary school, enrolling in junior high school, and completing junior high school in 2010, as well as increased years of schooling in 2015. Specifically, the impact of the reform on junior high school education attainment surpasses that on primary education, consistent with the baseline regression results in Table 1.

4.1.2 The Effects of Early-life Rainfall Shocks

Table 3 presents estimates of the impact of early-life rainfall shocks on individuals' years of schooling using equation (3). Columns (1) and (2) depict the results for years of schooling in 2010, while columns (3) and (4) show the results for 2015. Given that a quarter of our sample had not completed their schooling in 2010 and 96% had done so by then, the outcomes in 2015 provide a better understanding of the reform's effect on lifetime schooling. Additionally, we exclude individuals who hadn't completed schooling and present the results of completed years of schooling in online Appendix Table D4. The findings reveal a significant negative impact of early-life extreme rainfall exposure on individuals' long-term schooling.

To enhance the interpretability of these coefficients, the table also presents the results of scaling the marginal effects per month of exposure by the standard deviation and sample mean of the rainfall shock, which are approximately 3.7 (accounting for 5% of the total 6 years) and 22.7 (accounting for 31% of the total 6 years) months, respectively. The results indicate that a one standard deviation increase in early-life extreme rainfall exposure leads to a notable decline in years of schooling, ranging from 0.04 to 0.06. Furthermore, an increase equivalent to the sample mean of early-life extreme rainfall exposure corresponds to a significant reduction in years of schooling, ranging from 0.24 to 0.37.

In addition to examining the influence of rainfall shocks on overall years of schooling, we investigate their effects on enrollment and completion probability across various educational stages. The findings presented in online Appendix Table A3 indicate a significant negative impact of early-life rainfall shocks on the likelihood of both enrolling in and completing high school, as well as on the probability of enrolling in college. However, the impact on earlier educational stages, such as primary school and junior high school, seems relatively insignificant. This might be due to the already high rates of enrollment and completion in compulsory education, along with the lower financial burden associated with primary and junior high schooling compared to higher levels. Moreover, higher education and beyond are not included in the free education system. As individuals grow older, they may start earning income, raising the opportunity cost of further education. Hence, the reluctance to pursue education beyond compulsory levels could be attributed to perceived economic trade-offs, particularly among disadvantaged individuals who experienced adverse weather conditions in their early years.

Table 3. Effects of Rainfall Shocks on Years of Schooling

	Years of Schooling			
	(1)	(2)	(3)	(4)
Early-life Rainfall Shocks	-0.0160***	-0.0164***	-0.0106**	-0.0112***
	(0.0043)	(0.0044)	(0.0042)	(0.0042)
2005 city char.× birth year FE		YES		YES
Observations	556409	556409	169763	169763
R^2	0.1133	0.1134	0.1302	0.1305
Sample ages	16-25	16-25	21-30	21-30
Mean of Y	9.9407	9.9407	10.3253	10.3253
Effect size of 1 S.D. rainfall shock (years)	-0.0591	-0.0606	-0.0394	-0.0413
Effect size of average rainfall shock (years)	-0.3622	-0.3713	-0.2409	-0.2526

Notes: All regressions include the individual-level control variables (including gender and ethnicity), city fixed effects, birth year-month fixed effects, and province-birth year fixed effects. To calculate the effect size of 1 S.D. (average) rainfall shock, we multiply the point estimates by one S.D. (sample mean) of exposure months. * denotes for significance at 10%, ** at 5% and *** at 1%. Standard errors in parentheses are clustered at the city level.

4.1.3 The Interactive Effects Between Rainfall Shocks and FCE

We now turn our attention to the main focus of our analysis, which is the interactive effects between rainfall shocks and the FCE reform, obtained by estimating equation (4). Table 4 and online Appendix Table A4 present the findings regarding years of education and the educational outcomes across various stages. In Table 4, the main effect of FCE (β_1) on years of schooling is positive, while the effect of rainfall shocks (β_2) is significantly negative, consistent with previous findings. More importantly, the interaction between exposure to the FCE reform and early-life rainfall shocks, denoted as β_3 in equation (4), is consistently positive and statistically significant across all columns. It suggests that the FCE reform had a more pronounced impact on disadvantaged individuals who experienced extreme rainfall shocks during their early life.

To facilitate the interpretation of our estimates, we show effect sizes in the bottom part of the tables for five categories of individuals: those exposed to the FCE reform for only 1 semester (without exposure to rainfall shocks); those exposed to rainfall shocks for 1 S.D. of exposure (without exposure to the FCE); and children exposed to both the FCE for 1/2/3 semesters and rainfall shocks for 1 S.D. of exposure. Using the results from column (4), we find that for individuals not exposed to adverse rainfall, the FCE increased educational attainment by 0.025 years. However, for those exposed to adverse rainfall for one S.D. of exposure, the FCE increased educational attainment by 0.039 years, with the differential effect being statistically significant. In other words, the FCE facilitated substantial catch-up for individuals exposed to adverse rainfall. One S.D.'s exposure to negative rainfall shocks decreased educational attainment by 0.043 years. Yet, one semester of FCE exposure mitigated this reduction by 0.014 years, accounting for 32% of the disadvantage caused by the rainfall shock during their early life in years of schooling. This suggests that two additional exposures to free compulsory education could reverse the negative effects of one S.D. of extreme rainfall, although the gap between rainfall-affected individuals and unaffected ones would still exist. Nearly three additional exposures to the FCE could further mitigate this gap, allowing disadvantaged children affected by rainfall to catch up with their unaffected peers.

Table 4. Effects of FCE and Rainfall Shocks on Years of Schooling

	Years of Schooling			
	(1)	(2)	(3)	(4)
Semester	0.0144	0.0141	0.0250	0.0252
	(0.0313)	(0.0313)	(0.0620)	(0.0622)
Early-life Rainfall Shocks	-0.0162***	-0.0166***	-0.0109***	-0.0115***
	(0.0044)	(0.0044)	(0.0042)	(0.0042)
Semester × Early-life Rainfall Shocks	0.0032**	0.0031**	0.0036**	0.0037**
	(0.0014)	(0.0013)	(0.0016)	(0.0016)
2005 city char.× birth year FE		YES		YES
Observations	556409	556409	169763	169763
R^2	0.1133	0.1135	0.1303	0.1305
Sample ages	16-25	16-25	21-30	21-30
Effect: Rainfall shock=0, FCE=1 Semester	0.0144	0.0141	0.0250	0.0252
Effect: Rainfall shock=1 S.D., FCE=0 Semester	-0.0601	-0.0616	-0.0405	-0.0425
Effect: Rainfall shock=1 S.D., FCE=1 Semester	-0.0340	-0.0359	-0.0021	-0.0036
Effect: Rainfall shock=1 S.D., FCE=2 Semesters	-0.0078	-0.0103	0.0363	0.0353
Effect: Rainfall shock=1 S.D., FCE=3 Semesters	0.0183	0.0153	0.0747	0.0743

Notes: All regressions include the individual-level control variables (including gender and ethnicity), city fixed effects, birth year-month fixed effects, and province-birth year fixed effects. The bottom of the table displays the estimated effect sizes for five categories of individuals: those exposed to the FCE for only 1 semester (without exposure to rainfall shocks); those exposed to rainfall shocks for 1 S.D. of exposure (without exposure to the FCE); and children exposed to both the FCE for 1/2/3 semesters and rainfall shocks for 1 S.D. of exposure. * denotes for significance at 10%, ** at 5% and *** at 1%. Standard errors in parentheses are clustered at the city level.

Figure 3 illustrates the relationship between exposure to early-life rainfall shocks and years of (completed) schooling using lowess smoothing. Initially, we regress years of (completed) schooling and rainfall exposure, controlling for city effects, birth year-month fixed effects, province-birth year fixed effects, individual controls, and interaction terms between predetermined city characteristics and birth year dummies. Subsequently, we non-parametrically plot the relationship between the years of (completed) schooling residuals on the y-axis and the rainfall exposure residuals on the x-axis, separately for the treatment, control, and pooled groups. The treatment group consists of individuals who were exposed to the FCE reform for at least one semester, while the control group consists of individuals who were not exposed to the FCE reform. The green control group line exhibits a monotonically decreasing shape, reinforcing the notion that prolonged exposure to extreme rainfall is detrimental for children. Moreover, the red line representing the treatment group consistently exhibits a more gradual and smooth decline towards the right tails compared to the control group. This implies that exposure to FCE attenuates the adverse effects of early-life extreme rainfall on educational attainment, particularly for individuals experiencing prolonged exposure to adverse conditions. This finding aligns with our parametric regression estimates in Table 4.

Online Appendix Table A4 further examines the interactive effects on enrollment and completion probability across various educational stages. The results reveal that the impacts of FCE on completing primary school, enrolling in junior high school, and completing junior high school are positive and significant. However, the main effect of the rainfall shock turns negative and significant starting from enrolling in high school. For grades below this, early life disadvantage does not appear to influence education completion significantly, potentially due to the high completion rates of primary school (99%) and junior high school (88%) among our 16–25-year-old sample, aligning with the findings in Adhvaryu et al. (2024). Moreover, starting from enrolling in high school, significant positive interaction coefficients emerge, indicating that the effects of FCE are more pronounced for individuals who experienced adverse early-life rainfall compared to those who did not, consistent with our findings on years of schooling.

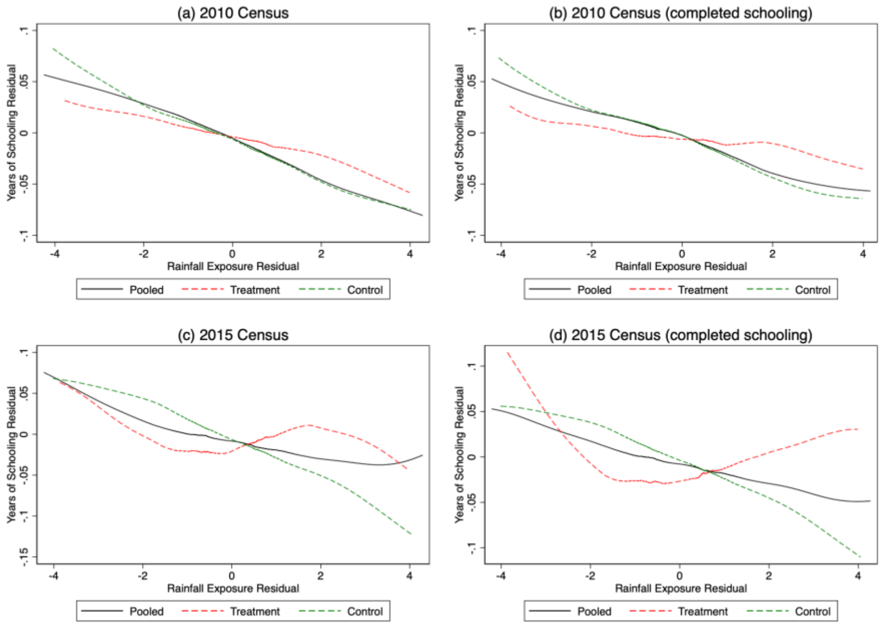

Figure 3. Years of Schooling Attainment by Extreme Rainfall Exposure

Notes: All three lines represent the lowess-smoothed residuals of years of schooling in 2010, years of completed schooling in 2010, years of schooling in 2015, and years of completed schooling in 2015 for the corresponding group in panels (a), (b), (c), and (d), respectively. The treatment group consists of individuals who were exposed to the FCE reform at least one semester, while the control group consists of individuals who were not exposed to the FCE reform, and the pooled group includes the treatment group and the control group. Years of (completed) schooling and rainfall exposure residuals are calculated after regressing each variable on city effects, birth year-month fixed effects, province-birth year fixed effects, the individual controls, and the interaction terms between pre-determined city characteristics and birth year dummies. Standard errors are clustered at the city level. Rainfall exposure residuals are trimmed at the 5th and 95th percentiles.

To summarize, these results reveal that, within our specific context, there is limited evidence supporting a positive interaction between shocks to family and school human capital investments. Instead, the evidence leans towards suggesting substitutability rather than complementarity between these two types of investments. This aligns with similar conclusions drawn by Rossin-Slater & Wüst (2020), Goff et al. (2023), and Adhvaryu et al. (2024).

4.2 Validity of Identification and Robustness

The key assumption for the cohort DID (DDD) strategy is the parallel-trend assumption: in the absence of the FCE reform, cohort trends in education should not be related to treatment exposure. To validate this assumption, we conduct the following tests.

First, we check whether non-eligible individuals from provinces with different effective dates of the reform have parallel cohort trends in outcomes by using a by-cohort specification in equation (2). Section 4.2.1 shows the corresponding results. Second, we perform several placebo tests, including using samples of urban residents, neither of which were affected by the reform, and a permutation placebo test by randomly assigning the effective date of the reform to provinces. Online Appendix C provides a discussion of the details of these tests. Third, we consider the potential confounding effects of contemporaneous historical events that could influence educational outcomes. This includes the large-scale closures and mergers of rural schools since 2001, the implementation of the New Cooperative Medical Scheme in rural China starting in 2003, and the Tax-for-Fee reform beginning in 2001. By accounting for these events, we aim to ensure that any identified effects are truly attributable to the FCE reform rather than other changes occurring during the same period. The results are presented in Section 4.2.2.

In addition, another concern of the identification strategy is the endogenous migration. Specifically, our specification implicitly assumes that rural individuals who received their compulsory education and during their early life still resided in the same county when the 2010/2015 population census was conducted. However, migration poses a threat to our identification if individuals are no longer living in the same county where they received their education and experienced early-life conditions. To address this concern, we conduct several robustness checks in Section 4.2.3. We finally provide further robustness checks in online Appendix D to test whether our baseline results are sensitive to alternative measurements of key variables, cohort definitions, and school cutoff dates.

4.2.1 Parallel Cohort Trends

We first check the pre-trends by using a by-cohort specification in equation (2). Online Appendix Figure A2 plots the point estimates $\beta_{1,k}$ and 95% confidence intervals for each cohort using the 2010 and 2015 census data. The x-axis represents individuals' ages at the time of the FCE reform, while the y-axis represents the corresponding coefficients. According to the Chinese education system mentioned previously, children enroll in primary school upon celebrating their 6th birthday (6-7 years old), graduate from primary school at the age of 12-13, and then proceed to junior high school. After 3 years, at the age of 15-16, they graduate from junior high school and move on to high school. Finally, at the age of 18-19, they graduate from high school. However, due to the possibility of some children returning to school after dropping out earlier, there may be spill-over effects for cohorts that exceed the usual enrollment age.

The top row of online Appendix Figure A2 displays the results for junior high school completion and enrollment in 2010. The dashed line represents the regular enrollment or completion age for junior high school. For the control group cohorts, we observe diminishing effects with increasing age, and none of the effects are statistically significant from zero, thus validating the parallel cohort trend assumption. The second row shows the results for primary school completion in 2010 and high school completion in 2015, revealing minimal FCE effects for each cohort, consistent with our baseline null estimation results. The third row presents the results for years of schooling in 2010 and 2015. Due to many younger individuals not having completed their education in 2010, there is a slight downward trend in the FCE effect. However, by 2015, when 96% of individuals had completed their schooling, we can better capture the reform's effect on lifetime schooling. We find that younger cohorts are more affected by the reform, while those aged 18 and above, with lower probabilities of re-enrollment after dropping out, are hardly affected by the FCE, thus further confirming the parallel trend assumption.

4.2.2 Confounding Factors

We re-estimate our baseline specification while considering the effects of concurrent shocks that could influence our results, with detailed results provided in online Appendix Table A5. Firstly, we account for the large-scale closures and mergers of rural schools since 2001. China initiated these closures and mergers to enhance education quality and reduce education expenditure, resulting in a significant reduction in the number of rural schools, particularly in remote villages. Consequently, students from these areas were transferred to schools in county towns or townships, leading to increased education costs. The closure and merger of rural schools may elevate dropout rates while overlooking enrollment rates could introduce bias into the estimation. Therefore, we control for changes in the number of primary schools at the city level between 2001 and 2005 and interact these changes with the Semester variable. Results show that the coefficients on Semester remain stable after controlling for school accessibility.

Secondly, we consider the implementation of the New Rural Cooperative Medical Scheme in rural China starting in 2003. This initiative aimed to provide health insurance coverage for the entire rural population by 2010. Access to public health insurance may enhance school enrollment among school-aged children in rural areas following negative health shocks. We calculate the city-level participation rates of rural medical insurance using the 2005 national 1% population sample survey data and interact them with the affected semesters in our regression. Our baseline results remain robust with the inclusion of these interactions.

Lastly, we examine the Tax-for-Fee reform implemented between 2000 and 2003. This reform aimed to introduce fiscal discipline at the local level by replacing various taxes, fees, and levies on farmers with a standardized agricultural tax. While the reform increased rural residents' per capita income, it had negative implications for public goods provision, including school construction. Hence, we interact Semester with the reform's starting year at the province level. Our estimates remain consistent even after accounting for the impact of this reform.

4.2.3 Migration

One may be concerned about potential bias in our estimation if individuals were no longer living in the same county where they received their education and experienced early-life conditions. To address this concern, we conduct several robustness checks based on equations (1), (3), and (4), displaying the point estimates of our key variables of interest along with their 95% confidence intervals in online Appendix Figure A1.[15] We refine our sample by considering individuals' registered residence, current residence, and residence five years prior. Firstly, we exclude individuals in first-tier cities (i.e., Beijing, Shanghai, Guangzhou, Shenzhen), where the proportion of migrants is highest. Secondly, we exclude new migrants who moved to their current residence after the implementation of the FCE reform. Thirdly, we remove individuals whose current residence province does not match their Hukou registration province. Fourthly, we exclude individuals whose current residence city does not match their Hukou registration city. Fifthly, we remove individuals whose current residence county does not match their Hukou registration county. Sixthly, we restrict the sample to individuals whose Hukou registration province, current residence province, and province of residence five years prior all match.

[15] We also examined whether the FCE reform influenced individuals to migrate across provinces, cities, and counties. Panel A of online Appendix Table A6 presents the results based on data matched with individuals' current residence places, while Panel B presents the results based on data matched with their Hukou registration places. We defined inter-province/city/county migrants as individuals whose current residence location differs from their Hukou registration location at the province/city/county level. Our results indicate limited evidence of families moving in response to the shocks. Given the small magnitude of migration responses due to the FCE, it is unlikely that migration is driving the effects of the FCE reform on children.

Finally, we replace the data matching based on current residence with matching based on Hukou registration place. We find that the results remain consistent across these specifications.

5 Discussion

5.1 Heterogeneity

In this section, we explore the heterogeneous effects of the FCE, early-life rainfall shocks, and their interactions on individuals' education attainment, which may vary depending on factors such as gender, rural income, and pre-determined agriculture factors.

In rural China, there is a traditional preference for male children over female children, which may lead to differential treatment in terms of education within families. Particularly in economically challenging circumstances, parents may prioritize their sons' education over their daughters', potentially resulting in girls receiving less education or even being excluded from enrollment altogether (Wu et al., 2023). Consequently, we hypothesize that both the beneficial effects of the FCE reform and the adverse impacts of extreme rainfall (and their interaction effects) might be more pronounced for girls. For girls, who are typically marginalized in terms of educational resources, the FCE reform could represent a significant improvement in access to and quality of education. This pattern is particularly evident for disadvantaged girls exposed to extreme rainfall during early life, highlighting heterogeneity in interaction effects. Conversely, extreme rainfall events could have a more detrimental impact on girls' education, as families under increased economic pressure may prioritize boys' schooling during crises.

Online Appendix Table A7 presents estimates of the heterogeneous effects by gender, largely confirming our hypothesis: all effects are more pronounced for girls compared to boys.

Interestingly, we find that the effects of the FCE reform are notably larger for girls across all education outcomes. While the overall impact of the reform on high school completion appears limited, the interaction terms between Semester and Female indicate that, compared to boys, girls experience a significant improvement in completing high school as a result of the FCE reform. A similar pattern is observed for the outcome of years of schooling, suggesting that the reform has a more substantial effect on girls' educational attainment.

We further investigate the heterogeneity by regional rural income and agricultural factors. We divide the sample based on the median of rural per capita annual income in 2005. Online Appendix Table A8 presents the results for subsamples, revealing that the effects of the FCE, rainfall, and their interactions are more pronounced in cities with lower rural household income, which typically face greater financial constraints on children's education.

Given the significant role of agriculture in household income in our analysis, the reliance on this sector plays a pivotal role, particularly for assessing the effects of rainfall and the interaction effects. It is likely that these effects vary depending on the degree of dependence on agriculture. We divide the sample based on the median of province-level grain yield per unit area in 1980. Online Appendix Table A9 presents the results, indicating that the subsample with higher grain yield is more susceptible to weather impacts on agricultural production, thus experiencing more pronounced adverse effects of rainfall shocks. Consequently, the safety net effect of the FCE reform is also more substantial for this group, as evidenced by the greater and more significant coefficients on interaction terms between FCE and early-life rainfall shocks.

5.2 Effects on Long-term Outcomes

Beyond direct educational attainment, we are interested in exploring whether rainfall shocks and exposure to the FCE reform yield similar effects on longer-term outcomes such as marriage, fertility, and labor market performance. We use data from the 2010 census to examine the impact on early marriage and fertility, as the sample ages range from 16 to 25 years old. By 2015, the cohort ages range from 21 to 30 years old, and most individuals have likely completed their marriage and fertility decisions. Our outcomes of interest include the marriage age for the 16-25 years old cohort and a binary indicator denoting whether girls gave birth before age 24 within the 16-25 years old group. Additionally, we use data from the 2015 census to investigate the impact on labor market performance. Since the minimum age in our sample is 21 years old, and the majority have entered the labor market, we are interested in whether the FCE increased the probability of rural residents engaging in non-agricultural employment.

The findings in columns (1)-(6) of online Appendix Table A10 suggest that the FCE reform results in a delay in the age of marriage and a decline in the probability of early childbirth among girls, especially for those who experienced adverse rainfall shocks during their early years. This finding aligns with the consistent pattern observed in online Appendix Table A7, where the FCE is found to significantly increase girls' educational attainment, consequently delaying their marriage and childbirth due to extended schooling. Moving to columns (7)-(9) of this table, we observe that the FCE reform is associated with an increased likelihood of non-agricultural employment. Conversely, early-life rainfall shocks are found to significantly reduce this probability. However, the interaction term coefficient, while positive, does not reach statistical significance, indicating that the combined effect of FCE and early-life rainfall shocks on non-agricultural employment is not statistically significant.

6 Conclusion

This paper provides novel insights into the interaction between early-life shocks and subsequent human capital investments by combining two sources of exogenous variation: extreme rainfall shocks and the implementation of free compulsory education reform in rural China. We demonstrate that exposure to the reform significantly enhances years of schooling and grade completion during compulsory schooling. Conversely, early-life adverse rainfall significantly diminishes educational attainment, resulting in an average decrease in schooling of 0.2-0.4 years. Nonetheless, longer exposure to the reform attenuates these adverse effects, indicating dynamic substitutability between family and government human capital investments. Specifically, nearly two additional exposures to the FCE could offset the negative effects of one standard deviation of extreme rainfall, while nearly three additional exposures to the FCE could further narrow the gap between the weather-affected group and the unaffected group, enabling disadvantaged children affected by rainfall to catch up with their unaffected peers. Our key findings remain robust across various robustness checks. Moreover, we observe heterogeneity in the effects of FCE reform, early-life rainfall shocks, and their interactions on individuals' education attainment across gender, rural household income, and the degree of dependence on agriculture. Additionally, we observe that apart from educational attainment, these effects persist and have implications for marriage, fertility, and labor market performance.

Our findings hold policy relevance on two fronts. Firstly, the interaction effects we observe indicate that public investment can effectively aid children who have experienced early-life adversity in bridging the gap with their peers. This provides governments with the potential to implement safety net policies for remedial actions, offering a pathway to address early-life shocks through interventions later in life. A second implication is that even universally applicable policies can effectively target the most vulnerable populations, such as low-income groups, within the targeted groups.

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