Early School Access and Educational Attainment: Evidence from China's School Starting Age Reform

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Abstract

Transitioning into formal schooling, a pivotal event in early childhood, shapes children's achievement trajectories. This paper presents the first causal estimates of the long-run impacts of the access timing of formal schooling. I exploit a unique education reform in China that lowered the ages of access to primary school from seven and eight to six and seven. Utilizing a generalized difference-in-differences method based on the staggered adoption of the reform from 1986 to 1994, I find that early access to formal schooling substantially increases post-compulsory educational attainment and potentially affects later adulthood outcomes. Additional evidence on mechanisms supports the theory of dynamic complementarities in skill formation.

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1 Introduction

Research shows that a child's early life circumstances have lifelong consequences (Havnes and Mogstad, 2011; Almond et al., 2018; Baker et al., 2019). Starting formal schooling is a pivotal moment in early childhood, introducing new expectations such as independent work, adherence to schedules, and the acquisition of literacy and numeracy skills (Li-Grining et al., 2010). Policies governing the age at which formal schooling begins vary widely across countries, yet there is little evidence about what age is optimal.

While evidence generally indicates positive effects for preschool-age education programs (McCoy et al., 2017), the impacts of early starts of formal schooling are uncertain due to the nuanced balance between the brain's adaptability and a child's school readiness. On the one hand, the brain's capacity to be modified by experience decreases with age (Nelson, 2000). Early starts of formal schooling can more effectively stimulate brain development, leading to improved achievement patterns in later life (Cunha et al., 2006). On the other hand, formal schooling is more structured and academically orientated. Allowing sufficient time to acquire the necessary skills before schooling begins is crucial for successful transitions and later achievement (Duncan et al., 2007; Pianta et al., 2007).

This paper examines the impacts of the timing of formal schooling. In particular, I estimate the long-run impacts of early access of formal schooling by leveraging a unique school starting age (SSA) reform in China. The SSA reform lowered the school starting age from seven and eight, for the majority of children, to six and seven. Students who reached the stated school starting age were allowed to enroll, and students who were below the stated

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age were prevented from enrolling. Therefore, the SSA reform extended formal schooling access to children aged six and seven, thus providing an opportunity to identify the effects of early access. The SSA reform was gradually adopted across provinces beginning in 1985 and was universally adopted by all provinces by 1994. Exploiting this variation in adoption timing, I use a generalized difference-in-differences method to estimate causal effects, relying on not-yet-treated provinces as the comparison group (Callaway and Sant'Anna, 2021).

In China, the compulsory schooling law mandates the completion of both primary and middle school education, rather than specifying a minimum school leaving age. Therefore, a change in the starting age of children does not automatically result in a change in the duration of compulsory schooling, a factor that could confound estimates of long-run impacts. Moreover, there were several curriculum systems across the country at the time of the SSA reform, specifying years of primary and middle school. This introduces variation in compulsory schooling duration independent of school starting age, enabling me to discern the underlying mechanisms.

Using 2005 census microdata, I find exposure to the SSA reform increased high school enrollment by 5.2 percentage points, equivalent to 18.4 percent of the pre-reform average high school enrollment. In comparison to other interventions, the impact is half of the estimated returns to participating in classroom-based early childhood education programs before age five (McCoy et al., 2017), and comparable to an additional $10,000 in parents' annual income from prenatal to age five (Duncan et al., 1998).

Heterogeneity analyses reveal larger effects for children from provinces with more preschool resources and children with longer compulsory schooling duration, consistent with the predictions of dynamic complementarities of skill formation (Cunha and Heckman, 2007). Further

analyses rule out alternative explanations. Additional findings suggest the effects of early access to formal schooling may persist into later adulthood through increased higher education participation, delayed labor market entrance, and delayed first marriage.

My paper makes three important contributions to the school starting age literature. First, I contribute to the growing decomposition literature by adding novel evidence on the access timing effects. Traditionally, school starting age effects are estimated using variations in birth dates and school entry cutoffs. The estimated effects encompass a combination of access timing of formal schooling ("access timing effects"), peer effects ("relative age effects"), and maturity when observed by researchers ("age-at-test effects" or "age-at-survey effects"). Recent literature shows increased evidence isolating relative age effects (Elder and Lubotsky, 2009; Cascio and Schanzenbach, 2016; Peña, 2017) and age-at-test effect (Black et al., 2011; Crawford et al., 2014). My analysis adds new evidence to the limited findings isolating access timing effects.

Second, existing studies have found that early access to formal schooling improves short- to medium-run test scores (Herbst and Strawiński, 2016; Rosa et al., 2019; Ryu et al., 2020). To the best of my knowledge, I present the first causal estimates of the long-run impacts of access timing. Causally estimating the long-run impacts faces two additional challenges: data and identification. The data challenge requires a policy change in a historical setting. The identification challenge demands a setting without a minimum school leaving age which creates perfect collinearity between school starting age and compulsory schooling duration. My study overcomes both challenges by examining a setting in 1980s China and thus provides causal evidence of the long-run impacts of early access to formal schooling.

Third, my analysis has clear advantages over previous research on school starting age in

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China, which relies on variation in birth dates and a two-way fixed effects regression method (Chen and Park, 2021; Zhang, 2022). My analysis estimates the reduced-form policy effects rather than the individual-level actual age-at-enrollment effects generated by idiosyncratic birthdays. The effect of the SSA reform is more policy-informing, as it captures the average treatment effect of being able to access formal schooling earlier across all birthdays and circumvents the identification challenges associated with manipulation around the school entry cutoff date. My paper also improves upon prior estimates by incorporating the recent advancements in the difference-in-differences literature by using a heterogeneity-robust estimating method. The method ensures my estimates are free of the potential issues of the regression estimates, including negative weights (Goodman-Bacon, 2021), cross-contamination (Sun and Abraham, 2021), and insensible aggregation weights (Callaway and Sant'Anna, 2021).

My analysis also contributes to China's Compulsory Education Laws (CEL) literature by proposing a method to isolate the impact of the SSA reform from the overall effects of the CEL package. The CEL, as the initial national education law, encompasses various policies governing aspects such as compulsory duration, school starting age, infrastructure, costs, school finance, and teacher training. While previous studies have shown that the CEL increased educational attainment (Fang et al., 2012; Huang, 2015), the specific contributions of each policy remain unclear. I isolate the effect of the SSA reform from other CEL policies by comparing cohorts exposed to CEL during preschool age to those exposed during school age. My estimates suggest that the SSA reform accounts for roughly one-fifth of the total increase in educational attainment resulting from the introduction of the CEL.

The remainder of the paper is organized as follows: Section 2 explains the institutional

background and details of the SSA reform; Section 3 describes the data, variables, and sample constructions; Section 4 discusses the empirical strategy; Section 5 presents the results; Section 6 concludes.

2   Institutional Background

2.1   Education in China at the Time of the Reform

This paper examines the school starting age (SSA) reform from the 1986 Compulsory Education Law (CEL), which is the first formal national education law in China. At the time of legislation, the overall level of education in China was low. In the 1982 Chinese census, the adult[1] literary rate was around 65%,[2] which was lower than the 1982 world average of 70%.[3] Around 33% of adults had any middle school education, fewer than 12% had any high school education, and 1% had any college education.[4]

Pre-primary education enrollment was uncommon, and access was limited. The gross preschool enrollment in China was below 20% before 1985 and remained below 25% between 1985 and 1994.[5] There were, on average, fewer than 3 preschools per thousand preschool-age kids.[6]

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[1]Aged between 18 and 69, following Deng and Treiman (1997).

[2]The adult literacy rate in China differed by gender, with the male rate of 79% and the female rate of 50%. Literacy was higher among younger generations. The 1982 literacy rate among people aged 18 to 30 was 83%, with the male rate of 93% and the female rate of 72%.

[3]Source: UNESCO Institute for Statistics (UIS). Accessed October 24, 2022. `apiportal.uis.unesco.org/bdds`.

[4]The economic development was also low. According to World Bank national accounts data and OECD National Accounts data files, Chinese GDP per capita in 1985 was 294.5 current US dollars, significantly lower than the world average of 2659.2 dollars and OECD member average of 9508.7 dollars.

[5]Source: UNESCO Institute for Statistics (UIS). UIS.Stat Bulk Data Download Service. Accessed October 24, 2022.`apiportal.uis.unesco.org/bdds`

[6]In China, preschool in China has three grades; thus, preschool age is defined as between ages 3 and 5. Defining preschool age as between 4 and 6 will not change the conclusion. The number of preschools from

The CEL was introduced after the upheaval of the Cultural Revolution (1966-1976).[7] During the Cultural Revolution, the education system was disrupted, and the importance of intellectual knowledge was denied (Liu, 1993; Deng and Treiman, 1997; Lu, 2020).[8] In particular, primary and secondary curriculum and duration underwent constant reforms [9] and the merit-based national college entrance exam was halted until 1977.

Figure 1 shows the trends in enrollment among the cohorts from 1960 to 1990, using data from the 2005 census.[10] The segment between two dashed vertical lines represents the educational attainment of my main analytical sample, with the red vertical line marking the first SSA reform-exposed cohort in my sample. Primary school enrollment remains consistently high across all cohorts. Middle school enrollment increased slightly for the 1960 to 1963 cohorts and declined for cohorts between 1963 and 1968. The 1960 cohort started school during the Cultural Revolution, and the short-run increase in middle-school enrollment may be attributed to the temporary positive effect of redistributing educational resources into rural areas(Liu, 1993). The subsequent drop reflects the long-term disruptive effects of the Cultural Revolution. The 1968 cohorts started primary school around the end of the Cultural Revolution, leading to a steady increase in middle school enrollment thereafter. A

Liu (1993). The number of preschool-age kids was calculated using population data and the birth rate from National Bureau of Statistics (2010).

[7]China's Cultural Revolution was a socio-political movement characterized by widespread social upheaval, political purges, and ideological fervor initiated by Mao Zedong in 1966. The Cultural Revolution's disruptions are commonly seen as lasting until Mao's death in 1976.

[8]Liu (1993) reviewed education policies from 1949 to 1990 and compiled a directory of regulations in his book "*Zhongguo Jiaoyu Dashidian, 1949-1990 [Book of Major Educational Events in China, 1949-1990]*." Deng and Treiman (1997) systematically summarized the education system, historical events, and policy interventions from the establishment of the People's Republic of China to the end of the Cultural Revolution. Lu (2020) documented the impact on Chinese culture by analyzing rhetoric, symbols, and symbolic practices of the Cultural Revolution.

[9]"Minutes of the National Education Work Conference." China's Communist Party Central Committee. August 13. 1971.

[10]A detailed discussion of the construction of the enrollment measure is provided in the data section.

similar pattern is observed for high school enrollment, except the decline started as early as the 1961 cohorts, indicating mostly disruptive effects at the high school level. High school enrollment steadily increased after the 1968 cohorts. The abrupt drop for the 1988 to 1990 cohorts is likely because they may have been too young to enroll in high school in 2005. College enrollment is low but exhibits a steady increase across the sample cohorts. The drop for 1986 and later cohorts is because they were too young to enroll in college in 2005.
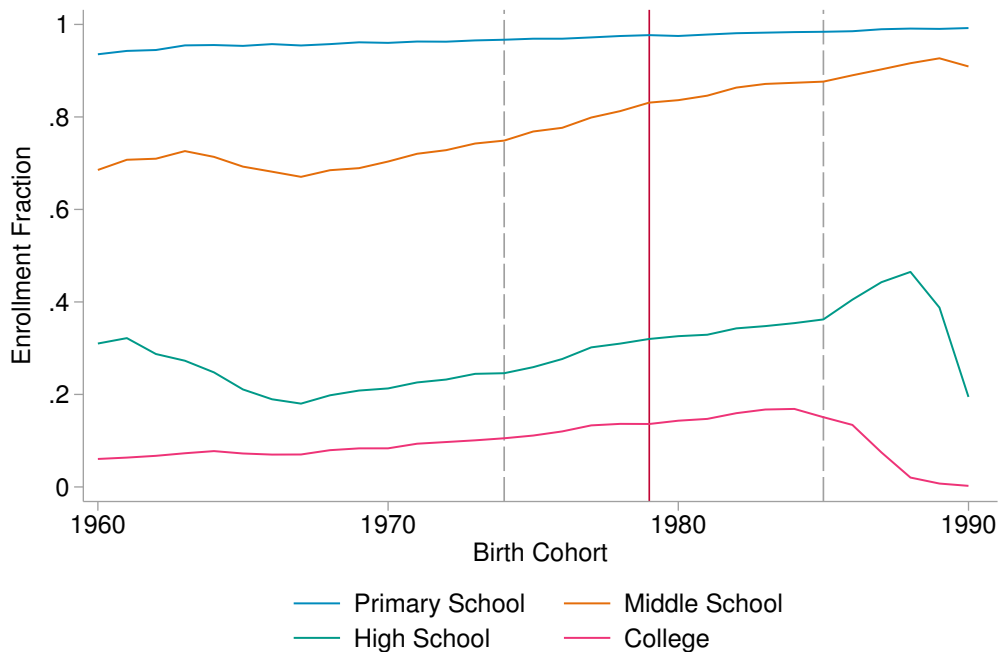

Figure 1: Trends in Educational Attainment in 2005, by Birth Cohort

Notes: The dashed grey lines suggest the main analytical sample. The red line indicates the first treated cohort of the school starting SSA reform. The cohort is defined as children born within the same school year, from September 1 to August 31 of the following year. Source: 2005 Census.

2.2 The School Staring Age Reform

On April 12, 1986, the Compulsory Education Law (CEL) was passed by the National People's Congress. The 1986 CEL consisted of 18 articles[11] that regulated various aspects

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[11]The articles of the 1986 CEL lacked specific details and suffered from vague wording. They failed to provide comprehensive and clear instructions and created challenges during implementation, especially for

of education encompassing compulsory duration,[12] school starting age,[13] infrastructure,[14] costs,[15] school finance,[16] teacher training.[17]

In this paper, I focus on the policy regulating school starting age. Prior to the SSA reform, there was no national standard for school starting age. The previous policy specified a national school starting age[18] dated back to 1951, shortly after the founding of the People's Republic of China. This hastily written education policy specified age seven as the school starting age. Little is available about the school starting age during the Cultural Revolution. After the Cultural Revolution, the 1978 Plan[19] advocated for admitting six-and-a-half or six-year-old children into primary schools if "conditions allow," without specific guidelines. Prior to the SSA reform, age six, age seven, and age eight rules had been reported to be used across the nation (Liu, 1993).

The SSA reform, Article 5 of the 1986 CEL, specified "all children who have reached

the section regarding school funding. The CEL went through major amendments and adoptions in 2006 and expanded to 63 articles. The current CEL is based on the 2006 CEL and had two revisions in 2015 and 2018.

[12]Article 2: "the state shall institute a system of nine-year compulsory education." In practice, the policy was implemented by making primary and middle school compulsory. Depending on the school system, the duration could be 8 or 9 years.

[13]Article 5: "all children reached the age of six shall enroll in school and receive compulsory education for the prescribed number of years, regardless of sex, ethnicity, or race. In areas where that is not possible, the beginning of schooling may be postponed to the age of seven."

[14]Article 9 specified local government to establish primary and middle schools to ensure convenient access to education and required urban and rural development plans to include compulsory education facilities.

[15]Aritcle 10: "the state shall not charge tuition for students receiving compulsory education." In practice, local governments and schools charged household miscellaneous fees (Tsang, 1996). The bottom income quintile households spent 14.2% of annual income on education and 9.7% of annual income on basic and secondary education. The household financial burdens were addressed by 2000s reforms on schools costs, "Two-Exemptions-One-Subsidy."

[16]Article 12 specified government should increase expenditure for education, levy a surtax for education, provide subsidies for financially challenged areas, encourage donations, and offer assistance to areas inhabited by minority nationalities. According to the calculation by Tsang (1996), provincial and local governments accounted for 99.98% of expenditure for primary education and 99.85% for secondary education in 1991.

[17]Article 13 specified strengthening normal schools and establishing a system to test teacher credentials.

[18]"Zhengwuyuan Guanyu Gaige Xuezhi de Jueding [Decisions to Reform the School System]." State Council of the Central People's Government. October 1, 1951.

[19]"Quanrizhi Shinianzhi Zhongxiaoxue Jiaoxue Jihua Shixing Cao'an [Full-Time Ten-Year Primary and Secondary Education Plan Trail Draft]". The Ministry of Education of the People's Republic of China. January 18, 1978.

the age of six shall enroll in school and receive compulsory education for the prescribed number of years regardless of sex, ethnicity or race. In areas where that is not possible, the beginning of schooling may be postponed to the age of seven."[20] Students were prevented from attending until they met the stated school starting age, but no consequences for the children who failed to enroll by the stated school starting age. As compliance was voluntary, the SSA reform created a national lower bound for primary school admission age and should be interpreted as extending access to formal schooling to children aged six or seven.

Decentralization in promoting compulsory education was emphasized by the CEL. Local People's Congresses formulated legislation on provincial Compulsory Education Law implementation processes, measures, and timeframes tailored to local development. Between 1985 and 1994, all mainland provincial People's Congress enacted local compulsory education legislation.[21] Local legislation followed the sample format as the 1986 CEL, regulating various aspects with a single effective date. Figure 2 shows that there was geographical and temporal variations in the adoption timing, with darker shading denoting earlier effective dates. I also show the extent to which timing variations can be explained by the pre-reform levels of provincial characteristics.

My regression analysis suggests provinces that were more economically developed, possessed larger populations and had limited educational resources tended to adopt the Compulsory Education Law (CEL) earlier. The seemingly contradictory observation regarding educational resources can be rationalized by the goal-setting of the Chinese government.

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[20]There was no clear standard for "impossible," and local implementation varied.(Chen and Guo, 2022)

[21]People's Congress of Zhejiang Province passed the local legislation, "Zhejiang Province implements the Nine-Year Compulsory Education Regulations," on June 13, 1985. People's Congress of the Tibet Autonomous Region passed its local legislation, "Measures for Implementing the Compulsory Education Law of the People's Republic of China in the Tibet Autonomous Region," on February 25, 1994.

The central government established deadlines for achieving universal basic education based on economic development. The 1985 Decisions stipulated that cities and economically developed coastal regions should accomplish universal middle school education by 1990; moderately developed townships and rural regions should prioritize achieving universal primary school education and aim for universal middle school education around 1995; underdeveloped regions should strive for universal primary education, aligned with economic development and support from the country. Consequently, provinces with larger student populations but fewer teachers had an incentive to adopt the policy earlier to allow more time before the deadline to attain the specified educational goals. The estimated coefficients and detailed discussions are presented in Column 1 of Table 2 in the empirical strategy section.
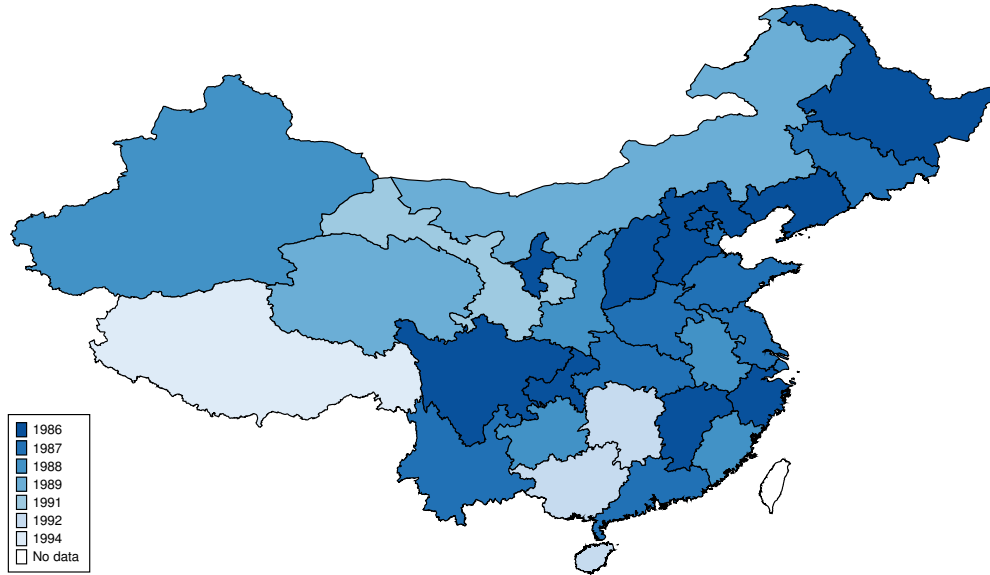

Figure 2: SSA Reform Adoption Map

Notes: The color of each province is indicative of its school year of SSA reform adoption, as detailed in the legend. A school year starts on September 1 and ends on the following August 31. For example, the school year 1985 started on September 1, 1985, and ended on August 31, 1986. Xinjiang and Tibet are included in this figure for completeness. SSA reform data data from Chen and Park (2021)

I utilize the 2010 China Family Panel Studies (CFPS) to offer suggestive evidence. A

detailed description of the data and estimation method is provided in the Appendix. I create

three five-cohort groups. Both the cohort-1969-to-1973 group and the cohort-1974-to-1978

group are considered untreated by the SSA reform because they were at least eight years

old or older when exposed to the SSA reform. The cohort-1981-to-1985 group is considered

treated because they were at most seven years old when exposed to the SSA reform. Figure

3 summarizes the estimated SSA distribution of the three groups.

Comparing the estimated SSA distribution between the cohort-1981-to-1985 and cohort-

1974-to-1978 groups, the fraction of students who started school at age eight or older is

smaller, and the fractions who started school at age seven and six are larger. When comparing

the estimated SSA distribution between the two untreated groups, there is a smaller increase

in the fraction enrolled at age six and seven, with no change in the fraction enrolled at age

eight. The differences in SSA distribution changes suggest that students altered enrollment

timing in response to the SSA reform.

A small fraction of children started school before the lowest minimum enrollment policy

age, age six, suggesting that the age rule as the minimum age is not perfectly enforced. The

increase in the fraction of people who started before six after the SSA reform suggests that

some parents use the stated school starting age as a reference and aim to enroll their children

one year earlier than the norm.

3   Data

To estimate the long-run effect of the SSA reform, my analysis requires data on the

province-level SSA reform adoption dates and data on adult outcomes. In my analysis, the
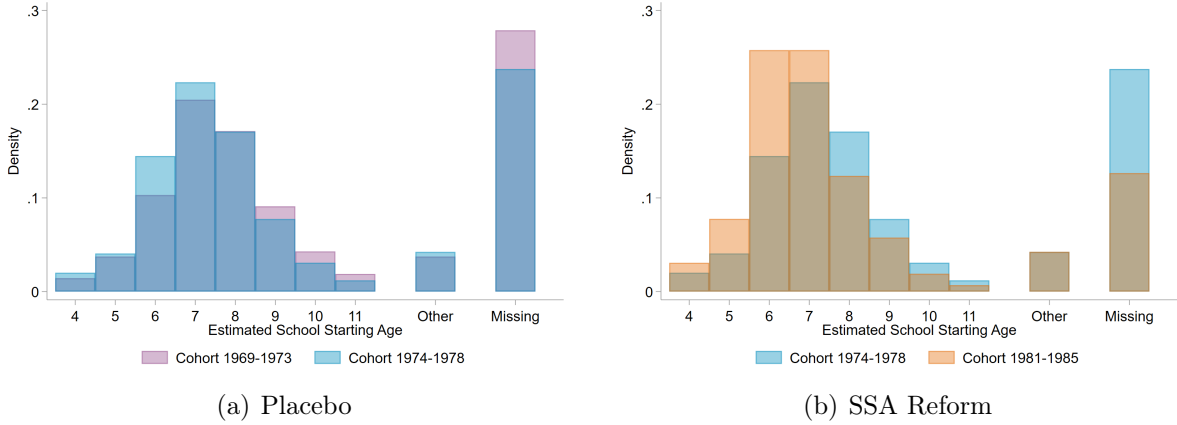
(a) Placebo	(b) SSA Reform

Figure 3: Suggestive Evidence on SSA Reform Compliance

Notes: The estimated school starting age is from the sample who completed primary school, reported non-missing values, and had estimated SSA values between 4 and 11. The school starting age is estimated using respondents' self-reported school leaving age, attainment, and duration. Construction details are discussed in Section A2. Data source: China Family Panel Studies 2010.

SSA reform adoption dates are identified as the first day of the first school year subsequent to the CEL adoption date. The CEL adoption dates are available in the official document on decisions about the Compulsory Education Law published on the provincial government websites. I rely on the adoption dates compiled and disclosed in a previous study by Chen and Park (2021).

I use the 2005 China 1% Intercensal National Population Sample Survey microdata for adult outcomes. The 2005 census comprises 2.59 million observations and collects information on demographics, educational attainment, labor market outcomes, and marriage outcomes. Demographic information includes gender, ethnicity, birth year, birth month, and hukou provinces[22]. I use the 2005 hukou province as a proxy for the province of residence at the time of the SSA reform and use it to merge in the SSA reform adoption date. [23]

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[22]"Hukou," also known as "household registration", refers to the system of household registration that ties an individual's legal residence and social services, including public schools and social safety nets, to a specific location. Its fundamental purpose is to control population movement and manage social services distribution.

[23]Province of residence in childhood is not collected by the 2005 census. I argue that the 2005 hukou

The SSA reform exposure age is estimated using birth month, birth year, and SSA reform adoption date in hukou province. The SSA reform exposure age is defined as an individual's integer age at the time of the SSA reform adoption date.

My primary outcome of interest is educational attainment. Respondents aged 6 or older at the time of the survey were asked questions about education. I use the answers to the following two questions to construct educational attainment variables: level of education and status of completion. Level of education has seven options: no schooling, primary school, middle school, high school, three-year college, four-year college, and graduate school and above. Status of completion has five: in school, graduate, incomplete, dropout, and others. I construct two categories of attainment outcomes: enrollment and completion. An individual is categorized as ever enrolled in a given level of education if an equal or higher level of education is reported, regardless of the completion status. An individual is categorized as completed a given level of education if a higher level of education is reported with any completion status or the same level with the completion status graduate.

The additional outcomes I investigate are employment and being married before the age of 20. Respondents aged 15 or above at the time of the survey were asked if they worked for income for at least an hour last week. The options include: worked, employed but did not work[24], and unemployed. I construct an employment indicator. An individual is categorized as employed if the first two options are reported. Respondents aged 15 or above at the time of the survey were asked about their marital status and the year and month of their first

province provides a good proxy for the residence in childhood because of the low inter-province migration and the rigid hukou system in China in 1980s and 1990s. The estimates using the 2010 wave of China Family Panel Studies provide supporting evidence: using a comparable sample to the main study, only five percent of individuals reported different birth provinces and 2010 hukou provinces; 0.8 percent of individuals reported different birth provinces and provinces of residence at age 3.

[24]For reasons including vacation, paid time off, training, etc.

marriage. I construct the age of the first marriage using birth year, birth month, and the year and month of the first marriage. If the age of the first marriage is strictly less than 20, an individual is categorized as being married before age 20.

I limit the sample to individuals whose dates of birth are between September 1, 1974, and August 31, 1985, to ensure that my sample is free from the effect of the Cultural Revolution and all individuals were at least age 20 at the time of the survey. I exclude all individuals who were exposed to the SSA reform after age 15 to ensure all observations in my sample are already under the treatment of other CEL policies. This restriction is important to isolate the SSA reform effect from the rest of the CEL bundle. Additionally, I exclude individuals whose hukou are registered in Xinjiang or Tibet, as ethnic-minority concentrated regions may not be comparable to other provinces and are allowed to diverge from national requirements(MDG Achievement Fund in China , UN).

I assign SSA reform treatment according to the SSA reform exposure age. An individual is considered treated by the SSA reform if the SSA reform exposure age is seven or younger and considered untreated if the exposure age is age eight or above. I set age eight as the last untreated age for two reasons. First, age seven was explicitly mentioned by the law as an option for reform implementation for some regions, so the untreated age should be at least age eight. Second, most children were already enrolled in primary school by age eight before the SSA reform[25], suggesting the SSA reform unlikely affected the primary school access timing for children exposed at age eight. I present descriptive statistics of individual characteristics by SSA reform treatment in Table 1.

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[25]I estimate pre-reform primary school enrollment rate using the 1982 census. The estimated enrollment rate for children aged 8 was 72.25%, 43.81% for children aged 7, and 15.37% of the children.

As the SSA reform treatment only varies at the hukou province and birth cohort[26] level, I collapse the sample into province-cohort cells, and the cohort is used as a time dimension for estimation. The cell-level variable is the average of all individuals with the same hukou province and from the same birth cohort.

Table 1: Descriptive Statistics

	All	Pre-Reform	Post-Reform
Outcomes			
Primary school enrollment	0.976	0.971	0.983
Middle school graduation	0.806	0.772	0.848
High school enrollment	0.315	0.283	0.354
College enrollment	0.140	0.122	0.164
Employed	0.811	0.857	0.755
Married before age 20	0.083	0.093	0.072
Demographics			
Female	0.522	0.518	0.528
Ethnic minority	0.102	0.111	0.091
Age	25.800	28.224	22.771
N	408,265	226,767	181,498

Notes: Observations whose birth cohorts are between 1974 and 1985, hukou province is not Tibet or Xinjiang, exposed to CEL SSA before or at age 15 are included in the main analytical sample. Data: Census 2005. SSA reform data from Chen and Park (2021).

4 Empirical Strategy

4.1 Difference-in-Differences with Staggered Adoption

I leverage the staggered adoption of the SSA reform across provinces as a natural experiment and use a generalized difference-in-differences framework to identify the treatment

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[26]As the SSA exposure age is integer age at the SSA reform adoption date, which is also the first day of the school year, the birth cohort in my analysis is defined as individuals born within the same school year, which runs from September 1 to August 31.

effect of primary school access timing on adult outcomes.

Provinces in my sample adopted the SSA reform between 1986 and 1992. To be consistent with using cohort as the time dimension, from now on, I define the treatment timing of a province by its first SSA reform exposed cohort[27]. All provinces with the same first SSA reform exposed cohort are considered to belong to the same treatment timing group. Figure 4 shows the distribution of SSA reform treatment status over six treatment timing groups. All provinces adopted the SSA reform.

(a) Cumulative Percentage of Provinces   (b) Number of Provinces
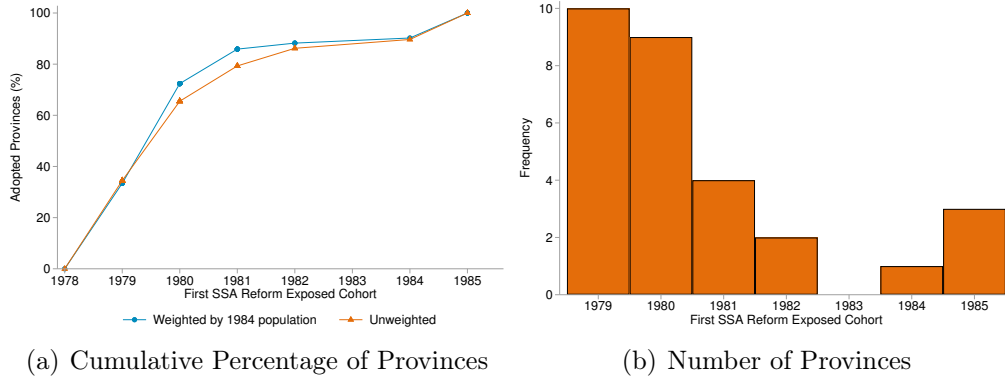
Figure 4: Distribution of SSA Reform Adoption Timing

Notes: A school year starts on September 1 and ends on the following August 31. For example, the school year 1985 started on September 1, 1985, and ended on August 31, 1986. The population data were from the 1982 census. Since Xinjiang and Tibet are excluded from the main analytical sample, they are also excluded in the figures above. Source: Chen and Park (2021)

The canonical two-group difference-in-differences (DiD) model estimates treatment effects by comparing changes in outcomes over time between a treatment group and a comparison group. In my setting with six different treatment timing groups and no never-treated group, I obtain estimates by comparing changes in outcomes over cohorts between provinces treated by the SSA reform and provinces not yet treated by the SSA reform.

The key identification assumption is that in the counterfactual where treatment had

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[27]the group of individuals exposed to the SSA reform at age seven.

not occurred, the average outcomes for all treatment timing groups would have evolved in parallel trends. Even though selection into treatment timing based on characteristics affecting the level of the outcomes is allowed, one might be concerned about selection based on characteristics affecting the trend of outcomes.

I address this concern by conducting regression analyses to investigate to what degree the pre-reform changes in province-level characteristics explain variation in adoption timing and predict adoption decisions. I choose a comprehensive set of provincial characteristics: I include population to capture the size of a province, female fraction and birth rate to capture social attitude, primary and secondary school student-teacher ratio to capture educational resources, primary industry share in provincial GDP and GDP per capita to capture economic development, and One-Child Policy fine to capture government responsiveness[28]. Overall, my finding suggests adoption timing is unlikely a strategic decision in response to pre-reform changes and therefore supports the parallel trend assumption.

I first investigate the extent to which timing variations can be explained by the pre-reform changes in provincial characteristics by estimating a province-level linear model. I regress the SSA reform adoption year on the three-year percentage changes from 1981 to 1984. Column 2 of Table 2 summarizes the estimated coefficients and statistics—the statistics of interest in adjusted R-squared. The adjusted R-squared is 0.054, meaning 5.4 percent of the timing variation is explained by the model. Despite one coefficient being statistically significant, the low adjusted R-squared suggests the variation in adoption timing cannot be explained by pre-reform changes.

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[28]One Child Policy (OCP) imposed mandated birth quotas and heavy penalties for "out-of-plan" births. The OCP was formally started in late 1979, and the implementation, such as monetary penalties for unauthorized birth, varied across provinces from 1979 to 2000.

I further investigate whether changes in a characteristic can predict adoption in subsequent years by estimating province-year-level models with both province- and year-fixed effects. I conduct separate linear regressions of adoption indicators on province characteristics from one, two, and three years prior to the adoption. The estimated coefficients are summarized in Columns 3 to 5. Taking Column 3 as an example, the coefficients should be interpreted as indicating how the change in a province's characteristics in a specific year influences the probability of adopting the Compulsory Education Law (CEL) in the next year, after accounting for overall shocks to each year and time-invariant province characteristics. Except for one, almost all estimated coefficients are not statistically significant, and the magnitudes are close to zero, suggesting that changes prior to adoption are unlikely to predict adoption decisions in the subsequent years. The high R-squared is due to the fact that most of the variability in the variables can be explained by the province and year-fixed effects.

Other identification assumptions include no anticipation and no compositional change. I estimated pre-reform placebo treatment effects to provide supporting evidence for no anticipation assumption and conducted falsification tests to provide evidence for no compositional change assumptions. These tests are discussed in detail in the results section.

4.2 Estimation and Inference

The goal of this study is to estimate the average treatment effects of the SSA reform on treated units and to examine how these average treatment effects evolve over time after the initial treatment. I estimate the dynamic treatment effects of the SSA reform using the

Table 2: Potential Predictors of the SSA Reform Adoption Timing

	SSA Reform Adoption Year		Adoption Indicator		
	1984 level	1981-to-1984 change	1-year lag	2-year lag	3-year lag
Log population	-0.616*	0.255	0.203	0.050	-0.033
	(0.351)	(0.376)	(0.390)	(0.422)	(0.423)
Percent of population female	0.084	0.649	-0.048	-0.028	-0.016
	(0.783)	(0.969)	(0.030)	(0.026)	(0.033)
Birth rate (‰)	0.063	0.021*	0.004	0.005	0.006
	(0.066)	(0.011)	(0.006)	(0.006)	(0.006)
Num of primary school students per teacher	-0.368***	0.126**	0.002	0.006	0.008*
	(0.108)	(0.060)	(0.004)	(0.005)	(0.005)
Num of secondary school students per teacher	-0.149*	-0.015	-0.002	-0.004	-0.001
	(0.076)	(0.037)	(0.004)	(0.005)	(0.005)
Percent of GRP primary industry	0.141***	-0.014	-0.001	-0.001	-0.000
	(0.045)	(0.020)	(0.002)	(0.002)	(0.002)
Per Capita GRP (yuan)	-0.001	0.042	-0.000	-0.000	-0.000*
	(0.001)	(0.032)	(0.000)	(0.000)	(0.000)
OCP fine in years of income	-1.630	-0.008	-0.003	-0.000	0.000
	(0.959)	(0.018)	(0.011)	(0.012)	(0.012)
Observations	29	29	688	686	683
R^2	0.631	0.325	0.884	0.884	0.883
Adjusted R^2	0.483	0.054	0.873	0.873	0.872
Province fixed effects			×	×	×
Year fixed effects			×	×	×

Notes: Tibet and Xinjiang are excluded following the main analytical sample. The observations in Columns 1 and 2 are provinces. Column 1 estimates are from the regression equation: $AdoptionYear_p = \mathbf{Z}_{p,1984}\beta + \epsilon_p$. The dependent variable is the first SSA reformed exposure cohort of province p. The independent variables are province characteristics levels measured at the end of 1984. Column 2 estimates are from the regression equation: $AdoptionYear_p = \Delta\mathbf{Z}_{p,1984}\beta + \epsilon_p$, where $\Delta z = z_{1984} - z_{1981}/z_{1981} * 100$. The dependent variable is the same as Column 2. The independent variables are change in province characteristics measured as percentage change between 1981 and 1984. The observations in Columns 3 to 5 are province-years between 1976 to 2000. The estimates are from the regression equation: $DummyAdopted_{p,y} = \mathbf{Z}_{p,y-n}\beta + \upsilon_y + \lambda_p + \epsilon_{p,y}, n = 1, 2, 3$. The dependent variable is a dummy variable that takes on the value 1 if the SSA reform was adopted in year y by province p. The independent variables are the province characteristics level n year prior to year y. Standard errors are in parenthesis. Standard errors in columns 3-5 are clustered on the province level. Statistical significance is denoted by asterisks: * p < 0.1, ** p < 0.05, *** p < 0.01. Data Source: China Compendium of Statistics 1949-2008. OCP fine data from Ebenstein (2010). CEL year from Chen and Park (2021)

DiD estimators for staggered adoption outlined in Callaway and Sant'Anna (2021).

First, I estimate all group-cohort-specific treatment effects on the treated (ATT) for $k \geq g$ using the following estimator:

$$\widehat{ATT}(g,k) = \frac{1}{N_g} \sum_{j:G_j=g} [Y_{j,k} - Y_{j,g-1}] - \frac{1}{N_{\mathcal{G}_{comp}}} \sum_{j:G_j \in \mathcal{G}_{comp}} [Y_{j,k} - Y_{j,g-1}] \tag{1}$$

$Y_{j,k}$ is the fraction attained a certain level of educational outcome of interest from birth cohort k. A cohort k consists of all individuals whose birthdays fall between September 1, year $k-1$ and August 31, year k. Treatment timing $G_j = g$ for a province j is defined by the first SSA reform exposed cohort. Group g consists of all provinces with the same treatment timing g. Each $\widehat{ATT}(g,k)$ is estimated by comparing the cohort of interest k and the last unaffected cohort $g-1$ between the group of interest g and the comparison group. The comparison group $\mathcal{G}_{comp}$ consists of all not-yet-treated groups g' whose first SSA reform exposed cohorts were younger than cohort k, $\mathcal{G}_{comp} = \{g' : g' < k\}$. Each province-cohort cell is weighted according to its population. Intuitively, this estimator can be viewed as first partitioning data into two-cohort/two-group subsamples and estimating the treatment effects using the canonical difference-in-differences method. As the last unaffected cohorts are those exposed to the SSA reform at age eight, the estimates can be interpreted as the additional change in outcomes relative to those exposed to the SSA reform at age eight in a treatment timing group in comparison with the not-yet-treated group.

The pre-treatment $(k < g)$ pseudo-ATTs are estimated using the first-difference method:

$$\text{pseudo-}\widehat{ATT}(g,k) = \frac{1}{N_g} \sum_{j:G_j=g} [Y_{j,k} - Y_{j,k-1}] - \frac{1}{N_{\mathcal{G}_{comp}}} \sum_{i:G_i \in \mathcal{G}_{comp}} [Y_{j,k} - Y_{j,k-1}] \quad (2)$$

The pseudo-ATT estimates provide intuition for the placebo effects. They can be interpreted as the additional change in outcomes relative to the cohort exposed to the SSA one year early in a treatment timing group in comparison with the not-yet-treated group.

I aggregate the cohort-group-specific ATT estimates into event-study estimates $\widehat{\theta}_{es}(e)$, where $e = k - g$. The event time e for any given cohort k is the relative number of cohorts since the first SSA reform exposed cohort of the group g. The aggregated event-study estimates $\widehat{\theta}_{es}(e)$ is the weighted average of group-cohort-specific ATT estimates across all groups g that are ever observed to have participated in the treatment for exactly e periods:

$$\widehat{\theta}_{es}(e) = \sum_{g=1979}^{1984} \hat{\omega}_{g,e} \widehat{ATT}(g, g+e) \quad (3)$$

The weight $\hat{\omega}_{g,e}$ associated with each group-cohort-specific ATT is the treated share from group g observed in event time e. Note that the last treatment timing group $(g = 1985)$ only functions as a comparison for the earlier treatment timing group, and no group-cohort-specific ATT is estimated for this treatment timing group. Therefore, the aggregation includes group-cohort-specific ATT estimates for groups 1979 to 1984.

The event-study estimates are my primary estimate of interest. As the first SSA reform exposed cohort had an exposure age of seven, the event-study estimates, $\widehat{\theta}_{es}(e)$, represent the weighted average of group-cohort-specific ATT estimates from the cohorts with exposure age

$7 - e$. I interpret them as the dynamic treatment effect e-periods after the initial exposure. For example, event time three estimates, $\widehat{\theta}_{es}(3)$, are the average group-cohort-specific ATTs of all cohorts exposed to the SSA reform at age four. I interpret the estimate as the treatment effect of the SSA reform three years after the initial adoption.

The standard errors are estimated using the Wild bootstrap procedure clustered at the province level. Wild bootstrap procedure multiplies randomly drawn scalar with the estimated influence function to obtain bootstrapped estimates. With province-level clustering, a scalar is drawn for a province. I report 95 percent "sup-t" simultaneous confidence bands for the path of the event-study-type estimates in addition to 95 percent point-wise confidence intervals. Simultaneous confidence bands account for the multiple-testing problem and make sensible comparisons of estimates at different event times (Montiel Olea and Plagborg-Møller, 2019). The "sup-t" critical values are estimated using the quantile-based bootstrap method (Callaway and Sant'Anna, 2021).

This method is chosen over the two-way fixed effect (TWFE) regression method for the following three reasons.[29] First, this method is robust to heterogeneous treatment profiles across different treatment-timing groups. On the other hand, the TWFE event study estimates may be biased due to "cross-lag contamination" and "negative-weighting" problems (Sun and Abraham, 2021) if the treatment effects of different treatment-timing groups evolve following different paths. Allowing for heterogeneous treatment paths is important in my setting because late-adopting provinces likely took a shorter time to fully realize the SSA reform treatment effect compared to early-adopting provinces. Education practitioners and government officials could learn from early adaptors through nationwide primary and

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[29]Detailed discussion about the TWFE regression specification is included in the Appendix.

secondary school principal training and educational research institutes.[30] Students and parents in late-adopting provinces accumulated more knowledge prior to SSA reform exposure through the Ministry of Education's CEL advocacy events.[31] Second, the magnitude of the aggregated "event-study" estimates is more policy-relevant, because the aggregation procedure of this method weights group-cohort-specific ATTs by size, while the TWFE regression estimates additional weight group-cohort-specific ATTs by variances. Third, this method circumvents the issue of making arbitrary choices for additional restrictions. Since there is no never-treated group in my sample, additional restrictions need to be imposed to address the multicollinearity issue when using TWFE regression. Common restrictions involve forcing equal coefficients beyond a certain event time, also known as "binned endpoints." The estimates can be very sensitive to such choices, as discussed by Miller (2023) and Schmidheiny and Siegloch (2023).

This particular method is chosen instead of other robust estimators for two reasons. First, as there are no never-treated groups and my last-treated group is small, using not-yet-treated groups as the comparison group may more a more credible comparison. Second, as the ATTs are estimated by comparing the target cohort and the last untreated cohort, the parallel trend assumption is only required for the post-treatment period. It is important because the untreated units in my setting were all treated by other CEL policies, while my treated units were additionally treated by the SSA reforms. The weaker parallel trend assumption allows the treatment effects of other CEL policies to unravel until right before the SSA reform and still yield an unbiased estimate of the SSA reform treatment effect.

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[30]Section two, number six and seven in "Key Points of Work for The Ministry of Education in 1990 (Guojia Jiaowei 1990 Nian Gongzuo Yaodian)."

[31]Section two, number two in "Key Points of Work for The Ministry of Education in 1991 (Guojia Jiaowei 1991 Nian Gongzuo Yaodian)."

5 Results

5.1 High School Enrollment

The primary outcome of interest is high school enrollment. High school education represents the initial non-compulsory phase and plays a crucial role in accessing higher education and various skilled professions. High school enrollment is a strong predictor of graduation, with 95.99 percent of the sample reporting graduation conditional on enrollment. I focus on enrollment to mitigate potential bias stemming from right-censored data, given that the youngest cohort in my sample was surveyed at age 20 and might be too young to have completed high school.[32]

Figure 5 presents event-study estimates of the SSA reform on high school enrollment. The consistently positive post-reform point estimates suggest an increased high school enrollment in response to SSA reform, although the estimates are imprecise with several confidence bands covering zero. The close-to-zero and statistically insignificant pre-reform estimates support the assumptions of no anticipation and parallel trends. Notably, the estimate for event time three exhibits a meaningful magnitude, and its simultaneous confidence band is clearly above zero. The coefficient shows that compared to cohorts exposed to the SSA reform at age eight, the cohort exposed to the SSA reform at age four in the treated provinces had, on average, a 0.05217 higher average fraction enrolled in high school. The estimate suggests that three years after the initial SSA reform adoption, high school enrollment increased by 5.217 percentage points, corresponding to 18.4 percent of the pre-reform average

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[32]In the case of right-censored outcomes, early adoption groups are more likely to be observed in the data, as the SSA reform may shift the entire timeline forward.

high school enrollment.

The increasing magnitude of the post-treatment point estimates suggests a rise in effect sizes for the first four years after the initial adoption. This pattern may be explained by the following reasons. First, after the SSA reform was adopted by the government, it likely took time for some parents to learn about the new requirements of local schools and enroll their children accordingly. The process of parent learning and potentially delayed reactions implied an increased compliance rate, which may explain the rising pattern in the initial years. Second, for teachers in some schools, it was their first time having students as young as six or seven years old in class. The original teaching methods may not have suited all children who were newly granted access to primary school education. As it might have taken the teachers a few years to adapt their pedagogical methods, this implies smaller initial benefits from schooling and a gradual increase in benefits through teacher adjustment.

In order to help build intuition about the magnitude of the estimates, I provide a few benchmarks. To simplify the comparison, I consider the magnitude of event time three estimates as the size of the SSA reform treatment effect. With a 95.99% conditional high school completion rate and a typical duration of high school education being 3 years, the estimated effect translates to a 5.01 percentage points increase in high school graduation and at least 0.15 years of increase in completed schooling. First, I compare my estimates to the impact of early childhood education (ECE) programs in the United States. In the meta-analysis conducted by McCoy et al. (2017), participation in classroom-based ECE programs for children under five, such as Head Start, is linked to an 11.41 percentage points increase in high school graduation. The estimated effect of the SSA reform is 44 percent of the impact observed in ECE programs. Second, I benchmark my estimates against the effect of early

childhood economic resources in the US. Analysis by Duncan et al. (1998) suggests that an additional \$10,000 of annual income from prenatal to age 5 is associated with 0.15 additional years of schooling. My estimates indicate that the years of schooling effect of SSA reform exposure is comparable to the impact of an additional \$10,000 of annual income. Last, I compare my estimates to the total effect of the Compulsory Education Law in China. The seminal paper by Fang et al. (2012) finds the CEL increased years of schooling by about 0.8 years. My estimates suggest the SSA reform explains about 18 percent of the total increase in educational attainment.
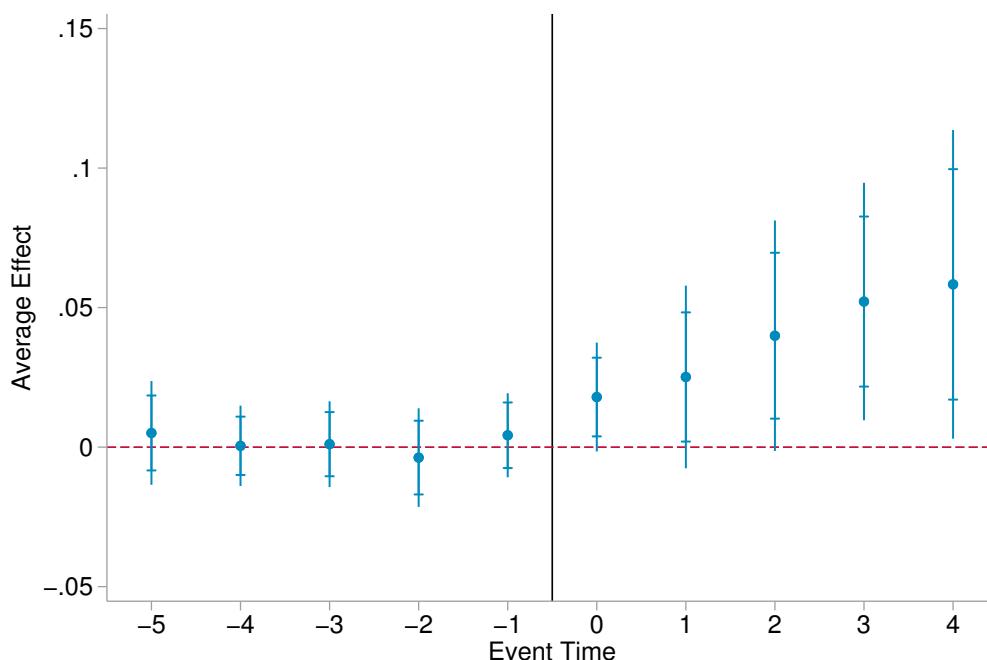
Figure 5: High School Enrollment

Notes: The sample include 29 provinces. The dependent variable is the fraction of observations enrolled in high school in province j and cohort k. The average high school enrollment for the last cohort before SSA reform was 0.325. The figure plots estimates of the average treatment effect on the treated using Callaway and Sant'Anna (2021), and the comparison groups are provinces not yet treated by the SSA reform. Pre-reform placebo average treatment effects on the treated are estimated using the first-difference method. Each province-cohort cell is weighted according to its size. Standard errors are estimated using the Wild bootstrap clustered at the provinces. 95 % sup-t simultaneous confidence bands are plotted using spikes. 95% pointwise confidence intervals are plotted using capped spikes (I-beams).

5.2 Ruling out Alternative Explanations

The evidence presented shows changes in outcomes only occur after the SSA reform. However, given the profound social changes that occurred in the 1980s and 1990s, a number of threats to a causal interpretation remain. Figure 6 summarizes robustness checks to rule out potential alternative explanations. Additional checks using alternative specifications are shown in Figure A3.

Explicit age rule effect –The SSA reform explicitly specified a school starting age. A potential alternative explanation is that parents used to fail to enroll their children because they didn't know when they should send their children to school. Now, with an explicit age rule, they had a reference point and were more likely to enroll their children. The observed increased educational attainment might be driven by the increase in overall enrollment as a result of having an explicit age rule. I investigate this channel by estimating the average effects on primary school enrollment. The lack of detectable effects on enrollment indicates that making the age rule explicit is unlikely to explain the increased educational attainment.

Strengthened enforcement of other CEL policies –As the SSA reform is additional to other CEL policies, one might be concerned that strengthened enforcement of other CEL policies in the later years is driving the results. As other CEL policies targeted compulsory schooling completion, if there were effective improved enforcement, an increased middle school completion rate would be observed. I investigated this channel by estimating the average effects on middle school completion. No detectable effects on middle school completion indicate that strengthening enforcement of other CEL policies is unlikely to explain the increased educational attainment.

Primary School Curriculum Reform.–A policy that extended the duration of primary school from 5 to 6 years rolled out over the country between 1981 to 2005. Using province and prefecture gazetteer records of curricula records, Eble and Hu (2019) find that the duration policy had no overall effect on schooling. Therefore, the curriculum reform itself is unlikely to be driving the increased educational attainment. I additionally investigate the exposure to curriculum reform as a response to the SSA reform to check for potential compositional change. No detectable effects are found, suggesting a limited compositional change in prescribed compulsory schooling duration.[33]

One Child Policy.–China's One Child Policy (OCP) imposed mandated birth quotas and heavy penalties for "out-of-plan" births. The OCP was formally started in late 1979, and the implementation, such as monetary penalties for unauthorized birth, varied across provinces from 1979 to 2000. The OCP might affect education attainments by reducing the number of siblings. I address this issue by estimating Model 1 with the birth-year OCP fine, measured in years of income, as the outcome variables. Even though the estimate is not statistically different from zero, the size of the point estimate (.082 years of income) may be concerning. Benchmarking the potential fertility effect associated with the fine according to McElroy and Yang (2000), an increasing fine by eight percent of annual income reduce the total number of birth per woman by 0.03. Given the size of the estimated fertility, the One Child Policy fine changes unlikely drivers the increased educational attainments.

Higher Education Expansion.–China's college enrollment drastically increased between 1999 to 2008 due to the expansion of university spots. As the early treated cohorts of college expansion may overlap with the youngest cohorts, one may be worried that the increased

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[33]I thank Alex Eble for sharing the data and code.

availability of higher education opportunities provides incentives for students to enroll in high school. Ou and Hou (2019) found that expanding university spots did not affect the likelihood of graduating from high school. In addition, I estimate a specification including cohorts who were at least age 16 at the time of initial expansion in 1999 and thus had enrolled in high school before the expansion. The estimate is robust to excluding younger cohorts. Therefore, college expansion is less likely to be a concern for my setting.

Compositional changes –No compositional change over cohorts is a key assumption of using cross-section for DiD estimates. I conduct falsification tests by estimating the change in the fraction of females, ethnic minorities, and birth months after the September 1 cutoff in response to SSA reform. As demographics are time-invariant, any observed effect indicates the presence of compositional change. The overall precise zero estimates suggest no detectable compositional change in the sample.

Alternative specifications –As each cell is weighted according to its population during estimation, there might be a concern that changes in population across cohorts are driving the results. To address this concern, I replace the weight of each cell with the pre-reform population of its corresponding provinces, using the 1984 population data from the National Bureau of Statistics of China. The estimates remain robust to this alternative weighting method. Additionally, as some treatment timing groups are not observed at later event times, there might be a concern that the change in the composition of groups is driving the results. This concern is addressed by estimating the aggregated event-study estimates only using balanced groups that are observed at least four cohorts post-treatment. The estimates remain robust when using only balanced groups.
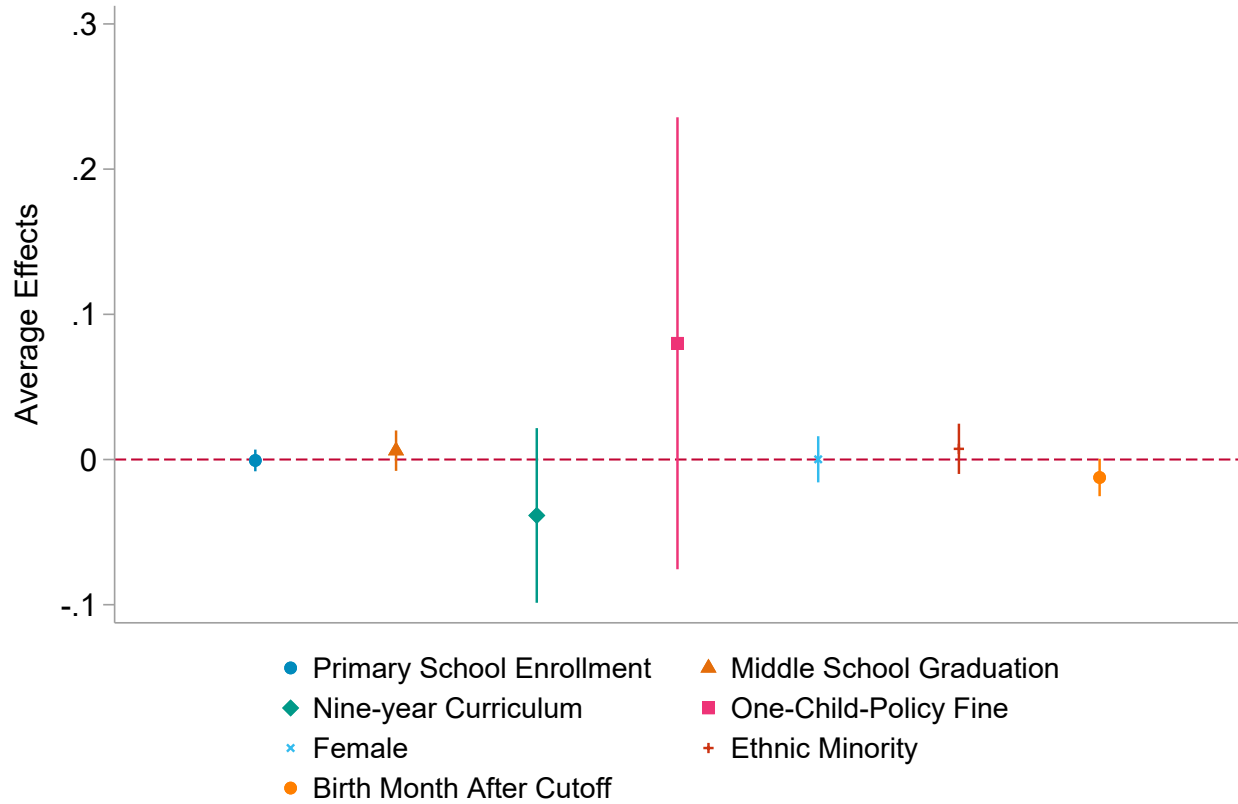
Figure 6: Robustness Checks

Notes: The sample include 29 provinces. The dependent variable is the fraction of observations enrolled in high school in province j and cohort k. The figure plots estimates of the simple average treatment effect on the treated over event time 0 to 4 using Callaway and Sant'Anna (2021), and the comparison groups are provinces not yet treated by the SSA reform. Pre-reform placebo average treatment effects on the treated are estimated using the first-difference method. Each province-cohort cell is weighted according to its size. Standard errors are estimated using the Wild bootstrap clustered at the provinces. 95 % sup-t simultaneous confidence bands are plotted using spikes.

5.3 Heterogeneity and Mechanisms

Theories identify two main channels whereby school starting age may affect adult educational attainment: the dynamic effect of initial development disparities ("developmental theory") and the lower opportunity cost of additional years of schooling ("opportunity cost theory"). I conduct heterogeneity analyses by pre-reform preschool access and by compulsory schooling duration and use differences between subsamples to find supportive evidence for potential mechanisms. The estimates are summarizes in Figure 7. The estimates imply that (i) subsample from provinces with above median preschool access, measured using province-level pre-reform number of preschool classes scaled by number of newly admitted primary school students in 1984,[34] exhibit larger effects; (ii) subsample obtained nine years of compulsory schooling, identified leaving primary school after the curriculum reform[35], exhibit larger effects. The findings are mostly consistent with the developmental theory.

Developmental theory suggests that, given the nature of developmental tasks, sensitivity to change, and interactions with the environment, early childhood is a period that may be particularly sensitive to environmental conditions. The timing of initial access to education might result in disparities in the early level of brain development. These early-stage disparities might persist or widen over the years due to skill self-productivity, dynamic complementarity, and reinforcement from the environment (Cunha and Heckman, 2007), leading to differences in educational attainments in adulthood. On the other hand, the level of initial development is not necessarily linear to the age of access, and the optimal access age

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[34]The list of above-median provinces are the same using the number of enrolled preschool students.

[35]constructed using birth year, birth month, hukou province, hukou prefecture following Eble and Hu (2019)

is unclear. In particular, the early childhood development literature sees the transition to the formal learning environment as one of the most crucial changes in early childhood. As children are expected to work independently, complete learning tasks, follow a strict class routine, and acquire literacy and mathematics skills (Li-Grining et al., 2010), an overly early transition to a formal learning environment may adversely affect achievement for children with insufficient school readiness (Duncan et al., 2007; Bruwer et al., 2014).

As preschool access can be viewed as a form of for school readiness, the difference between provinces with above or below median preschool access shown in Figure 7(a) is consistent with the prediction by developmental theory and school readiness. For students with sufficient school readiness, early school access stimulates early brain development, and the initial advantages persist into adulthood. Students with less sufficient school readiness do not accumulate early advantage as a result of early school access and thus show no effect in adult outcomes. The observed heterogeneity may also be explained by higher compliance among preschool-enrolled children or better overall educational and economic resources in provinces with better preschool access. Any explanation implies the SSA reform benefits the children already with advantages and increases educational inequality.

On the other hand, as the opportunity cost of an additional year of education increases with age and jumps at the legal minimum employment age of 16, the opportunity cost theory predicts that individuals with a younger compulsory school completion age exhibit a larger effect size. However, the difference between individuals with nine years and eight years of compulsory schooling, as shown in Figure 7(b), is inconsistent with this prediction. A potential explanation is that high school admissions are a selective process based on strong performance at the Senior High School Entrance Examination (*"Zhong Kao"*), an annual

academic exam to distinguish middle school graduates in the last year of middle school. Students with a shorter prescribed compulsory schooling duration were also younger at the time of the high-stakes exams. The literature has shown that age at the test has a strong positive effect on test scores (Black et al., 2011), and lower performance may discourage high school enrollment. The observed heterogeneity may be explained by the fact that the negative age-at-the-test effect outweighed the opportunity cost effect.

5.4 Additional Outcomes

Figure 8 presents suggestive evidence on additional outcomes, implying the effect of SSA reform might persist into later adulthood. The estimates suggest increased higher education participation, delayed labor market entrance, and delayed timing of the first marriage in response to the SSA reform. However, as my sample was aged 20 to 32 at the time of the survey, it was too early to draw conclusions regarding these outcomes. Future research using more recent data is needed for more concrete evidence.

Figure 8(a) summarizes the event-study estimates on college enrollment. Consistently positive post-treatment estimates indicate increased college enrollment in response to the SSA reform. A similar increasing pattern, seen in high school enrollment, continues to be evident in the college stage. Compared to the last cohort unaffected by the SSA reform, the average college enrollment for cohorts three years after the initial adoption increased by 5.065 percentage points. However, caution is warranted in interpreting the observed effects as fully driven by the SSA reform, as almost all SSA reform-treated cohorts were also affected by China's college expansion since 1999.[36] Instead, the observed substantial increase in college

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[36]The expansion drastically increased the quota on the number of newly admitted students, with the yearly

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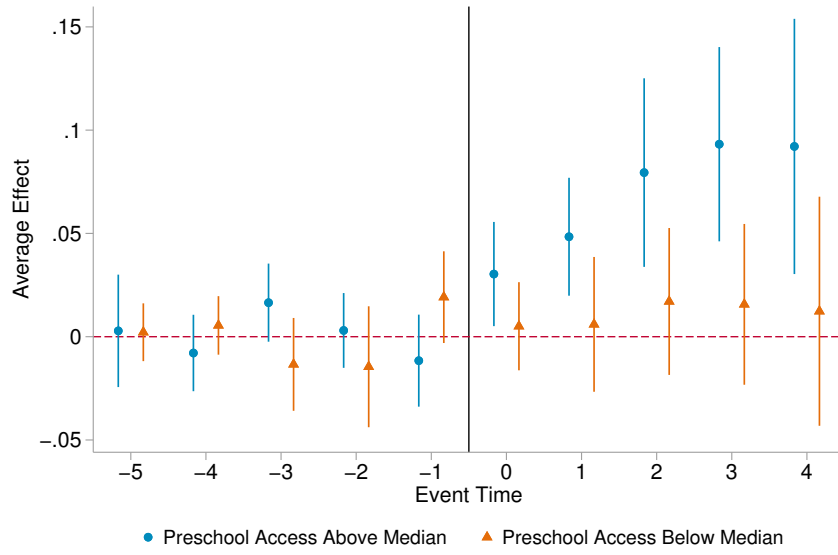
(a) By Pre-reform Preschool Access
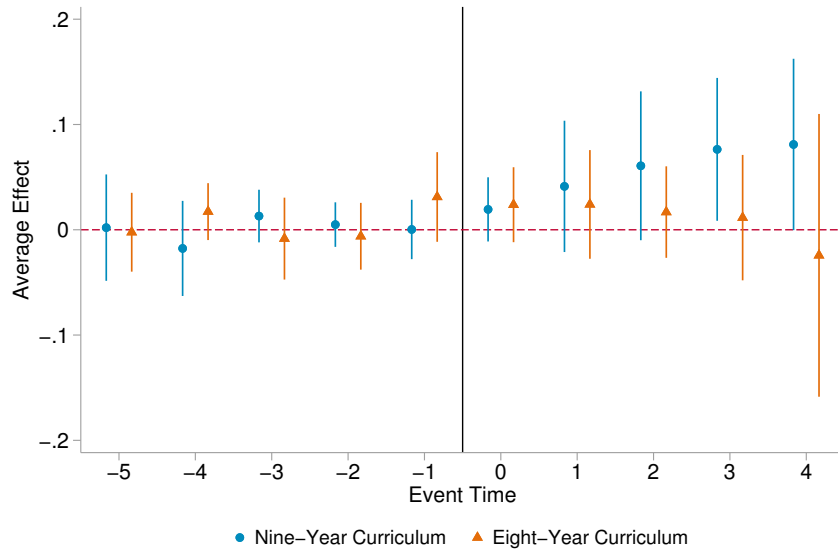
(b) By Compulsory Schooling Duration

Figure 7: Heterogeneous Effects

Notes: The sample include 29 provinces. The dependent variable is the fraction of observations enrolled in high school in province j and cohort k. The figure plots estimates of the average treatment effect on the treated using Callaway and Sant'Anna (2021), and the comparison groups are provinces not yet treated by the SSA reform. Pre-reform placebo average treatment effects on the treated are estimated using the first-difference method. Each province-cohort cell is weighted according to its size. Standard errors are estimated using the Wild bootstrap clustered at the provinces. 95 % sup-t simultaneous confidence bands are plotted using spikes. 95% pointwise confidence intervals are plotted using capped spikes (I-beams). preschool access was estimated using the 1984 province number of preschool classes scaled by the number of admitted primary school students. Curriculum duration was estimated as leaving primary school after the curriculum reform using birth month, birth year, hukou province, and hukou prefecture following Eble and Hu (2019).

enrollment should be interpreted as a significant fraction of students who enrolled in high school due to the SSA reform continuing their participation in higher education, with the expansion of college spots.

Figure 8(b) summarizes the event-study estimates on employment. Given that the sample was observed at various young ages when the sign of the employment effect is unclear, I interpret the pattern of no detectable employment effect for early event time and negative effect for later event time as suggestive evidence of delayed labor market entrance. The point estimates for event time zero to two are close to zero and not statistically significant. As the majority of the sample with event time zero to two aged 23 and above at the time of the survey, I interpret these estimates as suggesting that the SSA reform has no effect on later adulthood employment. The point estimates for event times three and four are negative and statistically significant. As the cohorts observed with event times three and four aged age 23 and below, I interpret these negative estimates for later event times suggesting delayed entrance into the labor market. This delay is consistent with an increase in higher education participation and may result in the selection into skilled occupations and increased prime-age earnings.

Figure 8(c) summarizes the event-study estimates regarding the fraction of females married before the age of 20. The choice of age 20 aligns with the youngest cohort's age. I specifically focus on the female sample because, in my dataset, an average of 13.8 percent of females reported their first marriage occurring before the age of 20, in contrast to the 4.4 percent reported by males. The overall negative point estimates suggest that a lower

number of newly admitted students increasing from 1 million in 1998 to 7–8 million in the 2010s (Ma, 2020). Gross enrollment rates increased from 9.76% in 1998 to 22% in 2006 (Ou and Hou, 2019).

proportion of females got married before the age of 20 in response to the SSA reform, indicating a delay in the timing of marriage.

6   Conclusion

In this paper, I present new evidence indicating that extending access to formal schooling for children at ages six and seven can lead to a significant improvement in adult educational attainment. Specifically, I leverage the staggered adoption of a school starting age reform in China, which lowered the stated school starting age to six and seven. The treatment effects on the treated provinces are identified and estimated using the difference-in-differences procedures outlined by Callaway and Sant'Anna (2021), suitable for a staggered adoption setting. Using the not-yet-treated group as the comparison group, I find that cohorts exposed to the School Starting Age (SSA) reform at preschool ages exhibit increased high school enrollment compared to cohorts exposed to the SSA reform at an early school age. These results withstand several robustness tests.

Furthermore, I provide suggestive evidence that the effect of the SSA reform may persist into later adulthood through increased higher education participation, delayed labor market entrance, and delayed age at first marriage for women. My heterogeneity analyses suggest a larger effect for children from provinces with more pre-primary educational resources. These findings align with the developmental theory of school readiness and indicate increased inequality between children with different early childhood educational resources.
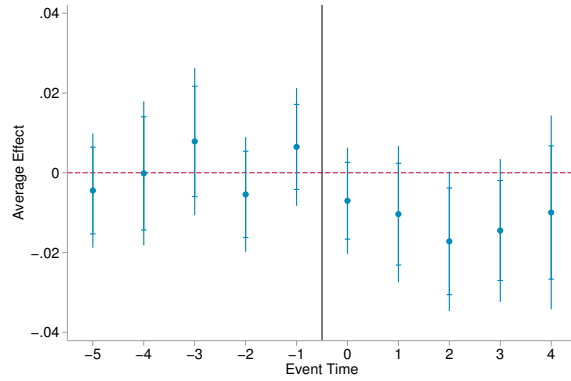
Overall, the results suggest that early access to formal schooling benefits children in the long run only if they are ready for the transition into formal schooling. This insight empha-
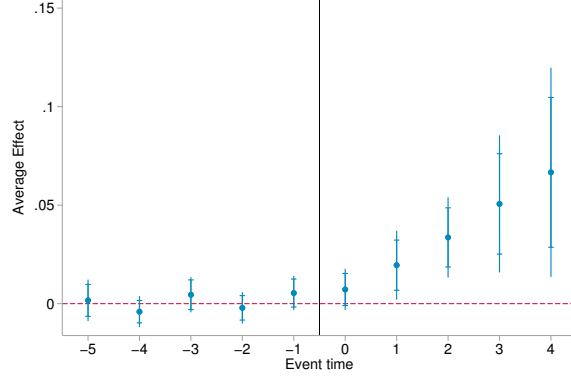
(a) College Enrollment
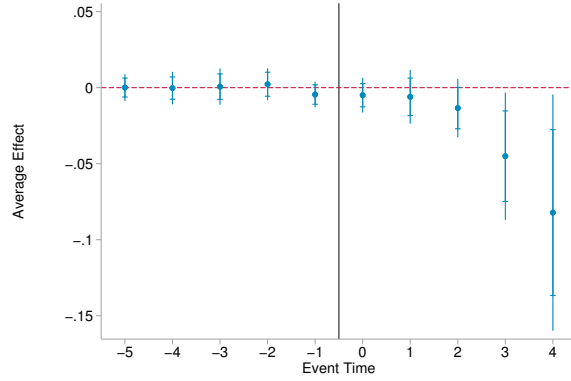
(b) Employment

(c) Women - Married before Age 20

Figure 8: Suggestive Evidence on Later Adulthood Outcomes

Notes: The sample include 29 provinces. The dependent variable is the fraction of observations reported college enrollment, employed in the last week, and first marital age strictly less than 20 in province j and cohort k. The figure plots estimates of the average treatment effect on the treated using Callaway and Sant'Anna (2021), and the comparison groups are provinces not yet treated by the SSA reform. Pre-reform placebo average treatment effects on the treated are estimated using the first-difference method. Each province-cohort cell is weighted according to its size. Standard errors are estimated using the Wild bootstrap clustered at the provinces. 95 % sup-t simultaneous confidence bands are plotted using spikes. 95% pointwise confidence intervals are plotted using capped spikes (I-beams). College enrollment and employment estimates are for the full sample. Married before age 20 estimates are for female subsample.

sizes the importance of considering developmental readiness when implementing policies to extend formal schooling access, providing valuable guidance for education policymakers.

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A1 Two-way Fixed Effects Regression Specification

I estimate the two-way fixed effects model as follows. First, I collapse individual-level cross-section data into province-cohort cells and estimate the event study model for cohort k in province j using cell-level data weighted by cell size:

$$y_{j,k} = \sigma_k + \lambda_j + \sum_{e=-5}^{6} \delta_e D_{e(j,k)} + \epsilon_{j,k}, \tag{4}$$

where $Y_{j,k}$ is 2005 self-report educational attainment in 2005. The cohort indicators, σ_k, capture cohort-varying national-level shocks, and province indicators, λ_j, absorb any time-invariant difference in outcomes between provinces. To allow for within-province correlation in unobservables, the standard errors are clustered at the province level. The number of overall clusters is 29.

The event time dummies, $D_{e(j,k)}$, indicate the age when cohort k in province j was exposed to the SSA reform relative to age 7. The event time is positive $e(j,k)$ if the treatment age was younger than age 7 and negative if older. As there is no never-treated group in my setting, I cannot estimate a fully dynamic specification because event time dummies are multicollinear with the combination of province and cohort fixed effects (see Borusyak et al., 2021 and Miller, 2023 for detailed discussions.) To make the model identifiable, in the baseline specification, I restrict the province fixed effects average to zero, the cohort fixed effects average to zero, and coefficients of event time -8 to -5 to be the same. The "binned endpoint" dummy variable $D_{-5(j,k)}$ takes value one when the treatment age is five or more years older than age 7. I choose event time -1 as the pre-event reference period by omitting

the corresponding event time dummy. Thus, the baseline coefficients of interest, δ_e, measure the changes in the outcome relative to the reference group ($e = -1$)), who were exposed to the reform at age 8.

The binning strategy is chosen to show the longest event periods without having the coefficients contaminated by the noise. Particularly, event time -5 is chosen as the beginning of the event window because all treatment timing groups have at least give pre-treatment periods. All post-treatment coefficients are shown because if I choose to bin after some post-treatment event time, but the actual treatment effect keeps evolving beyond the endpoint, the evolving effect will be picked up as a secular trend and bias all the coefficients in the model (Schmidheiny and Siegloch, 2023).

Figure A1 presents the event time coefficients on high school enrollment and their 95% confidence interval based on the dynamic TWFE model in Equation 4. The negative event time coefficients are estimated to test for parallel trend assumption, and the positive event time coefficients are estimated to study the dynamic treatment effect. The pre-treatment coefficients are neither individually nor jointly statistically significant, supporting the parallel pre-trend assumption. The post-treatment coefficients show an increasing pattern for the first four cohorts and a constant effect afterward. The average of the post-treatment event time coefficients is 0.053.

I also estimate alternative TWFE specifications imposing different restrictions, including alternative event windows, alternative estimation samples, and additional control variables. I also benchmark the TWFE estimates with the main estimates in the main text, labeled as heterogeneous robust estimates. All estimates are summarized in Figure A2.
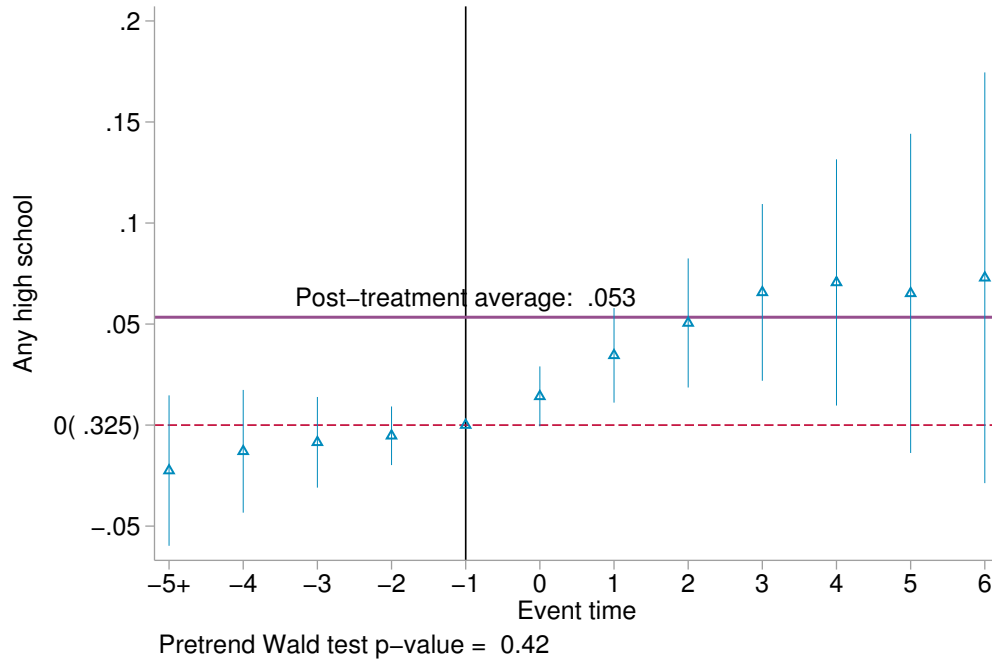
Figure A1: Estimated Effect of School Starting Age Rule on High School Enrollment

Notes: This figure shows the baseline dynamic TWFE estimates of exposure to the school starting SSA reform at different ages on the probability of completing high school. The pre-treatment coefficients from -8 and -5 are binned. The standard errors are clustered at the province level. Observations exposed to the treatment at age 8 are the reference group. The reference period average is the cell-size weighted mean high school completion rate of the reference group. Pretend Wald test p-value is the joint test statistics of if event time coefficients -5 to -2 are jointly statistically significant. The post-event average is the average of event time coefficients from 1 to 6.
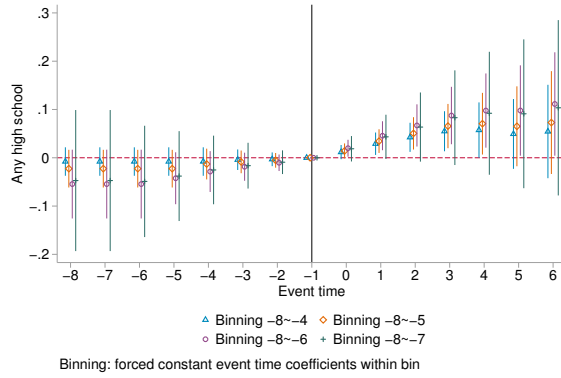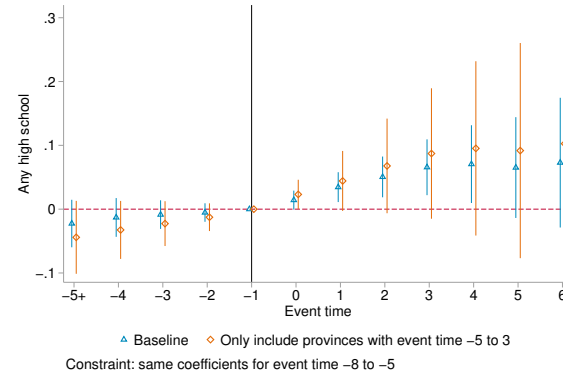
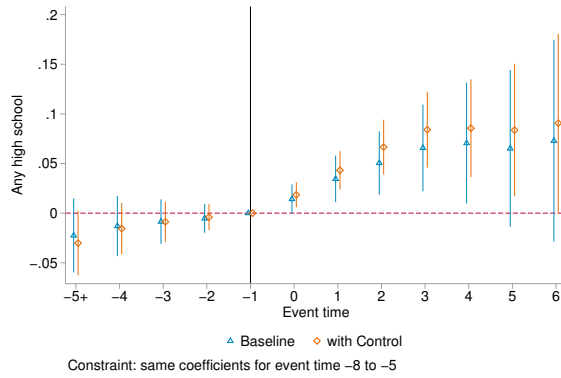(a) Various Pre-treatment binning
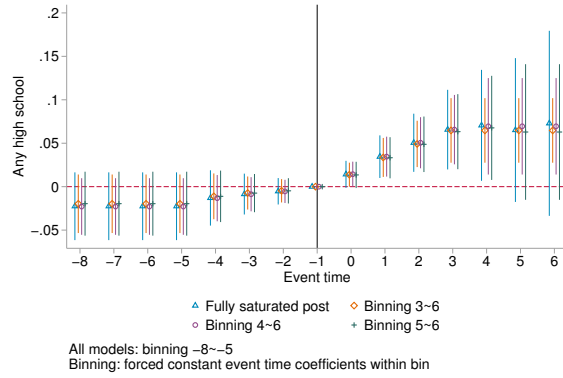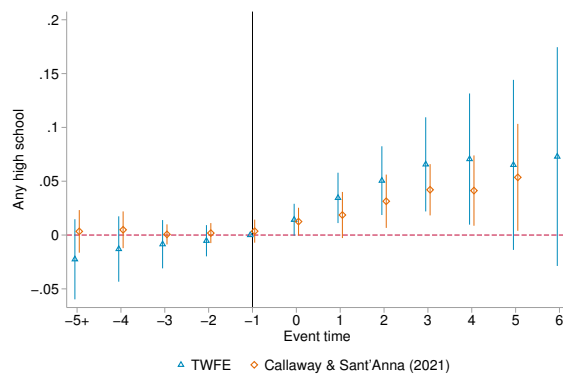
(b) Various Post-treatment binning
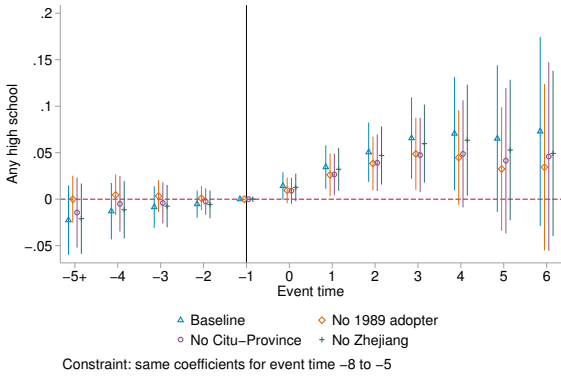
(c) Exclude special group

(d) Balanced panel

(e) Add Control

(f) Hetero-Robust Estimator

Figure A2: Sensitivity Analyses

Notes: These figures show the dynamic estimates of exposure to the school starting SSA reform at different ages on the probability of completing high school under alternative specifications.

A2 Estimated SSA Distribution

A2.1 China Family Panel Study 2010

–China Family Panel Studies (CFPS) is a nationally representative, biannual longitudinal survey of Chinese communities, families, and individuals launched in 2010. It is the Chinese counterpart of the Panel Study of Income Dynamics (PSID) in the United States. In the 2010 baseline survey, CFPS interviewed approximately 15,000 households in 25 provinces[37]. I employ the 2010 wave to estimate both the school starting age and school exit age distribution. The survey asked participants 'when graduated from/dropped out of primary school (year)?" and "duration of study in primary school (years)." I estimate individual school starting years using answers to these two questions and individual SSA using birth year and month. Previous literature has used the CFPS data to calculate school starting age (Chen and Park, 2021; Zhang, 2022). Using the same data for the same purpose helps benchmark the findings with the existing knowledge. However, the CFPS estimates suffer from recollection errors and non-reporting issues. I provide additional descriptive evidence of the first stage from another data set.

A2.2 Estimation using CFPS

I employ the 2010 CFPS to estimate the school starting age. The cohort is defined using the school year cutoff: cohort y are the observations with birth dates between September 1, $y-1$ and August 31, y, following the main analysis. I restricted the sample to individuals who

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[37]Excluding Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia, and Hainan

went to ordinary primary schools and had agricultural or non-agricultural Chinese hukou. I use cohorts 1974-1978 to show pre-reform SSA distribution because they were at least age 8 when exposed to the policy and use cohorts 1982-1985 to show the post-reform distribution because they were at most age 7 when exposed to the reform. I additionally increase the 1969-1973 cohorts to show how the SSA distribution might change without the SSA reform.

The CFPS does not directly collect age at the entrance to primary school. Instead, it asks about primary school's end year and duration: "when graduated from/dropped out of primary school (year)," "duration of study in primary school (years)," and "ever received a primary school diploma." Since I am estimating the policy effect, I am interested in the integer age by the enrollment cutoff date rather than the actual age at enrollment. Restricting the sample to people who completed primary school and reported primary school end year and duration, I estimate school starting age as follows:

$$
SSA_i = \begin{cases} EndYear_i - Dur_i - BirthYear_i \text{ if } BirthMonth_i \leq 8 \\ \\ EndYear_i - Dur_i - BirthYear_i \text{ if } BirthMonth_i \geq 9 \end{cases}
$$

I drop the observations whose estimated SSA is younger than 4 or older than 11 because they likely reflect measurement errors. Assuming the sample is representative and the self-reported end year and duration do not systematically differ from the actual value, the CFPS estimates show SSA distributions before and after the reform.

I acknowledge that the CFPS estimates may have several limitations. First, the primary school end year and duration information was collected in 2010, 13 to 25 years after the sample cohorts graduated from primary school. The self-reported information may suffer

from recollection error. Less educated people from disadvantaged backgrounds were more likely not to report or misreport the information. Second, the estimated school starting age is a function of duration. A policy that extended the duration of primary school from five to six years rolled out between 1981 and 2005 in China. One may be concerned that the duration policy drives the observed change in distribution, as people may want to start school early when expecting a more extended length in primary school. Suppose the duration change drives the change in distribution. In that case, I expect a parallel shift to the left because everyone is incentivized to start one year earlier to compensate for the longer duration. People with older starting age likely have a stronger incentive to change SSA observing a longer duration in primary school. Third, with retrospective data, the sample consists of people who survived in 2010. Literature shows that education is associated with a lower mortality rate, which means a higher chance of being observed in the 2010 survey. Mortality is less likely to be a concern because my sample was aged 35 to 36 when surveyed in 2010, while the life is between 64 and 68.
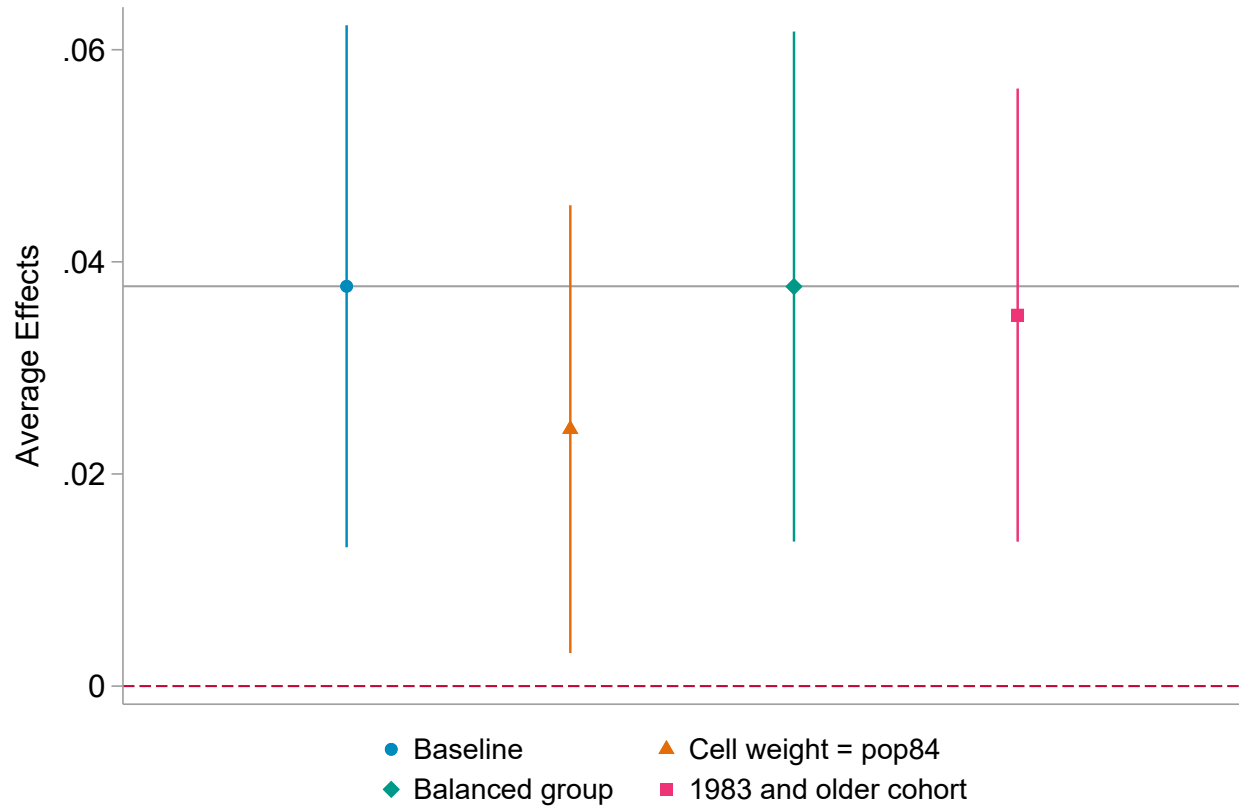
Additional Figures and Table

Figure A3: Alternative Specifications

Notes: The sample include 29 provinces. The dependent variable is the fraction of observations enrolled in high school in province j and cohort k. The figure plots estimates of the simple average treatment effect on the treated over event time 0 to 4 using Callaway and Sant'Anna (2021), and the comparison groups are provinces not yet treated by the SSA reform. Pre-reform placebo average treatment effects on the treated are estimated using the first-difference method. Each province-cohort cell is weighted according to its size. Standard errors are estimated using the Wild bootstrap clustered at the provinces. 95 % sup-t simultaneous confidence bands are plotted using spikes.

Table A1: Summary Statistics of Pre-reform Province Characteristics

| | 1984 level | | 1981-to-1984 change (%) | |
	Mean	SD	Mean	SD
Log population	7.904	(0.838)	0.595	(1.004)
Percent female	48.596	(0.505)	0.166	(0.368)
Birth rate(‰)	17.031	(4.568)	24.951	(35.797)
Num of primary school students per teacher	24.725	(3.837)	1.668	(7.055)
Num of secondary school students per teacher	15.678	(4.603)	1.099	(11.684)
Percent of GRP primary industry	33.029	(11.885)	3.276	(24.088)
Per capita GRP (yuan)	824.433	(619.283)	8.571	(14.244)
OCP fine in years of income	0.959	(0.335)	-16.973	(25.767)
Observations	29		29	

Note: Tibet and Xinjiang are excluded. Data Source: China Compendium of Statistics 1949-2008. OCP fine data from Ebenstein (2010). CEL year from Chen and Park (2021)