

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

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## **Abstract**

This paper studies the impacts of school starting age policies on educational attainment. I utilize a unique education reform in China, which lowered the age requirement for school entry while keeping the cutoff date and compulsory schooling duration constant. Unlike prior literature, I isolate the effects of starting school at a younger age from being younger than classmates. Leveraging recent advancements in difference-in-differences methodology and the staggered adoption of the reform, I provide causal estimates of the timing effects of formal schooling on adult outcomes. Contrary to the conventional wisdom that children benefit from later school entry, I find that allowing children to start school at a younger age substantially increases enrollment and graduation rates in post-compulsory education.

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# 1 Introduction

Education is an investment in future productivity for society and represents a significant portion of government expenditure (OECD, 2023). Policies governing the age at which formal schooling begins vary across countries,<sup>1</sup> influencing the timing of formal schooling. Recognizing that certain ages may play a critical role in human skill development (Knudsen et al., 2006), it is crucial to understand how the timing of formal schooling impacts long-term human capital accumulation to build a more effective education system.

In theory, it is ambiguous how the timing of formal schooling affects human capital accumulation. On the one hand, children who start school at a younger age may experience more brain development (Nelson, 2000) and foster more cognitive skills in early grades (Suziedelyte and Zhu, 2015). These early skills enhance the children’s ability to acquire later skills, leading to increased human capital accumulation (Cunha and Heckman, 2007). On the other hand, enrolling children before they are ready for the academic rigors of formal schooling may be less productive than waiting until they are more mature (Duncan et al., 2007; Pianta et al., 2007). Despite ambiguous theoretical predictions, the actions of policymakers and parents seem to believe that children benefit from later school entry.<sup>2</sup>

Empirically, there is little direct evidence on the timing effects of formal schooling. While a number of studies have estimated the effects of school starting age by comparing the

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<sup>1</sup>For example, in the Netherlands, children start primary education at the age of four, while in Finland, children start at the age of seven.

<sup>2</sup>Deming and Dynarski (2008) documents the trend toward increasing school starting age in the United States. Almost every state has increased the age at which children are allowed to start primary school. In addition, an increasing fraction of parents and teachers keep children out of primary school even when they are legally eligible to attend.

children born after the cutoff dates to the ones born before,<sup>3</sup> the estimates cannot distinguish the timing effect of starting school at a younger age from the peer effects of being younger than classmates.<sup>4</sup> A few studies directly estimate the timing effects suggest starting primary school at a younger age increases short- to medium-run test scores (DeCicca and Smith, 2013; Rosa et al., 2019; Ryu et al., 2020). It is unknown if these effects on test scores translated into increased human capital in adulthood.

To the best of my knowledge, this paper is the first to directly estimate the timing effects of formal schooling on educational attainment. I utilize a unique school starting age (SSA) reform in China that lowered the age requirement for school entry from seven to six while keeping the cutoff date and compulsory schooling duration unchanged. The reform was gradually adopted across provinces between 1985 and 1994, and by 1994 all provinces had adopted the reform. Exploiting this variation in adoption timing, I use a generalized difference-in-differences method (Callaway and Sant’Anna, 2021) to causally estimate the effects of SSA reform on educational attainment using 2005 census microdata.

Contrary to the conventional wisdom, I find exposure to the SSA reform increased the high school enrollment rate by 5.5 percentage points (19.4 percent of the pre-reform average high school enrollment rate) and increased the high school graduation rate by 5.3 percentage points (19.0 percent of the pre-reform average high school graduation rate). The magnitude

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<sup>3</sup>Recent studies using this variation find a consistent pattern that being older within a grade has various short- and medium-term advantages, such as higher test scores (e.g. Bedard and Dhuey, 2006; Dhuey et al., 2019) and higher non-cognitive skills (e.g. Dhuey and Lipscomb, 2008; Lubotsky and Kaestner, 2016) but mixed effects on adult outcomes (e.g. Black et al., 2011; Fredriksson and Öckert, 2014; Peña, 2017; Guo et al., 2023)

<sup>4</sup>For example, in California, children are eligible to enter kindergarten if they turn five years old on or before September 1. Assuming compliance, children born on September 1 enter school at the age of six, while those born on September 2 enter school at nearly age seven because of the cutoff date. Consequently, the September-2-born children start school at an older age and remain among the oldest in their grade throughout their school years.

of the SSA reform effects on years of schooling is comparable to a one-year increase in the minimum school leaving age as a result of compulsory schooling laws in the United States and Canada (Oreopoulos, 2006).<sup>5</sup> The direction of the effects is consistent with the positive effects of starting primary school at a younger age on fifth and tenth-grade test scores (DeCicca and Smith, 2013; Rosa et al., 2019), suggesting early skills translated into long-term increased human capital.

The heterogeneity analysis finds more pronounced effects in provinces with more preschool education resources, consistent with the persistent test score effects of early primary school for students with preschool access (Ryu et al., 2020). Unlike many studies,<sup>6</sup> I find the effects have little difference by gender or birth month. Overall, the findings are consistent with the channel of critical timing of development. In particular, starting primary school at a younger age enhances early skills formation, which in turn fosters long-term human capital accumulation through dynamic complementarity (Cunha and Heckman, 2007).

This paper contributes to the school starting age literature in two important ways. First, this paper provides new evidence to the sparse literature that directly evaluation the overall impact of school starting age policy impacts.<sup>7</sup> Policymakers can influence school starting age by changing the cutoff dates<sup>8</sup> or changing age requirements for school entry. This paper

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<sup>5</sup>Oreopoulos (2006) finds raising the minimum school leaving age by one year increases years of schooling by 0.11 years in the United States and 0.13 years in Canada. As the duration of the majority of high schools in China is three years, the years of schooling increased by at least 0.15 years with a 5.3 percentage points increase in high school graduation.

<sup>6</sup>For example, Guo et al. (2023) finds the bigger effect of being born after the cutoff on years of schooling for men in China, while Fredriksson and Öckert (2014) finds the bigger effect for women in Sweden.

<sup>7</sup>A primary distinction in the literature on school starting age is between the policy question (What is the optimal starting age for society?) and the individual decision (What is the optimal starting age for an individual given the starting age rules that exist?). Studies focusing on the policy questions evaluate the overall impacts of school starting age policy changes. Studies focusing on individual decisions estimate the effect of age differences generated by birthdays and cutoff dates. Most of the literature focuses on the individual decisions.

<sup>8</sup>For example, in 2010, California signed a legislature that changed its kindergarten cutoff date from

evaluation a policy that changed the age requirement at school entry and keep the cutoff date unchanged. Unlike the cutoff date policy changes,<sup>9</sup> the changing age requirements directly affect all children regardless of birth month and maintain the relative age within a grade given any birthday. In a setting closest to mine, [Rosa et al. \(2019\)](#) estimates a 2006 Brazilian reform that lowered the starting age in primary school from seven to six and finds the reform substantially increased fifth-grade math and reading scores. I complement the existing findings by looking at educational attainment to understand if the early positive effect translated into increased human capital in adulthood.

Second, this paper provides new insights into the long-run effects of school starting age. Prior studies using birthday variations find mixed results of school starting age on educational attainment. [Bedard and Dhuey \(2006\)](#), [Puhani and Weber \(2007\)](#), [Kawaguchi \(2011\)](#), [Fredriksson and Öckert \(2014\)](#), [Peña \(2017\)](#), and [Guo et al. \(2023\)](#) find being older have positive effects on educational attainment. [Angrist and Keueger \(1991\)](#), [Deming and Dynarski \(2008\)](#), [Dobkin and Ferreira \(2010\)](#); [Hurwitz et al. \(2015\)](#), and [Hemelt and Rosen \(2016\)](#) find negative effects. [Black et al. \(2011\)](#) find little to no effect. The reasons for finding mixed results across settings remain unclear. One possible approach to address this inconsistency is to differentiate between the timing effects and the peer effects. This paper establishes the existence of positive long-term effects associated with the early timing of

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December 2 to September 1. Before the law, children turned five by December 2 could enter kindergarten, which effectively allows children as young as four years and nine months (57 months) to enter kindergarten. After the law, the youngest eligible children are age five (60 months) at kindergarten entry. The children born between September 2 to December 2 start kindergarten one year later as a result of the law.

<sup>9</sup>Using the changes in kindergarten cutoff-date laws across states over time, research finds moving the cutoff date early in the year increases average scores ([Fletcher and Kim, 2016](#)), male earnings, but no effects on educational attainment ([Bedard and Dhuey, 2012](#)). These estimates capture both the direct effect of some children starting school at an older age and the spillover effects on other children who go to school with older peers and have different interpretations than this paper.

formal schooling. Hence, the inconclusive results are likely driven by the difference in size and direction of the peer effects across countries.

My empirical strategy also contributes to the estimation of China’s Compulsory Education Law (CEL) literature. The SSA reform is one of eighteen articles specified by the CEL. As the initial national education law, CEL 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 first document the school starting age around the time of the reform using various current and historical data sources. Then, 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.

Prior to this study, [Chen and Park \(2021\)](#) and [Zhang et al. \(2017\)](#) used the SSA reform to interact with birth month to estimate the age difference within grade effects. Both studies rely on estimated school starting age norms from survey data with a relatively small sample size to assign treatment of the SSA reform. My empirical strategy offers several advantages over previous studies. First, I assign treatment based on the stated policy, supported by an analysis of pre-reform school starting age distribution from various data sources. This approach minimizes the potential for measurement errors in treatment assignments. Second, I estimate the reduced-form policy effects instead of individual effects driven by idiosyncratic birthdays. This estimation captures the average treatment effect of gaining access to formal schooling earlier across all birthdates, making it more informative for policy analysis. Third,

by estimating the overall policy impacts, I circumvent the identification challenges associated with manipulation around the school entry cutoff date. Lastly, my paper improves upon prior estimates by incorporating recent advancements in the difference-in-differences literature, utilizing a heterogeneity-robust estimating method. This method ensures that my estimates are not susceptible to potential issues that can arise in regression estimates, such as negative weights (Goodman-Bacon, 2021), cross-contamination (Sun and Abraham, 2021), and insensible aggregation weights (Callaway and Sant’Anna, 2021).

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 describes the empirical strategy. Section 5 presents the main results. Section 6 check robustness under alternative specifications and ruling out potential confounding factors. Section 7 presents heterogeneity analysis and discusses potential mechanisms. Section 8 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<sup>10</sup> literary rate was around 65%,<sup>11</sup> which was lower than the 1982 world

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<sup>10</sup>Aged between 18 and 69, following Deng and Treiman (1997).

<sup>11</sup>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

average of 70%.<sup>12</sup> Around 33% of adults had any middle school education, fewer than 12% had any high school education, and 1% had any college education.<sup>13</sup> Pre-primary education enrollment was uncommon. The gross preschool enrollment in China was below 20% before 1985 and remained below 25% between 1985 and 1994.<sup>14</sup> There were, on average, fewer than 3 preschools per thousand preschool-age kids.<sup>15</sup>

The CEL was introduced after the upheaval of the Cultural Revolution (1966-1976).<sup>16</sup> 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).<sup>17</sup> In particular, primary and secondary curriculum and duration underwent constant reforms,<sup>18</sup> 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.<sup>19</sup> The segment between two dashed vertical lines represents the

was 83%, with the male rate of 93% and the female rate of 72%.

<sup>12</sup>Source: UNESCO Institute for Statistics (UIS). Accessed October 24, 2022. [apiportal.uis.unesco.org/bdds](https://apiportal.uis.unesco.org/bdds).

<sup>13</sup>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.

<sup>14</sup>Source: UNESCO Institute for Statistics (UIS). UIS.Stat Bulk Data Download Service. Accessed October 24, 2022. [apiportal.uis.unesco.org/bdds](https://apiportal.uis.unesco.org/bdds)

<sup>15</sup>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 Liu (1993). The number of preschool-age kids was calculated using population data and the birth rate from National Bureau of Statistics (2010).

<sup>16</sup>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.

<sup>17</sup>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.

<sup>18</sup>“Minutes of the National Education Work Conference.” China’s Communist Party Central Committee. August 13. 1971.

<sup>19</sup>A detailed discussion of the construction of the enrollment measure is provided in the data section.



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 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.

## 2.2 The School Starting 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<sup>20</sup> that regulated various aspects

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<sup>20</sup>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 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.

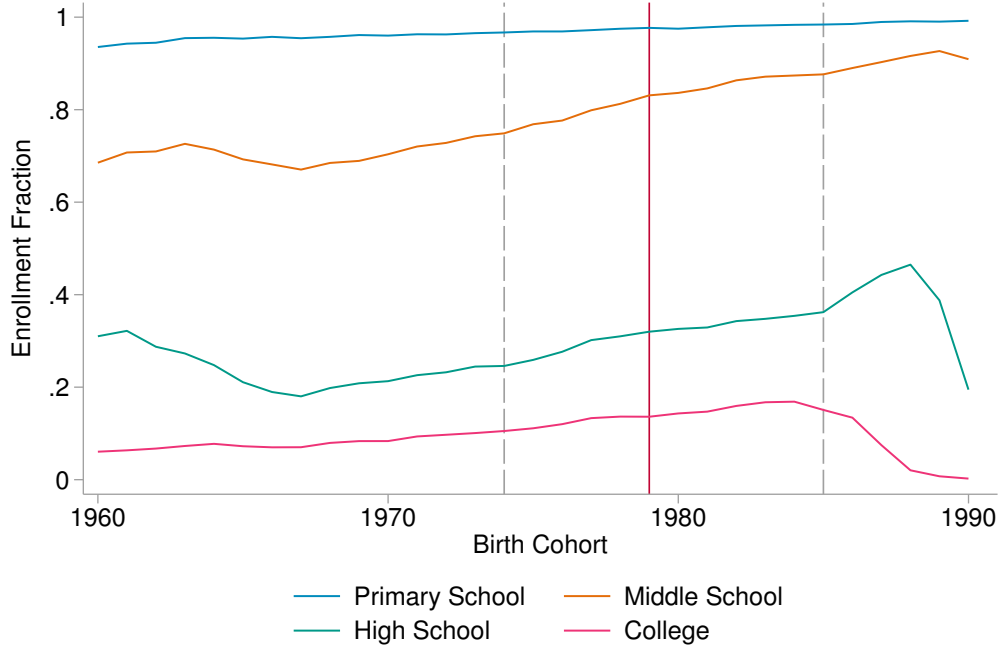


Figure 1: Trends in Educational Attainment in 2005, by Birth Cohort

*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.

of education encompassing compulsory duration,<sup>21</sup> school starting age,<sup>22</sup> infrastructure,<sup>23</sup> costs,<sup>24</sup> school finance,<sup>25</sup> teacher training.<sup>26</sup>

In this paper, I focus on the policy regulating school starting age. The previous policy

<sup>21</sup>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.

<sup>22</sup>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.”

<sup>23</sup>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.

<sup>24</sup>Article 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.”

<sup>25</sup>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.

<sup>26</sup>Article 13 specified strengthening normal schools and establishing a system to test teacher credentials.

prior to the reform specified a national school starting age dated back to 1951,<sup>27</sup> two years after the founding of the People’s Republic of China. The 1951 education policy specified age seven as the school starting age. Little is known about the school starting age during the Cultural Revolution. After the Cultural Revolution, the 1978 Plan<sup>28</sup> 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, students entering school had reported experiencing age six, age seven, or age eight as the required age for entry across the nation (Liu, 1993).

The SSA reform, Article 5 of the 1986 CEL, specified “all children who have reached the age of six should 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.”<sup>29</sup> In most cases, students were prevented from attending until they met the stated school starting age. No consequences for the children who fail 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.<sup>30</sup>

In China, the law specifies that children complete primary school and middle school education.<sup>31</sup> The SSA reform shifts the timing of compulsory schooling one year earlier

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<sup>27</sup>“Zhengwuyuan Guanyu Gaige Xuezhi de Jueding [Decisions to Reform the School System].” State Council of the Central People’s Government. October 1, 1951.

<sup>28</sup>“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.

<sup>29</sup>There was no clear standard for “impossible,” and local implementation varied (Chen and Guo, 2022).

<sup>30</sup>In the early years of the implementing the CEL, some provinces set age requirements for school entry to age seven. In such provinces, the legal school entry age was gradually shifted to six in later years (Zhang, 2022).

<sup>31</sup>In most cases, primary school consists of six grades, and middle school has three grades, making compulsory education last for nine years. A small fraction of the sample attended primary school with five grades,

without extending the duration.<sup>32</sup> Figure 2 illustrates the effects of the SSA reform on formal schooling entry and the corresponding compulsory schooling completion age.

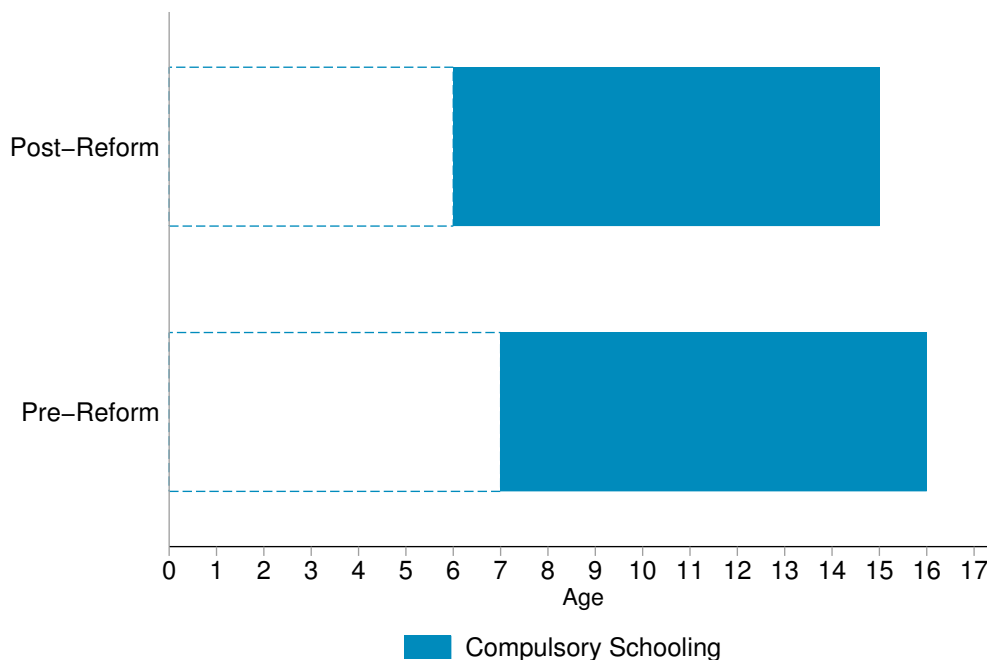


Figure 2: The Effects of SSA Reform on Compulsory Schooling Starting and Completion Ages

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.<sup>33</sup> Local legislation followed the sample format as the 1986 CEL, regulating various

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resulting in a total compulsory schooling duration of eight years. Any potential complications related to this are discussed in detail later in [Section 6](#).

<sup>32</sup>This is different from education reforms in other countries. For example, the ‘Reform 97’ in Norway lowered the school starting age from seven to six while extending the total compulsory duration from nine to ten years. Similarly, the 2006 reform in Brazil changed the age requirement from seven to six while extending the compulsory duration from eight to nine years.

<sup>33</sup>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.

aspects with a single effective date. [Figure 3](#) 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 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.<sup>34</sup> The seemingly contradictory observation regarding educational resources can be rationalized by the goal-setting of the Chinese government. 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.

As illustrated by [Figure 4](#), the SSA reform effectively changed the age of school entry for children in China.<sup>35</sup> Given the limitation of sample size, I pool the school starting age over five cohorts to estimate the distribution. The cohort-1969-to-1973 group and the cohort-

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<sup>34</sup>The estimates are summarized in [Table 2](#) and discussed in detail in [Section 4](#).

<sup>35</sup>In the main text, I present my analysis using the 2010 China Family Panel Studies (CFPS) following the literature [Chen and Park, 2021; Zhang, 2022](#)). Due to limitations of measurement errors and non-response of the retrospective survey data, I additionally employ the historical censuses and historical survey data to ensure the presence of the first stage. The detailed description of each dataset, school starting age estimation strategy, limitations, and strengths of estimates are presented in [Section A2](#).

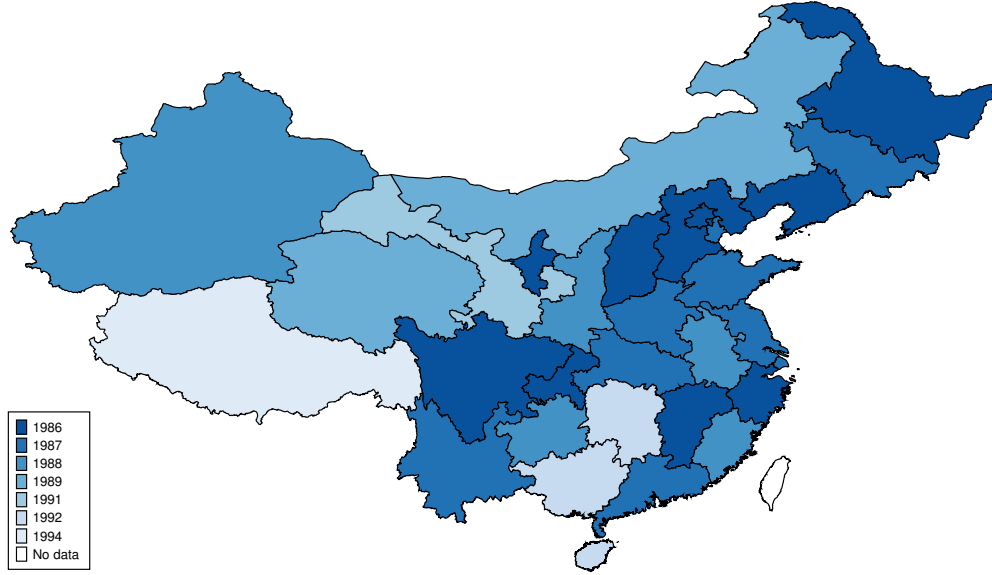


Figure 3: SSA Reform Adoption Map

*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 \(2021\)](#)

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.

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 ages 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.

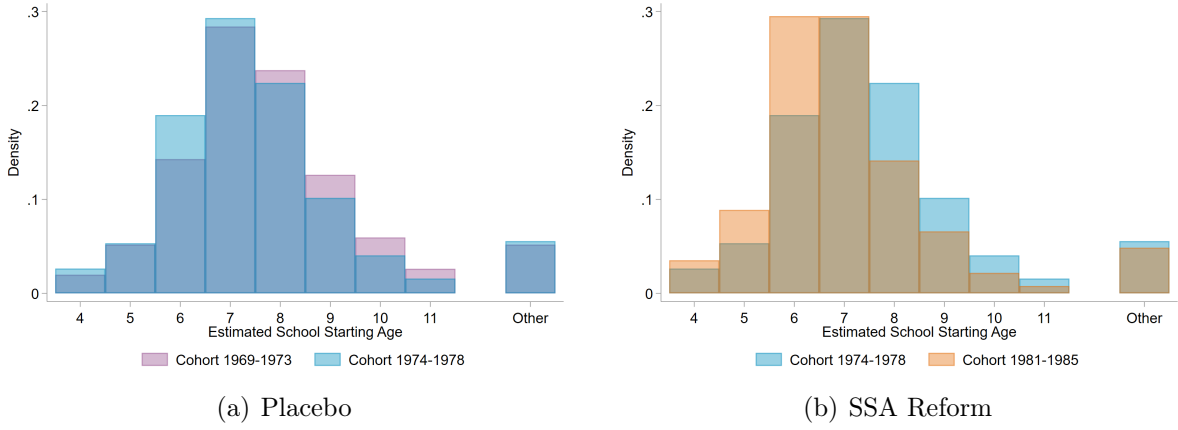


Figure 4: The Suggestive Effects of SSA Reform on Actual Age at School Entry

*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 category "Other" groups together all the observations with estimated school starting ages beyond 4 to 11. The school starting age is estimated using respondents' self-reported school leaving age, attainment, and duration:  $SSA_i = EndYear_i - Dur_i - BirthYear_i$  if  $BirthMonth_i \leq 8$ ,  $SSA_i = EndYear_i - Dur_i - BirthYear_i$  if  $BirthMonth_i \geq 9$ . Data source: China Family Panel Studies 2010.

### 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 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.<sup>36</sup> 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.<sup>37</sup> 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.

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<sup>36</sup>“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.

<sup>37</sup>Province of residence in childhood is not collected by the 2005 census. I argue that the 2005 hukou 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.



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.

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,<sup>38</sup> suggesting the SSA reform unlikely affected the primary school access timing for children exposed at age eight. I present descriptive statistics of individual

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<sup>38</sup>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.

characteristics by SSA reform treatment in [Table 1](#).

As the SSA reform treatment only varies at the hukou province and birth cohort level,<sup>39</sup> 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
<i>Outcomes</i>			
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
High school graduation	0.302	0.279	0.331
<i>Demographics</i>			
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

<sup>39</sup>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.<sup>40</sup> All provinces with the same first SSA reform exposed cohort are considered to belong to the same treatment timing group.

Figure 5 shows the distribution of SSA reform treatment status over six treatment timing groups. All provinces adopted the SSA reform.

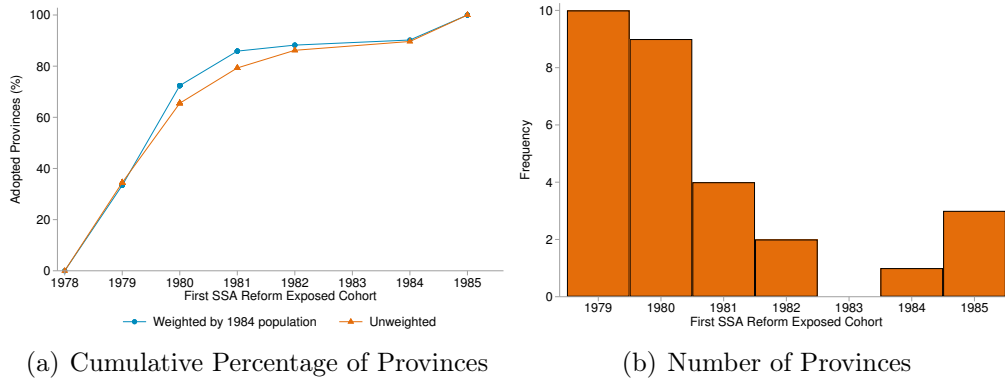


Figure 5: Distribution of SSA Reform Adoption Timing

*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

<sup>40</sup>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 analysis 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.<sup>41</sup> 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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<sup>41</sup>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

	(1)	(2)	(3)	(4)	(5)
	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* (0.351)	0.255 (0.376)	0.203 (0.390)	0.050 (0.422)	-0.033 (0.423)
Percent of population female	0.084 (0.783)	0.649 (0.969)	-0.048 (0.030)	-0.028 (0.026)	-0.016 (0.033)
Birth rate (‰)	0.063 (0.066)	0.021* (0.011)	0.004 (0.006)	0.005 (0.006)	0.006 (0.006)
Num of primary school students per teacher	-0.368*** (0.108)	0.126** (0.060)	0.002 (0.004)	0.006 (0.005)	0.008* (0.005)
Num of secondary school students per teacher	-0.149* (0.076)	-0.015 (0.037)	-0.002 (0.004)	-0.004 (0.005)	-0.001 (0.005)
Percent of GRP primary industry	0.141*** (0.045)	-0.014 (0.020)	-0.001 (0.002)	-0.001 (0.002)	-0.000 (0.002)
Per Capita GRP (yuan)	-0.001 (0.001)	0.042 (0.032)	-0.000 (0.000)	-0.000 (0.000)	-0.000* (0.000)
OCP fine in years of income	-1.630 (0.959)	-0.008 (0.018)	-0.003 (0.011)	-0.000 (0.012)	0.000 (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			Yes	Yes	Yes
Year fixed effects			Yes	Yes	Yes

*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:  $DummyAdopted_{p,y} = \mathbf{Z}_{p,y-n}\beta + v_y + \lambda_p + \epsilon_{p,y}$ ,  $n = 1, 2, 3$ . 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:

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

$Y_{j,k}$  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:

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

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,  $\hat{\theta}_{es}(3)$ , 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. To effectively compare magnitude with existing studies reporting a single parameter, I further aggregate the event-study estimates into an overall treatment effect parameter using the weighted average of event-study estimates, and the weight corresponds to the size of the treatment timing group.

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 two reasons.<sup>42</sup> First, this method is robust to heterogeneous treatment profiles across different treatment-timing groups. 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

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<sup>42</sup>Detailed discussion about the TWFE regression specification is included in the Appendix.

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 secondary school principal training and educational research institutes.<sup>43</sup> Students and parents in late-adopting provinces accumulated more knowledge prior to SSA reform exposure through the Ministry of Education’s CEL advocacy events.<sup>44</sup> 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.

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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<sup>43</sup>Section two, number six and seven in “Key Points of Work for The Ministry of Education in 1990 (Guojia Jiaowei 1990 Nian Gongzuo Yaodian).”

<sup>44</sup>Section two, number two in “Key Points of Work for The Ministry of Education in 1991 (Guojia Jiaowei 1991 Nian Gongzuo Yaodian).”

## 5 Results

I focus on high school enrollment and graduation as my primary measures of educational attainment. Compulsory schooling laws in China require the completion of primary school and middle school. High school represents the initial non-compulsory phase and plays a crucial role in accessing higher education and various skilled professions. In 2005, the average monthly income for high school graduates was 439 RMB higher than that of those who had only completed compulsory schooling. This increase corresponds to 71.6 percent of the mean income of adults aged 25 to 45 or 0.48 standard deviations. In comparison to individuals with no schooling, the financial return to holding a high school degree is four times higher than holding a middle school degree.<sup>45</sup> While in China, high school enrollment is a strong predictor of high school graduation and thus has a similar labor market return,<sup>46</sup> they potentially carry distinct interpretations. In this section, I separately discuss the estimates of the SSA reform on high school enrollment and high school graduation.

### 5.1 Main Results

[Figure 6\(a\)](#) presents estimated effects of the SSA reform on high school enrollment. The close-to-zero and statistically insignificant pre-reform estimates support the assumptions of no anticipation and parallel trends.<sup>47</sup> The consistently positive post-reform point estimates

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<sup>45</sup>Compared with individuals with no schooling, having a middle school degree is associated with an additional monthly income of 145 RMB, while a high school degree is associated with an additional monthly income of 584 RMB. For detailed estimates, refer to [Table A4](#).

<sup>46</sup>The estimated return to high school enrollment is similar to the return to high school degree due to the high conditional graduation rate (96 percent). For detailed estimates, refer to [Table A4](#).

<sup>47</sup>The pre-period effects presented in [Figure 6\(a\)](#) are estimated using the first-difference method. The conclusion does not change if using the long-difference method comparable to the traditional two-way fixed effects event study design.

suggest an increased high school enrollment in response to SSA reform.

The magnitude of point estimates increases over the first three cohorts following the initially treated cohorts and stabilizes thereafter. The estimates for event time 0 to 2, corresponding to three initially treated cohorts of each province, are not statistically different from zero. The increasing 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 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.<sup>48</sup> Third, as a result of the reform, a fraction of six-year-old children started school with the not directly affected seven-year-old children results in a bigger cohort size which might dilute the educational resources for each student. While the effects of a sudden increase in cohort size do not seem to affect the students in higher grades according to the flat pretend, it likely affected the quality of education of the directly affected cohorts.

Due to the potential increase in take-up spillover effects related to cohort size among the initially treated cohorts, the early event time estimates may not accurately reflect the

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<sup>48</sup>On the other hand, there are no known changes in curriculum design to accommodate the average younger grade cohort according to the content of textbooks.

SSA reform's treatment effect. Instead, I rely on estimates from event time three to gauge the potential effect size. When considering event times three and four, the overall estimated effect of the SSA reform suggests that cohorts fully treated by the reform experienced a 5.5 percentage point increase in high school enrollment rates, corresponding to 19.4 percent of the pre-reform average high school enrollment rate.

Figure 6(b) presents the event study estimates of the effects on high school graduation. Similar patterns are also observed in high school graduation, albeit with slightly less precision. The estimates suggest that the SSA reform increased the average high school graduation rate by 5.3 percentage points, equivalent to 19.0 percent of the pre-reform mean. The relative size between high school enrollment and graduation is consistent with the overall high school graduation rate. The persistence of these effects indicates that students who enrolled in high school due to the reform also largely graduated from high school, and there appears to be no significant difference in behavior between these students and those who would have enrolled in high school regardless of the reform.

In order to provide a better understanding of the magnitude of these estimates, I offer a few benchmarks. Since there is a limited direct evaluation of the timing effects of formal schooling, I draw insights from previous studies on compulsory schooling laws. To facilitate the comparison, I translate the estimated increase in high school graduation into an increase in years of schooling. High school education typically spans 3 years; therefore, the 5.2 percentage point increase corresponds to at least an additional 0.15 years of schooling, as some high school graduates may continue on to college. This effect size is comparable to the impact of a one-year increase in the minimum school leaving age in the United States and Canada (Oreopoulos, 2006). When comparing the total effect of the Compulsory Education

Law in China (Fan, 2008), the estimate of this paper suggests the SSA reform accounts for approximately 18 percent of the total increase in educational attainment.

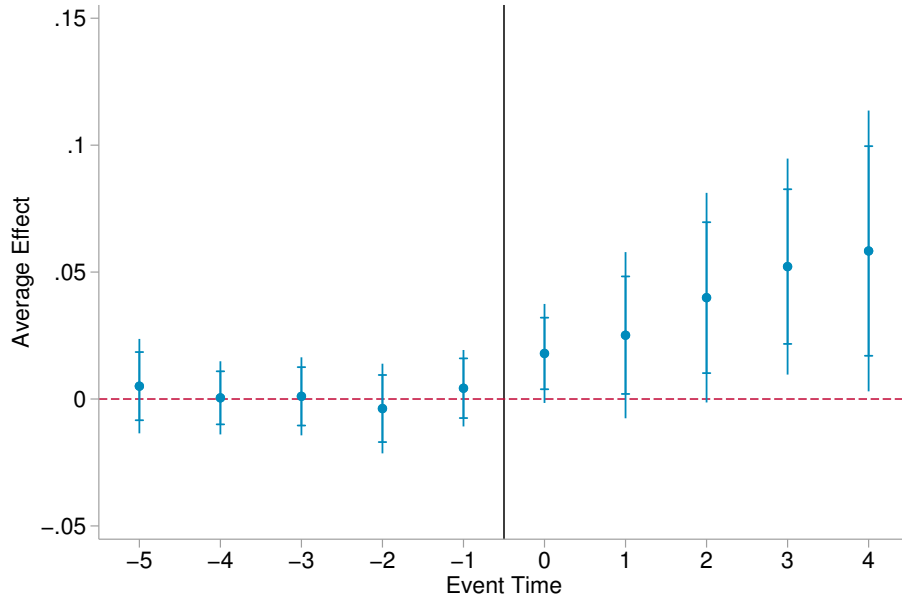
## 5.2 Alternative Specifications

Table 3 summarizes the overall effects using various specifications. Column 1 shows the overall effects of the baseline specification corresponding to the Figure 6. In the baseline specification, all province-cohort cells are weighted according to the cell size on an unbalanced panel. Columns 2 to 5 present estimates using alternative specifications. The conclusion of SSA reform increased educational attainment remains robust across all five specifications.

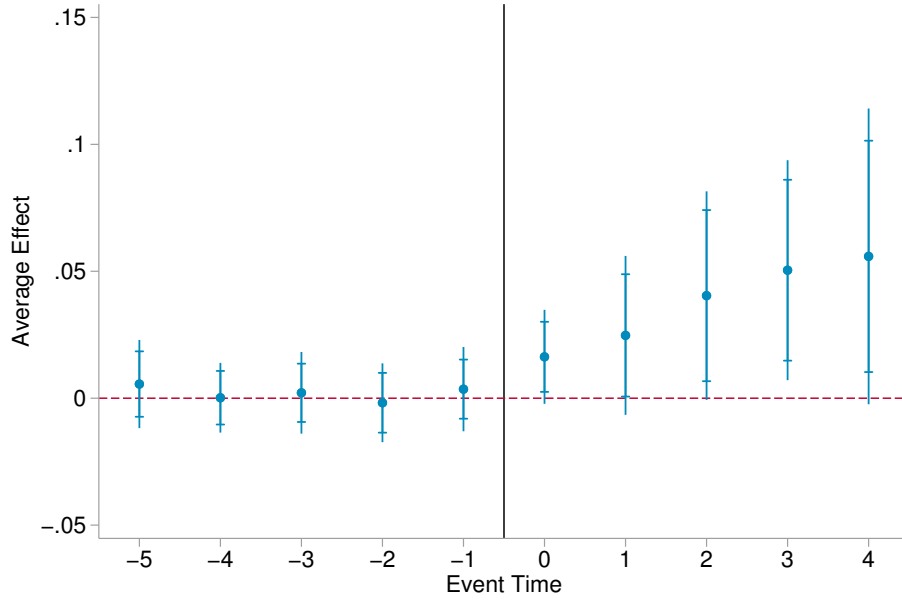
*Alternative weighting.* –As each cell is weighted according to its population during estimation, there may be a concern that changes in population across cohorts are influencing the results. To address this concern, I replace the cell weight with the pre-reform province-level population measured in 1984, as shown in Column 2, and with equal weight for each cell, as shown in Column 3. The estimates remain robust under these alternative weighting methods.

*Balanced panel.* –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 in at least four cohorts post-treatment, as shown in Column 4. The estimates remain robust when using only balanced groups.

*Excluding youngest cohorts* –As the 1985 cohorts just turned 20 at the time of the survey, some might be concerned that the youngest cohorts in the sample may be too young



(a) High School Enrollment



(b) High School Graduation

Figure 6: The Effects of SSA Reform on High School Attainment

*Notes:* The sample include 29 provinces. The pre-reform average high school enrollment was 0.283. The pre-reform average high school graduation was 0.279. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method. Standard errors are estimated using the Wild bootstrap clustered at the provinces. 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).

to graduate from high school. In this scenario, the age when the surveyed might bias the estimate. To address this concern, I use a sample with only 1983 and older cohorts, shown in Column 5. The estimates remain robust when excluding the two youngest cohorts.

Table 3: Average Treatment Effects of SSA Reform using Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: High School Enrollment</i>					
Effects of SSA Reform	0.0549*** (0.018)	0.0371*** (0.014)	0.0486*** (0.016)	0.0549*** (0.018)	0.0506*** (0.015)
Weight	Cell size	1984 population	Unweighted	Cell size	Cell size
Balanced	No	No	No	Yes	No
Cohorts	All	All	All	All	1983 and older
<i>Panel B: High School Graduation</i>					
Effects of SSA Reform	0.0529*** (0.019)	0.0325** (0.014)	0.0509*** (0.017)	0.0529*** (0.018)	0.0505*** (0.015)
Weight	Cell size	1984 population	Unweighted	Cell size	Cell size
Balanced	No	No	No	Yes	No
Cohorts	All	All	All	All	1983 and older

*Note:* Pre-reform average high school enrollment rate is 31.5 percent. Pre-reform average high school graduation rate is 30.2 percent. Comparison groups are provinces not yet treated by the SSA reform ([Callaway and Sant’Anna, 2021](#)), weighted by cell size. Effects of SSA Reform are the aggregation for event times 3 and 4. Clustered asymptotic standard errors in parenthesis. Data: Census 2005. 1984 Population data from the National Bureau of Statistics of China.

## 6 Robustness Check

Given the profound social changes that occurred in the 1980s and 1990s, there are several potential threats to a causal interpretation that need to be addressed. In this section, I present a series of robustness checks to rule out alternative explanations.

*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



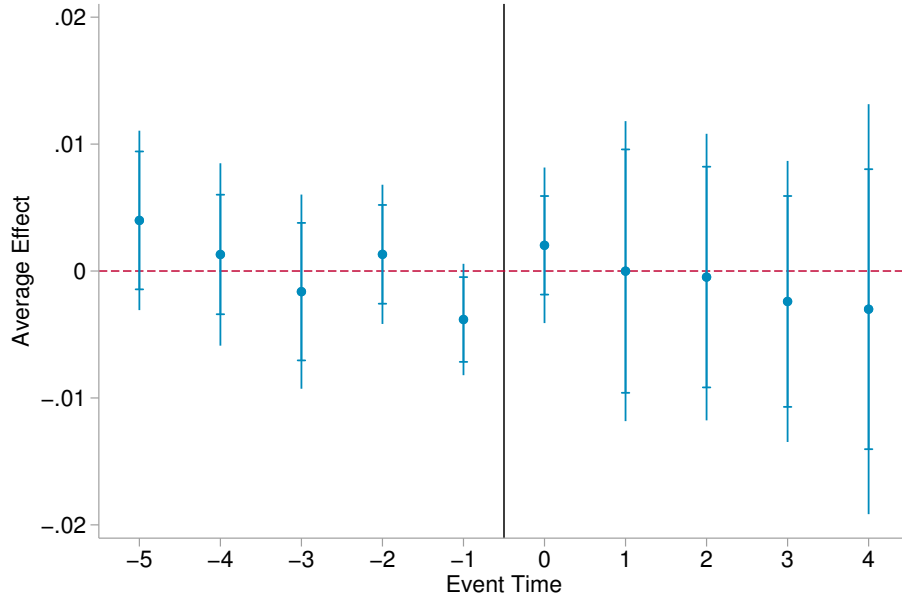
rule, they had a reference point and were more likely to enroll their children. The observed increased educational attainment might be driven by the increase in overall enrollment as a result of having an explicit age rule. I investigate this channel by estimating the average effects on primary school enrollment. As shown in [Figure 7\(a\)](#), no detectable effects on enrollment indicate 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. As shown in [Figure 7\(b\)](#), no detectable effects on middle school completion indicate that strengthening enforcement of other CEL policies is unlikely to explain the increased educational attainment.

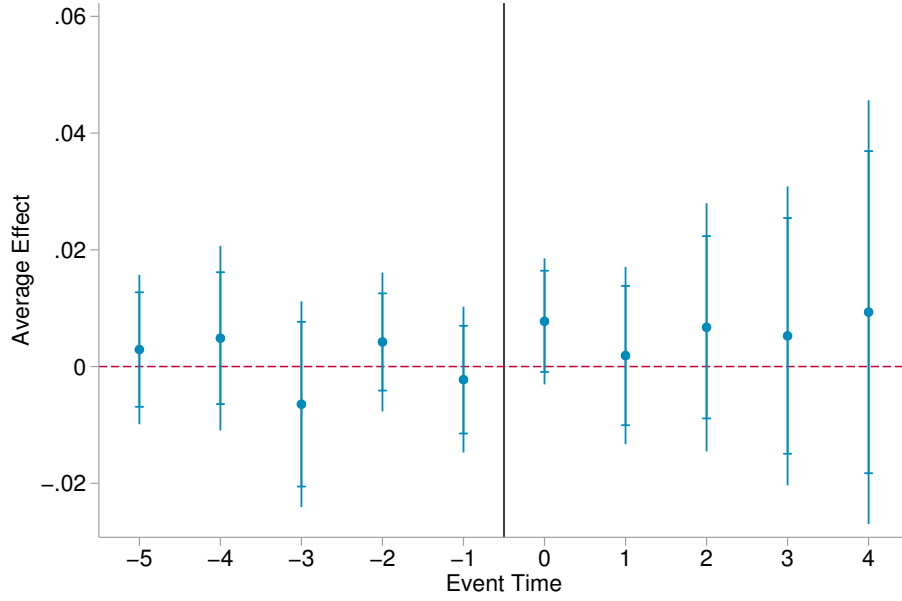
*Increase in Education Supply.* –One might be concerned that there is an increase in education supply, such as school constructions, that reduce the costs of attending schools, thus resulting in increased high school enrollment. I estimated the effects of SSA reform on the number of preschools, primary schools, middle schools, and high schools in each province since the year of adoption. As shown in [Table 4](#), there seem to be small negative effects on the number of schools, but the magnitude is small compared to both the mean and the standard deviation.<sup>49</sup> Therefore, the increased high school enrollment is unlikely to be driven

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<sup>49</sup>One potential explanation is that local provinces became more responsible for funding compulsory education as a result of the CEL. Thus, some schools were closed to reduce costs.



(a) Primary School Enrollment



(b) Middle School Graduation

Figure 7: The Effects of SSA Reform on Compulsory Schooling

*Notes:* The sample include 29 provinces. The average pre-reform primary school enrollment rate was 0.971. The average pre-reform middle school graduation rate was 0.772. Comparison groups are provinces not yet treated by the SSA reform ([Callaway and Sant'Anna, 2021](#)), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method. 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).

by an increase in education supply.

Table 4: Province-level Education Supply Measured in Number of Schools

	(1) Preschool	(2) Primary Schools	(3) Middle Schools	(4) High Schools
Effects of SSA Reform	298.707 (329.485)	-1,042.038* (591.018)	-261.591*** (81.565)	-30.970** (12.401)
Pre-reform Mean	6345.963	36840.593	3262.222	611.444
Std. Dev.	8473.66	22580.495	2385.118	285.954

*Note:* Tibet, Xinjiang, Chongqing, and Hainan are excluded. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant’Anna, 2021), weighted by cell size. The aggregated effects are the weighted average between event times 0 and 5 for preschool, primary school, and middle school and between event times 0 and 7 for high school. Wild bootstrap standard errors are clustered in the provinces. Data Source: Educational Statistics Yearbooks of China 1984 - 1998.

*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 if the fraction exposed to curriculum reform varies across event time. As shown in Column 1 of Table 5, no detectable effects are found, suggesting a limited compositional change in prescribed compulsory schooling duration.<sup>50</sup> I further restrict the sample to individuals who went to primary schools with six grades and middle schools with three grades, and the estimates remain robust.

*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,

<sup>50</sup>I thank Alex Eble for sharing the data and code.

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 the effects of SSA reform on birth-year OCP fine, measured in years of income, shown in Column 2 of [Table 5](#). 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.

*Falsification Tests.* –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. The estimates are summarized in Columns 3 to 5 of [Table 5](#).

The lack of effects on birth month composition is particularly crucial for the interpretation as one might suspect a differential compliance rate by birth month. For instance, some parents with children born right before the cutoff might not postpone entry for their children if starting at age seven but will postpone if starting at age six. Alternatively, parents with children born after the cutoff might choose to enroll their children early if the eligible age is seven but choose to enroll regularly if the eligible age becomes six. The estimates in

Column 5 suggest little parental response to the SSA reform. Even though the estimate is statistically significant, the magnitude is close to zero.

*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 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, as shown in Column 5 of [Table 3](#) in the earlier section. The estimate is robust to excluding younger cohorts. Therefore, college expansion is less likely to be a concern for my setting.

Table 5: The Effects of SSA Reform on Composition of Cells

	(1) Curriculum Reform	(2) OCP Fine	(3) Female	(4) Ethnic Minority	(5) Born After Cutoff
Effects of SSA Reform	-0.0385 (0.030)	0.0801 (0.076)	0.000144 (0.009)	0.00735 (0.009)	-0.0124** (0.006)
Pre-reform Mean	0.797	1.073	0.528	0.091	0.369
Std. Dev.	0.402	0.291	0.499	0.287	0.482

*Note:* Tibet and Xinjiang are excluded. Comparison groups are provinces not yet treated by the SSA reform ([Callaway and Sant’Anna, 2021](#)), weighted by cell size. The aggregated effects are the weighted average between event times 0 and 4. Wild bootstrap standard errors are clustered in the provinces. Data Source: Curriculum reform from [Eble and Hu \(2019\)](#), OCP fines from [Ebenstein \(2010\)](#), gender, ethnicity, and birth month from the 2005 census.

## 7 Heterogeneity Analysis and Potential Mechanisms

As the overall effects may mask potentially meaningful differences across groups. In this section, I present heterogeneity analysis by gender, birth month relative to the cutoff date, and local province preschool access. The estimates on high school enrollment are presented in [Figure 8](#). Estimates on high school graduation show very similar patterns and are shown in [Figure A6](#). Overall, I find little difference by gender or birth month but substantial differences by provincial preschool access. The estimates present in this section are estimated by first splitting the sample into subsamples, then collapsing the subsample into province-cohort cells and using [Callaway and Sant’Anna \(2021\)](#) as described in [Section 4](#). Thus, the subsample estimates are from within-subsample comparisons.

I use the presence or lack of differential effects to uncover potential mechanisms. As for potential mechanisms, the observed positive effects can be potentially explained by three channels. First, even though compulsory schooling was shifted one year younger, the norm of age to leave school might not change accordingly and continue to be the reference point for dropout decisions. One potentially important reference point is the legal minimum employment age of 16, even though the employment rate is smooth around age 16 (see [Figure A7](#)), potentially due to the large informal labor market and agricultural work. If the effect is indeed solely driven by the reference point channel, starting school at a younger age might mechanically increase schooling for a year, but there shouldn’t be an effect after the year. Since the effects persist into high school graduation, there are additional behavioral responses ([Lochner and Moretti, 2004](#)). In short, I refer to this first channel as the reference point

channel. Second, the opportunity cost of additional years of schooling increases with age. Starting school one year reduces the cost of obtaining more education. I refer to the second channel as the opportunity cost channel. Third, prior studies find early exposure to the academic curriculum and formal schooling increases cognitive skills (DeCicca and Smith, 2013; Fuller et al., 2017; Rosa et al., 2019; Ryu et al., 2020). Through dynamic complementarity of skill formation (Cunha and Heckman, 2007), early schooling increases human capital accumulation conditioning on years of schooling, which leads to pursuing of higher degrees. This channel only works if early starts increases cognitive skills. In the case when children are not ready for an academic-oriented education, they may experience short-term benefits but discourage learning in the long run (Cunningham and Stanovich, 1997). I refer to the third channel as the skill accumulation channel. The heterogeneity effects are consistent with the skill accumulation channel, but I cannot rule out the first two channels.

*Gender*— Some studies have found that the effect of delayed school entry differs by gender, but the gender with the larger effects varies across settings. For instance, Guo et al. (2023) found a more significant effect of being born after the cutoff on years of schooling for men in China, while Fredriksson and Öckert (2014) found a larger effect for women in Sweden. Figure 8(a) displays the estimated SSA reform effects for males and females. Overall, the effects for males are slightly larger than those for females, but the difference is small. Two opposing factors may jointly explain the lack of a substantial gender difference. Firstly, female children tend to be biologically more developed than males during early ages. On the other hand, due to the culture of son preference in China, parents might be more inclined to invest more in their sons than their daughters. The combination of these two factors might explain the slightly larger point estimates for males.

*Birth Month*– Prior studies find more pronounced effects for being born after the cutoff when the opportunity cost is higher (Zhang, 2022). Following this intuition, I estimate the effects separately for observations born before the cutoff date (birth month between January to August) and the ones born after the cutoff date (birth month between September and December). The children born after the cutoff face a higher opportunity cost than the children born before. Thus, starting school at a younger age means a larger reduction in opportunity costs for the students born after the cutoff and may lead to larger effects following the opportunity cost channel. Figure 8(b) suggests no differential effects between these two groups. The lack of effect might be explained by the small average differences are not salient enough to induce additional years of schooling.

*Preschool Access*– I measure preschool access at the province level using the number of preschool classes scaled by the number of students admitted into primary school in the year 1984 and split the sample into above-median provinces and below-median provinces.<sup>51</sup> Figure 8(c) suggests the effect sizes are more pronounced in provinces with more preschool access. For children in the provinces with less kindergarten access, the point estimates are still positive but imprecise, and the magnitude becomes smaller.

As preschool naturally leads to primary school enrollment, one might suspect the differential effects are solely driven by differences in take-up rates. To assess the degree of difference in takeup, I separately estimate the association between the SSA reform exposure and the average age at school entry by provincial preschool access.<sup>52</sup> Table 6 suggests a lower

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<sup>51</sup>I also tried using the number of preschool enrollees as an alternative measure, the provinces in the sample remain the same. Using the number of preschools is not an accurate measure as preschools in big cities are usually bigger and have more classes. I tried using the number of preschools, and results in big cities like Shanghai categorized into below-median provinces.

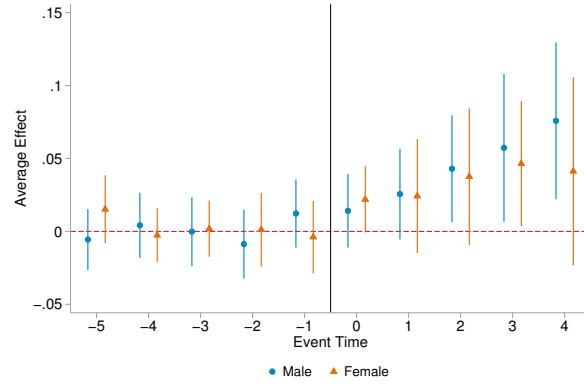
<sup>52</sup>See Section A2 for details on age at school entry estimation.



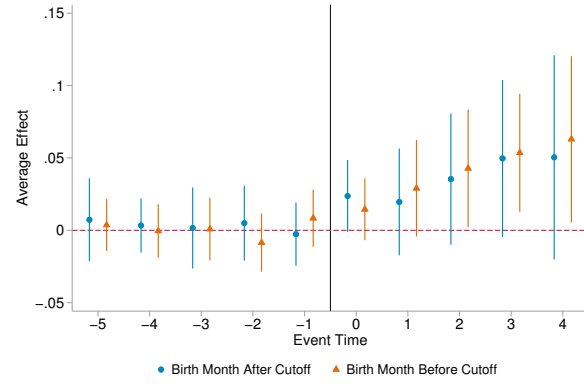
take-up in provinces with below-median preschool access. Even though the estimated age at school entry is subject to nontrivial measurement errors, taking the estimates as their face value, the difference in take-up doesn't seem to be enough to explain the differences in the effect size.

Another potential difference between provinces with high and low preschool access could be the quality of education supply. While provinces with below-median preschool access do have significantly lower per capita GDP, overall lower educational attainment, and fewer preschool resources, the resources for compulsory schooling stages, such as student-teacher ratios, are similar between the two groups. Detailed summary statistics are reported in [Table A5](#). Admitting there are unobservable characteristics, such as teacher quality or school facilities contributing to the quality of compulsory education, the differential effects of SSA reform are unlikely to be fully driven by the difference in the quality of education supply.

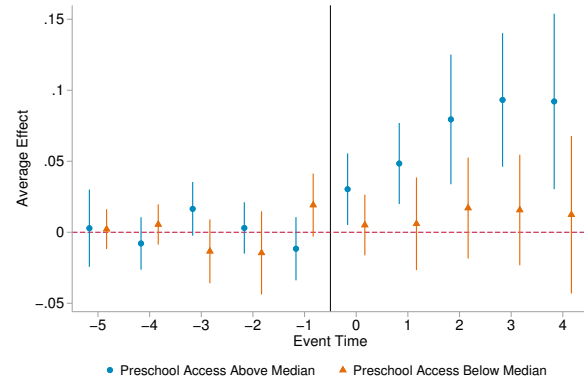
As preschool access can be viewed as a form of school readiness, the difference between provinces with above or below-median preschool access is consistent with the prediction by developmental theory and school readiness. For students with sufficient school readiness, early school access stimulates early brain development, and the initial advantages persist into adulthood. Students with less sufficient school readiness do not accumulate early advantage in cognitive skills as a result of early school access and thus show smaller effects on adult outcomes, potentially through reference point or opportunity cost channels. 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.



(a) By Gender



(b) By Birth Month



(c) By Preschool Access

Figure 8: Heterogeneous Effects on High School Enrollment

*Notes:* The sample include 29 provinces. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method. 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. Data Source: Census 2005. Educational Statistics Yearbook of China 1984.

Table 6: The Effects of SSA Reform on Estimated School Starting Age, by Preschool Access

	(1) All	(2) Preschool Below Median	(3) Preschool Above Median
SSA Reform Exposure	-0.407*** (0.042)	-0.335*** (0.070)	-0.449*** (0.053)
Pre-reform mean	7.107	7.023	7.187
Province Fixed Effects	Yes	Yes	Yes
Weighted	Yes	Yes	Yes
N	4016	1774	2242

*Note:* The estimated school starting age is from the sample of respondents who completed primary school, were born in school years 1974 to 1985, not living in Xinjiang or Tibet at age three, reported non-missing values, and had estimated SSA values between 4 and 10. The sample further excluded the observations whose hukou status is unknown, non-registered, or non-Chinese nationality. Preschool access was estimated using the 1984 province number of preschool classes scaled by the number of admitted primary school students. Data source: China Family Panel Studies 2010. Educational Statistics Yearbook of China 1984.

## 8 Conclusion

In this paper, I establish that the timing of formal schooling has long-lasting impacts, holding age rank within a grade and compulsory schooling duration constant. In particular, I leverage the staggered adoption of educational reform in China, which lowered the age requirement for school entry while maintaining the compulsory schooling duration and cutoff date unchanged. Using the difference-in-differences procedures outlined by [Callaway and Sant’Anna \(2021\)](#), I find that the reform substantially increased high school enrollment and graduation rates, and the estimates are robust to various checks. Aligning with existing findings of short- to medium-run positive effects of early academic curriculum on test scores ([DeCicca and Smith, 2013](#); [Fuller et al., 2017](#); [Rosa et al., 2019](#)), this paper suggests that the effects persist into adulthood, consistent with the dynamic complementarity of skill formation ([Cunha et al., 2006](#)).

My heterogeneity analysis finds little differential effects by gender or birth month but more pronounced effects in provinces with above-median preschool access, similar to the persistent effects on test scores in the Brazilian setting (Ryu et al., 2020). While some studies find early entry has larger effects on disadvantaged students in early grades (Suziedelyte and Zhu, 2015), my findings suggest that these large effects might be short-lived, potentially discouraging long-term learning (Cunningham and Stanovich, 1997). The differential effects imply increased inequality between children with different early childhood educational resources.

The findings of this paper add potential tools to reconcile the inconclusive findings using birthday variations in different countries. The effects of being born after the cutoff are the combined effects of starting school at an older age and being older than grade peers. The first part is the timing effect estimated in this paper, and the second part is the peer effects studied by Cascio and Schanzenbach (2016) and implied by Murphy and Weinhardt (2020). As peer effects can be sensitive to culture and educational settings, such as tracking or comprehensive (Peña, 2017), the direction or magnitude can vary substantially across countries. On the other hand, the timing effect has its roots in human developmental theory and is, therefore, more likely to be generalizable to any setting. The mixed effects might be explained by the magnitude of the peer effects relative to the timing effects.

All children starting school face a certain age requirement. The findings of this paper carry broad policy implications. Overall, the results suggest that having a younger age requirement increases the efficiency of human capital accumulation. While a small fraction pursue a higher degree, resulting in delayed entry into the labor market, more people are potentially benefiting from early entry into the labor market given a fixed number of

years of schooling and potentially higher lifetime earnings. Admittedly, later entry age might increase the average test scores in standardized tests [Fletcher and Kim \(2016\)](#). There could potentially be long-run costs in the loss of human capital at the time of entering the labor force. Moreover, the findings suggest that the endowment before the academic curriculum can lead to persisting differences in achievement. This insight emphasizes the importance of considering developmental readiness when implementing policies to introduce more academic-oriented curricula to younger kids, providing valuable guidance for education policymakers about practices to increase school readiness ([Pianta et al., 2007](#)).

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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:

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

where  $Y_{j,k}$  is 2005 self-report educational attainment in 2005. The cohort indicators,  $\sigma_k$ , capture cohort-varying national-level shocks, and province indicators,  $\lambda_j$ , 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,  $\delta_e$ , 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](#).

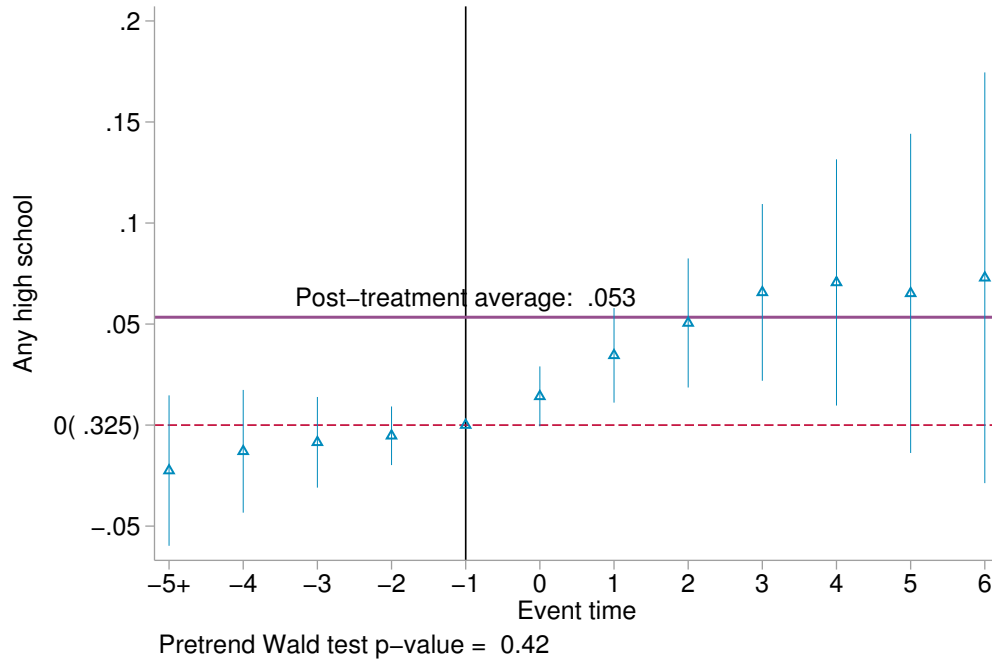
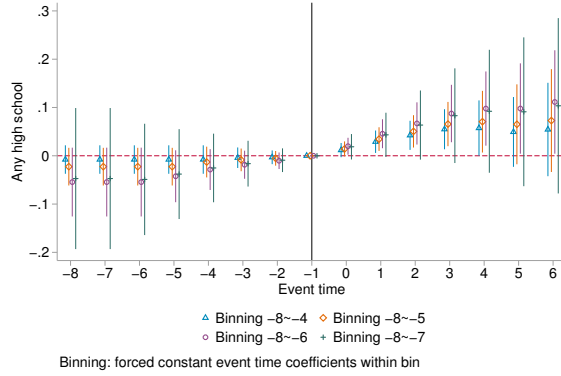
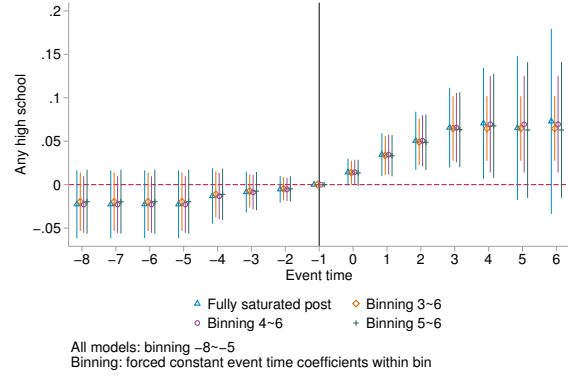


Figure A1: Estimated Effect of School Starting Age Rule on High School Enrollment

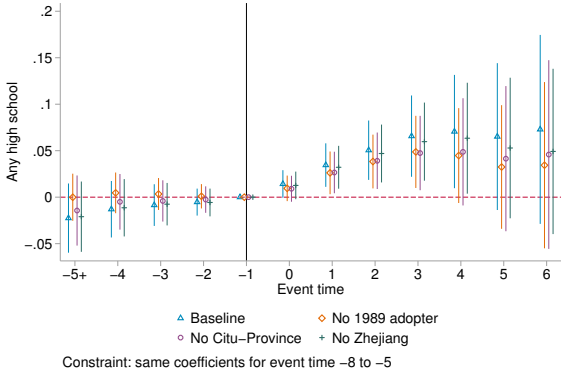
*Notes:* This figure shows the baseline dynamic TWFE estimates of exposure to the school starting SSA reform at different ages on the probability of completing high school. The pre-treatment coefficients from -8 and -5 are binned. The standard errors are clustered at the province level. Observations 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.



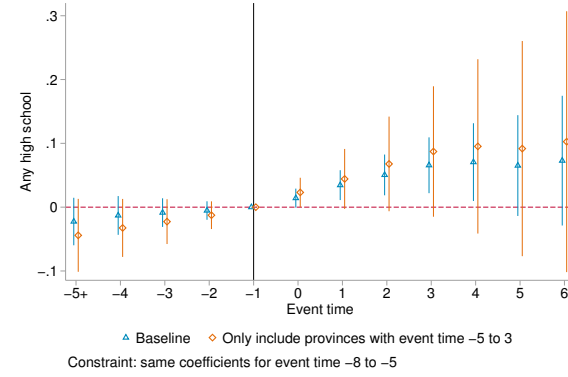
(a) Various Pre-treatment binning



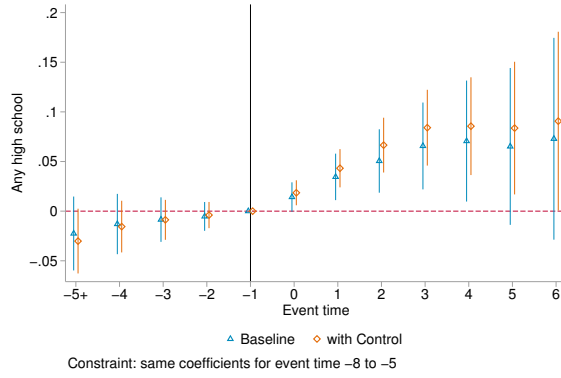
(b) Various Post-treatment binning



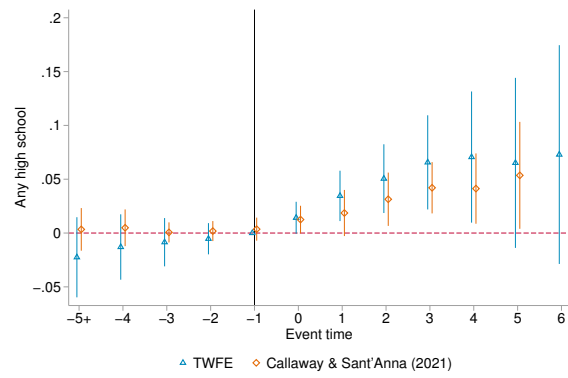
(c) Exclude special group



(d) Balanced panel



(e) Add Control



(f) Hetero-Robust Estimator

Figure A2: Sensitivity analysis

*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 First Stage**

### **A2.1 Supplementary Data for Descriptive Evidence for the First Stage**

To gain insights into whether the CEL SSA policy affected school starting age, I employ the 1982 and 1990 censuses, the 2010 wave of the China Family Panel Studies, and the 1989 and 1993 waves of the China Health and Nutrition Survey. According to my conversation with the office of information disclosure of the Ministry of Education, there are no official statistics on school starting age. Due to the lack of direct measures, I have to rely on indirect measures, such as end year and duration, to estimate the school starting age. In the rest of this section, I describe the data sets and the variables I used to comprehend the change in SSA distribution. Each data set has its limitations, and the details are discussed in later in this section.

#### **A2.1.1 1982 and 1990 Censuses**

The 1982 and 1990 censuses are considered reliable and high-quality. I use the 1982 census to estimate the pre-reform SSA distribution and the 1990 census to estimate the post-reform distribution. The censuses asked two questions on education for people aged 6 or older: educational attainment level and school attendance (such as “in school”, “graduated”, “dropout”, or “incomplete”). The current grade in school is not available. I construct an indicator of “ever attending any school” to gain insights into the minimum policy age of enrollment, assuming most people did not enroll in school before meeting the minimum age

requirement. The censuses only show cumulative enrollment rates by age, so I employ two additional data sets to estimate the probability mass function of school starting age.

### **A2.1.2 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<sup>53</sup>. I employ the 2010 wave to estimate the SSA distribution for pre- and post-reform cohorts. 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.

### **A2.1.3 China Health and Nutrition Studies 1989 and 1993**

The China Health and Nutrition Survey (CHNS) examines the nutritional and health status of the Chinese population and collects social, economic, and demographic data. The CHNS conducted surveys every two to four years, starting in 1989. While not nationally

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<sup>53</sup>Excluding Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia, and Hainan



representative<sup>54</sup>, the CHNS provinces provide significant variation in geography, health indicators, and economic development. I use the 1989 wave to estimate the starting age distribution of the pre-reform cohorts and the 1993 wave to estimate the post-reform distribution. In the baseline survey in 1989, CHNS surveyed 3,795 households from 8 provinces. The 1993 CHNS re-surveyed the original sample and added newly formed households from the original sample households resided in sample areas. The CHNS asked participants questions on “completed years of formal education in regular school” and if they were “currently in school”. Birth month and survey month are not available in CHNS. I estimate the difference between survey year and birth year for the sample currently in school to learn about the school starting age. Admitting the CHNS measures are cruder, and the CHNS sample is small and not nationally representative, the CHNS observations were less likely to suffer from the “recollection bias” and non-reporting issue and complement CFPS findings.

## A2.2 Estimation using CFPS

To argue that the SSA policy affected student outcomes by giving students early access to education, I need to show that the policy affected when students started school: the SSA distribution changed after the policy. Since the policy lowered the school starting age rules to six or seven, I expect to see an increase in the fraction of students starting school at six and a reduction at eight. The expected effect for age seven is ambiguous, and the direction

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<sup>54</sup>The survey covers nine provinces: Liaoning, Heilongjiang, Jiangsu, Shandong, Henan, Hubei, Hunan, Guangxi, and Guizhou. They vary substantially in geography, economic development, public resources, and health indicators. A multistage, random cluster process was used to draw the samples surveyed in each province. Counties in the nine provinces were stratified by income (low, middle, and high), and a weighted sampling scheme was used to select four counties in each province randomly. In addition, the provincial capital and a lower-income city were selected when feasible. In two provinces, other large cities had to be selected. Villages and townships within the counties and urban and suburban neighborhoods within the cities were selected randomly.

depends on the relative size moved from eight to seven and seven to six. If the minimum age rule was strictly enforced, I expect to see no one start school before six. Due to data limitations, cohorts and outcomes are defined differently in each data set. To match the event study sample, I restrict all three first-stage samples to provinces that implemented the SSA policy before the end of the 1989 school year other than Xinjiang or Tibet.

### A2.2.1 1982 and 1990 Censuses

The census estimates provide reliable insights into the overall school enrollment by age, given its large sample size and high response rate. In the 1982 census, birth year and birth month are unavailable. Only “age” is available. Thus, I define the *cohort* in historical censuses as the age when observations were surveyed. I restrict the sample to children aged 6 to 12 when surveyed. I estimate the fraction ever enrolled by age as a crude proxy for early access to education because students rarely started school before the required minimum age.<sup>55</sup> I consider an observation “ever enrolled in school” if reported school attendance was primary school or higher. I use the 1982 census to show the pre-reform enrollment and the 1990 census to show the post-reform distribution. The estimated fraction of ever enrollers by age surveyed are presented earlier in the paper in figure A3.

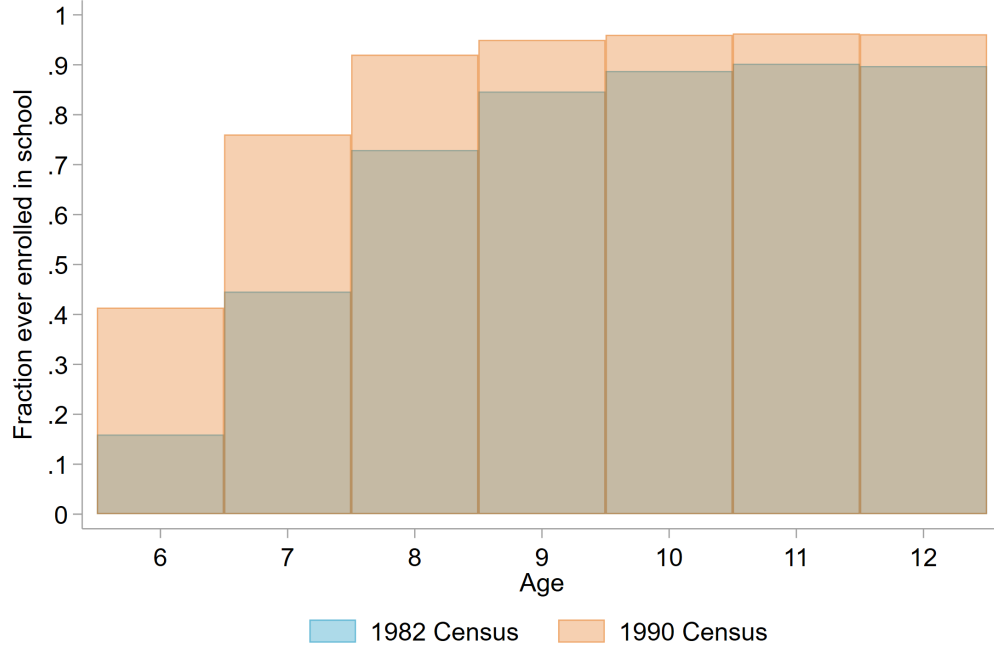
Compared to children aged 6 to 12 surveyed by the 1982 census, children in the 1990 census had higher enrollment at all age categories, which suggests an overall higher primary school enrollment. The increased enrollment may downward bias my main results if lower-ability students were drawn into school due to the SSA policy. Compared to other age

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<sup>55</sup> Anecdotal account suggests that parents in China prefer their children start formal schooling as early as possible. Nearly every year, representatives to the People’s Congress suggest lowering the school entry age from six to six and a half so that children born after September can enroll in the same year as peers born before the end of August.

groups, significantly larger fractions of students enrolled in school by age six and seven. The increase in age 6 and 7 fractions suggests that the new starting age rule brings earlier access to education: at least 25% more students received education at age 6, and at least 30 % more students received education at age 7. The enrollment rate by age 6 is only 0.4 suggesting a large fraction of localities took the age seven option. During the reform period, overall enrollment was potentially increasing for reasons unrelated to the CEL. It suggests that the later cohorts consist of a higher portion of disadvantaged or lower-ability children and might bias my results towards zero. I address the potential trend by including province-specific linear trends in my main analysis. The estimated effect should be interpreted as a lower bound of the effect.

Since the censuses are cross-sectional and do not collect information on the current grade, I can only estimate the cumulative density at a point in time using the censuses. I turn to CFPS to estimate the SSA distributions. Additionally, a concern about the observed change in fractions was that the updates on reporting and processing methods in the 1990 census might drive the difference. The data quality difference is unlikely the only factor causing the change. If the methods of the 1990 census made heads of households more likely to report school attendance than the 1982 census, the distribution would uniformly shift up unless the method was more likely to influence reporting of younger children's education. I use the same wave of CFPS for both pre-reform and post-reform cohorts to further address the data quality change concern.



*Notes:* age is defined as the age when surveyed. Tibet and Xinjiang are excluded from the sample.  
*Source:* 1982 and 1990 Censuses

Figure A3: Fraction Ever Enrolled in School by Age Using Historical Censuses

### A2.2.2 CFPS

I construct the sample from the baseline wave of the CFPS conducted in 2010. I define the *cohort* in CFPS using the school year cutoff: cohort  $y$  are the observations whose birth dates are between September 1,  $y - 1$  and August 31,  $y$ . The definition follows the *cohort* of the main analysis, and it most accurately aligns with the SSA policy. I restricted the sample to individuals who 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. The total number of pre-reform observations is 1464, and post-reform observations is 1175.

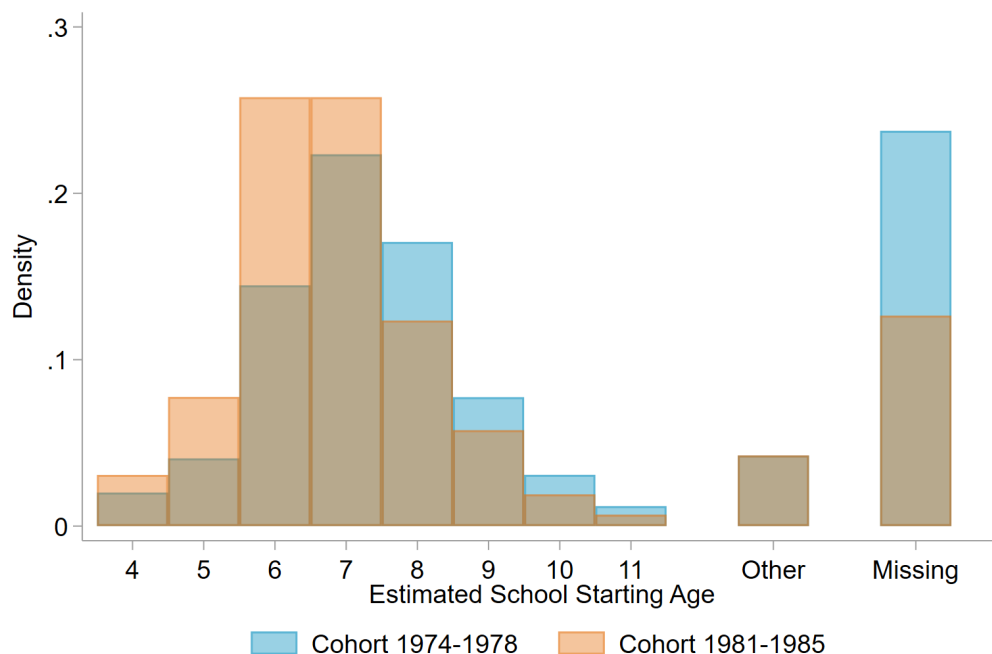
The CFPS does not directly collect age at the entrance to primary school. Instead, it

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 interested in the policy effect, I am interested in the integer age by the enrollment cutoff 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. Figure [A4](#) suggests the policy decreased the fraction of students who started school at age eight or older and increased the fraction who started school at age six or younger. The change in the fraction who started at age seven is small. The small change in the age 7 fraction suggests that the number of students exposed to a policy that moved the minimum age from seven to six is similar to that of students exposed to the age seven to six policy. It is worth mentioning that a non-trivial fraction of children started school before the lowest minimum enrollment policy age, age six. It suggests that the age rule as the minimum age is not perfectly enforced in some localities. Anecdotally, parents living in places with strict enforcement got around the minimum age restriction by enrolling their children in places with lenient enforcement and

transferring back after a few months. The increase in the fraction of people who started before six suggests the manipulation in enrollment use of the policy age as a reference age. Some parents always want to enroll their children one year earlier than the crowd.



*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. Pre-reform N = 1464. Post-reform N = 1175.

*Source:* China Family Panel Studies 2010

Figure A4: Estimated School Starting Age Distribution using CFPS 2010

The CFPS estimates have several limitations. First, the information on primary school end year and duration 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 observed change in distribution is

driven by the duration policy, 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 to see 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 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. The life expectancy of my cohorts is between 64 and 68. To address these limitations, I employ the CHNS to provide evidence on the descriptive first stage from a different perspective.

I also estimated the association between the SSA reform exposure and the average school starting age to understand takeup, summarized in [A1](#). The preferred specification is Column 3, OLS with province-fixed effects, and individual observations are weighted using national sampling weights. The average school starting age among the treated cohorts is 0.407 lower than unexposed cohorts.

### **A2.2.3 CHNS**

Birth months are unavailable in CHNS, so *cohort* in CHNS is defined using calendar year: cohort  $y$  are observations with birthdays between January 1,  $y$  and December 31,  $y$ . I estimate the age of the school starting year as a proxy for the school starting age. For individual  $i$  who was currently in school when surveyed, the age of the school starting year

Table A1: The Effects of SSA Reform on Estimated School Starting Age, Alternative Specifications

	(1) OLS	(2) OLS	(3) OLS	(4) CSDID	(5) CSDID
Effect of SSA Reform	-0.369*** (0.042)	-0.374*** (0.043)	-0.407*** (0.042)	-0.241 (0.203)	-0.333 (0.210)
Province Fixed Effects	No	Yes	Yes		
Weighted	No	No	Yes	No	Yes
N	4016	4016	4016		

*Note:* CSDID aggregation used event times 3 and 5. Tibet and Xinjiang are excluded. Data Source: China Family Panel Studies 2010.

is estimated as follows:

$$SSA_i = SurveyYear_i - YearsofSchooling_i - BirthYear_i$$

I restrict the sample to observations with an estimated SSA between 4 and 11. To maximize the probability of observing someone in school, I need to use the survey wave in which the SSA-reform-target cohorts were old enough to complete enrollment but not too old and have exited. Based on the census and CFPS estimates, most students started school by age 10, so the cohorts should ideally be around 10 when surveyed. According to official statistics of the Ministry of Education, only 64.8% of primary school graduates progressed to middle school in 1985 and 74.6% in 1990. The cohorts need to be surveyed before middle school entrance to avoid selection. Hence, I am limited on which cohorts to estimate in the CHNS sample. I can only observe the 1976-1977 cohorts as pre-reform cohorts and the 1982-1984 cohorts as post-reform cohorts.

I employ the 1989 CHNS to estimate the pre-reform distribution. I include the 1976

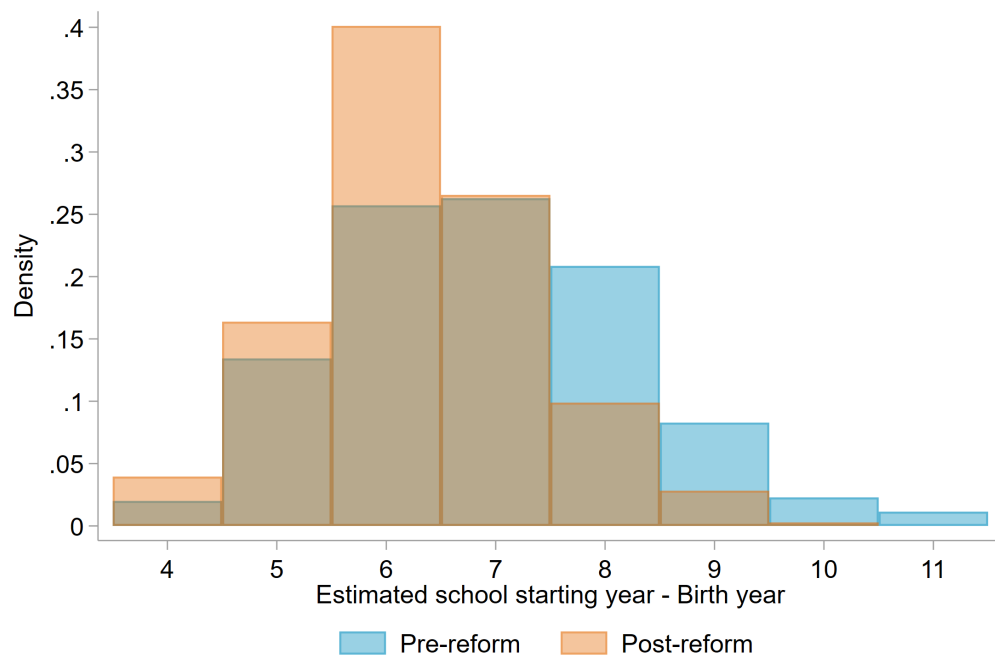


and 1977 calendar year birth cohorts as the pre-reform cohorts. They were aged 12-13 when surveyed. Ideally, I would like to have the same pre-reform cohort as CFPS. I exclude the 1988 cohort because I cannot determine SSA policy exposure without knowing the birth months. I exclude the 1974 and 1975 cohorts because 26.9% of the 1974 cohorts and 16.8% of the 1975 cohorts left school<sup>56</sup>. Since early starters also completed primary school early, thus less likely to be in school when surveyed, the 1974 and 1975 in-school samples were less representative. The 1976 cohort has an 8.9% dropout rate, slightly higher than the 4.2% primary school dropout rate from CFPS and the 4.4-4.9% dropout rate from cohorts 1977-1978 in CHNS. It suggests that about 4% of students completed primary school and did not continue to middle school by age 13. The pre-reform distribution may slightly underestimate the fractions enrolled before eight. I include the 1976 cohorts to increase the pre-reform sample size. The total number of observations for pre-reform cohorts is 350. I employ the 1993 CHNS for the post-reform estimation and include cohort 1982-1984. They were aged 9-11 when surveyed. I did not include the 1985 cohort because they were too young to complete enrollment. Although the 1984 cohort was only 9 when surveyed, its in-school fraction is 94.6%, as high as 94.8% for the 1983 cohort and 93.1% for the 1982 cohort. The post-reform CHNS estimate may slightly underestimate the fraction enrolled after nine. The total number of observations for post-reform cohorts is 354.

Figure A5 shows the estimated age distribution at the end of the school starting year. The distributions look different from CFPS because the CHNS sample is not nationally representative, there is a difference in the composition of the pre-reform and post-reform

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<sup>56</sup>Having left school is defined as an individual not currently in school but having received at least one year of regular education



*Notes:* The pre-reform cohorts are the 1976 and 1977 calendar year birth cohorts. The post-reform cohorts are the 1982-1984 calendar year birth cohorts. Pre-reform N = 350. Post-reform N = 354.

*Source:* China Health and Nutrition Studies 1989, 1993

Figure A5: Estimated Age at the End of School Starting Year Distribution using CHNS 1989 and 1993

cohorts, and the outcomes are estimated differently. The CHNS estimates show a significant increase in the fraction who started school at six and a significant decrease in the fraction who started school at eight. The CHNS estimates suffer from measurement error due to the lack of information on birth months and survey months. The CHNS estimates show a higher fraction of students starting school at 6. It was because the outcome was estimated as the difference between the school starting year and birth year. Such estimations systematically misassign people with birth months between September to December to the lower-age bins. Also, the estimated distribution is less preventative due to the survey design. I include them to support the findings of the CFPS estimates.

Since the estimated changes using the above datasets align with the expected effect of the SSA policy, it is plausible that the SSA policy affected students' timing of enrollment by providing early access to education.

## A3 Additional Tables and Figures

Table A2: Summary Statistics of Pre-reform Province Characteristics

	(1) 1984 level Mean	(2) SD	(3) 1981-to-1984 change (%) Mean	(4) 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. One-child policy fine data from [Ebenstein \(2010\)](#). Compulsory Educational Law implementation year from [Chen and Park \(2021\)](#)

Table A3: Average Monthly Income by Attainment (Age 25-45)

	(1) All mean/sd	(2) No schooling mean/sd	(3) PS mean/sd	(4) MS mean/sd	(5) HS mean/sd	(6) College and above mean/sd
Monthly Income	613.103 907.859	217.087 252.620	360.064 556.499	516.852 669.832	796.129 947.204	1601.572 1708.370
N	903,790	37,987	209,136	431,238	140,742	84,687

*Note:* Education is measured by enrollment level. Data Source: Census 2005

Table A4: Estimated income premium by education attainment (Age 25-45)

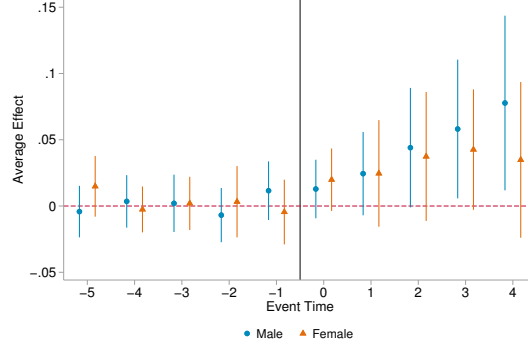
	(1) Monthly Income	(2) Monthly Income	(3) Monthly Income	(4) Monthly Income
Primary school graduate	111.304*** (12.500)	85.700*** (9.758)		
Middle school graduate	145.782*** (18.099)	120.162*** (10.076)		
High school graduate	587.043*** (79.728)	536.684*** (74.422)		
Primary school enrollee			141.498*** (17.832)	98.098*** (12.635)
Middle school enrollee			157.254*** (18.939)	129.216*** (10.724)
High school enrollee			584.580*** (79.577)	534.829*** (74.110)
Province fixed effects	No	Yes	No	Yes
Age fixed effects	No	Yes	No	Yes
Clustered SE	Yes	Yes	Yes	Yes
N	894,434	894,434	894,434	894,434

*Note:* Data Source: Census 2005

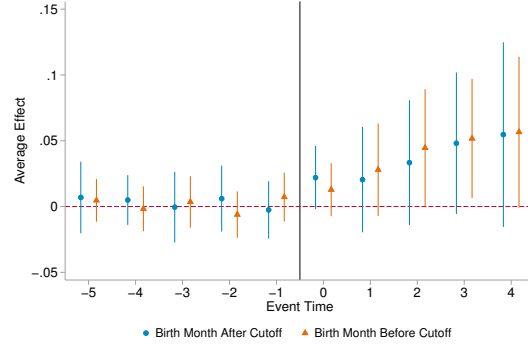
Table A5: Sample and Provincial Characteristics by Preschool Access

	Above Median		Below Median	
	Pre-reform	Post-reform	Pre-reform	Post-reform
<i>Outcomes</i>				
Primary school enrollment	0.990	0.994	0.953	0.964
Middle school graduation	0.848	0.907	0.700	0.757
High school enrollment	0.340	0.416	0.229	0.258
High school graduation	0.336	0.392	0.225	0.235
College enrollment	0.154	0.206	0.091	0.098
<i>Demographics</i>				
Female	0.522	0.524	0.514	0.534
Ethnic minority	0.031	0.029	0.186	0.187
Age	28.703	22.869	27.770	22.619
<i>Province Characteristics</i>				
Year-end population (10,000 persons)	4071.375	4124.577	4152.308	4430.688
Birth rate (‰)	16.638	16.508	18.705	18.158
Percent of population female	48.595	48.636	48.519	48.619
Per Capita GRP (yuan)	925.953	999.831	501.654	505.830
<i>Provincial Education Resources</i>				
Num of preschools by incoming 1st grade size	0.011	0.012	0.003	0.003
Num of preschools classes by incoming 1st grade size	0.031	0.034	0.009	0.009
Num of preschools enrollee by incoming 1st grade size	0.887	0.947	0.281	0.278
Num of primary school students per teacher	23.826	23.579	26.613	27.472
Num of secondary school students per teacher	16.651	16.183	16.005	16.705
N	110,248	110,498	116,519	71,000

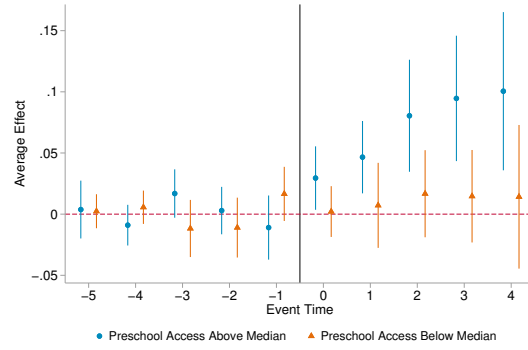
*Note:* Data Source: Census 2005. Educational Statistics Yearbook of China 1984. Nine-year compulsory from [Eble and Hu \(2019\)](#).



(a) By Gender



(b) By Birth Month



(c) By Preschool Access

Figure A6: Heterogeneous Effects on High School Graduations

*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. Data Source: Census 2005.

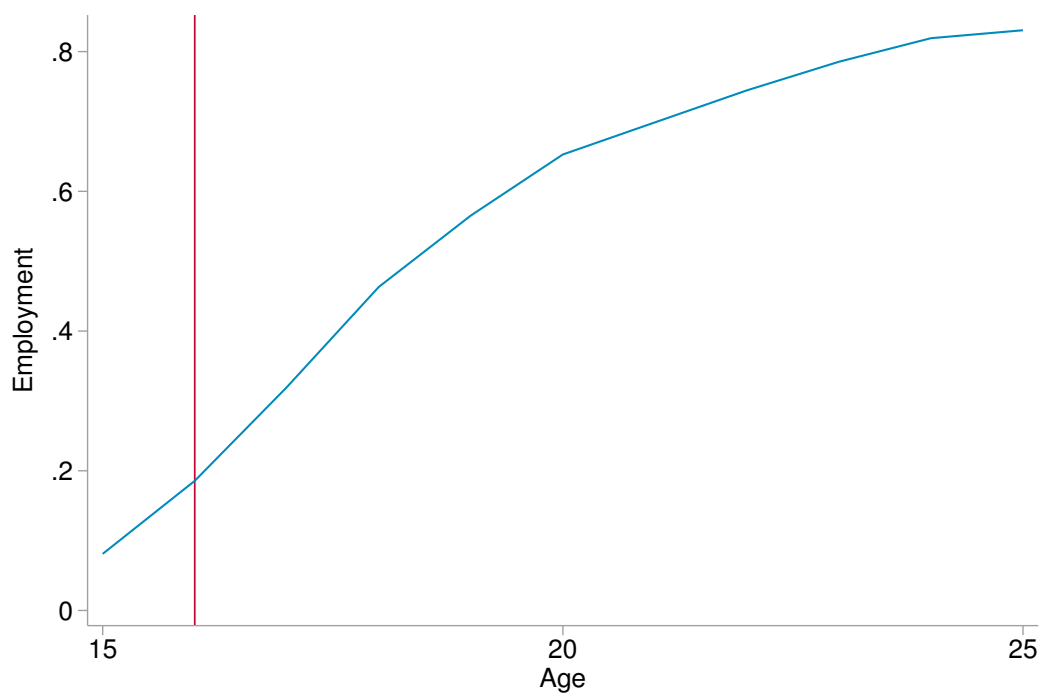


Figure A7: Employment Rate by Age

*Notes:* Data Source: Census 2005.