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Essays on Economics of Education

By

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DISSERTATION

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To the unsung heroes

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Abstract

Education policies could profoundly impact students' experience and achievement in school, as well as their long-term human capital accumulation and labor market returns. This dissertation focuses on school-entry policy reforms in China and South Korea and their impacts on students' school-entry decisions, educational attainment, and mental health. The first two chapters examine a school-starting age reform in China implemented in the 1980s and 1990s. This reform lowered the age requirement for primary school entry from age seven to age six while maintaining the birth date cutoff for enrolling in the current academic year and left the required duration of schooling unchanged. The third chapter focuses on a 2009 South Korean reform that moved the birth date cutoff for enrolling in the current academic year from March 1 to January 1.

The first chapter of my dissertation documents the change in the distribution of students' school-starting age in response to the reform. Utilizing data from the 1982 and 1990 censuses, China Family Panel Studies 2010, and China Health and Nutrition Surveys from 1989 and 1993 for estimation, I estimate the distribution of school starting age before and after the reform using three distinct proxies and document the changes in the distribution. I highlight the similarities and discrepancies of the observed shape and changes in distribution between each proxy and discuss the limitations and contributions of each approach. My findings indicate that the reform substantially increased the fraction of children who started school at age six and reduced the fraction of those who started school after age seven. These changes in school-starting age distribution are robust to the choice of data or proxy and are unlikely to be driven by trends in early enrollments.

The second chapter of my dissertation estimates the causal effects of the reform on educational attainment by exploiting the staggered adoption across provinces. Using Chinese 2005 census microdata and implementing heterogeneity-robust difference-in-differences

by Callaway and Sant'Anna (2021), I find that the reform increased high school enrollment by 5.5 percentage points and high school graduation by 5.3 percentage points. Heterogeneity analyses reveal more pronounced effects for students with better pre-primary education resources but little difference by gender. Additional analysis suggests that the dynamic complementarity of early skills could be a potential mechanism that explains why starting primary school at six instead of seven enhances long-term human capital accumulation.

The third chapter, co-authored with Estelle Shin, examines the impact of changing the school-entry birth date cutoff on students' mental health, including self-esteem and emotional/behavioral problems such as inattention, aggression, social withdrawal, physical symptoms, and depression. We utilize a difference-in-difference design and detailed mental health survey data from the Korea Children and Youth Panel Survey 2010 and 2018. Our analyses suggest that after the reform, both male and female students born in January and February showed substantially improved early-adolescent mental health relative to peers compared to students born in January and February before the reform. The female students exhibit lower levels of inattention and aggression, while the male students show high levels of self-esteem. Our findings add novel evidence to the growing economics literature on children and adolescent mental health.

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Chapter 1

The 1986 Compulsory Education Law and the School Starting Age Reform in China

1.1 Introduction

Developing countries seek to fuel economic growth by cultivating an educated workforce. A common way for governments to achieve such goals is through policies that extend compulsory education to higher grades or increase the minimum school leaving age. Recent research suggests that the most efficient strategy to strengthen the future workforce is to invest in children during early childhood (Cunha et al., 2006; Knudsen et al., 2006), which implies alternative ways for education policymakers to improve education efficiency. One possible policy is to allow children to start formal schooling early while holding the duration of schooling unchanged. However, as students and parents have the option of delaying primary school entrance, it is unclear how students and parents may respond to such policies if the policies are to be implemented by the government. Therefore, it is crucial to consider the behavioral response before evaluating the policy's impact on outcomes of interest. In this chapter, I examine a policy that gave children early access to primary school in a setting where enrollment in pre-primary education was low. In particular, I analyze a policy in China in the 1980s that lowered the legal minimum school starting age, which is also part of national education reform aiming to promote compulsory schooling. Through my analysis, I seek to document the change in school-starting age distribution around the time of the reform to understand whether students adjusted their school enrollment behavior in response to the change in the legal school-starting age.

In 1986, China enacted its first formal national law of education, the Compulsory Education Law (CEL), to promote compulsory schooling, protect the right to receive compulsory education, and lay the foundation for improved population quality. The CEL is a bundle of policies regulating various aspects of education, including making primary and middle school compulsory, standardizing age six as the primary school entry age, requiring compulsory education to be free-of-change, and restricting employment of "school-age" students. CEL was gradually adopted by provinces between 1985 and 1994.

As one of the most important education reforms in China, the adoption and implementation of the CEL has been widely studied in the literature. Using the geographical variation in CEL adoption dates, studies have found that the law increased completed education by 0.29 - 1.5 years, depending on the demographics of the sample. Using CEL exposure as an instrument for education, previous findings show an additional year of education induced by the CEL increases personal income (Fang et al., 2012; Campos et al., 2016), delays the age of marriage (Li and Cheng, 2019; Liang and Yu, 2022) and reduces the number of births (Chen and Guo, 2022), improves various children's outcomes (Cui et al., 2019) and parents' outcomes (Ma, 2019), mixed effect on self-health outcomes (Xie and Mo, 2014; Huang, 2015).

Existing literature mainly evaluates the total effect of the CEL. In other words, researchers in prior literature mostly consider the effects of all components of the CEL as a bundle. Different from the literature, I focus on one specific component: the schooling starting age (SSA) policy lowering the legal primary school starting age to age six. Before the SSA policy, the national standard for primary school entry age was seven. The policy before the CEL that specified a national starting age rule dated back to 1951. However, because the 1951 policy was premature and was disrupted by waves of education reforms during the political chaos in the 60s and 70s, the de facto legal minimum school starting age was unclear and varied across regions. Regions like Shanghai and Anhui experimented with the admission of six-year-olds (Liu, 1993), and while in regions that were less economically developed, like Gansu province, a large fraction of students did not start school until age 8. Even though all components of the CEL bundle had the same adoption date within a province, the SSA policy only treated children who had not yet started primary school. The rest of the CEL policies treated all children who had not yet completed middle school. Therefore, I can isolate the effect of the SSA policy from other aspects of the CEL by comparing cohorts of children who should have just started school to those just affected by the SSA reform.

Due to the lack of direct statistics on the actual age at enrollment, the effectiveness of the SSA policy and compliance rate remains unknown. As far as I know, this chapter represents the first attempt to measure the change in age at enrollment around the time of the SSA reform. To tackle the data challenge, I utilize three complementary sources of data to construct measures of school-starting age distribution. Firstly, I utilize the 1982 and 1990 Chinese censuses to estimate the change in overall enrollment by age before and after the reform. While the censuses provide reliable estimates of cumulative enrollment due to their large sample size and high response rates, they are less satisfactory as measures of the actual school starting age and cannot differentiate the reform effects from time trends. Secondly, I complement the census measures using two other measures from survey data. I measure the distribution of school starting age among children aged between four and eleven using the China Family Panel Study and the distribution of the age in the school starting year among children aged between four and eleven using the China Health and Nutrition Study. All three measures suggest that children responded to the SSA reform by starting primary school at a younger age. In particular, evidence from all three measures and datasets suggests a significant increase in the fraction of students starting primary school at age six. These findings indicate that the legal minimum school starting age was binding for children and parents in China, with a large fraction of students commencing school at the new minimum legal starting age in response to the change.

For the remainder of this chapter, Section 1.2 explains the institutional background and the policy, Section 1.3 describes the data, including the construction of the measures of school starting age distribution, and outlines the strengths and limitations of each measure. Section 1.4 discusses the observed changes, and Section 1.5 concludes and connects to the next chapter.

1.2 Institutional Background

1.2.1 The Education System in China before the Compulsory Education Law

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 adults aged between 18 and 69 had a literary rate of around 65%, which was lower than the 1982 world average of 70%.[1] The literacy rate in China differed by gender, with the male rate of 79% and the female rate of 50%. Literacy was higher among younger generations. The 1982 literacy rate among people aged 18 to 30 was 83%, with the male rate

[1]The age definition of adult follow Deng and Treiman (1997)

of 93% and the female rate of 72%.[2] Around 33% of adults had any middle school education, fewer than 12% had any high school education, and 1% had any college education. Pre-primary education enrollment was uncommon. In China, preschool has three grades. The gross preschool enrollment in China was below 20% before 1985 and remained below 25% between 1985 and 1994.[3] There were fewer than 3 preschools per thousand preschool-age kids between ages 3 and 5 on average.[4][5] 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.

The CEL was introduced after the upheaval of the Cultural Revolution (1966-1976). 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. 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).[6] In particular, primary and secondary curriculum and duration underwent constant reforms,[7] and the merit-based national college entrance exam was halted until 1977.

Figure 1.1 shows the trends in enrollment among the cohorts from 1960 to 1990, using

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

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

[4]Using the number of children between ages 4 and 6 will not change the conclusion.

[5]Source: 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).

[6]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.

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

data from the 2005 census. The segment between two dashed vertical lines represents the educational attainment of my main analytical sample, with the red vertical line marking the first SSA reform-exposed cohort in my sample. Primary school enrollment remains consistently high across all cohorts. Middle school enrollment increased slightly for the 1960 to 1963 cohorts and declined for cohorts between 1963 and 1968. The 1960 cohort started school during the Cultural Revolution, and the short-run increase in middle-school enrollment may be attributed to the temporary positive effect of redistributing educational resources into rural areas (Liu, 1993). The subsequent drop reflects the long-term disruptive effects of the Cultural Revolution. The 1968 cohorts started primary school around the end of the Cultural Revolution, leading to a steady increase in middle school enrollment thereafter. A 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 in the number of 1986 and later cohorts was because they were too young to enroll in college in 2005.

1.2.2 The 1986 Compulsory Education Law

On April 12, 1986, the Compulsory Education Law (CEL) was passed by the National People's Congress. The 1986 CEL consisted of 18 articles of law that regulated various aspects of education encompassing duration, starting age, infrastructure, costs, school finance, and teacher training. Article 2 regulates compulsory schooling duration: "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. Article 5 regulates school starting age: "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.” Article 9 specified that local government should establish primary and middle schools to ensure convenient access to education and required urban and rural development plans to include compulsory education facilities. Article 10 regulated costs of compulsory schooling: "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 school costs, “Two-Exemptions-One-Subsidy.” Article 12 specified that the 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. Article 13 specified strengthening normal schools and establishing a system to test teacher credentials.

It is worth noting that the 1986 CEL lacked specific details, suffered from vague wording, and often failed to provide comprehensive and clear instructions, which created challenges during implementation, especially for the section regarding school funding. As a result, 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.

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 time frames tailored to local development. Between 1985 and 1994, all mainland provincial People’s Congress enacted local compulsory education legislation. The first provincial lawmakers, the People's Congress of Zhejiang Province, passed the local legislation, "Zhejiang Province implements the Nine-Year Compulsory Education Regulations," on June 13, 1985. The lawmakers of the last province, the 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. Local legislation followed the sample format as the 1986 CEL, regulating various aspects with a single effective date. Figure 1.3 shows that there were geographical and temporal variations in the adoption timing, with darker shading denoting earlier effective dates. The detailed implementation dates are listed in Table 1.1.

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

1.2.3 The School Staring Age Reform

In this paper, I focus on documenting the salience and compliance of the policy regulating school starting age. The previous policy prior to the reform specified a national school starting age dated back to 1951,[8] 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 advocated for admitting six-and-a-half or six-year-old children into primary schools if "conditions allow," without specific guidelines.[9] 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 that "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.There was no clear standard for "impossible," and local implementation varied (Chen and Guo, 2022)." 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. In the early years of the implementation of 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).

In China, the law specifies that children complete primary school and middle school education. In most cases, the primary school consists of six grades and the middle school has three grades, making compulsory education last for nine years. A small fraction of the sample attended primary school with five grades, resulting in a total compulsory schooling

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

[9] "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.

duration of eight years. The SSA reform shifts the timing of compulsory schooling one year earlier without extending the duration. 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. Figure 1.2 illustrates the de jure effects of the SSA reform on formal schooling entry and the corresponding compulsory schooling completion age.

1.3 Data and Measures

There are no official statistics on the actual school starting age, according to my inquiry with the Office of Information Disclosure of the Ministry of Education. Due to the lack of direct measures, I rely on indirect measures, such as the year of leaving school and the duration of schooling in retrospective data, or the survey year and grade when surveyed, to estimate the school starting age in historical contemporaneous data. To gain insights into whether the SSA policy affected the actual school starting age, I employ three different sources of data: the 1982 and 1990 censuses, the 2010 wave of the China Family Panel Studies (CFPS), and the 1989 and 1993 waves of the China Health and Nutrition Survey (CHNS). As each source has its own advantages and limitations, I use multiple data sources and measures to provide evidence and increase the credibility of the documented patterns. In the rest of this section, I describe the data sources and how I construct measures to describe the patterns of actual school-starting age using each source of data. As education policies were enforced with different strengths in regions where non-Han Chinese were the ethnic majority, I excluded observations in Xinjiang or Tibet autonomous territories from the sample.

The Historical Chinese Censuses

The National Population Censuses of the People's Republic of China, also referred to as the Chinese Censuses, were conducted by the National Bureau of Statistics and cover all Chinese citizens living in mainland China and those living abroad on temporary visas. Since the establishment of the People's Republic of China on October 1, 1949, seven censuses have been conducted in 1953, 1964, 1982, and 1990, and once every ten years after 1990. In this paper, I use the 1982 and 1990 censuses to estimate the school-starting age distribution of the students who started school before and after the SSA reform. In particular, I use the 1982 census to estimate the pre-reform SSA distribution and the 1990 census to estimate the post-reform distribution. While the initial two censuses were criticized for being crude and subject to measurement errors, with the assistance of various international organizations, the 1982 and 1990 censuses are considered reliable and high-quality by international standards.

Ideally, determining the actual school starting age would involve using the number of years in school at the time of the survey, the survey date, and birthdays. However, the censuses collected only crude information. Educational attainment data was gathered only from individuals aged 6 or older at the time of the survey. In the 1982 census, respondents indicated their education level by selecting from six options: "illiterate", "primary school", "middle school", "high school", "in college or college dropout", "college graduate and above". In the 1990 census, a two-step approach was used: respondents first chose their current highest level of educational attainment from seven options, including "illiterate", "primary school", "middle school", "high school", "secondary vocational school (Zhong Zhuan)", "associate degree (Da Xue Zhuan Ke)", "Bachelor's degree (Da Xue Ben Ke)", and then selected one of four completion statuses: "in school", "graduate", "in complete", or "other". Although the censuses recorded exact dates of birth, these variables are not available for researchers accessing the data through IPUMS International. In the 1982 census, only age was available, while in the

1990 census, both birth year and birth month were available.

Given the large sample sizes and high response rate, the censuses offer reliable insights into overall school enrollment. To utilize this data to understand actual school starting age, I initially narrowed the sample to students aged six to twelve, corresponding to the typical age range for primary school attendance at the time of the census. Individuals who were eligible for higher levels of education were excluded to focus on primary school enrollment. Additionally, individuals from provinces that had not adopted the Compulsory Education Law (CEL) Single Start Age (SSA) policy by 1990 were excluded to ensure the post-reform distribution was captured. I estimated the fraction of individuals ever enrolled in school for each age cohort, defined by the reported age on the census. An individual was considered "ever enrolled in school" if any level of educational attainment other than "illiterate" was reported.

I utilize the estimated cumulative enrollment fraction by age between six and twelve to gain insights into the age at which students first accessed formal schooling. These estimates are informative because, unlike in the United States where it is common for parents to delay their children's school attendance, parents in China typically prefer that their children start formal schooling as early as possible. For instance, nearly every year, representatives to the People's Congress propose lowering the school entry age from six to five and a half so that children born after September can enroll in the same year as peers born before the end of August. The 1982 census sample comprises 1,477,637 observations, while the 1990 census sample consists of 1,233,364 observations.

While the cumulative enrollment rates by age cohort from the censuses offer insight into the pattern of school starting age, having an estimate of the school starting age is more convincing. To complement this evidence, I utilize two other surveys, the China Family Panel Study and the China Health and Nutrition Studies. These surveys have smaller sample sizes and lower response rates but offer more detailed questions that may improve the measure of the actual school starting age.

China Family Panel Study

China Family Panel Studies (CFPS) is a nationally representative, annual longitudinal survey of Chinese communities, families, and individuals launched in 2010 by the Institute of Social Science Survey (ISSS) of Peking University, China. It is the Chinese counterpart of the Panel Study of Income Dynamics (PSID) in the United States. I employ the 2010 baseline survey to construct samples to estimate the distribution of the actual school starting age because it has the largest sample size and is not subject to attrition. In the 2010 baseline survey, the CFPS successfully interviewed almost 15,000 families and almost 30,000 individuals within these families, for an approximate response rate of 79%. Families were selected from 25 provinces. The families living in the following province-level divisions were not included in the sampling process: Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia, and Hainan. The sample for the 2010 CFPS baseline survey was selected using a multi-stage probability with implicit stratification. Each subsample in the CFPS study is drawn through three stages: county (or equivalent), village (or equivalent), and then household.

The CFPS collects information on school leaving year and duration in school in addition to standard educational attainment; therefore, it has been used in the literature to estimate the actual school starting age (e.g., Chen and Park, 2021; Zhang, 2022). Specifically, adult survey participants were asked the following three questions: "In what year did you graduate from or drop out of primary school?" and "The number of years in primary school (years)," and "Have you ever received a primary school diploma?" Additionally, the CFPS collects information on birth year, birth month, and province of residence at age 3. The province of residence at age 3 is the closest to the province of the individual attending primary school. Restricting the sample to people who completed primary school and reported primary school end year and duration, I estimate the school starting age as follows:

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

In this measure of SSA_i, the estimated school starting age for individual i, $EndYear_i$, is the self-reported answer to the question "In what year did you graduate from or drop out of primary school"; Dur_i is the duration coming from the answer to "the number of years in primary school (years)" and "ever received a primary school diploma"; and $BirthYear_i$ and $BirthMonth_i$ are the self-reported year and month of birth. I group the observations whose estimated SSA is younger than 4 or older than 11 into "other" because they are more likely to be subject to recollection errors or other measurement errors. I estimated the integer age at the school entrance instead of the continuous school starting age because it better reflects the impact of the school starting age policy.

To gain insights into the change in the integer age at school entrance, I first estimate the distribution of the frequency of students with each integer school starting age for the five-year cohorts before and after the SSA reform. Specifically, I use cohorts born between 1974 and 1978 school years to illustrate the pre-reform distribution of the estimated school starting age because they were at least age 8 when exposed to the policy. For the post-reform distribution of the actual school starting age, I use cohorts born between 1981 and 1985 because they were at most age 7 when exposed to the reform.

I restrict the sample to individuals who attended ordinary primary schools and had agricultural or non-agricultural Chinese hukou, excluding those from Xinjiang or Tibet. In CFPS, I define the cohort y as a group of individuals whose birth dates are between September 1, $y-1$, and August 31, y, which aligns with the beginning and end of a school year in Chinese schools and is more accurately aligned with the SSA policy compared to calendar-year birth cohorts. Assuming the sample is representative and that the self-reported end year and duration do not systematically differ from the actual values, the CFPS estimates show SSA distributions before and after the reform.

One limitation of only comparing before and after is that the change in the school starting age distribution may simply reflect a time trend. To address this concern, I estimate the distribution for another five-year cohort before the SSA reform but still under the effect of the CEL: the cohorts born between the 1979 and 1973 school years. I compare the two pre-reform five-year cohorts as a placebo to demonstrate that time trends alone cannot explain the shift in SSA distribution. There are 3,076 observations from the cohort 1979-1973, 2,336 observations from the cohort 1974-1978, and 2,048 observations from the cohort 1981-1985.

To understand the magnitude of the shift and the level of compliance, I further narrow the sample to individuals born between 1974 and 1985, who attended at least primary school, and whose estimated school starting age falls between 4 and 11 years old. I then estimate the following Ordinary Least Squares (OLS) model:

$$SSA_i = \beta_0 + \beta_1 Treated_i + \beta_2 Province_i + \epsilon_i$$

The variable SSA_i represents the estimated school starting age for individual i. $Treated_i$ is an indicator variable that takes on the value of one if the individual was younger than age 7 in the year her province of residence adopted the SSA policy, and zero otherwise. $Province_i$ consists of dummy variables representing an individual's province of residence at the age of 3. The standard errors are clustered at the province level at age 3. The coefficient of interest is β_2, which captures the change in school starting age before and after the reform, controlling for time-invariant province-specific factors.

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 errors, and less educated individuals from disadvantaged backgrounds were more likely not to report or misreport the information. Second, the estimated school starting age is influenced by the duration of primary school. A policy that extended the duration of primary school from five to six years was implemented between 1981 and 2005 in China. There might be a concern that the observed change in distribution is driven by the duration policy, as individuals may choose to start school earlier when expecting a longer duration. If the duration change drives the distribution change, one would expect to see a parallel shift to the left, as everyone is incentivized to start one year earlier to compensate for the longer duration. Individuals with older starting ages are more likely to change their school starting age, expecting a longer duration in primary school. Third, with retrospective data, the sample consists of individuals who were still alive in 2010. The literature shows that education is associated with a lower mortality rate, meaning a higher chance of being observed in the 2010 survey. However, mortality is less likely to be a concern because the sample was aged 35 to 36 when surveyed in 2010, and the life expectancy of the cohorts is between 64 and 68. To address these limitations, I employ an additional survey conducted during the adoption of the Compulsory Education Law (CEL).

China Health and Nutrition Studies

The China Health and Nutrition Survey (CHNS) is an ongoing, open-cohort, international collaborative project between the Carolina Population Center at the University of North Carolina at Chapel Hill and the National Institute for Nutrition and Health (formerly the National Institute of Nutrition and Food Safety) at the Chinese Center for Disease Control and Prevention. The CHNS aims to examine the nutritional and health status of the Chinese population while collecting social, economic, and demographic data. Launched in 1989, the survey is conducted every two to four years.

Although not nationally representative, the CHNS covers nine province-level divisions that vary substantially in geography, economic development, public resources, and health indicators. These provinces are Liaoning, Heilongjiang, Jiangsu, Shandong, Henan, Hubei, Hunan, Guangxi, and Guizhou. The survey employs a multistage, random cluster process to draw samples surveyed in each province. Counties within the nine provinces are stratified by income (low, middle, and high), and a weighted sampling scheme is used to randomly select four counties in each province. Additionally, the provincial capital and a lower-income city are selected when feasible, while other large cities must be selected in two provinces. Villages and townships within the counties, as well as urban and suburban neighborhoods within the cities, are selected randomly.

As the provinces adopted the Compulsory Education Law (CEL) between 1985 and 1994, I utilize the first two waves of the CHNS to examine the change in school-starting age distributions. Specifically, 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, the 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 that resided in the sample areas. The CHNS questionnaire includes two questions useful for constructing a measure that can provide insights into the distribution of school starting age: "completed years of formal education in regular school" and whether they were "currently in school." Although the birth month is not available in the public-use data, after restricting the sample to students who were currently in school when surveyed, I can approximate the school starting year using the survey year and the number of years in school. Then, I estimate the difference with the birth year to learn about the school starting age.

I utilize the 1989 CHNS to construct a sample of students who started school before the SSA reform. 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 prefer to have the same pre-reform cohort as CFPS. However, I do not include the 1978 cohort because I cannot determine SSA policy exposure without knowing the birth months. Additionally, I do not include the 1974 and 1975 cohorts because 26.9% of the 1974 cohorts and 16.8% of the 1975 cohorts left school[10]. Since early starters also completed primary school early, they were less likely to be in school when surveyed, making the in-school samples for the 1974 and 1975 cohorts 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. This 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 the age of eight. However, I include the 1976 cohorts to increase the pre-reform sample size. The total number of observations for pre-reform cohorts is 350.

I utilize the 1993 CHNS for the post-reform estimation and include cohorts 1982-1984. They were aged 9-11 when surveyed. I did not include the 1985 cohort because they were too young to have completed enrollment. Although the 1984 cohort was only 9 when surveyed, its in-school fraction is 94.6%, which is comparable to 94.8% for the 1983 cohort and 93.1% for the 1982 cohort. This suggests that the post-reform CHNS estimate may slightly underestimate the fraction enrolled after the age of nine. The total number of observations for post-reform cohorts is 354. Compared to CFPS, the CHNS sample is less likely to be subject to bias caused by selected mortality or selected accuracy in recalling as the surveys were conducted while the students were still in school.

I estimate the distribution of school starting age by cohort. Cohorts in CHNS are defined using the calendar year: cohort y consists of observations with birthdays between January

[10]Having left school is defined as an individual not currently in school but having received at least one year of regular education

1, y, and December 31, y. The ages of the school starting year are estimated for individuals who were currently in school when surveyed. For individual i, the age of the school starting year is estimated as follows:

$$SSA_i = SurveyYear_i - YearsofSchooling_i - BirthYear_i$$

In this measure, $SurveyYear_i$ represents the year in which individual i participated in the CHNS, either 1989 or 1993, depending on the survey wave; $YearsOfSchooling_i$ represents the self-reported answer to the question asking about the "completed years of formal education in a regular school"; and $Birth_i$ represents the self-reported year of birth.

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 to have exited. Based on 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 from 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 bias. Hence, I am limited in estimating which cohorts to use 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. Although the measures using CHNS are cruder and the survey has a smaller sample size and is not nationally representative relative to CFPS, they are less likely to suffer from biases arising from selection into recollection accuracy and selection in survival. This can complement the evidence using CFPS.

1.4 The Changes in School Starting Age Distributions

In this section, I describe the age at which students in China started formal schooling in the 1980s to 1990s and investigate whether the SSA reform changed their school starting age using the data and measures described above. Since the SSA reform lowered the legal school starting age to six, if students complied with the new legal school starting age, I expect to see an increase in the fraction of students starting school at six. Additionally, I anticipate a decrease in the fraction of students starting school at age eight or older. The expected effect for age seven is ambiguous because some provinces initially lowered the legal school starting age from eight to seven before further reducing it to age six. Thus, the direction of change in the fraction of students starting at age seven depends on the relative size moved from eight to seven and seven to six. Moreover, if the minimum age rule was strictly enforced, I expect to observe no one starting school before the age of six.

Even though cohorts and outcomes are defined or measured differently across data sources, the estimated changes in distributions in all three data sources align with the expected effect of the SSA reform. The evidence suggests that it is plausible that the SSA reform affected students' decisions in the timing of enrollment when the policy lowered the legal minimum school starting age.

1.4.1 Evidence using Historical Censuses

I estimate the fraction ever enrolled in school between age six and twelve by age cohort using the 1982 and 1990 Chinese censuses and show the estimates in Figure 1.4. The evidence from historical censuses suggests that the percentage of primary school students by age six and seven was significantly higher after the SSA reform. The increase in the fraction of enrollment in primary school by age twelve also indicates an improved overall enrollment over time.

Compared to children aged six to twelve who participated in the 1982 census, children in the 1990 census had higher enrollment at all age categories, suggesting an overall higher primary school enrollment. However, this increased enrollment may downward bias my main results if lower-ability students were drawn into school due to the SSA policy. Significantly larger fractions of students enrolled in school by age six and seven compared to other age groups. 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. However, the enrollment rate by age 6 is only 0.4, suggesting that a large fraction of localities opted for the age seven option. During the reform period, overall enrollment was potentially increasing for reasons unrelated to the CEL. This suggests that the later cohorts may consist of a higher portion of disadvantaged or lower-ability children.

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 the reporting of younger children's education. I use the same wave of CFPS for both pre-reform and post-reform cohorts to address the data quality change concern further.

1.4.2 Evidence using CFPS

I estimate the distribution of school starting age for the 1969-1973 cohorts, the 1974-1978 cohorts, and the 1981-1985 cohorts, and the estimates are shown in Figure 1.5. The evidence using CFPS suggests that the SSA reform decreased the fraction of students who started school at age eight or older and increased the fraction of students who started school at age six or younger. These changes in the distributions of school starting age are less likely to be explained solely by time trends. Further regression analysis suggests that the initial compliance rate is around forty percent.

Compared to the distribution of the estimated school starting age of the 1974-to-1978 cohorts, the 1981-to-1985 cohorts exhibit a smaller fraction of students who started school at age eight or older and a larger fraction of students who started school at age seven and six. This change in distribution suggests that the SSA reform lowered the school starting age.

As a placebo test, I compared the estimated SSA distribution of the 1979-to-1973 cohorts to the 1974-to-1978 cohorts, both of which started school before the SSA reform. In this comparison, there is a smaller increase in the fraction enrolled at ages six and seven and no change in the fraction enrolled at age eight. Comparing the changes in distribution between the placebo comparison and the SSA reform comparisons suggests that students altered enrollment timing in response to the SSA reform.

It is worth noticing that 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.

To gain insights into the fraction of students who complied with the age six rule, I also estimate the correlation between SSA reform exposure and the average school starting age. The OLS regression estimates are summarized in Table 1.2. Column 1 shows a simple regression of the estimated school starting age and being treated by the SSA reform. Column 2 presents a specification of the effects of SSA reform on the estimated school starting age after controlling for time-invariant province-specific characteristics. Column 3 estimates the same regression as Column 2 but weights each observation using its sampling weight to better reflect the population. The preferred estimates are shown in Column 3. Being exposed to the SSA reform is correlated with a 0.425 younger school starting age on average. This suggests that about 42.5% of the students comply with the new rule. Using alternative specifications does not substantially change the size of the estimate.

1.4.3 Evidence using CHNS

Figure 1.6 shows the estimated age distribution at the end of the school starting year. The age at the end of the school starting year is estimated as the difference between the school starting year and the birth year. The distributions look different from those of CFPS. One potential explanation is that the CHNS sample is surveyed and constructed differently. In particular, the samples are from different provinces and some are from different birth cohorts. Another reason is that the school starting age measures are different. The actual school-starting integer ages are measured using CFPS, while the ages at the end of the school-starting year are measured using CHNS. Therefore, instead of focusing on the exact changes in the shape of the distribution, I focus on the direction of the changes in the distribution before and after the reform.

Compared to the pre-reform cohorts, born in 1976 and 1977 and surveyed in 1989, the post-reform cohorts, born between 1982 and 1984 calendar years and surveyed in 1993, exhibit a larger fraction starting school in the year they turned six and a smaller fraction starting school in the year they turned eight or later. Specifically, the percentage of those who started school in the year they turned six decreased from about 40 percent to around 25 percent. Interestingly, there is little change in the fraction of those who started school in the year they turned seven before and after the reform, which aligns with the evidence using CFPS.

1.5 Conclusion

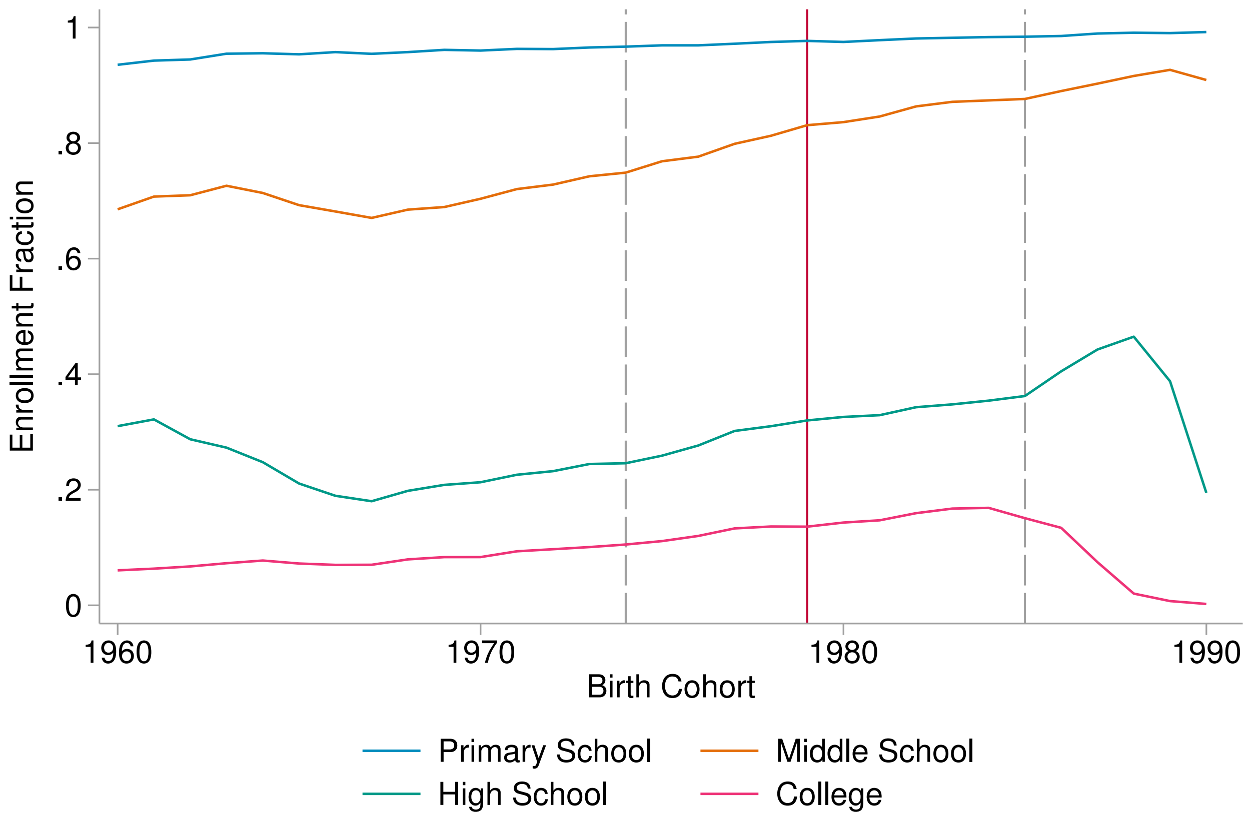
In conclusion, my analysis sheds light on the impact effect of the school starting age reform on the distribution of students' actual school-starting ages in China during the 1980s and 1990s. By utilizing data from multiple sources, including the Chinese censuses, Chinese Family Panel Studies (CFPS), and the China Health and Nutrition Survey (CHNS), I construct different measures of actual school-starting age distributions and provide evidence of changes using data with complementary characteristics. My findings indicate a clear shift in age at enrollment patterns following the implementation of the SSA policy, with a notable increase in the fraction of students starting school at age six and a corresponding decrease in those starting at age eight or later.

The evidence suggests that the SSA reform effectively lowered the school starting age, with a significant portion of students complying with the new minimum age requirement. Moreover, my analysis highlights the importance of considering alternative data sources and methodologies to corroborate findings and address potential biases. While challenges such as recollection errors and data quality variations exist, my results remain robust across different datasets and specifications.

Overall, this chapter contributes to the understanding of the Compulsory Education Laws and their effects in China and underscores the significance of policy reforms in shaping enrollment patterns and access to education. The documented change in actual school starting age serves as a robust foundation for further analysis of policy effects on various outcomes. Moreover, the findings of this chapter make a valuable contribution to the literature on school starting age and the broader field of economics of education. By identifying a plausible and significant policy change, this research paves the way for future studies examining the effects of absolute school starting age on students' development. Additionally, the established first stage presents opportunities for exploring spillover effects in labor economics, such as investigating the impact of early formal schooling on parental labor supply in urban areas and household production in rural areas.

1.6 Figures

Figure 1.1: 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.

Figure 1.2: The Compulsory Schooling Starting and Completion Ages

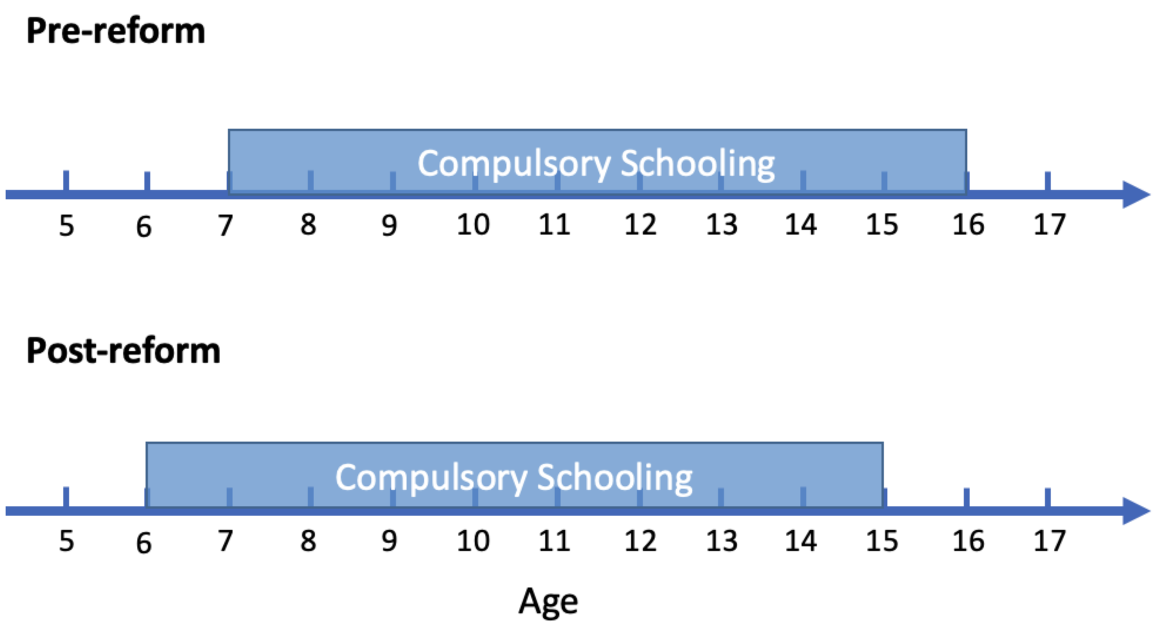
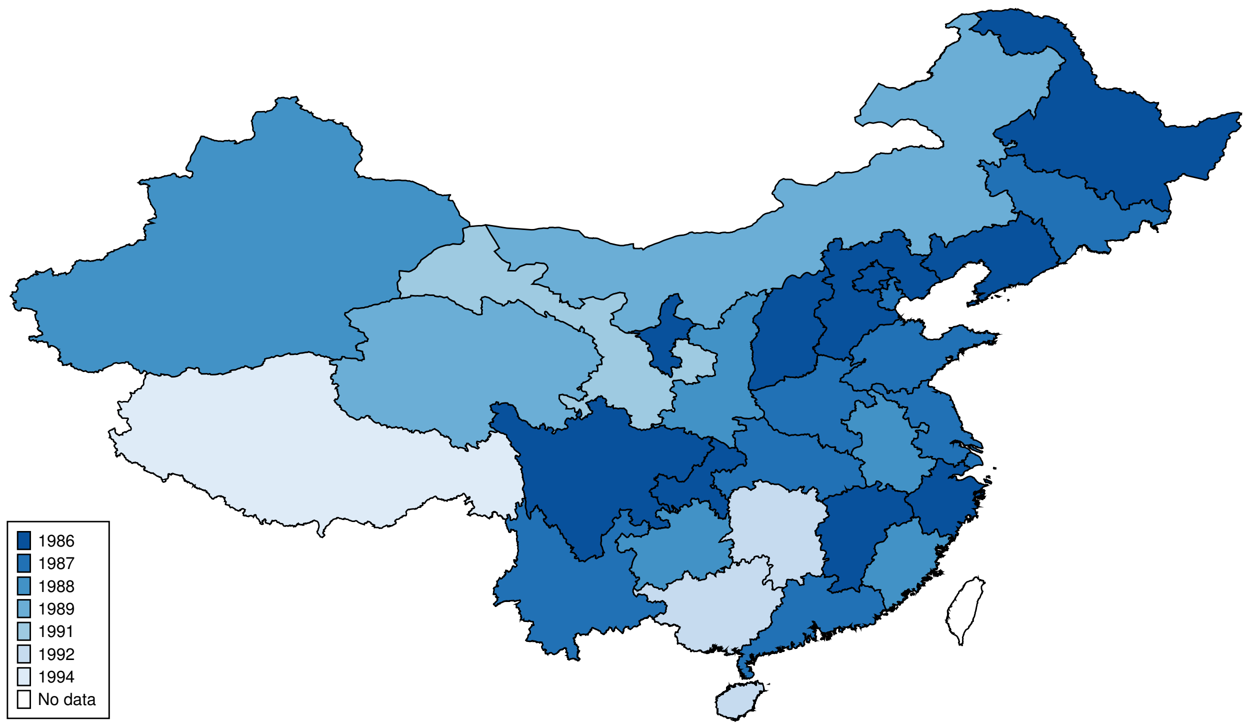
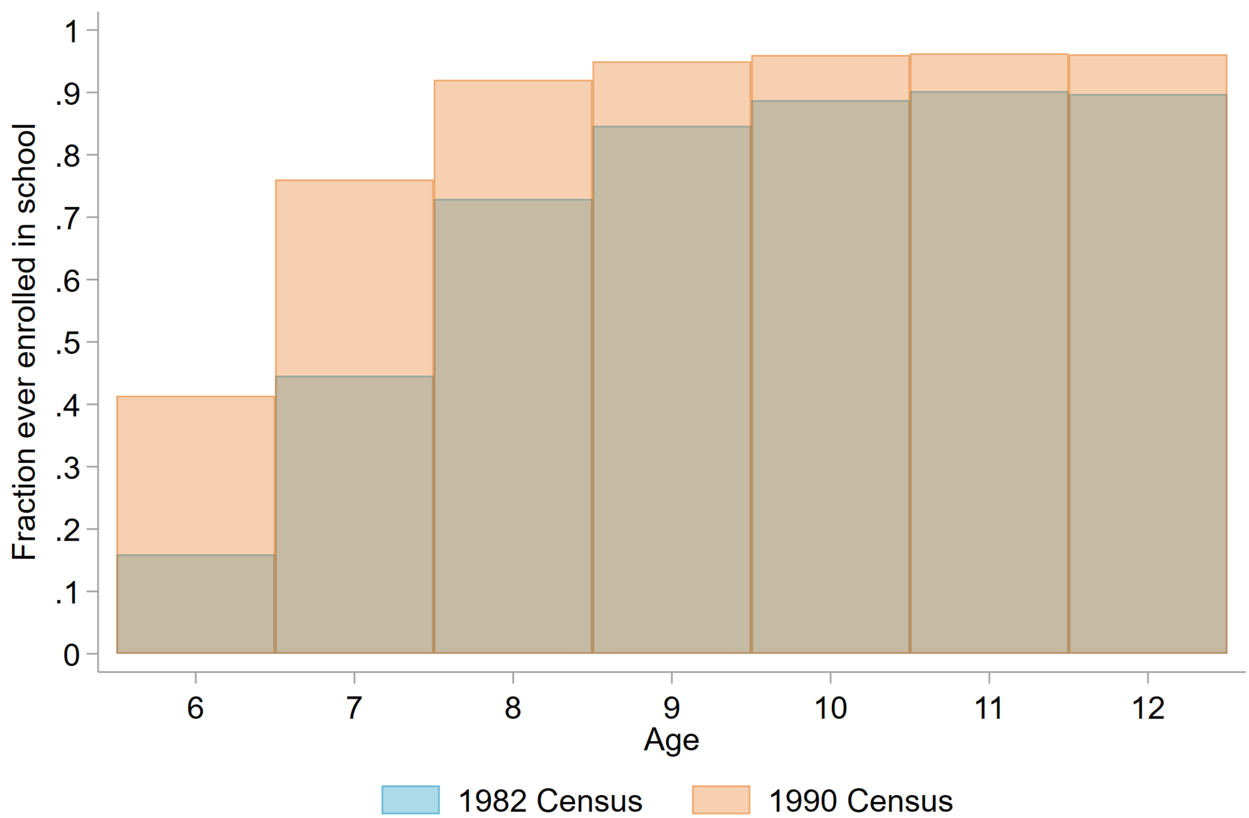


Figure 1.3: SSA Reform Adoption Map



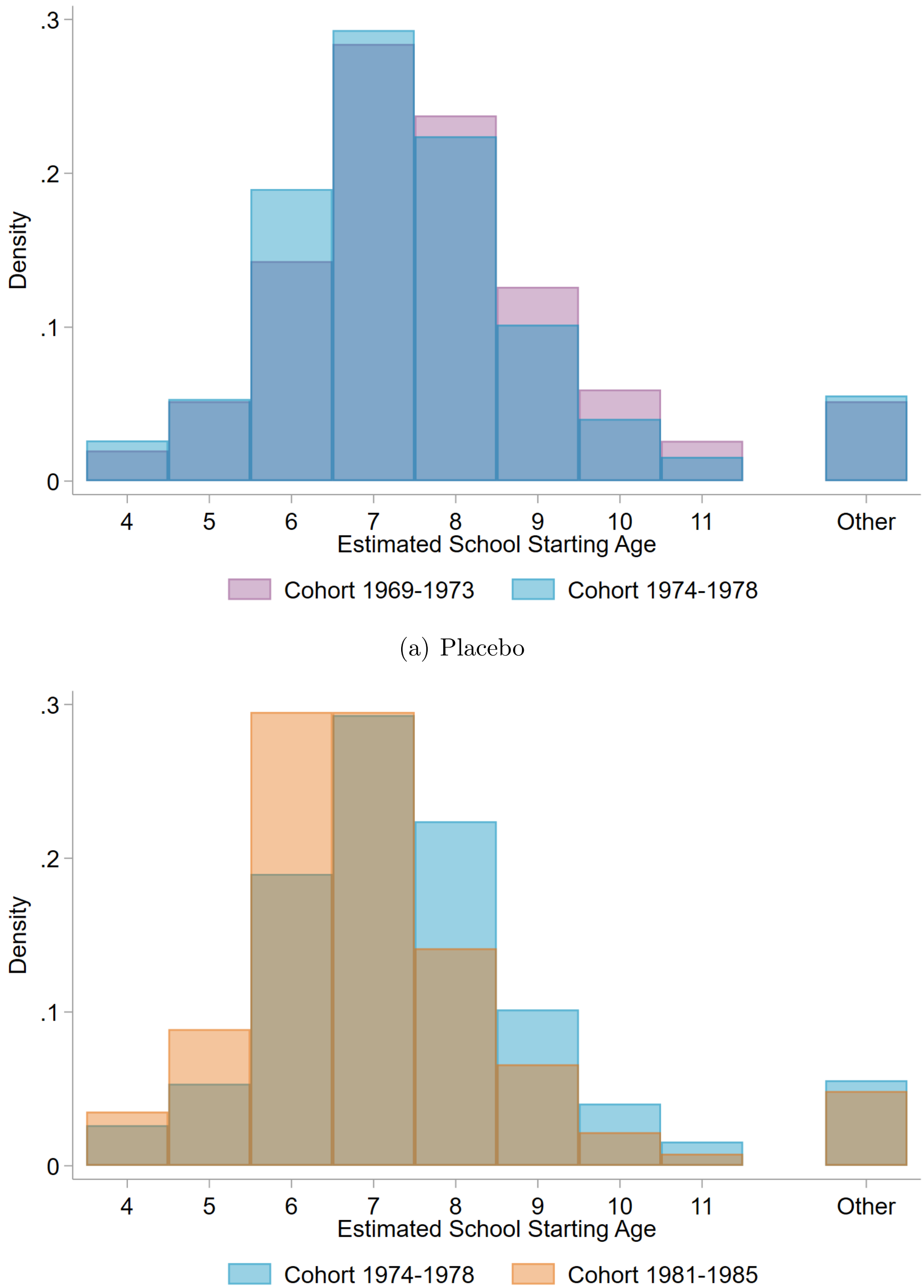
Notes: The color of each province is indicative of its school year of SSA reform adoption, as detailed in the legend. A school year starts on September 1 and ends on the following August 31. For example, the school year 1985 started on September 1, 1985, and ended on August 31, 1986. Xinjiang and Tibet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

Figure 1.4: Fraction Ever Enrolled in School by Age using Chinese Censues



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

Figure 1.5: The Distributions of Estimated Actual Age at School Entry using CFPS

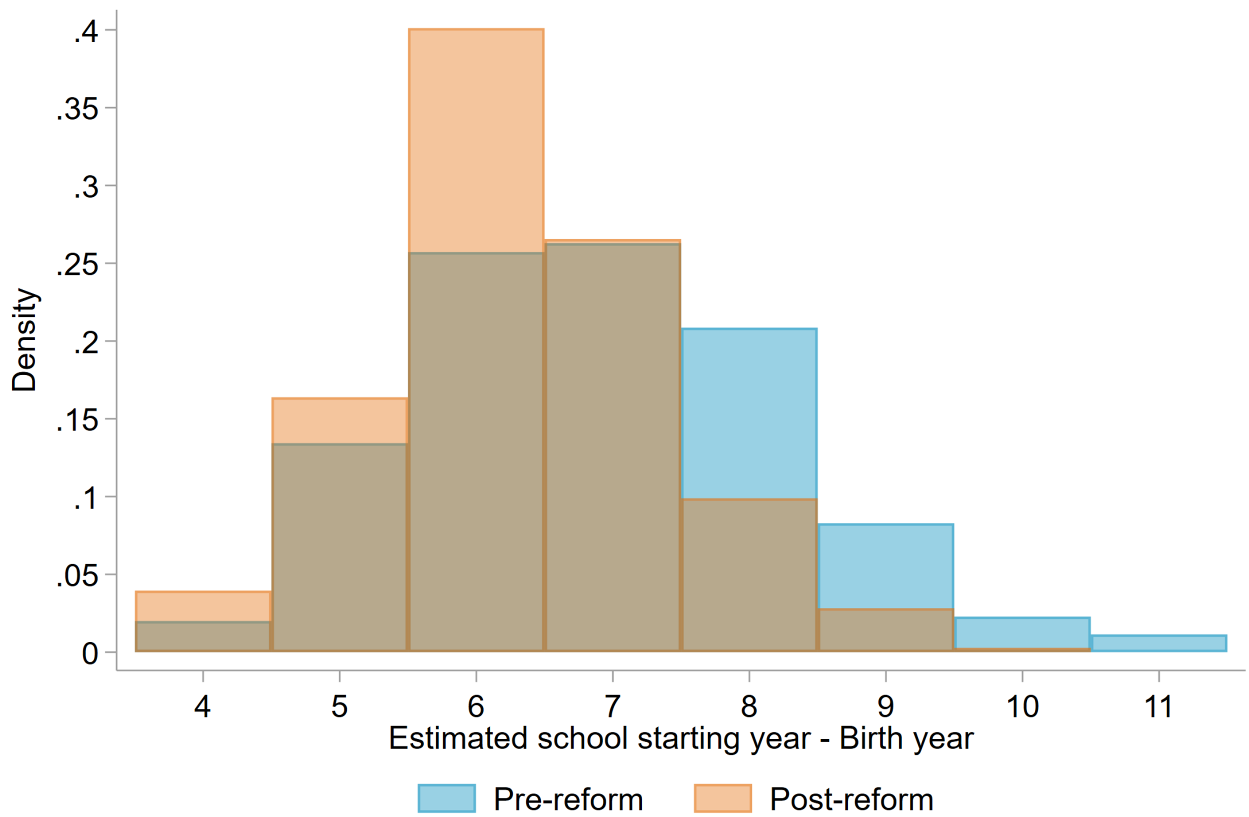


(a) Placebo

(b) SSA Reform

Notes: 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, duration in school, birth year, and birth month. Data source: China Family Panel Studies 2010.

Figure 1.6: The Distributions of Estimated Age at the End of School Starting Year using CHNS



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

1.7 Tables

Table 1.1: Implementation Date of Compulsory Education Law in Difference Provinces

Province	Implementation Date
Zhejiang	9/1/1985
Jiangxi	2/1/1986
Heilongjiang	7/1/1986
Liaoning	7/1/1986
Hebei	7/1/1986
Shanxi	7/1/1986
Ningxia	7/1/1986
Sichuan	7/1/1986
Chongqing	7/1/1986
Beijing	7/8/1986
Jiangsu	9/9/1986
Shanghai	9/10/1986
Shandong	9/12/1986
Henan	10/1/1986
Guangdong	10/7/1986
Yunnan	10/29/1986
Tianjin	11/6/1986
Jilin	2/9/1987
Hubei	3/1/1987
Shaanxi	9/1/1987
Anhui	9/1/1987
Guizhou	1/1/1988
Xinjiang	5/28/1988
Fujian	8/1/1988
Inner Mongolia	9/15/1988
Qinghai	10/1/1988
Gansu	9/3/1990
Hunan	9/1/1991
Guangxi	9/1/1991
Hainan	12/16/1991
Tibet	7/1/1994

Note: The dates are from Decisions about Compulsory Education Law, which can be found on each provincial government's website and complied by Chen and Park (2021)

Table 1.2: The Size of Effects of SSA Reform on Estimated School Starting Age

	(1) OLS	(2) OLS	(3) OLS
SSA Reform	-0.394***	-0.390***	-0.425***
	(0.066)	(0.035)	(0.040)
Province FE	No	Yes	Yes
Weighted	No	No	Yes
N	4065	4065	4065

Note: 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. Standard errors are clustered at the province level. Individual sampling weights are used if weighted. Data source: China Family Panel Studies 2010.

Chapter 2

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

2.1 Introduction

Policies governing the age at which formal schooling begins vary across countries. For example, in the United Kingdom, children begin primary school at the age of five, whereas in Finland, children begin at the age of seven. Does a specific school starting age policy prove more effective in fostering human capital development than others? To answer this question, it is crucial to understand how the school starting age impacts long-term human capital accumulation.

Theoretically, it is ambiguous how the school starting age affects human capital accumulation. The capacity to learn may vary by age (Knudsen et al., 2006). On the one hand, children who start school at a younger age may experience early brain development (Nelson, 2000) and foster cognitive skills in early grades (Suziedelyte and Zhu, 2015). These early skills might enhance the children's ability to acquire later skills, leading to increased human capital accumulation in later grades (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 some policymakers and parents seem to suggest that they believe that children benefit from later school entry. For example, Deming and Dynarski (2008) documents the trend toward increasing school starting age in the United States. Many states have moved their kindergarten-entry cutoff date earlier in the school year, which increased the average 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.

Empirically, there is little direct evidence on the effect of school starting age. A number of studies have estimated the effects of school starting age by comparing the children born after the cutoff dates to the ones born before. 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) However, the estimates cannot separately distinguish the effect of starting school at a younger age from the peer effects that may result from being younger than classmates. 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 kindergarten at five, while those born on September 2 enter at nearly age six. Consequently, the September-2-born children start school at an older age and remain among the oldest in their grade throughout their school years.A few studies directly estimate the school starting age effects using school-entry policy changes 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 remains unknown if the effects of school starting age on test scores translated into improved long-run educational attainment.

To the best of my knowledge, this paper is the first to causally estimate the effects of school starting age 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 birth date cutoff for enrolling in the current academic year and the required duration of schooling unchanged. The SSA reform was gradually adopted across all provinces between 1985 and 1994. 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 Chinese census microdata.

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 of the SSA reform effects on years of schooling is comparable to estimated effects of a one-year increase in the legal minimum school leaving age in the United States and Canada (Oreopoulos, 2006). In particular, 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. 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.

To better understand these results, I conduct heterogeneity analyses and find more pronounced effects in provinces with more preschool education resources. This aligns with the persistent test score effects of a younger primary school starting age, estimated among students with preschool access (Ryu et al., 2020). As the provinces with more preschool education resources may also possess higher overall resources, including higher regional GDP per capita, I cannot rule out alternative explanations, such as differences in existing infrastructure when reform occurred. Some prior studies find treatment effects vary by gender. 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. Unlike these previous findings, I find the effects have little difference by gender. My findings are consistent with the theory of early childhood development, suggesting that starting primary school at age six may enhance early skills formation, which in turn fosters long-term human capital accumulation through dynamic complementarity (Cunha and Heckman, 2007).

This paper contributes to the literature on school starting age in two important ways. First, this paper provides new evidence to the sparse literature that directly evaluates the overall impact of school starting age policy. Policymakers can influence school starting age by changing the birth date cutoff for entering school in the current school year or changing age requirements for school entry. For example, in 2010, California signed a legislature that changed its kindergarten cutoff date from 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. This paper evaluates a policy that changed the age requirement at school entry and kept the cutoff date unchanged. Unlike policies that change the cutoff date, policies that change the age by which students are eligible for primary school enrollment directly affect all children regardless of birth month, and maintain the distribution of ages within a given grade. 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. In a setting closest to mine, Rosa et al. (2019) estimates that a 2006 Brazilian reform that lowered the starting age in primary school from age seven to age six substantially increased fifth-grade math and reading scores. I complement the existing findings by looking beyond test scores to completed education to understand whether these effects persist into adulthood.

Second, this paper offers a potential explanation of the mixed results on educational attainment in prior studies that use variation in birthdays relative to the cutoff dates to estimate the school starting age effects. In particular, most prior studies use assigned age at enrollment based on birthdays and school entry policies or the discontinuity in age at enrollment around the cutoff dates as instruments for actual age at enrollment. Using these approaches, Bedard and Dhuey (2006), Puhani and Weber (2007), Kawaguchi (2011), Fredriksson and Öckert (2014), Peña (2017), and Guo et al. (2023) find that older age at enrollment has positive effects on educational attainment; Angrist and Krueger (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 no effect. One problem with these approaches is that the observed effects capture multiple factors, including the school starting age effect and the peer effects, which may move in different directions. My findings offer a potential way to resolve this problem by isolating the school starting age effects. I find that there are positive long-term effects associated with a younger school starting age, suggesting the inconclusive results in prior literature might be driven by the differences in the size and direction of the peer effects across countries.

My empirical strategy also advances previous analyses of China's Compulsory Education Law (CEL). The enactment of CEL encompassed many policy changes governing different aspects of education, such as compulsory duration, school starting age, infrastructure, costs, school finance, and teacher training. The SSA reform is one of eighteen policies specified by the CEL. While previous studies have shown that the CEL policies jointly increased educational attainment (Fang et al., 2012; Huang, 2015), the specific contributions of each policy remain unclear. Understanding the impact of each policy is crucial for effective policymaking. I isolate the effect of the SSA reform from other CEL policies by comparing cohorts exposed to CEL when they were under primary school enrollment eligible age to those after they had already enrolled in school. Since other CEL policies treated both under-age and already-enrolled cohorts, this comparison isolated the effects of the SSA reform. 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 improve the estimates of the old for grade effects. Both studies rely on the estimated school starting age mode from survey data with relatively small samples to assign treatment. This paper differs from these two studies because I estimate the reduced-form policy effects instead of the individual effects of being old for grade. Therefore, this paper captures the average treatment effect of gaining access to formal schooling earlier across all birth dates, making it more informative for policy analysis. My empirical strategy has two additional advantages. First, I assign treatment based on the stated policy, supported by pre-reform school starting age distribution analyses from various data sources. This approach minimizes the potential for measurement errors in treatment assignments. Second, I utilize a heterogeneity-robust difference-in-differences estimating method (Callaway and

Sant'Anna, 2021). This method ensures that my estimates are not susceptible to potential issues that can arise in traditional event studies, 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 2.2 describes the data, variables, and sample constructions. Section 2.3 describes the empirical strategy. Section 2.4 presents the main results. Section 2.5 check robustness under alternative specifications and rule out potential confounding factors. Section 2.6 presents heterogeneity analysis and discusses potential mechanisms. Section 2.7 concludes.

2.2 Data

The Compulsory Education Law (CEL) was passed by the National People's Congress on April 12, 1986. The 1986 CEL regulated various aspects of education, encompassing compulsory duration, school starting age, infrastructure, costs, school finance, and teacher training. In this paper, I focus on the policy regulating school starting age (SSA Reform) that lowered the legal minimum school starting age from age seven to age six. Details of the CEL and the SSA reforms are discussed in chapter 1.

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

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

[1] "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.

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,[2] suggesting the SSA reform unlikely affected the primary school access timing for children exposed at age eight. I present descriptive statistics of individual characteristics by SSA reform treatment in Table 2.1.

As the SSA reform treatment only varies at the hukou province and birth cohort level,[3] I collapse the sample into province-cohort cells, and the cohort is used as a time dimension

[2] 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.

[3] 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.

for estimation. The cell-level variable is the average of all individuals with the same hukou province and from the same birth cohort.

2.3 Empirical Strategy

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 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.[4] All provinces with the same first SSA reform exposed cohort are considered to belong to the same treatment timing group. Figure 2.1 shows the distribution of SSA reform treatment status over six treatment timing groups. All provinces adopted the SSA reform.

2.3.1 Isolating the SSA Reform from the CEL

Provincial authorities decide the adoption date for the CEL. All CEL policies, including the SSA, had the same adoption date. The provincial adoption dates for the CEL ranged from September 1, 1985, to July 1, 1994. Although the adoption date for all policies within the CEL bundle was the same for each province, the treated cohorts of each policy were different. Figure 2.2 illustrates a simplified step function of policy exposure. The treatment of "CEL other" was immediately effective after adoption because teachers or school officials could apply methods to keep students in school during the academic year. The treatment of the "SSA" policy was effective the following school year after adoption because admission decisions only affected students who started school in the next academic year.

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

As suggested in Figure 2.2, I can isolate the SSA reform from other CEL treatments as there were variations in treatment by cohorts. The first treated cohorts of the SSA reform were those who had not yet turned seven, the pre-reform legal minimum school starting age, at the time their province of residence adopted the CEL bundle. On the other hand, the first treated cohorts of the CEL policies other than the SSA reform were those in their last year of middle school, aged 15 or 16, when their hukou provinces adopted the CEL policies. In other words, the cohorts who had not started school when exposed to the CEL bundle were additionally treated by the SSA policy, compared to those who had started school but had not yet finished middle school at CEL exposure.

2.3.2 Difference-in-Differences with Staggered Adoption

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 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.[5] 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 2.2 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.

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

[5]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.

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.

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

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

$$\widehat{ATT}(g,k) = \frac{1}{N_g} \sum_{j:G_j=g} [Y_{j,k} - Y_{j,g-1}] - \frac{1}{N_{\mathcal{G}_{comp}}} \sum_{j:G_j \in \mathcal{G}_{comp}} [Y_{j,k} - Y_{j,g-1}] \tag{2.1}$$

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

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

$$\text{pseudo-}\widehat{ATT}(g,k) = \frac{1}{N_g}\sum_{j:G_j=g}[Y_{j,k} - Y_{j,k-1}] - \frac{1}{N_{\mathcal{G}_{comp}}}\sum_{i:G_i\in\mathcal{G}_{comp}}[Y_{j,k} - Y_{j,k-1}] \tag{2.2}$$

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:

$$\widehat{\theta}_{es}(e) = \sum_{g=1979}^{1984} \hat{\omega}_{g,e}\widehat{ATT}(g, g+e) \tag{2.3}$$

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

The event-study estimates are my primary estimate of interest. As the first SSA reform exposed cohort had an exposure age of seven, the event-study estimates, $\widehat{\theta}_{es}(e)$, represent the weighted average of group-cohort-specific ATT estimates from the cohorts with exposure age $7 - e$. I interpret them as the dynamic treatment effect e-periods after the initial exposure. For example, event time three estimates, $\widehat{\theta}_{es}(3)$, are the average group-cohort-specific ATTs of all cohorts exposed to the SSA reform at age four. I interpret the estimate as the treatment effect of the SSA reform three years after the initial adoption. 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.[6] 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 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

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

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.[7] Students and parents in late-adopting provinces accumulated more knowledge prior to SSA reform exposure through the Ministry of Education's CEL advocacy events.[8] 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.

2.4 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

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

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

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.[9] While in China, high school enrollment is a strong predictor of high school graduation and thus has a similar labor market return,[10] 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.

2.4.1 Main Results

Figure 2.3 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.[11] The consistently positive post-reform point estimates 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

[9]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 2.9.

[10]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 2.9.

[11]The pre-period effects presented in Figure 2.3 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.

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.[12] 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 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 2.4 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

[12]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.

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.

2.4.2 Alternative Specifications using CSDID

Table 2.3 summarizes the overall effects using various specifications. Column 1 shows the overall effects of the baseline specification. Panel A corresponds to Figure 2.3 and Panel B corresponds to Figure 2.4. 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 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.

2.4.3 Two-Way Fixed Effects Regression Specification

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

$$y_{j,k} = \sigma_k + \lambda_j + \sum_{e=-5}^{6} \delta_e D_{e(j,k)} + \epsilon_{j,k}, \tag{2.4}$$

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

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

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

Figure 2.5 presents the event time coefficients on high school enrollment and their 95% confidence interval based on the dynamic TWFE model in Equation 2.4. 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 2.6.

2.5 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 2.7, 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 2.8, 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 2.4, 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.[13] Therefore, the increased high school enrollment is unlikely to be driven by an increase in education supply.

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 2.5, no detectable effects are found, suggesting a limited compositional change in prescribed compulsory schooling duration.[14] 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, and the implementation, such as monetary penalties for unauthorized birth, varied across

[13] 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.

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

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 2.5. 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 2.5.

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 2.3 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.

2.6 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 2.9, Figure 2.10, and Figure 2.11. Estimates on high school graduation show very similar patterns and are shown in in Figure 2.12, Figure 2.13, and Figure 2.14. 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 2.3. 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 2.15), 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 pursing 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 2.9 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 2.10 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 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. Figure 2.11 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. Table 2.6 suggests a lower 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 2.10. Admitting there are unobservable characteristics, such as teacher quality or school facilities contributing the 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.

2.7 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

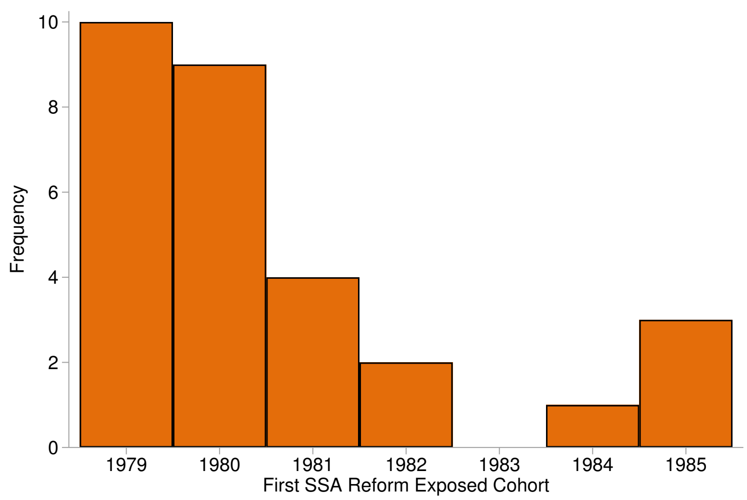
(2020). As peer effects can be sensitive to culture and educational settings, such as tracking or comprehensive (Peña, 2017), the direction or magnitude can vary substantially across countries. On the other hand, the timing effect has its roots in human developmental theory and is, therefore, more likely 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).

2.8 Figures

Figure 2.1: Distribution of SSA Reform Adoption Timing

(a) Cumulative Percentage of Provinces



(b) Number of Provinces

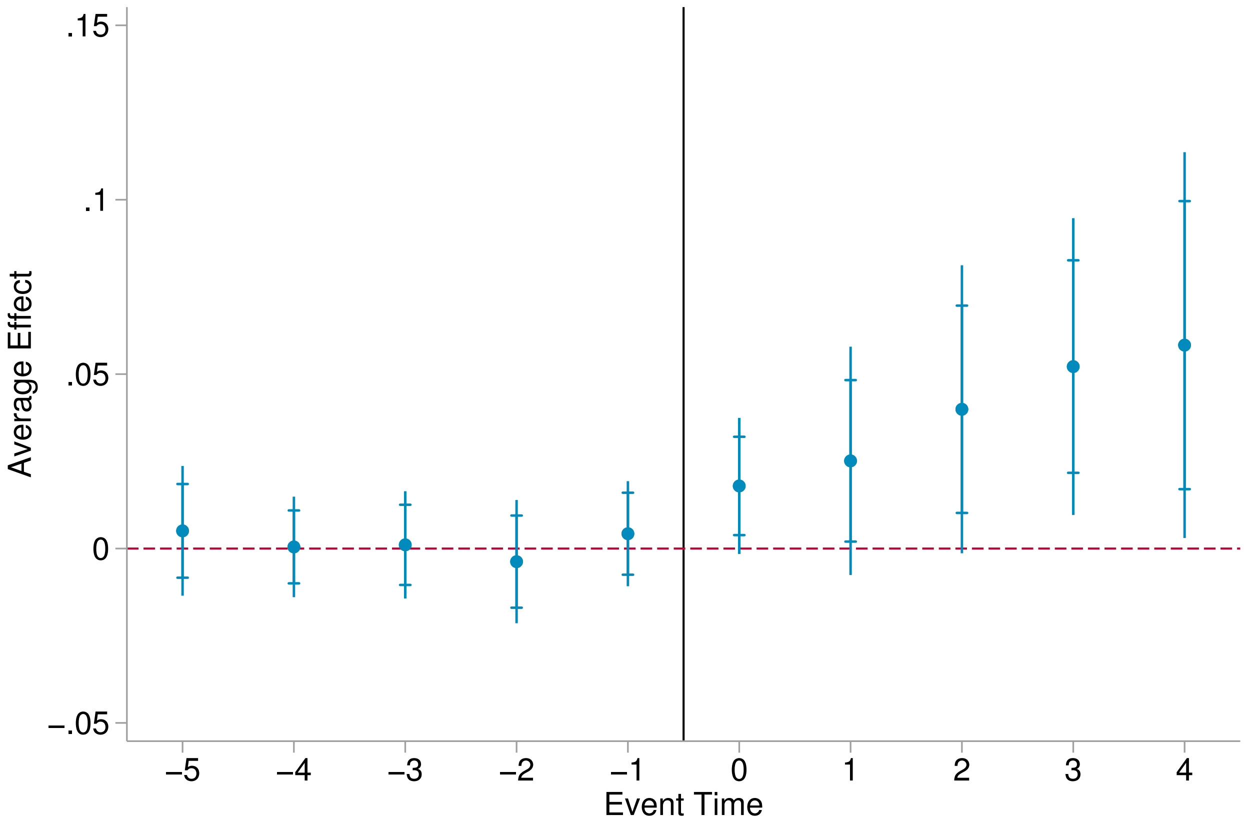
Notes: A school year starts on September 1 and ends on the following August 31. For example, the school year 1985 started on September 1, 1985, and ended on August 31, 1986. The population data were from the 1982 census. Since Xinjiang and Tibet are excluded from the main analytical sample, they are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

Figure 2.2: The Bundle of Treatment by Birth Cohort and Province

Cohort (k)	Treatment Timing Group (g) 1979	1980	1981	1982	1984	1985
1974	CEL other	CEL other	CEL other	CEL other		
1975	CEL other	CEL other	CEL other	CEL other		
1976	CEL other	CEL other	CEL other	CEL other	CEL other	
1977	CEL other	CEL other	CEL other	CEL other	CEL other	CEL other
1978	CEL other	CEL other	CEL other	CEL other	CEL other	CEL other
1979	CEL other + SSA	CEL other	CEL other	CEL other	CEL other	CEL other
1980	CEL other + SSA	CEL other + SSA	CEL other	CEL other	CEL other	CEL other
1981	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other	CEL other	CEL other
1982	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other	CEL other
1983	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other	CEL other
1984	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other
1985	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other + SSA	CEL other + SSA

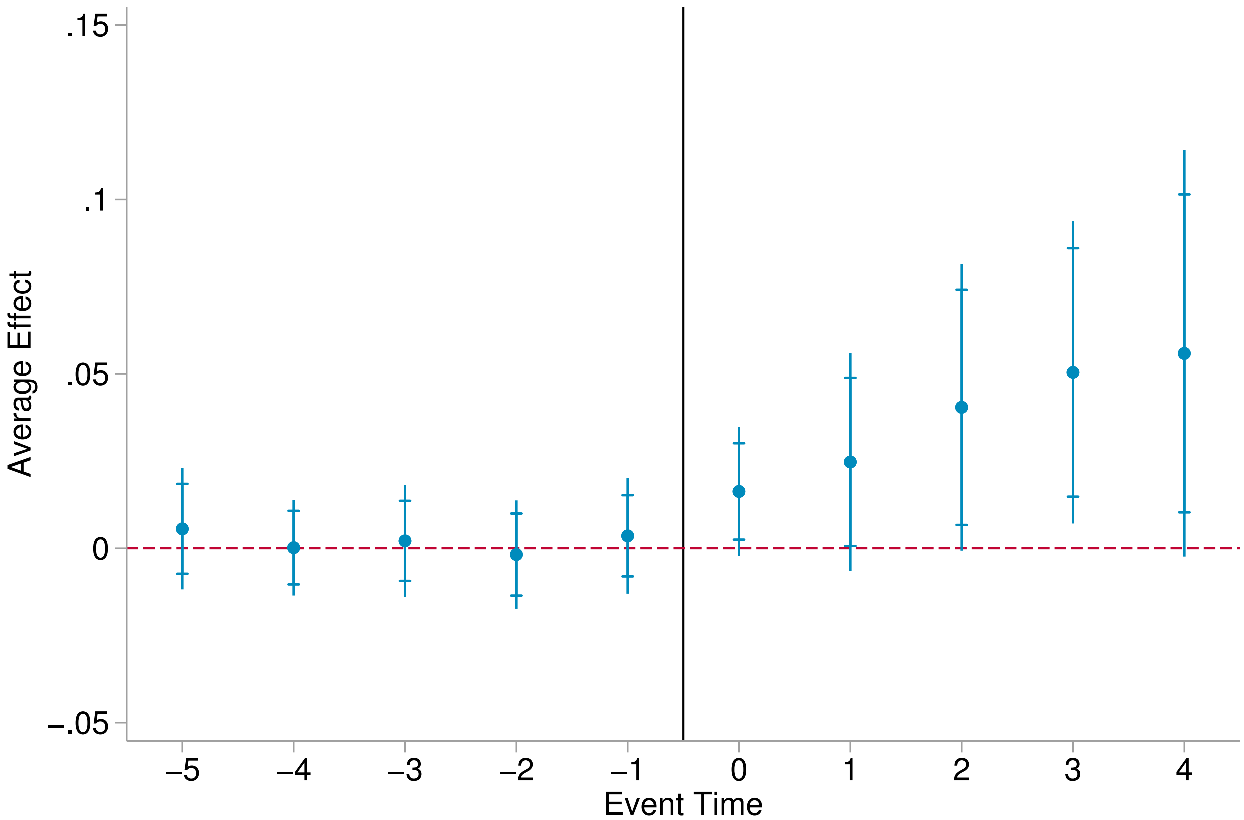
Notes: Cohort is defined using the Sep 1 school year cutoff: Cohort Y is defined as a birth date between September 1, Y-1, and August 31, Y. For simplicity, in this graph, an individual is considered treated by compulsory schooling policy ("9-yr") if she was age 14 or younger when her local province adopted CEL and treated by the starting age policy ("SSA"), if she reached age 7 or younger by the first school year after the CEL adoption. No provinces adopted the CEL between September 1, 1989, and August 31, 1990. Tibet adopted the CEL in July 1, 1994. It was excluded from this illustration because the residence of Tibet was not included in the analytical sample of this study.

Figure 2.3: The Effects of SSA Reform on High School Enrollment



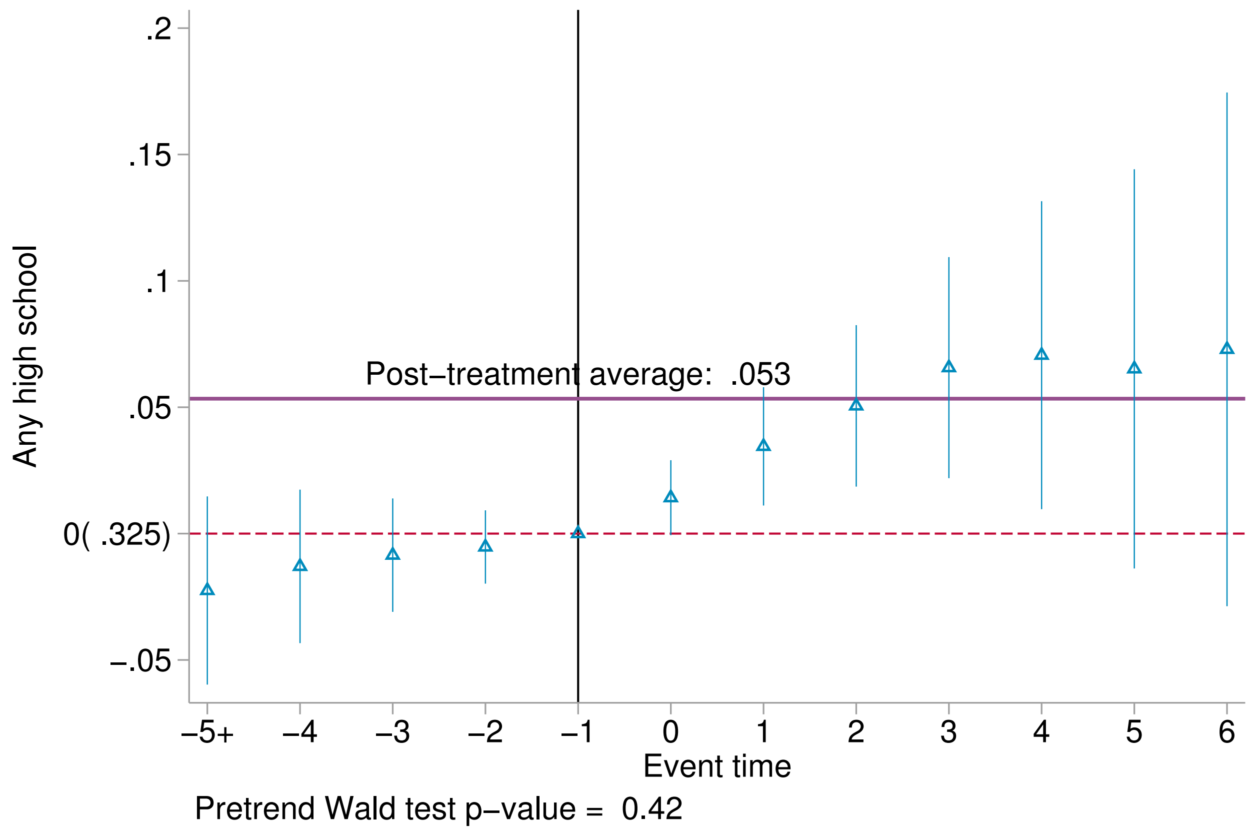
Notes: The sample include 29 provinces.The pre-reform average high school enrollment was 0.283. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method. Standard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

Figure 2.4: The Effects of SSA Reform on High School Graduation



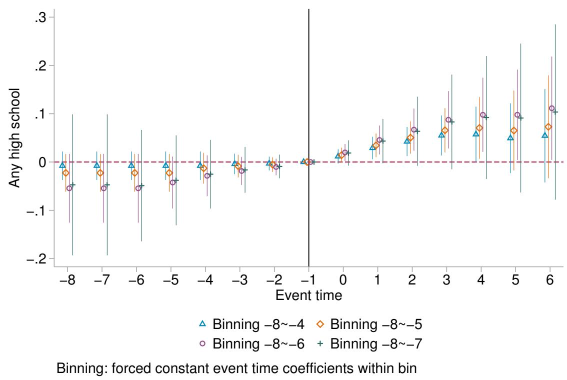
Notes: The sample include 29 provinces. The pre-reform average high school graduation was 0.279. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method. Standard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

Figure 2.5: Estimated Effect of School Starting Age Rule on High School Enrollment

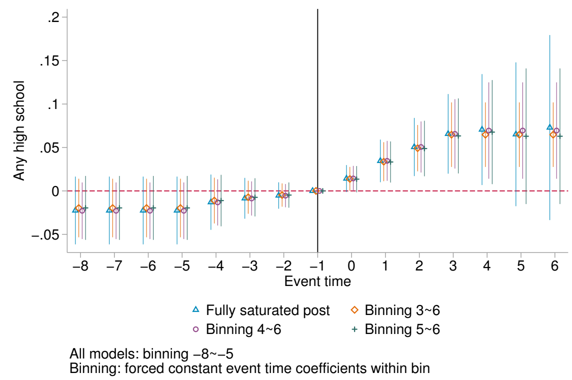


Notes: This figure shows the baseline dynamic TWFE estimates of exposure to the school starting SSA reform at different ages on the probability of completing high school. The pre-treatment coefficients from -8 and -5 are binned. The standard errors are clustered at the province level. Observations exposed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

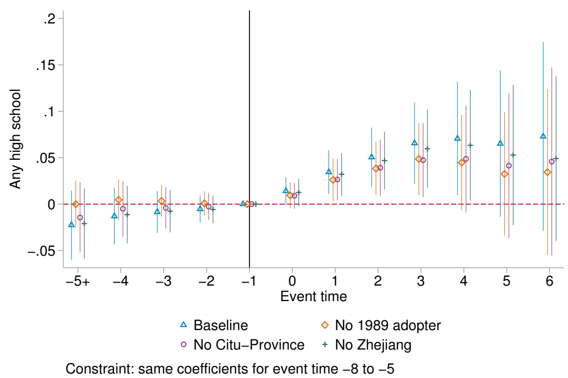
Figure 2.6: Sensitivity Analysis



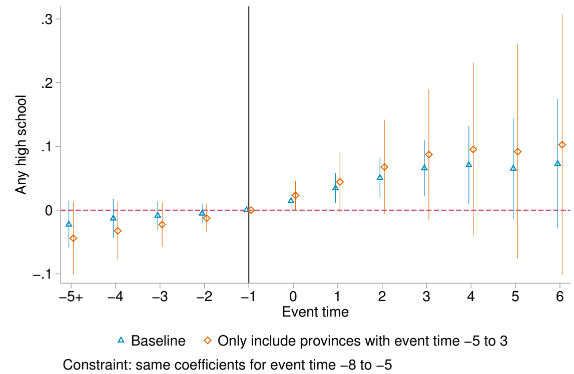
(a) Various Pre-treatment binning



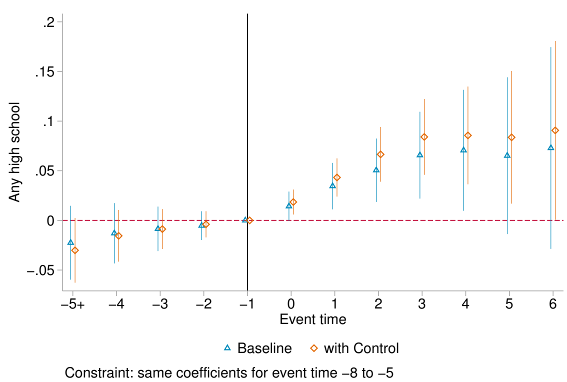
(b) Various Post-treatment binning



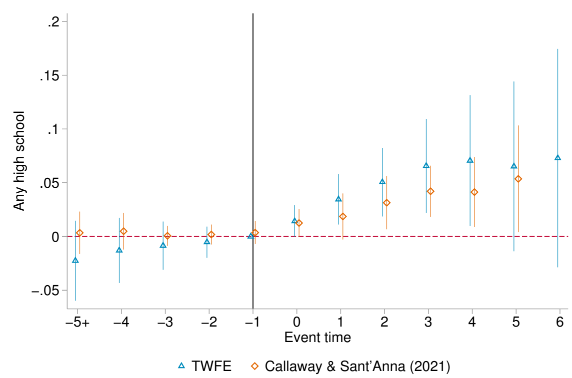
(c) Exclude special group



(d) Balanced panel



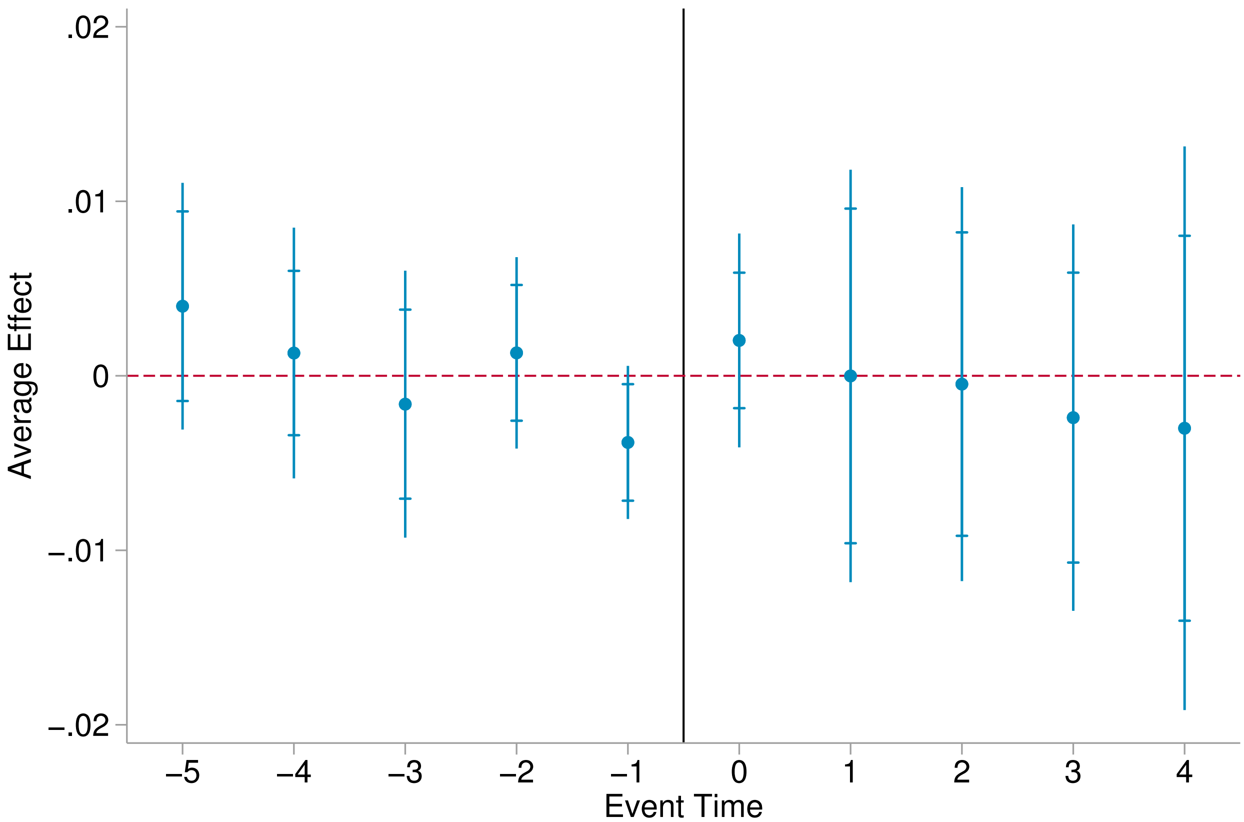
(e) Add Control



(f) Hetero-Robust Estimator

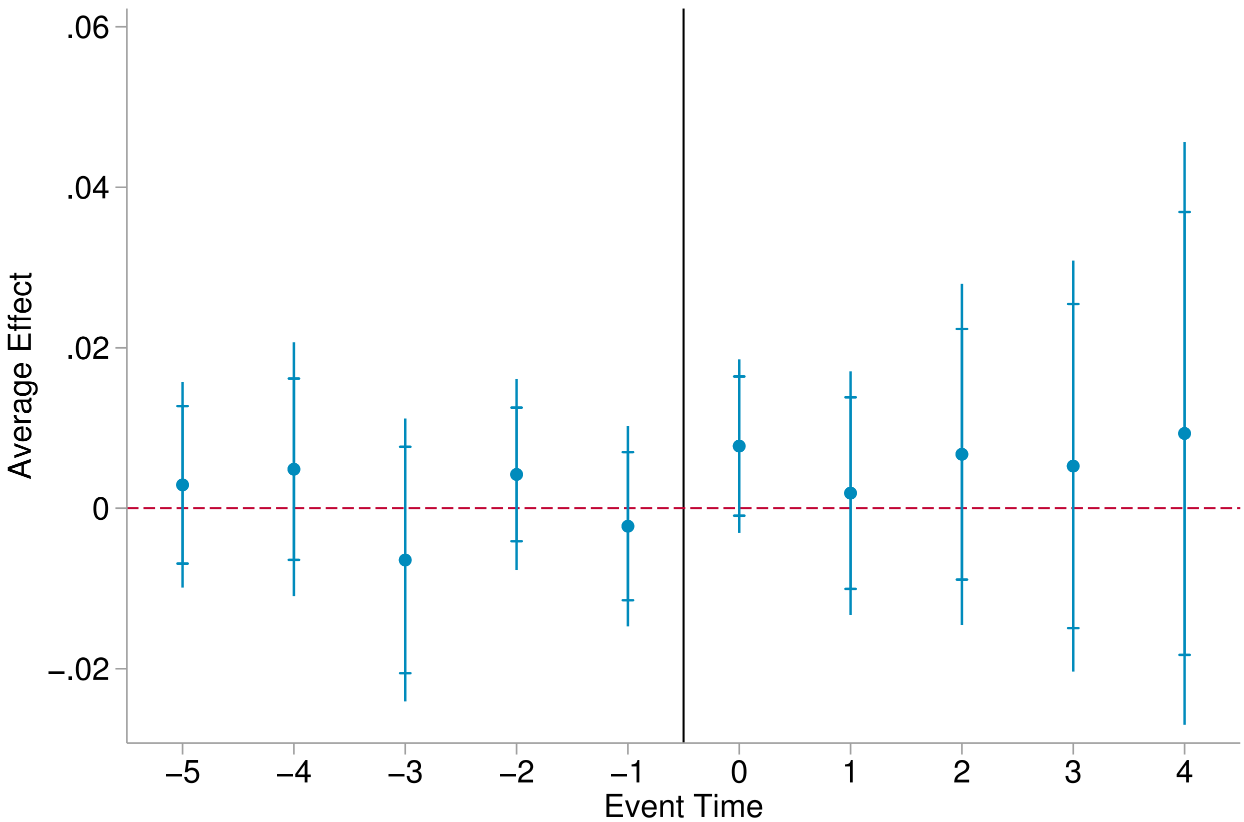
Notes: These figures show the dynamic estimates of exposure to the school starting SSA reform at different ages on the probability of completing high school under alternative specifications.

Figure 2.7: The Effects of SSA Reform on Primary School Enrollment



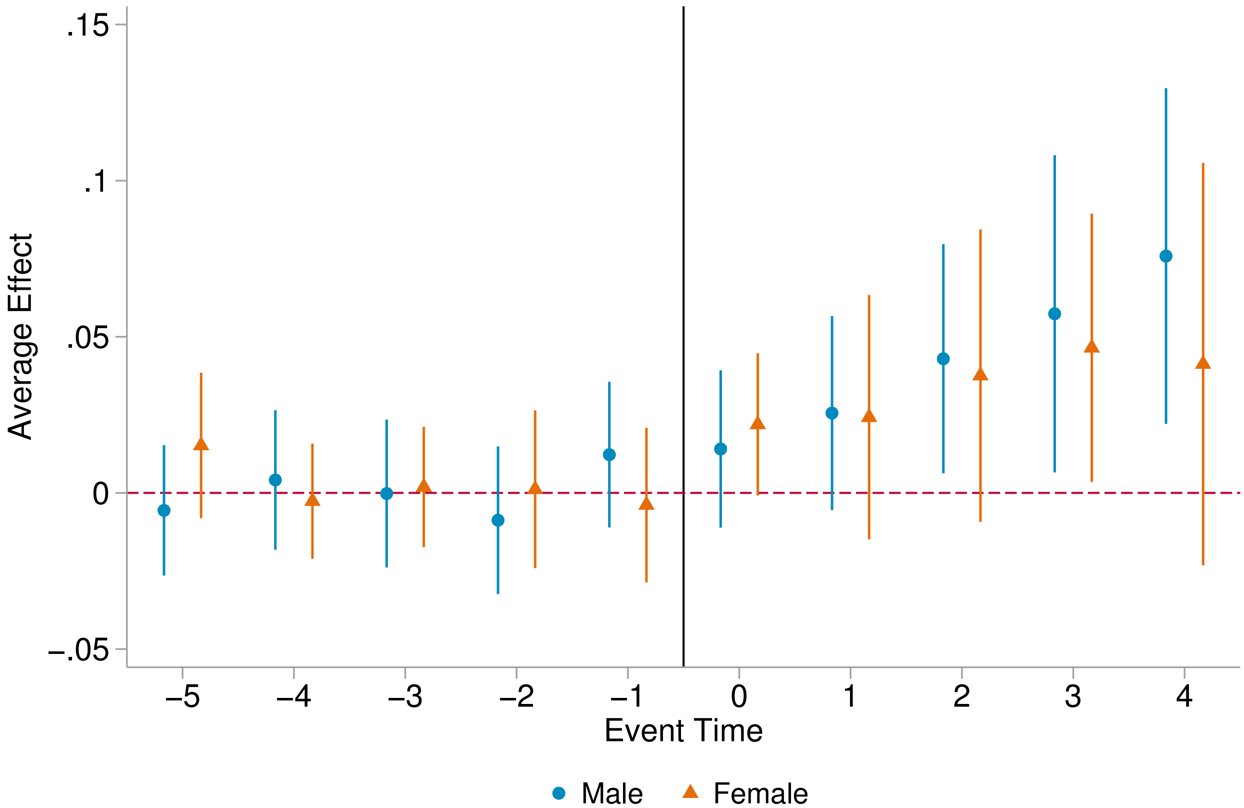
Notes: The sample include 29 provinces. The average pre-reform primary school enrollment rate was 0.971. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method.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

Figure 2.8: The Effects of SSA Reform on Middle School Graduation



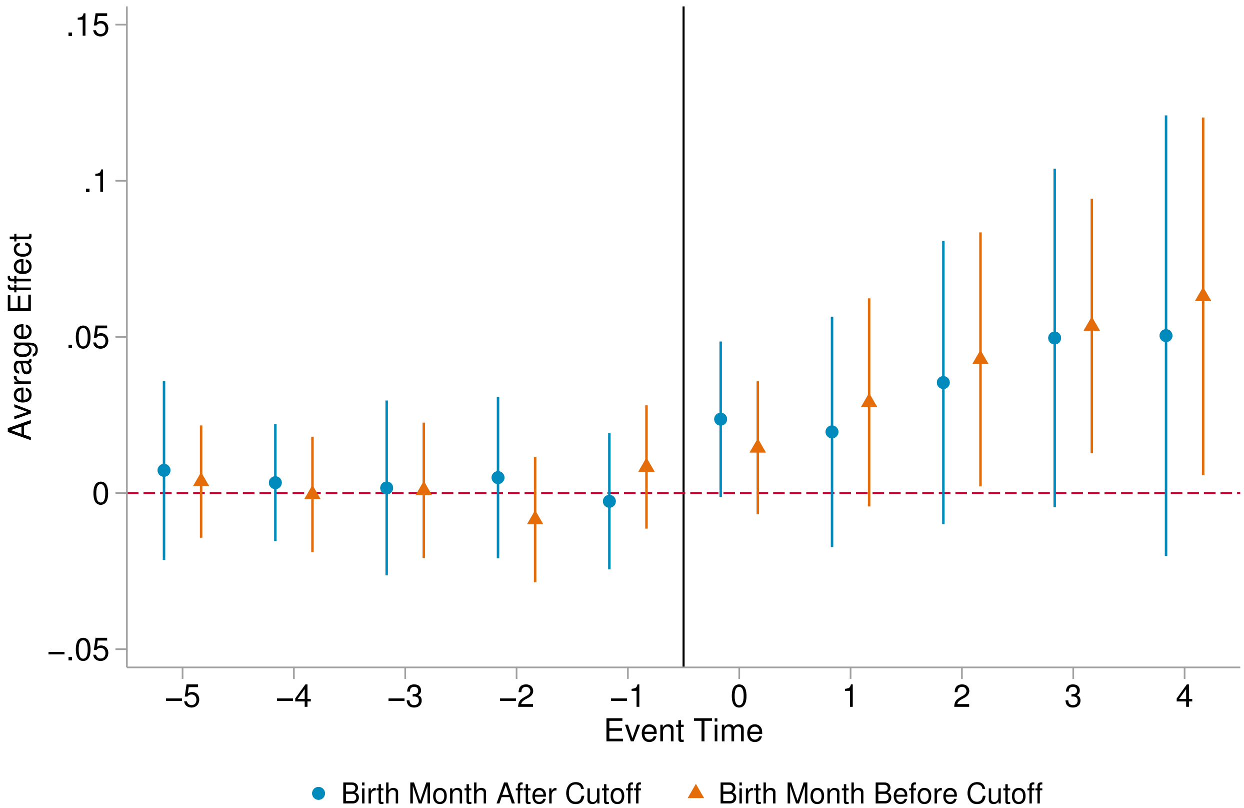
Notes: The sample include 29 provinces. The average pre-reform middle school graduation rate was 0.772. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method.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

Figure 2.9: Heterogeneous Effects on High School Enrollment, by Gender



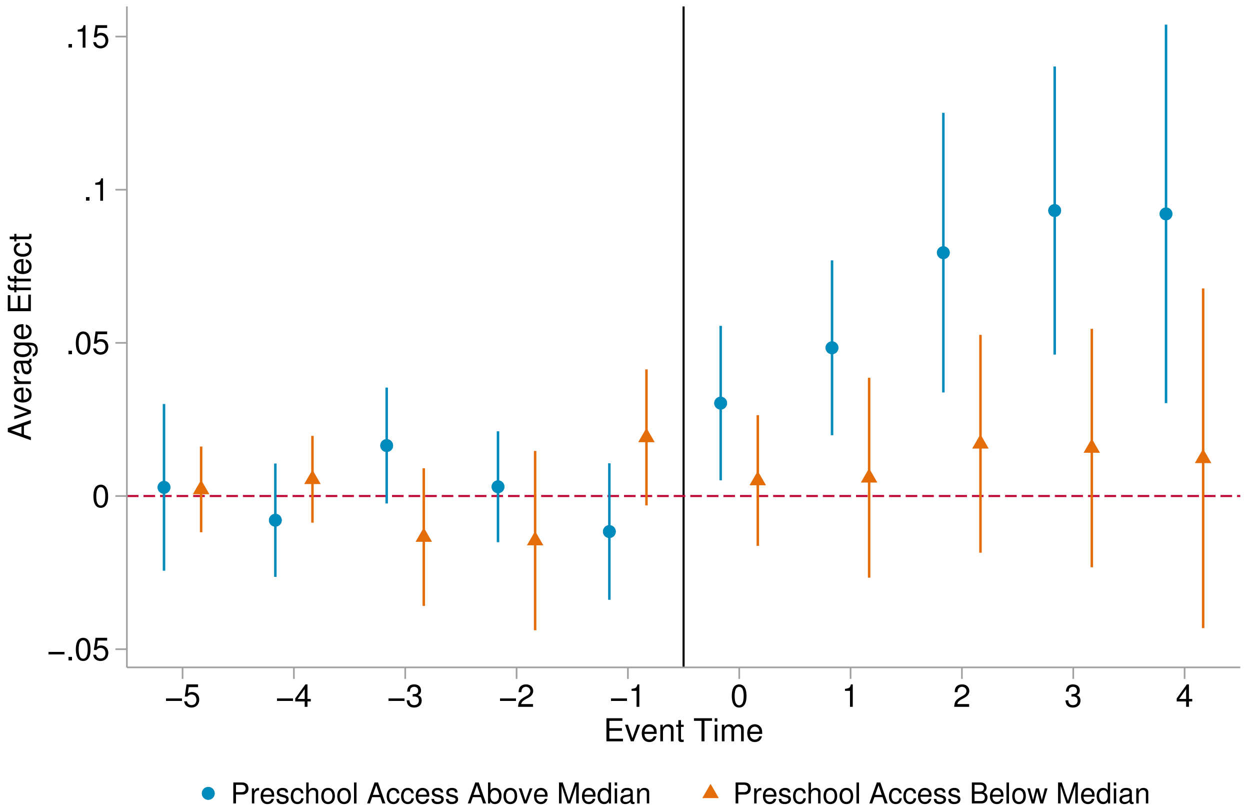
Notes: The sample include 29 provinces. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method. Standard errors are estimated using the Wild bootstrap clustered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

Figure 2.10: Heterogeneous Effects on High School Enrollment, by Birth Month



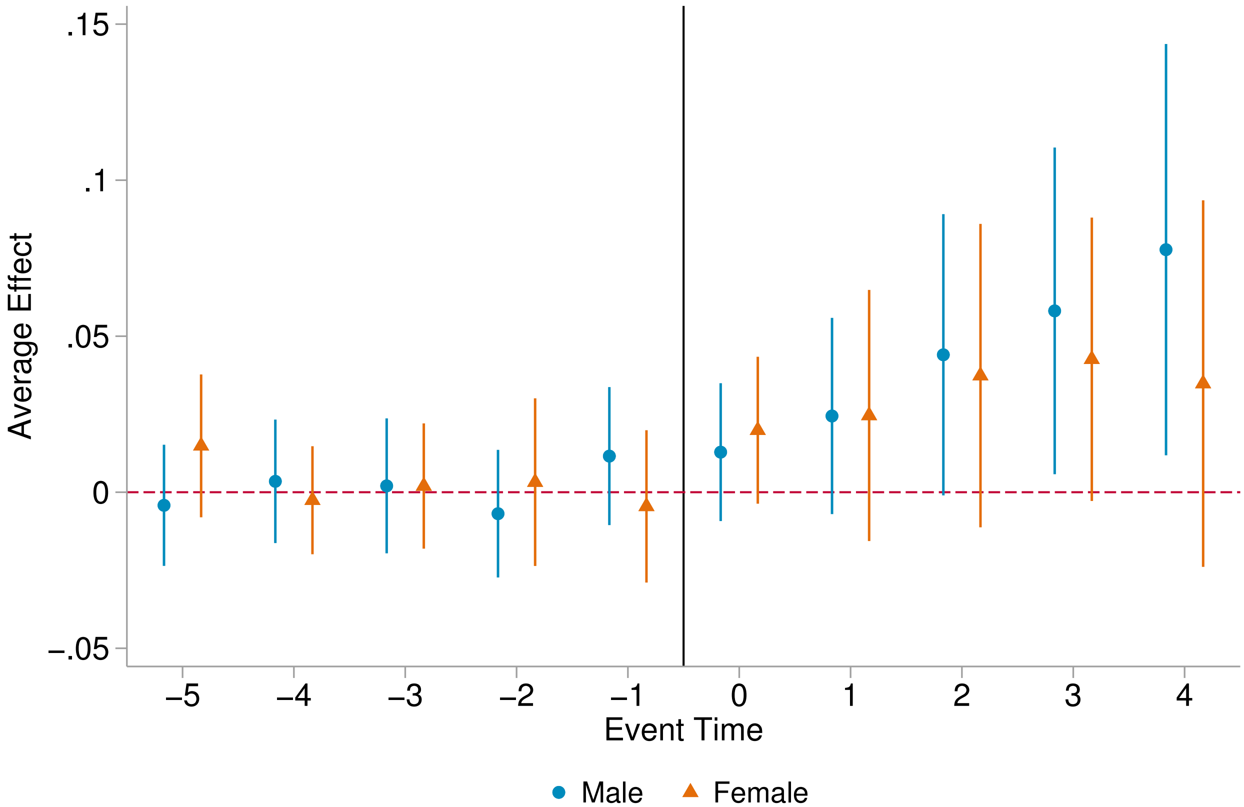
Notes: The sample include 29 provinces. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method. Standard errors are estimated using the Wild bootstrap clustered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

Figure 2.11: Heterogeneous Effects on High School Enrollment, by Preschool Access



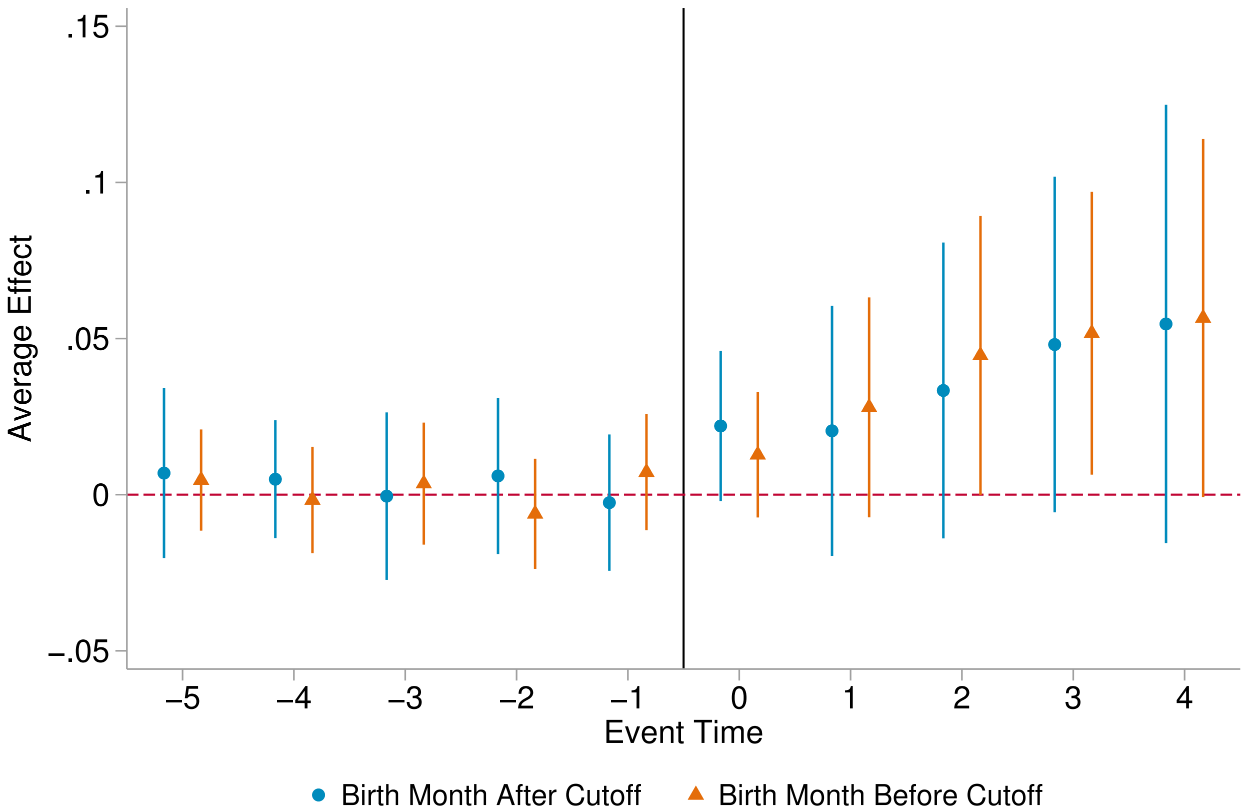
Notes: The sample include 29 provinces. Comparison groups are provinces not yet treated by the SSA reform (Callaway and Sant'Anna, 2021), weighted by cell size. Pre-reform placebo effects are estimated using the first-difference method. Standard errors are estimated using the Wild bootstrap clustered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

Figure 2.12: Heterogeneous Effects on High School Graduations, by Gender



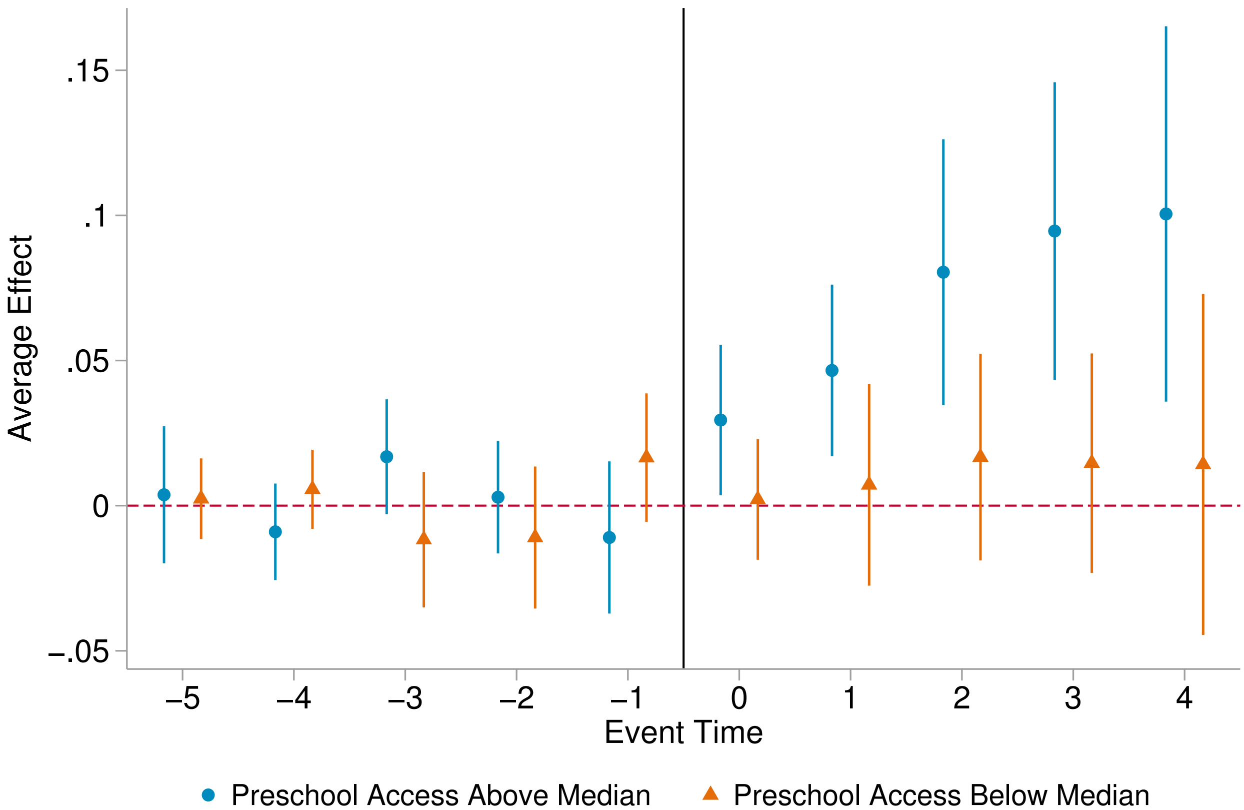
Notes: The sample include 29 provinces. The dependent variable is the fraction of observations enrolled in high school in province j and cohort k. The figure plots estimates of the average treatment effect on the treated using Callaway and Sant'Anna (2021), and the comparison groups are provinces not 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 2.13: Heterogeneous Effects on High School Graduations, by Birth Month



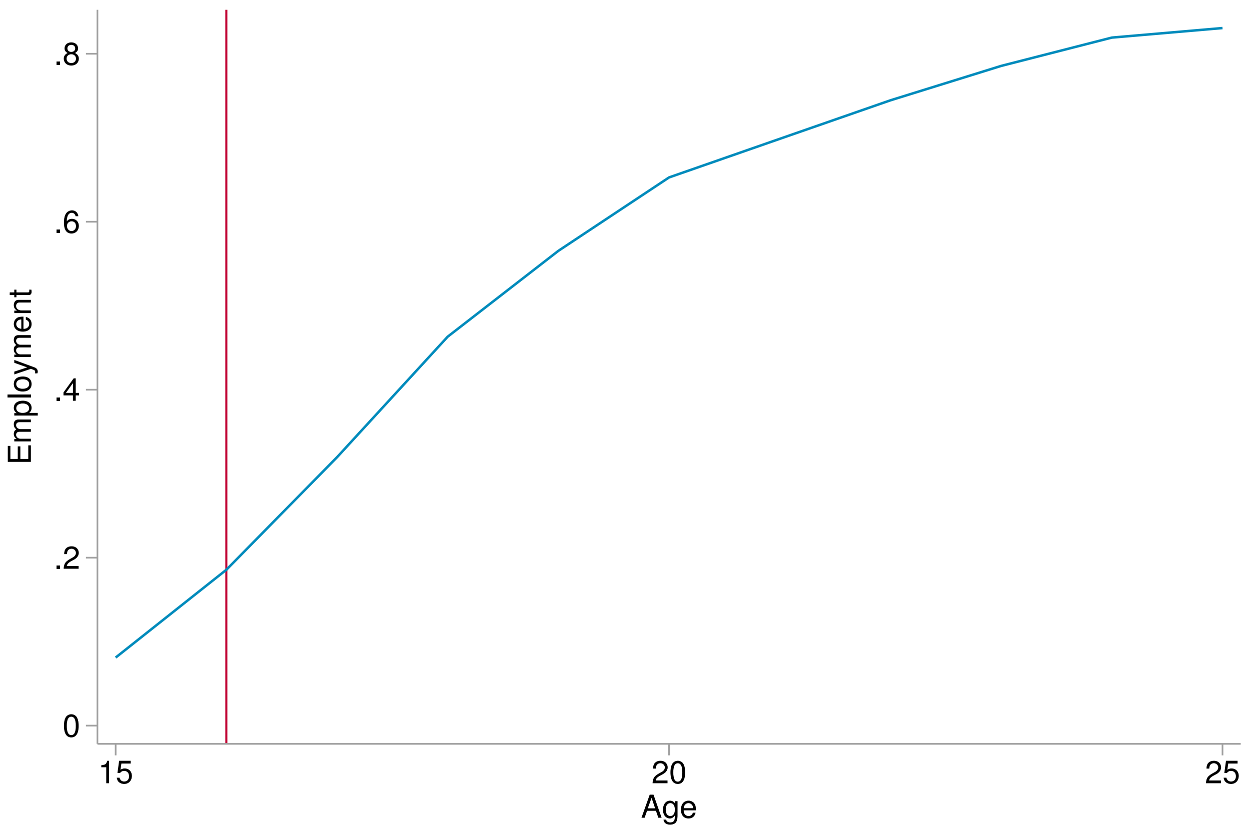
Notes: The sample include 29 provinces. The dependent variable is the fraction of observations enrolled in high school in province j and cohort k. The figure plots estimates of the average treatment effect on the treated using Callaway and Sant'Anna (2021), and the comparison groups are provinces not 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 2.14: Heterogeneous Effects on High School Graduations, by Preschool Access



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 2.15: Employment Rate by Age



Notes: Data Source: Census 2005.

2.9 Tables

Table 2.1: Descriptive Statistics

	All	Pre-Reform	Post-Reform
Outcomes			
Primary school enrollment	0.976	0.971	0.983
Middle school graduation	0.806	0.772	0.848
High school enrollment	0.315	0.283	0.354
High school graduation	0.302	0.279	0.331
Demographics			
Female	0.522	0.518	0.528
Ethnic minority	0.102	0.111	0.091
Age	25.800	28.224	22.771
N	408,265	226,767	181,498

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

Table 2.2: 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*	0.255	0.203	0.050	-0.033
	(0.351)	(0.376)	(0.390)	(0.422)	(0.423)
Percent of population female	0.084	0.649	-0.048	-0.028	-0.016
	(0.783)	(0.969)	(0.030)	(0.026)	(0.033)
Birth rate (‰)	0.063	0.021*	0.004	0.005	0.006
	(0.066)	(0.011)	(0.006)	(0.006)	(0.006)
Num of primary school students per teacher	-0.368***	0.126**	0.002	0.006	0.008*
	(0.108)	(0.060)	(0.004)	(0.005)	(0.005)
Num of secondary school students per teacher	-0.149*	-0.015	-0.002	-0.004	-0.001
	(0.076)	(0.037)	(0.004)	(0.005)	(0.005)
Percent of GRP primary industry	0.141***	-0.014	-0.001	-0.001	-0.000
	(0.045)	(0.020)	(0.002)	(0.002)	(0.002)
Per Capita GRP (yuan)	-0.001	0.042	-0.000	-0.000	-0.000*
	(0.001)	(0.032)	(0.000)	(0.000)	(0.000)
OCP fine in years of income	-1.630	-0.008	-0.003	-0.000	0.000
	(0.959)	(0.018)	(0.011)	(0.012)	(0.012)
Observations	29	29	688	686	683
R^2	0.631	0.325	0.884	0.884	0.883
Adjusted R^2	0.483	0.054	0.873	0.873	0.872
Province fixed effects			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: $DummyAdopted_{p,y} = \mathbf{Z}_{p,y-n}\beta + \upsilon_y + \lambda_p + \epsilon_{p,y}, n = 1, 2, 3$. The dependent variable is a dummy variable that takes on the value 1 if the SSA reform was adopted in year y by province p. The independent variables are the province characteristics level n year prior to year y. Standard errors are in parenthesis. Standard errors in columns 3-5 are clustered on the province level. Statistical significance is denoted by asterisks: * p < 0.1, ** p < 0.05, *** p < 0.01. Data Source: China Compendium of Statistics 1949-2008. OCP fine data from Ebenstein (2010). CEL year from Chen and Park (2021)

Table 2.3: Average Treatment Effects of SSA Reform using Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
Panel A: High School Enrollment					
Effects of SSA Reform	0.0549***	0.0371***	0.0486***	0.0549***	0.0506***
	(0.018)	(0.014)	(0.016)	(0.018)	(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
Panel B: High School Graduation					
Effects of SSA Reform	0.0529***	0.0325**	0.0509***	0.0529***	0.0505***
	(0.019)	(0.014)	(0.017)	(0.018)	(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.

Table 2.4: 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	-1,042.038*	-261.591***	-30.970**
	(329.485)	(591.018)	(81.565)	(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.

Table 2.5: 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	0.0801	0.000144	0.00735	-0.0124**
	(0.030)	(0.076)	(0.009)	(0.009)	(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.

Table 2.6: The Effects of SSA Reform on Estimated School Starting Age, by Preschool Acess

	(1) All	(2) Preschool Below Median	(3) Preschool Above Median
SSA Reform Exposure	-0.407***	-0.335***	-0.449***
	(0.042)	(0.070)	(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.

Table 2.7: Summary Statistics of Pre-reform Province Characteristics

	(1)	(2)	(3))	(4)
	1984 level		1981-to-1984 change (%)	
	Mean	SD	Mean	SD
Log population	7.904	(0.838)	0.595	(1.004)
Percent female	48.596	(0.505)	0.166	(0.368)
Birth rate(‰)	17.031	(4.568)	24.951	(35.797)
Num of primary school students per teacher	24.725	(3.837)	1.668	(7.055)
Num of secondary school students per teacher	15.678	(4.603)	1.099	(11.684)
Percent of GRP primary industry	33.029	(11.885)	3.276	(24.088)
Per capita GRP (yuan)	824.433	(619.283)	8.571	(14.244)
OCP fine in years of income	0.959	(0.335)	-16.973	(25.767)
Observations	29		29	

Note: Tibet and Xinjiang are excluded. Data Source: China Compendium of Statistics 1949-2008. One-child policy fine data from Ebenstein (2010). Compulsory Educational Law implementation year from Chen and Park (2021)

Table 2.8: Average Monthly Income by Attainment (Age 25-45)

	(1)	(2)	(3)	(4)	(5)	(6)
	All	No schooling	PS	MS	HS	College and above
	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd
Monthly Income	613.103	217.087	360.064	516.852	796.129	1601.572
	907.859	252.620	556.499	669.832	947.204	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 2.9: 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***	85.700***		
	(12.500)	(9.758)		
Middle school graduate	145.782***	120.162***		
	(18.099)	(10.076)		
High school graduate	587.043***	536.684***		
	(79.728)	(74.422)		
Primary school enrollee			141.498***	98.098***
			(17.832)	(12.635)
Middle school enrollee			157.254***	129.216***
			(18.939)	(10.724)
High school enrollee			584.580***	534.829***
			(79.577)	(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 2.10: Sample and Provincial Characteristics by Preschool Access

	Above Median		Below Median	
	Pre-reform	Post-reform	Pre-reform	Post-reform
Outcomes				
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
Demographics				
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
Province Characteristics				
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
Provincial Education Resources				
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).

Chapter 3

School-Entry Cutoff and Adolescent Mental Health: Evidence from a South Korean Reform (with Estelle Shin)

3.1 Introduction

Mental health is a fundamental aspect of overall well-being, profoundly influencing an individual's quality of life, productivity, and social relationships. Poor mental health during childhood through adolescence can lead to a range of short and long-term adverse consequences, including academic underachievement, substance abuse, and social difficulties (Fletcher, 2010; Liu et al., 2011). Children with poor mental health may have adverse external effects on their community (Kauten and Barry, 2020) and on their mothers' labor supply decisions (Coley et al., 2011; Richard et al., 2014).

The importance of socio-environmental factors in children's and adolescents' mental health is well-documented. Socio-environmental factors, such as social support, major and minor stressful events, and experiencing poverty, have significant impacts on mental health.

Studies in economics, sociology, psychology, and public health have documented evidence on outcomes including self-esteem and reporting or being treated for emotional and behavioral problems (i.e., DuBois et al., 1994; Brooks-Gunn and Duncan, 1997; Gleason et al., 2016). Children and adolescents spend a considerable amount of time in school and it shapes their social networks, peer groups, and skill development. In this paper, we focus on the mental health of students from late elementary school to high school.

Most education systems have a single birth date cutoff for entering school in the current academic year. The single school entry cutoff date creates variation in age among peers within a grade. As a result, how students' birth dates relate to the school entry cutoff date could have a substantial impact on experience and achievement in school and even later in life. Studies have shown that relatively older students perform better academically, socially, behaviorally, and psychologically. For example, older students tend to have higher test scores than their younger peers. They also demonstrate more leadership and report higher levels of confidence, risk tolerance, and grit, less ADHD diagnosis, fewer emotional symptoms, fewer peer problems, and less adolescent risky health behaviors (i.e., Dhuey and Lipscomb, 2008; Evans et al., 2010; Mühlenweg et al., 2012; Patalay et al., 2015; Cascio and Schanzenbach, 2016; Peña and Duckworth, 2018 Dhuey et al., 2019; Page et al., 2019; Johansen, 2021).

In this paper, we provide quasi-experimental estimates of the impact of the school entry cutoff date on students' mental health by leveraging a unique reform in South Korea that moved the school entry cutoff date from March 1 to January 1. All children in South Korea are eligible to enter elementary school if they turn six years old by the school entry cutoff date, with a possibility of entering early or delaying. Before the reform, a substantial fraction of students born in January and February delayed entry because their parents wanted their children to have the same Korean age as their grade peers.[1] In 2008, an amendment to the

[1]In South Korea, the traditional method of calculating age involves considering every individual to be one year old at birth and adding another year when the calendar transitions to January 1, meaning that a child born on December 31 is considered to be two years old the following day. This practice, known as the

Elementary and Secondary Education Act (ESEA) moved the school entry cutoff date from March 1 to January 1, effective in 2009. A few years after the reform, almost no one delayed entry.

Our paper broadens the existing empirical bases of the school starting age literature by shedding light on a less-explored context, South Korea, a Confucian-influenced East Asian country characterized by a strong age-based hierarchy. The cultural context plays an important role in understanding the effects of school starting age on mental health. Studies have shown that some aspects of culture, such as the level of individualism versus collectivism or societal equality versus hierarchy, affect how age relative to peers affects self-perceptions, decisions of cooperation, and social behaviors (Larsen et al., 2013; Bleidorn et al., 2016; Chao and Shin, 2022). All these elements might result in finding different effects of relative age on adolescent mental health in different settings (DuBois et al., 1994; Sowislo and Orth, 2013). In South Korea, small differences in the discrete Korean age could have salient social impacts. For instance, Koreans often use honorifics even when addressing individuals who are one year older than themselves. Research indicates that Korean students demonstrate a heightened awareness of age differences compared to their American counterparts, particularly in how they perceive peers who are either one year older or younger (Lim and Giles, 2007). Thus, our investigation into the effects of the cutoff date reform within this distinctive cultural framework offers valuable insights into the interplay between educational policy and emotional development.

Using a difference-in-differences empirical strategy and combining it with detailed survey questionnaires on multiple dimensions of mental health, we find that the reform improves the mental health of adolescents who were born in January and February. Under the prior rule, these students would have either delayed entry or had a different Korean Age compared

"Korean age" calculation, establishes that individuals born within the same calendar year are regarded as being the same age.

to their peers. We observe statistically significant effects for both female and male students; female students exhibit 0.28 SD lower levels of inattention and 0.36 SD lower levels of aggression, and male students show 0.23 SD higher levels of self-esteem. For the group of students who would have complied with the prior age rule, the sign of the estimates is consistent with the relative age literature. The effects may be explained by these students becoming relatively older after the reform or by these students now having the same Korean age as their grade peers. For the group of students who would have delayed entry, the estimates may be explained by these students feeling less alienated and a higher sense of belonging as they no longer see themselves as the "delayed entrants."

Our paper contributes to the growing economics literature about the determinants of mental health by making use of the exogenous variation created by this policy change. Prior studies have shown that exposure to poverty, unconditional cash transfer, in utero exposure to distress, economic downturns, and introduction of social media affect children's and adolescents' mental health (Paul and Moser, 2009; Haushofer and Shapiro, 2017; Persson and Rossin-Slater, 2018; Golberstein et al., 2019; Donati et al., 2022; Braghieri et al., 2022). We focus on an element of socio-environments that all students will experience, having a different age from their classmates, rather than extreme distress such as family rupture or an event that can only happen once, such as the introduction of the internet or social media.

Our paper also contributes to the literature on school starting age by using a difference-in-difference design. Many education systems have employed a single school-entry age cutoff, resulting in age variation within grade cohorts. The early literature, inspired by Angrist and Krueger (1991), commonly used birth month variation as instruments for years of schooling to study the return to education. However, concerns have been raised regarding the validity of using birth month as an instrument due to potential confounding factors such as weak instruments, the direct effects of the seasonality of birth, or endogenous selection into certain birth months (Bound et al., 1995; Cascio and Lewis, 2006; Buckles and Hungerman, 2013).

More recent literature has addressed these issues by leveraging exact birth dates and employing regression discontinuity designs (i.e. McCrary and Royer, 2011). Focusing on individuals born near the cutoff dates improves the estimates by circumventing the criticisms regarding the seasonality of births.

To the best of our knowledge, the only other study in economics looking at the effects of school starting age on mental health uses the linked Danish survey and registered data to estimate the effect of kindergarten entry age on the mental health of students between ages seven and age eleven (Dee and Sievertsen, 2018). Mental health outcomes in this study are measured using self-reported scales from the Strengths and Difficulties Questionnaire developed by English child psychiatrist Robert Goodman in the mid-1990s, which consists of 25 items (Goodman, 1997). Using a fuzzy regression-discontinuity design based on kindergarten entry cutoff date and exact dates of birth, the authors find that a one-year delay in kindergarten entry reduces inattention/hyperactivity at age seven for girls and persists at age eleven.

One common limitation of using fuzzy regression discontinuity designs to study the effects of school starting age is that the estimated effects are only identified for compliers of the school starting age policies, the students who enter school on time. In our paper, we adopt a difference-in-differences approach to investigate the differential month-of-birth effects before and after a significant reform in South Korea, which changed the school entry cutoff date from March 1 to January 1. We specifically focus on the effects of being born in January and February. Our analysis is based on the assumption that the direct effects of seasons of birth remain consistent before and after the reform and that there is no significant change in the selection into birth months. We interpret the differential effects as a combination of two effects from two groups of students: one group started school one year older and became one year older relative to March-to-December-born peers, and another group no longer could start school on time and no longer subject to the potential stigma associated with delaying entry after the reform. Our methodological approach allows us to capture the comprehensive impact of both those who comply with the cutoff date and those who delay school entry. This approach enriches the literature by providing a more holistic understanding of the effects of school starting age policies.

The remainder of the paper is organized as follows: Section 3.2 provides institutional background on education and mental health in South Korea; Section 3.3 describes the data and sample used in this analysis; Section 3.4 discusses empirical strategy; Section 3.5 presents the results; Section 3.6 concludes.

3.2 Institutional Background

3.2.1 The Traditional Korean Age-Counting Custom

In South Korea, the traditional age-counting custom ("Korean Age") considers every person one-year-old at birth and adds another year when the calendar hits January 1. This means that a child born on December 31 is one year old at birth in Korean Age and turns two the next day as the new calendar year begins. Following the Korean age calculation tradition, people born within the same calendar year are considered the same age. The Korean Age is prevalent in social settings and the workforce, where age hierarchies play a significant role in interpersonal relationships and organizational structures.

3.2.2 School Entry Practice and Cutoff Amendment in South Korea

All children in South Korea should enter elementary school in the year following the year of their sixth birthday. The academic calendar commences in March. The school entry cutoff was set to March 1 and moved to January 1 by the Elementary and Secondary Education Act (ESEA) amendment in 2008. Before the policy, kids born in January and February should have entered school in March of their internationally six-year-old year, while those born March-Dec entered in March of the following year at international age 7. After the policy, all children born in the same calendar year entered school in March of their international 7-year-old year.

It is possible to enter school early or late. Early entry is rare, with around 0.3 to 0.7 percent across years at the population level before the amendment. There was an increase in early entry patterns to 2.1 percent right after the amendment because some parents wanted to send January and February-born children to school in the year of their sixth birthday, same as the March 1 cutoff. The share of early entrants decreased to 0.9 percent in 2010 and further to 0.3 percent by 2015.[2] However, delayed entry was relatively common before the amendment.[3] When March 1 served as the school entry cutoff, children born in January and February had a younger Korean age compared to their grade peers. Consequently, parents of children born in January and February increasingly intentionally delayed their children's school entry to ensure they shared the same Korean age as their classmates. Our data suggest about 30 percent to 60 percent of the January and February-born children delayed entry before the ESEA amendment, and the share of delayed entry was higher for males by 1.0-1.2 percentage points. To address this issue, an amendment to ESEA moved the cutoff date from March 1 to January 1, effective in 2009. After the ESEA amendment, delayed entry became uncommon. About 0.8 percent of children delayed their entry in 2010, and the share decreased to less than 0.5 percent at the population level.

The amendment effectively increased the average school starting age of students born in January and February and did not change the school starting age of students born between March and December. In our data, among those who were born in January and February,

[2]Source: Statistical yearbook of education.

[3]To delay school entry, parents need to file an application for reasons such as child development, and the elementary school headmaster decides whether to accept the application following the deliberation of the compulsory education management committee.

the average school starting age increased from 6.47 - 6.85 before the reform to 7.01 - 7.11 after the reform, depending on the survey cohort and gender.[4] There is little change in the average school starting age of students born between March and December before and after the amendment for any subsample. (See the first row of Table 3.1 and Table 3.2.) The amendment also effectively reduced the share of entrants who delayed entry. If all students start school on time and their birth months are randomly drawn from a uniform distribution, the average school starting age of January and February-born students should be around 7.08. In our data, the post-reform average school starting age ranges between 7.01 and 7.11, which is close to 7.08 and suggests an overall on-time enrollment for these students. The detailed summary statistics of school starting age are reported in Table 3.4.

The amendment directly affects children born in January and February 2004 and after. For the would-have-been compliers who used to be the youngest within a grade without the amendment, after the amendment, these students start school a year later and become the oldest within a grade. For the would-have-been delayed entrants who are already the oldest, after the amendment, these students begin to comply with the school entry rules.

3.2.3 Adolescent Mental Health in South Korea

Mental health encompasses our psychological and social status, influencing our cognition, emotions, behaviors, and interpersonal interactions. It plays a crucial role in managing stress, fostering relationships, and guiding decision-making processes. Adolescent mental health in South Korea is a pressing concern, underscored by alarming statistics. According to the survey conducted by the Korea Center for Disease and Prevention Agency, one out of seven teenagers has contemplated suicide in 2022, reflecting a significant mental health crisis among the youth population. Children and teens grappling with mental health issues

[4]The school starting age is estimated using the month of birth, the year of birth, the survey year, and the survey grade using the following equation: $(SurveyYear - SurveyGrade + 1) + 3/12 - (YOB + MOB/12)$ This is because the exact date of birth was not surveyed in our data.

are seeking medical assistance at an alarming rate, with nearly 210,000 individuals visiting hospitals for depression or anxiety disorders from 2019 to the early half of 2022, as reported by the National Health Insurance Service. Furthermore, the suicide rate among 12 to 14-year-olds has displayed a fluctuating pattern over the years, starting from 1.1 per 100,000 in 2000 and reaching its peak at 5.0 per 100,000 in 2021. Similarly, for those aged 15 to 17, the suicide rate witnessed fluctuations, with a recent increase to 9.9 per 100,000 in 2020, although it slightly decreased to 9.5 per 100,000 in 2021.[5] Suicide remains a leading cause of death among teenagers in South Korea, accounting for over two in five deaths in this age group. This trend persists into early adulthood, with suicide rates remaining distressingly high among individuals in their 20s and 30s.[6]

3.3 Data

3.3.1 Korea Children and Youth Panel Survey

Our analysis draws from two rounds of the Korea Children and Youth Panel Survey (KCYPS), namely KCYPS 2010 and KCYPS 2018, nationally representative longitudinal datasets managed by the National Youth Policy Institute of South Korea. Students from schools across 17 province-level divisions were surveyed through face-to-face interviews by trained interviewers. Parents and/or guardians were also surveyed through phone in KCYPS 2010 and face-to-face in KCYPS 2018. The survey question for students encompasses a wide range of topics including personal development (such as time use, intellectual growth, career aspirations, social and emotional development, deviant behavior, and health) as well as development environment factors (such as family dynamics, school experiences, peer relations, local community engagement, and media/cultural activities). Similarly, both KCYPS 2010

[5]Source: Children and Youth Well-being in Korea 2022, published by Statistics Korea.
[6]Source: Statistics Korea and reiterated by BBC.

and 2018 cover background characteristics such as education, employment, and economic status in the parents' survey. Overall, the KCYPS datasets provide invaluable insights into the multifaceted experiences and trajectories of children and adolescents in South Korea, offering a holistic understanding of various facets of their lives encompassing education, health, and overall well-being.

The survey employs two-stage stratified cluster sampling, ensuring a nationally representative sample. The population was first stratified into geographical divisions such as provinces and metropolitan cities. The selected schools were further stratified into sub-province levels, based on the size of cities within the geographical divisions.[7] In each stratum, schools were randomly selected in proportion to the number of classes in the stratum. Each school selected has at least 2 classes and at least 50 students. If a selected school did not meet the criteria, it was replaced with a nearby school that met both criteria. The final survey sample includes observations only when both students and parents are surveyed. While the reference year for the student population differs between the two panels, the strata are comparable. Each observation wave has a cross-sectional weight, multiplying the probability of sampling weight, the non-response weight, and the post-stratification weight. The cross-sectional weighted sample represents the population of students in the survey year.

The survey was conducted every year between September and November in KCYPS 2010 and between August and November in KCYPS 2018. Figure 3.1 illustrate the survey year, survey grade, sample size, and retention rates of all survey cohorts in both KCYPS 2010 and 2018. In the KCYPS 2010 panel, the survey tracked cohorts that entered school in 2004, 2007, and 2010, from 2010 to 2016. The cohort who entered school in 2004 were in the first year of middle school (grade 7) in the first survey wave. They were surveyed between grade 7 (ages 12-13) and one year after high school graduation (ages 18-19). The cohort

[7] There were 27 strata in KCYPS 2010 and 28 strata in KCYPS 2018. The number of strata increased in KCYPS 2018 due to the introduction of Sejong-Si in 2012.

who entered school in 2007 were in the fourth year of elementary school in the first survey wave and surveyed between grade 4 (ages 9-10) and grade 10 (ages 15-16). The cohort who entered school in 2010 were in the first year of elementary school (grade 1) in the first survey wave and surveyed until grade 7. The KCYPS 2018 panel followed cohorts that began school in 2012 and 2015, tracking them from 2018 to 2024. The cohort who entered school in 2012 were in the first year of middle school (grade 7) in the first survey wave surveyed between grade 7 and one year after high school graduation. The cohort who entered school in 2015 were in the fourth year of elementary school in the first survey wave and surveyed between grade 4 and grade 10. The retention rates for the last wave (wave 7) of KCYPS 2010 are between 80 percent to 85.5 percent, and the retention rates for wave 5 of KCYPS 2018 are 86.9 percent to 88.6 percent.

Our current analysis is based on data up to the 2020 survey. We only observe cohorts who entered school in 2015 until they were in grade 6 and who entered school in 2012 until they were in grade 9. We plan to include the more recent data in the next version of this paper.

3.3.2 Sample

Our main analytical sample comprises childhood and adolescents drawn from both KCYPS 2010 and KCYPS 2018. Specifically, the childhood cohort consists of students first surveyed at grade 4 (age 10), while the early adolescence cohort comprises students first surveyed at grade 7 (age 13). Recognizing the influence of age and gender on developmental stages, we divide our data into four subsamples: childhood females, childhood males, early-adolescence females, and early-adolescence males. Each subsample is analyzed separately to account for potential differential effects across cohort-gender groups. Combining KCYPS 2010 and KCYPS 2018 panels, there are around 7000 observations per subsample. Each student grade serves as one observation in our analysis, and cross-sectional weights are applied to ensure that sample estimates accurately reflect the population.

Two cohorts in KCYPS 2010 start school before the school entry cutoff date amendment, adhering to the March 1 cutoff rule. Two cohorts in KCYPS 2018 start school after the amendment, adhering to the January 1 cutoff rule. We exclude the cohort that entered school in 2010 in KCYPS 2010 because there is no comparable cohort in KCYPS 2018. In addition, they entered school when the reform recently took place and had different compliance patterns from other years.

3.3.3 Mental Health Outcomes

KCYPS surveyed various aspects of students' mental health in detail. We select outcomes surveyed in both KCYPS 2010 and KCYPS 2018 using the same set of questions; the outcomes are self-reported and measured on a scale. In this paper, we focus on two main types of mental health outcomes: emotional/behavioral problems and self-esteem. In particular, we focus on five emotional/behavioral problems, including both externalizing and internalizing problems: aggression, inattention (hyperactivity), social withdrawal, physical symptoms (somatic complaints), and depression.[8][9] [10]

These outcomes are assessed with students responding to multiple questions for each construct. Using a four-point Likert scale ranging from 1 being "strongly disagree" to 4 being "strongly agree," students indicated their level of agreement with statements about various emotional problems and self-awareness constructs. The number of questions for each

[8] According to Kauten and Barry (2020), "Externalizing behavior comprises any of a wide variety of generally antisocial acts (i.e., acts that violate social norms and/or are harmful to others). These acts include those that are targeted at another individual (e.g., aggression), as well as acts that may be considered victimless (e.g., substance use)."

[9] According to Hansen and Jordan (2020), "Internalizing behaviors encompass a dimension of childhood psychopathology that includes behaviors that are directed inward or are over-controlled and are associated with a number of depressive and anxiety disorders. Examples of internalizing behaviors may include fearfulness, somatic complaints, worrying, and withdrawal."

[10] As for self-esteem, we focus on global self-esteem, defined as the "level of global regard that one has for the self as a person (Harter, 1993)," in contrast to the domain-specific evaluations of self.

construct ranges from five to ten, with seven questions for inattention, six questions for aggression, eight questions for physical symptoms, five questions for social withdrawal, ten questions for depression, and ten questions for self-esteem. We standardize each question, take averages, and standardize again to mean zero and unit variance. Here is an example statement for each emotional/behavioral problem:

- Inattention: "Even after being praised or punished, I quickly become distracted."
- Aggression: "There are times when I nitpick even over small matters."
- Physical Symptoms: "I sometimes lose my appetite."
- Social Withdrawal: "I feel awkward when there are many people around."
- Depression: "I often feel low in energy."
- Self-esteem: "I feel that I have a number of good qualities."

In addition to the standardized outcomes, we also estimate a simple average of all items within each problem or self-esteem to provide descriptive statistics of the baseline mental health of children and adolescents in South Korea. We construct these simple averages by calculating the student-level average of all items related to each problem or self-esteem. Table 3.3 present student-level descriptive statistics for constructed emotional/behavioral outcomes and self-esteem measures. Male students typically exhibit higher self-esteem than females, and self-esteem tends to decrease during adolescence. In our sample, female students demonstrate higher levels of both externalizing and internalizing emotional/behavioral problems. Furthermore, the cohorts after the reform exhibit higher self-esteem and lower levels of emotional/behavioral problems compared to the cohorts before the reform.

One potential caveat of the survey is that not all measures are surveyed for all cohorts in all waves. For emotional/behavioral problems, data were collected from early adolescent cohorts during waves 1, 3, 5, 6, and 7 in KCYPS 2010, and during waves 1, 2, and 3 in KCYPS 2018. Similarly, data from childhood cohorts were collected during waves 3, 5, 6, and 7 in KCYPS 2010, while during waves 1, 2, and 3 in KCYPS 2018. Regarding self-esteem, early adolescent data were obtained during waves 1, 3, 5, 6, and 7 in KCYPS 2010, and during waves 1, 2, and 3 in KCYPS 2018. For students in childhood, self-esteem data were gathered during waves 2, 4, 6, and 7 in KCYPS 2010, while during waves 1, 2, and 3 in KCYPS 2018.

3.3.4 Demographics

KCYPS collected detailed demographic information about the students and their families. It is well-documented that socio-environment affects students' emotional/behavioral problems and self-esteem (i.e., DuBois et al., 1994; Ridley et al., 2020). To control for socio-environmental differences, our analysis includes the following demographic information: whether a student is a firstborn, the educational attainment of both father and mother, and the student's home city or province at the time of the initial survey. Parental education levels were categorized into four groups: less than college, 2-3 years of college, 4 years of college, and graduate degree.[11] Detailed summary statistics of these demographics for each subsample are provided in Table 3.1 and Table 3.2.

[11] A negligible proportion, approximately 1.2 percent, of students' fathers and mothers reported education levels below high school, leading to the consolidation of the "less than high school" and "high school graduate" categories.

3.4 Empirical Strategy

3.4.1 Construction of the Primary Outcome Variables

In this paper, we analyze the effects of the school entry cutoff date reform on indices of six dimensions of emotional health: inattention, aggression, physical symptoms, social withdrawal, depression, and self-esteem. We construct each outcome to be an index using answers to multiple Likert-scale questions, which we will refer to as items. First, we recode all items regarding emotional/behavioral problems so that a larger value indicates a higher level of problem, and all items regarding self-esteem so that a larger value indicates a higher level of self-esteem. We check the consistency of student answers to each emotional/behavioral problem or self-esteem using Cronbach's alpha.[12] There is strong reliability, as nearly all alphas are larger than 0.8, making it reasonable for us to believe there is only one dimension and, thus, to construct one index for each emotional/behavioral problem or self-esteem.

To create each index, we first standardize each item for each cohort survey wave, compute the average of the items regarding a certain emotional/behavioral problem or self-esteem for each student, and then re-standardize the average to have a zero mean and unit variance. The initial standardization scales each item for each survey-wave-cohort, preserving as much variation as possible and ensuring uniform variation across all item-survey-wave-cohorts. This process ensures that the variation of the constructed indices is not driven by a few items with large variations or by certain survey-wave cohorts whose responses have large variations. Then, we construct the index as a simple average of the standardized items because of the high consistency of answers regarding each problem or self-esteem. We re-standardize the averages to have a zero mean and unit variance for easier interpretation of

[12] Cronbach's alpha, developed by Lee Cronbach in 1951 measures reliability or internal consistency. $\alpha = \frac{N \times \bar{c}}{\bar{v} + (N-1) \times \bar{c}}$, where N is the number of items; $\bar{c}$ is the average covariance between item-pairs; $\bar{v}$ is the average variance. We estimate the Cronbach's alpha for each cohort in each survey wave using the stata command alpha.

regression coefficients and comparison of effect sizes with the literature. These standardized indices are used as outcomes for our regression analysis. We exclude any observations with at least one non-response item within each problem or self-esteem.[13]

3.4.2 Baseline Model

We estimate the effects of the reform on mental health using a Difference-in-Differences model. We separately estimate the effects of each survey cohort by gender, allowing for heterogeneity between gender and cohorts. Our baseline model is specified as:

$$Y_{isg} = \beta_1 KCYPS2018_s + \beta_2 JanFeb_i + \beta_3 KCYPS2018_s \times JanFeb_i + \theta_g + X_i\Gamma + \epsilon_{isg}$$

Here, Y_{isg} denotes the mental health outcomes of student i in survey s with currently in grade g, encompassing emotional problems (attention, aggression, physical symptoms, social withdrawal, and depression) and self-esteem. The binary variable $KCYPS2018_s$ equals one if the student participated in the KCYPS 2018 survey and zero if in the KCYPS 2010 survey. Because all students in KCYPS 2010 started school before the reform and all students in KCYPS 2018 started school after the reform, the binary variable $KCYPS2018_s$ also indicates if starting school after the starting subsample cutoff reform. The binary variable $JanFeb_i$ equals one if a student was born in January or February and zero if a student was born in March to December. The interaction term $KCYPS2018_s \times JanFeb_i$ takes on value one if a student was born in January and February and participated in the KCYPS 2018, zero otherwise. The coefficient β_1 captures the difference in mental health trends. β_2 measures the difference in outcomes for January and February-born students compared to their peers before the reform. Our main coefficient of interest is β_3, which captures the effects of being born in January or February after the reform relative to before. Because the outcomes are

[13] As a result, we exclude 2 to 5 observations, depending on the outcome or subsample.

standardized indices, the unit of the estimated effects is in standard deviations. We control for grade fixed effect (θ_g) and a set of individual demographics (X_i), including an indicator of whether the student is a first-born child, a categorical variable for mother's education, a categorical variable for father's education, and province fixed effects.[14] ϵ_{isg} represents the error term. Each observation is weighted using cross-sectional survey weight to make the sample estimates representing the population, and standard errors are clustered at the school-survey year level.

3.4.3 Identifying Assumptions

The effect of the cutoff date reform on mental health for January-to-February-born students relative to the March-to-December-born students can be identified using β_3 of our Difference-in-Differences design under the assumptions that: (1) the March-to-December-born students are a valid comparison group for the January-and-February-born students; (2) the month of birth effects, including selection effects and physiological effects, were unchanged because of the reform. Our settings and our balance tests suggest that it is plausible that both the above assumptions are satisfied.

First, we conduct a balance test comparing predetermined characteristics between students born in January and February and those born in March through December to check if there are systematic differences between these two groups. The characteristics include whether a student is a first-born, the student's mother's education, the student's father's education, and the home city or province of a student. These characteristics are expected not to be correlated with whether a student was born in January or February. We estimate

[14]We use the student's province of home from the first wave of each survey, minimizing the impact of selective migration.

the following regressions for each characteristic

$$X_i = \delta_0 + \delta_1 JanFeb_i + \epsilon_i$$

where X_i represents the characteristic and $JanFeb_i$ is a binary variable indicating January or February born. The results are summarized in Table 3.4, where each cell displays coefficients from separate regressions for specific samples. Overall, we find minimal differences between January-to-February-born students and March-to-December-born students, both statistically and economically, with a few exceptions. Notably, among male early-adolescence cohorts, January and February births are associated with a higher proportion of fathers with graduate degrees and a lower proportion with 2-3 year college degrees, potentially influenced by multiple tests. However, these exceptions aside, our findings suggest the predetermined characteristics are balanced between these two groups in all subsamples.

Additionally, we assume that the selection into birth months remains unchanged before and after the reform. This is plausible because our sample in both KCYPS 2010 and KCYPS 2018 were born before the reform was announced. We also assume that the direct effects of birth season remain consistent before and after the reform. It is plausible because the physiological effects of the birth season are unlikely to change drastically within a few years.

It is important to note that students born between March and December became relatively younger within the classroom, and if relatively younger students do worse, our estimates would be biased upward. We discuss the interpretation of our estimates and present a back-of-the-envelope calculation to conceptualize the scale of the bias in Section 3.5,

3.5 Results

We summarize the coefficients of our baseline specification on mental health indices in Table 3.5. Each panel consists of a different subsample. Within each panel, each column represents the coefficients of a different outcome, with the outcome indicated in the column name. The first row of each panel shows the estimates of β_1, the second row shows the estimates of β_2, and the third row shows the estimates of β_3. As the outcomes are standardized, all the estimated coefficients should be interpreted as multiples of standard deviations (SD). For our key coefficient of interest, β_3, our estimates suggest that early-adolescent girls born in January and February indicate substantially lower levels of external emotional/behavioral problems after the reform, and early-adolescent boys born in January and February indicate substantially higher levels of self-esteem after the reform. There are no statistically significant differences in any outcomes for boys and girls in their childhood. We also find evidence of trends in mental health over time and supporting evidence of the importance of separately analyzing mental health by gender and developmental stages.

Panel A of Table 3.5 summarizes the estimates of mental health indicators using the early-adolescence female sample. We find that early-adolescent girls born in January and February who started school after the reform exhibit a 0.28 SD lower level of inattention and a 0.36 SD lower level of aggression relative to before, in comparison to their peers born between March and December who started school after the reform relative to before. Additionally, our estimates suggest that compared to the early-adolescent girls who participated in KCYPS 2010, those in KCYPS 2018 exhibited higher levels of aggression, lower levels of self-esteem, and suggestively higher levels of other emotional/behavioral problems. Before the reform, early-adolescent girls born in January and February exhibited higher levels of aggression and suggestively higher levels of inattention relative to girls born between March and December.

Panel B of Table 3.5 summarizes the estimates using the early-adolescence male sample.

We find that early-adolescent boys born in January and February who started school after the reform exhibit a 0.22 SD higher level of self-esteem relative to before, in comparison to their peers who started school after the reform relative to before. We find no statistically significant differences in any emotional/behavioral problems. Our estimates also indicate that compared to early-adolescent boys who participated in KCYPS 2010, those in KCYPS 2018 showed an overall higher level of depression and potentially higher levels of other emotional/behavioral problems. Before the reform, early-adolescent boys born in January and February exhibited higher levels of social withdrawal and potentially lower levels of self-esteem relative to boys born between March and December. The observed differences in mental health effects between boys and girls in early adolescence underscore the importance of conducting separate analyses by gender to comprehend adolescent mental health fully. Together with our findings in Panel A, our estimates suggest that the reform has substantial effects on mental health for both boys and girls in early adolescence, with the effects on girls primarily manifesting through externalizing emotional/behavioral problems and the effects on boys primarily through self-esteem.

Panel C of Table 3.5 summarises the estimates of mental health indicators using the childhood female sample. We find that girls in childhood who were born in January and February and started school after the reform exhibit no statistically significant differences in levels of emotional/behavioral problems or self-esteem relative to before the reform. Additionally, our estimates indicate that compared to childhood girls who participated in KCYPS 2010, those in KCYPS 2018 exhibit lower levels of emotional/behavioral problems, such as physical symptoms, social withdrawal, and depression. Before the reform, childhood girls born in January and February exhibit lower levels of self-esteem and higher levels of social withdrawal relative to girls born in March and December. The observed differences in estimated effects between girls in childhood and early adolescence underscore the importance of conducting separate analyses by developmental stages to understand students' mental health.

Panel D of Table 3.5 summarises the estimates of mental health indicators using the childhood male sample. We find that boys in childhood who were born in January and February and started school after the reform do not exhibit statistically significant differences in levels of emotional/behavioral problems or self-esteem relative to those before the reform. Additionally, our estimates indicate that compared to childhood boys who participated in KCYPS 2010, those in KCYPS 2018 exhibit higher levels of emotional/behavioral problems, such as physical symptoms, social withdrawal, and depression. Before the reform, childhood boys born in January and February exhibit little difference in emotional/behavioral problems and self-esteem relative to boys born in March and December. From Panel C and D, the overall small difference we found in both boys and girls in childhood is consistent with the findings of the literature on diminishing effects on non-cognitive skills from early childhood to late childhood (Datar and Gottfried, 2015; Lubotsky and Kaestner, 2016).

To interpret the estimates on the change in outcomes of January-to-February-born students after the reform relative to before, in comparison to March-to-December-born students, it is important to underscore that there are two distinct groups of students in the treatment group: those who always start school on time and those who always start after their seventh birthdays. In our sample, 40 percent to 70 percent of January-to-February-born students voluntarily delayed school entry by a year before the reform, suggesting 40 percent to 70 percent would have started after their seventh birthday regardless of the reform. The remaining January-to-February-born students started school on time after their sixth birthday before the reform and on time after their seventh birthday following the reform. Our estimates are weighted averages of the estimated treatment effects for these two groups.

For students born in January and February who always start school on time regardless of the cutoff date, they started school one year later and became the relatively oldest in their cohort after the reform. In our sample, around 30 percent to 60 percent of the January-to-

February-born students experienced this type of treatment. The change in absolute age at the survey, the absolute starting age, and the change in relative age might explain the better measures of mental health exhibited (Evans et al., 2010; Dee and Sievertsen, 2018). We cannot distinguish the exact mechanisms among the three. If the relative age mechanism was the driving factor, our estimates are subject to upward bias as the comparison group was also affected by the treatment.

To conceptualize the size of the bias, we conduct a simplified back-of-envelope calculation. We simplified the calculation by assuming the effects only come from the rank of birth month and that the effect of the ordinal rank of the birth month within the classroom on mental health outcomes is linear. Students' birth months rank from 1st to 12th, with 1st being the birth month of the oldest student. As a result of this reform, the birth month rank of students who were born in January and February and started school on time moved up by 10: the January-born students moved from the 11th to the 1st, and the February-born students moved from the 12th to the 2nd. The rank of the students who were born in March to December moved down by 2: for example, the March-born students moved from 1st to 3rd. The estimated effects of our specification capture the combined effects of the treatment group becoming relatively older and the comparison group becoming relatively younger–a one-year change in relative age. To interpret the effects of the treatment on the January-to-February-born students, the estimated coefficient should be scaled down by 5/6.

For students born in January and February who would start school after their seventh birthday regardless of the cutoff and would have delayed entry even without the reform, we hypothesize that the treatment arises from the stigma associated with delayed school entry. Analysis in Shin (2023) shows that students who delayed elementary school entry exhibit lower social-emotional skills in middle school and high school relative to students who entered school on time. One possible explanation could be that students who were held back for a year were self-conscious and had feelings of inadequacy or low self-worth, leading to worse emotional and psychological well-being. After the reform, even though their school starting age was not affected, the perception of self changed from "delayed" to "on time," and the stigma related to delayed entry is no longer present. Because these students are always relatively older in their grade cohort regardless of the reform, the reform did not change their absolute or relative school starting age. In this case, the March-to-December-born students were not treated and served as a valid comparison group.

Compared to the existing literature on mental health, the magnitude of our estimates is comparable with prior findings, suggesting substantial impacts of the reform. Relative to Dee and Sievertsen (2018), which employs a fuzzy RD design with Danish data and demonstrates that one-year later entry reduced inattention/hyperactivity by 0.7 SD at age seven and 0.68 SD at age eleven, we observed no effects around ages 10 to 13 but found effects half the size for students around ages 13 to 16. In addition to the difference in institutional settings, the difference in significance for students in late childhood might be explained by our estimates capturing a different type of treatment: the self-perception of the presence of delayed entrance stigma, which may start to gain salience during adolescence.

3.6 Conclusion

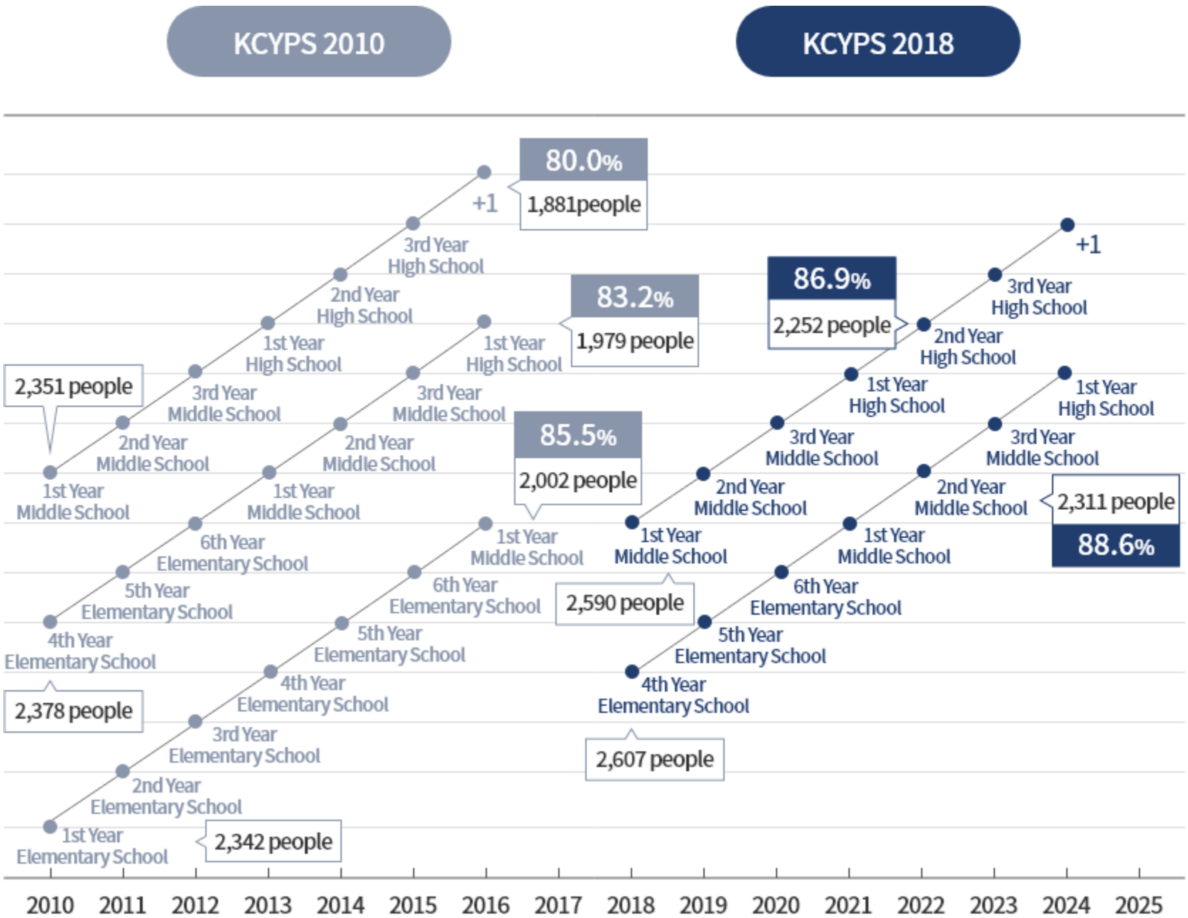
In this paper, we estimate the impact of a South Korean school starting age cutoff reform, which moved the cutoff from March 1 to January 1, on students' mental health outcomes, encompassing both internalizing and externalizing emotional/behavioral problems and self-esteem. We draw on comprehensive sets of Likert-scale questions on inattention, aggression, physical symptoms, social withdrawal, depression, and self-esteem surveyed among students at various developmental stages in the Korea Children and Youth Panel Survey (KCYPS). Using a difference-in-differences model, our analysis suggests that the reform substantially improved the mental health of students born in January and February.

In particular, we find that early-adolescent girls born in January and February who started school after the reform report lower levels of inattention and aggression compared to before. Early-adolescent boys born in January and February after the reform report higher levels of self-esteem relative to before. Additionally, our analysis uncover nuanced effects across different age groups and genders, underscoring the importance of considering developmental stages and gender in policy formulation. We hypothesize that the relative age within the classroom and the mindset of being the delayed entrants might explain the observed effects.

Looking ahead, our findings carry significant implications for policymakers and educators aiming to enhance mental health outcomes among students. By acknowledging the heterogeneous impacts of educational reform on mental health across gender and developmental stages, education policymakers and practitioners can design interventions tailored to address specific needs and vulnerabilities across diverse demographic groups. Future research into students' mental health and the underlying mechanisms driving these effects is crucial for informed education policy-making and the promotion of student well-being.

3.7 Figure

Figure 3.1: Korea Children and Youth Panel Survey Cohorts



3.8 Tables

Table 3.1: Descriptive Statistics of Predetermined Characteristics: Early Adolescence Cohorts

	Female				Male			
	Before reform		After reform		Before reform		After reform	
	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec
School starting age	6.466	6.620	7.010	6.644	6.699	6.647	7.087	6.643
	(0.463)	(0.290)	(0.316)	(0.240)	(0.487)	(0.288)	(0.224)	(0.250)
Firstborn	0.450	0.484	0.460	0.516	0.513	0.491	0.421	0.479
	(0.499)	(0.500)	(0.500)	(0.500)	(0.501)	(0.500)	(0.495)	(0.500)
Mother's education								
Less than college	0.427	0.488	0.293	0.270	0.503	0.475	0.347	0.290
	(0.496)	(0.500)	(0.456)	(0.444)	(0.501)	(0.500)	(0.477)	(0.454)
2-3 year college grad	0.147	0.113	0.274	0.293	0.061	0.132	0.276	0.256
	(0.355)	(0.317)	(0.447)	(0.456)	(0.240)	(0.338)	(0.448)	(0.436)
4 year college grad	0.390	0.366	0.363	0.386	0.372	0.364	0.343	0.372
	(0.489)	(0.482)	(0.482)	(0.487)	(0.485)	(0.482)	(0.476)	(0.484)
Graduate degree	0.036	0.033	0.071	0.050	0.064	0.029	0.033	0.083
	(0.187)	(0.178)	(0.257)	(0.219)	(0.246)	(0.169)	(0.180)	(0.275)
Father's education								
Less than college	0.341	0.369	0.259	0.259	0.366	0.367	0.322	0.256
	(0.475)	(0.483)	(0.439)	(0.438)	(0.483)	(0.482)	(0.468)	(0.437)
2-3 year college grad	0.105	0.081	0.163	0.205	0.046	0.086	0.125	0.189
	(0.307)	(0.273)	(0.370)	(0.404)	(0.211)	(0.280)	(0.331)	(0.392)
4 year college grad	0.489	0.487	0.459	0.422	0.451	0.474	0.451	0.442
	(0.501)	(0.500)	(0.500)	(0.494)	(0.499)	(0.500)	(0.499)	(0.497)
Graduate degree	0.065	0.064	0.119	0.113	0.137	0.073	0.103	0.112
	(0.247)	(0.244)	(0.325)	(0.317)	(0.345)	(0.260)	(0.304)	(0.316)
City/province of Home								
Seoul	0.148	0.178	0.155	0.163	0.203	0.168	0.239	0.147
	(0.355)	(0.383)	(0.363)	(0.370)	(0.403)	(0.374)	(0.427)	(0.354)
Busan	0.106	0.053	0.031	0.064	0.043	0.068	0.036	0.061
	(0.309)	(0.224)	(0.173)	(0.244)	(0.203)	(0.252)	(0.186)	(0.239)
Daegu	0.052	0.053	0.056	0.047	0.047	0.056	0.038	0.051
	(0.222)	(0.224)	(0.230)	(0.211)	(0.212)	(0.230)	(0.192)	(0.220)
Incheon	0.040	0.061	0.036	0.062	0.055	0.056	0.058	0.056
	(0.196)	(0.239)	(0.186)	(0.241)	(0.228)	(0.230)	(0.235)	(0.230)
Gwangju	0.028	0.037	0.042	0.028	0.011	0.040	0.025	0.036
	(0.167)	(0.189)	(0.200)	(0.166)	(0.106)	(0.196)	(0.158)	(0.185)
Daejeon	0.033	0.033	0.036	0.032	0.030	0.031	0.032	0.033
	(0.180)	(0.179)	(0.186)	(0.176)	(0.171)	(0.173)	(0.176)	(0.178)
Ulsan	0.034	0.024	0.029	0.022	0.023	0.027	0.014	0.026
	(0.182)	(0.153)	(0.169)	(0.147)	(0.149)	(0.163)	(0.117)	(0.160)
Sejong	0.000	0.000	0.009	0.007	0.000	0.000	0.011	0.006
	(0.000)	(0.000)	(0.096)	(0.082)	(0.000)	(0.000)	(0.106)	(0.076)
Gyeonggi-do	0.260	0.246	0.244	0.281	0.272	0.240	0.267	0.271
	(0.440)	(0.431)	(0.430)	(0.450)	(0.446)	(0.428)	(0.443)	(0.445)
Gangwon-do	0.033	0.030	0.070	0.021	0.020	0.031	0.040	0.028
	(0.178)	(0.171)	(0.255)	(0.144)	(0.140)	(0.174)	(0.196)	(0.165)
Chungcheongbuk-do	0.034	0.032	0.023	0.033	0.026	0.035	0.029	0.032
	(0.183)	(0.175)	(0.149)	(0.178)	(0.161)	(0.184)	(0.167)	(0.177)
Chungcheongnam-do	0.029	0.043	0.056	0.040	0.051	0.038	0.034	0.043
	(0.167)	(0.203)	(0.230)	(0.195)	(0.221)	(0.191)	(0.182)	(0.202)
Jeollabuk-do	0.037	0.039	0.045	0.041	0.055	0.035	0.041	0.038
	(0.189)	(0.193)	(0.207)	(0.198)	(0.228)	(0.185)	(0.198)	(0.190)
Jeollanam-do	0.043	0.040	0.046	0.033	0.035	0.039	0.017	0.039
	(0.203)	(0.195)	(0.210)	(0.178)	(0.184)	(0.194)	(0.130)	(0.194)
Gyeongsangbuk-do	0.040	0.051	0.040	0.047	0.051	0.053	0.028	0.052
	(0.197)	(0.220)	(0.197)	(0.212)	(0.221)	(0.224)	(0.165)	(0.223)
Gyeongsangnam-do	0.078	0.066	0.061	0.068	0.061	0.069	0.071	0.069
	(0.269)	(0.249)	(0.239)	(0.252)	(0.241)	(0.254)	(0.258)	(0.253)
Jeju-do	0.005	0.014	0.023	0.012	0.017	0.012	0.021	0.013
	(0.073)	(0.120)	(0.151)	(0.109)	(0.130)	(0.110)	(0.143)	(0.114)
Observations	209	966	193	992	202	974	228	1177

Note: Standard deviation in parentheses. Early adolescence cohorts are defined as the cohorts who first participated in the survey at grade seven. Before the reform estimates are from students who participated in the KCYPS 2010. After the reform estimates are from students who participated in KCYPS 2018.

Table 3.2: Descriptive Statistics of Predetermined Characteristics: Childhood Cohorts

	Female				Male			
	Before reform		After reform		Before reform		After reform	
	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec
School starting age	6.796	6.624	7.105	6.637	6.848	6.630	7.113	6.629
	(0.472)	(0.245)	(0.139)	(0.252)	(0.447)	(0.252)	(0.139)	(0.248)
Firstborn	0.467	0.476	0.460	0.425	0.432	0.445	0.475	0.490
	(0.500)	(0.500)	(0.500)	(0.495)	(0.496)	(0.497)	(0.501)	(0.500)
Mother's education								
Less than college	0.398	0.407	0.220	0.244	0.439	0.446	0.251	0.214
	(0.491)	(0.491)	(0.415)	(0.430)	(0.498)	(0.497)	(0.435)	(0.410)
2-3 year college grad	0.131	0.143	0.306	0.284	0.160	0.152	0.276	0.285
	(0.339)	(0.350)	(0.462)	(0.451)	(0.367)	(0.359)	(0.448)	(0.451)
4 year college grad	0.419	0.406	0.375	0.390	0.380	0.366	0.410	0.409
	(0.495)	(0.491)	(0.485)	(0.488)	(0.486)	(0.482)	(0.493)	(0.492)
Graduate degree	0.052	0.045	0.099	0.082	0.021	0.037	0.063	0.092
	(0.222)	(0.207)	(0.300)	(0.275)	(0.144)	(0.188)	(0.243)	(0.290)
Father's education								
Less than college	0.309	0.325	0.248	0.222	0.388	0.392	0.218	0.181
	(0.463)	(0.469)	(0.433)	(0.416)	(0.488)	(0.488)	(0.414)	(0.385)
2-3 year college grad	0.123	0.100	0.152	0.215	0.124	0.088	0.198	0.218
	(0.329)	(0.300)	(0.360)	(0.411)	(0.331)	(0.283)	(0.399)	(0.413)
4 year college grad	0.493	0.492	0.464	0.429	0.414	0.446	0.450	0.489
	(0.501)	(0.500)	(0.500)	(0.495)	(0.494)	(0.497)	(0.498)	(0.500)
Graduate degree	0.075	0.082	0.136	0.133	0.074	0.075	0.134	0.112
	(0.263)	(0.275)	(0.344)	(0.340)	(0.262)	(0.263)	(0.342)	(0.316)
City/province of Home								
Seoul	0.160	0.175	0.197	0.150	0.184	0.168	0.141	0.163
	(0.367)	(0.380)	(0.399)	(0.357)	(0.389)	(0.374)	(0.349)	(0.370)
Busan	0.096	0.052	0.048	0.058	0.079	0.057	0.047	0.059
	(0.295)	(0.222)	(0.213)	(0.234)	(0.270)	(0.231)	(0.212)	(0.235)
Daegu	0.065	0.047	0.048	0.047	0.062	0.049	0.046	0.047
	(0.248)	(0.211)	(0.214)	(0.212)	(0.243)	(0.216)	(0.209)	(0.212)
Incheon	0.052	0.056	0.027	0.067	0.077	0.050	0.082	0.052
	(0.222)	(0.231)	(0.161)	(0.250)	(0.267)	(0.218)	(0.274)	(0.223)
Gwangju	0.043	0.035	0.048	0.029	0.035	0.035	0.022	0.038
	(0.204)	(0.184)	(0.214)	(0.169)	(0.185)	(0.183)	(0.146)	(0.190)
Daejeon	0.043	0.031	0.031	0.032	0.023	0.035	0.036	0.030
	(0.203)	(0.174)	(0.175)	(0.176)	(0.151)	(0.184)	(0.186)	(0.171)
Ulsan	0.023	0.025	0.005	0.029	0.024	0.025	0.021	0.026
	(0.151)	(0.156)	(0.072)	(0.168)	(0.154)	(0.157)	(0.143)	(0.159)
Sejong	0.000	0.000	0.014	0.007	0.000	0.000	0.010	0.006
	(0.000)	(0.000)	(0.118)	(0.083)	(0.000)	(0.000)	(0.100)	(0.079)
Gyeonggi-do	0.205	0.268	0.258	0.282	0.196	0.268	0.297	0.269
	(0.405)	(0.443)	(0.438)	(0.450)	(0.398)	(0.443)	(0.458)	(0.444)
Gangwon-do	0.033	0.029	0.040	0.025	0.036	0.029	0.034	0.028
	(0.180)	(0.168)	(0.196)	(0.156)	(0.186)	(0.168)	(0.183)	(0.164)
Chungcheongbuk-do	0.036	0.031	0.048	0.027	0.039	0.030	0.009	0.038
	(0.187)	(0.172)	(0.214)	(0.161)	(0.195)	(0.171)	(0.093)	(0.191)
Chungcheongnam-do	0.059	0.039	0.041	0.044	0.046	0.041	0.055	0.040
	(0.236)	(0.193)	(0.198)	(0.204)	(0.210)	(0.199)	(0.228)	(0.196)
Jeollabuk-do	0.028	0.041	0.040	0.036	0.031	0.040	0.031	0.038
	(0.166)	(0.198)	(0.196)	(0.186)	(0.172)	(0.196)	(0.174)	(0.191)
Jeollanam-do	0.051	0.034	0.038	0.034	0.047	0.036	0.050	0.030
	(0.221)	(0.182)	(0.192)	(0.181)	(0.213)	(0.186)	(0.218)	(0.172)
Gyeongsangbuk-do	0.049	0.050	0.052	0.045	0.057	0.050	0.054	0.046
	(0.216)	(0.217)	(0.222)	(0.208)	(0.231)	(0.218)	(0.227)	(0.209)
Gyeongsangnam-do	0.037	0.074	0.050	0.075	0.052	0.074	0.057	0.074
	(0.190)	(0.262)	(0.218)	(0.263)	(0.222)	(0.262)	(0.232)	(0.262)
Jeju-do	0.019	0.013	0.016	0.014	0.011	0.014	0.009	0.016
	(0.136)	(0.113)	(0.126)	(0.119)	(0.105)	(0.116)	(0.094)	(0.125)
Observations	196	937	251	1043	219	1026	279	1034

Standard deviation in parentheses.

Note: Standard deviation in parentheses. Childhood cohorts are defined as the cohorts who first participated in the survey at grade four. Before the reform estimates are from students who participated in the KCYPS 2010. After the reform estimates are from students who participated in KCYPS 2018.

Table 3.3: Summary Statistics of Baseline Outcomes

	Entire sample				Female				Male			
	Before reform		After reform		Before reform		After reform		Before reform		After reform	
	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec	Jan - Feb	Mar - Dec
Panel A. Early Adolescence												
Inattention	2.388	2.392	2.080	2.191	2.456	2.370	2.027	2.148	2.323	2.413	2.135	2.230
	(0.586)	(0.525)	(0.556)	(0.569)	(0.559)	(0.508)	(0.555)	(0.579)	(0.605)	(0.539)	(0.553)	(0.557)
Aggression	2.165	2.129	1.851	1.942	2.289	2.149	1.824	1.977	2.048	2.111	1.879	1.911
	(0.596)	(0.582)	(0.578)	(0.601)	(0.578)	(0.568)	(0.572)	(0.621)	(0.590)	(0.595)	(0.584)	(0.581)
Physical symptoms	2.059	2.019	1.869	1.885	2.135	2.078	1.869	1.927	1.986	1.964	1.869	1.846
	(0.641)	(0.633)	(0.608)	(0.611)	(0.653)	(0.631)	(0.595)	(0.630)	(0.623)	(0.631)	(0.623)	(0.591)
Social withdrawal	2.254	2.228	2.132	2.138	2.288	2.243	2.145	2.161	2.221	2.214	2.118	2.117
	(0.711)	(0.711)	(0.777)	(0.693)	(0.706)	(0.699)	(0.819)	(0.694)	(0.715)	(0.721)	(0.733)	(0.691)
Depression	1.983	1.921	1.729	1.803	2.110	2.006	1.721	1.881	1.862	1.842	1.738	1.732
	(0.644)	(0.614)	(0.617)	(0.608)	(0.671)	(0.598)	(0.600)	(0.625)	(0.594)	(0.618)	(0.635)	(0.583)
Self-esteem	2.771	2.798	3.023	2.985	2.732	2.756	2.909	2.891	2.809	2.838	3.138	3.071
	(0.494)	(0.517)	(0.520)	(0.493)	(0.499)	(0.507)	(0.565)	(0.509)	(0.488)	(0.524)	(0.442)	(0.461)
Observations	396	1884	397	2041	198	930	181	939	198	954	216	1102
Panel B. Childhood												
Inattention	2.342	2.281	2.163	2.176	2.239	2.216	2.082	2.124	2.427	2.341	2.229	2.228
	(0.642)	(0.590)	(0.564)	(0.590)	(0.585)	(0.531)	(0.581)	(0.591)	(0.675)	(0.635)	(0.541)	(0.586)
Aggression	2.119	2.015	1.895	1.928	2.089	2.006	1.925	1.917	2.142	2.024	1.870	1.939
	(0.610)	(0.616)	(0.605)	(0.632)	(0.620)	(0.592)	(0.618)	(0.645)	(0.602)	(0.638)	(0.593)	(0.619)
Physical symptoms	1.922	1.878	1.811	1.809	1.980	1.946	1.840	1.816	1.874	1.815	1.787	1.803
	(0.690)	(0.624)	(0.616)	(0.639)	(0.721)	(0.620)	(0.619)	(0.641)	(0.662)	(0.621)	(0.614)	(0.636)
Social withdrawal	2.090	2.031	2.153	2.167	2.228	2.087	2.203	2.212	1.977	1.980	2.111	2.122
	(0.790)	(0.750)	(0.699)	(0.745)	(0.836)	(0.724)	(0.680)	(0.759)	(0.733)	(0.769)	(0.712)	(0.730)
Depression	1.753	1.696	1.741	1.755	1.826	1.788	1.823	1.790	1.694	1.610	1.672	1.721
	(0.663)	(0.592)	(0.610)	(0.617)	(0.670)	(0.605)	(0.642)	(0.632)	(0.652)	(0.567)	(0.575)	(0.600)
Self-esteem	2.987	3.022	3.090	3.079	3.017	3.037	3.067	3.082	2.962	3.008	3.110	3.077
	(0.463)	(0.461)	(0.462)	(0.470)	(0.484)	(0.462)	(0.487)	(0.493)	(0.444)	(0.460)	(0.439)	(0.448)
Observations	382	1837	486	1925	176	874	229	971	206	963	257	954

Note: Standard deviation in parentheses. The baseline is defined as a first overlapping grade (wave) across the 2010 panel and the 2018 panel. The first overlapping grade is grade 8 for inattention, aggression, physical symptoms, social withdrawal, and depression in the middle adolescent cohort; grade 7 for self-esteem in the middle adolescent cohort; grade 6 for inattention, aggression, physical symptoms, social withdrawal in the early adolescent cohort; and grade 5 for self-esteem in the early adolescent cohort. Data Source: Korea Children and Youth Panel Survey 2010 and 2018.

Table 3.4: Balance Test on Predetermined Characteristics

	Early adolescence			Childhood		
	Entire sample	Female	Male	Entire sample	Female	Male
Firstborn	-0.022	-0.042	-0.002	-0.002	0.007	-0.011
	(0.019)	(0.034)	(0.025)	(0.022)	(0.029)	(0.029)
Mother's education						
Less than college	0.011	-0.023	0.046	-0.012	-0.023	-0.002
	(0.021)	(0.033)	(0.032)	(0.018)	(0.030)	(0.025)
2-3 year college grad	-0.015	0.008	-0.040*	0.011	0.011	0.010
	(0.017)	(0.024)	(0.023)	(0.015)	(0.024)	(0.022)
4 year college grad	-0.001	0.005	-0.007	0.005	-0.001	0.010
	(0.022)	(0.031)	(0.031)	(0.022)	(0.032)	(0.028)
Graduate degree	0.005	0.009	0.001	-0.004	0.013	-0.018
	(0.013)	(0.014)	(0.019)	(0.010)	(0.017)	(0.012)
Father's education						
Less than college	0.007	-0.014	0.028	0.001	-0.000	0.002
	(0.024)	(0.030)	(0.034)	(0.018)	(0.029)	(0.026)
2-3 year college grad	-0.029**	-0.005	-0.052***	0.003	-0.014	0.018
	(0.013)	(0.021)	(0.016)	(0.014)	(0.021)	(0.019)
4 year college grad	0.003	0.017	-0.010	-0.011	0.014	-0.033
	(0.022)	(0.031)	(0.034)	(0.020)	(0.032)	(0.025)
Graduate degree	0.018	0.002	0.034*	0.007	-0.000	0.013
	(0.013)	(0.018)	(0.019)	(0.012)	(0.021)	(0.016)
City/province of home						
Seoul	0.018	-0.021	0.057*	0.005	0.014	-0.002
	(0.022)	(0.021)	(0.030)	(0.013)	(0.020)	(0.021)
Busan	-0.003	0.019	-0.025*	0.012	0.018	0.006
	(0.015)	(0.021)	(0.015)	(0.007)	(0.012)	(0.010)
Daegu	-0.004	0.003	-0.010	0.008	0.010	0.006
	(0.005)	(0.007)	(0.009)	(0.008)	(0.012)	(0.010)
Incheon	-0.011	-0.023*	-0.000	0.005	-0.021	0.028**
	(0.008)	(0.014)	(0.010)	(0.011)	(0.017)	(0.014)
Gwangju	-0.011	0.000	-0.021**	0.002	0.013	-0.007
	(0.007)	(0.007)	(0.010)	(0.005)	(0.012)	(0.007)
Daejeon	0.000	0.002	-0.001	0.001	0.006	-0.004
	(0.003)	(0.006)	(0.005)	(0.004)	(0.008)	(0.007)
Ulsan	0.001	0.009	-0.008*	-0.007	-0.012**	-0.003
	(0.005)	(0.011)	(0.004)	(0.004)	(0.006)	(0.007)
Sejong	0.001	0.001	0.002	0.003	0.004	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)
Gyeonggi-do	0.005	-0.007	0.017	-0.033*	-0.043*	-0.023
	(0.022)	(0.026)	(0.036)	(0.020)	(0.026)	(0.029)
Gangwon-do	0.009	0.021	-0.002	0.008	0.009	0.007
	(0.010)	(0.020)	(0.007)	(0.007)	(0.010)	(0.007)
Chungcheongbuk-do	-0.005	-0.002	-0.007	0.001	0.013	-0.009
	(0.006)	(0.010)	(0.004)	(0.004)	(0.009)	(0.007)
Chungcheongnam-do	0.001	-0.003	0.005	0.010	0.009	0.010
	(0.008)	(0.013)	(0.013)	(0.008)	(0.011)	(0.011)
Jeollabuk-do	0.007	0.000	0.013	-0.007	-0.005	-0.008
	(0.008)	(0.010)	(0.010)	(0.008)	(0.011)	(0.008)
Jeollanam-do	-0.002	0.007	-0.011	0.013*	0.011	0.015
	(0.005)	(0.010)	(0.007)	(0.007)	(0.008)	(0.012)
Gyeongsangbuk-do	-0.010*	-0.009	-0.010	0.005	0.002	0.007
	(0.006)	(0.011)	(0.008)	(0.008)	(0.008)	(0.014)
Gyeongsangnam-do	0.000	0.004	-0.004	-0.025***	-0.031**	-0.020**
	(0.009)	(0.011)	(0.015)	(0.009)	(0.013)	(0.010)
Jeju-do	0.002	-0.001	0.006*	-0.001	0.004	-0.005
	(0.003)	(0.006)	(0.003)	(0.003)	(0.007)	(0.004)

Note: Standard errors in parentheses. Statistical significance * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the first-wave school level. Weighted using the first-wave cross-sectional weights. Data Source: Korea Children and Youth Panel Survey 2010 and 2018.

Table 3.5: Differential MOB Effects on Emotional Well-beings - Baseline Model

	(1) inattention	(2) aggression	(3) physical symptoms	(4) social withdrawal	(5) depression	(6) self-esteem
Panel A. Early Adolescence, Female						
kcyps2018	0.0377	0.125**	0.0741	0.0330	0.0295	-0.132**
	(0.63)	(2.91)	(1.80)	(0.68)	(0.68)	(-2.98)
jan and feb	0.126	0.193**	0.0421	-0.0258	0.0418	0.0337
	(1.78)	(4.08)	(0.86)	(-0.45)	(0.55)	(0.61)
kcyps2018 × jan and feb	-0.277**	-0.356***	-0.0517	0.0311	-0.115	0.0638
	(-3.24)	(-4.68)	(-0.55)	(0.28)	(-0.81)	(0.92)
Observations	6777	6777	6781	6779	6776	6812
Panel B. Early Adolescence, Male						
kcyps2018	0.0648	0.0594	0.0669	0.0424	0.106*	0.0163
	(1.34)	(1.67)	(1.89)	(1.40)	(2.49)	(0.46)
jan and feb	-0.0442	0.0129	-0.0136	0.0965**	0.0599	-0.0771
	(-0.58)	(0.25)	(-0.26)	(2.82)	(1.47)	(-1.25)
kcyps2018 × jan and feb	-0.0502	-0.0209	0.00355	-0.0363	-0.0585	0.226**
	(-0.60)	(-0.21)	(0.05)	(-0.58)	(-1.01)	(2.83)
Observations	7208	7214	7213	7209	7212	7222
Panel C. Childhood, Female						
kcyps2018	-0.0242	-0.0371	-0.0997***	-0.0473**	-0.130***	-0.0415
	(-1.16)	(-1.65)	(-4.61)	(-2.58)	(-6.30)	(-1.84)
jan and feb	-0.0120	-0.00752	-0.0931	0.123**	-0.0209	-0.0747**
	(-0.27)	(-0.10)	(-1.55)	(2.76)	(-0.58)	(-2.85)
kcyps2018 × jan and feb	-0.0948	0.0388	0.101	-0.0927	0.0654	0.0720
	(-1.49)	(0.43)	(1.52)	(-1.36)	(1.41)	(1.30)
Observations	6701	6701	6700	6700	6699	6742
Panel D. Childhood, Male						
kcyps2018	0.0242	-0.00304	0.0722**	0.0580*	0.0722**	-0.0113
	(1.04)	(-0.11)	(3.38)	(2.52)	(2.79)	(-0.36)
jan and feb	0.0192	0.0263	-0.0288	-0.0709	-0.0201	-0.0141
	(0.35)	(0.46)	(-0.47)	(-1.35)	(-0.30)	(-0.23)
kcyps2018 × jan and feb	-0.0899	-0.119	-0.0345	-0.0352	-0.0841	0.117
	(-1.40)	(-1.58)	(-0.47)	(-0.50)	(-0.99)	(1.61)
Observations	7036	7037	7037	7035	7037	7061

Note: t statistics in parentheses. Statistical significance * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the school-grade level. Weighted using panel wave-specific cross-sectional weights. Data Source: Korea Children and Youth Panel Survey 2010 and 2018.

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