Does the early bird catch the worm? Evidence and interpretation on the long-term impact of school entry age in China☆

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ABSTRACT

The long-term impact of children's age at primary school entry on educational attainment and labor market outcomes is one of the primary concerns to families, educators, and policymakers. Using a nationally representative survey of families and individuals, this paper is among the first to explore these effects in a causal sense in the Chinese context and understand the underlying mechanisms. We use a regression discontinuity (RD) design that employs the threshold date for primary school entry set by the 1986 Compulsory Education Law of China as a source of exogenous variation in the timing of school entry. We first document a salient and robust compliance rate of school entry requirement. RD estimates indicate that being born right after the cutoff date significantly increases years of schooling and annual earnings for non-agricultural jobs for the full sample. We also observe remarkable heterogeneous effects on labor market performance by gender. Being born right after the cutoff increases the probability of being in the labor force for men, but decreases that for women. We find that the decline in female labor force participation is mainly driven by women who come from economically and socially disadvantaged families. Further evidence suggests that this decline can be explained by supply-side factors including fertility decision and childcare provisions.

1. Introduction

A key element in human capital investments for parents and policymakers to consider is at what age children should begin primary school. When making school enrollment decisions, parents from different countries often hold distinct views. In the United States, for

example, the average entry age to kindergarten has been rising over the past few decades largely due to parents voluntarily postponing the school entry of their children (Deming & Dynarski, 2008; Dhuey, Figlio, Karbownik, & Roth, 2019; Elder & Lubotsky, 2009). Quite the contrary, in China, many parents believe the old saying that "the early bird catches the worm", and thus have been increasingly seeking to advance the enrollment year for their children. An early transition to school is preferred by these parents because they hold the view that children would benefit from the transition. If it is not rewarding, they would still have more time to adjust and recover (Zhang & Xie, 2018). Moreover, enrollment at a younger age means earlier entry to the labor market given the same years of schooling completed. Therefore, these individuals can accumulate more work experience by the same age and have more time to explore alternative lifestyles and career options. They can also collect the returns from their human capital investments over a longer period of time. In a nutshell, parents that value early school enrollment treat it as a critical starting line for children's greater academic achievement and better labor market performance.

There is an extensive literature on the impact of school entry age on in-school performance[1]. However, the effects on educational attainment and long-run labor market outcomes are underexplored and the few existing studies reach different conclusions.[2] Even fewer studies are conducted in the context of developing countries where distinct social and cultural environments may lead to different implications.[3] This paper fills this gap in the literature by studying the long-term effects of school entry requirement in China and examining the possible mechanisms through which labor market performance is affected. China had never established formal regulations on primary school entry age until the passage of the Compulsory Education Law (CEL) in 1986. This national law requires that any children who have reached the age of six by August 31st of a calendar year should enter primary school in that year. This implies that children born after this threshold date in a calendar year are normally kept out of primary school until the next September.

We use the exogenous variation generated by the threshold date for primary school entry to examine the causal effect of school entry requirement on educational attainment and labor market outcomes including labor force participation (or LFP henceforth), employment status and personal annual earnings for non-agricultural jobs. Specifically, we employ a regression discontinuity (or RD henceforth) framework which enables us to identify the reduced-form impact of school entry requirement by comparing individuals who were born right before and after the cutoff date, as long as individuals cannot precisely control the birth month (the running variable in our setting) near the threshold. The comparison of outcome variables among individuals around the cutoff will distinguish the effect of school entry requirement from unobserved confounding factors such as parental preference.

We use data from wave 2012 of China Family Panel Studies (CFPS), a nationally representative survey of Chinese families and individuals. CFPS successfully interviewed about 13,000 families and around 35,000 individuals within these families in 2012. The data are apt for examining the research question addressed in this paper. The first advantage applies to the Chinese context and data on the whole. The CEL in China required all school-age children to receive universal nine-year education, regardless of their birth month. Unlike the United States where compulsory schooling laws specify minimum school leaving ages, children in China must complete a certain number of grades (that is, from 1st grade to 9th grade, or primary education and junior secondary education). This feature facilitates the identification of the school entry age effect.[4] A second advantage is that in CFPS specifically, we can obtain information on birth year, birth month, and entry year to primary school together with the outcome variables all from the same dataset. The school entry age can be easily computed based on birth year and primary school entry year. Therefore, we are able to estimate both the first-stage and reduced-form regressions using the same data. Another important feature of the data is that they provide a wealth of information covering demographic characteristics, educational background and employment history for both individuals and their family members (such as parents and children). They are ideally suited for providing credible estimates of the impact of school entry requirement and uncovering the potential mechanisms through which labor market performance is affected.

We begin our analysis by descriptively documenting an association of early primary school entry with higher levels of education completed. This is consistent with the motivation many Chinese parents maintain that children need to start school as early as possible so that they will "not lose at the starting line." However, OLS estimates are biased due to omitted variables and attenuation bias caused by parental preferences and measurement error in school entry age. We next consider the causal effects of school entry requirement.

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[1] See, for example: Fertig and Kluve (2005); Bedard and Dhuey (2006); Datar (2006); Lincove and Painter (2006); McEwan and Shapiro (2008); Elder and Lubotsky (2009); Smith (2009); Dobkin and Ferreira (2010); Mühlenweg and Puhani (2010); Kawaguchi (2011); Robertson (2011); Nam (2014); Cascio and Schanzenbach (2016); Fletcher and Kim (2016); Lubotsky and Kaestner (2016); Matta, Ribas, Sampaio, and Sampaio (2016); Peña (2017); Zhang et al. (2017); Attar and Cohen-Zada (2018); and Dhuey et al. (2019).

[2] In terms of educational attainment, prior studies show that delayed school entry could either raise (Bedard & Dhuey, 2006; Fredriksson & Öckert, 2014; Kawaguchi, 2011; Peña, 2017), or decrease (Dobkin & Ferreira, 2010) the level of education. Others find little evidence (Bedard & Dhuey, 2012; Black et al., 2011; Fertig & Kluve, 2005; Lincove & Painter, 2006; Nam, 2014). Research on labor market outcomes also yields mixed evidence. Bedard and Dhuey (2012), Fredriksson and Öckert (2014), Kawaguchi (2011), Matta et al. (2016), and Peña (2017) state that older school entrants earn higher wages. However, Black et al. (2011), Dobkin and Ferreira (2010), Lincove and Painter (2006), Nam (2014), and Larsen & Solli (2017) document no such wage effects in the long run.

[3] For example, Matta et al. (2016) explore the effect of school entry age in Brazil, and document that delaying primary school increases college admission test scores and probability of college admission. They also find that delaying school entry increases the early career earnings among males but not among females. Peña (2017) examines the effect of relative age on college attainment, employment status, and earnings in Mexico. The paper shows that students who were relatively older in their class attain more college education, have higher earnings, and have more educated spouses.

[4] Black et al. (2011), Kawaguchi (2011), and Fredriksson and Öckert (2014) argue that because of the minimum school leaving age requirement, children who start school at an older age complete lower levels of education when they reach the legal dropout age, compared to those who enter school at a younger age. The effect of school entry age could be contaminated by the effect of school leaving age requirement.

Our RD estimates indicate a salient and robust increase in primary school entry age for individuals born just after the cutoff date compared to their earlier-born counterparts due to the school entry requirement. Regarding educational attainment, we document that being born right after the cutoff results in a half-year increase of schooling completed on average. This impact is not only statistically but also economically significant. The education effect is more pronounced for males than females.

When examining labor market performance, we document statistically significant impacts on log annual earnings for non-agricultural jobs for the full sample. In addition, we observe remarkable heterogeneous effects by gender. To be specific, being born right after the cutoff increases the probability of being in the labor force for men, but decreases that for women. Moreover, males born right after the cutoff have better labor market performance for non-agricultural jobs compared to their earlier-born male counterparts. There is also a sizable earnings impact for non-agricultural jobs for later-born females. We argue that human capital accumulation serves as the main channel through which school entry requirement improves labor market performance. The finding that school entry requirement negatively affects LFP for women given the increased female educational attainment presents a puzzle. We first present that the significant decline in female LFP rate is mainly driven by women who come from economically and socially disadvantaged families. We further investigate the potential mechanisms between school entry requirement and female LFP, and find supportive evidence that labor supply-side factors account for the decline in female LFP. In terms of female labor supply, delayed school enrollment and increased education may postpone women's fertility decisions. Conditional on the same age, women with younger children need to spend more time on childcare, and therefore are less likely to enter the labor force. Consistent with our expectations, we observe that women born right after the cutoff are older at the first childbirth and more likely to raise younger child.

This paper contributes to two distinct strands of literature. First, we contribute to a growing body of research on the long-run effect of school entry age. It is an important policy-relevant question on how the short-run effects on academic achievement and non-cognitive performance translate into meaningful differences in adulthood outcomes, such as educational attainment, employment status, or earnings. However, current evidence on these topics is limited and inconclusive.[5] Furthermore, previous research on the long-term impact of school entry age largely focuses on developed countries.[6] There is limited understanding of the impact in developing countries.[7] This paper is among the first to examine the causal effect of primary school entry age on educational attainment and long-run labor market outcomes in the Chinese context.[8]

Second, our work extends a line of research studying the mechanisms through which school entry age affects labor market outcomes. Current studies argue that school entry age affects labor market performance in adulthood through academic performance (Datar, 2006; Dobkin & Ferreira, 2010; Elder & Lubotsky, 2009; Kawaguchi, 2011; Peña, 2017; Smith, 2009), human capital accumulation (Bedard & Dhuey, 2012; Dhuey et al., 2019; Lincove & Painter, 2006; Lubotsky & Kaestner, 2016), types of education (Mühlenweg & Puhani, 2010; Zweimüller, 2013), or educational attainment (Dobkin & Ferreira, 2010; Kawaguchi, 2011). Delayed school entry also implies less work experience at a given age, and this contributes to lower earnings in the early career (Black, Devereux, & Salvanes, 2011; Deming & Dynarski, 2008; Fredriksson & Öckert, 2014). This paper explores the mechanism from a different angle by investigating the supply-side determinants of labor force participation. To do so, we make full use of the demographic characteristics for both individuals and their family members from the nationally representative survey data. Our results provide supportive evidence that supply-side factors, including fertility and childcare, could explain the decline in female labor supply caused by the school entry requirement.

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[5] See footnote 2 for a list of relevant prior literature.

[6] For example, the United States (Bedard & Dhuey, 2012; Dobkin & Ferreira, 2010; Lincove & Painter, 2006), Austria (Zweimüller, 2013), Germany (Fertig & Kluve, 2005), Norway (Black et al., 2011; Larsen & Solli, 2017), Sweden (Fredriksson & Öckert, 2014), Japan (Kawaguchi, 2011), and South Korea (Nam, 2014).

[7] See footnote 3 for a list of relevant prior literature.

[8] Most pertinent to our paper with respect to the research question is the research by Di, Zhu, and Li (2018) and Li, Lou, and Zhang (2022). Both papers explore the impact of primary school entry age on educational attainment and labor market outcomes. However, they are fundamentally different from our study in terms of the research design and sample coverage. Di et al. (2018) use China 2005 mini-census data and draw similar conclusions that delayed school enrollment increases years of schooling completed and wages under a regression discontinuity framework. Our paper differs and complements Di et al. (2018) in three main aspects. First, regarding the identification strategy, we use birth cohorts who were affected by the school entry requirement to estimate the treatment effects, and then separately use birth cohorts who were not at all affected by the requirement to conduct falsification tests. However, Di et al. (2018) pool both groups together when identifying the treatment effects. Second, regarding the empirical analyses, we not only provide empirical evidence on the first stage estimate (i.e., the impact of school entry requirement on primary school entry age), but also explore the gender differences in labor market performance. However, these analyses are not available in Di et al. (2018). Third, regarding the analytical sample, our paper includes more cohorts (born between 1979 and 1991) who entered primary school after the implementation of the CEL, and both urban and rural sample. However, Di et al. (2018) only use urban sample, and their post-treatment birth cohorts end in 1981. Li et al. (2022) use the Chinese Adult Twins Survey and their within-twin fixed effects estimates show that a delay in primary school starting age lowers years of schooling but does not affect labor market outcomes. Our distinct conclusions are likely driven by considerable differences in the identification strategy and sample coverage. First, Li et al. (2022) use the within-twin fixed effects and compare the outcomes between each of the twin siblings who started primary school at different ages. They argue that family's financial situation is one possible explanation for why twins from a family started primary school at different ages. Our regression discontinuity framework addresses the endogeneity problem by comparing individuals who were born right before and after the school entry cutoff date. Second, Li et al. (2022) use only urban twins sample in five selected Chinese cities, and a very large proportion of the sample was born before the implementation of the CEL. Furthermore, given the inclusion of within-twin fixed effects in the regression model, only twins that have different school entry ages (5.5% of their total sample) contribute to the estimation of the impact of school entry age in practice. Our paper uses a nationally representative survey of individuals born between 1979 and 1991 who entered primary school after the implementation of the CEL. We also include both urban and rural samples.

The remainder of the paper proceeds as follows: Section 2 describes the institutional background about the educational system, school entry requirements, and *Hukou* system in China. Section 3 includes a description of the data and presents summary statistics for the analytical sample. Section 4 discusses our identification strategy, while Section 5 examines the validity of regression discontinuity design. Section 6 describes the regression discontinuity analysis of the effect of school entry age on educational attainment and labor market outcomes, in comparison with the OLS results. Section 7 provides a discussion on gender differences in labor market performance and explains the potential mechanisms between school entry requirement and female LFP. Section 8 concludes.

2. Institutional background

2.1. China's educational system

The educational system in China is composed of four stages: preschool, primary, secondary and postsecondary. Children usually enroll in preschool or kindergarten at the age of two or three, and many of these schools are privately owned. Primary school education typically starts at age six (or seven in less developed areas), and mostly spans six years (or five years for a small number of primary schools). Primary schooling is followed by secondary education, which consists of two parts: junior secondary education and senior secondary education. Junior secondary education normally requires three years of study. The nine-year schooling in primary and junior secondary schools pertains to the compulsory education. Different from the education laws in the United States, there is no minimum school-leaving age requirement in China's educational system.

Upon completing compulsory education, students could choose whether to enter three-year senior secondary schools such as general (academic) senior secondary (known as *putong gaozhong* in Chinese), vocational senior secondary (*zhongzhuan*), or technical secondary (*jixiao*). The postsecondary education in China, or higher education as it is usually called, is similarly structured as most western countries; undergraduate programs are typically four years long and a master's degree lasts for two or three years. The completion of a master's degree is required for admission to a doctoral program, which takes three to five years to complete depending on the field of study.

Senior secondary school admissions are based on test scores in a city-level entrance exam named *zhongkao*. Acceptances to undergraduate programs are entirely determined by test scores in the National College Entrance Examination (known as *gaokao*). Admissions at the graduate level mostly depend on National Postgraduate Entrance Examination (known as *yanjiusheng ruxue kaoshi*). Some students can be admitted into graduate schools through exam-free recommendations (known as *tuijian mianshi*).

2.2. The school entry requirement of the compulsory education law in China

Article 11 of the Compulsory Education Law in China that took effect on July 1, 1986 states that, "Any child who has attained to the age of 6, his/her parents or other statutory guardians shall have him/her enrolled in school to finish compulsory education*[sic]*. For the children in those areas where the conditions are not satisfied, the initial time of schooling may be postponed to 7 years old."[9] According to the interpretation of Article 11 from Legislative Affairs Commission of the NPC Standing Committee, children reaching age six by August 31st should enroll in primary school.[10] Academic redshirting is not permitted in China. The Law requires that only school-age children with illness, disabilities, or under other special circumstances are eligible to postpone their primary school enrollment; their parents shall file an application with the education administrative department of local governments for approval. The actual implementation year of the CEL varies by province and municipality, as shown in Appendix Table A1.[11] The majority of the country implemented the Law between 1986 and 1988, which indicates that the first birth cohorts impacted by the Law are those born in or after 1979.

2.3. The Hukou system in China

Hukou is the governmental system of household registration and social identity that originated in ancient China for population management. Contemporary *Hukou* records a resident's identifying information from birth such as name, date of birth, family relations and residential address. Based on the birth place (either rural or urban) and kinship at birth, an individual is assigned to either agricultural or non-agricultural *Hukou* status. Difference in *Hukou* status marks different social benefits an individual can obtain, such as education, health insurance and health care, retirement pension and land use rights. Residents with agricultural *Hukou* may convert

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[9] The Law created a system of nine-year compulsory education across the whole country, which does not charge any tuition or miscellaneous fees. More details are available on the official website of the Ministry of Education of China: http://en.moe.gov.cn/documents/laws_policies/201506/t20150626_191391.html.

[10] See Chunying Xin (2012), The Interpretation of Compulsory Education Law of the People's Republic of China (in Chinese: Zhonghua Renmin Gongheguo Yiwu Jiaoyufa Shiyi). We also confirmed the accuracy of the threshold date for primary school entry based on information from multiple sources: (1) The document, "Administrative Measures for Records of Students' Enrollment Status in Primary and Secondary Schools" (in Chinese: Zhongxiao Xuesheng Xueji Guanli Xiangguan Wenti Chuli Banfa), from the General Office of the Ministry of Education (available at http://www.moe.gov.cn/srcsite/A06/jcys_jyzb/201606/t20160627_269836.html); (2) Information provided on the official websites of the Department of Education for provinces and municipalities.

[11] We collected the information from the official websites of the Department of Education for provinces and municipalities.

their *Hukou* status to non-agricultural *Hukou* through education, employment in state sectors, military services, marriage, real estate purchase and investment in urban areas, and land expropriation (Xiang, 2015). In 2012, there were about 970 million people with agricultural *Hukou*, accounting for 72% of the total population.[12] Due to education inequality between rural and urban areas, this paper also investigates the heterogeneous effects by *Hukou* origin.

3. Data and descriptive statistics

The source of data for our analysis is China Family Panel Studies (CFPS), a nationally representative, annual longitudinal survey of Chinese families and individuals initiated by the Institute of Social Science Survey (ISSS) of Peking University in China. The baseline survey was launched in 2010. It covers 25 provinces, representing 95% of the Chinese population.[13] The biggest advantage of CFPS data is that it provides information on each individual's birth year and birth month, as well as the entrance year to primary school. This allows us to deduce each person's actual primary school entry age by subtracting birth year from entry year. Other individual characteristics at birth that are available in the survey include province of birth, *Hukou* status, parental educational attainment, gender, and ethnicity.[14] The survey also collected time-varying demographic characteristics, such as age, marital status, urbanicity, current *Hukou* status, and school enrollment status. For each individual, we observe both the highest level of education they have achieved, and their labor market outcomes such as labor force participation, employment status and personal annual earnings for non-agricultural jobs.

We use wave 2012 in our analysis with an initial sample of 35,719 adults.[15] We keep birth cohorts who were affected by the school entry requirement based on the actual implementation date of the 1986 Compulsory Education Law for each province (see Appendix Table A1). This drops our sample to 5025 adults. Furthermore, we restrict the sample to individuals aged 21 or older to ensure that most of the respondents in the data have completed their postsecondary education. To address the concerns about reporting and measurement errors in actual primary school entry age, we also eliminate the <2 % of individuals with an entry age younger than four or older than nine. Our final analytical sample consists of 3839 individuals born in the period between 1979 and 1991.

According to the Article 11 of the national CEL, when children have reached the age of six, their parents shall send them to schools to receive and complete compulsory education. To better understand this article, consider two persons: Ms. Early who was born in August in 1980 and Ms. Late who was born in September of the same year. Suppose that they both comply with the law. Ms. Early should enter primary school in 1986 at the actual age of 6.08 (or $6 + \frac{9-8}{12}$). However, Ms. Late had to wait until 1987 to enter primary school, because she did not attain to the age of six by August 31st in 1986. Therefore, the actual entry age for Ms. Late is close to seven. The actual primary school entry age is not directly reported in our data. Instead, we manually compute the entry age using information on birth year and birth month, as well as each person's primary school entry year.[16] The formula used to calculate the precise actual entry age is:

$$Entry\_Age_i = \left(Entry\_Year_i - Birth\_Year_i\right) + \left(\frac{9 - Birth\_Month_i}{12}\right),$$

where $Entry\_Age_i$ refers to the precise actual primary school entry age for individual i, $Entry\_Year_i$ is the primary school entry year, $Birth\_Year_i$ is the birth year, and $Birth\_Month_i$ is the birth month.[17] The relationship between the actual age at primary school entry and birth month represents the first stage in our RD framework.

Table 1 reports the summary statistics of the key variables for the analytical sample. Panel A describes the key dependent variables of interest. The average primary school entry age is about 7.2. Individuals complete 10.4 years of schooling on average, indicating that they just complete junior secondary education. Labor force participation, employment status and personal annual earnings for non-agricultural jobs are the main labor market outcomes of focus. 88% of the sample are in the labor force, among whom 76% are employed for non-agricultural jobs. Conditional on being employed for those jobs, the average annual earnings are about 26,000 RMBs (or 4000 USDs).

Panel B of Table 1 shows descriptive statistics for predetermined individual characteristics and time-varying demographics. 85% of

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Table 1
Summary statistics for the analytical sample.

	Mean	SD
Panel A: Key Dependent Variables of Interest		
Primary School Entry Age	7.163	0.866
Years of Schooling Completed	10.402	3.579
1 If in the Labor Force	0.882	0.323
1 If Employed for Non-Agricultural Jobs	0.759	0.428
Annual Earnings for Non-Agricultural Jobs (in RMBs)	25,705.240	44,620.591
Panel B: Individual Characteristics		
1 If *Hukou* Status at Age 3 is Non-Agricultural	0.155	0.362
1 If in Eastern Region at Birth	0.451	0.498
1 If in Central Region at Birth	0.341	0.474
1 If in Western Region at Birth	0.208	0.406
Father's Years of Schooling	7.691	3.737
Mother's Years of Schooling	5.734	4.340
1 If Female	0.507	0.500
1 If Han Ethnic Group	0.938	0.241
1 If Current *Hukou* Status is Non-Agricultural	0.293	0.455
1 If Currently Urban	0.481	0.500
1 If Currently Married	0.656	0.475
Age	25.864	3.127
Number of Individuals	3839	

Note: There are two types of *Hukou* Status: agricultural and non-agricultural. Han ethnic group has the largest population among the total 56 ethnic groups in China. The eastern region includes: Beijing, Tianjin, Hebei, Liaoning, Shanghai, Jiangsu, Zhejiang, Fujian, Shandong, Guangdong, and Hainan. The central region includes: Shanxi, Jilin, Heilongjiang, Anhui, Jiangxi, Henan, Hubei, and Hunan. The western region includes: Inner Mongolia, Sichuan, Chongqing, Guizhou, Yunnan, Shannxi, Gansu, Qinghai, Ningxia, Xinjiang, Guangxi, and Tibet. The eastern region is the most developed region in China, while the western region is the least developed.

the sample have agricultural *Hukou* at age 3. This fraction decreases to 71% at the time of survey, because some people converted their agricultural *Hukou* to non-agricultural. Parents only received limited education on average. Years of parental education are 7.7 years for fathers and 5.7 years for mothers. Almost everyone (94%) belongs to the Han ethnicity.[18] Half of adults in the sample are female, and roughly half the sample currently reside in urban areas.

4. Empirical methods

In this section, we discuss the identification strategy and empirical models used to identify the causal impact of primary school entry age. Our main aim is to examine how age at primary school entry affects educational attainment and labor market outcomes in adulthood. In Appendix B, we estimate a series of OLS models that explore the correlations between school entry age and outcomes of interest. However, the OLS estimates are prone to be biased downward due to the possibility of omitted variables correlated with both primary school entry age and the outcomes, and biased towards zero if the observed primary school entry age is measured with random errors (see more discussions in Appendix B).

To address these concerns, we adopt an identification strategy which takes advantage of the school entry requirement in the 1986 China Compulsory Education Law that mandates the primary school entry age and the cutoff date at which children should enroll in primary school. We use this cutoff date as a source of exogenous variation in the timing of primary school entry. Specifically, we apply a regression discontinuity framework to this policy context. RD design allows us to examine the effects by comparing "all else equal" individuals with different actual school entry ages due to the school entry requirement. Formally, we estimate the following first-stage and reduced-form equations:

$$Entry\_Age_i = \alpha_0 + \alpha_1 After_i + \alpha_2 Month_i + \alpha_3 (After_i \times Month_i) + \theta X_i + u_i, \tag{1}$$

$$Y_i = \beta_0 + \beta_1 After_i + \beta_2 Month_i + \beta_3 (After_i \times Month_i) + \gamma X_i + \varepsilon_i, \tag{2}$$

where i indexes individuals. Independent variables are exactly the same in both equations. $Entry\_Age_i$ is a continuous variable of the actual primary school entry age. Y_i denotes the long-term outcomes including years of schooling completed, an indicator of whether being in the labor force, an indicator of whether being employed for non-agricultural jobs, and annual earnings for non-agricultural jobs (in log level) for individual i. $After_i$ is a dummy variable indicating whether individual i was born in September or later months of a

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[18] There are 56 ethnic groups in China, among which Han has the largest population. Based on 2010 Population Census Data, the Han majority represents 91.6% of the total population, the remaining 8.4% being composed of 55 ethnic minorities. See http://english.gov.cn/archive/china_abc/ 2014/08/27/content_281474983873388.htm and http://www.stats.gov.cn/tjsj/pcsj/rkpc/6rp/indexch.htm for more details.

calendar year. *Month_i* is the running variable, and represents the number of months between individual *i*'s birth month and August. It is equal to zero for individuals born in August, positive for those born in or after September, and negative for those born prior to August. *X_i* is a set of control variables such as birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Birth year fixed effects account for national shocks that impact all individuals born in the same year. Birth region fixed effects control for variation in outcomes that are common across birth cohorts within a birth region. u_i and ε_i are the error terms. We cluster standard errors at the birth-month level, as suggested in Lee and Card (2008).

The key identifying assumption underlying our RD design is that there is no perfect manipulation around the mandated cutoff date (see Lee & Lemieux, 2010), which we assess below.[19] If this assumption is satisfied, unbiased estimates could be obtained by simply comparing the average outcomes of individuals just to the left and just to the right of the cutoff date. In our RD models, the coefficients of interest are α_1 and β_1. They reflect the discontinuity at the threshold for the first-stage and reduced-form regressions, respectively.

5. Validity checks for the regression discontinuity design

Regression discontinuity estimates will return unbiased estimation of the causal effects of primary school entry only if other determinants of educational attainment and labor market outcomes are balanced across the mandated cutoff date. One potential threat to our RD identification strategy is the possibility that parents precisely manipulate the birth month of their children either through conception or birth decisions. Parents who believe in the old saying that "the early bird catches the worm" may wish that their children enroll in primary school at a younger age, and therefore plan for the timing of birth with the hope that their children could be born before September. We consider two approaches to test the validity of the RD design and show that this is not a major concern in our context. First, we ask whether there is any evidence of sorting around the cutoff month. Under our identifying assumption, there should be no such manipulation. In contrast, if there is manipulation as discussed above, we expect to see an abrupt increase in the number of individuals born just to the left of the cutoff, and a noticeable decline just to the right of the cutoff. Fig. 1 plots the histogram of the relative birth month (i.e., the running variable). There is no evidence of density discontinuity around the cutoff month, which is consistent with our identifying assumption. Moreover, given that the running variable is discrete in our setting, we implement a test proposed by Frandsen (2017) for the presence of manipulation around the cutoff. The test cannot reject the null hypothesis of no manipulation, with *p*-values ranging from 0.186 ($k = 0$) to 0.567 ($k = 0.1$).[20] This provides supporting evidence of no difference in density at the discontinuity.

The second test of RD validity in our setting is to check the balance of predetermined individual observable characteristics across the threshold. These covariates include *Hukou* status at age 3, birth region, parental education, gender, and ethnicity. As displayed in Fig. 2, there is little evidence that these predetermined characteristics vary discontinuously across the mandated cutoff date. Appendix Table A2 shows the corresponding estimated discontinuities in these covariates around the cutoff. All discontinuities are small in magnitude with narrow confidence intervals, compared to the mean value and standard deviation as shown in Table 1. On the whole, there is no evidence of discontinuities in the covariates. This suggests that individuals born on either side of the cutoff are comparable. Taken together, these results show that perfect manipulation of the running variable is unlikely in this context and treatment is as good as randomly assigned near the cutoff.[21]

6. Main results

6.1. Graphical evidence

To address the potential biases in estimating the causal impact of school entry age, we employ the RD approach that exploits the mandated cutoff date as a source of exogenous variation in the timing of primary school entry. We begin with a visual representation of our regression results. Fig. 3 plots the unconditional mean value of actual primary school entry age, years of schooling completed, probability of being in the labor force, probability of being employed for non-agricultural jobs, and log annual earnings for non-

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[19] The monotonicity assumption is less likely to be violated in our context. First, as stated in Barua and Lang (2016), monotonicity can be particularly problematic when redshirting is prevalent. In China, however, redshirting is not possible because it is not permitted by the CEL. Second, a lack of monotonicity in our context suggests that there exist children whose parents determine to bear the costs of enrolling kids earlier than at the age specified by the CEL if born after the mandated cutoff but at the same time are willing to pay additional penalties of enrolling kids later than at the age specified by the CEL if born before the cutoff. This type of behavior would both be inconsistent with parental preferences for their children being among the oldest in the classroom as well as preferences for them being the youngest (Landersø, Nielsen, & Simonsen, 2017). More discussions on parental preferences in the Chinese context are available in Appendix B. Third, our empirical strategy in this paper is exclusively based on a reduced-form fuzzy RD approach rather than instrumental variables. This is to say, we aim to identify the intention-to-treat effects rather than the local average treatment effects.

[20] We implement the test using the Stata command *rddisttestk*. The choice of parameter k ($k \geq 0$) determines the maximal degree of nonlinearity in the probability mass function that is still considered to be compatible with no manipulation. A small k means even small deviations from linearity will lead the test to reject with high probability. Following Frandsen (2017), we choose $k = 0$ and $k = 0.1$ in the manipulation test. See Frandsen (2017) for more details.

[21] Huang et al. (2020) state that low-educated mothers are less likely to manipulate the birth timing in the Chinese context. This provides supporting evidence that perfect manipulation of the running variable is unlikely in our setting, given the fact that mothers' education is low in our analytical sample (around 5.7 years of schooling completed on average) and it is balanced around the primary school entry cutoff date.
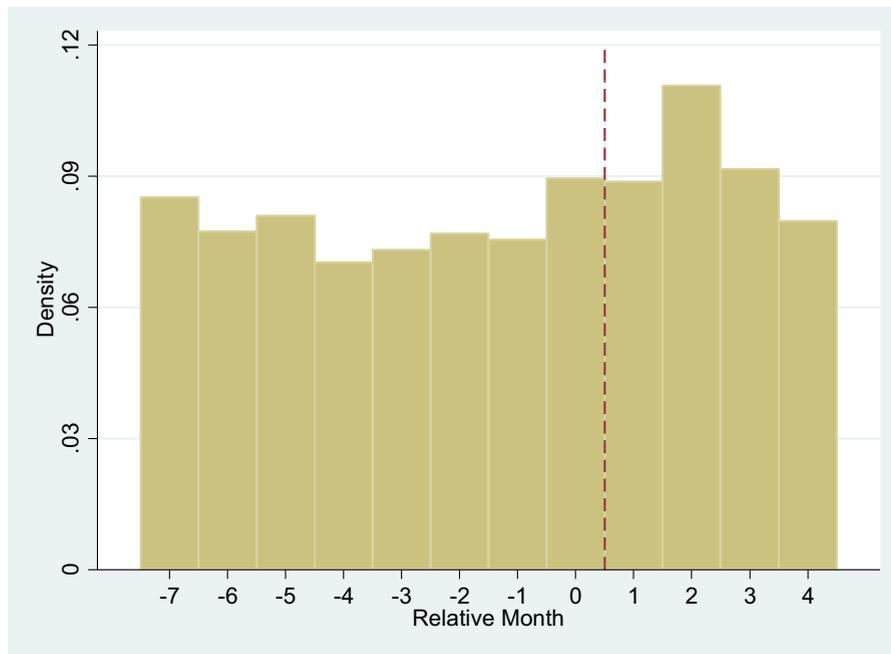
Fig. 1. Histogram of running variable.
Note: This figure shows the density of individuals born in each month based on the analytical sample. Birth month is the running variable. Relative month indicates the number of months between one individual's birth month and August. Therefore, "-7" for relative month refers to January for birth month; "0" for relative month refers to August for birth month; "4" for relative month refers to December for birth month.

agricultural jobs, respectively. We can observe from these figures that there is a distinct jump in primary school entry age, years of schooling completed, and log annual earnings for non-agricultural jobs at the cutoff, whereas no clear discontinuities are found in other outcomes. Moreover, we residualize each of these dependent variables respectively by using all controls discussed in Section 4, and plot the residuals against the running variable separately. These conditional scatterplots are presented in Appendix Fig. A1. Almost same patterns are observed with respect to the discontinuities at the threshold when comparing to Fig. 3. The fact that the discontinuities are insensitive to the inclusion of these controls offers further support for the validity of our RD design. The figures are consistent with the corresponding RD regression results, which will be discussed later in this section.

6.2. The impact of school entry requirement on primary school entry age

Table 2 reports the regression discontinuity estimates of coefficient α_1 in Eq. (1), which reflect the first-stage effects of primary school entry requirement on actual school entry age. All specifications produce statistically significant and robust estimates of the discontinuity at the 1% significance level. The estimated discontinuity is around 0.28 years, which means that because of the requirement children born right after the cutoff month are on average 0.28 years (or 3.4 months) older than their counterparts born right before the threshold at the time when they enroll in primary school. In other words, the compliance rate of primary school entry requirement among individuals born around the cutoff date is about 28%. This suggests that the school entry cutoff date mandated by the CEL results in a noticeable increase in primary school entry age for individuals around the cutoff, although compliance with the requirement is far from being perfect—the predicted discontinuity with perfect compliance is nearly one year.

Existing literature documents higher compliance rates with school entry requirements in developed countries, such as the United States (Elder & Lubotsky, 2009), Norway (Black et al., 2011), Sweden (Fredriksson & Öckert, 2014), and Japan (Kawaguchi, 2011). Our finding of relatively low compliance rate in China is consistent with current studies that examine the school entry requirement of the CEL of China.[22] There are four main reasons why a lower compliance rate is observed in our context. First, the CEL allows economically underdeveloped areas, areas inhabited by ethnic minority groups, agricultural and pastoral farming areas, and mountainous and desert areas to enroll students at age seven instead of age six, given that those areas may have limited facilities and teachers for primary education. Second, individuals in our analytical sample were born between 1979 and 1991, the birth cohorts affected by the school entry requirement of the CEL in its first decade of implementation. The requirement was not strictly enforced during the

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[22] Zhang and Xie (2018) show that the compliance rate for children who enrolled in primary school from 1986 to 1991 ranges from 30% to 40%. Zhang et al. (2017) use data for 7th graders in the 2013–2014 school year, and report that the compliance rate for these students is around 30%. Meng (2020) uses four waves of CFPS data and document a compliance rate of 23%.
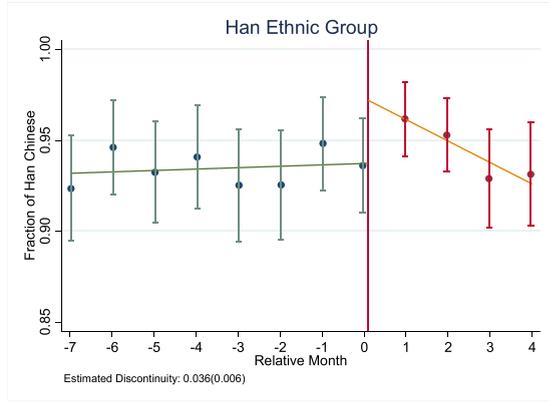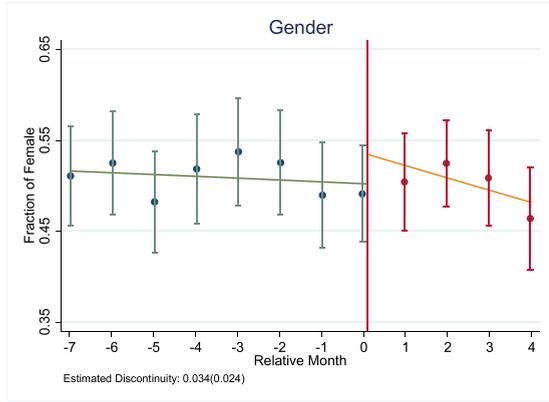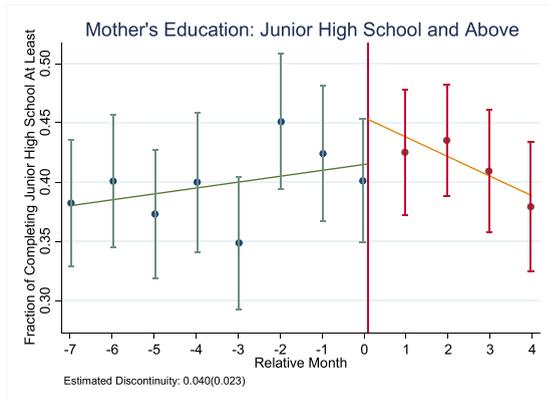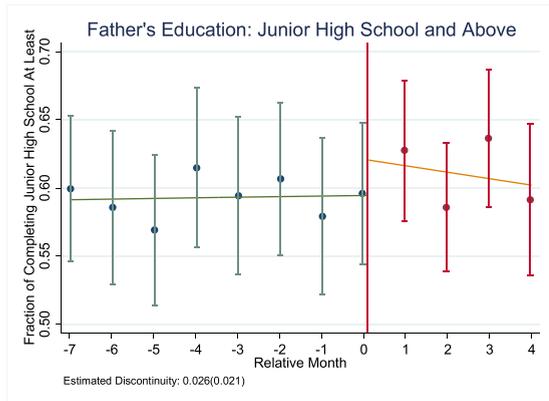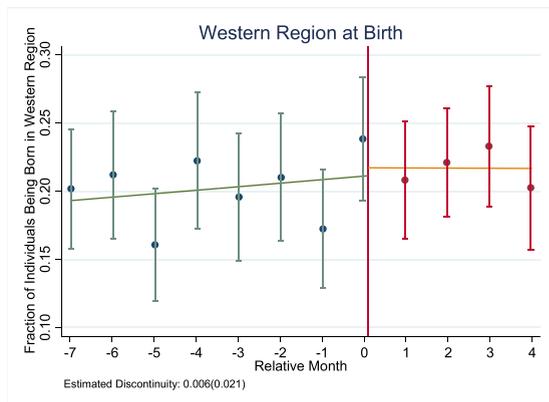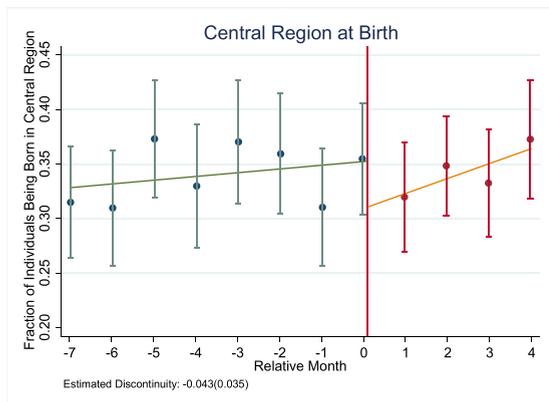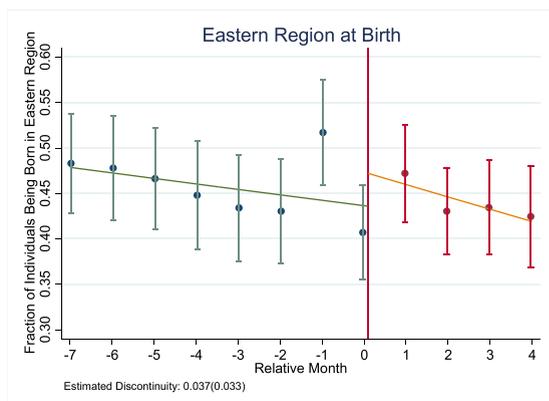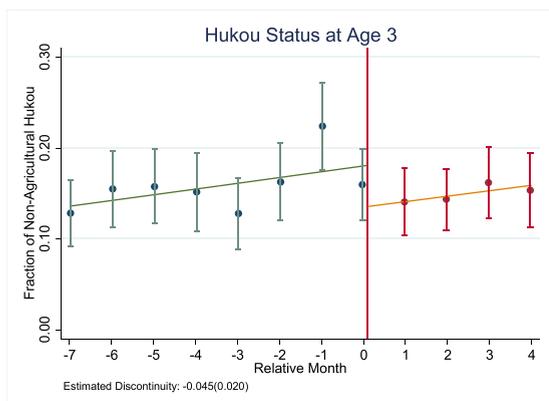
(caption on next page)

Fig. 2. Smoothness tests for predetermined individual covariates.
Note: Figures display means and linear fitted values of predetermined individual covariates by birth month relative to the threshold month August. Dependent variables are given by figure title and y-axis title. Points reflect the average of dependent variables by each birth month. 95% confidence intervals are presented for each plot. Discontinuities at the threshold are calculated and displayed in the notes below each figure. Standard errors in parentheses and clustered at the birth-month level.

initial years of implementation. Schools usually did not allow children to enroll in primary school earlier, but not all schools adhered to the rule (Zhang & Xie, 2018). Third, many Chinese parents, in particular those with more resources, might take advantage of their personal relationships or pay extra fees to ensure that their children can enter primary school ahead of schedule (Liu & Li, 2016). Forth, parents who need their children to help with household chores, childcare, farm work, or even wage work may want to delay the enrollment. These parents, particularly those in rural areas, complain that schooling is the additional drain upon family resources.

6.3. The impact of school entry requirement on educational attainment and labor market outcomes

In this section we estimate the reduced-form effects of school entry requirement on educational attainment and labor market outcomes. The estimated discontinuities and standard errors are presented in Table 3. Panel A shows that without controlling for any covariates (Column 1), there is a positive and statistically significant discontinuity of 0.577 years in years of schooling completed right at the cutoff. The discontinuity shrinks slightly to 0.520 when birth year fixed effects and birth region fixed effects are included (Column 2). The estimates remain constant across specifications with additional controls (Columns 3 and 4). The addition of control variables does not affect estimates in a meaningful way, which is consistent with the identifying assumption. Our results indicate that individuals born right after the cutoff date obtain about 6 more months of schooling than those born right before the cutoff on average. Current studies have investigated and documented supportive evidence on mechanisms through which the school entry requirement increases education attainment. First, children that start school later are more likely to have the necessary maturity to succeed in school than their younger counterparts. Older entrants therefore perform better, including higher test scores and less grade retention (Bedard & Dhuey, 2006; Datar, 2006; Elder & Lubotsky, 2009; McEwan & Shapiro, 2008). Second, children who have a greater stock of cognitive and non-cognitive skills at the beginning of schooling experience greater benefits from these skills in subsequent years, as stated by Heckman and various coauthors (see Cunha, Heckman, Lochner, & Masterov, 2006). Due to the delayed school enrollment, children have more time to develop skills through early investments made by their parents. This could contribute to children's human capital acquisition in the future (Elder & Lubotsky, 2009; Lubotsky & Kaestner, 2016).

Panels B to D of Table 3 show results for labor market outcomes. Panel B presents the RD estimates for the effect on the probability of being in the labor force with different sets of controls. Overall, the point estimates are small in magnitude with narrow confidence intervals, which suggests little impact of school entry requirement on LFP. In Panel D where the dependent variable is log annual earnings for non-agricultural jobs, the discontinuity coefficients are positive and largely insensitive to additional control variables to the baseline regression. We find that being born right after the cutoff date increases annual earnings of non-agricultural jobs by about 0.46 log points (or 59%) on average, or 3585 RMBs, conditional on being employed for this type of jobs. The earnings impact is not only statistically but also economically significant. RD estimates for the probability of being employed for non-agricultural jobs (see Panel C) are positive, which are consistent with the sign of coefficients for log annual earnings.

Compared to the corresponding OLS estimates in Appendix B, the RD coefficients for educational attainment and labor market outcomes for non-agricultural jobs flip signs, and the magnitude is substantially larger when taking into account the first-stage discontinuity. This likely reflects the elimination of downward bias due to the possibility of omitted variables correlated with both primary school entry age and the outcomes, and of attenuation bias due to measurement error in the observed primary school entry age (see more discussions in Appendix B).

For each outcome discussed above, we also consider how the estimated discontinuities vary according to the bandwidth chosen with all controls included in the regressions. Panel A of Table 4 shows the robustness of the first-stage results, while Panels B to E report how reduced-form estimates change across bandwidths for educational attainment and labor market outcomes. The estimates appear to be generally similar across bandwidth choices (Columns 1 to 4) relative to the window we use in the baseline RD regressions (i.e., Jan-Dec, Column 5 of Table 4). We also check the robustness of the findings to nonparametric estimation using optimal bandwidths developed by Calonico, Cattaneo, and Titiunik (2014) and Calonico, Cattaneo, Farrell, and Titiunik (2017). As shown in Columns 6 and 7, the results are robust to the parametric estimates and main conclusions remain unchanged.

6.4. Falsification tests

In this section, we perform a series of falsification tests that lend additional confidence in the validity of our results in Sections 6.2 and 6.3. First, we repeat the RD estimation using the sample of older cohorts who had entered primary school before the school entry requirement was implemented.[23] This group of individuals should have not been affected by the policy, and therefore we would expect

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[23] We restrict the sample by keeping individuals whose primary school entry year is before the implementation year of the CEL for each province (see Appendix Table A1). The sample consists of 10,703 individuals. The age of these individuals ranges from 28 to 62, with an average age equal to 46. The average primary school entry age is about 8.1.
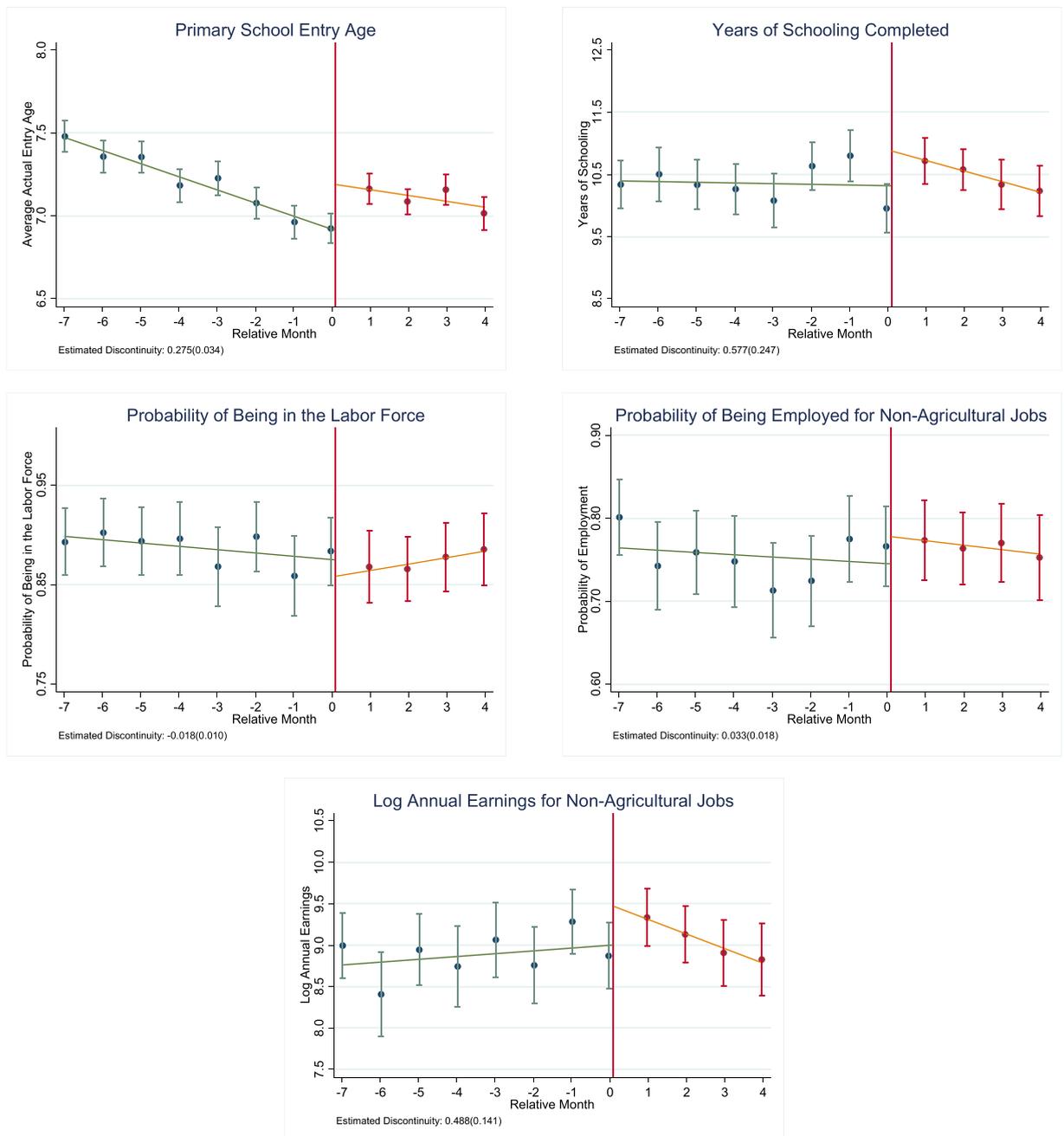
Fig. 3. Unconditional scatterplots for key outcomes of interest against birth month.
Note: Figures display means and linear fitted values of key outcomes of interest by birth month relative to the threshold month August. The outcome variables include primary school entry age, years of schooling completed, probability of being in the labor force, probability of being employed for non-agricultural jobs, and log annual earnings for non-agricultural jobs. Dependent variables are given by figure title and y-axis title. Points reflect the average of dependent variables by each birth month. 95% confidence intervals are presented for each plot. Discontinuities at the threshold are calculated and displayed in the notes below each figure. Standard errors in parentheses and clustered at the birth-month level.

Table 2
The impact of school entry requirement on primary school entry age (RD: First stage).

	Primary school entry age			
	(1)	(2)	(3)	(4)
Discontinuity	0.275***	0.282***	0.282***	0.283***
	(0.034)	(0.036)	(0.033)	(0.033)
Birth Year FE	No	Yes	Yes	Yes
Birth Region FE	No	Yes	Yes	Yes
Hukou Status at Age 3	No	No	Yes	Yes
Parental Education	No	No	Yes	Yes
Gender	No	No	No	Yes
Ethnicity	No	No	No	Yes
Observations	3839	3839	3839	3839

Note: This table presents estimates of coefficient α_1 in Eq. (1) where the dependent variable is the actual primary school entry age. Observations are at the individual level. Regression discontinuity models control for birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3
The impact of school entry requirement on educational attainment and labor market outcomes (RD: Reduced form).

	(1)	(2)	(3)	(4)
Panel A: Years of Schooling Completed				
Discontinuity	0.577**	0.520**	0.542**	0.528**
	(0.247)	(0.226)	(0.193)	(0.189)
Observations	3839	3839	3839	3839
Panel B: Probability of Being in the Labor Force				
Discontinuity	−0.018	−0.017	−0.019	−0.015
	(0.010)	(0.012)	(0.012)	(0.011)
Observations	3839	3839	3839	3839
Panel C: Probability of Being Employed for Non-Agricultural Jobs				
Discontinuity	0.033*	0.028	0.027	0.026
	(0.018)	(0.018)	(0.019)	(0.018)
Observations	3386	3386	3386	3386
Panel D: Log Annual Earnings for Non-Agricultural Jobs				
Discontinuity	0.488***	0.444***	0.434***	0.463***
	(0.141)	(0.123)	(0.120)	(0.115)
Observations	2429	2429	2429	2429
		Control Variables		
Birth Year FE	No	Yes	Yes	Yes
Birth Region FE	No	Yes	Yes	Yes
Hukou Status at Age 3	No	No	Yes	Yes
Parental Education	No	No	Yes	Yes
Gender	No	No	No	Yes
Ethnicity	No	No	No	Yes

Note: This table presents estimates of coefficient β_1 in Eq. (2) where the dependent variable is years of schooling completed (Panel A), probability of being in the labor force (Panel B), probability of being employed for non-agricultural jobs (Panel C), and log annual earnings for non-agricultural jobs (Panel D), respectively. Observations are at the individual level. Regression discontinuity models control for birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4

Robustness checks of estimated discontinuities.

	Bandwidth: Aug-Sep	Bandwidth: Jun-Nov	Bandwidth: Apr-Dec	Bandwidth: Feb-Dec	Bandwidth: Jan-Dec	Local Polynomial: 1st Order	Local Polynomial: 2nd Order
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Primary School Entry Age							
Discontinuity	0.231**	0.234***	0.284***	0.280***	0.283***	0.283***	0.250***
	(0.011)	(0.048)	(0.035)	(0.034)	(0.033)	(0.018)	(0.040)
Observations	685	2047	2904	3512	3839	3839	3839
Panel B: Years of Schooling Completed							
Discontinuity	0.714***	0.807***	0.519*	0.526**	0.528**	0.742***	0.596***
	(0.001)	(0.127)	(0.241)	(0.203)	(0.189)	(0.147)	(0.008)
Observations	685	2047	2904	3512	3839	3839	3839
Panel C: Probability of Being in the Labor Force							
Discontinuity	−0.018	−0.012	−0.015	−0.015	−0.015	−0.010	−0.010
	(0.003)	(0.015)	(0.013)	(0.012)	(0.011)	(0.008)	(0.007)
Observations	685	2047	2904	3512	3839	3839	3839
Panel D: Probability of Being Employed for Non-Agricultural Jobs							
Discontinuity	−0.013	−0.020**	0.006	0.015	0.026	0.013***	0.011***
	(0.009)	(0.007)	(0.011)	(0.013)	(0.018)	(0.005)	(0.001)
Observations	600	1791	2548	3094	3386	3386	3386
Panel E: Log Annual Earnings for Non-Agricultural Jobs							
Discontinuity	0.261	0.406***	0.428***	0.386***	0.463***	0.430***	0.455***
	(0.117)	(0.081)	(0.089)	(0.114)	(0.115)	(0.074)	(0.163)
Observations	440	1297	1836	2221	2429	2429	2429

Note: Columns (1)–(5) present estimates of coefficient α_1 in Eq. (1) where the dependent variable is the actual primary school entry age (Panel A), and estimates of coefficient β_1 in Eq. (2) where the dependent variable is years of schooling completed (Panel B), probability of being in the labor force (Panel C), probability of being employed for non-agricultural jobs (Panel D), and log annual earnings for non-agricultural jobs (Panel E), respectively. Columns (6) and (7) present estimates from local polynomial estimation (1st order polynomial in Column 6 and 2nd order polynomial in Column 7) using optimal bandwidths developed by Calonico et al. (2014, 2017). The coefficients in all columns are estimated by controlling for birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5

Falsification tests.

	Primary School Entry Age	Years of Schooling Completed	Probability of Being in the Labor Force	Probability of Being Employed for Non-Agricultural Jobs	Log Annual Earnings for Non-Agricultural Jobs
	(1)	(2)	(3)	(4)	(5)
Panel A: Using a Sample Not Affected by School Entry Requirement					
Discontinuity	0.035	0.045	0.012	0.018	0.026
	(0.027)	(0.060)	(0.013)	(0.013)	(0.061)
Observations	10,703	10,703	10,703	9080	4959
Panel B: Assuming July 31st as the Cutoff					
Discontinuity	0.021	−0.250	−0.001	0.048	0.184
	(0.123)	(0.344)	(0.017)	(0.031)	(0.274)
Observations	3839	3839	3839	3386	2429
Panel C: Assuming September 30th as the Cutoff					
Discontinuity	0.188	0.267	−0.009	0.019	0.092
	(0.103)	(0.212)	(0.008)	(0.014)	(0.127)
Observations	3839	3839	3839	3386	2429

Note: Columns (1)–(5) present estimated treatment effects using the same regression discontinuity identification strategy as shown in Eqs. (1) and (2). The dependent variable of the regression model is the actual primary school entry age (Column 1), years of schooling completed (Column 2), probability of being in the labor force (Column 3), probability of being employed for non-agricultural jobs (Column 4), and log annual earnings for non-agricultural jobs (Column 5), respectively. Estimated coefficients in Panel A are based on a sample of individuals who had entered primary school before the school entry requirement was implemented. Estimated coefficients in Panel B are based on the same regression discontinuity model but assuming July as the cutoff instead of August. Estimated coefficients in Panel C are based on the same regression discontinuity model but assuming September as the cutoff instead of August. Regression discontinuity models control for birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

to observe no treatment effects using the same RD estimation strategy in our context. The estimation results are presented in Panel A of Table 5. We document no effects of school entry requirement on primary school entry age, educational attainment, and labor market outcomes for individuals who enrolled in primary school in the pretreatment period. This finding is consistent with our expectation. This also directly supports the RD hypothesis that unobservables are balanced around the cutoff.

Second, we implement placebo tests by estimating the treatment effects at fake thresholds. We use July and September as the cutoff month instead of August, respectively. As shown in Panels B and C of Table 5, the jumps at these fake thresholds are never significantly different from zero, as one should expect.

7. Exploring and interpreting gender differences

China's rapid economic growth has been accompanied by widening gender gaps (Brussevich, Dabla-Norris, & Li, 2021). According to the World Bank, the ratio of female to male labor force participation rate has declined from 85.9% in 1990 to 81.5% in 2010, and then gradually increased to 82.9% in 2021.[24] It has always been a state policy in China to promote equality between men and women. The effect of school entry requirement has been so far estimated for the entire analytical sample. In this section, we explore whether the impact on each outcome variable differs by gender and provide explanations on gender differences in labor market performance.

7.1. Evidence on gender differences

As shown in Table 6, we find that the compliance rate is relatively lower for females (Panel A). Results also indicate that being born right after the cutoff date increases years of schooling completed by 0.62 years for men, which is higher than the 0.44-year increase for women (Panel B). In terms of labor market outcomes, we also document significant and sizable effects of school entry requirement by gender. Specifically, we find that being born right after the cutoff date significantly increases the probability of being in the labor force by 4.1 percentage points on average for males, but decreases for females by 7.5 percentage points (Panel C). The striking differential impacts on LFP between males and females will be further discussed in the next section. In addition, males born right after the cutoff are 5.4 percentage points more likely to be employed for non-agricultural jobs than their earlier-born male counterparts (Panel D). The impact on log annual earnings is also positive among males, albeit less precisely estimated. Remarkably, being born right after the cutoff leads to an earnings effect of 0.57 log points (or 77%) on average, or 3161 RMBs, conditional on being employed for non-agricultural jobs among females. The statistically significant discontinuities on labor market outcomes can also be observed clearly at the threshold in Fig. 4 where we display the unconditional scatterplots for these outcomes against the relative birth month by gender. Corresponding conditional scatterplots are presented in Appendix Fig. A2, which show consistent patterns regarding the discontinuities. To summarize, for both men and women, we find that being born right after the cutoff increases years of schooling and leads to larger earnings effects in non-agricultural jobs conditional on being employed. However, unlike males who experience a positive effect on LFP with delayed school entrance, the impact among females is to the opposite direction.

7.2. Interpretation on gender differences in labor market performance

In Section 7.1 we reported that being born right after the cutoff date increases labor force participation and labor market performance for non-agricultural jobs among males. Even though being born right after the cutoff contributes to a substantially large earnings effect for females conditional on being employed for non-agricultural jobs, it unexpectedly causes a significant decline in the female LFP rate. To begin with, the positive effects on labor market outcomes are consistent with human capital theories. There are sizable increases in years of schooling completed due to school entry requirement for both males and females, as shown in Panel B of Table 6. Individuals with higher levels of education are much more likely to participate in the labor market and have better labor market performance because of human capital accumulation (Becker, 1962; Mincer, 1974). Human capital investments build up the stock of knowledge, skills or characteristics for workers that are highly valued in the labor market because they increase workers' productivity. Therefore, educated workers are more likely to join the labor force and take higher-paying jobs.

But why does the progress in female educational attainment not translate to higher LFP for women? We now turn to a discussion of various explanations for the finding by using individual demographic information collected from the CFPS survey. Descriptively, as shown in Appendix Table A3, the average level of education attained for females is junior high school, about the completion of nine-year compulsory education. The average age is 26 years old. Around 20% of females are not in the labor force. Among these females, one third of them are out of the labor force because they need to take on housework and childcare.[25]

In addition, we document that the significant decline in female LFP rate is mainly driven by women who come from economically and socially disadvantaged families. Panel C of Table 7 shows the heterogeneous treatment effects on the probability of being in the labor force by *Hukou* status at age 3 and parental education for females. Column (1) presents that being born right after the cutoff decreases the LFP rate by 10.2 percentage points for women with agricultural *Hukou* status at age 3. There are also sizable negative

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[24] Source: Ratio of female to male labor force participation rate (%) (modeled ILO estimate) – China, World Development Indicators, the World Bank.

[25] In our analytical sample, there were in total 1946 females. 382 of them are not in the labor force. The two main reasons why they do not participate into the labor force are taking on housework and childcare (126 females) and being unable to find a suitable job (59 females). About 83% of those not in the labor force are currently married.

Table 6
The impact of school entry requirement by gender.

	Gender	
	Males	Females
	(1)	(2)
Panel A: Primary School Entry Age		
Discontinuity	0.327***	0.249***
	(0.046)	(0.062)
Observations	1893	1946
Panel B: Years of Schooling Completed		
Discontinuity	0.623**	0.436**
	(0.267)	(0.153)
Observations	1893	1946
Panel C: Probability of Being in the Labor Force		
Discontinuity	0.041***	−0.075***
	(0.011)	(0.020)
Observations	1893	1946
Panel D: Probability of Being Employed for Non-Agricultural Jobs		
Discontinuity	0.054***	−0.008
	(0.012)	(0.035)
Observations	1822	1564
Panel E: Log Annual Earnings for Non-Agricultural Jobs		
Discontinuity	0.321	0.570***
	(0.227)	(0.107)
Observations	1364	1065

Note: This table presents estimates of coefficient α_1 in Eq. (1) where the dependent variable is the actual primary school entry age (Panel A), and estimates of coefficient β_1 in Eq. (2) where the dependent variable is years of schooling completed (Panel B), probability of being in the labor force (Panel C), probability of being employed for non-agricultural jobs (Panel D), and log annual earnings for non-agricultural jobs (Panel E), respectively. Estimation in Column (1) uses the male sample only, and the female sample only in Column (2). The coefficients in all columns are estimated by controlling for birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

effects for women with less educated parents; estimates show that the magnitude of these effects is between 8.9 and 11.2 percentage points if the highest level of education for parents is less than junior high school completion (Columns 3 and 5). Panel A of Table 7 presents that women with better-educated parents on average have higher compliance rates. This is consistent with the finding in the Chinese context that parents with more schooling are more likely to comply with the school entry requirement and enroll their children on time (e.g., Huang, Zhang, & Zhao, 2020; Zhang, Zhong, & Zhang, 2017). In Panel B of Table 7, we document that women from better-educated families on average have larger positive effects on years of schooling.[26] As we will discuss later in this section, delayed school enrollment and increased education could affect women's fertility and childcare provisions, leading to the decline in LFP across subsamples. This effect is less pronounced among women from better educated families. One possible explanation is that women from advantaged families are more likely to remain attached to the labor force after marriage, postpone first births longer, and have shorter employment interruptions after childbirth (Amin & Behrman, 2014; Hook & Paek, 2020).[27]

We further explore factors accounting for the decline in female LFP rate from the perspective of female labor supply. Fertility and childcare provisions are important determinants of married women's labor supply choices. Increased education may be associated with postponed fertility because of the high costs to have a child in school (known as "incarceration effect") and the opportunity cost due to higher wage rate ("human capital effect") (see Black, Devereux, & Salvanes, 2008). Given the same age of mothers, delayed fertility suggests that children are younger and need more care from mothers, which could reduce the probability of mothers' LFP.

In order to probe the possibility that primary school entry affects female labor supply because of fertility choices and childcare, we

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[26] In Section 6.3, we discussed the mechanisms through which the school entry requirement increases education attainment. One potential explanation on the heterogeneity by parental education is that, since older enrollees have more time to accumulate basic skills with their parents before entering school, better educated parents are able to invest more in their children, who will then achieve more in school.

[27] In our analytical sample, among women with less educated parents, over 65% of them have children when surveyed; however, this proportion is <50% for women from better educated families, indicating that they are more likely to delay childbearing to an older age on average. Consistent with the findings on female LFP by parental education and mechanisms discussed in this section, unreported results show that there are positive effects on "probability of having a child under age 2" for women from less-educated families. The coefficients for women from better-educated families are also positive but much smaller in magnitude.

Panel A: Males

Panel B: Females
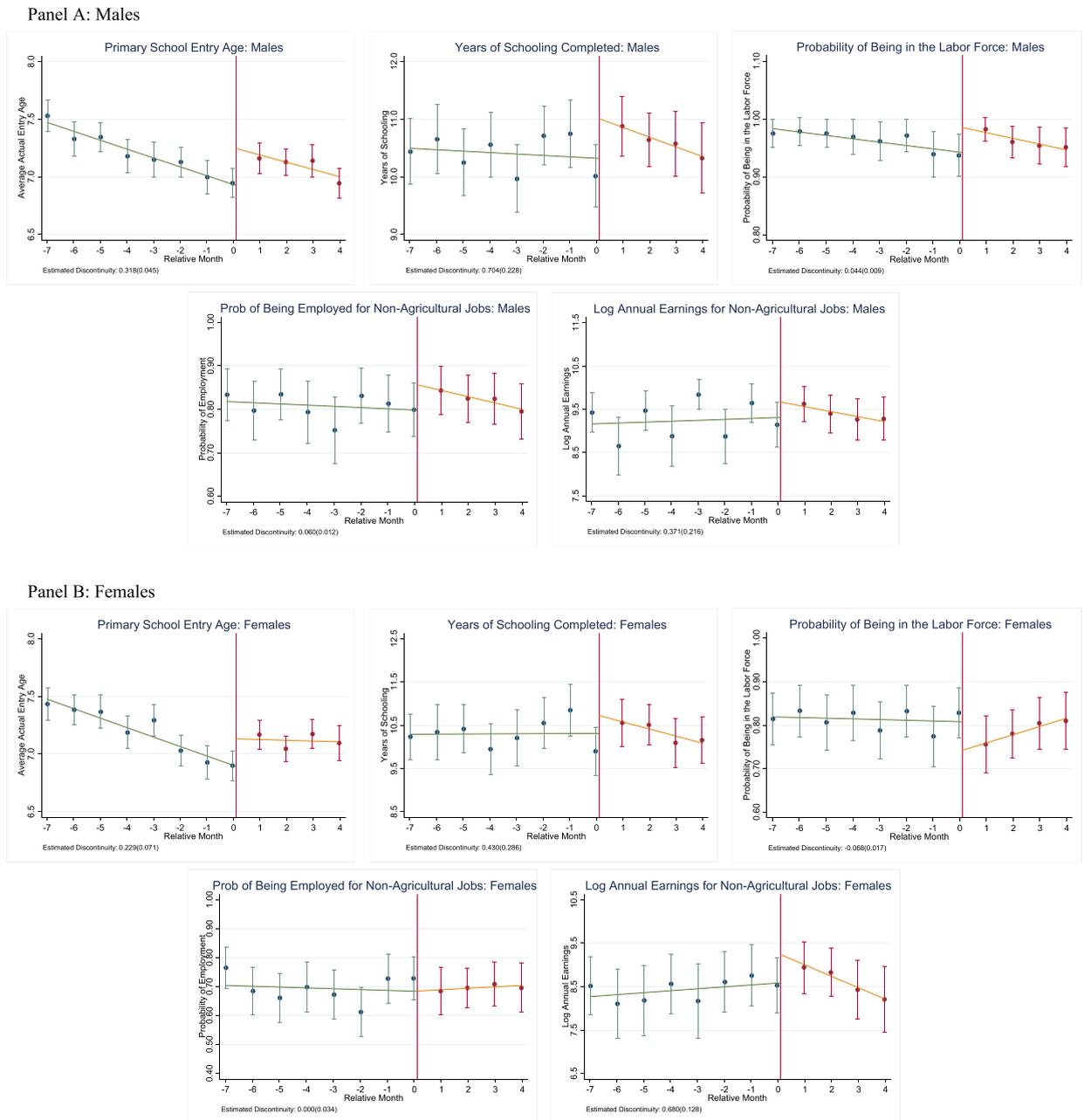
Fig. 4. Unconditional scatterplots for key outcomes of interest against birth month: By gender.
Note: Figures display means and linear fitted values of key outcomes of interest by birth month relative to the threshold month August. The outcome variables include primary school entry age, years of schooling completed, probability of being in the labor force, probability of being employed for non-agricultural jobs, and log annual earnings for non-agricultural jobs. Panel A shows the unconditional scatterplots for the male sample, and Panel B for the female sample. Dependent variables are given by figure title and y-axis title. Points reflect the average of dependent variables by each birth month. 95% confidence intervals are presented for each plot. Discontinuities at the threshold are calculated and displayed in the notes below each figure. Standard errors in parentheses and clustered at the birth-month level.

Table 7
Heterogeneous effects on school entry age, educational attainment, and labor force participation for females.

	Hukou Status at Age 3		Mother's Education		Father's Education	
	Agricultural	Non-Agricultural	Below Junior HS	Junior HS or Higher	Below Junior HS	Junior HS or Higher
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Primary School Entry Age						
Discontinuity	0.265***	0.156	0.166*	0.324***	0.065	0.318***
	(0.064)	(0.202)	(0.080)	(0.053)	(0.072)	(0.100)
Observations	1639	307	1144	802	774	1172
Panel B: Years of Schooling Completed						
Discontinuity	0.416*	0.448	0.417	0.722*	0.329	0.834**
	(0.198)	(0.339)	(0.268)	(0.340)	(0.603)	(0.324)
Observations	1639	307	1144	802	774	1172
Panel C: Probability of Being in the Labor Force						
Discontinuity	−0.102***	0.076	−0.112***	−0.025	−0.089**	−0.047
	(0.020)	(0.059)	(0.030)	(0.032)	(0.031)	(0.037)
Observations	1639	307	1144	802	774	1172

Note: This table presents estimates of coefficient α_1 in Eq. (1) where the dependent variable is the actual primary school entry age (Panel A), and estimates of coefficient β_1 in Eq. (2) where the dependent variable is years of schooling completed (Panel B) and probability of being in the labor force (Panel C), respectively, by dividing the female sample based on *Hukou* Status at Age 3 (Columns 1 and 2), mother's education (Columns 3 and 4), and father's education (Columns 5 and 6). The coefficients in all columns are estimated by controlling for birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8
Interpretations of female labor force participation.

	Females:
	Full Sample
Panel A: Probability of Being in the Labor Force	
Discontinuity	−0.075***
	(0.020)
Observations	1946
Panel B: Probability of Motherhood	
Discontinuity	−0.007
	(0.029)
Observations	1946
Panel C: Mothers' Age at First Birth	
Discontinuity	0.142
	(0.312)
Observations	1105
Panel D: Age of the Youngest Child	
Discontinuity	−0.187
	(0.282)
Observations	1105
Panel E: Probability of Having a Child Under Age 2	
Discontinuity	0.085**
	(0.036)
Observations	1105

Note: This table presents estimates of reduced form regressions for different mechanisms through which school entry requirement affects female labor force participation. The coefficients in the column are estimated by controlling for birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

examine the impact on four potential outcomes: probability of motherhood, mothers' age when she first gave birth, age of the youngest child, and probability of having a child under age 2. Children under age 2 require more intensive care and supervision from families, which makes mothers less likely to enter the labor force. As shown in Table 8, first, the impact of the school entry requirement on the probability of motherhood is very small and not statistically different from zero (Panel B). Women born right after the cutoff are on average older at the first childbirth (Panel C) and more likely to raise younger children (Panel D) than those born right before the cutoff. Although we do not find statistically significant effects on these two outcomes, the standard errors are large, so we cannot rule out large effects on postponed childbearing. Moreover, there is an 8.5 percentage point increase in the probability of currently having a child under age 2 due to the school entry requirement for mothers (Panel E). This further presents evidence on the role of childcare in reducing mothers' LFP.

Our empirical results above indicate that women born right after the cutoff are more likely to leave the labor force because of supply-side factors, such as fertility and childcare provisions. This is likely a result of late primary school enrollment, more education, and hence delayed childbearing and higher childcare burden given the same age. The explanation of the effects as being due to childcare is broadly consistent with the works by Maurer-Fazio, Connelly, Chen, and Tang (2011) and Hare (2016), who report that the presence of coresident preschool-aged children decreases women's likelihood of participating in the labor force in the Chinese context. Our interpretations shed light on supply-side mechanisms between school entry age and female LFP, and they can be generally applied to understand female labor market effects documented in other research that studies under similar social and cultural background but lacks relevant fertility and childcare information. It is quite possible that childbearing only drives a temporary labor market exit and the negative effect on female LFP fades away in the long term among women born on either side of the cutoff.

8. Conclusion

For decades it has been an important question looming in parents' mind whether their children should enroll in primary school at a younger or older age. School entry requirement that determines children's starting age for primary education brings long-run effects on their educational attainment and labor market performance in adulthood. We reach this conclusion by exploiting an exogenous variation generated by the threshold date for primary school entry in China under a regression discontinuity framework. Our paper is among the first causal research to document such effects in the Chinese context and unveil potential mechanisms to understand the labor market effects.

In this paper, we first document a noticeable and robust compliance rate of school entry requirement. Regression discontinuity estimates show that being born right after the cutoff date increases years of schooling completed by roughly one half of a year. The education effect is both statistically and economically significant, and more pronounced for men than women. While those born after the cutoff exhibit a sizable positive earnings impact for non-agricultural jobs conditional on being employed, we observe remarkable heterogeneous effects on labor force participation rate by gender: being born right after the cutoff increases the probability of being in the labor force for men, but decreases that for women.

Considering increased educational attainment, the finding of lower female LFP caused by school entry requirement presents a puzzle that motivates further investigation of the mechanisms explaining the striking gender differences. We show that the significant decline in female LFP rate is mainly driven by women who come from economically and socially disadvantaged families. Empirical evidence suggests that supply-side factors, such as fertility decision and childcare provisions, likely explain the finding. Specifically, our analyses present that females born right after the cutoff are more likely to delay pregnancy and raise younger child conditional on age. This is consistent with our expectations that a higher level of education is associated with delayed parenthood. Infants and toddlers require meticulous and individualized care from their mothers, and therefore women's participation in the labor force is hampered by childcare provisions.

The differential influences of primary school entry between males and females have clear and important policy implications. Despite comparable impacts on education and earnings, women are disadvantaged in the labor force relative to men because of fertility and childcare in their early career. Therefore, it is essential to implement policies that support women before, during and after childbirth, such as childcare subsidy policies and paid maternity leave, in order to facilitate work-family balance and encourage mothers to reenter the labor force. This becomes more crucial since China's universal two-child policy implemented in 2016, and the newly introduced three-child policy in May 2021. Legal prohibitions against gender and pregnancy-based discrimination in employment need to be advocated and fully enforced to protect mothers from labor market risk.

Data availability

Data will be made available on request.
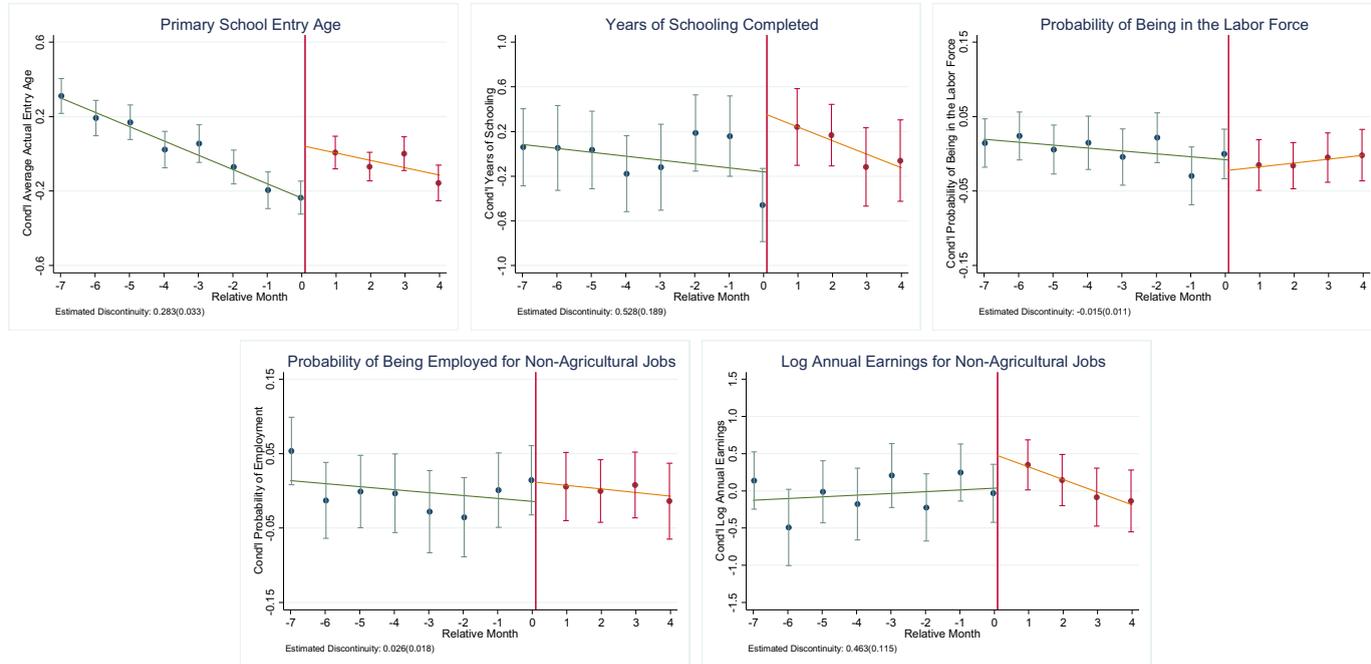
Appendix A

Fig. A1. Conditional scatterplots for key outcomes of interest against birth month.

Note: Figures display means and linear fitted values of key outcomes of interest by birth month relative to the threshold month August. The outcome variables include primary school entry age, years of schooling completed, probability of being in the labor force, probability of being employed for non-agricultural jobs, and log annual earnings for non-agricultural jobs. Each of the outcomes is residualized by using birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Dependent variables are given by figure title and y-axis title. Points reflect the average of dependent variables by each birth month. 95% confidence intervals are presented for each plot. Discontinuities at the threshold are calculated and displayed in the notes below each figure. Standard errors in parentheses and clustered at the birth-month level.

Panel A: Males

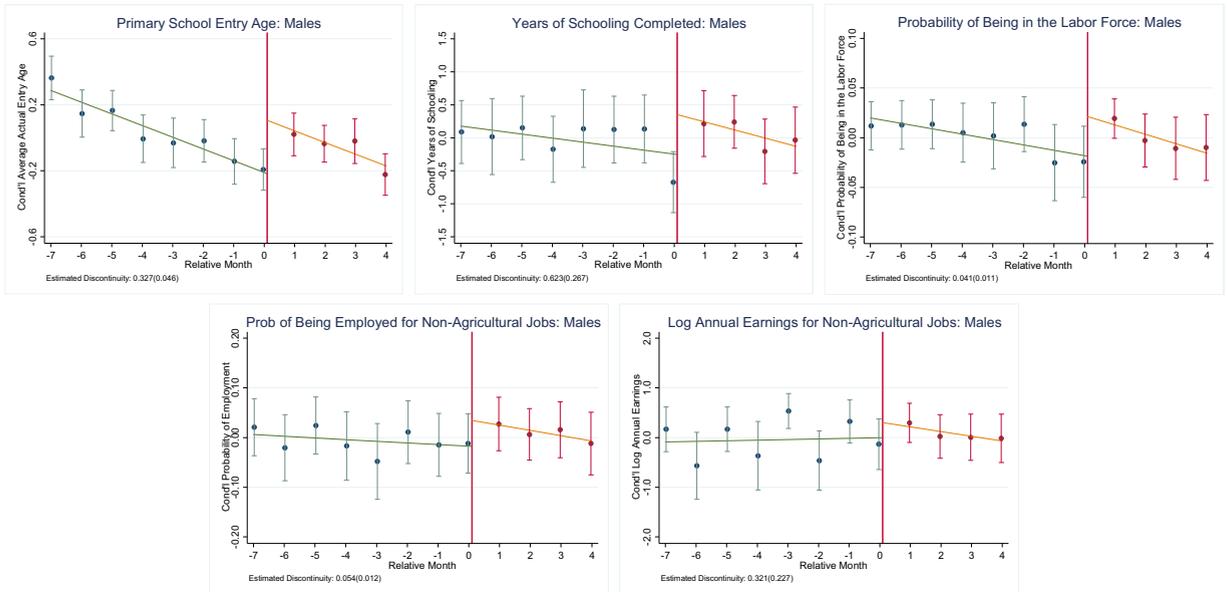Panel B: Females
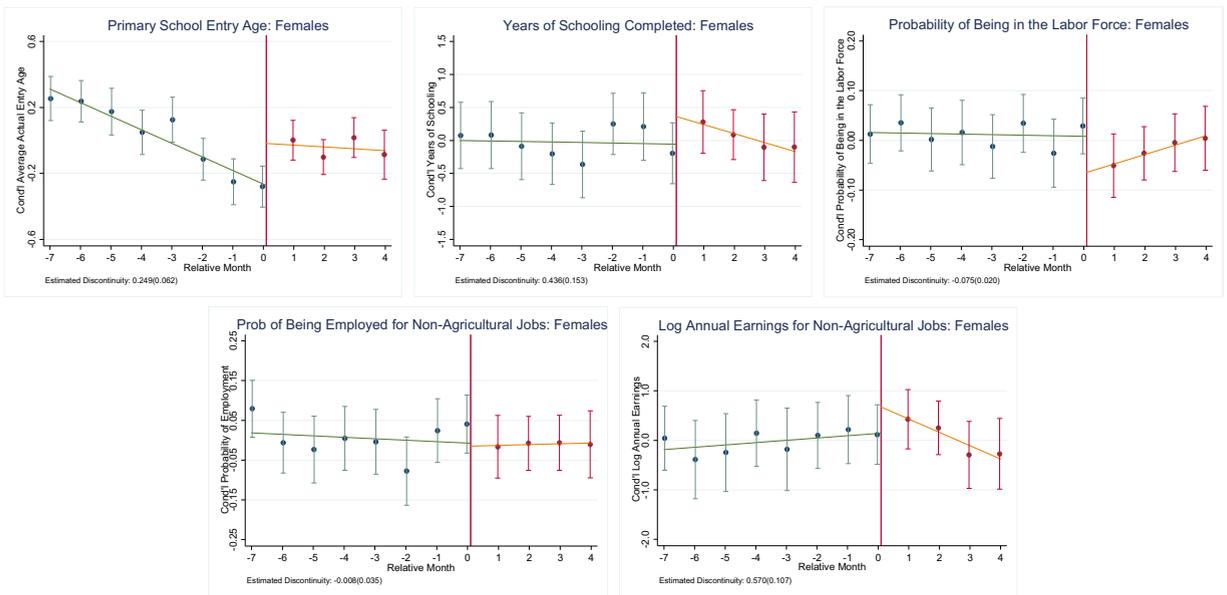
Fig. A2. Conditional scatterplots for key outcomes of interest against birth month: By gender.

Note: Figures display means and linear fitted values of key outcomes of interest by birth month relative to the threshold month August. The outcome variables include primary school entry age, years of schooling completed, probability of being in the labor force, probability of being employed for non-agricultural jobs, and log annual earnings for non-agricultural jobs. Panel A shows the conditional scatterplots for the male sample, and Panel B for the female sample. Each of the outcomes is residualized by using birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Dependent variables are given by figure title and y-axis title. Points reflect the average of dependent variables by each birth month. 95% confidence intervals are presented for each plot. Discontinuities at the threshold are calculated and displayed in the notes below each figure. Standard errors in parentheses and clustered at the birth-month level.

Table A1

Implementation year of the compulsory education law (CEL) by province.

Province	Implementation Year of CEL
Beijing	1986
Tianjin	1987
Hebei	1986
Shanxi	1986
Inner Mongolia	1989
Liaoning	1986
Jilin	1987
Heilongjiang	1986
Shanghai	1987
Jiangsu	1987
Zhejiang	1985
Anhui	1987
Fujian	1988
Jiangxi	1986
Shandong	1987
Henan	1987
Hubei	1987
Hunan	1991
Guangdong	1987
Guangxi	1991
Hainan	1992
Chongqing	1986
Sichuan	1986
Guizhou	1988
Yunnan	1987
Shaanxi	1987
Gansu	1991
Qinghai	1989
Ningxia	1986
Xinjiang	1988
Tibet	1994

Note: Data are collected from the official websites of the Department of Education for provinces and municipalities.

Table A2

Tests of covariate balance.

	1 If *Hukou* Status at Age 3 is Non-Agricultural	1 If in Eastern Region at Birth	1 If in Central Region at Birth	1 If in Western Region at Birth	1 If Completing Junior High School and Above (Father's Education)	1 If Completing Junior High School and Above (Mother's Education)	1 If Female	1 If Han Ethnic Group
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Discontinuity	−0.045*	0.037	−0.043	0.006	0.026	0.040	0.034	0.036***
	(0.020)	(0.033)	(0.035)	(0.021)	(0.021)	(0.023)	(0.024)	(0.006)
Observations	3839	3839	3839	3839	3839	3839	3839	3839

Note: This table presents estimated discontinuities at the school entry cutoff date with each covariate as the outcome variable. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3

Summary statistics for the analytical sample: By gender.

	Male		Female	
	Mean	SD	Mean	SD
Panel A: Key Dependent Variables of Interest				
Primary School Entry Age	7.161	0.866	7.164	0.867
Years of Schooling Completed	10.315	3.582	10.486	3.574
1 If in the Labor Force	0.962	0.190	0.804	0.397
1 If Employed for Non-Agricultural Jobs	0.813	0.390	0.695	0.461
Annual Earnings for Non-Agricultural Jobs (in RMBs)	29,440.062	54,719.399	20,921.861	25,825.380
Panel B: Individual Characteristics				
1 If *Hukou* Status at Age 3 is Non-Agricultural	0.152	0.359	0.158	0.365
1 If in Eastern Region at Birth	0.456	0.498	0.447	0.497
1 If in Central Region at Birth	0.326	0.469	0.356	0.479

Table A3 (*continued*)

	Male		Female	
	Mean	SD	Mean	SD
1 If in Western Region at Birth	0.218	0.413	0.198	0.398
Father's Years of Schooling	7.655	3.817	7.726	3.658
Mother's Years of Schooling	5.522	4.466	5.940	4.205
1 If Han Ethnic Group	0.932	0.252	0.945	0.229
1 If Current *Hukou* Status is Non-Agricultural	0.282	0.450	0.304	0.460
1 If Currently Urban	0.457	0.498	0.505	0.500
1 If Currently Married	0.595	0.491	0.715	0.452
Age	26.033	3.112	25.698	3.133
Number of Individuals	1893		1946	

Appendix B

In this appendix, we introduce the ordinary least squares (OLS) model that is employed to explore the correlations between school entry age and educational attainment and labor market outcomes in adulthood. We also present and discuss the OLS estimation results.

B.1. Empirical design

The OLS model takes the following form:

$$Y_i = \pi_0 + \pi_1 Entry\_Age_i + \delta X_i + \mu_i, \tag{3}$$

where i indexes individuals. Y_i, $Entry\_Age_i$, and X_i follow the same notation definitions as discussed in Eqs. (1) and (2) in the main text. To be specific, Y_i denotes the long-term outcomes including years of schooling completed, an indicator of whether being in the labor force, an indicator of whether being employed for non-agricultural jobs, and annual earnings for non-agricultural jobs (in log level) for individual i. $Entry\_Age_i$ is a continuous variable of the actual primary school entry age. X_i is a set of control variables such as birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Birth year fixed effects account for national shocks that impact all individuals born in the same year. Birth region fixed effects control for variation in outcomes that are common across birth cohorts within a birth region. μ_i is an error term with mean zero, which represents unobservable factors affecting dependent variables.

The coefficient of interest in the baseline OLS regression is π_1. However, it is inappropriate to interpret an OLS estimate of π_1 as the causal effect of primary school entry age on educational attainment and labor market outcomes. OLS estimates will be biased when unobserved omitted variables are correlated with both the actual school entry age and the outcome variables. For example, parental preference and attitude towards children's education can be potential confounders. In China, richer families are more likely to enroll their children in primary school as early as possible due to the belief that "never let your children lose at the starting line." These parents also have more resources to make significant long-term investment in the development of their children who therefore tend to have superior academic achievement, higher educational attainment, and better labor market performance. On the other hand, children born in low-income families, especially those from underdeveloped rural areas, may experience delayed school entry. This is because their parents are often in need of manual labor to help with family agricultural production, and they always have the perception of "the uselessness of study." Many of these children drop out of school once they complete the compulsory education and become legally authorized to work. The lack of education would negatively affect their labor market performance. In either scenario, OLS estimates will be biased downward. In addition, OLS estimates are biased towards zero if the observed primary school entry age is measured with random errors.[28]

B.2. Regression results

Table B1 reports the OLS estimates of Eq. (3), which serve as a benchmark for the RD results discussed in the main text. Panel A of Table B1 shows that unconditional correlation between actual primary school entry age and years of schooling completed (Column 1) is similar to the estimate when birth year fixed effects and birth region fixed effects are included (Column 2). However, when adding *Hukou* status at age 3 and parental education into the regression (Column 3), the magnitude of the estimate decreases by half (from 0.639 to 0.361). The estimate is insensitive to the inclusion of gender and ethnicity as control variables (Column 4). The coefficient in Column (4) of Panel A implies that a one-year increase in actual school entry age is associated with 0.360 fewer years of schooling completed on average, holding all other variables constant. The coefficient estimates remain statistically significant at the 1% level across all columns in Panel A. Panels B to D of Table B1 report results for labor market outcomes. Most estimates are not statistically

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[28] We assume that reporting errors in birth year and birth month, as well as primary school entry year, are random. Therefore, the calculated actual primary school entry age only contains classical measurement error. This is to say, the measurement error is independent of the true actual primary school entry age and of the error term μ_i.

significantly different from zero, even though the coefficients are negative when labor market performance for non-agricultural jobs serves as the dependent variables.

Overall, the OLS results suggest significant negative relationship between actual primary school entry age and years of schooling completed. In other words, early enrollment in primary school is associated with more years of schooling. This seems to be consistent with the belief that "the early bird catches the worm." However, these OLS estimations cannot be used to make causal inferences according to the discussion in the above section, where we argue that the OLS estimates are likely biased both downwards because of the possible presence of omitted variables, and towards zero if the actual school entry age is measured with random errors.

Table B1
Primary school entry age, educational attainment, and labor market outcomes (Naïve OLS).

	(1)	(2)	(3)	(4)
Panel A: Years of Schooling Completed				
Primary School Entry Age	−0.595***	−0.639***	−0.361***	−0.360***
	(0.077)	(0.069)	(0.061)	(0.060)
Observations	3839	3839	3839	3839
Panel B: Probability of Being in the Labor Force				
Primary School Entry Age	0.001	−0.001	0.003	0.003
	(0.005)	(0.006)	(0.006)	(0.005)
Observations	3839	3839	3839	3839
Panel C: Probability of Being Employed for Non-Agricultural Jobs				
Primary School Entry Age	−0.012	−0.017*	−0.010	−0.009
	(0.008)	(0.009)	(0.009)	(0.009)
Observations	3386	3386	3386	3386
Panel D: Log Annual Earnings for Non-Agricultural Jobs				
Primary School Entry Age	−0.089	−0.112	−0.094	−0.094
	(0.070)	(0.077)	(0.082)	(0.076)
Observations	2429	2429	2429	2429
Control Variables				
Birth Year FE	No	Yes	Yes	Yes
Birth Region FE	No	Yes	Yes	Yes
Hukou Status at Age 3	No	No	Yes	Yes
Parental Education	No	No	Yes	Yes
Gender	No	No	No	Yes
Ethnicity	No	No	No	Yes

Note: This table presents estimates of coefficient π_1 in Eq. (3) where the dependent variable is years of schooling completed (Panel A), probability of being in the labor force (Panel B), probability of being employed for non-agricultural jobs (Panel C), and log annual earnings for non-agricultural jobs (Panel D), respectively. Observations are at the individual level. OLS regressions control for birth year fixed effects, birth region fixed effects, *Hukou* status at age 3, parental education, gender, and ethnicity. Standard errors in parentheses and clustered at the birth-month level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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