The Scarring Effects of College Education Deprivation during China's Cultural Revolution

HONGBIN LI
Stanford University

LINGSHENG MENG
Chinese University of Hong Kong

I. Introduction

Prior literature has shown that exposure to adverse conditions in early life can have large and persistent impacts on adult outcomes. People have long-term losses in education, health, labor supply, and income after experiencing negative shocks in utero or during childhood.[1] Can adverse shocks to education during late adolescence and early adulthood also have a lasting negative effect? To what extent can people make up for the loss of education later in life?

In this study, we investigate how the suspension of college admissions during China's Cultural Revolution (1966–76) affected people's labor market and family-related outcomes. In June 1966, the Chinese central government decided to abolish the college entrance examinations and postponed the admission process for half a year. As the Revolution unfolded, the half-year delay eventually extended into a 4-year suspension of college enrollment. There were virtually no new entrants to China's universities during the period 1966–69 (fig. 1). Because of this radical policy change, many young people were deprived of the opportunities for higher education, and their life courses derailed.

We implement a regression discontinuity (RD) approach to examine how the abrupt suspension of college admissions affected the lives of these cohorts. For those who were just about to finish high school in 1966, the sudden and unexpected suspension of college admissions imposed a significant barrier for them to go to college during 1966–76. Their slightly older counterparts, however, had a better chance of going to college in 1965, when colleges were still open

We thank participants at the 2017 Asian Meeting of the Econometric Society, the 10th China Economics Summer Institute (2017), and the Asian and Australasian Society of Labour Economics 2017 Conference for helpful comments. Contact the corresponding author, Lingsheng Meng, at lingshengmeng@gmail.com.

[1] See Almond and Currie (2011), Currie and Almond (2011), and Currie and Vogl (2013) for comprehensive reviews.

Electronically published March 10, 2022

Economic Development and Cultural Change, volume 70, number 3, April 2022.

https://doi.org/10.1086/713935

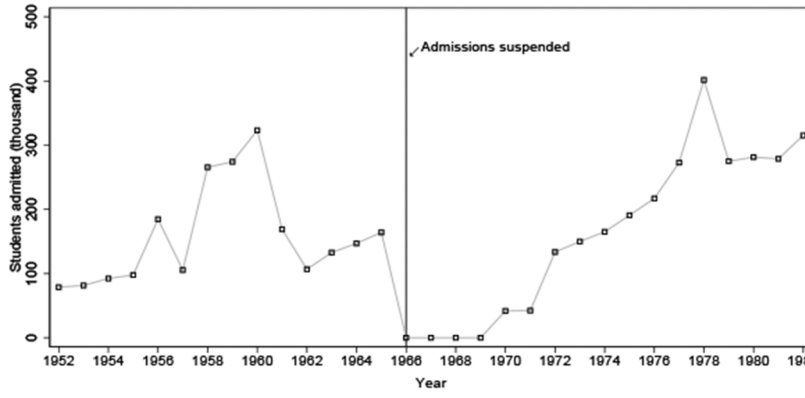

Figure 1. Admitted college students by year. Source: Department of Planning of the People's Republic of China (1984).

to new students. As illustrated in figure 2, people who were born in January 1947 had a lower probability of going to college than their luckier counterparts born just a month earlier.[2] If the unobserved characteristics of people born on the two sides of this birth-month cutoff are similar on average, then the observed difference in corresponding later-life outcomes between these two cohorts reflects the true damage of the policy that disrupted progression toward a comprehensive 4- or 5-year college education. This RD design also allows us to single out the effect of college enrollment suspension from other contemporaneous social and policy changes that took place in the midst of the decade-long Cultural Revolution, which involved a sweeping series of social upheavals and radical reforms that have had long-lasting confounding effects.

Our empirical analysis shows that the suspension of college admission indeed had a large impact on college education. In the 2000 Population Census, which captures the relevant cohorts in their 50s, 11.1% of the high school graduates in the pre–Cultural Revolution (CR) control group had a college education. Our RD estimate using data from the same census shows that the college suspension led to a 4.7 percentage point (more than 40% of the mean of the pre-CR level) reduction in the probability of having a college education among those who completed high school. A number of validity tests suggest the identification assumptions of our RD design are quite convincing, as there are neither discrete changes of baseline covariates nor endogenous sorting around the cutoff.

Comparing results from the 2000 Census with those from the 1990 Census, we are also able to see whether and by how much people can make up for their educational loss earlier in life. In fact, the fraction of people with a college

[2] People who were born in January 1947 and followed a typical schooling schedule were supposed to finish high school at the age of 19 in 1966, when colleges suspended new admissions.

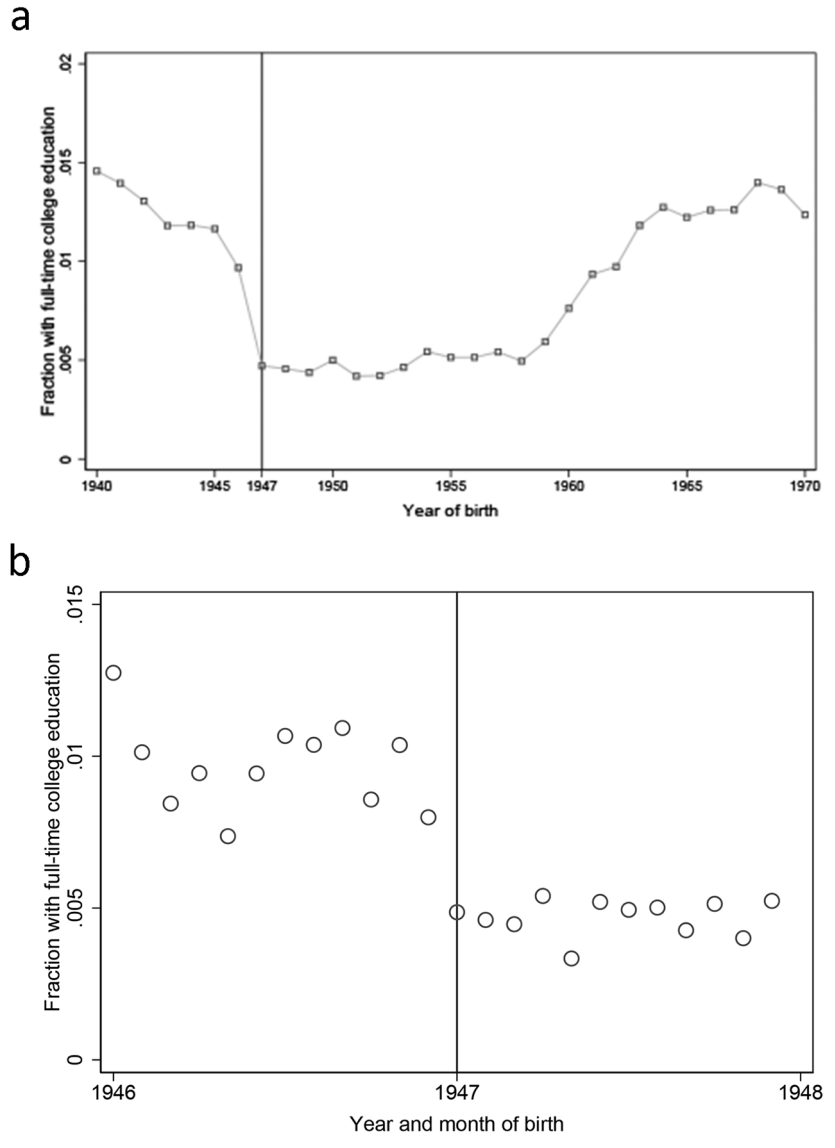

Figure 2. Fraction of population with a full-time college education. *a*, By year of birth. *b*, By month of birth. Source: 2000 Population Census of China.

education did not go to zero for the affected cohorts. Other than being able to get into college before the Cultural Revolution through earlier school entry or grade skipping, most of the college graduates in the affected cohorts either got into colleges after admissions resumed in 1970 or were able to get a college education as part-time adult students later in life. The RD estimate for college

education from the 2000 Census is 0.7 percentage points smaller in magnitude than that estimated using the 1990 Census, which suggests that the affected generation could make up for their loss by 13% over the decade (1990–2000) when they were already in their 40s and 50s.[3] However, we observe that there was still a large gap between their education level and that of the earlier cohorts, which implies that they were unable to fully remedy the loss even by 2000, when the affected cohorts were approaching retirement age.

This policy shock had long-term negative impacts on people's labor market outcomes and economic well-being in general, as observed in the 2000 Census. The labor force participation of the affected people was reduced when they reached their 50s. People in the affected cohort were 4.7 percentage points less likely to work. Our RD estimations indicate a 3.2% drop in earnings as a result of the policy. The enrollment suspension is also found to have reduced a person's house size, a proxy for wealth, by 3.5% around the birth-date cutoff. However, we find that the suspension of college admission had limited impacts on one's family-related outcomes.

This paper contributes to several strands of literature, with the first relating to a growing literature assessing the adverse effects of China's Cultural Revolution, one of the country's most significant transformative events in the past half-century. This decade-long nationwide crisis involved violence, social unrest, and radical reforms, all of which had profound and widespread consequences. Nevertheless, there have been few quantitative evaluations of this historical event so far. A number of earlier studies have provided initial evidence of the adverse effects of the Cultural Revolution on formal education, from elementary school to college (Deng and Treiman 1997; Meng and Gregory 2002, 2007; Zhang, Liu, and Yung 2007; Giles, Park, and Wang 2019). More recent studies have investigated issues other than education disruption, including the send-down movement (Li, Rosenzweig, and Zhang 2010; Lin 2013), the ideological campaign (Ou and Xiong 2018), the overall effects of the Cultural Revolution on economic development (Bai and Wu 2018), trust (Bai and Wu 2020), and behavioral preferences of future generations (Booth et al. 2018).[4] We add to this literature by revisiting the disruptive effects of the Cultural Revolution on education. Specifically, we systematically investigate the causal effects of the deprivation of college education, an optional final stage of formal learning. Our work is distinguished from the prior research on education

[3] Han, Suen, and Zhang (2019) systematically explore people's remedial efforts for their educational loss during the Cultural Revolution.

[4] In a related study, Zhang (2018) documents adverse labor market effects of a massive school destruction program in China. Unlike the policy considered in our study, that program shut down many rural high schools soon after the Cultural Revolution.

disruption in two main ways. First, unlike Meng and Gregory (2002) and Giles, Park, and Wang (2019), who assess the overall impact of the Cultural Revolution, we focus on its specific policies on college enrollment, the effects of which we argue can be better identified with an RD design.[5] Second, the census data we use have several advantages. The census samples are not only nationally representative and vast in size but also cover multiple years. The data enable us to observe the same cohorts at different ages and examine how the damaging effect evolves over their life cycles.

Second, our work is connected to a set of papers leveraging exogenous shocks generated by major political events for identification. Card (1990) relies on the unexpected and large influxes of immigrants caused by Cuba's Mariel boatlift crisis in 1980 to study the effects of immigrants on local labor market outcomes for natives in the United States. Angrist (1996) takes advantage of the exogenous decreases in the Palestinian labor supply following the 1987 Palestinian uprising to estimate the short-run demand for labor. Jones and Olken (2009) exploit randomness in the success or failure of assassination attempts to identify the impacts of leadership changes on the country's institutions and conflicts. Lan (2012, 2013) makes use of the abrupt change in US immigration policies triggered by China's Tiananmen Square incident in 1989 to estimate the effects of visa status on the career choices and earnings of foreign PhDs in the United States. Lee (2014) estimates the intergenerational effects of maternal stress in utero by using the 1980 Gwangju Uprising in South Korea as a natural experiment. Acemoglu, Hassan, and Tahoun (2018) examine whether popular mobilization under weak institutions can reduce rent-seeking abilities by using daily variation in the size of the protests during Egypt's Arab Spring in 2011. More recently, Gaete-Romeo (2018) exploits the 2011 Chilean student strikes as a source of exogenous variation to evaluate the effect of absenteeism on later academic outcomes. The most similar research to ours is that of Maurin and McNally (2008), who evaluate the long-run effects of a short-lived reform in the education system soon after the 1968 student revolution in France. Like the abrupt suspension of college enrollment in China 2 years earlier, the policy change in France was largely unexpected and affected the students at a relatively later stage of formal education. Moreover, both historical changes came as a surprise and affected people's education in a nonlinear way across birth cohorts.

[5] Giles, Park, and Wang (2019) exploit the city-by-cohort variation in educational supply shocks caused by the Cultural Revolution to estimate the return to education. By contrast, our research design relies on the differential impact of a nationwide college supply shock around a specific monthly birth threshold. Although not a focus of our paper, the implied return to college education from our estimates is larger than the estimated return in Giles, Park, and Wang (2019). The difference is likely explained by our proxy measure of income, as well as potentially different compliers in this and their studies.

Unlike the Chinese policy considered in this paper, however, the French policy in 1968 increased postsecondary educational attainment by lowering the passing threshold for the affected cohorts.[6]

Finally, our research also contributes to an extensive literature on the labor-market benefits of a college education.[7] Prior work on developed countries has relied on quasi-experimental variation provided by college proximity (Card 1995; Nybom 2017; Kamhöfer, Schmitz, and Westphal 2019), policies on veteran benefits (Bound and Turner 2002; Stanley 2003; Angrist and Chen 2011), and specific admissions rules (Zimmerman 2014) and dismissal rules (Ost, Pan, and Webber 2018) for causal inference. All these studies estimate substantial (average) economic returns to college education in the developed countries. As for the developing world, well-identified evidence on economic returns to college education is relatively scarce, with Rodríguez, Urzúa, and Reyes (2016) providing an exception. In the context of China, although there is no shortage of research documenting large and growing college premiums, evidence on the causal impact of college education is limited (see, e.g., Heckman and Li 2004; Li, Liu, and Zhang 2012). Our work contributes to the broad literature on higher education by offering new quasi-experimental evidence on the benefits of (or the costs of not having) a college education. Our empirical results suggest that the cost of being deprived of opportunity for college is large, multidimensional, and can be felt over the life cycle.

The remainder of this paper is organized as follows. In the next section, we briefly describe the Cultural Revolution and China's higher education since 1949. In section III, we illustrate our empirical strategy. We describe the data in section IV and report our RD estimation results in section V. In section VI, we examine the validity of our RD design. Finally, we provide our concluding remarks in section VII.

II. The Cultural Revolution and China's Higher Education

This section provides a brief account of China's higher education in three phases, namely, before, during, and after the Cultural Revolution.[8] For the pre-CR period, we will focus on how colleges selected students, as those who were admitted to colleges right before the Cultural Revolution are of the greatest interest in

[6] Our empirical setting is different from the French case in Maurin and McNally (2008). In their case, there is a one-time and brief surge in the probability of going to college for one birth cohort (1949). The probabilities for both the pre-1949 and post-1949 cohorts are normal. In our case, the probability of having a college education shifts downward and remains low for the next 10 cohorts because of the suspension of college enrollment. Thus, RD is a more appropriate empirical strategy in our case.

[7] More recently, research also suggests significant nonpecuniary benefits to higher education, including higher job satisfaction and better health outcomes (Oreopoulos and Salvanes 2011).

[8] This section draws heavily on MacFarquhar and Fairbank (1987, 1991) and Meisner (1999).

this study. We will then describe the Cultural Revolution (1966–76) and illustrate how it has affected China's culture, education, and society. Finally, China resumed a normal education system after Mao died in 1976, which we will briefly discuss toward the end of the section.

A. Higher Education before 1966

When the Chinese Communist Party came into power and founded the People's Republic of China in October 1949, the new government took over all 205 colleges from the previous regime. A reorganization of the entire system of higher education was soon under way. New forms of education would have to be devised that were appropriate for the immediate needs of building an industrialized nation. In order to train enough technical personnel for China's economic development, the system of higher education was gradually transformed along Soviet lines.

The national unified entrance examinations were first introduced in 1952 as an integral part of China's first Five-Year Plan to industrialize the newly founded republic. Enrollment was centralized and standardized. Every college was given an enrollment quota, and candidates would win admission to their preferred institution based on their performance on the national examinations that were given annually. From 1952 until 1958, the enrollment system was gradually refined.

However, during the Great Leap Forward (1958–61), the system that selected students purely based on academic achievements was questioned and eventually derailed. A new form of college enrollment was adopted in 1958. The unified national entrance examinations were not held that year; instead, universities gave their own examinations. This year also marked the start of using more political criteria in college admissions. Young people with certain family backgrounds, mainly those from capitalist and landlord families, were discriminated against. Individuals could be denied access to higher education altogether if their families had serious political problems, regardless of their own academic standing. In contrast, candidates of worker-peasant origins were given priority in admissions. In fact, in 1958, many workers and peasants were admitted to universities on the basis of recommendations, without having to take any written examinations. Although the national unified entrance examinations were restored in 1959 and held every year thereafter until the beginning of the Cultural Revolution in 1966, students from families of lower socioeconomic status (workers and peasants) could still be admitted to colleges on the basis of recommendations without having to sit for the entrance examinations. From the 1950s until the early 1960s, the fraction of college students from worker-peasant families steadily increased as a result of the change in admissions priorities. In 1951–52,

19% of college students were from worker-peasant backgrounds. The figure rose to 48% in 1958–59. Between 1960 and 1961, college students from worker-peasant backgrounds accounted for 67% of the total enrollment (Lee 1978).

B. The Cultural Revolution in 1966–76

The Cultural Revolution, which erupted in the summer of 1966, was largely the result of the decisions of Chairman Mao Zedong, for reasons of both power and policy. Before launching the Cultural Revolution, Mao felt shut out by his more conservative colleagues in power. Meanwhile, he was concerned that the extant bureaucracy was dominated by bourgeois ideology and that the country was in danger of a regression to capitalism. Mao made use of his unique charismatic standing and resorted to mass mobilization for political support. An organization, the Red Guards, was spontaneously established by college, middle school, and high school students who answered Mao's call to rebel against the existing political order.

Probably unexpected by Mao, however, the youth movement quickly spun out of control and degenerated into violence, factionalism, and anarchy. At first, the enthusiastic Red Guards organized mass meetings, posted wall posters in public areas, and published their own newspapers to attack their teachers, school administrators, and local party officials verbally. Soon, the most radical student rebels began to attack the established authority physically. Homes of the victims were ransacked. Beatings and torture were also very common. Later on, the factionalized student rebel groups started to war against one another. In cities like Shanghai, the working class also formed their own organizations and joined the battle. The whole country was thrown into turmoil, and the party bureaucracy to a large extent ceased to function as a national organization. To restore order, the army intervened, and even more blood was shed. Eventually, the Red Guards were disbanded and sent down to the countryside to do agricultural work by May of 1968.[9] Deprived of the opportunities of higher education and urban employment during the Cultural Revolution, the "rusticated youth" eventually grew to see themselves as the "lost generation."[10]

The mass movement indeed helped Mao to regain the reins of political power. His rivals in the core political circle, such as President Liu Shaoqi, were purged. Conservative officials from all levels of administration were removed from their posts and sent to labor camps. At the same time, Maoist cultural

[9] On December 22, 1968, the *People's Daily* conveyed Mao Zedong's instructions, saying, "It is necessary for educated youths to go to rural areas to be reeducated by poor peasants."

[10] They returned to the urban areas after the Cultural Revolution ended in 1976.

revolutionaries were promoted. The radical episode of the Cultural Revolution came to an anticlimactic end during the Ninth Congress of the Communist Party of China in 1969, which proclaimed the Cultural Revolution a great success and marked the reestablishment of a certain political normalcy. The struggle for power within the top bureaucracy continued, although such conflicts had been totally hidden from the public view until the downfall of Lin Biao, the military leader and anointed successor of Mao, in 1971. During the early 1970s, many efforts were made to restore the unity of the party and the pre-CR order. The Maoist policies were entirely dismantled and reverted by reformists led by Deng Xiaoping after Mao's death in 1976.

Inevitably, the national economy also fell victim to the political turmoil during the radical phase of the Cultural Revolution. Industry and transportation were the most affected. In cities, industrial production was halted due to absenteeism, strikes, and open physical clashes. The transportation system was severely disrupted, as millions of Red Guards were given free use of the railway, buses, and trucks wherever they went. As a result, the transport of essential commodities was displaced. Fortunately, the disruption of the economy was largely confined to the period from 1966 to 1968. Agricultural production was much less affected, as the Cultural Revolution was a distinctly urban movement, and a stable food supply was among the government's highest priorities.

C. Disruption of Culture and Education

A fundamental transformation of culture and the educational system was an essential part of the Cultural Revolution. On June 13, 1966, the Central Committee of the Chinese Communist Party and the State Council issued "A Notice on Postponing the Enrollment of Institutes of Higher Education by Half a Year." The directive pointed out that the extant enrollment system in higher education was inappropriate and must be changed completely. It also stipulated that the admission of new students to college in 1966 be postponed half a year to allow for a thorough reevaluation and reform of the system of higher education. The entrance examinations were not given in July, as usual, that year. As the Cultural Revolution unfolded, the half-year delay in enrollment stretched into 4 years before a handful of colleges were permitted to resume enrollment in 1970. As illustrated in figure 1, college admissions literally dropped to zero during the period 1966–69.

In October 1970, student recruitment resumed in a few selected universities, but in accordance with the instructions issued by the Central Committee of the Party, the new recruits were not to be chosen directly from fresh high school graduates but from the ranks of workers, peasants, and soldiers. No written examinations were held for student recruitment. To gain admission, the

candidates needed only recommendations from the "masses" and leadership approval from their production units, as well as approval of the college admissions office. The criteria were largely based on political reliability and personal connections. This new form of enrollment was expanded nationwide later on.[11] These new university students were later known as the worker-peasant-soldier college students.

Starting with the breakout of the Cultural Revolution, classes were promptly canceled in colleges. A few colleges reportedly reopened after 1969, but intellectuals were replaced by the proletariats in the administration of schools. Many academic courses were replaced with new courses with a practical bent. A hefty fraction of the students' time was allocated to working on farms or in factories. Even these courses could not last long because of continued violence and low attendance. Students were either actively participating in the movement or enjoying their cloistered way of life.

The overall cultural and intellectual life of the Chinese people was dismal during the Cultural Revolution. In the campaign against all symbols of the past ("the four olds"), old books and paintings were burned, museums and homes were ransacked, and historic relics were destroyed. The cult of Mao, however, reached its zenith during the Revolution, with everyone frenetically worshiping Chairman Mao as the deified great leader. The use of libraries was restricted, whereas Mao's works became the most popular—if not the only available—reading material for ordinary people. Normal artistic and literary creation was largely forbidden. Only a handful of artists and writers were allowed to work and only under supervision and for the purpose of political propaganda.

China's intellectuals were seriously scarred by the mass movement. Their homes were raided, books and manuscripts burned, and they themselves were often subject to painful sessions of "study and criticism." A great many of them were arrested and jailed or sent to labor camps in remote rural areas. Scientific research was paralyzed by political fear or lack of equipment and supplies, while research in social sciences and humanities had all but ceased. For Chinese intellectuals, the 1966–76 period was indeed the 10 lost years.

D. Post-Mao College Education

In August 1977, only 1 year after Mao's death, the 11th National Congress of the Party announced the end of the Cultural Revolution. The reinstitution of the national college entrance examinations was announced 2 months later. Since 1977, all students in colleges have been selected based on their performance on

[11] In 1973, there was a short-lived attempt to reintroduce examinations to improve the academic quality of recruits, which was soon rolled back by Mao's most fervent supporters.

the entrance examinations or some special talents. The admission of worker-peasant-soldier students was terminated accordingly thereafter, although they were allowed to continue their studies until graduation.

By the end of 1977, educational policies had largely reverted to normal ones. In 1978, the Ministry of Education issued the "Provisional Regulations for National Key Higher Education Institutions (Provisional Draft)," which emphasized that a major fraction of college time should be devoted to classroom instruction and learning. Soon, colleges were revived and started to function like colleges in the rest of the world.

III. Empirical Strategy: The RD Design

Our empirical analysis has two parts. First, we employ an RD framework to quantify the effect of college enrollment suspension on the probability of having a college education. In particular, we exploit the fact that the extent to which a person is affected by the policy is a discontinuous function of their birth date. Essentially, the discontinuity is created by the abrupt suspension of college enrollment in 1966, where students below a certain age (typically 19) had not yet completed high school and would miss the last window of opportunity to attend college before the Cultural Revolution. People who were born in December 1946 and had followed a typical schooling schedule would have finished high school by the age of 19 in 1965, when the universities were still open to new entrants. In contrast, those who were born in January 1947 and made normal progress in education had not completed high school until 1966, when colleges stopped admitting new students.[12] Under such a policy, the biological "accident" of a birth date just after December 1946 would significantly hurt one's opportunity of getting into college, relative to those born in December 1946. Specifically, we estimate the following equation:

$$C_{ij} = \alpha_0 + \alpha_1 D_{ij} + f\left(R_{ij}\right) + X_{ij}\theta + u_{ij}, \tag{1}$$

where the dependent variable C_{ij} is a college education dummy variable for individual i in month of birth j. Further, $D_{ij}(= 1(R_{ij} \geq 0))$ is an indicator variable

[12] Historical accounts suggest that January was used as the birthday cutoff for school entry in the 1950s in China. Throughout the 1950s, when the socialist republic was just founded, China largely followed the Renxu School System, which the previous regime (Republic of China) adopted in 1922 to emulate the US educational model (Chou and Spangler 2016; Rao, Zhou, and Sun 2017). Much of the nation inherited this American system and continued to use it until 1960. In a speech delivered in April 1960, Vice Premier Lu Dingyi pointed out that "the current school system was inherited from the time when Kuomintang was in power. It was copied from the US and is an outdated system" (Qu 1991). This implies that, until the 1950s, when the 1947 birth cohort was supposed to begin school education, elementary schools in China still followed the US standard of practice and kept using January as the birthday cutoff to admit new students.

that equals one for individuals born in or after January 1947. Meanwhile, R_{ij}, the running variable, is an individual's month of birth relative to January 1947; for example, $R_{ij} = -1$ for an individual born in December 1946, and $R_{ij} = 1$ for an individual born in February 1947. The variable X_{ij} includes predetermined personal characteristics such as gender and ethnicity. The smooth function $f(\cdot)$ captures the underlying relationship between month of birth and the attainment of higher education. In parametric specifications, we use low-order polynomials to model $f(\cdot)$ and allow its functional form to differ on both sides of the January 1947 cutoff.[13] Our main RD specification uses a second-order polynomial with different slopes on both sides of the cutoff. The key parameter of interest is α_1, which measures the reduced-form effect of one's birth month passing January 1947 on the probability of having a college education.

Then we use an equation of a similar form to estimate the effect of being born in or after January 1947 on a person's socioeconomic outcome, Y_{ij}:

$$Y_{ij} = \beta_0 + \beta_1 D_{ij} + g\left(R_{ij}\right) + X_{ij}\omega + v_{ij}, \tag{2}$$

where $g(\cdot)$ is a smooth function that captures the relationship between month of birth and the outcome and is, in practice, approximated with flexible low-order polynomials that have varying slopes on both sides of the discontinuity. The key identifying assumption underlying our estimation procedure is that the conditional expectation of potential outcome with respect to month of birth is smooth through the $R_{ij} = 0$ threshold.[14] In that case, we can attribute any discontinuity at this threshold to the causal effect of the suspension of college enrollment. The estimated β_1 can be interpreted as the intent-to-treat effect of the enrollment suspension. Because the running variable R_{ij} is discrete by nature, we follow Lee and Card (2008) to obtain robust standard errors by clustering at the month-of-birth level.

IV. Data

Our main data sets are from the 1990 and 2000 Population Censuses of China. There are two great advantages of the Population Censuses. First, the data have each individual's month and year of birth, based on which we can construct a relatively fine measure of the running variable in our RD design. Second, its sample size is enormous, with more than 11 million records in our 1% sample for each year. For our RD design, the sample of analysis contains more than 1 million observations for each census year. The massive sample size, combined with the

[13] The use of high-order polynomials in RD analyses can result in noisy estimates, sensitivity to the order of the polynomial, and poor inference (Gelman and Imbens 2019).

[14] While this assumption cannot be entirely verifiable, we are unaware of other policy changes that would create a discontinuity at the same birth-date cutoff.

information on the year and month of birth, allows us to focus contrasts narrowly around the discontinuity to increase the similarity of the unobservable factors while having sufficient statistical power for inference. Unfortunately, neither census has any direct measure of earnings or wealth. To get a proxy for income of those who were working in 2000, we construct a new variable named "occupation income score," which is the median annual income for each of the same double-digit occupations in the 2005 National Population Sample Survey.[15] Under the assumption that the relative earnings across occupations are stable over a 5-year period, the occupation income score calculated from the 2005 data is a good proxy for earnings in 2000. House size is used as a wealth proxy.

We impose a few sample restrictions. First, we use cohorts born between 1942 and 1951 in our sample or the neighboring cohorts around January 1947, the birth-date cutoff after which people were more affected by the suspended admissions policy.[16] Those who were born in 1942–46 are referred to as the pre-CR cohorts, while those who were born in 1947–51 are referred to as the post-CR cohorts. Second, we focus exclusively on individuals with at least a high school education, as they were the subpopulation at risk from the college enrollment suspension. Table 1 provides descriptive statistics for our estimation samples. We have 108,405 and 115,889 observations in the 1990 and 2000 samples, respectively. Occupation is not reported for 35,291 out of the 115,889 observations in the 2000 Census sample. House size is not available for 1,423 observations in the same sample. Among the high school graduates, 8% had a college education in 1990. The same figure reached 9% in 2000.

We also use an earlier census, for 1982, in some parts of the paper. Unfortunately, the 1982 Census does not report birth month. It only reports age in years as of July 1980, which prevents us from doing a rigorous RD analysis. Moreover, the 1982 Census does not distinguish between college and community college, so college education reported there includes both.

V. Main Results

In this section, we report our main RD results. We begin by reporting estimates of the impact of the suspension of college enrollment on the attainment of a college education. We then report estimates of the policy's impacts on income, wealth, and marriage market outcomes. Our RD samples, if not otherwise mentioned, are restricted to individuals born between 1942 and 1951 with at least a high school education. Standard errors are clustered at the month-of-birth level for the RD analysis.

[15] See table A1 for the distribution of one-digit occupations in our 2000 sample.

[16] We did not use the 2005 survey as our main data set because a lot of people in our sample (1942–51 birth cohorts) were retired by then and did not report income.

TABLE 1
SUMMARY STATISTICS

	A. Summary Statistics for Samples of Analysis from Population Censuses		
	Observations	Mean	SD
1990 Census:			
Age	108,405	43.24	2.86
Male	108,405	.68	.47
Minority	108,405	.05	.22
College education	108,405	.08	.27
2000 Census:			
Age	115,889	53.08	2.87
Male	115,889	.67	.47
Minority	115,889	.06	.23
College education	115,889	.09	.29
Occupational income score (1,000 yuan)	80,598	11.60	4.82
House area (m^2)	114,466	83.58	51.87
Working	115,889	.68	.47
Disabled	115,889	.004	.067
Dependent	115,889	.03	.17
Days worked (per week, conditional on working)	79,147	5.42	.93

	B. Means of Predetermined Variables by Birth Cohort in 2000 Census	
	Male	Minority
Pre-CR cohort:		
1942	.668	.055
1943	.673	.056
1944	.670	.058
1945	.676	.056
1946	.671	.050
Post-CR cohort:		
1947	.672	.051
1948	.677	.053
1949	.676	.056
1950	.672	.064
1951	.670	.056

Note. The 1990 Population Census sample contains individuals born between 1942 and 1951 and with at least a high school diploma. The 2000 Population Census sample contains individuals born between 1942 and 1951 and with at least a high school diploma. Means are shown for the individual characteristics. Minority refers to the non-Han ethnic groups. Occupational income score, reported only for working individuals, is the median income (1,000 yuan) for each occupation in the 2005 National Population Sample Survey. We denote individuals born in 1942–46 as the pre-CR cohort and those born in 1947–51 as the post-CR cohort.

A. College Education

To the extent that the enrollment suspension indeed matters, we should observe a break in college attainment as a function of birth cohort close to the birth-date cutoff. In figure 3*a*, we use data from the 1990 Population Census to plot the relationship between birth cohort and the attainment of a college education. People with community college or high school education are taken as the base

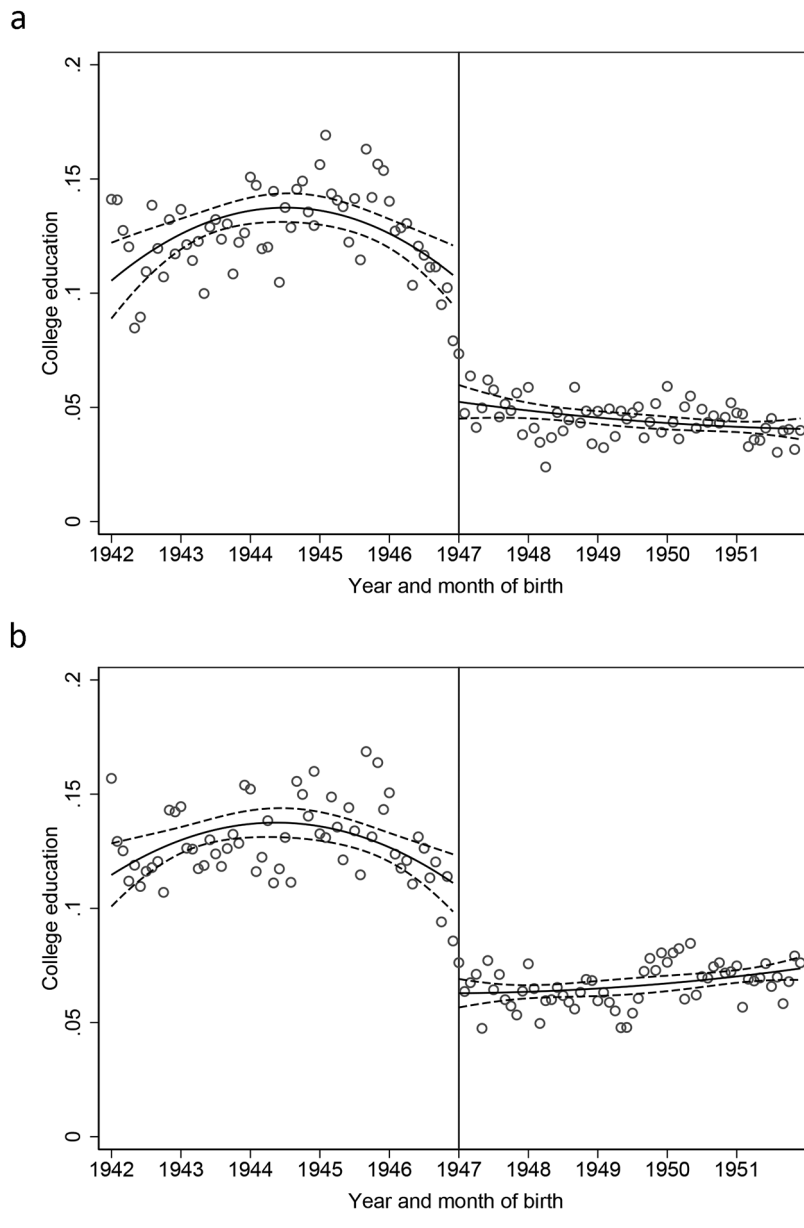

Figure 3. Impact of college enrollment suspension on college education. *a*, 1990 population census. *b*, 2000 population census. The samples are from the Population Censuses of China, restricted to individuals born between 1942 and 1951 and with at least a high school education. Open circles represent means in each month-of-birth cell. The continuous line is the predicted outcome from a regression that includes a second-order polynomial in the running variable and a dummy for observations above the cutoff. Dashed lines are the 95% confidence intervals.

group.[17] The relevant cohorts are observed in their 40s or late 30s. Open circles represent the share of cohorts having a college education for each month-year of birth. The vertical bar denotes January 1947, the month of birth after which cohorts were affected by the college enrollment suspension. Superimposed on the graph are the fitted values from a regression that includes a quadratic polynomial of month of birth and an indicator for whether the cohort was affected by the enrollment suspension. Visually, there is a stark change in the fraction of people with a college education around the January 1947 threshold. The quadratic fit predicts a decline in the probability of having a college education, between December 1946 and January 1947, by 5.3 percentage points, which is about a third of the mean for the pre-CR cohorts. The probability of having a college education did not drop to zero above the birth-date cutoff, probably because some people reinvested in college education after the Cultural Revolution.

The formal regression results using the same data (table 2, panel A) confirm the visual impression conveyed in figure 3*a*. In column 1, we report the most parsimonious specification, with only the indicator for being born after the January 1947 cutoff and a second-order polynomial in the running variable as independent variables. The coefficient of the discontinuity indicator is negative and statistically significant at the 1% level. The estimate suggests that the cessation of enrollment led to a 5.3 percentage point reduction in the probability of having a college education. Controlling for gender, ethnicity, and a proxy for pre-CR provincial college admission quota (col. 2) hardly changes the estimate and its precision.[18]

These baseline results are robust to different choices of bandwidth. Specifically, we limit our sample within increasingly narrower intervals around the cohort cutoff. In column 3 of table 2, panel A, we focus on individuals born within 48 months of the cutoff. The estimate is relatively insensitive to the narrowing of the estimation window and remains statistically significant. The last column shows the results from a local linear regression with a bandwidth of 24.[19] This nonparametric approach produces a statistically significant point estimate that is quantitatively similar to the parametric estimates.

We repeat the graphical and regression analysis using data from the 2000 Population Census, which captures the relevant cohorts in their 50s or late 40s. Similarly, figure 3*b* shows a significant downward shift in the college attainment rate

[17] Community college was nonexistent during the Cultural Revolution. Community college diplomas were often awarded through adult education programs after the Cultural Revolution.

[18] To obtain a measure of the provincial admission quota, we calculated the fraction of pre-CR cohort with a college education for each province using the 1982 Population Census data.

[19] We run an ordinary least squares regression of the outcome variable on a treatment dummy that equals one for cohorts born in or after January 1947, a linear term of the running variable, and an interaction of the treatment dummy with the running variable.

TABLE 2
EFFECTS OF COLLEGE ENROLLMENT SUSPENSION ON COLLEGE EDUCATION

	(1)	(2)	(3)	(4)
	A. 1990 Census Results			
1 (mob ≥ Jan. 1947)	−.053	−.054	−.039	−.041
	(.008)	(.008)	(.007)	(.006)
R^2	.023	.035	.034	.034
Observations	108,405	108,405	86,238	43,310
	B. 2000 Census Results			
1 (mob ≥ Jan. 1947)	−.047	−.047	−.036	−.041
	(.008)	(.007)	(.007)	(.007)
R^2	.011	.021	.021	.022
Observations	115,889	115,889	91,749	45,059
Order of polynomial in running variable	2	2	2	1
Bandwidth (months)	60	60	48	24
Other controls	No	Yes	Yes	Yes

Note. The sample includes individuals with at least a high school education. Standard errors, shown in parentheses, are clustered at the month-of-birth (mob) level; 1 (mob ≥ Jan. 1947) is an indicator variable that equals one for individuals born in or after January 1947. Other controls include a male indicator, a minority indicator, and the fraction of college-educated people among the pre-CR cohorts in 1982 at the provincial level (a proxy for pre-CR provincial college admission quota).

around the January 1947 birth-date cutoff. Interestingly, however, the fractions of people with a college education are slightly larger than their 1990 levels, both for the pre-CR cohorts and the post-CR cohorts. It seems, therefore, that people invested in college education upon reaching middle age. Perhaps less obvious in the graph is how the size of the discontinuity changes over the same period. As is evident from columns 1 and 2 of table 2, panel B, the absolute value of the estimated discontinuity in college education is 0.047, about 13% smaller than the 1990 estimate, which suggests that the post-1947 cohorts had put in some effort to make up for their educational loss relative to the older cohorts.[20] Similar to the 1990 results, the 2000 results are robust to the choice of bandwidth or functional forms (cols. 3 and 4 in table 2, panel B).[21]

Delayed enrollment or grade skipping can potentially affect our RD estimation.[22] The RD estimate would understate the true effect if a significant share of

[20] Based on different identifying assumptions and a smaller urban sample, Han, Suen, and Zhang (2019) estimate 0.028 years of extra educational investment between 1988–90 and 2001–3 for those whose college education was disrupted during the Cultural Revolution (table 4, col. 4). By comparison, our estimates imply a slightly smaller reinvestment effect ($(0.053 - 0.047) \times 4 = 0.024$ years of extra college education) from 1990 to 2000 for the affected cohort.

[21] We estimate a similar effect of the college suspension on college education for the same cohorts with data from the Urban Household Survey. The results are reported in the appendix, available online.

[22] The open circles lying on the vertical line at the birth cutoff in fig. 3 may reflect the influence of the late or early school enrollees. However, they could also reflect the relative importance of factors other than the threshold rule effect or sampling errors in finite samples.

the individuals did not follow the typical schooling schedule.[23] Although we are unable to quantify the fraction of late and early starters in our sample, there is indirect evidence that it may be small. As figure A2 (figs. A1–A4 are available in the online appendix) shows, the discontinuity estimated at January 1946 is small and statistically insignificant. It suggests that people who started school 1 year later account for a small fraction in our sample. Similarly, we estimate small and insignificant discontinuities around the January 1945 and January 1948 cutoffs. These results similarly indicate that people who started school 2 years later or a year earlier account for a negligible fraction of our sample. These results, taken together, suggest that our RD estimate should be close to the true effect of college suspension.

The 1982 Census captures the cohorts of interest in their 30s, which enables us to observe their education at a relatively young age. However, we cannot conduct an RD analysis because we observe neither the birth month nor the exact birth year. We plot the fraction of people with a college education for each age as of July 1982 with the 1982 data in figure 4.[24]

Comparing data from the three censuses (1982, 1990, and 2000) indeed shows that people make up for the loss of education later in life. In order to make meaningful comparisons across different censuses, we calculate the college attainment rate for each birth cohort represented by ages as of July 1982 using the 1990 and 2000 Censuses. First of all, figure 4 shows an obvious downward shift in college education for the post-CR cohorts relative to the pre-CR cohorts observed in the 1982 Census, as one would expect as a result of the college enrollment suspension. Interestingly, the college attainment rates for any given cohort tend to be higher in later censuses, which suggests that people had been making efforts to get more college education, regardless of their exposure to the 1966 policy change. Moreover, the age profiles of college attainment rates are flatter in the 1990 and 2000 Censuses, which indicates that people who were more affected by the policy had partially made up for their educational loss.[25]

[23] Conceptually, the discontinuity estimate at January 1947 can be expressed as a weighted average of the true effect of college suspension on the normal school starters and the discontinuity in college education around January 1947 for late or early starters, which should be zero. The weights are fractions of the typical and atypical school starters in the population, respectively. Ideally, we hope the weight of atypical school starters is zero.

[24] Note here that those who were 35 years old were born either in the second half of 1946 or the first half of 1947, which straddle the threshold of January 1947. Those aged 36 or older belong to the pre-CR cohorts (more affected by the suspension of college enrollment), while those age 34 or younger belong to the post-CR cohorts (less affected by the suspension of college enrollment).

[25] The post-CR cohort could catch up on higher education by taking the National College Entrance Exam restored in 1977 and going to conventional colleges or by taking advantage of a variety of higher-education alternatives. Alternatives to formal higher education include evening universities, radio and

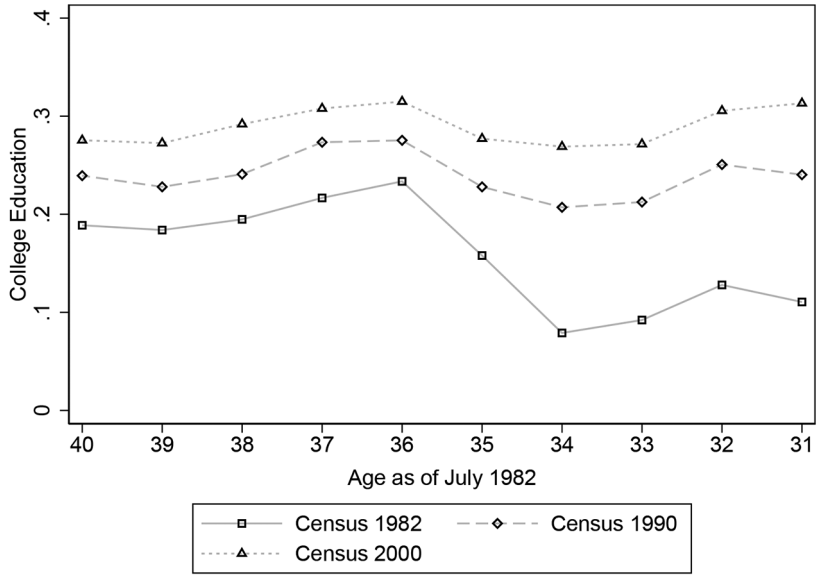

Figure 4. How college attainment changes over the life cycle for the same cohorts based on data from the 1982, 1990, and 2000 Population Censuses. College education includes both college and community college, because the 1982 Census does not distinguish between these two types of education. Cohort is defined by age in years as of July 1982, because the 1982 Census reports age instead of birth year. The bottom line plots the fraction of people with a college education for each age with the 1982 data. Those aged 36 or older belong to the pre–Cultural Revolution (CR) cohorts (less affected by the suspension of college enrollment), while those age 34 or younger belong to the post-CR cohorts (more affected by the suspension of college enrollment). The college completion rate for the same birth cohorts (defined by age as of July 1982) using the 1990 and 2000 Censuses are also displayed for cross-time comparisons.

B. Economic Outcomes

We now turn to an analysis of the effect of the enrollment suspension on our income measure, the occupation income score. Figure 5 shows the raw mean of log occupation income scores by birth cohort using data from the 2000 Population Census. Once again, open circles represent means for each month of birth. The polynomial fit, which includes second-order cohort trends with different slopes on the two sides of the 1947 cutoff, predicts that the average occupation income scores decreased by approximately 3% for the post-CR cohorts.[26]

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television universities, correspondence colleges, and higher education examinations for self-taught learners (for reviews, see Hawkins 1985; Latchem and Xinzheng 1999).

[26] The observed pattern of log earnings as a function of birth year may reflect a concave age-earnings profile. The individuals in our sample were between 49 and 58 years old in 2000. Without the break around 1947, the predicted curve in fig. 5 would look like a mirror image of a typical age-earnings profile (because age = 2000 − birth year). In 2000, those born in 1947 were 53 years old, at which age earnings would be close to the peak of the age-earnings profile.

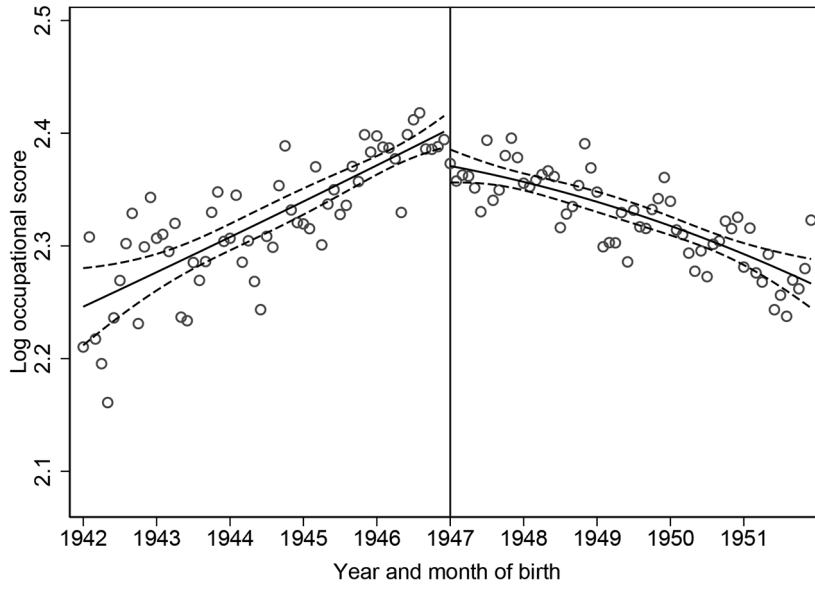

Figure 5. Impact of college enrollment suspension on log occupation income score. The sample is from the 2000 Population Census of China, restricted to individuals born between 1942 and 1951 and with at least a high school education. Occupation income score is the median income for each occupation in the 2005 National Population Sample Survey. Open circles represent means in each month-of-birth cell. The continuous line is the predicted outcome from a regression that includes a second-order polynomial in the running variable and a dummy for observations above the cutoff. Dashed lines are the 95% confidence intervals.

Consistent with the graphical evidence, estimates from regressions of different specifications (table 3) all suggest that the suspension had a large impact on our income measure. The simplest specification in column 1 shows that being born in January 1947 or later leads to a drop in the occupation income score by 3.3% (statistically significant at the 1% level). As shown in columns 2 and 3,

TABLE 3

EFFECTS OF COLLEGE ENROLLMENT SUSPENSION ON LOG OCCUPATION INCOME SCORE (2000 POPULATION CENSUS RESULTS)

	(1)	(2)	(3)	(4)
1 (mob ≥ Jan. 1947)	−.033	−.032	−.035	−.042
	(.011)	(.011)	(.011)	(.010)
Order of polynomial in running variable	2	2	2	1
Bandwidth (months)	60	60	48	24
Other demographic controls	No	Yes	Yes	Yes
R^2	.004	.006	.005	.004
Observations	80,598	80,598	63,822	31,428

Note. Occupation income score is the median income for each occupation in the 2005 National Population Sample Survey. The sample includes individuals with at least a high school education. Standard errors, shown in parentheses, are clustered at the month-of-birth (mob) level; 1 (mob ≥ Jan. 1947) is an indicator variable that equals one for individuals born in or after January 1947. Other demographic controls include a male indicator and a minority indicator.

controlling for more covariates and limiting the sample to a narrower interval around the threshold do not affect the estimate much. This finding is also robust to the use of a local linear specification with a 24-month bandwidth (col. 4).[27]

It is important to note that the suspension of college enrollment during the Cultural Revolution may have had general equilibrium effects by changing the aggregate relative supply of workers with and without a college education.[28] In particular, if workers from different cohorts are imperfect substitutes, the low average education of the affected cohorts could raise the return to college education and therefore widen the earnings gap between the affected and unaffected cohorts. This specific general equilibrium effect will not bias our estimate per se, because our RD approach still identifies the earnings gap between the two cohorts born before and after the cutoff, whose earnings would otherwise have been similar had the college enrollment not been suspended. However, this general equilibrium effect is likely to affect its interpretation, that is, whether and to what extent the estimated gap in earnings around the cutoff is due to the shortage of college-educated workers and therefore an increased return to college education.

To address this concern, we follow the procedure of Ichino and Winter-Ebmer (2004) to control for measures of cohort size and the fraction of college-educated workers in different cohorts in the regressions. We define three cohort size and relative education measures by prefecture (345 of them) and gender using the 2000 Census. The percentage of college-educated members of the CR (non-CR) cohorts is the fraction of college graduates among those high school graduates born between 1947 and 1956 (before 1947 or after 1956). The relative size of the CR cohorts is the size of the CR cohorts (born during 1947–56) as a fraction of total population. We expect these newly constructed variables to capture, at least partly, the policy-induced shifts in cohort-specific supplies of college-educated workers in the relevant labor market that could affect people's earnings.

Regression results are robust to including these cohort size variables (see table A5 [tables A1–A10 are available in the online appendix]). For better comparison, the first column of the table replicates results from column 2 of table 3. Columns 2–5 display the estimates augmented with the cell-specific cohort indicators. The RD estimate of enrollment suspension on log occupation income

[27] The implied return to college education can be estimated with a fuzzy RD design. The excluded instrument for college education is an indicator variable that equals one for individuals born in or after January 1947. The results are reported in table A4.

[28] There are other potential macroeconomic consequences of the Cultural Revolution. The loss of human capital, especially upper-tail human capital (Squicciarini and Voigtländer 2015), could significantly slow the progress of industrialization and inhibit growth potential. Politically, the Cultural Revolution may have paved the way for China's market-oriented economic reform and its subsequent economic boom (MacFarquhar and Schoenhals 2009). Our empirical framework, however, is unable to account for these macroeconomic effects of the Cultural Revolution.

score is largely insensitive to the inclusion of these indicators, which suggests that the estimated effect is more likely a result of lost education opportunities and less likely a result of a change in the return to college education.

Because the enrollment suspension affects the probability of working and information on occupation is available only for people who work, we need to deal with the sample selection that could bias our estimate of the treatment effect on the observed occupation income score. To assess the robustness of our results to this differential attrition, we use the conservative bounding approach of Lee (2009) to construct upper and lower bounds for the treatment effect. In our application, this requires trimming the upper or lower 5.7% of the occupation income score distribution for the treatment group, then reestimating the regression on this smaller sample.[29] The results of this conservative bounding exercise are presented in table A6. It appears that the Lee bounds are extremely tight, which suggests that our baseline RD estimate of the policy's effect on log occupation income score is robust to differential attrition induced by the treatment around the month-of-birth cutoff.

C. *Wealth*

In addition to the results on our income measure, we also examine the effects on household wealth, as proxied by the house size. As our estimates reported in table 4 suggest, being born in or after the 1947 cutoff leads to a large reduction in house size (over 3%), relative to those born on the other side of the threshold.[30] The estimate is largely insensitive to the inclusion of covariates and the use of a smaller bandwidth. We acknowledge that house size is an imperfect measure of household wealth. First, a few individuals in our sample did live with their parents, and the houses that we have information on might be their parents' assets. However, we believe that this measurement problem would not lead to significant bias in our estimation, because only less than 2% of the individuals were living in households headed by their parents. Second, the impact on house size could be related to the impacts on marriage outcomes. In fact, most (more than 92%) of the individuals in our sample were married at the time of being surveyed. Although the college enrollment suspension did not seem to have any effect on marriage (table 6), the estimated impact on house size could be partly driven by being married to someone with higher earnings. Finally, we performed the RD analysis of housing separately for urban

[29] To construct the Lee (2009) bounds, we trim the distribution of occupation income score for the treatment group (those born in or after 1947) by the difference in attrition rates between the treated and untreated groups as a proportion of the nonmissing rate of the treatment group.

[30] The corresponding graphic pattern of the relationship between log house size and birth cohort is shown in fig. A1.

TABLE 4
EFFECTS OF COLLEGE ENROLLMENT SUSPENSION ON LOG HOUSE SIZE (2000 POPULATION CENSUS RESULTS)

	(1)	(2)	(3)	(4)
1 (mob ≥ Jan. 1947)	−.032	−.035	−.038	−.043
	(.009)	(.008)	(.010)	(.010)
Order of polynomial in running variable	2	2	2	1
Bandwidth (months)	60	60	48	24
Other demographic controls	No	Yes	Yes	Yes
R^2	.001	.020	.021	.019
Observations	114,466	114,466	90,589	44,459

Note. The sample includes individuals with at least a high school education. Standard errors, shown in parentheses, are clustered at the month-of-birth (mob) level; 1 (mob ≥ Jan. 1947) is an indicator variable that equals one for individuals born in or after January 1947. Other demographic controls include a male indicator and a minority indicator.

and rural residents. The results, presented in table A7, suggest the effect of the policy on house size for rural residents is similar in magnitude to the effect for urban residents, although the latter was more precisely estimated.

D. Employment and Labor Supply

The detrimental effect of the enrollment suspension is also observed on employment later in life. As table 5 reveals, people who were born in or after January 1947 are 4.7 percentage points less likely to work in 2000, compared with people born right before that birth-date cutoff. No effect is found for a person being disabled or supported financially by other household members. This is not surprising because we are looking at people with at least a high school education, who are not very likely to be disabled or financially dependent on their families (table 1, panel A). Further, as column 4 of the same table shows, the policy is estimated to have no significant effect on labor supply, conditional on working. In summary, the suspension of college enrollment decreases the labor

TABLE 5
EFFECTS OF COLLEGE ENROLLMENT SUSPENSION ON LABOR MARKET PARTICIPATION (2000 POPULATION CENSUS RESULTS)

	Working (1)	Disabled (2)	Dependent (3)	Days Worked (4)
1 (mob ≥ Jan. 1947)	−.047	−.001	.001	.028
	(.012)	(.001)	(.003)	(.017)
Order of polynomial in running variable	2	2	2	2
Bandwidth (months)	60	60	60	60
Other demographic controls	Yes	Yes	Yes	Yes
R^2	.203	.000	.010	.007
Observations	115,889	115,889	115,889	79,147

Note. The sample includes individuals born between 1942 and 1951 and with at least a high school education. Standard errors, shown in parentheses, are clustered at the month-of-birth (mob) level; 1 (mob ≥ Jan. 1947) is an indicator variable that equals one for individuals born in or after January 1947. The regressions control for a second-order polynomial in the running variable. Other demographic controls include a male indicator and a minority indicator.

TABLE 6
EFFECTS OF COLLEGE ENROLLMENT SUSPENSION ON MARRIAGE MARKET OUTCOMES (2000 POPULATION CENSUS RESULTS)

	Unmarried (1)	Never Married (2)	Marriage Age (3)
		A. Men	
1 (mob ≥ Jan. 1947)	−.000	.000	−.148
	(.002)	(.001)	(.109)
Order of polynomial	2	2	2
Bandwidth (months)	60	60	60
Other demographic controls	Yes	Yes	Yes
R^2	.001	.000	.001
Observations	77,933	77,933	77,492
		B. Women	
1 (mob ≥ Jan. 1947)	−.002	.001	−.183
	(.005)	(.002)	(.102)
Order of polynomial	2	2	2
Bandwidth (months)	60	60	60
Other demographic controls	Yes	Yes	Yes
R^2	.004	.000	.025
Observations	37,956	37,956	37,753

Note. The sample includes individuals born between 1942 and 1951 and with at least a high school education. Standard errors, shown in parentheses, are clustered at the month-of-birth (mob) level; 1 (mob ≥ Jan. 1947) is an indicator variable that equals one for individuals born in or after January 1947. The regressions control for a second-order polynomial in the running variable. Other demographic controls include a minority indicator.

supply of the affected cohort at the extensive margin, while no significant effect is found at the intensive margin.

E. Marriage and Fertility

Aside from income and wealth, the suspension of college enrollment could also affect people's marriage market outcomes through its effect on education. As is evident from the results shown in table 6, the college enrollment suspension did not seem to have any effect on the chance of getting married, both for men and for women. This is not surprising, because we are focusing on people with at least a high school education, for whom marriage is nearly universal.[31] As for the timing of marriage, males were hardly affected by the policy (col. 3). Interestingly, however, women were married at earlier ages (around 2.2 months earlier) because of the policy (based on the estimate shown in col. 3). Previous research suggests that an improved level of female education tends to increase the age of marriage (e.g., Breierova and Duflo 2004), probably because it increases the women's opportunity cost of getting married early. In our case, by the same logic, the enrollment suspension would reduce women's chances

[31] In our sample of analysis, less than 0.6% of the men or women have never been married.

of getting a college education, thereby reducing the opportunity cost of getting married early.

Finally, we examine the policy's potential effect on fertility. We fit similar RD models with the sample of women from the 1990 Population Census, which captures the affected cohorts in their 40s. As table 7 shows, there are no significant impacts of the policy of college enrollment suspension on fertility.

F. Heterogeneity

We perform the same RD analysis separately for urban and rural residents (table A7). The negative effects of the shock on college education are larger for urban residents than for rural residents (col. 1). Moreover, the adverse employment effect is more pronounced for urban residents (col. 4). The smaller estimated impacts on rural people is not all that surprising because they had limited access to college education anyway. These results, however, should be interpreted with caution because they could be biased due to the potentially endogenous residential status measured later in life.[32] We also conduct the RD analysis separately for men and women. Compared with males, females are more adversely affected by the college suspension in terms of college education, log occupation income score, log house area, and labor force participation (table A8).[33]

G. Discussion

For those who were lucky enough to enter college right before the Cultural Revolution, the normal-style college education was mostly absent. The limited full-time college coursework for people born before January 1947 is unlikely to drive all the socioeconomic advantages they enjoyed later. Some other channels must be at work. First, students may have picked up some skills in college during the Cultural Revolution, even if it was not normal college education. Unfortunately, however, we are unable to measure these skills in the data. Second, college graduates may have enjoyed a network (or club) effect. Participation in revolutionary activities together may help form a stronger bond among schoolmates during the Cultural Revolution. However, it is very difficult to measure social network. Third, the students who went to college before the Cultural

[32] Ideally, we want to construct rural and urban subsamples based on the initial residential status. However, the residential status available in the data was reported as of 2000, when the respondents were already in their middle ages. Therefore, the residential status we observe is likely endogenous to the policy shock, as some of the formerly rural residents could obtain urban household registration after they finished their college education and found work in cities. Nevertheless, we are unable to bound the magnitude of this potential bias given the data we currently have.

[33] To account for the impact of mandatory retirement on the probability of working for females, we include an indicator for being younger than 55 years old in the regression as a robustness check. The estimated effect on working is largely insensitive to this exercise.

TABLE 7
EFFECTS OF COLLEGE ENROLLMENT SUSPENSION ON FERTILITY (1990 POPULATION CENSUS RESULTS)

	Births (1)	Childless (2)
1 (mob ≥ Jan. 1947)	−.051	.005
	(.043)	(.006)
Order of polynomial	2	2
Bandwidth (months)	60	60
Other demographic controls	Yes	Yes
R^2	.172	.002
Observations	20,011	20,011

Note. The sample includes individuals born between 1942 and 1951 and with at least a high school education. Standard errors, shown in parentheses, are clustered at the month-of-birth (mob) level; 1 (mob ≥ Jan. 1947) is an indicator variable that equals one for individuals born in or after January 1947. The regressions control for a second-order polynomial in the running variable. Other demographic controls include a minority indicator.

Revolution may have enjoyed an advantage during the early 1980s reform period, when China badly needed college-educated people to fill key management and technical positions in governments and factories. They could have received trainings on the job and learned from working experience, and the experience or skills acquired from work became valuable in the labor market to be developed later. Finally, the reputation of the colleges, which used to be more selective, might also be helpful for their careers. Given the data limitation, we are unable to test for the importance of any of these causal pathways, which we acknowledge as a weakness of this paper.

VI. Validity of the RD Design

For a first check, we examine the validity of the continuity assumption of the RD design, that is, whether people endogenously sort themselves around the cutoff line (McCrary 2008). Although manipulation of month of birth seems unlikely, we provide a visual check of its possibility. Figure 6*a* plots the number of high school graduates by year and month of birth for cohorts born between 1945 and 1948. There seems to be no indication of any discontinuous change in the density at the January 1947 cutoff. We also perform a formal nonparametric density test of month of birth in a smaller neighborhood around the January 1947 cutoff, the result of which is displayed in figure 6*b*. The test fails to reject the null hypothesis of no discontinuity in the density of month of birth with a p-value of .53. The results support the claim that there is no manipulation of the month of birth around the threshold.[34]

[34] For an alternative test, we regress the size of monthly cohorts on month dummies for a sample of 600 months (12 months × 50 years [1922–71]). Overall, none of the month-of-year dummies is statistically significant, which suggests that the months of birth are largely evenly distributed. In particular,

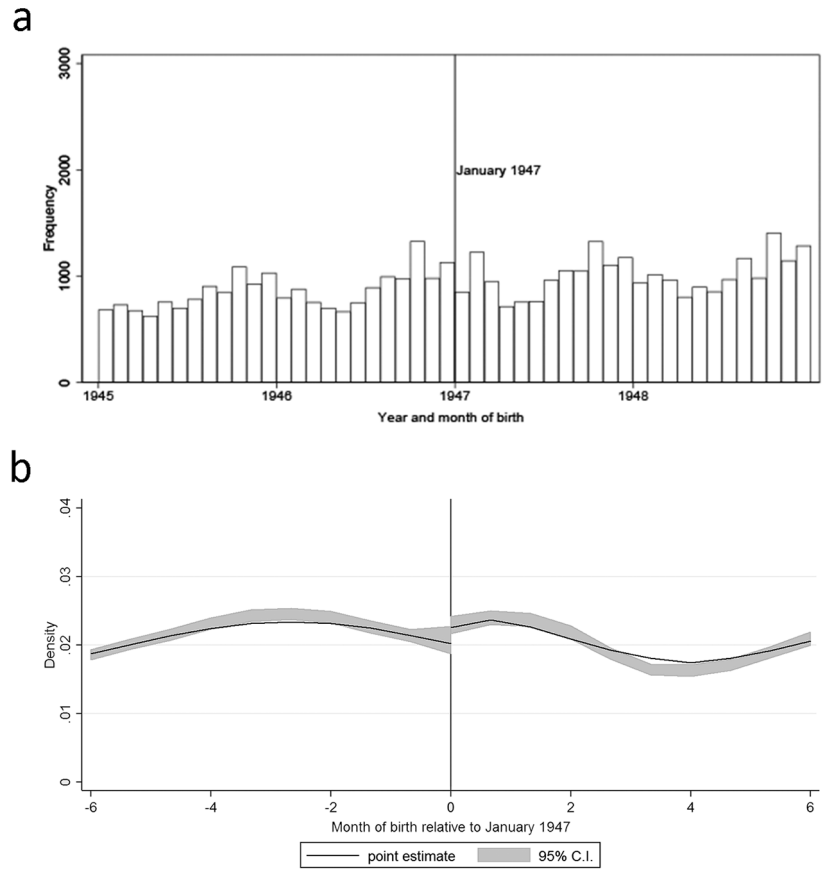

Figure 6. *a*, Cohort size by year and month of birth: 1945–48. *b*, Nonparametric density test of the running variable. Panel *a* is based on a sample from the 2000 Population Census of China, restricted to individuals with at least a high school education. For panel *b*, the figure and test are generated by the rddensity package for Stata using default settings on a sample of individuals with at least a high school education and born within 24 months of the cutoff.

For a second validity check, we test for potential discontinuities in a few predetermined individual attributes around the cutoff. For a valid RD design, the treatment variable should have no influence on the variables determined prior to the realization of the treatment. As a partial test of this assumption, we examine whether the observable pretreatment covariates are "locally" balanced around the threshold. Figure 7 plots the mean of some personal demographic characteristics, namely, gender and ethnicity, against the year and month of birth. Also plotted are the predicted values from regressions that include a second-order polynomial in the running variable and an indicator for observations above the

the January dummy is not significantly distinguishable from zero, alleviating the concern of manipulation across the January threshold.

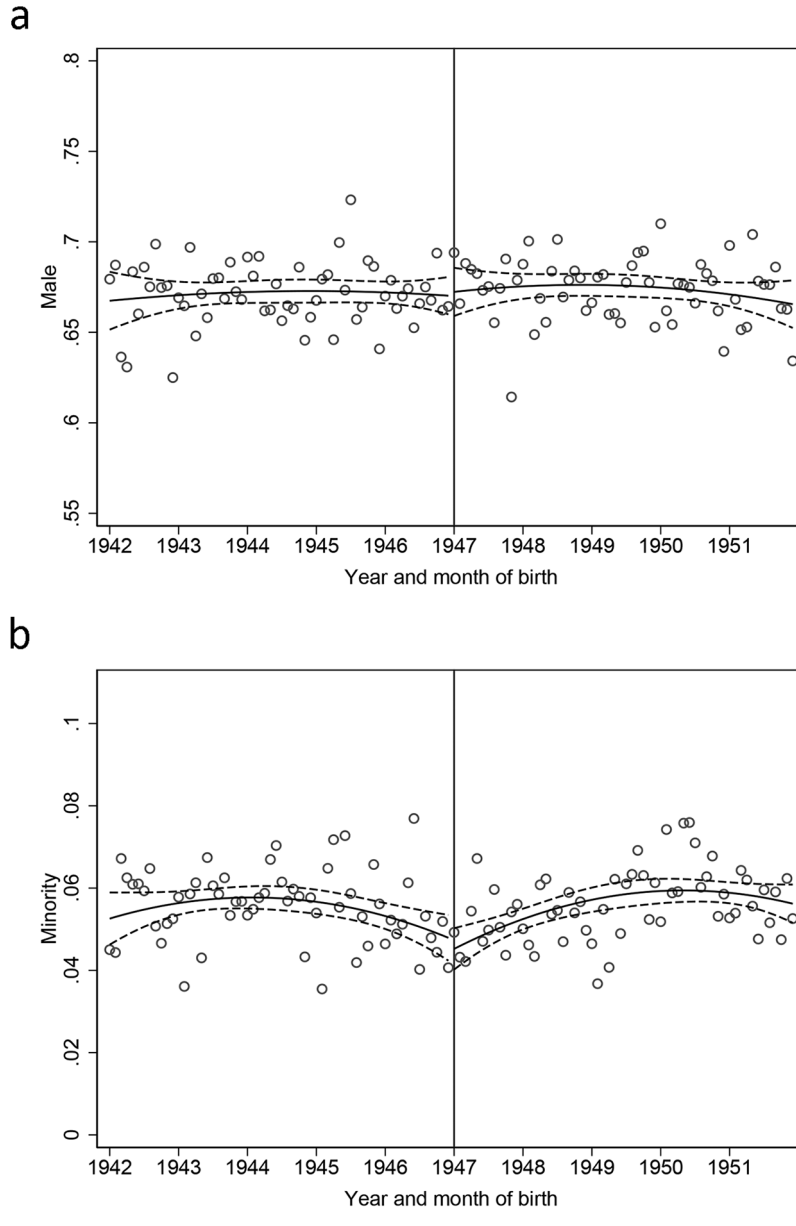

Figure 7. Similarity of individuals' pretreatment characteristics around the cutoff. *a*, Male. *b*, Minority. The sample is from the 2000 Population Census of China, restricted to individuals born between 1942 and 1951 and with at least a high school education. Open circles represent means in each month-of-birth cell. The continuous line is the predicted outcome from a regression that includes a second-order polynomial in the running variable and a dummy for observations above the cutoff. Dashed lines are the 95% confidence intervals.

threshold for both predetermined covariates. In general, the figures indicate that the difference at the 1947 threshold is small and statistically indistinguishable from zero.[35] Indeed, the means of both predetermined characters—gender and ethnicity—are balanced around the cutoff (table 1, panel B). The absence of a systematic difference in a predetermined variable around the January 1947 threshold supports the validity of our assumptions for equations (1) and (2).

For a third check, we examine whether there is discontinuity in selection into high school education. There may be concern about whether the composition of high school graduates differs on the two sides of the birth-date cutoff. To test this, we estimate a similar RD specification on a sample of individuals born between 1942 and 1951 and with at least a junior high school education from the 2000 Population Census. Figure 8 plots the mean of high school attainment rate against the year and month of birth. The figure also shows the predicted high school completion rate from a regression that includes a second-order polynomial in the running variable and an indicator for individuals above the cutoff. It is evident from the figure that the corresponding point estimate is small in magnitude and not statistically significant, suggesting no sharp changes in the opportunity or incentive to access high school education across neighboring cohorts. We also perform a similar discontinuity check on high school dropout rates around the same birth-date cutoff. The results are presented in figure 9, which indicates no discernible discontinuity in the dropout rate around January 1947 among the people who managed to enter high school.[36]

For a fourth check, we examine whether the suspension of college education has a similar discontinuous effect on people's "rusticated youth" experience around the same birth cohort cutoff.[37] In fact, many high school students who lost their last chance to attend college during the Cultural Revolution were sent down to rural areas for manual labor (Li, Rosenzweig, and Zhang 2010). To test whether someone's sent-down experience also changes discontinuously around birth cohort cutoff in 1947, we resort to another data set, the China Household Income Project, which contains data on whether the person has ever participated in the rustication movement.[38] Figure 10 plots the fraction of high school graduates who were sent to rural areas during the movement against the year of birth. It appears that the probability of having any sent-down experience is fairly smooth around the 1947 birth cohort, which lends more credence to the

[35] The RD regression results for both gender and ethnicity are reported in table A6.

[36] The regression results underlying figs. 8 and 9 are reported in table A9.

[37] Lin (2013) suggests that, compared with nonrusticates, the rusticates from the CR cohort are less likely to work in an elite occupation and more vulnerable to labor market retrenchment in their late-career stage.

[38] The Chinese Household Income Project conducted household surveys to track the dynamic changes in income distribution in China. We use the survey data from its 2002 wave, which covers 22 provinces.

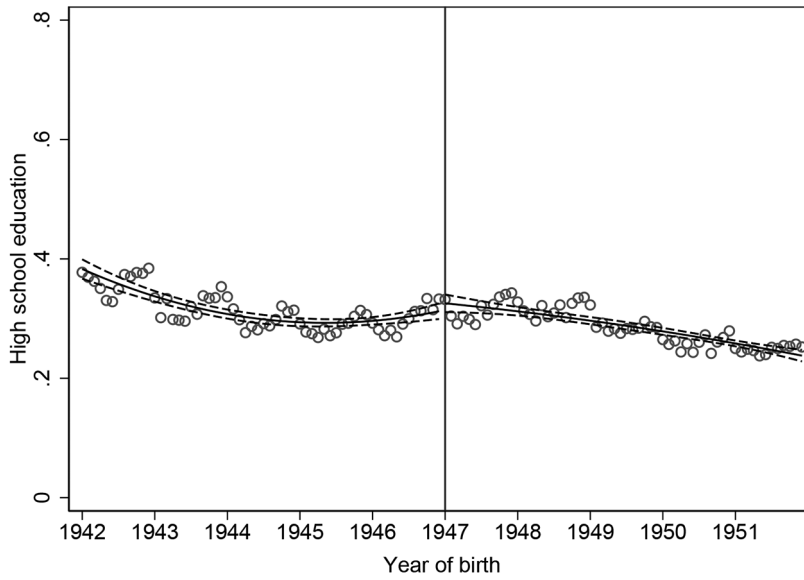

Figure 8. Testing for discontinuity in selection into high school education around the cutoff. The sample is from the 2000 Population Census of China, restricted to individuals born between 1942 and 1951 and with a junior high school education. Open circles represent means in each month-of-birth cell. The continuous line is the predicted outcome from a regression that includes a second-order polynomial in the running variable and a dummy for observations above the cutoff. Dashed lines are the 95% confidence intervals.

assumption that the unobserved characteristics of people born above and below the cutoff are largely comparable.

As a final check, we conduct falsification tests to make sure that the estimated effect of college enrollment suspension is not a "January birth effect" (see, e.g., Bedard and Dhuey 2006). Specifically, we implement similar RD analysis around January for all years in our sample. We start by presenting the estimation results for our main outcomes, college education, log occupation income score, and log house size (all measured in the 2000 Population Census) in table A10. One could simply look at the estimated treatment effects for the January cutoff in each year. For each of the three outcomes, the estimate corresponding to the true cutoff of January 1947 is noticeably larger (in absolute value) than those at the alternative January cutoffs (all of which should be close to zero). The estimates of the January 1947 cutoff are statistically significant at least at the 10% level for all three outcomes, while none of the alternative January cutoff estimates turns out to be precisely estimated. These estimated January discontinuities, taken together, seem to support the claim that our previously estimated discontinuity at January 1947 is real and systematic to the 1966 college enrollment suspension instead of a generic "January effect." For these three outcomes, the January RD graphs are also presented in figures A2–A4.

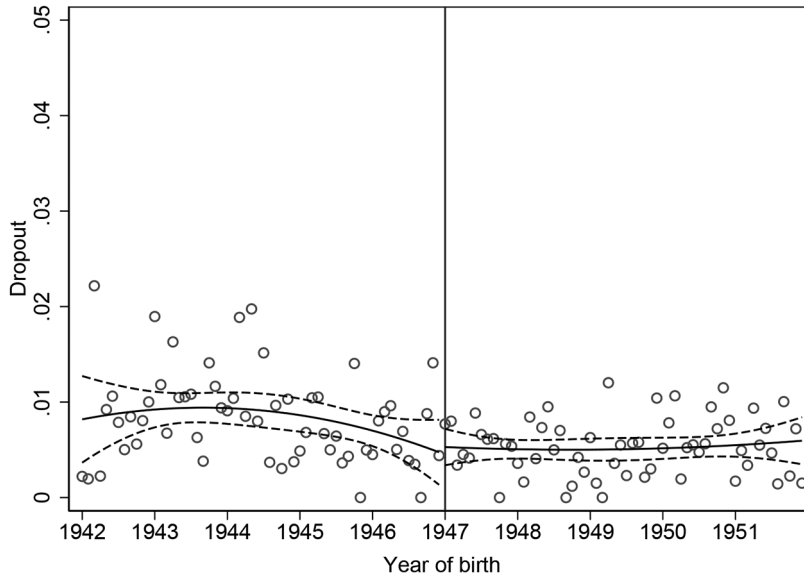

Figure 9. Testing for discontinuity in dropout rate among people with a high school education around the cutoff. The sample is from the 2000 Population Census of China, restricted to individuals born between 1942 and 1951 and with a high school (but no college) education. Open circles represent means in each month-of-birth cell. The continuous line is the predicted outcome from a regression that includes a second-order polynomial in the running variable and a dummy for observations above the cutoff. Dashed lines are the 95% confidence intervals.

We then perform this falsification test for additional labor market outcomes based on the 2000 Population Census (table A10). For all of the four labor market variables, none of the estimated January discontinuities is significant at conventional levels. We next repeat the same exercise for men's marriage outcomes. Once again, none of the estimated January discontinuities is statistically significant. Finally, we conduct the parallel analysis for women's marriage and fertility outcomes. To increase statistical power of the female results, we use a bandwidth of 24 months (2 years before and after the threshold) for the local linear estimation. Overall, the women's results are noisier than the men's results. We do find that the regressions tend to produce statistically significant estimates at a number of alternative January cutoffs but without a consistent pattern, as the estimates are sporadically distributed around zero. This is unlikely to be evidence of a consistent discontinuity at January but more likely a result of mere random chance.

VII. Conclusion

This paper evaluates the long-term detrimental effects of educational loss during the Cultural Revolution in China. The sudden suspension of college enrollment

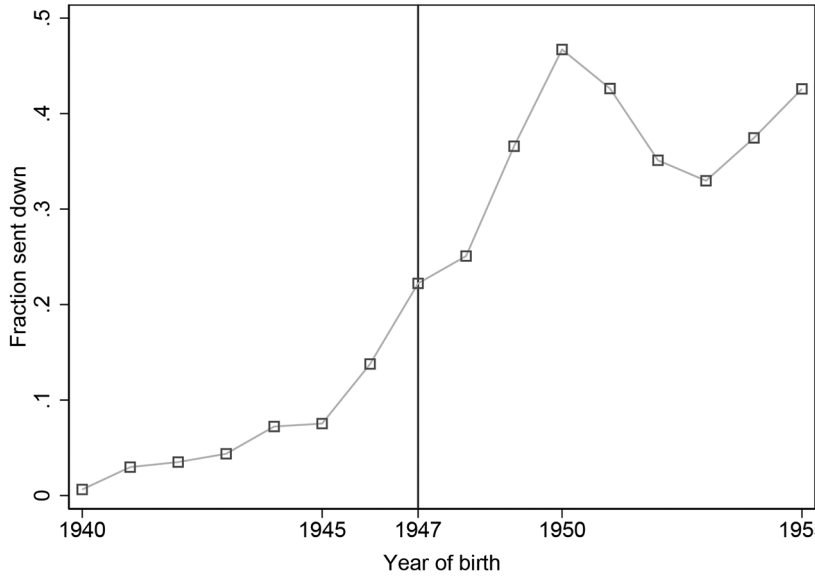

Figure 10. Fraction of cohort with sent-down experience. Source: Chinese Household Income Project, 2002.

in 1966 deprived individuals who had just completed high school of the opportunity for a college education. We use the discrete change in college education as a function of birth cohort to identify the disruption of college education in the Cultural Revolution. We find that this policy change led to a 5.4 percentage point decline in the chance of having a college education (observed in 1990) among those who finished high school. The analogous RD estimate for college education with the 2000 Census data is 0.7 percentage points smaller in size than that estimated with the 1990 Census data, which suggests that the affected generation had made a greater effort to make up for their loss of education later in life. However, the large RD estimate (of 4.7 percentage points) in 2000 implies that the affected cohorts were unable to fully catch up with the older cohorts as they approached retirement.

Using the same RD strategy, we find that the educational loss hurt the affected cohort along a wide array of dimensions. Most notably, the enrollment suspension decreased the labor supply and reduced earnings for those who worked. House size, which we use as a wealth proxy, was smaller for those who failed to achieve a college education because of this policy. The magnitude of the aforementioned effects is large and easily detectable more than 30 years after the policy was first implemented.

This study has several limitations. First, some important variables are not available in the Population Census data. Our results based on proxies of income

or wealth could be subject to bias from measurement error. In addition, we cannot check the robustness of our main findings by including control variables such as measures of family background. Second, we are unable to pin down the exact mechanism of why those who enrolled in colleges in 1965 and had limited academic training can enjoy all the socioeconomic advantages later on.

The Cultural Revolution has played a transformative role in Chinese history, but there has been little quantitative analysis of its broad social and economic effects. Our study finds large and enduring detrimental effects from its manipulation of higher education policies alone. Thus, more work is needed to fully evaluate the longer-term effects of the Cultural Revolution.

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