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1 February 2024

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February 2024

Abstract

This paper proposes an explanation for the increased college attainment of adults in China from 1990-2000. Using the timing of China's family planning policies implemented in the 1970s, we show that the early adoption of the policies increased the relative supply of young college labor, causing older adults to upgrade their education at the tertiary level. We find that the college wage premium for older adults increased from 1990-2000, matching the time when young college workers entered the labor market. Our results suggest that college rationing is costly to society, especially in the era of rising returns to education.

Key Words: adult labor, birth cohorts, Chinese family planning policies, migration.

JEL Classification: I24, J10, J20.

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1 Introduction

The number of college-educated workers in China has more than doubled between 1990 and 2000. Relatedly, the wage returns to a college education have increased substantially over this period (Maurer-Fazio 1999, Zhang, Zhao, Park, and Song 2005). However, an interesting and unusual component of these changes is the significant increase in the number of Chinese workers acquiring a college degree as adults (aged above 27). According to the Chinese censuses in 1990 and 2000, for people age 27-39 years old in the 1990 census (cohorts born between 1951 and 1963), the number with a college degree increased by nearly 78% from 1990 to 2000. The increase is smaller among the age group 40-49 in 1990 (cohorts born between 1941 and 1950), but still exceeded 26%. This pattern is not just a peculiarity of the census data: the number of college graduates increased similarly for adults in the 1941-1963 cohorts in the China Health and Retirement Longitudinal Study (see section 2.2).

Standard models of human capital investment predict low returns to investment during one's adulthood (Carneiro and Heckman 2003, Cunha and Heckman 2009), and empirical research has supported this conjecture (Silles 2007, Schwerdt, Messer, Woessmann, and Wolter 2012). However, in many countries, especially in East Asia, elitist ideas have restricted access to higher education, resulting in large unmet needs. Despite the high opportunity cost, adult education provides an alternative path towards higher education. The literature has rarely explored incentives for education upgrading for adults, with the exception of a strand of literature discussing the incentives to get a GED education while in prison (Tyler and Kling 2006, Darolia, Mueser, and Cronin 2021).

In this paper, we provide an explanation for the human capital accumulation of adults during the 1990s in China. We show that a decline in the young cohort size induced by China's "Later Longer and Fewer" family planning policies increased the relative supply of college workers—the ratio of the number of young college to non-college graduates—in the young cohort. In the presence of imperfect substitutability between college workers of different ages, an increase in the relative supply of young college workers raised the productivity of older college workers, thereby raising their wage and providing an incentive for adults without a college education to upgrade.

A challenge in studying the effect of the human capital level of one cohort on another is that the education

levels of both cohorts could be determined by unobserved factors.[1] In this paper we contribute to the existing literature on cross-cohort effects by using novel data we collected on the timing of China's "Later Longer and Fewer" family planning policies, which serves as a source of exogenous variation in the relative supply of young educated labor. We show that the "Later Longer and Fewer" policies (hereafter "LLF policies") led to a decline in the size of birth cohorts, which in turn significantly increased the education level of these smaller cohorts.[2] The LLF policies were implemented in different provinces at different times, allowing us to identify the cross-cohort effect using several waves of Chinese census data across provinces. Specifically, we compare the changes in the education level for older adult cohorts to the changes in the relative supply of the young educated, for provinces that implemented the policies at different times. This comparison only uses differences in the young cohort size and the number of the young educated that result from the policies, which we argue are exogenous to any factors affecting the incentive for an adult to upgrade their education. This empirical strategy allows us to isolate the causal effect of the human capital level of the young cohort on the upgrading of the older cohort, as long as the timing of the LLF policies is uncorrelated with other confounding factors across provinces and over time. As we discuss in section 2.3, characteristics of individual provincial leaders were the main factors determining the timing of a province's LLF policies, rather than current economic or human capital conditions. However, for additional robustness we examine other macroeconomic determinants of adult upgrading and find that the effect of the young cohort on the upgrading of the older cohort is the same when we further control for factors of economic growth, capital investment, population density, education supply, healthcare supply, and geographic location.

Our research is closely related to the seminal paper by Card and Lemieux (2001), whose model we use to formalize our argument. In their model the authors show that when younger and older workers are imperfect substitutes, changes in the relative supply of each group can lead to differences in the college wage premium across groups. They then present evidence arguing that the increase in the college premium for younger cohorts in the US, UK, and Canada, compared to a constant premium for older cohorts, resulted

[1] Recent work by Li, Liang, and Wu (2016) documents an increase in the college premium for older workers in China. The authors present an interesting theoretical model to suggest that the increase in the college premium for experienced workers is more likely to stem from a demand shock, specifically an increase in labor demand from productive foreign firms.

[2] The relationship between cohort size and educational attainment has also been studied in the context of the US baby boom (Stapleton and Young 1988, Wachter and Wascher 1984, Falaris and Peters 1992, Bound and Turner 2007). Unlike the paper by Bound and Turner (2007), we do not confront the issue of the supply constraint of college seats. In our context, the young cohort size shrank from the policies, therefore the change in their educational attainment fully reflected the quantity-quality trade-off within the family, given the elastic supply of college education.

from a decrease in the rate of college attainment of younger workers. We use their model to make a similar argument that China's LLF policies led to an increase in the relative supply of younger college workers, and that this supply shock increased the college wage premium for older cohorts. The higher premium then encouraged some older workers to obtain a college degree.

There have been few studies on the education upgrading of Chinese adults in this period. One notable exception is Han, Suen, and Zhang (2019), who also document the education upgrading of individuals over age 25.[3] In their paper the authors examine cohorts whose schooling was interrupted by the Cultural Revolution and show that education upgrading reflects the efforts by these cohorts to complete their education. They find evidence that the largest amount of upgrading occurs for pre-college students making up missed junior and senior high school; they find weaker evidence of an increase in college attainment and only for those cohorts already in college or senior high school at the start of the Cultural Revolution. In our paper we find evidence of additional college attainment for a wide range of cohorts, both those affected and unaffected by the Cultural Revolution (CR). However, there are several important differences between the methods and focus of their paper and this one. While their focus is on education upgrading resulting from the CR across all education groups, our focus is the effect of one cohort's education level on another cohort's, examining college attainment only. Further, they identify their key coefficients using cross-cohort variation resulting from the CR, while we use regional variation within a cohort and an instrument that is unlikely to be affected by the CR. The evidence presented in Han, Suen, and Zhang (2019) is persuasive and we agree that making up for interrupted education is an important explanation for some of the education upgrading we observe. Therefore we present our results as an additional explanation for upgrading in China and evidence of the importance of imperfect substitutability across birth cohorts.

Many papers have studied the features and effects of China's national one-child policy.[4] Our research examines an earlier period of the family planning policies which initiated in the 1960s and extended to the majority of provinces during the 1970s. According to the general fertility control guideline of the central

[3]The authors document this upgrading phenomenon using panel data from the China Health and Nutrition Survey (CHNS) and repeated cross-sections from the Urban Household Survey (UHS), while we use repeated cross-sections of the Chinese census.

[4]To list a few: Short and Zhai (1998), Attane (2002), and Gu, Wang, Guo, and Zhang (2007) study the specific implementations and variations of the one-child policy across locations. Bongaarts and Greenhalgh (1985), Yang and Chen (2004), and Li, Zhang, Zhu, et al. (2005) study the effect of the policy on the total fertility rate and the sibling size at the household level; Ebenstein (2010) and Li, Yi, and Zhang (2011) study the effect of the policy on sex ratio. Qian (2009) and Liu (2014) look at the effect of the policy on human capital investment of children. Other researchers also look at the effects on behavioral consequences, risk preferences, crime, etc (Cameron, Erkal, Gangadharan, and Meng 2013, Edlund, Li, Yi, and Zhang 2013).

government, some provinces started to advocate birth control programs as early as in 1963. Many provinces strengthened the family planning practices in the 1970s and started to implement the "Later Longer and Fewer" policies, i.e., later marriage, longer birth intervals, and fewer children.[5] We compare the differential declines in the young cohort size across provinces that carried out the LLF policies at different times. The strong enforcement of these policies allows us to predict the decline in the young cohort size, starting from different points of time for different provinces. Additionally, we discuss how the early enforcement of the policies might be related to the macroeconomic conditions of the provinces, and provide robustness checks by controlling for the differential effects of the initial conditions in the specification of the upgrading of older cohorts. To our knowledge, this is the first paper that uses the timing of the "Later Longer and Fewer" policies to study the effects on the educational outcomes of the young and older cohorts.

We find that the increase in the relative supply of young college graduates led many older adults to upgrade their education over the period 1990 to 2000. Our estimates of education upgrading are robust to various macroeconomic confounding factors. We also test for heterogeneous effects across birth cohorts and do not find strong evidence for heterogeneity. We evaluate two alternative explanations for our results. First, we decompose the increase in the number of adult college graduates into two channels: 1) in-migration of college graduates from another province versus 2) education upgrading of non-college graduates who were already in the province. We find that there was very little in-migration of college graduates, suggesting that the increase in older college graduates is mostly due to upgrading. Second, we investigate whether the LLF policies lead to upgrading not through a productivity change, but rather by reducing the number of children in the family, making it easier for parents to obtain a college degree. Our results show that family size plays a minor role in the upgrading of older cohorts in comparison to the effect of imperfect substitutability in productivity. Additionally, we also test whether the upgrading of older cohorts is due to a direct impact of the LLF policies rather than through the spillover effect of the youth. The results show that the policies do not directly affect these older cohorts.

At the end of our paper we estimate changes to the college wage premium. Consistent with the theory of imperfect substitutability of college workers of different ages, we find that the college wage premium

[5]During the period of "Later Longer and Fewer", the total fertility rate declined by a considerable magnitude in urban and rural China. According to Attane (2002), the total fertility rate declined from 3.22 in 1970 to 1.20 in 1980 in urban China; it declined from 6.31 in 1970 to 2.56 in 1980 in rural China. The trend of the total fertility rate remained flat in the 1980s for two regions. It remained roughly flat in the urban region and declined further in the rural region in the 1990s (Wang, Zhao, and Zhao 2017).

for older cohorts increases over the period and is matched with the timing when the young college workers enter the labor market. Again, this finding is not due to a direct effect of the LLF policies on wages and employment.

The results in this paper contribute to the literature on demographic transition and wage inequality. Our analysis of how a fertility decline affects human capital accumulation complements research by Bloom and Canning (2004) and Bloom, Canning, and Malaney (2000). These papers argue that during a demographic transition in which the mortality rate declines earlier than the fertility rate, the population-age distribution shifts towards more working adults and fewer children. A boom in the working-age population with a low dependency ratio can lead to a period of fast income growth and economic development. In this paper, we provide an example of how a decline in the young population caused by decreased fertility leads to regional disparities in human capital accumulation and wage growth. Through quantifying these results, we contribute to the literature on wage inequality (Autor and Katz 1999), and Mincer's analysis (1996) of economic development and the growth of human capital.

2 Background on Adult Education System and Family Planning Policies

In this section we first briefly describe China's education rationing and adult education system. Next, we show statistics on the education upgrading of older adults. We also provide evidence that the observed upgrading is not a spurious pattern resulting from measurement error by corroborating the basic upgrading using additional data sources. In the end of this section, we describe the family planning policies in China. While the policies date back to the 1950s, we focus our discussion on the period most relevant for our research, which started in the 1970s.

2.1 Education Rationing and Adult Education System in China

Chinese people have a tradition of emphasizing education, just like the rest of their East Asian neighbors. However, college admission is severely rationed. In 1981, for example, among 2.6 million high school graduates who took the national college entrance exams, only 11% were admitted. Such rationing was

lessened during the late 1990s and the admission rate reached 56% in 1999. Still, many young people wanted to attend college but could not. Education rationing creates an elevated competitive atmosphere and contributes to worsening mental health among children (Sun, Dunne, Hou, and Xu 2011, Zhu, Haegele, Liu, and Yu 2021).

With an unmet demand for education, many people opt for an adult education. The Chinese adult education system became popular in the late 1970s after the college entrance exams resumed following the end of the Cultural Revolution. The adult educational system consists of two different tracks: a diploma-oriented system and a non-diploma-oriented system. The non-diploma-oriented system caters to the demand for knowledge or skill improvement related to work or personal interests, such as job training. The focus of this paper is the diploma-oriented system, in which the goal is an academic diploma, here specifically, a tertiary-level diploma.

The diploma-oriented system was designed to help adults who exceeded the normal age of admission to earn a degree. To meet academic requirements, adult students took an entrance exam and then enrolled in a higher education institution. A typical program took two or three years, depending on whether it was part-time or full-time. Alternatively, adult students could participate in a self-study program, which provided a concentrated period of learning in night colleges, correspondence colleges, or television colleges organized by local colleges or universities.[6] In the end, students were required to take a set of exams and pass with a minimum score to get the diploma. There was no constraint on the amount of time it took to study or pass the exams. This "self-study" (zi-kao) regime was introduced as a policy in 1981 (Dahlman, Zeng, and Wang 2007).

Although it had existed for a long time, the diploma-oriented adult education started to emerge as a popular tool for achieving higher levels of education for people with some work experience and at least a high school diploma in the late 1980s. In 1999 there were about 0.8 million adults graduating from adult colleges, which was roughly the same number of college graduates from normal colleges in that year. Another 0.4 million adults became college graduates through the self-study examination (Ou 2001).

[6]Night colleges utilize the evening time. Correspondence colleges focus on the self-study and sometimes require a concentrated period to learn. TV colleges use a broadcast system and most often are equivalent to the correspondence colleges. Vocational colleges are special colleges founded by larger enterprises or local trade unions, in which the learning is part-time and linked to work practice. In recent years, the tuition of correspondence colleges ranges from 700-1500 US dollars per year.

2.2 Statistics on Adult Education Upgrading

Table 1 lists the national population with a college degree by birth cohort, as measured in the 1990 and 2000 Chinese censuses, separately. By observing the same birth cohort in different census years, we notice that for every cohort aged between 27 and 49 in the 1990 census (cohorts born between 1941 and 1963), the number in the cohort with a college degree increased noticeably in the 2000 census, and the increase differs across birth cohorts. The number of college graduates of birth cohorts 30 to 34 years old in 1990 increased nearly 90% by 2000, whereas the increase for the birth cohorts aged 45 and above is 17%.

To rule out that this is due to misreporting of educational attainment or measurement error in the census data, we examine an alternative variable only available in the 2000 census data: whether an individual has attended an adult school. Although not all upgrading is associated with attending an adult school,[7] we find that attending an adult school is consistent with much of the change in the educational attainment of birth cohorts across the 1990 and 2000 censuses. For the birth cohorts born between 1956 and 1963 (age 27-34 in 1990), the total number with a college degree increased by 2.7 million from 1990 to 2000. According to the alternative variable (whether attended an adult school) in the 2000 census, 2.6 million in these birth cohorts had graduated with a college degree from an adult school.[8] Two numbers are broadly consistent.

We further verify the fact of education upgrading with an additional data source: the 2014 life history survey of the China Health and Retirement Longitudinal Study (CHARLS). This survey provides a nationally representative sample of Chinese aged 45 and older in 2011, born before 1966. Using an event-history calendar method, the life history survey asked respondents to retrospectively report their entire residential, education, marital, fertility, work, and health history (Cui, Smith, and Zhao 2020). The survey was conducted by students hired by Peking University, not the government, and thus there was no incentive for respondents to lie about their education. The way the education history was asked also makes lying costly. Respondents reported their entire education history, including the beginning and ending year of each level of their education and whether they received a diploma or not at that level.

[7] As noted in section 2.1, the "self-study" regime became popular in the 1980s and therefore some adults may have acquired a college degree by passing an exam without ever enrolling in an adult school.

[8] Note that for the birth cohorts born between 1941 and 1955, the two measurements are different. The total number of college graduates increased by 1.7 million according the difference across the 1990 and 2000 census. The number who graduated from an adult college was 3.1 million according to the "adult school" variable in the 2000 census. The latter is significantly larger because these birth cohorts were more likely to have acquired a college degree before 1990, and the former only measures the change from 1990 to 2000.

Based on this sample, we calculated the number of college graduates for each year. From 1990 to 2000, the number of college graduates increased by 33%. In 1990 the youngest respondent was 24 years old and should have completed all formal education: thus all of this increase was from adult education. Among the older cohorts who upgraded to a college degree during the 1990s, 93% of them reported that they went to a formal university, while 7% reported that they acquired a degree through a "self-study" program or went to an informal institution, such as a radio and television university.

To summarize, we are confident that education upgrading was a real phenomenon. This phenomenon was also recognized and investigated by Han, Suen, and Zhang (2019). However, we recognize that dishonest behavior might exist. A fake diploma can be thought of as another form of upgrading, which saves effort but incurs legal risks. Additionally, we compare the differences in the increased college attainment across provinces in the empirical analysis. As long as the timing of the LLF policies is uncorrelated with misreporting or measurement error, our results should not be driven by this issue.

Lastly, a potential issue for our results is mortality selection. Although it cannot explain the increase in the number of college degree holders across the 1990 and 2000 censuses, it might bias our empirical results. Since less educated people have a higher mortality rate, the remaining population in a later census may appear to be more educated (Grossman 2006, Buckles, Hagemann, Malamud, Morrill, and Wozniak 2016). In our study, mortality selection is likely not an issue for the older cohorts in our sample since they were all below age 50 in the 1990 census. Nevertheless, we deal with the mortality selection issue by estimating the number of survivors in each birth cohort and controlling for this in our main regressions, and the empirical results remain the same.[9]

2.3 Family Planning Policies

In the early 1960s, the leaders of the central government started to advocate population control and argued that excessive population growth would lead to food shortages, unemployment pressure, and resource scarcity in the nation. Under this rationale, the first national family planning organization, named the Family Planning Committee of the State Council, was established in 1964. It became the central agency for making family planning policies and cooperating with local governments. The committee recommended a later age

[9]The impact of mortality selection on the education upgrading of older cohorts is not shown and available upon request.

of marriage, promoted new birth control pills, and advertised for birth control surgeries. The family planning policies were effective but were soon interrupted by the Cultural Revolution.

After 1970, the main steps of fertility control included making birth control pills and other methods free in the entire country, ordering local governments to establish family planning agencies, and increasing education intensity and media coverage to less dense areas. In 1973, the State Council initiated the "Later Longer and Fewer" family planning program nationwide. It encouraged every young couple to marry at a later age, adopt a longer birth spacing, and have fewer children.[10] The family planning work was quite successful in the 1970s, and the total fertility rate declined significantly. Nonetheless, in 1979 the government realized that it was difficult to meet the population target, hence the government developed a more forceful policy—the one-child policy.

From 1971 to 1979, all provinces except Tibet launched the "Later Longer and Fewer" program (Figure 1 is the geographic distribution of these provinces). Each province set up their own organizations and specified fertility rules following the general instruction of the State Council; provinces also set their own agendas for implementing the provincial policies (Peng 1997). Provinces differed in the strength of enforcement and whether they enforced the policies earlier or later. A number of reasons, such as the economic environment, social norms, or political power of the government may explain the differential enforcement (Attane 2002). Greenhalgh (2003) argues the Chinese government viewed the fertility policies as a strategic action to lead to modernization, thus the strength of government may play an important role in the enforcement. Based on evidence from media and interviews (Chen and Kols 1982), we argue that the strength of leadership in the provincial government largely determined whether the province implemented the policies earlier or later.

3 Impact of Young College Labor Supply on Adult Education Upgrading

We borrow the model from Card and Lemieux (2001) to explain how the relative supply of young college workers will increase the returns to a college education for older adults. The model predicts that the increased

[10] "Later" means a marriage age of 25 for men and 23 for women. Some more stringent rules specify age 28 for men and age 25 for women. "Longer" means the birth spacing should be more than three years. "Fewer" means that a couple is recommended to have two children or fewer. In some circumstances, rural couples are encouraged to have three or four children or fewer.

education level in the young cohort will increase the productivity of older college workers and therefore their returns to college education, and thus lead to adult college upgrading.

In the Card and Lemieux model, workers with the same education level but different ages are imperfect substitutes. This is modeled in equation (2) using constant elasticity of substitution (CES) functions for the aggregate labor of each education level, high school (H_t) and college (C_t), where i indicates cohort (Y for young, O for older), t indicates time, η is a parameter defining the elasticity of substitution between workers of the *same* education level, and α and β are efficiency parameters.

$$H_t = \left[\sum_i (\alpha_i H_{it}^{\eta})\right]^{1/\eta} = (\alpha_Y H_{Yt}^{\eta} + \alpha_O H_{Ot}^{\eta})^{1/\eta}, \tag{1}$$

$$C_t = \left[\sum_i (\beta_i C_{it}^{\eta})\right]^{1/\eta} = (\beta_Y C_{Yt}^{\eta} + \beta_O C_{Ot}^{\eta})^{1/\eta} \tag{2}$$

The production function itself, equation (3), is also a CES function with ρ defining the elasticity of substitution between workers of *different* education levels and the θ parameters measuring the technological efficiency of each education level at time t. The first order conditions for cost minimization imply that the ratio of wages for different groups is equal to the ratio of their marginal products. Using these conditions, the authors derive equation (4) showing the ratio of wages ("college premium") as a function of efficiency parameters, elasticities of substitution, cohort specific sizes of each worker type, and the aggregate quantities of each worker type.

$$y_t = (\theta_{ht} H_t^{\rho} + \theta_{ct} C_t^{\rho})^{1/\rho} \tag{3}$$

$$ln\left(\frac{w_{it}^c}{w_{it}^h}\right) = ln\left(\frac{\theta_{ct}}{\theta_{ht}}\right) + ln\left(\frac{\beta_i}{\alpha_i}\right) + \left[\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right] ln\left(\frac{C_t}{H_t}\right) - \frac{1}{\sigma_A} ln\left(\frac{C_{it}}{H_{it}}\right) + \epsilon_{it} \tag{4}$$

The key term for this paper is $\left[\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right] ln\left(\frac{C_t}{H_t}\right)$, which shows how the aggregate ratio of college workers to high school workers affects the cohort-specific college premium. The elasticity of substitution of workers of the same education level but different ages is $\sigma_A \equiv \frac{1}{1-\eta}$, while the elasticity of substitution for workers of different education levels is $\sigma_E \equiv \frac{1}{1-\rho}$. If workers of different education levels are more substitutable than workers of the same education but different ages, $\sigma_E > \sigma_A$, then an increase in the aggregate ratio of college workers to high school workers raises the cohort specific college premium. However, if workers of the same

education level are perfect substitutes then an increase in the aggregate ratio ($\frac{C_t}{H_t}$) lowers the college wage premium for all age cohorts.[11]

In the case of China, we will show that provinces which initiated the LLF policies earlier experienced earlier declines in cohort size. If these smaller cohorts had access to the same total supply of schooling then a greater percentage of the cohort would become college educated and thus the aggregate ratio ($\frac{C_t}{H_t}$) would rise. Assuming that $\sigma_E > \sigma_A$, this would raise the education premium for older cohorts, providing an incentive for older high school educated workers to upgrade their education.

However, this result will depend on the supply elasticity of college education to older adults. Studying the United States, Bound and Turner (2007) find that larger cohorts have lower college attainment, which they attribute to inelastic public investment in higher education, resulting in lower per-student public subsidies. Adult workers studied in this paper were born in a large birth cohort and endowed with a low level of education resources,[12] therefore limited education resources could be a constraint to their college enrollment in their early twenties.

4 Data and Trends in Young Cohort Size and Adult Education Upgrading

4.1 Data

We collect the timing of the LLF policies from Peng (1997), provincial chronicles, provincial population chronicles, provincial geographic chronicles, and China population chronicles: provincial volumes. We use Shanghai as an example to illustrate this data collection process. According to Peng (1997), in January 1958, the third meeting of the Second People's Congress of Shanghai passed a resolution: "in order to avoid the rapid expansion of the urban population, it is necessary to promote birth planning." The associated departments of the city promulgated the policy and required each family to have two or three children, with

[11] Perfect substitutability implies $1/\sigma_A = 0$, making equation (4): $ln\left(\frac{w_{it}^c}{w_{it}^h}\right) = ln\left(\frac{\theta_{ct}}{\theta_{ht}}\right) + ln\left(\frac{\beta_i}{\alpha_i}\right) - \frac{1}{\sigma_E} ln\left(\frac{C_t}{H_t}\right) + \epsilon_{it}$. In this setting an increase in the aggregate ratio ($\frac{C_t}{H_t}$) has an identical negative effect for all age groups.

[12] The college enrollment was halted during the Cultural Revolution. It was only in 1977 that the national higher education entrance examination (gaokao) was resumed. Birth cohorts born in 1947-55 (according to Meng and Gregory (2002)) were interrupted in the stage of senior high and/or college education during the CR and had to compete with fresh high school graduates in the gaokao held in 1977 and 1978.

a birth interval of three to four years. In 1963, the Family Planning Committee was established, and it spread family planning knowledge and provided free birth control surgeries to rural areas. All districts (equivalent to a prefecture level) of Shanghai promoted the "Later Longer and Fewer" program. In 1964, Shanghai started to develop new contraceptive pills and other methods. In 1967, the State Science and Technology Committee approved Shanghai's strategy and planned to promote family planning work in the rest of the country. Given that Shanghai made specific rules about the desired number of children and the birth spacing in January 1958, we use January 1958 as the initial time of the LLF policies.[13]

Columns (1) and (2) of Table A1 show the initial time of the implementation of the LLF policies in each province and columns (3) and (4) show the earliest birth cohort affected by the policies, i.e., born 9 months after the implementation month. Columns (5) and (6) list the age of the earliest cohort affected at the time of 1990 and 2000 censuses respectively. Anyone younger than this age is also affected by the policies. In columns (7) and (8), we list the affected age groups among the 20-26 year-olds in 1990 and 2000. Notice that the 20-26 year-olds are not affected in any provinces in 1990, except Beijing, Shanghai and Tianjin. However, by the year 2000 they are affected in all provinces, except Tibet.[14] Our identification strategy for the cross-cohort effect is to use the LLF policy time, combining both the policy year and policy month, as a source of exogenous variation in the educational attainment of the young cohort aged 20 to 26. Specifically, policy time is defined as $policy\ year + policy\ month/12$ (see column (9)). The earlier the policy time is, a wider age range of the young cohort is affected, i.e, the exposure of the young cohort is decreasing with the policy time. In section 6, we will show that the policy time has a significant effect on the population of the young cohort as well as their educational outcomes.

We define the young cohort as those aged 20-26, a range that captures young people just entering the labor market who might a have a potential impact on the education upgrading of older cohorts. We choose 20 as the lower age limit because 20 is the age cutoff when young people could have graduated from a college.[15] We choose 26 as the upper age limit for the young cohort because this is the average age when

[13]Chen and Fang (2021) study the effect of LLF policies on the life quality in old age of affected individuals. Their timing measure differs slightly from ours because they date the policies for each province from the establishment of the family planning leading group, whereas we use the initial government announcement.

[14]The "Later Longer and Fewer" family planning policies affected the young cohort in the 1990's. We restrict our attention to this period to ensure that our results are not confounded by other policies before 1990 or after 2000.

[15]A two-year college degree is very common in China: about 75% of college graduates aged 20 to 26 only have a two-year degree in 2000, and the percentage goes up to 90% for those aged 20 in 2000.

a college graduate finds their first job according to the China Urban Household Survey (UHS). The age of older cohorts is 27 and above. People in this age group are old enough such that they have likely been in the workforce for several years and any increase in college attainment would reflect adult education, rather than say delayed graduation of students attending college shortly after high school.

For descriptive purposes, we divide the provinces into an early adoption group and a later adoption group in column (10). The cut-off is the median time of the policy implementation—August 1973. We assign provinces that implemented the policies in August 1973 or earlier to the early adoption group. We use this variable for describing our data in the following section 4.2.

To track the education upgrading of adults and link it to the change in the relative supply of young college graduates, we need comprehensive data on the national geographic and temporal distribution of young and older adults, along with information on their education levels, migration status, and other demographic characteristics. We use samples of the 1990 and 2000 Chinese censuses, each with a sampling rate of 1%. The detailed characteristics in the censuses include sex, year of birth, month of birth, ethnicity, hukou type, current residence, residence five years ago (1990 and 2000 samples), province of birth (2000 sample), education levels, and educational status (whether one is currently enrolled or has graduated). We also use provincial-level economic and demographic variables from provincial statistical yearbooks to control for potential confounding effects across provinces that might be correlated with the timing of the LLF policies.

We construct the number of college and non-college graduates for individual birth year cohorts of older adults in each province and year, as well as the share of college graduates of older adults. When constructing these variables, we include both household and collective observations.[16] College graduates are those with at least a college degree, and non-college graduates are those with no more than a high school degree. We define older adults as those born between 1941 and 1963, who are aged 27-49 in 1990. For the young cohort, we group all young people aged 20-26 and calculate the ratio of college to non-college graduates at the province-year level. Since the LLF policies are implemented at the province level, ideally we would define the province of the young cohort using birth province. However, the 1990 census doesn't report birth province and thus we use the province they resided in five years ago (when they were 15-21 years old).

We exclude Beijing, Shanghai, Tianjin, and minority autonomous provinces in our sample. Beijing,

[16] A household is a unit of individuals who are related to each other. A collective unit is a group of people living together with no relationship with each other, for example, workers in a company's dorm or college students in a dorm.

Shanghai and Tianjin initiated the policies much earlier than the rest of the country. As a result, the young cohort from these provinces was already treated by 1990, and therefore there is no pre-treatment period for comparison. For the autonomous provinces, the policies were not strictly implemented and therefore they are not comparable to other provinces.[17] Figure 2 plots the total number of people born in the same year and from the same province using the birth province data from the 2000 census sample. In each panel, the red line indicates the first year when a birth cohort was born under the LLF policies. We see that the cohort size shrinks after the polices.[18] In section 5.1, we will plot the size of young cohorts aged 20-26 by event year to demonstrate how the LLF policies affected the group of young cohorts together.

4.2 Statistics on Education of the Young and Older Adults

Table 2 provides the census sample statistics for the young cohort and the older birth cohorts in 1990 and 2000, and the changes from 1990 to 2000, respectively. The upper panel shows statistics for cohorts who are 20-26 in 1990 and cohorts who are 20-26 in 2000, while the lower panel shows statistics for the same group of older cohorts in different census years. We find that the young population in the average province declines by 2.6 million, a 34 percent reduction from the 1990 level. The decrease in natural logarithms is 0.41. The number of young college graduates more than doubles, increasing by 160 thousand from 1990 to 2000 (a 134% increase). Consistently, the population of the young cohort without a college degree decreases even further than the reduction in the total population, by about 36%. The ratio of the number of young college to non-college graduates, i.e., the relative supply of young college graduates, increases from 0.017 in 1990 to 0.061 in 2000 (a 260% increase).

The number of older college graduates increases by 7.59 thousand within a provincial birth cohort from 1990 to 2000 (a 69% increase). Since there are 23 birth cohorts within a province, all the older birth cohorts combined lead to an increase of 174.57 thousand. Further, we define the native college graduates as those who lived in the same province five years ago. Likewise, the migrant college graduates are those who lived in a different province five years ago. A noticeable fact is that most of the increase in the number of college

[17] For example, the policies were only recommended in the counties with the density above 150 people per square kilometer in Guangxi. In Xinjiang province, the policies were only carried out in a few cities.

[18] We also notice that there is a substantial decline in the cohort size from 1959 to 1962—this period corresponds to the campaign of the Great Leap Forward and the Great Famine. This notch in the pattern appears in the majority of the provinces in our sample.

graduates comes from the native population, which contributes to 7.63 thousand within a provincial birth cohort. The number of migrant college graduates actually decreases by about 40 people per cohort during this period, a 12% drop. The share of college graduates increases by 1.4 percentage points.

To illustrate the causal effect of the human capital level of the young cohort on the education upgrading of older cohorts, we separate the levels and changes in education between provinces that adopted the policies early or later in Table 3 (see the definition of early or later adoption group in column (10) of Table A1). We show that the increase in the relative supply of young college graduates is 0.050 in provinces that adopted the policies early, compared to 0.034 in provinces that adopted them later. Under the assumption of timing exogeneity, this implies that the earlier adoption of the policies increases the relative supply of young college graduates by 0.016, or a 94% increase from the level in 1990 across all provinces in the sample. For older cohorts, the increase in the number of college graduates in each birth cohort is 8.93 thousand for early provinces, and 5.65 for later provinces. The difference between them is 3.28 thousand. Given that the average increase in the number of older college graduates is 7.59 thousand in our sample, about 40% of the average increase is caused by the early adoption of the LLF policies. The Wald estimate of the effect of the relative supply of young college graduates on the education upgrading of older cohorts is simply 3.28 divided by 0.016, or 205 thousand college graduates.

5 Econometric Specification and Identification Strategy

In this section, we lay out a simple difference-in-difference model to estimate the impact of the young cohort on adult education upgrading. We start with the following specification for the educational outcomes of older adults.

$$Older\_Coll_{jpt} = \alpha YC\_relativeColl_{pt} + \sum_{p} \beta_p Province_p + \gamma(Initial_p \times Year_{2000}) + \delta Year_{2000} + \sum_{j} \mu_j Z_j + \epsilon_{jpt} \tag{5}$$

The term $Older\_Coll_{jpt}$ is the total number of older college graduates or the share of older college graduates in birth cohort j, in province p, and observed at time t. The term $YC\_relativeColl_{pt}$ is the relative supply of young college graduates in each province in each year. The parameter α is the impact of the young cohort

on adult upgrading.

$Province_p$ is a province fixed effect, controlling for any time-independent factors that could contribute to the older adult outcome, while $Year_{2000}$ is an indicator for 2000. The initial condition $Initial_p$ is the mean population between 1960 and 1969; its interaction with $Year_{2000}$ captures any differential changes in the adult outcome that is driven by the initial population level. Z_j is a birth cohort fixed effect for those born in the same year. We use the birth cohort fixed effect to control for factors related to the population compositional change. It also captures characteristics specific to a birth cohort. For example, the Cultural Revolution may have delayed college entry for certain birth cohorts. The error term ϵ_{jpt} includes any unobserved characteristics at the cohort, province, and year level.

The OLS estimation of equation (5) could yield an inconsistent estimate of α if $YC\_relativeColl_{pt}$ is correlated with unobserved characteristics in ϵ_{jpt}, such as agglomeration economies and local subsidies for college education. Our identification strategy is to use the timing of the initiation of the "Later Longer and Fewer" program to generate an exogenous variation in the relative supply of young college graduates. The first-stage equation is:

$$\begin{aligned} YC\_relativeColl_{pt} = a_1(Policy\_time_p \times Post_{pt}) + \sum_p b_{1p} Province_p + c_1(Initial_p \times Year_{2000}) \\ + d_1 Year_{2000} + \sum_j e_{1j} Z_j + e_{pt} \end{aligned} \tag{6}$$

The left-hand side variable is the relative supply of young college graduates. The variable $Policy\_time_p$ is the initiation time of the LLF policies, which is defined as $policy\ year + policy\ month/12$. The variable $Post_{pt}$ indicates whether the young cohort is affected in province p in year t. As we showed in Table A1, the young cohort is not affected except in Beijing, Shanghai and Tianjin in 1990. They are all affected except in Tibet in 2000. Because we exclude Beijing, Shanghai, Tianjin and minority autonomous provinces in our sample, the variable $Post_{pt}$ takes the value zero for year 1990 and one for year 2000 for any given province p. This means that the young cohorts from all provinces in our sample are treated by year 2000 and therefore we cannot compare treatment and control groups. Instead, we use the variable $Policy\_time_p$ to capture the intensity of the treatment for the young cohorts. The parameter a_1 captures the change in the relative supply of young college graduates that is due to the timing of the LLF policies. As we show in Table A1, the earlier

the policy time is, the more exposure the young cohort has. The variables $Province_p$, $Year_{2000}$, $Initial_p$ and Z_j are the same as those in equation (5). The error term e_{pt} is the uncorrelated residual of the relative supply of young college graduates.

The key identification assumption is that the geographic variation in the timing of the initiation of the policies captured by $Policy\_time_p$ is uncorrelated with changes in the unobservable term $(\epsilon_{jp,2000} - \epsilon_{jp,1990})$, or the variation in the initiation of policies across provinces is not correlated with changes in other economic conditions affecting upgrading. Another way to interpret the identification strategy is that we assume that provinces that adopted the policies early would have had the same increase in the educational outcome of older adults as provinces that adopted the policies later. To validate this, we first show evidence in support of the assumption in the next subsection. Next, in section 6.3 we examine whether the factors of economic growth, such as capital investment and population density, could potentially affect the educational outcome of older adults. In section 7 we examine what is the mechanism that the relative supply of young college workers affected the education upgrading of older adults and further explore whether the decreased number of children in the household could cause the upgrading as well.

5.1 Evidence in Support of Identification Strategy

First, we provide evidence that the timing of the LLF policies is exogenous by showing that the provinces were similar to each other in macroeconomic conditions before the policies were implemented. We regress each initial macroeconomic characteristic on the policy time variable in separate regressions. Results are shown in Table 4. Column (1) lists the mean and standard deviation of each initial condition before the policies were initiated. Column (2) reports the coefficient on the policy time from separate regressions for each initial condition. Column (3) is the p-value of the F-statistic under the null hypothesis that the coefficient on the policy time is insignificantly different from zero. We see that the provinces that adopted the policies earlier tend to have higher levels of log real GDP, log number of teachers or schools, log number of doctors and hospital beds, and lower level of log real fixed asset investment. They also tend to be denser and have larger population. However, the differences between early and later adopters are mostly not statistically significant. One exception is that the difference in the log number of secondary schools is significant at the 10% level. In the robustness checks in section 6, we will control for these characteristics as well.

Next, to further evaluate the validity of our identification strategy, we show that there is no pre-trend in the young cohort size. In addition, we use these results to rule out other confounding factors that cause the changes in the young cohort size and might also contribute to the upgrading of older cohorts. Ideally, we want to show that there is no pre-trend in the relative supply of young college graduates. Unfortunately, we have no data on the education level of young cohorts by year. Instead, we use the birth province data from the 2000 census sample to predict the log number of young people aged between 20 and 26 in each province in each year. We exclude Beijing, Shanghai, Tianjin, and autonomous provinces and restrict the calendar years to be between 1990 and 2000. The specification is the following:

$$\begin{aligned} lg\_Young_{pt} = & \sum_{p} \eta_p Province_p + \sum_{t} \rho_t Year_t \\ & + \sum_{t} \phi_t (Initial_p \times Year_t) + \sum_{j} \lambda_j EventYear_j + \omega_{pt} \end{aligned} \tag{7}$$

The left-hand side variable is the predicted log number of young people aged 20-26 in province p in year t. On the right-hand side, we control for province fixed effects, calendar year fixed effects, the interactions of the initial conditions with calendar year indicators, and the event year indicators. Each event year variable indicates that it is the n_{th} year since the log number of young people aged 20-26 was treated by the policies. For example, event year "0" indicates that it is 21 years after the initiation of the LLF policies (20 years plus one year of conception).[19] Event years run from "-5" to "5" and the event year "-1" is omitted in the regression.

In Figure 3, we plot the estimates of coefficients λ's. The log number of young people declines after the event year "0" and the decline is statistically significant after the event year "1" when compared to the benchmark year "-1". More importantly, the figure shows that the levels of the log number of young people aged 20-26 from "-5" to "-2" are not significantly different from the level in the benchmark year "-1". In other words, there is no pre-trend in the log number of the 20-26 year-olds before the event occurs.

[19]Formally, we define this as $EventYear_j = \mathbf{1}(t - PolicyYear_p - 21 = j)$, where $PolicyYear_p$ is the year province p implemented the LLF policies.

6 Main Results

6.1 Impact of the "Later Longer and Fewer" Policies on the Young Cohort

Column (1) of Table 5 reports the first-stage result of equation (6): the impact of the LLF policy time on the relative supply of young college graduates who resided in the province five years ago. As mentioned in section 4.1, the province five years ago is a proxy for the birth province.[20] Since the treatment status of the young people is determined by their birth province and the calendar year, we cluster the standard errors at the province and year level.[21] The result in column (1) shows that for every one year earlier of the policy initiation time, the relative supply of young college graduates aged 20 to 26 increases by 0.004 from 1990 to 2000, or 24% of the 1990 level (0.017). The F-statistic for the significance of the policy time interacted with the post indicator is 26. The impact of the policy time on the relative supply of young college graduates is large. Given that the average increase in our sample during the same period is 0.044, implementing the policies one year earlier accounts for 9.7% of the increase in the relative supply of young college graduates in the decade, and implementing the policies one standard deviation earlier accounts for 19%.

Along with the result in column (1), we also show the impact of the policies on other outcomes of the young cohort in columns (2) to (5). In column (2), the population of the young cohort decreases more in provinces that adopted the policies early. The coefficient of the mean population between 1960 and 1969 is negative and significantly different from zero, suggesting the higher the mean population between 1960 and 1969 is, the more the number of young people decreases over time. For the change in the logarithm of the population, in column (3), the number of young people decreases by an additional 3.9% from 1990 to 2000 for every one year earlier of the policy time; this effect is substantial given that the average decrease in our sample during the period is 34%.

Columns (4) and (5) of Table 5 show the educational outcomes of young people. For the total number of young college graduates it increases by an additional 9,762 people for every one year earlier of the policy time. This accounts for 6.1% of the average increase in the number of young college graduates in our

[20] In our sample, we have the birth province information for those young college graduates in year 2000, we ran all the regressions using the birth province in 2000. The results, not shown here, suggest they are very similar to those in Table 5.

[21] The number of clusters would be too small if we cluster at the province level. We only have one period before and after the policy affected the young cohorts, so it is less likely to suffer from the serial correlation problem (Bertrand, Duflo, and Mullainathan 2004).

sample during the period. Column (5) shows the change in the logarithm of the number of young college graduates, which decreases by 0.7% for every one year earlier of the policy time.[22] These aggregate results are consistent with the quantity-quality trade-off theory: when family size decreases, the demand for quality of children increases and the children within the family are more likely to get higher education. Rosenzweig and Zhang (2009) use the incidence of twins at the second birth and find that increasing the number of children from two to three (an increase of 0.405 log points) decreases the expected college enrollment of all children in the family by 10 percentage points (or by 27%). In the urban sample, when the twinning incidence happens at the first birth, the number of children increases from one to two (increased by 0.693 in logarithm) and it decreases the expected college enrollment of all children by 11 percentage points (or by 14%). We find that a one-year earlier initiation of the LLF policies decreases the logarithm of the number of young people by 0.039, and increases the share of young college graduates by 0.5 percentage points (or by 32%).[23]

6.2 Impact of the Young on the Older Adult Educational Increase

Table 6 presents the OLS results for the relationship between the relative supply of young college graduates and the share and number of college graduates in each birth cohort for older adults. Column (1) shows that an increase of 0.01 in the relative supply of young college graduates, or 59% of the 1990 level, is associated with a 0.21 percentage point increase in the share of college graduates for the older birth cohorts (or by 11%). In terms of the total number of older college graduates, it corresponds to 1.46 thousand college graduates as shown in column (2) (or by 13%). Column (2) also shows that the mean population between 1960 and 1969 is associated with the increase in the number of college graduates over time. Columns (3) and (4) show the OLS results with a population weight for each birth cohort. The estimates are somewhat larger but the 95% confidence intervals overlap with those in columns (1) and (2).

Table 7 shows the 2SLS estimates of the coefficients on the relative supply of young college graduates

[22]The reason that the results in (4) and (5) have opposite signs is that Guizhou is an outlier in terms of the percentage increase in the number of young college graduates. Guizhou had a very low initial stock, and thus a small increase in the number of young college graduates becomes a large increase on a logarithmic scale. Guizhou implemented the policies very late, which drives the result in column (5) to be positive. If we exclude Guizhou, the results become -13.91 and -0.039 for columns (4) and (5) respectively.

[23]The impact of the policies on the share of young college graduates is not shown in Table 5 and available upon request.

for the share and number of older college graduates. According to columns (1) and (2), for a cohort born between 1941 and 1963, an increase of 0.01 in the relative supply of young college graduates increases the share of older college graduates in a cohort by about 0.24 percentage points (or by 13%) and increases the total number of college graduates by 1.34 thousand people (or by 12%). Both estimates are significantly different from zero. Column (3) and (4) show the 2SLS results with weighting: an increase of 0.01 in the relative supply of young college graduates increases the chance of getting a college degree by 0.25 percentage points for an older individual. The relative supply of young college graduates also increases the number of older college graduates that each individual faces in one's birth cohort. Again, the results with population weights are similar to the results without the weights.

6.3 Robustness to Other Macroeconomic Conditions

Our identification assumption is that the timing of the LLF policies is uncorrelated with any time-varying unobserved components that might contribute to upgrading. In this subsection, we examine whether our results are robust to other macroeconomic factors that might affect adult upgrading and are correlated with the timing of the policies. For example, faster economic growth is usually accompanied by capital investment, which could also increase the returns to college education. To do so, we re-run our 2SLS specification, equation (5), adding interactions between the year 2000 indicator variable and additional macroeconomic conditions. We use initial conditions, such as initial GDP and capital investment, rather than contemporaneous conditions, since the latter could be affected by adult upgrading. All the initial conditions are the mean values between 1960 and 1969. We group the initial conditions into five different channels by which they might affect the upgrading differently. First, we focus our robustness checks on adding each channel separately. We then apply factor analysis to combine all the channels together to check robustness.

In column (1) of Table 8, we control for log GDP and the log of fixed assets, which could affect returns to human capital investment (Becker 2009). In column (2) we control for the population density, in order to account for any agglomeration effects on the education upgrading of older adults. In column (3) we control for provincial healthcare endowments using the log number of doctors and hospital beds. These factors might affect the health of older adults and thus the returns to human capital investment. In column (4) we control for the log number of tertiary teachers and secondary schools, potential supply-side factors affecting

the initial stock of college and high school graduates.[24] Provinces with more tertiary teachers may better facilitate the continuing college education for working adults. Alternatively, people in provinces with more high school resources might be better able to attain a college education. In column (5), we control for the distance from the center of the province to the nearest coast. This factor captures potential effects from trade and foreign direct investment. We also re-run all the specifications in Table 8 changing the dependent variable to college share, rather than count, and show the results in Table A3. Additionally, we run population weighted versions of all specifications for both dependent variables, showing the results for count in Table A2 and for share in Table A4.

In the next step, we want to control for the initial conditions mentioned above all at once. Since they are highly correlated with each other, we use factor analysis to extract the main variation in all the initial conditions. In Table A5, we use the first and second principle components of all the initial conditions and re-run the specifications for the share and number of college graduates with and without population weights. Summarizing these robustness checks, the results show that the effect of the relative supply of young college graduates on the education upgrading of older adults is robust to these additional channels.

6.4 Heterogeneous Effects

In this subsection, we assess whether the effect of the relative supply of young college graduates on the education upgrading of older adults is the same across birth cohorts. The elasticity of the supply curve of older college workers may not be the same across birth cohorts. Older cohorts may be less likely to upgrade their education, either because they are closer to the age of retirement, or because the difficulty of completing a college degree increases with age.[25] We re-estimate equation (5) but now divide the older cohorts into five different groups and allow the coefficient on $YC\_relativeColl_{pt}$ to vary. We run separate regressions by age, using the age of adults in 1990, and show the results in Table 9. The effects decrease with age. However, the coefficients are not statistically different from each other, given that the 95% confidence intervals from column (1) to column (5) overlap with each other. We redo the regressions weighting by birth cohort size and find similar results. We also run the regressions by gender and show the results in Table A6 and Table

[24] We also tried the log number of secondary teachers, but it caused a weak instrument issue. We control for the log number of the secondary schools instead.

[25] The retirement age is 50 for women (55 for female cadres) and 60 for men.

A7. In Table A6 the point estimates for men suggest that the peak effect of the upgrading appears in the birth cohort who are 30-34 years old in 1990. For women, in Table A7, the effects decrease with age.

The main findings in section 6 suggest that the early adoption of the LLF policies increases the relative supply of young college graduates and that the relative supply has a positive causal effect on the number of college graduates in an older birth cohort. This effect is robust to the initial conditions. The point estimates of the upgrading effects by birth cohort group decrease with age, however, given the 95% confidence intervals, they are not significantly different from each other.

7 Mechanisms and Alternative Hypotheses

The previous section established a causal link between the relative supply of young college graduates and the number of older college graduates within a birth cohort. Our preferred explanation for this finding is that an increase in the number of young college workers raised the productivity of older college workers due to imperfect substitutability across college workers of different ages, thus encouraging older non-college workers to upgrade their education. In this section we consider several alternative explanations for our findings. We first investigate whether the observed increase in the number of older college graduates results from the education upgrading of local non-college residents (our hypothesis), or stems from an in-migration of college graduates from other provinces. Next we evaluate the hypothesis that the LLF policies increased the number of older college graduates by reducing the number of children in the household, thus making it easier for parents to upgrade their education, rather than through a change in productivity. Lastly, we examine whether the policies had a direct effect on the college attainment of older adults, and whether they changed the baseline secondary education level of these adults as well.

7.1 Mechanism: Migration or Upgrading

The theory of imperfect substitutability between college workers of different age predicts the increased productivity of older college workers. This will drive older adults to go back to school to acquire a college education, but will also drive college labor to move across regions to reap the productivity gain. If the relative supply of young college labor affects both native non-college labor and migrant college labor, we

would expect the effect of the relative supply of young college graduates on the number of migrant college graduates to be non-zero. To test this empirical hypothesis, we use the following specification:

$$MigC_{jpt} = \eta YC\_relativeColl_{pt} + \sum_{p} \beta_{1p} Province_p + \gamma_1 (Initial_p \times Year_{2000}) + \delta_1 Year_{2000} + \sum_{j} \mu_{1j} Z_j + v_{jpt} \tag{8}$$

where $MigC_{jpt}$ is the total number of college-educated migrants in birth cohort j, province p, and year t. Migrants are defined as those who are from a different province five years ago, while natives are those who remain in the same province as five years ago. The main identified impact is from the variable $YC\_relativeColl_{pt}$, which is the relative supply of young college graduates. The other variables on the right hand side are the same as those in equation (5). Our first-stage equation is the same as equation (6).

The result in column (1) of Table 10 is the previously estimated impact of the relative supply of young college graduates on the number of older college graduates, which is decomposed into the impacts on natives and migrants, separately reported in columns (2) and (3). In column (2) we find that an increase of 0.01 in the relative supply of young college graduates increases the number of older native college graduates by 1.33 thousand people. However, in column (3), an increase of 0.01 only increases the number of older migrant college graduates by 15 people, and this impact is not significantly different from zero. These results reject the hypothesis that the stock of young skilled labor will bring more skilled labor from other provinces. Columns (4), (5) and (6) show that the relative supply of young college graduates has no significant effect on the total, native and migrant population of an older birth cohort. The results suggest the impact of the relative supply of young college graduates on the number of older college graduates within a birth cohort is mostly driven by the upgrading of local unskilled labor.

7.2 Alternative Hypotheses

We test three alternative hypotheses and summarize the results here, with detailed explanations in Appendix A.2-A.4. First, we investigate whether the LLF reduced the number of children at home, which further affected the chance of getting a college education for adults. We study the birth cohorts 1941-1955 who by 1990 have given birth to all of their children. Simply adding the number of children to the existing

specification is problematic because the number of children could be endogenous to the decision to upgrade one's education. Instead, we use the fertility fine during the one-child policy as an additional source of variation in the relative supply of young college graduates, which is unlikely to affect the number of children at home for these adults (specification in Appendix A.2). The results show that family size does not have a great impact on the education upgrading of these adults and the effect of the relative supply of young college graduates remains the same.

Next we test whether the LLF policies had a direct impact on the education upgrading of older cohorts rather than through a spillover effect from the younger generation. We examine the education upgrading in the period 1982-1990, when the exposure to the LLF policies of older cohorts varied by province but the relative supply of young college graduates was the same across provinces. The detailed specification is in Appendix A.3 and the results show that the policies do not directly affect these older cohorts.

A third hypothesis is that the baseline level of secondary education is different across provinces, thus provinces with more high school graduates will have more individuals who upgrade to a college education. We check this by regressing the share and total number of older high school graduates in each birth cohort on the relative supply of young college graduates. The results shown in Appendix A.4 suggest that the baseline level of secondary education does not differ across provinces.

8 Wage Changes for Older Adults

The incentive for older adults to upgrade their education is the improved productivity of older college workers resulting from an increase in the relative supply of young college graduates. In this section, we provide some supportive evidence for this demand-side hypothesis and try to show that the LLF policies increased the wage differences between college and non-college graduates for older adults. Supply-side factors, such as the supply of adult education schools and/or general educational resources, are less likely to cause the college wage premium for older adults to increase. Additionally, this increased college wage premium is consistent with the timing when young college workers entered the labor market, which suggests that other macroeconomic confounding factors are unlikely to match with the timing and cause the increase in the college wage premium.

We pool the annual and cross-sectional data from the China Urban Household Survey (UHS) to look at wage changes. This survey samples individuals who are representative of urban residents in 17 provinces in China.[26] The individuals in the survey report their basic demographics, incomes, expenditures, and other non-cash transfers. The income data include detailed information on wage, interest income, pensions, transfers, and other income related to household production. We use the wage income to examine any changes in the wage differentials over time across provinces that are related to the LLF policies.

Panel A of Table 11 lists the summary statistics of wage and demographic variables at the cohort, province and year level from the UHS sample. We select the years from 1990 to 2000. To be consistent with the analysis in the previous sections, we exclude Beijing and Shanghai. This leads to 15 provinces selected in our sample.[27] We keep in our sample the individuals who participate in the labor force at the time of the survey.[28] The employment rate is high in our sample, which is about 97% at the cohort, province and year level. The average annual wage for college and non-college graduates is 7,342 yuan and 5,225 yuan respectively.

Panel B of Table 11 shows the comparison between two data sources: UHS and censuses. According to the UHS sample, the share of each birth cohort with a college degree increases from 0.126 in 1990 to 0.218 in 2000. When we select the census samples based on the same characteristics, i.e., living in cities, having a non-agricultural hukou and participating in the labor force at the time of the survey, the change in the share of college graduates is very similar. It increases from 0.108 in 1990 to 0.178 in 2000. The change in the census samples is slightly smaller than that in the UHS sample.[29]

Following the model in the paper by Card and Lemieux (2001), we use the log ratio of the average wage of college to non-college graduates as the measurement for the college premium. The econometric

[26]The 17 provinces are Anhui, Beijing, Gansu, Guangdong, Heilongjiang, Henan, Hubei, Jiangsu, Jiangxi, Liaoning, Shaanxi, Shandong, Shanghai, Shanxi, Sichuan, Yunnan, and Zhejiang.

[27]The province is the current residential province. The UHS does not have a migration history so we ignore the migration process across provinces.

[28]UHS has detailed information on occupation. We include everyone with a specific occupation in the sample. The unemployed individuals included in our sample are: (1) housekeepers; (2) those who are currently unemployed but want to find a job (daiye renyuan). Housekeepers are a relatively small group. Excluding the housekeepers in our sample doesn't change the regression results. Retired personnel, people with disabilities, students, and other non-participants are not in our sample. The average annual wage of the unemployed individuals is 363 yuan.

[29]The share of each birth cohort with a college degree calculated from the census sample is not precisely 0.108 in year 1990. It is for birth cohorts who live in cities in 1985. The 1990 census does not report the residential type in 1990, but in 1985. The precise number of the share with a college degree for urban residents in 1990 could be lower than 0.108, which makes the changes from two data sources even closer.

specification is the following:

$$
\begin{aligned}
ln(wage\_ratio)_{jpt} = a \times Post_{pt} + \sum_{p} b_p Province_p + \sum_{t} c_t Year_t + \sum_{j} d_j Cohort_j \\
+ \sum_{j,p} e_{jp} Cohort_j \times Province_p + \sum_{j,t} f_{jt} Cohort_j \times Year_t \\
+ \sum_{t} g_t Initial_p \times Year_t + v_{jpt}
\end{aligned} \tag{9}
$$

where $ln(wage\_ratio)_{jpt}$ is the log ratio of the average wage of college to non-college graduates for birth cohort j in province p at time t. The variable $Post_{pt}$ means that the older adults in province p are exposed to the relative supply shock of young college graduates at the time t. We define the post-exposure time as 23 years after the young birth cohorts were born under the LLF policies or 24 years after the policy initiation time.[30] Our estimation strategy differs from that of equation (6) because the UHS data is yearly, whereas the census data is decadal (1990 or 2000). For this reason the t subscript in $Post_{pt}$ now ranges from 1990 to 2000, rather than just taking the values of 1990 or 2000.

The coefficient a measures the effect on the college premium for older adults after the exposure to the shock of young college graduates. In the specification, we include province fixed effects $Province_p$'s, year fixed effects $Year_t$'s and birth cohort fixed effects $Cohort_j$'s. Due to the small sample size, we divide older adults into three birth cohorts: 1941-48, 1949-56 and 1957-63. We also control for cohort interacted with province indicators and cohort interacted with year indicators. These fixed effects and the interaction terms allow for a wide range of factors that might affect the college premium at the cohort, province and year level. For example, these fixed effects capture any differences in the college premium that are constant at the province, year or cohort level. The interaction terms capture any policies or factors that vary by province and/or year and that might have different effects even on the same cohort. They also capture differences in characteristics at the cohort level which might affect the college premium. Further, we control for the effects of the initial conditions on the college premium. We include the interaction terms of the initial conditions and a linear year trend. These terms capture any effects on the college premium from the macroeconomic environment, such as the GDP level, the investment stock, population level and density, education resources,

[30] In the following empirical analysis, we also modify the post-exposure time to be between 20 and 26 years after the young birth cohorts were born under the policies.

and medical infrastructure. The error term v_{jpt} is the idiosyncratic component of the college premium at the cohort, province and year level.

Columns (1)-(3) of Table 12 list the estimation results of equation (9) with different sets of controlled variables. The first row is the estimate of the coefficient a. On average, the college wage premium for older cohorts increases by 12-15% in the exposed provinces compared to unexposed provinces when it is 23 years after the young birth cohorts were born under the policies. The effect is robust to the different sets of controlled variables.

Notice that for our results in Table 12, we use 23 years as the cutoff in the definition of the post-exposure to the supply of young college graduates for older adults. However, we do not assume that the 23-year-cutoff is the correct cutoff for defining the post-exposure period ex ante. We examine other cutoffs to determine the exact timing when the college premium for older adults reacts to the supply shock of young college graduates. Figure 4 plots the estimates and 95% confidence intervals of a from using different cutoffs of years in the definition of the post-exposure period. The college premium for older adults rises and is significantly different from zero in the post-exposure period when we use 22, 23 or 24 years as the cutoff. However, for the cutoff of 20, 21, 25 or 26, the estimates of the college premium are closer to zero in magnitude and insignificant as well. These findings suggest that it is when young cohorts graduate from college and enter the labor market that they begin to have a positive impact on the productivity of older college workers, and therefore the college premium for older cohorts rises.

8.1 Alternative Hypothesis: Direct Effect of "Later Longer and Fewer" Policies on Wages and Employment

Similar to the concern in subsection A.3, we want to rule out that the LLF policies had a direct effect on wages and employment instead of through the relative supply of young college labor. We test the effect of the policies on wages and employment during the period of 1986-1993. This is a period before the relative supply of young college labor starts to differ across regions. We use a similar specification as in equation (9), but assume that the policies were initiated 6 years earlier than the true time. We examine whether the college wage premium for older adults is higher 23 years later in the regions with the falsified policies relative to region with no policies. We find no effects of the policies on the college wage premium and employment for

older adults as shown in columns of (1) and (2) of Table A12.

Moreover, we regress the relative wages and ratio of employment of college and non-college adults on the timing of the LLF policies directly. The hypothesis is that if provinces initiated the policies earlier, the cohorts in these provinces are exposed to the policies longer and may experience higher college wage premium and employment compared to provinces that initiated the policies later. The results in columns (3) and (4) of Table A12 show no effects of the policy timing on wages and employment.

We also falsified the timing of the LLF policies to be 4 to 8 years earlier than the actual date, including the 6-year earlier timing used in Table A12. Tables A13 and A14 show the policy effect on the relative wages and ratio of employment respectively. To summarize, we do not find any evidence that the LLF policies had a direct effect on wages and employment.

9 Summary and Conclusion

In this paper, we examine the effect of a change in the relative supply of young college graduates caused by the "Later Longer and Fewer" policies in China on the education upgrading of older adults born before the policies. We show that the LLF policies implemented through the 1970s reduced the population of young people aged between 20 and 26. We find that the LLF policies lead to educational improvement for the young and education upgrading for older adults (birth cohorts 1941-1963). A one percentage point increase in the relative supply of young college graduates increases the number of older college graduates by 1,343 people within a birth cohort over the period 1990-2000. A back-of-the-envelope calculation suggests that implementing the LLF policies one standard deviation earlier accounts for 15% of the national increase in the number of older college graduates.[31] A substantial literature has documented that the returns to education are higher for younger individuals. Learning at an old age may be associated with higher opportunity costs and additional difficulty. Therefore it would have been significantly more efficient if these adults had been able to obtain a college degree earlier in their lives, suggesting that China's college rationing policy in this

[31] One standard deviation in the policy time is 1.95 years. Implementing the LLF policies one standard deviation earlier causes an increase in the relative supply of young college graduates by 0.0083, which leads to 1.11 thousand older adults acquiring a college degree (the coefficient 134.3 from column (2) of Table 7 multiplied by 0.0083). Given that the average increase in the number of older college graduates in a birth cohort is 7.59 thousand in our sample, the increase caused by the early adoption of the LLF policies contributes to 15% of the national increase.

era was costly to society.

Our preferred explanation for the increase in the number of older college graduates is that the "Later Longer and Fewer" policies led to an increase in the relative supply of young college workers, which then increased the productivity of older college workers, providing an incentive for older workers to acquire a college degree. The increase in the number of older college graduates is mostly driven by education upgrading from non-college graduates already in local labor markets and our results are unlikely to be driven by migration. We find that while the LLF policies reduced family size, this reduction cannot explain adult education upgrading. However, we do find that the wage returns to a college degree increase over time and match the timing of the LLF policies. Further, we find that the policies do not have a direct effect on college attainment, wages or employment prospects of these adults. We conclude that the education upgrading in China over this period is consistent with the theory of imperfect substitutability between educated workers of different ages.

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Figure 1: A map of "Later Longer and Fewer" policy timing

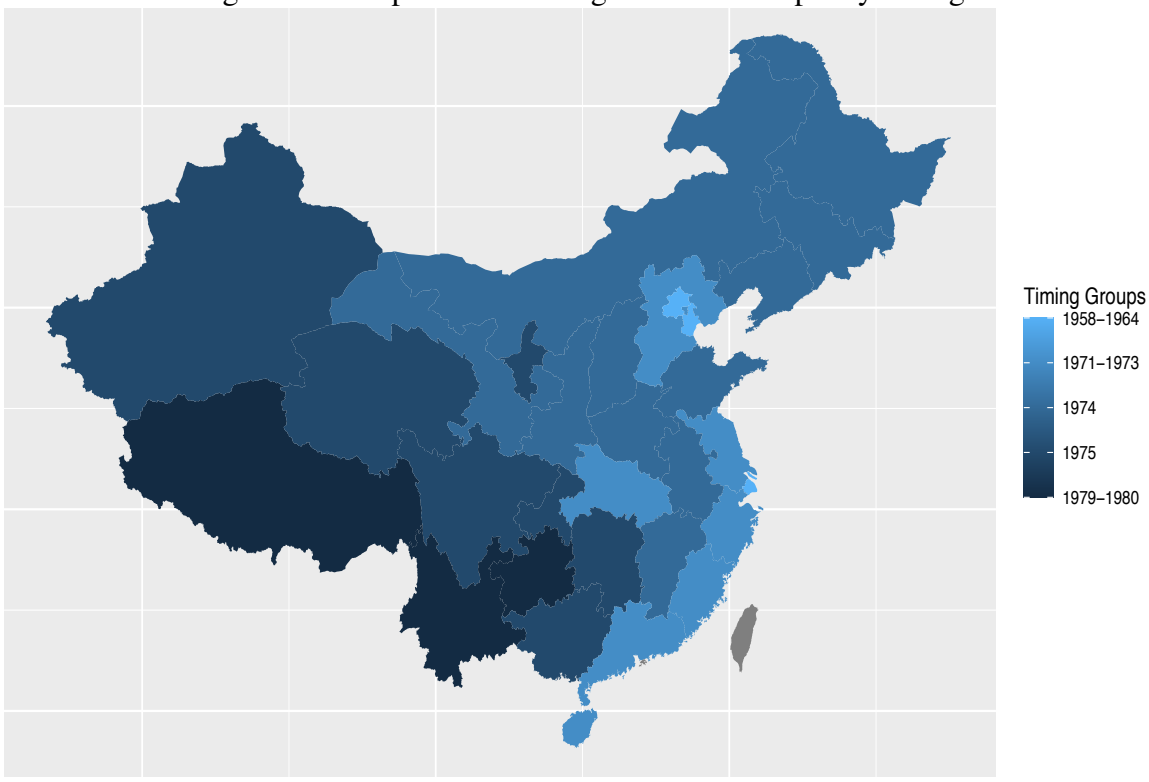
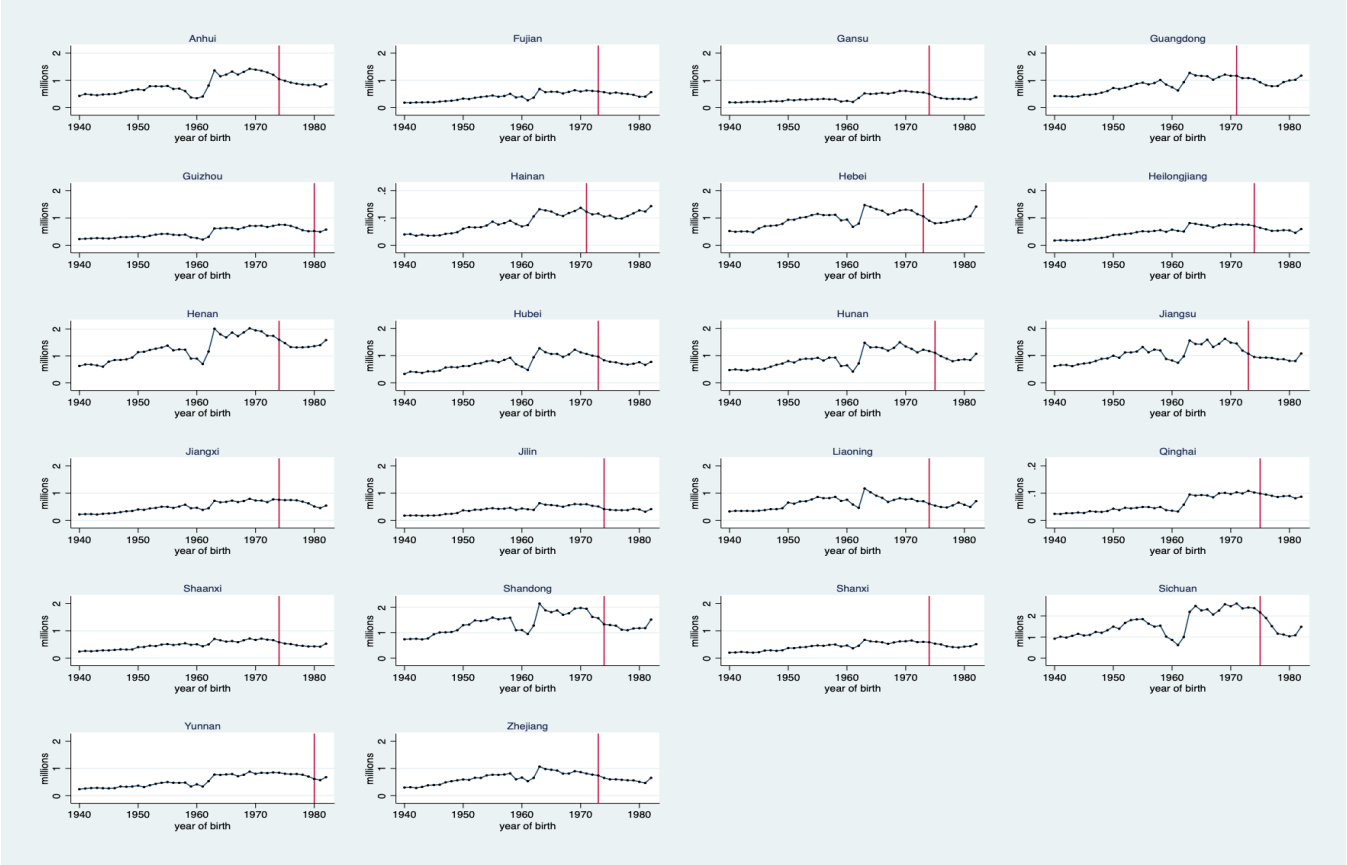
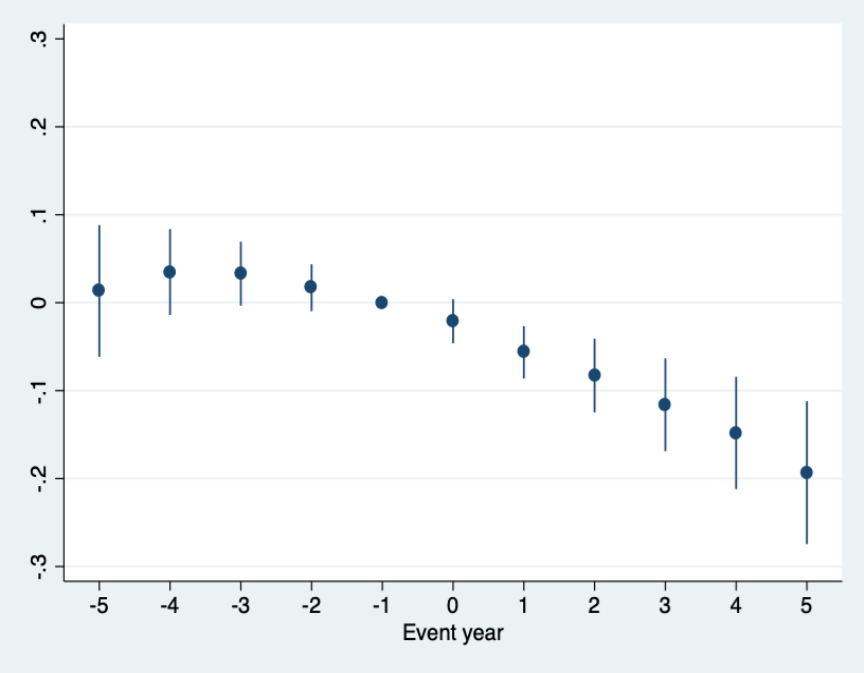


Figure 2: Cohort size by birth province



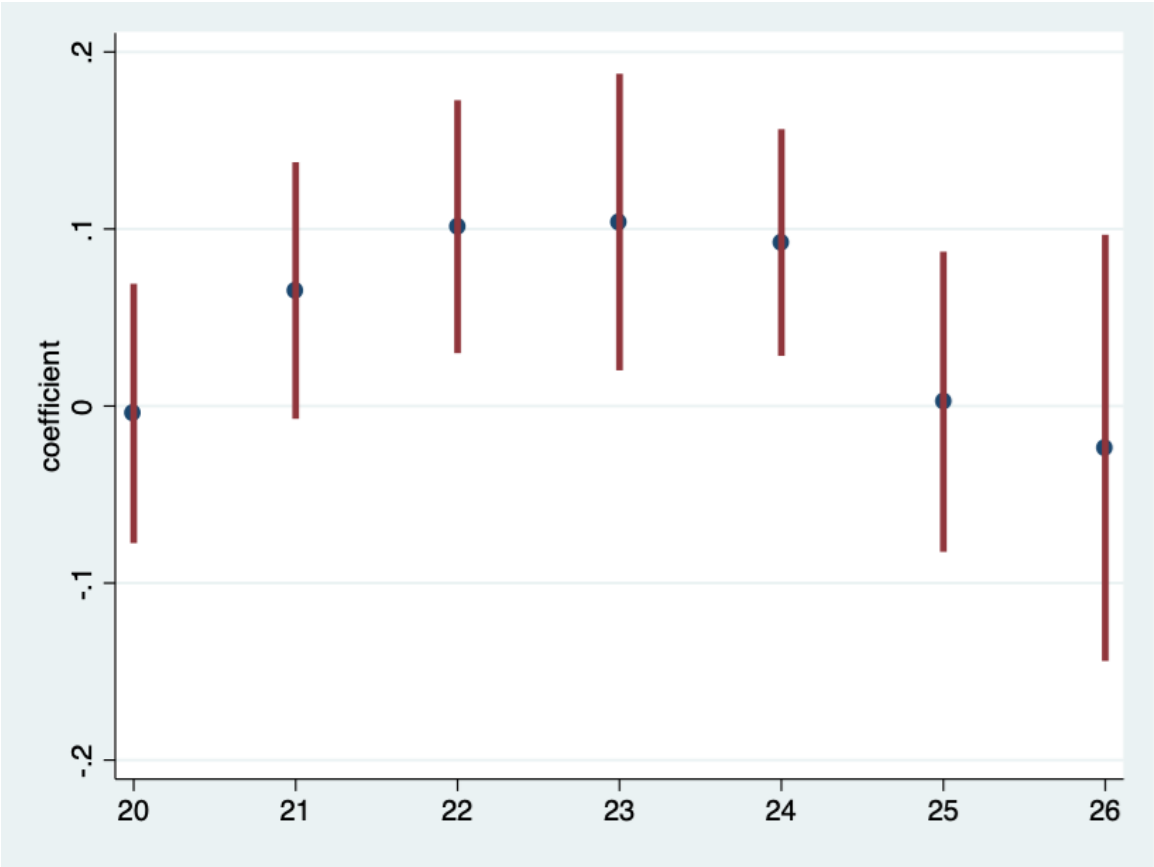
Notes: The red line indicates the first year when a birth cohort was born under the LLF policies. We exclude Beijing, Shanghai, Tianjin, and minority autonomous provinces. Data source: 2000 census.

Figure 3: The changes in log number of young people aged 20-26 by event years



Notes: The vertical axis plots coefficients and 95% confidence intervals from an event study model, with the event being LLF policy. The model controls for province fixed effects, calendar year fixed effects, and the interactions of the initial conditions with calendar year indicators. These initial conditions include the logarithm of real GDP, logarithm of real fixed asset investment, logarithm of population, logarithm of the number of tertiary teachers, logarithm of the number of secondary schools, logarithm of the number of doctors, and population density. We use 2000 census data and event time is defined as 21 years past the LLF implementation year.

Figure 4: Post-exposure college premium using different cutoffs



Notes: The vertical axis plots coefficients and 95% confidence intervals using different years since the LLF implementation as cutoffs in estimating equation (9). Results suggest that age 23 is an appropriate choice for the age of new college graduates to enter the job market.

Table 1: Statistics on the education upgrading of older adults

Year of birth	1961-63	1956-60	1951-55	1946-50	1941-45
Age in 1990	27-29	30-34	35-39	40-44	45-49
Number of college graduates in 1990 (thousands)	1,670	1,662	1,573	1,306	1,180
Number of college graduates in 2000 (thousands)	2,851	3,209	2,666	1,746	1,377
Incease in the number of college graduates from 1990 to 2000 (thousands)	1,181	1,547	1,092	440	197
Percentage increase in the number of college graduates from 1990 to 2000	71%	93%	69%	34%	17%

Notes: Statistics from 1990 and 2000 censuses. Population numbers are actual numbers at the national level, taking into account the sampling rates. All provinces are included.

Table 2: Sample statistics from 1990 and 2000 censuses

	1990	2000	2000 - 1990
Young Cohort			
Age	20-26		
Year of birth	1963-1970	1973-1980	
Population, 1 million	7.57 [4.14]	4.97 [2.56]	-2.60
Log population	15.65 [0.71]	15.25 [0.69]	-0.41
Population with a college degree (thousands)	119.24 [59.95]	279.38 [154.83]	160.15
Log population with a college degree	11.48 [0.79]	12.32 [0.80]	0.84
Population without a college degree (millions)	7.46 [4.10]	4.69 [2.43]	-2.76
Log population without a college degree	15.64 [0.71]	15.19 [0.69]	-0.45
Relative supply of young college graduates	0.017 [0.006]	0.061 [0.021]	0.044
Observations	22		
Older Cohorts			
Age	27-49	37-59	
Year of birth	1941-1963		
College graduate count of each birth cohort (thousands)	11.02 [7.95]	18.61 [15.35]	7.59
Native college graduate count of each birth cohort (thousands)	10.68 [7.70]	18.31 [15.05]	7.63
Migrant college graduate count of each birth cohort (thousands)	0.34 [0.53]	0.30 [0.62]	-0.04
Share of each birth cohort with a college degree	0.019 [0.009]	0.033 [0.015]	0.014
Non-college graduate count of each birth cohort (thousands)	609.1 [382.6]	546.7 [360.2]	-62.4
Observations	506		

Notes: Sample statistics from 1990 and 2000 censuses. Population numbers are actual numbers at the national level, taking into account the sampling rates. We exlude Beijing, Shanghai, Tianjin, and minority autonomous regions. There are 22 provinces in our sample. Standard deviations are shown in brackets.

Table 3: Education of the young and older adults by timing of policies and by year

	1990	2000	Difference
Young Cohort			
Early adoption group of provinces:			
Relative supply of young college graduates (observations: 13)	0.019 [0.004]	0.069 [0.019]	0.050(0.005)
Later adoption group of provinces:			
Relative supply of young college graduates (observations: 9)	0.014 [0.005]	0.048 [0.019]	0.034(0.006)
Difference	0.005 (0.002)	0.021 (0.008)	0.016(0.008)
Older Cohorts			
Early adoption group of provinces:			
College graduate count of each birth cohort (thousands, observations: 299)	12.50 [8.05]	21.42 [15.86]	8.93(1.02)
Later adoption group of provinces:			
College graduate count of each birth cohort (thousands, observations: 207)	8.89 [7.33]	14.54 [13.61]	5.65(1.07)
Difference	3.60 (0.690)	6.88 (1.317)	3.28(1.53)

Notes: Young cohort is defined as 20-26 year-olds in each year. Older cohorts are defined as born between 1941 and 1963. We exclude Beijing, Shanghai, Tianjin, and minority autonomous regions. There are 22 provinces in our sample. Standard deviations are shown in brackets. Standard errors are shown in the parentheses. Sources: 1990 and 2000 census samples. Population numbers are actual numbers, taking into account the sampling rates.

Table 4: Provincial statistics in the 1960's and correlation with LLF timing

	Mean values 1961-69 (n=22)	Correlation coefficient with policy time of LLF policies	P-values (|P|>F)
	(1)	(2)	(3)
Log real GDP (in 1950 price, unit: 100 million yuan)	3.47[0.71]	-0.07	0.40
Log real fixed asset investment (in 1950 price, unit: 100 million yuan)	1.25[0.49]	0.03	0.59
Log number of tertiary teachers	8.11[0.93]	-0.04	0.75
Log number of secondary schools	6.75[1.03]	-0.15	0.10
Log number of primary teachers (unit: 10,000)	2.24[0.86]	-0.06	0.45
Log number of hospital beds (unit: 10,000)	1.25[0.66]	-0.04	0.55
Log number of doctors (unit: 10,000)	0.80[0.79]	-0.05	0.58
Population density (unit: 100 persons per square kilometer)	0.94[0.90]	-0.02	0.38
Population (unit: 10,000)	29.3[17.1]	-0.67	0.60

Notes: We exclude Beijing, Shanghai, Tianjin, and minority autonomous regions. There are 22 provinces in our sample. Standard deviations are shown in the brackets.

Table 5: First-stage results

	(1) Relative supply of young college graduates	(2) Young population in thousands	(3) Log young population	(4) Number young college graduates in thousands	(5) Log number young college graduates
Policy time * Post	-0.00427***	134.7**	0.0392***	-9.762***	0.00697
	(0.000845)	(56.07)	(0.00974)	(3.095)	(0.0300)
Mean population 1960-69 in thousands * Year 2000		-90916.2***		5350.7***	
		(7316.7)		(328.9)	
Mean of log population 1960-69 * Year 2000			-0.0310		0.0550
			(0.0207)		(0.0409)
Observations	1012	1012	1012	1012	1012
Adjusted R^2	0.928	0.986	0.994	0.979	0.985
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Standard errors are clustered at the province-year level. The province level for the young cohort is where they were five years ago.
The relative supply of young college graduates is the ratio of the number of college to non-college graduates.

Table 6: OLS—effect of relative supply of young college graduates on older adults' education upgrading

	(1) Share older cohort with college degree	(2) Number older college graduates in thousands	(3) Share older cohort with college degree	(4) Number older college graduates in thousands
Relative supply of young college graduates: ratio of college to non-college graduates	0.211***	145.9***	0.261***	205.8***
	(0.0408)	(20.65)	(0.0327)	(30.15)
Mean population 1960-69 in thousands * Year 2000		219.2***		256.9***
		(25.83)		(42.38)
Observations	1012	1012	1012	1012
Adjusted R^2	0.797	0.805	0.857	0.824
Province Fixed Effects	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes
Weights	No	No	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990. Standard errors are clustered at the province-year level.
In (3) and (4), weights are each birth cohort's population size.

Table 7: 2SLS—effect of relative supply of young college graduates on older adults' education upgrading

	(1) Share older cohort with college degree	(2) Number older college graduates in thousands	(3) Share older cohort with college degree	(4) Number older college graduates in thousands
Relative supply of young college graduates: ratio of college to non-college graduates	0.241***	134.3***	0.248***	218.2***
	(0.0661)	(41.11)	(0.0481)	(66.44)
Mean population 1960-69 in thousands * Year 2000		221.9***		256.4***
		(26.96)		(41.97)
Observations	1012	1012	1012	1012
Adjusted R^2	0.579	0.370	0.694	0.475
Province Fixed Effects	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes
Weights	No	No	Yes	Yes
IV	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990. Standard errors are clustered at the province-year level.

In (3) and (4), weights are each birth cohort's population size.

The instrumental variable is Policy time * Post.

Table 8: Robustness checks—controlling for initial conditions

	Dependent variable: the number of older college graduates in thousands				
	(1)	(2)	(3)	(4)	(5)
Relative supply of young college graduates: ratio of college to non-college graduates	124.4**	137.7***	140.0***	119.1**	100.0***
	(57.3)	(41.5)	(43.4)	(46.2)	(34.5)
Mean population 1960-69 (in 1000) * Year 2000	218.4***	220.7***	259.5***	226.7***	217.1***
	(42.7)	(25.9)	(51.8)	(44.3)	(24.2)
Mean log real GDP 1960-69 * Year 2000	0.4				
	(1.5)				
Mean log real fixed asset 1960-69 * Year 2000	-0.5				
	(1.2)				
Mean density 1960-69 * Year 2000		-0.6**			
		(0.2)			
Mean log number doctors 1960-69 * Year 2000			-0.7		
			(2.1)		
Mean log number beds 1960-69 * Year 2000			-0.4		
			(2.4)		
Mean log number tertiary teachers 1960-69 * Year 2000				-0.9	
				(0.6)	
Mean log number secondary schools 1960-69 * Year 2000				0.6	
				(0.8)	
Mean distance to coast (in 1000 km) * Year 2000					-2.0**
					(0.9)
Observations	1012	1012	1012	1012	1012
Adjusted R^2	0.368	0.370	0.369	0.369	0.371
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	No
IV	Yes	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990. Standard errors are clustered at the province-year level. The instrumental variable is Policy time * Post.

Table 9: Education upgrading (the number of older college graduates in thousands) by birth cohort

	(1) 27-29 in 1990 Born in 1961-63	(2) 30-34 in 1990 1956-60	(3) 35-39 in 1990 1951-55	(4) 40-44 in 1990 1946-50	(5) 45-49 in 1990 1941-45
Relative supply of young college graduates	206.2***	191.2***	171.0***	84.29**	47.63**
	(73.87)	(52.86)	(48.23)	(40.21)	(22.73)
Mean population 1960-69 in thousands *Year 2000	434.9***	332.3***	226.6***	122.1***	78.53***
	(36.65)	(48.52)	(37.27)	(22.33)	(14.86)
Observations	132	220	220	220	220
Adjusted R^2	0.445	0.833	0.717	0.472	0.392
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	No
IV	Yes	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990. Standard errors are clustered at the province-year level. The instrumental variable is Policy time * Post.
The relative supply of young college graduates is the ratio of the number of college to non-college graduates.

Table 10: Mechanism check—education upgrading or migration

	Number of older college graduates in thousands			Population of an older cohort in thousands		
	(1) Total	(2) Native	(3) Migrant	(4) Total	(5) Native	(6) Migrant
Relative supply of young college graduates	134.3***	132.8***	1.5	-439.0	-582.9	143.9
	(41.1)	(38.1)	(3.5)	(591.9)	(499.7)	(128.7)
Mean population 1960-69 in thousands *Year 2000	221.9***	223.2***	-1.3	-353.5	-328.7	-24.8
	(27.0)	(27.7)	(1.5)	(384.2)	(388.5)	(22.4)
Observations	1012	1012	1012	1012	1012	1012
Adjusted R^2	0.370	0.380	-0.048	0.006	0.006	-0.077
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	No	No
IV	Yes	Yes	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990. Standard errors are clustered at the province-year level. The instrumental variable is Policy time * Post.
The relative supply of young college graduates is the ratio of the number of college to non-college graduates.

Table 11: Sample statistics from the Urban Household Survey and the comparison with census samples

Panel A: Basic statistics for older cohorts (3 birth cohorts: 1941-48, 49-56, 57-63) in the UHS sample

	Mean	[Std. Dev.]
Share of males in each birth cohort	0.52	[0.07]
Average age in each birth cohort	42.43	[6.78]
Share of each birth cohort employed	0.97	[0.04]
Annual wage of college graduates	7342.29	[4651.80]
Annual wage of non-college graduates	5225.29	[2838.44]
Real annual wage of college graduates, in 1990 Yuan	4540.79	[2448.20]
Real annual wage of non-college graduates, in 1990 Yuan	3271.89	[1439.74]
Logarithm of real annual wage of college graduates, in 1990 Yuan	8.24	[0.40]
Logarithm of real annual wage of non-college graduates, in 1990 Yuan	7.64	[0.44]

Observations: N=495 (3 birth cohorts in 15 provinces in 11 years)

Panel B: Changes in college attainment from the Urban Household Survey and censuses

	1990		2000		2000-1990
Older cohorts: age	27-49		37-59		
Older cohorts: year of birth	1941-1963				
	Mean	[Std. Dev.]	Mean	[Std. Dev.]	Mean(Std.Err.)
Share of each older birth cohort with a college degree from the UHS	0.126	[0.033]	0.218	[0.043]	0.093(0.008)
Share of each older birth cohort with a college degree conditional on having a non-agricultural hukou, living in cities and participating in the labor force (1990 and 2000 censuses)	*0.108*	*[0.024]*	*0.178*	*[0.030]*	*0.071(0.006)*
Observations: N=45					

Notes: In Panel A, we provide the basic statistics for older cohorts in the UHS sample. There are 15 provinces in the sample. In Panel B, we compare the level (in 1990 and 2000) and change in the share of college graduates within province and birth cohort between UHS and censuses.

Table 12: The effect of the "Later Longer and Fewer" policies on wages

	Dependent variable: log ratio of wages		
	(1)	(2)	(3)
Post	0.123***	0.123***	0.146***
	(0.043)	(0.046)	(0.050)
Observations	495	495	495
AdjR2	0.69	0.82	0.82
Province Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Cohort Fixed Effects	Yes	Yes	Yes
Cohort * Province dummies	No	Yes	Yes
Cohort * Year dummies	No	Yes	Yes
Initial conditions * Year trend	No	No	Yes

Notes: UHS data. Observations exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. We have 15 provinces in our sample. We include the years between 1990 and 2000. We select individuals who were born between 1941 and 1963 and participate in the labor force. We divide them into three birth cohorts: 1941-48, 1949-56, and 1957-63. Standard errors are clustered at the province-year level. The variable post indicates that it is 23 years after the first birth cohort was born under the "Later Longer and Fewer" policies, which is roughly 24 years after the initiation of the LLF policies.

A Appendix: For Online Publication

A.1 Additional Exhibits for Policy Timing, Robustness, and Heterogeneity Results

Table A1: Timing of the "Later Longer and Fewer" policies

Province	(1) Policy year	(2) Policy month	(3) Birth year of the earliest cohort	(4) Birth month of the earliest cohort	(5) Age of the earliest cohort in June 1990 (year, month)	(6) Age of the earliest cohort in June 2000 (year, month)	(7) Affected age groups among 20-26 yrds in 1990 (year, month)	(8) Affected age groups among 20-26 yrds in 2000 (year, month)	(9) Policy time =(policy year + policy month/12)	(10) Early(=1) or later(=2) adoption group
Beijing	1963	10	1964	7	25,11	35,11	20,0-25,11	All	1963.83	1
Shanghai	1958	1	1958	10	31,8	41,8	All	All	1958.08	1
Tianjin	1962	9	1963	6	27,0	37,0	All	All	1962.75	1
Anhui	1973	10	1974	7	15,11	25,11	None	20,0-25,11	1973.83	2
Fujian	1973	3	1973	12	16,6	26,6	None	20,0-26,6	1973.25	1
Gansu	1973	8	1974	5	16,1	26,1	None	20,0-26,1	1973.67	1
Guangdong	1971	1	1971	10	18,8	28,8	None	All	1971.08	1
Guizhou	1979	6	1980	3	10,3	20,3	None	20,0-20,3	1979.50	2
Hainan	1971	1	1971	10	18,8	28,8	None	All	1971.08	1
Hebei	1973	2	1973	11	16,7	26,7	None	20,0-26,7	1973.17	1
Heilongjiang	1973	8	1974	5	16,1	26,1	None	20,0-26,1	1973.67	1
Henan	1974	1	1974	10	15,8	25,8	None	20,0-25,8	1974.08	2
Hubei	1972	12	1973	9	16,9	26,9	None	20,0-26,9	1973.00	1
Hunan	1974	4	1975	1	15,5	25,5	None	20,0-25,5	1974.33	2
Jiangsu	1973	3	1973	12	16,6	26,6	None	20,0-26,6	1973.25	1
Jiangxi	1973	7	1974	4	16,2	26,2	None	20,0-26,2	1973.58	1
Jilin	1974	1	1974	10	15,8	25,8	None	20,0-25,8	1974.08	2
Liaoning	1973	5	1974	2	16,4	26,4	None	20,0-26,4	1973.42	1
Qinghai	1974	7	1975	4	15,2	25,2	None	20,0-25,2	1974.58	2
Shaanxi	1973	8	1974	5	16,1	26,1	None	20,0-26,1	1973.67	1
Shandong	1973	8	1974	5	16,1	26,1	None	20,0-26,1	1973.67	1
Shanxi	1973	6	1974	3	16,3	26,3	None	20,0-26,3	1973.50	1
Sichuan	1974	6	1975	3	15,3	25,3	None	20,0-25,3	1974.50	2
Yunnan	1979	7	1980	4	10,2	20,2	None	20,0-20,2	1979.58	2
Zhejiang	1973	3	1973	12	16,6	26,6	None	20,0-26,6	1973.25	1
Guangxi	1975	2	1975	11	14,7	24,7	None	20,0-24,7	1975.17	2
Inner Mongolia	1973	4	1974	1	16,5	26,5	None	20,0-26,5	1973.33	1
Ningxia	1975	1	1975	10	14,8	24,8	None	20,0-24,8	1975.08	2
Tibet	1980	1	1980	10	9,8	19,8	None	None	1980.08	2
Xinjiang	1975	3	1975	12	14,6	24,6	None	20,0-24,6	1975.25	2

Notes: Column 10 defines the early and later adoption group using the median time of the policy implementation as a cutoff. Data sources: Peng (1997), provincial chronicles, provincial population chronicles, provincial geographic chronicles, and China population chronicles: provincial volumes. Chongqing is included in Sichuan province.

Table A2: Robustness checks—controlling for the initial conditions (weighted)

	Number of older college graduates in thousands				
	(1)	(2)	(3)	(4)	(5)
Relative supply of young college graduates: ratio of college to non-college graduates	182.7*	222.4***	285.8**	251.8*	111.9**
	(105.6)	(66.4)	(108.0)	(139.8)	(51.3)
Mean population 1960-69 (in 1000) * Year 2000	229.2***	253.2***	389.4***	285.8***	275.9***
	(58.6)	(40.1)	(110.2)	(90.0)	(29.1)
Mean log real GDP 1960-69 * Year 2000	1.7				
	(2.6)				
Mean log real fixed asset 1960-69 * Year 2000	-0.4				
	(2.1)				
Mean density 1960-69 * Year 2000		-0.7			
		(0.4)			
Mean log number doctors 1960-69 * Year 2000			-5.5		
			(5.7)		
Mean log number beds 1960-69 * Year 2000			0.3		
			(3.9)		
Mean log number tertiary teachers 1960-69 * Year 2000				-4.0	
				(2.6)	
Mean log number secondary schools 1960-69 * Year 2000				1.2	
				(1.7)	
Mean distance to coast (in 1000 km) * Year 2000					-6.2***
					(1.6)
Observations	1012	1012	1012	1012	1012
Adjusted R^2	0.474	0.475	0.475	0.482	0.484
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes
IV	Yes	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990. Standard errors are clustered at the province-year level. The weights are each birth cohort's population size. The instrumental variable is Policy time * Post.

Table A3: Robustness checks—controlling for the initial conditions (shares)

	Share of an older cohort with a college degree				
	(1)	(2)	(3)	(4)	(5)
Relative supply of young college graduates: ratio of college to non-college graduates	0.318***	0.269***	0.283***	0.295***	0.278***
	(0.081)	(0.053)	(0.056)	(0.056)	(0.068)
Mean population 1960-69 (in 1000) * Year 2000	-0.009	-0.143***	-0.001	-0.020	-0.139***
	(0.043)	(0.048)	(0.066)	(0.073)	(0.047)
Mean log real GDP 1960-69 * Year 2000	-0.005				
	(0.003)				
Mean log real fixed asset 1960-69 * Year 2000	0.0004				
	(0.003)				
Mean density 1960-69 * Year 2000		-0.001**			
		(0.000)			
Mean log number doctors 1960-69 * Year 2000			-0.006*		
			(0.003)		
Mean log number beds 1960-69 * Year 2000			0.002		
			(0.004)		
Mean log number tertiary teachers 1960-69 * Year 2000				-0.003***	
				(0.001)	
Mean log number secondary schools 1960-69 * Year 2000				-0.001	
				(0.001)	
Mean distance to coast (in 1000 km) * Year 2000					0.001
					(0.002)
Observations	1012	1012	1012	1012	1012
Adjusted R^2	0.598	0.593	0.599	0.603	0.592
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	No
IV	Yes	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990. Standard errors are clustered at the province-year level. The instrumental variable is Policy time * Post.

Table A4: Robustness checks—control for the initial conditions (shares, weighted)

	Share of an older cohort with a college degree				
	(1)	(2)	(3)	(4)	(5)
Relative supply of young college graduates: ratio of college to non-college graduates	0.340***	0.265***	0.266***	0.297**	0.227***
	(0.108)	(0.045)	(0.090)	(0.119)	(0.052)
Mean population 1960-69 (in 1000) * Year 2000	-0.061	-0.072***	-0.079	-0.033	-0.062**
	(0.042)	(0.026)	(0.105)	(0.098)	(0.030)
Mean log real GDP 1960-69 * Year 2000	-0.003				
	(0.003)				
Mean log real fixed asset 1960-69 * Year 2000	0.004*				
	(0.002)				
Mean density 1960-69 * Year 2000		-0.001			
		(0.001)			
Mean log number doctors 1960-69 * Year 2000			-0.003		
			(0.006)		
Mean log number beds 1960-69 * Year 2000			0.003		
			(0.004)		
Mean log number tertiary teachers 1960-69 * Year 2000				-0.002	
				(0.002)	
Mean log number secondary schools 1960-69 * Year 2000				-0.00003	
				(0.002)	
Mean distance to coast (in 1000 km) * Year 2000					-0.002
					(0.002)
Observations	1012	1012	1012	1012	1012
Adjusted R^2	0.698	0.698	0.698	0.699	0.697
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes	Yes
IV	Yes	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990. Standard errors are clustered at the province-year level. The weights are each birth cohort's population size. The instrumental variable is Policy time * Post.

Table A5: Robustness checks—control for the principle components of the initial conditions

	(1) Share older cohort with college degree	(2) Number older college graduates in thousands	(3) Share older cohort with college degree	(4) Number older college graduates in thousands
Relative supply of young college graduates: ratio of college to non-college graduates	0.303***	136.305***	0.298***	125.806*
	(0.046)	(49.565)	(0.062)	(68.781)
Observations	1012	1012	1012	1012
Adjusted R^2	0.600	0.346	0.696	0.462
Province Fixed Effects	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes
Principle Componets * Year 2000	Yes	Yes	Yes	Yes
Weights	No	No	Yes	Yes
IV	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin.
Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990.
Standard errors are clustered at the province-year level.
We control for the principle components of the macroeconomic conditions interacted with the year 2000 dummy.
In (3) and (4), weights are each birth cohort's population size.
The instrumental variable is Policy time * Post.

Table A6: Education upgrading (the number of older college graduates in thousands) by birth cohort (men only)

	(1) 27-29 in 1990 Born in 1961-63	(2) 30-34 in 1990 1956-60	(3) 35-39 in 1990 1951-55	(4) 40-44 in 1990 1946-50	(5) 45-49 in 1990 1941-45
Relative supply of young college graduates	115.5**	140.3***	123.2***	60.19*	32.46 †
	(50.12)	(39.34)	(39.86)	(31.86)	(21.26)
Mean population 1960-69 in thousands *Year 2000	306.0***	230.6***	166.2***	99.49***	60.80***
	(22.35)	(33.30)	(28.35)	(17.27)	(11.12)
Observations	132	220	220	220	220
Adjusted R^2	0.437	0.817	0.717	0.451	0.325
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	No
IV	Yes	Yes	Yes	Yes	Yes

Notes: Men observations only.
1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin.
Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990.
Standard errors are clustered at the province-year level. The instrumental variable is Policy time * Post.
The relative supply of young college graduates is the ratio of the number of college to non-college graduates.
†: significant at the 15% level

Table A7: Education upgrading (the number of older college graduates in thousands) by birth cohort (women only)

	(1) 27-29 in 1990 Born in 1961-63	(2) 30-34 in 1990 1956-60	(3) 35-39 in 1990 1951-55	(4) 40-44 in 1990 1946-50	(5) 45-49 in 1990 1941-45
Relative supply of young college graduates	90.74***	50.91***	47.72***	24.10***	15.18*
	(26.01)	(15.60)	(10.18)	(8.844)	(7.947)
Mean population 1960-69 in thousands *Year 2000	128.9***	101.8***	60.38***	22.62***	17.73**
	(17.48)	(17.02)	(10.43)	(5.700)	(6.649)
Observations	132	220	220	220	220
Adjusted R^2	0.408	0.757	0.582	0.259	0.119
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	No
IV	Yes	Yes	Yes	Yes	Yes

Notes: Women observations only.

1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin.

Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990.

Standard errors are clustered at the province-year level. The instrumental variable is Policy time * Post.

The relative supply of young college graduates is the ratio of the number of college to non-college graduates.

A.2 Alternative Hypothesis: Number of Children on Education Upgrading

One potential explanation for the observed upgrading shown in Table 9 is that a reduction in the number of children caused by the LLF policies had a direct effect effect on the decision to upgrade one's education for adults born in 1941-55, those who are above 35 years old in 1990.[32] Fewer children at home could relax the budget constraint and increase parental time, allowing parents to re-invest in their human capital. On the other hand, fewer children to support could reduce the parental incentive to earn a higher degree, and therefore a higher income, when an economic opportunity turns up. What's more, a fertility decline may induce a family structural change which leads to less coresidence with grandparents. The absence of childcare and household chores provided by grandparents could shift the parental time away from the labor market activities (Guo, Li, Yi, and Zhang (2018), Maurer-Fazio, Connelly, Chen, and Tang (2011)). Thus, the impact of the number of children on education upgrading is uncertain.

We now re-run our main specification (5) while controlling for the average number of children at the birth-cohort-province level. Specifically, we add $Children_{jp} \times Year_{2000}$ to equation (5), where $Children_{jp}$ is the average number of children of older adults who were born in cohort j and from province p. The 1990 and 2000 censuses collect the information on the number of surviving children reported by adult women. So for the female adult sample, we use this information and aggregate to the birth-cohort-province level. For the male sample, we first find all the wife-husband matches, and use the information on the wives to construct the average number of children of men. We only focus on women and men who are above 35 years old in 1990.[33] We assume they have finished childbearing before education upgrading.

Simply controlling for another term, $Children_{jp} \times Year_{2000}$, in equation (5) could lead to a biased estimate of the effect of the average number of children on upgrading. One concern is that the number of children is likely endogenous in the older adults' human capital investment decision. Parents who particularly value education may be more likely to upgrade and have fewer children. Therefore, at the cohort and province level, it is possible that the average number of children is correlated with the upgrading we observe.

[32] Young people aged 20-26 in 2000 were mostly born during the "Later Longer and Fewer" program, or even caught up in the early trend of the one-child policy, which started in 1979. Assuming that the primary age for childbirth is 22-35, their mothers are from the birth cohort 1939-1958.

[33] 1990 and 2000 censuses report the number of surviving children of women age 15-50 years old. Therefore, for the women age group 40-49, we use the surviving count in the 1990 census, since they are not asked in the 2000 census. For the women age group 35-39, we use the 2000 census information. We also tried to use only the 1990 census information, the results remain the same.

To identify two endogenous variables in our specification, we need two instrumental variables. Our current IV—the policy time of the "Later Longer and Fewer" policies—affects both the relative supply of young college graduates and the average number of children (shown below), and therefore we need another IV that affects either of these endogenous variables. Following the literature, we use the out-of-quota birth fines implemented by the one-child policy to generate an additional source of exogenous variation in the educational attainment of young people.[34] Ebenstein (2010) shows that the fertility fines reduce the number of children at home when the first birth is a girl; these fines could also affect the educational attainment of children through the quantity-quality trade-off. Huang, Lei, and Sun (2016) show that the fertility fines that a girl was exposed to during teenage years affect her subsequent educational attainment due to a lower cost of further education, as well as the expectations of taking care of fewer children, a more career-oriented life, and being less dependent on children.

Columns (1) and (2) of Table A8 show our first-stage results for the women's sample. The two IV's are the policy time of the "Later Longer and Fewer" policies interacted with the post policy indicator, and the fertility fine of the one-child policy in 1986 interacted with the year 2000 indicator.[35] In column (1), for every one year earlier of the policy initiation time, the relative supply of young college graduates aged 20 to 26 increases by 0.003 from 1990 to 2000, which is smaller than the effect shown in Table 5. This is because the policy time of the "Later Longer and Fewer" policies is correlated with the fertility fine in 1986, and some of its effect is absorbed into the effect of the latter. Conditional on the policy time, the fertility fine has a positive effect on the relative supply: a fine equivalent to an annual income increases the relative supply by 0.013, and the effect is significant.[36] The joint significance level of the two IV's is 1%. For the other endogenous variable, column (2) shows that for every one year earlier of the policy initiation time, the average number of children is reduced by 0.06 (or by 2 percent). The fertility fine in 1986 has no impact on the average number of children for these women; this finding is expected since they have mostly passed their childbearing age by 1986. To summarize, the fertility fine in 1986 causes an additional variation in the relative supply of young college graduates that these women experience, beyond the impact from the "Later

[34] Later in the empirical result, we show that the fertility fines do not affect the average number of children.

[35] We use the fine in 1986 in our second IV because 1986 is the first year after 1979 that has the greatest variation in fines across provinces. Fines from other years are too weak to use as IV's.

[36] The 20-21 year-olds in 2000 were born under the one-child policy and faced the fertility fine in 1986. Their college attainment could be affected through a quantity-quality trade-off. The 22-26 year-olds in 2000 could be affected by the same mechanism, or the mechanism of the exposure to strict fines in their teenage-hood (Huang, Lei, and Sun 2016).

Longer and Fewer" policies. However, the fine does not affect the number of children these women have.

In column (3) of Table A8 we regress upgrading on the relative supply of young college graduates at the province level and the average number of children at the birth-cohort-province level interacted with the year 2000 indicator. We find that the relative supply of young college graduates has a positive effect on women's upgrading for the cohorts born in 1941-1955 (age 35-49 in 1990). The effects are not statistically different from those in Table A7, which do not control for the effect of children. We do not find any evidence that decreasing family size increases upgrading for women; in fact, we find a positive coefficient significant at the 10% level. When we bring autonomous provinces into the sample, in column (4), the positive effect of the average number of children on upgrading is even more significant.

As a check on the effect of the number of children on upgrading we also provide the analysis for men. Although men do not report the number of children they have, we find all wife-husband matches and use the number of the children reported by wives as that of husbands. The results are shown in Table A9 and similar to the results for women (Table A8). These results argue against the hypothesis that reduced fertility within a family led to upgrading and also show that the effect of the relative supply of young college graduates on upgrading is unchanged when conditioning on family size.

A.3 Alternative Hypothesis: Direct Effect of "Later Longer and Fewer" Policies on Education Upgrading

Another possible explanation for the observed upgrading of the older adults is that the LLF policies had a direct effect on these adults rather than through the spillover effect of the younger generation. Similar to the mechanism in Huang, Lei, and Sun (2016), it is possible that adults who were exposed to the LLF policies expect that they will have better prospects in the labor market and invest more in their human capital. Therefore, the subsequent education upgrading of these adults should be greater in regions that initiated the policies earlier and are exposed to the policies longer. It is difficult to test this hypothesis during 1990-2000, because the relative supply of young college graduates increases at the same time and depends on how long the regions are exposed to the policies. Therefore, we evaluate this hypothesis by looking at the education upgrading of adults during 1982-1990, when the exposure to the policies varies by province, but the relative supply of young college graduates stays the same. In section 8, we check whether the relative wages and

employment of older adults differ across regions as a consequence of the exposure to the LLF policies.

We use the following specification:

$$Older\_Coll_{jpt} = \alpha Exposure_{pt} + \sum_{p} \beta_p Province_p + \gamma(Initial_p \times Year_{1990}) + \delta Year_{1990} + \sum_{j} \mu_j Z_j + \epsilon_{jpt} \tag{A1}$$

The variable $Exposure_{pt}$ is years of exposure to the policies as of year t, and it is equal to the number of years from the initiation of the policies to year t. We set $Exposure_{pt}$ to be zero in all provinces in year 1982 because all colleges were shut down during the 1970s in the Cultural Revolution. Colleges reopened in 1977 with very low enrollment rates and favored recent high school graduates. We select older adults age 27-49 in 1982 in our sample. College education was practically inaccessible to these adults in 1982.

Table A10 shows that the exposure to the policies does not affect the share and number of older college graduates during 1982-1990, as shown in columns (1) and (2). When we weight the observations by the birth cohort population the results remain the same (see columns (3) and (4)). From these results we conclude that the exposure to the LLF policies did not affect the education upgrading directly.

A.4 Alternative Hypothesis: Baseline Level of Secondary Education

A third alternative hypothesis for the upgrading of older cohorts is that they reflect the baseline level of secondary education. Specifically, if the probability of getting a college degree is the same for every high school graduate, provinces with more high school graduates will have more individuals who upgrade to a college education. This will lead to a spurious relationship between the relative supply of young college graduates and the education upgrading of older cohorts. To test this hypothesis, we adopt the same 2SLS specification as in equation (5) and regress the share and total number of older high school graduates in each birth cohort on the relative supply of young college graduates. The results shown in Table A11 suggest that the baseline level of secondary education does not differ across provinces.

Table A8: The effect of the relative supply of young college graduates on women's education upgrading controlling for the average number of children (for women age 35-49 in 1990)

	First stage		Second stage	
	(1)	(2)	(3)	(4)
	Relative supply of young college graduates	Average number of children * Year 2000	Number of older college graduates in thousands	Number of older college graduates in thousands
Policy time * Post	-0.00315***	0.0593***		
	(0.000783)	(0.0180)		
Fine in years of income * Year 2000	0.0127***	-0.0326		
	(0.00444)	(0.0996)		
Relative supply of young college graduates			119.3**	81.77***
			(45.48)	(20.22)
Average number of children * Year 2000			6.046*	2.783**
			(3.052)	(1.073)
Mean population 1960-69 in thousands *Year 2000	0.242**	-12.56***	91.53**	67.09***
	(0.0996)	(2.434)	(34.68)	(14.93)
Observations	660	660	660	780
Adjusted R^2	0.919	0.954	-2.374	-0.607
Province Fixed Effects	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes
Weights	No	No	No	No
IV			Yes	Yes
Include autonomous regions	No	No	No	Yes

Notes: Women upgrading, controlling for the avaregae number of children. 1990 and 2000 census data.
We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin in columns (1)-(3). In column (4), we exclude Tibet, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1955. Standard errors are clustered at the province-year level.
The instrumental variables are Policy time * Post, and the fertility fine in 1986 interacted with the year 2000 dummy.
The relative supply of young college graduates is the ratio of the number of college to non-college graduates.

Table A9: The effect of the relative supply of young college graduates on men's education upgrading controlling for the average number of children (for men age 35-49 in 1990)

	First stage		Second stage	
	(1)	(2)	(3)	(4)
	Relative supply of young college graduates	Average number of children * Year 2000	Number of older college graduates in thousands	Number of older college graduates in thousands
Policy time * Post	-0.00315***	0.0750***		
	(0.000783)	(0.0144)		
Fine in years of income * Year 2000	0.0127***	0.0593		
	(0.00444)	(0.0880)		
Relative supply of young college graduates			204.7***	170.0***
			(54.76)	(45.80)
Average number of children * Year 2000			7.811***	4.777**
			(2.667)	(2.024)
Mean population 1960-69 in thousands *Year 2000	0.242**	-10.77***	169.6***	157.9***
	(0.0996)	(2.419)	(32.07)	(29.18)
Observations	660	660	660	780
Adjusted R^2	0.919	0.953	-0.568	-0.125
Province Fixed Effects	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes
Weights	No	No	No	No
IV			Yes	Yes
Include autonomous regions	No	No	No	Yes

Notes: Men upgrading, controlling for the avaregae number of children. 1990 and 2000 census data.
We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin in columns (1)-(3). In column (4), we exclude Tibet, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1955. Standard errors are clustered at the province-year level.
The instrumental variables are Policy time * Post, and the fertility fine in 1986 interacted with the year 2000 dummy.
The relative supply of young college graduates is the ratio of the number of college to non-college graduates.

Table A10: The effect of the exposure to LLF policies on education upgrading in 1982-1990

	(1) Share older cohort with college degree	(2) Number older college graduates in thousands	(3) Share older cohort with college degree	(4) Number older college graduates in thousands
Exposed years by 1990	0.0005	0.0789	0.0005	0.0265
	(0.0004)	(0.0993)	(0.0004)	(0.1580)
Mean log real GDP 1960-69 * Year 1990	0.0026	3.7355***	0.0008	4.4282***
	(0.0019)	(1.0546)	(0.0018)	(1.3684)
Mean log real fixed asset 1960-69 * Year 1990	0.0019	-0.1735	0.0005	0.8508
	(0.0028)	(0.9269)	(0.0028)	(0.8794)
Mean population 1960-69 (in 1000) * Year 1990		-60.5583**		-76.8723**
		(29.1797)		(37.8074)
Observations	1012	1012	1012	1012
Adjusted R^2	0.740	0.781	0.786	0.731
Province Fixed Effects	Yes	Yes	Yes	Yes
Year 1990 Fixed Effect	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes
Weights	No	No	Yes	Yes

Notes: 1982 and 1990 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. Each observation is at the cohort-province-year level. Birth cohorts were born between 1933 and 1955. They are of age 27-49 in 1982. We control for log GDP and the log of fixed assets interacted with the 1990 year dummy.
In (3) and (4), weights are each birth cohort's population size. Standard errors are clustered at the province-year level.

Table A11: 2SLS—effect of relative supply of young college graduates on older adults' high school attainment

	(1) Share older cohort with high school degree	(2) Number older high school graduates in thousands	(3) Share older cohort with high school degree	(4) Number older high school graduates in thousands
Relative supply of young college graduates: ratio of college to non-college graduates	-0.243	-139.3	-0.0131	-3.733
	(0.273)	(155.9)	(0.167)	(239.5)
Mean population 1960-69 (in 1000) * Year 2000		247.0***		447.4***
		(88.82)		(108.5)
Observations	1012	1012	1012	1012
Adjusted R^2	-0.012	-0.044	-0.005	-0.038
Province Fixed Effects	Yes	Yes	Yes	Yes
Year 2000 Fixed Effect	Yes	Yes	Yes	Yes
Birth Cohort Fixed Effects	Yes	Yes	Yes	Yes
Weights	No	No	Yes	Yes
IV	Yes	Yes	Yes	Yes

Notes: 1990 and 2000 census data. We exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin.
Each observation is at the cohort-province-year level. Birth cohorts were born between 1941 and 1963. They are of age 27-49 in 1990.
Standard errors are clustered at the province-year level.
In (3) and (4), weights are each birth cohort's population size.
The instrumental variable is the Policy time * Post.

Table A12: Falsification test—the effect of LLF policies on wages and employment in 1986-1993

	(1) log ratio of wages	(2) ratio of employment	(3) log ratio of wages	(4) ratio of employment
Post	-0.0096	0.0033		
	(0.0378)	(0.0037)		
Policy year			0.0091	0.0003
			(0.0116)	(0.0006)
Observations	238	238	238	238
AdjR2	0.73	0.74	0.73	0.74
Province Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Cohort Fixed Effects	Yes	Yes	Yes	Yes
Cohort * Province dummies	Yes	Yes	Yes	Yes
Cohort * Year dummies	Yes	Yes	Yes	Yes
Initial conditions * Year trend	Yes	Yes	Yes	Yes

UHS data. Observations exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. We have 15 provinces in our sample. We include the years between 1986 and 1993. We select individuals who were born between 1941 and 1959 and participate in the labor force. We divide them into two birth cohorts: 1941-49 and 1950-59. Standard errors are clustered at the province-year level. We falsify the timing of the LLF policies to be 6 years ahead of the true time. The variable post in (1) and (2) indicates that it is 23 years after the first birth cohort was born under the LLF policies, which is roughly 24 years after the falsified timing of the LLF policies. Policy year in (3) and (4) is the true inital year of the LLF policies.

Table A13: Falsification test—the effect of LLF policies on wages in 1986-93 with different timing

	Dependent variable: log ratio of wages				
	(1)	(2)	(3)	(4)	(5)
Post	-0.0049	0.0583	-0.0096	-0.0412	0.0174
	(0.0365)	(0.0366)	(0.0378)	(0.0361)	(0.0425)
Observations	238	238	238	238	238
AdjR2	0.73	0.74	0.73	0.73	0.73
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Cohort * Province dummies	Yes	Yes	Yes	Yes	Yes
Cohort * Year dummies	Yes	Yes	Yes	Yes	Yes
Initial conditions * Year trend	Yes	Yes	Yes	Yes	Yes

Notes: UHS data. Observations exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. We have 15 provinces in our sample. We include the years between 1986 and 1993. We select individuals who were born between 1941 and 1959 and participate in the labor force. We divide them into two birth cohorts: 1941-49 and 1950-59. Standard errors are clustered at the province-year level. From columns (1) to (5), the policies are falsified to be 4-8 years ahead of the true time.

Table A14: Falsification test—the effect of LLF policies on employment in 1986-93 with different timing

	Dependent variable: ratio of employment				
	(1)	(2)	(3)	(4)	(5)
Post	-0.00003	0.00538	0.00330	0.00116	0.00398
	(0.0041)	(0.0036)	(0.0037)	(0.0035)	(0.0033)
Observations	238	238	238	238	238
AdjR2	0.74	0.74	0.74	0.74	0.74
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Cohort Fixed Effects	Yes	Yes	Yes	Yes	Yes
Cohort * Province dummies	Yes	Yes	Yes	Yes	Yes
Cohort * Year dummies	Yes	Yes	Yes	Yes	Yes
Initial conditions * Year trend	Yes	Yes	Yes	Yes	Yes

Notes: UHS data. Observations exclude Guangxi, Inner Mongolia, Ningxia, Tibet, Xinjiang, Beijing, Shanghai, and Tianjin. We have 15 provinces in our sample. We include the years between 1986 and 1993. We select individuals who were born between 1941 and 1959 and participate in the labor force. We divide them into two birth cohorts: 1941-49 and 1950-59. Standard errors are clustered at the province-year level. From columns (1) to (5), the policies are falsified to be 4-8 years ahead of the true time.