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Estimating Education and Labor Market Consequences of China's Higher Education Expansion

Qiao Wen

Business School, Beijing Normal University, Beijing 100875, China; qwen@bnu.edu.cn

Abstract: China initiated a large-scale higher education expansion program in 1999. During the first three years of the expansion, postsecondary enrollment nationwide doubled. Leveraging the quasi-experimental nature of the program, I link administrative records on province-year level college admission quotas with individuals' education and labor market information in 2013 from the Chinese Household Income Project, and employ a two-way fixed-effect model to estimate the expansion's impacts on individuals' education and labor market outcomes, and then use the expansion-induced variation in college access as an instrument for college education to estimate causal returns to college. Results show that the expansion substantially boosts individuals' education outcomes, including completed years of schooling, and the probabilities of attending college and obtaining any postsecondary degrees. The expansion also improves individuals' labor market outcomes, including probability of working, job stability, and hourly wage. My 2-stage least squares estimators imply that returns to education remain substantial in 2013. Returns to attending college and obtaining any postsecondary degrees are estimated to be 54–76 and 59–90 percent respectively. My analysis reinforces that enhanced access to college could raise individuals' educational attainment and boost nations' average level of human capital, and eventually benefit their labor market outcomes.

Keywords: higher education expansion; human capital accumulation; labor market returns to college; quasi-experimental estimates of returns to education

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1. Introduction

The benefits of higher education are well-documented: college-educated individuals, on average, enjoy higher earnings and more stable employment [1,2], better health for oneself and one's offspring [1,3,4], less crime [5], and have higher level of civic engagement [6]. In addition, a better-educated labor force promotes technological advancement and contributes to sustained economic growth [7,8]. These benefits have encouraged both private and public investment in higher education. In fact, student enrollment in higher education have been increasing in many OECD and emerging countries during the past several decades [9]. These expansions generally have positive effects in terms of enhancing college access and improving educational attainment. However, labor market consequences of higher education expansion, such as its impacts on employment probabilities, earnings, and wage gap between college- and high school- educated labor, are much less straightforward.

For example, total enrolled in higher education quadrupled between 1960 and 2000 in UK. Meanwhile, returns to college remain substantial for graduates in 1990s and 2000s, and the returns for women even rise by 10 percent during this period [10]. College enrollment doubled between 1990 and 2005 in Russia. Previous work [11] finds large positive effects of higher education on employment and wages for those who are induced to attend college due to the expansion: returns to obtaining a postsecondary degree remain to be 20–80 percent during the expansion era. In France, the May 1968 Revolution makes the "baccalaureat," a test used for college admission, much easier for that year. Prior literature [12] finds that students of affected cohorts obtain more years of higher education and have higher subsequent wages and occupational attainment due to the reform. Their instrumental variable (IV) estimates of the returns to one year of schooling in college is about 14 percent. At similar time in Italy, a 1961 reform doubled college enrollment in science, technology, engineering, and math (STEM) majors in a decade. Using tax returns data, Bianchi [13] finds that those induced to enroll in STEM programs earn no more than students in prior cohorts who were denied entrance into college.

Besides expansions for the entire cohorts, there are also large-scale programs designed for specific groups of individuals. The Serviceman's Readjustment Act of 1944 (American G.I. Bill) provides generous financial aid for American veterans of the Second World War and then for the Korean- and Vietnam-era veterans to attend college. These programs increase post-war cohorts' college attendance and completion [14–16], but their effects on earnings are close to zero [16]. A similar program for Canadian veterans (the Veteran's Rehabilitation Act), however, is found to have positive effects on both education and earnings: IV estimates of the returns to one year of college education is approximately 15 percent [17].

The above-mentioned work does not reach a consistent conclusion regarding college expansion's effects on earnings. My study adds to this empirical literature on the education and labor market consequences of higher education expansion by analyzing one of the most radical and large-scale higher education expansion programs in modern history that was initiated in 1999 in China. Number of available spots in higher education institutions (HEIs) for high school graduates increased by nearly 50 percent in the first year of the expansion and increased by another 40 and 20 percent in the following two years respectively [18]. During the first three years of the expansion, college enrollment nationwide doubled.

Besides the unusual magnitude of China's higher education expansion, the quasi-experimental nature of the reform also enables me to adopt methods that could overcome "ability bias" in the relation between education and earnings. As a result, my work is also related to the larger literature on estimating (causal) returns to education.

Several theories have been proposed to explain the relationship between schooling and earnings. According to the human capital theory [19,20], human capital is the skills, knowledge, and attributes an individual possesses that have value in the labor market. Individuals invest in human capital through schooling (i.e., education makes people more productive), and later be paid according to their productivity. An alternative explanation for how education affects workers' earnings is the signaling model [21]. It says that education may not actually build up individuals' skills and knowledge, but to reveal individuals' unobserved ability and skills to employers who lack information about potential hires' true productivity level. Previous empirical works [22–24] find evidence showing earnings jumps at the receipts of education credentials (such as high school diplomas and bachelor's degrees), which suggests considerable signaling value of obtaining such degrees. This phenomenon is also called "sheepskin effect." A related theory considers education as a partial positional good [25–28]. The value of a positional good depends on the relative rank in comparison to others, rather than the absolute value of them [25,29,30]. The labor market value of education is likely to have some positional characteristics because its value depends on both the absolute level of education an individual has and how many other people own it (i.e., the relative level). Unlike the human capital and signaling models that both predict a positive relationship between attending college and future earnings, under the positional goods hypothesis, higher education would become less valuable as more people get access to higher education and obtain postsecondary degrees, even if skills and knowledge acquired from receiving such education keep constant.

Mincer [31,32] further develops the human capital earnings function that relates earnings with schooling and work experience (as a proxy for "on-the-job training") and predicts a positive relationship between years of education and natural logarithm of wages. The Mincer regressions provide a benchmark for later empirical works to estimate the returns to education. However, one caveat of the Mincer model is that if individuals self-select into different educational levels based on observed or unobserved factors that potentially also affect their future earnings, the estimated effect of education on earnings using a simple ordinary least squares (OLS) function will be biased. For example, if we cannot fully control for ability, which is positively correlated to both education and earnings, the resulting estimate of the returns to education omitting ability will be biased upward. Moreover, individual characteristics like personality that are either hard to observe or measure could also have direct effects on education and labor market outcomes. Linking student evaluation of teaching (SET) data and employment data for college graduates from one South Korean university during 2008–2012, researchers find that students with higher ratio of monotonic responses in SETs, as a proxy for student personality, have lower probabilities of being employed, and are more likely to be hired for lower prestige jobs [33].

This issue of the endogeneity of education inspires a large body of later work that aims to find credible exogenous variation in schooling that is unrelated to other individual characteristics that also affect earnings. I show in this paper that the substantially expanded access to HEIs over a short period of time during China's higher education expansion provides plausibly exogenous variation in the probability of college attendance for different cohorts of students who were scheduled to attend college before or after the expansion. In other words, the expansion provides variation in college admission that is arguably unrelated to pre-existing factors like students' ability, motivation, and family background. Moreover, the intensity of the expansion was not uniform across provinces, and there were substantial inter-provincial differences in the number of increased college admission spots during the expansion, which provides another dimension of exogenous variation in the probability of college attendance.

More specifically, I link province-cohort level administrative data on the college admission quota to a nationally representative sample of over 8000 adults who have detailed information on education and labor market outcomes in 2013 from the most recent round of the Chinese Household Income Project (CHIP 2013) [34]. Leveraging the plausibly exogenous variation in college attendance, I first use a two-way fixed effect model with province and cohort fixed effect to estimate the effects of the expansion on individuals' education and labor market outcomes, such as employment probability, work stability, and earnings. Then, with further assumptions I use the expansion-induced increases in college attendance as an instrument for education to identify the labor market returns to attending and completing college.

Results show that the expansion boosts individuals' education outcomes: a 10-percentage-point increase in the fraction of 18-year-olds who could access any regular HEIs (or one additional college admission spot for every ten 18-year-olds) leads to approximately 0.3 more completed years of schooling, 7 and 6 percentage points higher probabilities of attending college and eventually obtaining any post-secondary degrees respectively. In addition, the expansion-induced increases in college admission quota also translate to higher employment probability and earnings: for example, a 10-percentage-point increase in the fraction of 18-year-olds who could access any regular HEIs improves the probability of working for positive income by 3–4 percentage points and raises hourly wage by 4–5 percent for individuals in my sample.

Moreover, the 2-stage least squares (2SLS) estimates suggest that returns to attending and completing college remain substantially high after China's radical expansion of its higher education system: according to the more conservative estimates, for example, obtaining any postsecondary degree (including associate's and bachelor's degree) boosts the probability of working for positive income by 51 percent. Individuals with postsecondary degrees also enjoy more stable jobs: they are 60 percent and 58 percent more likely to work in formal sectors and work full-time and year-round respectively; their hourly wage in 2013 is roughly 59 percent higher than that for individuals who do not hold any postsecondary degrees.

The reminder of this paper is organized as follows. Section 2 introduces the background of China's higher education expansion program, and findings from previous work regarding the impacts of the expansion on education and labor market performance. In Section 3, I outline the data used for my analysis and how I use this "natural experiment" to identify the causal effects of reform-induced college attendance on subsequent labor market outcomes. Section 4 presents results on reduced-form estimates of the expansion's effects, and the 2SLS estimators of the returns to education in China. I also conduct a series of robustness checks to test the validity of my identification strategy and to see whether the estimated effects are sensitive to controlling for time-varying province-level covariates. Section 5 discusses the key findings, and points to the direction for future research.

2. The 1999 Higher Education Expansion in China

China has a centralized higher education admission system that follows the principal of distributing admission quotas by province and then admitting students by their College Entrance Examination (CEE) score [35]. (The CEE is a pre-requisite for entering almost all HEIs in China and is taken at the summer of the final year of senior high school. For the cohorts under this analysis, CEE score is the primary determinant for college admission). Every year the central government first sets a planned quota for the total number of students that could be admitted into regular HEIs nationwide during early months of each year. This aggregate nation-level quota is then distributed across provinces. On the students' side, they take the CEE that is administered at the provincial level at their province of household registration (*hukou*), and rank against each other within that province. (*Hukou* is the household registration system in mainland China. It first classifies individuals into either an agricultural (i.e., rural) or non-agricultural (i.e., urban) *hukou*. In most cases, the *hukou* province is equivalent to one's birth province, but children of parents who have not obtained the *hukou* of where they work/reside may have a *hukou* province equals to their parents' (birth) province. The type and province of *hukou* are linked with multiple benefits and restrictions, including where one can attend schools and take the CEE, the minimum wages and pension plans one can enjoy, and where one can purchase house and cars, etc). In principal, a student's *hukou* province and thus the number of admission quota assigned to that province, score on the CEE, and choices listed on the college application will determine whether he/she could attend any college at all, and which institution he/she could attend.

China now has the largest higher education system in the world in terms of enrollment and degrees conferred. According to the most recent statistics [36], China enrolled approximately 30.3 million undergraduate students in its degree-granting HEIs in the fall of 2019. By the summer of 2019, 7.6 million students earned an associate's or bachelor's degree in China [36]. Tertiary gross enrollment ratio, calculated as the ratio between total enrollment in tertiary education and the population of the 18–22 age group, has reached 51.6 percent by 2019 [37]. This is a recent phenomenon, however. Approximately two decades ago in 1998, the number of total enrollment and degrees granted are roughly 3.4 million and 0.4 million respectively [36], only 11 and 5 percent of the corresponding numbers in 2019. Tertiary gross enrollment ratio is 9.8 percent in 1998 [38], which is only about half of that year's world average (18.4 percent) [39]. The dramatic increases in postsecondary attendance and degree attainment can be attributed to China's higher education expansion that was initiated in 1999.

In early 1999, China's central government and the Ministry of Education (MoE) decided to admit approximately 470,000 more students into China's HEIs in that Fall [18]. As a result, the total number of first-time-first-year undergraduate students newly admitted into China's degree-granting HEIs increased by nearly 50 percent between 1998 and 1999 [36]. The radical expansion continued in the following years: number of newly admitted undergraduate students increased by about 40 percent in 2000 and kept increasing at an annual rate of around 20 percent till 2004 [36]. In only three years from 1998 to 2001, undergraduate enrollment in China's regular HEIs more than doubled; it nearly quadrupled by 2004. Figure 1 below depicts the trends in first-time-first-year and total undergraduate enrollment in, and graduates from China's degree-granting HEIs during the years of 1995 and 2020.

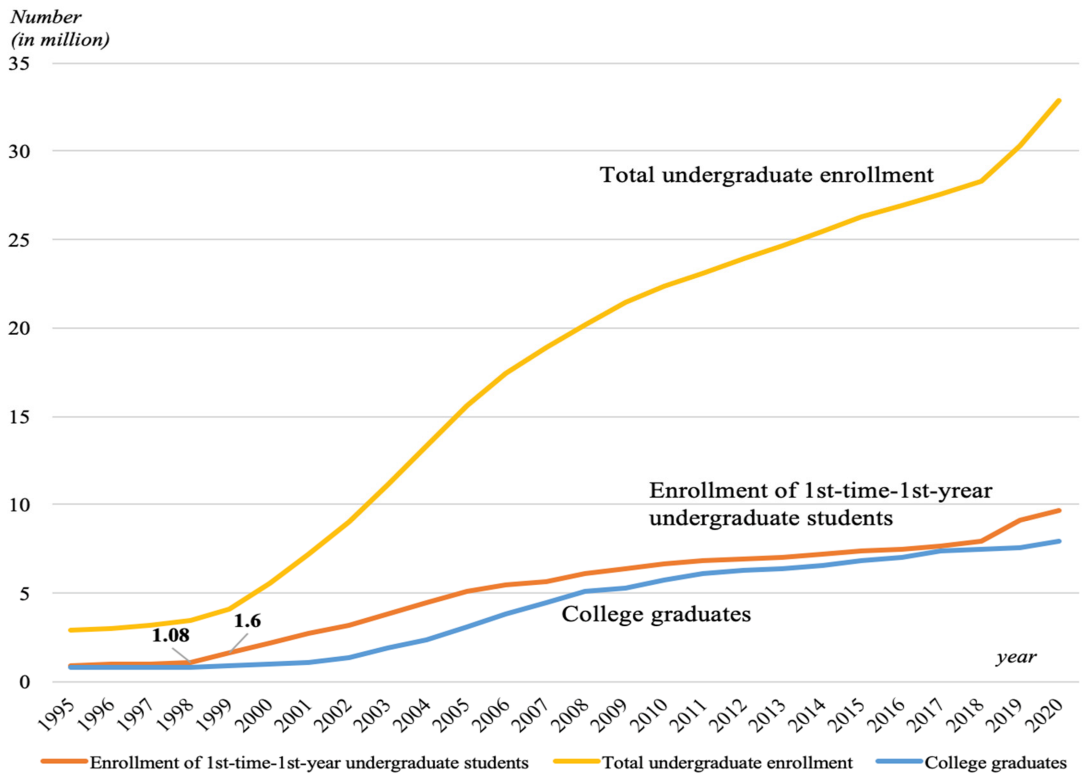

Figure 1. Undergraduate Enrollment in and Graduates from Regular Degree-granting Higher Education Institutions in China (in millions), 1995–2020.

This abrupt change of policy provides plausibly exogenous variation in college admission among students who are scheduled to attend college in different years. Moreover, the "intensity" in how much college admission quota is expanded is different across provinces, providing another dimension of variation. As a result, an individual's birth cohort and *hukou* province would primarily determine his or her degree of exposure to the expansion. For example, the program effects should be close to zero for older individuals who attend college before 1999, larger for younger cohorts who attend college after the expansion, and largest for those from provinces and cohorts that experience the most substantial increases in college admission spots.

Previous studies that use the quasi-experimental nature of this policy change have examined the expansion's effects on employment or earnings. For example, using the 2000 and 2005 Census data, Wu and Zhao [40] and Li, Whalley, and Xing [41] both use a difference-in-differences (DiD) method to compare trends in labor market outcomes for young and older college graduates from 2000 to 2005. The former study [40] finds that the expansion increases college graduates' unemployment probabilities by 3–5 percentage points and decrease their hourly earnings by 10 percent. (The 2000 Census dataset does not contain earnings information, and Wu and Zhao [40] replace it with comparable survey data that conducted in 2002 to estimate the expansion's earnings effect). The latter study [41] concludes that the expansion increases college graduates' unemployment rates by 6–9 percentage points. Xing, Yang, and Li [42] update this line of research by adding the 2010 Census data and reach similar conclusion: the expansion has negative impacts on college graduates' employment probability, but the magnitude of that negative effect is smaller in 2010 than that in 2005. Using data from the 2005 Census, Ou and Zhao (2016) [43] also use a DiD method to compare labor market outcomes for young exposed and old unexposed cohorts from heavily and weakly expanded provinces and find that the expansion lowered college graduates' probability of being unemployed, which contrasts with findings by the previously mentioned three studies. They also find that the expansion lowered college graduates' earnings. (They distinguish high and low expansion provinces depending on province-level changes in admission ratio from 1998 to 2001).

The above reduced-form studies restrict their analyses to college graduates only. However, part of the expansion's education effect is to induce some high school students who would formerly directly enter the labor market to attend college. Their analyses cannot reveal the expansion's employment impacts on these "treated" individuals. Most of them also use labor market information in 2005, when the earliest after-expansion cohort could possibly work for only 2–3 years.

There are other studies that explore returns to college during the expansion era. For a most recent work for instance, Hu [44] uses whether one is scheduled to attend college after the expansion as an instrumental variable (IV) for schooling and estimates the causal returns to college to be around 76 percent in 2013. The author also uses the admission ratio at the national level in year *t* as an IV for individuals who are scheduled to attend college in year *t* and finds the returns to college to be about 58 percent.

In terms of method, Hu [44] takes advantage of the expansion-induced variation in college admission ratio for different cohorts of individuals. However, this variation might also capture macro-economic trends that happened concurrently during the pre-expansion and expansion era. Moreover, the nationwide admission ratio may not accurately reflect individuals' exposure to the expansion as admission quota is distributed by students' provinces (as explained previously in this section). Lastly, both Hu [44] and Ou and Zhao [43] use the actual admission ratio calculated as number of admitted students divided by the number of CEE takers or high school graduates, both of which could be endogenous. For example, more students might decide to attend or persist in high school and/or to take the CEE due to increased prospects of college attendance after the expansion. As I will show in the "data and method" section, my study uses the number of *planned* college admission quota and college-aged population at the *province-year level* to proxy for exogeneous variation in individuals' college admission prospects.

Building upon these previous work, I will show in the following sections that my study uses more recent round of survey data that could capture the labor market outcomes for at least ten post-expansion cohorts of college graduates; I also use administrative records of the province-year level planned college admission quota to construct credible instrument for college education; I also account for very comprehensive sets of province-, cohort-, and province-year- level confounding influences to ensure that my estimates are robust to different model specifications. As a result, my study could depict a fuller picture on the expansion's education and labor market effects and provide new evidence on the returns to college in the 2010s.

3. Data and Method

In this section, I first introduce the datasets used for this study. Second, I show how I construct the sample for my analysis. Then, in sub-Section 3.3, I give details on how I determine an individual's "probable college cohort" and *hukou* province that together determine his/her degree of exposure to the expansion. I also introduce how I construct key dependent variables and covariates in this sub-section. Lastly, I present the equation for estimating the expansion's education and labor market outcomes in sub-Section 3.4.

3.1. Data

I use multiple sources of data for my analysis: the individual level data come from the Chinese Household Income Project (CHIP) [34], and the province-year level college admission quota are constructed using administrative records from various years of the *Educational Statistics Yearbook of China* [38,45–57]. I also use the Census data obtained from National Bureau of Statistics of China (NBS)'s data archive [36] to construct province-, year-, and province-year- level covariates.

The CHIP is a repeated cross-sectional household survey that has been conducted in 1988, 1995, 2002, 2007 and 2013. It uses multi-stage random sampling to draw nationally representative samples of households, which are sub-samples of the NBS's larger official annual household surveys [58]. The purpose of the survey is to measure and estimate the distribution of personal income in China, and thus the survey contains very detailed information on individuals' employment (including number of jobs held, month, weeks and hours worked) and income (including labor and capital income). CHIP also contains detailed information on individuals' schooling history and demographic information such as gender, year and month of birth, and province and type (urban/non-agriculture vs. rural/agriculture) of *hukou*.

For my analysis, I use the urban survey from the most recent round of CHIP (i.e., CHIP 2013), which covers individuals' earnings in 2013 and could potentially capture labor market outcomes for at least ten post-expansion cohorts of college graduates. (The CHIP 2013 also has a rural survey and survey for rural-to-urban migrant workers. I do not use them because primary source of income for these observations comes from agricultural work or leasing land, and very few of the observations have attended post-secondary education). For falsification tests, I also use the two prior-expansion waves (CHIP 1988 and CHIP 1995) [59,60] to test for the "parallel pre-trends" hypothesis.

3.2. Sample

My sample includes individuals aged 23–54 years old at the time of the survey in 2013. Individuals born before 1959 (i.e., older than 54 in 2013) were affected by the "Culture Revolution" during which all the HEIs were shut down. Older individuals in the survey are also likely to be retired by 2013. Meanwhile, individuals born after 1990 are likely to be still enrolled in school by 2013. I exclude those who are disabled, in maternal or long-term sickness leave, or still enrolled in school at the time of the survey. I further drop the few individuals who have received less than nine years of schooling by 2013 since China enforces a law on nine-year compulsory education (corresponding to the completion of lower-secondary school), (The 1986 Compulsory Education Law extends the 6-year compulsory education to 9 years. Among all the birth cohorts of the CHIP 2013 Urban survey, only 1 percent have not completed 9 years of schooling by 2013, and this group is excluded for this analysis. However, thanks to one reviewer's comment, I note that this 1986 law may affect other populations (e.g., marginal students who would not attend any secondary schools without this law) other than the sample being analyzed for this study) or whose post-secondary degrees were obtained from non-regular HEIs (including party schools and part-time adult education programs). (Those individuals usually attend non-regular HEIs and obtain such certificates after many years of work mainly for career promotion purposes, who are very likely to be different from the traditional college students who attend regular HEIs right after high school). The excluded categories account for less than 7 percent in total. The remaining full analysis sample comprises 8377 individuals.

3.3. Key Variables

3.3.1. Probable College Cohort and *Hukou* Province

As introduced in Section 2, an individual's degree of exposure to the expansion depends on the cohort year and *hukou* province when he or she is scheduled to attend college. For the former, CHIP 2013 does not provide information on individuals' year of college attendance. I infer their "probable college cohort" using individuals' year and month of birth assuming that individuals attend college at the age of 18. (School year begins on 1 September in China. For most provinces, students start primary school when they reached six by 31 August, and it takes 12 years to complete upper-secondary school before one can enter college). For example, I assign individuals born between 1 September 1989 and 31 August 1990 with a "probable college cohort" of 2008. For the latter, CHIP 2013 provides information on an individual's *hukou* province at the age of 14, when one took the CEE, and at the time of the survey in 2013. I determine individuals' exposure to the expansion based on their *hukou* province at 14 years old when most individuals are at the final year of their compulsory education (i.e., the final grade of lower secondary school). (Individuals could change their *hukou* province between 14 and before they take the CEE, and thus generate measurement error in the expansion intensity variable. However, migration across provinces is rather rare for the CHIP 2013 sample: among those who have information on the province where they took the CEE, only 1.8 percent take the CEE in a different province than their *hukou* province at 14). It is preferred over CEE province and current province in 2013 because both may be endogenously determined due to selective migration either to take the CEE or to work. (CEE province also has very high proportion of missing values (65.3 percent)).

3.3.2. Expansion Intensity

I use the increase in the proportion of people who can access college within one's probable college cohort and *hukou* province after the expansion to measure the degree of treatment an individual is potentially exposed to the higher education expansion program. More specifically, I first construct the province-cohort level college admission ratio as $Ratio_{p,c} = \frac{Quota_{p,c}}{18-year-olds_{p,c}}$. The numerator is the planned admission quota assigned to students of cohort c from province p. I use the quota that is set before students taking the CEE rather than the actual number of enrollment because the latter is partly determined by students' test scores and their application choices. The denominator is the province-level cohort size, or the number of population aged 18–19 years old in province p at year c. For the denominator, I compute province-cohort level college-age population using the Census data from the NBS [36]. The Census only contain province-level population data in the 15–19 age group. I infer the size of 18-year-olds using its proportion out of the 15–19 age group in the national aggregate sample, assuming the distribution of each age cohort within the 15–19 age group are the same across all provinces. (I also use two alternative imputation strategies (i.e., I use province-year level number of primary school and lower secondary school graduates six and three years before the "probable college cohort year" multiplied by their corresponding gross enrollment rates at respective educational levels) and derive similar numbers). Alternative candidates for the denominator such as the number of CEE-takers or high school graduates at the province-cohort level are more problematic because test-taking and high-school attendance are endogenous and likely to be affected by the expansion.

The CHIP 2013 sample covers 30 provinces (out of a total of 31 provinces in mainland China). Figure 2 below presents trends in college admission ratio for one thirds of those provinces that covers all the five larger geographic regions in China.

Figure 2 suggests that before the expansion, college admission ratio within each province was quite stable during 1995 to 1997 except for a relatively uniform dip in 1998, and trends were parallel across provinces. Starting from 1999, all provinces experienced substantial increases in admission ratio, but province-specific trends became much more idiosyncratic. For example, the increases in admission ratio are very sharp after the expansion in Shaanxi (red solid line), but the increases are relatively mild in Guangdong (grey dotted line).

I then define the province-year level "expansion intensity" measure as the increase in $Ratio_{p,c}$ after the expansion from the corresponding province level ratio in 1998 (i.e., before the expansion), that is, $Intensity_{p,c} = Ratio_{p,c} - Ratio_{p,1998}$. I use increases in $Ratio_{p,c}$ after the expansion, rather than the province-cohort level $Ratio_{p,c}$, to measure expansion intensity because I don't want to use any of the variation in the ratio that might occur before the expansion.

3.3.3. Dummy Variable for Before- and After-Expansion Cohorts

I construct the dummy variable, $Post_c$, that indicates whether student i is "scheduled" to attend college after the expansion. $Post_c$ equals 1 for those with probable college cohorts of 1999 and after and equals 0 for those of prior−1999 probable college cohorts.

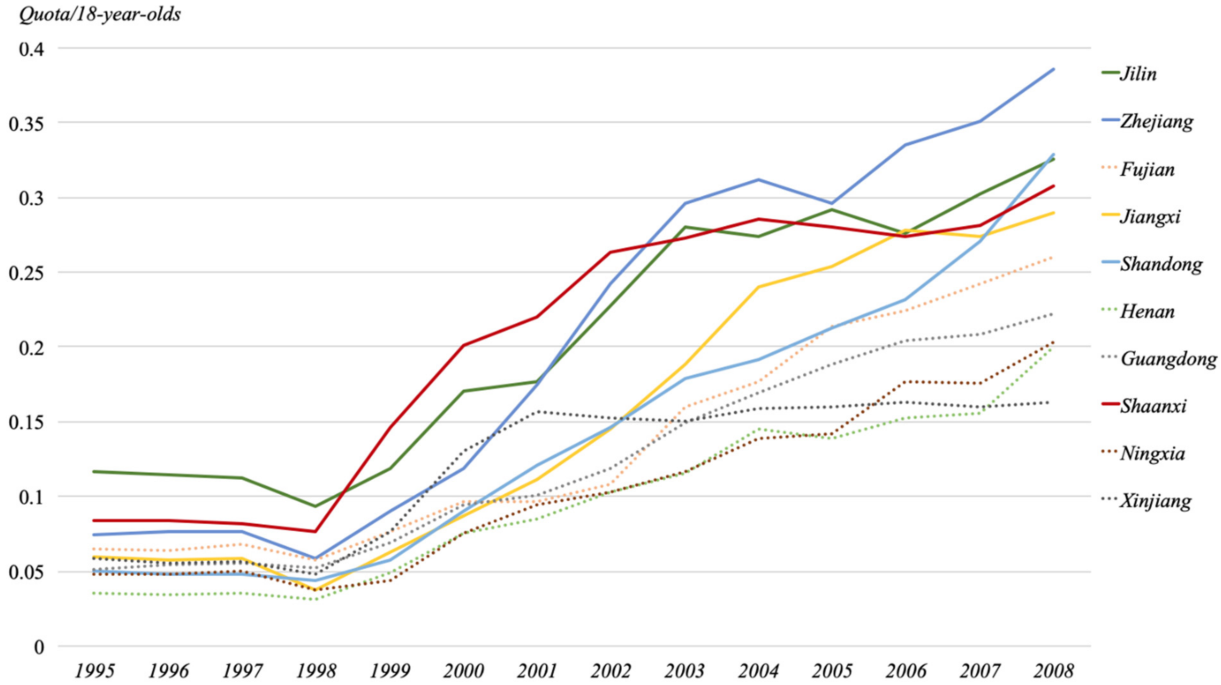

Figure 2. College Admission Ratio for Selected Provinces, 1995–2008.

3.3.4. Treatment Definition

The interaction between the dummy variable and the intensity measure ($Post_c * Intensity_{p,\ c}$) denotes whether an individual is exposed and how much he or she is exposed to the expansion. Variation in this treatment variable comes from two sources: changes within each province across different college cohorts, and differences across provinces within each college cohort after the expansion.

3.3.5. Outcomes

I construct three variables to measure the education effects of the expansion: the "years of schooling completed" variable measures total years of formal schooling one completed by the end of 2013, and six, nine and twelve years of schooling corresponds to the completion of primary, lower- and upper-secondary schools. The other two dummy variables are used to measure college attendance and degree attainment: the "ever attended regular HEIs" dummy equals one if individuals ever attended any regular HEIs, including 3-year community colleges, 4-year universities, and graduate schools. The "obtained any post-secondary degrees" dummy equals one if individuals ever obtained an associate's (AA), bachelor's (BA) or graduate degree from regular HEIs.

As for labor market outcomes, I construct outcomes for employment probability and stability, quantity of labor supplied, and earnings. The "probability of working and earning positive income" variable equals one if an individual worked for at least one month and received positive income from any type of job during 2013. The "worked in formal sector" variable equals one if an individual worked as an employee or employer in a formal sector, and thus excludes the following types of employment: self-employment, worked as own-account worker or contributing family worker. The "worked full-time and year-round" dummy variable equals one if an individual worked for 12 months and on average 35 or more hours per week during 2013. (National holidays, annual leave, vacations for certain jobs (e.g., summer and winter vacations for teachers) are regarded as working days in the CHIP 2013 survey). The "number of jobs held" variable measures the total number of jobs held during 2013, and the "total hours worked for all jobs" variable is calculated as the sum of self-reported total weeks worked multiplied by total hours worked per week for all the jobs held in 2013. For individuals who reported to have not worked during 2013, I assign zero values to these employment probability and stability outcomes, and thus include them in the analysis.

With regards to income, I construct four measures to capture both the total annual income and hourly wage in 2013 expressed in both the level and natural logarithm terms. More specifically, the "total annual income from all jobs" variable captures the sum of labor income from all jobs held in 2013, and income includes salary, bonus, and in-kind job benefits (such as discounted housing and food vouchers) transferred to Chinese yuan (RMB) values. The "hourly wage from all jobs" variable is the corresponding total annual income divided by the total hours worked in 2013. To capture the expansion's effects at the employment margins and avoid sample selection bias due to dropping individuals who do not have positive earnings, I keep people who do not work in 2013 and recode their earnings as zeros. Following the most common fix for dealing with log zeros [61], I add 1 RMB to all observations before taking the logs and add a dummy variable indicating whether the individual originally has zero earnings later in all the earnings regressions when using these imputed log earnings.

3.3.6. Covariates

Individual level covariates include a "female" dummy, a "rural" dummy indicating the type of *hukou* (it equals one when an individual holds a rural or agriculture *hukou* and zero when holding an urban or non-agriculture *hukou*), an "only-child" dummy indicating whether the individual is the only child in his/her family (it equals one if an individual is the only child of his/her parents and equals zero when he/she has other siblings), (Thanks to one reviewer's comment, I add this control variable to avoid any potential confounding influences caused by the "One-child Policy" that was initiated by the Chinese government in 1980, and then officially written into the constitution in 1982. The law requires couples to have only one child (with some exceptions for certain groups of populations)) and two interaction terms between the female dummy and the "rural" and "only-child" dummies respectively.

Table 1 below provides summary statistics for my full analysis sample, and for the prior- and post-expansion cohorts samples separately. There are 8377 individuals in total, and the mean age is 39 years old in 2013. On average, individuals in my sample complete 11.6 years of schooling (12 years of schooling correspond to the completion of high school). Approximately 32 percent of individuals have attended any regular HEIs, and 29 percent have obtained a postsecondary degree. On average, post-expansion cohorts have better educational outcomes than the prior-expansion cohorts. Moreover, for the full sample, approximately 85 percent have ever worked and earned positive earnings, and 73 and 64 percent worked in a formal sector and full-time-year-round during 2013. In terms of earnings, the means of total annual income and hourly wage in 2013 are 34,643 and 18 Chinese yuan (CNY) respectively, which are approximately \$7749 and \$4.0 in 2020 US dollars respectively (inflated to 2020 CNY first and converted to USD using an exchange rate of 1 USD = 6.8 CNY).

Moreover, approximately 27 percent (2281 observations) of the sample belongs to the post-expansion cohorts. Among them, the province-cohort level post-expansion intensity measure ranges from 0.011 to 0.495 with a mean of 0.142 and a standard deviation of 0.09. To put these numbers in perspective, the baseline admission ratio at the national level (which could be viewed as the weighted average across all provinces) is 0.056 in 1998 (i.e., before the expansion)–on average, less than 6 people out of one hundred 18-year-olds would be admitted into any regular HEIs in China in 1998. After the expansion, the expansion intensity measure implies that for the most extreme cases, there will be approximately one additional admission spot out of one hundred 18-year-olds for an individual in my sample who comes from the province and year that most weakly expanded its access to college, whereas an individual coming from the strongest expansion province and year would see roughly 50 additional admission spots (out of one hundred 18-year-olds) opening to him or her.

Table 1. Descriptive statistics for selective samples.

Variables	Full Sample Mean	Mean for Prior-Expansion Cohorts	Mean for Post-Expansion Cohorts	Difference in Means [1]
Education Outcomes in 2013:				
Years of schooling completed	11.6 (2.9)	11.1 (2.7)	13.1 (2.9)	2.0 * (0.1)
Ever attended any regular HEIs	0.320 (0.466)	0.243 (0.429)	0.524 (0.500)	0.281 * (0.012)
Obtained any postsecondary degrees	0.291 (0.454)	0.215 (0.411)	0.496 (0.500)	0.280 * (0.011)
Labor Market Outcomes in 2013:				
Ever worked and earned positive income	0.848 (0.359)	0.841 (0.365)	0.867 (0.340)	0.025 * (0.009)
Ever worked in formal sectors	0.728 (0.445)	0.709 (0.454)	0.779 (0.415)	0.070 * (0.011)
Worked full-time and year-round	0.653 (0.476)	0.650 (0.477)	0.660 (0.474)	0.010 (0.012)
Total hours worked for all jobs	1981 (1042)	1979 (1069)	1986 (968)	7 (26)
Total annual income from all jobs (¥)	34,643 (43,235)	35,084 (46,050)	33,459 (34,532)	1625 (1079)
Average hourly wage from all jobs (¥)	18.0 (74.1)	18.8 (85.5)	16.0 (24.4)	2.8 (1.9)
ln(total annual income)	8.82 (3.86)	8.76 (3.93)	8.98 (3.65)	0.22 * (0.10)
ln(hourly wage)	2.32 (1.20)	2.31 (1.23)	2.35 (1.13)	0.04 (0.03)
Treatment Variables:				
"Post"-expansion cohorts	0.272 (0.445)	0 (0)	1 (0)	--- ---
Expansion "intensity"	--- ---	0 (0)	0.142 (0.090)	--- ---
Control Variables:				
Female	0.506 (0.500)	0.504 (0.500)	0.509 (0.500)	0.005 (0.012)
Rural *hukou*	0.400 (0.490)	0.398 (0.490)	0.403 (0.491)	0.005 (0.012)
Female ∗ Rural *hukou*	0.202 (0.401)	0.199 (0.399)	0.210 (0.407)	0.011 (0.010)
"Only-child" in family	0.194 (0.395)	0.100 (0.301)	0.445 (0.497)	0.344 * (0.009)
Female ∗ "Only-child" in family	0.088 (0.283)	0.043 (0.203)	0.207 (0.406)	0.164 * (0.007)
Number of observations	8377	6096	2281	---

[1] "*" indicates the two-tailed p-value computed using the t distribution is less than 0.05. Standard deviation in parentheses.

3.4. Estimation Strategy

In the most parsimonious specification, my estimating equation can be written as a two-way fixed-effect model with controls:

$$Y_{i,p,c} = c_1 + \alpha Post_c * Intensity_{p,\,c} + \beta Cohort_c + \gamma Province_p + \theta X_i + \varepsilon_{i,\,p,\,c} \quad (1)$$

where $Y_{i,p,c}$ is the education or labor market outcomes for individual i from *hukou* province p and probable college cohort c; $Post_c$ indicates whether individual i would attend college after the expansion, $Intensity_{p,c}$ denotes province-cohort level intensity of the expansion, and the interaction term between "$Post_c$" and "$Intensity_{p,\,c}$" identifies the variation in expansion intensity among exposed cohorts. $Cohort_c$ is probable college cohort, and $Province_p$ is one's *hukou* province at 14 years old; X_i is a vector of individual characteristics, including gender (a female dummy), type of *hukou* (a rural dummy), an "only-child" dummy indicating whether the individual is the only child of his/her parents, and two interaction terms between the "female" dummy and the "rural" and "only-child" dummies respectively; $\varepsilon_{i,\ p,\ c}$ is the error term. The variable of interest is the coefficient before the interaction term $Post_c * Intensity_{p,\ c}$ (i.e., α) that identifies the effects of expansion-induced increases in college admission prospects on varies education and labor market outcomes. Similar methods have been used to evaluate the effects of a large-scale primary school construction program in Indonesia [62] and an education program that enables many Canadian veterans of the second World War to attend college [17].

The key assumption underlying my identification strategy is variation in the post-expansion province-cohort level increases in college admission quota from their respective pre-expansion levels is uncorrelated with potential unobserved province-year level confounding factors that may also influence students' education and subsequent labor market outcomes. In next section, I first present regression results from Equation (1), and then I conduct various checks to test the validity of my method and the robustness of my results to different model specifications.

4. Results

Intuition behind Equation (1) can be illustrated using a simpler Difference-in-Differences (DiD) approach that categorizes individuals into either the prior- versus post- expansion group and high- versus low- expansion intensity group. Tables A1 and A2 in Appendix A present the DiD estimators of the expansion's effects on education and labor market outcomes. Instead of using a dichotomous measure of the expansion intensity (either "high" or "low" expansion provinces, and either "post" or "prior" cohorts), below I present regression results using Equation (1) with a continuous measure of $Intensity_{p,\ c}$ that takes advantage of the full variation in treatment intensity across provinces and cohort years, together with full sets of cohort and province fixed effects.

4.1. The Higher Education Expansion's Effects on Education

Table 2 presents regression results of the effects of the expansion on education outcomes using Equation (1). Besides presenting the raw coefficients on the interaction term, in column 2 of Table 2, I also present the effects of a 10-percentage-point increase in expansion intensity (equivalent of one additional college admission spot out of ten 18-year-olds after the expansion) on the same set of education outcomes. (Note that instead of using a continuous measure of $Intensity_{p,\ c}$, I can also separate the post-expansion province-cohort level cells into either "high" or "low" expansion intensity groups, depending on whether $Intensity_{p,\ c}$ is above or below the nationwide $Intensity_{nationwide,\ c}$ for year c. The means of this "intensity" measure for the high and low expansion intensity groups are 0.201 and 0.102 respectively, and the difference in the means between the high and low groups is roughly 0.1 or one additional admission spot out of ten 18-year-olds. Therefore, the "10-percentage-point" difference in intensity is also roughly the difference in the means of expansion intensity between the high and low expansion groups).

Using variation in the expansion-induced increases of admission quota across cohorts and provinces, results in Table 2 show that the expansion boosts individuals' educational outcomes, including years of completed schooling, and the probabilities of attending and completing college. For example, a 10-percentage-point increase in expansion intensity (or one additional college admission spot out of ten 18-year-olds) leads to approximately 0.3 more years of completed schooling, 6.5 percentage points higher probability of attending college, and 5.5 percentage points higher probability of obtaining any post-secondary degrees. All coefficients on the education outcomes are statistically significant at the 1 percent level. Note that at the national level, the admission ratio increased by approximately 20 percentage points by 2008 from the baseline level of 5.6 percent in 1998, implying that for those who are scheduled to attend college in 2008 (i.e., the youngest cohort in my sample), they have on average 0.6 more years of schooling and their probabilities of attending and completing college are 14 and 12 percentage points higher respectively than those who are scheduled to attend college before the expansion in 1998.

Table 2. Effects of the expansion on education outcomes.

Education Outcomes	[1] Raw Coefficient		[2] 10 pp. Increase in "Intensity"	
Years of schooling completed	2.9	***	0.3	***
	(0.9)		(0.1)	
Ever attended any regular HEIs	0.652	***	0.065	***
	(0.152)		(0.015)	
Obtained any postsecondary degrees	0.549	***	0.055	***
	(0.148)		(0.015)	
Number of observations	8377			

All models control for cohort and province fixed effects, a female dummy, a rural *hukou* dummy, a dummy indicating whether the individual is the "only-child" in family, and two interactions (female ∗ rural and female ∗ only-child). Robust standard errors clustered at the province level are in parentheses. *** $p < 0.01$.

I also construct more detailed educational categories to explore at which level the expansion's educational effects are the most pronounced. The expansion's impacts on education could work through the following channels: first, it could increase high school graduates' chances of being admitted into college, and thus increase their probability of college attendance and attainment; and second, it could also induce students to finish high school or attend high school even if they do not eventually end up attending any post-secondary institutions, and thus increase their educational attainment at the upper secondary school level. Results (presented in Table A3 in Appendix B) suggest that the expansion's effects on education work through inducing high school graduates who would directly enter the labor market without attending any HEIs to attend and complete college and even beyond. The college expansion does not have statistically significant effects on educational outcomes for those who have lower educational attainment to begin with (i.e., for individuals who completed lower secondary school but attended no high school at all or for high school dropouts).

4.2. The Higher Education Expansion's Effects on Labor Market Outcomes

I then tend to the expansion's effects on individuals' labor market outcomes in 2013 when the post-expansion cohorts in my sample could have worked for at least one to eleven full years after college graduation. Table 3 presents regression results using Equation (1).

Table 3. Effects of the expansion on labor market outcomes.

Labor Market Outcomes	[1] Raw Coefficient		[2] 10 pp. Increase in "Intensity"	
Probability of working and earning positive income	0.386	***	0.039	***
	(0.117)		(0.012)	
Probability of working in formal sectors	0.408	***	0.041	***
	(0.150)		(0.015)	
Probability of working full-time and year-round	0.334	**	0.033	**
	(0.160)		(0.016)	
ln(Hourly wage)	0.507	**	0.051	**
	(0.226)		(0.023)	
Number of observations	8377			

All models control for cohort and province fixed effects, a female dummy, a rural *hukou* dummy, a dummy indicating whether the individual is the "only-child" in family, and two interactions (female ∗ rural and female ∗ only-child). Robust standard errors clustered at the province level are in parentheses. ** $p < 0.05$, *** $p < 0.01$.

Table 3 shows that the expansion has positive impacts on the probabilities of working and earning positive income from both any type of employment and formal sector employment only. More specifically, a 10-percentage-point increase in the intensity of the expansion (or one additional college admission spot out of ten 18-year-olds) is associated with 3.9 and 4.1 percentage points increases in the probabilities of ever worked for positive income and worked in formal sector respectively. The coefficients are statistically significant at the 1 percent level. The expansion also improves individuals' probability of working full-time and year-round: a 10-percentage-point increase in the intensity of the expansion is associated with a 3.3-percentage-point increase in the probability of working full-time and year-round during 2013. The expansion also has positive impacts on earnings: a 10-percentage-point increase in expansion intensity boosts hourly wage in 2013 by 5.1 percent (that is approximately 1 RMB according to the sample mean hourly wage of 18 RMB). These two latter coefficients are statistically significant at the 5 percent level.

4.3. Falsification Tests and Robustness Checks

The validity of my method relies on two major assumptions. First, pre-treatment trends in education and labor market outcomes should be parallel for provinces with different expansion intensity. Second, we also need to ensure that there are no post-expansion omitted variables that are correlated with both the treatment intensity and outcomes of interest since confounding influences might happen just at or after the expansion. Below I conduct a series of checks to test for these assumptions.

4.3.1. Falsification Tests

In Appendix C, I replicate Tables 2 and 3 using two survey rounds that were conducted before the expansion (i.e., CHIP 1988 & 1995) [59,60], and using two before-expansion cohorts (i.e., those who are scheduled to attend college during 1985–1991 and those during 1992–1998) to probe the parallel pre-trends hypothesis. As Table A4 indicates coefficients on none of the education or labor market outcomes are statistically significant at the 10 percent level, and magnitudes of many coefficients are very close to zero, and the signs for some coefficients are negative.

These results suggest that the growth trajectories in education and labor market outcomes during the pre-expansion period (from 1988 to 1995) are not systematically different for the high- and low- expansion provinces. Similarly, the 1992–1998 college cohorts (relative to the 1985–1991 college cohorts) in high-expansion provinces show no discernible differences in educational attainment, employment probability, employment stability, and earnings than those from low-expansion provinces.

Results from my falsification tests suggest that the divergence in educational outcomes after the expansion is unlikely to be correlated with pre-existing trends in education between provinces that experience high and low expansion intensity, but due to the policy change in 1999. Likewise, differences in labor market outcomes between either two untreated survey years or two untreated cohorts do not vary across provinces with different "expansion intensity" and there are no labor market effects for whom the expansion has no educational effects. These results are reassuring because it suggests that the statistically significant and positive labor market effects presented in Table 3 for the more intensively treated individuals should work through the channel of expansion-induced increases in educational attainment.

4.3.2. Robustness Checks

Even though Figure 2 shows that there were no systematic inter-provincial differences in the changes of college admission ratio before the expansion, and the falsification tests above suggest that there were no differential pre-trends in education and labor market outcomes across provinces with different degrees of expansion intensity, one could still argue that confounding factors that simultaneously affect the province-cohort level expansion intensity and the outcomes emerged just at or after the expansion. In other words, there might be some time-varying province-level variables that are correlated with both the expansion intensity and outcomes in 2013.

For this concern, I test for a series of potential province-year level confounding factors and concurrent events that might correlate with both the treatment and outcomes, and add them as covariates into Equation (1) using the following Equation (2), and thus using only the exogenous part of variation in the intensity measure to identify the expansion's causal effects on education and labor market outcomes:

$$Y_{i,p,c} = c_1 + \alpha Post_c * Intensity_{p,\,c} + \beta Cohort_c + \gamma Province_p + \delta Covariate_{p,\,t} + \theta X_i + \varepsilon_{i,\,p,\,c} \quad (2)$$

where $Covariate_{p,\,t}$ is a vector of time-varying province-year level confounding factors for post-expansion years (i.e., 1999–2012), including annual growth rate of per capita GDP, unemployment rate, and a dummy variable indicating whether province p at year t has an above-national-mean proportion of export and import industries and thus more strongly affected by another influential concurrent reform (i.e., China's accession into the World Trade Organization in late 2001).

Table 4 below compares results with and without post-expansion time-varying province-level covariates. Columns 1 and 2 in Table 4 repeat results on the expansion's education and labor market effects presented in Tables 2 and 3; Columns 3 and 4 present corresponding estimates using Equation (2) that further controls for time-varying provincial covariates.

Table 4. Robustness checks: Effects of the expansion on education and labor market outcomes, with and without time-varying provincial covariates.

Outcomes	**[1] Raw Coefficient**		**[2] 10 pp. Increase in "Intensity"**		**[3] Raw Coefficient**		**[4] 10 pp. Increase in "Intensity"**	
First Stage–Education Outcomes:								
Years of schooling completed	2.9	***	0.3	***	3.4	***	0.3	***
	(0.9)		(0.1)		(1.2)		(0.1)	
Ever attended any regular HEIs	0.652	***	0.065	***	0.681	***	0.068	***
	(0.152)		(0.015)		(0.194)		(0.019)	
Obtained any postsecondary degrees	0.549	***	0.055	***	0.611	***	0.061	***
	(0.148)		(0.015)		(0.189)		(0.019)	
Reduced Form–Labor Market Outcomes:								
Probability of working and earning positive income	0.386	***	0.039	***	0.313	**	0.031	**
	(0.117)		(0.012)		(0.151)		(0.015)	
Probability of working in formal sectors	0.408	***	0.041	***	0.358	*	0.036	*
	(0.150)		(0.015)		(0.192)		(0.019)	
Probability of working full-time and year-round	0.334	**	0.033	**	0.351	*	0.035	*
	(0.160)		(0.016)		(0.205)		(0.021)	
ln(Hourly wage)	0.507	**	0.051	**	0.390	*	0.039	*
	(0.226)		(0.023)		(0.227)		(0.023)	
Controls:								
Incl. female, rural *hukou*, being only-child in family	YES				YES			
Incl. cohort and province fixed effects	YES				YES			
Incl. post-expansion province-year level covariates	NO				YES			
Number of observations	8377							

Robust standard errors clustered at the province level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

For all the three education outcomes, magnitudes of the coefficients become 4–17 percent larger after controlling for additional confounders. These coefficients remain statistically significant at the 1 percent level. With regards to labor market outcomes, magnitudes of the majority coefficients shrink (by 12–23 percent) after controlling for additional covariates. Whereas magnitude of the expansion's effect on the probability of working full-time-year-round becomes slightly (5 percent) higher after including additional covariates. Coefficients on all the four labor market outcomes remain statistically significant at least at the 10 percent level. In a word, Table 4 shows that the expansion's positive effects on educational attainment, employment probabilities, job stability, and earnings remain quite robust, though the estimated education effects become more pronounced, and the labor market effects generally shrink after further controlling for time-varying province-level confounding factors.

4.4. Estimating Causal Returns to College

Apart from estimating the expansion's reduced-form impacts, in this section I show that this policy-induced variation in expansion intensity can be used as an instrument for educational attainment to estimate the labor market returns to education. More specifically, after controlling for cohort and province fixed effects, the interaction between dummy variables indicating whether individuals attend college after the expansion (i.e., $Post_c$) and the province-cohort level expansion intensity measure (i.e., $Intensity_{p,\,c}$) is a plausible exogenous variable for education. In practice, I use the interaction (i.e., $Post_c * Intensity_{p,\,c}$) as an instrument for the following three endogenous variables: years of schooling completed, the probability of attending any HEIs, and the probability of obtaining any post-secondary degrees, and use the following 2SLS models to estimate the labor market returns to an additional year of schooling, college attendance, and post-secondary degree attainment:

$$\widehat{Edu}_{i,p,c} = c_1 + \pi_1 Post_c * Intensity_{p,\,c} + \pi_2 Cohort_c + \pi_3 Province_p + \pi_4 X_i, \tag{3}$$

$$Y_{i,p,c} = c_2 + \alpha * \widehat{Edu}_{i,p,c} + \beta Cohort_c + \gamma Province_p + \theta X_i + \varepsilon_{i,\,p,\,c} \tag{4}$$

Dependent variable in Equation (3) ($\widehat{Edu}_{i,p,c}$) is predicted education outcomes due to the treatment (i.e., expansion-induced increases in college admission spots), including years of completed schooling, and the probabilities of attending college and obtaining any post-secondary degree. Dependent variable in Equation (4) ($Y_{i,p,c}$) is labor market outcomes in 2013. All variables are defined in the same way as in Equation (1). Note that the 2SLS estimator (α) of the returns to education is roughly the ratio between the reduced-form estimator of the expansion's labor market effects divided by the first-stage estimator of the expansion's effects on education.

Likewise, for fear of potential confounding influences and as a robustness check, I also add post-expansion province-year level covariates as explained in Section 4.3.2 to models (3) and (4) using the following equations:

$$\widehat{Edu}_{i,p,c} = c_1 + \pi_1 Post_c * Intensity_{p,\,c} + \pi_2 Cohort_c + \pi_3 Province_p + \pi_4 Covariate_{p,\,t} + \pi_5 X_i, \tag{5}$$

$$Y_{i,p,c} = c_2 + \alpha \widehat{Edu}_{i,p,c} + \beta Cohort_c + \gamma Province_p + \delta Covariate_{p,\,t} + \theta X_i + \varepsilon_{i,\,p,\,c} \tag{6}$$

Table 5 below presents the 2SLS estimators (α) estimated using Equations (4) and (6). More specifically, Column 1 presents 2SLS estimates of the effects of one additional year of schooling on labor market outcomes, such as employment probabilities, employment stability, and earnings, without any time-varying province fixed-effect. Column 2 shows the same sets of results after adding post-expansion province-year level covariates. Columns 3 and 5 present 2SLS estimates of the effects of college attendance and postsecondary degree attainment on the same set of labor market outcomes respectively, without additional province-year level covariates, and Columns 4 and 6 list the corresponding results when including these additional covariates.

Table 5 shows that expansion-induced improvement in education boosts employment outcomes. For example, an additional year of schooling increases the probability of working for positive income by 9–13 percent, with and without additional covariates. Attending and completing college have larger positive impacts on employment probability: attending college increases the probability of working for positive income by approximately 46–59 percent, and obtaining any postsecondary degrees raises this employment probability by roughly 51–71 percent. More schooling also leads to more stable jobs: for example, having obtained any postsecondary degrees increases the probability of working in formal sectors and working full-time and year-round by 60–76 and 57–61 percent respectively. Coefficients on all the employment outcomes are statistically significant at least at the 10 percent level.

Table 5. 2SLS estimates of the effects of expansion-induced increases in college admission spots on labor market outcomes, with and without time-varying provincial covariates.

Outcomes:		[1]		[2]	[3]	[4]	[5]	[6]
				Instrumented:				
		Years of Schooling Completed				**Attended Any HEIs**		**Obtained Any Postsecondary Degree**
Instrument(s): province-cohort level expansion intensity		Durbin-Wu-Hausman (p-value) [1]	First-stage F (p-value) [2]					
Probability of working and earning positive income	0.134 ***	8.151	9.874	0.091 **	0.592 ***	0.460 ***	0.705 ***	0.513 **
	(0.045)	(0.004)	(0.002)	(0.043)	(0.213)	(0.230)	(0.268)	(0.258)
Probability of working in formal sectors	0.141 **	4.363	9.874	0.104 **	0.625 ***	0.526 **	0.761 **	0.595 **
	(0.061)	(0.037)	(0.002)	(0.052)	(0.242)	(0.254)	(0.308)	(0.297)
Probability of working full-time and year-round	0.116 **	3.685	8.447	0.102 *	0.512 **	0.516 **	0.606 **	0.573 **
	(0.059)	(0.055)	(0.004)	(0.058)	(0.257)	(0.256)	(0.302)	(0.286)
ln(Hourly wage)	0.188 **	3.117	10.562	0.111 **	0.762 **	0.537 **	0.897 **	0.587 *
	(0.089)	(0.082)	(0.003)	(0.056)	(0.334)	(0.268)	(0.403)	(0.309)
Controls:								
Incl. female, rural *hukou*, being only-child in family		YES		YES	YES	YES	YES	YES
Incl. cohort and province fixed effects		YES		YES	YES	YES	YES	YES
Incl. post-expansion province-year level covariates		NO		YES	NO	YES	NO	YES
Number of observations					8377			

[1] p-values for the Durbin-Wu-Hausman tests are at least less than 0.1 for all my models, indicating that the education variables are endogenous. [2] The F statistics for the joint significance of all the instruments are around 10, and the corresponding p values are at least less than 0.01 for all the models, implying that the instruments are sufficiently strong. Robust standard errors clustered at the province level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

With regards to earnings, the last row of Table 5 indicates that the 2SLS estimates of the rate of return to one additional year of schooling are roughly 19 percent without additional controls, and 11 percent after controlling for time-varying province fixed-effect. Similarly, the 2SLS estimators of the returns to attending college are around 54–76 percent, and the returns to obtaining any post-secondary degrees are about 59–90 percent. These coefficients are all statistically significant at least at the 10 percent level. Note that these 2SLS estimators are a weighted average of the returns to education for those who are affected by the instruments (i.e., who attended college due to the expansion, and would not attend college in the absence of the expansion). If the returns to education are not constant among individuals, these estimates for the "treated" individuals may not equal to the sample (or population) average returns to education [63].

I also conduct various post-estimation tests for my 2SLS models. Table 5 reports results from the Durbin-Wu-Hausman test and the weak IV test for model 1 (test results are nearly identical for columns 2–6 and are available upon request). The p-values for the Durbin-Wu-Hausman tests are at least less than 0.1 for all my models, indicating that the education variables (i.e., years of schooling, college attendance and degree attainment) are endogenous. The first-stage F statistics for the joint significance of all the instruments are around 10, and the corresponding p values are at least less than 0.01 for all the models, implying that the instruments are sufficiently strong.

5. Discussion and Conclusions

In this study, I provide new evidence on the enduring question of whether and to what extent attending and completing college affects an individual's subsequent labor market outcomes. I take advantage of an abrupt and large-scale higher education expansion program to isolate exogenous changes in the probability of attending college that are uncorrelated with unobserved determinants of an individual's labor market outcomes. Results suggest that the expansion substantially boosts individuals' education and labor market outcomes. A 10-percentage-point increase in the fraction of 18-year-olds who could *access* any regular HEIs (or one additional college admission spot for every ten 18-year-olds) could translate to approximately 0.3 additional years of schooling, 7 and 6 percentage points increase in college attendance and postsecondary degree attainment respectively. The 10-percentage-point increase in college access would eventually contribute to rises in employment probabilities by 3–4 percentage points and hourly wage by 4–5 percent.

On top of estimating the expansion's reduced-form effects, I further show that this policy-induced variation in expansion "intensity" can be used as an instrument for educational attainment, and thus to estimate the causal returns to education. My 2SLS estimators imply that returns to education remain substantial in 2013, that is 14 years after the higher education expansion program was first initiated: the rate of return to one additional year of schooling is 11–19 percent and returns to attending college and obtaining any postsecondary degrees are 54–76 and 59–90 percent respectively.

I perform extensive sets of falsification and robustness tests to validate my identification strategy. These checks provide additional confidence that the expansion's positive impacts on education, employment, and earnings are not primarily driven by endogenous province-specific trends in expansion intensity or by other contemporaneous policies.

Findings from my analysis and numerous previous studies show that attending and completing college is associated with higher earnings and better employment prospects even during eras when the higher education sectors are expanding. My estimated returns to college that exploits cohort-province level variation is slightly higher than returns estimated by Hu [44] who uses cohort-level variation in college attendance. Moreover, my IV estimates of the rate of return to education for expansion-affected cohorts (i.e., 11–19 percent) also align with previous findings that use similar quasi-experimental methods to evaluate higher education expansions in Canada [17], France [12] and Russia [11].

Lastly, I address some major concerns for the interpretation of my results, including potential changes in education quality, selective migration, and education-job mismatch. One major concern with the expansion is that the quality of college education might change after expansion. If this is the case, then the expansion's education returns would capture a combination of increased quantity and decreased/increased quality of education. Particularly, changes in education quality are likely to be different for HEIs of different tiers and types (e.g., public flagship universities vs. private 3-year community colleges). Future work that can collect data on measures like student-faculty ratio and per-student expenditure at the institution or tier level could assess the expansion's heterogeneous effects on quality of education by institutional type.

The other concern with my analysis is selective migration. As explained in Section 3, I determine an individual's exposure to the expansion based on their *hukou* province at 14 years old. In practice, individuals could change their *hukou* province between 14 and before they take the CEE (therefore generating measurement error in the expansion "intensity" variable). Besides, individuals could migrate to other provinces to attend college and/or to work after graduation, and this selective migration may generate labor market premiums on top of the returns to human capital accumulated from attending and completing college. To probe these concerns, I crosscheck among the three province variables and results show that migration across provinces is rather rare for the CHIP 2013 sample: among those who have information on where they took the CEE, only 1.8 percent take the CEE in a different province than their *hukou* province at 14 years old; 2.4 percent out of the full sample reside or work in a different province in 2013 than their *hukou* province at 14. I re-run Tables 2, 3 and 5 excluding these individuals who ever changed their *hukou* province after 14 years old. Coefficients on key variables seldom change after excluding these migrants, and there is no sign showing that migrants in the CHIP 2013 sample are systematically different in any of the characteristics presented in Table 1 from non-migrants.

The last concern is about mismatch between education qualification employees possess and the skills and knowledge required by their jobs and occupations. Matching workers' educational qualification using the International Standard Classification of Education (ISCED) with their occupations using the International Standard Classification of Occupations (ISCO), one recent study [64] finds a rise in education-job mismatch in the labor market for EU member countries between 2000 and 2019. The author examines this (mis)match's long-run effects on per-capita gross domestic product (GDP) growth and finds that over-education among college-educated workers exhibits a negative or statistically insignificant impact on GDP growth. Whereas higher education-job match rates among both college- and high school-educated workers are conductive to economic growth. A promising direction for future research is to explore the degree to which education and occupations are (mis)matched in the Chinese context to see whether human capital acquired in school is fully utilized in jobs, and how it affects individuals' returns to education and the nation's sustainable economic growth.

In sum, this study reinforces that enhanced access to college could raise individuals' educational attainment and boost nations' average level of human capital, and eventually benefit their labor market outcomes. Since the estimated college premium also equals to the wage gap between college- and high school-educated labor, my work also has implications for education-related income inequality in the labor force. In respond to the growing number of student enrollment in higher education, and at the same time to maintain or even enhance education quality and improve equity in college access, countries need to build sustainable and more efficient higher education financing systems. Findings from my analysis on the magnitudes of the returns to education could provide information to guide individual decision on human capital investment, help firms to facilitate efficient allocation between physical and human capital, and help government to design effective and equitable higher education, labor market, tax, and welfare policies, which in turn could promote more sustained economic growth and more equitable social development.

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Appendix A. DiD Estimators of the Effects of the Expansion on Education and Labor Market Outcomes

Table A1. DiD estimators of the expansion's effects on educational outcomes.

	[1] Years of Schooling Completed	[2] Ever Attended Any Regular HEIs	[3] Obtained Any Post-Secondary Degrees
Post ∗ High	0.4 ***	0.075 ***	0.064 ***
	(0.1)	(0.022)	(0.021)
Post-expansion cohorts (*Post* = 1)	1.6 ***	0.219 ***	0.226 ***
	(0.1)	(0.015)	(0.014)
High expansion intensity provinces (*High* = 1)	0.3 ***	0.021 *	0.023 **
	(0.1)	(0.011)	(0.011)
Control Variables:			
Female	−0.4 ***	−0.062 ***	−0.059 ***
	(0.1)	(0.016)	(0.015)
Rural *hukou*	−0.6 ***	−0.029 **	−0.026 *
	(0.1)	(0.014)	(0.014)
Female ∗ Rural	−0.4 ***	−0.067 ***	−0.064 ***
	(0.1)	(0.020)	(0.019)
"Only-child" in family	0.6 ***	0.067 ***	0.060 ***
	(0.1)	(0.014)	(0.014)
Female ∗ "Only-child" in family	0.3 *	0.054 ***	0.051 ***
	(0.1)	(0.019)	(0.019)
Number of observations	8377	8377	8377

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2. DiD estimators of the expansion's effects on labor market outcomes.

	[1] Ever Worked & Earned Positive Income	[2] Ever worked in Formal Sectors	[3] Worked Full-Time-Year-Round	[4] Log of Hourly Wage
Post ∗ High	0.044 *	0.054 **	0.039 *	0.031 *
	(0.017)	(0.022)	(0.023)	(0.029)
Post-expansion cohorts (*Post* = 1)	0.008	0.043 ***	−0.011	−0.070 ***
	(0.011)	(0.014)	(0.015)	(0.022)
High expansion intensity provinces (*High* = 1)	0.031 ***	0.045 ***	0.076 ***	0.111 ***
	(0.009)	(0.011)	(0.012)	(0.017)
Control Variables:				
Female	−0.194 ***	−0.160 ***	−0.154 ***	−0.236 ***
	(0.012)	(0.016)	(0.017)	(0.024)
Rural *hukou*	0.016	−0.016	−0.037 **	−0.005
	(0.011)	(0.014)	(0.015)	(0.021)
Female ∗ Rural	−0.034 **	−0.049 **	0.037	−0.086 ***
	(0.015)	(0.019)	(0.021)	(0.030)
"Only-child" in family	−0.027 **	−0.014	0.005	0.053 **
	(0.011)	(0.014)	(0.015)	(0.022)
Female ∗ "Only-child" in family	0.058 ***	0.065 ***	0.038 *	0.074 **
	(0.015)	(0.019)	(0.021)	(0.029)
Number of observations	8377	8377	8377	8377

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The DiD estimators in the above two tables can be interpreted as the causal effects of the expansion on education and earnings, under the assumption that without the expansion, changes in educational attainment and earnings during the prior- to post-expansion period would not be systematically different in the high- and low-expansion intensity groups. The magnitudes of the DiD estimators imply that for individuals young enough to attend college after the expansion and belong to provinces and cohorts that experience above national-average increases in college admission quota, they receive on average 0.4 more years of schooling, and have 7.5 and 6.4 percentage points higher probabilities of attending college and obtaining any postsecondary degrees respectively. These DiD estimates on the educational effects are slightly larger than those estimated using a continuous measure of expansion intensity together with cohort- and province- level fixed effects (as reported in Table 2 and the upper panel of Table 4). Whereas the DiD estimator of the earnings effect is 3.1 percent (i.e., individuals young enough to attend college after the expansion and belong to provinces and cohorts that experience above national-average increases in college admission quota, on average, earn 3.1 percent more), which is slightly lower than that presented in Table 3 and the bottom panel of Table 4 that use a continuous measure of expansion intensity together with cohort- and province- level fixed effects. Coefficients on all the DiD estimators in the above two tables are statistically significant at least at the 10 percent level.

Appendix B. How the College Expansion Affects Individuals' Highest Educational Level Completed?

Table A3. Effects of the expansion on highest educational level completed.

Outcomes	[1] Raw Coefficient	[2] 10 pp. Increase in "Intensity"	[3] Raw Coefficient	[4] 10 pp. Increase in "Intensity"
Highest Educational Level Completed:				
Completed lower secondary school only, but no high school at all	−0.178 (0.156)	−0.018 (0.016)	−0.317 (0.201)	−0.032 (0.020)
Attended some high school, but no high school diploma	−0.003 (0.021)	0.000 (0.002)	−0.001 (0.027)	0.000 (0.003)
Obtained any high school diploma only (incl. academic or vocational diploma), but no college at all	−0.460 *** (0.163)	−0.046 *** (0.016)	−0.350 * (0.209)	−0.035 * (0.021)
Attended some college, but no post-secondary degree	0.089 * (0.046)	0.009 * (0.005)	0.055 * (0.027)	0.006 * (0.003)
Obtained any post-secondary degrees (incl. associate's, bachelor's and graduate degrees)	0.552 *** (0.148)	0.055 *** (0.015)	0.613 *** (0.188)	0.061 *** (0.019)
Controls:				
Incl. female, rural *hukou*, being only-child in family	YES		YES	
Incl. cohort and province fixed effects	YES		YES	
Incl. post-expansion province-year level covariates	NO		YES	
Number of observations	8377			

Robust standard errors clustered at the province level are in parentheses. * $p < 0.1$, *** $p < 0.01$.

Appendix C. Falsification Tests

Ideally, I would like to construct a long panel of province-cohort level education and earnings outcomes before 1999 to test the parallel pre-trends hypothesis. However, such data are unavailable. Instead, I provide evidence using two methods to show that there are no differential pre-trends in education and labor market outcomes for high and low expansion intensity provinces either across two before-expansion survey years or two before-expansion cohorts.

For the first test, there are two rounds of the CHIP surveys that were conducted before the college expansion (i.e., CHIP 1988 and CHIP 1995) [59,60]. I append these two pre-expansion surveys to explore whether the growth trajectories in educational attainment, employment and earnings from 1988 to 1995 follow similar temporal trends between the high and low expansion intensity provinces using the following equation:

$$Y_{i,p,s} = c_1 + \alpha_1 Survey95_s * High_p + \beta_1 Survey95_s + \gamma_1 Province_p + \theta_1 X_i + \varepsilon_{i,\ p,s} \quad \text{(A1)}$$

where $Y_{i,p,s}$ is the education or labor market outcomes for individual i from province p and survey round s; $Survey95_s$ indicates whether an individual is from the CHIP1995 survey (it equals zero if an individual is from the CHIP1988 survey); I separate provinces for pre-expansion years into either a "high" or "low" expansion intensity group, depending on whether their average $Intensity_p$ for post-expansion years is above or below the nationwide average $Intensity_{nationwide}$ during the same years (i.e., 1999–2008). $High_p$ equals one if an individual's province belongs to the "high" expansion intensity group, and zero if the individual comes from a low expansion intensity province. Other variables are defined the same way as in Equation (1) in the main text. Similarly, the coefficient of interest is α_1, which suggests whether individuals from high expansion intensity provinces enjoy larger increases in education and labor market outcomes from 1988 to 1995 than those from low expansion intensity provinces. Column 1 of Table A4 below presents regression results of the DiD estimators of the expansion's effects on education, employment, and earnings.

Table A4. Falsification tests: DiD estimators of the expansion's education and labor market effects using two pre-expansion survey rounds (i.e., CHIP 1988 & 1995) and two pre-expansion college cohorts (i.e., 1985–91 vs. 1992–98).

Outcomes	**[1] Using Two Pre-Expansion Survey Rounds** [1]		**[2] Using Two Pre-Expansion Cohorts** [2]	
	DiD Estimators		**DiD Estimators**	
Education Outcomes:				
Years of schooling completed	0.0		0.0	
	(0.0)		(0.0)	
Ever attended any regular HEIs	−0.001		−0.002	
	(0.001)		(0.004)	
Obtained any postsecondary degrees	---	---	0.001	
	---	---	(0.004)	
Labor Market Outcomes:				
Probability of working and earning positive income	0.001		0.000	
	(0.001)		(0.002)	
Probability of working in formal sectors	---	---	0.000	
	---	---	(0.004)	
Probability of working full-time and year-round	---	---	0.001	
	---	---	(0.004)	
ln(Hourly wage)	---	---	0.005	
	---	---	(0.006)	
ln(Total annual income)	0.001		---	---
	(0.002)		---	---
Number of observations	18,435		3519	

[1] Data source: CHIP 1988 and CHIP 1995. [2] Data source: CHIP 2013. Model 1 controls for province fixed effects, a female dummy, a rural *hukou* dummy, a dummy indicating whether the individual is the "only-child" in family, and two interactions (female ∗ rural and female ∗ only-child); Model 2 controls for cohort and province fixed effects, a female dummy, a rural *hukou* dummy, a dummy indicating whether the individual is the "only-child" in family, and two interactions (female ∗ rural and female ∗ only-child). Robust standard errors clustered at the province level are in parentheses.

The DiD estimators for none of the education and labor market outcomes in column 1 of Table A4 are statistically significant at the 10 percent level. Moreover, the magnitudes of the coefficients are all very close to zero with the signs of some being negative. These results prove that the growth trajectories in education and labor market outcomes during the pre-expansion period are not systematically different for the high and low expansion intensity provinces. (One caveat of the above analysis is that the CHIP 1988 and CHIP 1995 datasets contain fewer variables and less detailed information on education and labor market outcomes than the CHIP 2013 round. For example, CHIP 1988 does not ask for postsecondary degree attainment ("attended HEIs" "graduated from HEIs" and "obtained degree(s) from HEIs" are regarded as the same educational level as "college"); CHIP 1988 and 1995 do not have any information regarding the type of employment or quantity of labor supplied to construct some labor market outcomes reported in Table 3 in the main text. Nevertheless, results shown here suggest that there are no pre-existing trends at least in major education and labor market outcomes during pre-expansion period from 1988 to 1995).

For fear that the above test could not capture any trend between 1996 and 1998, or for any potential differences in sampling or variable construction methods between the CHIP 1988/1995 and CHIP 2013 datasets, I conduct another falsification test using the CHIP 2013 dataset. More specifically, I replicate the above DiD estimates using two before-expansion cohorts of individuals: those who are scheduled to attend college during 1985–1991 and those during 1992–1998 using the following equation:

$$Y_{i,p,s} = c_2 + \alpha_2 Cohort9298_c * High_p + \beta_2 Cohort_c + \gamma_2 Province_p + \theta_2 X_i + \varepsilon_{i,\ p,s} \tag{A2}$$

where $Y_{i,p,s}$ is the education or labor market outcomes for individual i from province p and survey round s; $Cohort9298_c$ indicates whether an individual would schedule to attend college during 1992 to 1998 (it equals zero if an individual is scheduled to attend college during 1985 to 1991). Other variables are defined the same way as in Equation (1) in the main text.

Regression results of the DiD estimators α_2 are presented in column 2 of Table A4 above. Coefficients on none of the education or labor market outcomes in column 2 of Table A4 are statistically significant at the 10 percent level. Moreover, magnitudes of many coefficients are very close to zero, and the signs for some coefficients are negative.

Combined, results from these two tests provide suggestive evidence that the observed positive educational and labor market effects for more intensively treated post-expansion cohorts presented in Tables 2 and 3 in the main text are not driven by some pre-trends that differentially affect the strongly and weakly expanded provinces in the absence of the higher education expansion program.

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