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Effects of cohort size on college premium: Evidence from China's higher education expansion

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ABSTRACT

In this paper, we document the lesser-known heterogeneous trends of college/non-college earnings premium across age groups from 1995 to 2013 in China. Specifically, the college premium in 2013 for the younger group (age 25–34) was about 30 percentage points, similar to the level in 1995, while the college premium in 2013 for the older group (age 45–54) increased to 50 percentage points, nearly double that of 1995. To attribute these divergent trends of the college premium to the changes in the relative size of college workers, we use the model by Card and Lemieux (2001), which incorporates imperfect substitution between similarly educated workers in different age cohorts. Due to the distinctions of these trends in China, our identification is free of the overestimation issue that the existing studies suffer. Our results are similar to those in the U.S., U.K., Canada, and Japan. Holding the age cohort and survey year constant, a one unit increase in log relative size of college workers is associated with about 10 percentage points decrease in college/non-college premium and about 18 percentage points decrease in college/high school premium. We further find that the negative effect is much more substantial among the new entrants (age 25–29) than experienced workers (age 30–54). By this pattern, we demonstrate that the new labor market entrants are more sensitive to their own cohort size and argue that the confounding ability composition effect should not be a serious issue.

1. Introduction

As a leading proximate cause of rising overall earnings inequality since the 1980s in the U.S., the increase in the college/high school wage premium has been well documented. Authors such as Katz and Murphy (1992), Acemoglu (2002), and Autor et al. (2008) have explained the rise as the consequence of an accelerated rise in the relative demand for college graduates and an abrupt slowdown in the growth of the relative supply of college graduates.[1] These studies focus on the aggregate trend of the college wage premium that may conceal independent trends by age groups. Card and Lemieux (2001) argue that heterogeneous trends of college premium by age groups may arise if workers in different age groups within the same education group are imperfectly substitutable and the trends of the

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[1] It is argued that the increase may have been driven by skill-biased technological change (SBTC) featured by the computer revolution and the outsourcing of manufacturing. Katz et al. (1999) and Autor et al. (2008) support the idea of SBTC, and Feenstra and Hanson (2001) support the idea of outsourcing. The growth of college graduation rates stagnated for cohorts born in the early 1950s and entered the labor market in the late 1970s. See Card and Lemieux (2001) for details.

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relative supply of college workers are heterogeneous by age groups. Using data from the United States, the United Kingdom and Canada, they demonstrate the imperfect substitution between age groups and attribute the observed relative rise in the college premium for younger workers since the early or mid 1980s to the stagnated growth of the relative supply of college-educated workers among the young during the same periods.[2] Walker and Zhu (2008) find that the college premium for younger workers stagnated along with the fast-growing higher education participation rate, focusing on the more recent period from 1994 to 2006 with the U.K. data. However, little evidence from other countries has been added until recently. Kawaguchi and Mori (2016) reveal the heterogeneous trends of the college premium by age groups between 1986 and 2008 in Japan. Our paper adds evidence to this literature by documenting the divergent trends of college premium by age groups between 1995 and 2013 in China, and examines how the college premium is affected by the age group specific relative size of college-educated workers.[3]

In the two studies of the U.S., U.K., Canada, and Japan, a vital identification issue arises: the college-educated population's relative size is likely responsive to the college premium. Identification typically rests upon exclusion restrictions for instrumental variables. China presents a unique environment where the decision of who obtains a college degree is determined by a national college entrance examination (NCEE) and a unique experience of higher education expansion. The NCEE was restored in 1977 after a 10-year suspension during the Cultural Revolution from 1966 to 1976. Fig. 1 depicts China's higher education expansion from 1977 to 2012. The college admissions increased gradually from 0.27 million in 1977 to 1.08 million in 1998. The number of NCEE takers dropped rapidly from over 6 million in 1978 to 1.67 million in 1983, then increased to 3.2 million till 1998.[4] In 1999, the Chinese government launched a mass expansion which led to a 7-fold increase in college admissions and a 3-fold increase in NCEE takers over a decade. The college admission rate saw a substantial increase and exogenous variation from 5 to 75 percent from 1977 to 2012. Hence the Chinese experience embeds a natural experiment allowing for arguably exogenous determination of college attainment.

Further, the identification for the four countries relies on the relative rise in college premium for younger workers since the early or mid 1980s and the associated relative slowdown in the relative supply of college workers among the young. This timing overlapped with the emergence of skill-biased technological change (SBTC) since the early 1980s with the onset of the computer revolution. And it is suggested that this computer-driven technological change may increase the relative demand for college workers and further increase the college premium among the young in particular (Krueger, 1993; Card and Lemieux, 1999; Freeman and Katz, 2007).[5] Therefore, the negative effect of age group specific relative size on age group specific college premium may have been confounded by SBTC and overestimated for the four countries. The distinct trends of college premiums and relative size of college workers during our study period of 1995 to 2013 in China allow for a probably underestimated magnitude of the negative cohort size effects. Finally, China is also worth examining due to its large population and workforce.

Using China Household Income Project (CHIP) 1995, 1999, 2002, 2007, and 2013, five repeated cross-sectional surveys, we find that the trends of the college premium between 1995 and 2013 by age groups are substantially different. In Fig. 2(a), the college premium as measured by log earnings ratio was very similar for younger (age 25–34) and older (age 45–54) groups, about 25 percentage points in 1995. As of 2013, the college premium for the younger group was about 30 percentage points, similar to the level in 1995, while the college premium for the older group was about 50 percentage points, nearly double that of 1995. In Fig. 2(b), we present the age group specific trends of the relative supply of college workers measured as log employment ratio. The relative supply for the younger group increased substantially while the older group was quite stable during the same period. Comparing these two figures, the stagnation of the college premium for the younger group between 1995 and 2013 was potentially due to the fast-growing relative supply of college workers. Figs. 3 and 4 show that in the U.S. and Japan, unlike in China, the college premium for the older group decreased relative to the younger group while the supply for the older group increased relative to the younger group.[6] If technological progress positively affects the college premium for the younger group particularly as the literature argues, the negative age group specific supply effects will be overestimated for the U.S. and Japan, and underestimated for China.

The underlying cause of the heterogeneous trends of relative supply by age groups is the non-monotonic increase in the college attendance rate determined by college capacity and birth cohort size. The expansion of college attendance ended in 1965 in the U.S. and 1975 in Japan.[7] Therefore, Card and Lemieux (2001) and Kawaguchi and Mori (2016) mainly study the post-expansion period for the U.S. and Japan.[8] In China, the growth in college attendance began in 1977 and did not slow down until 2008. This paper studies the period 1995–2013 which covers the expansion. Thus, this paper reveals the consequence of an ongoing college attendance expansion, supplementing previous studies on the consequence of past college attendance expansion.

In this paper, we follow the empirical strategy by Card and Lemieux (2001) to construct the college premium and relative supply by

[2] The relative rise in college premium for younger workers commenced 5 years later in the U.K. and Canada than in the U.S.

[3] Considering that there is a certain amount of workers below high school education in China, we focus on the college premium with respect to non-college workers. Results for the college/high school premium will also be discussed and compared with existing studies.

[4] The larger numbers of NCEE takers in 1977 and 1978 addressed the fact that the NCEE was suspended for ten years from 1966–1976 during the Cultural Revolution. Those high school graduates who were supposed to pursue higher education took the NCEE right after it was restored.

[5] Card and Lemieux (1999) use relative computer usage rates of college workers as a proxy indicator of the relative complementarity of college workers with new technology and finds little evidence supporting this hypothesis. However, we have no evidence to reject the hypothesis, and it may be argued that the proxy indicator may have failed to capture the relative complementarity exactly.

[6] These two figures are taken from the paper by Kawaguchi and Mori (2016), who compare the trends between the U.S. and Japan. The original figures are in black and white. We adjust the colors and layouts to keep the figure consistency throughout this paper.

[7] The fast growth in college attendance rate ended for U.S. birth cohort 1947 and Japanese birth cohort 1957 approximately (Kawaguchi and Mori, 2016). And suppose the college-age is 18.

[8] Even though the period studied by Card and Lemieux (2001) is from 1959 to 1996, the identification relies on data in years later than 1975.

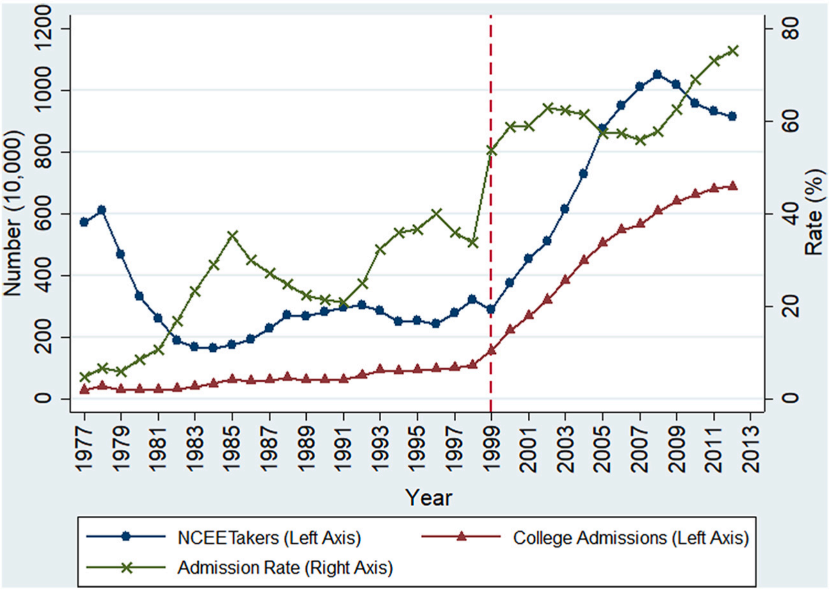

Fig. 1. The higher education expansion in China.

age and survey year and further regress the age-year cell-specific college premium against the relative supply. The supply effect on the college premium is estimated to be about -0.1 by our main specification. That implies, when holding the age cohort and survey year constant, a one unit increase in the log relative size of college workers is associated with about 10 percentage points decrease in the college premium. The more comparable result by focusing on the college/high school earnings premium is about -0.18, which is slightly lower than -0.2 in the U.S. and -0.23 in the U.K. while almost the same as the results for Japan and Canada. The negative supply effect in China is so close to the other four countries is remarkable given the very different economic development levels, trends of the college premiums and the relative supply, and higher education expansion phases between China and the other four countries. It is more interesting considering that the estimate of the supply effect should be a lower bound in China and an upper bound in the other four countries.

We further examine the heterogeneous supply effects by age groups and find that the entrant group between ages 25 and 29 is more substantially affected by their relative supply. This finding can be used to address the ability composition issue.[9] The ability effect is argued to be more substantial for the older group (Lillard, 1977). However, the estimated negative supply effects for the older groups are significantly lower than those for the entrant group. This implies that the ability composition effect is not dominant in the estimated supply effect, even if it may exist to some extent.

These findings contribute to the literature on the returns to education incorporating imperfect substitution between similarly educated workers across different age cohorts by presenting evidence from China, a fast-growing developing country with a large population and workforce. Furthermore, the uniqueness of China's case ensures the empirical analysis is free of the identification issues that the existing studies encounter (Card and Lemieux, 2001; Kawaguchi and Mori, 2016; Carneiro and Lee, 2011).

This paper also contributes to the literature on the labor market impacts of China's higher education expansion. Using census data sets, applying a difference-in-difference model, and treating the expansion in 1999 as a policy shock, Li et al. (2014) and Xing et al. (2018) find that the expansion policy increased the unemployment rate of new college graduates in the short run, but the impact mostly disappeared after 5 years. In addition to the similar short-run unemployment impacts, Wu and Zhao (2010) and Yu (2014) further find significant negative earnings impacts of the expansion policy on the young college graduates.[10] Using 2002 and 2007 waves of China Household Income Project and Urban Household Surveys 2002–2008, Knight et al. (2017) demonstrate that the higher education expansion reduced the employment opportunity and earnings for entry-period college graduates but had no significant effect on those incumbent college graduates. These studies reveal a comparable fact as our paper that the labor market new entrants are more easily affected by the increased cohort size due to the higher education expansion. However, they do not extend the analysis from the policy shock effects to the marginal effects of continuously changing cohort size until a recent study by Li et al. (2017). With Urban Household Surveys 1994–2009, the authors present similar divergent trends of college premiums across age groups as we depict with CHIP 1995–2013. Assuming a slightly different CES production function form, they propose that the college premium for senior

[9] It is argued that the increase in the relative supply of college workers might be associated with a decrease in the average ability gap leading to a decrease in the college premium.(Chay and Lee, 2000; Taber, 2001; Juhn et al., 2005; Carneiro and Lee, 2009, 2011) Thus, the negative supply effect tends to be overestimated.

[10] Wu and Zhao (2010) also use census data sets 2000 and 2005. Yu (2014) use 1997 and 2006 waves of China Health and Nutrition Survey.

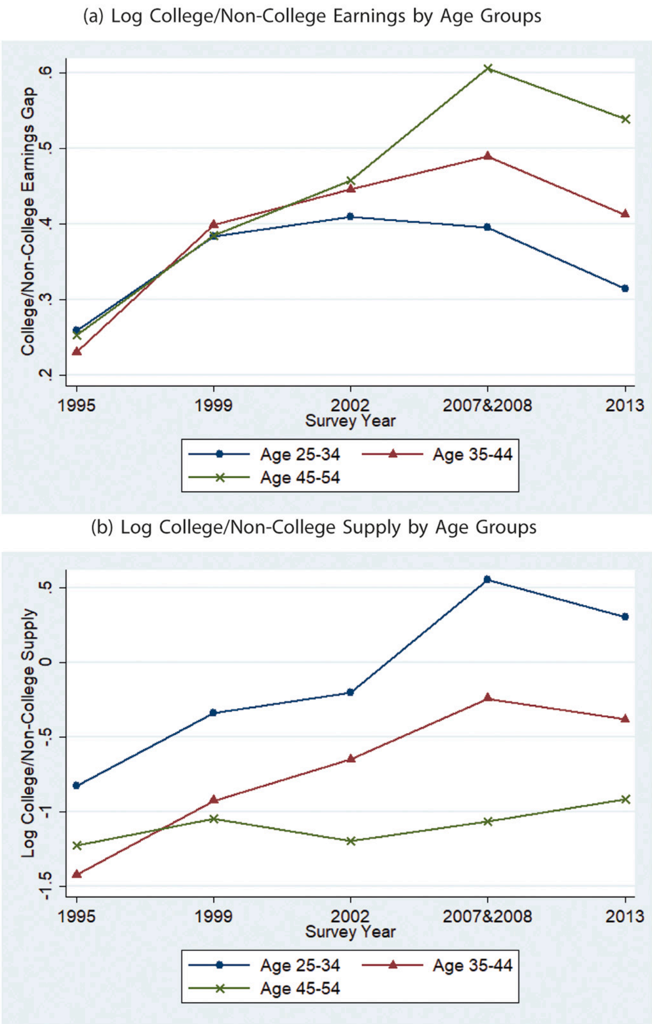

Fig. 2. Trends of college premium and relative supply of college workers by age groups: China. (a) Log College/Non-College Earnings by Age Groups. (b) Log College/Non-College Supply by Age Groups.

workers increases with the young college-educated workers. This proposition of complementarity is supported by the empirical results with China's data. However, it's hard to explain the cases of the U.S. and Japan by Figs. 3 and 4. Since the proposition is also based upon a CES production function, even though slightly different from Card and Lemieux (2001), it's not reasonable to argue that the imperfect substitution among workers of different ages does not hold in China context. The critical difference is that Li et al. (2017) examine how the young cohort size affects the college premium for the senior while our paper studies how the cohort size affects the college premium for the same cohort.

The rest of this paper is organized as follows. Section 2 presents the theoretical model by Card and Lemieux (2001). Section 3 discusses empirical strategy and potential identification issues. Section 4 introduces our data from China and details the trends of college/non-college earnings gap and relative supply of college workers. Section 5 presents the main results, and Section 6 reports a set of robustness checks. Finally, we conclude in Section 7.

2. Theoretical framework

2.1. Model setup

We start with a Cobb-Douglas aggregate production function that has been widely used in the macro-growth literature:

$$Y_t = A_t L_t^{\alpha} K_t^{1-\alpha} \tag{2.1}$$

where subscript t indexes year, Y_t is aggregate output, A_t is total factor productivity, L_t is aggregate labor force input, K_t is physical

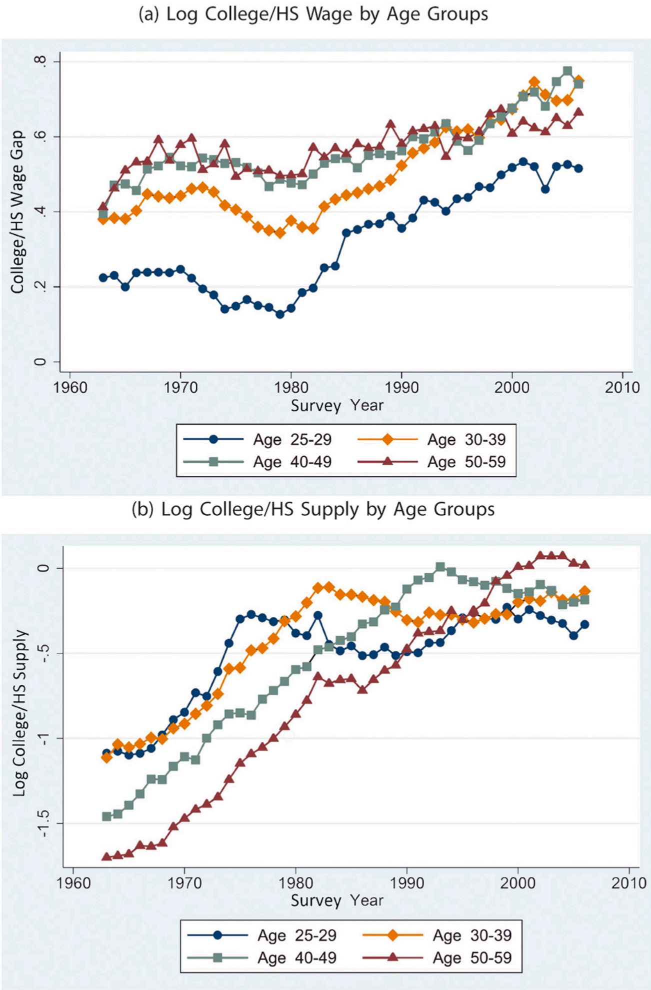

Fig. 3. Trends of college premium and relative supply of college workers by age groups: The U.S. (a) Log College/HS Wage by Age Groups. (b) Log College/HS Supply by Age Groups.

capital input, and α is the share of income allocated to the labor force.

Following the existing literature on the trend of wage differentials by education (Katz and Murphy, 1992; Autor et al., 2008), we assume the labor force input L_t in Equation (2.1) follows a CES aggregation of college and non-college labor

$$L_t = [\sum_s (\theta_{st} L_{st}^{\rho})]^{1/\rho} \tag{2.2}$$

where subscript s indexes education level which takes c for college labor and n for non-college labor, θ_{st} is the technological efficiency parameter, and $-\infty < \rho \leq 1$ is a function of the elasticity of substitution σ_A between college and non-college labor force ($\rho = 1 - 1/\sigma_A$). The underlying assumption is that different age cohorts within the same education group are perfect substitutes. To explain the divergent trends of the college premiums across age cohorts, following Card and Lemieux (2001), we relax the assumption of perfect substitution across age cohorts and further assume the labor force of each education level is aggregated by age cohorts by CES functional form

$$L_{st} = [\sum_j (\alpha_{sjt} L_{sjt}^{\eta_s})]^{1/\eta_s} \tag{2.3}$$

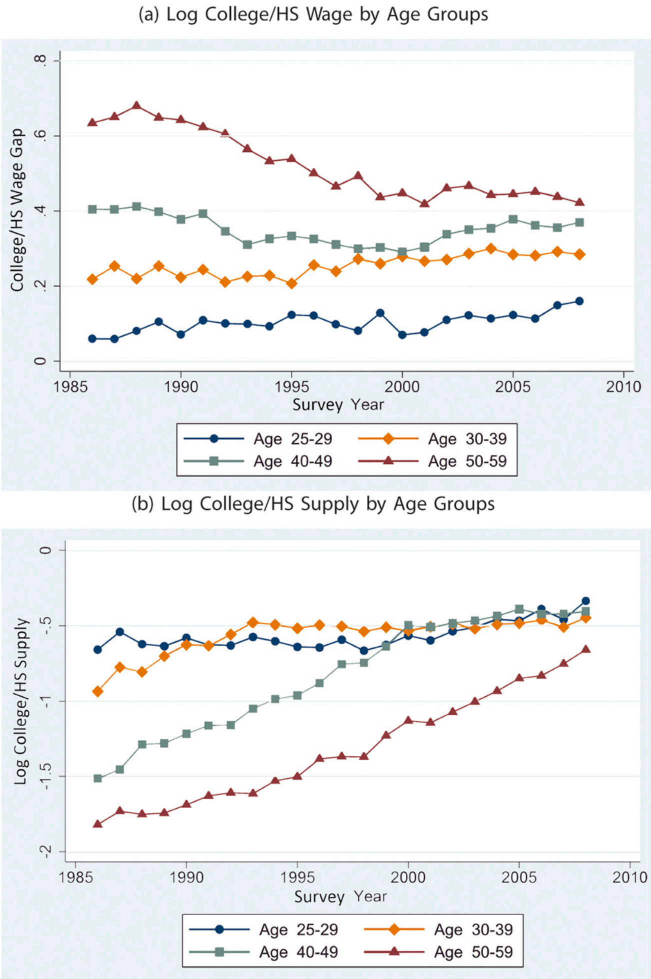

Fig. 4. Trends of college premium and relative supply of college workers by age groups: Japan. (a) Log College/HS Wage by Age Groups. (b) Log College/HS Supply by Age Groups.

where subscript j indexes age cohort, α_{sjt} is a relative efficiency parameter,[11] $-\infty < \eta_s \leq 1$ is a function of the elasticity of substitution σ_s among different age cohorts ($\eta_s = 1 - 1/\sigma_s$), and L_{sjt} is the size of the labor force for each education-age-year group.

2.2. *Profit-maximizing wage*

In this setup, assuming efficient utilization of labor force, we can derive the profit-maximizing wage of an average worker with education level s, among age cohort j, in year t as the value of corresponding marginal productivity in log form:

$$\log(w_{\mathrm{sjt}}) = \log(\Phi_t) + \log(\theta_{\mathrm{st}}) + (\frac{1}{\sigma_s} - \frac{1}{\sigma_A})\log(L_{\mathrm{st}}) + \log(\alpha_{\mathrm{sjt}}) - \frac{1}{\sigma_s}\log(L_{\mathrm{sjt}}) \tag{2.4}$$

where

$$\Phi_t = \alpha A_t K_t^{1-\alpha} L_t^{\alpha-\rho}$$

According to Equation (2.4), the age specific variation in wages is due to the age specific variation in the relative efficiency parameter α_{sjt} and the size of labor force L_{sjt}. The term $log(\Phi_t)$ represents a common year fixed effect across education levels while the terms $\log(\theta_{\mathrm{st}}) + (\frac{1}{\sigma_s} - \frac{1}{\sigma_A})\log(L_{\mathrm{st}})$ represents the year fixed effect for specific education level s. In this setup, the coefficient of $log(L_{sjt})$, $-1/$

[11] This relative efficiency parameter may be affected by labor complementarity with technology, skill composition, ability composition, etc. Card and Lemieux (2001) assume the relative efficiency parameter is constant over time. In our paper, we relax the strict assumption to allow for time variation, which will be helpful to explain potential identification issues.

σ_s, should be negative unless the labor forces are perfectly substitutable across age cohorts ($\sigma_s = \infty$).

2.3. Age specific relative size and college premium

It is straightforward to derive the college premium by taking the difference of the log wages between college and non-college labor force in terms of Equation (2.4),

$$\log(\frac{w_{\text{cjt}}}{w_{\text{njt}}}) = \log(\frac{\theta_{\text{ct}}}{\theta_{\text{nt}}}) + (\frac{1}{\sigma_c} - \frac{1}{\sigma_A})\log(L_{\text{ct}}) - (\frac{1}{\sigma_n} - \frac{1}{\sigma_A})\log(L_{\text{nt}}) + \log(\frac{\alpha_{\text{cjt}}}{\alpha_{\text{njt}}}) - \frac{1}{\sigma_c}\log(L_{\text{cjt}}) + \frac{1}{\sigma_n}\log(L_{\text{njt}}). \tag{2.5}$$

To simplify our explanation of the age specific college premiums, we assume that the extent of substitution across age cohorts is the same for the college and non-college labor force. That is, we assume $\eta_c = \eta_n = \eta$ (which is equivalent to $\sigma_c = \sigma_n = \sigma$). This assumption will be tested empirically. We can rewrite Equation (2.5) as:

$$\log(\frac{w_{\text{cjt}}}{w_{\text{njt}}}) = \log(\frac{\theta_{\text{ct}}}{\theta_{\text{nt}}}) + (\frac{1}{\sigma} - \frac{1}{\sigma_A})\log(\frac{L_{\text{ct}}}{L_{\text{nt}}}) + \log(\frac{\alpha_{\text{cjt}}}{\alpha_{\text{njt}}}) - \frac{1}{\sigma}\log(\frac{L_{\text{cjt}}}{L_{\text{njt}}}) \tag{2.6}$$

where $\log(\frac{\theta_{\text{ct}}}{\theta_{\text{nt}}})$ implies the year trend of the relative technological efficiency for college labor force, $\log(\frac{L_{\text{ct}}}{L_{\text{nt}}})$ measures the relative size of aggregate college labor fore in year t, $\log(\frac{\alpha_{\text{cjt}}}{\alpha_{\text{njt}}})$ is the age specific trend of the relative efficiency of college workers, and $\log(\frac{L_{\text{cjt}}}{L_{\text{njt}}})$ is the key variable of interest, the age specific relative size of the college labor force.

Notice that the first two terms at the right-hand-side of Equation (2.6) capture the year trend of the college premium common for all age cohorts. Thus, the heterogeneous trends of the college premium across age cohorts should be due to the last two terms. And, the negative effect of age specific relative size on the college premium is expected unless workers are perfectly substitutable across age cohorts (the substitution elasticity $\sigma = \infty$).

2.4. Birth cohort effects

The two age specific variables, $\log(\frac{L_{\text{cjt}}}{L_{\text{njt}}})$ and $\log(\frac{\alpha_{\text{cjt}}}{\alpha_{\text{njt}}})$, are measures for the birth cohort $t - j$. Thus, in addition to a fixed age profile and year fixed effect, $\log(\frac{L_{\text{cjt}}}{L_{\text{njt}}})$ should capture birth cohort effects that reflect the variation in college attendance rate while $\log(\frac{\alpha_{\text{cjt}}}{\alpha_{\text{njt}}})$ should capture birth cohort effects that mainly reflect the technological changes. We can decompose them into age cohort, year, and birth cohort fixed effects,

$$\log(\frac{L_{\text{cjt}}}{L_{\text{njt}}}) = F_{t-j} + F_j + F_t \tag{2.7}$$

$$\log(\frac{\alpha_{\text{cjt}}}{\alpha_{\text{njt}}}) = f_{t-j} + f_j + f_t. \tag{2.8}$$

Therefore, we can rewrite Equation (2.6) as

$$\log(\frac{w_{\text{cjt}}}{w_{\text{njt}}}) = F_t' + F_j' + f_{t-j} - \frac{1}{\sigma}F_{t-j} \tag{2.9}$$

where

$$F_t' = \log(\frac{\theta_{\text{ct}}}{\theta_{\text{nt}}}) + (\frac{1}{\sigma} - \frac{1}{\sigma_A})\log(\frac{L_{\text{ct}}}{L_{\text{nt}}}) + f_t - \frac{1}{\sigma}F_t$$

$$F_j' = f_j - \frac{1}{\sigma}F_j.$$

This implies that the college premium for age cohort j in year t can be decomposed into the fixed effects of year, age and birth cohort. Only if workers are not perfectly substitutable across age cohorts ($\sigma < \infty$) can birth cohort effects in relative size, F_{t-j}, contribute to the birth cohort fixed effects in the college premium.

3. Empirical approach

3.1. Construction of college premium and relative cohort size

Our primary goal in this paper is to estimate the effect of the age cohort specific relative size of college workers on the age cohort specific college premium. Since these two key variables are not directly observed in our data set, we need to construct measures of them before further analysis.

Following the standard approach in the literature on cohort size effects, we collapse individual data into cells based on single-year age and survey year. Then the age specific college premium in each survey year is estimated with the individual observations within corresponding cells by following specification,

$$y_i = \beta_0 + \beta_1 \text{college}_i + \varepsilon_i \tag{3.1}$$

where y_i is log annual earnings, β_0 is a constant, $college_i$ is a dummy variable that takes 1 for college workers and 0 for non-college workers, and β_1 is the college premium to be estimated. Some existing papers (Welch, 1979; Card and Lemieux, 2001; Brunello, 2010) on the effect of cohort size on earnings or the college premium use log weekly or hourly wages for analysis. However, in terms of Equations (2.4) and (2.5), we believe that using weekly or hourly earnings is inappropriate unless the age specific relative size is measured using total working weeks per year or total working hours per year correspondingly. Due to the lack of information on working hours, we use log annual earnings for our analysis.

Accordingly, we build the measure of age specific relative size based on the number of workers.[12] The age-year cell specific relative size is just the log ratio of the number of college workers to the number of non-college workers within each cell.

Following Card and Lemieux (2001), we also record the standard errors of estimated cell specific college premiums. The corresponding inverse variances will be used as weights for the regression analysis to put more weight on those precisely estimated college premiums and construct goodness-of-fit tests for the null hypothesis that the relevant specification has no specification error.[13]

To improve the precision of the estimated college premiums and to reduce the sampling variation in the relative size of college workers, we construct cells based on three-year age and survey year alternatively at the expense of reducing the number of cells for regression analysis by two-thirds. Nevertheless, this serves as a good robustness check.

3.2. Testing the assumption: equally substitutable college and non-college labor

In Section 2.3, we link age specific college premiums to age specific relative sizes by Equation (2.6) based on the assumption that the substitution elasticity across age cohorts, σ_s, is the same among college and non-college groups. It is a hypothesis that needs to be tested. Following the profit-maximizing wage Equation (2.4) for an average worker in age cohort j with education level s in year t, we decompose the unobserved three-way variable $log(\alpha_{sjt})$ into three two-way fixed effects (education level-year, education-age, and age-year fixed effects) and a conditional zero mean error term ε_{sjt}. Then we test the assumption by OLS estimation with the following specification:

$$\log(w_{\text{sjt}}) = F_{\text{st}} + F_{\text{sj}} + F_{\text{jt}} + \beta_1 \text{noncollege}_s \times \log(L_{\text{sjt}}) + \beta_2 \text{college}_s \times \log(L_{\text{sjt}}) + \varepsilon_{\text{sjt}} \tag{3.2}$$

where the dependent variable $\log(w_{\text{sjt}})$ is log mean earnings for j years old workers with education level s in year t, the education-year fixed effects F_{st} absorbs the terms $\log(\Phi_t) + \log(\theta_{\text{st}}) + (\frac{1}{\sigma_s} - \frac{1}{\sigma_A})\log(L_{\text{st}})$ from Equation (2.4) and the additional education-year fixed effect decomposed from $log(\alpha_{sjt})$, the education-age fixed effect F_{sj} captures the potentially different age-profile of earnings for college and non-college groups, the age-year fixed effect F_{jt} captures those unobserved factors that commonly affect both education groups, and $log(L_{sjt})$ is the age cohort size for education group s in year t. We allow for a different effect of cohort size on earnings by including the interaction terms between education group dummy and age cohort size, $college_s \times log(L_{sjt})$ and $noncollege_s \times log(L_{sjt})$. We test whether $\beta_1 = \beta_2$.

An equivalent test strategy as follows is based on Equation (2.5),

$$\log(\frac{w_{\text{cjt}}}{w_{\text{njt}}}) = F_t + F_j + \beta_1 \log(L_{\text{cjt}}) + \beta_2 \log(L_{\text{njt}}) + \varepsilon_{\text{jt}} \tag{3.3}$$

where the dependent variable is estimated college premium for age cohort j in year t, the age-year fixed effect in Equation (3.2) is canceled out by taking the difference between log earnings of college workers and non-college workers. Noticing that β_1 and β_2 represent $-\frac{1}{\sigma_c}$ and $\frac{1}{\sigma_n}$ respectively, we test if $\beta_1 + \beta_2 = 0$.

Since both dependent variables in Equations (3.2) and (3.3) are estimated first, the corresponding standard error can be obtained prior to the tests. Following the literature, we use inverse squared standard errors as weights to implement weighted-OLS estimation.

3.3. Estimating the effect of age specific relative size on college premium

Our basic specification to estimate the effect of age specific relative size on the college premium is based on Equation (2.6). We decompose the unobserved age-year log ratio of relative efficiency, $\log(\frac{\alpha_{\text{cjt}}}{\alpha_{\text{njt}}})$, into age fixed effect, year fixed effect, and age-year two-way variation. We use the following specification,

[12] Using annual earnings and the number of workers to build measures for the college premium and relative size highlights that our estimated effects of cohort size on the college premium have slightly different implications from those using weekly earnings or hourly earnings. Considering that working hours or working weeks are endogenously determined in the labor market, using them to measure relative size may suffer the identification issue of reverse causation.

[13] Essentially, it tests whether the recorded variances of the estimated college premiums are significantly different from the variances of the residual in the relevant specification. See Card and Lemieux (2001) for details.

$$r_{jt} = F_t + F_j + \beta_1 \log(\frac{L_{cjt}}{L_{njt}}) + \varepsilon_{jt} \tag{3.4}$$

where r_{jt} is the estimated college premium for age cohort j in year t, F_t captures all year specific factors, F_j is the age fixed effect decomposed from $\log(\frac{\alpha_{cjt}}{\alpha_{njt}})$, $\log(\frac{L_{cjt}}{L_{njt}})$ is the relative size of college workers measured as the log ratio of the number of college workers to the number of non-college workers within each age-year cell, and the error term ε_{jt} is assumed to be conditional zero mean to ensure the OLS estimate of β_1 identifies the relative size effect on the college premium.

However, a simple OLS estimate of β_1 may be biased in two ways. First, our specification is strictly based on the profit-maximizing wage functions that reflect only the labor market's demand side, whereas the estimated college premiums and the observed age specific relative sizes represent the realized general equilibrium because they are calculated based upon male employed individuals with positive earnings reported in the surveys. Therefore, $\log(\frac{L_{cjt}}{L_{njt}})$ may have been affected by the college premium through a supply channel. Due to this simultaneous causation issue, we need an instrumental variable not affected by the college premium. We use a broader sample including all individuals (male and female, employed and unemployed, any earnings and missing earnings reported) aged between 25 and 54, and calculate the age-year cell specific log ratios by the same formula, $\log(\frac{L_{cjt}}{L_{njt}})$. The ratios based upon this inclusive sample are not subject to the college premiums because they are determined by the gross college admission rates across years. Of course, the survey sampling variation may also affect the ratios. However, for no reason should we believe the sampling variation may be correlated to the age-year college premiums.

Second, the error term ε_{jt} captures not only those plausible zero mean sampling error and specification error but also the age-year two-way variation from the unobserved log relative efficiency ratio, $\log(\frac{\alpha_{cjt}}{\alpha_{njt}})$. The simple OLS estimate of β_1 will be biased due to the omission of relevant variables if $\log(\frac{L_{cjt}}{L_{njt}})$ is correlated with the unobserved two-way varying $\log(\frac{\alpha_{cjt}}{\alpha_{njt}})$. By the implication of the relative efficiency parameter α, we know it may be affected by the relative labor complementarity with technology, the relative skill composition, the relative ability composition, etc. Since it has been discussed that the skill-biased technological change favoring younger college workers allows for a lower bound of the estimates in the context of China, we focus on the potential ability composition effect and skill composition effect in this section.

3.3.1. *Ability composition effect*

It's widely believed that basic OLS estimates of college premium are biased due to unobserved ability or self-selection, which is reflected by the huge literature on isolating the returns to college from the returns to ability. However, in the literature on the evolution of college premium, the change in the ability composition effect receives much less attention. Some studies find that the changes in ability composition or self-selection indeed contribute to the observed college premium evolution, even if the extents are found to be different (Chay and Lee, 2000; Taber, 2001; Juhn et al., 2005; Carneiro and Lee, 2009, 2011).[14]

Before presenting our empirical strategy to address the ability composition effect, it is necessary to explain how it may confound the estimate of the relative size effect in this paper. As we noted in Section 2.4 and which will be empirically explored, the relative size $\log(\frac{L_{cjt}}{L_{njt}})$ captures strong birth year fixed effects which drive the age-year two-way variation in $\log(\frac{L_{cjt}}{L_{njt}})$. There has been an observed increase in college attainment along with the birth cohorts. And the observed increase stems from both demographic changes and an expanding capacity of China's higher education. In China's strict test score-based college admission system, it's plausible that marginal college students have lower ability than the average college students. When the expansion of college capacity outpaced the demographic changes in China, the share of college students increased, marginal students entered college, and the average ability of college students was lowered. By the same logic, the average ability of non-college students also has been lowered. The lowered average abilities for both education groups result in difficulty in predicting the sign of the correlation between relative size $\log(\frac{L_{cjt}}{L_{njt}})$ and relative average ability. However, some previous papers show that the ability effect on earnings for high school graduates is insignificant (Carneiro and Lee, 2011) and is less positive than that on college graduates (Lillard, 1977; Carneiro and Lee, 2011). This evidence implies that we should be careful that the negative correlation between the relative size of college workers and the earnings gap effect of relative average ability may lead our estimated relative size effect on the earnings gap to be downward biased. In the extreme case, what we estimated for β_1 by Equation (3.4) may just be an ability composition effect rather than a relative size effect.

Our strategy is to explore the age pattern of the potentially confounded relative size effect by allowing for heterogeneity across age groups,

$$r_{jt} = r_t + r_j + \beta \mathrm{Agp}_j \times \log(\frac{L_{cjt}}{L_{njt}}) + \varepsilon_{jt} \tag{3.5}$$

where Agp_j is a vector of age group dummies, β is the corresponding vector of coefficients which captures the relative size effects on college premium across age groups, and ε_{jt} is suspected to include ability composition effects negatively correlated with $\log(\frac{L_{cjt}}{L_{njt}})$. If the ability composition effects are significant and indeed negatively correlated with $\log(\frac{L_{cjt}}{L_{njt}})$, by simple OLS estimation, we will obtain an

[14] Among these studies, only Carneiro and Lee (2011) focus on isolating the ability composition effect within the age specific framework as we do in this paper, while others within an aggregate framework.

estimated age group pattern of relative size effect dominated by the age group pattern of ability composition effects.

Lillard (1977) uses NBER-Th data[15] including the measured ability (AFQT scores) and reveals that the earnings effect of measured ability increases as one ages. The increasing pattern is more significant for college graduates than for high school graduates.[16] More specifically, the ability effect is almost negligible or slightly negative under age 35 and peaks around age 50. Taking this pattern as also true in China's context,[17] the estimated relative size effects will be more negative for older groups if the ability composition effects exist and are negatively correlated with $\log(\frac{L_{cjt}}{L_{njt}})$. Therefore, if an opposite pattern is revealed by our estimation, we will be able to argue that the confounded ability composition effects are trivial, and the estimated effects for younger groups, especially those under age 35, should be uncontaminated by the ability composition effects at least. The opposite pattern can be explained as the younger groups tend to be affected by their own cohort relative size more substantially.[18]

3.3.2. Skill composition effects

We use occupation and industry composition to capture the skill composition approximately. The variation in age specific relative size, $\log(\frac{L_{cjt}}{L_{njt}})$, is mainly driven by China's higher education expansion since 1977 when the national college entrance examination was restored. One year later, in 1978, China started "the open and reform" through which China gradually switched from a central-planned economy to a market-oriented economy. Along with the transition, new labor market entrants with different education levels may have been reallocated into occupations and industries differently. Considering that the higher education expansion and economic transition took place during the same period, it is possible that the age-year variations in occupation and industry compositional differential between college and non-college groups are correlated with the age-year variation in college/non-college relative size. That means, in Equation (3.6), the omitted occupation and industry compositional effects are possibly correlated with $\log(\frac{L_{cjt}}{L_{njt}})$. Due to a sample size limitation,[19] we are not able to control for these compositional effects consistently for each age-year cell. Therefore, we turn to regression with individual data directly by the following specification,

$$\begin{aligned} y_{ijt} &= \beta_0 + \beta_1 \text{college}_{ijt} \times \log(\frac{L_{cjt}}{L_{njt}}) + \beta_2 \log(\frac{L_{cjt}}{L_{njt}}) \\ &+ F_t + F_j + \text{college}_{ijt} \times (F_t + F_j) + \gamma X_{ijt} \times F_t + \varepsilon_{ijt} \end{aligned} \tag{3.6}$$

where i, j, t denotes individual, and X_{ijt} includes a series of dummies for occupation and industry categories. We allow for the occupation and industry fixed effects to vary across years by the interaction term $X_{ijt} \times F_t$. With this specification, the OLS estimate of β_1 is the relative size effect on the college premium conditional on occupation and industry. Dropping the interaction term $X_{ijt} \times F_t$ should result in an estimated β_1 close to those by specification 3.4 since the earnings gap by specification 3.6 can be expressed in the same form:

$$E[Y_{ijt}|\text{college}_{ijt} = 1] - E[Y_{ijt}|\text{college}_{ijt} = 0] = \beta_1 \log(\frac{L_{cjt}}{L_{njt}}) + F_t + F_j. \tag{3.7}$$

By controlling for these labor market destinations, we also alleviate another concern about the college majors composition effect since it is plausible that majors determine college graduates' occupation and industry destinations to a substantial extent.[20]

4. Data

Our data are drawn from five repeated cross-section nationally representative surveys - China Household Income Project (CHIP) 1995, 1999, 2002, 2007 and 2013.[21] As indicated by its name, CHIP surveys detailed household income, education, employment, and family background information, which makes it a widely used data source in the literature on earnings differential across education or

[15] NBER-Th sample was based on a sample of men who had volunteered for pilot, bombardier, and navigator programs of the Air Force during World War II. Thomas Juster organized a resurvey of a subset of these men in 1969 and built a data set providing information on education, income, AFTQ test scores and detailed information on various measures of family background.

[16] One explanation is that the more able tend to invest more in on-job training or choose more promising jobs.

[17] Even if there is no evidence from China's data, we believe the underlying logic also holds in China's labor market.

[18] Welch (1979) finds that the cohort size effects are more negative for entrant cohorts with data of the U.S.

[19] On average, in our data set, each age-year cell contains about 90–210 individuals.

[20] Grogger and Eide (1995) reveal the trend away from low-skill subjects such as education and toward high-skill subjects such as engineering accounts for one-fourth of the rise in the male college wage premium with the U.S. data. Major's information is not available in our data set that we can't directly control for them.

[21] CHIP 2008 surveys the same individuals in 2007, so we pool them together and notate it as CHIP2007 in this paper.

other labor market-related topics in China.[22] In this paper, following the literature (Zhang et al., 2005; Ge and Yang, 2011; Wang, 2012; Wang et al., 2014) on China's college premium, we focus on the urban samples.[23] We further restrict our sample to males between 25–54. Only focusing on males avoids the selection issue due to intermittent female labor force participation.[24] The lower limit, age 25, is to make sure most college graduates have entered the labor market, while the upper limit, age 54, is to drop those near retirement age who may decide to retire non-randomly (Brunello, 2010).

We define individuals who have a three-year college degree, a four-year college degree or above as college graduates, and all other individuals as non-college graduates. This broad definition has the advantage of covering all workers in the labor market and obtaining more precise estimates for earnings gaps by keeping more observations, but the disadvantage of bringing the contamination of composition effects. Therefore, we will also present results based on only 4-year college and high-school graduates as a robustness check.

We use annual earnings to estimate the college premiums due to limited consistent information on working weeks and hours. However, CHIP (2007) only provides monthly earnings information without working months available. Fortunately, the potential inconsistency in estimated college premiums for wave 2007 should be captured by a fixed year effect which will be controlled for in our empirical analysis. Another concern about the wave 2007 is the province coverage which is different from other waves to study rural-urban migrants. The shares of college-educated workers may be measured inconsistently, and biased estimates for the cohort size effects may arise. Hence, the representativeness of wave 2007 should be carefully checked. Details are presented in the sample summary section.

We collapse the individuals between the ages 25–54 into 150 cells based on single-year age and survey year. For each cell, our estimated college premiums and the relative size of college workers are further based on those employed individuals reporting positive annual earnings. The instrumental variable for the relative size of college workers, as discussed in Section 3.3, is based on both employed and unemployed individuals between 25–54, including females. This broad inclusion makes sure we construct a pre-determined variable only affected by the exogenous demographic change and higher education expansion.

4.1. Sample summary

Before presenting a graphical description of cell-specific relative size and estimated college premium, we summary our filtered sample in Table 1. The number of observations in each survey year ranges between 2754 and 6461, and the variation is mainly due to the variation in the sample size of original surveys. The average log annual earnings show a steady increase.[25] The age structure is stable during the covered period, demonstrated by the stable averages and standard deviations. By categorizing occupations into three levels (high-skill, mid-skill, and low-skill levels), we can see a decrease in high-skill share and an increase in low-skill share.[26] Most industry shares are stable, except that manufacturing share decreased while service shares increased. The dominant industry by share of employment changed from manufacturing to service. As we discussed in Section 3.3.2, if these changes in occupation and industry shares were different between education groups and age groups, our estimated effect of the relative size on the college premium would be contaminated by occupation and industry compositional effects.

The share of college workers increased from 29% in 1995 to 45% in 2007 and dropped slightly to 42% in 2013, even if the higher education expansion should have pushed up the college share. By checking the detailed education levels, we further find the unexpected decrease in the share of 3-year college and an increase in the share of junior high school from 2007 to 2013. These unexpected changes in the education composition strengthen our concern about the representativeness of CHIP 2007 for its province coverage difference from other waves.

First, we compare CHIP and China Urban Household Survey (UHS) which samples households with Urban Household Registration (Hukou) for every province.[27] In Table 2, panel *A* presents the college shares by our CHIP sample, and panel *C* cites the figures from Meng et al. (2013), who construct their sample using UHS (1988–2009) with slightly different restrictions.[28] The share differences are no more than 5 percent for the four comparable waves. Even though the college share by UHS in 2013 is not available, the stagnation in college share also exists in UHS 2007 to 2009. Besides, we follow the same sample restrictions as Meng et al. (2013) and get much closer college shares in panel B. These comparisons relieve the concern about data inconsistency for CHIP 2007 but provide no answers why the unexpected stagnation in college share happens after 2007.

Second, by checking the rural-urban Hukou changes, we find the share of individuals who once changed Hukou from rural to urban

22 For instance, Gustafsson et al. (2008) write a whole book using CHIP to explore inequality and public policy in China.

23 The main reasons documented are that rural household income is generally indivisible, there is a relatively low share working in non-agriculture sectors, and few college graduates working in the rural area.

24 See Card and Lemieux (2001) and Brunello (2010). Even if this issue may not be as severe as that in western countries, considering that female labor force participation is relatively high in China(Meng, 2012), we focus on males for comparing results with existing literature mainly on western countries.

25 We use nominal annual earnings in this paper, so the increase captures both real income growth and inflation. Using nominal earnings does not affect our results since the inflation index is canceled out in the estimates of the college/non-college earnings gap.

26 High-skill level includes principals and professional technicians, mid-skill level includes clerical/office staff and low-skill level includes the other occupations.

27 See Meng et al. (2013) for details about China Urban Household Survey.

28 They restrict the individuals aged between 20 and 60 while ours aged between 25 and 54. They exclude those without urban hukou while we keep them in our base sample.

Table 1
Summary statistics: Male workers only.

CHIP	1995	1999	2002	2007	2013
Log Annual Earnings	8.76	9.07	9.32	10.21	10.50
	(0.55)	(0.57)	(0.62)	(0.69)	(0.71)
Age	39.84	40.59	41.41	40.48	40.74
	(7.70)	(7.52)	(7.62)	(8.28)	(8.18)
College	0.29	0.35	0.36	0.45	0.42
	(0.45)	(0.48)	(0.48)	(0.50)	(0.49)
4-Year College&Above	0.10	0.12	0.12	0.22	0.22
	(0.30)	(0.33)	(0.33)	(0.41)	(0.41)
3-Year College	0.19	0.23	0.23	0.23	0.20
	(0.39)	(0.42)	(0.42)	(0.42)	(0.40)
High School&Vocational High School	0.37	0.34	0.37	0.36	0.30
	(0.48)	(0.47)	(0.48)	(0.48)	(0.46)
Junior High School	0.30	0.30	0.25	0.17	0.25
	(0.46)	(0.46)	(0.43)	(0.37)	(0.43)
Primary School&Below	0.04	0.02	0.03	0.02	0.03
	(0.20)	(0.13)	(0.16)	(0.15)	(0.18)
High-Skill Occ.	0.40	0.41	0.40	0.34	0.25
	(0.49)	(0.49)	(0.49)	(0.47)	(0.43)
Mid-Skill Occ.	0.20	0.16	0.18	0.22	0.18
	(0.40)	(0.37)	(0.38)	(0.41)	(0.39)
Low-Skill Occ.	0.40	0.43	0.42	0.45	0.57
	(0.49)	(0.50)	(0.49)	(0.50)	(0.50)
Agriculture	0.02	0.01	0.01	0.01	0.02
	(0.14)	(0.11)	(0.11)	(0.10)	(0.14)
Mining	0.01	0.04	0.03	0.01	0.04
	(0.11)	(0.18)	(0.17)	(0.10)	(0.21)
Construction	0.03	0.05	0.04	0.05	0.07
	(0.18)	(0.22)	(0.20)	(0.21)	(0.26)
Manufacturing	0.43	0.32	0.27	0.20	0.15
	(0.49)	(0.47)	(0.45)	(0.40)	(0.36)
Transportation etc.	0.06	0.16	0.14	0.16	0.14
	(0.24)	(0.37)	(0.35)	(0.37)	(0.35)
Trade	0.12	0.08	0.10	0.11	0.10
	(0.33)	(0.27)	(0.30)	(0.31)	(0.30)
Finance	0.05	0.03	0.04	0.08	0.06
	(0.22)	(0.17)	(0.19)	(0.28)	(0.24)
Service	0.13	0.20	0.23	0.29	0.28
	(0.34)	(0.40)	(0.42)	(0.45)	(0.45)
Public Institutions	0.14	0.11	0.14	0.09	0.13
	(0.35)	(0.32)	(0.34)	(0.29)	(0.33)
Observations	4978	2754	4900	6461	4335

Table 2
College shares: Comparisons between CHIP and UHS.

	1995	1999	2002	2007	2008	2009	2013
Panel A:							
CHIP: Base Sample	0.29	0.35	0.36	0.45			0.42
Observations	4978	2754	4900	6461			4335
Panel B:							
CHIP: Alternative Sample	0.28	0.32	0.37	0.45			0.44
Observations	6420	3522	5009	6779			4176
Panel C:							
UHS: Sample by Meng et al. (2013)	0.26	0.30	0.37	0.43	0.41	0.43	
Observations	9295	8859	21034	23480	31394	30668	

Table 3
Education distribution by different sample restrictions.

CHIP	1995	1999	2002	2007	2013
Panel A:					
College	0.29	0.36	0.37	0.48	0.49
4-Year College&Above	0.10	0.13	0.14	0.24	0.27
3-Year College	0.19	0.23	0.24	0.24	0.22
High &Vocational High School	0.37	0.34	0.36	0.37	0.30
Junior High School	0.30	0.30	0.24	0.14	0.18
Primary School &Below	0.04	0.01	0.02	0.01	0.02
Observations	4978	2365	4084	5108	2922
Panel B:					
College	0.28	0.33	0.34	0.45	0.48
4-Year College&Above	0.10	0.12	0.12	0.23	0.27
3-Year College	0.19	0.21	0.22	0.23	0.22
High &Vocational High School	0.37	0.34	0.36	0.38	0.31
Junior High School	0.30	0.32	0.27	0.15	0.19
Primary School &Below	0.05	0.02	0.03	0.02	0.02
Observations	5516	2709	4691	5874	3297
Panel C:					
College	0.16	0.22	0.25	0.37	0.43
4-Year College&Above	0.05	0.06	0.06	0.15	0.22
3-Year College	0.11	0.16	0.18	0.22	0.20
High &Vocational High School	0.39	0.42	0.43	0.41	0.33
Junior High School	0.35	0.32	0.28	0.19	0.22
Primary School &Below	0.10	0.04	0.04	0.03	0.03
Observations	5834	2805	4924	6083	3212

was 17% in 2007 and soared up to 31% in 2013.[29] This significant population composition change contributes to the unexpected stagnation in college share because a large part of these rural-urban individuals is found to be lower educated. We exclude them from our base sample except for those obtained urban Hukou by education and summarize the education levels in panel *A* of Table 3.[30] The college share in 2013 is 0.49, which is not only 1 percent larger than 2007 but also 7 percent larger than 2013 with the base sample. Besides, the share of junior high school turns to be less abnormal than the base sample.

Last, since some previous studies (Li et al., 2014; Xing et al., 2018; Knight et al., 2017) find the mass higher education expansion in 1999 decreased the employment opportunity for the younger college graduates, we relax the sample restriction to include individuals unemployed or without positive earnings reported. Panel *B* in Table 3 summarizes education levels for males and panel *C* for females. More significant increases in college share were found between 2007 and 2013, particularly for females. These also support the fact of decreasing employment opportunities for college-educated workers.

In summary, the data consistency for CHIPs 1995 to 2013 is as good as UHS, and the unexpected stagnation in college share is mainly due to the changing urban population composition and the sample restrictions for our income analysis.

4.2. Relative sizes and estimated college premiums

For each age-year cell, we can estimate a college premium by Equation (3.1) and measure the corresponding relative size of workers as the log ratio of the number of college workers to the number of non-college workers. Fig. 5 provides pairs of these two variables. Due to the year fixed effects and the intrinsic age profile, it shows no clear linear relationship between the college premium and the relative size of college workers. Nevertheless, Fig. 5 reveals substantial variations in the two variables, which makes it possible for us to identify the potential relationship by regression analysis.

With the broader sample including all individuals (male and female, employed and unemployed, any earnings and missing earnings reported), we calculate the age-year log ratios as an instrumental variable.[31] A significant correlation between log ratios with restricted and inclusive samples is plotted in Fig. 6. Hence, the first-stage F-statistics are expected to be large in the IV regressions.

By exploring the changing age profiles for the college premium and the relative size of college workers, we can graphically reveal the relationship between them. To ensure our graphs suffer less estimation variation, we use 30 broader cells of five-year age and survey year. Figs. 7 and 8 present the age profiles across survey years. As the downward age profile of the relative size of college workers turned much steeper from 1995 to 2013 in Fig. 8, the age profile of the college premium departed from a flat pattern to an upward pattern in Fig. 7. The opposite switching age profiles reflect the negative relationship.

[29] Five ways to get urban Hukou are listed in CHIP: education, work (become a cadre, military service, etc.), land expropriation, housing purchase, marriage and unspecified others.

[30] The Hukou changes were not surveyed in CHIP 1995. Hence we keep all individuals in 1995.

[31] The size of the inclusive sample is more than doubled. The average size of age-year cell increases from 156 to 367.

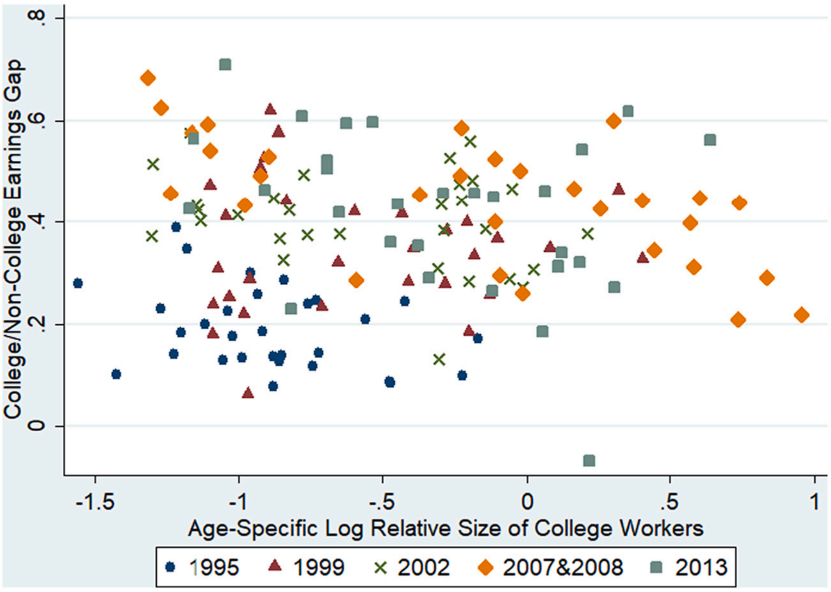

Fig. 5. Age-year cell specific log relative sizes and estimated college premiums.

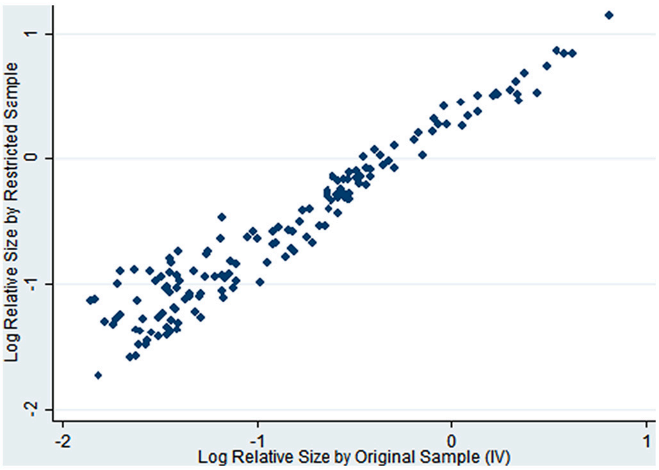

Fig. 6. Age-year cell specific log relative sizes: restricted sample and inclusive sample.

4.3. Relative size, college premium and higher education expansion

As we discussed in Section 2.4, the relative size for college workers in age cohort *j* and year *t* is measuring those born in year *t*– *j*, which implies that it should have captured strong birth year effects in addition to a fixed age profile and fixed year effect. To graphically illustrate the birth cohort effects, we plot the share of college workers against birth year groups in Fig. 10. Even if the profiles shift up and down across years and may have absorbed intrinsic age structure, it is revealed that there are steady rises in the share of college workers from birth year group 1953–1958 to 1984–1988. Considering that high school students usually take the national college entrance examination (NCEE) at about 18 years old, the rising birth year trends coincide with the restored NCEE and the expansion of higher education since 1977, as Fig. 9 shows.[32] The positive correlation implies that the rise in the relative size of college workers across birth years was mainly driven by the higher education expansion.

We also check if the college premiums also show strong birth year fixed effects, which would serve as preliminary evidence of the

[32] This figure depicts the nationwide trend using data from China's Statistics and Education Statistics Year Books covering urban and rural samples, while Fig. 10 is based on CHIP's urban samples only. The shares of college workers are much higher than those in Fig. 10. This implies that more college students are from urban areas or stay in urban areas.

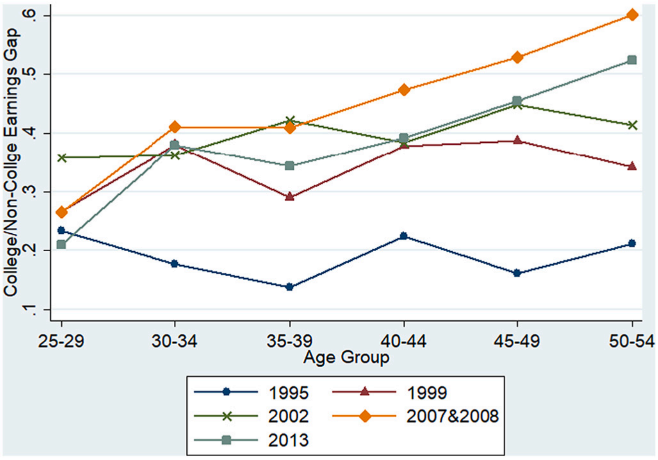

Fig. 7. Male workers' age profiles of the college premium across years.

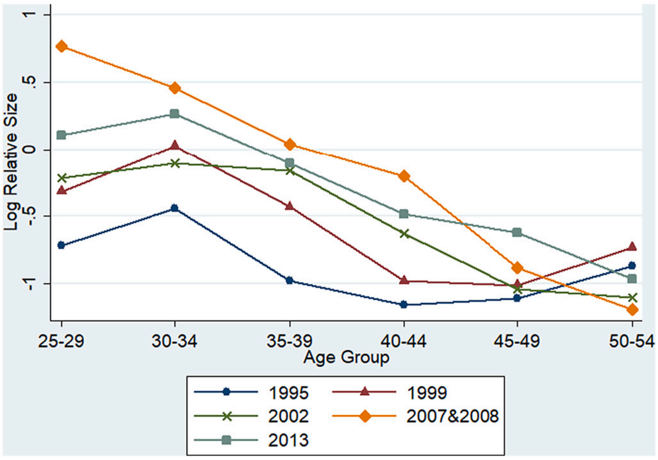

Fig. 8. Male workers' age profiles of relative size across years.

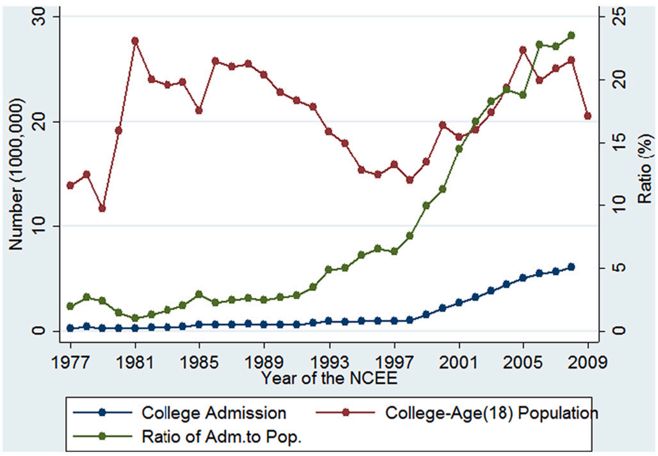

Fig. 9. Demographical change and higher education expansion in China.

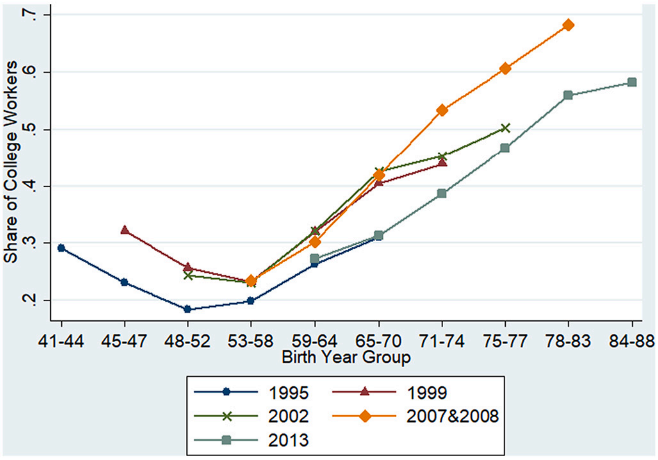

Fig. 10. Birth year profiles of the share of college workers.

Table 4
Birth year fixed effects on college premium and relative size.

	(1) College premium	(2) College premium	(3) College share	(4) Log relative size
Year Fixed Effects:				
1999	0.187***	0.183***	0.035**	0.159**
	(0.031)	(0.032)	(0.016)	(0.072)
2002	0.257***	0.252***	0.014	0.059
	(0.031)	(0.028)	(0.016)	(0.073)
2007	0.351***	0.349***	0.020	0.064
	(0.045)	(0.043)	(0.026)	(0.119)
2013	0.399***	0.383***	-0.096**	-0.427**
	(0.067)	(0.062)	(0.038)	(0.175)
Birth Fixed Effects:				
1959–64	-0.066	-0.053	0.088***	0.416***
	(0.043)	(0.038)	(0.025)	(0.114)
1965–70	-0.112*	-0.086	0.196***	0.894***
	(0.062)	(0.056)	(0.033)	(0.151)
1971–74	-0.184**	-0.149**	0.244***	1.098***
	(0.079)	(0.073)	(0.043)	(0.199)
1975–77	-0.151*	-0.135	0.312***	1.389***
	(0.090)	(0.085)	(0.051)	(0.236)
1978–83	-0.283***	-0.243**	0.443***	1.950***
	(0.108)	(0.098)	(0.064)	(0.292)
1984–88	-0.441***	-0.388***	0.462***	2.033***
	(0.150)	(0.137)	(0.076)	(0.349)
χ^2(p-value)		111(0.45)		
Observations	150	150	150	150
R-squared	0.943	0.951	0.985	0.910

Notes: Robust standard errors in parentheses. Reference year is 1995, reference birth group is 1941–1958. Age fixed effects are not shown. Weights used in specification 2 are inverse variances of estimated college premiums.
*** p<0.01.
** p<0.05.
* p<0.1.

effect of the relative size on the college premium, as we discussed in Section 2.4. Due to the more substantial variations in the college premium across years and age cohorts, the graph for the college premium suffers greater noise than the graph for shares of college workers. Therefore, we turn to regressions based on equations 2.7 and 2.9 decomposing the relative size and the college premium for age cohort j in year t into the year, age and birth cohort fixed effects.

Table 4 presents the results of the decompositions. We take the survey year 1995 and the birth group 1941–1958 as reference groups.[33] In column 1, we decompose college premiums by basic OLS estimation. In column 2, we weight our regression by the inverse sampling variance of estimated college premium with the χ^2 statistic for testing specification error reported.[34] Since the results are just slightly different between basic OLS and Weighted OLS estimation, we focus on the weighted-OLS results following the literature. Year fixed effects on the college premium increased by 38.3 percentage points from 1995 to 2013, and about half of the increase happened between 1995 and 1999. The estimated birth year fixed effects show a steady decreasing trend for those born after 1958. Specifically, compared with those born in 1941–1958, the college premium for the recent birth cohorts 1984–88 decreased by almost 39 percent. As the χ^2 static 111.07 and its p-value 0.45 indicate, we fail to reject the null hypothesis that there is no specification error in our model. The dependent variable in column 3 is the share of college workers, while the dependent variable in column 4 is the relative size of college workers which is also the explanatory variable in our main specification 3.4 to be estimated in the next section. Estimated year fixed effects capture both sampling variation and overall relative employment across survey years. As the results in column 3 show, compared with 1995 conditionally, about 3.5 percent more college workers were employed in 1999 and 9.6 percent fewer college workers were employed in 2013. The estimated birth year fixed effects show a steady rising trend for those born after 1958, which reveals a negative correlation with the estimated fixed effects on college premium in column 2. The predicted birth group fixed effects on the share of college workers and college premium, standardized to age 40 and year 2013, are plotted in Figs. 11 and 12. The contrasting trends together with the higher education expansion in Fig. 9 provide preliminary evidence that higher education expansion drove the rise in the share of college workers which further compressed the college premium.

By exploring the decomposed birth year fixed effects on the two key variables, we can find that their age-year two-way variations are mainly captured by the birth cohort fixed effects. The identification of the cohort size effect on the earnings gap relies on these two-way variations. Therefore, if any other birth cohort specific factors affecting college premium are correlated with the birth cohort specific variation in the relative size of college workers, our identification of the cohort size effect will fail. As we discussed in Section 3.4, the main contaminating factors are potentially correlated compositional effects due to the birth cohort specific variations in ability, occupation and industry compositions.

5. Results

In Table 5, we present our basic estimates of the effect of age specific relative size of college workers on the college premium based on specification 3.4 which regresses the age specific college premium against age and year fixed effects and the age specific relative size of college workers. The results by weighted/unweighted OLS estimation in columns 1 and 2 do not show significant differences. The estimated effects of the relative size of college workers on the college premium, -0.08 and -0.078, are similar and significant at the 5% level. They imply that holding year and age constant, a one unit increase in the relative size of college workers leads to about 8 percentage points decrease in the college premium. By the model implication, these estimates represent that the elasticity of substitution across age cohorts is about 12.5. The estimated year fixed effects show that the college premium increased steadily until 2007 and then fell slightly in 2013, consistent with the literature findings (Meng, 2012; CHLR, 2020).[35] The increase until the mid-2000s is argued due to the transition from administrative to market wage system. And, the decrease in the last decade may be related to the large influx of college-educated workers due to the higher education expansion in 1999.

As we discussed in Section 3.4, basic OLS estimation may suffer the issue of simultaneous causation which makes it biased. We use the predetermined variable, log ratio of the number of college graduates to the number of non-college individuals (including both male and female, employed and unemployed), as an instrumental variable for our independent variable based only on male workers. The corresponding results are presented in columns 3 and 4. The first-stage F-statistics, 447 and 655, are large enough to ignore the weak IV issue, as expected by the significant correlation plots in Fig. 6. The magnitudes of the estimated relative size effects increase by about 30 percent, even if these increases are not statistically significant. The slightly attenuated OLS estimates imply that the relative size of college workers might be positively affected by the college premium simultaneously. In other words, higher college premium induces relatively more college graduates to seek employment, which is consistent with basic intuition even if this is not empirically studied in this paper.

However, our results above may still suffer bias due to the omission of relevant variables as we discussed in Section 3.4, such as ability, occupation and industry compositional factors that may correlate with the relative size of college workers. To address the potential ability compositional effects, we explore the age group pattern of the relative size effect on the college premium based on Equation (3.5). The corresponding results are presented in Table 6. In column 1 of Table 6, we divide ages into 6 groups evenly: 25–29, 30–34, 35–39, 40–44, 45–49 and 50–54. The estimated effects are significant only for the new entrants between the age 25 and 29, -0.142, at the 1% level. Thus, we alternatively divide ages into two groups, new entrants 25–29 and all other ages 30–54. Corresponding results are presented in column 2. The estimated effect for the new entrant group is still negative and significant, -0.156, while for all other ages is insignificantly negative, at -0.049. The T-test statistic implies that the effects are significantly different at the 5% level. The magnitudes of IV estimates in column 3 increase slightly, revealing a similar pattern that new entrants are more substantially affected by their own relative size than the older group (age 30–54). Suppose our estimates are dominated by the ability

[33] Considering that most high school students apply for college at about 18 years old, those born before 1958 arrived at college-age before 1977, when the NCEE was restored. We do not divide our sample evenly into birth groups due to the uneven year gaps of our surveys.

[34] The null hypothesis is that there is no specification error conditional on included fixed effects. See Card and Lemieux (2001) for details.

[35] China Center for Human Capital and Labor Market Research presents a quadratic trend of returns to education in the annual report on China's human capital 2020. And, the peak happens to be around the mid-2000s.

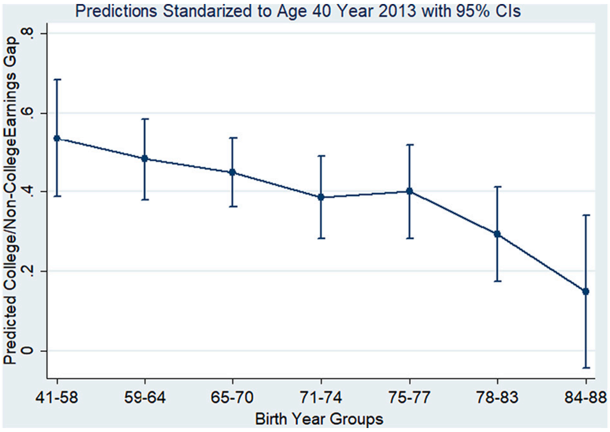

Fig. 11. Predicted birth group fixed effects on the college premium.

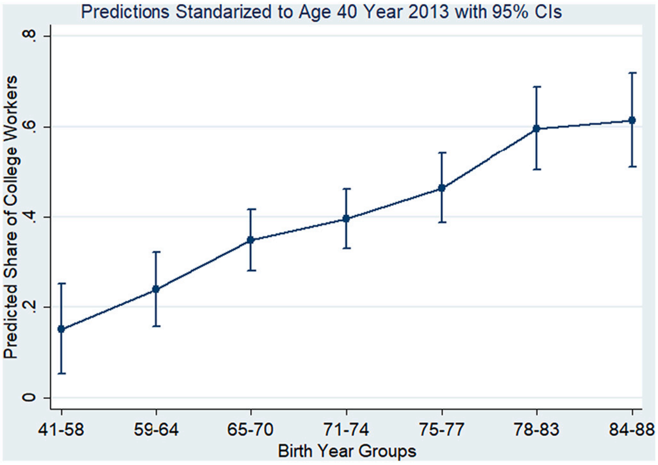

Fig. 12. Predicted birth group fixed effects on the share of college workers.

composition effect. In that case, the revealed pattern should be the opposite showing a smaller negative effect for new entrants because the conditional ability effects are more substantial for older workers by Lillard (1977) as we discussed in Section 3.4.1. Our estimated pattern is also consistent with the findings by Welch (1979) that entrant cohorts are more easily affected by the cohort size effect. As Welch (1979) argues, workers as learners gradually accumulate skills in the early career phase. Due to the substantial variance of the skills possessed, entrant workers are less easily substituted with each other, therefore, more easily affected by their own cohort size. As they enter later career phases and accumulate enough skills to fulfill different tasks, they are more substitutable and less easily affected by the cohort size.

In the specifications for our main findings above, we define the college premium and relative size of college workers based on broadly defined college workers including three-year college graduates or above and corresponding non-college graduates. We believe this definition has the advantage in covering all workers in the labor market and keeping as many observations as possible to obtain precisely estimated college premium for further analyzing the relative size effect on the college premium. However, the estimated college premium by our definition is different from the college premium referring to the earnings gap between 4-year college and high-school graduates, which leads our analysis to be less relevant to the huge literature on college premiums and less comparable to several studies on the effect of relative size on college premium (Card and Lemieux, 2001; Carneiro and Lee, 2009; Kawaguchi and Mori, 2016). Another disadvantage is that the potential varying average years of schooling for broadly defined college and non-college groups may bring in additional sources of variation in the estimated college premium.[36]

Therefore, we measure the relative size of college workers and estimate the college premiums based on the sample including only four-year college workers and high-school workers. Results are presented in Table 7. The magnitudes of our OLS and IV estimates

[36] The average year of schooling for the non-college group increased substantially because of family income growth and China's nine-year compulsory education program implemented since 1985.

Table 5
Basic estimates for effects of age specific relative size of college workers on college premiums.

Dependent variable: College premium	(1) OLS	(2) Weighted-OLS	(3) IV	(4) Weighted-IV
Log Relative Size	-0.080**	-0.078***	-0.111***	-0.103***
	(0.032)	(0.030)	(0.032)	(0.029)
Year Effects:				
1999	0.187***	0.188***	0.197***	0.195***
	(0.029)	(0.031)	(0.026)	(0.027)
2002	0.245***	0.246***	0.256***	0.254***
	(0.027)	(0.023)	(0.024)	(0.020)
2007	0.316***	0.328***	0.338***	0.345***
	(0.031)	(0.030)	(0.029)	(0.028)
2013	0.277***	0.293***	0.295***	0.307***
	(0.032)	(0.031)	(0.028)	(0.028)
F Statistic			447	655
χ^2(p-value)		116(0.46)		113(0.53)
Observations	150	150	150	150
R-squared	0.938	0.949	0.938	0.949

Notes: Robust standard errors in parentheses. The dependent variable for all specifications is the college premiums by age and year. All specifications also include age fixed effects not reported. The instrumental variable for log relative size is log ratio of the number of college degree holders (including both male and female, employed and unemployed) to the number of non-college degree holders. Weights for specifications in columns 2 and 4 are the inverse sampling variance of estimated college premiums. Reference year is 1995.
*** p<0.01.
** p<0.05.
* p<0.1.

Table 6
Heterogeneous relative size effects across age groups.

Dependent variable: College premium	(1) Weighted-OLS	(2) Weighted-OLS	(3) Weighted-IV
Log Relative Size:			
Age 25–29 (New Entrants)	-0.142***		
	(0.050)		
Age 30–34	0.007		
	(0.060)		
Age 35–39	0.015		
	(0.055)		
Age 40–44	-0.064		
	(0.059)		
Age 45–49	-0.075		
	(0.090)		
Age 50–54	-0.172		
	(0.099)		
Age 25–29 (New Entrants)		-0.156***	-0.190***
		(0.047)	(0.044)
Age 30–54		-0.049	-0.069**
		(0.033)	(0.033)
T-test(p-value)		4.51(0.04)	6.62(0.01)
F statistic			303
χ^2(p-value)	108(0.54)	112(0.54)	109(0.52)
Observations	150	150	150
R-squared	0.953	0.951	0.951

Notes: Robust standard errors in parentheses. The dependent variables for all specifications are estimated college premiums by age and year. All specifications also include age and year fixed effects not reported. The instrumental variable for log relative size is log ratio of the number of college degree holders (including both male and female, employed and unemployed) to the number of non-college degree holders. All specifications are weighted by the inverse sampling variance of estimated college premiums.
*** p<0.01.
** p<0.05.
* p<0.1.

presented in columns 1 to 4 increase slightly, but the increases are not significant compared with the results by the broader definition of college and non-college. To make our results more comparable with Card and Lemieux (2001) using data from the U.S., U.K., and Canada, we follow their method for measuring relative size. They use the college premium (earnings gap between 4-year college and

Table 7
The results to sample including only high-school and 4-year college workers.

Dependent variable: College premiums	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) OLS	(6) OLS
Log Relative Size	-0.101***		-0.128***			
	(0.028)		(0.028)			
Log Relative Size (Age 25–29)		-0.219***		-0.225***		
		(0.045)		(0.040)		
Log Relative Size (Age 30–54)		-0.081***		-0.099***		
		(0.029)		(0.029)		
LRS (Alternative Measure)					-0.178***	
					(0.043)	
LRS (Age 25–29)						-0.323***
						(0.067)
LRS (Age 30–54)						-0.147***
						(0.045)
Year Fixed Effects:						
1999	0.257***	0.252***	0.264***	0.257***	0.267***	0.258***
	(0.040)	(0.039)	(0.036)	(0.035)	(0.039)	(0.040)
2002	0.319***	0.318***	0.325***	0.321***	0.358***	0.351***
	(0.033)	(0.032)	(0.029)	(0.028)	(0.035)	(0.035)
2007	0.560***	0.565***	0.589***	0.582***	0.588***	0.580***
	(0.048)	(0.048)	(0.045)	(0.045)	(0.050)	(0.052)
2013	0.359***	0.353***	0.383***	0.369***	0.399***	0.386***
	(0.043)	(0.044)	(0.039)	(0.040)	(0.048)	(0.050)
F statistic			326	164		
Observations	150	150	150	150	150	150
R-squared	0.929	0.933	0.928	0.933	0.930	0.932

Notes: Robust standard errors in parentheses. The dependent variables for all specifications are the estimated college premiums by age and year. All specifications also include age fixed effects not reported. In column 3 and 4, the instrumental variable for log relative size is log ratio of the number of all college degree holders (including both male and female, employed and unemployed) to the number of non-college degree holders. To compare with the result by Card and Lemieux (2001), the alternative measure for log relative size, LRS in columns 5 and 6 is constructed based on all education levels rather than only high-school and 4-year college. Reference year is 1995.

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

high school) as the dependent variable while using a relative size measure based on all education levels as the independent variable.[37] We follow their measure for relative size notated as *LRS* in Table 7. The estimated effect, -0.178, in column 5 is much larger by magnitude than -0.101 in column 1 and becomes very similar to the results by Card and Lemieux (2001), -0.203 for the U.S., -0.233 for U.K. and -0.165 for Canada.[38] That the negative supply effect in China is so close to these three countries is remarkable in view of the very different economic development levels, trends of the college premiums and the relative supply, and higher education expansion phases between China and the other three countries. It is more interesting considering that the estimate of the supply effect should be a lower bound in China and an upper bound in the other three countries.

6. Robustness checks

In this section, we first test the underlying assumption of our main specification 3.4. After presenting the positive results for the assumption that college workers and non-college workers are equally substitutable across age cohort, we use several alternative specifications to check the robustness of the effect of age specific relative size of college workers on the age specific college premium.

6.1. Testing the assumption: equal education-specific elasticity of substitution

As we discussed in Section 2.3, to directly link the relative size of college workers and the college premium like Equation (2.6) entails the assumption of identical elasticity of substitution across age cohorts for college and non-college groups. The testing results are presented in Table 8. In column 1, we estimate a model based on Equation (3.2) without controlling for the age-year two-way fixed effects. The estimated effect of college workers' size on college workers' average earnings is significantly negative, -0.146, while that for non-college workers is insignificantly positive, 0.04. By the high F statistic with a nearly zero p-value, we have to reject the null

[37] To account for differences in the effective labor supply by different education levels, they also assign a weight to each level with the average earnings. However, we have to point out that they use hourly wage rates and annual working hours to construct their college premium and relative size. In our data, information about working hours is not available.

[38] The larger estimated absolute effects by this alternative measure *LRS* come from its high correlation with the basic measure and its smaller variation. A one unit change in this alternative measure is associated with about 1.5 units change in the basic measure.

Table 8
Testing assumption: Identical elasticity of substitution for college and non-college workers.

	(1) Average earnings	(2) Average earnings	(3) College premium
Log Size of College Workers	-0.146*** (0.029)	-0.096** (0.037)	0.093*** (0.035)
Log Size of Non-College Workers	0.040 (0.031)	-0.067 (0.042)	-0.062 (0.039)
F Statistic testing “Identical Effects”	28.95 (0.000)	0.41 (0.522)	0.47 (0.495)
χ^2 Statistic testing “No Specification Errors”	409.86 (0.000)	120.64 (0.313)	115.39 (0.446)
Age × *Year* Fixed Effects	NO	YES	NO
Observations	300	300	150
R-squared	0.989	0.997	0.949

Notes: Robust standard errors in parentheses. All regressions are weighted by the inverse sampling variance of the corresponding dependent variable. Specifications in column 1 and 2 also include a set of year and age effects fully interacted with college dummy variable. Specification in column 3 also includes age and year fixed effects.
*** p<0.01.
** p<0.05.
* p<0.1.

hypothesis of identical effects. However, we can reject the hypothesis of no specification error at the 1% level as the corresponding χ^2 statistic indicates. After we control for the age-year two-way fixed effects in the specification for column 2, we find that the age-specific size effects for college workers and non-college workers are similar, and we can’t reject the null hypothesis of identical effects by the corresponding F statistic, 0.41 with p-value 0.522. Meanwhile, the χ^2 statistic testing the hypothesis that there is no specification error reduces substantially from 409.86 in column 1 to 120.64 with a p-value of 0.313. The comparison implies a common age-year fixed effect on average earnings for college and non-college workers. In column 3, the equivalent specification to that for column 2 is based on Equation (3.3), which leads to estimates with almost identical magnitudes. The opposite signs of the estimated effects are consistent with the model implication since the dependent variable is the estimated college premium instead of education-specific average earnings. The corresponding F and χ^2 statistics have large p-values, which indicates that we can’t reject the null hypothesis of identical effects and the null hypothesis of no specification error.

6.2. *Controlling for occupation and industry*

To deal with the potential confounding factors due to occupation and industry compositions, we directly control for these factors with individual data based on Equation (3.6). Results are presented in Table 9. In columns 1 and 2, we present results without

Table 9
Results using individual data controlling for province, occupation and industry.

Dependent variable: Log annual earnings	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) IV	(6) IV
College × *Log RelativeSize*	-0.074** (0.031)		-0.083*** (0.028)		-0.106*** (0.031)	
College × *Log RelativeSize* (Age 25–29)		-0.191*** (0.049)		-0.177*** (0.044)		-0.208*** (0.047)
College × *Log RelativeSize* (Age 30–54)		-0.044 (0.034)		-0.057* (0.031)		-0.069** (0.034)
Log RelativeSize	0.166*** (0.019)	0.296*** (0.034)	0.144*** (0.017)	0.243*** (0.031)	0.184*** (0.019)	0.281*** (0.034)
Log RelativeSize× 1[Age 30–54]		-0.170*** (0.036)		-0.128*** (0.033)		-0.128*** (0.036)
(Province,Occupation,Industry)×Year	NO	NO	YES	YES	YES	YES
F-statistic					16000	6731
Observations	23,428	23,428	23,428	23,428	23,428	23,428
R-squared	0.573	0.573	0.653	0.653	0.653	0.654

Notes: Robust standard errors in parentheses. All specifications also include college, age, and year fixed effects, the interaction between college and year fixed effects, and the interaction between college and age fixed effects.
*** p<0.01.
** p<0.05.
* p<0.1.

Table 10
Robustness of the results to female sample and pooled sample including both male and female.

Dependent variable: College premiums	(1) Women only	(2) Women only	(3) Men and women	(4) Men and women
Log Relative Size	-0.044		-0.071**	
	(0.042)		(0.028)	
Log Relative Size (Age 25–29)		-0.128***		-0.161***
		(0.045)		(0.033)
Log Relative Size (Age 30–54)		-0.016		-0.034
		(0.041)		(0.026)
Year Fixed Effects:				
1999	0.132***	0.124***	0.175***	0.165***
	(0.039)	(0.037)	(0.023)	(0.022)
2002	0.178***	0.172***	0.226***	0.216***
	(0.042)	(0.041)	(0.023)	(0.023)
2007	0.243***	0.233***	0.307***	0.295***
	(0.055)	(0.053)	(0.030)	(0.029)
2013	0.205***	0.193***	0.273***	0.255***
	(0.056)	(0.054)	(0.030)	(0.030)
Observations	150	150	150	150
R-squared	0.955	0.957	0.972	0.975

Notes: Robust standard errors in parentheses. The dependent variables for all specifications are estimated college premiums by age and year. All specifications also include age fixed effects not reported. All specification are weighted by the inverse sampling variance of the estimated college premiums.
*** $p<0.01$.
** $p<0.05$.
* $p<0.1$.

controlling for occupation or industry as a comparison with the results by structural specifications in which these composition effects are not controlled either. As expected, we obtain very similar results of the effects of relative size on the college premium. The estimated average effect is -0.074 in column 1, while the effect is -0.191 for the entrant group and -0.044 for the older group in column 2. After controlling for year-varying fixed effects of occupation, industry and province, the results change slightly and the changes are not significant. This implies that these suspected confounding composition effects are not serious issues. In columns 5 and 6, we present IV estimates that are very similar to corresponding estimates in Tables 5 and 6.

6.3. *Results for women only and pooled women and men*

Focusing on men is appropriate conditional on a strong assumption that men and women in the same age cohort, education level, and survey year are not substitutable. Therefore, we first replicate our analysis for women only and then pool women and men under the assumption that men and women in the same age cohort, education level and survey year are perfectly substitutable. For the sake of brevity, we only present OLS estimates in Table 10. The results with women only in columns 1 and 2 are smaller by magnitude and less precise than those with men only, while the results with both men and women are very similar. Another interesting finding comes from the difference in year trends between women and men. Comparing the estimated fixed year effects in column 1 of Table 10 and column 2 in Table 5, we can find that men's college premium increased more rapidly than women's from 1995 to 2013.

6.4. *Several other specification checks*

We have performed several other specification checks of which the results are presented in Table 11.

Firstly, we notice that CHIP 1999 and 2007 draw samples from provinces that are partially different from those in CHIP 1995, 2002 and 2013 even though each wave is nationally representative. Therefore, it is natural to check the robustness using CHIP 1995, 2002 and 2013 only to keep the province composition constant.[39] The corresponding results are presented in columns 1 and 2.

Secondly, by checking individual's rural-urban migration status, we find that the proportion of rural-urban migrants increased steadily from about 18 percent in 1999 to about 32 percent in 2013.[40] Considering that including rural-urban migrants may introduce an added source of variation in the college premium due to endogenous self-section, we focus on those non-migrants to check the robustness of relative size effect on the college premium and present the results in columns 3 and 4.

Lastly, to reduce sampling variations, we also construct broader cells based on three-year age and survey year at the expense of

[39] Even though we have controlled for province fixed effects in our previous specification with individual data, we perform the estimation with a structural model as a double-check.

[40] We define those born with rural residence registration changed to urban residence registration. In CHIP 1995, we can't accurately identify the migration status that we only use the waves 1999, 2002, 2007 and 2013.

Table 11
Robustness of the results to several alternative specifications.

Dependent variable: College premium	(1) Same provinces	(2) Same provinces	(3) Non-migrants only	(4) Non-migrants only	(5) 3-age-year cells	(6) 3-age-year cells
Panel A: OLS Estimates:						
Log Relative Size	-0.069		-0.069		-0.101***	
	(0.045)		(0.043)		(0.026)	
Log Relative Size (Age 25–29)		-0.290**		-0.132*		-0.158***
		(0.117)		(0.071)		(0.030)
Log Relative Size (Age 30–54)		-0.045		-0.041		-0.077**
		(0.041)		(0.052)		(0.031)
R-squared	0.955	0.959	0.956	0.956	0.986	0.987
Panel B: IV Estimates:						
Log Relative Size	-0.090**		-0.113**		-0.109***	
	(0.046)		(0.045)		(0.022)	
Log Relative Size (Age 25–29)		-0.302***		-0.191***		-0.172***
		(0.089)		(0.061)		(0.029)
Log Relative Size (Age 30–54)		-0.053		-0.074		-0.084***
		(0.042)		(0.055)		(0.026)
F-statistic	208	90	259	98	475	174
R-squared	0.955	0.959	0.955	0.956	0.986	0.987
Observations	90	90	120	120	50	50

Notes: Robust standard errors in parentheses. The dependent variables for all specifications are estimated college premiums for by age and year (or by 3-age and year). All specifications also include age and year fixed effects not reported. All specification are weighted by the inverse sampling variance of the estimated college premiums. The instrumental variable for log relative size is log ratio of the number of college degree holders (including both male and female, employed and unemployed) to the number of non-college degree holders.

*** $p<0.01$.

** $p<0.05$.

* $p<0.1$.

reducing the number of cells by two-thirds, from 150 to 50. The corresponding results are presented in columns 5 and 6.

Even if the OLS and IV estimates in columns 1–4 are less precise than our previous main results due to the drop of CHIP 1995 (or both 1995 and 2007), their magnitudes are similar. The results with broader cells shown in columns 5 and 6 are significantly negative with similar magnitudes. Overall, these alternative specifications show robust results of the relative size effects on college premium.

7. Conclusion

In this paper, we document the divergent trends of the college premiums across age groups from 1995 to 2013 in China. Comparing with the well-studied increasing overall trend during the same period, this divergence has received little attention. Specifically, the college premium in 2013 for the younger group (age 25–34) was about 30 percentage points, similar to the level in 1995, while the college premium in 2013 for the older group (age 45–54) increased to 50 percentage points nearly double that of 1995. To attribute these divergent trends of college premium to the changes in the relative size of college workers, we use the model by Card and Lemieux (2001) which incorporates imperfect substitution between similarly educated workers in different age cohorts. Due to the distinctions of these trends in China, our identification is free of the overestimation issue due to the technological progress that possibly favored younger college workers in particular. Our results are similar to those in the U.S., U.K., Canada, and Japan. Holding the age cohort and survey year constant, a one unit increase in the relative size of college workers is associated with about 10 percentage points decrease in college/non-college premium and about 18 percentage points decrease in college/high school premium. That the negative supply effect in China is so close to the other four countries is remarkable given the very different economic development levels, trends of the college premiums and the relative supply, and higher education expansion phases between China and the other four countries.

We further find that the negative effect is much more substantial among the new entrants (age 25–29) than experienced workers (age 30–54). By this pattern, we demonstrate that the new labor market entrants are more sensitive to their own cohort relative size and argue that the confounding ability composition effect should not be a severe issue.

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