

Higher Education Expansion, Labor Market, and Firm Productivity in Vietnam^{*}

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Abstract

We study the role of higher education on workers and firms by examining a national expansion of higher education in Vietnam, which established over 100 universities from 2006 to 2013. Collecting a dataset on the timing and location of university openings, we estimate that individual's exposure to a university opening increases their chances of completing college by over 30%. It also raises their wage by 3.9% and household expenditure by 14%. At the market level, the expansion increases the relative supply of college-educated workers, thus reducing the college wage premium. In response, firms substitute non-college workers with skill-biased capital, allowing them to raise productivity and hire more college-educated workers. We also find that opening a university in one district has substantial spatial spillover effects on to the labor market of nearby districts.

Keywords: higher education, local labor market, labor productivity, Vietnam

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1 Introduction

Higher education can be an important driver of economic development. On the one hand, it can speed up the structural transformation process by allowing workers move into skill-intensive sectors. On the other hand, it can also induce firms to upgrade their capital or technology, thus raising the overall productivity.¹ Universities, such as land-grant colleges in the United States, can also be a frontier of knowledge discoveries and innovative activities.² Yet outside developed countries, there is skepticism towards the value and quality of higher education, raising concerns about efforts to expand access to higher education instead of focusing on basic education (Hanushek, 2016).

We study the effects of higher education on workers and firms by examining a national expansion of higher education in Vietnam caused by Decree 121/2007/QĐ-TTg. This policy led to the establishment of over 100 new universities between 2006 and 2013; the share of young adults with a college education tripled between 2004 and 2018 (see Figure 1). Studying such a policy would provide valuable lessons not only to Vietnamese policymakers but also those in other fast-growing low-middle income countries like Bangladesh, India, Ghana, and Cambodia. These economies, including Vietnam, have faced a rapid increase in demand for high-skilled labors as their products become increasingly more complex over the last two decades, as captured by the Economic Complexity Index (see Figure 2).³

To assess this policy shock, we collect a new dataset on timing and locations of all university openings in Vietnam⁴ and combine with labor force survey and firm census data. In the first part of the paper, we estimate the effects on individual workers using a difference-

¹See, e.g., Acemoglu (1998), Aghion et al. (2009), and Verhoogen (2021).

²See, e.g., Hausman (2020) and Andrews (2020).

³The Economic Complexity Index measures the number and complexity of a country’s exported products, which is driven by skill and knowledge (Hausmann et al., 2007). As countries export more complex products, they likely require more high-skilled labors to produce them.

⁴All public and private university openings require official approvals from the government, which are publicly available and contain information about the location and timing of university openings. We hand-entered these information for all universities established in Vietnam since 1975.

in-differences (DiD) model that compares outcome across birth cohorts and provinces on the Labor Force Survey (LFS). Being exposed to a new university at college-going age increases the probability of completing college by 34% and monthly wage by 3.9%. These results remain robust when we use a change-in-changes (CiC) model to relax the parallel trends assumption (Athey and Imbens, 2006) and when we account for treatment effect heterogeneity and the small number of clusters. We show that accounting for inter-provincial migration does not affect our findings.

These results suggest that college education is highly valuable in a fast-growing developing economy like Vietnam. Using a Wald estimator combined with the DiD and CiC models, we find the implied returns to college education to be around 139% for those who completed college because of the expansion. Consistent with this result, we observe that expanding access to higher education reduces the likelihood of agricultural employment and increases that of service employment. Workers are also likely to be in higher-paid occupations such as scientists, experts, and managers. In other words, the expansion contributes to the structural transformation process. Using the Vietnam Household Living Standard Survey (VHLSS), we find that the expansion also raises individuals' household expenditure substantially, which also supports our conclusion about high returns to higher education.

In the second part of the paper, we explore the expansion affects firms through changes in the local labor market. A capital-skill complementarity model predicts that an increase in the supply of college-educated workers would decrease the college wage premium; in response, firms would substitute non-college workers with skill-biased capital, allowing firms to absorb some of the labor supply shock (Lewis, 2011, 2013). After presenting this model, we use a DiD model comparing local labor markets at the district level over time that allows for spatial spillovers⁵ using aggregated LFS data.

⁵We relax the conventional Stable Unit Treatment Value Assumption (SUTVA) by only using districts sufficiently far from treatment districts as the control group. This allows us to estimate the effects on both treatment districts and districts that are closer to treatment (Butts, 2021).

Our results indicate that the expansion raises the relative supply of college-educated workers by 39.8% and reduces the college wage premium by 14.9%. The inferred elasticity of substitution between college and non-college workers is about 2.67, which is consistent with the empirical literature in the US (Acemoglu and Autor, 2011). The expansion also raises the employment of college-educated workers and decreases that of non-college workers, indicating that firms adjust their input mix in response to the expansion.

Applying the same empirical strategy on firm-level data from the Vietnam Enterprise Census (VEC), we observe that firms experience an increase in productivity, measured by both total factor productivity (TFP)⁶ and labor productivity, shortly after a university opening. Firms also hire more college-educated workers after the increase in productivity. The timing of the productivity shock, i.e., right after a university opening, suggests that firms respond to a shortage of non-college workers (who now attend university) by substituting them with skill-biased capital. This substitution raises their productivity and allows them to hire more college-educated workers, which is consistent with a capital-skill complementarity model (Lewis, 2013).⁷

Our approach also allows us to document substantial spillovers on districts located within 25 kilometers of a district with new universities but not for districts that are further away. This result is consistent with a growing literature on the effects of college proximity on enrollment and other labor market outcomes (e.g., Frenette, 2006, 2009; Doyle and Skinner, 2016; Lapid, 2017). We also highlight the importance of spatial spillover which is often unaddressed in the school opening literature in developing countries.

Our findings contribute to three important bodies of literature. First, higher education as an important driver of economic growth has mainly been studied in developed countries. An

⁶We estimate production functions using Akerberg et al. (2015)'s approach to obtain TFP.

⁷We also consider three other alternative explanations, including adjustment in production technology (Beaudry et al., 2010; Clemens et al., 2018), human capital spillover (Moretti, 2004a,b), and agglomeration effects of new universities (Glaeser and Resseger, 2010; Liu, 2015).

increase in college-educated workers can affect economic growth through the R&D market or productivity spillover.⁸ A relatively recent literature established the importance of land-grant colleges in the United States as they drive local educational attainment Russell et al. (2022), innovation (Hausman, 2020; Andrews, 2020), consumption (Liu and Yang, 2021), and agglomeration (Liu, 2015). These channels are less relevant in developing countries given the concerns about low quality of higher education (Hanushek, 2016) and lacks of incentive and a formal market for innovative activities (Acemoglu et al., 2006; Vandenbussche et al., 2006). Even so, our findings suggest that higher education can still play an instrumental role in shaping structural transformation and firms’ productivity growth.

Second, we contribute to the literature on education in developing countries,⁹ which often overlooks post-secondary education and firms’ responses to educational policies. While the benefits of basic education is well-documented (e.g., Duflo, 2001; Khanna, 2015; Akresh et al., 2018; Hsiao, 2022), there is often skepticism regarding the value of higher education (Hanushek, 2016).¹⁰ Furthermore, very few studies consider how firms respond to changes in educational policies (e.g., Khanna, 2015; Che and Zhang, 2018), which is an important channel through which education can affect economic growth. We also provide an estimate for spillover effects of higher education, which is rarely studied in developing countries.

There are also a number of existing studies exploring the effects of a higher education expansion in China in 1999 on labor market outcomes,¹¹ firm productivity (Che and Zhang, 2018), and technological adoption (Feng et al., 2018). Unlike the expansion in Vietnam, which established new universities, the expansion in China raised the college admission quotas

⁸See, e.g., Acemoglu (1998); Moretti (2004a,b); Vandenbussche et al. (2006).

⁹This literature includes a small but growing literature on education in Vietnam. Dang and Glewwe (2018) and Dang et al. (2021) explore Vietnam’s exceptional performance in basic education relative to its past and other countries. Phan and Coxhead (2013) examine the economic forces behind changes in returns to schooling. Coxhead and Shrestha (2017) study how foreign direct investment affects schooling decisions. Several studies examine the overall changes in returns to schooling in Vietnam (e.g., Patrinos et al., 2018; Doan et al., 2018; McGuinness et al., 2021)

¹⁰A related literature studies whether the value of higher education in developing countries is due to human capital or signaling (Arteaga, 2018; Barrera-Orsorio and Bayona-Rodríguez, 2019).

¹¹See, e.g., Li et al. (2014, 2017b,a); Xing et al. (2018); Huang et al. (2022)

(Li et al., 2017a), which also raised college enrollment and attainment. The key difference between our study and these studies is that we can exploit the variation in locations and timing of new universities, while other studies rely on enrollment rates or indirect measures of exposure such as rural/urban status which may proxy for many factors. Our approach allows us to directly capture the effect of expanding access to higher education.

The rest of the paper is organized as follows. In Section 2 and 3, we briefly discuss about the national expansion of higher education and the data sources that we use. In Section 4, we discuss the empirical strategy to evaluate the effects of individual exposure to the expansion and the results from our estimations. In Section 5, we present a capital-skill complementarity model to understand how the expansion may affect firms through the labor market. We then discuss how we evaluate the effects on these outcomes and the results from our estimations. We discuss the implications and shortcomings of our study in Section 6.

2 Background of the higher education expansion in Vietnam

The Decree 121/2007 was signed by the Prime Minister Nguyen Tan Dung, stating the overall plan of the government to expand the network of higher education institutions across the country during the 2006-2020 period. As a result of this plan, the number of universities in Vietnam went from 150 in 2005 to over 250 in 2014 (see Figure 1). In 2013, the government announced that they already reached the number of universities planned for this period, so no new universities were established after that (with a few exceptions).¹²

Universities are also distributed unevenly across 63 provinces of Vietnam, as shown in Figure 3. Ho Chi Minh City, Hanoi, and Hue were the three "centers" before the expansion,

¹²See Parajuli et al. (2020) and Vu and Nguyen (2018) for overviews of this policy.

with more than 6 universities in each city/province. After the expansion, other provinces also saw an increase in the number of universities, such as Thai Nguyen, Nghe An in the North and Binh Duong, Dong Nai in the South. Many provinces had the first university open during this period.

To understand the impacts of these new universities, we collect data on all universities (including existing universities) from official documents of the government. Both public and private universities were established during this expansion. While public universities are typically “upgraded” from two-year colleges, private universities tend to be established directly. Public universities are established by officials at the province level. However, both types of universities have to justify to the government that they have enough staff, faculty, infrastructure, and land to operate a university.

New universities tend to be opened in provinces with workers in more skill-intensive sectors. We present provincial characteristics (before treatment) for three groups of provinces: those that do not have any university ever (our control group), those that opened a university for the first time (our treatment group), and provinces that already have a university, i.e., the “already-treated” group, in Table 1. The treatment group has a smaller share of agricultural workers and larger shares of workers in the manufacturing and service sectors. It is also unsurprising to see that the treatment group has a lower poverty rate than the control group. Besides these characteristics, the treatment and the control groups are relative similar in terms of other economic indicators such as employment rate, self-employment rate, and firm-level performances such as total factor productivity, labor productivity, and capital-labor ratio, we find little differences between the treatment group and the control group.

It is worth pointing out that the already-treated provinces are substantially different from the control group. The adult population tends to have better education, which is expected given that there are already existing universities. These provinces also have lower percentage of self-employment, higher share of employment, higher shares of manufacturing and service

workers, and better economic conditions.

These results reflect the nonrandom nature of where new universities are located. Provinces that open new universities likely respond to growing demands for higher education and a growing workforce in industries that require more education. However, the overall economic conditions are more similar to those in the control group than the already-treated group. These results strongly suggest that simply comparing across provinces may yield biased estimates because of selection into establishing new universities.

3 Data

Our study draws from several data sources. Data on individual and market-level labor market outcomes are based on the Labor Force Survey (LFS). We use the individual-level data for 2010-2018 to examine the individual benefits of being exposed to the higher education expansion. For the market-level analysis, we aggregate this data at the district-by-year level. It is also important to note that this data only has information on districts for 2011 and 2015-2019. This limits our ability to estimate event-study specifications at the district level.

For the individual-level analysis, we focus on college education, monthly wage, and employment as the main outcomes of interest. The LFS survey asks respondents for their highest educational attainment, which we use to construct a binary variable for whether an individual has obtained a 4-year university degree or more. Employment is measured by whether respondents are employed, as opposed to being self-employed or not working. We also use whether individuals earn wage as an alternative measure for employment. Monthly wage only includes wage compensation and not other benefits such as bonuses for consistency across all years. We have this information for most workers, including those who are self-employed.

For the market-level analysis in the second part of the paper, we aggregate data at the

district-by-year level. Our first outcome variable is the share of working-age adults who have completed college education. As guided by a capital-skill complementarity model discussed later, we examine the effects of the expansion on the relative supply of college-educated workers, $\ln(\frac{H}{L})$. We measure H and L as the number of college-educated and non-college adults with non-zero monthly wage. The second main outcome of interest is college wage premium, also known as the relative wage of college-educated workers, measured as the log ratio of college-educated monthly wage to non-college monthly wage, i.e., $\ln(\frac{w_H}{w_L})$. All zero wage is treated as missing and dropped before aggregation. For both types of analyses, we restrict our analyses to those who are between age 22 and 54 since they are most likely out of school and active in the labor market. Given that Hanoi and Ho Chi Minh city are both the centers of economic growth and universities of Vietnam, we also exclude them from the analyses.

Lastly, we study firms' response to the skill mix shock along three dimensions, total factor productivity (TFP), labor productivity, and capital intensity using firm-level data from the Vietnam Enterprise Census (VEC) data for 2006-2018. This data contains detailed accounting information on firms' annual operations, such as short and long-term assets, total labor, total revenue, and industries. We measure labor productivity by value added per labor, where value-added is defined as profit plus wage (Newman et al., 2015). To measure TFP of each firm, we first estimate the relevant production function for all 2-digit industries using Akerberg et al. (2015)'s approach then use the estimated parameters to obtain the total factor productivity (see Appendix C for a detailed discussion about the estimation process and results). We measure capital intensity as the ratio of capital to revenue.

4 Impacts on individual-level outcomes

In this section, we explore the effects of the expansion on individual-level outcomes. The expansion increases accessibility to higher education for younger cohorts; specifically, those who were at the college-going age when there were more new universities because of the reform. In contrast, it does not affect older cohorts who might already be too old to benefit from the increase in number of universities. As the younger cohorts have better access to higher education, they may be more likely to obtain a university degree, and hence, may do better than older cohorts in terms of employment and wage. We discuss this difference-in-differences design and estimation in Section 4.1 and the results in Section 4.2.

We can also assume that the effect of individual exposure on wage only operates through the college completion effect, i.e., an exclusion restriction assumption, to calculate the implied rate of returns to college education in Vietnam, which we discuss in Section 4.3. We reconcile the difference between our relatively high estimated returns and the literature on returns to higher education by looking at the effects on sectoral employment in Section 4.3 and household expenditure in Section 4.5. Our findings indicate that expanding higher education substantially improves individual welfare in terms of wage and expenditure by allowing them to work in higher-paid jobs and more skill-intensive industries.

4.1 Empirical strategy

In a standard difference-in-differences design, we compare provinces that never had a university with provinces that had a new university for the first time during our study period across different birth cohorts (see Figure 5). In other words, we identify the effect from variation in exposure across birth cohorts and provinces. Using never-treated provinces as the control group allows us to avoid the potential concern that provinces that opened a university in the past might have still been affected by that university. We map the treatment and control

provinces in Figure 4.

Our main model is a standard difference-in-differences model (DiD), in which T_p denotes whether province of current residence p established a university for the first time and $Exposed_c$ denotes whether the birth cohort c were at college-going age during the expansion. Since the expansion started in 2006, we define the exposed cohorts as those who were born in 1986 or later.

$$Y_{i,p,c,t} = \delta.(T_p \times Exposed_c) + \mathbf{X}\boldsymbol{\theta} + u_{p,t,c} + \gamma_{p,t} + \eta_c + \epsilon_{i,p,c,t} \quad (1)$$

where $Y_{i,p,c,t}$ denotes the outcome variable of individual i in province p of cohort c in year t ; $\gamma_{p,t}$ and η_c are province-by-year fixed effects and cohort fixed effects. \mathbf{X} is a vector of control variables which include age, age squared, and gender. One potential concern is that both treated and untreated provinces may be affected by universities in neighboring provinces. While the province-by-year fixed effects would absorb any contemporaneous effect from this factor, it may still bias our results if its effects differ across birth cohorts. Therefore, we control for the number of universities in neighboring provinces interacted with birth cohort fixed effects; we denote this $u_{p,t,c}$.

We estimate Equation 1 on the Labor Force Survey (LFS) sample in 2015-2019. Since we are interested in the effects on employment and wage, we restrict our analysis to a working-age sample, i.e., individuals between age 22 and 55. We further restrict our analysis to the birth cohorts born between 1970 and 1994.

This difference-in-differences (DiD) model imposes a conditional parallel trends assumption, i.e., without the expansion, the outcome variables would have evolved similarly for the treatment and control provinces, conditional on the fixed effects and control variables. For the rest of this section, we discuss when this assumption might be violated and other concerns about estimation and inference strategy as well as how we address these concerns.

4.1.1 Threats to the parallel trends assumption

There are two major threats to this research design. First, the decision to establish a university is not random as we show in the previous section; provinces that established a university differ from provinces that did not. Although the province-year fixed effects can account for province-level characteristics that may confound with educational attainment, the TWFE estimate can still be biased by confounders that affect this outcome differentially across cohorts. Figure 5 provides reassuring evidence that this is not the case; the gap between the control and treatment groups is relatively stable across cohorts up until the exposed cohorts.

We can also control for province-specific cohort trends, effectively allowing the outcome variables to trend differently across provinces. Alternatively, we control for pre-expansion characteristics interacted with cohort trends to absorb the cohort-specific shocks of these potentially confounding factors. We specifically control for share of college enrollment, shares of workers in agriculture, manufacturing, and service sectors, as well as poverty rates since the two groups appear to differ along these characteristics, as shown in Table 1.

These controls may or may not sufficiently account for cohort-variant confounders. For example, new universities may be located in provinces where there is a rise in demand for skilled labor. In such a scenario, college attainment and labor market outcomes would have been affected even in the absence of new universities. One useful placebo test is to examine the effect of exposure to new university opening on high school completion. New universities do not directly affect whether students complete high school but labor market conditions do (e.g., Coxhead et al., 2022). Therefore, if college completion (and other outcomes) are affected by factors unrelated to new university opening, e.g., provincial economic conditions, the model would yield significant effects on high school completion.

One potential concern with this placebo test is that new universities may change the attitude towards higher education and, thus, motivates high school students to complete

their education (Russell et al., 2022). We can assess this possibility since we also estimate an event study model to look at effects for different birth cohorts. For the 1986-1989 birth cohorts, they would be too old for high school and still young enough for college and, thus, they would not be affected by this potential attitude change. The effects on these cohorts, therefore, can still serve as a placebo test for our purpose.

A more severe threat to our identification strategy is inter-province migration. New universities increase the supply of college-educated workers, thus reducing their wage level at the market level. This may induce college-educated workers to migrate to other provinces. Alternatively, new universities may create more job opportunities for college-educated workers and attract even more of these workers. In other words, our results might be driven by the educational attainment of the out-migrants and in-migrants. We address this issue by running the same regression on province of origin instead of province of current residence. The results, therefore, should not be affected by changes in the composition of provinces of current residence.

For the continuous monthly wage variable, we can further check if our results are robust when relaxing the parallel trends assumption of the standard DiD model, i.e., the average outcome of the treated provinces would have evolved similarly as the average outcome of the control provinces in the absence of treatment. In other words, in the absence of treatment, we assume no distributional change of the outcome variable. Athey and Imbens (2006) propose a change-in-changes (CiC) model that relaxes this distributional assumption. Specifically, it allows the distribution of outcome to vary in terms of mean and variance in the absence of treatment. The identifying assumption of this model is that in the absence of treatment, the distribution of the unobservables can vary between the treatment and control groups, but not across time within group (Athey and Imbens, 2006; Imbens and Wooldridge, 2009). If this alternative model yields a similar result to that of the standard DiD model even though it does not rely on the parallel trends assumption, we can be more confident that our results

are not driven by a violation of such assumption.

4.1.2 Other concerns about estimation and inference

One limitation of the main DiD model is that it does not account for the fact that universities are opened in different years, i.e., varying treatment timing across provinces, and hence, may be misspecified. This is also related to a larger concern about estimating treatment effects in a staggered timing design (Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021; Borusyak et al., 2021; Baker et al., 2022). Specifically, the conventional TWFE estimator may suffer from negative weights in a staggered timing setting, as it compares a control group, comprised of already-treated provinces, to a treatment group, comprised of not-yet-treated provinces.

Another important concern about our results is that we only use 15 untreated and 21 treated clusters. It is well-documented that cluster-robust inference on a small number of clusters may lead to downward bias in the variance matrix estimate; as a result, t-statistics based on cluster-robust inference may severely over-reject the null hypothesis (Cameron et al., 2008; Cameron and Miller, 2015). While wild cluster bootstrap is a common solution for this problem, MacKinnon and Webb (2017) and MacKinnon and Webb (2018) show that this approach may still be invalid in a DiD setting with a small number of *treated* clusters. Instead, a subcluster wild bootstrap may be more appropriate.

In Appendix B, we show that using different inference and estimation methods that are robust to these problems would yield very similar results to our main findings. Specifically, we show that the cluster-robust inference, i.e., clustering on province, provide similar p-values compared to different bootstrap procedures that account for both problems of too few clusters and too few treated clusters. More importantly, we also show that applying a staggered timing DiD design also yields similar estimates for the treatment effects of the expansion, especially after we account for treatment effect heterogeneity.

4.2 Results

In Table 2, we show the descriptive statistics of the relevant variables for this analysis. Specifically, we present the mean and standard deviation for the unexposed cohorts who were born between 1970 and 1985 and the exposed cohorts who were born between 1986 and 1994. We do this separately for the control provinces, i.e., those that never had a university, the treatment provinces, i.e., those that had a new university for the first time due to the expansion, and the already-treated provinces, i.e., those that had an existing university before the expansion.

In Table 3, we present the results from estimating the cohort-level DiD model in Equation 1. We estimate three different specifications of the TWFE model. In specification 1, we only control for individual characteristics (age, age squared, and female), province-by-sample year fixed effects, and birth cohort fixed effects. In specification 2, we further control for pre-treatment characteristics at the province level interacted with a linear cohort trend term. This accounts for the possibility that these characteristics may have differential effects across cohorts. In specification 3, we instead control for province-specific linear cohort trends; this allows provinces to trend differently across cohorts. In specification 4, we estimate the same model as specification 3 while using an exposure measure based on provinces of origin instead of provinces of current residence. This allows us to assess the role of inter-provincial migration in our main findings. It is important to note that the sample size changes for this specification because it depends on the number of observations came from the first-time treated and never-treated provinces (instead of those who are currently residing in these provinces).

We find that the expansion raises the chance of completing college of exposed individuals by 2.3 to 3.2 percentage points, as indicated by the results in column (1). In the base specification, the estimate is 3.2 percentage points and statistically significant at the 5%

level. Controlling for pre-treatment characteristics interacted with linear trends reduces this estimate to 2.3 percentage points. When we control for province-specific trends instead, the result is 2.7 percentage points. When we use provinces of origin instead of provinces of current residence to construct the exposure variables (in specification 4), the estimate is still 2.7 percentage points. In other words, inter-provincial migration plays a minimal role in driving our main result. The result in the least parsimonious specification suggests that opening a university would increase the probability of completing college education by 2.3 percentage points or 28% given that the probability of completing college or higher of the post-expansion cohorts in the control provinces is 8.2% (see Table 2).

In column (2), we show the effects on high school completion as a placebo test. That is, if the estimate for the effect on college completion is driven purely by new university opening and not other unrelated factors, we would expect to see minimal and insignificant effect on high school completion. This is indeed what we find. In the base specification, the estimate is 0.8 percentage points. This is drastically smaller than the effect on college completion. Controlling for pre-treatment characteristics interacted with cohort trends bring the estimate down to 0.4 percentage points. Controlling for province-specific trends yields an estimate of negative 0.6 percentage points. Using province of origin yields a similar estimate of negative 0.7 percentage points. The results from all four specifications are close to zero and statistically insignificant. These results provide reassuring evidence that our main result is not driven by unrelated factors.

Since college-educated individuals are more likely to be employed, it is unsurprising that we also find a positive and significant effect of exposure to the expansion on employment, measured by whether an individual is employed in either formal or informal sectors (see column (3)). The estimate from the base TWFE model is 8.5 percentage points and statistically significant. Unlike college education, controlling for pre-expansion characteristics or province-specific trends lowers the estimation result considerably. The estimate for the

former is 3.2 percentage points and that of the latter is 4.3 percentage points, although the estimates remain statistically significant. This suggests that pre-expansion characteristics are important confounding factors of employment outcome than college education outcome. Accounting for migration yields an estimate of 3.9 percentage points, which is, again, similar to our main results.

We also find that the expansion significantly increases the chance that an exposed individual works in the formal sector, defined as having a job that provides social insurance, in column (4). The estimate is slightly lower than the effect on employment, 7.7 percentage points in the base specification. When further controlling for pre-treat characteristics, the estimated coefficient is reduced to 2.5 percentage points. When allowing for province-specific trends, the estimate becomes 3.9 percentage points. Using province of origin brings this estimate down to 3.6 percentage points.

In column (5), we consider the effects of being exposed to the higher education expansion on log monthly wage. Note that this measures any amount of income that individuals receive from their main job and, thus, includes self-employment income. In the base specification, the estimate is 7.7 percentage points and statistically significant. Controlling for pre-treatment characteristics reduces this estimate to 4.6 percentage points, which is no longer significant. When we instead control for province-specific trends, the estimate becomes 5.3 percentage points and significant again. Using provinces of origin instead of provinces of current residence does not affect the magnitude of our estimate, but yields larger standard errors.

This result indicates that being exposed to the higher education expansion raises individual's wage by 4.6 to 5.3 percentage points. This effect may be driven by exposed individuals are more likely to work and earn income, i.e., sample selection, or exposed individuals earn higher wage because of their college education conditional on working. We check the extent to which our results are driven by sample selection, we follow Duflo (2001)'s approach by obtaining the predicted probability of having positive monthly wage and include it in the

regression. Specifically, we estimate an event study model:

$$\mathbf{1}\{Wage_i > 0\} = \sum_c \delta_c \mathbf{1}\{Cohort = c\} + \gamma_{p,t} + \eta_c + e_i$$

and obtain the predicted probability of having a positive wage. We then include the predicted probability and its squared in the DiD regression to correct for sample selection (Heckman and Hotz, 1989; Duflo, 2001). We find that the expansion still has a positive and significant effect on log monthly wage. In the base specification, the estimate is 5.9 percentage points. Controlling for pre-treatment characteristics increases the estimated coefficient to 7.5 percentage points. However, when controlling for province-specific linear trends, the result is 3.9 percentage points. In other words, exposure to the expansion raises monthly income by 3.9% in the least parsimonious specification. Accounting for inter-provincial migration brings the estimate down slightly, to 3.5%. Our estimate also becomes less precise as the standard errors become larger and, thus, the result is no longer statistically significant.

Next, we turn to different tests to assess whether the parallel trends assumption is likely violated. First, the event study estimations in Figure 6 indicate that all pre-treated estimates are statistically indistinguishable from zero. Starting from the 1986 cohort, the treatment effects become positive and significant for all outcome variables. This suggests that our results are not driven by different pre-treatment trends between the treated and control provinces.

It is worth noting that the effects on high school completion are virtually small across all birth cohorts, including those who were born during 1986 and 1989, who already passed the high school age. Even if new universities might have affected high school completion by changing students' attitude towards higher education (as found in Russell et al. (2022)), these cohorts would not have been affected by such an effect. The fact that we do not find any effect even among this group, thus, further supports the validity of our placebo test.

Second, Figure A4 presents the estimates from a change-in-changes (CiC) model, which

relaxes the parallel trends assumption, for the quantile treatment effects on log monthly wage.¹³ All treatment effects are positive and statistically significant; the treatment effects in most quantiles are also in line with the treatment effect estimated from the TWFE model above. These results further confirm the validity of our main findings.

In short, these robustness checks indicate that our findings are not (1) driven by pre-treatment characteristics of provinces and (2) driven by inter-provincial migration. Thus, we are reassured that the parallel trends assumption is not violated. Other concerns such as misspecification and invalid inference remain. Specifically, our main findings do not incorporate the staggered timing design and the t-statistics based on the cluster-robust inference may be invalid with a small number of clusters. In Appendix B, we show that these concerns do not have substantial influence on our main findings.

4.3 Returns to college education among marginal students

The results in the previous section raise an interesting question about the implied returns to college education in the context of Vietnam, especially among those who choose to complete a college education because there is a new university in their province. We can calculate the returns to college education among these “switchers” by dividing the wage effect in Panel D by the college completion effect in Panel A in Table 3.

This belongs to a class of Wald estimator for estimating the effect of college completion on log monthly wage, in which college completion is instrumented with the DiD interaction term (De Chaisemartin and d’Haultfoeuille, 2018).¹⁴ To interpret the estimate from this calculation as a causal effect of college on earnings, we have to further assume that being exposed to a university opening only affects individual’s wage through their educational attainment

¹³We use the Stata command *cic* (Melly and Santangelo, 2015) to estimate this CiC model.

¹⁴Other studies, e.g., Zimmerman (2014); Ost et al. (2018); Lovenheim and Smith (2022), and Bleemer and Mehta (2022), also use this approach to estimate the returns to college education in the US.

(exclusion restriction) and that there are no individuals who would choose to complete college when not exposed and choose not to complete college when exposed (monotonicity).

Using the least parsimonious specification in column (4), the returns to college education is about 134%. We can also apply the change-in-changes (CiC) model to this Wald estimator to estimate heterogeneous returns to college education (De Chaisemartin and d’Haultfoeuille, 2018; Hsiao, 2022). Figure ?? reveals substantial variation in returns to college education. Interestingly, the Wald-DiD estimate is lower than the returns across all quantiles, although the difference is relatively small. The returns at the bottom 20% of the wage distribution are implausibly high relative to the rest of the distribution.

These returns are, nonetheless, substantially higher than those from the literature. Zimmerman (2014) uses a regression discontinuity design on grade cutoff to find that receiving a college degree in Florida raises earnings by 90%. Fan et al. (2018) use a fuzzy RDD on university entrance test score in China and estimate the returns to completing any college education to be 29% to 36%. Our results are also very different than other studies that estimate the returns to college education in Vietnam. For example, McGuinness et al. (2021) estimate a Mincer regression and find that the returns to college education are 60% to 80% for male and female, respectively.

One possible explanation for such a large discrepancy is that these estimates reflect the returns for very different populations. The Mincerian estimates reflect the returns for the entire population, while the IV estimates yield the returns only for those who were induced by the policy shock to complete a college education, i.e., the compliers. The two IV designs also yield returns for two different populations. On one hand, the IV estimate based on an RDD design reflect the returns for those who are just above the grade cutoff relative to those who are just below the cutoff (Zimmerman, 2014). On the other hand, the IV based on a DiD design reflect the returns for those who suddenly live closer to a university due to the expansion compared to those who do not.

4.4 Effects of higher education on occupation and industry of work

The high returns of higher education may also be explained by the possibility that such educational attainment would allow workers to move into better paid occupations or industries. To explore this explanation, we estimate the main DiD model on various occupational and sectoral employment outcomes and present the results in Table A1. The event study estimations are presented in Figure A2.

First, we consider the effect on occupational pay level, which measures the wage percentile of the 2-digit occupational code;¹⁵ a positive effect means individuals work in a relatively higher-paid job. In the base specification, the estimate is 1.854 and statistically significant; this result indicates that being exposed to the expansion leads to having an occupation that is almost 2 ranks higher in terms of wage. In other words, exposed individuals have a better-paid job than those who are unexposed. We also consider the effect on industry's skill intensity, measured by the share of college-educated workers in the 3-digit industry code. For both outcomes, the effects are positive and statistically significant, suggesting that exposed individuals are more likely to work in better paid jobs and industries.

We also find that exposed individuals are less likely to work in the agricultural sector and more likely to work in the service sector. The results in Table A1 and the event study estimations in Figure A2 indicate that these results are not driven by pre-trends. In contrast, the result on manufacturing employment is likely driven by pre-treatment trends: the estimate is reduced to almost zero when controlling for pre-treatment characteristics or province-specific trends. The event study result also indicates substantial pre-treatment trends for manufacturing employment, but not for agricultural and service employment. An important implication of our findings is that expanding access to higher education can contribute to the structural transformation and, thus, productivity growth.

¹⁵This variable is also known as the skill percentile in the US labor economics literature (Autor et al., 2003).

We can also look specifically at the type of occupations being affected by the higher education expansion. In Figure A3, we present the DiD estimates for the effects on different types of occupation using the same specification as before. We first observe that the expansion significantly reduces the likelihood of being an elementary worker (e.g., domestic helpers or manual labor). We also find positive effects on clerks (e.g., secretaries and bank tellers), technicians, scientists, experts, and managerial positions. The effect on being a scientist or expert is the most profound. These results strongly suggest that having access to higher education is valuable because it allows workers to access occupations and industries that typically require such a level of educational attainment.

4.5 Consumption spillover

We complement the wage analysis by estimating the effects of individual exposure on household expenditure per capita. One potential concern about our IV estimate for the returns to college education is that the log monthly wage variable may not fully capture the income of non-college workers. For example, individuals working for an agricultural household may not report the revenue from selling their farm products. It would, therefore, be informative to also estimate the effects on household expenditure, which may better reflect the monetary returns of a college education.

We estimate a similar cohort-based difference-in-differences model in Equation 1 where the outcome variables are household expenditure per household member. The household expenditure data comes from the Vietnam Household Living Standard Survey (VHLSS) in 2014-2018. It is important to note that the sample size of the VHLSS sample is much smaller in each sample and it also has fewer sample years because it is a bi-annual survey.

We present the regression results in Table 4. First, we confirm that the expansion has a positive effect on college completion in this sample (see column (1)), although the magnitude

is larger. The effect in the VHLSS sample is 5.4 percentage points, while the effect found in the LFS sample is 3.3 percentage points in the base specification. Controlling for pre-treatment trends make the result no longer statistically significant, but the estimates do not vary substantially. This is likely because sample size is too small for these many control variables. We also find that household expenditure per capita is increased by 12.1%. Recall that the wage effect from the LFS sample is only 5.9%. When controlling for pre-treatment trends, the estimate is reduced to 11.3% and no longer significant. While our estimates are noisy, these results are consistent with the wage effect of the expansion. In other words, as exposed individuals are more likely to complete college, they tend to have higher wage and, hence, higher household expenditure per capita. From columns (3) to (6), we specifically look at different categories of expenditure, including food and non-food items in daily life and in longer-term. These items exclude expenditure on health, education, utility, and other consumption. We find that exposure also has a positive effect on daily and yearly food expenditure as well as yearly non-food expenditure.

While some of these results are noisy, the fact that we find positive and sizable effects across different categories of consumption provides reassuring evidence that the college education has relatively high monetary returns. More importantly, this suggests that expanding access to higher education may boost the demand for labor as there is an increase in consumption. For example, Mazzolari and Ragusa (2013) find that college-educated workers consume more home production services in the US, raising demands for non-college workers. Similarly, Liu and Yang (2021) estimate that an increase in the share of college-educated workers raises the wages in the service sector, which also reflects a higher demand.

4.5.1 Effects by gender

The CiC estimation in the previous part reveals interesting heterogeneity in wage effect across the wage distribution. One potential dimension in which this heterogeneity manifests is

gender. In Figure A5, we provide the event study results estimated by gender. As suspected, the effects on college education of female are relatively large, while those of male are small and statistically insignificant. As a result, the positive effects on labor market outcomes such as employment and wage are also larger among female than male.

One possible explanation is that parents are less willing to let daughters to travel far for college education compared to sons. Opening a new university in a province thus allows girls in that area to access to higher education without traveling.¹⁶

5 Impacts on local labor markets

In the previous section, we find that expanding access to higher education greatly benefits individuals at college-going age. Having a university in their provinces significantly increases the probability of completing college and, hence, improve their employment and wage outcomes. However, as noted by Duflo (2004) and Khanna (2015), it is important to consider the effect of the expansion at the equilibrium.

As the supply of college-educated workers increases, one may expect the college wage premium to decrease in a canonical model of labor supply and demand (Katz and Murphy, 1992; Goldin and Katz, 2010). In the equilibrium, firms may respond to the change in relative input price by adjusting their capital and technology of production (Acemoglu, 1998; Duflo, 2004; Lewis, 2013; Khanna, 2015; Verhoogen, 2021) which may, in turn, affect college wage premium and relative demand for college-educated workers.

To guide our empirical exploration, we consider a theoretical model of capital-skill complementarity developed by Lewis (2013) in Section 5.1. We discuss a difference-in-differences

¹⁶Sánchez and Singh (2018) offer an alternative explanation: in Vietnam, aspiration for higher education of girls is higher than that of boys. Aspiration is also an important predictor of enrollment for girls but not for boys. Therefore, improving access unlocks the possibility that girls can enroll in higher education in Vietnam.

strategy to evaluate the effect of the expansion at the local labor market level and how we can allow for spatial spillover in our model in Section 5.2. We then discuss the estimation results in Section 5.3.

We find that the expansion indeed reduces college wage premium. However, we also find that smaller firms experience a productivity shock following the expansion and hire more college-educated workers. We argue in Section 5.4 that these results suggest that firms substitute non-college workers with skill-biased capital, thus allowing them to raise their productivity and hire more college-educated workers. We also discuss how our results may not be consistent with other theoretical explanations such as choice of production technologies (Beaudry et al., 2010; Clemens et al., 2018) and human capital externalities (Moretti, 2004a).

5.1 Theoretical framework

In this section, we briefly discuss Lewis (2013)'s framework and the predictions of the model. Consider the following production function $Y = g(K, C, N)$ where K, C and N denote capital, college-educated, and non-college workers and g is a homogeneous, twice continuously differential function and $g_i > 0$, $g_{ii} < 0$. We assume capital-skill complementarity, i.e., $\frac{\partial \ln w_C/w_N}{\partial \ln K} > 0$, which means that an increase in capital raises the relative price of college-educated workers.

The capital-skill complementarity can be restated in terms of elasticities of complementarity between capital and the two types of workers, i.e.,

$$c_{i,j} = \frac{\partial \ln p_i/p_j}{\partial \ln x_i/x_j} = Y_{ij} \frac{\partial^2 g_{ij}}{\partial g_i g_j}$$

is the elasticity of complementarity of two inputs i and j .

$$\frac{\partial \ln w_C/w_N}{\partial \ln K} > 0 \rightarrow \frac{\partial \ln w_C}{\partial \ln K} - \frac{\partial \ln w_N}{\partial \ln K} > 0 \rightarrow \frac{g_{K,C}}{g_C} > \frac{g_{K,N}}{g_N} \rightarrow c_{KC} > c_{KN}$$

where the third arrow is a result of setting wage of each type of worker to the marginal product of that worker, i.e., $w_C = \partial g / \partial C$ and $w_N = \partial g / \partial N$. The last arrow indicates that the elasticity of complementarity between capital and college-educated worker is higher than that between capital and non-college workers.

We also follow Lewis (2011) and assume that capital rent is fixed at r ; in other words, supply of capital is elastic. In the long-run equilibrium, it means $g_K = r$. Lewis (2013) derives the following results:

$$d \ln s_K = Q \frac{(1 - s_C - s_K) s_C (\frac{g_{KC}}{g_C} - \frac{g_{KN}}{g_N})}{C \cdot g_{KC} + N \cdot g_{KN}} (d \ln C - d \ln N) \quad (2)$$

$$\frac{\partial \ln(w_C/w_N)}{\partial \ln(C/N)} = -c_{CN} + (\frac{g_{K,C}}{g_C} - \frac{g_{K,N}}{g_N}) (-\frac{C g_{KC}}{g_{KK}}) \quad (3)$$

We can now summarize the main results of this model. Since we established that $\frac{g_{K,C}}{g_C} > \frac{g_{K,N}}{g_N}$, Equation 2 indicates that $d \ln s_K$ is positive, which means s_K increases in C/N . In other words, as the share of college-educated workers increases, firms also increase the capital share of output. Equation 3 quantifies the response of relative wage to changes in relative supply of college-educated workers. In the short run, K is held fixed so the short-run response of relative wage to an increase in relative wage is simply $-c_{C,N}$, where $c_{C,N} > 0$ is the elasticity of complementarity between college and non-college workers. In the long run, capital-skill complementarity condition means $(\frac{g_{K,C}}{g_C} - \frac{g_{K,N}}{g_N}) > 0$ and $g_{K,C} > 0$, which means the second term of Equation 3 is positive. Therefore, the short-run relative wage response is negative, and the long-run response is smaller than the relative wage response in the short run.

Intuitively, these results state that firms cannot adjust their capital in the short run, so the relative wage of college-educated workers *decreases* as the relative supply of college-educated workers *increases*. In the long run, firms substitute non-college workers with capital by increasing the capital share of output. Given the capital-skill complementarity, this would increase the demand for college-educated workers and offset (some of) the negative effect on relative wage in the short run. In other words, the effect on relative wage is likely still negative but smaller in the long run.¹⁷

The capital-skill complementarity framework is closely related to three other theoretical models.¹⁸ Early model of human capital externalities assumes that an increase in the stock of college-educated workers would make everyone more productive (Moretti, 2004a). Therefore, an increase of college-educated workers would make the labor force more productive, offsetting some of the negative effect on relative wage. Beaudry et al. (2010) and Clemens et al. (2018) develop a model of choices of production techniques. Specifically, firms can choose a combination of traditional and modern technologies. Modern technology is skill-biased, so an increase of skilled labors would make this production technology more productive. As a result, more firms would adopt such a technology (and become more productive), absorbing *all* of the negative effect on wage. Lastly, Acemoglu (1998, 2002) develops a model of directed technical change, in which an increase of college-educated workers would create an incentive for R&D firms to create innovations that are biased towards college-educated workers, raising the overall productivity level in sectors that use college-educated workers more. These models are equally useful to conceptualize the link between the educational level of the labor force and firm-level productivity and technology; however, as we discuss in the result section, they may not fit the context and findings of this study.

¹⁷When capital and non-college workers are perfect substitutes, the long-run effect on wage would be zero (Lewis, 2011).

¹⁸See a more thorough discussion of these related frameworks in Lewis (2013).

5.2 Identification strategy

In this section, we discuss the empirical strategy to assess the effects of the expansion on the local labor market as well as how firms respond by adjusting their capital as laid out in the theoretical model. We define local labor market at the district level. Two main outcomes of interest are relative supply and relative wage of college-educated workers, defined as $\ln C/N$ and $\ln w_C/w_N$ for each district and year. Besides these outcome variables born out of the theoretical model, we are also interested in the overall impacts of the expansion on employment and wage. Specifically, we use share of workers who are wage earners for all workers as well as for college and non-college workers and their average wage as the outcome variables.

At the firm level, we are interested in how the expansion affects firms' choice of input. We first consider the effects on capital-output ratio, which is the main outcome of the theoretical model. However, as noted by Lewis (2011), this sign on this outcome may not be consistent with theory because of measurement issues. Therefore, we also consider the effects on three alternative outcomes. Specifically, skill intensity, i.e., the ratio of college to non-college workers, within firm is a useful indicator for changes in capital; under capital-skill complementarity, we would expect firms to employ more of both capital and college-educated workers. A positive effect on skill intensity also implies that firms increase capital. Two other related measures are log total factor productivity (TFP) and labor productivity, measured as log value added per worker. As firms shift away from non-college workers and towards skill-biased capital, one may expect an increase in firms' productivity as well.

It is important to note that the expansion creates *two* shocks on the ratio of college to non-college workers in the labor market. When a new university is established, it draws potential non-college workers off the labor market and into attending a university, raising the ratio by decreasing the denominator. After 4-5 years of operation, the new university

releases the first cohort of college-educated workers into the labor market, raising the ratio yet again by raising the numerator. In other words, we may expect the ratio of college to non-college workers to increase during 2007-2011 as new universities established during the expansion start to train new students, and a further increase after 2011 as the new students graduate and join the labor market.¹⁹

The labor market outcome variables are constructed from the Labor Force Survey data, while the firm-level data comes from the Vietnam Enterprise Census (VEC) data. An important caveat of the Labor Force Survey is that our data only contains district information for 2011 and 2015-2019, which means we can only compare 2015-2019 to 2011 as the pre-expansion period. In other words, we can only observe the effect of the second shock, i.e., the increase of college-educated workers, at the local labor market level. Since we do not observe the first shock, i.e., the decrease of non-college workers who choose to attend university, our results from the LFS are also likely the lower bounds for the true effects of the expansion on the relative supply and wage. Unlike the LFS data, we observe all data for 2006-2018, so we can estimate the treatment effects on firm for both types of shocks.

a) No spatial spillover

Our starting point is a difference-in-differences model that compares districts that open new universities to districts that do not open any new school during the expansion over time (Duflo, 2004). Note that 56 districts established one new university during the expansion and 27 districts established more than one new university. Formally, we estimate the following model:

$$Y_{d,t} = \kappa_n \cdot (U_{n,d} \times Post_t) + \mathbf{X}\boldsymbol{\theta} + \gamma_t + \zeta_d + \epsilon_{d,t} \quad (4)$$

¹⁹Our individual-level analysis indicates that the expansion only raises the probability of completing college starting in 2012.

where $Y_{d,t}$ is the outcome of district d in year t ; $U_{n,d}$ is a binary variable that indicates whether district d has a new university during the expansion; $Post_t$ indicates whether it is after the expansion; γ_t and ζ_d are year and district fixed effects. Similarly to the previous section, we control for age and gender aggregated at the district-by-year level.

There are three important caveats of this model that we attempt to address. First, it does not account for the fact that many districts already have existing universities before the expansion. This can potentially confound the effects of establishing new universities if districts with existing universities are more likely to establish new universities during the expansion. We address this problem by controlling for the number of existing universities interacted with a linear trend term.

Second, this approach assumes no spillover effects between districts that establish new universities with districts that do not, including nearby districts. If individuals from nearby districts can commute to the treatment districts, then we would underestimate the true effects of the expansion. Furthermore, the effects on labor market outcomes may also spill over to nearby local labor markets if workers can travel to neighbor districts for work.

Third, universities are almost always established in the capital district of any given province (with exceptions for major cities such as Ho Chi Minh City or Hanoi), which can grow very differently than districts that are far away over time. By including all districts with no new universities in the control group, our parallel trends assumption might be violated since the two groups may not evolve on parallel trends.

b) With spatial spillover

To assess the extent of spatial spillover, we split the district-level sample into 4 groups based on their distance to the treatment group, i.e., districts that have new universities located there: those that are over 100 kilometers, 50-100, 25-50, and 0-25 kilometers away from a

treatment district. We use the districts that 50-100 kilometers away from the treatment district as the control group, assuming that 50-100 kilometers is a sufficient distance for no spillover effect. We also drop districts that are over 100 kilometers away from a treatment unit – this allows us to have a more comparable control group, assuming that those that are 50-100 kilometers away from a treatment district do not differ much from the treatment district except for having new universities. In the Appendix, we show that our results are robust when using districts that are over 100 kilometers away from a treatment district as the control group.

Denote $dist_{n,d}$ the treatment group status n of district d , where $n = 0, 1, 2, 3, 4$ are for districts with new universities, districts that are 0-25, 25-50, 50-100, and over 100 kilometers away from districts with a new university, respectively. We then estimate the following DiD model:

$$Y_{d,t} = \sum_{n=0,1,2} [\eta_n(dist_{n,d} \times Post_t)] + \mathbf{X}\boldsymbol{\theta} + \gamma_t + \zeta_d + \epsilon_{d,t} \quad (5)$$

where η_n captures the treatment effects on treatment group n ; specifically, η_0 is the treatment effect on the actual treatment group, while $\eta_{1,2}$ capture the spillover effects on districts that are 0-25 kilometers and 25-50 kilometers away from a treatment district. In this model, we also control for district and year fixed effects to account for any district-level heterogeneity and national shocks.

c) Demands for non-college and college-educated workers

The market-level analysis also subject to major shifts in labor market demands due to major trade shocks at the national level ²⁰ or FDI inflows at both national and local levels (Coxhead and Shrestha, 2017; Pham et al., 2021; Le et al., 2022). These factors may confound the effects of the expansion especially on the labor market outcomes.

²⁰See, e.g., McCaig (2011); Vu-Thanh (2017); McCaig and Pavenik (2018) among many others.

At the minimum, we control for province-by-year fixed effects as a robustness check for our results. This would absorb any province-level shocks. To account for demand shocks specific to non-college and college-educated workers, we follow Moretti (2004a) and construct two shift-share variables as control variables. Specifically, we construct a measure of local exposure to national employment shock for each 2-digit industry and by their education level. We first calculate the employment share for all 2-digit industries in each district by their college education using the 2009 Population and Housing Census data and multiply with national year-to-year change in employment of each industry:

$$demand_{d,e,t} = \sum_k s_{d,e,k} \times \Delta employment_{k,t}$$

where $demand_{d,e,t}$ measures demand shock for district d for workers with education e (non-college or college-educated) in year t , $s_{d,e,k} = \frac{employment_{d,e,k}}{\sum_k employment_{d,e,k}}$ is the employment share of industry k in district d for worker type e , and $\Delta employment_{k,t,t-1}$ denotes national changes in employment of industry k from year $t - 1$ to year t .²¹

d) Assessing firms' responses

We extend the model in Equation 5 to study the effects of the expansion on firm-level outcome. As discussed previously, the expansion first increases the ratio of college to non-college workers as it draws potentially non-college workers away from the labor market to attend university, then increases the ratio again as it releases college-educated adults into the labor market. The local labor market analysis above cannot account for the first shock because the Labor Force Survey data does not cover district information before 2011. The firm-level data, in contrast, allows us to estimate the effects of both shocks.

²¹This shift-share variable, often known as the Bartik instrument, is typically used as an instrumental variable for exogenous exposure to national shock in employment or migration (Goldsmith-Pinkham et al., 2020). The validity of this instrument in the Vietnam context is discussed in Vu et al. (2022).

Formally, let $T = 0, 1, 2$ denote the pre-treatment period during 2006-2007, the training period during 2008-2011, i.e., the period when non-college workers attend the new universities, and the post-training period during 2012-2018, i.e., when the new college graduates join the labor market, respectively. Also recall that $n = 0, 1, 2, 3, 4$ are for districts with new universities, districts that are 0-25, 25-50, 50-100, and over 100 kilometers away from districts with a new university, respectively. The difference-in-differences model we estimate takes the following form:

$$Y_{j,d,t,k} = \sum_{T=1,2} \sum_{n=0,1,2} [\eta_{n,T}(dist_{n,d} \times Post_t)] + \mathbf{X}\boldsymbol{\theta} + \zeta_d + \gamma_{p,t} + \Omega_{k,t} + \epsilon_{j,d,t} \quad (6)$$

where $Y_{j,d,t,k}$ denotes the productivity of firm j in industry k in district d in year t , and ζ_d is district fixed effects. To account for industry-specific and province-specific shocks, we control for province-by-year and industry-by-year fixed effects denoted by $\gamma_{p,t}$ and $\Omega_{k,t}$, respectively. $\eta_{n,T}$ denote the treatment effect on district type n in period T . There are likely heterogeneous effects across different types of firms and industries. Specifically, we consider the effects on the service and manufacturing sectors separately as well as by firm size.

5.3 Results

a) Effects on the local labor markets

Table 5 displays the key statistics for the variables used in this analysis. Note that all variables are at the year-by-district level. We do not have access to district information for 2012-2014, so our analysis uses 2011 as the pre-expansion year and 2015-2019 as the post-expansion years. As our cohort-based analysis shows previously, the expansion only took effect on college education starting in 2012. We present the results from estimating Equations 4 and 5 in Table 7. Each column corresponds to an outcome variable. In all models, we control for

mean age, mean age squared, percentage of female, number of existing universities in 2005 (before the expansion) interacted with linear trend, local demand shocks for non-college and college-educated workers.

Panels A and B correspond to Equation 4 in which we assume no spatial spillover on districts that do not have any new universities. In other words, the control group includes all districts without any new university regardless of their distance to the closest treatment district. In Panel A, we only control for district and year fixed effects. In Panel B, we further control for province-by-year fixed effects to soak up any macro shocks at the province level.

Panel C corresponds to Equation 5, in which we relax the no spillover assumption. Specifically, we estimate a DiD model that uses districts that are without any new universities and are 50 to 100 kilometers away from a treatment district as the control group. Assuming that there is no spillover effect on this group, we also estimate the effects on districts that are 25 to 50 kilometers and 0 to 25 kilometers away from a treatment district, along with the effects on the treatment districts. We still control for province-by-year fixed effects in this model.

In column (1), the treatment effect on the share of college-educated adults estimated using the base specification is 1.1 percentage points and statistically significant. This result is robust to controlling for province-by-year fixed effects. However, when we only use control districts that are 50 to 100 kilometers away from a treatment unit, the estimated effect on the treatment district is 2.5 percentage points. The results also indicate that districts without new universities that are 0 to 25 kilometers away from a treatment unit also experiences a positive spillover effect of 1.7 percentage points. This confirms our hypothesis that individuals residing in nearby control districts can travel to a treatment district for school. The positive spillover effect also explains why the conventional DiD approach would underestimate the true effect on schooling outcome.

We now turn to the effects on the relative supply and college premium in columns (2) and

(3). In the base specification, the estimated effect on the relative supply is 25.7% and on the college wage premium is -9.3%. Adjusting for province-by-year fixed effects absorbs some of the effects, so the estimates are 17.5% and 5.8%, respectively. However, when we allow for spatial spillover, the estimated treatment effects on the treatment districts are 39.8% and -14.9%. The spillover effects for districts that are 0-25 kilometers away from a treatment unit are 35.9% and -12.2%; this is consistent with the result in column (1) that the expansion also increases the share of college-educated adults in these districts. We also present the dynamic treatment effects from estimating an event-study specification in Figure 7.

These results indicate that (a) province-specific shocks can bias the results upwards, and (b) spatial spillover can bias the results downwards. In other words, the conventional approach would underestimate the true effects of the expansion because it violates the SUTVA assumption. When we account for spatial spillover effects, our results imply that raising the relative supply of college-educated workers by 10 percent would decrease the skill premium by 37.4 percent.²² Similarly, the results from the 0-25 km group imply that increasing the relative supply by 10 percent would decrease the skill premium by 34 percent. These results imply that the elasticity of substitution between college-educated and non-college workers are 2.67 and 2.94 for the treatment and spillover districts. Acemoglu and Autor (2011) estimate the elasticity of substitution between the two types of workers to be 2.9.

In columns (4)-(6), we present the results for the employment effect on all workers as well as by their education. The expansion decreases the overall share of wage-earning workers by more than 8 percentage points. There is no evidence of spatial spillover in this outcome. We also find that the expansion increases the share of college-educated workers with positive wage by more than 4 percentage points and decreases that of non-college workers by 8 percentage points.

²²The effects of the expansion on the relative supply and premium correspond to the first-stage and reduced form estimates of a Wald estimator (Hsiao, 2022), so we can divide the former by the latter to obtain the effect of relative supply on premium.

Columns (7) to (9) show the results for the wage effects for similar groups of workers. We observe that the expansion raises the overall monthly wage in the treatment districts by 16.9%, but also that in the nearby districts by 17.2%. Notably, the effect on wage of college-educated workers is relatively small and insignificant across all specifications. In contrast, we observe that the expansion increases the monthly wage of non-college workers by similar magnitudes when estimating it for all workers.

In short, these results indicate that the expansion indeed has raised the relative supply of college-educated workers and thus reduced the relative wage of these workers. As laid out in the theoretical framework, the observed negative effect on relative wage is net of two effects: the short-run negative effect of the increase in supply when firms cannot adjust capital, and the long-run effect of capital adjustment that is biased towards college-educated workers. Specifically, the increase in the skill mix of the labor force would induce firms to employ more capital to substitute non-college workers because their relative price is higher. Because of capital-skill complementarity, firms also hire more college-educated workers. This explains why the expansion has a positive effect on the share of college-educated workers who are employed and negative effect on the share of non-college workers who are employed. However, it also means that fewer workers are employed and the overall wage level is higher.

b) Firm responses to changes in relative supply

The results in the previous section suggest that firms employ more college-educated workers and fewer non-college workers, implying that firms adjusted towards skill-biased capital that replace non-college workers. In this section, we formally assess this implication from estimating Equation 6 on firm-level outcomes. We present the results in Table 9 by sector. Each row provides the estimates for the interaction term between treatment groups and period, and each column corresponds to an outcome variable.

First, we observe that the expansion has very little effect on firms' capital intensity, measured as capital-revenue ratio (see column (1)). However, as noted by Lewis (2011), the measure of capital share of output is often imperfect,²³ so the empirical relationship between capital-output ratio and labor supply may not be as predicted by the theoretical model.

However, the model also predicts that firms would replace non-college workers with skill-biased capital and, thus, also hire more college-educated workers. Therefore, we estimate the effect of the expansion on the share of college-educated workers being employed by firms; the results are reported in column (2). We find positive and significant effects on the share of college-educated workers hired by the exposed firms for both sectors, indicating that the expansion has indeed induced firms to employ more college-educated workers in both service and manufacturing sectors. Notably, we also find that firms employ more college-educated workers in both 2008-2011 and 2012-2018, but the effects in the latter period are twice as large as those in the former period. We also observe positive (but smaller) spillover effects on skill intensity in districts 0 to 25 kilometers away from a treatment district.

Similarly, the expansion also has positive and significant effects on log total factor productivity for both service and manufacturing firms, as indicated in column (3). For service firms, the effect in 2008-2011 is also smaller than that in 2012-2018; in contrast, for manufacturing firms, the effects are similar across the two periods. These results suggest that firms become more productive starting in 2008. The results for labor productivity, measured as log value-added per worker, provide a similar story (see column (4)). For both service and manufacturing firms, the effects in both periods are positive and mostly statistically significant. For both log TFP and labor productivity, we observe significant spillover effects on districts 0 to 25 kilometers away from a treatment district, but no spillover effects on districts 25-50 kilometers away. In contrast, there are significant spillover effects on both outcomes for manufacturing firms in districts that are 25-50 kilometers away from a treatment district.

²³For example, firms may replace machines that are complement to non-college workers with machines that substitute for these workers, so the total values of these capital input may not change.

This is likely because manufacturing plants tend to be placed just outside the capital districts where universities are located.

To explore heterogeneous effects of the expansion on firms, we also estimate the model separately for firm size below and above median. The results are presented in Table A3. First, we note that the effect on skill intensity is concentrated among small firms in both service and manufacturing sectors. The effects on TFP and labor productivity are larger among small firms in the service sector. The opposite is true with the manufacturing sector: larger gains in TFP and labor productivity among larger firms.

We also estimate an event-study specification and present the results in Figure 8. We observe that negative but insignificant effect on log capital intensity among manufacturing firms. However, for service firms, the effect on capital intensity is negative and mostly significant between 2011 and 2018. Firms hire more college-educated workers during 2011 in both service and manufacturing sectors, but the effects become smaller in the second period. Consistent with the main results, we find that the expansion increases log TFP and log labor productivity of firms in both sectors starting in 2009. We also observe no substantial change to the productivity effects after that, indicating that firms likely only adjust to the first shock when there are fewer non-college workers in the labor market but not to the second shock when the new college graduates enter the labor market.

5.4 Interpretations

In the previous section, we observe that the expansion increases the relative supply and reduces the relative wage of college-educated workers. In other words, the expansion makes non-college workers relatively more expensive. We also find that the expansion increases the employment of college-educated workers while reducing that of non-college workers. We then observe that firms respond to the expansion by raising the level of productivity in terms

of TFP and labor productivity during 2008-2011 and 2012-2018. We also find that firms hire more college-educated workers during 2008-2011 when the expansion draws potential non-college workers away from the labor market to attend university. They hire even more college-educated workers during 2012-2018 when the new college graduates from the new universities join the labor market.

These results can be explained with a general capital-skill complementarity framework presented above. Expanding access to higher education makes non-college workers become relatively more expensive; this is reflected by higher relative supply of college-educated workers and lower college wage premium. Firms then substitute them with skill-biased capital, causing more college-educated workers to be employed; these are reflected by higher productivity and higher share of college-educated workers within firms. However, this only absorbs part of the labor supply shock caused by the expansion; therefore, the effect on relative wage is still negative.

There are three alternative explanations for these results. First, new universities increases the level of education of the labor force, which has a spillover effect on productivity. Consider a model of human capital externalities in which labor productivity is a function of the share of college-educated workers (Moretti, 2004a). The expansion increases the relative price of non-college workers, so firms substitute non-college with college-educated workers, raising firm-level productivity. As a result, non-college workers' wage is increased through two channels: substitution effect and productivity spillover. Similarly, the effect on college-educated workers' wage is ambiguous: substitution reduces their wage but productivity spillover increases (part of) their wage. However, the timings of the effects on productivity and on share of college-educated workers employed by firms do not support this explanation. As we observe in Figure 8, firms experience an increase in log TFP right around 2009 but they only experience an increase in share of college-educated workers in 2011. In other words, the TFP shock is unlikely to be driven by the increase in college-educated workers which

happens later.

A related explanation is that new universities attract more population to districts that opened a new school (and also nearby districts) and this population growth has a positive effect on productivity. An extensive literature on agglomeration economies, although mainly in developed countries, suggests that a more populated city can foster learning and knowledge sharing (Glaeser, 1999; Glaeser and Resseger, 2010). Firms in a more populated city may also face harsher competition which eliminates low-productivity firms, thus leaving behind more productive firms (Combes et al., 2012). However, this alternative explanation is not supported by recent literature on the minimal relationship between city size and productivity in developing countries (Bryan et al., 2020; Grover et al., 2021).

Another potential explanation of our results is that firms adopt more advanced production technologies, and not simply substituting non-college workers with skill-biased capital. Consider a model of choice of production techniques in which firms can choose between traditional and modern production functions (Beaudry et al., 2010; Clemens et al., 2018). The modern production function is skill-biased, so an increase in the ratio of college to non-college workers would make the modern production technique more productive. Therefore, as the expansion increases the relative supply of college-educated workers, there would be more firms adopting the modern technology, absorbing the increase of supply of college-educated workers. This model predicts that firms become more productive as the ratio of college to non-college workers increases.

However, the model also predicts that firms would fully absorb the supply of college-educated workers and, thus, there would be no effect on the relative wage (Lewis, 2011, 2013; Clemens et al., 2018). However, our results contradict with the prediction of this model. As shown in Table 7 column (3), the expansion decreases the college wage premium, i.e., the relative wage by 14.9%. This result implies that the model of choices of production technique does not apply here.

6 Conclusion

In the past three decades, Vietnam has experienced rapid economic growth due to both within-sectors productivity growth and structural transformation (McCaig and Pavcnik, 2013; McMillan et al., 2017; Liu et al., 2020). As noted by McMillan et al. (2017), structural transformation may lead to episodic growth but it requires more fundamental changes such as human capital investment and institutional changes to sustain economic growth. Decree 121/2007 is an effort to push for such fundamental changes, as it allowed Vietnam to establish over 100 new universities in a short period.

At the individual level, the expansion increases the probability of completing college by 34%. It also raises their monthly wage by 3.9%. These results imply that college education is highly valued in Vietnam. This is because it allows them to work in skill-intensive sectors, better-paid jobs, and, thus, have higher expenditure. In other words, expanding higher education can speed up the process of structural transformation by allowing workers to move into more productive sectors. The expansion also affects firms through changes in local labor markets. Specifically, it reduces college wage premium and raises the relative price of non-college workers. This creates an incentive for firms to substitute non-college workers with skill-biased capital. This explains why firm-level productivity and college-educated employment rise shortly after a new university opening. However, this also has a negative effect on employment of non-college individuals.

These findings have two important implications. First, there is often skepticism about the quality of higher education and the lack of demand for college-educated workers in developing countries. These concerns were also apparent in Vietnam (The World Bank, 2020). Nonetheless, we show that expanding access to higher education brings considerable benefits to individual workers and also induce firms to become more productive. Although the college wage premium is reduced by the supply shock, some of the negative effect on wage is

absorbed as firms adjust their capital.

Second, higher education plays a different role in fostering productivity growth in developing countries. Since less developed countries may lack a formal market for innovation (Acemoglu, 1998, 2002) or an incentive to focus on innovative activities (Acemoglu et al., 2006; Vandenbussche et al., 2006; Aghion et al., 2009), an expansion of high-skilled labors may not lead to more innovation activities. However, an expansion of higher education creates a shortage of non-college workers, forcing firms to adopt skill-biased capital, thus, raising their total factor productivity.

It is also important to recognize the downsides of expanding access to higher education, as documented in this study. Such expansions would cause firms to substitute non-college workers with capital, taking jobs away from non-college individuals. In an economy with a large share of non-college workers, this means that the expansion would decrease the overall employment. The expansion also raises the wage level of non-college workers who are employed. One important question for future studies, then, is how expanding access to higher education affects the overall level of inequality in less developed countries.

Our study provides an important case study on the effects of expanding access to higher education in developing countries; however, generalizing its results to other developing countries would require careful consideration because Vietnam may differ from other developing countries in many dimensions. Despite being a low-middle income country, Vietnam has been well-recognized for its success in basic education in terms of enrollment and international assessment scores (Dang and Glewwe, 2018; Dang et al., 2021). Such a factor may contribute to the effectiveness of the expansion. Since we focus on new universities in provinces that never had a university before, we mostly capture the effects of public universities.²⁴ Therefore, our results cannot be generalized to the effects of private universities.

²⁴Private universities tend to be located in large cities where there have already been existing universities before the reform.

Furthermore, it is important to note that Vietnam has enjoyed rapid growth in both GDP per capita as well as economic complexity during the expansion and, thus, it has also enjoyed a relatively high demand for high-skilled labor (Patrinos et al., 2018). This factor likely plays a crucial role in explaining the positive effects of the expansion on labor market outcomes. Thus, we can also expect that our results can be generalized to countries with a similar growth experience such as Bangladesh, Cambodia, Ethiopia, and Philippines (see Figure 2).

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TABLES

Table 1: Balance table of treatment status at the province level

	Control (N = 14)	Treatment (N = 21)		Already treated (N = 28)	
	Mean	Mean	Difference	Mean	Difference
% college graduates	0.033	0.042	0.009 (0.011)	0.052	0.019* (0.011)
% high school graduates	0.243	0.256	0.013 (0.025)	0.302	0.060** (0.024)
% college enrolment	0.190	0.295	0.105*** (0.033)	0.301	0.111*** (0.031)
% self employed	0.889	0.858	-0.031 (0.028)	0.821	-0.068** (0.026)
% employed	0.111	0.142	0.031 (0.028)	0.179	0.068** (0.026)
% agricultural worker	0.748	0.603	-0.146*** (0.052)	0.498	-0.250*** (0.049)
% manufacturing worker	0.076	0.159	0.083*** (0.026)	0.212	0.136*** (0.025)
% service worker	0.146	0.211	0.066** (0.032)	0.264	0.118*** (0.030)
Log income per capita	8.555	8.755	0.200** (0.094)	8.881	0.326*** (0.089)
Urban	0.154	0.213	0.059 (0.054)	0.283	0.128** (0.051)
% in poverty	0.182	0.134	-0.047** (0.021)	0.108	-0.074*** (0.020)
% age 0-5	0.238	0.214	-0.024* (0.014)	0.201	-0.037*** (0.013)
% age 6-18	0.176	0.180	0.005 (0.006)	0.170	-0.005 (0.006)
TFP	11.628	11.751	0.124 (0.225)	11.702	0.075 (0.214)
Labor productivity	16.982	16.990	0.008 (0.110)	16.997	0.015 (0.104)
Capital-labor ratio	19.255	19.230	-0.025 (0.104)	19.383	0.128 (0.099)
Total N	64				

This table shows the means of pre-treatment, province-level characteristics by treatment status and the results from their balance tests. The *Mean* columns display the mean values of these covariates for each treatment group. The *Difference* columns show the results from estimating a regression with the characteristics as the dependent variable and the treatment status dummy variables as the independent variables. Data on the pre-treatment characteristics are aggregated from the 2004-2006 VHLSS data.

Table 2: Summary statistics by cohort and province

	Control		Treatment		Already treated	
	1970-1985	1986-1994	1970-1985	1986-1994	1970-1985	1986-1994
Age	35.216 (5.655)	25.715 (2.804)	35.271 (5.791)	25.705 (2.847)	35.359 (5.777)	25.699 (2.826)
Female	0.506 (0.500)	0.492 (0.500)	0.509 (0.500)	0.491 (0.500)	0.513 (0.500)	0.503 (0.500)
Completed college or higher	0.100 (0.300)	0.082 (0.275)	0.092 (0.290)	0.100 (0.300)	0.130 (0.336)	0.156 (0.363)
Employment	0.331 (0.471)	0.358 (0.479)	0.410 (0.492)	0.523 (0.499)	0.494 (0.500)	0.655 (0.475)
Formal employment	0.206 (0.404)	0.199 (0.400)	0.216 (0.412)	0.279 (0.449)	0.298 (0.457)	0.413 (0.492)
Log monthly wage	3.155 (0.800)	2.988 (0.802)	3.246 (0.685)	3.186 (0.614)	3.390 (0.665)	3.365 (0.579)

Table 3: Difference-in-differences estimates for the effect of exposure to the higher education expansion on individual-level outcomes

	Complete College (1)	Complete High school (2)	Employment (3)	Formal employment (4)	Log wage (5)	Log wage (corrected) (6)
Specification 1: TWFE						
	0.032*** (0.009)	0.006 (0.016)	0.079*** (0.023)	0.070*** (0.025)	0.069*** (0.025)	0.056** (0.024)
N	788629	784540	725395	725495	599808	599808
Specification 2: Pre-treat characteristics \times cohort trends						
	0.023** (0.009)	0.007 (0.021)	0.036** (0.016)	0.031 (0.020)	0.058** (0.025)	0.081*** (0.028)
N	788629	784540	725395	725495	599808	599808
Specification 3: Province \times cohort trends						
	0.027** (0.010)	-0.006 (0.009)	0.041*** (0.012)	0.037*** (0.012)	0.052** (0.021)	0.041** (0.017)
N	788629	784540	725395	725495	599808	599808
Specification 4: Same as 3, exposure using province of origin						
	0.027** (0.011)	-0.004 (0.011)	0.043*** (0.014)	0.035** (0.016)	0.051* (0.026)	0.035* (0.020)
N	484152	481142	446534	444698	365029	365029

This table reports the difference-in-differences estimate for the effects of the expansion on college attainment and labor market outcomes. Only the coefficient of the Post \times Exposed is reported. All models control for age, age squared, gender, province-by-year fixed effects, and birth cohort fixed effects. In Specification 2, we further control for pre-treat province-level characteristics interacted with linear cohort trends. The characteristics include college enrollment, share of workers in agriculture, manufacturing, and service, as well as average income per capita. In Specification 3, we instead control for province-specific linear cohort trends. In Specification 4, we use province of origin to construct the exposure measure and estimate a similar model as Specification 3. All samples include individuals between age 22 and 55. All standard errors are clustered at the province level. Data is drawn from LFS 2015-2018. Employment is defined as whether a person is employed for a job, regardless of whether it is formal or informal. Formal employment is defined as having a job that provides social insurance. Log monthly wage is for all individuals reporting having a wage, including those with an informal job.

Table 4: Difference-in-differences estimates for the effect of exposure to the higher education expansion on household expenditure

	Complete	Log	Daily		Yearly	
	college	Expenditure	Food	Non-food	Food	Non-food
	(1)	(2)	(3)	(4)	(5)	(6)
Specification 1: TWFE						
	0.050***	0.091*	0.072**	0.067*	0.137***	0.130**
	(0.018)	(0.053)	(0.034)	(0.036)	(0.047)	(0.058)
N	16517	17915	17914	17911	17912	17778
Specification 2: Pre-treat characteristics \times cohort trends						
	0.025	0.092	0.061*	0.055	0.106**	0.085
	(0.019)	(0.057)	(0.033)	(0.038)	(0.051)	(0.069)
N	16517	17915	17914	17911	17912	17778
Specification 3: Province \times cohort trends						
	0.049**	0.140***	0.078**	0.051	0.165***	0.162*
	(0.023)	(0.050)	(0.035)	(0.041)	(0.052)	(0.086)
N	16517	17915	17914	17911	17912	17778

This table reports the difference-in-differences estimate for the effects of the expansion on college attainment and labor market outcomes. Only the coefficient of the Post \times Exposed is reported. All models control for age, age squared, gender, province-by-year fixed effects, and birth cohort fixed effects. In Specification 2, we further control for pre-treat province-level characteristics interacted with linear cohort trends. The characteristics include college enrollment, share of workers in agriculture, manufacturing, and service, as well as average income per capita. In Specification 3, we instead control for province-specific linear cohort trends. All samples include individuals between age 22 and 55. All standard errors are clustered at the province level. Data is drawn from LFS 2015-2018. Occupational pay level is measured as the wage percentile of 2-digit occupation code, so positive effect means being in an occupation with higher pay level. Skill intensity of industry is measured by the share of college-educated workers of the 3-digit industry code.

Table 5: Summary statistics by year and district

	Control		Treatment	
	2011	2015-2019	2011	2015-2019
% of adults who complete college or higher				
	0.064	0.086	0.157	0.206
	(0.054)	(0.073)	(0.110)	(0.127)
% of adults who are employed				
	0.325	0.400	0.526	0.596
	(0.136)	(0.165)	(0.125)	(0.110)
% of non-college adults who are employed				
	0.285	0.353	0.454	0.513
	(0.129)	(0.164)	(0.106)	(0.104)
% of college-educated adults who are employed				
	0.924	0.891	0.903	0.893
	(0.095)	(0.130)	(0.076)	(0.058)
Log monthly wage				
	10.111	10.299	10.329	10.643
	(0.218)	(0.362)	(0.227)	(0.257)
Log monthly wage of non-college workers				
	10.034	10.245	10.199	10.568
	(0.223)	(0.386)	(0.200)	(0.238)
Log monthly wage of college-educated workers				
	10.399	10.664	10.577	10.848
	(0.226)	(0.254)	(0.235)	(0.259)
Skill premium				
	0.360	0.408	0.378	0.280
	(0.252)	(0.363)	(0.193)	(0.179)

Table 6: Difference-in-differences estimates for the effect of the expansion on educational and labor market outcomes at district level

	College share	Relative supply	College premium	Wage earning			Monthly wage		
	(1)	(2)	(3)	All	College	Non- college	All	College	Non- college
Panel A: Specification 1 (All districts w/o new universities as control)									
Treatment \times Post	0.011* (0.006)	0.257*** (0.072)	-0.093*** (0.030)	-0.081*** (0.012)	0.033*** (0.012)	-0.071*** (0.012)	0.060** (0.026)	-0.014 (0.024)	0.084*** (0.030)
N	4107	4020	4020	4107	4029	4107	4107	4020	4107
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✗	✗	✗	✗	✗	✗	✗	✗	✗
Panel B: Specification 2 (All districts w/o new universities as control)									
Treatment \times Post	0.010* (0.006)	0.175** (0.081)	-0.058* (0.031)	-0.088*** (0.012)	0.026** (0.012)	-0.077*** (0.011)	0.026 (0.025)	-0.016 (0.024)	0.043 (0.028)
N	4107	4020	4020	4107	4029	4107	4107	4020	4107
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Panel C: Specification 2 allowing for spatial spillover (Districts 50-100 kilometers away from a treatment district as control)									
25-50 km \times Post	0.004 (0.008)	-0.060 (0.136)	-0.041 (0.051)	0.015 (0.016)	0.017 (0.019)	0.016 (0.014)	0.078* (0.042)	0.030 (0.035)	0.077* (0.044)
0-25 km \times Post	0.017** (0.009)	0.359** (0.140)	-0.122** (0.053)	-0.018 (0.018)	0.014 (0.019)	-0.021 (0.017)	0.172*** (0.046)	0.024 (0.040)	0.163*** (0.048)
Treatment \times Post	0.025*** (0.009)	0.398*** (0.138)	-0.149*** (0.055)	-0.089*** (0.019)	0.042** (0.021)	-0.080*** (0.018)	0.169*** (0.048)	0.019 (0.042)	0.182*** (0.050)
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Table reports DiD estimates for the effects of the expansion at the district level. Panels A & B report results from estimating a DiD model using all districts without new universities as the control group. Panel C shows results from estimating a DiD model using districts without new universities and 50-100 kilometers away from a treatment district as the control group; districts that are over 100 kilometers away from a treatment district are omitted from the sample. We report the estimates for the interaction terms between treatment/spillover districts and post (equals 1 for after 2011). All models control for average age, age squared, share of female, and the number of existing universities in 2005 interacted with a linear time trend term. Specification in Panel A controls for district and year fixed effects. Specification in Panels B and C further controls for province-by-year fixed effects. Relative supply of college-educated workers is measured by $\log(\text{number of college-educated employed}/\text{number of non-college employed})$. Skill premium is measured by $\log(\text{wage of college-educated}/\text{wage of non-college})$. All outcomes are of individuals between age 22 and 54 aggregated at the district-by-year level. All standard errors are clustered at the district level. Data drawn from LFS 2011 and 2015-2018.

Table 7: Difference-in-differences estimates for the effect of the expansion on educational and labor market outcomes at district level by skill intensity

	Relative	College	Wage earning			Monthly wage		
	supply	premium	All	College	Non-college	All	College	Non-college
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Low skill intensity								
25-50 km \times Post	0.256 (0.167)	-0.074 (0.109)	0.014 (0.013)	0.035** (0.016)	0.015 (0.013)	0.042 (0.043)	-0.008 (0.091)	0.049 (0.044)
0-25 km \times Post	0.605*** (0.172)	-0.012 (0.112)	-0.027* (0.015)	0.059*** (0.019)	-0.024 (0.015)	0.109** (0.047)	0.128 (0.098)	0.113** (0.048)
Treatment \times Post	0.685*** (0.171)	-0.045 (0.107)	-0.105*** (0.016)	0.045** (0.020)	-0.087*** (0.016)	0.122** (0.050)	0.106 (0.095)	0.144*** (0.050)
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Panel B: High skill intensity								
25-50 km \times Post	-0.136 (0.107)	0.025 (0.048)	0.000 (0.010)	-0.019 (0.024)	0.001 (0.006)	0.002 (0.037)	0.010 (0.034)	-0.003 (0.042)
0-25 km \times Post	-0.003 (0.114)	-0.047 (0.050)	0.007 (0.011)	-0.045* (0.026)	0.001 (0.007)	0.031 (0.039)	-0.003 (0.038)	0.053 (0.044)
Treatment \times Post	-0.123 (0.113)	-0.014 (0.053)	0.015 (0.011)	-0.004 (0.028)	0.007 (0.007)	0.030 (0.039)	0.031 (0.043)	0.050 (0.043)
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Table reports DiD estimates for the effects of the expansion at the district level. Panels A & B report results from estimating a DiD model using all districts without new universities as the control group. Panel C shows results from estimating a DiD model using districts without new universities and 50-100 kilometers away from a treatment district as the control group; districts that are over 100 kilometers away from a treatment district are omitted from the sample. We report the estimates for the interaction terms between treatment/spillover districts and post (equals 1 for after 2011). All models control for average age, age squared, share of female, and the number of existing universities in 2005 interacted with a linear time trend term. Specification in Panel A controls for district and year fixed effects. Specification in Panels B and C further controls for province-by-year fixed effects. Relative supply of college-educated workers is measured by $\log(\text{number of college-educated employed}/\text{number of non-college employed})$. Skill premium is measured by $\log(\text{wage of college-educated}/\text{wage of non-college})$. All outcomes are of individuals between age 22 and 54 aggregated at the district-by-year level. All standard errors are clustered at the district level. Data drawn from LFS 2011 and 2015-2018.

Table 8: Summary statistics by year and district

	Control		Treatment	
	2006-2011	2012-2018	2006-2011	2012-2018
Log TFP				
	2.512	2.530	2.530	2.541
	(0.141)	(0.141)	(0.131)	(0.143)
Labor productivity				
	17.686	17.962	17.812	17.986
	(0.972)	(1.133)	(0.943)	(1.231)
Capital-labor ratio				
	19.839	20.014	20.007	20.197
	(1.225)	(1.332)	(1.168)	(1.265)
Log total employment				
	2.431	2.364	2.330	2.222
	(1.322)	(1.530)	(1.235)	(1.464)

Table 9: Difference-in-differences estimates for the effect on firm outcomes by sector

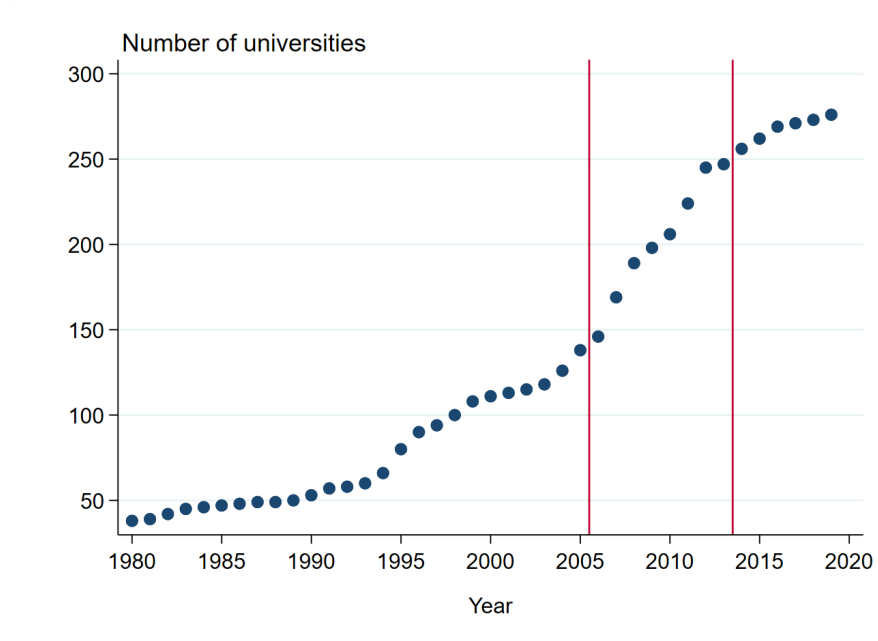
	Capital intensity	Share of college-educated workers	TFP	Labor productivity
	(1)	(2)	(3)	(4)
Panel A: Service				
25-50 km x 2008-2011	0.021 (0.047)	0.005 (0.006)	-0.001 (0.003)	0.000 (0.028)
25-50 km x 2012-2018	0.027 (0.049)	-0.012 (0.012)	0.003 (0.003)	0.031 (0.031)
0-25 km x 2008-2011	-0.067 (0.054)	0.013* (0.008)	0.008*** (0.003)	0.060** (0.027)
0-25 km x 2012-2018	-0.015 (0.054)	0.013 (0.012)	0.013*** (0.003)	0.078** (0.031)
Treatment x 2008-2011	-0.003 (0.043)	0.018*** (0.006)	0.006*** (0.002)	0.032 (0.025)
Treatment x 2012-2018	-0.059 (0.047)	0.027** (0.011)	0.013*** (0.003)	0.068** (0.030)
N	940,028	285,698	1,053,546	1,053,546
Panel B: Manufacturing				
25-50 km x 2008-2011	-0.112 (0.083)	0.009 (0.006)	0.020*** (0.006)	0.206*** (0.058)
25-50 km x 2012-2018	-0.113 (0.081)	0.007 (0.008)	0.020*** (0.006)	0.184*** (0.058)
0-25 km x 2008-2011	-0.096 (0.085)	0.011* (0.007)	0.020*** (0.006)	0.162*** (0.057)
0-25 km x 2012-2018	-0.048 (0.081)	0.015* (0.009)	0.021*** (0.006)	0.137** (0.059)
Treatment x 2008-2011	-0.079 (0.084)	0.016** (0.006)	0.022*** (0.006)	0.179*** (0.057)
Treatment x 2012-2018	-0.027 (0.080)	0.030*** (0.008)	0.021*** (0.006)	0.149** (0.059)
N	307,844	95,108	321,892	321,894

Note: The table shows the results from estimating a difference-in-differences model on firm-level outcomes. TFP (in log) is estimated using Akerberg et al. (2015). Labor productivity (in log) is measured as value-added per worker. Capital intensity (in log) is measured as capital-revenue ratio. Skill intensity is measured as the share of workers with a college education. The table reports the estimated interaction term between district treatment status and post. All models control for the number of universities existing before the expansion interacted with a linear trend, district fixed effects, province-by-year fixed effects, and industry-by-year fixed effects.

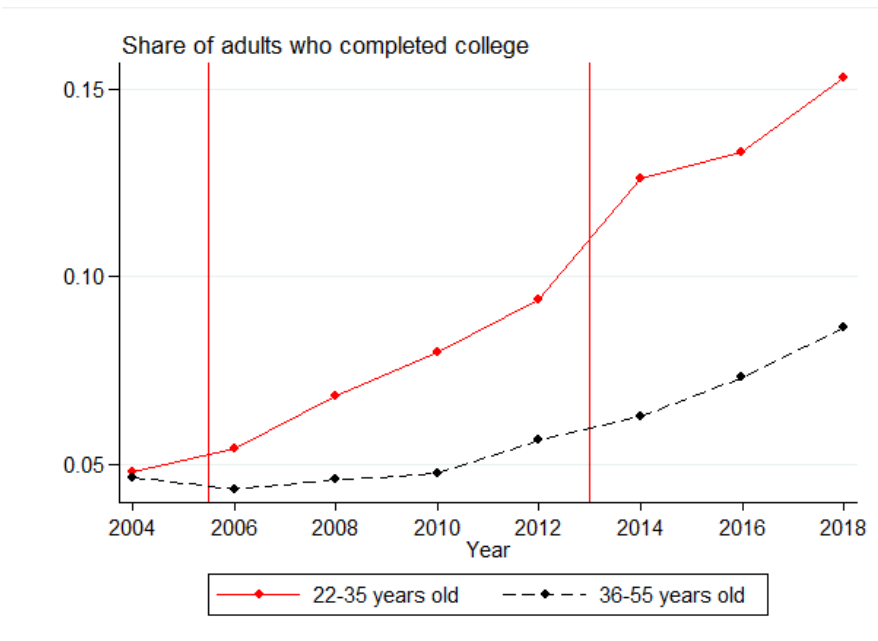
FIGURES

Figure 1: Number of universities and share of adults completing college education over time

(a) Number of universities by year

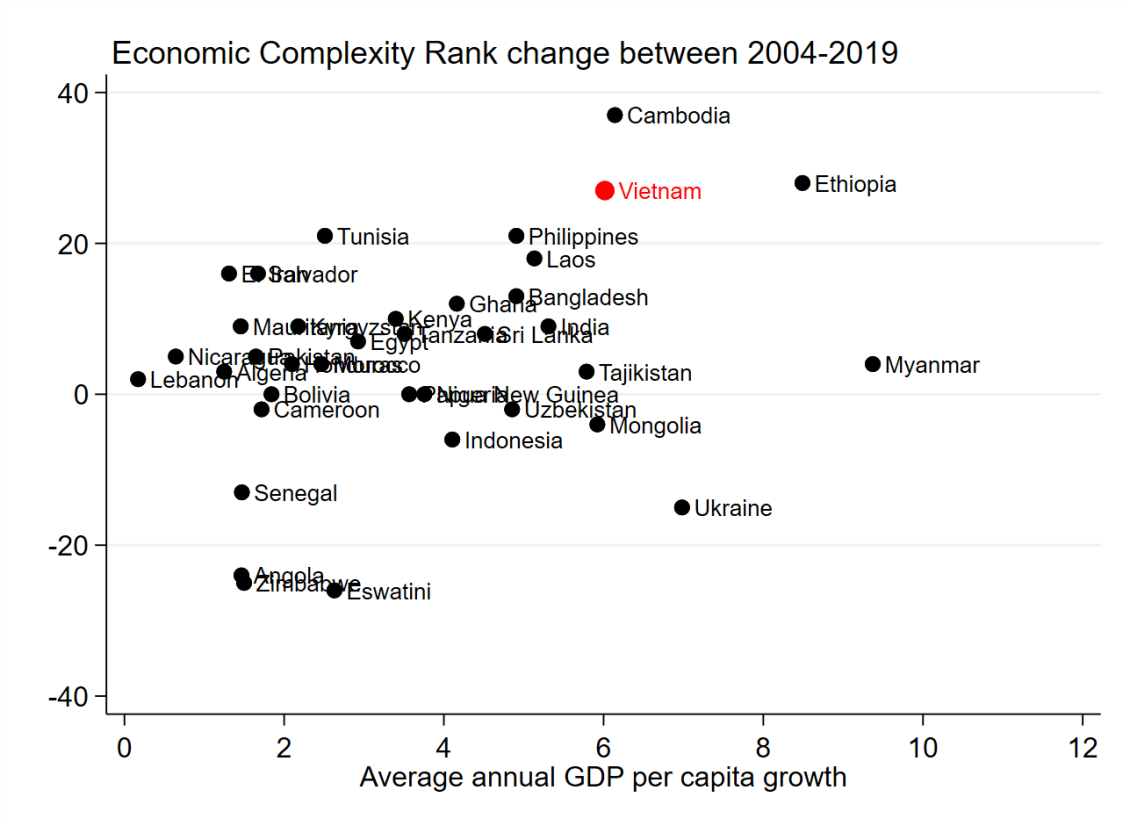


(b) Share of college-educated adults by age and year



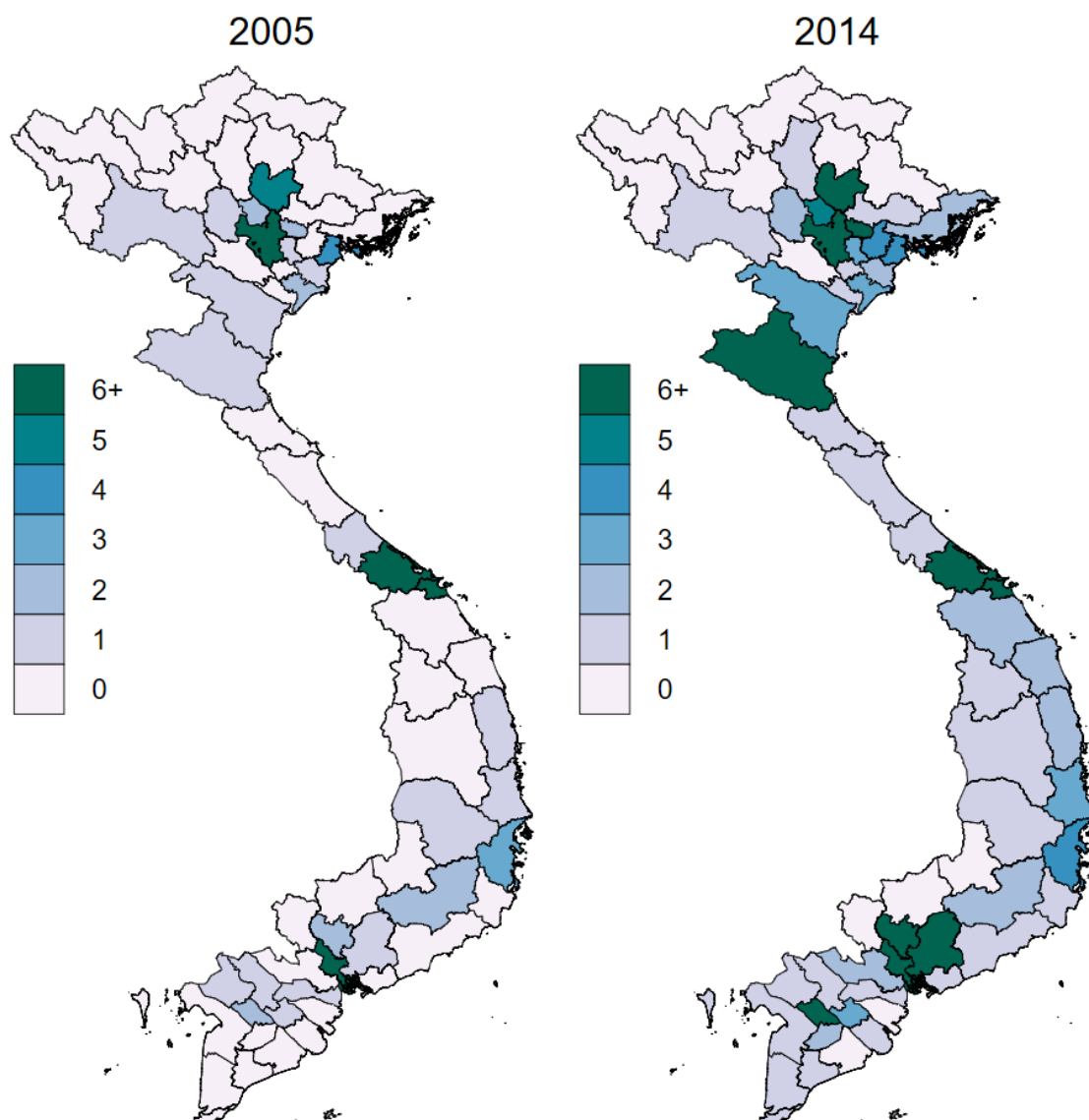
Note: Figure (a) shows the number of universities by year based on data collected from official documents. Figure (b) shows the share of adults between age 22-55 who completed college education based on data from the Vietnam Household Living Standard Survey for 2004-2018.

Figure 2: Economic Complexity Rank change and Annual GDP growth of lower middle income countries



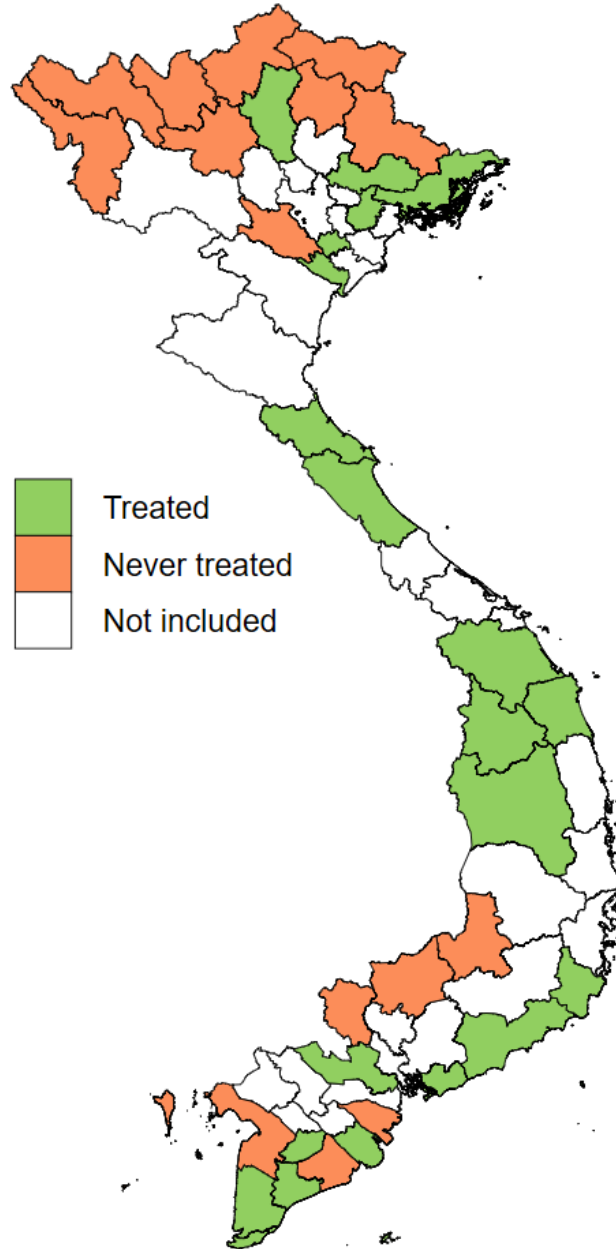
Note: Annual growth in GDP per capita is from the World Development Indicators database provided by the World Bank. Economic complexity rank is publicly available in The Observatory of Economic Complexity database (Hausmann et al., 2014).

Figure 3: Number of universities by provinces in 2005 and 2014



Note: The graph shows the number of universities in each province in 2005 and 2015.

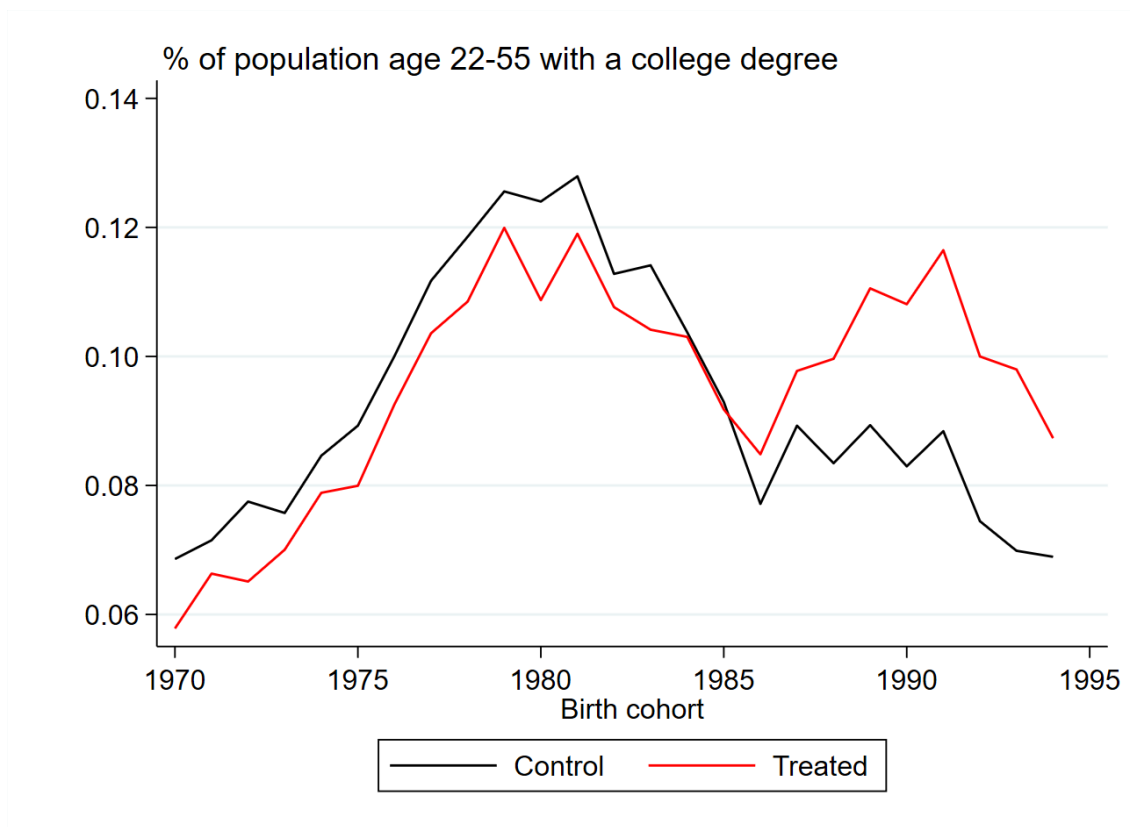
Figure 4: Treatment status of provinces



Source: Data on university location and opening date are collected by the authors from official documents.

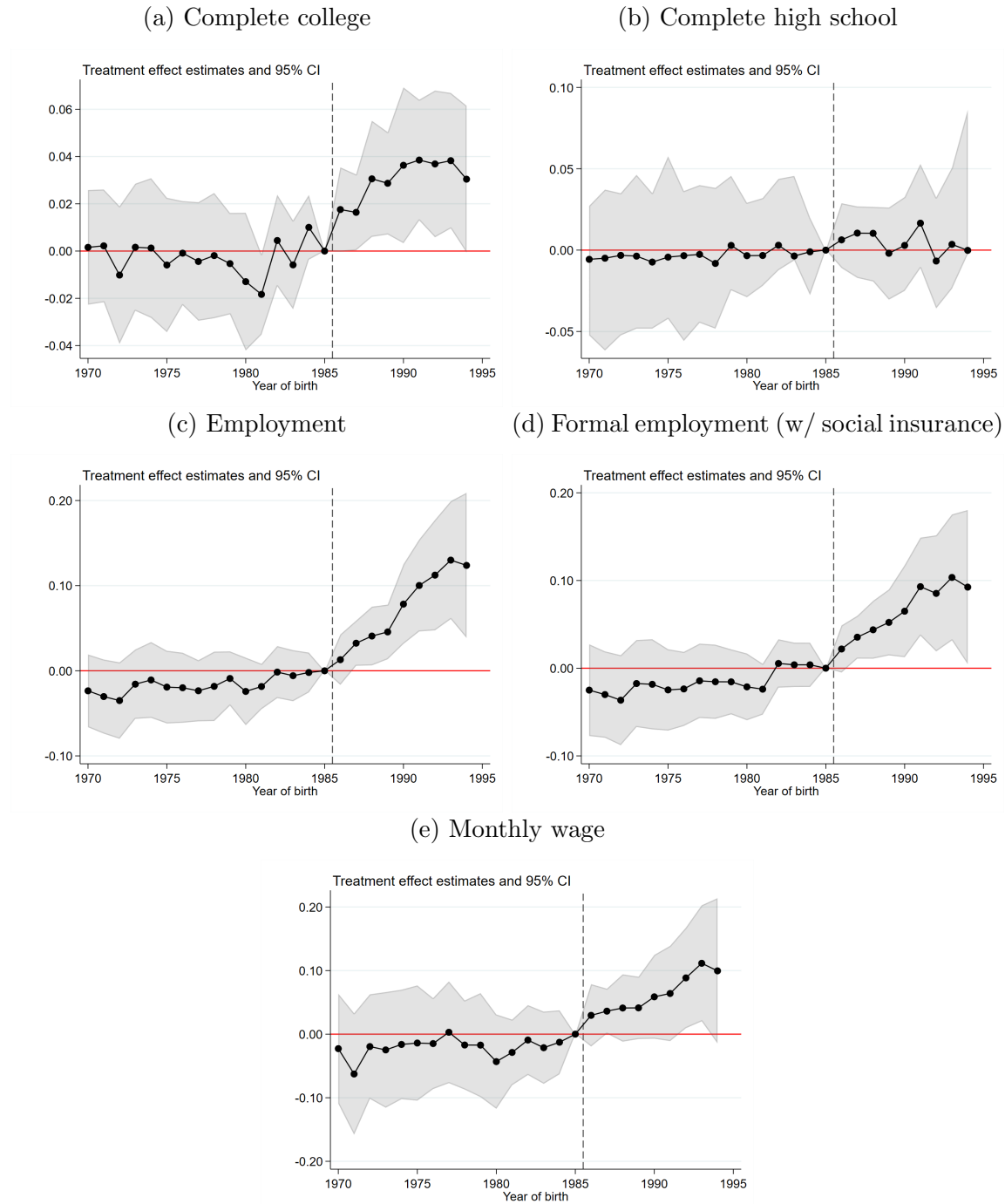
Note: Treated provinces are those with a new university for the first time during the expansion. Never-treated provinces are those that never had a university before. Provinces that already had existing universities are not included in the cohort-based difference-in-differences analysis.

Figure 5: Probability of having completed university education by birth cohorts and provinces



Source: Authors' own estimation from the 2015-2019 LFS.

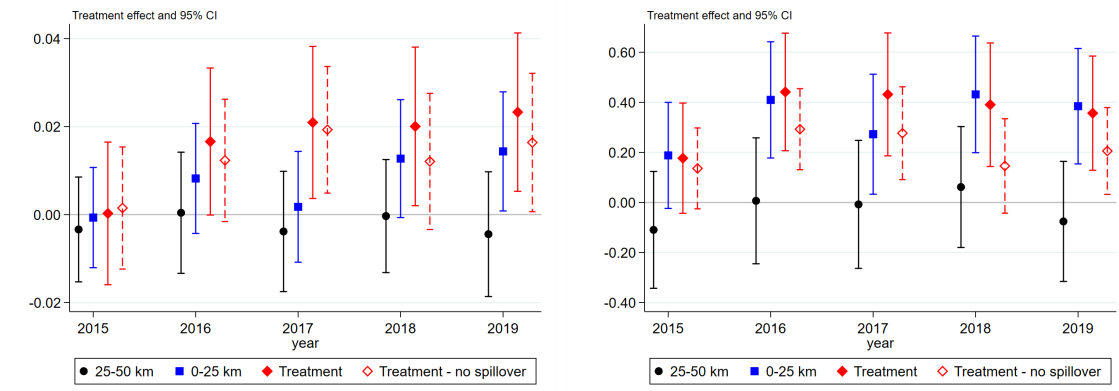
Figure 6: Event study estimation (cohort) for the effects of the higher education expansion



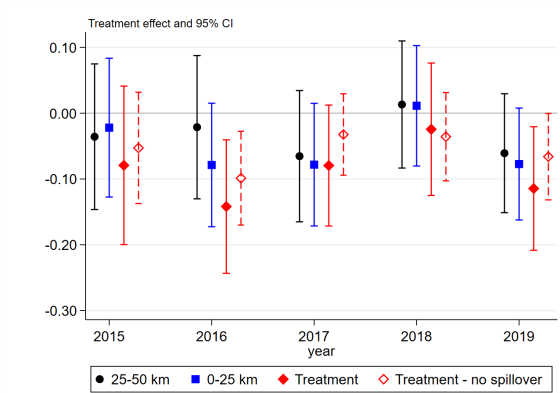
Note: The graphs display event study estimation for the effects of the higher education expansion on college education, employment, log monthly wage, occupational pay level, skill intensity of industry, and formal employment. Occupational level is measured by occupational wage percentile in 2011; formal employment is measured by having a job with social insurance. All models control for province, year, and cohort fixed effects. Standard errors are clustered at the province-level and 95% confidence intervals are displayed.

Figure 7: Event study results for the effects of the expansion on district-level outcomes

(a) Share of adults with college education (b) Relative supply of college-educated workers

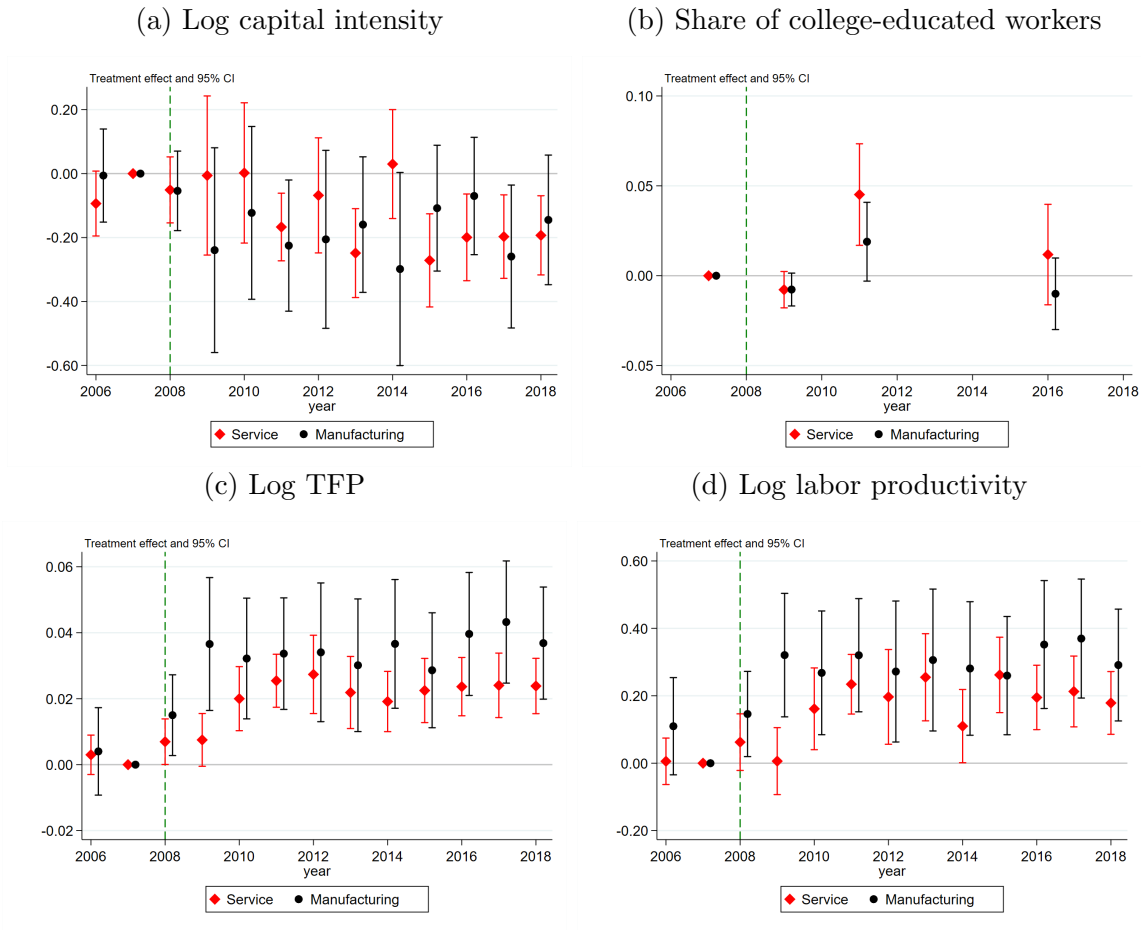


(c) College wage premium



Note: These graphs show the event-study estimation results for the effects of the higher education expansion on district-level outcomes. Each estimate shows the coefficient of the interaction term between year dummy and treatment status. NOTE that the graphs show the results of TWO models: the first three coefficients of each year correspond to the spatial spillover model, and the last coefficient corresponds to the model without spillover. In the spatial spillover model, the control units are districts that are without new universities but are 50-100 kilometers away from districts with new universities. In the non-spillover model, the control units include any districts without new universities. All models control for district and year fixed effects, average age and age squared, share of female, local demand shocks, and number of existing universities interacted with a time trend. Standard errors are clustered at the province-level and 95% confidence intervals are displayed. Sample includes individuals between 22 and 55. Relative supply is measured by log ratio of college to non-college adults who earn monthly wage. College wage premium is measured by log ratio of monthly wage of college to non-college workers.

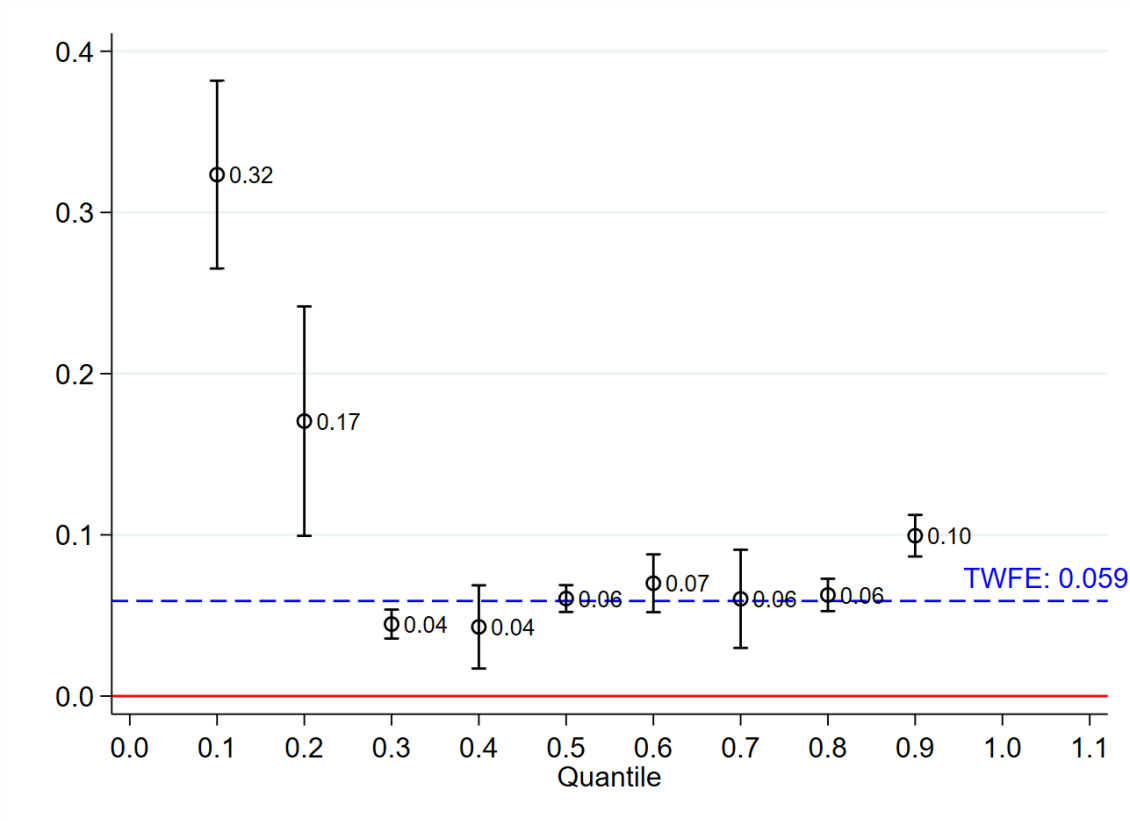
Figure 8: Event study results for the effects of the expansion on firm-level productivity



Note: These graphs show the event-study estimation results for the effects of the higher education expansion on firm-level outcome. Each estimate shows the coefficient of the interaction term between year dummy and treatment status. The model accounts for spatial spillover, but we only show the results for the treatment district. All models control for district and year fixed effects and 3-digit industry code-by-year fixed effects. Standard errors are clustered at the province-level and 95% confidence intervals are displayed. Capital intensity is measured by total value of capital divided by total revenue. Labor productivity is measured by value added per worker.

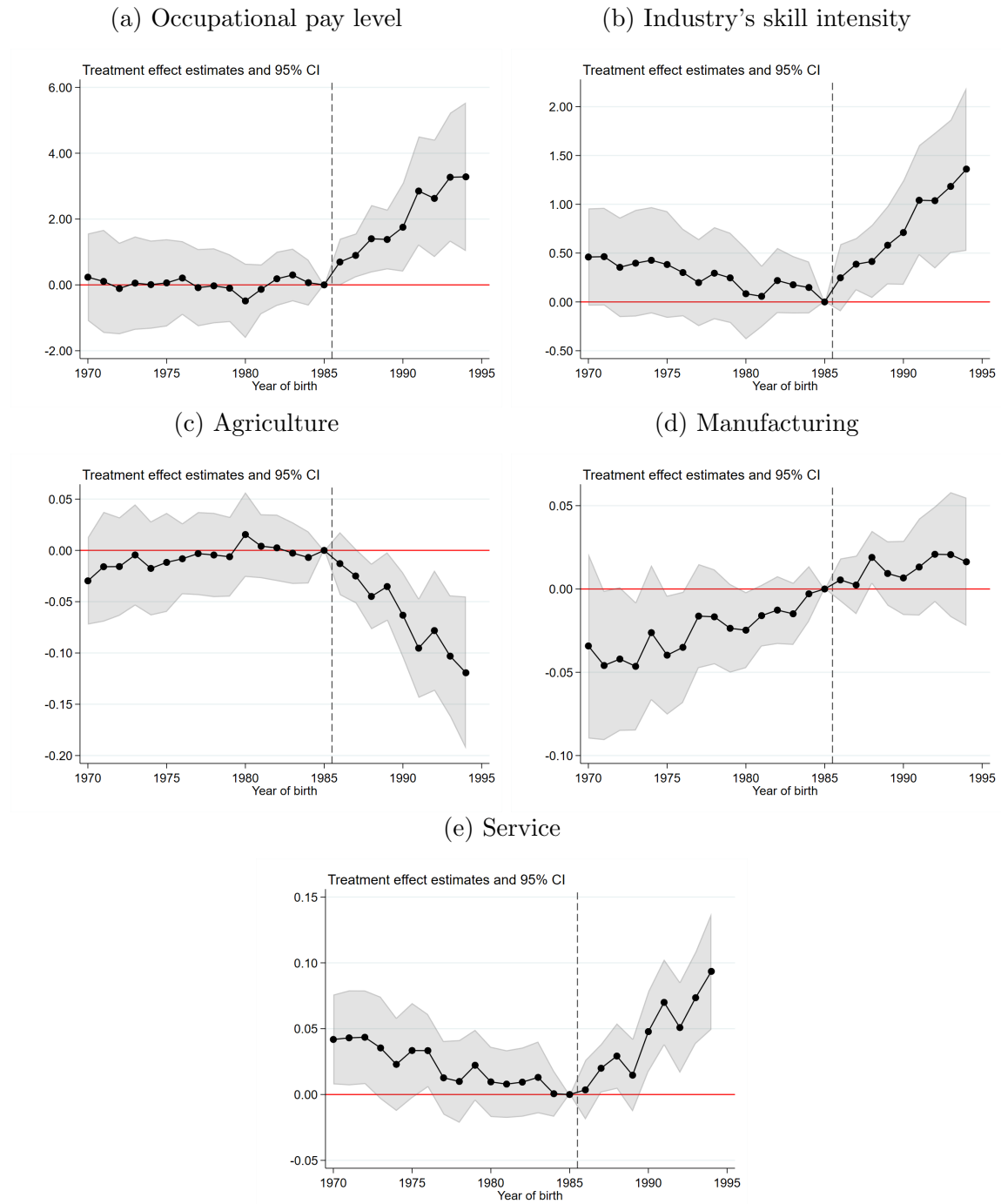
Appendix

Figure A1: Change-in-changes Estimates for the effect of the expansion on log monthly wage



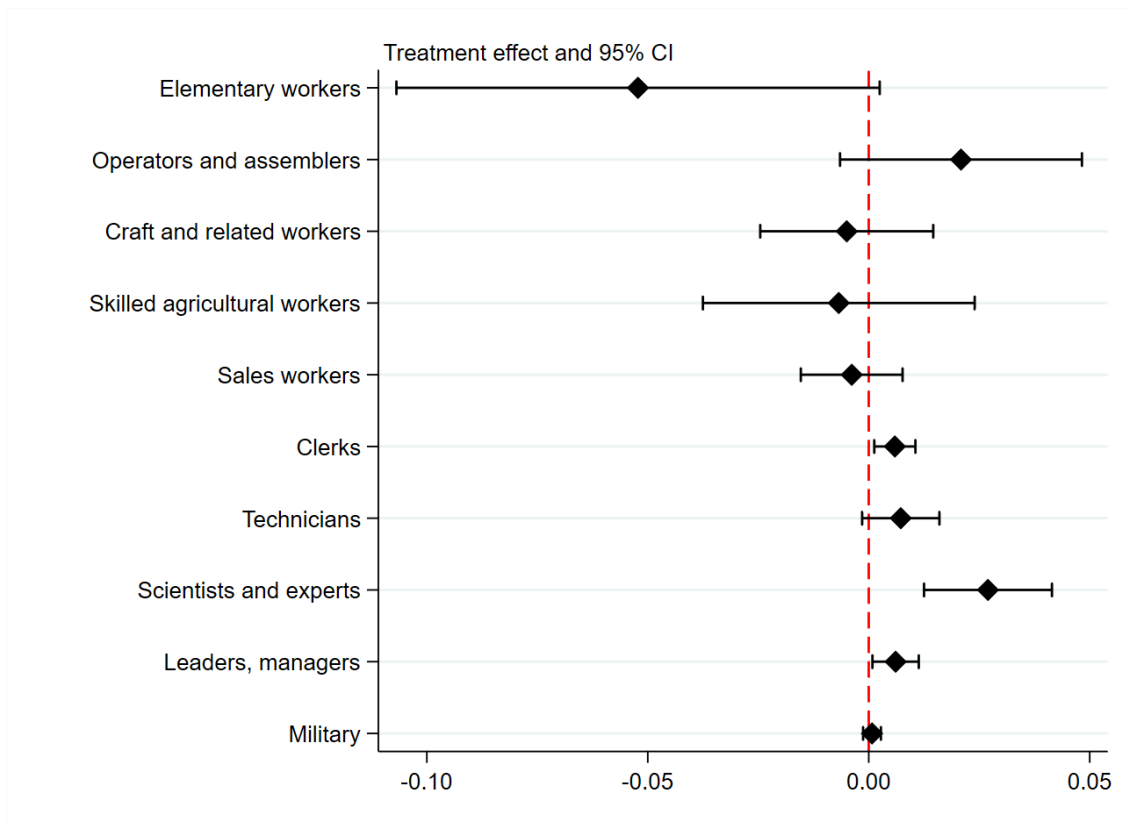
Note: The graph shows the quantile treatment effects estimated from a change-in-changes model (Athey and Imbens, 2006). The treatment effects are bounded.

Figure A2: Event study estimation (cohort) for the effects of the higher education expansion on sector-specific employment



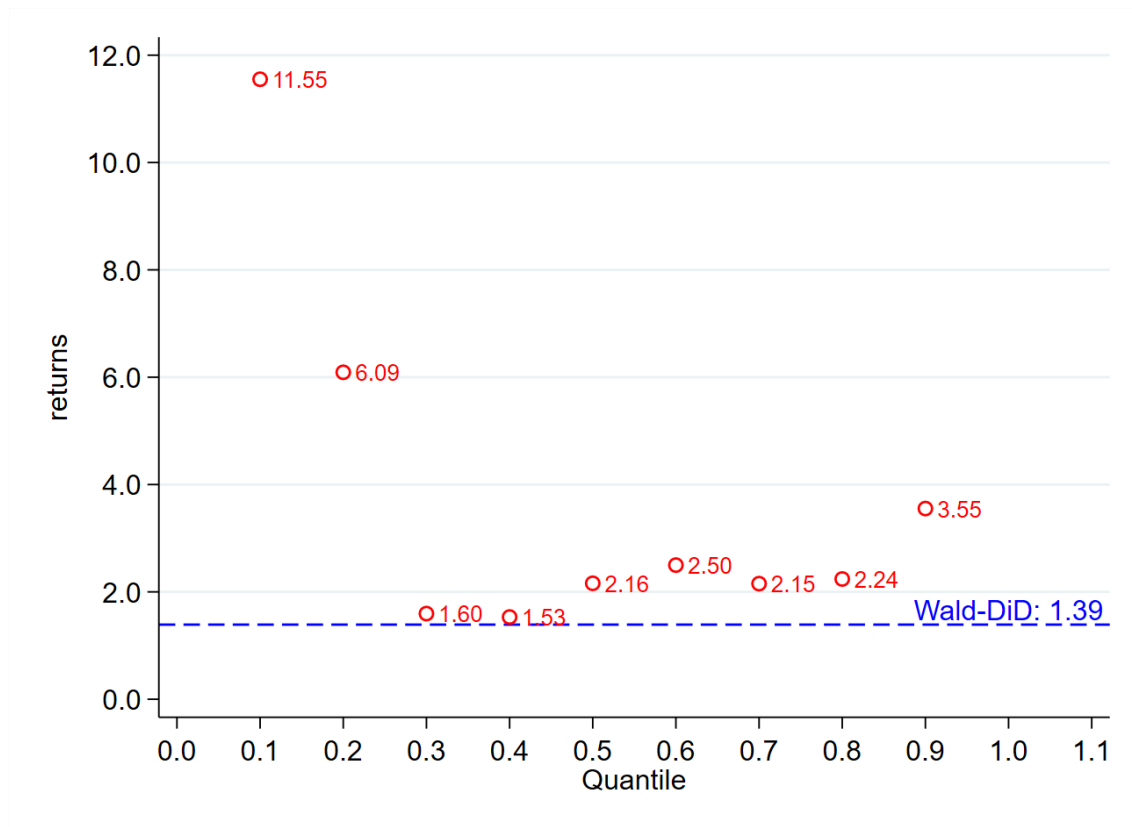
Note: The graphs display event study estimation for the effects of the higher education expansion on the probability of working in different sectors. All models control for province, year, and cohort fixed effects. Standard errors are clustered at the province-level and 95% confidence intervals are displayed.

Figure A3: DiD estimate for the effects on type of occupations



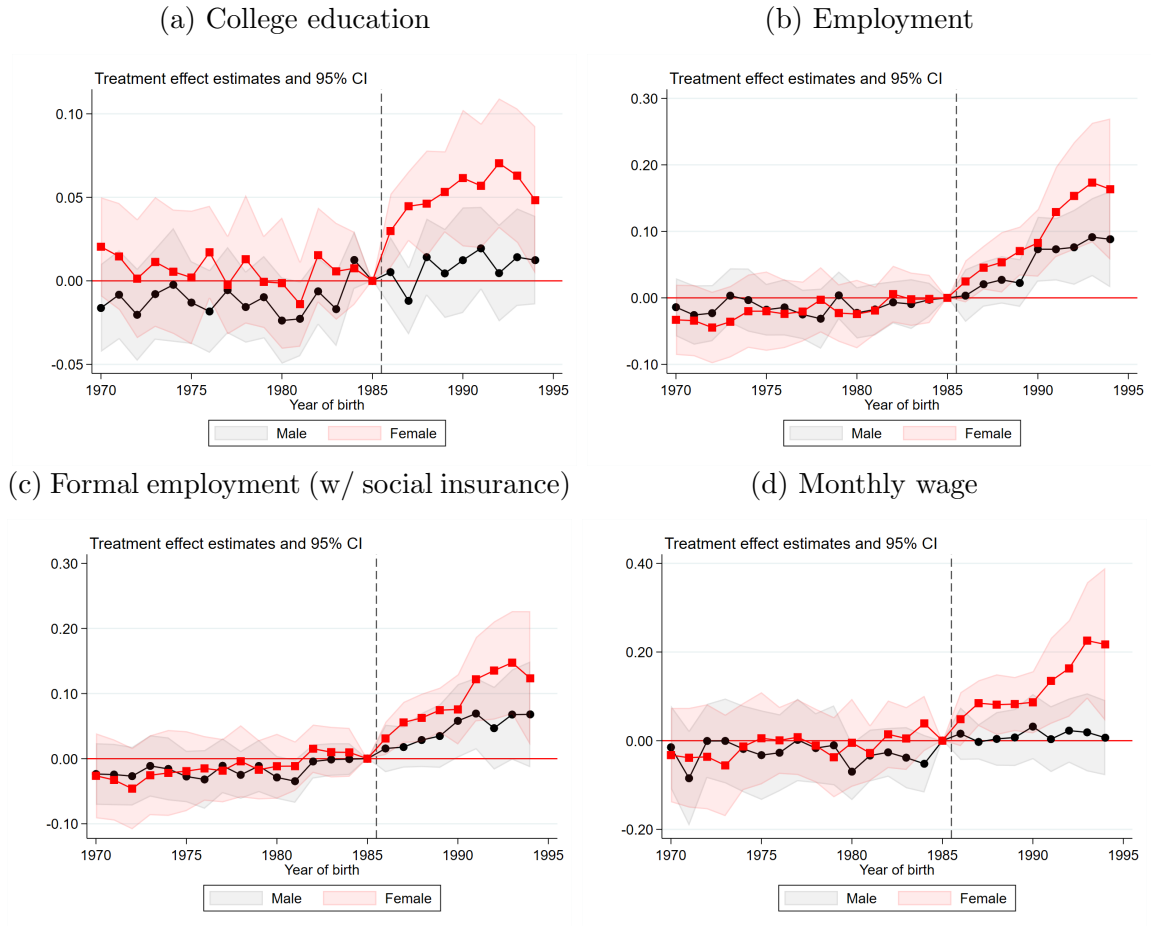
Note: Graph shows the coefficient of the DiD interaction term and its 95% confidence interval. Each outcome is a binary variable indicating whether an individual works in the given occupation. All models control for province-by-year fixed effects and birth cohort fixed effects. Standard errors are clustered at the province level.

Figure A4: Wald-CiC and Wald-DiD estimates for returns to college education



Note: Graph presents the Wald-CiC estimates for the quantile returns to college education. Each dot is calculated by taking the quantile treatment effect on wage in Figure A4 divided by the ATT on college completion in Table 3, i.e., 0.028. The Wald-DiD estimate is calculated by dividing the ATT on wage by the ATT on college completion in Table 3.

Figure A5: Event study estimation (cohort) for the effects of the higher education expansion by gender



Note: The graphs display event study estimation for the effects of the higher education expansion on college education, employment, formal employment, and log monthly wage. Formal employment is measured by having a job with social insurance. All models control for province, year, and cohort fixed effects. Standard errors are clustered at the province level.

Table A1: Difference-in-differences estimates for the effect of exposure to the higher education expansion on other labor market outcomes

	Occupational pay	Industry skill	Employment		
	level	intensity	Agriculture	Manufacturing	Service
	(1)	(2)	(3)	(4)	(5)
Specification 1: TWFE					
	1.854***	0.467**	-0.052**	0.038*	0.022***
	(0.676)	(0.174)	(0.023)	(0.021)	(0.006)
N	725508	788629	725395	725395	725395
Specification 2: Pre-treat characteristics \times cohort trends					
	0.871	0.358**	-0.021	-0.008	0.019***
	(0.586)	(0.166)	(0.020)	(0.018)	(0.007)
N	725508	788629	725395	725395	725395
Specification 3: Province \times cohort trends					
	1.346***	0.528***	-0.049***	0.007	0.028***
	(0.422)	(0.188)	(0.015)	(0.005)	(0.008)
N	725508	788629	725395	725395	725395
Specification 4: Same as 3, exposure using province of origin					
	1.492***	0.567**	-0.055***	0.003	0.033***
	(0.511)	(0.218)	(0.019)	(0.006)	(0.010)
N	444701	484152	446534	446534	446534

This table reports the difference-in-differences estimate for the effects of the expansion on college attainment and labor market outcomes. Only the coefficient of the Post \times Exposed is reported. All models control for age, age squared, gender, province-by-year fixed effects, and birth cohort fixed effects. In Specification 2, we further control for pre-treat province-level characteristics interacted with linear cohort trends. The characteristics include college enrollment, share of workers in agriculture, manufacturing, and service, as well as average income per capita. In Specification 3, we instead control for province-specific linear cohort trends. In Specification 4, we use province of origin to construct the exposure measure and estimate a similar model as Specification 3. All samples include individuals between age 22 and 55. All standard errors are clustered at the province level. Data is drawn from LFS 2015-2018. Occupational pay level is measured as the wage percentile of 2-digit occupation code, so positive effect means being in an occupation with higher pay level. Skill intensity of industry is measured by the share of college-educated workers of the 3-digit industry code.

Table A2: District-level difference-in-differences estimates with spatial spillover for the effect of having any new universities on educational and labor market outcomes with an alternative control group

	College share	Employment			Monthly wage		
		College	Non-college	Log (2)/(3)	College	Non-college	Skill premium
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Specification 1							
50-100 km \times Post	-0.007 (0.006)	0.022 (0.014)	0.023* (0.014)	0.121 (0.109)	-0.020 (0.028)	0.141*** (0.054)	-0.173*** (0.053)
25-50 km \times Post	-0.009* (0.005)	0.027** (0.013)	0.029** (0.013)	0.109 (0.100)	0.005 (0.027)	0.206*** (0.049)	-0.211*** (0.050)
0-25 km \times Post	0.001 (0.005)	0.033*** (0.012)	0.007 (0.013)	0.494*** (0.096)	0.003 (0.027)	0.238*** (0.046)	-0.233*** (0.048)
Treatment \times Post	0.009 (0.007)	0.060*** (0.015)	-0.054*** (0.015)	0.515*** (0.099)	-0.009 (0.032)	0.260*** (0.049)	-0.270*** (0.050)
N	4107	4029	4107	4020	4020	4107	4020
District FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✗	✗	✗	✗	✗	✗	✗
Panel B: Specification 2							
50-100 km \times Post	-0.002 (0.008)	0.039** (0.019)	-0.014 (0.016)	0.214 (0.150)	-0.056 (0.035)	-0.015 (0.047)	-0.052 (0.053)
25-50 km \times Post	0.002 (0.010)	0.058*** (0.022)	-0.000 (0.020)	0.165 (0.178)	-0.030 (0.046)	0.062 (0.058)	-0.091 (0.066)
0-25 km \times Post	0.016 (0.011)	0.052** (0.022)	-0.040* (0.022)	0.616*** (0.180)	-0.035 (0.050)	0.150** (0.062)	-0.173** (0.068)
Treatment \times Post	0.022* (0.011)	0.076*** (0.023)	-0.103*** (0.023)	0.634*** (0.181)	-0.043 (0.052)	0.158** (0.063)	-0.195*** (0.069)
N	4107	4029	4107	4020	4020	4107	4020
District FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓

This table reports the difference-in-differences estimates for the effects of the expansion at the district level. Relative supply of college-educated workers is measured by $\log(\text{number of college-educated employed}/\text{number of non-college employed})$. Skill premium is measured by $\log(\text{wage of college-educated}/\text{wage of non-college})$. All outcomes are of individuals between age 22 and 54 aggregated at the district-by-year level. The results are reported for the treatment group and control groups that are 0-25 kilometers and 25-50 kilometers away from a treatment district. The control group here are districts that are over 100 kilometers away from a treatment university. We show the estimates for treatment status interacted post, where post is an binary variable for any year after 2011. All models control for average age, age squared, share of female, and the number of existing universities in 2005 interacted with a linear time trend term. Specification 1 in Panel A controls for district and year fixed effects. Specification 2 in Panel B further controls for province-by-year fixed effects. All standard errors are clustered at the district level. Data drawn from LFS 2011 and 2015-2018.

Table A3: Difference-in-differences estimates for the effect on firm-level outcomes by firm size

	Capital intensity		% college workers		TFP		Labor productivity	
	Small	Large	Small	Large	Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Service								
25-50 km x 2008-2011	0.061 (0.058)	-0.032 (0.063)	0.003 (0.006)	0.007 (0.013)	-0.001 (0.003)	-0.002 (0.004)	-0.002 (0.034)	-0.038 (0.048)
25-50 km x 2012-2018	0.045 (0.063)	0.011 (0.079)	0.002 (0.014)	-0.019 (0.014)	0.002 (0.003)	0.001 (0.004)	0.025 (0.035)	-0.009 (0.054)
0-25 km x 2008-2011	0.037 (0.064)	-0.089 (0.060)	0.017*** (0.006)	0.020 (0.020)	0.002 (0.003)	0.003 (0.003)	0.011 (0.033)	0.032 (0.045)
0-25 km x 2012-2018	0.078 (0.071)	-0.022 (0.070)	0.040** (0.017)	0.012 (0.020)	0.009*** (0.003)	0.002 (0.004)	0.068* (0.037)	-0.012 (0.051)
Treatment x 2008-2011	0.149*** (0.057)	-0.077 (0.053)	0.034*** (0.005)	-0.010 (0.012)	0.001 (0.003)	0.002 (0.003)	-0.010 (0.031)	-0.023 (0.040)
Treatment x 2012-2018	0.076 (0.069)	-0.081 (0.066)	0.077*** (0.015)	-0.020 (0.014)	0.009*** (0.003)	0.003 (0.004)	0.061* (0.036)	-0.042 (0.047)
N	573,374	366,614	165,242	120,418	674,793	378,713	674,793	378,713
Panel B: Manufacturing								
25-50 km x 2008-2011	-0.157 (0.177)	-0.110* (0.065)	0.013 (0.018)	0.008 (0.007)	0.013* (0.007)	0.018** (0.007)	0.116* (0.067)	0.199*** (0.069)
25-50 km x 2012-2018	-0.370** (0.164)	-0.077 (0.071)	0.024 (0.027)	0.007 (0.007)	0.013* (0.007)	0.017** (0.007)	0.095 (0.071)	0.189*** (0.067)
0-25 km x 2008-2011	-0.258 (0.176)	-0.058 (0.070)	0.011 (0.016)	0.010 (0.007)	0.010 (0.007)	0.019*** (0.007)	0.106* (0.064)	0.161** (0.069)
0-25 km x 2012-2018	-0.294* (0.164)	-0.001 (0.073)	0.018 (0.028)	0.016** (0.008)	0.010 (0.007)	0.018** (0.007)	0.107 (0.070)	0.135* (0.071)
Treatment x 2008-2011	-0.258 (0.174)	-0.053 (0.068)	0.026* (0.016)	0.011 (0.007)	0.012* (0.007)	0.021*** (0.007)	0.120* (0.063)	0.185*** (0.068)
Treatment x 2012-2018	-0.308* (0.162)	0.021 (0.072)	0.046* (0.027)	0.022*** (0.008)	0.013* (0.007)	0.018** (0.007)	0.122* (0.069)	0.152** (0.070)
N	85,019	222,789	23,350	71,698	96,059	225,798	96,060	225,799

Note: The table shows the results from estimating a difference-in-differences model on firm-level outcomes. TFP (in log) is estimated using Akerberg et al. (2015). Labor productivity (in log) is measured as value-added per worker. Capital intensity (in log) is measured as capital-revenue ratio. Skill intensity is measured as the share of workers with a college education. The table reports the estimated interaction term between district treatment status and post. All models control for the number of universities existing before the expansion interacted with a linear trend, district fixed effects, province-by-year fixed effects, and industry-by-year fixed effects.

A Derive results for Lewis 2013 model

Lewis (2013) assume that capital is supplied elastically in the long run, resulting in $g_K = r$, where r is fixed. Totally differentiating this first order condition to get

$$K.g_{KK}.d \ln K + C.g_{KC}.d \ln C + N.g_{KN}.d \ln N = 0 \quad (7)$$

Homogeneity assumption implies that

$$K.g_K + C.g_C + N.g_N = g(K, C, N)$$

Taking derivative of K on both sides to get

$$g_K + K.g_{KK} + C.g_{KC} + N.g_{KN} = g_K$$

$$\rightarrow K.g_{KK} + C.g_{KC} + N.g_{KN} = 0$$

Combine this with Equation 7 to get

$$d \ln K = \frac{C.g_{KC}}{C.g_{KC} + N.g_{KN}} d \ln C + \frac{N.g_{KN}}{C.g_{KC} + N.g_{KN}} d \ln N \quad (8)$$

Note that by Euler's theorem, we also get

$$Q = r.K + w_C.C + w_N.N$$

$$\rightarrow d \ln Q = \frac{r.K}{Q} d \ln K + \frac{w_C.C}{Q} d \ln C + \frac{w_N.N}{Q} d \ln N$$

$$d \ln Q = s_K d \ln K + s_C d \ln C + (1 - s_K - s_N) d \ln N$$

We can subtract $d \ln rK$ on both sides to get

$$d \ln Q - d \ln rK = s_K d \ln K - d \ln rK + s_C d \ln C + (1 - s_K - s_N) d \ln N$$

$$\rightarrow -d \ln s_K = s_K d \ln K - d \ln rK + s_C d \ln C + (1 - s_K - s_N) d \ln N$$

then substitute Equation 8 in $d \ln K$ and rearrange to get

$$d \ln s_K = Q \frac{(1 - s_C - s_K) s_C (\frac{g_{KC}}{g_C} - \frac{g_{KN}}{g_N})}{C.g_{KC} + N.g_{KN}} (d \ln C - d \ln N)$$

B Robustness checks for the DiD at the individual level

As discussed briefly in the paper, two important concerns about the empirical strategy we use to assess the individual-level effects of the expansion are (1) misspecification when not incorporating the staggered timing design and (2) invalid cluster-robust inference based on small number of clusters. We outline how to address both concerns in this Appendix and the results relative to our main findings.

B.1 Staggered difference-in-differences and treatment effect heterogeneity

We check whether our results are robust when applying a staggered difference-in-differences design by estimating a staggered DiD model on the same data. Denote g the year that province p opens a new university, consider the following two-way fixed effects (TWFE) regression model:

$$Y_{i,p,c,t} = \delta \cdot Exposed_{p,c} + \mathbf{X}'\boldsymbol{\theta} + \gamma_{p,t} + \eta_c + \epsilon_{i,p,c,t} \quad (9)$$

where $Exposed_{p,c} = \mathbf{1}\{g - c \leq 21\}$ indicates whether cohort c is 21 years old or younger when province p opens a new university in year g , thus measuring the exposure of the given cohort in the given province. The model controls for covariates X , province-by-year fixed effects $\gamma_{p,t}$, and cohort fixed effects η_c . We cluster the standard errors on province.

It is important to note that δ in the TWFE model is a variance-weighted average of multiple 2x2 DiD estimates which compare different treatment and control groups (Goodman-Bacon, 2021). One particular troublesome comparison is between the already-treated observations as control and later-treated observations as treatment. This comparison would introduce bias to the TWFE estimate when the treatment effects are heterogeneous (De Chaisemartin

and d’Haultfoeuille, 2020; Goodman-Bacon, 2021; Baker et al., 2022).²⁵

To allow for treatment effect heterogeneity, we also consider a stacked regression approach which is robust against this problem (Cengiz et al., 2019). Since we are comparing across provinces, birth cohorts, and sample years, we extend the original approach to allow for heterogeneity across all three dimensions. In each sample year, we construct several datasets in which the treatment group is comprised of all observations in provinces that open new universities in the same year and the control group is comprised of all observations in provinces that never have a university. In other words, we have one dataset for each group of provinces with the same opening year g and each sample year t . Since the treatment group in each dataset has the same treatment date and the control group is never treated, the standard TWFE estimator would not be biased by treatment effect heterogeneity (Cengiz et al., 2019; Goodman-Bacon, 2021; Baker et al., 2022).

Formally, let s denote each dataset, the TWFE model for each dataset is:

$$Y_{i,p,c} = \delta_s.(T_p \times Exposed_c) + \mathbf{X}\boldsymbol{\theta} + \gamma_{p,t} + \eta_c + \epsilon_{i,p,c} \quad (10)$$

where $Exposed_c = 1\{g - c \leq 21\}$ indicates whether the cohort c were 21 years or younger in year g ; δ_s is the treatment effect of dataset s and is unaffected by any treatment effect heterogeneity. Equation 10 is particularly useful in assessing treatment effect heterogeneity across treatment group g and sample year t .

We can also stack the dataset for each event g and sample year t together and estimate a similar model while controlling for province-by-dataset and cohort-by-dataset fixed effects:

$$Y_{i,p,c,s} = \delta.(T_{p,s} \times Exposed_{c,p,s}) + \mathbf{X}\boldsymbol{\theta} + \gamma_{p,s} + \eta_{c,s} + \epsilon_{i,p,c,s} \quad (11)$$

²⁵This is because the DiD estimator would subtract average change of the not-yet-treated group and the treatment effect from average changes of the already-treated group, potentially leading to the opposite sign of the true ATT (Goodman-Bacon, 2021), contributing the problem of *negative weights* documented by De Chaisemartin and d’Haultfoeuille (2020); Borusyak et al. (2021).

This step is essentially equivalent to averaging δ_s from each dataset, but we let OLS determine the weight for each dataset. In short, we bypass the treatment effect heterogeneity problem by estimating a clean treatment effect for each event and aggregate these treatment effects to obtain the weighted average treatment effect.

a) Inference for few clusters

Our sample is comprised of 15 control provinces and 21 treatment provinces, or 36 clusters in total. This raises two potential problems that may invalidate the conventional cluster-robust inference. First, when there are too few clusters, the variance matrix estimate of this approach may be biased downward and the Wald t-test tends to over-reject the null hypothesis (Cameron et al., 2008; Cameron and Miller, 2015). Second, MacKinnon and Webb (2017, 2018) show that when there are too few treated clusters, the cluster-robust t-statistics also tends to over-reject in difference-in-differences models.

To check if our inference is affected by the small number of clusters, we first consider a wild cluster bootstrap approach that imposes the null hypothesis (Cameron and Miller, 2015). However, the wild cluster bootstrap approach does not address the problem of too few treated clusters, as the bootstrap t-statistics can still have less variance than the actual t-statistics, leading to over-rejection (MacKinnon and Webb, 2017, 2018). Therefore, we also report the p-values based on a subcluster wild bootstrap procedure developed by MacKinnon and Webb (2018) that accounts for few treated clusters. We report both restricted and unrestricted version of this bootstrap, i.e., imposing and not imposing the null; MacKinnon and Webb (2018) argue that they should be similar if the procedure works well.

B.2 Results

In Table A4, we present the results when using different estimation and inference methods. In Panel A, we report the main TWFE estimation results from before; we also present the p-values from different bootstrapping procedures. Given that our number of cluster is 36, it is possible that the conventional cluster-robust inference is invalid because the variance matrix estimate is biased downward (Cameron et al., 2008). One way to avoid this is using wild cluster bootstrap that imposes the null hypothesis, i.e., wild cluster restricted bootstrap. Yet this approach may still fail when there are too few treated or untreated clusters (MacKinnon and Webb, 2017). Hence, we also apply the subcluster wild bootstrap procedure that imposes (restricted) and does not impose (unrestricted) the null hypothesis (MacKinnon and Webb, 2018).²⁶ Fortunately, we observe that the p-values calculated from different inference procedures are close to each other across all outcome variables. In other words, the statistical significance of our main results is robust to different inference procedures that account for small numbers of clusters and treated clusters.

In Panel B, we report the TWFE estimates in a staggered timing setting (see Equation 9) where we do not account for any treatment effect heterogeneity. We also present the p-values from different inference procedures. In Panel C, we report the TWFE stacked regression (see Equation 11) that accounts for treatment effect heterogeneity. We do not include bootstrapping p-values in for this estimator in the current version because they are computationally heavy.

We observe that the standard TWFE estimator in a staggered design yields mostly lower estimates than the main results (see Panel B). For example, for the effect on college completion, the estimate from the staggered design is 2.4 percentage points compared to 3.3 percentage points in the main DiD result. However, these estimates might be biased downward due to treatment effect heterogeneity in the staggered setting due to the negative weights from

²⁶We use the Stata command *boottest* to conduct these bootstrap procedures (Roodman et al., 2019).

comparing already-treated provinces (as control) to not-yet-treated provinces (as treatment group). When we use the stacked regression approach, it corrects this bias by first aligning the treatment timing and comparing with a clean control group (Cengiz et al., 2019). As a result, we find that the stacked regression estimates are larger and also more in line with the main results. We arrive at similar conclusions when estimating the event study specification for the TWFE and stacked regression approach, which are reported in Figure A7. While the treatment effects during the pre-treatment periods are relatively close between the two methods, the stacked regression yields higher estimates for each post-treatment period.

The stacked regression also provides a useful diagnosis for where the treatment effect heterogeneity may arise. As described in the previous section, we construct a separate dataset for each sample year and each group of provinces with the same treatment timing with a clean control group comprised of never-treated provinces. Instead of estimating a stacked regression on all datasets, we estimate a TWFE model on each dataset (see Equation 10) and report the results in Figure A6. Similar-colored dots represent the ATT estimates of the same group of provinces in different sample years. In most datasets, the estimates are very similar; however, the treatment effects on provinces that opened a new university in 2010 and 2011 are quite different from the rest.

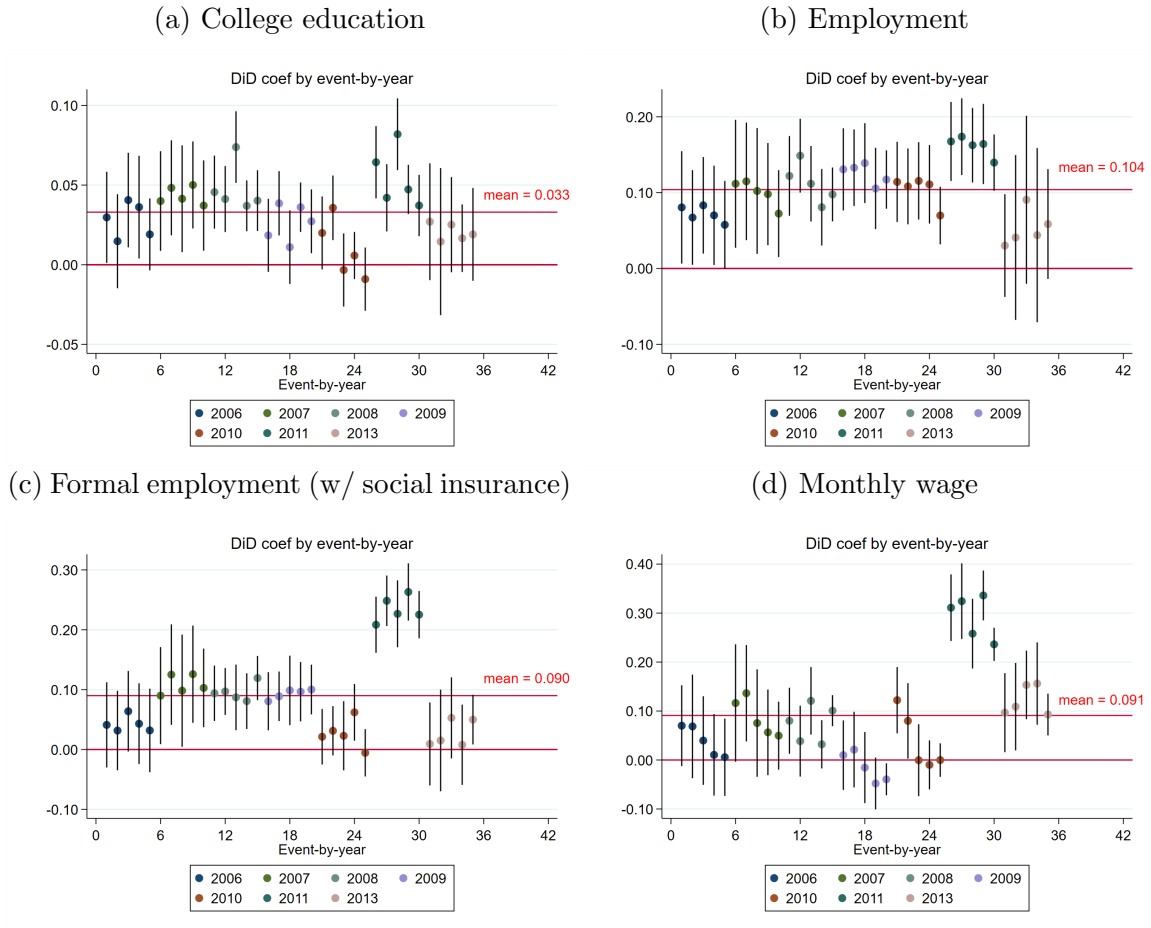
In short, these robustness checks indicate that the small numbers of clusters and treated clusters do not invalidate our cluster-robust inference. Furthermore, our main results are very similar when using a staggered design and applying a stacked regression approach to correct for negative weights, the statistical significance and magnitudes of the two estimations are very close to each other.

Table A4: Robustness checks for stacked regression and inference for few clusters

	Complete College	Complete High school	Employment	Formal employment	Log wage	Log wage (corrected)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: TWFE						
	0.033*** (0.010)	0.008 (0.016)	0.085*** (0.025)	0.077*** (0.027)	0.077** (0.030)	0.059** (0.027)
P-value						
Cluster-robust	[0.002]	[0.594]	[0.002]	[0.007]	[0.016]	[0.037]
Wild Cluster	[0.008]	[0.604]	[0.008]	[0.006]	[0.020]	[0.038]
Subcluster Wild Restr.	[0.004]	[0.634]	[0.002]	[0.010]	[0.014]	[0.052]
Subcluster Wild Unrestr.	[0.000]	[0.614]	[0.004]	[0.008]	[0.016]	[0.044]
N	788629	784540	725395	725495	599808	599808
Panel B: TWFE – Staggered						
	0.028*** (0.009)	0.000 (0.014)	0.077*** (0.022)	0.069*** (0.024)	0.043 (0.026)	0.047* (0.024)
P-value						
Cluster-robust	[0.004]	[0.979]	[0.001]	[0.008]	[0.111]	[0.055]
Wild Cluster	[0.010]	[0.978]	[0.000]	[0.014]	[0.112]	[0.062]
Subcluster Wild Restr.	[0.004]	[0.974]	[0.002]	[0.002]	[0.152]	[0.062]
Subcluster Wild Unrestr.	[0.004]	[0.972]	[0.002]	[0.004]	[0.120]	[0.052]
N	744793	740747	685006	685079	567728	567728
Panel C: Stacked TWFE – Staggered						
	0.034*** (0.010)	0.007 (0.014)	0.097*** (0.025)	0.084*** (0.027)	0.073** (0.032)	0.078** (0.034)
N	2524195	2517281	2370856	2374949	1850498	1850498

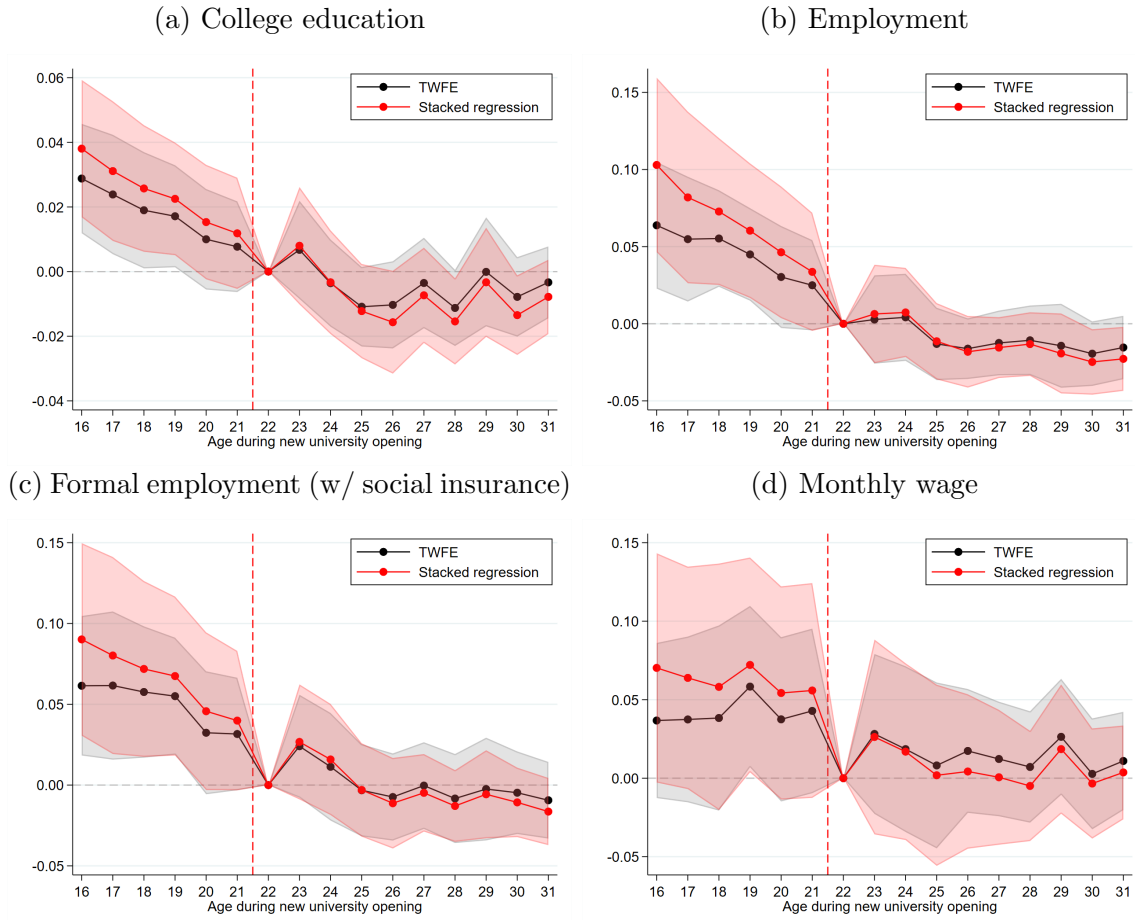
This table reports the staggered difference-in-differences estimate for the effects of the expansion on college attainment and labor market outcomes. Only the coefficient of the *Exposed* variable is reported. First model is the standard TWFE for staggered timing controlling for age, age squared, gender, province-by-year fixed effects, and birth cohort fixed effects. Second model is a modified stacked regression that accounts for treatment effect heterogeneity in staggered DiD design (Cengiz et al., 2019). Cluster-robust p-values come from clustering on province. Wild cluster restricted p-values come from wild cluster bootstrapping that imposes the null hypothesis to account for small number of clusters (Cameron et al., 2008). Subcluster wild restricted and unrestricted (imposing vs. not imposing the null) p-values come from subcluster wild bootstrapping to account for few treated clusters (MacKinnon and Webb, 2018). Data is drawn from LFS 2015-2018. Employment is defined as whether a person is employed for a job, regardless of whether it is formal or informal. Formal employment is defined as having a job that provides social insurance. Log monthly wage is for all individuals reporting having a wage, including those with an informal job.

Figure A6: Stacked event study estimation (cohort) for the effects of the higher education expansion



Note: For each sample year and for each group of provinces with the same treatment timing, we create a dataset with these treatment provinces and control provinces comprised of those that never had a university before. We estimate the standard TWFE estimate on each dataset (see Equation 10) and report the estimated coefficient with 95% CI, which corresponds to each dot on the graph. Dots vary for treatment timing (different color) and sample year. Standard errors are clustered at the province level.

Figure A7: Stacked event study estimation (cohort) for the effects of the higher education expansion



Note: Each graph shows the standard TWFE and stacked regression estimates for the event study. Standard errors are clustered at the province level.

C Estimating productivity

Consider the following Cobb-Douglas production function for firm i in year t :

$$VA_{it} = \beta_l L_{it} + \beta_k K_{it} + \omega_{it} + u_{it}$$

where VA_{it} is the annual value-added, L_{it} is total labor, K_{it} is capital, measured as the value of assets at the beginning of the year (Newman et al., 2015), and ω_{it} is the unobserved productivity shock. Given that OLS is typically biased as both L_{it} and K_{it} are likely affected by the unobserved productivity shock, we first assume that firms' investment decision is a function of labor, capital, and productivity shock, i.e., $I_{it} = f_t(L_{it}, K_{it}, \omega_{it})$,²⁷ which makes ω_{it} observable in the production function (by inverting f_t):

$$VA_{it} = \beta_l L_{it} + \beta_k K_{it} + f_t^{-1}(L_{it}, K_{it}, I_{it}) + u_{it}$$

This approach by Akerberg et al. (2015) (ACF) is different from two other convention approaches to estimate production functions, namely Olley and Pakes (1996) (OP) and Levinsohn and Petrin (2003) (LP), who do not include labor input as part of firms' investment decision. Not allowing labor input to enter the investment function means that L_{it} is a deterministic function of capital and investment and, hence, would be functionally dependent on the inverse function of investment; in other words, the coefficient of labor would not be identified.

Assuming that the productivity shock follows a first order Markov process, we can write $\omega_{it-1} = g(\omega_{it-1}) + \zeta_{it}$ where $g(\omega_{it-1})$ is the predictable component and ζ_{it} is the unpredictable/innovation component of productivity (Olley and Pakes, 1996). We also assume the following capital formation process: $K_{it} = (1 - \delta)K_{it-1} + I_{it-1}$. These assumptions give us

²⁷Investment is measured as annual change in value of fixed and long-term assets plus accumulated depreciation (Newman et al., 2015).

$E[\zeta_{it}|I_{it-1}] = 0$ and $E[\zeta_{it}|K_{it}] = 0$ (since K_{it} is determined at $t - 1$). Lastly, we assume that $E[\zeta_{it}|L_{it-1}] = 0$ (Akerberg et al., 2015). Given this set of moment conditions, we can estimate β_l and β_k .

Given that this approach requires panel data, we aggregate the firm-level variables to the district-by-year-by-industry level. We then estimate the production function by sectors and present the estimation results in Table A5. Once we obtain the estimates for β_l and β_k , we can use these estimates to calculate ω_{it} for each firm in each year, which is also our measure of total factor productivity (TFP).

Table A5: Production function estimation results

	Capital		Labor
Agriculture	0.271***	(0.098)	1.143***
Mineral	0.222***	(0.031)	1.063***
Manufacturing	0.342***	(0.031)	0.919***
Waste and electricity	0.380***	(0.065)	0.980***
Construction	0.198***	(0.028)	0.948***
Wholesale and retail	0.201***	(0.036)	1.386***
Transportation	0.057	(0.038)	1.103***
Hospitality	0.222***	(0.071)	0.682***
Information and communication	0.314***	(0.062)	1.035***
Finance, banking, and real estate	0.175***	(0.024)	1.348***
Science and technology	0.123***	(0.035)	1.384***
Administrative and support	0.156***	(0.041)	1.144***
Education, health, and social support	-0.116	(0.191)	1.535***
Entertainment	-0.112	(0.166)	1.677***
Other services	0.236	(0.250)	1.220**