Demand and Supply Effects and Returns to College Education - Evidence from a Natural Experiment with Engineers in Denmark

by

Hans-Peter Y. Qvist, Anders Holm and Martin D. Munk

Working Paper # 2016-4 **November 2016**

Centre for Human Capital and Productivity (CHCP)

Working Paper Series

Department of Economics
Social Science Centre
Western University
London, Ontario, N6A 5C2
Canada

Demand and Supply Effects and Returns to College Education – Evidence from a Natural Experiment with Engineers in Denmark

Hans-Peter Y. Qvist*, Anders Holm, Martin D. Munk*****

Abstract:

The demand and supply model predicts that a larger relative net supply of a particular skill group will negatively affect its relative wage. To test this, we use the opening of a new university in Denmark as a natural experiment. We show that the opening of Aalborg University created a shock to the supply of structural engineers in the mid-1980s. Because Aalborg University did not have a chemical engineering program, we use chemical engineers as a control group and find that the wages of structural engineers dropped in and around 1984, when the supply of structural engineers peaked.

***** Department of Sociology and Social Work, Aalborg University

** Department of Economics and Department of Sociology, University of Western Ontario (corresponding author)

*** Department of Political Science, Aalborg University

Introduction

Education is an important factor in modern societies. From a societal perspective, education can serve to maintain and enhance prosperity and growth, and estimates of returns to education are therefore of central interest to policymakers hoping to expand the educational system in light of anticipated returns. From an individual perspective, education can potentially improve skills and knowledge, which, in turn, if used to raise productivity, increases chances of employment opportunities and can be converted into higher wages, but a sudden change in the educational system or at the labor market may have an affect even in very prosperous sectors of the labor market. An exogenous shock in the supply of structural engineers is empirically identified, and we find that wage is sensitive to the relative supply of engineers.

Since the pioneering work of Becker (1962, 1964/1993) and Mincer (1958, 1962, 1974), who formulated the now-standard Human Capital Earnings Function, which explains individual wage as a function of education and labor market experience, an abundance of empirical studies have confirmed the relationship between education and labor market returns (Card 1999). College graduates command higher wages, experience less unemployment, and secure better jobs than their less educated counterparts (Hout 2012). A surge of research on the returns to college education have carefully and often ingeniously addressed whether this result holds in the presence of various challenges to the exogenous selection assumption (also known as the no-confounding assumption). These studies use various research design features to mitigate the effect of non-random selection on unobservable factors, such as individual-level innate ability – including twin studies and sibling fixed effects (Angrist and Krueger 1991; Ashenfelter and Krueger 1994; Altonji and Dunn 1996; Behrman, Rosenzweig, and Taubman 1996; Ashenfelter and Rouse 1998; Rouse 1999; Duflo 2001; Heckman and Vytlacil 2001). However, studies typically uphold the stable-unit-treatment-value assumption (SUTVA) (Rubin 1980, 1990). The SUTVA implies that the potential outcomes for a given individual do not vary with the treatment assigned to other individuals (Morgan and Winship 2015, 48). Accordingly, the causal effect of a particular college education on labor market outcomes cannot be a function of the other individuals receiving that same education (Imbens and Rubin 2015). Clearly, this assumption is problematic because there is likely to be interference between individuals, given that college graduates compete in the labor market. Thus, if sufficiently large numbers enroll in the same graduate program, thereby increasing the supply of suitable candidates within that particular skill group, the competition for jobs will increase, resulting in a lower price of labor within that skill group; consequently, employment rates and wages drop. This is also known as a general equilibrium effect, as opposed to a partial equilibrium effect, which is typically what is estimated with a variant of the standard earnings function.

In general, estimating the returns to college education with the standard earnings function (under the SUTVA) is incompatible with the demand and supply model, which predicts that a larger relative net supply of a particular skill group will negatively affect their relative wage. Conversely, a smaller relative net supply of a particular skill group will affect their wage positively (Katz and Autor 1999; Katz and Murphy 1992). If this is the case, the SUTVA is violated because the returns to a particular college education for an individual depend on other individuals receiving that same college education. Accordingly, a clear drawback of the standard earnings function is that it ignores basic demand and supply factors (Heckman, Layne-Farrar, and Todd 1996, 563). This is problematic because previous empirical studies show that wages respond to the supply of skilled labor (Fallon and Layard 1975; Angrist 1995; Johnson 1997; Topel 1997; Card and Lemieux 2001). Empirical evidence in favor of the demand and supply model include a study by Angrist (1995) that uses a natural experiment with rapid educational expansion in the West Bank and Gaza Strip from 1981 to 1991 to study how a swiftly increasing supply of skilled labor affects the wage premium for highly skilled labor. Interestingly, the study provides evidence that the rapid expansion of higher education lowered the wage premium of highly skilled labor from 1981 to 1988, thus showing that a rapid increase in the supply of skilled labor can decrease the returns to education.

In this paper, we use high-quality Danish register data and exploit a natural experiment in a part of the closed Danish labor market, where the supply of structural engineers spiked following the opening of a new university to examine supply effects. Here, we follow studies that have investigated whether and to what extent returns to college education vary by type (Grogger and Eide 1995; Brewer, Eide, and Ehrenberg 1999; Light and Strayer 2004). In the mid-1970s, a new university, Aalborg University (AAU), was founded in Denmark and offered predominantly engineering programs. As we later show, this event created a sudden exogenous increase in the supply of structural engineers in the mid-1980s. Accordingly, if supply effects are important for returns to education, recently graduated structural engineers should experience a drop in wages and employment rates because of the exogenous increase in the supply of structural engineers. Given that Aalborg University did not have a chemical engineering program at that time, we use chemical engineers as a control group. We find that the wages of structural engineers and, to some extent, their employment rates dropped in and around 1984, with the most marked effect in 1984, as the supply of structural engineers peaked. This finding provides evidence that a sudden exogenous increase in the supply of a particular skill group in high demand can negatively affect the returns to that particular education.

The case of engineers may be of particular interest as engineer's educations are part of science, technology, engineering, and mathematics (STEM) education. STEM is believed to play key part in competitiveness and future economic prosperity (Wendler et al. 2010; Klein, Rice, and Levy 2012). As such, STEM educations are believed to be in excess demand (Goldin and Katz 2008; Lacey and Wright 2009) and therefore not very sensitive to supply side effects. Our study makes two key contributions to the literature on returns to college education. First, we follow Angrist (1995) and show that wage is responsive to supply and that contextual factors thus matter to the return to education. Second, we add to the study of Angrist (1995) by showing that wages of educational groups, i.e., engineers, who are expected to be in high demand, c.f. Wilson (2009), are also susceptible to supply effects. In this respect, we add to the discussion of the importance of general equilibrium effects when estimating the returns to education (Heckman, Layne-Farrar, and Todd 1996).

In order to mitigate the potential quality of student bias, which might arise if lower-ability students self-select into the additional student places created by AAU's opening, we estimate individual fixed effects models as an addition to ordinary least squares (OLS) models. Fixed effects models control for time-invariant baseline differences between individuals, such as innate ability. The fixed effects models and the OLS models provide very similar results, and we are thus satisfied that the results could not be generated by differences in students' quality.

Moreover, to alleviate bias for the quality of education, which might arise if, for the time the immature AAU provided a substandard education, we estimated separate models solely for Technical University of Denmark (DTU) engineers following studies that have investigated whether quality matters to the returns to college education, that is, whether the returns of attending highly selective or elite colleges are larger than those of attending less selective colleges (Dale and Krueger 2002; Dale and Krueger 2014). Using only structural and chemical engineers from DTU, which arguably did not change its quality during the period of investigation, we still find evidence of a wage drop in and around 1984.

Finally, we investigate whether the wage drop is sensitive to heterogeneous effects of social background, possibly showing a social gradient. We find that the effect of the exogenous supply shock is relatively insensitive to social background, providing evidence that structural change in the supply of skilled labor affects candidates from all social backgrounds.

This paper proceeds as follows. Section II more formally describes the SUTVA in the context of the returns to a college degree. Section III outlines the background of the study and explains how we use the opening of AAU to conduct a natural experiment. Section IV discusses data and variables. Section V describes the empirical strategy, which allows us to identify the effect of the exogenous supply shock of structural engineers. Section VI presents the results. Section VII concludes.

Returns to a college degree and the SUTVA

When estimating the returns to education under the SUTVA, it is assumed that the potential wages and employment rates of individual *i* cannot depend on whether another individual *j* receives an education. As previously explained, this situation is incompatible with the standard earnings function that does not account for demand and supply effects because we expect that the wage individual *i* is able to command is dependent on the supply of graduates who are able to perform individual *i*'s profession. Consequently, if the market is temporally oversupplied with suitable candidates for individual *i*'s profession, we expect it to negatively affect the wage individual *i* is able to command. Thus, because the potential outcome for individual *i* is likely to be a function of the number of individuals receiving an education, the SUTVA effectively breaks down.

To illustrate this problem more formally, define a vector, **d**, which is an N × 1 vector of treatment indicator variables for *N*. Furthermore, define the potential outcomes of each individual as functions across all elements of vector **d**. Therefore, the outcome for individual *i* under treatment is $y_i^1(\mathbf{d})$, and the outcome under the control is $y_i^0(\mathbf{d})$. The causal effect of interest can then be defined as follows:

$$\delta_i(\mathbf{d}) = y_i^1(\mathbf{d}) - y_i^0(\mathbf{d})$$

The SUTVA is the assumption that $y_i^1(\mathbf{d}) = y_i^1$, and $y_i^0(\mathbf{d}) = y_i^0$. Consequently, the SUTVA breaks down if individual-level causal effects vary with the treatment assigned to other units, that is, if $y_i^1(\mathbf{d}) \neq y_i^1$, and $y_i^0(\mathbf{d}) \neq y_i^0$. In our case, in a sample of *n* individuals, we expect the returns to a particular college education to decrease as $|\mathbf{d}| \rightarrow$ n, that is, as a larger share of individuals choose that particular college education. In other words, as more individuals are assigned to a treatment, the effectiveness of the treatment (here: college engineering education) decreases, clearly violating SUTVA. Note that the above notation does not take into account any assignment ordering. If this ordering is dependent on a particular factor, e.g., ability, then the average treatment effect depends on the ordering at which individuals are sorted into the treatment.

In this case, SUTVA also depends on the particular ordering. To take account of ability-ordering effects, we employ a fixed effects regression below.

Background: The opening of Aalborg University as a natural experiment

Before the opening of Aalborg University, the only place to receive an engineering master degree was the Polytechnic University, which was founded in 1829 and is located in Denmark's capital, Copenhagen. Later, the Polytechnic University changed its name to the Technical University of Denmark (DTU), which is how it is known today. Initially, the school offered two types of engineering programs – Chemical Engineering and Mechanical Engineering. In 1857, Structural Engineering was added, and in 1903, Electro-technical Engineering was added, completing the list of the four traditional types of engineering. From 1829 to 1974, DTU was the only place to receive an engineering master degree in Denmark. Thus, "Master of Engineering" (cand.polyt.) was a generic term for candidates from DTU stipulated as a five-year program.

However, in 1957, the Danish Engineering Academy (DEA) was founded, and opened a department in Aalborg in 1966. DEA offered only undergraduate programs, or Bachelors of Engineering, which in Denmark are now known as "Diploma engineers"[1]. However, in 1974, AAU was founded with DEA as an important component, and beginning in 1974, AAU offered an MA program in engineering, thus becoming the first institution of higher education in Denmark apart from DTU to train "Masters of Engineering". The fact that there were now two institutions of higher education offering MAs in engineering resulted in an increase in the supply of engineers in the mid-1980s.

The first MAs in engineering from AAU graduated in the early 1980s. It was not until the mid-1980s that the supply of structural engineers momentarily boosted. This was due to a buildup of a student body delaying their enrollment awaiting the opening of the MA engineering program at AAU. In addition, the reason for the relative long time span from the opening of the MA program to the boost in number of candidates was because an MA in engineering in the 1970-1980s took on average about 1 - 3 more years of study than the formally stipulated five-year program.

AAU did not educate all types of engineers from the beginning; in fact, the university trained only structural engineers until the mid-1980s. In 1984, it began training other types of engineers as well, but importantly, it did not train chemical engineers before the mid-1990s.

[1] A "Diploma in Engineering" is a shorter, more practically oriented education than that of a Master of Engineering. Diploma engineers are composed of two types of practical engineers known in Denmark as Academy engineers (akademiingeniører) and Technikum engineers (Teknikumingeniører).

Therefore, we can construct a natural experiment. Because the opening of AAU significantly increased the supply of structural engineers and did not change the supply of chemical engineers, we assume that structural engineers are treated with the exogenous supply shock, while chemical engineers form a valid control group because they should be unaffected by a supply shock of structural engineers. Furthermore, we assume that, conditional on baseline differences between structural engineers and chemical engineers, as well as year-to-year fluctuations, the wage development of structural and chemical engineers would have shown similar patterns in the absence of an exogenous shock to the supply of structural engineers.

Although, as argued above, we can reasonably form a treatment and control group, a challenge to the design is that we do not control the exact timing of treatment. However, when we look at the supply of structural and chemical engineers in greater detail, we discover that the supply of structural engineers shows a spike in 1984. Figure 1 shows the supply of structural and chemical engineers from AAU and DTU from 1981 to 1991.

***** Figure 1 about here *****

From Figure 1, it is evident that the supply of structural engineers shows a spike in 1984, in particular candidates from AAU (highlighted in the figure), while the supply of chemical engineers goes in the other direction. What we observe is a momentary boost in 1984 resulting from a graduation wave of several cohorts of students that attended the new AAU in the late 1970's typically spending more than 5 years of study to obtain their MA degree. Accordingly, given that supply effects are important, we should see a marked difference between wages and employment rates of structural and chemical engineers around 1984.

The 1984 boost is naturally followed by a period of waiting time before a new wave of engineers enters the labor resulting in an additional spike in the supply of structural engineers in 1988-1989; however, around this year, the supply of chemical engineers also increased, which made this year a less clear-cut case for studying supply effects and, in addition, the demand for engineers is decreasing in that period[2].

[2] The demand for engineers is generally high, also in the years from 1979 to 1991 with some of the lowest unemployment rates among graduates. Official Statistics shows that the general rates of unemployment of engineers compared to other occupational groups lie between 2.3 and 4.2 percent in the period 1979-1988 (3.3 percent in 1984, and 2.6, 2.5 and 3.2 percent in the following three years) and rising to 4.8-7.3 in 1989-1991. This also applies when using the average degree of unemployment over the year (cf. Statistics Denmark 1985, 1990, 1992).

Data, variables and empirical strategy

For the analyses, we use high-quality register data from Denmark collected annually by Statistics Denmark. In Denmark, all residents have a unique personal identification number, which makes it possible to merge information from several different administrative registers. Information on wages and employment rates is obtained from employers and is normally used for administrative tax purposes. Information on education is collected from educational institutions. Accordingly, we are able to create a sample containing all MAs in structural and chemical engineering who received their degrees in the period from 1979 to 1991. However, in the regression analysis we choose to limit our sample to a six-year group (1979-1985) for two reasons. First, we believe that structural engineers at risk of being affected by the supply shock in the mid-1980s had to graduate before the supply shock occurred. Second, high-quality information on the labor market experience was not available in the registers until 1980. Failing to measure labor market experience accurately could severely bias the results because certain engineers from DTU will have more years of experience.

The impact of supply shock is evaluated by looking at wages and rates of employment in the 1981-1991 period. Since we use register data, we avoid measurement error due to self-reporting[3]. Moreover, full information on wages is available, i.e., wages are not top-coded, as is often the case with U.S. data. Furthermore, since the administrative registers contain information on everyone living in Denmark, we do not risk wage-selective attrition, which might arise in survey data.

Wages are measured as the yearly taxable wage, including fringe benefits and severance pay. If an individual had not graduated in the year of observation, the individual's wage is set to missing – even if he or she had an observable wage – in order to avoid bias from observing student job wages or similar wages that bias wage estimates downwards.

Rate of employment is measured as the inverted rate of unemployment per mille, i.e., the rate of employment $= 1000 - (\frac{U}{W})$ where U is net unemployment during the work year per mille excluding holidays, and W is the number of weeks in the work year excluding the number of holiday weeks in the relevant year. The variable takes on values from 0-1000 in each year.

[3] Although there is no measurement error due to self-reporting, measurement error in the graduation year cannot be ruled out entirely, as educational institutions report to Statistics Denmark.

Our basic OLS regression model controls for gender, labor market experience, and social background. The latter is measured by whether at least one parent has some college education or whether both have no college education. Labor market experience is approximated from information on pension payments and is measured per mille. We have divided this measure of labor market experience by 1,000 to avoid scaling problems in the models.

We use a limited set of controls because we also estimate individual fixed effects models that effectively sweep out all baseline observed and unobserved differences between individuals, including innate ability.

Table 1 shows descriptive statistics for the structural and chemical engineers. The differences between structural engineers and chemical engineers in terms of wage and employment rates are negligible. However, most of the differences are statistically significant due to the large sample sizes. The largest differences between the two groups are that a larger proportion of chemical engineers are female and have parent(s) with college education.

**** Table 1 about here ****

Empirical strategy

In order to empirically examine the effect of the sudden increase in the supply of structural engineers in and around 1984 on their relative wages and employment rates, we follow Angrist (1995) and use an augmented earnings function that allows the returns to education to depend on the timing of graduation. Since the supply of structural engineers peaked in and around 1984, this should cause their wage and employment rates to drop relative to those of chemical engineers in exactly this period.

In order to identify this effect, we assume that, except for baseline differences and year-to-year fluctuations, structural engineers' and chemical engineers' wages and employment rates would have developed in a similar pattern in the absence of changes in the supply of structural engineers. We find this assumption reasonable because AAU did not train chemical engineers during the period of study. Accordingly, the control group, chemical engineers, did not experience any unusual changes in supply during the period of study. If we further invoke the exogenous selection assumption, we can identify the effect of the increased supply shock of structural engineers with equation 1:

$$(1)\ \ln(y)_{it} = \tau_t + \beta \cdot STE_i + \pi_t \cdot \tau_t \cdot STE_i + \delta' \boldsymbol{X}_{it} + \varepsilon_i$$

where *i* indexes individuals and *t* indexes time; $Ln(y)_{it}$ is the natural log of wage standardized to 1991 DKK, and the log of employment rates, respectively. τ_t is a time fixed effect absorbing wage and employment rate variation, which is the result of year-to-year fluctuations; STE_i is an indicator equal to 1 if individual *i* is a structural engineer, and β is the corresponding coefficient. The interaction term $\tau_t \cdot STE_i$ captures wage deviations across time for structural engineers compared to chemical engineers, $\boldsymbol{X}_{it}$ is a vector of individual characteristics that affect wages and employment rates (labor market experience, gender and social background), and ε_{it} is an error term.

The coefficients of interest are the vector of coefficients, π_t, which identifies the time-varying effect of being a structural engineer. For t = 1984, π_t measures the impact of the increased supply of structural engineers in 1984 using chemical engineers as a control group. For t < 1985, π_t provides misspecification checks or potentially anticipatory effects. For t > 1985, π_t provides misspecification checks or potentially lagged effects.

One might argue, however, that the exogenous selection assumption is likely to be violated. This situation arises if engineers with higher unobserved ability choose to study at DTU because it is a mature institution with a good reputation, whereas engineers with low unobserved ability choose AAU because it is (was) a new and untested institution and therefore assumed to be "easier" to graduate from. In order to make the exogenous selection assumption more credible, we add individual fixed effects to equation 1:

$$(2)\ \ln(w)_{it} = \gamma_i + \tau_t + \pi_t \cdot \tau_t \cdot STE_i + \beta' \boldsymbol{X}_{it} + \varepsilon_i$$

where γ_i are individual fixed effects that absorb unobserved time-invariant differences between individuals, which affect wages and employment rates. The vector $\boldsymbol{X}_{it}$ is now composed only of labor market experience because gender and social background are time-invariant and hence included in γ_i. Note also that STE_i is also time-invariant and is consequently included only when interacting with time.

If we find that structural engineers earn smaller wages and have lower employment rates than chemical engineers around the supply shock, conditional on year-to-year fluctuations and individual characteristics, one might still argue that this result arises only because structural engineers from AAU received their diplomas from a new and untested institution without DTU's reputation. This is equivalent to arguing that the supply shock would affect only the wage and employment rates of structural engineers from AAU, not those of structural engineers from DTU. Therefore, we exclude engineers from AAU and re-estimate equation 2 only with engineers from DTU to check whether engineers from DTU remain unaffected by the increased supply shock.

Finally, we might expect that the effect of the supply shock will have heterogeneous effects and therefore a social gradient, so we expect that the supply shock of structural engineers will have a stronger negative impact on wages and employment rates of structural engineers with parents without a history of college education. To test this notion, we re-estimate equation 2 separately for individuals who have at least one parent with a college education.

As a robustness check we also conduct the analysis as a difference in difference analysis.

Results

Table 2 provides OLS regression estimates for equation 1, controlling for gender, parent(s) with some college education and labor market experience.

**** Table 2 here ****

Table 2 provides clear evidence of a marked decline in wages for structural engineers compared to chemical engineers in 1984. The estimated coefficient of the interaction term between the indicator of being a structural engineer and the year 1984 is -0.18 (0.05) in the baseline model and -0.19 (0.05) when control variables are added; both estimates are significant at the 1% level. Table 2 also provides evidence of smaller declines in wages in the years up to 1984, and the following year of 1985. This result suggests that the increased supply of structural engineers already began to impact the returns to education in 1982. However, the impact was strongest in 1984 as the supply of new candidates from AAU peaked, and lost its importance thereafter as the supply of structural engineers declined in both absolute and relative terms.

We then ask whether some groups are more or less affected by the sudden increase in the supply of engineers. In order to check for heterogeneous effects, we controlled for social background. We measure this dummy variable as parents with some college education, completed in either university colleges or universities, or no college education. The estimates (not shown, but part of the control variables) are small and insignificant (-0.01), so, apparently, the wage drop and the insignificant decrease in employment rate do not show a social gradient. We find that the effect of the exogenous supply shock is relatively insensitive to social background.

Individual fixed effects – eliminating quality of student bias

A source of bias in OLS estimation of equation 1 is the quality of students. If lower-ability students tried their luck in the newly opened AAU, the effects of the increased supply of structural engineers would be exaggerated because lower-ability students would have earned lower wages and suffered lower employment rates even in the absence of an exogenous supply shock. To eliminate this source of bias, we use individual fixed effects estimations, as in equation 2. Since fixed effects estimates are used only within individual variation, and thus control for all observed and unobserved, time-invariant baseline differences between individuals, fixed effects estimates will be consistent even in the presence of unobserved differences in quality of students. Table 3 provides results from the fixed effects estimation.

*** Table 3 here ****

The results are very similar to OLS estimates in Table 2, suggesting that the unobserved quality of students cannot explain the sudden decrease in wages observed for structural engineers in 1984. However, this may not be very surprising because we study a dynamic relationship across time that is likely unrelated to individual attributes. Many important individual attributes are time invariant and therefore also unrelated to time-varying factors, such as the relative supply of structural engineers.

Furthermore, the fixed effects estimates in Table 3 provide evidence of a decrease in employment rates around 1984 and 1985, although these negative effects are not large enough to be significant.

Rather than using time dummies, one could use the supply of structural engineers relative to that of chemical engineers as explanatory variables. We have also tried this combination both for OLS and fixed effects regressions. We find that the relative supply of structural engineers has the expected negative sign, and the effect is almost significant at the 5% level. However, it seems obvious that this approach is less efficient than using time dummies interacted with the supply of structural engineers, as in Tables 2 and 3. As mentioned above, we believe that the reason why time dummies are more efficient than the relative supply is that the relative supply shows too little variation over and above the supply "shock" in and around 1984. We have also tried to use the relative supply rather than time dummies (results are available upon request), finding similar results but a significantly worse fit.

As mentioned we have also formulated our model as a difference in difference estimation where we choose a treatment dummy for structural engineers and three period dummies equal to pre-treatment (1981-1983), treatment (1984) and post-treatment (1985-1991) periods. We parametrize the periods in this way, because we expect that the post treatment period is heterogeneous in terms of supply for both structural and chemical engineer's and because we believe supply side effects to be short lived.

Results are reported in Table A1 in the appendix. Results are a little more efficient compared to the results in Tables 2 and 3 but strikingly similar.

Individual fixed effects with DTU engineers only – eliminating quality of education bias

Although fixed effects estimation eliminates baseline differences in quality of students, which might bias the results, another source of bias is differences in the quality of education. An explanation for the results in Table 2 might be that AAU provided students with a lower-quality education, decreasing wages and employment. If this were the case, we would expect that only structural engineers from AAU would have been affected because they had a lower-quality education than that offered by DTU.

To eliminate this potential source of bias, we re-estimate the fixed effects excluding structural engineers from AAU. Table 4 provides results from fixed effects estimation with structural and chemical engineers from DTU.

**** Table 4 here ****

.

Although the coefficients of -0.13 and 0.14 in the controlled model are slightly smaller than previous estimates, these results document that structural engineers from DTU were not unaffected by the increased supply of structural engineers created by the opening of AAU.

Table 4 also shows positive estimates for employment rates in 1982 and 1983, indicating a demand for structural engineers.

Is the supply effect socially heterogeneous?

Again, we ask whether some candidates are exempted from the sudden increase in the supply of engineers, which could be the case if networks play a role or if some groups can better afford to wait for the right job. In other words, we are testing more for heterogeneous effects. To do so, we ran a model with another specification in which parental education is interacted with the two dependent variables, but we do not find a social gradient. Until 1984, the supply of structural engineers is increasing. Table 5 shows that structural engineers with parents without a college education experience a negative effect on their wages in 1982 (with a small fluctuation) and in 1984; likewise, structural engineers with parents with college education experience a negative effect in 1984 and 1985. We find that the negative effect of the increased supply of structural engineers appears for both groups in 1984 and then at staggering times; however, both groups are affected. All in all, there does not seem to be a social gradient. Later in time, the estimates are positive for both wages and employment for both groups.

**** Table 5 here ****

Conclusion

The primary contribution of this paper is that it provides evidence that, if a larger share of graduates from the same graduate program, the returns to that particular college degree will decrease, at least in the short run. We argue that a plausible explanation for this finding is that, if sufficiently large numbers of new engineers enters the labor market providing a saturation effect, even when in high demand, the supply of suitable candidates strongly increases, and, consequently, competition for jobs increases; this results in a lower price of labor within that skill group and, consequently, lower wages.

A more general interpretation of our findings is that individual-level estimates of returns to a particular college degree should be extrapolated with caution because interference between individuals on the labor market can cause returns to decrease as many more individuals earn the same degree. Here, we follow Angrist (1995, p. 1084), who uses a rapidly expanding education system in the West Bank and Gaza strip in 1981-1991 to argue "…*that contemporaneous schooling coefficients can be a poor indicator of the ultimate economic value of additional schooling when large numbers of new graduates enter the labor market*". We extend this finding by showing that returns to a particular college degree can also decrease as an increasing supply of candidates within that particular skill group enters the labor market. In that sense, we find a general equilibrium effect. Policymakers should be aware of this adverse effect of expanding certain fields of study; otherwise, they risk making misguided decisions based on individual-level estimates of returns, which are likely to change as a consequence of increasing student intake.

It should be noted that we study engineers who are generally in high demand. In other fields of study, decreasing returns might be even larger or their effects prolonged.

References

Altonji, Joseph G. and Thomas A. Dunn. 1996. "Using Siblings to Estimate the Effect of School Quality on Wages." *Review of Economics & Statistics* 78(4):665–71. doi:10.2307/2109953.

Angrist, Joshua D. 1995. "The Economic Returns to Schooling in the West Bank and Gaza Strip." *American Economic Review* 85(5):1065–87.

Angrist, Joshua D. and Alan B. Krueger. 1991. "Does Compulsory School Attendance Affect Schooling and Earnings?." *Quarterly Journal of Economics* 106(4):979–1014. doi:10.2307/2937954.

Ashenfelter, Orley and Alan Krueger. 1994. "Estimates of the Economic Return to Schooling from a New Sample of Twins." *Economics of Education Review* 84(5):1157–73.

Ashenfelter, Orley and Cecilia Rouse. 1998. "Income, Schooling, and Ability: Evidence from a New Sample of Identical Twins." *The Quarterly Journal of Economics* 113(1):253–84. doi:10.1162/003355398555577.

Becker, Gary S. 1962. "Investment in Human Capital: A Theoretical Analysis." *Journal of Political Economy* 70(5, Part 2):9–49. doi:10.1086/258724.

Becker, Gary S. 1964/1993. *Human Capital* (3. ed.). Chicago: The University of Chicago Press.

Behrman, Jere R., Mark R. Rosenzweig, and Paul Taubman. 1996. "College Choice and Wages: Estimates Using Data on Female Twins." *The Review of Economics & Statistics* 78(4):672–85. doi:10.2307/2109954.

Brewer, Dominic J., Eric R. Eide, and Ronald G. Ehrenberg. 1999. "Does It Pay to Attend an Elite Private College? Cross-Cohort Evidence on the Effects of College Type on Earnings." *The Journal of Human Resources* 34(1):104–23. doi:10.2307/146304.

Card, David. 1999. "The Causal Effect of Education on Earnings." *Handbook of Labor Economics* 3:1801–1863.

Card, David and Thomas Lemieux. 2001. "Can Falling Supply Explain the Rising Return to College for Younger Men? A Cohort-Based Analysis." *The Quarterly Journal of Economics* 116(2):705–46. doi:10.1162/00335530151144140.

Dale, Stacy Berg and Alan B. Krueger. 2002. "Estimating the Payoff to Attending a More Selective College: an Application of Selection on Observables and Unobservables." *The Quarterly Journal of Economics* 117(4):1491–527. doi:10.1162/003355302320935089.

Dale, Stacy Berg and Alan B. Krueger. 2014. "Estimating the Effects of College Characteristics over the Career Using Administrative Earnings Data." *Journal of Human Resources* 49(2):323–58. doi: 10.3368/jhr.49.2.323

Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review* 91(4):795-813. doi:10.1257/aer.91.4.795.

Fallon, Peter R. and P. Richard G. Layard. 1975. "Capital-Skill Complementarity, Income Distribution, and Output Accounting." *Journal of Political Economy* 83(2):279–302. doi:10.1086/260323.

Goldin, Claudia and Lawrence F. Katz. 2008. *The Race Between Education and Technology*. Cambridge. MA: Harvard University Press.

Grogger, Jeff and Eric Eide. 1995. "Changes in College Skills and the Rise in the College Wage Premium." *Journal of Human Resources* 30(2):280–310. doi:10.2307/146120.

Heckman, James, Anne Layne-Farrar, and Petra Todd. 1996. "Human Capital Pricing Equations with an Application to Estimating the Effect of Schooling Quality on Earnings." *The Review of Economics & Statistics* 78(4):562–610. doi:10.2307/2109948.

Heckman, James and Edward Vytlacil. 2001. "Identifying the Role of Cognitive Ability in Explaining the Level of and Change in the Return to Schooling." *Review of Economics & Statistics* 83(1):1–12. doi:10.1162/003465301750159993.

Hout, Michael. 2012. "Social and Economic Returns to College Education in the United States." *Annual Review of Sociology* 38(1):379–400. doi:10.1146/annurev.soc.012809.102503.

Imbens, Guido W. and Donald B. Rubin. 2015. *Causal Inference for Statistics, Social, and Biomedical Sciences*. New York: Cambridge University Press.

Johnson, George E. 1997. "Changes in Earnings Inequality: the Role of Demand Shifts." *Journal of Economic Perspectives* 11(2):41–54. doi:10.1257/jep.11.2.41.

Katz, Lawrence F. and David H. Autor. 1999. "Changes in the Wage Structure and Earnings Inequality." *Handbook of Labor Economics* 3:1463–1555.

Katz, Lawrence F. and Kevin M. Murphy. 1992. "Changes in Relative Wages, 1963-1987: Supply and Demand Factors." *The Quarterly Journal of Economics* 107(1):35–78. doi:10.2307/2118323.

Klein, Joel I., Condoleezza Rice, and Julia Levy. 2012. "U.S. Education Reform and National Security." *Council on foreign Relations*. Independent Task Force Report No. 68.

Lacey, Alan T. and Benjamin Wright. 2009. "Occupational employment projections to 2018." *Monthly Labor Review* 132(11):82-123.

Light, Audrey and Wayne Strayer. 2004. "Who Receives the College Wage Premium? Assessing the Labor Market Returns to Degrees and College Transfer Patterns." *Journal of Human Resources* 39(3):746–73. doi:10.2307/3558995.

Mincer, Jacob. 1958. "Investment in Human Capital and Personal Income Distribution." *Journal of Political Economy* 66(4):281–302. doi:10.1086/258055.

Mincer, Jacob. 1962. "On-the-Job Training: Costs, Returns, and Some Implications." *Journal of Political Economy* 70(5):50–79. doi:10.1086/258725.

Mincer, Jacob. 1974. *Schooling, Experience, and Earnings*. New York: Columbia University Press.

Morgan, Stephen L. and Christopher Winship. 2015. *Counterfactuals and Causal Inference: Methods and Principles for Social Research*. Cambridge, MA: Cambridge University Press.

Rouse, Cecilia Elena. 1999. "Further Estimates of the Economic Return to Schooling From a New Sample of Twins." *Economics of Education Review* 18(2):149–57. doi:10.1016/S0272-7757(98)00038-7.

Rubin, Donald B. 1980. "Discussion of 'Randomization Analysis of Experimental Data in the Fisher randomization Test' by Basu." *Journal of the American Statistical Association* 75(371):591–3. doi:10.1080/01621459.1980.10477517.

Rubin, Donald B. 1990. "Comment: Neyman (1923) and Causal Inference in Experiments and Observational Studies." *Statistical Science* 5(4):472–80.

Statistics Denmark 1985. *Statistical Yearbook*. Copenhagen: 142-143.

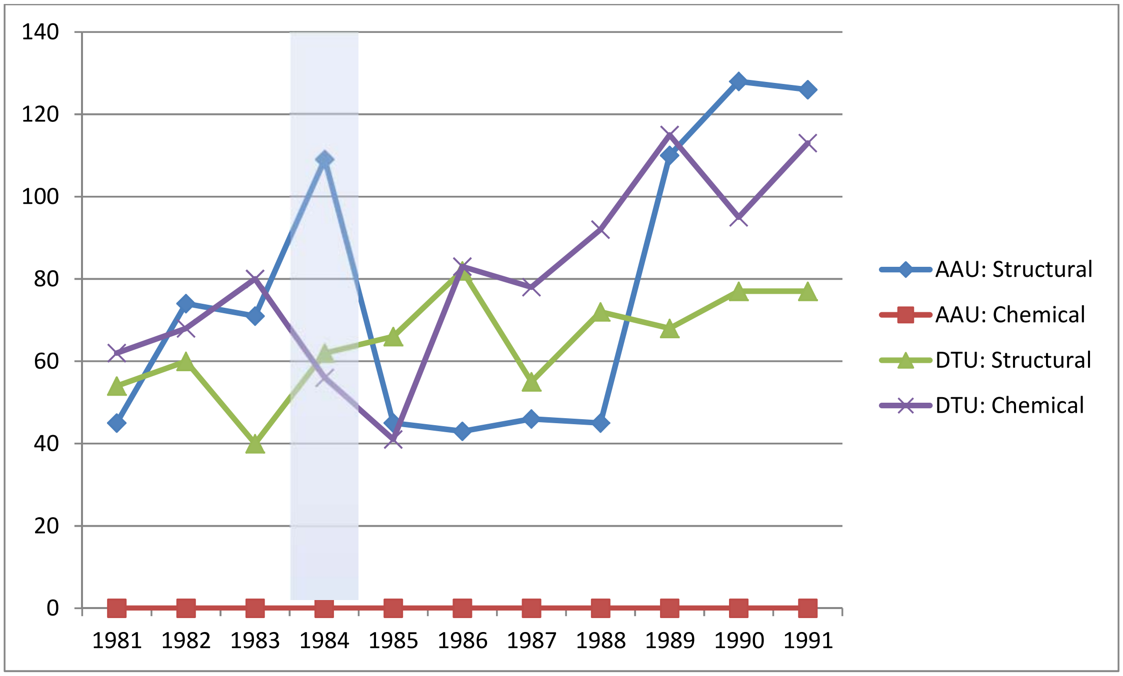
Statistics Denmark 1990. *Statistical ten-year review 1990*. Copenhagen: 47.

Statistics Denmark 1992. *Statistical ten-year review 1992*. Copenhagen: 48.

Topel, Robert H. 1997. "Factor Proportions and Relative Wages: The Supply-Side Determinants of Wage Inequality." *Journal of Economic Perspectives* 11(2):55–74. doi:10.1257/jep.11.2.55.

Wendler et al. 2010. *The path forward: The future of graduate education in the United States.* Princeton. NJ: Educational Testing Service

Figure 1: The supply of structural and chemical engineers in each year from AAU and DTU. Period 1981-1991.



Note: Years of observations are represented by the x-axis and the number of graduated engineers is represented by the y-axis. The period of 1981-1991 is the observation period we are looking at wage and job effects.

Table 1: Descriptive statistics.

	Structural engineers (treatment group)			Chemical engineers (control group)			Difference in means (t-test)
	OBS	Mean	SD	OBS	Mean	SD	
Wage	5726	284782.30	105546	3219	290466	109819.70	*
Employment rate	5875	965.78	113.91	3263	972.66	107.92	**
AAU (versus DTU)	5886	0.54	-	3266	-	-	**
Female	5886	0.08	-	3266	0.20	-	**
Labor market experience	5886	4.54	2.84	3266	4.94	2.94	**
Parent(s) with college	5886	0.38	-	3266	0.51	-	**

Note: *, ** indicate statistical significance at the 5% and 1% levels, respectively.

Table 2: Initial results from OLS. Structural and chemical engineers. Period 1981-1991.

Dependent variable:	LN(wage)		LN(rate of employment [RoE])	
Structural engineer × 1981	0.05	0.01	-0.00	-0.01
	(0.11)	(0.11)	(0.04)	(0.04)
Structural engineer × 1982	-0.11	-0.15	-0.01	-0.01
	(0.09)	(0.08)	(0.04)	(0.04)
Structural engineer × 1983	-0.03	-0.07	-0.00	-0.01
	(0.07)	(0.07)	(0.03)	(0.03)
Structural engineer × 1984	-0.18**	-0.19**	-0.01	-0.01
	(0.05)	(0.05)	(0.03)	(0.03)
Structural engineer × 1985	-0.04	-0.03	-0.01	-0.01
	(0.05)	(0.05)	(0.02)	(0.02)
Structural engineer × 1986	0.02	0.03	0.02	0.03
	(0.03)	(0.03)	(0.02)	(0.02)
Structural engineer × 1987	0.04	0.04	0.02	0.02
	(0.03)	(0.04)	(0.02)	(0.02)
Structural engineer × 1988	0.02	0.02	0.02	0.02
	(0.03)	(0.03)	(0.02)	(0.02)
Structural engineer × 1989	0.05	0.04	0.01	0.01
	(0.03)	(0.03)	(0.01)	(0.01)
Structural engineer × 1990	0.05	0.04	-0.01	-0.01
	(0.04)	(0.04)	(0.02)	(0.02)
Year fixed effects	YES	YES	YES	YES
Control variables	NO	YES	NO	YES
Observations	8945	8945	9138	9138

Note: Control variables include education institution, gender, parent(s) with some college education, and labor market experience. Standard errors clustered at the individual level are displayed in parentheses. *, ** indicate statistical significance at the 5% and 1% levels, respectively. 1991 is the baseline year.

Table 3: Individual fixed effects regressions. Structural and chemical engineers. Period 1981-1991.

Dependent variable:	LN(wage)		LN(RoE)	
Structural engineer × 1981	-0.00	0.01	-0.01	-0.00
	(0.11)	(0.11)	(0.03)	(0.03)
Structural engineer × 1982	-0.15	-0.16	0.00	-0.00
	(0.08)	(0.08)	(0.03)	(0.03)
Structural engineer × 1983	-0.08	-0.08	0.00	0.00
	(0.06)	(0.06)	(0.03)	(0.03)
Structural engineer × 1984	-0.20**	-0.20**	-0.01	-0.01
	(0.05)	(0.05)	(0.03)	(0.03)
Structural engineer × 1985	-0.05	-0.05	-0.02	-0.02
	(0.04)	(0.04)	(0.02)	(0.02)
Structural engineer × 1986	0.03	0.03	0.02	0.02
	(0.03)	(0.03)	(0.02)	(0.02)
Structural engineer × 1987	0.03	0.03	0.02	0.02
	(0.03)	(0.03)	(0.02)	(0.02)
Structural engineer × 1988	0.04	0.04	0.02	0.02
	(0.03)	(0.03)	(0.02)	(0.02)
Structural engineer × 1989	0.05	0.05	0.01	0.01
	(0.03)	(0.03)	(0.01)	(0.01)
Structural engineer × 1990	0.04	0.04	-0.01	-0.01
	(0.04)	(0.04)	(0.02)	(0.02)
Year fixed effects	YES	YES	YES	YES
Control variables	NO	YES	NO	YES
Observations	8945	8945	9138	9138

Note: Control variables include labor market experience. Standard errors clustered at the individual level are displayed in parentheses. *, ** indicate statistical significance at the 5% and 1% levels, respectively. 1991 is the baseline year.

Table 4: Fixed effects regressions. Structural and chemical engineers from DTU. Period 1981-1991.

Dependent variable:	LN(wage)		LN(RoE)	
Structural engineer × 1981	0.13	0.13	0.05	0.05
	(0.12)	(0.12)	(0.03)	(0.03)
Structural engineer × 1982	0.00	-0.00	0.08*	0.08*
	(0.09)	(0.09)	(0.03)	(0.03)
Structural engineer × 1983	0.05	0.04	0.06*	0.06*
	(0.07)	(0.07)	(0.03)	(0.03)
Structural engineer × 1984	-0.13*	-0.14**	0.02	0.02
	(0.05)	(0.05)	(0.03)	(0.03)
Structural engineer × 1985	-0.10	-0.10	-0.01	-0.01
	(0.05)	(0.05)	(0.03)	(0.03)
Structural engineer × 1986	0.01	0.01	0.03	0.03
	(0.03)	(0.03)	(0.02)	(0.02)
Structural engineer × 1987	0.01	0.00	0.02	0.02
	(0.04)	(0.04)	(0.02)	(0.02)
Structural engineer × 1988	0.02	0.02	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)
Structural engineer × 1989	0.04	0.03	0.02	0.02
	(0.03)	(0.03)	(0.02)	(0.02)
Structural engineer × 1990	0.04	0.03	-0.01	-0.02
	(0.04)	(0.04)	(0.02)	(0.02)
Year fixed effects	YES	YES	YES	YES
Controls	NO	YES	NO	YES
Observations	5875	5875	5974	5974

Note: Control variables include: labor market experience. Standard errors clustered at the individual level are displayed in parenthesis. *, ** indicate statistical significance at the 5% and 1% levels, respectively. 1991 is the baseline year.

Table 5: Fixed-effects regressions. Structural and chemical engineers from DTU and AAU. Separate models according to social background. Period 1981-1991.

	Parent(s) without college				Parent(s) with college			
	Ln(wage)	Ln(wage)	Ln(RoE)	Ln(RoE)	Ln(wage)	Ln(wage)	Ln(RoE)	Ln(RoE)
Structural engineer × 1981	0.01	0.01	0.04	0.05	0.03	0.02	-0.05	-0.05
	(0.18)	(0.18)	(0.05)	(0.05)	(0.11)	(0.11)	(0.04)	(0.04)
Structural engineer × 1982	-0.26*	-0.26*	-0.03	-0.03	0.01	0.01	0.04	0.04
	(0.10)	(0.10)	(0.04)	(0.04)	(0.13)	(0.12)	(0.05)	(0.05)
Structural engineer × 1983	-0.08	-0.08	-0.01	-0.01	-0.09	-0.09	0.02	0.02
	(0.08)	(0.08)	(0.03)	(0.03)	(0.10)	(0.10)	(0.04)	(0.04)
Structural engineer × 1984	-0.21***	-0.21***	-0.03	-0.03	-0.18*	-0.17*	0.00	0.00
	(0.06)	(0.06)	(0.02)	(0.02)	(0.07)	(0.07)	(0.05)	(0.05)
Structural engineer × 1985	0.05	0.05	0.02	0.02	-0.19**	-0.18**	-0.06	-0.06
	(0.06)	(0.06)	(0.02)	(0.02)	(0.07)	(0.07)	(0.04)	(0.04)
Structural engineer × 1986	0.06	0.06	0.04*	0.04*	-0.01	0.00	-0.00	-0.00
	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)
Structural engineer × 1987	0.07	0.07	0.03	0.03	-0.01	-0.00	0.01	0.01
	(0.04)	(0.04)	(0.01)	(0.01)	(0.05)	(0.05)	(0.03)	(0.03)
Structural engineer × 1988	0.05	0.05	0.03	0.03	0.02	0.01	-0.00	-0.00
	(0.03)	(0.03)	(0.02)	(0.02)	(0.05)	(0.05)	(0.04)	(0.04)
Structural engineer × 1989	0.04	0.03	0.03	0.02	0.06	0.06	-0.01	-0.01
	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Structural engineer × 1990	-0.00	-0.00	-0.01	-0.01	0.08	0.08	-0.01	-0.01
	(0.03)	(0.03)	(0.02)	(0.02)	(0.06)	(0.06)	(0.02)	(0.02)
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Control variables	NO	YES	NO	YES	NO	YES	NO	YES
N	5138	5138	5246	5246	3807	3807	3892	3892

Note: Control variables include labor market experience. Standard errors clustered at the individual level are displayed in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001 (two-tailed tests). 1991 is the baseline year.

Appendix. Difference in difference analysis.

Table A1. Difference in difference analysis.

Dependent variable:	LN(wage)		LN(RoE)	
Treatment (1984)	0.38**	0.27**	0.03	0.02
	(0.04)	(0.04)	(0.02)	(0.02)
Post (1985-1991)	0.80**	0.42**	0.09**	0.07**
	(0.03)	(0.03)	(0.01)	(0.01)
Structural × Treatment (1984)	-0.20**	-0.20**	-0.01	-0.01
	(0.04)	(0.04)	(0.03)	(0.03)
Control variables	NO	YES	NO	YES
Observations	8945	8945	9138	9138

Note: Control variables include labor market experience. Standard errors clustered at the individual level are displayed in parentheses. *, ** indicate statistical significance at the 5% and 1% levels, respectively. Pretreatment (1981-1983) is the baseline period.