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The effect of increased general education in vocational schools: Evidence from a Hungarian vocational school reform

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**BUDAPEST WORKING PAPERS
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JORIS GHYSELS, ZOLTÁN HERMANN, IRYNA RUD AND MELLINE SOMERS

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Abstract

This paper aims at the evaluation of the reform of vocational education introduced in 1998 in Hungary. The reform extended the duration of education by one year, and increased teaching time spent on non-vocational subjects. The reform affected two of the three tracks in upper-secondary education in Hungary, vocational secondary school and vocational school. We estimate the effect of the reform on educational attainment, employment and wages in a comparative interrupted time series (CITS) framework, using the academic track and secondary school drop-outs as control groups. The results suggest that the reform has had heterogeneous effects. First, we detect no effect for the vocational secondary track, while the reform has improved labour market outcomes of vocational school students. Second, in the vocational school group the reform has increased men's wages, while not affected their employment. For women we found a positive employment effect, while wages have increased only for the younger cohorts.

Keywords: vocational education, reform, employment, wages, skills

JEL: J08, J01, D00

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A közismereti oktatás kiterjesztésének hatása a szakiskolai képzésben – Egy magyarországi szakképzési reform tanulságai

Joris Ghysels, Hermann Zoltán, Iryna Rud és Meline Somers

Absztrakt

A tanulmány az 1998-as magyarországi szakképzési reform hatását értékeli. A reform egy évvel növelte a képzés időtartamát és növekedett a közismereti képzés óraszámja is. A reform a három középiskolai programtípus közül kettőt érintett: a szakiskolai és szakközépiskolai képzést. Három kimenetet vizsgálunk: az iskolai végzettséget, a foglalkoztatást és a béreket. Az elemzés módszere a összehasonlított megszakított idősorok elemzése. Kontrollcsoportként az általános iskolai és gimnáziumi végzettségűeket használjuk. Az eredmények azt mutatják, hogy a reform hatása heterogén. Egyrészt a szakközépiskolai végzettségűek esetében nincs hatás, míg a szakiskolai végzettségűekre pozitív hatások mutathatók ki. Másrészt a reform nyomán nőtt a szakmunkás végzettségű férfiak keresete, miközben a foglalkoztatottságuk nem változott. A nők esetében a foglalkoztatottság emelkedett, míg a bérek csak a legfiatalabb kohorszok esetében nőttek.

Kulscszavak: szakképzés, reform, foglalkoztatás, kereset, képesség

JEL: J08, J01, D00

1. INTRODUCTION

The economic returns to investments in education reflect whether governments and individuals are investing optimally. Consequently, economists have dedicated much attention to identifying the causal impact of schooling on earnings. Most of this work is built on the idea that the acquisition of useful skills and knowledge can be treated as a form of (human) capital investment (Becker, 1962; Mincer, 1958; Schultz, 1961). The literature is replete with studies that estimate this return using the simple Mincer specification. In this empirical approximation of the human capital theoretical framework, the estimated return is obtained as the coefficient on years of schooling variable in a log wage equation that accounts for work experience and other individual characteristics that are assumed to affect earnings. This simple specification yields a return of 10 percent in the original study of Mincer (1974), and Psacharopoulos and Layard (1979) report estimates of a similar magnitude. However, to answer counterfactual questions of the sort ‘What would certain individual’s (expected) age earnings profile be if he/she chose e_2 rather than e_1 years of education?’, the Mincer specification is not particularly suitable. In the simple Mincer specification, the acquisition of schooling is treated as exogenously determined. However, the estimates obtained through the Mincer specification do not disentangle the contribution of individuals’ ability and socioeconomic background to the return to education.

This paper exploits a reform in the Hungarian vocational education system that was introduced in the 1998/99 academic year to estimate the causal impact of the acquisition of additional years of schooling on graduates’ earnings. Before the introduction of the reform, students entered vocational education in grade 9 and received three years of practical training. After the reform, students in vocational education only received general education in the first two years and practical training was organised in the subsequent two grades. Hence, the amount of schooling that students receive and the amount of time spent with general education has increased substantially after the reform. The introduction of this reform generates a potentially exogenous source of variation in the acquired years of schooling that can be used to estimate the returns to an additional year of schooling for vocational education graduates. This reform provides a natural experiment that allows us to compare individuals who are equal in expectations, but only differ in the amount of schooling they have received. Concurrently, the reform brought about a change in the content of vocational education, enlarging the amount of general schooling, skills that have a higher degree of transferability than specific practical skills. Thus, the effect we will observe derive from two sources: an extension of the learning period (more education) and a substantive change (more general skills).

Empirically, we use data from the Hungarian national wage register to estimate the effect on wages. Furthermore we look into two building blocks of the wage outcome: the distribution of educational credentials and employment. For the latter analyses, we rely on the Hungarian labour force survey (LFS). We use a CITS approach to identify the effect of the reform.

The remainder of this paper is organized as follows. The next section provides a brief discussion of related literature on the relation between increased education and earnings. In Section 3 we provide a detailed description of the Hungarian education system and the 1998 reform. Section 4 describes the data and the main variables of interest. Section 5 discusses the estimation procedure and Section 6 presents the results of the empirical analyses of educational outcomes, employment and wages. Section 7 concludes.

2. LITERATURE

We briefly go into three aspects of the literature that are relevant to our paper and to which we provide contributions: reliable estimates of returns to education, heterogeneity of these effects and the importance of general skills.

2.1 RELIABLE ESTIMATES OF RETURNS TO EDUCATION

Various approaches have appeared in the literature to correct for ability bias. First, numerous studies have directly included ability measures such as IQ into the Mincer specification (Chamberlain & Griliches, 1975; Griliches, 1977). Studies that try to correct for ability bias by including IQ scores in their models obtain estimates of the returns to schooling varying from three (Griliches, 1977) to nine percent (Chamberlain & Griliches, 1975). One of the methodological downsides of this approach is that IQ scores can be subject to measurement errors. Moreover, ability measures need to be uncontaminated by the ability enhancing effects of education (Harmon, Oosterbeek & Walker, 2003). Otherwise, the ability measure will not be able to isolate the effect of individuals' innate ability that caused them to sort into specific types of education in the first place. A second stream of literature estimates the return to schooling using identical twins and finds estimates between around five and eleven percent (Ashenfelter & Rouse, 1998; Ashenfelter & Zimmermann, 1997; Bonjour et al., 2000; Isacson, 1999; Miller, Mulvey & Martin, 1995; Rouse, 1999). This approach is based on the belief that twins are more identical than a randomly selected pair of individuals. However, the within-family approach may not yield less biased estimates as ability has an individual as well as a family component which is not independent of the schooling students receive. Moreover, twins might be rationally more inclined to differ from one another in their

educational choices (Bound and Solon 1999; Sacerdote 2010). With the exception of Bonjour et al. (2000), little attention is dedicated to what determines that twins differ in the amount of schooling they acquire.

A more recent body of research exploits changes in the compulsory schooling age as a source of exogenous variation in the amount of schooling that students receive (Acemoglu & Angrist, 2000; Angrist & Krueger, 1991; Harmon & Walker, 1995; Oosterbeek & Webbink, 2007; Oreopoulos, 2006, 2007; Pischke & Von Wachter, 2005; Staiger & Stock, 1997). Angrist and Krueger (1991) indicate that individuals born in the beginning of the year start school at a slightly older age as students are required to have turned six by January 1 of the year. Individuals born at the beginning of the year are therefore eligible to drop out of school after attending less years of schooling than individuals born near the end of the year. When the individual's quarter of birth is used as an instrument for educational attainment, an estimate of the return to education of around 7.5% is obtained (Angrist & Krueger, 1991). Other studies that use a similar approach confirm these findings (Acemoglu & Angrist, 2000; Harmon & Walker, 1995; Oreopoulos, 2006). Albeit instrumental variables have been used, most frequently to estimate the causal impact of education on individual's age earnings profile, the methodological flaws of this approach have been the subject of debate recently in the literature. Heckman and Urzua (2009) point out a number of potential problems associated with estimates that are obtained in an instrumental variables (IV) framework. They indicate that weak instruments can give biased estimates and that IV estimates rest on strong, a priori data assumptions. Moreover, different instruments will yield different estimates. We contribute to this part of the literature with estimates that derive of a natural experiment, a policy change in Hungary.

2.2 HETEROGENEOUS RETURNS: VOCATIONAL EDUCATION

Second, studies are largely focused on the concept of a single return to education. However, the returns to education can differ across sub-populations (Card, 1999). We contribute to the literature by focussing on graduates from vocational education. We examine changes in the amount of schooling that take place within one educational track, whereas other studies focus on the returns that are obtained when acquiring higher levels of education. Our study compares graduates who both attended vocational education but received different amounts of schooling within vocational education. Hence, our study potentially sheds light on whether employers consider students to have acquired additional relevant skills or whether their degree simply reflects their ability.

2.3 THE IMPORTANCE OF GENERAL EDUCATION

Although vocationally educated graduates generally experience a smoother transition to the labour market (Ryan, 2001; Van der Velden & Wolbers, 2003; Levels, Van der Velden & Di Stasio, 2014), their advantage in terms of employment appears to reverse later in life (Hanushek et al., 2016). Graduates who have mainly acquired occupation specific skills appear to be less adaptive as a result of technological and structural changes in the economy. As occupation specific skills can become obsolete over time, general skills allow individuals to remain employable as these skills can be applied to a wider variety of occupations.

The role of general skills across the life time, however, varies across the different types of vocational education (Van der Velden, Buisman & Levels, 2017). Van der Velden et al. (2017) find evidence that the largest life-time earnings are incurred by vocational education graduates who have attended a school-based system. In school-based systems, the development of occupation specific skills and general skills occur simultaneously. Provided that the Hungarian reform resulted in a vocational education system that allowed students to acquire general skills as well as practical training, the reform is likely to have triggered a positive impact on graduates' wages.

3. THE HUNGARIAN SYSTEM OF EDUCATION AND THE REFORM IN 1998

3.1 THE SYSTEM OF EDUCATION IN HUNGARY (BEFORE 2010)

This section provides a description of the Hungarian education system before and after the 1998 reform. Because the Hungarian system of education has experienced new substantial changes in 2010, we have limited our description to the educational system before 2010.

Compulsory education - Schooling in Hungary is compulsory from age 6 to 18 for those who began their studies in 1998 or later. A child who reaches the level of development required for schooling becomes liable for compulsory education when he or she reaches the age of six years between the beginning of the calendar year and May 31.¹ Students who began their primary school studies before 1 September 1998 were obliged to stay in school before they turn 16 (see the Act LXXIX of 1993 on Public Education).² Compulsory education (pre-primary, primary and secondary levels) is publicly founded. The pre-school (kindergarten) education is followed by an 8-year long primary school education. After the completion of the primary school pupils can continue their education in grammar schools which offer general

¹ <http://www2.ohchr.org/english/bodies/cescr/docs/E.C.12.HUN.3-Annex11.pdf>

² https://www.oktatas.hu/pub_bin/dload/nyelvvizsga_honositas/elismertetes_honositas/kozokttv_angol.pdf

education or in vocational education track, choosing between secondary vocational schools, vocational schools or special vocational programs.

General secondary grammar school (gymnasium) takes 4 years (5 years in bilingual schools) and provides students with theoretical qualifications. It prepares students to access higher education. Graduates of the grammar school can also continue their study in post-secondary vocational education, but this occurs not very often.

Secondary vocational school also last the same period of time as the general secondary track and provides students with theoretical and practical qualifications. The graduates of this track can choose to obtain a vocational qualification (takes 1-3 years) and to enter the higher education. At both the general secondary track and secondary vocational school students are assessed by the maturity exam, leading to a secondary school leaving certificate.

Vocational school provides students with general skills and knowledge in the first two years. After the first two years and in order to obtain a vocational qualification students track have to follow one to three years of VET. Graduates can enter the labour market. This track does not lead to entering tertiary education, however graduates can enter higher education after obtaining additional qualifications (this, however, occurs very rarely).

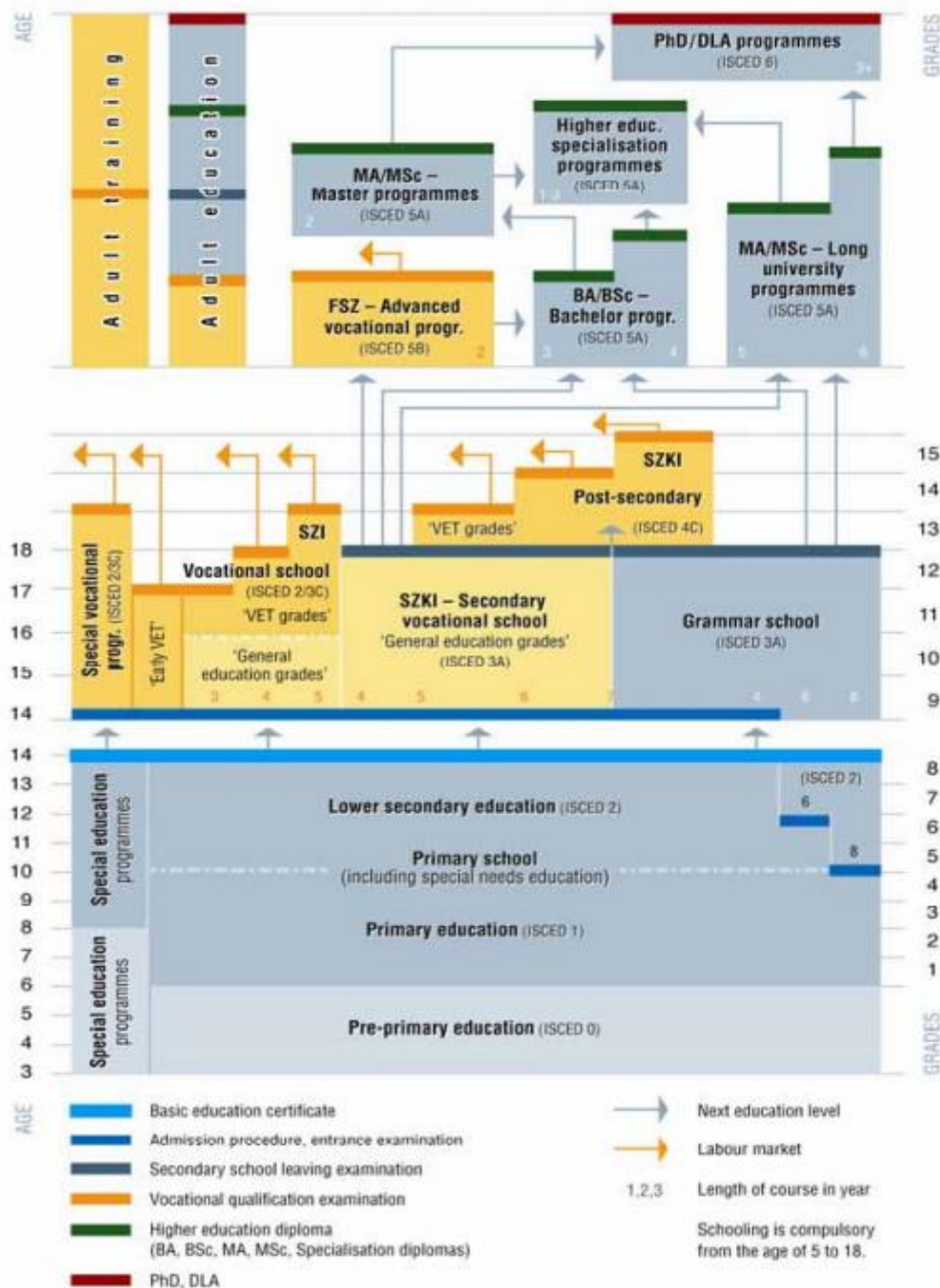
Special vocational programs train students with special needs. They provide students with basic working skill.

Advanced vocational programmes (felsőfokú szakképzés, FSZ) prepare for high quality professional work and at the same time, through transferability of credits, they help transition from VET to tertiary level education. They appear to be closely related to the VET system, but in practice it is rather part of higher education. The share of students in advanced vocational programmes is relatively low and most of them regard this as a stepping stone to higher education: typically these programmes are chosen by those who were not admitted to higher education but apply to higher education next year again.

Tertiary education in Hungary offers a variety of programmes: Bachelor, Master and single-cycle long programmes, doctoral programmes and post-graduate specialisation courses. Tertiary education can be either full time or part time.

Figure 1

The system of education in Hungary, see also <http://www.unevoc.unesco.org/go.php?q=Hungary>



Source: Adapted from Ministry of National Resources.

3.2 THE 1998 EDUCATIONAL REFORM

The division of the VET tracks was established in 1990s.³ Some schools have already launched some structural changes in VET from the early 90s. In the academic year 1998/1999 the reform was introduced in every VET school, which was aimed to change the structure, content and outcomes of VET at upper secondary level.

Changes in details

The reform has affected both vocational school and the vocational secondary tracks, but the changes were different. Before the reform in 1998 the total length of practical vocational training in vocational education was three years. After the reform practical training in the first years was substituted by general education (the so-called “general education grades”): four years of general education in secondary vocational school (SZKI) and two years in vocational school (grade 9 and 10 in SZI). Practical training in vocational schools is organised in the following one or two grades (the so-called “VET grades”) and it leads to obtaining a vocational qualification. Therefore, the reform 1998 signifies the change from practical vocational education to the vocational education with certain general education background. Moreover, the share of student population in vocational schools and secondary vocational school has been decreasing substantially after the reform. It is important to mention that the dropout level in vocational education (especially in SZI) has been remaining high.

The reform in 1998 has also introduced higher (advanced) level of VET (*felsőfokú szakképzés*, FSZ). This level is organized by higher education institutions, sometimes in cooperation with ‘secondary vocational schools’. Credits obtained in higher level of VET can be transferred to a bachelor programme in the same field. Although the number of participants in this level of education has risen continuously since it was introduced in 1998, the labour market has been not particularly enthusiastic towards.

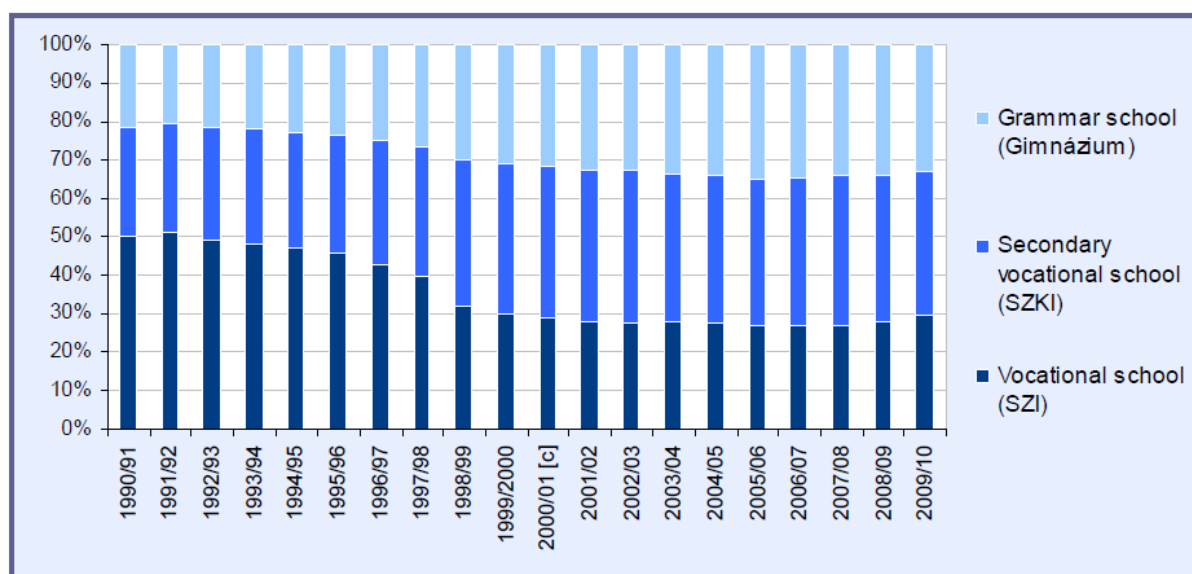
Another important moment aspect the reform 1998-1999 is the changes in curricula in secondary school with accordance to the new regulations of the national core curriculum (*nemzeti alaptanterv*) and recommendations of framework curricula issued by the Minister for Education.⁴ The new curriculum has introduced a number of new subjects (e.g. IT, career orientation) while the teaching hours for some subjects have also changed (e.g. history, languages).

³ The exception is “early VET programmes” which were established in 2010.

⁴ See for more details <http://ofi.hu/nemzeti-alaptanterv-bevezetese-az-iskolak-helyi-tanterve-1998-oszen-szemelvenyek-egy-kutatasi>

Figure 2

Distribution of students in year 9 by school type 1990-2010, see <http://www.unevoc.unesco.org/go.php?q=Hungary>



In sum, the reform extended the years of education in vocational school and vocational secondary school and changed the composition of the education programmes, increasing the part dedicated to general skills.

4. METHOD

4.1 EXPECTED EFFECTS OF THE REFORM AND IDENTIFICATION

As described in section 3, the Hungarian government reformed vocational education (vocational school and vocational secondary) to increase the skill level of the Hungarian labour force. Both the content and the duration of these types of education were changed, introducing more general education and extending the schooling by one year.

In a *ceteris paribus* framework, both aspects of the reform are expected to reinforce the skills of the vocationally trained, improving their productivity at work and, hence, their wages, the latter being a reflection of the marginal productivity of labour.

However, it is likely that the *ceteris paribus* framework will not hold, for a variety of reasons: selection into education, structural changes in the economy and temporary shocks.

With selection into education we mean that students may choose different educational paths because of the reform. Assuming educational choices to follow a human capital investment logic, the choice made reflects the optimum of a lifetime income maximization process balancing future labour income against foregone earnings when studying. If so, rising

wages for vocational education may motivate students who would before the reform not continue into vocational education (stop at lower secondary education, i.e. become a drop-out before the end of compulsory education) after the reform to continue and obtain a diploma of vocational education (most likely vocational school). Similarly, those who would stop at vocational school before the reform, may opt for vocational secondary after the reform. Theoretically, the opposite may also be true, because the cost of foregone earnings in one additional year of schooling is not necessarily higher than the lifetime benefit of obtaining the diploma. In any case, the sorting of abilities into educational credentials may change because of the reform, which may have an influence on wages apart from the direct influence of the reform on skill formation.

Structural changes in the economy reflect the fact that it is a priori unclear whether the changing economy will lead to higher or lower demand for vocationally educated. In general it is believed that the globalisation of the Hungarian economy (more trade) will lead to more demand for general education. This is an underlying reason for the reform which it reflects enhancing general education within vocational education. Yet, it is unlikely that the reform leads to a synchronised change in the supply and demand of generally educated. Hence, the changing economy may induce wage pressure because of an imbalance of supply and demand, rather than the content of the reform.

Finally also temporary shocks (business cycle) may affect the wage generation process. It is, for instance, clear that the 2008 crisis has hit the Hungarian economy quite strongly, depressing the previously high rates of economic growth.

To identify the effect of the reform, we develop an estimation procedure that accounts for these potentially intervening elements.

4.2 ESTIMATION PROCEDURE

For the estimation, we follow a comparative interrupted time series (CITS) approach, as in Dee and Jacob (2011). The basic equation is:

$$y_{it} = \beta_1 + \beta_2 year + \beta_3 experience + \beta_4 reformcohort + \beta_5 timesince + \beta_6 vocational + \beta_7 vocational * reformcohort + \beta_8 vocational * timesince + \beta_n X_{nit} + \varepsilon_{it} \quad (1)$$

Where

Year	Year of observation
Experience	Years of assumed experience in the labour market, i.e. time passed since the year that the last educational qualification has been obtained (or would have been obtained if the educational career happened without delays?)
Reformcohort	Dummy, 1 = belonging to a cohort that is affected by the reform. = Intercept change, average effect for all cohorts of this group
Timesince	Number of cohorts passed since the reform (negative before the reform, 0 in the year of the reform, 1 after a year, etc.) In fact this is a centered cohort variable. = Cohort coefficient (for all cohorts, before and after the reform)
Vocational	Dummy, for observations of persons who were in vocational education (secondary or school, depending on the analysis)
X_{nit}	N person and year specific control variables
ε_{it}	Random error

However, estimating equation (1) is not possible, because of the classic “Age, period, cohort”-problem, which in our case means that experience (age), the year of observation (period) and timesince (cohort) are perfectly collinear.

To circumvent this problem, we follow the approach suggested by Papay and Kraft (2015), who suggest thinking of the mechanism that is likely to stay constant, independent of the other variables. In our case, we assume that the effect of the year of observation applies equally to all, irrespective of their experience or cohort.

If assumed independent of cohort, the suggestion would be to estimate in a first stage a year effect, omitting the cohort part (timesince), as in:

$$y_{it} = \gamma_1 + \beta_2 year + \gamma_3 experience + \gamma_7 vocational + \gamma_n X_{nit} + \rho_{it} \quad (2)$$

The identifying assumption is then that the year effect is conditional on experience and other characteristics, but unrelated with the cohort (year of birth, in a centered version: timesince):

$$Cov(f(year), g(timesince) | h(experience, vocational, X_{nit})) = 0 \quad (3)$$

This means, for example, that the average outcome y of a worker observed in 2001 is the same for a person born in 1970 and in 1980, given that they do not differ on other (observed) characteristics.

To maximise the flexibility of the functional form of the year effect, we use a dummy specification.

In a second stage, we then estimate the remaining coefficients, imputing the estimated year coefficient $\widehat{\beta}_2$.

$$y_{it} = \beta_1 + \widehat{\beta}_2 year + \beta_3 experience + \beta_4 reformcohort + \beta_5 timesince + \beta_6 vocational + \beta_7 vocational * reformcohort + \beta_8 vocational * timesince + \beta_n X_{nit} + \varepsilon_{it} \quad (4)$$

Note that the above specification (equation 4) is fairly general in the sense that it assumes experience profiles not to depend on the type of education, neither the period (before/after reform). We use this general specification as our basic model and extend it with a more fine-grained specifications (adding interaction between vocational training and experience).

The threats to identification we discussed above, are dealt with in the two-stage estimation procedure. The business cycle is taken into account by incorporating the year of observation as a control variable. By using a dummy specification, we make sure that business cycles are captured well. The assumption is that business cycle shocks affect all educational groups in a similar fashion.

To make sure that the concern of structural change is taken into account, we will extend the model with a time trend separately for the various educational groups, given that we observe in all groups a large number of individuals affected and not affected and before and after the reform took place, which ensures that we can identify the policy change separately. Yet, we estimate first the base model and look into extensions later (see robustness checks).

Finally, we acknowledge that our first concern (selection into education) remains a worry insofar the estimated effect does not only reflect how the change of the organization of vocational education (content+duration) affected wages (marginal productivity principle), but also reflects the consequences of compositional changes.

To gauge the importance of these changes, we will first study changes in the educational outcomes at the time of the reform. Important compositional changes, may suggest higher educational attainment after the reform (and a concurrent decrease of drop-out in high school, i.e. persons finishing their education without a certificate of any type of higher secondary education). It can be assumed that the upward shift in the education hierarchy decreases the ability level of the average graduate in all types of education. Subsequently, it is an empirical issue whether the downward wage pressure of this decrease in ability level is larger, equal or smaller than the upward wage pressure that can be expected by the educational reform.

However, in our analyses we will show that the number of high school dropouts declines, while the number of individuals with vocational school rises (slightly). This suggests that individuals with a vocational school credential may experience two parallel sources of upward wage pressure: the reform made them more skill-full (marginal productivity mechanism) AND the reform decreased the number of competitors with lower skill levels (i.e. the dropouts)(wage formation mechanism, supply versus demand). We will return to these issues when discussing the estimation results.

As a final element, we need to clarify that we do not incorporate the complete labour force of Hungary, but engage in the comparison of two adjacent educational categories in the

labour market (cfr.the CITS idea). In practice we compare the reform in vocational school with individuals with a credential of lower secondary education only. When looking into the effect of the reform for vocational secondary, we compare with individual who have a degree of grammar school. In a following chapter (see section 0), we will substantiate this choice of comparison groups with an empirical analysis showing the concentration of specific educational credentials in particular occupations. This analysis confirms that in many occupations adjacent categories in the educational hierarchy are the two dominant categories and competing.

4.3 SAMPLING CONSIDERATIONS

The models are estimated for men and women separately, assuming structural differences by gender.

Furthermore, we will estimate the models on three different samples to check for robustness. First we look into individuals born between 1979 and 1986, a relatively wide sample which should allow for a reliable estimate of the trends before and after the reform. Second, we restrict the view to a more limited sample (individuals born between 1974 and 1991) to check whether the effect of the reform remains visible and does not follow from our particular choice of sample. Finally, we will elaborate on a so-called “doughnut sample”, which leaves two cohorts in the middle out. These are the cohorts that experienced the preparation and introduction of the reform, which means that they may have run into the downsides of the introduction (starting-up problems, for example) and that it is not a priori clear if they have been touched by the complete reform. Moreover, empirical data suggest that many in the cohort right before the first reform cohort were also touched by the reform, because of grade repetition in earlier years of the educational system. Therefore, leaving out two cohorts may clarify our view of the true impact of the reform on the skills of vocationally trained individuals.

5. DATA

To study the effect of the vocational education reform on educational attainment and employment, we use the Labour Force Survey, conducted by the Hungarian Statistical Office, while wage data come from the Wage Survey of the National Employment Office.

5.1 DESCRIPTIVE STATISTICS, LFS DATA

This section describes the data used in the analyses of employment. We have first restricted the LFS sample to respondents aged 23 and older. Most people in Hungary have usually completed their educational attainment at age 23. This resulted in sample with 61936 unique observations. Table 0-1 presents descriptive statistics of this sample. The average age of persons in this sample is 29 years old. The birth year varies between 1976 and 1988 and it has the mean of 1980. This is the maximum number of cohorts that could be included using the age restriction in these data. There are almost equal proportions of men and women in the sample. The largest share of respondents has a Vocational school diploma as the highest educational attainment (23%). The lowest share is of a General secondary education diploma (10%). The majority of respondents were employed at the time of survey (69%). The year of survey wave varies from 2001 to 2013.

Table 0-1

**Summary statistics of the employment dataset
(subsample of the Labour Force Survey)**

Variable	Mean	Std. Dev.	Min	Max
Age	29	3.11	25	37
Year of birth	1980	3.02	1976	1988
Female	0.49	0.50	0	1
Primary education or lower secondary	0.17	0.37	0	1
Vocational school	0.29	0.46	0	1
Vocational secondary school	0.23	0.42	0	1
General secondary education	0.10	0.30	0	1
Tertiary education	0.21	0.41	0	1
Employed	0.69	0.46	0	1
Year of the wave	2009	3.29	2001	2013
N (number of observations)	61,936			

5.2 WAGE SURVEY OF THE NATIONAL EMPLOYMENT OFFICE

The Wage Survey is a yearly repeated representative survey covering the employees of firms and public sector institutions with at least five employees. The sample consists of all employees in a sample of small firms (below 50) and a sample of employees from each larger firm. Probability weights ensure that the sample represents the employee population. The yearly sample size varies in the range of 200-250,000, which amounts to 5-6 percent of the employed population. The respondents are the firms, reporting detailed wage data, working hours, occupation, education, gender and age, the employees themselves are not interviewed.

We use the 2002-2014 waves of the Wage Survey. As the vocational reform was introduced in 1998, no employees exposed to the reform can be observed before 2002. Our

estimation sample consists of 18 birth cohorts, 9 of these exposed to the reform and 9 that mostly graduated in secondary education in the pre-reform period. The first cohort was born in the 1974-75 academic year, while the last one is from 1991-92. We excluded younger cohorts as they were also exposed to another major reform, increasing the compulsory education age from 16 to 18 years. Moreover, these younger cohorts can be observed only in the very last years the dataset. Note that although there is an equal number of before and after reform cohorts in the estimation sample, it is still unbalanced as the number of pre-reform observations is much higher. In the early years we can observe only members of the first after reform cohorts, then other cohorts enter the labour market year by year.

Beside this sample, we also estimate the reform effect for two subsamples of cohorts. First, we restrict the sample to the 4 cohorts before the reform and 4 cohorts after. Second, we define a sample of similar size, i.e. 8 cohorts, but leaving out two the cohorts before the reform. These cohorts were not affected, assuming a regular school career for everyone. However, due to delayed school starting after kindergarten or repeating a grade, a considerable share of these cohorts happened to graduate in the reform regime.

The estimation sample regarding the wage outcome contains four education groups, lower-secondary education and the three tracks of upper-secondary education; vocational school, vocational secondary and academic secondary (grammar school). The final sample includes 588,809 observations.

Our key variable is the hourly wage rate, which we construct using gross monthly wages and working hours per week and express in 2015 Forints. Work experience is estimated from the cohort, survey year and the minimum number of years required to get the given education qualification. Table 0-2 provides summary statistics of the data.

To compare the labour market positions of the education groups, we calculated occupation segregation indices. For a given pair of education groups, the Duncan segregation index (Duncan-Duncan, 1955) gives the share of workers who should be replaced into another occupation to have an identical distribution over occupations in the two groups. In other words, a low value of the index suggests that members of the two groups are employed in the same occupations, while high values indicate no overlap in their labour market positions. Table 0-3 provides the pairwise occupation segregation indices, also including higher education. The results suggest that academic and vocational secondary education are close substitutes. There is a significant overlap in labour market positions of the lower-secondary and vocational school group on the one hand, and the vocational school and vocational secondary school group on the other.

Table o-2

Summary statistics of the wage dataset

	Mean	s.d.	Min	Max
wage	977,3	617,5	8,2	57956,2
firm (0 denotes government institutions)	0,84	0,37	0	1
female	0,44	0,50	0	1
part-time employment	0,11	0,32	0	1
birth cohort	1980,00	4,43	1974	1992
experience	10,88	5,04	0	23
education				
schooled after the reform	0,26	0,44	0	1
lower-secondary	0,15	0,36	0	1
vocational school	0,37	0,48	0	1
vocational secondary	0,32	0,47	0	1
academic secondary	0,16	0,36	0	1

Note: 1974-91 cohorts.

Table o-3

Pairwise occupational segregation indices by education groups

	lower secondary	vocational school	vocational secondary	academic secondary
men				
vocational school	0,458			
vocational secondary	0,635	0,416		
academic secondary	0,671	0,561	0,226	
higher education	0,952	0,917	0,701	0,687
women				
vocational school	0,442			
vocational secondary	0,720	0,576		
academic secondary	0,715	0,618	0,170	
higher education	0,936	0,888	0,620	0,614

Note: 1974-91 cohorts.

Occupational segregation indices at 4-digit occupation level.

6. RESULTS: EDUCATIONAL COMPOSITION, EMPLOYMENT AND WAGES

As described earlier, we expect the reform to have improved the marginal productivity of labour of vocationally educated in Hungary. As such our outcome of interest is the wage of this group (relative to not-directly-affected others). Yet, because the introduction of the reform may have motivated students to change their educational choices, and hence may have altered the educational composition of the labour force, we first study the latter. Furthermore, the reform was realised in a changing economy, with business cycle and long term developments that may have altered the demand for vocational skills independently of the reform, yet influenced the wage rate as well. Therefore, we also study the employment rate of the affected and unaffected. Bearing the results of both analyses in mind, we close this chapter with the analysis of wages (section 0).

6.1 RESULTS: EDUCATIONAL ATTAINMENT

Table 0-4 provides the before-after regression estimates of the effect of the reform on educational attainment which are based on the Hungarian Labour Force Survey. The table distinguishes between three potential educational outcomes: vocational school, vocational secondary school and grammar school. The vocational school outcome variable is equal to 1 if a graduates' highest attained level of education is vocational school or higher and 0 otherwise. The vocational secondary school outcome variable is equal to 1 if a graduates' highest attained level of education is secondary vocational school or higher and 0 otherwise. Finally, the grammar school outcome variable is equal to 1 if a graduates' highest attained level of education is grammar school or higher and 0 otherwise. The sample is restricted to individuals who are older than 22 years to reduce the probability that individuals' observed educational attainment serves to enrol into higher levels of education. All models control for graduates' age, age squared, and include a cohort trend.

As already explained in the methodology section (see 0) , we consider the outcomes for women and men separately and estimate the models on three different samples to check robustness.

The estimates based on the sample that includes cohorts 1974 until 1991 suggest that men and women were less likely to obtain their highest degree of education from vocational schools after the reform. A weakly significant decrease in the probability of obtaining a degree from secondary vocational school is also found for men in the sample includes individuals born between 1974 and 1991. No significant changes are found in the attainment

of a grammar school degree. Caution applies when interpreting these coefficients because they are not reproduced in the two other estimation samples.

As such, the results suggest relatively little change in the educational composition, with changes of 2 to 3% of categories that represent each more than a quarter of the Hungarian population (see Table o-1 for descriptives). If at all effective, the reform seems to have reduced the proportion of students with a vocational degree and increased the proportion of individuals with a lower degree (lower secondary education). It may therefore be that students with degree of vocational school (and to a lesser extent in secondary vocational school) are more selective students after the reform than before. When looking at the wage and employment results in the subsequent sections, we should therefore bear in mind that selection may explain part of the observed effects (apart from the change in the curriculum).

Table o-4

Effect of the reform on educational attainment, before-after estimates

	Vocational school	Vocational secondary	Grammar school	Vocational school	Vocational secondary	Grammar school
	males			females		
1979-86 cohorts	0.00598	0.0198	0.0113	-0.0134	-0.000466	-0.00420
	(0.0107)	(0.0143)	(0.0130)	(0.0107)	(0.0139)	(0.0146)
	[20,961]	[20,961]	[20,961]	[20,099]	[20,099]	[20,099]
1978-1981, 1984-88 cohorts	0.0130	-0.00854	0.00258	-0.0215	-0.0393**	-0.0231
	(0.0147)	(0.0198)	(0.0178)	(0.0147)	(0.0192)	(0.0198)
	[22,024]	[22,024]	[22,024]	[21,253]	[21,253]	[21,253]
1974-91 cohorts	-	-0.0171*	-0.00819	-	-0.0134	-0.00616
	0.0232***			0.0298***		
	(0.00709)	(0.00920)	(0.00793)	(0.00715)	(0.00924)	(0.00909)
	[52,375]	[52,375]	[52,375]	[50,243]	[50,243]	[50,243]

Note: Each coefficient is from a separate regression. All models control for age, age squared, and year of birth. Number of observations in square brackets. *** p<0.01, ** p<0.05, * p<0.1

6.2 THE EFFECT OF THE REFORM ON EMPLOYMENT

We provide results for the effect of the reform on employment where we allow age to differ across education groups, as we will do with experience in the wage estimates. In the methodology section (o) we explained that this version of the CITS is the most flexible regarding the potential impact of the reform. However, we first present estimates of a more restricted specification which does not allow for variable age patterns before and after the

reform. The latter was our base specification in the methodology section, but is obviously restrictive.

The regressions are estimated separately for men and for women controlling for age, age-squared, months of birth and reform year (time-trend before and after the reform). Table 0-5 and

Table 0-6 show the estimates for vocational school, where lower secondary school is the reference category. Table 0-7 and Table 0-8 show the estimates for vocational secondary school, where general secondary education is the reference category. Each of the models refers to different sample specifications based on year of birth with the cut-off of June 1 (school starting age). Sample 1 refers to birth cohorts 1976-1988. Sample 2 estimates the effect of the reform for those who were born in 1977-1980 or 1983-1986. Sample 3 is restricted to those who are born between 1974 and 1991. Also note that we follow a two-stage approach which estimates year effects to be independent of the cohorts (see section 0 for further explanation). Below we focus on the results of the second stage and look into the significance of the effect of the reform on those who were affected by it.

The results suggest that the reform has had a positive effect on women, both for vocational school (Table 0-5) and vocational secondary school models (Table 0-7). For men, the coefficient of the reform is not statistically significant for vocational school (

Table 0-6), but positive and statistically significant for vocational secondary school (Table 0-8).

However, if we use the flexible specifications of our model by allowing the age profiles to be different between the educational groups (Table 0-9 to Table 0-12), the significance of the estimates is dramatically reduced. Only for women with a degree of vocational school we still observe statistically significant positive employment effect of the reform. Thus, the reform does seem to have affected many individuals with a vocational training. For women with a vocational school degree employment prospects improved, while for men and women with a degree of vocational secondary the effect was more indirect, reducing their likelihood of employment at a particular age.

Table o-5

The effect of the reform in vocational schools on employment for Women, CITS estimates (restricted specification, relative to lower secondary school)

	Sample 1			Sample 2			Sample 3		
	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.
Vocational school * reform	0.099	0.032	0.002	0.084	0.031	0.007	0.078	0.029	0.006
Reform cohort (dummy)	-0.054	0.034	0.119	-0.064	0.039	0.103	-0.031	0.026	0.225
Vocational school	0.210	0.017	0.000	0.226	0.013	0.000	0.232	0.010	0.000
Age	-0.139	0.075	0.064	-0.104	0.043	0.015	-0.093	0.028	0.001
Age-sq	0.003	0.001	0.023	0.002	0.001	0.011	0.002	0.000	0.001
Month of birth	0.003	0.002	0.111	-0.002	0.002	0.252	-0.001	0.001	0.506
Years since reform	-0.012	0.025	0.625	0.023	0.021	0.261	0.002	0.015	0.911
Const.	1.907	1.069	0.075	1.908	0.638	0.003	1.697	0.424	0.000
Adj. R-sq.	0.0750			0.0610			0.0619		
No.	4,627			6,549			11,124		

Note: Reference group is lower secondary education. Model 1 refers to birth cohorts 1976-88. Model 2 refers to birth cohort 1977-80 and 1983-86. Model 3 refers to birth cohort 1974-91. Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. *** p<0.01, ** p<0.05, * p<0.1

Table o-6

The effect of the reform in vocational schools on employment for Men, CITS estimates (restricted specification, relative to lower secondary education)

	Sample 1			Sample 2			Sample 3		
	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.
Vocational school * reform	-0.007	0.024	0.782	0.012	0.022	0.589	0.03	0.02	0.13
Reform cohort (dummy)	-0.004	0.026	0.885	-0.035	0.029	0.225	-0.04	0.02	0.02
Vocational school	0.268	0.013	0.000	0.249	0.010	0.000	0.24	0.01	0.00
Age	0.001	0.054	0.982	0.016	0.030	0.588	0.02	0.02	0.37
Age-sq	0.001	0.001	0.500	0.000	0.001	0.707	0.00	0.00	0.84
Month of birth	0.001	0.001	0.358	0.002	0.001	0.109	0.00	0.00	0.04
Years since reform	0.011	0.018	0.545	-0.002	0.015	0.888	-0.01	0.01	0.37
Const.	0.104	0.774	0.893	-0.262	0.453	0.564	-0.18	0.30	0.54
Adj. R-sq.	0.1075			0.1004			0.0904		
No.	6,960			9,712			16,420		

Note: Reference group is lower secondary education. Model 1 refers to birth cohorts 1976-88. Model 2 refers to birth cohort 1977-80 and 1983-86. Model 3 refers to birth cohort 1974-91. Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. *** p<0.01, ** p<0.05, * p<0.1

Table o-7

The effect of the reform in vocational secondary schools on employment for Women, CITS estimates (restricted specification, relative to general secondary school)

	Sample 1			Sample 2			Sample 3		
	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.
Vocational school * reform	0.080	0.029	0.006	0.107	0.027	0.000	0.088	0.025	0.000
Reform cohort (dummy)	-0.041	0.033	0.207	-0.079	0.038	0.038	-0.060	0.024	0.013
Vocational school	0.106	0.017	0.000	0.079	0.015	0.000	0.097	0.011	0.000
Age	-0.001	0.072	0.991	-0.006	0.043	0.883	-0.005	0.029	0.857
Age-sq	0.000	0.001	0.889	0.000	0.001	0.922	0.000	0.000	0.643
Month of birth	-0.001	0.002	0.781	-0.004	0.002	0.024	0.000	0.001	0.777
Years since reform	0.004	0.023	0.859	0.034	0.021	0.102	0.004	0.016	0.801
Const.	0.553	1.014	0.585	0.894	0.640	0.162	0.706	0.436	0.105
Adj. R-sq.	0.0210			0.0209			0.0174		
No.	5,256			6,603			10,784		

Note: Reference group is general secondary education. Model 1 refers to birth cohorts 1976-88. Model 2 refers to birth cohort 1977-80 and 1983-86. Model 3 refers to birth cohort 1974-91. Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. *** p<0.01, ** p<0.05, * p<0.1

Table o-8

The effect of the reform in vocational secondary schools on employment for Men, CITS estimates (restricted specification, relative to general secondary school)

	Sample 1			Sample 2			Sample 3		
	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.
Vocational school * reform	0.069	0.025	0.005	0.115	0.023	0.000	0.105	0.020	0.000
Reform cohort (dummy)	-0.009	0.028	0.751	-0.066	0.032	0.040	-0.054	0.020	0.009
Vocational school	0.203	0.015	0.000	0.157	0.013	0.000	0.169	0.010	0.000
Age	0.319	0.061	0.000	0.287	0.037	0.000	0.248	0.024	0.000
Age-sq	-0.005	0.001	0.000	-0.005	0.001	0.000	-0.004	0.000	0.000
Month of birth	-0.002	0.002	0.277	-0.001	0.001	0.572	-0.001	0.001	0.642
Years since reform	-0.007	0.019	0.716	0.000	0.017	0.997	0.000	0.013	0.995
Const.	-3.900	0.873	0.000	-3.659	0.542	0.000	-3.206	0.370	0.000
Adj. R-sq.	0.1055			0.1079			0.1045		
No.	5,176			6,073			9,675		

Note: Reference group is general secondary education. Model 1 refers to birth cohorts 1976-88. Model 2 refers to birth cohort 1977-80 and 1983-86. Model 3 refers to birth cohort 1974-91. Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. *** p<0.01, ** p<0.05, * p<0.1

Table o-9

**The effect of the reform in vocational schools on employment for Women,
CITS estimates (flexible specification)**

	Sample 1			Sample 2			Sample 3		
	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.
Vocational school * reform	0.105	0.034	0.002	0.084	0.033	0.011	0.085	0.030	0.005
Reform cohort (dummy)	-0.057	0.035	0.104	-0.065	0.040	0.105	-0.035	0.026	0.182
Vocational school	-0.058	2.140	0.978	0.562	1.282	0.661	-0.229	0.822	0.780
Age	-0.146	0.116	0.208	-0.090	0.070	0.202	-0.110	0.043	0.011
Age-sq	0.003	0.002	0.131	0.002	0.001	0.184	0.002	0.001	0.010
Month of birth	0.003	0.002	0.110	-0.002	0.002	0.252	-0.001	0.001	0.504
Years since reform	-0.012	0.025	0.623	0.023	0.021	0.262	0.002	0.015	0.914
Voc.school * Age	0.015	0.151	0.919	-0.023	0.088	0.790	0.029	0.056	0.597
Voc.school * Age-sq	0.000	0.003	0.937	0.000	0.001	0.786	0.000	0.001	0.621
Const.	2.033	1.640	0.215	1.698	1.029	0.099	1.964	0.646	0.002
Adj. R-sq.	0.0748			0.0607			0.0619		
No.	4,627			6,549			11,124		

Note: Reference group is lower secondary education. Model 1 refers to birth cohorts 1976-88. Model 2 refers to birth cohort 1977-80 and 1983-86. Model 3 refers to birth cohort 1974-91. Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. *** p<0.01, ** p<0.05, * p<0.1

Table o-10

**The effect of the reform in vocational schools on employment for Men,
CITS estimates (flexible specification)**

	Sample 1			Sample 2			Sample 3		
	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.
Vocational school * reform	-0.006	0.026	0.824	0.022	0.024	0.358	0.028	0.021	0.194
Reform cohort (dummy)	-0.004	0.027	0.869	-0.041	0.030	0.164	-0.043	0.020	0.034
Vocational school	1.629	1.643	0.321	-0.927	0.972	0.340	-0.403	0.624	0.519
Age	0.067	0.096	0.483	-0.036	0.056	0.515	-0.014	0.035	0.691
Age-sq	-0.001	0.002	0.754	0.001	0.001	0.267	0.001	0.001	0.295
Month of birth	0.001	0.001	0.369	0.002	0.001	0.113	0.002	0.001	0.035
Years since reform	0.011	0.018	0.536	-0.002	0.015	0.888	-0.010	0.011	0.377
Voc.school * Age	-0.097	0.116	0.404	0.078	0.066	0.243	0.046	0.042	0.278
Voc.school * Age-sq	0.002	0.002	0.402	-0.001	0.001	0.261	-0.001	0.001	0.258
Const.	-0.822	1.359	0.545	0.543	0.823	0.509	0.264	0.528	0.618
Adj. R-sq.	0.1073			0.1003			0.0904		
No.	6,960			9,712			16,420		

Note: Reference group is lower secondary education. Model 1 refers to birth cohorts 1976-88. Model 2 refers to birth cohort 1977-80 and 1983-86. Model 3 refers to birth cohort 1974-91. Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. *** p<0.01, ** p<0.05, * p<0.1

Table 0-11

The effect of the reform in vocational secondary schools on employment for Women, CITS estimates (flexible specification)

	Sample 1			Sample 2			Sample 3		
	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.
Vocational school * reform	0.035	0.031	0.258	0.050	0.030	0.096	0.038	0.026	0.147
Reform cohort (dummy)	-0.015	0.033	0.644	-0.047	0.039	0.228	-0.029	0.025	0.240
Vocational school	3.546	2.131	0.096	3.533	1.324	0.008	3.409	0.889	0.000
Age	0.109	0.123	0.377	0.120	0.075	0.109	0.129	0.049	0.009
Age-sq	-0.001	0.002	0.509	-0.002	0.001	0.141	-0.002	0.001	0.024
Month of birth	0.000	0.002	0.851	-0.004	0.002	0.020	0.000	0.001	0.777
Years since reform	0.003	0.023	0.903	0.034	0.021	0.102	0.003	0.016	0.831
Voc.school * Age	-0.218	0.152	0.151	-0.218	0.091	0.017	-0.212	0.060	0.000
Voc.school * Age-sq	0.003	0.003	0.210	0.003	0.002	0.031	0.003	0.001	0.001
Const.	-1.253	1.731	0.469	-1.107	1.087	0.308	-1.389	0.737	0.059
Adj. R-sq.	0.0240				0.0258		0.0196		
No.	5,256				6,603		10,784		

Note: Reference group is general secondary education. Model 1 refers to birth cohorts 1976-88. Model 2 refers to birth cohort 1977-80 and 1983-86. Model 3 refers to birth cohort 1974-91. Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. *** p<0.01, ** p<0.05, * p<0.1

Table 0-12

The effect of the reform in vocational secondary schools on employment for Men, CITS estimates (flexible specification)

	Sample 1			Sample 2			Sample 3		
	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.	Coef.	Std. Err.	Sign.
Vocational school * reform	-0.031	0.027	0.244	0.004	0.025	0.865	-	0.022	0.999
Reform cohort (dummy)	0.061	0.029	0.033	0.013	0.033	0.692	0.021	0.021	0.311
Vocational school	6.767	1.879	0.000	6.086	1.167	0.000	5.14	0.803	0.000
Age	0.564	0.112	0.000	0.532	0.068	0.000	0.45	0.047	0.000
Age-sq	-0.009	0.002	0.000	-0.008	0.001	0.000	-0.007	0.001	0.000
Month of birth	-0.002	0.002	0.315	-0.001	0.001	0.620	-0.0004	0.001	0.709
Years since reform	-0.011	0.019	0.564	-0.003	0.017	0.869	-0.003	0.013	0.837
Voc.school * Age	-0.410	0.134	0.002	-0.369	0.081	0.000	-0.306	0.055	0.000
Voc.school * Age-sq	0.006	0.002	0.009	0.006	0.001	0.000	0.005	0.001	0.000
Const.	-7.889	1.583	0.000	-7.645	0.997	0.000	-6.592	0.698	0.000
Adj. R-sq.	0.1228			0.1227			0.1174		
No.	5,176			6,073			9,675		

Note: Reference group is general secondary education. Model 1 refers to birth cohorts 1976-88. Model 2 refers to birth cohort 1977-80 and 1983-86. Model 3 refers to birth cohort 1974-91. Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. *** p<0.01, ** p<0.05, * p<0.1

6.3 WAGE EFFECTS

Before we turn to the CITS estimates Table O-13 provides simple before-after regression estimates of the reform effect for the vocational and vocational secondary graduates. In these estimates survey year fixed effects account for changing labour market conditions and real wage levels. We also control for experience but do not allow for a cohort trend, in order to avoid collinearity (see section 4 for further discussion). These estimates suggest a positive reform effect for both vocational school and vocational secondary graduate men, and for women with a vocational secondary degree. The reform seems to increase wages in a range of 3-6 percentage points.

Figures 6.1 and 6.2 present a graphical illustration of the mean log wages by experience for the cohorts born between 1975 and 1990. The top panels display graduates from the education tracks that were affected by the 1998 reform, while the bottom panels present wages of workers with a non-vocational degree. Figure 6.1 suggests that the log wages of graduates from vocational schools are higher in the cohorts exposed to the reform, both in the case of men and women. At the same time, wages of workers with lower secondary education appear to remain unchanged. Figure 6.2 displays the wage profiles of graduates from vocational and academic secondary schools. In the vocational secondary group, wages seem to have increased, especially among men. However, similar increases can be observed within the academic secondary group, despite the fact that the vocational reform has not affected the academic track. Obviously, these raw differences between older cohorts and the cohorts exposed to the reform might reflect several confounding effects, beside the true effect of the vocational reform. For example, if real wages increase over time, younger cohorts earn more at a given level of experience than the older ones. Moreover, cohort trends, like changes in skills over time in the student population entering the vocational school track, may also affect these differences.

Altogether, these descriptive analyses suggest that the reform had a positive effect on wages, at least in some groups. The reform seems to have had the strongest effect in the group of men with a vocational school degree. However, these results are potentially confounded by any other changes that affect only the cohorts after 1983, e.g. unobserved changes in primary schooling or labour market shocks affecting workers in their early years of career disproportionately. The effect of these unobserved changes will be absorbed by the estimated reform effect, thus providing biased estimates. Table O-14 and Table O-15 provide the CITS estimates, that should account for these biases.

In these models, we estimate whether the treatment group deviates from its pre-reform trend by a greater amount than the comparison group does. The models include a cohort trend and an interaction term of cohort and vocational education in order to represent the pre-reform trends in the two groups. The restricted specifications in columns (1) and (3) restrict the experience profiles to be identical in the treatment and control groups. The more flexible models in columns (2) and (4) allow the experience profile to differ across education groups. In other words, we allow the pre-reform trends to differ between the treatment and control groups not only across cohorts, but also with respect to experience and survey years at the same time.

The results indicate that the reform had a positive wage effect only for men with a vocational school degree. In this group the reform has increased wages by 4.7-6.8 percentage points, depending on the sample and specification. This effect size is close to that suggested by the before-after regression. The restricted and flexible specifications provide almost identical results, while the coefficients are larger in the full sample than in the smaller ones.

The effect estimated for men is substantial in economic terms. The average returns to the vocational school degree over lower secondary education before the reform, estimated by the main effect of vocational school in column (1), was about 13-14 percent. In other words, among men, the wage premium of the vocational school degree compared to unskilled wages has increased by 33-50 percent after the reform.

At the same time, the results suggest no effect on the wages of vocational secondary school graduates and women with a vocational school degree.

Note that the estimates in Table O-14 represent the net effect of the extended schooling in the vocational tracks, assuming that the years of experience is fixed. However, students not only gain an additional year in school but also lose a year of work experience. The latter effect can be expected to decrease both wages at a given age and total earnings over the worker's lifetime. Hence the total effect of the reform is the sum of the gains from the additional schooling and the losses incurred by reduced experience. This total effect can be estimated by controlling for age instead of experience (Oosterbeek and Webbink, 2007).

Table O-16 provides these estimates for vocational school graduates. As the reform has no effect in the vocational secondary school in any of our CITS models, this group is omitted henceforth. As expected, the results are qualitatively similar to the net effects in Table O-14 but muted towards zero.

The effect size varies between 3 and 5 percentage points for men. The coefficients in the leave-out sample are not statistically significant. However, larger standard errors in the smaller samples are not surprising, as four data points before the treatment is a lower limit for estimating a CITS model (see e.g. Somers et al., 2013). Comparing this to the 13-14

percent wage returns of the vocational school degree, the reform seems to incur a total 20-40 percent increment.

Finally, we assess the heterogeneity of the results across the cohorts exposed to the reform. Instead of assuming a level effect only, we also allow the slope of the cohort trend to change both in the treatment and control groups after the reform had taken place. Formally, we augment the flexible specification of Table O-14 and Table O-16 with an interaction term of reform and cohort, and a triple interaction of reform, cohort and vocational education. The coefficient of the latter represents the difference in slope between the treatment and control groups after the reform.

Table O-17 provides the results for vocational school graduates. As estimating the level and slope effects at the same time with acceptable precision requires a larger number of cohorts, we present the results only for the largest sample of nine post- and nine pre-reform cohorts. Columns (1) and (3) contain the net level and slope effects of the school reform, while columns (2) and (4) provide the total effects, i.e. the difference between the gains of extended schooling and the loss related to decreased experience.

For men, both the level and slope effects are positive and statistically significant. In other words, wages increased immediately after the reform, but younger cohorts gained more. The slope effects are also positive and significant, though only at the ten percent level, for women. This suggests that younger cohorts of women also gained from the reform, though their wages increased less than the wages of men. One possible explanation for the reform effect increasing over cohorts is that it took some time for schools to adapt to the reform and use the additional teaching time relatively efficiently. Alternatively, the increased number of students in schools probably increased the workload of teachers in the first years after the introduction of the reform, which might have muted the gains.

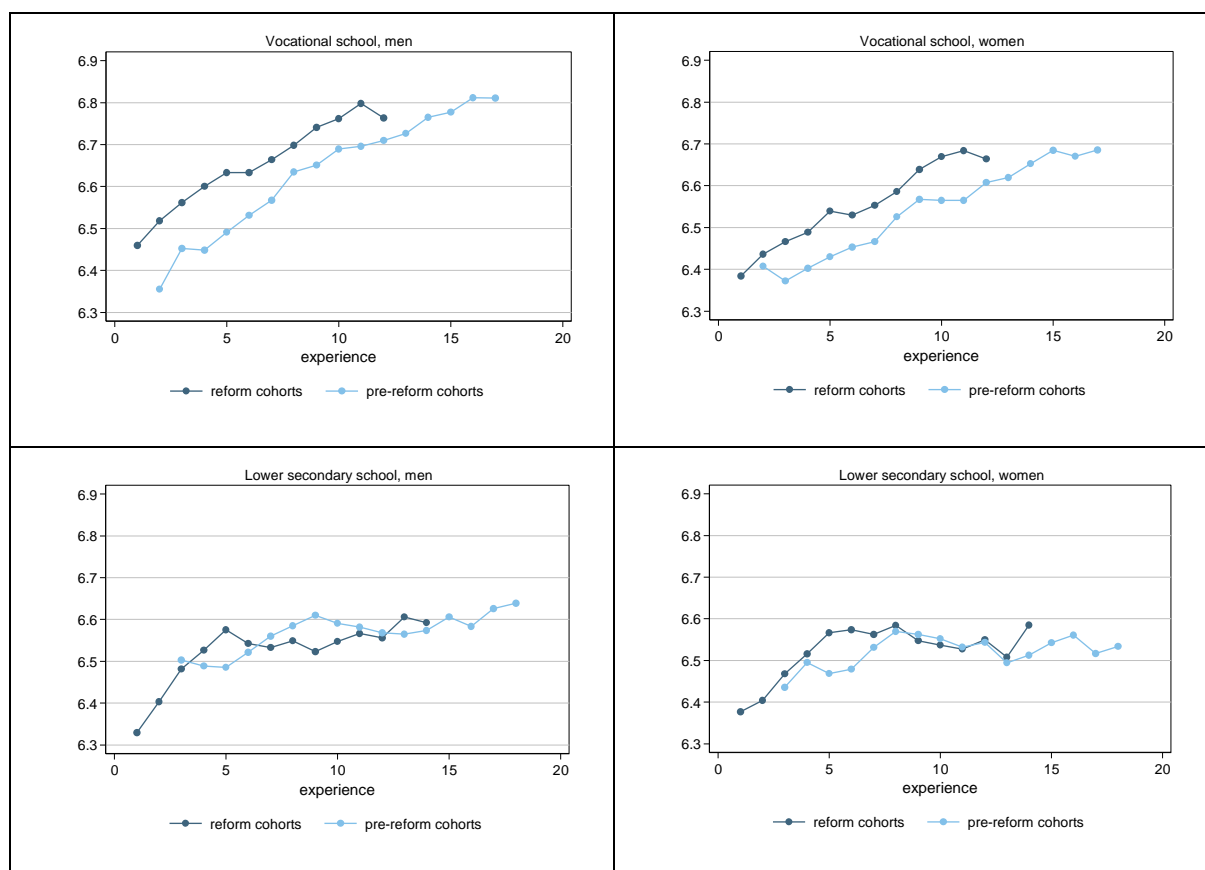
Altogether, the results suggest that the reform had a positive effect on wages of vocational school graduates, while there is no effect in vocational secondary schools. One possible explanation for this difference is diminishing returns to skills. The math and reading skills of vocational school students lag far behind those of secondary school pupils in Hungary. It is possible that an improvement at this low level yields significant gains.

The results also show that within the vocational school group, the reform had a larger effect on male wages. The average effect among females is not significantly different from zero, while some wage gain can be detected for the younger cohorts. What does explain this difference? One factor can be that men and women with a vocational school degree typically work in different economic sectors. Men are overrepresented in industries and agriculture, while more women are working in services, including health and social services in the public

sector. Higher returns to skills in male occupations, or in some industrial sectors might explain the larger effect of the reform.

Figure 6-1

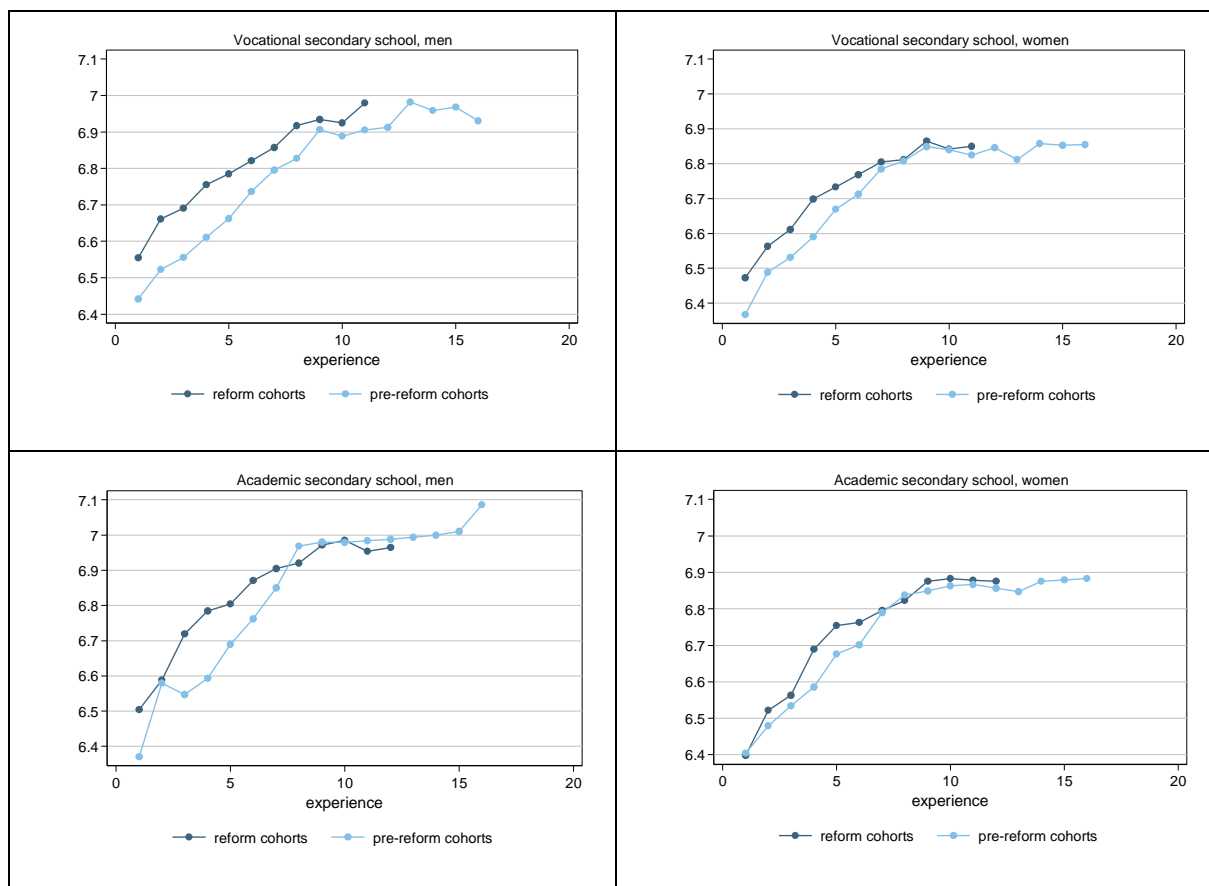
Mean log wages by experience and gender, employees with vocational school and lower secondary school degree



Note: Log hourly wages at 2015 prices, in years 2002-2014. Full-time employees only. Before reform cohorts were born June 1979 - May 1983, reform cohorts were born June 1983 - May 1986.

Figure 6-2

Mean log wages by experience and gender, employees with vocational and academic secondary school degree



Note: Log hourly wages at 2015 prices, in years 2002-2014. Full-time employees only. Before reform cohorts were born June 1979 - May 1983, reform cohorts were born June 1983 - May 1986.

Table o-13

Effect of the reform on log wages, before-after estimates

	Vocational school		vocational secondary	
Samples	males	females	males	females
	(1)	(2)	(3)	(4)
1979-86 cohorts	0.0276** (0.0132) [60,821]	0.0147 (0.0171) [25,579]	0.0389* (0.0204) [39,313]	0.0412** (0.0162) [44,233]
1977-80, 1983-86 cohorts	0.0572*** (0.0181) [71,554]	0.0200 (0.0205) [29,872]	0.0899*** (0.0270) [44,124]	0.0828*** (0.0219) [49,922]
1974-91 cohorts	0.0637*** (0.00905) [152,044]	0.00766 (0.00982) [64,907]	0.0438*** (0.0125) [87,353]	0.0462*** (0.0110) [101,388]

Note: Each coefficient is from a separate regression. Number of observations in square brackets, robust standard errors clustered at the firm level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table o-14

Effect of the reform in vocational schools on log wages, CITS estimates

	males		females	
Samples	restricted	flexible	restricted	flexible
	(1)	(2)	(3)	(4)
1979-86 cohorts	0.0554*** (0.0199) [85,118]	0.0546*** (0.0199) [85,118]	0.0132 (0.0211) [38,571]	0.0111 (0.0213) [38,571]
1977-80, 1983-86 cohorts	0.0473* (0.0261) [98,577]	0.0528** (0.0263) [98,577]	0.00613 (0.0306) [44,871]	0.00645 (0.0312) [44,871]
1974-91 cohorts	0.0655*** (0.0132) [209,384]	0.0683*** (0.0134) [209,384]	-0.00705 (0.0144) [98,222]	-0.00301 (0.0156) [98,222]

Note: Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. Number of observations in square brackets, robust standard errors clustered at the firm level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 0-15

**Effect of the reform in vocational secondary schools on log wages,
CITS estimates**

	males		females	
Samples	restricted	flexible	restricted	flexible
	(1)	(2)	(3)	(4)
1979-86 cohorts	0.00800	0.00736	0.0113	0.0155
	(0.0307)	(0.0303)	(0.0231)	(0.0230)
	[56,182]	[56,182]	[69,612]	[69,612]
1977-80, 1983-86 cohorts	0.0537	0.0686	0.0277	0.0385
	(0.0452)	(0.0449)	(0.0311)	(0.0311)
	[62,284]	[62,284]	[77,814]	[77,814]
1974-91 cohorts	0.00836	0.0172	0.0127	0.0190
	(0.0208)	(0.0209)	(0.0164)	(0.0165)
	[123,191]	[123,191]	[157,853]	[157,853]

Note: Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. Number of observations in square brackets, robust standard errors clustered at the firm level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 0-16

Total effect of the reform in vocational schools on log wages, CITS estimates

	males		females	
	restricted	flexible	restricted	flexible
	(1)	(2)	(3)	(4)
1979-86 cohorts	0.0393**	0.0392**	0.00155	0.00120
	0.0200	0.0200	0.0213	0.0214
	[85,118]	[85,118]	[38,571]	[38,571]
1977-80, 1983-86 cohorts	0.0278	0.0334	-0.00465	-0.00195
	0.0262	0.0263	0.0312	0.0312
	[98,577]	[98,577]	[44,871]	[44,871]
1974-91 cohorts	0.0522***	0.0482***	-0.00298	-0.0105
	0.0134	0.0135	0.0148	0.0156
	[209,384]	[209,384]	[98,222]	[98,222]

Note: Each coefficient is from a separate regression. Survey year effects are estimated in a separate regression. Number of observations in square brackets, robust standard errors clustered at the firm level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**Net and total effect of the reform in vocational schools on log wages,
CITS estimates**

	males		females	
	net	total	net	total
	(1)	(2)	(3)	(4)
level effect	0.0461***	0.0264*	-0.0186	-0.0253
	(0.0139)	(0.0139)	(0.0187)	(0.0187)
slope effect	0.0119***	0.0113***	0.00882*	0.00836*
	(0.00315)	(0.00313)	(0.00457)	(0.00456)
	[209,384]	[209,384]	[98,222]	[98,222]

Note: 1974-91 cohorts. Survey year effects are estimated in a separate regression. Number of observations in square brackets, robust standard errors clustered at the firm level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

7. CONCLUSION

In this paper, we look into the effect of the reform of vocational education introduced in 1998 in Hungary, which entailed an extension of the duration of education by one year and an increase of the general skills included in the educational programmes. Ultimately, we expect the reform to produce a positive productivity shock, which should become visible in the wage rate of those affected. Yet, because selection into education may also be influenced and wages are only observed for those with a job, we also look into two intermediate outcomes: the educational composition of the population and employment rates.

Altogether, the wage estimates suggest that the reform had a positive effect on wages of vocational school graduates, while there is no effect in vocational secondary schools. One possible explanation for this difference is diminishing returns to skills. The math and reading skills of vocational school students lag far behind those of secondary school pupils in Hungary. It is possible that an improvement at this low level yields significant gains. A potentially complementary explanation stems from the results of the analysis of the educational composition. The latter suggested that the reform led to an increase in persons who do not obtain a full secondary school qualification (i.e. remain with lower secondary education as their highest degree), while the proportion of other degrees remained constant. As such (proportionally) less persons with a degree of vocational school entered the labour market after the reform, which may have improved their wages (relative to the group with lower secondary education).

The wage estimates also show that within the vocational school group the reform had a larger effect on male wages. The average effect among females is not significantly different from zero, while some wage gain can be detected for the younger cohorts. What does explain

this difference? One factor can be that men and women with a vocational school degree typically work in different economic sectors. Men are overrepresented in industries and agriculture, while more women are working in services, including health and social services in the public sector. Higher returns to skills in male occupations, or in some industrial sectors might explain the larger effect of the reform.

Another factor is that our employment estimates suggest that the reform has benefitted the relative employment rates of women more than of men, which means that women may have benefitted from the reform by higher employment rates rather than higher wages, while for men employment rates were already higher and the reform resulted in wage effect rather than employment effect.

All in all, our estimates suggest that the reform of vocational education has had heterogeneous effects which were positive for those who continued in vocational education, but also discouraged some to pursue this type of education.

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