

Forced to Stay in School: Does it Pay Off? Private Returns to Vocational Education in the Netherlands¹

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Abstract

This paper questions the widespread view that having an upper secondary diploma results in higher earnings. Using unique and complete administrative panel data from vocational education in the Netherlands, we compare the earnings of high school graduates to the earnings of high school dropouts. Hereby, we exploit an educational policy reform in 2007 in which the compulsory education age was extended from the age of 17 until the age of 18 years. In contrast to the widespread view, our findings suggest that obtaining a vocational diploma does not lead to significantly higher earnings. We find a small effect of 2.4% that is not statistically significant. We suggest modularization as a potential mechanism to explain zero returns to schooling. A modular vocational system may result in students enhancing their productivity in some parts of the curriculum needed to reside in the labour market, without obtaining the actual qualification.

Keywords: Returns to Education; Instrumental Variables; High School Dropout; Compulsory Schooling; Vocational Education

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1. Introduction

Early models of returns to education view schooling as a financial investment. Individuals spend time and money either to acquire or to signal their human capital in hope of receiving higher wages. In these models, education either serves as a means to increase productivity (Becker, 1962), or to signal a worker's ability upon which the employer will select him (Spence, 1973). Whatever the model, earlier literature has traditionally agreed that the returns to education are positive (Card, 1999).

From this perspective, school dropout, or early school leaving, contradicts a successful transition to the labour market. Yet, in 2015, 11.1 percent of people aged 18 to 24 in EU-28, or 5.6 million early leavers from education or training, did not obtain a valid school-leaving credential (at least ISCED level 3) (Eurostat, 2017). Although early school-leaving in the European Union is still fairly high compared to the official dropout rates of the United States of America (6.8 percent of people aged 16 to 24) and Japan (3.4 percent of total), countries such as Australia (26 percent by the age of 19) or New Zealand (25.3 percent of people aged 15-24) display even higher percentages of school dropout.

Given the prevalence of early school leaving throughout OECD countries, recent literature has revamped the returns to education research and has come to a very different conclusion. In several European countries, education appears to have little to no effect on earnings (Grenet, 2013; Oosterbeek & Webbink, 2007; Pischke & von Wachter, 2008; Stephens & Yang, 2014). Consequently, it is unclear whether early school leaving is a consequence of high discount rates and irrationality as traditionally believed (Oreopoulos & Salvanes, 2011), or whether it is simply a consequence of low private returns to education in the labour market.

The overall objective of this study is to estimate the private returns to vocational education in the Netherlands. Hereby, we use an Instrumental Variables (IV) framework to exploit the ‘Qualification Law’ of August 1st 2007 that extends compulsory education from the age of 17 until the age of 18. To our knowledge, this is the first time the Qualification Law has been employed to estimate returns to education. The only other study in the Netherlands includes Oosterbeek & Webbink (2007) who exploit the raise in the school leaving age in 1975 from the age of 15 until the age of 16 and find no effect of education on earnings.

Our contribution is threefold. First, we present, for the first time, causal estimates of the return to obtaining a high school diploma based on the 2007 raise in the school leaving age in the Netherlands. Given that returns to education may depend on institutions and the organization of the school system (Pischke & von Wachter, 2008), the Dutch education system is particularly interesting as it is characterized by early ability tracking and a strong vocational orientation. Thus, it is possible that returns to education in the Netherlands strongly differ from returns to education in other European countries and in the US.

Second, we focus on Vocational Education or Training (VET). In fact, most studies (Angrist & Krueger, 1991; Grenet, 2013; Harmon & Walker, 1995; Stephens & Yang, 2014) do not distinguish between education tracks. This may be misleading as most students actually drop out from VET-programs (European Commission, 2013). These students have, on average, different observed and unobserved characteristics that distinguish them from students in other educational tracks, such as low socioeconomic status, fractured school carriers, and lower abilities (Cabus & De Witte, 2011). Therefore, it is unclear whether average estimates on the return to education are representative for students in vocational education.

Third, we benefit from a unique and complete administrative panel data that include all vocational students in the Netherlands from 2003 until 2014. This poses two important advantages. On the one hand, we can precisely determine whether and when a vocational student either obtained a certificate or dropped out. By contrast, earlier literature mainly uses survey data which may be prone to non-response and reporting error due to survey respondents either over-reporting their attainment, not knowing if the schooling they have had counts as a qualification, or simply not remembering their degree (Battistin, De Nadai, & Sianesi, 2014). On the other hand, our dataset is a large panel indicating that if an individual is missed in any one year due to unemployment, he can reoccur in the dataset in later years. By contrast, survey data is mostly repeated cross-section. This may lead to selection-effects as only the employed individuals will be observed in the data.

Our results show no evidence of benefits to a Dutch vocational education diploma on earnings. Ruling out various potential mechanisms including change in school quality, signalling, heterogeneous returns, and wage rigidity, we suggest that the explanation lies in specificity of the Dutch modular vocational system. A modular vocational system may result in students enhancing their productivity in some parts of the curriculum needed to reside in the labour market, without obtaining the actual qualification.

The paper proceeds as follows. In section 2, we provide a brief overview of the previous literature. In section 3, we explain the Dutch education system, with particular emphasis on vocational education. Sections 4 and 5 explain the methodology and sample construction. In section 6, we present the results. In section 7, we discuss different potential mechanisms and section 8 concludes.

2. Literature

Compulsory schooling laws have widely been used to correct for abler students self-selecting into the graduate group. This ability bias originates both in human capital as in signalling theory. According to the former, abler persons receive a high marginal rate of return and thus have an incentive to invest more in education than others have (Becker, 1962). According to the latter, abler persons find school less difficult and thus have an incentive to invest in education to signal their high ability (Spence, 1973). Consequently, OLS estimates will be biased upward. To correct for this, researchers exploit exogenous changes in compulsory schooling laws in an Instrumental Variables framework as raise in the school leaving age is assumed to affect educational attainment, but not wages.

An early example is the seminal paper of Angrist and Krueger (1991). They show that, due to compulsory schooling laws, individuals from the US 1970 and 1980 Census born earlier in the year have less schooling than men born later in the year. Consequently, they use an individual's quarter of birth (whether or not interacted with year of birth or state of birth) as an instrument for schooling and estimate returns to education of around 7.5%. In a similar approach using the 1950-1970 Censuses, Acemoglu and Angrist (2000) confirm this finding.

However, several authors have challenged this finding. In a different approach using Limited Information Maximum Likelihood (LIML) estimation on the same data, Staiger & Stock (1997) argue that Angrist and Krueger (1991) employ weak instruments and that the real returns to education are around 9%. By contrast, Stephens & Yang (2014) let year of birth effects across regions in the 1960-1980 US censuses and finds no significant returns to education.

Such findings contrast with the even larger estimates by Harmon and Walker (1995). Exploiting the British minimum school leaving age changes in 1947 and 1973, they argue that the returns to schooling are around 15%. However, Card (1999) has criticised these findings based on the premise that they do not adequately control for cohort effects.

Similar positive returns to education using the same British data emerged from Oreopoulos (2006) who finds returns to education in the order of 10-15%. However, after replicating the study by Oreopoulos (2006) and improving the estimation strategy, Devereux and Hart (2010) find no evidence of any positive return for women and small returns of 3% to 4% for men. Positive returns to education between 7.5%-8.2% using compulsory schooling laws have also been found in Portugal (Vieira, 1999), and around 9.4% in Norway (Aakvik, Salvanes, & Vaage, 2010).

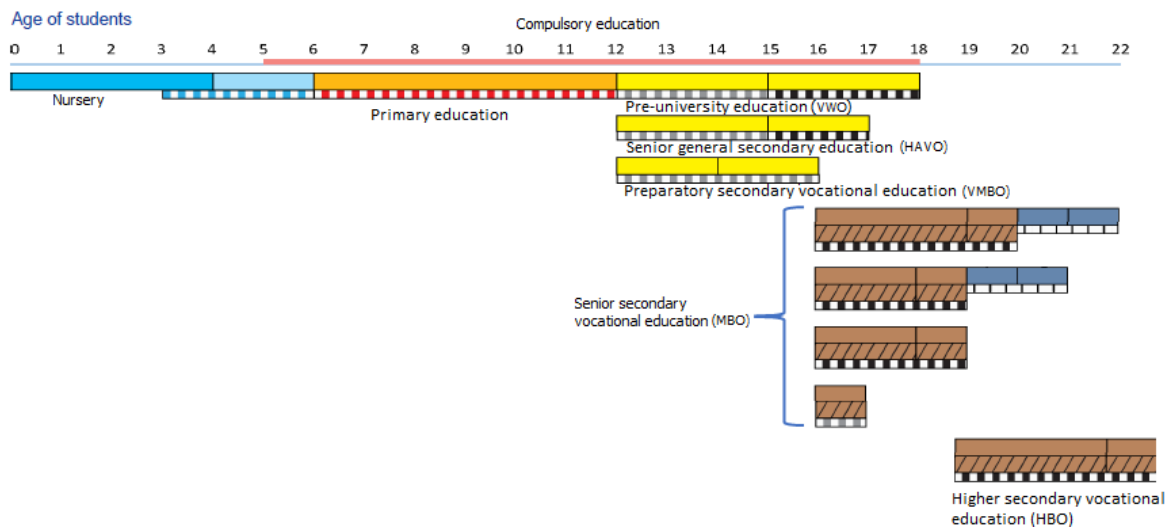
However, as noted by Ashenfelter, Harmon, and Oosterbeek (1999), there could be a publication bias present in the estimation of the returns to schooling. After correcting for publication bias in a meta-analytic approach, the authors find much smaller returns to education that are often insignificant. In line with this, several studies estimate no effect of education on earnings. Exploiting the raise in compulsory schooling age from the age of 15 until the age of 16 in the Netherlands, Oosterbeek and Webbink (2007) find zero returns to education. Similarly, employing a difference-in-differences strategy on a German dataset covering the 1948-1970 period, Pischke and von Wachter (2008) observe no effect of compulsory education change on earnings. Furthermore, puzzling results have been found by Grenet (2013) who shows that raise in compulsory schooling age to the age of 16 led to a 6%-7% increase in hourly wages per additional year of compulsory schooling in England and Wales, but no effect of the same raise in compulsory schooling age in France. Other studies in Scandinavia include small and insignificant returns to

education in Sweden (Meghir & Palme, 2005), and in Norway (Black, Devereux, & Salvanes, 2005).

The aforementioned papers estimate returns to education along the intensive margin (i.e. an extension to an existing compulsory schooling law). On the other hand; Clay, Lingwall, and Stephens (2012) investigate the returns to education along the extensive margin (i.e. an introduction of a compulsory schooling law) in the United States. Using data from the 1940 census, they observe large returns to education ranging from 11%-14%.

3. Dutch Education System

Figure 1: Dutch education system



Source: Own construction based on European Commission, EACEA, and Eurydice (2015).

Figure 1 presents the Dutch education system. Primary education starts from the age of five and lasts six years. Prior to primary education, parents can enroll their children into nursery. From the age of twelve, children go directly to high school that operates through a tracking system. This tracking occurs based in part on the score on a national and standardized cognitive test (CITO),

taken at the end of primary education (age 12), and in part on the advice given by the school. The secondary education consists of three tracks: pre-university education (VWO), senior general secondary education (HAVO), and preparatory secondary vocational education (VMBO). VWO lasts six years and is meant to prepare students for the university, while HAVO lasts five years. By contrast, preparatory secondary vocational education (VMBO) lasts four years and is more vocationally oriented. After finishing the VMBO curriculum, the student can enroll in the senior secondary vocational education (MBO) which prepares students for either work or future studies. Depending on the chosen program, the duration of MBO varies from two to four years. In addition, Dutch education system also offers higher vocational education (HBO) which lasts two years for an associate degree and four years for a Bachelor's degree. (Dutch Ministry of Education, Culture and Science, 2017a).

4. Empirical strategy

Model

To measure the effect of obtaining a diploma on subsequent earnings, we need to correct for the ability bias. In particular, abler pupils tend to obtain more diplomas than less able ones and thus self-select into the treatment group. To deal with this selection bias, we exploit a compulsory schooling age change in the Netherlands and identify exogenous variation in educational attainment.

The 'Qualification Law' of August 1, 2007 dictates that pupils cannot leave compulsory education until their eighteenth birthday or until they have obtained an upper secondary education certificate. This policy replaced the law of 1969 that prescribed compulsory schooling until the age of seventeen. Consequently, the extension of compulsory schooling from the age of seventeen until

the age of eighteen creates an opportunity to circumvent the ability bias and study the effect of obtaining a diploma on earnings in an Instrumental Variables (IV) framework. More specifically, the treatment variable that assigns students into pupils who have obtained a diploma and people who have dropped out of school can be instrumented by the compulsory education age change.

Our approach closely follows the approach by Harmon & Walker (1995). We can identify two groups. On the one hand, pupils who were born before August 1, 1990 form the comparison group. These pupils are seventeen years old on the date of the policy reform and are obliged to stay in school until their seventeenth birthday. Thus, they are not subjected to the Qualification Law. On the other hand, pupils who were born after August 1, 1990 form the treatment group. These pupils are subjected to the Qualification Law and are therefore obliged to stay in school until their eighteenth birthday.

The IV estimate is given by the following equation (Devereux & Hart, 2010):

$$IV = \frac{E(earnings|Z=1) - E(earnings|Z=0)}{E(qualification=1|Z=1) - E(qualification=1|Z=0)} \quad (1)$$

In equation (1), Z is a dummy variable given value of 1 for the 1991 cohort affected by the raise in the compulsory education age, and 0 for the 1990 cohort not affected by the raise in the compulsory education age.

In practice, the model is estimated in two stages (Two Stage Least Squares – 2SLS) and can be formulated as follows (Devereux & Hart, 2010):

First stage: $D_i = \alpha_1 + \varphi_1 C_i + \gamma_1 X_i + \varepsilon_i$ (2)

Reduced form: $\log(Y)_i = \alpha_2 + \varphi_2 C_i + \gamma_2 X_i + \varepsilon_i$ (3)

$$\text{Second stage: } \log(Y)_i = \alpha_3 + \delta_{2SLs} \widehat{D}_i + \gamma_2 X_i + \eta_i \quad (4)$$

In equations (1) and (2), D_i is a dummy variable given value of one if pupil i obtained a diploma and zero otherwise, C_i is a dummy variable given value of one if pupil i was born after August 1st 1990 and was subjected to the extended compulsory education age of eighteen years, X_i is a vector of covariates (gender, nationality, age, and age squared), and ε_i and ϵ_i are error terms. In equation (4), $\log(Y)_i$ represents the logarithm of a pupil's daily wage at the age of 22, and \widehat{D}_i are the fitted values per pupil estimated in the first stage. The coefficient of interest is δ_{2SLs} which represents the causal effect of obtaining a diploma on earnings.

5. Data

Data has been provided by the Central Bureau for Statistics (CBS). It is a unique and complete administrative dataset that includes all vocational students in the Netherlands from 2003 until 2014. Moreover, it is a panel dataset indicating that if an individual is missed in any one year due to unemployment, he can reoccur in the dataset in later years. This places our dataset in sharp contrast with most studies using survey data that are repeated cross-sections.

We solely focus on students in the vocational education track MBO as this track traditionally has highest dropout rates (Cabus & De Witte, 2011). Students with incomplete information, as well as students in adult education³ have been removed from the analysis. Following the approach by Harmon & Walker (1995), we focus on two cohorts: the 1990 cohort not influenced by the raise in the school leaving age, and the 1991 cohort that is the first cohort influenced by the raise in the

³ These students have returned to education and are therefore older. Consequently, we cannot measure their outcomes at the age of 21 as we do for other students (see *infra*).

school leaving age⁴. The obvious advantage of using only two cohorts is the higher credibility of the estimates due to fewer years between the before and after group⁵.

The endogenous variable of interest is qualification attainment. Traditionally, earlier literature has used a continuous measure (years of schooling) to estimate the returns to education. However, this may not be the most adequate way of measuring the educational benefits of compulsory schooling laws. Namely, in many European countries, the objective of compulsory schooling laws is not to increase the number of years in education, but to increase the number of attained qualifications (Grenet, 2013). These measures differ as individuals with the same number of years in education can have different educational outcomes (Battistin, De Nadai, & Sianesi, 2014). In the Netherlands, as the name of the law suggests, the Qualification Law of 2007 had as main objective the increase in attained qualifications and not the increase in the number of years in education (Ministry of Education, Culture, and Science, 2017b).

A diploma is defined as a certificate upper secondary education, or ISCED level 3. This variable is given value of one if the student obtained an upper secondary education qualification at the age of 18, and value of zero if the student either passed to the next year without a diploma, or dropped out of education between the age of 18 and the age of 23⁶. If a student already obtained a qualification before the last observation in the data, he/she retains this qualification in the analysis. Thus, we

⁴ Pupils' enrolment is registered on October 1st. However, a school year in the Netherlands starts on August 1st and ends on July 31st. Therefore, the registration takes place after the start of a school year. To account for this, we redefine the cohorts to start from October 1st until September 30th. For instance, cohort 1990 includes students who were born from January 1st 1990 until September 30th 1990, but also students who were born from October 1st 1989 until December 31st 1989. This cohort assignment is used both by the Dutch government (Government of the Netherlands, 2017) as in empirical practice (Grenet, 2013).

⁵ We have also performed the analyses for cohorts 1988 until 1992. The results are not particularly different and are available upon request.

⁶ Harmon & Walker (1995) have also used an upper limit for diploma attainment. From a theoretical perspective, younger people can collect the returns of education over a longer period and have a greater incentive to invest in education (Becker, 1962). Consequently, the percentage of individuals who return to education at an older age should be rather low.

take students' past into account. The instrument is a dummy variable given value of one if the student belongs to the 1991 cohort, and value of zero if the student belongs to the 1990 cohort. The control variables include dummy variables gender (male = 1), and ethnicity (Dutch = 1). Given that earnings typically increase with age at a decreasing rate regardless of the compulsory schooling laws (Becker, 1962), we include age and age squared to account for experience. The outcome of interest is the daily wage at the age of 21 calculated as yearly earnings divided by the number of days worked in a given year. Consistent with the literature, we employ the natural log of daily wages.

Table 1: Descriptive statistics

	Mean	Std. Dev.	Minimum	Maximum
Diploma (diploma=1)	0.23	0.42	0	1
Eligible for compulsory schooling (yes=1)	0.39	0.49	0	1
Gender (male=1)	0.71	0.45	0	1
Ethnicity (not Dutch=1)	0.17	0.53	0	1
Age	18.58	0.28	18.08	19.00
Total observations	7,042			

Table 1 presents the descriptive statistics. The final dataset consists of 7,042 observations. Only 23% of students has obtained an upper secondary vocational education degree in our sample. Furthermore, 71% of the sample are males and 17% are immigrants.

6. Results

In what follows, we present the first stage, reduced form, and second stage results. We also present the OLS estimates as a comparison. However, as previously noted, OLS estimates are likely to be influenced by the ability bias in which abler students self-select into the graduate group, and by the discount-rate bias in which students with high discount rates self-select into the school dropout group. Another source of bias may include measurement error in the education variable. However, given the nature of our data, we expect this bias to be rather small.

First stage

Table 2: First stage results

	Diploma
Compulsory schooling (yes=1, no=0)	0.096*** (0.011)
Gender (male=1, female=0)	-0.091*** (0.012)
Ethnicity (not Dutch = 1)	-0.044*** (0.010)
Age	0.074*** (0.019)
F	57
N	7,042

Note: *** indicates $p < 0.01$

Table 2 shows that the raise in the school leaving age had a significantly positive effect on educational attainment. In particular, after the compulsory education age change, the number of

graduates increased by 9.6%. Furthermore, both males and immigrants have a lower probability of obtaining a diploma by 9.1% and 4.4%, respectively. Moreover, the F statistic testing the hypothesis that the instrument does not enter the first stage regression is higher than the cut-off value of 10 in both regressions⁷. This indicates that the instrument is strong.

Reduced form

Table 3: Reduced form results

	Log (wage)
Compulsory schooling (yes=1, no=0)	0.013 (0.010)
Gender (male=1, female=0)	-0.091*** (0.012)
Ethnicity (not Dutch = 1)	-0.044*** (0.010)
Age	0.074*** (0.019)
N	7,042

Note: *** indicates $p < 0.01$

Table 3 shows that the effect of compulsory education on daily earnings is small (1.3%) and insignificant. This provides an indication of possible zero returns to schooling in the second stage.

⁷ It is possible that F statistic is artificially large due to our large sample size (Staiger & Stock, 1997). However, LIML estimates (available upon request) which hold the F-statistic constant in expectation as the sample size increases provide similar results.

Second stage

Table 4: Second stage results for wages three years and one year after graduation

	Log (wage)	Log (wage)
Diploma (yes=1, no=0)	0.161*** (0.014)	0.024 (0.131)
Gender (male=1, female=0)	0.497*** (0.014)	0.489*** (0.018)
Ethnicity (not Dutch = 1)	-0.173*** (0.012)	-0.177*** (0.013)
Age	0.152*** (0.023)	0.159*** (0.025)
Method	OLS	IV
N	7,042	7,042

Note: *** indicates $p < 0.01$

Table 4 presents the second stage estimates of the returns to education. OLS estimates show a positive effect of a diploma on earnings amounting to 16.1%. However, as previously noted, these estimates are likely biased. Once we correct for the bias using an Instrumental Variables framework, we observe no effect of education on subsequent earnings. Furthermore, females incur a wage penalty of 48.9% on average. Similar income loss of 17.7% holds true for individuals of non-Dutch nationality.

7. Mechanisms

Alternative explanations

Earlier literature divides possible mechanisms for zero returns to schooling into four groups: wage rigidity, signaling, change in school quality, and local effects (Grenet, 2013; Pischke & von Wachter, 2008). The first mechanism posits that wage-rigidity prevents graduates from turning their higher productivity into higher wages. One possible way for wage-rigidity to result into zero returns to education may be that institutions in the Netherlands prevent education from translating into higher wages. However, an implication of this argument is that employers then have an incentive to hire graduates that are assumed more productive, instead of high school dropouts that are less productive and equally expensive. To test this, we estimate the reduced form specification with employment at age 21 as the dependent variable. The results in **table 5** show that compulsory education age change had no effect on employment probability.

Table 5: Second stage results for employed on October 1 at the age of 21

	Employed on October 1
Compulsory schooling (yes=1, no=0)	0.011 (0.042)
Gender (male=1, female=0)	0.006 (0.006)
Ethnicity (not Dutch = 1)	-0.032*** (0.004)
Age	0.013* (0.008)
N	7,042

Note: * indicates $p < 0.10$, *** indicates $p < 0.01$

The second mechanism posits that graduates are not at all more productive than dropouts. In the Dutch education system, the signal may be vocational track chosen rather than the actual diploma. Consequently, the compulsory education age change would not affect the signaling value of vocational education. However, evidence in the Netherlands suggests that education may also have a causal effect on non-pecuniary outcomes such as health (Groot & Maassen van den Brink, 2007) and crime (Groot & Maassen van den Brink, 2010). The third mechanism assumes a change in school quality. However, this is rather unlikely as suggested by the rather unchanged pupil/teacher ratio from 2007 (13.317) until 2008 (13.182) (The World Bank, 2017).

The fourth mechanism for zero returns to education could be that we only estimate a Local Average Treatment Effect (LATE). In particular, we estimate returns to education for compliers. These students obtained a diploma due to the policy change and would have not done so without the policy change⁸. We thus do not observe the results for always-takers (students who would have obtained a diploma regardless) and never-takers (students who would have not obtained a diploma regardless). However, given that the policy reform was implemented at a national level, and that LATE should converge to ATE as the number of compliers increases (Oreopoulos, 2006), it is unlikely that this mechanism results to zero returns to education in the Netherlands.

⁸ This is only true if monotonicity holds. This assumes the absence of defiers. These students did not obtain a diploma due to the policy change and would have done so in the absence of the policy. This assumption is likely to hold, because, after the compulsory education age change, law forbids dropping out of school. To enforce implementation, the Dutch government devised an improved registration of pupils; assigned truant officers if the child stays absent from school for a longer period, and allowed truant officers to fine parents of the truant in case the law is breached. Therefore, it is unlikely those pupils opted out of the treatment because of the policy implementation and that the monotonicity assumption is breached (Dutch Ministry of Education, Culture, and Science, 2017c).

Probable explanation

We propose a novel mechanism that may lead to zero returns to education. The vocational system in The Netherlands has been modularized (a curriculum has been divided into small homogenous parts) (CEDEFOP, 2015). It is therefore possible that modules completed during a curriculum increase productivity, without students obtaining the actual qualification. Consequently, those modules may be relevant for the job and we should not see any difference in employment rates or earnings.

8. Conclusion

The overall objective of this study was to estimate the private returns to vocational education in the Netherlands. Hereby, we used an Instrumental Variables (IV) framework to exploit, for the first time, the ‘Qualification Law’ of August 1, 2007 that extends compulsory education from the age of 17 until the age of 18. Our results show no evidence of benefits to a Dutch vocational education diploma on earnings. Ruling out various potential mechanisms, we suggest that the explanation lies in the specificity of the Dutch modular vocational system. A modular vocational system may result in students enhancing their productivity in some parts of the curriculum needed to reside in the labour market, without obtaining the actual qualification.

It should be noted however that there are several limitations to our study. The main limitation is that this before-after analysis does not include a control group. Consequently, we cannot neutralize the systematic inter-cohort changes in educational attainment and earnings (Grenet, 2013). Any other change that affects the cohorts differently will lead to biased estimates (Oosterbeek & Webbink, 2007). For example, the financial crisis in 2008 could be influencing our results. The second limitation is that, although we find no returns to education in the vocational track, we make

no statement about the actual value of the compulsory schooling law. On the one hand, we only estimate the effect of education on pecuniary benefits; we do not consider other possible non-pecuniary effects of compulsory schooling laws such as a reduction in crime, an increase in political participation, marriage rates and others. On the other hand, raise in the compulsory schooling age may also result in a social cost as students who are forced to stay in school may interfere with learning of other students (Angrist & Krueger, 1991).

Furthermore, we only consider private returns to schooling. We do not estimate social returns to schooling which may be higher (human capital hypothesis) or lower (signaling theory) than the private ones (Acemoglu & Angrist, 2000). Lastly, it should be made clear that our results are averaged over the population of compliers. Within these individuals, there will be some who will benefit less and some who will benefit more from education. Future research should focus on these issues.

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