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Wage effects of an extra year of basic vocational education

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Abstract

Until 1975 around half of all graduates from Dutch basic vocational schools finished a 3-years program, the other half finished a 4-years program. In 1975 all 3-years programs were extended to four years. This was accompanied by an increase of the compulsory school leaving age with one year. We evaluate the long-term wage effects of this extra year of basic vocational education using a difference-in-differences approach. The control group consists of graduates from basic vocational programs that did not change in length. We find no beneficial effect from the change. This result suggests that the target group of this policy gains equally from an extra year in vocational school as from an extra year of work experience.

JEL classification: I21; I28; J24

Keywords: Vocational education; Returns to education; Difference-in-differences

1. Introduction

Policymakers often express concerns about the inadequate skill levels of their low skilled citizens. Boosting the skills of this group is regarded as a means against poverty, unemployment and social exclusion (see for instance OECD, 1996). A common belief is that the skill levels of the low skilled can be improved by offering them more education or training. In relation to this it is often argued that low skilled workers should receive more general education or training because this equips them to participate in the "knowledge economy". The effectiveness of an intervention of this type is, however, unknown.

Around 1975 the Dutch government implemented a reform that did exactly what recent proposals advise. Until 1975 basic vocational education programs had a length of either three or four years. Around half of graduates from basic vocational schools finished a 3-years program; the other half finished a 4-years program. The reform extended the length of the 3-years programs to four years, thereby affecting 10% of the relevant population. The programs that already took four years were unaffected. The focus of the extra year was on general skills rather than on vocational skills. The policy document that announced the policy stated this in the following terms: "In line with this vision lie the changes in the contents of basic vocational

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education towards more general education and the change from knowledge of facts towards training in thinking and personality development'' (Grosheide and Roolvink, 1970, p. 21). The change in the program length was accompanied by an increase of compulsory education in the Netherlands from nine to 10 years thereby raising the minimum school leaving age from 15 to 16.

This paper evaluates the effect of the increased program length on the wages of graduates of the extended courses. To identify this effect we use a difference-in-differences (DD) approach. The first difference is along the time dimension: before versus after the implementation of the reform. To make the before–after comparison credible, there should not be too many years between the before and after groups. In the analyses we present results comparing graduates from at most 5 (10) years before the reform to graduates from at most 5 (10) years after the reform. The second difference is based on a comparison of graduates of the programs that were subject to the change (treatment) to graduates of programs that were very similar but unchanged (control). The natural candidates to serve as the control group are graduates from the basic vocational programs that were unaffected by the reform because their pre-1975 duration was already four years.

As a percentage of the relevant age cohorts, the treatment group equals around 10%, and the control group around 8%. To acquire sufficient numbers of observations for treated and controls we have to rely on (semi-) administrative data. We use data from the Wage Structure Survey from 1995. This data set contains administrative data on wages, gender, age and job characteristics. For around 100,000 observations this data set is supplemented with survey data on their education. The main advantages of this data set are the large number of observations and the accuracy with which the available variables are measured. Most importantly, the education information is at the level of separate programs so that we know whether an individual graduated from a basic vocational program, and if so whether this program was affected by the reform or not.

The main shortcoming of the data set is that we do not know the year of graduation nor how many years an individual stayed in school. As a result we have no direct information as to whether a person who graduated from a program that was affected, finished the 3-years or the 4-years version of this program. Instead we use the information on individuals' month and year of birth to infer which version of the program someone was (intended to be) exposed to (see Section 4 for details). It is important to notice that the absence of information on actual years of schooling prevents us from estimating a first-stage relation of the effect of the reform on actual years of schooling. We do, however, report results from a mechanical first stage.

Various other studies have also exploited changes in compulsory school laws to obtain credible estimates of the wage effect of an extra year of schooling. The next section provides a brief summary of recent studies dealing with this topic. A distinguishing feature of the reform examined in this paper is the simultaneity of the extension of compulsory school leaving age and program length. This allows us to identify which graduates have been affected by the extension of the compulsory school leaving age. Most, but not all, other studies cannot identify the individuals affected.

Our results point to small and sometimes even negative wage effects for the graduates of the extended programs. This result suggests that the target group of the reform gains equally from an extra year in vocational school as from an extra year of work experience. While this result contrasts with most of the literature about returns to schooling, it is consistent with the findings from some other recent studies estimating returns to basic (vocational) programs.

The remainder of this paper is organized as follows. Section 2 reviews recent studies that also exploit changes in compulsory school laws to obtain credible estimates of the wage effect of an extra year of schooling, and discusses how the current paper relates to these studies. Section 3 provides further details about the reform and the background. Section 4 describes the data and Section 5 the empirical strategy. Results are presented and discussed in Section 6. Section 7 discusses and concludes.

2. Related studies

The seminal paper that uses features of compulsory schooling laws in the US to estimate returns to education is Angrist and Krueger (1991). Due to these features the minimum amount of schooling for someone who leaves school as soon as this is allowed, differs by the quarter of birth (and also across states and cohorts). Angrist and Krueger report IV-estimates of the returns to education of about 7.5%; these estimates tend to be slightly above the OLS-estimates obtained using the same data.

Harmon and Walker (1995) exploit changes in the minimum school leaving age in the UK to estimate the return to an extra year of schooling. The relevant changes are increased from 14 to 15 in 1947 and from 15 to 16 in 1973. The first change led to an average increase in the amount of schooling for working aged men in 1978 to 1986, of 0.54 year. The effect of the second change equals 0.11 year of schooling. The combined effect of these changes in the amount of schooling on earnings is 15% per extra year of schooling. The IV-estimate is substantially above the OLS-estimate of 6.1%. Since the earnings equations only include quadratic controls for age, it can be argued that the dummies for the changes in the minimum school leaving age capture higher order effects of age or other cohort effects (cf. Card, 1999).

Vieira (1999) replicates the analysis of Harmon and Walker (1995) using changes in compulsory schooling laws in Portugal as instruments. The minimum school leaving age increased from 11 to 12 in 1956 and from 12 to 14 in 1964. The results show substantial effects of these changes on the amount of actual schooling and on wages. According to the estimates an extra year of schooling caused by the reforms increased wages by about 5%. Interestingly, this IV-estimate is below the OLS-estimate of about 7.5%. Obviously, the same concerns about the validity of the instruments apply to this analysis.

Aakvik, Salvanes and Vaage (2003) use an increase in the amount of compulsory schooling from seven to nine years in Norway to identify the wage effect of an extra year of schooling. To identify effects for different levels of education, they interact the reform indicator with information about local availability of schools of different levels. Since the reform was not implemented in all municipalities at the same time, they can use a DD approach. For an extra year of the lowest level of vocational education a return of less than 1% is reported. For the next level of vocational education a return of 3–4% per year is found. For higher levels of education (upper secondary to university) returns are substantially larger.

Meghir and Palme (2003) evaluate a social experiment in Sweden. The experiment consisted of three ingredients: the number of years of compulsory schooling was increased from seven or eight to nine years, streaming was delayed, and means-tested subsidies for education were provided. The experiment started in a few municipalities only and was then rolled out to other municipalities. Since selection of municipalities was not random, Meghir and Palme use propensity score matching to correct for differences in a rich set of observable characteristics. The key findings are that: (1) the reform increased participation in education among students with unskilled fathers, especially for such students with below median ability, and (2) overall earnings increased, which is mainly due to a large impact of the reform on earnings of individuals with above median ability and unskilled fathers. Taken together, these results suggest that extra education obtained by those with low ability did not significantly affect their earnings.

Oreopoulos (2003) analyses changes in school leaving laws for the US, Canada and the UK thereby concentrating on the effects for dropouts. For the UK he uses the increase in the school leaving age from 14 to 15 in 1947. This reform raises the school leaving age of students with less than high school by almost half a year, but does not significantly affect the school leaving age of students with more than high school. The students with less than high school experienced an earnings increase of 5.2% as a consequence of the reform. For Canada and the US school leaving laws are not only different over time but also across provinces or states. Oreopoulos uses this to obtain DD estimates of the effects of changes in compulsory schooling for dropouts in these countries. Increasing the minimum school leaving age has substantial positive effects on the number of years of schooling of dropouts in the US and on the highest grade attended by dropouts in Canada. Also the wage effects for dropouts in both countries are substantial.

Pischke (2003) studies the impact of the length of a school year on earnings. To identify this impact he uses variation in length of the school year resulting from short school years in West Germany in 1966–1967. Prior to these years the timing of the school year differed across states. To unify this timing, many states went through an episode of short school years giving students up to two thirds of a year less time in school. This reform caused variation in length of the school year across cohorts, types of secondary school track and states. The results indicate that the short school years had no adverse effects on the number of students attending the highest secondary school track or on later earnings.

Finally, Pischke and Von Wachter (2005) study an intervention very similar to the intervention that we investigate. Also in Germany the duration of the basic track was extended by one additional year. But whereas the change in the Netherlands took place in one specific year, the exact timing in Germany varied across states. This difference in timing across states is exploited by Pischke and Von Wachter in a DD framework. Their key finding is also very similar to our finding: there is no return to compulsory schooling in terms of higher wages. Their favored explanation for this result is that the basic skills most relevant for the labor market are learned earlier in Germany than in other countries.

Our analysis differs from most of the studies summarized above because it aims to identify the effect of a precisely defined extension of an education program, namely an extension of 3-years vocational programs with an extra year of primarily general training. Only the studies by Aakvik et al. (2003) and Pischke and Von Wachter (2005) are comparable in this respect as they estimate returns to specific levels of education.

3. Background and reform

The reform evaluated in this paper took place in 1975. We therefore describe the system of Dutch secondary education prevailing at that time.[1] After six years of primary school (at the age of twelve), pupils entered a highly differentiated system of secondary education. The first differentiation is between general and vocational programs. Within the general track, programs varied in length from three years to six years.[2] Also within the vocational track programs varied in length and could take either three or four years. Vocational programs also differed in the occupations or industries for which they prepared their students. The main fields were: technical, domestic, economic, agricultural and commercial. The first year of all basic vocational programs had a more general orientation.

Assignment of students to the general track versus the vocational track is mainly determined by their ability as perceived by primary school teachers. The same is true for the allocation of students within the general track over the programs of different length. The allocation of students within the vocational track over programs with different duration and different fields is mainly driven by differences in motivation and the future vocation a student wants to pursue.

In 1975 the length of the 3-year basic vocational programs was extended to four year, while the length of the 4-year basic vocational programs was unchanged. This reform was the outcome of a debate on equal opportunities for students with low social backgrounds. It was thought that these students would benefit from increasing the general contents of basic vocational programs. The reform therefore incorporated that all schools of basic vocational education should implement a second general year. The extension of the 3-year basic vocational programs was accompanied by an increase of full-time compulsory education from nine to 10 years (Hulst & Van Veen, 2000). This meant that from that year onwards also 15-year olds have to attend full-time education.

After the reform the first two years of all basic vocational programs consisted of education in general subjects. These years were similar for all types of basic vocational education thereby enabling students to switch from one type of education to another during this period. In total, students attended 29 or 30 weekly lessons of 50 min during four years. In the first year, at least 25 lessons in general subjects were attended and in the second year at least 20 (cf. Wolthuis, 1999).

Table 1 lists the various basic vocational programs by their length prior to 1975. All domestic and economic/administrative programs had a pre-1975 length of three years while all agricultural and commercial courses had a pre-1975 length of four years. Within the broad field of technical studies, programs varied in length, some had a length of three years, others of four years.

The implementation of the extension of basic vocational education started in 1973. Starting August 1, 1973 all basic vocational programs had a length of four year. Students who at that date were

[1]Although it should be noted that the basic structure is still in place.

[2]Before 1975, students at the lowest level of general education (MAVO) could either follow a 3-years or a 4-years program. Together with the change studied in this paper, these 3-years programs were also extended to four years in 1975. A similar type of evaluation of this change is not possible because after the change everyone follows the same 4 years program so that we cannot identify those who are affected by the change and those who are not. We mention this because it explains why we cannot use graduates from this type of education as an alternative control group.

Table 1
Basic vocational programs by before-1975 length

Type of education	Duration Before 1975	
	Three year	Four year
Technical education		
Confectioning, Bread-baking, Graphic techniques, Metal working, Lay bricks, Furniture making, (House-) painting, Shoe making, Carpeting	X	
Electro techniques, Fine metal working, Installation techniques, Motor car techniques and Process technique		X
Domestic education	X	
Agricultural and horticulture		X
Economic and administrative education	X	
Commercial education		X

Table 2
Graduates from full-time secondary education by year

Year	Total	Basic vocational				General
		Technical	Economic	Domestic	Comm.	
1970	69 772	35 103	524	23290	1248	49 193
1971	81 927	36 391	5004	28183	1449	61 338
1972	83 559	35 625	5043	30699	1600	64 661
1973	86 801	36 514	5895	30837	1646	59 758
1974	87 247	37 102	4294	31702	1704	61 102
1975	***37 948***	***24 714***	***3305***	***2297***	***1878***	***68 457***
1976	80 736	34 320	6873	26733	1919	70 477
1977	87 360	36 103	8288	28110	1833	72 088
1978	89 964	36 946	8874	27664	1860	75 677
1979	91 847	37 355	9336	27384	1820	73 250

Source: CBS, Statline. *Note*: This table shows the number of students graduating from secondary education by type of program general versus basic vocational (subdivided by type of program). Year of reform is 1975.

beyond the second year could still graduate from a 3-years program. Students who started a 3-years program of basic vocational education on August 1, 1971 could still graduate in 1974. All the following cohorts had to do a 4-years program. Hence students who started on August 1, 1972 could not obtain their diploma before 1976.

Historic accounts of this episode in Dutch education (Meijers, 1983; Wolthuis, 1999) confirm that the reform was implemented from one year to the next without any indication of schools lacking behind or schools being ahead. As a consequence, the outflow of graduates from basic vocational programs was much smaller in 1975 because in that year only students from the 4-years programs could graduate. The pattern in Table 2 corroborates this.

The table shows the numbers of graduates from different types of education during the period 1970–1979. For both general programs and basic vocational programs we observe an increasing pattern of the numbers of graduates during the 1970s. The important feature of Table 2 is that the outflow from basic vocational programs dropped by more than half between 1974 and 1975 and is back at its 1974-level in 1976. This is consistent with immediate and full implementation of the reform and the share of the extended basic vocational programs slightly above that of the non-extended basic vocational programs (see also Table 3 in the next section). We see this as evidence that there was perfect compliance to the reform; the entire target group of the reform—students in the basic vocational programs that were extended—attended an extra year of (mainly general) instruction.

The immediate implementation of the reform is also reflected by the sharp increase of total expenditures on basic vocational schools. Between 1974 and 1975 these expenditures rose by an extra

Table 3
Distribution (in %) of males by highest level of education attained per cohort

Cohort	1953	1954	1955	1956	1957	1959	1960	1961	1962	1963
1. Primary	8.9	7.8	8.1	7.2	6.9	6.1	6.5	6.5	7.2	6.4
2. Ext. basic technical	8.8	8.5	8.0	8.5	7.8	7.6	7.2	8.8	8.7	8.8
3. Ext. other basic voc.	1.3	1.1	1.5	1.6	1.3	2.2	1.3	1.5	2.0	2.1
2+3	10.1	9.6	9.5	10.1	9.1	9.8	8.5	10.3	10.7	10.9
4. Not ext. basic tech.	3.5	3.5	2.9	3.5	3.9	3.7	4.6	3.8	3.4	4.6
5. Not ext. other basic voc.	3.9	3.8	5.4	5.2	4.5	4.6	5.0	6.3	6.4	5.7
4+5	7.4	7.3	8.3	8.7	8.4	8.3	9.6	10.1	9.8	10.3
6. 4 year sec. General	3.8	2.8	3.2	3.5	4.0	3.6	4.4	3.5	3.8	3.7
7. Intermediate voc.	41.8	39.1	40.4	37.6	39.5	39.0	39.6	39.3	40.4	41.2
8. 5/6 year sec. General	2.2	4.1	3.0	4.2	5.1	4.7	5.1	5.1	4.5	4.4
9. Higher education	25.8	29.5	27.5	28.8	27.1	28.4	26.4	25.2	23.6	23.2
Number of observations	2218	2193	2226	2156	2255	2216	2324	2248	2213	2298

Source: LSO95-data set; 1958-cohort is omitted. *Note*: This table shows for each birth cohort between 1953 and 1963 the distribution of highest education level attained. The bottom row shows the number of observations from each birth cohort in the data set.

10% relative to changes in other years and for other types of education (CBS, 1994, p. 253). This figure is consistent with about half of the students in basic vocational schools being enrolled in an extended course (which would lead to an increase with 14%) and some economies of scale.

The reform also led to a sharp drop of the share of 15-year olds who are not in full-time education from above 5% in the pre-reform years to 1% immediately after the reform (CBS, 1977, 1978). The 1% of 15-year olds who were not in full-time education after the reform are dropouts who end up with primary school as highest level attained.

4. Data

Data come from the Wage Structure Survey (LSO) conducted by Statistics Netherlands. This data set contains administrative data on wages, gender, age and job characteristics, and survey data on education of around 100,000 individuals. The sampling frame guarantees that the data set is representative for the Dutch workforce aged 16–65.

We use data from the survey of 1995. Gross hourly wage is used as dependent variable.[3] The education variable refers to the highest level of education attained and is coded according to the Standard Education Coding. This is a five-digit code; the first digit refers to the level of education and the other four digits refer to the type of education. This coding allows us to exactly identify the highest program a person attained, and if this highest program was at the basic vocational level whether this was a program that was affected by the reform. We also use age (in months) and age squared. To rule out labor force participation effects, we restrict the sample to men.

The data contain, however, no information on the year in which respondents finished their education. As a result we have no direct information as to whether persons who graduated from a program that was affected by the reform, did so before or after the reform. We have to infer individuals' treatment status by their month and year of birth. If everyone would finish his education in the nominal duration, a person's date of birth would identify exactly whether someone who graduated from an extended program did so before or after the reform. In that case, Dutch regulations regarding school attendance age would imply that everyone born before September 1959 belongs to a pre-reform cohort, whereas everyone born after that month belongs to a post-reform cohort.

Grade repeating confounds this assignment-to-treatment rule. Since our data set does not contain information on grade repeating, we do not know whether someone belonging to the last pre-reform cohort (born between August 1958 and September 1959; cohort 1958), graduated from the 3-years program without delay, or graduated from the 4-years program with a one-year delay. Because

[3]This is calculated from gross monthly labor earnings and working hours per month, corrected for holidays.

grade repeating is fairly common especially among students enrolling into basic vocational education, we decided to delete the 1958-cohort from our data.

Table 3 shows for the birth cohorts from 1953 to 1963 the distribution of males over different levels and types of highest diploma attained. The first five years (1953–1957) are before the reform; the last five years (1959–1963) are after the reform. This information comes from the LSO-data set containing around 2200 observations per cohort. For cohorts born between 1953 and 1957 we observe a steady decline of the share of people that entered the labor market with only primary education, this share stabilizes around 6.5% after 1957.[4] The share of men with a degree from an extended vocational program is fairly constant over the entire period 1953–1963 and equals around 10%. Within this group the division over technical programs and other programs is also fairly constant over the entire range of cohorts.

The table also shows that the shares of graduates of non-extended basic vocational programs are quite stable for the cohorts from 1953–1963. We tested whether the changes in the distributions over education levels from one birth cohort to the next were significantly different from zero. It turns out that only the difference between 1953 and 1954 is significant at the 5%-level and the difference between 1954 and 1955 is significant at the 10%-level. The *p*-value for the difference between the last pre-reform cohort (1957) and the first post-reform (1959) cohort (from a chi-square test) equals 0.421. This indicates that the implementation of the reform has not led to a shift in graduation patterns across different levels and types of education.[5,6]

While the omission of data on actual schooling attainment precludes estimation of a first stage relationship, we can use the attainment data to inquire whether a sufficient number of observations have been affected by the reform to detect any effect. We imputed 4 years of schooling for everyone who graduated from a non-extended basic vocational program, and 4(3) years for everyone who graduated from an extended basic vocational program and was born after (before) 1958. If we then regress years of schooling on a reform-dummy and restrict the sample to individuals who graduated from any basic vocational program, we obtain an estimate of 0.505 (s.e. 0.025) for cohorts 1953–1963; 0.547 (s.e. 0.016) for cohorts 1948–1968 and 0.570 (s.e. 0.017) for the all cohorts. These mechanical first stage results basically reflect that somewhat over 50% of graduates from basic vocational programs graduated from a program that was extended after the reform. While this method is mechanical because we do not observe actual years of schooling, it should be noticed that there are no clear reasons why actual years of schooling for the groups considered would deviate from the years of schooling assigned to them.

5. Empirical strategy

For the evaluation of the extension of 3-years vocational programs, we take hourly wages in 1995 as the relevant outcome variable. To estimate the effect of interest we adopt a DD approach. Denote by $w_{\text{ext}}^{\text{after}}$ the after the reform log wage rate of following a program that was extended from 3 to 4 years, and by $w_{\text{ext}}^{\text{before}}$ the before the reform log wage rate of following such a program. The difference $(w_{\text{ext}}^{\text{after}} - w_{\text{ext}}^{\text{before}})$ is then an estimator of the effect of the reform. This estimator is comparable to those used by Harmon and Walker (1995) and Vieira (1999). This estimator is, however, confounded to the extent that it also captures the effect of other changes that had different impacts on wages before and after the reform. To correct for that, we contrast this difference with the difference between (log) wages before and after the reform of a suitable control group. As control group we use graduates from basic vocational programs that were not extended because they already had a length of four years. We denote the second difference by $(w_{\text{not-ext}}^{\text{after}} - w_{\text{not-ext}}^{\text{before}})$, so that our DD estimator equals:

$$\delta = (w_{\text{ext}}^{\text{after}} - w_{\text{ext}}^{\text{before}}) - (w_{\text{not-ext}}^{\text{after}} - w_{\text{not-ext}}^{\text{before}}).$$

[4]Notice that the reform does not necessarily reduce the share of people with only primary education. On the one hand there is an extra year of compulsory education making it less likely that people end up with no more than primary education. On the other hand the lowest levels of education above primary school are now more demanding because these programs require an extra year.

[5]The same stable pattern of the distribution of highest grade attained for these cohorts is also found using the LSO survey from 1997. This survey can, however, not be used for our main analyses because it does not contain information on month of birth making it impossible to assign observations to school year cohorts.

[6]While in many countries the 1970s were years in which education participation substantially increased, this expansion occurred in the Netherlands much later, and is thus not reflected in Table 3.

Graduates from the non-extended basic vocational programs constitute a suitable control group because they enter the same segments of the labor market. An alternative control group would be graduates from the lowest level of secondary general education. Using this group as a control group is, however, not feasible because the program for a part of this group was also extended from 3 to 4 years in 1975. Before the reform there were 3 and 4-year basic general programs and after the reform there are only 4-year programs. The data do, however, not allow us to identify who graduated from a basic general program that was extended from 3 to 4 years and who graduated from a non-extended basic general program.

In practice we estimate δ using regression analysis in which we also include controls for age, age squared and cohort, and main effects for before/after cohorts and extended/not-extended programs. Standard errors are corrected for clustering at the cohort level, which is the unit of treatment. Estimates are presented for three different (sub-) samples. The first sub-sample consists of individuals belonging to the school-year cohorts 1953–1963. The second sub-sample consists of school-year cohorts 1948–1968, while the third sample covers all cohorts. The idea behind limiting the sample to cohorts close around the 1959-cohort is that this makes the before and after groups more equal, obviously in terms of age but probably also in the educational programs they could follow, except of course for the changes due to the reform.

The DD approach relies on the parallel trend assumption. The after–before difference for the unaffected programs measures what would have been the after–before difference for the affected programs in the absence of the reform. This identifying assumption cannot be tested, but we can test whether the wage–cohort relation was similar for these two groups before the change. This turns out not to be the case for all combinations of sub-samples and field of study (technical and non-technical). Consequently, we extend the regressions with an interaction term of the dummy for extended/not-extended programs and a continuous cohort variable. This interaction should capture the effect of the difference in cohort-wage profiles, but comes at the cost of reducing the precision of our estimate of δ. This can be thought of as a triple difference estimator as it takes a third difference into account.

6. Results

Figs. 1 and 2 provide a graphical illustration of the mean log wages for all cohorts born between 1948 and 1968 by type of basic vocational program. Fig. 1 shows mean log wages for extended and non-extended technical programs. Fig. 2 shows mean log wages for extended and non-extended other programs. Eyeballing Fig. 1 suggests that log wages of graduates from the extended technical programs are quite similar to these of graduates from the not-extended technical programs. This is true prior to the reform as well as after the reform. Fig. 2 reveals a constant wage differential between graduates from extended other (i.e. non-technical) programs and graduates from non-extended other programs. This difference appears to be of similar magnitude before and after the reform. Figs. 1 and 2 thus suggest that the extension of 3-years programs had no impact on the (log) wages of the graduates from these

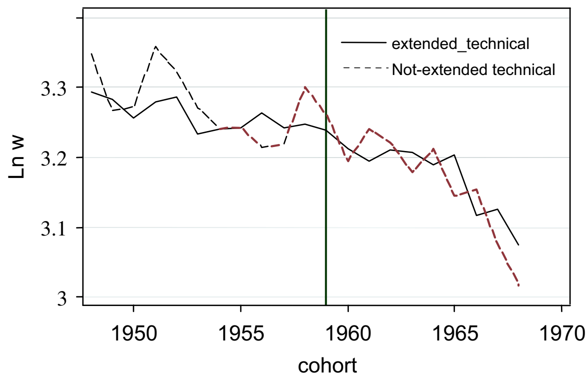

Fig. 1. Mean log wages by cohort and extended versus not-extended program, technical programs.

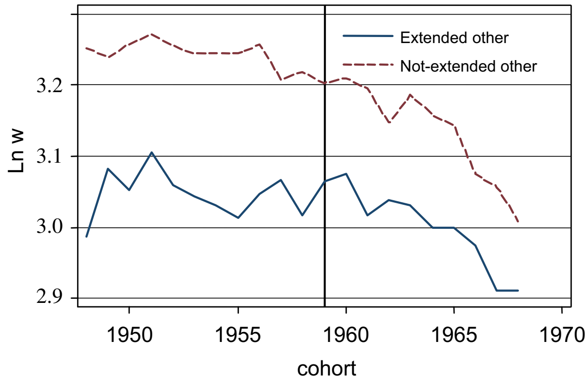

Fig. 2. Mean log wages by cohort and extended versus not-extended program, other programs.

programs. The remainder of this section presents estimates of the exact size of this effect together with their standard errors.

Before we present the DD estimates of the effect of an extra year of basic vocational education, Table 4 first presents simple before–after estimates. Rows relate to different sub-samples, columns to different fields of study. The first column gives results for the extended technical programs. For the sub-sample closest around the reform the point estimate is negative but not significantly different from zero. For the larger sub-sample (C48–68) and for all cohorts together the point estimates suggest that there is a small positive return to the extra year of technical education. For the regression based on all cohorts this estimate is significant at the 5%-level.

For the extended non-technical programs the second column reports an effect of basically zero for the smallest sub-sample (C53–63). For the larger sub-sample and for the entire sample the point estimates are positive, and the estimate based on all cohorts is significantly different from zero.

Taking the technical and non-technical extended programs together, we find a small negative but insignificant effect for the smallest sub-sample. For the other two samples the estimates in the final column are both positive and statistically significant. According to the estimates, the return to an extra year of basic vocational education for males is 3–4%. For the entire sample of men (all cohorts and all levels of education), estimating a Mincer equation gives a precisely estimated return to a year of schooling of 6.7% (this regression is not reported in a table).

Table 4
Effect of extra school year on log wage rate; before–after equations

	Extended technical programs	Extended other programs	All extended courses
C53–63	−0.022	0.006	−0.013
	(0.022)	(0.019)	(0.019)
	[1845]	[353]	[2198]
C48–68	0.024	0.034	0.029*
	(0.014)	(0.034)	(0.015)
	[3573]	[638]	[4211]
All cohorts	0.030**	0.061*	0.039**
	(0.014)	(0.031)	(0.015)
	[5132]	[852]	[5984]

Note: Each coefficient comes from a separate regression that also includes age and age squared; standard errors corrected for clustering by cohort in parentheses, number of observations in square brackets; sample is restricted to men; 1958 cohort is excluded. */** indicates significance at the 10%/5%-level.

The estimates reported in Table 4 are obtained through a method similar to the one used in Harmon and Walker (1995) and Oreopoulos (2003) for the UK, and Vieira (1999) for Portugal. The potential problem with these before–after estimates is that any other change that affects the cohorts born before and after 1958 differently, will also be absorbed in the return estimates and will thus bias these estimates. Table 5 reports DD estimates that should take such biases into account.

For each combination of (sub-)sample and field of study the top panel of Table 5 presents two estimates. The odd numbered columns report the usual DD estimates based on a regression equation including main effects of the before/after and the extended/not-extended indicators and controls for age, age squared and cohort. The even numbered columns report estimates from regressions that also include an interaction of cohort and the extended/non-extended indicator. First consider the results obtained without this interaction.

Column (1) reports estimated returns for extended technical programs close to zero and not significantly different from zero. The point estimates increase somewhat when we include more cohorts. For non-technical programs, the estimated returns are all negative and for the larger (sub-)samples these negative returns are significant and substantial (column 3). For all programs together the DD estimates of the returns to the extra year are close to zero and not significantly different from it.

The estimates in columns (1), (3) and (5) in Table 5 differ from the estimates in Table 4 by subtracting from the original after–before estimate the difference between the after–before wages for the non-extended programs. This procedure assumes that the after–before difference for the programs that did not change in length measures what would have been the after–before difference for the affected programs in the absence of the reform. This identifying assumption cannot be tested, but we can test whether the wage–cohort relation was similar for these two groups before the reform was implemented. We ran regressions of log wages on age, age squared, cohort (measured as a continuous variable) and the extended/not-extended dummy and an interaction of cohort and this dummy. Table 6 reports the coefficients for these

Table 5
Effect of extra school year on log wage rate; difference-in-differences equations

	Extended technical programs vs. non-extended technical programs (1)	(2)	Extended other programs vs. non-extended other programs (3)	(4)	Extended programs vs. non-extended programs (5)	(6)
C53–63	−0.015	−0.064**	−0.033	−0.034	−0.004	−0.054
	(0.019)	(0.029)	(0.042)	(0.101)	(0.014)	(0.030)
	[2682]	[2682]	[1485]	[1485]	[4167]	[4167]
C48–68	0.009	−0.048**	−0.052*	0.005	0.007	−0.018
	(0.017)	(0.024)	(0.028)	(0.049)	(0.011)	(0.019)
	[5098]	[5098]	[2833]	[2833]	[7931]	[7931]
All cohorts	0.011	−0.014	−0.103**	0.019	−0.013	0.028
	(0.016)	(0.024)	(0.026)	(0.044)	(0.010)	(0.021)
	[7223]	[7223]	[4377]	[4377]	[11600]	[11600]
C53–57, 62–63	0.002	−0.083**	−0.064	−0.268**	0.002	−0.113**
	(0.021)	(0.033)	(0.068)	(0.132)	(0.015)	(0.034)
	[1876]	[1876]	[1014]	[1014]	[2890]	[2890]
C48–57, 62–68	0.017	−0.053	−0.068**	−0.044	0.010	−0.019
	(0.019)	(0.030)	(0.031)	(0.086)	(0.012)	(0.029)
	[4292]	[4292]	[2362]	[2362]	[6654]	[6654]
All but 58–61	0.020	−0.000	−0.119**	0.007	–0.009	0.043
	(0.017)	(0.029)	(0.028)	(0.056)	(0.010)	(0.025)
	[6417]	[6417]	[3906]	[3906]	[10323]	[10323]
Control for interaction between type of program and cohort	No	Yes	No	Yes	No	Yes

Note: Each coefficient comes from a separate regression that also includes main effects for extended/non-extended program and pre/post change cohort, and age and age squared; standard errors corrected for clustering by cohort in parentheses, number of observations in square brackets; sample is restricted to men; 1958 cohort is excluded. */** indicates significance at the 10%/5%-level.

Table 6
Difference in cohort effects on log wages for non-extended vs. extended programs

	Technical programs	Other programs	All courses
C53–57	−0.014	−0.038**	−0.018**
	(0.011)	(0.019)	(0.008)
	[1301]	[650]	[1951]
C48–57	−0.007*	−0.001	−0.003
	(0.004)	(0.005)	(0.003)
	[2516]	[1269]	[3785]
C32–57	−0.001	0.007**	0.003**
	(0.002)	(0.002)	(0.001)
	[4442]	[2697]	[7139]

Note: Each coefficient comes from a separate regression and reports the effect of an interaction of type of program (extended vs. non-extended) and (continuous) cohort on log wages. Regressions also include age, age squared, a dummy for extended/non-extended program and (continuous) cohort. Standard errors are in parentheses, number of observations in square brackets; sample is restricted to men and pre-reform cohorts. */** indicates significance at the 10%/5%-level.

interaction terms. For some combinations of subsample and field of study, the results reject the null-hypothesis of equal cohort effect for the extended and non-extended programs.

To accommodate these unequal cohort effects, the even numbered columns in Table 5 report the estimates obtained from wage regressions that also include an interaction of cohort and the extended/not-extended dummy. A negative coefficient in Table 6 implies that among graduates from not-extended programs wages decrease more rapidly with cohort (younger cohorts earning less) than among graduates from extended programs. Not taking this into account (as in columns 1, 3 and 5 of Table 5) leads then to an overestimation of the effect of the reform. The estimates in the top panel of Table 5 express this. For technical programs all returns estimates are adjusted downwards. For non-technical programs the returns for the two larger (sub-)samples are adjusted upwards, thereby annihilating the large negative estimates from column 3.

For all programs together the final column reports estimates that are not significantly different from zero.

A possible explanation for the negligible effects in the top panel of Table 5 is that it takes time to fully implement the reform so that the first cohorts that were confronted with the reform could not really benefit from it. To examine this explanation, the bottom panel of Table 5 reports the results of regressions that exclude the first three cohorts immediately after the reform (1959–1961). These results come at the cost of making before and after groups less comparable. The estimates in the bottom panel are typically somewhat below those in the top panel; this invalidates the proposed explanation.

7. Conclusion

In this paper we evaluated the effect of an extension of 3-years basic vocational programs with one year of general education on later wages of graduates. We fail to find a significantly positive effect of this reform. Our best estimate is –0.018 with a standard error of 0.019, thereby excluding positive effects of 0.02 or more with 95% probability. This result cannot be explained by the fact that it took some period to fully implement the change. The different results for the simple difference specifications and the DD specifications suggest that previous results based on simple difference specifications are biased.

Our findings are consistent with those of Pischke (2003) and Pischke and Von Wachter (2005) for Germany and also with results reported by Meghir and Palme (2003) and Aakvik et al. (2003) who also report negligible effects for groups comparable with the group affected by the reform we study.

A possible explanation for zero returns is that the old 3-years program was spread out more thinly over the new 4-years program (cf. Pischke, 2003). The fact that the reform was accompanied by a substantial increase in expenditures makes this explanation in our view a less plausible one. A more credible explanation for the zero returns is that the extra year was placed in between the first (general) year and the last two (vocational) years and that the contents of the extra year were mainly general. As a result the last two years of the program were not affected by the reform.

Pischke and Von Wachter (2005) attribute the zero return to extending the basic track in Germany by one year to the fact that the affected students already possessed a relatively high level of basic (general) skills before the extension. Supportive evidence for this comes from the International Adult Literacy Survey as well as from First International Mathematics Study from 1964. People at the bottom of the skill distribution in Germany (the target group of the intervention in Germany, as well as the intervention in the Netherlands) perform much better on quantitative and mathematics tests than people in a similar position in the US, the UK and Canada. They also do somewhat better than low skilled people in Sweden, Norway and the Netherlands, but the low skilled in these countries also outperform their counterparts in the US, the UK and Canada. The explanation that Pischke and Von Wachter advance for the results for Germany may thus also explain the findings for these other European countries.

For some combinations of (sub-)samples and field of study we even report a negative return. A negative effect of an extra year of schooling is an uncommon finding. It should be realized, however, that an extra year of schooling necessarily comes at the cost of a year less of work experience. Apparently, the target group of the reform benefits at least as much from an extra year of work experience as from an extra year of schooling.

The findings of this paper suggest that individuals attending basic vocational programs do not benefit (in terms of later wages) of additional general education. This finding contrasts with current policy initiatives that aim to provide young people with minimum levels of general skills. Of course our results relate to a different period of time and a different situation, which limits their external validity. Yet, we believe that our results at least cast some doubt on the effectiveness of the current initiatives.

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