Wage effects of an extra year of lower vocational education: Evidence from a simultaneous change of compulsory school leaving age and program length

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ABSTRACT. Until 1975 lower vocational education in the Netherlands had a program length of either 3 or 4 years. In 1975 all 3-year programs were extended to 4 years. This was accompanied by an increase of the compulsory school leaving age with one year. We evaluate the long-term wage effects of the extra year of lower vocational education using a difference-in-differences approach. We find no beneficial effect from the change.

JEL Codes: I21, I28, J24
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I. Introduction

Policymakers often express concerns about the skill levels of their low skilled citizens. Boosting the skills of this group is regarded as a mean against poverty, unemployment and social exclusion (see for instance OECD 1996). Improving the skill levels of the low skilled requires them to follow more education or training. In relation to this it is often argued that low skilled workers should receive more general education or training because it equips them better to participate in the so-called "knowledge economy". The effectiveness of such an intervention is, however, unknown.

In the mid-seventies the Dutch government implemented a reform that did exactly what the current proposals aim at. Until then lower vocational education programs had a length of either three or four years. The reform extended the length of all three-year programs to four years, and left the programs that already took four years unchanged. The focus of the extra year had to be 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 lower vocational 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). This change in the program length was accompanied by an increase of compulsory education in the Netherlands from nine to ten 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 where we use as control group the graduates of the lower vocational courses that did not change in length. The analysis in this paper is related to previous studies that have exploited changes in compulsory school laws to obtain credible estimates of the wage effect of an extra year of schooling. Below follows a brief summary of this line of research.

Aakvik, Salvanas 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 0.7 percent is reported. For the next level of vocational education a return is reported of 8.6 percent for a two or three year program. 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 only a few municipalities and was then extended 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 (1) that the reform increased participation in education among students with unskilled fathers, especially for such students with below median ability, and (2) that 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 the extra education obtained by those with low ability did not significantly affect their earnings.

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 increases 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 percent per extra year of schooling; the results presented by Harmon and Walker do not allow to identify the wage effects of the two changes separately. The IV-estimate is substantially above the OLS-estimate of 6.1 percent. 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 percent. Interestingly, this IV-estimate is below the OLS-estimate of about 7.5 percent. Obviously, the same concerns about the validity of the instruments apply to this analysis.

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 no 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 percent as a consequence of the reform. For Canada and the US school leaving laws are not only different over time but also differ 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.

Related is also the paper by Pischke (2003) that studies the impact of length of the school year on student performance and earnings. To identify these impacts Pischke uses variation in length of the school year resulting from short school years in West Germany in 1966-67. Before 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.

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 three-year lower vocational programs with an extra year of mainly general training. Only the study by Aakvik, Salvanas and Vaage (2003) is comparable in this respect as they estimate returns to specific levels of education. Their identification strategy differs, however, from ours. A distinguishing feature of the reform we examine is the simultaneity of the extension of compulsory school leaving age and program length. This allows us to identify the graduates that have been affected by the reform. For our analysis we use data from administrative sources that only include information on a limited number of variables. As a result we are not able to investigate whether effects differ by social background or ability. Our results rule out a substantially positive effect of the extra year of vocational education on earnings. This finding holds up against a variety of specification checks. This finding is in line with the previous results for Norway, Sweden and Germany and differs from the results obtained for the Anglo-Saxon countries and Portugal.

The remainder of this paper is organized as follows. Section II provides further details about the policy. Section III describes the estimation strategy and Section IV the data. Results are presented and discussed in Section V. Section VI discusses and concludes.

II. Background

The policy change evaluated in this paper took place in the mid-1970s. We therefore describe the system of Dutch secondary education at that time. After six years of primary school (at the age of twelve), pupils entered a highly differentiated system of secondary education. The main differentiation is between general and vocational programs. Within the general track, programs varied in length from four years to six years. 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 that they prepared students for. The main fields were: technical, domestic, economic, agricultural and commercial. The first year of all lower vocational programs had a more general orientation.

In 1975 the law on compulsory education was changed (Hulst and Van Veen, 2000). Full-time compulsory education increased from nine to ten years. This meant that also 15-year olds had to follow full-time education. This reduced the share of 15-year olds not in full-time education from 5 percent in 1974 to 1 percent in 1975 (CBS, 1977, 1978). The increase in the compulsory school leaving age was accompanied by an extension of three-year lower vocational programs to four years.

This increase was necessary because otherwise students of such three-year programs could graduate one year before they reached the minimum school leaving age. The reform was the outcome of a debate on equal opportunities for students with a low social background. It was thought that these students would benefit from increasing the general contents of lower vocational programs. The reform therefore incorporated that all schools of lower vocational education should implement a second general year. In this year the curriculum ought to include at least 20 weekly lessons in general training.

Students born before August 1 1959 had to follow nine years of full-time education. This is also the last cohort of students that could receive a certificate after three years of lower vocational education. Students born on or after August 1 1959 had to follow ten years of full-time education. This is also the first cohort of students to encounter a complete four-year lower vocational education regime.

The implementation of the extension of lower vocational education started in 1973. Since August 1 1973 all lower vocational programs had a length of four year. Students who were beyond the second year could still graduate in a three-year course. Students who started a three-year course of lower vocation education on August 1 1971 could still graduate in 1974. All the following cohorts had to take a four-year course. Hence students who started on August 1 1972 could not obtain their diploma before 1976. As a consequence, outflow of graduates with a three-year course of lower vocational education in 1975 is much smaller. There is only outflow from four-year courses and outflow from three-year courses of delayed graduates. Table 1 shows that the number of graduates from lower vocational education in 1975 was much lower than in the previous and subsequent years.

Table 2 lists the lower vocational programs by their length before 1975. All domestic and economic/administrative programs had a before-1975 length of three years while all agricultural and commercial courses had a before-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.

III. Empirical strategy

For the evaluation of the extension of three-year 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_{ext}^{after} the after-the-extension log wage rate of following a program that was extended from 3 to 4 years, and by w_{ext}^{before} the before-the-extension log wage rate of following such a program. The difference $(w_{ext}^{after} - w_{ext}^{before})$ is then an estimator of the effect of the extension. This estimator is comparable to those used by Harmon and Walker (1995), Vieira (1999) and Oreopoulos (2003). This estimator is, however, confounded to the extent that it also captures the effect of other changes that had different impacts on after and before log wages. To correct for that, we contrast this difference with the difference between after and before log wages of a suitable control group. As control group we use graduates from lower vocational programs that were not extended because they already had a length of four years. We denote the second difference by $(w_{not-ext}^{after} - w_{not-ext}^{before})$, so that our DD estimator equals $d = (w_{ext}^{after} - w_{ext}^{before}) - (w_{not-ext}^{after} - w_{not-ext}^{before})$.

In practice we estimate d 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 to 1963. The second sub-sample consists of school-year cohorts 1948 to 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 cohort the dummy for extended/not-extended programs. 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 d. This can be thought of as a triple difference estimator as it takes a third difference into account.

We end this section with discussing how different pre-intervention groups may have responded to the reform. We assume that individuals are either insensitive to the reform and do not respond or that they comply with the reform. For would-be graduates from three-year programs, compliance implies they may dropout from the extended program. For would-be dropouts from four-year programs, compliance implies that they may graduate because they have to stay one more year in school. These rules lead to the following responses for the various groups:

1. Those who would have graduated from a three-year vocational program before the reform may after the reform either graduate from an extended vocational program or dropout because a four-year program is too long or too demanding for them.
2. Individuals who would have dropped out from a three-year vocational program are likely to remain dropouts from an extended vocational program.
3. Students who before the reform would have graduated from a four-year vocational program are not affected by the reform and remain graduates from a four-year vocational program

4. Individuals who before the reform would have dropped out from a four-year vocational program may either remain dropouts or become graduates from a four-year vocational program as a result of the increase in the compulsory school leaving age.
5. Students who before the reform would have graduated from a secondary general education or from higher education are not affected by the reform and remain graduates from these respective levels.
6. Finally, individuals who before the reform would have dropped out from a secondary general program may either remain dropouts or become graduates from such a program as a result of the increase in the compulsory school leaving age.

On the basis of these responses we thus expect that the share of graduates from an extended vocational program will remain the same or decrease, that the shares of graduates from non-extended vocational programs, secondary general programs and higher education will increase or remain the same. The share of people with only primary education (dropouts) may decrease, increase or stay the same. Results that we present in Section IV indicate that in practice the share of graduates from an extended vocational program remained the same, that the share of graduates from a non-extended vocational program increased, and that the share of people with only primary education decreased. In Section V we discuss how this pattern may affect our estimation results.

IV. Data

Data come from the so-called Wage Structure Survey (LSO) conducted by the Dutch Central Bureau of Statistics. This dataset 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 dataset is representative for the Dutch workforce aged 16 to 65.

We use data from the survey of 1995. We use gross hourly wage as dependent variable. The education variable refers to the highest level of education attained and is coded according to the so-called Standard Education Coding (SOI). 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 lower vocational program a person attended, and in particular whether this was a program that was affected by the reform or not. We also use age (in months) and age squared. To rule out labor force participation effects, we restrict the sample to men.

If everyone would finish his education within the nominal duration a person's date of birth would identify whether someone who graduated from an extended program did so before or after the extension. Grade repeating confounds this identification. Since our data set does not contain information on grade repeating we do not know whether someone belonging to the last cohort before the extension graduated from the three-year program without delay, or graduated from the four-year program with one year delay. Because grade repeating is fairly common especially among students with lower vocational education, we decided to delete the cohort born between July 1958 and August 1959 (cohort 1958) from our data.

Table 3 shows for the birth cohort 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. 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 percent after 1957. 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 percent. This overall constant share hides, however, an increasing share of technical programs and a decreasing share of other programs. The shares of technical and non-technical vocational programs change abruptly after the reform, suggesting that the extra general year made technical programs more attractive relative to non-technical programs. For our empirical analysis this implies that the before and after groups within the technical and non-technical programs may not be completely comparable. This reduces the confidence of our estimates of the wage effects for separate programs. It does however, not affect our estimate of the wage effect of the reform for graduates from all lower vocational programs together.

Table 3 shows an increase of the share of graduates of non-extended lower vocational programs after the reform. During the five years before the reform the share is on average 5.9 percent, while it is 9.6 percent during the five years after the reform. This increase is attributable to both the not-extended technical programs and the not-extended non-technical programs. This increase may confound the before-after wage difference of the control group. Given the decrease of the share of people with only primary education, it seems likely that the increased share of not-extended vocational programs is due to lower dropout rates in these programs. If those who would otherwise have dropped out are on average of lower ability than those who would graduate independent of the change in the compulsory school leaving age, then this change is likely to bias the DD estimate upwards (because it leads to underestimating $w_{not-ext}^{after}$).

V. Results

Before we present the DD estimates of the effect of an extra year of lower 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 change 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 all cohorts this estimate is marginally significant.

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 for all cohorts is significantly different from zero.

Taking the technical and non-technical extended programs together thereby capturing possible biases due to switches between programs, 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 lower vocational education for males is 3 to 4 percent. This return can be compared with an OLS-estimate from a Mincer equation of the return to a year of schooling of 0.067 (s.e. 0.000) for all cohorts of men.

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 simple difference 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 aim to take exactly these 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 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 DD 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. Table 6 shows the results of regressions of log wages on age, age squared, cohort and the extended/not-extended dummy and an interaction of cohort and this dummy. For some combinations of sub-sample 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 DD 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.

VI. Conclusion

In this paper we evaluated the effect of an extension of three-year lower 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 percent probability. This result cannot be explained by the fact that it took some period to fully implement the change. Our result may be biased due to changes in the composition in the control group. In that case, our results are likely to provide upper bounds of the true effect. 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 seem to be at odds with the many studies that report highly significant and substantial returns to a year of schooling. Explanations for this may be that the extra year of schooling did not change the highest degree obtained, and that it is conceivable that the old three-year program was spread out more thinly over the new four-year program. Pischke (2003) offers comparable explanations for his finding of no adverse earnings effect from less time in school. Our finding is consistent with that of Pischke and also with results reported by Meghir and Palme (2003) and Aakvik, Salvanas and Vaage (2003) who also report negligible effects for groups comparable with the group affected by the reform we study.

The findings of this paper suggest that individuals attending lower vocational programs do not benefit (in terms of later wages) of additional general education. This finding contrasts sharply 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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Table 1: Graduates from full-time secondary education by year

		Lower vocational				General
Year	Total	Technical	Economic	Domestic	Comm.	
1970	69772	35103	524	23290	1248	49193
1971	81927	36391	5004	28183	1449	61338
1972	83559	35625	5043	30699	1600	64661
1973	86801	36514	5895	30837	1646	59758
1974	87247	37102	4294	31702	1704	61102
1975	*37948*	*24714*	*3305*	*2297*	*1878*	*68457*
1976	80736	34320	6873	26733	1919	70477
1977	87360	36103	8288	28110	1833	72088
1978	89964	36946	8874	27664	1860	75677
1979	91847	37355	9336	27384	1820	73250

Source: CBS, Statline

Table 2: Lower 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 3: Distribution of males by highest level of education attained per cohort

Cohort	*1953*	*1954*	*1955*	*1956*	*1957*	*1959*	*1960*	*1961*	*1962*	*1963*
1. Primary	9.0	8.2	8.0	7.3	6.6	6.1	6.5	6.5	7.2	6.4
2. Ext. lower technical	6.0	5.9	5.4	5.5	5.0	7.6	7.2	8.8	8.7	8.8
3. Ext. other lower voc.	4.2	4.4	4.2	4.2	4.2	2.2	1.3	1.5	2.0	2.1
2+3	10.2	10.3	9.6	9.7	9.2	9.8	8.5	10.3	10.7	10.9
4. Not ext. lower tech.	2.3	2.3	1.9	2.2	2.5	3.7	4.6	3.8	3.4	4.6
5. Not ext. other lower voc.	3.5	3.0	4.4	3.7	3.6	4.6	5.0	6.3	6.4	5.7
4+5	5.8	5.3	6.3	5.9	6.1	8.3	9.6	10.1	9.8	10.3
6. 4 year sec. general	5.0	4.6	4.7	4.4	4.6	3.6	4.4	3.5	3.8	3.7
7. Intermediate voc.	40.5	38.0	38.8	39.1	39.8	39.0	39.6	39.3	40.4	41.2
8. 5/6 year sec. general	2.4	3.9	3.8	4.4	4.7	4.7	5.1	5.1	4.5	4.4
9. Higher education	27.1	29.7	28.7	29.2	29.1	28.4	26.4	25.2	23.6	23.2

Source: LSO95-dataset; 1958-cohort is omitted.

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.

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

	Extended technical programs vs. non-extended technical programs		*Extended other programs vs. non-extended other programs*		*Extended programs vs. non-extended programs*	
	(1)	(2)	(3)	(4)	(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.

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

	Extended technical programs	*Extended other programs*	*All extended 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 that also includes age and age squared; standard errors are in parentheses, number of observations in square brackets; sample is restricted to men.