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Changing Returns to Education Across Cohorts: Selection, School System or Skills Obsolescence?

Abstract:
This paper investigates whether economic returns to education in Norway differ across cohorts. Differences in returns to education may arise from selection effects - the large increase in educational attainment in postwar years may have changed selection into education. They may also result from changes in the school system, having been transformed towards a more egalitarian system. Finally, cohort effects may arise from skills obsolescence - technological change may make old education less worth in the labor market. The empirical results suggest that there has been a decline in the returns to education across cohorts. Controlling for self-selection into education, however, the cohort differences vanish. There is no strong evidence in favor of the skills obsolescence explanation, and no support for the hypothesis that the quality of schooling has declined over time. Cohort differences in returns to education seem to have been driven by selection effects.

Keywords: Returns to education, cohort effects, educational policy, self-selection.

JEL classification: J31

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1 Introduction

How well do new and old skills fare in the labor market? Do today's students learn more in school than their parents did, or has the quality of education deteriorated?

In most Western countries, there has been a large increase in educational attainment during postwar years. While higher education used to be restricted to a social and economic elite, people from many different backgrounds now attend universities and colleges. There have been worries, however, that the transformation from elite to mass institutions has deteriorated the quality of education (see e.g. OECD, 1997), and that today's young actually learn less in school than those who were young a generation ago[1]. Worries of crises in education have also been expressed in public debate in Norway. The debate has mostly focused on sciences (e.g. Løvlie, 1998), but also in fields like humanities experts have claimed that there has been a decline in what students know and learn (e.g. Nessheim, 1998). The decline in quality is often documented by pointing out that an increasingly larger share of beginner students at colleges and universities fail basic exams, indicating that primary and secondary schools do a poorer job than before in preparing young people for higher education and working life. On the other hand, a widespread, cross-country phenomenon is the positive trends in IQ scores over time, see Flynn (1987) and Dickens and Flynn (2001). In explaining these trends, many researchers point to improvements in the school system, see Emanuelsson and Svensson (1990).

In addition to how much knowledge students acquire, there is also a question of the relevance of what they learn. Old skills eventually become obsolete. This is even more so in a world where the pace of technological change is increasing. The Economist (2000) states that young people will be the ones benefiting the most from faster technological change: "Whatever technology is dominating, it is sure to change so quickly that those who have never known anything else will have the advantage." They also predict that young workers will be increasingly demanded, most of all because they are technologically adept, welcome change and think differently than older workers.

In this paper, I evaluate how the skills of different cohorts are rewarded in the labor market, by investigating whether there are any differences in income levels and economic returns to schooling across cohorts. In light of the discussion above, there have been surprisingly few studies focusing on cohort effects from this viewpoint[2]. Using an extended Mincer regression

[1] A famous example from the U.S. is the report "A Nation at Risk" (National Commission on Excellence in Education, 1984). Cizek (1999) and Gibbs and Fox (1999) review the American debate.

[2] Boockmann and Steiner (2000), Beaudry and Green (2000) and Card and Lemieux (2000) are exceptions. Other studies, like Welch (1979), Freeman (1979), Berger (1985), Stapleton and Young (1998) and Macunovich (1999) focus on the effects of *cohort size* on earnings.

framework, I compare returns to education and earnings levels for different cohorts. Since different cohorts are studied in the same labor market at the same point in time, the effects of labor market conditions and wage setting institutions are also controlled for. The data set covers the Norwegian birth cohorts from 1942 to 1970, who have been subject to different "educational regimes". A major challenge when estimating economic returns to education is that schooling choices are affected by many different factors which are often unobserved, like preferences, ability, financial constraints and individual heterogeneity in returns to education. As long as individuals self-select into education based on such unobserved factors, they create endogeneity problems when estimating returns to education. To identify the returns to education in this case requires econometric techniques to control for self-selection.

The main challenge in this study is to try to disentangle three possible effects:

- *Selection*: How has the increase in educational attainment affected the composition of students in terms of ability? Has there been a decline in selectivity with respect to ability, or has the increase in attainment followed from an increase in educational propensity among new groups? This has implications for the development of average student ability over time, and hence for economic returns to education.

- *School system*: Has the quality of schooling declined, so that a year in school produces less human capital that is valued in the labor market than before? Norwegian educational policy in the postwar years has been dominated by an "egalitarian", as opposed to "elitist", trend where streaming and sorting by ability have been gradually reduced, and resources to an increasing extent have been geared towards the less skilled. Given an average quality (however defined) of schooling, such a development should imply a gradual decline in educational premiums across cohorts.

- *Skills obsolescence*: If human capital is partly technology-specific, technological change will erode some human capital. This will mostly affect older workers, making their "old skills" less worth, but also low educated workers, whose skills are less generalizable, see Bartel and Sicherman (1998) and Ahituv and Zeira (2000). If skills obsolescence is important for cohort effects in earnings, one should expect to see the strongest effects in industries with high rates of technological change.

The main finding in this paper is that there has been a decline in the economic returns to schooling across cohorts. This decline is mostly the result of changing selection patterns over time. When the effect of self-selection into education is ignored, returns to education are lower

for younger cohorts. However, controlling for the effect of self-selection, estimated returns to education are found to be quite stable across cohorts. Selection-corrected returns to schooling are lower than the uncorrected estimates for older cohorts, but somewhat higher for younger cohorts. This indicates that there was positive selection into education for older cohorts, i.e. that individuals with high returns to schooling chose more schooling, while this does not seem to be the case for younger cohorts. The results give no strong arguments in favor of a declining quality of the school system itself. Looking at *earnings levels*, I find that younger cohorts *cet. par.* (also controlling for differences in labor market experience) earn more than older cohorts. This is particularly the case for individuals with low levels of schooling. An explanation that is consistent with this finding is that the egalitarian Norwegian educational policy has been fairly successful in reducing earnings inequality, particularly by increasing earnings levels of individuals with only compulsory schooling. The estimated levels effects could also be the result of skills obsolescence among old, low-educated workers. However, comparison of industries and types of workers that are assumed to be differently affected by technological change does not give any support to the skills obsolescence interpretation.

The paper is organized as follows. The next section gives an overview of educational policy in the postwar years. The model and data are described in Section 3 and 4, while Section 5 present the results. The final section provides conclusions and points out directions for further research.

2 Educational attainment and policy in postwar Norway: Towards egalitarianism

Norwegian educational policy: A brief sketch[3]

The major objectives of Norwegian educational policy in the postwar years have been to increase overall attainment and to reduce educational inequality by promoting equality of opportunity. One of the main policy goals was to establish a comprehensive school system to ensure that the quality of basic education was the same for all, regardless of geographic and social background[4]. The seven-year comprehensive school, which was in place until 1959, was not a complete success to this end, although a system without ability streaming had been in place since 1921. There were significant rural-urban differences, and up to 1959 different sets of legislation governed primary schooling in rural and urban areas. Curricula were different, and pupils in urban areas

[3] See also Jørgensen (1997) and OECD (1976, 1998, 2000).

[4] Primary education in Norway has always been completely dominated by public schools. There have been only a few private primary schools, and they have received public support only if their curricula were in line with those of the public school system. There is no tradition in Norway for private elite schools.

attended school every day, while this was not the case in rural areas. This led to geographical differences in the quality of basic education. During the period 1959-73, a nine-year compulsory education was gradually implemented. In the eighth and ninth grades, the last two years of the nine-year comprehensive school, there was initially streaming by ability. Pupils chose among different tracks, one theoretical for those aiming at further education, and several vocational. Believing that this system led to a too early selection, it was replaced by a system based on tempo differentiation ("kursplaner") in main subjects. Pupils and parents themselves decided which level to choose. However, also this system was believed to create a too early selection and ranking of students by theoretical ability, being counterproductive in the pursuit of equality of educational opportunity. It was therefore abolished in 1973. From then on, all pupils were in the same class, but there was room for individual differentiation, which typically meant extra teaching resources geared towards the less skilled.

Secondary education has also been subject to reforms, mainly in the seventies. The main aim of the reforms in secondary education was to reduce the difference between vocational and more theoretical educations that traditionally paved the way towards higher education. The scope of vocational education did also increase.

The reports from the Ottosen commission[5] in the late sixties played a central role in reforming higher education in Norway. It led to the establishment of regional colleges, and an expansion of shorter tertiary educations at the expense of traditional university educations[6]. The opportunities to take higher education of relatively short duration in all parts of the country was meant to ease the accessibility to higher education for youths from less favorable social backgrounds. The rationale for establishing the State Educational Loan Fund in 1947 was to induce young people from low-income families to take more education by giving them grants and favorable loans to cover their living expenses during their studies. Support to higher education from the State Educational Loan Fund was means-tested up to 1972. Means-testing for support to secondary education continued.

Trends in educational attainment

As in most developed countries, educational attainment in Norway has increased substantially over the last decades. This is seen in Figure 1, which displays average years of schooling and standard deviation by cohort, as observed in 1997. The average level of schooling has increased from about ten and a half years for the 1942 cohort to more than twelve years for the 1970

[5] See Ministry of Education and Church Affairs (1968).

[6] The trend towards a change in the student body and shorter tertiary educations was common across the OECD, see OECD (1998).

cohort. There has also been a decline in the standard deviation of education within cohorts. Figure 2 shows the distribution of years of schooling for selected cohorts. The figure shows that the increase in average educational attainment for the main part is due to a shift from primary (seven to nine years) to secondary education (ten to twelve years). It reflects the gradual extension of compulsory schooling from seven to nine years, and also shows that there has been a large increase in the proportion of the cohorts that complete twelve years of schooling.

To evaluate how social segregation in education has developed over time, educational attainment in 1997 was regressed, separately for each birth cohort from 1942 to 1970, on a range of individual background variables from the population censuses in 1960 and 1970 [7]. Educational propensity was much lower for females than males in the early cohorts, with an estimated difference of more than half a year of schooling, as shown in Figure 3. The gap has narrowed over time, and from the 1960 cohort onwards the estimated educational propensity is slightly higher for females. Figure 4 shows that family income matters for educational attainment. The estimates are quite stable across cohorts, although the effect is somewhat smaller for the youngest cohorts. The estimated effect of parental income in the highest quintile relative to the lowest quintile is 0.5-0.75 years of education. Conditional on parents' education and other background variables, this is quite substantial. It may suggest that there are capital market imperfections for educational investment, even after the expansion of student loans through the State Educational Loan Fund in the 1970s. However, it may also reflect that education in part is a normal consumption good. Figure 5 shows that the estimated effect of father's level of education is substantial. Having a father with upper tertiary education increases the expected amount of schooling with two years, relative to having a father with primary education[8]. Estimated effects of mother's education (not reported) are of similar magnitude. Figure 6 shows the estimated effect on schooling of growing up (living there in the census year) in a specific county, relative to Oslo (the capital city). The estimates are negative for older cohorts, but gradually the (conditional) gap narrows, and for the younger cohorts the educational propensity in most counties is higher than in Oslo.

To sum up, the results presented in this section suggest that the Norwegian educational policy succeeded in narrowing the gap in educational attainment (measured in years of schooling) between males and females and between regions. However, the effects of parents' income and education are large and surprisingly stable over time. Thus, while there may not be geographical

[7] See Section 4 for a description of data sources and variable construction.

[8] For father's education, we see a drop in the coefficients from the 1952 cohort onward. This is because we do not have income information for the early cohorts, and that the education variable picks up some of the income effect.

segregation, there still seems to be social and economic segregation. These findings are in line with Hansen (1997).

Resource use

Figure 7 shows that there has been a large decline in the number of pupils per teacher in primary school. In the early sixties there were around 20 pupils per teacher, while in 1995 there were about ten. The decline in the student/teacher ratio in secondary education has been smaller, while there has been an increase in tertiary education. Real expenditures per student (numbers are available from 1978 onwards) reveal a similar pattern, see Figure 8. Expenditures per student have increased for primary education, but decreased for tertiary education. For secondary education, there is no clear pattern. Compared to other countries, Norway spends relatively more per student more on the primary and secondary level than on the tertiary level (see OECD, 2000). Looking at pupils' test scores towards the end of primary education (see OECD, 2000), Norway is close to the OECD average, but the variance in test scores is much smaller in Norway than in most other OECD countries.

The information on resource use and test scores probably reflects the egalitarian principles of Norwegian educational policy, aiming at bringing low-performance pupils up to an acceptable level, at the expense of resources spent on the most talented. Since the least talented students are less likely to continue their education beyond compulsory schooling, this may lead to smaller human capital differences between school-leavers and those who continue into secondary and tertiary education. The egalitarian policy has been implemented gradually, and one may therefore expect to see that the income level of the least educated is higher the younger the cohorts. Similarly, we may expect to find lower returns to schooling at higher levels for younger cohorts.

3 Model and econometric issues

The discussion so far suggests that cohort effects may arise both in returns to schooling and in overall earnings levels. I therefore specify the following random coefficient model of log annual earnings[9]:

$$\begin{aligned} w_i &= \alpha + (\beta_1 + \varepsilon_i^s)\, S_i + \sum_{c=2}^{C} \beta_c D_{ci} S_i + \gamma_1 X_i + \gamma_2 X_i^2 + \gamma_3 X_i^3 + \gamma_4 X_i^4 \qquad (1) \\ &\quad + \delta_1 S_i X_i + \delta_2 S_i X_i^2 + \delta_3 S_i X_i^3 + \delta_4 S_i X_i^4 + \sum_{c=2}^{C} \psi_c D_{ci} + \theta Z_i + \varepsilon_i \end{aligned}$$

[9] A similar setup is used by Boockmann and Steiner (2000). However, they do not consider unobserved heterogeneity in returns to schooling.

where w_i is log annual earnings of individual i, S_i is years of schooling, D_{ci} is a dummy variable that equals one if individual i belongs to cohort c, and zero otherwise[10], X_i is labor market experience and Z_i is a vector of other variables (including dummies for each of the first five years of experience, county of residence, 2-digit industry, type of education such as technical, business/administrative etc., and parents' education and income). In addition to measurement errors in earnings, the error term ε_i represents unobserved individual characteristics ("ability") affecting *earnings* regardless of the level of schooling, whereas ε_i^s represents unobserved characteristics affecting the *returns to schooling*. Individual heterogeneity in returns to schooling is assumed to be independent of cohort[11]. The average return to education for an individual of cohort c, ignoring interaction between schooling and experience, is $\beta_1 + \beta_c$.

In this study, I apply a measure of labor market experience based on individual labor income histories (see Section 4), thereby utilizing breaks in employment and time out of work to separately identify, education, experience and cohort effects[12]. Behind such a calculation of labor market experience and the specification in (1) lies an assumption that time out of work has no role in the earnings equation, i.e. that it contains no investment in and no excess depreciation of human capital. This is not an innocuous assumption, see e.g. Mincer and Polachek (1974) and Bebelo and Wolf (2000), who suggest that time out of employment contributes to depreciation of human capital. In the earnings equation, experience is included as a quartic. Other studies have found that the returns to labor market experience increase with the level of education[13], and interaction terms between experience and years of schooling are therefore included in (1). The specification of experience effects may influence the estimated cohort parameters, since experience and cohort variables are highly (though not perfectly) correlated. If the specification of the experience profile is not flexible enough to capture the true experience effect, the unexplained part of the experience profile will partly be captured by the cohort variables. Therefore, I have experimented with a number of specifications of the experience profiles. Using even more flexible specifications than in (1) had only minor effects on the results.

Several studies on Norwegian data, see e.g. Hægeland, Klette and Salvanes (1999) and Raaum and Aabø (2000), have found that the marginal effect of schooling on earnings is not

[10] Note that I do not impose linear cohort effects, but represent different cohorts by dummies. In the empirical analysis, the following birth-year cohorts are considered: 1942-45, 1946-49, 1950-53, 1954-57, 1958-61, 1962-65 and 1966-70.

[11] Estimating a model where unobserved individual heterogeneity in returns to schooling is allowed to differ between cohorts, showed that this assumption has only minor effects on the results.

[12] In estimating earnings equations it is common to use *potential* experience, defined as *age - years of schooling - school starting age.* This leads to a fundamental identification problem when also estimating cohort effects, see the discussion in Section 4.

[13] See e.g. Hægeland, Klette and Salvanes (1999), Møen (2000) and Hægeland (2001).

constant across years of schooling. Since the distribution of years of schooling differs substantially between cohorts, see Figure 2, estimated differences in education premiums by cohort may be an artifact. Marginal returns at each level of schooling may be constant across cohorts, but different distributions of schooling across cohorts may generate cohort differences in estimated returns to schooling in a constant marginal returns model, such as (1). To see whether allowing for differences in marginal returns to schooling influence cohort effects, the following model has been estimated:

$$\begin{aligned} w_i &= \alpha + \sum_{c=1}^{C} \sum_{s=S^{l+1}}^{S^h} (\varphi_{sc} + \varepsilon_i^s) E_{si} D_{ci} + \gamma_1 X_i + \gamma_2 X_i^2 + \gamma_3 X_i^3 + \gamma_4 X_i^4 \qquad (2) \\ &+ \delta_1 S_i X_i + \delta_2 S_i X_i^2 + \delta_3 S_i X_i^3 + \delta_4 S_i X_i^4 + \sum_{c=2}^{C} \psi_c D_{ci} + \theta Z_i + \varepsilon_i^l \end{aligned}$$

where E_{si} is a dummy variable that equals one if individual i has schooling level s and zero otherwise, and ε_i^s now captures heterogeneity in returns to schooling at schooling level s[14].

When estimating (1) and (2) by OLS, there are two possible sources of bias. Schooling may be correlated with absolute earnings capacity or ability (ε_i). The direction of the bias in estimated returns to education resulting from this correlation is not obvious a priori, as discussed by Griliches (1977). An additional source of bias comes from ε_i^s. If individuals differ in their relative earnings capacity (or ability) at different education levels, they have comparative advantage in certain levels of education. If individuals know their comparative advantage and act upon it when choosing their level of education, they will self-select into the level of education yielding the highest return, see Willis and Rosen (1979), Heckman and Robb (1985a) and Bjørklund and Moffitt (1987). Then ε_i^s and the level of schooling will be correlated, causing a bias in the estimated returns to schooling.

A large number of studies have used instrumental variables (IV) methods to overcome endogeneity problems when estimating returns to schooling. However, when schooling is correlated with unobserved individual heterogeneity in *returns* to schooling, standard IV methods fail to give a consistent estimate of *average* returns to schooling unless stronger assumptions are invoked, see Wooldridge (1997), Heckman (1997) and Heckman and Vytlacil (1998). In the case of heterogeneous returns, Angrist, Imbens and Rubin (1996) develop an alternative interpretation of the standard IV estimator as identifying a "local average treatment effect" (LATE). In the schooling context, the LATE parameter reflects the returns to schooling for the group of individuals whose level of schooling is affected by the instrument used. In analyses where e.g. school reforms are used as instruments (Harmon and Walker, 1995, Meghir and Palme, 2000, Aakvik,

[14] The interaction between experience and schooling in (2) is assumed to be continuous, as in (1)

Salvanes and Vaage, 2001) this is a parameter of primary interest, since it measures the returns to schooling for those who changed their level of schooling because of the reform. In this study, we are also interested in whether the school system itself has changed with respect to generating economic returns for students. In this respect, the relevant parameter is the "average treatment effect" (ATE), which reflects the returns to schooling for a random individual.

To overcome the problems of obtaining an estimate of the average returns to schooling by cohort, $\beta_1+\beta_c$ in (6) and φ_{sc} in (2), I have used a two-step control function approach. The setup and strategy is similar to Meghir and Palme (2000) and has also much in common with Vella and Gregory (1996) and Hægeland, Klette and Salvanes (1999) who considered a homogeneous returns model. In the homogeneous returns case, standard IV and the control function approach are numerically equivalent, see Vella and Verbeek (1999) and Card (1999). As in IV estimation, the strategy is to find a variable (instrument) that is correlated with the level of schooling, but has no independent impact on earnings.

First, choice of schooling is estimated using an ordered probit model[15]. Consider the latent specification:

$$S_i^* = Z_i^S\beta + u_i. \tag{3}$$

where S_i^* represents the unobserved, optimal level of schooling, Z_i^S is a vector of family background variables and other variables affecting the level of schooling. In the empirical analysis, I include gender, presence of parents, region (county) of residence, guardian persons' education and country of birth, and guardian persons' labor market status and income[16]. The error term u_i is assumed to be normally distributed. Observed choices S_i can then be modelled as

$$\begin{aligned} S_i &= S^l \qquad \text{if } S_i^* \leq \mu_{S^l} \\ &= S^l + 1 \qquad \text{if } \mu_{S^l} < S_i^* \leq \mu_{S^l+1} \\ &\vdots \\ &= S^h - 1 \quad \text{if } \mu_{S^h-2} < S_i^* \leq \mu_{S^h-2} \\ &= S^h \qquad \text{if } S_i^* > \mu_{S^h-1} \end{aligned}$$

where S^l is the lowest and S^h is the highest attainable schooling level.

Denote exogenous variables in (2) and (3) by r_i, and consider the conditional expectation of (2), suppressing experience terms:

[15] Cameron and Heckman (1998) derive the conditions where ordered probit can be regarded as an appropriate reduced form model of educational choice.

[16] See Section 4 for details on sample and variable construction.

$$E\left(w_i\left|r_i,S_i\right.\right)=\sum_{c=1}^{C}\sum_{s=S^{l+1}}^{S^h}\varphi_{sc}E_{si}D_{ci}+\sum_{c=2}^{C}\psi_c D_{ci}+\theta Z_i+\sum_{s=S^{l+1}}^{S^h}E\left(\varepsilon_{si}\left|r_i,S_i\right.\right)E_{si}+E\left(\varepsilon_{li}\left|r_i,S_i\right.\right)$$

Assume, see Vella (1998), that $E\left(\varepsilon_{si}\left|r_i,S_i=s\right.\right)=\kappa^s\lambda_i^s$, where λ_i^s is a generalized residual from the ordered probit estimation of educational choice. This is the case if ε_i^s, $s=S^l,...,S^h$ and the error term in the ordered probit model have joint normal distributions. The estimated generalized residual is given by

$$\widehat{\lambda}_i^s=E_{si}\frac{\phi(\widehat{\mu}_s-Z_i^S\widehat{\beta})-\phi(\widehat{\mu}_{s-1}-Z_i^S\widehat{\beta})}{\Phi(\widehat{\mu}_s-Z_i^s\widehat{\beta})-\Phi(\widehat{\mu}_{s-1}-Z_i^s\widehat{\beta})}\qquad S^l\leq s\leq S^h \tag{4}$$

where the functions ϕ and Φ are the standard normal density and cumulative density, respectively. $\widehat{\mu}_{S^l-1}=-\infty$ and $\widehat{\mu}_{S^h}=\infty$.

Calculating a generalized residual according to (4) and including it in (2) gives the following estimating equation:

$$\begin{aligned}w_i &= \alpha+\sum_{c=1}^{C}\sum_{s=S^{l+1}}^{S^h}\varphi_{sc}E_{si}D_{ci}+\gamma_1X_i+\gamma_2X_i^2+\gamma_3X_i^3+\gamma_4X_i^4\\&\quad+\delta_1S_iX_i+\delta_2S_iX_i^2+\delta_3S_iX_i^3+\delta_4S_iX_i^4\\&\quad+\sum_{c=2}^{C}\psi_cD_{ci}+\theta Z_i+\sum_{s=S^l}^{S^h}\kappa^s\widehat{\lambda}_i^sE_{si}+u_i\end{aligned} \tag{5}$$

OLS on (5) gives consistent estimates of φ_{sc}, average returns to schooling by cohort[17]. To avoid identification by functional form only we need an instrument, i.e. a variable that is an element in Z^s, but not in Z. The interaction of $\widehat{\lambda}_i^s$ with dummies for levels of education purges the relationship between schooling and earnings of the effect of heterogeneity in *returns* to different levels of schooling. Including $\widehat{\lambda}_i^s$ without interaction eliminates only the bias from overall unobserved *earnings capacity*. Following Angrist, Imbens and Rubin (1996), this gives an estimate of the local average treatment effect.

Controlling for endogeneity bias in (1) is done in a similar manner. In (1), schooling is treated as a continuous variable, and the method suggested by Garen (1984) (see also Card, 1999) is appropriate. There, schooling is estimated by linear regression, and the residual from this estimation is included in the earnings equation both separately and interacted with years of schooling. In estimating (1), I stick with the ordered probit specification in the first step, since

[17] Standard errors must be corrected for estimated regressor bias.

(1) may be regarded as a special case of (2) with restrictions on the φ_{sc} parameters:

$$\begin{aligned} w_i &= \alpha + (\beta_1 + \varepsilon_i^s)\, S_i + \sum_{c=2}^{C} \beta_c D_{ci} S_i + \gamma_1 X_i + \gamma_2 X_i^2 + \gamma_3 X_i^3 + \gamma_4 X_i^4 \\ &\quad + \delta_1 S_i X_i + \delta_2 S_i X_i^2 + \delta_3 S_i X_i^3 + \delta_4 S_i X_i^4 \\ &\quad + \sum_{c=2}^{C} \psi_c D_{ci} + \theta Z_i + \zeta \widehat{\lambda}_i^s + \upsilon \widehat{\lambda}_i^s S_i + \varepsilon_i \end{aligned} \tag{6}$$

The ordered probit model is estimated separately for each (single year) cohort, allowing for different effects of family background variables (and different cut-off levels) across cohorts. Following Hægeland, Klette and Salvanes (1999), the main identifying instrument (included in the education model but not in the earnings equation) is the region (county) where the person grew up. It is well documented that educational choices vary considerably across regions in Norway. This is true also when conditioning on e.g. family background variables, as reported above. The instrument is in the spirit of Card (1995) who used college proximity as instrument, but may be interpreted in a more general sense as variations in the opportunity cost of education. The maintained hypothesis in the empirical analysis is that the region where the person grew up does not by itself influence earnings when conditioning on the level of education and other variables[18]. Notice that dummies for county of residence are included in the earnings regression. Returns to schooling are thereby identified from earnings differentials between individuals who live in the same region, but grew up in different regions and therefore chose different levels of schooling.

4 Sample and variable construction

The main data source for this study is the Norwegian system of register data, where individual information about essentially all Norwegian residents is gathered from a number of governmental administrative registers. In addition to basic demographic information, the data set contains information about education, income and employment relations. In this study, I use data for employees in manufacturing, private services and the public sector. 1997 is the main year of analysis, but I also use data for 1989 and 1993. The analysis is restricted to cohorts born between 1942 and 1970, since I have information of family background for these cohorts only (see below)[19].

[18] I have also experimented with specifications where the instrument was region interacted with family background variables such as family income and parents' education. This had only minor effects on the results.

[19] When estimating earnings equations, it is common to include a sample as large as possible. In several Norwegian studies using register data, the sample usually consists of individuals aged 20-64. In this study, the sample is limited to individuals born between 1942 and 1970. To check how this different sample composition

The dataset contains information on the highest completed level and type of education for each individual. The measure of years of schooling is the standard number of years necessary to complete this level. It does not necessarily reflect the actual number of years spent in school, since an individual may use less or more than standard time to complete an education. Observations with missing education information, or reported education of less than seven years, which was the compulsory amount of schooling in early postwar years, were excluded. Due to a considerably lower quality of data on educational attainment, foreign-born individuals have been excluded from the sample. In addition, family background information is often missing for the foreign-born.

Self-employed and part-time workers were excluded from the earnings equation sample. Information about hours worked are available in broad intervals only: Less than 4 hours, 4-19 hours, 20-29 hours, and 30 hours or more per week. I have restricted the sample to full-time workers, defined as individuals working 30 hours or more per week.

The earnings measure used is total annual taxable labor income. (Wages and earnings will be used interchangeably throughout the paper). Since the earnings measure reflects *annual* earnings, observations whose employment relationships started or terminated within the actual year were deleted. Holders of multiple jobs and individuals that have received labor market compensation or have participated in active labor market programs have been excluded. The individuals in the sample have thus been working full time for their only employer the entire year. Individuals with annual earnings below NOK 60 000 have been excluded from the sample. This exclusion had only minor effects on the results.

Details of the trimming procedure for 1997 is given in Table A1 . The final sample consists of 704 000 observations for 1997. Table A2 provides summary statistics for key individual level variables for 1997.

Family background information

Family background information is taken from the National Censuses of the Population and Housing in 1960 and 1970 (documented in Vassenden, 1987). The censuses give information of a number of social background variables. This information is available for individuals who are defined to have guardian persons, which in most cases are parents. I have made use of information of the presence of parents, country of birth, region (county) of residence in the census year,

may affect results, I estimated a "traditional" Mincer equation for both the actual sample and for individuals aged 20-64. Regressors were a female dummy, years of schooling, a quadratic in experience, and industry and county dummies. The differences were negligible, and a sample of individuals aged 27-55 seems representative for the whole labor force.

guardian persons' education and country of birth, and guardian persons' labor market status and income. (The 1960 census does not include income information). The majority of individuals with guardian persons are children, but also other individuals may be registered with guardian persons in the data, for several reasons. I have therefore restricted the sample to individuals up to 18 years of age with guardian persons, i.e. the 1942-1960 cohorts in the 1960 census and the 1952-1970 cohorts in the 1970 census. The variable construction is straightforward and identical for the two years, except for income and education. Income, defined as the sum of the guardian persons' total income in the census year, is only available in the 1970 census. In the 1970 census, guardian persons' education was coded according to a 4-digit classification, from which an aggregation into primary, lower secondary, upper secondary, lower tertiary and upper tertiary is straightforward. In the 1960 census, education was coded according to a 2-digit classification. Here the aggregation was not straightforward, but was carried out after a detailed examination of each educational category.

Some of the family background information (education and income of guardian persons) was used as covariates in the earnings equation. To estimate the ordered probit model of educational choice, the background information from the censuses was matched with information of the individuals' level of educational attainment from the register data for the years, 1989, 1993 and 1997, respectively. There is an overlap for the cohorts 1952-1960, as we have background information from both the 1960 and 1970 census for these cohorts. Since the 1970 census is richer in information of family background, it is used for these cohorts.

Individuals who were not registered with guardian persons or had missing information of any family background or education variables, were excluded from the sample. For each cohort, the estimating sample consists of on average 90 percent of the original birth cohort (sample size varies between 50 000 and 63 000 per cohort).

Constructing the experience measure

In many empirical studies, labor market experience is proxied by *potential* experience, i.e *age - education - 7*. This is a problematic measure for several reasons. First, it represents an upper bound for experience, and it is more upward biased for females, who on average have a looser connection to the labor market. Second, and more serious when analyzing cohort effects, use of potential experience introduces a serious identification problem, because of the linear dependency between birth year, years of schooling and potential experience. Heckman and Robb (1985b) give a thorough discussion of this issue. Hægeland, Klette and Salvanes (1999) used potential experience, but attempted to get around the identification problem by imposing parameter re-

strictions, such as assuming that cohort effects are not linear, but constant within groups of adjacent birth cohorts. This breaks the linear dependency and allows for separate identification of education, experience and cohort effects, but the parameter restrictions are admittedly somewhat *ad hoc*. In this study, I construct a measure of *real* experience based on earnings histories. The main data source for constructing the experience measure is the Norwegian National Insurance Scheme, which was established in 1967. Individuals' pension entitlements in this scheme are linked to their income histories, and the income used in calculating entitlements roughly corresponds to labor income. I construct a measure of actual labor market experience from this income information, which is available for all Norwegian residents from 1967 onwards. The data set contains income data only, and no demographic or other information. When using income histories to construct experience measures, I assume that income in a given year reflects labor market activity. Thus, a person with no or low income has not accumulated labor market experience that year. How to transform income into accumulated labor market experience is not obvious. One must decide how large income that corresponds to a full-time job in terms of accumulating labor market experience[20]. Since wages are known to differ between individuals of different education, sex and age, the relevant income limit for experience accumulation should vary accordingly. There are several reasonable procedures for constructing experience measures based on income data. However, what is most important is that individuals with zero or very low income do not accumulate experience. The Appendix gives further details on how the experience measure is constructed.

5 Empirical results

To investigate whether and how much returns to schooling and earnings levels differ between cohorts, and to what extent such differences can be attributed to selection effects, I have estimated three different models. First, I estimate (1) using ordinary least squares. Here, self-selection effects are not controlled for. Second, I estimate the two-step model (6), but without the $\upsilon\widehat{\lambda}_i^s S_i$ term. This model controls for self-selection into education based on unobserved earnings capacity, and is numerically equivalent to standard instrumental variable estimator which has the interpretation as the "local average treatment effect" (LATE) in a model with heterogeneity in returns to education, see Section 3. Finally, I estimate the full model (6), where both self-selection based on unobserved earnings capacity and unobserved heterogeneity in returns are

[20] To the extent that earnings reflect individual fixed effects, an experience measure that varies with earnings may be correlated with the error term in the earnings equation. This implies that the level of earnings that corresponds to full accumulation of experience should not be set too high.

controlled for. Since estimated returns to schooling in this model may be interpreted as the "average treatment effect" (ATE), see Section 3 and the references cited there, this is the relevant parameter when evaluating whether the school system has changed with respect to generating economic returns for students.

The results show that younger cohorts have lower returns to education than older cohorts, but that the differences vanish when controlling for self-selection into education. Earnings levels are found to be higher for younger cohorts, although the differences are smaller when self-selection is controlled for.

Table 1 presents estimation results for the three models. In the OLS estimation, the coefficients for the interactions terms between schooling and cohort are more negative the younger the cohort, indicating lower returns to education for younger cohorts. On the other hand, the coefficients for the cohort dummies are larger the younger the cohort, indicating that the level of earnings is higher for younger cohorts. The results for the LATE model in the second column show that the negative interactions between younger cohorts and schooling are markedly smaller, and in the ATE model in the third column they are almost absent. The "pure" cohort effects are positive also when controlling for self-selection, although they are smaller than what we find using OLS. Estimated experience effects and the male-female earnings differential are quite similar for all three specifications. Since the models allow for interaction between experience and schooling, the estimated returns to education vary with the level of experience. Similarly, earnings differentials between cohorts will vary with educational level since returns to education are cohort-specific. Table 2, 3 and 4 present estimated returns to education cohort, evaluated at fifteen years of experience, as well as differences in earnings levels across cohorts at different levels of schooling for the three models[21].

Looking first at returns to schooling, the OLS results in Table 2 show a quite marked decline in returns to schooling, from 6.2 percent for the 1942-45 cohort to 4.9 percent for the 1966-70 cohort[22]. Table 3 gives the results for the local average treatment effect (LATE) specification. The estimated returns to schooling are higher than what is found using OLS, and vary between 6.0 percent and 6.8 percent. The estimated average treatment effects (ATE) are reported in

[21] Such a calcuation implies out-of-sample predictions for some of the younger cohorts. Note, however, that the experience profile is assumed to be common across cohorts, and fifteen years of experience is well within the range of the sample.

[22] The results for the 1966-70 cohorts should be interpreted with caution. In the estimating year, 1997, they are between 27 and 31 years old. At that age, the effects of education on earnings may not be fully materialized. I have tried to accommodate for this by including dummy variables for each of the first five years of labor market experience. In addition, parts of the cohorts have not fully entered the labor market, and the full-time workers of the younger cohorts may therefore be a somewhat selected sample in this respect. However, all the main conclusions still hold when ignoring the 1966-70 cohorts.

Table 4. They are more stable across cohorts than the OLS estimates. For older cohorts, the estimated returns to schooling are lower than what I find using OLS, while the opposite is true for younger cohorts. Figure 9 plots the estimated returns to schooling by cohorts using the three different methods.

Taken together, the results on returns to schooling show that there has been a decline in returns to education across cohorts, but that this largely is due to changing self-selection mechanisms and not declining quality of the school system itself: The estimated average treatment effect (see Table 4), is quite stable across cohorts. That the OLS estimates are above the ATE estimates for the older cohorts, indicates that there was *positive selection* into education for these cohorts: Individuals with high returns to schooling chose education to a greater extent than others. This positive selection effect gradually diminishes across cohorts, and for younger cohorts there is in fact a small *negative* selection effect. The local average treatment effect estimates are (as found in many studies) higher than the OLS estimates for all the cohorts considered. This estimator captures the effect of schooling for individuals where the instrument affected their schooling decisions. In this study, the instrument is the county where the individual grew up. As described in Section 2, a major goal of Norwegian educational policy has been to reduce regional differences in educational attainment. In this respect, the LATE estimates are interesting, since they indicate that individuals that were induced to take more education by geographical factors (regional differences in opportunity costs of schooling) had a higher than average return to schooling, so that extending the geographical coverage of the education system recruited high-return individuals into education. This is also the case for younger cohorts. Rather than "digging deeper in the distribution", the increase in educational attainment along the geographical dimension seems to have attracted new groups of the population with high returns to education. But there has also been a general increase in educational attainment. Comparing the OLS and ATE estimates suggests that the general increase in education attainment in excess of changes in relative regional educational differences[23] may largely have been an increase in educational attainment by individuals with low returns to education, since the positive selection effects for older cohorts are not present for younger cohorts. Separate OLS estimates by county of upbringing (not reported) suggest that this may be the case, since there is a clear tendency that the counties with largest increase in educational attainment have had the largest decline in returns to schooling by cohort.

Looking at cohort earnings differentials at different educational levels in Table 2, 3 and 4

[23] Comparing the 1942 and 1970 cohorts, about half of the increase in educational attainment can be attributed to changes in relative educational propensity between regions.

(see also Figure 10), the results show that younger cohorts have higher earnings (controlling for differences in labor market experience). Focusing on the results from model (6), the differences between educational levels are small, reflecting the stability in returns to schooling across cohorts. The 1962-65 cohort has an earnings premium of around nine percent relative to the 1942-45 cohort. Together with the estimated stability in returns to schooling, the results are interesting in light of the egalitarian Norwegian educational policy and the debate on the quality of the schooling system. The results give no support to the view that the quality of the school system has declined, since the estimated returns to education are stable across cohorts. On the contrary, the higher earnings levels for younger cohorts, almost uniform across levels of schooling, may be interpreted as an *increase* in the quality of basic education, thereby increasing the human capital of all individuals. This is consistent with the relative shift in resource use towards primary education, see Section 2.

Differences between schooling levels

As mentioned above, several Norwegian studies have shown that marginal returns to schooling vary with the level of education. To allow for variation in marginal returns to schooling across years of schooling and to check whether the conclusions on cohort effects made above are sensitive to the assumption of a constant school-year effect, the average treatment effect model was estimated with a specification with dummies for each year of schooling and cohort, cf. (5). Results are presented in Table 5 and 6. Although there is some heterogeneity between years of schooling, the results are qualitatively in line with what I found above using the constant school-year effect model. Returns to education are quite stable across cohorts for low educational levels, while the picture is more mixed for higher levels. Earnings levels are higher for younger cohorts at all educational levels. The differences seem to be larger for lower levels of education, but they are substantial also for higher levels. All in all, the main conclusions are not altered when I use a more flexible model.

Differences between sectors and types of workers

The discussion so far has pointed at selection as the main cause of cohort differences in returns to schooling. Technological change may also be a cause of cohort effects by making old skills obsolete, as suggested in the introduction. If human capital is partly technology-specific, some of it will be eroded by technological change. If skills obsolescence is important for cohort effects in earnings, one should expect to see the strongest effects in industries with high rates of technological change and for types of workers who are strongly exposed to technological change,

e.g. workers with technical education employed in manufacturing. To investigate this further, I estimated an extended version of (6), where pure cohort effects and cohort-specific returns to education are allowed to vary with gender (male, female), industry of employment (manufacturing, private services, public sector) and type of education (technical, business/administrative, humanities, other). For simplicity, I assume that gender, industry and type of education effects are independent, i.e. that gender effects are the same across industries and types of education, and vice versa.

Results are presented in Tables 7 to 12 and Figures 11 to 16. Table 7 presents the results for the reference category, which is males with technical education working in manufacturing. Returns to education are higher for older cohorts, while cohort effects on earnings levels are stronger for this group than for the full sample. Given that technically educated workers in manufacturing are more exposed to technological change than others, this may be an effect of skills obsolescence adversely affecting comparatively older workers with less education. Looking at the results for other groups of workers makes such an explanation less plausible, however. As seen from Figure 12, the same pattern is present in the public sector (see also Table 10) with respect to returns to education, while the opposite is the case for private services (Table 9). Figure 13 shows that the decline in the returns to schooling across cohorts is just as strong for individuals educated within the humanities (Table 12), while business/administrative education shows an increase in returns (Table 11). Looking at earnings levels in Figures 15 and 16, cohort effects are in fact smaller in manufacturing and for workers with technical education than for other sectors and types of workers.

In sum, there are substantial differences in returns to education and in cohort differentials between sectors and types of workers. Since cohort effects are stronger for workers and sectors where technological change has not been pervasive (humanities, public sector), the results give no strong support for the skills obsolescence explanation of cohort effects.

Differences over time

The strategy of the analysis in this paper is to identify cohort effects by comparing different cohorts at the same point in time in the same labor market. To check whether the results presented above are sensitive to the year chosen, I have estimated (6) for several years. To make estimates for different years comparable, the sample was restricted to the same age group in the different years. I estimated (6) for the years 1989, 1993 and 1997. Since the original sample is the 1942-70 cohorts, the largest comparable samples consist of individuals aged 27-45 in the respective years. Results are presented in Tables 13-15 and Figures 17 and 18. Overall returns to

education have declined somewhat over the period considered, but the relative cohort patterns are quite stable. The results discussed above for 1997 thus seem to reflect stable cohort patterns and not temporary labor market conditions.

6 Concluding remarks

Selection, school system or skills obsolescence? In light of the large increase in educational attainment and large changes in the Norwegian education system since World War II, and the widely held belief that the quality of the education system has declined over time, I have investigated whether the returns to education vary across post-war cohorts in Norway, and tried to attribute the findings to selection effects, changes in the school system and skills obsolescence arising from technological change. Economic returns to education are found to be lower for younger cohorts. However, controlling for self-selection into education within a random coefficient model, average returns to education are quite stable across cohorts. This indicates that the observed decline in returns can be attributed mostly to changes in self-selection mechanisms over time: More "low-return" individuals take higher education than before. The estimates give no support to the view that the quality of the school system itself has deteriorated, and that students learn less in school than before. To the extent that workers are paid their marginal product, and labor market productivity is the relevant measure for what students learn, the estimates show that the quality of schooling has not declined. Critics that blame the school system for the fact that students at universities and colleges lack basic skills to a greater extent than before, do not fully take into account that student composition has changed. The fact that younger cohorts score higher on IQ tests is also hard to reconcile with a claim that average "cohort quality" is lower than before. An additional interesting finding is that younger cohorts have higher earnings levels. In light of Norwegian educational policy, this may be an indication that the gearing of resources towards less able at low levels of schooling has paid off in higher earnings, without a decline in returns to education at higher levels.

I have also investigated whether skills obsolescence due to technological change may be driving cohort differences, but found no clear indication that this was the case. This is something that deserves further investigation, however, using more direct measures of exposure to technological change.

In this paper the focus has been on average returns to education. An interesting area for future research is to consider heterogeneity in returns to education within cohorts more explicitly using quantile regression techniques, see e.g. Buchinsky (1994).

Implicitly assuming that individuals of different cohorts with the same schooling levels are perfect substitutes in terms of efficiency units, supply effects have been ignored in this paper. Estimates on U.S. data, see e.g. Card and Lemieux (2000), suggest that the elasticity of substitution between workers of different age groups are large, but finite. This implies that differences in relative supply of educated workers across cohorts may play a role in explaining cohort effects in returns to schooling, see Card and Lemieux (2000). My preliminary investigations along these lines suggest that differences in relative supply across cohorts are of minor importance for the reslults in this paper, but this deserves further investigation in future work.

Since the sample is limited to full-time wage earners, the estimates of returns to education are conditional upon having a job. If education improves the chances of getting a job, particularly for the low-skilled, this may underestimate the returns to education. In particular, this may be the case for younger cohorts, where having no or little education beyond compulsory schooling probably is a worse signal than among older workers. To the extent that education improves employment prospects, this may compensate for a low return given employment. This is an issue that deserves further investigation, but requires more information of unemployment and earnings histories than what is currently available.

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Appendix: Details on the construction of the experience measure

From the National Censuses of the Population and Housing in 1980 (documented in Vassenden, 1987) I constructed median annual earnings for full-time workers in categories classified by sex, years of education and age (in five-year intervals). This median income by category was then inflated/deflated by an aggregate wage index from the Norwegian National Accounts. Hence, when constructing the experience variable, I implicitly assume that relative income between categories have been constant over the period considered. The experience accumulated by an individual in a given year is then calculated as income/$(0.75 *$ median income in the relevant category), or equal to 1 if income$\geq 0.75*$median income. The rationale behind this is that a person who earns less than 75 percent of the median income of full-time workers of the same sex, education and age is unlikely to work full time and hence does not accumulate full experience that year. The choice of limit may be disputed. However, experimenting with different limits showed that the results are not very sensitive to reasonable variations. It turns out that what matters is that individuals-years without labor income do not count as experience-years. For the oldest cohorts, the earnings histories do not cover the earliest years of their (potential) careers. For these years, experience was calculated according to the potential experience formula.

Table 1: Estimation results, 1997

	OLS	*Two-step (LATE)*	*Two-step (ATE)*
Female	-0.2010	-0.1982	-0.1991
	(0.0008)	(0.0008)	(0.0008)
Schooling	0.0826	0.0881	0.0708
	(0.0030)	(0.0030)	(0.0031)
Schooling*Cohort 1946-49	-0.0026	-0.0018	-0.0008
	(0.0005)	(0.0005)	(0.0005)
Schooling*Cohort 1950-53	-0.0061	-0.0044	-0.0027
	(0.0005)	(0.0005)	(0.0005)
Schooling*Cohort 1954-57	-0.0049	-0.0022	-0.0005
	(0.0006)	(0.0006)	(0.0006)
Schooling*Cohort 1958-61	-0.0046	-0.0007	0.0011
	(0.0007)	(0.0007)	(0.0007)
Schooling*Cohort 1962-65	-0.0064	-0.0017	0.0006
	(0.0008)	(0.0008)	(0.0009)
Schooling*Cohort 1966-70	-0.0138	-0.0083	-0.0047
	(0.0010)	(0.0010)	(0.0010)
Experience	0.0922	0.0923	0.0814
	(0.0081)	(0.0081)	(0.0081)
$Experience^2*10^2$	-0.4736	-0.4857	-0.4342
	(0.0601)	(0.0602)	(0.0604)
$Experience^3*10^3$	0.1115	0.1173	0.1068
	(0.0184)	(0.0184)	(0.0184)
$Experience^4*10^4$	-0.0090	-0.0099	-0.0096
	(0.0020)	(0.0020)	(0.0020)
Schooling*Experience	-0.0046	-0.0048	-0.0042
	(0.0006)	(0.0006)	(0.0006)
$Schooling*Experience^2*10^2$	0.0333	0.0360	0.0339
	(0.0049)	(0.0049)	(0.0049)
$Schooling*Experience^3*10^3$	-0.0088	-0.0099	-0.0098
	(0.0016)	(0.0016)	(0.0016)
$Schooling*Experience^4*10^4$	0.0007	0.0009	0.0010
	(0.0002)	(0.0002)	(0.0002)
0-1 years of experience	0.4300	0.4298	0.4340
	(0.0200)	(0.0200)	(0.0201)
1-2 years of experience	0.1917	0.1909	0.1827
	(0.0238)	(0.0239)	(0.0240)
2-3 years of experience	-0.0185	-0.0207	-0.0277
	(0.0137)	(0.0137)	(0.0137)
3-4 years of experience	-0.0317	-0.0328	-0.0370
	(0.0075)	(0.0075)	(0.0075)
4-5 years of experience	-0.0129	-0.0133	-0.0160
	(0.0053)	(0.0053)	(0.0053)
Cohort 1946-49	0.0558	0.0448	0.0345
	(0.0057)	(0.0057)	(0.0057)
Cohort 1950-53	0.1271	0.1044	0.0872
	(0.0065)	(0.0066)	(0.0067)
Cohort 1954-57	0.1278	0.0902	0.0743
	(0.0075)	(0.0078)	(0.0078)
Cohort 1958-61	0.1305	0.0790	0.0637
	(0.0085)	(0.0090)	(0.0090)
Cohort 1962-65	0.1604	0.0997	0.0818
	(0.0097)	(0.0103)	(0.0104)
Cohort 1966-70	0.2550	0.1858	0.1546
	(0.0114)	(0.0121)	(0.0118)
λ^S	0.2550	-0.0228	-0.0486
	(0.0114)	(0.0013)	(0.0017)
λ^S*Schooling			0.0036
			(0.0002)
R^2 adjusted	0.4580	0.4583	0.4588
Number of observations	703452	703542	703542

Dependent variable is log of annual earnings. Dummies for type of education (ten categories), industry (2-digit ISIC), county of residence, parents' education and income in census year and cohort size are also included, but not reported. Robust standard errors, corrected for estimated regressor bias (column 2 and 3), in parentheses.

Table 2: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Ordinary least squares

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	0.0624	0.0000	0.0000	0.0000	0.0000
	(0.0006)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	0.0598	0.0323	0.0245	0.0166	0.0088
	(0.0005)	(0.0024)	(0.0021)	(0.0025)	(0.0034)
1950-53	0.0563	0.0725	0.0542	0.0360	0.0178
	(0.0005)	(0.0024)	(0.0019)	(0.0024)	(0.0036)
1954-57	0.0575	0.0833	0.0685	0.0537	0.0388
	(0.0004)	(0.0028)	(0.0022)	(0.0028)	(0.0042)
1958-61	0.0578	0.0892	0.0754	0.0617	0.0479
	(0.0004)	(0.0030)	(0.0023)	(0.0031)	(0.0047)
1962-65	0.0560	0.1025	0.0832	0.0639	0.0446
	(0.0005)	(0.0034)	(0.0026)	(0.0036)	(0.0055)
1966-70	0.0486	0.1304	0.0889	0.0474	0.0059
	(0.0007)	(0.0039)	(0.0033)	(0.0047)	(0.0070)

Estimates are calculated from the results in Table 1, column 1.

Table 3: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (LATE) model

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	0.0684	0.0000	0.0000	0.0000	0.0000
	(0.0007)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	0.0666	0.0288	0.0234	0.0181	0.0127
	(0.0007)	(0.0024)	(0.0021)	(0.0026)	(0.0036)
1950-53	0.0639	0.0644	0.0510	0.0377	0.0243
	(0.0007)	(0.0024)	(0.0019)	(0.0026)	(0.0039)
1954-57	0.0662	0.0707	0.0642	0.0578	0.0513
	(0.0007)	(0.0029)	(0.0023)	(0.0030)	(0.0045)
1958-61	0.0677	0.0730	0.0711	0.0691	0.0671
	(0.0007)	(0.0032)	(0.0024)	(0.0033)	(0.0051)
1962-65	0.0667	0.0847	0.0797	0.0746	0.0696
	(0.0008)	(0.0036)	(0.0027)	(0.0039)	(0.0059)
1966-70	0.0601	0.1112	0.0863	0.0614	0.0366
	(0.0009)	(0.0041)	(0.0033)	(0.0048)	(0.0074)

Estimates are calculated from the results in Table 1, column 2

Table 4: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (ATE) model

	Returns to schooling	Earnings levels relative to 1942-45 cohort			
		9 years	12 years	15 years	18 years
Cohort					
1942-45	0.0566	0.0000	0.0000	0.0000	0.0000
	(0.0009)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	0.0558	0.0274	0.0251	0.0227	0.0204
	(0.0008)	(0.0024)	(0.0021)	(0.0026)	(0.0037)
1950-53	0.0538	0.0626	0.0545	0.0463	0.0381
	(0.0008)	(0.0024)	(0.0020)	(0.0026)	(0.0039)
1954-57	0.0561	0.0700	0.0685	0.0671	0.0656
	(0.0008)	(0.0029)	(0.0023)	(0.0031)	(0.0046)
1958-61	0.0577	0.0738	0.0772	0.0806	0.0840
	(0.0008)	(0.0031)	(0.0024)	(0.0034)	(0.0052)
1962-65	0.0571	0.0869	0.0886	0.0903	0.0920
	(0.0009)	(0.0036)	(0.0027)	(0.0039)	(0.0061)
1966-70	0.0519	0.1126	0.0986	0.0846	0.0706
	(0.0010)	(0.0041)	(0.0034)	(0.0050)	(0.0076)

Estimates are calculated from the results in Table 1, column 3

Table 5: Marginal returns to schooling, evaluated at 15 years of experience, 1997. Model with school-year dummies. Two-step (ATE) model

	10 years	11 years	12 years	13 years	14 years	15 years	16 years	17 years	18 years
Cohort									
1942-45	0.0278	0.0503	0.0465	0.1436	0.0033	0.0349	0.0741	0.0847	0.0642
	(0.0100)	(0.0039)	(0.0044)	(0.0048)	(0.0061)	(0.0092)	(0.0089)	(0.0086)	(0.0123)
1946-49	0.0665	0.0495	0.0548	0.1386	0.0035	-0.0037	0.0913	0.0995	0.0461
	(0.0056)	(0.0035)	(0.0039)	(0.0044)	(0.0056)	(0.0073)	(0.0073)	(0.0078)	(0.0115)
1950-53	0.0786	0.0412	0.0440	0.1365	0.0061	-0.0029	0.0892	0.0900	0.0575
	(0.0049)	(0.0034)	(0.0037)	(0.0043)	(0.0052)	(0.0064)	(0.0064)	(0.0076)	(0.0117)
1954-57	0.0782	0.0511	0.0421	0.1237	0.0377	-0.0283	0.1007	0.0899	0.0299
	(0.0051)	(0.0033)	(0.0035)	(0.0043)	(0.0052)	(0.0057)	(0.0061)	(0.0076)	(0.0120)
1958-61	0.0765	0.0444	0.0532	0.1086	0.0658	-0.0527	0.1201	0.0567	0.0600
	(0.0057)	(0.0035)	(0.0034)	(0.0050)	(0.0060)	(0.0056)	(0.0061)	(0.0077)	(0.0126)
1962-65	0.0667	0.0427	0.0519	0.0994	0.0678	-0.0243	0.1121	0.0217	0.0646
	(0.0063)	(0.0037)	(0.0034)	(0.0046)	(0.0059)	(0.0055)	(0.0058)	(0.0075)	(0.0128)
1966-70	0.0616	0.0337	0.0460	0.0861	0.0410	0.0165	0.0785	0.0051	0.1202
	(0.0065)	(0.0037)	(0.0032)	(0.0043)	(0.0058)	(0.0053)	(0.0051)	(0.0069)	(0.0134)

Results are calculated from estimates based on (4). Dependent variable is log of annual earnings. Included independent variables are cohort-specific dummies for each year of schooling, cohort dummies, a female dummy, a quartic in experience, a quartic in experience interacted with years of schooling, dummies for the first five years of experience, dummies for type of education (ten categories), industry (2-digit ISIC), county of residence, parents' education and income in census year, cohort size and inverse Mills' ratios interacted with dummies for each year of schooling. Robust standard errors, corrected for estimated regressor bias in parentheses.

Table 6: Earnings relative to 1942-45 cohort at different levels of schooling, 1997. Model with school-year dummies. Two-step (ATE) model.

	9 years	10 years	11 years	12 years	13 years	14 years	15 years	16 years	17 years	18 years
1946-49	-0.0025	0.0363	0.0355	0.0438	0.0388	0.0390	0.0005	0.0177	0.0324	0.0143
	(0.0110)	(0.0028)	(0.0046)	(0.0038)	(0.0047)	(0.0056)	(0.0091)	(0.0042)	(0.0082)	(0.0067)
1950-53	0.0301	0.0809	0.0718	0.0694	0.0623	0.0651	0.0274	0.0424	0.0478	0.0411
	(0.0105)	(0.0028)	(0.0046)	(0.0038)	(0.0047)	(0.0055)	(0.0086)	(0.0042)	(0.0083)	(0.0069)
1954-57	0.0413	0.0918	0.0926	0.0882	0.0684	0.1028	0.0396	0.0663	0.0715	0.0372
	(0.0106)	(0.0031)	(0.0047)	(0.0038)	(0.0051)	(0.0056)	(0.0082)	(0.0049)	(0.0083)	(0.0076)
1958-61	0.0538	0.1025	0.0967	0.1035	0.0685	0.1310	0.0435	0.0895	0.0615	0.0573
	(0.0108)	(0.0034)	(0.0048)	(0.0038)	(0.0058)	(0.0059)	(0.0082)	(0.0055)	(0.0085)	(0.0087)
1962-65	0.0781	0.1171	0.1095	0.1149	0.0708	0.1352	0.0761	0.1141	0.0511	0.0515
	(0.0111)	(0.0038)	(0.0051)	(0.0040)	(0.0058)	(0.0064)	(0.0083)	(0.0059)	(0.0087)	(0.0096)
1966-70	0.1090	0.1429	0.1263	0.1259	0.0684	0.1061	0.0877	0.0921	0.0125	0.0685
	(0.0113)	(0.0043)	(0.0055)	(0.0044)	(0.0060)	(0.0070)	(0.0089)	(0.0069)	(0.0095)	(0.0113)

See notes to Table 5.

Table 7: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (ATE) model. Reference category

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	0.0741	0.0000	0.0000	0.0000	0.0000
	(0.0027)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	0.0702	0.0385	0.0268	0.0152	0.0036
	(0.0022)	(0.0070)	(0.0048)	(0.0073)	(0.0119)
1950-53	0.0636	0.0937	0.0623	0.0309	-0.0005
	(0.0018)	(0.0084)	(0.0057)	(0.0083)	(0.0134)
1954-57	0.0662	0.1098	0.0862	0.0625	0.0389
	(0.0015)	(0.0093)	(0.0064)	(0.0092)	(0.0148)
1958-61	0.0661	0.1328	0.1089	0.0850	0.0610
	(0.0013)	(0.0102)	(0.0069)	(0.0101)	(0.0164)
1962-65	0.0602	0.1733	0.1317	0.0900	0.0483
	(0.0012)	(0.0111)	(0.0074)	(0.0112)	(0.0183)
1966-70	0.0502	0.2281	0.1566	0.0852	0.0137
	(0.0016)	(0.0122)	(0.0080)	(0.0123)	(0.0203)

Results are calculated from estimates based on (6), but where pure cohort effects and cohort-specific returns to schooling are allowed to vary with gender, sector (manufacturing, private services and public sector) and type of education (technical, business/administrative, humanities, other). Reference category is males with technical education working in manufacturing. For other included variables see column 3 in Table 1 and notes to Table 1. Robust standard errors, corrected for estimated regressor bias in parentheses.

Table 8: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (ATE) model. Partial female effect

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	-0.0024	0.0000	0.0000	0.0000	0.0000
	(0.0017)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	-0.0025	-0.0136	-0.0139	-0.0141	-0.0144
	(0.0015)	(0.0048)	(0.0036)	(0.0045)	(0.0067)
1950-53	-0.0006	-0.0225	-0.0171	-0.0117	-0.0063
	(0.0013)	(0.0064)	(0.0046)	(0.0053)	(0.0079)
1954-57	-0.0016	-0.0436	-0.0411	-0.0386	-0.0361
	(0.0011)	(0.0075)	(0.0053)	(0.0060)	(0.0090)
1958-61	-0.0004	-0.0838	-0.0777	-0.0717	-0.0657
	(0.0009)	(0.0086)	(0.0060)	(0.0068)	(0.0102)
1962-65	0.0031	-0.1229	-0.1064	-0.0900	-0.0736
	(0.0009)	(0.0099)	(0.0068)	(0.0077)	(0.0117)
1966-70	0.0084	-0.1583	-0.1259	-0.0936	-0.0613
	(0.0012)	(0.0111)	(0.0075)	(0.0085)	(0.0131)

See notes to Table 7.

Table 9: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (ATE) model. Partial service sector effect

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	-0.0185	0.0000	0.0000	0.0000	0.0000
	(0.0025)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	-0.0126	0.0151	0.0328	0.0505	0.0681
	(0.0021)	(0.0049)	(0.0050)	(0.0088)	(0.0135)
1950-53	-0.0068	0.0136	0.0486	0.0836	0.1186
	(0.0018)	(0.0058)	(0.0057)	(0.0100)	(0.0154)
1954-57	-0.0016	0.0177	0.0683	0.1190	0.1696
	(0.0015)	(0.0064)	(0.0062)	(0.0111)	(0.0172)
1958-61	0.0009	0.0258	0.0840	0.1421	0.2003
	(0.0012)	(0.0070)	(0.0067)	(0.0122)	(0.0191)
1962-65	0.0046	0.0322	0.1017	0.1711	0.2406
	(0.0011)	(0.0079)	(0.0073)	(0.0136)	(0.0213)
1966-70	0.0047	0.0288	0.0986	0.1683	0.2381
	(0.0016)	(0.0091)	(0.0080)	(0.0148)	(0.0235)

See notes to Table 7.

Table 10: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (ATE) model. Partial public sector effect

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	-0.0087	0.0000	0.0000	0.0000	0.0000
	(0.0021)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	-0.0073	-0.0102	-0.0060	-0.0018	0.0025
	(0.0018)	(0.0048)	(0.0045)	(0.0075)	(0.0115)
1950-53	-0.0080	-0.0197	-0.0176	-0.0155	-0.0134
	(0.0015)	(0.0057)	(0.0051)	(0.0085)	(0.0131)
1954-57	-0.0135	-0.0169	-0.0313	-0.0457	-0.0601
	(0.0013)	(0.0063)	(0.0056)	(0.0095)	(0.0147)
1958-61	-0.0186	0.0022	-0.0277	-0.0576	-0.0875
	(0.0011)	(0.0070)	(0.0061)	(0.0105)	(0.0164)
1962-65	-0.0161	0.0017	-0.0206	-0.0429	-0.0652
	(0.0012)	(0.0081)	(0.0068)	(0.0117)	(0.0184)
1966-70	-0.0132	-0.0077	-0.0213	-0.0349	-0.0484
	(0.0016)	(0.0092)	(0.0075)	(0.0129)	(0.0205)

See notes to Table 7.

Table 11: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (ATE) model. Partial business/administrative education effect

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	-0.0079	0.0000	0.0000	0.0000	0.0000
	(0.0026)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	-0.0059	0.0028	0.0088	0.0148	0.0208
	(0.0022)	(0.0088)	(0.0058)	(0.0081)	(0.0131)
1950-53	-0.0038	0.0042	0.0166	0.0289	0.0413
	(0.0018)	(0.0102)	(0.0068)	(0.0090)	(0.0144)
1954-57	-0.0020	0.0093	0.0269	0.0444	0.0620
	(0.0015)	(0.0111)	(0.0074)	(0.0099)	(0.0160)
1958-61	0.0024	-0.0040	0.0269	0.0577	0.0886
	(0.0013)	(0.0119)	(0.0079)	(0.0108)	(0.0174)
1962-65	0.0074	-0.0182	0.0275	0.0733	0.1191
	(0.0013)	(0.0130)	(0.0085)	(0.0117)	(0.0190)
1966-70	0.0106	-0.0284	0.0269	0.0822	0.1375
	(0.0018)	(0.0143)	(0.0093)	(0.0127)	(0.0208)

See notes to Table 7.

Table 12: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (ATE) model. Partial humanities education effect

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	-0.0129	0.0000	0.0000	0.0000	0.0000
	(0.0040)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	-0.0208	0.0624	0.0388	0.0152	-0.0084
	(0.0035)	(0.0232)	(0.0150)	(0.0095)	(0.0118)
1950-53	-0.0183	0.0694	0.0530	0.0367	0.0203
	(0.0027)	(0.0251)	(0.0161)	(0.0106)	(0.0139)
1954-57	-0.0256	0.1160	0.0778	0.0395	0.0013
	(0.0024)	(0.0275)	(0.0175)	(0.0118)	(0.0161)
1958-61	-0.0288	0.1544	0.1066	0.0588	0.0109
	(0.0023)	(0.0290)	(0.0184)	(0.0132)	(0.0187)
1962-65	-0.0255	0.1398	0.1020	0.0642	0.0264
	(0.0026)	(0.0312)	(0.0197)	(0.0147)	(0.0215)
1966-70	-0.0180	0.1258	0.1104	0.0950	0.0797
	(0.0034)	(0.0336)	(0.0211)	(0.0164)	(0.0245)

See notes to Table 7.

Table 13: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1989. Two-step (ATE) model. Age 27-45.

	Returns to schooling	Earnings levels relative to 1942-45 cohort 9 years	12 years	15 years	18 years
Cohort					
1942-45	0.0644	0.0000	0.0000	0.0000	0.0000
	(0.0010)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1946-49	0.0639	0.0128	0.0112	0.0096	0.0080
	(0.0009)	(0.0029)	(0.0024)	(0.0028)	(0.0039)
1950-53	0.0613	0.0309	0.0214	0.0119	0.0024
	(0.0009)	(0.0030)	(0.0023)	(0.0029)	(0.0044)
1954-57	0.0648	0.0306	0.0316	0.0326	0.0336
	(0.0010)	(0.0034)	(0.0026)	(0.0035)	(0.0053)
1958-61	0.0649	0.0384	0.0399	0.0414	0.0430
	(0.0012)	(0.0038)	(0.0028)	(0.0044)	(0.0070)
1962-65	0.0627	0.0521	0.0468	0.0414	0.0361
	(0.0016)	(0.0049)	(0.0036)	(0.0062)	(0.0101)

Model specification as in column 3 of Table 1.
Robust standard errors, corrected for estimated regressor bias in parentheses.

Table 14: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1993. Two-step (ATE) model. Age 27-45.

	Returns to schooling	Earnings levels relative to 1946-49 cohort 9 years	12 years	15 years	18 years
Cohort					
1946-49	0.0607	0.0000	0.0000	0.0000	0.0000
	(0.0010)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1950-53	0.0597	0.0073	0.0042	0.0011	-0.0021
	(0.0009)	(0.0030)	(0.0025)	(0.0030)	(0.0041)
1954-57	0.0623	0.0067	0.0114	0.0160	0.0207
	(0.0009)	(0.0030)	(0.0024)	(0.0032)	(0.0047)
1958-61	0.0644	0.0038	0.0149	0.0259	0.0370
	(0.0010)	(0.0034)	(0.0029)	(0.0041)	(0.0060)
1962-65	0.0623	0.0155	0.0203	0.0250	0.0297
	(0.0011)	(0.0039)	(0.0035)	(0.0051)	(0.0076)
1966-70	0.0604	0.0165	0.0154	0.0143	0.0133
	(0.0015)	(0.0051)	(0.0041)	(0.0065)	(0.0100)

Model specification as in column 3 of Table 1.
Robust standard errors, corrected for estimated regressor bias in parentheses.

Table 15: Returns to schooling at 15 years of experience and relative earnings levels by cohort in 1997. Two-step (ATE) model. Age 27-45.

	Returns to schooling	Earnings levels relative to 1950-53 cohort 9 years	12 years	15 years	18 years
Cohort					
1950-53	0.0553	0.0000	0.0000	0.0000	0.0000
	(0.0010)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
1954-57	0.0578	0.0004	0.0079	0.0155	0.0230
	(0.0010)	(0.0026)	(0.0017)	(0.0024)	(0.0039)
1958-61	0.0600	0.0042	0.0181	0.0321	0.0461
	(0.0010)	(0.0030)	(0.0020)	(0.0029)	(0.0048)
1962-65	0.0612	0.0105	0.0280	0.0456	0.0632
	(0.0010)	(0.0035)	(0.0024)	(0.0036)	(0.0058)
1966-70	0.0577	0.0243	0.0315	0.0388	0.0460
	(0.0012)	(0.0042)	(0.0032)	(0.0048)	(0.0074)

Model specification as in column 3 of Table 1.
Robust standard errors, corrected for estimated regressor bias in parentheses.

Table A1: Sample trimming procedure 1997

Total number of observations of individuals born 1942-1970 in manufacturing. private services and public sector	1 324 659
- Self-employed	16 096
- Part time workers	280 190
- Multiple job holders	61 610
- Individuals registered as unemployed or on active labor market programs during the year	32 714
- Individuals with missing education information	35 468
- Foreign born individuals	26 981
- Individuals with earnings below NOK 60 000	14 882
- Individuals not employed throughout the year	121 307
- Individuals with missing background information	31 869
Estimating sample for earnings equation	703 542

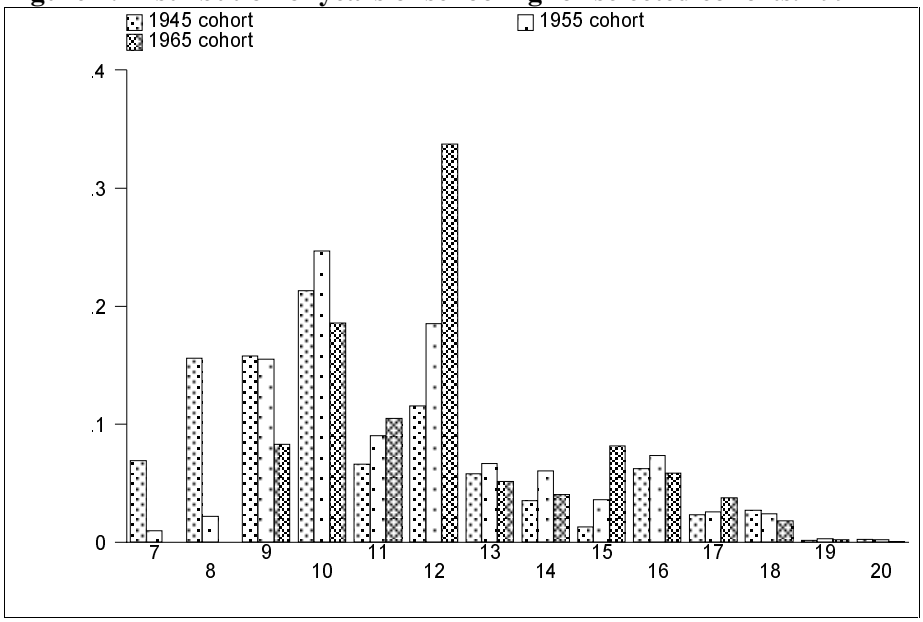
Table A2: Summary statistics earnings equation sample

	Mean	Standard deviation
Log earnings	7.869	0.341
Years of schooling	12.073	2.576
Years of experience	20.988	8.492
Female (share)	0.363	
Manufacturing (share)	0.315	
Private services (share)	0.386	
Public sector (share)	0.299	
Technical education (share)	0.307	
Business/adm. education (share)	0.202	
Humanities education (share)	0.032	
1942-45 cohort (share)	0.123	
1946-49 cohort (share)	0.153	
1950-53 cohort (share)	0.144	
1954-57 cohort (share)	0.147	
1958-61 cohort (share)	0.140	
1962-65 cohort (share)	0.137	
1966-70 cohort (share)	0.155	

Figure 1: Average educational attainment and standard deviation by birth cohort, 1997

Source: Register of highest completed education, Statistics Norway

Figure 2: Distribution of years of schooling for selected cohorts. 1997

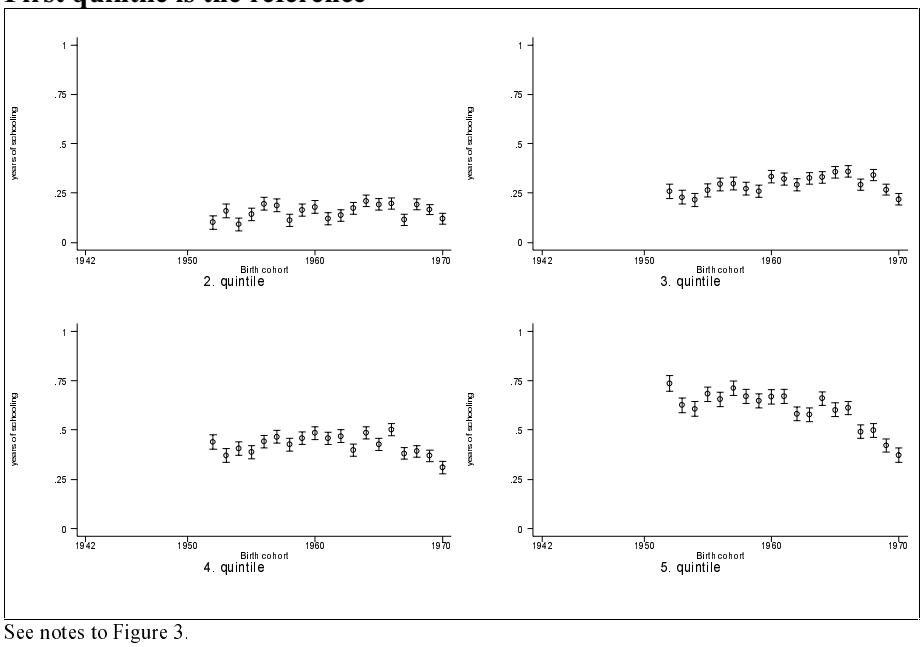


Source: Register of highest completed education, Statistics Norway

Figure 3: Educational attainment by cohort. Partial female effect

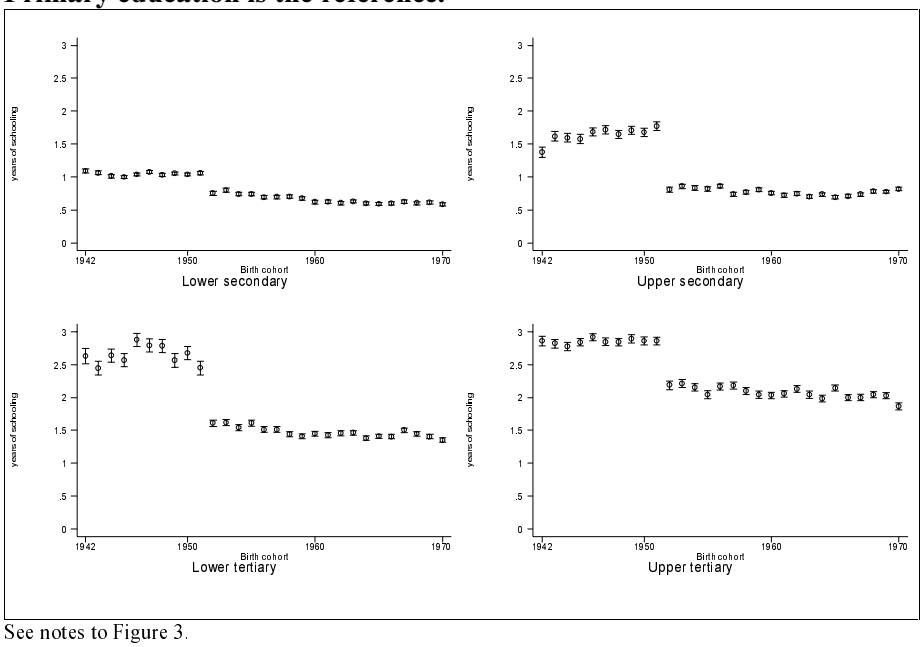
OLS estimates. The data used are the same as in the ordered probit model for educational choice in Section 3, and they are described in more detail in Section 4. The variables included in the regression are gender and dummy variables for presence of parents, country of birth, region (county) of residence, guardian persons' education and country of birth, and guardian persons' labor market status and income. (The 1960 census, from which the background information for the 1942-51 cohorts is taken, does not include information on parents' income.)

Figure 4: Educational attainment by cohort. Partial effect of parent income by quintile First quintile is the reference



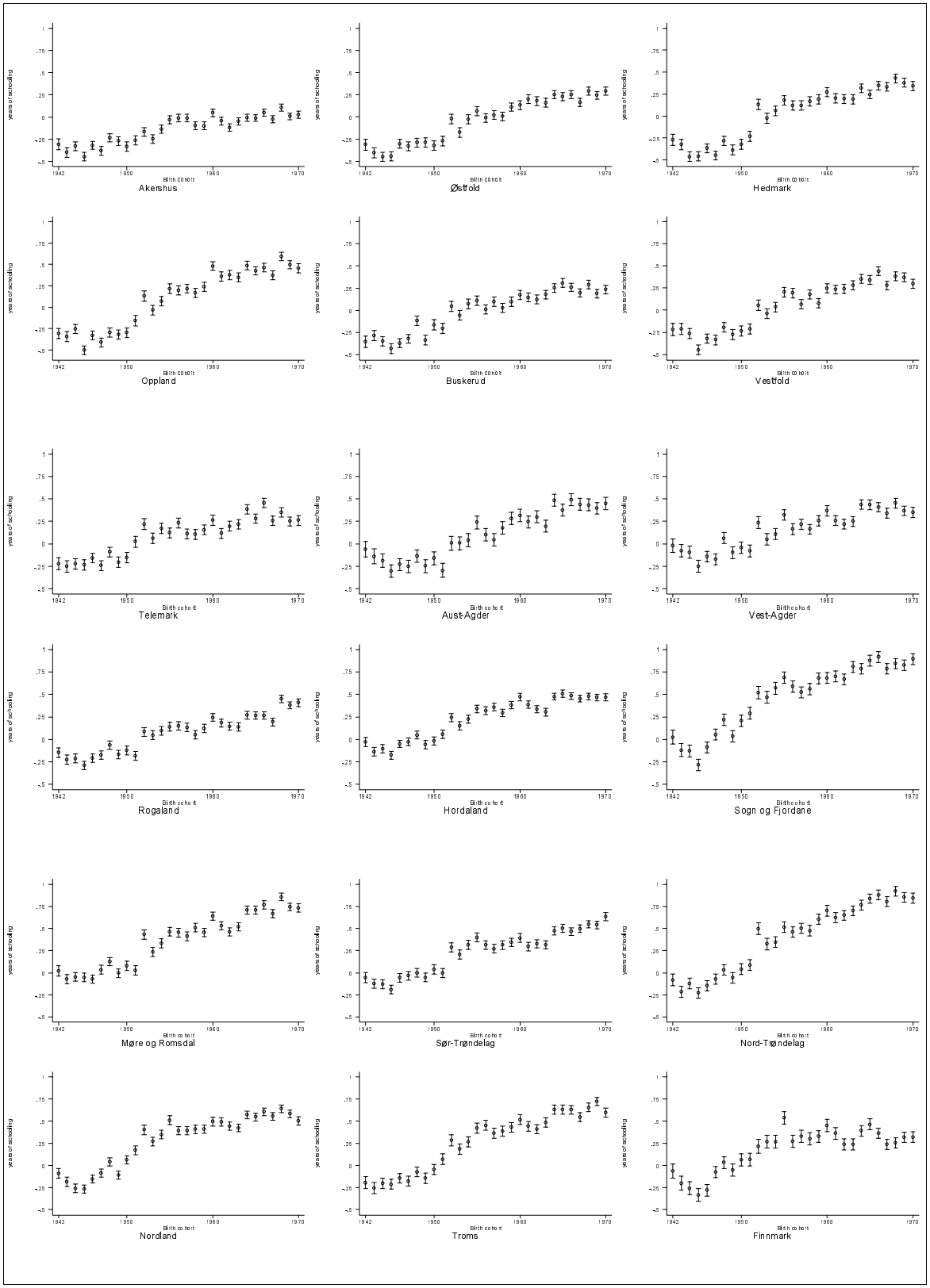
See notes to Figure 3.

Figure 5: Educational attainment by cohort. Partial effect of father's education. Primary education is the reference.



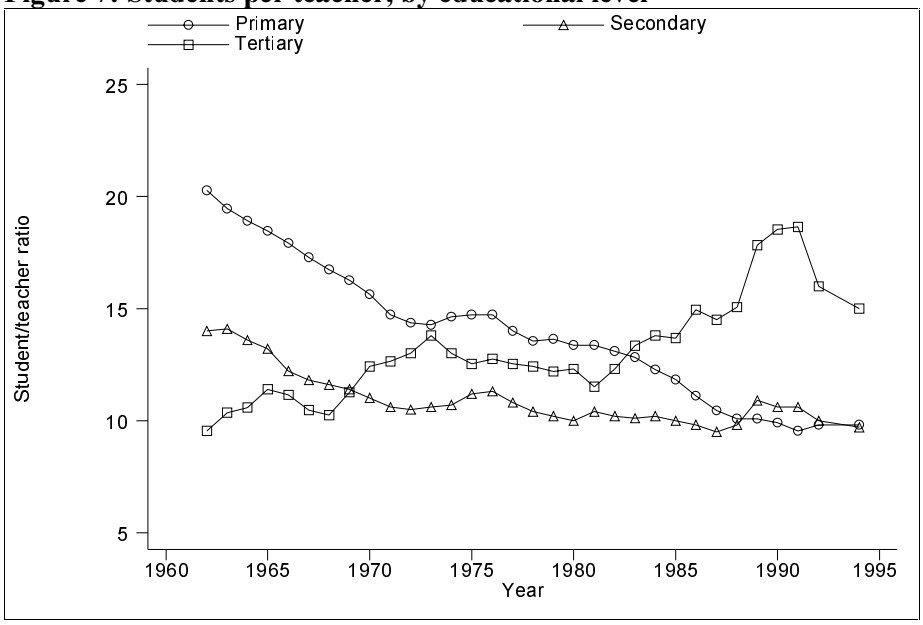
See notes to Figure 3.

Figure 6: Educational attainment by cohort. Partial effect of county of upbringing Oslo is the reference.



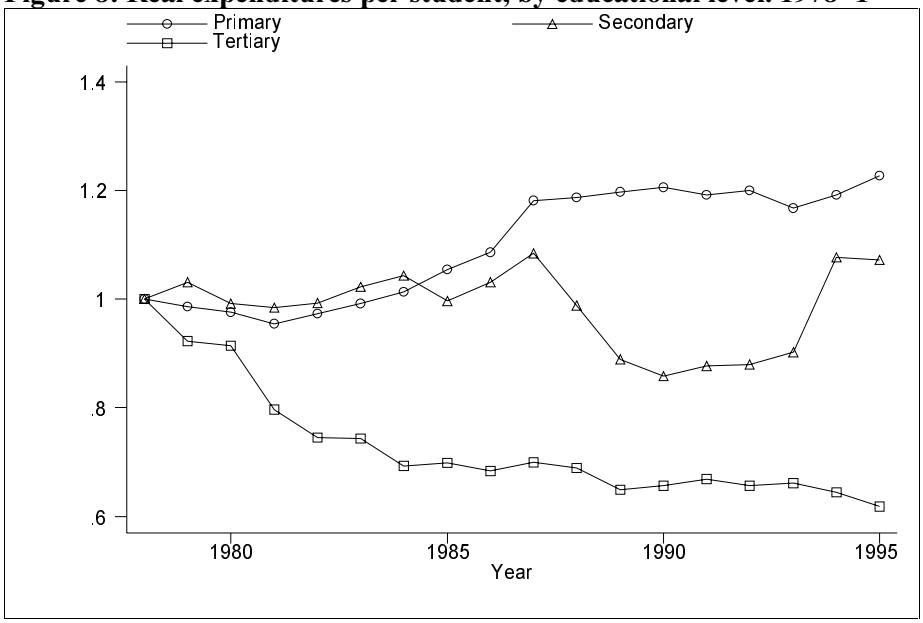
See note to Figure 3.

Figure 7: Students per teacher, by educational level



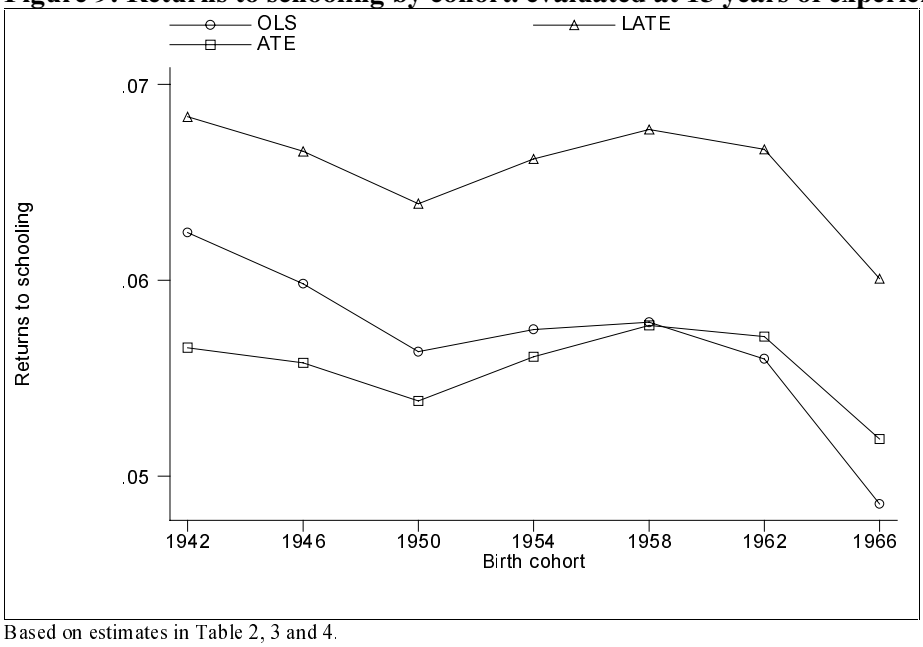
Source: Educational statistics, Statistics Norway, and author's calculations.

Figure 8: Real expenditures per student, by educational level. 1978=1



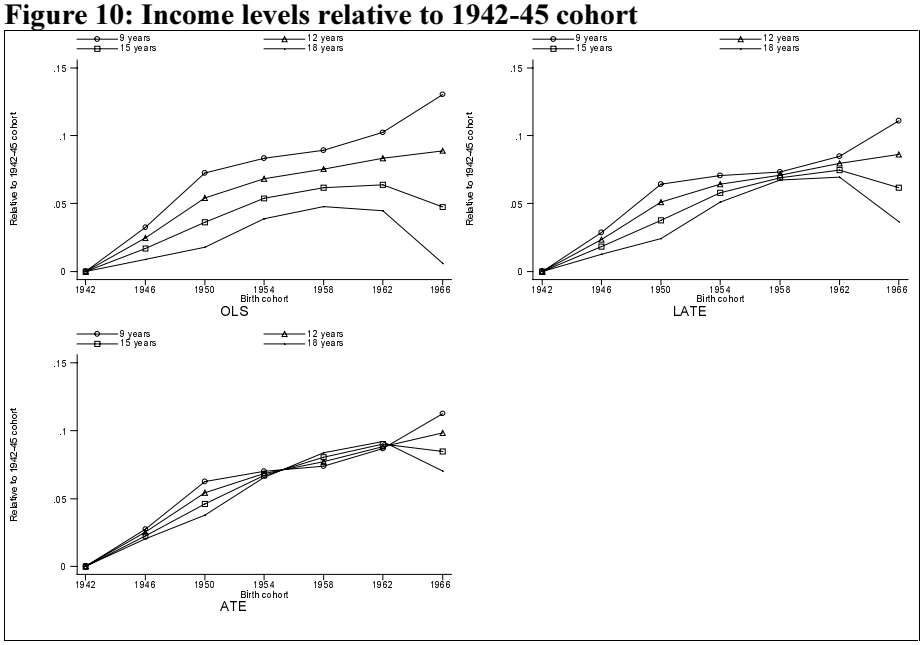
Source: Educational statistics and statistics for public sector spending, Statistics Norway and author's calculations

Figure 9: Returns to schooling by cohort. evaluated at 15 years of experience



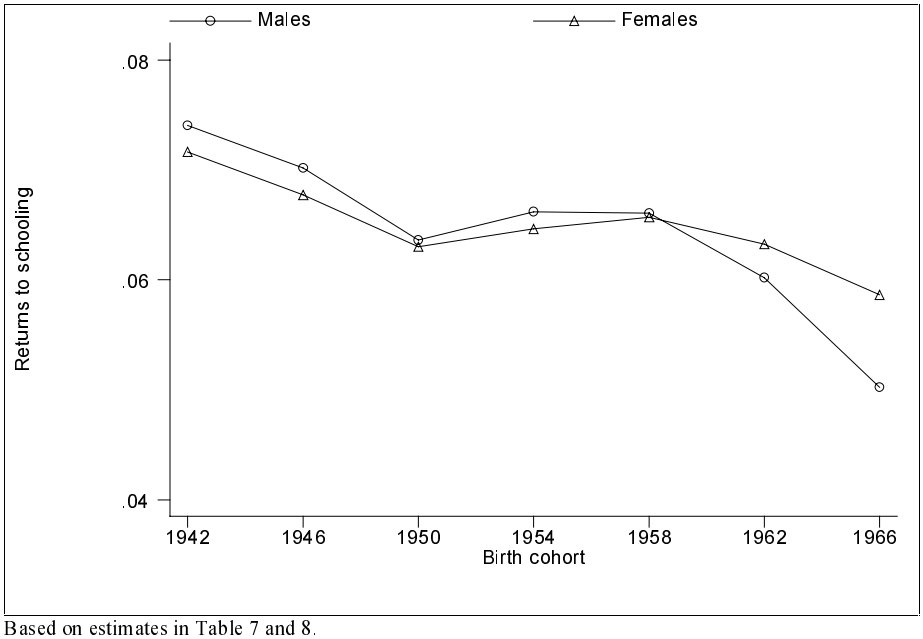
Based on estimates in Table 2, 3 and 4.

Figure 10: Income levels relative to 1942-45 cohort



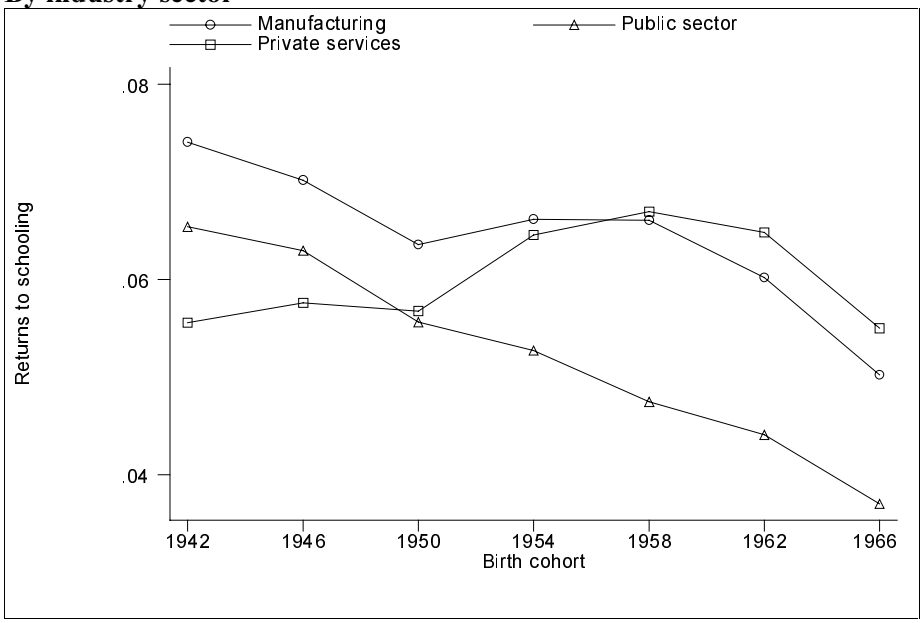
Based on estimates in Table 2, 3 and 4.

Figure 11: Returns to schooling by cohort. evaluated at 15 years of experience
Males vs. females



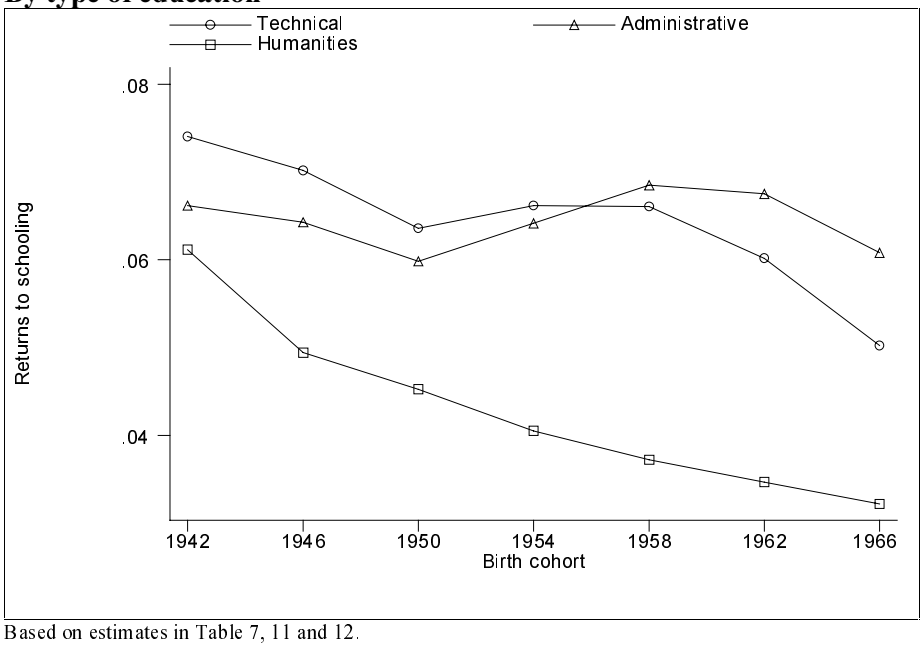
Based on estimates in Table 7 and 8.

Figure 12: Returns to schooling by cohort. evaluated at 15 years of experience
By industry sector



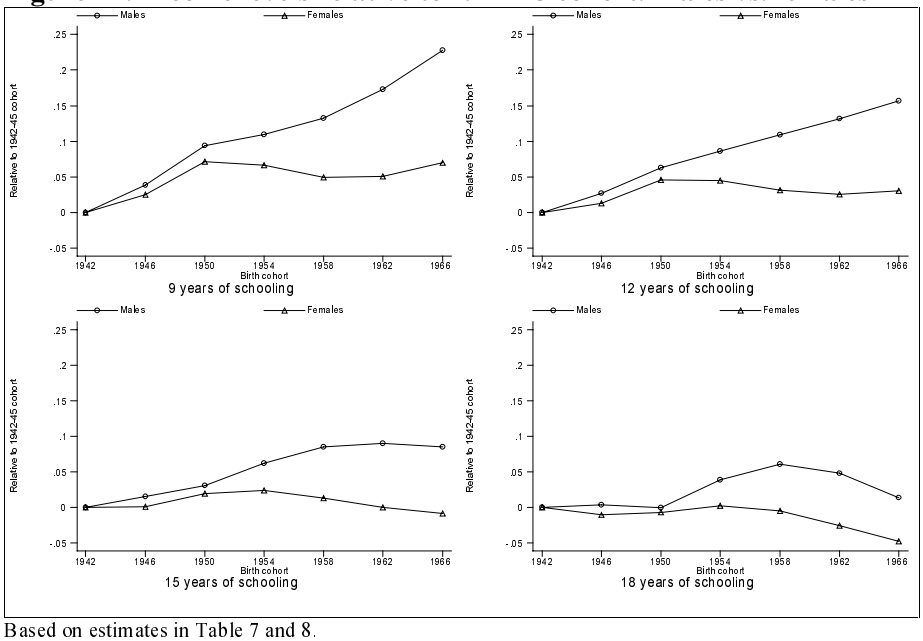
Based on estimates in Table 7, 9 and 10.

Figure 13: Returns to schooling by cohort. evaluated at 15 years of experience
By type of education



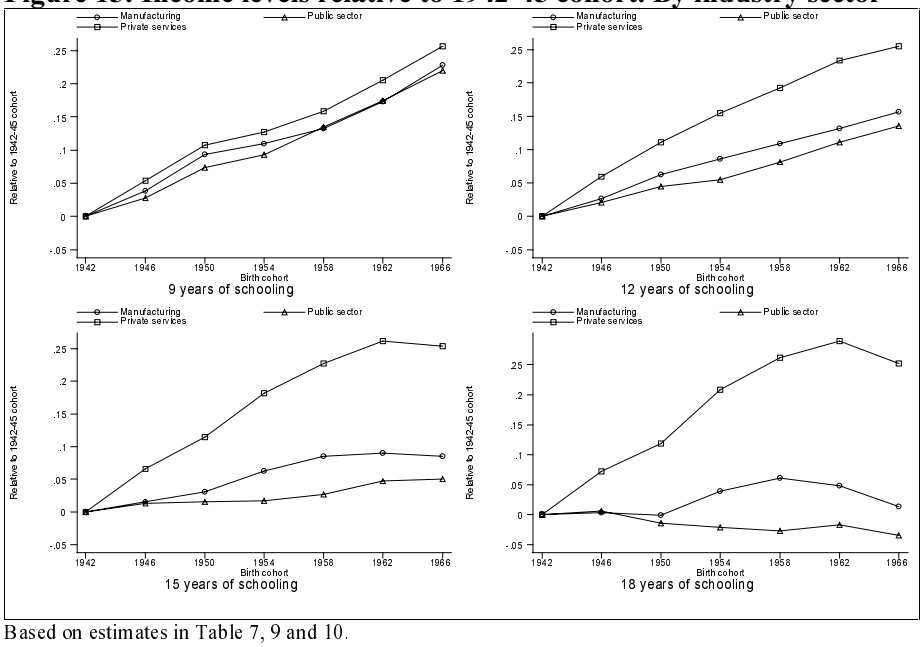
Based on estimates in Table 7, 11 and 12.

Figure 14: Income levels relative to 1942-45 cohort. Males vs. females



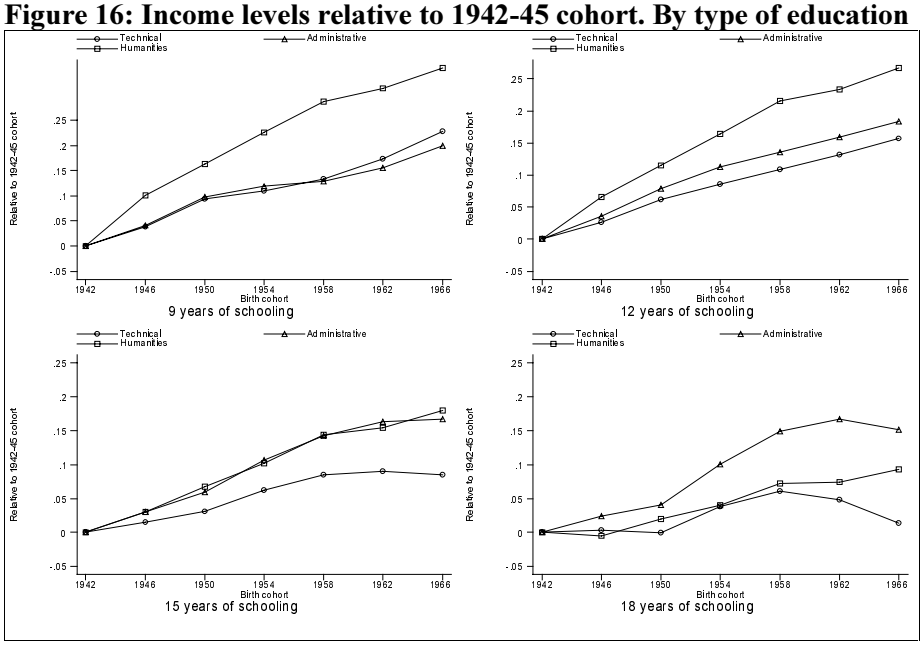
Based on estimates in Table 7 and 8.

Figure 15: Income levels relative to 1942-45 cohort. By industry sector



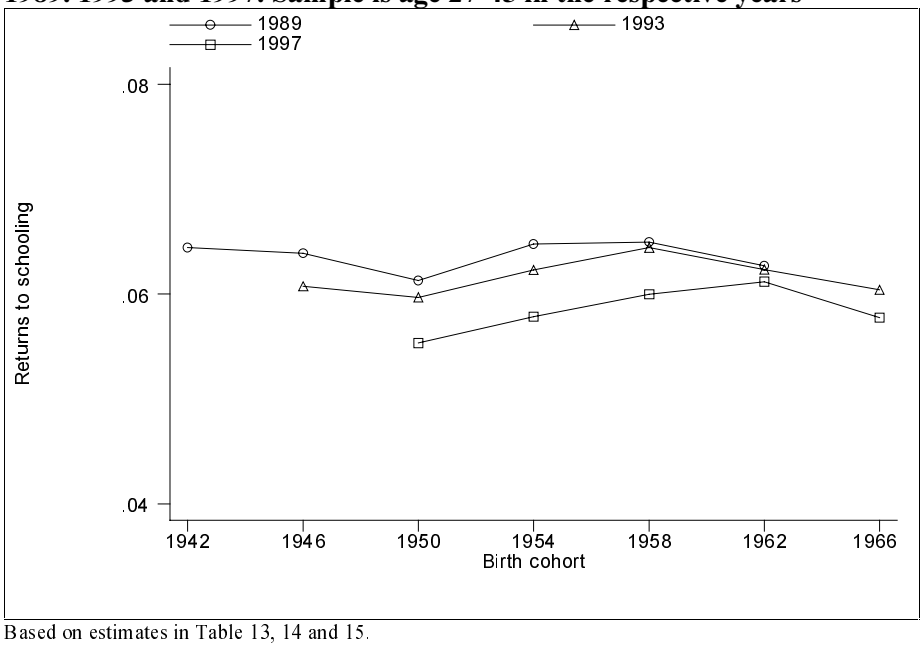
Based on estimates in Table 7, 9 and 10.

Figure 16: Income levels relative to 1942-45 cohort. By type of education



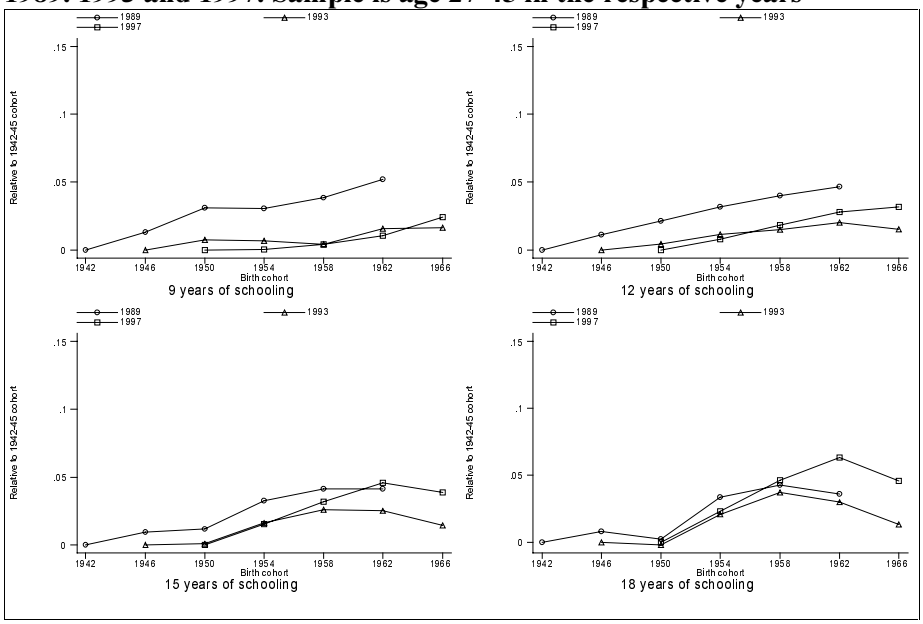
Based on estimates in Table 7, 11 and 12.

Figure 17: Returns to schooling by cohort. evaluated at 15 years of experience 1989. 1993 and 1997. Sample is age 27-45 in the respective years



Based on estimates in Table 13, 14 and 15.

Figure 18: Income levels by cohort.
1989. 1993 and 1997. Sample is age 27-45 in the respective years



Based on estimates in Table 13, 14 and 15.

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