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# Education effects on Health: Causal or from Unobserved Components? A Panel Data Analysis with Endogenous Education

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## Abstract

This paper investigates education effects on health, motivated by hypotheses that unobserved variables explain the correlation between health and education. Parametric models taking account of heterogeneity in health and endogenity of education are estimated separately for men and women using self-reported health, body mass index, high blood pressure and smoking as health variables. Education is instrumented by Danish school reforms and Hausman-Taylor type of instruments, and has significant effects on health for men and women. Endogeneity of education can be rejected for some health measures, but not all, whereas heterogeneity is present in all estimations. Rather than diminishing gradients, heterogeneity and endogeneity magnifies gradients. The results are inconsistent with two commonly postulated hypothesis about the effects of unobservables and support a hypothesis of causal educational effects on health.

Keywords: Health Gradient; Education; Selection Bias; Panel Probit Model, Two-Step Estimation

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## *Contents*

## 1 Introduction

During the last couple of decades, a considerable amount of attention, academic as well as public, has been given to differences in health and mortality that are related to social status. In European countries, several transnational working groups have addressed the issue[1], and many public authorities have set as a main health goal to fight social inequalities in health[2]. Most efforts to date have aimed at regulating damaging health behavior, notably smoking and drinking, through price regulation and information campaigns. There is evidence however, see e.g. Marmot (1994), that this will only have small affects on the distribution of health and that the processes behind health determination are poorly understood. In this paper we address how to interpret educational gradients in health[3]. Just as it has been shown that education enhances job opportunities and increases market productivity, we may interpret educational health differences as a return to education. Whether education related health differentials can be interpreted causally, is of tremendous importance for our understanding of determinants of health and for possible mechanisms through which we may affect its distribution. Much empirical work is not suited for this kind of interpretation however. One reason is that analysis using regression techniques often assume exogeneity of education, which, if violated, obviously produce biased results. Another problem is that many analysis are relegated to the use of non-linear models due to the nature of the available health measures. In non-linear models, biases of estimated regression coefficients arise, even under exogeneity, when serial correlation of unobservables is present. We address the issues of endogeneity of education and unobserved heterogeneity in health using a Danish panel data set.

There are different theoretical reasons why education may not be exogenous to adult health. We focus on two common explanations, which both imply that estimated education effects not allowing for endogeneity are upward biased, because important variables determining both health and education are unobserved. The first explanation, which we refer to as the endowment hypothesis, states, in brief, that when those with higher "ability" obtain more education and when those with a high health "endowment" (both interpreted broadly including genetics and investments by parents) are more healthy as adults, any positive correlation between ability and health endowments will imply a positive correlation between education and health. Notably, the endowment story also implies that health may have important unobserved persistent components. These will imply biases in non-linear models if not taken into account. The second explanation states that individuals with higher time preference rates are more likely to engage in activities with current costs and future benefits such as schooling and

[1] E.g. the ECuity group (led by Adam Wagstaff and Eddy van Doorslaer) and the working group at the Erasmus University Rotterdam (led by Anton Kunst and Johan Mackenbach).

[2] See for instance the British Acheson report (Acheson 1998) or the Danish Health program 1999-2008 (Sundhedsministeriet 1999).

[3] The term "the gradient" was used by psychologists Adler et al. (1994) to describe the fact that differences in health was graded, i.e. not limited to e.g. poor versus non--poor, but exist at all levels of socioeconomic status.

health investments, and dates back to at least Fuchs (1982). The endowment story has a long history in empirical work in many fields, where it is often dealt with using proxies for family background to control for the "unobserved" components. One of the most successful variants of this method is when information is available on twins, such that at least genetically determined endowments can be controlled for.

In spite of the vast number of studies on education related differences in health, few allow for endogeneity or unobserved components. One obvious reason is lack of data, because data needed to do for instance the twin estimations rarely is available. We pursue an approach, using panel data techniques to allow for unobservables. Moreover, we employ the strategy of Berger and Leigh (1989), applying supply side instruments for education: Danish school reforms. This approach is likely to be profitable both against the endowment and the time preference rate hypotheses as discussed below.

We use self-reported health status (SRH), body mass index (BMI), an indicator for never been smoking (NS) and an indicator for high blood pressure (HBP) as health outcomes from a two-period panel of Danish workers interviewed in 1990 and 1995 (The Danish National Work Environment Cohort Study (WECS)). We also evaluate the robustness of parameter estimates, by comparing the parametric model with estimates obtained using the semi-parametric monotone rank estimator suggested by Cavanagh and Sherman (1998). Semiparametric estimates of education effects are significant and, while differing from, has the same sign and qualitatively same size as standard parametric Probit and Logit estimates. To allow for endogeneity of education we specify a panel data quantal response model, that allows a time-invariant regressor to be correlated with unobserved individual effects. To identify education effects we use Hausman Taylor type of instruments and a regression discontinuity design obtained from Danish school reforms. We find that education gradients are significant, except for HBP, and homogeneity in unobserved health components is rejected in all estimations. Hypothesis of exogeneity of education can not be rejected for SRH for men, nor for NS and BMI for women. When heterogeneity and endogeneity is accounted for, education gradients are magnified rather than attenuated. Moreover, the correlation between unobservables and health is of the opposite sign as the correlation between education and health, which is inconsistent with the endowment and the time preference rate hypotheses. However, the estimated education effects vary depending on instruments and estimator used. Our main conclusion is, however, that we find significant education gradients and that they are not accounted for by unobservables.

The paper is organized as follows. In the next section we discuss theory and evidence of education effects on health. In section 3 we discuss the empirical model. Section 4 contains a discussion of the use of school reforms as instruments for education, section 5 describes the data and section 6 contains the empirical analysis. Section 7 concludes.

## 2 Education and Health : Theory and Evidence

We will now discuss different interpretations of the relationship between health and education in a simple reduced form model of health determination. We then pay some attention to identification strategies that have been implemented to identify education effects, and highlight a few empirical results.

An example of a reduced form model of health, explicitly derived from a structural model of health production is given in Grossman (1972) and extensions of the model and empirical work based on the model are discussed in Grossman (2000). Although important issues regarding specification of such structural models remain unresolved we find it convenient to uphold the framework of thoughts: Health depreciates over time but is maintained by health investments depending on consumed goods and activities that affect health. These are costly and time consuming, and optimizing behavior determines optimal inputs as a function of prices, income and technology parameters. A useful reduced form model is:

$$H_{it} = \alpha_w w_{it} + \alpha_e E_i + \alpha_a Age_{it} + \alpha_t D_t + B_{it} \tag{2.1}$$

Where $H_{it}$ is health stock of individual i at time t, $w_{it}$ is the wage, $E_i$ is education, $D_t$ are time effects capturing e.g. prices of health inputs and $B_{it}$ is an unobserved component. In Grossman (1972), the education effect is interpreted as an efficiency effect; the more educated are more efficient producers of health, given a set of health inputs. Others have conjectured that more educated choose better allocations of health inputs through a higher level of health knowledge, e.g. Kenkel (1991)[4]. We can obtain an estimate of education effects on health by estimating equation (2.1). The problem is that $\alpha_e$, the coefficient on education, is biased under alternative hypotheses about how health, education and unobservables, $B_{it}$, are related, which we now consider.

First of all, it is believed that many health components have genetic markers (see for instance Sickles and Taubman (1997) p.585-87, and Cawley (2000) for specific results and references on obesity), or at least, that child environment play a large role in health determination with possible long-reaching influences, see e.g. Vaagerö and Illsley (1995) or Grossman and Kaestner (1997). Therefore $B_{it}$ could be a persistent health endowment determined early in life[5]. This is one part of what we call the endowment hypothesis, that therefore implies the existence of unobserved heterogeneity in health. At this point we stress that when models of health determination are non-linear, any persistence of error terms over time will bias all estimated parameters, even if all regressors are strictly exogenous. However, there is more reason to worry.

The presence of heterogeneity in educational attainment is at the heart of most work in the human capital literature of educational choice, where education is chosen to maximize the present value of future earnings, and where the optimal choice depends on individual returns (often interpreted as ability) and individual marginal costs, see e.g. Card (1999). It seems undisputable that relative intergenerational educational immobility is large in most countries[6],

[4]Another way education affects health is assumed in Muurinen (1982), where health depreciation rates are lower for higher educated. This is discussed in Grossman (2000) and the empirical consequences of this assumption may be hard to distinguish from the one that is made in Grossman (1972).

[5]In terms of the model in Grossman (1972), $B_{it}$ can be interpreted as an unobserved efficiency parameter entering the production function in the same fashion as education.

[6]Where we by "relative" mean a ranking according to education levels of ones own generation.

see Mulligan (1999), and that performance in school is highly related to parents socioeconomic status, see Heinesen (1999) and Hansen (1999) for Danish results. It therefore makes good sense to model educational attainment as a function of individual ability and family characteristics. When individual ability is positively correlated with health endowments, estimated education effects are upward biased in a linear model, when education is treated as exogenous. This is the endowment hypothesis[7].
An alternative interpretation of education coefficients was given by Fuchs (1982). Fuchs suggested that differences in time preferences may explain the correlation between education and health; those paying more attention to future welfare are more likely to engage in activities with current costs and future benefits, such as education and health investments[8]. We stress that in order for this to be the explanation, either must health and education affect utility directly, or income must yield utility. If they are wanted only for investment purposes, as is the case in Grossmans pure investment model and in most human capital models of education, time preferences play no part in optimal solutions[9]. However, a priori it cannot be ruled out that unobserved components, $B_{it}$, depends on time preferences. If they do, and education does as well, $\alpha_e$ is again upward biased. This is the time preference hypothesis.

For a comprehensive review of empirical findings within economics, see Grossman and Kaestner (1997). In the current context, we stress that in order to be able to distinguish different explanations of why education and health are related, different identification schemes are needed. We briefly look at some of the methods that have been used.

The by far most common technique is regression analysis, where identification hinges upon use of adequate controls. One of the most extensive analysis of this kind in this literature, is Grossman (1973). Grossman corrects for "omitted" variables and reverse causality between education and health by including four different testscore-indices, mother and fathers education and health in high school respectively. He finds large and significant direct effects of both education on SRH and mortality. The drawback of this approach is that it does not allow for effects of unobservables. It is likely that for instance test-scores are only crude proxies of what we intend to measure, and in some cases this may exacerbate the bias that exist when the proxy is not included, see e.g. Rosenzweig and Wolpin (1994).

A second approach that circumvents the problems of the first, is to instrument education. Again, different hypotheses require different instruments. If the worry is reverse causality, one might instrument education with childhood health. If one is worried because of the endow-

[7]A slightly different story of inverse causality is given in Grossman (1973). There, $B_{it}$ is an initial (thus time invariant) health depreciation value, which affects both child and adult health. When more healthy children obtains more education, education becomes indirectly related to $B_{it}$, and $\alpha_e$ will be upward biased. Grossman (1973) later notes that $B_{it}$ may be related to schooling, essentially because it depends on family characteristics, bringing us back to a common factor explanation. Our approach is more general in distinguishing between two unobserved components, affecting health and schooling respectively. The bias of $\alpha_e$ is not determined a priori, but depends on the sign of the correlation between "health endowment" and "ability".

[8]Fuchs also mentions the possibility that schooling has an effect on time preferences, but in his empirical investigations he cannot distinguish between the direction of causality between schooling and time preferences. The latter causality-path has been considered recently at more length by Becker and Mulligan (1997).

[9]This argument is valid with perfect borrowing markets.

ment and the time preference rate hypothesis, one need instruments that are orthogonal to health endowments or time preference rates. As discussed by Grossman (2000), the study by Berger and Leigh (1989) is interesting in this respect, because they use per capita income and per capita expenditures on education in the state of birth as instruments for education, which seems more plausible candidates than e.g. family characteristics and childhood health. Berger and Leigh find that correcting for endogeneity, education effects are reduced slightly but remains significant. Since they do not discuss or test the validity of their instruments, we mention that in their first stage regression, state per capita expenditure on education has no significant effect on education. They apply the method of Garen (1984), which gives direct estimates of the effect of unobservables on health. Under the endowment and time preference hypothesis, unobservables have positive effects on health. In their application, the effects of unobservables on health are insignificant. Grossman and Kaestner (1997) mention other studies investigating the time preference hypothesis, looking at health behavior notably by smoking, and does therefore not deal with the education gradient in health per se. They do conclude that the existing, sparse evidence does not favor the time preference hypothesis.

Another identification scheme is the use of within family correlations. An example of an application of this method is Behrman and Wolfe (1989). They use education of the respondents sister as control for endowments and, as suggested by Chamberlain (1980), get an estimate of the true education effect as the difference between coefficients on own education and sisters education. Behrman and Wolfe also apply the related method of household fixed effect estimates. These identification strategies are valid when differences in school levels between sisters are unrelated to unobserved determinants of health. Compared to the case when no account of unobserved factors is taken, the first method yields smaller education effects in the study by Behrman and Wolfe, but they remain of substantial size and are significant. The household fixed effect estimator produces larger education effects, as well as larger imprecision, leaving education significant only at a 10 percent level.

We finally note that none of these studies allow for unobserved individual heterogeneity in health over time. As both the endowment and the time preference rate hypotheses imply that health is affected by unobserved (possibly) time-invariant factors, this is worth considering. If unobserved heterogeneity is an issue, and not taken into account, all non-linear models and models which condition on past health (linear as well as non-linear), as in Berger and Leigh (1989) and Grossman (1973), produce inconsistent parameter estimates.

As it stands, although none of these studies suggest that taking care of endogeneity alters the effect of education by much, no firm conclusion can be reached from the sparse evidence. Furthermore, only one study uses instruments most likely to be robust to heterogeneity in time preferences and, to our knowledge, none allow for individual specific heterogeneity over time.

## 3 Empirical Model

Our empirical model is a reduced form model of health and educational determination. Although we will use other health measures in the empirical analysis, in this section we specify the empirical model using SRH as dependent variable. Education is measured in years of education. We model health and education with an ordered discrete response and a linear regression specification respectively:

$$H_{it}^* = X_{1it}\beta + \gamma E_i + \varepsilon_{it}$$

$$(3.1) \qquad H_{it} = \sum_{j=1}^{K} 1(H_{it}^* > c_j)$$

$$E_i = X_i \Pi + V_i$$

where $H^*$ is latent health, $H_{it}$ is observed health category for individual i in period t, changing value when $H^*$ crosses an unknown threshold, $c_j$, $E_i$ is years of education and $X_{1it}$ are exogenous regressors in the health equation and $X_i$ are exogenous regressors in the education equation.

We allow for endogeneity of education through correlation between $V_i$ and $\varepsilon_{it}$. If $\varepsilon_{it}$'s are independent over time and normal distributed, we can estimate this model, using the efficient limited dependent variable estimator described in Newey (1987), which we refer to as the AGLS estimator, since it is obtained as a modification to the generalized least squares estimator in Amemiya (1978). This is described in the appendix. To capture the idea that unobserved individual components determined prior to educational attainment selectively sorts individuals into high and low health groups, we specify an individual effects model:

$$(3.2) \qquad \varepsilon_{it} = \alpha_i + v_{it}$$

where $v_{it}$ is assumed to be random. If $\alpha_i$ is non-degenerate, but uncorrelated with covariates, we have a random effects model, and non-linear estimation methods like the AGLS not taking this into account are inconsistent. When $\alpha_i$ is allowed to covary with education, a random effects model is not valid. Other standard methods of non-linear panel models cannot be applied either. This includes the conditional Logit model and the minimum distance method suggested by Chamberlain (1984), which cannot identify the coefficients on time-invariant variables such as education. Because we apply a linear index specification, we need exclusion restrictions on X to identify $\gamma$ when $V_i$ and $\varepsilon_{it}$ are correlated. Let therefore $X_{2it}$ be instrumental variables in the education equation. The contents of $X_{2it}$ will be discussed below.

To deal with this problem, we use a control function approach[10]. The estimator works with the conditional distributions of $\varepsilon_{it}$ given $V_i$, which we assume has a mean linear in $V_i$[11]:

$$(3.2) \qquad E(\varepsilon_{it}|V_i, X) = E(\alpha_i|V_i, X) = \eta V_i + w_i$$

where $\eta$ is the regression coefficient of $\alpha_i$ on $V_i$, such that $w_i$ is orthogonal to $E_i$ by construc-

[10]This can be shown to be a special case of the two stage conditional maximum likelihood estimator considered by Vella and Verbeek (1999)

[11]Clearly this can be motivated from a joint normality assumption, and we could allow for non-linear effects to incorporate other joint distributions

tion. Substituting this into the model we get:

$$H_{it}^{*} = X_{1it}\beta + \gamma E_i + \eta V_i + w_i + \nu_{it} \tag{3.2}$$

such that γ can be estimated consistently by random effects, when $V_i$ is replaced by a consistent estimate. We call this estimator the random effects two stage conditional maximum likelihood estimator, abbreviated 2SMLR. Without the random effects, this is the two stage conditional maximum likelihood estimator (2SCML) considered for the Probit model by Rivers and Vuong (1988). Note that under the time preference hypothesis, and if education ability is positively correlated health endowment also under the endowment hypothesis, we expect η to be of the same sign as γ.

We accentuate three attractive properties of this model. First, it provides an easy accessible test for exogeneity of E, namely the t-test that η is zero, see Rivers and Vuong (1988). Second, testing exclusion restrictions that identify the education coefficient, is easily done in the spirit of tests suggested by Hausman (1978) in a linear model, see e.g. Bollen et al. (1995). The validity of excluding variable $X_{2k}$ can be tested with a t-test that the coefficient corresponding to $X_{2k}$ is zero in the second stage estimation, where $X_{2k}$ is included and the residual $V_i$ is estimated using the other excluded $X_2$ variables. Finally, the specification allows the use of instrumental variables generated from $X_{1it}$ as suggested by Hausman and Taylor (1981). We note that corrections of standard errors for the first-stage regression are needed for results to be asymptotically correct, but we have not done that. It therefore might be worth mentioning that Bollen et al. (1995) mention findings from Monte Carlo investigations, showing no gain in correcting for first-stage estimates for the similar cross-sectional estimator considered by Rivers and Vuong (1988).

## 4 School Reform as Instrumental Variable

In the last section we mentioned that "mechanical" Hausman Taylor instruments can be applied in the panel model we suggested. These instruments rely on assumptions on the covariance structure between error terms and regressors, as well as exclusion restrictions of individual means over time of exogenous regressors in the health equation. In this section we discuss the use of other instrumental variables, using information on two school reforms that took place in Denmark in 1958 and 1975. We use information from yearbooks from the Danish Parliament (Folketingets Årbog, 1903-4, 1957-58 and 1974-75) and Bryld et al. (1990).

In 1904 the middle school (Danish: mellemskolen), classes from grade 6 to 9, was introduced aimed at preparing pupils for gymnasium. Pupils had to pass a test after 5th grade, and those who did not pass would continue education for two more years only. The major change initiated by the 1958 reform, was that the partition into preschool and middleschool was abolished. From this year everybody received the same first 7 years of schooling, and formal tests to continue into grades 8-10, preparing for gymnasium, were replaced by teachers recommendations. In addition, the village school (Danish: landsbyskole) gained the same right as the city schools (Danish: Købstadsskole), to form classes for grade 8-10, which increased the proximity of schools necessary for further education at the countryside. The reform in 1975 raised the minimum school-leaving-age, increasing the compulsory years of education from 7 to 9 years. The extent to which this had an impact on mean education is however very dubious, since most children continued into grade 9 voluntarily already. Moreover, many factors contributed to the increasing demand for further education in the post-war decades, many of which long before the reforms. For a viewpoint that the 1958-reform was an important contributor to this development see e.g. Hansen (1982).

We generate two dummy instrumental variables from the 1958 and the 1975 reforms, one for individuals aged below 34 in 1995 and one for individuals aged between 33 and 51 in 1995. Individuals in the first group were less than 14, i.e. in 7th grade or below, in 1975 and were therefore affected by the 1975 reform. Individuals in the second group were 14 or above in 1975 and less than 14 in 1958, i.e. they were affected by the 1958 reform, but not by the 1975 reform. The comparison group is those not affected by any of the reforms.

When are the dummies for whether or not individuals were affected by a school reform valid instruments? The identification scheme used, is known as a regression discontinuity design[12]. The literature distinguishes between a "sharp" and a "fuzzy" regression discontinuity design, see e.g. Hahn et al. (2001). In the former, the instrumented variable as a function of the instrument has mechanically determined discontinuity points, in the latter the conditional probability distribution of the instrumented variable given the instrument has discontinuity points. In an ideal world these breaks generate natural experiments, creating exogenous changes in endogenous variables. In our case, the 1975 school reform ensures that the length of education is at least 9 years, such that those who wanted to stop after seven years are forced into education, thus resembles a sharp design. If anything, the 1958 reform is a fuzzy design, in that it lowers barriers for further educational beyond grade 7. Below we check the fuzzy design by calculating the share with given educational attainment at given ages in our sample.

There are other concerns related to the use of discontinuity designs. First, as all instruments, it must be uncorrelated with the unobservables determining the main outcome variable. Therefore we need to take account of upward drifts over time in health, which are automatically positively correlated with the increases in education implied by the instruments. To do this we have to assume that other events, driving the upward drifts, can be captured by preferably linear cohort effects, to diminish the multicollinearity between school reform dummies and the drifts.

There are several other points that must be considered when using IV estimation. It has been shown that when instruments and endogenous explanatory variables are only weakly correlated, inconsistency of IV-estimates relative to OLS may blow up, see e.g. Bound et al. (1995). The relative inconsistency is inversely proportional to the partial R-squared of the instruments in the first stage estimation. A second problem is that IV estimates are biased towards OLS in finite samples, a bias which can be substantial even in very large samples. The bias has been shown to be inversely proportional to the F-statistic on instruments in the first stage. It is therefore informative to report the partial F-statistic and R-squared on the instruments from the first stage estimation. Finally, a linear IV-estimator identifies a weighted average of the

[12]A well-known example in economics is Maimonides' rule, applied as an instrument for class size effect on test-scores, see e.g. Angrist and Krueger (1999). An example of use of school reforms is given in Harmon and Walker (1995), estimating the returns to schooling on wages.

effects in the sub-populations affected by the school-reform, see e.g. Card (1999) p.1821. In our specific case, if education effects are heterogeneous, the estimated effect will be a weighted mean of effects for those individuals affected by the reform (a local average treatment, or LATE, effect). These are most likely to be individuals from families from the countryside and individuals who were least likely to have been sorted in to the middleschool. Because these individuals are likely to have low health, we argue that even with heterogeneous education effects, the estimates are of considerable interest.

## 5 Data

We use a two-period data set of Danish workers interviewed in 1990 and 1995 (The Danish National Work Environment Cohort Study (WECS)). Since we will use the panel structure of the data, we only use observations with valid data in both 1990 and 1995. The data set includes self-reported health, graded as poor, fair, good, very good and excellent, which will be our main health outcome, as well as socio-economic information on education, occupation, work type-and time, and a large array of work environment variables. Because the education variables differ in the two surveys, we choose to use the 1995 question, combined with information on primary schooling if no education is undertaken. A more detailed description of the data and our specific sample, education, occupation, industry and regional variables, as well as the construction of the hourly wage, is found in the appendix. We end up with 2023 men and 1732 women who are observed in both 1990 and 1995. As our sample does not differ much with respect to age, education and occupational distribution, from the unbalanced sample, see Borg and Burr (1997), we preserve a reasonable representative national sample of individual workers employed within the last two months who have finished their education.

Finally a word on the SRH variable. SRH it is generally believed to be a useful summary measure of health, since it allows to measure health at the individual level, and is believed to capture important intervening mechanisms leading to increased risk of functional disability and mortality, see e.g. Idler (1994). Furthermore, the common inclusion of SRH in health and socioeconomic surveys makes it easy to compare results with other findings. However, a large debate on possible measurement error in SRH exist, see e.g. Butler et al. (1987) or Benítez-Silva et al. (1999), but since we instrument education, measurement error in SRH that is related to education is likely to be of minor importance[13].

For robustness, we also apply the following alternative measures of health: self-reported weight and height to form the body mass index[14], self-reports of whether never been smoking, years of smoking and whether a doctor has informed the interviewed that he or she has high blood pressure (HBP).

## 6 Results

[13]Given that our instruments are unrelated to the measurement error.

[14]Weight in kg, divided by height in meters squared.

### 6.1 Descriptive Statistics

In table 1 we present means of important variables as well as mean SRH across different groups. From the column named mean SRH, we see that SRH decreases with length of education and varies with age, occupation and number of subordinates. There are some notable non-linearities at especially 11 and 13 years of education. In table 2 we present simple logit estimates of SRH on dummies for each length of educational attainment and age groups, to check whether non-linearities arise due to age differences in educational attainment and whether age has linear effects. As seen, the same non-linearities arise when controlling for age. For age, there is tendency to lower age effects at higher ages for men, than that implied by a linear effect. These are however insignificant, so age can be entered linearly. This is consistent with age patterns for mortality for the age group in consideration. The non-linearity of education effects is primarily due to very poor health of semi-skilled people who are categorized as having 11 years of education. These have at most one year of education on top of primary school, and are often employed in physically very demanding jobs, which probably explain the particularly poor health of this education group. Excluding this group of workers changes simple logit estimates of the education coefficient by approximately 10 percent, from -0.045 to -0.040 for men and from -0.049 to -0.044 for women. Because a key goal of our analysis is to compare overall effects on education gradients from application of different estimation methods, a simple representation of the education gradient is preferable, and we assume linearity in the following. Table 3 shows transition probabilities between health status groups from 1990 to 1995. The marginal distributions in 1990 and 1995 are quite alike, but the table shows that there is substantial mobility between health groups over time.

In figure 1 we depict the distribution of education across age, to check whether school reforms have a fuzzy design impact on education. We show the share with more than 7, 9 and 12 years of education as well as mean education, along the year in which they aged 14. This is to make it easier to compare whether they were affected by laws altering the transmission into 7th grade. We see a general trend towards increasing education. This is particularly large for individuals aged 14 from 1956 to 1960, when looking at those obtaining more than 12 years of education, and those aged 14 between 1956 and 1972 when looking at the share who obtains more than 7 years of education. It therefore seems as if the fuzzy design is fulfilled for the 1958 reform, by the large differences in education between those aged 14 in 1957 and those aged 14 in 1958.

### 6.2 Model Estimations

In this section we consider the results from ordered quantal response estimations of SRH groups. We start by comparing parametric and semi-parametric estimates and continue by examining the impact of endogeneity and heterogeneity. All categories of SHR are used in order not to throw away information. As control variables we include a year dummy and, to take account of wage and other job related factors affecting health, we use a dummy for being in a position with subordinate workers and for being white- or blue collar worker, unskilled, in other work, using those out of work as the reference category. Age and education are included linearly.

#### 6.2.1 Semi-Parametric Estimates

As a starting point, we consider robustness issues assuming all regressors are exogenous. Results from an ordered probit model are presented in column (1) in table 4 for men, and in column (4) for women. One alternative estimator for our model, is the semiparametric monotone rank (MR) estimator suggested by Cavanagh and Sherman (1998). This estimator makes no distributional assumptions, is very general in functional form and is consistent under certain types of heteroscedasticity. It is described in the appendix. To compare probit estimates with semiparametric estimates, we have normalized all coefficients setting the age coefficient to one[15]. The standard errors of the probit estimates are calculated using the delta method[16]. The MR estimates are presented in column (3) and (6) of table 4. Logits are included in columns (2) and (5) for completeness. The semiparametric estimates differ from the parametric estimates. A priori the conditions are fulfilled for conducting a Hausman misspecification test, since both the MR and MLE estimators are root-n asymptotic normal under the null, that the conditions for the MLE are fulfilled. In practice, when covariance matrices are estimated, the difference in covariance matrices between the MLE and MR is indefinite[17], so the Hausman test cannot be carried out. We note though that MR-education coefficients are of the same sign as ML-coefficients and they are significant. The MR estimates for the education coefficient is 26 percent higher for men and 10 percent lower for women than the logit estimate. The key assumption driving consistency of the MR estimator is that the mean of the dependent variable is monotonic in the estimated index Xβ. This is required for ordered logit and probit models as well. We check this by non-parametric regression of SRH on the estimated index Xβ, using the MR estimates of β, see the appendix

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[15]The MR estimator is estimated with GAUSS[TM], ver. 3.2.4, using a Fortran minimization procedure from Numerical Recipes (www.nr.com) and is written in GAUSS by Bo E. Honoré. We thank him for making the code available to us, which we of course use on our own responsibility. The variance estimates are obtained using the formulas in Cavanagh and Sherman (1998), and conditional means and densities in these formulas are estimated by kernel estimation. We use gaussian kernels and Silvermans rule of thumb bandwidth.

[16]Obtained as follows, where $\beta_a$ is the age coefficient and $\beta$ are all other coefficients:

$$\mathrm{cov}(\frac{\beta}{\beta_a}) = \nabla(\frac{\beta}{\beta_a})\,\mathrm{cov}(\beta,\beta_a)\,\nabla(\frac{\beta}{\beta_a}), \text{ where}$$

$$\nabla(\frac{\beta}{\beta_a}) = [\frac{\partial\beta/\beta_a}{\partial\beta}, \frac{\partial\beta/\beta_a}{\partial\beta_a}] = [\frac{1}{\beta_a}, -\frac{\beta}{\beta_a^2}]$$

[17]It has both positive and negative eigenvalues.

for details. The results are shown with 95 percent pointwise confidence bands in figure 2 for women and in figure 3 for men. The plots are very smooth and although there are small regions of nonmonotonicity, they are insignificant. The consistency of the MR estimator is supported by the finding that results are not affected by excluding the fifth of individuals with low index values, which seems to be the region with largest possibility of non-monotinicities.

### 6.2.2 Logit Estimations

In this section we present results from estimations based on parametric estimators including the 2SMLR estimator discussed earlier. We have seen in the last section that parametric estimations are fairly robust to functional and ditributional form. Since it does not seem to matter whether to use a probit or a logit specification, we choose the logit.

Table 5a and 5b contain the results applying different versions of the ordered logit model, namely the simple ordered logit, the ordered logit with random effects, the AGLS and the 2SCML, both with no random effects but endogenous education, and finally the 2SMLR estimator. Recall that our health measure is 1 for excellent health and 5 for poor health, implying that a negative coefficient shows that the corresponding variable varies positively with better health.

Starting with the results for men, in table 5a column (1), the simple ordered logit is presented. Both education and occupation variables are significant, education with a coefficient of -0.041, such that more educated have better health, white collar workers are more healthy than blue collar and unskilled workers, but of similar health as those in other types of work. It is also observed that having any subordinate workers indicates better health also in this multi-variate framework. In column (2) we take account of unobserved heterogeneity, allowing for individual random effects being correlated over time for each individual. We use a discrete factor approximation to estimate the distribution of individual effects, see for instance Mroz and Guilkey (1995), i.e. we do not make any distributional assumptions[18]. The location and masspoints of these points are estimated freely. For the 2SMLR estimator presented in column (6), a discrete distribution with three points of support is found to fit the data better than two and equally good as four. We use three points of support in other random effects estimations as well. We see from column (2), that when random effects are included, the education coefficient increases in magnitude and remains significant.

The presence of random effects is tested, comparing estimations in (1) and (2) by means of a likelihood ratio test. Minus two times the log likelihood ratio is presented in the first row below parameter estimates. This is chi-squared distributed with five degrees of freedom, the number of parameters in the random effects distribution[19]. As the critical value in this distribution at a five percent level is 11.07, and the observed test statistic is 219.3, we reject the null of no heterogeneity.

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[18]Estimations are conducted using Stata$^{TM}$ ver. 6.0, using the program GLLAMM ver. 6, see Rabe-Hesketh et al. (2001).

[19]Three mass points and two probabilities that vary freely.

In column (3) and (4), results from AGLS estimation are presented[20]. These are valid under the assumption that individual errors are uncorrelated over time, but allow for endogeneity of education. In column (3), we use school reforms and means of exogenous variables as instruments, the latter being Hausman-Taylor type of instruments. This AGLS estimate of the education effect is of equal size to the random effects logit estimate, but the standard errors blow up leaving education insignificant. Looking at the bottom of the table, we present results from the first stage estimation. As expected from looking at figure 1, the school reform in 1958 comes out highly significant, implying that those affected by the reform obtain half a year longer education than those not affected. The effect of the 1975 reform is surprisingly almost as large in size, but insignificant. Just above these, we present the partial R-squared for all instruments, and the partial F-statistic for the Hausman-Taylor instruments and for the school reform instruments. The F-statistics are high for both sets of instruments, 38.66 for the former and 9.81 for the latter, indicating that given validity of the instruments, the bias relative to non-instrumented estimates in finite samples is low. The partial R-squared of 0.054 is also high, so neither does there seem to be a problem of weak instruments. We perform t-tests of the validity of the instruments in a 2SCML estimation, as described in the section where the empirical model is presented. The t-statistics are presented in table 6. We start out assuming all but mean age are valid instruments. Moving from top to bottom row, instruments not passing the test are left out of the first stage for the following tests. With this procedure, sequence of testing might matter, but it turns out that alternating sequences does not alter the results. From this table, it is confirmed that school reforms do not affect health directly for both men and women, as necessary for their validity as instruments. We note though that the t-statistic on the 1975 reform is rather high, 1.57, but it does not alter results if this is left out. Neither does the mean of age and dummies for being blue collar, unskilled and other worker have any significant effect on SRH for men, nor the mean of age, and dummies for being white collar, other worker and having any subordinates for women.

Continuing in table 5a, column (4) presents AGLS results with only those instruments which passed the test of validity. The F-statistics increase and the partial R-squared decrease a bit, with no reason to worry though. The coefficient on education decreases compared to column (3) but is still insignificant. In column (5) we use the 2SCML estimator to allow for endogeneity. Using the same instruments as in (4), the coefficient on education is -0.113, which is significant at the 10 percent level. Note that only the 2SCML and the AGLS estimators are directly comparable, since they are derived in the same distribution (namely $\varepsilon|V$, rather than the simple logit using the marginal distribution of $\varepsilon$ or the random effects models, where individual effects are integrated out), which is important since we only identify the ratio between parameters and the standard deviation of the error tem. We have no immediate explanation for the difference in the AGLS and 2SCML estimates. An advantage of the conditional two-stage estimators is that they provide a test of exogeneity: the t-test on the education residual. The coefficient on the residual is positive but insignificant. Column (6) contains the 2SMLR estimates, with endogenous education and individual specific effects. The null of no random effects is again rejected. The coefficient on the education residual increases, but it is still not significant at a 10 percent level, that is, we cannot reject exogeneity of education with respect to SRH for men. Finally, in column (7), we use only school reforms

[20]This is Neweys version of the AGLS with corrected standard errors. We modified a stata program written by J. B. Gelbach implemented for the probit model (Probitiv, ver. 5 1997), to do ordered logit models.

as instruments in the first stage estimation. As seen from the bottom of the table, while the F-statistic hardly changes, this dramatically reduces the partial R-squared, indicating a problem of weak instruments. We note that a partial R-squared of 0.005 is about 50 times higher than the famous quarter of birth instruments used by Card and Krueger, see Bound et al. (1995). However, this does not alter that most of the identification in the previous estimations came from the assumptions justifying use of Hausman-Taylor instruments. Both education and residuals are now insignificant.

Moving forward to table 5b, we look at results for women. The estimators with no random effects are again rejected, where a better fit is obtained using three rather than two points of support for the random effect, and no gain is obtained using four points. From the first stage estimations, we see that the F-statistic on school reform instruments is a bit lower than was the case for men, 3.78, although still being above the 95 percent quantile. Overall, with the exception of column (7), there is no evidence of substantial finite sample bias nor of weak instruments. The AGLS estimate in column (3) with all means of exogenous variables as instruments, is now very large in magnitude and significant. When excluding instruments which did not pass the validation test, see table 6 again, the AGLS estimate of the education coefficient is however similar to the random effects estimate, but insignificant. In the first stage estimation, the effect of the 1975 school reform on education is small and insignificant, and the 1958 reform increases length of education now only by a quarter of a year, which is significant at a 10 percent level. The 2SMCL estimate of the education coefficient is -0.188 and significant. The coefficient on the education residual is positive, but now, as opposed to the case for men, highly significant. Moving to column (6), taking account of heterogeneity, the education residual and the education effect increases further in magnitude, both still being significant. In column (7) we only use school reforms as instruments, and although the residual and education effects increase further in magnitude, they become insignificant.

### 6.2.3 Alternative Health Measures

Because health has many dimensions, more robust results are obtained by using several health measures. We therefore repeat estimations from the last section using the BMI, HBP and smoking variables mentioned earlier. One might argue that these variables reflects "risk factors" that are more closely related to health inputs rather than health outcomes, and for this reason we also estimate a model using SRH as outcome, including these health three other variables as explanatory variables. This can be interpreted as an estimate of a production function of health, and can be used to assess the size of the education gradient given healh inputs, thus to distinguish between productive and allocative efficiency. We do not at this stage account for endogeneity of health inputs[21]. When the dependent variable is an input to health production, education effects reflect allocative efficiency, but may be biased for the exact same reasons as they are in estimations with health outcomes.

In table 7 means and standard deviations of these other health measure are presented, as well as their correlation with education and SRH. We have included indicators of being fat and obese, defined as BMI above 25 and 30 respectively, as these are commonly applied health measures in the medical literature. Although there is no general agreement on optimal BMI for

[21]See Gilleskie and Harrison (1998) for a thorough investigation of this type.

good health, obesity in particular is seen as a large health threat, which besides implying possible physical restrictions, stigmas and psychological pains, increases the risk of e.g. cardiovascular diseases and diabetes, see e.g. the report edited by Thomas (1995). From table 7, we see that men have larger BMI; the mean of BMI is higher and the shares of fat and obese men are higher for men than for women. In our sample, 44 percent of the men, whereas only 19 percent of the women, are classified as being fat. Moreover, men smoke more and around seven percent of both men and women have been diagnosed having high blood pressure. As seen in the third column of the table, all, except high blood pressure, these alternative health measures have quite high correlations with education. The fourth column shows that all the alternative health measures are correlated with SRH, including high blood pressure.

Results for BMI and the indicator of never been smoking (NS) are presented in table 8. The same estimations were conducted for HBP, but are left out, since only age showed up significant and education effects were small in size as well as insignificant. Three estimations are presented for each health measure. One with exogenous regressors and no individual effects, one with random effects and one with random effects and endogenous education (the 2SMLR estimator). Because most occupation dummies were insignificant, we conducted 2SMLR estimations with only the one occupation group, that deviated most from the others. Tests of validity of instruments are presented in table 9. We use linear regression model for BMI and a logit model for NS. The null hypothesis of no random effects is rejected in all models. Education is significant at a five percent level in all estimations, except for the 2SMLR estimation of womens BMI.  However, the null hypothesis of exogeneity of education is rejected for both BMI and NS for men but cannot be rejected for women, that is, the opposite to that found using SRH. Moreover, while education has the expected sign; more educated have less BMI and a higher share have never smoked, education residuals work in the opposite direction, as found for SRH.

Table 10 contains estimations of a health production function using simple ordered logit. From these results we note that for both men and women, the education coefficients remain significant, although reduced by 31 percent for men, and 16 percent for women when compared to the model without health inputs. All three risk factors have the expected sign, and have larger effects on men than women on SRH. Although the interpretation of this estimation as a production function estimate may be dubious, the results show that SRH is highly related to other common measures of (threats to) health conditions. Moreover, it also shows that SRH has a social gradient on top of these standard risk factors, and as such confirms our initial hypothesis that something else than healthy behavior explains the gradient. However, the result may arise because of reporting error in SRH, when this error is related to social status. A suggestive test for how this additional dimension of SRH relates to cognitive aspects influencing feelings of well being, was conducted by Grossman (1973). He included an indicator of job satisfaction, on the grounds that if (less) better educated on average are more (dis-) satisfied with their lives, they may tend to (under-) over report SRH. Even when life satisfaction is not related to health, we would therefore observe a education gradient in SRH. If life satisfaction is captured by job satisfaction, education gradients should vanish when this is included as control. As seen in column (2) and (4) job satisfaction also comes out highly significant in magnitude and in statistical terms, but more importantly, the social gradient in SRH does not diminish, supporting hypothesis that SRH captures conditions important to health on top of risk factors.

## 7 Discussion and Conclusion

It has been documented extensively that educational differences in health exist. Far less investigations have considered whether the observed relationship has a causal component from e.g. education to health. Can the education related differences in health we observe be interpreted as partly reflecting a return to education? The answer to this question is of tremendous importance to our understanding of how health is determined, and how it may be affected.

We have tried to investigate to what extent heterogeneity in health and endogeneity of education explained the education gradient in health, using SRH, BMI and indicators for high blood pressure (HBP) and never been smoking (NS). With one exception the results of our analysis do not invalidate hypotheses about direct educational effects on SRH, BMI and NS variables. Although e.g. Berger and Leigh (1989) did find an educational gradient in blood pressure, we did not. This might because our HBP variable is not very precise, indicating whether the individual has ever been diagnosed as having blood pressure. For the three other measures education gradients are present, even when accounting for occupational differences using the semiparametric MR estimator, which is consistent under far more general functional forms than standard parametric models. Moreover, with our particular data set and assuming only exogenous regressors, simple ordered probit and logit estimators of SRH did pretty well compared to this estimator. For simplicity we have focused on the logit parameters rather than marginal effects. To illustrate the magnitude of the estimated differences, we have calculated odds ratios of having excellent health using predicted probabilities at mean of other regressors than education. For the simple logit model (column (1) in table 5), comparing with those with only seven years of education, odds ratios of having excellent health is 1.5 and 2.1 for men and 1.55 and 2.25 for women with thirteen and eighteen years of education respectively. We also showed that we could not reject the presence of heterogeneity in health, accounted for by random individual effects in parametric models, in all our models for both men and women.

The evidence on endogeneity of education is more mixed, and we discuss this at some length in this section. Using SRH we reject that education is exogenous for women, but not for men. However, using BMI and NS we found the opposite. Education effects are hardly altered when taking account of endogeneity with the AGLS estimator, which however, in spite of asymptotic efficiency had a very low small sample precision in our application. When using the 2SCML or 2SMLR estimators, education effects are significant. We tried to capture exogenous variation in education by means of school reforms as instruments for education. We showed that education did jump to another level in the years following the 1958 reform, implying that those men affected by this reform have almost half a year longer education, while women have a third of a year more, than those not affected by the reform, on top of a common age trend. Moreover, no clear indication on small sample bias is found, although some indication of weak instruments when using only school reforms was present. Taking the 2SMLR estimators at face value, for all health measures where education is endogenous, the coefficient on education residuals have the opposite sign as that on education. Since the coefficient on the education residual estimates the regression coefficient of unobserved health components on unobserved education components, the latter result is inconsistent with an hypothesis that education and health are determined by a common unobserved factor like time preferences. Moreover, the result would indicate that individuals with higher school ability have lower health endowments, or put in another way, that if high school ability individuals did not receive high education, they would have had worse health. Indeed, although logit and 2SMLR estimates are not directly comparable (because they are derived in a marginal and a conditional distribution) there is a tendency for education effects to magnify when accounting for endogeneity. In fact, using the 2SCML estimates to calculate odds ratios of excellent health, the large simple logit differences increase to 3.5 and 9.9 for men and 7.4 and 38.8 for women. In terms of a local average treatment effects interpretation, see section 4, it means that those high ability children who received more education due to the school reforms, gained substantially in health from the increased education. This interpretation, although worth noting, is perhaps too strong. First of all, we only follow individuals over five years, which is perhaps too low a time span, if the aim is to capture individual persistence in health possibly dating back to childhood. Second, the large coefficients on education and education residuals may stem from a multicollinearity problem, especially when few instruments are used. More knowledge on the behavior of the 2SCML estimator in small samples is needed to judge about this. For comparison we estimate simple linear probability models for SRH. An advantage of linear probability models is that individual effects can be differenced out, 2SLS techniques can be used, which more applied researchers are familiar with and finally, the conditional maximum likelihood estimator, which we call the control function estimator in the linear version, is algebraically identical to 2SLS.  Results are presented in table 11. Comparing the OLS and the 2SLS estimates, we see that instrumenting education, education effects increases, in particular for women. For women, education effects are in fact significant at a ten percent level when only using school reforms as instruments, although this is far from the case for men. Adding fixed effects to the 2SLS estimation, using a Hausman-Taylor estimator, increases education effects further. This is also the case for the control function estimates in the last column. These results support the findings from the 2SCML and 2SCMLR estimates, that education effects are substantial and larger when accounting for endogeneity of education and heterogeneity in health.

Another issue is that because the instruments we generate from the school reforms, essentially compare individuals of very different age, numerous sources may cause health to differ between the comparison groups, even when accounting for trends in age. Therefore we conduct 2SMLR estimations for SRH in the sample of individuals aged 47 to 54 in 1995, that is, for individuals entering 7'th grade in one of the four years just before the 1958 reform and individuals who entered in the four years just after. For this group of individuals there are 854 men and 890 women. This does in fact change the results, since the effect of the education residual become significant at a 10 percent level for men, whereas it becomes insignificant for women. For both however, the education coefficient is substantial and significant, for men though at a ten percent level.

We make a further note on the intepretation of education effects. Since we were not able to control for wages or income in the panel analysis, education effects may reflect wage or income effects. We argue that this is likely to be part of, but not the entire story. First of all, we did control for occupational groups and having subordinate workers, which to some extent capture wage differences. Second, existing evidence suggest that whereas education effects are quite strong at most education levels, income effects are not as strongly graded, but are confined to a higher extent among low income levels, showing evidence of education effects separate of wage or income effects. We confirmed this in another study, using the same data, Arendt (2001b), where we used wage information in the data set used here, for the 1995 period only, and on US data in Arendt (2001a).

Finally we showed that education is related to SRH when controlling for the three other health measures, which can be interpreted as inputs in health production. As such it confirms Grossman's efficiency hypothesis. It also supports hypotheses of other non-behavioral hypothesis of social gradients in health, e.g. relying on psychosocial factors as described for instance by Adler et al. (1994).

Although, as summarized above, there are a number of issues regarding both methods and data that can be improved, we have added to the existing literature, applying identification techniques not previously used in this literature and showed that an overall conclusion, which is supportive of a direct effect, through both allocation (health behavior) and efficiency, of education, is fairly robust.

# 8 Appendix

8.1 Definition of Variables and Sample Selection

The WECS was collected by the Danish National Institute of Occupational Health (AMI) and the National Institute of Social Research (SFI) as a representative survey of Danish citizens aged 18-59 in 1990 with a follow-up survey in 1995, added with persons to make it representative as well. The sampling method and representativeness of the data is described in detail in Nord-Larsen et al. (1992) and Borg and Burr (1997). The total sample of individuals selected for interviews in 1990 and 1995 consists of 11084 individuals, each with an individual identifier. The response rate was 89.7 percent in 1990 and 80.2 percent in 1995.

We create our sample in the following way. We drop 911 observations with missing education information in both 1990 and 1995. Then we drop 702 of the remaining individuals who are under education in 1990, since we are interested in the effect of completed education. 38 are excluded because they have missing, no or untold education in 1995, in addition to missing or untold schooling in 1995 and untold education in 1990. 2946 observations are deleted because individuals were not interviewed in both years. For 10 individuals, their age (defined as year minus year of birth) is reported as more than 6 or less than 4 years in 1995 from the reporting in 1990, i.e. erroneously, so we delete these. Among the now 6477 remaining observations, we only keep the 3755 with non-missing SHR in both 1990 and 1995, consisting of 2023 men and 1732 women who are observed in both 1990 and 1995. The large numbers of missing observations are present because this question were only asked to people who were employed within the last two months of the interview. Among the 3755, 3673 are in work in 1990 and 3675 in 1995.

The education variables differ in 1990 and 1995, because of a finer distinction between vocational educations in 1995, and because short advanced degree is not referred to as advanced (videregaaende) in 1990. We therefore chose to use the 1995 classification in 1990 as well to get a consistent time-invariant measure of education. When both education variables are missing, we use the schooling variable in 1995. Years of education are defined as follows: 7 years: 7th Grade, 9 years: 9th Grade, 10 years: 10th Grade, 11 years: one year EFG or semi-skilled (specialarbejdere), 12 years: Gymnasium or Other Short Educ, 13 years: Apprenticeship/EFG, 14 years: Short Advanced, 16 years: Medium Advanced, 18 years: Long Advanced.

Occupational categories also differ between 1990 and 1995. In 1995 there is only one group of white collar workers (funktionære), but in 1990, there are two (funktionær med mindst et aa rs uddannelse og en underordnet, samt underordnede funktionære), which therefore are collapsed. Blue collar and unskilled workers (faglærte, ufaglærte) are defined in both years, and used. Finally we define a dummies for people in other types of work (arbejde iøvrigt) and a rest category.

In Borg and Burr (1997), it is documented that the sample of interviewed employed persons is representative to reasonable degree with respect to distribution of age, education and occupation. Comparing our sample with the original sample, see Borg and Burr (1997) table 2.5-2.7, people with advanced education and white collar workers are slightly overrepresented, whereas younger and those with only primary school are underrepresented in our sample. One reason could be that we exclude those under education, who are included in the tables in Borg and Burr (1997) if they have been employed within two months. Overall, we think the sample is reasonable representative of people who have finished their education and employed within two months.

## 8.2 The AGLS Estimator

Neweys version of the AGLS estimator is based on the conditional distribution of ε given V (as opposed to Amemiyas estimator (1978) which was obtained in the marginal distribution of ε) in (3.2), using the reduced form of the H*-equation:

$$H_{it}^{*} = X_{1it}\alpha_1 + X_{2it}\alpha_2 + \lambda V_i + u_{it}$$

where $\lambda=\eta+\gamma$, and the αs are related to the structural parameters Π, β and γ by:

$$\alpha_1 = \beta + \gamma\Pi_1,\ \alpha_1 = \gamma\Pi_2$$

where Π is split into $\Pi_1$ and $\Pi_2$ according to the variables $X_1$ and $X_2$. From the latter equations, β and γ can be found by minimum distance, using estimated αs and Πs.

## 8.3 Cavanagh and Shermans Semiparametric Estimator

The semiparametric estimator suggested by Cavanagh and Sherman (1998) is used as an alternative estimator. The estimator is applicable to models of the form:

$$y = D(F(X\beta, \varepsilon))$$

where ε is a random error term, D is a non constant and monotone function and F is a strictly monotonic function. This specification is very general and includes e.g. linear regression, censored regression, transformation, duration and ordered quantal response models, see Han (1987). Cavanagh and Sherman suggest to estimate β by the monotone rank (MR) estimator defined by:

$$\hat{\beta} = \arg\max_{\beta} \sum_i M(y_i) R(X_i\beta)$$

where M is an increasing function and R is the rank function, i.e. the number of observations with index less than or equal to the index of the i'th observation. It is assumed that E(M(y)|X) depends only on X through Xβ and that it is increasing in Xβ. Cavanagh and Sherman prove that the estimator is root-n consistent when M is deterministic or equal to R. We apply the version of the estimator with M(y)=y. The estimator is developed for exogenous regressors and we are therefore relegated to study the biases of functional form misspecification separately from that of endogeneity and heterogeneity biases. The semiparametric MR is asymptotic normal and consistent under suitable regularity conditions far more general than those needed for the MLE.

When checking the assumption that E(y|Xβ) is monotonic, we use the local linear smoother, see below, with a qubic weight function, and a bandwidth, which we weight with density estimates to get a variable bandwidth. The density is estimated using Silvermans rule-of-thumb bandwidth: 0.9*min(stdev(x), interquartile range(x)/1.34). The confidence bands are calculated, ignoring the bias, using expressions of the asymptotic variance, see below.

8.4 The local linear smoother

The local linear smoother estimate of the conditional mean of y given x is given as pointwise weighted least square estimations:

$$E(y_i|x) = a_x + b_x x, \text{ where:}$$
$$(a_x, b_x) = \arg\min_{(\alpha,\beta)} \sum_{i=1}^{n} (y_i - (\alpha + \beta x))^2 K\left(\frac{x - x_i}{h}\right) w_i$$

where K is a kernel function (typically a distribution function), h is a bandwidth (a window that determines over what range of x's the smoothing is done) and w is a population weight[22]. If the nearest neighbour method is used instead of a fixed bandwidth, the Lowess estimator by Cleveland (1979) is obtained. According to Fan (1992) the estimator is asymptotic normal, with asymptotic variance given by:

$$V = \frac{\sigma^2(x)}{f(x)nh} \int K^2(u)du$$

where $\sigma^2(x)$=V(y|x) is the conditional variance of y given x, f(x) the density of x and n is the sample size. $\sigma^2(x)$ can be estimated by nonparametric regression methods as well. We use the Kernel estimator to estimate both $\sigma^2(x)$ and f(x). The kernel regression and density estimators are given as:

$$\hat{E}(y|x) = \sum_{i=1}^{n} \frac{K\left(\frac{x_i - x}{h}\right) y_i}{\hat{f}(x)}, \hat{f}(x) = \frac{1}{nh} \sum_{i=1}^{n} K\left(\frac{x_i - x}{h}\right)$$

[22]The local linear smoother estimator has been shown to possess good properties compared to other non-parametric smoothing methods. Compared to the Nadaraya-Watson kernel estimator, the asymptotic bias is smaller, the asymptotic variance the same, it is not more biased near the boundary points of the support of regressors and the small sample behavior is better, see Fan (1992).

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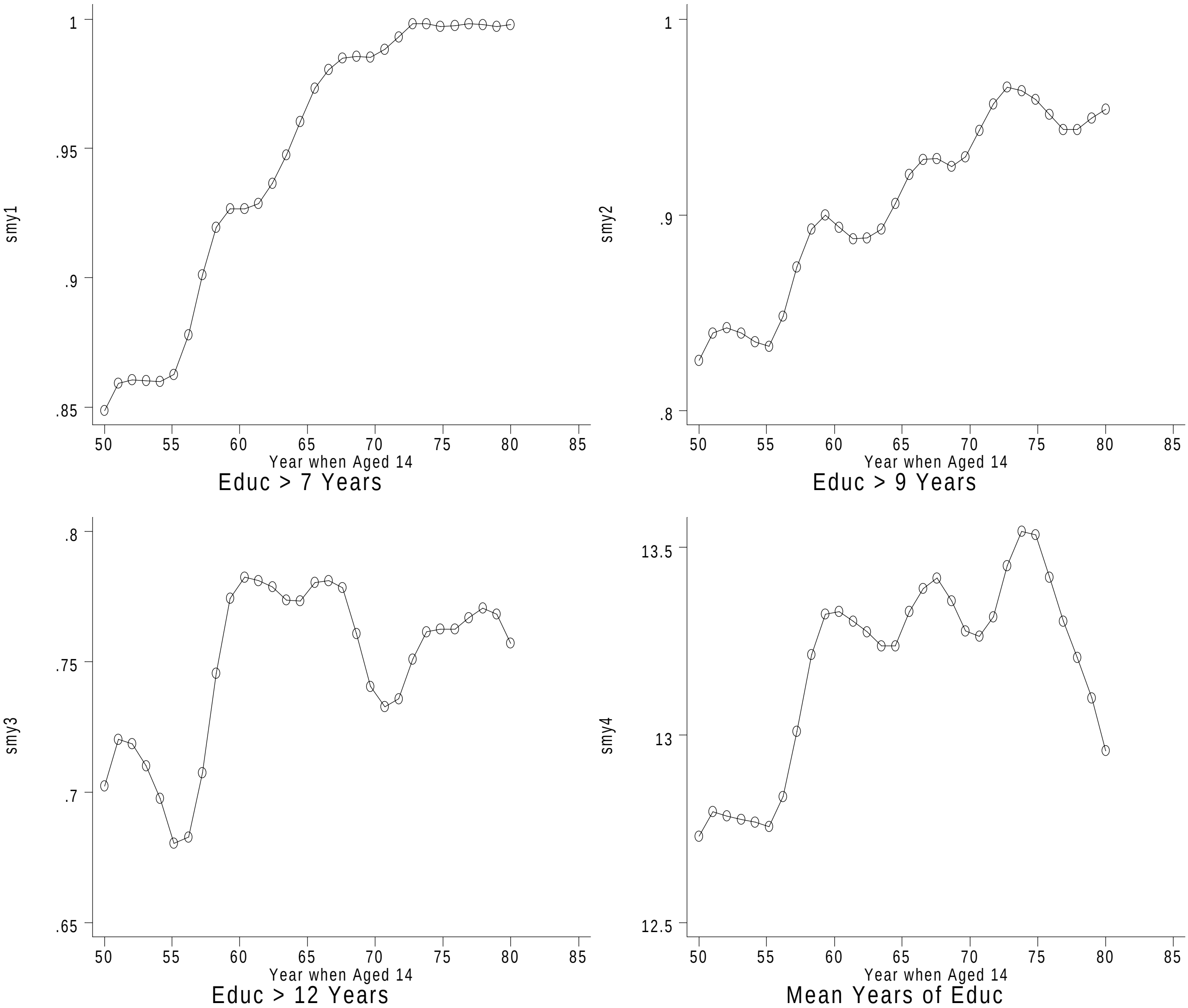
# 10 Figures and Tables

10.1 Figures

**Figure 1.** Distribution of Educational Attainment given by year when Individuals are 14 of age.



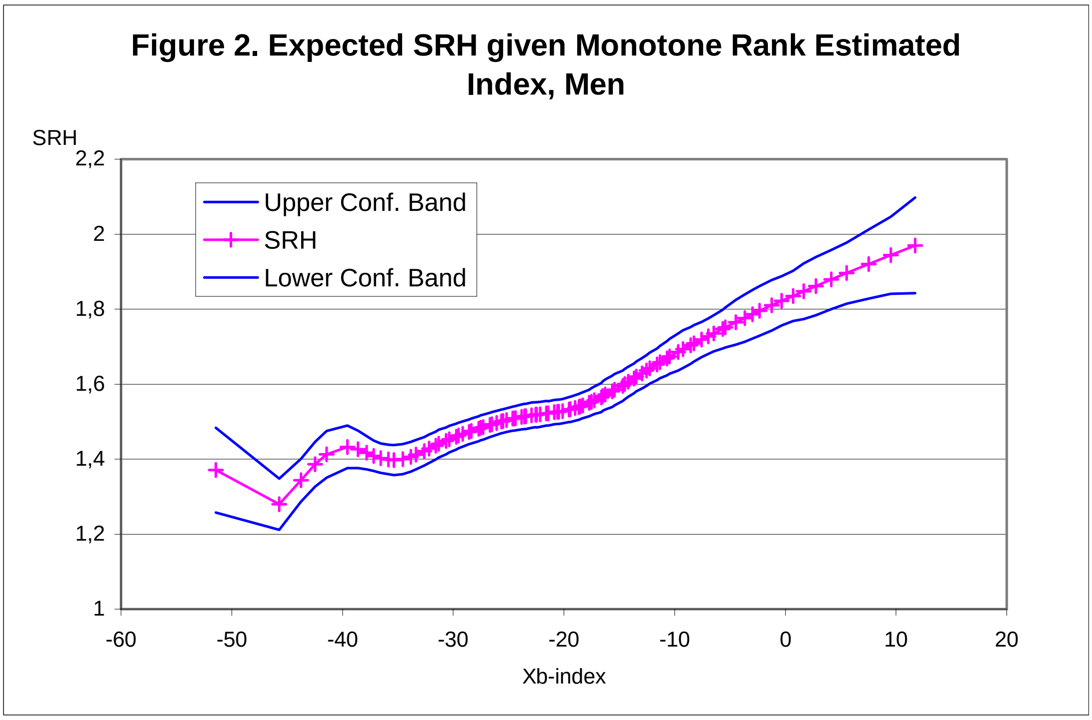


Figure 2. Expected SRH given Monotone Rank Estimated Index, Men
SRH
2,2
2
1,8
1,6
1,4
1,2
1
-60
-50
-40
-30
-20
-10
0
10
20
Xb-index
Upper Conf. Band
SRH
Lower Conf. Band

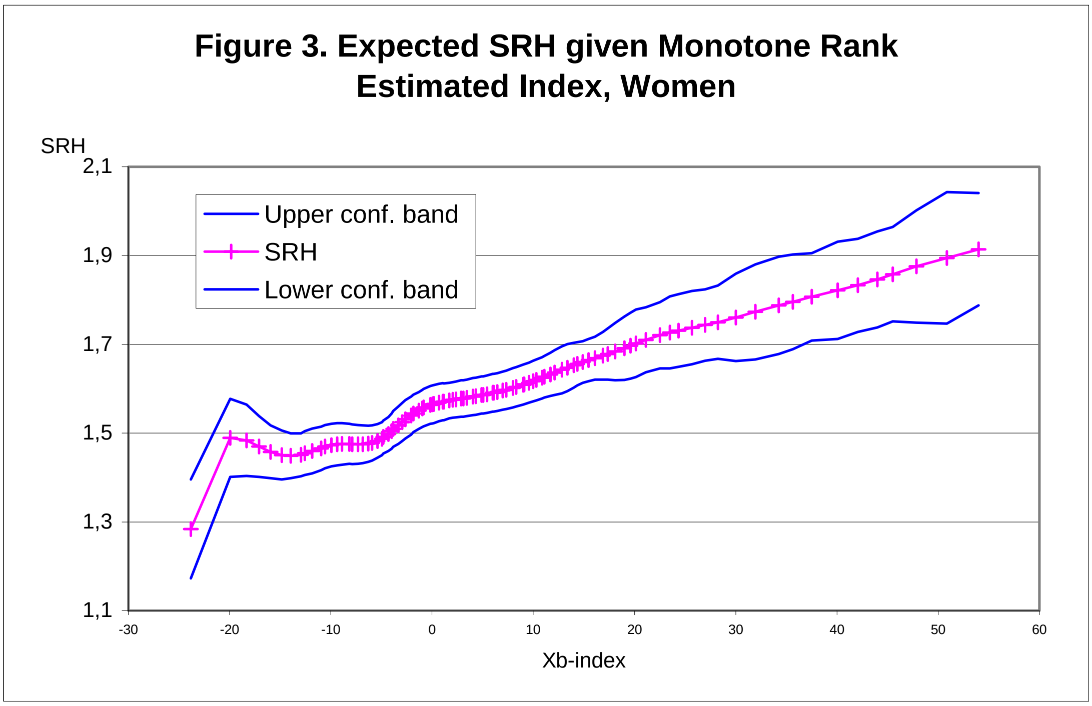


Figure 3. Expected SRH given Monotone Rank Estimated Index, Women
SRH
2,1
1,9
1,7
1,5
1,3
1,1
-30
-20
-10
0
10
20
30
40
50
60
Xb-index
Upper conf. band
SRH
Lower conf. band

10.2 Tables

**TABLE 1. SUMMARY STATISTICS**

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| Variable | Share | Mean SHR | Share PH | Share | Mean SHR | Share PH |
| Years of Education | | | | | | |
| 7 | 0,040 | 1,794 | 0,181 | 0,057 | 1,903 | 0,225 |
| 9 | 0,041 | 1,643 | 0,113 | 0,046 | 1,838 | 0,225 |
| 10 | 0,032 | 1,585 | 0,108 | 0,047 | 1,593 | 0,105 |
| 11 | 0,056 | 1,832 | 0,156 | 0,053 | 1,777 | 0,177 |
| 12 | 0,046 | 1,521 | 0,099 | 0,106 | 1,623 | 0,135 |
| 13 | 0,495 | 1,600 | 0,112 | 0,323 | 1,549 | 0,093 |
| 14 | 0,070 | 1,521 | 0,081 | 0,146 | 1,640 | 0,119 |
| 16 | 0,122 | 1,537 | 0,100 | 0,183 | 1,541 | 0,088 |
| 18 | 0,098 | 1,471 | 0,050 | 0,039 | 1,508 | 0,090 |
| Occupation | | | | | | |
| White Collar | 0,590 | 1,535 | 0,090 | 0,790 | 1,572 | 0,098 |
| Blue Collar | 0,180 | 1,640 | 0,128 | 0,027 | 1,777 | 0,160 |
| Unskilled | 0,204 | 1,693 | 0,138 | 0,154 | 1,774 | 0,190 |
| Other Work | 0,014 | 1,518 | 0,052 | 0,016 | 1,571 | 0,107 |
| No Work | 0,010 | 1,942 | 0,262 | 0,012 | 1,932 | 0,318 |
| Age | | | | | | |
| 18-24 | 0,067 | 1,407 | 0,041 | 0,048 | 1,464 | 0,054 |
| 25-34 | 0,293 | 1,498 | 0,075 | 0,249 | 1,521 | 0,084 |
| 35-44 | 0,313 | 1,600 | 0,108 | 0,351 | 1,600 | 0,116 |
| 45-54 | 0,246 | 1,687 | 0,157 | 0,280 | 1,693 | 0,140 |
| 55-65 | 0,082 | 1,767 | 0,148 | 0,072 | 1,843 | 0,197 |
| Total | | | | | | |
| Number of Subordinates | | | | | | |
| 0 | 0,643 | 1,644 | 0,123 | 0,775 | 1,636 | 0,123 |
| 1-10 | 0,230 | 1,505 | 0,090 | 0,175 | 1,550 | 0,102 |
| 11-50 | 0,100 | 1,523 | 0,087 | 0,043 | 1,553 | 0,087 |
| 50+ | 0,028 | 1,351 | 0,026 | 0,007 | 1,380 | 0,041 |
| 1+ | 0,357 | 1,500 | 0,084 | 0,225 | 1,540 | 0,098 |
| All | No. Obs | Mean SH | Share PH | No. Obs | Mean SH | Share PH |
| | 4046 | 1,5922 | 0,1092 | 3464 | 1,6159 | 0,1177 |

*Notes*: Calculated for pooled data. "Share" is the share of observations in the given group. Mean SHR is mean self-reported health status, with value 1 corresponding to very excellent health and 5 to very poor. PH is poor health, which corresponds to the three worst outcomes.

## TABLE 2. SIMPLE LOGITS, CONTROLLING LINEARITY OF EDUCATION AND AGE.

| | Men | | Women | |
|---|---|---|---|---|
| Variable | Estimate | Std.Err | Estimate | Std.Err |
| 25<age<30 | -0,19 | 0,12 | -0,23 | 0,15 |
| 29<age<40 | 0,07 | 0,08 | -0,17 | 0,09 |
| 39<age<50 | 0,24 | 0,09 | 0,09 | 0,09 |
| 49<age<60 | 0,56 | 0,10 | 0,44 | 0,11 |
| age>59 | 0,46 | 0,28 | 0,61 | 0,40 |
| 9 Years | -0,13 | 0,21 | 0,02 | 0,21 |
| 10 Years | -0,29 | 0,23 | -0,57 | 0,21 |
| 11 Years | 0,22 | 0,20 | -0,18 | 0,20 |
| 12 Years | -0,60 | 0,21 | -0,54 | 0,17 |
| 13 Years | -0,36 | 0,16 | -0,74 | 0,15 |
| 14 Years | -0,56 | 0,19 | -0,46 | 0,16 |
| 16 Years | -0,59 | 0,18 | -0,73 | 0,16 |
| 18 Years | -0,75 | 0,18 | -0,85 | 0,22 |
| LR chi2 (9) | 99,25 | | 105,52 | |
| Log-Likelihood | -3926 | | -3441 | |
| N | 4046 | | 3464 | |

*Notes*: Results are from ordered logit estimations of SRH.

## TABLE 3. TRANSITION PROBABILITIES OF CHANGES IN SRH, 1990-95

**Men**

| 1990\1995 | | 1 | 2 | 3 | 4 | 5 | Row Total | Marginal |
|---|---|---|---|---|---|---|---|---|
| | 1 | 0,610 | 0,322 | 0,056 | 0,012 | 0,000 | 947 | 0,547 |
| | 2 | 0,396 | 0,469 | 0,105 | 0,023 | 0,007 | 608 | 0,351 |
| | 3 | 0,163 | 0,361 | 0,440 | 0,030 | 0,000 | 166 | 0,096 |
| | 4 | 0,125 | 0,375 | 0,375 | 0,125 | 0,000 | 8 | 0,005 |
| | 5 | 0,000 | 0,250 | 0,750 | 0,000 | 0,000 | 4 | 0,002 |
| Column Total | | 847 | 654 | 196 | 31 | 4 | 1732 | |
| Marginal | | 0,489 | 0,378 | 0,113 | 0,018 | 0,002 | | |

**Women**

| 1990\1995 | | 1 | 2 | 3 | 4 | 5 | Row Total | Marginal |
|---|---|---|---|---|---|---|---|---|
| | 1 | 0,641 | 0,299 | 0,053 | 0,007 | 0,000 | 1115 | 0,552 |
| | 2 | 0,381 | 0,487 | 0,123 | 0,007 | 0,001 | 713 | 0,353 |
| | 3 | 0,178 | 0,406 | 0,378 | 0,033 | 0,006 | 180 | 0,089 |
| | 4 | 0,000 | 0,333 | 0,400 | 0,200 | 0,067 | 15 | 0,007 |
| | 5 | 0,000 | 0,000 | 0,000 | 1,000 | 0,000 | 1 | 0,000 |
| ColumnTotal | | 1019 | 758 | 221 | 23 | 3 | 2021 | |
| Marginal | | 0,504 | 0,375 | 0,109 | 0,011 | 0,001 | | |

*Notes*: Transition Probabilities between SRH states. 1 is excellent, 5 is very poor.

## TABLE 4. COMPARISON OF PROBIT, LOGIT AND MONOTONE RANK ESTIMATES

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Education | -1,269 | -1,290 | -1,638 | -1,789 | -1,803 | -1,631 |
| | (0,464) | (0,472) | (0,489) | (0,667) | (0,666) | (0,732) |
| White Coll | -34,704 | -34,811 | -38,309 | -30,087 | -31,036 | -22,686 |
| | (6,812) | (7,081) | (14,564) | (8,513) | (8,307) | (7,742) |
| Blue Coll | -25,950 | -25,964 | -29,180 | -9,16 | -8,935 | 10,326 |
| | (6,774) | (7,040) | (14,677) | (10,919) | (10,499) | (10,337) |
| Unskilled | -25,253 | -24,873 | -30,097 | -16,807 | -17,740 | -13,806 |
| | (6,756) | (7,004) | (14,712) | (8,512) | (8,232) | (8,236) |
| Other Work | -36,517 | -35,203 | -36,399 | -27,927 | -28,055 | -10,845 |
| | (11,382) | (11,441) | (17,682) | (13,213) | (12,771) | (10,852) |
| Subord.>0 | -9,621 | -9,842 | -9,878 | -4,099 | -4,453 | -3,990 |
| | (2,269) | (2,336) | (2,384) | (3,256) | (3,275) | (4,171) |
| Year | 1,261 | 1,192 | 0,969 | 5,095 | 4,613 | 4,940 |
| | (1,979) | (2,025) | (1,999) | (3,002) | (2,935) | (3,188) |

*Notes*: All coefficients are normalized by the age coefficient. (1) and (4) are Probit, (2) and (5) are Logit and (3) and (6) are Monotone Rank Estimations. Standard errors in parentheses.

## TABLE 5a. ORDERED LOGITS OF SRH, MEN

| Estimator | Logit (1) | RE-Logit (2) | AGLS (3) | AGLS (4) | 2SCML (5) | 2SMLR (6) | 2SMLR (7) |
|---|---|---|---|---|---|---|---|
| Education | -0,041 | -0,065 | -0,078 | -0,046 | -0,113 | -0,175 | 0,032 |
| | (0,014) | (0,022) | (0,062) | (0,095) | (0,061) | (0,075) | (0.320) |
| Age | 0,032 | 0,044 | 0,187 | 0,085 | 0,032 | 0,043 | 0,044 |
| | (0,003) | (0,005) | (0,124) | (,109) | (0,003) | (0,005) | (0.005) |
| White Coll | -1,100 | -1,177 | -1,121 | -1,117 | -0,983 | -0,959 | -1,351 |
| | (0,240) | (0,306) | (0,245) | (0,269) | (0,262) | (0,339) | (0.649) |
| Blue Coll | -0,826 | -0,858 | -0,958 | -0,779 | -0,771 | -0,741 | -0,934 |
| | (0,244) | (0,313) | (0,260) | (0,290) | (0,248) | (0,323) | (0.402) |
| Unskilled | -0,790 | -0,806 | -0,623 | -0,719 | -0,885 | -0,878 | -0,684 |
| | (0,242) | (0,308) | (0,250) | (0,245) | (0,253) | (0,312) | (0.504) |
| Other Work | -1,119 | -1,399 | -1,124 | -1,219 | -1,047 | -1,269 | -1.508 |
| | (0,349) | (0,452) | (0,345) | (0,373) | (0,355) | (0,461) | (0.576) |
| Subord.>0 | -0,311 | -0,361 | -0,298 | -0,343 | -0,290 | -0,320 | -0,393 |
| | (0,070) | (0,097) | (0,073) | (0,078) | (0,072) | (0,101) | (0.143) |
| Educ. Residual | | | | | 0,076 | 0,121 | -0,098 |
| | | | | | (0,063) | (0,079) | (0.321) |
| Tests for RE: | | 219,3 | | | | 243,8 | 242,5 |
| First Stage Estimation | | | | | | | |
| F-value, HT-Instruments | | | 38,66 | 52,58 | 52,58 | 52,58 | |
| F-value, Reform-Instruments | | | 9,81 | 10,96 | 10,96 | 10,96 | 9,900 |
| Partial R-squared, All instruments | | | 0,059 | 0,054 | 0,054 | 0,054 | 0,005 |
| Number of IV | | | 8 | 6 | 6 | 6 | 2 |
| Reform 1958 | | | 0,480 | 0,502 | 0,502 | 0,502 | 0,451 |
| | | | (0,138) | (0,138) | (0,138) | (0,138) | (0,148) |
| Reform 1975 | | | 0,402 | 0,411 | 0,411 | 0,411 | 0,044 |
| | | | (0,232) | (0,233) | (0,233) | (0,233) | (0,241) |

*Notes*: All estimations are from ordered logits. In (1) all regressors are exogenous. In (2) random effects are added. (3) and (4) are estimated by AGLS, instrumenting education with means of all exogenous variables and School Reforms in (3) and including only those which pass the test of validity (see table 6) in (4). (5) is 2SCML with the same instruments as in (4) and no random effects. (6) is equal to (5) with random effects. (7) is with shcool reforms and age only in 1.stage. Distributions of random effects are estimated freely as a discrete three point distribution. LR tests of RE in row below estimates; -2 times log Likelihood Ratio between model with and without RE is presented, which is Chi2(5)-distributed. Standard Errors in Parenthesis. AGLS standard errors are Newey-Corrected.

## TABLE 5b. ORDERED LOGITS OF SRH, WOMEN

| Estimator | Logit | RE-Logit | AGLS | AGLS | 2SCML | 2SMLR | 2SMLR |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Education | -0,0463 | -0,0632 | -0,430 | -0,062 | -0,188 | -0,227 | -0,362 |
| | (0,015) | (0,023) | (0,141) | (0,057) | (0,054) | (0,069) | (0.365) |
| Age | 0,0256 | 0,0359 | 0,805 | 0,112 | 0,023 | 0,032 | 0,027 |
| | (0,003) | (0,005) | (0,254) | (0,078) | (0,004) | (0,006) | (0.011) |
| White Coll | -0,794 | -1,039 | -0,692 | -0,808 | -0,588 | -0,883 | -0,578 |
| | (0,213) | (0,284) | (0,221) | (0,221) | (0,228) | (0,290) | (0.631) |
| Blue Coll | -0,2283 | -0,528 | -0,767 | -0,324 | -0,093 | -0,397 | -0,291 |
| | (0,284) | (0,373) | (0,330) | (0,292) | (0,287) | (0,376) | (0.473) |
| Unskilled | -0,4543 | -0,684 | -0,252 | -0,424 | -0,670 | -0,948 | -1,109 |
| | (0,224) | (0,297) | (0,230) | (0,229) | (0,236) | (0,314) | (0.600) |
| Other Work | -0,7180 | -0,856 | -0,477 | -0,702 | -0,613 | -0,808 | -0,644 |
| | (0,334) | (0,432) | (0,359) | (0,342) | (0,336) | (0,434) | (0.503) |
| Subord.>0 | -0,1146 | -0,156 | -0,074 | 0,132 | 0,032 | -0,018 | 0,145 |
| | (0,083) | (0,115) | (0,087) | (0,281) | (0,099) | (0,127) | (0.384) |
| Educ. Residual | | | | | 0,154 | 0,186 | 0,302 |
| | | | | | (0,057) | (0,073) | (0,368) |
| Tests for RE: | | 190,1 | | | | 189,2 | 189,7 |
| First Stage Estimation | | | | | | | |
| F-value, HT-Instruments | | | 51,18 | 66,27 | 66,27 | 66,27 | |
| F-value, Reform-Instruments | | | 4,29 | 3,78 | 3,78 | 3,78 | 7,070 |
| Partial R-squared, All instruments | | | 0,085 | 0,075 | 0,075 | 0,075 | 0,004 |
| Number of IV | | | 8 | 6 | 6 | 6 | 2 |
| Reform 1958 | | | 0,267 | 0,259 | 0,259 | 0,259 | 0,611 |
| | | | (0,140) | (0,141) | (0,141) | (0,141) | (0,153) |
| Reform 1975 | | | 0,117 | 0,125 | 0,125 | 0,125 | 0,231 |
| | | | (0,243) | (0,244) | (0,244) | (0,244) | (0,258) |

*Notes*: See notes to table 5a.

## TABLE 6. t-TESTS OF VALIDITY OF INSTRUMENTS

| Variable | Men | Women |
|---|---|---|
| Reform 1958 | -0,907 | -0,353 |
| Reform 1975 | -1,573 | 0,461 |
| | | |
| Means of: | | |
| Age | 0,861 | 0,415 |
| White Coll | -3,617 | -1,066 |
| Blue Coll | 1,774 | 2,702 |
| Unskilled | 0,934 | 2,311 |
| Other Work | 1,239 | -0,936 |
| Subord.>0 | -3,118 | 0,435 |
| | | |
| Chi2(6), All Means | 28,64 | 13,55 |
| P-value | 0,000 | 0,0351 |

*Notes*: T-values corresponding to each instrument included in separate second stage 2SCML estimation of SRH, conditional on other instruments not previously rejected.

## TABLE 7. SUMMARY STATISTICS OF OTHER HEALTH MEASURES

| **Men** Variable | Mean | Std. | Correlation with Educ. | Correlation with SRH. |
|---|---|---|---|---|
| BMI | 24,849 | 3,059 | -0,167 | 0,114 |
| Fat | 0,422 | 0,494 | -0,138 | 0,089 |
| Obese | 0,064 | 0,245 | -0,115 | 0,093 |
| High BP | 0,065 | 0,025 | 0,018 | 0,111 |
| Never Smoked | 0,325 | 0,468 | 0,102 | -0,104 |
| Years Smoked | 12,99 | 12,506 | -0,089 | 0,152 |

| **Women** Variable | Mean | Std. | Correlation with Educ. | Correlation with SRH. |
|---|---|---|---|---|
| BMI | 22,706 | 3,335 | -0,166 | 0,112 |
| Fat | 0,196 | 0,397 | -0,126 | 0,069 |
| Obese | 0,044 | 0,205 | -0,097 | 0,069 |
| High BP | 0,070 | 0,255 | -0,025 | 0,075 |
| Never Smoked | 0,403 | 0,491 | 0,098 | -0,021 |
| Years Smoked | 10,21 | 11,339 | -0,014 | 0,076 |

*Notes*: BMI is defined as weight in kg. divided by height in meters squared. Fat is defined as BMI > 25 and Obese is BMI > 30. High BP indicates self-reports that a doctor has diagnozed high blood pressure.

## TABLE 8a. EDUCATION GRADIENTS IN BMI AND NEVER SMOKED, MEN

| | Body Mass Index | | | Never Smoked | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Education | -0,207 | -0,232 | -0,355 | 0,077 | 0,721 | 1,444 |
| | (0,022) | (0,025) | (0,059) | (0,017) | (0,105) | (0,364) |
| Age | 0,056 | 0,055 | 0,058 | -0,047 | -0,237 | -0,231 |
| | (0,005) | (0,006) | (0,006) | (0,004) | (0,205) | (0,028) |
| White Coll | 0,178 | 0,095 | | 0,575 | 1,011 | 0,130 |
| | (0,363) | (0,208) | | (0,279) | (1,065) | (0,524) |
| Blue Coll | 0,266 | 0,075 | | 0,502 | 0,601 | |
| | (0,370) | (0,214) | | (0,283) | (1,043) | |
| Unskilled | 0,427 | -0,027 | -0,225 | 0,049 | -0,387 | |
| | (0,368) | (0,207) | (0,113) | (0,284) | (1,003) | |
| Other Work | 0,032 | -0,027 | | 0,224 | -0,618 | |
| | (0,523) | (0,282) | | (0,392) | (1,237) | |
| Subord.>0 | 0,037 | -0,010 | | -0,006 | -0,211 | |
| | (0,104) | (0,070) | | (0,076) | (0,296) | |
| Educ. Residual | | | 0,155 | | | -1,012 |
| | | | (0,049) | | | (0,417) |
| Tests for RE: | | 1403,8 | 1407,4 | | 1620 | 1688 |
| First Stage Estimation | | | | | | |
| F-value, HT-Instruments | | | 96,25 | | | 60,14 |
| F-value, Reform-Instruments | | | 10,84 | | | 8,88 |
| Partial R-squared, All instruments | | | 0,05 | | | 0,033 |
| Number of IV | | | 4 | | | 4 |
| Reform 1958 | | | 0,530* | | | 0,457* |
| Reform 1975 | | | 0,382 | | | 0,380 |

*Notes*: BMI equations are estimated using linear regression. Indicator for Never Smoked is estimated with logit models. (1) and (4) are with exogenous regressors and no individual effects. (2)and (5) are with random effects and in (3) and (6), we leave out insignificant variables and only use instruments passing the test of validation, see table 9, using the 2SMLR estimator, see text. Distributions of random effects are estimated as discrete with three points of support. Presence of random effects is tested using a Breusch-Pagan LM test for BMI and -2log likelihood ratio test for Never Smoked. Both are asymptotic Chi2(5)-distributed. A * indicates that school reforms are significant at 5 percent.

## TABLE 8B. EDUCATION GRADIENTS IN BMI AND NEVER SMOKED, WOMEN

| | Body Mass Index | | | Never Smoked | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Education | -0,173 | -0,188 | -0,224 | 0,057 | 0,085 | 0,803 |
| | (0,025) | (0,031) | (0,443) | (0,016) | (0,026) | (0,395) |
| Age | 0,060 | 0,059 | 0,057 | -0,002 | -0,002 | -0,007 |
| | (0,006) | (0,008) | (0,018) | (0,004) | (0,006) | (0,030) |
| White Coll | -0,115 | -0,463 | -0,013 | 0,181 | 0,287 | 0,615 |
| | (0,354) | (0,182) | (1,128) | (0,228) | (0,319) | (0,473) |
| Blue Coll | 0,390 | -0,644 | | -0,081 | 0,024 | |
| | (0,481) | (0,251) | | (0,311) | (0,437) | |
| Unskilled | 0,318 | -0,404 | | -0,039 | -0,006 | |
| | (0,375) | (0,191) | | (0,242) | (0,339) | |
| Other Work | 0,858 | -0,311 | | -0,142 | -0,076 | |
| | (0,558) | (0,287) | | (0,360) | (0,502) | |
| Subord.>0 | 0,114 | -0,141 | | -0,104 | -0,136 | |
| | (0,137) | (0,088) | | (0,086) | (0,129) | |
| Educ. Residual | | | 0,031 | | | -0,581 |
| | | | (0,445) | | | (0,569) |
| Tests for RE: | | 1222,7 | 1229,4 | | 752 | 1570 |
| First Stage Estimation | | | 139 | | | 169,8 |
| F-value, HT-Instruments | | | 106,14 | | | 106,14 |
| F-value, Reform-Instruments | | | 4,34 | | | 4,34 |
| Partial R-squared, All instruments | | | 0,062 | | | 0,062 |
| Number of IV | | | 4 | | | 4 |
| Reform 1958 | | | 0,369* | | | 0,315* |
| Reform 1975 | | | 0,130 | | | 0,158 |

*Notes*: See table 8a.

## Table 9. T-Tests of validity of Instruments

| | Men | | | Women | | |
|---|---|---|---|---|---|---|
| Variable | BMI | Not Smok. | High BP | BMI | Not Smok. | High BP |
| Reform 1958 | 0,49 | -0,271 | 1,447 | -0,261 | -0,809 | -0,459 |
| Reform 1975 | -0,523 | 0,831 | 2,041 | -0,357 | 0,985 | -0,934 |
| | | | | | | |
| Means of: | | | | | | |
| Age | -0,045 | -0,019 | 0,056 | 0,110 | -0,029 | 0,084 |
| White Coll | | 1,610 | | -2,207 | 1,220 | -2,471 |
| Unskilled | 1,869 | | | | | |

Notes: See table 6.

## Table 10. Ordered Logit of SRHS, including Health Inputs.

| | Men | | Women | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Education | -0,028 | -0,033 | -0,039 | -0,043 |
| | (0,015) | (0,015) | (0,015) | (0,015) |
| Age | 0,026 | 0,029 | 0,022 | 0,024 |
| | (0,003) | (0,003) | (0,004) | (0,004) |
| High BP | 0,657 | 0,629 | 0,345 | 0,333 |
| | (0,124) | (0,125) | (0,130) | (0,130) |
| BMI | 0,041 | 0,044 | 0,036 | 0,040 |
| | (0,011) | (0,011) | (0,010) | (0,010) |
| Never Smoked | -0,301 | -0,291 | -0,074 | -0,082 |
| | (0,069) | (0,069) | (0,069) | (0,068) |
| Satisfied w/ Job | | -0,664 | | -0,500 |
| | | (0,067) | | (0,071) |

Notes: Simple ordered logits of SRH. Other covariates include occupation dummies, a dummy for subordinates a

## Table 11. Linear Probability Models of SRH

| **Men** | | | | | | | |
|---|---|---|---|---|---|---|---|
| Estimator | OLS | 2SLS | 2SLS | FE | 2SLSFE | CF | CFRE |
| Education | -0,015 | -0,032 | -0,017 | -0,027 | -0,048 | -0,032 | -0,041 |
| | (0,005) | (0,022) | (0,069) | (0,005) | (0,010) | (0,022) | (0,020) |
| Educ. Residual | | | | | | 0,018 | 0,026 |
| | | | | | | (0,022) | (0,020) |
| **Women** | | | | | | | |
| Education | -0,018 | -0,073 | -0,173 | -0,033 | -0,084 | -0,073 | -0,071 |
| | (0,006) | (0,021) | (0,116) | (0,011) | (0,011) | (0,021) | (0,019) |
| Educ. Residual | | | | | | 0,061 | 0,058 |
| | | | | | | (0,022) | (0,020) |
| IV | no | all valid | reform | no | all valid | all valid | all valid |

*Notes*: Linear regressions where occupation, subordinates, age and year are also included. FE is the Hausman-Taylor estimator where education is estimated on within transformed residuals of SRH. 2SLSFE is the Hausman-Taylor estimator applying 2SLS in the second stage. CF is the control function estimator, including education residuals, from a first stage estimation, and CFRE adds random individual effects. Valid instruments are school reforms and Hausman-Taylor instruments passing tests in table 6.