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In sickness and in health – till education do us part: Education effects on hospitalization

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Abstract

Previous studies addressing the question of causality between health and education have provided inconclusive results due to among others small samples and imprecise two-stage estimators. This study improves upon this using large administrative panel data sets and simultaneous models of hospitalization and education, where we distinguish between the first hospitalization in a given year and subsequent ones. Changes in education levels between rural and urban areas after a Danish school reform are applied to identify a causal effect of education on hospitalization. Education has a negative (significant) effect on the first incidence of hospitalization, whereas conditional on being hospitalized once, it cannot be rejected that education has no effect for men and a positive effect for women on subsequent hospital stays.

JEL: I12, I20, J18, C33, C35

Keywords: Hospitalization; Education; School reform; Simultaneous models; Hurdle model

1. Introduction

The aim of this paper is to explore whether education improves health. A large number of studies have documented that there is a robust relationship between socioeconomic status (SES) and health, but very few of these address the direction of causality between SES and health, and those who do often yield imprecise results due to the use of small data sets or inefficient methods.

There are several pathways through which education may affect health. It could be better knowledge of good health behaviour, a faster implementation of new health technologies and higher income, improved job conditions, and higher job satisfaction.

A reason for the concern that education related differences in health are not causal stems among others from strong evidence that childhood circumstances play a vital role for both educational attainment and adult health, as indicated by research by e.g. Behrman and Wolfe (1984) and more recently by Case et al. (2002; 2003). Some evidence even points to the importance of conditions in uterus on adult health (Barker et al. 1991), which is sometimes referred to as the "Barker Hypothesis". Some pathways from childhood to adulthood health may depend on genetically inherited traits, others have their foundations in the family's lifestyle, some of which are the results of a cultural process, and others again are the results of conscious choices including time invested in educating children in healthy behaviour. Since the time horizon between time of investment in health and the time where benefits are gained may be long, the preference for today versus tomorrow may play a role in the determination of health as conjectured by Fuchs (1982). The connection to education comes in to play because all these pathways, transmitting health from parent and childhood to health in adulthood, are likely to play a role for decision-making, as well as for possibilities, when educational decisions are made as adolescents. If these relationships are the driving force behind the relation between health and education, it implies that raising education does not improve health per se.

This study applies a large Danish administrative register data set covering the period from 1990 to 2000 with information education, hospitalization and diagnoses. Hospitalization is modelled using a hurdle model, distinguishing among the first and subsequent hospitalizations in a given year. The basic hurdle model is extended to a simultaneous model of hospitalization and education. The combination of a large panel data set and the use of simultaneous modelling should improve upon a problem that has plagued most previous studies of education effects on health, namely precision. Most previous studies have applied two-step methods on small survey data sets, which are less efficient and which to a higher extent may be plagued by biases from weak instruments (Hahn & Hausman, 2002). Finally, a Danish school reform is used in a novel way to provide exogenous variation in education that identifies a causal effect of education on hospitalization.

The main results are conducted using an indicator of whether hospitalization with either of the following diseases has been diagnosed: endocrine or nutritional, heart, other circulatory, digestive or respiratory diseases. This serves the purpose of focusing on hospitalization as an indicator of a rather severe disease (as opposed to e.g. examinations or pregnancy-related diagnoses) and in order to - on a rough level - include diagnoses which is believed to be related to life-style and education. The results indicate that estimates of the effect of education which assumes that education is exogenous are biased. Still, when endogeneity of education is accounted for, education reduces hospitalization by 1.9 percentage point for women and 1.5 point for men on average (corresponding to relative effects of respectively 39.7% and 32.2%.) and the effects are statistically significant. However, once hospitalized, educated women have more hospitalizations in a given year than women with no education. We interpret this as reflecting a higher demand for health care for educated women.

The paper is structured as follows. The next section provides a brief review of central findings in the literature and a discussion of hospitalization as measure of health. Section 3 describes the data and section 4 the school reform used to identify exogenous variation in education. The econometric models are presented in section 5 and section 6 describes the results from the empirical analyses. Section 7 discusses the findings.

2. Previous evidence

Education and Health

In the literature on education effects on health a common hypothesis is that education enhances healthy behaviour and use of health technology (e.g. Grossman, 1972; Kenkel, 1991; Goldman & Lakdawalla, 2001). An alternative explanation for education effects on health is that education is a proxy for social status, which may affect health through levels of stress, see Deaton (2001) for a thorough discussion. The theoretical background in economics for health determination is often taken to be the health production model from Grossman (1972). In this model, better health is valuable because it means less sickness time, during which it is assumed that no work or leisure consumption takes place. This introduces incentives for health investments which may differ at different education levels. Within this literature there is a concern that the strong correlation between education and health reflects an unobserved variable common to health and schooling determination. The most prominent candidates for the unobserved variables are time preferences (Fuchs, 1982) and health endowments.

Comprehensive reviews of empirical studies of education effects on health within economics are provided in Grossman and Kaestner (1997) and Grossman (2000; 2003). Grossman (1975) and Auld & Sidhu (2004) examine the extent to which the effect of schooling on health is accounted for by cognitive ability. Treating education as exogenous they find that about 20-25% of the education effect is accounted for. When Auld & Sidhu (2004) instrument education, using parental education as instruments, the effect is reduced by 50% (and becomes insignificant). However their instrument might be questionable. They also find that the effect of education on health is larger for people with low levels of education and with low ability scores. Other papers using instrumental variable techniques are e.g. Berger and Leigh (1989), who use per capita income and per capita expenditures on education in the state of birth as instruments for education in a regression with level of blood pressure as dependent variable. They find that correcting for endogeneity of education reduces education effects but they remain significant. Arkes (2002) applies within-state differences in unemployment rates as instruments for education. He finds significant education effects for two of the three health measures.

At least four recent studies of education effects on health have applied more credible quasi-social experiments to identify exogenous variation in education. This is Arendt (2004), Lleras-Muney (2004), Adams (2001) and Spasojevic (2003). In Arendt (2004) a Danish school reform taking place in 1958, described in the next section, is used to identify education effects on self-reported health status, body mass index and indicators of having high blood pressure and never been smoking[1]. Although the estimated effects of education are large in magnitude, the findings are inconclusive due to large standard errors. Lleras-Muney (2004) uses changes in compulsory schooling laws across US states to instrument education effects

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[1] Compared to this study, we use additional information about differences in the impact of the reform in 1958 in urban and rural areas.

on mortality. Her study yields a significant education effect for grouped census data but the effect is insignificant when applying individual data. Adams (2001) adapts the identification strategy suggested by Angrist and Krueger (1991), using quarter of birth in a U.S. cross-section of individuals. He uses self-reported health and a number of variables describing functional limitations as health measures. The estimated education effect is again insignificant and F-tests on the instruments indicate a problem of weak instruments. Finally Spasojevic (2003) uses changes in compulsory schooling in Sweden in the fifties, using answers to questions about fifty different symptoms and body mass index. Once again, some of her results may point towards weak instrument problems. Most of the estimated effects of education are reported to be significant using one-tailed tests. However, with two-tailed tests, none of the instrumented education effects are significant at conventional significant levels. In all these studies, instrumented education effects are larger than non-instrumented. Although this result may have plausible explanations (e.g. because of negative correlation between unobserved components of health and education or in terms of local average treatment effects, see Imbens & Angrist 1994), it may also be because of biases of 2SLS or control function type of estimators.

Hospitalization as a health measure

In many studies hospitalization is viewed as a measure for the demand for health care (e.g. Cameron et al., 1988; Ripahn et al., 2003). However, the ambiguous nature of hospitalization as both a health measure and a measure for health care demand has been emphasized (Geil et al., 1997). Given information on diagnoses, such that e.g. examinations and stays during pregnancy can be sorted out, the event of hospitalization is an indicator of a rather severe health condition, possibly diminishing the impact from health care demand. Nevertheless, the possible ambiguous nature must be kept in mind when interpreting results.

One way to handle the ambiguous nature of hospitalization is to distinguish between the event of being hospitalized and the number of hospitalizations within a year. In this two-part process, hospitalization may to a larger extent be affected by a large enough deterioration of health, whereas given one hospitalization in a year, the number of hospital stays may to a larger extent be affected by other factors (the severity of the illness, decision to seek additional treatments, supply and nature of medical treatments and in part, decisions taken by doctors). This is not to say that health care demand does not affect the first hospitalization, but given conditioning on the presence of severe diseases, it is likely to be limited.

Although the number of hospital days is available in the data used for this study, it has been chosen not to use this. The reason is that the number of days hospitalized is likely to be affected by changes over time in types and duration of treatments.

Other studies of hospitalization include among others Lancaster and Intrator (1998) who model frequency of hospitalization events for AIDS-patients as a Poisson process jointly with survival time, Browning et al. (2003) who apply the same data on hospitalization as the present study, when studying the relation between health and unemployment and Crystal et al. (1999), modelling transitions between the states: hospitalization, not in hospital, and death, in their study of AIDS-patients.

3. Data

An extensive individual micro panel data set is used. This is a sample from administrative register data from Statistics Denmark covering the years 1981-2000. The sample consists of 10% randomly drawn individuals from the Danish population aged 15 or older, and does not suffer from common problems such as attrition, except for those due to death or immigration. The data includes register information on numerous individual outcomes, ranging from labour market variables and household characteristics to various health related variables.

The data includes information on highest educational attainment. The health variables used in this study are from the Danish National Register of Patients. The hospitalization information includes information on specific diagnoses, treatments and length of stay for discharges from hospital departments for somatic diseases. There is only information on patients from 24-hour surveillance inpatient clinics. It is stressed that nearly all hospitalizations are at public hospitals where costs are covered by the Danish state. Even at private hospitals, costs are partly covered by a public health insurance system, covering nearly all individuals with residence in Denmark. The diagnoses are classified according to the International Statistical Classification of Diseases and Related Health Problems (ICD). From 1981 to 1993 the 8th revision is applied (ICD8) and from then on the 10th revision (ICD10) (see table A1 in the appendix).

As mentioned in the introduction, information on a Danish school reform will be applied to identify education effects on hospitalization. Therefore the analysis is narrowed to specific cohorts born from 1943 to 1950 who were enrolled in the school system a few years before and after the reform. The period of observation is been limited to 1990-2000. This limits the number of observations, but increase the frequency of hospitalizations, because older are more frequently hospitalized. It also limits possible problems with disentangling age, cohort and time effects.

4. The school reform

In this section the main features of the Danish school reform that is applied to identify exogenous variation in education is presented. School reforms have previously been applied to construct instruments for education, as mentioned above, see also e.g. Harmon and Walker (1995) or Meghir and Palme (2001).

The school reform considered took place in 1958. The reform had a possible general impact on all schools, by lowering barriers for further education, as well as an impact specific for schools in urban areas.

The general impact came about because the reform abolished a partition of elementary schooling before and after 5th form, Yearbook of the Danish Parliament (1958). Prior to this reform, if the pupils did not pass a test after 5th form, they could not enter the middle-school (lower secondary: 6- 9th or 10th form), and would continue schooling in another track, ending schooling after 7th form. The partition before 1958 implied that pupils in the different school tracks in 6th and 7th form experienced substantial differences in curriculum level, whereas these differences were mitigated from 1958 and onwards. Further, to continue schooling after 7th form, formal tests after 5th form were replaced by recommendations made by teachers after 7th form. That is, a selection mechanism was postponed (and substituted with another) allowing late developed children a later chance to develop schooling skills.

The impact specific to schools in urban areas came about because prior to the reform in 1958, a distinction was made between rural and city elementary schools and the reform eliminated this distinction. The city schools ("købstadsskole") were placed in cities of some reasonable size which for historical reasons were given special trading rights ("købstæder"). Prior to the reform the curriculum level and the number of lessons were lower in the rural school (in most rural schools, school days were limited to every second day), and this was brought to the same level as in the city schools. The rural schools also gained the same right as the city schools to form classes for 8th to 10th form (compulsory years preparing for upper secondary schooling). It is a common view that the schooling structure prior to 1958 limited access to further education especially for children from less educated backgrounds and for children living outside urban areas with city schools, which the 1958 reform helped to alleviate (Bryld et al. (1990) and Hansen (1982)).

The variable used to provide exogenous variation in education is based on the fact that the 1958 reform differed for urban and city schools. To be specific, when modelling educational attainment we assume there is a general effect of the reform as well as a specific effect in rural areas. It is described how these are measured in the following.

The reform was enforced in 1958, which means that pupils who enter 6th form in 1958 are the first who are not divided on basis of a test following 5th form, and therefore the first who are affected by the general effect of the reform. However, no information on the year in which individuals enter primary school, nor of the year in which they took their final exam, is available, and only age by January 1 is known. Since most Danish children enter school (1st form) in the year where they turn 7, it is assumed that individuals enter school in the year that they have turned 6. The variable that indicates whether individuals are affected by the general effect of the reform is therefore a dummy variable taking value one if individuals are 11 or less in 1958 (by Jan. 1.). These individuals are at most 34 years of age when first observed in 1981 and at most 53 years of age in 2000.

To measure the effect of the reform specific to rural areas, ideally information on whether individuals attended a rural or city school is needed. This is not available. Instead, an indicator of whether individuals were born in an area (on a parish level), which is likely to have had a city school. The latter is proxied using two types of information: A list of Danish "købstæder" (there are 87 of them) and an indicator of cities with more than 2.000 inhabitants in 1960 to form an indicator of city schools (Christophersen, 2000; Matthiessen, 1985)[2]. To distinguish the actual city school from schools in these areas, which are "approximated" to be city schools, we call the latter urban schools.

One may argue that the main effect of the reform is to lower educational barriers and therefore there is still a potential endogenous choice involved in the variation in educational attainment caused by the interaction variable. To this criticism it is noted that this is the case with most identification schemes relying on supply-side changes or reductions in costs of schooling, see e.g. the discussion in Card (2000). In addition, because the reform increased school quality (the curriculum level and number of lessons) in rural areas as well as decreasing the proximity of schools available at the 8th to 10th level of schooling (by providing rights to form these classes in rural areas), the set-up used in this study is based on *changes* in proximity to school as well as *changes* in school quality in rural areas, and is therefore more likely to provide exogenous variation than proximity to school per se. The latter has been used e.g. in Kane and Rouse (1993), Card (1995) and Conneely and Uusitalo (1997).

Furthermore, while there is an imprecision involved in using parish of birth rather than parish of school attendance, it gives a variable more likely to provide exogenous variation in schooling than the actual information of whether attending school in rural areas. This is because the latter to a higher degree may be affected by school- or housing location selection. All that is required for the region of birth to be useful is that those born in rural areas are more

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[2] Rural schools could apply for the same rights as city schools, and especially schools in cities, which are not a "købstad", but of some size did. When merging our list of "købstæder" to a list of parishes, parishes of different denominations in the same "købstad", are all listed as a "købstad". All parishes in the current municipalities of Copenhagen, Frederiksberg and the northern part of Copenhagen are also listed as "købstæder".

likely to attend schooling in rural areas, hence to be affected by the reform in rural areas. The relation between parish of birth and parish of school attendance depends among other things on the share of families moving between a rural parish and an urban parish between the birth of the child and the time at which the child enters 6^{th} form. The share of children in 1976 (earliest year, in which the information was found) who change municipality some time between their birth and the age of 16 is around 2-15%, with an average of 6,5%, so judging from this, birth in a rural parish should be a good predictor of school attendance in rural areas.

In order to obtain effects which can reliably be interpreted as arising from the reform, attention is restricted to individuals from birth cohorts three years prior to and five years (in order to allow the reform to have some effect) after the 1958 reform. This sample includes individuals aged 31 to 38 in 1981 who can be followed over time until year 2000, where they are 50 to 57.

5. Econometric models

In this section a simultaneous model of hospitalization and education is presented with the purpose of providing consistent estimates of the effect of education on hospitalization.

The number of hospital stays during a year is used as quantitative measure of the presence of a disease. It is likely that the process leading to hospitalization is different from the process determining number of stays, given hospitalization. As shown by e.g. Mullahy (1986) and Pohlmeier and Ulrich (1995), when the processes are different, one can estimate a hurdle model of the joint distribution of hospitalization and number of hospital stays during a year in two separate models, because the likelihood of number of stays, S, splits into two separate parts:

$$
\begin{aligned}
L &= \sum_i \log P(S_i = s \mid \phi_1, \phi_2) \\
&= \sum_i 1(s_i = 0)\log P(s_i = 0 \mid \phi_1) + 1(s_i > 0)(\log P(s_i > 0 \mid \phi_1) + \log P(s_i \mid s_i > 0, \phi_2)) \qquad (5.1) \\
&= \sum_i 1(s_i = 0)\log P(s_i = 0 \mid \phi_1) + 1(s_i > 0)\log P(s_i > 0 \mid \phi_1) + \sum_{i:v>0} \log P(s_i \mid s_i > 0, \phi_2)
\end{aligned}
$$

The first part of this likelihood is the binomial likelihood of hospitalization and the second part is a count data model conditional on positive counts. The first part of the likelihood is referred to as the binomial part of the model and the second as the hurdle part of the model in order to distinguish it from a simple count model. ϕ_1 and ϕ_2 are unknown parameters from the two processes. For separate estimates of the two parts of this likelihood to be consistent it is required that the two sets of parameters are variation independent.

A model with individual effects is suitable to account for differences in early determined endowments affecting later educational abilities and adult health. As most individuals do not change education level, a fixed effect procedure (e.g. Hausman et al., 1984) cannot be applied. Let *H* be an indicator of hospitalization, *E* an indicator of a given education level and *S* the number of hospital stays within a year. Binomial models are used for the two first and a modified Poisson model for the latter. A standard modification of the Poisson model is applied, allowing a log-linear error term to affect the frequency rate, λ. This serves as a mean to avoid several restrictive assumptions of the Poisson[3], and have been applied frequently both in cross-sectional models (e.g. Cameron et al., 1988; Geil et al., 1997) and panel models

[3] The error term loosens the restrictive assumptions that mean and variance of the count process are equal, i.e. allows e.g. for over-dispersion, and contagion effects (that the probability of an additional event in a given period does not depend on the number of previous events). Both are likely to be violated in practice.

(Hausman et al., 1984). So, given observed covariates, X, the main outcomes are described by the following equations:

(5.2)
$$
\begin{aligned}
&H_{it} = 1(\beta_0 + \beta_1 E_i + \Pi_1 X_{1it} + \varepsilon_{1it} > 0),\ \varepsilon_{1it} = \alpha_{1i} + u_{1it} \\
&P(S_{it} = s_{it}) = e^{-\lambda_{it}} \frac{\lambda_{it}^{s_{it}}}{s_{it}!},\ \ln(\lambda_{it}) = \gamma_0 + \gamma_1 E_i + \Pi_2 X_{2it} + \alpha_{2i}, \\
&E_i = 1(\mu + \Pi_3 X_{3i} + \varepsilon_{3i} > 0)
\end{aligned}
$$

The following is assumed about the error terms:

(5.3)
$$\operatorname{cov}(\varepsilon_3, u_1) = \operatorname{cov}(\alpha_1, u_1) = 0,\ E(\varepsilon_1 \mid X_1) = E(\alpha_2 \mid X_2) = E(\varepsilon_3 \mid X_3) = 0,$$

It is stressed that the α's are assumed to be uncorrelated with both u and the explanatory variables other than education, such that endogeneity of education only arises through correlation between α_j ($j=1,2$) and ε_3. This correlation is assumed to arise from a common factor structure, which is introduced on an ad-hoc basis for each of the latent hospital effects (depending on which part of the hurdle model that is modelled):

(5.4)
$$\varepsilon_3 = \tau_j \alpha_j + u_3$$

Where u_3 is a random error term. It is assumed that the α's follow a discrete distribution with K points of support (see e.g. Heckman and Singer 1984). For the bivariate model of hospitalization and education, the contribution to the likelihood for a given individual therefore is:

(5.5)
$$
\begin{aligned}
&P(\{H_{it}\} = h_i, E_i = e_i) = \sum_{k=1}^{K} p_k P(\{H_{it}\} = h_i \mid \alpha_{1k}) P(E_i = e_i \mid \alpha_{1k}) \\
&= \sum_{k=1}^{K} p_k P(E_i = e_i \mid \alpha_{1k}) \prod_{t=1}^{T_i} P(H_{it} = h_{it} \mid \alpha_{1k}),\ h_{it} \in \{0,1\} \text{ and } e_{it} \in \{0,1\}
\end{aligned}
$$

$\{H_{it}\}$ is a vector containing the T_i observations for individual i. E_i is the first observation on education. The first equality is due to the assumption that u is independent of ε_1. Assuming normality of u_3 and u_1 the conditional probabilities in (5.5) become:

(5.6) $$P(E_i = 1 \mid \alpha_{1k}) = \Phi(b + \tau_1 \alpha_{1k}),\ \ P(H_{it} = 1 \mid \alpha_{1k}) = \Phi(a + \alpha_{1k})$$

Here, a and b are appropriate index functions of covariates and their parameters shown in (5.2) and Φ is the standard cumulative normal distribution function. Note that for the model to be consistent with the hurdle model, hospitalization should be given by the binomial part of the Poisson distribution (since $H = 1(S > 0)$). In the empirical analyses both are used.

The model yields a simple test of exogeneity of education, namely by the test that the factor loading τ_1 is zero. In order not to rely entirely on identification of the education effects by non-linearity, exclusion restrictions are required, i.e. assumptions that some variables affect E but no H or S. This is where the school reforms come in to play.

For the number of hospital stays, the likelihood is similar to that in (5.5) with the binomial distribution of hospitalization replaced by:

(5.7) $$P(S = s \mid s > 0) = \frac{P(S = s)}{1 - P(S = 0)} = \frac{e^{-\lambda}\lambda^s / s!}{1 - e^{-\lambda}} = \frac{\lambda^s}{(e^{\lambda} - 1)s!}$$

Note that the individual specific effect is not identified when a constant term is included. Therefore the distribution is normalised in the same fashion as e.g. Van der Klauuw (1996), such that one point of support for α is zero. In order to secure that the probabilities are between one and zero, they are specified as logistic probabilities. With two points of support this is:

(5.8) $$P(\alpha_{1i}) = p_i = \exp(L_i)/(1 + \exp(L_2)), L_1 = 0, \alpha_{11} = 0, \quad \alpha_{12} = a.$$

6. Evidence

Before the model estimates are presented, some descriptive statistics are provided. Recall, that focus is upon a cohort born between 1943 and 1950. After the descriptive statistics, the binomial models of hospitalization are presented, followed by the count models for the number of hospital stays in a given year.

The distributions of education and sample means of age, region of living and the indicators of whether affected by the reform and parish of birth are shown in table 1. 17% of the women in the sample and 19% of the men have a high level of education and almost four out of ten women only have primary school as the highest level of education, whereas this is only the case for three out of ten men [4]. 41% are born in an urban parish (BUP) and 65% are in a cohort which is affected by the 1958 reform (SAR). Combining these show that 27% are born in an urban parish in a cohort affected by the reform.

Table 1. Descriptive statistics, 1943-50 birth-cohorts. 1990-2000.

	Women		*Men*	
Variable	**Mean**	**Std**	**Mean**	**Std**
Education level				
Medium	0.42	0.49	0.50	0.50
High	0.17	0.37	0.19	0.39
Age	48.41	3.86	48.39	3.86
Large city other than capital	0.36	0.48	0.35	0.48
Minor city	0.27	0.44	0.25	0.44
Rural district	0.31	0.46	0.32	0.47
Born in urban parish (BUP)	0.41	0.49	0.41	0.49
Went to school after reform (SAR)	0.65	0.48	0.65	0.48
Interaction between BUP and SAR	0.27	0.44	0.27	0.44
Number of observations	341,373		350,485	

Note: Includes repeated observations for individuals more than once in the sample. Medium education refers to high school, vocational or short-course advanced. High education refers to medium- or long-course advanced studies. The residual group is primary schooling only. The residual group for region of living is the municipality of Copenhagen.

[4] See descriptions to the table. A short-course degree is of 1-2 years of length, a medium-course degree is of 3-4 years of length and a long-course degree are from 5 years and longer.

Focus is given to hospitalizations where either one of the following diagnoses are given: endocrine and nutritional diseases, including among other diabetes type II, heart diseases and other circulatory diseases and diseases in respiratory or digestive organs. These admittedly rough groups at least include some diseases which are connected to individual life style, where social status may play a role e.g. through levels of stress and health behaviour. It is important however to acknowledge that these groups of diagnoses cover broad ranges of diseases that may have different risk-factors associated with them. To group diagnoses properly, if possible at all, one would have to construct new groups, selecting out separate diagnoses that are believed to be highly associated with social and life style risk factors. This is not pursued in the present study.

Table 2 shows the incidence rate for these five groups of diseases and illustrates how they are related to education. It is seen that hospitalization is a quite rare event, the total incidence rate for all five diagnoses being only about 4 percent, even though the numbers shown are over an eleven year-period, and that for a given hospitalization, several diagnoses may be given.

Table 2. Diagnose incidence and relation to education for the 1943-1950 birth-cohorts, 1990-2000.

	Women			*Men*		
	Incidence rate (%)	***Relation to education*** [a]		***Incidence rate (%)***	***Relation to education*** [a]	
Diagnosis		*Medium*	*High*		*Medium*	*High*
Endocrine or nutritional disease	0.004	0.59*	0.45*	0.005	0.83*	0.53*
Heart disease	0.004	0.65*	0.55*	0.009	0.92**	0.51*
Disease in other circulatory organs	0.024	0.84*	0.75*	0.023	0.86*	0.65*
Disease in respiratory organs	0.005	0.58*	0.49*	0.005	0.76*	0.52*
Disease in digestive organs	0.010	0.71*	0.58*	0.013	0.85*	0.71*
All five diagnoses	0.039	0.77*	0.67*	0.041	0.86*	0.66*
Number of Hospital stays (means)	0.063	0.75*	0.66*	0.075	0.86*	0.61*
Number of observations	341,373			350,485		

[a] Shows relative ratios of being diagnosed with a given diseases for intermediate and high levels of education,

compared to the short level. Significance refers to a test comparing mean hospitalization of intermediate and high educated with low educated. For number of hospital stays, it shows the relative means. * p < 0.01, ** p < 0.05.

The incidence rates for men and women are quite alike. The mean number of hospital stays in a given year is given in the last row, and is 0.063 for women and 0.075 for men. These low numbers reflect the large share with no hospitalizations. For those with a hospitalization event for the mentioned diagnoses, the mean number of hospital stays is approximately 1.6 for women and 1.8 for men. This is calculated from the frequency distributions of hospital stays, which is shown in table 3. From this table it is seen that among the 341,373 individual-year observations for women, 13,143 includes a hospitalization, and among the 350,485 observations for men, 14,388 includes a hospitalization. Among those hospitalized in a given year, 38% of the women and 45% of the men are hospitalized more than once in the same year.

Table 3. Number of hospital stays per year, 1943-1950 birth-cohorts, 1990-2000.

Number of Hospital visits	Women	Men
0	328230	336097
1	8149	7842
2	3260	4037
3	844	964
4	589	964
5	125	200
6	103	229
7	25	61
8	23	51
9	12	14
10	5	16
11	4	2
12	2	5
13	2	2
14	0	1

Note: Only diagnoses from Table 2 are included..

Educational differences in hospitalization

Crude measures of the relation between diagnoses and education are provided in the 3rd and 4th column of Table 2 for women and in the 6th and 7th column for men. The table shows the relative ratios of being hospitalized with a given diagnosis between those with the medium or the high level of education compared against those with the low level.

The relative risk ratio of becoming hospitalized with an endocrine or nutritional disease is 0.59 for women with a medium level education compared to those with no education beyond primary school, and it is significantly different from one. The relative risks decrease with educational level for all diagnoses and the differences in incidence rates across education levels are significantly different for all diagnoses. Whereas the overall education gradients in hospitalizations are about the same size for men and women, a larger part is concentrated between the primary and medium level of education for women. Most of the relative risks are quite small, that is, educational differences in hospitalization are large. The bottom row shows that education related differences in the mean number of hospital stays in a given year with either one of the five selected diagnoses are also large and significant.

Model estimates

The rest of the paper focuses entirely on hospitalization with either one of the five selected diseases: endocrine or nutritional diseases, heart diseases and other circulatory diseases, respiratory diseases or digestive diseases. As mentioned, these are – in broad terms – all diseases where life style and social status have been shown to be an important risk factor.

Because the estimation procedure described in section 5 assumes that all other regressors except education are exogenous, the number of control variables is limited.

Common controls such as income, labour market status and marital status are likely to be endogenous with respect to health and are therefore not included. The implications of this are discussed below. Education is allowed to be affected by whether born in a parish with urban schools, a linear cohort effect, whether affected by the 1958 reform and interactions between these. Hospitalization is affected by the same variables, except the interaction between the reform and urban school indicators, a specific age effect above age 50, 4 regions of living (the capital area, other larger cities, other cities and the rest) and education. It is therefore the interaction between the general reform effect and being born in an urban area that is excluded from the hospital equation. The education effect is therefore identified from differences in the response to the school reform between rural and urban areas.

We limit attention to whether individuals obtain any education beyond primary school. This is the level where the 1958 reform is most likely to have an effect. The level or number of years of education would give additional variation, but is also more likely to be affected by individual choices.

The impact of the school reform on educational attainments

Table 4 presents results from Probit estimations of educational attainment.

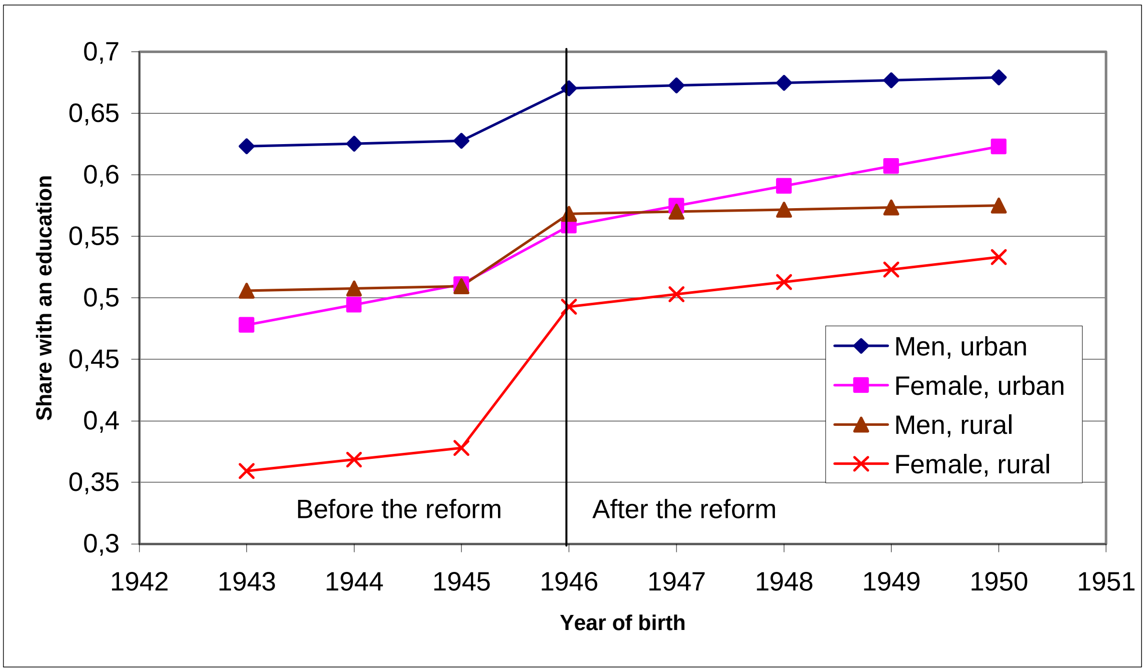
Table 4. Probit estimates of educational attainment.

	Women		Men	
Parameters	Estimates	Std.err	Estimates	Std.err
constant	-0,064	0,005	0,273	0,005
Cohort	0,048	0,002	0,024	0,002
Born in urban	0,337	0,008	0,328	0,009
Reform	0,034	0,011	0,060	0,011
Cohort*B_Urban	0,009	0,004	-0,009	0,004
Reform*B_Urban	-0,136	0,017	-0,045	0,017
Log-likelihood	-0,665		-0,611	
Observations	341373		350485	

Note: The outcome is an indicator of any education beyond primary school.

It is seen from the table that for both gender, the indicator of being going to school after the 1958 reform has a positive coefficient and is significant. This means that, on top of a general cohort effect, a higher share of those affected by the reform than those not affected obtain an education beyond primary schooling. The coefficient to the indicator of being born in an urban parish is also positive and significant, and about the same size for men and women. The interaction between the reform indicator and the dummy for being born in an urban area is negative and significant for both men and women. This is the variable excluded from the hospital equations, and the necessary condition that it affects education is therefore fulfilled. The explanatory power of the variable excluded from the hospital equation for women is rather high, since the F-value is 64.3. This indicates that if the interaction variable was used as an instrument, there is no "weak instrument" problem, in the terminology of e.g. Staiger and Stock (1997), who recommend an F-value of the instrument of at least size 5 to 10. For men there might be a problem, since the F-value is only 6.6. Furthermore, the interaction between parish of birth and the reform dummy was insignificant in the 2% sample for men. Since only one exclusion restriction is applied it cannot be tested. The underlying assumptions of the exclusion restriction are discussed further below. The impact of the 1958 school reform on the share who obtains an education beyond primary school is shown in figure 2, visualizing the upward cohort drift, the large rural-urban difference and the change after the reform. Measuring the difference in the probability of obtaining an education beyond primary school between urban and rural districts and taking the before-and-after reform-change of this difference gives the variation that is used to identify the education effect. Possible explanations for why this change is larger for women than for men are discussed below.

Figure 2. Education beyond primary school and the 1958 school reform.



Note: From estimates in table 4. Urban and rural refers to parish of birth.

Hospitalization

Next, hospitalization outcomes are considered, beginning with binomial models of any hospitalization (with one of five selected diagnoses as mentioned earlier). Comparison of estimates from a binomial Poisson and a Probit model have been conducted, and although the size of the effect of education seems to depend somewhat on the choice of the distribution function, the substantive conclusions are the same from the two models. Here we present the Probit model. First, results for women are examined.

Table 5 contains estimates of the parameters in the hospitalization equation using a binomial Probit model for women. The first column contains results for a standard Probit model, and because the parameter to the education variable is negative (-0.144) and significant, it confirms that those with any education beyond primary school are less likely to be hospitalized. There are also significant age and regional patterns in hospitalization rates, whereas the parameters to the variables describing whether born in an urban parish or being born after the reform are not significant. The next estimates are from a random effects Probit model. A simple way to judge the importance of the random effects is to examine whether introducing them makes a difference to the estimated parameters. Table 5 shows that particularly the estimate of the education coefficient is close to the estimate without random effects, although the distribution of the random effect is not degenerate. Recall that the distribution is assumed to be discrete with two points of support, and that one point of support is normalized to be zero. The other point is given by the A-parameter, which is significantly different from zero. The L-parameter shows (see (5.8)) that the highest value of the random effect has a mass point probability of 71% ($e^{0,898}/(1+e^{0,898})$). The model has been estimated with three points of support, and this produced almost exactly the same results.

Table 5. Binomial Probit model of Hospitalization, 1990-2000, women.

	Probit		RE-Probit		Bivar ML	
Parameters	Estimates	Std.err	Estimates	Std.err	Estimates	Std.err
constant	-1,659	0,019	-2,239	0,033	-2,061	0,026
Education	-0,144	0,008	-0,150	0,009	-0,060	0,016
Age	0,192	0,009	0,230	0,009	0,240	0,009
Large city	-0,137	0,016	-0,017	0,017	-0,076	0,021
Minor city	-0,152	0,017	-0,033	0,018	-0,092	0,022
Rural district	-0,130	0,017	-0,008	0,017	-0,054	0,021
Cohort	0,001	0,004	0,000	0,004	0,017	0,005
Born in urban	0,006	0,015	-0,038	0,016	0,030	0,020
Reform	0,002	0,015	0,003	0,019	-0,044	0,020
Cohort*B_Urban	0,006	0,004	0,017	0,004	0,000	0,005
Gen. Residual						
R						
A			0,955	0,018	1,199	0,011
L			0,898	0,076	2,289	0,044
τ					-0,264	0,042
Log-likelihood	-0,1618		-0,1580		-0,2184	
Observations	341373		341373		341373	

Notes: The dependent variable is an indicator taking value one, if a hospitalization takes place in a given year, where either endocrine or nutritional diseases, heart diseases, other circulatory diseases, respiratory or digestive diseases are diagnosed. Bivar ML is the estimator based on the likelihood in (5.5). The three regional dummies

(Large city, Minor city and Rural district) refers to current residence and has Copenhagen as reference. A and L are point of support and log-odds of the probability of being a high-hospitalization-propensity-type. τ is a factor loading on the health specific individual effect in the education equation.

The next pair of columns contains results from estimates of the model based on the bivariate likelihood with individual contributions described in (5.5). The education coefficient in the simultaneous model is a third of the size in the RE Probit, but is still negative and significant (the standard error is 0.016). Note though that the sizes of the coefficients are not comparable since they are derived in conditional respectively simultaneous distributions. The τ-parameter is also significant. Since this is the factor loading on the individual specific hospital effect in the education equation, exogeneity of education with respect to hospitalization is rejected. Therefore the estimates from the bivariate model are preferred to the other estimates. The τ-parameter is negative, meaning that an unobserved propensity to become educated is negatively correlated with an unobserved propensity to become hospitalized. This makes sense if it is believed that people with better schooling endowments also have better health endowments. It is also in agreement with the hypothesis that a common factor, like time preferences, is an important determinant of both health investment and schooling decisions.

Table 6 contains results from similar estimations for men. More or less the same results appear for men as for women. The education coefficient is negative and significant in all estimations. Although the education coefficient in the RE Probit is a lot smaller for men than for women (-0.10 as opposed to -0.15) the bivariate estimates are almost exactly equal to the results for women. Since the τ-parameter is significant, the estimates from the bivariate model are again preferred to the other estimates.

Table 6. Binomial Probit model of Hospitalization, 1990-2000, men.

	Probit		RE-Probit		Bivar ML	
Parameters	Estimates	Std.err	Estimates	Std.err	Estimates	Std.err
constant	-1,613	0,018	-1,981	0,021	-2,083	0,025
Education	-0,102	0,008	-0,108	0,009	-0,062	0,015
Age	0,172	0,008	0,217	0,008	0,228	0,009
Large city	-0,113	0,016	-0,058	0,017	0,011	0,021
Minor city	-0,111	0,016	-0,052	0,017	0,003	0,021
Rural district	-0,113	0,016	-0,050	0,017	0,003	0,021
Cohort	-0,010	0,004	-0,007	0,005	-0,004	0,005
Born in urban	-0,008	0,015	-0,008	0,016	-0,070	0,020
Reform	0,011	0,014	0,009	0,019	-0,037	0,020
Cohort*B_Urban	0,002	0,004	0,002	0,004	0,014	0,005
Gen. Residual						
R						
A			1,128	0,011	1,307	0,010
L			1,997	0,045	2,090	0,033
τ					-0,076	0,031
Log-likelihood	-0,170		-0,163		-0,219	
Observations	350485		350485		350485	

Notes: See notes to table 5.

Number of hospital stays

Next, the number of hospital stays within a year is considered. In the rest of the paper, only the simple random effect results and the results from the full bivariate model are presented. As a starting point, Poisson models were estimated, i.e. assuming that the same process is driving hospitalization and number of hospital stays in a year. This assumption is tested below. These are placed in Table A.2 in the appendix. The single and bivariate equation models yield comparable education coefficients. They are large, negative and significant.

Table 7 presents the results from the hurdle part of the model in (5.1). Only observations with a positive number of hospital stays within a year are included. This amounts to 13.143 in the period 1990-2000 for women and 14.388 for men. For both men and women, the education effect is negative and significant in the model assuming exogeneity. When endogeneity of education is allowed for, the education coefficient becomes positive and is still significant for women, whereas it becomes insignificant for men. The τ-parameter is significant for both gender so exogeneity of education is once again rejected.

Table 7. Poisson hurdle estimates of number of hospital stays, 1990-2000.

	Women				Men			
	RE-Hurdle		Bivar ML		RE-Hurdle		Bivar ML	
Parameters	Estimates	Std.err	Estimates	Std.err	Estimates	Std.err	Estimates	Std.err
constant	-1,011	0,058	-1,192	0,066	-0,828	0,050	0,224	0,051
Education	-0,071	0,021	0,126	0,036	-0,038	0,019	0,025	0,031
Age	0,951	0,026	0,914	0,025	0,856	0,021	0,820	0,021
Large city	0,061	0,045	0,067	0,050	0,070	0,037	0,050	0,039
Minor city	0,052	0,046	0,062	0,050	0,120	0,038	0,130	0,040
Rural district	0,099	0,045	0,124	0,050	0,131	0,037	0,129	0,040
Cohort	0,066	0,011	0,049	0,011	0,063	0,009	0,059	0,010
Born in urban	-0,056	0,038	-0,037	0,038	0,047	0,031	0,039	0,035
Reform	0,015	0,042	0,050	0,042	-0,037	0,037	-0,049	0,037
Cohort*B_Urban	0,009	0,010	0,002	0,010	-0,007	0,008	-0,005	0,009
A	1,190	0,029	1,224	0,023	1,090	0,020	1,178	0,019
L	2,165	0,113	1,763	0,085	1,509	0,083	1,377	0,067
τ			-0,385	0,087			-0,162	0,070
Log-likelihood	-1,047		-1,487		-1,211		-1,583	
Observations	13143		13143		14388		14388	

Note: The dependent variable is the number of annual hospitalizations, where either endocrine or nutritional diseases, heart diseases, other circulatory diseases, respiratory or digestive diseases are diagnosed. The estimates are obtained from the sample with positive number of hospitalizations. See also notes to table 5.

In order to test the hurdle structure, Hausman tests are applied as suggested in Mullahy (1986). The Hausman test compares the restricted estimates, Poisson estimates, with unrestricted estimates. The unrestricted estimates are either from the binomial Poisson or the hurdle part of the model. The Poisson and binomial Poisson estimates are found in Table A2 and A3 in the appendix. The Hausman tests are presented in Table 8. The two first rows show tests that compare all parameters except the constant and the random effect parameters, and the bottom rows show test for the education coefficient alone. The Poisson is rejected in all cases except for men when only education estimates from the Poisson and the binomial Poisson are compared.

Table 8. Hausman test of Poisson versus Hurdle models.

	Women		Men	
	Test	DF	Test	DF
Poisson vs. Hurdle	1424,9	9	393,3	9
Poisson vs. Bin. Poisson	446,6	9	857,2	9
Only education				
Poisson vs. Hurdle	87,2	1	100,9	1
Poisson vs. Bin. Poisson	107,9	1	2,8	1

Note: All tests are from single equation RE models. Constant and RE are not included.

The tests are χ^2(DF)-distributed.

The effect of having an education

Only the index function coefficients have been presented so far. For the models considered this is enough for evaluation of the sign of the effect of a given covariate on mean outcomes. It says nothing however on the magnitude of the effect, not even in relative terms comparing models, because the estimates are identified up to scales and these are different when random effects are included or not, and because both conditional and simultaneous models are estimated.

Table 9 therefore presents predicted statistics given education, the main parameter of interest, from the different models. The statistics presented are mean probabilities of hospitalization conditional on education or not, mean number of stays and "average treatment effects" (ATE), defined as:

$$\begin{aligned} ATE_H &= \frac{1}{n}\sum_{i=1}^{n}\frac{1}{T_i}\sum_{t=1}^{T_i}\{P(H_{it}=1 \mid E_i=1)-P(H_{it}=1 \mid E_i=0)\} \\ ATE_V &= \frac{1}{n}\sum_{i=1}^{n}\frac{1}{T_i}\sum_{t=1}^{T_i}\{E(V_{it} \mid E_i=1, V_{it}>0)-E(V_{it} \mid E_i=0, V_{it}>0)\} \end{aligned} \tag{6.1}$$

For models with individual specific health effects, these are integrated out for each probability[5]. In addition to ATE, average relative treatment effects (ARTE) are presented, taking ratios rather than differences of hospitalization probabilities and mean number of stays, as is common in binomial response models.

Table 9. Predicted effect of education on hospitalization and number of hospital stays.

	Women				Men			
	P(H=1|E=1)	P(H=1|E=0)	ATE	ARTE	P(H=1|E=1)	P(H=1|E=0)	ATE	ARTE
Probit	0,034	0,046	-0,012	0,736	0,038	0,048	-0,009	0,807
RE-Probit	0,034	0,045	-0,011	0,750	0,038	0,047	-0,008	0,820
Bivariate	0,028	0,047	-0,019	0,603	0,032	0,048	-0,015	0,678
	E(V|E=1)	E(V|E=0)	ATE	ARTE	E(V|E=1)	E(V|E=0)	ATE	ARTE
Hurdle	1,555	1,595	-0,041	0,973	1,738	1,788	-0,050	0,970
RE-hurdle	1,734	1,788	-0,054	0,968	2,100	2,176	-0,076	0,963
Bivariate	1,405	1,352	0,053	1,041	2,783	2,729	0,054	1,021

Note: Predicted probability of hospitalization given education from estimates in table 5 and 6 and mean number of hospital stays given education from estimates in Table 8. ATE is the average treatment effect given as the mean of differences in individual probabilities and mean stays for those with and without education. ARTE is the average relative treatment effect given as the mean of ratios of individual probabilities and mean stays for those with and without education.

The table shows that when using the standard Probit estimates, women with an education are predicted to have 3.4 % probability of being hospitalized, compared to 4.6 % for those with no education. The ATE is -1.2 percentage points and the ARTE is 24.4% (1-0.736). The RE Probit estimates gives results that are very close to these. Even though the estimated coefficient to education is lower in the bivariate model, the conditional effect of

[5] For the bivariate hospital model, this is e.g.:

$$P(H_{it}=1\,|\,E_i=1)=\left[\sum_{k=1}^{2}p_k P(E_i=1\,|\,\alpha=\alpha_k)P(H_{it}=1\,|\,\alpha=\alpha_k)\right]\Big/\left[\sum_{k=1}^{2}p_k P(E_i=1_k\,|\,\alpha=\alpha_k)\right]$$

education is larger: the ATE is -1.9 percentage points and the ARTE is 39.7 %. The same is observed for men, where the ATE and ARTE of education from the Probit estimates are -0.9 % and 19.3 % whereas results from the bivariate model yields an ATE of -1.5 % and an ARTE of 32.2 %.

With respect to the number of hospital stays, the random effects have a larger influence on predicted number of stays, as noted previously. The ATE of education on number of stays is -0.054 for women and -0.076 when RE hurdle estimates are applied, whereas they are +0.053 and +0.054 for men when the bivariate hurdle model estimates are applied. Recall though that the effect of education in the latter case was insignificant.

7. Discussion

This study has presented different estimates of the effect of having an education on respectively hospitalization events and number of hospital stays, focusing on five life-style related diagnoses: endocrine and nutritional diseases, heart diseases and other circulatory diseases, respiratory diseases and digestive diseases. For this purpose a differences-in-differences identification strategy is used: the change in the difference in educational attainment between individuals born in rural and urban areas, before and after a school reform at the primary level in 1958. The idea is that the reform has both a general impact as well as a rural school specific impact. Estimates of educational attainment shows that the reform has the expected effect: there is a general increase in the share who obtains an education and the difference in this share between individuals born in rural and urban areas diminishes significantly after the reform.

The use of policy reforms is quite useful as a way to identify causal effects. Given that one can argue that the reform provides exogenous variation in the variable of interest, they provide an insight into real world changes. Even when effects of education are heterogeneous, which seems likely, the estimated effect can be given a useful interpretation, using the insights on local average treatment effects from Imbens & Angrist, 1994. They show that 2SLS estimates of a common parameter, when true parameters are heterogeneous, recover a weighted effect over individuals who are affected by the instrumental variables. If a given reform, used as an instrument, is targeting a specific population group, this is certainly a relevant policy parameter. As mentioned earlier, those most likely to be affected by the 1958 reform are mainly low income people in rural areas, particularly girls. The estimated effect is therefore in accordance with findings by Auld & Sidhu (2004), that education has an effect on health mainly for low educated. Although the estimates have been obtained for cohorts who went to school nearly half a century ago, it is certainly still relevant to look at the effect of having completed an education beyond primary schooling, since around 20% of young people living in Denmark today have not completed any education at age 29.

A hurdle model is applied to analyse hospitalization events. This model allows the processes that determine the first hospitalization and the subsequent hospitalizations in a year to be different. The hurdle model is extended in this study so that each part, the binomial and the count hurdle part, is estimated simultaneously with education.

All bivariate estimations indicate that estimates of the effect of education, which assume that education is exogenous, are biased because of the presence of a positive correlation between early determined endowments “selecting” individuals into low and high education groups and health levels. When accounting for this correlation, results suggest that both men and women with an education, as opposed to having none, are less frequently hospitalized. However, once hospitalized, it seems that educated women have more hospitalizations than women with no education, whereas there is no significant difference in the number of hospitalizations for men with and without an education. Since the indicator of being hospitalized are for given rather severe diagnoses, the negative effect of education on hospitalization is interpreted as showing signs of a causal impact of education on health. One possible interpretation of the positive effect of education on number of hospitalizations in a given year for women is that it reflects a higher demand for health care for more educated women. Since further treatments in hospital often will require additional visits to ones doctor (although obviously not if the person dies) this seems to be consistent with some research showing that women are less reluctant to visit their doctor than men with supposedly the same health conditions (e.g. Kruse & Helweg-Larsen, 2004) and that women in general seems to have a better health behaviour. We finally note that if the hurdle model does not entirely disentangle health components from health care demand, the effect of education in the binomial hospital equation may partly reflect health care demand, which means that the effect of education is understated. The positive effect of education on number of stays for women is small though.

The channels through which education may affect health are of course important, but these are not the objective for this paper. Some of the possible pathways were mentioned in the introduction. Therefore to some degree the education effect estimated in this paper may represent indirect factors like wages and job conditions. Note however, that all time-invariant variation in these factors (e.g. effects of persistent income on health and life-time occupation conditions) have been sorted out with the correlation between the individual specific health effect and education.

To examine the robustness of the results, different additional analyses have been conducted. Although the number of explanatory variables has been limited, controls for region of living were included as is common practice. These may however also be endogenous, since people with more resources, and possibly better health, may choose special regions of living. Estimating the model without region of living gives an estimate of the education coefficient in the simultaneous model of -0.075 for women and -0.046 for men, i.e. not that different from estimates reported above. Both are significant.

A second robustness check is conducted by calculation of Wald estimates of the effect of education, using different information from the reform as instrumental variable. First a simple before-after type of instrument is used. Then a before-after estimate for individuals born in rural areas is used, and finally a differences-in-differences estimate is calculated. With U and R denoting urban and rural parish of birth and AR and BR denoting after and before the reform, this is:

$$(7.1) \qquad W_{DD} = \frac{\left[E(H \mid U, AR) - E(H \mid R, AR)\right] - \left[E(H \mid U, BR) - E(H \mid R, BR)\right]}{\left[E(educ \mid U, AR) - E(educ \mid R, AR)\right] - \left[E(educ \mid U, BR) - E(educ \mid R, BR)\right]}$$

We calculate these for the sample as before and for the cohorts 1945-46 only to isolate effects arising from other reasons than the reform. The latter can be viewed as a regression-discontinuity estimator using a one-sided uniform kernel, see Hahn et al. (2001). Because primary schooling only changes annually, it is not possible to use more precise regression-discontinuity estimators as discussed e.g. in Hahn et al. (2001). Table 10 shows the results.

Table 10. Wald estimates of the effect of education on hospitalization.

1943-50	B-A	B-A, rural	D-in-D
Men	-0.17	-0.14	-0.04
Women	-0.07	-0.06	-0.03

1945-46	B-A	B-A, rural	D-in-D
Men	-0.22	-0.11	0.01
Women	0.05	-0.00	-0.03

Note: B-A denotes that a before and after the reform comparison is used as instrument. B-A, rural denotes the same only for those with birth in rural areas and D-D denotes diffs-in-diffs, where the identification strategy used in the bivariate parametric models above is used.

From Table 10 it is seen that the simple before-after instrument provide huge estimates of the effect of education for men, possibly because other differences than those implied by the reform between cohorts born under and after the reform are captured. The effect is a bit smaller if the before-after differences are calculated only for rural areas, and a lot smaller and similar to those obtained with the simultaneous model, when the differences-in-differences strategy is used. An exception is men, when the 1945-46 sample is applied. This might be because the interaction variable between the reform dummy and the parish-of-birth dummy had less explanatory power for men (see table 4). Overall, these results seem to support the findings of the parametric models.

Two possible problems might hamper the validity of the exclusion restriction. The first is that the excluded variable (the interaction between the reform dummy and the born-in-urban-area dummy) is picking up a general increasing trend in schooling, occurring at a different pace in rural and urban areas. However, re-estimates of Probit models of educational attainment, where the reform is superficially supposed to set in for the 1945 cohort or for the 1948 cohort shows, that no large changes in differences in rural-urban educational attainment occurred.

A second problem could be that rural-urban health differences changed between cohorts before and after the reform. It happens that our set-up by coincidence compares individuals born under the Second World War, and individuals born after. Note though, that in order for the exclusion restriction to be invalid, it is needed that a disproportionate abrupt change in living conditions with persistent health effects occurs in rural and urban areas at the end of the war. Such an effect could go through nutritional living conditions. However, on one hand, Denmark did not suffer at great length during the war (at least relatively to other countries, or in terms of casualties), and on the other, those restrictions that the war did put e.g. on food supplies, lasted until the 1950s (Hammerich, 1976). Furthermore, historical evidence does not give any indication that for instance longevity or child mortality changed dramatically after the war (Statistics Denmark, 2000). Looking at the share of babies dying at birth, the number decreases after the war, but in equal amounts in rural and urban areas (Statistics Denmark, 1946; 1949). Childhood diseases did decrease at a faster rate in urban areas than in rural areas, but the rural-urban difference change rather smoothly over time rather than discontinuously and may therefore be picked up by the interaction between the indicator of born in an urban area and the cohort effect.

One may wonder why the reform has a larger effect on women than on men born in urban areas. One interpretation of this result is that before the reform, where young people in rural areas had to travel longer in order to reach schools offering 8^{th} to 10^{th} form, parents to a higher degree paid for their boys to travel to school. Another interpretation is that the selection into the middle school before the reform was biased against girls, and that this bias was larger in rural areas.

It is stressed that the results have consequences both at the individual and the national level. On the one hand our results indicate that there might be a health return to completing an education, but on the other hand, the results also point to the possibility that health inequalities may increase if the distribution of educational attainment widens. It is finally worth noticing that there are large education related differences in hospitalization for several other diagnoses than the five considered in detail here. This may reveal that the risk of low social status operates in far more complex ways than the usual ones suggested by health behaviour and life style.

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Dansk sammenfatning

Tidligere studier af sammenhængen mellem helbred og uddannelse har ikke produceret afgørende resultater til fastlæggelse af den kausale sammenhæng mellem de to. Dette på trods af, at flere studier de seneste par år har haft dette specifikt for øje. Disse har imidlertid produceret usikre resultater på grund af en kombination af et sparsomt data materiale og inefficiente estimationsmetoder. I nærværende projekt forsøges dette udbedret ved anvendelse af et stort administrativt datamateriale samt simultane modeller af hospitalsindlæggelser og uddannelse. En dansk skolereform fra 1958, der ændrede adgangen til uddannelse efter grundskolen, anvendes til at identificere kausale effekter af uddannelse på hospitalsindlæggelser. Mere specifikt udnyttes, at reformen havde forskellig virkning i købstads- og landsbykoler. Vi finder, at reformen øgede uddannelsesniveauet både i land og i

by, men mest på landet. Uddannelse har en negativ og signifikant effekt på sandsynligheden for at blive indlagt på et hospital, mens efterfølgende antal indlæggelser ikke har nogen sammenhæng med uddannelse for mænd og udviser en positive signifikant sammenhæng for kvinder. Robusthed og fortolkning af resultaterne diskuteres afslutningsvist.

Appendix

Table A1. Main diagnoses according to the ICD8 and the ICD10 reference lists.

Diagnosis	**ICD8 (-1993)***	**ICD10 (1994+)***
1. Endocrine disease and nutritional disease	2400-2799	E000-E999
2. Heart disease	4100-4299	I200-I599
3. Disease in other circulatory organs	3900-4099, 4300-4599	I600-I999
4. Disease in respiratory organs	4600-5199	J000-J999
5. Disease in digestive organs	5200-5799	K000-K999

The years indicate when the ICD8 respectively ICD10 are applied in the Danish health care system.

Table A2. Poisson estimates of Hospitalization, 1990-2000.

	Women				Men			
	RE-Poisson		Bivar ML		RE-Poisson		Bivar ML	
Parameters	Estimates	Std.err	Estimates	Std.err	Estimates	Std.err	Estimates	Std.err
constant	-4,316	0,044	-4,170	0,032	-4,654	0,044	-4,223	0,026
Education	-0,215	0,014	-0,195	0,012	-0,234	0,013	-0,183	0,009
Age	0,820	0,012	0,840	0,010	0,762	0,012	0,868	0,009
Large city	-0,486	0,028	-0,288	0,026	-0,354	0,025	-0,195	0,020
Minor city	-0,521	0,029	-0,343	0,026	-0,307	0,026	-0,184	0,020
Rural district	-0,396	0,029	-0,263	0,026	-0,291	0,026	-0,198	0,020
Cohort	0,016	0,008	0,015	0,005	0,027	0,008	0,031	0,004
Born in urban	-0,134	0,026	-0,107	0,020	-0,086	0,024	-0,015	0,015
Reform	0,084	0,036	0,029	0,021	0,023	0,033	-0,006	0,016
Cohort*B_Urban	0,031	0,006	0,045	0,005	0,016	0,006	0,004	0,004
A	2,818	0,026	2,959	0,018	3,150	0,030	3,205	0,015
L	1,365	0,025	1,771	0,021	1,280	0,022	1,852	0,019
τ			-0,055	0,008			-0,018	0,007
Log-likelihood	-0,219		-0,284		-0,239		-0,303	
Observations	341373		341373		350485		350485	

Note: The dependent variable is the number of annual hospitalizations, where either endocrine or nutritional diseases, heart diseases, other circulatory diseases, respiratory or digestive diseases are diagnosed. See also notes to table 5.

Table A3. Binomial Poisson estimates of Hospitalization, 1990-2000.

	Women				Men			
	RE-Bin. Poisson		Bivar ML		RE-Bin. Poisson		Bivar ML	
Parameters	Estimates	Std.err	Estimates	Std.err	Estimates	Std.err	Estimates	Std.err
constant	-3,851	0,048	-3,416	0,050	-3,664	0,043	-3,699	0,049
Education	-0,327	0,018	-0,418	0,032	-0,252	0,017	-0,269	0,029
Age	0,486	0,018	0,469	0,018	0,447	0,016	0,431	0,017
Large city	-0,134	0,035	-0,532	0,042	-0,218	0,032	-0,182	0,041
Minor city	-0,161	0,036	-0,540	0,044	-0,180	0,033	-0,174	0,042
Rural district	-0,072	0,036	-0,467	0,043	-0,168	0,033	-0,191	0,042
Cohort	0,021	0,009	0,026	0,010	-0,008	0,009	-0,019	0,010
Born in urban	0,125	0,033	0,208	0,040	0,045	0,031	0,007	0,038
Reform	0,000	0,039	-0,005	0,040	0,019	0,038	0,015	0,038
Cohort*B_Urban	-0,005	0,008	-0,026	0,010	-0,009	0,008	-0,001	0,010
A	2,028	0,022	2,350	0,019	2,251	0,019	2,554	0,018
L	1,950	0,062	2,342	0,047	2,043	0,044	2,100	0,034
τ			0,044	0,023			0,007	0,016
Log-likelihood	-0,158		-0,219		-0,163		-0,219	
Observations	341373		341373		350485		350485	

Note: The dependent variable is the indicator of hospitalizations, where either endocrine or nutritional diseases, heart diseases, other circulatory diseases, respiratory or digestive diseases are diagnosed, using the binomial part of the Poisson. See also notes to table 5.

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