The Effect of Education on Mortality

Gerard van den Berg*, Lena Janys†, Kaare Christensen‡

September 9, 2015

Abstract

Education and its effect on health and mortality have been studied extensively in the economic literature, as well as in the epidemiological literature. In this paper we examine the effect of education on the mortality of a completely deceased cohort of Danish twins. We exploit an exogenous variation in the level of schooling caused by a large scale schooling reform in the beginning of the 20th century to identify a causal effect of education on mortality. We have a unique data set that includes individual level information on schooling that we collected from paper records from the Danish Ministry of Education, the exact lifetime duration of both twins and important individual characteristics at birth from the Danish twin registry. We estimate a mixed proportional hazard model and control for unobserved heterogeneity by exploiting the statistical dependence in the lifetime duration of twins and model the unobserved effect as a correlated frailty model, where the individual random effect is a combination of an individual effect and a term that is shared between the two twins. We find that education has a protective effect on the longevity of males, but not on females. This result holds whether we use actual schooling or eligibility for the reform as an exogenous proxy for schooling. These results suggest that the channel through which education influences mortality in these old cohorts is not information capital, but rather the choice of less hazardous occupations.

*University of Mannheim (Department of Economics), IFAU Uppsala, Netspar, CEPR, IFS and IZA, corresponding author: gerard@uni-mannheim.de

†University of Mannheim (Department of Economics)

‡University of Southern Denmark (Department of Epidemiology), Danish Twin Registry

1 Introduction and Motivation

The influence of education on outcomes other than wages and employment has been the subject of a vast body of theoretical and empirical work in the past. Especially the influence of education on mortality and other health outcomes, such as hospitalization and chronic diseases, has been the topic of extensive research, since identifying causal effects of education on these non-market outcomes is challenging. In this paper we will contribute to the literature by examining the effect of education on the mortality of a completely deceased cohort of Danish twins that were exposed to a large-scale schooling reform in the early 20th century. We are able to expand the literature by controlling for unobserved heterogeneity in a way that makes efficient use of the twin data and that, as far as we know, has not been used in the literature. We use a high quality data set on Danish twins that contains important information on early life conditions that otherwise could confound the analysis. Unlike other papers that make use of historical data, we do not need to rely on repeated cross sections to generate synthetic cohorts, but can use exact data (including day of birth and death) on lifetime duration and individual data on educational attainment that we collected, digitalized and merged with the Danish twin registry data.

Almost all empirical studies find relatively large positive correlations between education and health outcomes. Different strands of theoretical literature have tried to model this relationship to motivate why we would expect this result.[1] The Grossmann model (which describes investment of schooling into non-market outcomes, such as health) develops the approach that more and/or better information on health hazards of lifestyle choices etc., will influence health behavior as an investment into good health. For example, better information on the hazards of smoking will make it more likely that someone quits smoking, which then in turn will promote longevity (lower risk of dying) and general health. This would manifest itself in fewer and shorter hospital stays, fewer visits to the doctor, less sick leave and so forth. Another possible explanation brought forward is that a higher schooling degree will shift the occupational opportunity set faced by the worker. Occupations for low- and uneducated workers are often hazardous due to physical stress, handling of hazardous substances, poor working conditions and the like. This is especially true for earlier cohorts, that were not subject to labor regulations. Interestingly, this mechanism could at least theoretically also mask potential positive health effects of education. If workers have a comparative advantage in physical occupations due to their physical condition, then there will be a negative selection of workers into further education due to general health. If potentially hazardous occupations require a certain level of physical fitness, then the influence of hazardous working conditions is underestimated. This is known in the epidemiological literature as the "healthy worker effect"(for a review of the effect see for example Ly and Sung (1999)).

There exists a large empirical literature on health and education with evidence collected in many industrialized countries, that can be roughly sorted into three main categories:[2] the first strand of literature

[1] For a complete overview over the theoretical foundations of education and non-market outcomes, see Grossman (2006).

[2] Naturally, many papers examine more than one outcome so that some papers fall into two or more categories

attempts to link education and mortality directly. This is related to a larger literature on how events that occur very early in life might be related to outcomes very late in life such as van den Berg et al. (2009a). The second strand of literature attempts to link education and other health outcomes, such as hospitalization and chronic diseases. The third strand of this literature tries to uncover possible *channels* through which education might influence health outcomes by examining health behaviors, such as smoking and obesity.

General Identification Issues There are two main difficulties associated with identifying the causal effects of education on health outcomes: firstly, there could be third factors causing simultaneously better health, better health behavior and a higher level of schooling. Secondly, there is potentially reverse causality, meaning that good health enables further schooling. Perri et al. (1984) and Currie and Hyson (1999) have examined this and find that poor health generally leads to less education.

Empirical papers have tried to deal with these identification issues in mainly two ways: using instrumental variables (IV) or regression discontinuities, exploiting some exogenous variation in the level of schooling, and twin or sibling study designs. While using twins to control for unobserved factors influencing both health and mortality can remove much of the unobserved factors, these studies fail to eliminate reverse causality. Studies using an instrumental variables approach can potentially control for both these factors, but there is much discussion about possible selection issues in instruments without perfect compliance (without perfect compliance the local average treatment effect will be larger than the average treatment effect, since the individuals that have the most to gain from a policy will comply). Twin and sibling study designs using fixed effects, on the other hand, rely on the assumption that any difference in schooling between twins or siblings is of a random nature (for example having a particularly motivating teacher). Whether this assumption is justified or whether selection issues play a greater role is difficult to assess.

One difficulty with using educational reforms as an instrument (or regression discontinuity) for educational attainment to assess health outcomes is that very often several social reforms were conducted at roughly the same time. One of the most expansionary reforms of the UK, for example, was conducted almost at the same time as the National Health Service (NHS) was introduced, which would increase the magnitude of the estimated effects of education. Another concern is that some reforms might pose an independent effect on health: abolishing child labor laws for example might have a strong effect on mortality which is independent of the education effect.

A common argument against using instruments is that they estimate a LATE parameter that potentially differs substantially from the average treatment effect, especially in reforms that were small of scale. This is because only those parts of the population comply that have a lot to gain from compliance. While this is potentially difficult to test for, a study by Oreopoulos (2006) compares educational reforms of different scope and concludes that the average treatment effect and the local average treatment effect do not differ much in size, whether the reform affects small or large parts of the population.

Furthermore, it is not always intuitively clear in which direction education influences health and mortality. It is generally assumed that the effect of education is positive, based on OLS estimates and raw correlations. While there are theoretically good reasons to believe that this is true, we can also suspect the opposite effect. Children who are "forced"into schooling (through changes in compulsory schooling laws) cannot contribute to the family income anymore. This might lead to, among other things, malnourishment in critical stages of childhood development, which can have adverse effects on later health and mortality.[3] This could be especially relevant for children in developing countries where compulsory schooling laws are changed without compensating families for the loss in income.

Measures of health other than mortality sometimes suffer from confounding factors that might bias coefficient estimates to zero. Hospital attendance or the number of physician visits might be positively related to education because more educated people are more aware of potentially serious symptoms, which may still result in a decreased hazard (if other medical conditions are caught earlier). Higher hospitalization rates or more physician visits might then only reflect a higher incidence of diagnosis as opposed to being an indicator of poor health. In using these measures it is probably very important to have a large enough longitudinal data set to control for the incidence of diagnosis and mortality and morbidity.

In this paper the health outcome that is used is mortality. Mortality is arguably the most objective measure of health, as it does not rely on self-reporting which is often biased and is often heterogenous across different population subgroups.[4] Mortality has the added advantage that it measures the "net effect"of education, instead of focusing on a few indicators of better health : Johnson et al. (2010) for example find that while smoking behavior is negatively related to education level, alcohol consumption is the highest among highly educated people. In order to identify causal effects, we combine two techniques used in the literature: we control for unobserved heterogeneity that is correlated within twin pairs (thereby making efficient use of the twin data without assuming that unobserved factors are equal between twins) and additionally utilize an education reform in Denmark in 1903 to account for possible within twin pair selection effects.

Literature Review We will briefly outline and discuss the existing empirical literature on the effects of education on mortality, of education on other health outcomes and of education on health behavior.[5]

Lleras-Muney (2005) uses a compulsory schooling reform and changes in child labor laws in different states in the US for identification. She finds that the IV estimates, despite good instruments (since she

[3]For example show van den Berg et al. (2009b) that there are critical stages in child development which significantly influences adult height

[4]Hernández-Quevedo et al. (2005) find that when asked to report their health status less education people are less consistent in their answers.

[5]We will generally use education and schooling interchangeably, although what we want to measure is education (i.e. how much someone has learned), while what we usually measure (and what external wwvariations are usually used for identification) is schooling, i.e. the time spent in school, not the knowledge that results from an educational process.

lacks individual education data she uses group data at the first stage), are not statistically different from the OLS estimates. This seems to suggest that the endogeneity problem is not very important. Albouy and Lequien (2009) use changes in French compulsory education to assess the effects on mortality. They use longitudinal data to compute survival rates at any given age and use a nonparametric regression discontinuity approach for identification. The data is significantly right censored due to unavailability of older cohort data. The authors find no causal relationship between mortality and education, even though there was a significant increase in compulsory education. Madsen et al. (2010) investigate the impact of education on mortality on a more recent cohort of Danish twins, using a twin difference study design. Their findings indicate that the effect of schooling was strongest for the older cohorts of males.

In a recent paper Behrman et al. (2010) study whether schooling has a causal impact on mortality and hospitalization. The authors use the Danish twin registry data together with register data on hospitalization records. The outcome measure for mortality is being alive in 2003 and they use a twin difference study design. There was also significant censoring due to the fact that a large fraction of the investigated cohorts was still alive in 2003. The authors find that schooling reduces the probability of being dead in 2003 when using OLS, but fail to find significant effects once only twin differences are considered. They find the same result when the number of hospital days per year is used as a measure for other health outcomes. The OLS estimates suggest that schooling decreases the number of days spent in a hospital, while the twin difference estimates are again not significant. Clark and Royer (2009) use a 1944 UK school reform, raising school leaving age from 14 to 15, in order to investigate the effect of schooling on mortality and some self-reported measures of health. The study uses a fuzzy regression discontinuity design, because individual-level data on education is not available and compliance was not perfect (about 40% of non-eligible pupils chose to remain in school for one more year and not all eligible pupils complied). The outcome measures are mortality and self-reported measures of health. The authors cannot find a causal relationship between mortality and schooling, even though the point estimates using IV suggest that education reduces the probability of reporting fair or bad health, although the IV estimates are usually not significant. The same holds for self-reported smoking, BMI and high blood pressure. The authors also examine different channels of how education might influence health outcomes. They test for functional literacy and they do not find evidence supporting the human capital model of health and education. Also, their results do not support the hypothesis that education influences health via earnings.

Lundborg (2008) uses US twin data to assess the health returns of education. The outcome measures are self-reported health and chronic diseases. He exploits within pair differences in education for identification. He finds a causal impact of years of education on self-reported health and fewer chronic diseases. Arendt (2005) uses a random sample of Danish workers and follows their self-reported health outcomes and health behavior in a panel setting. He uses a quantal response panel model and a linear regression model as the two-stage least squares estimator and exploits two different school reforms for identification. The author finds that the point estimates of the two-stage least squares estimator show a larger effect of education on health outcomes than for the simple OLS estimator, but they are not significant (the standard errors also increase). Kemptner et al. (2011) use several school reforms in Germany as an exogenous variation in the level of schooling and find that schooling significantly reduces long-term illnesses in men, but not in women. Arkes (2003) uses data from the US 1990 Census of Population and Housing and finds that high unemployment rates during teenage years lead to higher educational attainment because the opportunity costs of education decrease. More schooling lowers the probability of suffering from a work-limiting health condition later on.

Silles (2009) conducts split sample regressions on different outcome measures of general health for two different age groups. For the older group, the IV estimates for these outcomes are larger than the OLS estimates. For the younger group, the point estimates are larger than for the older group, but the results are largely insignificant. She also points to a bigger problem with this particular UK data set: in 1948 the national health service (NHS) was introduced. Adams (2002) uses a survey of adults aged 51-61 to examine the link of educational attainment and several measures of self-reported health. IV estimation is performed using quarter of birth and a set of parental and sibling characteristics as instruments for educational attainment. The OLS estimates are positive for women and significant and positive but not significant for men. The IV estimates indicate that there might be a causal impact on health, even though the estimates are not significant, which could be due to weak instruments.

To examine different channels through which education and health might be linked, Webbink et al. (2010) examine a longitudinal sample of identical (monozygotic) twins to account for differences in BMI, or more precisely the probability of being overweight. Cross-sectional evidence shows a negative relationship between education and being overweight. Within pair differences are different for men and women. For men, education reduces the probability of being overweight also within pairs of identical twins and the estimated effect of education on overweight status increases with age. For women, the authors find no negative effect of education on weight, when family fixed effects are taken into account. JR2 (2010) exploit a German reform where secondary school fees were abolished to identify effects of higher education on different health behaviors. They find strong associations between schooling and health behavior when using OLS, but the instrumental variables estimations are insignificant and they conclude that there is no causal link.

In a different paper JRS (2009) use changes in compulsory schooling laws that were implemented step by step in the different states in Germany, creating an exogenous source of variation. In this paper they find strong effects of education on delayed starting ages of smoking, but no causal effect on obesity. Cowell (2006) develops a theoretical model on health behavior and education. He considers different causal pathways between education and better health behavior. In the empirical application smoking and binge drinking are modeled as a binary outcome. The author uses the discrete factor method to account for unobserved heterogeneity. The claim is that this controls for any unobserved factors that cause both educational attainment and health behavior. College tuition and school expenditures which differed by state are used as instruments. Controlling for unobserved heterogeneity reduces coefficient estimates and significance, but the main results that education significantly reduces the likelihood to smoke and to engage in binge drinking persist even when IV estimates are considered. Degree effects are also found to be important: the highest reduction in the probability to smoke was obtained for people with post-graduate degrees. This result suggests, that if returns to schooling are nonlinear (i.e. degree effects matter) then using years of schooling might bias estimated coefficients toward zero.

Fujiwara and Kawachi (2009) conduct a twin fixed-effect study using the National Survey of Midlife Development in the United States. They monitor a wide range of different health indicators and behaviors. While for monozygotic twins' perceived health is better with more education, even in the fixed effects analysis, for all other outcomes that are examined (being overweight, number of depressive symptoms) the point estimates of the fixed effects regression are very small.

Fletcher and Lehrer (2009) examine whether there is a reverse causality of better health leading to higher educational degrees, by exploiting genetic differences among siblings. The authors use a combination of genetic markers and family fixed effects as instruments for health problems in childhood, and find that attention deficit orders have a significant negative effect on educational attainment. Childhood obesity also reduces educational attainment, although the effects are much smaller. Whether this result holds also for other diseases that are less directly related to schooling itself is not clear.

Overall the surveyed evidence indicates mixed results on whether there is a causal link running from education to health outcomes and health behavior. Most papers studying health outcomes find at least small causal effects of education. Generally, the evidence seems to suggest that the effects are more pronounced for men than for women. The results on health behavior are less clear, although the effects also seem to be heterogeneous for males and females. The available evidence on the mechanisms behind education and health outcomes seems not to back the human capital model by Grossman and also dismisses the earnings channel.

The remainder of the paper is structured as follows: In section (2) we will describe the institutional details of the educational reform that took place in Denmark in 1903. In section (3) we describe the data used in this study. In particular we describe the twin data in section (3.1) and in section (3.2) we describe our method of collecting and matching the individual education data to the twin registry data on mortality in detail. In section (4) we describe the empirical strategy, including the correlated gamma frailty model that is used to control for unobserved heterogeneity. In section (5) we report all results of the estimations on the full sample and separately for male and female twins. Section (6) concludes.

2 The Reform

We have an exogenous source of variation in the form of an extensive school reform that was introduced in 1903 in a law called the law for higher general schools (lov om højere almeneskoler). The law was passed in 1903. The purpose of the reform was to provide pupils with a “bridge”between elementary school[6] and further education. As the Ministry of Education[7] describes it: “Before 1903 there was no coherent connection between elementary school and further education”.[8]

The relevant section from the law is:

> I tilslutning til Folkeskolens Undervisning for Børn i 11-12 Aars Alderen meddeles den højere Almenundervisning først i Mellemskolen, siden i Gymnasiet. Mellemskolen er en Barneskole, som gennem 4 eetaarige Klasser meddeler sine Elever en for det givne Alderstrin egnet højere Almenundervisning, der føres frem til en passende Afslutning. Mellemskolens Undervisning kan fortsættes efter den afsluttende Aarsprøve gennem en Realklasse (Realafdeling)

Which translates to:

> In connection with the elementary school instruction for children at the ages 11-12, the higher general education first in the Middle School (Mellemskole), and subsequently in the Gymnasium is announced. The middle school is a children’s school that through 4 year classes communicates to their pupils appropriate higher general education, that leads to the appropriate termination . The middle school education can, after passing the concluding yearly examination, be continued in the “Realklasse”.

The reform obligated elementary schools to extend grade levels beyond sixth grade and to introduce a middle school. They were allowed to do so step by step, introducing 7th grade first and then continue upwards adding a grade level each year. The first year where the “Studentereksamen”(High School Exam) was taken under the new regime was therefore in 1910 (specified in the law). Since pupils had to be at least 17 years old to be allowed to take this exam, we can conclude that the first cohort affected by the reform were born in 1893. The reform was not compulsory, however it build a bridge for pupils where parents did not have the funds to finance private education (in boarding schools and private courses). There is reason to suspect that the reform was not carried out in equal parts over the country. The Ministry of Education noted in their report that “it was especially city/town children that had the opportunity to follow the way through the middle school and further to other qualifications, because middle schools were almost exclusively established in cities and towns”.[9] School starting age in Denmark was and still is determined by the year the pupil turns seven, so there are no midyear cut-off points. Therefore, calendar year is an appropriate eligibility criterion. In theory pupils could delay school entry, so compliance was not perfect, but a general provision in the law states that a pupil’s age should not exceed average age in

[6] “almueskole”or folkeskole

[7] Undervisningsministeriet

[8] ”Før 1903 var der ikke nogen sammenhængende forbindelse mellem almueskolen og den lærde skole.“(Danish Ministry of Education)

[9] Det var især bybeboernene, som fik mulighed for at slippe igennem enhedsskolen og videre i systemet, fordi mellemskolerne næsten udelukkende blev placeret i hovedstaden, i købstæder og i stationsbyer. (Danish Ministry of Education)

the grade by more than two years. Tabulating the pupil's birth years with the year they took the exam shows that pupils generally complied with the grade they were assigned to and that there was no unusual spike in older pupils that took their exam later in order to be eligible for the reform. It is important to notice that the reform did not automatically lead to an exam (as is our outcome for education). This means that some pupils have been enrolled in school longer than 6th grade, but dropped out without an exam. There is some evidence of attrition: just between the grades 3 and 4 of the middle school, the number of enrolled pupils in grade 3 in 1912 dropped from 5401 to 4500 in grade 4 in 1913 (see Statistik (1914)). This means that our coefficient estimates will be downward biased, and that we will possibly find an intent to treat effect on the general population that did not take an exam. Finally, the reform also introduced many improvements in the formal education system for girls, and the uptake was generally good, also among females, which is also reflected in the aggregate data. In section (7) we present some descriptive statistics for the aggregate sample of all pupils and for the twin registry data. For example, the number of girls taking the "Studentereksamen", rose by 100 percent from 1909 to 1910 (see Table (6) for aggregate statistics of the entire population on passed high school examinations and see also Statistics Denmark publication on degree-awarding schools from 1912 ((Danmarks Statistik (1914)) 1912) for more details.

The limitations of this reform are obviously that we cannot estimate an average treatment effect (ATE), since there was no perfect compliance. When estimating the reduced form model we instead estimate an average of the effect on those pupils who complied and the zero effect on the never-takers. Even though the relative increases in schooling uptake were large (see 7), the absolute share of pupils who graduated with a degree was very small and remained so, even after the reform. Who were the compliers: As we explain above, those most affected by the reform were those living in cities and towns and not pupils in rural areas. Furthermore, we can assume that the pupils who remained in school came from a relatively wealthy background, who had a comparative advantage in learning and/or whose opportunity costs of staying in school were relatively low in comparison to the expected payoff and who did not face liquidity constraints. There were also entry requirements for the middle school, which further restricted access to a higher education. Since we assume that the compliers have a comparative advantage in education, the academic restrictions are not that important, although they certainly exacarbate the problem. These are all factors that have to be taken into account when interpreting the results of our estimations. However, Oreopoulos (2006) compares schooling reforms of different scope and finds that the ATE and the LATE do not differ very much.

3 Data

3.1 Twin Data

The Danish twin registry contains detailed information on mortality (including exact day of birth and death and the cause of death) and information on some initial conditions at birth which include the general area (Jutland, Funen, Sealand and Copenhagen), the degree of urbanization (rural, town or Copenhagen) and the zygosity (i.e. identical twins (monozygotic) or not (dizygotic)) of the twins. For a detailed overview over the entire data set and data collection methods, see Skytthe et al. (2002). We restrict our sample to twins who were born 5 years before and 5 years after the cut-off for eligibility for the reform. This means that we restrict the sample to twins born in the years 1888-1897. The first birth cohort that was formally eligible for the reform was born in 1893. This comprises 5,695 individuals that survived at least until 1943 (the first year that reliable mortality records are available). If we only keep observations with status information on both twins, then we have mortality records for 2559 twin pairs. A survey was conducted in 1966 which added more detailed information on educational status, family background, occupational history and other socioeconomic information to the twin registry data. Unfortunately, if we had to rely on information from this survey, we would severely limit the number of observations we could use. We would have to limit our sample to individuals who lived at least until 1966. This is a serious constraint, since the first cohort we want to consider in our analysis was born in 1888. In section (3.2) we describe in detail how we instead collected the individual-level educational data and how we merged them with the data from the Danish twin registry.

Table (1) shows the proportion of twins that we have reliable status information on. Reliable status information refers to whether we the exact time of death or the year of emigration. There are some unknowns in the sample, even though there are few of them. Most efforts to uncover complete information were made for monozygotic twins, the share of twins that are reported to be dead is 95.63%, with 3.81% emigrated. The second priority in uncovering mortality information were same-sex twins in general. The least effort was made for different-sex twins, where accordingly the share of twins with an unknown status is the largest (36.37%). But there are still ways that we can utilize the information that we have on these twins: we can censor most of the observations that are reported unknown, since in 1943 the Danish Mortality Register started recording deaths systematically. All deaths that occurred before that were not subject to systematic recording. For monozygotic twins, more efforts were made to uncover time of death even before that date. Observations where the exit status is recorded as emigrated we censor at the date of emigration[10]. Tables (2), (4) and (3) show some descriptive information about the twin data set.

3.2 Education Data

Because the relevant birth cohorts used in this study are comparatively old, the survey information on the educational status is scarce. However, the archive of the Danish Ministry of Education (Formerly the Danish Ministry of Education and the Church) contains paper records of all pupils who completed a schooling degree for each year from 1848 until the records were digitalized. These schooling records, called "Meddelelser om de hoejre Almeneskoler", are very thorough and contain information on the pupils' complete name, date of birth, place of birth, their final grade and the subjects that they graduated in. We digitalized these paper records by scanning the pages individually and then extracted the names and

[10] We actually only know the year of emigration, so the date of emigration was then set as July 1st in that year.

birthdays with a text recognition program[11], that is able to recognize textual information from a scanned document. Appendix B shows one scanned example page and the same page after it was treated with the text recognition program. After extracting the data, we were then able to merge these digitalized names with the information from the twin registry. We used a data base program and pre-sorted on last name (including common spelling variations such as Christensen and Kristensen) and birth year and then matched twins that we found by hand (by comparing the exact day of birth and in cases of doubt the place of birth). This very laborious procedure ensures, that the twins that we were not able to match, did not receive a schooling degree. The reported schooling degrees are High School Exam ("Studentereksamen"), which also was compulsory to proceed to university, an intermediate exam after around 10 or 11 years of school ("Almindelig Forberedelseseksamen" and "Realeksamen" which slowly replaced the AF), "Fjerde Klasse Hovedeksamen" (which was the predecessor of the Almindelig Forberedelseseksamen) and the girl's school exam "Pigeskoleeksamen". However, in the analysis we treat schooling as a binary variable, i.e. we code schooling as having an educational degree ([eks==1]) or not ([eks==0]). The reason why we do not exploit the additional information of the different schooling degrees is: firstly, because of the number of observations and secondly, because there were different age restrictions on girls and boys for the same exam and therefore imputing years of schooling from the degree is difficult.[12]

4 The Empirical Model

Since we have the exact date of birth and the exact date of death, we have continuous information on lifetime duration so that we treat lifetime as a continuous random variable. Using a duration model instead of OLS estimates or binary estimators has the advantage that we can include censored observations.
As a starting point we model the individual hazard rate as a simple mixed proportional hazard model. The individual hazard rate is a function of individual characteristics at birth x and a dummy variable that indicates a schooling degree S. The parameter of interest therefore is η.

$$log\theta(t|S,x) = h(t) + \beta' x + \eta' S \qquad (1)$$

$h(t)$ is also called the baseline hazard. In our estimations we will always assume a Gompertz function as the baseline hazard:

$$h(t) = Bt \qquad (2)$$

Alternatively, we can estimate an "intent to treat" effect on the entire population of twins. As described earlier, the effect of the reform was widespread in the sense that more people had access to schooling

[11] The Abbyy Finereader

[12] The age restriction for girls to take the Realeksamen is 17, for boys it is 15.

beyond sixth grade, but this did not automatically lead to a degree. Therefore, we might assume that there was an effect of the reform through further education on mortality. R is a dummy variable indicating whether or not a person was born in a year which made them eligible for the reform.[13] Equation (3) therefore measures the reduced form equation of education on lifetime duration using eligibility for the reform as an exogenous variation on the educational level.

$$log\theta(t|R,S,x) = h(t) + \beta' x + \omega' R \tag{3}$$

The covariates are region of birth (Jutland, Funen, Sealand except for Copenhagen and Copenhagen), degree of urbanization (Copenhagen, Town and Rural), and a term ($\log(1888 - birthyear)$) capturing the long run effects of the birth year, as well as sex and zygosity.

Unobserved Heterogeneity Frailties are an extension of the proportional hazard model that aim to capture effects of unobserved covariates on the baseline hazard. In principle, it is a random effect duration model, where the random effect (or the frailty) has a multiplicative effect on the baseline hazard function.[14] In order to introduce unobserved heterogeneity, we need to make an assumption about the functional form of the baseline hazard $h(t)$. In accordance with the literature on duration, we a assume a Gompertz distribution for $h(t)$ and then assume that the parameter V captures the unobserved heterogeneity. The individual hazard rate then becomes:

$$log\theta(t|S,x,V) = h(t) + \beta' x + \eta' S + \log V \tag{4}$$

Above we assumed that the unobserved heterogeneity terms of twins indexed $1,2$ are statistically independent (i.e. Cov$(V_1, V_2) \neq 0$). It is reasonable to assume that the unobserved heterogeneity is correlated within twin pairs, such that the individual heterogeneity consists of a shared part W and an individual part V_i^0

$$V_i = W + V_i^0 \tag{5}$$

where $i = 1,2$

[13]Eligible for the real reform were all persons born in and after 1893

[14]For an introduction into frailty modeling, see Wienke (2003).

We assume that the joint distribution of V_1, V_2 follows a bivariate gamma distribution. The marginal distributions are identical and the mean is included in the constant term of $\beta' x$. The joint distribution has two parameters: the variance σ of V_i and the correlation ρ of V_1, V_2. The correlation is defined as the fraction of the variance of V_1, V_2 that is explained by the variance of the shared term W:

$$\rho = \frac{var(W)}{var(V_i)} \tag{6}$$

There are two extremes of this assumption: the first one is that $\rho = 0$, i.e. unobserved heterogeneity terms of twins are uncorrelated with each other. This is very likely false, as twins share not only the same (or in the case of dizygotic twins similar) genetic make up, but usually also similar living conditions in childhood. In the other extreme, V_1, V_2 are perfectly correlated ($\rho = 1$ or $V_1 = V_2$), which is also likely to be false. This means that there is still an individual unobserved component if we control for similar/equal genetic make up and living conditions in childhood. While assuming that $\rho = 0$ underestimates standard errors of the coefficients, assuming that $\rho = 1$ will overestimate the standard errors and therefore underestimate statistical significance of coefficients (if $0 < \rho < 1$ is the true model).

The closed form solution for the shared frailty model ($\rho = 1$) is presented in Gutierrez (2002). The derivation of the bivariate likelihood function for the correlated frailty model is shown in Yashin et al. (1995). In Appendix A we note explicitly all parts of the likelihood function and the log-likelihood function that is later used for estimation.

When estimating the correlated frailty model we explicitly estimate two parameters: ρ and σ. ρ denotes the correlation of the two frailties and σ is the variance parameter of the individual frailties. Since we assume that the marginal distributions of the random effects are the same for both twins, we can write that $\sigma_1 = \sigma_2 = \sigma$.

The bivariate density function used for estimation is given by:

$$\begin{aligned} f(x_1, x_2) \quad = \quad & \frac{(1-\rho)^2 S(x_1)^{-\rho} S(x_2)^{-\rho} f(x_1) f(x_2)}{(S(x_1)^{-\sigma} + S(x_1)^{-\sigma} - 1)^{\rho/\sigma}} + \frac{(1-\rho)\rho S(x_1)^{-\rho} S(x_2)^{-\rho-\sigma} f(x_1) f(x_2)}{(S(x_1)^{-\sigma} + S(x_2)^{-\sigma} - 1)^{(\rho/\sigma)+1}} \\ & + \frac{(1-\rho)\rho S(x_1)^{-\rho-\sigma} S(x_2)^{-\rho} f(x_1) f(x_2)}{(S(x_1)^{-\sigma} + S(x_2)^{-\sigma} - 1)^{(\rho/\sigma)+1}} + \frac{\rho(\rho+\sigma) S(x_1)^{-\rho-\sigma} S(x_2)^{-\rho-\sigma} f(x_1) f(x_2)}{(S(x_1)^{-\sigma} + S(x_2)^{-\sigma} - 1)^{(\rho/\sigma)+2}} \end{aligned} \tag{7}$$

The likelihood contributions for censored or partly censored pairs are given in the Appendix A.

where $S(x_i)$ is the survival function of the univariate frailty model and $f(x_i)$ is the univariate density function of the univariate frailty model.

5 Results and Discussion

- Gender and being born in Copenhagen and in a town as opposed to the countryside increase mortality risk.
- Exam coefficient is positive (meaning having a schooling degree would raise mortality risk) but is completely insignificant.
- Having a schooling degree in Copenhagen significantly reduces mortality risk. The magnitude of the effect is about twice as large as the effect of gender or being born in Copenhagen.
- While the coefficient for men having a schooling degree is negative, it also insignificant.
- These results are mirrored when eligibility for the reform is used instead of having a schooling degree, although the coefficient for having a schooling degree while being born in Copenhagen is much lower and measured less precisely.
- Standard errors of the correlated frailty lie between the univariate and the shared model.
- The correlation ρ_{mz} is 0.97, which means that for mz twins the shared model holds fairly well, while ρ_{dz} is 0.70, which is in line with previous estimates for the correlation of the unobserved heterogeneity for dz twins (see Gerard and Ina paper) and justifies the use of the correlated frailty model.

In Table (**??**) we report the results for the baseline model with unobserved heterogeneity that is statistically independent (which we call the univariate frailty case, column (1)) and the model where the unobserved heterogeneity term is shared between twins, i.e. they have the same V (the shared frailty model, column (2)).[15] Finally, column (3) reports the results of the correlated frailty model, where the individual frailty terms are allowed to be correlated within twin pairs, but are not constrained to be the same as in the shared frailty case. We programmed the correlated frailty model with the matrix language Mata.[16]

All estimations are performed assuming a Gompertz function as the baseline hazard and using the Newton-Raphson algorithm for iteration.[17]

[15] Which is a command implemented in Stata and other statistical software packages and frequently used in the literature.

[16] We also attempted to estimate a fixed effect model using stratified partial likelihood, since there could be a concern that the schooling decision is correlated with the unobserved random effect. However, the model does not converge, probably owing to the fact that the only covariate that differs within twin pairs is the schooling degree and even that is highly correlated: there are only 28 twin pairs that have different schooling outcomes. Notably this is only a problem where we attempt to estimate the effect of actual schooling on lifetime duration, since the year of birth (and therefore eligibility for the reform) is exogenous.

[17] We also performed some estimations with the Broyden-Fletcher-Goldfarb-Shanno (bfgs) method which yields the same results.

Table (**??**) shows the estimation results for the full sample of same-sex twins. Having a schooling degree is included as a dummy variable. The point estimates for schooling are negative in all three models, although they are small and not significant. In contrast to the shared frailty model, the correlated frailty model has lower point estimates and the standard errors lie in between those of the univariate frailty model, where we ignore statistical dependencies between the twins and the shared frailty model where the unobserved heterogeneity is the same for both twins. This is what we expected, since if the correlated frailty model is the true model, the univariate frailty model will underestimate the standard errors, while the shared frailty model will overestimate them. The other coefficient estimates are generally in line with previous research: being male significantly increases the mortality hazard, the same is true for being born in Copenhagen and in a town (the baseline is rural). We also observe the well documented time trend in mortality, i.e. that being born in a later cohort reduces the mortality risk, though the estimates are not significant which is possibly due to the small number of years that are considered in this analysis.

In Table (**??**) and Table (**??**) we show the same estimations, performed separately for male and female twin pairs. The coefficients for having a schooling degree are large and borderline significant and negative for males (the correlated frailty model has a p-value of p=0.11), suggesting that for males education has a protective effect. For females the coefficients are negative, but the point estimates are much smaller and insignificant.

One of the possible explanations for this apparent zero result for women is that the sample size of women who take a schooling degree is too small. This is certainly a valid concern. We have tried to evaluate this problem by simulating a larger data set, using the original structure and results from our data, but increasing the number of observations by expanding the resulting data. Results are reported in Table (**??**). Even if we quadruple the number of observations, we cannot find any significant effect of having a schooling degree on the hazard rate. On the other hand, this strong difference between males and females potentially serves to uncover more about the way that education may delay mortality. There are not many channels where we should expect large gender differences: the "information channel" where individuals can make better informed choices about their health situation should work in the same direction, at least there seems to be no reason why the effects on men would be so much more pronounced. Another possibility is that (at least at that time) education enabled men to make different occupational choices, shifting from physically demanding work to white-collar work, which can have large effects on life expectancy. The employment participation of women during that time was very low and the single highest mortality risk factor for women in childbearing age was giving birth. Even in Sweden, which was comparable to Denmark in having some of the lowest maternal death rates in the world at that time, there were still about 250 maternal deaths per 100000 live births in the beginning of the 20th century (see Loudon (2000)). A surprising finding of Loudon (2000) is that the risk of maternal death was actually found to be inverse to social class, meaning that obtaining a higher social class through education might even have increased the risk of dying in child birth. Furthermore, van den Berg et al. (2008) examine fertility and early life conditions as factors for mortality in a cohort of Dutch women in the end of the 19th century, and find that education delayed the time of first fertility even in these earlier cohorts and that age at first fertility was significantly positively related to the mortality hazard. They also show that maternal mortality was the leading cause of death and responsible for 5.4% and 10.1% of all deaths among women aged 20-49.[18] It is also reasonable to assume, that while death is an extreme outcome, there are other complications (such as fistulas and infections) that can increase mortality and morbidity years after the last birth.

To examine the differences in the impact of education on mortality more closely, we use the reform that was described in section (2) above as an exogenous source of variation in the level of education. Table (**??**) shows the results when we use being eligible for the reform as a proxy for educational attainment for the full sample and Table (**??**) and Table (**??**) report the results for males and females separately. The coefficient estimates for males are large and significant, while the point estimates for females are much smaller in magnitude and insignificant. This seems to corroborate the notion that it is not the small sample size that is masking aggregate treatment effects, but that there was only a protective effect of education for males and not for females, as there was a marked increase in females who prolonged schooling due to the reform (see section (2)) and our interpretation of the above results regarding the channel through which education affected mortality outlined above. The other coefficients can also give us more insight into who the reform affected. For males, being born in Copenhagen or in a town (as opposed to being born in a rural environment) carries a penalty in terms of longevity. The interaction effect of the reform with the town and Copenhagen dummy is large and negative and is in line with the observation that exposure to the reform was concentrated in urban areas, see section 2. Interestingly, the results for women are also significant but *positive*, which further confirms that schooling had heterogenous effects on mortality across genders. In terms of economic significance the results indicate that, at least for men, longer schooling is quite significant in promoting longevity and comparable in size to being born during favorable economic conditions (i.e. in a boom as opposed to a recession), see van den Berg et al. (2008) and slightly smaller than the benefits of being married (which only prolongs longevity for males and not for females).

6 Conclusion

In this paper we examined the effects of education on longevity in a unique high-quality data set of completely deceased cohorts of Danish twins. We used individual level information on schooling and were able to control for unobserved heterogeneity using a correlated gamma frailty model, where the unobserved effects are correlated within twin pairs. Additionally, we were able to exploit an exogenous form of variation in the degree of schooling in the form of a schooling reform which took place in 1903 and which extended the (voluntary) public elementary school education by four years. We found that education has a protective effect on the longevity of males, but not on the longevity of females. This result holds whether

[18] In contrast, today birth complications are not even among the 10 leading causes of death among women in that age bracket, according to Statistics Denmark.

we use actual schooling or eligibility for the reform as an exogenous proxy for schooling and in both cases controlling for unobserved heterogeneity. The reasons for this result cannot be comprehensively explained using our data, but it at least suggests that the channel through which education acts on longevity is heterogeneous across genders in some populations. Since the implementation and compliance with the reform was more widespread in urban areas than in rural Denmark, we interact the eligibility for the reform with the degree of urbanization at birth and find that for males being born in a urban environment increases mortality hazard, but the interaction effect with the reform is large and negative, suggesting that the effect of the reform was larger in urban areas (due to differences in implementation) and therefore urban pupils were more likely to comply. Furthermore, we find that for women the opposite effect holds, meaning that the effect of the reform was also larger for women in urban areas, but the coefficient sign was positive, indicating that women did not benefit from schooling in terms of a reduced mortality hazard in the same way males did.

References

Does schooling affect health behavior? evidence from the educational expansion in western germany. IZA Discussion Papers 4330, Institute for the Study of Labor (IZA), 2009.

Secondary school fees and the causal effect of schooling on health behavior. *Health Economics*, 19(8), 2010.

Scott J Adams. Educational attainment and health: Evidence from a sample of older adults. *Education Economics*, 10.1:97–109, 2002.

Valerie Albouy and Laurent Lequien. Does compulsory education lower mortality? *Journal of Health Economics*, 28(1):155 – 168, 2009. ISSN 0167-6296.

Jacob Nielsen Arendt. Does education cause better health? a panel data analysis using school reforms for identification. *Economics of Education Review*, 24(2):149 – 160, 2005. ISSN 0272-7757.

Jeremy Arkes. Does schooling improve adult health? *RAND Working Paper*, DRU-3051, 2003.

Jere R. Behrman, Hans-Peter Kohler, Vibeke Myrup Jensen, Dorthe Pedersen, Inge Petersen, Paul Bingley, and Kaare Christensen. Does more schooling reduce hospitalization and delay mortality? *Demography (forthcoming)*, 2010.

Damon Clark and Heather Royer. The effect of education on adult mortality and health: Evidence from britain. *submitted*, 2009.

Alexander J. Cowell. The relationship between education and health behavior: some empirical evidence. *Health Economics*, 15(2):125–146, 2006.

Janet Currie and Rosemary Hyson. Is the impact of health shocks cushioned by socioeconomic status? the case of low birthweight. *American Economic Review*, 89(2):245–250, May 1999.

Undervisningsministeriet Danish Ministry of Education. Undervisningsministeriet: Et link til historien.

Jason M. Fletcher and Steven F. Lehrer. The effects of adolescent health on educational outcomes: Causal evidence using genetic lotteries between siblings. *Forum for Health Economics & Policy*, 12(2), 2009.

Takeo Fujiwara and Ichiro Kawachi. Is education causally related to better health? a twin fixed-effect study in the usa. *International Journal of Epidemiology*, 38 (5):1310–1322, 2009.

Michael Grossman. Chapter 10 education and nonmarket outcomes. In E. Hanushek and F. Welch, editors, *Handbook of the Economics of Education*, volume 1 of *Handbook of the Economics of Education*, pages 577 – 633. Elsevier, 2006.

Roberto G. Gutierrez. Parametric frailty and shared frailty survival models. *Stata Journal*, 2(1):22–44, February 2002.

Cristina Hernández-Quevedo, Andrew Michael Jones, and Nigel Rice. Reporting bias and heterogeneity in selfassessed health. evidence from the british household panel survey. 2005.

Wendy Johnson, Kirsten Ohm Kyvik, Erik L. Mortensen, Axel Skytthe, G. David Batty, and Ian J. Deary. Does education confer a culture of healthy behavior? smoking and drinking patterns in danish twins. *American Journal of Epidemiology*, 2010.

Daniel Kemptner, Hendrik Jürges, and Steffen Reinhold. Changes in compulsory schooling and the causal effect of education on health: Evidence from germany. *Journal of Health Economics*, 30(2):340–354, 2011.

Adriana Lleras-Muney. The relationship between education and adult mortality in the united states. *Review of Economic Studies*, 72(1):189–221, 01 2005.

Irvine Loudon. Maternal mortality in the past and its relevance to developing countries today. *American Journal of Clinical Nutrition*, 72(1):241S–246, 2000.

Petter Lundborg. The health returns to education: What can we learn from twins? *Institute for the Study of Labor (IZA), IZA Discussion Papers*, (3399), March 2008.

C.-Y. Ly and F.C. Sung. A review of the healthy worker effect in occupational epidemiology. *Occupational Medicine*, 49(4):225–229, 1999.

Mia Madsen, Anne-Marie Nybo Andersen, Kaare Christensen, Per Kragh Andersen, and Merete Osler. Does educational status impact adult mortality in denmark? a twin approach. *American Journal of Epidemiology*, 172(2):225–234, 2010.

Philip Oreopoulos. Estimating average and local average treatment effects of education when compulsory schooling laws really matter. *American Economic Review*, 96(1):152–175, March 2006. hhhhh.

M.G. Perri, R.M. Shapiro, W.W. Ludwig, C.T. Twentyman, and W.G. McAdoo. Maintenance strategies for the treatment of obesity: an evaluation of relapse prevention training and posttreatment contact by mail and telephone. *Journal of Consulting and Clinical Psychology*, 52(3):404–413, 1984.

Mary A. Silles. The causal effect of education on health: Evidence from the united kingdom. *Economics of Education Review*, 28(1):122–128, February 2009.

Axel Skytthe, Kirsten Kyvik, Niels V. Holm, James W. Vaupel, and Kaare Christensen. The danish twin registry: 127 birth cohorts of twins. *Twin Research*, 5:352–357(6), 2002.

Danmarks Statistik. Eksamensskolerne 1912, 1914.

Gerard J. van den Berg, Sumeda Gupta, and France Portrait. Fertility and mortality: The role of early life conditions. *Unpublished Working Paper*, 68, 2008.

Gerard J. van den Berg, Gabriele Doblhammer, and Kaare Christensen. Exogenous determinants of early-life conditions, and mortality later in life. *Social Science & Medicine*, 68(9):1591–1598, May 2009a.

Gerard J. van den Berg, Petter Lundborg, Paul Nystedt, and Dan-Olof Rooth. Critical periods during childhood and adolescence: A study of adult height among immigrant siblings. *Institute for the Study of Labor (IZA), IZA Discussion Papers*, (4140), April 2009b.

Dinand Webbink, Nicholas G. Martin, and Peter M. Visscher. Does education reduce the probability of being overweight? *Journal of Health Economics*, 29(1):29–38, January 2010.

Andreas Wienke. Frailty models. *MPIDR Working PapersMax Planck Institute for Demographic Research, Rostock, Germany*, (WP-2003-032), September 2003.

Anatoli I. Yashin, James W. Vaupel, and Ivan A. Iachine. Correlated individual frailty: An advantageous approach to survival analysis of bivariate data. *Mathematical Population Studies: An International Journal of Mathematical Demography*, 5(2):145–159, 1995.

7 Tables: Descriptives and Main Results

Table 1: *Descriptives* Status Information

	All twins	Same-sex twins	Monozygotic twins
Emigrated	1.56%	2.51%	3.81%
Dead	62.07%	78.82%	95.63%
Unknown	36.37%	18.66%	0.56%

Reports the number of twins (in percent), per (last known) status

Table 2: *Descriptives* Life conditions at birth

Birthyear	Frequency	Percentage	Exam	Copenhagen	Town	Rural
1888	544	9.55%	1.47%	10.47%	18.38%	71.15%
1889	475	8.34%	1.68%	13.77%	17.61%	68.62%
1890	585	10.27%	4.79%	11.29%	18.10%	70.61%
1891	559	9.82%	3.58%	14.45%	16.21%	69.34%
1892	499	8.76%	3.01%	15.20%	18.28%	66.52%
1893	562	9.87%	3.02%	13.31%	20.55%	66.14%
1894	563	9.89%	3.20%	14.45%	20.83%	64.73%
1895	670	11.76%	4.63%	12.66%	21.88%	65.47%
1896	636	11.17%	3.46%	14.14%	19.76%	66.10%
1897	602	10.75%	4.32%	13.56%	19.89%	66.55%

Reports the number of twins per birth year that are included in the twin registry, the share of twins born in Copenhagen, in towns or in the countryside, as well as the share of twins with a schooling degree

Table 3: *Descriptives* Personal characteristics, by eligibility for reform

	Eligible for reform	Not eligible for reform
With exam	3.76%	2.97%
Without exam	96.24%	97.03%
Copenhagen	13.60%	12.94%
Town	20.61%	17.71%
Rural	65.80%	69.35%
Male	50.18%	49.47%
Female	49.82%	50.53%
Monozygotic (MZ)	15.50%	15.89%
DZ (same-sex)	25.29%	24.38%
Uncertain	-	0.04%
Unknown	18.07 %	19.65%
Same Sex	58.58%	59.95%
Different Sex	41.15%	40.05%

Descriptive statistics on individual and twin pair characteristics and conditions at birth, split according to being eligible for the reform.

Table 4: *Descriptives* Personal Characteristics, by exam status

	Overall Sample	Exam	No exam
Copenhagen	13.29%	6.66%	93.34%
Town	19.26%	5.67%	94.33%
Rural	67.45%	2.15%	97.85%
Female	50.15%	2.2%	97.8%
Male	49.85%	4.58%	95.42%
Monozygotic (MZ)	15.68%	5.04%	94.96%
DZ (same-sex)	24.86%	3.81%	96.19%
Uncertain	0.02%	0.02%	0
Unknown	18.81%	19.23%	6.90%
Same Sex	59.37%	3.31%	96.69%
Different Sex	40.63%	3.50%	96.50%

Descriptive statistics on twin pair characteristics and conditions at birth, tabulated by exam status.

Table 5: *Descriptives* Number of twins with an educational degree, per year and birth year

	1905	1906	1907	1908	1909	1910	1911	1912	1913	1914	1915	1916
1888	3	4	2									
1889	2	6	1					1	1			1
1890	8	8	10	4	2	0	3	1				
1891	0	0	4	5	2	7	2	1	1	0	0	0
1892	0	0	0	1	8	4	2	1	0	0	0	0
1893	0	0	0	0	7	4	3	4	0	0	1	0
1894	0	0	0	0	0	4	4	7	1	2	1	0
1895	0	0	0	0	0	1	7	13	6	3	2	0
1896	0	0	0	0	0	0	0	8	4	8	0	2
1897	0	0	0	0	0	0	0	2	10	9	4	3
1898	0	0	0	0	0	0	0	0	0	11	9	2
1899	0	0	0	0	0	0	0	0	0	0	5	0
1900	0	0	0	0	0	0	0	0	0	0	1	0

Absolute number of twins in our sample that took an exam, tabulated by year of birth and the year of the exam.

Table 6: *Descriptives* Aggregate number of high school graduates

Year	**Number of pupils passing the "Studentereksamen"**	**Women**	**Men**
1907	479 (+ 3.68%)	NA	NA
1908	518 (+8.14%)	78	440
1909	**516 (-0.386%)**	**54 (-30.77%)**	**462 (+4.00%)**
1910	**671 (+30.04%)**	**108 (+ 100.00%)**	**563 (+21.86%)**
1911	764 (+13.86%)	154 (+42.59%	610 (+8.35%)
1912	799 (+ 4.58%)	155 (+0.649%)	644 (+5.57%)

Aggregate number of pupils who passed the high school exam in the years around the reform, 1910 was the first year under the new regime. The number in parentheses shows the percentage increase from the previous year.

Table 7: Correlated Frailty Model Reform

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

Table 8: Univariate Model Exam

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

Table 9: Univariate Model Exam, Monozygotic

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

Table 10: Univariate Model Reform

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

Table 11: Univariate Model Reform, Monozygotic

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

Table 12: Shared Model Exam

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

Table 13: Shared Model Exam, Monozygotic

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

Table 14: Shared Model Reform

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

Table 15: Shared Model Reform, Monozygotic

Variable Name	**Parameter Estimate**	Standard Errors	**p-value**

A Technical Appendix

The likelihood function for the correlated frailty model for all observation pairs is given by:

$$\begin{aligned} L(t_1,t_2) &= \delta_1\delta_2 S_{t_1,t_2}(t_1,t_2) + \delta_1(1-\delta_2)(S_{t_2}(t_1,t_2)+S_{t_1}) \\ &+ \delta_2(1-\delta_1)(S_{t_1}(t_1,t_2)+S_{t_2}) + (1-\delta_1)(1-\delta_2)(S(t_1,t_2)) \end{aligned} \tag{8}$$

where $\delta_i = 0$ if observation i is uncensored and 1 otherwise. For two uncensored observations ($\delta_1 = \delta_2 = 1$) the bivariate probability density function is given by the following expression:

$$\begin{aligned} f(x_1,x_2) &= \frac{(1-\rho)^2 S(x_1)^{-\rho} S(x_2)^{-\rho} f(x_1) f(x_2)}{(S(x_1)^{-\sigma} + S(x_1)^{-\sigma} - 1)^{\rho/\sigma}} + \frac{(1-\rho)\rho S(x_1)^{-\rho} S(x_2)^{-\rho-\sigma} f(x_1) f(x_2)}{(S(x_1)^{-\sigma} + S(x_2)^{-\sigma} - 1)^{(\rho/\sigma)+1}} \\ &+ \frac{(1-\rho)\rho S(x_1)^{-\rho-\sigma} S(x_2)^{-\rho} f(x_1) f(x_2)}{(S(x_1)^{-\sigma} + S(x_2)^{-\sigma} - 1)^{(\rho/\sigma)+1}} + \frac{\rho(\rho+\sigma) S(x_1)^{-\rho-\sigma} S(x_2)^{-\rho-\sigma} f(x_1) f(x_2)}{(S(x_1)^{-\sigma} + S(x_2)^{-\sigma} - 1)^{(\rho/\sigma)+2}} \end{aligned} \tag{9}$$

For $\delta_1 = \delta_2 = 0$ the bivariate survivor function has the following form:

$$S(x_1,x_2) = \frac{S(x_1)^{1-\rho} S(x_2)^{1-\rho}}{(S(x_1)^{-\sigma^2} + S(x_2)^{-\sigma^2} - 1)^{\rho/\sigma^2}} \tag{10}$$

For $\delta_1 = 1, \delta 2 = 0$ the pairwise contribution to the likelihood function is

$$\begin{aligned} \partial S(t_1,t_2)/\partial t_1 &= (1-\rho) S_1^{-\rho} S_2^{1-\rho} (-f_1)(S_1^{-\sigma^2} + S_2^{-\sigma^2} - 1)^{\frac{-\rho}{\sigma^2}} \\ &- S_1^{1-\rho} S_2^{1-\rho} (S_1^{-\sigma^2} + S_2^{-\sigma^2} - 1)^{(-1-\frac{\rho}{\sigma^2})} S_1^{-\sigma^2-1} f_1 \end{aligned} \tag{11}$$

For $\delta_1 = 0, \delta 2 = 1$ the pairwise contribution to the likelihood function is

$$\begin{aligned} \partial S(t_1,t_2)/\partial t_2 &= (1-\rho) S_2^{-\rho} S_1^{1-\rho} (-f_2)(S_2^{-\sigma^2} + S_1^{-\sigma^2} - 1)^{\frac{-\rho}{\sigma^2}} \\ &- S_2^{1-\rho} S_1^{1-\rho} (S_2^{-\sigma^2} + S_1^{-\sigma^2} - 1)^{(-1-\frac{\rho}{\sigma^2})} S_2^{-\sigma^2-1} f_2 \end{aligned} \tag{12}$$

where, if we assume a Gompertz function as the baseline hazard and the shape parameter of the Gamma distribution is equal to the scale parameter, i.e. $k = \gamma$, which means that $\gamma = \frac{1}{\sigma^2}$

- $S(t_i) = (1 + e^{x_i}\gamma^{-1}B^{-1}(e^{Bt_1} - 1))^{-\gamma}$
- $f(t_i) = e^{x_i'\beta} e^{Bt_i}(1 + \gamma^{-1}B^{-1}(e^{Bt_1} - 1))^{-\gamma-1}$

B Data Collection

To document our data collection process for the educational data, we have included an example page of our scans and another page showing the result of the text recognition process. The first column shows the name of the pupil, the second column the parish of birth and the date of birth, the third column shows the father's occupation and the fifth column shows the admission date. As can be seen from the processed page (2), the names and the parish of birth were recognized with a very high degree of accuracy. The exact birth dates were added by hand later and some random checks were made as well. We then matched this retrieved data using a data base program and pre-filtered by the first three letters of the last name (including spelling variations) and the year of birth. We then manually matched on birth date and exact name, cases that were questionable were later double checked in the original scans.

Figure 1:

Eksaminandernes Navne.	Fødested, Fødselsaar og -dag.	Faderens Stilling.	Naar optaget i Skolen og i hvilken Klasse?
Cramer, Hans Billeskov Jansen	Trælund 27/5 91	Gaardejer.	Aug. 07 1 G.
Gøtzsche, Carl Viggo	København 26/12 92	Sognepræst.	— 07 1 —
Hvilsom, Knud Emil Frederik	Ans 19/8 91	Læge.	— 03 1 M.
Jensen, Jeppe	Mønsted 21/2 90	Skovløber.	Sept. 04 2 —
Larsen, Laurits Kristian	Hundborg 11/3 88	Husmand.	Okt. 07 1 G.
Nielsen, Jensen	Ejerslev 20/1 91	Gaardejer.	Aug. 07 1 —
Olsen, Elise Dorothea	Viborg 5/4 91	Rektor.	— 03 1 M.
Pedersen, Jens Carl	Grødde 3/6 89	Husmand.	— 07 1 G.
Petersen, Knud Honoré	Viborg 18/11 92	Oversergent.	Apr. 06 3 M.
Rossen, Ejnar	Bjerringbro 17/8 91	Læge.	Aug. 05 3 —
Siersted, Hans Christian	Herning 15/2 93	Herredsfoged.	— 07 1 G.
Den matematisk-naturvidenskabelige Linie.			
Jørgensen, Jørgen Hansen Viggo	Terpling 8/2 92	Realskolebest.	Aug. 07 1 G.
Obbekjær, Andreas Thorup	Helligkilde 29/12 90	Lærer.	— 07 1 —
Rendtorff, Theodor Herman Otto	Hemmestrup 15/6 92	Proprietær.	— 03 1 M.
Sørensen, Christen Nordentoft	Thaliasminde 29/2 92	Gaardejer.	— 07 1 G.
Aarhus Katedralskole.			
Den klassisk-sproglige Linie.			
Børup, Marinus	Aarhus 27/6 91	Skrædder.	Aug. 04 2 M.
Sejr, Emanuel Jensen	Nølev 29/7 91	Lærer.	— 04 2 —
Den nysproglige Linie.			
Andresen, Gerda Johanne	Aarhus 4/2 91	Sagfører.	Aug. 04 2 M.
Jensen, Thomas Agner	Ry 5/9 91	Lærer.	— 07 1 G.
Paasgaard, Marie Elisabeth	Aarhus 14/8 90	Overpolitibetj.	— 03 1 M.
Pøckel, Emmy	— 24/8 90	Fuldmægtig.	— 04 2 M.
Rud, Ejnar Jensen	Viby 7/2 92	Redaktør.	— 04 2 —
Sejersen, Ellen	København 23/10 92	Købmand.	— 04 2 —
Smith, Karen Wiggers	Holbæk 16/2 93	Konsul.	— 07 1 G.
Sørensen, Ingeborg	Jelling 22/10 91	Biskop.	— 07 1 —
Thomsen, Holger de Fine	Hjørring 6/6 92	Herredsfoged.	— 03 1 M.
Den matematisk naturvidenskabelige Linie.			
Efsen, Axel Valdemar	Silkeborg 1/1 93	Fabrikant.	Aug. 07 1 G.
Fricke, Hugo	Aarhus 15/8 92	Stabssergent.	— 07 1 —
Hammelev, Kaj	København 2/1 92	Oberst.	Okt. 07 1 —
Kühnel, Poul Oscar	Aarhus 24/2 92	Arkitekt.	Juli 03 1 M.
Lauesen, Henrik Møller Langkilde	Skanderborg 19/5 92	Overretssagf.	Aug. 06 4 —
Nielsen, Marinus Michael	Aarhus 16/10 91	Købmand.	— 04 2 —
Qvist, Peter Martin	København 17/3 92	Laboratorieforst.	— 03 1 —
Sand, Ernst Frederik	Aarhus 3/3 92	Fabrikbestyrer.	— 03 1 —
Ribe Katedralskole.			
Den klassisk-sproglige Linie.			
Binzer, August Reinhardt	Godthaab, Grønland 28/12 90	Distriktslæge.	Aug. 02 1 st.
Dela, Hans Peter	Ribe 14/1 92	Avlsbruger.	— 03 1 —
Fogh, Knud Adolf Thorvald Amelius	Holstebro 16/3 92	Amtsforvalter.	— 03 1 M.

Example page, high quality scan

Figure 2:

40 De bøjere Almenskoler 1909—10.

Eksaminandernes Navne.	Fødested, Fødselsaar og -dag.		Faderens Stilling.	Naar optaget i Skolen og i hvilken Klasse?
Cramer, Hans Billeskov Jansen..........	Trælund	²⁷/, 91	Gaardejer.	Aug. 07 1 G.
Gøtzsche, Carl Viggo	København	²¹/i, 92	Sognepræst.	— 07 1 —
Hvilsom, Knud Emil Frederik...........	Ans	¹⁹/s 91	Læge.	— 03 1 M.
Jensen, Jeppe.........................	Mønsted	²¹/₂ 90	Skovløber.	Sept. 04 2 —
Larsen, Laurits Kristian................	Hundborg	ⁿ/e 88	Husmand.	Okt. 07 1 G.
Nielsen, Jensen........................	Ej erslev	²⁰/, 91	Gaardejer.	Aug. 07 1 —
Olsen, Elise Dorothea..................	Viborg	7, 91	Rektor.	— 03 1 M.
Pedersen, Jens Carl..........•_........	Grødde	³/e 89	Husmand.	— 07 1 G.
Petersen, Knud Honoré.................	Viborg	¹⁸/n 92	Oversergent.	Apr. 06 3 M.
Eossen, Ejnar..........................	Bjerringbro	¹⁷/₈ 91	Læge.	Aug. 05 3 —
Siersted, Hans Christian................	Herning	»/, 93	Herredsfoged	— 07 1 G.
Den matematisk-natur-videnskabelige Linie.				
Jørgensen, Jørgen Hansen Viggo.........	Terpling	⁸/₂ 92	Realskolebest.	Aug. 07 1 G.
Obbekjær, Andreas Thorup............	Helligkilde	²⁹/₁₂ 90	Lærer.	07 1 -
Rendtorff, Theodor Herman Otto.........	Hemmestrup	¹³/₆ 92	Proprietær.	03 1 M.
Sørensen, Christen K ordentoft.,. -........	Thaliasminde	²⁹/₂ 92	Gaardejer.	07 1 G
Aarhus Katedralskole.				
Den klassisksproglige Linie.				
Børup, Marinus........................	Aarhus	²⁷/₆ 91	Skrædder.	Aug. 04 2 M.
Sejr, Emanuel Jensen.................	Nølev	²⁹/, 91	Lærer.	— 04 2 -
Den nysproglige Linie.				
Andresen, Gerda Johanne...............	Aarhus	7,	Sagfører.	Aug. 04 2 M.
Jensen, Thomas Agner..................	Ry		Lærer.	— 07 1 G.
Paasgaard, Marie Elisabeth.,............	Aarhus	¹⁴/₈ 90	Overpolitibetj.	— 03 1 M.
Pøckel, Emmy..........................		¹7, 90	Fuldmægtig.	— 04 2 M.
Bud, Ejnar Jensen.....................	Viby	7, 92	Redaktør.	_ 04 2 —
Sejersen, Ellen.........................	København	⁸/io 92	Købmand.	— 04 2 —
Smith, Karen Wiggers..................	Holbæk	¹⁴/₂ 93	Konsul.	— 07 1 G.
Sørensen, Ingeborg.....................	Jelling	7io 91	Biskop.	— 07 1 —
Thomsen, Holger de Fine...............	Hjørring	% 92	Herredsfoged.	— 03 1 M.
Den matematisk naturvidenskabelige Linie.				
Efsen, Axel Valdemar..................	Silkeborg	7i 93	Fabrikant.	Aug. 07 1 G.
Fricke, Hugo...........................	Aarhus	¹³/₈ 92	Stabssergent.	— 07 1 —
Hammelev, Kaj.........................	København	²/, 92	Oberst.	Okt. 07 1 —
Kuhnel, Poul Oscar....................	Aarhus	²⁴/₂ 92	Arkitekt.	Juli 03 1 M.
Lauesen, Henrik Møller Langkilde.......	Skanderborg	¹⁹/₃ 92	Overretssagf.	Aug. 06 4 —
Melsen, Marinus Michael...............	Aarhus	¹⁶/₁₀ 91	Købmand.	— 04 2 —
Qvist, Peter Martin.....................	København	¹⁷/₃ 92	Laboratorieforst.	— 03 1 —
Sand, Ernst Frederik.................			Fabrikbestyrer.	— 03 1 —
Ribe Katedralskole.				
Den klassisksproglige Linie.				
Binzer, August Keinhardt........	Godthaab, Grønland	²⁸/₁₂ 90	Distriktslæge.	Aug. 02 1 st.
Dela, Hans Peter.______________	Ribe	¹⁴/i 92	Avlsbruger.	— 03 1 —
Fogh, Knud Adolf Thorvald Amellus	Holstebro	¹⁶/₃ 92	Amtsforvalter.	— 03 1 M.

Example page, processed with text recognition