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Does More Schooling Reduce Hospitalization and Delay Mortality? New Evidence Based on Danish Twins

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Abstract Schooling generally is positively associated with better health-related outcomes—for example, less hospitalization and later mortality—but these associations do not measure whether schooling causes better health-related outcomes. Schooling may in part be a proxy for unobserved endowments—including family background and genetics—that both are correlated with schooling and have direct causal effects on these outcomes. This study addresses the schooling-health-gradient issue with twins methodology, using rich data from the Danish Twin Registry linked to population-based registries to minimize random and systematic measurement error biases. We find strong, significantly negative associations between schooling and hospitalization and mortality, but generally no causal effects of schooling.

Keywords Health-schooling gradients · Schooling · Mortality · Hospitalization · Twins

Introduction

Schooling differences are a major component of socioeconomic differences. Schooling also generally is positively associated with better health-related behaviors

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and better health/mortality outcomes. Those with more schooling tend to have better physical and mental health and to have longer lives, and these results are often interpreted to mean that increasing schooling improves health and that there is an important schooling-health gradient that disadvantages those with less schooling.[1] However, these associations do not measure by how much schooling causes better health-related behaviors and better health/mortality outcomes. Schooling may in part be a proxy for unobserved endowments—including family background and genetics—that have direct causal effects, additional to any effects through schooling, on health behaviors and health/mortality outcomes.

This study's principal goals are to describe the associations between schooling and (a) hospitalization in 1980–2002 and (b) mortality by 2003 for the 1921–1950 birth cohorts in Denmark, and to investigate for the same cohorts the causal impact of schooling on health and mortality, net of endowments, using rich data on twins from the Danish Twin Registry that are linked to Danish population-level registries. We explore four central questions:

1. What are the cross-sectional associations between schooling attainment and hospitalization and mortality for Danish adults?
2. What are the estimated causal impacts of schooling on hospitalization and mortality for these individuals when the monozygotic (MZ) or identical twin design is used to control for unobserved family endowments?
3. To what extent do these within–MZ twins estimated impacts differ from what cross-sectional associations suggest?
4. To what extent do within–MZ and within–DZ (dyzygotic, or fraternal) twin estimates differ, as they will if individual-specific endowments (as contrasted with common family endowments) are important?

The reduced-form relations that we investigate in this study may change with market conditions, public policies, aggregate economic conditions, other secular trends, and life-cycle stage. Because of this, for all these questions, we consider the 1921–1935 and 1936–1950 birth cohorts separately and test whether they can be combined in the same estimates for the 1921–1950 birth cohorts. Because there may be gender differences in such relations, we also estimate the relations for females and males separately and test whether they can be combined. The linked twins data, conditional on the assumptions necessary for the within-twins estimates to be consistent, permit us to answer these questions; the data thereby significantly increase our understanding of causal effects of schooling on health-related behaviors and mortality outcomes, net of endowments, and therefore on what underlies the schooling-health gradient.

The contribution of this article rests on using within-twins estimators with unusually rich data. We therefore first describe the methodology, previous related

[1] Many studies have considered such possibilities, some of which have focused on the relative schooling (and other aspects of socioeconomic status) effects on health (e.g., Adams et al. 2002; Adler et al. 1994; Brooks-Gunn et al. 1997; Case et al. 2002a, b; Deaton 2001a, b; Deaton and Paxson 1998, 1999, 2001; Elo and Preston 1992, 1996; Kawachi et al. 1999; Marmot 1999; Mellor and Milyo 2002; Preston 1975; Preston and Elo 1995; Ross and Mirowsky 1999; Smith 1999; Strauss and Thomas 1998; Wilkinson 1992, 1996, 2000). Most of these studies did not control for the endogenous choice of schooling, but a relatively few studies did (see the Previous Related Studies section).

studies, and the data; and then we turn to the estimates. The estimates question, in brief summary, the standard inference that the strong negative association between schooling and outcomes such as hospitalization and mortality reflects important direct causal effects of schooling. Instead, schooling seems primarily to serve as a marker for parental family and individual-specific endowments that are uncontrolled in the usual estimates. This raises questions about the usual attribution to schooling of substantial positive effects on health-related behaviors and outcomes. In terms of causal effects, despite the strong associations with schooling, the real stratification appears to be with regard to parental family and individual-specific background endowments.

Methodology

We are interested in simple reduced-form relations between schooling and health-related behaviors and outcomes because we are interested in schooling's total or gross associations with, and effects on, health-related behaviors and outcomes. This approach does not permit us to identify the underlying structural channels through which schooling may be operating, and indeed, our data do not permit the identification of those channels. Still, understanding better the gross associations with and effects of schooling on these outcomes is valuable in itself, as noted in the introduction.

We do not have very strong prior assumptions about even the sign of the schooling effects in these reduced forms, particularly for hospitalization. More schooling, for example, may have countervailing effects on hospitalization if it reduces the probability of negative health conditions that might lead to hospitalization yet increases the probability of hospitalization (e.g., through greater income, more knowledge, or better connections) for given health conditions. Likewise, more schooling may have countervailing effects on mortality if it increases income and access to health-system care but also increases higher-risk behaviors and selection into more stressful occupations. Of course, which opposing effect dominates depends on preferences, resources, technologies, markets, and policies that prevail in the context being studied. Schooling effects through increasing access to health services through higher income, for example, *a priori* would seem to be less important in the context of the Danish health care system, with its broad coverage and access, than in a health-care system such as that in the United States, in which many individuals are not covered, particularly prior to the recent reforms, by health insurance.

Consider the following linear representation of a reduced-form equation relating adult health-related behaviors or health/mortality outcomes H_{ij} for the ith member of paternal family j to his or her schooling S_{ij} and to three sets of unobserved variables representing (1) endowments h_j that are common among all children of the paternal family j (e.g., exogenous features of the paternal family environment in childhood, including prices, family income, parents' human capital characteristics, average genetic endowments among siblings, local schooling, and health-related options); (2) the component of endowments or "innate health" specific to child i in j, represented by a_{ij} (e.g., individual-specific deviations from average family genetic endowments);

and (3) a random health shock specific to i in j, inclusive of measurement errors in health, represented by v_{ij}[2]:

$$H_{ij} = \beta_S S_{ij} + h_j + a_{ij} + v_{ij}, \qquad (1)$$

where β_S is the effect of schooling. S_{ij} is itself a function of (usually largely unobserved) variables that pertain to the paternal family and to the individual children in the paternal family[3]:

$$S_{ij} = \alpha_h h_j + \alpha_a a_{ij} + \alpha_s a_{kj} + u_{ij}, \qquad (2)$$

where α_h is the effect of the family-specific endowment h_j on child schooling investment, α_a is the effect of the individual-specific endowment a_{ij} of child i on schooling investment in that child, α_s is the effect of the individual-specific endowment a_{kj} of sib k (which is a vector if there are multiple siblings) on schooling investment in child i,[4] and u_{ij} is a disturbance that affects S_{ij} but not H_{ij} except indirectly through S_{ij} (a critical assumption for the within-MZ estimators below to yield consistent estimates, as we discuss further below with reference to the Bound and Solon (1999) critique of twins studies. The stochastic term, for example, might reflect the chance event of whether one twin was assigned a more inspiring teacher than the other or happened to encounter more inspiring role models than the other. As is well known, the parameter β_S is not identified in Eqs. 1 and 2 if α_a or α_h is not zero. β_S is estimated with bias if Eq. 1 is estimated across individuals with different values of h_j and a_{ij}. The regression coefficient for an ordinary least squares (OLS) estimate of relation (1) is $\mathrm{cov}(H_{ij}, S_{ij}) / \sigma^2(S_{ij}) = [\beta_S(\sigma^2(S_{ij}) + \mathrm{cov}(S_{ij}, h_j) + \mathrm{cov}(S_{ij}, a_{ij})] / \sigma^2(S_{ij})$, which is a biased estimate of β_S unless $\mathrm{cov}(S_{ij}, h_j) + \mathrm{cov}(Sij, a_{ij}) = 0$. Thus, the cross-sectional estimate of the association between schooling and health is generally a biased estimate

[2] Many studies have documented the association of genetic endowments with physical and mental health (e.g., Amouyel et al. 1996; Bartres-Faz et al. 2000; Chen et al. 2001; Christensen et al. 1995, 1998, 2000a; Clee et al. 2001; Eicher et al. 2002; Forsberg et al. 2002; Frosst et al. 1995; Humbert et al. 1993; Jenny et al. 2002; Jiang et al. 2001; Kelly et al. 2002; Kluijtmans et al. 1996; Morita et al. 1997; Myllykangas et al. 2001; Pericak-Vance and Haines 1995; Roses 1998; Sawano et al. 2001; Voetsch et al. 2002). For simplicity, we represent the endowments here as if they were scalars. However, they may be vectors with different components, for example, that differentially affect schooling versus health and that are not perfectly correlated and, indeed, not necessarily positively correlated. In fact, some recent studies have suggested that innate education and health components of endowments may be negatively correlated (Behrman et al. 2008; Behrman and Rosenzweig 2004).

[3] This formulation is consistent with standard models of intrahousehold allocation of investments in children (e.g., Becker 1991; Becker and Tomes 1976; Behrman et al. 1982, 1995). Behrman et al. (1994) used a similar formulation with MZ and DZ twins to estimate whether such intrahousehold allocations reinforce or compensate for individual-specific endowment differences among siblings (and found reinforcement).

[4] In general, endowments of all members of a sibship affect the investments in any member of the sibship, but the effects of all siblings other than the other twin drop out in the within-twin estimators because the characteristics of other siblings are the same for the two members of a twinship. We expect the direct effect to be greater than the cross effect ($\alpha_a > \alpha_s$). If the coefficient of the cross effect is positive, there is compensating behavior in that parents invest more in sibling j if sibling k is better endowed. If the coefficient of the cross effect is negative, there is reinforcing behavior (i.e., reinforcing endowment differences) in that parents invest less in sibling j if sibling k is better endowed. In the within-MZ estimator, the two twins have identical individual-specific endowments, so a_{ij} and a_{kj} cancel out. The only estimates of which we are aware that are consistent with this framework and that estimate the cross-twin effect using a combination of MZ and DZ twins indicated reinforcing behavior (Behrman et al. 1994).

of the causal impact of schooling on health because schooling is partially a proxy for genetic, family background, and other endowments in such estimates.

Within–MZ Twins Estimators

With no further assumptions, it is clear that β_S (the health impact of schooling) is not identified even if sibling-pair data are used to control in the estimation of β_S for the covariant common components of the endowment and environment h_j and s_j. This is because of the existence of the specific component of the endowments a_{ij}. As long as families or individuals respond to individual-specific differences in endowments, and such differences are important, then sibling estimators may not be very useful.[5] In recognition of this problem, researchers (e.g., Ashenfelter and Krueger 1994; Ashenfelter and Rouse 1998; Behrman et al. 1980, 1994, 1996; Behrman and Rosenzweig 1999; Behrman and Taubman 1976; Bonjour et al. 2003; Miller et al. 1995, 1997) have employed samples of pairs of MZ twins, between whom endowment differences at conception are as minimal as possible,[6] to identify β_S in estimates of models parallel to Eqs. 1 and 2 with ln(earnings) as the dependent variable of interest (and in Behrman and Rosenzweig (2002, 2005), with child schooling as the dependent variable). To our knowledge, only one published study used this approach to explore the relation between schooling and health (Fujiwara and Kawachi 2009; see the section Previous Related Studies). Equations 1 and 2 can be rewritten for MZ twins:

$$H_{ij}{}^M = \beta_S S_{ij}{}^M + h_j{}^M + v_{ij}{}^M \tag{1A}$$

$$S_{ij}{}^M = \alpha_h h_j{}^M + u_{ij}{}^M, \tag{2A}$$

where the superscript M refers to MZ twins. Relations parallel to Eqs. 1A and 2A can be written for the kth MZ twin in the jth family. Within–MZ twin estimators are obtained by subtracting such relations from Eqs. 1A and 2A. With a within–MZ twin estimator, all unobserved endowment components in Eqs. 1A and 2A are eliminated.

These within-MZ estimators can be used to identify the true reduced-form impact of schooling on health-related behaviors and health/mortality outcomes under the assumptions of this section for the own-health behavioral and outcome variables in the data that are introduced in the Data section. As noted, comparisons between the

[5] Behrman et al. (1994, 1996) reported evidence that the individual-specific endowments are significant and important in schooling-earning models estimated for the United States.

[6] Differences in birth weights between MZ twins have been used to estimate the impact of *in utero* nutrition on subsequent life cycle outcomes, but such differences could be due to factors such as differential placement in the womb relative to the placenta even if the two members of a twinship were identical at conception (e.g., Behrman and Rosenzweig 2004; Conley et al. 2006). However, for some observable outcomes for which MZ twins are discordant in a few percent of births, such as congenital malformations, some studies suggested that the differences originate at least in part in differences in genes, perhaps related to the process of twinning (e.g., Hall 2003; Kondo et al. 2002; Lubinsky and Hall 1991), and not just in the environment in the womb. For such reasons, within–MZ twins estimates may not control perfectly for endowments at conception (as we assume herein), but they nevertheless control better than other options for such endowments.

within-MZ estimates and OLS estimates of relation (1) may be made for the same health-related behaviors and outcomes to learn the extent to which the estimates of the impact of schooling on health β_S are biased in standardly used cross-sectional estimates because of the failure to control for unobserved endowments h_j and a_{ij}, which affect health and which are correlated with S because they partly determine schooling in relation (2). Comparisons also can be made of the within-MZ estimates for females versus males and across birth cohorts to check for significant gender or cohort effects in the reduced-form impact of schooling on health-related behaviors and health/mortality outcomes. Further, comparisons can be made between within-MZ and within-DZ twins to assess whether the unobserved individual-specific endowments a_{ij} are important; if so, then within-sibling estimates that control only for common family endowments h_j are misleading because they still include $a_{ij}-a_{ik}$, which is correlated with $S_{ij}-S_{ik}$, in the disturbance term.

We now consider the key assumption, which has been questioned by Bound and Solon (1999) and others, that is necessary for the twins estimates to produce unbiased estimates: the disturbance terms in the health and schooling relations are uncorrelated ($\text{cov}(u_{ij}, v_{ij}) = 0$).[7] Suppose that this assumption does not hold because there is some unobserved individual twin-specific factor (persistent shock) c_{ij} that directly affects both the schooling and the health of twin i and that differs from any individual twin-specific factor (persistent shock) c_{kj} that affects both the schooling and the health of twin k in the same family j, so that Eqs. 1 and 2 are replaced by Eqs. 1B and 2B:

$$H_{ij} = \beta_S S_{ij} + h_j + a_{ij} + c_{ij} + v_{ij}, \tag{1B}$$

$$S_{ij} = \alpha_h h_j + \alpha_a a_{ij} + \alpha_s a_{kj} + \gamma_c c_{ij} + \gamma_k c_{kj} + u_{ij}. \tag{2B}$$

If γ_c is positive, then the impact of the persistent shock on schooling is in the same direction as the impact on health (and vice versa if γ_c is negative). As noted earlier with regard to the endowment effects, the own effects are plausibly greater than the cross effects, so $\alpha_a > a_{kj}$ and $|\gamma_c| > |\gamma_k|$. Equations 1B and 2B can be rewritten for MZ twins to incorporate these persistent shocks:

$$H_{ij}^{M} = \beta_S S_{ij}^{M} + h_j^{M} + c_{ij}^{M} + v_{ij}^{M} \tag{1C}$$

$$S_{ij}^{M} = \alpha_h h_j^{M} + \gamma_c c_{ij}^{M} + \gamma_k c_{kj}^{M} + u_{ij}^{M}. \tag{2C}$$

Again, relations parallel to Eqs. 1C and 2C can be written for the kth MZ twin in the jth family. Within–MZ twin estimators are obtained by subtracting such relations from Eqs. 1C and 2C. With a within–MZ twin estimator, the unobserved endowment components h_j^{M} in Eqs. 1B and 2B again are eliminated. In this case, though, the difference in the unobserved twin-specific persistent shocks remains:

[7] Schnittker and Behrman (2010) and Kohler et al. (2011) provided recent evaluations of this and other criticisms of within-twins estimates and other twins designs used in the social sciences.

$$\Delta H_{ij}{}^{M} = \beta_S \Delta S_{ij}{}^{M} + \Delta c_{ij}{}^{M} + \Delta v_{ij}{}^{M} \tag{1D}$$

$$\Delta S_{ij}{}^{M} = (\gamma_c - \gamma_k)\Delta c_{ij}{}^{M} + \Delta u_{ij}{}^{M}, \tag{2D}$$

where Δ is the difference between the kth and ith MZ twin in the jth family. Estimating Eq. 1D, therefore, does not give an unbiased estimate of β_S because $\Delta S_{ij}{}^{M}$ and $\Delta c_{ij}{}^{M}$ are correlated (see Eq. 2D); and $\Delta c_{ij}{}^{M}$ is, by assumption, unobserved. The sign of this bias is determined by the sign of the correlation, which is the sign of $(\gamma_c - \gamma_k)$. This sign is positive (negative) if the impact of the shock on schooling is in the same (opposite) direction as the impact of the shock on health; the estimate of β_S from Eq. 1D, then, is an overestimate (underestimate) of the true value of β_S. For example, if more favorable *in utero* environments owing to proximity to the placenta increase both schooling and health beyond any effect through schooling, as suggested by the results in Behrman and Rosenzweig (2004), the estimate of β_S from Eq. 1D is an overestimate of the true value of β_S[8]: likewise, if an accident or illness limits schooling and has persistent negative effects on later health. Also note that if β_S truly is positive, but the estimate of β_S from Eq. 1D is not significant as we find (see Estimates section), and this is because of the failure to control for $\Delta c_{ij}{}^{M}$ in the estimates, that means that γ_c must be negative so that the unobserved factor c_{ij}, which positively affects long-run health, has negative effects on schooling (or vice versa).

Previous Related Studies

Most of the previous literature has estimated relations, such as in Eq. 1 with OLS (or logit or probit), but as noted earlier, β_S (the health impact of schooling) is not identified in such estimates. However, a relatively small number of studies have used alternative estimators to try to identify the causal impact of schooling on health (for reviews of these studies, see Cutler et al. 2006; Cutler and Lleras-Muney 2008; and Grossman 2006).

Almost all these studies have used instrumental variable (IV) estimators. Selected examples are summarized here.[9] In a relatively early study, Berger and Leigh (1989) used per capita income and per capita expenditures on education in the state of birth as instruments for schooling in their study of schooling effects on health in the United States. They found that with the IV estimates, schooling effects were slightly reduced but remained significant. Their instruments, however, may have been related to expenditures on health, which, if health expenditures affect health, raises questions about the validity of instruments. Most of the more recent studies used "natural experiments"—often school reforms, school age regulations, and labor

[8] Amin et al. (2010) tested for the impact of this possibility by including birth weights in their estimates and found that this does not change their estimates of the schooling coefficient substantially.

[9] Other examples that mostly focus on health-related behaviors, such as smoking, are Chou et al. (2007), de Walque (2007), Grimard and Parent (2007), Kenkel et al. (2006), and Oreopoulos (2006).

market dimensions—as instruments for schooling. Lleras-Muney (2005), for example, used changes in compulsory school attendance and child labor laws from 30 states in the United States from 1915 to 1939 to identify schooling effects on mortality. She used grouped census data as well as individual data from 1985. Her results from the grouped data suggested that one additional grade of schooling lowered the 10-year death rate by 3.6%, but her estimated schooling effects from individual data were not significant. Adams (2002) adapted the identification strategy suggested by Angrist and Krueger (1991), using quarter of birth in a U.S. cross-section of individuals from 1992. Adams' dependent variables were self-reported health and a number of variables describing functional limitations as health measures. When IV estimation was used, schooling effects increased slightly but were insignificant. Furthermore, F values on the instruments were just slightly more than 1, indicating a problem of weak instruments. Arkes (2003) estimated schooling effects on work-limiting health problems, need for personal care, and mobility limitations, using a large U.S. sample of white males aged 47–56. Arkes used within-state differences in unemployment rates as instruments for schooling. He found that schooling effects increased somewhat, allowing for endogeneity, and were significant for two of the three health measures. Spasojevic (2003) used changes in compulsory schooling in Sweden in the 1950s as instruments to analyze schooling effects on an index of bad health and on body mass index (BMI) in a healthy range over the period 1981–1991. She found positive significant effects (although only when using one-tailed tests) from completed years of schooling on both health indicators. Arendt (2005) used schooling reforms in Denmark in 1958 and 1975 as instruments but did not find any significant effects of instrumented schooling on health.

As indicated earlier, many of these IV studies seem to have suffered from problems of having weak instruments. Also, these IV estimates tended to be LATE (local average treatment effects) estimates (see Angrist et al. 1996; and Lundborg 2008), and the estimated schooling effects are therefore relevant for individuals who are at the margin of being affected by the instruments used (e.g., at the margin of unemployment or compulsory schooling levels). In heterogeneous populations, these LATE estimators can differ substantially from the more generally relevant ATE (average treatment effects) estimators that measure the average causal effect of schooling on health outcomes.

Behrman and Wolfe (1989) used adult sisters (sibling) estimators with Nicaraguan data and found that significant inverse associations of their schooling with three of four disease categories for adult women in the level estimates became much less precisely estimated and no longer significant at the standard .05 level with within-sibling estimates. As noted earlier, within-sibling estimates can identify β_S under the assumption that cov(u, v) equals zero and that the covariance between the individual-specific component of endowments and schooling is zero. The latter seems a strong assumption, and was not supported by the Behrman et al. (1994) finding of significant impacts of individual-specific endowments on schooling and earnings using MZ and DZ twins (the former to estimate unbiased coefficients and the latter, conditional on those coefficients, to investigate individual-specific endowment effects). Similarly, Fletcher and Lehrer (2009), using a subset of the AddHealth data for which DNA markers are available, documented that academic achievement among DZ twins was partially related to unobserved genetic differences among DZ twins, thus further questioning the suitability of sibling estimators to obtain causal effects of schooling on health outcomes later in life.

To our knowledge, the only published study that applied within–MZ twins estimators to this problem is that by Fujiwara and Kawachi (2009). They used 702 MZ twins from the 1995 Midlife in the United States (MIDUS) survey with a continuous measure of schooling attainment. They reported generally no significant causal effects of schooling on seven health outcomes and 10 health behaviors in within-MZ estimates when males and females were combined. (An exception was for perceived global health if male and female samples were combined, but not in separate estimates for females and males.)[10] Their coefficient estimates did not change substantially between the two sets of estimates, but their significance did. In an as-yet unpublished study, Lundborg (2008) used a sample of 694 MZ twins from the same MIDUS survey with a nonlinear representation of schooling by including dichotomous variables for high school, some college, and college degrees.[11] His within-MZ results suggested a causal impact of self-reported schooling levels on health, with a significant positive impact on self-reported health and negative impacts on the number of chronic conditions.[12] He found no evidence of bias owing to the unobserved endowments that twins share in common; in fact, his within–MZ twins estimates of schooling impacts were larger for self-reported health than were his OLS estimates even though he was not able to control for measurement error in self-reports of schooling, which in itself was likely to bias his coefficient estimates toward zero (see the end of the Data section). He noted that within–MZ twins estimates are likely to be closer to ATE than are IV estimates identified by factors like school reforms, such as those discussed earlier, that are likely to be LATE estimators. The Fujiwara and Kawachi (2009) and Lundborg (2008) studies appear to differ importantly only in their representation of schooling as a continuous variable in the former case and a nonlinear variable in the latter case. We therefore explore both options in the Estimates section.

Data

Our analyses of schooling differences and health-related behaviors and outcomes use a rich new longitudinal data set obtained through a link between the Danish Twin

[10] In their discussion, they state: "In summary, the current study showed possible causal effects of education on perceived global health and on smoking habits among males, but did not suggest direct associations between schooling and the other health outcomes studied." (Fujiwara and Kawachi 2009:1320). The significant outcomes for males to which they refer, however, did not occur in within-MZ estimates, but only in within-MZ and -DZ combined estimates.

[11] Two other unpublished studies that present within–MZ twin estimates of the impact of schooling on health outcomes and health-related behaviors are Amin et al. (2010), with estimates for the United Kingdom, Australia, and the United States; and Behrman et al. (2006), with estimates for China.

[12] Lundborg (2008) also explored the impacts of schooling on health-related behaviors and reported little evidence of significant impacts.

Registry[13] with various population-based registers at Statistics Denmark. This link provides rich longitudinal information for schooling and health-related behaviors and outcomes for all Danish twins born between 1921 and 1950[14] that have been identified in the Danish Twin Registry. Of particular relevance for this study is the link to the Danish National Hospital Register compiled by the National Board of Health from information provided by local health authorities for all discharges from somatic hospital overnight stays. The Danish National Hospital Registry, established in 1977, includes information on about 99.4% of all discharges from nonpsychiatric hospitals in Denmark. Recorded information includes the Civil Personal Registration number (CPR) and the dates of admission and discharge (Andersen et al. 1999). In addition to the twins, the data set that we use includes a 5% same-age random sample of the Danish population. In order to assure confidentiality, access to these data is limited to a dedicated computer at the Danish Twin Registry at the University of Southern Denmark that is linked to limited-access computers at Statistics Denmark.

The data set used for our analysis encompasses 87,773 singletons in the 5% sample, 5,294 MZ twins in complete pairs, and 11,234 DZ twins in complete pairs (Tables 1 and 2). For the 5% sample, the 1936–1950 birth cohort includes about one-quarter more individuals than the 1921–1950 cohort; this reflects overall population growth between these two cohorts. For the MZ and DZ complete-pair twins samples, the 1936–1950 birth cohort includes about one-half more individuals than the 1921–1950 cohort, probably reflecting that a twin pair is eliminated if one or both twins migrated before 2003 or died before 1980—and the older birth cohort had more exposure to both migration and mortality (at older ages for mortality)—in addition to the overall population growth between these two cohorts.[15] The male/female ratio is close to 1 for both cohorts (0.99, 1.02), but is a fair amount higher for both MZ and DZ twins for the 1936–1950 cohort than for the 1921–1935 cohort, with a surprisingly high value for the younger cohort for DZ twins (1.24). This apparently reflects that the recruitment of twins in 1943–1968 was partly conducted using conscription boards that focused more on men (see Kyvik et al. 1996).

In this article, we consider three health-related outcomes. First, we measure the number of days hospitalized per year of exposure (with exposure terminated by mortality) during 1980–2002. Although previous studies have used such measures

[13] This has been a valuable source of data for study of a number of biomedical, biodemographic, and socioeconomic topics. For more information and some examples, see Andersen-Ranberg et al. (1999); Bingley et al. (2009); Christensen et al. (1995, 1996, 1998, 1999, 2000b); Gaist et al. (2000); Hauge (1981); Kohler and Kohler (2002); Kohler and Rodgers (1999, 2000); Kohler et al. (1999, 2001, 2002, 2003, 2005); Kyvik et al. (1996); Rodgers et al. (2008); and Skytthe et al. (1998, 2002). Some have been concerned that twins have a different health and aging trajectory profile owing to the fact that they experience growth restrictions in the womb, but extensive tests generally have found no differences in health and aging trajectories between twins and singletons in high-income populations (Christensen and McGue 2008).

[14] The data includes birth cohorts up to 1975, but we limit this analysis to the 1921–1950 birth cohorts because the dependent variables on which we focus are primarily relevant for older adults.

[15] If migration or early mortality is affected by endowments that also affect schooling and the health outcomes that we study, the inability to include individuals who migrated or experienced early mortality biases the estimated impact of schooling on health using standard OLS individual estimates. However, within–MZ twins estimates control for the first-order effects of such endowments and thus do not suffer from the same biases of sample selection due to endowment-related migration or early mortality as do OLS estimates.

Table 1 Descriptive statistics

		5% Sample		MZ Twins		DZ Twins	
Cohort and Gender	Variables and Sample Sizes	Mean	SD	Mean	SD	Mean	SD
1921–1935, Males	Schooling (years)	10.4	3.5	10.6	3.6	10.0	3.5
	Hospital days/exposure	3.2	8.3	3.5	14.8	3.2	10.9
	HD/Exp < 2y death	1.8	4.0	1.7	3.7	1.8	5.0
	Mortality by 2003	0.44	0.50	0.35	0.48	0.41	0.49
	N	19,373		970		2,140	
	N for HD/Exp < 2y death	19,016		956		2,099	
1921–1935, Females	Schooling (years)	9.2	3.0	9.2	3.1	9.0	3.0
	Hospital days/exposure	3.0	8.2	2.7	7.2	3.0	10.7
	HD/Exp < 2y death	1.8	4.4	1.6	3.6	1.6	3.4
	Mortality by 2003	0.30	0.46	0.24	0.43	0.28	0.45
	N	19,525		1,092		2,184	
	N for HD/Exp < 2y death	19,305		1,082		2,156	
1936–1950, Males	Schooling (years)	11.7	3.4	11.8	3.4	11.3	3.5
	Hospital days/exposure	1.0	3.7	0.9	3.3	1.0	3.9
	HD/Exp < 2y death	0.7	2.5	0.7	2.6	0.8	3.1
	Mortality by 2003	0.11	0.32	0.09	0.29	0.12	0.33
	N	24,649		1,664		3,828	
	N for HD/Exp < 2y death	24,546		1,660		3,802	
1936–1950, Females	Schooling (years)	10.9	3.3	10.9	3.2	10.7	3.3
	Hospital days/exposure	1.1	4.2	1.0	2.6	1.0	2.6
	HD/Exp < 2y death	0.8	2.1	0.8	2.0	0.8	1.8
	Mortality by 2003	0.08	0.27	0.07	0.25	0.07	0.25
	N	24,226		1,568		3,082	
	N for HD/Exp < 2y death	24,174		1,566		3,075	
1921–1950, Males and Females	Schooling (years)	10.6	3.4	10.8	3.4	10.4	3.4
	Hospital days/exposure	1.9	6.3	1.8	7.6	1.8	7.3
	HD/Exp < 2y death	1.2	3.3	1.1	2.9	1.1	3.4
	Mortality by 2003	0.22	0.41	0.16	0.37	0.19	0.39
	N	87,773		5,294		11,234	
	N for HD/Exp < 2y death	87,041		5,264		11,132	

Notes: Hospital days/exposure refers to the 1980–2002 period. HD/Exp < 2y death is hospital days/exposure up through 2000 or up to two years before death if died prior to 2003.

Source: Danish Twin Registry linked with population registry data, as described in text.

for much shorter periods of time or based on respondent recall, we are unaware of previous studies that have been able to use such registry-based data for more than two decades. The days hospitalized per year of exposure during these 23 years vary substantially across individuals within gender-cohort groups (Table 1). The means are about three times as high for the 1921–1935 birth cohort as for the 1936–1950 birth cohort (a little more for males, a little less for females) for the 5% sample and

Table 2 Summary measures of sample composition in Table 1

	5% Sample	MZ Twins	DZ Twins
Total *N*	87,773	5,294	11,234
1936–1950/1921–1935	1.26	1.57	1.60
Males/Females 1921–1935	0.99	0.89	0.98
Males/Females 1936–1950	1.02	1.06	1.24

both twins samples, reflecting the increased hospitalization rates with age. In 1980, the members of the older cohort were 45–59 years old, and those of the younger cohort were 30–44 years old; in 2002, the members of the older cohort were 67–81 years old, and those of the younger cohort were 52–66 years old. For the older cohort, the mean days hospitalized for females is slightly smaller for all three samples than for males, but there are almost no differences for the younger cohort.[16] For the older cohort, this pattern may reflect that females have higher life expectancies than males, but it contrasts with some studies that suggest that females nevertheless use health services more than males.

Our second health-related outcome is the number of days hospitalized per year of exposure during 1980–2000 up to two years before mortality or before the end of the sample in 2002.[17] This measure is closely related to the previous one, but attempts to abstract from hospitalization related to imminent mortality by not including the last two years of life in order to obtain a measure of the extent of hospitalization prior to the onset of terminal conditions. Because substantial hospitalization is not uncommon in the two years prior to death, the means for this measure are only about three-fifths as large as those for the first measure above for the 1921–1935 birth cohort group; the difference is not quite as large for the 1936–1950 birth cohort group because of the lower prevalence of mortality before 2003. For both cohorts, the standard deviations for this variable are considerably smaller than for the previous one, implying that a substantial portion of the variance in the hospitalization per year over the life cycle is due to large variation in the two years before death.

Third, we measure whether an individual died prior to 2003. Again, our mortality data are based on registry data, not on recall data of relatives or others (which probably is important particularly for reducing measurement error in calculating the relevant exposure periods for the health-related outcome variables described earlier). For the older, 1921–1935 birth cohort (who were 67–81 years old by 2002), about 40% of the males and 30% of the females died prior to 2003, with somewhat higher mortality rates for the 5% sample than for the twins samples, perhaps reflecting the selectivity with regard to survival of both twins to 1980, as noted earlier. For the younger, 1936–1950 birth cohort (who were 52–66 years

[16] The hypothesis of equal means for men and women is rejected at with a p value of .016 for the cohort 1921–1935, but the means for men and women are not statistically different for the 1936–1950 cohort.

[17] The *N*s are slightly smaller for this variable than for the other two because of some mortality in 1980–1982 (see Table 1).

old by 2002), about 11% of the males and 8% of the females died prior to 2003, with somewhat higher mortality rates again for the 5% sample than for the twins samples, except for male DZ twins pairs.

Our key explanatory variable is years of schooling attainment,[18] as calculated from the highest vocational and academic training and the months/years of schooling associated with the highest attained degree. This information comes from administrative registers based on individual self-reports from the 1970 population census that were updated with educational institution reports following the 1973 establishment of the integrated student register, which covers about one-half of our sample (e.g., 48% of the MZ twins).[19] That means that measurement error related to recall errors, which has played a major role in the twins (and siblings more generally) literature because of the exacerbation of the noise-to-signal ratio in within-twins (siblings) estimates (e.g., Ashenfelter and Krueger 1994; Behrman et al. 1994; Bishop 1976; Griliches 1979), is not likely to be as serious a problem for this study as for those studies with only recall data for their samples; we consider measurement error further later in this section. Years of schooling attainment in the 5% sample averaged 10.4 for males and 9.2 for females in the 1921–1935 cohort, and 11.7 for males and 10.9 for females in the 1936–1950 cohort. Thus, both cohorts had high levels of schooling, on average, in comparison with most countries (see UNESCO 2011) with some increase between the two periods (12% for males, 18% for females) and a reduction of the gender gap favoring males. The mean schooling grade attainment for MZ twins is about the same as for the 5% sample for each gender-birth cohort group, and that for DZ twins is a little lower. This may reflect a tendency, for these birth cohorts, for DZ twinning to be more common among lower-income families with more children.

As noted in the section Previous Related Studies, the question arises whether differences exist in schooling between twins over substantial ranges in the distribution of schooling so that ATE estimates over this range rather than LATE estimates over a much narrower range can be obtained. A detailed analysis of these schooling differences within MZ twin pairs is provided in Table 3. For the combined male and female MZ sample that is used for most of our analyses, the mean difference in years of schooling within twin pairs is 1.6, with a standard deviation (SD) of 2.5. The difference in the years of schooling is zero for about one-half of the twin pairs, but considerable variation exists in the other half. This variation in years of schooling occurs across the complete range of schooling attainment by the twins. To illustrate this pattern of schooling differences, Table 3 tabulates and summarizes

[18] We use "years of schooling," as is conventional in this literature, to mean the years of schooling required to attain a given level or grade of schooling with full-time school attendance and no grade repetition or skipping. If there is grade repetition or part-time attendance, the calendar years attending school may exceed the years of schooling as we use the term (and if there is skipping of grades, the reverse may hold).

[19] Both individual population census self-reports and education institution reports are for training undertaken and qualifications attained. The Ministry determined which is the highest qualification separately along academic and vocational lines according to "normal" completion times. The data that are available to us are normal completion times for highest qualification achieved. In a few cases where the highest vocational/academic training is unknown in the data, the minimal required years of schooling for these cohorts (= 7 years) was used for years of schooling.

Table 3 Difference in years of schooling within twin pairs (MZ twins only), by education levels

	Twin Pairs in Which at Least One Twin Has Education Category (%)							
Gender, Differences	1	2	3	4	5	6	7	Total
Males								
0	52.3	15.2	29.5	20.3	25.6	33.0	32.1	49.0
0 < diff. ≤ 1	2.3	15.2	25.3	43.5	25.0	10.6	0.0	17.9
1 < diff. ≤ 2	2.2	24.2	6.6	17.4	18.3	16.0	3.6	6.1
2 < diff. ≤ 3	4.0	6.1	4.9	4.3	11.0	6.4	3.6	3.8
3 < diff. ≤ 4	2.6	12.1	4.1	4.3	3.7	16.0	0.0	2.9
4 < diff. ≤ 5	4.6	18.2	3.5	1.4	1.8	4.3	3.6	2.7
diff. > 5	32.0	9.1	26.1	8.7	14.6	13.8	57.1	17.6
Mean	2.70	2.64	2.46	1.53	2.23	2.50	4.43	1.79
SD	3.14	1.89	2.74	1.89	2.70	2.88	3.61	2.62
N	650	33	712	69	164	94	28	1,293
Females								
0	58.7	22.2	36.8	11.4	26.5	43.2	18.2	58.1
0 < diff. ≤ 1	4.9	22.2	12.5	31.8	15.3	18.9	13.6	10.2
1 < diff. ≤ 2	3.1	11.1	6.0	18.2	14.8	5.4	0.0	5.7
2 < diff. ≤ 3	5.4	5.6	9.1	9.1	12.2	0.0	9.1	5.3
3 < diff. ≤ 4	3.6	11.1	5.9	6.8	8.5	18.9	4.5	3.6
4 < diff. ≤ 5	11.3	27.8	16.2	2.3	5.8	5.4	4.5	7.9
diff. > 5	13.1	0.0	13.5	20.5	16.9	8.1	50.0	9.2
Mean	1.84	2.35	2.37	2.45	2.69	1.98	4.46	1.50
SD	2.55	1.93	2.38	2.27	2.75	2.59	3.37	2.28
N	835	18	562	44	189	37	22	1,309
Males and Females Combined								
0	55.9	17.6	32.7	16.8	26.1	35.9	26.0	53.6
0 < diff. ≤ 1	3.8	17.6	19.6	38.9	19.8	13.0	6.0	14.0
1 < diff. ≤ 2	2.7	19.6	6.4	17.7	16.4	13.0	2.0	5.9
2 < diff. ≤ 3	4.8	5.9	6.8	6.2	11.6	4.6	6.0	4.5
3 < diff. ≤ 4	3.2	11.8	4.9	5.3	6.2	16.8	2.0	3.3
4 < diff. ≤ 5	8.4	21.6	9.1	1.8	4.0	4.6	4.0	5.3
diff. > 5	21.3	5.9	20.6	13.3	15.9	12.2	54.0	13.4
Mean	2.22	2.54	2.42	1.89	2.47	2.35	4.44	1.64
SD	2.85	1.89	2.59	2.09	2.73	2.80	3.47	2.46
N	1,485	51	1,274	113	353	131	50	2,602

Notes: Education categories are 1 = Compulsory schooling/Basic schooling; 2 = Upper secondary schooling/High school diploma; 3 = (3–4 years of) vocational training; 4 = Short-cycle higher education; 5 = Medium-cycle higher education; 6 = Master's and PhD degrees; and 7 = Unknown (which, for the calculations in this table was replaced with the minimum required years of schooling, 7 years for these cohorts).

the difference in years of schooling separately for all twin pairs in which at least one member attained one of the following broad educational categories[20]: (1) compulsory schooling/basic schooling; (2) upper secondary schooling/high school diploma; (3) (3–4 years of) vocational training; (4) short-cycle higher education; (5) medium-cycle higher education; (6) master's and Ph.D. degrees; (7) unknown educational levels were replaced with the minimum required years of schooling (= 7 years for these cohorts). The fraction of twin pairs for which the within-twin pair difference in years of schooling equals zero is highest in the lowest educational category (category 1 in Table 3) at about 56%, and it is considerably below 50% in the remaining educational groups. This high fraction of twin pairs with zero schooling difference in the first category is due to the fact that in these cohorts, a relatively large number of twins completed the legal minimum of seven years of schooling (compulsory schooling only), resulting in a years-of-schooling difference of zero for about 56% of all twin pairs in which at least one twin has only compulsory/basic schooling. Nevertheless, in the remaining 44% of these pairs, the schooling difference is sizable, and the mean difference in the years of schooling in these pairs is 2.2 (SD = 2.9). The difference in the years of schooling within twin pairs continues to average between about 1.9–2.5 (with SDs ranging from 1.9 to 2.8) for twin pairs in which at least one twin has attained educational categories 2 through 6, and the mean and standard deviation are slightly higher among the small number of twin pairs in which at least one twin has unknown education.

The clustering of small differences in schooling among twin pairs in which at least one twin has only compulsory education, and the fairly sizable within-twin pair variation for twin pairs in which at least one twin attained more than compulsory education, imply that our estimates are ATE for about the 70% of the sample with more than seven years of schooling; this contrasts with LATE estimates for schooling close to compulsory levels that would be obtained with IV estimates using as instruments increases in compulsory schooling levels.

A related issue in assessing the properties of within–MZ twin pair estimators pertains to measurement error in schooling because there may not be much information in the within-MZ schooling differences if measurement error is large. As is well-known, the bias toward zero owing to random measurement error generally is exacerbated in fixed-effects estimates, of which within-MZ estimates are one example. Assume that measured schooling (S_{ij}') is linearly related to true schooling (S_{ij}) but is measured with random measurement error w_{ij}: $S_{ij}' = S_{ij} + w_{ij}$. Bishop (1976) and Griliches (1979) showed that if measurement error is not correlated across siblings,[21] the bias toward zero in $b_S{}^w$, the estimated within coefficient β_S, is plim $b_S{}^w = \beta_S[1 - \sigma^2(w_{ij}) / (\sigma^2(S_{ij})(1 - \rho_S))]$, where ρ_S is the correlation in schooling

[20] This approach is advantageous relative to an alternative tabulation of schooling differences by average twin pair schooling level because by construction, the mean difference in the years of schooling will tend to become small for twin pairs that have either a very high or very low mean schooling level.

[21] If the correlation in measurement error between siblings (ρ_w) is nonzero, plim $b_S{}^w = \beta_S[1 - \varphi\sigma^2(w_{ij}) / \sigma^2(S_{ij})]$, where $\varphi = (1 - \rho_w)/ (1 - \rho_S)$. Note that the measurement error bias in the within-sibling estimate is decreasing in ρ_w and is less in the within-sibling estimate than in the standard estimate if $\rho_w > \rho_S$. We are not aware of any estimates of ρ_w. What appears to be random noise in cross-sectional data may have a family component if the measurement error is due to such unobserved factors as exaggeration or modesty or to failure to control for school quality, all of which may be shared by siblings.

Table 4 Implications of random measurement error for individual, within-DZ, and within-MZ estimates

Noise-to-Signal Ratio	Biases Toward Zero in Estimated βs (percentages)			Ratio of estimated βs Estimated Owing to Measurement Error Biases Alone	
$\sigma^2(w_{ij})$ / $\sigma^2(S_{ij})$	Individual	Within-DZ	Within-MZ	Within-DZ/ Individual	Within-MZ/ Individual
(1)	(2)	(3)	(4)	(5)	(6)
0.02	2	4	5	0.98	0.97
0.04	4	7	11	0.96	0.93
0.06	6	11	16	0.95	0.89
0.08	8	15	22	0.93	0.85
0.10	10	19	27	0.91	0.81
0.12	12	22	32	0.88	0.77
0.14	14	26	38	0.86	0.72
0.16	16	30	43	0.84	0.68
0.18	18	33	49	0.81	0.63
0.20	20	37	54	0.79	0.57

Notes: Based on the expression for measurement error in the text, with $\rho = 0$ for individuals, 0.46 for DZ twins, and 0.63 for MZ twins. (MZ and DZ correlations are estimated from the twins data used in this study).

between siblings (which is zero in standard individual estimates). This bias toward zero owing to measurement error is likely to be greater for within-DZ estimates than for individual estimates and for within-MZ estimates than for within-DZ estimates because ρ_S is likely to be positive and greater for MZ than for DZ twins.

Table 4 gives some illustrations, with each row representing a different noise-to-signal ratio ($\sigma^2(w_{ij})/\sigma^2(S_{ij})$) as given in column 1; the percentage biases in individual, within-DZ, and within-MZ estimates owing to measurement error are in columns 2–4, and the ratios of the coefficients from DZ estimates and MZ estimates to individual estimates owing to measurement error are in columns 5 and 6. Other twins studies that have reports from other respondents (i.e., the other member of a twin pair, the twins' adult children) so that they can estimate measurement error models found estimated noise-to-signal ratios of 0.06–0.12 (Ashenfelter and Krueger 1994; Ashenfelter and Rouse 1998; Behrman et al. 1994). Therefore, the row with a noise-to-signal ratio of about 0.10 in Table 4 is suggestive of the extent of biases from measurement error alone in these studies and of how they differ across the three types of estimates: 10% for individual estimates, 19% for within-DZ estimates, and 27% for within-MZ estimates. Thus, fairly substantial drops in the coefficient estimates for the within-DZ and within-MZ estimates occur because of measurement errors of this magnitude even if there are no biases from unobserved endowments. These measurement error biases would result in the coefficient estimates for the within-DZ and within-MZ estimates being, respectively, 9% and 19% smaller in absolute magnitude than those for the individual estimates (columns 5 and 6).

We infer, however, that measurement error is likely to be lower for our study than for the studies mentioned in the previous paragraph for three reasons. First, the self-reported schooling data are from the 1970 census, when the respondents were of ages at which their recall probably was more accurate than if they had been asked to recall their schooling more than 30 years later in 2002 at the end of the period for our health data, as in data sets that start with older individuals. Second, for 48% of the MZ twins in the sample, the administrative data on schooling that we use presumably are measured with very low error. Third, for this same subsample, we also have self-reported data, so we can estimate the measurement error relation for this subsample: the estimated noise-to-signal ratio is 0.04. If we are conservative and assume that this noise-to-signal ratio applies to the whole sample (even though we have administrative data for almost one-half of the sample), then measurement error biases would result in the coefficient estimates for the within-DZ estimates being 4% and those for the within-MZ estimates being 7% smaller in absolute magnitude than those for the individual estimates (columns 5 and 6).

Estimates

To answer the questions posed in the introduction, we apply the methodology outlined earlier to the special data described in the Data section. Thereby, we attain alternative estimates of the gross reduced-form relations between years of schooling attainment that are ATE estimators for the approximately 70% of the sample with more than seven years of schooling and (1) hospitalization per year of exposure between 1980 and 2002 (or until mortality if mortality occurred prior to 2003), (2) hospitalization per year of exposure between 1980 and 2000 (or until two years prior to mortality if mortality occurred prior to 2003), and (3) mortality prior to 2003. As noted in the introduction, we initially estimated our analyses separately for men and women in each of two cohorts (1921–1935, 1936–1950). We then tested whether the analyses can be pooled across cohorts and gender. Except for the mortality outcomes, the statistical tests suggest that the analyses can be pooled across both gender and cohorts. For hospital days, we therefore present estimates for the combined sample for men and women born in 1921–1950; and for mortality, we present gender-specific estimates for each of the two cohorts.

Tables 5 and 6 give our estimated schooling coefficients for days hospitalized per year of exposure during 1980–2002 (Table 5, top panel); days hospitalized per year of exposure up to two years before death during 1980–2000 (Table 5, bottom panel); and for the probability of mortality prior to 2003 (Table 6) for the 5% random sample of singletons, MZ twins, and DZ twins. All the estimates control for birth-year fixed effects and additive gender differences. The twins sample standard estimates control for within–twin pair clustering. Robust standard errors are reported for all analyses.[22] The estimates in these tables permit us to address the questions posed in the introduction:

1. What are the cross-sectional associations between schooling attainment and hospitalization and mortality for Danish adults in the 1921–1950 birth cohort?

[22] The specifications for these estimates are all linear. The basic thrust of the results summarized below is robust to using a semi-log specification (because the distributions are skewed to the right) for the two hospitalization outcomes. See Tables 8 and 9 in the Appendix.

Table 5 Estimates of schooling coefficients for days hospitalized

		Standard			Within Twins		Within/Standard	
Dependent Variable		5%	MZ	DZ	MZ	DZ	MZ	DZ
Number of Hospital Days per Year	Coefficient	−0.056	−0.060	−0.053	0.005	−0.008	−0.083	0.145
	SE	0.005	0.020	0.012	0.034	0.028		
	t Statistic	−12.09	−3.06	−4.31	0.15	−0.28		
Number of Hospital Days per Year, up to 2 Years Prior to Death (or end of observation period)	Coefficient	−0.042	−0.050	−0.041	0.000	−0.027	0.010	0.650
	SE	0.003	0.012	0.008	0.025	0.014		
	t Statistic	−13.10	−4.03	−4.92	−0.02	−1.84		

These associations are given in the first column of Tables 5 and 6 for the 5% sample. They are significantly negative, consistent with the dominant finding in the literature, and fairly substantial for all three outcomes. The 5% sample estimates indicate that a one SD increase in schooling attainment is associated with declines of 9.8% of the mean days hospitalized per year prior to 2003, or death if that occurs earlier; of 7.4% of the mean days hospitalized per year prior to two years prior to death; and of 6.8%–20.7% in the mean probability of mortality by 2003 (Table 7, column 1). The associations are similar (and apparently not statistically different from those for the 5% sample) for the MZ and DZ samples, although less precisely estimated (but still

Table 6 Linear probability estimates of schooling coefficients for mortality prior to 2003

		Standard			Within Twins		Within/Standard	
Gender, cohort		5%	MZ	DZ	MZ	DZ	MZ	DZ
Males, 1921–1935	Coefficient	−0.0084	−0.0108	−0.0076	0.0051	−0.0071	−0.471	0.935
	SE	0.0010	0.0046	0.0031	0.0084	0.0052		
	t Statistic	−8.58	−2.33	−2.47	0.60	−1.35		
Males, 1936–1950	Coefficient	−0.0070	−0.0056	−0.0091	−0.0012	−0.0035	0.218	0.383
	SE	0.0006	0.0022	0.0016	0.0040	0.0027		
	t Statistic	−11.44	−2.53	−5.75	−0.31	−1.28		
Females, 1921–1935	Coefficient	−0.0085	−0.0099	−0.0060	0.0127	−0.0006	−1.282	0.095
	SE	0.0010	0.0039	0.0031	0.0079	0.0055		
	t Statistic	−8.09	−2.52	−1.93	1.61	−0.10		
Females, 1936–1950	Coefficient	−0.0047	−0.0032	−0.0047	−0.0015	−0.0059	0.476	1.248
	SE	0.0005	0.0021	0.0014	0.0038	0.0026		
	t Statistic	−8.71	−1.52	−3.30	−0.40	−2.27		

Table 7 Estimated percentage changes with 1 SD increase in schooling attainment

	5% Sample	MZ Twins Within	DZ Twins Within
Hospital Days/Exposure	−9.8	1.0	−1.5
Hospital Days/Exposure < 2y Death	−7.4	−0.1	−5.0
Mortality by 2003			
Males, 1921–1935	−6.8	5.2	−6.0
Males, 1936–1950	−20.7	−4.5	−10.1
Females, 1921–1935	−8.4	16.3	−0.6
Females, 1936–1950	−19.6	−7.3	−27.6

Note: Based on means and SDs from Table 1 and estimates from Tables 5 and 6.

significantly nonzero at the .05 level in 10 of the 12 cases and at the .10 level in 11 of the 12 cases).

2. What are the estimated causal impacts of schooling on hospitalization and mortality for these individuals when the MZ-twin design is used to control for unobserved family endowments? The within-MZ estimates (Tables 5 and 6, column 4) are insignificant at the .10 level for both measures of hospitalization per year in the pooled analyses, and in all four gender/cohort analyses for mortality by 2003. These estimates might be consistent with a true positive effect of schooling on health, but this seems unlikely. In particular, one possibility consistent with a true positive effect of schooling on health is that a systematic persistent factor affects schooling and health in opposite directions, as discussed at the end of the Methodology section. Although it is conceivable that unobserved persistent shocks affect health and schooling in the opposite direction, what generally are considered to be more plausible shocks are likely to affect health and schooling in the same direction (e.g., *in utero* environments or later illnesses or accidents that reduce both schooling and health)—and that would mean that the within-MZ estimate of the impact of schooling on health is upwardly biased. A second possibility is that measurement error bias toward zero, which is exacerbated by within-MZ estimates as is discussed at the end of the Data section, accounts for the differences. However, our conservative estimate of the extent of measurement error in our data presented at the end of the Data section would account for a drop of about 7% in the estimated coefficient between the individual and within-MZ estimates in contrast with the much larger drops that we obtain: 93% to 99% for days hospitalized, and 53% to 79% for the two estimates for mortality that do not change sign. A third possibility emerges in light of the Lundborg (2008) results of significant nonlinear school effects for the United States in contrast with the Fujiwara and Kawachi (2009) results of generally no significant casual effects with a continuous schooling measure using the same data: there are significant

nonlinear effects for our Danish data that are obscured with our linear specification. If nonlinearities are introduced through dichotomous variables for completing different schooling levels, the standard cross-sectional estimates suggest some strong differences in outcomes across these educational categories. However, with the within-twin estimates, the null hypothesis of all schooling coefficients equaling zero (and therefore no differences across completed schooling categories) is not rejected (Table 9 in the Appendix). Thus, our basic results do not seem to be an artifact of unobserved factors that affect schooling and health in the same direction, measurement errors of the magnitudes estimated in other studies, or nonlinearity in schooling effects of the sort that apparently underlie the differences for the United States between the significance of the within-MZ results in Lundborg (2008) and Fujiwara and Kawachi (2009) using the same data set.

3. To what extent do these within–MZ twins estimated impacts differ from what cross-sectional associations suggest? Column 6 in Tables 5 and 6 gives the ratio of the within-MZ point estimates to the MZ cross-sectional point estimates. A ratio of 1 indicates that the schooling coefficient estimate is not affected by using the within estimate rather than the standard estimate. A negative sign indicates that the estimate for the within-MZ case is opposite in sign from that for the MZ standard estimate. This comparison emphasizes that the implications of the within-MZ estimates in a number of cases are very different from those of the cross-sectional MZ associations. Three of the six ratios in Tables 5 and 6 are negative, and none of the ratios is between 0.5 and 1.5: that is, in none of the cases is the standard estimate within 50% of the within-twin estimate. Of course, a number of the underlying point estimates are estimated very imprecisely, so the differences between the two estimators are not always statistically significant. If one considers the MZ standard estimates in column 2 in Tables 5 and 6 in isolation, though, all the point estimates are negative, and all but one are so at the standard .05 significance level; this is similar to, although less precise than, the pattern in column 1 for the 5% sample estimates. These negative estimates in columns 1 and 2, if a causal interpretation is given, presumably lead to a conclusion that more schooling leads to less hospitalization and lower mortality by 2003. In contrast, if one considers the within-MZ estimates in column 4 in Tables 5 and 6 in isolation, none of the six estimated coefficients is significantly different from zero at the .10 level, and three of six have positive signs (for hospitalization per year up to two years before death, and for mortality prior to 2003 for both males and females in the 1921–1935 cohort).[23] The conclusion about the impact of schooling based on the within-MZ estimates is therefore much different than with the standard estimates (also see column 1 versus column 2 in Table 7).

[23] With the alternative specifications in the Appendix (Tables 8 and 9), the differences between the cross-sectional estimates and the within-MZ estimates appear even stronger. For example, for the two measures of hospitalization per year both of the cross-sectional estimates are significantly negative at high levels, but in the within-MZ estimates, both of these are positive, although not significant at the .05 level.

4. To what extent do within–MZ and within–DZ twin estimates differ, as they will if individual-specific endowments (as contrasted with common family endowments) are important? Once again, a number of the estimates are imprecisely estimated, and the differences between the within-MZ and within–DZ twin estimates in Tables 5 and 6 are not statistically significant. However, there is a tendency—that holds in all but one of the six cases in Tables 5 and 6—for the within–DZ twin estimates to fall between the standard cross-sectional estimates and the within-MZ estimates. Thus, the patterns in the estimates suggest that individual-specific endowments may be playing a role in the DZ-within estimates, particularly because the within-DZ estimates look more like the standard estimates than do the within-MZ estimates (i.e., with more negative coefficient estimates for the within-DZ estimates, including the two that are significant at the .10 level).

Conclusions

We contribute to the literature by obtaining—together with the recently published study by Fujiwara and Kawachi (2009) and unpublished studies by Amin et al. (2010), Behrman et al. (2006), and Lundborg (2008)—among the first estimates of the impact of schooling on health using the within–MZ twins methodology to control for unobserved endowments. We use objective health measures, over long time periods for hospitalization, from Danish administrative hospital discharge records and the Danish national mortality register. Our estimates arguably are closer to average treatment effect (ATE) estimates for most of the sample that have more than seven years of schooling than are most of the instrumental variable (IV) estimates that recently have been used to address this question and that tend to be local average treatment effects (LATE) estimates because they depend on instruments such as school reform that change compulsory schooling levels.

Our results are very suggestive for the questions that are posed in the introduction. They imply, in summary, strong negative associations between schooling attainment and hospitalization per year during 1980–2002 and hospitalizations per year up to two years before death and mortality by 2003 for both males and females for both the 1921–1935 and 1936–1950 birth cohorts. However, our estimates also question the standard inference from other, similar estimates that these strong negative associations reflect important direct causal effects of schooling. Instead, schooling seems primarily to serve as a marker for parental family and individual-specific endowments that are uncontrolled in the usual estimates. This raises questions about the usual attribution to schooling of substantial positive effects on health-related behaviors and outcomes and about whether there is an important causal schooling-health gradient. In terms of causal effects, despite the strong associations with schooling, the real stratification appears to be with regard to parental family and individual-specific background endowments. Better endowments apparently tend to lead to more schooling, less hospitalization, and less mortality by 2003, but the resulting negative associations between schooling on one hand and hospitalization and mortality on the other do not appear to reflect significant causal effects of schooling in reducing hospitalization and mortality in the context studied. Our results are similar to estimates for a subset of the same sample that significant cross-sectional associations among individual adult social class, health, and mortality and education were not significant in within-twin estimates (Madsen et al. 2010; Osler et al. 2007). Our results contrast with those presented in the recent study using a very similar methodology by Lundborg (2008), who found that using the within–MZ twins approach did not suggest that schooling was largely a proxy for family background in the United States if schooling was allowed to have nonlinear effects. However, functional form apparently is important for the Lundborg results because Fujiwara and Kawachi (2009) did not find significant causal effects of schooling on health using the same data as Lundborg (2008) but with a continuous representation of schooling. In contrast, we find no significant effect of schooling on health whether we use a nonlinear representation of schooling (as did Lundborg) or a continuous measure (as did Fujiwara and Kawachi).[24]

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Appendix

Table 8 Estimates of schooling coefficients for days hospitalized with semi-log specification

		Standard			Within Twins		Within/Standard	
Dependent Variable		5%	MZ	DZ	MZ	DZ	MZ	DZ
Number of Hospital Days per Year	Coefficient	−0.0159	−0.0118	−0.0151	0.0084	−0.0079	−0.708	0.521
	SE	0.0007	0.0030	0.0020	0.0052	0.0035		
	t Statistic	−22.300	−3.990	−7.720	1.610	−2.230		
Number of Hospital Days per Year, up to 2 Years Prior to Death (or end of observation period)	Coefficient	−0.0133	−0.0110	−0.0123	0.0054	−0.0074	−0.495	0.603
	SE	0.0006	0.0026	0.0017	0.0048	0.0029		
	t Statistic	−21.320	−4.240	−7.230	1.130	−2.520		

[24] Amin et al. (2010) also reported no significant effect of schooling regardless of whether a nonlinear or continuous measure of schooling was used.

Table 9 Estimates of nonlinear schooling effects using dummy variables for completed schooling levels

		Standard			Within Twins	
Dependent Variable		5%	MZ	DZ	MZ	DZ
Number of Hospital Days per Year						
Education category[a]						
Upper secondary schooling/High School diploma	coefficient	−0.193	−0.623	0.027	0.400	0.913
	SE	0.135	0.267	0.905	0.309	1.054
	t Statistic	−1.440	−2.340	0.030	1.290	0.870
(3–4 years of) Vocational training	coefficient	−0.232	−0.352	−0.188	0.028	0.050
	SE	0.036	0.141	0.094	0.225	0.165
	t Statistic	−6.460	−2.490	−2.000	0.120	0.300
Short-cycle higher education/Two-year college degree	coefficient	−0.407	−0.486	0.156	0.036	0.705
	SE	0.076	0.220	0.681	0.279	0.851
	t Statistic	−5.330	−2.210	0.230	0.130	0.830
Medium-cycle higher education/Three years college or university degree	coefficient	−0.488	−0.488	−0.522	0.141	−0.236
	SE	0.046	0.198	0.107	0.306	0.235
	t Statistic	−10.630	−2.470	−4.860	0.460	−1.010
Master's and PhD degrees/Five-year university degree and PhD graduates	Coefficient	−0.723	−0.701	−0.887	0.116	−0.551
	SE	0.065	0.251	0.139	0.388	0.435
	t Statistic	−11.210	−2.790	−6.370	0.300	−1.270
Unknown educational level (mostly immigrants or ongoing basic schooling)	Coefficient	0.401	0.146	−0.066	−0.049	0.286
	SE	0.185	0.476	0.437	0.738	0.555
	t Statistic	2.170	0.310	−0.150	−0.070	0.520
F Statistic for null hypothesis that all schooling coefficients are equal to zero		35.49	2.45	9.38	0.39	0.94
p Value		<.01	0.02	<.01	.89	.46
Number of Hospital Days per Year, up to 2 Years Prior to Death (or end of observation period)						
Education category[a]						
Upper secondary schooling/High school diploma	Coefficient	−0.234	−0.503	0.514	0.199	1.199
	SE	0.111	0.187	1.098	0.270	1.264
	t statistic	−2.100	−2.700	0.470	0.740	0.950
(3–4 years of) Vocational training	Coefficient	−0.177	−0.278	−0.175	0.000	−0.070
	SE	0.025	0.089	0.065	0.172	0.093
	t Statistic	−7.100	−3.130	−2.720	0.000	−0.760
Short-cycle higher education/Two-year college degree	Coefficient	−0.294	−0.281	−0.500	0.028	−0.275
	SE	0.056	0.179	0.089	0.254	0.161
	t Statistic	−5.290	−1.570	−5.640	0.110	−1.710
Medium-cycle higher education/Three years college or university degree	Coefficient	−0.359	−0.372	−0.357	0.154	−0.375
	SE	0.032	0.120	0.082	0.231	0.190
	t Statistic	−11.120	−3.090	−4.330	0.670	−1.970
Master's and PhD degrees/Five-year university degree and PhD graduates	Coefficient	−0.517	−0.603	−0.610	−0.187	−0.216

Table 9 (continued)

Dependent Variable		Standard 5%	Standard MZ	Standard DZ	Within Twins MZ	Within Twins DZ
	SE	0.045	0.171	0.121	0.266	0.189
	t Statistic	−11.390	−3.530	−5.050	−0.710	−1.140
Unknown educational level (mostly immigrants or ongoing basic schooling)	Coefficient	0.318	0.002	−0.226	0.384	0.197
	SE	0.163	0.326	0.227	0.371	0.290
	t Statistic	1.950	0.010	−0.990	1.030	0.680
F statistic for null hypothesis that all schooling coefficients are equal to zero		37.06	3.42	8.66	0.8	1.27
p Value		<.01	<.01	<.01	.80	.27

[a] Reference category is basic and compulsory primary and upper secondary schooling.

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