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The Health Returns to Schooling – What Can We Learn from Twins?

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Abstract

This paper estimates the health returns to schooling, using data on identical twins. In addition, it provides new evidence on the determinants of schooling differences within twin pairs and to what extent such differences can be regarded as exogenous, the latter being a crucial assumption in the twin-differences literature. The results suggest that completing high school improves health, as measured through self-reported health and chronic conditions, but that further schooling does not improve health. Moreover, the results suggest that early life factors that may vary within twin pairs, such as birth weight, early life health, time preferences, personality, parental treatment, and parent-child relations, in most cases do not predict within-twin pair differences in schooling.

JEL Classification: I12, I11, J14, J12, C4

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There is a long tradition in economics to estimate the private returns to schooling, as reflected through the effect of additional schooling on earnings. If schooling affects social well-being beyond its effect on earnings, however, these estimates will only partially capture the total returns to schooling. Evidence is now mounting that schooling is associated with several non-market outcomes, such as health, fertility choices, and crime (see Wolfe and Haveman 2002 and Grossman 2006 for recent overviews). Accounting for such outcomes may lead to different conclusions regarding the individual and social values of schooling.

There has recently been a growing interest in the health returns to schooling. Schooling is strongly associated with a range of different health measures and the relationship has been observed in a large number of countries and time periods (Cutler and Lleras-Muney 2006). If this relationship reflects a causal effect of schooling on health, important policy implications could follow. Increased expenditures on education may then be a cost effective way to improve population health, compared to other means, such as increased health care expenditures. Whether schooling has a causal effect of health remains much disputed, however.

In this paper I make two distinct contributions to the literature. First, I estimate the effect of schooling on health, using a twin-differences design. By relating within-twin-pair differences in schooling to within-twin-pair differences in health and health behaviors, I am able to difference out the influence of genetic traits and family endowments that may otherwise bias the schooling coefficient. To the best of my knowledge, there is to date no previously published study using a twin-differences design to study this topic.[1]

Second, I investigate the determinants of within-twin-pair differences in schooling. A crucial assumption in the twin-differences literature is that such differences in schooling are

[1] In parallel work, Behrman et al. (2006a) and Behrman et al. (2006b) are considering the effect of schooling on health outcomes, using data on Danish and Chinese twins, respectively.

exogenous. This assumption remains much debated and there is still scarce evidence on the role that various factors play for the schooling achievement of identical twins. Using rich and unique data on early-life circumstances and characteristics of twins, I am able to address this issue and provide new evidence on the validity of the twin-differences design.

What new knowledge could a twin-differences approach bring to the literature on education and health? A number of recent studies have relied on various natural experiments, such as schooling reforms, to estimate the causal effect of education on health (see Grossman 2006 for an extensive overview). These studies rely on natural experiments that affect individuals whose return to schooling is likely to be different from the average returns in the population. Changes in mandatory schooling laws, for instance, were typically intended to increase the schooling of those at the lower end of the education distribution, while having little or no effect on those planning to proceed to further studies anyway. Since the resulting estimates therefore reflect Local Average Treatment Effects (LATE), the degree to which the results could be generalised may be questioned. Several of these recent studies, reviewed in greater detail in Section 2, also face a problem of weak instruments, yielding imprecise and inconsistent estimates.

A twin-differences design may overcome some of these problems. Twin differences as a "natural experiment" relies on the existence of differences in schooling within identical twin pairs. If such differences are equally distributed across twin pairs, the resulting estimate would come close to reflect an Average Treatment Effect (ATE). Moreover, a twin-differences strategy does not rely on any weak and often doubtful instrument. I believe that a twin-differences strategy therefore has the potential to provide new knowledge about the causal effect of schooling on health.

While a twin-differences design has some distinct advantages, it also brings problems of its own. The main criticism of twin studies has been that differences in education within twin pairs may not be exogenously given, i.e. that while twin differencing will remove the influence of unobserved factors *common* to a twin pair, there may still remain within-twin-pair differences in unobserved factors that affect schooling. Bound and Solon (1999) showed that any differences between the twins that are not removed in a twin-fixed-effects model could potentially even increase the endogeneity bias compared to OLS estimates.[2] As a candidate for such within-twin-pair differences, Bound and Solon (1999) mention birth weight, since even identical twins may differ in birth weight.[3] It should be noted, however, that there is to date no conclusive evidence as to whether differences in birth weight are also associated with within-twin-pair differences in schooling. In Bonjour et al. (2003) and Miller et al. (2005), no signicant effects of birth weight on educational attainment are obtained. On the other hand, Behrman and Rosenzweig (2004) and Royer (2009) found significant effects.

Besides birth weight, there may be a host of other reasons why even identical twins may turn out different in terms of schooling. Twins may, for instance, be treated differently by their parents and therefore end up with different schooling levels. Since identical twins may differ in IQ, for instance, parents may try to compensate for such differences or amplify them by investing more or less in the more able twin. While such parental treatment has not been observable in the previous twin-differences literature, I am able to exploit rich information on parental treatment and relations in early life along a number of dimensions, such as time inputs, strictness, and physical abuse. Moreover, I am able to consider how differences in early life health, as measured through birth weight and early physical and mental health, relates to differences in schooling among identical twins. This is of importance, since there

[2] To see this, first note that the ability bias is determined by the ratio of exogenous variation to total variation. *If* differencing reduces the fraction of exogenous variation, ability bias may increase.

[3] There is to date mixed evidence as to whether differences in birth weight are also associated with within-twin-pair differences in schooling (see e.g. Behrman and Rosenzweig 2004; Miller 2005; Royer 2009).

may exist an inverse relation between education and health, where early life health affects educational attainment. In addition, differerence in time preferences between twins may explain why even identical twins end up with different schooling levels. I will consider this possibility by exploiting detailed questions about attitudes towards the future. Finally, I am able to assess the role of factors such as classroom placement, peer groups, and personality for attained schooling. All in all, this allows me to investigate the "exogeneity" assumption in much greater detail compared to the previous literature using a twin-differences design, such as Ashenfelter and Kreuger (1994), Ashenfelter and Rouse (1998), and Bonjour et al. (2003).

Finally, twin-differencing raise the issue of measurement errors in reported schooling. If individuals misreport educational attainment, the role of such errors is exaggerated by differencing, and even more so when differencing between identical twins, causing the estimate of schooling to be downward biased (Griliches 1979). The typical solution, proposed by Ashenfelter and Kreuger (1994), is to instrument for schooling, using a co-twin's report on the other twin's schooling. While I am able to address the issue of within-twin pair differences in factors potentially associated with within-twin pair differences in schooling, the data does not allow me to address the measurement error problem by instrumenting. In the literature on the health returns to schooling, however, it still remains to settle whether or not there exists a causal effect *at all*. For my purposes it is therefore more important to address the potential upward bias in the estimates due to omitted variables than any potential downward bias caused by measurement errors. I will, however, make use of previous estimates of the role of measurement errors in order to get an idea of the likely downward bias in the estimates.

My results suggest a causal effect of schooling on health. Relative to high school dropouts, people with greater schooling are significantly healthier, as measured through self-reported health and chronic conditions. No evidence of any additional positive effect of post-high school education is obtained, however. I find similar effects for physical activity but not for smoking and overweight. For years of schooling, the twin-differences point estimates are close to the OLS point estimates but not significant.

I find little evidence that some of the most commonly suspected variables causing difference in schooling within twin pairs actually do so. Following the approach of Ashenfelter and Rouse (1998) and Bonjour et al. (2003), I first estimate the correlation between average twin-pair schooling and average twin-pair early life circumstances and characteristics. This gives me an indication of the expected bias in the "between-twin-pair"-regressions. I then compare these estimates to those obtained from regressions on within-twin-pair differences in schooling on within-twin-pair differences in the same early life circumstances and characteristics. The comparison reveals to what extent bias is removed in the within-twin-pair regressions. My results suggest that very few of the factors that may vary within twin pairs, such as birth weight, time preferences, early life health, parental treatment, parental relations, and personality are associated with differences in schooling *within* twin pairs, whereas many of the same factors are significantly associated with differences in schooling *between* twin pairs. This suggests that my twin-differences estimates are less biased than estimates from a pooled cross-section of twins.

I start the paper by giving some theoretical background and discuss some recent findings. I then discuss the data and compare it to data from CPS in order to assess its generalizability. Next, I discuss the empirical model. I then report the results, where the results from the pooled twin sample are contrasted with those obtained when applying a twin-differences strategy. Finally, the results are discussed and some conclusions are drawn.

I Background

There are basically three ways in which the link between schooling and health has been explained. First, educated people may be more efficient in producing their own health, suggesting a causal effect running from education to health. This is how schooling enters the demand-for-health model, where educated people obtain a larger health output from a given amount of health inputs (Grossman 1972). Schooling may also increase the allocative efficiency of health production, in which case educated people are able to pick a better mix of inputs (Rosenzweig and Schultz 1982; Kenkel 1991).

Second, schooling and health may be related through unobservables, such as family background and genetic traits. Fuchs (1982) proposed time preferences as such an unobserved variable, where less future-oriented people will invest less in both education and health than more future-oriented people, since the benefits of such investments are of long-run character. Labelling all such kind of unobserved factors "ability", their omission in a regression will bias the schooling coefficient. Third, health may affect educational attainment. Poor health early in life may intervene with learning. Some evidence suggests, for instance, that low birth weight, being an early health marker, is associated with less schooling (Behrman and Rosenzweig 2004; Black et al. 2007).

The discussion above suggests that schooling may be endogenous. To deal with this, early studies used various instruments such as parental education (Berger and Leigh, 1989; Sander, 1995; Leigh and Dhir, 1997). The earliest study, Berger and Leigh (1989), used IQ, per capita income and per capita expenditures on education in the state of birth, and parental schooling as instruments. For several reasons, the exogeneity of these instruments may be questioned. IQ may be correlated directly with health and parental schooling may be correlated with child health, which in turn affects later life health. Moreover, state income and education expenditures may be correlated with health expenditures and other state characteristics that affect health.

The exclusion restrictions are easier to defend in a number of recent studies, utilizing various "natural experiments" to estimate the effect of schooling of health. Grossman (2006) identifies six such studies (Adams 2002; Spasojevic 2003; Arkes 2004; Arendt, 2005; Lleras-Muney, 2005; de Walque 2007). Since then, an additional number of studies using natural experiments have appeared, such as Kenkel et al. (2006), Oreopoulos (2006), Grimard and Parent (2007), and Chou et al. (2007).[4] In comparison with the extensive literature on the wage returns to education, the evidence base is thus small.

Five of the studies use educational reforms as a mean to identify the effect (Spasojevic, 2003; Lleras-Muney, 2005; Arendt, 2005; Oreopoulos 2006; Chou et al. 2007). The success of this strategy has varied, however, since, among other things, several of the studies face a problem of weak instruments, leading to inconsistency of the IV-estimator. Arendt (2005) uses schooling reforms in Denmark in 1958 and 1975 that affected the entire population. This makes it difficult to distinguish between cohort effects and the influence of the schooling reform. Moreover, the study suffered from weak instruments and when instrumenting for schooling, Arendt (2005) does not find any significant effect of schooling on health. This is similar to Lleras-Muney (2005), who also faced a problem of weak instrument when using individual-level data.[5] Spasojevic (2003) found a significant effect of schooling on health only after applying one-tailed tests.

Similar problems of weak instruments exist in studies using other types of natural experiments to study the schooling/health gradient. Adams (2002) adopts the strategy of Angrist

[4] Chou et al. (2007) consider the effect of parental education on child health.

[5] She found a significant and positive effect of schooling on health when using data on synthetic cohorts and instrumenting with state-level schooling reforms, though. Mazumder (2007) finds, however, that the results are not robust to the inclusion of state-specific trends.

and Kreuger (1991), using quarter of birth as an instrument for education. The F-values on the instruments are only about 1, indicating a problem of weak instruments. Unsurprisingly, no significant effect of schooling on health was obtained.

As discussed in the introduction, these studies identify local average treatment effects (LATE).[6] de Walque (2007), for instance, used the fact that college enrollment was one way to avoid being drafted for the Vietnam war. Risk of induction is then used as an instrument for going to college.[7] This means that the effect of schooling is only estimated for the subgroup of males that decided to go to college in order to avoid getting drafted. These individuals return to schooling may very well be different from the average returns in the population and the estimates thus represent local average treatment (LATE) effects.

The studies discussed above commonly find that instrumenting for schooling increases the magnitude of the effect, although the estimated effects in many cases are not significant. The increase in magnitude has been explained in two ways. First, the instruments are based on policy interventions that affect the educational attainment of people only at the lower end of the education distribution. The returns to education for this group is likely to be greater than for the population in general. Second, random measurement errors in the schooling variable lead to a downward bias in the OLS estimates. Instrumenting for schooling may help remedy this problem, as long as the instruments are not correlated with the error (Card 2001).

To summarize; while recent studies have provided new and interesting findings on the association between schooling and health, there still exists a great deal of uncertainty about the causal effect. Many of the studies in the literature suffer from low precision and iden-

[6] The exception is Oreoupoulos (2006), who claims that his estimate comes close to reflect an Average Treatment Effect. He exploits the change in minimum school leaving age from 14 to 15 in the United Kingdom that affected half of the population of 14-year-olds

[7] de Walque (2007) also uses an alternative instrument, indicating the risk of induction times the risk of being killed in the war.

tification being based on specific subgroups of the population, calling into question the representativeness of the results.

II Data

My estimates are based on data from the first wave of the Midlife in the United States (MIDUS) survey. The first wave collected data in 1995 on a total of 7,108 individuals. This baseline sample was comprised of individuals from four subsamples: (1) a national RDD (random digit dialing) sample (n=3,487); (2) oversamples from five metropolitan areas in the U.S. (n=757); (3) siblings of individuals from the RDD sample (n=950); and (4) a national RDD sample of twins (n=1,914). To be eligible for the survey, participants had to be non-institutionalized, English-speaking, living in the United States, and aged 25 to 74. The study was originally set up with the purpose of investigating the role of behavioral, psychological, and social factors in understanding age-related differences in physical and mental health.

The response rate for the telephone interviews in the first wave of MIDUS was 70%. Among these, 86.3% also completed a self-administered questionnaire, giving an overall response rate of 60.8%.

The twin sample consists of 1,914 twins, participating in the MIDUS Twin Screening Project. The recruitment of the twins followed a two-stage sampling design. In the first stage, a representative national sample of approximately 50,000 households was screened to identify families with twins. Respondents were asked whether they or any of their immediate family members were members of intact twin pairs. In 14.8% of the cases, the respondent reported the presence of a twin in the family. These respondents were then asked whether the research team was allowed to contact the twins in order to solicit their participation in the survey. About 60% of the respondents agreed and where subsequently enrolled in the MIDUS recruitment process.

Second, twin households were contacted and offered to participate in the MIDUS survey. Twins that agreed to participate were asked to provide contact information for the co-twin. In a smaller number of cases, several twin pairs per family existed. To be included in the MIDUS twin study, the respondent through which twins were identified had to be related to the twin by being a spouse or partner, a sibling, a child (also for the spouse or partner), or a father or mother. Moreover, the twins had to be between the ages 25 and 74. Both twins also had to have a residential phone number, excluding individuals living in prisons, nursing homes, and college dormitories etc. In addition, both twins had to live in continental US, both had to speak English, and both twins had to be mentally and physically able to do the interview.

Applying these eligibility criteria, almost half (49%) of the identified twin pairs were ineligible for the survey. The major reason (52%) was ineligibility due to the age criteria. The second most important reason was that the main respondent was not related to the identified twin according to the eligibility criteria (30%). In addition, 25% of the eligable twins were not included in the final sample for various reasons. The most common reason (41%) was that the interviewer was unable to reach the twin or contact person, whereas the second most important reason (32%) was that the twin or contact person refused to participate.

It should be noted that MIDUS was the first national sample of twins that was ascertained randomly via telephone. Using nationally representative data is an improvement compared to prior economic studies using twin data, such as Ashenfelter and Kreuger (1994) and Ashenfelter & Rouse (1998). These studies used highly selective data, collected during the Twinsburg twins festival. As noted by Ashenfelter and Rouse (1998), the twin pairs attending this festival may be more alike than a random selection of twin pairs, since the festival emphasizes the similarity of the twins and the pairs attend in similar clothes and hairstyles.

By using information collected as part of the initial twin screening questionnaire, twin pairs were diagnosed as identical or fraternal twins. The questions used in the diagnosis included, for instance, whether the twins had the same eye color, natural hair color, and complexion, whether individuals mistook them for each other when they were young, and whether they had ever undergone testing or been told by a doctor whether they were genetically identical or fraternal. Based on their answers to the questions, the twins were assigned points, which were subsequently totaled. "High" scores indicated identical twin pairs and "low" scores indicated fraternal twin pairs. In a small number of cases, the pair's score fell in the middle of the range and no diagnosis was given. This method of diagnosing twin zygosity has proven reliable and has shown to be over 90% accurate in diagnosing twin zygosity (e.g., Nichols and Bilbro, 1966).

Out of the 1,996 twins, 32 twins were dropped due to uncertainty regarding zygosity. Of the remaining twins, 734, or 37%, were identical twins, which were then selected for the analysis. I dropped 3 twins who had yet not finished their education. In addition, I dropped 19 twins where id-numbers were lacking and 18 twins where information on the co-twin was lacking. This resulted in a final sample size of 694 identical twins.

A *Explanatory variables*

Educational attainment was measured in 12 categories in MIDUS, ranging from no school/some grade school to PhD. For my analyses, I categorized this variable into four categories: college degree, college but less than a BA degree; a high school diploma; less than a high school diploma.

While years of schooling has been a common measure in many prior studies on the wage returns of education, there are several reasons for also considering educational categories. First, it is not straightforward how to impute years of schooling from categories. Since measurement errors would inevitably increase from such as a procedure, this would accentuate the measurement error problem, which is already serious when using a twin-differences approach. Second, in terms of health, the educational degree may be as relevant, or even more relevant, as years of schooling. In de Walque (2007), for instance, there is a sharp increase in the effect of number of years of schooling on smoking, once reaching college. Similar evidence for non-linear effects have been obtained in the literature on the wage returns to education (Hungerford and Solon 1987; Belman and Haywood 1991; Isacsson 2004). Based on such findings, some economists argue that credentials matter more than years of schooling (for a discussion on this, see Card 1999). I will also, however, provide estimates based on years of schooling, using the imputations suggested by Jaeger (1997).

In line with previous studies using a twin-differences design, the twins in MIDUS often end up with similar educational attainment. Using the educational categories described above, 67% of the identical twins in MIDUS report the same level of education. For imputed years of schooling, 42% reported the same number of years.

In the regressions using educational categories, I use the category less than a high school diploma as the omitted reference category. This category in principle indicates being a high school dropout. Besides schooling, the regressions control for age, gender, marital status, race, total household income,and retrospective self-assessed health at age 16. In the twin fixed effects regressions, age, race, and gender were not included, since it does not differ between the twins. The latter was obtained by summarizing all sources of income for all members in the household.

B Health outcomes

My two main measures of health is self-assessed health and the number of chronic conditions. The former was assessed through the following question: "Using a scale from 0 to 10 where 0 means "the worst possible health" and 10 means "the best possible health," how would you rate your health these days?" Self-assessed health has been found to be a strong predictor of subsequent mortality (see for instance Idler and Benyiamini 1997). There are some concerns, however, about the interpretation of the responses. Older individuals often report similar self-reported health as younger persons, despite having "objectively" worse health (Groot 2000). I will therefore also consider a more "objective" health measure, measuring the number of chronic conditions.

Besides health measures, I will also examine measures of lifestyle. These are smoking, Body Mass Index, and physical exercise. Physical exercise is measured through the number of occasions during past month that the individual engages in vigorous physical activity.

C Representativeness of the sample

The external validity of twin-based estimates is commonly put into question. It is well known that twins have lower birth weight than non-twins, for instance, and that low birth weight may lead to later health problems. It should be noted that this need not threaten the external validity of the estimates if the *association* between schooling and health is the same for twins and non-twins. I will therefore consider this in the Results section. In this section, I will consider to what extent the sample of identical twins resembles the main MIDUS sample and the US population in general. For the latter purpose, I will make some comparisons with data from the Current Population Survey (CPS) of 1995. In Table 1, descriptive statistics for the three samples are shown.

A comparison between the MIDUS main sample and the twin sample reveals that the twins are significantly younger, are more likely to be white, are more likely to be married or cohabitating, have a higher income, have better health, are more physically active, and have a lower Body Mass Index compared to the main sample. There are no significant differences in the level of education, however. Moreover, there are no significant differences in the smoking rate or the overweight rate.

The comparison with the CPS data reveals that the both the twin sample and the MIDUS main sample are better educated than the US population in general. Similar patterns were found in several previous twin studies, reflecting a selection of better educated twins into the surveys (Ashenfelter and Kreuger 1994; Ashenfelter and Rouse 1998; Bonjour et al. 2003). While similar in terms of gender distribution, the twin sample also contains more whites and has a slightly more compressed age distribution than the CPS sample. Regarding marital status, the CPS from 1995 does not contain a straightforward estimate of the number of cohabitating or married couples. Considering marriage alone, however, the fraction of married in CPS in 1995 was 67.5%, compared to 71.6% in the twin sample and 62.6% in the MIDUS main sample.

III Empirical strategy

In order to see how a twin-differences strategy may help to estimate the causal effect of schooling on health, consider first an individual i, whose health stock H_i is determined by:

$$H_i = \beta S_i + \alpha A_i + u_i, \tag{1}$$

where S_i denotes schooling, A_i denotes unobserved "ability", and u_i is an unobserved random component. In this context, "ability" is taken to mean both unobserved genetic traits affecting health, as well as unobserved non-genetic factors, such as family background. Next, let schooling be determined by:

$$S_i = \delta A_i + \xi_i, \tag{2}$$

where A_i denotes the same unobserved "ability" components that affects health and ξ_i denotes a schooling-specific random term.

This gives the standard result that an OLS estimate of β is biased such that:

$$p\lim(\beta_{OLS}) = \beta + \alpha \frac{\sigma_{AS}}{\sigma_S^2}. \tag{3}$$

Since unobserved ability is likely to be positively correlated with both schooling and health, is it usually assumed that an estimate of β_{OLS} will be upward biased.

Next, I turn to the twin-differences strategy. Let H_{1j} and H_{2j} denote the health of the first and second twin in the jth twin pair. The unobserved component is again made up of two parts. The first part, μ_j, denotes unobserved factors that vary between twin pairs but not within pairs. This could, for instance, be genetic characteristics and early life environmental factors. Finally, ε_{1i} and ε_{2i} denote unobserved factors specific to each twin. This can then be written as:

$$H_{1j} = \beta S_{1j} + \mu_j + \varepsilon_{1j}, \tag{4}$$

$$H_{2j} = \beta S_{2j} + \mu_j + \varepsilon_{2j}, \tag{5}$$

Next, I take the difference between (4) and (5), giving:

$$H_{1j} - H_{2j} = \beta_{WTP}(S_{1j} - S_{2j}) + \varepsilon_{1j} - \varepsilon_{2j}. \tag{6}$$

where β_{WTP} is the within-twin-pair estimate of the association between schooling and health. In this specification, all factors that are common to both twins in a given twin pair will be differenced out. Since twins share common genes, their influence will vanish, as well as the influence of common family background. This means that an OLS estimate of (6) will no longer be biased due to unobserved twin-pair specific variables. Any remaining unobservables that remain in the error term after differencing may still, however, bias the results, if these unobservables are still related to both schooling and health. This issue is the focus of Section IV F in the paper.

It is well known that measurement errors in years of schooling are exaggerated by differencing and even more so when differencing between identical twins (Griliches 1979). This will cause twin FE estimates to be downward biased, under the assumption of classical measurement errors. The extent of the downward bias may be calculated in the case where one has a measure of the reliability of self-reported schooling and a measure of the correlation in schooling within twin pairs. As shown by Griliches (1979), in the presence of classical measurement error, the twin FE estimate is biased according to:

$$\beta_{WTP} = \left(1 - \frac{\text{Var}(\nu)}{[\text{Var}(S)]\,(1\text{-}\rho_{\text{S}})}\right),$$

where Var(ν) denotes the assumed common variance of the twins measurement error, Var(S) is the variance in the true schooling levels, and ρ_{S} is the correlation between the measured schooling levels of the twins. The part $\frac{\text{Var}(\nu)}{\text{Var}(S)}$ is called the reliability ratio. Previous research suggests that the reliability of self-reported schooling is about 90 percent, a figure that has been remarkable stable across studies (Card 1999). Moreover, the correlation in schooling within identical twin pairs is commonly found to be about 0.75 (see e.g. Ashenfelter and Rouse 1998). Taking these estimates together, an attenuation bias of about 30% is typically obtained.

To obtain an estimate of the reliability ratio, previous studies have exploited data where several measures of the schooling of the respondent are given. Often, this has been a measure given by a co-twin (see e.g. Ashenfelter and Rouse 1998). Isacsson (1999), however, used a second measure on the respondent's schooling, taken from register data. While I do not have data on the co-twin's report on the other twins schooling or access to register data, I do have a second measure of the respondent's schooling at the follow-up survey in 2004. The correlation between these measures suggest a reliability ratio of 0.90, being very much in line with previous estimates.[8] This is under the assumption, however, that the measurement errors of the two measures are uncorrelated, which is a strong assumption. If the measurement errors are positively correlated, the reliability ratio is overestimated. On the other hand, it should be noted that for some individuals, there may have been real

[8] Not all twins participated in the follow-up survey and my estimate is based on a sample of 541 identical twins.

changes in educational attainment between the waves, suggesting some downward bias in the reliability ratio. The estimated correlation in schooling within twin pairs in MIDUS is 0.72, which is also rather similar to the figures obtained in previous studies. Taken together with the estimated reliability ratio, this indicates that the twin FE estimator is biased downward by about 36%. Assuming reliability ratios of 0.85 or 0.95 instead, the downward bias would be 53% and 18%, respectively.

For the dummy variables indicating schooling categories, measurement errors are non-classical. The reason is that individuals in the lowest category cannot under-report the education level, whereas individuals in the highest category cannot over-report (Aigner, 1973). With non-classical measurement error, one cannot generally sign the bias in the estimates. A common method approach to obtain unbiased estimates has been to use external information of the measurement error probabilities to correct for the bias (Isacsson 2004). This method requires having a validation data set, with both an actual and a noisy measure, which is not available in the present context. It should be noted that degrees are in general much more accurately reported than years of schooling, though (Kane et al. 1999).

IV Results

A Self-reported health

Table 2 shows the OLS and twin-differences results for self-reported health. In order to assess the external validity of estimates based on the twin sample, I will start by comparing the association between schooling and health in the non-twin sample and the twin sample. Starting with the MIDUS non-twin sample, the results show a strong and positive association between schooling and health. These results are largely mirrored in the pooled twin sample, although the magnitude of the associations is now somewhat increased. This suggests that the differences between the MIDUS main sample and the MIDUS twin data in the distribution of characteristics does not lead to any radical differences in the estimated returns to schooling, suggesting that the results are reasonably generalizable.

In the third column, the results from the twin-differences approach are shown. Relative to high school dropouts, people with greater schooling report significantly better health. The effect is to increase self-reported health with about one unit, measured on a 0-10 scale. Interestingly, the magnitude of the associations between the educational categories and self-reported health is about double the magnitude of the associations in non-twin sample. These results are surprising, since one could expect a weaker relationship, once the influence of genes and family background common to the twins is controlled for. It should also be noted that there is no significant differences between the point estimates of the three dummy variables indicating different educational degrees.

For years of schooling, the OLS estimate based on the non-twin sample suggests a significant and positive assocation between schooling on self-reported health that is rather similiar to the estimated association in the pooled twin sample. One additional year of schooling is associated with a 0.045 increase in self-reported health. The point estimate based on twin-differences is similar, 0.053, but not significant.

Besides education, it is interesting to note that income shows a positive and significant effect on self-rated health in all three specifications. Controlling for twin-pair specific unobserved heterogeneity does thus not seem to reduce the magnitude of the income effect, which is remarkable stable across the specifications. Among the other variables it can be noted that self-reported health at age 16 is positively related to self-reported health as an adult in all specifications.

B Chronic conditions

Next, I consider the association between schooling and the somewhat more objective measure of health; the number of chronic conditions. Table 3 show the results for the three samples.

In the MIDUS main sample, schooling shows a strong and negative association with the number of chronic conditions, the association being strongest for the highest education category. In the latter case, having at least a college degree is associated with a decrease in the number of chronic conditions by 1.2 compared to being a high school droput. Income is again significant and is associated with a decrease in the number of chronic conditions.

In the pooled twin sample, schooling again shows a significant and negative association with the number of chronic conditions. The magnitude of the associations is greater than the corresponding ones in the main sample, with the two highest education categories now being associated with a decrease in the number of conditions by 1.9. Income is no longer significant and being white is now associated with a decline in the number of conditions.

The twin-differences estimates tell a similar story. The significant associations between schooling and the number of conditions remain for all education categories, except for the highest one, where the point estimate is still negative, though. The point estimates are in-between the ones obtained in the pooled twin sample and those obtained for the MIDUS main sample. There are no significant differences in the point estimates of the three dummy variables indicating schooling levels.

For years of schooling, there is a significant and negative correlation, -0.11, with the number of chronic conditions in the MIDUS main sample. In the pooled twin sample, the correlation is somewhat smaller in magnitude and only significant at the 10% level. Here, one additional year of schooling is associated with a 0.07 decrease in the number of chronic conditions. In contrast, the twin FE estimate no longer suggests any significant relationship between schooling and the number of chronic conditions.

C Investigating the mechanisms

In order to examine the potential mechanisms through which schooling affects health, I will next investigate the association between schooling and various lifestyle factors. I will focus on smoking and overweight, since these are the two main causes of preventable deaths in the US. In addition, I will consider the association between schooling and physical activity. In order to preserve space, I will from now on only compare the results from the pooled twin sample with the twin FE estimates.

Smoking.-While a number of studies have found a negative correlation between smoking and schooling, there are reasons for interpreting these results with some caution (see, for instance, de Walque 2007 and Grimard and Parent 2007). First of all, smoking is usually initiated before schooling is completed, suggesting that part of the association between schooling and smoking may be explained by unobserved third factors. Second, the dangers of smoking are well known and several studies show that people in general overestimate the risks (Viscusi 1990; 1991, Lundborg and Lindgren 2004, Lundborg 2007). If anything, more educated people should hold risk perceptions more closely related to the actual risks, suggesting that education should be associated with lower risk perceptions. So, if the association is mainly due to unobserved factors affecting both schooling and smoking, such as time preferences, and if these factors are common to twins, we would expect the effect to vanish when employing twin FE.

Starting with the pooled twin sample, the first column of Table 4 shows a strong association between schooling and smoking, that increases with the level of schooling. In contrast, the twin FE estimates are only between one fourth and one tenth of the magnitude and insignificant in all cases. This is consistent with the hypothesis that unobserved factors, such as genetic traits and family endowments, are driving the results for the pooled twin sample.

Using years of schooling, a significant and negative assocation between schooling on smoking is again obtained in the pooled twin sample. Here, one additional year of schooling is associated with a 4 percentage points decrease in smoking. The twin FE point estimate is only one third in magnitude, however, and, again, not significant.

Physical activity and overweight.-Next, I investigate the association between schooling and physical activity and Body Mass Index. Recent evidence from Kenkel et al. (2006) suggests a causal link between schooling and physical activity and overweight. Since MIDUS contains several measures of physical activity, I opted for the one that is most likely to reflect deliberate attempts to be physically active; vigourous physical activity during the winter.

In the first two columns of Table 5, I show the associations obtained for the pooled twin sample and the results from the twin FE estimation. In the pooled twin sample, having some college or having a college degree is associated with about two more occasions of physical activity per month compared to the reference category, whereas having graduated high school shows no significant effect. Surprisingly, the results get even stronger when employing the twin FE estimator. Now, having some college or having a college degree are associated with an increase in the number of occasions of physical activity per month by more than three.[9] The point estimates of the three dummies indicating educational attainment were not significantly different.

To investigate to what extent the higher physical activity of educated individuals also transforms itself into lower body mass and a lower prevalence of overweight and obesity, I next

[9]Similar results were obtained when using alternative measures, such as moderate activity during the summer. Only for vigorous activity during the summer were the results from the twin FE not significant. The point estimates were, however, rather similar to those from the pooled twin sample, where the two highest educational categories were positive and significant at the 10% level.

examine the direct association between schooling and these outcomes. Column 3 to 6 of Table 5 show the results for the pooled twin sample and the results from the twin FE estimator. As shown in column 4 and 6, in the pooled twin sample, schooling shows a strong and negative significant correlation with both BMI and overweight for all educational categories. Belonging to the highest educational category is associated with a 3 points decrease in BMI compared to the omitted reference category. The significance of these associations is completely swept away in the twin FE estimates, however. The point estimates of schooling are now in most cases only a tiny fraction of those obtained from the pooled twin sample and are no longer significant. For instance, belonging to the highest educational category is now associated with a 0.03 increase in BMI, with a p-value of 0.98.[10]

Table 6 shows the corresponding results for years of schooling. In the pooled twin sample, years of schooling show a significant and positive association with physical activity and a negative and significant association with Body Mass Index. These associations are no longer significant when employing the twin FE estimator, however. For overweight, years of schooling is insignificant in both specifications.

D Twin differences in schooling by parental schooling

Are twins who differ in schooling more likely to come from low-educated families? If so, and if the effect of schooling is non-linear, the estimated health returns to schooling would represent a Local Average Treatment Effect. To address this, I first analyse if the probability of observing a difference in educational categories within a twin pair is systematically related to parental schooling.[11] Measuring parental schooling by the same educational categories as for the main respondent, and having no high school as the omitted reference category,

[10] Similar results were obtained when using alternative measures, such as waist-to-hip ratio and obesity.

[11] In ordert to preserve space, I do not show these results in tables, but results are available on request.

the association between the mother's schooling and the probability of observing a difference in educational categories within a twin pair is negative for the two highest educational categories, but small and insignificant. For the father's schooling, the variables indicating educational categories are negative, with the highest educational category being significant at the 10% level. The result suggests that twin pairs having a father with a college degree are 15.3 percentage points less likely to be different in terms of schooling compared to twin pairs where the father did not have a high school exam. This provides some evidence that differences in schooling are less common in more educated families, in line with the results in Bonjour et al. (2003), for instance. It should be noted, though, that if the health returns to schooling are linear, it does not matter for estimating the Average Treatment Effect if we observe differences in schooling more often in twin pairs coming from high-educated or low-educated backgrounds. This is the issue that I investigate in the next section.

E Heterogenous returns to schooling by parental schooling

In order to investigate whether the health returns to schooling are decreasing in parental schooling, I re-run the twin FE models, this time including interactions betweeen own schooling and the parent's schooling.[12] The results for self-reported health, shown in columns 1 and 2 of Table 7, suggest that the health returns to educational categories (column 1) and years of schooling (column 2) are decreasing in parental schooling, as the point estimates of the interaction terms are always negative. The point estimates are small, however, and insignificant in all cases. For chronic conditions, the interaction terms for the educational categories are positive but insignificant (column 3). Finally, for years of schooling the interaction term

[12] In order to reduce the number of interaction terms, I create a 1-4 variable of parental schooling, measuring the average parental educational attainment on a linear scale. The steps correspond to the dummy variables measuring educational categories. In order to address measurement error in parents' education, I followed the approach of Ashenfelter & Kreuger (1994) and averaged the twin reports of their parent's schooling before creating the variable. In cases where only one of the twins reported, I used that measure.

is positive and significant at the 10% level, providing some weak evidence that the association between education and health is less negative for families with higher parental schooling. In sum, the results provide some weak evidence that there is diminishing returns to schooling, in terms of self-reported health, by parental schooling. This, in combination with the fact that differences in schooling are less prevalent in twin pairs coming from families where the father is highly educated, suggests that the estimates of the relation between schooling and self-reported health are slightly more likely to reflect the health returns among the twins coming from less educated families. This may also explain why the estimated coefficients are somewhat greater in magnitude in the twin FE models on self-reported health compared to the results based on the pooled cross section of twins.

F Why do twins differ in schooling?

Next, I turn to the question so crucial to any study using a twin-differences design: why do even identical twins differ in schooling? The underlying assumption is that such differences are exogenously given. If there are twin-specific unobserved factors that are related to both schooling and health, however, twin differencing will not help and the estimates may be biased. In this section, I will explore this issue by using the rich twin data in MIDUS. First, I will exploit the answers to a series of question about how similar the twins were treated and how similar their upbringing was, along a number of dimensions. I will then investigate to what extent twin pairs that were treated more similary by their parents also more often end up with the similar schooling levels and whether the association between education and health is different for twin pairs who were treated more similarly. Finally, I will analyse the correlation between various potentially important early life conditions and schooling between twin pairs and within twin pairs. These early life conditions include birth weight, early life physical and mental health, time preferences, parental relations, parental treatment, personality, and age at first marriage. Once I identify factors that predict within-twin-pair differences in schooling, I will re-run my regressions on schooling and health, while controlling for these factors.

Parental treatment, classroom placement, and peers.- If parents treat a less able twin differently from a more able twin, this may affect schooling attainment and later life health, thereby potentially biasing the twin FE estimates. Suggestive evidence is given in Ashenfelter and Rouse (1998), however, where twins are found to be given similar names with a frequency that is much higher than what would be expected by chance alone. On the other hand, anecdotal evidence also suggests that parents may try to emphasize the differences between the twins, for instance by dressing them differently or giving them different haircuts. Data from MIDUS does not, however, support the latter kind of parental behavior. In the first column of Table 8, the results from a question about how often their parents, or the people who raised them, in various ways emphasized the difference between them. The answer was Never in 85% of the cases. In only 8% of the cases was the answer Most or All of the time. In a similar vain, the results in the second column suggest that parents in most cases even try to dress their twins similarly. This provides some suggestive evidence that parents try to treat their twins as similar as possible rather then emphasize the differences.[13]

Another choice that parents of twins face is whether or not to put their twins in the same school and/or the same class. As shown in the third column of Table 8, however, the majority of parents prefer to keep twins in the same class, as 57% answered that they were in the same classroom always or most of the time. In only 14% of the cases was the answer Never, suggesting that the early schooling environment of twins is very similar in most cases.

[13]These numbers were obtained from the answers from the first-born twin. The numbers did not change to any important extent when using the answers from the second-born twin.

A potential source of within-twin-pair differences in educational attainment is differences in the exposure to peer groups. Twins may self-select into different peer groups or face different peer groups due to classroom placement policies. This worry appears largely unfounded, however, since the fourth column of Table 8 shows that as many as 90% of the twins report that they always or most of the time had the same playmates. Only 2% report that they never had the same playmates.

MIDUS also contains information on the number of years that the twins had been living together. Obviously, twins who lived longer together will also have had a shared environment for a longer time. Moreover, if certain twins were separated early they may be more likely to end up with different education levels. The average twin pair had been living together for 19.6 years, however. Only 1% reported living together for less than 16 years and the minimum number of years was 11. Potentially endogenous separation of twins at early ages does, thus, seem unlikely to be an important source of education differences in twins.

All in all, the results above suggest that whereas in the majority of cases, twins were treated similar along a number of dimensions, there is also a small fraction of twin pairs where circumstances and parental treatment differed to a large extent. Next, I therefore investigate if the existence of a difference in schooling within a twin pair is related to differences in some of the above cited factors. Table 9 summarizes the regression results, where the coefficients show the association between these factors and the probability of observing a difference in schooling within a twin pair.

The results provide some evidence that twins who are treated more differently also end up with different schooling levels more often. Twins who dressed the same were significantly less likely, at the 10% level, to differ in schooling attainment. For the other factors, the point estimates were negative, although not significant. Since differential treatment by parents may suggest the existence of ability differences between the twins, including such twins in my main analyses may bias the results. I therefore re-ran my regressions of the effect on schooling and health, excluding twin pairs that reported differences along the lines discussed above. Table 10 shows the results when I excluded twin pairs that reported that they never went to the same class, never had the same playmates, never dressed alike, and were never treated similarly by parents. For self-reported health, the estimated coefficients of the education dummies are still significant and even increase somewhat in magnitude. For continuous schooling, the estimated coefficient does not change much and is still insignificant. For chronic conditions, however, the schooling dummies are no longer significant but have the same signs, whereas the results do not change much for continuous schooling. In sum, it seems that at least for self-reported health, the results are robust to only including twins that faced similar conditions early in life, whereas for chronic conditions the effects are less precisely measured.

Birth weight.-Next, I examine the association between a range of indicators of early life circumstances and schooling, both between twin-pairs and within-twin pairs, as explained in the Introduction. This will give me an indication on whether the twin fixed-effects estimates are less biased than estimates based on a pooled cross-section.

I will start with one of the earliest within-twin-pair differences that can arise; differences in birth weight. Even though identical twins share common genes, the first born is usually heavier than the second born. This is confirmed in the MIDUS data, where the first born is significantly heavier, 77 grams, than the second born. Such differences may correlate with ability, cognitive functioning and later health and, thus, also with educational attainment. In MIDUS, I have complete information on the birth weight for 206 twins, or 104 twin pairs.

In the first row of Table 11, the between-twin-pair correlation in schooling and birth weight is shown. Education is here measured in years of schooling. I also tried alternative measures of schooling, such as binary indicators of high/low schooling, but the results did not change to any important extent. The between-twin-pair correlation in average birth weight and average education is positive but very small, 0.0002, and not significant. I also tried an indicator of low birth weight, i.e. below 2,500 grams. This resulted in a small, insignificant, positive correlation, 0.171.

The real question is, however, to what extent differences in birth weight within twin pairs affect within-twin-pair differences in schooling. The second column of Table 11, shows the correlation between differences in schooling within twin pairs and differences in birth weight within twin pairs. The correlation is again very small, 0.0001, and insignificant. Again, I obtain similar results for the indicator of low birth weight. The latter shows a positive correlation with schooling, but is not significant (p=0.20). These results are consistent with Miller (2005), whereas negative, but small, effects, where obtained in Royer (2009), for instancec. These studies were also based on twin samples. An explanation for the insignificant findings may parents try to compensate for birth weight differences, by investing more in the twin with low birth weight.

Early life health.-The remaining rows of Table 11, shows between-twin-pair and within-twin-pair correlations along other dimensions. An important source of differences in obtained schooling may be early life differences in health. To address this, I use measures of self-reported physical and mental health at age 16, which is given retrospectively by the respondents. These measures capture health differences that exist prior to completing schooling. The variables ranges from 1 to 5, where 1 denotes poor health and 5 excellent health. The second row of Table 9 shows the between-twin-pair and within-twin-pair correlation in early health and educational attainment.

For physical health, the between-twin-pair correlation is positive but insignificant; 0.281. The within-twin-pair correlation is 0.179, but is again not significant. For mental health, the correlation is 0.124 between twin-pairs and 0.043 within twin pairs and in both cases the correlation is insignificant. This suggests that any differences within twin pairs in self-reported physical and mental health at age 16 does not predict differences in educational attainment.

Parent-child interactions -Next, I consider a range of indicators of parental treatment and parent-child relations. These measures reflect factors such as time and attention given by parents, love and affection, strictness about rules, punishments, rating of relationship, parents expectations, and physical abuse.[14] Starting with the between-twin-pair analyses, the correlation between average schooling and several of these factors is significant, as shown in rows 6-19 of Table 11. For instance, the results show that the time and attention given by the mother is significantly and positively related to educational attainment. Moreover, having a father who was strict about rules and harsch when punishing show a significant and negative association with schooling. Having a mother or a father who held higher expectations about the respondent is positively related to educational attainment. Finally, having a mother or a father who had beaten or hit the respondent show a negative correlation with educational attainment.

A smaller number of these associations are significant in the within-twin-pair regressions as well. Interestingly, more time and attention by the mother or the father is now *negatively* and significantly related to schooling. This most likely reflects some compensatory behaviour

[14]These measures concern the situation when the respondent was growing up and the answers are thus given retrospectively. To assess the relationship to the father/mother when the respondent was growing up, the respondent was asked to rate it on a 1-5 scale, where 1 means poor and 5 means excellent. For the other questions indicating parental treatment, the scale went from 1 to 4, where 1 indicates not at all and 4 not at all. Physical abuse was assessed by asking how often the mother/father pushed, grabbed, shoved, slapped, or threw something at the respondent. The scale went from 1 (never) to 4 (often).

by the parents, since it is hard to believe that more time and attention could *cause* lower schooling. In other words, parents may spend more time and attention on the less able twin in order to compensate for any existing ability differences between the twins. This may even suggest that parental time and attention may act as a proxy for ability differences between the twins. A similar effect is found for the variable indicating how much love and affection the father showed, which show a negative correlation with educational attainment. Again, this may reflect that the father shows more love and affection to the less able twin, rather than love and affection *causing* lower schooling. I will return to the potential influence these results may have for my estimates on the relation between education and health.

Attitudes towards the future- In the next three rows of Table 11, I consider the relationship between three different measures of the individual's attitude towards the future, proxying for time preferences, and schooling. These measurements are based on a set of question that were asked to all MIDUS respondents:

1. I live life one day at a time and don't really think about the future.
2. I like to make plans for the future.
3. I find it helpful to set goals for the near future.

The responses are measured on Likert scales, reflecting the extent to which the respondent agrees with the statement. For the first question, the Likert scale has 6 points, ranging from Agree Strongly to Disagree Strongly, whereas for the two other questions, the Scale runs from A lot to Not at all. Respondents who agree that they 'make plans for the future' or 'set goals for the near future' could be considered to be more future-oriented, whereas people who agree that they 'live life one day at a time' could be considered to be more present-oriented. These measures were used as proxys for time preferences in Knowles & Postlewaite (2005), for instance, where they were found to predict savings behavior in the expected direction.

To the extent that time preferences are stable over time, one could relate these to educational attainment, the hypothesis being that more future-oriented people also invest more in schooling.[15] In the between-family regressions, the relationship is significant at at least the 10% level for all the three measures employed. The relationship is in the expected direction, so that a higher degree of future-orientation is associated with more schooling. This suggests that results based on pooled cross-sections may be biased due to the omission of time preferences. For the purpose of this paper, the more important question is whether or not there is a significant relationship within twin pairs. The results suggest much smaller point estimates in the within-twin pair regressions and the associations are not significant for any of the three measures employed. This suggests that differences in time preferences within twin pairs are not an important factor in explaining schooling differences and that the twin FE estimates are less biased than results based on pooled cross-sections.

Early-life personality differences- Twins may differ in personality and since personality may affect both schooling and health, it's omission may bias the estimate of the association between schooling and health. The twin questionnaire in MIDUS contained a question, where each twin was asked to assess which one of them were more likely to be a leader and which one a follower when they were doing things together.[16] The next-to-last row of Table 11 shows, however, that being the "leader" is not significantly associated with schooling, neither in the between-pair-regressions, nor in the within-pair-regressions.[17]

Age at first marriage-Finally, I consider the role that age at first marriage plays for

[15]It has been suggested, though, that schooling in itself can have an impact on time preferences (Becker & Mulligan 1997). In either case, we would expect a positive correlation between time preferences and schooling.

[16]The exact wording of the question was: "When you were children and just the two of you were doing things together, which one of you was more likely to be the leader and which one the follower?".

[17]I used the answer from the first-born twin. There were some small differences in the perceptions on who was the leader in the twin pair. There seems to be a general agreement, however, that the first born was more often the leader. In only one third of the cases was it reported that leadership was equal between the twins. The results did not change when using the report of the second-born twin.

educational attainment. In Bonjour et al. (2003), age at first marriage was significantly related to educational differences within identical twin pairs. As shown in the last row of Table 11, there is a significant and positive association between age at first marriage and educational attainment in the between-twin-pair regression. The association is no longer significant, however, in the within-twin-pair regression, where the coefficient is close to zero, 0.006.

To summarize, the within-twin pair differences in schooling are significantly correlated with these rather detailed measures of early within-twin-pair differences in only 3 out of 22 regressions. In the between-twin-pair regressions, however, the corresponding numbers are 11 out of 22 cases. This suggests that the twin-differences estimates are less biased than the pooled cross-section results. It should be noted though that these within-twin-pair estimates may be downward biased, due to measurement error. On the other hand, in all cases where the between-twin-pair estimates are significant, the corresponding within-twin-pair point estimates are substantially lower in magnitude. The between-twin-pair coefficient of mother's expectations, for instance, is 0.513, while the corresponding within-twin-pair coefficient is only -0.013. While I am unable to calculate the downward bias in the within-twin-pair estimates, the differences between the between-twin-pair and within-twin-pair estimates in many cases seem too large to be explained by measurement errors alone.

Finally, since time and attention given by parents, and love and affection by father, was found to be negatively related to schooling even within twin pairs, it remains to settle what consequence this will have for the twin-differences estimates of the effect of education on health. It should be noted that if parents try to compensate differences in ability and/or health between the twins by giving more time and attention to the less able one/weaker one, that would imply that the estimated associations between education and health are downward biased. To investigate this, I re-ran my main analyses, controlling for the factors that were significantly correlated with schooling within twin pairs, i.e. parental time and attention and the measure of love and affection of the father. Table 12 shows that the results for self-reported health are robust to inclusion of these measures, whereas for chronic conditions, the estimated coefficients are reduced in magnitude and are no longer significant. The latter results could be due to the lower sample size, however.

In sum, while there could obviously still be other unmeasured factors that give rise to endogenous schooling differences within twin pairs, I have investigated the role of some of the variables commonly suspected to give rise to such differences. These findings support those of Ashenfelter and Rouse (1998) and Bonjour et al. (2003) and lend credibility to the results in this paper, as well as to the general validity of using a twin-differences design to study the returns to schooling.

V Conclusion and discussion

In this paper, I show that relative to high school dropouts, people with greater schooling are significantly healthier, as measured through self-reported health and chronic conditions. No evidence of any additional positive effect of post-high school education is obtained, however. This suggests that the main effect of education on health is found when moving from low levels of education to higher levels. These results were based on a twin-differences design, netting out the influence of genetics and family endowments. When measuring schooling through years of schooling, point estimates were similiar in both the twin-differences model and the OLS model, but insignificant in the former case. Moreover, whereas schooling was significantly associated with physical exercise, no corresponding effect was obtained for two of the most common causes of preventable deaths in the US; smoking and body weight. This is puzzling and future research should aim at better understanding the mechanisms through which schooling affects health.

My twin-differences estimates would still have been biased if there were twin-specific unobserved factors that related to both schooling and health. I therefore exploited the rich and unique MIDUS data on the early life conditions of twins and provided new evidence on the determinants of schooling differences within twin pairs. While many of these factors, such as parental time inputs, parental expectations, parental abuse, and time preferences, predicted schooling differences *between* twin pairs, few of them predicted such differences *within* twin pairs. Moreover, for the few variables that did predict schooling differences within twin pairs, my sensitivity analysis showed that accounting for these factors did not affect the schooling/health gradient to any important extent. In sum, this provides new evidence that the twin-differences estimator is less biased than the OLS estimator for studying the returns to schooling.

Some caveats should be mentioned. With the data at hand, I was not able to deal with the problem of measurement error in schooling. This means that the results for years of schooling are downward biased, which, in part, may explain why no significant effect was obtained. Furthermore, years of schooling was imputed from educational categories, which accentuated the measurement error problem. I argued, however, that the potential upward bias resulting from the omission of twin-specific factors potentially associated with schooling and health was more important to deal with in the present context, since there is still little evidence as to whether schooling has any causal effect on schooling at all.

To conclude, my findings suggest that schooling has a causal effect on health. This is a very relevant finding for the current policy debate about the future of the health care systems. It is well known that increased health care expenditures have a very small effect on population health on the margin. It is therefore important to consider alternative policies to improve population health and the results in this paper suggest that education may be such an alternative policy.

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VII Tables

Table 1: Descriptive statistics.

Variables	**Means (std. err.)**		
	Main sample	**Twin sample**	**CPS**
Socio-economic and demographic			
Female (percent)	0.505	0.527	0.517
	(.009)	(.019)	
Age 25-34	0.215	0.226	0.276
	(.007)	(.016)	
Age 35-44	0.246	0.317	0.270
	(.007)	(.018)	
Age 45-54	0.236	0.249	0.192
	(.007)	(.016)	
Age 55-64	0.192	0.141	0.139
	(.007)	(.013)	
Age 65-74	0.109	0.066	0.122
	(.005)	(.009)	
White	0.880	0.934	0.848
	(.006)	(.010)	
Married or cohabitating	0.678	0.776	-
	(0.008)	(.015)	
High school graduate	0.297	0.318	0.342
	(.008)	(.018)	
Some college (no bachelor)	0.309	0.330	0.276
	(.008)	(.018)	
College graduate	0.283	0.291	0.226
	(.008)	(.017)	
Years of schooling	13.421	13.684	
	(.045)	(.084)	
Income ($1,000)	23,549	25,979	
	(463.724)	(1001.463)	
Health variables			
Health 0-10 scale	7.348	7.856	
	(.031)	(.057)	
Number of chronic conditions	2.569	1.982	
	(.049)	(.086)	
Smoking	0.243	0.213	
	(.007)	(.016)	
Physical activity	5.158	5.822	
	(.100)	(.214)	
Body Mass Index	26.893	26.025	
	(.104)	(.191)	
Overweight	0.495	0.490	
	(.009)	(.019)	

Table 2: Regressions on self-reported health.

	Schooling in categories			Years of schooling		
	Main	Pooled	FE	Main	Pooled	FE
Age	-0.042**	-0.088**		-0.042**	-0.093**	
	(0.018)	(0.036)		(0.018)	(0.036)	
Age squared	0.000**	0.001***		0.000**	0.001***	
	(0.000)	(0.000)		(0.000)	(0.000)	
Female	0.023	0.090		0.022	0.091	
	(0.062)	(0.115)		(0.062)	(0.114)	
White	-0.152	-0.488**		-0.140	-0.504**	
	(0.096)	(0.233)		(0.096)	(0.233)	
Married/Part.	-0.008	0.021	0.058	0.006	0.035	0.030
	(0.071)	(0.144)	(0.197)	(0.071)	(0.144)	(0.195)
Income	0.002***	0.002*	0.003*	0.002***	0.002*	0.003*
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
High school	0.451***	0.521**	0.813*			
	(0.114)	(0.245)	(0.432)			
Some college	0.348***	0.558**	1.014**			
	(0.114)	(0.247)	(0.446)			
College degree	0.547***	0.761***	0.970*			
	(0.118)	(0.251)	(0.510)			
Years of schooling				0.045***	0.061**	0.053
				(0.013)	(0.026)	(0.054)
Health at age 16	0.193***	0.457***	0.393***	0.197***	0.457***	0.414***
	(0.035)	(0.077)	(0.109)	(0.035)	(0.077)	(0.109)
Constant	6.998***	7.254***	4.968***	6.780***	7.135***	5.037***
	(0.437)	(0.908)	(0.615)	(0.458)	(0.949)	(0.866)
Observations	2877	622	622	2875	622	624
R^2	0.03	0.10	0.08	0.03	0.09	0.06

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Regressions on the number of chronic conditions.

	Schooling in categories			Years of schooling		
	Main	Pooled	FE	Main	Pooled	FE
Age	0.104***	0.112**		0.105***	0.134**	
	(0.028)	(0.054)		(0.028)	(0.055)	
Age squared	-0.001***	-0.001*		-0.001***	-0.001**	
	(0.000)	(0.001)		(0.000)	(0.001)	
Female	0.568***	0.698***		0.575***	0.685***	
	(0.098)	(0.170)		(0.098)	(0.173)	
White	0.047	-0.672*		0.023	-0.591*	
	(0.151)	(0.345)		(0.151)	(0.351)	
Married/Part.	-0.074	-0.174	-0.310	-0.091	-0.233	-0.337
	(0.112)	(0.213)	(0.264)	(0.112)	(0.217)	(0.264)
Income	-0.003***	-0.002	-0.002	-0.003***	-0.002	-0.002
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
High school	-0.754***	-1.652***	-1.310**			
	(0.180)	(0.363)	(0.580)			
Some college	-0.708***	-1.954***	-1.053*			
	(0.180)	(0.365)	(0.599)			
College degree	-1.156***	-1.858***	-0.790			
	(0.186)	(0.372)	(0.685)			
Years of schooling				-0.111***	-0.073*	0.070
				(0.020)	(0.040)	(0.073)
Health at age 16	-0.201***	-0.289**	-0.031	-0.208***	-0.286**	-0.070
	(0.055)	(0.115)	(0.147)	(0.055)	(0.117)	(0.147)
Constant		2.643*	3.500***	1.730**	1.348	1.736
		(1.347)	(0.826)	(0.723)	(1.431)	(1.164)
Observations	2886	620	620	2884	620	620
R^2		0.11	0.03	0.07	0.08	0.02

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Regressions on smoking.

	Schooling in categories		Years of schooling	
	Pooled	FE	Pooled	FE
Age	0.010		0.011	
	(0.009)		(0.009)	
Age squared	-0.000		-0.000	
	(0.000)		(0.000)	
Female	0.026		0.026	
	(0.031)		(0.031)	
White	0.044		0.043	
	(0.061)		(0.061)	
Married/Part.	-0.049	0.044	-0.053	0.038
	(0.038)	(0.044)	(0.038)	(0.044)
Income	-0.001*	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
High school	-0.195***	-0.022		
	(0.068)	(0.101)		
Some college	-0.182***	0.017		
	(0.068)	(0.104)		
College degree	-0.363***	-0.109		
	(0.070)	(0.118)		
Years of schooling			-0.043***	-0.015
			(0.007)	(0.012)
Smoking at age 16	0.100***	0.027	0.103***	0.015
	(0.031)	(0.042)	(0.031)	(0.042)
Constant	0.271	0.198*	0.584**	0.375**
	(0.233)	(0.105)	(0.245)	(0.171)
Observations	664	664	664	664
R^2	0.10	0.02	0.10	0.01

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Regressions on physical activity, BMI, and overweight. Schooling in categories.

Variables	Pooled	FE	Pooled	FE	Pooled	FE
	Physical activity		BMI		Overweight	
Age	-0.134		0.163		0.021	
	(0.138)		(0.117)		(0.013)	
Age squared	0.001		-0.002		-0.000	
	(0.001)		(0.001)		(0.000)	
Female	-0.696		-1.649***		-0.246***	
	(0.441)		(0.376)		(0.041)	
White	-0.063		-1.831**		-0.088	
	(0.892)		(0.759)		(0.081)	
Married/Part.	-0.504	-1.299*	0.158	-0.180	0.017	-0.057
	(0.549)	(0.777)	(0.472)	(0.408)	(0.051)	(0.058)
Income	0.013***	0.020***	-0.005	-0.001	-0.000	-0.000
	(0.004)	(0.006)	(0.004)	(0.003)	(0.000)	(0.000)
High school	1.325	2.414	-1.652**	1.127	-0.150*	0.086
	(0.938)	(1.701)	(0.818)	(0.867)	(0.089)	(0.125)
Some college	1.809*	3.298*	-2.312***	0.354	-0.166*	0.066
	(0.944)	(1.756)	(0.822)	(0.901)	(0.090)	(0.130)
College degree	2.352**	3.639*	-2.982***	0.341	-0.205**	0.150
	(0.959)	(2.007)	(0.830)	(1.034)	(0.091)	(0.149)
Constant	8.230**	2.345	26.889***	25.434***	0.429	0.477***
	(3.389)	(1.725)	(2.890)	(0.889)	(0.314)	(0.128)
n	618	618	582	582	590	590

Table 6: Regressions on physical activity, BMI, and overweight. Years of schooling.

Variables	Pooled	FE	Pooled	FE	Pooled	FE
	Physical activity		BMI		Overweight	
Age	-0.158		0.200*		0.023*	
	(0.137)		(0.116)		(0.013)	
Age squared	0.001		-0.002		-0.000*	
	(0.001)		(0.001)		(0.000)	
Female	-0.712		-1.600***		-0.245***	
	(0.438)		(0.375)		(0.041)	
White	-0.146		-1.767**		-0.085	
	(0.890)		(0.762)		(0.081)	
Married/Part.	-0.459	-1.304*	0.091	-0.123	0.011	-0.053
	(0.548)	(0.774)	(0.473)	(0.408)	(0.051)	(0.058)
Income	0.013***	0.020***	-0.005	-0.001	-0.000	-0.000
	(0.004)	(0.006)	(0.004)	(0.003)	(0.000)	(0.000)
Years of schooling	0.227**	0.185	-0.238***	-0.050	-0.012	0.009
	(0.100)	(0.214)	(0.085)	(0.111)	(0.009)	(0.016)
Constant	7.490**	2.729	27.024***	26.677***	0.376	0.443**
	(3.560)	(2.974)	(3.032)	(1.550)	(0.328)	(0.222)
n	618	618	582	582	590	590

Table 7: Pooled twin sample and twin FE of the health returns to education by parents' education.

Variables	Self-reported health		Chronic conditions	
	(1)	(2)	(3)	(4)
Years of schooling*parents' educ		-0.007		0.015*
		(0.007)		(0.009)
High school * parents' educ	-0.273		-0.002	
	(0.169)		(0.226)	
Some college * parents' educ	-0.084		0.011	
	(0.126)		(0.169)	
College degree * parents' educ	-0.142		0.206	
	(0.106)		(0.142)	
Constant	0.215	0.124	-0.304	-0.567**
	(0.236)	(0.213)	(0.316)	(0.285)
n	294	294	293	293

Table 8: Differences within twin pairs in early life.

Variables	Parents emphasized differences between the twins	Twins dressed the same	Twins shared same classroom	Twins shared playmates
Always	1.5%	32.9%	35.1%	52.5%
Most of the time	6.2%	28.2%	22.2%	37.2%
Some of the time	7.1%	32.3%	28.6%	8.5%
Never	85.2%	6.5%	14.0%	1.8%
n	337	340	342	341

Table 9: Regressions on determinants of observing a difference in schooling within a twin pair.

Variables	△Probability of observing a difference in schooling in a twin pair
Always or almost always had the same friends (n=333)	-0.123
	(.092)
Always of almost always dressed the same (n=331)	-0.108*
	(.056)
Always of almost always went to the same class (n=333)	-0.092
	(.057)
Always of almost always treated similarily by parents (n=331)	-0.002
	(.102)
Number of years living together (n=320)	-0.005
	(.008)

Table 10: Sensitivity analysis. Regressions on education and health, only including twins that are alike.

	Self-reported health		Chronic conditions	
High school	1.250**		-0.719	
	(0.571)		(0.671)	
Some college	1.194**		-0.166	
	(0.585)		(0.687)	
College degree	1.340**		-0.075	
	(0.672)		(0.789)	
Years of schooling		0.066		0.077
		(0.067)		(0.079)
Observations	502	502	500	500

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Correlation of education and other characteristics between twin pairs and within twin pairs.

Variables	Between-pair regressions	Within-pair regressions
	Education	ΔEducation
Health		
Birthweight (n=206)	0.0002	0.0001
Phys. health at 16 (n=692)	0.281	0.179
Ment. health at 16 (n=690)	0.124	0.043
Parental treatment and relations		
Mother: time and attention (n=614)	0.372**	-0.250**
Father: time and attention (n=578)	0.201	-0.194*
Mother: love and affection (n=616)	0.134	-0.201
Father: love and affection (n=574)	-0.041	-0.289**
Mother: strictness (n=618)	-0.058	-0.112
Father: strictness (m=578)	-0.286*	-0.040
Mother: harsch (n=616)	-0.084	0.007
Father: harsch (n=576)	-0.404***	-0.072
Mother: relationship rating (n=618)	0.078	0.060
Father: relationship rating (n=582)	0.113	-0.049
Mother: expectations (n=620)	0.513***	-0.013
Father: expectations (n=576)	0.309***	-0.013
Mother: physical abuse (n=598)	-0.332**	-0.158
Father: physical abuse (n=562)	-0.460***	-0.049
Attitudes towards the future		
Live one day at a time (n=604)	0.302***	0.027
Set goals for the future (n=618)	-0.868***	-0.002
Like to make plans for the future (n=620)	-0.34*	0.039
Other		
Respondent was a leader (n=600)	-0.013	-0.000
Age at first marriage (n=552)	0.141***	0.017

Table 12: Sensitivity analysis. Regressions on education and controlling for parental time and attention and love and affection of the father.

	Self-reported health		Chronic conditions	
High school	0.995**		-0.694	
	(0.454)		(0.568)	
Some college	1.168**		-0.164	
	(0.476)		(0.596)	
College degree	1.164**		0.210	
	(0.536)		(0.671)	
Years of schooling		0.056		0.136
		(0.056)		(0.078)
Observations	564	564	562	562

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1