Parental Schooling and Child Development: Learning from Twin Parents*

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Parents with more schooling tend to have children with better outcomes. We use unique Danish data for identical and fraternal twin parents and their children to estimate the effect of parental schooling on short- and long-term outcomes. By differencing within identical twin pair we take into account heritable endowments. For all outcomes OLS is upward biased. Fathers' schooling has no causal effect on short-term outcomes, and mothers' schooling increases both short- and long-term outcomes. For parents born before 1945, results replicate Behrman and Rosenzweig (2002) findings where fathers' schooling but not mothers' has a positive causal effect on child schooling. This effect is reversed for later cohorts, when mothers' schooling has a positive causal schooling effect but fathers' no longer does.

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1. INTRODUCTION

Parents with more schooling often have children with better outcomes, such as heavier birth weight, better health, higher test scores and more schooling. This correlation may result from selection, whereby parents and children share unmeasured traits that are correlated with parental schooling and drive the associations. But a causal relationship in which otherwise similarly endowed parents' differences in education actually make a difference for child outcomes might also be a factor. Distinguishing between selection and causation can inform theories of intergenerational transmission and affect the case for schooling interventions where benefits to future generations might be captured.

A growing literature aims at identifying causal effects of parental background on child outcomes. Considering a number of parent and child characteristics, the typical study involves a pair-wise comparison, most commonly education-education or income-income. Studies differ in context, data, method and outcome of interest, and results are mixed. The most convincing studies indicate that parental schooling has a causal role to play in child outcomes but much less so than the previous generation of correlation studies suggested. Aside from instrumental variables studies that estimate local average treatment effects, both twin-parent and recent adoption studies find a larger causal effect for fathers' schooling on child schooling than mothers'.

This paper applies the strategy of Behrman and Rosenzweig (2002), who use identical, i.e. monozygotic (MZ), twins, and their children to estimate the impact of parental schooling on children's schooling. We are the first to apply the identical twin-parent differences method of Behrman and Rosenzweig on a different dataset. Using a large sample of twins from the Danish Twins Registry and combining this data with high quality administrative data, we estimate not only the effect of parental schooling on various child outcomes from birth weight to length of schooling, but also heterogeneous effects. By calculating simple differences in mothers' schooling where the mothers are identical twins, and by estimating this correlation with differences in the outcomes of their children, we are able to take into account any heritable traits transmitted from parent to child. Furthermore, twins

share similar rearing talents because they are raised in the same household, and by applying within twin pair differences, we also account for this.

Given concerns about measurement error and endogeneity in twin-difference studies (Bound and Solon 1999; Griliches 1979; Neumark 1999), we apply instrumental variables as robustness checks. The high quality administrative data substantially reduce measurement errors in contrast to survey self-reports, because they are not affected by recall bias or respondent misreporting. Nevertheless, we instrument institution-reports with self-reports of schooling differences for a large subsample of our data (Ashenfelter and Krueger 1994). Furthermore, a school proximity reform is used to provide supply-side instrumental variables for schooling differences of parents attending 7^{th} grade in the late 1950's and early 1960's.

Our dataset contains information on both identical and fraternal,i.e. dizygotic (DZ), twins.Fraternal twins, like identical twins, share the same upbringing, but like non-twin siblings they only share one half of their segregating genes, or heritable endowments. The sample of fraternal twins constitutes an informative comparison group because not all endowments are differenced out; but they are likely to be equally sensitive to measurement errors because data (mis)reporting is by institutions. In levels the positive relationship between parental schooling and child outcomes is the same for samples of singletons, fraternal and identical twins. Identical twin differences show the relationship is much less strong and estimates based on fraternal-twin differences are almost always in-between estimates from levels and identical twin differences. This increases the validity of the findings from the sample of identical twins.

We find OLS to be upward biased because of unobserved endowments transmitted from parents at all stages of child development. A result that is robust to measurement errors and endogeneity. We find some causal effects for mother's schooling on birth weight, high school completion, and length of schooling. The effect on birth weight is smaller than in the existing studies using U.S. or U.K. data (e.g., Currie and Moretti 2003 and Chevalier and O'Sullivan 2007). However, in line with Chevalier and O'Sullivan we find the effect of maternal schooling on birth weight driven by mothers who grew up in less educated homes.

The effect of maternal schooling on birth weight is also sensitive to time trends and decreases across parental birth cohorts. We relate the small and diminishing effects to the use of relatively recent child cohorts (1978-2004), and universal public health care. In line with Behrman and Rosenzweig (2002) and Plug (2004) and others, we find no effect of mother's schooling on child's length of schooling. However, the effect of maternal schooling on child's length of schooling is also sensitive to time trends. When mothers are born after 1945, mother's schooling has a positive effect on child length of schooling. We explain this change by a reduction in the bias caused by endowments, a change driven by an increase in educational opportunities in general and for women in particular. We also find that mother's schooling affects child high school completion.

In line with most studies we find no effects of father's schooling on infant or early childhood health. However, we find father's schooling to modestly reduce 9th grade point average (GPA) and, in line with Behrman and Rosenzweig (2002) and Plug (2004), find a positive although small effect on child's length of schooling. The small positive effect of father's schooling on child schooling decreases across birth cohorts and for fathers born after 1945 the effect becomes insignificant.

The rest of this paper is organised as follows. Section 2 outlines the recent literature estimating causal effects of parental schooling on child outcomes. Section 3 describes our identification strategy. Section 4 presents our data. Section 5 covers our results from infant's health to schooling completion. Section 6 discusses criticisms of twin studies and deals with these by applying robustness checks. Section 7 concludes.

2. LITERATURE REVIEW

The literature on estimating the causal relationship between parental schooling and child outcomes has taken three different approaches: adoption studies, policy-induced variation, and twin studies.

First, adoption studies show that, if adopted children are genetically unrelated and randomly matched to the families that raise them, then the effect of family environment and endowments at birth are perfectly separated. Examples of recent studies are Plug (2004), Björklund, Lindahl and Plug (2006) and Sacerdote (2007). Plug (2004), using U.S. adoption data, finds a causal effect of father's schooling but not of mother's. Although intuitively appealing, adoption studies have been criticized for non-random placement of adoptions, a placement that can create a bias in the estimates (Scarr and Weinberg 1994). Assuming random placements of Korean American adoptees, Sacerdote (2007) avoids this bias, finding that the adopting mother's schooling has an impact on child's schooling. Björklund, Lindahl and Plug (2006) using a large sample of Swedish adoptions, deal with non-random placement by introducing information on the adoptee's biological parents. They find that while the education of both sets of parents matters, it has only a small effect for adopting mothers when fathers are taken into account. Overall, the most convincing adoption studies dealing with placement issues find that selection is important and that, when properly controlled, parental schooling has a causal effect on child schooling. Furthermore, father's schooling has a larger causal effect than mother's.

Second, policy-induced variation in parental schooling is the most popular research approach. Both Black, Devereux and Salvanes (2005) and Chevalier (2004) use changes in compulsory schooling laws for parents to estimate the effect on children's schooling. At the lower end of the schooling distribution, both find evidence that maternal schooling increases the schooling of their children. Several studies use the same strategy to estimate the impact of parental schooling at other stages of development. One example is Currie and Moretti (2003), who use U.S. college openings as an instrument, and find that

mother's schooling decreases the probability of low birth weight.[1] Despite the popularity of this strategy, it identifies local average treatment effects that do not generalize elsewhere in the parental education distribution (Imbens and Angrist 1994).

The third approach, twin studies, is the one we employ, i.e.,using parents who are themselves identical twins and their children to difference out heritable endowments transmitted from parent to child and parents rearing talents. Antonovics and Goldberger (2005), who re-estimate the Behrman and Rosenzweig (2002) study by re-coding the same data, find father's schooling to increase the child's schooling but no effect of mother's schooling. Recently, Holmlund, Lindahl and Plug (2008), in an extensive review of the causal intergenerational transmission of schooling literature, test the three approaches to estimating causal effects on data for Swedish parents and children. On the twin sample, in line with Behrman and Rosenzweig (2002), they find intergenerational schooling effects for fathers and minor, or no, effects of mother's schooling. However, as they use a pooled sample of fraternal and identical twins they do not claim to difference out heritable endowments to the same extent as when identical twins are separated out. Ours is the first paper replicating Behrman and Rosenzweig (2002) on a new dataset in which identical twins can be distinguished.

The literature thus provides mixed evidence of a causal effect of parental schooling on child development. A general finding is that OLS estimates suffer from upward ability bias. Despite this commonality, a great many differences appear in context, data, outcomes considered, and identification. Holmlund et al. show that identification strategy can explain disparate findings for education-education associations and that twin parent and adoption studies both find strong father and weaker mother causal effects. They analyze all three ways of identifying causal effects while fixing context (Sweden), data (administrative) and outcome (education). By contrast, our study analyzes a range of outcomes while fixing context (Denmark), data (administrative) and identification (twin parents). We show

[1] Lindeboom, Llene-Nozal, and van der Klaauw (2006) and Doyle, Harmon, and Walker (2007) find no effects of mother's schooling on infant and early childhood health. Chevalier and O'Sullivan (2007) find mother's schooling to increase the average birth weight using the same legislative changes as Lindeboom, Llene-Nozal and van der Klaauw (2006), though only by differentiating by grand parents social class.

that identical twin parents provide robust estimates of intergenerational transmission. Parents' schooling does not affect child health or academic achievement, but does affect educational attainment. For recent cohorts mother's schooling increases child's schooling but father's does not.

3. IDENTIFICATION STRATEGY

In this section we describe the empirical strategy. Recent studies estimating the causal impact of parental schooling use only minor variations on a standard reduced-form intergenerational mobility model. It is a model consistent with household resource allocation, where both parents have an effect on their child's outcome. This can be expressed as:

$$Y_i^c = \delta_1 S_i^m + \delta_2 S_i^f + \Gamma_1 h_i^m + f_i^m + \Gamma_2 h_i^f + f_i^f + x_i'\gamma + \varepsilon_i^c \quad (1)$$

Where Y_i^c denotes the outcome of child *c* at adult age, and subscript *i* indexes the family in which the child is born and raised. S_i^m is the schooling of the mother, S_i^f the schooling of the father. h_i^m and h_i^f are the unobserved heritable endowments genetically transmitted from parent to child and f_i^m and f_i^f express the unobserved child-rearing talents of each parent. x_i is a vector of family specific variables such as indicators for fertility decisions like mother's age at birth and total household size; and child demographic variables like gender and year of birth. ε_i^c is the idiosyncratic error term of the child assumed independent of all other terms in the equation. δ_1 and δ_2 are the coefficients of interest. They capture the net effect of mother's and father's schooling on the quality and quantity of time and goods each parent devotes to the child. However, without sufficient proxies for the unobserved heritable endowments *(h)* and parents rearing talent *(f)*, δ_1 and δ_2 will be biased.

Using parents who are pairs of identical twins, model (1) can estimate the causal effect of parental schooling on child outcomes taking into account the effect of the unobserved heritable endowments *(h)* and parents rearing talent *(f)*. A pair of identical twins

have similar genes (Stachan and Read 2004). By definition they therefore share same endowments (h_i^m) *or* (h_i^f) at time of conception, which they transmit to their children. Pairs of identical twins also share same rearing talent (f_i^m) or (f_i^f) partly from growing up together and partly from sharing the same inherited aptitude for parenthood. To estimate the causal effect of mother's schooling on child outcomes we calculate simple differences in mothers' schooling, when mothers are pairs of identical twins. We then estimate its correlation with differences in the outcomes of their children, who are cousins. This eliminates the part of a mother's endowments and rearing talent reflected in her length of schooling. Formally this means:

$$\Delta Y_j^c = \delta_1 \Delta S^m + \delta_2 \Delta S^f + \Gamma_2 \Delta h^f + \Delta f_i^f + \Delta \gamma x_j + \Delta \varepsilon^c, \quad (2)$$

where $\Delta h_i^m = 0$, $\Delta f_i^m = 0$

ΔY_i^c is the difference in outcome between cousins, when their mothers are a twin pair. ΔS^m is the difference in mothers' schooling, when they are an identical twin pair and each is a parent to at least one child. Δh_i^f and Δf_i^f are the differences between the endowments and rearing talents of the (non-twin) fathers, and $\Delta \gamma x_j$ are the differences in parental background and $\Delta \varepsilon_i^c$ is child specific error term, respectively.

By including father's schooling in the equation we can partially eliminate the effect of father's endowments and rearing talent. According to model (2), differencing between pairs of identical female twins only differences out the effect of mother's endowments and rearing talent. Thus we are still left with bias from the part of unobserved endowment effects of the father (h^f) and father's rearing talents (f_i^f) that are correlated with maternal schooling. By applying a few additional assumptions we can minimize this bias. Given the high within twin pair correlation in schooling and the existence of assortative mating, the correlation between schooling of twins' spouses is high (32 % in our sample). Furthermore, heritable endowments have been shown to be correlated with schooling

(Björklund, Lindahl and Plug 2006; Plug 2004). We, therefore, assume heritable endowments between spouses to be correlated. Differencing between (non-twin) father's schooling in model (2) will therefore reduce the effects of father's heritable endowments and unobserved rearing talents that affect the estimate of mother's schooling.

3.1. Same sex fraternal twins as a comparison group

In the twin literature in economics comparisons between samples of fraternal and identical twins have been used to discuss differences between nature and nurture (e.g. Behrman and Taubman 1989), as reference points to the population at large (e.g. Miller, Mulvey, and Martin 2004), and to investigate if zygocity information is needed when taking endowments into account[2] (e.g. Black, Devereux, and Salvanes 2007; Isacsson 1999). Same sex fraternal twins share (on average) 50% of their segregating genes whereas identical twins share 100% of their genes (Stachan and Read 2004). Taking differences within identical twin pair removes all heritable traits. Within fraternal twin pair some heritable traits are removed, but not all. Comparing cross-sectional to within-identical estimates shows the bias due to unobserved heritable traits. We would expect within-fraternal estimates to lie somewhere in-between the cross-sectional and within-identical because not all heritable traits have been removed, and some bias should remain.

The classic epidemiological difference in differences design uses within-identical compared to within-fraternal twins. Both types of twins are assumed to share environments to the same extent, which are removed by the first difference. The differences that remain are attributable to hereditable traits which haven't been removed for fraternal twin pairs.

Combining these two insights in triple difference is informative about the decomposition of the bias. If the within-identical and within-fraternal twin pair estimates are close together but both far apart from cross-sectional estimates, this suggests shared (grandparen-

[2] The trade-off is between a larger sample when pooling identical and same sex fraternal twins or a more homogenous group of twins, when estimating the effects separately. Plomin, DeFries, and McClearn (1990), Goldberger and Karmin (1998) and Stachan and Read (2004) all discuss how samples of identical and fraternal twins besides their share of genetic similarity can differ. E.g. identical twins could be treated more similarly than same sex fraternal twins.

tal) environments when the parents themselves were growing up are more important than heritable traits in explaining the bias. Whereas if within-identical estimates are much father away from cross-sectional estimates than are within-fraternal twin pair estimates, this suggests heritable traits are more important than shared (grandparental) environment.

Apart from being an informative group in their own right, fraternal twins are an ideal comparison group against which to validate our identification strategy. Using identical twins and their children takes endowments and rearing talent into account, but potential obstacles remain in terms of measurement errors and endogeneity, which can bias the estimates (Bound and Solon 1999; Griliches 1979; Neumark 1999).[3] The concern is whether our fixed effect estimates are reduced due to attenuation or selection bias or because we difference out the effect of endowments as we claim. We address measurement errors and endogeneity in section 6.3, but we are not able to provide error corrected measures on the full sample.

Same sex fraternal twins and identical twins are likely to be equally sensitive to errors in variables. Because our data source is administrative, measurement errors are largely due to institutional rather than individual survey respondent level misreporting, making the samples of identical and fraternal twins similarly influenced by errors and not subject to, for example, recall bias.

For an internal validity check of the identification strategy we therefore use a comparison sample of same sex fraternal twins for which differencing does not wash-out all endowments – but are equally sensitive to measurement errors and endogeneity – when estimating the effect of parental schooling on child outcomes. Intuitively this means that as long as estimates on the sample of same sex fraternal twins are at least partially supportive of conventional wisdom that there are positive correlations between parental schooling and child outcomes, we can have greater confidence that smaller estimates found for the sample of identical twins are due to removal of shared endowments rather than attenuation bias.

[3] While classical measurement errors attenuate the estimates, it has been variously suggested that endogeneity can upward bias the estimates, or that the bias can go both ways.

4. DATA

4.1. Data description

We base our estimates on high quality administrative data from Statistics Denmark combined with survey data from the Danish Twin Registry, University of Southern Denmark. In total, we have 6314 (9748) identical (same sex fraternal) twins who are born between 1935 and 1977 and where both twins in the pair are parents of at least one child that survives until 1980.

From the Twin Registry we find information on zygocity (whether twins are identical or fraternal). Begun in 1954, the Danish Twin Registry is the world's oldest survey-based twin registry. It comprises more than 65,000 twin pairs born in 1870 through present (Skytthe et al. 2002).[4] Zygocity classification for all same sex twins is constructed on the basis of four survey questions about twin similarity; a method validated to an overall accuracy of 96 percent (Christiansen et al. 2003). Given zygocity information is only valid for those who completed the survey questionnaire (approximately 70 percent of all twins), we discuss sample selection issues in section 6.5.

All other information is found in databases which are derived from administrative registers and held at Statistic Denmark: The Medical birth Register, National Hospital Patient Register, the Integrated Student Register, and the Integrated Database for Labour Market Research. Using each individual's unique social security number allows us to combine information from the different databases; establish family relations like spouse, siblings, birth order (parity), and household size[5]; and to link information from the registers to the individual twin survey responses.

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[4] We begin our analysis for parents born in 1935 because this is the first cohort for which identification of children in the registers is complete.

[5] Information on family relations before 1968, when the Danish civil registration system began, comes from the last national census which was held in 1970. Here families are asked to identify parents and children residing at the same address. A validation study of parent-child links in the Danish Civil Registration System shows that effectively all children of mothers born from 1935 and onwards are observed in the registers. For

We study the following outcomes:

Infant's health status is obtained from the medical birth register where data is collected directly form medical records in the hospitals. We use information from all children born to twins between 1978 and 2001 on birth weight, birth length, and gestation. As birth weight measure we use actual birth weight which is observed in 10 gram intervals[6] and a dummy for low birth weight using the WHO threshold at 2500 grams. Birth length is measured in centimetres, and we convert this measure into Ponderal index (PI). This is a measure of the relationship between length and weight at birth and mimics the more familiar measure of Body Mass Index (BMI) that is applied to adults, but has been found to be more accurate at predicting future obesity than BMI for infants.[7] We use gestation measured in days, which we convert into a binary variable for pre-term birth, using WHO definition of less than 37 full weeks. Finally, we measure foetal growth as birth weight in grams divided by weeks of gestation to account for complex interaction effects between birth weight and length of gestation.

Early childhood health indicators are found in the National Hospital Patient Register from 1977-2004 and we use information on hospitalization up until age 1 and age 6. Information in this dataset also comes directly from medical journals and contains admissions for each individual. As an indication of health status during childhood we use number of days spent in hospital and calculate the totals until ages 1 and 6. Admission to a hospital reflects both poor health and the behaviour of concerned parents, so the net correlation between parental schooling and admission is not obvious. We therefore use overnight hospital stays rather that the number of contacts with hospital services because the health of the child needs to be assessed by a physician to be poor enough to merit overnight observation.

Academic achievement is first systematically measured in Denmark at 9th grade and we draw this information from the Integrated Student Register. 9th grade is the final

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fathers almost all their children are observed from this date, but not before 1945 is there complete fertility (Pedersen et al. 2006).

[6] The register goes back to 1973, although from 1973 until 1977, birth weight is measured in 250 gram intervals. We restrict the sample in order to limit measurement error.

[7] Ponderal Index is determined by the ratio of the cube root of weight (in grams) divided by length (in cm), multiplied by 100 (Murphy et al 2006). We standardize this measure into percentiles.

year of compulsory schooling and 9th grade GPA is an admission criteria for high school. Most students take the final exams when they are 16 years old, although some enter school early, enter late, or complete 9th grade as adults. We restrict the sample to students completing 9th grade in the age range 15-17. Given 9th grade GPA is only available 2002-2006, we at best observe 87% of a birth cohort with 9th grade GPA.

Schooling of the child is measured as high school completion and completed schooling length.[8] For children born 1956 -1979 we create a binary variable for high school completion, measured as whether or not children have completed one of the three different high school tracks[9] before turning 25 years old. Length of schooling for both parents and children is found in the Integrated Database for Labour Market Research. For all individuals born after 1920 and surviving and residing in Denmark from 1980, this database contains labour market information from 1980 to 2004.[10] Length of schooling is measured with monthly precision which is recoded to years (and fractions of years) for ease of interpretation. For outcomes of infant and early childhood health, parental schooling is measured in the same year that the child outcome is observed.

Children's length of schooling is measured the last observed year in the dataset (mostly 2004) and we exclude children who have not turned 25 in 2004. This leaves us with child birth cohorts 1956-1979 and parental birth cohorts 1935-1957. Furthermore, given that a significant minority of children in Denmark complete education in their late 20's, we also estimate length of schooling for all children at age 30 to address possible concerns about censoring and late completers.

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[8] After high school, students can continue to university/college. University or college is classified into three levels: 2 year college degree; BA is 3 (4) years; or MA which is an additional 2 years after completing a 3 year BA. There are two types of admission criteria. First there is high school GPA; second, partly high school GPA and partly a personal statement. Finally the student can choose to take the 3 year PhD-program. An MA degree is needed to enter a PhD program. Besides the academic tracks of schooling, students can enter vocationally oriented schooling. Students enter vocational training after completing compulsory lower secondary school, and tracks vary in length between 1-4 years (Danish Ministry of Education and Research 2005).

[9] The first track is general academic training; whereas the second and third are vocationally-oriented programs which are either business or technical high school. All three tracks give direct access to college level schooling.

[10] We only use parents born from 1935 when we are confident of observing all children born to each mother. Parental birth cohorts 1935-1957 are also equivalent to the parental birth cohorts used by Behrman and Rosenzweig (2002).

Due to the wide age range, we use different parental cohorts to estimate the effect of their schooling on children's short and long run outcomes. Appendix table A1 outlines the different cohorts we use for the different outcomes. Given that there is little overlap in the cohorts we use for different outcomes, we can not rule out possible cohorts effects, even though we do include child cohort dummies in the regressions. In section 6.3 we comment on estimates from the sub-sample where we have both short and long run outcomes – namely infant health measures and 9th grade academic achievement.

4.2. Summary statistics

In this section we present descriptive statistics. Table 1 lists mean and standard deviations of all variables used in the analysis, whereas Table 2 list within twin-pair variation in these variables; the cornerstone of the identification strategy. Both tables are split into samples of same sex fraternal twins (columns 1 and 2) and identical twins (columns 3 and 4), females and males respectively.

Table 1 shows small differences between the group of identical and same sex fraternal twins. Average length of schooling is 11.7 (11.2) years for female identical (fraternal) twins. For males we find similar differences.[11] For household size the tendency is a higher proportion of female identical twins having two children compared to fraternal twins (50.1 percent compared to 45.7 percent), whereas female fraternal twins in turn have more than two children (42.6 percent compared to 37.9 for female identical twins). Similar differences are not found in the sample of male twins.

For all groups the proportion of married parents is large (around 72 percent). The sample construction partly explains this because valid information on both parents is needed to take endowments from both parents into account.[12] However, during 1957 to

[11] For both samples of male and female twins, mean length of schooling for the sample of identical twins is somewhat higher.

[12] For each dependent variable we do mean comparison between the group of children where we observe both parents and the group where we only observe one of the parents (primarily the mother). The t-tests generally show a significant positive sample selection, although economically the differences are quite small. The selection process follows the same patterns within the groups of singletons, same sex fraternal and identical twins, which assuages to some extent concern about this selection.

2001 (the period of our sample), the total proportion of children born to married couples was 80 percent until 1975, 70 percent in 1980, and stable at around 55 percent after 1985.[13] The 72 percent married couples in our sample therefore also reflect a generally high marital rate at birth of children in Denmark.[14]

Child outcomes for mothers who are identical or fraternal twins are very similar. There is only a 5 gram difference in birth weight, less than 2 grams per week in foetal growth, and less than half a day in hospitalization by age 6. The largest differences are found in length of schooling where children of female identical twins on average have 1.5 months longer schooling than children of female fraternal twins. Further, a t-test reveals that the means of hospitalization and length of schooling are significantly different in the sample of children of identical twins compared to the sample of children of same sex fraternal twins.

Table 1 showed minor differences between identical and fraternal twins and even smaller differences between the children of identical and fraternal twins indicating fraternal twins to be a valuable comparison group for the sample of identical twins. Table 2 reports within twin-pair and within cousin-group variation. We find some differences in within twin pair variation for the samples of identical and fraternal twins, but no differences within the children of twins. 56.0 percent of female identical twin-pairs have different length of schooling and 57.6 percent of male identical twin pairs have different length of schooling. The fractions are similar to those found in previous twin-based studies in economics (Ashenfelter and Krueger 1994; Isacsson 1999; Miller, Mulvey, and Martin 2004). In comparison, 67.0 percent of female fraternal twins and 71.1 percent of male fraternal twins have different lengths of schooling, which does imply more variation within the sample of same sex fraternal twins. For the share of twin-pairs with variation in length of schooling, the average length of schooling is 8.7 months for identical twins and 12.5 months for same sex fraternal twins. The high proportion of twin-pairs with same length of schooling is not

[13]It is quite common in Denmark, not to get married before first the child is born. We code cohabitants as married after 1980 where this information is available.
[14] Data is available from dynamic statistics at Statistic Denmark via www.dst.dk.

to the same extent carried over to the next generation. In general, only 10% of the cousin-groups have exactly the same outcome. The exception is for hospitalizations, where only 26.7 percent of children of identical twins and 36.3 percent of children of fraternal twins have different numbers of days spent at the hospital.

5. RESULTS

We now turn to the estimates of parental schooling on short and long run outcomes in which we compare OLS with twin fixed effect estimates. Table 3 presents the average of these outcomes for both the sample of identical and same sex fraternal twins, female and male respectively. As stated in the previous section, different cohorts are available for different outcomes. All outcomes therefore represent estimations from separate regressions.[15] All regressions are controlled for schooling of the other parent, marital status, gender, whether the child in question is first born, household size, mother's age at birth, and child's birth year.[16]

5.1. Infant and Childhood Health

Birth weight, low birth weight, ponderal index, preterm birth, foetal growth, hospitalization during 1st year, and hospitalization up until age 6 are our infant and childhood health measures. On average, OLS estimations indicate a positive transmission of mother's schooling to each of these outcomes. Birth weight increases by 29.2 grams and ponderal index by 0.5 percentile with an additional year of mother's schooling when mothers are iden-

[15] The tradeoff is an increasing risk of our results being sensitive to the cohorts that we use. For a sub-sample we can estimate both short and long run outcomes in terms of birth weight, foetal growth and 9th grade GPA. The estimates are less precise but the magnitude is similar, when using the same cohorts to estimate both short and long run outcomes. Tables are available upon request.

[16] Several studies estimating the transmission of parental schooling to child outcomes also include a measure of parental income. To include parental income we would reduce sample size significantly. Furthermore whereas schooling changes very little after age 30, income and earnings are time varying. This raises the question when, during a child's upbringing, it would be appropriate to include this variable. However, on a subsample we test the estimates' robustness to including paternal income or earnings, along with the estimates' robustness to excluding the current set of control variables. For this test we use paternal income or earnings the year the outcome is observed when estimating the short run outcomes; and compute average income or earnings over a 10 year period (age 40-49) when estimating the effect of parental schooling on long run outcomes. When applying the fixed effect estimations, there is little change in both size and significance of the coefficients. Tables are available upon request.

tical twins. The effect on foetal growth is practically zero and the probability of low birth weight decreases by 0.5 percentage points for each year of mother's schooling. Similar results are found within fraternal female twin indicating small differences between our sample of interest and our reference group. The exception is the effect of pre-term birth where we do not find any effect using female identical twins, but do find a negative effect using female fraternal twins.

Taking mothers' heritable endowments into account the average effect of her schooling on birth weight is reduced to 18.1 grams for each year of schooling, indicating a causal effect of mother's schooling on birth weight. We find no average causal effect on any other infant health measure, except for foetal growth where the effect is negative and driven by the negative effect of gestation.[17]

The OLS estimates also report a positive effect of mother's schooling on hospitalization at age 1 and hospitalization up to age 6 with one year of additional schooling reducing the risk of hospitalization by 0.05 days by age 1 and 0.1 days by age 6. Both effects disappear when taking endowments into account.

For father's schooling we find a positive effect on birth weight in OLS but the effect disappears when taking heritable endowments into account. Furthermore, there are no effects of father's schooling on ponderal index, gestation or hospitalization in the cross section estimates, indicating that fathers have little measurable impact on the early stages of their children's lives except for a genetic effect on birth weight.[18]

Placing our estimates in the context of previous studies, our average results on birth weight are lower than the 0.5 percentage points decrease in the probability of low birth weight per year of mother's schooling found by Currie and Moretti (2003) in the upper end of mother's education distribution, and the 75 grams increase in birth weight at the lower end of mother's education distribution (Chevalier and O'Sullivan's 2007). Most of

[17] We also estimate the effect on APGAR scores of vital functioning at one and five minutes after birth, but did not find any effects in the cross section.

[18] We also estimate the effect of parental schooling on observed number of accidents, which lead to contact with a hospital, diagnosis of asthma in the childhood, and diagnosis of diabetes in childhood. No cross section estimates showed parental schooling related to these outcomes. This could be due to small sample sizes since we only observe some of these variables for a 10 year period.

our average estimates are in line with the insignificant point estimates found in Lindeboom, Llene-Nozal, and van der Klaauw (2006) and Doyle, Harmon and Walker (2007).

In the birth weight literature, mother's smoking habits are found to be a child health risk. Given that length of schooling affects birth weight through health care decisions and awareness of health risks, the positive effect of mother's schooling can potentially be driven by a strong sorting of people's smoking habits. For example Currie and Moretti (2003) finds mothers smoking habits and assortative mating explains around 80 percent of the total effect of mothers schooling. For children born 1990-1995 we have information on mother's smoking habits during the first trimester of the pregnancy. On this sub-sample we create a dummy for mother's smoking during pregnancy. Because of the small sample size we do not find any causal effect of mother's schooling with or without the smoking indicator. Although, estimated imprecisely, using the 1990-1995 sample of mothers, the effect of mother's schooling on birth weight is +1.7 when no smoking indicator is included. This estimate decreases to -1.7 grams per year of mother's schooling when a smoking indicator is included.[19] Furthermore, the smoking indicator decreases birth weight by 95 grams and is significant at a 10 percent level, indicating that the positive effect of mother's schooling to some extent is confounded by the effect of smoking.

5.2. 9th Grade Academic Achievement

Turning to academic achievement we estimate the average effect of parental schooling on 9th grade GPA. Given that GPA is standardized, cross section estimates report one year of female identical twins' schooling to increase their children's 9th grade GPA with 0.07 of a standard deviation. The results are similar for the group of female fraternal twins.

Taking endowments into account we find no evidence of a causal effect of mother's schooling on 9th grade GPA. The insignificant effects of mother's schooling resembles the findings in Carneiro, Megihr, and Parey (2007) who use an instrumental vari-

[19] The table is available upon request. Total identical twin sample size is 968, within twin pair variation is 22%

ables strategy and find no evidence of mother's schooling on children's math and reading scores, when the children are 12-14 years old.[20]

Compared to mother's schooling, cross section estimations of father's schooling give a slightly lower impact on 9th grade GPA. OLS estimates on the sample of male identical twins show a positive effect of father's schooling increasing 9th grade GPA with 0.04 of a standard deviation. The causal effect of father's schooling is, however, quite different. One year of father's schooling decreases 9th grade GPA by 0.05 of a standard deviation. This is puzzling and might be a negative effect of fathers with long working hours. However, given its small magnitude does not merit further study. As expected, the fixed effect estimations on the sample of fraternal twins are between the OLS estimates and the fixed effect estimates using identical twins.

5.3. High School Completion and Years of schooling

Our final outcomes are high school completion, length of schooling at age 25, and length of schooling at age 30. The OLS estimates show a significant positive effect of parental schooling on all three outcomes. One additional year of female identical twins' schooling increases the probability of high school completion by 0.04 percentage points and 1.4 months of schooling at age 30. The OLS estimates also show the effect of father's schooling to be similar to that of mother's schooling.

The fixed effect estimations on the sample of female identical twins show one year of mother's schooling to increase the probability of high school completion by 1.4 percentage points. For children's length of schooling we find no evidence of a causal effect from mother's schooling. Conversely, we find no evidence of a causal effect of father's schooling on high school completion (the estimate is both insignificant and small in magnitude), but find father's schooling to increase length of schooling by 0.8 months.

The causal impact of father's schooling and no impact of mother's schooling on length of children's schooling corresponds with the findings of Behrman and Rosenzweig

[20] They also find mother's schooling to have 0.10- 0.08 of a standard deviation effect on math and reading scores when the children are 8 years old.

(2005) and Antonovics and Goldberger (2005), and a recent adoption study by Björklund, Lindahl and Plug (2006). The latter find children's schooling increases by 1 month for each year the adoptive father's schooling but an insignificant effect for increase in the adoptive mother's schooling.[21] Plug (2004) also finds similar results on American adoption data.

It is interesting to note that mother's schooling affects high school completion but not length of schooling. If more schooled mothers are better at giving educational inputs than less schooled mothers, we would expect mothers to influence both high school completion and length of schooling. However, one explanation could be that students are more influenced by their parents when they are younger and making secondary school choices compared to tertiary school choices. In a publicly provided education system as in Denmark, students face minimal budget constraints[22] which make their educational choices less dependent of their parents. Nonetheless, most students still live at home when entering upper secondary schooling, and are thereby still influenced by parental human capital on a daily basis. Furthermore, the average age of entering tertiary education is 23.4[23] (Statistics Denmark 2009), an age where most students already have moved away from home and are no longer influenced by their parents to the same extent.

5.4. Possible Explanations for the Reduced Effects of Parental schooling

The results suggest that OLS is substantially upward biased because of endowments and unobserved rearing talents of the parents. In addition, mother's schooling has an effect on infant's health and high school completion, but no effect on academic achievement and length of schooling. In contrast, father's schooling has no effect on early childhood health, but a positive, albeit minor effect on children's length of schooling. In addition to heritable endowments reducing the effect of parental schooling, there are several com-

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[21] This is true except when measuring the probability of children getting a university degree, where they find mothers with a university degree to have a significant impact.

[22] The publicly provided financial student support system was introduced in 1970. The financial support was given to everyone enrolled at high school or above regardless of parental financial situation. Before this financial support system was implemented, students could take up discounted student loans, guaranteed by the government.

[23] Age is estimated using the total population of 15-29 year olds.

peting explanations that might contribute to the generally small effects of parental schooling we have found.

First, several studies have found evidence for a quantity/quality trade-off in terms of family size and length of child schooling. Given that each sibling is given the same statistical weight in estimation we are potentially over-representing larger families and thereby less-schooled parents. To address this concern we estimate the effect of parental schooling on birth weight and length of schooling of firstborn children only, but we do not find this to have any effect on either birth weight or length of schooling estimates. In the same vein, we also estimate the impact of parental schooling on married couples only, also without changing the estimates.

Second, many mothers participate in the labour market while children attend day-care. Since the majority of Danish day-care is heavily subsidized and publicly provided, we would expect this to diminish the effect of parental schooling. We, therefore, take into account mother's labour market participation. This begs questions about the relevant timing of mother's working hours. We consider mother's labour market participation from birth to age 12 when estimating the effect of mother's schooling on 9th grade GPA.[24] This is coded as a dummy variable for full time employment for most of the period.[25] We find similar results as in Table 3.

Third, exclusion of non-twin spouse education as an explanatory variable, in the presence of assortative mating, might load the effects of both parents schooling onto the twin parent. Our preferred estimated include non-twin spouse education to account for spousal endowments. But it might be expected that coefficients on twin parent schooling would be lower in magnitude as a consequence. Appendix tables A2 and A3 show that this is not the case and indeed estimates are always robust to exclusion of non-twin spouse schooling. It may still be the case however that all endowments correlated with schooling

[24] We are not able to estimate the effect of parental schooling on outcomes other than 9th grade GPA when taking working hours into account due to data limitations.
[25] This is derived from observed full-time unemployment insurance status in week 48 each year.

are not differenced out for the spouses. This is addressed with instrumental variables for spousal schooling in the next section.

6. ROBUSTNESS CHECKS

In this section we test robustness of our preferred identification strategy. First we investigate the model specification in terms of (1) non-linearity and (2) time trends in the transmission of parental schooling, (3) estimating both long and short run outcomes, using the same sample (4) measurement errors and endogeneity, and (5) twin-singleton differences.

6.1. Non-linearity and grandfathers schooling

Several studies have found non-linearities in the transmission of human capital, but results are mixed. Higher returns to the bottom of the schooling distribution are found in Black, Devereux, and Salvanes (2005); Chevalier and O'Sullivan (2007) and Chevalier (2004), whereas stronger intergenerational associations are also found at the higher end of the education distribution in Björklund, Lindahl, and Plug (2006) and Currie and Moretti (2003). In this section we investigate if coming from less or more educated families, as defined by the schooling level of the grandfather[26], yield different effects of the mother on the outcome of her child (and his grandchild). Because this requires data from three generations, we are only able to estimate the effect on infant and early childhood health outcomes. We find that our average estimates are primarily driven by the effect of mother's schooling where the grandfather has no more than a high school degree. We find no causal effect of mother's schooling on birth weight when the mother grew up in a family with more schooling, whereas the effect is 27.8 grams when the mother grew up in a family with less schooling.

[26] Level of schooling is defined as less or more schooling. Less schooling is below or equal to 12 years of schooling, and more schooling is above 12 years of schooling. 12 years of schooling is equivalent to high school completion or 3 years of vocational training, we simplify this for the sake of exposition and interpret it as high school completion.

6.2. Time Trends in the Transmission of Parental Schooling

In this subsection we investigate if the estimates are sensitive to time trends. Data provide information on several parental cohorts and throughout we include as many cohorts as are available in order to maximize sample size. However, there are differences in educational opportunities over the span of our sample of parents born 1935-1957 and effects on child outcomes may vary over time. To analyze this issue we present estimates for two outcomes: birth weight and length of schooling. Figure 1 shows OLS and fixed effect estimates of birth weight effects where we consider rolling 10-year windows of parental birth cohorts. In figure 2 we perform a similar exercise with child's length of schooling (beyond age 25) as the outcome.

Figure 1 conveys two important messages. First, a parallel structure between the fixed effect and cross section estimations indicates a constant effect of endowments upward biasing the cross section estimations; second, a downward trend across parental birth cohorts for both the effect mother's and father's schooling on birth weight. The decreasing effect of parental schooling across parental birth cohorts seems reasonable given improved health care information and a generally increasing quality of health practice over time. The causal effect of father's schooling is insignificant across all parental birth cohorts. The effect of mothers schooling is not significant for the last three groups of parental birth cohorts where the coefficients are close to zero.

Figure 2 also shows OLS to be substantially upward biased for both the effect of mother's and father's schooling, and a constant gap between the OLS and the fixed effect lines for the effect of father's schooling. The effect of father's schooling on children's length of schooling is also decreasing and becomes insignificant for the parental cohorts born after 1965. On the contrary for mothers, the gap between the OLS and the fixed effect lines narrows due to a moderate increase in the effect of mother's schooling once taking heritable endowments into account. In general the insignificant effect of mother's schooling remains except for mothers born after 1945 where we find a causal effect of mother's schooling on children's length of schooling.

There are competing explanations of the causal effect of mother's schooling in the later years, no causal effect in the earlier years, and the opposite trend for the effect of father's schooling. That father's schooling has a larger causal effect than mother's in the earlier years is reasonable, given males were dominant in the education system. The large expansions of the education system that occurred during the 1960's and 70's increased equal opportunities for schooling participation. At the same time, female schooling participation was increasing. This opening up of access to education reduced the relative importance of selection on heritable endowments. It is consistent with the reduction in bias implied by within identical twin mother estimates gradually approaching cross section estimates for more recent cohorts.

Another explanation is a reduction in the ratio between variation in schooling and measurement errors because of a general increase in quality of the administrative reported data. We explore this possibility further by estimating the outcome on a sample where we suspect the data quality to be similar. We do not find this to change the results found in Table 3. In any case we are able to estimate a significantly positive causal effect of mother's schooling on children's length of schooling for recent cohorts of parents, but still find the OLS estimates to be upward biased.

6.3. Estimating Short and Long-run Effects, Using Same Cohorts

The former subsection showed the results to be quite sensitive to time trends. In this subsection we estimate the effect of short-run outcomes using the only over-lapping sample of children where we can estimate both short- and long-run outcomes – namely the effect of parental schooling on infant health and student achievement in 9^{th} grade.

Table A4 shows the effect of parental schooling on infant health using the sample of children born 1986-1990, which is also use to estimate the effect of parental schooling on student achievement. With few exceptions, all outcomes are imprecisely estimated because of the reduced sample. However, in line with the graphs in the previous section, the estimates are smaller for all outcomes. This supports the theses that parental schooling has little effect on infant health, when children are born within the recent years. In addition, the

Table also shows the effect of parental schooling on 9th grade GPA, conditional on birth weight. There are no changes in the results, when child's birth weight is included in the model, which indicates that for younger child cohorts are birth weight not a mediator through which parental schooling affects 9th grade GPA.

6.4. Measurement Errors and Endogenous Variation in Twin Pairs' Schooling

Griliches (1979) shows that estimates obtained from differencing sibling or twin pairs suffer from measurement error to a greater extent than cross-section estimates and this may lead to greater attenuation bias. Furthermore, if observed differences within twin pair are endogenous, or driven by unobservables then this may lead to upward bias. We will address both issues in this sub-section, but because of data restrictions we will only provide error corrected estimates for high school completion and length of schooling.

Classical measurement error can be corrected for by instrumenting with an independent measure of the same object, for example Ashenfelter and Krueger (1994). However, Bound and Solon (1999) and Neumark (1999) show that correcting for measurement errors might exacerbate (presumably upward) omitted variable bias. Also, if the measurement error is non-classical, for example mean-reverting, then fixed effect instrumental variables estimates can be more biased than OLS (Black, Berger, and Scott 2000).

We correct for measurement errors by applying instrumental variables. In general we expect a minimum level of measurement errors in our schooling measure because administrative reported schooling stems directly from educational institutions, which reduces noise from e.g. recall bias. Assuming some measurement errors in the administrative reports, we are more confident that they are classical errors and not mean reverting. The first reason is that schooling is measured in months and not rounded to the same extent as self-reports; and second, it is difficult to see why administrative personnel would make reporting errors to a different extent at different educational levels. We investigate our models sensitivity for measurement errors by using two different instruments. The first is an average completion time for each schooling type and the second is self-reported schooling.

The first instrument is average actual completion time for each schooling type. Normal length of schooling for each type of education is defined by the Ministry of Education, but actual schooling length often departs from the norm. Our schooling measure is based on the Ministry of Education norm and we use average completion time for highest completed schooling as our second administrative report of schooling length.[27] The second measurement error instrument comes from a twin survey conducted on cohorts born 1910 to 1955. Twins here reported their highest vocational and academic track, so we combine this information and construct a measure of self-reported length of schooling.[28]

The second point made by Griliches asks why identical twins differ in terms of schooling and outcome of interest, when they are otherwise assumed to be identical. We also address this issue by way of an IV strategy. We use a school proximity reform as an indicator for increased schooling opportunity that increases variation between treated twin pairs; then we use number of years the twins had been living together as an indicator for within twin pair correlation in schooling choice. The original critique of endogeneity builds upon omitted variable bias caused by an imperfect control for endowments in the twin fixed effect estimations. If endowments are positively correlated with length of schooling, then the within twin pair variation in schooling can partly be driven by this omitted variable. There are several reasons for identical twins not to be identical. One example is differences in intrauterine nutrient intake (because of different placement in the womb), which leads to differences in birth weight. In recent research, birth weight has been shown to have a small causal impact on both short and long run outcomes such as mortality, APGAR score, IQ, schooling, and earnings (Behrman and Rosenzweig 2004; Black, Devereux, and Salvanes

[27] For average schooling length to be a valid instrument, measurement errors in parental schooling or in the error term must not be correlated with measurement errors in the instrument. One can question the assumption that measurement errors in parental schooling are uncorrelated with measurement errors in the instrument because both measures stems from the same data source. However, the instrument is constructed on the basis of the general (non-twin) population, using a much broader time window. This means that a possible correlation in measurement errors must be provided by systematic misreporting independent of the specific individual and consistent throughout different time periods.

[28] We use the registers to match the length of schooling for the educational tracks reported by the twins.

2007[29]). Although, Behrman, Rosenzweig and Taubman (1994) and recently also Lundborg (2008) use birth weight differences among other instruments for twin differences, and do not find this to predict within twin pair differences in schooling very well.

More random factors can also explain variation in schooling within a twin pair. Twins separated into different classes, inspired by different teachers, socialize in different peer groups, and so forth, can through different channels be inspired to choose different schooling tracks. Further, in the search for personal identity each twin might even choose to avoid the whole 'who is who' mix-up. Even so, if we fail to take endogenous schooling into account, the literature suggests an upward omitted variable bias particularly when measurement errors are minimal.

Assuming a minimum level of measurement errors, we use an instrumental variable strategy to take this possible endogeneity problem into account. The first instrument we use is a change in the national school legislation of 1958, increasing opportunities to attend school grades 8 and 9 in rural areas in order to match urban access. Before the reform, children brought up in rural areas would transfer to schools in urban areas in order to continue schooling beyond 7th grade. School proximity reform status is determined by municipality of residence at age 13. Given both members of a twin pair are born in the same municipality, both twins in a pair are simultaneously influenced by the reform. Therefore, the reform does not directly create exogenous variation in length of schooling within twin pair. Increasing schooling from 7th to 9th grade was, however, an important threshold for continuous schooling. Therefore the reform not only increased the mean of schooling length but also increased the variance in length of schooling, simply because more pupils had the possibility to enter post-compulsory schooling. Twin pairs exposed to the reform therefore also had larger differences in their schooling choices, lead by the increased range of opportunities beyond the level of 9th grade. We use this policy-induced variation in within twin pair differences in schooling choices as the first instrument.

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[29] Black, Devereux, and Salvanes (2007) use a pooled sample of identical and same sex fraternal twins. We would therefore expect endowments to upward bias their estimates. The statistical significance is reduced to a 10 percent level when separating the samples of identical and same sex fraternal twins.

The second instrument relates to a twin pair's endogenous schooling choice because of growing up together. Assuming equal endowments within a pair we simply argue that twin pairs who continue living together as young adults are also more likely to make similar choices in terms of schooling.

Twin pair's different exposure to school legislation and twins living together are our instruments for variance in schooling within twin pair. The instruments do not explain which one of the twins has most schooling or in other words, they may explain which twin pairs are different, but not the sign of the difference.

Table 4.1 reports first stage estimates for the two measurement error instruments and the two endogeneity instruments[30]. Due to data restrictions we can only instrument for the outcomes: high school completion and length of schooling. The variables IV1 and IV2 are the measurement error instruments; average completion time and twins self-reported schooling respectively. IV3 and IV4 are endogeneity instruments; number of years the twins have been living together and schooling reform, respectively. The schooling reform is coded as four indicators for the distance to nearest school offering 9th grade teaching.

For most instruments the first stage is strong and significant. For the sample of female identical twins, first stage is weak for average completion time and years living together; but the school proximity reform passes an F-test when the outcome is length of schooling.

Table 4.2 reports second stage and baseline estimates for the reduced samples. For baseline estimates we find the twin fixed effect estimates in IV1 and IV4 to mimic those found in Table 3, which is not surprising since the sample size is relatively similar. For IV2 and IV3, the samples are reduced at least by half, which in particular reduces the statistical significance for the sample of female twins. For the measurement error corrected estimates, we find IV1, average completion time, to produce estimates of similar magnitude and significance, except for the effect of female identical twin's schooling on child's length of

[30] As in the main table the specification includes control for spousal schooling. The estimates do not change, if I exclude this variable. Therefore I only present results where I include control for spousal schooling.

schooling where the first stage was not significant. IV2, self-reported education, produces estimates that are a little larger than the baseline estimates, which is consistent with measurement errors attenuating the estimates. However, the estimates are not near the magnitudes of OLS, suggesting that twin fixed effects are an effective way to take endowments into account. For endogenous schooling the evidence is mixed for both instruments, although the trend is that estimated effects are smaller when endogenous schooling is accounted for. Taken at face value these estimates suggest that neither mother's nor father's schooling has an effect on child schooling.

Appealing to assortative mating in the data, we also go further and instrument directly for differences within a twin pair. As described, school proximity reform status is determined by municipality of residence and therefore the school reform only imposes differences between twin pairs. However, their spouses, the non-twin parents, may have been born in different municipalities and faced different reform status. Therefore the idea is that spousal reform status provides exogenous variation in twin parent's education. Allowing for non-linear effects of school proximity (distance quartile to 8th and 9th grade provision) we can instrument the sign and magnitude of twin parents education differences.

Table A5 in the appendix show first and second stage for the estimates where mother's or father's schooling is instrumented by spousal schooling. None of second stage estimates are, however, significant, and only first stage, for the sample of female MZ twins predict spousal school-reform exposure to be a valid instrument for endogeneity in twin mother's schooling choices.

6.5. Twin/non-twin similarity and establishing external validity

Are twins different from non-twins? This is a crucial question if estimates based on twin samples are to be used to make inferences about the (largely non-twin) population in general. A special concern is the long run consequences of twins' lower birth weight. Although current epidemiological studies focusing on twin non-twin comparisons find little effect of twins lower birthweight on their intellectual achievement. The crucial point is that, while the effect of low birth weight is clear, differences in birth weight at the means are less

clear. Twins, whose average birth weight is around 2500 grams have an average intellectual achievement; whereas non-twins, whose average birthweight is 3400 grams, score in the lower 10^{th} percentile, therefore twins and non-twins have different reference points in terms of a low birth weight threshold (Christensen et al 2006 and Christensen and McGuire 2008).

To investigate the issue of twin non-twin comparison in our sample, Table 5 shows basic descriptive statistics and Table 6 show cross section estimates for a 5 percent random sample of the Danish population and for the samples of identical and same sex fraternal twins, equivalent to Tables 1 and 3. While descriptive statistics show similar mean and standard deviation in infant's and early childhood health there is a tendency for OLS estimates from the twin sample to be higher than estimates from the non-twin sample. E.g. one additional year of mother's schooling is correlated with an increase of 29.2 grams in birth weight for the sample of identical twins' children, 32.5 grams of same sex fraternal twin's children, whereas the partial correlation for the non-twin population is 16.1 grams. Likewise one year of mother's schooling is correlated with 0.10 days less spent at the hospital up until age 6, when the mothers are twins whereas the correlation is only 0.03 days less spent at the hospital for mothers who are singletons. In comparison to the short run outcomes, long run outcomes are similar. Summary statistics show children's length of schooling to be 12.8 and 12.9 years for children of twins and non-twins respectively. Furthermore one year of mother's schooling increases the probability for children's high school completion by 3 percentage points when mothers are singletons and 4 percentage points when mothers are identical or same sex fraternal twins. One explanation for this difference between long and short term outcomes is that low birth weight is carried over to the next generation (Black, Devereux, and Salvanes 2007) and since twins are generally smaller at birth than singletons, their children will be more sensitive to health behaviour of the mother. This difference doesn't continue until adulthood, when schooling is measured.

Another reason for observed twin versus singleton differences is that we have a selective sample of identical twins, given the 70 percent response rate for the twin zygocity

survey. We know, for example that response rates are lower for twins with lower schooling.[31]

In summary, the tables show only minor differences between twins' and singletons' length of schooling, despite the fact that twins in general weigh 900-1000 grams less than singletons. While twin parents are observationally quite similar to singletons as adults, the children of both groups as shown in Table 5 are not statistically different from each other. Partial correlations in the cross-section for twins versus singletons show parent schooling is more strongly related to child birth weight for twins, but these differences diminish quickly during childhood.

7. CONCLUSION

Understanding the causal relationship between parental schooling and child development is important for informing theories of intergenerational transmission and for making the case for schooling interventions if benefits to future generations can be measured. The conventional wisdom from a previous generation of studies has been that maternal schooling has a widespread and substantial effect on child development. An effect that tends to be larger than that of the father's schooling, creating a stronger case for public policy programs raising the schooling level among women. Recent papers have used adoptions, twin parents and policy reforms to estimate the causal effect of parental schooling on especially child education. It is this literature that we contribute to by considering a range of child development measures, and extending the twin parents design by using zygocity information which allows us to difference out any effect due to heritable endowments for identical twins, but only partially for fraternal twins.

Same sex fraternal twins are a useful comparison group because although they might not be as similar in other non-endowment related aspects as identical twins, this

[31] We have register information on twins of unknown zygocity. Assuming that mixed-sex and same-sex fraternal twins have similar survey response rates, and knowing the natural ratio of fraternal/identical twinning before introduction of fertility treatment for older cohorts, we can approximate the proportion of identical twins in the unknown zygocity group. We leave the possibility of using this mixed zygocity group for future work.

group still has many similarities to the group of identical twins. In most cases we find the fixed effect estimates on the sample of same sex fraternal twins to be in between the upward biased OLS estimates and fixed effect estimates from the sample of identical twins. This goes some way towards validating our identical twin fixed effect estimates. We also use instrumental variables to attempt to account for measurement errors and endogeneity of differences. We find some evidence of measurement errors attenuating the estimates and some evidence of endogeneity upward biasing the estimates. In general our second stage instrumental variables estimates are not significantly different from zero. Sample sizes are smaller and standard errors are larger of course, but we don't find instrumented fixed effect estimates to be significantly above OLS. This provides further support to the idea that twins allow us to take endowments into account.

We find OLS to be significantly upward biased for all outcomes considered throughout child development. For infant's health we find the largest causal impact of mother's schooling on birth weight, with one year of mother's schooling increasing birth weight by 18.1 grams. A smaller effect of mother's schooling on birth weight than found in the literature that uses American or British data (e.g. Currie and Moretti 2003; Chevalier and O'Sullivan 2007). Similar to Chevalier and O'Sullivan (2007), we find the effect to be driven by mothers who grew up in less educated homes, since the effect is not significant when the grandfather has more than a high school degree. We also find the effect of mother's schooling on birth weight to decrease across parental birth cohorts. There is no effect of mother's schooling when mothers are born after 1965. We find no causal effects of parental schooling on any of the other infant health indicators or on health later in childhood. We relate the small and decreasing effects to universal publicly provided health care and the generally improving public awareness of lifestyle effects on foetal growth.

One year of father's schooling decreases 9th grade GPA by 0.05 of a standard deviation but one year of mother's schooling had no effect. Our final outcome is high school completion and length of schooling. We find one year of mother's schooling to increase the probability of high school completion by 1.4% but find no effect of father's schooling. We are able to replicate the findings of Behrman and Rosenzweig (2002) for Denmark using

the same parental birth cohorts 1935-1957: No causal effect of mothers schooling on child length of schooling, but one additional year of father's schooling to significantly increase children's length of schooling. However, expanding the range of parental birth cohorts and taking time trends into account, we find the downward bias due to endowments to decrease over time. One additional year of mother's schooling increases child length of schooling by one month when mothers are born after 1945. Furthermore, the effect of father's schooling decreases across parental birth cohorts and is not significant when fathers are born after 1945. We relate the zero effect of mothers schooling in the earlier years and the positive effect in the later years to women's increased schooling opportunities and thereby reducing the bias from endowments.

In summary, we find that endowments upward bias OLS estimates, but there is a causal effect of mother's schooling on birth weight, high school completion and length of schooling. Birth weight effects are falling for recent cohorts and schooling effects are increasing. Father's schooling has no impact on early childhood health, reduces 9^{th} grade GPA slightly, and has a positive although small effect on length of schooling. The latter however diminishes over time leading to a generally zero effect of father's schooling on child development in our data.

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TABLE 1: SUMMARY STATISTICS

	Same sex DZ twins				MZ twins			
	Female		Male		Female		Male	
Parental (twin) characteristics								
N	[5,134]		[4,614]		[3,450]		[2,864]	
Age giving birth (firstborn)	26.72	(4.51)	28.54	(4.54)	27.04	(4.44)	28.65	(4.46)
Schooling (years)	11.37	(2.87)	11.82	(2.94)	11.85	(2.77)	12.16	(2.91)
Schooling of spouse (years)	12.19	(2.78)	11.74	(2.70)	12.57	(2.63)	12.02	(2.64)
Child characteristics								
% parents married at birth of child	70.94	[7,591]	71.49	[7,574]	68.63	[4,805]	69.32	[4,461]
% parents married first 10 years of child's life	87.75	[5,008]	83.74	[4,440]	87.66	[2,608]	83.78	[2,273]
% male	51.49	[5,510]	50.75	[5,377]	51.44	[3,601]	51.61	[3,321]
% firstborn	47.98	[5,134]	48.66	[5,156]	49.28	[3,450]	48.7	[3,134]
% one sib household	11.97	[1,281]	12.63	[1,338]	12.53	[877]	12.9	[830]
% two sib household	24.39	[2,610]	48.04	[5,090]	20.95	[1,467]	47.24	[3,040]
% >two sib household	63.64	[6,810]	39.33	[4,167]	66.52	[4,657]	39.86	[2,565]
Infant's health (1978-2001 birth cohorts)								
Birth weight (grams)	3411.47	(630.86)	3430.96	(643.80)	3412.2	(627.76)	3448.93	(631.62)
% low birth weight (<2500 grams)	6.70	[334]	6.02	[174]	6.72	[268]	6.73	[249]
% pre-term birth (gestation<37 week)	5.80	[289]	5.08	[188]	5.22	[208]	4.39	[127]
Fetal growth (grams/weeks)	85.54	(14.17)	86.1104	(14.40)	85.29	(14.24)	86.36	(14.14)
Ponderal Index (percentiles)	51.01	(28.43)	51.09	(28.86)	51.138	(28.64)	51.46	(28.38)
Early childhood health (1977-2001 birth cohorts)								
Hospitalization until 0-12 month of age (days)	1.37	(4.57)	0.96	(3.38)	1.05	(4.10)	0.86	(3.35)
Hospitalization until 0-5 years of age (days)	1.99	(6.41)	1.37	(5.22)	1.61	(6.20)	1.25	(4.68)
Academic Achievement								
9^{th} grade GPA (std dev.) (1986-1990 births)	0.00	(0.96)	0.08	(1.00)	0.05	(0.98)	0.05	(1.03)
Schooling								
% High school completion (1956-1979 births)	40.79	[2328]	41.93	[2,223]	43.39	[1,291]	43.13	[1,170]
Length of schooling (1956-1979 births)	12.86	(2.18)	12.81	(2.24)	12.976	(2.16)	12.91	(2.31)

Standard deviations are given in parentheses, number of observations in brackets. The characteristics of the children are divided according to type of twin parent. Birth weight is measured in 10 gram intervals, low birth weight as below 2500 grams, pre-term birth as before 37^{th} week. Fetal growth is calculated as grams divided by weeks of gestation. Hospitalization is number of overnight stays at a hospital during the first year of life and up until the child turns 6. 9^{th} grade GPA are for pupils taking the exam and receiving a test score, born between 1986 and 1990. High school completion is graduating and obtaining a GPA. Length of schooling is measured in years and calculated the final year the individual is in the dataset above the age of 25.

TABLE 2: DISTRIBUTION OF WITHIN TWIN PAIR DIFFERENCES

	Same sex DZ twins				MZ twins			
	female		male		female		male	
Parent's (twin) characteristics								
Within twin pair variation in schooling	0.99	(1.04)	1.06	(1.176)	0.71	(0.91)	0.71	(1.01)
Fraction twin pair variation >0	67.82	[3,482]	72.96	[3,762]	55.94	[1,930]	58.97	[1,848]
Within twin-spouse variation in schooling	2.67	(2.52)	2.45	(2.202)	2.53	(2.41)	2.30	(2.07)
Fraction twin-spouse variation >0	81.22	[2,792]	77.72	[2,170]	81.91	[1708]	80.15	[1,369]
Child's (of twin) characteristics								
Infant's health								
Within cousin variation in birth weight	603.27	(504.01)	671.75	(555.55)	581.89	(545.55)	664.91	(572.49)
Fraction birth weight variation >0	98.42	[5,341]	98.56	[3,692]	98.51	[4,350]	98.48	[2,920]
Within cousin variation in PI	31.11	(22.69)	33.50	(24.43)	30.606	(22.23)	31.66	(23.47)
Fraction PI variation >0	98.63	[5,096]	98.72	[3,698]	98.66	[4,181]	99.09	[2,938]
Within cousin variation in fetal growth	13.67	(11.20)	15.24	(12.67)	13.14	(11.84)	15.71	(13.04)
Fraction fetal growth variation >0	88.44	[2,066]	99.63	[3,732]	89.55	[1,689]	99.70	[2,956]
Early child hood health								
Within cousin variation in hospitalization (age 0-1)	2.08	(5.26)	1.67	(4.41)	2.08	(5.73)	1.48	(4.68)
Fraction hospitalization variation >0 (age0-1)	30.47	[1,612]	25.22	[918]	23.22	[965]	21.89	[623]
Within cousin variation in hospitalization (age 0- 6)	3.25	(8.35)	2.62	(7.50)	2.67	(8.04)	2.42	(7.77)
Fraction hospitalization variation >0 (age 0-6)	50.81	[2,688]	45.77	[1,666]	46.39	[1,928]	46.87	[1,334]
Academic Achievement								
Within cousin variation in 9^{th} grade GPA	0.88	(0.71)	0.92	(0.69)	0.91	(0.68)	0.98	(0.75)
Fraction 9^{th} grade GPA variation >0	98.91	[362]	99.70	[334]	99.39	[327]	99.64	[273]
Schooling								
Within cousin variation in length of schooling	2.26	(1.841)	2.20	(1.80)	2.08	(1.71)	2.20	(1.80)
Fraction schooling variation>0	88.84	[5,578]	88.69	[4,855]	86.57	[2,790]	86.56	[2,499]

Standard deviations are given in parentheses; number of observations is in brackets. The characteristics of the children are divided into the groups of which their parents belongs e.g. children of female MZ twins. Within twin pair variation is calculated as differences from the mean. The same goes for within cousins' variation. Schooling is measured in months then converted into years; birth weight in grams; Ponderal Index (PI) is estimated as grams/cm^3*100 and then converted into percentiles; fetal growth is estimated as grams divided by weeks of gestation; hospitalization is number of overnight stays at a hospital during the first year and up until the child turns 6; and GPA is measured in standard deviations; high school completion is completion of either of the three types of high school in Denmark.

TABLE 3: THE EFFECT OF PARENTAL SCHOOLING ON CHILD OUTCOMES

	Same sex DZ twins				MZ twins			
	Female		Male		Female		Male	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Infant's health								
Birth weight	32.422***	20.569***	7.181	18.791**	29.222***	18.089**	16.878***	4.858
(grams)	(4.557)	(6.251)	(4.673)	(7.219)	(5.259)	(7.997)	(5.383)	(9.578)
Low birth weight	-0.008***	-0.003	-0.004	-0.006	-0.005**	-0.004	-0.002	0.005
(<2500 grams)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.004)
	[4949]		[3674]		[3938]		[2870]	
Ponderal Index	0.276	0.403	0.477**	0.932**	0.471*	0.556	0.291	-0.006
(grams/cm^3 *100)	(0.213)	(0.336)	(0.218)	(0.366)	(0.251)	(0.433)	(0.251)	(0.480)
	[4840]		[3627]		[3874]		[2825]	
Preterm birth	-0.010***	-0.006*	-0.001*	-0.001*	0.000	0.003	-0.003	-0.003
(<37th week)	(0.002)	(0.003)	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.005)
Fetal growth	-0.001	-0.002	0.000	0.000	-0.003***	-0.005**	-0.001	0.000
(grams/weeks)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
	[3412]		[2387]		[2595]		[2387]	
Early childhood health								
Hospitalization	-0.062**	-0.002	0.021	0.014	-0.053*	0.030	-0.003	-0.020
(age 0-1)	(0.028)	(0.043)	(0.019)	(0.033)	(0.028)	(0.051)	(0.022)	(0.044)
	[4632]		[3454]		[3667]		[2718]	
Hospitalization	-0.110***	-0.042	0.068	0.046	-0.096**	-0.021	-0.057	-0.011
(age 0-6)	(0.036)	(0.058)	(0.042)	(0.068)	(0.039)	(0.073)	(0.052)	(0.068)
	[4753]		[2739]		[3772]		[2057]	

*:10 percent significance level, **:5 percent significance level, ***: 1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. Birth weight is measured in grams, low birth weight as below 2500 grams; Ponderal Index (PI) is estimated as grams/cm^3*100 and then converted into percentiles; preterm birth as before 37th week; fetal growth is estimated as grams divided by weeks of gestation; hospitalization is number of overnight stays at a hospital during the first year and up until the child turns 6; GPA is standardized; high school completion is completion of either of the three types of high school in Denmark. Length of schooling is measured at age 25 as minimum and at age 30. There are multiple observations per child in 9th grade GPA due to different marks for different subjects. We cluster on the family to take multiple observations into account. All regressions have the following covariates: schooling of spouse, birth year of the child, household size, gender, and mother's age at birth of child.

TABLE 3 THE EFFECT OF PARENTAL SCHOOLING ON CHILD OUTCOMES *(Continued)*

	Same sex DZ twins				MZ twins			
	Female		Male		Female		Male	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Academic Achievement								
9^{th} grade GPA	0.073 ***	0.048 ***	0.064 ***	0.051 ***	0.068 ***	0.000	0.040 *	-0.038 ***
	(0.014)	(0.007)	(0.018)	(0.006)	(0.014)	(0.008)	(0.022)	(0.008)
	[608]		[543]		[535]		[426]	
Schooling								
High school completion	0.037 ***	0.025 ***	0.017 ***	0.010 ***	0.035 ***	0.014 ***	0.016 ***	0.004
	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)	(0.006)	(0.003)	(0.006)
Length of schooling	0.140 ***	0.083 ***	0.066 ***	0.042 ***	0.135 ***	0.033	0.120 ***	0.071 ***
	(0.023)	(0.049)	(0.010)	(0.016)	(0.010)	(0.017)	(0.014)	(0.027)
	[5707]		[5302]		[2975]		[2713]	
Length of schooling at age 30	0.119 ***	0.063 ***	0.075 ***	0.060 ***	0.113 ***	0.010	0.143 ***	0.096 ***
	(0.012)	(0.020)	(0.013)	(0.021)	(0.012)	(0.020)	(0.018)	(0.035)
	[4114]		[3415]		[2090]		[1772]	

*:10 percent significance level, **:5 percent significance level, ***: 1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. Birth weight is measured in grams, low birth weight as below 2500 grams; Ponderal Index (PI) is estimated as grams/cm^3*100 and then converted into percentiles; preterm birth as before 37^{th} week; fetal growth is estimated as grams divided by weeks of gestation; hospitalization is number of overnight stays at a hospital during the first year and up until the child turns 6; GPA is standardized; high school completion is completion of either of the three types of high school in Denmark. Length of schooling is measured at age 25 as minimum and at age 30. There are multiple observations per child in 9^{th} grade GPA due to different marks for different subjects. We cluster on the family to take multiple observations into account. All regressions have the following covariates: schooling of spouse, birth year of the child, household size, gender, and mother's age at birth of child.

TABLE 4.A. EFFECT OF PARENTS' SCHOOLING ON CHILD OUTCOME, MEASUREMENT ERROR AND ENDOGENOUS SCHOOLING ARE INSTRUMENTED, FIRST STAGE

	Outcome: High School Completion				Outcome: Length of Schooling			
	same sex DZ twins		MZ twins		same sex DZ twins		MZ twins	
	Female	Male	Female	Male	Female	Male	Female	Male
IV1	0.670***	11.142***	0.266	10.931***	0.056***	0.928***	0.022	0.911***
	(0.159)	(0.044)	(0.200)	(0.059)	(0.013)	(0.004)	(0.017)	(0.005)
IV2	8.213***	9.099***	6.655***	7.102***	8.213***	8.798***	6.655***	7.007***
	(0.460)	(0.436)	(0.465)	(0.483)	(0.460)	(0.410)	(0.465)	(0.420)
	[728]	[632]	[873]	[763]	[728]	[632]	[873]	[763]
IV3	0.857***	-3.046***	0.424**	-1.329***	0.857***	-3.046***	0.424**	-1.329***
	(0.238)	(0.322)	(0.170)	(0.272)	(0.238)	(0.322)	(0.170)	(0.272)
	[1337]	[1057]	[1650]	[1337]	[1337]	[1057]	[1650]	[1337]
IV4 distance 1	-5.933***	-0.106	-9.091***	4.181*	-5.933***	1.767	-0.106	4.181*
	(1.774)	(1.894)	(1.816)	(2.375)	(1.774)	(1.817)	(1.894)	(2.375)
IV4 distance 2	-4.604***	9.530***	2.373	-10.135***	-4.604***	5.405***	9.530***	-10.135***
	(1.449)	(1.889)	(1.854)	(2.538)	(1.449)	(1.804)	(1.889)	(2.538)
IV4 distance 3	-6.555***	-7.372***	-8.689***	12.288***	-6.555***	-2.975**	-7.372***	12.288***
	(1.286)	(1.682)	(1.565)	(2.169)	(1.286)	(1.607)	(1.682)	(2.169)
IV4 distance 4	-5.738***	-3.039*	-10.550***	-10.709***	-5.738***	-4.092***	-3.039*	-10.709***
	(1.339)	(1.685)	(1.587)	(2.204)	(1.339)	(1.636)	(1.685)	(2.204)
	[4232]	[3975]	[2139]	[2004]	[4232]	[3975]	[2139]	[2004]

*:10 percent significance level, **:5 percent significance level, ***:1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. IV1: Average schooling length for each schooling level. IV2: Twins self-reported own schooling IV3: # years the twin pair has been living together. IV4, 8-9th grade school proximity dummies for 1958 reform. IV1 & IV2: correct for measurement errors. IV3 & IV4: correct for endogeneity in schooling choice within a twin pair i.e. explain why the variance in schooling within a twin pair vary. All variables are therefore coded as the variance between the twin pair. High school completion is students with high school GPA scores at age 18-21. Length of schooling is measured at age 25 as minimum. All regressions have the following covariates: schooling of spouse, birth year of the child, mother's age at birth of child, household size, and gender.

TABLE 4.B EFFECT OF PARENTAL SCHOOLING ON CHILD OUTCOMES, MEASUREMENT ERROR AND ENDOGENOUS SCHOOLING ARE INSTRUMENTED, SECOND STAGE

	Outcome: High School completion				Outcome: Length of schooling			
	same sex DZ twins		MZ twins		same sex DZ twins		MZ twins	
	Female	Male	Female	Male	Female	Male	Female	Male
Baseline, restricted sample, IV1	0.025***	0.009***	0.014 **	0.005	0.083 ***	0.040***	0.033	0.072***
	(0.004)	(0.003)	(0.006)	(0.006)	(0.017)	(0.016)	(0.027)	(0.027)
IV1	1.179***	0.006*	2.938	0.002	16.789 ***	0.041***	42.998	0.058**
	(1.179)	(0.004)	(2.214)	(0.006)	(3.984)	(0.017)	(32.396)	(0.028)
	[5704]	[5289]	[2975]	[2702]	[5704]	[5289]	[2975]	[2702]
Baseline, restricted sample, IV2	0.000	0.139***	0.002	0.105	0.031	0.138***	0.011	0.103**
	(0.001)	(0.049)	(0.001)	(0.052)	(0.048)	(0.049)	(0.058)	(0.052)
IV2	0.000	0.194***	0.001	0.117	0.022	0.195***	-0.174	0.116
	(0.001)	(0.069)	(0.002)	(0.095)	(0.076)	(0.069)	(0.118)	(0.095)
	[728]	[632]	[873]	[763]	[728]	[632]	[873]	[763]
Baseline, restricted sample, IV3	0.001	0.003***	0.002***	0.000	0.046	0.139***	0.041	0.110***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.032)	(0.033)	(0.037)	(0.031)
IV3	-0.003	0.001	-0.002	0.007*	0.547*	0.001	0.469	0.302
	(0.006)	(0.002)	(0.011)	(0.004)	(0.330)	(0.104)	(0.592)	(0.224)
	[1337]	[1057]	[1650]	[1337]	[1337]	[1057]	[1650]	[1337]
Baseline, restricted sample, IV4	0.024***	0.010**	0.019***	0.003	0.079***	0.018**	0.060*	0.063**
	(0.004)	(0.004)	(0.008)	(0.007)	(0.020)	(0.018)	(0.036)	(0.031)
IV4	0.080***	0.006	-0.056*	-0.083***	0.370***	-0.048	0.115	0.003
	(0.029)	(0.031)	(0.029)	(0.027)	(0.138)	(0.147)	(0.130)	(0.130)
	[4232]	[3975]	[2139]	[2004]	[4232]	[3975]	[2139]	[2004]

*:10 percent significance level, **:5 percent significance level, ***:1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. IV1: Average schooling length for each schooling level. IV2: Twins self-reported schooling IV3: # years the twin pair has been living together. IV4, 8-9th grade school proximity dummies for 1958 reform. IV1 & IV2: correct for measurement errors. IV3 & IV4: correct for endogeneity in schooling choice within a twin pair i.e. explain why the variance in schooling within a twin pair vary. All variables are therefore coded as the variance between the twin pair. High school completion is students with or without a High school GPA scores at age 18-21. Length of schooling is measured at age 25 as minimum. All regressions have the following covariates: schooling of spouse, birth year of the child, mother's age at birth of child, household size, and gender.

TABLE 5 SUMMARY STATISTICS TWINS AND NON-TWIN COMPARISONS

	Same sex DZ twins				MZ twins				Singletons			
	female		male		female		male		female		male	
Parental (twin) characteristics												
N	[5,134]		[4,614]		[3,450]		[2,864]		[56819]		[51907]	
Age giving birth (firstborn)	26.72	(4.51)	28.54	(4.54)	27.04	(4.44)	28.65	(4.46)	26.53	(4.57)	28.73	(4.84)
Schooling (years)	11.37	(2.87)	11.82	(2.94)	11.85	(2.77)	12.16	(2.91)	11.61	(2.90)	12.08	(2.89)
Schooling of spouse (years)	12.19	(2.78)	11.74	(2.70)	12.57	(2.63)	12.02	(2.64)	12.14	(3.04)	12.14	(2.40)
Child characteristics												
Birth weight (grams)	3411.5	(630.9)	3431.0	(644.0)	3412.2	(627.8)	3448.9	(631.6)		3416.1	(2.8)	
% low birth weight (<2500 grams)	6.70	[334]	6.02	[174]	6.72	[268]	6.73	[249]		6.60	[3430]	
% pre-term birth (gestation<37 week)	5.80	[289]	5.08	[188]	5.22	[208]	4.39	[127]		4.71	[2448]	
Fetal growth (grams/weeks)	85.54	(14.17)	86.1104	(14.40)	85.29	(14.24)	86.36	(14.14)		85.69	(0.07)	
Ponderal Index (percentiles)	51.01	(28.43)	51.09	(28.86)	51.138	(28.64)	51.46	(28.38)		51.12	(0.13)	
Hospitalization until 0-12 month of age (days)	1.37	(4.57)	0.96	(3.38)	1.05	(4.10)	0.86	(3.35)		1.20	(0.02)	
Hospitalization until 0-5 years of age (days)	1.99	(6.41)	1.37	(5.22)	1.61	(6.20)	1.25	(4.68)		1.93	(0.04)	
9^{th} grade GPA (std dev.) (1986-1990 births)	0.00	(0.96)	0.08	(1.00)	0.05	(0.98)	0.05	(1.03)		0.00	(0.01)	
% High school completion (1956-1979 births)	40.79	[2328]	41.93	[2,223]	43.39	[1,291]	43.13	[1,170]		41.45	[24081]	
Length of schooling (1956-1979 births)	12.86	(2.18)	12.81	(2.24)	12.976	(2.16)	12.91	(2.31)		12.78	(0.01)	

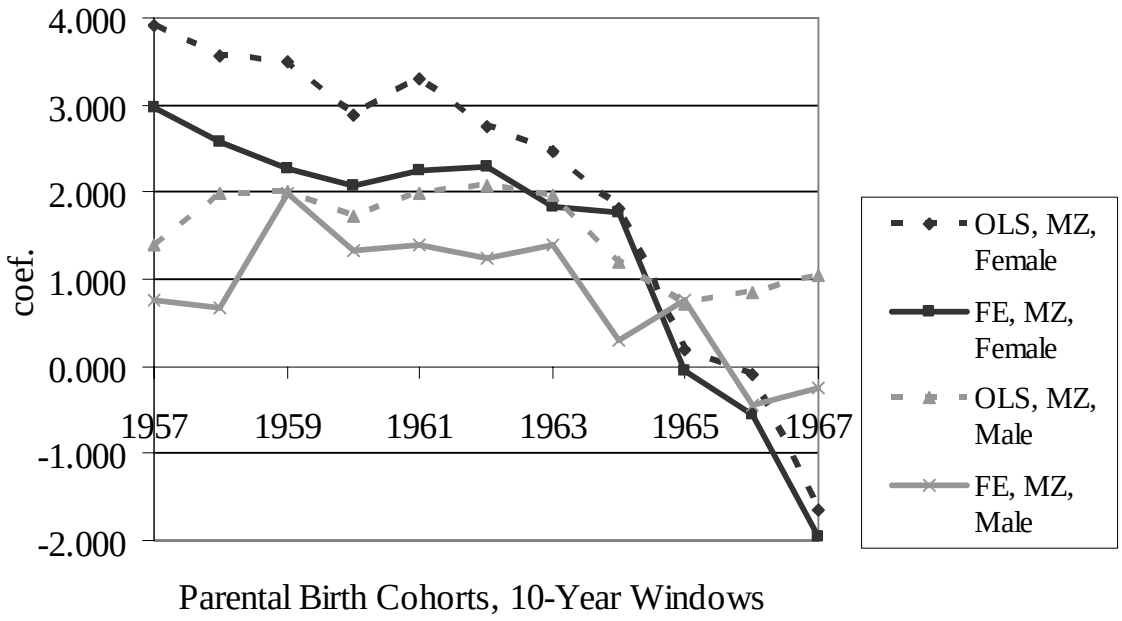
Standard deviations are given in parentheses, number of observations in brackets. Birth weight is measured in 10 gram intervals. Ponderal Index (PI) is estimated as grams/cm^3*100 and then converted into percentiles. Hospitalization is number of overnight stays at a hospital during the first year of life and up until the child turns 6. 9^{th} grade GPA are for pupils taking the exam and receiving a test score (96-98%). Length of schooling is measured in years and calculated the final year the individual is in the dataset above the age of 25.

TABLE 6 TWIN SINGLETON COMPARISON IN CROSS SECTION ESTIMATIONS

	Singletons		same sex DZ twins		MZ twins	
	Female	Male	Female	Male	Female	Male
Infant's health						
Birth weight (grams)	15.053***	5.112***	32.422***	7.181	29.222***	16.878***
	(1.324)	(1.300)	(4.557)	(4.673)	(5.259)	(5.383)
Low birth weight (<2500grams)	-0.002***	-0.001	-0.008***	-0.004	-0.005**	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
	[51499]	[43733]	[4949]	[3674]	[3938]	[2870]
Ponderal Index (grams/cm^3*100)	0.027	0.041	0.276	0.477**	0.471*	0.291
	(0.063)	(0.061)	(0.213)	(0.218)	(0.251)	(0.251)
	[50799]	[43059]	[4840]	[3627]	[3874]	[2825]
Preterm birth (<37thweek)	-0.001***	0.000	-0.010***	-0.001*	0.000	-0.003
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
Fetal growth (grams/weeks)	0.355***	0.083***	-0.001	0.000	-0.003***	-0.001
	(0.036)	(0.036)	(0.001)	(0.001)	(0.001)	(0.001)
	[35901]	[28368]	[3412]	[2387]	[2595]	[2387]
Early childhood health						
Hospitalization (age 0-1)	-0.021***	0.005	-0.062**	0.021	-0.053*	-0.003
	(0.007)	(0.005)	(0.028)	(0.019)	(0.028)	(0.022)
	[48668]	[41591]	[4632]	[3454]	[3667]	[2718]
Hospitalization (age 0-6)	-0.032**	0.003	-0.110***	0.046	-0.096**	-0.057
	(0.014)	(0.008)	(0.036)	(0.068)	(0.039)	(0.052)
	[49774]	[21200]	[4753]	[2739]	[3772]	[2057]
Academic Achievement						
9th grade GPA	0.076***	0.051***	0.073***	0.064***	0.068***	0.040*
	(0.003)	(0.022)	(0.014)	(0.018)	(0.014)	(0.022)
	[11879]	[12246]	[608]	[543]	[535]	[426]
Schooling						
High school completion	0.033***	0.021***	0.037***	0.017***	0.035***	0.016***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
	[58099]	[49935]	[5707]	[5302]	[2975]	[2713]
Length of schooling	0.148***	0.003***	0.140***	0.066***	0.135***	0.120***
	(0.003)	(0.106)	(0.023)	(0.010)	(0.010)	(0.014)
	[58099]	[73123]	[5707]	[5302]	[2975]	[2713]
Length of schooling at age 30	0.138***	0.004***	0.119***	0.075***	0.113***	0.143***
	(0.004)	(0.105)	(0.012)	(0.013)	(0.012)	(0.018)
	[42716]	[34231]	[4114]	[3415]	[2090]	[1772]

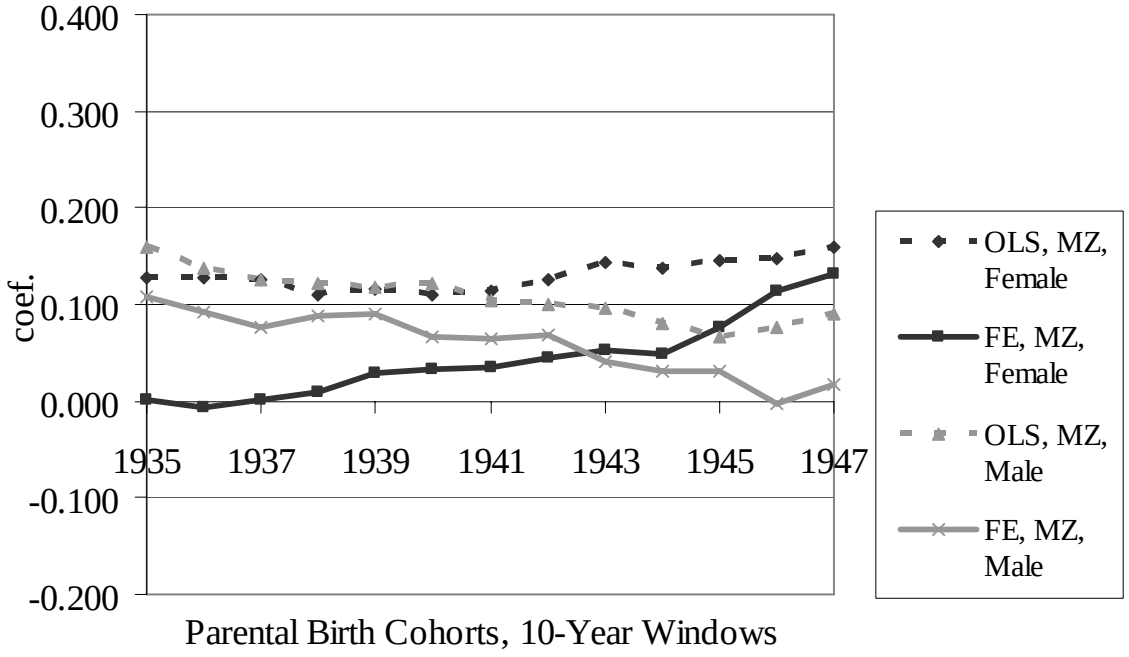
*:10 percent significance level, **:5 percent significance level, ***: 1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. Birth weight is measured in grams, low birth weight as below 2500 grams; Ponderal Index (PI) is estimated as grams/cm^3*100 and then converted into percentiles; preterm birth as before 37th week; fetal growth is estimated as grams divided by weeks of gestation; hospitalization is number of overnight stays at a hospital during the first year and up until the child turns 6; GPA is standardized; high school completion is completion of either of the three types of high school in Denmark. Length of schooling is measured at age 25 as minimum and at age 30. There are multiple observations per child in 9th grade GPA due to different marks for different subjects. We cluster on the family to take multiple observations into account. All regressions have the following covariates: schooling of spouse, birth year of the child, household size, gender, and mother's age at birth of child.

FIGURE 1: THE EFFECT OF PARENTAL SCHOOL ON BIRTH WEIGHT



Each estimate is computed using a 10-year birth cohort window. I.e. the point estimate in 1957 defines parental birth cohorts 1957-1967. For the sample of MZ female twins, the OLS and FE estimates are significant at a 5% level up until 1964. For the sample of MZ male twins the OLS estimates are significant at a 5% level up until 1964, whereas the FE estimates are not significant at a 5% level.

FIGURE 2: THE EFFECT OF PARENTAL SCHOOLING ON LENGTH OF SCHOOLING



Each estimate is computed using a 10-year birth cohort window. I.e. the point estimate in 1935 defines parental birth cohorts 1935-1945. For the sample of MZ female and male twins, the OLS estimates are significant at a 5% level during the whole period. For the sample of MZ female twins the fixed effects estimates are significant at a 5% level for parents born in 1945 and onwards For males fixed effects estimates are significant up until 1943.

APPENDIX TABLES

TABLE A1: PARENTAL AND CHILDREN COHORT DISTRIBUTION

Infant Health	Birth year		
Children cohorts	1978	-	2001
Parental cohorts	1957	-	1977
Hospitalization			
Children cohorts	1977	-	2001
Parental cohorts	1957	-	1977
9th Grade GPA			
Children cohorts	1978	-	2001
Parental cohorts	1952	-	1969
High School Completion			
Children cohorts	1956	-	1979
Parental cohorts	1935	-	1957
Schooling			
Children cohorts	1956	-	1979
Parental cohorts	1935	-	1957

TABLE A2: THE EFFECT OF MATERNAL SCHOOLING ON CHILD OUTCOMES

	DZ Female Twins				MZ Female Twins			
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Control for spousal schooling	No		Yes		No		Yes	
Birth weight	26.88 ***	18.13 ***	25.21	19.09 ***	25.56 ***	17.30 **	29.22 ***	16.84 **
(grams)	(4.26)	(6.20)	(4.44)	(6.25)	(4.96)	(7.81)	(5.21)	(7.84)
Low birth weight	-0.006 **	-0.003	-0.005 ***	-0.003	-0.003 *	-0.003	-0.004 *	-0.003
(<2500grams)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	[4949]				[3934]			
Ponderal index	0.400 *	0.471	0.257	0.400	0.289	0.501	0.432 *	0.529
(grams/cm^3*100)	(0.20)	(0.33)	(0.21)	(0.34)	(0.24)	(0.43)	(0.25)	(0.43)
N	[4840]				[3870]			
Preterm birth	-0.007 ***	-0.005	-0.007 ***	-0.006 *	-0.003	0.002	-0.001	0.003
(<37th week)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Fetal growth	0.489 ***	0.294 *	0.476 ***	0.309 *	0.677 ***	0.300	0.694 ***	0.286
(grams/weeks)	(0.11)	(0.17)	(0.12)	(0.17)	(0.14)	(0.22)	(0.15)	(0.22)
N	[3405]				[2581]			
	-0.047 *	0.003	-0.062 **	-0.002	-0.072 ***	0.023	-0.053 *	0.030
Hospitalization (0-1 years)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)
N	[4632]				[3667]			
	-0.125 ***	0.008	-0.138 ***	0.006	-0.150 ***	0.000	-0.141 ***	0.001
Hospitalization (0-6 years)	(0.04)	(0.07)	(0.04)	(0.07)	(0.05)	(0.11)	(0.05)	(0.11)
N	[3894]				[2989]			
9th grade GPA	0.094 ***	0.060 ***	0.073 ***	0.048 ***	0.094 ***	0.008	0.068 ***	0.000
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	[608]				[535]			
	0.044 ***	0.027 ***	0.037 ***	0.025 ***	0.044 ***	0.018 ***	0.035 ***	0.014 **
High school completion	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
	0.175 ***	0.099 ***	0.140 ***	0.083 ***	0.176 ***	0.046 *	0.135 ***	0.033
Length of schooling	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)
N	[5707]				[2975]			

*:10 percent significance level, **:5 percent significance level, ***: 1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. Birth weight is measured in grams, low birth weight as below 2500 grams; Ponderal Index (PI) is estimated as grams/cm^3*100 and then converted into percentiles; preterm birth as before 37th week; fetal growth is estimated as grams divided by weeks of gestation; hospitalization is number of overnight stays at a hospital during the first year and up until the child turns 6; GPA is standardized; high school completion is completion of either of the three types of high school in Denmark. Length of schooling is measured at age 25 as minimum and at age 30. There are multiple observations per child in 9th grade GPA due to different marks for different subjects. We cluster on the family to take multiple observations into account. All regressions have the following covariates: schooling of spouse, birth year of the child, household size, gender, and mother's age at birth of child.

TABLE A3: THE EFFECT OF PATERNAL SCHOOLING ON CHILD OUTCOMES

	DZ Female Twins				MZ Female Twins			
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Control for Spousal schooling	No		Yes		No		Yes	
Birth weight	12.237 ***	21.010 ***	6.259	19.335 ***	25.929 ***	9.140	16.823 ***	8.205
	(4.42)	(7.12)	(4.59)	(7.15)	(5.01)	(9.49)	(5.30)	(9.49)
Low birth weight,	-0.006 ***	-0.006 **	-0.004 **	-0.006 *	-0.006 ***	0.003	-0.003	0.004
(<2500grams)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	[3674]				[2870]			
Ponderal index	0.555 ***	0.899 **	0.481 **	0.923 **	0.643 ***	0.336	0.448 *	0.319
(grams/cm^3*100)	(0.21)	(0.36)	(0.22)	(0.36)	(0.24)	(0.48)	(0.25)	(0.48)
N	[3627]				[2825]			
Preterm birth	-0.001	-0.002	0.000	-0.002	-0.004	-0.003	-0.003	-0.003
(<37th week)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Fetal growth	0.233 *	0.411 **	0.149	0.381 *	0.521 ***	0.339	0.318 **	0.308
(grams/weeks)	(0.12)	(0.20)	(0.12)	(0.20)	(0.14)	(0.28)	(0.15)	(0.28)
N	[2379]				[1742]			
Hospitalization	0.010	0.006	0.021	0.014	0.004	-0.017	-0.003	-0.020
(0-1 years)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)	(0.02)	(0.04)
N	[3454]				[2718]			
Hospitalization	0.020	0.030	0.046	0.051	-0.027	0.001	-0.032	-0.001
(0-6 years)	(0.04)	(0.07)	(0.04)	(0.07)	(0.04)	(0.10)	(0.05)	(0.10)
N	[2755]				[2073]			
9th grade GPA	0.092 ***	0.063 ***	0.064 ***	0.051 ***	0.066 ***	-0.032 ***	0.040 *	-0.038 ***
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
N	[543]				[426]			
High school completion	0.030 ***	0.015 ***	0.017 ***	0.009 ***	0.031 ***	0.009	0.017 ***	0.005
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Length of schooling	0.121 ***	0.067 ***	0.065 ***	0.041 **	0.175 ***	0.080 ***	0.121 ***	0.071 ***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.03)
N	[5302]				[2713]			

*:10 percent significance level, **:5 percent significance level, ***: 1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. Birth weight is measured in grams, low birth weight as below 2500 grams; Ponderal Index (PI) is estimated as grams/cm^3*100 and then converted into percentiles; preterm birth as before 37th week; fetal growth is estimated as grams divided by weeks of gestation; hospitalization is number of overnight stays at a hospital during the first year and up until the child turns 6. All regressions have the following covariates: schooling of spouse, birth year of the child, household size, gender, and mother's age at birth of child.

TABLE A4: THE EFFECT OF PARENTAL SCHOOLING ON CHILD OUTCOMES, Selected sample (child birth cohorts 1986-1990)

	Same sex DZ twins				MZ twins			
	Female		Male		Female		Male	
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Birth weight	11.810	7.350	24.900 **	14.170	9.490	13.610	-3.950	56.100 **
	(12.42)	(21.29)	(11.32)	(20.05)	(11.33)	(19.71)	(11.61)	(27.03)
Low birth weight	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Ponderal index	-0.420	-0.120	1.210 **	0.020	-0.200	0.610	0.100	-1.420
	(0.63)	(1.13)	(0.59)	(1.13)	(0.64)	(1.23)	(0.64)	(1.49)
N	[585]		[522]		[521]		[404]	
Preterm birth	0.000	0.000	0.000	0.000	0.000	0.010	0.000	-0.010
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Foetal growth	0.120	0.000	0.580 **	0.260	0.270	0.210	-0.050	1.19*
	(0.27)	(0.47)	(0.26)	(0.46)	(0.25)	(0.43)	(0.27)	(0.61)
N	[599]		[538]		[532]		[419]	
9^{th} grade GPA	0.070 ***	0.050 ***	0.060 ***	0.060 ***	0.060 ***	0.000	0.040 ***	-0.030 ***
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Control for birth weight	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	[12528]		[11129]		[11002]		[8558]	

*:10 percent significance level, **:5 percent significance level, ***: 1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. Birth weight is measured in grams, low birth weight as below 2500 grams; Ponderal Index (PI) is estimated as grams/cm^3*100 and then converted into percentiles; preterm birth as before 37th week; fetal growth is estimated as grams divided by weeks of gestation; hospitalization is number of overnight stays at a hospital during the first year and up until the child turns 6. All regressions have the following covariates: schooling of spouse, birth year of the child, household size, gender, and mother's age at birth of child.

TABLE A5: THE EFFECT OF PARENTAL SCHOOLING ON CHILD OUTCOME, ENDOGENOUS SCHOOLING INSTRUMENTED BY SPOUSAL REFORM EXPOSURE

	Outcome: High School Completion				Outcome: Length of Schooling			
	DZ twins		MZ twins		DZ twins		MZ twins	
	Female	Male	Female	Male	Female	Male	Female	Male
First stage								
IV distance 1	-0.11	0.53 **	-0.50 *	0.47	-0.11	0.53	-0.50 *	0.47
	(0.23)	(0.27)	(0.28)	(0.36)	(0.23)	(0.27)	(0.28)	(0.36)
IV distance 2	-0.23	0.35	-0.47 **	-0.32	-0.23	0.35	-0.47 **	-0.32
	(0.21)	(0.26)	(0.24)	(0.31)	(0.21)	(0.26)	(0.24)	(0.31)
IV distance 3	-0.13	0.61 **	-1.23 ***	-0.21	-0.13 **	0.61	-1.23 ***	-0.21
	(0.21)	(0.24)	(0.24)	(0.28)	(0.21)	(0.24)	(0.24)	(0.28)
IV distance 4	-0.15	1.06 ***	-1.28 ***	0.66 **	-0.15 ***	1.06 **	-1.28 ***	0.66
	(0.21)	(0.24)	(0.27)	(0.32)	(0.21)	(0.24)	(0.27)	(0.32)
Second stage	0.02	-0.13	0.04	-0.19	1.04	0.23	0.37 *	0.23
	(0.19)	(0.09)	(0.04)	(0.40)	(1.13)	(0.26)	(0.20)	(0.20)
N	[2227]	[3788]	[4099]	[1856]	[2227]	[3788]	[4099]	[1856]

*:10 percent significance level, **:5 percent significance level, ***: 1 percent significance level. Standard errors are in parenthesis, number of observations in brackets. Covariates included are equivalent to table 3.