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The Intergenerational Transmission of Schooling – Are Mothers Really Less Important than Fathers?

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- Are Mothers Really Less Important than Fathers?

by

Vikesh Amin^{*}, Petter Lundborg[†] and Dan-Olof Rooth[‡]

Abstract: *There is a “puzzle” in the literature on the intergenerational transmission of schooling, where twin studies emphasize the importance of fathers’ schooling, whereas IV-studies often emphasize the importance of mothers. We provide new evidence on this “puzzle” using register based Swedish data on the largest sample of twins used so far in the literature. In contrast to previous twin studies, our results confirm the importance of mothers’ schooling. We also provide the first twin-based evidence of role model effects, where our estimates suggest that only mother’s schooling matters when allowing the effects to differ between sons and daughters. One additional year of mothers’ schooling raises daughter’s schooling by a tenth of a year, which is similar to some of the previous IV-based estimates in the literature. Finally, we bring in new US twin data that for the first time allow a replication of previous twin-based estimates of the intergenerational transmission of schooling in the US. The results show that that mothers’ and fathers’ schooling has no effect on children’s schooling in the US. Our results have implications for assessing the efficiency of policies that subsidize the schooling of men and women and are in contrast to most previous findings in the twin literature.*

Keywords: intergenerational mobility, education, schooling, twins, twin-fixed effects

JEL Classification: J0, J1, I0, I21

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The education of one's parents is often argued to be the most important predictor of one's own education (Haveman and Wolfe 1995). A large number of studies, in both developed and developing countries, confirm this by showing the existence of substantial intergenerational persistence in education across generations (Behrman et al. 2001; Hertz et al. 2007; Chevalier et al. 2009).¹ For policy purposes, however, it is important to understand to what extent such correlations represent causal relations, in which case education policies may have important spillover effects to the next generation. An emerging literature therefore exploits research designs that under certain assumptions allow the researcher to pin down the causal effect of parents' schooling on children's schooling, using for instance a twin design or an instrumental variables design.^{2,3}

While this recent literature has advanced our understanding of the causal mechanism behind the intergenerational transmission of schooling, there remains a puzzle in the literature. Typically, studies based on the twin design and the instrumental variables design come to different conclusions regarding the role of mothers and fathers schooling. While most IV-based studies confirm the common wisdom that mothers' schooling matters the most, twin-based studies almost exclusively come to the opposite conclusion.⁴ In a widely cited twin paper, Behrman and Rosenzweig (2002) showed that father's schooling, but not mother's schooling, mattered for children's schooling in the US. While their study was based on a small sample of twins from Minnesota, a number of recent twin-based studies, based on much larger Scandinavian data sets, also conclude that father's schooling matters more than mother's schooling (Bingley et al. 2009; Hægeland et al. 2010; Holmlund et al. 2011; Pronzato 2012). Summarizing the twin-based literature, Holmlund et al. (2011) conclude that: "...using the difference in twin parents' schooling to identify the effect of schooling on their children, the father is shown to be more important than the mother, a pattern that holds in the U.S. as well as in Scandinavia".

The results from the twin-based literature are controversial, since they contrast to the

¹ Similar patterns are found for income across generations; see for instance Aaronson & Mazumder (2008) for a US context and Björklund and Jäntti (1997) for a Swedish context.

² For recent overviews of this literature, see Björklund and Salvanes (2010), Black and Devereux (2011), and Holmlund et al. (2011).

³ A related literature considers the importance of parental education for children's health and their skills, see Currie (2009) and Currie (2011) for overviews and Lundborg et al. (2014) and the references therein for the most recent studies on the topic.

⁴ We review this literature in Section I.

commonly held wisdom regarding the importance of women's education. If anything, mothers' education is often assumed to be of *greater* importance than fathers' education (Schultz 1993; Haveman and Wolfe 1995; Behrman and; Rosenzweig 2002). One intuitive argument behind this is that greater maternal schooling increases the efficiency of time investments in children and since mothers usually spend more time on child-rearing activities than fathers, the efficiency gain from increased schooling would be greater for mothers (Behrman 1997).

The differences across methods are not of mere academic interest; the conflicting twin-based and instrumental variables evidence gives rise to different policy implications. For instance, an important goal for many societies is to increase educational mobility across generations and the twin-based evidence to date would suggest that it is more efficient to invest in paternal schooling to do so, whereas the instrumental variable evidence suggests investing in maternal schooling. Moreover, in some developing countries, educational policies have been targeted directly towards girls (Behrman and Rosenzweig 2005). Correctly assessing the relative role of mother's and father's schooling for children's schooling is therefore of great policy relevance.

In this paper, we provide new evidence on the relative importance of mother's and father's schooling that contrasts in important ways to previous twin-based findings in the literature. Using large scale data from the Swedish twin registry, we show that the findings of Behrman and Rosenzweig (2002) do not translate to a Swedish context. Our results also contrast to the results obtained in recent studies based on Scandinavian data and are more in line with most of the evidence from instrumental variable studies. Specifically, we find that mother's schooling matters *at least as much as* father's schooling. By further allowing the effects to differ between sons and daughters, we provide the first twin-based evidence suggesting role model effects, where we find that *only* mothers' schooling matter and that it only matters for daughter's schooling.

The effect of increasing mothers' schooling by one year is to increase daughters' schooling by 0.11 additional years. To give a sense of the magnitude of this effect, we can put it in relation to other interventions that affect children's educational attainment. Note also that in Sweden, and probably other countries as well, an important policy issue is to increase college admission rates, meaning a discrete shift by 3 (or 4) additional years of schooling. Our results suggest that this would also increase the schooling of daughters by a third of a year. We can compare this effect with the effect of class size reductions, which has attracted enormous attention in the literature.

The estimates in the recent paper by Fredriksson et al. (2013) suggest that it would take a class size reduction of 6 pupils in primary school classes in Sweden to obtain the same effect on years of schooling. This reduction in class size also happens to be close to the “treatment” in the STAR class size experiment.⁵

Our data has a number of advantages compared to the twin data used in previous studies, which allows us to make several contributions. First, our sample of twins is by far the largest used so far in the literature. We have register-based information on almost 16,000 twin parents, where about 6,450 are monozygotic (MZ) twin parents. Through linkage to the Swedish twin registry, we are able to distinguish between MZ and DZ (dizygotic, fraternal) twins, whereas most studies replicating Behrman and Rosenzweig (2002) rely on samples of twins with unknown zygosity. Our ability to make this distinction turns out to be of great importance. First, we replicate the common finding in the twin literature that father’s schooling matters more than mother’s schooling in a combined sample of MZ and DZ twin parents. In contrast, when we base our estimates on MZ twin parents only, we find that mother’s schooling matters at least as much as father’s schooling. This estimate is clearly more reliable, as the influence of genetics is controlled in the sample of MZ twins. We also show that attempts to calculate the implied MZ estimate by comparing estimates from closely spaced siblings and a pooled sample of MZ and DZ twins, as done in several previous studies, are based on assumptions that do not always hold in the data.

Second, the large sample size allows us to provide, for the first time, estimates of the effect of parents’ schooling on son’s and daughter’s schooling using the within-twins approach. While gender specific intergenerational effects have been ignored in previous twin-based research, they may be important if role model effects are at play. Such patterns have been obtained in descriptive work, where mother’s schooling has been found to have a stronger association with daughter’s schooling than sons’ schooling, while the opposite has been found for father’s schooling (e.g. Behrman 1997). Consistent with these findings, we show that average effects hide important gender specific links, and that the effect of parental schooling is mainly driven by the effect of mother’s schooling on daughter’s schooling.

Third, we are able to provide precise estimates of how the relative importance of mother’s

⁵ We also note that our estimate of 0.11 means that the nurture component of the intergenerational transmission of schooling, which is the policy-relevant one, constitutes a large part of the intergenerational association, which is 0.22 in our sample.

and father's schooling has changed over time. Since educational opportunities have increased over time, the schooling decision of more recent cohorts may be less dependent on their parent's schooling. We show this is the case for fathers, whose schooling exhibited a strong effect on their son's schooling in earlier cohorts but has no effect in more recent cohorts. The effect of mother's schooling on their daughter's schooling has remained constant over time, whereas the effect for son's schooling disappears in recent cohorts. This again suggests that role model effects may be important, where women's entry into the labor market may have had a strong role model effect on daughters, and where this effect dominates any reduced importance of parental background that comes from increased educational opportunities.

Fourth, we are able to address an important criticism of the within-twins approach. The key assumption needed for estimating a causal effect of parental schooling is that schooling differences between twin parents are due to factors that do not directly affect their children's schooling. If for instance cognitive skills differ within twin pairs, and if such skills have an independent effect on their children's schooling, then the within-twins approach will yield upward biased estimates. We address this concern by exploiting data on IQ test scores in a subsample of twin fathers, and find no evidence that the intergenerational schooling effect is affected by controlling for father's IQ.⁶

Fifth, in order to shed more light on the differences between our results and previous US results, we bring in a new twin data set from the US; the Mid-Atlantic Twin Registry. The latter data allows, for the first time, a US-based replication of Behrman & Rosenzweig (2002) using a different data set. With these new data, and in contrast to their study, we find no significant effect of mother's or father's schooling on children's schooling.

Finally, we make some attempts to understand the mechanisms behind our results. In particular, we focus on a number of explanations as to why mother's schooling is at least as important, or more important than father's schooling, such as gender differences in measurement

⁶ Still, we fully recognize that the twin design requires the strong assumption that differences in schooling are unrelated to unobserved differences in other factors that may affect the children's schooling. Since this is unlikely to hold in many circumstances, twin estimates are usually considered to be upward biased estimates of the true "effect", as long as measurement error problems are properly accounted for. It is therefore interesting that many previous twin studies find zero "effects" of mothers' schooling, suggesting that the true effect might actually be negative. Some indication of this is also found in Behrman and Rosenzweig (2002).

error, sample composition, fertility behavior, assortative mating, and social interactions. We are able to rule out some of these explanations and instead argue that our results are in line with recent findings, which suggest that well-educated mothers spend more time with their children compared to low-educated mothers. We also speculate that the difference between our results for Sweden and the US reflect institutional differences regarding maternity leave and childcare that affects child development. In contrast to Behrman and Rosenzweig (2002), we find that although more educated mothers are more likely to participate in the labor market, controlling for labor market participation does not affect the transmission of schooling in Sweden. We also rule out some explanations for the finding that the estimates change more for fathers than for mothers when moving from our pooled sample of MZ and DZ twins to our sample of MZ twins, such as a greater genetic contribution to schooling among fathers than mothers and a greater earnings endowment among fathers.

The paper is organized as follows. The next section reviews the previous twins-based studies. The within-twins methodology and data are described in the third and fourth sections. Our results are presented in the fifth section and are discussed in the sixth section. Finally, the seventh section concludes.

I. Previous work

Behrman and Rosenzweig (2002) was the first study that used the within-twins design to estimate the effect of parental schooling on children's schooling. Using data on 688 MZ twins from the Minnesota Twin Registry they found that an additional year of father's schooling significantly increases children's schooling by 0.36 of a year. For mothers, they found a negative, and marginally significant, effect.⁷ They also showed that increased maternal schooling leads to increased labor market participation and decreased home time of mothers. If home time of

⁷ Their results were criticized by Antonovics and Goldberger (2005), who argued that their results were not robust to alternative coding schemes for schooling and alternative sample selection criteria. Behrman and Rosenzweig (2005) contested Antonovics and Goldberger's recoding, however, and showed that their results were robust to an independent coding scheme. Moreover, in a very narrow sample restricted to MZ twins parents, with children aged 18 or over, all having finished school, Antonovics and Goldberger found a significant effect of father's schooling of 0.40 and no significant effect for mother's schooling.

mothers is a particularly important input in childrearing, then this may explain the negative effect of mother's schooling on children's schooling.

A series of recent papers have replicated the main findings of Behrman and Rosenzweig (2002), using much larger samples of twin parents from Scandinavian register data. In Holmlund et al. (2011), an additional year of father's schooling was found to increase Swedish children's schooling by 0.11 years, whereas the effect of mother's schooling was small and statistically insignificant in models accounting for assortative mating. Similar findings were reported in Norway by Pronzato (2012), where the effect of father's schooling was 0.16. Pronzato (2012) does however find a significant effect for mother's schooling of 0.10. Hægeland et al. (2010) study the relationship between parents' schooling and children's test scores in Norway and also find greater point estimates for father's schooling than for mother's schooling.

Although being able to replicate the gender patterns obtained in Behrman and Rosenzweig (2002), the studies by Holmlund et al. (2011), Pronzato (2012), and Hægeland et al. (2010) all use registry data where they are unable to separate between MZ and DZ twins. Unlike MZ twins, DZ twins are like non-twin siblings, and only share one-half of their innate endowments. The within-twins approach used in these studies will therefore not fully control for the influence of unobserved endowments in a combined sample of MZ and DZ twin parents. If there is no measurement error in schooling, estimates from a sample that includes both MZ and DZ twin parents will most likely be biased upwards. It therefore remains unclear to what extent these replications of Behrman and Rosenzweig's result are biased by unobserved endowments that are not differenced out in a combined sample of MZ and DZ twin parents.

To our knowledge, a working paper by Bingley et al. (2009) is the only other study that uses data on MZ twin parents and their children to estimate the intergenerational transmission of schooling. Using data from the Danish twin registry, they also find that father's schooling is more important than mother's schooling. They find an extra year of father's schooling increases children's schooling by 0.07 of a year, whereas the effect of mother's schooling is insignificant. However, when they divide the sample into twin parents born before and after 1945 they find that the effect of mother's schooling increases and becomes significant, while the effect of father's schooling decreases and becomes insignificant. This may suggest that the effect of mother's schooling becomes greater when more women enter the education system and labor market. The authors do not consider if there are differential effects across sons and daughters.

In sum, the twin-based evidence suggests, that mother's schooling has little impact on children's schooling, although there is some evidence that this may have changed in recent cohorts. This is contrast to the instrumental variables literature on the topic, where most studies find that mother's education matters and in some cases matters more than father's education. In Black et al. (2005), for instance, only mother's education is found to affect children's schooling. Similar results are found in Holmlund et al. (2011), using the Swedish compulsory schooling reform as an instrument for parental schooling, in specifications that limits the sample to the groups affected by the reform. Chevalier (2004) finds positive effects of mother's education, but not of father's education, on children's post-compulsory schooling education, using a change in the compulsory schooling law in Britain in 1957. Lundborg et al. (2014), using the Swedish compulsory schooling reform, showed that mothers' education, but not fathers' education, affected their son's skills and health. Carneiro et al. (2013) focused only on mother's education and found that greater education decreased grade repetition among their children.⁸

An exception from the typical gender patterns described above is Oreopoulos et al. (2006), where the effect of parental education on grade repetition was estimated, using changes in compulsory schooling laws in the US. When they estimate the effect of father's and mother's education separately, they find that father's and mother's education has equally important effects on grade repetition.⁹ Still, and in contrast to the twin based literature, the weight of the evidence from these recent IV studies points to the importance of mothers rather than fathers. This conclusion is also drawn in Holmlund et al. (2011): "Recent twin and adoption studies point to the father, whereas recent IV studies point to the mother as having the strongest impact" (p. 626).

As revealed by the literature review, there is a puzzle in the literature, where studies using the twin design and the instrumental variables method in many cases reach different conclusions. In an ambitious attempt to understand what drives these differences, Holmlund et al. (2011) were able to apply both designs, and the adoption design, to a common data set. In this way, they could rule out that any differences were caused by differences in institutional contexts or across data sets. The difference in results between the designs persisted however, causing the authors to conclude that the difference reflected violations of the identifying assumptions invoked in the

⁸ The importance of mothers' education has also been established for child birth outcomes, such as birth weight (e.g. Currie and Moretti 2003).

⁹ A related study is Maurin and McNally (2008) focusing on the effect of fathers' education on children's probability of grade repetition in France. They find a negative effect of fathers' schooling on grade repetition.

respective methods. Since they were not able to distinguish between monozygotic and dizygotic twins, this might be one source of violation of the identifying assumptions in the twin design, since it is normally assumed that all genetics, and common background, are purged out. This is one important thing we will consider in this paper.

II. Methodology

Consider a reduced-form intergenerational mobility model where the schooling of child i in family j (S_{ij}^c) is related to his/her mother's schooling (S_j^m), father's schooling (S_j^f), unobserved innate endowments of the mother and father (h_j^m and h_j^f), unobserved child-rearing endowments of the mother and father (f_j^m and f_j^f) and a child-specific stochastic term (ε_{ij}^c):

$$S_{ij}^c = \beta^m S_j^m + \beta^f S_j^f + h_j^m + h_j^f + f_j^m + f_j^f + \varepsilon_{ij}^c \quad (1)$$

An OLS regression of relation (1) provides an estimate of the intergenerational association between parents' and children's schooling, which is a biased estimate of the causal intergenerational effect, because parents' schooling is related to the unobserved innate and child-rearing endowments that also directly affect children's schooling. The influence of these unobserved endowments can be controlled for in the within-twins approach, which relates schooling differences between children who are cousins to schooling differences between parents who are twins and to differences in parental unobserved innate and child-rearing endowments.

$$\Delta S_j^c = \beta^m \Delta S_j^m + \beta^f \Delta S_j^f + \Delta h_j^m + \Delta h_j^f + \Delta f_j^m + \Delta f_j^f + \Delta \varepsilon_j^c \quad (2)$$

Relation (2) can be estimated separately for twin mothers and twin fathers. Consider the case of MZ twin mothers. The unobserved endowments Δh_j^m and Δf_j^m should be completely differenced out, as MZ twin mothers are genetically identical and share the same family environment. The effect of mother's schooling is then identified by MZ twin mothers who have different levels of schooling. For DZ twin mothers, these unobserved endowments will not be fully differenced out as DZ twins are not genetically identical. For twin mothers, relation (2) also does not difference out the unobserved endowments of the spouse, Δh_j^f and Δf_j^f , as the spouses

are (usually) not twins. Given assortative mating in the marriage market, schooling differences between twin mothers will be correlated with differences in the unobserved endowments of non-twin fathers. This could cause an upward bias in the estimated effect of mother's schooling on children's schooling. We follow the previous literature by estimating specifications both with and without controls for the spouse's schooling.

There are two inherent limitations with the within-twins approach that may prevent identification of causal effects. First, if MZ twins are truly identical, why do they ever display schooling differences? If schooling differences between MZ twin parents are due to factors that also directly affect their children's schooling, then the within-twins estimate of β^m in relation (2) will be biased. There is little evidence on what causes differences in schooling between MZ twins, but a recent working paper by Sandewall et al. (2009) finds that the within-twins approach does not fully control for innate ability. Using data on 890 pairs of male MZ Swedish twins, they show that cognitive (IQ) test score differences obtained at age 18 when Swedish men enlist in the military, are a significant predictor of schooling differences. They also find that the estimated effect of schooling on earnings is reduced by 15 percent when controlling for cognitive test score differences. If such differences between MZ twin parents also have an independent effect on the children's schooling, then our estimates will be upward biased. While the results of Sandewall et al. (2009) may seem compelling, it is important to note that cognitive test scores at age 18 are likely to have been affected by previous schooling. Meghir et al. (2011) using the same Swedish cognitive test score data as Sandewall et al. (2009) show that the Swedish compulsory schooling reform increased the cognitive test ranking of boys by 2.5 percent of a standard deviation. Similar findings have been reported on US data (Cascio and Lewis 2006) and for Norway (Brinch and Galloway 2012).^{10,11} Given such results, it is still possible that the twins in the Sandewall et al. (2009) study started out with rather equal abilities but due to more or less random circumstances ended up with different levels of schooling during childhood and adolescence, which generated subsequent differences in cognitive test scores. For example, it is

¹⁰ See also Cahan and Davis (1987) for an overview of studies finding a causal effect of schooling on achievement.

¹¹ Sandwell et al. (2009) acknowledge this concern, and as a robustness test they use birth weight differences as a proxy for ability differences, arguing that this source of variation should partly capture variation in ability that is unaffected by schooling. When they control for birth weight, the estimated effect of schooling on earning is only reduced by 0.5 percent.

common to put twins in separate classes at school, where differences in teacher characteristics could generate differences in schooling investments between otherwise similar twins.

Our results could also be biased if birth weight differences between MZ twin parents also directly affect their children's schooling. However, twin-based estimates on the effect of birth weight on schooling are mixed in the literature. Royer (2009) finds a small significant effect, where increasing birth weight by 250 grams (which would be quite a policy achievement), only leads to 0.03-0.04 of a year of additional schooling. Black et al. (2007) find that a 10 percent increase in birth weight increases the probability of high school completion by just 1 percentage point. Behrman and Rosenzweig (2004) find a slightly large effect with a 1 pound (453 grams) increase in birth weight increasing schooling by a third of a year. Miller et al (2005), using data from the Australian twin registry, find no significant relationship between birth weight and schooling. In our sensitivity analysis, we address the concern that differences in cognitive test scores between our twin parents may affect our estimates, by controlling for IQ test scores for a subsample of twin fathers where we have information on IQ test scores at age 18. We do not have access to birth weight data, so we are unable to assess the sensitivity of our results when controlling for birth weight.¹²

The second concern with twin-based estimates is the attenuation bias due to measurement error in schooling. Assume that measured schooling (S_{ij}^*) is linearly related to true schooling (S_{ij}) but is measured with random measurement error w_{ij} : $S_{ij}^* = S_{ij} + w_{ij}$. Then the within-twin intergenerational effect is biased downwards by: $\sigma^2(w_{ij})/(\sigma^2(S_{ij})(1-\rho_S))$. Here, ρ_S is the correlation in schooling levels between the twins and $\sigma^2(w_{ij})/\sigma^2(S_{ij})$ is the reliability ratio. In our sensitivity analysis, we also address to what extent measurement error is of importance for our findings.

¹² Another source of bias is differential treatment of twins by parents. Parents may try to compensate for, or amplify small differences between the twins by investing more or less in the less able or less healthy twin. Such compensating behavior would be present in a model in which the returns to child investments are greater for the least able child in the family and the parents only care about maximizing the returns to the investments (Becker and Tomes, 1976; Behrman et al. 1982; Rosenzweig and Schultz, 1982). The small twin-based evidence to date, using various measures of parental inputs, does not suggest that parents systematically reinforce or compensate for early life differences (see for instance Royer 2009 and Almond and Currie 2011). Moreover, Isacson (1999) finds no relation between psychological instability early in life, being an imperfect proxy of parental rearing skills, and years of schooling among Swedish twins.

III. Data

Our empirical analysis is based on a data set constructed by integrating registers from Statistics Sweden and the Swedish Twin Registry. The former register contains register-based information on the Swedish population aged 16–64 years old in 1999. Data on children’s education, in terms of years of schooling, is taken from a 2007 update, while parents’ years of schooling are taken from the 1999 register. Our data also links parents to children, including information on whether the parent is a twin and who his/her twin sibling is.

Our sample consists of all twin parents born 1925–1965 and their children born 1945–1983. In the twin registry, zygosity has been determined based on survey questions regarding co-twin similarity during childhood. The twin registry has validated this method repeatedly with DNA and found it to have a 98% or higher accuracy (Lichtenstein et al. 2002). Our data set includes 8,568 same-sex twin parental pairs with 32,667 children. Of these, we have access to zygosity information for 92 per cent of the pairs. Our final sample size is 7,867 twin pairs, out of which 3,225 are (classified as) MZ. Educational attainment is expressed in terms of the highest degree attained. Our measure of years of schooling is assigned based on the standard number of years of schooling associated with this degree. The measure is based on register data from Statistics Sweden.

IV. Results

A. Descriptives

One of our aims is to understand whether estimates of the effects of parents’ schooling obtained from a combined sample of MZ and DZ twin parents differ substantially from those obtained by using MZ twin parents only. Table 1 first provides summary statistics for our pooled sample of MZ and DZ twin parents and MZ twin parents only in the first two columns. The distribution of birth cohorts (for both twin parents and children) is the same in both samples. Average years of schooling for children, twin parents and spouses is slightly higher in the MZ twin parents sample compared to the combined MZ and DZ twin parents sample. The third column shows average years of schooling for the corresponding non-twin population, weighted

by the age distribution in the twin sample. Average years of schooling for mothers, fathers, sons and daughters in the general population are not very different from average years of schooling of twin parents and their children.

B. Results for monozygotic and dizygotic twins pooled

In order to facilitate a comparison to previous results in the literature, Table 2 reports intergenerational associations (OLS) and effects (within-twins) in years of schooling for our combined sample of MZ and DZ twin parents and their children. The estimated intergenerational associations, shown in the first two columns of panel A, are 0.23 for both mothers and fathers. In the third column, when we restrict the effect of father's and mother's schooling to be equal, the estimated effect of parental schooling is also 0.23. These OLS estimates are very similar to those obtained in Holmlund et al. (2011). When we control for assortative mating, by controlling for the spouse's schooling, the estimated intergenerational associations fall to 0.17. In columns four to six, we show the estimated intergenerational effects. As expected, these intergenerational effects are smaller in magnitude than the intergenerational associations, since we now partly control for unobserved endowments. Moreover, we are able to replicate the finding in the literature that father's schooling matters more than mother's schooling. Our results suggest that one additional year of father's and mother's schooling increases the child's schooling by 0.12 and 0.06 years, respectively, when controlling for spousal schooling. The magnitude of our point estimates are very similar to those obtained by Holmlund et al. (2011) of 0.11 and 0.04 for their pooled sample of MZ and DZ Swedish twin parents.

Panels B and C report intergenerational associations and effects for sons and daughters respectively. There is some weak evidence from the intergenerational associations that role model effects may be important. Father's schooling has a larger effect on son's schooling (0.20) than daughter's schooling (0.13), although they are not significantly different from each other. In contrast, mother's schooling affects son's and daughter's schooling almost equally by about 0.16. The intergenerational effects show that father's schooling is more important than mother's schooling. The effect of father's schooling on son's schooling is triple the size of the corresponding effect of mother's schooling. For daughter's, the effect of father's schooling is also

larger, but not significantly different from the effect of mother's schooling.¹³ Again, there is some indication of role model effects, with the point estimates of father's (mother's) schooling being larger for son's (daughter's) schooling.

C. Results for monozygotic twins

We next turn to our sample of MZ twins (Table 3), where we are able to fully account for the influence of genetics. The intergenerational associations suggest that father's and mother's schooling affects children's schooling equally. The magnitude of the intergenerational associations for our sample of MZ twin parents are identical to those obtained in our sample of MZ and DZ twin parents. As expected, controlling for more unobserved endowments using MZ twin parents produces intergenerational effects that are smaller than the corresponding intergenerational associations. The intergenerational effects of father's schooling are now substantially smaller than those obtained from the combined sample MZ and DZ twin parents in Table 2, however. The estimated effect decreases from 0.12 to 0.05 when we restrict the sample to MZ twin fathers (and control for assortative mating). Interestingly, there is no such decline in the estimated effect of mother's schooling, which is still 0.06. This suggests that unobserved endowments are more important when estimating the effect of father's schooling than mother's schooling. Mother's schooling is now at least as important as father's schooling for children's schooling and both the effects of mother's and father's schooling are significant.¹⁴

The intergenerational associations by child gender in panels B and C in Table 3 again indicate possible role model effects. Father's schooling is more strongly associated with son's schooling and mother's schooling affects sons and daughters equally. However, the intergenerational effects lead to a different conclusion. Neither father's nor mother's schooling

¹³ Although the estimates of mother's schooling are smaller than the estimates for father's schooling, whether or not we allow for gender specific effects, they are still significant at the 1 percent level. This contrasts to most of the previous studies that generally find insignificant effects of mother's schooling. To some extent, this can probably be explained by the much larger sample size exploited in our analyses.

¹⁴ We have also restricted our sample to MZ twin parents born 1936-1955 and children born 1945-1983 to exactly replicate Behrman and Rosenzweig (2002). The intergenerational effects for mother's and father's schooling are 0.052 and 0.051 respectively, (both are statistically significant), again indicating that mother's schooling is at least as important as father's schooling.

significantly affects son's schooling when accounting for spousal schooling. There is also no longer any significant effect of father's schooling on daughter's schooling. Instead, there is a striking result, where one additional year of mother's schooling increases daughter's schooling by 0.10 of a year.¹⁵ This result is consistent with the existence of role model effects, where female role models are important for daughters.¹⁶ The estimate is also remarkably similar to the IV estimate in Holmlund et al. (2011), where an increase in mothers' schooling is found to increase children's schooling by 0.11 years.

D. Why do implied MZ intergenerational effects differ from estimated MZ effects?

We reach the same conclusions as Holmlund et al. (2011), Pronzato (2012), and Hægeland et al. (2010) that father's schooling matters more than mother's schooling, when we base our sample on MZ and DZ twin parents. In contrast, using MZ twin parents only, mother's schooling is at least as important as father's schooling. In the absence of zygosity data, it is possible to calculate the implied intergenerational effect for MZ twins under certain assumptions. The intergenerational effect estimated on a pooled sample of MZ and DZ twins is a weighted average of MZ and DZ intergenerational effects. If one assumes that (1) estimates for DZ twins and closely spaced siblings are similar and (2) the proportion of MZ twins is about 50 per cent in a sample of same-sex twins, then the implied MZ intergenerational effect is twice the intergenerational effect from the pooled sample minus the intergenerational effect from a sample of closely spaced siblings.¹⁷ Based on these calculations Holmlund et al. (2011) and Pronzato

¹⁵ While there are no other twin-based studies that estimate the effects of parents' schooling separately by child gender, there are a number of IV and siblings-based studies that do. Black et al. (2005) and Piopiunk (2011) find that mother's schooling matters for son's schooling, but not for daughter's schooling, based on IV estimates using changes in compulsory school leaving laws in Norway and Germany respectively. Chevalier (2003) finds that mother's schooling affects daughter's schooling but not son's schooling using IV estimates for the United Kingdom. Ermisch and Pronzato (2010) also find the same as Chevalier (2003) for Norway using the within-siblings approach.

¹⁶ We also investigated non-linearities in the effect of parental schooling by also including years of schooling squared in our regressions. The coefficient on years of schooling squared was always insignificant.

¹⁷ The average effect of parents' schooling on a pooled sample of MZ and DZ twin parents is $\beta_{\text{Twins}} = \theta\beta_{\text{MZ}} + (1-\theta)\beta_{\text{DZ}}$ where θ is the share of MZ twins. Assuming that θ is 0.5 and closely spaced siblings can be used to proxy for DZ twins, then $\beta_{\text{MZ}} = 2\beta_{\text{Twins}} - \beta_{\text{SIB}}$. A complete derivation of the formula is provided in Holmlund et al. (2008).

(2012), who are both unable to distinguish between MZ and DZ twins, argue that the conclusion that father's schooling is more important than mother's schooling would still be reached had they been able to identify and use MZ twin parents and their children.

If we were unable to separate out MZ and DZ twins, we would also reach the same conclusion as Holmlund et al. (2011) and Pronzato (2012). In column one of Table 4, we first reproduce our estimates of intergenerational effects from the pooled sample of MZ and DZ twins. In column two, we then show the intergenerational effects estimated on a sample of siblings born one to two years apart. The third column shows the calculated implied intergenerational effect for MZ twins, where the share of MZ twins is 0.41 in our data.¹⁸ As in Holmlund et al. (2011) and Pronzato (2012), our *implied* MZ intergenerational effects show that father's schooling is still more important than mother's schooling for children's schooling. However, our *estimated* intergenerational effects, based on the actual sample of MZ twins in Table 3, show that mother's schooling is at least as important as father's schooling for children's schooling and that mother's schooling is more important than father's schooling for daughter's schooling.

Why are the *implied* MZ intergenerational effects substantially different from our *estimated* MZ intergenerational effects? Clearly, the difference in results casts doubts on the assumptions used when calculating the implied MZ estimate. Although it may appear reasonable to assume that intergenerational effects from a sample of closely spaced siblings are similar to intergenerational effects from a sample of DZ twins, this is apparently not true in the data. For example, the intergenerational effect of father's schooling controlling for assortative mating, on children's schooling is 0.16 based on DZ twins only (results available on request), which is 60 per cent larger than the corresponding estimate of 0.10 obtained on our sample of closely spaced siblings. In contrast, the estimate for DZ twin mothers is 0.06, which is smaller than the corresponding estimate of 0.10 obtained on our sample closely spaced sibling mothers. This shows that it is vital to have information on zygosity, because the conclusions drawn by looking directly at data on MZ and DZ twins are substantially different from the conclusions obtained

¹⁸We do not know why the share of MZ twins is not exactly 0.5. One reason could be that some MZ twins have been misclassified as DZ. In this case, the true difference in intergenerational effects between the pooled MZ and DZ sample and the MZ sample should be even greater. We note that in Bingley et al. (2009) the share of MZ twins is 0.39.

using assumptions about the effects among DZ twins and closely spaced siblings.

E. Estimates across cohorts

Table 5 reports intergenerational effects for MZ twin parents born 1925–1939 and 1940–1965, to investigate whether the relative importance of mother’s and father’s schooling has changed over time. Educational opportunities have increased over time in Sweden so the importance of parental schooling may have declined in later cohorts. At the same time, the labor force participation rate of women in Sweden has increased dramatically. In 1965, the labor force participation rate was 54 per cent, which increased to 82 per cent by 1990 (see Stanfors, 2007, Table 3.2).¹⁹ The entrance of Swedish women in the labor market may also have affected the transmission of schooling between mothers and children. As shown in panel A, the effect of parents’ schooling on children’s schooling has decreased over time, which is what one would expect as educational opportunities also increased. In the pre-1940 cohorts an extra year of father’s or mother’s schooling increased children’s schooling by 0.10 of a year.²⁰ However, in the post-1940 cohorts there is no longer any significant effect of father’s or mother’s schooling on their children’s schooling. A similar conclusion is reached in panel B, where both mother’s and father’s schooling is found to affect son’s schooling in the pre-1940 cohorts, but where there is no such effect in the post-1940 cohorts. In contrast, the effect of mother’s schooling on daughter’s schooling is very robust over time. In both the pre-1940 cohorts and the cohorts born 1940–1965, an additional year of mother’s schooling increases daughter’s schooling by about 0.10 of a year. This finding indicates that Swedish women’s entrance into the labor market may have positively affected their daughter’s investment in schooling, which may counteract the diminished influence of parental schooling that followed from increased educational

¹⁹The corresponding figures for men are 89 and 87 percent respectively.

²⁰Table A1 in the Appendix also reports intergenerational effects for MZ and DZ twin parents born 1925–1939 and born 1940–1965. The results show that the importance of father’s schooling has fallen over time, but remains more important than mother’s schooling over time. This again illustrates that it is vital to have information on zygosity to separate out MZ and DZ twins, since the results obtained for MZ twin parents show that mother’s schooling is at least as important as father’s schooling in the post-1940 cohorts.

opportunities. It is striking that that the effect of father's schooling disappears, where there was no change in their labor market participation rate during the study period.²¹

F. The role of ability differences between twins

Our intergenerational effects will still be upward biased if there are individual twin-specific endowments which are not differenced out in the within-twins approach, as evidenced in Sandewall et al. (2009). We test the robustness of our results by controlling for IQ test scores at age 18 for a subsample of 1382 twin fathers and their children. The intergenerational association in column 1 of Table 6 shows that an extra year of fathers schooling increases children's schooling by 0.25 of a year based on a sample of MZ and DZ twin fathers. This is very similar to the intergenerational association for the full sample of MZ and DZ twins in Table 2, indicating that our subsample with information on tests scores appears representative. When we control for IQ test scores in column 2, the intergenerational association decrease by a third to 0.16. The within-twins intergenerational effect in column 3 is 0.077. Controlling for IQ test scores in column 4 does not affect this estimate. It remains at 0.077 and is significant at the 10 percent level. Moreover, the estimated effect of cognitive test scores on children's schooling is very close to zero and insignificant. Columns 5 to 8 repeat the exercise using MZ twin fathers only. The intergenerational association in column 5 is 0.28, similar to the result in Table 3. Controlling for IQ test scores reduces the intergenerational association to 0.22. The intergenerational effect in column 7 is 0.09 and is insignificant. The estimate however does not decrease when adding IQ test scores in column 8. This suggests that while the criticism of Sandewall et al. (2009) may be valid when estimating the effect of schooling on earnings, it is not necessarily valid when studying outcomes across generations, since it is less obvious that there is a causal effect of parental cognitive ability on children's schooling, conditional on the parent's schooling. We do not know to what extent the findings generalize to twin mothers. If one argues that ability differences may still play a role for our results, one would also have to argue that such ability

²¹ The insignificant results for fathers in the later cohorts is not driven by sample size, as there are more twin pairs with schooling differences in the post-1940 cohorts compared to the pre-1940 cohorts.

differences are more important predictors of mother's schooling than of father's schooling. We are unaware of any evidence showing that ability is a more important predictor for women's schooling than for men's schooling.²²

G. Summary

In summary, we have replicated the finding that father's schooling is more important than mother's schooling for children's schooling based on a pooled sample of MZ and DZ twins. In contrast, using MZ twins only we find that mother's schooling is at least as important as father's schooling. The finding that mother's schooling matters at least as much as father's schooling appears to be driven by the effect of mother's schooling on daughter's schooling. Our conclusion that mother's schooling matters, is however different from Bingley et al. (2009), who find no significant effect of mother's schooling based on Danish MZ twin mothers, except for the most recent cohorts. This is surprising as Sweden and Denmark are neighboring countries with many similarities in the institutional contexts. There are differences in the sample restrictions and sample size, which may account for the different conclusions. We have a much greater sample size, 13,623 children of MZ twin parents, whereas Bingley et al. (2009) sample consists of 3,862 children when studying years of schooling as an outcome. We also use a wider span of cohorts, with children born 1945-1983 and parents born 1925-1965, whereas Bingley et al. (2009) sample consists of children born 1956-1979 and parents born 1935-1957. When we restrict our sample to the same cohorts as in Bingley et al. (2009), our results do not change to any important extent.²³

V. Discussion of results

Our results raise a number of questions. First of all, and perhaps most importantly, why does mother's schooling appear to matter at least as much or more than father's schooling in Sweden? Second, why do our results differ from Behrman and Rosenzweig (2002) for the US,

²² We also analyzed if there is heterogeneity in the effect of father's schooling by level of IQ. We found no evidence of this, as the interaction term between schooling and IQ was insignificant.

²³ The effect of mother's and father's schooling change slightly to 0.063 and 0.065 respectively, and both are still significant at the 1 percent level.

where father's schooling is found to matter more than mother's schooling? Third, why do our estimates for the sample of MZ twins differ from our estimates for the pooled sample of MZ and DZ twins? In this section, we address these three questions, with the main focus on the first question. The tone is admittedly speculative, since we lack the information to investigate a number of potentially important explanations for our findings.

A. Why does mother's schooling appear to matter at least as much or more than father's schooling in Sweden?

We first consider possible gender differences in measurement error in parents' schooling. A purely mechanical explanation for the finding that mother's schooling matters at least as much or more than father's schooling would be that the estimated intergenerational effects for MZ twin fathers suffer from a greater attenuation bias than the corresponding effects for mothers due to more severe measurement error in schooling for fathers. In the within-twins design, the attenuation bias depends both upon the reliability ratio and the correlation between schooling levels of the twins. We do not expect such differential measurement error by gender to be of importance for our results for two reasons. First, the schooling data are based on register information and not on self-reports. Holmlund et al. (2008) report that the reliability ratio for both fathers and mothers is 0.95, using Swedish register data. Second, the correlation in schooling for MZ twin fathers and mothers is also the same, at about 0.68 in our data which, together with the reliability ratio, means that any attenuation bias should be the same for mothers and fathers.

Another explanation for the change in the relative importance of mother's and father's schooling when we move from our cross-sectional estimates to our within-twins estimates could be changes in sample composition. The OLS intergenerational associations are identified using variation in schooling both within and between twin pairs, whereas the within-twins intergenerational effects are only identified for twin pairs who differ in schooling. Perhaps discordant twin mothers differ substantially from discordant twin fathers so that we are

effectively comparing apples to peers.²⁴ An indication of such a difference would be if the intergenerational association obtained for the full population of twin mothers differs from the intergenerational association obtained for the sample of discordant twin mothers, while the corresponding association for twin fathers is smaller. To investigate this, Table 7 shows intergenerational associations for concordant and discordant MZ twin parents. The intergenerational association for concordant twin mothers controlling for spousal education is 0.17, similar to the intergenerational association of 0.15 for discordant twin mothers. These associations are very similar to those for concordant and discordant twin fathers (0.18 and 0.14 respectively). This suggests that our results regarding the role of mother's and father's schooling are not driven by changes in sample composition when moving to our sample of MZ twins.

There are a number of possible indirect mechanisms that would generate a pattern where mother's schooling appears to be as important as or more important than father's schooling. Such a pattern could arise if highly educated women have fewer children of higher quality and if this reasoning does not apply to highly educated fathers. We check this by running within-twins regressions on the relationship between schooling and the number of children for both MZ twin mothers and twin fathers in Appendix Table A2. The first two columns indicate that there is no significant relationship between mother's schooling and the number of children for the sample of twin mothers with daughters. Since we only obtained a significant effect of mother's schooling on daughter's schooling, we can rule out possible gender differences in the quality–quantity trade-off as an explanation for our finding.²⁵

The greater effect of mother's schooling may also be driven by gender differences in assortative mating, where the role of the spouse's characteristics may differ between twin mothers and twin fathers. Our intergenerational effects only partially control for assortative mating because unobserved endowments of the spouse are not differenced out in the within-twins approach. The positive effect of mother's schooling may therefore instead reflect positive traits of the spouse that are correlated with the twin mothers' schooling. In addition, well-educated

²⁴For example, if the sample of discordant female twins is more highly educated than the corresponding sample of male twins, one would then effectively compare estimates obtained from a sample of highly educated women with those obtained from a sample of less highly educated men, which makes little sense.

²⁵There is a very small, -0.027 , and significant negative relation between mother's schooling and the number of children for the sample of twin mothers with sons.

mothers are perhaps able to attract high ability spouses to a greater extent than well-educated fathers. This would then imply that the schooling of twin mothers is also more strongly correlated with other unobserved traits of the spouse, which we are not able to control for. Some suggestive evidence of such a pattern would be if there is a stronger correlation between the schooling of twin mothers and the schooling of their spouses than between the schooling of twin fathers and their spouses. Column three in Appendix Table A2 shows that mothers with more schooling have higher educated spouses. This is however also the case for twin fathers, as shown in column four. The estimates for twin mothers and twin fathers are very similar, which does not provide any evidence that twin mothers' schooling is more strongly correlated with positive traits of the spouse.²⁶

The difference in effects between mother's and father's schooling could also be explained by differences in social interactions across genders. As discussed by Ermisch and Pronzato (2010), social interactions between the children of twin parents will tend to diminish the differences in schooling attainment between them, resulting in smaller estimates of the effect of parents' schooling. Moreover, twin parents may interact more than other parents, also creating increased similarity among them. Since it is usually assumed that females interact more than males, it is unlikely that differences in social interactions between genders can explain our results.

A more direct explanation for our findings has to do with the time use patterns of mothers and fathers. While we lack the information needed to provide such evidence, we note that results from some previous studies suggest a *positive* relation between education and time spent on child care. Ichino and Sanz de Galdeano (2005) and Dribe and Stanfors (2011) provide evidence showing that high-educated mothers in Sweden spend more time with their children than low-educated mothers. Guryan et al. (2008) also show this to be the case in the US and in many other

²⁶ Another such potentially important trait would be the unobserved child-rearing endowments of the spouse. Child-rearing skills are usually assumed to be more important among mothers, which would lead to an upward bias in the estimated effect of twin father's schooling, since differences in the spouses' child-rearing skills are not differenced out. Assuming that child-rearing skills matter less for fathers, this upward bias would not occur to the same extent among twin mothers. This explanation is therefore also unlikely to drive our result that twin mothers' schooling matters at least as much or more than father's schooling.

Western countries.²⁷ This means that greater maternal education not only increases the efficiency of time use in child-rearing as theory suggests, but also time spent on child-rearing. As mothers spend more time on child-rearing than fathers, this may explain why the effect of mother's schooling on children's schooling is sometimes greater than the effect of father's schooling. Moreover, since we find that mother's schooling is more important for daughters, this could suggest that mothers spend more time with daughters than sons. Alternately, this could also suggest that highly educated mothers serve as a role model for their daughters, who aim to be highly educated like their mothers.

Finally, a recent paper by Lundborg et al. (2012) may provide some hints to the mechanisms behind our results. They provide twin-based estimates of the effect of maternal and paternal schooling on the son's achievement test scores and health, also using register-based data on Swedish twins. Their findings suggest that mother's schooling improves both the son's cognitive test scores and health, measured at military enlistment at age 18. Father's schooling affects cognitive test scores in a similar way but has no effect on son's health. To the extent that health during adolescence is important for later life schooling, this could be another possible explanation as to why mother's schooling matter at least as much as father's schooling.

In sum, we are able to rule out some possible explanations for our findings, such as gender differences in measurement error, sample composition, fertility behavior, and social interactions. Instead we speculate that a possible explanation for our finding has to do with time use patterns of highly educated mothers and fathers.

B. Why are our results so different from Behrman and Rosenzweig (2002) for the US?

One possible reason why mother's schooling matters in Sweden but not in the US relates to the negative consequences of short maternity leave and maternal employment during the first year on children's development. As noted by Behrman and Rosenzweig (2002), their results are consistent with the hypothesis that women's time in the home is a critical determinant of the human capital of children and that highly educated mothers spend less time at home, for which

²⁷Among the findings are that mothers with a college education or greater spend roughly 4.5 hours more per week in childcare than mothers with a high school degree or less. At the same time, higher-educated women with children are also much more likely to be working.

they find some support for in their data. In line with this, Baum (2003) reports that over a third of new mothers who worked during pregnancy return to work within three months of childbirth in the US. His results show that maternal work in the first year has negative effects on child cognitive development.

In our data, we also find some indirect evidence that highly educated mothers spend less time at home. Replacing children's schooling with a binary indicator of labor market participation, and running within-twins regressions for MZ twins, we find that one additional year of mother's schooling increases the labor market participation rate by 1.8 percentage points.²⁸ For fathers, the effect is close to zero and insignificant. However, the effect of mother's and father's schooling changes very little when we control for labor market participation in our within-twins regressions.²⁹ Moreover, the coefficients on the labor market participation variable are small and insignificant, suggesting that labor market participation of parents has no negative effect on the schooling attainment of children. Our results may reflect that in Sweden few mothers would work during the child's first birth year, since mothers were entitled to generous periods of maternity leave during our study period. Moreover, even when mothers do return to work, most children attend high-quality childcare centres, which means that any negative effect of less time spent with their children during may be offset by a positive effect of high-quality childcare. In line with this, Havnes and Mogstad (2011) find that the expansion of subsidized childcare in Norway had a strong positive effect on children's long-run educational attainment.³⁰ In the US, on the other hand, Herbst and Tekin (2010) show that publicly subsidized childcare has negative effects on children's cognitive ability in kindergarten and that the adverse effects are largely concentrated among children of highly educated mothers.

²⁸For this analysis, we used data from the 1975 census and defined labor market participation as having an income larger than zero. For the estimates to make sense, we also dropped mothers born after 1950.

²⁹ For mothers, the coefficient is 0.07 and is significant at the 1 percent level. The coefficient for fathers is 0.065, which is also significant at the 1 percent level. The results were obtained from within-twins regressions on the samples of MZ mothers and fathers and including both sons and daughters. Sample sizes were the same as in panel A for the MZ twin sample in Table 3.

³⁰We are not aware of any studies that estimate the causal effect of childcare on children's outcome in Sweden. The results for Norway are likely to be similar to Sweden, as the countries share many common institutional factors, such as subsidized childcare.

C. New twin results from the US

The results from Behrman and Rosenzweig (2002) do not necessarily extend to other twins datasets from the US. Table 8 provides intergenerational associations and effects for a sample of twin parents and their children from the Mid-Atlantic Twin Registry. A description of the dataset and summary statistics is reported in appendix 2. In the pooled sample of MZ and DZ twin parents there are strong intergenerational associations for father's and mother's schooling, which are similar in magnitude. An extra year of father's or mother's schooling increases children's schooling by 0.17-0.16 of a year controlling for assortative mating. These results are not very different from the results obtained from the Swedish sample. However, the intergenerational effects are close to zero (and negative for fathers) and statistically insignificant. A similar conclusion holds when examining the sample of MZ twin parents and their children in panel B. We acknowledge that the intergenerational effects are imprecisely estimated due to the small sample size. We have a slightly larger sample MZ twin mothers compared to Behrman and Rosenzweig (2002) (517 vs 424) but a smaller sample of MZ twin fathers (188 vs 244).

In summary, there is some suggestive evidence that the difference in results between the US and Sweden is partly due to differences in the impact of labor market participation on the transmission of schooling. Differences in institutional contexts may explain why an increase in labor market participation that follows from increased maternal schooling has a negative effect on the children's schooling in Behrman and Ro

senzweig's study, whereas we obtain no such result.

D. Why are our results for MZ twins different from our results obtained for the pooled sample of MZ and DZ twins?

In the regressions using the pooled sample of MZ and DZ twins, genetic endowments are not fully differenced out, since DZ twins only share 50 per cent of their genes. One simple reason for the greater effect of father's schooling obtained in this sample may be that there is a stronger genetic component to father's schooling than mother's schooling. This would be consistent with the pattern obtained when moving from the MZ and DZ twin parents sample to the MZ sample (from Table 2 to 3), where the effect of father's schooling falls towards zero in the MZ twin

parents sample but the effect of mother's schooling changes very little. We are able to check if there is a greater genetic component to father's schooling using classical twin gene-environment decomposition techniques. We find that the variance in schooling due to genes is very similar for fathers and mothers, 40 and 34 per cent respectively.³¹ Hence gender differences in the genetic contribution to schooling do not explain why our results change when moving from the pooled sample of MZ and DZ twins to the sample of MZ twins.

The fact that the genetic component to schooling is similar across mothers and fathers suggests that there is some other unobserved endowment that is correlated with father's schooling (and their children's schooling) that is not differenced out in the sample of DZ twins but that is then differenced out in our sample of MZ twins. One candidate would be earnings endowments, assuming that father's income is more important for children's schooling than mother's income. This would mean that the effect of father's schooling is upward biased in the sample of DZ fathers due to omitted earnings endowments. We can check this by adding income (taken from the tax registers) as an additional covariate to our regressions. Doing so reveals that unobserved earnings endowments are not a likely explanation for our findings for father's schooling, since the effect of schooling changes very little when adding income to the regressions on both the pooled sample of MZ and DZ twins and the sample of MZ twins (results available on request).

We are not able to provide any definitive answer to the question why the results for father's schooling change when moving from the combined sample of MZ and DZ twins. Besides income, which we could rule out, there may be many other unobserved traits that are correlated more strongly with the schooling of the fathers than the mothers and that affect children's schooling. There are no major differences between the pooled sample of MZ and DZ twins and MZ twins as the intergenerational associations are 0.23 in both samples. Moreover, using MZ twins is the "golden standard", since more unobserved endowments are controlled for compared to using only DZ twins and we do not need to know exactly what those unobserved traits are in order to cancel out their influence.

³¹The variance in schooling due to genes is calculated as $2(r_{mz} - r_{dz})$ where r_{mz} and r_{dz} are the schooling correlations between MZ and DZ twins respectively.

VI. Summary and Discussion

The twin-based literature on the intergenerational transmission of schooling has found that father's schooling is more important than mother's schooling for children's schooling. This is a controversial finding and conflicts with results from instrumental variable studies where mother's schooling matters. Using a large dataset of twin parents and their children in Sweden, we provide new results which are in contrast to the previous twin-based results and put mothers back at the forefront of the debate.

We find that mother's schooling matters *at least as much as* father's schooling for children's schooling. Moreover, we show that average effects of parental schooling hide important gender specific links. Our large sample size allows us to estimate the effect of parents' schooling on son's and daughter's schooling separately. We find that the effect of mother's schooling on children's schooling is entirely driven by the impact of mother's schooling on daughter's schooling, which may reflect important role model effects.

Moreover, we show that the importance of parental schooling has fallen over time. In recent cohorts, there is no significant effect of father's or mother's schooling on children's schooling and son's schooling. This may be due to increased educational opportunities which reduce the influence of parental schooling. However, the effect of mother's schooling on daughter's schooling has remained stable. This could be due to the increased labor market participation of Swedish women, which may have positively affected their daughter's investment in schooling.

Our results also illustrate the importance of having access to zygosity data in order to separate out DZ and MZ twins. Most of the previous Scandinavian twin-based studies do not have access to zygosity data and find that father's schooling is more important than mother's schooling based on a pooled sample of DZ and MZ twin parents. We also reach the same conclusion based on a combined sample of MZ and DZ twin parents. However, when we restrict the sample to MZ twins, this conclusion no longer holds.

In order to shed further light on the difference between our results and those of Behrman and Rosenzweig (2002), we bring in a new twin data set from the US and try to replicate the findings. In contrast to their study, we do not find any effect of mother's and father's schooling on children's schooling in the US.

Understanding the relative role of mother's and father's schooling for children's schooling is of great importance for policy. If maternal schooling is more important than father's schooling for children's schooling, there may be a stronger case for subsidizing maternal schooling than father's schooling. A similar conclusion would be drawn regarding policies that aim to increase educational mobility across generations. It is therefore important to understand why twin-based and instrumental variables-based studies often reach different conclusions. Our results show that this “puzzle” is perhaps less of a “puzzle” than previously thought and that mother’s schooling, in line with more instrumental variables results, appears more important than father’s schooling.

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Table 1. Summary statistics.

	MZ + DZ (1)	MZ (2)	Population (3)
Sons:			
Years of schooling	12.24 (2.18)	12.31 (2.13)	12.17 (2.23)
Year of birth:			
1945–1955	0.08	0.07	
1956–1965	0.27	0.25	
1966–1975	0.41	0.41	
1976–1983	0.24	0.27	
Daughters:			
Years of schooling	12.73 (2.15)	12.79 (2.14)	12.61 (2.22)
Year of birth:			
1945–1955	0.08	0.07	
1956–1965	0.28	0.27	
1966–1975	0.40	0.40	
1976–1983	0.24	0.26	
Twin fathers:			
Years of schooling	10.48 (3.06)	10.61 (3.05)	10.76 (3.01)
Year of birth:			
1925–1935	0.26	0.25	
1936–1945	0.42	0.42	
1946–1955	0.30	0.31	
1956–1965	0.02	0.02	
Wife years of schooling	10.85 (2.78)	10.85 (2.75)	
Twin mothers:			
Years of schooling	10.21 (2.77)	10.54 (2.85)	10.65 (2.75)
Year of birth:			
1925–1935	0.28	0.25	
1936–1945	0.37	0.38	
1946–1955	0.31	0.33	
1956–1965	0.04	0.04	
Husband years of schooling	10.45 (3.05)	10.69 (3.10)	

Notes: Column 3 “Population” refers to the average years of schooling for the corresponding non-twin population. Standard deviations in parentheses (.).

Table 2. Intergenerational associations/effects in years of schooling. MZ and DZ twins. Years of schooling as of 2007 for children. Parents born 1925–1965 and children born 1945–1983.

		Controlling for spouse's schooling	Intergenerational associations			Intergenerational effects		
			Father (1)	Mother (2)	Father + Mother (3)	Father (4)	Mother (5)	Father + Mother (6)
A. Sons + daughters	No		0.233*** (0.006)	0.231*** (0.006)	0.232*** (0.004)	0.151*** (0.011)	0.079*** (0.010)	0.113*** (0.008)
	Yes		0.165*** (0.006)	0.166*** (0.006)	0.167*** (0.004)	0.124*** (0.011)	0.060*** (0.011)	0.090*** (0.008)
	<i>N</i>		13,451	19,216	32,667	13,451 [3,256; 1,777]	19,216 [4,611; 2,288]	32,667 [7,867; 4,065]
B. Sons	No		0.263*** (0.009)	0.235*** (0.010)	—	0.160*** (0.018)	0.062*** (0.017)	—
	Yes		0.204*** (0.010)	0.158*** (0.010)	—	0.137*** (0.018)	0.039** (0.017)	—
	<i>N</i>		5,152	7,345		5,152 [1,791; 993]	7,345 [2,514; 1,262]	
C. Daughters	No		0.206*** (0.009)	0.227*** (0.009)	—	0.122*** (0.020)	0.089*** (0.017)	—
	Yes		0.136*** (0.010)	0.169*** (0.010)	—	0.101*** (0.020)	0.072*** (0.018)	—
	<i>N</i>		4,735	6,943		4,735 [1,657; 898]	6,943 [2,385; 1,183]	

Notes: Panel A does not distinguish between whether there is a difference between if the child is a son or a daughter. Panel B reports estimates for sons, while panel C reports estimates for daughters. All regressions control for children's year of birth (and gender of child in panel A). Specifications to control for assortative mating include spousal schooling as an additional covariate. The first and second number in square brackets [.] are the number of twin pairs and the number of twin pairs with a difference in parental schooling. Robust standard errors in parentheses (.). ***significant at 1%; **significant at 5%; *significant at 10%.

Table 3. Intergenerational associations/effects in years of schooling. MZ twins. Years of schooling as of 2007 for children. Parents born 1925–1965 and children born 1945–1983.

		Controlling for spouse's schooling	Intergenerational associations			Intergenerational effects		
			Father (1)	Mother (2)	Father + Mother (3)	Father (4)	Mother (5)	Father + Mother (6)
A. Sons + daughters	No		0.233*** (0.009)	0.234*** (0.009)	0.233*** (0.006)	0.065*** (0.022)	0.071*** (0.018)	0.069*** (0.015)
	Yes		0.161*** (0.010)	0.164*** (0.009)	0.164*** (0.007)	0.046** (0.022)	0.058*** (0.018)	0.052*** (0.015)
	<i>N</i>		5,403	7,860	13,263	5,403 [1,309; 620]	7,860 [1,916; 839]	13,263 [3,225; 1,459]
B. Sons	No		0.264*** (0.015)	0.249*** (0.015)	—	0.064** (0.033)	0.051* (0.030)	—
	Yes		0.192*** (0.016)	0.161*** (0.015)	—	0.043 (0.033)	0.028 (0.029)	—
	<i>N</i>		2,053	3,061		2,053 [715; 353]	3,061 [1,041; 456]	
C. Daughters	No		0.208*** (0.015)	0.216*** (0.014)	—	0.045 (0.037)	0.106*** (0.030)	—
	Yes		0.142*** (0.016)	0.157*** (0.014)	—	0.032 (0.037)	0.095*** (0.030)	—
	<i>N</i>		1,932	2,770		1,932 [668; 315]	2,770 [970; 430]	

Notes: Panel A does not distinguish between whether there is a difference between if the child is a son or a daughter. Panel B reports estimates for sons, while panel C reports estimates for daughters. All regressions control for children's year of birth (and gender of child in panel A). Specifications to control for assortative mating include spousal schooling as an additional covariate. The first and second number in square brackets [.] are the number of twin pairs and the number of twin pairs with a difference in parental schooling. Robust standard errors in parentheses (.). ***significant at 1%; **significant at 5%; *significant at 10%.

Table 4. Intergenerational effects in years of schooling. MZ and DZ twins, and closely spaced siblings. Years of schooling as of 2007 for children. Parents born 1925–1965 and children born 1945–1983.

	Controlling for spouse's schooling	MZ + DZ twins (1)	Closely spaced siblings (2)	Implied MZ (3)
A. Sons + daughters				
Fathers	No	0.151*** (0.011)	0.129*** (0.003)	0.183*** (0.027)
	Yes	0.124*** (0.011)	0.100*** (0.003)	0.156*** (0.027)
	<i>N</i>	13,451 [3,256; 1,777]	157,142 [38,310; 24,449]	
Mothers	No	0.079*** (0.010)	0.127*** (0.003)	0.010 (0.024)
	Yes	0.060*** (0.011)	0.101*** (0.003)	0.001 (0.027)
	<i>N</i>	19,216 [4,611; 2,288]	195,078 [46,693; 28,964]	
B. Sons				
Fathers	No	0.160*** (0.018)	0.140*** (0.005)	0.189*** (0.044)
	Yes	0.137*** (0.018)	0.112*** (0.005)	0.173*** (0.044)
	<i>N</i>	5,152 [1,791; 993]	59,415 [20,460; 12,969]	
Mothers	No	0.062*** (0.017)	0.123*** (0.005)	−0.025 (0.042)
	Yes	0.039** (0.017)	0.092*** (0.004)	−0.037 (0.042)

	<i>N</i>	7,345 [2,514; 1,262]	74,794 [25,462; 15,816]	
C. Daughters Fathers	No	0.122*** (0.020)	0.116*** (0.005)	0.130** (0.049)
	Yes	0.101*** (0.020)	0.088*** (0.005)	0.120** (0.049)
	<i>N</i>	4,735 [1,657; 898]	54,461 [19,139; 12,176]	
Mothers	No	0.089*** (0.017)	0.129*** (0.005)	0.031 (0.042)
	Yes	0.072*** (0.018)	0.107*** (0.005)	0.021 (0.044)
	<i>N</i>	6,943 [2,385; 1,183]	68,601 [23,832; 14,783]	

Notes: Panel A does not distinguish between whether there is a difference between if the child is a son or a daughter. Panel B reports estimates for sons, while panel C reports estimates for daughters. All regressions control for children's year of birth (and gender of child in panel A). Specifications to control for assortative mating include spousal schooling as an additional covariate. Intergenerational effects in column two are based on siblings born 1 to 2 years apart. The implied MZ intergenerational effect in column three is calculated as $[\beta_{MZ+DZ} - (1-\theta)\beta_{sib}]/\theta$, where θ is the share of MZ twins, 0.41. In columns one and two, the first and second number in square brackets [.] are the number of twin pairs and the number of twin pairs with a difference in parental schooling. In columns one and two robust standard errors are in parentheses (.). In column three, standard errors calculated using the delta method: $var(\beta_{mz}) = var(\beta_{MZ+DZ})(1/\theta)^2 + var(\beta_{sib})(1-\theta/\theta)^2$. ***significant at 1%; **significant at 5%; *significant at 10%.

Table 5. Intergenerational effects in years of schooling. MZ twin parents born 1925–1939 and 1940–1965.

Controlling for spouse's schooling		Monozygotic twins born 1925–1939			Monozygotic twins born 1940–1965		
		Father (1)	Mother (2)	Father + Mother (3)	Father (4)	Mother (5)	Father + Mother (6)
A. Sons + daughters	No	0.130*** (0.034)	0.110*** (0.031)	0.119*** (0.023)	0.011 (0.030)	0.041 (0.026)	0.027 (0.020)
	Yes	0.099*** (0.034)	0.099*** (0.030)	0.098*** (0.022)	0.000 (0.029)	0.025 (0.025)	0.013 (0.019)
	<i>N</i>	2,047 [459; 213]	2,911 [651; 279]	4,958 [1,110; 492]	3,356 [850; 407]	4,949 [1,265; 560]	8,305 [2,115; 967]
B. Sons	No	0.145*** (0.051)	0.158*** (0.049)	—	−0.013 (0.043)	−0.031 (0.038)	—
	Yes	0.106** (0.053)	0.140*** (0.048)	—	−0.019 (0.042)	−0.060 (0.036)	—
	<i>N</i>	782 [263; 125]	1,127 [366; 156]		1,271 [452; 228]	1,934 [675; 300]	
C. Daughters	No	0.077 (0.062)	0.097** (0.047)	—	0.028 (0.046)	0.110*** (0.040)	—
	Yes	0.057 (0.061)	0.084* (0.046)	—	0.022 (0.046)	0.100*** (0.039)	—
	<i>N</i>	748 [243; 108]	1,119 [367; 153]		1,184 [425; 207]	1,651 [603; 277]	

Notes: Panel A does not distinguish between whether there is a difference between if the child is a son or a daughter. Panel B reports estimates for sons, while panel C reports estimates for daughters. All regressions control for children's year of birth (and gender of child in panel A). Specifications to control for assortative mating include spousal schooling as an additional covariate. The first and second number in square brackets [.] are the number of twin pairs and the number of twin pairs with a difference in parental schooling. Robust standard errors in parentheses (.). ***significant at 1%; **significant at 5%; *significant at 10%.

Table 6. Intergenerational associations/effects in years of schooling. Father's schooling with and without control for cognitive test scores. Years of schooling as of 2007 for children. Parents born 1925–1965 and children born 1945–1983.

	Intergenerational associations (MZ+DZ)		Intergenerational effects (MZ+DZ)		Intergenerational associations (MZ)		Intergenerational effects (MZ)	
Father's schooling	0.248*** (0.021)	0.165*** (0.025)	0.077* (0.041)	0.077* (0.042)	0.277*** (0.034)	0.215*** (0.045)	0.089 (0.070)	0.107 (0.071)
Cognitive test score	-	0.377*** (0.058)	-	-0.008 (0.114)	-	0.254** (0.107)	-	-0.289 (0.227)
Control for cognitive test score	No	Yes	No	Yes	No	Yes	No	Yes
<i>N</i>	1382	1382	1382	1382	511	511	511	511

Notes: Robust standard errors in parentheses (.). ***significant at 1%; **significant at 5%; *significant at 10%. All regressions control for children's year of birth and gender of child. All regressions are without control for assortative mating.

Table 7. Intergenerational associations for concordant and discordant MZ twins, born 1925–1965.

	Concordant twin mothers		Discordant twin mothers	
	(1)	(2)	(3)	(4)
Mother's schooling	0.246*** (0.012)	0.168*** (0.013)	0.214*** (0.013)	0.152*** (0.014)
Father's schooling	—	0.161*** (0.012)	—	0.172*** (0.013)
<i>N</i>	4,421	4,421	3,439	3,439

	Concordant twin fathers		Discordant twin fathers	
	(1)	(2)	(3)	(4)
Mother's schooling	—	0.186*** (0.016)	—	0.201*** (0.016)
Father's schooling	0.251*** (0.012)	0.176*** (0.014)	0.211*** (0.014)	0.144*** (0.015)
<i>N</i>	2,849	2,849	2,554	2,554

Notes: Robust standard errors in parentheses (.). ***significant at 1%.

Table 8. Intergenerational associations/effects in years of schooling. MZ and DZ twins from the Mid-Atlantic Twin Registry. Parents born 1900–1935 and children born 1925–1969.

		Intergenerational associations		Intergenerational effects	
Controlling for spouse's schooling		Father (1)	Mother (2)	Father (3)	Mother (4)
A. MZ + DZ Twins	No	0.189*** (0.049)	0.206*** (0.024)	-0.079 (0.105)	0.014 (0.073)
	Yes	0.174*** (0.051)	0.158*** (0.025)	-0.086 (0.105)	-0.022 (0.069)
	<i>N</i>	286	692	286 [86; 31]	692 [212; 78]
B. MZ Twins	No	0.295*** (0.064)	0.216*** (0.026)	0.033 (0.163)	0.013 (0.090)
	Yes	0.273*** (0.067)	0.159*** (0.027)	0.018 (0.156)	-0.024 (0.089)
	<i>N</i>	188	517	188 [57; 17]	517 [154; 51]

Notes: All regressions control for children's year of birth and gender of child in panel A. Specifications to control for assortative mating include spousal schooling as an additional covariate. The first and second number in square brackets [.] are the number of twin pairs and the number of twin pairs with a difference in parental schooling. Robust standard errors in parentheses (.). ***significant at 1%; **significant at 5%; *significant at 10%.

Appendix 1

Table A1. Intergenerational effects in years of schooling. MZ and DZ twins parents born 1925–1939 and 1940–1965.

		MZ and DZ twins born 1925–1939			MZ and DZ twins born 1940–1965		
		Controlling for spouse's schooling					
		Father (1)	Mother (2)	Father + Mother (3)	Father (4)	Mother (5)	Father + Mother (6)
A. Sons + daughters	No	0.172*** (0.017)	0.070*** (0.017)	0.121*** (0.012)	0.133*** (0.015)	0.085*** (0.013)	0.106*** (0.010)
	Yes	0.140*** (0.017)	0.054*** (0.017)	0.098*** (0.014)	0.109*** (0.015)	0.062*** (0.013)	0.083*** (0.010)
	<i>N</i>	5,292 [1,186; 628]	7,481 [1,664; 786]	12,773 [2,850; 1,414]	8,159 [2,070; 1,149]	11,735 [2,947; 1,502]	19,894 [5,017; 2,651]
B. Sons	No	0.192*** (0.027)	0.071*** (0.027)	—	0.130*** (0.024)	0.058*** (0.022)	—
	Yes	0.164*** (0.028)	0.047* (0.027)	—	0.112*** (0.024)	0.033 (0.022)	—
	<i>N</i>	2,050 [681; 368]	2,889 [947; 451]		3,102 [1,110; 625]	4,456 [1,567; 811]	
C. Daughters	No	0.149*** (0.031)	0.065** (0.029)	—	0.105*** (0.026)	0.105*** (0.022)	—
	Yes	0.124*** (0.031)	0.052* (0.029)	—	0.086*** (0.026)	0.085*** (0.022)	—
	<i>N</i>	1,896 [636; 331]	2,824 [919; 422]		2,839 [1,021; 567]	4,119 [1,466; 761]	

Notes: Panel A does not distinguish between whether there is a difference between if the child is a son or a daughter. Panel B reports estimates for sons, while panel C reports estimates for daughters. All regressions control for children's year of birth (and gender of child in panel A). Specifications to control for assortative mating include spousal schooling as an additional covariate. The first and second number in square brackets [.] are the number of twin pairs and the number of twin pairs with a difference in parental schooling. Robust standard errors in parentheses (.). ***significant at 1%; **significant at 5%; *significant at 10%.

Table A2. Effect of father's and mother's years of schooling on number of children and spouse's years of schooling.

Samples	Number of children (MZ twins)		MZ twin spouse years of schooling	
	Mother (1)	Father (2)	Mother MZ twin (3)	Father MZ twin (4)
A. Sons + daughters	-0.011 (0.007)	-0.017** (0.008)	0.116*** (0.017)	0.149*** (0.019)
	[1,916; 839]	[1,309; 620]	[1,916; 839]	[1,309; 620]
B. Sons	-0.027* (0.013)	-0.011 (0.014)	0.143*** (0.028)	0.169*** (0.032)
	[1,041; 456]	[715; 353]	[1,041; 456]	[715; 353]
C. Daughters	0.001 (0.013)	-0.012 (0.016)	0.128*** (0.030)	0.101*** (0.035)
	[970; 430]	[668; 315]	[970; 430]	[668; 315]

Notes: All estimates are from within-twins regressions. Panel A reports estimates for all twin parents for the sons + daughters sample. Panel B reports estimates for twin parents with sons, while panel C reports estimates for twin parents with daughters. The first and second number in square brackets [.] are the number of twin pairs and the number of twin pairs with a difference in parental schooling. Robust standard errors in parentheses (.). ***significant at 1%; **significant at 5%; *significant at 10%.

Appendix 2: Description of Data from the Mid-Atlantic Twin Registry

The results from table 8 are based on the Virginia 30K sample from the Mid Atlantic Twin Registry. The sample was ascertained from public birth records in the Commonwealth of Virginia for twins born in Virginia between 1915 and 1972 and responses to a letter published in the newsletter of the American Association of Retired Persons. Twins were mailed a health and lifestyle questionnaire in 1987. Completed questionnaires were obtained from 14,763 twins, approximately 70 percent. The original twin questionnaire was modified slightly to provide two additional forms, one appropriate for parents of twins and another for spouses, children and siblings of twins. Response rates from relative (44.7%) was much lower than that from twins.

We specifically have data on twin parents enlisted from the American Association of Retired Persons and their children. The questionnaire asked respondents “what is your highest schooling attainment?” with options for (1) 0-7 years of elementary school (2) 8 years of elementary school (3) 1-3 years of high school 4 years of high school (4) 1-3 years of college (5) 4 or more years of college. We create a variable for years of schooling by recoding these categories as 5, 8, 10, 12, 14 and 16 years of schooling respectively.

The final sample consists of twin pairs with children aged 18 or over. Appendix table A3 shows descriptive statistics for twin fathers and twin mothers Twin parents are on average 64 years old, have 14 years of education, and approximately a third of the twin parents differ on educational attainment. There are no any major differences in the combined MZ and DZ sample to the MZ only sample. Appendix Table A4 gives summary statistics for children. Children are 18-70 years old at time of survey with average age of 33, and 14 years of education and 38% are male.

Appendix Table A3: Descriptive Statistics For Twin Parents, Mid-Atlantic Twin Registry

	MZ + DZ (1)	MZ (2)
Twin Fathers		
Years of education	14.36 (2.03)	14.56 (1.69)
Absolute within-twin Pair difference in years of schooling	0.94 (1.40)	0.70 (1.15)
Proportion of twin pairs that Differ on schooling attainment	0.36 (0.48)	0.29 (0.45)
Age	64.44 (6.94)	64.45 (2.92)
Distribution of birth cohorts		
1895-1899	---	---
1900-1904	0.01 (0.10)	0.02 (0.13)
1905-1909	0.05 (0.21)	0.03 (0.18)
1910-1914	0.05 (0.21)	0.05 (0.22)
1915-1919	0.24 (0.42)	0.23 (0.42)
1920-1924	0.24 (0.42)	0.26 (0.44)
1925-1929	0.26 (0.44)	0.23 (0.43)
1930-1934	0.12 (0.33)	0.15 (0.36)
1935-1939	0.02 (0.15)	0.02 (0.13)
1940-1944	---	---
Wife's years of education	13.51 (1.86)	13.66 (1.83)
N	172	114
Twin Mothers		
Years of education	13.46 (1.93)	13.48 (1.97)
Absolute within-twin Pair difference in years of schooling	0.88 (1.29)	0.81 (1.29)
Proportion of twin pairs that Differ on schooling attainment	0.37 (0.48)	0.33 (0.47)
Age	61.38 (6.51)	61.12 (6.58)
1905-1909	0.01 (0.09)	0.01 (0.08)
1910-1914	0.04 (0.18)	0.04 (0.19)
1915-1919	0.15 (0.35)	0.15 (0.36)
1920-1924	0.23 (0.42)	0.22 (0.41)
1925-1929	0.26 (0.44)	0.22 (0.41)
1930-1934	0.21 (0.41)	0.25 (0.43)
1935-1939	0.10 (0.29)	0.10 (0.30)
1940-1944	---	---
Husband's years of education	13.91 (2.13)	14.01 (2.06)
N	424	308

Appendix Table A4: Descriptive Statistics for Children, Mid-Atlantic Twin Registry

	MZ + DZ (1)	MZ (2)
Male	0.38 (0.48)	0.39 (0.49)
Age	33.66 (6.89)	33.62 (6.96)
Distribution of Birth cohorts		
1925-1929	0.004 (0.06)	0.005 (0.07)
1930-1934	0.009 (0.09)	0.010 (0.10)
1935-1939	0.11 (0.11)	0.11(0.11)
1940-1944	0.07 (0.26)	0.07 (0.25)
1945-1949	0.19 (0.39)	0.18 (0.39)
1950-1954	0.28 (0.45)	0.27 (0.44)
1955-1959	0.26 (0.44)	0.27 (0.44)
1960-1964	0.13 (0.34)	0.14 (0.35)
1965-1969	0.04 (0.21)	0.04 (0.19)
Years of education	14.84 (1.49)	14.81 (1.48)
N	978	705