Do More-Schooled Women have Fewer Children and Delay Childbearing? Evidence from a Sample of U.S. Twins

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Abstract:

Using data on MZ (monozygotic, identical) female twins from the Minnesota Twin Registry, we estimate the causal effect of schooling on completed fertility, probability of being childless and age at first birth, using the within-MZ twins methodology. We find strong cross-sectional associations between schooling and the fertility outcomes and some evidence that more schooling causes women to have fewer children and delay childbearing, though not to the extent that interpreting cross-sectional associations as causal would imply. Our conclusions are robust when taking account of (1) endogenous within-twin pair schooling differences due to reverse causality and (2) measurement error in schooling. We also investigate possible mechanisms and find that the effect of women's schooling on completed fertility is not mediated through husband's schooling but rather through age at first marriage.

JEL Codes: I2, J13

Key words: twins, twins fixed-effects, schooling, fertility

1. Introduction

The impact of women's schooling on human fertility is of major interest because of the huge private and social resource use implications of fertility. Standard theoretical models of fertility have ambiguous empirical predictions because substitution and income effects are in opposing directions. More schooling for women is likely to increase the price of the number of children and induce substitution of consumption of other goods and services including child quality through increasing the opportunity cost of women's time in the labour force. More schooling may also increase women's effectiveness in using contraceptives and thus have a price effect through lowering the costs of avoiding unwanted fertility. On the other hand, more schooling increases full income and children are thought to be normal goods for which income elasticities are positive. Whether the substitution or the income effect dominates, thus, is an empirical question.

There are strong *cross-sectional associations* indicating that women with more schooling tend to have fewer children, are more likely to be childless, and delay childbearing. However, these associations do not necessarily reflect causal relationships because of unobserved factors that are correlated with both schooling and fertility decisions. For example, women with high innate ability, strong preferences for work or low discount rates are more likely to finish high school and attend college. At the same time, for any given level of schooling they are more likely to pursue a professional career and delay childbearing. A negative association between schooling and fertility will therefore be observed even in the absence of any causal relationship.

Recent studies have attempted to address the omitted variable bias problem using instrumental variable (IV) estimates based on natural experiments regarding schooling. Breierova and Duflo (2002), and Osilii and Long (2008) use time and regional variation in large school construction programs in Indonesia and Nigeria respectively, to construct instruments for schooling attainment. Both studies find that more schooling for women reduces early fertility, with cross-sectional estimates understating the magnitude of the impact of schooling. Breirova and Duflo (2002) also find that more schooling does not reduce completed fertility. Duflo et al. (2010) provide evidence for Kenya based on a randomized experiment in which some students randomly received free uniforms for the last three years of primary school from 2003 to 2005. They find a persistent effect of this subsidy for schooling on early fertility- that by the end of 2007 girls who received uniforms were 8 percent less likely to have started childbearing.

For developed countries, the most common instrument used is changes in compulsory schooling laws. Black et al. (2008) examine the reduced-form relationship between compulsory schooling laws and the probability of teenage childbearing. They find that compelling women to stay in school until age 16 reduced the probability of a teen birth by 4.7 percent in the U.S. and 3.5 percent in Norway. Monstad et al. (2008) extend the work of Black et al. (2008) for Norway, examining the effect of schooling on the number of children and the probability of being childless in addition to age at first birth, using changes in compulsory schooling laws as their instrument. Their IV estimates indicate no significant effect of schooling on the number of children or the probability of being childless, but they do find that schooling reduces the probability of a teen birth and delays first births into the 20s and late 30s. Fort et al. (2011) estimate the effect of schooling on completed fertility and probability of being childless using changes in compulsory schooling laws in eight European countries. They surprisingly find that more schooling leads women to have more children and reduces the incidence of childlessness. They argue that this is because IV estimates reflect local average treatment effects (LATE), as they target a specific group of the population. Compulsory schooling laws affect those at the lower end of the schooling distribution, for whom the income effect may outweigh the substitution effect, leading to higher fertility. These individuals may also have preferences for large families, which would be negatively correlated with schooling and lead to cross-sectional associations being biased downwards. Using compulsory schooling and child labour laws as instruments, León (2004) finds that an additional year of schooling reduces the number of children by 0.33 and increases the probability of being childless by 4 to 13 percent in the U.S. Lavy and Zablotsky (2011) use the de facto revocation of the Military Government of Arabs in Israel as a natural experiment that immediately enabled a large part of the Arab population to regain access to schooling. Their IV estimates indicate that an extra year of schooling causes a decline in completed fertility of 0.5 children. The IV estimates in León (2004), and Lavy and Zablotsky (2011) indicate stronger schooling effects than the cross-sectional estimates. The majority of the evidence thus suggests that more schooling does cause women to delay childbearing in both developed and developing countries, but the evidence remains mixed as to whether schooling causes women to have fewer children and increases the probability of being childless.[1]

We contribute to the literature by using an alternative method to estimate the causal effect of schooling on completed fertility, the probability of being childless and timing of first births. The approach we take is to use data on MZ (monozygotic, identical) female twins from the Minnesota Twin Registry and relate twin pair differences in the fertility measures to twin pair differences in schooling, thereby eliminating the effect of unobserved endowments common to both twin sisters. To our knowledge this is the first paper that uses the within-MZ twins approach to causally identify the effect of schooling on completed fertility, probability of being childless and timing of first births.

There are two inherent limitations with the within-MZ twins approach that may prevent identification of causal effects. Perhaps the most-emphasized limitation is that schooling differences between MZ twins are likely to be caused by factors that also affect directly the outcome of interest (Bound and Solon 1999). If a there is some unobserved factor that affects schooling and the outcome of interest in the same (opposite) direction, the within-MZ twins estimates are an upper (lower) bound of the true schooling effects (Behrman et al. 2011). A natural related concern in the context of fertility is that of reverse causation. For example an unintended pregnancy during high school/college or an unexpectedly early marriage that disrupts schooling and directly affects fertility, could cause the within-MZ twins estimates to be biased, perhaps more biased than the cross-sectional associations. Our data contains information on the timing of schooling completion, fertility and marriage that allows us to address this concern. Second, it is well known that first differencing exacerbates the attenuation bias due to measurement error in schooling. To deal with this, Ashenfelter and Krueger (1994) and Behrman et al. (1994) suggested using schooling reports on the twin by another individual (e.g., the co-

[1] One exception is McCrary and Royer (2011). They exploit the fact that school entry dates in California and Texas are a function of date of birth: children aged 5 on December 1st (California) or September 1st (Texas) can start their first year of kindergarten, while others have to delay their entrance by one year. They compare outcomes for women born just before and after the school entry dates, and find no significant effect of schooling on age at first birth.

twin, some other relative) as an instrument for self-reported schooling. Our data also includes co-twin reports on schooling, so we can address this concern as well.

Within-MZ twins estimates have two major advantages relative to the IV estimates that are summarised above. First, IV estimates based on compulsory schooling laws yield local average treatment effects. Twins estimates, in contrast, depend on within-twin pair schooling differences that may, and in fact in most data sets are, distributed across a range of schooling levels (Behrman et al. 2011). Second, changes in compulsory schooling laws or in school supply not only increase schooling of individual women affected and thereby the schooling of their mates, but also the whole distribution of schooling among individuals in women's marriage market (i.e., potential partners). Hence the IV estimates indicate schooling effects that are over and beyond any normal gross schooling effects, inclusive of both own schooling and assortative mating effects. For example, Black et al. (2008) note "compulsory schooling laws affected potential fathers and this may be an independent force leading to a reduction in teenage births" (pp.1045). In comparison, although matching in the marriage market, partner characteristics and related aspects of household bargaining may also systematically differ between MZ twins sisters, within-MZ twins estimates will reflect partial equilibrium gross effects inclusive (but not beyond) the impact of assortative mating on schooling.

The paper is organised as follows. We first describe the methodology, data and then turn to the estimates. The estimates illustrate strong cross-sectional associations between schooling and the fertility measures and some evidence that schooling *causes* women to have fewer children and delay childbearing, though not to the extent that interpreting cross-sectional associations as causal would imply.

2. Methodology

Consider a reduced-form model where the fertility measure of twin i in pair j (Y_{ij}) is related to her schooling (S_{ij}), unobserved endowments that are common to both MZ twins in a given pair (μ_j) and an unobserved stochastic term (ε_{ij}).

$$Y_{ij} = \beta S_{ij} + \mu_j + \varepsilon_{ij} \tag{1}$$

But S_{ij} is also likely to depend on unobserved endowments μ_j and an unobserved stochastic term v_{ij}.

$$S_{ij} = \delta\mu_j + v_{ij} \tag{2}$$

An OLS regression of relation (1) provides an estimate of the association between schooling and fertility, which is a biased estimate of the causal impact because schooling is partially related to unobserved endowments that also directly influence fertility.

Within-MZ twins or MZ fixed-effects estimators are used to estimate the causal effect of schooling by eliminating the influence of individual unobseverved endowments resulting from genetic dispositions (MZ twins share the same genetic information), shared parental households (the vast majority of MZ twins grow up together) and other socioeconomic contexts (e.g, schools, neighbourhoods). The within-MZ twins estimator is based on estimating the differenced relation (3), which eliminates the influence of unobserved common endowments shared by the MZ twins μ_j.

$$Y_{1j} - Y_{2j} = \beta(S_{1j} - S_{2j}) + (\varepsilon_{1j} - \varepsilon_{2j}) \qquad (3)$$

Comparisons can be made between standard cross-sectional estimates in (1) and within-MZ twins estimates in (3), to reveal the extent to which the presence of unobserved endowment heterogenity distorts the results of standard cross-sectional estimates.

As we note in the introduction, estimates of relation (3) may be contaminated by reverse causality and by measurement error in schooling. We investigate these two possibilities after presenting our main results in Section 4.

3. Data

Our data comes from the Minnesota Twin Registry (MTR). The MTR is the largest birth-record twins registry in the United States, with birth records on all twins (both monozygotic and dizygotic) born in Minnesota between 1936 and 1955. The specific data used are from the Socioeconomic Survey of Twins, which was a questionnaire sent out by Jere Behrman, Mark Rosenzweig and the late Paul Taubman to the MTR twins in 1994 collecting information on schooling, martial history, labour markets and fertility.

Our analyses is based on a sample of 808 MZ female twins aged 40 or older, who do not have any adopted or step children, and for whom there is complete information on self-reported schooling and the fertility measures. The fertility outcomes considered are (1) number of children- as all twins are aged 40 or older this essentially measures completed fertility; (2) probability of being childless; (3) probability of age at first birth occurring between before 20, 20-24, 25-29, 30-34, 35 or later. Additionally, we use a sub-sample of 628 MZ female twin mothers to examine schooling effects on age at first birth.

Our key explanatory variable is schooling attainment. The questionnaire contains several questions pertaining to schooling attainment. Questions were asked regarding (1) highest grade 1 through 12 of schooling completed; (2) vocational schooling (whether the respondent has a vocational qualification, year first attended vocational school, year last attended vocational school); (3) college schooling (degree obtained, year first attended college, year last attended college); (4) graduate/professional schooling (degree obtained, year first attended graduate/professional school, year last attended graduate/professional school) and (5) schooling at time of first marriage and current marriage. We first determine the highest qualification obtained and then assign (1) actual grades of schooling if no high school diploma, (2) 11 if GED, (3) 12 if high school diploma, (4) 13 if vocational diploma, (5) 14 if associate degree, (6) 16 if college degree, (7) 18 if masters degree, (8) 19 if JD or MBA and (9) 20 if doctoral degree. As there has been some debate about the appropriate coding of schooling in this data set (Antonovics and Goldberger 2005, Behrman and Rosenzweig 2005), we also test to see if our results are robust to the alternative coding scheme used by Kohler et al. (2011), and find that they are robust.

Table 1 presents descriptive statistics for the sample of twins from the MTR and for women born in the U.S. between 1936 and 1954 from the June 1995 Fertility and Martial History supplement of the Current Population Survey (CPS) to assess the representativeness of our twins samples. Comparing the sample of all twins and all women in the CPS in columns 1 and 3, the twins are quite similar to women in the CPS in terms of some fertility measures. The average number of children for the twins is 2.27 and 2.26 for women in the CPS. A similar proportion of the twins and women in the CPS are childless (14 percent and 15 percent respectively). The sample of twins mothers in column 2 is also fairly similar in terms of fertility outcomes compared to mothers in the CPS in column 4. Twins mothers have on average 2.71 children, slightly more than the mothers in the CPS, who have 2.64 children on average. Mothers in the CPS had their first birth on average at age 23, whereas the twins mothers on average first gave birth when they were one year older at age 24.

There are two differences between the twins and the representative population. First, only 9 percent of the twins first gave birth as a teenager compared to 24 percent of women in the CPS. Second, our female twins have more reported schooling than women in the CPS. The proportion of women who report high school graduation as their highest schooling attainment is identical in the twins and CPS samples (38 percent). However, only 2 percent of twins have not completed high school compared to 11 percent of women in the CPS. The proportion of women who have some post-high school schooling (but no bachelor's degree) is higher in the twins sample compared to the CPS sample (33 percent compared to 27 percent).

That our female twins are not fully representative of women in the general population does not necessarily threaten the external validity of within-MZ twins estimates for two reasons. First, if sample selection is related to unobserved endowments, then these are controlled for in the within-MZ twins approach. Second, if the cross-sectional association between schooling and the fertility measures is the same for twins and non-twins then the within-MZ twins approach can still be informative about the direction and magnitude of the bias in cross-sectional estimates.

We noted in the introduction that IV estimates are LATE estimates focused on a narrow range of schooling while within-MZ twins estimates are not likely to be limited to such a narrow range of schooling. Table 2 tabulates and summarises the differences in grades of schooling for all twins pairs in which at least one member attained one of the three following broad educational categories:[2] (1) completed high school; (2) some college education and (3) college degree or higher. For the full sample, the twins pairs on average have an absolute difference of 0.88 grades in schooling with a standard deviation of 1.29. Over half of twins pairs have no difference in schooling attainment. Across the three educational categories, the least variation occurs in twins pairs where at least one twin had completed high school in which case approximately half of the twins pairs have no differences in grades of schooling. However, in twin pairs where at one twin has some college education or obtained a college degree, only a third of twin pairs have no schooling differences. The mean absolute difference in grades of schooling for this category is 1.06 with standard deviations of 1.29. In twins pairs where at least one twin has a college degree, the mean absolute difference in grades of schooling is 1.88 with a standard deviation of 1.74.

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

Differences in twins schooling are thus spread over a range of schooling levels rather than being limited to those with relatively low schooling levels affected by compulsory schooling.

4. Results

The main estimates are presented in Table 3. For completed fertility and age at first birth we use OLS regressions to measure cross-sectional associations and standard within-MZ twins regressions. For binary fertility outcomes, cross-sectional and within-MZ twins estimates are based on linear probability models.[3]

The cross-sectional estimates in column 1 indicate that more schooling is significantly associated with having fewer children, more likely to be childless and delaying childbearing from early ages (before 20, 20-24) till later ages (25-29, 30-34). Column 2 provides within-MZ-twins estimates that attempt to control for unobserved endowments affecting both schooling and fertility. The within-MZ twins estimate for number of children is -0.14, the same as the cross-sectional estimate. This suggests that an extra grade of schooling reduces number of children by 0.14 or women with 4 additional grades of schooling have on average 0.56 less children. In contrast, the within-MZ twins estimate for being childless falls to less than half and becomes insignficant. This suggests that the unobserved endowments that affect schooling have different affects on completed fertility and childlessness, with almost no effects on the former but substantial effects on the latter.[4] The within-MZ twins estimates also show that more schooling leads women to postpone their first births from age 20-24 to 25-29.

For the sample of twins mothers, the cross-sectional estimates in column 3 also indicate that women with higher schooling attainment delay childbearing. Women with an extra grade of schooling delay childbearing by 0.79 of a year. The within-MZ twins estimates in column 4 are also generally smaller in absolute magnitude than the corresponding cross-sectional estimates and show that more schooling significantly causes women to delay childbearing, by postponing first births from the 20-24 year-old age range to the 25-29 year-old age range.

[3] Linear probability models can yield predictions outside the unit interval. Our conclusions are robust to using conditional fixed-effects logit models. Our conclusions for number of children are also robust to estimating the cross-sectional relationship with Poisson regressions and within-MZ twins estimates with Poisson fixed-effects.

[4] We have also repeated this exercise for DZ (dizygotic, fraternal) twins sisters, who are not genetically identical but share the same family environments. The within-DZ twins estimates indicate that an extra grade of schooling decreases (increases) number of children (probability of being childless) by 0.23 (4.7 percentage points) similar to the cross-sectional association of 0.20 (3.1 percentage points). We do not focus on DZ twins because within-DZ twins estimates do not control for the influence of individual-specific endowments that differ from the family mean endowment.

4.1 Reverse Causation

The primary criticism of the within-MZ twins approach has been the assumption that the factors determining the twin pair schooling difference do not have direct effects on the outcome of interest. In the present context, twin pair schooling differences may be due to reverse causality, if for example an unintended/early pregnancy or unexpectedly early marriage prevents one sister from completing her schooling. We now address this concern by exploiting the detailed nature of the questionnaire that contained questions pertaining to the timing of fertility, marriage and schooling completion. For fertility, respondents were asked for the year of birth for their first four children. For marriage, respondents were asked for the age when they married their first spouse and current spouse. Finally, in terms of schooling timing, the following series of questions were asked concerning vocational, undergraduate and graduate schooling:

Vocational Schooling
In what year did you first attend vocational/technical school after high school?
In what year did you last attend vocational/technical school?
How many months in total did you attend vocational/technical school?

Undergraduate Education
In what year did you first attend a college/university as an undergraduate?
In what year did you last attend a college/university as an undergraduate?
How many years in total did you attend a college/university as an undergraduate?

Graduate & Professional Education
In what year did you first attend a graduate/professional school?
In what year did you last attend a graduate/professional school?
How many years in total did you spend attending a graduate/professional school?

Thus, for those twins with some post-high school schooling it is possible to determine whether an unintended/early pregnancy or unexpectedly early marriage could have disrupted and prevented schooling completion by examining the responses to these questions.

Appendix Table A1 provides a list of twins for whom one could possibly argue that an unintended pregnancy or an early marriage or both of these prevented schooling completion.[5] For an example illustrating the possibility of fertility disrupting schooling, consider twin 1 in pair 3. She reports going to college between 1954 and 1956. She was married in 1955, has two children who were born in 1954 and 1956. Hence the birth of the second child in 1956 may have prevented completion of college education. Twin 1 in pair 188 is an example of a case where marriage may have prevented completion of college education. This twin reports going to college in 1965-1966 but was also married in 1966. Twin 1 in pair 10 is an example of a case where perhaps both marriage and fertility disrupted schooling. Here the twin reports going to college in 1954-1956. She was married in 1956 and her first child was born in 1957.

[5] For the twins listed in appendix Table A1 we cannot of course be certain that fertility/marriage prevented schooling completion. These are essentially our own judgment calls.

Table 4 investigates whether twins pairs where fertility/marriage may have disrupted the schooling of one twin have larger schooling and fertility differences compared to twins pairs where fertility/marriage did not disrupt schooling of either twin. More specifically, Table 4 reports estimates from the following regression:

$$\text{Absolute}Y_j = b_o + b_1 \text{dummy}_j + u_j$$

AbsoluteY_j is the absolute within-MZ twin pair difference in grades of schooling, number of children or age at first birth for twin pair j. Dummy$_j$ is a dummy variable equal to 1 if fertility/marriage may have prevented schooling completion for one twin in pair j but not the other. Panel A defines the dummy variable to equal 1 if fertility is the sole cause for schooling disruption. There are 12 twins pairs (3 percent) where fertility appears to have prevented one twin in the pair from completing vocational or college education. Column 2 shows that these twins pairs have a significantly larger difference of almost one grade of schooling compared to twins pairs where fertility did not affect schooling completion. They do not have a significantly larger difference in completed fertility, illustrated by the small and insignificant point estimate in column 3. For the sample of twins mothers in panel A, twins pairs where one twin was affected by reverse causality have a 0.8 larger difference in their grades of schooling, which is marginally insignificant. Their difference in age at first birth is almost two years larger than for twins pairs for which there is no reverse causality. Panel B focuses solely on marriage as the cause for schooling disruption. Although the estimates are all positive, they are imprecisely estimated with large standard errors. In panel C there are 14 twins pairs in both the all-twins and twins-mothers samples (3.5 and 4.5 percent respectively) for whom both fertility and marriage appear to have prevented some post-high school schooling.[6] The estimates in column 1 for panel C also indicate that these twins pairs have a significantly larger mean difference of almost one grade of schooling compared to the other twins pairs. There is no significant difference in completed fertility or age at first birth. Finally in panel D, the dummy variable is defined to equal 1 if either marriage, fertility or both prevented schooling completion. There are 31 such twins pairs in the full sample of all twins and 29 in the sub-sample of twins mothers. Again, these twins pairs have a significantly larger mean difference in their grades of schooling and in age at first birth, but not in completed fertility.

Table 5 provides descriptive statistics for a "no reverse causality" sample of twins. This sample only contains twins where fertility does not affect schooling because first births occur at least two years after schooling completion. We detail this in appendix 2. We also excluded twins in appendix Table A1, where marriage appears to have disrupted schooling. There are two major differences between this sample of twins and our original main sample. First, there are only two twins who were teenage mothers (and they are in the same pair). Second, there is much less variation in schooling differences between twins. Two-thirds of the twins pairs have no difference in their grades of schooling and the average within-MZ twins pair difference in grades of schooling is 0.57 grades, substantially lower than 0.88 grades for the original full sample. Table 6 provides cross-sectional and within-MZ twins estimates for the no reverse causality

[6] In Appendix Table A1 there are 16 twin pairs listed where both fertility and marriage appear to have prevented schooling. In twin pair 77, both twins' schooling appears to have been prevented by fertility and marriage. We exclude this pair from the regressions in panel C in Table 4, which is why the sample size for all twins and twins mothers is 403 and 313 pairs respectively.

sample. The general qualitative conclusions are similar to our main results. The cross-sectional estimates indicate that more schooling is associated with women having fewer children, more likely to be childless and delaying childbearing. As before, controlling for unobserved endowments through the within-MZ twins approach does not alter the magnitude of the effect of schooling on completed fertility, but the estimated effect for being childless falls by two-thirds and is insignificant. This suggests that the unobserved endowments that affect schooling are only weakly associated with completed fertility. More schooling still appears to lead to a delay in childbearing, driven by reducing the probability of first births between ages 20-24. As an alternative to the no reverse causality sample, we used our full sample of 808 MZ twins, and included a dummy variable equal to 1 if the schooling of twin i in pair j was prevented by fertility, marriage or both in the cross-sectional and within-MZ twins regressions. The results are qualitatively similar.

4.2 Measurement Error in Schooling

Another critique of within-MZ twins estimates pertains to measurement error in schooling. Because much more of the variation in schooling is across twins pairs rather than within twins pairs, the within-MZ twins estimator filters out much of the true signal of schooling without reducing measurement error (Bishop 1976, Griliches 1979). Because of this larger noise-to-signal ratio, the within-MZ twins estimator is subject to more measurement error bias towards zero than the simple cross-sectional estimator. If the within-MZ twins estimate is smaller than the cross-sectional estimate, it may be because it controls for the endogenously determined part of schooling or because of the larger bias due to measurement error or some combination of these two factors. Twins studies that have reports from other respondents (i.e., the other member of a twins pair, the twins' adult children), can estimate measurement error models where self-reported schooling is instrumented using reports from other respondents.

Table 7 presents within-MZ twins estimates of the schooling impact on number of children and age at first birth corrected for measurement error from both the main and no reverse causality samples. As we require each twin to report their sister's schooling, the sample size falls to 740 twins in the main sample and 346 twins in the no reverse causality sample. Columns 2 and 3 provide estimates correcting for random measurement error under the assumption that the measurement error terms in the two measures of schooling are uncorrelated. In this case the within-MZ twin difference in schooling (i.e., the difference between twin 1's report of her own schooling and twin 2's report of her own schooling) is instrumented with the difference between twin 2's report of twin 1's schooling and twin 1's report of twin 2's schooling. The within-MZ twins IV estimates in column 3 are suprisingly smaller in absolute magnitude compared to the within-MZ twins estimates in column 2. We are not sure how to explain this unexpected result. There is no problem of explantory power or weak instruments in the first stage. Columns 4 and 5 provide estimates assuming that the measurment error terms are correlated. For example, a twin who reports an upward-biased measure of her own schooling is more likely to report a higher measure of her sister's schooling. In this case the within-MZ twin difference in schooling is defined as the difference between twin 1's report of her own schooling and her report of twin 2's schooling. This is instrumented with the difference between twin 2's report of twin 1's schooling and twin 2's report of her own schooling. Now the within-MZ twins IV estimates in column 5 and large in absolute magnitude compared to the within-MZ twins estimates in column 4. As an alternative to instrumenting, in column 6 we restrict the sample to twin pairs who agree on their schooling differences. The estimates for number of children in column 6 are smaller than the within-MZ twins IV estimates in columns 3 and 5 and also statistically insignificant. For age at first birth, the estimates are significant and fairly similar in magnitude to the within-MZ twins IV estimates. They indicate that an additional grade of schooling leads to a half a year delay in childbearing (based on the main sample) and a one year delay (based on the no reverse causality sample).

4.3 Possible Mechanisms

The effect of schooling on fertility may be mediated through other channels such as marriage and assortative mating. Table 8 estimates the effect of schooling on four possible mechanisms- (1) probability of ever being married, (2) number of times married, (3) age at first marriage and (4) husband's schooling. The cross-sectional associations show that women with more schooling are less likely to have ever been married, been married a fewer number of times, delayed marriage and have more-schooled husbands. The association between schooling, the probability of being ever married and number of times married appears to be driven by unobserved endowments as the within-MZ twins estimates are zero. The within-MZ twins estimate for age at first marriage is significant and surprisingly larger than the cross-sectional association. The final row shows estimates of the relationship between women's schooling and husband's schooling. Here the sample is restricted to twins whose husband is the biological father of all their children. Similar to Behrman and Rosenzweig (2002), women with more schooling have more-schooled husbands and the cross-sectional estimates are biased upwards.

Table 9 (10) provide estimates of the effect of schooling on completed fertility and age at first birth when controlling for age at first marriage (husband's schooling) as an additional covariate. Table 9 shows that age at first marriage is a significant factor affecting both completed fertility and age at first birth. Controlling for age at first marriage reduces the magnitude of both the cross-sectional and within-MZ twins estimates. This is the case in both the main and preferred no reverse causality sample. In Table 10 however, husband's schooling has no effect on completed fertility. It appears that women's schooling is more important than husband's schooling for completed fertility. There is some indication that having more-schooled husbands leads to later first births.

5. Summary

We employ for the first time the within-MZ twins methodology to estimate the impact of schooling on completed fertility, probability of being childless and age at first birth, to provide alternative evidence to the mixed IV results in the literature. Our main estimates replicate previous findings that more schooling is *associated* with having fewer children, more likely to be childless and delaying childbearing. The within-MZ twins estimates that account for unobserved endowments also indicate that more schooling may cause women to have fewer children and delay childbearing, though not more likely to be childless. This pattern suggests that controlling for unobserved endowments has different impacts for differernt fertility-related outcomes. This is possible because the components of endowments that determine schooling and also determine directly the outcomes differ across outcomes such as completed fertility and being childless. We find that although more-schooled women have more-schooled husbands, husband’s schooling does not significantly affect compeleted fertility. Rather, the negative relationship between schooling and completed fertility is driven by more-schooled women delaying marriage. Our results are robust to two problems that have received considerable attention in the twins literature, the possibility of endogenous schooling differences due to reverse causality and measurement errors in schooling.

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Table 1: Descriptive Statistics

	MTR Twins		CPS Women	
	All Twins (1)	Mothers Only (2)	All Women (3)	Mothers Only (4)
Demographics				
Age	47.17 (5.38)	47.37 (5.45)	48.23 (5.40)	48.41 (5.41)
Schooling				
Grades of schooling	13.60 (2.06)	13.40 (1.96)	---	---
Proportion with				
Under high school	0.02 (0.14)	0.02 (0.15)	0.11 (0.31)	0.12 (0.32)
High School	0.38 (0.49)	0.41 (0.49)	0.38 (0.48)	0.39 (0.49)
Post-high school but no bachelor's degree	0.33 (0.47)	0.33 (0.47)	0.27 (0.45)	0.28 (0.45)
Bachelor's degree or higher	0.27 (0.44)	0.23 (0.42)	0.24 (0.42)	0.21 (0.41)
Fertility				
Number of children	2.27 (1.44)	2.71 (1.21)	2.26 (1.60)	2.64 (1.42)
Childless	0.14 (0.35)	---	0.15 (0.35)	---
Age at first birth: <20	0.09 (0.29)	0.11 (0.31)	0.24 (0.42)	0.28 (0.45)
Age at first birth: 20-24	0.40 (0.49)	0.49 (0.50)	0.37 (0.48)	0.43 (0.50)
Age at first birth: 25-29	0.26 (0.44)	0.29 (0.45)	0.16 (0.40)	0.19 (0.39)
Age at first birth: 30-34	0.07 (0.26)	0.08 (0.28)	0.06 (0.24)	0.07 (0.26)
Age at first birth: 35 or later	0.04 (0.19)	0.03 (0.16)	0.17 (0.37)	0.03 (0.17)
Age at first birth	---	24.15 (4.33)	---	23.16 (4.91)
N	808	628	14902	12721

Notes: Standard deviations in parentheses.

Table 2: Differences in grades of schooling within-MZ twins pairs, by schooling attainment

	Twins pairs in which at least one twin has			
	Completed High School (12 grades of schooling)	Some College Education (13-15 grades of schooling)	College Degree or higher (16 grades of schooling or more)	Total
0 difference in grades	50%	33%	39%	55%
1 grade difference	38%	46%	1%	23%
2 grades difference	3%	10%	30%	12%
3 grades difference	0%	7%	10%	3%
4 grades difference	7%	1%	13%	4%
5 or more grades difference	2%	3 %	7%	2%
Mean	0.84	1.06	1.81	0.88
Standard Deviation	1.28	1.14	1.74	1.29
N	206	190	134	404

Table 3: Cross-Sectional and Within-MZ Twins Estimates of Schooling on Fertility

	All Twins		Mothers Only	
	Cross-Section	Within-MZ Twins	Cross-Section	Within-MZ Twins
	(1)	(2)	(3)	(4)
Number of children	-0.140 (0.023)***	-0.141 (0.038)***	-0.080 (.021)***	-0.137 (.041)***
Childless	0.028 (0.007)***	0.013 (0.011)	---	---
Age at first birth: <20	-0.028 (0.005)***	-0.018 (0.011)	-0.033 (0.006)***	-0.020 (0.014)
Age at first birth: 20-24	-0.066 (0.007)***	-0.044 (0.019)**	-0.064 (0.010)***	-0.052 (0.023)**
Age at first birth: 25-29	0.044 (0.008)***	0.035 (0.020)*	0.069 (0.010)***	0.054 (0.023)**
Age at first birth: 30-34	0.018 (0.005)***	0.007 (0.012)	0.023 (0.007)***	0.013 (0.015)
Age at first birth: 35 over	0.004 (0.003)	0.006 (0.010)	0.004 (0.003)	0.006 (0.012)
Age at first birth	---	---	0.793 (0.078)***	0.508 (.159)***
N	808	404	628	314

Notes: All cross-sectional estimates control for a quadratic in year of birth-1936. Robust standard errors in parentheses. ***significant at 1% **significant at 5% *significant at 10%

Table 4: The Effect of Unexpected Fertility/Marriage of Schooling, Number of Children and Age at First Birth

	Mean of explanatory dummy (1)	Abs. schooling difference (2)	Abs. difference in number of kids (3)	Abs. difference in age at first birth (4)
Panel A				
Schooling disruption due to fertility				
All Twins	0.030	0.901	-0.056	---
		(.493)*	(.242)	
		[404]	[404]	
Mothers Only	0.035	0.876	---	1.87
		(0.538)		(0.75)**
		[314]		[314]
Panel B				
Schooling disruption due to marriage				
All Twins	0.017	0.998	0.090	---
		(.823)	(.248)	
		[404]	[404]	
Mothers Only	0.019	1.309	---	1.42
		(0.910)		(1.97)
		[314]		[314]
Panel C				
Schooling disruption due to fertility & marriage				
All Twins	0.035	0.937	0.237	---
		(.470)**	(.426)	
		[403]	[403]	
Mothers Only	0.045	0.943	---	0.477
		(.472)**		(1.04)
		[313]		[313]
Panel D				
Schooling disruption due to either fertility, marriage or both				
All Twins	0.08	1.11	0.116	---
		(.333)***	(.226)	
		[402]	[402]	
Mothers Only	0.09	1.18	---	1.46
		(.351)***		(0.709)**
		[312]		[312]

Notes: Robust standard errors in (.) and N in [.]. The sample size of all twins is 404 pairs and for twin mothers 314 pairs, but in panel C we exclude twin pair 77 from the regressions as both twins schooling in this pair has been affected by fertility and marriage (see Appendix Table A1). Similarly in panel D we exclude twin pairs 37 and 77. ***significant at 1%, **significant at 5%, *significant at 10%

Table 5: Descriptive Statistics “No Reverse Causality Sample”

	All Twins (1)	Mothers Only (2)
Demographics		
Age	46.40 (5.42)	47.69 (5.51)
Schooling		
Grades of schooling	13.40 (1.90)	13.03 (1.62)
Proportion of twins with		
0 difference in schooling	0.67 (0.47)	0.69 (0.46)
1 grade difference	0.21 (0.41)	0.22 (0.41)
2 grades difference	0.06 (0.23)	0.05 (0.22)
3 grades difference	0.02 (0.14)	0.02 (0.13)
4 grades difference	0.03 (0.18)	0.02 (0.15)
5 grades difference or more	0.01 (0.10)	0.00 (0.00)
Within-MZ twin difference in grades of schooling	0.57 (1.06)	0.46 (0.89)
Fertility		
Number of children	1.91 (1.41)	2.58 (1.06)
Childless	0.23 (0.42)	---
Age at first birth: <20	0.01 (0.07)	0.01 (0.09)
Age at first birth: 20-24	0.33 (0.47)	0.47 (0.50)
Age at first birth: 25-29	0.28 (0.45)	0.36 (0.48)
Age at first birth: 30-34	0.09 (0.29)	0.12 (0.32)
Age at first birth: 35-40	0.05 (0.22)	0.03 (0.18)
Age at first birth	---	25.52 (4.22)
N	378	246

Notes: Standard deviations in parentheses

Table 6: Cross-Sectional and Within-MZ Twins Estimates of Schooling on Fertility, "No Reverse Causality Sample"

	All Twins		Mothers Only	
	Cross-Section	**Within-MZ Twins**	**Cross-Section**	**Within-MZ Twins**
	(1)	**(2)**	**(3)**	**(4)**
Number of children	-0.167 (0.034)***	-0.162 (0.058)***	-0.031 (.038)	-0.205 (.067)***
Childless	0.061 (0.012)***	0.040 (0.027)	---	---
Age at first birth: <20	---	---	---	---
Age at first birth: 20-24	-0.083 (0.010)***	-0.043 (0.023)*	-0.099 (0.016)***	-0.103 (0.044)**
Age at first birth: 25-29	-0.001 (0.012)	-0.018 (0.030)	0.042 (0.020)**	0.026 (0.048)
Age at first birth: 30-34	0.026 (0.010)***	0.007 (0.019)	0.061 (0.017)***	0.051 (0.032)
Age at first birth: 35 over	0.002 (0.005)	0.014 (0.016)	0.004 (0.005)	0.026 (0.025)
Age at first birth	---	---	1.02 (0.150)***	1.10 (.365)***
N	378	189	246	123

Notes: All cross-sectional estimates control for a quadratic in year of birth-1936.. Robust standard errors in parentheses. ***significant at 1% **significant at 5% *significant at 10%

Table 7: Within-MZ Twins Estimates Corrected For Measurement Error

		Random Measurement Error		Correlated Measurement Error		Twin pairs who agree on schooling differences
	Cross-Section	**Within-MZ**	**Within-MZ IV Twins**	**Within-MZ Twins**	**Within-MZ IV Twins**	**Within-MZ Twins**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Sample & Fertility Outcme						
All twins, full original sample Number of children	-.132 (.023)*** [740]	-.138 (.038)*** [370]	-.097 (.051)* [370]	-.109 (.039)*** [370]	-.145 (.051)*** [370]	-.059 (.045) [284]
All twins, no reverse causality Sample, Number of children	-.163 (.034)*** [346]	-.172 (.062)*** [173]	-.131 (.107) [173]	-.124 (.090) [173]	-.197 (.082)** [173]	-.086 (.076) [146]
Twin mothers, full original Sample, age at first birth	.783 (.082)*** [574]	.552 (.162)*** [287]	.422 (.239)* [287]	.474 (.177)*** [287]	.559 (.239)** [287]	.495 (.212)** [218]
Twin mothers, no reverse causality sample, age at first birth	1.02 (.155)*** [244]	1.27 (.418)*** [112]	1.16 (.793) [112]	1.00 (.615) [112]	1.48 (.524)** [112]	1.06 (.476)*** [98]

Notes: : All cross-sectional estimates control for a quadratic in year of birth-1936. In columns 2 and 6 the within-MZ twins difference in schooling is defined as the difference between twin 1's report of twin 1's own schooling and twin 2's report of twin 2's own schooling. For the within-MZ twins IV estimates in column 3 this difference is instrumented with the difference between twin 2's report of twin 1's schooling and twin 1's report of twin 2's schooling. In column 4 the within-MZ twins difference in schooling is defined as the difference between twin 1's reports of twin 1's own schooling and twin 1's report of twin 2's schooling. For the within-MZ twins IV estimates in columns 5 this difference is instrumented with the difference between twin 2's report of twin 1's schooling and twin 2's report of twin 2's own schooling. Robust standard errors in (.) and N in [.]. ***Significant at 1%, **significant at 5%, *significant at 10%

Table 8: The Effect of Schooling on Mediating Mechanisms

	Main Sample		No Reverse Causality Sample	
	Cross-Section	Within-MZ Twins	Cross-Section	Within-MZ Twins
	(1)	(2)	(3)	(4)
Ever Married	-0.023 (.006)*** [808]	0.000 (.008) [404]	-0.055 (.012)*** [378]	-0.014 (.023) [189]
Number of Times Married	-0.028 (0.007)*** [808]	0.006 (.013) [404]	-0.062 (.012)*** [378]	-0.011 (.027) [189]
Age at first marriage	0.503 (.124)*** [638]	0.655 (.223)** [319]	0.825 (.206)*** [280]	1.29 (.470)*** [140]
Husband's Schooling	0.691 (.054)*** [546]	0.386 (.122)*** [273]	0.893 (.083)*** [266]	0.545 (.242)** [133]

Notes: All cross-sectional estimates control for a quadratic in year of birth-1936.. Robust standard errors in parentheses (.) and N in [.]***significant at 1% **significant at 5% *significant at 10%

Table 9: The Effect of Schooling on Completed Fertility and Age at First Birth, controlling for Age at first marriage

	Cross-Section (1)	Cross-Section (2)	Within-MZ Twins (3)	Within-MZ Twins (4)
Number of children, Main Sample				
Schooling	-0.091	-0.073	-0.171	-0.151
	(.024)***	(.024)***	(.046)***	(.046)***
Age First Married	---	-0.036	---	-0.031
		(.011)***		(.012)**
N	638	638	319	319
Number of children, No Reverse Causality Sample				
Schooling	-0.072	-0.030	-0.247	-0.175
	(.041)*	(.041)	(.078)***	(.070)**
Age first married	---	-0.051	---	-0.056
		(.018)***		(.023)**
N	280	280	140	140
Age at first birth, Main Sample				
Schooling	0.781	0.685	0.594	0.530
	(.080)***	(.087)***	(.175)***	(.177)***
Age First Married	---	0.235	---	0.122
		(.054)***		(.054)**
	556	556	278	278

Age at first birth, No Reverse Causality Sample				
Schooling	1.08 (.162)***	0.774 (.174)***	1.04 (.374)***	0.834 (.309)***
Age First Married	---	0.436 (.119)***	---	0.319 (.122)**
N	222	222	111	111

Notes: All cross-sectional estimates control for a quadratic in year of birth-1936. Robust standard errors in parentheses.***significant at 1% **significant at 5% *significant at 10%

Table 10: The Effect of Schooling on Completed Fertility and Age at First Birth, controlling for Husband's Schooling

	Cross-Section (1)	Cross-Section (2)	Within-MZ Twins (3)	Within-MZ Twins (4)
Number of children, Main Sample				
Schooling	-0.074	-0.050	-0.122	-0.119
	(.025)***	(.030)*	(.048)**	(.049)**
Husband's Schooling	---	-0.035	---	-0.007
		(.025)		(.040)
N	546	546	273	273
Number of children, No Reverse Causality Sample				
Schooling	-0.084	-0.079	-0.246	-0.249
	(.042)***	(.050)	(.075)***	(.081)***
Husband's Schooling	---	-0.006	---	0.007
		(.003)		(.041)
N	266	266	133	133
Age at first birth, Main Sample				
Schooling	0.691	0.453	0.235	0.143
	(.086)***	(.106)***	(.192)***	(.188)
Husband's Schooling	---	0.335	---	0.218
		(.092)***		(.115)**
	446	446	233	233

Age at first birth, No Reverse Causality Sample				
Schooling	1.06 (.157)***	0.754 (.186)***	1.12 (.375)***	1.000 (.356)***
Husband's Schooling	---	0.326 (.134)**	---	0.202 (.142)
N	206	206	103	103

Notes: All cross-sectional estimates control for a quadratic in year of birth-1936. Robust standard errors in parentheses. ***significant at 1% **significant at 5% *significant at 10%

Appendix 1

Table A1: Twins where schooling completion may have been prevented by fertility and/or marriage

Case Number	Grades of Schooling	Number of Children	Year of birth of Children	Year of Marriage	Schooling Disrupted due to
Twin 1 in pair 3	12. But reports going to college 54-56	2	54, 56	55	Fertility: Birth of second child in 56 may have prevented completion of college education
Twin 1 in pair 37	12. But reports going to college 57-58	3	59, 65, 69	57	Fertility: Birth of first child in 59 may have prevented completion of college education
Twin 2 in pair 74	12. But reports going to vocational school 58-60	2	60, 63	Never married	Fertility: Birth of first child in 60 may have prevented completion of vocational schooling
Twin 2 in pair 122	12. But reports going to vocational school in 62	4	63, 65, 67, 70	60	Fertility: Birth of first child in 63 may have prevented completion of vocational schooling
Twin 1 in pair 126	12. But reports going to vocational school 63-64 for 7 months	1	64	70	Fertility: Birth of first child in 64 may have prevented completion of vocational schooling
Twin 2 in pair 213	12. But reports going to college 68-74	2	75, 79	72	Fertility: Birth of first child in 72 may have prevented completion of college education
Twin 1 in pair 243	12. But reports going to college 66-69	2	69, 73	66	Fertility: Birth of first child in 69 may have prevented completion of college education
Twin 2 in pair	12. But reports going	3	72, 74, 76	68	Fertility: Birth of first child in 72

251	to college 68-71				may have prevented completion of college education
Twin 1 in pair 307	12. But reports going to vocational school 73-74	1	74	70	Fertility: Birth of first child in 74 may have prevented completion of vocational schooling
Twin 1 in pair 333	12. But reports going to college for half a year in 77	2	78, 80	71	Fertility: Birth of first child in 78 may have prevented completion of college education
Twin 2 in pair 348	12. But reports going to vocational school 71-72 for 7 months	3	73, 76, 80	71	Fertility: Birth of first child in 73 may have prevented completion of vocational schooling
Twin 1 in pair 375	16. Went to college 72-76. Also was at graduate school for 1 year in 84	3	85,89, 92	76	Fertility: Birth of first child in 85 may have prevented completion of graduate education
Twin 1 in pair 188	12. But reports going to college 65-66	2	69, 72	66	Marriage in 66 may have prevented completion of college education
Twin 2 in pair 194	12. But reports going to college 66-67	2	69, 72, 75	67	Marriage in 67 may have prevented completion of college education
Twin 2 in pair 197	12. But reports going to college 66-67	2	69, 72	67	Marriage in 67 may have prevented completion of college education
Twin 2 in pair 226	12. But reports going to college 67-69	3	72, 77, 79	69	Marriage in 69 may have prevented completion of college education
Twin 1 in pair 232	12. But reports going to college 67-68	2	77, 81	68	Marriage in 68 may have prevented completion of college education
Twin 1 in pair	12. But reports going	3	74, 75, 77	70	Marriage in 70 may have prevented

277	to college 69-70				completion of college education
Twin 1 in pair 391	12. But reports going to college 73-75	2	78, 81	75	Marriage in 75 may have prevented completion of college education
Twin 1 in pair 10	12. But reports going to college 54-56	4	57, 59, 60, 61	56	Marriage and fertility may have both prevented completion of college education
Twin 1 in pair 24	12. But reports going to college 55-56	4	57, 58, 59, 65	56	Marriage and fertility may have both prevented completion of college education
Twin 2 in pair 37	12. But reports going to college 56-58	4	58, 61,63, 65	58	Marriage and fertility may have both prevented completion of college education
Twin 1 in pair 60	13. Went to college 59-63, obtained a diploma but no degree	4	64, 65, 67, 68	63	Marriage and fertility may have prevented completion of degree course at college
Twin 2 in pair 71	12. But reports went to vocational school in 59 for 9 months	8	60, 62, 63, 65	60	Marriage and fertility may have prevented completion of vocational schooling
Twin 1 in pair 77	12. But reports going to vocational school 59-60	2	61, 62	60	Marriage and fertility may have prevented completion of vocational schooling
Twin 2 in pair 77	12. But reports going to vocational school 59-60	2	60, 62	59	Marriage and fertility may have prevented completion of vocational schooling
Twin 1 in pair 85	12. But reports going to vocational school 60-61 for 7 months	3	60, 61, 63	61	Marriage and fertility may have prevented completion of vocational schooling

Twin 1 in pair 143	12. But reports going to college 63-65	3	62, 65, 81	65	Marriage and fertility may have both prevented completion of college education
Twin 1 in pair 156	12. But reports going to college 64-66	2	66, 69	66	Marriage and fertility may have both prevented completion of college education
Twin 2 in pair 159	12. But reports going to vocational school in 64 for 9 months	1	65	64	Marriage and fertility may have both prevented completion of vocational schooling
Twin 2 in pair 174	12. But reports going to college 65-67	2	68, 69	67	Marriage and fertility may have both prevented completion of college education
Twin 1 in pair 184	12. But reports going to college 65-66	4	67, 70, 74, 76	66	Marriage and fertility may have both prevented completion of college education
Twin 1 in pair 185	16. But reports going to graduate school 68-70	4	71, 74, 81, 81	70	Marriage and fertility may have both prevented completion of graduate education
Twin 1 in pair 211	12. But reports going to professional school 67-69	3	70, 72, 88	69	Marriage and fertility may have both prevented completion of post-high school education
Twin 1 in pair 325	12. But reports going to college 70-71	1	72	71	Marriage and fertility may have both prevented completion of college education

Appendix 2

In order to deal with the issue of reverse causality, the estimates in Table 6 are based on a sample of twins for whom fertility should not affect schooling, as first births (if any) take place at least 2 years after schooling completion. Here, we detail that fertility should not affect schooling for this particular sample of twins.

For the full sample of All Twins:

1. The total sample consists of 378 twins. There are 88 twins who are childless and thus fertility cannot possibly have influenced schooling for these 88 twins.
2. There are two twins pairs where both twins have less than 12 grades of schooling and we assume that fertility did cause them to drop out of high school as the first birth happened at sufficiently late ages. In particular, in twin pair 33 both twins have 9 grades of schooling and first gave birth at age 18. In pair 116 both twins have 10 grades of schooling and first gave birth at age 26. However, there is no schooling difference in these 2 twins pairs, so they do not contribute to the identification of any of the within-MZ twins estimates.
3. There are 116 twins whose highest schooling attainment is a high school diploma (12 grades of schooling). Reverse causality should not matter here because first birth happens sufficiently after high school completion. If we assume they graduated high school at age 18 then on average first births happened 6.29 years after high school graduation.
4. There are 24 twins with 12 grades of schooling. However, these twins also undertook some post high school schooling (vocation or college education) but they did not obtain any qualifications. Again we assume that reverse causality did not prevent schooling completion for these twins, as their first births occurred at least 2 years after they left vocational school/college. On average first births occurred 4.93 years after leaving vocational school/college without a qualification.
5. There are 146 twins who have some post-high school schooling (i.e., more than 12 grades of schooling) and whose first birth occurred at least 2 years after schooling completions.
6. Figure 1 below shows the distribution years after leaving schooling when the first birth occurred for (1) twins whose highest schooling qualification is a high school diploma only, (2) twins who have a high school diploma, undertook some vocational or college education but did not obtain a qualification and (3) twins with more than 12 grades of schooling (post-high school qualifications).
7. The sample of twin mothers is a just a sub-sample of the sample of all twins.

Figure 1: Distribution of Years After Schooling Completion When First Birth Occurred