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# Do Educated Women Make Bad Mothers? Twin Studies of the Intergenerational Transmission of Human Capital

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## Abstract

“Does increasing women’s schooling raise the schooling of the next generation?” is the question posed by Jere Behrman and Mark Rosenzweig (2002) in their eponymous article. Their answer to the question is no. In fact, they conclude that raising women’s schooling may even lower the schooling of the next generation. In this paper, we show that Behrman and Rosenzweig’s results are not robust to alternative coding schemes and sample selection rules, and we show that the policy inference may be misguided.

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# Do Educated Women Make Bad Mothers?

## Twin Studies of the Intergenerational Transmission of Human Capital

“With the spectacular growth in the number of women going to college (they now outnumber men), the Pentagon faces a daunting prospect: some day, those legions of educated mothers will, at the same time, be setting a standard at home that will steer their children more surely toward college, even as their added income will help insure that the family has the money to pay for college without turning to military service.”

- New York Times, April 6, 2003, “Is This Really an All-Volunteer Army?”

“Our findings thus clearly suggest that ... increasing men’s schooling would raise the level of schooling of the next generation by a small amount, while raising the level of schooling attainment of women would not, and may even lower it.”

- J. R. Behrman and M. R. Rosenzweig, American Economic Review, March 2002

## 1 Introduction

“Does increasing women’s schooling raise the schooling of the next generation?” is the question posed by Jere Behrman and Mark Rosenzweig (2002) in their eponymous article. Their answer to the question is no: “[while] increasing men’s schooling would raise the level of schooling of the next generation by a small amount ... raising the level of schooling attainment of women would not, and may even lower it”. This rather startling conclusion emerges from an elaborate modeling exercise that employs a mail survey of adult identical twins (MZs) born between 1936 and 1955, drawn from the Minnesota Twin Registry. But the core empirical finding on which their conclusion rests is that

regressions of child’s schooling on parent’s schooling, run within pairs of parents, show a negative slope for the female parents, a positive one for the male parents. (To avoid possible confusion, we emphasize that the parents who are paired are identical twins and have the same sex, the children are cousins who needn’t be of the same sex).

Behrman and Rosenzweig (henceforth often B&R) write that the regressions refer to “424 (244) individuals from currently married female (male) MZ twin-pairs in which each twin in the pair was married and had at least one child aged 18 or older”. (The authors since have told us that only one child, the oldest, was used for each twin). For the female MZ parent sample, the slope of the regression of child’s schooling on mother’s schooling across the 424 individuals is 0.332. But when the difference in the child’s schooling is regressed on the difference in the mother’s schooling across the 212 pairs, the slope is -0.245. For the male MZ parent sample, with “father’s” replacing “mother’s”, the corresponding slopes are 0.466 and 0.356.

According to Behrman and Rosenzweig, it is the within-pair slopes that provide proper estimates of policy effects, the cross-section slopes being confounded by common causes of parent and child education. Hence their provocative answer to the question that engaged them, and now us. We learned that the extraordinarily detailed set of raw survey responses was available online at <http://www.pop.upenn.edu/projects/sestwins/statement.html>, so we started to explore alternative analyses. The authors generously provided us with the data set that they had extracted from the raw survey responses and adapted for their analyses, so we were able to match those cases with the online raw records, as well as to assemble our own extracts. We found that the transition from the raw survey responses to a working data set is by no means trivial.

The relevant education measure for intergenerational studies is that of ultimate completed schooling, which is presumably why B&R would not want to use children younger than 18. *It turns out that B&R’s cutoff age was 10, not 18, years.* These younger children were distributed across 59 of the 212 female MZ pairs. In addition, of the 308 older children, only 209 had completed school. Similarly for Behrman and Rosenzweig’s male MZ extract. Eliminating kids under the age of 18 and those still in school would leave ultimate completed schooling available for both cousins in only 80 of the 212 female MZ pairs, and in only 40 of the 122 male MZ pairs. One issue that merits examination is the sensitivity of Behrman and Rosenzweig’s results to the criteria for sample inclusion.

Another issue is sensitivity to the construction of the schooling variables. The primary survey

question (q83) used to construct the children’s schooling variable read as follows:

“What is the highest grade in school that this child has completed?

*(Primary grades.....0 to 8,  
secondary grades.....9 to 11,  
completed high school..... 12,  
college.....13 to 16,  
some post-bachelor’s work .. 17,  
master’s degree ..... 18,  
some post-master’s work .. 19,  
Ph.D. or prof. degree ... 20)”*

We refer to this measure as schooling-to-date. A scalar numerical answer was no doubt anticipated, but some parents reported categorically, as the structure of the question may have encouraged. For them, the mapping to a scalar measure is ambiguous. Recall that the parents were filling out a mail questionnaire, with no interviewer present to resolve ambiguities.

So how did Behrman and Rosenzweig handle the many children with unfinished education, those whose parents responded “yes” to the item, “Does this child attend school now?” (q86). The article does not mention this issue at all. The authors informed us that for such children, they generally used the response to another item (q84):

“What is/was the highest grade in school that you expect(ed) this child to complete?”

*The time reference of this question is quite unclear.* Some parents of young children apparently interpreted it to mean “expected to complete as of now”, while some parents of older children, evidently pleasantly surprised by their children’s thirst for knowledge, reported an expectation that was less than their report of the child’s schooling-to-date.

In any event, treating, in a model of intergenerational transmission, parental expectations for young children as if they were realizations is disconcerting. Further, it turns out that Behrman and Rosenzweig did not consistently apply their rule for assigning child’s schooling. For example, in their female MZ sample, for some 31 children, they used expected schooling even though the child was

not in school, and for 6 children they used schooling-to-date even though the child was in school.<sup>1</sup> Admittedly, B&R may have handled those not-in-school cases correctly. Most of the surveys were completed during the summer months, and respondents may have taken q86, “Does this child attend school now?” very literally.

The difficulties of constructing a scalar measure of schooling are magnified when one turns to the parents’ education. One item (q33) asks, “At the time of your marriage, what was the highest grade in school that you . . . had completed?”, accompanied by the same categorical guide as at q83. But that item relates only to education as of the marriage date. Behrman and Rosenzweig developed their parent’s schooling variable from a series of survey items (q1 - q15) that concern highest grade 1 to 12 completed, type of diploma awarded, college (calendar year of first and last attendance, length of attendance, major, type of degree), graduate or professional (calendar year of first and last attendance, length of attendance, major, type of degree), and vocational education. In view of the complexity of that series of questions, and the vagaries of individual histories, considerable scope exists for alternative judgments in coding the parent’s schooling variable.

We understand from B&R that their procedure drew on additional sources of information, relied on the authors’ judgment, and cannot be captured by a formal algorithm. We lack access to those other sources, and can not always infer their coding scheme from the extract they provided. So we use our best judgment. For child’s schooling, we use q83 if not in school, and max(q83,q84) if in school, and take the upper end of all intervals for categorical answers. For parent’s schooling, we take our reading of the items q1 - q15, and apply this framework : assign actual years if no high school diploma, 11 if GED, 12 if high school diploma, 13 if vo-tech degree/diploma, 14 if associate degree, 16 if college diploma, 18 if master’s degree, 19 if J. D. or MBA, 20 if doctor’s degree. In addition, for individuals who pursued (without ever completing) a degree we assign the number of years that corresponds to their highest degree *plus* the number of years they spent earning the uncompleted degree. In the event that an individual pursued the degree for more years than the number that corresponds to the degree levels listed above, we assign to that individual 1 year shy of that number.<sup>2</sup> For both parents and children, we do not count partial years of schooling.

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<sup>1</sup>Here, we infer B&R’s coding rule by comparing the education measure that they use in their analysis with the original data. There are 17 cases in which we could not determine how the child’s education was constructed.

<sup>2</sup>For example, if an individual reports that she does not have a college degree but attended college for 5 years, then we assign her 15 years of schooling (rather than, say, 16 or 17).

## 2 Child-Parent Schooling Regressions

We begin our sensitivity analysis with the key results in Behrman and Rosenzweig's Tables 4 and 5, namely the slopes of child's schooling on twin-parent's schooling for female and male MZ twin pairs, run cross-sectionally and then run within-pairs. As previously indicated, Behrman and Rosenzweig sent us the data sets they used for regression, for their samples of 212 female, and 122 male, twin pairs. Our run of the same regressions is given in row 1 of Table 1, which coincides with the corresponding results in their tables. For the female twin pairs, as one goes from cross-section to within-pair, the slope turns to a large negative number, namely  $-.245$ , the striking result that permeates their article. For the male twin pairs, the slope drops only slightly.

As we have seen, to be included in their samples, both members of the twin pair had to be currently married, each with a child aged 10 years or more. When we searched the online database we found 251 female, and 171 male, twin pairs that met those criteria. We presume that our counts are higher because unlike Behrman and Rosenzweig, we did not require that data be available on spouse earnings and experience, and on co-twin's report of schooling, variables that they needed for implementation of their full model. Using our coding for schooling of parents and children on these larger samples, we obtained row 2 of Table 1. The negative within-pair slope for females is gone, and the within-pair slope for males is sharply reduced. This indicates that Behrman and Rosenzweig's answer to the question in the title of their article is not robust.

Now, that comparison of our results with those of Behrman and Rosenzweig is confounded by two factors – differences in sample inclusion rules, and differences in coding. One way to avoid the first confound is to focus on those cases which are common to their data set and ours. This gives us 202 female, and 120 male, matched cases. (One might expect that all of B&R's pairs would appear in our list, but there was some slippage. In some cases, they had taken data from the second-born rather than the oldest child).

Regression results for the matched samples are reported in rows 3 and 4 of our Table 1. The number of missing cases being small leaves row 3, which uses their coding, quite close to row 1. In row 4, which uses our coding, results for the male MZs are somewhat different than those in row 2, which had 51 more cases. Evidently, the sample inclusion criteria play a role: including male MZ pairs even though they apparently are missing data on earnings or some other factor produces a

Table 1: Slopes in regressions of child's schooling on twin-parent's schooling.

Age groups

BROAD = child aged 10 or older

NARROW = child aged 18 or older

VERY NARROW = child aged 18 or older, not in school

Database: source of cases

BR = File sent to us by Behrman & Rosenzweig

WS = Located by us in website

BR/WS = Matched (i.e. found in both those sources)

Coding of the education variables:

BR = Behrman & Rosenzweig

AG = Antonovics & Goldberger

Database	Coding	n	FEMALE MZ PAIRS		n	MALE MZ PAIRS	
			cross	within		cross	within
BROAD							
1) BR	BR	212	.332 (.050)	-.245 (.145)	122	.466 (.064)	.356 (.156)
2) WS	AG	251	.426 (.052)	.064 (.132)	171	.310 (.058)	.147 (.123)
3) BR/WS	BR	202	.330 (.051)	-.180 (.146)	120	.456 (.065)	.356 (.157)
4) BR/WS	AG	202	.360 (.056)	.000 (.154)	120	.430 (.062)	.369 (.145)
NARROW							
5) BR	BR	152	.330 (.059)	-.280 (.194)	76	.433 (.080)	.333 (.197)
6) WS	AG	178	.400 (.062)	.072 (.166)	102	.327 (.066)	.116 (.140)
7) BR/WS	BR	148	.332 (.060)	-.239 (.196)	75	.428 (.081)	.333 (.198)
8) BR/WS	AG	148	.364 (.067)	.129 (.204)	75	.386 (.079)	.358 (.188)
VERY NARROW							
9) BR	BR	80	.256 (.083)	-.340 (.339)	40	.569 (.102)	.474 (.206)
10) WS	AG	90	.277 (.090)	.030 (.272)	47	.493 (.090)	.479 (.160)

Standard errors in parentheses. n = number of twin pairs.

Vocational education is counted in AG coding. Runs with vocational education excluded in AG coding showed only slight differences from those given here: at most .050 absolute difference in the slope, except for the last entry in row 10, which went from .479 to .410.



different intergenerational education relation. On the other hand, row 4 results for the female MZs differ only slightly from those in row 2, which had 49 more cases.

Next, we restrict the analysis in a way that the authors claimed, but failed, to implement, namely we confine attention to pairs in which both children were at least 18 years old. By that age, some children are finished with their schooling, while others are far enough along that their parents' expectation is perhaps well-founded. If we cull the "adolescent" pairs – those with one or both children aged 10 through 17 – out of Behrman and Rosenzweig's extract, we lose about one-third of their samples, and get the slopes in row 5 of our Table. These are for practical purposes the same as those in row 1, so to our surprise, Behrman and Rosenzweig would have obtained much the same results had they implemented their "aged 18 and over" rule.

If we cull the adolescent pairs out of our samples, we again lose about one-third of the cases, and get the slopes in row 6 of the Table. These are not unlike those in row 2, but we doubt that finding estimates  $0.072 \pm 0.166$  and  $.116 \pm 0.140$  would have occasioned an article on the differences between maternal and paternal influences on the education of children. Moving to matched cases in rows 7 and 8, we see that for females, but not for males, coding differences make our results sharply different from Behrman and Rosenzweig's.

Our interpretation is that the coding differences, as well as the sample inclusion differences, have a nontrivial impact. Our plausible attempt to follow Behrman and Rosenzweig's announced plan, working with children 18 years and older, led to results quite different from theirs. Indeed, even though the estimated coefficient of father's education is still larger than that on mother's education in most specifications, these differences are generally small, and do not strongly suggest that mother's education and father's education play dramatically different roles in determining the education levels of children.

What happens if one takes a very narrow sample inclusion rule, discarding all pairs in which one or both children are still in school? The results, in rows 9 and 10 of the Table, appear to restore some credibility to Behrman and Rosenzweig's contention. But, it is worth noting that we are now dealing with a rather select cohort of people. Of the 90 female MZ pairs in row 10, only 9 were born after 1949, the youngest in 1955; of the 47 male MZ pairs, the youngest was born in 1948.

### 3 Comparison of Education Coding

As Table 1 indicates, the regression results are quite sensitive to the manner in which education is coded. For the “NARROW” sample, Table 2 specifies the number of individuals for whom our measure of education differs from that of B&R, and the resulting number of cases in which the within-twin *difference* in education also does not match. We break down the sources of these discrepancies into 5 broad categories. First, B&R do not count years spent in vocational training, whereas we do. Differences also occur because, for individuals who indicate that they pursued (without ever completing) a degree for more years than the number that corresponds to that degree, B&R set that individual’s years of schooling equal to 6 months shy of that number, whereas we set it equal to 1 year shy. In addition, the within-twin differences do not always match because B&R count partial years of schooling, and we do not. Another reason for the mismatch between our education measure and that of B&R is that B&R assign 17 years of schooling to individuals who have a master’s degree whereas we assign 18 years. The remaining discrepancies (the “Other” category in Table 2) occur (evidently) because of subtler differences in our judgment about how to code the data.

As Table 2 shows, of the 296 female MZ twins (148 pairs) that are in both our sample and B&R’s sample, the education level differs for 105 individuals, and, as a result, the within-twin *difference* in mother’s education differs for 58 pairs. Similarly, of the 150 male MZ twins (75 pairs) that are in both our sample and B&R’s sample, the education level differs for 63 individuals, and the within-twin difference does not match for 35 pairs. For both male and female MZ twins, a large fraction of the education differences occur because we count years spent earning a vocational degree, whereas B&R do not. As it turns out, if we change our education measure so that, like B&R’s measure, it does *not* include vocational education, there are no substantive changes to our coefficient estimates in Table 1. In fact, no single one of these coding discrepancies can, alone, explain the differences between our estimates and those of B&R.

However, the numbers in Table 2 point to a more serious issue. It appears that B&R did not use the same coding rules for male and female MZ twins. For female MZ twins, no coding discrepancies are attributed to B&R’s assigning 17 years of schooling to a master’s degree rather than our 18. The reason: B&R assigned 18 years of schooling to a master’s degree for female MZ twins. For male MZ twins, no coding discrepancies are attributed to B&R’s subtracting a half-year for uncompleted

Table 2: Number of Individuals and Within-Twin Differences Affected by Coding Discrepancies

Source of Discrepancy <sup>†</sup>	FEMALE MZ TWINS		MALE MZ TWINS	
	Individuals	Within-Twin Difference	Individuals	Within-Twin Difference
B&R do not count vocational education	64	31	25	17
B&R subtract 6 months from next degree	8	7	0	0
B&R allow partial years	9	6	1	1
B&R assign 17 years to master's	0	0	17	6
Other	24	14	20	11
<b>Total</b>	<b>105</b>	<b>58</b>	<b>63</b>	<b>35</b>

<sup>†</sup>To avoid double counting, for observations that fall into more than one category, we include them only in the first category listed.

degrees rather than our full year. The reason: B&R subtracted a full year for male MZ twins.

We also examined the education coding of the male and female *spouses* of the twin respondents. We found that whereas B&R give male respondents 17 years of schooling for a master's degree, they give male spouses (i.e. men who are married to female twins) 18 years. Similarly, whereas B&R subtract 6 months from the education level of female respondents with uncompleted degrees, they subtract 1 year from the education level of female spouses (i.e. women who are married to male twins). Thus, it appears that the education discrepancies between men and women are related to inconsistencies in the coding algorithm rather than actual differences in the education patterns of men and women.

The inconsistency in the way in which Behrman and Rosenzweig treat men and women becomes particularly bothersome when considering the 15 male twins and 15 women twins who attended graduate school for at least a year without receiving a post-bachelor's degree. B&R gave the males 16 years of education; they gave the females 17.5 if they attended graduate school for at least two years.

In any case, while their article is focused on comparing the coefficients on father's and mother's schooling, the authors coded those two variables differently.

As it turns out, small discrepancies in education coding can lead to relatively large differences in

the coefficient estimates. To see this, Figures 1 and 2 compare our coding of education with that of Behrman and Rosenzweig for both female and male MZ twins in the matched NARROW samples. In both figures, the first panel plots the education levels, while the second plots the within-twin differences.<sup>3</sup> The size of the circles is proportional to the number of observations. As the figures reveal, even though the relationship between our education coding and B&R’s coding is fairly close for *levels* of education, we are much further off for the within-twin *differences*. Thus, it is perhaps not surprising that, while our cross-sectional estimates of the coefficient on education are close to those of B&R, the within-twin estimates are quite different.

These figures also highlight the well-known problem of measurement error in first-differenced data. B&R use co-twin’s report to correct for measurement error, but we doubt that a conventional errors-in-variables model applies. First, unlike classical measurement error, the noise introduced by coding difficulties is likely to be correlated with the true value of education. Second, even if the measurement error were classical, co-twin’s report would almost certainly be an invalid instrument since the coding error in one twin’s report is likely to be correlated with the coding error in the other twin’s report. In a data set like this one in which respondents complete mail-in surveys and often give answers that are difficult to interpret, these issues become even more severe.

The high proportion of 0’s in the second panel of Figure 1 suggests that this natural experiment may have been singularly uninformative about the effect of changing mother’s education: in 92 of the 148 pairs, according to B&R’s coding the twins showed no difference in years of schooling.

## 4 Models

For Behrman and Rosenzweig, the within-pair regression of child’s schooling on the twin-parent’s schooling serves as the empirical entry point for an analysis of the intergenerational transmission of education. In their specification, the central role is played by a schooling transmission equation, written here as:

$$S_c = \delta_1 S_m + \delta_2 S_f + \gamma_1 h_m + \gamma_2 h_f + \epsilon_c, \tag{1}$$

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<sup>3</sup>We plot the within-twin difference twice: one in which subtracting twin one’s education level from that of twin two and the other in which we reverse the roles of twin one and twin two.

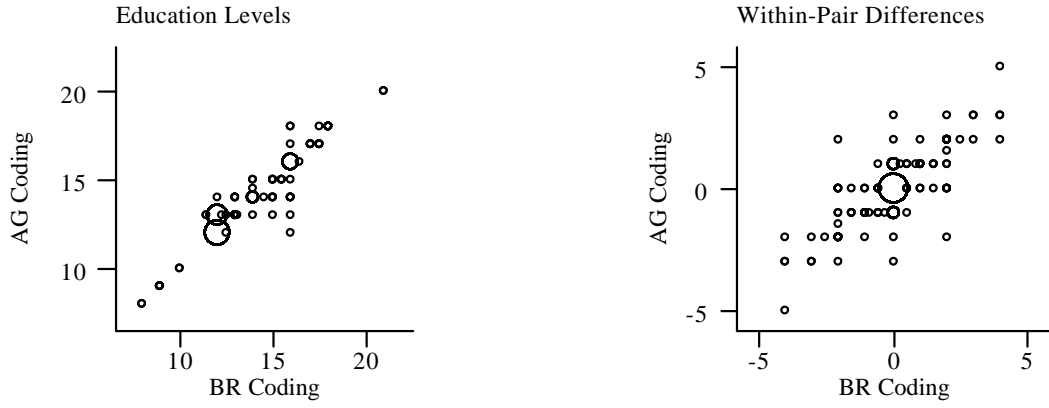


Figure 1: Comparison of Female MZ Twin's Education, NARROW Sample

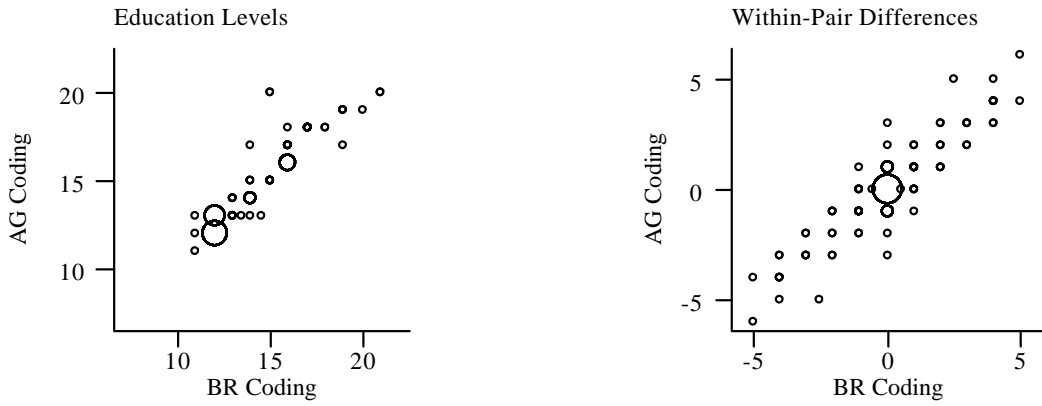


Figure 2: Comparison of Male MZ Twin's Education, NARROW Sample

where  $c$ ,  $m$ ,  $f$  denote *child*, *mother*, and *father*. Here  $S$  refers to schooling (observed), while  $h$  (unobserved) is variously referred to as “earnings endowment”, “heritable endowment”, “pre-school human capital (genetics, family background)”, “heritable pre-school endowment”, and “heritable ‘ability’ ”. The disturbance  $\epsilon_c$  is taken to be independent of the four parental variables. (We ignore a “talent for childrearing” variable (unobserved) that appears when dealing with twin mothers, but not with twin fathers; it serves to rationalize differences in parameter estimates between the two samples). Elsewhere in the model, the individual’s endowment enters, along with experience and schooling, in the determination of his/her earnings. Further, the individual’s endowment and schooling enter the determination of his/her spouse’s endowment and schooling, via assortative marriage equations.

The parental schooling variables are presumed to be correlated with the parental endowment variables, so that cross-section regression of  $S_c$  on the parental schooling variables is inappropriate for estimating the parameters of interest, namely  $\delta_1$  and  $\delta_2$ . That is what motivates their within-pair approach.

Operationally, the distinctive feature of  $h$  is that its value is assumed to be common to the twins in an MZ pair. For concreteness we focus on female MZ pairs. Differencing (1) within each twin pair gives

$$\Delta S_c = \delta_1 \Delta S_m + \delta_2 \Delta S_f + \gamma_2 \Delta h_f + \Delta \epsilon_c, \quad (2)$$

because the identical-twin mothers had identical endowments. The within-pair regression of  $\Delta S_c$  on  $\Delta S_m$  omits the paternal variables  $\Delta S_f$  and  $\Delta h_f$  which, according to B&R, are correlated with  $\Delta S_m$  as a result of assortative marriage. Consequently, B&R first extend the within-pair analysis by including  $\Delta S_f$  in a multiple regression. Our Table 3 reports the multiple regression slopes for each of the samples used in Table 1. Row 1 coincides closely with the corresponding results in their Tables 4 and 5; we have no explanation for the minor discrepancies.

That still leaves  $\Delta h_f$  omitted. They attempt to control for  $\Delta h_f$  by tapping the fathers’ earnings data. For B&R, observed spousal earnings are not directly a measure of financial resources, but merely an indicator of unobserved spousal endowment. In their extended within-pair analysis, reported in later columns of their Tables 4 and 5, they try several variants: one is log earnings itself, another is log earnings residualized on schooling and experience. As it happens, those refinements do not substantially alter the pattern they found in simple regression: in their female MZ sample, the

coefficient on mother’s schooling ranges only from -0.274 to -0.199; in their male MZ sample, the coefficient on father’s schooling ranges only from 0.356 to 0.340. Hence nothing much is lost by our decision not to re-estimate their elaborate model employing our versions of the samples and coding.

Still, some explication of their model is needed so that we can address policy issues.

We are told (in (1) above) how parental characteristics affect the child’s schooling, but not how they affect the child’s endowment. B&R do not explicitly specify the transmission of the endowment – “heritable endowment” after all – from parents to children. Consequently, there is only a heuristic rationale for the correlation between an individual’s schooling and his or her endowment, although that is the correlation that motivates the use of twin data in the first place.

Evidently, what underlies the reduced-form schooling equation (1) is a structural schooling equation in which the child’s own endowment  $h_c$  enters:

$$S_c = \delta_1 S_m + \delta_2 S_f + \gamma_1^* h_m + \gamma_2^* h_f + \gamma_3^* h_c + \epsilon_c^*, \quad (3)$$

and a structural inheritance equation

$$h_c = \alpha_1 h_m + \alpha_2 h_f + w_c. \quad (4)$$

Substituting (4) into (3) and collecting terms gives (1) with

$$\gamma_1 = \gamma_1^* + \gamma_3^* \alpha_1$$

$$\gamma_2 = \gamma_2^* + \gamma_3^* \alpha_2$$

$$\epsilon_c = \epsilon_c^* + \gamma_3^* w_c$$

As B&R put it (p. 324), “The [unstarred gamma] coefficients ... reflect parental income and time allocation effects on child outcomes, but reflect endowment heritability as well”.

Notice that parental earnings themselves do not explicitly affect child’s schooling; only the parental endowments do. Thus the customary paths from parental endowments through parental earnings to child’s schooling have been suppressed with a casual reference to “parental income and time allocation effects”. Similarly, in the assortative marriage equations, spousal endowments—not earnings—are matched; a woman can observe her fiance’s endowment but not his future earnings.

## 5 Policy

With this background, we turn to evaluate the policy analysis of Behrman and Rosenzweig. They write, “The challenge is to obtain an estimate of the intergenerational effects from increasing the *overall* level of women’s schooling, which would leave existing distributions of abilities and marital matches essentially unchanged”. Alternatively put (p. 325), “the policy question [is] how the schooling of children would change if the schooling of all women were increased, for the same distribution of available spouses”.

This is their argument for taking  $\delta_1$  to be a parameter of interest, and for their subtle estimation approach. For B&R, the relevant intervention consists of increasing  $S_m$  for all women, while holding mother’s endowment  $h_m$  constant, along with father’s characteristics  $S_f$  and  $h_f$ , and child’s endowment  $h_c$  as well – “existing distributions of abilities and marital matches essentially unchanged” (p. 323). If that is the intervention, then  $\delta_1$  is the effect, and a within-pair approach to estimation is sensible.

B&R offer no concrete example of what such an intervention would consist of, and their formal specification of the intervention strikes us as gratuitous. Why would child endowments be immutable in the face of an overall change in educational conditions? Why would investment in children not change? Would there not be a change in the child disturbances  $\epsilon_c^*$  and  $w_c$ ? Even if the endowment is thought to be exclusively genetic, the endowment is not immutable: in economic models, the genetic endowment is not the DNA package, but the market valuation of that DNA package. And market valuations are pliable: a legislative increase in the minimum wage, or increase in the payroll tax, could change one’s endowment. Similarly we would expect that a subsidization of women’s schooling would change the market’s valuation of a woman even if her schooling were unchanged. In this manner, the endowment of her children would change – surely that of her daughters!

However, even apart from these general equilibrium considerations, the within-twin estimator that B&R employ is inappropriate for evaluating the intergenerational effects of increasing women’s schooling. To see this, note that increasing a woman’s schooling will presumably affect the (unobservable) endowment of her offspring. Thus, a policy like the one B&R seem to have in mind would ultimately affect the endowments of women who become mothers (because *their* mothers are also now more educated), which, in turn, will affect the schooling level of children. However, B&R’s method-



ology is incapable of capturing this indirect effect because maternal endowments drop out of the first-differenced equations. This point calls into question the validity of using within-twin estimates to make predictions about the intergenerational implications of *any* policy that affects unobservable endowments.

Once a more realistic formulation of the intervention is adopted, the rationale for taking  $\delta_1$  as the parameter of interest vanishes, and with it the rationale for within-pair regressions. Our argument against within-pair regressions here is reminiscent of that made by previous authors in various contexts: see Appendix A.

To be clear, we do not wish to imply that the within-twin estimates are *worse* than the cross-sectional estimates; we merely wish to point out that the within-twin estimates may not, and probably do not, yield the policy parameters of interest.

## 6 Conclusion

Behrman and Rosenzweig begin their article with the remark that “Widely held conventional wisdom is that an important return to investments in women’s schooling is manifested in the increased schooling of the next generation” (p. 323), but obtain the conclusion that “[O]ur results indicate that an increase in the schooling of women would not have beneficial effects in terms of the schooling of children” (p. 333). We have seen that the results are not robust and that the policy inference may be misguided. Their unconventional conclusion must have surprised the authors as well. They offer an explanation (pp. 332-333): More educated mothers will spend more time in the labor market, hence less time at home with their children, thus exerting a negative influence on the educational attainment of their children. In that sense, their answer to the question raised in the title of our paper, namely, “Do educated women make bad mothers?” would be “yes”. They do concede that other outcomes for children, such as health, may be influenced positively, and further that their result may be context dependent (p. 334). They offer rural India in recent decades as a context in which their results would not apply. We are left wondering just which contexts and policy interventions would evoke the same forces that led Minnesotan identical twins born 1936-1955 to acquire non-identical years of schooling.

Table 3: Slopes in MULTIPLE regressions of kid's schooling on both parents' schooling.

Rows: R=Respondent, S=Spouse

Database	Coding	n	FEMALE MZ PAIRS				MALE MZ PAIRS			
				cross	within	n		cross	within	
BROAD										
1) BR	BR	212	R	.137	-.274	122	R	.327	.344	
			S	.288	.133		S	.249	.043	
2) WS	AG	251	R	.260	.040	171	R	.197	.104	
			S	.213	.069		S	.258	.176	
3) BR/WS	BR	202	R	.128	-.207	120	R	.324	.347	
			S	.297	.157		S	.243	.031	
4) BR/WS	AG	202	R	.142	-.041	120	R	.271	.313	
			S	.282	.149		S	.330	.223	
NARROW										
5) BR	BR	152	R	.162	-.287	76	R	.367	.359	
			S	.264	.159		S	.116	-.096	
6) WS	AG	178	R	.248	.044	102	R	.223	.085	
			S	.197	.078		S	.215	.130	
7) BR/WS	BR	148	R	.157	-.241	75	R	.370	.364	
			S	.271	.161		S	.105	-.117	
8) BR/WS	AG	148	R	.181	.097	75	R	.261	.335	
			S	.236	.152		S	.243 <sup>†</sup>	.085	
VERY NARROW										
9) BR	BR	80	R	-.023	-.432	40	R	.504	.392	
			S	.388	.358		S	.160	.250	
10) WS	AG	90	R	.098	-.003	47	R	.503	.477 <sup>†</sup>	
			S	.205	.072		S	-.024	.009 <sup>†</sup>	

Standard errors omitted here. n = number of twin pairs.

Vocational education is counted in schooling. Runs with vocational education excluded in AG coding showed some differences from those given here: at most .065 absolute difference in the slope, except for the ones marked above with a <sup>†</sup>, which went from .243 to .175, .477 to .382 and .009 to .098.

In line 1, there are some slight differences, none more than .005, between several of our slopes and those given by Behrman and Rosenzweig Tables 4 and 5.

## A Quotations

Bound & Solon (1999), studying the income-education relation, write p. 174, “[T]he twins-based literature has been motivated by the hope that, although conventional cross-sectional estimation of the return to schooling is rendered inconsistent by the endogeneity of schooling, the covariance estimator [i.e. within-twin-pair regression] is not. The hope rests on the very strong assumption that any within-family variation in the endogenous schooling factor  $h_{ij}$  is purely genetic, so that any schooling differences between monozygotic twins are as exogenous as if they were determined by coin flips. Why would anyone be convinced that this is so? Maybe wishful thinking has played a role, but perhaps it has seemed plausible to some that, since monozygotic twins are identical, any variation in their schooling must be purely random. But, if monozygotic twins are perfectly identical, why do they ever display any schooling difference at all?”

Kuh (1963), studying capital formation, works with a panel of firms over years, with a cross-section regression for each year, and a time series regression for each firm. He writes pp. 182-183, “[C]ross-sections typically will reflect long-run adjustments whereas annual time series will tend to reflect shorter-run reactions. Because disequilibria among firms tend to be synchronized in response to mutual market forces and the business cycle, many disequilibrium effects wash out (or appear in the regression intercept) so that the higher cross-section slope estimates can be interpreted as predominantly reflecting long-run equilibrium behavior.”

Durlauf & Quah (1999), studying GDP growth rates, work with a panel of countries over years. They write p. 286, “[I]n the so-called ‘fixed-effects’ or within estimator, one takes deviations from time-averaged sample means ... and then applies OLS to the transformed equation to provide consistent estimates for the regression coefficients. But note that in applying such an individual-effects annihilating transformation, the researcher ends up analyzing a left-hand side variable purged of its long-run (time-averaged) variation across countries. Such a method, therefore, leaves unexplained exactly the long-run cross-country growth variation originally motivating this empirical research.”

## References

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