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The Causal Effect of Parents' Schooling on Children's Schooling: A Comparison of Estimation Methods

Helena Holmlund, Mikael Lindahl, and Erik Plug*

We review the empirical literature that estimates the causal effect of parent's schooling on child's schooling, and conclude that estimates differ across studies. We then consider three explanations for why this is: (a) idiosyncratic differences in data sets, (b) differences in remaining biases between different identification strategies, and (c) differences across identification strategies in their ability to make out-of-sample predictions. We conclude that discrepancies in past studies can be explained by violations of identifying assumptions. Our reading of past evidence, together with an application to Swedish register data, suggests that intergenerational schooling associations are largely driven by selection. Parental schooling constitutes a large part of the parental nurture effect, but as a whole does not play a large role. (*JEL* I21, J13)

1. *Introduction*

It is widely known that more educated parents get more educated children. For example, in a literature review published in the *Journal of Economic Literature*, Robert Haveman and Barbara Wolfe (1995) conclude that the education of parents is probably the most fundamental factor in explaining the child's success in school. A natural question to raise then is why this is. Is it because more able parents have more able children? Or is it because more educated parents have more resources—caused by their higher education—to provide a better environment for their children to do well in school? It is only recently that empirical studies have begun to focus on establishing a causal relationship between the education of parents and their children. The growing number of papers in this spirit makes a clear distinction

* Holmlund: Swedish Institute for Social Research, Stockholm University, and Centre for Economic Performance, London School of Economics. Lindahl: Uppsala University, IFAU, and UCLS. Plug: University of Amsterdam and TIER. The authors wish to thank Roger Gordon, three reviewers, Adrian Adermon, Anders Björklund, Sandra Black, Alan Krueger, Gary Solon, Marianne Sundström, Björn Öckert, and numerous seminar and conference participants for their valuable comments. Thanks also to Valter Hultén for help with the coding of the reform. Financial support from Jan Wallander's and Tom Hedelius's foundation, The Swedish Council for Working Life and Social Research (FAS), Berch and Borgström's foundation, the Scientific Council of Sweden, the Dutch National Science Foundation [NWO-VIDI45203309], and the European Research Council [ERC starting grant 241161] is gratefully acknowledged. Lindahl is a Royal Swedish Academy of Sciences Research Fellow supported by a grant from the Torsten and Ragnar Söderberg Foundation.

between *intergenerational associations* and *intergenerational causal effects*, and therefore strongly relates to one of the oldest questions in the social sciences: is it nature, is it nurture, or a combination of the two, that explains individual outcomes such as educational achievement and labor market success?

The causal intergenerational schooling effect can also help us better understand the production function of nurture. The key nurture question is what are the responsible technologies and inputs in the womb and postnatal years that affect child outcomes? Even though nurture itself is a rather abstract concept, with many potential components that are difficult to manipulate, we believe that parental education is one of the most promising inputs to consider. Education is malleable and, if Haveman and Wolfe are correct, important.

It is also of policy interest to deepen our understanding of the nurture production function. For example, policymakers would want to take into account externalities, such as spillover effects on the next generation, when considering new education policies. If parental schooling is largely responsible for creating an environment where children can learn and prosper, increasing the schooling of one generation will have long-term consequences; the educational achievement of future generations would then improve as well, and inequality in educational opportunity may be reduced. If, on the other hand, it is children's inherited ability that is responsible for their success in school, an improved school environment may help the less able children to overcome their disadvantages. However, these improvements are only short-lived and probably come at greater costs; educational expenses are repeatedly made across generations since the ability of future generations remains unequally distributed.

Returning to our initial question: why do more educated parents get more educated children? Drawing on the existing literature, we conclude that we do not know for two reasons. First, there is not much evidence available. And second, the empirical studies on the intergenerational causal effects of education that are around tend to reach conflicting conclusions. To understand the nurture production function, and why more educated parents have more educated children, it is important to learn more about the origins of these conflicting results.

The recent literature with its focus on causal effects has moved away from estimating cross-sectional ordinary least squares (OLS) regressions on samples of children and their biological parents, and proposes alternative identification strategies. Three strategies that are currently in use rely on identical twins, adoptees, and instrumental variables (IV). In case of the IV strategy, educational reforms have commonly been used as instruments for education. If different strategies lead to different results, we may wonder whether this is due to the use of different identification strategies, each of which may or may not violate assumptions regarding both internal and external validity, or to different data sources, gathered in different countries at different times.

The aim of this review is to bring together the recent advancements in the literature on causal intergenerational schooling effects and to clarify and understand the discrepancies in the existing studies. To this end, we propose a simple procedure, the first of its kind, in which all the available identification strategies are applied to one particular data set. Using register data from Sweden, we are able to apply all three identification strategies to one data set, thus using the same country and institutional context, and the same cohorts, in all three methods. With the results from this replication exercise, we move on to discuss in detail issues relating to internal and external validity in order to deepen our understanding of the intergenerational process and better understand the advantages and pitfalls of the estimation procedures that are in use.

This paper continues as follows. Section 2 briefly surveys the empirical work done since the review paper of Haveman and Wolfe, and presents studies that focus on causation and not association. In the remaining sections, we discuss why we may see differences in results across identification strategies. We replicate, present, and compare our parameter estimates to those estimates reported in previous studies in section 3. Section 4 addresses aspects of internal validity and how this may give rise to different results across methods, and section 5 continues to discuss external validity. Section 6 concludes.

2. *A Review of Recent Empirical Studies*

Recent years have seen an upsurge of intergenerational effects studies that contrast with earlier efforts and make a distinction between causation and association. Table 1 summarizes the studies that estimate intergenerational schooling effects and attempt to control for the role of unobserved endowments.

The studies we refer to have been based on three different identification techniques: twins, adoptees, and IV. Identification in the twins approach comes from differences in education within pairs of identical twins; the difference in twin parents' education is used to identify effects on their children. The adoption strategy relies on the idea that genetic transmission between adoptive parents and adopted children is absent. And finally, the studies adopting IV take advantage of education reforms where—in our case—changes in compulsory schooling laws are used to instrument for parental education.[1]

Besides variation among identification methods, another complication in comparing the findings of different intergenerational studies is the variation in estimation techniques, model and variable specifications, and the choice of control variables in the model. With respect to estimation techniques and model and variable specifications, the studies we focus on show little variation. Almost all studies use least squares and regress school outcomes of children on the same school outcomes of parents, mostly measured by the number of years of schooling attained. With respect to the choice of control variables in the model, however, there appears to be less overlap. In particular, there is variation among the studies in whether or not to include a control variable for spousal education. We briefly list the main arguments in favor of and against including this control.

It is not a priori clear whether one should include spousal education as an additional explanatory variable. Without the inclusion of the partner's schooling, the effect of parental schooling as it is estimated

[1] Apart from studies based on twins, adoptees, and IV for identification, parallel research has taken on a different approach, of a more structural or econometric kind, to estimate intergenerational causal schooling effects. These studies all need to make assumptions about the structure of the error term in the intergenerational schooling equation in order to arrive at causal effects. This is in stark contrast to the studies based on twins, adoptees, and IV, which all obtain identification by adding more information about the error term. For this reason, we concentrate our review only on papers using twins, adoptees, or IV. A few representatives of the above mentioned literature are worth mentioning however: Christian Belzil and Jörgen Hansen (2003) apply a structural dynamic programming model and find that parental background explains relatively more of children's educational attainment than ability, under the assumption that unobserved ability is orthogonal to family background. Monique De Haan (forthcoming) applies a nonparametric bounds analysis, assumes that maternal skills and schooling affect children's schooling in similar directions, and concludes that the causal estimates are larger than zero, but lower than the OLS estimates. Lídia Farré, Roger Klein, and Francis Vella (2009) propose a conditional moment approach to estimate causal effects, where they make assumptions regarding the intergenerational transmission of unobserved ability, and about heteroskedacticity of the error term, in order to obtain identification. They also find that the causal effect is smaller than the OLS estimate. Further, Valentino Dardanoni, Antonio Forcina, and Salvatore Modica (2009) estimate secondary intergenerational schooling effects, that is, the transfer of schooling given the child's ability, using a finite mixture model. They find positive secondary effects for transmission of schooling from mother to daughter, and from father to son.

TABLE 1
Causal Estimates of Intergenerational Effects of Schooling—Summary of Previous Literature

Author	Sample characteristics[a]	Child's outcome	Controls for assortative mating	Intergenerational associations[b]			Intergenerational effects[b]		
				OLS estimates			Difference estimates		
	A. Twin studies			Father (1)	Mother (2)	Father +Mother (3)	Father (4)	Mother (5)	Father +Mother (6)
Behrman and Rosenzweig (2002)	MTR: 244 twin fathers and 424 twin mothers; average birth year parent 1947; average birth year child 1971.	Years of schooling	(no)	0.47 (0.05)**	0.33 (0.05)**	—	0.36 (0.16)**	–0.25 (0.15)+	—
			(yes)[c]	0.33 (0.07)**	0.14 (0.05)**	—	0.34 (0.16)**	–0.27 (0.15)+	—
Antonovics and Goldberger (2005)	MTR: 92 twin fathers and 180 twin mothers; subsample from Behrman and Rosenzweig (2002).	Years of schooling	(no)	0.49 (0.09)**	0.28 (0.09)**	—	0.48 (0.16)**	0.03 (0.27)	—
			(yes)	0.50 (NA)	0.10 (NA)	—	0.48 (NA)	–0.00 (NA)	—
Bingley, Christensen, and Jensen (2009)	DAR: 2,713 twin fathers and 2,975 twin mothers, children born 1956–1979, children aged 25 or older in 2004.	Years of schooling	(no)	0.18 (0.01)**	0.18 (0.01)**	—	0.08 (0.03)**	0.05 (0.03)	—
			(yes)	0.12 (0.01)**	0.14 (0.01)**	—	0.07 (0.03)**	0.03 (0.02)	—
Pronzato (forthcoming)	NAR: 1,606 twin fathers and 1,609 twin mothers, children aged 23 or older in 2001.	Years of schooling	(yes)	0.21 (0.02)**	0.24 (0.02)**	—	0.16 (0.03)**	0.10 (0.04)*	—
Haegeland et al. (2010)	NAR: 1,375 twin fathers and 1,571 twin mothers, 16-year-old children born 1985–91.	Exam marks (Standardized)	(yes)	—	—	—	0.04 (0.02)	–0.01 (0.02)	—

(*continued*)

represents both the direct transfer from the given parent and the indirect transfer from the other parent, which is due to assortative mating effects. With the inclusion of the partner's schooling, the estimated transmission effects arguably measure the effect of an increase in one parent's schooling on the schooling of his or her child, net of assortative mating effects. The interpretation of the schooling coefficients for fathers and mothers separately, however, remains complicated because of the strong collinearity

TABLE 1
Causal Estimates of Intergenerational Effects of Schooling—Summary of Previous Literature
(*continued*)

				OLS estimates using own-birth children			OLS estimates using adopted children		
	B. Adoption studies			(1)	(2)	(3)	(4)	(5)	(6)
Dearden, Machin, and Reed (1997)	NCDS: 4,030 own birth children and 41 adopted children. Birth year child: 1958.	Years of schooling	(no)	0.42 (0.02)**	—	—	0.36 (0.12)**	—	—
Sacerdote (2000)	NLSY: 5,614 own birth and 170 adopted children. Average birth year child: 1961.	Years of schooling	(no)	0.28 (0.01)**	0.35 0.01**	—	0.16 (0.04)**	0.22 (0.06)**	—
			(yes)	—	—	—	0.11[e] (0.04)*	0.11 (0.07)	—
Plug (2004)	WLS: 15,871 own birth and 610 adopted children. Birth year mother: 1940, average birth year adopted and birth child: 1969 and 1965.	Years of schooling	(no)	0.39 (0.01)**	0.54 (0.02)**	—	0.27 (0.04)**	0.28 (0.10)**	—
			(yes)	0.30 (0.01)**	0.30 (0.02)**	0.30[e] (0.01)**	0.23 (0.04)**	0.10 (0.08)	0.19[e] (0.03)**
Sacerdote (2007)	HICS: 1,051 own birth and 1,256 adopted children from Korea. Average birth year adopted and birth child, 1975 and 1969.	Years of schooling	(no)	—	0.32 (0.04)**	—		0.09 (0.03)**	—
Björklund, Lindahl, and Plug (2004)	SAR: 148,496 own birth and 7,498 adopted children all born in Sweden; average birth year adoptive parent 1934; average birth year child 1966.	Years of schooling	(no)	0.23 (0.00)**	0.24 (0.00)**	—	0.13 (0.01)**	0.11 (0.01)**	—
			(yes)	0.16 (0.00)**	0.16 (0.00)**	—	0.10 (0.01)**	0.06 (0.01)**	—
Björklund, Lindahl, and Plug (2006)	SAR: 94,079 own birth and 2,125 adopted children all born in Sweden; average birth year parent 1932; average birth year child 1964.	Years of schooling	(no)	0.24 (0.00)**	0.24 (0.00)**		0.11 (0.01)**	0.07 (0.01)**	
			(yes)	0.17 (0.00)**	0.16 (0.00)**	0.16[e] (0.00)**	0.09 (0.01)**	0.02 (0.01)	0.06[e] (0.01)**
Haegeland et al. (2010)	NAR: 271,452 own-birth and 558 adopted 16-year-old Korean children born 1985–91.	Exam marks (Standardized)	(no)	0.10 (0.00)**	0.10 (0.00)**	—	0.04 (0.01)**	0.03 (0.01)**	—
			(yes)	0.07 (0.00)**	0.07 (0.00)**	—	0.02 (0.01)	0.02 (0.01)*	—

(*continued*)

TABLE 1
CAUSAL ESTIMATES OF INTERGENERATIONAL EFFECTS OF SCHOOLING—SUMMARY OF PREVIOUS LITERATURE
(*continued*)

				OLS estimates			IV estimates		
	C. IV studies			(1)	(2)	(3)	(4)	(5)	(6)
Black, Devereux, and Salvanes (2005)	NAR: 239,854 and 172,671 children born 1965–75; birth year parent: 1947–58; instrument MSLA reform in 1960–1972.	Years of schooling	(no)	0.22 (0.00)**	0.24 (0.00)**	—	0.03 (0.13)	0.08 (0.14)	—
			(no)[d]	0.21 (0.02)**	0.21 (0.02)**	—	0.04 (0.06)	0.12 (0.04)**	—
Chevalier (2004)	BFRS: 12,593 children aged 16–18; birth year parent: 1938–67; instrument MSLA reform in 1972.	Post-compuls. school attend.	(yes)	0.04[f] (0.00)**	0.04[f] (0.00)**	—	–0.01[f] (0.06)**	0.11[f] (0.04)**	—
Oreopoulos, Page, and Stevens (2006)	IPUMS: 711,072 children aged 7–15; average birth year father and child: 1920–40 and 1950–70; instrument: MSLA reforms between 1915–70.	Grade repetition (actual-normal)	(no)	—	—	–0.01 (0.00)**	–0.06 (0.01)**	–0.05 (0.01)**	–0.03 (0.00)**
			(no)[d]	—	—	–0.02 (0.00)**	—	—	–0.04 (0.01)**
Maurin and McNally (2008)	FLFS: 5,087 children aged 15 in 1990–2001; birth year father 1946–52; instrument: university reform in 1968.	Grade repetition (actual-normal)	(no)	–0.08 (0.00)**	—	—	–0.33 (0.12)**	—	—
Carneiro, Meghir, and Parey (forthcoming)	NLSY: 2,512 white children aged 12–14; instruments: local tuition fees, unemployment rates and wages.	Grade repetition (actual-normal)	(no)	—	–0.01 (0.00)**	—	—	–0.02 (0.01)*	—

Notes: [a] Abbreviations: MTR—Minnesota Twin Registry 1994; DAR—Danish Administrative Records, SAR—Swedish Administrative Records; NCDS—National Child Development Survey 1991; NLSY—National Longitudinal Survey of Youth 1979; WLS—Wisconsin Longitudinal Study 1992; HICS—Holt International Children's Service; NAR—Norweigan Administrative Records; BFRS—British Family Resources Survey 1994–2002; IPUMS—Integrated Public Microdata Series 1960–1980; FLFS—French Labor Force Survey 1990–2001; MSLA—Minimum School Leaving Age.
[b] Standard errors in parentheses; ** significant at 1 percent level; * significant at 5 percent level; + significant at 10 percent level. Each coefficient is from a separate regression of the child's outcome on parent's years of schooling. Most regressions include individual controls for the child's age and gender and parent's age.
[c] These coefficients come from regressions that include the years of schooling of both parents simultaneously. Resulting estimates take into account the intergenerational effect of the marriage partner.
[d] These coefficients come from a restricted sample of parents with less than ten (twelve) years of schooling in Norway (the United States).
[e] For the purpose of this paper, we have run these specifications, which were not included in the original papers.
[f] These coefficients come from probit regressions.

between the parents' schooling. Also, in IV settings, the strong correlation between the compulsory schooling reform instruments for mothers and fathers may hinder the interpretation of IV results. If the separate reform indicators are simultaneously used as instruments, the IV estimates will likely come with larger standard errors. As an alternative, Philip Oreopoulos, Marianne E. Page, and Ann Huff Stevens (2006) propose to use the sum of mother's and father's schooling as the endogenous regressor of interest. The coefficient of interest will then represent the effect on child's schooling of a one-year increase in either parent's years of schooling. If the effects of mother's and father's schooling do not differ from each other, this is the same regression model where both parents' schooling are included simultaneously. Their restricted model has the advantage that it controls for assortative mating, avoids multicolinearity, and produces more precisely estimated coefficients.

Of the three specifications, the preferred specification depends, we think, on the (policy) question that is raised. If, for example, we are interested in the schooling of the children, we should not care whether parental schooling effects run through assortative mating or something else, and we can estimate separate regressions for mothers and fathers, without controlling for the spouses' schooling. On the other hand, if we are interested in the consequences of raising the schooling of mothers but not fathers, we must quantify assortative mating effects and include the schooling of both parents simultaneously. If we also restrict the coefficients on mother's and father's schooling to be the same, we gain statistically speaking by increasing precision, but may find the estimated effect to be less informative, since gender-specific policy effects cannot be discerned. In some contexts, such effects are highly relevant—for example, in some developing countries there are gender-specific programs that aim to raise the schooling of girls but not boys (T. Paul Schultz 2002; Jere R. Behrman and Mark R. Rosenzweig 2005).

In table 1, we tabulate the main characteristics of data sources, identification strategies, relevant model and variable specifications, and the corresponding intergenerational estimates of the studies in this literature. In particular, the table is organized to present six estimates that aim to measure the effect of the parent's education on that of her child. We begin with two intergenerational associations for fathers and mothers that ignore the correlation of educational attainment with unmeasured ability (columns 1 and 2). We present the corresponding intergenerational effect estimates that intend to control for ability transmissions in columns 4 and 5. Some of the studies also aimed to control for assortative mating effects by including both mothers' and fathers' education simultaneously as regressors; if so, we include those intergenerational effect estimates as well. To complete the summary of the previous literature, columns 3 and 6 also present estimates of the sum of mother's and father's education, the former estimate disregarding ability transmissions across generations, while the latter takes those into account. Note that these estimates are only available in one study and, where possible, we have completed table 1 with previously unpublished estimates.

In the first three columns, we present estimates that come from simple OLS regressions of the schooling of child i in family j (S^c_{ij}) on parent's schooling (S^p_{ij}) of the form

$$(1) \qquad S^c_{ij} = \delta_0 + \delta_1 S^p_{ij} + v^c_{ij}.$$

There are a number of features these cross-sectional estimates share. All the estimates indicate that higher parental education is associated with more years of schooling of own children and that, in most cases, the influence of the mother's schooling is somewhat larger than that of the father. The results are, as such, fully in line with those findings summarized in Haveman and Wolfe (1995). Second, those studies that control for assortative mating by including both parents' education simultaneously indicate that the partial effects of both parents' schooling fall, yet always remain positive. It is interesting to see that the partial schooling effects of both parents are almost always identical, except for Behrman and Rosenzweig (2002) who find that the father's schooling is the most important.

In the last three columns, we shift our attention to intergenerational causal effects. We begin with the within-twin estimates. The twins approach exploits the idea that unobserved differences that would bias the least squares parameter are removed within twins. These studies therefore regress the difference in schooling between the children of twin parents (ΔS_j^c) on the difference in schooling between the twin parents (ΔS_j^p):

$$\Delta S_j^c = \delta_{1TW} \Delta S_j^p + \Delta v_j^c. \tag{2}$$

Based on monozygotic (MZ) twin parents from Minnesota, identical in their endowments including inborn abilities and shared environment but different in their educational attainment, Behrman and Rosenzweig (2002) find that the mother's education has little, if any, negative impact on the education of her child. Once they look at twin fathers and difference out endowments that influence their children's education, the influence of father's education remains positive and statistically significant. Kate L. Antonovics and Arthur S. Goldberger (2005) challenge these results, and test the robustness of Behrman and Rosenzweig's findings to alternative school codings and sample selections. Yet with the twin sample restricted to twins with children 18 years or older, all having finished school, they also produce positive schooling effects for fathers and no (or much smaller) effects for mothers. In fact, in most of their alternative samples using various parental schooling measures, within-twin estimates of maternal schooling effects are lower than those for fathers, which are always positive.

More recently, twin studies have also been carried out on Scandinavian register data. Paul Bingley, Kaare Christensen, and Vibeke Myrup Jensen (2009) find, using Danish MZ twins, smaller paternal effects than those based on the Minnesota twins and an insignificant effect of mother's education. While the results from their pooled sample replicate the pattern from the Minnesota studies, with positive paternal effects and no effects of mother's education, their study also points at trends in the twin-difference estimates: maternal effects are becoming larger and become statistically significant in the more recent cohorts, whereas paternal effects are decreasing and become less pronounced for younger cohorts.

Similarly, estimates based on a mixture of dizygotic (DZ) and MZ twins from Norway also indicate positive effects of mother's education, albeit smaller than the effects of father's education (Chiara Pronzato forthcoming). These estimates are in general larger than those obtained for Danish MZ twins, most likely reflecting that while MZ twins share all their genes, DZ twins share only about 50 percent of their genes, which means that the "twin difference" only nets out part of the effect of inherited ability. Our conclusion from summarizing the twin literature is, therefore, that the mother's schooling has little impact on the schooling of her child, holding everything else (including unobserved ability factors of either mother or father) constant. That said, recent results indicate that the intergenerational causal effects may change over time, as women are attaining higher levels of education.

The next strategy to account for genetic effects is to use data on adopted children. These studies regress the schooling of adopted children (S_{ij}^{ac}) on schooling of the adoptive parent (S_{ij}^{ap}):

$$(3) \qquad S_{ij}^{ac} = \delta_{0AD} + \delta_{1AD} S_{ij}^{ap} + v_{ij}^{ac}.$$

If adopted children share only their parents' environment and not their parents' genes, any relation between the schooling of adoptees and their adoptive parents is driven by the influence parents have on their children's environment and not by parents passing on their genes. In the economics literature, a series of recent papers (Lorraine Dearden, Stephen J. Machin, and Howard Reed 1997; Bruce Sacerdote 2000, 2002, 2007; Erik Plug 2004; Anders Björklund, Mikael Lindahl, and Plug 2004, 2006) have begun to estimate intergenerational schooling effects on samples of parents and their adopted children. On relatively small samples, the studies of Dearden, Machin, and Reed (1997) and Sacerdote (2000) regress the adopted son's years of schooling on his adoptive father's years of schooling, and report positive and significant effects that are not much lower than the effects found for fathers and their own-birth sons. They therefore conclude that environmental factors are indeed important for intergenerational transmissions. The other studies that obtain identification from adopted children using much bigger samples find that the parental effect estimates fall somewhat for fathers, but much more so for mothers, when moving from samples of own-birth children to samples of adoptees.

One concern, however, is that in most adoption studies it is difficult to establish a causal relationship between the schooling of parent and child because of selective placements. If adoptions are related or if adoption agencies use information on the natural parents to place children in their adoptive families, the parental schooling estimates possibly pick up selection effects. Two adoption studies control for this matching correlation. Sacerdote (2007) uses information on Korean American adoptees who were randomly assigned to adoptive families. Björklund, Lindahl, and Plug (2006) use additional information on the adoptees' biological parents to control for the impact of selective placements. Sacerdote finds that the adoptive mother's education has an impact on the education of the children. Björklund, Lindahl, and Plug find both adoptive (as well as biological) parents' education to be important, even though the impact of the adoptive mother's education is very small when education of the spouse is controlled for. The conclusion from both studies is that parental education has an impact on the education of the children, even when selective placement is taken into account.

In sum, whether adoptees are raised in Wisconsin, other U.S. states, or Sweden, these studies always find positive and statistically significant schooling effects when mother's and father's schooling are included as separate regressors. Provided that these models are correctly specified, estimates on how much family genes contribute to the intergenerational schooling association range from 30 to 80 percent, but the majority of estimates are close to 50 percent. Note that these percentages are inclusive of education passed on via assortative mating. When these adoption studies control for assortative mating effects and include mother's and father's schooling simultaneously, they find that mother's schooling effect is not bigger but mostly smaller than that of her husband. The bulk of the evidence, thus, indicates that for the child's schooling, nurture is indeed an important factor.[2] Since these studies also lend some support to the notion that the nurturing contribution of father's schooling

[2] Sacerdote (2000, 2002) and Plug and Wim Vijverberg (2003) focus on nature/nurture decompositions and interpret the difference between own-birth and adoption effects to measure the relative importance of inherited abilities.

is somewhat bigger than that of his wife, these results are in this respect comparable to those obtained in previous twin studies. However, a difference is that adoption studies generally find positive effects of mother's education, at least when the control for spouse's education is omitted.

Among the adoption results in table 1, we also find previously unpublished estimates of the effect of raising either mother's or father's education (the regressor of interest is the sum of mother's and father's education), on the education of the child. Unsurprisingly, these estimates lie in between the two coefficients obtained for mothers and fathers, and importantly, using the sum of mother's and father's education increases precision.

Recent IV studies exploit reforms (often in the compulsory schooling legislation) to identify the effect of parent's schooling on that of their children. They typically estimate a two-stage least squares model where the first stage is

$$S_{ij}^{p} = \alpha_0 + \alpha_1 REF_{ij}^{p} + u_{ij}^{p} \tag{4}$$

and the second stage is

$$S_{ij}^{c} = \delta_{0IV} + \delta_{1IV} S_{ij}^{p} + v_{ij}^{c}. \tag{5}$$

In most studies discussed below, additional controls for region- (state, municipality) and cohort-indicators are also included, which often is the aggregated level at which the reform is implemented.

Sandra E. Black, Paul J. Devereux, and Kjell G. Salvanes (2005) use changes in compulsory schooling laws introduced in different Norwegian municipalities at different times during the 1960s and early 1970s. Compulsory schooling increased from seven to nine years, with the consequence that some parents experienced two extra years of schooling compared to other parents similar to them on any other point but their year and municipality of birth.[3] As such, the reform generates exogenous variation in parental schooling that is independent of endowments. Using the timing of the reform to instrument for parental schooling, Black, Devereux, and Salvanes (2008) produce estimates that are imprecise and statistically insignificant. When they restrict the sample to those parents with no more than nine years of education, assuming that the reform has little bite for those acquiring more than that, their precision increases. They then find no effect of father's schooling and a positive but small effect of mother's schooling (which is primarily driven by a relationship between young mothers and their sons). The larger variation in compulsory schooling reforms together with their sample-selection rule should enable Black, Devereux, and Salvanes (2008) to arrive at more precise estimates than comparable IV studies. Arnaud Chevalier (2004) also uses a change in the compulsory schooling law in Britain in 1957. He finds a large positive effect of mother's education on her child's education but no significant effect of paternal education. Note, however, that a limitation of his study is that the legislation was implemented nationwide; as a result, there is no cross-sectional variation in the British compulsory schooling reform.

If information on the children's years of schooling is not available because children are too young and still live with their parents, researchers often rely on intermediate schooling outcomes that are available, such

[3] Many empirical studies make use of comparable changes in the compulsory schooling legislation. In the United States, for example, school reform variation comes from fifty different states. In Norway, Black, Devereux, and Salvanes (2008) exploit a much larger source of municipality-variation. The Norwegian reform increased compulsory schooling from seven to nine years and was phased in across more than 700 municipalities between the years 1959 and 1973.

as test scores or grade repetition.[4] To date, there are only four studies that link the years of schooling of parents to these intermediate outcomes of children. One study follows a twin and adoption strategy and considers examination performance at the end of compulsory education in Norway (Torbjørn Hægeland et al. 2010). With cousins from same-sex twins and Korean adoptees, they find negligible effects of parental schooling on examination grades. The other three studies are IV studies (Oreopoulos, Page, and Stevens 2006; Pedro Carneiro, Costas Meghir, and Matthias Parey forthcoming; Eric Maurin and Sandra McNally 2008). We restrict our discussion to grade repetition, which is one of the outcomes these IV studies have in common. The study by Oreopoulos, Page, and Stevens (2006) uses U.S. compulsory schooling reforms, which occurred in different states at different times, and finds that the influences of the mother's and father's schooling on grade repetition are equally important. They also present the effect of the sum of mother's and father's education, that is, the effect on child's education if either the mother's or father's education increases by one year. By doing so, they increase precision and conclude that increasing either parent's education reduces the likelihood of grade repetition. Results do not change when they use a restricted sample of low-educated parents. These IV studies, and the one by Black, Devereux, and Salvanes (2008), obtain identification from compulsory schooling extensions and therefore estimate intergenerational effects among lower educated parents. One concern could be that parental schooling is transmitted differently, and perhaps more successfully, among higher educated parents. The two remaining studies, by Carneiro, Meghir, and Parey (forthcoming) and Maurin and McNally (2008), address this concern and consider grade repetition as outcomes but focus on variation in higher education. With instruments that are very different (county-by-year variation in tuition fees and college location in the United States versus year-by-year variation in the quality of entry exams in French universities), their results suggest that parental education matters in lowering repetition probabilities.

Most of the IV studies we refer to suffer from two weaknesses. First, the instruments used require identification assumptions/exclusion restrictions that may not hold in practice. Except for the compulsory schooling instruments in Norway and the United States, the instruments used are either statistically weak (tuition fees and college location) or depend too much on year by year variation, or do not distinguish instrument from cohort variation and are therefore less convincing (exam quality, U.K. school reforms). Second, it remains unclear how informative the intermediate outcomes are when it comes to assessing intergenerational schooling effects. If young children who repeat a grade are treated differently in ways related to their parents' schooling, it is possible that the corresponding intermediate schooling estimates will not capture the parental treatment effects that children receive beyond their compulsory schooling years, and therefore miss the true impact of parental schooling on child schooling.[5] With these weaknesses in mind, we are inclined

[4] Intermediate outcomes are often used to analyze child development. For an overview of the economic literature on child development and skill formation, see Flavio Cunha et al. (2006).

[5] Another alternative to intermediate outcomes is to use methods to correct for the censored observations. De Haan and Plug (2011) investigate the consequences of three different methods that deal with censored observations: maximum likelihood approach, replacement of observed with expected years of schooling, and elimination of all school-aged children. Of the three methods, the one that treats parental expectations as if they were realizations performs best.

to take the results of Black, Devereux, and Salvanes (2008) most seriously.

In sum, we think that all these twin, adoption, and IV findings suggest that schooling itself is in part responsible for the intergenerational schooling link: more educated parents get more educated children because of higher education. It is unclear, however, whether it is the schooling of the mother, the schooling of the father, or the schooling of both parents that is the decisive factor. The estimates in columns 4 and 5 of table 1 appear to be too diverse to establish one consistent pattern. Recent twin and adoption studies point to the father, whereas recent IV studies point to the mother as having the strongest impact.

At this point, we do not know where these differences come from. In the following sections, we will try to understand why results in the previous literature vary with the choice of identification strategy. We explore three possible mechanisms: (a) results differ because of different data sources gathered in different countries at different times, (b) results differ because imperfections in different identification strategies introduce different biases (internal validity violations), and (c) results differ because different identification strategies differ in their ability to make out-of-sample predictions (external validity violations). We will discuss each in turn, although there is no reason to believe that they are mutually exclusive. In fact, a combination of factors could lead to the results we have discussed so far.

3. *Replication Using Swedish Register Data*

Having established that previous studies based on different identification strategies have generated varying estimates of intergenerational schooling effects, we now investigate whether this is because these studies have used different samples from different countries and cohorts. The natural test is to apply all three strategies to one particular data set, thus holding country and cohorts constant. We are the first to do this. If the three strategies produce similar estimates for Sweden, we argue that different samples in earlier studies are responsible for the differences in results and that each method likely provides consistent estimates of intergenerational schooling effects. If, on the other hand, estimates still vary by identification, we have to look for alternative explanations, related to the methods per se.

3.1 *The Swedish Data Set*

We use a large data set compiled from several different Swedish registers, administered by Statistics Sweden. The data set is based on a 35 percent random sample of each cohort born in Sweden from 1943 to 1955. These are the cohorts exposed to the compulsory schooling reform. Through population registers and censuses, we identify and match the parents, partners, siblings, and children (both biological and adopted) to the sampled individuals. We restrict our estimation sample to those individuals who are married or cohabiting, have children, and live together (as registered in a census) when their children are between 6 and 10 years old.

The main variable years of schooling is created using information taken from either the education register or census. With detailed information on completed level of education, we construct years of schooling in the following way: 7 for (old) primary school, 9 for (new) compulsory schooling, 9.5 for (old) postprimary school (*realskola*), 11 for short high school, 12 for long high school, 14 for short university, 15.5 for long university, and 19 for a PhD university education. To avoid the problem that some children may still be in school at the time of data collection, we restrict the sample to those children that are at least 23 years of age in 2006. This is our baseline sample from which we cut the Swedish twin, adoption, and IV samples.

We construct the sample of twins by singling out those full biological siblings that are of the same sex, born in the same year and month. In the Swedish registers, it is not possible to separate MZ from DZ twins. However, by using only same-sex twins we know that about half of the twin sample will consist of MZ twins. Each twin must at least have one biological child. This gives us a sample of 5,050 same sex twins (2,110 fathers and 2,940 mothers), born 1943–55, with a total of 9,947 children born 1983 or earlier.

The adoption sample has been constructed using an adoption indicator available in the registers from Statistics Sweden, which tells us whether the child is registered as being adopted. We select foreign-born and Swedish-born adoptees who were adopted by two parents born in Sweden. We limit the sample of foreign-born adoptees to those who were adopted no later than at six months of age (to reduce the impact of the preadoption environment). We limit the sample of Swedish-born adoptees to those individuals with information on their biological mother's education and birth year (to account for the possible impact of selective placement). This gives us a sample of 5,389 adopted children, of which 496 are Swedish-born and 4,893 are born outside Sweden, adopted by 4,011 couples where at least one of the parents is born between 1943 and 1955.

The IV strategy uses variation introduced by the Swedish compulsory school reform, which was implemented gradually across the country's 1,037 municipalities beginning in 1949 and continuing through the 1950s to reach complete coverage in the early 1960s. The reform implied an extension of compulsory education by two years, from seven to nine, and a comprehensive and nonselective system up to age 16 replaced the previous early tracking regime. This reform allows us to compare individuals that were affected by the reform to similar individuals of the same cohort, living in a different municipality not affected by the reform in the same year, using a difference-in-difference setup. To implement the IV strategy, we need to limit the sample to those individuals for which we can observe whether they went through a reform school (minimum nine years of schooling) or a nonreform school (minimum seven years of schooling). In order to do this, we need to know the year of reform implementation for a school cohort in a municipality and the municipality-of-residence when the child was of school age. By dropping those individuals for which the necessary municipality information is not available, the sample decreases by 23 percent compared to the random sample. We end up with an IV sample of 466,697 children with at least one of the parents born 1943–55. Summary statistics on twin, adoption, and IV samples as well as on the representative baseline sample appear in table 2.

3.2 *Results*

We now regress years of education of the child on years of education of parents, applying the different estimation methods that we have described. The results are presented in table 3, which has a structure similar to that of table 1. For the estimations, we require the main parent to be born 1943–55, whereas the partner/spouse can be born any year. An exception is for the estimates where we have restricted both parents to have the same transmission. Here it is sufficient if either one of the parents is born 1943–55. All specifications include controls for the gender of the child and the parent's year of birth.[6]

[6] Some of the studies surveyed in section 2 control for the birth year of the child. Since such controls are potentially endogenous, we have decided to focus on models where we do not control for this variable. However, results remain very similar if we include this variable in the model.

TABLE 2
Descriptive Statistics, 1943–55 Cohorts

Means (standard deviations)

Variable	Random sample (1)	Twin sample All (2)	Twin sample Twin-pairs with different years of education (3)	Adoptee sample Swedish-born (4)	Adoptee sample Foreign-born (5)	IV sample All (6)	IV sample Reform (7)	IV sample No reform (8)
Father's schooling	11.25	10.99	11.28	11.48	12.41	11.11	11.63	10.90
	(2.80)	(2.82)	(2.66)	(2.93)	(2.89)	(2.82)	(2.31)	(2.98)
Mother's schooling	11.29	10.83	11.01	11.62	12.37	11.20	11.61	11.01
	(2.52)	(2.52)	(2.55)	(2.52)	(2.54)	(2.53)	(2.12)	(2.68)
Child's schooling	12.76	12.82	12.85	11.83	12.66	12.76	12.75	12.76
	(2.04)	(2.01)	(2.03)	(1.87)	(1.86)	(2.03)	(1.94)	(2.09)
Father's year of birth	1947.7	1947.4	1947.3	1946.2	1946.6	1947.8	1951.1	1946.5
	(3.40)	(3.26)	(3.21)	(2.80)	(2.78)	(3.44)	(2.67)	(2.75)
Mother's year of birth	1948.2	1947.8	1947.7	1946.5	1947.2	1948.3	1951.5	1946.8
	(3.54)	(3.40)	(3.34)	(3.03)	(3.01)	(3.58)	(2.67)	(2.86)
Child's year of birth	1974.4	1974.4	1974.3	1976.4	1978.5	1974.5	1977.3	1972.4
	(5.29)	(5.03)	(5.01)	(3.76)	(3.17)	(5.29)	(4.18)	(5.07)
Father's reform status = 1						0.29	1	0
						(0.45)		
Mother's reform status = 1						0.32	1	0
						(0.47)		
Number of observations	607,214	9,947	5,109	496	4,893	466,697	194,839	271,858

Notes: Summary statistics are for fathers born 1943–55 and for mothers born 1943–55, whereas spouses can be born in any year; Child's summary statistics are for the sample where one of the parents are born 1943–55. Number of observations refers to the number of children. Both parents have to be married or cohabiting and have at least one child born no later than 1983. The twin sample includes only same-sex twins who each have at least one child born no later than 1983. The IV sample includes all those in the random sample who at the age of 10–17 resided in a municipality that introduced the reform 1943 or later and where we have been able to code reform status.

TABLE 3
CAUSAL ESTIMATES OF INTERGENERATIONAL EFFECTS OF SCHOOLING—
RESULTS USING ONE DATA SET FOR SWEDEN

Sample characteristics (all samples are based on Swedish Registry data):	Child's outcome	Controls for assortative mating	Intergenerational associations[b]			Intergenerational effects[b]		
			Father (1)	Mother (2)	Father +Mother[g] (3)	Father (4)	Mother (5)	Father +Mother[g] (6)
Random sample			OLS estimates					
607,214 children; at least one of the parents is born 1943–55.	Years of schooling	(no)	0.23[b] (0.00)**	0.28 (0.00)**	—	—	—	—
		(yes)[c]	0.15 (0.00)**	0.19 (0.00)**	0.17 (0.00)** $p = 0.00$	—	—	—
		N	471,940	547,399	607,214			
A. Twins			OLS estimates			Difference estimates		
2,110 twin fathers and 2,940 twin mothers (where at least one is) born 1943–55 with 9,947 children.	Years of schooling	(no)	0.21 (0.01)**	0.25 (0.01)**	—	0.12 (0.03)**	0.06 (0.03)*	—
		(yes)	0.15 (0.01)**	0.17 (0.01)**	0.16 (0.01)** $p = 0.31$	0.11 (0.03)**	0.04 (0.03)	0.07 (0.02)** $p = 0.10$
		N	4,061	5,886	9,947	4,061	5,886	9,947
B. Adoption families			OLS estimates using own-birth children[h]			OLS estimates using adopted children		
160 own-birth children and 496 adopted children born in Sweden. At least one of the parents is born 1943–55.	Years of schooling	(no)	0.21 (0.09)*	0.28 (0.07)**	—	0.10 (0.03)**	0.11 (0.03)**	—
		(yes)	0.11 (0.09)	0.25 (0.08)**	0.13 (0.04)** $p = 0.47$	0.06 (0.04)+	0.05 (0.04)	0.08 (0.02)** $p = 0.20$
		N	106	155	160	325	470	496
912 own-birth children and 4,893 adopted children born abroad (foreign-born adoptees). At least one of the parents is born 1943–55.	Years of schooling	(no)	0.19 (0.03)**	0.22 (0.03)**	—	0.04 (0.01)**	0.04 (0.01)**	—
		(yes)	0.13 (0.04)**	0.15 (0.04)**	0.14 (0.02)** $p = 0.51$	0.04 (0.01)*	0.03 (0.01)**	0.03 (0.01)** $p = 0.35$
		N	665	861	912	3,793	4,690	4,893

(*continued*)

TABLE 3
Causal Estimates of Intergenerational Effects of Schooling—
Results using One Data Set for Sweden
(*continued*)

			Father (1)	Mother (2)	Father +Mother[g] (3)	Father (4)	Mother (5)	Father +Mother[g] (6)
C. IV using reform			OLS estimates			IV estimates[i]		
466,697 children; at least one of the parents is born 1943–55. Instrument: Compulsory schooling reform (Whether or not an individual attended a reform school)	Years of schooling	(no)	0.23 (0.00)**	0.28 (0.00)**	—	0.09 (0.04)* $F = 160$	0.11 (0.06)+ $F = 126$	—
		(yes)	0.15 (0.00)**	0.20 (0.00)**	0.17 (0.00)** $p = 0.00$	0.08 (0.04)* $F = 161$	0.09 (0.06) $F = 145$	0.07 (0.04)+ $p = 0.91$ $F = 100$
		N	365,461	419,381	466,697	365,461	419,381	466,697
		(no)[d]	0.13 (0.01)**	0.16 (0.01)**	—	0.02 (0.02) $F = 1115$	0.07 (0.03)** $F = 820$	—
		(yes)[d]	0.09 (0.01)**	0.13 (0.01)**	0.09 (0.01)** $p = 0.04$	0.01 (0.02) $F = 1098$	0.05 (0.04) $F = 857$	0.02 (0.03) $p = 0.51$ $F = 697$
		N	107,903	100,694	61,636	107,903	100,694	61,636

Notes: [b] Standard errors in parentheses; ** significant at 1 percent level; * significant at 5 percent level; + significant at 10 percent level. The standard errors are clustered on twin-pair level (panel A), parent level (panel B), and municipality level (panel C). Each coefficient is from a separate regression of the child's outcome on parent's years of schooling. All regressions include individual controls for the child's gender and parent's year-of-birth indicators. The samples are restricted so that the father (in columns 1 and 4), the mother (columns 2 and 5) and the father or the mother (columns 3 and 6) has to be born 1943–1955.

[c] These coefficients come from regressions that include the years of schooling of both parents simultaneously. Resulting estimates take into account the intergenerational effect of the marriage partner.

[d] These estimates are from a sample where parents have less than or equal to nine years of education.

[g] The reported p-values are from tests of equality of coefficients for fathers and mothers underlying the father+mother estimates in column 6. Note that the father+mother estimates in column 3 (and 6) are not exactly equal to the average of the separate estimates in columns 1 and 2 (and 4 and 5), since the sample restrictions are different.

[h] The sample is biological children raised by parents that also adopt. These are subsamples of the adopted parents used for estimations in columns 4–6.

[i] The F-statistics are from test of the reform impact on parent's years of schooling.

The specifications including spouse's education also control for spouse's year of birth. The parameter estimates for these spousal variables are not reported.

We first focus on estimates from cross-sectional OLS estimations using parents and children in the random sample (columns 1–3). We find that the estimate is 0.23 for father's education and 0.28 for mother's education. When we control for spouse's education, the estimates fall to 0.15 for fathers and 0.19 for mothers; this reduction is due to assortative mating. These results are very much in line with earlier results for Sweden (see for example Björklund, Lindahl, and Plug 2006). When we restrict the parents to having the same transmission coefficient, the estimate for parents' education is 0.17. This restriction, however, is rejected. A comparison between these baseline OLS estimates and the corresponding cross-sectional OLS estimates obtained with the twin, adoptee, and IV samples suggests that differences across samples are mostly small. Exceptions are the estimates based on own-birth children in families with foreign-born adoptees, and the restricted IV sample based only on low-educated parents.

In columns 4, 5, and 6, we turn to the causal effect estimates. In panel A, we apply the twins approach. The introduction of family-fixed effects reduces the schooling transmission coefficients, and much more so for mothers than for fathers. The fixed-effect coefficients for fathers and mothers are 0.12 and 0.06, respectively. Moving to the second row, where controls for education of the spouse are included, the schooling effects fall somewhat further to 0.11 for men and 0.04 for women. The estimate of mother's schooling, when controlling for spouse's schooling, is no longer statistically significant. These results indicate that mother's education is no more than half as important as father's education. In the last column, we stack the data sets of mothers and fathers and restrict their coefficients to be identical. We find an estimate of parents' education to be 0.07, which is precisely estimated. The restriction of equal parental coefficients is rejected, but only on the margin with a p-value of 0.10.

We should keep in mind that the twin estimates presented here are based on a twin sample containing both MZ and DZ twins, and that no correction has been made for possible measurement error in parental schooling. Not being able to isolate MZ twin pairs will likely lead to estimates that are too high, and not being able to control for measurement error will likely lead to estimates that are too low. In Helena Holmlund, Lindahl, and Plug (2008), however, we show that our results remain practically unchanged when we try to account for the biases introduced by the combination of less identical DZ twins and measurement error.

The intergenerational education estimates for adoptees are presented in columns 4–6 of panel B. We show results for Swedish-born adoptees in the first two rows and the corresponding results for foreign-born adoptees in the next two rows. To deal with selective placement of Swedish-born adoptees, we control for observable characteristics (years of schooling, age and age squared) of the biological mother of the adopted children born in Sweden.[7] In regressions on foreign-born adoptees, we control for adoption age, country/region-of-birth of the child, and the logarithm of GDP per capita in the child's country of birth at the time of birth. The latter measures are thought of as rough proxies for the quality of the prenatal and very early childhood environment prior to adoption.

For the sample of Swedish-born adoptees, the estimates are positive and very similar for fathers and mothers. An additional year of education for a parent is associated with 0.10–0.11 more years for the child. When we control for spouse's education, the estimates decrease to 0.05–0.06. Restricting the father and the mother to have the same coefficient gives a precisely estimated effect equal to 0.08, as reported in column 6. A formal test of impact similarity is supportive with a p-value of 0.20.

[7] We restrict ourselves to including controls for the biological mother, but not the biological father, of the adopted child since the sample is too small with the latter included. Information on the biological mother is about twice as common (and more accurate) compared to biological father information.

We then move on to the estimates for foreign-born adopted children and their adoptive parents. Because of the large sample, intergenerational effects are precisely estimated. We find estimates that are small but statistically significant for both fathers and mothers. An additional year of parental education is associated with 0.04 more years of education for the child, whether or not we control for the other parent's education. In the last column, we again restrict the father's and mother's coefficients to be the same (something we cannot reject, p-value $= 0.35$). The estimated effect is then 0.03.[8]

Next, in panel C, we present estimates using the Swedish compulsory schooling reform as an instrument for parent's years of schooling. These estimates should be interpreted as the reform-induced intergenerational education effects. The equation we estimate includes a full set of municipality-of-residence indicators and these municipality indicators interacted with a linear birth-cohort trend.[9] When we include controls for spouse's education, we also include a full set of municipality-of-residence indicators for the spouse and these municipality indicators interacted with a linear birth-cohort trend. In these specifications, we treat both parents' education as endogenously determined and the education of the spouse is instrumented with a reform assignment for the spouse; that is, we make use of two instruments and identify the effect of both parents' education in the IV estimation. We present intergenerational IV estimates from two different samples: unrestricted parental education (first two rows) and parental education restricted to be nine years or less (next two rows). The first stage results always show that the reform has a very strong effect on years of education: corresponding F-statistics are very high, typically above 100.

In the first row, we find estimates of 0.09 for fathers and 0.11 for mothers. Both are reasonably precise and both are statistically significant. We next expand our empirical model to take into account assortative mating effects by estimating regressions also including controls for spouse's education. With assortative mating controls, the estimates for fathers and mothers remain practically unchanged, but become imprecise. Finally, restricting the coefficients to be the same for fathers and mothers (which cannot be rejected: p-value $= 0.91$) gives a statistically significant estimated effect of parental education of 0.07.

Black, Devereux, and Salvanes (2008), whose estimates are less precise than ours, managed to improve precision by focusing on those parents where the reform has the strongest bite. Since the reform extended compulsory schooling, it mainly affected individuals at the lower end of the educational distribution and focusing exclusively at this part of the distribution thus likely improves precision. Just like Black, Devereux, and Salvanes, this is what we find. The estimates reported in the third and fourth rows of panel C come from a restricted sample of parents with nine or fewer years of education. The IV estimates for fathers are now small and statistically insignificant. For mothers, the estimate is positive and statistically significant when omitting the control for spouse's education. The results indicate that one more year of mother's education (caused by the reform shift) generates 0.07 more years of schooling for the children of mothers at the bottom of the education distribution.

[8] Using a sample of Korean adoptees similar in size to the sample used in Sacerdote (2007), we find small estimates in the range of 0.01–0.03 that are never statistically significantly different from zero. These estimated effects are smaller than the estimates for mother's education found in Sacerdote's study.

[9] With regard to the compulsory school reform in Sweden, there is evidence of a reform effect on parental schooling before the reform is implemented. We therefore focus on IV results from the more general specification with municipality-specific time trends. Compared to those obtained without municipality-specific trends, our IV estimates are arguably more convincing as well as observably more precisely estimated.

3.3 *Comparison with Results from Previous Studies*

When we compare the results using twin differences to those found in the literature, two points are worth noting. First, our twin sample is much larger than the U.S. samples used by Behrman and Rosenzweig (2002) and Antonovics and Goldberger (2005) and, because of that, our effects are much more precisely estimated. Second, our twin estimates for fathers are smaller than those found for the United States and similar in magnitude to those from other Scandinavian countries (Bingley, Christensen, and Jensen 2009; Pronzato forthcoming), but it is striking that, throughout all twin studies, the effect of father's education is always higher than that of mother's.

Our estimates using adoptees come out as relatively small compared to most of the previous literature. Using foreign-born adoptees, we find estimates in the range of 0.03–0.04, which are smaller than those of Plug (2004) and Sacerdote (2007). For Swedish-born adoptees, we find somewhat larger effects, 0.05–0.11 for mothers and 0.06–0.10 for fathers, which are closer in size to the estimates in Sacerdote (2007) but, at least for adoptive mothers, still smaller than the estimates in Plug (2004). In a comparison with Björklund, Lindahl, and Plug (2006), a study also based on Swedish data, our results are mostly similar.

To the best of our knowledge, the only IV study that is similar to ours is Black, Devereux, and Salvanes (2008). We therefore compare our results to theirs, and find that they are similar in the following respects. First, using the same sample restriction (parents with nine or fewer years of schooling), we find a positive and statistically significant effect for mothers, but a small and insignificant effect for fathers. Second, when they relax this restriction on parental education and use the full sample, thus allowing for spillover effects of the reform, they find effects that are imprecise and not statistically different from each other, and from zero. When we base the estimation on the full sample, the effect for fathers is not statistically different from the effect for mothers. However, our estimates are much more precisely estimated.

Based on the results of our replication, we conclude that differences across methods exist also in Sweden and clearly follow the pattern found in other studies; in particular, it is clear that the twins approach produces higher effects for fathers than for mothers and that the IV on a low-educated sample produces positive effects for mothers but no effects of father's education. However, thanks to the large samples generating precisely estimated effects (especially with the sums-specification), we can see that the differences across methods in Sweden are small and that parental schooling associations to a significant degree are driven by selection. This is not surprising, given that education in Sweden (and other Scandinavian countries) is heavily subsidized. From previous studies, however, we know that the differences across methods in the United States are much more pronounced and, given that our replication has established that the variability in estimates follow a pattern, we believe it is interesting to understand where these differences come from. This will be the focus of the next two sections.

4. *Internal Validity*

The next step of our analysis is to investigate the internal consistency of each estimate and whether violations to internal validity assumptions could potentially explain the discrepancies in results across methods.

Empirical research on intergenerational schooling transmission has concentrated on reduced-form intergenerational schooling models with theoretical origins in the intergenerational income mobility models (Gary S. Becker and Nigel Tomes 1979, 1986; Gary Solon 1999, 2004). Consistent with these models, the typical estimated reduced-form equation can be represented as follows:

$$S^c = \delta_0 + \delta_1 S^p + \Gamma_1 h^p + \Upsilon_1 f^p + e^c, \tag{6}$$

where the child's schooling (S^c) is explained by parent's schooling (S^p), heritable traits that are passed on automatically from parent to child (h^p), parenting and child-rearing skills (f^p), and a child-specific characteristic (e^c) that represents everything else that is associated with a child's schooling and that is orthogonal to S^p, h^p, and f^p. The δ_1 coefficient measures the causal effect of parent's schooling on child's schooling and includes, among others, income effects in the presence of capital market imperfections; parenting effects in case the parent becomes a better parent because of more education; and role model effects in case the parent's schooling acts as a standard for the child. The Γ_1 and Υ_1 coefficients capture how much the parent's inherited and parenting endowments influence the child's schooling. If endowment effects operate through income as well, Γ_1 and Υ_1 also include income effects.[10]

Using representative samples of parents and their own-birth children, a bivariate OLS regression of S^c on S^p is unlikely to identify δ_1. Assuming that equation (6) represents the true model the least-squares estimator has the following properties

$$\text{plim}\ \hat{\delta}_{1OLS} = \delta_1 + \Gamma_1 \frac{\text{cov}(S^p, h^p)}{\text{var}(S^p)} + \Upsilon_1 \frac{\text{cov}(S^p, f^p)}{\text{var}(S^p)}. \tag{7}$$

Identification of δ_1 requires either that Γ_1 and Υ_1 are zero or that the unobserved endowments h^p and f^p are unrelated to the parent's years of schooling. These assumptions are obviously too strong. If, for example, more able parents have more schooling and if part of this ability is transmitted to their children by nature, nurture, or both, it follows that the correlations between S^p, h^p, and f^p and Γ_1 and Υ_1 are nonzero and positive, and that the estimate of δ_1 is too high. But the bias could go the other way as well. If people with child-rearing talents prefer children over schooling, and the correlation between schooling and child-rearing endowments is negative, it is also possible that the estimate of δ_1 is too low. Whether the bias is pushing δ_1 up or down is, in the end, an empirical question.

Using less representative samples, the intergenerational coefficients in (6) may be different. In the case of twin parents, adoptive parents, and reform affected parents, we let these coefficients vary and recognize that even internally valid estimates can be different because of external validity concerns. Later, in section 5, we discuss these concerns in more detail.

4.1 *On the Internal Validity of Twin Results*

The twins approach exploits the idea that unobserved differences in h^p and f^p that bias the least squares parameter δ_1 are removed, or at least reduced, within twins. If we take the difference in schooling between the

[10] It is fairly simple to modify the reduced-form model such that both parents have an impact on their child's schooling. Joint identification of maternal and paternal schooling effects is, however, less straightforward and may require additional assumptions. These assumptions, and their credibility, will be discussed in this section as well.

children of twin parents, the true model (6) becomes

$$(8) \qquad \Delta S^c = \delta_{1TW}\,\Delta S^p + \Gamma_{1TW}\,\Delta h^p + \Upsilon_{1TW}\,\Delta f^p + \Delta\varepsilon^c.$$

Using only monozygotic twin parents who are genetically identical ($\Delta h^p = 0$) the least square estimator from a regression of ΔS^c on ΔS^p has the following properties:

$$(9) \qquad \text{plim}\ \delta_{1TW} = \delta_{1TW} + \Upsilon_{1TW}\,\frac{\text{cov}(\Delta S^p, \Delta f^p)}{\text{var}(\Delta S^p)}.$$

There are two identifying assumptions here: (a) twin parents are *identical* in f^p and (b) some twin parents are *nonidentical* in their amounts of schooling. Given these assumptions, the impact of h^p and f^p is differenced out and the twin-fixed effects estimator of δ_{1TW} is consistent. These assumptions, however, may not always hold in practice.

How identical are identical twins with different school outcomes? Not everyone is convinced that identical twins have identical abilities. Although the importance of unobserved heterogeneity in within-twin estimates is often mentioned (Zvi Griliches 1979; John Bound and Solon 1999), there is little empirical work documenting the extent to which unobserved heterogeneity within MZ twins-pairs is random or not. Indicative evidence in Orley Ashenfelter and Cecilia Rouse (1998) shows that parents of twins tend to select names that are very similar in sound and/or writing, suggesting that parents find it difficult to treat their twin children in any other way than identically. Behrman and Rosenzweig (2004) report that within-twin differences in schooling correlate strongly with birth-weight differences and argue that much of the unobserved heterogeneity can be traced back to nongenetic birth-weight differences. Black, Devereux, and Salvanes (2007) find twin-differences in birth weight to be weakly correlated with twin-differences in schooling in Norway, whereas in a sample of U.K. twins there is no such evidence at all (Dorothe Bonjour et al. 2003). Gunnar Isacsson (1999) considers various psychological measures, including the degree of psychological instability, as potential sources of heterogeneity among Swedish MZ twins. Psychological instability is self-reported and, although not ideal, should be a relevant proxy for parenting skills f^p that can affect educational outcomes of both twin parents themselves and of their children. However, Isacsson (1999) finds no effect of psychological instability on schooling using twin-fixed effects.

Another source of bias in twin studies is the possible influence of twin spillovers. Given the close relationship that typically exists between twins, there is a possibility that a child is affected not only by his/her parent's education but also by the education of the aunt/uncle, and that this "twin-spillover" effect will violate the strict exogeneity assumption necessary for an unbiased estimate. If we believe that this source of bias is especially important for twin sisters, this could explain the low intergenerational estimates found for mothers in twin studies. In addition, we would expect the cross-sectional twin estimate to be more biased. Table 3 reports a small difference suggesting that the effects that run through twin interactions are small.

Apart from the problem of unobserved heterogeneity within twin pairs, there is also the issue that twin parents are, almost by definition, different from each other because they are married to different spouses. If both parents, including the twin parent and spouse, shape the school outcomes of children, this means that the parental schooling effects as estimated in (9) will not only capture the impact of the schooling of twin parents but also, in the presence of assortative mating, the impact of the inborn endowments and schooling of their spouses. There is some confusion in the literature as to whether we should classify the unobserved heterogeneity caused by the spouse as bias or not (see discussions in Antonovics and Goldberger 2005 and Behrman and Rosenzweig 2005). If we interpret the within-twin parent estimator inclusive of assortative mating effects, we do not have to worry about the characteristics of the spouse. Unobserved heterogeneity bias is however present if we would like to estimate parental schooling effects net of assortative mating effects. It turns out to be difficult to separate out the influence of the twin parent from the influence of the spouse. The reason is that potential influences of unobserved characteristics of the spouse are not canceled out in our within-twin regressions. With spousal schooling included in (8), the within-twin parent estimator would still be biased upwards if more schooled twin parents marry partners with more favorable endowments.

Another issue that has received much attention is measurement error. It is well known that random measurement error leads to bias toward zero and that within-twin differencing likely amplifies the downward bias.[11] Ashenfelter and Alan B. Krueger (1994) warn us that twins' schooling is often measured with error. They propose to correct for measurement error by instrumenting one measure of schooling with another independent measure of the same variable. Using survey data on twins, they exploit information on twin 1's schooling as reported by twin 2, and take the schooling difference between the twins (as reported by twin 2) as an instrument for the schooling difference (as reported by twin 1). This approach is also followed by Behrman and Rosenzweig (2002) in the context of identifying intergenerational education effects using twins. Twin studies on Scandinavian register data tend to assess the degree of measurement error in the data by combining the register schooling information with survey data on self-reported schooling (Isacsson 1999). Our own calculations point to high reliability ratios for Swedish register data: 0.95 for the cross-section and 0.88 for twin-differences in years of schooling. Bingley, Christensen, and Jensen (2009) also present twin-differences that have been adjusted for measurement error using an IV approach combining register and survey data. But as discussed in Bound and Solon (1999), correcting for measurement error bias by instrumenting with another independent schooling measure also has its drawbacks; it leads to a too high IV-twin estimate in case of mean-reverting measurement error.[12]

4.2 *On the Internal Validity of Adoption Results*

The intergenerational model of schooling for adoptees can be expressed as follows:

$$(10)\qquad S^{ac} = \delta_{0AD} + \delta_{1AD} S^{ap} + \Gamma_{1AD} h^{bp} + \Upsilon_{1AD} f^{ap} + \varepsilon^{ac},$$

where *ap* refers to the adoptive parent and *bp* to the biological parent of the child. The least-squares estimator from a bivariate regression of S^{ac} on S^{ap} has the following properties

$$(11)\qquad \text{plim}\,\hat{\delta}_{1AD} = \delta_{1AD} + \Gamma_{1AD}\frac{\text{cov}(S^{ap}, h^{bp})}{\text{var}(S^{ap})} + \Upsilon_{1AD}\frac{\text{cov}(S^{ap}, f^{ap})}{\text{var}(S^{ap})}.$$

[11] Here we have not explicitly modeled measurement error in parent's schooling. If such measurement error is present and is classical, we simply need to multiply our OLS estimates by the inverse of the reliability ratio for the parent's schooling measure, or in case of twins, the difference in parent's schooling between twins, to get internally valid estimates.

[12] Mean-reverting error may occur with bounded outcomes. In case of years of schooling, for example, twins with the lowest and highest grade completed can only deviate from the truth by either over- or underreporting.

The adoption strategy to identify δ_{1AD} exploits the idea that adoptees do not share their adoptive parents' genes. Identification of δ_{1AD} now rests on three assumptions: (a) adoptees are randomly assigned to adoptive families ($\mathrm{cov}(S^{ap}, h^{bp}) = 0$), (b) the adoptive parents' child-rearing talent and schooling are unrelated ($\mathrm{cov}(S^{ap}, f^{ap}) = 0$), or unobservable child-rearing skills have no impact on schooling of the adopted child ($\Upsilon_{1AD} = 0$), and (c) children are adopted at birth and can receive the full impact of adoptive parents' education.

One of the difficulties with an adoption approach is that for adoptees the assignment process is not always random. Nonrandom matches involve both related and unrelated adoptions. In the case of related adoptions, genetically related matches are obvious. Parents who raise and adopt their relatives' children share genes with their adoptees' own birth parents because they are family. In the case of unrelated adoptions, nonrandom matches occur less frequently but are still possible when better educated parents manage to adopt children with more favorable backgrounds or when adoption agencies use corresponding qualities of both natural and adoptive parents as a matching device.[13]

The extent to which adoptees are randomly placed likely differs for domestic and international adoptions. In case of foreign-born adoptees, the assignment mechanism is fairly random. Related matches are absent, and information on the adopted child's natural parents is often limited. Adoptive parents typically do not know who the biological parents of their adopted children are. They know—like we do—the adoptees' gender, age, and country of origin. To shed some light on whether foreign-born adoptees are randomly matched to their adoptive parents, pretreatment characteristics of the adoptees can be regressed on the schooling variables of the adoptive parents. With random assignment, we expect no relationship between adoptee's and parent's characteristics. Sacerdote (2007) finds evidence in support of random assignment for pretreatment variables such as gender of adoptee and age of adoption, using a sample of Korean-born adoptees adopted by U.S. parents. We, however, find some evidence of selection: higher educated parents are more likely to adopt younger children and children from more economically developed countries, but the magnitudes of the effects are very small, and including these controls do not impact our estimates. In case of domestic adoptions, it is less likely that children are randomly assigned to adoptive parents. Björklund, Lindahl, and Plug (2006) find that schooling of the adoptee's biological parents is positively correlated with schooling of the adoptive parents, and this result is confirmed in our study. Intergenerational estimations using native-born adoptees will therefore be too high unless we include controls for the characteristics of the biological parents. In our study, the results do not change when we add such controls.

The second assumption for identification of δ_{1AD} requires that the unobserved child-rearing endowments of the adoptive parent are unrelated to parent's schooling or that this endowment has no impact on schooling of the adopted child. This is an untestable assumption. It means that we must interpret δ_{1AD} as the combined effect of parent's schooling *and* all other factors that are correlated with the adoptive parent's schooling and that have an independent effect on child's schooling, net of the genetic transmission. Thus, the adoption estimates reflect the total parental "nurture" effect, which operates both through parental schooling and through good parenting skills.

13 If better educated parents seek the most unfortunate children, the matching process need not be positive.

The final assumption requires that children move to their adoptive parents immediately at birth. Information on age of adoption is not always available. In most censuses, however, one can infer age of adoption for foreign-born adoptees through age of immigration. In our analysis, we have restricted the sample of foreign-born adoptees to those adopted within the first six months of their lives. But if the first months and/or the time in the womb are particularly important, it is possible that the estimates on foreign-born adoptees reported in table 3 are still too low. This is not the case. When we restrict the sample of foreign-born children even further, so that they are adopted already within the first month of their lives, our results remain practically unchanged. For Swedish-born adoptees, we do not know the age of adoption, but we have looked at Swedish-born adoptees that were born exactly one year prior to the census date in 1965 and found that about 80 percent of all adoptees were adopted within one year. Björklund, Lindahl, and Plug (2006) also found that Swedish-born adoptees in general are adopted at an early age.

If we simultaneously want to estimate schooling impacts for both parents, generalization of the adoption framework is straightforward; we simply add spouse's schooling to equation (10). The bias caused by both parents' heritable endowments is then eliminated. The inborn child-rearing talents of both adoptive parents still remain, however, and if better educated parents choose their marriage partner for his/her parenting skills, the bias due to unobserved parenting skills will be exacerbated.

4.3 *On the Internal Validity of IV Results*

The third strategy estimates the intergenerational schooling effect by exploiting a reform extending compulsory schooling. We have defined a reform indicator (REF^p) that takes the value one if the parent belongs to a birth cohort and municipality that was subject to the educational reform and zero otherwise. The empirical model is estimated by regressing S^c on S^p using two-stage least squares, where (4) serves as the first stage using REF^p as the instrument for S^p. The estimate has the following properties:

$$\text{(12)} \qquad \text{plim}\ \hat{\delta}_{1IV} = \delta_{1IV} + \frac{\text{cov}(\varepsilon^c, REF^p)/\text{var}(REF^p)}{\text{cov}(S^p, REF^p)/\text{var}(REF^p)},$$

where the inconsistency term consists of a numerator, which is the coefficient from a regression of the error term on the reform indicator, and a denominator, which is the first-stage coefficient estimate. Both are conditional on all other variables included in the two-stage least squares estimation, which are birth cohort indicators, municipality indicators, and, in our case, also municipality-specific trends. To obtain an internally consistent estimate of δ_{1IV}, we need to impose two assumptions: (a) REF^p is not correlated with the unobservables in the main equation $(\text{cov}(\varepsilon^c,\ REF^p)) = 0$ and (b) REF^p is strongly correlated with parental schooling. Both assumptions guarantee that the inconsistency term in (12) disappears.

The key assumption that the compulsory schooling reform affects the schooling of children exclusively through the schooling of parents may not always hold. Although we do believe that the institutionalized change in compulsory education is unrelated to parental endowments (conditional on birth cohort indicators, municipality indicators and variations thereof), our concern is that there are possibly other factors that are affected by the reform as well. Among these factors, we will briefly discuss reform-induced changes in teacher quality, accompanying school reforms, peers, and spousal education.[14]

It is likely that expansion of compulsory education, whether in U.S. states or in Europe, also affected the demand for teachers. If new and inexperienced teachers are more likely to teach those individuals affected by the reform, the IV estimates will incorporate the change in teacher quality and its possible direct effects on parent and child outcomes. It is also likely that institutional changes, like mandatory schooling reforms, are accompanied with other simultaneous changes to the education system. The Scandinavian compulsory school reforms, for example, did not only imply an increase in the number of compulsory years of schooling but also postponed ability tracking (Meghir and Palme 2005).

Another, typically overlooked, issue is that the reform indicator is not defined over individuals but over groups of individuals (in our case groups are defined by cohort and municipality). This means that the reform forced *all* parents to stay in school for two additional years, thereby affecting the peer composition of each parent individually. The IV estimate will therefore capture both individual and peer effects of schooling on the next generation. It is not clear whether this leads to an over- or underestimation of the individual effect of parental schooling on the next generation. It is possible, for example, that individuals with a college degree before the reform would have attained less schooling had the reform been in effect because of increased exposure to peers that typically concentrate at the bottom of the educational distribution. To separate the individual from the external effects, one needs two instruments.[15] Note that in case peer effects do not affect the schooling of the next generation directly, they still complicate the comparison with the other effect estimates that rely on individual-specific variation in the schooling of twins and adoptive parents.

Another source of heterogeneity stems from education and unobservable characteristics of the partner. If we treat partner's years of schooling as an additional endogenous variable, we need to use the compulsory schooling reform for the partner as an additional instrument. Omitting partner's years of schooling may lead to inconsistent IV estimates. As Maarten Lindeboom, Ana Llena-Nozal, and Bas van der Klaauw (2009) point out, if partners match on age and municipality, reform status of partners are correlated and part of the estimated school impact of one parent will come from the school impact of the partner. Including partner's years of schooling, on the other hand, may lead to imprecise estimates. Again, if both parents are similar in age and municipality, there is possibly too little variation in reform status of two parents to generate precision.

The second identifying assumption is testable; that is, the instrument must be a strong predictor of parent's schooling. The instrument is considered weak if its impact on, in our case, parental schooling is either statistically insignificant or small. It is easy to see that such a weak instrument, which affects the denominator in (12), can easily lead to misguided estimates (see the work of Bound, David A. Jaeger, and Regina M. Baker 1995 and Jinyong Hahn and Jerry Hausman 2003). In case of compulsory schooling reforms,

[14] Another potential threat to identification from compulsory school reforms that are rolled out gradually across a country is that individuals respond by moving to or from reform municipalities depending on their preferences for the old or new system. In the Swedish case, selective mobility seems to be a minor problem. As reported in Costas Meghir and Mårten Palme (2003) and Holmlund (2007), the degree of mobility was low and does not seem to follow a systematic pattern.

[15] The identification of externalities is addressed in Daron Acemoglu and Joshua D. Angrist (2001). They combine information on the individual's quarter of birth with state of birth to separately identify the individual and external returns to schooling.

however, there exists no study that rejects the relevance assumption: first stage effects are typically sizable and come with high F-statistics and, as mentioned in section 4, this is the case also in this study.

An additional issue arises when the sample is restricted to only those individuals with the lowest level of education as in Black, Devereux, and Salvanes (2008), where the sample was restricted to those parents with at most nine years of schooling. In order to obtain consistent estimates on this restricted sample, we need to make the assumption that the individuals who completed nine years of schooling or less in the absence of the reform would not complete more than nine years if the reform had been in effect. This assumption rules out dynamic effects of the reform for the individual. If this assumption does not hold, the composition of individuals with only compulsory education in a municipality will differ pre- and postreform. This will likely bias the intergenerational estimate downwards because those individuals who gained the most from the reform (and continued their education longer than what was required) are now excluded. In case of the Swedish reform, we have tested for dynamic responses by estimating the effect of the reform on the probability of attaining postcompulsory schooling. We find increased probabilities of attending both high school and university: a one percent increase for both high school and university for mothers, and a two percent increase for respectively high school and university for fathers. All these estimates are statistically significant. These percentages indicate that the dynamic effects are large, in particular for fathers. About one-fifth of all affected fathers continue into higher education because of the reform. We therefore conclude that using the restrictive sample of low-educated parents leads to inconsistent estimates and that such estimates are likely too low since those with the highest returns to the reform are excluded. The relatively larger dynamic effects for fathers might explain why the intergenerational effects for mothers and fathers are more similar when using the full sample, compared to those effects found using the restricted sample.

4.4 *Synthesis*

In our view, none of the three identification strategies is perfect. In each case, there are internal validity assumptions that are easily violated, resulting in intergenerational effect estimates that are arguably biased. In this section, we consider all three strategies jointly and explore the extent to which these internal validity violations can lead to some of the discrepancies in results observed across strategies.

To understand how the key inconsistencies for each method can be mapped onto the empirical patterns found in the literature, it is useful to consider the special case where the true intergenerational mobility process as described in (6) is the same for all parents and their children, including twin parents, parents that adopt, and those parents that were affected by the reform. In this special case, the twin and adoption strategies still yield inconsistent estimates of the parental schooling effect parameter. With twins, the estimates of parental schooling effects are biased to the extent that schooling differences between twin parents are not randomly determined. We calculate the bias as[16]

$$\text{plim}\ \hat{\delta}_{1TW} = \delta_1 + \Upsilon_1 \frac{1-\rho_f}{1-\rho_s} \frac{\text{cov}(S^p, f^p)}{\text{var}(S^p)}.$$

[16] To arrive at this bias expression, we have rearranged (9), assumed that twins exhibit the same variance of schooling $\text{var}(S) = \text{var}(S_1) = \text{var}(S_2)$ and covariance between schooling and child-rearing endowments $\text{cov}(S, f) = \text{cov}(S_1, f_1) = \text{cov}(S_2, f_2)$ and $\text{cov}(S_1, f_2) = \text{cov}(S_2, f_1)$, that the child-rearing endowments of twins are linearly related, and written $\rho_s\,\text{var}(S) = \text{cov}(S_1, S_2)$ and $\rho_f\,\text{cov}(S, f) = \text{cov}(S_1, f_2)$, where ρ_s and ρ_f represent correlations between s_1 and s_2, and f_1 and f_2 respectively.

With adoptees, we consider the bias due to f^p as most troublesome. If children who are given up for adoption are randomly placed in their adoptive families, the adoption estimates are biased to the extent that unobserved parental nurturing is correlated with parental education,

$$\text{plim } \hat{\delta}_{1AD} = \delta_1 + \Upsilon_1 \frac{\text{cov}(S^p, f^p)}{\text{var}(S^p)}.$$

The similarity in bias expressions suggests that inconsistent twin and adoption estimates are biased in similar directions.

The conditions for internally valid IV estimates are probably fulfilled. In Sweden, for example, the reform is a strong predictor of schooling, and conditional on the controls we include in the regressions, the reform is unlikely to be correlated with parental endowments. The uncertainty lies in whether the direct effects of the reform on children's education, not operating through parent's education, are small.

If we next formalize the empirical pattern such that $\delta^f_{1TW} \geq \delta^f_{1AD} \geq \delta^f_{1IV}$ holds for fathers, $\delta^m_{1TW} \leq \delta^m_{1AD} \leq \delta^m_{1IV}$ holds for mothers, and $\delta^f_{1TW} + \delta^m_{1TW} \approx \delta^f_{1AD} + \delta^m_{1AD} \approx \delta^f_{1IV} + \delta^m_{1IV}$ holds for both parents, it is easy to show that the results in tables 1 and 3 can be attributed to the remaining inconsistencies. To illustrate how, suppose the IV estimates are the true estimates. If the intergenerational school impact of mothers is truly stronger than (or at least as strong as) the intergenerational school impact of fathers, the theoretical bias expressions indicate that there is a negative correlation between the child-rearing skills and schooling of twin mothers, and possibly a positive correlation between the child-rearing skills and schooling of twin fathers. In addition, if $\rho_f < \rho_s$, the bias expressions further predict that the inconsistencies are more pronounced among twin parents than among adoptive parents. If women with child-rearing talents have children rather than go to school, the twin estimates of maternal schooling effects are clearly biased downwards. If men with child-rearing talents, on the other hand, are less responsive to having children and rather invest in their schooling, it is possible that the twin estimates of paternal schooling effects are biased upwards. Together, however, the biases appear to offset each other as the maternal and paternal schooling effects together appear constant across methods. Another compelling conclusion can be drawn under the assumption that the IV estimate represents the true effect. Recall that the adoption estimates likely represent the total parental nurture effect. The small differences between the IV and Swedish-born adoption estimates therefore means that parental schooling is one of the most important inputs in the child's nurture production function.[17]

To conclude, we have shown that it is possible to predict our findings, and those obtained by others under a rather restrictive assumption that all three strategies aim to estimate one single parental schooling effect parameter that is the same for all parents and children. In the following section, we relax this assumption and allow for different parental schooling effects for different parents and children.

5. *External Validity*

In this section, we turn to the external validity of the three different methods.

[17] Note that, in Sweden, the effect estimates display a similar pattern as mentioned above, even though they are much more compressed, and therefore more similar, across the different strategies than the other estimates. There is, however, one exception. In case of foreign-born adoptees in Sweden we find that the schooling of *both* the father and mother has little, if any, impact on the schooling of their adopted children. This means that, for foreign-born adoptees in Sweden, we must interpret the theoretical bias expressions a bit differently to predict our findings. If the IV estimates represent the true estimates, we must assume that the years spent in school and child-rearing talents of *both* parents are negatively correlated.

The question we address is whether intergenerational effect estimates obtained with samples of twin parents and their children, parents with adopted children, or parents whose behavior has been affected by compulsory school reforms are (informative about and) generalizable to a larger population of representative parents and children. In particular, we are interested in the assumptions we must make in order to extrapolate three different estimates and the extent to which these assumptions hold in practice.

All the empirical work we have discussed thus far can be represented by one unifying heterogeneous effect model; that is, we assume that the effect estimates come from an intergenerational transmission model where the impact of a one-year change in parental schooling on child schooling may vary over subgroups of children. The basic model we have in mind is a bivariate random-coefficient model

$$(13) \qquad S_i^c = \delta_{0j} + \delta_{1j} S_i^p + u_i,$$

where subscript i denotes the family in which the child is brought up, and where δ_{1j} represents the parental schooling effect for j distinctive groups of parents and children ($i \in j$), all similar in the way parents transmit their schooling to their children. Model (13) is a generalized version of (6), where u_i incorporates the unobserved endowments and the random error term. If we assume (for now) that the schooling and unobserved endowments of parents are uncorrelated, least square estimation of (13) using a representative sample of parents and children will give us an estimate with the following properties

$$(14) \quad \text{plim}\, \hat{\delta}_{1OLS} = E[\omega_j\, \delta_{1j}] = \Sigma_j\, \omega_j\, \delta_{1j},$$

where the weight ω_j measures how much each subgroup of children and parents contributes to $\hat{\delta}_{1OLS}$.[18] Using this expression, it is clear that under these particular exogeneity conditions $\hat{\delta}_{1OLS}$ represents the population average of all parental schooling effects. Using the same expression, it is also clear that when parental schooling effects are estimated on atypical and unrepresentative samples the corresponding parameter estimates need not be equal.

5.1 *On the External Validity of Twin Results*

With twins we take the difference in schooling between the children of twin parents. With heterogeneous transmission coefficients (that vary across but not within twin pairs), we get

$$(15) \qquad \Delta S_i^c = \delta_{1j}\, \Delta S_i^p + \Delta u_i.$$

If we assume that all variation in ΔS_i^p is exogenously determined, the least square estimator from a regression of ΔS^c on ΔS^p using a representative sample of twin parents (being different in their levels of schooling) and children will give us an estimate with the following properties

$$(16) \qquad \text{plim}\, \hat{\delta}_{1TW} = \Sigma_j\, \omega_j^{twin}\, \delta_{1j},$$

where $\hat{\delta}_{1TW}$ represents the twin sample average of all parental schooling effects. If we compare (16) to (14) it is easy to see that there exist two assumptions that make extrapolation of twin results possible. The *identical distribution assumption* restricts the sample subgroup weights to be similar across different samples but allows the true

[18] Note that we do not need to define subgroups or observe weights directly. In case parental schooling effects vary from child to child, we know that each child contributes equally to $\hat{\delta}_{1OLS}$ and that weights are constant and the same for all children. Note that ω_j is defined such as $\Sigma_j\, \omega_j = 1$ for the representative sample in (14) and that also $\Sigma_j\, \omega_j^K = 1$ for the various subpopulations, e.g., for k = *twins, adoptees, reform*.

intergenerational effect parameter to vary across subgroups ($\delta_{1j} \neq \delta_1$, $\omega_j^{twin} = \omega_j$). The twin estimate then collapses to (14). And alternatively, the *constant effect assumption* imposes that the intergenerational effect parameter is constant and the same for all parents and children, but allows twin parents (with different amounts of schooling) and their children to come from atypical and unrepresentative distributions ($\delta_{1j} = \delta_1$, $\omega_j^{twin} \neq \omega_j$). Hence, since the weights sum to one, the twin estimate collapses to δ_1.

Both assumptions can be partly checked by comparing summary descriptives and regression results using different samples of twins and non-twins. In the end, the external validity of the twin-fixed effects estimator depends on whether any of the two assumptions seems plausible.

5.1.1 *Characterizing Twin Parents and Their Children*

We first ask whether twin parents (with different amounts of schooling) and their children are as representative as all other parents and children? When we consider twins we believe they are. In Sweden, we find that twins with different amounts of schooling and singletons born between 1943 and 1955 do equally well in school and give birth to children who do equally well in school. A simple comparison with other representative samples also suggests that educational differences between same sex twins and singletons, and between their children, are typically very small (Black, Devereux, and Salvanes 2007; Heather Royer 2009; Bingley, Christensen, and Jensen 2009). The other question we should ask is whether parental schooling effects may be different for the children of twins and non-twins. Table 3 indicates that cross-sectional schooling associations between parent and child are only marginally smaller for twin than for singleton parents. Because the intergenerational transmission estimates are more similar than different for twin and singleton parents, we believe that there is nothing special in the way the children of twins respond to a one-year change in parental schooling.

As a cautionary note, we do not want to claim that twin results based on representative samples of same sex twins are by definition generalizable to the rest of the population. With the increased popularity of reproductive technologies, including in vitro fertilization, we expect that, among twins, the fraction of DZ twins will grow, that future same sex twin samples will be much more choice based, and because of that much less likely to be representative.

5.2 *On the External Validity of Adoption Results*

If we estimate the effect of parental schooling on child schooling on a representative sample of adoptive parents and their adopted children, we obtain the following adoption estimator

$$\text{(17)} \qquad \operatorname{plim} \hat{\delta}_{1AD} = \Sigma_j \, \omega_j^{adop} \, \delta_{1j},$$

which represents the adoption sample average of all parental schooling effects. Analogue to the twin method, there are two assumptions under which the adoption estimates have broader predictive power: the identical distribution assumption ($\delta_{1j} \neq \delta_1$, $\omega_j^{adop} = \omega_j$) and the constant effect assumption ($\delta_{1j} = \delta_1$, $\omega_j^{adop} \neq \omega_j$).

5.2.1 *Characterizing Adoptive Parents and Their Adopted Children*

We have a reasonably clear picture on how the school outcomes of adoptees and their adoptive parents compare to that of other children and parents; that is, they considerably differ. Adoptees are typically lower educated than other children, probably because of emotional problems that come from the adoption experience, poor biological family background, or selection. If parents could choose, they would probably put their least talented child up for adoption (see Becker 1991). Adoptive parents, on the other hand, are typically higher educated than other parents. They may be self-selected to take up the task of raising children or may be selected on some characteristics by adoption agencies. Another possibility is that many of the prospective parents start to think about adopting a child after having experienced fertility problems; it has been shown that fertility falls with the level of education of both the mother and father. In sum, given the particular combination of adopted children, often with disadvantaged backgrounds, raised by adoptive parents, often with more favorable characteristics, it is impossible to come up with a comparable sample of own-birth children and their parents. This sample simply does not exist. This also means that extrapolation cannot be based on the identical distribution assumption.

What about the more restrictive constant effect assumption? The adoption literature has put forward various heterogeneity mechanisms to explain why adopted children may respond differently to the same impact of a one-year change in parental schooling. We discuss the three main mechanisms.

The first mechanism is that children who are given up for adoption are inherently different from other children. These inherent differences may threaten external validity if they are somehow related to parental schooling effects. Possible differences that make adoptees typically less receptive to their parents' attention are emotional problems that are typical to adoption, and initial language and cultural differences between the adoptive parent and foreign-born adoptee. It is rather difficult to account for these differences other than by allowing separate intercepts in regressions using adoption and nonadoption samples. Evidence against this heterogeneity concern is the perhaps surprising intergenerational transmission pattern found for adoptees: the total impact of the adoptive and biological parents' schooling on the school outcomes of adoptive children is remarkably similar to the impact of the biological parents' schooling for that of biological children. This is found in Björklund, Lindahl, and Plug (2006), but also found by us when we restrict our already small adoption sample to those adoptees for which we have information on all biological and adoptive parents.

A second and related mechanism is that children who are given up for adoption are not only inherently but also distributionally different from other children. As we already mentioned, adoptees likely come from low-educated families. In a model where interactions between genes and environment are positive, we would then find that adoptees benefit less from an improved environment than other, more able, children do. Björklund, Lindahl, and Plug (2006) analyze whether interactions play a role in the intergenerational transmission of schooling and find evidence of a positive interaction for mother's schooling, but not for father's schooling. If we take their estimated interaction effect for mothers, it is possible to predict how much less these adoptees would benefit from a one-year change in maternal schooling. In their study, the intergenerational transmission model is given by

$$(18) \quad S^{ac} = \alpha_0 + \alpha_1 S^{bp} + \alpha_2 S^{ap} + \alpha_3 S^{bp} S^{ap} + \epsilon^{ac}.$$

Assuming that for own-birth children ($S^{bp} = S^{ap}$) the predicted difference in maternal schooling effect, evaluated around the schooling sample means of the birth-mothers of adoptees and nonadoptees, can be written as

$$(19) \quad \left[\frac{\partial S^{ac}}{\partial S^{ap}}\right]_{S^{bp}=E[S^{ap}]} - \left[\frac{\partial S^{ac}}{\partial S^{ap}}\right]_{S^{bp}=E[S^{bp}]} = \alpha_3(E[S^{ap}] - E[S^{bp}]).$$

With an α_3 estimate of 0.02, and sample means of 9.76 and 11.29 for own-birth mothers' schooling of adoptees and nonadoptees, we find that the difference in effect size is about 0.03. This is a small number, and too small, we think, to believe that intergenerational effect estimates are affected much by genes–environment interactions.

The third mechanism works through the way in which parents respond to these differences. The question we raise is whether parents treat their adopted and own-birth children differently because they are different. It is not a priori clear in which direction treatment differentials affect parental schooling effects. Differences in upbringing, for example, lead to smaller parental schooling effects for adoptees (compared to other children) if parents respond to Cinderella motives and rather invest in their own-birth children because of some biological imperative, as suggested by Anne Case, I. Fen Lin, and Sara McLanahan (2000, 2001), or if parents respond to efficiency motives and allocate more educational funding to their own-birth children because of some talent or information advantage. On the other hand, differences in upbringing may also lead to larger parental schooling effects if parents are instructed to be more patient and tolerant toward their adopted children, as proposed in some of the handbooks we read for social workers dealing with adoptions, or if parents respond to compensating motives and choose to invest more in their less talented, adopted children.

To check whether treatment differentials matter, it is possible to take advantage of the fact that some parents raise both adopted children and their own biological children. Two comparisons can be made. The first is a comparison between intergenerational OLS estimates based on parents and own-birth children raised in families with and without adopted children. The argument is that, in the first case, the parents can treat their children differently being both adoptive and own-birth parents, whereas in the second case they are only own-birth parents and cannot differentiate their treatment. Similarity of effects in these two cases would indicate that treatment differentials are absent and that adoptive and nonadoptive parents are comparable. The second comparison is between intergenerational OLS estimates based on adoptees with and without own-birth siblings. As before, one can argue that in the first case, adoptees compete for treatment with own-birth children, whereas this is not possible in the second case. If intergenerational transmission effects for adoptees with own-birth siblings are comparable to those found for other adoptees, we would conclude that treatment differentials do not exist. Table 3 reports intergenerational associations estimated on samples of own-birth children with adopted siblings that are statistically similar to (and in case of foreign born adoptees sometimes even smaller than) those found for all own-birth children. In analysis not tabulated in the paper, we switch to samples of foreign-born adoptees with own-birth siblings and find point estimates of 0.047 [0.022] and 0.037 [0.023] for fathers' and mothers' schooling, respectively. These estimates are very comparable to those found for all foreign-born adoptees. A comparison with results based on similar specifications reported in Plug (2004), for U.S. adoptees and their siblings, and in Björklund, Lindahl, and Plug (2006), using a larger sample of Swedish-born adoptees than is the case in this study, also supports similarity in upbringing. These findings suggest that there are no apparent differences in the way (at least native-born) adoptees and own-birth children are brought up.

Although we observe that Swedish-born adoptees and own-birth children have very different school outcomes, we haven't found any evidence that they respond strikingly differently to a one-year change in parental schooling. If the intergenerational transmission process for adoptees can indeed be approximated by a restricted transmission model where the true intergenerational effect parameter is fixed and the same for all children, we must conclude that the adoption estimates found for native-born adoptees likely are externally valid. When it comes to foreign-born adoptees, we are a bit more hesitant to generalize the results. First, we know much less about foreign-born adoptees. Without accurate information on the biological background of foreign-born adoptees, we are unable to address omitted variable and interaction concerns. Second, foreign-born adoptees are less comparable to nonadopted Swedish children in that they have experienced early separation from their parents and in that they look different than Swedish children. Swedish-born adoptees are, in some respects, more comparable to nonadopted Swedes in that they look the same and in that they spend the time prior to adoption (both in the womb and as infants) in Sweden. Hopefully, with the growing interest in this field of research, researchers will collect more detailed background data on foreign-born adoptees in the near future.

5.3 *On the External Validity of IV Results*

The external validity assumptions that lay behind the IV approach can be derived more formally by introducing a heterogeneous first stage relationship; that is, we assume that parental schooling is, on average, affected by changes in the compulsory schooling legislation, but that the impact of the compulsory schooling reform may be different for different subgroups of parents:

$$(20) \quad S_i^p = \gamma_0 + \gamma_{1j}\, REFORM_i^p + v_i,$$

where subscript i denotes the parent and γ_{1j} represents the heterogeneous reform effect. To obtain the IV estimator, we first substitute (20) into (13) to get the reduced-form relationship:

$$(21) \quad S_i^c = \delta_0 + \gamma_0\, \delta_{1j} + \delta_{1j}\, REFORM_i^p + u_i + \delta_{1j}\, v_i.$$

If we assume that there is a monotonic and positive relationship between the reform and parental schooling, and that the reform itself is exogenous and excludable, the instrumental variable estimator is given by

$$(22) \quad \text{plim}\ \hat{\delta}_{1IV} = \frac{E[\omega_j^{ref}\, \gamma_{1j}\, \delta_{1j}]}{E[\omega_j^{ref}\, \gamma_{1j}]} = \Sigma_j\, \nu_j^{IV}\, \delta_{1j},$$

where ν_j^{IV} (defined as $\omega_j^{ref}\, \gamma_{1j}/E[\omega_j^{ref}\, \gamma_{1j}]$) represents some unobserved weight that measures how much each subgroup of parents contributes to $\hat{\delta}_{1IV}$. The higher the latent weight, the larger the reform impact on parental schooling.

All the recent studies that estimate the effect of parental schooling on child schooling using changes in compulsory schooling laws are quite clear on how to interpret the corresponding estimate; that is, the average effect among those parents who were forced to stay in school for at least one or two more years because of the reform. Guido W. Imbens and Angrist (1994) call this the local average treatment effect (LATE).[19] Only if the compulsory schooling reform impacted a substantial share of the population, one could argue that the estimated effect provides information about what the intergenerational schooling effect is for the more general population of parents and children (Oreopoulos 2006). In general, the LATE estimates will convert into ATE estimates

[19] In case of a variable treatment, such as years of schooling, Angrist and Imbens (1995) refer to the average causal response parameter instead.

if one imposes the identical distribution assumption ($\delta_{1j} \neq \delta_1$, $\nu_j^{IV} = \omega_j$) or the constant effect assumption ($\delta_{1j} = \delta_1$, $\nu_j^{IV} \neq \omega_j$).

5.3.1 *Characterizing Those Individuals Who Are Sensitive to Compulsory Schooling Reforms*

In Sweden, the change in compulsory schooling from minimum seven to nine years only affected a small share of the population. We have estimated that, even though a large proportion was exposed, only 12 percent of all Swedish parents born between 1943 and 1955 were affected from reform-exposure. This is because many individuals attained levels higher than the (new) minimum regardless of the reform and, as such, their behavior was unaffected. Loss of generalizability is likely not only because there are only quite few affected parents but also because those who are affected are very different from other parents. Affected parents typically concentrate at the bottom of the educational distribution, where dropouts and individuals with a distaste for learning are overrepresented. For nonaffected parents, which include those parents who would have had more schooling anyway, the reform has arguably exerted no influence. Hence, the answer to the external validity question is clear cut. Unless we assume a constant effect model, we cannot say much about the reform-induced intergenerational estimate for nonaffected parents.[20]

The constant effect assumption can be partly checked by comparing the cross-sectional transmission estimates for affected and nonaffected parents. A small difference would then suggest that affected and nonaffected parents, although very different in terms of their observed and unobserved characteristics, are not so different in the way they transmit their educational attainment to their children. Two other aspects are worth emphasizing if we could conduct much of the same analysis on the restricted sample of reform-affected parents. First, we should observe the same but much more precisely estimated intergenerational transmission effect simply because we would estimate a much stronger first stage relationship. Second, we should observe that the cross-sectional estimate converges to the IV effect estimate because much of the variation in parental schooling would now be exogenous and come from the introduction of more restrictive compulsory schooling laws. Note that, in this case, the constant effect assumption implies that cross-sectional estimates and IV estimates are the same and do not change when the representative sample of all parents and children is replaced with the restricted sample of reform-affected parents and their children.

The problem, however, is that it is impossible to identify those parents that are affected by the reform directly. In the absence of a dynamic response to the reform, an intuitive alternative is a sample restricted to lower educated mothers/fathers. Although various researchers have worked with these restricted samples to obtain more precise IV estimates (and not so much to assess their broader predictive power), they would all have rejected external validity based on the constant effect conditions formulated above (Black, Devereux, and Salvanes 2005, 2008; Machin, Panu Pelkonen, and Salvanes forthcoming; Lindeboom, Llena-Nozal, and van der Klaauw 2009; Oreopoulos, Page, and Stevens 2006).

Regrettably, it is difficult to assess the plausibility of the constant effect assumption. Samples restricted to lower educated

[20] Most of the recent work that used the more restrictive compulsory school laws share this problem. In Norway, Canada, and the United States, for example, comparable reforms affected about 10 percent of population (Black, Devereux, and Salvanes 2005; Oreopoulos, Page, and Stevens 2006). In the United Kingdom, however, a similar reform affected more than 80 percent of the population (Chevalier 2004).

mothers/fathers typically miss some of the reform-affected parents. If dynamic responses are small, most of the evidence goes against the external validity of compulsory reform estimates. If, on the other hand, dynamic effects of the reform are substantial, we have little indication on whether the constant effects assumption holds in practice (or not).

5.4 *Synthesis*

So far, we have discussed the external validity of each method separately. With three different identification methods combined, however, we may go one step further. If all three methods had produced the same effect estimate, the external validity answer would have been clear cut; that is, the causal impact of parental schooling on child schooling (both measured as the number of years spent in school) appears to be constant and arguably similar for all parents and children. This is not what we have found, at least not when we consider *all* available intergenerational effect studies. There are differences across methods; that is, twin and adoption studies suggest that the schooling of fathers has a somewhat bigger effect on the child's schooling than that of mother's, whereas IV studies that exploit compulsory schooling reforms suggest that the impact of schooling of mothers is slightly bigger than that of fathers. On the other hand, although the reported intergenerational schooling effects appear to follow the pattern found in all the studies under review, the differences across method turn out to be quite small when we consider only those intergenerational effect studies that come from Denmark, Norway, and Sweden. In fact, we rather believe that the differences across studies in the Scandinavian countries are far too small to reject that a simple homogeneous linear transmission effect model can be used to describe the parental schooling effects in all three samples, in particular when we consider the transmission effect models that measure the effect of a one year increase in parental schooling of either the mother or father.

We do not want to argue that all three methods identify one single causal effect of parental schooling on child schooling that is representative for all parents and children. As we already mentioned in the previous section, we expect the intergenerational effect parameters to differ because of identification strategies being flawed. What we do want to argue, however, is that the intergenerational effect estimates obtained with samples of twin parents and their children, parents with adopted children, or parents whose behavior has been affected by compulsory school reforms have some predictive power for the larger population of representative parents and children

6. *Concluding Remarks*

It is only a decade ago that research on the intergenerational transmission of human capital started to put more emphasis on causal intergenerational effects. That is, do more educated parents get more educated children because of their education? By now there are multiple studies that try to obtain an estimate of the intergenerational effects from increasing parent's schooling, using various identification strategies, and finding answers that appear too disparate to be informative. We have set ourselves the goal to understand why this is and documented and described the results that come from three alternative identification strategies: identical twins, adoptees, and IV, the latter method often using educational reforms as instruments. At the end of this overview, we believe that five conclusions can be drawn, at least given the current state of our knowledge.

First, the intergenerational effect estimates differ systematically across identification strategies. Within each identification strategy, however, the intergenerational effect estimates appear quite robust. For example, using the difference in twin parents' schooling to identify the effect of schooling on their children, the father is shown to be more important than the mother, a pattern that holds in the United States as well as in Scandinavia. Results based on an educational reform instrument instead show that mothers play a more predominant role than fathers in passing on education to their offspring.

Second, we are quite skeptical that any of the three identification strategies we discuss can deliver an estimate of the intergenerational effects from increasing the *overall* level of parent's schooling, leaving the existing distribution of inherited and child-rearing abilities unchanged. In our view, none of the three identification strategies is perfect. In each case, there are either internal or external validity assumptions that are easily violated, resulting in intergenerational effect estimates that are either biased or that have limited predictive power to a representative population.

Third, the intergenerational effect estimates for Denmark, Norway, and Sweden are all very similar. If we focus on the estimates that do not account for assortative mating, those estimated with precision vary between 0.03 and 0.13. Most intergenerational effect estimates, however, are concentrated around 0.10. These results appear to be similar enough to suggest that, despite threats to identification, the estimated causal effect parameter is informative for the larger Scandinavian population. In other words, the inconsistency threats related to each method and the sample particularities do not seem to play a huge role here.

Fourth, the causal effects are relatively small. All reviewed studies find that cross-sectional estimates are substantially larger than the causal estimates, thus revealing the importance to control for selection. In Scandinavian countries, the causal estimate of 0.10 indicates that the child stays in school for one more year for every ten additional years of parent's schooling. This is a very small effect.

Fifth, the causal parental schooling effects, albeit small, represent a large part of the nurture component. A comparison with the Scandinavian intergenerational schooling associations that range from 0.20 to 0.30 suggests that more than half of the schooling persistency across generations is driven by inherited abilities. For the smaller half, however, we think that the evidence laid out in our review clearly shows that parental schooling matters. As we have argued, the adoption estimates constitute the combined effect of parental schooling and other forms of nurture and, given that our estimated coefficients for Swedish-born adoptees are very similar to those based on the other methods, we must conclude that parental schooling is responsible for most of the nurture effect. From a policy perspective, this is an encouraging conclusion. Parental education is probably more easily malleable than other nurture inputs, such as parenting skills for example, and thus provides a viable option for policy makers that wish to affect child outcomes.

Overall, the shift from cross-sectional estimates to a focus on causal effects in the intergenerational schooling literature has been successful in that we now know much more about the component of the intergenerational transmission caused by parental education. From our perspective, the roadmap for future research lies in a better understanding of the mechanisms that explain how parental schooling is passed on to the next generation. One natural mechanism to propose is income—higher education leads to higher parental resources that can be used to invest in children's education. But education could also affect characteristics such as parenting style and patience that in turn influence child outcomes. Parents are probably also the most important role models that you can think of, and education can be passed on by this mechanism if children seek to reach the educational achievements of their parents. Learning more about these mechanisms is a challenge for future research.

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