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The intergenerational transmission of education. A meta-regression analysis

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ABSTRACT

In this article, we evaluate the extent of the causal effect of parental education on the education of their children. We review this empirical literature and propose a multivariate meta-regression analysis. Our database is composed of a large set of both published and unpublished papers written in the period 2002–2014. The articles considered differ in the data sources, explanatory variables, econometric strategy applied, and the type of publication. In spite of the large heterogeneity of studies and evidence for publication bias, we find a transmission of education from parents to their children that amounts to 0.15.

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1. Introduction: evaluating the causal impact of parents' education on that of their children

In this article, we evaluate the extent of the effect of parents' education on that of their children by using a quantitative and innovative method, the meta-regression analysis, to review and analyze the empirical estimates of the existing works that deal with this matter.

Indeed, within the literature that deals with education transmission from parents to their children, there are at least three kinds of streams (Bjorklund and Salvanes, 2011). One of these strands tries to answer to the following question: how important the family background is for final educational attainment? In this case, studies compute sibling correlations to try to tell how much of the inequality in educational attainment is accounted for by factors that siblings share. Given the importance of family backgrounds in explaining educational outcomes, two other strands focus on mechanisms that explain children's outcomes such as education, income or health. The first stream tries to disentangle the relative importance of 'nature' (genetic) and 'nurture' (environmental transmission) factors; it tries to compute the part of variations in schooling that is either due to genetic factors, or to environmental factors shared by siblings or by individual factors not shared by siblings. Nevertheless, such a job is not an easy one because of the possible presence of interactions between nature and nurture. Moreover, within the framework of this approach, causal interaction effects between nature and nurture are often ruled out. The second stream is the most recent. Since the beginning of the 2000s, a large strand of empirical work has tried to evaluate the causal impact of parents' education on that of their children. The question is whether higher parental education causally affects their children's own, or whether there are other confounding factors, such as genetic or other pre-birth effects that create the strong cross-sectional relationship between parents and children. Indeed, empirical studies show that (raw) intergenerational *correlations* related to education amount to about 0.4 for Western Europe, 0.46 for the United States, and 0.6 for South America (Black and Devereux 2011)

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while *causal* estimates of the effect of parents' education on children's schooling usually exhibit smaller values (Holmlund, Lindahl, and Plug 2011). Beyond correlations between education levels of parents and children, an important question is how parental education impacts children's education through the education production technology. As it is mentioned in Holmlund, Lindahl, and Plug *(*2011), 'what are the responsible technologies and inputs in the womb and postnatal years that affect child outcomes?'. The literature refers to this investigation as estimating an intergenerational causal effect of parents (Lochner 2008; Holmlund, Lindahl, and Plug 2011). This 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 (Bjorklund and Salvanes, 2011; Black and Devereux 2011; Holmlund, Lindahl, and Plug 2011): the use of sample data for twins as parents, for adoptees, or application of instrumental variables (IV) strategies where educational reforms have commonly been used as instruments for education.

However, debate remains open on the size of the causal effect of parental schooling. There is a large uncertainty on the 'real' value of the coefficient of intergenerational transmission of education or of intergenerational causal effect: a large range of values exists for this coefficient, notably depending of the method used for the estimates. The meta-regression analysis suggests an approach that could be considered as a quantitative survey of the literature (Stanley and Jarrell 1989; Stanley 2005; Stanley et al. 2013). In fact, empirical studies that deal with causal effects of parental education on that of their children are characterized by three main features: *(i)* a large heterogeneity in the datasets used in the literature on the effect of parental schooling; *(ii)* a large variation in the covariates included in the analysis or the econometric methods in use; *(iii)* a potential 'publication bias' in this field of literature. According to Stanley (2008), publication bias is '*the consequence of choosing research papers for the statistical significance of their findings*' (p. 104), which may results from behaviors of researchers, reviewers and/or editors. Such features entail large and biased variations in results found in the empirical estimations. Besides, the meta-analysis represents in particular a framework '*to review more objectively the literature and explains its disparities*'[1] (Stanley and Jarrell 1989, 168). Indeed, as mentioned in Stanley (2001), the purpose of multivariate MRA is to model, estimate and explain the excess variation among reported empirical results. MRA offers a methodology in which to understand the research process itself and to map the effect of the researchers' choices about data, estimation techniques, and econometric models onto a research literature.

Consequently, our article proposes the first meta-regression analysis (MRA) to a large set of empirical studies (2002–2014) that evaluate the transmission of human capital from parents to their children. This research differs from the previous reviews of the literature like those of Bjorklund and Salvanes (2011), Black and Devereux (2011) or Holmlund, Lindahl, and Plug (2011) under narrative form[2], because it takes the form of a quantitative survey of the literature based on the existing empirical works. It also brings together both published and *unpublished* papers as long as it follows some strict rules for the inclusion of articles in the set of works considered for the analysis (Stanley et al. 2013). In this paper, the aim is to get a value for the 'true' effect of parents' education on that of their children taking account of the heterogeneity of empirical studies and of potential publication bias (Stanley and Jarrell 1989; Stanley 2005). The existing empirical studies are characterized by several features including the population group considered, the explanatory variables employed, the econometric strategy, the data sources, and the type of publication. These features may explain why the results of these studies differ (Stanley and Jarrell 1989; Stanley 2001). On the other hand, as Begg and Berlin (1988) point out in the case of medical studies, positive results are more likely to be published than studies showing negative results. More generally, published results may overstate or understate the true effect (Stanley and Jarrell 1989; Ashenfelter, Harmon, and Oosterbeek 1999; Doucouliagos and Stanley 2009; Havranek and Irsova 2011).

We contribute to the literature on three levels. First, we show that previous empirical studies have given rise to a large range of values of the education transmission coefficient due to differences in the population studied, the explanatory variables included, the econometric strategy, data sources, and characteristics of the publications. Second, we test for publication bias in the literature on the causal impact of parental schooling on children's schooling. Third, we provide evidence of a genuine empirical effect of parental schooling on children's schooling, net of potential publication bias and heterogeneity of the studies.

The paper is organized as follows. Section 2 presents the motivations of the paper. Section 3 displays the meta-regression analysis dataset and shows heterogeneity in considered empirical studies. Section 4 first displays some evidence on publication bias, and second considers the multivariate meta-regression analysis framework to provide new evidence for the causal effect of parental education on children's one. Section 5 concludes.

2. Evaluating the causal impact of parents' education on that of their children

In the empirical literature dealing with the estimation of individual human capital accumulation (see *e.g.* Mulligan 1997), a child's education attainment is explained by a large set of individual, familial and other environmental variables. The following basic equation can be estimated:

$$EDU_c = \alpha + \beta EDU_p + \gamma X + \varepsilon \quad (1)$$

where EDU_c is the child's education attainment and EDU_P is the parents' education attainment; X is a set of control variables for individual (*e.g.* gender or date of birth) or familial (*e.g.* parents' income, rank among siblings, grandparents' education) features, or geographical variables. ε is the error term that represents all what is unobserved and also explain EDU_c namely heritable endowments of both parents, talents for child-rearing and parental skills, as well as a child-specific characteristic (Bjorklund and Salvanes, 2011).

β is the coefficient of intergenerational causal effect or transmission of education (or ETC, education transmission coefficient in this article). It refers to the transmission of parents' schooling to their child. In this article, we focus on this coefficient which corresponds to the (intergenerational) causal effect of parents' education on that of their children in many existing studies.

Estimating Equation (1) to get the causal effects raises at least two main questions.

On the one hand, one relevant question related to the estimation of Equation (1) is the following: should both parents' education be included on the right-hand side of the equation? There is a relative consensus in the literature on the answer whether the spouse's schooling should be included or not, which depends on the goal of the study: what is the question raised? (Bjorklund and Salvanes, 2011; Holmlund, Lindahl, and Plug 2011). For instance, do we want to evaluate the intergenerational transmission of education of fathers on that of their children, controlling for assortative mating? If only one parental education is included in the equation, there might exist some difficulty in interpreting the related ETC because of the 'assortative mating' phenomenon. It refers to the inclination for one spouse to mate with somebody which has similar socio-economic status (education, income, for instance). This feature may indeed entail large collinearity between parents schooling (Holmlund, Lindahl, and Plug 2011).

On the other hand, another relevant question is: what kind of identification strategy should be used? As mentioned for instance in Bjorklund and Salvanes (2011) or in Holmlund, Lindahl, and Plug (2011), since the beginning of the 2000s, three main methods are implemented to estimate the causal effect of parental education on their child's education: the use of an instrumental variables strategy, the use of twins data and the use of adoptees data. It is mainly due to the fact that OLS on samples of children and their biological parents may in most cases entail biased econometric estimates, and this even in spite of a (potentially) large set of control variables.

The first identification strategy to estimate the coefficient β uses instrumental variables (IV). The related studies generally exploit the existence of natural experiments provided by educational reforms in compulsory schooling that create exogenous variation in parental education. It allows estimating the causal effect of parental education on children's education. The blueprint of these works is the influential study by Black, Devereux, and Salvanes (2005) that uses educational reforms from 1960 to 1972 in Norway, extending the 'period' of compulsory schooling from the seventh grade to the ninth grade. The authors found strong OLS relationships, but no significant impact (at the 5% level) of parental education considering IV, except for the mother–child relationship of the less educated parents. IV represents an interesting strategy, and with the ability of being replicated for a large number of countries that experienced similar extension of the period of compulsory schooling.[3]

However, this method presents some potential flaws. First, one major failure is the risk of choosing non relevant instruments: the instruments must be exogenous, and also much correlated with the treatment variable (Wooldridge 2002). More specifically, most of the literature considers compulsory schooling laws for the secondary school; other instruments are sometimes used (grandparents education, parental birth order). Second, as mentioned in Black, Devereux, and Salvanes (2005), '*It is plausible that a policy change that increased enrollment in higher education would have been transmitted more successfully across generations*' (p. 447): IV estimates could refer to local average treatment effects ('LATE') and may not apply to all kinds of enlarged education policies. In particular in the case where mandatory reforms are used as instruments of parental education, the causal impact that is evaluated must be considered as an effect for the group of people who are influenced by mandatory reforms (that have extended education) and are most likely at the bottom of the educational distribution. Third, '*the asymptotic variance of the IV estimator is always larger, and sometimes much larger, than the asymptotic variance of the OLS estimator*' (Wooldridge 2002, 467).

The second main identification strategy uses data on adoptees. In this approach, the idea is to capture the effect of parental education on the schooling of children who are not their biological children. Hence, this strategy would allow estimating causal impact of parental schooling net of genetic transmission. Studies in this field use OLS on the sample of adoptees and typically find smaller (and not always significant) values for the ETC of adoptees than for biological children. Hereafter we refer to this strategy using the term 'OLS-adoptees'. For instance, Plug (2004) or Sacerdote (2007) use such a strategy. Plug (2004) uses US data (the Wisconsin Longitudinal survey) and finds that heritable abilities ('nature') play an important role in intergenerational transmission, with no causal effect ('nurture') of parental education on adoptees. The study by Sacerdote (2007) mobilizes a Korean American dataset and finds a far smaller – but significant – transmission of education coefficient from the considered parents to her/his adopted child relatively to the biological child.

More generally, despite the sophisticated nature of data, there seems to exist some potential flaws in that strategy. First, the strategy relies on the assumption of randomly assigned parents: the adoptive parents should not be a strongly selected group of parents relatively to rest of the population (Bjorklund and Salvanes, 2011). Second, the size of the considered samples of adoptees is often small: such datasets contains information about a few hundred of individuals and sometimes even less. Third, the age of the adoption matters on educational outcomes. Indeed, especially for adopted children who are not born in the country where they are raised (but not only), there is some acculturation at work (Rumbaut 2004), depending on the age of arrival (here adoption) that may largely influence education. Fourth, this approach allows eliminating confounding genetic effects but unobserved child-rearing endowments remain; because of a positive correlation between the mother's education and child-rearing endowments, these will potentially entail upward bias; it may thus be difficult to disentangle nature from nurture.

The third and last main identification strategy relies on the use of data on twins. This approach also aims at 'controlling' for the unobserved effect to identify differences in schooling of twin parents, by removing the genetic heritable transmission. Studies using this approach typically present OLS estimations and difference between twins (WITHIN estimator). Hereafter we refer to this strategy using the term 'WITHIN-twins'. For instance, Behrman and Rosenzweig (2002) use US data from the State of Minnesota and find significant OLS estimates of ETC for twin fathers and twin mothers. They find significant impact of father's education but not of the mother's by using the WITHIN estimator. Holmlund, Lindahl, and Plug (2011) use data from Sweden and find similar results, except for the case where they take account for assortative mating.

This identification strategy also suffers from potential flaws. First, non-randomness in the educational choices of the twin is a criterion that has to be met in order to have adequate identification (Bjorklund and Salvanes, 2011). Second, it seems more relevant to use data on monozygotic twins instead of (fraternal) dizygotic twins: in the latter case, the twins share different genetic codes which may introduce additional (upward) bias in the ETC estimation because these differences between the twins is likely to contribute to a nonzero correlation between differences in unobserved inheritable endowments of parents and education of parents (Bjorklund and Salvanes, 2011). Third, other problems may also occur, like measurement errors in education level. Finally, as mentioned in the second approach, the rather small size of the datasets and the possibility of selection in these samples also represent serious potential flaws that may limit the generalization of the results elaborated with such data.[4]

Overall, a large range of values for the estimates of the ETC exists in the literature. This feature is confirmed by studies that implement the three different identification strategies on a same dataset (e.g. Holmlund, Lindahl, and Plug 2011 on Swedish data). One solid conclusion from the literature using the three main estimation strategies exposed present values for the ETC that are lower than the coefficients found with (simple) OLS. Meanwhile, there is no clear further conclusion in the results. The estimation value of the ETC varies notably with the econometric strategy, with the data (country, for instance), or with the parent (father or mother?)[5] considered for the intergenerational transmission. All these different features provide support for the implementation of meta-regression analysis that would propose an estimation of the 'true effect' of parental schooling on children's education.[6]

3. Meta-regression analysis: a dataset of education transmission coefficient estimates

In this section, we discuss the empirical framework and present the data set on which our MRA is based.[7]

3.1. Studies included in the MRA dataset

All empirical works that estimate an intergenerational coefficient of transmission of education are candidates for inclusion in the meta-regression analysis. To collect the set of studies to be included, we consider works that aim at evaluating the causal impact of parental education on children's education. Thus, we consider papers that adopt one of the three identification strategies aforementioned.[8] Moreover, within a given paper among those we include in our file drawer, we won't consider some results as soon as they are used as a comparison basis. This is mainly the case of regressions that use OLS to get estimates of the beta coefficient: OLS that are used in IV studies, or that applied to own birth child in studies on adoptees or to twins samples.

Besides, to be included the studies should report an explicit value of the effect of parental education on children's education. Parents' and children's education levels should be expressed in years of schooling. Completed years of education are the basis for a large body of empirical work on the individual transmission of education.[9] The coefficient of intergenerational transmission should not correspond to an elasticity: most of this literature does not express years of education in logarithms. Finally, studies which use degree level as the education variable cannot be included because the econometric results in these ordered logit/probit models are not directly comparable with those obtained in linear models (years of schooling).

We performed several searches of scholarly databases and other internet searches between December 2013 and February 2014, using a large set of keywords closely related to the topic of impact of parental education in the human capital literature.[10] First, we searched EconLit databases (Cairn, JSTOR, Science Direct, Springer Link) for published academic papers. Second, we extended the search to specialized research institution websites (IZA, NBER, SSRN) for working papers or research reports on labor/education economics. Third, we searched numerous Google web page results to identify work in progress and other non-published research. Fourth, we tried to ensure that no relevant work was overlooked by searching the references in the selected papers. Where different versions of the same paper exist, we consider the published or most recent version available. We consider only papers with cross-sectional data, *i.e.* individual observations covering a fairly large range of birth cohorts, such as samples representative of a population or specific samples (*e.g.* twins or adopted children) but not one or a small number of cohorts. The final dataset was checked for coherence and for possible errors in the coding of the different variables.

Our final dataset contains information on 23 articles published or written in the period 2002–2014. This set of 166 estimates of the education transmission coefficient corresponds to effect size (Stanley and Jarrell 1989). A given effect size corresponds to an estimate of the intergenerational transmission of education from parents to their children in the estimation of Equation (1).

Table 1 presents the articles in our dataset. We find an average of 7 estimated values for the effect size for each study. The average coefficient in the empirical studies is 0.147. In comparison, corresponding average associations amount to 0.211. Those associations are provided in every articles of the file drawer and are obtained as follows. They correspond to *(i)* OLS estimates instead of IV in the case of natural experiment (first identification strategy), *(ii)* OLS estimates using ownbirth children and their biological parents (second identification strategy), and *(iii)* OLS instead of within estimates using the sample of twins (third identification strategy).

3.2. Descriptive statistics: heterogeneity in estimated values of ETC

The estimated coefficient of parental transmission of education is provided for a specific estimation within a given study. In particular, this coefficient is related to a specific survey, a given population of children or parents, a given set of control variables included when estimating the effect size for a

Table 1. Studies included in the meta-regression analysis.

Author(s)	Nb. of effect sizes in study	Average effect size	Average estimated association
Antonovics and Goldberger (2005)	2	0.254	0.385
Amin, Lundborg, and Rooth (2011)	28	0.079	0.203
Agüero and Ramachandran (2010)	2	0.078	0.089
Behrman and Rosenzweig (2002)	10	0.058	0.276
Bingley, Christensen, and Jensen (2009)	8	0.057	0.114
Black, Devereux, and Salvanes (2005)	6	0.054	0.227
Björklund, Jäntti, and Solon (2007)	6	0.058	0.172
Björklund, Lindahl, and Plug (2004)	12	0.099	0.197
Bjorklund, Lindahl, and Plug (2006)	4	0.093	0.203
Havari and Savegnago (2013)	6	0.458	0.263
Hoffman (2013)	6	0.030	0.075
Holmlund, Lindahl, and Plug (2011)	12	0.071	0.198
Kallioniemi (2014)	4	0.044	0.249
Lindahl et al. (2013)	2	0.315	0.265
Meng and Zhao (2013)	15	0.330	0.187
Plug (2004)	6	0.196	0.344
Pronzato (2012)	2	0.127	0.228
Plug and Vijverberg (2005)	6	0.158	0.250
Sacerdote (2000)	2	0.192	0.316
Sacerdote (2004)	3	0.056	0.275
Sacerdote (2007)	2	0.093	0.315
Stella (2013)	10	0.410	0.276
Tsou, Liu, and Hammitt (2012)	12	0.075	0.134
Sample average	**7.2**	**0.147**	**0.211**

Sources: Authors'compilation. See Appendix for full references.

given econometric estimator, and/or a particular type of publication. Therefore, a given estimated value of the education transmission coefficient might be linked to all of those specific features.

In what follows, we propose a definition and coding of the moderator variables, *i.e.* variables that describes empirical studies and may be relevant for explaining why effect size β_j might differ across empirical studies.

Those meta-independent variables belong to one of seven groups of variables:

- *Data type*: contains general information on data sources (country surveyed).
- *Children*: provides information on characteristics (boy, girl or both genders) considered to get estimate of β.
- *Parents*: provides general information on the parent considered (*e.g.* mother, father or both) to get estimate of β.
- *Socioeconomic control variables*: to indicate whether or not some control variable are included in the econometric specification to arrive at a given estimate of β. The control variables include children's characteristics (age, gender) or their family's characteristics (household income, number of siblings, for instance). Geographical indicators are also taken into account.
- *Estimator*: refers to the econometric methodology used in the research (OLS on sample of adoptees, Within on sample of twins or IV strategy).
- *Publication characteristics:* characterizing the type of publication: academic journal, working paper, book chapter, conference proceedings.

Appendix Table A1 provides definitions and sample statistics (means and standard deviations) for those variables for the full set of publications in our meta-database.

Table 2 reports the mean difference between the effect sizes for the target group and the effect sizes for the remaining (reference) group of estimated effect sizes for every moderator variable that corresponds to a dummy variable.

For instance, relatively to any other group of countries, the effect size on average is greater for estimates that rely on America related data. Also, relative to other children, effect size is smaller for girls. The same holds when considering ETC evaluated for mothers. Besides, effect size tends to be larger among estimations that include (in the specification of the estimated equation) variables related to the gender of the individual, or the professional status of her / his parents. On the contrary, taking account in the list of the explanatory variables of factors related to the birth of parents or the education level of the spouse (assortative mating) of the considered parent seem to be negatively correlated with the estimated ETC. Effect sizes are also lower on average for estimates based on the WITHIN estimator (thus while considering estimates on twins as parents), or on the OLS estimator (thus using data on adoptees); the contrary holds for IV estimates, in comparison with OLS or WITHIN estimates. Hence, effect size varies with the characteristics of studies in most cases.

The next step in our analysis is multivariate meta-regression to take account of this heterogeneity.

4. Exploring heterogeneity from education transmission coefficient: a multivariate meta-regression analysis

In Section 3, we showed that the set of studies considered displays a large range of values for the effect size (see Table 1). We also show that all those studies are characterized by several specific features that may explain why estimations of the ETC might differ (Table 2).

In this section, we first provide some evidence for potential publication bias, while estimating the causal effect of parents' education on that of their children, considering funnel plots asymmetry and testing for it (Egger et al. 1997; Sutton et al. 2000a, 2000b; Stanley 2005, 2008). Second, we use Multivariate Meta-Regression Analysis (Stanley and Jarrell 1989; Stanley 2001; Doucouliagos and Stanley,

Table 2. Difference in the mean effect size by characteristics of the study.

Variable name	Difference[a]	Significance[b]
Data type:		
Africa	−0.070 (0.041)	0.303
America	−0.079 (0.030)	0.023**
Asia	0.038 (0.027)	0.164
Europe	−0.017 (0.025)	0.507
Children:		
Boy	0.026 (0.032)	0.423
Girl	−0.075 (0.031)	0.021**
All gender	0.031 (0.026)	0.227
Parents:		
Mother	−0.080 (0.024)	0.001***
Father	−0.032 (0.022)	0.152
Both parents	0.252 (0.028)	<0.001***
Socioeconomic control variables:		
Gender	0.074 (0.024)	<0.001***
Age/Birth	−0.032 (0.038)	0.422
Number of siblings	0.124 (0.039)	0.003***
Rank among siblings	0.007 (0.038)	0.846
Assortative	−0.065 (0.024)	0.006***
Birth parents	−0.113 (0.018)	<0.001***
Professional status	0.306 (0.050)	0.019**
Income	0.063 (0.042)	0.147
Local	0.098 (0.028)	<0.001***
No covariates	−0.033 (0.135)	0.822
Estimator:		
OLS (on adoptees)	−0.072 (0.020)	<0.001***
IV	0.181 (0.028)	<0.001***
Within (on twins)	−0.090 (0.022)	<0.001***
Publication characteristics:		
Academic	0.067 (0.025)	0.789

Note: Population: full sample (all publications).
[a]Difference refers to the mean difference between the effect sizes for the target group and the effect sizes for the remaining (reference) group.
[b]*P*-value (probability to reject the alternative hypothesis) for the statistical significance of the group difference.
*** (resp. ** and *) stands for significance at a 1% (resp. 5% or 10%) level.

2009; Cipollina and Salvatici 2010) to get the (genuine empirical) causal effect of parents' education, while controlling for the heterogeneity of studies.

4.1. Publication bias: funnel plots and asymmetry testing

The literature that deals with meta-regression analysis first often provides evidence on publication bias**.** Publication bias may be defined as '*the consequence of choosing research papers for the statistical significance of their findings*' (Stanley 2008, 104), which may results from behaviors of researchers, reviewers and/or editors. For instance, Begg and Berlin (1988) in the case of medical studies show that papers that provide positive results (*i.e.* indicating a positive effect of the 'treatment') are more likely to be published. More generally, and particularly in economics, these different features entail that published results can overstate or understate the true effect (Stanley and Jarrell 1989) such that the estimated effects of parental schooling might be correlated with sampling errors. If these effects are correlated with other variables, then the conclusions about the determinants of children's schooling may be seriously biased. The existence of such bias is due to the natural workings of a scientific process designed to discover important new results (Ashenfelter, Harmon, and Oosterbeek 1999).

Funnel plot is a first approach to detecting publication bias (Stanley 2005; Sutton et al. 2000b). For all the studies in a MRA dataset, it displays an empirical relationship between the estimated beta coefficient and its precision (usually the inverse of the standard error estimate). The intuition is that in absence of publication selection, estimates will vary randomly, hence symmetrically, around the true effect (Stanley 2008, 107). As defined in Sutton et al. (2000b), a funnel plot is a plot of each trial's effect size against some measure of its size, such as the precision, the overall sample size, or the standard error. These plots are referred to as 'funnel plots' because they should be shaped like a funnel if no publication bias is present. This particular shape is expected because trials of smaller size (which are more numerous) have increasingly large variation in the estimates of their effect size as random variation becomes increasingly influential. It follows that such variation are observed in reference to the true 'effect', *i.e.* the real value of the estimated coefficient if no bias applies. However, since very frequently, smaller or non-significant studies are less likely to be published, trials in the bottom left hand corner (when a desirable outcome is being considered) of the plot are often omitted, creating a degree of asymmetry in the funnel. Because small-sample studies with typically larger standard errors and hence less precision are at the bottom of the graph, the plot will be more spread out at the bottom than it is at the top. Otherwise, the distribution does not need to contain both positive and negative correlations; a funnel plot can be symmetrical with all positive (or negative) valued observations as it is sometimes the case (Abdullah, Doucouliagos, and Manning 2015). Hence, Sutton et al. (2000b) refer to an overweighted plot on one side the left or another on the right around what would be the true effect of parental education could be a sign of the existence of publication selection, respectively negative (under-estimation of the effect) or positive (over-estimation of the effect).

Thus, we first perform funnel plots on the whole sample, and then we consider two article type sub-samples: academic publications, and unpublished papers (including working papers). Figure 1 shows that there may be some publication bias: considering the whole sample leads to an over-weighting on the right side, even if it is not so clear-cut. This appears to be due to the effect of 'unpublished papers', for which there are also sometimes large values for effect size, but also for which precision is often greater than for academic papers, even for estimated effect of small size.

However, funnel plots are only graphs that represent the empirical relationship between an estimate and its precision. The presence of asymmetry in the shape of the funnel would indicate possible publication bias. This graph asymmetry can be formally tested through the funnel asymmetry test (FAT, Stanley 2005). The starting point for FAT is the relationship between the reported coefficient of parental transmission of education and its standard error (Egger et al. 1997):

$$b_j = \beta_1 + \beta_0 SE_j + u_j. \tag{2}$$

Where b_j denotes the estimated coefficient of transmission of education from parents to their children. This coefficient is reported in the j^{th} study in our final dataset ($j = 1, 2, \ldots N$). SE_j is the standard error of b_j, and u_j is a random error term. If there is no publication bias, the estimated effects should vary randomly around the true value β_1 of parents' effect on that of their children. The FAT consists of a *t*-test performed on the intercept (β_0). If β_0 is different from zero, there is evidence for funnel asymmetry, and therefore publication bias.[11] The sign of the estimate of β_0 indicates the direction of this bias. Thus testing H_0: $\beta_0 = 0$ becomes a test of funnel asymmetry (Sutton et al. 2000a). Otherwise, testing H_0: $\beta_1 = 0$ becomes a test for the existence of empirical effect of parents' education on that of their children (Precision Effect Testing, PET, see Stanley (2005)).

Equation (2) is heteroscedastic (Stanley 2008). Since a measure of this heteroscedasticity is available (namely the standard error of the given estimate of the effect, SE_j), weighted least squares (WLS) are recommended to obtain efficient estimates (Maddala 1977). The following equation is thus estimated by OLS:

$$t_j = \beta_0 + \beta_1 (1/SE_j) + \varepsilon_j \tag{3}$$

Where $t_j = b_j/SE_j$ and $\varepsilon_j = u_j/SE_j$, and the intercept and the slope coefficient are now reversed. However, as reported for instance in Cipollina and Salvatici (2010), coefficients provided by Equation (3) may still lead to inefficient (though consistent) estimators because it does not consider the dependence of estimates that are displayed in a given study. To take account for this, 'robust with cluster'

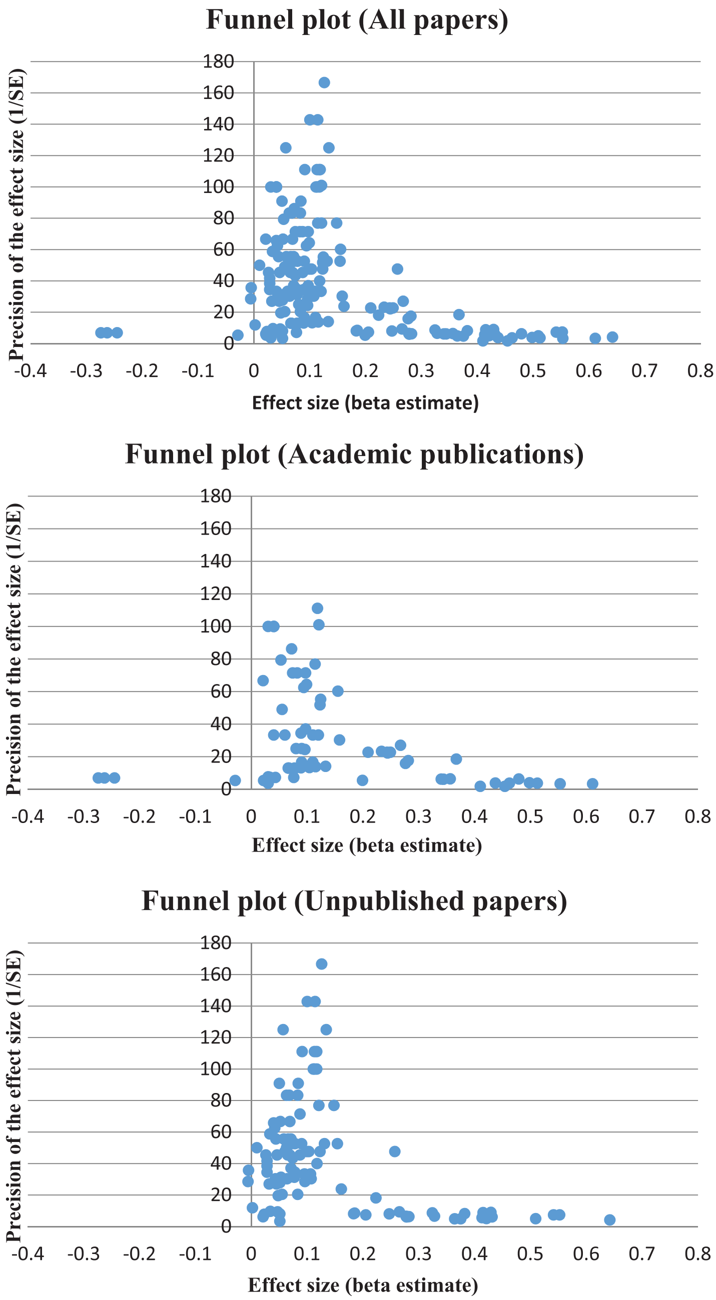

Figure 1. Funnel plots for the intergenerational transmission of education, different sub-samples of observations.

procedure is adopted, adjusting standard errors for intra-study correlation (Sterne, Gavaghan, and Egger 2000; Macaskill, Walter, and Irwig 2001).

As mentioned in Egger et al. (1997), FAT is characterized by a low power. To take account for the fact we do not have a necessarily large sample of effect sizes at hand, we thus proceed to FAT at a 10 percent level.[12] As reported in Table 3, there seems to be no publication bias because β_0 is not statistically significant. Nevertheless, publication bias may be more linked to a particular sample of

Table 3. Funnel Asymmetry and Precision Effect Testing.

Moderator variable / Considered sample	Full sample	Academic publications	Unpublished papers
Intercept	0.565	1.135*	0.097
	(0.473)	(0.541)	(0.900)
1/Se	0.083***	0.065**	0.093***
	(0.013)	(0.022)	(0.013)
Sample size: number of estimates	166	72	94
R2	0.626	0.472	0.697

Notes: clustered standard error within parentheses. Cluster is equal to the number of estimates in a given study.
*** (resp. ** and *) stands for significance at a 1% (resp. 5% or 10%) level.

articles. That is why we also report FAT on both published and unpublished papers: the first sample seems to exhibit positive publication bias. This finding motivates the fact that we proceed to our meta-regression analysis considering the full sample of both published and unpublished articles.[13] Otherwise, β_1 is always statistically significant; it therefore indicates there is a non-zero effect of parents' education on that of their children.

4.2. Multivariate MRA and genuine empirical effect

So far, to get an estimate of the causal effect of parental education, we also have to take account of heterogeneity of studies. Indeed, this may be of importance because Table 2 in Section 3 shows that effect sizes seem to differ across studies according to several features of articles. The multivariate meta-regression approach generalizes the FAT analysis. It allows us to estimate the 'true' (or 'genuine') effect of parental education on children's education, *i.e.* net of heterogeneity of the studies and publication bias (Stanley 2005).

To proceed, we generalize Equation (2) by adding the moderator variables, *i.e.* dummies that take account for features of the considered studies, divide all these variables by the effect size standard error (SE_j) to take account for heteroscedasticity and thus apply WLS (Stanley, Doucouliagos, and Jarrell 2008):

$$t_j = \beta_0 + \beta_1(1/SE_j) + \sum_{k=1}^{K} \alpha_k(Z_{jk}/SE_j) + \varepsilon_j \tag{4}$$

where Z_{jk} are the K moderator variables or meta-independent variables (Stanley 2001). Their coefficients refer *ceteris paribus* to difference in terms of estimated education transmission coefficient between studies that are characterized by the given criterion ($Z_k = 1$) and the reference group.[14] β_1 represents the 'true' value of the transmission coefficient or intergenerational causal effect, once heterogeneity of studies is taken into account (that is β_1 is conditional on the values of Z), and corrected for publication bias (represented by β_0) that is measured for the reference group ($Z_k = 0$) (Havranek and Irsova 2011). Finally, u_j is the meta-regression disturbance term. Equation (4) is estimated using OLS, while still considering clustered standard errors at the study level.

4.3. Results

Table 4 presents evidence of publication bias and empirical genuine effect after controlling for heterogeneity of empirical studies.

First, the most important scientific issue when considering the transmission of education from parents to their children is the size of the 'genuine empirical' effect. Table 4 shows a value for estimated β_1 coefficient at 0.146 (and significant at the 5% level). This value does not depend on the characteristics of studies (kind of sample, data type, control variables under consideration, econometric estimator, and so on), as well on the fact that the considered articles are published or not.

Table 4. Multivariate Meta-Regression Analysis of the effect of parental education on children's education.

Moderator variable	Estimates
Intercept	0.1106 (0.514)
Precision (1/Se)	0.146** (0.064)
Data type:	
Africa	−0.119 (0.076)
America	Ref.
Asia	0.087 (0.052)
Europe	−0.037 (0.053)
Children:	
Boy	−0.022 (0.056)
Girl	−0.035 (0.057)
All gender	Ref.
Parents:	
Mother	−0.026* (0.014)
Father	−0.001 (0.017)
Both parents	Ref.
Socioeconomic control variables:	
Gender	−0.029 (0.055)
Age/Birth	0.090** (0.038)
Number of siblings	−0.016 (0.012)
Rank among siblings	−0.025 (0.033)
Assortative	−0.031*** (0.005)
Birth parents	0.035 (0.027)
Professional status	0.320*** (0.075)
Income	0.036 (0.036)
Local	−0.041 (0.040)
No covariate	0.063 (0.066)
Estimator:	
OLS (on adoptees)	−0.121* (0.071)
IV	Ref.
Within (on twins)	−0.097 (0.065)
Publication characteristics:	
Academic	Ref.
Unpublished	−0.034 (0.037)
Number of estimates	166
R^2	0.823

Notes: Dependent variable is the t-statistic (effect size related to effect standard error). WLS estimates with clustered standard errors are computed at the study level. Some publication variables (fields of research, number of citations and social science impact factors) are only available for journals (academic publications).

Second, we also examine the potential existence of publication bias. As mentioned in the previous sub-section, within the framework of the multivariate MRA approach publication bias is measured for the reference group by the intercept. According to Table 4, estimated intercept is not significant. It indicates no publication bias for the reference group.

Third, the results displayed in Table 4 provides evidence for the fact that the empirical effect size (*i.e.* each value of the effect found in the existing studies) is partly explained by heterogeneity among studies that analyze the impact of parents' education on children's education. For instance, *ceteris paribus*, effect size is smaller if the education transmission coefficient is related to the mother of the considered individual. Among the socioeconomic control variables, effect size is smaller in studies that take account for assortative mating. On the contrary, effect size is greater in articles where estimations include as explanatory variables information related to the age of the individual, or related to the professional status of her/ his parents. Moreover, *ceteris paribus*, effect size is lower among estimates that were obtained using the OLS-adoptees (thus based on a sample of adoptees) estimator rather than considering the IV estimator.

Overall, we find a significant and positive genuine empirical effect of parental education children's education, irrespective of publication selection and heterogeneity of studies.

4.4. Discussing the main results

The FAT-MRA regressions give evidence for a causal effect of parental schooling on their children's education. The transmission education coefficient is estimated to around 0.15, which is of significant magnitude and in the MRA literature corresponds to the 'true' or 'genuine' empirical effect of the interest variable (Stanley 2005). On the one hand, corresponding average estimated associations over this sample of studies amounts to 0.21. Estimated intergenerational causal effect of education is thus equal to two thirds times the amount of average estimated associations that refer to estimates where OLS are applied to sample of ownbirth children, twins or for sample where the IV strategy is considered.

On the other hand, the 0.146 coefficient is not significantly different from the average effect size provided by the articles included in the file drawer under consideration for this evaluation (*i.e.* 0.141 – see Table 1). However, descriptive statistics in Table 2 show that a large number of moderators are significantly related to the estimated coefficient of parental transmission of education. Hence, the heterogeneity of studies explains a large part of the variation in the coefficient of parental transmission of education in related empirical studies. Moreover, there is hardly any evidence for publication bias.

Thus our results show it was important to consider meta-regression analysis to provide new evidence on the causal effect of parental education on that of their children.

5. Conclusion

In this article, we provide new evidence for the causal effect of parental education on children's education, *ie.* the intergenerational causal effect. Indeed, since the beginning of the 2000s there is a growing strand of literature that aim at evaluating this effect. Corresponding articles are characterized by a large range in estimated values of the education transmission coefficient. Thus we built a set of empirical studies that aim at evaluating the causal effect of parental education on that of their children. We then conducted a multivariate meta-regression analysis to estimate the causal effect of parents' schooling on that of their children, irrespective of the heterogeneity among the studies considered, or any potential publication bias. We found evidence of a significant and positive causal effect of parental schooling, net of potential publication bias and of the heterogeneity of the studies. This amounts to about 0.15. This is an important result for public policy actions: raising parental levels of education clearly benefit to offspring's education, through direct effect.

In our study, we focus on years of schooling. However, there are alternative measures of education. They include highest level of diploma achieved by the individual. It would be interesting to analyze the impact of parental education on the probability for her/his child of achieving a low or a high level of diploma. This will be investigated in future research.

Notes

1. Indeed, the "narrative literature reviews" could *"introduce a substantial bias by omitting portions of the literature, usually on alleged methodological grounds"* (Stanley 2001, 144).
2. The notable exception is the paper of Holmlund, Lindahl, and Plug (2011) who also propose an original quantitative analysis by implementing the three main identification strategies on a same Swedish dataset.
3. Otherwise, rather than considering compulsory schooling, alternative instrumental variables are sometimes used, like the birth order of parents in Havari and Savegnago (2013).
4. As mentioned in Bjorklund and Salvanes (2011), given the importance of family backgrounds in explaining educational outcomes, one strand of literature tries to disentangle the relative importance of "nature" (genetic) and "nurture" (environmental transmission) factors. This research stream has its roots in quantitative genetics and uses correlations among relatives with different genetic and environmental connectedness to infer the relative importance of nature and nurture for the outcome of interest. It consists in a variance decomposition approach; it tries to compute the part of variations in schooling that is either due to genetic factors, or to environmental factors shared by siblings or by individual factors not shared by siblings, and rules out any interaction between

nature and nurture. Another approach uses a regression framework with an intergenerational association between the educational attainment of parents and children, and make a distinction between biological parents (who transfer nature), and adoptive parents (who transfer nurture). However, this approach focuses only on the observed parental characteristics, whereas the variance decomposition approach focuses on factors shared by siblings, irrespective of whether they are observed factors. Within this strand of literature, Bjorklund, Lindahl, and Plug (2006) also address the problem of the existence of interaction between nature and nurture. They show that in both the regression and variance decomposition frameworks, there exist interaction effects, thus implying that a straightforward decomposition into nature and nurture is not possible or at least a very difficult job. Moreover, the conditions under which the estimated intergenerational causal effect is unbiased worsen the job. This is why we decide to focus in this article only on the search for the intergenerational causal effect of parents' education and not on disentangling nature from nurture effects, even if it would been of large interest.

5. There is also variation in the estimated ETC depending on if there is assortative mating in the estimation performed.
6. An additional and important argument for implementing of a meta-regression analysis is to control for possible "publication bias" (Sutton et al., 2000b; Stanley 2005), that has been proven as an empirical feature for a number of research questions (Ashenfelter, Harmon, and Oosterbeek 1999; Stanley 2005; Stanley, Doucouliagos, and Jarrell 2008; Doucouliagos and Stanley 2009; Havranek and Irsova 2011).
7. See for instance Stanley et al. (2013) for guidelines on this task.
8. Other ways were recently used to identify the causal effect of parental schooling on that of their children. It is for instance the case of de Haan (2011), who considers bounding the causal effect of parental education on that of their children, by using an analysis based on Manski and Pepper (2000). However, we won't consider such studies because only a limited number of results are available by now. Moreover, some articles also apply IV to twins sample (such as Behrman and Taubman (1985) or some estimates provided in Bingley, Christensen, and Jensen (2009); we also exclude these estimates from our file drawer because they refer to strategies that are also rarely used.
9. Alternative measures of education include highest diploma level achieved by the individual; this will be investigated in future research.
10. These expressions include: intra-family transmission of education, intergenerational transmission of education, educational intergenerational mobility, intergenerational education/schooling mobility, educational persistence, correlation between parents and child's schooling or education, intergenerational education correlation, intergenerational effects, intergenerational associations/transmissions, causal effect of parent's schooling on child's schooling, intergenerational schooling associations, transmission of human capital/education, causal relationship between parents' and children's education, and accumulation of human capital.
11. As mentioned in Stanley (2008), this equation can be derived from the statistical theory. Indeed, we can consider the conventional *t*-statistic: $t_j=(b_j - \beta_1)/SE_j$, for β_1 representing the true effect. This will be approximately normal in large samples. If there is strict selection for significance yet no genuine effect, then a study is published only when its t_j exceeds the critical value, t_c. Thus, observed t_js will have a truncated non-central, *t*-distribution. We may define 0 as the mean of the large sample approximation to this truncated non-central *t*-distribution. Hence, conditional on this strict selection, $\beta_0 =E(b_j - \beta_1)/SE_j$, implying Equation (2).
12. Indeed the authors mention that any analysis of heterogeneity within the framework of the meta-regression analysis should depend on the number of articles and thus of the number of effect sizes that are taken into account in the study. They thus consider testing evidence of asymmetry for a significance level smaller than or equal to 0.1 rather than 0.05 or 0.01. Even if 23 articles and 166 effect sizes are included in our file drawer, we also consider the 10 percent level as a benchmark.
13. Stanley (2008) recognizes that Equation (2) has several statistical problems. First, the MRA estimates may be biased because the standard errors (SE_j) are themselves estimates (Macaskill, Walter, and Irwig 2001). Second, publication selection is likely to cause additional problems for the estimation of MRA model (2): when only significant effects are reported, the sampling errors of the observed effects are drawn from a truncated distribution. These errors will be thereby skewed and no longer normal. Third and lastly, Equation (2) will misspecify the relationship between observed t-values and standard errors when some studies do not engage in publication selection. Nevertheless, Stanley (2008) mentions that the Monte Carlo simulations led in his article reveal that PET is surprisingly effective in separating the "wheat from the chaff".

    Moreover, the author also discusses another method to detect, namely that of Hedges' Maximum Likelihood Publication Selection Effect (Hedges 1992). It suggests a sophisticated econometric model of the publication selection process, assuming that the likelihood of publication is an increasing step function of the complement of a study's p-value. However, as reported in Stanley (2008), of the six studies that made use of it (among other: Ashenfelter, Harmon, and Oosterbeek 1999; Florax 2002; Abreu, de Groot, and Florax 2005) in the set of economic research literature, three are problematic. Among them, Abreu, de Groot, and Florax (2005) and Florax (2002) find studies with larger p-values are more likely to be published than statistically significant ones. Thus there seems to be no best solution to detect / test for publication bias.

14. In the case of our article (Table 4), the reference group refers to American people, children of all gender, both parents, IV estimates, academic publication and all the socioeconomic variables equal to zero.

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Appendix

Table A1. Summary statistics of the moderator variables in the meta-regression analysis.

Variable name	Variable Description	Mean (Standard Deviation)
Meta-Dependent variable		
T-stat	=Student t-statistic associated to the effect size.	3.76 (0.29)
Meta-Independent variables		
Estimate's accuracy:		
Inverted squared error (ISE)	=Inverted standard error (effect size precision).	38.43 (2.73)
Data type:		
Africa	=1, if the survey deals with a country in Africa.	1.20 (0.82)
America	=1, if the survey deals with a country in America.	4.82 (1.67)
Asia	=1, if the survey deals with a country in Asia.	33.13 (3.66)
Europe	=1, if the survey deals with a country in Europe.	60.84 (3.80)
Children:		
Boy	=1, if the estimated coefficient is only related to boys.	19.88 (3.11)
Girl	=1, if the estimated coefficient is only related to girls.	18.67 (3.03)
All gender	=1, if the estimated coefficient is related to both genders.	61.44 (3.79)
Parents:		
Mother	=1, if the estimated coefficient is related to the mother of the child.	44.58 (3.87)
Father	=1, if the estimated coefficient is related to the father of the child.	39.16 (3.80)
Both parents	=1, if the estimated coefficient is related to both parents.	19.88 (3.11)
Socioeconomic control variables:		
Gender	=1, if the gender is considered as a control variable.	63.25 (3.71)
Age/Birth	=1, if age or birth cohorts are considered as control variables.	79.52 (3.14)
Number of siblings	=1, if the number of siblings is considered as a control variable.	17.47 (2.96)
Rank among siblings	=1, if the rank of the individual among siblings is considered as a control variable.	10.24 (2.26)
Assortative	=1, if assortative mating is controlled for.	47.59 (3.88)
Birth parents	=1, if dummies for parents' year of birth are included as explanatory variables.	36.14 (3.74)
Professional status	=1, if professional status of parents work is included as an explanatory variable.	1.81 (1.04)
Income	=1, if income of parents is included.	18.86 (2.69)
Local	=1, if local dummies are included as control variables.	31.93 (3.63)
No covariates	=1, if no control variables are included.	2.41 (1.19)
Estimator:		
OLS	=1, if OLS estimator is considered.	36.75 (3.75)
IV	=1, if an IV estimator is considered (instrumenting parents' education).	30.72 (3.59)
Within	=1, if a Within estimator is considered (mainly for adoptees data)	32.53 (3.65)
Publication characteristics:		
Academic	=1, if the study is published in an education economics journal.	43.37 (3.86)
Unpublished	=1, if the study is unpublished.	56.63 (3.86)