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The impact of parental income on early schooling transitions
A re-examination using data over three generations

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Abstract

This paper applies new semi-parametric techniques to estimate the effects of parental income on the probability of being held back in elementary school in France. We use information on grandparents' past socioeconomic status and parents' education level to separate the parental income effects from the effects of the unmeasured determinants of children's performance at school that are correlated with parental income. When considering the probability of being held back in elementary school, we find that the effects of parental poverty are much larger than the effects of a child's sex or age (age within his/her class) or of the parents' education level. Our findings suggest that policy decisions that increase income transfers to relatively poor families have a potentially very large impact on children's early performance at school.

Keywords: Parental income; Performance at school; Elementary school; Binary choice; Semi-parametric method; Endogenous regressor

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1. Introduction

An increasing body of work based on American data suggests that the

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differences in children's performance at school are not necessarily due to the family's income level (see, for example, Shea (2000) or Mayer (1997)). According to these studies, the inequalities between children from rich and poor families are mostly due to unmeasured factors that simultaneously explain parents' income and their children's outcomes. They suggest that income transfers to poor families would not have any effect on their children's performance at school.

In this paper, we come to very different conclusions using new French data, instruments and estimation strategies. When considering the probability of being held back in elementary school,[1] we find substantial differences between boys and girls or between children born during the first half of the year (the oldest in their class) and the second half of the year (the youngest). A lot more differences exist, however, between children from rich and poor families than between boys and girls or children born at the beginning and the end of the year. According to our econometric analysis, a 10% increase in parental income is associated with about a 6.5-point decrease in the probability of being held back in elementary school.[2] In France, public policy decisions that increase income transfers to relatively poor families could have a potentially very large impact on children's early performance at school.

To obtain these results, this paper shows that (a) standard cross-sectional household surveys can be very informative concerning the schooling careers of children (i.e. children of the respondents),[3] (b) the impact of an endogenous factor (parental income) on children's schooling transitions can be analyzed without making any assumptions about the distribution of unobserved residuals using the new semi-parametric estimators for qualitative response models introduced by Lewbel (2000), (c) information on parents' education and grandparents' past socioeconomic status make it possible to correct for any biases that may arise from errors in the measurement of parental permanent income and to test for the existence of hereditary determinants in education and income.

1.1. The importance and difficulties of identifying the impact of income

There are several reasons why parental income is potentially a very important determinant of children's performance at school. The most basic reason is perhaps that rich parents can purchase better food, better housing and medical care. In

[1] The first selection point in the French educational system occurs at the end of elementary school: at the end of 5th grade, a number of pupils (about 20%) are held back in elementary education, because of low academic performance. To repeat a grade in elementary school is the most direct indicator of early performance at school in France.

[2] This impact corresponds to about 16% of a standard deviation in the dependent variable, i.e. children's age when they enter junior high-school.

[3] In other words, we do not need longitudinal data to analyze the impact of parental income on children outcomes. Our study could be replicated in any country where the household surveys have the same design as in France.

other words, they can purchase more of all the basic goods and services that favor children's development and help them perform well at school. Assuming that the parental demand for these specific goods and services actually increases with parental income, we should observe a substantial impact of income on children's performance.

It is very important to understand the actual income elasticity of children's performance at school. Defining a policy that favors equal educational opportunities is directly dependent on this understanding. The young children of families with little financial means often have a difficult time when they start school. They seem condemned from the start to encounter the same dead ends as their parents did. How can the social costs linked to the persistence of unemployment and poverty across generations be avoided? What kind of policy is needed? Should a policy of transferring resources to the disadvantaged parents be adopted or should another kind of support be created for the advancement of their children's education? A very important issue is the relative effectiveness of income transfers and direct intervention programs in augmenting the human capital of poor children.

These simple questions are much more difficult to answer than they may seem. A large body of research exists, which analyzes the relationships between parents' social status and their children's attainments (see the survey by Haveman and Wolfe (1995)). Rare, however, are the studies that have gone beyond the naive statistical analysis to test if the relationship between parents' income and their children's achievements is one of cause and effect.

The first challenge is to obtain a direct measurement of parents' income while the children are still in school. In France, the analysis of educational inequalities is based almost exclusively on what children have said — after becoming adults — on the kind of occupation their fathers had during their adolescence.[4] To our knowledge, no studies to date have been conducted on French data that link the inequalities of educational performance to the inequalities of family income.[5] The situation is very different in the United States where the Panel Study of Income Dynamics (PSID) or the National Longitudinal Survey of Youth (NLSY) have made it possible to obtain direct measurements of parents' income at the time their children were still in school.[6]

The second challenge is to sort out the income effects from the effects of unmeasured factors that are correlated with income. When unobserved factors (ultimately, hereditary traits) start determining both the parents' results as workers

[4]For a seminal French contribution, see INED (1970). For a French survey, see Duru-Bellat and Van Zanten (1999).

[5]Occupations are reliable indicators of the large income categories. Occupations are not well adapted, however, to identifying low-income effects and analyzing income redistribution policies.

[6]See, for example, the studies edited by Duncan and Brooks-Gunn (1997), and in particular, that of Smith et al. (1997).

and the children's educational capacities, the children from rich families do better at school, even if their parents' income has no direct effect on their performance. To isolate the income effects, at least one factor of parental income variation must be identified that has nothing to do with the children's educational capacities.

A related issue is to correct for biases that arise from measurement errors. Our French survey only provides one single-year measurement of parental income. Because of transitory variations in earnings, single-year measurements for parental income only represent an approximation for the permanent income that constrains parental behavior and determines children's material well-being during early childhood. To properly estimate the true income effects based on the short-term measurement of parental income, instrumental variables must also be found, which are correlated with the permanent component of parental income, not with its transitory component. In this paper, our goal is to provide an original answer to these problems and to test the consistency of our response compared to those from previous studies.

1.2. Re-evaluation using new data and techniques

The rare studies that have tried to confront head-on the problems of identifying the 'true' effects of parental income are mostly American. These studies have heavily relied on the richness of the American PSID and NLSY data. In France, longitudinal data do not exist that compare with those of the PSID or NLSY. On the other hand, most of the national surveys conducted by the French statistical office (hereafter, INSEE, Institut National de la Statistique et des Etudes Economiques), contain relatively detailed information on respondents' children, including their sex, the year and month they were born and their grade in school. From these data, it is possible to make files on the respondents' children, containing information on (a) whether the children have been held back at school, and (b) their family situation, giving, in particular, their parents' income level. Notice that this income information is provided first-hand from the parents (i.e. the respondents).

In other words, standard data from a standard statistical office make it possible for family income to be directly measured while their children are still in school. A supplementary advantage to the INSEE survey used for this paper is that it contains high-quality, detailed information about the past occupation of the respondents' parents, meaning the children's grandparents. For the remainder of this study, we will mainly use this information to correct for biases that arise from measurement errors and to identify the parental income effects. Our basic identification assumption is that the unmeasured factors that simultaneously determine family income and school performance do not survive past two generations. We will develop a simple structural framework in which it is possible to test this identifying assumption.

Information about the respondents' social origins are also available from the

PSID data. To our knowledge, this information has never been used to identify parental income effects. This is perhaps due to the poor quality of the data collected on social origin.

As a matter of fact, the American studies that analyze the 'true' income effects on children's outcomes usually use information provided by the parents' source of income (for example, see Shea (2000) or Mayer (1997)) or by the variations in income over time or within dynasties[7] (Blau, 1999). In the first case, the children's outcomes are assumed to be sensitive to their parents' income level, but not to how the income is earned. In the second case, the identifying assumption is that the factors that simultaneously determine the parents' income and their children's performance do not vary over time or within dynasties.[8]

This paper is organized in the following way. The next section is a review of recent literature on the true income effects. Section 3 develops a simple theoretical model of parental investment in young children's human capital. Our objective is to obtain a framework for our econometric analysis. Section 4 presents the econometric strategy, and Section 5 describes our French data. The results are presented in Section 6. The last section explains how our results compare to recent US estimates on the income elasticity of children's early performance at school.

2. Literature overview

From an empirical viewpoint, there is a vast body of research showing that children's outcomes are correlated with parental income.[9] Only a few studies, however, have tried to deal with the potential endogeneity of parental income.

One of the most influential studies is perhaps that of Scarr and Weinberg (1978), which compares adopted and biological children's outcomes.[10] Studying a sample of families from Minnesota, they found no correlation between parental income and adopted children's outcomes, whereas a positive correlation was found for biological children. The children's outcomes were thus not found to be linked to parental income as such, but rather to genetic factors that were not transmitted

[7] That is between families with similar ascendants.

[8] Similar to the PSID and NLSY data, the INSEE data contain information on the parents' source of income (wage earner/non-wage earner) as well as the kind of industry they work in. This information makes it possible to construct instrumental variables, similar to those used up to now in the literature, and to test their consistency with the ones constructed from the grandparents' information. The differences found between ours and the preceding results could be due to either the inconsistency of the two types of identifying assumptions or to differences in the quality of these assumptions.

[9] For instance, Wolfe (1981) analyzes the links between parental income and age-seven IQ, while Hanusheck (1992) studies early schooling achievements of children from low-income black families, interpreting the effects of parental income as a measurement of the effects of the quality of parents. Solon (1992) or Zimmerman (1992) analyze inter-generational income correlations in the US.

[10] See also Weinberg et al. (1992) for an analysis of what became of the Minnesota sample.

in adoption cases. One of the problems with Scarr and Weinberg's approach is that their sample of adoptive families was very small (about 100) and homogenous (only relatively well-off families). Another problem with their approach is that adoptive parents may treat their children differently than average biological parents.

Mayer (1997) also analyzes an American case, but uses very different estimation strategies. She assesses that capital income is not as strongly correlated with the parents' education level as other forms of income. From there, Mayer goes on to make the assumption that capital income is not as strongly correlated with parents' abilities (observed or unobserved) than other forms of income. Under this assumption, the variations in capital income make it possible to better identify the 'true' income effects on children's performance at school than the other forms of income variations.[11] And yet, Mayer finds that capital income has less effect on some children's outcomes, such as teenage childbearing, than other forms of income. According to the author, this result suggests that the 'true' income effects are, in reality, much less significant than the apparent effects. For Mayer, it is not income, per se, that matters, but the parents' unmeasured capacities that are correlated with their income.

At least two main weaknesses can be identified in her arguments. Firstly, independent from children's behavioral problems, she found that capital income has just as much effect as work income on the number of years that children stay in school, or subsequently on their wages (see Table 5.1, page 84). These results are difficult to reconcile with her main thesis.

Secondly, it is difficult to confirm that capital income effects are the best representation of 'true' income effects, based only on the fact that capital income is the least correlated to parents' education level. As university degrees are less useful on the financial market than the job market, someone from a family closely linked to the financial market has less incentive and is less likely to obtain a higher degree than other children with similar schooling abilities. That could explain the stronger correlations between work income and degrees than capital income and degrees, regardless of all the other considerations concerning measured or unmeasured capacities. Furthermore, families that access financial markets are a priori much less representative of families as a whole than families that rely on their work incomes or unemployment benefits.

In her book, Mayer offers other strategies for identifying 'true' income effects. Most notably, she compares the income effects received before and after the measurement of children's outcomes. She interprets the net effect of income received after the measurement of the outcomes as the effect of the permanent factors, which simultaneously affect parental income and the children's performance.

[11] This idea is also in Hill and Duncan (1987).

Here again, the results obtained are ambiguous: the income received after the measurement of the outcomes appears to have a significant net effect on the children's reading tests or on the probability of them leaving school before getting their high school diplomas. However, post-outcome parental income has no significant effect on mathematics tests, the number of years spent in school, degrees obtained or the wages earned after leaving school. Notice that this second strategy assumes that the variables that affect both parental income and children's outcomes are constant over time (i.e. have the same values before and after the measurement of the outcomes).

In another recent contribution, Blau (1999) analyzes children's scores on various development assessment tests, which measure academic achievement, verbal aptitude, memory performance, behavioral problems and motor development. We should emphasize that these items do not cover end-of-the-grade test scores for school-aged children and are conceptually different from the measurement of early schooling performances analyzed in this paper. Blau (1999) estimates some models using a measurement of income that corresponds to the calendar year prior to the year of the development assessment tests. He also presents estimates using average parental income over about a 12-year-period. He finds that standard OLS estimates using the single-year income measurement are about twice as small as the OLS estimates using the permanent income measurement.

To correct for endogeneity biases, Blau (1999) uses strategies that are based on the variations of single-year, income measurements over time. He compares children's performance during high- and low-income periods. In that case, no significant effect is found: the variations in single-year income over time do not help explain the variations in children's performance. Blau (1999) also compares pairs of cousins' performances (i.e. the mothers are sisters). Within this framework, the estimated effects of permanent income are stronger than the effects obtained using the OLS method.

One problem with Blau's approach is that he limits his study to only examining the impact of transitory variations in income, or only focuses on small, not very representative, sub-samples of cousin pairs. His approach also assumes that some unobserved determinants in income variations are not simultaneously explanatory factors for the variations in test scores. This approach is only valid when some unobserved factors have the same properties as good instrumental variables.

Shea (2000) estimates the impact of parental income[12] by comparing the children of union, or high-wage industry fathers to the children of non-union, or low-wage industry fathers with similar observable skills. He also compares the children of displaced fathers to the children of non-displaced fathers with similar observable skills. To be more specific, Shea (2000) analyzes two-stage least square

[12]To measure permanent income, Shea (1997) averages parental income over all the years in which the father is observed in the PSID as a household head aged 25 to 64.

estimates of the parental income effects on children's outcomes using industry, union membership and involuntary job loss as instrumental variables, using PSID data. His identifying assumption is that the differences in income linked to industry, union status or displacement reflect differences in luck rather than differences in unobserved abilities.[13] In general, when he works on a representative sample of the American population, Shea (2000) obtains significant OLS income effects, but no significant IV effects on children's outcomes. Most of the time, the estimated effects are even negative. As a matter of fact, the IV effects reported by Shea (2000) are often very poorly estimated.[14] He can only obtain a significant (and positive) income effect when he works on a sub-sample of poor families. However, in that case, the over-identifying restrictions are rejected, which indicates that some of the instruments are not valid.

In conclusion, Shea (2000) suggests that other studies need to be carried out using other data and other kinds of instruments. Indeed, only very few studies have truly succeeded in isolating a group of instruments that can identify, with reasonable precision, permanent income effects. That is exactly the goal of our study. Where possible, we have also tested the consistency of our instruments with those used in previous studies.

3. The theoretical framework

In this section, our main purpose is to give a precise definition of what we mean by the 'true' effects of parental income on children's performance at school. Moreover, we try to give some clear theoretical grounds to the econometric strategy we have followed to estimate this effect.

Generally speaking, parental income has an impact on children's performance at school if (a) the material well-being of the children is an important determinant of their performance at school, and (b) the parents' demand for things that improve their children's material well-being is income elastic. Income buys the goods and services that children need, such as high-quality food, medical care and non-overcrowded housing. If the demand for these things is income elastic, and if these things help children acquire new skills, income transfers to disadvantaged families could have a substantial impact on their children's performance. To build an identification strategy for this impact, it is necessary to specify a production

[13] As noted by Shea himself, this identifying assumption is obviously of questionable value. Some strong evidence exists that inter-industry wage differentials only reflect inter-industry differences in workers' unobserved abilities. For a recent treatment of this issue, see Goux and Maurin (1999).

[14] For example, the confidence interval of the parental income's logarithmic impact on the number of years in the school system is about [−1.27; 0.33]. Thus, when analyzing the impact of a multiplication of the income by two, it is impossible to know whether this multiplication decreases the school term by 2 years or increases it by 1 year.

function for children's outcomes and a parental utility function that contains children's outcomes as arguments. We also must specify how education affects income. Our model relies on the following four hypotheses.

(i) Children's schooling abilities depend on two types of inputs: inputs that can be purchased by parents on the market of goods and services and inputs that cannot be purchased. Assuming that the underlying production function is Cobb–Douglas, we have:

$$S_g = S(D_{g-1}, u_{g-1}) = D_{g-1}^{a} \exp(u_{g-1}), \tag{1}$$

where S_g represents g's schooling ability, D_{g-1} the parental expenditure for g's material well-being and u_{g-1} a measurement for the determinant of S_g that cannot be purchased. These non-monetary inputs include the parents' abilities to educate their children. The a parameter represents the basic elasticity that we want to identify.

(ii) Child g is held back in elementary school if and only if S_g is lower than a minimum threshold. This threshold depends only on whether g is young or old within his/her class. Assuming that this threshold is a log-linear function of the child's age within his/her class (A_g), we have:

$$F_g = 1 \quad \ln S_g + bA_g < T \tag{2}$$

where F_g represents a dummy variable whose value is 1 when g is held back in elementary school, T an intercept and b the impact of the date of birth on the probability of being held back.

(iii) Child g's parents choose their level of consumption (C_{g-1}) and the level of expenditures for their children's development (D_{g-1}) in order to maximize a utility function $U(C_{g-1}, S_g)$ subject to the budget constraint ($C_{g-1} + pD_{g-1} = R_{g-1}$), where p represents the relative price for the children's expenditure. Assuming that U can be approximated by a Cobb–Douglas function, the optimal level of expenditure D_{g-1} is proportional to R_{g-1}.[15] Within this framework, Eq. (2) can be rewritten:

$$F_g = 1 \quad F_0 + a \ln R_{g-1} + A_g + u_{g-1} < T \tag{3}$$

where F_0 is an intercept and where $b = 1$ is chosen as a normalization constraint.

(iv) The performance at school of an individual represents the main determinant of his/her permanent income when he/she is an adult. Assuming that income is a log-linear function of education, the income of g's parents can be written as:

$$\ln R_{g-1} = \ln R(S_{g-1}, v_{g-1}) = R_0 + c \ln S_{g-1} + v_{g-1} \tag{4}$$

[15] If $U(x, y) = x^{\alpha} y^{\beta}$, we verify that the optimal D_{g-1} is γR_{g-1} with $\gamma = \beta a/(\beta ap + \alpha)$.

where R_0 is an intercept and where S_{g-1} represents the father's educational ability, while v_{g-1} represents the parental productive abilities that are unmeasured in the surveys but validated on the job market.

The empirical model that holds interest for us corresponds to Eqs. (3) and (4). The problem is how to evaluate a in Eq. (3) given that we also have Eq. (4). Before describing the different estimators used, we will briefly outline our identification strategy and the procedures we used to test the validity of this strategy.

4. Identification issues

For simplicity's sake, we temporarily assume that $F_g - A_g$ can be treated as a direct measurement for ln S_g, and as a linear function of ln R_{g-1} and u_{g-1}. In the following subsection, we explain the conditions that make this assumption valid. Within this framework, our French survey provides measurements for children's and parents' performance at school, for parents' and grandparents' income. Thus, relations (1), (3) and (4) give rise to the following system of linear relations (where F_g stands for $F_g - A_g$):

$$\begin{aligned}
&F_{g+1} = F_0 + a \ln R_g + u_g,\\
&\ln R_g = F_0 + cF_g + v_g,\\
&F_g = F_0 + a \ln R_{g-1} + u_{g-1},\\
&\ln R_{g-1} = R_0 + cF_{g-1} + v_{g-1}.
\end{aligned}$$

Let us emphasize that in the empirical application, the u_g variable captures both the non-monetary determinants of children's outcomes and the errors that potentially affect the measurement of R_g and F_{g+1}. Similarly, v_g represents both the unobserved factors of parental income and the measurement errors. Our survey provides a measurement of a single-year parental income, which is only an approximation of the average income that was available during early childhood and that actually affected the child's development. Generally speaking, measurement errors generate a potentially negative correlation between u_g and v_g.

Given the dynamic structure of the links between education and income, the 'right' strategy for estimating a clearly depends on the variance–covariance structure of u_g and v_g. A number of cases can be envisioned.

(i) In the first case, u_g is orthogonal to u_{g-1} and v_g and all their past realizations. This is the case where (a) the measurement errors are negligible and (b) the non-monetary determinants of the children's schooling performance are neither correlated with the non-monetary determinants of the parents' schooling performance nor with the parents' productive capacities. Under this assumption, $E(R_g u_g) = 0$ and a simple OLS regression of F_{g+1} on R_g can be used to estimate the income effects.

(ii) In the second case, u_g is orthogonal to u_{g-1} and v_{g-1} (and all their past realizations), but not to v_g. This is the case where (a) the measurement errors are not negligible and/or (b) there is a link between the quality of the parents' results as workers and the quality of their results as educators of their children. Under these assumptions, R_g is no longer necessarily orthogonal to u_g and the OLS regression of F_{g+1} on R_g provides a potentially biased estimator of the income effects of income. In such a case, however, both the parents' education (F_g) and the grandparents' income (R_{g-1}) have the dual properties of being correlated with R_g and not with u_g. The income elasticity of children's outcomes can, therefore, be identified using F_g and/or R_{g-1} as instrumental variables.

(iii) In the third case, u_g is orthogonal to u_{g-2} and v_{g-1} (and all their past realizations), but not to v_g or u_{g-1}. This is the case where the parents' performance as students and their performance as educators of their children share common, unobserved determinants. In such a case, the OLS estimate of the income effect (and IV estimates that use parents' education as instrumental variables) is affected by an endogeneity bias. However, the grandparents' income remains a valid instrument.

Our problem in choosing between the different estimators and instrumental variables is obviously due to the fact that we do not know the true variance–covariance structure of the unobserved determinants of income and performance at school. The main advantage we have for addressing this issue is our wealth of data. These data can be used to estimate the impact of R_g on F_{g+1} in three different ways: without using an instrumental variable procedure, using our measurements of the grandparents' income and/or using the parents' education level as instrumental variables. If the structure of the residuals corresponds to case (i), then the three estimates have to give identical results. If the structure of the residuals corresponds to case (ii), only the instrumental regressions should give identical results. In case (ii), the over-identification test should not reject the hypothesis that F_g is orthogonal to the estimated residuals obtained using R_{g-1} as the instrumental variable. If the structure of the residuals corresponds to case (iii), then the three regressions should give different results and the over-identification tests should reject the hypothesis of F_g being consistent with R_{g-1}. In other words, by comparing the results obtained using the three different estimation strategies, we can test to what extent the necessary conditions for the absence of endogeneity biases are or are not satisfied.

In general, these conditions are necessary, but not sufficient. The possible structure of the residuals needs to be specified in more detail in order to identify sufficient conditions for the absence of endogeneity biases. In Appendix B, we analyze the case where the residuals can be written as: $u_g = w_g + \varepsilon_{1g}$ and

$v_g = w_g + \varepsilon_{2g}$, where ε_{1g} and ε_{2g} are potentially correlated, but serially uncorrelated, random variables, while w_g is an AR(1) process (i.e. $w_g = \rho w_{g-1} + \eta_g$ with η_g white noise). The ε_{1g} and ε_{2g} variables represent the transitory determinants of our measurement for family outcomes. From an empirical viewpoint, the errors in the parental income measurement (children's performance) negatively (positively) affect ε_{1g} and positively (negatively) affect ε_{2g}, so that ε_{1g} and ε_{2g} are potentially negatively correlated. The w_g represents the hereditary traits that persist across generations and determine the performances of both parents and children. Within this framework, testing for the absence of hereditary biases amounts to testing whether $\rho\sigma_w^2 = 0$, where σ_w^2 represents the variance of w_g. In Appendix A, we show that a necessary and sufficient condition for $\rho\sigma_w^2 = 0$ is that the IV estimate using F_g as the instrumental variables produces the same results as the IV estimate using R_{g-1}.

4.1. A semi-parametric estimator

In this paper, we look at the probability of being held back in elementary school. The dependent variable is a dummy variable whose value is 1 when the child is held back. The model used is not a linear one, but a binary choice model. Within this framework, the problem of identifying the income effects is more complicated than suggested by the previous discussion.

For a linear model, identification simply calls for the observation of an instrumental variable Z_g in the usual sense (i.e. such that $E(Z_g u_g) = 0$ and $E(Z_g R_g) \neq 0$). These conditions are no longer sufficient for the binary choice model. A fair amount of literature has recently been developed to explore the different supplementary hypotheses whereby the identification of the effects of an endogenous explanatory variable becomes possible again in a non-linear model (see the Blundell and Powell (2000) survey). This paper draws on Lewbel's recent contribution, which is particularly well-suited to our problem and the data we have at our disposal. Lewbel (2000) shows how to identify the effects of an endogenous explanatory variable y_{2g} in a binary choice model of the form $y_{1g} = I(\alpha y_{2g} + \beta x_g + \varepsilon_g)$, where x_g is a set of exogenous variables, and $I(s)$ represents the dummy variable that indicates $(s > 0)$. The method requires observing (a) an instrumental variable Z_g (i.e. such as $E(Z_g \varepsilon_g) = 0$ and $E(Z_g y_{2g}) \neq 0$) and (b) an explanatory variable x_{0g} in x_g that is continuous,[16] such that the conditional distribution of ε_g on y_{2g} and x_{1g} is independent from x_{0g}, where x_{1g} corresponds to x_g minus x_{0g}.[17] In our case, the problem of identifying income effects on the probability of being

[16] Generally speaking, to implement semi-parametric methods, we need to observe at least one continuous, exogenous explanatory variable (see Horowitz, 1998). This is a condition which makes it possible to recover the distribution of the residuals.

[17] The variation interval of x_{0g} also has to be broad and contain zero (even if it means redefining the variable).

held back in elementary school is the same as an identification problem in a linear model when an exogenous and continuous determinant of the probability of being held back is observed. Lewbel (2000), moreover, establishes that, when an exogenous and continuous x_{0g} variable is observed, the effects of the endogenous variable y_{2g} is identified by applying the usual instrumental variables method to the linearized model $Ly_{1g} = \alpha y_{2g} + \beta x_{1g} + \varepsilon_g$, where Ly_{1g} corresponds to $y_{1g} - I(x_{0g} > 0)$ divided by the conditional density of x_{0g} on (x_{1g}, Z_g).

In other words, once we can observe a continuous and exogenous determinant of the probability of being held back in elementary school, the problems of identifying and estimating parameter a are exactly the same as those analyzed in the previous subsection by replacing the dummy variable F_{g+1} with its linearization LF_{g+1}.

In our specific case, the most obvious candidate for x_{0g} is the child's date of birth during the year (A_g), which determines his/her age within his/her class. This variable can reasonably be assumed to be exogenous. It determines the child's relative level of maturity within his/her class. For a given class, the later in the year the child is born, the younger he or she is, and the smaller are his or her chances of starting junior high-school with the others from the same year. Within our sample, 28% of those born in the second half of the year did not start junior high-school when they were supposed to as opposed to only 15% of those born in the first half of the year. In the following sections, we will estimate Eq. (4) by applying Lewbel's (2000) semi-parametric method with $x_{0g} = A_g$ as a special regressor. In the next section, we present our French data.

5. Data

The data come from the survey on living conditions (hereafter, EPCV, Enquête Permanente sur les Conditions de Vie des Ménages) that was carried out in January, May and October 1997, by the French National Institute for Statistics and Economics Studies (hereafter, INSEE, Institut National de la Statistique et des Etudes Economique). Each survey period corresponds to a representative sample of about 3000 households. This survey, like most of the surveys carried out by INSEE, provides information on each member of the household. For each child living in the household, we know the sex, the year and month of birth and the grade in school.

In January and May 1997, the children born in 1985 were in junior high school (seventh grade) if they had not been held back, and still in elementary school if they had. In October 1997, the children born in 1986 were in junior high school if they had not repeated a year, and still in elementary school if they had. The information from the EPCV survey makes it possible to create a sample of 592 junior-high-school-aged children, of whom two-thirds were born in 1985 and one-third in 1986, with a variable whose value is 1 when the child is still in elementary school.

It is important to understand the meaning of this variable within the French context. The French educational system is very centralized and standardized. Teachers' training, programs, curricula and examinations meet the same standards nationwide, under the responsibility of the National Ministry of Education. The application of the directions given at the ministry level are supervised at the local level by Academies and Inspections, which are directed by officers ('Recteurs') chosen and appointed by the Ministry. The first selection point in this system occurs at the end of elementary school (i.e. 6th grade). At that point, a number of pupils are held back in elementary school because of low academic performance. The average probability of being held back is about 20%. Within this system, to repeat a grade in elementary school is the most direct indicator of early performance at school.

As a matter of fact, we can see that to repeat a grade at the elementary level is strongly correlated with future outcomes. According to the survey on Education and Occupational Qualifications conducted in 1993 by the French statistical office, workers who repeated a grade in elementary school are on average much less educated and much less paid than those who did not repeat a grade. More specifically, French male workers aged 30–34 who repeated a grade in elementary school earn on average 28% less than those who did not. In 1993, among the male population between the ages of 30 and 34, the probability of not having obtained an educational degree is 0.33 for those who repeated a grade in elementary school and 0.18 among those who did not. The total number of years of schooling is on average about 13 for people between the ages of 30 and 34 who did not repeat a grade and about 11.5 for those in the same age group who repeated a grade in elementary school.

For each child, the EPCV survey also provides variables that describe the composition of the household (number of brothers and sisters, the parents' marital situation), and most importantly give the father's education level and the family's income level. To once again emphasize the originality of this information: the data have not been obtained retrospectively from adults on their parents' situation when they were children, but obtained directly from the parents at the time their children were in school.

For each child, we also have detailed information on the grandparents' past and the parents' current occupations (French PCS code). This information makes it possible to evaluate the past position of the grandfather and the current position of the father on the socioeconomic scale built by Chambaz et al. (1998). This scale attributes a score to each occupation, which captures how individuals rank the occupation according to income, career prospect and social prestige.[18] Chambaz et

[18] To build this scale, Chambaz et al. (1998) conducted a survey on a representative sample of 3000 French people, asking respondents to rank occupations according to income, career prospects and social prestige.

Table 1
The socioeconomic status of fathers and grandfathers: descriptive statistics

Parental income quintiles	Score on the socioeconomic scale for . . .	
	Grandfathers	Fathers
Q1	−0.026	−0.074
Q2	−0.036	−0.058
Q3	−0.018	−0.042
Q4	−0.011	+0.011
Q5	+0.026	+0.080
Standard deviation	0.77	0.99
Regression coeff.	0.077	0.132
	(0.012)	(0.008)
Correlation coeff.	0.25	0.54

Reading: the average socioeconomic score for fathers' occupations in the bottom quintile of the parental income distribution is −0.074. The standard deviation for the distribution of fathers' scores is 0.99, the coefficient of the regression of (ln) parental income on fathers' score is 0.132 and the correlation coefficient between the two variables is 0.54.

al. (1998) found that the scores were highly correlated with the average income of each occupation, thus, their scale can be understood (in first approximation) as a measurement for permanent income. Table 1 confirms that the score of the father's occupation is strongly correlated with parental income ($\rho = 0.54$). The score of the grandfather's occupation is also correlated with parental income. That means that the measurement of the grandfather's socioeconomic level can be considered as a potential instrument for estimating the true parental income effects on children's outcomes.

The French occupational classification (PCS code) also distinguishes jobs by their income source (wage/non-wage earner), as well as by the type of industry to which they correspond. For both the parents and grandparents, a set of dummy variables can be constructed from this information that correspond to the crossing of an 'income source' variable (wage/non-wage) with an industry variable (manufacturing/non-manufacturing). For the empirical application, we have distinguished four main categories of employment for the father and the grandfather: (a) manufacturing/wage-earning positions, which correspond to engineers, technicians, foremen, skilled/manual laborers, (b) manufacturing/non-wage-earning positions (craftsmen and farmers), (c) non-manufacturing/non-wage-earners (all the other non-wage-earners), (d) non-manufacturing/wage earners (all the other wage earners). The corresponding identifying assumption is that these dummy variables measure an occupational capital (in the large sense), which has three main features: (a) it can be passed on from one generation to the next, (b) it is an income source and (c) it has no links with children's intellectual and educational abilities. Like the PSID or NYSL data, our's make it possible to identify some factors that affect parental income, but do not, a priori, have net effects on schooling. The manufacturing/non-wage-earning (manufacturing/wage-earning) positions represent 15% (20%) of the fathers in the top quintile of the income distribution and 3.5% (22%) in the bottom quintile. The non-manufacturing/wage-earning (non-manufacturing/non-wage-earning) positions represent 56.5% (8.5%) of the fathers in the top quintile and 68% (6.5%) in the bottom quintile.

Table 2 provides some supplementary basic statistics. As it turns out, there are very important variations in the probability of being held back in elementary school. This probability is one and a half times greater for boys than for girls, or for children in large families than for children in small families. These basic figures are very close to those obtained by previous French studies with different data (see for instance Duthoit (1989)). The main apparent effect is, however, the income effect. The probability of being held back is almost three times greater for children from families in the bottom quintile than for those from families in the

Table 2
The probability of being held back: some descriptive statistics

	Number of observations	Held back in elementary school (%)
Gender		
Male	315	25.4
Female	277	16.2
Parental income		
First quintile	118	34.7
Second quintile	118	26.3
Third quintile	118	19.5
Fourth quintile	118	11.9
Fifth quintile	120	13.6
Siblings		
Two or more	277	26.0
Zero or one	315	16.8
Quarter of birth		
First	131	16.8
Second	173	12.7
Third	152	29.6
Fourth	136	26.5
All	592	21.1

Source: Enquête Permanente sur les Conditions de Vie, 1997, INSEE. Field: 6th grade age children (i.e. first grade of the junior high-school).

top quintile. The issue is now to test whether this statistical relationship is one of cause and effect.

6. Econometric results

In this section, we present an econometric analysis of the impact of parental income on the probability of being held back in elementary school using the identification and estimation strategies described in Section 4. The basic regressions are given in Table 3. The explanatory variables are parental income (in natural logarithm form), the child's gender and the child's date of birth. The last variable is used as a reference auxiliary variable for the construction of the semi-parametric estimator.[19] We provide four estimates of the income effect, (a) an ordinary least squares (OLS) estimate (model 1), (b) a generalized method of

Table 3
The effect of income on the probability of being held back in elementary school: a semi-parametric approach

	Model 1	Model 2	Model 3	Model 4
Independent variables				
Intercept	1.72	4.95	1.35	5.11
	(0.35)	(1.54)	(3.60)	(2.36)
Log (income)	−0.30	−0.93	−0.34	−1.06
	(0.06)	(0.29)	(0.69)	(0.45)
Male	0.10	0.13	0.13	0.09
	(0.03)	(0.04)	(0.06)	(0.06)
Number of instruments	–	1	3	4
Over-identification test				
Sargan stat.	–	–	2.5	1.8
(degrees of freedom)			(2)	(3)
P	–	–	0.29	0.61

Field: 6th grade age children (i.e. first grade of the junior high-school). Note: the dependent variable is the transformation of 'to be held back in elementary school'. Model 1 corresponds to OLS estimates. Models 2, 3, 4 correspond to two-stage instrumental variable estimates. In these models, the endogenous regressor is log (income). The different sets of instruments are the following: (a) past socioeconomic status of the grandfather (model 2), (b) three dummies indicating the type of sector and employment status of the father (model 3), (c) social status of the grandfather and three dummies indicating the type of sector and employment status of the father (model 4).

[19] The estimated effects should be considered relative to the date-of-birth effects.

moments' estimate (GMM, see the presentation given by Lee, 1996) using the grandfather's past socioeconomic level as an instrument (model 2), (c) a GMM estimate using three dummies indicating the father's type of industry and employment status as an instrument (model 3), (d) a GMM estimate using both the grandfather's socioeconomic status and the three dummies indicating the father's type of industry and employment status as instruments (model 4). For each GMM model, we provide the Sargan test for over-identifying restrictions.

6.1. Basic results

The first model confirms what the basic statistics suggest: there is a significant statistical link between parental income and the probability of being held back in elementary school. The estimated OLS impact of (ln) parental income is three times higher than the estimated impact of gender.

This OLS evaluation is only valid to the extent that endogeneity biases can be disregarded. Model 2 corresponds to the GMM re-estimation of parameter a using the grandfather's past socioeconomic status as an instrumental variable. The parental income effects re-estimated in this way are about three times higher than the OLS effects. The difference between the two estimators is significantly different from zero. The IV estimate of the impact of parental income is about seven times higher than the IV impact of gender. Given that the probability of being held back is about nine points lower for girls (16.2) than for boys (25.4), our IV estimates mean that a 10% increase in parental income is associated to about a 6.3-point decrease in the probability of being held back in elementary school $(6.3=7\times0.1\times9)$.

Our IV estimator suggests that the usual estimators obtained by the OLS technique are affected by relatively large endogeneity biases. The most plausible reason is that our single-year income measurement only represents an approximation of the permanent income and that the OLS estimates are affected by biases linked to measurement errors. Another potential reason is that some unobserved parental characteristics (such as the degree of parental altruism) negatively affect parental income, but positively affect children's outcomes.

In model 3, the father's industry and employment status is used to identify the income effects. This identification strategy is close in spirit to some strategies used in the literature (see Shea (2000) or Mayer (1997)). When we use this instrument, the income effects are very poorly estimated. The father's industry and employment status does not allow for parental income to be precisely identified in our French data. This diagnosis is not very different from Shea's conclusions using American data.

In model 4, we use both the grandfather's past socioeconomic status and the father's industry and employment status as instruments. The results are close to those obtained using the grandfather's past socioeconomic status only. According to our over-identification test, there is no correlation between the estimated residuals and the instruments. In other words, the grandfather's socioeconomic status and the father's industry and employment status are compatible instruments, but the latter is not strong enough to identify, on its own, the impact that income effects have on children's schooling outcomes.

6.2. Testing for the direct effects of parental education

As was pointed out in earlier sections of this paper, the basic IV estimates given in Table 3 are potentially exposed to endogeneity biases linked to hereditary factors. Within the framework developed in Section 4, testing for the existence of hereditary determinants of F_{g+1} and R_g amounts to testing that F_g is not correlated with the residuals of the IV regression of F_{g+1} on R_g. To perform this test, we have regressed F_{g+1} on R_g using both R_{g-1} and F_g as instruments, and have tested the corresponding over-identification restriction (Table 4, models 5 and 7). In model 5, we use a dummy indicating that the father does not have a high-school diploma as a measurement for F_g. In model 7, we use a dummy indicating that the mother does not have a high-school diploma as a measurement for F_g. In both cases, neither over-identification test indicates any significant correlation between the

Table 4
Testing for a direct effect of parental education

	Model 5	Model 6	Model 7	Model 8
Independent variables				
Intercept	3.97	7.34	0.86	5.82
	(2.11)	(2.69)	(4.16)	(2.43)
Ln (income)	−0.83	−1.26	−1.38	−1.14
	(0.41)	(0.52)	(0.51)	(0.67)
Male	0.05	0.08	0.04	0.16
	(0.05)	(0.06)	(0.06)	(0.07)
Father's education	–	−0.14	–	–
		(0.12)		
Mother's education	–	–	–	0.05
				(0.10)
Number of instruments	2	1	2	1
Over-identification test				
Sargan stat.	1.42	–	0.27	–
(degrees of freedom)	(2)		(2)	
P	0.23	–	0.60	–

Field: 6th grade age children (i.e. first grade of the junior high-school). Note: the dependent variable is the transformation of 'to be held back in elementary school'. Father's (mother's) education is a dummy variable indicating that the father (the mother) is a high-school graduate. Models 5, 6, 7 and 8 correspond to two-stage instrumental variable estimates. In these models, the endogenous regressor is log (income). The different sets of instruments are the following: (a) past socioeconomic status of the grandfather (models 6 and 8), (b) past socioeconomic status of the grandfather and father's education (model 5), (c) past socioeconomic status of the grandfather and mother's education (model 7).

estimated IV residuals and F_g. In other words, these tests do not provide any significant evidence of unobserved income and education determinants that persist across generations.

In models 6 and 8, we regress F_{g+1} on R_g using R_{g-1} as an instrument and F_g as a supplementary control variable. These models confirm that the direct effect of F_g on F_{g+1} is weak and not significantly different from zero. The introduction of F_g does not significantly affect the estimated IV impact of R_g.

Some studies have found that the grandparents' characteristics have a direct OLS effect on children's outcomes (see for instance Hernandez-Iglezias and Riboud (1988)). It should be made clear that such findings do not mean that the grandparents' characteristics cannot be valid instruments for estimating parental income effects. In fact, once parental income R_g is endogenous and R_{g-1} a valid instrument for estimating its effect, R_{g-1} should have a direct OLS impact on children's outcomes (this is shown in Appendix B). The fact that the grandparents' characteristics have a direct OLS impact on children's outcomes should be understood to mean that these characteristics are potentially valid instruments.

6.3. An alternative specification of the income effects

In the previous subsections, we assumed that schooling outcomes (S_g) were a log-linear function of the quantity (D_{g-1}) of goods and services provided to the children by their parents, and consequently, a log-linear function of parental income (R_{g-1}). Another plausible hypothesis is that S_g depends only on whether D_{g-1} (and R_{g-1}) is above or below a given poverty threshold. We have re-estimated models 1, 2, 3 and 4 using this categorical specification for the income effects. To be more specific, we have focused on the impact of being in the lowest quintile of the income distribution.[20] Generally speaking, the results using this categorical specification are very consistent with those obtained using the previous log-linear specification (see Table 5).

(a) The estimated OLS effect of parental poverty is significant at standard level. This confirms what basic statistics show: children from poor families are more often held back in elementary school than children who are not from poor families.
(b) The grandfather's past socioeconomic level makes it possible to identify a poverty effect, which is much stronger than the OLS effect. This basic IV poverty effect is only significant at the 20% level, however, and less well-identified than the basic IV income effect (ln) in model 2.
(c) When we use the dummy variables indicating the father's industry and

[20] When analyzing the impact of being in the lowest decile, the conclusions are the same.

Table 5
The impact of low-parental income on the probability of being held back in elementary school: a semi-parametric approach

	Model 9	Model 10	Model 11	Model 12
Independent variables				
Intercept	−0.70	−0.94	−0.39	−0.53
	(0.05)	(0.36)	(0.13)	(0.12)
Low parental income	0.19	2.84	0.60	1.09
	(0.07)	(1.65)	(0.58)	(0.54)
Male	0.09	0.17	0.04	0.04
	(0.06)	(0.12)	(0.06)	(0.07)
Number of instruments	–	1	3	4
Number of observations	592	592	592	592
Over-identification test				
Sargan stat.	–	–	0.44	5.1
(degrees of freedom)			(2)	(3)
P	–	–	0.80	0.17

Source: INSEE surveys on Living Conditions, 1997. Field: 6th grade age children (i.e. first grade of the junior high-school). Note: the dependent variable corresponds to the transformation of 'to be held back in elementary school'. Model 9 corresponds to OLS. The estimated effects of models 10, 11 and 12 correspond to two-stage instrumental variables estimators. Each model corresponds to a different set of instruments: (1) past socioeconomic level of the grandfather (model 10), (2) three dummies indicating the type of sector and employment status of the father (model 11), (3) the socioeconomic level of the grandfather and three dummies indicating the type of sector and employment status of the father (model 12).

employment status as instrumental variables, the parental poverty effect remains higher than the OLS estimates (model 11), but becomes poorly estimated. Regardless of whether we use a categorical or continuous specification for the income effect, the father's industry and employment status do not allow for the parental poverty effect to be precisely identified.

(d) When we use both the grandfather's past socioeconomic level and the dummy variables indicating the father's industry and employment status as instrumental variables, the results are not significantly different from those obtained using only the grandfather's socioeconomic level. The poverty effect is better identified and more significant at the 5% level. The Sargan test does not reject the over-identifying restrictions. This finding confirms that the grandfather's past social position and the father's industry and employment status are compatible instruments, but the latter is not powerful enough to identify, on its own, the effects of parental poverty on the children's schooling outcomes.

We have re-estimated model 12 with log income as a supplementary regressor (results not reported). Within this framework, the IV poverty effect remains large, but is very poorly estimated. Given the size of the sample, it seems impossible to identify the best specification (i.e. continuous or categorical) for the income effect.

6.4. An alternative estimation method

Until now, we have made no specific assumption about the distribution of the unobserved factors. To test the robustness of our findings, we have re-estimated models 10, 11 and 12 assuming that the distribution of (u_g, v_g) could be satisfactorily proxied by a bivariate normal distribution and by applying standard maximum likelihood techniques (Table 6).

In the basic model (model 13), the grandfather's past socioeconomic level is used as the only instrumental variable. The first equation of the bivariate probit confirms that this instrumental variable does indeed correlate with parental income: the greater the grandfather's socioeconomic level, the lower the risk of belonging to a low-income family. The second equation confirms that this instrument makes it possible to identify a significant effect of parental poverty on children's schooling outcomes. As a matter of fact, the poverty effect is better identified within this parametric framework than within the semi-parametric one. The parametric poverty effect is five times greater than the gender effect (the difference between girls and boys [1.63/0.33], meaning that the impact of being poor increases the probability of being held back by about 35 points).

In model 14, the father's industry and employment status is used to identify the parental poverty effect. The first equation of the bivariate probit confirms that these industry and employment status dummy variables correlate with parental income. The second equation confirms, however, that these instruments are much weaker than the grandfather's socioeconomic level, even when a strong assumption is made on the distribution of the residuals.

Model 15 corresponds to model 13, to which we added the father's industry and employment status in the two equations of the bivariate probit. As we can see, the estimated income effect remains significant and large, while none of the dummy variables significantly help in explaining why children are held back. Within the parametric framework, this means that if the grandfather's past situation is a valid instrument, then the father's current industry and employment status is a valid instrument too. This is consistent with what we found within the semi-parametric framework: there are no strong inconsistencies between our different identifying assumptions, but the grandfather's past socioeconomic level provides a stronger instrument than our measurement of the father's industry and employment status.

6.5. Extension: the effects of family size

Most existing studies use models that are less parsimonious than ours, with a lot more control variables. The problem is that these additional regressors very often correspond to inputs that vary in response to changes in income or that are chosen

Table 6
The effect of low parental income on the probability of being held back in elementary school: a parametric approach

	Model 13	Model 14	Model 15
Equation (A) — dependent variable: low parental income			
Intercept	−0.87	−0.98	−1.88
	(0.06)	(0.48)	(0.37)
Grandfather's socioeconomic status	−0.19		−0.15
	(0.07)		(0.07)
Father's industry (ref: manufact. non-wage earner)			
Trade/services, non-wage earner		0.69	0.98
		(0.34)	(0.33)
Manufacturing, wage-earner		0.14	0.39
		(0.21)	(0.19)
Trade/services, non-wage earner		0.11	0.58
		(0.25)	(0.21)
Equation (B) — dependent variable: being held back			
Intercept	−1.27	−1.21	−1.18
	(0.09)	(0.21)	(0.34)
Low parental income	1.63	0.90	1.20
	(0.48)	(0.66)	(0.72)
Gender (ref: female)			
Male	0.33	0.35	0.35
	(0.10)	(0.11)	(0.11)
Date of birth	0.58	0.65	0.65
	(0.20)	(0.21)	(0.19)
Father's industry (ref: manufact. non-wage earner)			
Trade/services, non-wage earner			−0.12
			(0.20)
Manufacturing, wage-earner			0.16
			(0.18)
Trade/services, non-wage earner			0.05
			(0.38)
Residuals' correlation coefficient	−0.75	−0.20	−0.39
	(0.47)	(0.82)	(0.47)
Mean log-likelihood	−0.981	−0.983	−0.969
Number of observations	592	592	592
Number of parameters	7	9	13

Source: Enquête Permanente sur les Conditions de Vie, 1997, INSEE. Field: 6th grade age children (i.e. first grade of the junior high-school).

jointly with income.[21] The interpretation of the income effect in such models is unclear.

Until now, we have focused on the parental income effect and have avoided such interpretation problems. We have deliberately skipped over analyzing other family dimensions. In particular, we have not tried to evaluate the effects of family size on schooling. This kind of variable can potentially be correlated with income as well as with the non-observed characteristics that simultaneously determine income and parental choices. Identifying the effects of family size raises the same problems as evaluating income effects: a factor must be found that determines the number of children without determining each child's schooling outcomes.

Given that the grandmother's past occupational situation does not affect current parental income or the children's schooling outcomes, we have lengthened the preceding analysis by (a) introducing the number of brothers and sisters as supplementary determinants of children's schooling outcomes, and (b) using the assumption that the grandmother's former occupational situation is a possible instrument for identifying the effects of the family size measurement. It must be understood that the purpose is not to decompose the parental income effects into a direct net effect, and an indirect effect linked to family size. If our identifying assumptions are correct, introducing family size into the analysis should not lead to substantial modifications in the estimation of the 'true' income effects. The introduction of family size should simply deliver something more, that is an estimation of the 'true' effects of family size.

Table 7 presents the results of a trivariate parametric approach. Family size is measured by a dummy variable with a value of 1, where the child has at least two brothers and sisters, and a value of 0 where the child has less than two siblings. This variable is instrumented by a dummy variable with a value of 1, where the grandmother did not have an occupation, and 0 where she did. Three main results are obtained:

(a) a grandmother without an occupation significantly increases the probability of belonging to a large family. Moreover, the grandmother's past situation makes it possible to precisely identify the effects of family size;
(b) belonging to a large family significantly increases the risk of being held

[21] For instance, when analyzing the 'true' income effects, Mayer (1997) controls for the household's size and the parents' age at the birth of the child. These variables, however, are under the parents' control and vary in response to changes in permanent income. The meaning of Mayer's results are not very clear. Blau (1999) does not control for such choice variables. He controls, however, for the mother's race, the location of her birth, the education of her parents and the household structure of her family when she was 14. The idea is to control for variables that measure the unobserved characteristics of the dynasty. We could not control for such variables, since they were not a part of our survey. We did, however, control for mother's education. In Section 6.2, we show that this variable can be considered exogenous and has no significant direct impact on the specific outcome analyzed in this paper.

Table 7
Family size, parental income and the probability of being held back in elementary school: a trivariate parametric approach

Dependent variable (1): large family (two or more siblings)	
Intercept	−0.21
	(0.06)
Grandmother's occupation (ref: with an occupation)	
No occupation	0.40
	(0.10)
Dependent variable (2): low-income family	
Intercept	−0.59
	(0.09)
Grandfather's socioeconomic status	−0.10
	(0.03)
Dependent variable (3): to be held back in elementary school	
Intercept	−1.68
	(0.12)
Two or more siblings	1.17
	(0.33)
Low parental income	0.97
	(0.60)
Gender (ref: female)	
Male	0.34
	(0.10)
Date of birth	0.59
	(0.19)
Correlation coeff. (1)–(2)	−0.01
	(0.07)
Correlation coeff. (1)–(3)	−0.62
	(0.32)
Correlation coeff. (2)–(3)	−0.26
	(0.36)
Mean log-likelihood	−1.65091

Field: 6th grade age children (i.e. first grade of the junior high-school). Source: Living Condition Survey, INSEE, 1997.

back before entering junior high school: the effects of having a lot of brothers and sisters are even slightly higher than those of being in a low-income family (nevertheless, the difference is not significant);
(c) the income effects estimated using this trivariate model remain much greater than gender or birth date effects. The income effects are not significantly different from the effects estimated above using the previous bivariate models.

Table 8
Family size, parental income and the probability of being held back in primary school: a semi-parametric approach

	Model 14	Model 15
Independent variables		
Intercept	−1.20	8.42
	(0.48)	(3.60)
Low parental income	2.41	–
	(1.48)	
Log (parental income)	–	−1.73
		(0.67)
Two siblings or more	0.73	0.41
	(0.73)	(0.66)
Male	0.15	0.08
	(0.12)	(0.08)
Over-identification test		
Number of instruments	4	4
Number of observations	592	592
Sargan test	0.03	0.01
(degrees of freedom)	(1)	(1)
P	0.87	0.92

Note: the dependent variable corresponds to the transformation of 'to be held back in elementary school'. Models 14 and 15 correspond to two-stage instrumental variables estimate. In model 14, the endogenous regressors are 'low parental income' and 'two siblings or more'. In model 15, the endogenous regressors are 'log (income)' and 'two siblings or more'. For both models the instruments are the occupational level of the grandfather, the occupational level of the grandmother, a dummy indicating whether she participated in the labour market.

Table 8 presents the results of the corresponding semi-parametric analysis. As it turns out, the income effect is high, significant and similar to the estimated effect in the previous semi-parametric analysis. In other words, the introduction of family size into the analysis does not affect our semi-parametric estimates of the income effects.

As for family size, the effects of having two or more brothers and sisters is substantial. Taken literally, the estimated parameters mean that the true effect of having two siblings or more is about six times higher than the gender effect, which corresponds to about a 36-point-increase in the probability of being held back in elementary school. This effect is not very efficiently estimated, however.

Generally speaking, the results in Tables 7 and 8 suggest that an income redistribution policy in favor of large families would have two kinds of effects on the overall quality of schooling. This policy would have a positive, direct income effect, but it would also encourage an increase in family size and have an indirect, negative effect on family size.

7. Discussion

All in all, our econometric analysis suggests that parental income has a substantial effect on the probability of being held back in elementary school in France. Generally speaking, it is not easy to evaluate exactly how this finding compares with findings obtained by the recent studies that have tried to identify the causal effect of parental income on children's outcomes. The data and the econometric methods are, indeed, very different. It remains possible, however, to identify the main similarities between our French findings and the existing American ones and to discuss why the interpretation for these findings are different.

As already noted, the results of Mayer (1997) cast some doubt on the actual effect of parental income on some behavioral problems, such as teenage childbearing. As for schooling outcomes (or outcomes related to schooling performance), her results are much less convincing. Her evaluations of the 'true' effect of permanent income on the total number of years of education, on earnings or on basic cognitive test scores are, for example, large and no lower than the evaluation she obtains using OLS techniques. These results are not inconsistent with ours.

Even if he does not study exactly the same type of outcomes we do, some of Blau's (1999) results go in the same direction as ours. Analyzing the impact of parental income on children's scores on various development assessments, Blau (1999) finds that (a) the OLS effect of current income is small and the OLS effect of permanent income is much larger, and (b) the effects of permanent income estimated using within dynasty variations of this variable are as large as[22] the OLS effects. Thus, when he focuses on the impact of permanent income, Blau (1999) formulates the same type of diagnosis as we do: the correlation between children's outcomes and current income strongly underestimates the true effects of permanent income.

In spite of this, Blau (1999) insists on arguing that the income effect is small. His main argument is that (according to his estimates) the recent policy changes that increased[23] family income in the US led to increases of at most 3% of a standard deviation of children's outcomes. According to Blau, this is very small, and he concludes that it would take an unprecedentedly large income transfer to poor families to have a substantial impact on children's outcomes.

The argument is interesting, but not totally convincing. Firstly, we should emphasize that Blau (1999) does not regress children's test scores on the logarithm of parental income (or on any non-linear function of parental income), but on the

[22]It is actually larger, but the difference between the two estimates is not significant at standard level.

[23]Blau suggests that these policy changes can be treated as increases of about 500/1000 US dollars in permanent income.

level of parental income. In other words, his econometric analysis implicitly assumes that one additional dollar has the same effect on rich and poor families.[24] Yet, as noted by Blau himself, one of the most often established results in the literature is that the income effect is non-linear, with larger effects at lower income levels (see for instance, Korenman et al. (1995), who use the same data set as Blau). The income effect estimated by Blau corresponds no doubt to an average between a large effect on poor children and a small effect on rich children, and his discussion potentially underestimates the impact of income transfers.

In this paper, our specification of the income effect is non-linear and we can verify that this yields a very different evaluation for the impact of income transfers. According to the French statistical office, the level of child benefits represents about 20% of household incomes in the bottom decile and only a residual share of the income in the top decile in France (INSEE, 2000). Similarly, the recent variations in housing benefits observed in France during the 1980s have contributed to increasing the average income of households in the bottom decile by about 20% and has had no significant impact on the average income in the top decile. According to our estimates, the level of child benefits and/or the recent variations in the housing benefits contribute to reducing the gap between poor and rich children by about 13 points[25], i.e. 50% of the observed gap. In other words, our estimated income effect is large enough for public-policy decisions that modify income transfers to relatively poor families to have a potentially very strong impact on children's outcomes.

Independent from the issue of the income effect specification, there exists another potential problem with Blau's argument. He implicitly assumes that 3% of a standard deviation of an ability score is a small impact. From our viewpoint, it is not obvious that having an IQ 3% of an S.D. below average is similar to having an IQ 3% of an S.D. above average. Generally speaking, we need more information about the distribution of ability scores and about the links between ability and individual well-being to judge whether 3% of an S.D. is negligible or not.

8. Conclusion

In France, the inequalities between children from low-income families and high-income families are considerable. Repeating a school year before starting middle school affects about 35% of children from low-income families and only about 10% of children from high-income families. As striking as these inequalities may be, they do not necessarily mean that income has a real effect on children's

[24] On the contrary, Shea (2000) and Mayer (1997) estimate the income effects using the same functional form of income as the one used in this paper, i.e. the natural logarithm of income.

[25] This corresponds to about a 33% standard deviation in the dependent variable.

education. They do not mean that an income redistribution policy in favor of low-income families would decrease inequalities in educational opportunities.

In this paper, we have tried to go beyond the simple statistical analysis by concentrating on the inequalities of parental income linked to the grandparents' past situations and on the inequalities linked to the parents' industry or income sources. Our working assumption is that these dimensions of income inequalities reflect more the hazards of birth and professional life than the abilities (observed or not) parents use on the job market and for raising their children. We develop very simple tests that do not reject these basic identifying assumptions. Income differences between parents definitely seems to have a considerable effect on schooling, and particularly on early schooling transitions. Public investment in children has no doubt had a very substantial impact on children's performance at school in France, but is still not sufficiently re-distributive to counteract the inequalities in parents' resources spent on children.

Appendix A

Let us consider the following equations,

$$F_{g+1} = aR_g + u_g \tag{A.1}$$

$$R_g = cF_g + v_g, \tag{A.2}$$

with $u_g = w_g + \varepsilon_{1g}$, $v_g = w_g + \varepsilon_{2g}$ and $w_g = \rho w_{g-1} + \eta_g$, where ε_{1g}, ε_{2g} and η_g represent serially uncorrelated random variables, with $E(\varepsilon_{1g}\varepsilon_{2g}) = \sigma_{12}$, $E(\varepsilon_{1g}\varepsilon_{1g}) = \sigma_1^2$, $E(\varepsilon_{2g}\varepsilon_{2g}) = \sigma_2^2$. $E(\varepsilon_{1g}\eta_g) = E(\varepsilon_{2g}\eta_g) = 0$. We verify,

$$R_g = (ac)R_{g-1} + v_g + cu_{g-1} \text{ and } F_g = (ac)F_{g-1} + av_{g-1} + u_{g-1} \tag{A.3}$$

which yields,

$$R_g = \sum_{k=0}^{\infty} (ac)^k (v_{g-k} + cu_{g-1-k}) \text{ and } F_g = \sum_{k=0}^{\infty} (ac)^k (av_{g-1-k} + u_{g-1-k}), \tag{A.4}$$

which implies,

$$E(u_gR_g) = \sigma_{12} + (1+\rho c)\sigma_w^2/(1-ac\rho), E(u_gF_g) = \rho(1+a)\sigma_w^2/(1-ac\rho),$$
$$\text{and } E(u_gR_{g-1}) = \rho\,(1+\rho c)\sigma_w^2/(1-ac\rho), \tag{A.5}$$

which implies,

$$[E(u_gR_{g-1}) = 0] \Leftrightarrow [E(u_gF_g) = 0] \Leftrightarrow [\rho\ \sigma_w^2 = 0].$$

Once the IV estimate of a using R_{g-1} as instrument ($\hat{a}_{ivr}$) is unbiased, the IV estimate using F_g as instrument ($\hat{a}_{ivf}$) is unbiased too. A necessary condition for

$\hat{a}_{ivr}$ to be unbiased is that the two IV estimators yield similar results (i.e. plim $\hat{a}_{ivr}$ = plim $\hat{a}_{ivf}$).

Now, let us assume that [plim $\hat{a}_{ivr}$ = plim $\hat{a}_{ivf}$] and [plim $\hat{a}_{\mathrm{OLS}}$ < plim $\hat{a}_{ivr}$]. Let us show that this implies [$\rho\sigma_w^2 = 0$]. Firstly, assuming that the coefficients that determine outcomes' intergenerational correlation (i.e. ρ and ac) are small, we verify,

$$\begin{aligned} &E(R_g R_g) \approx \sigma_1^2 + c^2\sigma_2^2 + (1+c)\sigma_w^2, \text{ and } E(R_{g-1}R_g) \\ &\qquad \approx c(\sigma_{12} + \sigma_w^2), E(F_g R_g) \approx c(\sigma_2^2 + \sigma_w^2) \end{aligned} \tag{A.6}$$

which implies,

$$\begin{aligned} &[\text{plim } \hat{a}_{\mathrm{OLS}} < \text{plim } \hat{a}_{ivr}] \\ &\Leftrightarrow [E(u_g R_g)/E(R_g R_g)] - [E(R_{g-1}u_g)/E(R_{g-1}R_g)] < 0 \\ &\Leftrightarrow (\sigma_{12} + \sigma_w^2) < 0. \end{aligned} \tag{A.7}$$

But we also verify,

$$\begin{aligned} &[\text{plim } \hat{a}_{ivr} = \text{plim } \hat{a}_{ivf}] \\ &\Leftrightarrow [E(u_g R_{g-1})/E(R_{g-1}R_g)] - [E(F_g u_g)/E(F_g R_g)] = 0 \\ &\Leftrightarrow \rho\sigma_w^2\,[(1/(\sigma_{12} + \sigma_w^2)) - ((1+a)/(\sigma_2^2 + \sigma_w^2))] = 0. \end{aligned} \tag{A9}$$

Once $(\sigma_{12} + \sigma_w^2) < 0$, [plim $\hat{a}_{ivr}$ = plim $\hat{a}_{ivf}$] is equivalent to [$\rho\sigma_w^2 = 0$] and implies that both $\hat{a}_{ivr}$ and $\hat{a}_{ivf}$ are unbiased. Once the OLS estimate ($\hat{a}_{\mathrm{OLS}}$) is smaller than the two IV ones, [plim $\hat{a}_{ivr}$ = plim $\hat{a}_{ivf}$] is not only a necessary condition for these two IV estimates to be unbiased, but a sufficient condition too.

Appendix B

Let us consider our basic model,

$$F_{g+1} = aR_g + u_g, \tag{B.1}$$

and let us assume that R_g is endogenous (i.e. $E(u_g R_g) \neq 0$) and that R_{g-1} is a good instrument for identifying a (i.e. $E(R_{g-1}R_g) \neq 0$ and $E(u_g R_{g-1}) = 0$). Within this framework, an OLS regression of F_{g+1} on both R_{g-1} and R_g gives the following results (where $X'Y$ denotes the empirical covariance for X and Y),

$$\hat{a}_{\mathrm{OLS}} = a + [(R'_{g-1}u_g)/D], \tag{B.2}$$

$$\hat{a}_{1\mathrm{OLS}} = [(a(R'_{g-1}u_g)(R'_{g-1}R_g) - (R'_g R_g)(R'_g u_g)]/D, \tag{B.3}$$

where $\hat{a}$ represents the estimated impact of R_g and $\hat{a}_1$ the estimated impact of R_{g-1} and where

$$D = (R'_{g-1}R_{g-1})(R'_gR_g) - (R'_{g-1}R_g)^2.$$

Thus, assuming $E(u_gR_g) \neq 0$ and $E(R_{g-1}R_g) \neq 0$, we have,

$$\text{plim } \hat{a}_{1\text{OLS}} = -E(R_{g-1}R_g)E(R_gu_g)/E(D) \neq 0. \quad \text{(B.4)}$$

In other words, once R_g is endogenous and R_{g-1} a good instrument, R_{g-1} has necessarily a direct OLS impact on F_{g+1}.

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