Evaluating the Effectiveness of Private Education Across Countries : a Comparison of Methods

Forthcoming in *Labour Economics*

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Abstract: *This paper aims at estimating the effect of private vs. public education on pupils achievement, using the 2000 OECD PISA survey and taking into account the potential bias due to the existence of unobserved confounding factors. To deal with these selection biases, three methods are implemented, in a comparative perspective: (1) IV regression, (2) Heckman's two-stage approach and (3) propensity score matching. This exercise underlines important divergences between the results of parametric and non-parametric estimators. All results, however, show that private education does not generate systematic benefits.*

JEL classification: I28 (Education: Government Policy). H520 (National Government Expenditures and Education).

Keywords: educational economics, human capital, private education, IV & Heckman models, propensity score matching.

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The authors gratefully acknowledge financial support from the Belgian French Community's program "Action de Recherches Concertée" 99/04-235.

1. Introduction

It is clear that the production of education requires monetary resources. However, several studies (e.g. Hanushek, 1986, 2003; Hoxby, 1996, 2000a; Betts, 2001) have repeatedly highlighted over the last two decades the fact that there is no mechanical relationship between the level of public spending and pupils' results. In this context, economists and other social scientists have come to consider that more attention should be paid to the organizational characteristics of schools, in particular whether it makes a difference that they are privately run or directly governed by a central or local public authority. Is there some (robust) evidence that students could gain/loose by transferring from a public to a private school? And if so, what is the magnitude of the differential?

The study of existing education systems provides part of the answer to this question. Indeed, in many countries around the world, although education is funded by public money, its production is far from being a public monopoly. In the Netherlands, and to a lesser extent in Belgium, Ireland, Spain or Denmark, significant portions of the student/pupil population attend schools operated by non-profit private boards. There is indeed an old tradition of education entrepreneurship within the non-profit sector. The Catholic and Protestant churches for example have been very active in establishing schools that are now largely funded by public money.

It is thus not a real surprise that both private and public schools are represented in the latest OECD survey on academic achievement. We are here referring to the Program for International Student Assessment (PISA). This survey, carried out in 2000, is aimed at testing the skills in Mathematics, Science and Reading of representative samples of 15 year-old students across OECD and non-OECD countries[1]. The resulting data set is very rich and can be used to address many questions relevant to education policy, one of them being the presence and the magnitude of a private/public achievement differential.

[1] Australia, Austria, Belgium (French-Speaking), Belgium (Dutch-Speaking), Brazil, Canada, China, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Hong Kong China, Korea, Latvia, Luxembourg, Mexico, The Netherlands, New Zealand, Norway, Poland, Portugal, Russian Federation, Spain, Sweden, Switzerland, United Kingdom, United States.

To avoid any confusion, the reader should take good note of the way private/public categories are defined by the OECD. A public school is a school managed directly or indirectly by a public education authority, government agency, or governing board appointed by government or elected. A private school is a school managed directly or indirectly by a non-government organization (e.g., a church, trade union, business, or any other private institution). In brief, the underlying criteria is not that of the origin of financial resources, but the legal status of the school board.

The rest of this paper is organized in 4 sections. Section 2 briefly exposes the econometric and conceptual framework of our empirical analysis. The problem at hand is formulated in the terms of the more general Evaluation Problem, with an emphasis on selection/endogeneity biases. Three ways of dealing with these biases (Instrumental Variables, Heckman 2 stages, and Propensity Score Matching) are implemented. Section 3 presents the international data set we use (PISA 2000), while Section 4 presents the empirical results obtained with the three different methods. Conclusions are given in Section 5.

2. Estimation of Private School Effect: a special case of the Evaluation Problem

We are interested in measuring the effect of private school attendance (our treatment[2] variable) on educational achievement as measured by a standardized test score. This problem can be seen as a specific case of the more general Evaluation Problem (e.g. Smith, 2000; Schmidt, 2001). We observe the outcomes of pupils attending a private school and the achievement of those attending a public school. To know the 'true' effect of private education on a particular individual, we must compare the observed outcome with the outcome that would have resulted had that student not attended a private school. However, only one outcome is actually observed. What would have resulted had the student not been 'treated' – the counterfactual -- cannot be observed. And this is precisely what gives rise to the Evaluation Problem. Yet, information on non-participants can be used to derive the counterfactual for participants.

[2] Note that, in the evaluation literature, 'treatment' conventionally refers to the individuals who participate in the "program" (here, it refers to experiencing a private education).

Before stating how this idea can be implemented, it is important to specify the parameters of interest when estimating treatment effects. Three types of estimates are mentioned in the literature (Heckman & Navarro, 2003; Bryson, Dorsett, Purdon, 2002). In this paper, we will focus on the impact that private school attendance has on individuals who were actually treated – i.e., the average effect of treatment on the treated (hereafter ATT). However, one could also be interested in the effect of private schooling on a random individual – i.e., the average treatment effect (ATE). These two effects are identical if we assume homogeneous responses to treatment among individuals; should the responses be allowed to vary across individuals, ATT and ATE would differ. The third parameter of interest is known as the local average treatment effect, or LATE (Angrist & Imbens & Rubin, 1996); it measures how a treatment affects people at the margin of participation, i.e. it gives the mean effect of a program on those people whose participation changes as a result of the program.

Of these three parameters (ATT, ATE and LATE), ATT constitutes an obvious start: it easily makes sense for policy makers, who may consider it as the most relevant. The first questions policy makers want to see addressed is of course whether a program has any impact. Very often, they also want to know whether the expansion of a given program is worth considering (for instance, increasing the share of pupils attending a private school). While ATT may provide answers to these questions, other measures (ATE, for instance) are needed to go further. For instance, if only individuals with the largest expected gains attend a private school, ATE will be *smaller* than ATT. A generalisation of the program may thus produce lower effect that the one measured by ATT. The empirical analysis outlined in this paper, however, is mostly exploratory. It will therefore focus on ATT only, but will propose different ways of measuring it.

2.1. The OLS common effect model

Until recently, the standard (and almost only) way to estimate the effect of a treatment on educational outcomes with cross-section data was to control for observable differences between treated and non-treated individuals, using linear regression and Ordinary Least Squares (OLS). For example, Summers & Wolfe (1977) and Toma & Zimmer (2000), assume that student *i*'s achievement (A_i) in a given country can be explained by linear, common effect models of the form:

$$A_i = \beta . X_i + \delta . PRIV_i + \varepsilon_i \qquad (1)$$

where $PRIV_i$ is a dummy variable indicating whether or not the i^{th} student attended private school. In this basic "benchmark" case, the dummy has a constant coefficient, which gives the average effect of the treatment on the treated (ATT).

If the independent variables X_i perfectly control for the other determinants of achievement (mainly the student's background and other characteristics), then estimating Equation (1) with OLS yields unbiased estimates of ATT. In this case, ATT and ATE are equivalent, since a homogeneous and constant response to the treatment is assumed. Implicit in this approach is the assumption that (having controlled for X_i) the treatment is independent of the process determining outcomes (i.e. $PRIV_i$ and ε_i are uncorrelated). The rest of this paper focuses on the sensitivity of OLS results to the relaxation of two assumptions: first, the absence of any selection bias beyond what is observed by the statistician, and, second, the linearity of the 'private school' effect across individuals.

2.2. Cross-section estimators dealing with selection on unobserved variables

Since the early 1980s, the literature has repeatedly emphasized that the OLS approach to treatment effect is likely to be biased by the imperfect measurement or omission of some variables. For example, more able or motivated students – dimensions that remain unobserved by the statistician – could select themselves into private schools. Equivalently, private schools may select such students (e.g. from a waiting list, if admissions are over-subscribed). Technically, the OLS measure of ATT -- the parameter associated to the $PRIV_i$ dummy in Equation (1) -- could be confounded with the effect of the unobserved (selection) variables. Means of controlling for this selection bias (i.e. for the endogeneity of $PRIV_i$) consist in implementing the Instrumental Variable (IV) and the "Heckman Selection"[3] estimators.

2.2.1. Instrumental variables 2-stage least square

The IV method consists in estimating a two-stage regression model. The second stage equation (Equation 3) uses the linear prediction $PRIVHAT_i$, obtained by regressing $PRIV_i$ against all other exogenous variables plus one D_i (Equation 2). This variable, known as the 'instrument', introduces an element of randomness into the assignment which approximates the effect of an experiment.

$$PRIV_i = \gamma X_i + \theta . D_i + \mu_i \qquad (2)$$

$$A_i = \beta . X_i + \delta . PRIVHAT_i + \varepsilon_i \qquad (3)$$

Provided D_i exists, the estimation of equations (2) & (3) gives an estimate of ATT[4]. The main drawback to the IV approach, however, is that it will often be difficult to find a suitable instrument. To be valid as an instrument candidate (D_i) should influence the probability to be treated, without being itself determined by any confounding factors affecting outcome, i.e. without being correlated to the error term ε_i (Wooldridge, 2002). Since this last condition can never be tested, the choice of a valid instrument largely depends on intuition and economic reasoning.

[3] A variant of the two-steps correction model suggested initially by Heckman (1979)

2.2.2. Heckman 2-steps

The Heckman Selection estimator is the other extensively used method to control for selection on unobserved variables. It relies on the assumption that a specific distribution of the unobservable characteristics jointly influence participation and outcome[5]. By explicitly modelling the participation decision (estimating a first-step equation similar to equation (2), generally using a Probit specification), it is possible to derive a variable that can be used to control[6] for the potential correlation between the residual of the achievement equation and that of the selection equation. By including this new variable alongside the observable variables (X_i) and the private school dummy in the second-step (or outcome) equation, Heckman can generate unbiased estimates of ATT. However, as with the IV approach, credible implementation requires the selection equation to contain an instrument (Goldberger, 1983, Puhani, 2000). And the identification of a suitable instrument is often an obstacle to proper implementation.

2.3. Non–parametric estimators: propensity score matching

A major drawback of the IV and Heckman methods (as well as OLS) is that they impose a linear form on the outcome equation. The private school effect is assumed to be uniform across the distribution of covariates, and adequately captured by the (constant) coefficient of a dummy variable. But economic theory provides no justification for such a linear restriction. Following Heckman & Navarro (2003) and others, we therefore complement our analysis with the non-parametric *matching approach* (Rosenbaum & Rubin, 1985).

The underlying principle consists of matching treatment with comparison units (i.e. pupils attending, respectively, private and public schools) that are similar in terms of their observable characteristics. As stated by Bryson, Dorsett, Purdon (2002), this approach has an intuitive appeal, but rests on a very strong assumption: that any selection on unobserved variables is trivial, in the sense that

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[4] Note that if we relax the constant coefficient assumption ($\delta_i = \delta\ \forall i$) and if the variation in gains is related to the instrument, the parameter estimated is LATE (Imbens and Angrist, 1994).
[5] The error terms are usually assumed to follow a Bivariate Normal distribution.
[6] Hence the conventional term of 'control function method'.

the latter do not affect outcomes in the absence of treatment. This identifying assumption for matching, which is also the identifying assumption for OLS regression, is known as the Conditional Independence Assumption (CIA).

Under the CIA, estimators relying on matching techniques can yield unbiased estimates of ATT. They allow the counterfactual outcome for the treatment group to be inferred, and, therefore, for any differences between the treated and non-treated to be attributed to the treatment. To make this approach credible, a very rich dataset is need since the evaluator should be confident that all variables affecting both participation and outcome are observed. This said, some researchers (Dehejia & Wahba, 1998) conclude that propensity matching generally replicates experimental results[7] reasonably well. But other researchers disagree with this conclusion (Smith & Todd, 2003).

Matching pupils directly on their vector of covariates would be computationally demanding, especially when the number of covariates to control is large. The number of 'cells' into which the data has to be divided would then augment exponentially. Rosenbaum and Rubin (1985) suggest a clever way to overcome this problem. They demonstrate that matching can be done on a single-index variable, the *propensity score*, defined as $p(X_i) \sim \Pr(PRIV_i = 1 \mid X_i)$, which considerably reduces the dimensionality problem, since conditioning is done on a scalar rather than a vector.

The propensity score, however, must verify the *balancing property*. This means that individuals with the same propensity score must have the same distribution of observed covariates. In other words, the function used to compute the propensity score should be such that individuals with a similar propensity to attend a private school display, on average, similar values of X_i.

Moreover, when doing propensity score matching, it is possible that, for a particular individual in the treatment group, no match can be found (i.e. nobody in the non-treatment group has a propensity score that is 'similar' to that particular individual). This is known as the *common support* problem. One way of addressing it is to drop treatment observations whose propensity score is higher than the

[7] Results from 'real' experiments in which participants are randomised between treatment and control groups.

maximum or less than the minimum of the controls. ATT has then to be redefined as the mean treatment effect for those treated falling *within* the common support. This may play in favour of the matching technique. The overlap requirement across treated and non-treated units, in a sense, avoids making questionable extrapolations outside common support, as all parametric methods do. However, enforcement of the common support can result in the loss of a sizeable proportion of the treated population. For these discarded individuals, the programme effect cannot be estimated.

Finally, even within the common support, the probability of observing two pupils with exactly the same value of $p(PRIV_i = 1 \mid X_i)$ is in principle zero, since this index is a continuous variable. Various methods have been proposed to overcome this difficulty, two of which will be implemented here. The first one is the *nearest neighbour* matching approach; it consists of an algorithm that matches each pupil attending a private school with the public school peer displaying the nearest propensity score. The resulting match is as good as it is possible to achieve, in that the bias across the treatment and comparison groups is minimised. However, this method disregards potentially useful observations. Over-reliance on a reduced number of individuals (the nearest neighbours) can result in ATT with large standard errors. This legitimates a second method: *kernel* matching. In kernel matching, all members of the non-treatment group are used, to some extent, to build a match for each member of the treatment group (although the contribution of those for whom the match is poor may be negligible). The kernel is indeed a function that weights the contribution of each non-treated group member, according to distance of propensity scores. Exact matches get a large weight, and poor matches get a small weight.

3. Data set and estimation strategy

3.1. Data and variables

The data we use to assess the impact of private vs. public school by using IV, Heckman and Propensity Score Matching methods is relatively unique and fairly recent. It comes from the 2000 OECD survey (the PISA project, Program for International Student Assessment). This database contains math, science and reading test scores of students aged 15 across 34 OECD and non-OECD countries. These students are nested within schools, potentially attending different grades in countries with grade repetition. The test score variable has been normalized to mean 0 and variance 1, by country and by topic. This allows estimates of ATT to be interpreted directly as percentage of standard deviation.

Our analysis is carried out only on countries where the number of students sampled and attending private school is above a 10% threshold. This leads to a subset of countries/regions containing Dutch-Speaking Belgium, French-Speaking Belgium, Mexico, Ireland, Spain, France, Denmark, Austria and Brazil[8]. Justifications for this restriction are twofold. First, it makes little sense, statistically speaking, to assess a private school effect in a particular country using test scores of just of few dozen students. Second, policy-makers who currently discuss the opportunity to expand (or not) the private sector (using vouchers for example) are interested in knowing whether private schools make a difference when attended by a large (and heterogeneous) population. This justifies focusing on countries for which the (sample) share of private education is quite large[9], as in Belgium or Ireland where more than 50% of secondary school students attend a private school.

Table 1 below gives the students' repartition between public and private schools, by country, for each one of the PISA samples we used (Mathematics, Reading and Science).

[Insert Table 1 about here]

In order to implement the techniques presented in Section 2 (OLS, IV, Heckman and Propensity Score Matching), we have built a data set (see Tables 2-4 for summary statistics) that is relatively rich in terms of individual characteristics and family/socio-economic background known to affect academic achievement. This includes, besides gender (GIRL), the presence of siblings (SIB), whether the student is first born or not (FIRSTBORN), whether his mother has some post-secondary education (MPOSTSEC), whether her father is an immigrant (FATHIM), the highest socio-economic index of both parents

[8] Although the Netherlands meet these two criteria we decided not to include them in the analysis, as the OECD indicates that "concerns with sampling outcomes and compliance problems with PISA standards resulted in recommendations to place constraints on the use of the data for (...) the Netherlands. (...)The Netherlands' response rate was very low" (OECD, 2002).

[9] Assuming that the PISA sample is representative of the private/public division in reality.

(HISEI)[10] as well as an index of cultural resources available at home (HEDRES)[11]. Finally, private schools are identified by a dummy variable (PRIV) equal to 1 if a pupil attends a private school, and to 0 if he/she attends a public school.

We also try to account for potential peer effects[12], using the *average* parental socio-economic index of the student's schoolmates (PHISEI) as a proxy. We assume that the peer effect is better captured by the socio-economic mix of the peer group[13]. We are fully aware that the proper estimation of the true contribution of peer effects is a methodological issue *per se*. In particular, Rivkin (2001) underlines that the composition of the peer group is likely to be endogenous. However, dealing with this problem would be beyond the scope of the present paper, which focuses instead on the endogeneity of private school attendance. Given the endogeneity of school attendance, one could even go as far as to say that it might make sense *not* to attempt to control for the endogeneity of the peer variable. The latter's (upward) biased coefficient might somehow capture part of the bias we want to extract from our treatment estimate.

[Insert Tables 2-4 about here]

3.2. Estimation strategy

We logically focus on the magnitude of the private/public school differential. We first measure gross differentials. We simply compare the mean values of students' math, science and reading test scores for each type of school (the gross differential being equal to private mean minus public mean). Using the independent variables presented above, we then run a traditional OLS model to get a first estimate of ATT, accounting for socio-economic status and peer endowments at the school level.

The next step is to implement the IV and Heckman models, in order to control for the potential endogeneity of the treatment. As stated in Section 2, both models crucially depend on the presence of a

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[10] The last variable is the result of the conversion of Isco-88 (International Standard Classification of Occupations) into International Socio-economic Index of Occupational Status (ISEI). For further details see http://www.fss.uu.nl/soc/hg/pisa/index.htm.
[11] The last variable is built by the PISA team using several items available in the surveys (c.f. OECD, 2002 for technical details).
[12] For examples of studies focusing on this issue see Coleman (1966), Jencks & Meyer (1987), Brueckner & Lee (1989), Bénabou (1996), Glewwe (1997), Vandenberghe (2002).
[13] The student's own parental socio-economic index (HISEI) is thus excluded from the average.

proper instrument in the first equation (a.k.a. the 'choice equation'). We have opted for a dummy variable, SCHLOC, equal to 1 if a pupil attends a school located in a big city (more than 100.000 inhabitants) and to 0 otherwise.

This variable fulfils the first condition to be an instrumental variable candidate (Wooldridge, 2002): to be correlated with the endogenous or 'choice variable' PRIV, *ceteris paribus*. As can be seen in Table 5, the (marginal) effect of being located in a big city on the probability of attending a private school is important and strongly significant in all countries (although possibly less in Denmark).

As stated in Section 2, the second condition for a variable to be an instrumental candidate (non-correlation with the residuals of the 'achievement equation') cannot be tested, which makes the choice of an instrument largely dependent on sensible arguments. We believe that there are plausible circumstances that would make school location a valid instrument. Hoxby (2000) judiciously argues that geographical and topographic features can be used as 'natural' instruments for endogenous characteristics of school systems[14]. In our case, one could argue that variation in the supply of private schools between big cities and other areas primarily reflects historical (country-specific) factors that can be assimilated to supply-side accidents.

Some critics would immediately say that big cities are synonymous with higher incidence of social problems (non-observed by the econometrician) negatively impacting results. If private schools are more frequent in more rural areas, with less social problems, the risk of overestimating their effectiveness is serious. The evidence extracted from PISA does not entirely support this view. In Table 5, the sign of coefficients capturing the relative importance of private provision of school according to location varies across countries. In Austria, Denmark, France, Ireland, Mexico and Spain, big cities are synonymous with a higher probability of attending a private school. But in Belgium and Brazil, it is the reverse. We consider that this asymmetry somehow reduces the risk of overestimating the effectiveness of private schools.

[14] In Hoxby's paper, geographical features (like the number of streams) are used to instrument the importance of school choice (number of school districts) available to a given population.

However, it could still be the case that the relative prevalence of private/public schools according to location somehow reflects demand-side factors (for example, biased residential choice with subsequent adjustment of the supply of private education, or the reverse) in which case the endogeneity problem would remain.

[Insert Table 5 about here]

The last step is to implement the Propensity Score Matching approach exposed in Section 2. This is done[15] using a Probit model to compute propensity score, and '*nearest neighbour'* and '*kernel'* as matching algorithms, under the condition that the common support is satisfying. The matching algorithms use the same set of covariates (X_i) as in all previous estimations. Following Heckman & Navarro-Lozano (2003), we also estimated ATT *with and without* the instrument (i.e. the SCHLOC variable used in the IV and Heckman models) in the list of variables on which matching occurs.

The reader should bear in mind that to make this approach credible (and particularly for the CIA to hold) a very rich dataset is desirable : the evaluator should be confident that all relevant variables (potentially affecting both participation and outcome) are observed. Although PISA is extremely rich in terms of background variables, its greatest weakness as regards to the CIA is the lack of repeated measure of achievement. Most observers indeed agree that the best way to capture the impact of unobserved heterogeneity would be to control for the complete history of the outcome measure before treatment. Since the PISA 2000 study is cross-sectional without any retrospective information on student achievement prior to the test, it could be that the CIA is violated.

4. Results and discussion

In Tables 6-8 below, are detailed the five types of results of interest: [1] the gross score differential between private and public students; [2] ATT as captured by the PRIV dummy (δ) in an OLS regression model without control for selection biases; [3] ATT estimated via IV two-stage least-squares;

[4] ATT obtained with the Heckman two-stage estimates; [5] ATT from both nearest neighbour and kernel propensity score matching[16].

[Insert Tables 6-8 about here]

4.1. Comparison of methods

As a preliminary remark, it is worth noting that all methods investigated here lead to estimates of ATT that diverge from the OLS results. This said, it is relatively obvious that OLS and Propensity Matching (particularly kernel matching) generate results that are relatively similar. The real differences emerge with IV and Heckman estimates, and quite logically for cases where selection biases – as detected by the correlation between error terms in the Heckman model (rho in tables 6-8) – are significant. The correction for selection bias can be dramatic, putting ATT close or sometimes above 1 (i.e. effectiveness differential between private and public schools of about 1 standard deviation). This correction can be positive (rho>0 with Heckman), suggesting that OLS exaggerates the effectiveness of private education. But it can also be significantly negative (rho<0), then supporting the idea that private effectiveness can be underestimated by OLS.

As to propensity score matching, we would like to emphasize the similarity of ATT estimates obtained with nearest neighbour and kernel matching. A look at Table 9 provides some diagnostics on the performance of the match. Each cell should be interpreted as the average (absolute) difference between covariates as a percentage of standard error. There is no clear reference against which to judge the performance of the match, but comparing the values of Table 9 with those of other studies (Bryson, Dorsett, Purdon, 2002) suggests an adequate match, particularly for kernel.

[Insert Table 9 about here]

The effect of enforcing the common support requirement is shown in Table 10. The overall result is that less than 5 per cent of all pupils attending private education were dropped. This level is low and is

[15] With STATA 7 software called PSMATCH2 developed by E. Leuven & B. Sianesi, and available at http://econpapers.hhs.se/software/bocbocode/S432001.htm

[16] Due to the lack of significant variation, we only report the results for the case in which the list of variables on which matching takes place does not include the instrument SCHLOC.

therefore unlikely to affect ATT. However, for some countries and/or topic (Brazil/reading, French-Speaking Belgium/math & reading, Flemish-Speaking Belgium/science), up to 20% of students are dropped. For these discarded students, the effect of private school attendance cannot be estimated.

[Insert Table 10 about here]

4.2. Substantive results.

Regarding the evaluation or the effectiveness of private education *per se*, two main statements can be made. First of all, some private-public school differentials remain statistically significant after controlling for selection on both observed and unobserved variables. These differentials can be of great magnitude. For example, in Brazil, private schools seem to outperform public ones by 20% to 100% of a standard deviation depending on topic and estimation methods. However, other results – particularly with IV and Heckman – suggest that public schools can outperform private ones, in France for instance (by 50 to 90% of a standard deviation), and in Austria (by 100% or even more of a standard deviation). Compared with the size of estimates generally obtained in the education production function literature, these can be considered as sizeable.

Second, topics (math, reading, science) matter less than countries. In other words, within a country, private-public differences tend to appear with similar sign and magnitude for each of the three topics.

But not all countries display differences between private and public schools, and the range of existing differences can be large. On average for math, reading and science, achievement can be said to be *much higher* in private schools in the Dutch-speaking community of Belgium and Brazil (about 55-60% of a standard deviation), and *higher* (about 40-45%) in the French-Speaking Community of Belgium. The private school effect is generally not statistically significant in Mexico, Denmark and Spain. By contrast, their might be a private school disadvantage in Austria (-60%) and to a smaller degree in France and Ireland (-20%). But for France and Ireland, these results are totally driven by the correction for selection on unobserved variables (i.e. appears only when using the IV and Heckman approaches).

5. Conclusion

The objective of this paper was to estimate the relative effectiveness of private education on the academic achievement of a population aged 15. This problem was formulated as a specific case of the Evaluation Problem. A comparative empirical analysis, involving several countries and using cross sectional data, was implemented to build counterfactuals. Methods used were essentially twofold. IV and Heckman on the one hand, in an attempt to control for potential selection on unobserved variables (ability, motivation) and Propensity Score Matching (with two alternative matching algorithms) on the other in order to depart from the linearity restriction arbitrarily imposed by OLS linear estimators.

From a methodological perspective, this paper underlines the main obstacle to the implementation of the IV and Heckman approaches, namely the difficulty to find a valid instrument. The Propensity Score Matching technique helps overcome this obstacle, but at the cost of a risky assumption: that the differences in 'treated' and control individuals are fully embedded in the observed variables. This assumption may be risky when using cross-section data (as the PISA 2000 data used here), since the effect of past events is not fully taken into account[17]. Running this analysis, exploiting longitudinal or repeat cross-section data, would certainly be a source of improvement.

As regards to the effectiveness of private education *per se*, we found – consistently across all methods used – a significant positive effect in a small group of countries: Dutch and French-Speaking Belgium and Brazil. Quite invariably for all these methods, ATT is not statistically significant in Mexico, Denmark and Spain. For the remaining countries (Austria, France and Ireland), we observe a divergence between the results of the selection models (IV and Heckman) and those of the Propensity Score

[17] The approach we implement here corresponds to what Todd & Wolpin (2003) call the "contemporaneous" specification of the education production function.

Matching. It is thus rather difficult to formulate definitive conclusions regarding the effect of private education in those countries.

Regarding the persistent advantage of private schools in Belgium and Brazil, it would be interesting to examine McEwan (2000, 2001)'s hypothesis. Rather than talking about 'private' school effects, it might make more sense – at least in these countries and maybe in others – to talk about 'religious' school effects. Indeed, a majority of private schools are, *in fine*, run by religion-affiliated boards (Mc Ewan gives Catholic Schools as a representative example). According to this cultural interpretation, the better education received in private schools could be explained by religious values, such as hard work, effort, obedience, discipline, and dedication to a task, for both students and teachers. To examine thoroughly this interpretation would require more detailed data, allowing to distinguish private schools with a religious affiliation, and those that are secular or simply for profit.

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Table 1 - Number of students: breakdown by country and type of school (public/private).

Country	Topic	Number in private	Number in public	% in private	% in public
AUSTRIA	Math	330	2310	12.50%	87.50%
	Read	591	4154	12.46%	87.54%
	Science	332	2337	12.44%	87.56%
BEL_FR	Math	1074	474	69.38%	30.62%
	Read	1901	834	69.51%	30.49%
	Science	1070	448	70.49%	29.51%
BEL_NL	Math	1681	530	76.03%	23.97%
	Read	2996	894	77.02%	22.98%
	Science	1666	514	76.42%	23.58%
BRAZIL	Math	391	2319	14.43%	85.57%
	Read	693	4187	14.20%	85.80%
	Science	390	2313	14.43%	85.57%
DENMARK	Math	531	1745	23.33%	76.67%
	Read	942	3107	23.27%	76.74%
	Science	521	1727	23.18%	76.82%
FRANCE	Math	501	1841	21.39%	78.61%
	Read	902	3303	21.45%	78.55%
	Science	502	1832	21.51%	78.49%
IRELAND	Math	1339	771	63.46%	36.54%
	Read	2416	1406	63.21%	36.79%
	Science	1337	779	63.19%	36.81%
MEXICO	Math	374	2174	14.68%	85.32%
	Read	677	3889	14.83%	85.17%
	Science	374	2156	14.78%	85.22%
SPAIN	Math	1349	2079	39.35%	60.65%
	Read	2453	3761	39.48%	60.52%
	Science	1372	2085	39.69%	60.31%

Table 2 – Summary statistics: math (Mean. *Standard Deviation*)

Country	N	% girls	% with sibling	% first born	% mothers with post-secondary education	% father immigrant	HISEI	Index of cultural resources	PHISEI	SCHLOC
AUSTRIA	2640	0.50	0.87	0.36	0.65	0.12	48.96	0.26	49.02	0.31
		0.50	*0.33*	*0.48*	*0.48*	*0.33*	*14.03*	*0.79*	*7.50*	*0.46*
BEL_FR	1573	0.51	0.91	0.31	0.63	0.28	50.25	0.07	50.11	0.34
		0.50	*0.28*	*0.46*	*0.48*	*0.45*	*17.26*	*1.00*	*9.90*	*0.47*
BEL_NL	2211	0.48	0.88	0.36	0.76	0.10	48.20	0.25	48.18	0.13
		0.50	*0.32*	*0.48*	*0.43*	*0.30*	*16.37*	*0.86*	*8.32*	*0.33*
BRAZIL	2717	0.52	0.94	0.33	0.31	0.01	42.56	-1.44	42.44	0.50
		0.50	*0.24*	*0.47*	*0.46*	*0.11*	*17.18*	*1.33*	*11.21*	*0.50*
DENMARK	2382	0.49	0.94	0.34	0.73	0.10	49.77	-0.21	49.68	0.18
		0.50	*0.24*	*0.47*	*0.44*	*0.30*	*15.95*	*0.95*	*7.55*	*0.39*
FRANCE	2597	0.51	0.92	0.33	0.64	0.19	48.33	0.15	48.15	0.19
		0.50	*0.28*	*0.47*	*0.48*	*0.39*	*16.89*	*0.91*	*8.85*	*0.39*
IRELAND	2128	0.52	0.96	0.34	0.58	0.06	48.18	-0.16	48.16	0.28
		0.50	*0.19*	*0.47*	*0.49*	*0.23*	*15.21*	*1.05*	*6.24*	*0.45*
MEXICO	2567	0.50	0.97	0.27	0.27	0.04	43.02	-0.68	42.92	0.42
		0.50	*0.16*	*0.45*	*0.45*	*0.19*	*16.99*	*1.28*	*10.62*	*0.49*
SPAIN	3428	0.52	0.89	0.45	0.37	0.04	44.94	0.19	44.90	0.45
		0.50	*0.31*	*0.50*	*0.48*	*0.18*	*16.41*	*0.83*	*8.99*	*0.50*

N= number of students sampled by country
HISEI = Highest Socio-Economic Index of the two parents
PHISEI = average parental socio-economic index of schoolmates
SCHLOC = school location (urban / rural area)

Table 3 – Summary statistics: reading (% or Mean. *Standard Deviation*)

Country	N	% girls	% with sibling	% first born	% mothers with post-secondary education	% father immigrant	HISEI	Index of cultural resources	PHISEI	SCHLOC
AUSTRIA	4745	0.50	0.87	0.35	0.65	0.13	48.93	0.25	48.90	0.31
		0.50	*0.33*	*0.48*	*0.48*	*0.33*	*13.99*	*0.80*	*7.08*	*0.46*
BEL_FR	2780	0.51	0.91	0.31	0.64	0.28	50.07	0.10	50.02	0.34
		0.50	*0.29*	*0.46*	*0.48*	*0.45*	*16.99*	*0.97*	*9.25*	*0.47*
BEL_NL	3890	0.48	0.88	0.35	0.77	0.10	48.63	0.30	48.60	0.13
		0.50	*0.32*	*0.48*	*0.42*	*0.30*	*16.35*	*0.80*	*7.82*	*0.34*
BRAZIL	4893	0.52	0.94	0.33	0.31	0.01	42.57	-1.45	42.42	0.50
		0.50	*0.24*	*0.47*	*0.46*	*0.11*	*17.09*	*1.34*	*10.29*	*0.50*
DENMARK	4235	0.50	0.94	0.35	0.73	0.10	49.72	-0.22	49.66	0.18
		0.50	*0.23*	*0.48*	*0.44*	*0.30*	*16.02*	*0.93*	*6.51*	*0.39*
FRANCE	4673	0.51	0.92	0.34	0.64	0.19	48.09	0.16	47.98	0.19
		0.50	*0.27*	*0.47*	*0.48*	*0.40*	*16.85*	*0.89*	*8.23*	*0.39*
IRELAND	3854	0.52	0.96	0.34	0.57	0.06	48.46	-0.14	48.50	0.27
		0.50	*0.20*	*0.47*	*0.49*	*0.24*	*15.58*	*1.03*	*6.09*	*0.44*
MEXICO	4600	0.50	0.97	0.27	0.26	0.04	42.79	-0.68	42.71	0.42
		0.50	*0.17*	*0.44*	*0.44*	*0.20*	*17.15*	*1.28*	*10.17*	*0.49*
SPAIN	6214	0.51	0.90	0.45	0.37	0.04	44.97	0.20	44.94	0.45
		0.50	*0.31*	*0.50*	*0.48*	*0.19*	*16.37*	*0.84*	*8.70*	*0.50*

N= number of students sampled by country
HISEI = Highest Socio-Economic Index of the two parents
PHISEI = average parental socio-economic index of schoolmates
SCHLOC = school location (urban / rural area)

Table 4 – Summary statistics: science (% or Mean. *Standard Deviation*)

Country	N	% girls	% with sibling	% first born	% mothers with post-secondary education	% father immigrant	HISEI	Index of cultural resources	PHISEI	SCHLOC
AUSTRIA	2669	0.50	0.87	0.34	0.66	0.13	48.90	0.23	48.82	0.46
		0.50	*0.33*	*0.47*	*0.47*	*0.34*	*13.79*	*0.81*	*7.33*	*0.34*
BEL_FR	1542	0.51	0.91	0.29	0.64	0.29	49.86	0.09	49.79	0.47
		0.50	*0.29*	*0.45*	*0.48*	*0.45*	*16.89*	*0.96*	*9.75*	*0.13*
BEL_NL	2180	0.47	0.88	0.34	0.77	0.11	48.45	0.28	48.44	0.34
		0.50	*0.32*	*0.47*	*0.42*	*0.31*	*16.41*	*0.82*	*8.47*	*0.49*
BRAZIL	2710	0.53	0.94	0.34	0.31	0.01	42.79	-1.45	42.62	0.50
		0.50	*0.24*	*0.47*	*0.46*	*0.10*	*17.34*	*1.33*	*11.15*	*0.18*
DENMARK	2346	0.50	0.94	0.35	0.73	0.10	49.51	-0.21	49.43	0.39
		0.50	*0.23*	*0.48*	*0.44*	*0.29*	*16.09*	*0.91*	*7.21*	*0.19*
FRANCE	2592	0.51	0.92	0.34	0.64	0.19	48.24	0.16	48.20	0.39
		0.50	*0.27*	*0.47*	*0.48*	*0.39*	*17.02*	*0.89*	*8.74*	*0.27*
IRELAND	2134	0.52	0.96	0.33	0.57	0.06	48.48	-0.15	48.54	0.45
		0.50	*0.20*	*0.47*	*0.50*	*0.25*	*15.68*	*1.03*	*6.68*	*0.42*
MEXICO	2548	0.50	0.97	0.27	0.27	0.04	42.69	-0.67	42.68	0.49
		0.50	*0.17*	*0.45*	*0.44*	*0.20*	*17.28*	*1.28*	*10.73*	*0.27*
SPAIN	3457	0.51	0.90	0.44	0.37	0.04	44.97	0.20	44.99	0.50
		0.50	*0.30*	*0.50*	*0.48*	*0.18*	*16.36*	*0.84*	*9.12*	*0.46*

N= number of students sampled by country
HISEI = Highest Socio-Economic Index of the two parents
PHISEI = average parental socio-economic index of schoolmates
SCHLOC = school location (urban / rural area)

Table 5 - Sensitivity of private school attendance to being located in a large city* (Probit estimates)

Country	Topic	Marginal effect (SCHLOC=1)	Std Dev.	*z*	*p*
AUSTRIA	Math	0.45	0.08	5.9827	0.0000
	Read	0.48	0.05	8.7917	0.0000
	Science	0.52	0.07	7.2259	0.0000
BEL_FR	Math	-0.19	0.08	-2.3423	0.0192
	Read	-0.15	0.06	-2.5228	0.0116
	Science	-0.14	0.08	-1.8102	0.0703
BEL_NL	Math	-0.75	0.10	-7.6150	0.0000
	Read	-0.65	0.07	-8.9442	0.0000
	Science	-0.56	0.10	-5.8500	0.0000
BRAZIL	Math	-0.29	0.10	-3.0248	0.0025
	Read	-0.41	0.08	-5.1846	0.0000
	Science	-0.41	0.11	-3.8765	0.0001
DENMARK	Math	0.18	0.08	2.2376	0.0252
	Read	0.10	0.06	1.5884	0.1122
	Science	0.07	0.08	0.8224	0.4109
FRANCE	Math	0.38	0.08	4.6884	0.0000
	Read	0.28	0.06	4.6331	0.0000
	Science	0.25	0.08	3.0880	0.0020
IRELAND	Math	0.42	0.07	5.7242	0.0000
	Read	0.48	0.06	8.3980	0.0000
	Science	0.39	0.07	5.2127	0.0000
MEXICO	Math	0.58	0.10	5.7587	0.0000
	Read	0.54	0.08	6.9651	0.0000
	Science	0.57	0.10	5.6410	0.0000
SPAIN	Math	0.25	0.05	4.8832	0.0000
	Read	0.19	0.04	4.8012	0.0000
	Science	0.18	0.05	3.4004	0.0007

* More than 100.000 inhabitants.

Table 6 - Gross and Net differences between private and public school achievement: math

Country	Gross Diff.	OLS		IV		HECKMAN				Propensity matching (kernel)		Propensity matching (nearest neighbour)	
		ATT	t	ATT	t	ATT	P(rho=0)	rho	t	ATT	t	ATT	t
AUSTRIA	0.12	-0.30	-5.3527	-1.36	-4.0006	-0.98	0.0001	0.46	-5.6396	-0.11	-1.0624	-0.26	-2.5090
BEL_FR	0.50	0.14	3.0736	0.47	1.4131	0.38	0.7014	-0.20	0.5808	0.20	1.5294	0.13	1.2049
BEL_NL	0.64	0.19	4.2588	1.19	5.1009	0.96	0.0000	-0.55	8.1879	0.14	1.1718	0.32	2.5824
BRAZIL	1.08	0.38	5.7445	0.93	3.2218	0.61	0.0116	-0.19	5.2087	0.35	2.4098	0.29	2.2614
DENMARK	0.01	0.01	0.2954	0.30	0.8371	0.23	0.3457	-0.14	0.9636	0.03	0.3182	0.02	0.2799
FRANCE	0.07	0.16	3.6397	-0.56	-1.7755	-0.91	0.0000	0.66	-7.3799	0.15	1.8561	0.18	2.1848
IRELAND	0.29	0.07	1.5212	-0.85	-2.7076	-0.62	0.0009	0.45	-2.9170	0.11	1.2449	0.00	0.0528
MEXICO	0.78	-0.09	-1.3527	0.31	0.7759	-0.64	0.0255	0.40	-2.6930	0.18	1.6830	0.03	0.2407
SPAIN	0.40	0.13	3.3675	0.41	1.3427	0.34	0.3646	-0.14	1.4369	0.00	0.0080	0.07	0.7143

Table 7 - Gross and Net differences between private and public school achievement: reading

Country	Gross Diff.	OLS		IV		HECKMAN				Propensity matching (kernel)		Propensity matching (nearest neighbour)	
		ATT	t	ATT	t	ATT	P(rho=0)	rho	t	ATT	t	ATT	t
AUSTRIA	0.30	-0.22	-5.7698	-1.19	-4.4070	-1.12	0.0000	0.61	-12.7873	-0.05	-0.9358	-0.15	-2.1736
BEL_FR	0.55	0.24	6.9390	0.55	1.9835	0.49	0.0637	-0.24	3.0242	0.27	1.9093	0.32	2.3418
BEL_NL	0.68	0.27	8.1773	1.40	6.8484	0.87	0.0000	-0.44	10.6565	0.25	3.5821	0.25	4.6589
BRAZIL	1.11	0.31	6.2776	0.46	2.1394	0.38	0.4206	-0.06	3.6417	0.28	1.9284	0.20	1.0846
DENMARK	0.01	-0.06	-1.6392	0.05	0.1292	0.15	0.5411	-0.13	0.4361	0.00	-0.0092	0.05	0.7962
FRANCE	0.03	0.06	1.7919	-0.56	-2.1021	-0.66	0.0000	0.49	-4.3888	0.05	0.9323	0.15	3.1995
IRELAND	0.43	0.12	3.4098	-1.18	-4.0292	-0.35	0.0004	0.32	-2.5507	0.14	2.2449	0.15	2.3644
MEXICO	0.90	-0.15	-3.3195	-0.12	-0.4198	-0.58	0.0000	0.35	-6.0090	-0.03	-0.2379	-0.22	-1.9347
SPAIN	0.45	0.11	3.9919	0.45	1.7086	0.27	0.4288	-0.11	1.3357	0.14	0.6228	0.11	0.4208

Table 8 - Gross and Net difference between private and public school achievement: sciences

Country	Gross Diff.	OLS		IV		HECKMAN				Propensity matching (kernel)		Propensity matching (nearest neighbour)	
		ATT	t	ATT	t	ATT	P(rho=0)	rho	t	ATT	t	ATT	t
AUSTRIA	0.15	-0.19	-3.5300	-0.81	-2.7792	-0.99	0.0000	0.51	-6.0402	-0.01	-0.1315	-0.14	-1.5754
BEL_FR	0.40	0.19	3.9995	0.26	0.7806	1.15	0.0000	-0.65	5.6690	0.20	1.7514	0.21	2.0512
BEL_NL	0.57	0.14	2.9829	0.62	2.3513	0.52	0.0031	-0.27	3.7496	-0.03	-0.1383	0.05	0.2357
BRAZIL	0.87	0.34	4.7742	1.15	3.4930	0.65	0.0002	-0.25	5.7166	0.22	1.3764	0.21	1.1197
DENMARK	-0.02	-0.06	-1.2023	0.17	0.3767	0.10	0.5352	-0.10	0.3584	0.00	0.0363	-0.13	-1.2426
FRANCE	0.04	0.04	0.9794	-0.10	-0.3224	-0.98	0.0000	0.63	-5.5722	0.02	0.2397	0.01	0.0687
IRELAND	0.41	0.20	4.1273	-0.45	-1.3249	-0.34	0.0189	0.35	-1.4344	0.21	2.2625	0.19	2.4559
MEXICO	0.67	-0.09	-1.2851	0.25	0.6012	-0.17	0.6605	0.04	-1.1234	0.04	0.2617	0.09	0.4010
SPAIN	0.38	0.07	1.7809	0.03	0.1420	-0.15	0.4412	0.15	-0.5235	0.13	0.6221	0.06	0.2739

Table 9 - Balancing of covariates: average absolute standardised bias before/after propensity score matching (kernel)

Country	Topic	Nearest Neighbour		Kernel	
		unmatched	matched	unmatched	matched
AUSTRIA	Math	28.80	3.10	28.31	3.60
	Read	27.03	3.87	26.79	3.15
	Science	26.13	4.54	26.51	3.03
BEL_FR	Math	23.26	9.56	23.80	1.80
	Read	19.98	5.90	20.51	1.93
	Science	16.17	3.62	15.61	1.73
BEL_NL	Math	27.18	10.53	28.73	2.99
	Read	24.86	4.25	25.84	3.16
	Science	27.22	4.03	28.82	3.33
BRAZIL	Math	78.54	7.33	78.97	5.62
	Read	81.69	3.90	81.15	4.22
	Science	80.45	13.91	79.91	9.26
DENMARK	Math	6.22	4.49	5.14	2.05
	Read	8.42	3.33	7.49	1.75
	Science	10.27	3.31	9.39	1.50
FRANCE	Math	4.26	4.48	4.50	3.85
	Read	2.37	3.99	2.27	3.97
	Science	4.00	7.35	3.63	2.64
IRELAND	Math	27.02	2.19	26.99	2.97
	Read	29.85	5.88	29.84	1.89
	Science	29.99	5.70	29.89	3.87
MEXICO	Math	71.07	9.44	71.79	6.54
	Read	73.99	12.35	74.76	4.80
	Science	72.34	6.78	72.88	5.58
SPAIN	Math	33.88	8.74	34.10	2.06
	Read	35.59	5.41	35.61	1.60
	Science	35.03	4.64	35.59	2.04

Note: this table reports for each country and each topic the average (absolute) standardised bias of the different covariates. For a given covariate/regressor. the standardised (absolute) difference after matching is defined as the (absolute value of the) difference of the sample means in the treated and matched comparison sub-samples as a percentage of the square root of the average of the sample variances in the treated and comparison groups (Rosenbaum & Rubin. 1985). The values in each cell can be interpreted as bias as a percentage of standard error.

Table 10 - Common support: % of the treated matched to a control observation (kernel and nearest neighbour)

Country	Topic	On support	Off support	% total
AUSTRIA	Math	317	0	100.00%
	Read	563	6	98.95%
	Science	321	3	99.07%
BEL_FR	Math	842	179	82.47%
	Read	1442	370	79.58%
	Science	939	75	92.60%
BEL_NL	Math	1635	28	98.32%
	Read	2875	88	97.03%
	Science	1220	420	74.39%
BRAZIL	Math	347	30	92.04%
	Read	458	215	68.05%
	Science	299	79	79.10%
DENMARK	Math	496	4	99.20%
	Read	884	10	98.88%
	Science	489	1	99.80%
FRANCE	Math	468	0	100.00%
	Read	852	0	100.00%
	Science	477	1	99.79%
IRELAND	Math	1278	30	97.71%
	Read	2331	36	98.48%
	Science	1247	57	95.63%
MEXICO	Math	348	3	99.15%
	Read	586	61	90.57%
	Science	300	55	84.51%
SPAIN	Math	1237	56	95.67%
	Read	1816	538	77.15%
	Science	1048	266	79.76%