Private, Private Government-Dependent and Public schools. An International Effectiveness Analysis

Vincent Vandenberghe*

IRES-ECON-UCL
16/01/2004

Abstract: This paper aims at estimating the effect on achievement of various types of schools: private, private but government-dependent and public ones. It is based on the analysis of Reading test scores of 15-year-old students surveyed in 2002 across OECD and non-OECD countries. The estimation of the effect of private vs. public school attendance may be biased by the existence of confounding factors. An obvious start is to use standard (OLS) regression models to isolate the effect of private/public status from the other determinants of achievement like family resources or socio-economic background. But regression estimates are highly dependent on the validity of the linearity assumption. Hence, the rational for using non-parametric propensity score matching. The main result is that private government-dependent schools can have a significant positive effect on 15 year-olds' academic achievement. Regarding private independent schools, the conclusion is rather the opposite. Our results also support the view that, in most cases, expanding the size of the more effective sector would improve average achievement.

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

Keywords: education economics, human capital, resource allocation, school choice, multiple treatments evaluation, propensity score

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*Economics Department, IRES, Université Catholique de Louvain, 3, place Montesquieu, B-1348 Louvain-la-Neuve, Belgium ; tel (+32) 10 47 41 41 ; Fax(+32) 10 47 39 45 ; email : vandenberghe@ires.ucl.ac.be. We would like to thanks participants of the IRES-UCL and CEE-LSE seminars as well as Jean Ries for useful comments on earlier versions of this paper. The author assumes sole responsibility for remaining omissions and errors.

Introduction

It is clear that the production of education requires monetary resources. Yet, several studies (e.g. Hanushek, 1986, 2003; Hoxby, 2000; 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 funded or directly governed by 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 can provide part of the answer to this question. Indeed, in many countries around the world, production of education is far from being a public monopoly. It is thus not a real surprise that both private and public schools are represented in the latest OECD survey (on academic achievement used in this paper (OECD, 2002). We are here referring to the Program for International Student Assessment (PISA). This survey, carried out in 2000, is aimed at testing the competencies in Math, 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.

To avoid any confusion, the reader should take good note of the way private/public categories are defined by the OECD and also the logic underlying this classification. A school was first classified as either public or private according to whether a public agency or a private entity had the ultimate decision-making power concerning its affairs. A school is public if the principal reported that it was managed directly or indirectly by a public education authority, government agency, or by a governing board appointed by government or elected by public franchise. A school is considered as private if the principal reported that it was managed directly or indirectly by a non-government organisation (e.g., a church, a trade union, business or another private institution).

[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.

But not all privately managed schools are privately funded as often assumed. 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 largely (up to 90%) funded by public money. The Catholic and Protestant churches for example have been very active in establishing private schools. The point is that they are now largely integrated into the public system via the public funding mechanism. This specificity should be accounted for in an analysis aimed at comparing the efficiency of various types of schools. A distinction needs to be made between government-dependent and independent private schools according to the degree of dependence on government funding.

In the OECD survey school principals were asked to specify the percentage of the school's total funding received in a typical school year from: government sources; student fees or school charges paid by parents; benefactors, donations, bequests, sponsorships or parental fund-raising and other sources. Schools were classified as government-dependent private if they received 50 per cent or more of their core funding from government agencies and independent private if they received less than 50 per cent of their core funding from government agencies.

In brief, this means that in the rest of the paper we will try to assess the relative efficiency of tree types of schools: private government independent (less that 50% of public funding), private government-dependent (more than 50% of public funding) and public schools.

This paper is organized in 4 sections. Section 1 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. Section 2 presents the international data set we use, while Section 3 contains the results of our empirical analysis that we confront to those of previous studies. The last section concludes and also discusses the potential causes of private/private-government-dependent/public effectiveness differentials.

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

We are interested in measuring the effect of private (or private government-dependent) 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. Heckman, Lalonde

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

& Smith, 1999). We observe the outcomes of pupils who attend a private or a private government-dependent school and the achievement of those who attend a public school. To know the 'true' effect of a certain type of school on a particular individual, we must compare the observed outcome with the outcome that would have resulted had that student not attended that type of 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, by making some assumptions, information on non-participants can be used to derive the counterfactual for participants.

Before stating how this idea can be implemented, it is important to specify the parameters of interest when estimating treatment effects. Many types of estimates are mentioned in the literature (Heckman & Navarro, 2003; Heckman, Lalonde & Smith, 1999; Bryson, Dorsett & Purdon, 2002). The empirical analysis led in this paper will focus on two. First, 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). Second, what effect private schooling would have on an individual drawn randomly from the population – 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.

Of these two parameters, the ATT constitutes an obvious start, as it immediately makes sense for policy makers who may consider it as the most directly relevant. The first of their concerns is of course to determine whether the program has any impact. Another important concern is whether the expansion of a given program is worth considering (for instance, increasing the share of pupils attending a certain type of school). While ATT may provide answers to the question of the impact, ATE is needed to go further and assess the opportunity of a program's expansion. 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 highlighted by ATT.

1.1. The standard regression common effect model

Until recently, the standard way to estimate the effect of a treatment on educational outcomes with cross-section data was to control for observable differences between the treated and the non-treated using parametric regression models. 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:

$$A_i = X_i \beta' + \delta PRIV_i + \gamma PRIVGD_i + \varepsilon \qquad (1.)$$

where *PRIV* is a dummy variable indicating whether or not the *i*th student attended a private government independent school and *PRIVGD* another dummy capturing attendance of a private government-dependent school. If the set of independent variables X_i perfectly controls for the other determinants of achievement (mainly the student's background and other characteristics), then estimating Equation (1) with OLS yields unbiased estimates of the treatment effect. In this case, ATT and ATE are equivalent, since a homogeneous and constant response to the treatment is assumed. The rest of the paper focuses on the sensitivity of regression results to the relaxation this assumption.

1.2. Propensity Score Matching:

i) The basic idea

A major drawback of the regression model exposed above is that it imposes a linear form on the outcome equation. The private or private government-dependent school effect is assumed to adequately captured by the constant coefficient of a dummy variable. But nor economic nor education theory provides justification for imposing a particular (here linear) relation between achievement and its determinants. This same is also true of the assumed distribution of the error term. 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 or private government-dependent 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 the latter do not affect outcomes in the absence of the treatment. This identifying assumption for matching, which is also the identifying assumption for the OLS regression, is known as the Conditional Independence Assumption (CIA).

Under CIA estimators relying on matching techniques can yield unbiased estimates of the 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 desirable since the evaluator needs to be confident that all the variables affecting outcome are observed. This said, some researchers (Dehejia & Wahba, 2002) conclude that propensity matching generally replicates experimental results reasonably well.

Matching pupils directly on their vector of covariates would be infeasible, 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 $p_i = Pr(PRIV_i = 1 \mid X_i)$. In others words, counterfactual is provided by an individual j attending a public school but characterized by similar propensity to participate p_j. This considerably reduces the dimensionality problem, since the 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 covariates.

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. 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 the treated and non-treated, 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.

Even within the common support, the probability of observing two pupils with exactly the same propensity score ($p_i = p_j$) is in principle zero, since this index is a continuous variable. Various methods have been proposed to overcome this difficulty (Smith & Todd, 2000). One is to implement caliper/radius matching. Caliper matching is a variation of nearest neighbour matching that attempts to avoid "bad" matches (those for which p_j is far from p_i) by imposing a tolerance on the maximum distance $|p_i - p_j|$

allowed. That is, a match for person i is selected only if $|p_i - p_j| < \varepsilon$, where ε (the caliper) is a pre-specified tolerance. Treated persons for whom no match can be found (within the caliper) are excluded from the analysis. Thus, caliper matching is the way we impose the common support condition[3]. This matching algorithm is also labelled 'radius' as the counterfactual consists of the *mean* score of all the comparison group members within the caliper.

Finally, to obtain ATE using propensity score matching it is necessary to repeat the exercise, simply by considering the (symmetric) case where treatment means attending a public school, and computing the average treatment on the untreated (ATU). This means using pupils attending private schools to estimate the counterfactual for those registered in public schools. ATE is a weighted average of ATT and ATU, where weights are relative frequency counts.

ii) Propensity score matching and k-type treatments

Yet, given the nature of our problem (comparing the effectiveness of three types of schools), the framework developed by Rosenbaum & Rubin for a two-type situation (treated vs. non-treated) needs to be generalized. Fortunately for us this has already been done by Lechner (2000) and applied in the context of labour market program evaluation by Sianesi (2001).

Lecher (2000) and Sianesi (2001) assume a set of k different kinds of mutually exclusive treatments[4] be available to individual i. In the context of this paper the choice set of a student and his/her family in some countries[5] contains 2 types of private schools (private & private government-dependent) as well as a public school option. But in other countries it is the case that the set of choice is more limited, containing only two possibilities: a public school or one of the two types of private schools (private government-dependent in the case of Belgium). In the latter case, the Lechner-Sianesi model simplifies and can be equated to the original propensity score matching model developed by Rosenbaum & Rubin.

Interest lies in the causal average effect of a treatment relative to another treatment on achievement. A set of potential outcomes is correspondingly associated to each of the potential treatments: A_0, A_1,... A_k, with $A_{,k}$ denoting achievement for individual i receiving treatment k. Let $T\{0, 1, \ldots, K\}$ denote the actual

[3] For other ways of enforcing Common Support see Smith & Todd (2000)

[5] Austria, France, Spain

assignment to a specific treatment (i.e. attendance of a certain type of school), so that $T_i=k$ if individual i attends type k school.

In what follows, the focus will be on the pair-wise comparisons of the average effect of treatment k relative to treatment k' conditional on assignment to treatment k, for all combinations of k and k':

$$E(A_k – A_{k'}|T=k) = E(A_k|T=k) – E(A_{k'}|T=k) \text{ for } k, k'\{0, 1, \ldots, K\}, k\ \overline{k}' \qquad (2.)$$

The first term of equation (2) -- the average outcome following treatment k for individuals who have participated in k -- is observed in the data. But it is not the case of the counterfactuals of the type $E(A_{k'}|T=k)$. Assuming CIA, matching on the set of covariate X using propensity score should deliver unbiased estimates of ATT. But we are in a multi treatment context. Can propensity scores still be used to solve the dimensionality problem? Lechner (2000) demonstrates that they can. When interested in pair-wise comparisons of the various treatments, the conditioning variable of minimal dimension which ensures the balancing of observables X in the two sub-populations of interest k and k' is still given by a scalar: the *conditional* probability of treatment k given either treatment k or k':

$$p^{k|kk'}(X_i) = \frac{\Pr(T=k|X)}{\Pr(T=k|X)+\Pr(T=k'|X)} \equiv \frac{P^k(X)}{P^k(X)+P^{k'}(X)} \qquad (3.)$$

For any pair of treatments k and k', the average outcome experienced by the set of k'-participants – whose conditional propensity to participate in k is similar to that of k- participants -- identifies the counterfactual outcome participants in k would have experienced, on average, had they taken treatment k' instead.

2. Data set and estimation steps

2.1. Data and variable categories

The data we use to assess the impact of type of school on achievement is relatively unique and fairly recent. It comes from the 2000 OECD survey (the so-called 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.

Although math and science scores were available we retained only reading test scores and the corresponding data. The reason is simply that the sample size is about twice larger for the reading test, reflecting the initial choice by the PISA consortium that reading literacy should be the main domain of assessment. The reader should bear in mind that we normalized[6] the reading score variable (mean 0 and variance 1) such that all reported treatment effect estimates can be immediately interpreted as % of a standard deviation.

We only selected countries for which the *number of students* sampled and attending private or private government-dependant school i) is superior or equal to 150 and ii) represents more than 1% of the total sample. This leads to a subset of 20 countries or regions containing AUSTRIA, French-Speaking Belgium (BEL_FR), Dutch-Speaking Belgium (BEL_D), BRAZIL, Czech Republic (CZ), DENMARK, FINLAND, FRANCE, GERMANY, HUNGARY, IRELAND, ITALY, JAPAN, LUXEMBOURG, MEXICO, New Zealand (NZ), SPAIN, SWEDEN, SWITZERLAND and the UK[7]. Justification for limiting ourselves to this list is twofold. First, it makes no 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 the private sector (using vouchers for example) are interested in knowing whether private or private government-dependent schools make a difference when attended by a large (and heterogeneous) population. We are tempted to add that the second argument

[6]Normalisation to mean $\boldsymbol{M}$ and standard deviation $\boldsymbol{S}$, simply transforming x to y with formula $y= \boldsymbol{S}*(x-E(x))/S(x) + \boldsymbol{M}$

[7] We excluded KOR due to important missing data frequency among the variables of interest. We also excluded POLAND and PORTUGAL because these countries only report results for reading. 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).

suggests paying more attention to countries for which the (sample) share of private education is large[8], 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, private and private government-dependent schools, by country.

[Insert Table 1 about here]

In order to implement the techniques presented in Section 2 (OLS and Propensity Score Matching), we have built a data set that is extremely rich in terms of individual characteristics and family/socio-economic background known to affect academic achievement (see Tables 2 &3 for summary statistics by country and type of school). We retained besides age in month (*AGE*), gender (*GIRL*), the highest degree of father *(FISCED=1* if he completed some post-secondary degree, *FISCED*=0 otherwise), the immigration status of father (*FATHIM=1* if father born outside country of test, *FATHIM=0* otherwise), the highest socio-economic index of both parents (*HISEI*)[9]. PISA also contains potential determinants of achievement that are rarely available: an index of cultural resources available at home (*HEDRES*)[10], two indexes of parental communication (*CULTC*[11] & *SOCC*[12]), and index of family material wealth (*WEALTH*)[13], educational support (*FAMSUP*)[14] and two indexes reflecting the student's cultural activities (*CULTA*)[15] and potential access to cultural goods (books...) (*CULTP*)[16]. All these variables combined

[9] 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

[10] The PISA index of home educational resources was derived from students' reports on the availability and number of the following items in their home: a dictionary, a quiet place to study... This PISA index – like all the others -- corresponds to the most likely value of an implicit/latent variable from an Item Response Model.

[11] The PISA index of *cultural communication* was derived from students' reports on the frequency with which their parents (or guardians) engaged with them in: discussing political or social issues; discussing books, films or television programs; and listening to classical music.

[12] The PISA index of *social communication* was derived from students' reports on the frequency with which their parents (or guardians) engaged with them in the following activities: discussing how well they are doing at school; eating <the main meal> with them around a table; and spending time simply talking with them *a times a month* and *several times a week*.

[13] The PISA index of *family wealth* was derived from students' reports on: (i) the availability in their home of a dishwasher, a room of their own, educational software, and a link to the Internet; and (ii) the number of cellular phones, televisions, computers, motor cars and bathrooms at home.

[14] The PISA index of *family educational support* was derived from students' reports on how frequently the mother, father, or brothers and sisters worked with the student on what is regarded nationally as schoolwork. Scale scores are standardised *Warm* estimates where positive values indicate higher frequency and negative values indicate lower frequency of cultural activities during the year.

[15] The PISA index of student's activities related to classical culture was derived from students' reports on how often they had, during the preceding year: visited a museum or art gallery; attended an opera, ballet or classical symphony concert; or watched live theatre. Students responded to each statement on a four-point scale with: never or hardly ever, once or twice a year, about three or four times a year, and more than four times a year.

provided one of the best background profiles ever made available to statisticians in the field of education, probably reducing the intensity of selection on individual unobserved characteristics ; in other words, legitimising the CIA assumption underlying both OLS and matching.

Finally, private schools are identified by dummy variables (*PRIV, PRIVFG*) equal to 1 by contrast to the public schools for which these dummies equal 0.

[Insert Tables 2 & 3 about here]

We also try to account for potential peer effects[17], by including the *average* parental socio-economic index of the student' s schoolmates (*PHISEI*) in the list of covariates. We assume that the peer effect is better captured by the socio-economic mix of the peer group[18]. 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 liable to be endogenous. However, dealing with this problem would be beyond the scope of the present paper.

2.2. Estimation steps

We logically focus on the magnitude of the private/public and private government-dependent/public school differentials. We first measure *gross* differentials. We do so simply by comparing the mean values of reading test scores for each type of school.

Using the different covariates potentially explaining academic results we then run the traditional OLS model to get a first estimate of the *net* private school effect i.e. accounting for socio-economic status or profile and peer endowments.

The last and more consequent step is to implement the propensity score approach developed in section 1. The propensity score (p_i) itself is estimated via a binomial (1 treatment case) or a multinomial (2

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[16] The PISA index of possessions related to classical culture in the family home was derived from students' reports on the availability of the following items in their home: classical literature (examples were given), books of poetry and works of art (examples were given).
Scale scores are standardised Warm estimates, where positive values indicate a greater number of cultural possessions while negative values indicate fewer cultural possessions in the student's home.
[17] For examples of studies focusing on this issue see Coleman (1966), Jencks & Meyer (1987), Brueckner & Lee (1989), Bénabou (1996), Glewwe (1997), Vandenberghe (2002).
[18] The student's own parental socio-economic index (HISEI) is thus excluded from the average.

treatments case) logit models[19]. Matching is based on the caliper/radius approach (Smith & Todd, 2000) which we implement using a software called PSMATCH2[20] and imposing that caliper=0.01. All standard errors were obtained by bootstrapping (50 iterations).

3. Results and analysis

In Tables 4 & 5 below, we present into great details the three types of results of interest: [1] the gross score differential between private, private government-dependent and public students, [2] the coefficient associated to the *PRIV* and *PRIVGD* dummy *(δ, γ)* in an OLS regression model, [3] the estimates of the Average Treatment effect on the Treated (ATT) obtained via propensity score matching, using a radius algorithm and imposing common support (caliper=0.01). We also report the other estimates derived from our propensity score matching analysis: the average treatment effect on the untreated (ATU), and the average treatment effect (ATE).

Tables 6 & 7 give detailed information about the performance of the match. Each cell contains the *average* standardised bias of the different covariates, before and after matching. For each covariate, the standard bias is computed as the absolute difference in means divided by the square root of the average of the two associated variances, and multiplied by 100 (Rosenbaum & Rubin. 1985). Standard bias thus expresses covariate imbalance in percent of (average) standard deviation.

There is no clear reference against which to judge the performance of the match, but comparing the values of Tables 6 & 7 with those of other studies (Bryson, Dorsett & Purdon, 2002) suggests an adequate match. Exceptions are the UK and New-Zealand (Table 6) where, even after matching, characteristics of pupils attending private schools are still significantly different from those of the pupils in public schools.

The effect of enforcing the common support requirement is shown in Tables 8 & 9. The overall result is that the condition does not lead to dramatic loss of observations when matching pupils attending private government-dependent schools to pupils in public schools. Even when computing ATE the percentage of no-match is generally well below 5%. This level is low and is therefore unlikely to affect the robustness of ATE. However, Table 8 shows that for some countries like New Zealand, Brazil and Switzerland, up to

[19] Both the binomial and the multinomial logit are estimated on: a constant plus the whole list of covariates, without interaction or higher order terms.
[20] developped for STATA 8 by E. Leuven & B. Sianesi, and available at http://econpapers.hhs.se/software/bocbocode/S432001.htm

17% of students are dropped by imposing common support while estimating ATT. For these discarded students, the effect of private school attendance cannot be estimated.

[Insert Tables 4 & 5 about here]

3.1. Effect of treatment on the treated (ATT)

For observers and decision-makers, the first concern is of course to determine whether a particular treatment (here attending a type of schools) has any impact on achievement. In other words, they are interested by estimates of the average treatment effect of the treated (ATT). Those that we report in Tables 4 and 5 suggest that school type can have a significant effect on reading scores. Compared with the size of estimates generally obtained in the education production function literature (Hanushek, 1986), these (positive and negative) effects can be considered as sizeable.

More detailed results about ATT are essentially twofold.

First, ignoring the specific results for each country and focussing on general trends, it turns out that private schools and private but government-dependent schools do not perform equally. While the general tendency is for private schools (Table 4) to perform *less well* than public schools (-10% to -50% of a standard deviation), it is the opposite that we observe for private government-dependent schools (Table 5). The latter outperform public schools by +5 to +30% of a standard deviation, on average.

The second result is that the relevant and accurate comparison must be carried out country by country. There is only one country where students attending *private* schools outperform those attending public schools: Brazil. A close look at Table 4 indicates that the former have an advantage in the range of +21 to +30% of a standard deviation.

There is then a group of European countries (Table 5) where students attending *private government-dependent* schools significantly outperform students in public schools: French-Speaking Belgium (from +22 to +30%), Dutch-Speaking Belgium (from +22 to +24%), France (from +10 to +13%), Ireland (from +9 to +11%) and Spain (from +5 to +7%).

There is also the case of Denmark (Table 5) where private government-dependent schools appear slightly less efficient than *public* schools. Other examples of private schools (Table 4) that seem to perform significantly less than public ones are Switzerland (from -46 to -56%), Austria (from -26 to -34%), Japan (from -21 to -25%), Mexico (form -16 to -22 %) and France (from -10 to -13%).

3.2. Beyond ATT

Another important concern for decision-markets is whether *the expansion* of a given program is worth considering; or instance, increasing the share of pupils attending a certain type of school. While ATT answers to the question of a program's impact, ATE is needed to assess the opportunity of its expansion. Would schools earmarked by ATT estimates as more effective also benefit to those who *do not* currently participate? If so ATE should have *the same sign* as ATT. This seems to be the case almost everywhere[21]. In other words, expanding the size of the private sector in Brazil would lift average achievement. The latter would also rise in Belgium, France, Ireland or Spain with a bigger private government-dependent sector. Expanding the public sector would make sense in Denmark or Japan.

Another (complementary) issue is that of diminishing returns to scale. Is it for example the case that only individuals with the largest expected gains attend more effective private-government dependent schools in French-Speaking or Dutch-Speaking Belgium? If so ATE should be *smaller* than ATT. The last column of table 5 suggests that ATE is of the same magnitude as ATT. Thus, the currently observed advantage of the private-government sector would not erode in the case of expansion. And this pleads for expansion.

Quite surprisingly, there are even cases compatible with the rising return assumption. In Spain or France ATE for private government-dependent schools is *higher* than ATT. This is also true of the private sector in Brazil or the public sector in Denmark[22] and maybe Japan. If true, these results further support the relevance of policy aimed at expanding these sectors.

Signs of diminish returns are to be found in Ireland with private-government dependent schools for which ATE (+6%) is slightly lower that ATT (from +9 to +11%). Similarly, the advantage of public schools over private ones could be eroded by expansion in Mexico, Austria, France and maybe also Switzerland.

[Insert Tables 6 & 7 about here]

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[21] Mexican public sector being a possible exception.

[22] In the latter case, results reported in table 5 show that ATE is more negative than ATT, suggesting that the size of the private governement-dependent sector should be reduced to increase average achievement.

[Insert Tables 8 & 9 about here]

4. Conclusion and further comments

Results presented here derive from the analysis of PISA reading literacy test scores with both regression (OLS) and non-parametric propensity score matching. They essentially suggest that private government-dependent schools can have a significant positive effect on 15 year-olds' academic achievement. Regarding private independent schools, the conclusion is rather the opposite. And this conclusion is in line with other research paper using the same data, addressing the same issue, although with different methodology (Dronkers & Robert, 2003)[23].

But it is worth commenting these results into more details as there are exceptions to the general rule enounced above. In Brazil, private schools outperform public schools, while in Denmark, public schools do slightly better than private government-dependent schools.

In Belgium, France, Ireland and Spain, the effectiveness premium goes to the very large sector of private government-dependent schools. By contrast, Switzerland, Austria, Japan, Mexico and France are countries where private independent schools perform less well than public schools.

Still, for many of the countries and configurations examined here we would rather conclude to the absence of systematic advantage for any of the three school types.

Our results also support the view that expanding the size sectors that are more effective -- be it private (like in Brazil) or private government-dependent (as in Belgium, France, or Spain) of public (as in Denmark) – should improve average achievement.

Some questions remain unanswered however. If private government-dependent schools positive effects hold only for some countries, how can they be explained? And similarly how can one explain that in some other countries privately run schools seem to be less efficient than public ones? Two alternative, sometimes conflicting, interpretations coexist to explain private, private government-dependent vs. public

[23] Dronkers & Robert pool all countries and estimate private/private government-dependent and public effects using a fixed-random effect model.

effect. The first interpretation, which would be favoured by economists, is that the private and public dichotomy in fact points to regulation differences. This is the ‘organizational’ interpretation of achievement difference. Following this line of reasoning, private schools in Brazil, Belgium, France, Ireland or Spain could possibly perform better because they are granted more autonomy. And maybe private or private government-dependent schools have no more autonomy than public ones in all the other countries.

The problem with that interpretation is that is doesn't fit very well with our results. It is indeed hard to reconcile the 'more autonomy-more effectiveness assumption' with the poor performance of private schools in Switzerland or Austria, and – more importantly -- the fact that in the countries where both private and private government-dependent schools coexist, the latter – presumably less autonomous -- are more efficient than the former.

This leads us to a second more cultural interpretation of private/public school differential suggested by McEwan (2000) and Dronkers & Roberts (2003). Rather than talking about ‘private schools’ effects, it might make more sense – at least in some countries like Ireland, Belgium, Spain or France -- to talk about ‘religious’ school effect. Indeed, a majority of private government-dependent schools are, *in fine*, run by religion-affiliated boards (Mc Ewan refers to Catholic Schools in Latin America, Dronkers & Roberts to Protestant and Catholic Schools). According to this cultural interpretation, the better education received in private government-dependent schools could be explained by religious values. In fact, the main religions enhance values such as hard work, effort, obedience, discipline, and dedication to a task for both students and teachers (maybe also parents). This is a very seductive interpretation that tends to fit better to our results than the previous one. But it also has its limits. Results presented in this paper suggest indeed that private government-dependent schools in Germany for example do not outperform public ones. But it is an undisputable fact that most of private government-dependent schools in that country are religion-affiliated.

Further research is needed to explore these two categories of assumptions and maybe other ones. This means that we need more detailed data about the regulatory environment and management style of both public and private schools in countries in which these two types of school cohabit. And as regards private schools, following Mc Ewan's remarks, we would also need to distinguish private schools with a religious affiliation (catholic, protestant, ...), from those that are secular or simply for-profit.

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Table 1 - Number of students, breakdown by country and type of school (private/private government-dependent/public)

Country	Private	Private Government-Dependent	Public	Total	% Private	% Private Government-Dependent	% Public
AUSTRIA	253	240	4008	4501	0.06	0.05	0.89
BEL_FR	.	1800	766	2566	.	0.70	0.30
BEL_D	.	2900	814	3714	.	0.78	0.22
BRAZIL	429	.	3527	3956	0.11	.	0.89
CZ	.	313	5003	5316	.	0.06	0.94
DENMARK	.	929	3082	4011	.	0.23	0.77
FINLAND	.	150	4714	4864	.	0.03	0.97
FRANCE	321	581	3178	4080	0.08	0.14	0.78
GERMANY	.	188	4254	4442	.	0.04	0.96
HUNGARY	.	201	4500	4701	.	0.04	0.96
IRELAND	.	2279	1405	3684	.	0.62	0.38
ITALY	190	.	4468	4658	0.04	.	0.96
JAPAN	1513	.	3672	5185	0.29	.	0.71
LUXEMBOURG	.	373	2878	3251	.	0.11	0.89
MEXICO	584	.	3521	4105	0.14	.	0.86
NZ	152	.	3302	3454	0.04	.	0.96
SPAIN	491	1705	3622	5818	0.08	0.29	0.62
SWEDEN	.	154	4262	4416	.	0.03	0.97
SWITZERLAND	223	.	5416	5639	0.04	.	0.96
UK	421	.	8255	8676	0.05	.	0.95

Source: PISA (2000)

Table 2 – Summary statistics (mean), private independent (PRIV) vs public schools (PUB)

Country	Schltype	Achieve-ment Score	Age in months (AGE)	Father immigrant (FATHIM)	Father with tertiary degree (FISCED)	(GIRL)	Cultural resources (HEDRES)	Parental socio-economic index (HISEI)	Average parental socio-economic index among Peers (PHISEI)	Family wealth (WEALTH)	Parental cultural com. (CULTC)	Parental social com. (SOCC)	Family education support (FAMSUP)	Student cultural activities (CULTA)	Student cultural goods (CULTP)
AUSTRIA	PRIV	527.54	190.30	0.11	0.76	0.48	0.18	57.70	57.81	0.41	0.08	-0.22	-0.06	0.50	0.38
	PUB	494.19	189.57	0.13	0.64	0.49	0.25	47.96	47.92	0.23	-0.21	-0.31	-0.02	0.03	-0.10
BRAZIL	PRIV	476.53	188.11	0.01	0.70	0.53	-0.32	58.03	57.96	-0.27	0.59	0.43	0.21	0.01	0.16
	PUB	376.78	188.34	0.01	0.24	0.52	-1.62	39.63	39.49	-1.68	0.02	-0.04	0.27	-0.32	-0.54
FRANCE	PRIV	518.84	189.74	0.11	0.74	0.47	0.25	52.72	52.73	0.06	0.42	0.20	0.00	-0.15	-0.11
	PUB	500.29	189.40	0.19	0.62	0.51	0.14	47.78	47.69	-0.17	0.27	0.16	0.05	-0.38	-0.33
ITALY	PRIV	515.26	188.56	0.02	0.67	0.58	0.35	52.75	52.66	0.49	0.37	0.80	-0.54	0.12	0.58
	PUB	488.32	188.62	0.02	0.54	0.52	0.18	46.87	46.78	0.10	0.43	0.78	-0.57	0.00	0.35
JAPAN	PRIV	515.10	188.45	0.00	-	0.44	-0.02	52.60	53.27	-0.15	0.12	-0.22	-0.14	-0.63	-0.16
	PUB	526.80	188.65	0.00	-	0.51	0.05	49.48	50.53	-0.12	0.09	-0.18	-0.09	-0.75	-0.29
MEXICO	PRIV	495.17	188.14	0.02	0.75	0.51	0.16	58.71	58.89	-0.10	0.39	0.29	0.18	0.51	0.11
	PUB	421.26	188.12	0.04	0.25	0.50	-0.80	40.34	40.27	-1.62	-0.04	-0.08	0.22	-0.16	-0.67
NZ	PRIV	593.32	188.29	0.33	0.88	0.61	0.33	64.03	64.03	0.95	0.30	-0.02	0.21	0.38	0.38
	PUB	524.97	188.40	0.28	0.64	0.49	-0.04	51.39	51.32	0.19	0.05	-0.30	0.27	-0.10	-0.25
SPAIN	PRIV	539.52	189.73	0.03	0.79	0.47	0.36	61.59	61.44	0.49	0.51	0.36	-0.01	0.49	0.70
	PUB	478.78	189.45	0.04	0.36	0.50	0.14	41.18	41.16	-0.30	0.10	0.18	-0.11	-0.12	0.04
SWITZERLAND	PRIV	512.91	188.20	0.25	0.79	0.52	0.38	61.11	61.08	0.54	0.41	-0.03	0.19	0.45	0.45
	PUB	494.38	188.15	0.27	0.59	0.49	0.28	48.11	48.16	0.01	-0.02	-0.25	-0.01	0.02	-0.15
UK	PRIV	609.15	187.68	0.23	0.88	0.42	0.21	64.76	64.78	0.90	0.56	0.13	-0.21	0.72	0.66
	PUB	516.96	187.62	0.09	0.66	0.50	-0.06	49.55	49.46	0.32	-0.05	-0.03	0.17	-0.22	-0.25

Source: PISA (2000)

Table 3 – Summary statistics (mean), private government-dependent (PRIVGD) vs public schools (PUB)

Country	**Schltype**	**Achieve-ment Score**	**Age in months (AGE)**	**Father immigrant (FATHIM)**	**Father with tertiary degree (FISCED)**	**(GIRL)**	**Cultural resources (HEDRES)**	**Parental socio-economic index (HISEI)**	**Average parental socio-economic index among Peers (PHISEI)**	**Family wealth (WEALTH)**	**Parental cultural com. (CULTC)**	**Parental social com. (SOCC)**	**Family education support (FAMSUP)**	**Student cultural activities (CULTA)**	**Student cultural goods (CULTP)**
AUSTRIA	PRIVGD	532.98	189.09	0.16	0.73	0.63	0.30	54.53	54.57	0.35	0.03	-0.25	-0.06	0.57	0.29
	PUB	494.19	189.57	0.13	0.64	0.49	0.25	47.96	47.92	0.23	-0.21	-0.31	-0.02	0.03	-0.10
BEL_FR	PRIVGD	501.66	188.49	0.27	0.70	0.51	0.17	51.66	51.70	-0.02	0.04	-0.15	-0.13	-0.07	-0.34
	PUB	448.28	188.35	0.28	0.63	0.51	-0.06	47.80	47.63	-0.19	-0.03	-0.11	0.05	-0.29	-0.59
BEL_D	PRIVGD	550.58	188.58	0.09	0.82	0.48	0.35	49.85	49.81	-0.09	-0.42	-0.08	-0.37	0.03	-0.31
	PUB	488.11	188.70	0.15	0.71	0.46	0.12	44.54	44.57	-0.10	-0.46	-0.13	-0.23	-0.14	-0.47
CZ	PRIVGD	503.93	190.07	0.05	0.92	0.72	0.09	47.73	47.73	-0.88	-0.19	0.19	0.04	0.58	0.26
	PUB	496.48	188.82	0.05	0.92	0.52	0.10	48.75	48.70	-0.85	-0.14	0.28	0.11	0.62	0.19
DENMARK	PRIVGD	499.76	188.78	0.10	0.74	0.51	-0.17	51.27	51.18	0.53	0.14	0.11	0.24	0.49	0.05
	PUB	497.67	188.52	0.09	0.71	0.49	-0.23	49.46	49.43	0.50	0.11	0.23	0.33	0.22	-0.17
FINLAND	PRIVGD	553.09	187.35	0.09	0.63	0.53	0.02	54.27	54.34	0.02	0.19	-0.18	0.04	0.22	0.28
	PUB	544.63	187.58	0.02	0.61	0.52	0.01	49.90	49.95	0.24	-0.02	-0.20	0.05	-0.17	0.11
FRANCE	PRIVGD	494.69	189.77	0.25	0.56	0.50	0.15	45.31	44.91	-0.28	0.22	0.14	0.08	-0.35	-0.29
	PUB	500.29	189.40	0.19	0.62	0.51	0.14	47.78	47.69	-0.17	0.27	0.16	0.05	-0.38	-0.33
GERMANY	PRIVGD	561.25	188.07	0.08	0.88	0.79	0.47	57.04	56.86	0.47	0.03	-0.17	-0.15	0.56	0.43
	PUB	494.92	188.33	0.17	0.74	0.50	0.37	49.40	49.31	0.21	-0.15	-0.25	-0.09	0.02	-0.01
HUNGARY	PRIVGD	492.87	188.80	0.05	0.86	0.37	0.03	51.20	51.19	-0.78	0.34	0.43	0.10	0.95	0.42
	PUB	482.30	188.72	0.02	0.87	0.49	0.10	49.11	49.10	-0.87	0.31	0.55	0.15	0.68	0.33
IRELAND	PRIVGD	539.19	188.34	0.06	0.50	0.57	-0.06	49.99	50.05	0.09	-0.05	0.00	-0.09	0.03	0.00
	PUB	500.93	188.52	0.05	0.39	0.45	-0.29	44.57	44.60	-0.10	-0.18	-0.10	-0.05	-0.14	-0.23
LUXEMBOURG	PRIVGD	445.32	188.44	0.44	0.42	1.00	0.27	41.14	41.41	0.16	-0.26	-0.08	-0.03	-0.04	-0.15
	PUB	453.40	188.28	0.38	0.51	0.44	0.32	45.29	45.33	0.35	-0.19	-0.22	-0.11	-0.16	-0.07
NETHERLANDS	PRIVGD	544.54	187.33	0.11	0.53	0.50	0.39	52.09	51.97	0.19	-0.34	0.32	-0.12	-0.24	-0.39
	PUB	530.96	187.50	0.18	0.49	0.52	0.29	50.86	50.93	0.14	-0.30	0.30	-0.05	-0.21	-0.47
SPAIN	PRIVGD	509.08	189.68	0.04	0.50	0.53	0.28	47.79	47.82	-0.03	0.24	0.15	-0.10	0.14	0.31
	PUB	478.78	189.45	0.04	0.36	0.50	0.14	41.18	41.16	-0.30	0.10	0.18	-0.11	-0.12	0.04
SWEDEN	PRIVGD	529.62	188.50	0.29	0.79	0.57	-0.09	56.04	55.54	0.45	0.02	-0.06	0.26	0.46	0.40
	PUB	514.84	188.67	0.15	0.71	0.49	0.04	50.44	50.43	0.66	-0.15	-0.04	0.27	-0.15	0.03

Source: PISA (2000)

Table 4: Gross and Net differences between private (PRIV) and public schools (PUB)

Country	Gross Diff.	OLS		Propensity Score Matching					
		ATE=ATT	std	ATT	std	ATU	std	ATE	std
AUSTRIA	0.352	-0.340 **	(0.055)	-0.264 **	(0.053)	0.069	(0.320)	0.051	(0.302)
BRAZIL	1.096	0.298 **	(0.055)	0.215[c] **	(0.054)	0.497 **	(0.155)	0.473 **	(0.144)
FRANCE	0.199	-0.132 **	(0.047)	-0.101 **	(0.049)	0.033	(0.063)	0.022	(0.061)
ITALY	0.297	-0.064	(0.063)	0.040	(0.058)	0.189 **	(0.066)	0.184 **	(0.065)
JAPAN	-0.134	-0.214 **	(0.040)	-0.249 **	(0.050)	-0.293 **	(0.068)	-0.278 **	(0.057)
MEXICO	0.833	-0.166 **	(0.048)	-0.223 **	(0.070)	0.333 **	(0.100)	0.259 **	(0.087)
NZ	0.643	0.024	(0.078)	0.029[u][c]	(0.152)	0.179	(0.170)	0.173	(0.164)
SPAIN	0.707	-0.008	(0.055)	-0.064	(0.058)	-0.240	(0.203)	-0.225	(0.187)
SWITZERLAND	0.189	-0.563 **	(0.061)	-0.467[c] **	(0.093)	-0.133	(0.169)	-0.143	(0.165)
UK	0.917	0.019	(0.049)	-0.155[u]	(0.101)	0.404 *	(0.227)	0.378 *	(0.218)

[u]: average standardised bias after matching >10%, available only for ATT
[c]: more than 10% of individuals dropped by imposing common support
* significant at 5%
** significant at 2.5%

Table 5: Gross and Net differences between private government-dependent (PRIVGD) and public schools (PUB)

Country	Gross Diff.	OLS		Propensity Score Matching					
		ATE=ATT	std	ATT	std	ATU	std	ATE	std
AUSTRIA	0.409	-0.072	(0.054)	0.016	(0.050)	0.144 **	(0.069)	0.137 *	(0.067)
BEL_FR	0.478	0.225 **	(0.034)	0.301 **	(0.063)	0.252 **	(0.040)	0.286 **	(0.051)
BEL_D	0.675	0.237 **	(0.032)	0.219 **	(0.037)	0.254 **	(0.037)	0.227 **	(0.036)
CZ	0.079	0.052	(0.044)	0.063	(0.042)	0.095 **	(0.045)	0.093 **	(0.044)
DENMARK	0.021	-0.078 **	(0.034)	-0.066 *	(0.033)	-0.106 **	(0.038)	-0.097 **	(0.036)
FINLAND	0.096	0.026	(0.075)	0.044	(0.096)	0.049	(0.103)	0.049	(0.103)
FRANCE	-0.060	0.127 **	(0.037)	0.100 **	(0.044)	0.141 **	(0.044)	0.136 **	(0.043)
GERMANY	0.645	-0.038	(0.055)	0.049	(0.056)	0.358 **	(0.072)	0.345 **	(0.070)
HUNGARY	0.116	0.016	(0.055)	0.054	(0.072)	-0.071	(0.077)	-0.066	(0.077)
IRELAND	0.412	0.110 **	(0.035)	0.091 **	(0.039)	0.019	(0.040)	0.064 *	(0.032)
LUXEMBOURG	-0.078	0.048	(0.048)	-0.004	(0.064)	0.028	(0.050)	0.024	(0.045)
SPAIN	0.353	0.070 **	(0.028)	0.055 **	(0.026)	0.095 **	(0.032)	0.083 **	(0.029)
SWEDEN	0.159	0.034	(0.076)	0.071	(0.085)	0.109	(0.093)	0.107	(0.092)

* significant at 5%
** significant at 2.5%

Table 6: *Balancing of covariates*: average (absolute) standardised bias before and after propensity score matching : private (PRIV) vs. public schools (PUB)

Country	Before Matching	After Matching
AUSTRIA	34.52	2.21
BRAZIL	69.85	4.57
FRANCE	21.17	2.63
ITALY	22.51	6.93
JAPAN	17.60	3.46
MEXICO	72.43	6.42
NZ	55.74	15.69
SPAIN	62.21	4.82
SWITZERLAND	46.72	5.80
UK	70.65	16.34

Note: this table reports for each country and each topic, but only for ATT, the *average* (absolute) standardised bias of the different covariates. For a given covariate the standardised bias defines **as the ratio between i) the (absolute value of the) difference between the treated and matched means ii) the square root of the average of the sample variances of the two groups** (Rosenbaum & Rubin. 1985).
source: PISA (2000)

Table 7: *Balancing of covariates*: average (absolute) standardised bias before and after propensity score matching : private government-dependent (PRIVGD) vs. public schools (PUB).

Country	Before Matching	After Matching
AUSTRIA	25.92	3.91
BEL_FR	19.50	6.18
BEL_D	16.75	2.53
CZ	12.72	1.46
DENMARK	11.58	0.87
FINLAND	19.74	6.54
FRANCE	9.80	2.20
GERMANY	34.23	5.29
HUNGARY	13.34	2.87
IRELAND	17.70	2.89
LUXEMBOURG	27.33	4.05
SPAIN	15.31	2.24
SWEDEN	28.59	7.51

Note: this table reports for each country and each topic, but only for ATT, the *average* (absolute) standardised bias of the different covariates. For a given covariate the standardised bias defines **as the ratio between i) the (absolute value of the) difference between the treated and matched means ii) the square root of the average of the sample variances of the two groups** (Rosenbaum & Rubin. 1985).
source: PISA (2000)

Table 8 – *Common support*: % of the treated **not** matched to a control observation (caliper=0.01): private (PRIV) vs. public schools (PUB)

Country	ATT	ATU	ATE
AUSTRIA	0.01	0.00	0.00
BRAZIL	0.18	0.00	0.02
FRANCE	0.00	0.00	0.00
ITALY	0.01	0.00	0.00
JAPAN	0.00	0.02	0.02
MEXICO	0.04	0.00	0.01
NZ	0.14	0.00	0.01
SPAIN	0.00	0.00	0.00
SWITZERLAND	0.17	0.00	0.01
UK	0.00	0.00	0.00

source: PISA (2000)

Table 9 – *Common support*: % of the treated **not** matched to a control observation (caliper=0.01): private government-dependent (PRIVGD) vs. public schools (PUB)

Country	ATT	ATU	ATE
AUSTRIA	0.00	0.00	0.00
BEL_FR	0.02	0.00	0.02
BEL_D	0.00	0.01	0.00
CZ	0.00	0.00	0.00
DENMARK	0.00	0.00	0.00
FINLAND	0.01	0.00	0.00
FRANCE	0.00	0.00	0.00
GERMANY	0.00	0.00	0.00
HUNGARY	0.00	0.00	0.00
IRELAND	0.00	0.06	0.02
LUXEMBOURG	0.00	0.09	0.08
SPAIN	0.00	0.00	0.00
SWEDEN	0.00	0.00	0.00

source: PISA (2000)