A Reexamination of Private School Effectiveness: the Netherlands

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Abstract

This paper readdresses the issue of relative private school effectiveness in the Netherlands. Using both PISA 2006 and 2009 data, the results show that the instrumental variable approach used in Patrinos (2011) is incomplete, highly unstable and unlikely to yield credible school type effects. Therefore, a propensity score matching strategy is proposed instead. The results point to small and statistically insignificant achievement differences between public- and private school students, across all three subjects measured in the PISA data set. The institutional arrangements in the Dutch secondary education sector further support the notion that large achievement differences are not to be expected between school types, despite the extremely large between-school variances in student achievement. The findings are relevant for the ongoing debate on public-private partnerships (PPPs) in education.

Keywords: education, privatization, effectiveness

JEL: C31, I22, L33

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1. Introduction

There is a widespread consensus on the notion that a well-educated labor force is vital for any economy.[1] Consequentially, all OECD countries invest a substantial proportion (close to 6%) of national resources in education (Hansson and Charbonnier 2010).[2] Much less is known, however, on how to fund and provide education in the most effective way.

The literature provides mixed results on the effects of input-based policy measures.[3] However, nations around the world thus seek to improve their schools in order to enhance the skills and employability of their youth or to reduce inequalities in outcomes found within societies (Hansson and Charbonnier 2010). Especially in the wake of the current global economic crisis, countries face the challenges of making public finances sustainable, while also building the foundations for continued long-run economic growth.

This relationship provides incentives for establishing greater effectiveness and efficiency in schooling. One of the questions in this ongoing debate is whether private (religious) schools provide better education than public schools. Over the past decades, several policy actions have argued for the promotion of private educational systems throughout the world.[4] Some countries make a sharp distinction between the role of the public sector as education financier and that as education provider. In the Netherlands, (virtually) all education is publicly financed, including the private schools that accommodate over two-thirds of all students.[5]

[1] For example, Hanushek and Kimko (2000) find international evidence that direct measures of labor-force quality from international mathematics and science test scores are strongly related to economic growth. These results are supported by a recent study of Hansson and Charbonnier (2010) showing the potential economic gains from

[2] On average, OECD countries invest 5.7% of GDP in education. Non-tertiary education accounts for 3.7% of GDP. Figures for The Netherlands are remarkably similar, with 5.6% and 3.6% respectively.

[3] For example, Hanushek (1997) and Hanushek (2003) provide a survey of input-based policies and finds no consistent pattern in optimal resource allocations. Loeb and McEwan (2010) show some positive small-scale results, but point to the potentially unanticipated general equilibrium effects of scaling up an intervention.

[4] For example, as part of an education initiative, the World Bank has started to explore the benefits of public- private partnerships in education (Patrinos et al. 2009).

[5] Source: CBS StatLine (http://statline.cbs.nl/statweb/)

A solid body of empirical results on the causal effects of private- and public school attendance on student achievement is important for the policy debate on how (compulsory) education should be funded and provided. Patrinos (2011), using PISA 2006 data, finds that private school attendance in the Netherlands has a positive effect on student achievement of approximately 0.2σ in math and science and 0.3σ in reading. This study re-addresses this issue, using both PISA 2006 and 2009 data.

This reanalysis, together with incorporating both 2006 and 2009 PISA data, point to a number of remarkable conclusions. The instrumental variable approach proposed in Patrinos (2011) is arguably incomplete, highly unstable over time and likely to generate unreliable treatment effect estimates. Acknowledging the importance of controlling for endogenous selection processes, and with the absence of a credible instrument, a propensity score matching strategy is proposed instead. This approach sets out to control for important selection on observable characteristics. The preferred model, a weighted regression-adjusted matching model shows no effect for private school attendance. The results are remarkably stable across different model specifications. A multi-level model is added to the analysis; highlighting the large between-school differences in student achievement in the Dutch context.

Overall, based on the two PISA data sets, the results suggest that there is no empirical support for a specific school type effect in the Dutch secondary education sector. In addition, selection on unobservables is considered to be an important question for further inquiry. In particular, self-selection processes (e.g. parental motivation) are important to understand, and to observe, in order to isolate these effects from potential school type effects. Arguably, research on school type effects should distinguish between different types of private schools, control properly for exposure to previous (e.g. primary) schools, and preferably prior achievement. The PISA data set does not seem to meet these requirements.

2. Theoretical- and Policy Background

More than half a century ago, Friedman (1955) argued that, based on the benefits to society at large, it is probably appropriate for governments to finance compulsory education. However, this does not automatically imply that government should also provide education as well (Friedman, 1962). Instead, the literature provides several mechanisms through which private schools could increase student achievement.[6] First, competition across schools requires all schools to increase efficiency. Second, private entities are potentially more efficient in allocating resources in providing services (to particular groups) than the government. A third, and related, mechanism that potentially affects school- and student level achievement in large scale voucher systems is the more efficient sorting of students across different schools and school types, according to educational needs, in the presence of school choice (Ladd (2002)).

A number of studies (predominantly within the US) have contributed to this debate on the relative effectiveness of private- and public schooling. Despite the prevalence of publicly funded private schooling, European results are relatively scarce. However, the evolution of more elaborate (international) data sets is likely to contribute to this discussion. The PISA data set, in this respect, is particularly important.[7] Ever since the first cycle in 2000, the PISA data set has been used for cross-national comparisons to better understand the determinants underlying student achievement. The data set is also increasingly being utilized for providing estimates on school (type) effectiveness.[8]

[6] Focusing on student achievement only has it limitations though. For example, Levin (1998) points out, when discussing parental choice, that school environment and other family goals are important choice determinants as well. In addition, as a measure of social outcome, student achievement is also limited and ignores important skills, values, attitudes and other behaviors.

[7] The Programme for International Student Assessment (PISA) takes place every three years and the survey has been designed to collect information about 15-year-old students in participating countries. Every three years, it assesses how far students near the end of compulsory education have acquired some of the knowledge and skills essential for full participation in society (OECD (2007))

[8] For an example, see Vandenberghe and Robin (2004)

Also important in the context of this study is the current debate on public-private partnerships (PPPs) in education. A recent WorldBank report defines the concept of public-private partnerships in education as a system that recognizes the existence of alternative options for providing education services besides public finance and public delivery (Patrinos et al., 2009). While different systems of PPPs are conceivable, the report examines PPPs in which government guides policy and provides funding with the private sector involved in delivering the actual education services. It seems that the Dutch education context is one that fits this specific definition of PPPs. In particular, the Netherlands are mentioned as exemplary of a country in which such a sharp distinction between the role of the public sector as education financier and that of the private sector as education provider is in place.[9]

From a theoretical perspective, several arguments have been raised in favor and against the increasing emergence of PPPs. Arguments in favor of PPPs, are that (i) PPPs increase competition for students, providing all schools with incentives to increase educational quality, (ii) PPPs have greater flexibility in providing a more optimal match between demand and supply of education services, (iii) PPPs allow governments to engage in contracting with private providers through which specific quality requirements are ensured at the lowest potential cost, and (iv) PPPs achieve increased risk-sharing between the public and private sector, further enhancing efficiency in resource allocation in education. On the opposing side, it has been argued that PPPs will actually (i) lead to privatization of education, thereby reducing the government's ability to control a public service, (ii) increase household choice which could promote (socioeconomic) segregation and (iii) leave poorer students behind in those low-quality public schools abandoned by the more educated parents (Patrinos et al., 2009).

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[9] More on (the appropriateness of) this interpretation in the next section, describing the Dutch Context in more detail.

In terms of the available empirical evidence on PPPs, Woessmann (2006) finds that systems of PPPs that combine private operation with public funding do best among all possible operation-funding combinations, and that the advantage of private operation is particularly strong in countries with large shares of public funding. While it is acknowledged that more rigorous evidence is needed, the WorldBank evaluation on PPPs concludes that, in general, private management of public schools tends to be more efficient and yields higher test scores than public institutions, when students reach the end of basic education (Patrinos et al., 2009). Distinguishing between public and private provision of education, a recent OECD evaluation of achievement argues that, after accounting for the socio-economic and demographic profiles of students and schools, students in OECD countries who attend private schools show performance that is similar to that of students enrolled in public schools (OECD, 2010).

In the latest analysis of a PPP system, that of Dutch secondary education, Patrinos (2011) uses PISA 2006 data and an instrumental variable approach. Private school attendance is estimated to lead to higher test scores in math, reading and science achievement. The effects are relatively large with 0.19, 0.31 and 0.21 of a standard deviation respectively (Patrinos, 2011). The author concludes that, not only is private school attendance contributing to achievement, but that this is made possible through the educational choice- and financing model used in the Netherlands (Patrinos, 2011 and Lewis & Patrinos, 2011).

3. The Dutch Context

3.1 Freedom of Education and Parental Choice

Historically, in the Netherlands, the choice between private- and public schools was closely related to choices made in other areas of social life (e.g. church, voting, clubs membership, media etc.).[10] In the 19th century, parental choice for their children's school was at the heart of political debate. The issue revolved around the notion of "freedom of education" and the separation of church and state.[11] The Dutch solution to the issue was the creation of a system in which "money follows the student", irrespective of whether this is a public- or private school. Initially, parental choice was largely based on the religion and specific subculture they belonged to.

Parental choice on educational grounds (i.e. quality of schooling) arguably did not exist until the first half of the 20th century (Dronkers (1995)). Private non-religious schools have increasingly been established. These are based on ideological or pedagogical grounds and currently account for little over a quarter of all secondary schools. The private government-funded sector thus comprises both religious and non-religious schools. Currently, at the secondary level, these schools collectively account for 72% of all enrollments.[12]

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[10] Consequentially, all parts of social life were segmented up to 1960, referred to as "pillarisation" (James (1984))

[11] Religious schools, that received no government support up to that point, demanded equal financial treatment compared to their public government-funded counterparts. The Dutch called it "De Schoolstrijd" (The Battle of the Schools). The current debate on school vouchers and school choice in America is arguably quite similar (Hooker (2009))

[12] Source: CBS StatLine (http://statline.cbs.nl/statweb/)

3.2 Financial Arrangements and Standardization

All government-funded schools are thus funded equally. It is required that all primary and secondary schools receiving public funds must be not-for-profit.[13] Money follows students, and each school receives for each student enrolled a sum equivalent to the per capita cost of public schooling (Patrinos (2002)).[14] The funding of personnel costs, as well as most other current expenses, is based on the number of students enrolled. There is no parental ”topping up”.[15] Consequentially, there does not exist a financially induced stratified system of schools. In particular, financial criteria are considered to be irrelevant with respect to school choice (Dronkers (1995)).

Teacher salaries are based on nationally determined scales, from which schools have little discretionary power to deviate.[16] However, schools are relatively autonomous in the organization of teaching; especially regarding pedagogy and textbooks. On the other hand, the Ministry of Education, Culture and Science imposes numerous standards referring to the quality of education; in particular regarding curriculum and yearly instruction hours.

A special national education inspectorate oversees whether schools fulfill these responsibilities. In addition, nationally organized exams take place in the Netherlands, both at the end of primary school and at the end of secondary schools.[17] All in all, Dutch public- and private government funded schools can be characterized as centrally controlled, but with strongly decentralized management and administration.

[13] Private independent schools are allowed to be for profit, though they are unimportant with enrollment around 1-2% of total enrollment (e.g Hirsch, 2002; de Regt & Weenink, 2005). Parents often turn to these private independent institutions after a period of educational problems (i.e. student failed for examinations, was referred to a lower school type, played truant or expelled from school).

[14] Schools also receive additional funds when accommodating relatively many students from disadvantaged neighborhoods. In addition, when opening a new school, the government is required to provide initial capital costs and ongoing expenses, while the municipality provides buildings. A small fund exists for operating expenses that the school may allocate at its discretion among activities such as maintenance, cleaning, heating, libraries and teaching aids.

[15] Schools are allowed to ask for a voluntary contribution from parents. Approximately 73% of the parents report paying such an amount; the median value of which is approximately $100 dollars. Likewise, schools can ask for voluntary participation in extra-curricular activities. The median value of such contributions is around $85. (Baarsma and van Leeuwen, 2002).

[16] Teacher salaries alone account for approximately 80-90% of all current school expenditures (Patrinos, 2011).

[17] The results of these exams have been open to the public since the late 1990s.

3.3 Selectivity and Student Distribution

Government-funded schools, both public- and private dependent schools, are restricted in their ability to reject students. In general, public schools are required to accept all applicants.[18] Private government- dependent schools are allowed to reject students if they can convincingly argue the school's (religious) identity does not fit well with that of the student. Parents, on their part, are essentially free to choose whatever school they like for their child and can thus self-select to the school (type) they prefer. Of all 15-16 year old students, almost 75% attend a private dependent school. Children from whom at least one parent was born abroad account for over 20% of the student population.[19] Immigrant students are relatively overrepresented at public schools (Wieringen, 1996). However, Dronkers (2004) argues school choice in the Netherlands does not lead to a large degree of social segregation between school sectors. But, based on individual primary schools' enrollment in the four major cities, Ladd et al. (2009) find segregation levels that are high both absolutely and relative to those found in major U.S. cities.

[18] However, valid reasons to reject a student are severe capacity constraints or a strong discrepancy between the primary school's advice (and/or standardized exit exam test results) and the level of the academic tracks offered.

[19] Source: CBS StatLine (http://statline.cbs.nl/statweb/)

3.4 Student Achievement

The Netherlands perform well on international assessments of student achievement. Ever since the first cycle of TIMSS in 1995, the Netherlands scored in the top region. The same appears to hold for the Dutch performance in PISA. Unfortunately, due to a very poor response rate, results for the Netherlands were not reported for PISA 2000.[20] However, in the three consecutive cycles (2003, 2006 and 2009), the Netherlands scored well above the OECD average in all three subjects (i.e. mathematics, science and reading).

Despite these apparently reassuring results, the Dutch national authority responsible for the nation's implementation of the PISA process expresses concerns. In particular, there appears to be a noticeable downward trend for Mathematics (i.e. a drop from 538 in 2003 to 526 in 2009). Also, the relative low performance of immigrant students is deemed worrisome (De Knecht-van Eekelen et al., 2007). Based on available international data for 1964-2003, Minne et al. (2007) perform an analysis on the Dutch skill distribution and confirm that the Netherlands do not (anymore) belong to the best-performing countries. In addition, they indicate there's a declining pattern in the ranking of Dutch students along the percentiles. These observations, and particularly the declining performance in mathematics, have been restated more recently by a report put out by the Netherlands Bureau for Economic Policy Analysis (CPB, 2011).

[20] The Netherlands with 27% and the UK with 61% were the only two countries in that cycle that did not meet the threshold level of 65% and were therefore excluded from the results (www.pisa.oecd.org).

4. Review of the Empirical Literature

4.1 Endogeneity of School Choice

In the language of Rubin's Causal Model (see Holland, 1986), each student has two potential outcomes: (1) his or her achievement score under the treatment condition (i.e. private school attendance) and (2) his or her achievement score under the control condition (i.e. public school attendance). Unfortunately, for each student, only one outcome is observed. It is thus not straightforward to measure the effect of private school attendance on schooling outcomes. The main problem is that of non-random selection of students into private- and public schools. This makes it crucial to extract selection effects from school type effects. Studies that compare the schooling outcomes of private and public schools use several approaches to overcome this issue. Given the absence of randomized controlled trials (e.g. lotteries or experiments), researchers generally look for a source of variation in private school attendance that is exogenous to potential schooling outcomes.

In dealing with this problem of school choice endogeneity, research has mostly relied on three types of models to address the issue of selection bias: (1) covariate adjustment (regression) models, (2) instrumental variables models and (3) matching estimators.

The validity of the covariate adjustment approach depends on whether it is possible to eliminate selection bias by directly controlling for observable covariates (Murnane and Willett, 2010). Coleman et al. (1982) provide one of the earliest estimates of private Catholic school attendance. However, their results have been criticized on the grounds that the regression models do not adequately control for initial achievement and unobserved selection mechanisms; resulting in biased estimates for Catholic schooling (e.g. Goldberger and Cain, 1982).

In addition, the modeling of the school-choice selection process appears to be crucial and can lead to results that vary substantially across subgroups (e.g. Murnane et al., 1985; Grogger et al., 2000). The main criticism on results derived from covariate adjustments stems from the fact that the comparability of the Catholic and public school students upon entry into high school cannot be ensured (Reardon et al., 2009).

The validity of any instrumental variable approach is based on whether or not the instrument is successful in carving out exogenous variation in treatment (Murnane and Willett, 2010). Altonji et al. (2005) evaluate the credibility of religious affiliation and geographical proximity as instruments for identifying the effect of Catholic school attendance on schooling outcomes. They conclude that their results are sufficient to rule out religious affiliation as a useful instrument.

The validity of Propensity Score Matching (PSM) hinges on the ability to match treatment and control groups in such a way that differences between the two are due to treatment only and free from selection bias (Murnane and Willett, 2010). The main idea behind the PSM technique is that it approximates a quasi-experimental design with observational data by comparing individuals in a treatment group (e.g. students in private schools) to those in a control group (e.g. students in public schools), in which both groups are similar in the probability of receiving treatment based on observable characteristics. In their PSM approach, Dronkers and Avram (2010) acknowledge the limitations of this design. Two important assumptions need to be met to effectively deal with selection bias. First, all observable variables influencing both the treatment (private school attendance) and the outcome (student achievement) must be included in the propensity score model. This is necessary to assure the necessary assumption of conditional independence. Second, all (unobserved) selection processes need to be captured by the observed variables used to predict the propensity to treatment. There is no way to truly rule out this latter problem.

The above arguably explains why mixed results are found in terms of the ability of PSM approaches to replicate results from random-assignment experiments (Murnane and Willett, 2010). Morgan (2001) also highlights these weaknesses of PSM estimates and suggests using them in addition to estimates based on other designs. Dronkers and Avram (2010) agree with this and also point to the issue of no considerable overlap in the propensities of those in the treatment- and control group. In this case, the differences in achievement will not reflect the average treatment effect of attending a private school for the full sample. Instead, it will only apply to those students who actually have the possibility to attend a private school. Therefore, the results of propensity score matching presented should be complemented with estimates based on alternative designs.

All in all, the evidence for US and international analyses with different empirical methods is rather inconclusive about the effect of private school attendance on student learning (e.g. see McEwan, 2001). Covariate adjustment approach suffers from omitted variable bias; the instrumental variable approach requires a non-problematic instrument, which is hard to find. The PSM models share the similar weaknesses of covariate adjustment approaches.

4.2 Empirical Results for the Netherlands

Rutkowski and Rutkowski (2009) show that higher private school achievement is not uniform across educational systems. Therefore, evidence from cross-national comparisons may not be relevant for the Netherlands. One of the earliest empirical studies on the Dutch system, by van Van Laarhoven et al. (1990), indicates a positive effect of private religious schools on student attainment in secondary schools. To eliminate selection bias, the authors used covariates to control for student-intake on observed socioeconomic characteristics.

Levin (2004) examines Catholic primary schooling in the Netherlands, using two different instrumental variables (i.e. student's religion and school availability). He finds Catholic primary schools outperform Protestant and public schools, and also that the results cannot be solely explained by teaching- and school characteristics.

As mentioned, Patrinos (2011) addresses this issue for secondary education using PISA 2006 data. For this purpose, he also applies an instrumental variable strategy based on parental endorsement of the school's religious (or pedagogical) views. He finds that private school attendance in the Netherlands has a positive effect on student achievement of approximately 0.2σ in math and science and even 0.3σ in reading.

5 Data and Methodology

5.1 Data

The analysis is based on the PISA 2006 and 2009 surveys organized by the OECD under the OECD Program for International Student Assessment (PISA). The survey aims to provide internationally comparable evidence on the performance of 15-year-old students in all of the OECD- and participating partner countries. Every three years, it assesses how far students near the end of compulsory education have acquired some of the knowledge and skills essential for full participation in society (OECD, 2006). In PISA 2006 and 2009, students have been examined in terms of their skills in language, mathematics and science. For PISA 2006 and 2009, while all subjects were assessed, the main emphasis was on science and reading respectively.[21]

A representative sample of 15-year old students is selected through a two-stage sampling design. Schools are first selected and, within each selected school, classes or students are then randomly sampled. For the Netherlands, 186 schools participated in the 2009 survey, from different secondary education modes.[22] The students take pencil-and-paper tests of approximately two hours and answer their background questionnaire (approximately 20-30 minutes). Questionnaires were given to school principals (approximately 20 minutes). Further, PISA also uses its student questionnaires to collect information from students on various aspects of their home, family and school background.

[21] This focus changes with every cycle (i.e. PISA 2000 focused on reading and PISA 2003 on mathematical skills). A total of about seven hours of test items is covered, with different students taking different combinations of test items.
[22] In particular, the data set consists of 95 vmbo-schools (i.e. offering a mixture of vocational and theoretical education), 86 havo/vwo-schools (i.e. preparing students for higher professional/university education) and 5 "praktijkscholen" (i.e. offering mainly vocational education) (CITO, 2010).

School questionnaires are used to collect information from schools about various aspects of organization and educational provision in schools.[23] The full data set thus consists of standardized tests (i.e. student assessments) and self-reports (i.e. student- and school questionnaires).

The PISA data set is cross-sectional and provides no information about the length of the stay of the pupils at the current school or about the characteristics of former schools of the students. There is also no indicator for the entry-level performance at the current school. This might lead to a misspecification of the effect of the characteristics of the current school. This is particularly true if all 15-year-old pupils in a specific country have moved recently to another school (e.g. from primary- to secondary education).

5.2 Methodology

5.2.1. Ordinary Least Squares: covariate control

The first empirical approach conducted is based on ordinary least squares (OLS). An attempt is being made to isolate a potential private school effect; thereby controlling for observed student- and school-level background characteristics that are considered to be non-manipulable by the school. The objective is that, by controlling for observed selection bias, any unobserved selection is adequately dealt with as well. The OLS model to predict student achievement is given by:

$$Y_{ij} = \beta_0 + \sum_{k=1}^{m} \beta_k X_{kij} + \sum_{k=1}^{s} \delta_k S_{kj} + \gamma \text{Private}_{ij} + \varepsilon_{ij}$$

where Y_{ij} is the achievement score for student i in school j, X_{kij} a set of m student-level background characteristics and S_{tj} a set of s school-level background characteristics, γ the treatment effect of attending a private school and an error term ε_{ij}.

[23] To implement PISA nationally, each of the participating countries appoints a National Project Manager (NPM). Among the NPM's tasks is to ensure that internationally agreed common technical and administrative procedures were properly employed.

5.2.2. Instrumental Variable design: exogenous variation in treatment

In order to compare the results to those found earlier by Patrinos (2010), the identification strategy of that paper is replicated as well. The first stage in the 2SLS is given by:

$$D_{ij} = \alpha_0 + \sum_{k=1}^{m} \alpha_k X_{kij} + \sum_{k=1}^{s} \phi_k S_{kj} + \delta IV_{ij} + \upsilon_{ij}$$

where D_{ij} is private school attendance, IV_i is the student's school principal response to whether parents' endorsement of the instructional or religious philosophy of the school is taken into consideration at the time of admission (D_{ij} = 1 if "yes" and 0 otherwise). As before, X_{kij} is a set of m student-level background characteristics and S_{tj} a set of s school-level background characteristics. The predicted values for private school attendance are then used in the second stage:

$$Y_{ij} = \beta_0 + \sum_{k=1}^{m} \beta_k X_{kij} + \sum_{k=1}^{s} \delta_k S_{kj} + \gamma \hat{\mathrm{D}}_{ij} + \varepsilon_{ij}$$

where γ is the 2SLS estimate for the effect of private school attendance on student achievement. The analysis is performed three times with both PISA surveys. First of all, the same set of variables used by Patrinos (2011) is used. Next, important control variables are added (e.g. program type and immigrant status). Also, error terms are clustered at the school level, an adjustment not found in Patrinos (2011). In the third specification, control variables are chosen that are considered to be more appropriate and suffer less from missing data.

5.2.3. Propensity Score Matching: matching students on observed characteristics

In an attempt to more strictly control for observed selection bias, a propensity score matching (PSM) identification strategy is conducted. The treatment effect for an individual student of attending a private school is given by:

$$\tau_i = Y_i(1) - Y_i(0)$$

which can be described by an average treatment on the treated (ATT) effect as:

$$\tau_{ATT} = E(\tau \mid D=1) = E[Y(1) \mid D=1] - E[Y(0) \mid D=1]$$

Obviously, the counterfactual *Y(0)* is never observed. However, if matching students on the propensity score is successful, the unconfoundedness assumption holds:

$$Y_i(1), Y_i(0) \perp D_i \mid X_i$$

The unconfoundedness assumption implies that potential outcomes are independent of treatment status, conditional on certain background characteristics. If this holds, the propensity score can be expressed as an identity:

$$P_i(D) \equiv \Pr\left\{D=1 \mid X_i\right\} = E_i\left\{D_i \mid X_i\right\}$$

The equation implies that in this case, public school student achievement results can be used as a credible estimate of the unobserved counterfactual. The PSM treatment estimate is then:

$$\tau_{ATT}^{PSM} = E_{P(X \mid D=1)}\left\{E[Y(1) \mid D=1, P(X)] - E[Y(0) \mid D=0, P(X)]\right\}$$

In the first stage of this estimation strategy, the propensity score of attending a private school is predicted for all students. This is done through estimating a logistic regression model. A nearest-neighbor strategy, with replacement, is then used to estimate the treatment effect of attending a private school. In order to reduce the risk of matching students who, although similar on propensity score, are different on important determinants, a Mahalanobis distance-matching approach is conducted, which includes the propensity score as well as other variables that are considered to be potentially important confounders.

5.2.4. Multi-Level Modeling: between- and within school variances

In acknowledging the large between-school variances in the Netherlands, a multi-level-model is estimated to compare the other results with:

$$Y_{ij} = \gamma_{00} + \sum_{k=1}^{m} \beta_k X_{kij} + \sum_{k=1}^{s} \delta_k S_{kj} + U_{0j} + \lambda \text{Private}_i + \varepsilon_{ij}$$

where Y_{ij} is the achievement score for student i in school j, X_{kij} a set of m student-level background characteristics and S_{kj} a set of s school-level background characteristics, U_{0j} the aggregate achievement difference for school j, λ the treatment effect of attending a private school and an error term ε_{ij}. The model allows for fixed-effects (i.e. random intercepts) only and does not acknowledge potential heterogeneous effects (i.e. random slopes)

6. Results

The raw achievement differences between public- and private school students for PISA 2006 and 2009 are displayed in Tables 1a and 1b respectively. For PISA 2006, private school students perform relatively better for mathematics, and slightly above public school students for science and reading. However, in PISA 2009, private school students appear to perform worse on all three subjects; ranging from approximately 6 points lower (approximately 0.06σ) in science to almost 11 points (or 0.11σ) in reading. It would be naive to attribute such differences in achievement to private school attendance, since private school students are different in some observed characteristics. For example, from Table 1b it becomes clear that private school students, on average, are relatively more often to be found in vocational tracks, in lower level program types and in lower grades. Also, household characteristics, as measured by ESCS[24] and number of books at home, are relatively disadvantageous for private school students in PISA 2009. On the other hand, home educational resources tend to be relatively better for these students. Importantly, private school students are somewhat less likely to be a non-western immigrant. In what follows, the objective of all the research designs proposed in this paper is to address such potential selection biases in estimating an effect of private school attendance on achievement.

[24] The Programme for International Student Assessment (PISA) index of economic, social and cultural status (ESCS) was created on the basis of the following variables: the International Socio-Economic Index of Occupational Status (ISEI); the highest level of education of the student's parents, converted into years of schooling; the PISA index of family wealth; the PISA index of home educational resources; and the PISA index of possessions related to "classical" culture in the family home. Source: http://stats.oecd.org

6.1 Ordinary Least Squares: inability to control for unobserved bias

The results of this first attempt in estimating the effect of private school attendance, by controlling for observed student- and school level characteristics are displayed in Tables 2a and 2b. Models 1-3 give the estimated effects on student achievement for the three different subjects (i.e. mathematics, science and reading), while controlling for a parsimonious set of background characteristics. Relative to the raw achievement differences in Tables 1a and 1b, controlling for these factors largely reduces the observed achievement gaps. The only statistically significant and positive effect found for private school attendance is for mathematics in PISA 2006.

Controlling more extensively on household background characteristics further diminishes the observed achievement differences. Given the two-stage sampling design of the data, selected students attending the same school should not be considered as independent observations, as would be the case in a simple random sample. In fact, students within a school are usually more similar to each other, than compared to students attending distinct educational institutions. To account for this effect, standard errors are clustered at the appropriate (school) level of randomization.[25]

In both model specifications, all observed achievement differences between private- and public school students are statistically insignificant for PISA 2009. In particular, in the more comprehensive models (i.e. models 4-6), private school students' achievement is almost indistinguishable from public school students. The only statistically significant effect found is a positive effect of private school attendance on mathematics achievement in PISA 2006 (i.e. 0.06σ). An important limitation of this approach, as discussed, is the inability to control for unobserved selection effects; or omitted variable bias.

[25] Students within a school are offered the same school resources, may have the same teachers and, therefore, are taught a common implemented curriculum, and so on. Furthermore, it is well known that within a country, within sub-national entities and within a city, people tend to live in areas according to their financial resources. As children are more likely to attend schools close to their house, it is probable that students attending the same school come from similar social and economic backgrounds (OECD, 2006).

6.2. Instrumental Variable approach: unstable and troublesome results

Ideally, an instrumental variable (IV) approach is successful in overcoming the aforementioned limitations of the ordinary least square results. In order to have exogenous variation in treatment (i.e. private school attendance), the approach in Patrinos (2011) suggests that the school principal's answer, to whether parental endorsement of the school's religion or philosophy is taken into consideration in the student's admission process, is to be used as an instrument to predict private school attendance. Table 4 shows the instrument is a stronger predictor for private school attendance in the 2006, relative to 2009.

The objective in this section is (i) to replicate the approach in Patrinos (2011), (ii) to extend this approach by including a few important control variables (e.g. immigrant status and program type), and (iii) to propose a more refined IV approach which excludes controls that could be considered part of the treatment effect (e.g. achievement data used, and student-teacher ratio) and, furthermore, suffers less from missing data. This strategy is applied to PISA 2006, as well as to the more recent PISA 2009 data set. The results of the corresponding 2SLS estimates can be found in Table 3a and Table 3b respectively.

Most strikingly, for PISA 2006, the results show that once the models control for ESCS, non-western immigrant and –language, and program type, and with clustering error terms at the school level, the private school effects drop dramatically and turn statistically insignificant (at the 5% level of significance) for all three subjects, except for reading at the 10% level of significance (models 4-6). In addition, adding these controls considerably improves the predictive power of the model, as measured by R-squared (e.g. from 43% to 76% for mathematics). The model specifications excluding potential treatment variables and including controls with less missing data (i.e. models 7-9), drastically improve sample size (from 3737 to 4463), and yield similar results.

The results for PISA 2009 (Table 3b) display potential problems of a weak and unstable instrument. With the same model specification used in Patrinos (2011), the 2SLS estimates suggest a similarly large, but negative, effect of private school attendance on student achievement in 2009 instead. In particular, the results in models 1-3 imply an effect size of -0.3σ for mathematics, -0.2σ for reading, and even more negative for science (i.e. -0.4σ). Importantly, while much more imprecise due to a weaker first stage, the results again become statistically insignificant (for all subjects) after including important control variables (models 4-6). Again, the refined specifications drastically improve sample size (i.e. 3054 to 4271) and yield similar negative, but statistically insignificant, results for private school attendance on student achievement (models 7-9).

There are several concerns regarding the validity of this IV approach. Table 5a highlights that in PISA 2006 the instrument is associated with relatively disadvantaged minority students in vocational tracks, and in lower grades, for public schools, whereas the opposite is true for the private schools (i.e. relatively advantageous majority students in academic tracks). Also, for public schools, the instrument captures relatively more boys. For PISA 2009, the instrument captures students who have lower ESCS scores, are more likely to attend vocational tracks but, this time, are more likely to be found in lower program types across both school types (Table 5a).[26] With these different patterns for public- and private school students, using this instrument could amount to relying on endogenous variation in treatment status (i.e. private school attendance).

[26] Program type comparisons are only allowed within one data set since the categorization of this variable is different in PISA 2006 and 2009.

To get a sense of the potential implied bias in using a religious instrument, Altonji et al. (2002) propose to regress schooling outcomes on the instrument for a subset of the sample that cannot be affected by the treatment. Such a subset, in this case, would be the students who attend a public school. After obtaining the coefficient on the instrument in this regression, using the same set of control variables used earlier, this number is then to be inflated by the pick-up rate from the first-stage in the instrumental variable approach. This coefficient can be interpreted as an estimate of the implied bias for the IV estimate, based on the data used. The results of this approach show that part of the potential positive effect on mathematics in 2006 can be explained by this implied bias, whereas this bias actually largely suppresses the effect found for reading (Table 5b). This would mean that the "true" private schooling effect in PISA 2006 on reading, taking into account the implied bias from using this instrument in the data, would actually be as large as 0.8σ. For PISA 2009, the implied biases more than outweigh the negative effects for private school attendance found when applying the IV-approach to the more recent wave of data (Table 5b). Such large estimates for implied biases further enhance concerns regarding the stability of the IV approach.

In addition to the above, several other issues are worth mentioning. First of all, the first stage is relatively weak, particularly for the PISA 2009 data set (Table 4). Also, the instrument itself is quite obscure in that it can relate to both religious and/or educational philosophy endorsement of the parents. The instrument is only measured after the school choice decision has already been made (i.e. the instrument is the answer from the school principal of the attending school). This also introduces monotonicity concerns in that it is unsure whether the instrument operates in the same direction for all students. Such a violation of the monotonicity assumption occurs when, for example, households with a non-western background are more likely to pick up treatment (i.e. private school attendance) when the instrument takes on a value of 0 (e.g. a non-religious school) instead. Furthermore, the results in Dronkers (1995) depict a Dutch society that is increasingly secularizing, in which most parents base their (private) school choice decision on non-religious grounds. This increases the risk of non-random private school choice decisions (e.g. parental motivation). The fact that already over 36% of all private schools are based on non-religious grounds further supports the notion that it is difficult for this religious instrument to estimate a valid effect of private secondary school effectiveness in the Netherlands.

These issues, together with the highly unstable results portrayed across both PISA data sets are, arguably, reasons to adopt a different approach in estimating a credible estimate for private school attendance on student achievement.

6.3. Propensity Score Matching: stable and non-significant school type effects

Because the IV approach generates conflicting, unstable and (potentially) unreliable results, an alternative (i.e. PSM) approach is proposed instead, using both PISA data sets. In the first stage of this estimation strategy, the propensity score of attending a private school is predicted for all students, using the same set of covariates used before. Nearest-neighbor matching with replacement generates treatment estimates of attending a private school highly similar to the OLS results. They are presented in Table 6a and 6b for 2006 and 2009 respectively. The results for 2006 imply that academic achievement for private school students is, in general, insignificantly different from the public school counterparts to which they have been matched. For 2009, the results seem to point to small negative effects of private school attendance (i.e. -0.05σ), particularly for reading and science (Table 6b).

The results are complemented with those based on Mahalonibis distance matching, in which students are matched on the basis of their propensity score to attend a private school, whether they attend a vocational school, whether the school is in a rural area, which program and grade they are currently enrolled in, the household's educational resources as well as highest parental education, and, finally, the student's own gender and non-western immigrant status. Tables 7a and 7b gives the corresponding results, which are qualitatively similar to the estimates obtained from matching on propensity score only.

Tables 8a and 8b give insight into whether the balance of the matched samples improved, relative to the unmatched samples. Importantly, matching students on propensity scores seems to have improved the balance on important covariates such as gender, academic dummy, program type, ESCS, highest parental education and non-western immigrant status.[27] Figures 1a and 1b highlight that propensity scores sufficiently overlap for both subgroups (i.e. treated and non-treated).

Bootstrapping the standard errors (since the propensity scores they are based on are estimated) does not reveal large discrepancies. For both PISA 2006 and 2009, the bias-corrected 95% confidence intervals all include zero; thus implying statistically insignificant effects of private school attendance on academic achievement.

The difference-in-means results across matched pairs are further complemented with weighted regression-adjusted matched estimates, which allows for including any potential confounding covariates (Tables 9a and 9b). As could be expected, given the large level of overlap in the sample, these results are almost identical to the difference-in-means PSM results.[28] A last extension is to apply inverse probability treatment weights in order to increase sample size (at the expense of a stronger reliance on the validity of the propensity scores). After reweighting the control group to look like the treatment groups (i.e. creating a "pseudo-population" of controls), these models yield the average treatment on the treated (ATT) estimates reported in Tables 10a and 10b. Again, the treatment effect estimates remain similar and insignificant across all subjects, except for a small positive effect on mathematics in 2006 (i.e. 0.06σ).

[27] The only covariate that really seems to show a poorer balance is that of home educational resources; which is one of the reasons why this variable has been included in the Mahalanobis distance approach. The similarity of these results suggests this relative imbalance does not jeopardize the results.

[28] One could argue there is one difference in that a negative insignificant treatment effect for mathematics is now a small positive, but insignificant, effect instead. However, given that both results are insignificant, they are treated as qualitatively the same.

In order to compare these results quantitatively to those presented by Patrinos (2011), Rosenbaum bounds for the PSM ATT effect sizes are shown in Tables 11a and 11b. The hidden bias factors Γ, necessary to make the 2006 results found in this paper compatible with those presented by Patrinos (2011), are 1.3, 1.4 and 1.7 for mathematics, science and reading, respectively. The confidence interval for the effect found in this paper would only include the result found by Patrinos (2011) if some unobserved variable caused the odds ratio of treatment assignment to differ between the treatment and comparison groups by those factors. A factor of 1.4 roughly means matching students with actual treatment probabilities of 0.65 and 0.57. Similarly, a hidden bias factor Γ of 1.7 roughly translates to matching two students whose actual treatment probabilities are 0.67 and 0.55 respectively.

6.4. Multi-Level Model: large between-school variances, but no school type effects

In order to adhere to the two-stage sampling design of the PISA data sets, a multi-level model with fixed effects is estimated. The results are qualitatively identical to those obtained by propensity-score matching (Table 12a and 12b). In addition, this specification highlights a remarkable feature of the Dutch education sector. The between school variances in student achievement, in this system with early tracking, account for close to 70% of all the observed variation. However, despite these large between-school variances, none of the above PISA data results suggest that this variation in student achievement can be easily attributed to school type; neither public, nor private.

7. Discussion and Policy Implications

The results indicate that the observed raw differences between private- and public school students' achievement across all three subjects (i.e. mathematics, science and reading) can be largely explained by observed characteristics. In order to deal with potential unobserved selection bias, in estimating the effect of private school attendance, some source of exogenous variation in treatment needs to be present. The instrumental variable approach used by Patrinos (2011) tries to do this by exploiting a question in the school principal's questionnaire regarding parental endorsement of school (religious and/or pedagogical) values. However, the results in this paper show such an approach to be incomplete, highly unstable over time, and most likely to generate unreliable treatment effect estimates. In addition, the Dutch secularized context suggests that any instrument based on religion might not be appropriate (anymore).

This paper adopts a propensity score matching approach instead. The results are similar to the ordinary least squares estimates and suggest there is no statistically significant difference between the achievement of public- and private school students; once they are matched on observable characteristics. The results are remarkably stable across the different matching models that have been presented. The favorable amount of overlap and increase in balance after matching suggest this is a useful framework to use in the Dutch context. A sensitivity analysis based on Rosenbaum bounds points out that these results are hard to reconcile with those shown by Patrinos (2011).

The policy implications of these findings are particularly relevant for the current debate on public-private partnerships (PPPs) in education. One of the main arguments in a recent WorldBank report on this topic is that private management of public schools tends to be efficient and yields higher test scores than public institutions when students reach the end of basic education (Patrinos et al., 2009). The Dutch education sector is one of the examples used in that report. However, as shown, the empirical results for the Dutch context presented here do not support such a conclusion.

There are several potential explanations for the absence of school type effects in the Netherlands. The Dutch education system, despite being largely decentralized in terms of management and pedagogy, is centrally controlled and monitored. In particular, many of the potential mechanisms for increased student achievement (i.e. funding, teacher input, instruction hours and curriculum) are not allowed to significantly differ across school types. Zoontjens (2003) describes a converging trend in the Dutch education sector over the last few decades by pointing to an ongoing decentralization of public schools, to a decrease in religious orientation in private schools, and to the increase in cross-school type mergers. Zoontjens (2003) concludes it has become more and more difficult to actually distinguish between private- and public schools in the Netherlands.

8. Limitations and Suggestions for Further Research

The propensity score matching results indicate that matching students on observable achievement determinants yields no private school attendance effects on student achievement. While remarkably stable across different subjects and specifications, this approach is not free of threats to validity either. The validity of the PSM approach hinges on the assumption that no (non-random) selection on unobserved characteristics is present across school types. The Dutch system of universal school choice is particularly subject to risks of unobserved self-selection on behalf of the households. To what extent such self-selection (e.g. parental motivation) jeopardizes these results is an issue for further inquiry. Unfortunately, parental school choice is largely unobserved in the Dutch PISA data set.

Another important limitation is that the PISA data set does not allow distinguishing between different types of private schools (e.g. Catholic, Protestant or non-religious). This could explain the discrepancy of the results shown here with those found in Levin (2004). Alternatively, it could be that private school effects are only to be found in primary education. Another hypothesis is that initial private school effects have disappeared over the last couple of years with the ongoing decentralization of public schools.

The school (type) effects estimates are based on 2-3 years of attendance only. Students in the Netherlands have generally been exposed to 8 years of primary schooling. However, this remains unobserved, and its academic results are not taken account of, in the PISA data. In addition, the findings in this report relate to academic achievement only. There are many other relevant school type effects (e.g. graduation rates, non-cognitive skills, labor market outcomes, parental satisfaction, segregation and social cohesion). In this context, the results presented here should be interpreted alongside research on other dimensions of the Dutch education system, such as student segregation (e.g. Ladd et al., 2009; Dronkers, 2010).

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Appendix A: Tables

Table 1a: Mean Characteristics by School Type, PISA 2006

		Public		Private		All	
		mean	sd	mean	sd	mean	sd
mathematics		525.59	(86.68)	533.11	(85.45)	530.68	(85.91)
reading		504.92	(90.88)	507.67	(94.54)	506.78	(93.37)
science		523.79	(95.33)	525.42	(91.77)	524.90	(92.93)
private		0.00	(0.00)	1.00	(0.00)	0.68	(0.47)
school can fire teachers		0.98	(0.13)	1.00	(0.00)	0.99	(0.07)
achievement data used		0.76	(0.43)	0.71	(0.45)	0.73	(0.45)
school sets teacher increase		0.65	(0.48)	0.71	(0.45)	0.69	(0.46)
parents involved in budget		0.15	(0.36)	0.10	(0.30)	0.11	(0.32)
student-teacher ratio		16.25	(3.52)	15.84	(4.67)	15.96	(4.36)
math hours		2.88	(1.51)	2.87	(1.51)	2.87	(1.51)
proportion certified teachers		0.91	(0.21)	0.88	(0.18)	0.89	(0.19)
rural		0.02	(0.13)	0.03	(0.16)	0.02	(0.15)
SES of school		0.28	(0.53)	0.24	(0.43)	0.25	(0.47)
proportion academic schools		0.45	(0.50)	0.41	(0.49)	0.42	(0.49)
Students' grade:	9th	0.44	(0.50)	0.46	(0.50)	0.45	(0.50)
	10th	0.50	(0.50)	0.51	(0.50)	0.51	(0.50)
	11th	0.01	(0.08)	0.00	(0.06)	0.00	(0.07)
age		15.72	(0.29)	15.72	(0.28)	15.72	(0.28)
female		0.48	(0.50)	0.50	(0.50)	0.49	(0.50)
Mother's education:	primary	0.06	(0.23)	0.05	(0.22)	0.05	(0.22)
	lower-secondary	0.13	(0.34)	0.13	(0.33)	0.13	(0.33)
	upper-secondary	0.39	(0.49)	0.46	(0.50)	0.44	(0.50)
	university	0.39	(0.49)	0.34	(0.47)	0.35	(0.48)
books at home:	11-100	0.45	(0.50)	0.44	(0.50)	0.44	(0.50)
	101-500	0.41	(0.49)	0.42	(0.49)	0.42	(0.49)
one or more computer(s) at home		0.99	(0.09)	0.99	(0.09)	0.99	(0.09)
students' ESCS-index		0.28	(0.92)	0.24	(0.88)	0.25	(0.89)
proportion non-western immigrants		0.14	(0.35)	0.10	(0.30)	0.11	(0.32)
proportion non-western language at home		0.06	(0.24)	0.04	(0.20)	0.05	(0.22)
program type		6.98	(2.97)	6.95	(2.85)	6.96	(2.89)
home educational resources		0.08	(0.88)	0.14	(0.85)	0.12	(0.86)
home possessions		0.23	(0.89)	0.23	(0.84)	0.23	(0.86)
N		1553		3306		4859	

Table 1b: Mean Characteristics by School Type, PISA 2009

		Public		Private		All	
		mean	sd	mean	sd	mean	sd
mathematics		531.36	(88.22)	522.75	(83.75)	525.68	(85.39)
reading		515.23	(86.99)	504.50	(85.28)	508.14	(86.01)
science		525.74	(101.37)	519.66	(88.48)	521.72	(93.09)
private		0.00	(0.00)	1.00	(0.00)	0.66	(0.47)
school can fire teachers		1.00	(0.00)	1.00	(0.00)	1.00	(0.00)
achievement data used		0.69	(0.46)	0.72	(0.45)	0.71	(0.45)
school sets teacher increase		0.75	(0.43)	0.74	(0.44)	0.75	(0.44)
parents involved in budget		0.14	(0.34)	0.06	(0.24)	0.09	(0.28)
student-teacher ratio		15.56	(3.95)	15.67	(5.28)	15.63	(4.90)
math hours		166.30	(41.02)	165.93	(41.65)	166.05	(41.44)
proportion certified teachers		0.87	(0.20)	0.80	(0.24)	0.82	(0.23)
rural		0.11	(0.32)	0.22	(0.41)	0.18	(0.39)
SES of school		0.31	(0.48)	0.24	(0.44)	0.27	(0.45)
proportion academic schools		0.47	(0.50)	0.38	(0.49)	0.41	(0.49)
Students' grade:	9th	0.45	(0.50)	0.47	(0.50)	0.46	(0.50)
	10th	0.52	(0.50)	0.49	(0.50)	0.50	(0.50)
	11th	0.01	(0.08)	0.00	(0.07)	0.01	(0.07)
age		15.73	(0.28)	15.72	(0.29)	15.72	(0.29)
female		0.50	(0.50)	0.50	(0.50)	0.50	(0.50)
Mother's education:	primary	0.04	(0.19)	0.04	(0.19)	0.04	(0.19)
	lower-secondary	0.09	(0.28)	0.08	(0.27)	0.08	(0.27)
	upper-secondary	0.41	(0.49)	0.49	(0.50)	0.46	(0.50)
	university	0.33	(0.47)	0.29	(0.45)	0.30	(0.46)
books at home:	11-100	0.45	(0.50)	0.49	(0.50)	0.48	(0.50)
	101-500	0.39	(0.49)	0.35	(0.48)	0.37	(0.48)
one or more computer(s) at home		0.98	(0.12)	0.99	(0.08)	0.99	(0.09)
students' ESCS-index		0.32	(0.86)	0.24	(0.85)	0.27	(0.86)
proportion non-western immigrants		0.14	(0.35)	0.11	(0.32)	0.12	(0.33)
proportion non-western language at home		0.06	(0.23)	0.04	(0.20)	0.05	(0.21)
program type		4.59	(2.34)	4.47	(1.97)	4.51	(2.10)
home educational resources		-0.03	(0.89)	0.08	(0.81)	0.04	(0.84)
home possessions		0.32	(0.81)	0.32	(0.75)	0.32	(0.77)
competition		1.66	(0.52)	1.77	(0.49)	1.73	(0.50)
parental pressure		1.25	(0.65)	1.43	(0.58)	1.37	(0.61)
N		1795		2872		4667	

Table 2a: OLS estimates for private school attendance effect, PISA 2006

	(1) Mathematics	(2) Reading	(3) Science	(4) Mathematics	(5) Reading	(6) Science
private	6.56**	1.64	0.75	6.39**	1.64	0.59
	(3.123)	(3.923)	(2.778)	(3.142)	(3.908)	(2.802)
rural	18.12**	23.51***	11.61*	18.09**	23.35***	11.49
	(8.754)	(2.860)	(6.955)	(8.809)	(2.788)	(7.131)
vocational_dummy	-25.84***	-4.50	-24.74***	-25.45***	-4.45	-24.43***
	(4.399)	(6.949)	(4.295)	(4.388)	(6.916)	(4.322)
program type	19.27***	24.49***	20.90***	19.27***	24.26***	20.79***
	(0.773)	(1.632)	(0.748)	(0.761)	(1.627)	(0.752)
grade	20.10***	11.26***	11.66***	20.19***	11.38***	11.79***
	(1.736)	(2.105)	(2.161)	(1.750)	(2.088)	(2.188)
age	-9.40***	-3.07	0.72	-9.60***	-2.43	1.48
	(2.262)	(2.705)	(2.787)	(2.295)	(2.742)	(2.806)
female	-25.18***	13.04***	-19.04***	-25.36***	12.54***	-19.28***
	(1.415)	(1.713)	(1.703)	(1.412)	(1.694)	(1.721)
student's ESCS-index	-3.25***	0.28	-0.15	0.77	10.94***	8.49***
	(1.148)	(1.294)	(1.289)	(2.136)	(2.397)	(2.398)
books at home	11.19***	3.29**	12.54***	11.48***	4.65***	13.88***
	(1.419)	(1.606)	(1.327)	(1.489)	(1.730)	(1.326)
home educational resources	2.99***	0.37	0.18	3.39***	2.48**	2.17*
	(0.876)	(1.119)	(0.992)	(0.987)	(1.160)	(1.111)
non-western immigrant	-17.96***	-10.47**	-24.84***	-18.28***	-11.33**	-25.65***
	(3.517)	(4.759)	(3.778)	(3.478)	(4.686)	(3.710)
nonwestern-language at home	-8.06**	-25.33***	-20.14***	-8.42**	-26.38***	-20.90***
	(3.998)	(6.854)	(5.252)	(4.072)	(6.825)	(5.164)
highest parental education				-1.75**	-3.82***	-2.88***
				(0.837)	(1.035)	(0.855)
home possessions				-2.98**	-9.86***	-8.66***
				(1.408)	(1.894)	(1.668)
Constant	365.45***	271.76***	269.03***	375.49***	278.20***	268.25***
	(32.864)	(37.813)	(38.566)	(33.584)	(38.721)	(38.687)
Observations	4,588	4,588	4,588	4,528	4,528	4,528
R-squared	0.77	0.71	0.73	0.77	0.71	0.73
Adj. R-squared	0.769	0.711	0.729	0.768	0.711	0.731

Robust clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2b: OLS estimates for private school attendance effect, PISA 2009

VARIABLES	(1) Mathematics	(2) Reading	(3) Science	(4) Mathematics	(5) Reading	(6) Science
private	-3.50	-4.12	-3.05	-1.41	-0.73	-4.26
	(3.627)	(4.377)	(4.429)	(3.458)	(4.354)	(4.848)
rural	-0.98	-6.91	3.44	-0.90	-5.75	3.11
	(4.423)	(5.484)	(5.246)	(4.487)	(5.361)	(5.449)
vocational_dummy	-14.97**	-22.10***	-3.85	-12.38	-15.60**	-10.22
	(7.467)	(7.954)	(7.779)	(7.659)	(7.520)	(7.527)
program type	27.67***	25.61***	32.37***	27.76***	27.21***	29.54***
	(1.812)	(1.800)	(1.686)	(1.845)	(1.513)	(1.520)
grade	16.93***	11.67***	10.46***	18.39***	11.45***	12.38***
	(4.771)	(2.116)	(2.460)	(5.237)	(1.961)	(2.129)
age	-17.82***	-6.89**	-11.41***	-19.14***	-7.36**	-12.94***
	(3.990)	(2.887)	(3.095)	(4.128)	(2.966)	(3.013)
female	-26.54***	15.07***	-13.41***	-26.38***	14.83***	-13.89***
	(1.392)	(1.511)	(1.623)	(1.397)	(1.516)	(1.657)
student's ESCS-index	-0.80	-0.91	0.79	3.25*	5.19**	5.30**
	(1.206)	(1.272)	(1.437)	(1.887)	(2.285)	(2.423)
books at home	11.45***	12.34***	10.74***	13.25***	13.82***	12.37***
	(1.299)	(1.388)	(1.617)	(1.268)	(1.487)	(1.712)
home educational resources	3.38***	1.65	2.96***	4.60***	3.35***	4.47***
	(1.039)	(1.083)	(1.079)	(1.035)	(1.152)	(1.183)
non-western immigrant	-17.85***	-10.65**	-26.88***	-17.62***	-11.88***	-27.56***
	(4.333)	(4.563)	(6.993)	(4.227)	(4.023)	(7.121)
nonwestern-language at home	-8.02*	-9.34*	-13.12**	-4.02	-5.13	-9.14*
	(4.321)	(4.761)	(5.522)	(4.236)	(4.587)	(5.469)
single parent household				-6.64***	-1.99	-4.35*
				(1.813)	(2.488)	(2.318)
highest parental education				-1.08	-2.57**	-1.13
				(1.048)	(1.161)	(1.242)
home possessions				-5.98***	-7.07***	-7.36***
				(1.964)	(2.520)	(2.555)
competition				-5.69*	0.82	2.03
				(3.132)	(4.092)	(4.077)
parental pressure				-3.45	-5.30	-2.60
				(3.189)	(3.390)	(3.519)
Constant	704.57***	504.47***	562.82***	741.34***	514.06***	611.55***
	(72.313)	(48.781)	(53.720)	(78.123)	(49.833)	(51.140)
Observations	4,379	4,379	4,379	4,216	4,216	4,216
R-squared	0.76	0.69	0.71	0.75	0.69	0.68
Adj. R-squared	0.757	0.694	0.711	0.749	0.689	0.683

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3a: Replication and refinement of the IV approach results in Patrinos (2011), PISA 2006

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Mathematics	Reading	Science	Mathematics	Reading	Science	Mathematics	Reading	Science
private	18.96***	32.74***	20.91***	6.72	19.61*	9.12	10.91	18.92*	5.91
	(4.615)	(5.283)	(5.220)	(9.552)	(10.244)	(7.897)	(9.305)	(10.201)	(7.927)
school can fire teachers	0.36	-32.24**	14.45	4.43	-26.85***	18.15***			
	(10.501)	(12.856)	(12.101)	(7.600)	(7.426)	(5.763)			
achievement data used	5.57***	7.73***	5.17**	2.05	3.74	1.37			
	(2.025)	(2.269)	(2.215)	(4.520)	(5.001)	(3.535)			
school sets teacher increase	-3.32*	-1.04	-0.33	-5.07	-2.20	-2.61			
	(1.726)	(2.055)	(1.966)	(3.308)	(4.067)	(2.976)			
parents involved in budget	8.32***	17.01***	13.81***	2.28	10.35	7.55			
	(2.848)	(3.279)	(3.143)	(5.584)	(6.462)	(4.991)			
student-teacher ratio	0.92***	0.28	1.19***	0.09	-0.59	0.32			
	(0.257)	(0.346)	(0.293)	(0.333)	(0.795)	(0.378)			
math hours	5.20***	3.98***	4.72***	3.89***	2.30***	3.38***			
	(0.613)	(0.731)	(0.690)	(0.665)	(0.656)	(0.597)			
proportion certified teachers	-1.68	14.68***	14.16***	-1.68	13.42	14.13			
	(4.095)	(5.041)	(5.182)	(7.563)	(10.850)	(10.036)			
rural	20.87***	24.67***	12.94**	15.09**	19.20***	7.57	16.62**	21.61***	10.35*
	(4.467)	(5.010)	(5.278)	(7.649)	(6.042)	(6.116)	(8.022)	(6.340)	(5.766)
SES of school	19.06***	34.35***	35.65***	-1.86	10.61	11.61**	4.22	17.33**	12.94***
	(3.028)	(3.712)	(3.519)	(5.634)	(6.525)	(4.690)	(5.301)	(7.150)	(4.349)
academic program	84.45***	77.66***	79.84***	26.37***	14.55**	20.24***	22.19***	-1.21	18.71***
	(2.414)	(2.778)	(2.767)	(5.558)	(6.392)	(5.182)	(5.092)	(8.251)	(4.790)
student's grade: 9th	29.40***	42.38***	27.07***	-11.00*	-2.79	-16.91**	-15.31***	-13.15*	-17.99***
	(4.779)	(6.799)	(6.046)	(5.923)	(5.392)	(7.547)	(5.658)	(6.895)	(6.857)
10th	74.40***	78.32***	64.93***	15.54**	14.02***	1.66	12.17**	4.04	0.37
	(4.977)	(6.883)	(6.241)	(6.336)	(5.405)	(7.976)	(5.774)	(6.652)	(7.040)
11th	129.78***	123.52***	111.13***	58.48***	42.50***	33.28**	57.93***	33.54***	35.95***
	(10.513)	(9.393)	(11.849)	(11.118)	(9.205)	(13.524)	(11.045)	(11.247)	(13.484)
age	-18.56***	-10.44***	-8.27**	-11.56***	-3.53	-0.97	-12.28***	-5.25*	-1.36
	(3.197)	(3.610)	(3.569)	(2.622)	(3.088)	(3.127)	(2.277)	(2.870)	(2.845)
female	-23.71***	12.55***	-18.65***	-24.13***	12.65***	-18.62***	-25.67***	12.18***	-19.53***
	(1.654)	(1.909)	(1.875)	(1.501)	(2.032)	(1.827)	(1.426)	(1.708)	(1.683)
mother's education: primary	19.75***	9.69	12.69	9.80	0.74	-3.86	7.77	-5.42	-8.54
	(7.002)	(8.292)	(8.448)	(6.167)	(7.498)	(9.420)	(4.962)	(7.232)	(7.308)
lower-secondary	18.37***	8.47	18.32**	6.90	0.86	-0.13	8.22*	-5.62	-3.65
	(6.544)	(7.828)	(7.882)	(5.769)	(5.913)	(7.314)	(4.767)	(5.586)	(5.804)
upper-secondary	16.84***	13.08*	19.51***	4.10	3.00	-2.00	5.12	-3.68	-5.65
	(6.226)	(7.513)	(7.557)	(5.792)	(6.058)	(7.457)	(4.597)	(5.568)	(5.885)
university	14.81**	6.74	14.33*	5.14	-2.20	-5.25	5.44	-13.07**	-10.56
	(6.298)	(7.573)	(7.625)	(6.158)	(6.775)	(7.980)	(5.042)	(6.484)	(6.536)
books at home: 11-100	14.72***	10.56***	19.29***	10.73***	5.72**	13.31***	12.44***	8.15***	17.25***
	(2.790)	(3.505)	(3.239)	(2.533)	(2.666)	(2.472)	(2.158)	(2.740)	(2.327)
101-500	32.52***	22.14***	38.60***	22.45***	9.91***	24.57***	23.20***	11.40***	29.38***
	(2.998)	(3.659)	(3.482)	(3.157)	(3.272)	(3.014)	(2.877)	(3.579)	(2.698)
one or more computer(s) at home	19.73*	8.70	29.22**	13.38	-6.13	17.59*			
	(11.718)	(17.401)	(12.827)	(10.676)	(12.471)	(10.169)			
student's ESCS-index				-3.47***	-0.11	-0.22	-3.15**	5.91***	3.78*
				(1.314)	(1.538)	(1.523)	(1.497)	(2.021)	(1.946)
non-western immigrant				-20.12***	-4.98	-22.77***	-16.50***	-6.34	-22.72***
				(3.917)	(5.882)	(4.382)	(3.513)	(5.125)	(3.736)
non-western language at home				-9.73*	-18.38**	-21.53***	-9.13**	-25.62***	-21.97***
				(5.670)	(9.302)	(6.682)	(4.456)	(6.875)	(5.416)
program type				19.08***	20.54***	19.83***	19.50***	23.39***	20.46***
				(0.786)	(0.977)	(0.894)	(0.715)	(1.517)	(0.782)
home educational resources							3.42***	2.22*	2.01*
							(0.957)	(1.184)	(1.156)
home possessions							-1.21	-8.15***	-6.85***
							(1.344)	(1.890)	(1.723)
constant	645.17***	510.69***	429.81***	531.01***	402.89***	328.11***	565.66***	408.99***	397.40***
	(54.497)	(63.085)	(60.339)	(49.517)	(55.830)	(53.828)	(36.007)	(46.544)	(46.807)
Observations	3,838	3,838	3,838	3,737	3,737	3,737	4,463	4,463	4,463
R-squared	0.67	0.58	0.63	0.76	0.68	0.72	0.78	0.71	0.74
Adj. R-squared	0.669	0.579	0.632	0.757	0.679	0.718	0.774	0.708	0.736

Robust standard errors in parentheses
(clustered in models 4-9)
*** p<0.01, ** p<0.05, * p<0.1

Table 3b: Replication and refinement of the IV approach results in Patrinos (2011), PISA 2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Mathematics	Reading	Science	Mathematics	Reading	Science	Mathematics	Reading	Science
private	-28.02**	-23.23*	-43.90***	-32.89	-26.16	-50.81	-32.56	-32.30	-34.74
	(11.650)	(12.741)	(14.260)	(27.142)	(28.630)	(35.131)	(20.775)	(24.380)	(24.724)
school can fire teachers	3.68	0.29	1.87	2.87	-0.28	0.82			
	(2.547)	(2.930)	(3.041)	(4.869)	(6.282)	(6.635)			
achievement data used	2.52	1.24	4.80	1.69	-0.12	4.23			
	(2.627)	(2.829)	(3.206)	(5.813)	(5.333)	(6.889)			
school sets teacher increase	-6.00*	-11.40***	-16.14***	-6.16	-10.99	-16.38			
	(3.331)	(3.685)	(4.132)	(7.738)	(8.372)	(10.844)			
parents involved in budget	0.00	0.23	0.70**	-0.52	-0.33	0.12			
	(0.225)	(0.279)	(0.283)	(0.493)	(0.678)	(0.636)			
student-teacher ratio	0.16***	0.14***	0.14***	0.12***	0.10***	0.09***			
	(0.027)	(0.032)	(0.037)	(0.027)	(0.030)	(0.032)			
math hours	-12.38**	-16.90***	-12.62**	-17.38	-21.77	-18.65			
	(5.266)	(5.788)	(6.226)	(14.166)	(16.292)	(17.853)			
proportion certified teachers	7.22**	2.45	17.39***	7.78	3.56	17.36*	2.42	-2.89	7.85
	(3.304)	(3.702)	(4.013)	(7.016)	(8.056)	(9.212)	(5.791)	(6.853)	(6.985)
rural	22.88***	25.29***	31.21***	1.59	6.32	4.13	2.99	4.96	13.63
	(3.946)	(4.291)	(5.310)	(10.102)	(10.628)	(14.970)	(6.345)	(7.193)	(8.907)
SES of school	90.59***	91.13***	88.90***	19.54**	15.56*	9.90	11.62	14.55	-5.17
	(2.399)	(2.780)	(2.939)	(7.870)	(8.456)	(10.014)	(8.425)	(9.923)	(9.880)
academic program	34.13***	23.31***	22.41**	-10.92	-24.36***	-31.91***	1.54	-17.49*	-19.68*
	(7.383)	(7.983)	(10.124)	(8.952)	(9.040)	(11.814)	(8.194)	(9.425)	(10.944)
student's grade: 9th	79.63***	54.43***	57.60***	17.81*	-10.32	-15.73	28.22***	-1.79	-4.41
	(7.563)	(8.200)	(10.330)	(9.240)	(9.816)	(12.105)	(9.095)	(10.858)	(11.890)
10th	118.03***	83.90***	94.24***	43.71***	6.31	6.74	45.03***	14.31	8.19
	(13.493)	(11.952)	(15.630)	(16.238)	(13.294)	(18.310)	(13.379)	(13.545)	(15.612)
11th	-31.03***	-14.04***	-21.35***	-26.33***	-9.66**	-16.50***	-24.28***	-9.22***	-14.36***
	(3.813)	(4.277)	(4.676)	(3.288)	(3.838)	(3.765)	(2.829)	(3.098)	(3.274)
age	-27.12***	15.10***	-14.09***	-28.26***	13.92***	-15.15***	-27.78***	14.35***	-14.09***
	(1.981)	(2.130)	(2.409)	(1.820)	(1.958)	(2.214)	(1.451)	(1.604)	(1.786)
female	1.36	3.16	2.04	9.26*	8.27	11.53	6.93*	2.46	7.03
	(7.729)	(7.565)	(10.565)	(5.400)	(6.093)	(7.143)	(4.084)	(5.545)	(6.660)
mother's education: primary	13.07***	14.16***	12.79**	11.29***	14.47***	8.81	12.37***	10.22**	6.23
	(4.331)	(4.898)	(5.868)	(3.797)	(4.649)	(6.463)	(3.471)	(4.825)	(5.374)
lower-secondary	10.28***	10.29***	11.34***	10.95***	13.05***	9.61*	9.97***	7.70*	6.65*
	(3.284)	(3.541)	(4.279)	(3.134)	(3.368)	(4.913)	(2.995)	(3.981)	(4.013)
upper-secondary	5.61*	7.75**	12.30***	4.65	8.52**	9.05**	1.69	1.95	4.98
	(3.345)	(3.643)	(4.300)	(2.907)	(3.341)	(4.604)	(2.536)	(3.400)	(3.433)
university	15.84***	16.11***	12.18***	13.51***	13.97***	8.82**	14.65***	16.87***	9.82***
	(3.221)	(3.439)	(3.994)	(3.055)	(4.117)	(4.011)	(2.608)	(2.838)	(3.084)
books at home: 11-100	31.82***	32.75***	30.56***	25.05***	26.26***	21.67***	24.93***	26.88***	20.88***
	(3.371)	(3.648)	(4.166)	(3.198)	(3.761)	(4.213)	(2.739)	(2.864)	(3.307)
101-500	30.16*	29.75*	39.25**	29.27	29.92	37.61			
	(16.327)	(15.312)	(19.579)	(18.248)	(19.828)	(25.886)			
one or more computer(s) at home				-20.76***	-12.79**	-32.11***	-15.67***	-8.55*	-23.91***
				(4.057)	(5.351)	(6.573)	(4.020)	(4.586)	(6.104)
program type				25.40***	26.62***	28.54***	27.15***	26.50***	32.36***
				(1.616)	(1.681)	(1.999)	(1.903)	(2.078)	(2.360)
student's ESCS-index							-0.35	-1.59	-2.30
							(1.307)	(1.536)	(1.553)
home educational resources							3.54***	2.01	3.83***
							(1.156)	(1.365)	(1.407)
non-western language at home							-7.23*	-10.04**	-15.27**
							(4.282)	(4.804)	(6.001)
constant	875.45***	588.18***	721.88***	801.96***	514.05***	655.77***	780.89***	530.30***	628.33***
	(63.675)	(71.035)	(78.824)	(61.322)	(70.835)	(71.790)	(45.290)	(49.378)	(52.065)
Observations	3,086	3,086	3,086	3,054	3,054	3,054	4,271	4,271	4,271
R-squared	0.67	0.62	0.58	0.73	0.69	0.65	0.74	0.67	0.68
Adj. R-squared	0.666	0.617	0.578	0.732	0.688	0.647	0.737	0.669	0.682

Table 4: First Stages for IV approach in Patrinos (2011): 2006 and 2009

VARIABLES		(1) Private 2006	(2) Private 2009
instrument		0.38***	0.17***
		(0.014)	(0.018)
school can fire teachers		0.60***	0.00
		(0.018)	(0.000)
achievement data used		-0.03*	0.08***
		(0.016)	(0.021)
school sets teacher increase		0.03**	0.04**
		(0.015)	(0.021)
parents involved in budget		-0.08***	-0.13***
		(0.025)	(0.031)
student-teacher ratio		0.00	0.00
		(0.002)	(0.001)
math hours		0.00	0.00
		(0.005)	(0.000)
proportion certified teachers		0.01	-0.30***
		(0.044)	(0.037)
rural		-0.04	0.19***
		(0.037)	(0.016)
SES of school		0.01	-0.18***
		(0.027)	(0.028)
academic program		-0.10***	-0.01
		(0.019)	(0.022)
student's grade:	9th	0.10**	-0.08
		(0.046)	(0.053)
	10th	0.11**	-0.10*
		(0.048)	(0.054)
	11th	-0.01	0.02
		(0.113)	(0.164)
age		-0.00	-0.02
		(0.029)	(0.032)
female		0.01	-0.04**
		(0.014)	(0.017)
mother's education:	primary	0.04	0.00
		(0.057)	(0.058)
	lower-secondary	0.06	0.02
		(0.051)	(0.040)
	upper-secondary	0.09*	0.05
		(0.048)	(0.031)
	university	0.06	0.03
		(0.049)	(0.032)
books at home:	11-100	-0.00	0.05*
		(0.025)	(0.026)
	101-500	-0.01	0.04
		(0.026)	(0.028)
one or more computer(s) at home		0.21**	0.42**
		(0.103)	(0.170)
Constant		-0.41	0.68
		(0.475)	(0.531)
Observations		3,838	3,086
R-squared		0.20	0.16
Adj. R-squared		0.195	0.149

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5a: Univariate regressions of covariates on instrument, 2006 & 2009

DEPENDENT VARIABLES	All 2006	Private 2006	Public 2006	All 2009	Private 2009	Public 2009
rural	0.55***	0.75***	-3.88***	0.22**	0.45***	-1.28***
	(0.191)	(0.234)	(0.189)	(0.086)	(0.109)	(0.193)
grade	0.04*	0.06***	-0.12**	-0.04	-0.08*	0.04
	(0.019)	(0.021)	(0.052)	(0.030)	(0.049)	(0.039)
age	-0.01	-0.02*	0.02	0.01	0.02	-0.01
	(0.009)	(0.01)	(0.023)	(0.010)	(0.013)	(0.015)
female	-0.08	-0.07	-0.32*	0.03	0.07	-0.02
	(0.064)	(0.074)	(0.166)	(0.067)	(0.090)	(0.108)
student's escs	0.02	0.06**	-0.07	-0.14***	-0.16***	-0.08*
	(0.028)	(0.032)	(0.068)	(0.029)	(0.038)	(0.049)
non-western immigrant	-0.20**	-0.25**	0.69***	-0.15	0.20	-0.61***
	(0.102)	(0.122)	(0.200)	(0.106)	(0.151)	(0.179)
vocational program	-0.07	-0.25***	0.29*	0.56***	0.59***	0.38***
	(0.064)	(0.075)	(0.166)	(0.068)	(0.09)	(0.108)
program type	0.63***	0.89***	-0.05	-0.61***	-0.51***	-0.82***
	(0.088)	(0.105)	(0.223)	(0.07)	(0.09)	(0.133)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5b: Implied biases of the IV estimates based on the approach in Altonji et al. (2002), PISA 2006 and 2009

VARIABLES		PISA 2006 Mathematics	PISA 2006 Reading	PISA 2006 Science	PISA 2009 Mathematics	PISA 2009 Reading	PISA 2009 Science
implied bias		7.78	-51.22***	-0.64	-78.11***	-49.14**	-60.21***
		(13.621)	(13.578)	(15.408)	(16.995)	(19.780)	(20.543)
school can fire teachers		4.61	-15.26	26.21**	0.00	0.00	0.00
		(10.434)	(12.368)	(11.708)	(0.000)	(0.000)	(0.000)
achievement data used		1.63	7.97**	-3.50	0.36	-1.95	-11.77***
		(3.351)	(3.787)	(4.044)	(3.617)	(4.003)	(4.313)
school sets teacher increase		-6.91**	-5.18	4.29	-5.91	-4.20	-4.68
		(3.196)	(3.576)	(3.696)	(3.893)	(4.322)	(4.704)
parents involved in budget		26.41***	25.94***	24.53***	0.06	-17.43***	-8.31*
		(3.962)	(4.135)	(4.569)	(3.708)	(4.330)	(4.591)
student-teacher ratio		1.69***	0.81	2.37***	-1.70***	0.56	0.07
		(0.636)	(0.704)	(0.762)	(0.537)	(0.619)	(0.608)
math hours		5.50***	5.51***	5.08***	0.12***	0.07	0.09*
		(1.125)	(1.421)	(1.407)	(0.036)	(0.048)	(0.053)
proportion certified teachers		-12.35*	27.36***	6.24	-2.83	14.34*	26.81***
		(6.891)	(7.946)	(9.168)	(7.883)	(7.875)	(8.833)
rural		4.38	27.00***	-4.10	-5.47	-27.95***	-2.56
		(6.914)	(10.126)	(10.164)	(5.610)	(6.084)	(6.665)
SES of school		22.14***	34.36***	33.46***	41.84***	51.24***	87.76***
		(5.490)	(7.255)	(6.895)	(5.673)	(6.197)	(6.785)
academic program		72.21***	71.23***	68.50***	87.03***	74.53***	57.54***
		(4.776)	(5.680)	(5.644)	(4.735)	(5.371)	(5.356)
student's grade:	9th	18.95**	26.04**	12.35	38.02***	34.68**	41.84**
		(7.415)	(11.044)	(9.518)	(11.240)	(16.710)	(18.764)
	10th	61.08***	60.45***	49.32***	84.98***	61.34***	74.18***
		(7.824)	(10.951)	(9.906)	(11.517)	(16.906)	(19.049)
	11th	102.88***	94.28***	97.84***	109.96***	92.45***	103.96***
		(13.400)	(13.750)	(19.138)	(15.508)	(21.543)	(23.193)
age		-16.85***	-9.66	-9.58	-24.82***	-0.67	-6.44
		(5.503)	(6.225)	(6.322)	(5.618)	(6.454)	(6.713)
female		-19.59***	13.16***	-16.80***	-26.93***	15.91***	-9.79***
		(2.991)	(3.495)	(3.612)	(2.909)	(3.213)	(3.404)
mother's education:	primary	10.22	5.56	2.14	-9.82	-8.55	-9.89
		(12.992)	(16.201)	(14.835)	(9.542)	(8.743)	(11.252)
	lower-secondary	16.53	13.19	19.73	16.00**	15.44**	18.21**
		(12.070)	(15.065)	(13.310)	(6.338)	(6.514)	(7.787)
	upper-secondary	20.14*	22.20	19.63	9.87**	7.31	13.30***
		(11.196)	(14.358)	(12.512)	(4.157)	(4.440)	(4.917)
	university	17.04	14.59	16.47	7.54*	8.83*	18.86***
		(11.204)	(14.313)	(12.523)	(4.360)	(4.717)	(5.148)
books at home:	11-100	20.41***	15.61**	21.95***	14.29***	18.73***	14.22**
		(5.324)	(6.849)	(6.430)	(4.767)	(5.486)	(5.741)
	101-500	41.77***	27.94***	43.21***	29.66***	33.05***	31.53***
		(5.708)	(7.100)	(7.058)	(5.244)	(5.979)	(6.232)
one or more computer(s) at home		4.43	-9.85	7.17	28.95	29.19*	28.25
		(9.026)	(25.239)	(13.609)	(24.474)	(17.351)	(24.600)
Constant		641.10***	500.59***	476.18***	792.39***	348.26***	449.35***
		(90.230)	(107.277)	(104.026)	(91.102)	(103.698)	(108.437)
Observations		1,155	1,155	1,155	1,118	1,118	1,118
R-squared		0.72	0.65	0.67	0.74	0.67	0.69
Adj. R-squared		0.718	0.640	0.668	0.730	0.659	0.683

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6a: Propensity Score Matching estimates for private school attendance effect, PISA 2006

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
mathematics	Unmatched	540.18	539.50	0.69	2.601201	0.26
	ATT	540.27	534.38	5.89	3.3971	1.73
reading	Unmatched	515.77	518.89	-3.13	2.780303	-1.12
	ATT	515.82	512.71	3.11	3.588391	0.87
science	Unmatched	531.81	536.69	-4.89	2.824199	-1.73
	ATT	531.88	531.62	0.26	3.72258	0.07

Note: S.E. does not take into account that the propensity score is estimated.

Table 6b: Propensity Score Matching estimates for private school attendance effect, PISA 2009

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
mathematics	Unmatched	534.90	539.20	-4.30	2.523936	-1.7
	ATT	534.90	536.00	-1.10	3.34737	-0.33
reading	Unmatched	517.35	523.75	-6.40	2.525496	-2.53
	ATT	517.35	518.52	-1.17	3.358344	-0.35
science	Unmatched	532.79	538.17	-5.38	2.723384	-1.98
	ATT	532.79	534.94	-2.15	3.700853	-0.58

Note: S.E. does not take into account that the propensity score is estimated.

Table 7a: Propensity Score Mahalanobis Distance Matching estimates, PISA 2006

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
mathematics	Unmatched	540.21	539.51	0.70	2.60	0.27
	ATT	540.21	534.02	6.19	3.41	1.82
reading	Unmatched	515.82	518.91	-3.09	2.78	-1.11
	ATT	515.82	513.81	2.01	3.57	0.56
science	Unmatched	531.85	536.72	-4.88	2.82	-1.73
	ATT	531.85	531.93	-0.09	3.70	-0.02

Note: S.E. does not take into account that the propensity score is estimated.

Table 7b: Propensity Score Mahalanobis Distance Matching estimates, PISA 2009

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
mathematics	Unmatched	534.92	539.29	-4.37	2.525406	-1.73
	ATT	534.92	536.67	-1.76	3.290553	-0.53
reading	Unmatched	517.37	524.00	-6.63	2.524586	-2.63
	ATT	517.37	519.79	-2.42	3.310435	-0.73
science	Unmatched	532.80	538.44	-5.64	2.722492	-2.07
	ATT	532.80	538.04	-5.24	3.51647	-1.49

Note: S.E. does not take into account that the propensity score is estimated.

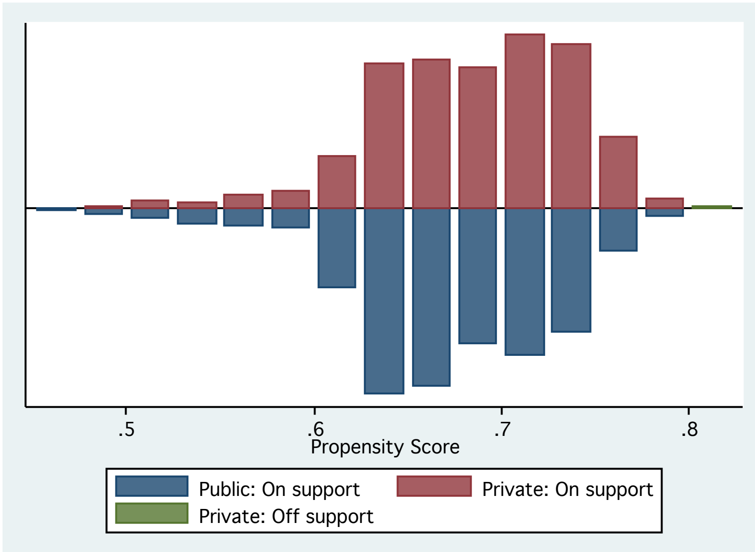
Table 8a: Balance Improvements in Matched Sample, PISA 2006

		Mean		SD	
Variable	Sample	Treated	Control	Treated	Control
female	Unmatched	0.49483	0.47963	0.5	0.5
	Matched	0.4942	0.47819	0.5	0.5
age	Unmatched	15.721	15.72	0.3	0.3
	Matched	15.721	15.711	0.3	0.3
grade	Unmatched	9.4926	9.4923	0.6	0.6
	Matched	9.4933	9.4829	0.6	0.6
program type	Unmatched	7.1854	7.4115	2.7	2.8
	Matched	7.187	7.1481	2.7	2.7
academic program	Unmatched	0.43313	0.50501	0.5	0.5
	Matched	0.43395	0.42014	0.5	0.5
student's ESCS-index	Unmatched	0.27421	0.36529	0.9	0.9
	Matched	0.27815	0.26131	0.9	0.9
home educational resources	Unmatched	0.17456	0.14252	0.8	0.9
	Matched	0.17715	0.10013	0.8	0.9
home possessions	Unmatched	0.27037	0.30785	0.8	0.9
	Matched	0.27303	0.23264	0.8	0.8
rural	Unmatched	0.02599	0.01737	0.2	0.1
	Matched	0.02447	0.02322	0.2	0.2
highest parental education	Unmatched	4.7861	4.8991	1.5	1.5
	Matched	4.7913	4.7725	1.5	1.5
non-western immigrant	Unmatched	0.09552	0.13828	0.3	0.3
	Matched	0.0957	0.10574	0.3	0.3
books at home	Unmatched	1.2961	1.3412	0.7	0.7
	Matched	1.297	1.2798	0.7	0.7
non-western language at home	Unmatched	0.03877	0.0531	0.2	0.2
	Matched	0.03884	0.03851	0.2	0.2
home cultural possessions	Unmatched	-0.28797	-0.28067	1	1
	Matched	-0.28698	-0.3581	1	1

Table 8b: Balance Improvements in Matched Sample, PISA 2009

		Mean		SD	
Variable	Sample	Treated	Control	Treated	Control
female	Unmatched	0.50505	0.5139	0.5	0.5
	Matched	0.50505	0.50361	0.5	0.5
age	Unmatched	15.704	15.722	0.3	0.3
	Matched	15.704	15.714	0.3	0.3
grade	Unmatched	-0.52274	-0.4664	0.6	0.6
	Matched	-0.52274	-0.51661	0.6	0.6
program type	Unmatched	4.796	4.8447	2	2.2
	Matched	4.796	4.7942	2	2.1
academic program	Unmatched	0.47256	0.50232	0.5	0.5
	Matched	0.47256	0.4704	0.5	0.5
student's ESCS-index	Unmatched	0.288	0.37388	0.9	0.8
	Matched	0.288	0.30498	0.9	0.8
home educational resources	Unmatched	0.11757	0.03005	0.8	0.8
	Matched	0.11757	0.01827	0.8	0.8
home possessions	Unmatched	0.34543	0.35906	0.7	0.8
	Matched	0.34543	0.32954	0.7	0.8
rural	Unmatched	0.20325	0.11414	0.4	0.3
	Matched	0.20325	0.21336	0.4	0.4
highest parental education	Unmatched	4.5227	4.6703	1.2	1.1
	Matched	4.5227	4.5628	1.2	1.2
non-western immigrant	Unmatched	0.10975	0.12283	0.3	0.3
	Matched	0.10975	0.11011	0.3	0.3
books at home	Unmatched	1.2359	1.2641	0.7	0.7
	Matched	1.2359	1.2494	0.7	0.7
non-western language at home	Unmatched	0.03662	0.04762	0.2	0.2
	Matched	0.03662	0.05052	0.2	0.2
home cultural possessions	Unmatched	-0.29418	-0.25855	1	1.1
	Matched	-0.29418	-0.30736	1	1.1

Figure 1a: Checks for common support and standard errors of PSM results, PISA 2006

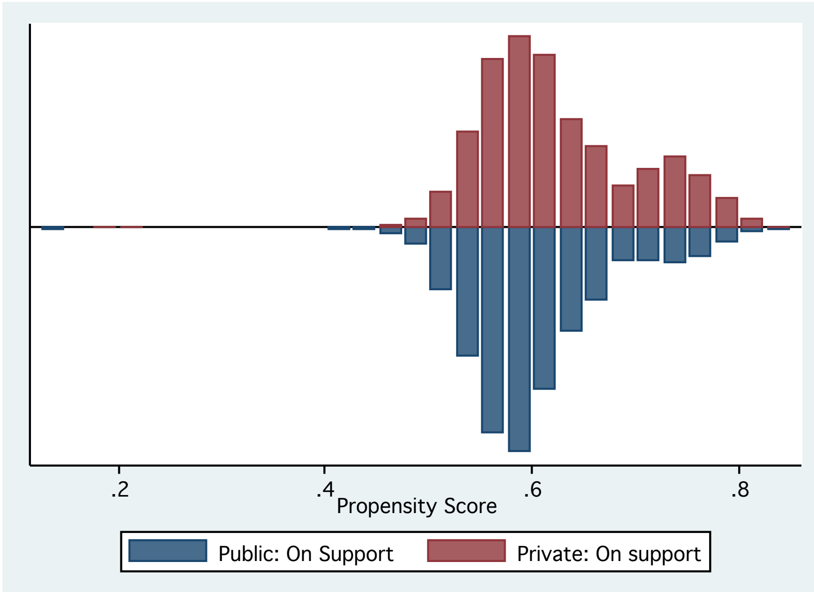


Treatment assignment	off support	on support	total
Untreated	0	1,497	1,497
Treated	6	3,187	3,193
Total	6	4,684	4,690

Bootstrapped Standard Errors for PSM ATT effects

Effect	Reps	Observed	Bias	S.E.	95% C.I.	
ATT mathematics	1000	5.8932	1.7207	2.9025	-2.2032	10.0096
ATT reading	1000	3.1128	0.1234	3.0641	-2.8464	8.9302
ATT science	1000	0.2634	1.7899	3.2128	-8.0172	5.0530

Figure 1b: Checks for common support and standard errors of PSM results, PISA 2009



Treatment assignment	off support	on support	total
Untreated	0	1,726	1,726
Treated	0	2,770	2,770
Total	0	4,496	4,496

Bootstrapped Standard Errors for PSM ATT effects

Effect	Reps	Observed	Bias	S.E.	95% C.I.	
ATT mathematics	1000	-1.1014	-0.7507	3.4266	-6.9357	6.2648
ATT reading	1000	-1.1740	-1.5542	3.3433	-6.2639	6.1796
ATT science	1000	-2.1506	-1.9901	3.8309	-7.4623	7.8854

Table 9a: Regression Adjusted Propensity Score Matched estimates, PISA 2006

VARIABLES	(1) Mathematics	(2) Reading	(3) Science
ATT attending private school	4.17	1.70	-1.16
	(3.138)	(3.828)	(3.049)
rural	4.06	21.70***	-0.40
	(11.568)	(2.885)	(9.563)
academic program	22.23***	8.08	16.24***
	(4.360)	(6.297)	(5.199)
program type	19.68***	23.01***	21.82***
	(0.787)	(1.380)	(0.878)
grade	19.57***	11.65***	12.64***
	(1.890)	(2.414)	(2.368)
age	-10.19***	-1.92	0.05
	(2.992)	(3.938)	(3.671)
female	-24.44***	11.22***	-20.24***
	(1.818)	(2.209)	(2.077)
highest parental education	-1.53	-2.34	-3.42***
	(1.102)	(1.550)	(1.285)
student's ESCS-index	1.92	8.96**	11.48***
	(2.601)	(3.745)	(3.334)
books at home	10.07***	4.89***	13.09***
	(1.597)	(1.717)	(1.554)
home educational resources	3.87***	3.23**	3.36**
	(1.080)	(1.543)	(1.469)
home possessions	-4.85***	-10.90***	-12.31***
	(1.759)	(2.645)	(2.275)
non-western immigrant	-24.72***	-15.49***	-32.83***
	(3.862)	(4.303)	(3.568)
non-western language at home	2.13	-20.85***	-11.14*
	(5.502)	(7.101)	(6.369)
constant	368.24***	266.43***	261.36***
	(42.248)	(58.884)	(50.957)
Observations	4,150	4,150	4,150
R-squared	0.76	0.69	0.72
Adj. R-squared	0.758	0.685	0.717

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9b: Regression Adjusted Propensity Score Matched estimates, PISA 2009

VARIABLES	(1) Mathematics	(2) Reading	(3) Science
ATT attending private school	0.96	-0.55	-2.07
	(2.855)	(3.808)	(3.990)
rural	-5.48	-12.53**	0.31
	(4.167)	(5.543)	(5.496)
competition	-5.93*	0.33	2.77
	(3.503)	(4.067)	(3.613)
parental pressure	-2.62	-3.91	-2.50
	(3.033)	(3.405)	(3.128)
academic program	21.87***	19.31***	7.68
	(5.593)	(5.918)	(6.487)
program type	25.75***	25.50***	30.96***
	(1.517)	(1.517)	(1.655)
grade	17.07***	8.08***	5.62*
	(3.865)	(2.145)	(3.066)
age	-17.69***	-8.67**	-12.20***
	(4.138)	(3.982)	(4.377)
female	-26.75***	16.07***	-12.51***
	(1.698)	(1.983)	(2.199)
highest parental education	-3.35***	-4.58***	-2.42*
	(1.073)	(1.208)	(1.378)
student's ESCS-index	3.11	6.21***	4.70*
	(2.109)	(2.382)	(2.657)
books at home	12.37***	14.50***	13.98***
	(1.335)	(1.514)	(1.726)
home educational resources	4.95***	4.96***	5.41***
	(1.178)	(1.436)	(1.390)
home possessions	-5.26***	-9.52***	-7.84***
	(1.763)	(2.327)	(2.424)
non-western immigrant	-23.59***	-19.07***	-32.90***
	(3.815)	(4.243)	(5.213)
non-western language at home	-8.51*	-7.14	-16.76***
	(4.664)	(4.845)	(6.064)
constant	719.73***	532.96***	581.47***
	(68.625)	(63.294)	(71.180)
Observations	3,774	3,774	3,774
R-squared	0.76	0.70	0.71
Adj. R-squared	0.761	0.694	0.708

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10a: Regression Adjusted PSM estimates, using IPTW weighting, PISA 2006

VARIABLES	(1) Mathematics	(2) Reading	(3) Science
ATT attending private school	5.77**	1.65	0.56
	(2.893)	(3.783)	(2.701)
rural	8.26	20.56***	2.12
	(8.822)	(2.711)	(8.220)
academic program	23.15***	7.60	19.73***
	(3.897)	(6.001)	(4.364)
program type	19.47***	22.89***	21.30***
	(0.644)	(1.342)	(0.736)
grade	21.20***	12.26***	13.99***
	(1.658)	(2.286)	(2.086)
age	-11.08***	-3.83	-1.06
	(2.286)	(3.272)	(3.051)
female	-23.98***	12.53***	-19.26***
	(1.453)	(1.667)	(1.651)
highest parental education	-1.34*	-2.71***	-2.61***
	(0.780)	(1.039)	(0.888)
student's ESCS-index	1.16	9.09***	8.93***
	(2.017)	(2.501)	(2.391)
books at home	10.85***	5.06***	13.18***
	(1.335)	(1.458)	(1.248)
home educational resources	2.86***	2.04*	1.56
	(1.005)	(1.153)	(1.168)
home possessions	-3.02**	-9.24***	-9.11***
	(1.391)	(1.876)	(1.554)
non-western immigrant	-19.73***	-15.68***	-28.46***
	(2.885)	(4.429)	(3.724)
non-western language at home	-7.01	-22.63***	-19.52***
	(4.508)	(7.339)	(6.322)
constant	363.80***	292.50***	261.89***
	(31.821)	(46.548)	(41.411)
Observations	4,528	4,528	4,528
R-squared	0.76	0.69	0.72
Adj. R-squared	0.760	0.686	0.714

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10b: Regression Adjusted PSM estimates, using IPTW weighting, PISA 2009

VARIABLES	(1) Mathematics	(2) Reading	(3) Science
ATT attending private school	0.73	-1.25	-3.37
	(2.851)	(3.699)	(3.687)
rural	-5.99	-12.51**	0.55
	(4.115)	(5.352)	(4.883)
competition	-6.92**	-0.81	1.92
	(3.421)	(4.088)	(3.719)
parental pressure	-1.69	-3.69	-2.31
	(3.102)	(3.340)	(3.068)
academic program	24.13***	20.53***	10.82*
	(5.230)	(5.948)	(5.947)
program type	24.97***	24.86***	30.21***
	(1.171)	(1.510)	(1.431)
grade	23.40***	12.93***	11.88***
	(2.536)	(2.100)	(2.233)
age	-24.32***	-10.45***	-15.36***
	(2.841)	(3.177)	(3.431)
female	-26.20***	15.52***	-12.85***
	(1.265)	(1.421)	(1.528)
highest parental education	-2.00**	-3.35***	-1.85*
	(0.984)	(1.148)	(1.105)
student's ESCS-index	3.02*	5.92***	5.43***
	(1.768)	(1.972)	(2.066)
books at home	12.33***	14.08***	12.71***
	(1.210)	(1.251)	(1.314)
home educational resources	4.79***	4.11***	4.82***
	(0.950)	(1.068)	(1.090)
home possessions	-6.18***	-10.91***	-8.95***
	(1.627)	(1.913)	(2.002)
non-western immigrant	-21.46***	-15.10***	-30.30***
	(3.737)	(4.052)	(4.976)
non-western language at home	-6.90*	-10.05**	-16.12***
	(3.927)	(4.461)	(5.011)
constant	824.34***	563.90***	638.09***
	(47.559)	(50.169)	(54.392)
Observations	4,319	4,319	4,319
R-squared	0.76	0.69	0.71
Adj. R-squared	0.760	0.689	0.707

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11a: Unobserved bias in PISA 2006 PSM-estimates to be compatible with Patrinos (2011)

	mathematics		reading		science	
Unobserved Bias	lower bound	upper bound	lower bound	upper bound	lower bound	upper bound
1	3.155	10.165	2.149	9.468	-3.404	4.383
1.1	-1.052	14.333	-2.246	13.796	-8.019	9.045
1.2	-4.868	18.149	-6.254	17.718	-12.262	13.288
1.3	**-8.413**	**21.616**	-9.988	21.337	-16.178	17.158
1.4	-11.723	24.848	-13.467	24.687	**-19.769**	**20.748**
1.5	-14.761	27.808	-16.692	27.774	-23.172	24.105
1.6	-17.604	30.612	-19.731	30.655	-26.296	27.228
1.7	-20.291	33.222	**-22.612**	**33.352**	-29.233	30.119
1.8	-22.823	35.675	-25.329	35.894	-31.984	32.870
1.9	-25.238	38.012	-27.897	38.290	-34.595	35.481
2	-27.497	40.193	-30.348	40.562	-37.066	37.952

Note: Alpha is 0.95 for lower- and upper bounds

Table 11b: Unobserved bias in PISA 2009 PSM-estimates to be compatible with Patrinos (2011)

	mathematics		reading		science	
Unobserved Bias	lower bound	upper bound	lower bound	upper bound	lower bound	upper bound
1	-4.94601	3.89403	-4.88498	4.13301	-7.04201	2.564
1.1	-9.85799	8.80199	-9.918	9.14697	-12.403	7.92599
1.2	-14.334	13.284	-14.488	13.693	-17.344	12.822
1.3	-18.462	17.407	-18.711	17.83	-21.819	17.298
1.4	**-22.316**	**21.189**	-22.621	21.677	**-25.969**	**21.446**
1.5	-25.899	24.732	-26.246	25.256	-29.839	25.271
1.6	-29.21	28.041	-29.625	28.615	-33.431	28.906
1.7	-32.326	31.118	**-32.785**	**31.737**	-36.833	32.267
1.8	-35.247	33.999	-35.771	34.674	-40.004	35.434
1.9	-38.05	36.728	-38.599	37.435	-42.988	38.419
2	-40.696	39.299	-41.245	40.031	-45.831	41.219

Note: Alpha is 0.95 for lower- and upper bounds

Table 12a: Multi-level model estimates for private school attendance, PISA 2006

VARIABLES	(1) mathematics	(4) reading	(7) science
private	7.34**	2.86	2.31
	(3.124)	(4.262)	(2.762)
rural	18.42*	25.44*	11.85
	(10.044)	(13.722)	(8.844)
SES school	13.14***	26.33***	17.81***
	(4.723)	(6.396)	(4.281)
vocational school	-23.94***	-10.43*	-20.88***
	(4.457)	(6.001)	(4.101)
non-western immigrant	-17.98***	-13.50***	-27.70***
	(2.001)	(2.340)	(2.444)
female	-23.35***	12.94***	-19.05***
	(1.112)	(1.295)	(1.374)
age	-10.35***	-2.85	0.93
	(2.187)	(2.549)	(2.705)
grade	20.66***	14.95***	13.50***
	(1.262)	(1.475)	(1.545)
program type	18.09***	19.22***	19.32***
	(0.466)	(0.551)	(0.552)
books at home	11.19***	3.37***	12.23***
	(0.957)	(1.116)	(1.182)
student's ESCS-index	-2.07***	-0.14	-0.73
	(0.779)	(0.906)	(0.965)
constant	377.64***	265.88***	251.08***
	(31.006)	(36.244)	(38.192)
Observations	4,662	4,662	4,662
Number of groups	184	184	184

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12b: Multi-level model estimates for private school attendance, PISA 2009

VARIABLES	(1) mathematics	(4) reading	(7) science
private	-0.07	-1.95	-1.24
	(3.383)	(3.740)	(3.932)
rural	-4.46	-10.71**	-0.83
	(4.390)	(4.854)	(5.103)
SES school	10.41**	9.29*	20.36***
	(4.874)	(5.400)	(5.677)
vocational school	-18.99***	-20.66***	-13.20**
	(4.649)	(5.164)	(5.430)
competition	-7.47**	-1.04	2.63
	(2.975)	(3.289)	(3.458)
non-western immigrant	-18.38***	-13.29***	-26.94***
	(2.001)	(2.276)	(2.396)
female	-25.39***	15.66***	-13.15***
	(1.090)	(1.241)	(1.306)
age	-16.53***	-3.36	-6.63***
	(2.102)	(2.392)	(2.518)
grade	18.46***	10.51***	10.13***
	(1.138)	(1.295)	(1.363)
program type	25.41***	24.10***	27.03***
	(0.664)	(0.755)	(0.794)
books at home	11.40***	11.48***	11.11***
	(0.933)	(1.062)	(1.118)
student's ESCS-index	-0.47	-1.09	-0.03
	(0.806)	(0.917)	(0.966)
constant	704.13***	454.22***	506.79***
	(34.102)	(38.775)	(40.814)
Observations	4,529	4,529	4,529
Number of groups	182	182	182

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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