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April 2016

EGC Report No: 2016/05

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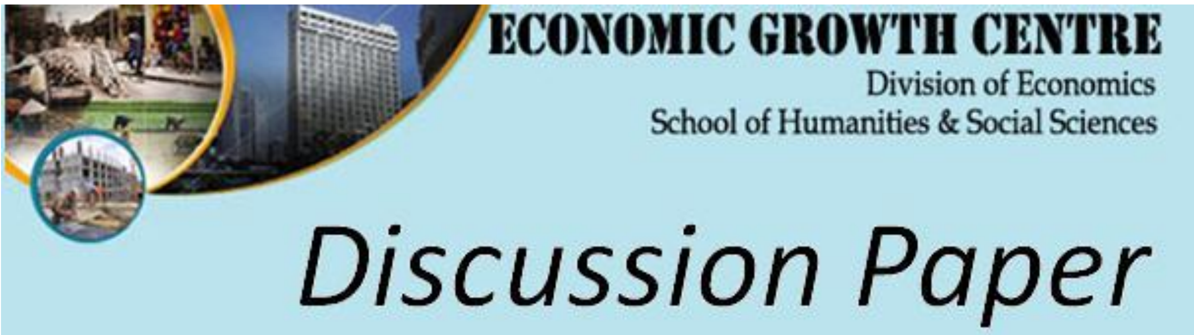

The "true" private school effect across countries using PISA-2012 Mathematics

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Abstract: It is known that in most countries, students of private schools perform better in international assessments compared to students in the public school system. However, when one controls for observable socioeconomic background characteristics at the individual and school level, public school students perform equally well. Furthermore, sorting to private vs. public schools based on unobservable characteristics takes place, which biases econometric estimates. I account for selection on unobservables using an approach based on the idea that the amount of selection on the observed explanatory variables in a model provides a guide to the amount of selection on the unobservables (Altonjie et al. 2005; Oster 2013). I use PISA-2012 data for Mathematics to derive bias-corrected estimates of the "true" private-dependent and independent school effect for 40 countries. With few exceptions, public schools outperform private schools (especially independent schools). Accounting for both peer effects and selection is necessary when evaluating school effectiveness.

JEL Classification Codes: C52, I24, L33
Keywords: School choice, private school advantage, selection.

1. Introduction

Parents decide whether to enroll their children to private schools or the public school system, by assessing the benefits and costs associated with this decision and whether it is worth the cost of private schooling. Perceived benefits relate to more autonomy and flexibility in deciding curricula, more resources, better peer groups, safer school environment, among others. Private schools tend to attract not only students from more privileged backgrounds, but also better performing students (OECD, 2011). Differences in socioeconomic backgrounds between private and public schools can be very large (examples are several countries in Latin America, such as Uruguay, Brazil and Peru as well as in Middle Eastern countries).

There is definite evidence that, based on a simple comparison without accounting for student background, private school students perform better in the majority of countries and the raw PISA score difference ranges from only a few points to more than 100 points (for example Qatar and Brazil); in a minority of countries public schools outperform private schools (for example Taiwan and Thailand). After accounting for student socioeconomic background and other observables, the evidence is mixed. In fact PISA finds that when public schools attract similar student population as private schools and given similar levels of autonomy, the private school advantage is no longer evident (PISA in focus, 2011).

Assessing the true effect of private school attendance hinges on addressing both the effect of observable covariates associated with student and family background characteristics, school demographics as well as selection effects There is sorting of students into private and public schools; for example, students from better socioeconomic backgrounds enrolling in private schools. This results in endogeneities, as more privileged/higher ability private school students achieve better results, which in turn leads to an overestimation of the private school effect (French and Kingdon, 2010; Mcloughlin, 2013).

When estimating the true effect of private school attendance one should be cognizant that the estimation model is likely vulnerable to omitted variable bias, as parental background is expected to be correlated with various unobserved student and school characteristics (especially those which relate to school quality), which affect student performance (for example, students' ability). One would then need a suitable measure or proxy for ability, which may prove elusive; such problems magnify if the objective is to estimate such effects across countries rather than for a single country, since then consistency and comparability issues are important.

In this paper I draw on recent methodological developments on causal inference under unobserved confounds and bias-adjusted treatment effects. During the last decade or so, new estimation methods have been proposed which use the proportional selection assumption (Altonji, Elder and Taber, 2005; Oster, 2013; 2015). I use the proposed approach by Oster (2013; 2015) which connects more explicitly the intuitive methodologies used in empirical work of evaluating bias. This approach makes explicit the link between coefficient movements, R-squared movements and omitted variable bias, following the estimation of a fully controlled regression.

I use the PISA-2012 mathematics assessment for 15-year old students to evaluate the effect of unobservable selection on coefficient estimates, because it is a better proxy for school effectiveness. Section 2 provides a non-exhaustive summary of the literature. Section 3 describes the methodology and data used, while section 4 presents and discusses the estimation results.

2. Literature review

A review of the literature on the effectiveness of private schools reveals a general agreement that in OECD and partner countries most of the 30-point difference in PISA scores in favor of private school students can be attributed to the ability of private schools to attract students from better socioeconomic backgrounds, better performing students and possessing better material resources. There is no evidence that the private school sector contributes to raising the performance of the school system as a whole (PISA in focus, 2011). However, studies using controlled regressions often find a private school advantage, or at least advantages associated with sub-types of private schools.

Donkers and Robert (2003; 2008) used earlier data (PISA 2000) to compare the effectiveness of private-dependent, private-independent and public schools for 15-year olds in 19 OECD countries after controlling in a step-wise manner for sociological, demographic, behavioral and attitudinal characteristics of parents and students, as well as teaching and learning conditions and school climate. They also used multi-level analysis, such as nested multilevel models, but their analysis does not account for unobserved selection. They find that private government-dependent schools are more effective than comparable public schools and attribute this to better school climate in private schools; however, private-independent schools were found to be less effective than comparable public schools.

The challenges of uncovering the true effect of private education across countries using different available econometric techniques are also evident in the literature. Vandenberghe and Robin (2004) attempted to estimate the effect of private vs. public education on pupils' achievement, using the 2000 OECD PISA survey, taking into account the potential bias due to the existence of unobserved confounding factors. They used three methods and compared the estimates: IV regression, Heckman's 2-stage approach and Propensity Score Matching. Overall, they find that private education does not generate systematic benefits. However, they found significant differences in results between parametric and non-parametric estimators. Using different methods is expected to result in different estimates, as each method has its own weakness. For example, OLS, IV and Heckman methods impose linearity in the outcome equation, while matching approaches do not account for unobservable selection. The authors also find that the main obstacle in deriving reliable estimates using the IV and Heckman approaches is the difficulty in finding valid instruments. This problem is overcome when using Propensity Score Matching; however, this method is undermined (especially when using cross-section data) by its inability of dealing with unobserved selection.

The literature on developing countries also stresses the presence of heterogeneity in findings and the difficulties associated with ascertaining whether any private school advantage found can be fully ascribed to private schools. This is because not all empirical studies deal convincingly with unobserved/unmeasurable differences in the socio-economic backgrounds of private and public school pupils; hence, the private school advantage is likely to be over-estimated. Day-Ashley et al. (2014) reviewed 21 studies (mostly of medium quality and only three high quality studies) on six developing countries, the majority of them for India. They find only moderate evidence supporting the assumption that private school pupils achieve better learning outcomes than pupils in the public school system.

Similarly, Mcloughlin (2013), after reviewing the literature in developing countries concludes that, while evidence broadly supports the view that students attending private schools are achieving better results even after accounting for social background, the extent of the true private sector advantage, when found, is often marginal. Ultimately, claims of a "private sector effect" can be made only after rigorously using both covariates to separate the effect of schools from that of student and parental background and accounting for selection effects, i.e., the endogenous advantage arising from sorting of children from better-off, better informed families into private schools.

Studies which employ empirical strategies which use selection on observables to assess the bias arising from selection on unobservables to account for selection bias are increasingly coming to light. This emerging evidence points to private schools performing no better than public schools (or even worse). Cardak and Vecci (2013) estimated the effect of Catholic school attendance on high school completion and university commencement in Australia after accounting for selection on unobservables using the assumption of equality between selection on observables and unobservables. They find that this effect is smaller than previously estimated and negative treatment effects cannot be ruled out. Elder and Jepsen (2014) find that Catholic schools do not appreciably boost test scores and that findings point to sizeable negative effects of Catholic schooling on mathematics achievement. Earlier, Altonji, Elder and Taber (2005) estimated the effect of attending a Catholic high school on a variety of outcomes in the US. They conclude that Catholic high schools substantially increase the probability of graduating from high school and possibly attending college; however, they find little evidence of an effect on test scores.

In this paper I use an identical methodological framework to derive bias-adjusted estimates after accounting for selection on unobservables in 40 countries (from Europe, North America, Oceania, Middle East, South America and East Asia), in which the size of the private sector is non-trivial. The evidence generalizes certain country-specific findings (presented in the preceding paragraph) and suggests that positive private sector effects are exceptions rather than the rule.

3. Data and Methodology

3.1 Data

PISA-2012[1], which follows on the steps of previous PISA surveys (2000/2002, 2003, 2006 and 2009/2010), intends to measure what 15-year old students in grade 7 or higher (that is students approaching the end of their compulsory schooling) can do with what they learn at school. It emphasizes the mastery of processes, the understanding of concepts, and the ability to function in various types of situations. Over the years country participation in PISA has increased and

[1] The development of PISA 2012-Mathematics was coordinated by an international consortium of educational research institutions contracted by the OECD, under the guidance of a group of mathematics experts from participating countries.

the 2012 survey was conducted in 34 OECD countries and 31 partner countries/economies. The 2012 survey focused mainly on mathematics with more than half a million students competing in the participating countries, representing a population of 28 million 15-year olds (OECD, 2014).

The PISA-2012 surveys contain information and simple indices on student, family background, school and teacher characteristics, as well as information on occupational status and educational level of parents, immigration and language background, measures of engagement with and at school (such as skipping classes, index of sense of belonging and index of attitude towards learning activities and outcomes), student drive and motivation (such as index of perseverance, index to openness in problem solving and index of intrinsic and instrumental motivation in mathematics), mathematics self-beliefs (such as index of mathematics self-efficacy, index of mathematics self-concept and index of mathematics anxiety) and various indices of disposition towards mathematics, disciplinary climate at school and teacher-student relations.

School level simple indices contain information on school and class size, student-teacher ratio, school type, availability of computers, quality of teaching staff, ability grouping, use of assessments and school responsibility for curriculum and assessment. School level scale indices contain information on teacher shortages, quality of school resources and infrastructure, teacher and student behavior and teacher morale.

The survey also contains the constructed index of Economic, Social and Cultural Status (ESCS), which is derived from three other indices: (a) highest occupational status of parent, (b) highest educational level of parents in years of education and (c) home possessions. The index of home possessions incorporates all items of the indices on household wealth (weather students lived in a household which had certain possessions, such as a room of their own for the student, internet connection, number of cellular phones, number of TVs, etc.); household cultural possessions (such as books on literature, poetry, etc.); household educational resources (such as a desk, computer, etc.); and books at home. The ESCS index was derived using principal component analysis of standardized variables (OECD mean of zero and standard deviation of one), taking the factor scores of the first principal component as the measure of the ESCS index.

Schools are classified as either public or private, according to whether the ultimate decision maker is a private entity or a public one. This is combined with information on the percentage of total funding which comes from government sources. The generated index of school type allows for the identification of three categories: (1) government-independent private schools controlled by a non-government organization or with a governing board not selected by a government agency that receive less than 50% of their core funding from government agencies, (2) government-dependent private schools controlled by a non-government organization or with a governing board not selected by a government agency that receive more than 50% of their core funding from government agencies, and (3) public schools controlled and managed by a public education authority or agency.

The proportion of missing values for important variables such as school type and index of social cultural and economic status is small (generally under 3%). Missing values were imputed using the method of relating observations in the original data to a set of fundamental variables with no or few missing values.

Table 1A in the Appendix contains information on size of public school sector, proportion of government-dependent vs. private-independent private schools and proportion of schools with a positive peer group index by school type (index of Economic, Social and Cultural Status at the school level) for the 40 countries in this study in which private schools account for at least 3%, but not exceeding 90% of all schools[2]. Over all countries, private

[2] Countries with a very small public school sector and hence excluded, are Macao and Hong Kong.

schools account for nearly a quarter of all schools[3], but there is heterogeneity both between and within groups of countries. In European countries private schools are predominantly government-dependent (exceptions are Greece and Switzerland); the opposite is the case in other areas of the world, especially in the Middle East where almost all private schools are independent. Across all countries, about half of all private schools are government-dependent.

As expected, private schools, especially independent schools have better peer groups compared to public schools. Across all countries, about two-thirds of private-dependent schools and three-quarters of independent schools have positive peer groups, compared to only one-third of public schools. Differences are particularly severe in Latin American countries, where only 2% of public schools have a positive peer group index, compared to 42% and 62% for private-dependent and independent schools, respectively. The opposite is the case in East Asian countries, in which peer groups in independent and public schools are much more similar.

3.2 Methodology

Isolating causal effects is difficult in most contexts and particularly so when seeking the true effect of attending private rather than public schools. The often cited private school advantage may be due to unobserved student, family and other characteristics being correlated with private school attendance; such characteristics are expected to influence student performance, while at the same time influencing decisions on which school type students will be enrolled in.

Altonji et al. (2002; 2005) considered the problem of estimating causal relationships under selection on unobservables when good instrumental variables are not available and proposed a new estimation method based on the idea that the amount of selection on the observed explanatory variables in a model provides a guide to the amount of selection on the unobservables. This informal approach was then used to determine the amount of selection on

[3] This is higher than the 18% of private schools in PISA-2012, because countries with only a small proportion of private schools are excluded from the study.

unobservables that would be required, so that one could attribute the entire estimated effect (in their application, the effect of attending Catholic schools) to selection bias.

Following Altonji et. at. (2005) and related work, Oster (2015) built on the previous intuitive methodologies and connected the theory to empirical work. The additional insight is to recognize that coefficient stability is not informative on its own, but it needs to be combined with information on R-squared movements; furthermore, in the presence of multiple controls, using the derived general estimator is important, as it is possible to observe stable coefficients and large R-squared movements even when large biases exist. This methodology allows for more flexibility in examining the robustness of estimated effects under selection, the estimation of selection bias and bias-adjusted effects.

In the simple case of a treatment X and single observable, consider the linear equation:

$$Y = \beta X + \gamma_1 w_1^0 + W_2 + \epsilon \qquad (1)$$

where X is the treatment of interest with coefficient β, index $W_1 = \gamma_1 w_1^0$ is the observed control with coefficient γ_1, W_2 is a vector (linear combination) of unobserved controls multiplied by their true coefficients and the error ϵ is orthogonal to X, W_1 and W_2. Under equal selection (selection on unobservables as important as selection on observables), consider the regression of Y on X with a sample coefficient $\beta°$ and R-squared $R°$, and the regression of Y on X and w_1^0 with coefficient $\tilde{\beta}$ and R-squared, $\tilde{R}$. Furthermore, define R_{max} as the R-squared that would have been obtained had we been able to regress Y on X, w_1^0 and W_2. Then, using standard omitted variable bias formulas, it can be shown that the bias, Π:

$$\Pi = [\beta° - \tilde{\beta}] \frac{\text{Rmax} - \tilde{R}}{\tilde{R} - R°}, \qquad (2)$$

and the bias-corrected estimate of β is:

$$\beta^* = \tilde{\beta} - [\beta° - \tilde{\beta}] \frac{\text{Rmax} - \tilde{R}}{\tilde{R} - R°} \qquad (3)$$

Given that $\Pi = \tilde{\beta} - \beta^*$, it follows that: $\frac{\tilde{\beta} - \beta*}{\beta^\circ - \tilde{\beta}} = \frac{Rmax - \tilde{R}}{\tilde{R} - R^\circ}$. So, under the equal selection assumption, the ratio of the change in coefficients is equal to the ratio of the change in R-squared.

Oster (2013; 2015), then derives a general estimator under proportional selection (selection on unobservables is proportional to selection on observables) with a coefficient of proportionality δ and multiple observables. Under proportional selection, this estimator can produce multiple solutions for β, but the solution is unique under equal selection (δ = 1) under the added assumption that the bias from unobservables is not so large that it biases the direction of the covariance between the observables (index) and the treatment variable. When δ ≠ 1, if there are multiple solutions, the natural choice would be to select the treatment effect which is closer to the controlled coefficient, if it is assumed that the bias is not large.

As discussed in Altonjie et. al. (2002) and Oster (2015), the value δ = 1 is a suitable bounding value. In applications, one can either calculate the value of δ so that β = 0 (degree of selection necessary to explain away the estimate), or calculate the bias-adjusted β while assuming that δ = 1. However, the approach is application specific. In our case, the objective is to select bounding assumptions on R_{max} and δ to generate bounds for the treatment effect; the particulars of the application along with intuition can help in selecting an R_{max} between $\tilde{R}$ and 1 and δ values which are bounded below at 0 and above at some arbitrary δ.

4. Results and discussion

4.1 Raw score differences

Tables 1a and 1b report results separately for the private-dependent vs. public (Table 1a) and the private-independent vs. public school differentials (Table 1b). A country enters into the table when the size of the private school subsector is not trivial (at least three schools in the subsector). In total, 32 countries are reviewed in Table 1a and 31 countries in table 1b (40 countries in total). Countries with small private-independent school subsector (and a very small number of schools and students in the PISA data) which were included are: Belgium (3 schools/100 students), the Czech Republic (3 schools/54 students), Ireland (4 schools/118 students) and Denmark (8 schools/167 students); countries with small private-dependent school subsector included in the analysis are Switzerland (6 schools/130 students) and Taiwan (9 schools/329 students).

The mean raw score difference between both types of private schools and public schools in all regions except East Asia is, with very few exceptions, heavily in favor of private schools; in East Asia the opposite is the case, especially when private-dependent schools are concerned. Across all countries, the mean gap is much larger when private-independent schools are compared to public schools (about 0.6 of a standard deviation), compared to the corresponding gap when private-independent schools are compared to public schools (0.28 of a standard deviation). The mean gaps are much larger in the Latin America and Middle East. In East Asia the gap is in favor of public schools when compared to private-dependent schools, while the evidence is mixed for private-independent schools when compared to public schools.

4.2 Model specification and controlled effects

The methodology applied requires, first estimating a fully controlled regression using the available relevant observables in the data. The dependent variable is the standardized mathematics score, to facilitate comparison between countries. As in many other studies using PISA data I use the average of the five mathematics plausible values in the data[4,5].

The list of controls includes the following student and family characteristics: Age, gender, preschool attendance, being first generation immigrant, being second generation

[4] In the PISA test, individual students only answer a minority of questions, i.e., we do not observe the true score, rather a set of observed test scores. Given values of the model parameters, a score can be imputed to individuals. Given model uncertainty, this is done multiple times generating multiple 'plausible values'. Since the statistical model used to generate the plausible values is likely inadequate, the uncertainty in the national scores is underestimated.

[5] For comparison I also used the specialized Stata program *pv* (authored by Kevin Macdonald), for application in the education assessment literature, which estimates statistics when there are multiple plausible values. There were no substantial differences in estimates that would affect the conclusions on the nature of the private-public school differentials. Furthermore, derivation of bias-adjusted estimates requires a first-stage OLS based estimation of controlled estimates.

immigrant, community size, family structure, Social, Cultural and Economic Status index (with components: highest occupational status of parents; highest educational level of parents in years of education according to ISCED; and home possessions[6]), out of school study time, parental and student attitudes (index of parental pressure and index of perseverance). It also includes the following school and institutional characteristics: Private school sub-type (private-dependent or independent) and their interaction with proportion of positive Social, Cultural and Economic status index values at the school level, number of schools in the community, attending same sex schools (all-boy, all-girl school), class size, school size, proportion of certified teachers, dummies for ability grouping at school, school autonomy and teacher shortages, as well as indices on student climate and school educational resources. Finally the regression also controls for peer group quality at school (derived from the Social, Cultural and Economic Status index), while estimates are also derived excluding peer group quality controls, to evaluate their contribution to the private school effect.

The sample size varies by country and for the majority of countries is around 5,000 observations. Larger samples are available for the UK, Finland, Belgium, Switzerland, Colombia, UAE, Qatar and Jordan, ranging from 7,000 to 12,000 observations, while for Italy, Spain, Canada, Mexico and Brazil they are even larger ranging from 14,000 to more than 33,000 observations. R-squared values from the controlled regression also varied substantially, ranging from just over 0.2 (Finland and Canada), to more than 0.5 (France, Hungary, Netherlands and Qatar).

[6] The index of Home Possessions comprises all items on the indices of wealth, cultural possessions (books on poetry and works of art at home) and home educational resources, as well as books in the home recoded into a four-level categorical variable.

Table 1a: Estimated effects on the standardized Mathematics PISA score – Private-dependent vs. Public schools

Country	**(1) Raw score diff: Private - Public**	**(2) Controlled effect: No interaction**	**(3) Bias-adj. effect: No interaction**		**(4) Controlled effect: With interaction[a]**	**(5) Bias-adj. Effect: With interaction[a]**	
Europe/North America/Oceania			**δ = 0.5**	**δ = 1**		**δ = 0.5**	**δ = 1**
Austria*	0.359 (0.045)	-0.153 (0.154)	-0.276	-0.414	-0.126	-0.252	-0.390
Belgium	0.575 (0.023)	0.233 (0.052)	0.156	0.077	0.218	-0.006	-0.270
Czech Republic	0.030 (0.059)	-0.141 (0.106)	-0.184	-0.227	-0.050	-0.072	-0.099
Denmark	0.330 (0.033)	0.040 (0.069)	-0.031	-0.106	-	-	-
Finland	0.108 (0.046)	0.038 (0.074)	0.022	0.010	0.065	0.048	0.033
France*	0.317 (0.037)	-0.079 (0.093)	-0.178	-0.283	-0.241	-0.398	-0.571
Germany	0.439 (0.064)	-0.282 (0.142)	-0.450	-0.637	-	-	-
Hungary	0.210 (0.040)	-0.047 (0.102)	-0.109	-0.172	-0.056	-0.133	-0.200
Ireland	0.243 (0.045)	0.221 (0.074)	0.248	0.274	0.242	0.245	0.248
Italy	-0.301 (0.033)	-0.093 (0.124)	-0.040	0.010	0.006	0.078	0.155
Netherlands	-0.104 (0.031)	0.124 (0.087)	0.175	0.231	0.129	0.123	0.112
Poland	0.575 (0.067)	0.069 (0.192)	-0.046	-0.174	-0.193	-0.383	-0.592
Portugal	0.484 (0.056)	-0.194 (0.108)	-0.342	-0.509	-0.207	-0.365	-0.545
Slovakia	0.537 (0.055)	0.048 (0.114)	-0.081	-0.208	-0.173	-0.357	-0.566
Spain	0.353 (0.013)	0.042 (0.054)	-0.034	-0.116	0.059	-0.045	-0.157
Sweden	0.168 (0.039)	0.087 (0.063)	0.068	0.049	-0.077	-0.143	-0.213
Switzerland	0.586 (0.088)	-0.034 (0.267)	-0.183	-0.350	-0.704	-	-
United Kingdom	0.254 (0.025)	0.025 (0.063)	-0.021	-0.068	0.015	-0.047	-0.110
Canada	0.570 (0.035)	0.340 (0.091)	0.287	0.232	0.438	0.400	0.356
Australia	0.830 (0.026)	-0.045 (0.076)	-0.256	-0.509	-	-	-
Mean	*0.328*	*0.010*	*-0.062*	*-0.143*	*-0.039*	*-0.082*	*-0.176*
Middle East							
UAE	0.641 (0.026)	-0.118 (0.083)	-0.212	-0.311	-	-	-
South America							
Argentina	0.846 (0.027)	0.338 (0.085)	0.212	0.071	0.355	0.186	-0.006
Brazil	1.25 (0.034)	0.248 (0.141)	0.022	-0.245	0.196	-0.020	-0.282
Chile	0.423 (0.024)	0.002 (0.089)	0.072	0.144	0.137	0.191	0.247
Colombia	0.220 (0.034)	-0.116 (0.129)	-0.165	-0.215	-0.088	-0.157	-0.230
Costa Rica	0.927 (0.059)	-0.591 (0.314)	-0.921	-1.32	-0.376	-0.667	-1.01
Mexico	0.368 (0.034)	-0.273 (0.135)	-0.419	-0.585	-0.413	-0.587	-0.783
Peru	-0.055 (0.007)	0.093 (0.139)	-0.038	-0.182	0.058	-0.088	-0.249
Mean	*0.568*	*-0.043*	*-0.177*	*-0.333*	*-0.019*	*-0.163*	*-0.330*
East Asia							
Indonesia	-0.538 (0.035)	0.011 (0.178)	0.171	0.357	-	-	-
Korea	-0.087 (0.031)	-0.007 (0.073)	0.050	0.110	0.018	0.064	0.111
Taiwan	-0.995 (0.055)	-0.601 (0.097)	-0.542	-0.480	-	-	-
Thailand	-0.588 (0.043)	-0.398 (0.076)	-0.355	-0.312	-0.563	-0.544	-0.523
Mean	*-0.552*	*-0.249*	*-0.169*	*-0.081*	*-0.272*	*-0.240*	*-0.206*
Overall mean	***0.280***	***-0.038***	***-0.105***	***-0.182***	***-0.051***	***-0.119***	***-0.221***

Note: Standard deviations/clustered standard errors in parentheses.

* For Austria and France there is no information to separate private from dependent and independent.

[a] The regressions in column (4) include an interaction term with the "positive socioeconomic school index" dummy and the estimated effect is evaluated at the mean.

Table 1b: Estimated effects on the standardized Mathematics PISA score – Private-Independent vs. Public schools

Country	**(2) Raw score diff: Private - Public**	**(2) Controlled effect: No interaction**	**(3) Bias-adj. effect: No interaction**		**(4) Controlled effect: With interaction[a]**	**(5) Bias-adj. Effect: With interaction[a]**	
Europe/North America/Oceania			**δ = 0.5**	**δ = 1**		**δ = 0.5**	**δ = 1**
Belgium	1.61 (0.097)	0.593 (0.089)	0.509	0.351	-	-	-
Czech Republic	0.448 (0.137)	0.480 (0.135)	0.488	0.497	-	-	-
Denmark	0.415 (0.078)	0.112 (0.098)	-0.048	-0.018	-	-	-
Greece	0.738 (0.062)	0.223 (0.222)	-0.103	-0.031	-	-	-
Ireland	0.796 (0.096)	0.268 (0.174)	0.196	0.119	-	-	-
Italy	0.155 (0.046)	-0.148 (0.120)	-0.223	-0.303	-	-	-
Poland	0.839 (0.082)	-0.036 (0.177)	-0.239	-0.478	-	-	-
Portugal	1.17 (0.069)	-0.049 (0.110)	-0.349	-0.711	-	-	-
Spain	0.470 (0.028)	0.042 (0.078)	-0.037	-0.122	-0.005	-0.120	-0.247
Switzerland	-0.290 (0.055)	-0.709 (0.146)	-0.809	-0.916	-	-	-
United Kingdom	0.895 (0.045)	0.266 (0.133)	0.116	-0.056	-	-	-
Canada	0.736 (0.037)	0.247 (0.120)	0.128	-0.008	-	-	-
USA	-0.001 (0.056)	-0.490 (0.105)	-0.623	-0.75	-0.480	-0.581	-0.687
Australia	0.326 (0.018)	-0.115 (0.052)	-0.191	-0.273	-0.113	-0.190	-0.275
New Zealand	0.927 (0.219)	0.332 (0.112)	0.181	0.009	-	-	-
Mean	0.616	0.064	*-0.067*	*-0.179*	*-0.199*	*-0.297*	*-0.403*
Middle East							
Jordan	0.885 (0.037)	0.440 (0.110)	0.294	0.136	0.250	0.051	-0.184
Qatar	1.11 (0.017)	0.323 (0.100)	-0.280	-1.01	-0.128	-0.965	-2.16
UAE	0.760 (0.019)	-0.163 (0.072)	-0.375	-0.624	-0.200	-0.494	-0.841
Mean	*0.918*	*0.165*	*-0.120*	*-0.499*	*-0.026*	*-0.469*	*-1.06*
South America							
Argentina	1.03 (0.042)	0.294 (0.166)	0.167	0.026	0.159	-0.042	-0.276
Brazil	1.28 (0.021)	0.303 (0.134)	-0.035	-0.474	0.094	-0.289	-0.784
Chile	1.39 (0.026)	0.174 (0.141)	-0.385	-1.22	0.232	-0.530	-1.66
Colombia	1.06 (0.031)	0.181 (0.158)	-0.094	-0.437	0.167	-0.131	-0.501
Costa Rica	1.34 (0.048)	-0.251 (0.349)	-0.723	-1.33	-0.547	-1.13	-1.88
Mexico	0.528 (0.018)	-0.180 (0.090)	-0.365	-0.583	-0.369	-0.604	-0.881
Peru	0.986 (0.033)	0.096 (0.132)	-0.162	-0.474	0.006	-0.306	-0.691
Uruguay	1.16 (0.033)	0.145 (0.232)	-0.246	-0.762	-0.087	-0.616	-1.32
Mean	*1.10*	*0.095*	*-0.230*	*-0.657*	*-0.043*	*-0.456*	*-0.999*
East Asia							
Indonesia	0.167 (0.032)	0.364 (0.184)	0.383	0.403	-	-	-
Japan	0.094 (0.028)	-0.613 (0.076)	-0.806	-0.805	-0.546	-0.71	-0.897
Korea	0.641 (0.039)	0.152 (0.088)	0.005	-0.163	0.132	-0.080	-0.328
Taiwan	-0.458 (0.026)	-0.623 (0.083)	-0.689	-0.756	-0.691	-0.786	-0.907
Thailand	-0.598 (0.064)	-0.502 (0.143)	-0.492	-0.481	-0.563	-0.544	-0.523
Vietnam	-0.356 (0.053)	-0.750 (0.166)	-0.842	-0.941	-	-	-
Mean	*-0.085*	*-0.332*	*-0.407*	*-0.457*	*-0.417*	*-0.530*	*-0.664*
Overall mean	***0.633***	***0.007***	***-0.177***	***-0.381***	***-0.149***	***-0.448***	***-0.836***

Note: Standard deviations/ clustered standard errors in parentheses.

[a] The regressions in column (4) include an interaction term with the "positive socioeconomic school index" dummy and the estimated effect is evaluated at the mean.

In Table 1a (private-dependent vs. public schools), after controlling for observables[7] using the specification without the interaction of private-dependent with the positive peer group index (column 2), only in 5 out of 28 countries in Europe, North America, Oceania,

[7] To derive appropriate standard errors in the controlled regression in the presence of clustering of students within schools and other units, OLS standard errors were adjusted for clustering; otherwise standard errors would be underestimated.

Middle East and Latin America (namely Belgium, Ireland, Canada, Argentina and Brazil) a positive and significant effect associated with attending a private-dependent school remains. In four East Asian countries, the effect in favor of public schools remains, but is substantially reduced in Taiwan and Thailand and disappears in Indonesia and Korea. Over all countries, a negative but very small effect is found. In the specification which includes the interaction term (column 4) the estimates generally mirror those in column 2, with Chile entering the group of countries with a significantly positive estimated effect associated with attending private-dependent schools.

In Table 1b (independent vs. public schools), the controlled effect in column 2 is positive and significant in several countries (10 out of 31), but the 31-country average is very close to zero. Including the interaction term generally reduces the estimated effect of attending independent schools. This is expected since independent schools have much more favorable peer groups compared to both public and private-dependent schools.

4.3 The importance of peer groups

The contribution of school peer group effects was evaluated by comparing regressions without and with peer group controls. This contribution varies both between private school types (dependent vs. independent) and between country groups. Table A2 in the Appendix summarizes the controlled effects (column 2 in Tables 1a and 1b), without and with peer group controls by country grouping.

Accounting for peer effects are particularly important[8] in deriving the independent school effect, as without such peer group controls the average effect would have been about 0.18 of a standard deviation in favor of independent schools, compared to about zero in Table 1b. They are much more important in Latin American countries (0.42 of a standard deviation in favor of independent schools compared to 0.095). On the other hand, in the group of East Asian

[8] Accounting for peer effects can also reduce the extent of selection on unobservables.

countries peer effects contribute very little in explaining the private-independent school effect. These findings are consistent with Table 1A in the Appendix, which reported stark differences in quality of peer groups between independent schools and public schools, especially in Latin American countries.

When comparing private-dependent schools to public schools, peer effects are much less important (cross-country average of 0.059 compared to -0.038 of a standard deviation without and with peer group controls), as dependent schools are not much different from public schools in peer group composition; however, in the Latin American group of countries such effects are still important (0.17 compared to -0.043 of a standard deviation without and with peer group controls).

4.4 Bias-corrected estimates of effects

Robustness checking in the presence of selection is conducted using assumptions on the value of R_{max} and δ, in order to obtain bounding values of β. Such bounding values can then be compared to the controlled estimates given in columns 2 and 4 of Tables 1a and 1b (i.e., assuming $\delta = 0$, $R = \tilde{R}$, $\beta = \tilde{\beta}$). R_{max} is bounded between $\tilde{R}$ and 1. As argued in Oster (2015), it is reasonable to assume an $R_{max} < 1$, considering the possibility of measurement error in the dependent variable, Y, or variation in Y which is not related to X, i.e., variation arising from choices made after X is determined.

I use the value of $R_{max} = 1.5\tilde{R}$,[9] without explicitly considering higher values, since any higher value would simply reinforce the conclusions of this study. With respect to the extent of selection on unobservables in relation to observables, I use two alternative values: $\delta = 0.5$ (unobservables are half as important as observables) and equal selection ($\delta = 1$). Between the two, the first which assumes a moderate amount of unobservable selection is more reasonable, since $\delta = 1$ is considered to be an upper bounding value.

[9] Here R_{max} is parametrized as: $R_{max} = \min\{\Pi\tilde{R}, 1\}$, with $\Pi = 1.5$.

In Table 1a, columns (3) and (5) report the bias-corrected estimates[10,11] of the effect of attending private-dependent schools relative to public schools from the two model specifications (without and with an interaction term). With few exceptions, the bias-corrected estimates are lower than the controlled estimates of the private school effect, and generally any negative controlled effect is magnified; over all countries, the estimated effect is moderately negative (i.e., 0.1 and 0.2 of a standard deviation of a standard deviation in favor of public schools, for $\delta = 0.5$ and $\delta = 1$ respectively). However, in a small number of countries private-dependent schools perform better than public schools under both assumptions about the extent of unobservable selection. These are Ireland, Netherlands (where almost all private schools are dependent), Canada and Indonesia, Argentina and Chile.

Table 1b reports the corresponding estimates of the bias-corrected effect for attending independent schools. A first observation is the huge difference between the raw score differentials on the one hand and the controlled and bias-corrected effects on the other. Across all countries, the raw score difference of 0.63 of a standard deviation (equivalent to almost 1.5 years of formal schooling) in favor of independent schools disappears once we control for observables. When we account for selection, even assuming moderate selection, the estimated average effect across countries is between 0.18 and 0.45 of a standard deviation in favor of public schools. The higher estimate is from the specification with the interaction of the independent school dummy with socioeconomic background of students at the school level; this reflects the large differences in the composition of independent schools compared to public schools with respect to student socioeconomic background.

Again, in a small number of countries the independent school advantage remains robust. These are Belgium, the Czech Republic, Indonesia and New Zealand, with some

[10] The Stata program *PSACALC* provided by Emily Oster was used.
[11] Deriving standard errors would require a bootstrap approach, which would depend on the estimator displaying asymptotic normality. Absence of standard errors would be more of a problem in a single country study, but less so for the objectives of the present study.

evidence that this may be the case for Ireland and possibly Jordan. However, Belgium, the Czech Republic and Ireland have a very small independent school sector (only 3-4 schools in the PISA sample), so private-independent schools could have been excluded from the analysis in Table 1b. Therefore, despite these few exceptions, there is strong evidence that public schools outperform independent schools (as well as private-dependent schools) in both the developed and less developed groups of countries for which data was available.

For the purpose of comparison on the potential relevance of selection in different contexts, I derived estimates of the well-known gender gap in favor of boys in mathematics after correcting for selection using the same methodology. While is it clear that unobservables are expected to bias the estimates of the effect of school choice on student performance, prior expectation is that this is much less so in the case of the gender effect. This is confirmed in the results given in Table A3 in the appendix. After controlling for observables the average male advantage over the 40 countries increases slightly (from 0.121 to 0.143 of a standard deviation in favor of boys); however, the estimates after accounting for selection hardly change.

4.5 Comparison of findings to the empirical literature

Existing empirical literature is more concentrated to OECD countries as well as Latin America, while little evidence is available for East Asia. Comparing the findings of this study to those in the empirical literature allows for the conclusion that, on the one hand certain past findings are contradicted (especially those pertaining to the comparison of private-dependent schools to public schools) and on the other, the findings of this study are in agreement to emerging literature suggesting that public schools may outperform private schools.

The study by Dronkers and Robert (2008) (cited in the literature review), is representative of the evidence for OECD countries. It investigated the effectiveness of public, private-dependent and independent schools in 19 OECD countries using PISA 2000 data and a multilevel approach; the authors controlled for a variety of characteristics, but did not account for unobservable selection. They conclude that, while private independent schools are less effective than public schools with the same students, parents, and social composition, private dependent schools are more effective than comparable public schools. In this study I find that for approximately the same group of OECD countries but using PISA 2012, the two types of schools are of similar effectiveness; accounting for selection points to a small but possibly insignificant public school advantage over private-dependent schools. On the other hand, the finding about independent schools is in agreement with Dronkers and Robert (2008).

Somers, McEwan and Willms (2004) questioned the common supposition that private schools are relatively more effective than public schools at improving student outcomes ("private school effect") in Latin American countries using UNESCO data for 10 countries and a multilevel approach. They find that private-public differences in achievement are only partly accounted for by better socioeconomic status of private schools and much more by differing peer group characteristics between the two types of schools. After accounting for peer group characteristics, they find that the average private school effect is near zero, with some variance around the mean. They consider this an upper bound effect, since selection may be biasing the estimates in favor of private schools.

Of the few countries in which this study finds a robust private school advantage, findings for the Netherlands, Ireland, Indonesia, Argentina and Chile can be compared to existing empirical evidence. Patrinos (2013) used PISA 2006 data for the Netherlands and an IV approach to find that that private school attendance is associated with higher test scores; for mathematics the private school effect is close to 0.2 of a standard deviation (three times larger than the OLS estimate). On the other hand, Cornelisz (2012) used PISA 2006 and 2009 and propensity score matching instead of instrumental variable estimation, arguing that the approach by Patrinos is "incomplete, highly unstable and unlikely to yield credible school type effects". He found small and statistically insignificant achievement differences between public-and private school students. In this study, after correcting for selection, the finding is essentially identical to that of Patrinos (2013), at about 0.2 of a standard deviation in favor of private (dependent) schools.

Evidence for Ireland can be found in the comparative study by Vandenberghe and Robin (2004), which used PISA 2000 data and three other approaches besides OLS regression: IV regression, Heckman's two-stage method and Propensity Score Matching. Matching results show an insignificant private school advantage, while using IV regression and Heckman's approach the opposite is the case, with the results driven by selection. However, the authors point to the main obstacle of implementing the IV and Heckman approaches, which is the difficulty of finding a valid instrument. In this study I find an advantage for students of private-dependent schools in the order of about 0.25 of a standard deviation, independently of estimation model.

Evidence for Indonesia is also contradictory. Bedi and Garg (2000) used labor market earnings as the measure of effectiveness and controlled for observable personal characteristics and school selection. They find that graduates of private secondary schools perform better in the labor market, contrary to the widely held belief in Indonesia that public secondary schools are superior. Newhouse and Beegle (2006) on the other hand, used Indonesian data on the national exit exam of grade 7-9 students. After controlling for various characteristics and considering two possible sources of bias (positive selection to public schools based on perceived higher quality on the one hand and wealthier households' preference for private schools on the other). They find that public school students score 0.17 to 0.3 of a standard deviation higher than private school students. However, they acknowledge that if public schools benefit from positive selection, their fixed effects - OLS estimates of the public school premium will be biased upwards. The finding of the present study, based on PISA 2012, is that private junior secondary school students in Indonesia (especially independent schools) perform better and this advantage is enhanced after accounting for selection.

Argentina and Chile have a long history of public support for private schools, i.e., funding salaries of teachers in Argentina and funding tied to enrolment in Chile (akin to a voucher system). McEwan (2000) compared the academic outcomes of seventh- and eighth-graders in public and private schools (Catholic subsidized, non-religious subsidized and private non-subsidized) in Argentina and Chile. In both countries, Catholic subsidized schools were found to be more effective than public schools in producing student outcomes, although it is suggested that these effects are probably an upper bound to the true effects, due to selection bias. These findings are consistent with those in this study which finds moderately better outcomes for private-dependent, but not for independent school students in both countries.

New research has been recently emerging from the US, which casts doubt on existing beliefs that private schools perform better. The book: *The Public School Advantage* by Lubienski and Lubienski (2013) is one of the most comprehensive studies of private vs. public school performance in mathematics, utilizing data on test scores from more than 300,000 students and more than 15,000 schools. Controlling for student characteristics, student socioeconomic background along with school demographic factors, the private school advantage disappears and often becomes negative.

Similarly, Elder and Jepsen (2014) examined the effect of Catholic school attendance in the US, using data on 8,000 elementary school students who were followed from kindergarten to grade 8. They employed different empirical strategies, including using selection on observables to assess the bias arising from selection on unobservables. They find that selection bias is entirely responsible for the initial Catholic school advantage and that the results point to sizeable negative effects of Catholic schooling on mathematics achievement.

4.6 Implications and conclusions

The empirical literature on school choice and effectiveness can be seen as consisting of two distinct strands. One, centered on the voucher programs (see for example, Coleman & Hoffer, 1987; Coleman, Hoffer, & Kilgore, 1982), using predominantly randomized studies, which finds gains for students switching to private schools. The other employs large datasets and suitable methodological approaches (multilevel modeling, IV regression, or other techniques) to account for both observables and selection, with findings which more often than not cast doubt on the existence of a positive private school effect and often point to a negative effect. The present study belongs in the second strand of empirical literature.

The attractiveness of the voucher studies and the consequent perception of a near-consensus that such programs are effective (hence a positive private school effect), can to a large extent be attributed to being randomized. However, there is no lack of criticism and objections to the conclusion that such studies are superior. Lubienski, Weitzel and Lubienski (2009) provide an insightful review of the literature which identifies major weaknesses of such randomized studies, thus refuting claims by school choice advocates that there is a consensus that vouchers for private schools lead to higher academic achievement. These weaknesses relate to: first, problems in properly accounting for selection arising from likely differences in the composition between treated and control groups, as such studies mainly account for observables; and second, inability to account for peer group effects (school level selection bias).

Randomized school choice studies vary widely in their design and are based on a subpopulation of students (those who apply for a voucher), not a population of school children; the fact that parents apply for a voucher on behalf of their child may be associated with unobservable qualities (such a motivation, value placed on education, etc.) which are different from those who do not apply, thus introducing a bias and making it difficult to generalise study findings. Similarly, over time attrition due to some students declining the offered voucher or private schools formally or informally being selective by discouraging less well performing students from applying or enrolling, would make the two groups increasingly different from one-another.

In assessing the effectiveness of (especially large scale) voucher programs from a policy makers perspective (but not necessarily from a parent's perspective[12]), one needs to consider whether a large part of private advantage found in voucher studies is due to positive spillover effects from better peer groups (as empirical evidence shows); if this is so, the results of such studies are further undermined. As argued in Somers, McEwan and Willms (2004), since the stock of good peers is finite, expanding the private school sector requires enrolling increasingly diverse populations drown from middle and lower income groups, gradually weakening private school effects at the margin.

While there is no ideal methodological approach, this study finds that accounting for both peer effects and selection is necessary when evaluating school effectiveness. Peer effects account for a substantial part of the private-public test score differences, especially in Latin America; not controlling for such effects would overestimate any private school advantage after accounting for observables. This study finds that in OECD, Middle East and Latin American countries, using peer controls eliminates the private-dependent school advantage and severely reduces the estimated independent school advantage, before accounting for selection. On the other hand, after accounting for selection allows for the conclusion that public schools are more effective in every part of the world examined, with few individual countries as exceptions.

[12] Arguably, parents wouldn't care if a school's effectiveness is enhanced by spillovers related to peer group composition, as long as their child benefits from such spillovers.

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Appendix

Table A1: Summary statistics of variables of interest by country

Country	**(2) % Private**	**(3) (%) Dependent / Independent schools**	**(4) % positive peer index (Private-dependent)**	**(4) % positive peer index (Private-independent)**	**(4) % positive peer index (Public)**
Europe/North America/Oceania					
Austria*	11.4	n/a	93.9	n/a	49.5
Belgium	70.8	98/2	68.0	100	51.5
Czech Republic	7.8	85/15	64.7	0	51.1
Denmark	16.5	86/14	82.1	97.0	72.0
Finland	5.9	100/0	87.7	-	86.5
France*	20.1	n/a	68.7	n/a	46.3
Germany	6.4	91/9	96.5	100	55.3
Greece	5.2	0/100	-	100	42.2
Hungary	15.3	100/0	45.6	-	36.5
Ireland	60.1	92/8	68.6	95.3	53.4
Italy	4.7	66/34	33.2	87.7	46.7
Netherlands	67.4	98/2	68.6	100	75.2
Poland	8.2	60/40	80.3	100	26.2
Portugal	9.2	61/39	33.4	100	16.0
Slovakia	8.0	93/7	78.1	100	33.3
Spain	38.4	86/14	60.1	84.6	18.8
Sweden	16.6	100/0	85.0	-	77.0
Switzerland	4.2	28/72	96.1	91.8	54.4
United Kingdom	19.6	79/21	84.3	100	69.0
Canada	8.3	52/48	85.2	100	87.0
USA	8.7	10/90	100	98.9	61.1
Australia	38.9	28/72	98.5	91.5	47.5
New Zealand	6.0	9/91	45.0	98.1	51.0
Mean	*19.9*	*68/32*	*74.6*	*81.5*	*52.5*
Middle East					
Jordan	11.1	4/96	0	57.5	9.0
Qatar	38.0	2/98	100	92.2	76.4
UAE	57.5	27/73	91.3	88.9	45.7
Mean	*35.5*	*11/89*	*63.8*	*79.5*	*43.7*
South America					
Argentina	36.5	77/23	49.1	71.3	6.4
Brazil	14.3	27/73	47.3	56.3	0.7
Chile	71.7	57/43	23.4	94.1	1.9
Colombia	20.7	45/55	21.2	58.0	0.6
Costa Rica	14.1	40/60	65.8	84.6	1.7
Mexico	12.3	21/79	66.4	68.1	3.7
Peru	20.8	22/78	18.3	25.5	0.7
Uruguay	16.5	0/100	-	38.2	0.9
Mean	*25.9*	*36/64*	*41.6*	*62.0*	*2.1*
East Asia					
Indonesia	40.5	44/56	0	4.3	1.0
Japan	27.7	0/100	-	67.6	31.2
Korea	47.1	66/34	34.3	81.7	53.7
Taiwan	40.5	13/87	0	25.5	19.6
Thailand	12.5	71/29	0	12.4	17.2
Vietnam	8.2	0/100	-	0	1.5
Mean	*29.4*	*32/68*	-	*31.5*	*20.7*
Overall mean	***23.7***	***51/49***	***65.4***	***73.4***	***37.0***

Note: No information is available on proportion government-dependent private schools for Austria and France.

Table A2: Controlled private school effects without and with peer group controls

	Private-dependent vs. Public schools		Independent vs. Public schools	
	Without	**With**	**Without**	**With**
Europe/North America/Oceania	0.101	0.010	0.204	0.064
Middle East	-	-	0.330	0.165
South America	0.168	-0.043	0.417	0.095
East Asia	-0.340	-0.249	-0.315	-0.332
Overall mean	***0.059***	***-0.038***	***0.172***	***0.007***

Table A3: Estimated effects on the standardized Mathematics PISA score –The gender gap in Mathematics

Country	**(1)**	**(2)**	**(3)**	
	Raw score diff.	**Controlled effect**	**Bias-adj. effect**	
Europe/North America/Oceania			**δ = 0.5**	**δ = 1**
Austria	0.253 (0.029)	0.316 (0.024)	0.332	0.348
Belgium	0.105 (0.022)	0.145 (0.016)	0.154	0.164
Czech Republic	0.125 (0.027)	0.208 (0.021)	0.229	0.250
Denmark	0.169 (0.023)	0.105 (0.019)	0.088	0.072
Finland	0.011 (0.021)	-0.040 (0.018)	-0.052	-0.065
France	0.100 (0.029)	0.163 (0.021)	0.179	0.194
Germany	0.152 (0.028)	0.198 (0.023)	0.209	0.220
Greece	0.098 (0.028)	0.168 (0.023)	0.184	0.202
Hungary	0.133 (0.029)	0.202 (0.021)	0.219	0.236
Ireland	0.193 (0.028)	0.197 (0.032)	0.198	0.199
Italy	0.229 (0.011)	0.307 (0.010)	0.326	0.346
Netherlands	0.094 (0.030)	0.168 (0.021)	0.186	0.204
Poland	0.040 (0.029)	0.032 (0.025)	0.030	0.028
Portugal	0.129 (0.026)	0.161 (0.022)	0.168	0.176
Slovakia	0.087 (0.029)	0.141 (0.023)	0.154	0.167
Spain	0.172 (0.013)	0.181 (0.011)	0.184	0.186
Sweden	-0.023 (0.029)	-0.089 (0.025)	-0.104	-0.119
Switzerland	0.129 (0.019)	0.147 (0.016)	0.151	0.156
United Kingdom	0.141 (0.018)	0.120 (0.017)	0.115	0.110
Canada	0.125 (0.014)	0.115 (0.012)	0.113	0.111
USA	0.085 (0.028)	0.071 (0.025)	0.067	0.064
Australia	0.122 (0.017)	0.113 (0.015)	0.111	0.109
New Zealand	0.137 (0.030)	0.108 (0.030)	0.097	0.085
Mean	*0.122*	*0.141*	*0.145*	*0.150*
Middle East				
Jordan	-0.328 (0.024)	-0.201 (0.047)	-0.166	-0.131
Qatar	-0.165 (0.019)	-0.057 (0.022)	-0.030	-0.003
UAE	-0.072 (0.019)	-0.046 (0.025)	-0.040	-0.034
Mean	*-0.188*	*-0.101*	*-0.079*	*-0.056*
South America				
Argentina	0.174 (0.026)	0.211 (0.020)	0.220	0.229
Brazil	0.240 (0.014)	0.235 (0.012)	0.234	0.233
Chile	0.329 (0.024)	0.308 (0.018)	0.302	0.296
Colombia	0.403 (0.021)	0.368 (0.017)	0.357	0.347
Costa Rica	0.399 (0.029)	0.345 (0.024)	0.330	0.315
Mexico	0.196 (0.011)	0.227 (0.009)	0.235	0.243
Peru	0.264 (0.026)	0.320 (0.021)	0.335	0.349
Uruguay	0.155 (0.027)	0.170 (0.022)	0.174	0.178
Mean	*0.270*	*0.273*	*0.273*	*0.274*
East Asia				
Indonesia	0.059 (0.027)	0.094 (0.023)	0.102	0.111
Japan	0.213 (0.025)	0.187 (0.020)	0.181	0.175
Korea	0.171 (0.028)	0.064 (0.031)	0.038	0.011
Taiwan	0.044 (0.026)	0.057 (0.021)	0.061	0.064
Thailand	-0.162 (0.025)	0.009 (0.019)	0.051	0.082
Vietnam	0.123 (0.028)	0.212 (0.023)	0.235	0.257
Mean	*0.075*	*0.104*	*0.111*	*0.117*
Overall mean	***0.121***	***0.143***	***0.149***	***0.154***

Note: Standard deviations/standard errors in parentheses.