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The Effectiveness and Equity of Public-Private Partnerships in Education: A Quasi-Experimental Evaluation of 17 Countries

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Abstract: I estimate achievement effects of education public-private partnerships (PPPs) in 17 countries on the 2009 PISA assessment. Enrollment in PPP schools is tied to student wealth and prior academic ability. PPP students outperform their public peers on half of all outcomes. After accounting for selection, the PPP performance advantage remains on one-quarter of outcomes. However, nearly all of these performance differences are accounted for by school-level peer group effects. PPP schools are outperforming their public counterparts not through any advantages in productive efficiency but through sorting of more capable students.
Keywords: public-private partnerships; vouchers; school choice; peer effects; PISA

La efectividad y la equidad de las alianzas público-privadas en educación: Una evaluación cuasi-experimental de 17 países
Resumen: Estimo los efectos de logro de las alianzas público-privadas (PPP) educativas en 17 países en la evaluación PISA 2009. La inscripción en las escuelas PPP está ligada a la

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riqueza estudiantil y la capacidad académica previa. Los estudiantes de PPP superan a sus pares públicos en la mitad de todos los resultados. Después de tomar en cuenta la selección, la ventaja de rendimiento de PPP permanece en una cuarta parte de los resultados. Sin embargo, casi todas estas diferencias de rendimiento se explican por los efectos del grupo de iguales a nivel escolar. Las escuelas PPP están superando a sus contrapartes públicas no por ninguna ventaja en la eficiencia productiva, sino por la clasificación de estudiantes más capaces.
Palabras clave: alianzas público-privadas; cupones; elección de escuela; efectos de pares; PISA

A eficácia e a equidade das parcerias público-privadas na educação: Uma avaliação quase experimental de 17 países
Resumo: Estimo os efeitos de realização de parcerias de educação público-privadas (PPPs) em 17 países na avaliação do PISA 2009. A inscrição em escolas de PPP está vinculada à riqueza dos estudantes e à capacidade acadêmica anterior. Os alunos de PPP superam seus pares em metade de todos os resultados. Depois de levar em consideração a seleção, a vantagem de desempenho do PPP permanece em um quarto dos resultados. No entanto, quase todas essas diferenças de desempenho são explicadas pelos efeitos do grupo de pares no nível da escola. As escolas de PPP estão superando suas contrapartes públicas não por causa de qualquer vantagem em eficiência produtiva, mas por causa da classificação de alunos mais capacitados.
Palavras-chave: parcerias público-privadas; cupões; escolha escolar; efeitos de pares; PISA

Introduction

Government interaction with the private sector has become increasingly common in education policy making in countries around the world. In particular, states seem to be exploring the potential benefits of market-related education interventions by funding various types of private education initiatives (Jimenez, Hofman, Velez, & Patrinos, 2011; LaRocque & Lee, 2011). The term 'public-private partnership' (PPP) is a generic descriptor, which captures any form of shared responsibility between government and private actors across the spheres of school finance, ownership, and management (LaRocque & Lee, 2011; Patrinos, Barrera-Osorio, & Guaqueta, 2009). Education PPPs are not a particularly new phenomenon. Different combinations of school finance, management, and ownership are prevalent across the world, and have historically been important components of national education systems in low-, middle- and upper-income countries (Baum, Lewis, Lusk-Stover, & Patrinos, 2014; Patrinos et al., 2009). An increasing amount of research demonstrates the growth of such arrangements for delivering and financing basic education services in systems across Europe, Latin America, the Middle East, Asia, the Pacific, and Africa (Brewer & Hentschke, 2009; Epple, Romano, & Urquiola, 2015; Morgan, Petrosino, & Fronius, 2015; Patrinos, 2013). Government interest in establishing PPPs in education is typically driven by one or more of the following aims: (1) to increase the quality of education services, (2) to increase access to (and participation in) basic education, and (3) to meet one or more of these objectives at a lower cost relative to public provision (Patrinos et al., 2009).

Notwithstanding the existing research on the subject, empirical evidence investigating the achievement and equity impacts of education PPPs on students is still greatly lacking, with relatively few rigorously sound evaluations of these programs. Many countries' PPP models have received little empirical attention; and, to my knowledge, no study has yet attempted to assess the impact of education PPPs systematically across multiple countries. This study provides a quasi-experimental evaluation of delivery-based PPPs (i.e., partnerships that combine public finance with private provision) across a range of countries, using data from the 2009 Programme for International Student Assessment (PISA). Using propensity score matching to account for the non-random selection of students into schools, I estimate the achievement effects of attending a PPP school within 17 different countries. I investigate whether any performance differences can be explained by the productive efficiency of schools, or are simply attributable to differences in student peer group composition between public and PPP schools. Lastly, drawing upon the robust body of research that finds privatization to be related to social stratification (Epple & Romano, 2008; Hsieh & Urquiola, 2006; Ladd, Fiske, & Ruijs, 2010), I analyze the extent to which participation in PPP schools is conditioned by student background characteristics such as socioeconomic status and prior academic ability.

Public-Private Partnerships in Education

Public-private partnerships disrupt the traditional approach to delivering education services. Whereas, historically, most education systems have been built upon public finance and provision of education, partnerships between state and non-state entities have explored approaches for sharing these responsibilities in various combinations. Although private school voucher programs—those which direct government funds to students to attend the private schools of their choice—are the strategy that seem to have received the most attention in the research literature (Epple, Romano, & Urquiola, 2015; Neal, 2002), other PPP models include government subsidies to private schools (sometimes known as 'aided-schools' or 'dependent' private schools), and privately managed schools—such as in the U.S. charter school model.

The Theoretical Rationale of Privatization and Public-Private Partnerships

The theoretical rationale underlying private education suggests that market forces have the potential to improve school governance, increase accountability to students and parents, and improve student cognitive outcomes at a lower cost than providing all basic education services through the state (Chubb & Moe, 1990; Hoxby, 2000a; Macleod & Urquiola, 2012). Public-private partnerships are meant to facilitate high performance of schools, teachers, and students by leveraging private sector activity and increasing levels of school choice, competition, accountability, and autonomy (Baum et al., 2014; Gauri & Vawda, 2003). These improvements are made possible through contractual relationships between providers and the state (Jimenez et al., 2011; LaRocque & Lee, 2011), and by fostering competition and choice while controlling standards and objectives through state regulation and quality assurance mechanisms (Neal, 2002; Sakellariou & Patrinos, 2009).

The primary critical argument against private education centers on the social impacts of market-based education delivery: namely, that increased privatization attenuates social disparities of educational access (Apple, 2001; Klees, 2012; Levin & Belfield, 2003). For example, studies across countries have shown rather consistently that universal school choice programs are likely to facilitate sorting of more socioeconomically privileged students into private schools, thus reinforcing social inequalities in accessing the highest quality education services (Epple & Romano, 2008; Hsieh & Urquiola, 2006; Ladd et al., 2010).

Evidence on the Effectiveness of Delivery-based PPPs

In general terms, the existing research finds mixed results regarding the impacts of education PPPs on student academic achievement. In the United States, although there are some clear cases of high performing charter schools (Abdulkadiroğlu, Angrist, Dynarski, Kane, & Pathak, 2011; Angrist, Cohodes, Dynarski, Pathak, & Walters, 2013; Tuttle et al., 2013), systematic reviews and cross-state studies find that charter schools, on average, tend to perform on par with traditional public schools, with the exception of superior charter performance for more disadvantaged schools and students (for example, in poorer, urban locations; Clark, Gleason, Tuttle, & Silverberg, 2015; CREDO, 2015; Epple, Romano, & Zimmer, 2015). Experiences with privately-managed public schools in the Netherlands and Latin America, however, have demonstrated some positive results in terms of student access, progression, and achievement (Barrera-Osorio, 2006; Patrinos, 2013; Sánchez, Clavijo, Espinel, & Arias, 2015; Swope & Latorre, 2000).

Policy experience with school voucher programs, both in and out of the United States, has been met with similar mixed results (Epple, Romano, & Urquiola, 2015; Neal, 2002). At the macro level, universal voucher programs have been ineffective in their attempts to spark system-wide performance improvements through increased choice and competition (Macleod & Urquiola, 2012). However, targeted vouchers in multiple countries have experienced marked success in increasing access to schooling for previously underserved students, and in raising student learning for targeted groups of underperforming students (Adelman, Holland, & Heidelk, 2017; Angrist, Bettinger, Bloom, King, & Kremer, 2002; Barrera-Osorio et al., 2013).

The Role of Peer Effects in School Sector Performance

A concern with respect to research on the relative effectiveness of school sectors questions whether differences in performance are due to increased productive efficiency of public or private schools, or simply due to a redistribution (i.e., "cream skimming") of high-ability students into a more privileged school sector, as has been found in the case of private school voucher programs (Arenas, 2004; Epple, Romano, & Urquiola, 2015; Kremer & Holla, 2009). If the impact of PPP programs is due solely to sorting of students, such programs would benefit the privileged at the expense of the disadvantaged (as the overall quality of peers in the system is fixed), and would create negative externalities for the larger education system (Bettinger, Kremer, & Saavedra, 2010; Hsieh & Urquiola, 2006; Uribe, Murnane, Willett, & Somers, 2006). Within the empirical literature there is strong evidence to suggest that peer group composition has substantial import for the performance of students within private and PPP schools—wherein students will perform better when surrounded by higher-achieving peers (Hanushek, Kain, Markman, & Rivkin, 2003; Hoxby, 2000b). Abdulkadiroğlu et al. (2011), Angrist, Cohodes, et al. (2013), and Nichols-Barrer, Gill, Gleason, & Tuttle (2014) find that peer effects account for some of the performance advantage of charter schools in the United States. In their study of achievement of Bogota's public and private subsidized schools, Uribe et al. (2006) also found peer effects to be important determinants of student academic performance. Conversely, when increasing the share of lower-performing peers in a school, one will find negative impacts on classroom behavior, on teacher pedagogical practices, and on the achievement of average students (Imberman, Kugler, & Sacerdote, 2012; Lavy, Paserman, & Schlosser, 2011).

Somers, McEwan, and Willms, (2004) argue that many studies of the effects of private schools have incorrectly mistaken a private sector peer group effect for a private school advantage in productive efficiency, as a result of failing to account for the effect of student peer groups. In their own study of private school effectiveness across 10 countries in Latin America, these researchers find no performance difference between public and private schools, after accounting for student peer group differences. As such, research into school performance that fails to account for student peer group characteristics is liable to overestimate productive efficiency, attributing the performance spillovers of the student peer group to the school itself.

Study Sample

The populations of interest for this study include students in participating PISA countries that are enrolled in (1) public schools and (2) publicly-funded private schools. Of the 65 countries that participated in the 2009 PISA assessment, 43 include samples from both of these populations. For each of these countries, I conducted *a priori* power analyses to determine which had large enough student and school sample sizes to produce significant and meaningful results from the statistical tests. Through this analysis, I identified 17 countries with sample sizes large enough to produce significant results at $\alpha = .05$, with a .30 effect size and statistical power of .80, accounting for the degree of between-school variation in PISA outcomes (the intraclass correlation coefficient (ICC)): Argentina, Australia, Austria, Belgium, Chile, Denmark, Hungary, Indonesia, Ireland, Netherlands, Portugal, Slovak Republic, South Korea, Spain, Sweden, Thailand, and Trinidad and Tobago. In practice, the majority of these countries have sample sizes far above the minimum threshold for detecting significant effects at this level—such that detection of much smaller effect sizes is possible.

There are important differences in the size of the PPP sector across countries. Figure 1 presents shares of total enrollment by school sector. In four of the 17 countries, more than 40% of students are enrolled in state-funded private schools: Belgium (61%), the Netherlands (61%), Ireland (44%), and Chile (40%). For Belgium, the Netherlands, and Chile, these large PPP sectors are the result of national voucher programs, wherein the state pays for the education of students in a large number of eligible private schools. In Ireland the government subsidizes a large number of students to study within private secondary institutions. In an additional five countries—Argentina, Australia, Denmark, Korea, and Spain—between 15% and 25% of age-15 students are enrolled in state-funded private schools. Averaging across the set of countries, PPP schools receive the same amount of funding from the state as government schools (roughly 81% of total school funds).

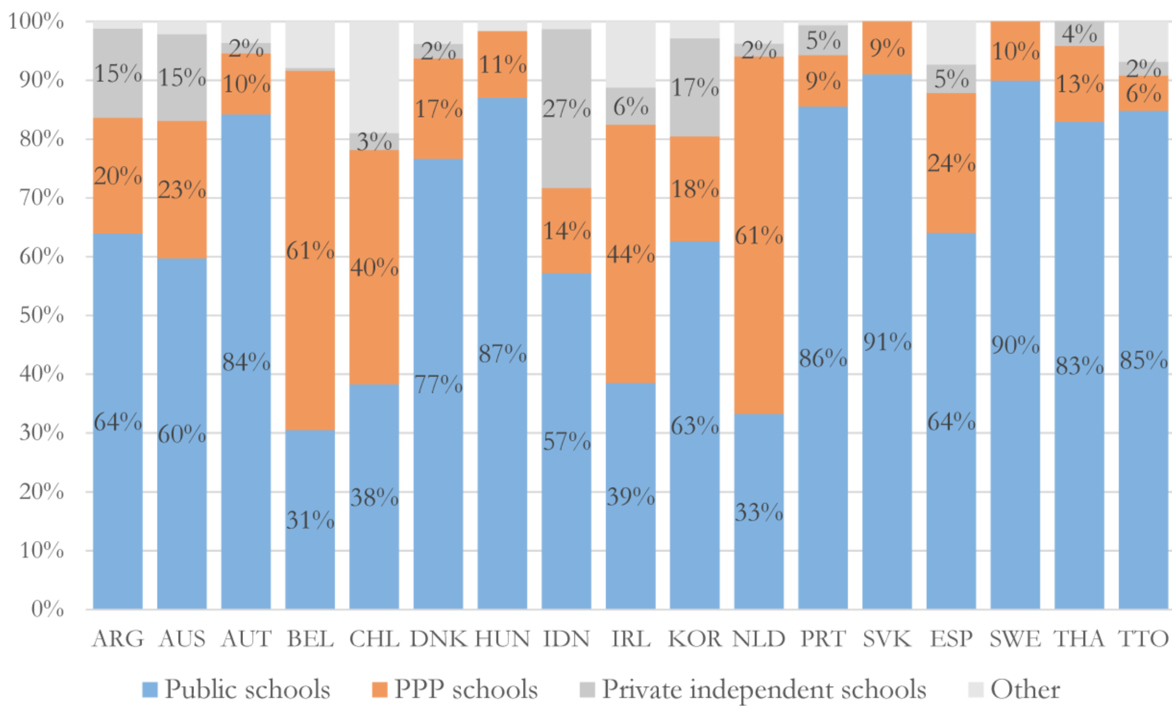

Figure 1. *Share of total enrollments by school sector and country*

Additionally, there are differences in the composition of students between sectors across countries. Point-biserial correlations show that enrollment in a PPP school is associated with higher student socioeconomic status in 13 of the 17 countries. The magnitudes of these correlations range from .025 in Thailand to .311 in Argentina, with a cross-country mean correlation of .088. In Indonesia, the Netherlands, South Korea, and Portugal, the correlation between school sector and student SES favors the public sector, ranging from -.088 in Hungary to -.283 in South Korea, with a cross country mean correlation of .181. All 17 of these relationships are significant at $p < .001$. The higher SES levels of the private school students appears to be driven in part by the higher household cost of attending these schools. Across countries, PPP schools fund roughly 4% more of their school budget through student fees than do public schools. In 11 countries, average SES is significantly higher for students in PPP schools than for students in public schools. In only two countries—Indonesia and South Korea—do students in public schools have higher SES than students in PPP schools.

Data and Variables

This study uses student achievement data from the 2009 Programme for International Student Assessment (PISA), the global initiative that aims to assess the preparedness of 15-year-olds to meet the needs of today's knowledge-based economies (OECD, 2009). The exam assesses student abilities across the subjects of reading, mathematics, and science. More than 470,000 students participated in the PISA assessment in 2009, representing populations of roughly 26 million students across 65 participating countries. The PISA exam utilizes a two-stage stratified sampling design, with schools as the primary sampling unit, and students as the secondary sampling unit. To account for the unequal probability of student selection across schools (and sampling strata) all statistical analyses include probability weights to accurately reflect representation of the population of 15-year-old students nationwide.

The dependent variables of interest in this study are student math, reading, and science scores. The PISA testing procedure utilizes a method for estimating a range of each student's performance across a set of five plausible values (OECD, 2009). PISA reports these scores using a normed scale with an OECD mean of 500 and a standard deviation of 100. Accurate analysis of these plausible values requires that each statistical test is computed five times, and the average of the results reported.

The treatment variable of interest—school sector—is represented by a binary variable (*PUBPRIV*), which equals 1 for PPP schools and 0 for government schools.[1] A PPP school is defined as being operated privately while receiving at least 50% of total funding from the government. As such, this category captures private schools receiving government funds through subsidies, voucher programs, or private management contracts; although, it is unfortunately unable to distinguish between the three. The covariates selected for propensity matching within this study were those, described above, that the research literature shows to significantly predict selection into private schools, including student sex, grade, immigration status, native language, attitudes towards school, family structure, and school location.

The PISA variable *GRADE* indicates the student grade relative to the modal grade for 15-year-olds in each particular country. For example, values of -1 and 2 represent students that are respectively one year below and two years above the modal grade.

For measuring student attitudes towards school, I use a composite factor variable derived from four indicators of student school attitudes: (1) school has done little to prepare me for adult life; (2) school has been a waste of time; (3) school has helped give me confidence to make decisions; (4) school has taught me things which could be useful in a job. Negatively phrased items are reverse coded. Higher values on this variable indicate a more positive attitude towards school.

A PISA variable of immigration status (*IMMIG*) is used to create dichotomous variables representing each immigration category: (1) native; (2) second-generation (student born in the country, parents born in another country); and (3) first-generation (student and parents born outside the country). Immigration status has been shown to significantly predict private school attendance and school cognitive outcomes and is particularly impactful in many European contexts (Dronkers, van der Velden, & Dunne, 2011).

A dichotomous variable with value of 1 indicates that the language of the test is the primary language spoken at home. A value of 0 indicates that the language of the test is not the primary language spoken at home.

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[1] Students in private independent schools (i.e., private schools that receive less than half of their total funding from the state) are not included in this study.

The PISA composite factor variable *ESCS* contains information from numerous variables representing a range of family economic, wealth, and cultural information. Larger values represent higher socioeconomic status. This variable combines information from three separate indices:

1. The family home possession index captures information from four separate PISA measures:
    a. Family wealth: a measure combining information pertaining to a number of student and family possessions: student has a room of his/her own, internet link, dishwasher, DVD or VCR, three country-specific wealth items, cell phones, TVs, computers, cars, rooms with bath or shower.
    b. Home educational resources: a measure combining information regarding various educational resources at home: student has a desk to study at, a quiet place to study, a computer he/she can use for work, educational software, his/her own calculator, books to help with school work, a dictionary.
    c. Cultural possessions: this measure combines information pertaining to a number of cultural possessions at home, such as whether the home has classic literature, books of poetry, and works of art.
    d. Number of books in the home.
2. Highest occupational status of parents: this variable uses Ganzeboom, De Graaf, and Treiman's (1992) internationally-comparable index of occupational status. If the student has more than one parent, the highest value is used. Higher scores indicate higher occupational status.
3. Highest educational level of parents, recorded in number of years of education. If the student has more than one parent, the highest value is used.

I use the PISA family structure variable (*FAMSTRUC*) to create dichotomous indicators for family type: (1) single-parent family, (2) two-parent nuclear family, and (3) mixed family. Using the PISA school community variable (*SC04Q01*), I create dichotomous indicators to signify each community size: (1) village (fewer than 3,000 people), (2) small town (3,000 to about 15,000 people), (3) town (15,000 to about 100,000 people), (4) city (100,000 to about 1,000,000 people), and (5) large city (more than 1,000,000 people).

I assess the impact of peer effects on student achievement using a composite measure of average school socioeconomic status (*AGGSES*), an aggregated variable created from the individual student SES variable, *ESCS*. At the student level, *ESCS* has an OECD mean of 0 and a standard deviation of 1. Additionally, the explanatory variable, *GFUND,* indicates the share of school funding provided by the government. This school finance variable is included in some of the statistical models, as the level of funding received from the state may influence the behavior of recipient schools (Wößmann, Lüdemann, Schütz, & West, 2007).

Descriptive statistics (pooled across country samples) are provided for the key covariates in Table 1. Mean differences show that, on average, students in PPP schools tend to come from more advantaged households, are more likely to come from two-parent families, and are more likely to reside in urban areas. Meanwhile, public school students, on average, are reporting more positive attitudes towards school, are more likely to be native-born, and are generally found in higher grades.

Table 1
International descriptive statistics for student-level pre- and post-matching covariates

	Public schools		Publicly-funded private schools	Standardized mean difference		
	Pre-match mean	Post-match mean	Mean	Pre-match	Post-match	Percent balance improvement
Sample size	269,251	42,128	42,128			
Student covariates						
Propensity score	.121	.219	.227	.753	.053	93.2
Female	.507	.510	.510	.006	.013	78.2
Attitude towards school	.093	-.010	-.028	-.129	.067	85.6
Grade	-.170	-.271	-.306	-.189	.146	74.8
Language	.892	.846	.879	-.041	.109	-156.8
Immigration status						
Native	.897	.776	.787	-.267	.115	89.6
Second generation	.044	.132	.127	.252	.107	94.1
First generation	.036	.076	.069	.131	.046	80.7
Family covariates						
Socioeconomic status	-.356	-.116	-.161	.184	.154	77.2
Family structure						
Single parent family	.169	.158	.148	-.059	.029	51.0
Two parent family	.753	.791	.805	.132	.035	72.7
Mixed family	.039	.027	.025	-.087	.024	87.4
School covariates						
School community						
Village	.098	.044	.046	-.245	.027	94.9
Small town	.241	.134	.136	-.307	.042	98.1
Town	.331	.258	.270	-.137	.039	80.9
City	.228	.325	.345	.248	.107	82.3
Large city	.099	.238	.201	.253	.122	64.2
Share of gov. funding	.809	.812	.815	.006	.013	12.2

Note: Descriptive statistics presented here are pooled across countries in the international sample

Methodological Approach

Linear Regression

I apply three methodological approaches—all used in prior studies of the relative effectiveness of public and private schools (French & Kingdon, 2010)—to produce estimates of the effect of attending a PPP school. The first approach estimates two-level linear regression models (via maximum likelihood estimation), with the school sector dummy variable—*PUBPRIV*—included to indicate student enrollment in a government-funded private (as opposed to a public) school. The first model examines the unconditional achievement differences between students in public schools and PPP schools. This step applies a 'means-as-outcomes' model (Raudenbush & Bryk, 2002):

$$Y_{ij} = \gamma_{00} + \gamma_{01} (PUBPRIV_j) + u_{0j} + r_{ij} \quad (1)$$

In this mixed (two-level) model, Y_{ij} represents the achievement outcome of student *i* in school *j*. The coefficient γ_{01} captures the treatment effect of attending a PPP school (*PUBPRIV*). Model 1 is run separately for each country and exam subject (17 countries × 3 exams subjects = 51 models). However, given the absence of student- or school-level covariates, apart from the school sector dummy, the results for γ_{01} provide biased estimates of the effect of attending a PPP school. Model 2 adds a vector of observed student demographic characteristics (***X***) to account for systematic observable differences between public and PPP students.

$$Y_{ij} = \gamma_{00} + \gamma_{01} (PUBPRIV_j) + \gamma_{10} \boldsymbol{X}_{ij} + u_{0j} + r_{ij} \quad (2)$$

Insofar as ***X*** captures the full set of student characteristics that determines selection of a PPP versus a private school, γ_{01} from model 2 will provide the causal effect of PPP school enrollment on student achievement. However, if there are any unobserved factors (e.g., ability, motivation, or resources of the student or household) outside of ***X*** that contribute to private school enrollment and also influence student achievement, the estimated private school causal effect will be biased. Such bias will be captured by the student level-error term in the model, r_{ij}.

To account for the effect of school peers on a student's academic achievement within public and PPP schools, I follow studies such as Ammermueller & Pischke (2009), McEwan (2003), and Schneeweis & Winter-Ebmer (2007), which use school-level data on student socioeconomic status to account for peer effects. By adding a school-level aggregate measure of student socioeconomic status (*AGGSES*), the model ensures that the effect of being in a school with different compositions of peers is not conflated with relative differences in public and private school effectiveness. Additionally, this model includes a school-level variable—*GFUND*—indicating the share of school funding provided by the state, which has been shown in prior research to predict performance of students in private schools (Wößmann et al., 2007):

$$Y_{ij} = \gamma_{00} + \gamma_{01} (PUBPRIV_j) + \gamma_{02} (AGGSES_j) + \gamma_{03} (GFUND_j) + \gamma_{10} \boldsymbol{X}_{ij} + u_{0j} + r_{ij} \quad (3)$$

Oaxaca-Blinder Decomposition

The second approach used for investigating the performance differences between students in public and PPP schools is a decomposition of public and PPP student achievement using the method of Oaxaca (1973) and Blinder (1973). Although the method was initially developed for the estimation of labor market wage differentials, it has more recently been applied in a number of studies on the relative performance of public and private school students, as a means of parsing out the school sector achievement difference into the portion accounted for by school productivity and the portion attributable to differences in student endowments (i.e., observable characteristics). Whereas the linear regression models 2 and 3 fit single achievement functions for the pooled public and PPP students, the Oaxaca-Blinder approach fits separate achievement models for public and PPP students:

$$Y_{i\text{Pub}} = \beta_{\text{Pub}} \mathbf{X}_{i\text{Pub}} + \mu_{i\text{Pub}} \quad (4)$$

$$Y_{i\text{PPP}} = \beta_{\text{PPP}} \mathbf{X}_{i\text{PPP}} + \mu_{i\text{PPP}} \quad (5)$$

In these models, *Y* represents the performance of student *i,* controlling for observed student characteristics **X***,* in both public (Pub) and PPP schools. Models 4 and 5 are used to predict the outcome for a hypothetical student with the average characteristics of the pooled student population, first using the coefficients obtained from model 4 and then from model 5. This estimates the achievement of our hypothetical student, if enrolled in a public and then a PPP school. The relative effectiveness of public versus PPP schools is given by the difference in predicted scores: ($\hat{Y}_{\text{Pub}} - \hat{Y}_{\text{PPP}}$). The Oaxaca-Blinder framework separates the difference in school sector performance into a component due to observed student population differences and a component representing school effectiveness, conditional on student characteristics (however, this second component may also reflect the influence of unobservable student characteristics). Although the approach does not account for unobservable differences between school sectors, at the very least it proves useful in quantifying the extent to which observable differences in student compositions are responsible for any school sector advantage.

Propensity Score Matching

As a final approach for estimating the private versus PPP achievement differential, I run models 2 and 3 on adjusted samples of public and PPP students in each country, after constructing equivalent treatment and comparison groups through a propensity score matching framework. This approach attempts to address the endogenous bias introduced by non-random selection of students into schools. The propensity score is defined as the conditional probability of assignment to a particular treatment (e.g., enrollment in a PPP school) as a function of a vector of observed student characteristics (Rosenbaum & Rubin, 1983; Rubin, 1997). The ability of PSM to closely match the estimates produced by randomized experiments is dependent upon the assumption of a treatment assignment that is strongly ignorable (Rosenbaum & Rubin, 1983). Conceptually, this is satisfied in the existence of treatment and control units that are perfectly matched on the propensity score (Rosenbaum and Rubin, 1984). Thus, for public and private students with the same probability of attending a private school, the difference between means is an unbiased estimate of the average treatment effect (D'Agostino & Rubin, 2000).

Satisfying this condition requires a set of covariates capable of matching public and private students by likelihood of private school attendance—an aim best met through observable covariates that have been shown to determine private school attendance (Bai, 2011; Rubin, 1997). I draw upon the large body of previous empirical research to identify the determinants of this selection process—finding certain family, student, and community characteristics to be predictive of student enrollment in private schools. For example, in many cases, where private education requires more private investment than public education, family income is a significant predictor of private school attendance (Figlio & Stone, 2001; Long & Toma, 1988). Other important covariates that are empirically linked to private school attendance include family structure, family socioeconomic status, family size, homeownership, parent education, parent occupation, parent and student immigration status, parental involvement in the child's education, student sex, race, educational ability, school average socioeconomic status and size of the city of residence (Andersen, 2008; Betts & Fairlie, 2001; Buddin, Cordes, & Kirby, 1998; Escardibul & Villarroya, 2009; Figlio & Stone, 2001; Lankford & Wyckoff, 2001; Long & Toma, 1988). As presented in the data section above, those variables that are mentioned here as important to private school enrollment, and are included (or some similar measure) in the PISA 2009 database, are used to calculate the propensity score.

Propensity scores were calculated by regressing the dichotomous school sector variable on the set of student and household covariates. In line with Dehejia & Wahba (2002), I use a 'nearest-neighbor' matching algorithm to match every PPP student to the public student with the nearest propensity score. The result is a statistically-constructed comparison group of public school students, equal in sample size to the treatment group, and much more similar on important background measures than the original data (Rutkowski & Rutkowski, 2008). The extent of this balance is assessed after each matching iteration.

In the case of three countries—Chile, Ireland, and the Netherlands—there were insufficient students in the public sector to allow for nearest-neighbor matching. In these instances, a process of *exact matching* using only three covariates (student socioeconomic status, grade, and attitude towards school) was followed. These results should be treated with caution, as treatment and control groups may not be balanced across omitted covariates; however, given that SES is the covariate with potentially the largest impact on private enrollment, and is composed of nearly 20 other student background characteristics, the exact matches still capture a rich set of self-selection determinants. The more pertinent issue is the substantial loss of data resulting from incomplete matching (McKinlay, 1975; Rosenbaum & Rubin, 1985). In Chile, Ireland, and the Netherlands, only 13.7%, 7%, and 19.4% of the cases remain after exact matching. Thus, while the ability to compare these students with identical family backgrounds is beneficial, the results should not be considered representative of the PPP sector effects at the national level for these three countries.

Assessing propensity score overlap and covariate balance. To satisfy the ignorability condition assumed within the propensity score causal framework, in the absence of perfect propensity score matches between treatment and control groups, adequate overlap in the distribution of propensity scores between treatment and control groups must be demonstrated. Without an empirical test to assess overlap, a visual inspection of distributions is typically used. Figure 2 shows the overlap in the distribution of propensity scores in treatment and comparison groups following the matching process, with high levels of overlap apparent in nearly all country samples[2]. Perhaps the one exception is the case of Spain, wherein the propensity score distribution for PPP students (dotted line) shows a bimodal distribution with a higher mean than the unimodal distribution of the public student distribution (solid line). For the remaining countries, however, results suggest that there is sufficient overlap to satisfy the ignorability assumption.

Following the matching process within each country, I assess covariate balance between groups. In line with Cochran (1968), I use standardized mean differences of less than 0.2 between treatment and control groups as the threshold for covariate balance (Ho, Imai, King, & Stuart, 2007; Imai, King, & Stuart, 2008). Table 1 displays the changes in balance for the pooled international sample of students before and after propensity score matching. The matching process was successful in producing standardized public-private differences less than the critical value (.2) on all covariates. I call particular attention to the changes in the average propensity score, where standardized differences were significantly reduced (from .753 to .053). Figure 3 shows the pre-post changes in standardized mean differences between treatment and control groups on all covariates for all countries, with reduction in bias between public and PPP sectors after propensity score matching.

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[2] Common support is not assessed for Chile, Ireland, and the Netherlands. The exact matching process used for these countries inherently results in perfect matches, and thus satisfaction of the ignorability condition.

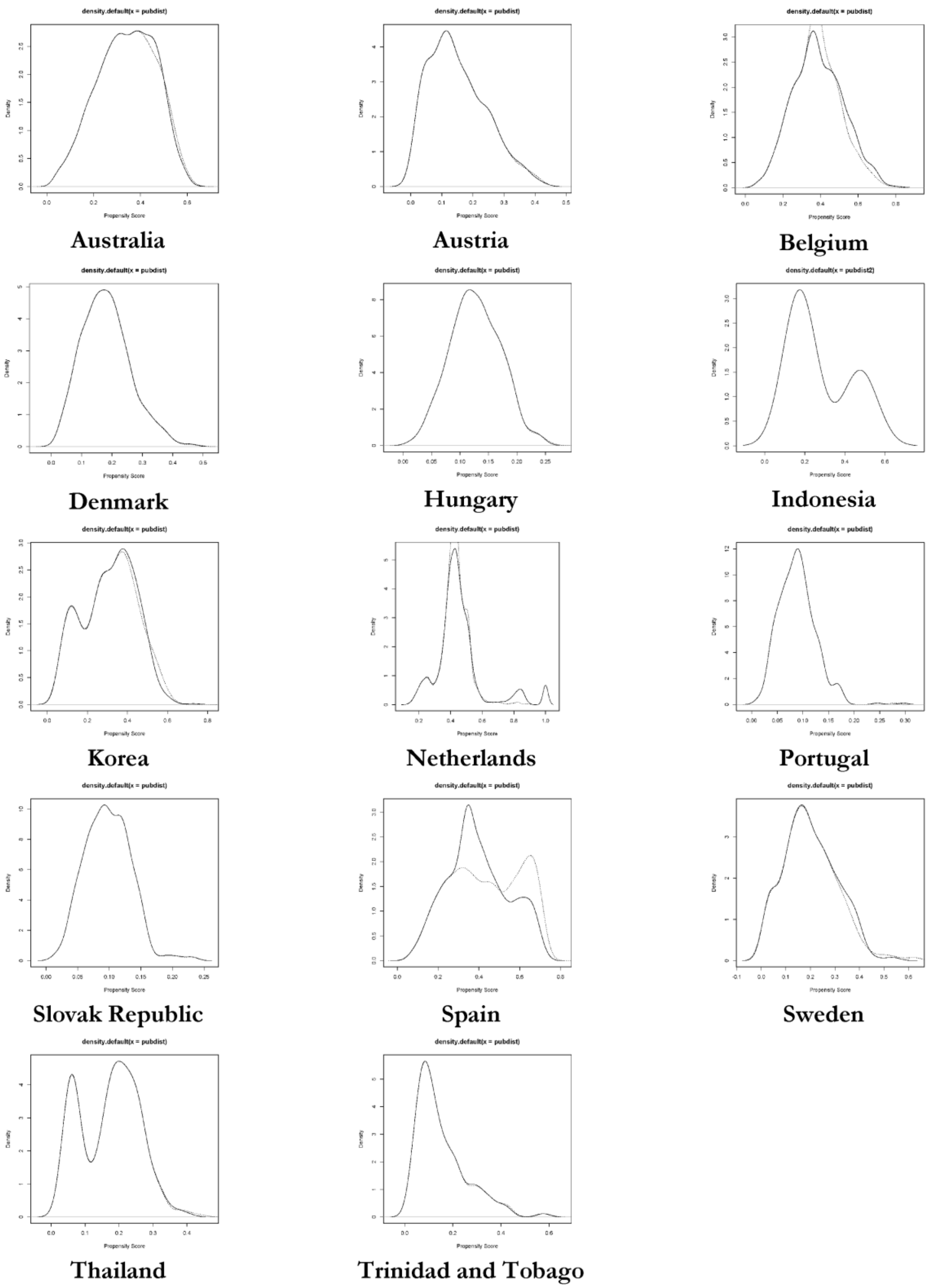

Figure 2. Common support between treatment and matched propensity scores

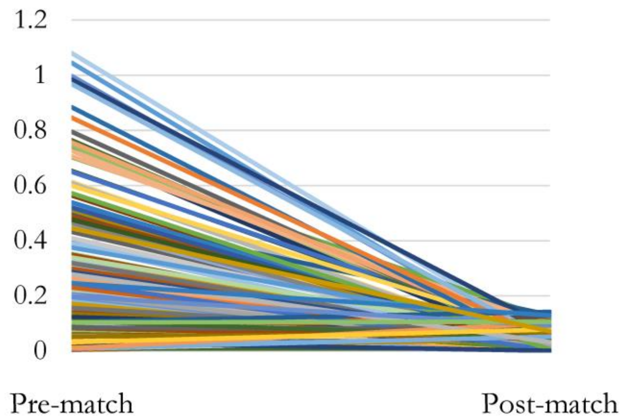

Figure 3. Pre-post changes in all covariate standardized mean differences (all countries)

Findings[3]

Linear Regression Models and Oaxaca-Blinder Decompositions

Before accounting for the selection bias between public and PPP schools, I asses the unconditional performance differences between students in these two sectors. Across countries and subjects (on a total of 51 outcomes), I find 27 positive significant[4] effects, 18 non-significant effects, and 6 negative significant effects (Table 2). That is, without adjusting for systematic differences in student populations, PPP schools outperform public schools in all PISA subjects in just over half (9) of the countries. The means of all positive significant effects are 37.4 in math (0.41 SD), 44.1 in reading (0.48 SD), and 43.4 in science (0.46 SD) PISA points. In unconditional differences, public schools outperform PPP schools in two countries: Indonesia and Trinidad & Tobago, with an average difference of 39.7 (math), 54.4 (reading), and 37.8 (science).

Averaging across countries, the unadjusted difference in pooled school sector performance is 16.1 (math), 19.4 (reading), and 20.6 (science), in favor of PPP schools. The results from the Oaxaca-Blinder decomposition (Table 2) show that, for countries with significant differences in outcomes between sectors, observed student characteristics account for between 25.3% (Chile, science) and 90.5% (Hungary, math) of the school sector differential. Averaging across those countries with significant sector differences, between 45% and 50% of the difference in achievement outcomes between sectors is attributable to observed differences in student characteristics.

Recognizing the importance of these student characteristics in explaining the achievement of public and PPP schools, my second two-level linear regression (model 2) adds to the unconditional model a set of student-level control variables, to adjust for differences in student characteristics. Table 3 shows that, after controlling for the set of student covariates (in the columns labelled "Sector Effect" under *model 2*), the number of positive significant effects decreases from 27 to 19 across countries and subjects, while the number of negative significant effects remained at 6. The means of all positive significant coefficients decreased from 37.4 to 20.9 in math, from 44.1 to 29.2

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[3] Due to the amount of data produced by running models for 17 countries and 3 exam subjects, the page limits of the paper restrict me from providing the full set of results from each statistical model. As such, the key results most pertinent to interpreting the school sector effect are presented here, and any additional results are available by request from the author.

[4] A positive effect signifies a performance advantage for students in PPP schools, while a negative effect signifies a performance advantage for students in public schools.

in reading, and from 43.4 to 32.2 in science. The means of the negative significant effects decreased from 39.7 to 32.0 in math, from 54.4 to 45.8 in reading, and from 37.8 to 17.5 in science. The mean school sector coefficient, averaged across countries, decreased from 16.1 to 8.1 in math, from 19.4 to 10.2 in reading, and from 20.6 to 10.6 in science. The results presented in the columns labelled "Oaxaca," provide the amount of the unadjusted school sector differences as explained by student population differences ('endowment') and either school effectiveness or unobservable student characteristics ('sector effect'). In summary, after controlling for observed student characteristics within a two-level linear regression model, the PPP sector advantage was reduced, with fewer significant effects and smaller coefficients. However, there remains a measurable performance gap in favor of PPP schools across a number of countries and subjects.

Propensity Score Matching

Following the matching procedure, I find further reductions in the PPP sector effect in terms of the size of the school sector coefficients and the marginal share of explained variation in PISA performance. Before matching, school sector accounts for as much as 20.9% (Chile, reading) of the variation in achievement scores across countries (Table 4). However, this effect is reduced by more than 50% across countries after propensity score matching. In Spain, school sector accounts for one-tenth the amount of variation in reading scores after matching (from 14.2% to 1.3%). Similar trends are seen in many of the countries of interest (Table 4).

The number and magnitude of significant school sector coefficients is also reduced after propensity score matching. Compared to the pre-match linear regression models with student covariates, the number of positive significant effects decreases from 19 to 13 after propensity score matching (while still controlling for student characteristics in the analytical model, Table 5). After matching, there are no longer any public school achievement advantages (down from 6 at the previous stage). The means of the positive significant coefficients increased from 20.9 to 26.8 (0.30 SD) in math, decreased from 29.2 to 26.1 (0.29 SD) in reading, and decreased from 32.2 to 28.8 (0.32 SD) in science. In only three countries—Argentina, Belgium, and Chile—do PPP students outperform matched public students on all three PISA subjects. The largest private sector advantages are found in Argentina. Findings from the Oaxaca-Blinder decomposition provide strong evidence in support of the effectiveness of the propensity score matching in balancing treatment and control groups by observable characteristics. Following the matching procedure, in only two countries—Spain and Trinidad and Tobago—do observed student characteristics explain any of the difference in achievement between public and PPP schools after matching (see 'Endowments' column in Table 5). Additionally, following the matching procedure, only in 8 country-subjects do private school productivity or unobservable characteristics provide a PPP learning advantage (see 'Sector Effect' columns in Table 5).

These results suggest that after matching students on important background characteristics, in the majority of cases (74%) there are no differences between school sectors. When differences do exist, they are more likely to favor PPP schools. These differences, however, are not large in magnitude. Averaging coefficients across countries, students in PPP schools score 3.3, 4.4, and 6.0 PISA points higher in math, reading, and science than their matched public school peers (Table 5).

Assessing the Role of Peer Effects

To determine whether performance differences between public and PPP schools may be attributable to the composition of the student's peer group rather than to any particular behavior of the school itself, I run two-level models (model 3) before and after propensity score matching, which include the full set of student-level controls and three school-level factors: school sector, share of government funding, and average student socioeconomic status.

Table 2
Unconditional PPP school effect (model 1) and Oaxaca-Blinder estimates representing the share of sector difference attributable to student observable characteristics

Country	Math		Reading		Science		**Mean**
	Sector effect	Oaxaca	Sector effect	Oaxaca	Sector effect	Oaxaca	**Coefficient**
Argentina	69.2***	0.322	43.6**	0.327	80.3***	0.311	**64.4**
	(13.0)		(14.0)		(14.5)		
Australia	25.0***	0.624	37.3***	0.560	32.0***	0.543	**31.4**
	(5.7)		(5.6)		(6.0)		
Austria	41.5**	0.308	56.0***	0.416	56.0***	0.292	**51.2**
	(13.9)		(14.6)		(14.6)		
Belgium	24.9*	0.343	30.5**	0.372	25.7*	0.347	**27.0**
	(11.1)		(10.9)		(11.7)		
Chile	40.5***	0.366	52.0***	0.328	41.4***	0.253	**44.6**
	(9.9)		(10.2)		(8.3)		
Denmark	3.5		9.2		7.7		**6.8**
	(6.4)		(6.1)		(6.7)		
Hungary	40.6*	0.905	56.2***	0.582	48.8**	0.850	**48.5**
	(17.4)		(17.3)		(16.5)		
Indonesia	-31.1***	0.356	-33.7***	0.355	-27.2***	0.310	**-30.7**
	(7.9)		(7.9)		(7.9)		
Ireland	34.9***	0.582	45.2***	0.491	39.6***	0.556	**39.9**
	(8.7)		(9.7)		(10.1)		
Korea	0.1		-0.5		-3.1		**-1.2**
	(16.6)		(13.3)		(13.8)		
Netherlands	5.3		-0.9		14.6		**6.3**
	(12.0)		(11.6)		(14.1)		
Portugal	16.2		18.7		14.6		**16.5**
	(10.6)		(10.5)		(9.7)		
Slovak Republic	7.1		12.4		11.4		**10.3**
	(14.5)		(13.1)		(15.3)		
Spain	22.7***	0.780	30.0***	0.620	22.4***	0.795	**25.0**
	(3.1)		(3.0)		(3.1)		
Sweden	36.0***	0.694	45.7***	0.620	43.8***	0.555	**41.8**
	(9.5)		(9.2)		(9.8)		
Thailand	-14.1		3.8		-9.7		**-6.7**
	(10.8)		(8.5)		(8.3)		
Trinidad & Tobago	-48.4*	0.327	-75.1**	0.299	-48.4*	0.253	**-57.3**
	(22.5)		(25.2)		(24.1)		
Mean	**16.1**	**0.509**	**19.4**	**0.451**	**20.6**	**0.459**	**18.7**

Note: Standard errors in parentheses. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

Table 3
Pre-match model 2: Linear regression models, controlling for student covariates and Oaxaca-Blinder decomposition of sector endowments and sector achievement advantage

	Math			Reading			Science		
	(2)	Oaxaca		(2)	Oaxaca		(2)	Oaxaca	
	Sector effect	Endow-ments	Sector effect	Sector effect	Endow-ments	Sector effect	Sector effect	Endow-ments	Sector effect
Argentina	56.8***	-20.5*	-40.0***	71.9***	-27.1***	-50.7***	63.1***	-23.3**	-47.0***
	(16.7)	(8.6)	(10.5)	(17.6)	(8.2)	(11.2)	(16.8)	(8.1)	(11.2)
Australia	9.4	-14.4***	-4.4	17.8*	-18.8***	-10.9*	15.1	-15.1***	-6.80
	(7.8)	(2.5)	(5.2)	(7.8)	(2.4)	(5.2)	(7.7)	(2.3)	(5.4)
Austria	32.8	-6.5	-9.1	34.4*	-18.3	-17.4	33.5	-7.2	-7.7
	(17.2)	(12.4)	(13.5)	(17.2)	(11.1)	(13.0)	(17.5)	(11.7)	(11.1)
Belgium	28.1**	-15.4***	-28.1***	29.5**	-16.2***	-24.5***	28.9**	-15.0***	-24.8***
	(10.4)	(3.8)	(4.4)	(10.1)	(3.1)	(4.3)	(10.4)	(3.6)	(4.4)
Chile	26.0*	-8.2	-8.9	28.7**	-9.7	-12.9	26.6*	-6.1	-11.4
	(11.2)	(5.5)	(7.4)	(11.1)	(5.6)	(7.1)	(10.5)	(5.1)	(6.6)
Denmark	-2.7	-7.8	-3.0	1.7	-10.6	-6.1	0.23	-13.6*	-8.3
	(6.2)	(5.5)	(7.1)	(6.4)	(5.6)	(5.6)	(7.8)	(5.9)	(7.2)
Hungary	8.0	-15.9	7.0	21.2	-8.6	3.5	17.4	-13.1	6.08
	(16.8)	(10.4)	(12.8)	(14.8)	(9.2)	(10.1)	(14.0)	(7.9)	(9.0)
Indonesia	-19.6**	17.0***	21.7**	-21.6*	17.1**	24.5**	-17.5*	14.2*	20.8**
	(7.5)	(4.5)	(7.8)	(8.7)	(6.4)	(8.8)	(7.6)	(5.8)	(8.0)
Ireland	21.6*	-15.3***	-13.7*	24.7**	-16.7***	-13.4*	24.6*	-16.8***	-14.5*
	(8.5)	(4.2)	(5.4)	(9.01)	(4.5)	(5.4)	(10.3)	(4.3)	(6.3)
Korea	4.8	14.6	2.3	1.17	7.2	1.5	-2.4	10.8	5.24
	(12.9)	(8.8)	(11.3)	(10.6)	(7.4)	(8.4)	(10.2)	(7.5)	(10.0)
Netherland	8.7	-0.56	14.2	0.37	0.35	13.9	18.7	-0.88	16.3
	(14.7)	(5.2)	(11.2)	(12.7)	(5.1)	(12.1)	(18.8)	(5.2)	(12.1)
Portugal	22.1**	0.44	-16.4	20.9**	-0.61	-20.8**	18.0*	0.53	-12.2
	(7.9)	(5.1)	(9.1)	(8.1)	(6.1)	(7.2)	(7.8)	(4.6)	(7.2)
Slovak	-5.3	-17.6	-1.06	-12.2	-25.8*	0.23	-4.91	-10.2	0.60
Republic	(14.1)	(16.0)	(13.0)	(14.6)	(12.2)	(13.1)	(14.0)	(10.7)	(11.2)
Spain	6.9	-21.2***	-4.9	11.6*	-20.8***	-9.4*	6.8	-22.0***	-6.60
	(4.4)	(3.2)	(4.1)	(4.5)	(3.7)	(4.3)	(4.7)	(3.6)	(4.2)
Sweden	17.14	-22.3*	-11.9	23.9*	-22.5	-14.8	22.1	-16.8	-11.1
	(9.7)	(10.3)	(10.1)	(9.5)	(12.6)	(8.3)	(14.0)	(12.4)	(9.9)
Thailand	-28.9*	1.05	22.2*	-10.40	4.7	15.1*	-22.9	0.71	23.1*
	(13.5)	(7.9)	(8.73)	(10.3)	(7.8)	(7.2)	(12.4)	(8.06)	(9.6)
Trinidad &	-47.5*	-16.6***	5.04	-70.1*	-15.9***	12.4*	-45.9	-14.8***	4.02
Tobago	(22.6)	(3.3)	(5.1)	(29.7)	(4.2)	(5.01)	(25.7)	(3.6)	(6.1)
Mean	**8.1**	**-8.7**	**-4.1**	**10.2**	**-10.7**	**-6.4**	**10.6**	**-8.7**	**-4.3**

Note: Standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

Accounting for peer socioeconomic composition at the school level adds substantial explanatory power to the analytic models. In model 1, school sector accounts for only 3.0%, 3.8%, and 3.3% of the variation in student math, reading, and science scores, averaged across countries, and up to 16.2% in Ireland (reading; Table 4). In most cases, however, after matching, public vs PPP school status accounts for a minimal amount of student achievement. In contrast, the peer group composition within the school explains a sizable amount of variation in student PISA outcomes. Upon addition of the aggregate SES variable to the school-level model, the amount of explained variance increases dramatically, from between 7.4% in Indonesia (reading) to 83.1% in Hungary (reading). The average share of variance explained by school peer effects, above and beyond that explained by the school sector variable is 50.5% in math, 46.5% in reading, and 46.9% in science, averaged across countries (Table 4).

Table 4
Marginal share of variation (Pseudo-R2 change) in PISA performance explained by school sector (pre- and post-match) and peer effects (post-match)

	Math			Reading			Science		
		(1)	(3)		(1)	(3)		(1)	(3)
	Pre-match (sector)	Post-match (sector)	Post-match (peer effect)	Pre-match (sector)	Post-match (sector)	Post-match (peer effect)	Pre-match (sector)	Post-match (sector)	Post-match (peer effect)
Argentina	15.9	5.5	40.7	20.6	8.3	36.3	17.1	5.6	38.3
Australia	7.5	1.6	44.5	14.7	4.4	45.2	10.5	3.2	51.1
Austria	3.5	0.2	72.9	5.8	0.1	67.0	5.8	0.1	72.4
Belgium	2.2	1.0	71.7	3.4	1.7	71.9	2.2	1.1	70.6
Chile	18.1	9.6	46.9	20.9	11.0	38.6	19.2	13.0	25.9
Denmark	0.5	2.4	41.0	2.4	0.6	44.5	1.5	1.0	31.5
Hungary	3.4	0.2	82.3	5.8	0.4	83.1	5.1	0.1	78.2
Indonesia	12.9	5.8	21.4	15.4	6.2	7.4	11.1	2.9	11.3
Ireland	15.4	13.0	26.3	19.4	16.2	11.5	15.0	15.1	11.5
Netherlands	0.1	0.2	49.9	0.1	0.8	48.2	0.7	0.2	44.0
Portugal	1.3	1.1	59.0	1.7	3.3	53.1	1.3	2.2	62.8
Slovak Republic	0.2	0.9	60.8	0.7	1.9	51.4	0.4	1.2	64.6
South Korea	0.1	0.2	65.2	0.1	0.2	73.5	0.01	0.7	75.7
Spain	8.2	0.2	29.9	14.2	1.3	32.4	8.4	0.3	33.0
Sweden	9.1	1.7	60.3	14.6	3.9	59.7	11.5	2.0	55.7
Thailand	0.9	5.2	20.3	0.2	1.1	12.8	0.8	5.6	10.7
Trinidad & Tobago	3.7	2.4	66.3	6.9	3.9	54.7	3.3	1.8	61.1
Mean	**6.1**	**3.0**	**50.5**	**8.6**	**3.8**	**46.5**	**6.7**	**3.3**	**46.9**

Note: The variation listed in column 3 denotes the explained variation added by school aggregate SES in addition to school sector. These estimates follow the approach of Bryk & Raudenbush (1992, p. 65) and Singer (1998) for estimating pseudo-R^2 in multilevel models: $(\sigma^2_{\text{Unconditional Model}} - \sigma^2_{\text{Conditional Model}}) / \sigma^2_{\text{Unconditional Model}}$.

Table 5

Post-match model 2: linear regression models, controlling for student covariates and Oaxaca-Blinder decomposition of sector endowments and sector achievement advantage

	Math			Reading			Science		
	(2)	Oaxaca		(2)	Oaxaca		(2)	Oaxaca	
	Sector effect	Endow-ments	Sector effect	Sector effect	Endow-ments	Sector effect	Sector effect	Endow-ments	Sector effect
Argentina	35.7*	1.1	-24.4*	48.4**	0.55	-31.2**	39.2*	0.64	-29.6**
	(14.8)	(7.5)	(10.2)	(16.5)	(7.1)	(10.5)	(16.5)	(7.1)	(10.8)
Australia	10.0	1.7	-4.1	17.8*	0.80	-10.5*	15.8*	1.0	-6.6
	(7.9)	(2.1)	(5.4)	(7.8)	(2.3)	(5.3)	(7.8)	(1.8)	(5.6)
Austria	-0.23	-2.1	-3.1	4.7	0.12	-12.1	-4.1	-1.7	-1.2
	(17.2)	(11.8)	(14.1)	(17.2)	(11.5)	(13.9)	(17.3)	(10.7)	(12.4)
Belgium	22.7*	1.0	-19.2***	23.9*	1.12	-17.5***	24.3*	1.02	-19.6***
	(10.3)	(3.8)	(4.8)	(9.9)	(3.5)	(4.8)	(10.3)	(3.6)	(4.7)
Chile	22.1*	1.9	3.0	21.5*	0.44	-2.2	24.4**	2.3	-4.4
	(9.3)	(6.3)	(9.6)	(8.6)	(7.1)	(8.5)	(8.9)	(5.5)	(8.0)
Denmark	-6.7	2.0	3.1	-3.7	-0.39	-0.39	-3.0	2.2	-3.1
	(6.5)	(5.4)	(8.1)	(7.2)	(6.1)	(6.9)	(8.6)	(6.5)	(8.5)
Hungary	-12.0	1.4	0.71	0.57	1.8	-1.8	-0.13	0.25	-0.04
	(18.3)	(8.1)	(13.4)	(15.9)	(7.0)	(10.8)	(14.7)	(5.8)	(10.0)
Indonesia	-14.5	2.3	15.4	-17.5	-0.27	16.8*	-10.5	1.15	13.2
	(8.2)	(3.1)	(8.3)	(8.1)	(3.1)	(8.5)	(8.1)	(3.5)	(7.6)
Ireland	29.8	-15.1	-23.1	28.1	-25.1	-24.1	37.3*	-24.9	-32.7
	(15.7)	(17.1)	(15.4)	(17.2)	(17.2)	(15.1)	(18.4)	(18.3)	(17.3)
Korea	-1.60	-2.4	5.4	-2.5	-1.31	4.3	-7.4	-1.27	8.7
	(12.5)	(6.7)	(10.6)	(9.8)	(6.7)	(8.5)	(11.2)	(6.5)	(9.9)
Netherlands	10.8	-6.1	12.6	1.7	-5.7	13.1	20.1	-6.7	14.1
	(14.9)	(4.8)	(11.2)	(12.9)	(4.7)	(12.4)	(19.1)	(5.0)	(12.1)
Portugal	16.4	-0.06	-16.8	18.9*	-1.9	-20.8*	15.8	-0.06	-12.8
	(9.1)	(5.1)	(11.5)	(9.6)	(6.0)	(10.1)	(9.5)	(4.5)	(9.4)
Slovak	-17.3	-7.3	6.2	-25.6	-7.6	9.4	-18.2	-2.7	11.5
Republic	(15.5)	(11.5)	(15.6)	(15.5)	(9.9)	(15.1)	(14.0)	(7.6)	(12.7)
Spain	1.5	-6.2*	-1.8	5.3	-5.5*	-5.7	1.9	-6.3*	-3.6
	(4.5)	(2.7)	(4.1)	(4.7)	(2.8)	(4.3)	(4.8)	(2.8)	(4.2)
Sweden	13.2	-1.2	-6.5	20.1	-2.0	-11.6	17.7	-2.2	-7.4
	(9.8)	(8.0)	(12.1)	(10.4)	(8.2)	(9.6)	(13.8)	(9.08)	(11.7)
Thailand	-19.1	-1.6	18.8*	-11.5	-3.1	14.2	-18.9	-1.3	19.6*
	(14.3)	(6.0)	(8.7)	(10.4)	(5.7)	(7.5)	(11.6)	(5.0)	(9.7)
Trinidad &	-34.6	-13.0*	-9.7	-54.4	-16.1*	-6.2	-31.7	-14.4*	-12.7
Tobago	(22.7)	(5.1)	(7.3)	(29.6)	(6.9)	(7.5)	(26.2)	(5.8)	(8.7)
Mean	**3.3**	**-3.4**	**64.9**	**4.4**	**2.1**	**62.3**	**6.0**	**-1.4**	**63.6**

Note: Standard errors in parentheses. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

Table 6
Post-match model 3: Linear regression models, controlling for student covariates, school aggregate SES, and share of government funding

	Math			Reading			Science		
	Sector effect	Peer effect	Gov't funding	Sector effect	Peer effect	Gov't funding	Sector effect	Peer effect	Gov't funding
Argentina	3.4	52.8***	-0.39	11.4	56.3***	-0.52*	3.10	56.8***	-0.35
	(14.7)	(11.1)	(0.25)	(16.1)	(9.9)	(0.24)	(16.5)	(10.2)	(0.25)
Australia	-15.7	59.4***	-0.83**	-7.2	59.9***	-0.76**	-13.1	68.3***	-0.91***
	(9.2)	(17.5)	(0.27)	(8.6)	(15.8)	(0.29)	(8.5)	(16.2)	(0.26)
Belgium	-1.3	94.4***	-0.54*	4.9	92.5***	0.10	1.73	95.7***	-0.13
	(7.8)	(6.3)	(0.22)	(8.1)	(6.4)	(0.24)	(7.7)	(6.8)	(0.22)
Chile	11.7	32.9***	-0.10	13.5	32.1***	0.14	16.3	27.9***	0.19
	(9.3)	(9.8)	(0.24)	(8.8)	(9.4)	(0.24)	(9.1)	(8.2)	(0.26)
Denmark	-20.7	29.5**	-0.27	-13.1	31.7**	-0.06	-12.2	29.9*	-0.07
	(12.9)	(10.6)	(0.56)	(14.9)	(10.6)	(0.56)	(15.3)	(11.9)	(0.61)
Hungary	-20.0*	104.8***	0.09	-8.5	95.3***	0.12	-6.2	87.3***	0.39
	(9.7)	(7.8)	(0.43)	(8.9)	(9.2)	(0.51)	(9.9)	(9.1)	(0.48)
Indonesia	-9.5	25.5**	-0.073	-13.3	21.8*	-0.27	-6.1	24.0**	-0.28
	(8.0)	(9.0)	(0.16)	(9.5)	(10.8)	(0.20)	(7.7)	(7.4)	(0.17)
Ireland	19.6	46.2	-0.29	19.0	30.4	-0.13	24.3	41.8	-0.61
	(14.5)	(25.7)	(0.91)	(16.1)	(25.6)	(0.91)	(16.9)	(31.2)	(0.99)
Korea	20.9	45.8*	-0.98***	16.4	39.9*	-0.76***	11.3	38.2	-0.78**
	(11.4)	(19.5)	(0.27)	(8.7)	(16.0)	(0.19)	(11.3)	(23.0)	(0.25)
Netherlands	8.16	92.9***	1.80	0.55	81.9***	1.67	13.2	103.***	2.11
	(10.6)	(17.0)	(1.32)	(9.8)	(16.3)	(1.2)	(13.2)	(22.6)	(1.81)
Portugal	21.2*	35.3***	-0.11	24.0*	40.6***	-0.13	19.7*	34.0***	-0.07
	(8.6)	(7.7)	(0.24)	(9.4)	(8.1)	(0.23)	(8.4)	(7.3)	(0.21)
Slovak Republic	-5.79	107.0***	0.55	-13.7	107.9***	0.39	-7.6	99.2***	0.55
	(10.4)	(15.8)	(0.64)	(9.1)	(12.8)	(0.51)	(10.2)	(13.4)	(0.56)
Spain	-4.90	21.4***	-0.00	0.27	22.2***	0.10	-4.1	22.7***	0.038
	(5.6)	(5.8)	(0.19)	(5.1)	(5.3)	(0.19)	(5.4)	(5.3)	(0.17)
Sweden	1.69	32.3	-2.68*	7.7	31.1	-3.22*	2.0	36.9	-4.36**
	(11.2)	(16.5)	(1.35)	(12.8)	(20.1)	(1.52)	(16.0)	(24.0)	(1.55)
Thailand	-31.0**	14.2	-0.57	-19.8	15.0	-0.26	-28.0*	11.1	-0.56
	(11.1)	(11.9)	(0.31)	(10.3)	(8.6)	(0.28)	(11.2)	(9.09)	(0.35)
Trinidad & Tobago	-21.8	147.3***	-0.08	-34.9	168.2***	0.32	-22.0	152.7***	-0.23
	(17.5)	(15.8)	(0.42)	(23.3)	(17.4)	(0.50)	(21.2)	(17.6)	(0.49)
Mean	**-2.7**	**58.9**	**-0.28**	**-0.78**	**57.9**	**-0.20**	**-0.48**	**58.1**	**-0.31**

Note: Standard errors in parentheses. *** p<0.001, ** p<0.01, * p<0.05

While adding levels of explanatory power to the variation in student achievement, the peer effect variable also substantially reduces the PPP achievement advantage found previously (Table 4). The prior models, which (after propensity score matching) regressed student outcomes on school sector while controlling for student characteristics, resulted in 13 significant effects across countries and subjects in favor of PPP schools. After matching and controlling for student characteristics, as well as accounting for the school level factors of government funding and school aggregate SES, private school achievement advantages are only found in a single country—Portugal. In Portugal, students in PPP schools outperform those in public schools by 21.2 (0.24 SD), 24.0 (0.27 SD), and 19.7 (0.23 SD) points (Table 6). In no other country is even a single PPP performance advantage found after accounting for sector differences in student characteristics and the composition of peers in the school. Similarly, this model produces 3 significant results demonstrating a public school achievement advantage. In Hungary public math students perform 20.0 points (0.23 SD) higher than their equivalent peers in PPP schools. In Thailand there is a significant public school advantage in math (31.0 points; 0.34 SDs) and science (28.0 points; 0.31 SDs).

These findings suggest that, after accounting for differences in student characteristics between school sectors, a large share of the remaining PPP achievement advantage is the result of peer group composition rather than any productive efficiency inherent to the sector. The implication is that, in the majority of countries, students with similar qualities, and in similar peer-group environments, experience no achievement advantage in either the public or government-funded private school sector. The effects of accounting for systematic differences in student and peer-group characteristics are demonstrated in Figure 4 and Figure 5, which show the change in the school sector coefficients (displayed as 95% confidence intervals) between the unconditional school sector model and the post-match model accounting for student characteristics, level of government funding, and school average SES.

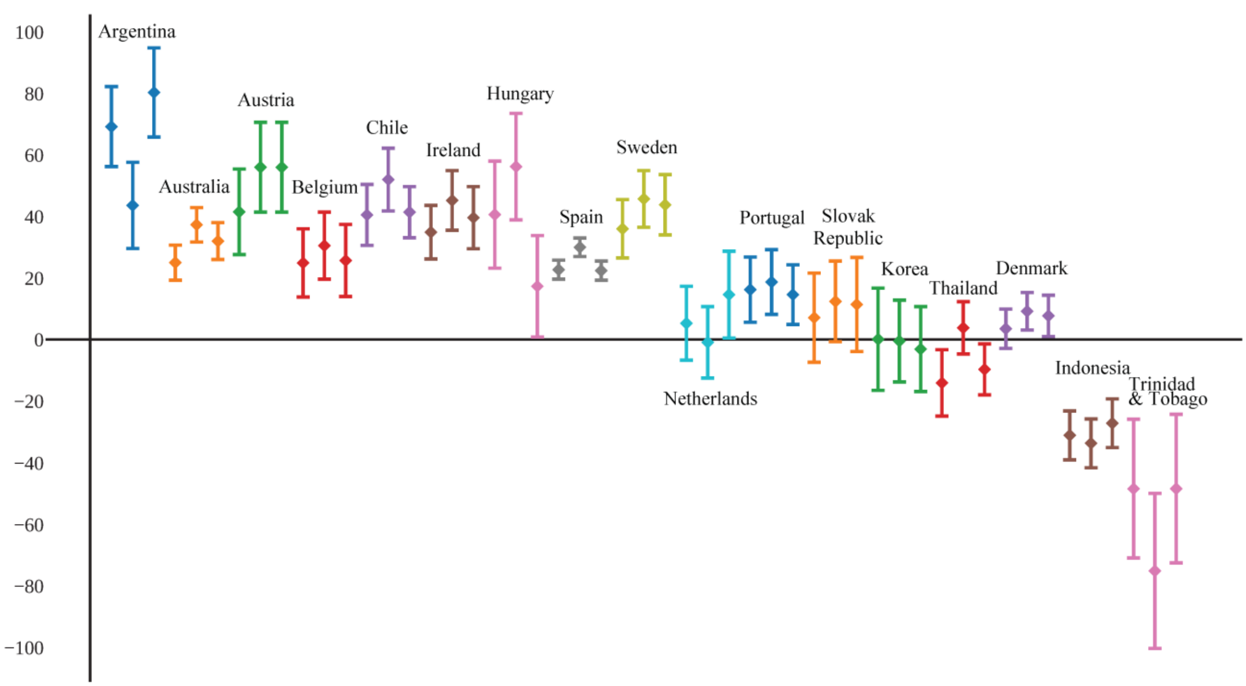

Figure 4. Unconditional PPP school effect (Model 1) 95% confidence intervals (in PISA points)

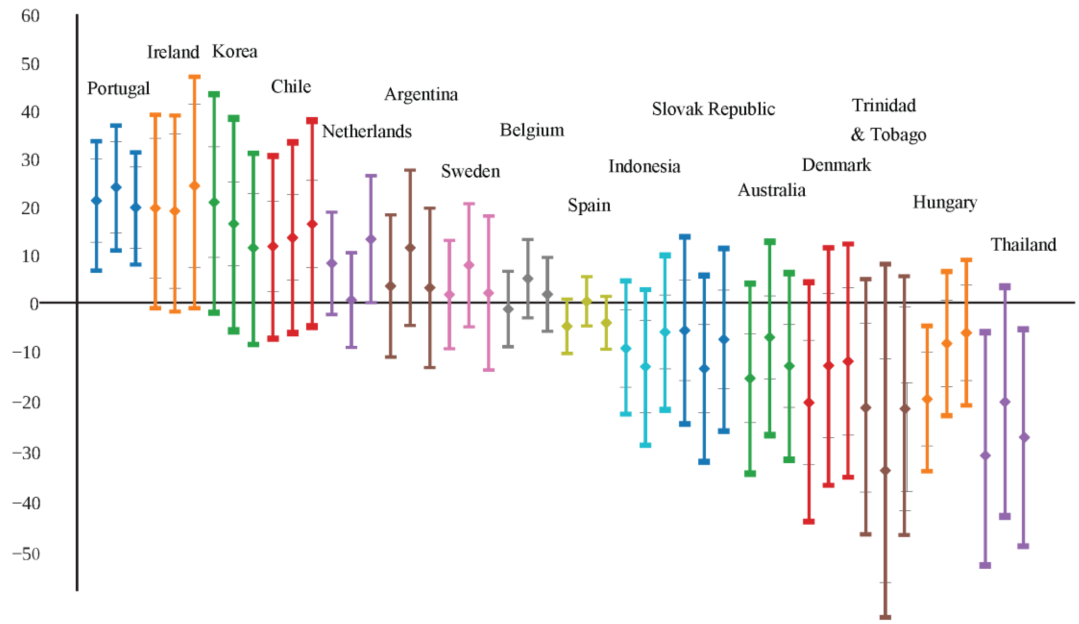

Figure 5. Post-match sector effect (Model 3) 95% confidence intervals (in PISA points), controlling for school mean SES and level of government funding

Limitations

The study faces a few limitations. First, the primary variable of interest—school sector—does not provide information sufficient to distinguish between different types of PPP models. It is not apparent from the data whether PPP schools receive funding through vouchers, government subsidies, or whether they might be classified as charter-type or privately contracted schools. As a result, the data only provide the opportunity to assess a generalized view of public-private partnership against the traditional school that is publicly-funded and publicly-managed. Future studies utilizing more detailed data about PPP policies could substantially improve the current knowledge of privatization strategies by distinguishing the performance effects of specific types of educational partnerships.

Second, it cannot be assumed that balance between observable covariates can be equated with balance between unobservable covariates that may impact assignment to treatment status (Rubin, 1997). Propensity score models are only able to reduce bias insofar as covariates that accurately predict the outcome are balanced across treatment and control groups. However, I have shown through the empirical research above that, given adequate overlap in the distribution of propensity scores, closely matched groups can significantly reduce the selection bias and thus produce more accurate estimates of treatment effects than some other techniques (Bai, 2011; Caliendo & Kopeinig, 2008; Cochran, 1968; Rosenbaum & Rubin, 1983). At the very least, results from the Oaxaca-Blinder decompositions provide evidence that a sizable portion of the school sector achievement gap is due to the selected set of observed student characteristics (just under 50% across countries and subjects where a sector differential exists); as such, there is some level of confidence that at least this portion of the selection bias has been reduced. Recognizing that the findings may not represent perfect estimates of the treatment effect, I nevertheless move forward under the assumption that the results still offer a policy-relevant perspective on the influence of PPP schools on student performance.

Lastly, one of the primary advantages of using data from the PISA assessment is that large-scale trends can be assessed across countries. Unfortunately, however, this also makes it difficult to consider in detail any of the individual country contexts, to understand the makeup and functioning of these education public-private partnerships. I intend for this study to offer a starting point, upon which future research can build, to uncover more about the individual PPP models and their operation within each of the included countries.

Discussion and Conclusion

This study has provided an examination into the effectiveness of education public-private partnerships for producing positive student learning outcomes. Despite the existing economic theories, which predict that growth in market involvement will lead to gains in school productivity, the findings reinforce the existing mixed evidence on the productive efficiency of education PPPs globally. There is no consistent cross-country effect with respect to PPP sector performance. In terms of unconditional performance differences, students in PPP schools outperform public students in 53% of PISA outcomes across countries. However, a sizable amount of these performance differences appear to be attributable to positive student selection into PPP schools. After accounting for selection bias through propensity score matching, school sector accounts for only 3.4% of the variation in PISA performance. Moreover, after matching, the cross-country average PPP coefficient decreased from 18 points (.2 standard deviations) to 4 points (.04 standard deviations). Although after matching there remains a PPP performance advantage in 25% of outcomes across all countries and subjects, nearly all of these performance differences are accounted for by school-level peer group effects. In the majority of cases, after accounting for peer effects, students perform as well in public as in PPP schools.

These findings contribute towards an ongoing discussion of the overall effectiveness of school choice and market-based education interventions—that is, whether they increase school productivity or simply redistribute educational ability (Bettinger et al., 2010; Epple, Romano, & Urquiola, 2015; Hsieh & Urquiola, 2006; Uribe et al., 2006). From the productivity perspective, school choice and competition are the tide that lifts all boats, raising student performance in both the public and private sectors by incentivizing improvements in school innovation, effort, and operation. From the redistribution perspective, school choice simply shifts educational talent from one set of schools to another, by sorting the most capable students into the most desirable institutions. This matters for public policy, because if PPP schools are only more effective due to the fact that they systematically select or attract more talented students, then policies aiming to increase PPP enrollments will experience diminished effectiveness (Uribe et al., 2006). Such sorting would benefit the privileged at the expense of the disadvantaged, and would create negative externalities for the larger education system. In this environment, school choice and competition would offer little in the way of additive value to the overall improvement of school systems.

The findings presented above offer evidence in support of the theory of redistribution. In the sample of 17 PISA countries, PPP schools appear to be outperforming public schools not through any superior or innovative practices, but rather by cream-skimming more capable students into the private sector. Enrollment in a PPP school is tied to powerful socioeconomic indicators such as student wealth and prior academic ability. This distribution of students is reflected in the unconditional performance differences between PPP and public school students. However, PPP schools do not appear to be adding any marginal value to their students, beyond what they would receive in the public school system. In short, PPP schools are not improving the productive efficiency of most educational systems; and in a majority of countries, they are reinforcing social disparities, by disproportionately serving students in the upper income quintiles.

These findings have direct implications for education policy globally. In much of the existing literature, the inclusion of PPPs within education policy frameworks is justified by their potential to improve productive efficiency in the education system without negatively impacting social efficiency. The findings from this research urge caution against the expectations for superior performance within education PPP models. Moreover, governments with policy interest in such interventions should be cognizant of the potential impacts of such programs on less-socially-privileged populations. For example, Chile's national voucher system had significantly negative effects on the ability for the poorest households to access high-quality educational services (Carnoy, 1998; Verger, Bonal, & Zancajo, 2016) until a 2008 revision to the policy increased the funding allotment for the poorest students, and in turn, significantly reduced gaps of enrollment and performance for the poorest students (Neilson, 2013). Education PPP programs that are most likely to increase both social equity and productive efficiency are those which target specific student populations (typically based on need). For policy makers, this suggests that governments ought to carefully design education PPP programs to be particularly sensitive to the needs of the most vulnerable.

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