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How Effective Are Private Schools in Latin America?

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Abstract – Using multilevel modelling and data from 10 Latin American countries, this paper provides new evidence on the relative effectiveness of public and private schools. There are substantial differences in the achievement of private and public schools, usually around one-half a standard deviation. A small portion of these differences is accounted for by the higher socioeconomic status of students in private schools. A quite substantial portion is explained by the varying peer group characteristics in private and public schools. After accounting for peer characteristics, the average private school effect across all 10 countries is zero, though with some variance around this mean (the effects range between -0.2 and 0.2 standard deviations). Evidence on selection bias is inconclusive, but the paper argues that these effects may constitute an upper bound to the true effects.

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1 Introduction

Nearly a half century ago, Friedman (1955) argued that parents should receive tuition coupons, or vouchers, allowing them to send their children to private schools rather than public schools. Throughout the 1990s, his arguments were vigorously restated by scholars in the United States (Chubb & Moe, 1990; Hoxby, 1998; Peterson & Hassel, 1998). Other authors have issued similar calls for vouchers in low-income countries (West, 1997; Patrinos, 2000).

Voucher plans can vary widely in their scope and design.[1] However, they rest on a common supposition: that private schools are relatively more effective than public schools at improving student outcomes. Relative effectiveness—which has also been called the "private school effect"—is defined here as the difference between public and private school outcomes, net of students' socioeconomic status and other factors pertaining to their family background. In other words, privatization schemes assume that private schools produce greater amounts of desirable outcomes, regardless of the background of their students.[2]

Before pursuing these policies, it is reasonable to inquire whether this assumption is empirically supported.[3] While much ink has been spilled over the U.S. case, there is considerably less empirical evidence from low- and middle-income countries—notwithstanding the claims made by supporters and opponents of privatization. This is unfortunate, because

[1]For example, vouchers may be offered to a restricted number of students, based on criteria like family income or residence, or they may be offered to every student attending a public school. See Levin (1991) for a discussion.

[2]The effect of vouchers on students who use them to transfer from public to private schools has overwhelmingly consumed the attention of politicians and researchers. It bears emphasis, however, that vouchers could have a multitude of effects on students and schools—and that a full evaluation of vouchers must address them all. It is commonly argued that the exodus of students from public schools will provide incentives for them to improve, thus improving the outcomes of students who do not use vouchers. However, critics of vouchers are concerned that vouchers will lead more privileged or able students to leave public schools. If student outcomes are affected by the characteristics of their peers, this sorting could affect student outcomes. For a discussion of these mechanisms, see McEwan (2000).

[3]For reviews of the U.S. literature, see Levin (1998), McEwan (2000), Neal (1998), and Witte (1996).

Latin America provides a fascinating institutional context in which to compare the relative effectiveness of private and public schooling. Several countries have already experimented with public funding for private schools. For example, both Argentina and Chile provide extensive subsidies to most private schools (Carnoy, 1998; McEwan & Carnoy, 2000; Morduchowicz et al., 1999). Less ambitiously, Colombia provided a limited number of secondary school vouchers to students who lived in poor neighborhoods in the 1990s (Angrist, Bettinger, Bloom, King, & Kremer, 2001).

In 1997, the Santiago office of UNESCO implemented an assessment of student achievement in Latin America, working in collaboration with 13 Ministries of Education. Using a common sampling methodology and survey instruments, researchers in each country collected representative samples of data on third- and fourth-grade achievement in language and mathematics, as well as background surveys from students, parents, teachers, and principals.

This paper uses these data and multi-level modelling to assess the relative effectiveness of private and public schools in 10 of these countries. In particular, the paper argues that many prior studies have misrepresented the private school effect, by failing to control for the characteristics of student peer groups. In these studies, the achievement gap between the two sectors may partly or entirely reflect the effects of "better" peer group characteristics, as opposed to any substantive impact of private school practices or efficiency on the outcomes of their students.[4] The results suggest that conditioning on a complete set of student, family, and peer characteristics explains a large portion of the observed difference in achievement between public and private schools. Across the 10 countries considered in this paper, the mean private school effect is approximately zero, ranging between -0.2 and 0.2 standard deviations.

[4]This is conceptually and empirically distinct from the issue of selection bias, in which private school effects are biased by omitted student and family variables. We also consider the direction and magnitude of this bias.

The following section describes the institutional diversity of private schooling in Latin America. Section 3 provides a review of empirical research on the relative effectiveness of public and private schools in the region. Section 4 discusses the analytical strategy of this paper, while Section 5 describes the main features of our data set. Section 6 discusses the general patterns to emerge from the multilevel findings. The robustness and validity of these findings is evaluated in Section 7. Section 8 is a discussion of the usefulness of our findings to voucher policy, while Section 9 summarizes and concludes.

2 Private Schooling in Latin America

There are a wide range of institutional settings across Latin American countries that may encourage or discourage private school enrollments. Table 1 reports the percentage of private enrollments in primary and secondary schools, focusing on the 10 countries in this study. Two patterns are immediately evident. First, there is enormous variation across countries in private enrollment shares. Private enrollments at the primary level range from 6% in Mexico to over 40% in Chile (by way of comparison, around 11% of U.S. students are enrolled in private schools). Second, the private sector generally plays a larger role at the secondary level.

The exact determinants of this variation—either across or within countries—have been subjected to little empirical inquiry, and are beyond the scope of this study.[5] However, a simple economic framework suggests three categories of explanations: demand, supply, and the government policies that can affect them.[6] The following discussion will review each category, providing some specific examples.

[5]For one exception—an analysis of Brazilian private education—see James, Primo Braga, and Andre (1996).

[6]See James (1993) and James et al. (1996) for a full discussion; the following discussion relies heavily on their arguments.

2.1 Demand and Supply

The market share of private schools may increase for several reasons. The first is the existence of excess demand for public education that cannot be met. This is particularly germane to Latin American secondary education, where some public sectors cannot absorb every student. Alternatively, demand may increase if differentiated tastes for educational content—perhaps stemming from diverse religious backgrounds—lead individuals to exit the public sector in search of a better educational "fit" for their child. Most Latin American countries are predominantly Catholic, perhaps diminishing the validity of this explanation. However, there are burgeoning Protestant movements in Latin America that may increase the demand for schools with a Protestant emphasis.[7] Lastly, differential preferences for school quality may stimulate demand for private education. In particular, a low-quality public sector may induce parents to switch their children from a public to a private school, even if it involves some financial burden.

Supply factors also influence private school market share. Most private schools are established as non-profit, and these are often operated by religious organizations.[8] Where religious organizations like the Catholic Church are particularly numerous or active, the supply of non-profit schooling may be larger. This explanation is pertinent to most Latin American countries. On the other hand, there is a relative scarcity of Catholic clergy in certain areas of the region, particularly in Mexico and Bolivia (Gill, 1998). This may tend to increase the costs—and reduce the supply—of non-profit schools that cannot pay subsidized wages to religious personnel.

[7]See Gill (1998) for a general account of the growth of Protestantism in Latin America. In Chile, one of the first countries with a sizable Protestant population, about 1.5% of primary students are enrolled in publicly-funded schools managed by a range of Protestant churches (McEwan & Carnoy, 2000).

[8]There are notable exceptions, such as Chile and Brazil, where for-profit schools can operate (James et al., 1996; McEwan & Carnoy, 2000).

2.2 Government Policies

A range of government policies stand to affect demand and supply, and consequently the private market share. First, the quality and content of public education may have an impact on the demand for private schools. Second, the degree of private school regulation can affect the desirability of operating a private school, and hence the supply. One prominent example is the tuition level set by private schools, which is regulated in several countries. For example, Brazilian law places restrictions on the tuition that can be charged (James et al., 1996). Colombia also regulates private school tuition, explicitly linking it to inflation (Vergara, Davila, Jimenez, Laverde, & Simpson, 2001). Until 1993, Chilean private schools that charged tuition were not eligible to receive government subsidies, but this provision has been softened during the last decade.

Third, a wide range of financial incentives may be used to increase demand or supply. In Latin America, there are several salient examples. In 1980, Chile began providing public and most private schools with equivalent per-student subsidies.[9] Soon thereafter, the share of private enrollment increased dramatically, mainly in non-religious, for-profit schools. Between 1992 and 1997, Colombia offered over 125,000 publicly-funded vouchers for attendance at private secondary schools (Angrist et al., 2001; Carnoy & McEwan, 2001). The vouchers, initially large enough to cover tuition at low-to-middle cost private schools, were targeted towards families living in poor neighborhoods.

Since the 1940's, Argentina's government has provided financial subsidies for the payment of teacher salaries in private schools (Morduchowicz et al., 1999). Schools that do not charge tuition can receive a 100% salary subsidy, and lesser subsidies are available to schools charging tuition. However, Morduchowicz et al. (1999) maintain that these provisions are inconsistently applied. In light of the substantial subsidies in Argentina and Chile, it is

[9]For a detailed description, see Carnoy (1998) and McEwan and Carnoy (2000).

perhaps not surprising that they have relatively large private school enrollment shares.

Fe y Alegría, a non-governmental organization that began in Venezuela in 1965, constitutes another type of subsidization scheme. Affiliated with the Catholic Church, it operates schools in poor communities of several Latin American countries (Latorre & Swope, 1999). By agreement with each government, teacher salaries are publicly-funded, while other costs are covered by communities and the private sector. In 1995, 514 *Fe y Alegría* schools served over 200,000 students. In Bolivia, they account for 3% of primary enrollments, and around 1% in Venezuela. There are also a number of these schools in Colombia, Ecuador, Guatemala, Peru, Nicaragua, and Paraguay.

Finally, tax laws have provided financial incentives to both the demand- and supply-sides of the private school market. In Brazil, for example, families used to receive a federal income tax allowance for educational expenses, including tuition and transportation costs, although this was eliminated in a 1989 reform (James et al., 1996). Despite Uruguay's notability for not providing monetary subsidies to private schools, it does provide tax exemptions to private schools. These tax exemption schemes also exist in other countries.

In sum, numerous demand and supply factors affect the market share of private schools. To the extent that vouchers and other financial incentives lead to increases in demand or supply, government policy contributes to the privatization of schooling. Ultimately, the success or failure of such policies can only be judged by assessing whether student outcomes are improved. An important component of this judgement is the relative effectiveness of public and private schooling.

3 Research on Private School Effectiveness in Latin America

Table 2 summarizes the extant literature on private school effectiveness in Latin America.[10] At least two questions should frame an evaluation of this literature. First, are the estimates of private school effects unbiased? Second, are the standard errors of the estimates correct, allowing us to infer that private school effects are different from zero? The following sections will search for answers to these questions, in addition to informing this paper's empirical analysis.

3.1 Estimates of Private School Effects

Private school effects are unbiased if they perfectly control for non-school variables that predict both achievement and private school attendance. These variables can be divided into two categories: student and family variables, and peer variables.

3.1.1 Student and family variables

In comparing the mean achievement of students in public and private schools, each study in Table 2 controls for differences in students' family background. However, it is possible that some individual determinants of achievement are omitted. If these are correlated with the probability of attending a private school, then estimates of private school effects are afflicted by selection bias.

One remedy, pursued in a handful of studies, is the implementation of two-step statistical corrections (Heckman, 1979; Maddala, 1983). In the first step, researchers specify a probit

[10] There is also a burgeoning literature in African and Asian countries that is not addressed here (Bedi & Garg, 2000; Kingdon, 1996; Lassibille & Tan, 2001). For a thorough review of the international literature that precedes these studies, see Riddell (1993).

regression, where the dependent variable is private school attendance. This regression is used to construct a selectivity variable, which is included in the second-step regression that explains achievement. For this "corrected" equation to be identified, however, the private school attendance model must contain at least one variable which is not included in the achievement model.[11] The excluded variable(s) must be correlated with private school attendance but uncorrelated with the error term of the achievement model. Identifying and measuring variables that meet these criteria can be trying, especially since choosing an invalid exclusion may fail to correct the bias (and may even bias results further due to the improper omission of variables from the achievement regression).

Several studies implement corrections, although they make very different choices about exclusion restrictions. Studies of Bolivia and Colombia both use measures of socioeconomic status in the private school attendance model, but these also clearly belong in the achievement model (Mizala & Romaguera, 2000; Cox & Jimenez, 1991). In each case, it is doubtful whether the "corrections" ameliorate bias, and they may actually exacerbate it.

One study of the Dominican Republic makes a more compelling case for its exclusion restriction, using private school tuition levels, but it also finds mixed evidence. An "elite" category of private schools ("F-type") has negative effects on achievement, while another has positive effects (Jimenez, Lockheed, Luna, & Paqueo, 1991). In a recent study of Chilean schools, McEwan (2001a) assumes that the local availability of private schools will affect choice, but not achievement.[12] Overall, the results suggest that the largest category of private schools—subsidized schools operated by non-religious organizations—produce results similar to public schools. In contrast, other private schools have positive effects relative to public schools (including Catholic subsidized schools and non-subsidized, fee-paying schools). This

[11] If an exclusion is not imposed, one must rely on the non-linearity of the parameters for identification, which may or may not be appropriate.

[12] A similar exclusion restriction has occasionally been employed in the U.S. literature (Neal, 1997; Grogger & Neal, 2000).

is roughly consistent with other Chilean evidence, using different data and not correcting for selection bias (McEwan & Carnoy, 2000; Mizala & Romaguera, 2000).

Perhaps the best attempt to deal with bias is in Angrist et al. (2001). To estimate the effects of offering vouchers to secondary students in Colombia, they rely upon the fact that students were randomly awarded or denied vouchers. This randomization ensures that each group of students is roughly similar, obviating the need for selection bias corrections. The effect on students who actually attended private schools—because not every student used their voucher—was 0.29.[13]

Non-experimental studies in other countries yield a mixed bag of results. For example, some types of private schools in Argentina are more effective than public schools, and some less (McEwan, 2001c). Brazilian private schools appear more effective in raising Portuguese achievement, but no more effective in raising math scores (Lockheed & Bruns, 1990).

3.1.2 Peer variables

The private school effect is typically defined in the literature as the achievement difference between public and private schools, net of student ability, socioeconomic status, and other family background characteristics. In addition to their own background, however, students' outcomes may be affected by the characteristics of their peers. There is a large empirical literature suggesting that "good" peer group characteristics—such as the mean socioeconomic status of a school—are associated with higher achievement, *ceteris paribus*.[14]

[13] In a larger sample, they find a range of positive effects on other outcomes which are not addressed in this paper.

[14] For reviews of the literature, see Jencks and Mayer (1990), Moffitt (2001), and McEwan (2000). Like comparisons of public and private achievement, this literature faces challenges related to selection bias. While a substantial number of studies find positive peer-group effects (Henderson, Mieszkowski, & Sauvageau, 1978; Willms, 1986) there are concerns that positive peer-group effects may stem from the sorting behavior of families. More specifically, it is possible that peer-group variables are, in part, spuriously reflecting unmeasured characteristics of more privileged families that have chosen schools with "good" peers (Evans, Oates, & Schwab, 1992). In the Chilean context, some evidence indicates that such biases are not severe, but there is rather little evidence on this point (McEwan, 2001b).

In Latin America, private schools tend to have a higher concentration of high-SES students than public schools. Consequently, if one defines the private school effect as the achievement difference between public and private schools net of peer group characteristics, then "typical" private school effects are probably biased: instead of reflecting school-based differences between private and public schools—related to resource levels, school practices or efficiency of resource use —private school effects will partly reflect the more privileged status of peer groups.

Raudenbush and Willms (1995) label the typical definition—excluding controls for peer characteristics—the "Type A" effect, and the second the "Type B" effect. They argue that the appropriate definition of the school effect depends on the person or organization that will make use of the information. The Type A effect is most relevant to parents: they will want to send their child to the school with the largest Type A effect, regardless of whether this effect arises from school practices or its favorable peer groups. On the other hand, the Type B effect—which is meant to isolate the effect of school practices, resources, and efficiency of resource use—is most relevant to policymakers and school officials when evaluating the performance of schools.

Some studies in Table 2 control for peer characteristics, but others control exclusively for individual characteristics (Psacharopoulos, 1987; Angrist et al., 2001; Cox & Jimenez, 1991; Mizala, Romaguera, & Reinaga, 1999).[15] Note that randomized assignment does not control for peer-group characteristics. Angrist et al. (2001), for example, analyze an experiment in which students were randomly awarded or denied vouchers. Although randomization is a convincing means of controlling for student and family characteristics, the estimates clearly do not condition on the different peer groups to which voucher and non-voucher students are

[15] Another study estimates models with and without peer variables (Jimenez et al., 1991). However, it emphasizes results from the simpler models when drawing policy conclusions.

exposed.[16] Hence, the estimates are inclusive of school and peer influences on achievement.

3.2 Standard Errors of Estimates

To determine whether an estimate derived from a sample is *statistically* different from zero, it is necessary to calculate its standard error. It has been exhaustively noted in the educational and economic literature that the ordinary least-squares formulae for standard errors are incorrect in the presence of clustering of students within schools, classrooms, households, or other units.[17] In most cases, the standard errors will be underestimated, leading to unwarranted findings of statistical significance.

There are two techniques which correct for bias in standard errors. The first is multilevel modelling, which is increasingly used in educational research, but which is applied in only one of the studies in Table 2 (Lockheed & Bruns, 1990). Second, it is possible to adjust standard errors for clustering within the context of ordinary least-squares regression.[18] McEwan (2001a, 2001c) uses this approach in studies of Argentina and Chile. Moreover, several other studies, by virtue of using data available only at the school-level, or data which are not subject to extensive clustering, are spared the necessity of making such corrections (Mizala & Romaguera, 2000; McEwan & Carnoy, 2000; Angrist et al., 2001).

Several authors, however, do not make corrections for clustering, thus casting some doubt on their inferences, quite apart from issues of bias in estimates of private school effects (Cox & Jimenez, 1991; Mizala et al., 1999; Psacharopoulos, 1987).[19]

This paper will employ several strategies to resolve the empirical concerns raised in this

[16]This also applies to recent randomized evaluations of U.S. voucher programs (Myers, Peterson, Mayer, Chou, & Howell, 2000).

[17]For lucid explanations, see Deaton (1997) and Angeles and Mroz (2001).

[18]For details, see Rogers (1993), who generalizes from the robust standard error calculation of Huber (1967).

[19]At least one study corrects for heteroscedasticity, but this is not equivalent to correcting for clustering (Cox & Jimenez, 1991).

section. First, it will make detailed controls for both individual and peer characteristics. Second, it will assess the direction and magnitude of selection bias, which is particularly important given the non-experimental nature of our data set. Third, it will correct standard errors of estimates within the framework of multilevel modelling.

4 Empirical Strategy

This section describes the empirical strategy—multilevel modelling—which is used to gauge the contribution of private schools to academic achievement. It further describes a simple approach to meta-analysis that is used to estimate the region-wide effect of private schools.

4.1 Multilevel Modelling

Many types of data—and in particular educational data—have a nested or hierarchical structure. In the present analysis, students are nested within schools. Conceptually this implies that the outcomes of students within a given school are not independent of each other, since these students are all influenced to some extent by the same school-level factors.

Taking into account the multilevel nature of organizational settings is important for two reasons, the first practical and the second technical. When individuals function within an hierarchical structure, their outcomes can be targeted either through policies which directly affect their personal attributes, or indirectly via reforms of the characteristics and processes of the hierarchy. From a practical stand-point, therefore, multilevel structures are often subject to a "level of analysis" problem. In the case of educational policy, student outcomes are typically the variable of interest, yet because of the difficulty of targeting students' familial circumstances—not to mention concerns about cost-effectiveness—schooling reforms are often implemented at the school or classroom level. In such a situation, a knowledge of the average effect of the reforms across all students is no longer sufficient: researchers and policymakers must also understand the within-school and between-school effects of the proposed changes on students' outcomes, which requires that the hierarchical structure of the school system be taken into account (Raudenbush & Willms, 1991).

Disregarding multilevel structures is also technically incorrect. Because student outcomes are correlated within schools, the standard OLS assumption of independent observations is violated (Bryk & Raudenbush, 1992). As such, OLS regression overlooks the extra random component that is introduced into the standard errors by the fact that schools are clustered units. For example, the difference in expected achievement between two students will be greater if they come from different schools than if they come from the same school. OLS consequently yields standard errors of the estimates that are misleadingly small, which decreases the power of hypothesis tests (Paterson, 1991). For both the practical and technical reasons just discussed, a new class of models—called multilevel or hierarchical models—was developed to take advantage of the information made available by nested structures.[20]

Due to the hierarchical nature of our data set, the present analysis will be specified as a two-level model, with students i at the first level and schools j at the second. The first level can be expressed as follows:

$$\mathbf{Y_{ij}} = \beta_{0j} + \mathbf{X_{ij}}\beta_j + \varepsilon_{ij} \tag{1}$$

where $\mathbf{Y}$ is academic achievement, $\mathbf{X}$ is a set of student-level regressors, and $\varepsilon \sim N(0, \sigma^2)$ where σ^2 is the within-school variation.

This model is estimated across students for each school j; consequently, β_{0j} is a vector of j intercepts, and β_j contains the estimated coefficients of $\mathbf{X}$ for each school j. In an

[20]For an in-depth treatment, see Goldstein (1995), Bryk and Raudenbush (1992), Kreft and De Leeuw (1998), Snijders and Bosker (1999).

educational context, the intercepts in β_{0j} gauge the effectiveness of each school, while the slopes in β_j are measures of different types of equity within schools. For the purposes of this paper, we will model the variation in the former but not the latter.[21] Our school-level model can therefore be expressed as:

$$\beta_{0j} = \psi_0 + \mathbf{Z_j}\psi + u_0 \tag{2}$$

$$\beta_j = \phi_0 \tag{3}$$

where $\mathbf{Z}$ are school-level variables, and $u_0 \sim N(0, \tau_0)$.[22] ψ_0 is the Bayesian "grand mean" of achievement: it is constructed by weighting each school's β_0 by how reliably it has been estimated, where reliability is inversely proportional to σ^2 and τ_0, and directly proportional to the number of schools.

Substituting equation (2) and (3) into (1) yields:

$$\mathbf{Y_{ij}} = \psi_0 + \mathbf{X_{ij}}\phi_0 + \mathbf{Z_j}\psi + \varepsilon_{ij} + u_0 \tag{4}$$

This is the most general formulation of our analytical approach. More specifically, our analysis consists of three different models—all variants of equation (4)—estimated for each country. Model I regresses academic achievement on *PRIVATE*, a dichotomous $\mathbf{Z}$ variable

[21] Our model specification falls under the category of a "random intercept" model. Other possible specifications are the "slopes-as-outcomes" model, which regresses the slopes β_j on school-level factors, and the "random coefficient" (RC) model, in which slopes are not modelled but are allowed to vary randomly across schools. Ideally, our model should have been constructed as an RC model, since fixing a slope when it should be random can bias all estimates in the model (Snijders & Bosker, 1999). However, when within-school sample sizes are small, as is the case with our data, slope estimates become unstable (Kreft & De Leeuw, 1998), and it is therefore more difficult to judge whether a slope varies significantly across schools. Moreover, the estimation of RC models requires degrees of freedom whose loss is more difficult to justify when sample sizes are small. Also, when variables are centered, as was the case in our analysis, variation across slopes is less of a concern. For greater stability in our estimates, and because very few of the coefficients varied significantly across schools, we therefore elected to use a "random intercept" model.

[22] τ_0 is between-school variation.

denoting whether a school is private rather than public, and whose coefficient represents the difference between private school and public school achievement. It also includes students' grade level (third or fourth) as a control variable. This model therefore provides a simple estimate of the "unadjusted" achievement difference between private and public schools.

Model II further controls for student and family background variables ($\mathbf{X}$), in order to assess whether the higher achievement of private schools arises, in part, from the higher socioeconomic status of their students. Finally, Model III adds controls for the peer group characteristics of the school (part of $\mathbf{Z}$), to evaluate whether the private school effect stems from different peer group characteristics.

4.2 Meta-Analysis

Meta-analysis uses a multilevel framework to model variation among the estimated effects of different studies.[23] Here the term "studies" is defined loosely: in the present analysis, for example, we will treat each country's estimate as a study. In particular, we will use a meta-analysis to create a Latin American summary measure of the achievement difference between private and public schools.

The multilevel structure of the meta-analysis is very similar to that already discussed, although in this case, the first level is the within-study level, while the second is the between-study level. The first-level model can be expressed as follows:

$$d_c = \delta_c + \varepsilon_c \tag{5}$$

where c denotes the study or country, and $\varepsilon_c \sim \mathrm{N}(0, V_c)$. δ_c is a vector of the "true" achievement difference between public and private schools in every country, while d_c is a

[23]For a detailed treatment, see Bryk and Raudenbush (1992), Glass, McGaw, and Smith (1981), Hedges and Olkin (1985).

vector of the *estimated* achievement difference between the two sectors.[24] This equation therefore states that d_c estimates δ_c with a known sampling variance V_c.[25]

The purpose of the second level is to model the variation in the true achievement differences:

$$\delta_c = \eta_0 + u_0 \tag{6}$$

where $u_0 \sim \mathrm{N}(0, \nu)$, and is the variation in the public-private achievement gap across countries. η_0 is the Bayesian mean of d_c, and is therefore a summary measure of the achievement difference between public and private schools across the region.[26] This average achievement difference will be estimated for all models and outcomes.

5 Data and Variables

5.1 The *Primer Estudio Internacional Comparativo*

The analyses in this paper use data from the *Primer Estudio Internacional Comparativo (PEIC)*—the first international study in Latin America to use common tests and questionnaires across multiple countries. Conducted in thirteen countries in 1997, this study was funded by the Inter-American Development Bank, the Ford Foundation, UNESCO, as well as the participating countries, and coordinated by the *Laboratorio Latinoamericano de Evaluación de la Calidad de la Educación.*

The data collection process entailed the testing of more than 50,000 third and fourth grade students in language and mathematics, as well as the administration of a set of comprehensive questionnaires to students and their parents, teachers, and school principals. In

[24] Both δ_c and d_c are standardized.

[25] We say that V_c is known because it is derived from the standard errors of the estimates in d_c.

[26] η_0 is estimated by weighting each country's d_c by how reliably it has been measured, where reliability is inversely proportional to V_c and ν, and directly proportional to the number of countries.

every country, private schools were over-sampled to allow for precise comparisons between public and private schools. For a technical overview of the study and descriptive analyses of the data, see UNESCO-OREALC (1998), and for the results of a multilevel analysis of the student- and school-level factors which affect achievement, see Willms and Somers (2001, in press).

Thirteen countries participated in the *PEIC*: Argentina, Bolivia, Brazil, Colombia, Costa Rica, Cuba, Chile, Honduras, Mexico, Paraguay, Peru, the Dominican Republic, and Venezuela. However, only ten of these countries were included in our analyses. The Costa Rican data were not used because of a problem in matching test scores to student data, and Cuba was excluded because it has no private school sector.[27] Honduras was also omitted given its abundance of missing data, which reduced its sample to only 140 students in 27 schools.

The analyses also omit schools in rural areas. The rationale for doing so is threefold. First, the *PEIC* sampling strategy designated rural schools as a single stratum dominated by the public sector (in contrast, urban areas were divided into public and private strata), which reflects the fact that the rural sectors of most Latin American countries are overwhelmingly public. Yet in a few countries, more notably Chile, rural private schools are more common, and hence there may have been some miscoding of school sector in these areas. Second, rural students are typically poorer than students in urban areas, probably in ways that are unobserved by researchers, which raises the specter of additional omitted variable bias. Third, there is a particularly acute missing data problem in the rural school subset.

[27] Notwithstanding this exclusion, it should be noted that Cuban students scored about two standard deviation above those of other countries in the region (Willms & Somers, 2001, in press).

5.2 Missing Data Issues

The *PEIC* dataset, like similar datasets from Latin America, has a large amount of missing data. We adopted two strategies to deal with this problem. First, we excluded school and teacher characteristics from the analysis, since missing data were acute for these questionnaires. While this prevents us from assessing whether any observed sector differences are related to school resources, policy or practice, it preserves a substantial amount of data and may diminish biases associated with non-random attrition. Second, when a case was missing data for a key variable, we imputed the overall mean of the variable; in regression analyses, we then included a dummy variable denoting whether an observation in the original variable was missing, following a procedure outlined in Little and Rubin (1987).

Nevertheless, Table 3 shows that samples sizes used for the empirical analyses declined substantially from their original levels. Fortunately, the proportion of private schools and students in each country remains fairly stable. This is at least suggestive that sample attrition was not markedly different across public and private schools. Moreover, the sample sizes in most countries are still sufficiently large to obtain multilevel estimates with acceptable levels of statistical power (the requirements for these levels are discussed along with the findings).

5.3 Dependent and Independent Variables

Table 4 reports the definitions of the dependent and independent variables. The analysis will focus on two dependent variables: language achievement and mathematics achievement, as measured by test scores. Both outcomes were scaled by country on their mean and standard deviation.[28] This transformation expresses the achievement gap between public

[28]Standardizing an outcome for use in a multilevel model is somewhat different than the more straightforward procedure for single-level models. Using the results from a random intercept null model, the outcome must be scaled using the Bayesian "grand mean" ψ_0 and the square root of the sum of σ^2 (within-school variance) and τ_0 (between-school variance) (see Section 4.1 for definitions of these measures).

and private schools (as well as all other estimates) as a fraction of a standard deviation, facilitating comparisons with studies in Table 2.

A wide range of explanatory variables was chosen, in order to adequately control for both the effects of student-level and school-level socioeconomic status. Variables in the student-level subset reflect the level of educational resources in the home, and the processes by which parents use these resources to contribute to their child's cognitive development. Variables in the school-level subset capture the advantages of being surrounded by peers who come from more advantaged backgrounds. Beyond these two subsets, we further control for gender and grade, and whether the school was located in a city (more than 5,000 inhabitants) or a mega-city (more than 1 million inhabitants).

5.4 Descriptive Analyses

Table 5 presents the descriptive statistics for both the outcomes and the explanatory variables, by sector and country. Both mathematics and language achievement are consistently greater, on average, in private schools, in many cases by up to half of a standard deviation.

Our results also indicate that private school students have access to more educational resources in their home, and are part of families in which their academic endeavors are more likely to be encouraged. In every country, the educational level of private school parents is roughly one standard deviation (or 3 years) above that of public school parents. The percentage of private school students who have at least ten books in their home is approximately 30 percent greater across countries. In addition, the parents of private school students appear to be consistently more engaged in the academic life of their child, as measured by several indicators. These differences are remarkably uniform across countries, despite diverse institutional contexts.

There are also sharp differences in the average peer group characteristics of private and public schools. In all countries, the mean socioeconomic status of private schools is well over one standard deviation above that of public schools. Moreover, private schools have, on average, higher levels of parental involvement, and more favorable disciplinary climates.

6 Multilevel Results

6.1 General Patterns

Table 6 presents the results from our multilevel analyses. Although Models I, II, and III condition on the variables described in previous sections, the table presents only the coefficient on the private school dummy variable.[29]

Across all countries and for both dependent variables, the "unadjusted" achievement difference between public and private schools is positive, statistically significant, and usually quite large (see Model I).

After the effects of student background have been taken into account, the achievement differences decline markedly (see Model II). This is unsurprising given the large differences in average socioeconomic status between public and private schools. If we further control for peer group characteristics—as in Model III—the achievement gap between the two school types becomes even smaller, and in some instances negative. This is suggestive that peer group effects may account for a substantial portion of the private school effect as it is typically measured.

The "goodness of fit" for each of these three models corroborates this pattern. In the language models, the amount of between-school variance explained begins at 20% (on average) in Model I, climbs to 33.7% in Model II, and rises to 51% in Model III. As for within-school variance explained, it only increases substantially in Model II. This is to be expected, given

[29]Coefficients on other control variables are available from the authors.

that the extra controls in Model III are school-level variables.

Figures 1 and 2 are graphical representations of the results in Table 6. These figures further emphasize three patterns in the results. First, the downward trend in the achievement gap across the three models becomes even more evident. Second, there is some variance across countries in the size of the private school effect. In Model III, for example, the effects range from -0.17 to 0.19 for language, and from -0.17 to 0.22 for mathematics. To some extent, this may reflect the institutional diversity across the countries in our sample. Third, the effects in Model III appear to be clustered around zero (this is examined more carefully in the meta-analysis).

In general, the estimates from Model III are not statistically significant, but there is a caveat. The magnitudes of several estimates in Table 6 are non-negligible, despite their lack of statistical insignificance (see especially the results for Bolivia, the Dominican Republic and Venezuela). In part, this is due to reduced sample sizes—particularly the number of schools in the sample—and the reduced statistical power it implies.[30]

6.2 Meta-Analysis

Given the variation of the estimates across countries, we conducted a meta-analysis to derive a regional summary measure of the achievement difference between public and private schools. An advantage of summarizing our results in this way is that countries which are less

[30]Power is the probability of rejecting the null hypothesis when it is false. There are two key determinants of power in a multi-level context: sample size (both the number of schools and the number of observations within them) and intra-class correlation (which is the percentage of total variance which can be attributed to between-school variation). Although there have been attempts to determine proper guidelines for the sample size required to achieve a power of 0.90 (see Leeden & Busing, 1994; Kim, 1990; Bassiri, 1988), the suggested sample sizes are based on certain assumptions about intra-class correlation that are not necessarily satisfied. Consequently, these guidelines are difficult to use and interpret. The only consistent finding to emerge from this literature is that, for level-2 effects in particular, a large number of schools is deemed more important than the number of students per school. This point is probably relevant to the estimates of Bolivia, the Dominican Republic, and Venezuela, given that these countries have the smallest number of schools in their sample.

reliably estimated—i.e. that have a smaller number of schools and larger standard errors, such as Bolivia, the Dominican Republic and Venezuela—are not weighted as heavily into the summary. Table 7 presents our results by model and outcome.

These measures follow the same downward pattern already noted: the estimates for Model I are positive and large, those for Model II are roughly two-thirds of those of Model I (yet still of a considerable magnitude), and those for Model III are approximately zero. Even though the differences between public and private schools in mathematics achievement appeared to vary more across countries than those in language (see Figures 1 and 2), the summary estimates for both of the outcomes are similar in size.

7 Evidence on Selection Bias

It is possible that estimates of private school effectiveness are biased by the exclusion of family or student variables that are correlated with private school choice. This section reviews several types of evidence which provide clues on the direction and magnitude of bias.

7.1 Evidence from Statistical Corrections

We estimated a variety of additional models, following Heckman's two-step procedure.[31] For lack of an adequate instrument, we did not impose an exclusion restriction, since selection bias can be exacerbated if an invalid restriction is used.[32] Thus, the identification of our model rested upon the assumption that private school attendance is a non-linear function of the independent variables. Given the frailty of this assumption, it is not surprising that the

[31] We corrected all standard errors for clustering at the school level in an ordinary least-squares model, given the (as yet) unexplored possibility of correcting for selection bias in a multilevel framework.

[32] Initially, we experimented with several variables, including rural/urban location. However, probit regressions, not reported here, suggested that the only consistent determinant of private attendance was school SES. Given that it was also one of the strongest variables in the achievement models, this variable could not be used as an exclusion.

results from our attempts to control for selection were, in most countries, vastly different than our multilevel estimates—in both positive and negative directions—and highly sensitive to subtle variations of the specification. Thus, we do not report these results and place little stock in their implications.

Two studies that were reviewed in Section 3 make a more compelling case for their exclusion restrictions (Jimenez et al., 1991; McEwan, 2001a). It is noteworthy that neither study finds strong evidence of selection bias.[33]

7.2 Evidence from Proxy Variables

Instead of appealing to sophisticated corrections, another approach to ameliorating bias is to identify reasonable proxies for unobserved variables. We consider two classes of proxy variables: selective admissions into private schools, and peer variables.

In Chile, Parry (1996) shows that 63% of private subsidized schools in Santiago use one of several methods to select students for admission, including entrance exams, interviews, and minimum grade requirements. Similarly, Gauri (1998) shows that 37% of students in private subsidized schools and 82% of students in private, fee-paying schools took exams in order to enroll in their present school.

At least in Chile, private schools are more likely to exercise selective admissions policies. If private schools select their students based on characteristics that are unobserved to researchers but still correlated positively with achievement, as seems likely, then estimates of private school coefficients are biased. Parry (1996) tests this by including a variable measuring school selection in achievement regressions similar to ours. The selection variable's coefficient is strongly positive, while the coefficient on a private school dummy becomes statistically insignificant, suggesting upward bias.

[33] In Jimenez et al. (1991), we refer to the full specification, including peer variables, in Table 3.6.

Finally, the inclusion of peer variables in regressions may proxy for some unobserved characteristics of students and families. This has been extensively noted in the empirical literature on the estimation of peer effects (Evans et al., 1992; Moffitt, 2001). For example, highly-motivated families may choose schools with “better” peer groups. If motivation is unobserved, then peer-group status will partly reflect the influence of family variables like motivation. While this may prevent the unbiased estimation of peer effects, it yields unexpected dividends for a study of private school effects. By including peer variables, we may diminish selection bias by further controlling for unobserved characteristics of students and families.

Overall, there is some evidence that typical estimates of private school effects are biased upward by selection, and some evidence that estimates are not biased. There is, however, little evidence that effects are biased downward. At least tentatively, one might regard the estimates from this study as roughly accurate, or at least an upper bound to the magnitude of private school effects.

8 Interpreting the Evidence

This section assesses whether research on private school effectiveness provides guidance on the potential impact of voucher programs or similar attempts to increase private enrollments. We argue that two issues must be considered before generalizing empirical results to policy. The first is whether private school effects reflect the influence of peer characteristics. The second is the degree of institutional heterogeneity in the private sector.

8.1 The Importance of Peer Characteristics

Despite the fact that they confound the effects of schools and peers, private school effects that do not condition on the socioeconomic status of peer groups—like Model II—provide a useful first-order measure of the potential impact of a small-scale voucher program. From the family's perspective, this estimate is perhaps the most relevant one, since families may care little whether their child's achievement is enhanced by schools or peers.

From society's perspective, however, the answer may be different. Arguments for vouchers rely on the notion that private schools are more effective because of their private governance —but not because of spillover benefits from the privileged students that they happen to enroll (Chubb & Moe, 1990). If "private" effects are largely peer effects, then it becomes problematic to assess the potential impact of large-scale voucher programs, if only because the stock of "good" peers is finite. At the margin, expanding private schools must enroll an increasingly diverse group of students, perhaps drawn from middle- or lower-income groups (Chile provides the best example of this in Latin America). This, in turn, might gradually attenuate private school effects that do not condition on peer-group status. Hence, an empirical estimate of private school effects that does not condition on peer groups may give a poor predictor of private school effectiveness after an expansion of private schooling.

In light of these arguments, we have emphasized the coefficients from Model III. These results are not indicative of strong and consistent private school effects across countries. Some coefficients imply modestly positive effects, while roughly an equal number imply negative effects, and only one of them is statistically significant. On average, the effect is zero.

8.2 The Importance of Institutional Heterogeneity

The estimates in this study consider only one category of private schooling, due to the limitations of the data. As Section 2 emphasized, however, private schools in Latin America are managed and financed in a variety of ways. There are religious and non-religious institutions; some of the latter maximize profits, while others maximize a range of broad range of objectives. Some are funded exclusively by the private sector, but most rely upon a diverse portfolio of public and private resources.

A small-scale voucher plan is unlikely to alter this institutional landscape, if only because a modest growth in demand for private schools can be absorbed by existing institutions. Hence, the empirical evidence in Tables 2 and 6 might helpfully predict the average impact on a student of switching from a public to a private school.

In contrast, a large-scale voucher plan may produce large increases in demand for private schooling, thus encouraging the expansion and creation of many private schools. There is little evidence on whether "new" private schools, created in response to vouchers, will replicate the effectiveness of "old" private schools.

In the Chilean case, for example, newly-created private schools appear to differ markedly in their results. Prior to the implementation of its 1980 reforms, most private schools were religiously affiliated and some received partial subsidies (McEwan & Carnoy, 2000; Gauri, 1998). Following the reform, a new class of private schools emerged, with a non-religious and for-profit orientation. The empirical literature on the Chilean case generally suggests that these schools—now the largest category of private schooling—are no more effective in producing achievement than public schools (see Table 2). Other categories of private schooling—including Catholic schools and elite, fee-paying schools—are somewhat more effective, even when conditioning on peer characteristics.[34]

This evidence suggests that caution is warranted when using evidence to predict the impact of large-scale voucher plans. Particularly when the private sector is small or ho-

[34]This paper's estimates suggest private school effects in Chile of 0.19 and 0.17 in language and mathematics (statistically significant only for language). These lie between the estimates of studies reported in Table 2 that divide private schooling into a larger number of categories. This is also the case for Argentina and the Dominican Republic.

mogeneous, the empirical results in Tables 2 and 6 may provide few guides on the relative effectiveness of newly-created private schools.

9 Summary and Conclusions

This paper has sought to advance the literature on the relative effectiveness of private and public schooling in Latin America. Using UNESCO data from 10 countries, it estimated a range of multilevel models with two dependent variables: language and mathematics achievement.

There are substantial and consistent differences in the achievement of private and public schools, usually around one-half a standard deviation. A small portion of these differences is accounted for the higher socioeconomic status of students in private schools. A quite substantial portion is explained by the varying peer group characteristics in private and public schools. After accounting for the latter, the average private school effect across all ten countries is zero, though with some variance around this mean (typically ranging between -0.2 and 0.2 standard deviations). Evidence on selection bias is hardly conclusive, but we argued that these effects are most likely to constitute an upper bound to the true effects.

To some extent, these results may be helpful in predicting the impact of relatively small voucher programs. In this respect, transferring students to private schools would yield substantial increases in student achievement. There is strong evidence, however, that most of these gains would arise from the beneficial effect of "better" peer groups, as opposed to greater school effectiveness.

We argued that the evidence is not as helpful in predicting the impact of large-scale voucher programs. Even when conditioning on peer characteristics—which some studies in the current literature neglect to do—there is the issue of institutional heterogeneity. There are simply few guarantees that the effectiveness of newly-created private schools will bear any resemblance to that of existing ones. Hence, the policy usefulness of the current empirical evidence should not be overstated.

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Table 1: **Private Enrollment (As a Percentage of Total Enrollment)**

	Primary		**Secondary**	
	1990	1996	1990	1996
Argentina	—	20	—	—
Bolivia	10	—	—	—
Brazil	14	11	—	—
Chile	39	42	42	45
Colombia	15	19	39	—
Dominican Republic	—	16	—	33
Mexico	6	6	12	11
Paraguay	15	14	22	27
Peru	13	12	15	16
Venezuela	14	18	29	—

Source: UNESCO (2000)

Table 2: **Studies of Private School Effectiveness in Latin America**

Country	Study	Year of sample(s)	Grade level at post-test	Level of analysis	Method	Controls	Exclusion restriction	Outcome	Difference in standard deviations (type of private school)
Argentina	(McEwan, 2001c), Table 6	1997	7	Student	OLS	Individual SES; peer SES	n/a	Spanish	**0.17** (Catholic subsidized) **0.29** (non-religious subsidized) 0.11 (non-subsidized)
								Math	0.07 (Catholic subsidized) **0.27** (non-religious subsidized) -0.03 (non-subsidized)
Bolivia	(Mizala et al., 1999), Table 3, Model 4[a]	1997	6	Student	OLS with selectivity	Individual SES	"Sociocultural" level of family, regional dummy variables, others	Spanish	**0.22**
Brazil	(Lockheed & Bruns, 1990), Table 5	1988	Secondary	Student	HLM	Individual SES; peer SES	n/a	Portuguese	**0.55**
								Math	-0.07

Continued on next page

Table 2: **Continued**

Country	Study	Year of sample(s)	Grade level at post-test	Level of analysis	Method	Controls	Exclusion restriction	Outcome	Difference in standard deviations (type of private school)
Chile	(McEwan & Carnoy, 2000), Table 4	1990-1996	4	School	OLS	School-wide SES	n/a	Spanish	**0.27** (Catholic subsidized) **-0.07** (non-religious subsidized) **0.57** (fee-paying private)
								Math	**0.22** (Catholic subsidized) **-0.07** (non-religious subsidized) **0.57** (fee-paying private)
	(Mizala & Romaguera, 2000), Table 4, Model 5	1996	4	School	OLS	School-wide SES	n/a	General achievement	**0.19** (private fee-paying) 0.05 (private subsidized)
	(McEwan, 2001a), Table 8[b]	1997	8	Student	OLS with selectivity	Individual SES; peer SES	Density of private school supply	Spanish	**0.18** (Catholic subsidized) 0.04 (non-religious subsidized) **0.48** (fee-paying private)

Continued on next page

Table 2: **Continued**

Country	Study	Year of sample(s)	Grade level at post-test	Level of analysis	Method	Controls	Exclusion restriction	Outcome	Difference in standard deviations (type of private school)
								Math	**0.26** (Catholic subsidized) 0.02 (non-religious subsidized) **0.53** (fee-paying private)
Colombia	(Psacharopoulos, 1987), Table 4, Model 4	1981	Secondary	Student	OLS	Individual SES	n/a	General achievement	**0.20** (private academic)
	(Cox & Jimenez, 1991), p. 115[c]	1981	Secondary	Student	OLS with selectivity	Individual SES	Parental education, father's occupation and income		0.58 (private academic)
	(Angrist et al., 2001), Table 8, Model 3[d]	1999	Secondary	Student	IV	Randomized assignment, individual SES	Voucher awarded to student	General achievement	**0.29**
Dominican Republic	(Jimenez et al., 1991), Table 3.7[e]	1983	8	Student	OLS with selectivity	Individual SES; peer SES	Private school tuition	Math	-0.55 ("F-type" schools) 0.27 ("O-type" schools)

Note: Achievement differences in bold are statistically significant at the 0.05 level. Effect sizes were calculated by dividing regression coefficients by the standard deviation of the dependent variable, unless coefficient were already standardized or the standard deviation was not reported. n/a indicates not applicable to the study. OLS stands for ordinary least-squares, HLM stands for hierarchical linear modelling, and IV stands for instrumental variables. Another study on Chile was excluded because it interacted a private school dummy variable with 8 independent variables—thus estimating 9 private school effects for different, and apparently arbitrarily chosen categories of schools (Parry, 1997).

[a]The authors do not report the standard deviation of the dependent variable; hence, the effect size—specifically, Hedge's g—was estimated with the T-statistic of the private school variable and the sample sizes of the private and public sectors (Rosenthal, 1994).

[b]Results are from models that are not corrected for selection bias, since the hypothesis of no selection bias could not be rejected.

[c]The effect of 5.82 is divided by the standard deviation of the dependent variable in the academic subsample of non-INEM public schools, taken from Psacharopoulos (1987), who uses the same data set. The statistical significance of the effect could not be verified, because the standard error of the prediction was not calculated.

[d]The estimate corresponds to the effect of the "treatment-on-the-treated" (i.e., attending a private school) rather than the effects of the "intent-to-treat" (i.e., being offered a voucher.

[e]The statistical significance of the effects could not be verified, because the standard errors of the predictions were not calculated.

Table 3: **Distribution of the Original and Final Samples by School Sector**

	Original Sample†		**Final Sample**‡	
	Students	**Schools**	**Students**	**Schools**
Argentina	3701 (21.6)	102 (21.6)	2286 (24.4)	95 (22.1)
Bolivia	3608 (43.5)	42 (43.2)	3030 (44.7)	40 (55.0)
Brazil	3628 (26.3)	109 (26.4)	2018 (26.5)	109 (29.4)
Chile	3449 (45.4)	88 (49.2)	1200 (53.3)	86 (45.3)
Colombia	3095 (34.4)	85 (35.3)	2215 (36.0)	85 (32.9)
Dominican Republic	2398 (40.5)	63 (41.1)	1631 (43.8)	60 (41.7)
Mexico	3284 (29.2)	85 (27.9)	2232 (27.3)	80 (27.5)
Paraguay	3053 (45.1)	79 (45.5)	1237 (44.9)	65 (46.2)
Peru	3055 (31.4)	82 (32.0)	2748 (32.5)	82 (31.7)
Venezuela	2875 (28.7)	92 (28.1)	912 (28.9)	50 (28.0)
Region	32146 (34.4)	827 (34.2)	19509 (35.5)	752 (34.3)

Note: The first entry is the total number of observations; the second entry (in parentheses) is the percentage which is associated with the private sector (private schools were over-sampled, however, such that these percentages are not representative of the actual public-private distribution).

†The original sample size excludes rural students and schools, as well as students who were tested in neither mathematics nor language.

‡The final sample size includes only those students who took *both* the mathematics and language test, and have no missing values for the independent variables.

Table 4: **Definition of Variables**

Variable	Description
Student Socioeconomic Status	
PARENTED	Mean of the responding parent's and his/her spouse's (if applicable) years of schooling
TWOPARNT	Dummy variable denoting whether there are two parents in the home (whether married or not)
TENBOOKS†	Dummy variable denoting whether there are at least ten books in the home
PARINVLV‡	Index created from three categorical variables, denoting the frequency of the responding parent's involvement in school-related activities (seldom, sometimes, always), the extent to which the parent knows his/her child's teacher (not at all, a little, a lot), and the frequency of the parent's attendance to parent-teacher meetings (never or seldom, almost always, always)
READING	Categorical variable denoting how frequently the parent read to the student when s/he was younger (less than once a month, at least once a month, almost every day)
Peer Group Characteristics	
SCHLSES⋆	Average socioeconomic status (SES) of the students in the school (where SES‡ is student-level index created from *PARENTED*, *TWOPARNT*, and *TENBOOKS*)
SCLPARNT⋆	School-level mean of *PARINVLV*
DISCIP‡	Disciplinary index created from three dummy variables denoting whether there are no disruptive students within classrooms, whether fights infrequently happen, and whether students within classrooms are good friends
Other School and Student Characteristics	
PRIVATE	Dummy variable denoting whether the school is private (*vs* public)
URBAN	Dummy variable denoting whether the school is in an urban area (*vs* a mega-city area)
FEMALE†	Dummy variable denoting whether the student is female (*vs* male)
GRADE	Dummy variable denoting whether the student is in grade 4 (*vs* grade 3)

†Missing values for this variable were replaced by its country-level mean; therefore, when it is included in regressions, also included is a dummy variable indicating which values were missing in the original variable.
‡Constructed using the first principal component extracted from a factor analysis, and standardized by country to have a mean of 0 and a standard deviation of 1.
⋆Standardized by country to have a mean of 0 and standard deviation of 1.

Table 5: **Descriptive Statistics by School Type**

	Argentina			Bolivia			Brazil			Chile			Colombia		
	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T
Academic Achievement[†]															
LANG	-0.08	0.49	0.00	-0.07	0.27	0.00	-0.07	0.46	0.00	-0.27	0.24	0.00	-0.15	0.34	0.00
	(0.98)	(0.99)	(1.00)	(0.98)	(1.01)	(1.00)	(0.97)	(1.06)	(1.00)	(1.00)	(0.94)	(1.00)	(0.98)	(0.97)	(1.00)
MATH	-0.08	0.47	0.00	-0.10	0.39	0.00	-0.08	0.48	0.00	-0.23	0.21	0.00	-0.12	0.29	0.00
	(0.97)	(1.04)	(1.00)	(0.94)	(1.12)	(1.00)	(0.95)	(1.16)	(1.00)	(0.97)	(0.98)	(1.00)	(0.96)	(1.04)	(1.00)
Student Socioeconomic Status															
PARENTED	9.25	12.31	9.68	8.93	11.24	9.42	5.65	10.27	6.29	8.76	11.49	10.20	8.19	10.91	9.01
	(3.39)	(3.16)	(3.52)	(3.54)	(4.07)	(3.78)	(3.45)	(4.35)	(3.93)	(3.09)	(3.08)	(3.37)	(3.42)	(3.43)	(3.64)
TWOPARNT	0.83	0.88	0.84	0.81	0.84	0.82	0.80	0.87	0.81	0.80	0.86	0.83	0.70	0.78	0.73
	(0.38)	(0.33)	(0.37)	(0.39)	(0.37)	(0.39)	(0.40)	(0.34)	(0.39)	(0.40)	(0.35)	(0.38)	(0.46)	(0.42)	(0.45)
TENBOOKS	0.54	0.83	0.58	0.39	0.63	0.44	0.37	0.74	0.42	0.43	0.73	0.59	0.42	0.68	0.50
	(0.49)	(0.38)	(0.49)	(0.48)	(0.48)	(0.49)	(0.48)	(0.44)	(0.49)	(0.49)	(0.44)	(0.49)	(0.49)	(0.46)	(0.50)
PARINVLV[‡]	-0.04	0.27	0.00	-0.01	0.05	0.00	-0.03	0.17	0.00	0.03	-0.02	0.00	0.05	-0.11	0.00
	(1.01)	(0.86)	(1.00)	(1.01)	(0.96)	(1.00)	(1.01)	(0.90)	(1.00)	(1.07)	(0.94)	(1.00)	(0.98)	(1.04)	(1.00)
READING	1.09	1.30	1.12	0.97	0.99	0.98	1.16	1.32	1.18	1.11	1.25	1.18	0.93	1.01	0.95
	(0.79)	(0.73)	(0.78)	(0.76)	(0.73)	(0.75)	(0.82)	(0.77)	(0.81)	(0.78)	(0.73)	(0.76)	(0.77)	(0.72)	(0.76)
Peer Group Characteristics															
SCHLSES[‡]	-0.16	1.05	0.00	-0.39	1.19	0.00	-0.28	1.48	0.00	-0.56	0.70	0.00	-0.37	0.96	0.00
	(0.91)	(1.01)	(1.00)	(0.59)	(1.09)	(1.00)	(0.72)	(1.02)	(1.00)	(0.71)	(0.86)	(1.00)	(0.80)	(0.84)	(1.00)
SCLPARNT[‡]	-0.11	0.74	0.00	0.00	0.00	0.00	-0.07	0.38	0.00	0.05	-0.06	0.00	0.19	-0.48	0.00
	(0.94)	(1.12)	(1.00)	(1.00)	(1.06)	(1.00)	(0.98)	(1.07)	(1.00)	(1.07)	(0.91)	(1.00)	(0.92)	(1.07)	(1.00)
DISCIP[‡]	-0.08	0.55	0.00	-0.30	0.93	0.00	-0.06	0.34	0.00	-0.02	0.03	0.00	-0.14	0.37	0.00
	(1.02)	(0.66)	(1.00)	(0.92)	(0.59)	(1.00)	(1.01)	(0.89)	(1.00)	(1.01)	(1.00)	(1.00)	(1.02)	(0.86)	(1.00)
Other Characteristics															
URBAN	0.91	0.79	0.89	0.84	0.55	0.77	0.83	0.65	0.80	0.74	0.49	0.63	0.70	0.40	0.62

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Table 5: **Continued**

	Dominican Republic			**Mexico**			**Paraguay**			**Peru**			**Venezuela**		
	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T
	(0.29)	(0.42)	(0.31)	(0.37)	(0.52)	(0.43)	(0.38)	(0.49)	(0.40)	(0.45)	(0.51)	(0.49)	(0.46)	(0.50)	(0.49)
FEMALE	0.51	0.42	0.50	0.55	0.48	0.53	0.48	0.50	0.49	0.52	0.48	0.50	0.50	0.47	0.49
	(0.47)	(0.46)	(0.47)	(0.49)	(0.49)	(0.49)	(0.50)	(0.50)	(0.50)	(0.49)	(0.49)	(0.49)	(0.50)	(0.50)	(0.50)
GRADE	0.47	0.45	0.47	0.49	0.52	0.50	0.46	0.44	0.46	0.47	0.52	0.50	0.51	0.51	0.51
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)

	Dominican Republic			**Mexico**			**Paraguay**			**Peru**			**Venezuela**		
	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T
Academic Achievement†															
LANG	-0.06	0.10	0.00	-0.06	0.61	0.00	-0.09	0.25	0.00	-0.12	0.50	0.00	-0.06	0.26	0.00
	(0.99)	(1.01)	(1.00)	(0.99)	(0.93)	(1.00)	(0.99)	(0.98)	(1.00)	(0.96)	(1.02)	(1.00)	(0.98)	(1.03)	(1.00)
MATH	-0.01	0.01	0.00	-0.05	0.52	0.00	-0.09	0.23	0.00	-0.14	0.62	0.00	-0.13	0.50	0.00
	(0.94)	(1.10)	(1.00)	(0.99)	(0.92)	(1.00)	(0.96)	(1.06)	(1.00)	(0.91)	(1.12)	(1.00)	(0.97)	(0.97)	(1.00)
Student Socioeconomic Status															
PARENTED	8.56	10.25	9.21	8.64	11.94	8.92	9.03	11.64	9.73	8.95	12.92	9.71	9.07	11.52	9.56
	(3.80)	(4.05)	(3.98)	(3.19)	(2.85)	(3.29)	(3.63)	(3.69)	(3.82)	(3.85)	(3.40)	(4.08)	(3.53)	(3.50)	(3.66)
TWOPARNT	0.73	0.68	0.71	0.88	0.90	0.88	0.79	0.88	0.82	0.82	0.85	0.83	0.67	0.77	0.69
	(0.45)	(0.47)	(0.45)	(0.33)	(0.31)	(0.32)	(0.40)	(0.32)	(0.39)	(0.38)	(0.35)	(0.38)	(0.47)	(0.42)	(0.46)
TENBOOKS	0.26	0.41	0.32	0.31	0.72	0.35	0.43	0.67	0.49	0.29	0.63	0.35	0.44	0.65	0.48
	(0.43)	(0.48)	(0.45)	(0.46)	(0.45)	(0.47)	(0.49)	(0.47)	(0.49)	(0.45)	(0.48)	(0.48)	(0.49)	(0.48)	(0.49)
PARINVLV‡	0.03	-0.04	0.00	-0.02	0.19	0.00	-0.04	0.10	0.00	-0.01	0.06	0.00	-0.05	0.19	0.00
	(1.01)	(0.98)	(1.00)	(1.00)	(0.96)	(1.00)	(1.02)	(0.93)	(1.00)	(1.02)	(0.92)	(1.00)	(1.03)	(0.83)	(1.00)
READING	1.11	1.23	1.16	0.89	1.10	0.91	1.04	1.15	1.07	0.98	1.25	1.03	1.13	1.17	1.14
	(0.78)	(0.73)	(0.76)	(0.73)	(0.74)	(0.74)	(0.75)	(0.73)	(0.75)	(0.76)	(0.69)	(0.75)	(0.74)	(0.72)	(0.73)
Peer Group Characteristics															
SCHLSES‡	-0.27	0.43	0.00	-0.15	1.53	0.00	-0.34	0.87	0.00	-0.27	1.19	0.00	-0.27	1.11	0.00

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Table 5: **Continued**

	Dominican Republic			Mexico			Paraguay			Peru			Venezuela		
	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T	Pu	Pr	T
	(0.52)	(1.39)	(1.00)	(0.89)	(0.76)	(1.00)	(0.71)	(1.13)	(1.00)	(0.79)	(0.96)	(1.00)	(0.81)	(0.98)	(1.00)
SCLPARNT‡	0.18	-0.28	0.00	-0.07	0.68	0.00	-0.08	0.21	0.00	-0.04	0.18	0.00	-0.14	0.57	0.00
	(0.89)	(1.12)	(1.00)	(0.97)	(1.09)	(1.00)	(1.01)	(0.96)	(1.00)	(1.04)	(0.79)	(1.00)	(1.01)	(0.76)	(1.00)
DISCIP‡	-0.04	0.07	0.00	-0.07	0.70	0.00	-0.08	0.19	0.00	-0.09	0.38	0.00	-0.11	0.47	0.00
	(1.08)	(0.88)	(1.00)	(1.00)	(0.75)	(1.00)	(0.99)	(1.04)	(1.00)	(1.02)	(0.84)	(1.00)	(0.99)	(0.93)	(1.00)
Other Characteristics															
URBAN	0.69	0.43	0.59	0.81	0.72	0.81	1.00	1.00	1.00	0.72	0.46	0.67	0.90	0.76	0.87
	(0.47)	(0.51)	(0.50)	(0.39)	(0.49)	(0.40)	(0.00)	(0.00)	(0.00)	(0.45)	(0.52)	(0.47)	(0.30)	(0.45)	(0.34)
FEMALE	0.55	0.45	0.51	0.50	0.55	0.50	0.51	0.56	0.53	0.50	0.49	0.50	0.56	0.50	0.55
	(0.47)	(0.48)	(0.48)	(0.49)	(0.49)	(0.49)	(0.42)	(0.45)	(0.43)	(0.49)	(0.49)	(0.49)	(0.46)	(0.48)	(0.47)
GRADE	0.50	0.48	0.49	0.50	0.49	0.50	0.51	0.47	0.50	0.49	0.49	0.49	0.41	0.45	0.41
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)	(0.49)

Pu — Public School Sector; Pr — Private School Sector; T — Total

Note: The first entry is the variable mean; the second entry (in parentheses) is the standard deviation. Results are weighted to correct for the over-sampling of certain school types. See Table 3 for sample sizes.

†The test scores presented here are standardized by country on the "ordinary" mean and standard deviation, and not the multilevel mean and standard deviation (see Section 5.3 for further explanation); however, our multilevel analyses used test scores that were standardized on the latter.

‡Indices standardized by country to have a mean of 0 and standard deviation of 1.

Table 6: **Achievement Differences Between Public and Private Schools, Controlling for Student Background and Peer Group Characteristics**

	Language			**Mathematics**		
	(I)	**(II)**	**(III)**	**(I)**	**(II)**	**(III)**
AR	0.563**	0.382**	0.113	0.508**	0.312*	-0.053
	(0.112)	(0.099)	(0.083)	(0.147)	(0.128)	(0.117)
	0.200/0.062	0.450/0.084	0.621/0.084	0.067/0.092	0.281/0.120	0.512/0.119
BO	0.300*	0.172	-0.173	0.327	0.236	-0.065
	(0.134)	(0.132)	(0.223)	(0.162)	(0.159)	(0.273)
	0.096/0.001	0.208/0.028	0.275/0.028	0.069/0.000	0.106/0.012	0.128/0.012
BR	0.578**	0.319**	-0.128	0.631**	0.412**	-0.160
	(0.111)	(0.096)	(0.092)	(0.128)	(0.113)	(0.099)
	0.258/0.078	0.478/0.116	0.679/0.119	0.256/0.097	0.467/0.122	0.711/0.125
CH	0.434**	0.349**	0.194*	0.379**	0.285**	0.168
	(0.083)	(0.076)	(0.094)	(0.093)	(0.087)	(0.112)
	0.407/0.069	0.722/0.090	0.722/0.094	0.144/0.067	0.210/0.084	0.519/0.083
CO	0.521**	0.392**	0.016	0.775**	0.319**	0.015
	(0.102)	(0.101)	(0.085)	(0.111)	(0.103)	(0.096)
	0.234/0.092	0.365/0.101	0.623/0.101	0.211/0.089	0.411/0.102	0.626/0.102
DR	0.302*	0.142	-0.048	0.168	0.121	-0.168
	(0.136)	(0.125)	(0.111)	(0.160)	(0.181)	(0.171)
	0.073/0.034	0.210/0.039	0.316/0.041	0.000/0.010	0.035/0.014	0.196/0.014
ME	0.663**	0.464**	0.129	0.546**	0.381**	0.030
	(0.107)	(0.103)	(0.114)	(0.097)	(0.101)	(0.147)
	.266/0.096	0.470/0.117	0.617/0.117	0.082/0.087	0.250/0.097	0.250/0.099
PA	0.399**	0.282*	-0.048	0.323*	0.212	-0.025
	(0.131)	(0.119)	(0.117)	(0.147)	(0.138)	(0.071)
	0.138/0.081	0.334/0.087	0.529/0.089	0.269/0.059	0.343/0.071	0.451/0.072
PE	0.582**	0.418**	0.067	0.695**	0.582**	0.224
	(0.128)	(0.110)	(0.099)	(0.144)	(0.132)	(0.116)
	0.214/0.058	0.356/0.076	0.479/0.076	0.276/0.089	0.467/0.108	0.467/0.109
VE	0.409*	0.360*	0.151	0.629**	0.530**	0.123
	(0.167)	(0.170)	(0.191)	(0.141)	(0.123)	(0.143)
	0.128/0.014	0.109/0.042	0.236/0.042	0.275/0.058	0.371/0.083	0.646/0.083

(I) — Controlling for school type and grade; (II) — Additional controls for student socioeconomic status and school location; (III) — Additional controls for peer group characteristics

Note: The first entry is the coefficient on the private school dummy variable; the second entry (in parentheses) is its standard error; the third entry is the proportion of between- and within-school variance explained. Regressions are weighted to correct for the over-sampling of certain school types. See Table 3 for sample sizes.

$*\quad p < 0.05$

$**\, p < 0.01$

Table 7: **Meta-Analysis of the Achievement Differences Between Public and Private Schools**

Model	**Language**	**Mathematics**
(I)	0.489**	0.510**
	(0.037)	(0.058)
(II)	0.341**	0.351**
	(0.034)	(0.038)
(III)	0.038	0.013
	(0.037)	(0.042)

Note: The first entry is the Bayesian mean of the country-level estimates; the second entry (in parentheses) is the standard error.

$*\quad p < 0.05$

$**\, p < 0.01$

Figure 1: **Achievement Difference Between Public and Private Schools in Language, Controlling for Student Background and Peer Group Characteristics**

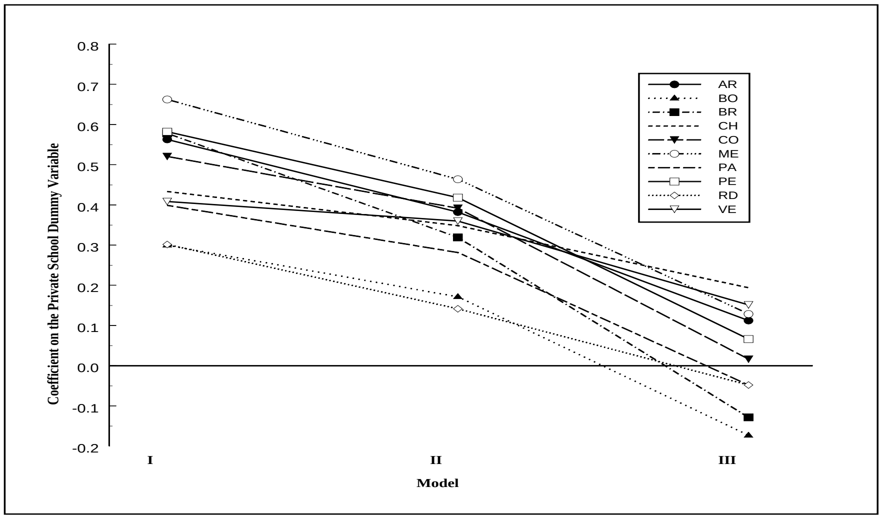

Figure 2: **Achievement Difference Between Public and Private Schools in Mathematics, Controlling for Student Background and Peer Group Characteristics**