How Effective Are Private Schools in Latin America?

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I. Introduction

Nearly half a century ago, Milton Friedman argued that parents should receive tuition coupons, or vouchers, allowing them to send their children to private schools rather than public schools.[1] Throughout the 1990s, his arguments were vigorously restated by scholars in the United States,[2] while others issued calls for vouchers in low-income countries.[3]

Voucher plans can vary widely in their scope and design.[4] 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 (SES) 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.[5]

The authors are grateful to Clive Belfield and Henry Levin for their comments. Opinions expressed herein are those of the authors.

[1] Milton Friedman, "The Role of Government in Education," in *Economics and the Public Interest*, ed. Robert A. Solo (New Brunswick, N.J.: Rutgers University Press, 1955).

[2] John E. Chubb and Terry M. Moe, *Politics, Markets, and America's Schools* (Washington, D.C.: Brookings Institution, 1990); Caroline M. Hoxby, "What Do America's 'Traditional' Forms of School Choice Teach Us about School Choice Reforms?" *Federal Reserve Bank of New York Economic Policy Review* 4, no. 1 (1998): 47–59; Paul E. Peterson and Bryan C. Hassel, *Learning from School Choice* (Washington, D.C.: Brookings Institution Press, 1998).

[3] Harry A. Patrinos, "Market Forces in Education," *European Journal of Education* 35, no. 1 (2000): 61–79; Edwin G. West, "Education Vouchers in Principle and Practice: A Survey," *World Bank Research Observer* 12, no. 1 (1997): 83–103.

[4] For example, vouchers may be offered to a restricted number of students based on criteria such as family income or residence, or they may be offered to every student attending a public school. For a discussion, see Henry M. Levin, "The Economics of Educational Choice," *Economics of Education Review* 10, no. 2 (1991): 137–58.

[5] 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 emphasizing, 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 Patrick J. McEwan, "The Potential Impact of Large-Scale Voucher Programs," *Review of Educational Research* 70, no. 2 (2000): 103–49.

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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, *World Education Report 2000* (UNESCO Publishing, 2000).

Before pursuing these policies, it is reasonable to inquire whether this assumption is empirically supported.[6] While much ink has been spilled over the U.S. case, there is less empirical evidence from low- and middle-income countries. This is unfortunate, because Latin America provides a fascinating institutional context in which to compare the relative effectiveness of private and public schooling. Many countries have already experimented with public funding for private schools. Argentina and Chile provide extensive subsidies to most private schools.[7] Not surprisingly, the private school market share is relatively larger in these countries (see table 1). Colombia provided secondary school vouchers to students who lived in poor neighborhoods in the 1990s.[8] A range of schemes in other countries divert public funds to private schools.[9]

[6] For reviews of the U.S. literature, see Helen F. Ladd, "School Vouchers: A Critical View," *Journal of Economic Perspectives* 16, no. 4 (2002): 3–24; Henry M. Levin, "Educational Vouchers: Effectiveness, Choice, and Costs," *Journal of Policy Analysis and Management* 17, no. 3 (1998): 373–91; McEwan, "The Potential Impact of Large-Scale Voucher Programs"; Patrick J. McEwan, "The Potential Impact of Vouchers: An Update," *Peabody Journal of Education* (forthcoming); Derek Neal, "How Vouchers Could Change the Market for Education," *Journal of Economic Perspectives* 16, no. 4 (2002): 25–44.

[7] Martin Carnoy, "National Voucher Plans in Chile and Sweden: Did Privatization Reforms Make for Better Education?" *Comparative Education Review* 42, no. 3 (1998): 309–37; Patrick J. McEwan and Martin Carnoy, "The Effectiveness and Efficiency of Private Schools in Chile's Voucher System," *Educational Evaluation and Policy Analysis* 22, no. 3 (2000): 213–39; A. Morduchowicz, A. Marcon, G. Iglesias, M. Andrada, J. Perez, V. Campan, and L. Duro, *La Educación Privada en la Argentina: Historia, Regulaciones y Asignación de Recursos Públicos* (unpublished manuscript, Buenos Aires, 1999).

[8] Joshua Angrist, Eric Bettinger, Erik Bloom, Elizabeth King, and Michael Kremer, "Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment," *American Economic Review* 92, no. 5 (2003): 1535–58.

[9] This includes *Fe y Alegría*, a program of the Catholic Church that operates schools in poor communities of several Latin American countries. Teacher salaries are generally paid by the government through negotiated agreements; see Marcela Latorre and John Swope, "Fe y Alegría: An Alternative Proposal for Primary Education in Latin America," in *Schooling for Success: Preventing Repetition and Dropout in Latin American Primary Schools*, ed. Laura Randall and Joan B. Anderson (Armonk, N.Y.: Sharpe, 1999). Occasionally, tax law provides indirect subsidies to families. In Brazil, 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; see Estelle James, Carlos A. Primo Braga, and Paulo de Tarso Afonso de Andre, "Private Education and Public Regulation," in *Opportunity Foregone: Education in Brazil*, ed. Nancy Birdsall and Richard H. Sabot (Washington, D.C.: Inter-American Development Bank, 1996).

In 1997, the Santiago office of UNESCO implemented an assessment of student achievement in Latin America, working in collaboration with 13 Latin American 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 article uses these data and multilevel modeling to assess the relative effectiveness of private and public schools in 10 of these countries. In particular, the article 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. 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 article, the mean private school effect is approximately zero, ranging between −0.2 and 0.2 standard deviations. The relative consistency of the findings is striking, given the diversity in the size and institutional features of the private sector across countries.

The following section describes prior research on the relative effectiveness of public and private schools in Latin America and identifies the key methodological challenges. Section III discusses the empirical strategy of this article, while Section IV describes the data. Section V discusses the general patterns to emerge from the multilevel findings and caveats to their interpretation, and Section VI concludes.

II. Prior Research

Table 2 summarizes the extant literature on private school effectiveness in Latin America.[10] Two questions should frame an evaluation of this literature. First, are the estimates of private school effects unbiased? That is, do they reflect the unique contribution of private schools to academic achievement, or do they also reflect the influences of unmeasured variables related to families? Second, are the standard errors of the estimates correct, allowing one to test whether private school effects are statistically different from zero?

[10] For recent studies in Africa and Asia, see Arjun S. Bedi and Ashish Garg, "The Effectiveness of Private versus Public Schools: The Case of Indonesia," *Journal of Development Economics* 61, no. 2 (2000): 463–94; Geeta G. Kingdon, "The Quality and Efficiency of Private and Public Education: A Case-Study of Urban India," *Oxford Bulletin of Economics and Statistics* 58, no. 1 (1996): 57–82; Gerard Lassibille and Jee-Peng Tan, "Are Private Schools More Effective than Public Schools? Evidence from Tanzania," *Education Economics* 9, no. 2 (2001): 145–69.

Each study in table 2 controls for observed student and family variables that may affect achievement. 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.[11] In the first step, researchers specify a probit 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, which explains achievement.

For this corrected equation to be credibly identified, however, the private school attendance model must contain at least one variable that is not included in the achievement model.[12] 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 because of the improper omission of variables from the achievement regression). Several studies implement corrections, although they sometimes make dubious choices about exclusion restrictions. For example, two studies include measures of SES in the model that explains private school attendance but exclude them from the achievement model.[13] In such cases, it is doubtful whether corrections ameliorate bias, and they may actually exacerbate it.[14] Other studies make a more compelling case for the validity of their exclusion restrictions.[15]

So far, J. Angrist, E. Bettinger, E. Bloom, E. King, and M. Kremer make

[11] James J. Heckman, “Sample Selection Bias as a Specification Error,” *Econometrica* 47, no. 1 (1979): 153–61; G. S. Maddala, *Limited-Dependent and Qualitative Variables in Econometrics* (Cambridge: Cambridge University Press, 1983).

[12] If an exclusion restriction is not imposed, one must rely on the nonlinearity of the parameters for identification. A failure to impose an exclusion restriction often results in regression estimates that are sensitive to small changes in model specification.

[13] Alejandra Mizala, Pilar Romaguera, and Teresa Reinaga, “Factores que inciden en el rendimiento escolar en Bolivia” (Serie económica no. 61, Centre de Economía Aplicada, Universidad de Chile, Santiago, 1999); Donald Cox and Emmanuel Jimenez, “The Relative Effectiveness of Private and Public Schools: Evidence from Two Developing Countries,” *Journal of Development Economics* 34 (1991): 99–121.

[14] More generally, the U.S. literature has become increasingly skeptical about the validity of common exclusion restrictions used to identify private school effects, such as Catholic religious status. For a discussion, see Joseph G. Altonji, Todd E. Elder, and Christopher R. Taber, “An Evaluation of Instrumental Variables Strategies for Estimating the Effects of Catholic Schools,” Working Paper no. 9358 (National Bureau of Economic Research, Cambridge, Mass., 2002).

[15] For example, Emmanuel Jimenez, Marlaine E. Lockheed, Eduardo Luna, and Vicente Paqueo, in “School Effects and Costs for Private and Public Schools in the Dominican Republic” (*International Journal of Educational Research* 15, no. 5 [1991]: 393–410), assume that local private school tuition levels in Dominican Republic affect private school attendance but not achievement. Patrick J. McEwan, in “The Effectiveness of Public, Catholic, and Non-religious Private Schools in Chile’s Voucher System” (*Education Economics* 9, no. 2 [2001]: 103–28), assumes that the local availability of private schools in Chile affects attendance but not achievement.

TABLE 2
PRIVATE SCHOOL EFFECTS IN LATIN AMERICA

Country and Studies	Year of Sample(s)	Grade Level at Posttest	Level of Analysis	Method	Controls	Exclusion Restriction	Difference in Standard Deviations (Type of Private School)
Argentina:							
McEwan*	1997	7	Student	OLS	Individual SES, peer SES	N.A.	Spanish: **.17** (Catholic subsidized) **.29** (nonreligious subsidized) .11 (nonsubsidized) Math: .07 (Catholic subsidized) **.27** (nonreligious subsidized) −.03 (nonsubsidized)
Bolivia:							
Mizala et al.†	1997	6	Student	OLS with selectivity	Individual SES	Sociocultural level of family, regional dummy variables, others	Spanish: **.22**
Brazil:							
Lockheed and Bruns‡	1988	Secondary	Student	HLM	Individual SES, peer SES	N.A.	Portuguese: **.55** Math: −.07
Chile:							
McEwan and Carnoy§	1990–96	4	School	OLS	Schoolwide SES	N.A.	Spanish: **.27** (Catholic subsidized) **−.07** (nonreligious subsidized) **.57** (fee-paying private) Math: **.22** (Catholic subsidized) **−.07** (nonreligious subsidized) **.57** (fee-paying private)
Mizala and Romaguera‖	1996	4	School	OLS	Schoolwide SES	N.A.	General achievement: **.19** (fee-paying private) .05 (private subsidized)
McEwan#	1997	8	Student	OLS with selectivity	Individual SES, peer SES	Density of private school supply	Spanish: **.18** (Catholic subsidized) .04 (nonreligious subsidized) **.48** (fee-paying private) Math: **.26** (Catholic subsidized) .02 (nonreligious subsidized) **.53** (fee-paying private)

Colombia:							
Psacharopoulos**	1981	Secondary	Student	OLS	Individual SES	N.A.	General achievement: **.20** (private academic)
Cox and Jimenez††	1981	Secondary	Student	OLS with selectivity	Individual SES	Parental education, father's occupation and income	General achievement: .58 (private academic)
Angrist et al.‡‡	1999	Secondary	Student	IV	Randomized assignment, individual SES	Voucher awarded to student	General achievement: **.29**
Dominican Republic:							
Jimenez et al.§§	1983	8	Student	OLS with selectivity	Individual SES, peer SES	Private school tuition	Math: −.55 (F-type schools) .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 coefficients were already standardized or the standard deviation was not reported. N.A. = not applicable to the study, SES = socioeconomic status, OLS = ordinary least squares, HLM = hierarchical linear modeling, and IV = instrumental variables. Another study on Chile was excluded because it interacted a private school dummy variable with eight independent variables—thus estimating nine private school effects for different and apparently arbitrarily chosen categories of schools (Taryn R. Parry, "Theory Meets Reality in the Education Voucher Debate: Some Evidence from Chile," *Education Economics* 5, no. 3 [1997]: 307–31.)

* Patrick J. McEwan, "Public Subsidies for Private Schooling: A Comparative Analysis of Argentina and Chile," *Journal of Comparative Policy Analysis* 4, no. 2 (2002): 189–216, esp. table 6.

† Alejandra Mizala, Pilar Romaguera, and Teresa Reinaga, "Factores que inciden en el rendimiento escolar en Bolivia" (Serie económica no. 61, Centre de Economía Aplicada, Universidad de Chile, Santiago, 1999), esp. table 3, model 4. 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 (R. Rosenthal, "Parametric Measures of Effect Size," in *The Handbook of Research Synthesis*, ed. H. Cooper and L. V. Hedges [New York: Russell Sage, 1994]).

‡ Marlaine E. Lockheed and Barbara Bruns, "School Effects on Achievement in Secondary Mathematics and Portuguese in Brazil" (WPS 525, World Bank, Washington, D.C., 1990), esp. table 5.

§Patrick J. McEwan and Martin Carnoy, "The Effectiveness and Efficiency of Private Schools in Chile's Voucher System," *Educational Evaluation and Policy Analysis* 22, no. 3 (2000): 213–39, esp. table 4.

‖ Alejandra Mizala and Pilar Romaguera, "School Performance and Choice: The Chilean Experience," *Journal of Human Resources* 35, no. 2 (2000): 392–417, esp. table 4, model 5.

Patrick J. McEwan, "The Effectiveness of Public, Catholic, and Non-religious Private Schools in Chile's Voucher System," *Education Economics* 9, no. 2 (2001): 103–28, esp. table 8. Results are from models that are not corrected for selection bias, since the hypothesis of no selection bias could not be rejected.

** George Psacharopoulos, "Public vs. Private Schools in Developing Countries: Evidence from Colombia and Tanzania," *International Journal of Educational Development* 7, no. 1 (1987): 59–67, esp. table 4, model 4.

†† Donald Cox and Emmanuel Jimenez, "The Relative Effectiveness of Private and Public Schools: Evidence from Two Developing Countries," *Journal of Development Economics* 34 (1991): 99–121; see p. 115. 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 Psacharopolous, 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.

‡‡ Joshua Angrist, Eric Bettinger, Erik Bloom, Elizabeth King, and Michael Kremer, "Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment," *American Economic Review* 92, no. 5 (2003): 1535–58; see table 8, model 3. 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).

§§ Emmanuel Jimenez, Marlaine E. Lockheed, Eduardo Luna, and Vicente Paqueo, "School Effects and Costs for Private and Public Schools in the Dominican Republic," *International Journal of Educational Research* 15, no. 5 (1991): 393–410, esp. table 3.7. The statistical significance of the effects could not be verified, because the standard errors of the predictions were not calculated.

the best attempt to deal with bias. To estimate the effects of offering vouchers to secondary students in Colombia, they rely on the fact that students were randomly awarded or denied private school vouchers. The use of randomized assignment ensures that each group of students is roughly similar, obviating the need for selection bias corrections.[16]

The private school effect is usually defined as the achievement difference between public and private schools, net of student ability, SES, and other family 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 SES of a school, are associated with higher achievement, all else equal.[17] 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—private school effects will partly reflect the more privileged status of peer groups.

Stephen W. Raudenbush and J. Douglas Willms label the typical definition—excluding controls for peer characteristics—the "type A effect" and the second the "type B effect."[18] 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 the school's favorable peer groups. On the other hand, the type B effect, which is meant to isolate the effect of school practices, resources, and efficiency, is most

[16] Angrist et al.

[17] For reviews of the literature, see Christopher Jencks and Susan E. Mayer, "The Social Consequences of Growing Up in a Poor Neighborhood," in *Inner-City Poverty in the United States*, ed. Laurence E. Lynn and Michael G. H. McGeary (Washington, D.C.; National Academy Press, 1990); Robert A. Moffitt, "Policy Interventions, Low-Level Equilibria, and Social Interactions," in *Social Dynamics*, ed. Steven N. Durlauf and H. Peyton Young (Cambridge, Mass.: MIT Press, 2001); and Patrick J. McEwan, "Peer Effects on Student Achievement: Evidence from Chile," *Economics of Education Review* 22, no. 2 (2003): 131–41. Like comparisons of public and private achievement, this literature faces challenges related to selection bias. While many studies find positive peer group effects (Vernon Henderson, Peter Mieszkowski, and Yvon Sauvageau, "Peer Group Effects and Educational Production Functions," *Journal of Public Economics* 10, no. 1 [1978]: 97–106; J. Douglas Willms, "Social Class Segregation and Its Relationship to Pupils' Examination Scores in Scotland," *American Sociological Review* 51 [1986]: 224–41), 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 (William N. Evans, Wallace E. Oates, and Robert M. Schwab, "Measuring Peer Group Effects: A Study of Teenage Behavior," *Journal of Political Economy* 100, no. 5 [1992]: 966–91). In Chile, some evidence indicates that such biases are not severe (McEwan, "Peer Effects on Student Achievement").

[18] Stephen W. Raudenbush and J. Douglas Willms, "The Estimation of School Effects," *Journal of Educational and Behavioral Statistics* 20, no. 4 (1995): 307–35.

relevant to policy makers and school officials when evaluating the performance of schools.

Some studies in table 2 control exclusively for individual characteristics, thus estimating a type A private school effect.[19] Note that randomized assignment, at least as implemented in Angrist et al., does not control for peer group characteristics. Although randomization is a convincing means of controlling for student and family characteristics, the estimates do not condition on the different peer groups to which voucher and nonvoucher students are exposed.[20] This article will suggest that type A private school effects may largely reflect the influences of being exposed to better peers.

Finally, 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 (OLS) formulae for standard errors are incorrect in the presence of clustering of students within schools, classrooms, households, or other units.[21] In most cases, the standard errors will be underestimated, leading to unwarranted findings of statistical significance.

There are two approaches to estimating correct standard errors. The first is multilevel modeling, which is increasingly used in educational research but is applied in only one of the studies in table 2.[22] Second, it is possible to adjust standard errors for clustering within the context of OLS regression.[23]

When studies use school-level data, they are not required to correct for clustering.[24] Other studies simply do not make corrections for clustering,

[19] Angrist et al.; Cox and Jimenez; Mizala, Romaguera, and Reinaga; and George Psacharopoulos, "Public vs. Private Schools in Developing Countries: Evidence from Colombia and Tanzania," *International Journal of Educational Development* 7, no. 1 (1987): 59–67. Other studies estimate models with and without peer variables; see Jimenez et al.; and McEwan, "The Effectiveness of Public, Catholic, and Non-religious Private Schools").

[20] This observation also applies to recent randomized evaluations of U.S. voucher programs; see William G. Howell and Paul E. Peterson, *The Education Gap: Vouchers and Urban Schools* (Washington, D.C.: Brookings Institution Press, 2002); and Alan B. Krueger and Pei Zhu, "Another Look at the New York City School Voucher Experiment," Working Paper no. 9418 (National Bureau of Economic Research, Cambridge, Mass., 2003).

[21] For lucid explanations, see Angus Deaton, *Analysis of Household Surveys: A Microeconometric Approach Development Policy* (Washington, D.C.: World Bank, 1997); Gustavo Angeles and Thomas A. Mroz, "A Simple Guide to Using Multilevel Models for the Evaluation of Program Impacts," technical report (University of North Carolina, Chapel Hill, 2001).

[22] Marlaine E. Lockheed and Barbara Bruns, "School Effects on Achievement in Secondary Mathematics and Portuguese in Brazil," Policy Research Working Paper 525 (World Bank, Washington, D.C., 1990).

[23] McEwan, "The Effectiveness of Public, Catholic, and Non-religious Private Schools," and "Public Subsidies for Private Schooling: A Comparative Analysis of Argentina and Chile," *Journal of Comparative Policy Analysis* 4, no. 2 (2002): 189–216. For methodological details, see W. H. Rogers, "Regression Standard Errors in Clustered Samples," *Stata Technical Bulletin* 13 (1993): 19–23, which generalizes from the robust standard error calculation of P. J. Huber, "The Behavior of Maximum Likelihood Estimates under Non-standard Conditions," in *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability*, vol. 1 (Berkeley: University of California Press, 1967).

[24] Alejandra Mizala and Pilar Romaguera, "School Performance and Choice: The Chilean Experience," *Journal of Human Resources* 35, no. 2 (2000): 392–417; McEwan and Carnoy (n. 7 above); Angrist et al.

thus casting some doubt on their inferences, quite apart from any issues of bias in estimates of private school effects.[25]

III. Empirical Strategy

This section describes the empirical strategy—multilevel modeling—that 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 regionwide effect of private schools.

Many types of data—and, in particular, educational data—have a nested or hierarchical structure. 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:

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

where Y is academic achievement, 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 X for each school j. In an 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 article, we will model the variation in the former but not the latter.[26] Our school-level model can therefore be expressed as:

$$\beta_{0j} = \psi_0 + Z_j\psi + u_{0,} \tag{2}$$

$$\beta_j = \phi_{0,} \tag{3}$$

where Z are school-level variables and $u_0 \sim N(0,\tau_0)$.[27] The variable 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.

[25] Cox and Jimenez (n. 13 above); Mizala, Romaguera, and Reinaga; Psacharopolous. At least one study corrects for heteroscedasticity, but this is not equivalent to correcting for clustering; see Cox and Jimenez.

[26] 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 modeled but are allowed to vary randomly across schools. When within-school sample sizes are small, as is the case with our data, slope estimates become unstable, and it is therefore more difficult to judge whether a slope varies significantly across schools; see Ita Kreft and Jan De Leeuw, *Introducing Multilevel Modelling* (London: Sage, 1998). 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 elected to use a random intercept model.

[27] The variable τ_0 represents between-school variation.

Substituting equations (2) and (3) into (1) yields

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

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 Z variable 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 (3 or 4) 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 (X), in order to assess whether the higher achievement of private schools arises, in part, from the higher SES of their students. Finally, Model III adds controls for the peer group characteristics of the school (part of Z) in order to evaluate whether the private school effect stems from different peer group characteristics. The exact variables are described in the next section.

Meta-analysis uses a multilevel framework to model variation among the estimated effects of different studies.[28] 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 $\varepsilon_c \sim N(0, V_c)$. The variable δ_c is a vector of the true achievement difference between public and private schools in every country, while d_c is a vector of the estimated achievement difference between the two sectors.[29] This equation therefore states that d_c estimates δ_c with a known sampling variance V_c.[30]

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

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

[28] For a detailed treatment, see Gene V. Glass, Barry McGaw, and Mary Lee Smith, *Meta-analysis in Social Research* (Beverly Hills, Calif.: Sage, 1981); Larry V. Hedges and Ingram Olkin, *Statistical Methods for Meta-analysis* (Orlando, Fla.: Academic Press, 1985).

[29] Both δ_c and d_c are standardized.

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

where $u_0 \sim N(0, \nu)$ and is the variation in the public-private achievement gap across countries. The variable 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.[31] This average achievement difference will be estimated for all models and outcomes.

IV. Data

Primer Estudio Internacional Comparativo

The analyses in this article 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 13 countries in 1997, this study was funded by the Inter-American Development Bank, the Ford Foundation, UNESCO, and the participating countries and was coordinated by UNESCO's Latin American Laboratory for Evaluation and Quality in Education.

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 every country, private schools were oversampled to allow for precise comparisons between public and private schools.[32]

Thirteen countries participated in the PEIC: Argentina, Bolivia, Brazil, Colombia, Costa Rica, Cuba, Chile, Dominican Republic, Honduras, Mexico, Paraguay, Peru, and Venezuela. However, only 10 of these countries were included in our analysis. The Costa Rica 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.[33] Honduras was also omitted because of its abundance of missing data, which reduced its sample to only 140 students spread over 27 schools.

The analysis also omits schools in rural areas. The rationale for doing so is threefold. First, the PEIC sampling strategy designated rural schools as a single, public-sector stratum, reflecting the fact that the rural sectors of most Latin American countries are overwhelmingly public. (In contrast, urban areas were divided into public and private strata.) Yet, in a few countries,

[31] The variable 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.

[32] For a technical overview of the study and descriptive analysis of the data, see UNESCO and Oficina Regional de Educación de la UNESCO para América Latina y el Caribe, "Primer Estudio Internacional Comparativo," technical report (Latin American Laboratory for Evaluation and Quality in Education, Santiago, 1998), and for the results of a multilevel analysis of the student- and school-level factors that affect achievement, see J. Douglas Willms and Marie-Andrée Somers, "Family, Classroom and School Effects on Children's Educational Outcomes in Latin America," *School Effectiveness and School Improvement* 12, no. 4 (2001): 409-45.

[33] Notwithstanding this exclusion, it should be noted that Cuban students scored about two standard deviations above those of other countries in the region; see Willms and Somers.

most notably in Chile, rural private schools are more common, and, hence, there may have been some miscoding of school sector in rural 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 introducing additional selection bias. Third, there are particularly acute problems with missing data among rural schools.

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.[34] This transformation expresses the achievement gap between public and private schools as a fraction of a standard deviation, facilitating comparisons across countries and with studies in table 2.

Table 3 describes the independent variables that measure student and family SES. They 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 and are generally consistent with variables used in other school effectiveness studies. Table 3 also describes peer group variables that are school-level means of student and family variables. Beyond these two subsets, we further control for gender and grade and for whether the school is located in a city (more than 5,000 inhabitants) or a megacity (more than 1 million inhabitants).

Table 4 shows that samples sizes decline substantially when cases that are missing values for one of the dependent or independent variables are excluded.[35] Fortunately, the proportion of private schools and students in each country remains fairly stable. This at least suggests 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 in the findings).

Descriptive Statistics

Table 5 presents the descriptive statistics for the dependent and independent 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 a standard deviation. The results also indicate that private

[34] 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).

[35] To partially address this problem, we impute the country-level mean for missing cases of selected independent variables (see table 3). Then, in regression analyses, we include a dummy variable denoting whether an observation in the original variable was missing, following Roderick J. A. Little and Donald B. Rubin, *Statistical Analysis with Missing Data* (New York: John Wiley and Sons, 1987).

TABLE 3
DEFINITION OF VARIABLES

Variable	Description
Student SES:	
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 10 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 she/he was younger (less than once a month, at least once a month, almost every day)
Peer group characteristics:	
SCHLSES‡	Average SES of the students in the school (where SES[b] 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 megacity 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)

NOTE.—SES = socioeconomic status.
* 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 zero and a standard deviation of one.
‡ Standardized by country to have a mean of zero and standard deviation of one.

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 10 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, de-

TABLE 4
DISTRIBUTION OF THE ORIGINAL AND FINAL SAMPLES BY SCHOOL SECTOR

	Original Sample*		Final Sample†	
	Students	Schools	Students	Schools
Argentina	3,701	102	2,286	95
	(21.6)	(21.6)	(24.4)	(22.1)
Bolivia	3,608	42	3,030	40
	(43.5)	(43.2)	(44.7)	(55.0)
Brazil	3,628	109	2,018	109
	(26.3)	(26.4)	(26.5)	(29.4)
Chile	3,449	88	1,200	86
	(45.4)	(49.2)	(53.3)	(45.3)
Colombia	3,095	85	2,215	85
	(34.4)	(35.3)	(36.0)	(32.9)
Dominican Republic	2,398	63	1,631	60
	(40.5)	(41.1)	(43.8)	(41.7)
Mexico	3,284	85	2,232	80
	(29.2)	(27.9)	(27.3)	(27.5)
Paraguay	3,053	79	1,237	65
	(45.1)	(45.5)	(44.9)	(46.2)
Peru	3,055	82	2,748	82
	(31.4)	(32.0)	(32.5)	(31.7)
Venezuela	2,875	92	912	50
	(28.7)	(28.1)	(28.9)	(28.0)
Region	32,146	827	19,509	752
	(34.4)	(34.2)	(35.5)	(34.3)

NOTE.—The first entry is the total number of observations; the second entry (in parentheses) is the percentage that is associated with the private sector (private schools were oversampled, 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.

spite diverse institutional contexts. They suggest that private schools succeed, on average, in skimming the most able and privileged students.[36]

As one would expect, there are also sharp differences in the average peer group characteristics of private and public schools. In all countries, the mean SES 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.

V. Results

Multilevel Models

Table 6 presents results from the multilevel analyses. It reports estimates of the private school dummy variable for each combination of dependent variable (language or math), country, and model specification (I, II, or III).[37] Across all countries and for both dependent variables, the unadjusted achieve-

[36] For similar evidence in the U.S., see David N. Figlio, "Can Public Policy Affect Private School Cream Skimming?" *Journal of Urban Economics* 49, no. 2 (2001): 240–66.

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

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	−.08	.49	.00	−.07	.27	.00	−.07	.46	.00	−.27	.24	.00	−.15	.34	.00
	(.98)	(.99)	(1.00)	(.98)	(1.01)	(1.00)	(.97)	(1.06)	(1.00)	(1.00)	(.94)	(1.00)	(.98)	(.97)	(1.00)
MATH	−.08	.47	.00	−.10	.39	.00	−.08	.48	.00	−.23	.21	.00	−.12	.29	.00
	(.97)	(1.04)	(1.00)	(.94)	(1.12)	(1.00)	(.95)	(1.16)	(1.00)	(.97)	(.98)	(1.00)	(.96)	(1.04)	(1.00)
Student SES:															
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	.83	.88	.84	.81	.84	.82	.80	.87	.81	.80	.86	.83	.70	.78	.73
	(.38)	(.33)	(.37)	(.39)	(.37)	(.39)	(.40)	(.34)	(.39)	(.40)	(.35)	(.38)	(.46)	(.42)	(.45)
TENBOOKS	.54	.83	.58	.39	.63	.44	.37	.74	.42	.43	.73	.59	.42	.68	.50
	(.49)	(.38)	(.49)	(.48)	(.48)	(.49)	(.48)	(.44)	(.49)	(.49)	(.44)	(.49)	(.49)	(.46)	(.50)
PARINVLV†	−.04	.27	.00	−.01	.05	.00	−.03	.17	.00	.03	−.02	.00	.05	−.11	.00
	(1.01)	(.86)	(1.00)	(1.01)	(.96)	(1.00)	(1.01)	(.90)	(1.00)	(1.07)	(.94)	(1.00)	(.98)	(1.04)	(1.00)
READING	1.09	1.30	1.12	.97	.99	.98	1.16	1.32	1.18	1.11	1.25	1.18	.93	1.01	.95
	(.79)	(.73)	(.78)	(.76)	(.73)	(.75)	(.82)	(.77)	(.81)	(.78)	(.73)	(.76)	(.77)	(.72)	(.76)
Peer group characteristics:															
SCHLSES†	−.16	1.05	.00	−.39	1.19	.00	−.28	1.48	.00	−.56	.70	.00	−.37	.96	.00
	(.91)	(1.01)	(1.00)	(.59)	(1.09)	(1.00)	(.72)	(1.02)	(1.00)	(.71)	(.86)	(1.00)	(.80)	(.84)	(1.00)
SCLPARNT†	−.11	.74	.00	.00	.00	.00	−.07	.38	.00	.05	−.06	.00	.19	−.48	.00
	(.94)	(1.12)	(1.00)	(1.00)	(1.06)	(1.00)	(.98)	(1.07)	(1.00)	(1.07)	(.91)	(1.00)	(.92)	(1.07)	(1.00)
DISCIP†	−.08	.55	.00	−.30	.93	.00	−.06	.34	.00	−.02	.03	.00	−.14	.37	.00
	(1.02)	(.66)	(1.00)	(.92)	(.59)	(1.00)	(1.01)	(.89)	(1.00)	(1.01)	(1.00)	(1.00)	(1.02)	(.86)	(1.00)
Other characteristics:															
URBAN	.91	.79	.89	.84	.55	.77	.83	.65	.80	.74	.49	.63	.70	.40	.62
	(.29)	(.42)	(.31)	(.37)	(.52)	(.43)	(.38)	(.49)	(.40)	(.45)	(.51)	(.49)	(.46)	(.50)	(.49)
FEMALE	.51	.42	.50	.55	.48	.53	.48	.50	.49	.52	.48	.50	.50	.47	.49
	(.47)	(.46)	(.47)	(.49)	(.49)	(.49)	(.50)	(.50)	(.50)	(.49)	(.49)	(.49)	(.50)	(.50)	(.50)
GRADE	.47	.45	.47	.49	.52	.50	.46	.44	.46	.47	.52	.50	.51	.51	.51
	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.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	−.06	.10	.00	−.06	.61	.00	−.09	.25	.00	−.12	.50	.00	−.06	.26	.00
	(.99)	(1.01)	(1.00)	(.99)	(.93)	(1.00)	(.99)	(.98)	(1.00)	(.96)	(1.02)	(1.00)	(.98)	(1.03)	(1.00)
MATH	−.01	.01	.00	−.05	.52	.00	−.09	.23	.00	−.14	.62	.00	−.13	.50	.00
	(.94)	(1.10)	(1.00)	(.99)	(.92)	(1.00)	(.96)	(1.06)	(1.00)	(.91)	(1.12)	(1.00)	(.97)	(.97)	(1.00)
Student SES:															
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	.73	.68	.71	.88	.90	.88	.79	.88	.82	.82	.85	.83	.67	.77	.69
	(.45)	(.47)	(.45)	(.33)	(.31)	(.32)	(.40)	(.32)	(.39)	(.38)	(.35)	(.38)	(.47)	(.42)	(.46)
TENBOOKS	.26	.41	.32	.31	.72	.35	.43	.67	.49	.29	.63	.35	.44	.65	.48
	(.43)	(.48)	(.45)	(.46)	(.45)	(.47)	(.49)	(.47)	(.49)	(.45)	(.48)	(.48)	(.49)	(.48)	(.49)
PARINVLV†	.03	−.04	.00	−.02	.19	.00	−.04	.10	.00	−.01	.06	.00	−.05	.19	.00
	(1.01)	(.98)	(1.00)	(1.00)	(.96)	(1.00)	(1.02)	(.93)	(1.00)	(1.02)	(.92)	(1.00)	(1.03)	(.83)	(1.00)
READING	1.11	1.23	1.16	.89	1.10	.91	1.04	1.15	1.07	.98	1.25	1.03	1.13	1.17	1.14
	(.78)	(.73)	(.76)	(.73)	(.74)	(.74)	(.75)	(.73)	(.75)	(.76)	(.69)	(.75)	(.74)	(.72)	(.73)
Peer group characteristics:															
SCHLSES†	−.27	.43	.00	−.15	1.53	.00	−.34	.87	.00	−.27	1.19	.00	−.27	1.11	.00
	(.52)	(1.39)	(1.00)	(.89)	(.76)	(1.00)	(.71)	(1.13)	(1.00)	(.79)	(.96)	(1.00)	(.81)	(.98)	(1.00)
SCLPARNT†	.18	−.28	.00	−.07	.68	.00	−.08	.21	.00	−.04	.18	.00	−.14	.57	.00
	(.89)	(1.12)	(1.00)	(.97)	(1.09)	(1.00)	(1.01)	(.96)	(1.00)	(1.04)	(.79)	(1.00)	(1.01)	(.76)	(1.00)
DISCIP†	−.04	.07	.00	−.07	.70	.00	−.08	.19	.00	−.09	.38	.00	−.11	.47	.00
	(1.08)	(.88)	(1.00)	(1.00)	(.75)	(1.00)	(.99)	(1.04)	(1.00)	(1.02)	(.84)	(1.00)	(.99)	(.93)	(1.00)
Other characteristics:															
URBAN	.69	.43	.59	.81	.72	.81	1.00	1.00	1.00	.72	.46	.67	.90	.76	.87
	(.47)	(.51)	(.50)	(.39)	(.49)	(.40)	(.00)	(.00)	(.00)	(.45)	(.52)	(.47)	(.30)	(.45)	(.34)
FEMALE	.55	.45	.51	.50	.55	.50	.51	.56	.53	.50	.49	.50	.56	.50	.55
	(.47)	(.48)	(.48)	(.49)	(.49)	(.49)	(.42)	(.45)	(.43)	(.49)	(.49)	(.49)	(.46)	(.48)	(.47)
GRADE	.50	.48	.49	.50	.49	.50	.51	.47	.50	.49	.49	.49	.41	.45	.41
	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.50)	(.49)	(.50)	(.49)

NOTE.—Pu = public school sector; Pr = private school sector; T = total; SES = socioeconomic status. The first entry is the variable mean; the second entry (in parentheses) is the standard deviation. Results are weighted to correct for the oversampling of certain school types. See table 4 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; however, our multilevel analyses used test scores that were standardized on the latter.

† Indices standardized by country to have a mean of zero and standard deviation of one.

TABLE 6
Achievement Differences between Public and Private Schools

	Language			Mathematics		
	I	II	III	I	II	III
AR	.563**	.382**	.113	.508**	.312*	−.053
	(.112)	(.099)	(.083)	(.147)	(.128)	(.117)
	.200/.062	.450/.084	.621/.084	.067/.092	.281/.120	.512/.119
BO	.300*	.172	−.173	.327	.236	−.065
	(.134)	(.132)	(.223)	(.162)	(.159)	(.273)
	.096/.001	.208/.028	.275/.028	.069/.000	.106/.012	.128/.012
BR	.578**	.319**	−.128	.631**	.412**	−.160
	(.111)	(.096)	(.092)	(.128)	(.113)	(.099)
	.258/.078	.478/.116	.679/.119	.256/.097	.467/.122	.711/.125
CH	.434**	.349**	.194*	.379**	.285**	.168
	(.083)	(.076)	(.094)	(.093)	(.087)	(.112)
	.407/.069	.722/.090	.722/.094	.144/.067	.210/.084	.519/.083
CO	.521**	.392**	.016	.775**	.319**	.015
	(.102)	(.101)	(.085)	(.111)	(.103)	(.096)
	.234/.092	.365/.101	.623/.101	.211/.089	.411/.102	.626/.102
DR	.302*	.142	−.048	.168	.121	−.168
	(.136)	(.125)	(.111)	(.160)	(.181)	(.171)
	.073/.034	.210/.039	.316/.041	.000/.010	.035/.014	.196/.014
ME	.663**	.464**	.129	.546**	.381**	.030
	(.107)	(.103)	(.114)	(.097)	(.101)	(.147)
	.266/.096	.470/.117	.617/.117	.082/.087	.250/.097	.250/.099
PA	.399**	.282*	−.048	.323*	.212	−.025
	(.131)	(.119)	(.117)	(.147)	(.138)	(.071)
	.138/.081	.334/.087	.529/.089	.269/.059	.343/.071	.451/.072
PE	.582**	.418**	.067	.695**	.582**	.224
	(.128)	(.110)	(.099)	(.144)	(.132)	(.116)
	.214/.058	.356/.076	.479/.076	.276/.089	.467/.108	.467/.109
VE	.409*	.360*	.151	.629**	.530**	.123
	(.167)	(.170)	(.191)	(.141)	(.123)	(.143)
	.128/.014	.109/.042	.236/.042	.275/.058	.371/.083	.646/.083

Note.—I, controlling for school type and grade; II, additional controls for student socioeconomic status and school location; III, additional controls for peer group characteristics. 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 oversampling of certain school types. See table 3 for sample sizes.

* $P < .05$.

** $P < .01$.

ment 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 SES 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 suggests 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

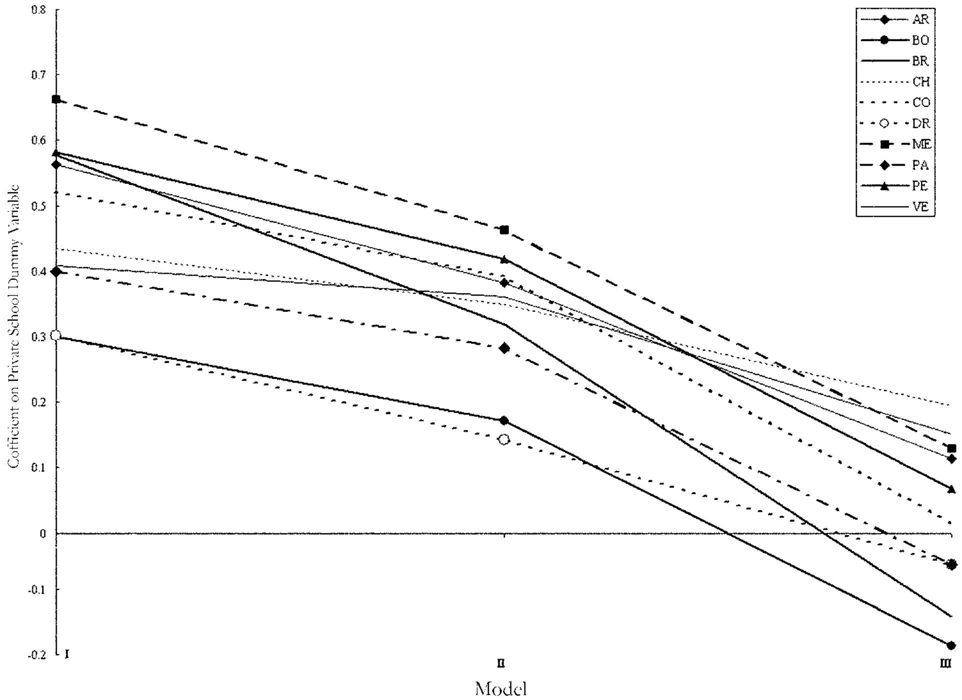

FIG. 1.—Achievement differences between public and private schools in language

explained begins at 20 percent (on average) in Model I, climbs to 33.7 percent in Model II, and rises to 51 percent in Model III. As for within-school variance explained, it only increases substantially in Model II. This is to be expected, given that the extra controls in Model III are school-level variables.

Figures 1 and 2 provide a visual summary of the results in table 6. They further emphasize three patterns in the results. First, the downward trend in the achievement gap across the three models is quite 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. 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 nonnegligible, despite their lack of statistical significance (see esp. the results for Bolivia, Dominican Republic, and Venezuela). In part, this is because of reduced sample sizes—particularly the number of schools in the sample—and the reduced statistical power that this implies.[38]

[38] Power is the probability of rejecting the null hypothesis when it is false. There are two key

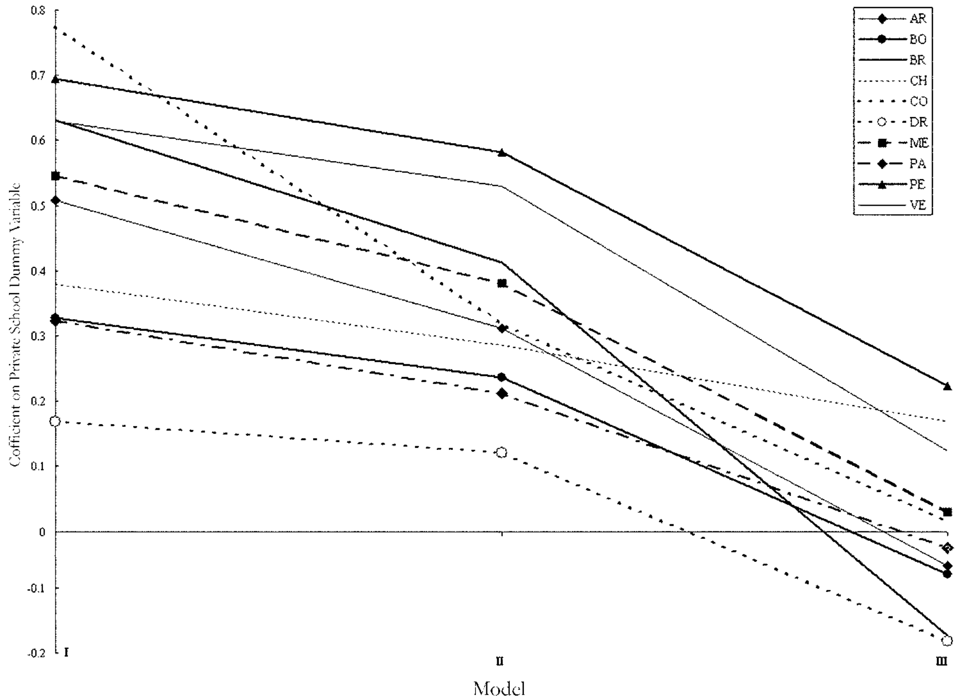

FIG. 2.—Achievement differences between public and private schools in mathematics

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 that are less reliably estimated 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

determinants of power in a multilevel context: sample size (both the number of schools and the number of observations within them) and intraclass correlation (which is the percentage of total variance that can be attributed to between-school variation). There have been attempts to determine proper guidelines for the sample size required to achieve a power of 0.90; see R. van der Leeden and F. M. T. A. Busing, “First Iteration versus IGLS/RIGLS Estimates in Two-Level Models: A Monte Carlo Study with ML3,” Leiden Psychological Report PRM 02-94 (Department of Psychometrics and Research Methodology, University of Leiden, 1994); K.-S. Kim, “Multilevel Data Analysis: A Comparison of Analytical Perspectives” (Ph.D. thesis, University of California, Los Angeles, 1990); D. Bassiri, “Large and Small Sample Properties of Maximum Likelihood Estimates for the Hierarchical Linear Model” (Ph.D. thesis, Department of Counseling, Educational Psychology and Special Education, Michigan State University, 1988). However, the suggested sample sizes are based on certain assumptions about intraclass 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, Dominican Republic, and Venezuela, given that these countries have the smallest number of schools in their sample.

TABLE 7
META-ANALYSIS OF THE ACHIEVEMENT DIFFERENCES BETWEEN PUBLIC AND PRIVATE SCHOOLS

Model	Language	Mathematics
I	.489*	.510*
	(.037)	(.058)
II	.341*	.351*
	(.034)	(.038)
III	.038	.013
	(.037)	(.042)

NOTE.—The first entry is the Bayesian mean of the country-level estimates; the second entry (in parentheses) is the standard error.
* $P < .01$.

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 figs. 1, 2), the summary estimates for both of the outcomes are similar in size.

Selection Bias

It is possible that estimates of private school effectiveness are biased by the exclusion of variables that are correlated with private school choice. To assess this possibility, we estimated a variety of additional models, following Heckman's two-step procedure.[39] For lack of appropriate variables, we did not impose an exclusion restriction. Thus, the identification of our model rested on the assumption that private school attendance is a nonlinear function of the independent variables. Given the frailty of this assumption, it is not surprising that the 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.[40]

Instead of appealing to sophisticated corrections, a simpler approach to ameliorating bias is to identify reasonable proxies for unobserved variables. In Chile, for example, Taryn R. Parry shows that 63 percent of private subsidized schools in Santiago use one of several methods, including entrance exams, interviews, and minimum grade requirements, to select students for

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

[40] A previous section noted that two studies reviewed in table 2 make a more compelling case for their exclusion restrictions. See Jimenez et al.; McEwan, "The Effectiveness of Public, Catholic, and Non-religious Private Schools" (n. 15 above). It is noteworthy that neither of these studies found strong evidence of selection bias (in Jimenez et al., we refer to the full specification, including peer variables, in their table 3.6.).

admission.[41] Similarly, Varun Gauri shows that 37 percent of students in private subsidized schools and 82 percent of students in private, fee-paying schools took exams in order to enroll in their present school.[42]

At least in Chile, therefore, 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 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.

The inclusion of peer variables in regressions may also proxy for some unobserved characteristics of students and families. This has been extensively noted in the economic literature on the estimation of peer effects.[43] For example, highly motivated families may choose schools with more educated families (better peer groups). If motivation is unobserved, then peer group status will partly reflect the influence of such unobserved variables. 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. The strong influence of peer variables in our study suggests that they are capturing at least some unobserved variance in family and student background.

As a final piece of suggestive evidence, one might compare our results to those of Angrist et al. in Colombia. They estimated a private school effect of 0.29 standard deviations, which is credibly unbiased, given their use of randomized assignment. Their model specification is roughly akin to our Model II, which does not control for peer group characteristics. In our sample, the estimated private school effect in Colombia is 0.32–0.39 standard deviations (referring to Model II).

In sum, there is indirect evidence that typical estimates of private school effects in Latin America are biased upward by selection. At least tentatively, then, one might regard the estimates from this study as an upper bound to the magnitude of private school effects.

VI. Conclusions

This article has sought to advance the literature on the relative effectiveness of private and public schooling in Latin America. Using UNESCO

[41] Taryn R. Parry, "Will Pursuit of Higher Quality Sacrifice Equal Opportunity in Education? An Analysis of the Education Voucher System in Santiago," *Social Science Quarterly* 77, no. 4 (1996): 821–41.

[42] Varun Gauri, *School Choice in Chile* (Pittsburgh: University of Pittsburgh Press, 1998).

[43] Evans et al. (n. 16 above); Moffitt (n. 16 above).

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 of a standard deviation (Model I). A small portion of these differences is accounted for by the higher SES of students in private schools (Model II). A quite substantial portion is explained by the varying peer group characteristics in private and public schools (Model III). After accounting for the peer group characteristics, the average private school effect across all 10 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. The consistency of the findings is striking, especially given the hetereogeneity in the size and composition of the private school sector across countries.

An important question is whether these results provide guidance on the impact of policies, such as vouchers, that encourage private school attendance. Despite the fact that they confound the effects of schools and peers, private school effects that do not condition on the SES of peer groups—as in 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 a government'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 privileged students that they happen to enroll.[44] This is especially so in the case of large-scale voucher programs that would extend eligibility to most students. 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.[45] This, in turn, might gradually attenuate private school effects that do not depend on peer group status. Hence, the estimates from Model II 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, which do not indicate a strong and consistent private school effect across countries.

[44] Chubb and Moe (n. 2 above).

[45] Chile provides the best example of this in Latin America. For direct evidence, see Chang-Tai Hsieh and Miguel Urquiola, "When Schools Compete, How Do They Compete? An Assessment of Chile's Nationwide School Voucher Program," Working Paper no. 10008 (National Bureau of Economic Research, Cambridge, Mass., 2003).