Competition and Public School Quality in Chile

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I. Introduction

School vouchers are government-funded tuition coupons that are redeemable at public or private schools.[1] In a seminal paper, Milton Friedman argued that vouchers would "permit competition to develop" if used on a large scale, thus leading to the "improvement of all schools."[2] Almost 40 years later, there have been few attempts to empirically test the argument, and none in education systems where vouchers have been implemented on a large scale (the small number of empirical studies that do examine this point—all with U.S. data—are reviewed in a later section).[3]

This paper assesses whether increasing competition from private schools, spurred by vouchers, elicits quality improvements from public schools. We focus on the case of Chile, where the military government instituted a national voucher plan in 1980—one of many social reforms inspired by the Chicago School of economics.[4] Not coincidentally, the Chilean system reflects many elements of Milton Friedman's voucher proposal. The reform decentralized the administration of public schools to municipalities, and began funding public and most private schools according to their monthly enrollments and a fixed per-pupil subsidy (or voucher, as it is frequently interpreted). Henceforth, school revenues were directly penalized for lost enrollments. The reform catalyzed the creation of new private schools, and a rapid growth in private enrollments. Some claim that this created incentives for struggling schools—mainly public—to improve their quality.

Our paper tests whether an increasing share of private enrollments in Chilean municipalities—a proxy for competition—produced gains in the academic achievement of fourth-grade students in public schools. The methodological challenges to doing so are substantial. Partial correlations between private enrollments and achievement, even controlling for background variables, are likely to provide biased estimates of the effects

of competition.[5] First, private enrollments may be correlated with unmeasured determinants of achievement. In Chile, for example, private enrollments tend to be higher in more privileged municipalities. If we do not perfectly control for municipal wealth or socioeconomic status—both likely determinants of achievement—then we confound the effects of competition and unmeasured municipal characteristics. Second, private enrollments and achievement may be simultaneously determined. That is, increasing private enrollments may lead to higher achievement in public schools, but low levels of achievement in public schools may encourage more parents to choose private schools or encourage private schools to locate in these communities.

We use two empirical strategies to address these biases. The first employs repeated observations on public schools between 1982 to 1996, including measures of mean fourth-grade achievement and the local share of private enrollments. We first-difference the data, akin to including fixed effects for each school, thus controlling for unobserved determinants of achievement that are constant across time for individual schools. By differencing the data a second time—a "difference-in-difference" approach—we further control for time-trends in each school's achievement. This empirical strategy does an adequate job of removing bias in competitive effects that is attributable to omitted variables. However, it will not purge estimates of simultaneity bias that may exist. Thus, a second set of estimates applies an instrumental variables approach.

Neither set of analyses suggests that competition produced higher academic achievement among fourth-graders in public schools. Yet, they also do not suggest that competition produced substantial losses. Even so, Chile's unique political context provides intriguing explanations for this pattern of results. As a byproduct of our analysis, we find that targeted central government investments in teacher training and classroom materials—referred to as the "P-900" program—produced consistent gains in achievement of around 0.2 standard deviations.

The paper is developed in the following manner. Section 2 reviews the base of empirical research on private school competition and public school quality, and section 3 describes the historical context of education reform. In section 4, we describe the empirical strategies of this paper in greater detail, followed in section 5 by a description of the data sets. The sixth section estimates and interprets several models that relate the private enrollment share in municipalities to public school achievement.

II. Evidence on Competition and Public School Quality

There is a small but growing literature that tests whether private school competition leads to improvements in public school outcomes.[6] These papers have much in common. First, they use the share of local enrollments in private schools as a proxy for the degree of competition in local schooling markets.[7] Second, they employ one or more cross-sections of U.S. data to correlate this proxy with a variety of student outcomes, as in the following linear model:

$$A_{ij} = \beta_1 C_{ij} + X_{ij}\beta_2 + \varepsilon_{ij} \tag{1}$$

where A is an outcome such as achievement (with i indexing individuals and j indexing schools), C is the private enrollment share confronting each student's school, and X is a vector of student control variables. Third, most allow for the potential endogeneity of private enrollments—due to omitted independent variables or simultaneity bias—by specifying a second linear model and estimating equations (1) and (2) by two-stage least squares:[8]

$$C_{ij} = Z_{it}\alpha_1 + X_{ij}\alpha_2 + \omega_{ij} \tag{2}$$

where Z is a vector of instrumental variables that are, ideally, strongly correlated with C but uncorrelated with ε.

Using instrumental variables, Couch and his colleagues find positive effects of competition.[9] However, their instruments includes several socioeconomic characteristics that probably belong in the school outcome regression. In a later paper, Newmark shows that their results are not robust to alternative model specifications.[10] Two authors have found that competition improves measures of student outcomes, using the local percentage of Catholics as instruments for the private enrollment share.[11] They assume, perhaps more reasonably, that private enrollments increase as the density of Catholic adherents increases in a given community, but that the Catholic population share is uncorrelated with unexplained variance in outcomes. However, other authors have been unable to identify competitive effects using a similar empirical strategy and a variety of alternative data sets.[12] Given the inconsistent results, one author believes that "the conclusions that can be drawn from the private school competition literature are limited."[13]

The present analysis advances the debate in two ways: (1) it uses data from a school system where public schools are financed by vouchers and where private schooling has undergone a large-scale expansion, and (2) it uses a rich variety of data, including repeated observations on public schools. The data facilitate the application of additional empirical methods, such as differencing, that can usefully address the problems of endogeneity.

III. Education Reform in Chile

1973-1980

At the time of the military coup d'etat in 1973, Chile's education system was one of the most developed in Latin America. It had achieved near-universal enrollment in primary education, a feat that still eludes much of Latin America.[14] A dirigiste Ministry of Education was responsible for administering the public schools. Even so, important

numbers of private schools operated, about half of these under the auspices of the Catholic Church.[15] Following a long tradition of public support of private education, many received partial subsidies from the national government that covered about 30 percent of costs in 1980.[16]

Upon assuming power in 1973, the military government disbanded the teachers union and fired teachers with leftist views.[17] It also initiated a massive administrative reorganization, dividing the country into 13 regions, and the latter into provinces and over 300 municipalities. General Pinochet appointed governors and mayors, drawn mainly from the ranks of the military.[18] During the 1970s the Ministry of Education, in addition to other ministries, devolved some powers to Regional Ministry Secretariats (SEREMIs) which were charged with administrative and supervisory duties formerly performed by the central ministry. Despite the apparent move towards decentralization, the system often functioned as a military chain of command, organized to implement central government directives.[19] Mayors of municipalities would not be elected democratically until 1992.

1980-1990

In 1980 the military government initiated a sweeping reform. It first transferred responsibility for public school management from the Ministry of Education to local municipalities.[20] Teachers lost their status as civil servants, reverting to municipal contracts, and schools buildings and land were signed over to municipal control. Initial transfers proceeded rapidly, encouraged by financial incentives, and by 1982, around 84 percent of schools were operated by municipalities.[21] The process was interrupted by economic crisis in 1982 when the central government was unable to cover the costs of transfers, though all schools were transferred by 1987.[22]

As schools were transferred to municipalities, public teachers were offered severance pay and became municipal rather than national employees.[23] Instead of conforming to the national *Escala Única de Remuneraciones,* their wages and working conditions were henceforth governed by the more flexible *Código de Trabajo*. Teachers lost guarantees of job security, the right to salary during vacations, standard wage scales, a 30 hour work-week, and the right to collectively bargain.[24] Teachers in private schools also lost some legal protections, including minimum wage guarantees and a system of annual salary adjustments.

Coupled with decentralization, the government altered how public and most private schools were financed. Prior to 1980, as in much of Latin America, school budgets were largely determined by the need to sustain an existing plant of teachers and facilities. If budgets adjusted in response to the level of student enrollments, they only did so at a sluggish pace. Under the reform, the Ministry of Education began disbursing monthly payments to municipalities based on a fixed voucher multiplied by the number of students enrolled in their schools; private schools received equivalent per-student payments if they did not charge tuition. Thus, payments to public or private schools began fluctuating in direct proportion to student enrollments.

The law established a base voucher level, which varies according the level of education and the location of the school.[25] Though the real value of the voucher was originally intended to keep pace with inflation, it was de-indexed following the economic crisis of the early 1980s. Over the course of the 1980s, as copper prices fell, the real value of the per-pupil voucher declined precipitously, reaching its lowest point in 1988. It rebounded thereafter with improved economic growth and has continued to rise since. The voucher plan precipitated a massive redistribution of enrollment across private and public schools. At the beginning of decade, around 15 percent of students were enrolled in private voucher schools, and almost 80 percent in public schools. By 1996 around 34

percent of enrollments were in private voucher schools. This growth occurred mostly at the expense of public schools (see Figure 1). Throughout this period, between five and nine percent of students enrolled in elite private schools that charged tuition.

1990-1996

The military ceded power to a democratic government in 1990. The form and function of Chile's voucher system were maintained, although new policies were grafted onto the system. The government focused on improving the quality of poor primary schools through direct investments. The "900 Schools Program," referred to as P-900, was targeted at high-poverty and low-achieving schools.[26] Classrooms received a package of basic teaching materials and infrastructure improvements, while teachers received additional in-service training. Funds were also provided to train local secondary graduates as tutors for the lowest achieving students. In 1992, the Program to Improve the Quality and Equity of Pre-primary and Primary Education (MECE) was initiated with World Bank financing. More ambitious in scope than P-900, it sought to endow all publicly-funded schools—whether publicly- or privately managed—with textbooks, libraries, and some infrastructure improvements.[27]

Obstacles to free-market reform

In many respects the market for educational services in Chile was transformed. In others, it fell short of the free-market blueprint envisioned by its architects.[28] These limitations were evident in at least three areas: (1) the financial incentives facing municipalities, (2) the teacher labor market, and (3) obstacles to market entry by private schools.

At several moments in the 1980s, a soft budget constraint may have dampened incentives for municipal schools to improve. In the context of falling enrollments and

voucher revenues, some municipalities began running deficits. This should have created incentives for local public school managers to cut costs, raise quality, and generally administer their existing funds more efficiently. Instead, it led many mayors to overspend and then solicit extraordinary outlays from the Ministry of Education, which they often received.[29] In 1985, 208 out of more than 300 municipalities received some central government support to cover deficits.[30] In 1986 and 1989, the government attempted to phase out deficits, but was not completely successful in eliminating the soft budget constraint.

The 1980 reform sought to eliminate regulations on hiring, firing, and remuneration of teachers. Nevertheless, a series of formal and informal pronouncements in the 1980s and 1990s scaled back some of these changes. Wary of mass unemployment during the economic crisis in the early 1980s, the military government forbade municipalities from firing teachers, despite falling student enrollments in public schools.[31] In a reversal, municipalities were told to dismiss around 8,000 teachers in 1986.[32] These decisions were made at high levels, without mayoral input.

The return to democracy in 1990 brought renewed political pressures from teachers seeking improved wages and working conditions, culminating in the passage of the 1991 *Estatuto Docente*. The law subjected teacher labor markets—particularly for public school teachers—to additional regulation.[33] Wage floors were set for teachers with various levels of experience and training, while restrictions on hiring and firing of public teachers were also introduced. Public school teachers could either be hired as tenured or contracted teachers.[34] Tenured teachers were to be hired through public contests in each municipality and severe restrictions were placed on their firing or reassignment. Contracted teachers had fewer restrictions placed on their hiring and firing, but could account for no more than 20 percent of a municipality's teacher workforce. The contracts

of private teachers were still governed by the *Código de Trabajo,* which permitted more flexibility in hiring and firing.

The rapid growth in the number of private schools suggests that many private entrepreneurs could enter the market. However, some restrictions on private school supply did exist. The 1980 law which initiated the reforms contains a clause specifying that Regional Ministry Secretatiats (SEREMIs) must certify that no "excess supply" exists in an area prior to authorizing a new private school.[35] The irony is that the voucher plan's architects intended that private schools should enter and directly compete for students, "excess" supply or not.

IV. Empirical Strategy

We use two empirical strategies to identify the effects of private school enrollment share on public school achievement: differencing and instrumental variables. The following two sections describe each.

Differenced estimates

We begin with the following linear model:

$$A_{it} = \delta_t + \beta_1 C_{it} + X_{it}\beta_2 + \upsilon_{it} \qquad (3)$$

where A is the mean achievement of school i at time t, C is the time-varying private enrollment share facing each school, and X is a vector of time-varying controls for each school. The model further includes a series of year-specific constants (δ) that capture nationwide trends in public school achievement. These trends could result from several factors: (1) year-to-year changes in the difficulty of the Spanish and mathematics exams, (2) a nationwide move towards "teaching the test" that might increase scores; or (3) the

effects of resource investments that are applied to all public schools, such as MECE (see the previous section).

A composite error term (υ) has three components:

$$\upsilon_{it} = \mu_i + \tilde{\mu}_i T + \omega_{it} \tag{4}$$

These include a school-specific, time-constant component (μ); a school-specific, constant time-trend component ($\tilde{\mu}$, with T indicating time); and a school-specific, time-varying component (ε).

The goal is to obtain unbiased estimates of β_1, the marginal effect of the private enrollment share on public school achievement. To provide baseline estimates, we estimate equation (3) by ordinary least squares. However, this is likely to yield biased estimates of β_1 because C is correlated with error term (for reasons described in the introduction). To address the bias, we estimate equation (3) in first-differenced form. This removes the school-specific, time-constant error component (μ). By doing so, we control for unobserved, time-constant features of individual schools. It is possible that unobserved determinants of achievement are not constant over time, and that their omission as regressors will bias the results even after first-differencing. Since we have more than two periods of data, we are able to difference the data a second time—a "difference-in-difference" approach. This eliminates the school-specific, constant time-trend component of the error term ($\tilde{\mu}$).[36] It controls for steady changes in each school's achievement, perhaps due to a constant increase or decline in unobserved neighborhood wealth or socioeconomic status of pupils.

IV estimates

While the prior specifications may diminish omitted variables bias, they do not purge estimates of simultaneity bias. Thus, a further set of estimates will apply instrumental variables. We again use equation (3), treating C as endogenous by specifying equation (5) and estimating both via two-stage least squares:

$$C_{it} = \eta_t + \alpha_1 Z_{it} + X_{ij}\alpha_2 + \omega_{it} \tag{5}$$

where η_t are year-specific dummies, Z is the instrument for private enrollment share, and X is a vector of control variables. As an instrument, we use the natural log of the municipal population density.

Three issues merit further discussion. First, the IV analysis is not combined with differencing (even though, in principle, this is possible). In exploratory analyses, there was little temporal variation in the instrument, despite extensive cross-sectional variation. Hence, the partial correlation between population density and private enrollment share was statistically insignificant in the differenced data, rendering the instrument invalid. Instead, we simply apply IV to the undifferenced data, while still including year-specific dummy variables.

Second, we do not have data on local population densities in earlier years of the data set, which reduces the number of observations used in the IV estimates. Despite this drawback, we can include a richer set of control variables that are only available in recent years. These data are described in section 5.

Third, the appropriateness of the instrument rests on two assumptions: that it is strongly correlated with private enrollment share, and that it is uncorrelated with unexplained achievement. While the first assumption is ultimately an empirical question, there are two reasons to suspect that it will hold. In Chilean surveys, like many other countries, parents express a strong preference for enrolling their children in schools that are close to the home.[37] This is likely due to the substantial opportunity costs of sending

children to distant schools. Therefore, private schools that locate in densely populated communities are likely to have access to a larger potential market.

There are also institutional reasons to suspect that densely-populated communities will have a larger share of private enrollments. In section 3, we described how the Chilean government promulgated a law that restricted private school entry in communities with "excess supply." There is little evidence on how and when this legal provision was applied. Nonetheless, it is reasonable to presume that authorities were less likely to find "excess supply" in rapidly growing cities that were receiving a steady influx of rural migrants. In contrast, many isolated rural communities, already served by one school, probably lacked the market share to sustain a second. To some extent, authorities may have sought to insulate these schools from competition, given the financial costs of maintaining a struggling school and the political costs of closing one. Thus, population density may be a good proxy for the application of the law and, perhaps, a good source of exogenous variation in private school enrollment share.

Even if population density is correlated with the enrollment share, it must be uncorrelated with unexplained variation in achievement. In our analysis, the danger is that population density is a proxy for unobserved determinants of achievement. This is further addressed in the presentation of results.

V. Data

Differenced estimates

Tables 1 and 2 provide definitions and descriptive statistics of the variables. The *Programa de Evaluación del Rendimiento Escolar* (PER) administered achievement tests in Spanish and mathematics to the majority of fourth-graders between 1982 and 1984.[38] Between 1988 and 1996, the *Sistema de Evaluación de Calidad de la Educación* (SIMCE) administered tests to fourth-graders in even years. Each source reports the

mean achievement of fourth-graders in each public school, as well as a categorical indicator of each school's socioeconomic status.

The private enrollment share in each municipality was obtained from enrollment files of the Ministry of Education. The municipal enrollment share is likely to be a good measure of the competitive environment facing public schools. For example, the Metropolitan Region—dominated by the sprawling capital of Santiago—is composed of more than 50 autonomous municipalities. Each municipality manages its own public schools and other public services. Thus, governments are likely to view the success of private schools within their borders as a measure of local competition.

We include a dummy variable indicating whether a public school is managed by the national government instead of local municipalities. Most schools were immediately decentralized to municipalities following the 1980 reforms. As described in section 3, the economic crisis of the early 1980s temporarily halted this process, and around 11 percent of the schools in the PER sample were managed by the national government until 1986. This permits an assessment of whether decentralization improved achievement.

Finally, we include a single measure of schooling resources: whether a school participated in the P-900 program. Beyond constraints imposed by limited data, there is a clear rationale for limiting the number of school resource variables. If competition increases in a municipality, then public schools might respond by choosing a new mix of schooling inputs (e.g., increasing the number of textbooks or altering the class size). Viewed in this light, the typical measures of schooling inputs are endogenous and thus candidates for exclusion from regressions. Their inclusion would prevent the estimation of an "overall" effect of competition. In a few cases, however, changes in school resources are clearly exogenous and uninfluenced by local competition. As described in Section 3, the Ministry of Education's P-900 program funded investments in poor schools, and participation was determined at the national level.

IV estimates

The IV estimates employ much the same data, though with two key differences. First, the sample uses a smaller number of years for reasons mentioned in the last section. Second, a wider array of control variables are used, including more detailed measures of parental socioeconomic status and educational attainment. Details are provided in Tables 1 and 2.

VI. Results

This section proceeds in several steps. It first provides additional background on the level and growth of private enrollment shares. The following two sections interpret results from the differenced and IV estimates, while a final section explores several explanations for the pattern of results.

Private enrollment shares in Chile

Figure 2 shows kernel densities of the private enrollment share that confronts each public school in the sample, focusing on three cross-sections: 1982, 1990, and 1996. In addition to the graph for the full sample, separate graphs are provided for the Metropolitan Region—including the capital of Santiago—and the rest of the country. It provides a simple illustration of several stylized facts about private school enrollments in Chile.

First, the level of private enrollment share is generally high in comparison with other countries, even in the early years of the sample. In 1982, for example, the mean enrollment share is about 25%. Second, there is substantial cross-sectional variance in the private enrollment share. Again looking at 1982, there are public schools in communities with shares that range between zero and more than 50%. Third, the

enrollment shares are, on average, higher in the Metropolitan Region than in Chile's other 12 regions. This holds true for all three years. Outside the Metropolitan Region, there are even a number of schools located in communities with zero private enrollments (shown by the peak on the left tail of the distribution). Fourth, the distributions for successive years tend to shift rightward, indicating a growth in the enrollment share. The shift is especially pronounced between 1982 and 1990, and more moderate between 1990 and 1996. Finally, the shift is more substantial in the Metropolitan Region.

In sum, there is substantial variance in private enrollment shares, both within cross-sections and across time. Even so, much of the growth appears to have occurred in the 1980s, and the level and growth of enrollments in the Metropolitan Region is notably higher. In light of this, the following sections will always provide separate results for the Metropolitan and non-Metropolitan areas in addition to full sample results.

Differenced estimates

Tables 3 and 4 present regression estimates using Spanish and mathematics, respectively, as dependent variables. Each table reports three specifications: (1) ordinary least squares, (2) first-differenced, and (3) difference-in-difference.

In the full sample, the OLS coefficients on private enrollment share are positive, albeit rather small in magnitude (see model 1 in Tables 3 and 4). Consider that the average public school in the sample faced a private enrollment share of about 21 percent in 1982, increasing to 28 percent by 1996 (see Table 2). Given the coefficient on PERPRIV in the Spanish regression, this implies an increase in mean achievement of only 0.3 percentage points. Nonetheless, there is good reason to suspect that the coefficients are biased.

The OLS coefficients on the variable indicating participation in the P-900 program are strongly negative. Either the program actually diminishes achievement, or its presence is confounded with unmeasured characteristics of schools and students that lower

achievement.[39] The allocation of P-900 to primary schools was clearly not random. Rather, it was based on subjective judgments of program personnel—who favored high-poverty schools—as well as measured school characteristics. Because our controls for school SES are rudimentary, it is probable that the negative program effects are reflecting the poor background of students in P-900 schools. The OLS specification does not suggest that national instead of local management of schools is associated with achievement. The coefficients on SES, however, indicate that higher-SES schools obtain much higher averages on the fourth-grade Spanish and mathematics tests.

The first-differenced estimates present a number of contrasts. The coefficients on PERPRIV turn negative and statistically significant. The dif-in-dif estimates do not drastically alter this result, although coefficients are estimated less precisely. In both first-differenced and dif-in-dif specifications, the coefficients on P900 are strongly positive, suggesting that participation in the P-900 program raises mean fourth-grade achievement in Spanish or mathematics by just over 2 percentage points, or roughly 0.2 of a standard deviation. As in OLS estimates, there is no evidence that public schools managed by the national Ministry of Education are less effective than those managed by municipalities. Finally, coefficients on SES dummy variables become smaller and are estimated with less precision. This is not surprising, given the limited temporal variation in these variables. By differencing, we control for the socioeconomic status that is measured by these dummy variables.

Tables 3 and 4 also present regressions estimated in two subsamples: the Metropolitan Region and other regions. Focusing on the differenced estimates, the coefficients on PERPRIV are not statistically significant in the Metropolitan sample. In other regions, the coefficients are negative and statistically significant. Notwithstanding their statistical significance, the magnitudes are not large. In the non-metropolitan sample, the average

enrollment share increased from 19 to 23 percent between 1982 and 1996. This implies a decline of about 0.4 percentage points in Spanish and mathematics, or less than 10 percent of a standard deviation.

Instrumental variable estimates

First-stage results. Table 5 reports the first-stage results from the IV procedure, using the private enrollment share as the dependent variable. In all three models, the coefficient on LOGDENS is positive and highly significant. The full sample coefficient implies that a one percent increase in population density will lead to a 0.05 percentage point increase in the private enrollment share. The other results are generally suggestive that enrollment shares are higher in communities when local public school parents are more privileged. SESINDEX, a measure of family "vulnerability," is scaled between 0 (least vulnerable) to 100 (most vulnerable). A 28-point increase (or one standard deviation) implies a reduction in the private enrollment share of 0.7 percentage points.

The validity of the IV estimates hinges on whether variation in population density identifies exogenous variation in the enrollment share. This is not the case, for example, if population density is merely serving as a proxy for unobserved family or student characteristics that belong in the second-stage regression. Because we have a single instrument, formal overidentification tests are not possible. Instead, we tested whether LOGDENS was correlated with achievement, conditional on the other independent variables.[40] For Spanish, the coefficient on LOGDENS was not statistically different from zero ($p=0.8$). For mathematics, however, the coefficient was positive and statistically significant ($p<0.05$). Given the latter, we only report second-stage results for Spanish achievement.

Second-stage results. In the full sample, both the OLS and IV coefficients on PERPRIV are negative, statistically significant, and similar in magnitude. Moreover, the

IV point estimate in the full sample is not markedly different from the differenced result (see Table 3). Disaggregating the sample does not fundamentally alter this conclusion. Both the Metropolitan and non-Metropolitan coefficients on PERPRIV are negative, although neither is statistically significant.

The other coefficients hold no surprises. The P-900 coefficient is strongly negative, and probably confounded with unobserved socioeconomic status. Variables that measure parental education and socioeconomic status are consistent with a large literature on educational production functions in developing countries, as well as prior research on Chile.[41]

Explaining the results

Neither empirical approach was successful in identifying positive effects of competition on academic achievement. There is some limited evidence of negative effects, particularly outside the Metropolitan region. Nevertheless, the magnitudes of these effects were never large. Given the constraints of our data, we cannot fully explain these patterns. Yet, it would be mistaken to take them as evidence "against" vouchers. Rather, they must be interpreted in Chile's particular institutional context.

There are compelling reasons why public school achievement may not have responded to added competition in the Chilean context. First, it is possible that some public schools lacked adequate incentives to improve, even when confronted by declining voucher revenues. Section 3 described how some municipalities responded to competition by lobbying the national government for additional funding to maintain the existing plant of teachers and infrastructure. A soft budget constraint may have encouraged some schools to focus more intently on lobbying for deficit coverage than on quality improvement.

Second, constraints on resource allocation may have limited the ability of public schools to improve their quality, even if prodded by adequate incentives. Section 3 also

described how resource allocation in public schools—especially related to the hiring, firing, and remuneration of teachers—was nationally regulated during the 1980s and 1990s. Still other constraints were imposed directly by municipalities. Though schools were "decentralized" in 1980, the allocation of school budgets and personnel was firmly in the hands of municipal governments. Individual public schools had very little control over these resources, especially when compared with private schools. Using data from a 1997 survey of Chilean principals, Table 7 suggests that public schools have almost no autonomy in the management of these resources. Particularly in larger municipalities, it is questionable as to whether the local bureaucracy was sufficiently agile to improve quality at the school level. The lack of school-level autonomy also provides a ready explanation as to why our results failed to uncover effects of public school decentralization. The 1980 reforms merely transferred rigid, centralized management to a lower level of government.

Third, one must admit the possibility that some public schools were not, on average, operating at substantially less than maximum effectiveness even prior to the implementation of vouchers. Thus, there was little room for increasing competition to spur improvements in public school quality. In several cross-sections of data from the 1990s—after the voucher system had operated for some time—we found that public schools and most voucher schools are similarly effective, once controls are made for student background and selection bias.[42] Yet, this begs an important question: if public schools were not less effective than private schools, then why did so many families reveal a clear preference for private schooling during the 1980s and 1990s? One answer is that families opted for private schools because their children would attend classes with peer groups of a higher ability or socioeconomic status. Given a choice between public and private schools of similar effectiveness, it is plausible that families would choose the

more privileged peer group, especially if peer effects stood to improve their own children's outcomes.

VII. Conclusions

Using two empirical strategies, we did not find positive effects of competition on public school achievement. Complementing these findings, there is no evidence that the achievement of nationally-managed schools is any different from that of schools managed by local municipalities. Finally, the differenced estimates suggest that the P-900 program had positive effects of roughly 0.2 standard deviations on Spanish and mathematics achievement.

The results do not provide strong support for the notion that competition will lead to improvements in the quality of public schools (and, to some extent, they parallel the rather mixed findings of U.S. research). Nevertheless, we should avoid the facile interpretations that are the hallmark of this literature: that competition "works" or "does not work." A nuanced examination of the Chilean political context suggests that the system may have dampened incentives for public schools to compete, or constrained the efforts of public schools to improve. The Chilean experience demonstrates that politicians and interest groups will seek to alter, often with great success, the form and function of voucher policies. This modifies the incentives and constraints faced by public school managers and, ultimately, the effects that vouchers may have on student outcomes. A cogent lesson from Chile is that an economic understanding of vouchers and competition cannot divorce itself from the politics of school choice.

Notes

1. Voucher policies might differ according to the amount of the voucher, whether private schools are permitted to charge "add-on" tuition payments, whether vouchers are restricted to low-income students, and whether all schools (e.g., religious) are allowed to accept them. For a detailed discussion, see Henry M. Levin, "The Economics of Educational Choice," Economics of Education Review 10 (1991): 137-158.

2. Milton Friedman, Capitalism and Freedom (Chicago: University of Chicago Press, 1962), p. 93.

3. There is a substantial body of research that compares the outcomes of students in public and private schools, occasionally in the context of small-scale voucher programs such as those of Colombia or Milwaukee. For a review of the experimental and non-experimental evidence, see Patrick J. McEwan, "The Potential Impact of Large-Scale Voucher Programs," Review of Educational Research 70 (2000): 103-149. This evidence is helpful in assessing whether students who use vouchers to attend private rather than public schools might reap benefits in the form of higher outcomes. Nonetheless, these studies are not designed to tell us whether vouchers encourage competition that improves the outcomes of students who remain in public schools.

4. Varun Gauri, School Choice in Chile: Two Decades of Educational Reform (Pittsburgh: University of Pittsburgh Press, 1998); Juan Gabriel Valdes, Pinochet's Economists: The Chicago School in Chile (Cambridge: Cambridge University Press, 1995).

5. For a similar discussion see Thomas S. Dee, "Competition and the Quality of Public Schools," Economics of Education Review 17 (1998): 419-427; Christopher Jepsen, "The

Effects of Private School Competition on Student Achievement," mimeographed (Evanston: Northwestern University, Department of Economics, 1999).

6. Richard Arum, "Do Private Schools Force Public Schools to Compete?" American Sociological Review 61 (1996): 29-46; Jim F. Couch, William F. Shughart, and Al F. Williams, "Private School Enrollments and Public School Performance," Public Choice 76 (1993):301-312; Dee; Caroline Minter Hoxby, "Do Private Schools Provide Competition for Public Schools?" (Working Paper No. 4978, National Bureau of Economic Research, 1994); Jepsen; Robert McMillan, "Parental Pressure and Private School Competition: An Empirical Analysis of the Determinants of Public School Quality," mimeographed (Stanford: Stanford University, Department of Economics, 1998); Craig M. Newmark, "Another Look at Whether Private Schools Influence Public School Quality," Public Choice 82: 365-373; William Sander, "Private Schools and Public School Achievement," Journal of Human Resources 34 (1999): 697-709.

7. The definition of "local" varies according to the study (e.g., MSA, county-wide, district-wide, etc.). Among empirical studies, Jepsen's is notable for its use of several different measures in testing for the effects of competition.

8. Of the studies referred to in note 6, only Arum's assumes that private enrollments are exogenous.

9. Couch, Shugart, and Williams.

10. Newmark.

11. Dee; Hoxby.

12. Jepsen; McMillan; Sander.

13. Jepsen, p. 20.

14. Tarsicio Castaneda, Combating Poverty: Innovative Social Reforms in Chile During the 1980s (San Francisco: ICS Press, 1992); Ernesto Schiefelbein, "Restructuring

Education through Economic Competition: The Case of Chile," Journal of Educational Administration 29 (1991):17-29.

15. Viola Espinola, "The Educational Reform of the Military Regime in Chile: The System's Response to Competition, Choice, and Market Relations" (Ph.D. dissertation, University of Wales, 1993).

16. Osvaldo Larrañaga, "Descentralización de la Educación en Chile: Una Evaluación Económica" (Educational Decentralization in Chile: An Economic Evaluation), Estudios Públicos (1995): 243-286.

17. Taryn Rounds Parry, "Achieving Balance in Decentralization: A Case Study of Education Decentralization in Chile," World Development 25 (1997): 211-225.

18. Frances Stewart and Gustav Ranis, "Decentralization in Chile" (Occasional Paper No. 14, Human Development Report, 1994).

19. Parry; Stewart and Ranis.

20. Once transferred to municipalities, schools were placed under the control of one of two kinds of institutions. Most opted to manage their schools with a *Departmento de Administración de la Educación Municipal* (DAEM). DAEMs exist under the larger umbrella of the municipal bureaucracy and, as such, are governed by municipal rules. Corporations are non-profit organizations that are not subject to direct mayoral control, though the mayor does preside over a governing board. Their operations are generally subject to fewer regulations. In contrast to DAEMs, the corporation head is not required to be a teacher and corporation employees are not subjected to municipal rules regarding the hiring and remuneration of municipal employees.

21. Municipalities received an overhead grant of three to five percent on total municipal wages and salaries as an inducement to begin administering schools (Parry, 1997; Winkler & Rounds, 1996).

22. Gerardo Jofré, "El Sistema de Subvenciones en Educación: La Experiencia Chilena" (Educational Subsidies: The Chilean Experience), Estudios Públicos (1988): 193-237.

23. Castaneda.

24. Patricio Rojas, "Remuneraciones de los Profesores en Chile" (Teacher Pay in Chile), Estudios Públicos (1998): 121-175.

25. Chilean law specifies a factor by which the base voucher is adjusted for students at every grade level. Furthermore, selected municipalities receive "zone assignments" to compensate for high poverty or isolation. Since 1987, rural schools within municipalities have received upward adjustments. For details, see Parry.

26. Juan Eduardo García-Huidobro, "Educational Policies and Equity in Chile," Unequal Schools, Unequal Chances: The Challenges to Equal Opportunity in the Americas, ed. F. Reimers (Cambridge: Harvard University Press, 2000).

27. Cristián Cox, "La Reforma de la Educación Chilena: Contexto, Contenidos, Implementación" (Chilean Education Reform: Context, Content, Implementation) (Programa de Promoción de la Reforma Educativa en América Latina, 1997).

28. For an extended discussion, see Gauri, esp. chapter 2.

29. On experiences of municipal deficit-coverage in the 1980s, see Jofré; Gauri.

30. Jofré.

31. Jofré; Gauri.

32. Gauri.

33. Rojas.

34. Chilean Spanish makes the distinction between *titulares* and *contratados.*

35. Jofré.

36. For similar empirical approaches, see Caroline Minter Hoxby, "How Teachers' Unions Affect Education Production," Quarterly Journal of Economics 111 (1996): 671-

718; Susanna Loeb and Marianne E. Page, "Examining the Link Between Teacher Wages and Student Outcomes: The Importance of Alternative Labor Market Opportunities and Non-pecuniary Variation," Review of Economics and Statistics 82 (2000): 393-408.

37. In a survey of eighth-grade parents, the most frequently-cited determinant of school choice was "proximity" (35 percent of parents). "Test scores" were cited by 9 percent of parents. For details, see Patrick J. McEwan, "Choice Between Private and Public Schools in a Voucher System: Evidence from Chile," mimeographed (Stanford: Stanford University, School of Education, 1998).

38. The original PER data tapes were damaged, but we obtained the printed volumes, allowing us re-code the data. In collaboration with a team from the Universidad de Chile, we then combined each school's data with a unique numeric code used by the Chilean Ministry of Education to identify schools. These codes are essential to attach each school's achievement data for 1982-84 with those of later years. Not all schools could be so identified, and we were forced to eliminate roughly 15-25% of the available observations between 1982 and 1984. We are grateful to David Bravo and his colleagues for their contribution to the process of data entry and cleaning.

39. Several authors have shown that a failure to account for program allocation based on unobserved characteristics can bias estimates of program effects. In particular, see Mark M. Pitt, Mark R. Rosenzweig, and Donna M. Gibbons, "The Determinants and Consequences of the Placement of Government Programs in Indonesia," The World Bank Economic Review 7 (1993): 319-348; Mark R. Rosenzweig and Kenneth I. Wolpin, "Evaluating the Effects of Optimally Distributed Public Programs: Child Health and Family Planning Interventions," American Economic Review 76 (1986): 470-482.

40. These results are available from the authors.

41. Bruce Fuller and Prema Clarke, "Raising School Effects While Ignoring Culture?" Review of Educational Research 64 (1994): 119-157; Patrick J. McEwan and Martin Carnoy, "The Effectiveness and Efficiency of Private Schools in Chile's Voucher System," Educational Evaluation and Policy Analysis (2000).

42. Patrick J. McEwan, "The Effectiveness of Public, Catholic, and Non-religious Private Schooling in Chile's Voucher System," Education Economics (forthcoming); McEwan and Carnoy.

Table 1: Variable definitions

	Definition	Years available	Source
Variables used in differenced estimates			
SPANISH	Mean percentage of items answered correctly by fourth-graders in each school	82-84 (all), 88-96 (even)	PER, SIMCE
MATH	Mean percentage of items answered correctly by fourth-graders in each school	82-84 (all), 88-96 (even)	PER, SIMCE
PERPRIV	Percentage of primary students (grades 1-8) in municipality that are enrolled in privately-managed school	82-84 (all), 88-96 (even)	Enrollment files, Ministry of Education
P900	1=school participates in P-900 program, 0=does not	82-84 (all), 88-96 (even)	P-900 database, Ministry of Education
NATIONAL	1=school managed by national government, 0=school managed by municipal government	82-84 (all), 88-96 (even)	PER
SES1	1=low socioeconomic status, 0=not	82-84 (all), 88-96 (even)	PER, SIMCE
SES3	1=middle-high socioeconomic status, 0=not	82-84 (all), 88-96 (even)	PER, SIMCE
SES4	1=high socioeconomic status, 0=not	82-84 (all), 88-96 (even)	PER, SIMCE
Additional variables used in IV estimates			
LOGDENS	Log of population per square kilometer in each municipality	90-96 (even)	INE
BASINC	Percent of first-grade mothers with less than 8 years of schooling	90-96 (even)	JUNAEB
BASCOM	Percent of first-grade mothers with 8 years of schooling	90-96 (even)	JUNAEB
BASMISS	1=BASINC and BASCOM are missing, 0=not	90-96 (even)	JUNAEB
SESINDEX	Index of socioeconomic "vulnerability", ranging from 0 (least vulnerable) to 100 (most vulnerable)	90-96 (even)	JUNAEB
SESMISS	1=SESINDEX is missing; 0=not	90-96 (even)	JUNAEB
CITY2	Dummy variables indicating city size	90-96 (even)	SIMCE
CITY3		90-96 (even)	SIMCE
CITY4		90-96 (even)	SIMCE
CITY5		90-96 (even)	SIMCE
RURAL	1=rural school; 0=urban school	90-96 (even)	SIMCE

Note: Abbreviation are as follows: PER (*Programa de Evaluación del Rendimiento Escolar*), SIMCE (*Sistema de Evaluación de Calidad de la Educación*), INE (Instituto Nacional de Estadística), and JUNAEB (*Junta Nacional de Auxilio Escolar y Becas*).

Table 2: Variable means and standard deviations

	Full sample	1982	1983	1984	1988	1990	1992	1994	1996
Sample used in differenced estimates									
SPANISH	57.06	56.06	52.01	57.82	48.05	53.53	60.54	61.17	65.44
	(11.54)	(8.99)	(7.92)	(9.10)	(10.91)	(10.85)	(10.45)	(8.77)	(9.78)
MATH	56.29	50.98	51.04	52.20	46.97	52.90	60.45	63.31	65.62
	(12.26)	(9.47)	(8.83)	(8.91)	(9.57)	(11.11)	(11.28)	(10.08)	(10.22)
PERPRIV	25.97	21.26	23.71	25.57	23.52	24.76	28.18	29.80	27.59
	(18.44)	(11.42)	(13.73)	(14.22)	(18.17)	(18.95)	(19.81)	(19.92)	(19.95)
P900	0.19	0.00	0.00	0.00	0.00	0.22	0.37	0.37	0.27
NATIONAL	0.02	0.12	0.11	0.11	0.00	0.00	0.00	0.00	0.00
SES1	0.21	0.09	0.15	0.16	0.33	0.31	0.05	0.11	0.28
SES3	0.16	0.44	0.44	0.33	0.06	0.09	0.12	0.14	0.09
SES4	0.003	0.01	0.01	0.01	0.00	0.00	0.01	0.00	0.00
N	19,792	1,338	1,290	1,298	3,467	3,353	2,955	2,681	3,410
Sample used in IV estimates									
LOGDENS	4.42	--	--	--	--	4.18	4.54	4.70	4.35
	(2.30)					(2.16)	(2.33)	(2.37)	(2.32)
SPANISH	59.98	--	--	--	--	53.34	60.39	61.05	65.30
	(10.96)					(10.81)	(10.40)	(8.72)	(9.77)
MATH	60.31	--	--	--	--	52.74	60.29	63.19	65.50
	(11.78)					(11.09)	(11.26)	(10.05)	(10.23)
PERPRIV	27.50	--	--	--	--	24.77	28.24	29.96	27.63
	(19.79)					(19.01)	(19.88)	(19.95)	(20.02)
P900	0.30	--	--	--	--	0.22	0.37	0.37	0.27
SES1	0.20	--	--	--	--	0.31	0.05	0.12	0.28
SES3	0.11	--	--	--	--	0.08	0.12	0.14	0.09
SES4	0.001	--	--	--	--	0.001	0.004	0.001	0.00
BASINC	49.68	--	--	--	--	60.23	53.06	46.35	39.01
	(27.85)					(27.67)	(27.00)	(25.30)	(26.29)
BASCOM	15.24	--	--	--	--	10.33	16.09	18.81	16.54
	(12.88)					(11.41)	(12.56)	(13.14)	(12.94)
BASMISS	0.02	--	--	--	--	0.01	0.02	0.02	0.02
SESINDEX	61.82	--	--	--	--	55.19	67.30	67.05	59.49
	(28.17)					(26.37)	(27.28)	(28.42)	(28.76)
SESMISS	0.02	--	--	--	--	0.04	0.02	0.02	0.01
CITY2	0.30	--	--	--	--	0.23	0.31	0.31	0.37
CITY3	0.14	--	--	--	--	0.14	0.14	0.15	0.12
CITY4	0.09	--	--	--	--	0.07	0.10	0.11	0.09
CITY5	0.29	--	--	--	--	0.23	0.33	0.35	0.27
RURAL	0.48	--	--	--	--	0.54	0.45	0.40	0.52
N	12,085	--	--	--	--	3,268	2,880	2,608	3,329

Note: Standard deviations for continuous variables are in parentheses.

Table 3: The effects of private enrollment share on fourth-grade Spanish achievement in public schools (differenced estimates)

	Full sample			Metropolitan Region			Other Regions		
	OLS	Differenced	Dif-in-dif	OLS	Differenced	Dif-in-dif	OLS	Differenced	Dif-in-dif
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PERPRIV	0.045	-0.056	-0.035	0.102	-0.032	0.060	0.026	-0.085	-0.096
	(0.005)	(0.022)	(0.027)	(0.009)	(0.032)	(0.041)	(0.005)	(0.031)	(0.036)
P900	-2.967	2.140	2.153	-4.408	1.623	1.258	-2.649	2.232	2.317
	(0.176)	(0.200)	(0.260)	(0.380)	(0.496)	(0.644)	(0.197)	(0.218)	(0.284)
NATIONAL	-0.243	0.133	-0.518	1.569	-0.773	-1.607	-1.751	0.435	-0.966
	(0.370)	(0.541)	(1.394)	(0.552)	(0.811)	(2.163)	(0.498)	(0.737)	(1.657)
SES1	-5.606	0.430	0.442	0.048	2.163	2.000	-5.914	0.241	0.337
	(0.198)	(0.361)	(0.496)	(0.863)	(1.355)	(1.847)	(0.204)	(0.376)	(0.517)
SES3	5.981	-0.108	-0.310	5.275	-0.025	-0.845	6.122	-0.112	-0.150
	(0.173)	(0.191)	(0.274)	(0.330)	(0.352)	(0.539)	(0.199)	(0.224)	(0.316)
SES4	14.273	1.423	1.348	18.168	1.672	2.792	12.345	1.322	1.060
	(0.952)	(0.715)	(1.061)	(1.649)	(1.145)	(1.541)	(1.043)	(0.851)	(1.270)
N	19,792	15,498	11,792	3,612	2,938	2,310	16,180	12,560	9,482
Year dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies?	Yes	No	No	No	No	No	Yes	No	No

Note: Huber-White standard errors are in parentheses.

Table 4: The effects of private enrollment share on fourth-grade mathematics achievement in public schools (differenced estimates)

	Full sample			Metropolitan Region			Other Regions		
	OLS	Differenced	Dif-in-dif	OLS	Differenced	Dif-in-dif	OLS	Differenced	Dif-in-dif
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PERPRIV	0.037	-0.059	-0.046	0.106	-0.033	0.057	0.013	-0.085	-0.110
	(0.005)	(0.023)	(0.031)	(0.009)	(0.031)	(0.043)	(0.006)	(0.034)	(0.042)
P900	-2.976	2.070	2.091	-4.232	1.216	1.053	-2.684	2.220	2.277
	(0.193)	(0.226)	(0.292)	(0.395)	(0.541)	(0.672)	(0.218)	(0.249)	(0.323)
NATIONAL	0.621	1.680	1.330	2.559	1.792	2.459	-1.268	1.594	0.704
	(0.388)	(0.594)	(1.770)	(0.573)	(0.835)	(2.171)	(0.517)	(0.832)	(2.177)
SES1	-4.533	0.681	0.512	0.119	0.857	0.978	-4.796	0.604	0.452
	(0.208)	(0.410)	(0.565)	(0.922)	(1.302)	(1.924)	(0.215)	(0.431)	(0.592)
SES3	5.896	0.147	-0.417	5.229	0.014	-0.686	6.005	0.200	-0.319
	(0.178)	(0.212)	(0.303)	(0.335)	(0.365)	(0.564)	(0.205)	(0.251)	(0.353)
SES4	13.939	1.234	-0.327	17.812	-1.145	0.163	11.885	1.839	-0.342
	(1.102)	(1.081)	(1.225)	(1.988)	(1.304)	(1.381)	(1.207)	(1.322)	(1.516)
N	19,792	15,498	11,792	3,612	2,938	2,310	16,180	12,560	9,482
Year dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies?	Yes	No	No	No	No	No	Yes	No	No

Note: Huber-White standard errors are in parentheses.

Table 5: Determinants of private school enrollment share (first-stage IV estimates)

	Full sample (1)	Metropolitan Region (2)	Other Regions (3)
LOGDENS	5.087	4.077	5.018
	(0.108)	(0.253)	(0.118)
P900	0.634	0.176	0.903
	(0.259)	(0.646)	(0.280)
SES1	2.165	-2.893	2.275
	(0.404)	(1.812)	(0.418)
SES3	0.540	1.035	0.376
	(0.396)	(0.863)	(0.426)
SES4	6.780	12.939	3.410
	(2.654)	(3.588)	(2.148)
BASINC	-0.007	-0.120	0.012
	(0.007)	(0.022)	(0.007)
BASCOM	0.009	-0.138	0.039
	(0.010)	(0.029)	(0.011)
BASMISS	0.043	-5.578	0.893
	(1.190)	(3.512)	(1.134)
SESINDEX	-0.026	-0.022	-0.021
	(0.011)	(0.032)	(0.011)
SESMISS	-0.483	6.681	-2.910
	(1.212)	(3.012)	(1.119)
CITY2	0.750	-0.863	0.968
	(0.403)	(1.843)	(0.412)
CITY3	0.258	-3.005	1.757
	(0.616)	(2.161)	(0.649)
CITY4	7.522	3.969	9.211
	(0.679)	(1.981)	(0.738)
CITY5	6.745	-1.327	9.193
	(0.686)	(1.959)	(0.719)
RURAL	-0.243	-10.529	1.210
	(0.552)	(1.487)	(0.593)
N	12,085	2,231	9,854
Year dummies?	Yes	Yes	Yes
Regional dummies?	Yes	No	Yes

Note: Huber-White standard errors are in parentheses.

Table 6: The effects of private enrollment share on fourth-grade Spanish achievement in public schools (second-stage IV estimates)

	Full sample		Metropolitan Region		Other Regions	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
PERTOT	-0.026	-0.029	0.031	-0.029	-0.044	-0.009
	(0.006)	(0.014)	(0.012)	(0.030)	(0.007)	(0.019)
P900	-1.911	-1.910	-2.538	-2.539	-1.724	-1.750
	(0.177)	(0.177)	(0.367)	(0.367)	(0.197)	(0.197)
SES1	-1.850	-1.845	0.358	-0.004	-1.820	-1.876
	(0.311)	(0.312)	(1.502)	(1.510)	(0.320)	(0.322)
SES3	3.152	3.153	2.562	2.654	3.205	3.194
	(0.238)	(0.238)	(0.449)	(0.462)	(0.272)	(0.274)
SES4	10.925	10.935	8.101	8.855	11.212	11.241
	(1.472)	(1.473)	(2.734)	(2.894)	(1.645)	(1.695)
BASINC	-0.076	-0.076	-0.118	-0.126	-0.065	-0.066
	(0.006)	(0.006)	(0.015)	(0.016)	(0.006)	(0.006)
BASCOM	-0.016	-0.016	-0.094	-0.102	0.002	0.001
	(0.008)	(0.008)	(0.018)	(0.018)	(0.009)	(0.009)
BASMISS	-4.273	-4.273	-5.836	-6.182	-3.899	-3.933
	(1.010)	(1.010)	(2.007)	(2.096)	(1.103)	(1.105)
SESINDEX	-0.096	-0.096	-0.097	-0.099	-0.091	-0.090
	(0.008)	(0.009)	(0.020)	(0.020)	(0.009)	(0.009)
SESMISS	-4.672	-4.674	-1.632	-1.305	-5.688	-5.583
	(0.955)	(0.956)	(1.486)	(1.559)	(1.124)	(1.124)
CITY2	2.743	2.745	-3.629	-3.707	2.980	2.940
	(0.329)	(0.329)	(1.279)	(1.292)	(0.339)	(0.340)
CITY3	2.824	2.821	-2.954	-3.313	3.195	3.112
	(0.433)	(0.434)	(1.364)	(1.405)	(0.468)	(0.469)
CITY4	2.869	2.901	-3.202	-3.035	3.597	3.046
	(0.476)	(0.494)	(1.442)	(1.443)	(0.524)	(0.586)
CITY5	2.106	2.160	-4.211	-3.787	3.170	2.367
	(0.462)	(0.521)	(1.241)	(1.260)	(0.517)	(0.650)
RURAL	0.800	0.795	3.277	2.106	0.458	0.377
	(0.384)	(0.384)	(0.909)	(1.078)	(0.425)	(0.428)
N	12,085	12,085	2,231	2,231	9,854	9,854
Year dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies?	Yes	Yes	No	No	Yes	Yes

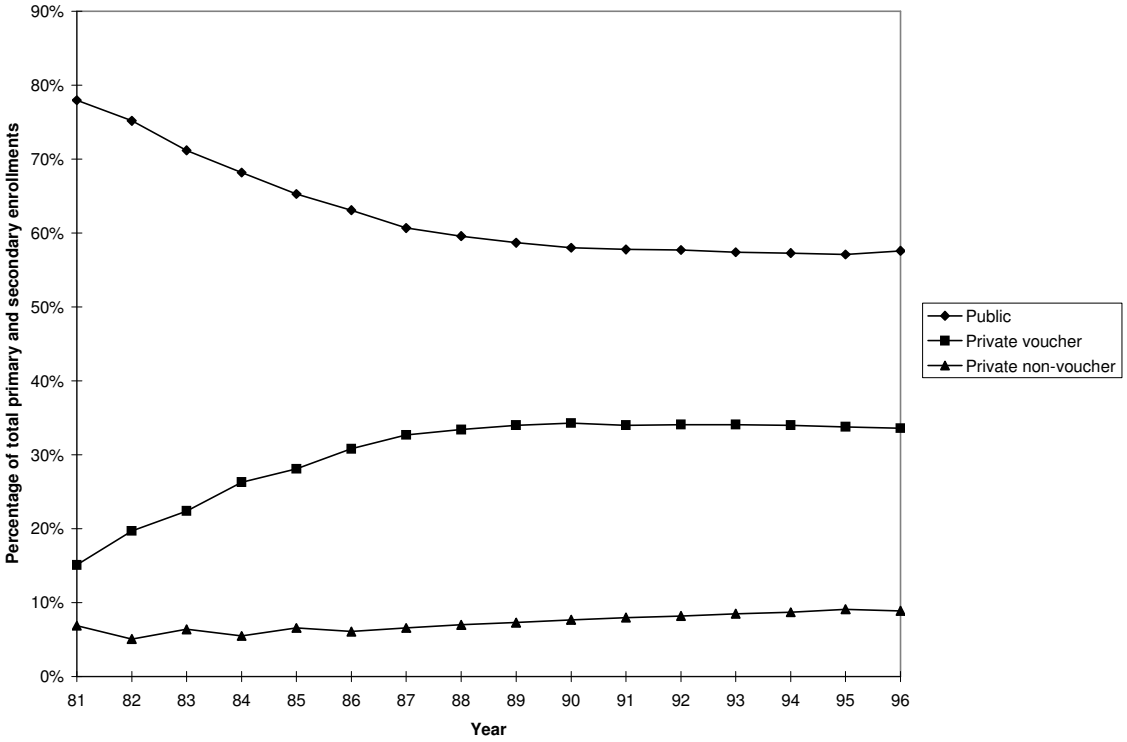
Note: Huber-White standard errors are in parentheses.

Table 7: Autonomy of resource allocation in public and private schools, 1997

	Urban public				Urban private			
	Percent of principals who report having:				Percent of principals who report having:			
	No autonomy	Partial autonomy	Full autonomy	Total	No autonomy	Partial autonomy	Full autonomy	Total
Hiring and firing of personnel	87.5	12.5	0.0	100	3.2	20.9	75.8	100
Budget allocation	92.0	5.3	2.7	100	6.8	38.8	54.4	100
Selection of textbooks and other materials	19.9	61.2	18.9	100	7.6	35.1	57.3	100

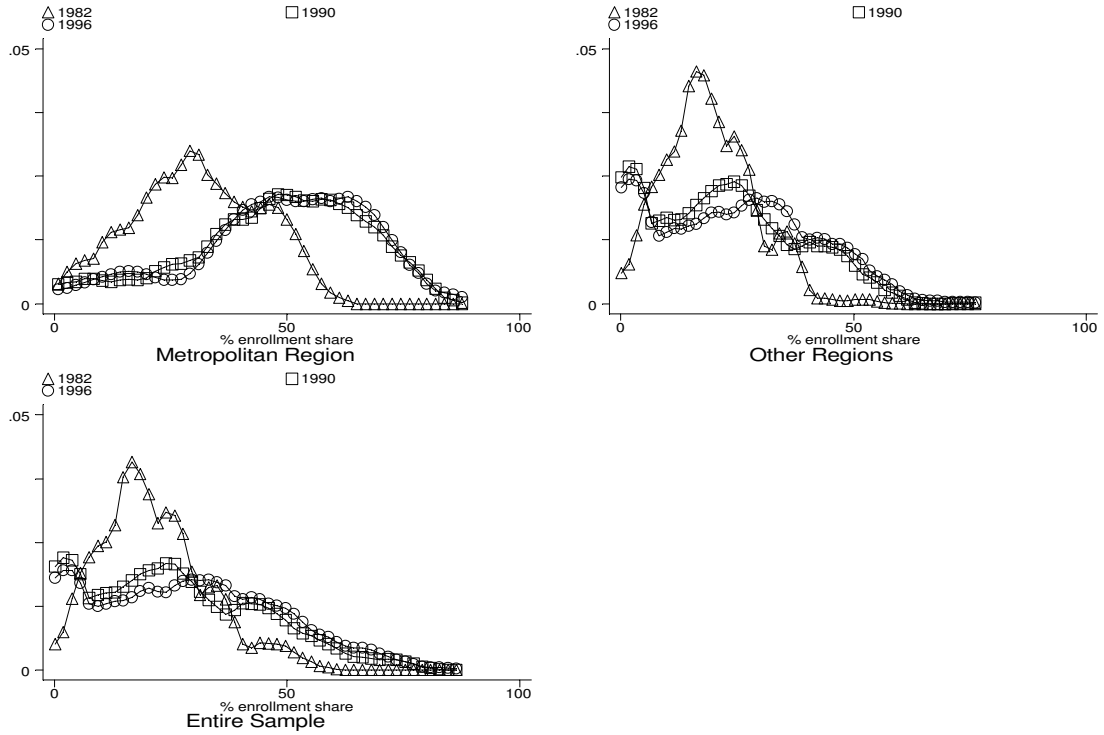
Source: *Laboratorio Latinoamericano de Calidad de Educación* and author's calculations.

Figure 1: Private and public enrollment shares, 1981-1996



Source: Jaime Vargas, "Mercado, Competencia y Equidad en la Educación Subvencionada" (Market, Competition and Equity in Subsidized Education), Persona y Sociedad 11 (1997): 59-69.

Figure 2: Kernel densities of the private enrollment shares (1982, 1990, and 1996)



Metropolitan Region: 1982 (N=276), 1990 (N=505), 1996 (N=597)
Other Regions: 1982 (N=1062), 1990 (N=2848), 1996 (N=2813)
Entire sample: 1982 (N=1338), 1990 (N=3353), 1996 (N=3410)