

Abstract

Increasing quality and equity in education: The Case of Chile

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Education is universally recognized as a key sector to be able to compete in a world increasingly based in knowledge. It also constitutes a necessary condition to provide equal opportunities to all members of the society. Countries with a population without adequate competencies will be laggards, while people within countries without access to educational opportunities will be excluded. Coverage of education, particularly at primary and increasingly at secondary level, has rapidly expanded and is becoming universal in most countries, particularly those of middle and higher income. The challenge today is to increase quality and equity, since growth in enrollments and graduates has not being accompanied by increased knowledge and decreased inequality of the system. Fortunately, the recent introduction of systematic national tests at different levels allows for performance evaluations both between schools in a given country and internationally, between countries. Awareness of weaknesses in the education system and priorities of reforms have as a result, increased.

One of the most important questions confronting education policy makers is whether the efficiency of the education system could be improved by introducing some degree of competition into the supply of education services. Friedman (1955) argued that

private schools are inherently more efficient than publicly-operated schools, and advocated a competitive system of publicly-funded student vouchers with the expectation that parents choice will favor private schools and public schools will have to compete by increasing quality. As a result freedom to choose, an objective by itself, will result in greater quality. Recently, the voucher idea has gained increasing credence in the United States. Several cities, including Milwaukee, have made vouchers available for certain students to attend private schools at the taxpayers' expense (Rouse, 1998). Similarly, the State of Florida has introduced a plan that provides vouchers to students in low-performing school districts (Figlio and Rouse, 2000). Nevertheless, vouchers are still a controversial policy, and as yet no state or district has made them available to all students.

As many other countries in Latin America, Chile's ongoing education reform (that started in the early eighties) is aimed at improving the quality and equity of education in the public sector. In its desire to improve quality by reducing inefficiencies derived from the bureaucratic nature of the central government administration, it decentralized the education administration by transferring school management from the central government to the municipalities. Additionally, it established a voucher program similar in spirit to Friedman's "ideal" system. In particular, under the Chilean system parents can send their children to public schools, or to private schools that agree to take a voucher as full payment for the cost of education.

The legacy of the reform is a tripartite education system, consisting of municipal schools which receive central government financing (subvention) and are administered by municipalities, private schools which receive the same central government subsidy and

are administered privately, and privately financed, privately managed schools. The share of enrollment in the third type of schools has remained around 8-10%, while the share of public school enrollment shrunk with the implementation of the voucher-type program from around 90% to 65% in the late 90's. Most of the students that moved out of the public schools and into the new private schools came from less disadvantaged areas, leaving the public schools with a higher proportion of the students that are most difficult to educate.

Chile constitutes an excellent case of analysis because of this policy experience in a context of universal primary and secondary education. In addition school tests have become a standard practice. Good disaggregated data by schools on test results and characteristics of establishments is available and periodic household surveys allow the identification of family characteristics of the students.

Several analyses have been made using the aggregate data and mostly showing the average performance of the schools differentiated by their public, private subsidized or fully private characteristic. The results show better performance linearly increasing from public to private. Hence, confirming the superiority of privately owned and managed schools. This has reinforced conventional views and policy orientation, without affecting the existence of a large share of public schools which cater mostly for children coming from less advantaged family situations and mostly located in disadvantaged areas of the country. The data aggregation in previous studies can generate misleading conclusions and do not contribute to identify the key determining factors of performance. Not only the analysis does not contribute to knowledge, but also policy orientation can be misguided. The study undertaken by this researcher is based on disaggregated data and

incorporates the use of frontier econometric methodologies to avoid or at least, diminished statistical biases. A more rigorous analysis can then be attempted based on a more accurate database.

The first chapter uses the unique experiences of Chile to provide new evidence on the central question of whether private schools are indeed more efficient than publicly operated schools. Several features of the Chilean system make this a particularly useful exercise. First, as already mentioned above, relatively high quality data are available on student and school characteristics, and on school wide average test scores on standardized national tests. Second, unlike the limited voucher programs in the U.S., vouchers in Chile are available to all families, and are indeed used by a wide range of families.

The results of my analysis suggest that public schools are neither uniformly worse nor uniformly better than private schools. Rather, public schools appear to be relatively more effective for students from disadvantaged family backgrounds. Such a system of comparative advantage is consistent with the observation that public and private schools continue to co-exist in most Chilean communes. Moreover, it is consistent with other features of the Chilean data, including the under-representation of disadvantaged students in the private schools (despite the fact that these schools are free), and in larger class sizes in private versus public schools.

The findings lead to policy recommendations that differ from those traditionally proposed. Since it is not true that public schools are worse, it is not necessary to eliminate them, as some have suggested. Additionally, since they are an important service to less advantaged kids, not only must we not eliminate them but also design policies focalized on those schools. Chapter II uses panel data techniques to obtain estimates of the impact

of one of such focalized programs in Chile: The P900 program. The findings suggest that the program's effect in test score has been different every year, it has proven to be effective to shorten the achievement gaps. A learning process in the implementation allowed for an increased efficiency in time.

Chapter I. Is private Education Better? Evidence from Chile

1. Introduction

One of the most important questions confronting education policy makers is whether the efficiency of the education system could be improved by introducing some degree of competition into the supply of education services. Friedman (1955) argued that private schools are inherently more efficient than publicly-operated schools, and advocated a competitive system of publicly-funded student vouchers in which parents have free choice among schools. Recently, the voucher idea has gained increasing credence in the United States. Several cities, including Milwaukee, have made vouchers available for certain students to attend private schools at the taxpayers' expense (Rouse, 1998). Similarly, the State of Florida has introduced a plan that provides vouchers to students in low-performing school districts (Figlio and Rouse, 2000). Nevertheless, vouchers are a controversial policy, and as yet no state or district has made them available to all students.

In 1981, Chile introduced a massive reform to its education system that included a voucher program similar in spirit to Friedman's "ideal" system. In particular, under the Chilean system parents can send their children to public schools, or to private schools that agree to take a voucher as full payment for the cost of education. Private schools have flourished under the Chilean voucher system, and now account for 36% of elementary enrollment in the country.

In this chapter, I use the unique experiences of Chile to provide new evidence on the central question of whether private schools are indeed more efficient than publicly operated schools. Several features of the Chilean system make this a particularly useful

exercise. First relatively high quality data are available on student and school characteristics, and on school wide average test scores on standardized national tests. Second, unlike the limited voucher programs in the U.S., vouchers in Chile are available to all families, and are indeed used by a wide range of families.

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2. Education System in Chile

In 1981 the Chilean military government implemented a voucher-style system of publicly-funded education (i.e. per pupil subvention) that transfers funds from the central government to both public and private schools on an equal basis¹. In order to be eligible to receive voucher payments, subsidized schools must meet certain minimal safety, attendance, infrastructure and curriculum requirements. They may not charge tuition. The per pupil voucher is paid on a monthly basis by the central government directly to the school in the case of private subsidized schools and to the municipality in the case of

¹ It is quite important to mention that the political scenario in which this national policy was implemented was fundamental in making it possible. Trying to replicate the same policy under alternative political conditions than those that existed in Chile during the early 1980's may require more convincing empirical

public schools². The per student stipend is independent of the public or private status of the schools, but varies somewhat across regions in an effort to benefit high cost or otherwise disadvantaged areas of the country.

The organization of the Chilean voucher system closely follows the ideal system envisioned by education choice theorists. Moreover, some of the differences between public and private schools portrayed in theory are present: unlike private subsidized school, public schools have an internal organization that reduces the potential benefits of the voucher program from induced competition. Public schools depend on the municipal government and the voucher is paid to the municipality, not to the school. The municipality then allocates school expenditures between all the schools that depend on them. Principals can influence expenditure decisions by lobbying, but they don't have a formal right over the funds. Profits or losses are returned to the municipality and are distributed between the schools. Therefore, school personnel does not reap the benefits or costs of inefficient education provision. In general, schools are not perceived badly if they have deficits and principals are not held accountable for the education outcomes.

There is no demand side selection in the Chilean voucher system. Public and private subsidized schools compete for the same kind of students, those that can't or don't pay the private tuition costs, reducing demand side selection. Furthermore, there is no restriction on the location of the school the child can attend. Except for the time

evidence.

² This is different from the traditional voucher given to the student. Benefits of student based voucher: student families really understand that they can hold schools accountable and exert their "voice and exit" behaviour to increase their children's education. Additionally, it allows differentiating between students needs. The benefits of school based vouchers is that lower administrative costs and the possibility of making the benefit a function of school characteristics.

constraint and safety issues, children can travel free of charge to any part of town to attend the school of their choice³.

On the supply side, slots at public school are rationed on a *first come first serve* basis. Public schools cannot select students using tests or interviews. The same is not true for private subsidized schools. They do select students according to family characteristics and previous performance. This introduces potential selection bias that has to be incorporated in the model and interpretation of the results.

Such student screening by private schools is likely to limit the choices of students with disadvantaged backgrounds under the Chilean system. Also, screening by private schools may drain public schools of the best students. The incentives faced by public schools to increase quality may be reduced since the remaining students are "locked in" and cannot exercise the exit option that would drive competition-induced improvements.

3. Key Issues

3.1. School selection or non-random assignment of students

Assessing the achievement differential between school types requires comparing the outcome variable $T_{i,PS}$ and $T_{i,PU}$ (i.e. test score, future wage, entry to college rate, etc.) of the same student i in both types of schools (private (PS) and public (PU)). To infer causality, assignment into schools must be random. In such cases (i.e. in actual randomized experiments), the treatment effect on the treated is given by the difference in the average outcomes between public and private schools:

Treatment on the treated: $\tau_{|PS=1} = E(T_{i,PS}|PS=1) - E(T_{i,PU}|PS=1)$

³ This freedom of choice between schools is less for younger children since it is probable that their families

Treatment on the not treated: $\tau_{|PS=0} = E(T_{i,PS}|PS=0) - E(T_{i,PU}|PS=0)$

With non-experimental data the treatment effect is not observable. We do not observe the outcome variable of the treatment group if not treated $E(T_{i,PU}|PS=1)$ or of the control group if treated $E(T_{i,PS}|PS=0)$ (i.e. the outcome of private (public) school students if they went to public (private) schools). This is so because students will sort and be selected into schools according to unobservable characteristics and thus will not be comparable.

Student selection or non random assignment may result from several processes. In the first place, self selection or sorting of students into schools may arise from the discretion granted to families to choose school and the way in which they make their choices. Family and school characteristics may be systematically related, resulting in a segmented educational system in which students from similar backgrounds will attend the same schools and hardly ever have contact with students from other realities. For instance, less educated families may invest less in the school choice decision and hence, be less informed than families that place greater value on educating their children. Alternatively, the screening of students through family interview, previous achievement, etc., may result in nonrandom selection. Schools affected by the competition induced by the voucher system (i.e. mostly private schools, because of their organizational structure), will accept and attract students that raise the perceived quality of the school (i.e. by increasing the test score and presence in higher achievement-SES segment of the population), which attracts more and better students. Additionally, the relative institutional uniqueness of private schools may also be an artifact of the student

will not want them to travel around the city alone and going with them is costly.

population. Schools get reputations in communities: “Better schools will attract better students and teachers”. The quality of the students in terms of both achievement and behavior, may allow for greater administrative restraint, more teacher autonomy, and greater satisfaction among personnel. And further, all these factors may not only affect, but also be affected by student achievement in a reciprocal causal process. Another source of selection comes from only considering students that have kept up with their grade. In other words, those that flunk are not observed and therefore not included in the estimation.

With non-experimental data, estimated treatment effect may be biased due to selection. In terms of the notation introduced above, non-random assignment will cause that the term in parenthesis to be non-zero:

$$\tau_o = E(T_{i,PS}|PS=1) - E(T_{i,PU}|PS=0) = \tau|_{PS=1} + [E(T_{i,PU}|PS=0) - E(T_{i,PU}|PS=1)]$$

If selection is on unobservables, this bias cannot be eliminated through regression adjusting. This occurs when we do not observe the variables that determine assignment and they are related with the outcome variable, such as IQ which influences the school decision and also the expected outcome. In this case, techniques such as IV estimates and first stage selection models included in second stage outcome estimates are used to obtain bias free estimates. But finding good instruments is not easy or available in every study case.

Fortunately, identification is possible if we assume selection on observables. In this case, the assignment mechanism conditional on the observable variables (X) is like a randomized experiment (Rubin 1977). The bracket term in the above equation is still not

zero because assignment is non-random but we observe the variables that determine selection and therefore can obtain ignorable treatment assignment. Hence,

$$\tau_{PS=1} = E(T_{i,PS}|PS=1) - E(T_{i,PU}|PS=1) = E_x \{ E(T_i|X_i, PS=1) - E(T_i|X_i, PS=0) | PS_i=1 \}$$

$$\tau_{PS=0} = E(T_{i,PS}|PS=0) - E(T_{i,PU}|PS=0) = E_x \{ E(T_i|X_i, PS=1) - E(T_i|X_i, PS=0) | PS_i=0 \},$$

where $T_i = PS_i * T_{i,PS} + (1 - PS_i) * T_{i,PU}$.

The assumption made is that since treatment is dependent on observables, one can take assignment to treatment conditional on X as a random variable, just like in an experiment. Therefore, comparing the outcomes for two schools with identical observable characteristics, one of which is private subsidized and the other public, is like comparing those two schools in a randomized experiment. This is what most of the previous studies have done. They have included an extensive list of variables in the outcome equation trying to control for all source of selection bias that results from observable characteristics.

As with other studies, accounting for selection bias will be an important task of this chapter. However, as was explained earlier, thanks to the design of the voucher system in Chile, it is lessened. In addition, I make use of an unusually large set of controls taken from the merge of the school data sets with household surveys to further control for selection on observables. This individual level socioeconomic data allows the modeling of selection explicitly, and its introduction in a second stage equation of test scores. Finally, models controlling for unobserved selection assuming joint normality of the error terms are run using the traditional Heckman selection models.

Unfortunately, student level data of the outcome variable is not available, and therefore the analysis will be limited to school averages. This implies that the differential

performance within schools will remain unobserved and unused to pursuit the objective of this chapter.

3.2. Standardized Test Scores as the Outcome Measure

Another key element to consider is the selection of a measure for the relative effectiveness of schools. What is it that we want from schools? Better standardized test scores, better wages, better social skills, lower criminality, etc... Even though all these are desirable outputs, this chapter will use standardized test scores that are a partial measure of quality, but have the advantage of allowing objective comparisons. The use of 4th grade test scores limits the amount of other factors that might be playing a role in explaining the outcome. That is, since education is cumulative, test scores for higher grades or even university degrees or PAA⁴ scores, would require controls for changes in schools and other external factors which might influence the result. Similarly, when using wages, there might be factors, such as luck and personal contacts, involved in the outcome that we can't control for. Furthermore, there are studies that show that achievement test scores are positively correlated with future labor market outcomes.

My key dependent variable is math scores. Past research has shown that math scores appear to be more related to school characteristics (Madaus et al. 1979). Additionally, achievement in math often has a higher correlation with future earnings (Murnane et al. 1995).

On the down side, there is some evidence that test scores are a short-term measure of school effectiveness. For example, teachers may train students to perform well on a particular type of test, without any long-term effects on human capital skills. (They even

⁴ PAA is equivalent to SAT in US.

may select the better students to take the test, or give out the answers). Also, availability of better teachers and more school resources may not have an impact on the test scores in the short run, but may have an influence in the long run.

4. Literature Review

4.1. Theory

School choice theory and relative efficiency of private education starts with Freedman's' 1955 chapter. Simply put, the argument for education vouchers is that by increasing competition between schools the quality of education will improve. As a by-product, the increased competition will motivate expansion in private provision of education, which is claimed to be more efficient. In theory, certain attributes in private schools, such as less bureaucratic structure and profit motive, enable them to provide higher quality education than public schools because of its flexibility and adaptability to changes in family needs and context.

In other words, school choice via vouchers is expected to have an impact on the education quality of all schools (including public schools), by introducing competition into the system. This is the dynamic effect of voucher induced competition. Additionally, there is a static effect that refers to the increasing provision of private education that is presumed to be relatively more efficient.

It has been argued that positive education externalities (such as poverty reduction, economic growth and the pursuit of common values) yield social benefits that exceed private benefits that families take into account when making the decision. Positive

externalities indicate that a free market will under provide education services relative to the efficient level (Krashinsky 1986, Levin 1980, Spicer and Hill 1990). Additionally, opponents to school choice argue that public funding for private schools will drain public schools of many of the best students, leaving the public schools with a disproportionate share of the students most difficult to educate. Proponents counter that the largest gains from private education is for the low-achieving, low-income, minority students.

4.2. Empirical

The first round of studies starts with the very influential report by Coleman et al. in 1981-82. Using data from the High School and Beyond Survey, they concluded that private high schools were more efficient than public high schools. Later, Chubb and Moe (1990) corroborated these results. This led to a second round of studies aimed to prove Coleman was over simplifying the analysis by not controlling for the differences in the students characteristics. Most of these studies, (Alexander 1987, Alexander and Pallas 1983, Blinfield 1993, Bryke and Lee 1993, Goldberger and Cain 1982, Noel 1982, Sukstorf et al 1993, Willms 1983), find minimal or no superiority of private schools.

In developed countries, the recent debate has centered largely on the relative performance of public and Catholic schools. Evans and Schwab (1995), Sander (1996), Goldhaber (1996), Figlio and Stone (1997) and Neal (1997) compare the effects of school type on outcomes such as standardized achievement tests, the probability of completing high school, and the probability of starting college. The results from these studies are mixed. Evans and Schwab and Neal find that Catholic school students are more likely to complete high school and start college. Using test scores as an outcome measure, Sander

finds no significant Catholic school effect, while Figlio and Stone find a significant advantage for students in private non-religious schools, but no difference between public and Catholic schools.

In developing countries, the evidence is more clear-cut. In a series of chapters, Cox and Jimenez (1991) and Jimenez et al. (1991) use data from Colombia, the Dominican Republic, the Philippines, Tanzania and Thailand, to study the relative effectiveness of private versus public schools. Typically, these chapters examine the differences in student achievement scores in a particular grade. After controlling for various background factors, these chapters report a significant private school achievement advantage. The magnitude of this advantage (on math scores) ranges from 13% in Colombia to 47% in the Dominican Republic. In a related study, Jimenez and Lockheed (1995) find that per pupil costs are lower in private schools (based on data from the same countries listed above). These findings corroborate the efficiency advantage of private over public schools.

Most of the studies using Chilean data have similarly concluded private schools generate higher test scores. Rodriguez (1988), using a sample of 281 schools in the metropolitan area concludes that private schools outperform public ones in the 1984 PER exam. Aedo and Larranaga (1994), using data on 1990-91 and Mizala et. Al. (1997 and 2000), using data for 1994-95 and 1996 arrive at the same conclusion. Bravo, Contreras and Sanhueza (1999) use data from 1982 onwards to run a series of cross sectional regressions, finding that the performance gap favorable to private schools is positive for the earlier years but decreases and turns insignificant for the later ones. Winkler and Rounds (1993) analyze school expenditures and conclude that private schools are more

efficient. However, Parry 1996, finds no significant difference between the achievement of both types of schools. Schiefelbein (1991) and Rodriguez (1988) found that non-profit private subsidized schools provide higher quality education than profit maximizing private subsidized schools.

5. Estimation Strategy

A school can be thought of as a firm that is producing an output (in our case, test score (T)), with a set of observed (X) and unobserved inputs (μ). The production function for both types of schools can be expressed as:

$$(1) \quad T_{PS,i,j} = \alpha_{PS} + X'_{i,j} \beta_{PS} + \mu_{PS,i,j}$$

$$(2) \quad T_{PU,i,j} = \alpha_{PU} + X'_{i,j} \beta_{PU} + \mu_{PU,i,j}$$

Where: PS=Private School, PU= Public School, i=1-N schools and j=1-J students.

Selection can be modeled by assuming that the attendance to PS school, or treatment, is a linear function of observable characteristics X and an error (v).

$$(3) \quad PS_{i,j} = 1[W'_{i,j} \Pi + v_{i,j} > 0]$$

Since I do not have student level data, estimates are based on school-based aggregations. Mean test score is the dependent variable and mean school, teacher, and student characteristics are the independent variables. In terms of equation (1)-(3) we will be estimating the following:

$$(1') \quad \bar{T}_{PS,i} = \alpha_{PS} + \bar{X}'_i \beta_{PS} + \mu_{PS,i}$$

$$(2') \quad \bar{T}_{PU,i} = \alpha_{PU} + \bar{X}'_i \beta_{PU} + \mu_{PU,i,j}$$

$$(3') \quad \bar{PS}_i = 1[\bar{W}'_i \Pi + v_i > 0]$$

Where the overbars represent school means. For ease of notation, the overbars will be omitted in the rest of the chapter. All variables with sub index i and no j are school means.

5.1. Case I: Random Treatment Assignment or No Selection bias

The first set of models estimate the treatment effect by assuming that assignment to treatment is random or not correlated with the outcome variable (i.e. test scores). For such purpose we assume that μ_i and v_i are iid and $E(\mu_i|X_i, v_i) = E(\mu_i|X_i) = 0$. In this case, the population regression function and the regression functions for the observed subsamples are identical.

$$(4) \quad E[T_{PS,i} | X_i, PS_i = 1] = E[T_{PS,i} | X_i] = \alpha_{PS} + X_i' \beta_{PS}$$

$$(5) \quad E[T_{PU,i} | X_i, PS_i = 0] = E[T_{PU,i} | X_i] = \alpha_{PU} + X_i' \beta_{PU}$$

Therefore, the treatment effect or relative efficiency differential can be simply calculated as the difference between the mean test scores conditional on the observable characteristics in private and public schools. In this case, the estimation of equation (6) by OLS leads to an unbiased estimate of the treatment effect.

$$(6) \quad E[T_{PS} | X_i] - E[T_{PU} | X_i] = \alpha_{PS} - \alpha_{PU} + X_i' (\beta_{PS} - \beta_{PU})$$

Equation (6) estimates the impact on the test score of being in a private school, with respect to a public school, controlling for observed family-student-school characteristics. In theory, the coefficient measures what happens to the test score if we take a public school, with its students, teachers and families intact, and transform it into a private school by changing its administration, but not its resources. Alternatively stated,

the coefficient provides the test score difference between two identical schools, except for the fact that one is private and the other public.

Previous studies for Chile have estimated an additive constant treatment effect, which in terms of equation (6) implies that they are restricting the β 's of both types of schools to be equal but allowing the α 's to vary. In other words, they are assuming that the production functions are parallel and that their difference between the test scores (treatment effect) is constant and equal to the difference between the α 's.

In terms of the model that is being estimated it corresponds to some version of equation (7), where the treatment effect is $\gamma = \alpha_{PS} - \alpha_{PU}$ and corresponds to the absolute advantage model in which private schools are assumed to be more efficient for all types of students.

$$(7) \quad T_i = \alpha_{PU} + PS_i \gamma + X_i' \beta + \mu_i$$

However, linearity and additivity of the treatment effect are not necessary assumptions. A more realistic scenario is to assume that the achievement differential varies with the students-school-teacher characteristics. If the organizational differences of private subsidized schools make them more prone to competition and more adjustable to students needs and if these factors generate their efficiency gain with respect to public schools, it is not irrational to expect that their advantage will be higher the more resources they have to adjust to changing needs. This is so because if they are resource constrained they will be less likely to adjust and therefore be much more like public schools. Another possibility is that since private schools will select the “better”⁵ students, they will be likely to direct their efforts and resources towards meeting the needs of these

⁵ Better refers to students coming from families with higher education and income.

“better” students and not those of the “worst” ones. Therefore, one might expect that the benefits for students from less advantaged backgrounds of attending a private school are relatively lower (if he actually gets to go to one at all).

To capture the possibility of differential effects by school-teacher-family characteristics under the selection on observables assumption, I estimate equation (8). The inclusion of interaction terms is an innovation to previous literature that increases flexibility in the estimation and allows for heterogeneous treatment effects. The treatment effect is equal to $\gamma + X_i'\delta = \alpha_{PS} - \alpha_{PU} + X_i'(\beta_{PS} - \beta_{PU})$.

$$(8) \quad T_i = \alpha_{PU} + PS_i\gamma + PS_iX_i'\delta + \mu_i$$

Equation (8) allows for the estimation of the distribution of the effect, which is not allowed for in previous estimates of production functions similar to equation (7) that estimates the average effect. It is my opinion that if treatment is in fact heterogeneous one must not only observe averages but also the distribution of the effects. If one believes that the winners from this type of school choice policies are students from less advantaged areas, as school choice proponents do, then one should look specifically at the effects on those students, which might be different from that of students from less disadvantaged backgrounds. This is what equation 8 is capturing.

5.2. Case II: Non Random Treatment Assignment

The last set of estimations consider the possibility of non-random assignment by assuming that $F(\mu_{PS}, \mu_{PU}, v)$ is a trivariate normal distribution. In this case assignment and test scores are no longer independent and therefore the population regressions differs from the observed samples regressions by $E[\mu_{PS,i}|X_i, v_i]$ and $E[\mu_{PU,i}|X_i, v_i]$. But by using

the properties of the normal distribution that term can be calculated and included in the regression:

$$(9) \quad E[T_i | X_i, PS_i = 1] = \alpha_{PS} + X_i' \beta_{PS} + E[\mu_{PS,i} | X_i, v_i]$$

$$(10) \quad E[T_i | X_i, PS_i = 1] = \alpha_{PS} + X_i' \beta_{PS} + E[\mu_{PS,i} | X_i, v_i > -W_i \Pi]$$

$$(11) \quad E[T_i | X_i, PS_i = 1] = \alpha_{PS} + X_i' \beta_{PS} + \frac{\sigma_{\mu_{PS},v}}{\sigma_v^2} \lambda_{PS}(W_i \Pi)$$

Analogously for public schools:

$$(12) \quad E[T_i | X_i, PS_i = 0] = \alpha_{PU} + X_i' \beta_{PU} + E[\mu_{PU,i} | X_i, v_i < -W_i \Pi]$$

$$(13) \quad E[T_i | X_i, PS_i = 0] = \alpha_{PU} + X_i' \beta_{PU} + \frac{\sigma_{\mu_{PU},v}}{\sigma_v^2} \lambda_{PU}(W_i \Pi)$$

Where,

$$(14) \quad \begin{aligned} \lambda_{PS} &= \frac{\phi(W_i \Pi)}{\Phi(W_i \Pi)} \\ \lambda_{PU} &= -\frac{\phi(W_i \Pi)}{1 - \Phi(W_i \Pi)} \end{aligned}$$

Following Heckman (1979) the λ 's are computed by running a first stage probit model of $P(X)$ as a function of individual SES variables (W_i) and using the estimated coefficients in the λ 's formulas. The treatment effect can then be computed as the difference between (12) and (13). The estimated treatment effect will differ from the one estimated by OLS because it will include an additional term that controls for the selection bias ($\rho_{\mu_{PS},v} \lambda_{PS} - \rho_{\mu_{PU},v} \lambda_{PU}$).

5.2.1. 1-Factor Model of Latent Test Scores or Absolute Advantage

One common assumption made in this models is that the correlation between test score and assignment (ρ 's) of both types of schools is the same. In this case, following the absolute advantage story, students selecting one kind of school (i.e. private) would outperform the other students in any type of school. That is, if there is positive selection into private schools ($\rho(\mu_{ps}, v) > 0$) there must be negative selection into public schools ($\rho(\mu_{pu}, v) < 0$). Thus, the expected test score for the subsample of students that go to private schools exceeds the population expectations ($E(T_i/X_i, PS=1) > E(T_i/X_i)$) and the opposite is true for public school students ($E(T_i/X_i, PS=0) < E(T_i/X_i)$), implying that the treatment effect estimates that ignore the selection bias are upward biased.

To be consistent with the above estimates, we estimate the constant and heterogeneous treatment effects with equal ρ 's from equations (15) and (16).

$$(15) \quad \begin{aligned} E[T_i | X_i, PS = 1] - E[T_i | X_i, PS = 0] &= \alpha_{PS} - \alpha_{PU} + \rho(\lambda_{PS} - \lambda_{PU}) \\ E[T_i | X_i, PS = 1] - E[T_i | X_i, PS = 0] &= \gamma + \rho \frac{\phi}{\Phi(1 - \Phi)} \end{aligned}$$

$$(16) \quad E[T_i | X_i, PS = 1] - E[T_i | X_i, PS = 0] = \gamma + X_i' \delta + \rho \frac{\phi}{\Phi(1 - \Phi)}$$

5.2.2. 2-Factor Model of Latent Test Scores or Comparative Advantage

In contrast to the absolute advantage story, students may select the schools that benefit them the most and therefore there could be positive selection into both types of schools. To allow for this we let $\rho(\mu_{ps}, v)$ to differ from $\rho(\mu_{pu}, v)$. In the case of positive selection into PS and PU ($\rho(\mu_{ps}, v) > 0$ and $\rho(\mu_{pu}, v) < 0$) we would have $E(T_i/X_i, PS=1) > E(T_i/X_i)$ and $E(T_i/X_i, PS=0) > E(T_i/X_i)$.

$PS=1) > E(T_i/X_i)$ and $E(T_i/X_i, PS=0) > E(T_i/X_i)$, and the impact on the treatment effect will be ambiguous.

The models estimated in this case correspond to equations (17) and (18).

$$(17) \quad E[T_i | X_i, PS = 1] - E[T_i | X_i, PS = 0] = \gamma + \rho_{\mu_{PS},v} \frac{\phi}{\Phi} + \rho_{\mu_{PU},v} \frac{\phi}{1-\Phi}$$

$$(18) \quad E[T_i | X_i, PS_i = 1] - E[T_i | X_i, PS_i = 0] = \gamma + X_i' \delta + \rho_{\mu_{PS},v} \frac{\phi}{\Phi} + \rho_{\mu_{PU},v} \frac{\phi}{1-\Phi}$$

6. The Data

The data used come from the Ministry of Education and the Socioeconomic Household survey. The school level data sets of the Ministry provide outcome variables (i.e. test scores) as well as school and teacher characteristics. Student characteristics are obtained from the Household Socio-economic surveys (CASEN). The data sets are merged together by using the school id number. Only elementary schools are included in the analysis in order to limit the uncontrolled switching between schools and the cumulative aspect of education. Below is an outline of the data sets and variables.

Data Sets:

Ministry of Education Data Sets: All data is school level, no individual observations on students

1. Since Enrollment directory
2. Teachers Directory
3. Socioeconomic Vulnerability Index JUNAEB

Variables:

School Characteristics

1. 4th Grade average math and Spanish test scores
2. Internal efficiency: Promotion, repetition and dropout rates)
3. Administrative Dependence: Municipal, Private Subsidized, Private
4. Enrollment (total, per grade, male, female)

5. Number of students per class (per grade/total)
6. Number of teachers per school
7. Percentage of titled teachers
8. Number of years teaching
9. Number of hours per teacher (real and contract)
10. Percentage of male teachers
11. Part/full day education
12. Presence of other Ministry of Education programs: Enlaces, PME, JEC, AFC, P900

Socioeconomic Characteristics of Students

1. Vulnerability Index : Function of mother education and a group of health indicators for the child (dental cavities, malnutrition, hearing problems, eye problems and posture problems).
2. Average parental education index: Average education of the students' parents is coded from 1 to 4.
3. Average family spending in school supplies.

CASEN (Socioeconomic characteristic household survey)

Variables:

1. Household size (number of people in family)
2. Poverty line (rank 1-3 with respect to poverty line)
3. Total household income
4. Father's Education (years, degrees)
5. Mother's Education (years, degrees)
6. Students age, grade and sex

The focus of this chapter will be on the 1996 cross section of schools. Unfortunately, since the data does not cover the period before the vouchers were implemented there is no good reason to use the data in a time series way.

When using the Ministry of Education data sets we are able to identify 5630 schools whose dependency composition mimics that of the universe of schools, that is 61.5% correspond to public, 29% to private subsidized and 9.5% to paid private schools. Unfortunately the information available on family background is very scarce⁶. In an attempt to make results less susceptible to selection bias we averaged family

⁶ The only SES information available from the ministry is the vulnerability index, parental education index, and average family spending in schooling index.

characteristics from the household survey data at school level⁷. (To increase precision both surveys for 1996 and 1998 were merged to calculate the average family characteristics assuming that there is not much change between those years). This surveys do not allow us to match all schools contained in the ministry of education data, further restricting our sample to 3500 to 4000 schools, of which 57% are public schools, 34% private subsidized schools and 9.1% private paid schools. When testing for non-random exclusion of schools we find no statistical significant difference between the coefficients of the restricted and unrestricted samples.

Table 1 presents the sample means of the school, teacher and student characteristics of the three types of schools. Private subsidized schools don't appear to have better learning conditions than public schools. They tend to be larger (in terms of enrollment) and with larger classes (calculated as the number of students enrolled per grade divided by the amount of classes in each grade). One could argue that these conditions are detrimental to education if personalized teaching is beneficial. Of course, economies of scale, compensatory classes and measurement errors point in the opposite direction.

Teacher characteristics in this data are measured by percentage of male teachers, years of experience, hours worked/contractual hours and percentage of teachers with a degree in education⁸. Again, private subsidized schools don't have a "better" teacher

⁷ Most of the other studies done with chilean data restrict the variables to those available from the ministry. The rest, rely on in school surveys to include additional variables on student-family-school characteristics. Unfortunately, these surveys are non universal and the samples get restricted substantially.

⁸ This measures are not so indicative of the teachers quality, some measure of wages would also be desirable but is unavailable. With respect to teachers with university degrees, the data allows for controlling what type of degree they have (i.e. education, physics, etc) and even though one could think that having a degree in some other area (not education) may be more beneficial to teaching than having an education degree, I believe that this is true for older children and therefore just include the degree in education information in

team: They have relatively less teachers with a degree in education and teachers with less years of experience, working on average less hours. They also have a higher percentage of female teachers.

Demand side selection is still present and evident from the means presented in family background characteristics presented in the table. Private subsidized and public schools tend to attract student from a lower socioeconomic status than private schools (as measured by higher parental income and education, lower vulnerability index), and between private subsidized and public schools there is still some sorting going on. Children from relatively better family backgrounds appear to be attracted to private subsidized schools.

The observed systematical differences in resources and student characteristics plague direct outcome comparisons with selection bias. The 5-6 percentage point difference in private subsidized and public schools' average test scores could very possibly just be the effect of non-random assignment of students into schools (i.e. of having better students and not really teaching them better).

Graph 1 shows the distribution for 4th grade math scores in 1996 by school type. It is evident from the graph that the public schools concentrate in lower achieving portions of the distribution, while private paid schools do so in higher achieving portions. Private subsidized schools lie in the middle. In terms of standard deviation of the test scores, private paid schools have the lowest inter school variance, followed by public schools and private subsidized schools, respectively⁹. When testing for equal distributions, we can not reject equality between public and private subsidized schools score distributions

my analysis.

at a 95% confidence. Private paid score distribution is significantly different from both private subsidized and public score distributions. This simple test corroborates the previous statements assuming less dispersion within PS and PU schools, than with private paid schools. Together with the following description on the school-family-teacher characteristics, it helps explain why the working sample will be limited to PS and PU schools only. Private paid schools are excluded from the analysis because of its inherently different distribution of family as well as school characteristics that make comparisons misleading.

Table 2 computes relative performance within sub-samples, as a first approach to reducing the bias in the computed differentials. The first thing worth noticing is that now there are several large and negative relative difference indicators for private subsidized schools (with respect to public schools). When dividing the sample into socio-economic status (SES) sub-samples, as measured by average parental education, maternal education or vulnerability index, one observes that public schools cater low SES families, private subsidized schools do so for intermediate SES families and paid private schools do so for high SES families. As expected, test scores increase as the average SES variable increase. Within those categories, private subsidized school's relative advantage over public schools remains only for higher SES groups, but reverses for lower SES groups.

Private schools tend to concentrate in urban areas (50% of the schools in the urban area are private subsidized and paid). 81% of the rural schools are public. The relative advantage of private schools over public schools remains only in urban areas. In rural areas, public schools have on average 2-3% score advantage over private subsidized

⁹ Unfortunately, at the time of this study, the intra-school variance information was not available.

schools. One possible explanation is that in rural areas the selection of students is lessened, as well as the average SES of the student's families, and therefore private subsidized schools no longer have better students to educate.

With respect to class size (both total and 4th grade) public schools have an advantage over private subsidized schools in smaller classes, but not in bigger classes. Not surprisingly, they normally have smaller class sizes.

Graphs 2 through 5 show the scatter plot and trend lines for average school 4th grade math score by log of household income, log of parental income, maternal education and vulnerability index. Consistently it is found that for any one of this measures of parental background, private subsidized schools perform better than public schools only when the students come from a less disadvantaged background (i.e. higher maternal income, higher log household income, etc). That is, if we choose to compare the average test score for schools with students that come from the less advantaged families, we would find that public school's achievement is higher, and the opposite is true for students coming from higher socioeconomic status¹⁰. These findings are consistent with the comparative advantage theory. It is not that private schools have an absolute advantage on producing higher test scores, they only have a comparative advantage in teaching children that come from better socioeconomic background.

Given the characteristics of the students attending each type of school, it appears as if there is a specialization of schools by which each type attracts the students they can most efficiently educate. That is, private subsidized schools attract higher income/parental education students and public schools attracts lower income/parental

¹⁰ This is consistent to figure 1.b. in the model section of this chapter.

education students because they can perform relatively better than the other type of school with students with similar socioeconomic characteristics.

Again, even though these graphs compare average test scores by socioeconomic characteristics, it is not controlling simultaneously by all characteristics. That is done in the regression analysis presented in the next section.

7. Estimation of the Treatment Effect

7.1. Case I: Random Assignment Case

This section will estimate the models presented in section 5. As explained earlier, to answer the private vs. public education question, the interest lies in the sign and magnitude of all the coefficients that accompany PS, not only the additive one, but also the multiplicative ones, since the production functions may have different slopes and intercept. The production functions present the predicted test scores at each set of teacher-family-school characteristics, the difference between them is the test score gain (or loss) of private subsidized schools over public schools at each of this sets of characteristics (i.e. the treatment effect), which will be different at different sets of characteristics when slopes are different. If this is the case, then it is better to present the distribution of the effects and not just the average effect or the treatment on the treated or not treated effect. Estimations based on equation (8) that allow for heterogeneous treatment effects (i.e. different slope and intercepts) allow the identification of the distribution of the effects, which is a more complete and relevant result.

To maintain consistency and comparability with previous research, models like the one in equation (7) are also estimated. The inclusion of models based on equation (8) is an innovation to earlier research and is presented after the traditional estimations.

Tables 1 through 4, in annex #2 present the complete set of OLS results for the average 4th grade math test scores controlling for dependency, school and teacher characteristics and family background. Regressions were run for the sample of all schools, private subsidized and public schools together, and private subsidized and public schools independently. The sample was also divided according to the rural/urban index. Each table contains the estimated models for a different set of schools. Models are arranged from least to most complex. The first ones are estimations of the average treatment effect by equation (7) that restricts the production functions to be parallel. The last ones are estimations of heterogeneous treatment effects by including all the controls as well as interaction terms for type of schools with school and family characteristics, as in equation (8).

Table 3 presents the estimated “intercept effect” as controls and interaction terms are sequentially added in the model. The effect presented in the first three rows is theoretically equivalent to the average treatment effect estimated in previous studies (except for the differences in samples and controls used), since it estimates equation (7) without allowing for heterogeneous effects by not including interaction terms. The results are consistent with previous studies: As we move towards more inclusive models we find that the magnitude of the treatment effect (i.e. the gain of private subsidized schools over public schools in test scores) diminishes from 4.07 to -0.14 points. This diminution reflects the selection effect mentioned above, that is, private subsidized schools select and

attract “better students”, therefore the uncontrolled effect is upward biased. It is also worth mentioning that when the school controls are included with no SES controls the effect is bigger since, as shown in table 1, the school characteristics of private subsidized are worse than that of public schools.

The fourth row of table 3 allows for heterogeneous treatment effects by including interaction terms in the analysis. The model estimated corresponds to equation (8). The interacted terms correspond to the private subsidized dummy with the deviation of the SES variables for the schools with respect to the mean. Now, the coefficient for PS is no longer the average treatment effect. It can be interpreted as the effect of being a private subsidized school at the mean X's.

Table 3 suggests that when we allow for heterogeneous treatment effect the effect for the average school is lower than the average treatment effect and is not significantly different from zero when urban and rural schools are included in the analysis. If only urban school are included then the effect on the average school is still less than the average treatment effect and significantly different from zero. For rural schools the effect turns negative, but no significant.

If we are socially motivated, what we are really interested in is the effect of the policy in those kids that are in most need of better education¹¹. This motivates the introduction of the heterogeneous treatment effect models to capture the differential effects along the X-axis, and to be able to observe the predicted distribution of such effects.

¹¹ In theory the gains to “lower-end” students from the voucher system are not exclusive to attending the private schools but to having the possibility to do so. It is this possibility of switching between schools that increases competition and rises overall school quality (public and private). Unfortunately, we do not have

Model IV in Table 4 presents the estimated coefficients for the heterogeneous treatment effect model. The first thing to notice is that the coefficients for the SES variables (not interacted with PS dummy) are positive¹², and therefore there is an increase in the test scores as students come from less disadvantaged backgrounds, or that the test score-SES slope is positive for both types of schools. This is consistent with previous literature in that family characteristics matter in school achievement. Additionally, the PS*SES interaction coefficients are positive (again except for the vulnerability index by construction) implying that as the socioeconomic characteristics of the students' families get better the increase in test scores in private subsidized schools is higher than in public schools. In other words, the test score-SES slope of the private subsidized schools is larger. Therefore, our findings suggest that case 1.b. is the relevant case in the Chilean scenario (of 1996).

Graph 6 confirms the above findings, and those presented in the raw data analysis, by showing the predicted test scores for private subsidized and public school for 5 representative households. Households 1 to 5 are ranked from least to most rich, educated and invulnerable¹³. The treatment effect (or gain at private subsidized schools) for each representative household is $T_{PS,i} - T_{PU,i}$, or the difference between the lines.

Just as the simple plots of the raw data suggested, there is a negative treatment effect on students from less advantaged backgrounds. This negative effect is reduced as the characteristics of the families get better and turns positive for the less disadvantaged families.

data on school quality before the voucher system was implemented and therefore cannot evaluate the impact on education quality as a whole.

¹² Note that the vulnerability index increases as the family is more vulnerable, and therefore a negative

In sum, these results suggest that private subsidized schools only have a comparative advantage in teaching students from more advantaged backgrounds and not all students as most of the people believe. It will not be beneficial for less educated/income families to put their children in private subsidized schools. In fact, they will do better (on average) in a public school than in a comparable private subsidized school. This raises the question on what do public schools have that makes them “better” than private subsidized schools for this type of students. Or, inversely, what do private subsidized schools do differently that benefit students from a higher SES family. These questions can be in part answered by analyzing the coefficients of the school-teacher variables in Table 4.

In general, the sign and magnitudes of the control coefficients show what characteristic are relate to better achievement. Additionally, the regression results for each school type show how the different characteristics affect achievement in different ways. In terms of school characteristics school size, teacher experience, teacher education certification and percentage of female teacher are all positively related to higher test scores. The average number of hours worked by the teachers is negative but not statistically significant.

Additionally, consistent with international evidence and other studies for Chile (Romaguera and Mizala (1998) and Romaguera, Mizala and Farren (1997), same sex schools have significantly higher average test scores.

One interesting result is that class size is negatively related to test scores in public schools but positive in private subsidized schools. This can be seen in model IV (i.e.

coefficient is consistent with having better test scores for schools with less vulnerable students.

significantly negative class size coefficient and significantly positive and bigger coefficient for class size interacted with PS dummy). It can also be seen from the regressions run on each type of school separately.

One possible explanation (to the different sign in the class size effect), is that public schools are more limited by infrastructure and therefore when class size gets bigger it does so at the expense of crowding students in the class, where as private subsidized schools don't have that infrastructure constraint. Another possibility is that the causal relation goes in the other direction: better schools attract more students and therefore, the classes get larger. It is also possible that better student groups don't need to have personalized attention, as do less advantaged students. Peer effects may be larger in private schools because they are composed of students from better socioeconomic backgrounds, and this effect is relatively stronger than the small class size effect.

As mentioned earlier, the signs of the socioeconomic characteristics coefficients are as expected. Schools with students with higher parental education and income tend to perform better, on average, in the 4th grade achievement test. The coefficient for vulnerability index is negative and significant in all models and samples, implying that schools with more vulnerable students on average do worse. Consistently with previous studies, maternal education matters more than paternal education for achievement.

The above results are also observed in the separate regressions for public and private subsidized schools. Maternal education, vulnerability index, household income, relationship with the poverty line are significant in all specifications. Paternal education

¹³ Household 3 corresponds to the mean household.

is negatively correlated with test scores in public schools but positively correlated in private subsidized schools.

7.2.1. Case II: Non Random Treatment Assignment

The last set of results incorporate nonrandom assignment to treatment by assuming that $f(\mu_{ps}, \mu_{pu}, v)$ is a trivariate normal distribution. The results are shown in table 5. When restricting to equal ρ 's (i.e. absolute advantage in selection) the coefficient for the selection correction term (λ) is significant and negative for both the constant and the heterogeneous treatment effect models. This would mean that selection into private subsidized schools is negatively related to test scores in both private and public schools, and therefore the OLS treatment effect would be downward biased by the omission of selection correction terms.

When the ρ 's are not restricted to be equal, to allow for comparative advantage type of sorting into schools, and the treatment effect is assumed to be constant, the coefficients on the selection terms are still negative and significant. This would again imply that the average treatment effect from the OLS models is downward biased. But, when the interaction terms are included in the model to allow for differential effects along the X 's, the selection coefficient for private schools turn positive and non significant indicating that selection into private schools is mostly captured by the interaction terms on the observable characteristics. On the other hand, the selection correction coefficient for public schools is still negative and significant, implying that selection into public schools is unaccounted for by controlling for observables and that it is positively related to test scores in that type of schools. These findings are confirmed

when running the regressions for each type of school independently.

Positive sorting into public schools would mean that the observed test scores at public schools are above the population mean test scores. Thus leading to a downward biased positive treatment effect and an upward biased negative treatment effect.

Graph 6 shows the predicted test scores for public and private schools from the estimation of the heterogeneous treatment effect model with unequal ρ 's for five representative households. The results are consistent with all previous results suggesting that there is a positive treatment effect only for students that do not come from the worse socioeconomic backgrounds.

8. Conclusions

This chapter analyses the relative efficiency of private and public schools by looking at elementary schools in Chile in 1996. By introducing a more detailed set of control variables to account for selection and estimating models with selection correction terms "a la Heckman" this chapter has dealt with the traditional pitfalls of most of the studies of private versus public education: Selection Bias. Moreover, by introducing interaction terms of the observable characteristics and the private dummy it allows for the estimation of heterogeneous treatment effects and its distribution and not just an average treatment effect as all of the previous studies using Chilean data have done.

The results suggest that public schools are neither uniformly worse nor uniformly better than private schools. Rather, public schools appear to be relatively more effective for students from disadvantaged family backgrounds. Such a system of comparative advantage is consistent with the observation that public and private schools continue to

co-exist in most Chilean communes. Moreover, it is consistent with other features of the Chilean data, including the under-representation of disadvantaged students in the private schools (despite the fact that these schools are free), and in larger class sizes in private versus public schools.

The findings lead to policy recommendations that differ from those traditionally proposed. Since it is not true that public schools are worse, it is not necessary to eliminate them, as some have suggested. Additionally, since they are an important service to less advantaged kids, not only must we not eliminate them but also design policies focalized on those schools.

The objective of the second chapter is to evaluate the impact of a focalized government intervention program aimed at increasing the quality of the poorest schools. The goal is to provide estimates of the program impact in test scores taking into account both school and student characteristics. In particular this chapter will provide estimates that are free of omitted variable bias due to the presence of unmeasured school specific effects, which are correlated with participation, and compares them with uncorrected effects and oversimplified model assumptions usually used in the literature.

Chapter II. Evaluation of a Focalized Education Program: The P900

1. Introduction

The previous chapter demonstrated using Chilean data that, contrary to conventional wisdom about the superiority of subsidized and private schools over free public ones, the latter perform better during the first years of schooling for the children of more disadvantaged origin. The key explanatory factor seems to be that schools specialized in the education of different kinds of students. Public schools adapted the level of teaching to the reduced ability to learn of this specific group and rendered a better performance than what they would have obtained in a private school.

In addition, particularly in less developed countries, the potential freedom between public and private schools is limited and could involve high costs. This is so because the free public schools available for the less advantaged families are located in rural areas or in marginal zones of larger cities. Additionally, free private schools select students from less disadvantaged backgrounds that are easier to educate. Therefore, the choice is in some cases non-existent or imply a high transportation and time cost to exercise it.

This justifies focusing in the group of schools catering for the children of poor family background, over and above the general support provided by government to upgrade the quality of education in general. In fact, this will involve a positive discrimination for these establishments to reduce the existing gap on the supply side. This is what the Chilean government decided to do in 1990, as part of a more comprehensive policy aimed at raising the quality of primary and secondary education and at introducing more flexibility for school choice. The program focused on the 10% of schools that have registered the lowest achievements and mostly located in disadvantaged areas.

The objective of this chapter is to evaluate the impact of a focalized government intervention program aimed at increasing the quality of the poorest schools. The goal is to provide estimates of the program impact in test scores taking into account both school and student characteristics. In particular this chapter will provide estimates that are free of omitted variable bias due to the presence of unmeasured school specific effects, which are correlated with participation, and compares them with uncorrected effects and oversimplified model assumptions usually used in the literature.

2. The P900 Program

Consistently with the reforms aim of improving quality with equity and the recognition of the importance in students characteristics in the schools achievements the *Programa de Mejoramiento de la Calidad de las Escuelas Basicas de Sectores Pobres* or *Programa de la 900 Escuelas* (P900) is born in 1990. Based in the principles of positive discrimination, the program provides technical and material support (no cash) to 10% of the free schools that have the lowest achievement in the SIMCE exams and are located in the most disadvantaged neighborhoods.

The program supports the schools in four different areas: 1. Teacher training; 2. Special attention and help to students with higher education disadvantages; 3. Classroom library and didactic materials; and 4. Infrastructure improvements and repairs. Schools "graduate" when they exceed the regional average test scores and/or when they win a *Proyecto de Mejoramiento de la Educacion (PME)*. They may remain in the program for unlimited time. Some schools (most of them private) decide not to take

the program because of the stigma it carries, but practically 100% of the public schools that are entitled to the program take it.

The program is executed at the regional offices of the ministry. The Education Ministry sends out guidelines for the selection process and the implementation of the program, and the regional secretary of the ministry has some discretion on it. The secretary, in part based on information not observed by the researcher (or the public), such as school debt or personal evaluations selects schools. The implementation is also done at the regional level and may vary from one region to the other or even between schools in one region depending on the supervisor assigned to them.

The program is dynamic and flexible. It has changed according to the changes in the Chilean society and education system. Even though it has the same name and objective today than in 1990, in practice, it is a different program every year.

Its focalized nature is ideal for the experimentation with new education policies that, when found effective, are included as part of the universal programs of the ministry. One example of such programs is the provision of textbooks to all free schools that emerged from the pilot implementation in the P900 schools.

Until 1997 only primary education schools could enter the program based on the 4th grade Simce score and vulnerability index. Starting in 1998 the program was extended to include pre-school and secondary school. Eighth grade Simce scores were also considered in the assignment process.

The program has an approximated annual cost of US\$2.6 millions. It was financed through international cooperation from the Swedish and Danish governments in 1990-1991 and forms part of the national budget since 1992. The cost of the program

represents less than 1% of the total education budget and around 9% of the budget in primary education.

On average the annual number of schools participating in the program is 1000. The average number of covered teachers is 7,100 and the average number of covered students is 201,000, which represent around 8% of the total number of children enrolled in primary education (See table 6).

3. Evaluation of the Program's Effect

In theory, given the design of the selection guidelines, the evaluation of the P900 program should be straightforward. The way the program is presented to the public and the researcher makes it clear that schools are selected on the basis of their previous fourth grade average test score and their vulnerability, and since such information is public, a simple comparison of the schools in and out of the program, controlling by such characteristics, should be enough to identify the programs effect in test scores. Unfortunately, as explained earlier and corroborated in the data section, such guidelines are followed loosely and selection is made, in part, on the basis of characteristics unobserved by the researcher. If those unobserved characteristics affect the ability of the school to obtain a high-test score, not including them will lead to an omitted variable bias in the estimated program effects. If schools are selected in a (unobserved) compensatory manner, the uncontrolled comparison would give us a downward biased estimate of the real impact of the program since those schools would have lower achievement in the Since exams even after controlling for observed characteristics. On the other hand, positive unobserved selection would lead to an upward biased estimated effect.

Two approaches to this omitted variable bias have been used frequently in the literature. The first one is to account for the endogeneity of the program participation (i.e. school selection) using instrumental variables that are correlated with assignment into the program but not with the outcome variable (i.e. test scores). The second adjusts the estimated effects by using Heckman type selection correction terms in a second stage equation. Both approaches have drawbacks. In the IV case, you must be able to find the right instruments that satisfy the exclusion assumption. In the Heckman selection models the arbitrary distribution assumption may be unacceptable.

The use of panel data allows estimates of the treatment effect without making the above assumptions. In general, if the unobserved characteristics of the schools that are correlated with assignment into the P900 program and test scores are constant in time, then one can obtain unbiased estimates from fixed-effect models. Moreover, panel data allows for a model that incorporates the school effect without having to impose the additional restrictions of the traditional fixed effects models such as constant coefficients and fixed school effects. Additionally, the traditional fixed effects model can be nested in the general model and its restrictions tested.

The objective of this paper is to use panel data to obtain estimates of the P900 program effects that are free of omitted variable bias due to the presence of unmeasured school specific effects that are correlated with the participation variable. Special emphasis will be put in designing the least restrictive models and comparing the results to the more traditional/restrictive models. Additionally, it will analyze the temporal pattern of the effects as well as the different effects of the yearly programs since the programs are not exactly the same every year. The correlation of the fixed effect and the

participation variable will be explicitly considered in order to be able to estimate if and how the assignment discretion has changed in time and what its impact in the uncorrected estimated effect is.

4. The Model¹⁴

Using data on a panel of S schools observed over T years, we assume that test score of school s in time t (Y_{st}) is correlated with a set of fixed observed and unobserved characteristics (F_s and C_s), variable school characteristics (X_{st}), and a yearly P900 participation dummy ($P900_t$), as describe in equation 1.

$$(1) Y_{st} = \varphi'_t F_s + \alpha'_t X_{st} + \beta'_t P900_{st} + \gamma'_t C_s + \varepsilon_{st}$$

Where the fixed unobserved school effect (C_s) is uncorrelated with ε_{st} but possibly correlated with the other fixed and varying characteristics. In particular if schools are selected in a compensatory manner (though not observed by the researcher), then one expects a negative covariance between the unobserved school effect and the participation dummy that results in a downward biased treatment effect estimated from the cross section data uncontrolled for the fixed effect.

In general, if C_s is correlated with $P900_{st}$ it will also be correlated to its leads and lags, as expressed in (2)¹⁵. (Only three time periods are considered because of the data available).

$$(2) c_s = \lambda_1 P900_{s1} + \lambda_2 P900_{s2} + \lambda_3 P900_{s3} + \xi_s = \lambda' P900_s + \xi_s$$

¹⁴ This section follows closely Jackubson's (1991) paper.

¹⁵ C_s may also be correlated with the other X 's, but since we are only interested in estimating an unbiased treatment effect we will assume for simplicity that it is only correlated with the P900 dummy. Later on in the paper, when we include interactions between the P900 dummy and other variables, the correlation will be included explicitly.

Substituting C_s into (1) we obtain the model based on observable variables (3) which is a restricted specification of the reduced form unrestricted model in (4).

$$(3) Y_{st} = \varphi'_t F_s + \alpha'_t X_{st} + \beta'_t P900_{st} + \gamma'_t \lambda' P900_s + (\gamma'_t \xi_s + \varepsilon_{st})$$

$$(4) Y_s = \Phi X_s + \Pi P900_s + e_s$$

where $Y_s = (Y_{s1}, Y_{s2}, Y_{s3})'$, $P900_s = (P900_{s1}, P900_{s2}, P900_{s3})$ and $e_s = (e_1, e_2, e_3)$. Model (3) implies the following nonlinear restrictions:

$$(5) \tilde{\Pi} = \begin{bmatrix} \beta_1 + \gamma_1 \lambda_1 & \gamma_1 \lambda_2 & \gamma_1 \lambda_3 \\ \gamma_2 \lambda_1 & \beta_2 + \gamma_2 \lambda_2 & \gamma_2 \lambda_3 \\ \gamma_3 \lambda_1 & \gamma_3 \lambda_2 & \beta_3 + \gamma_3 \lambda_3 \end{bmatrix}$$

that can be tested against the unrestricted reduced form model using minimum distance estimators (Chamberlain 1982).

Using restricted GLS estimates of the effect of the program in test scores (β 's), the effects of the fixed unobserved school characteristics in test scores (γ 's) and the correlation between participation in the P900 program and the unobserved fixed characteristics (λ 's) can be obtained for different points in time. The increased flexibility of the model as compared to a model with fixed coefficients (i.e. traditional fixed effects models) seems desirable since the program has evolved and consequently its effect and biases probably have changed in time. We can compare our less restrictive model with the conventional fixed effect model and test the viability of the additional restrictions imposed:

$$(6) \tilde{\Pi} = \begin{bmatrix} \beta + \lambda_1 & \lambda_2 & \lambda_3 \\ \lambda_1 & \beta + \lambda_2 & \lambda_2 \\ \lambda_1 & \lambda_2 & \beta + \lambda_3 \end{bmatrix}$$

In sum, following Jakubson(1991) and Chamberlain (1982) the most unrestricted model will be estimated and additional restrictions imposed by alternative models will be tested against it using traditional specification tests. The traditional fixed effects results will be compared with less restrictive models and the acceptability of its assumptions will be evaluated. Results will also be compared to the cross section estimates and explicit estimations of the bias will be calculated.

5. Empirical Analysis

5.1. The Data

The data set used is obtained by merging several school level yearly data sets provided by the Chilean Education Ministry. The variables are: average school (math and spanish) since 4th grade exam scores for 1988, 1992, 1994, 1996; P900 participation dummies for 1990-1996; vulnerability index¹⁶ for 1990, 1992, 1993 and 1996; and 12 regional dummies. Unfortunately, at the time the research was done, the test scores for 1990 were unavailable and therefore we will consider only 1992, 1994 and 1996 P900 program effects.

The sample was restricted to public schools to avoid dealing with private schools not accepting to participate in the program. Such restriction to the sample should not be important since over the past 10 years more than 80% of the participating schools have been public. The findings in this paper will be interpreted as the effect of the P900 program in public schools and does not say what the effects on private subsidized schools is. On average we observe 3600 non-participating schools and 530 participating schools.

¹⁶ The vulnerability index is coded from 0 to 100 and its definition changes yearly in terms of the

The average characteristics for those in and out of the program can be seen in Table 7 and Graph 8. The data shows that participants on average have lower previous test scores than non-participants, which is consistent with the positive discrimination objective based on previous test scores. Additionally, the gap tends to decrease and even disappear in time and could be interpreted as showing a positive impact of the P900 program in test scores. Unfortunately, such an observation may not necessarily coincide with reality because the comparison group is not a good counterfactual for what the test scores would have been if they had not participated. This is so because participants are different from non-participants both in observed and unobserved ways.

If eligibility to the program was determined only by pre-test scores and vulnerability, the plots presented in graphs 9 and 10 would show all participating schools in the lower right area and non participants further to the left and up. This does not appear to be the case. We observe nonparticipating schools with lower test scores and higher vulnerability than participating schools. We also observe some schools that participate but have high-test scores and low vulnerability, and therefore should not be participating in the program. If the factors that explain such divergence between predicted and actual participation are correlated with the schools ability to obtain higher test scores, and the researcher does not observe them, estimating the effect of the P900 program in test scores by comparing participating and non-participating schools will be biased.

Table 8 presents the results of probit selection/participation regressions as a function of the observed characteristics. Following the policy design, we estimate the

characteristics included and the way they are weighted (i.e. it is not strictly comparable between years).

probability of participation as a function of previous test scores and vulnerability (except for 1992, where test scores for 1988 were used due to the lack of 1990 test scores). Regional dummies are also included since assignment is done at the regional level. As expected, the probability of participating in the P900 program increases as the previous test scores decrease and is highly significant. In terms of the vulnerability index, it is positively correlated with the participation probability in 1994 and 1996, but negative in 1992. Thus reflecting divergences in the selection even based on observed characteristics.

The probability of correctly predicting participation is between 62 and 69%. Thus indicating the presence of unexplained participation. If the portion of participation that cannot be explained by the observed variables is randomly assigned between schools it does not present a problem. But if it depends on characteristics that affect the schools test outcomes, then uncontrolled comparisons between participants and non-participants will give biased estimates of the program effects. We will deal with such possibility by explicitly estimating the correlation of the participation dummies with fixed unobserved school effects as mentioned in the previous section. Additionally, since it appears that not all years are equally predictable, we will allow for a varying correlation/bias in time.

5.2. Cross-Section Estimates

The first set of estimates shown in Table 9 consists of the traditional cross section OLS estimates that ignore the time pattern of the data and unobserved school effects in the analysis. It is what the most naïve researcher would do and therefore will be used as a benchmark. All the regressions control for current and lagged vulnerability, lagged test scores, regional dummies and total previous years in the P900 program. The 1992

equation differs from the other two by controlling for the 1988 test instead of the 1990 test, due to the lack of data for that year.

The estimated effects of the program appear to be substantially different every year, ranging from significantly negative in 1992 to positive in 1996. If we take these estimates at face value we would conclude that the program initially had a negative impact, but as the program matured it became effective in improving the schools relative test score. Such conclusions would be wrong if, as explained earlier, schools are selected based on unobserved ability to produce higher/lower test scores, since the estimates are biased. Additionally, if the program executors, eligibility guidelines and the strictness of its application change in time, the bias may change in size and direction impeding us to infer the real yearly effect or even if the estimates are lower or upward bounds without further analyzing selection every year.

Assuming no omitted variables bias the estimates (i.e. initially negative and then positive), may be implying that program participation takes time to impact test scores. Probably, as the program matured and was included as a regular ministry program, it became more effective in generating a faster improvement in test scores¹⁷. The second part of table 9 includes lagged participation dummies to capture the possibility of such delay in the programs' impact. The coefficients reported are for the current and lagged participation dummies. The immediate or current effects are presented in the first row and are conceptually equivalent to those presented earlier. The signs and significance remain. The diagonal shows the impact of the 1992 P900 program in 1992, 1994 and 1996 test scores. It does not appear as if the program's positive effect just took time to show up.

¹⁷ This would be the case if the administration became more efficient in delivering the resources or if the

The effect on 1994 is still significantly negative and null for 1996. These findings could be interpreted as the program having a negative effect but it could also be the reflex of the underlying characteristics of the schools selected that would have performed even worse without the program. The diagonal for the 1994 program presents a positive current effect and no effect in 1996.

In sum, the cross-section regressions find different program effects in time. The 1992 program appears to be detrimental to the schools test scores. The 1994 and 1996 programs appear to be effective in increasing the test scores of the affected schools. These results could be the outcome of the evolution of the program that has been modified to better serve the participating schools. But it could also be masking the real outcomes by not including the biases that arise from the non-random selection process. Such biases may be varying in time and therefore they may not even reflect an upper or lower bound for each year misleading the reader to the wrong conclusions. The following section uses panel data and explicit assumptions on the correlation of the participation dummy and the fixed unobserved school variables to explore these possibilities.

5.3. Panel Data Estimates: Explicit modeling the correlation between C_s and $P900_{st}$

Following Chamberlain (1982) and Jakubson (1991) various panel data models are estimated by first estimating the unrestricted regression that includes all leads and lags of the P900 dummy variable (i.e. equation 4 in the model section) and then testing

training was focused in short run effects.

the restrictions the models impose in the reduced form π matrix. If we use the minimum distance estimator

$$\arg \min((\Pi - f(\delta)\Omega^{-1}(\Pi - f(\delta)))$$

together with an appropriate estimate of Ω to estimate the restricted parameter set δ , then the resulting estimates are asymptotically efficient. The optimal estimate of Ω in this case is given by the sample covariance of w_s , where

$$w_s = (y_s - \Pi P900_s) \otimes S_{P900}^{-1} P900_s$$

and S_{P900} is the sample variance of the P900 dummy variables. Note that it does not imply independence within each school nor homoskedasticity.

Table 10 reports the unrestricted π matrix (i.e. the coefficients on the leads and lags of the P900 dummy). The model also includes current and previous vulnerability, previous test score, total previous participation years, and regional dummies¹⁸. The reported standard errors are heteroskedastic consistent. The unrestricted effects follow the same pattern as those presented in the cross-section regressions (see the diagonal elements of table 10): negative for 1992; positive and increasing for 1994 and 1996. All highly significant. Thus suggesting that the initial implementation of the program was deficient, but as the program matured it was modified and made efficient. But, as stated earlier, this interpretation is not necessarily true given the possible bias that arises from unobserved nonrandom selection/sorting of schools.

If previous participation does not affect current test scores, then the coefficient for non-current participation dummies should be different from zero only if they are

¹⁸ Variables are included as deviations from the mean.

correlated with the unobserved school effect. Such correlation appears possible, as the off-diagonal elements in table 10 are significantly different from zero.

Table 11 presents the estimated program effects of the successively restricted models. The first model is the least restrictive one. It assumes the existence of an explicit form for the correlation of the school effect and the participation dummies but allows complete flexibility in the coefficient estimates in time for all variables including the effects of the program, the school fixed effects and the correlation terms.¹⁹ Different effects in time are expected given the dynamic nature of the program design, and therefore allowing for different β 's is desirable. Additionally, the size and sign of the omitted variable bias is jointly determined by the school effect and the correlation coefficient (γ and λ). Not allowing for them to vary may be seriously affecting the results. Fortunately, the framework allows for us to test such restrictions.

There is no evidence against the restrictions imposed by our most general model described in equation (3) or restriction (5) (i.e. the $\chi^2(1)=0.06$ is not rejected)²⁰. The estimated effect of the 1992 P900 program is not statistically different from zero. The effects for both the 1994 and 1996 programs are positive and significant (3.90 and 9.34, respectively). The estimated fixed school effects are positive and significant. The correlation between the P900 participation dummy and the unobserved school fixed characteristics (λ 's) is negative and statistically significant in the three years.

The model suggests that in every year considered sorting/selection into the program is compensatory (i.e. negative λ 's). Schools that participate in the program have

¹⁹ Corresponds to imposing the restrictions in (5) above.

²⁰ γ_1 is normalized to 1.

unobserved characteristics that make them less able to achieve a good test score, conditional on the vulnerability of their kids, previous test scores, and regional distribution. Such negative selection implies that comparisons of schools in and out of the program is misleading since the participating schools are in worse shape and would perform worse than the other schools had they not participated in the program. In other words, the uncontrolled effects are underestimated (i.e. biased downward). Moreover, the bias is different every year and increasing in time suggesting that compensatory selection has grown with the programs successive implementations.

The rest of table 11 tests additional restrictions to the model up to the traditionally estimated fixed effects model and compares the results from such a restrictive model with the ones obtained above.

The first model (part II) assumes a constant effect of the P900 program. That is, the P900 program in 1992, 1994 and 1996 are equivalent in terms of how much they help the schools in their relative performance. The estimated program effect is positive and significant, but the model is rejected when compared to the previous less restrictive model and to the fully unrestricted one. Thus confirming the intuition that every year the program is different, in terms of the resources they provide, as well as the way they provide them, and therefore each yearly program must be considered as an independent unit.

Part III estimates a model in which the school fixed effect is constant (and equal to one). Such model is not rejected by the data when compared to the fully unrestricted model, but is rejected when compared to the least restrictive model in Part I of table 5. The estimated program effects are similar to those estimated in our less restrictive model,

but smaller in magnitude. The correlation coefficient is now larger and significant at 99% confidence every year.

Thirdly, part IV analyzes the possibility that the bias due to unobserved non-random selection is the same every year. It assumes that the correlation between the unobserved fixed variables and the P900 participation dummies is fixed. Again we reject the model when compared to our least restrictive model (i.e. additional chi-square(2) is 7.05), implying that even if selection based on unobserved fixed school characteristics is compensatory every year it is not the same for all years and thus cannot not be estimated as one. The estimated program effects have the same signs as those obtained above, but their dispersion is less. This is because the effects are now corrected by the average bias which is low in 1996 and high in 1992 if relation to the real bias.

Traditionally researchers estimate a fixed effects model that assumes constant program effects in time (which we already showed was rejected by the data) and constant school effects (also rejected by the data). The results of imposing such restrictions in the data is presented in part V. The estimated constant program effect is positive and significant (3.79), the correlation coefficients are negative and significant. The underlying conclusion is that the program is efficient in improving test scores and therefore we should keep investing in it. Unfortunately, the studies do not test the validity of such results in terms of the underlying restrictions as we do in this paper. If they did, they would find evidence (as we did) against their restrictive model that would shed lights on the simplified conclusion.

The last set of results in Table 11 presents the traditional complete fixed effects model that would be obtained from any panel data estimation software. The additional

restrictions are constant coefficient for all other variables included in the model. Obviously the model is rejected. Still, the estimated program effect is positive and significant.

In sum, we find that the traditional fixed effects model is not supported by our data and that the less restrictive model estimated above is more appropriate. It suggests that schools selected to participate in the program have a lower unobserved ability to obtain higher test results than similar schools (i.e. schools with similar vulnerability, region, previous test scores, etc). In other words, schools are selected in a compensatory manner and thus the estimated effects that do not control for such selection underestimate the real effects. Moreover, we find significantly that such selection is different every year and higher for the latter one.

Additionally, the results suggest the programs differ from year to year in terms of its effect on test scores. When controlling for the bias that arises from selection based on fixed unobserved characteristics the 1992 P900 program appears to have negative effects in test scores. Whereas the 1994 and 1996 programs appear to have been effective in increasing the test scores of participating schools. Moreover, the effect is bigger for 1996.

6. Conclusion

The paper evaluates a focalized education program implemented in Chile since 1990. Its positive discrimination nature has lead to both adherents and adversaries and the lack of serious empirical evaluations has left the debate in a highly theoretical level. The debate has been centered on totally uncontrolled comparisons of the schools in and out of

the program and cross-section regression type analysis that control for very few observed school characteristics.

The ministry's web site and media communications argue that the program is highly effective by presenting a table of the changes in the P900 average test scores and that of the rest of the free schools. They argue the effectiveness of the program by looking at a larger increase in average test scores for the P900 schools than the rest of the schools. Obviously this comparison is misleading since increases in the test scores at lower levels is much easier than at higher level. Also, it does not tell us anything about how the selected schools would have performed if the program had not been implemented, since they differ systematically from the schools that are not selected into the program both in observed and unobserved ways.

Cross-section regression analysis is better, at least it allows for comparisons between schools that are similar in observed characteristics. This would be enough if the schools in the program were selected based only on the characteristics that are observed by the researcher. Unfortunately, contrary to what the program design specifies, selection is not totally explained by observed characteristics, thus rendering biased cross section estimates.

This chapter tries to contribute to the discussion by estimating the effect of the P900 program free of omitted variable bias due to the presence of unmeasured school specific effects, which are correlated with participation. And does so by using panel data techniques proposed by Chamberlain (1982). It explicitly estimates the biases that arise from this non-random selection on fixed characteristics and estimates individual effects

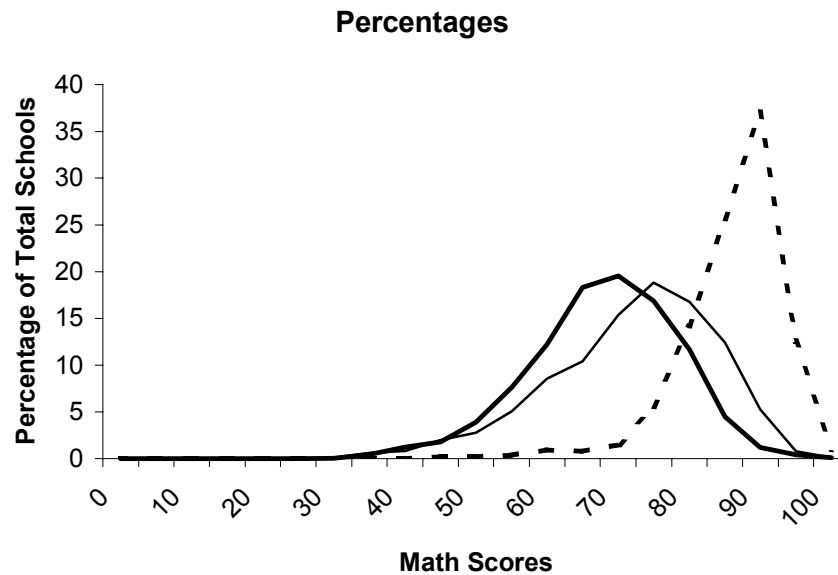
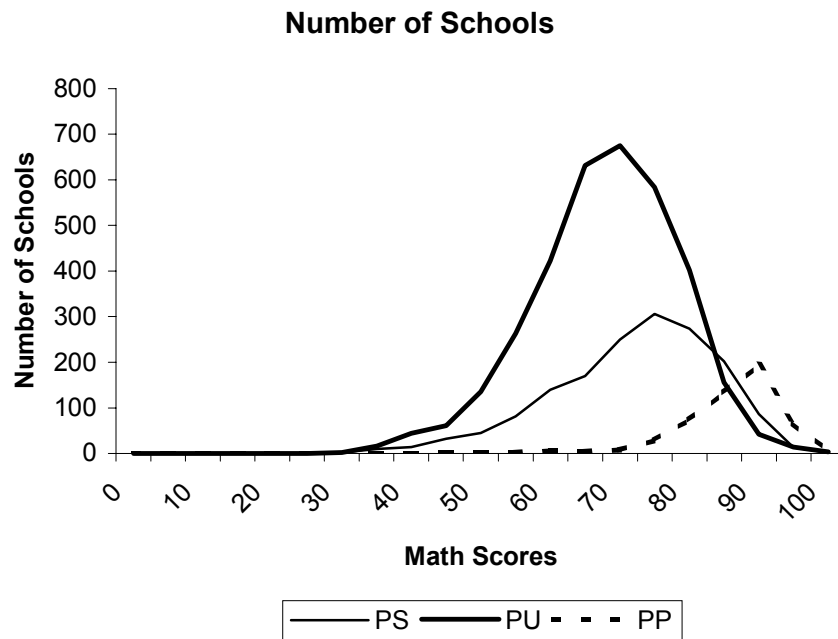
for each year analyzed. It further compares the results to those obtained by the traditional cross-section regressions and fixed effects models.

The findings suggest that the schools that were selected into the P900 program were selected in a compensatory manner, even after controlling for vulnerability and previous test scores. That is, selected schools have a lower ability to achieve a high-test score than schools with similar observed characteristics. Thus leading to downward biased uncontrolled estimates. Moreover, the bias is increasing in time.

Additionally, the findings suggest that the effect of the P900 program is different every year and that it has increased in efficiency as the program has become consolidated in the regular Ministry of education programs. The estimated effect for the 1992 program is not significantly different from zero, as opposed to negative uncontrolled effects. The effects for 1994 and 1996 are significantly positive and higher than the cross-section estimates. The effect for the 1996 program is significantly higher than the previous ones.

To sum up, the program has proven to be effective to shorten the achievement gaps. A learning process in the implementation allowed for an increased efficiency. The researcher is tempted to conclude with one final suggestion. While the flexibility given to the regional secretaries in the selection of the schools to be benefited by the program permits to adapt to local and school specificities not incorporated in the general selection criteria, the ministry should request and tabulate the main arguments to include or exclude schools that meeting the general selection criteria were not program beneficiaries. This would allow not only for better analysis of performance in future studies, but also and perhaps more importantly could serve to identify systemic factors that could improve the program design.

**Graph #1 Distribution of the Average 4th Grade
Math Score by Type of School (1996)**



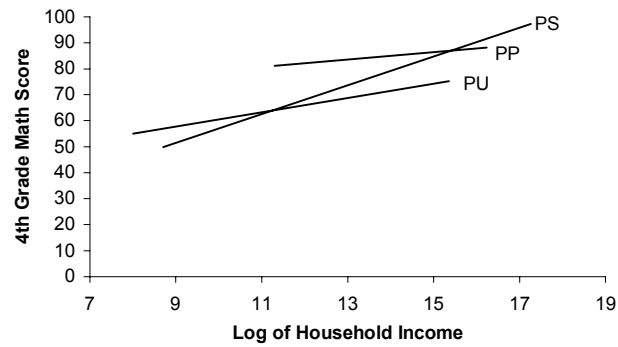
	PS	PU	PP
Average Scores		69	66
Std. Deviation		12	10

Test for Equal Distributions (Komogorov-Smirnov)

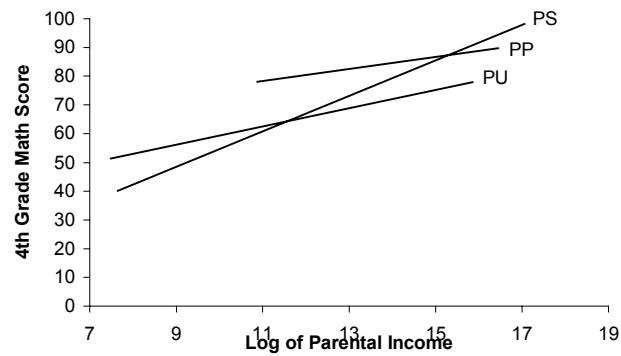
PU vs PS	1.000	
PU vs PP	0.006	*
PS vs PP	0.010	*

* Reject Equal Distribution at 95% Confidence

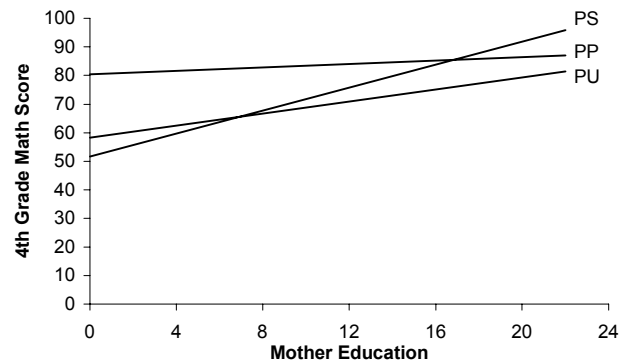
Graph #2 Plot of Math Score * Log Household Income



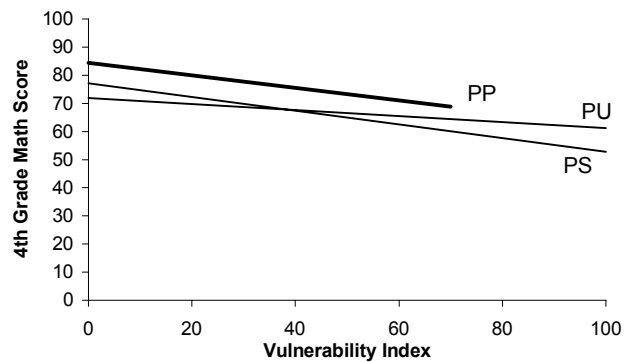
Graph #3 Plot of Math Score * Log Parental Income



Graph #4 Plot of Math Score * Mother Education

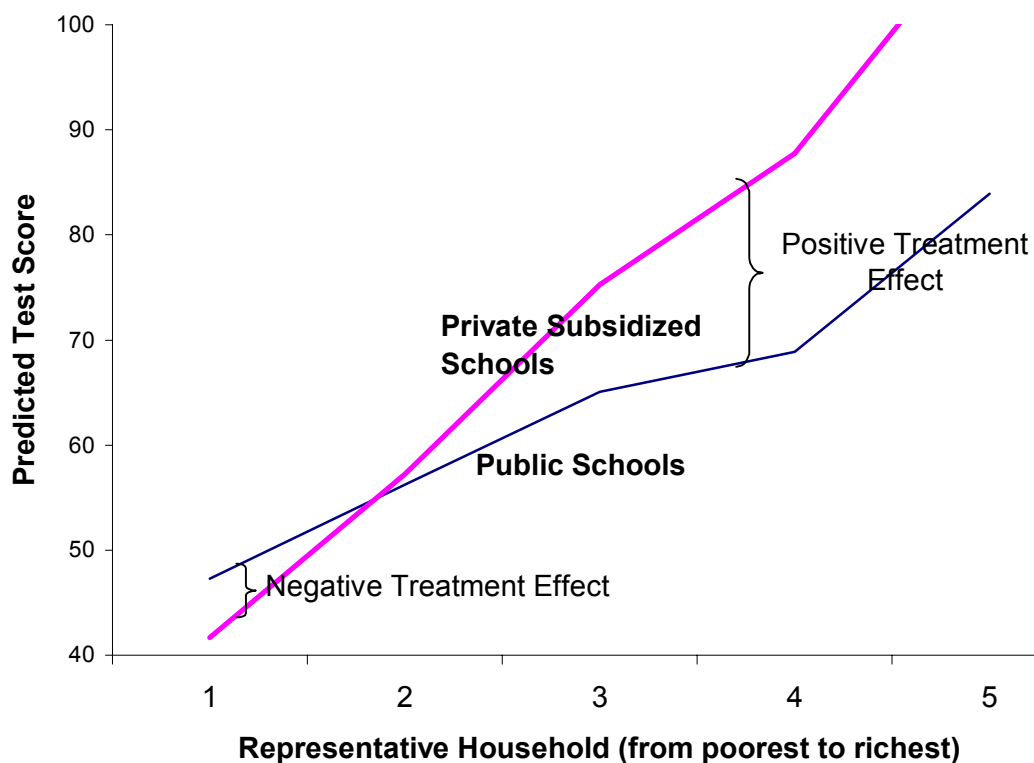


Graph #5 Plot of Math Score * Vulnerability Index



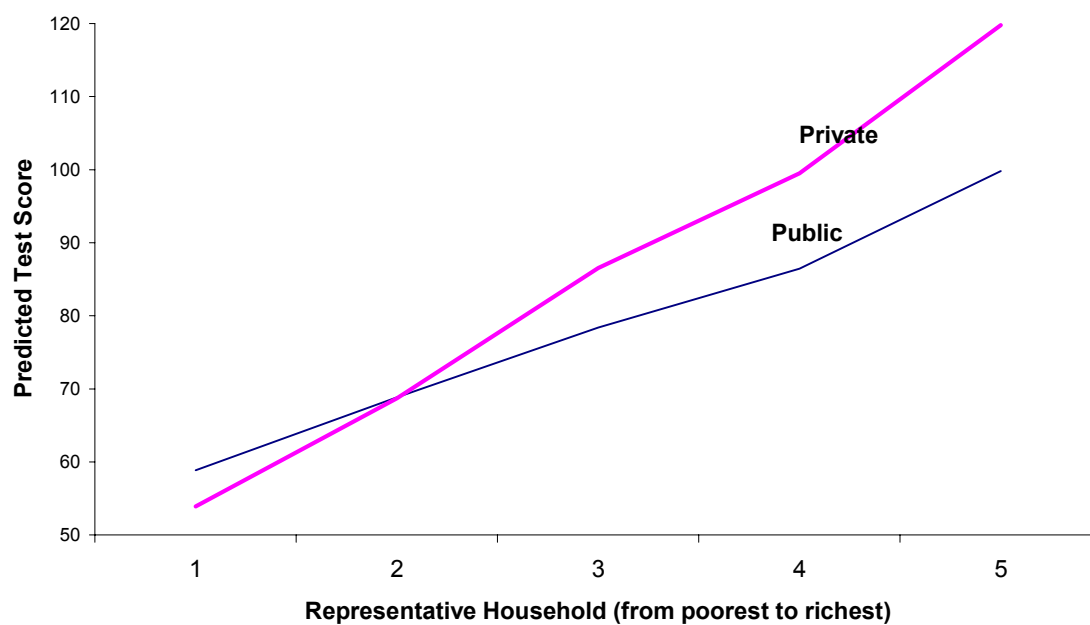
Graph #6. Predicted Test Scores for Five Representative Households

Average School Characteristics for PS and PU Used in Estimation



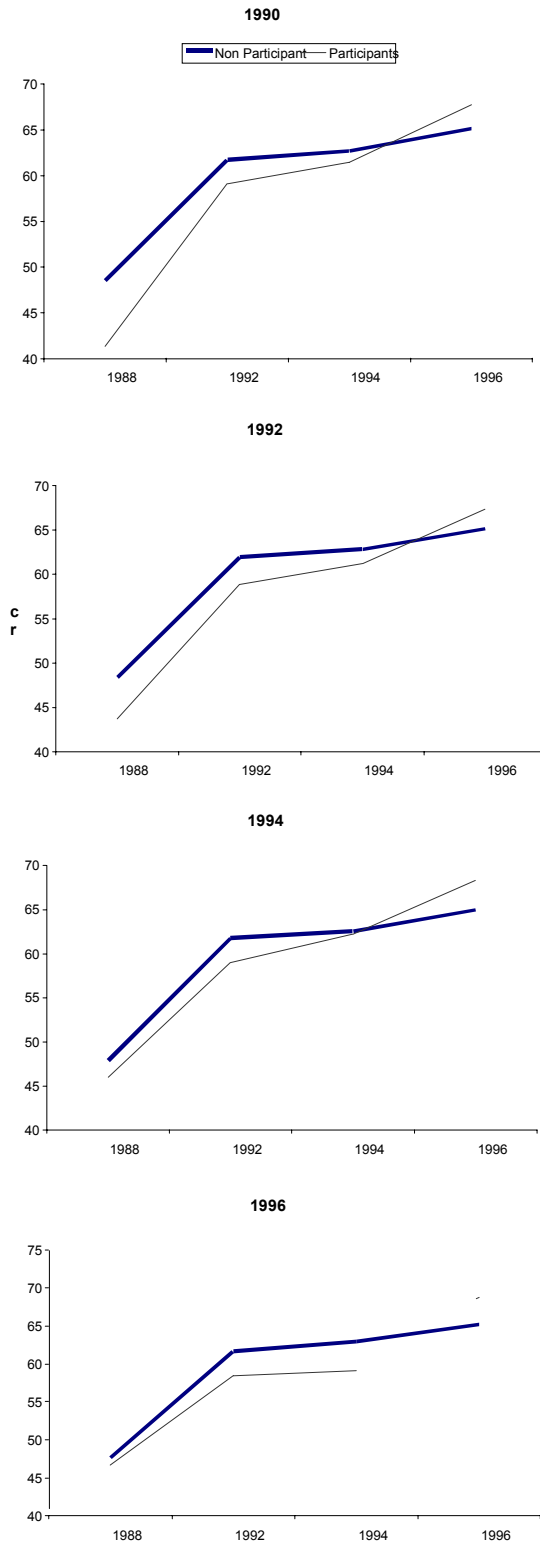
Representative Household Data					
	1	2	3	4	5
Vulnerability Index	100	75	50	25	0
Number of people in the household	10	8	6	4	2
Poverty line index	1	1.5	2	1.5	3
Log of Household Total Income	6	8	12	14	17
maternal education	0	4	8	12	18
paternal education	0	4	8	12	18

Graph #7. Predicted Test Scores for Five Representative Households From the Selection Models
Average School Characteristics for PS and PU Used in Estimation



Representative Household Data					
	1	2	3	4	5
Vulnerability Index	100	75	50	25	0
Number of people in the household	10	8	6	4	2
Poverty line index	1	1.5	2	1.5	3
Log of Household Total Income	6	8	12	14	17
maternal education	0	4	8	12	18
paternal education	0	4	8	12	18
Predicted Probability of being a Private School	0	0.25	0.5	0.75	1

Graph 8
Test Scores by Yearly Participation

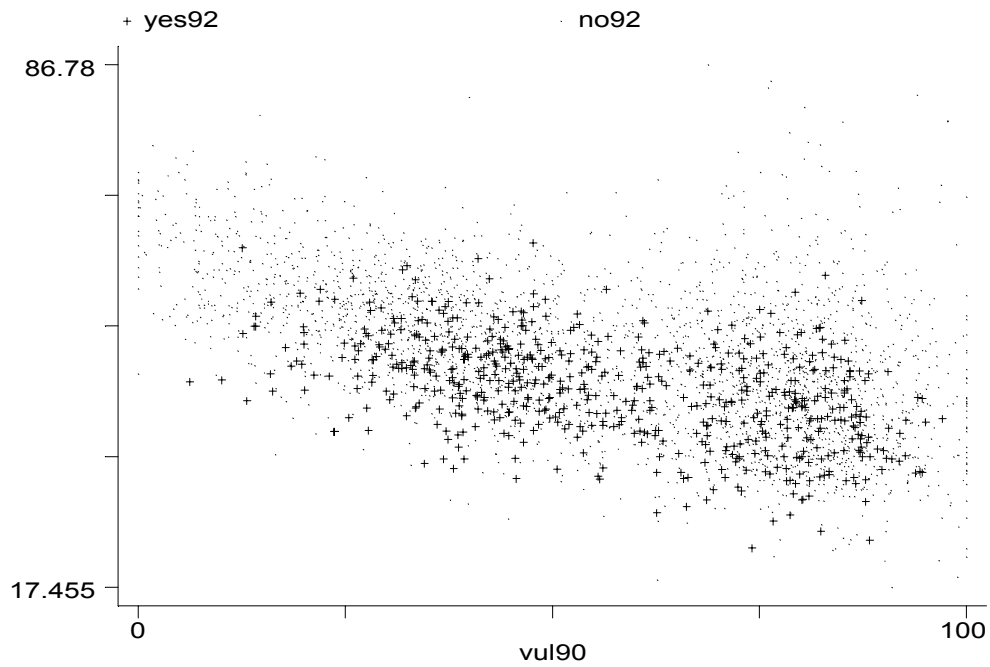


Graph 9

P900 Selection by Year (Test $t-1$ * Vulnerability $t-1$)

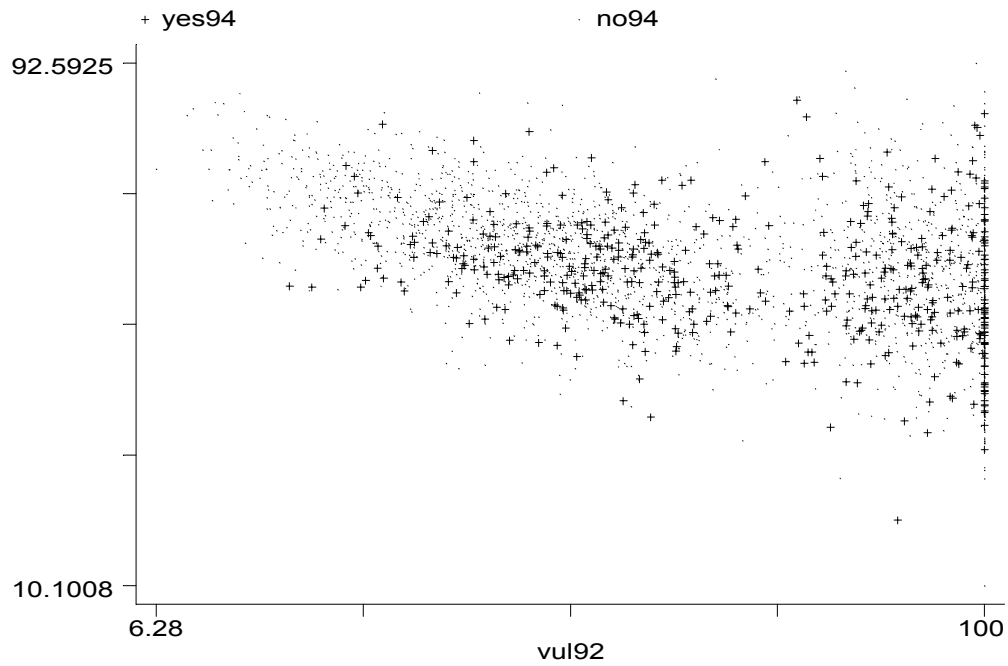
P900 in 1992

Test Score 1990 * vul 1990

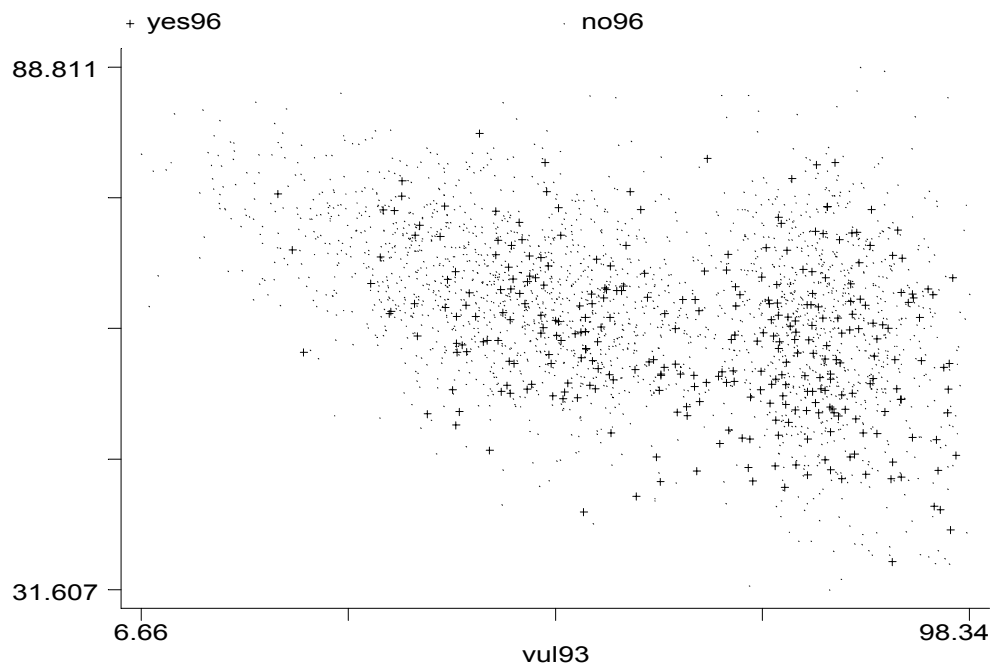


P900 1994

Test Score 1992* Vul 1992



P900 1996
Test Score 1994 * Vul 1993



Graph 9

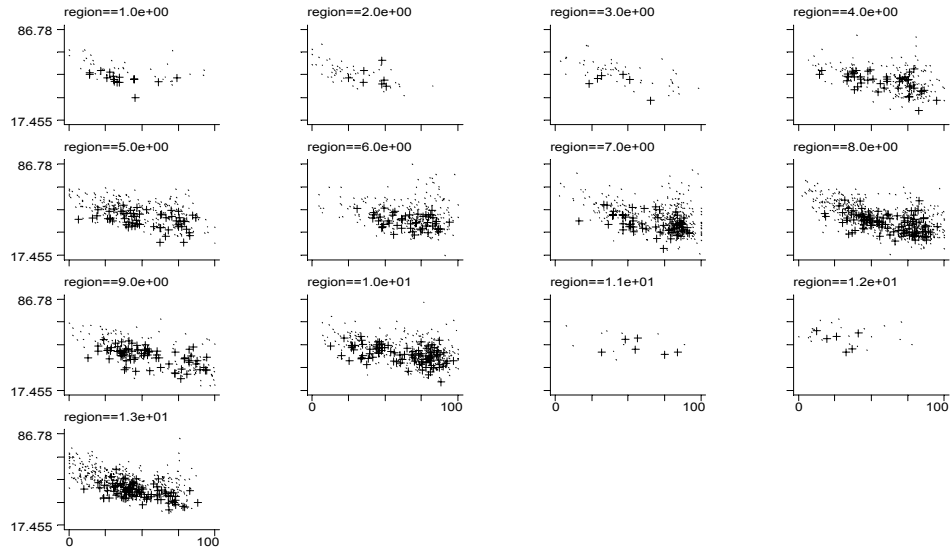
P900 Yearly Participation By Region (Test t_{-1} * Vulnerability t_{-1})

1992 P900 Program

Test Score 1988 * Vul 1990

+ yes92

no92



vul90

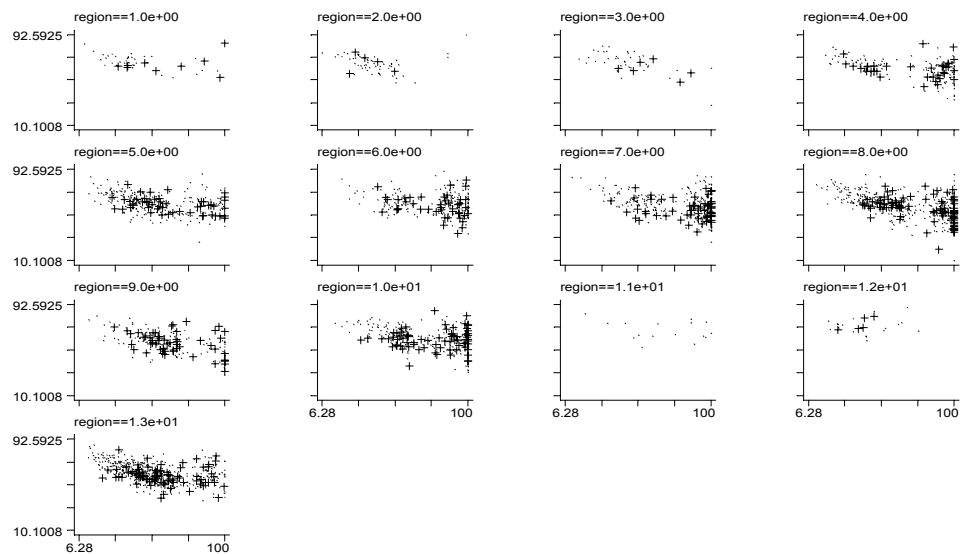
Graphs by region

1994 P900 Program

Test Score 1990 * Vul 1990

+ yes94

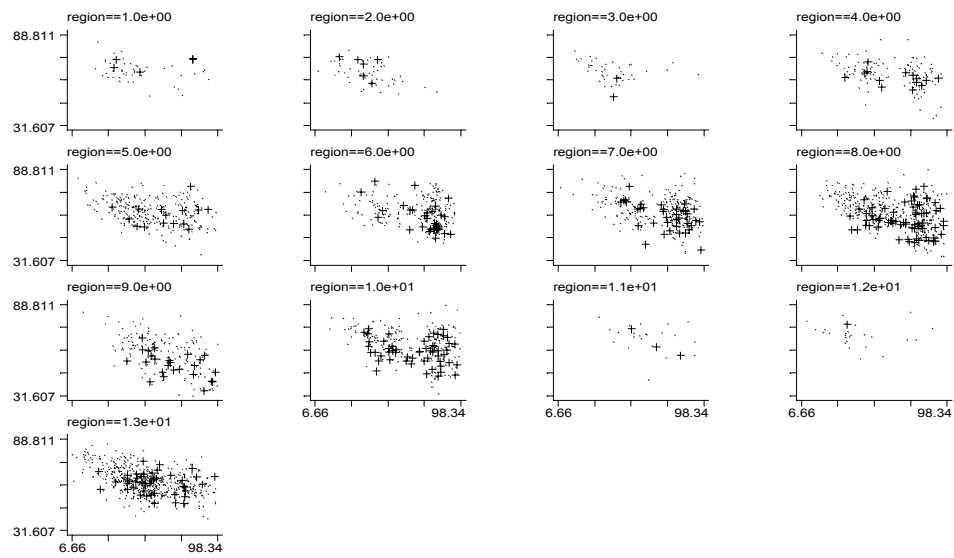
no94



vul92

Graphs by region

1996 P900 Program
Test Score 1994 * vul 1993
+ yes96



no96

vul93
Graphs by region

Table 1
Sample Means

	Public Schools		Private Subsidized		Paid Private Schools	
	N	Mean	N	Mean	N	Mean
School Characteristics						
rural dummy	3470	0.53	1635	0.21	525	0.12
4th grade enrollment	3470	44.89	1635	53.06	525	39.35
total enrollment	3441	353.28	1547	431.93	470	342.90
% of male only schools	3470	0.01	1635	0.02	525	0.05
% of female only schools	3470	0.01	1635	0.06	525	0.08
4th grade class size	2432	30.06	1313	34.83	465	24.50
class size	2727	31.06	1390	35.60	468	24.08
Teacher Characteristics						
N. of years of teaching experience	3408	18.20	1588	12.60	451	10.96
Hours worked	3409	26.34	1588	24.07	451	23.27
Contractual hours	3409	31.89	1588	28.71	451	28.37
% of teachers with education degree	3409	0.97	1588	0.91	451	0.96
% of male teachers	3409	0.32	1588	0.28	451	0.26
Family Background Characteristics						
Vulnerability Index	3466	59.83	1572	32.39	438	0.62
Number of people in a the household	2337	5.37	1330	5.06	402	4.75
Poverty line index	2336	2.46	1330	2.65	401	2.97
Age of the students	2337	10.34	1330	10.04	402	9.87
Education of the students	2337	4.33	1330	4.21	402	4.20
Dummy for full day school	2337	0.13	1330	0.12	402	0.27
Household Total Income	2033	266699.55	1145	411245.16	334	1678574.51
Maternal total income	2046	88964.40	1162	142515.77	347	563643.99
Paternal total income	2275	179317.01	1280	291994.66	379	1262193.18
maternal education	2322	7.74	1326	9.52	402	13.94
paternal education	2281	7.79	1285	9.82	380	14.67
maternal age	2330	37.91	1328	37.77	402	38.33
paternal age	2289	41.09	1287	40.74	380	41.86
parental education	2331	7.76	1328	9.65	402	14.27
parental total income	2323	253967.87	1322	407985.24	400	1684889.21
Test Scores						
Math	3440	66	1631	69	527	84
Spanish	3422	65	1616	70	523	84

Table 2
Average 4th Grade Test Scores by School Type

		Public		Private Subsidized		Paid Private		RELATIVE DIFFERENCE	
		SCORE	N. SCHOOLS	SCORE	N. SCHOOLS	SCORE	N. SCHOOLS	PS/PU	PP/PU
Average School Parental Education									
NONE	MATH SCORE	59	34	57	17			-4.0%	
	SPANISH SCORE	60	34	58	18			-2.5%	
INCOMPLETE ELEMENTARY	MATH SCORE	66	1985	64	514	70	4	-1.9%	
	SPANISH SCORE	66	1979	65	511	74	3	-1.7%	
INCOMPLETE HIGH SCHOOL	MATH SCORE	70	583	74	676	68	3	4.2%	
	SPANISH SCORE	71	582	75	673	72	3	5.2%	
INCOMPLETE COLLEGE	MATH SCORE	78	40	79	225	75	8	1.6%	
	SPANISH SCORE	79	40	81	223	79	8	2.2%	
MORE	MATH SCORE			85	7	81	4		
	SPANISH SCORE			86	7	81	4		
MOTHER'S EDUCATION (CASEN)									
INCOMPLETE ELEMENTARY	MATH SCORE	64	2364	63	634	80	126	-2.2%	20.2%
	SPANISH SCORE	64	2349	63	622	81	123	-1.4%	21.7%
COMPLETE ELEMENTARY	MATH SCORE	65	91	69	46	76	5	6.8%	15.2%
	SPANISH SCORE	64	90	70	46	78	4	7.8%	17.2%
COMPLETE HIGH SCHOOL	MATH SCORE	66	51	74	69	82	53	10.9%	19.5%
	SPANISH SCORE	66	51	75	68	84	53	12.6%	21.8%
MORE THAN HIGH SCHOOL	MATH SCORE	72	63	78	198	85	302	7.7%	15.2%
	SPANISH SCORE	74	62	79	198	86	302	7.2%	14.1%
RURAL/URBAN									
0	MATH SCORE	68	1643	72	1277	84	462	5.6%	19.3%
	SPANISH SCORE	68	1639	73	1271	85	458	6.3%	19.6%
1	MATH SCORE	64	1797	62	354	82	65	-3.3%	22.2%
	SPANISH SCORE	63	1783	62	345	83	65	-2.3%	24.0%
GIRLS SCHOOL									
	MATH SCORE	72	51	79	94	86	43	8.7%	15.8%
	SPANISH SCORE	74	50	82	94	87	43	9.0%	15.1%
BOYS SCHOOL									
	MATH SCORE	71	31	81	32	86	30	12.6%	17.6%
	SPANISH SCORE	71	29	81	32	86	30	11.9%	17.5%
VULNERABILITY INDEX									
1	MATH SCORE	73	278	76	765	84	523	4.0%	13.2%
	SPANISH SCORE	74	276	77	762	85	520	4.4%	12.7%
2	MATH SCORE	68	855	69	405			1.9%	
	SPANISH SCORE	68	855	70	400			2.2%	
3	MATH SCORE	66	667	62	184	60	2	-4.8%	
	SPANISH SCORE	65	666	63	185	63	1	-4.8%	
4	MATH SCORE	65	504	59	97	73	2	-11.1%	
	SPANISH SCORE	65	501	59	94	73	2	-9.9%	
5	MATH SCORE	62	1136	56	180			-10.5%	
	SPANISH SCORE	61	1124	55	175			-10.8%	
4TH GRADE CLASS SIZE									
<16	MATH SCORE	63	1197	61	373	80	143	-2.6%	21.0%
	SPANISH SCORE	62	1184	61	364	81	139	-0.9%	23.6%
>15 & <31	MATH SCORE	66	947	69	337	84	269	4.1%	21.8%
	SPANISH SCORE	66	944	70	335	85	269	5.2%	22.4%
>30	MATH SCORE	68	1205	73	883	86	100	7.2%	21.4%
	SPANISH SCORE	68	1203	74	879	87	100	7.9%	21.5%

Note: When sample size is too small, the relative difference is not computed.

Table 3

**Impact of Sequentially Including Controls on the Estimated Intercept
Difference between Private Subsidized and Public School Production Functions**

	PS coef.	std error	F-stat	R2 adj
URBAN + RURAL Private Subsidized and Public Schools N=2789				
Uncontrolled	4.07	0.45 *	82.65 *	0.03
School Controls	4.77	0.52 *	37.65 *	0.12
SES Controls	-0.14	0.50	75.67 *	0.30
Interactions Included	-0.22	0.57	58.67 *	0.32
URBAN Private Subsidized and Public Schools N=2219				
Uncontrolled	4.36	0.24 *	140.97 *	0.06
School Controls	6.32	0.43 *	59.29 *	0.19
SES Controls	2.01	0.42 *	85.70 *	0.36
Interactions Included	1.53	0.42 *	67.45 *	0.39
RURAL Private Subsidized and Public Schools N=569				
Uncontrolled	-0.35	1.37	0.07	0.00
School Controls	1.02	1.52	3.57 *	0.04
SES Controls	-0.17	1.44	7.91 *	0.15
Interactions Included	-2.06	1.76	6.20 *	0.16

Note: First three rows of each panel have no interaction terms and the PS coef is the vertical distance between parallel production functions (i.e. constant additive treatment effect). The fourth row is the PS coefficient for the model with interactions of PS with (Xi-X) and corresponds to the vertical distance between non-parallel production function at the mean value of X.

* = statistically significant with 95% confidence.

Weights= Number of students form CASEN/total enrollment

Table 4
OLS Regression Results

Variable	Sample: Public and Private subsidized Schools								Sample: Private Subsidized Schools		Sample : Public Schools	
	MODEL I		MODEL II		MODEL III		MODEL IV		Coef	S.E.	Coef	S.E.
intercept	66.58	0.22 *	59.12	2.19 *	47.23	4.60 *	58.59	5.33 *	31.83	5.93 *	59.03	6.21 *
private subsidized dummy	4.07	0.45 *	4.77	0.52 *	-0.14	0.50	-0.22	0.57				
rural dummy			0.37	0.48	2.49	0.50 *	1.50	0.53 *	5.58	1.03 *	1.44	0.61 *
class size			-0.13	0.02 *	-0.15	0.02 *	-0.20	0.02 *	0.06	0.03 *	-0.20	0.02 *
male school			6.98	2.03 *	2.80	1.81	1.68	1.79	4.65	1.75 *	-0.55	2.96
female school			5.99	1.22 *	3.67	1.09 *	2.41	1.08 *	1.55	0.99	2.51	2.01
number of teachers			0.24	0.02 *	0.07	0.02 *	0.07	0.02 *	0.06	0.03 *	0.07	0.03 *
N. of years of teaching experience			0.07	0.03 **	0.03	0.03	0.04	0.03	0.05	0.03	0.04	0.04
Hours worked			-0.05	0.03 *	-0.02	0.03 *	-0.01	0.03	-0.04	0.04	-0.01	0.03
% of teachers with education degree			9.33	1.99 *	9.82	1.77 *	8.18	1.77 *	9.16	1.99 *	7.25	2.58 *
% of male teachers			-3.90	1.16 *	0.31	1.05	-0.20	1.04	-7.84	1.63 *	1.79	1.32
Vulnerability Index					-0.11	0.01 *	-0.11	0.01 *	-0.13	0.02 *	-0.11	0.01 *
Number of people in a the household					-0.92	0.22 *	-0.95	0.21 *	-1.09	0.31 *	-0.91	0.27 *
Poverty line index					4.76	0.61 *	5.17	0.68 *	2.38	0.94 *	5.21	0.77 *
Log of Household Total Income					0.41	0.40	-0.05	0.47	1.40	0.50 *	-0.09	0.54
maternal education					1.20	0.14 *	1.09	0.16 *	1.24	0.20 *	1.12	0.18 *
paternal education					-0.29	0.13 *	-0.61	0.15 *	0.32	0.17 **	-0.62	0.17 *
class size							0.26	0.04 *				
rural dummy							3.93	1.52 *				
Vulnerability Index							-0.03	0.03				
Poverty line index							-2.12	1.44				
Log of Household Total Income							1.29	0.82				
maternal education							0.21	0.31				
paternal education							0.89	0.28 *				
N	2789		2789		2789		2789		982		1806	
R2	0.03		0.12		0.3		0.32		0.55		0.21	

Note: * is significant at 95% confidence level. ** is significant at 90% confidence.

Interaction terms is the Private Subsidized Dummy interacted with the deviation of the X from its mean.

Weights=Number of students from Casen/Total enrollment

Table 5
Selection Correction Coefficients in the Heckman Selection Models

	lps		lpu		F-stat	R2 adj
	coef	st error	coef	st error		
Absolute Advantage Model: Equal Covariance Between Selection and Test Scores						
Constant Treatment Model	-20.27	6.25 *	-20.27	6.25 *	64.11 *	0.29
Heterogenous Treatment Model	-19.16	6.18 *	-19.16	6.18 *	50.81 *	0.31
Comparative Advantage Model: Unequal Covariance Between Selection and Test Scores						
Constant Treatment Model	-33.10	7.66 *	-16.47	6.39 *	61.19 *	0.29
Heterogenous Treatment Model	6.73	11.89	-28.22	7.12 *	48.13 *	0.31
Private Schools Only	1.76	8.57			65.19 *	0.52
Public Schools Only			-28.60	8.10 *	30.14 *	0.21

Note: Constant treatment effect models are those with no interaction terms of PS with X. The heterogenous treatment model includes interactions of PS with the deviation of the X's from its mean.

* = statistically significant with 95% confidence.

Weights= Number of students from CASEN/total enrollment

Table 6
P900 Participation

Number of	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
Schools	969	1278	1123	1097	1060	988	900	862	893	913	909
Teachers	5237	7129	6494	5406	5626	5135	4806	4414	10795	11367	11384
Students	160182	219594	191415	170214	165758	152326	141316	137689	285766	294003	295201
Monitors	2086	2800	2500	2350	2300	2186	1802	1745	1800	1826	1818

Table 7
Means by Yearly Participation in P900 Program

Variable	1990				1992				1994				1996			
	NO		YES		NO		YES		NO		YES		NO		YES	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Number of Schools	3625		524		3485		664		3563		584		3785		364	
Test 1988	48.58	9.96	41.33	5.61	48.38	10.15	43.68	6.74	47.82	10.01	45.96	8.31	47.62	9.93	46.66	8.29
Test 1992	61.73	10.09	59.08	9.39	61.95	10.19	58.86	9.02	61.78	10.12	58.98	9.25	61.61	10.02	58.36	9.51
Test 1994	62.70	9.13	61.42	8.09	62.85	9.21	61.16	7.89	62.54	9.20	62.18	7.88	62.93	8.93	59.07	8.42
Test 1996	65.16	9.90	67.70	7.50	65.13	10.07	67.35	7.05	65.00	9.97	68.32	6.99	65.16	9.86	68.80	6.39
Vulnerability	56.02	26.04	58.93	20.42	72.62	26.39	74.32	20.74	70.10	21.04	69.20	18.27	69.97	20.94	69.98	17.65
Total Previous Years	0.00	0.00	0.00	0.00	0.10	0.43	1.49	0.65	0.40	1.02	2.29	1.59	0.79	1.58	2.88	1.88
P900 Participation																
1990	0.00	0.00	1.00	0.00	0.04	0.20	0.58	0.49	0.09	0.28	0.37	0.48	0.11	0.32	0.26	0.44
1991	0.06	0.24	1.00	0.00	0.04	0.20	0.91	0.28	0.12	0.32	0.56	0.50	0.16	0.37	0.40	0.49
1992	0.08	0.27	0.73	0.44	0.00	0.00	1.00	0.00	0.09	0.28	0.61	0.49	0.14	0.34	0.41	0.49
1993	0.09	0.28	0.59	0.49	0.03	0.16	0.80	0.40	0.05	0.22	0.75	0.43	0.12	0.33	0.45	0.50
1994	0.10	0.30	0.41	0.49	0.07	0.25	0.54	0.50	0.00	0.00	1.00	0.00	0.10	0.30	0.59	0.49
1995	0.10	0.30	0.26	0.44	0.08	0.27	0.34	0.48	0.03	0.16	0.70	0.46	0.06	0.24	0.77	0.42
1996	0.07	0.26	0.18	0.39	0.06	0.24	0.23	0.42	0.04	0.20	0.37	0.48	0.00	0.00	1.00	0.00
Regional Dummies																
1st region	0.02	0.13	0.02	0.14	0.02	0.13	0.03	0.16	0.02	0.13	0.02	0.15	0.02	0.13	0.01	0.12
2nd region	0.02	0.13	0.01	0.09	0.02	0.13	0.01	0.10	0.02	0.13	0.01	0.10	0.02	0.13	0.02	0.13
3rd region	0.02	0.13	0.01	0.11	0.02	0.14	0.01	0.09	0.02	0.13	0.01	0.11	0.02	0.13	0.01	0.09
4th region	0.07	0.25	0.08	0.27	0.07	0.25	0.07	0.26	0.07	0.25	0.06	0.24	0.07	0.25	0.04	0.21
5th region	0.10	0.29	0.09	0.29	0.10	0.30	0.09	0.28	0.10	0.29	0.09	0.29	0.10	0.30	0.06	0.24
6th region	0.09	0.29	0.07	0.26	0.09	0.29	0.07	0.26	0.09	0.29	0.08	0.27	0.09	0.28	0.09	0.29
7th region	0.13	0.34	0.09	0.29	0.13	0.34	0.10	0.30	0.13	0.33	0.10	0.31	0.13	0.33	0.12	0.32
8th region	0.17	0.38	0.17	0.38	0.17	0.38	0.17	0.37	0.17	0.38	0.17	0.38	0.17	0.37	0.22	0.41
9th region	0.07	0.26	0.14	0.34	0.07	0.26	0.13	0.34	0.08	0.26	0.12	0.33	0.08	0.27	0.10	0.30
10th region	0.14	0.34	0.17	0.38	0.14	0.35	0.15	0.36	0.14	0.35	0.15	0.36	0.14	0.34	0.18	0.39
11th region	0.01	0.09	0.01	0.11	0.01	0.09	0.01	0.10	0.01	0.10	0.00	0.04	0.01	0.09	0.01	0.09
12th region	0.01	0.09	0.00	0.06	0.01	0.09	0.01	0.09	0.01	0.09	0.01	0.10	0.01	0.09	0.01	0.07

Table 8
Probit Regressions

	1992		1994		1996	
Previous Test	-0.08	0.01 *	-0.02	0.01 *	-0.04	0.01 *
Previous Vulnerabili	-0.01	0.00 *	0.01	0.00 *	0.01	0.00 **
Concordant	69.30%		62.90%		67.30%	

Note: All regression include regional dummies as well.

Table 9
Cross-Section OLS Regressions of Test on current P900 Status

	Coef	Std. Error
1992	-2.09	0.55 *
1994	1.31	0.42 *
1996	3.83	0.44 *

Cross Section Including Lagged P900 Status

	1992		1994		1996	
	Coef	Std. Error	Coef	Std. Error	Coef	Std. Error
t	-2.09	0.66 *	1.28	0.46 *	4.00	0.48 *
t-2	0.00	1.44	-2.08	1.18 **	-0.21	0.85
t-4			-0.23	0.91	-0.60	0.89
t-6					1.12	0.71

Note: all regression control for region, total previous participation current and lagged vulnerability and lagged test score.

Table 10
Unrestricted GLS

	Test 1992		Test 1994		Test 1996	
	coef	std. Error	coef	std. Error	coef	std. Error
P900 1992	-1.81	0.61 *	-1.34	0.90	-1.70	0.75 **
P900 1994	-0.67	0.49	2.53	0.47 *	-1.95	0.62 *
P900 1996	-1.98	0.59 *	-3.42	0.47 *	4.26	0.47 *

Note: all equations have total_t, ivet, ive t-1, lagged test score and regional dummies with free parameters.

Significance at 10% is coded as ***, at 5% ** and at 1% *.

Table 11

I. Presence of Personal Effect Model: Non linear restriction imposed					
Coef	std. Error	coef	Std. Error	coef	std. Error
β_1		β_2		β_3	
-1.12	0.76	3.90	1.03 *	9.34	3.37 *
λ_1		λ_2		λ_3	
-0.67	0.34 **	-0.74	0.39 ***	-1.92	0.53 *
γ_1		γ_2		γ_3	
		1.80	0.59 *	2.64	1.56 ***
$\chi^2 (1)$	0.06				
II. Restrict P900 coefficient to be constant: one program effect					
β					
3.21	0.39 *				
λ_1		λ_2		λ_3	
-4.10	0.57 *	-0.87	0.39 **	-2.73	0.46 *
		γ_2		γ_3	
		0.84	0.16 *	-0.05	0.15
$\chi^2 (3)$	21.83				
III. Restrict fixed school effect: $\gamma=1$					
β_1		β_2		β_3	
-0.18	0.77	3.32	0.67 *	6.60	0.62 *
λ_1		λ_2		λ_3	
-1.33	0.47 *	-1.01	0.38 *	-2.66	0.34 *
$\chi^2 (3)$	6.27				
IV. Restrict constant correlation coefficient: equal lambda's					
β_1		β_2		β_3	
-0.26	0.75	5.34	0.68 *	5.69	0.69 *
λ					
-1.27	0.32 *				
		γ_2		γ_3	
		2.31	0.69 *	1.29	0.52 **
$\chi^2 (3)$	7.11				
IV. Restrict constant correlation coefficient and school fixed effects=1					
β_1		β_2		β_3	

0.63	0.65	4.22	0.56 *	5.58	0.51 *
λ					
-1.88	0.22 *				
$\chi^2 (5)$	16.31				
V. Partial Fixed Effects Model					
β					
3.79	0.42 *				
λ_1		λ_2		λ_3	
-3.14	0.38 *	-1.64	0.31 *	-1.60	0.29 *
$\chi^2 (5)$	49.99				
V. Total Fixed Effects Model					
β					
3.00	0.33 *				
λ_1		λ_2		λ_3	
-1.25	0.01 *	-0.56	0.33 ***	-2.24	0.21 *
$\chi^2 (37)$	14062.78				

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