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The effects of generalized school choice on achievement and stratification: Evidence from Chile's voucher program

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Abstract

In 1981, Chile introduced nationwide school choice by providing vouchers to any student wishing to attend private school. As a result, more than 1000 private schools entered the market, and the private enrollment rate increased by 20 percentage points, with greater impacts in larger, more urban, and wealthier communities. We use this differential impact to measure the effects of unrestricted choice on educational outcomes. Using panel data for about 150 municipalities, we find no evidence that choice improved average educational outcomes as measured by test scores, repetition rates, and years of schooling. However, we find evidence that the voucher program led to increased sorting, as the "best" public school students left for the private sector.

JEL classification: H4; I2
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1. Introduction

A central argument in the school choice debate is that public schools are inefficient local monopolies, and that educational quality would improve dramatically if only parents were allowed to freely choose between schools. For example, Hoxby (2003) asks "what is the range of productivity over which choice could cause productivity to vary? Recent history suggests that school productivity could be much higher than it is now—60% to 70% higher." Two arguments

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underlie this view. First, there is a widely held belief that private schools are more effective than public schools. Although the evidence from quasi-experiments with vouchers is mixed, if private schools are in fact more efficient, then school choice could raise students' achievement merely by facilitating their transfer to the private sector.[1] A second, perhaps even more compelling argument for choice comes from the notion that organizations respond to incentives. Therefore, by correctly aligning the incentives public schools face, choice would force their seemingly ossified bureaucracies to improve.

This paper assesses these arguments by examining the impact of a comprehensive school voucher program introduced in Chile. Specifically, in 1981 Chile's government began to provide vouchers to any student wishing to attend private school, and to tie the budget of public schools to their enrollment. We show that this program, whose essential features remain unchanged 20 years later, created a dynamic educational market: more than a thousand private schools entered the market, and the private enrollment rate increased from 20% to 40% by 1988, surpassing the 50% mark in many urban areas. The Chilean case thus provides a unique opportunity to analyze the transition from a centrally controlled public school system, to one in which *all* families can freely choose between public and private schools.

To measure the effects of the competitive forces unleashed by the voucher program, we exploit the fact that it had a greater impact in communities with larger markets, and in those where the demand for private schooling appears to have been greater. For example, from 1981 to 1988, the private enrollment rate grew by 11 percentage points more in urban than in rural communities.

As long as this differential impact is driven by community characteristics that are fixed over time, we can measure the impact of the voucher program by comparing the *change* in educational outcomes in urban and wealthier communities, to that in communities where private schooling increased by less. Using this approach with panel data for roughly 150 communities in Chile, we consistently fail to find evidence that school choice improved average academic outcomes.[2] Specifically, we find that average test scores did *not* rise any faster in communities where the private sector made greater inroads, and that average repetition and grade-for-age measures *worsened* in such areas (relative to other communities).

This evidence thus suggests that school choice did not improve average schooling outcomes in Chile. However, a natural alternative explanation is that the reallocation of students did *raise* achievement, but that these gains were masked by pre-existing negative trends in communities where the private sector grew by more. We cannot rule out this possibility, but we provide two pieces of evidence that are inconsistent with it. First, we show that our estimates do not change when we introduce a battery of controls for pre-existing and con-current trends, nor when we use a number of pre-program community characteristics—such as the initial population, urbanization rate, and degree of inequality—as instruments for the differential impact of the voucher program. Admittedly, the controls we use may not capture unobservable trends in school quality, and the instruments may not be ideal, but it is still puzzling that we continue to find no evidence that choice improved schooling quality.

Second, we explore another way to measure whether school quality has improved in Chile, one that does not rely on the differential impact of the voucher program across markets. Namely, we compare the performance of Chilean students in international tests in science and mathematics (widely known as the TIMSS), in which Chile participated in 1970 and 1999. This comparison indicates that despite nearly two decades under an unrestricted school choice

[1] See Ladd (2002) and Neal (2002) for recent surveys of the large literature on school vouchers.

[2] As described later in the paper, we define a community (or school "market") as a Chilean municipality.

regime, the performance of the median Chilean student has not improved relative to that of the median student in other countries.[3]

This collective body of evidence presents an enormous puzzle. How can we reconcile it with our instinct that when parents are able to choose between schools, they will select the most effective ones, and that schools should respond to this pressure? Again, it is possible that our estimates are biased by unobserved trends in schooling outcomes. However, an alternative explanation is that when parents are allowed to freely choose between schools, they select those that provide "good" peer groups for their children, which might not necessarily be the most productive. In turn, schools might respond by competing to attract better students, rather than by raising their productivity. Both forces are obviously complementary, and although they will not necessarily improve average school quality, they will tend to result in more stratification between schools.

We provide suggestive evidence that this appears to have happened in Chile—that the main effect of unrestricted school choice was an exodus of "middle-class" students from the public sector. Specifically, we find that in communities where private schools grew by more, there is a greater decline in the socioeconomic status (SES, measured by parental schooling and income) of public school students relative to the community average. In addition, we show that the loss of these students had a major effect on academic outcomes in the public sector. Namely, the performance of public schools (measured by test scores and repetition rates relative to the community average) worsened by more in markets where the voucher program had a larger effect.

The rest of the paper proceeds as follows. We begin by reviewing the institutional details of Chile's voucher program. We then describe the challenges evaluating the impact of school choice presents, and discuss our empirical approach. Finally, we assess how choice affected achievement and sorting across communities in Chile.

2. Chile's school voucher program: a brief overview

In 1981, as part of the Pinochet government's sweeping market-oriented reforms, Chile introduced a nationwide school voucher program. The easiest way to explain this reform is to discuss how it modified the manner in which schools were governed and funded. Before the reforms, there were three types of schools in Chile:

1) *Fiscal schools*. These public schools were controlled by the national Ministry of Education, which was responsible for all aspects of their operation. It hired and paid teachers, maintained facilities, and designed the curriculum. In 1981, 80% of all students were in such institutions.
2) *Unsubsidized private schools*. These private institutions did not receive public funding. They charged relatively high tuition and catered primarily to upper income households. Prior to the reforms, they accounted for about 6–7% of enrollment.
3) *Subsidized private schools*. These institutions did *not* charge tuition, received public subsidies, and were generally religious.[4] The size of the subsidy they received depended on the government's fiscal condition, but averaged 50% of nominal per-student spending in the fiscal schools. This aid was supposed to be disbursed at the end of the school year, but was

[3] In addition to Chile, twelve other countries participated in the TIMSS in 1970 and 1999. As we document below, after controlling for variables such as per capita GDP growth, changes in enrollment rates, and educational spending per student, the performance of the median Chilean student appears to have worsened slightly between 1970 and 1999.

[4] Espínola (1993) states that in 1970, 53% of private schools were Catholic and the remaining were Protestant or run by private foundations.

typically delayed by several months, and was therefore eroded by inflation.[5] Prior to the reform, these schools accounted for 15% of enrollment.

The 1981 reforms sought to create a nationwide voucher program with financial incentives for both public and private institutions.[6] This initiative had three main components:

1) *Decentralization of public schools*. Fiscal schools were transferred from the Ministry of Education to roughly 300 municipalities or "communes", such that they became known as municipal schools. The contract between the Ministry and the national teachers' union was abrogated, and public school teachers had to either transfer to municipal schools as common public employees, or resign and reapply for teaching jobs as regular private sector workers. To encourage the latter, the Ministry offered substantial severance payments.
2) *Public school funding*. Municipal schools continued to be funded centrally, but municipalities started to receive a *per-student* payment for every child attending their schools. As a result, enrollment losses came to have a direct effect on their education budgets.
3) *Public funding for private schools*. Most importantly, (non-tuition charging) subsidized private schools began to receive exactly the same per-student payment as the municipal schools.[7] These payments were distributed on a monthly basis, and their initial level was set 30% higher than the pre-1981 average spending per student in the public sector. To distinguish these institutions from the subsidized private schools that existed before the reforms, we will call them *voucher* private schools.[8] These retained wide latitude regarding student selection policies (public schools can only legally turn away students when oversubscribed), and were allowed to receive outside donations. They were not permitted, however, to charge tuition.[9]

Tuition-charging private schools mostly continued to operate without public funding. While they could have stopped charging tuition and started to accept vouchers, these elite institutions in general chose not to do so.

Finally, because voucher programs are often short-lived, it is worth mentioning that the essential features of this system have remained in place over the last 20 years. The center-left coalitions in power since 1990 have chosen to focus their efforts on channeling additional resources to "vulnerable" schools, increasing real educational spending and teacher salaries, and financially rewarding schools with high test scores.[10] Nevertheless, the core of the system—the

[5] See Schiefelbein (1971). Inflation averaged 5.2% per month in the 1970s. Assuming that public school teachers are paid every month, the real value of the stipend would be only 35% of real per-student expenditures in the public sector if the stipends were paid on time (at the end of the school year), and 26% if the payments were delayed by 6 months.

[6] For further discussion of the school choice reforms in Chile, see McEwan (2000), and McEwan and Carnoy (2000).

[7] The size of the voucher payment each school receives varies according to: 1) the educational level at which it operates, 2) whether it offers special programs, and 3) its distance from urban centers. Importantly, a given private school receives the same payment as a municipal school with similar characteristics.

[8] In Chile, they continue to be known as subsidized private schools.

[9] This restriction was largely eliminated in the mid-1990s, but was in place for essentially all of the periods we will analyze below.

[10] These are mainly policies aimed at: i) the worst performing schools—the P900 (Programa de las 900 Escuelas) program, ii) the entire K-12 system—the MECE (Programa de Mejoramiento de la Calidad y Equidad de la Educación Preescolar y Básica) initiative, iii) rural schools—the MECE-Rural, and iv) rewarding teachers in schools that perform well—the SNED (Sistema Nacional de Evaluación del Desempeño de los Establecimientos Educativos Subvencionados). Here we focus on the 1980s because it is the period in which the voucher program had its largest effects and was the key educational intervention, with the government refraining from compensatory initiatives.

per-student voucher payments and the freedom to attend any school, religious or not—has been left intact.

3. The industrial organization effects of school choice

These reforms led to significant changes in the Chilean educational market. Fig. 1 shows that the public sector's enrollment share hovered around 80% throughout the 1970s, but fell rapidly after 1981, dipping below the 60% level by 1990. The figure also describes the evolution of private schools' participation, which beginning in 1981, can be decomposed into that of voucher and tuition-charging schools. This makes clear that the rise of private enrollment in the 1980s is almost entirely due to the growth of voucher private institutions. By 1986, only five years after the per-student payments were introduced, these schools' market share crossed the 30% level, doubling relative to that of the pre-1981 subsidized private sector. In contrast, the participation of the "elite" private schools remained roughly constant over the 1980s, and experienced a gradual but sustained increase during the 1990s.

This transfer of students was accompanied by a large reallocation of resources towards private schools. First, because of voucher financing, the 20 percentage point enrollment shift means that a corresponding percentage of the Ministry of Education's school-related operational expenditures were reallocated to private schools. Second, although the transfer of teachers was more gradual than the shift in enrollment, by 1990 the fraction of teachers working in public schools had also fallen by 20 percentage points.

The aggregate trends in Fig. 1 conceal considerable variation in the growth of the private sector across different educational markets. Using Chile's approximately 300 communes as proxies for such markets, Fig. 2 (panel A) presents kernel densities of the change in private enrollment ratios from 1982 to 1996 for all communes in Chile, and for a subset of urban communes.[11] As can be seen, there was a substantial heterogeneity in the impact of the school voucher program across communes, although it was generally greater in urban communities.

Table 1 provides further information on the characteristics of the communities that were more affected by the availability of vouchers.[12] The first four columns indicate that the voucher program had a larger effect in urban and populated communes. For example, our point estimates indicate that the private enrollment rate grew by 11 percentage points more in a fully urban than in a wholly rural community. The next two columns suggest that the voucher program also had a larger effect in more unequal communities, where we proxy inequality by the inter-quartile range in years of schooling among working age adults.

Over time, such differences have produced substantial cross sectional variation in private enrollment, as described in Fig. 2 (panel B), which presents density estimates of private participation in 1996.[13] In roughly 40% of the urban communes the public sector has become a minority player, and in extreme cases, it accounts for only 20% to 25% of all enrollments. Further, this supply response was not limited to growth in pre-existing schools. Fig. 3 shows that more than 1000 private schools were created from 1982 to 1985, increasing their number by almost 30%.

[11] Defined as those with urbanization rates above 80% and populations above ten thousand.
[12] We defer a discussion of the data sources until Section 5.1. Descriptive statistics are in Table 1.
[13] As all other data presented henceforth, this figure refers only to the primary school sector (grades 1–8).

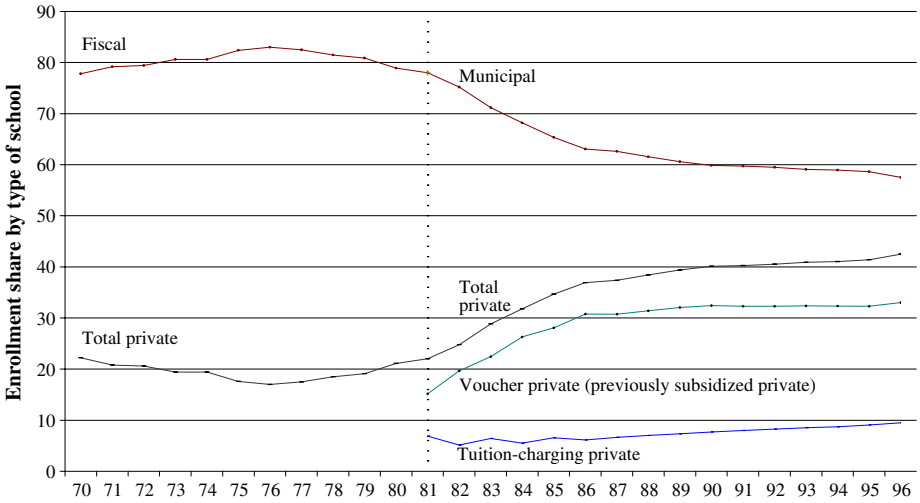

Fig. 1. National enrollment shares by sector, 1970–1996. Data assembled from several issues of the Ministry of Education's *Compendio Estadístico*.

A notable fact is that despite extensive private entry and sustained declines in public enrollments, the aggregate number of municipal schools has barely fallen. Municipal officials seem to have been unable or unwilling to close public schools. This leaves open the possibility that public schools did not face strong incentives to compete. This is reinforced by the fact that for these schools, revenue losses are mediated by municipal educational budgets, which makes it possible for them to lose students without automatic consequences on their resources. If indeed incentives were completely blunted for this sector, the gains from school choice would be entirely due to the reallocation of students to the (presumably) more productive private sector.

Finally, we note two interesting differences between the subsidized schools which existed prior to 1982 (which we label incumbent voucher schools) and those that entered thereafter (which we label voucher entrants). First, while the incumbent voucher schools are almost entirely religious institutions, the entrants are largely for-profit. For example, 84% of the entrants

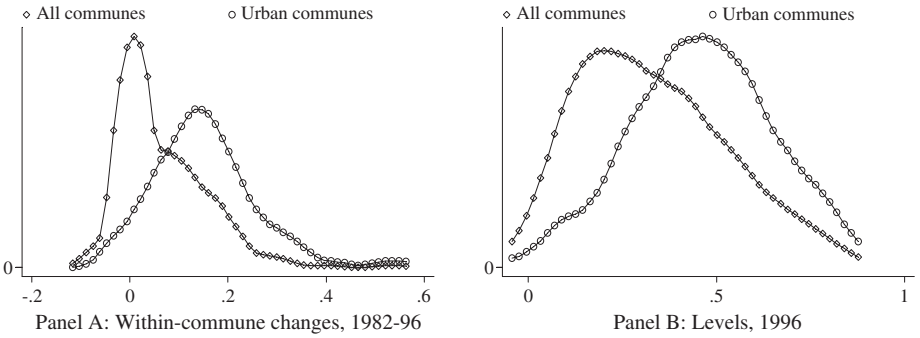

Fig. 2. Private enrollment among communes. Panel A is based on administrative information, data sources (8) and (10) in Table A.1. It covers all communes in Chile. Panel B refers to communes with positive private enrollment.

Table 1
Explaining the private sector's growth, 1982–1988

Independent variable—1982 observation of:	Dependent variable—1982–88 change in private enrollment[a]							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Urbanization rate[b]	0.11***	0.08***					0.09***	0.04*
	(0.01)	(0.02)					(0.01)	(0.02)
	[0.45]	[0.33]					[0.37]	[0.16]
Population[b]			0.67***	0.54***			0.40***	0.40***
			(0.19)	(0.11)			(0.07)	(0.06)
			[0.21]	[0.17]			[0.13]	[0.13]
Inter-quartile range in years of schooling[b]					0.16***	0.18***	0.06**	0.13***
					(0.02)	(0.04)	(0.03)	(0.04)
					[0.34]	[0.39]	[0.13]	[0.28]
Controls: 1982–1988 changes in population, years of schooling among adults, and income[c]	No	Yes	No	Yes	No	Yes	No	Yes
N	297	171	297	171	297	171	297	171
R^2	0.204	0.242	0.046	0.205	0.121	0.263	0.232	0.310

Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Huber–White standard errors are in parenthesis. Brackets contain the proportion of a standard deviation change in the dependent variable brought about by a one standard deviation increase in the independent variable. Sample sizes vary due to the addition of controls with missing observations.

[a] Based on administrative information, data sources (8) and (9), as described in Table A.1.

[b] Calculated using census micro and summary information, data sources (16) and (17). Urbanization is expressed as a proportion; population is in units of 10 million; and the inter-quartile range is in units of 10 years of schooling.

[c] Controls for concurrent trends are the 1982–1992 change in population (from data sources 17 and 18), and the 1982–1996 change in mean years of schooling and imputed labor income among adults (from census and household survey information, sources 13 and 16).

observed in 1988 are profit seeking institutions.[14] Second, the entrants generally attract students from lower socioeconomic backgrounds. For example, students in the new voucher schools come from families with less schooling and lower incomes, and have lower test scores than those in the incumbent voucher schools.[15]

4. Measuring the effects of school choice

There are two issues one has to address to credibly measure the effects of school choice on educational outcomes. The first is how to separate those effects that operate through enhanced school *productivity*, from those that operate through *sorting*. The second concerns the need for an adequate control group or counterfactual. This section addresses these issues in turn.[16]

[14] This number is from a sample of communes for which we have a panel of schools from 1982 to 1988. The communes in this panel account for about 70% of total enrollment in the country. See Section 5.1 for details on the data.

[15] Using the Chilean household survey (CASEN), we find that parents in the incumbent voucher schools have 1.35 (S.E.: 0.186) more years of schooling and 0.168 log points higher incomes (S.E.: 0.038) than parents in the entrant voucher schools. Additionally, SIMCE data reveal that in 1988, incumbents' average scores were 0.35 standard deviations higher in math, and 0.4 standard deviations higher in language. See Section 5.1 for additional information on the data.

[16] A formal version of the arguments in this section is available in Hsieh and Urquiola (2003), which also contains more extensive references on the school choice literature. For models of competition between schools, see also Epple and Romano (1998) and Manksi (1992).

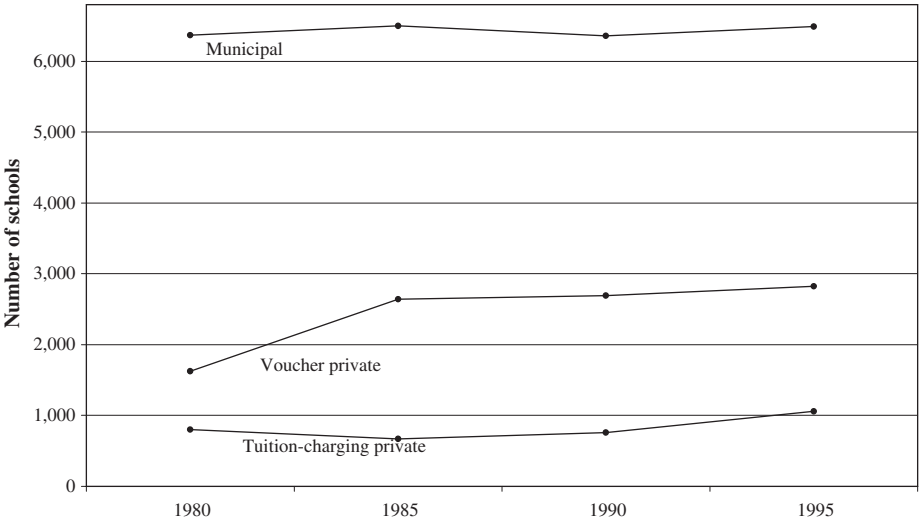

Fig. 3. Number of schools by sector, 1980–1995. Data assembled from several issues of the Ministry of Education's *Compendio Estadístico*.

4.1. Disentangling sorting from changes in productivity

A central issue in measuring the effect of school choice is that it can simultaneously affect both schools' productivity *and* the extent of sorting or stratification observed in the educational system. If it influences schooling outcomes through both of these channels, then it is nearly impossible to disentangle their respective magnitude.

To illustrate this point, put aside for now the endogeneity of competition. That is, imagine a setting in which the extent of private school availability is exogenously assigned across markets, and consider trying to measure the two sources of productivity gains from school choice: the possibility that private schools are more effective (so that aggregate achievement improves simply by shifting students into them), and the possibility that competition prompts public schools to improve.

A first problem is that if cream skimming takes place, even if competition forces public schools to improve, their average academic outcomes might still fall simply because the best students leave for private schools. On the other hand, if private schools attract lower income households, then public school performance might improve simply because the lowest performing students have exited them.[17] Put simply, when choice leads to sorting, there is simply no instrument that would allow one to isolate the effect of choice on the public sector's productivity.

A second problem arises if either type of sorting takes place and additionally, peer effects are important. In this case, it is difficult to measure whether students that switch to private

[17] Later in the paper, we will provide suggestive evidence that "cream skimming" is much closer to what happened in Chile, where the voucher program seems to have lead to an exodus of "better" students from the public sector. Of course, this need not be the case. In the U.S., for instance, choice programs might attract lower income households unable to settle into good districts or catchment areas. For instance, Bettinger (1999) provides suggestive evidence that in Michigan lower income students were the ones that took advantage of charter schools. The bottom line is that either type of sorting will complicate the analysis.

schools improve because private schools are more productive, or simply because they now interact with better peers. This matters because the overall peer quality in a community is fixed, so if there is no private productivity advantage, the gain to students shifting into private schools may come at the expense of the students who remain in the public sector.

In short, as long as choice also leads to sorting, one will generally not be able to separately measure the two potential sources of productivity gains from school choice.[18] What we can do is to approximate a weighted average of these two productivity effects by measuring the average change in academic outcomes of *all* students in a given community. This is not a perfect measure because it also encompasses the net peer effects of sorting induced by school choice. Nonetheless, the key advantage is that it nets out the "direct" effect of changes in each sector's student composition.

4.2. Empirical implementation and endogenous private entry

Thus far, our discussion suggests that to adequately study the productivity effects of choice, one has to look at its effects at the aggregate market level, and preferably in situations in which it has produced substantial and sustained changes in the educational market. From this point of view, the Chilean experience is very valuable. On the other hand, we have focused on measuring the effects of choice in situations in which the private enrollment share is as good as randomly assigned. Such an experiment would be very difficult to implement, and was not carried out in Chile, where the voucher program was introduced across the entire country at once. The Chilean case still offers empirical leverage, however, since in response to this program, the private sector grew substantially more in some markets.

This differential response is endogenous to the characteristics of a community, but as long as these characteristics do not change over time, one can difference them away by comparing the *change* in outcomes in a given community with the *change* in its private share. The identifying assumption is that the rate of improvement in educational outcomes (or the rate of change in sorting measures) that would have been observed without vouchers is not systematically related to characteristics that affected the extent of private entry.

There are, however, three reasons why this may not be the case. First, there could be differences in *pre-existing* trends that are correlated with the growth of the private sector. For example, if performance had been falling over time in markets where private enrollment grew rapidly after 1982, our estimates could understate the improvement due to choice.

Second, differential *concurrent* trends also pose potential problems. For example, it could be that the areas where private schools entered more were also ones that subsequently experienced rapid income growth, and that it was this growth, rather than any productivity effects stemming from vouchers, that improved outcomes. In this case, our estimates would overstate the gains from choice.

Third, the existence of heterogeneous treatment effects would also affect both private entry and subsequent achievement growth. For example, it could be that the voucher program resulted in greater entry in communities in which the private productivity advantage was greater. In this case, comparing the change in achievement in communities with more private growth (and a greater private advantage) with communities with less entry (and a smaller private advantage)

[18] One could narrow the bias due to sorting with detailed data on students' background, but there is still the obvious problem posed by unobservable characteristics potentially correlated with academic outcomes.

would *overstate* the impact of choice in an average community.[19] Put differently, what we would be doing is to estimate the average *marginal* impact of choice, which would be larger than the average effect.

There are two ways in which we address these concerns. First, we introduce a number of controls for pre-existing and concurrent trends. Second, we look for instrumental variables that affect the extent of private entry, but are ideally uncorrelated with trends in academic outcomes, or with the productivity advantage of the private sector. While the controls and instruments we use are not ideal, by comparing how the estimate changes with these modifications, we can obtain some sense of the magnitude and the direction of bias in our base estimates.

5. Results

Based on the framework presented, we now measure the impact of the voucher program. We first briefly describe our data, and then present results on academic outcomes. Finally, we turn to the program's impact on sorting.

5.1. Data and coverage

The framework sketched above suggests that the proper way to assess the impact of vouchers is to measure changes in educational outcomes at the *aggregate* market level. To implement this, we make use of Chile's (approximately) 300 communes as proxies for educational markets. Communes have a median area of about 55 km^2 and an average population of 39,000. In 1988, the average commune had 27 schools, 18 of which were public, 7 private voucher, and 2 tuition charging. Each commune has an autonomous government that manages schools and other public services.[20]

We use three types of outcome measures. The first consists of the average mathematics and language test score in each commune, which the PER testing program provides for 1982, and the SIMCE for later years.[21] This information is provided at the school level, which we aggregate to create weighted averages for each commune. A potential problem with these data is that several rural communes were not covered in the initial year (1982). However, it still reached 90% of all students, and if the test was administered in a given commune, all the schools in the commune participated.[22]

Our second outcome measure is the average repetition rate, which is defined as the fraction of students who have repeated the same grade at least twice, the official measure of repetition in Chile. We compiled these data from school-level administrative records collected by the Ministry of Education for 1982 and 1988. It covers *all* schools in the country, so it allows us to check that our results with test scores are not driven by the choice of communes.

Our third outcome variable is the average years of schooling among 10–15-year old children. This measure captures several dimensions of the educational system's performance, since it

[19] This point is formally set out in Hsieh and Urquiola (2003).

[20] With the exception of 50 communes in the Santiago metropolitan area, virtually all students attend school in the same commune in which they live. Because we want to use these as markets, we aggregate the 50 Santiago communes and consider them as a single school market.

[21] PER stands for Programa de Evaluación del Rendimiento Escolar, and SIMCE for Sistema de Evaluación de Calidad de la Educación. These tests have been conducted every year during the period we consider below (with the 4th grade in even and the 8th in odd years) since 1982, with a suspension during 1985–1987.

[22] See Espínola (1993).

reflects factors like age at entry, repetition, and dropout patterns. We compiled this variable from the population census and CASEN household survey micro data.

Finally, we use two sources of data to measure students' socioeconomic status. First, the Ministry of Education classifies each school into three to four categories, based on the educational background of the parents. We use this classification, but it is obviously rather coarse. To complement it, our second measure is based on household survey data. The Chilean National Household Survey (CASEN) is unusual in that it identifies the precise school attended by the children surveyed. With this school identifier, we can link its information to administrative records and obtain detailed information on the SES profile of individual schools. The summary statistics for the data are in Table 2, and Table A.1 in the appendix contains further detail on the precise data sources used.

5.2. Measuring the effects of choice on achievement

We begin by measuring the impact of the voucher program on four measures of academic achievement: 1) language test scores, 2) math test scores, 3) repetition rates, and 4) average years

Table 2
Descriptive statistics at the commune level

	1982			1988			1996		
	N	Mean	SD	*N*	Mean	SD	*N*	Mean	SD
Outcomes									
Language score[a]	97	56.0	6.3	293	50.2	6.9	298	68.3	5.8
Math score[a]	97	50.7	6.4	293	48.3	5.9	298	68.0	5.7
Repetition rate[b]	299	0.12	0.05	304	0.08	0.04			
Years of schooling, 10–15-year olds[c]	170	5.2	0.6	125	6.3	0.4	170	6.2	0.4
Sorting measures									
Average among public schools/average among all schools for:									
Language score[a]	101	0.97	0.04	292	0.98	0.05	298	0.98	0.04
Math score[a]	101	0.97	0.04	292	0.98	0.04	298	0.99	0.04
Repetition rate[b]	299	1.06	0.13	300	1.07	0.17			
Socioeconomic status (SES) index[a]	101	0.96	0.06	292	0.97	0.08	298	0.96	0.07
Household income[d]							185	0.87	0.16
Private enrollment rate [e]	299	0.12	0.14	304	0.17	0.17	304	0.18	0.18
Controls									
Population (hundreds of thousands)[f]	303	0.37	2.1	310	0.43	2.48			
Years of schooling, household heads[g]	303	6.2	1.5				177	8.5	1.5
Log of average imputed labor income[g]	303	10.4	0.3				177	12.2	0.3
Poverty rate[h]							164	0.19	0.07
Household income[h]							164	0.33	0.13
Literacy rate[h]							303	0.90	0.05

[a] Calculated using test system information, data sources (1), (2), and (4), as described in Table A.1.
[b] Variable comes from administrative information, data sources (8) and (9). It is not available for subsequent years.
[c] Based on micro census information for 1982 (data source 16), and household survey information for 1990 and 1996 (sources 11 and 13).
[d] Variable based on household survey information, pooled data sources (11) and (12).
[e] Variable comes from administrative information, data sources (8), (9), and (10).
[f] Calculated using census summary information, data sources (17) and (18).
[g] For household heads at least 18 years of age. Calculated using census micro data (source 16), and household survey (data source 13).
[h] Variable based on household survey information, data source (14).

of schooling among 10–15-year old children. The key independent variable is the change in the private enrollment rate. These estimates are shown in Table 3. We focus on the 1982–1988 period (panel A) since this is the period where we see the largest changes in private enrollment.

Columns 1 and 4 present the basic bivariate OLS regression for language and math, respectively. Although statistically insignificant, the point estimates suggest that, if anything, test scores experienced a relative *decline* in communities where the private sector made greater inroads. Columns 7 and 10 turn to repetition rates and years of schooling (among 10–15-year old children). Once again, the simplest bivariate OLS estimates provide no evidence of a relative improvement in communes where the private sector grew by more. In fact, column 7 indicates that repetition rates experienced a relative increase in communes where private schooling grew by more. The coefficient is statistically significant and large—a one standard deviation *increase* in the 1982–88 private enrollment growth increases the observed change in repetition by a quarter of a standard deviation.

As previously discussed, these estimates are robust to the endogeneity of the growth in private enrollment to the extent that it is driven by community characteristics that are fixed over time. However, there could be differential trends in academic outcomes that are correlated with the differential increase in private enrollment. For example, it might be the case that the private sector grew by more in areas where schooling outcomes had been worsening over time. To address this concern, columns 2, 5, 8, and 11 add three controls for pre-existing trends.

First, we include the 1970–1982 change in average years of schooling, which summarizes several aspects of the educational system's performance up to the introduction of vouchers.[23] It is an ideal control for our age for grade measure, and also indirectly captures previous performance on repetition. A second control is the 1980–1982 change in private enrollment. While this is not a direct outcome measure, the logic is that as a reaction to declining public performance prior to 1982, households may have started moving to the private sector even before the introduction of vouchers.[24] We would have liked to include data on private enrollment from years prior to 1980, but unfortunately this is not available at the commune level. Using information from maps,[25] however, we were able to also include the 1978–1982 change in the proportion of schools *private* in each commune. When we add these variables, the point estimates are essentially unchanged, and in the case of repetition rates, they continue to be significant at the 5% level.

The differential impact of the voucher program might also be correlated with *concurrent* trends. For example, if areas with greater private entry also experienced greater income growth which independently raised achievement, then our results might overestimate the effect of choice. Columns 3, 6, 9 and 12 add further controls to address this possibility. Specifically, they include 1982–1988 changes in population, labor income, and average years of schooling among adults.[26] Again, the point estimates are essentially unchanged, and continue to suggest that greater private growth might have even lowered average achievement.[27]

[23] We compiled this data from the 5% sample of the 1970 and 1982 population censuses.

[24] We obtained this information from administrative data provided by the Ministry of Education.

[25] Data source 19 in Table A.1.

[26] This information is compiled from the 1982 population census and the CASEN; see Table A.1.

[27] As a further check for the test score results, one can also implement regressions 1–9 focusing only on the performance of the tuition-charging private sector, which was not directly affected by the reform. Here again we find no clear effect on achievement, although the estimates are imprecise in part because the sample of municipalities drops substantially. For instance, for column 1 of Table 3, the corresponding estimate is −6.4 with a standard deviation of 28.5 and a sample size of 31. The remaining specifications produce similar results, so we omit them for reasons of space.

Table 3
OLS regressions for achievement, 1982–1988 and 1982–1996

	Dependent variable—change in average											
	Language score[a]			Math score[a]			Repetition rate[b]			Years of schooling[c]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A—1982–1988												
Change in priv. enrollment[b]	−5.5	−6.7	−3.4	−7.2	−9.4	−9.2	0.10***	0.09**	0.07*	−0.84	−0.72	−0.84
	(7.5)	(7.7)	(8.7)	(7.6)	(7.5)	(8.9)	(0.03)	(0.03)	(0.04)	(0.70)	(0.67)	(0.68)
	[−0.08]	[−0.10]	[−0.05]	[−0.10]	[−0.13]	[−0.12]	[0.24]	[0.21]	[0.17]	[−0.11]	[−0.10]	[−0.11]
Controls: previous trends[d]	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls: concurrent trends[e]	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
N	84	84	84	84	84	84	145	145	145	85	85	85
R^2	0.006	0.073	0.105	0.010	0.087	0.156	0.057	0.078	0.095	0.013	0.203	0.239
Panel B—1982–1996												
Change in priv. enrollment[b]	−13.8*	−12.3	−8.9	−15.8**	−15.0**	−12.8				−2.2***	−2.1***	−2.1***
	(7.9)	(7.7)	(9.9)	(6.5)	(6.7)	(8.0)				(0.4)	(0.4)	(0.4)
	[−0.24]	[−0.21]	[−0.15]	[−0.27]	[−0.25]	[−0.22]				[−0.42]	[−0.40]	[−0.40]
Controls: previous trends[d]	No	Yes	Yes	No	Yes	Yes				No	Yes	Yes
Controls: concurrent trends[e]	No	No	Yes	No	No	Yes				No	No	Yes
N	84	84	84	84	84	84				145	145	145
R^2	0.056	0.106	0.145	0.072	0.117	0.171				0.179	0.229	0.250

Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Huber–White standard errors are in parenthesis. Brackets contain the prop. of a standard deviation change in the dependent variable brought about by a one standard deviation increase in private enrollment.

[a] Calculated using test system information, data sources (1), (2), and (4), as described in Table A.1.

[b] Variable comes from administrative information, data sources (8) and (9), and (10). Repetition is available only up to 1988.

[c] Based on micro census data for 1982 (data source 16), and household survey data for 1990 and 1996 (sources 11 and 13).

[d] Controls for previous trends are: the 1970–1982 change in average years of schooling (from census micro data, sources 15 and 16), the 1980–1982 change in private enrollment (sources 7 and 8), and the 1978–1982 change in the proportion of schools private (sources 19 and 8).

[e] Controls for concurrent trends are the 1982–1992 change in population (from data sources 17 and 18), and the 1982–1996 change in mean years of schooling and imputed labor income among adults (from census and household survey information, sources 13 and 16).

We have so far focused on the 1982–1988 period, since these were the years in which the voucher program had the greatest effect. However, because it is possible that six years is not enough time for the productivity effects of choice to be observed, panel B (Table 3) presents estimates for the impact of the voucher program from 1982 to 1996. Measured by language scores, math scores, and years of schooling (among 10–15 year old children), the impact of the voucher program appears to have been even more negative over this longer time period.[28]

5.3. Robustness check: instrumental variables

An alternative strategy to check for biases is to identify pre-existing commune characteristics that explain the differential impact of vouchers. These can then be used as instruments for the private enrollment growth after 1982, under the assumption that they are uncorrelated with subsequent achievement changes. We use three instruments below. Our first two variables are the urbanization rate and the population of a commune in 1982. These capture the effect of market size on the extent of private entry. A third instrument is the inter-quartile range in years of schooling observed among adults (also in 1982). We use this as a measure of heterogeneity. The idea is that if parents consider peer group quality when choosing schools, then the demand for private schools that are able to admit "good" peer groups will be larger in less homogeneous communities.

Table 1 presents the first stage estimates from OLS regressions of the 1982–1988 change in private enrollment shares on the three candidate instruments. As can be seen, all three are highly correlated with the growth in private enrollment after 1982.[29]

As with any instruments, the estimates these variables yield have to be interpreted with caution, if only because there is ultimately no way of guaranteeing the instruments' validity. We will present standard over-identification tests, but we cannot rule out the possibility that these instruments are correlated with trends in unobserved determinants of academic outcomes, or that our controls for trends do not capture such determinants. Nonetheless, a comparison of the IV and the OLS estimates can provide us with a further sense of the direction of biases in our base specifications.

With this in mind, Table 4 presents the IV results (Table A.2 in the appendix contains the corresponding reduced form estimates). The instrumental variables are ordered across columns, with the last ones presenting the combination of the three variables.[30] In each case we present two specifications, one without and one with the controls for the pre-existing and concurrent trends introduced above. As the table shows, these estimates continue to suggest that greater private growth resulted in lower achievement. In fact, the IV estimates suggest that, if anything, the OLS estimates *overstate* the impact of the voucher program. Further, the negative effects for years of schooling also become statistically significant. The only exception arises when we use population as an instrument and the change in average language scores as the outcome of interest. Further, our estimates are not affected when we introduce controls for trends (with the same exception), and all the estimates comfortably pass a

[28] We do not have repetition data for 1996.

[29] We also considered population density as a candidate for an instrumental variable. The results are qualitatively similar, so we omit them for reasons of space.

[30] The samples vary according to the outcome measure because of the interaction of two factors: i) in the case of test scores, the 1982 PER system did not cover all communes and, ii) in some of the household surveys, there are not enough observations in some communes to estimate a reliable measure of several of the variables we use as proxies for pre-existing and concurrent trends. We checked that our results are robust to changes in the sample of communes.

Table 4
IV regressions for achievement, 1982–1988

Dependent variable—1982–1988 change in average	IV: Urbanization rate[d]		IV: Population[d]		IV: Inter-quartile range in years of schooling[d]		IV: All three variables	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A—Language score[a]								
Change in private enrollment[b]	−38.7	−27.2	4.3	11.7**	−19.9	−10.1	−15.8	−4.5
	(30.9)	(35.2)	(5.2)	(5.2)	(18.7)	(20.3)	(14.7)	(14.9)
	[−0.55]	[−0.39]	[0.06]	[0.17]	[−0.28]	[−0.14]	[−0.23]	[−0.06]
Controls for trends[e]	No	Yes	No	Yes	No	Yes	No	Yes
N	84	84	84	84	84	84	84	84
Over-identification test, *p*-value[f]	0.99	0.99	0.99	0.99	0.99	0.99	0.97	0.97
Panel B—Mathematics score[a]								
Change in private enrollment[b]	−99.0**	−103.5**	−8.0	−1.0	−57.5**	−46.4*	−49.6**	−37.6*
	(45.7)	(46.7)	(6.2)	(7.2)	(23.7)	(25.6)	(21.9)	(22.4)
	[−1.34]	[−1.40]	[−0.11]	[−0.01]	[−0.78]	[−0.63]	[−0.67]	[−0.51]
Controls for trends[e]	No	Yes	No	Yes	No	Yes	No	Yes
N	84	84	84	84	84	84	84	84
Over-identification test, *p*-value[f]	0.99	0.99	0.99	0.99	0.99	0.99	0.92	0.92
Panel C—Repetition rate[b]								
Change in private enrollment[b]	0.33***	0.37***	0.17***	0.15***	0.28***	0.28***	0.29***	0.28***
	(0.08)	(0.11)	(0.04)	(0.04)	(0.08)	(0.10)	(0.07)	(0.08)
	[0.78]	[0.88]	[0.40]	[0.36]	[0.66]	[0.66]	[0.69]	[0.66]
Controls for trends[e]	No	Yes	No	Yes	No	Yes	No	Yes
N	145	145	145	145	145	145	145	145
Over-identification test, *p*-value[f]	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Panel D—Years of schooling[c]								
Change in private enrollment[b]	−4.0***	−5.1***	−2.0***	−2.5***	−2.9*	−2.7**	−3.2***	−3.5***
	(1.4)	(1.6)	(0.7)	(0.8)	(1.6)	(1.2)	(1.1)	(1.1)
	[−0.54]	[−0.69]	[−0.27]	[−0.34]	[−0.39]	[−0.36]	[−0.43]	[−0.47]
Controls for trends[e]	No	Yes	No	Yes	No	Yes	No	Yes
N	85	85	85	85	85	85	85	85
Over-identification test, *p*-value[f]	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.90

Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Huber–White standard errors are in parenthesis.
[a], [b], and [c] as in Table 3.
[d]Variables calculated using census information, data sources (16) and (17), as described in Table A.1.
[e]Controls for pre-existing and concurrent trends, as described in Table 3, notes 4 and 5.
[f]The over-identification test is based on Sargan (1958). We report the *p*-value for the statistic constructed by multiplying the number of observations and the R^2 from a regression of the residuals from the second stage regression on the instrument(s).

standard over-identification test, with the usual caveat about the assumptions behind and power of such tests.

In short, we have looked at three measures of educational achievement so far: repetition rates, years of schooling, and test scores. For the first two (particularly repetition), taken at face value, the point estimates and standard errors we estimate (both under OLS and IV) would rule out that choice had net beneficial effects. In the case of test scores, the majority

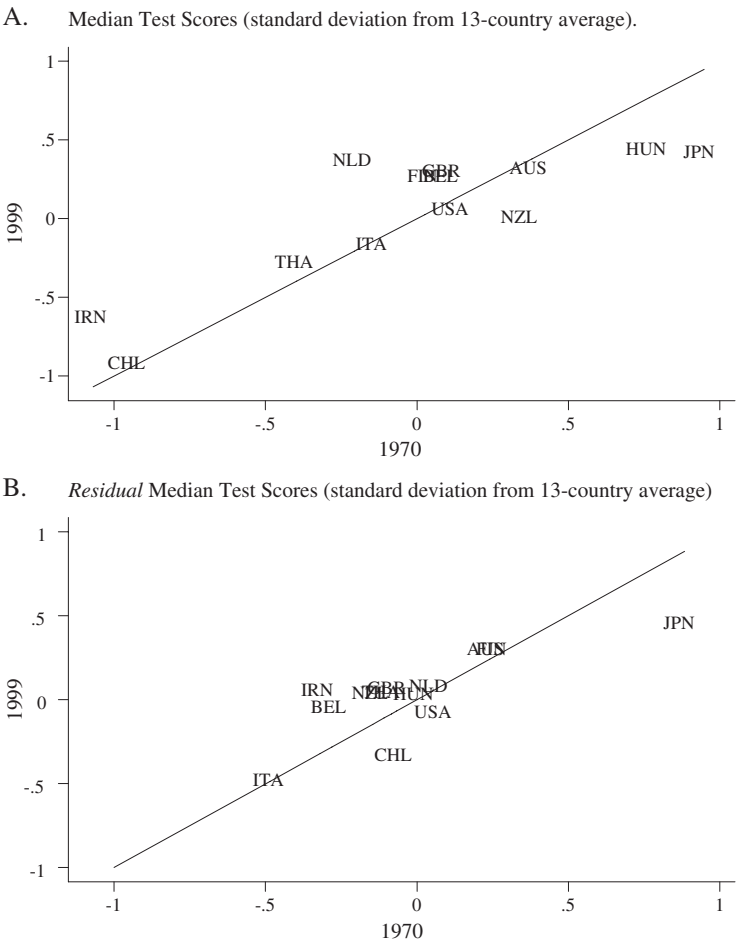

Fig. 4. Chile's performance in international tests, 1970 and 1999. (Note: The scores for each country subtract the mean score for the 13 countries and are divided by the standard deviation of U.S. scores in the given year. Residual test scores are residuals from regression of median test score on GDP/worker, enrollment rate, and ratio of spending per student to GDP per capita).

of our point estimates are indicative of a negative effect on outcomes, but a 95% confidence interval around many of them would still include substantial positive effects. Partially in light of this, in the next section we look at a couple of further robustness checks.

5.4. Robustness check: international and sectoral comparisons

An alternative manner to determine whether school choice improved schooling quality in Chile is to measure the country's performance in international tests in math and science, widely known as the TIMSS. This is possible because Chile participated in the TIMSS in 1999, and in its precursor, the IEA, in 1970. While international comparisons should always be interpreted with caution, in this case they have the advantage of not relying on the differential impact of vouchers across different markets within Chile. We summarize the results graphically in Fig. 4. Panel A shows that during the last three decades, the score of the median Chilean student did not

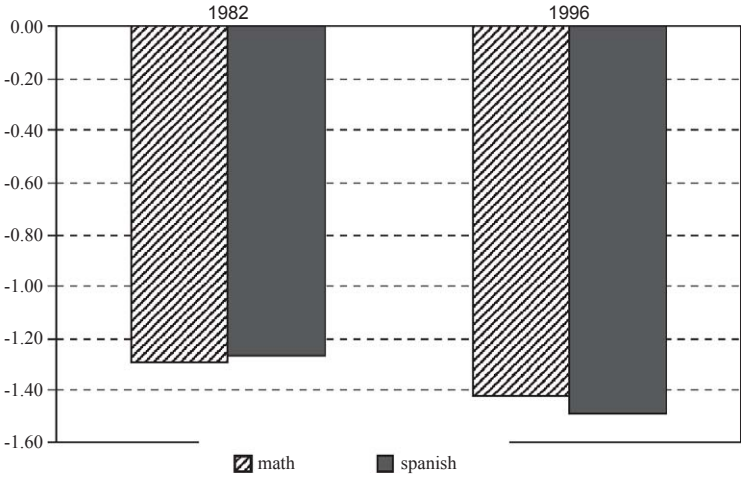

Fig. 5. Average test score among municipal and voucher schools, relative to tuition-charging private schools, 1982 and 1996 (Standard deviations below tuition charging).

change relative to that of the median student in the other 12 countries that also participated in both years.[31] This is all the more surprising since Chile's economy has performed quite well over the last two decades.[32] In fact, when one introduces controls for per capita income growth, and changes in enrollment rates and school spending, the performance of the median Chilean student appears to have slightly worsened over the last 30 years (panel B).[33]

A final way to measure whether average school quality has improved is to use the average test scores from the PER and SIMCE. Clearly, it would not make much sense to compare the change in average test scores, since we have no way of knowing that the tests are comparable over time. However, since the tuition-charging private schools were plausibly unaffected by the voucher program, we can use these schools as a control and measure the gap between the test scores of the elite private schools and those of publicly funded (voucher and public) institutions. This evidence, presented in Fig. 5, similarly provides no indication that vouchers improved outcomes in the schools they affected. Here the data show a well-known feature of the Chilean education system, namely the large gap in test scores between the subsidized (voucher and municipal) sector and the tuition-charging private schools. In 1982 the average score of the publicly funded schools is about 1.3 standard deviations below the elite private schools. By 1996, this gap had actually become larger.[34]

[31] The unit is the standard deviation of U.S. students taking the TIMSS in 1970 and 1999.

[32] From 1970 to 1999, per capita GDP grew at an annual rate of 4.3% in Chile and at average annual rate of 2.8% in the other 12 countries (authors' calculations using the International Financial Statistics).

[33] The cross-sectional data tells a similar story. For example, the math score of the median Chilean student on the 1999 TIMSS was 1.08 standard deviations below that of the average student in the other 38 countries, while the science score was 0.7 standard deviations lower (again, the unit is the standard deviation of the U.S. in 1999). After controlling for GDP per worker, school spending (per student relative to per capita GDP), and enrollment rates, the "residual" score of the median Chilean student was 0.78 standard deviations below that of 38 other countries in math and 0.33 standard deviations lower in science. We took the figures on GDP per worker from the Penn World Tables and those on school spending and enrollment rates from UNESCO's yearbook.

[34] In part, as we discuss in the next section, this in itself may be capturing some sorting, since the tuition-charging private sector did grow significantly (although from a small base) during this period, presumably "cream skimming" some students from voucher and even municipal schools.

5.5. Sorting

We now turn to the effect of the voucher program on sorting. We begin by describing the relation between a commune's private enrollment rate, on the one hand, and the ratio between the average "quality" of its public school students and the commune-wide average, on the other. Note that the latter variable is a within-commune observation—it does not compare public school students in one commune with those in a different market. The idea is that if private schools cream skim, then this measure should fall with private enrollment.

Panel A in Table 5 first looks at the cross sectional evidence. The dependent variable in columns 1 and 2 is the ratio of the educational background of public school students to the average in the community (for 1996). This data is based on an index of the educational background of each school, provided by the Ministry of Education. This measure is crude, but the estimates are nonetheless precisely estimated, and suggest that the relative educational attainment of parents in public schools is lower in communes with higher private enrollment.[35] Using more detailed household survey data on parental income for 1990/1992, columns 3 and 4 suggest a similar conclusion. The point estimates are again precisely estimated and quite large: they suggest that a one standard deviation increase in the private enrollment rate is associated with 38–43% of a standard deviation decline in the relative income of public school parents.

Building on this evidence, columns 5–10 turn to indirect measures of sorting, namely the ratios of the average performance (on test scores and repetition) of public school students and the average in the entire commune. These are indirect measures, because they are a function of three effects: sorting, the productivity advantage of private schools, and the public productivity response. First, if private schools take the best students from the public sector, the sorting effect suggests that the relative test scores (or relative repetition rates) of public schools should be lower (higher) in communes with greater private sector penetration.

Second, if private schools are better than public schools, then even in the absence of sorting, the mere reallocation of students to the private sector will raise average test scores, and thus lower the relative position of the public sector. Third, as for the public response, there are effects going in opposite directions. On the one hand, if public schools improve by more in communes with more competition from the private sector, then the relative grades of public schools should be higher in communes with greater private enrollment. On the other hand there is the possibility of endogenous entry: if the private sector grew by more in communes where public schools were under-performing (prior to the voucher program), then this would suggest that the relative grades of public schools should be lower in communes with a larger private enrollment rate.[36]

In the event, the estimates in columns 5–10 uniformly indicate that when measured by math scores, language scores, and repetition rates, public schools do worse in communes with a higher private enrollment rate. All the estimates are precisely estimated, are robust to the introduction of controls for community characteristics, and suggest that the private enrollment rate has a first

[35] For each variable featured in Panel A, we present results using the most recent cross-section in our data. However, we obtain very similar estimates using the cross-sections from other years. For instance, for the 1988 and 1990 cross-sections, the point estimates in columns 1 and 2 are −0.15 and −0.15, and −0.16 and −0.14, respectively. In every case these are significant at the 5% or 1% level (these results are available upon request). Similar robust findings emerge for the math and language results we discuss below. For income, 1990/1992 is the only cross-section for which we matched household survey and school level data.

[36] These points are set out more formally in Hsieh and Urquiola (2003).

Table 5
Sorting among communes, 1990's cross-section and 1982–1988 changes

	Dependent variable—within commune observations of average characteristic in public schools/average characteristic in all schools									
	SES index[a]		Income[b]		Language[a]		Mathematics[a]		Repetition[c]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A—1990's cross sections[d]										
Private enrollment[c]	−0.20***	−0.16***	−0.37***	−0.33***	−0.08***	−0.08***	−0.09***	−0.09***	0.42***	0.28***
	(0.02)	(0.03)	(0.07)	(0.09)	(0.02)	(0.02)	(0.02)	(0.03)	(0.07)	(0.07)
	[−0.58]	[−0.46]	[−0.43]	[−0.38]	[−0.39]	[−0.39]	[−0.42]	[−0.42]	[0.44]	[0.29]
Commune controls[e]	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Thirteen regional dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	296	296	184	184	296	296	296	296	299	299
R^2	0.313	0.493	0.171	0.285	0.188	0.396	0.215	0.346	0.193	0.447
Panel B—1982–1988 changes										
Change in private enrollment[c]					−0.21**	−0.22**	−0.14*	−0.19**	0.51**	0.38*
					(0.10)	(0.10)	(0.08)	(0.08)	(0.24)	(0.24)
					[−0.24]	[−0.26]	[−0.17]	[−0.23]	[0.24]	[0.18]
Controls: concurrent trends[f]					No	Yes	No	Yes	No	Yes
N					84	84	84	84	163	163
R^2					0.060	0.065	0.027	0.097	0.054	0.100

Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Huber–White standard errors are in parenthesis.

[a] Calculated using test system information, data sources (1) and (2), as described in Table A.1.

[b] Based on household survey information, pooled from data sources (11) and (12).

[c] Based on administrative information, data sources (8), (9), and (10). Repetition data is available only up to 1988.

[d] In the cross-section, the data for test scores and the SES index are for 1996, for income they are for 1990/1992 (pooled data) and for repetition they are for 1988. For each variable, these are the latest cross-sections available in our data.

[e] Cross-sectional controls include: literacy rate, mean years of schooling, poverty rate, average household income (all from household survey information, data source 14), population and population squared (from census summary information, data source 18).

[f] Controls for concurrent trends are the 1982–1992 change in population (from data sources 17 and 18), and the 1982–1996 change in mean years of schooling and imputed labor income among adults (from census and household survey information, sources 13 and 16).

order effect on the relative performance of public schools. For example, a simple bivariate OLS regression suggests that a one-standard deviation increase in the private enrollment rate lowers the relative math score of public schools by about 40% of a standard deviation.

Again, we want to emphasize that the relative performance of public schools is only an indirect measure of sorting. As we discussed, there are two reasons why the relative performance of public schools would be lower in communes with a higher private enrollment rate. First, it might be the case that the public sector improves in response to the competition induced by private entry. Second, it could also be that private schools are better and the reallocation of students towards this sector raises average test scores. As we discussed, when choice also results in sorting, the proper way to measure whether these two productivity effects are present is to look at aggregate measures of achievement, and the evidence we presented in the previous section suggests that these productivity effects are not there.

Nonetheless, we cannot rule out that these results may be influenced by the endogenous entry of private schools in communes where public schools are weakest. One way to deal with this is to again difference out fixed commune characteristics by looking at changes over time. We do this in panel B with regressions of the 1982–1988 change in the relative "quality" of public schools (again measured by language and math scores, and by the repetition rate) and the change in the private enrollment rate. These estimates indicate that the composition effect of choice seems to dominate any effect of competition on the public schools' productivity. Although not as precisely estimated as the cross-sectional estimates, they are generally statistically significant and indicate that the relative "quality" of public schools has worsened in communes where private enrollment grew by more.[37]

For completeness, we again use the urbanization rate, population, and the interquartile range in years of schooling (of working age adults) as instruments for the differential impact of the voucher program. Table 6 presents these results (the reduced form estimates are in Table A.3). These estimates provide further evidence that the main effect of school choice in Chile has been to facilitate greater sorting. In fact, the IV estimates generally indicate that choice led to *more* sorting than that suggested by the OLS estimates.

In sum, there are two points we take away from this evidence. First, private schools attracted students from families with higher levels of income and schooling. Second, because these characteristics are important determinants of educational outcomes, it will be virtually impossible to isolate whether public schools improved in response to the competitive forces unleashed by the private sector. As our estimates show, the relative grades of public school students fell by more in communes with a larger increase in private enrollment. This does not necessarily imply that public schools did not improve—it simply indicates that if a productivity effect is present, it is overwhelmed by the sorting effect.

We note that our findings are consistent with the only two studies that we are aware of that measure the consequences of comprehensive school choice on sorting. Although they do not have the data to assess the effect on educational productivity, Fiske and Ladd's (2000) analysis of the open-enrollment program among public schools in New Zealand suggests that a major effect of choice has been to induce greater segregation. A second study, by Berry Cullen et al. (2005), on Chicago's open-enrollment program at the high school level, also suggests that the

[37] We do not have data on sorting over time based on income measures, since the 1982 population census does not identify whether a child is enrolled in a public or a private school (this information is only contained in the CASEN household survey, which is available starting in 1987). We also do not use the SES index, since the way in which it is calculated has changed over the years.

Table 6
IV regressions for sorting, 1982–1988

Dependent variable—change in average characteristic in public schools/ average characteristic in all schools	IV: Urbanization rate[a]		IV: Population[a]		IV: Inter-quartile range in years of schooling[a]		IV: All three variables	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Language score[b]								
Change in private enrollment[c]	−1.04**	1.54*	−0.16***	−0.16***	−0.58**	−0.78*	−0.50**	−0.57*
	(0.51)	(0.91)	(0.06)	(0.06)	(0.29)	(0.40)	(0.23)	(0.30)
	[−1.21]	[−1.79]	[−0.19]	[−0.19]	[−0.67]	[−0.91]	[−0.58]	[−0.66]
Controls: concurrent trends[d]	No	Yes	No	Yes	No	Yes	No	No
N	84	84	84	84	84	84	84	84
Over-identification test, *p*-value[e]	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Panel B: Math score[b]								
Change in private enrollment[c]	−0.69**	−1.01*	−0.07	−0.08*	−0.61**	−0.73*	−0.42**	−0.45*
	(0.31)	(0.59)	(0.06)	(0.05)	(0.24)	(0.38)	(0.18)	(0.26)
	[−0.83]	[−1.22]	[−0.09]	[−0.10]	[−0.74]	[−0.88]	[−0.51]	[−0.55]
Controls: concurrent trends[d]	No	Yes	No	Yes	No	Yes	No	No
N	84	84	84	84	84	84	84	84
Over-identification test, *p*-value[e]	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Panel C: Repetition rate[c]								
Change in private enrollment[c]	1.62***	1.71**	0.50***	0.47***	0.63	0.54	1.03**	0.88**
	(0.46)	(0.66)	(0.14)	(0.15)	(0.39)	(0.44)	(0.33)	(0.34)
	[0.75]	[0.79]	[0.23]	[0.22]	[0.24]	[0.25]	[−0.39]	[−0.33]
Controls: concurrent trends[d]	No	Yes	No	Yes	No	Yes	No	No
N	163	163	163	163	163	163	163	163
Over-identification test, *p*-value[e]	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99

Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Huber–White standard errors are in parenthesis.

[a] Based on census summary and micro information, data sources (16) and (17), as described in Table A.1.

[b] Calculated using test score information, data sources (1) and (2).

[c] Variable based on administrative information, data sources (8) and (9).

[d] Controls for concurrent trends are the 1982–1992 change in population (from data sources 17 and 18), and the 1982–1996 change in mean years of schooling and imputed labor income among adults (from census and household survey information, sources 13 and 16).

[e] The overidentification test is based on Sargan (1958). We report the p-value for the statistic constructed by multiplying the number of observations and the R^2 from a regression of the residuals from the second stage regression on the instrument(s).

main effect has been to induce segregation, without any evidence of increased academic outcomes (except for career academies).

Finally, we have focused on sorting between the public and the private voucher schools, but the sorting that took place is clearly more complicated. For example, as previously discussed, the new private voucher schools generally attracted students from lower SES backgrounds than those in incumbent institutions. This suggests that it was largely the entrants, and not the incumbents, that were attracting students from the public schools. To take another example, the gradual growth of the private tuition-charging school in the 1990s could also reflect "cream skimming", albeit from the private voucher schools. Consistent with this, there is evidence that the gap in the socioeconomic background of students in the private elite schools and the publicly funded schools narrowed from 1987 to 1998.[38] This is consistent with a story in which the SES of students who switch to the elite private schools is higher than the average SES in the publicly funded sector, but lower than that in the elite schools, and thus lowers the average SES in both sectors.

6. When schools compete, how do they compete?

In sum, the central effect of the school voucher program in Chile appears to have been to facilitate the exodus of the Chilean middle class from public schools, without much evidence that it has improved aggregate academic outcomes. While it is not surprising that choice could result in sorting, what accounts for the surprising lack of improvement on achievement?

One possibility, often raised by Chilean observers, is that public schools may in fact not have experienced significant incentives to compete. We presented evidence consistent with this view in Fig. 3, which suggested that few public schools have been forced to close. In addition, Chilean authorities might not have provided enough information for parents to determine a school's quality. It was only after 1995 that the authorities made test scores widely available, and newspapers began publishing school rankings based on these.

Nonetheless, even if the public schools were not forced to compete and thus did not improve, as long as private schools are more productive than public schools, we should still see better aggregate performance given the large number of students that transferred to the private sector, and we simply find no evidence of this. What can account for the lack of a private productivity advantage?

One possibility is that private schools responded to the competitive pressures unleashed by the voucher program, not by raising their productivity, but rather by choosing better students. School administrators in Chile, as in the rest of the world, can raise their schools' outcomes by doing things such as identifying and hiring effective teachers, and then supporting and monitoring their work; but they also realize that this is costly and may not always work. In contrast, it is easier to improve outcomes simply by picking the best students. Parents can also be willing participants in this, and their demand for good peer groups obviously reinforces the desire of school administrators to cream skim.[39]

In fact, there is abundant institutional evidence that in Chile, private schools do compete by attempting to select better students. As previously mentioned, private schools are allowed to reject students, and Gauri (1998) presents evidence that the majority of them do exercise this

[38] We use the national household survey (CASEN) to measure student characteristics in the private elite schools and in the publicly funded sector (voucher and municipal schools). This indicates that the average years of schooling of parents in the private elite schools was 4.49 years (S.E.: 0.08) higher than in the publicly funded schools in 1987, but this gap had fallen to 2.65 years (S.E.: 0.06) by 1998. When measured by income, the gap between private elite school parents and publicly funded school parents also narrowed, from 0.969 log points (S.E.: 0.018) in 1987 to 0.655 log points (S.E.: 0.015) by 1998.

[39] For suggestive evidence of this in the U.S., see Rothstein (2004).

ability, screening children either by requiring a parental interview, or by using admissions tests. Chilean observers have also pointed out that new voucher schools have sought to attract students by endowing themselves with "symbols" previously associated only with elite, tuition-charging institutions, such as uniforms, and the use of foreign and particularly English names.[40]

7. Conclusion

This paper makes two contributions to the school choice debate. First, we make the point that if choice leads to greater segregation, one will not able to isolate the extent to which public schools improve their productivity in response to the competitive threat induced by choice, from the effect of sorting on the public sector's performance. On the one hand, if choice results in cream-skimming (as we suggest happened in Chile), the average performance of public schools might fall even if they become more effective, simply because they have lost their best students. On the other hand, if low SES students leave the public sector, as Bettinger (1999) suggests happened in Michigan with charter school entry, then the average performance of public schools might improve even if they do not raise their productivity. We argue that the best one can do is to measure changes in outcomes at the aggregate level.

Second, we focus on a country that implemented an unrestricted nationwide school choice program. We show that the first order consequence of the voucher program in Chile was middle-class flight into private schools, and that this shift does not seem to have resulted in achievement gains, certainly not of the magnitude claimed by some choice advocates. Again, we cannot rule out the possibility that our estimates are biased by unobserved trends in schooling outcomes, but we show that our results do not change when we introduce a number of controls for such trends.

We want to make three points clear. First, we are not claiming that vouchers have not produced any gains at all. It might be the case, for instance, that after twenty years of choice, Chilean schools are spending their money in ways that parents value more. For instance, they may now be emphasizing freshly painted walls more than reduced teaching loads. Additionally, many families surely value the availability of subsidized religious instruction. In short, school choice might improve welfare even if it does not improve academic achievement.

Second, it should be clear that the underlying institutions and the precise details of the program implemented are critically important in thinking about the potential impacts of school choice. For instance, a choice program in a decentralized schooling system, such as that in the U.S., is likely to result in a different type of sorting. Additionally, choice programs that do not allow private schools to select their students, or those that provide incentives to schools that attract low SES children, might result in less sorting than we find. Finally, it is possible that a choice program in a school system where private schools are less important than was the case in Chile in the early 1980s may see a smaller response from the private sector.

Third, we interpret the Chilean evidence as providing strong support for the notion that schools do respond to incentives. The key question is incentives for what? It seems that if schools are provided with incentives to improve their absolute outcomes, and are also allowed to choose their student body, they are likely to respond by attempting to select better students. This should not be surprising to those familiar with elite universities, since an

[40] See Espínola (1993).

integral part of the perceived quality of these institutions is their ability to skim the very best students. While there are enormous rewards for the institutions that succeed in this endeavor, from a societal perspective it may be a zero-sum game, since one school's selectivity gain is another's loss. Therefore, an important topic for further research is the design of mechanisms that would preserve the competitive effects of vouchers, but force schools to improve by raising their value added, and not by engaging in rent-seeking behavior.[41]

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Appendix A

Table A.1
Data sources

Data type and source	Original unit of observation	Year									
		1970	1978	1980	1982	1988	1990	1992	1996	1998	1999
Test scores											
PER[a]	School				(1)						
SIMCE[b]	School					(2)	(3)		(4)		
TIMSS[c]	Country										(5)
International science exams[d]	Country	(6)									
Administrative information											
Enrollment files	School			(7)	(8)	(9)			(10)		
Household surveys											
CASEN[e]	Individual						(11)	(12)	(13)	(14)	
Census											
Micro data	Individual	(15)			(16)						
Summary files	Commune				(17)			(18)			
Other											
School maps[f]	Commune		(19)								

[a] Programa de Evaluacicen del Rendimiento Escolar.
[b] Sistema de Evaluacicen de Calidad de la Educación.
[c] Third International Mathematics and Science Study, see http://timss.bc.edu.
[d] International Science Exams, International Education Association, see Comber and Keeves (1973).
[e] Encuesta de Caracterización Socioeconómica Nacional.
[f] See Instituto Geografico Militar (1983).

[41] See for instance Epple and Romano (2002).

Table A.2
Reduced form regressions for achievement, 1982–1988

Dependent variable—1982–88 change in	Independent variable—1982 observation of					
	Urbanization rate[a]		Population[a]		Inter-quartile range in years of schooling[a]	
	(1)	(2)	(3)	(4)	(5)	(6)
Private enrollment[b]	0.09***	0.08***	0.44***	0.42***	0.19***	0.19***
	(0.03)	(0.03)	(0.07)	(0.07)	(0.04)	(0.05)
	[0.32]	[0.28]	[0.31]	[0.30]	[0.38]	[0.38]
Controls for trends[c]	No	Yes	No	Yes	No	Yes
N	84	84	84	84	84	84
R^2	0.101	0.222	0.091	0.238	0.150	0.281
Average language score[d]	−3.5	−2.2	1.9	5.0**	−3.8	−1.9
	(2.4)	(2.8)	(2.2)	(2.2)	(3.5)	(3.8)
	[−0.18]	[−0.11]	[0.02]	[0.05]	[−0.11]	[−0.06]
Controls for trends[c]	No	Yes	No	Yes	No	Yes
N	84	84	84	84	84	84
R^2	0.031	0.113	0.000	0.105	0.012	0.106
Average Math score[d]	−9.0***	−8.2***	−3.5	−0.4	−11.0****	−8.8**
	(2.2)	(2.4)	(3.0)	(3.1)	(3.6)	(4.0)
	[−0.43]	[−0.39]	[−0.03]	[−0.00]	[−0.30]	[−0.24]
Controls for trends[c]	No	Yes	No	Yes	No	Yes
N	84	84	84	84	84	84
R^2	0.182	0.277	0.001	0.144	0.091	0.194
Average repetition rate[b]	0.04***	0.04***	0.09**	0.08**	0.06***	0.06***
	(0.01)	(0.01)	(0.04)	(0.04)	(0.02)	(0.02)
	[0.34]	[0.34]	[0.09]	[0.08]	[0.26]	[0.26]
Controls for trends[c]	No	Yes	No	Yes	No	Yes
N	145	145	145	145	145	145
R^2	0.119	0.151	0.009	0.078	0.085	0.122
Mean years of schooling[e]	−0.5***	−0.7***	−1.0**	−1.2**	−0.8**	−0.7**
	(0.2)	(0.2)	(0.5)	(0.5)	(0.3)	(0.3)
	[−0.26]	[−0.37]	[−0.09]	[−0.11]	[−0.22]	[−0.19]
Controls for trends[c]	No	Yes	No	Yes	No	Yes
N	85	85	85	85	85	85
R^2	0.077	0.326	0.007	0.239	0.043	0.261

Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Huber–White standard errors are in parenthesis. Brackets contain the proportion of a standard deviation change in the dependent variable brought about by a one standard deviation increase in the private enrollment rate.

[a] Based on census summary and micro information, data sources (16) and (17), as described in Table A.1.

[b] Calculated using administrative information, data sources (8) and (9).

[c] Controls for pre-existing and concurrent trends, as described in Table 3, notes 4 and 5.

[d] Based on test score information, data sources (1) and (2).

[e] Calculated using census and household survey information, data sources (16) and (11).

Table A.3
Reduced form regressions for sorting, 1982–1988

Dependent variable—1982–88 change in	Independent variable:					
	Urbanization rate[a]		Population[a]		Inter-quartile range in years of schooling[a]	
	(1)	(2)	(3)	(4)	(5)	(6)
Private enrollment[b]	0.08**	0.06**	0.44***	0.41***	0.16***	0.15***
	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)
	[0.31]	[0.23]	[0.33]	[0.31]	[0.36]	[0.34]
Controls: concurrent trends[c]	No	Yes	No	Yes	No	Yes
N	84	84	84	84	84	84
R^2	0.104	0.183	0.110	0.228	0.138	0.220
Sorting measure for Language[d,e]	−0.09***	−0.10***	−0.07**	−0.06**	−0.09**	−0.11**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)
	[−0.40]	[−0.45]	[−0.06]	[−0.05]	[−0.24]	[−0.29]
Controls: concurrent trends[c]	No	Yes	No	Yes	No	Yes
N	84	84	84	84	84	84
R^2	0.151	0.171	0.004	0.011	0.062	0.080
Sorting measure for Math[d,e]	−0.06***	−0.06**	−0.03	−0.03	−0.10***	−0.11**
	(0.02)	(0.03)	(0.02)	(0.02)	(0.04)	(0.05)
	[−0.28]	[−0.28]	[−0.02]	[−0.03]	[−0.28]	[−0.30]
Controls: concurrent trends[c]	No	Yes	No	Yes	No	Yes
N	84	84	84	84	84	84
R^2	0.073	0.128	0.001	0.052	0.076	0.120
Sorting measure for repetition[b,d]	0.17***	0.14***	0.28**	0.07	0.25**	0.09
	(0.04)	(0.05)	(0.11)	(0.10)	(0.08)	(0.07)
	[0.31]	[0.26]	[0.03]	[0.02]	[0.11]	[0.09]
Controls	No	Yes	No	Yes	No	Yes
N	163	163	163	163	163	163
R^2	0.100	0.130	0.003	0.076	0.014	0.080

Notes: *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Huber–White standard errors are in parenthesis. Brackets contain the proportion of a standard deviation change in the dependent variable brought about by a one standard deviation increase in the private enrollment rate.

[a] Based on census summary and micro information, data sources (16) and (17), as described in Table A.1.

[b] Based on administrative information, data sources (8) and (9).

[c] Controls for concurrent trends are the 1982–1992 change in population (data sources 17 and 18), and the 1982–1996 change in mean years of schooling among adults, and imputed labor income (from census and household survey information, sources 13 and 16).

[d] The sorting measure is: average characteristic in public schools/average characteristic in all schools.

[e] Based on test score information, data sources (1) and (2).

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