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When does Inter-School Competition Matter? Evidence from the Chilean "Voucher" System*

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Abstract

I investigate the effects of voucher-school competition on educational outcomes. I test whether voucher-school competition 1) improves student outcomes and 2) has stronger effects when public schools face a hard budget constraint. Since both voucher school competition and the degree of hardness of the budget constraint for public schools are endogenous to public school quality, I exploit (i) the interaction of the number of Catholic priests in 1950 and the institution of the voucher system in Chile in 1981 as a potentially exogenous determinant of the supply of voucher schools and (ii) a particular feature of the electoral system that affects the identity of the mayors of different counties (who manage public schools) as a source of exogenous variation in the degree of hardness of the public schools budget constraints. Using this information, I find that: 1) an increase of one standard deviation of the ratio of voucher-to-public schools increases tests scores by just around 0.10 standard deviations; and 2) the effects are significantly bigger for public schools facing more binding minimum enrollment levels.

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1 Introduction

Creating competition between schools is a cornerstone of voucher school proposals. Proponents have argued that by creating competition, vouchers create stronger incentives for public schools to increase quality. However, critics counter that school competition may increase student segregation and harm poor students. In parallel, a line of research on the effects of inter-school competition on student outcomes has not reached a consensus on the causal effects on academic outcomes. While some papers find positive and significant –but typically small–effects of school competition and school choice (e.g., Bayer and McMillan, 2005; Card et al., 2010; Gallego and Hernando, 2008, 2009; Hoxby, 1994, 2000, 2007; Lavy, 2005; and Sandstrom and Bergstrom, 2005), other papers do not find significant effects (e.g., Hsieh and Urquiola, 2006; Rothstein, 2006, 2007).

This paper contributes to the literature in two main areas. First I study the effects of inter-school competition on the academic outcomes of Chilean students who attend publicly subsidized schools in the only case in which a developing country has operated the complete K-12 sector under a "voucher" system for a long period of time (since 1981). In particular, I study a period in which the program was more mature. Voucher schools (that is, private schools that receive a voucher for each enrolled student) currently serve over 40% of all students. However, voucher school enrollment varies widely across different areas. While in 10% of the educational markets the voucher enrollment is more than 50%, about 20% of Chilean municipalities have no voucher schools in operation. Second, I study a critical dimension of the potential effects of inter-school competition (typically not considered in previous papers): the role of public schools budget constraints. If public schools face a soft-budget constraint (eg., in which public school deficits are financed with "non-voucher" transfers), one will not expect to see a significant responses from public schools to inter-school competition.[1] In the case of Chile we see a lot of variation in terms of proxies for the degree of softness of the constraints that public schools face, with some municipalities having persistent educational deficits because of the existence of non-voucher transfers from the central government. I argue and present evidence consistent with the claim that the municipalities with stronger political connections with

[1] In Gallego (2006) I present a model to rationalize the effects of competition on student outcomes. The main idea is that there are two types of schools in a market: public schools with no direct incentives to produce quality beyond meeting a minimum enrollment level; and voucher schools that face explicit competitive incentives. In this context, the model predicts (i) positive effects of voucher school entry on the quality offered by both voucher and public schools and (ii) that the extent of the response of public schools to voucher school entry depends on the minimum enrollment level needed by a public school to operate and on the size of the school age population. McMillan (2005), Ferreyra and Liang (2012), and Tapia (2010) present models with the same prediction regarding the ways in which competition affects public schools rents and through this channel thereby affects student outcomes.

the central government face softer constraints.

As a potentially exogenous determinant of voucher school competition in different markets, I exploit the interaction of the number of Catholic priests per person in 1950 in different areas of Chile with the establishment of the voucher system in 1981. I show that the number of priests per person is historically determined; and, the number of Catholic priests is correlated with the entry of both Catholic and non-Catholic voucher schools after 1981. Enrollment in *formally* Catholic voucher schools increases from, at most, 5% of the school-age population before the voucher reform to about 14% in 2002.[2] In addition, I present evidence that the entry of non-Catholic voucher schools to different markets is also related to the number of priests and present some hypotheses and evidence to explain this correlation. Moreover, before the 1981 reform, private school competition had no financial effects on public schools because revenues of public schools *did not depend* in any way on enrollment in private schools. Consistent with this, I also present some evidence suggesting that the number of priests per person had little effect on educational outcomes prior to 1981 and had a larger effect on outcomes after the voucher system was established. In other words, the potential validity of my identification strategy relies on the assumption that Catholic priests were present in the pre-voucher period, but that their effects on educational outcomes only became evident during the period when the voucher system was established. The evidence supports this view.

I estimate the *level effects* of the ratio of voucher-to-public schools on test scores in an educational market for a cross-section of students in 2002 (and also present additional results with data for 2006 and 2010).[3] Because my reduced-form results imply that priests had little effect on educational outcomes before 1981, I use the number of priests in different areas in 1950 as a potentially valid source of exogenous variation in the supply of voucher schools during the *post*-reform period. I find that once I instrument for the ratio of voucher-to-public schools in different educational markets, a one standard deviation increase in the ratio of voucher school to public schools in a market increases tests scores by about 0.10 standard deviations. The results are similar for students attending public schools. In contrast, the OLS estimates do not show a strong effect of voucher-school competition on test scores.

[2]Notice that by the Cannon Law of the Catholic Church, a school is formally recognized as Catholic when either (i) the Church directly appoints the school principal or (ii) the Church approves the appointment of the school principal. Therefore, many schools that are related to the Catholic Church are not considered *formally* Catholic.

[3]I focus on the results using 2002 test scores because the source of identification I use for the presence of soft-budget constraints in public schools is not available outside the 1999-2003 period.

Central to the main topic of this paper, the data support the existence of *interaction effects* of voucher school competition. Public schools located in municipalities that have relatively low proxies for minimum enrollment levels and high education deficits do not react strongly to voucher school competition. In fact, my results imply that the effects of voucher school competition are about six times larger for public schools with educational deficits below the median (my proxy for non-voucher transfers) than for schools with educational deficits above the median. In order to deal with potential endogeneity of municipalities with softer budget constraints, I use a short-lived change in the Chilean electoral law that allows me to identify the short-run variation in deficits and average school size and, therefore, to control for potential selection bias in my estimates. The electoral law which was in effect between 1999 and 2003 establishes that a mayor (who manages the public schools) is the elected member of the municipal council who receives the most votes, conditional on receiving at least 20% of the votes. This source of variation is useful for my identification strategy because it produced many experiments in which some municipalities presenting the same support for the government coalition (in terms of votes received) often had mayors that belonged to the opposition coalition. This is important because mayors receive discretionary transfers from the central government and, therefore, pro-government mayors are able to raise more of these funds.

This paper also contributes to the previous literature on the effects of voucher school entry on school quality in Chile (Auguste and Valenzuela, 2003; Carnoy and McEwan, 1998; Contreras and Macias, 2002; Gallego, 2002; Gallego and Hernando, 2008, 2009; and Hsieh and Urquiola, 2006) by providing new IV estimates using a potentially valid source of exogenous variation for voucher school entry (and providing a number of indirect tests to support its potential validity) and it also presents additional evidence that allows me to study the mechanism through which competition may affect the outcomes of public schools. The contribution in term of having potentially valid instruments is relevant because previously used instruments include variables like population size, urbanization rates or socioeconomic characteristics that are unlikely to meet the exclusion restrictions.

The remainder of the paper is organized as follows: Section 2 briefly describes the Chilean education sector. Section 3 presents the data used in this paper. Section 4 describes the identification strategy. Section 5 presents the results of difference-in-difference regressions using data on educational outcomes before and after 1981. Section 6 presents estimates of the level effects of voucher-school competition on student outcomes using a cross-section of primary students in 2002. Section 7 presents estimates of interaction effects and section 8 briefly concludes.

2 Institutional Setting: Primary and Secondary Education in Chile

Before 1981, the Chilean central government was involved in the funding and provision of education, the supervision and regulation of curricula, the handling of human resources and investment. The 1981 educational reform transferred the management of public education from the central to the local governments (municipalities), established a per-student subsidy (voucher) to be received by voucher and public schools according to enrollment, allowed parents to choose among all publicly-financed schools, and allowed would-be schools to enter the market.

Three types of schools emerged: publicly owned schools (managed by local governments), voucher schools (owned by private agents), and non-voucher schools.[4] The first two types of schools receive vouchers, the non-voucher private schools do not receive public funds, charge high tuition fees, and serve upper- income students. Voucher schools increased their student enrollment rate to about 42% in 2002 from 15% in 1981. Public school enrollment dropped from 78% in 1981 to 49% in 2002. The remaining enrollment corresponds to non-voucher private schools, which I do not include in my sample.

Public and voucher schools present important differences in terms of their incentive structures and the amount of non-voucher resources they receive. Voucher schools tend to display similar tendencies to competitive firms, whereby revenues received are proportional to enrollment. While some voucher schools are operated by for-profit firms, others are operated by non-profit organizations that raise additional funds in a relatively competitive market for donations to be spent in schools (Aedo, 1998).[5] In contrast, public schools work under "softer" budget constraints: where necessary, they can receive transfers to pay their expenses if they are losing students which are in addition to their vouchers (Sapelli, 2003). In addition, while vouchers were the only public intervention in the K-12 sector during the 1980s, governments during the 1990s channeled additional resources to "vulnerable" schools and increased non-voucher spending. My estimates are that around 30% of public expenditure on education for the average student is not related to the voucher (data for 2002). In terms of teacher regulations, the public school teachers faced flat wage structures and could not be easily removed from their positions– only from the mid-1990s could public school teachers be moved to other schools in the same municipality (Mizala and Romaguera, 2004). The number of teachers per student

[4] A small group of subsidized private schools did operate before the 1981 reform. These schools enrolled about 7% of the school-age population (estimates using data from the 2002 Social Protection Survey) and were financed through small public subsidies and private donations (Aedo-Richmond, 2000).

[5] Elacqua (2009) estimates that about 66% (59%) of voucher school students were enrolled in for-profit schools in 2008 (1990). Note that some for-profit schools are formally Catholic.

is 25% higher in public schools than in voucher schools (CENDA, 2002) and public schools are highly unionized.

The closing of public schools is a second institutional feature of the Chilean system that is interesting for this paper. The data suggest that about 8% of public schools stopped reporting test scores during the 1990s. The most likely reason for this is that these schools were closed or merged with other public schools. The closed schools tended to have fewer students than other public schools and to under-perform relative to other public schools (De Iruarrizaga, 2008). Interestingly, the teachers' union is always actively lobbying against the closing and merging of public schools. Moreover, the opinions of teachers reported in Bellei et al. (2003) suggest that teachers do not want to be moved from one public school to another. This evidence, in conjunction with the evidence on fixed wages, suggests that public school teachers earn significant rents in public schools.

Previous papers analyzing the effect of voucher school entry on academic achievement in Chile include Carnoy and McEwan (1998) who use a panel of schools from the early 1980s to the mid 1990s and find no effect of the share of voucher school enrollment on average test scores at the school level. Given that the entry of voucher schools may be endogenous, all the other papers in the literature present some instrumental variable approach to deal with the endogeneity problem. These papers could be classified according to the time period studied. Hsieh and Urquiola (2006) study the change in educational outcomes at the municipality level between 1982 and the early and mid 1990s and find no effect of the change in voucher school enrollment on outcomes. Even though the focus of that paper is on OLS estimates, Hsieh and Urquiola (2006) also present some IV estimates using population size, urbanization rate and the inter-quartile range of education in 1982 as instruments for voucher school enrollment.

These results contrast with other papers analyzing the effect of school competition on test scores using data for the 1990s: Gallego (2002) finds a positive effect of voucher school enrollment on test scores using school level data for 1994-1997 and IV estimates (the instruments are urbanization rates and population size). Contreras and Macias (2002), also using school level data, find a positive effect of competition (measured using a Herfindahl index) on pre-college tests for 1998 using instrumental variables (population size, area, lagged Herfindahl index, and availability of private schools, among others). Auguste and Valenzuela (2003) present the paper most directly comparable to that of Hsieh and Urquiola (2006). They present both student and market level regressions for the effect of voucher school enrollment on test scores using data for the late 1990s and

find a positive and significant effect of voucher school entry on test scores.[6]

Differences in the time period studied are important to enable an understanding of the differences in results between the two papers. It is hard to argue that the system operating in the 1980s was a real voucher system from the point of view of several agents: (i) public school budgets were not affected by the voucher reform (in part because the decentralization of public schools was not completed until the late 1980s), (ii) employment of public school agents was quite rigid until the mid-1990s, (iii) test scores were not public until the mid/late 1990s, (iv) local governments were not elected democratically until 1992, and (v) the real value of the voucher decreased steadily during the 1980s and only recovered in the early 1990s. All these factors may explain why the effects are positive in the 1990s (when the educational system was operating under rules that are closer to the textbook version of a voucher system) and insignificant in the 1980s.[7] My paper presents additional evidence of positive effects of voucher school competition in the 2000s.

3 Data

I use several datasets in this paper. Table 1 presents the main variables used, the level at which each variable is collected, and the descriptive statistics of each variable. I use data on students' educational outcomes, their backgrounds, school characteristics, and the characteristics of the area in which they attend school from the 2002 SIMCE (*Sistema de Medición de la Calidad de la Educación*) test dataset, which was administered to 4th graders.[8] This test has been administered nationally since 1988 to more than 90% of students in a given grade which varies each year (4th, 8th, or 10th graders). I use the average of the Math and Spanish portions of the test (standardized to have an average of 0 and a standard deviation of 1) as my measure of academic outcomes. I use income per household member and mother's education to measure the socioeconomic background of students.[9]

Second, I use the CASEN (*Encuesta de Caracterización Socioeconómica*) 2000 survey, which collects information on socioeconomic variables for a representative cross-section

[6]Using a structural model of school choice, Gallego and Hernando (2009) present evidence of a negative effect of school market power on the level and growth rate of test scores.

[7]Another important difference between the 1990s and the 1980s is that test scores for the complete population are only available for the 1990s.

[8]As mentioned above, I also ran regressions for 2006 and 2010, also using the SIMCE dataset but I have not presented descriptive statistics to save space.

[9]I use five categories to measure mother's education (having attained, at most, primary education, secondary general education, secondary technical education, post-secondary technical education, and college or postgraduate education).

of the population. I use a high school graduation dummy as a measure of educational attainment for members of different cohorts that attended school in different places and a measure of participation in organizations, as a proxy for social capital at the municipality level. Third, I use the 2002 Social Protection Survey (called "Labor History and Social Security"), which collects life-time information for a sample of individuals. I use information on high school graduation rates at the market level for individuals attending school before the 1981 reform, the migration decisions of parents with school-aged children in 2002, and information on the type of school attended (public, subsidized private, and paid private).

I measure the degree of voucher school competition as the ratio of voucher schools to public schools in each educational market. I use 297 municipalities and the Metropolitan Area of Santiago as proxies for local educational markets. Municipalities are separate educational markets because, with the exception of municipalities in the Metropolitan Area of Santiago, most students attend schools in the town where they live (Hsieh and Urquiola, 2006; and Sapelli and Vial, 2002). Data on the availability of schools in each market come from the Ministry of Education files.

Data on religious variables at the diocese level for my identification strategy are from the Vatican´s yearly publication called *Annuario Pontificio* (the number of priests, the share of Catholics, and the ratio of order to total priests in each Chilean diocese).[10]

I also use other sources of data in some empirical exercises. Data on Catholic schools come from the school directory of the Chilean Catholic Church (http://www.feducech.cl/ and http://www.iglesia.cl/). Data on municipal variables such as the size of public schools and the number of community organizations per 1,000 people in the county come from the Chilean Municipal Dataset (available at http://www.sinim.cl/). Finally, I use information on electoral outcomes at the municipality level from the Chilean Electoral Office when analyzing the interaction effects of inter-school competition.

[10]In particular, I use data on priests per-capita for 1950 from the Annuario Pontificio. I estimate the number of priests in the different dioceses in 1950 by considering the territorial division existing in the 1990s (which includes 26 dioceses) and the number of priests in 1950. I make this adjustment because some dioceses (namely Santiago) included disproportionately large areas of the population in 1950. Between the 1960s and the 1990s, new dioceses appeared when some dioceses were split up—so the number of dioceses increased from 19 to 26. I assume that the distribution of priests *within* the split dioceses is given by the distribution when the new dioceses were created (the distribution of priests within dioceses in the following periods is quite stable). In all the empirical applications, I cluster standard errors at the 1950 dioceses level.

4 Identification Strategy

One major challenge for an empirical analysis of the relationship between voucher school competition and educational outcomes is the potential endogeneity of the number of voucher schools. In this section, I argue that the interaction of the number of Catholic priests per person in 1950 and the school reform of 1981 allows me to identify the exogenous variation in the number of voucher schools in different educational markets.

The potential endogeneity between the number of voucher schools and public school quality is a relevant issue. Increases in the quality of public schools decrease the number of voucher schools operating in an area and have a positive effect on the quality offered by public schools. Thus, entry into the market is endogenous to public school quality and therefore, simple correlations or OLS estimates will produce *downward* biased estimates of the causal effect of voucher-school competition on educational outcomes. Alternatively, OLS estimates could be biased upward if voucher school entry responds to some unobserved (to the econometrician) characteristic of the market that has a positive effect on school quality.

My identification strategy exploits the interaction of the (log of the) number of Catholic priests per person in 1950 and the 1981 reform to identify the exogenous variation in the number of voucher schools in an area, after controlling for the share of Catholic population.[11] The basic motivation for this identification strategy is two-fold. On the one hand, there are direct effects of the number of priests on Catholic schools. The involvement of priests in educational activities is understood as a key element of their religious mission (Garrone, 1977). However, although Catholic priests were involved in schools before the reform (my estimates are that, at most, 5% of the school-age population attended Catholic schools that were publicly subsidized),[12] there is a large increase in the enrollment in Catholic voucher schools after the reform, which raises to a level close to 14% of the school-age population in 2002. This increase takes place along three margins: (i) new Catholic schools were installed, (ii) existing Catholic schools in-

[11] By controlling for the share of the Catholic population, I take potential direct effects of this variable on educational results into account (as suggested by recent research on the effects of religious affiliation on income, education, and other social and economic variables, e.g., Barro and McCleary, 2003 and Gruber, 2005).

[12] These estimates are computed as follows: the Social Protection Survey implies that 7% of those in pre-voucher cohorts attended subsidized private schools. Espinola (1993, quoted in Hsieh and Urquiola, 2006) reports that 53% of pre-reform subsidized private schools were Catholic. If Catholic and non-Catholic schools are of the same size, then I estimate that 3.5% of the school-aged population attended Catholic subsidized schools before the reform. However, I know that today Catholic schools tend to be bigger than non-Catholic schools. Therefore, using the ratio of size between Catholic and non-Catholic schools in 2002, I estimate that around 4.7% of the school-aged population before the reform was in Catholic subsidized schools.

creased in enrollment, and (iii) formerly paid Catholic schools became voucher schools after the reform.[13] [14] The main reason for this large increase in enrollment in Catholic voucher schools is that Catholic schools start to receive vouchers only after the reform (the voucher represented an increase of about 160% in the value of the subsidy received by Catholic schools from the government),[15] allowing priests to establish new schools or to expand enrollment among middle- and lower-class students.

Priests are important actors in Catholic schools. These schools can be owned directly by the Church, by religious orders, or by people supported by the Church, but they always have at least one priest acting as a chaplain. Nowadays, priests tend to focus on pastoral ministries (e.g., being chaplains and teaching religious education classes) and on the management of schools. Priests' time commitments in school include significant amounts of time working with students, teachers, and parents. Priests working in schools receive wages that are comparable to those of other teachers. Pasalacqua (2004) reports that around 5% of the teaching staff and 10% of the non-teaching staff in Catholic voucher schools are religious personnel (including not only priests, but also brothers and nuns).

In addition, the number of priests may have effects on the establishment of non-Catholic schools. First, as previously discussed, the formal definition of a Catholic school is restrictive and therefore, there may be some schools that are somewhat informally connected to the Church.[16] Second, some non-Catholic voucher schools may have been established by former teachers of Catholic schools (or even priests/nuns that retired from their religious orders). A slight variation of this mechanism is non-Catholic voucher

[13]Unfortunately, there are no systematic data on enrollment in Catholic schools before and after the reform. My estimates, using data from Pasalacqua (2004), are that 25% of enrollment in Catholic voucher schools in 2002 corresponds to brand new Catholic schools and between 15 and 25% of the enrollment corresponds to schools that became voucher schools after the reform.

[14]Anecdotal evidence from one of the most important Catholic groups in Chile (the Marist brothers) may help to understand the increase in enrollment in Catholic voucher schools after the reform. Enrollment in Marist schools was 5,000 students in 1980, with about 10% of these students in subsidized schools. In contrast, enrollment was 14,800 in 2002, with 40% corresponding to voucher schools. About two thirds of enrollment in voucher schools corresponds to schools established after the reform (Personal communication with Br. Aldo Pasalcqua).

[15]This value is computed as follows. The intial value of the voucher was 30% higher than expenditure per student in public schools before the reform. Before 1981, private schools received an average of 50% of public schools expenditure per-student (Hsieh and Urquiola, 2006). Therefore, the nominal value of the voucher increased by 160%.

[16]Again, I have no systematic data to quantify this hypothesis, but two examples may help us to understand the magnitude of this phenomenon. About 40% of enrollment in Jesuit-connected voucher schools (through the *Red Educacional Ignaciana* and *Fe y Alegria Chile*) is not counted as enrollment in Catholic schools. Most of these schools were established after the reform. Similarly, enrollment in the (two large) Opus Dei voucher schools in Santiago is not counted as in Catholic schools because Opus Dei schools are not connected to the local Catholic bishop.

schools that establish religious practices, names and curricula in their schools to try to mimic Catholic schools in the same area. And, third, the propensity of parents to send their children to private schools may be affected by the presence of old Catholic schools (and, therefore, priests) in the same area. I do not have systematic evidence of these three channels, but I present evidence in the next two sections that (i) the number of priests is correlated with the entry of non-Catholic schools and (ii) the share of for-profit schools with explicitly Catholic names is increasing in areas with more Catholic schools.[17]

In addition, there has been a fundamental change in the incentive scheme related to the 1981 reform. Even if enrollment in Catholic schools were constant after the reform, the effect of priests on the behavior of public schools changes dramatically: from a system where public school funds did not depend at all on enrollment in private schools to a system where total funds are fixed and are allocated proportionally to enrollment. The actual degree of school choice should have changed dramatically during the reform period, even if the enrollment in private schools were the same. This further motivates the use of the interaction between the number of priests in 1950 and the establishment of the voucher system as an instrument for voucher school competition and expecting the effects of the number of priests to be significantly larger over the voucher period.

Finally, the number of priests per person is historically determined and varies widely across Chilean dioceses, despite the majority of the population being Catholic.[18] While the average diocese has a ratio of about 0.15 priests per 1,000 people, the diocese with the highest ratio (0.23 priests per 1,000 people) has more priests per person than most Latin American countries; and, the diocese with the lowest ratio (0.06 priests per 1,000 people) is comparable to what is observed in a poor (and non-Catholic) country such as Kenya. The variation in the number of priests across Chilean dioceses has to do mainly with the fact that religious orders are more numerous in some areas than others.

Religious orders established themselves in a non-uniform way in different dioceses in the past. The allocation of orders to different dioceses had mainly to do with the desire to bring priests to some Chilean areas beginning in the mid-nineteenth to the mid-twentieth centuries (Aliaga, 1989; Araneda, 1986; Barrios, 1992). Some areas ended up with more order priests because the bishops of some dioceses either belonged to orders themselves or were more open to receiving order priests. While 71% of the order priests

[17]In Gallego (2006) I also show that priests do not affect the propensity to attend Catholic vis-à-vis non-Catholic voucher schools at the individual level.

[18]In the Catholic Church, a diocese is an administrative territorial unit, composed of many parishes and governed by a bishop. Technically, each diocese is independent of the others and the bishop is answerable only to the Pope.

in Chile in 2002 belonged to orders that entered the country between 1810 (the year of independence) and the 1950s, only 5% of these priests belonged to orders that entered the country after the 1950s.[19] In general, there are more order priests than non-order priests, conditional on the establishment of order in a given location. Therefore, dioceses where religious orders work tend to have more priests. The historical roots of the presence of orders in different areas creates a positive correlation between the number of priests today and in the past: the correlation between the number of priests per capita during the 1990s and the 1950s is 0.78.

Table 2 illustrates the importance of orders in explaining the cross-diocese variation in (the log of) priests per capita in 1950. I use the ratio of order priests to the total number of priests as a proxy for the presence of orders in different dioceses. Results in columns 3 and 4 support the claim that the variation of priests per capita is related to the presence of orders in different dioceses.

I use priests per capita in 1950 as the main instrument for voucher school entry in different areas during the voucher period, and the ratio of order-to-total priests as an alternative instrument in some regressions. The next section empirically studies the validity of this identification strategy.

5 Reduced Form Estimates: Difference-in-Difference Analysis and Robustness Checks

To validate the identification strategy discussed in the previous section, I need to show that the number of priests is not related to educational outcomes before the voucher reform and are more related to educational outcomes after the reform. Table 3 presents the basic evidence giving some support to this identification strategy. In particular, I focus on three different cohorts. People older than 37 in 2000 (the year the CASEN survey was collected) attended school before the reform was implemented. People between 26 and 37 years old attended school between one year and 11 years after the reform was implemented; and those under 26 received their complete K-12 education after the reform was implemented. Because I do not have data on test scores before the reform, I use high school graduation as a proxy for school quality.

I estimate the impact of my main instrument on high school enrollment by running regressions of the form:

$$h_{cmr} = \bar{\alpha} Z_m + \bar{\beta} * D_{26-37} * Z_{mr} + \bar{\gamma} * D_{<26} * Z_{mr} + X_{cmr}^{'} \bar{\delta} + \bar{\varepsilon}_{cmr}$$

[19] These numbers were computed using information from the 2002 directory of the Catholic Church in Chile.

where h is high-school graduation rate in county m for a person belonging to cohort c (considering the three cohorts mentioned above), and region r.[20] Z is the log of per capita priests in 1950, D_{26-37} is a dummy that takes a value of one for the cohort being between 26 and 37 years old in 2000, and D_{26-37} is a dummy that takes a value of one for the cohort being less than 26 years old in 2000. X is a vector with control variables (including interactions of the log of total population, urbanization rates, and the rate of Catholic affiliation with dummies for the different cohorts) and $\bar{\varepsilon}$ is an error term (which may include fixed effects that vary at different levels in different specifications). Thus, $\bar{\beta}$ and $\bar{\gamma}$ identify whether Z has any differential effect for the younger cohorts with respect to the cohort of people older than 37 in 2000. My identification strategy will be valid if I find that $\bar{\alpha} = 0$ and a positive effect for the cohorts attending school after the 1981 reform.

I start presenting a specification in which I control for region and cohort effects (column 1). Results suggest that the number of priests has no relationship to high school graduation rates for people attending school before 1981 and that both $\bar{\beta}$ and $\bar{\gamma}$ are statistically different from 0; implying that priests are more correlated with educational outcomes after the reform. In fact, interestingly, $\bar{\gamma} > \bar{\beta}$, which is expected as people under 26 years old were exposed to the reform over a longer period of time.[21] Next, in column (2) I add $region * cohort$ fixed effects and find that my instrument is not significantly related to high school enrollment for the older cohorts (as in column 1) and the point estimates of $\bar{\beta}$ and $\bar{\gamma}$ do not change a lot with respect to column (1). In turn, the standard errors increase in a significant way and, therefore, the estimates for both coefficients are not different from 0. However, it seems that most of the increase in the standard errors comes from including too much noise in the estimation because (i) the $region * cohort$ fixed effects are individually and jointly not different from 0 (in the case of the joint test with a p-value of 0.83) and (ii) a Hausman test suggests that the estimates without the $region * cohort$ fixed effects are not statistically different than those excluding them (with a p-value of 0.83). Overall, my conclusion from these two exercises is that my main instrument is not statistically related to educational outcomes before the reform and the estimates seem to suggest a positive correlation with my instrument for the period after the reform.

Figure 1 presents a more detailed exercise to evaluate the differentiated effects of priests on educational outcomes for cohorts that have different degrees of exposure to

[20] Municipalities in Chile belonged to 13 different regions in 2002.

[21] In addition, it is worth mentioning that both $\left(\bar{\alpha} + \bar{\beta}\right)$ and $(\bar{\alpha} + \bar{\gamma})$ are statistically different from 0 (with p-values of 0.07 and 0.04, respectively).

the 1981 reform. This figure plots the relation between high school graduation at the individual level and priests in the decade closer to school attendance for individuals of different ages in 2000. This exercise also allows me to study whether the effect of the reform varies for individuals of different ages. The results suggest that the number of priests is only correlated with the level of education for cohorts that attended school after the reform was implemented. As importantly, the effect of the number of priests on high school attainment increases according to the number of years that people attended school after the reform.[22]

Overall, these results suggest that priests are only correlated with educational outcomes after the 1981 reform, and they confirm the rationale presented in Section 4. Having established this central result, I present a number of additional exercises to validate my identification strategy. Columns (1) and (2) in Table 4 present estimates of the relationship between priests per capita, the ratio of order-to-total priests in 1950 and the ratio of voucher-to-public schools in each market in the voucher period. Priests and the ratio of order-to-total priests both have a positive and significant effect on the number of voucher schools per public school. The effect of Catholic affiliation is also positive and significant, as expected.[23] Column (3) presents the same regression but using voucher school enrollment as the measure of voucher school entry (as in all the other papers in the literature). Again, priests have a positive and significant effect on voucher school enrollment.[24]

The next columns study the correlation between priests and Catholic and non-Catholic voucher schools. Column (4) presents the correlation between priests and Catholic schools, which is positive and statistically different from 0. In the remaining columns I study a margin that is particularly relevant in the case of Chile: for-profit and non-for profit schools (Elacqua, 2009; Tapia, 2010). Using data used in Tapia (2010)

[22]In addition to these exercises, using individual data for a small sample of individuals from the 2002 Social Protection Survey, I studied whether the effect of priests on attendance at voucher schools changes for members of cohorts that attended school after and before the voucher reform, controlling for a dummy for urban areas, and region and age dummies. Results, available in Gallego (2006), suggest that the number of priests is more connected with the decision to attend a voucher school after the reform, as expected.

[23]Results for other variables included in these regressions are similar to other papers in the literature: mean (standard deviation of) education and income have positive (negative) effects on the availability of voucher schools, and more populated areas have more voucher schools.

[24]I also carried out an exercise in which I estimated the effect of the number of priests on the *change* in public school enrollment from the pre-reform period. I was able to compute public enrollment rate for 52 geographical areas for 1975 from MINEDUC (1975)–the geographical classification corresponds to the division of 26 national Chilean provinces into urban and rural areas. I define the change in enrollment in public schools as enrollment today minus enrollment in 1975 in the area where the school is located. Results, available in Gallego (2006), imply that public school enrollment decreased more in areas with more priests, confirming my argument in Section 4.

I classify voucher schools in for-profit and non-for profit schools. Columns (5) and (6) imply that priests are positively correlated with both for-profit and non-for profit schools. However, as expected, results in column (7) imply that the ratio of profit to voucher schools decreases in areas with more priests. This is interesting as it suggests that areas with many priests have a lower share of profit schools, as is implicit in my identification strategy. Moreover, among the for profit schools, I identified those with religious names (as a proxy for having a religious orientation) and using this information I ran regressions of the supply of both types of for profit schools on my instrument.[25] Interestingly, the results in columns (8) and (9) imply that my instrument is only correlated with the supply of for profit schools with religious names, which gives additional support to my identification strategy.[26] AQUI

One potential concern for my identification strategy is that priests may have affected education outcomes after the reform through channels other than voucher-school competition. In this context, I study two dimensions that may be important. First, I study whether the migration decisions of families with school-aged children are correlated with the presence of priests in different areas. If in-migration rates of families with children are higher in areas with more priests, then an alternative explanation may be selection by families based on their taste or motivation for education. I do not expect this channel to be important, given the available evidence showing that migration in Chile is low because of public housing policies (Soto and Torche, 2004; Tironi, 2003). The results in Panel A of Table 5 confirm this presumption and show that both micro estimates of in-migration decisions of families with children and macro estimates of immigration rates at the regional level do not support the view that in-migration rates are higher in areas with more priests.

Second, another potential challenge to my identification strategy is related to the fact that the 1981 reform also included decentralization of public schools to the local level. If the effects of this reform are stronger in areas with more social capital and social capital is higher in places with more priests (because the Catholic Church is stronger), my instrument may have direct effects on school quality above and beyond its effects through voucher school competition. To address this point (in addition to controlling

[25] I thank a referee for suggesting this exercise.

[26] One example may help us understand the working of this correlation. Let us consider the city of Los Angeles (a big city located in the South of Chile) in which the supply of Catholic schools is high (about 44% of voucher schools belong to Catholic orders) and more than half of the for-profit schools have religious names. Interestingly, more than half of these for-profit schools with religious names were funded in the 1990s (in contrast, just around 1/3 of the other for-profit schools started operating in the 1990s) suggesting that entry of these schools to the market was related to a latent demand for "religious" education.

for the share of the Catholic population in an area in all the relevant regressions), in Panel B of Table 5, I ran regressions of two measures of social capital (participation in organizations and the number of community organizations per 1,000 people at the municipality level) on priests per capita, controlling for the other variables included in my regressions. Results in both columns imply that the number of priests is not significantly correlated with the two measures of social capital.

Overall, I document that the number of priests per person is historically determined; prior to 1981, it had little effect on educational outcomes. I also show that the number of Catholic priests affected educational outcomes after the voucher system was established and are correlated with several proxies of the levels of and changes in voucher school entry. Moreover, I present evidence that suggests that the number of priests is also correlated with the entry of non-profit schools and also with the entry of for-profit schools with religious names. These results suggest that I have a potentially valid source of exogenous variation of the supply of voucher schools in different areas during the voucher period. Using these results, the next section estimates the level effects of voucher-school competition on student test scores in a cross-section of students in 2002.

6 Estimating Level Effects

In this section, I present the results of regressions using information on test scores from a cross-section of students in the voucher period. This approach has several advantages over the reduced form estimates I presented before. First, I have detailed information on the degree of voucher-school competition in the educational market where the student attends school. Second, I have a more direct measure of school outcomes (test scores) than in the previous exercises (high school graduation), which allows me to estimate more precisely the effect of voucher-school competition on test scores. Third, I am able to study whether the interaction effects predicted by the model are supported by the data. Since the results in Section 5 suggest that the number of priests only affects educational outcomes after the reform, the number of priests per capita prior to 1981 is a valid instrument for voucher-school competition during the reform (as well as for the interaction of priests with the reform). Thus, I estimate the impact of voucher-school competition on student test scores by running a regression of the form:

$$q_{im} = \pi R_m + X_{im}^{'}\alpha + M_m^{'}\beta + Y_m^{'}\rho + \varepsilon_{im}. \quad (1)$$

Subscript i refers to students and m to educational markets. q is test scores. R is the ratio of voucher to public schools in market m. X is a vector including pre-school

characteristics of students (mother's education and log of income per household member), M is a vector including the mean and standard deviation of mother's education and income at the market level, Y is a vector including exogenous variables (Catholic population, total school age population, urbanization rate, and region dummies), and ε is a student-specific error term.[27]

I use the ratio of voucher-to-public schools as my measure of voucher-school competition—R_m in equation (1)—at the market level. When I compute the number of voucher schools per public school at the market level, I obtain the average availability of voucher schools per public school in each neighborhood (assuming that one public school exists in each neighborhood, which is reasonable in the case of Chile).

I estimate equation (1) using the log of Catholic priests per person in 1950, and/or the ratio of order-to-total priests (only in some regressions to save space), as my instrument for R_m. In addition, as discussed in Section 4, the ratio of order-to-total priests corresponds to a more basic source of variation in the number of priests in different areas in Chile. Thus, I also present IV estimates using the ratio of order-to-total priests as an alternative instrument (in one specification I use both variables as instruments for voucher-school competition). I use variables measured in 1950, which corresponds to the end of the period of entry of Catholic orders into Chile.

I first estimate equation (1) using the complete sample of students attending public and voucher schools in the 2002 SIMCE dataset. Table 6 presents OLS estimates (column 1) and IV estimates of equation (1) using log of priests in 1950 (column 2) and the ratio of order-to-total priests (column 3) as my instruments for R_m, including market and student-level controls.[28] The IV estimates are larger than the OLS estimates as suggested by my model, because school entry may be endogenous to quality. While the OLS estimate is -0.02, the IV estimates are in the interval between 0.13 and 0.15.[29] The IV point estimates are always positive and significant. These results imply that the effects of voucher-school competition on test scores are also economically relevant. An increase of one voucher school per public school in a market is associated with an increase in test scores of between 0.13 and 0.15 standard deviations. This is equivalent to about half of the effect of increasing mother's attainment from primary to secondary

[27]Since I include multiple observations of variables in the same area, I use the White/Huber estimator of the variance-covariance matrix to compute corrected standard errors that are robust to arbitrary heteroskedasticity and clustered standard errors.

[28]In Gallego (2006) I also present parsimonious models without including controls and only including my measure of voucher-school competition. The results are very similar.

[29]Measurement error in voucher-school competition may also explain why my OLS estimates are smaller than my IV estimates.

general education.

Estimates for other variables included in the regression have the expected signs. All socioeconomic controls are significant and have the expected sign: students with more educated mothers tend to perform significantly better, and students from households with higher incomes have higher test scores. The only two market-level variables with a statistically significant effect are mean per capita income (a positive effect) and the school-aged population of the market (negative effect). The effect of the share of Catholics is negative, but it is not precisely estimated.

Next, column (4) in Table 6 presents estimates using *both* priests and the ratio of order-to-total priests as instruments. As expected, the estimated effect falls between the estimates in columns (2) and (3). More importantly, an over-identification test of this specification does not reject the null hypothesis that the instruments are valid. Formally, the result of the over-identification test implies that IV estimates using each of the two instruments separately are not statistically different among them.

These estimates of the effect of voucher school competition on test scores are in the range of the estimated results of previous studies for Chile. My estimates imply that a one-standard deviation increase in voucher school competition increases test scores by about 0.10 standard deviations. Auguste and Valenzuela (2003) find an effect of the same size at the student level, Contreras and Macias (2002) report an effect of a one-standard deviation increase in the Herfindahl index in the interval of 0.08 and 0.17 standard deviations of test scores and Gallego (2002) presents estimates that imply an increase of between 0.03 and 0.18 standard deviations of the test scores when his measure of competition increases by one standard deviation. In turn, Gallego and Hernando (2008, 2009), using a structural model of school choice, present evidence that a one standard deviation decrease in market power increases test scores by about 0.10 standard deviations. In contrast, my estimates are slightly smaller than the effects of inter-school competition reported in papers for the US. Bayer and McMillan (2005) and Hoxby (2000) report that a one-standard deviation increase in their proxies for the degree of inter-school competition increases test scores by about 0.15 standard deviations.[30]

In the next two columns of Table 6 I study whether the estimated effects I find for 2002 are also present in the 2006 and 2010 cross-section of students taking the SIMCE

[30]In additional exercises I replace my measure of voucher-school competition with three alternative measures: the share of enrollment in voucher schools (Auguste and Valenzuela, 2003; Gallego, 2002; Hsieh and Urquiola, 2006), the Herfindahl index (Contreras and Macias, 2002), and the change in enrollment in public schools between the pre-reform (1975) and post-reform periods (2002). Results reported in Gallego (2006) imply a positive effect of the three alternative measures of voucher school competition on test scores.

test.[31] Results imply that the standardized effect for the 2006 and 2010 cross-sections of the variable of interest of 0.12 is very similar to our main results for 2002. In all, these results suggest that the positive and statistically significant effect of voucher-school competition are also present in 2006 and 2010.

Finally, in the last column of Table 6 I present estimates for sub-samples of students in public schools. This sub-sample allows me to analyze whether the estimated effect of voucher-school competition on student outcomes in public schools is different than for the whole population. These exercises are of interest from two perspectives: (i) I am able to study whether voucher or public schools react more strongly to voucher-school competition and (ii) I am able to test the conjecture of some papers that voucher-school competition may increase average test scores but students attending public schools may be harmed if voucher schools are able to cream-skim. Indeed, I cannot disentangle the contributions of both perspectives without an additional source of exogenous variation.

Since I estimate (1) for a sub-sample of students, I implement a Heckman selection model with endogenous variables (Wooldridge, 2002). This procedure allows me to control for potential selection bias if the students included in these regressions are not randomly selected from the population.[32] The results in the last column of Table 6 imply that public schools react similarly to the total sample to the entry of an additional voucher school; suggesting that either voucher schools do not tend to cream-skim, peer effects do not have a causal impact on test scores, *or* the incentive effects created by the voucher system on public school students dominate any cream-skimming effect.

Overall, the evidence presented in this section presents a consistent pattern of positive effects of voucher-school competition on test scores for the average student and for students attending public and voucher schools.[33]

[31] I only present the estimates using both instruments to save space and to be able to report over-identification tests. The fact that my data pass the over-identification tests implies that estimates using just one of the instrumental variables are not statistically different among them.

[32] To implement this procedure, I need to find a variable that affects the selection of students in different schools and has no direct effect on test scores. My instrument in the selection equation is a dummy that takes a value of one if the teaching of values was among the top three criteria used by parents for choosing schools. Since the mention of "teaching of values" (i.e., *la enseñanza de valores* in Spanish) has a religious connotation in Chile, this variable may capture relative preferences for voucher vis-a-vis public schools, or Catholic vis-a-vis non-Catholic voucher schools. Results for the selection equation are reported in Table Appendix 1 and discussed in detail in Gallego (2006). Using this selection equation, I include the inverse of the Mills ratio in the regression reported in the last column of Table 6.

[33] I have implemented several robustness and specification checks to the results in Table 6 (reported in Gallego, 2006): (i) I estimate the regression excluding parents that attended school after the 1981 to address the fact that my instruments could be correlated with their educational outcomes and the estimated effect of voucher-school competition is mainly unchanged; (ii) I estimate regressions excluding schools located in rural areas because the high physical transportation costs of moving from one school

7 Estimating Interaction Effects

In this section, I expand the previous analysis by studying the hypothesis that public schools responses to exogenous changes in voucher school competition depends on how binding their budget constraints are. We expect that the effect of voucher school entry on test scores is smaller for schools that have soft budget constraints (which tend to correspond to receiving big non-voucher transfers as documented in Sapelli, 2003 and Gallego et al., 2010). If this is the case, public school agents face less strong incentives to react to the competition because they can receive additional resources to finance their expenses. For instance, in Gallego (2006) I present a model that predicts that the effect of voucher school entry on test scores is smaller for schools that have low minimum enrollment levels (which in the model corresponds to receiving large non-voucher transfers) because public schools can meet the minimum enrollment constraint more easily.

To test this hypothesis against the data I face two empirical challenges because I need to find both empirical measures of the presence of soft budget constraints and some valid source of exogenous variation to identify causal effects of my measures of soft budget constraints. In order to measure the presence of soft budget constraints I use two proxies at the municipal level: (i) the size of the education deficit as a percentage of education revenues (a proxy for non-voucher transfers) and (ii) the average size of public schools in different municipalities. I interpret these characteristics as the degree of softness of the public school budget constraints. In general, non-voucher transfers are not allocated to local government in a transparent way. Serrano and Berner (2002) document the transfer process for the case of local government education debts related to teacher pensions. As the authors document, it is not easy to follow the actual decision process; in part because central government authorities did not want to establish precedents to be used by other

to another in those areas should decrease the impact of competition: results confirm this idea as the point estimate of inter-school competition increases when excluding rural schools; (iii) I include controls for systematic differences in pre-reform educational outcomes using three different proxies for pre-reform outcomes (high school graduation rate in public schools for cohorts that attended school before 1980 from the Social Protection Survey, high school graduation rate at the municipality level for cohorts that attended school before 1980 from the CASEN survey, and the average 1991 SIMCE test scores at the municipality level) and find that my main estimates of the effect of the ratio of voucher-to-public schools change slightly in value, but remain statistically significant; (iv) I introduce controls for the composition of students at the school level: mean and standard deviation of mother's education and per-capita income (following Hoxby (2000) in including these variables without giving a formal interpretation to the estimates) and find that the point estimate of the effect of voucher school competition is basically unchanged with respect to most other estimates I present in Table 6; and (v) I estimate quantile regression estimates of voucher-school competition for students in different positions of the distribution and find that the estimated effects do not vary a lot across quantiles, but are slightly smaller for the students in the 1st and 10th quantile than for the other students.

mayors.

In terms of the motivation for the second proxy I use, Gallego et al. (2010) study the determinants of average school size (measured using actual enrollment) in a context in which many public schools operate at an inefficient scale. We find that average public school size is lower in municipalities with pro-government mayors and in areas with a lot of political competition, controlling for other observable variables at the municipal level. The basic intuition behind our result is that in these areas the political consequences of closing or merging schools are significantly higher in terms of the probability that a mayor may lose her post. We also show that these differences in school size are significantly correlated with educational expenses, which suggest that municipalities with low average enrollment have access to alternative (non-voucher) sources of financing.

I study whether differences in these proxies affect the response of public schools to voucher-school competition. To do so, I split the sample of public schools into those located in municipalities that have education deficits above and below the median, and public schools located in municipalities that have average school sizes above and below the median of the distribution. I expect the effect of voucher-school competition to be larger in the samples in which educational deficits are relatively low and average school sizes are relatively big.

I use a short-lived change in the Chilean electoral law that allows me to identify the short-run variation in deficits and average school size and, therefore, to control for potential selection bias in my estimates. The electoral law operating between 1999 and 2003 established that the mayor (who manages the public schools) is the elected member of the municipal council who receives the most votes (conditional on getting at least 20% of the votes).[34] This source of variation is useful for my identification strategy as it produces a number of experiments in which municipalities with the *same* support for the pro-government coalition have in some cases pro-government mayors and, in others, opposition mayors.[35] The alignment of mayors to the government coalition is important because, as previously discussed, municipalities receive discretionary transfers from the

[34]This is the reason why I do not implement the same exercise for 2006 and 2010. During this period the mayoral election was different: there were candidates specific for the mayor post (in contrast to the 1999-2003 period in which mayors were elected among the city council candidates) and, therefore, I cannot use the same identification strategy.

[35]A specific example may clarify the working of my identification strategy. Let us consider El Bosque and San Bernardo, two neighboring municipalities located in Santiago. The total support for all the candidates of the opposition coalition who were standing for the municipality council was very similar –32.3% in El Bosque and 34.6% in San Bernardo– but in San Bernardo the mayor belonged to the opposition coalition. The explanation for this is simple: the main candidate to the municipal council belonging to the opposition coalition obtained higher support with 27.7% of the vote, while the main candidate from the pro-government coalition got only 18.5%.

central government and pro-government mayors are able to raise more of these funds.

I implement a difference-in-difference regression to study the effects of the 1999 electoral law on my two proxies for the degree to which the public school budgets constraints are binding in the context of a selection model of the form:

$$P_m = \mathbf{1}\left(\varphi V_m K_m + \varpi V_m + \phi K_m + \zeta Z_m + M_m^{'}\varkappa + Y_m^{/}\tau + \mu_m > 0\right), \tag{2}$$

where P_m is an indicator function that takes a value of one if the municipality has an education deficit above the median or an average public school size below the median; V_m is the share of votes that goes to the pro-government coalition; and K_m is a dummy that takes a value of one if the mayor belongs to the pro-government coalition. I exclude the interaction of V_m and K_m from (1) and include each variable separately, as well as the estimated inverse of the Mills-ratio in (1). Panel A of Table 7 presents my marginal probit estimates of equation (2). The results indicate that the interaction variable has a positive and significant effect: the probability that a municipality has an education deficit above the median increases by 50% and average school size below the median increases by about 40% if the mayor belongs to the pro-government coalition for the same shares of votes obtained by the pro-government coalition.

Panel B of Table 7 presents estimated interaction effects. I present estimates including selection correction in the top panel and without including selection correction in the bottom panel. Results in both cases are qualitatively similar. The results for the subsample of students attending public schools that have high education deficits and low average school sizes tend to react by less than the other schools (and only in one case the reaction is positive and statistically significant). Overall, these results show that proxies for the degree to which the public school budget constraints are binding affect the degree of response of public schools to voucher-school competition, supporting the hypothesis stated in this paper and the existence of heterogeneous effects of voucher school competition on public schools related to the incentives given by the degree of softness of the budget constraint. Intuitively, my results imply that a public school operating in a municipality with soft budget constraints react by less –actually the reaction is not different from 0 in most of my estimates–than public schools operating under hard budget constraints.

8 Concluding Comments

The potential effects of school vouchers and inter-school competition on student outcomes has been a much debated topic in the US and elsewhere. My study of the Chilean

voucher system, which has operated for more than 20 years in the complete K-12 system, can help us to understand the effects of vouchers on educational outcomes. Previous research has been stymied by endogeneity problems. I argue that the interaction of the variation in the number of priests per person across Chilean areas in 1950, and the implementation of the voucher system in 1981, allows me to identify the effects of voucher school competition on test scores. I document that the number of Catholic priests *is not* correlated with educational outcomes in the pre-voucher period and *is* correlated with educational outcomes in the post-1981 period. This result allows me to use the number of priests per person in 1950 as an instrument for voucher-school entry during the voucher period.

I find that once I instrument for the ratio of voucher-to-public schools in an area, one additional voucher school per public school increases test scores by about 0.14 standard deviations. The magnitude of this effect on test scores is equivalent to about half of the effect of increasing a mother's attainment from primary to secondary education. These results are roughly similar for students attending public schools.

My estimates of the effects of school competition on test scores are smaller for students attending public schools that face less binding minimum enrollment, measured using two alternative proxies. While agents operating voucher schools receive higher payoffs if they increase enrollment, agents operating public schools receive fixed wages and only have to meet some minimum enrollment constraints. Therefore, agents operating in areas in which the minimum enrollment constraint is less binding react less to voucher school competition. Overall, the evidence is consistent with a theoretical rationale that emphasizes the role of incentives provided by voucher-school competition.

My results do not imply that selection or segregation are not relevant issues in the Chilean case. Rather, controlling for characteristics of students and markets, there are sizeable direct effects of competition on test scores. More than 20% of educational markets in Chile have no voucher school in operation. Similarly, there are heterogeneous effects of voucher school competition for public school students, depending on how soft the budget constraints are. Thus, the introduction of the voucher system does increase educational inequality in Chile. The paradox, though, is that the Chilean system does not become more unequal because of the existence of voucher schools, but rather because of the absence of voucher schools in some areas, and the absence of strong incentives for some public school agents. The government could correct this inequality while preserving school choice by using the right incentives, such as letting per-student subsidies depend upon student characteristics, as proposed by Gonzalez et al. (2002) Hoxby (2001), and

Sapelli (2003) and implemented in Chile since 2009, or by creating explicit incentives that relate the welfare of public school agents to student outcomes.

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Figure 1:
Estimated relationship of priests with high-school graduation rate, by year of birth.

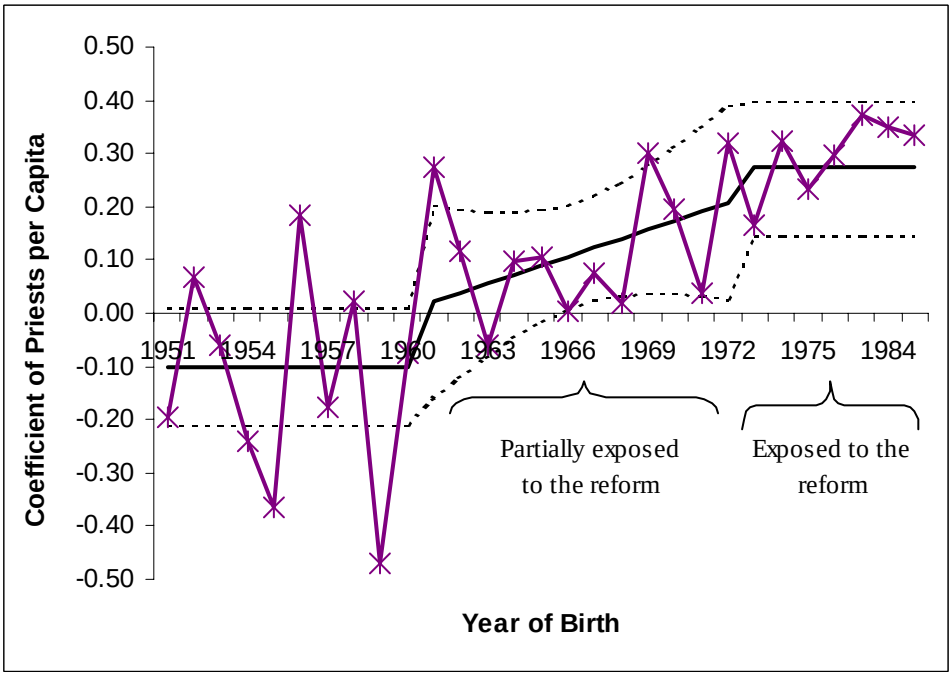

Table 1: Descriptive Statistics

Variable	Obs.	Mean	Median	Standard Deviation	5th percentile	95th percentile	Source
Student-level variables for 2002							
Test scores (standardized)	187610	-0.03	-0.03	0.97	-1.71	1.49	SIMCE
Mother's education							
Primary	187610	0.35	0.00	0.48	0.00	1.00	SIMCE
Secondary-General	187610	0.32	0.00	0.47	0.00	1.00	SIMCE
Secondary-Technical	187610	0.16	0.00	0.37	0.00	1.00	SIMCE
Higher-General	187610	0.08	0.00	0.27	0.00	1.00	SIMCE
Higher-Technical	187610	0.09	0.00	0.28	0.00	1.00	SIMCE
Average Years of Schooling	187610	9.84	11.00	3.31	5.00	15.00	SIMCE
Log(Per-Capita Income)	187610	10.27	10.31	0.90	8.87	11.83	SIMCE
Voucher school	187610	0.43	0.00	0.50	0.00	1.00	SIMCE
Catholic voucher school	187610	0.15	0.00	0.39	0.00	1.00	http://www.iglesia.cl/
Individual-level variables							
High school graduation dummy	75805	0.40	0.00	0.49	0.00	1.00	CASEN
Immigration dummy	8857	0.17	0.00	0.37	0.00	1.00	Social Protection Survey
Market-level variables							
Ratio of Voucher to Public Schools	298	0.47	0.25	0.70	0.00	1.6	Ministry of Education
Mean years of schooling	298	8.82	8.66	1.29	7.21	10.96	SIMCE
Standard Deviation of Years of schooling	298	3.45	3.47	0.47	2.70	12	SIMCE
Mean Log Income	298	10.02	9.94	0.37	9.55	10.71	SIMCE
Standard Deviation of Log Income	298	0.85	0.85	0.14	0.66	1.08	SIMCE
Log School-Age Population	298	8.38	8.36	1.28	6.32	10.7	http://www.sinim.cl/
Urbanization Rate	298	0.34	0.25	0.28	0.00	0.93	Ministry of Education
High-School Graduation Rate							
37<age	304	0.28	0.26	0.14	0.08	0.56	CASEN
25<age<38	304	0.46	0.45	0.17	0.20	0.74	CASEN
age<26	304	0.52	0.53	0.16	0.24	0.79	CASEN
Participation in Organizations	331	0.30	0.28	0.12	0.13	0.53	CASEN
Number of Community Organizations per 1000 people	331	18.7	17	13	4	42	http://www.sinim.cl/
Pro-Government Mayor	331	0.51	0.00	0.50	0.00	1.00	Electoral Office
Pro-Government Vote	331	0.53	0.52	0.15	0.25	0.78	Electoral Office
Diocese-level variables							
Priests per 1000 people in 1950	26	0.32	0.31	0.13	0.15	0.54	Annuario Pontificio
Priests per 1000 people in 2000	26	0.15	0.14	0.04	0.08	0.21	Annuario Pontificio
Catholic Affiliation	26	0.78	0.79	0.08	0.68	0.90	Annuario Pontificio
Ratio of Religious to Total Priests	26	0.45	0.44	0.16	0.17	0.66	Annuario Pontificio
Region x Urban/Rural variables							
High-School Graduation Rate							
39<age	26	0.27	0.27	0.17	0.05	0.5	Social Protection Survey
27<age<40	26	0.47	0.59	0.22	0.17	0.71	Social Protection Survey
age<28	26	0.60	0.65	0.24	0.22	1	Social Protection Survey
High-School Graduation Rate in Public Schools							
39<age	26	0.26	0.27	0.16	0.08	0.5	Social Protection Survey
27<age<40	26	0.45	0.54	0.20	0.17	0.67	Social Protection Survey
age<28	26	0.55	0.62	0.26	0.00	1	Social Protection Survey
Voucher school attendance (% of total population)							
39<age	26	0.05	0.05	0.04	0.00	0.12	Social Protection Survey
27<age<40	26	0.08	0.07	0.06	0.00	0.23	Social Protection Survey
age<28	26	0.11	0.12	0.09	0.00	0.27	Social Protection Survey

Notes: Detailed definitions of each variable appear in the main text

Table 2
Determinants of Priests per Capita in 1950: Religious Order Effects

Dependent Variable:	Log of Priests per 1,000 people in 1950			
	(1)	(2)	(3)	(4)
Log(income)	0.28		0.10	
	(0.27)		(0.26)	
Schooling		0.13		0.06
		(0.09)		(0.09)
Ratio of order to total priests			1.25	1.19
			(0.52)	(0.53)
R^2	0.0462	0.0783	0.2415	0.2494
Number of dioceses	26	26	26	26

Cross section regressions, each observation represents the value for a dioceses.
Robust standard errors in parentheses. Constants are not reported

Table 3
Municipal level regressions for eduactional outcomes *before* and *after* the reform

Dependent Variable:	High-School Graduation Rate	
	(1)	(1)
Log(Priests per 1000 people)	0.01	0.05
	(0.05)	(0.10)
Log(Priests per 1000 people) *	0.08	0.03
Dummy age [26,37]	(0.04)	(0.14)
Log(Priests per 1000 people) *	0.10	0.12
Dummy age<26	(0.04)	(0.13)
Catholic affiliation	-0.06	0.02
	(0.13)	(0.17)
Catholic Affiliation	0.05	0.07
* Dummy age [26,37]	(0.20)	(0.30)
Catholic Affiliation	0.10	-0.01
* Dummy age<26	(0.20)	0.26
R^2	0.61	0.62
Number of observations	900	900
Number of markets	300	300
Region fixed effects	Yes	Yes
Cohort fixed effects	Yes	Yes
Region*Cohort fixed effects	No	Yes

Cross section regressions, each observation represents a value for a cohort living in a market. Estimates for the log of total population and urbanization rate (and their interactions with the cohort dummies) and fixed effects are not reported. Standard errors clustered at the diocese level in parenthesis.

Table 4
Availability of Voucher Schools

Dependent Variable:	Ratio of voucher to public schools		Share of enrollment in voucher schools	Ratio of Catholic to public schools	Ratio of for-profit to public schools	Ratio of non-for profit to public schools	Ratio of for-profit to voucher schools	Ratio of for-profit with religious names to public schools	Ratio of for-profit with non-religious names to public schools
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean of mother's schooling	0.22	0.21	0.04	0.05	0.03	0.06	0.13	-0.11	-0.04
	(0.08)	(0.09)	(0.01)	(0.02)	(0.06)	(0.04)	(0.11)	(0.05)	(0.10)
Standard Deviation of mother's	-0.16	-0.07	0.02	-0.06	0.05	-0.01	-0.05	0.03	-0.10
schooling	(0.23)	(0.33)	(0.04)	(0.04)	(0.08)	(0.04)	(0.03)	(0.08)	(0.06)
Mean of Log Income	0.16	0.20	-0.02	0.00	0.11	0.00	-0.01	0.48	-0.06
	(0.13)	(0.11)	(0.02)	(0.01)	(0.17)	(0.04)	(0.09)	(0.19)	(0.07)
Standard Deviation of Log	-1.20	-1.28	-0.11	-0.15	-0.30	-0.05	0.38	-0.47	0.58
Income	(0.24)	(0.55)	(0.07)	(0.09)	(0.53)	(0.14)	(0.24)	(0.24)	(0.19)
Log(Population)	0.22	0.21	0.05	0.02	0.20	0.04	0.02	0.11	0.00
	(0.08)	(0.09)	(0.01)	(0.02)	(0.05)	(0.02)	(0.03)	(0.04)	(0.03)
Urbanization Rate	0.38	0.30	0.17	0.02	0.02	0.06	0.13	0.01	0.11
	(0.23)	(0.23)	(0.05)	(0.01)	(0.16)	(0.04)	(0.11)	(0.09)	(0.08)
Log(Priests per 1,000 people in	0.50		0.06	0.05	0.08	0.16	-0.11	0.03	0.00
1950)	(0.10)		(0.02)	(0.02)	(0.04)	(0.03)	(0.04)	(0.01)	(0.01)
		1.69							
Ratio of order to total priests		(0.69)							
Share of Catholics in total	1.69	2.99	0.87	0.90	0.91	1.78	-1.35	0.38	-0.73
population	(0.69)	(0.75)	(0.14)	(0.27)	(0.50)	(0.45)	(0.38)	(0.14)	(0.42)
R^2	0.72	0.76	0.71	0.47	0.35	0.47	0.16	0.51	0.13
Number of markets	298	298	298	298	298	298	203	298	298

Cross section regressions using the actual first stages. Clustered standard errors at the diocese level in parenthesis. Only estimates for variables at the market level are reported.

Table 5

Panel A:

Priests and In-Migration Decisions

Dependent Variable:	In-migration Dummy	In-migration Rates
	(1)	(2)
Log(Priests per 1000 people, destination/origin)	0.04	0.03
	(0.10)	(0.13)
Econometric Technique	Marginal Probit	OLS Estimates
Number of observations	8857	13

Standard errors clustered at the region level in parenthesis. Constants are not reported.

Panel B:

Priests and Social Capital

Dependent Variable:	Rate of Participation in Organizations	Community Organizations per 1,000 people
	(1)	(2)
Mean of mother's schooling	-0.01	-8.99
	(0.04)	(5.31)
Standard Deviation of mother's schooling	-0.02	-3.45
	(0.03)	(3.34)
Mean of Log Income	-0.07	-3.22
	(0.03)	(5.67)
Standard Deviation of Log Income	0.19	24.64
	0.07	(18.37)
Log(Population)	-0.03	-3.44
	(0.01)	(0.89)
Urbanization Rate	-0.01	-8.99
	(0.04)	(5.32)
Log(Priests per 1,000 people in 1950)	0.00	-0.15
	(0.01)	(0.80)
Share of Catholics in total population	-0.13	-5.80
	(0.10)	(9.21)
R^2	0.580	0.367
Number of observations	324	329

Standard errors clustered at the region level in parenthesis. Region fixed effects are not reported.

Table 6

Test Scores: OLS and IV Results

Dependent Variable:	Test scores						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Estimation Technique	OLS	IV	IV	IV	IV	IV	IV + Heckman selection model
Sample	All students, 2002	All students, 2002	All students, 2002	All students, 2002	All students, 2006	All students, 2010	Public school students, 2002
Voucher school competition	-0.02	0.15	0.13	0.14	0.09	0.09	0.15
	(0.02)	(0.05)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
	[-0.02]	[0.11]	[0.10]	[0.11]	[0.12]	[0.12]	[0.12]
Mother Education:							
Secondary-General	0.27	0.28	0.28	0.28	0.24	0.22	0.23
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Secondary-Technical	0.40	0.40	0.40	0.40	0.48	0.42	0.34
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Higher-General	0.64	0.64	0.64	0.64	0.69	0.70	0.53
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Higher-Technical	0.49	0.50	0.50	0.50	0.54	0.51	0.40
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Log(Per-Capita Income)	0.23	0.23	0.23	0.23	0.21	0.15	0.16
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Market Level Variables:							
Mean Education	0.02	-0.01	-0.01	-0.01	0.08	0.04	-0.04
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Standard Deviation of Education	0.03	0.05	0.05	0.05	-0.01	0.04	0.04
	(0.04)	(0.07)	(0.07)	(0.07)	(0.11)	(0.05)	(0.05)
Mean Log(Per-Capita Income)	0.18	0.12	0.13	0.13	-0.17	0.00	0.23
	(0.03)	(0.07)	(0.05)	(0.06)	(0.11)	(0.17)	(0.06)
Standard Deviation of Log(Per-Capita Income)	-0.27	-0.02	-0.05	-0.04	0.05	0.14	-0.08
	(0.05)	(0.11)	(0.08)	(0.08)	(0.21)	(0.19)	(0.05)
Log(Population)	-0.03	-0.06	-0.06	-0.06	-0.06	-0.09	-0.05
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)
Urbanization Rate	0.00	-0.08	-0.07	-0.07	-0.19	-0.13	-0.22
	(0.09)	(0.14)	(0.12)	(0.13)	(0.09)	0.12	(0.12)
Share of Catholic Population	-0.20	-0.48	-0.44	-0.46	-0.25	-0.06	-0.58
	(0.17)	(0.26)	(0.21)	(0.22)	(0.11)	(0.30)	(0.23)
Instrumental Variable:	-	Priests	Order	Priests, Order	Priests, Order	Priests, Order	Priests
Over-identification test (p-value)	-	-	-	0.83	0.99	0.43	-
Number of students	187610	187610	187610	187610	230476	231457	106094
Number of schools	5433	5433	5433	5433	6870	7167	3355
Number of markets	298	298	298	298	305	307	298

Cross section regressions. Standard errors clustered at the diocese level in parenthesis and standardized effects of voucher-school competition in brackets. Region dummies and constants are not reported.

Table 7

Panel A:
Proxies for Soft Budget Constraints in Public Schools: Marginal Probit Estimates

Dependent Variable	Dummy takes a value of 1 if municipality has Education Deficit above the Median
	(1)
Pro-Government Mayor*Pro-Government Vote	0.50
	(0.26)
Pro-Government Mayor	-0.07
	(0.14)
Pro-Government Vote	-0.51
	(0.17)
Mean of Mother Education	-0.14
	(0.03)
Standard Deviation of Mother Education	-0.30
	(0.08)
Mean of Log(Per-Capita Income)	0.25
	(0.07)
Standard Deviation of Log(Per-Capita Income)	-0.37
	(0.21)
Log(Population)	0.06
	(0.02)
Urbanization Rate	0.30
	(0.10)
Log of (Priests per 1,000 people)	-0.13
	(0.05)
Share of Catholic Population	-0.05
	(0.27)
Pseudo R^2	0.17
Number of municipalities	299

Cross section regressions, each observation represents a value a for a municipality, estimates from the selection equation actually used in the paper.

Panel B
Test Scores: IV Estimates, Interaction Effects

	(1)	(2)	(3)	(4)
	Second stage estimates with selection correction			
Ratio of voucher to public schools	0.09	0.52	0.03	0.18
	(0.07)	(0.31)	(0.20)	(0.06)
	Second stage estimates without selection correction			
Ratio of voucher to public schools	0.08	0.52	0.06	0.17
	(0.04)	(0.21)	(0.20)	(0.03)
Sample: Municipalities with	Big education deficits	Small education deficits	Low school size	High school size
Number of students	46819	59275	18234	87860
Number of schools	1309	2046	885	2470
Number of municipalities	108	191	104	194

Cross section regressions, each observation represents a value a for a student. Standard errors clustered at the diocese level in parenthesis. Coefficients of all other control variables included in the second and first stage equations are not reported.

Appendix Table 1
Choice of Schools: Marginal Probit Estimates

Dependent Variable: Dummy takes a value of 1 if Student attends a public school versus a voucher school	
	(1)
Values among top priorities when choosing among schools	-0.36
	(0.01)
Mother Education:	
Secondary-General	-0.07
	(0.01)
Secondary-Technical	-0.10
	(0.01)
Higher-General	-0.21
	(0.02)
Higher-Technical	-0.19
	(0.02)
Log(Per-Capita Income)	-0.11
	(0.01)
Market Level Variables:	
Mean of Mother Education	-0.02
	(0.02)
Standard Deviation of Mother Education	-0.09
	(0.06)
Mean of Log(Per-Capita Income)	-0.11
	(0.01)
Standard Deviation of Log(Per-Capita Income)	0.00
	(0.10)
Log(Population)	-0.06
	(0.01)
Urbanization Rate	-0.14
	(0.10)
Log of (Priests per 1,000 people)	-0.07
	(0.02)
Share of Catholic Population	-1.07
	(0.15)
Pseudo R^2	0.22
Number of students	172309
Number of schools	5433
Number of markets	285

Cross section regressions, each observation represents a value a for a student. Standard errors clustered at the diocese level in parenthesis. Region dummies and constants are not reported.