DEPARTMENT OF ECONOMICS

WORKING PAPER SERIES

2010-01

McMASTER UNIVERSITY

Department of Economics
Kenneth Taylor Hall 426
1280 Main Street West
Hamilton, Ontario, Canada
L8S 4M4

http://www.mcmaster.ca/economics/

School Competition and Efficiency with
Publicly Funded Catholic Schools

David Card — UC Berkeley
Martin Dooley — McMaster University
Abigail Payne — McMaster University

January 2010

ABSTRACT

The province of Ontario has two publicly funded school systems: secular schools (known as public schools) that are open to all students, and separate schools that are limited to children with Catholic backgrounds. A simple model of inter-system competition predicts that incentives for effort are higher in areas where there are more Catholic families who are relatively uncommitted to one system or the other. We measure the willingness of Catholic families to switch systems by studying the effect of school openings on enrollment at nearby schools in the competing system. The results suggest that families in rapidly growing areas have the weakest attachment to a particular system. We then relate student test score gains between 3rd and 6th grade to measures of potential cross-system competition. We find that competition for Catholic students has a significant effect on test outcomes in both systems, particularly in fast-growing areas. Our estimates imply that expanding competition to all students would raise average test scores in 6th grade by 6-8% of a standard deviation.

* We thank the editor and two referees for helpful comments and suggestions. We also thank Joseph Altonji, Todd Elder, David Green, Philip Haile, Steven Jones, Patrick Kline, Patrick McEwen, Derek Neal, Craig Riddell, Arthur Sweetman, and many other seminar participants for comments on earlier drafts. We gratefully acknowledge the efforts of Olesya Kotlyachkov and the research assistants in the Public Economics Data Analysis Laboratory. This research has been funded from grants from the Canadian Foundation for Innovation, the Ontario Innovation Trust, the Social Sciences and Humanities Research Council, the Council of Ministers of Education, and McMaster University.

Rising costs and growing concerns over student achievement have led to renewed interest in the design of publicly funded education programs.[1] Building on the insights of Friedman (1955), many analysts have suggested a voucher system that limits the monopoly power of local school districts. For a consumer-driven system to work, families must be willing and able to choose between schools to access higher quality education, and school administrators must be rewarded for attracting additional students. So far the choice initiatives enacted in the U.S. have gone only part way toward satisfying these conditions.[2]

Alternatives to the local monopoly model of public school provision have a much longer history outside the U.S.[3] In this paper we study the effects of competition among elementary schools in Ontario Canada, which for over a century has operated two parallel, publicly funded school systems: secular schools (known as *public schools*) that are open to all students, and *separate schools* that are open to children with Catholic backgrounds.[4] The two systems are run independently but follow a standardized curriculum and receive equal funding per student.

For non-Catholics, the Ontario system functions like a typical public system in the U.S. with a single monopoly provider. For the 40% of children with Catholic backgrounds, however, the system is effectively a voucher program with two competing suppliers. Although choice is limited to Catholics, the financial incentives to compete for Catholic students potentially impact the quality of schooling for *all students*. Our goal is to measure the effects of these incentives using standardized student test score gains between 3rd and 6th grade.

---

[1] See Hanushek (2003) for an overview of trends in school spending and achievement in various countries.
[2] Two main initiatives to loosen the control of local school districts are charter schools and school vouchers. See Hoxby (2004), Booker et al. (2005) Bifulco and Ladd (2004) and Carnoy et al (2005) on the issue of charter schools, and Howell and Petersen (2002), Krueger and Zhu (2003) and Ladd (2002) on the issue of vouchers.
[3] Clark (2009) studies the efficiency of English high schools that opt out of local school district control, while Gibbons, Machin, and Silva (2008) analyze competition between public schools in England. Ahlin (2003) studies the effects of competition in Sweden. Hsieh and Urquiola (2006) study the voucher system in Chile.
[4] More precisely, we study the incentives for administrative effort and efficiency when students can choose between school systems. We follow the literature in referring to this as "school competition."

We frame our analysis using a simple model of school choice and effort competition between public and separate schools. We assume that Catholic families choose between systems depending on a combination of tastes, proximity, and the quality of instruction. We also assume that administrators of the competing systems are rewarded for larger market shares. Under these conditions, equilibrium effort – and student achievement in both systems – will be higher in areas with a larger share of Catholic families that are relatively uncommitted to a particular system, and are therefore more responsive to quality differentials between the two systems.

To identify the characteristics that are correlated with the willingness of Catholics to switch systems we study the impact of school openings on enrollment trends at nearby schools. When Catholic parents are highly committed to one system or the other the opening of a new school will have no effect on enrollment at nearby schools *in the competing system*. When they are relatively uncommitted, however, some families will switch systems. We find significant cross-system flows following new school openings, with a magnitude that is proportional to the local fraction of Catholics and is bigger in fast-growing areas, where families appear to have weaker ties to a particular system.

Building on these findings we go on to test the prediction that test score gains between 3rd and 6th grade are larger for students in both systems in more competitive markets (i.e., fast-growing neighborhoods with a higher fraction of Catholics). We specify an econometric model for individual test score gains that includes student, school, and neighborhood characteristics, as well as measures of potential competitive pressure based on the local fraction of Catholics and the neighborhood growth rate. The key identifying assumption is that variation in the local fraction of Catholics has no direct effect on average test score gains. We use two auxiliary data sets to investigate differences between Catholics and non-Catholics. We find that Catholic and

Protestant parents and children have very similar characteristics, although both differ from other religious groups (in particular, people with no religious affiliation). Thus, our preferred specifications isolate the effect of variation in the local fraction of Catholics, holding constant the total fraction of Catholics and Protestants in an area.

We find statistically significant impacts of the market characteristics associated with greater potential competition on the growth rate of achievement between 3rd and 6th grades. Both the fraction of Catholics and its interaction with a measure of population growth are associated with faster student test score gains. We verify that these effects extend to students in both systems, and that they are robust to controls for potential selection biases. Our estimates imply that expanding choice to all students in the province would have a modest effect on 6th grade test scores, raising achievement in 6th grade by 6-8% of a standard deviation.

I. Previous Research

Our research builds on several existing strands of research on public school performance. One set of studies addresses the effect of private school competition on public school achievement. Couch et al. (1993) related public-school test scores to the enrollment rate in private schools, and interpreted the positive correlation as a competition effect.[5] Hoxby (1994) noted the potential endogeneity bias in this specification and used the local fraction of Catholics as an instrument for the private enrollment rate. Subsequent studies using a similar approach (e.g., Arum, 1996 and Jepsen, 2003) have found weaker effects, and an extensive re-analysis by Jepsen (2002) concludes that the impact of private school competition in the U.S. is probably small. Our research design is similar and is intended to identify the *equilibrium effect* of potential competition. Importantly, however, we measure the effect on students <u>in both systems</u>,

[5] Subsequent studies using the same approach (Newmark, 1995; Sander, 1999; Geller et al. 2006) have found generally insignificant effects. An exception is Dee (1998), who estimates a positive effect on graduation rates.

avoiding selectivity biases that would result if only the scores of students who remain in the public schools are considered.

A second group of studies, beginning with Borland and Howsen (1992), examines Tiebout competition between public school districts in the same area.[6] Although early studies tended to find small (or even a wrong signed) effects, Hoxby (2000) used the number of rivers and streams running through a metropolitan area as an instrument for the Herfindahl index of district enrollment shares, and obtained a positive competition effect. Rothstein (2007) re-analyzed these results and found smaller and generally insignificant effects. In a related study Rothstein (2006) analyzed the effect of district fragmentation on sorting between districts, and concluded that Tiebout competition effects are probably modest.

Direct evidence on voucher-based competition comes from a study of Chilean schools by Hsieh and Urquiola (2006). Using comparisons across municipalities they found no significant relationship between private school entry rates and district-wide gains in student achievement. Consistent with theoretical analyses by Epple and Romano (1998) and Nechyba (2000), however, they find that the introduction of vouchers led to an increase in the stratification of SES-groups across schools.

A third literature, starting with Coleman, Hoffer and Kilgore (1982), compares the test scores of students at public and private schools. Cain and Goldberger (1982) highlighted the potential selectivity biases in this comparison, and subsequent studies have used Catholic religion, distance to a Catholic school, and/or their interaction as instruments for private school choice (e.g., Evans and Schwab, 1995; Neal, 1997; Grogger and Neal, 2000; Figlio and Ludwig, 2000). Recently, Altonji, Elder, and Taber (2005b) have shown that all three instruments are

[6] In an interesting district-level analysis, Millimet and Rangaprasad (2007) test for strategic interactions between the input choices of nearby school districts in Illinois, and report positive and generally significant effects of nearby competitors' choices on a district's choices over pupil/teacher ratios, spending per pupil, and average school size.

correlated with the graduation rates of Catholic students who attend public schools in 8th grade, calling into question the validity of the IV designs.

Like these studies our identification strategy relies on comparisons between areas with different fractions of Catholic families, and could lead to biased inferences if the fraction of Catholics exerts an effect on test scores gains independent of any competition effect. To address the concerns raised by Altonji et al.'s findings, we focus on comparisons that vary the local fraction of Catholics, holding constant the combined fraction of Catholics and Protestants in an area. We show that in the U.S. sample used by Altonji et al., Catholics and Protestants have very similar test scores at 8th grade, and similar test score gains between 8th and 10th grade, although both groups have better outcomes than children with no religious affiliation. Likewise, Catholic and Protestant parents in Ontario have very similar earnings and education (though again both groups have better outcomes than parents with no affiliation). Both findings are consistent with our maintained assumption that variation in the fraction of Catholics, while holding constant the fraction of Protestants and Catholics, has no direct effect on average student achievement.

II. Institutional Detail and Conceptual Framework

a. Institutional Background

Ontario has operated two publicly-funded school systems since 1841. Originally both systems were financed by local property taxes, with ratepayers choosing which system received their property tax payments. An equalization program was introduced in the 1930s, and since 1985 the province has provided (roughly) equal per student funding for the two systems.[7] Public schools are secular and are required to accept all students, whereas separate schools limit

[7] Since 1998 the system has provided full provincial funding on a capitation basis.

enrollment to children of Catholic backgrounds.[8] The schools are administered by local school "boards" (equivalent to U.S. school districts), with independent boards for the public system and the separate system. As of 2003, there were 31 public school boards in the province (with an average enrollment of 44,000 students) and 29 separate school boards (with an average enrollment of 18,000).[9] There were also 23 "school authorities" that operated schools in remote rural areas, and a handful of French-language school boards.[10]

Elementary teachers in the two systems are represented by separate unions (with board-specific collective agreements) but salaries and benefits are very similar across the province. Both systems also follow the same standardized curriculum, and administer the same set of standardized tests in mathematics, reading, and writing. School construction costs for both systems are financed by a provincial grant program that is designed to equalize the ratio of enrollment to capacity across boards.

Public and separate schools are readily available in all neighborhoods throughout the province. For example, Figure 1 shows the distribution of elementary schools in the cities and towns on the western edge of Toronto, superimposed over a graph of postal-code-based neighborhoods, known as Forward Sortation Areas, or FSA's. A typical FSA in the area depicted has 8-10 elementary schools with roughly 2 public schools per separate school. In both systems children are assigned to schools using attendance zones. Thus, a given residential address falls into the attendance zone of <u>one</u> public elementary school and <u>one</u> separate

---

[8] Our understanding is that separate elementary schools strictly enforce this restriction.

[9] The boundaries of the public and separate boards typically coincide in more heavily populated areas.

[10] About 4% of students attend the schools operated by French language boards. Another 5% attend private schools (which are mainly Protestant religious schools). We do not model the outcomes of students in private and French language schools, but we report models that include selection correction terms to control for the possible non-random selection into publicly funded English language schools.

elementary school.[11] Every Catholic family therefore has two basic choices for publicly-funded education: their designated public school, and their designated separate school. Non-Catholic families have only one basic choice. In most areas, the boards also offer French immersion schools, though enrollment in these programs is small.[12] Finally, some boards allow students to attend schools other than their designated school, subject to space availability and other criteria. Participation in these "optional attendance" programs is very low.[13]

b. A Simple Model of Enrollment Demand, Managerial Effort, and School Quality

In this section we present a simplified model of enrollment demand and effort determination in a stylized model with two competing schools: a public school open to all students, and a separate school open to Catholics. We show how changes in the fraction of Catholics, and in the degree to which these families are committed to one choice or the other, would be expected to affect the equilibrium efficiency of both schools. Although the model ignores other avenues of choice (in particular, the option of attending a private school) we believe it captures the salient features of the Ontario system. We extend the model to the situation where there are multiple schools operated by each board in Appendix 1.

Consider an area with n_1 non-Catholic families and n_2 Catholic families, each with one school-age child. There are two available schools: a public school with quality Q_p and a separate school with quality Q_s. Catholic family i associates values U_{ip} and U_{is} to the choices, where

(1a) $$U_{ip} = \alpha_{ip} + \beta Q_p - \gamma t_{ip} + \varepsilon_{ip}$$

[11] Similarly, every address is allocated to one public high school and one separate high school. The attendance zones of the competing systems typically do not coincide.
[12] French immersion programs are offered by the English language boards and are targeted at children whose parents are not French speaking. In a typical program students take some of their classes in French and others in English, depending on their grade. Fewer than 10% of public elementary students and under 5% of separate elementary students are enrolled in French immersion. We include these students in our test score analysis below.
[13] For example, Halton District School Board (serving cities and towns on the western edge of Toronto) reported a total of 177 students in their optional enrollment program in the 2007-08, out of a total enrollment of about 35,000 elementary students. See Halton District School Board (2009).

(1b) $U_{is} = \alpha_{is} + \beta Q_s - \gamma t_{is} + \varepsilon_{is}$.

Here, α_{ip} and α_{is} represent random taste components, t_{ip} and t_{is} represent travel costs, and $(\varepsilon_{ip},\varepsilon_{is})$ are i.i.d. random shocks.[14] Conditional on $(\alpha_{ip},\alpha_{is},Q_p,Q_s,t_{ip},t_{is})$, the probability that family i selects the public school is $F[\delta_i + \beta\Delta Q - \gamma\Delta t_i]$, where F is the distribution function of the random variable $v_i \equiv \varepsilon_{ip} - \varepsilon_{is}$, $\delta_i \equiv \alpha_{ip} - \alpha_{is}$ represents the family's relative taste for public schools, ΔQ is the quality gap between the schools, and Δt_i is the difference in travel costs to the two schools.

Assume that the area is divided into a set of neighborhoods k=1, 2,…K, and that for all homes in neighborhood k the travel cost differential is Δt_k. The share of Catholic families in k who choose the public school is

(2) $s_k(\Delta Q, \Delta t_k) = \int F[\delta_i + \beta\Delta Q - \gamma\Delta t_k]\, h(\delta_i|k)\, d\delta_i$,

where $h(\delta_i|k)$ is the density of relative tastes among Catholic families in the neighborhood.[15] Letting n_{2k} represent the number of Catholic families in neighborhood k, the total fraction of Catholics who choose the public school is

$$s(\Delta Q) \equiv \sum_k n_{2k}/n_2 \times s_k(\Delta Q, \Delta t_k) .$$

Total public and separate school enrollments are

(3a) $E_p = n_1 + n_2\, s(\Delta Q)$,

(3b) $E_s = n_2\, [1 - s(\Delta Q)]$.

Using equation (2), the responsiveness of enrollment to the quality gap between schools is:

(4) $\partial E_p/\partial \Delta Q = - \partial E_s/\partial \Delta Q = n_2\, s'(\Delta Q)$,

where

$$s'(\Delta Q) = \sum_k n_{2k}/n_2 \times \beta \int f[\delta_i + \beta\Delta Q - \gamma\Delta t_k]\, h(\delta_i|k)\, d\delta_i ,$$

[14] A similar random utility formulation is widely used in models of demand for differentiated products. See Nevo (2000) for discussion and references, and Hastings, Kane and Staiger (2006) for an application to school choice.

[15] As an example, suppose δ_i is normally distributed with mean δ_k and variance σ^2 in neighborhood k. Then $s_k = \int \sigma^{-1} F[\sigma \cdot z+\delta_k+\beta\Delta Q-\gamma\Delta t_k]\varphi(z)dz$ where $\varphi(z)$ is the standard normal density.

and f [ ] is the density function associated with F [ ].

Assume that school quality is an increasing concave function of the level of effort (e) exerted by school managers:

$Q_\ell = q(e_\ell)$, for $\ell = \{s,p\}$,

and that preferences of each managerial team are represented by a linear function of effort and the *share* of local students attending their school:

$U_\ell(E_\ell, e_\ell) = \theta E_\ell/n - e_\ell$,

where $\theta>0$ reflects the relative weight on market share.[16] The first order conditions for optimal effort are:

(5a) $\theta\, (n_2/n)\, s'(\Delta Q)\, q'(e_p) - 1 = 0$

(5b) $\theta\, (n_2/n)\, s'(\Delta Q)\, q'(e_s) - 1 = 0$.

In equilibrium both teams supply the same level of effort e^*, with

(6) $q'(e^*) = 1 / [\, \theta\, (n_2/n)\, s'(0)\,]$.

Since $q'(e)$ is decreasing in e, equilibrium effort is an increasing function of $\theta\, (n_2/n)s'(0)$, which depends on the strength of incentives faced by school managers (θ), the local fraction of Catholics (n_2/n), and the willingness of Catholic families to consider switching systems to access higher quality schools (i.e., the magnitude of s').

c. Assessing the Sensitivity of Enrollment Demand to Relative Quality

We do not directly observe responses of enrollment to variation in the relative quality of the public and separate schools. Under the assumptions of standard characteristics-based choice model, however, it is possible to identify the features of the local population that lead to

[16] Equivalently one could assume that managers are rewarded for the number of students they attract. Friedman's (1955) original voucher proposal was predicated on the idea that managers receive a payoff proportional to the number of students they attract.

enhanced (or reduced) sensitivity to quality by studying how the same characteristics affect the reaction to changes in the proximity of different choices. The reason is that local demand responses to variation in quality and travel distance are both governed by heterogeneity in tastes for a religious versus secular education.[17]

To illustrate, suppose that a second separate school is opened with the same quality as the existing separate school. The effect on demand for enrollment at the public school is:

$$\partial E_p/\partial Open_s = n_2 \sum_k (n_{2k}/n_2) \ \partial s_k(\Delta Q, \Delta t_k)/\partial \Delta t_k \times \partial \Delta t_k/\partial Open_s$$

where

$$\partial s_k(\Delta Q, \Delta t_k)/\partial \Delta Q = \beta \int f [\delta_i + \beta \Delta Q - \gamma \Delta t_k] h(\delta_i|k) d\delta_i$$

and $\partial \Delta t_k/\partial Open_s$ represents the relative change in travel costs in neighborhood k to attend a public versus separate school.[18] In neighborhoods that are closer to the new separate school than the old one this expression is negative, leading some families to switch systems. From equation (2), however, the responses to changes in travel costs and quality are proportional:

(7) $$\partial s_k(\Delta Q, \Delta t_k)/\partial \Delta t_k = - \gamma/\beta \ \partial s_k(\Delta Q, \Delta t_k)/\partial \Delta Q \ .$$

The enrollment loss when a nearby school is opened can be expressed as a weighted sum of the derivatives of the market shares in each neighborhood with respect to quality:

(8) $$\partial E_p/\partial Open_s = - \gamma/\beta \ n_2 \sum_k (n_{2k}/n_2) \ \partial s_k(\Delta Q, \Delta t_k)/\partial \Delta Q \times \partial \Delta t_k/\partial Open_s \ ,$$

where the weight, $\partial \Delta t_k/\partial Open_s$, depends on the change in relative travel costs experienced by families in each neighborhood.

Assuming that the travel cost changes are a function of local geography and do not covary with the distribution of tastes, we can use observed changes in enrollment following

---

[17] A similar idea is used by Bucklin, Russell, and Srinivasan (1998) to show that the cross-price elasticity of market shares for competing brands is proportional to the probabilities of switching between brands.

[18] For simplicity we are assuming that the changes in travel times $d\Delta t_k$ are small enough that the change in $\int F[\delta_i+\beta\Delta Q-\gamma\Delta t_k]h(\delta_i|k)d\delta_i$ can be approximated by $-\gamma d\Delta t_k \int f[\delta_i+\beta\Delta Q-\gamma\Delta t_k]h(\delta_i|k)d\delta_i = -\gamma d\Delta t_k \ \partial s_k/\partial \Delta t_k$.

nearby openings by the competing system to infer the relative sensitivity of enrollment demand to quality. In particular, the same characteristics that are associated with bigger enrollment losses when a competing school is opened nearby would be expected to generate more elastic enrollment demand with respect to quality, leading to more intensive effort competition.

d. Extension and Implementation

Contrary to our simplified model, the public and separate systems operate multiple schools. Both systems, however, use attendance zones to assign addresses to specific schools. Thus, each family's choice problem is the same as in the two-school case. In particular, each Catholic family always faces two choices (their designated public and separate schools) while each non-Catholic family has only one choice (their designated public school). The effort-setting game is more complicated because each school manager competes with multiple managers in the opposing system (depending on the overlap of the attendance zones in the competing systems). Nevertheless, as we show in Appendix 1, the equilibrium has the same qualitative properties as in the two-school case. In particular, the incentives for effort depend on the local fraction of Catholic students, and on the degree of taste heterogeneity among Catholic families for a secular versus Catholic education. These same characteristics also affect the magnitude of the enrollment losses experienced by an existing school when the competing system opens a new school nearby. We use this insight to develop measures of the cross-system competitive pressure in different geographic areas.

III. Enrollment Responses to Nearby Openings and Closings

In this section we use detailed enrollment data for public and separate elementary schools to study the effects of nearby openings and closings on cross-system enrollment shifts. Our goal is to identify the demographic characteristics of residents that are associated with more or less flexibility in the system preferences of Catholic families. We then use these results in Section IV to specify an empirical model of cross-system competition effects on test scores.

a. Identification of Openings, Closings, and Affected Schools

We obtained information on annual enrollments by grade at all publicly-funded schools in Ontario for the period after 1990. We used these data to identify elementary schools that opened or closed over the period from 1990 to 2004.[19] (Details are presented in Appendix 2). Table 1 summarizes the set of 421 elementary school openings and 314 closings that occurred over these 15 years.[20] As shown in the second column of the table, closings were particularly likely in the second half of the sample period, reflecting a consolidation of school boards that took place in 1998. Geographically, openings are concentrated in the growing cities and suburbs around Toronto, while closings were concentrated in outlying rural areas and in inner-city Toronto. We also matched opening and closing schools to Census data tabulated at the FSA level to compare areas with openings and closings. Not surprisingly, openings tend to occur in areas with a high fraction of newly-built houses, while closings are more likely in slow-growth areas.[21]

Focusing on non-rural areas, we then identified nearby schools that were potentially affected by an opening or closing event. We began by identifying schools within a circle of radius equal to the average travel distance from home to school for students at local elementary

---

[19] In brief, we define the opening year for a school as the first year with positive enrollment in grades 2-4, and the closing year as the last year with positive enrollment in these grades. For administrative purposes schools are sometimes paired: we do not count these events as openings or closings.

[20] The sample includes all schools operated by English language school boards. During the 1990s there were roughly 2300 public elementary schools and 1100 separate schools in operation in a year.

[21] See Appendix Table 1. There is also some variation in income and family structure. Neighborhoods with only openings have the highest average income and lowest fraction of single parents, whereas those with only closings have the lowest family incomes and the most single parents.

schools (typically around 1-2 kilometers).[22] We then used satellite images and maps to eliminate potentially affected schools that were separated from the newly opened or closed school by a major travel barrier (see Appendix 3). We have checked the sensitivity of our results to the inclusion of these "rejected" schools and find similar (though typically weaker) evidence of cross-system enrollment flows when they are included.[23] The mean distance from opened schools to affected schools is about 1.2 kilometers, while the mean distance from closed schools to affected schools is about 1.1 kilometers.

The third column in Table 1 shows the fractions of opening and closing events in non-rural areas for which we were able to identify at least one affected school. This ranges from 45 to 75 percent. Column 4 shows average total enrollment (for grades 1-6) at the opened and closed schools that have a nearby affected school. Newly opened schools have about 300 students (roughly 2 classes per grade), while the closing schools are a little smaller.

b. Enrollment Effects of Nearby Openings and Closings

We identified a total of 945 non-rural elementary schools that were in operation for at least two years and were affected by a nearby opening or closing event between 1990 and 2004.[24] Using this sample we estimate enrollment models of the form:

$$(9) \quad \Delta E_{sat} = X_{sat}b + \sum_{j=1}^{4} Event_{jst} \times \{ D_s d_{j-pub} + (1-D_s) d_{j-sep} \} + \alpha_s + \omega_t + e_{sat} ,$$

where ΔE_{sat} is a measure of enrollment growth at affected school s in area a in year t; X_{sat} is a vector of time-varying school and area characteristics; $Event_{jst}$ is a set of dummies for recent

[22] We obtained information for one year on the postal codes of all students attending each elementary school in the province. We use the centroids of the postal codes for the schools and the homes to compute local travel distances.
[23] We also created a data set that included only the schools near an opening or closing that are excluded from the main analysis sample. The estimated effects of openings and closings on these "excluded" schools are small and statistically insignificant.
[24] A cross-classification of affected schools by the number of "affecting events" (i.e. nearby openings and closings) is presented in Appendix Table 2. Two-thirds were affected by only one event, and another quarter was affected by exactly two events. Only about 10% were affected by three or more opening/closing events.

opening/closing events at nearby schools, D_s is an indicator equal to 1 if school s is public, α_s and ω_t are school and period fixed effects, and e_{sat} is an error term. We consider four types of events -- public openings, separate openings, public closings, and separate closings -- and allow separate coefficients (d_{j-pub} and d_{j-sep}) for the effect of each event type on nearby public and separate schools. Note that since the dependent variable is a growth rate, and the models include school fixed effects, these coefficients measure deviations of enrollment from a school-specific trend in the period following nearby opening or closing events.

Our primary dependent variable is the percentage change in enrollment from grades 1-5 in the previous year to grades 2-6 in the current year.[25] As a check we also use the change in grade 1 enrollment from the previous year to the current year. This is somewhat noisier than the change in continuing enrollment, reflecting fluctuations in the size of the grade 1 entry cohort. To the extent that parents make a "once and for all" selection when their children first enter school, however, the growth in first grade enrollment is a better indicator of sensitivity to changes in the presence of other local schools.

Table 2 presents summary statistics for these two enrollment growth measures. A typical public school in the sample has about 50 students per grade, while a typical separate school is a little smaller (45 students per grade). Between consecutive years grade 1 enrollment rises at an average rate of around 2%, with substantial variation across schools (the standard deviation is 26% for public schools and 31% for separate schools). Average enrollment growth from grades 1-5 to grades 2-6 is a little smaller (under 1%) and less variable.

[25] Our school level database includes enrollment by grade for each school, as well as the enrollment of ungraded students in special education and other programs. We allocated these students uniformly across all grades offered by the school. For schools that do not offer all grades from 1 to 6, we modify the enrollment measure to reflect only those grades for which the school consistently has enrollment.

The control variables in equation (9) are mean FSA characteristics interpolated from the 1991, 1996, 2001, and 2006 Censuses.[26] These include the shares of the population aged 5-9 and 10-14 in the neighborhood, the share of recently constructed houses, and various demographic characteristics, including the fraction of Catholics. In addition, we use our enrollment database to estimate the local fractions of children enrolled in public Francophone schools and private schools.

An issue for the specification of equation (9) is the duration of the effects of openings and closings on enrollment trends at nearby schools. We found that the cross-system effects emerge with some lag and persist for at least 3 years, whereas the effects on schools in the same system occur quickly (as might be expected if attendance zones are adjusted immediately after openings and closings). We therefore allow the opening and closing events to affect enrollment at affected schools in each of the 3 following years, with no impact before and after. Our findings are very similar if we allow 4 or 5 year impacts.

Table 3 shows the estimated effects of school openings on enrollment changes at nearby schools in the same and in the opposing system from a number of different variants of equation (9). All the models also include effects for nearby closings. As discussed in more detail below, however, the cross-system effects of closings are never large, and in the interest of space we report the closing effect coefficients in Appendix Table 3. Columns 1 and 2 present basic models that assume a constant effect of nearby openings and closings on the change in grade 1 enrollment (column 1) or the change in continuing enrollment between grades 1-5 and 2-6 (column 2). The estimates in column 1 suggest that nearby openings cause losses in first grade enrollment of 8-9% per year at nearby schools in the same system in each of the three years after

[26] In assigning schools to FSA's we use 1996 boundaries – see Appendix 2 for more details. We link FSA's across Census years using information provided by Statistics Canada. In cases where FSA-level data are unavailable in a year we use values from the closest available year. Summary statistics for all variables are in Appendix Table 1.

the event (i.e., a cumulative impact of about 25%). More interesting from our perspective are the cross-system effects, which are also negative, but about one-third as large (i.e., losses of 2-3% per year). The cross-system effects on continuing-cohort enrollment changes are a little smaller, potentially reflecting the "stickiness" of system choice once students have started at a particular school.

Although not reported in the table, the estimated closing effects from the models in columns 1 and 2 show enrollment gains of 13-20% per year for the next three years following the closing of a nearby school in the same system. Unlike openings, however, closings seem to have no systematic cross-system effects. One explanation for the difference is that parents have a preference for new schools. In this case, we would expect to see larger enrollment losses following a nearby opening than the gains following a nearby closing. To test this explanation we estimated the cross-system responses to openings, allowing different impacts at newer affected schools (those under 5 years old) and older schools. Contrary to our initial expectations the estimates showed somewhat larger losses at relatively new schools.

An alternative explanation is that cross-system flows are driven by the decisions of newly arriving (or recently arrived) Catholic families who are weakly connected to existing schools. Since openings tend to occur in fast-growing areas with a higher fraction of new residents, while closings occur in slow-growing areas, the cross-system response to nearby openings will be relatively larger than the response to closings. This hypothesis also explains the tendency for bigger cross-system enrollment losses at newer schools, since these tend to be located in fast-growing areas.

To test this explanation we fit a model that allows the effect of nearby openings to vary with a simple proxy for the newness of the neighborhood − the growth rate of the housing stock over the 1990s, as measured by the fraction of units in 2001 that were built after 1991.[27] The results are presented in column 3 of Table 3. The interaction coefficients show that nearby openings have larger (i.e., more negative) cross-system effects in newer neighborhoods.[28] Interestingly, the implied responses in slow-growth areas are essentially zero – comparable to the cross-system enrollment flows following school closings.

Since only Catholic students can switch between school systems, the cross-system enrollment losses from nearby openings should vary proportionally with the fraction of Catholics in the local area.[29] To test this we include interactions of the cross-system opening indicators with the fraction of Catholics in the FSA in which the school is located. Results for four versions of this specification are reported in columns 4-7 of Table 3. The specification in column 4 allows the cross system effect of a recent opening to depend on a dummy and an interaction with the local fraction of Catholics. Judging by the sampling errors we do not have enough power to separately identify the two effects, so we fit the restricted model in column 5 that excludes the indicator, effectively imposing the assumption that cross-system enrollment responses are proportional to the fraction of Catholics in the area. The estimated coefficients suggest that in very-high Catholic areas, the cross-system reactions to nearby openings are relatively large – roughly one-half the magnitude of the own-system responses.

The model in column 6 of Table 3 allows the cross effects to vary with both the local Catholic share and the growth rate of the local housing stock. Echoing the results in column 3, the triple interaction terms (in rows 8 and 12) are large in magnitude and statistically significant,

[27] We have also used an alternative based on local population growth. This yields qualitatively similar but somewhat less precise estimates for the models presented below.
[28] The mean of the new housing share variable is 0.17, with a range between 0.01 and 0.98, and a standard deviation of 0.18. The new housing share in neighborhoods with school openings is about 5 times bigger than in neighborhoods with school closings.
[29] Strict proportionality will only hold if the distribution of preferences does not vary with the fraction of Catholics.

whereas the two-way interactions of the Catholic share with the opening indicator (in rows 7 and 11) are small. Our final specification in column 7 therefore includes only the triple interaction Opening×Catholic Share×New Housing Share. This model fits as well as the preceding one, and suggests that schools in neighborhoods with more Catholic families and a larger fraction of new homes face a greater threat of enrollment losses to the competing system.

IV. Impacts of Competition on Student Achievement

In this section we turn to the primary focus of our empirical analysis, which is to test whether potential competition between the competing school systems leads to bigger test score gains. Building on the insights of our theoretical model, and the results in the previous section, we develop indexes of competitive pressure in a local area that depend on the fraction of Catholics and the fraction of new homes in an area.

a. Modeling the Effect of Effort Competition on Student Achievement

Since 1998 students at publicly funded schools in Ontario have written standardized achievement tests in grades 3, 6, and 9. We use the grade 3 and 6 results to measure the "value added" of elementary schools. Building on the existing literature we assume that the test score of student i in grade g 0{3,6} who attends school s in area a depends on his or her observed characteristics X_{isa}, on school characteristics Z_{sa} (including an indicator for whether the school is public or separate), on the characteristics of the local area W_a, on a measure of competitive pressure in the area, I_a, and on unobserved ability and random factors e_{gisa}:

(10) $$T_{gisa} = X_{gisa}b_{gx} + Z_{sa}b_{gz} + W_a b_{gw} + I_a b_{gI} + e_{gisa} .$$

Consistent with our theoretical model, competitive pressure in this specification is a <u>market characteristic</u> that affects the achievement of all students – not just those who attend a specific school. Note too that the coefficients in equation (10) are all grade-specific. Assuming that competitive pressure leads to a better learning environment we expect that $b_{6I}>b_{3I}$.

Because of neighborhood sorting, unobserved student abilities are likely to be correlated with school and neighborhood characteristics, leading to biases in OLS estimation of (10). When longitudinal student data are available the conventional solution is to estimate the model in first differences (e.g., Rivkin, Hanushek, and Kain, 2005). Since we cannot link 3rd and 6th grade test takers in most years of our data base, we follow an alternative approach of including school×cohort fixed effects and entering the time-invariant school and neighborhood variables as <u>interactions</u> with an indicator for test takers in 6th grade ($Gr6_{ig}$):

(11) $T_{gisa} = X_{gisa}b_{gx} + Z_{sa}Gr6_{ig}\, b_z + W_aGr6_{ig}\, b_w + I_aGr6_{ig}\, b_I + \xi_{coh(i),s} + e'_{gisa}$.

In this specification $\xi_{coh(i),s}$ represents a fixed effect for the cohort of students who were in third grade at school s at the same time as student i (or in 6th grade at the same school 3 years later), and e'_{gisa} represents the unexplained component of student i's score in grade g. In the presence of cohort×school fixed effects, we can only identify the differential (or "value-added") effects of the time-invariant variables. Thus, the coefficient on the competitive index is $b_I=b_{6I}-b_{3I}$, with similar expressions for b_z and b_w.

If all students remained at the same school between 3rd and 6th grades, estimates based on this approach would be numerically identical to those from a model of test score gains between 3rd and 6th grades. In the presence of student mobility, however, the two approaches will yield different estimates. For consistent estimation of the value-added coefficients using (11), any difference in average abilities between the leavers and joiners at a school must be uncorrelated with the index of competition – an assumption that may or may not be true.

Although longitudinal student identifiers are unavailable for most years of our sample, we obtained access to a file that allows us to link 6th grade test takers in 2007 with their 3rd grade outcomes in 2004. This "validation sample" allows us to compare the test score gains of the entire cohort of students at a given school with the gains for the stayers only, and to test whether use of the cohort-based data leads to any bias in the estimated effect of our key independent variables. Full details are presented in Appendix 4. In brief, we compare the test score gains for all students at a given school between 3rd and 6th grades with the change for the roughly 70% of stayers. We then regress this gap on the same covariates included in our main value-added models. Reassuringly, we find that the estimated effects of our competition measures are insignificantly different from zero. In fact, the point estimates suggest that a full cohort analysis tends to under-state the effects of competition on the stayers only. We use the implied estimates of the bias effects to construct "corrected" estimates below.

A second key assumption is that our measures of competitive pressure are orthogonal to any unobserved determinants of achievement *growth*. Based on the analysis in Table 3 we use two measures of local competitive pressure. The first is just the fraction of Catholic families in an area. The second is the product of the local fraction of Catholics and the growth rate of the local area. Since we do not observe the religion of individual test takers, either of these choices poses a potential problem if Catholic children have systematically different test score gains than other students, or if the presence of more Catholics exerts an independent effect on the efficiency of schools. We study this issue in detail in subsection c., below.

b. Test Score Data

We obtained individual test results for 3rd and 6th grade students at all elementary schools in the Province from 1998 to 2005, allowing us to track 5 cohorts of students, starting with children who were in 3rd grade in 1997-98, and ending with those in 3rd grade in 2001.[30] The sample includes roughly 65,000 public school students and 32,000 separate school students in each cohort. The test data file has a limited set of individual characteristics, including gender, whether a student is classified as "exceptional" (i.e., special needs) or "gifted" (i.e., advanced), whether he or she attended kindergarten, and whether he/she is enrolled in an English-as-a-Second-Language (ESL) or French Immersion program. (We do not know whether a student is Catholic). Means for these characteristics by grade and public versus separate school status are shown in the upper rows of Table 4. There are some small but statistically significant differences between students in the two systems. For example, separate school students are a little more likely to have attended kindergarten, are less likely to be classified as exceptional or gifted, and are less likely to be enrolled in ESL or French Immersion.

We limit our analysis sample to children in school-cohort groups that have at least 10 test takers in both 3rd and 6th grades. We also compare the number of test takers in a cohort in 3rd and 6th grades, and eliminate groups for which the ratio is greater than 140% or less than 71%.[31] Our final sample includes 65-70% of all public school test takers and a higher fraction (85-96%) of all separate school test takers. Students in the sample are drawn from approximately 9000 school-cohort groups who attended 2000 different schools.

Summary statistics for the test outcomes of the analysis sample are presented in the bottom rows of Table 4.[32] We show mean test scores by grade and public/separate school status

[30] The tests are administered by the Education Quality and Accountability Office (EQAO). EQAO will not release test records for schools with fewer than 15 students enrolled in the grade of the test. Thus, our analysis does not include those schools with low enrollments.
[31] We have estimated our main models using samples with different exclusion rules and find that the coefficients estimates are similar, though typically a little smaller in absolute value, when we retain school-cohorts with a wider range of variation in the number of test takers.
[32] Appendix Table 4 shows the mean test score outcomes for the overall samples of test takers and the fractions of students whose scores are missing. Many missing observations are attributable to exceptional (special needs) students, who are not required to take the test.

for the three main test components: reading, mathematics, and writing. The results are reported on a relatively coarse 4-point scale, with a mean of roughly 2.7 and a standard deviation of 0.6 to 0.8.[33] In contrast to the case in the U.S., where students at Catholic schools tend to have higher scores than those at public schools, mean test scores are quite similar in the two systems. Presumably this reflects the much different selection process in the Ontario system, where Catholic schools are free and readily available in all neighborhoods. The similarity of scores between the separate schools (where 100% of children have a Catholic background) and the public schools (where only about 20% are Catholic) suggests that on average Catholic children do not have characteristics that cause higher achievement.

c. Differences Between Catholic and non-Catholic Families

A critical assumption in our research design is that students in areas with a higher or lower fraction of Catholics have similar unobserved factors that affect achievement *growth*. While the similarity of the mean test scores in public and separate schools is consistent with this assumption, we cannot directly test it because we do not observe the religious affiliation of test-takers. Instead, we turn to two other data sources. The first is the 2001 Canadian Census, which includes information for a large sample of Ontario residents on education, earnings, and religious affiliation.[34] Since parental education is a powerful predictor of test scores (e.g., Jencks and Phillips, 1998), any differences in the education of Catholic versus non-Catholic parents would indicate a problem for our research design. Likewise, since wages are strongly affected by cognitive skills (Murnane, Willet, and Levy, 1995), comparisons of earnings potentially reveal

[33] This limited scale poses a potential attenuation problem, although the fraction of students coded with the minimum score is less than 10% while the fraction coded with the top score is less than 15%.
[34] The public use files of the 2001 Canadian census do not allow users to construct families. We classify as "parents" individuals between the ages of 24 and 62 who are either the head or spouse of the head of the household for a household with at least one child under the age of 16.

differences in cognitive skills of parents that may be correlated with those of their children. Our second data source is the National Education Longitudinal Study of 1988 (NELS), which interviewed children in 8th grade in 1988 and followed them for the next two decades. Although NELS is U.S.-based, it has the advantage of including test score information at different ages, as well as detailed information on family background and religion.

Table 5 presents comparisons of education and wages by religious affiliation for Ontario parents in the 2001 Census. We present models in which the years of education or log weekly wages of a parent are regressed on dummies for religious affiliation, Census Metropolitan Area (CMA, the lowest level of geography identified in the public use file), and country of origin. The set of controls is relatively parsimonious, reflecting the controls in our test score models (see below). The odd-numbered columns of Table 5 present models that include only a dummy for Catholic religion. In these models Catholics are compared to all other parents, including Protestants (about 35% of parents), those with no religious affiliation (about 15% of parents), and those with other affiliations (18% of parents). The specifications in the even-numbered columns include dummies for affiliates of other religions and non-affiliates. In these models Catholics are directly compared to Protestants. Looking across the columns, the estimates show that Catholic parents have a (modest) education and wage advantage relative to other parents as a whole, but are statistically indistinguishable from Protestant parents.

A very similar pattern holds with respect to test score outcomes of children in the NELS. Table 6 presents models for 8th grade test score outcomes of NELS students, and for their test score gains between 8th and 10th grade.[35] We present results for all students (in columns 1-4) and for the subset who were attending public schools in 8th grade (columns 5-8). The latter group is particularly interesting because few of them attend Catholic high schools (Altonji,

[35] NELS also has test scores in 12th grade, but a much larger fraction of students (25%) have missing test data.

Elder, and Taber, 2005a, 2005b). Thus, differences in test score gains between 8th and 10th grade arguably measure the direct impact of a Catholic family background, rather than a combination of family background effects and Catholic schooling effects.

As shown by the estimates in columns 1 and 5, when Catholics are compared to all other children, they have significantly higher 8th grade test scores. Likewise, the models in columns 3 and 7 show faster test score gains between 8th and 10th grade. When the comparison group is narrowed to Protestants, however, the differences are much smaller and are uniformly insignificant. As with Ontario parents, Catholic and Protestant children in the NELS are quite similar, although both groups do better than children with other religions or no affiliation.[36] We interpret the NELS findings as strongly confirming the conclusions from Table 5. In particular, Catholic children in Ontario and in the U.S. appear to have similar family background characteristics and similar test scores to Protestants. Both groups have better family characteristics and better test scores than children affiliated with other religions or with no religions.[37]

d. Models for Test Score Outcomes

Table 7 presents estimation results for four alternative specifications of equation (11), fit separately to individual scores in reading, mathematics, and writing. In addition to the explanatory variables listed in the table, the models include a dummy for 6th grade test takers, and 8 student-level controls (gender, ESL status, French immersion status, gifted or exceptional

[36] We have fit a wide variety of alternative models to the NELS data, including models that are fit by weighted OLS, using cross-sectional or panel weights, and models for 10th grade scores that include 8th grade scores on the right hand side. These models yield very similar results to the specifications reported in Table 6. Models for parental education in the NELS sample yield results that are quite similar to those in Table 5, though Catholic fathers have somewhat higher education than Protestant fathers. One important difference between Protestants in Canada and the U.S. is the higher fraction of "non-mainstream" Protestants in the U.S. In the NELS data children with Baptist and Pentecostal affiliation have lower test scores than mainstream Protestants, and their parents have lower education.
[37] See Lehrer (2009) for a review of evidence from the U.S. which generally concludes that religious affiliation has a positive effect on schooling outcomes.

status, a dummy for kindergarten attendance, and dummies for unknown gender and kindergarten attendance). These variables are all entered with grade-specific coefficients. We also include school-cohort means of the student variables, along with the mean fraction of the group with missing test scores and the fraction with a missing score who are coded as exceptional, all interacted with an indicator for grade 6. In addition, the models include the fraction of immigrants in the FSA, and the fractions in the FSA who report East Asian, South Asian, Northern European, Southern European, and Eastern European ethnicity, all interacted with grade 6 status.[38] The models include school×cohort fixed effects, with estimated standard errors "clustered" by school to allow for arbitrary correlation across the students from any one school.

Columns 1, 5, and 9 present a basic model that uses the local fraction of Catholics as an indicator of competitive pressure. The estimated coefficients are statistically significant and suggest that 6^{th} grade scores in reading and writing are about 0.06 points higher, and scores in math are about 0.09 points higher, in a 60% Catholic neighborhood than in a 20% Catholic neighborhood, holding all else constant. Since the standard deviations of 6^{th} grade scores across students are approximately 0.75, these represent effect sizes of 7-12% of a standard deviation.

The models in columns 2, 6, and 10 include both the local fraction of Catholics and the fraction of Catholics interacted with the share of new housing in the neighborhood as indicators of competitive pressure. Consistent with the pattern in our enrollment growth models, the estimated interaction terms suggest that competitive pressure is much stronger in newer neighborhoods. The specifications in columns 3, 7, and 11 exclude the Catholic share variable – a restriction that is not rejected at conventional significance levels for the reading or writing models but is rejected in the mathematics model. Again, all three estimates of the competition

[38] As Canada is historically comprised of immigrants from the United Kingdom and France, we exclude ethnicities affiliated with these countries from our European ethnic measures. In addition, we include Southern European countries previously aligned with Russia under the Eastern European ethnic grouping.

effect are highly statistically significant. To interpret the magnitude of the estimated effects, consider the 0.41 coefficient estimate from the reading score model. Comparing an area with a 60% Catholic share and a 20% new housing share (close to the sample average) to an area with the same new housing share but only 20% Catholics, reading scores in 6th grade are increased by 0.03 points, or an effect size of 4% of a standard deviation. The implied effect in a relatively new neighborhood (35% new housing share) is 8% of a standard deviation. The predicted effects for writing scores are slightly lower while the effects for mathematics are 54% larger.

Our investigation of differences between Catholics and non-Catholics suggested that Catholics are very similar to Protestants, but both groups are advantaged relative to families with other religious affiliations, or no affiliation. In view of this finding, we added two additional controls to the models in columns 4, 8, and 12: the fraction of people in the FSA who express no religious affiliation, and the fraction affiliated with religions other than Protestant or Catholic (i.e., Judaism, Islam, Hinduism, etc.). In these models, the effect of local competition is identified by variation in the fraction of Catholics relative to Protestants, holding constant the fractions with other religion and no religion.

The addition of these two controls attenuates the coefficient of our competition indicator slightly in the models in reading and writing, and somewhat more for mathematics, though all three coefficients remain significant at conventional levels. Consistent with the models for NELS test outcomes, a higher local fraction of people who report no religious affiliation is associated with slower test score gains, particularly in mathematics. By comparison the effects of the share of families with religious affiliations other than Protestant or Catholic are slightly positive, but not statistically significant. Interpreting the magnitudes of the competition effects, a rise of 40 percentage points in the fraction of children with choice between the systems is associated with 6^{th} grade test outcomes that are 3-5% of a standard deviation higher in a neighborhood with an average share of new houses.

d. Specification Checks

One potential concern with the specifications in Table 7 is that they ignore potential sample selection biases arising from the decision of parents to opt out of English language public schools and choose either private schools or publicly funded French language schools. To address this we follow Gronau (1974) and augmented our models with a control function representing the fraction of elementary students in the local area (FSA) who attend English language public schools (interacted with the dummy for grade 6).[39] Estimation results for a specification using the interaction between the fraction Catholic and the fraction of new homes in an area as the index of competition are presented in Table 8. For reference, columns 1, 4, and 7 reproduce the baseline specifications from columns 4, 8, and 12 of Table 7, while the corresponding models with the selection term are presented in columns 2, 5, and 8. The estimated selection effects are all negative (but insignificant), suggesting that students who opt out of the English language public schools have lower test score gains than average. Since the opt-out rate is slightly higher in more competitive areas, there is a slight upward selection bias (~0.02) in the uncorrected estimates of the competition effects. On balance, however, we conclude that selection effects are relatively small and can be safely ignored.

A second specification issue is the assumption, implicit in the models in Table 7, that local competitive pressure has the same effect on test score gains of students at public and

[39] If test score gains were normally distributed, and the decision to attend English language public schools was based on a single index choice model with a normally distributed random term, then the selection bias in the observed test score gains for students in an area where a fraction p of students are enrolled in the English language public schools would be proportional to the function $\lambda(p)=\varphi(\Phi^{-1}(p))/p$ where $\Phi^{-1}(.)$ is the inverse normal distribution function and $\varphi(.)$ is the standard normal density. In our sample $p>0.85$, and in this range $\lambda(p)$ is essentially linear in p. We therefore use the estimate of p as the control function for selectivity biases.

separate schools. While this is predicted by the simple symmetric equilibrium of our model, in principle the effects of local competitive pressure could be different in the two systems. To test this possibility, we estimated the models in columns 3, 6, and 9 of Table 8, which allow different effects of the index of local competitive at public and separate schools. Interestingly, these models all show a larger effect of competition on gains in the public schools, though the differences are never statistically significant.

A final issue is the potential bias caused by our use of test score gains for synthetic cohorts of students at a school, rather than for stayers only. In Appendix 4 we evaluate the magnitude of this bias by estimating models similar to the ones in Tables 7 and 8, but using as a dependent variable the gap in the estimates of value added at a given school based on the full cohort of students and the stayers. We can use the results from these models to construct "bias-corrected" estimates of the effect of competition on the test score gains of the stayers only. For example, using the basic specifications in columns 4, 8, and 12 of Table 7 we obtain the following results:

	Table 7 Coefficient	Estimated Bias (Appendix 4 Table B)	Bias-Corrected Coefficient
Reading	0.34	-0.08	0.42
	(0.15)	(0.09)	(0.24)
Mathematics	0.46	-0.14	0.50
	(0.21)	(0.09)	(0.23)
Writing	0.26	-0.04	0.30
	(0.12)	(0.07)	(0.14)

Because there is a negative relationship between the bias in the full cohort estimate of value added and the measure of competitive pressure, the bias-corrected competition effects are all larger in magnitude that the coefficients in Table 7, though less precisely estimated. We conclude that the estimates in Table 7 are, if anything, slightly conservative.

VI. Summary and Conclusions

Can a reduction in the monopoly power of local school districts improve the efficiency of publicly-funded education? We answer this question by studying the effects of school competition in Ontario, Canada, which operates two publicly-funded school systems: "public schools" that are open to all students, and "separate schools" that are limited to children with Catholic backgrounds. The fraction of families who can exercise choice between the competing systems varies widely across the province, providing the basis for our research design.

Our estimation results reveal significant but relatively modest effects of enhanced competition on the test score gains of students. Comparing markets where only 20 percent of children have choice to markets where 60 percent can choose between systems, we estimate that the gain in test scores between 3rd and 6th grades is increased by 0.03-0.05 of a standard deviation. The impacts are bigger in rapidly growing areas, where competition between schools for newly arriving students appears to be most intense. Importantly, we also find that potential competition raises test scores in both the public and separate systems, with somewhat larger impacts for public school students.

It is worth noting two limitations of our analysis. First, we can only measure test score gains over 3 years, or one-quarter of the time that most students spend in school. If similar effects were present at all stages of elementary and secondary schooling the benefits of competition would be commensurately greater. Second, it is possible that in more competitive markets teachers and principals spend more time and effort preparing for standardized tests, and less on other aspects of learning. If "test skills" have limited intellectual value, the effort devoted to competing over test outcomes is socially wasteful, and the higher test score gains observed in more competitive markets may be counter-productive.

Our findings have at least two implications for the design of alternative public education systems. First, we have shown that a significant fraction of families are willing to move between publicly-funded schools to access a combination of higher quality or more convenient schools. This willingness to move provides the basis for cross-system competition that can lead, at least in principle, to improved efficiency of publicly-funded schools. Second, our results underscore the critical importance of research on the links between parental choice decisions and the incentives faced by competing school systems.

References

Ahlin, Åsa, 2003. "Does School Competition Matter? Effects of a Large-Scale School Choice Reform on Student Performance," Working Paper Series 2003:2, Uppsala University, Department of Economics

Altonji, Joseph G., Todd E. Elder and Christopher R. Taber (2005a). "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools." *Journal of Political Economy* 113 (February), pp. 151-184.

Altonji, Joseph G., Todd E. Elder and Christopher R. Taber (2005b). "An Evaluation of Instrumental Variable Strategies for Estimating the Effects of Catholic Schools." *Journal of Human Resources* 40 (Fall), pp. 791-821.

Arum, R. (1996). "Do Private Schools Forces Public Schools to Compete?" *American Sociological Review* 61 (1), pp. 29-46.

Belfield, Clive R. and Henry M. Levin (2002). "The Effects of Competition on Educational Outcomes: A Review of U.S. Evidence." *Review of Educational Research* 72(2), pp.279-341

Bifulco, Robert and Helen F. Ladd (2004). "The Impacts of Charter Schools on Student Achievement: Evidence from North Carolina." *Education Finance and Policy* 1(1), pp.50-90.

Booker, Kevin, Scott Gilpatric, Timothy Gronberg, and Dennis Jansen (2005). "The Effect of Charter Schools on Traditional Public School Students in Texas: Are Children Who Stay Behind Left Behind?" Unpublished manuscript. Santa Monica: Rand Corportation.

Borland, M. V. and R. M. Howsen (1992). "Student Academic Achievement and the Degree of Market Concentration in Education." *Economics of Education Review* 11(1), pp. 31-39.

Bucklin, Randolph E., Gary J. Russell, and V. Srinivasan (1998). "A Relationship Between Market Share Elasticities and Brand Switching." *Journal of Marketing Research*, 35 (1), pp. 99-113.

Carnoy, Martin, Rebecca Jacobsen, Lawrence Mishel, and Richard Rothstein (2005). *The Charter School Dust-Up: Examining the Evidence on Enrollment and Achievement*. Washinton DC: Economic Policy Institute.

Chubb, John E. and Terry M. Moe (1990). *Politics, Markets, and America's Schools.* Washington DC: Brookings Institution.

Clark, Damon (2009). "The Performance and Competitive Effects of School Autonomy." *Journal of Political Economy* 117 (December), pp. 745-783.

Coleman, James S., Thomas Hoffer and Sally Kilgore (1982). *High School Achievement: Public, Catholic and Private Schools Compared*. New York: Basic Books.

Couch, J.F., William F. Shughart II. And A. L. Williams (1993). “Private School Enrollment and Public School Performance.” *Public Choice* 76(4), pp. 301-312.

Dee, Thomas S. (1998). “Competition and the Quality of Public Schools.” *Economics of Education Review* 17(4), pp. 419-427.

Epple, Dennis and Richard E. Romero (1999). “Competition Between Private and Public Schools, Vouchers, and Peer-Group Effects.” *American Economic Review* 88 (March), pp. 33-62.

Evans, William N. and Robert M. Schwab (1995). “Finishing High School and Starting College: Do Catholic Schools Make a Difference?” *Quarterly Journal of Economics* 110 (November), pp. 941-974.

Figlio, David and Jens Ludwig (2000). “Sex, Drugs, and Catholic School: Private Schooling and Adolescent Non-Market Behavior.” National Bureau of Economic Research Working Paper #7990. Cambridge MA: NBER.

Friedman, Milton. (1955). “The Role of Government in Education.” In Robert A. Solo, editor, *Economics and the Public Interest*. New Brunswick New Jersey: Rutgers University Press.

Geller, Christopher R., David L. Sjoquist, and Mary Beth Walker (2006). “The Effect of Private School Competition on Public School Performance in Georgia.” *Public Finance Review* 34 (January) , pp. 4-32.

Gibbons, Stephen, Stephen Machin, and Olmo Silva. (2008). "Choice, Competition and Pupil Achievement." *Journal of the European Economic Association*, 6(4), pp.912-947.

Goldberger, Arthur S. and Glen C. Cain (1982). “The Causal Analysis of Cognitive Outcomes in the Coleman, Hoffer and Kilgore Report.” *Sociology of Education* 55 (April-July), pp. 103-122.

Grogger, Jeffrey and Derek Neal (2000). “Further Evidence on the Benefits of Catholic Secondary Schooling.” *Brookings-Wharton Papers Urban Affairs*, pp. 151-193.

Gronau, Reuben (1974). “Wage Comparisons: A Selectivity Bias.” *Journal of Political Economy* 82 (November/December), pp. 1119-1143.

Halton District School Board (2009). “Memo on Elementary Optional Attendance.” Report Number 09189. Available at http://www.hdsb.ca/ParentInfo/Pages/OptionalAttendance-Elementary.aspx

Hanushek, Eric A. (2003). “The Failure of Input-Based Schooling Policies.” *Economic Journal* 113 (February), pp. F64-F98.

Hastings, Justine, Thomas Kane, and Douglas Staiger. (2006). "Parental Preferences and School Competition: Evidence from a Public School Choice Program." National Bureau of Economic Research Working Paper No. 11805. Cambridge, MA: NBER.

Howell, William G. and Paul E. Petersen (2002). *The Education Gap: Vouchers and Urban Schools.* Washington DC: Brookings Institution.

Hoxby, Caroline M. (1994). "Do Private Schools Provide Competition for Public Schools?" National Bureau of Economic Research Working Paper No. 4978. Cambridge, MA: NBER.

Hoxby, Caroline M. (2004). "Achievement in Charter Schools and Regular Schools in the United States: Understanding the Differences" Unpublished manuscript. Cambridge MA: Harvard University Department of Economics.

Hsieh, Chang-Tai and Miguel Urquiola (2006). "The Effects of Generalized School Choice on Achievement and Stratification: Evidence from Chile's Voucher Program." *Journal of Public Economics* 90, pp. 1477-1503.

Jencks, Christopher and Meredith Phillips, editors (1998). *The Black-White Test Score Gap*. Washington DC: Brookings Institution.

Jepsen, Christopher (2002). "The Role of Aggregation in Estimating the Effects of Private School Competition on Student Achievement." *Journal of Urban Economics* 52, pp. 477-500.

Jepsen, Christopher (2003). "The Effectiveness of Catholic Primary Schooling." *Journal of Human Resources* 38 (Fall), pp. 928-941.

Krueger, Alan B. and Pei Zhu (2004). "Another Look at the New York City School Voucher Experiment." *American Behavioral Scientist* 47, pp. 658-98

Ladd, Helen F. (2002). "School Vouchers: A Critical View." *Journal of Economic Perspectives* 16 (Fall), pp. 3-24.

Lehrer, Evelyn L. (2009). "Religious Affiliation and Participation as Determinants of Women's Educational Attainment and Wages." Christopher Ellison and Robert Hummer (eds.) *Religion, Families and Health in the United States: New Directions in Population Based Reserch,* Rutgers University Press, *forthcoming*.

McFadden, Daniel and Kenneth Train (2000) "Mixed MNL Models for Discrete Response." *Journal of Applied Econometric*s 15(5), pp 447-470.

Murnane, Richard J., John B. Willett, and Frank Levy. (1995). "The Growing Importance of Cognitive Skills in Wage Determination." *Review of Economics and Statistics*, 77(2), pp. 251-66.

Nechyba, Thomas. (2000). "Mobility, Targeting and Private School Vouchers." *American Economic Review* 90 (March), pp. 130-146.

Neal, Derek (1997). "The Effects of Catholic Secondary Schooling on Educational Attainment." *Journal of Labor Economics* 15 (January), pp. 98-123.

Nevo, Aviv.(2000) "A Practitioner's Guide to Estimation of Random Coefficients Logit Models of Demand," *Journal of Economics & Management Strategy,* 9(4), pp. 513-548.

Newmark, Craig M. (1995). "Another Look at Whether Private Schools Influence Public School Quality." *Public Choice* 82(3/4), pp. 365-373.

Millimet, Daniel L. and Vasudha Rangaprasad (2007). "Strategic Competition Amongst Public Schools." *Regional Science & Urban Economics,* 37, pp. 199-219.

Ontario Ministry of Education (2005). "The Ontario Curriculum, Grades 1-8: Mathematics." Available at http://www.edu.gov.on.ca/eng/curriculum/elementary/math18curr.pdf.

Rivkin, Steven G., Eric Hanushek, and John F. Kain (2005). "Teachers, Schools, and Academic Achievement." *Econometrica* 73, pp. 417-458.

Rothstein, Jesse (2006). "Good Principals or Good Peers: Parental Valuation of School Characteristics, Tiebout Equilibrium, and the Incentive Effects of Competition Among Jurisdictions." *American Economic Review* 96(4), pp. 1333-1350.

Rothstein, Jesse (2007)."Does Competition Among Public Schools Benefit Students and Taxpayers? A Comment on Hoxby (2000)." *American Economic Review* 97(5), pp. 2026-2037.

Sander, William (1999). "Private Schools and Public School Achievement." *Journal of Human Resources* 34 (Autumn), pp. 697-709.

Appendix 1: Multiple Schools Equilibrium

This appendix extends the model developed in the text to the case where there are multiple schools operated by each of the two competing systems. To simplify notation, define a neighborhood by the identity of its assigned schools: thus students in neighborhood (j,k) can attend either public school j or separate school k. Let n_{2jk} represent the number of Catholic students in neighborhood (j,k) and let

$$s_{jk}(\Delta Q_{jk}, \Delta t_{jk}) = \int F[\ \delta_i + \beta\Delta Q_{jk} - \gamma\Delta t_{jk}\ ]\ h(\delta_i|j,k)\ d\delta_i$$

represent the share of these students who attend public school j, given the quality differential ΔQ_{jk} and relative travel costs Δt_{jk}. Public school j's attendance zone includes n_{1j} non-Catholic students and $n_{2j} = \Sigma_k\ n_{2jk}$ Catholic students (with similar expressions for separate school k). Total enrollment at public school j is therefore

$$E_j = n_{1j} + n_{2j}\ \Sigma_k\ n_{2jk}/n_{2j}\ \ s_{jk}(\Delta Q_{jk}, \Delta t_{jk}) ,$$

while total enrollment at separate school k is

$$E_k = n_{2k}\ \Sigma_j\ n_{2jk}/n_{2k}\ \ (1 - s_{jk}(\Delta Q_{jk}, \Delta t_{jk})) .$$

Assuming that school quality depends on managerial effort as before, and that school managers have the same objective function specified earlier, the first order condition for the effort choice of the manager of the j^{th} public school is

(A1) $\theta\ (n_{2j}/n_j)\ \{\ \Sigma_k\ (n_{2jk}/n_{2j})\ \partial s_{jk}(\Delta Q_{jk}, \Delta t_{jk})/\partial\Delta Q\ \}\ \ q'(e_j) - 1 = 0$,

while the corresponding condition for the manager of the k^{th} separate school is

(A2) $\theta\ (n_{2k}/n_k)\ \{\ \Sigma_j\ (n_{2jk}/n_{2k})\ \partial s_{jk}(\Delta Q_{jk}, \Delta t_{jk})/\partial\Delta Q\ \}\ q'(e_k) - 1 = 0$.

As a benchmark, consider the case in which: (i) the distribution of tastes is the same in all neighborhoods (i.e., $h(\delta_i|j,k) = h(\delta_i)$); (ii) relative travel costs are the same in all neighborhoods (i.e., $\Delta t_{jk} = \Delta t$); (iii) the relative fraction of Catholic students is constant and equal to n_2/n across all neighborhoods. Under these conditions,

$$s_{jk}(\Delta Q_{jk}, \Delta t_{jk}) = s(\Delta Q_{jk}, \Delta t) \equiv \int F[\delta_i + \beta\Delta Q_{jk} - \gamma\Delta t]\, h(\delta_i)d\delta_i ,$$

and the effort game has a symmetric equilibrium with $e_j = e_k = e^*$, where e^* satisfies the condition

(A3) $\theta\,(n_2/n)\,\partial s(0, \Delta t)/\partial\Delta Q\;\; q'(e^*) - 1 = 0.$

This is the same as the equilibrium condition in the two-school case given by equation (6) in the text.

More generally, in a multi-school setting the incentives for effort of the manager of a given school depend on the fraction of students in the catchment area who can potentially move to the other system, and on a weighted average of the derivatives of the enrollment share in each neighborhood with respect to relative school quality (i.e., $\Sigma_k\,(n_{2jk}/n_{2j})\,\partial s_{jk}(\Delta Q_{jk}, \Delta t_{jk})/\partial\Delta Q$). As in the simpler two-school setting, this derivative is closely related to the sensitivity of enrollment to a change in the number of nearby schools operated by the competing system. In particular, using a slight modification of equations (7) and (8) it is easy to show that schools with market shares that are more sensitive to quality will lose more students when the opposing system opens a new school nearby.

Appendix 2: Construction of Schools and Test Score Data

All data on Ontario schools were obtained from the Ministry of Education under several Freedom of Information Requests. The following basic school information was provided: school identification number, school name, school type, board affiliation, and last known address.[1] This information was requested for all schools that were in existence at any point from 1990 to the present. From this information, we identified a set of publicly funded, English speaking public and separate schools. This set of schools includes French Immersion programs in English speaking schools. From this set of schools, we excluded any school that we could identify as being a school operated for the mentally ill, prisons, and other types of special populations.[2]

For each school year, the Ministry provided enrollments for each grade based on the fall enrollment reports the schools were required to complete. From these enrollment figures we identified the set of schools for which a school had positive enrollment for one or more grades between 1st and 6th grades during the sample period.

Identification of an Opening or Closing

We tracked openings and closings of schools that offer grades 2, 3, and/or 4 in the opening or closing year.[3] To be classified as an opening school, enrollment in these grades must be positive in a given year (the "opening year") and *total enrollment* must be zero in previous years. Similarly, to be classified as a closing school, enrollment in grades 2, 3, and/or 4 must be positive in a given year and *total enrollment* must be zero in the next year (the "closing year") and all subsequent years. We ignore schools that open and close in the same year (i.e., only have

[1] If a school moved locations during the period under study, we do not observe the move.
[2] In the data cleaning process we excluded the following types of schools: schools whose address is located outside of the province; schools whose primary population are prisoners or infirmed individuals; schools that only offer kindergarten; schools on First Nation reserves; schools that never report a positive enrollment.
[3] This results in our excluding from an analysis "middle" schools that open or close during the sample period. In Ontario, most schools offer all grades between 1st and 8th grade.

positive enrollment in a single calendar year). Note that schools that expand or contract their grade offerings **are not** treated as opening or closing. Similarly, in a few situations, schools are paired together for administrative purposes. When this occurs provincial records show that both schools remain in operation but enrollment for the two is reported at only one of the schools. We identified these "pairing events" and validated their status with information from the Ministry of Education. We ignore enrollment changes arising from pairing events in the identification of an opening or closing.

Special considerations:

- Schools that change grades. There are a few schools that add or drop grades over time. Because these schools were in existence and continue to be in existence we do not treat them as openings or closings.

- There are some schools that close, remain closed for several years and then reopen. After confirming that the school has not been an annexed school in the intervening years (effectively remaining open during the period it appears to have been closed), we treat these events as separate events. We identified the following three events:
    - School closed in 1991 and then reopened in 1995.
    - School closed in 1993 and reopened in 1999.
    - School closed in 1995 and reopened in 1997.

- There are a few schools that appear to close in one year and within the next two years another school opens in the same location. Depending on changes in enrollments we either classify the schools as separate events or assume the events represent more of a "name" change than a true closing and opening. We identified seven sets of events that we concluded should not be treated as either closing or opening events.

- If a school slowly opens or slowly closes (e.g. increases/decreases the grades offered), we will modify the enrollment figures used in our analysis to reflect the change in enrollment for the appropriate cohorts of students (e.g. if a school opens and initially offers grades 1-3 but then expands to include grades 4-6, we will measure the change in enrollment to reflect enrollments for grades 1-3 in year t-1 and grades 2-4 in year t) if that school is used in the analysis (it is affected by another school that opens or closes). The year used to identify the opening or closing, however, is the first/last year the school is observed with positive enrollment, respectively.

Linking of school data to test scores

Beginning in school year 1998, all publicly funded schools were required to participate in the testing of students in grades 3 and 6 using a test instrument developed by the Educational Quality and Accountability Office ("EQAO"). The EQAO tests were designed to help schools and school boards obtain a better understanding of the effectiveness of the curriculum on obtaining student achievement. To date, performance on the EQAO test does not formally affect a school's budget. The test is given in the spring of each academic year. For each of three components mathematics, reading, and writing), a student is scored on a scale of 1-4. Over time the duration and other aspects of the test have changed. The scale, however, has remained constant with 1 representing a well below expectations and 4 representing an exceeds expectation score.

For schools with more than 15 students, we obtained through a series of Freedom of Information requests student level data that contain information on student characteristics and performance on the three components of the test (mathematics, reading, and writing). We were provided with records for all students that should have sat the EQAO test. Thus, we were provided with records of students who only sat for part of the test and who did not sit for any of the test. To help control for issues of selection bias from students that might not have randomly not sat the exam, we were able to identify for each grade and school the share of test takers with no test score and whether these test takers were identified as receiving special education status.[4]

We compared the number of potential test takers by grade with the fall enrollment figures we had for the schools. Given the enrollment figures were obtained in the fall and the test was administered in the spring, we expected there to be some slippage in the enrollment and test taker counts. In instances where there was a substantial discrepancy in these counts, we investigated the data further. In some instances the school's unique identifier was miscoded. Because we were

[4] Over time, the method used to classify students as receiving special education has changed slightly. For each test year we attempted to use a consistent method for identifying these students given these constraints. For more information on how we addressed and various other issues on student characteristics, please contact the authors.

given the name of the school, we were able to use hand checking to identify the appropriate school number to use in order to match the test level data with the school level data.

As explained in more detail in the paper, we observed that some schools had dramatically low numbers of students for whom we observe a test score. To refine our estimation, we excluded schools with a high number of non-test takers.

Linking of school data to Census and location measures

For each school we were given the last known address. We used the first three characters of the postal code to identify the "Forward Sortation Area" (FSA) of the school. Using the FSA we then matched census data from 1991, 1996, and 2001 to schools. If the current FSA did not exist for earlier years, we identified the FSA that most likely was covered historically and used census measures across all three periods that corresponded to the area covered by the school for all three census years. In some instances the FSA census data were suppressed and/or it was clear that the area covered by the FSA did not represent the area that was likely to be the school's catchment area. This usually occurred in rural areas where there was a small town that had a distinct FSA from the rural parts. We used the census measures for the broader area when it was clear that a school's enrollment included families residing in both the rural area and the small town.

For each school address, we used data provided by researchers at Carleton University to identify the longitude and latitude of each school location. If instances where the school address as given as a post office box, we used the longitude and latitude for the centroid of the postal code. For more information on the data from this source, please see www.geocoder.ca.

Appendix 3: **Construction of Circle Data Set**

For each opening and closing school we constructed a "pre-defined" circle based on the average distance traveled by students to schools in the area.[1] We then refined the circle by excluding schools that were identified to be within the circle for which there is a physical obstacle preventing it from being a reasonable competitor. These obstacles include expressways, ravines, and industrial/commercial areas. We also included schools that were outside of the pre-defined circle if it appeared that the school was close enough to the opening/closing school to be a potential competitor. Our judgments were based on an examination of detailed satellite images that mapped the school addresses. In instances where the satellite image was unclear and/or the few school addresses that could not be found by the mapping software, we used print maps of Ontario streets that contain markers for existing and many previously existing schools.[2]

Across the 735 identified changes, we identified at least one school in 559 circles. There are 58 public openings, 35 separate openings, 74 public closings, and 10 separate closures for which there were no existing schools within a reasonable distance. We then eliminated circles that contained only rural schools that were affected by the change. This leaves a total of 442 changes that affected at least one non-rural school. Appendix 3 Table A presents summary statistics on the refined circles we have selected by type of change.

---

[1] For more recent years of the school enrollment data, we were able to obtain counts of students attending the school based on their postal codes. This type of data is somewhat noisy as when compared with the location of the school there can be unrealistic distances between the students home postal code and the school. Moreover, we have this information for only those schools that were operating in the latter years of the sample. We, therefore, used this information to identify a baseline circle size of the catchment area of schools located in a given region.

[2] To define the circles, we used the latitude and longitude of the school based on its most recent street address. While information on latitude and longitude is publicly available from several sources, we found the most reliable source of this information from www.geocoder.ca. The individuals that provide this service have taken publicly available data and corrected them. Through our examination of printed maps and satellite images, we randomly confirmed that the information we received from Geocoder was better than the information from government sources.

In Panel B of Appendix 3 Table A we report statistics on the circles for which we identified at least one non-rural affected school. The share of circles with existing public schools ranges from 86 to 100 percent. The share of circles with existing separate schools ranges between 73 and 95 percent. For approximately 20-25 percent of the opening circles and 60-65 percent of the closing circles we excluded schools that were identified in the pre-defined circles. For approximately 55-65 percent of the opening schools and 43-50 percent of the closing schools we added schools that are located outside of the pre-defined circle. A small proportion of the openings and closings only use schools located outside of the pre-defined circle.

Example of Circle Modification

Elkhorn Public School opened in 1996 in North York, a community that is a part of the Toronto District School Board.[3] In 1996 it had a total enrollment of 297 students. Students were enrolled in grades from kindergarten to grade 4. In 1997, enrollment grew to 371 and the school had students enrolled from kindergarten to grade 5. For the rest of the sample period, this school has had students enrolled in all of these grades. Approximately 65 percent of the students have a primary language other than English.

For this area, we estimated an average distance to school of 2.2 kilometers. We identified and mapped all schools that were in operation at the time of the opening up to 3.2 kilometers. For these schools we mapped the location (based on their addresses) using a satellite image and using printed maps that contain the specific location of schools. Appendix 3 Figure A presents a depiction of those schools that were within a radius of just less than 2.2 kilometers. We do not depict the school that are beyond 2.2. kilometers from the school as the decision of whether to keep it was based on the decision regarding Lescon Public School (a school within the 2.2 kilometer radius).

[3] On the location of this school, there was a public school that closed in 1985.

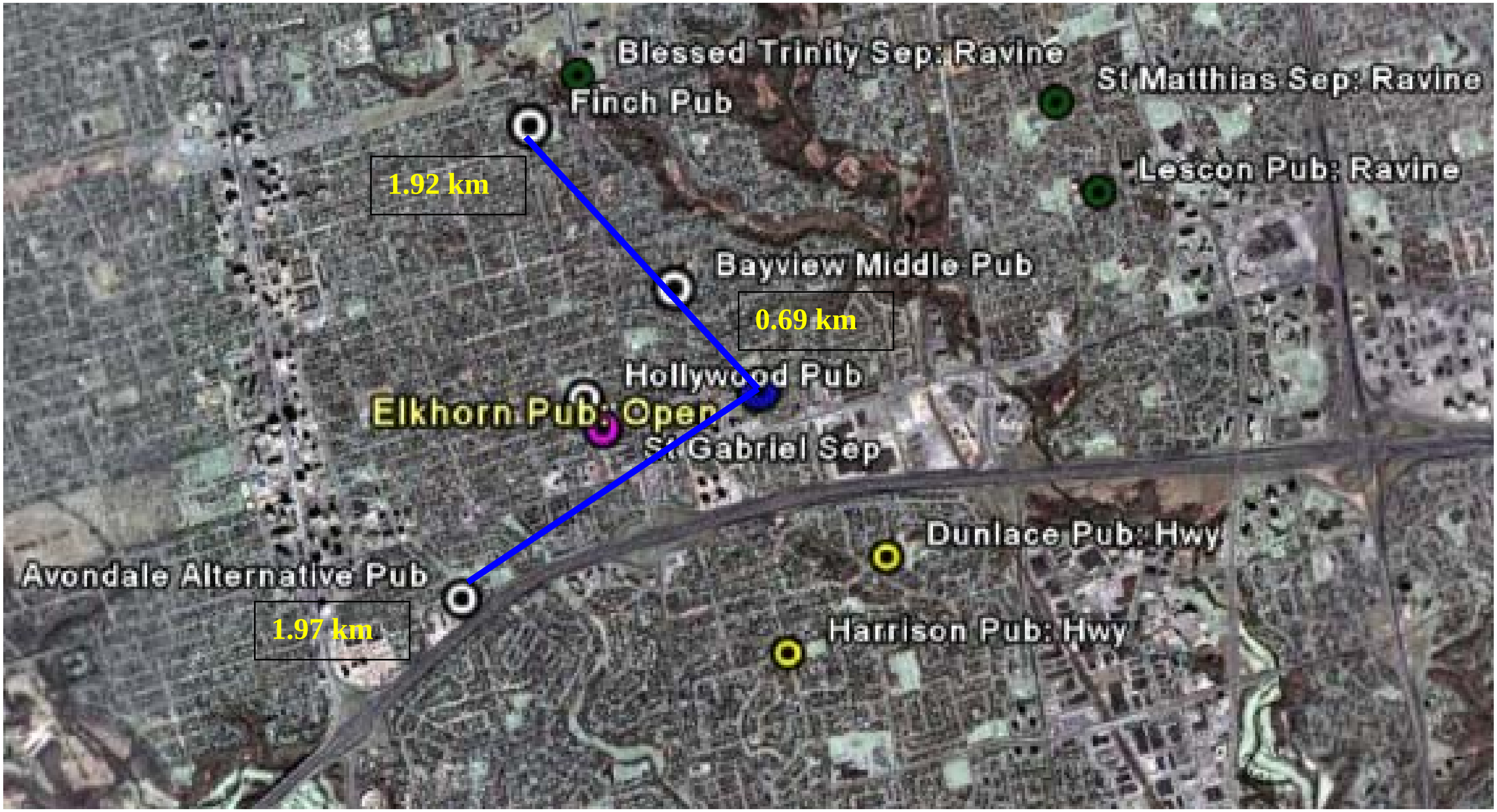
Depicted are 10 schools, 7 are public and 3 are separate. Among the public schools is Bayview Middle School. Until 1995 it offered grades kindergarten to grade 8. From 1996 onwards, the school has only offered grades 6 to 8. Thus, it appears that, in part, Elkhorn was established to take over the enrollment for Bayview. Another public school in the area is Avondale Elementary Alternative School. The school is alternative in that it allows for self-directed learning. It covers all elementary grades. Since opening (in 1992), the enrollment has been just slightly under 100 students. The remaining 5 public schools have average enrollments in grades 1 to 6 during the sample period that range between 126 and 281 students. Of the three separate schools depicted, average enrollment in grades 1 to 6 ranged between 163 and 296 students over the sample period.

There are two issues that caused us to restrict the sample of schools treated as being within a close distance of the opened school. First, there is a major freeway (Highway 401) that is located south of Elkhorn. This resulted the in the exclusion of Dunlace and Harrison Public Schools. Second, there is a ravine. This excluded two of the three separate schools (Blessed Trinity and St. Mathias) and one of the public schools (Lescon). The remaining schools are located within 2 kilometers of Elkhorn. Given students could reside in areas between Elkhorn and these schools, it seems reasonable to include these schools as ones that are potentially affected by the opening.

This leaves, however, only one potentially competing separate school. Blessed Trinity is just beyond the ravine and is close to Finch Public School, a school that is treated as within the circle of the opening. Appendix 3 Figure B provides a more detailed image of the area around Blessed Trinity. The figure shows that Blessed Trinity and Finch schools are separated by two major roads. Moreover, there are few houses that lie in between these schools. It appears that

Blessed Trinity draws its students from the houses that are located north east of the school, an area that is farther away from Elkhorn. Therefore, we decided that this school should not be treated as being potentially affected by the opening.

Appendix 3 Figure A



Appendix 3 Figure B

Appendix 3 Table A: Statistics on Circles Around Opening and Closing Schools

Panel A	Total number of events	Number with NO nearby school	Number with at least one non-rural school
Public School Opening	252	58	159
Separate School Opening	169	34	107
Public School Closure	212	74	97
Separate School Closure	102	10	79

Panel B: Characteristics of Circles That Include Non-Rural Affected Schools	Percent with 1+ Public Schools	Percent with 1+ Separate Schools	Percent that have at least 1 school in intial circle dropped	Percent that have at least 1 school outside intial circle added	Percent that have all included schools outside intial circle
Public School Opening	86.2%	92.5%	23.9%	54.7%	13.8%
Separate School Opening	87.9%	72.9%	20.6%	64.5%	16.8%
Public School Closure	96.9%	94.8%	64.9%	43.3%	5.2%
Separate School Closure	100.0%	81.0%	62.0%	49.4%	1.3%

Appendix 4: Comparison of Gain in Scores for All Students and Stayers

We obtained a file with test results for 138,650 grade 3 students in 2004 and 143,869 grade 6 test takers in 2007. This file also includes student identifiers that allow us to classify students into 2 broad groups: stayers (those who were at the same school in grade 3 and grade 6); and movers. Movers can be further classified into students who are observed in different schools in grade 3 and grade 6, and those who are only observed once, either in grade 3 or grade 6. The latter includes students who entered or left publicly-funded schools in the Province of Ontario between grade 3 and grade 6, as well as those whose student identifiers are missing or miscoded.

For comparability with our main estimation sample, we deleted records for students attending French language schools (11,600 students) and rural schools (51,000 students). We also deleted students attending schools with more than 10% missing identifiers (approximately 12,800 students), those attending schools with fewer than 10 test takers in each grade, and those attending schools where the number of test takers in grade 6 is less than 71% or more than 140% of the number of test takers in grade 3. We also deleted students at schools that cannot be matched to neighborhood-level data (from the Census), and students with missing or duplicate identifiers. These deletions result in a sample of 102,240 students who attended one of 1,734 non-rural elementary schools in grade 3 in 2004 or in grade 6 in 2007. Of these, 54,241 (approximately 70% of the students present in either grade 3 or grade 6) are classified as stayers.

Appendix 4 Table A presents a comparison of test score results in grades 3 and 6, as well as the changes in average test scores between these grades, for all students and for the subgroup of stayers. Stayers have somewhat higher scores in all three tests (reading, mathematics and writing), and also have a lower rate of missing test scores. The gap between all test takers and stayers widens slightly between grade 3 and grade 6. As a result, the test score change for all students between grades 3 and 6 tends to understate the test change for stayers. The deviation between the two changes is presented in column 7 of Appendix 4 Table A. Expressed as a fraction of the standard deviation of test scores (approximately 0.75), the deviation is relatively small: 2% of a standard deviation for mathematics, and 1.2% of a standard deviation for reading and writing.

Estimating the Bias in Models Using Gain Scores Based on Full Cohort of Test Takers

For our main analysis (Table 7) we data on all test takers in a given school-cohort (i.e., all students observed in that school in grade 3 in year t and in grade 6 in year t+3). In the presence of student mobility, the estimates from our approach will differ from the estimates that would be obtained using only stayers. To evaluate the biases arising from our "full cohort" approach relative to an analysis based on stayers, we constructed school-level estimates of the deviation between the change in test scores between grades 3 and 6 for all test takers and the change for the stayers only. We then estimated a series of regression models using the gap in estimated test score gains as the dependent variable and the same covariates as in Table 7. The results are presented in Appendix 4 Table B.

The models in columns 1, 4, and 7 include only the local Catholic share. The estimated coefficients of this variable are relatively small and statistically insignificant (t-ratio less than 0.6 in all cases). The models in the remaining columns include the interaction of the Catholic share with the share of new housing, either in combination with the Catholic share variable or alone, or as the sole measure of local competition.

Appendix 4 Table A: Comparison of Test Score Levels and Gains for All Students and Stayers (2004-2007 cohort only)

	Grade 3 Students in 2004		Grade 6 Students in 2007		Test Score Gains:		
	All	Stayers	All	Stayers	All	Stayers	Bias
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of Students	77,391	54,241	79,090	54,241			
Fraction Stayers		0.701		0.686			
Reading Test:							
Share Missing Test	0.082	0.066	0.049	0.027			
Average Test Score (1-4 Scale)	2.639	2.663	2.737	2.770	0.098	0.107	-0.009
Mathematics Test:							
Share Missing Test	0.054	0.041	0.046	0.026			
Average Test Score (1-4 Scale)	2.774	2.794	2.725	2.759	-0.050	-0.035	-0.015
Writing Test:							
Share Missing Test	0.072	0.056	0.044	0.024			
Average Test Score (1-4 Scale)	2.730	2.749	2.811	2.840	0.081	0.091	-0.009

Notes: Sample consists of students in grade 3 in 2004 or grade 6 in 2007 in a school included in estimation sample. "All" columns refer to all students in the specified grade and year. "Stayers" refer to subset of students who are observed in the same school in 2004 and 2007. Bias estimate in column (7) is difference in test score gains between all observed students and stayers.

The coefficients associated with the interaction are uniformly negative, and in the case of reading and mathematics are also relatively large in magnitude, though insignificant by conventional standards (t < 1.6 in all cases). Focusing on the specification in columns 3, 6 and 9 that also controls for the fractions of other religious groups, the estimates suggest that the effect of local competition – as measured by the interaction of the Catholic share with the share of new housing – is biased in a negative direction (i.e., toward 0) by using the change in test scores for the full cohort, rather than for the stayers.

In the text we use the estimates from columns 3, 6 and 9 to construct "bias corrected" estimates of the effect of local competition on gain scores between 3^{rd} and 6^{th} grades. Assuming that the "true" effect of interest is the effect on stayers, the bias-corrected estimate is the coefficient estimate based on the full cohort (i.e., the estimates in columns 4, 8, and 12 in Table 7) minus the estimated bias term from the corresponding models in columns 3, 6 and 9 of Appendix 4 Table B. Since the latter are obtained from a sample of tests that are not used in the estimation sample in Table 7, we assume that the estimated coefficients are independent, allowing us to easily construct sampling errors for the bias-corrected estimates.

Appendix 4 Table B: Estimated Models for the Bias in Full-Cohort versus Stayers Estimate of Gain Score

	Reading			Mathematics			Writing		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Local Competition Measures:									
Share of Catholics	0.026	0.038		0.004	0.023		0.013	0.016	
	(0.047)	(0.049)		(0.048)	(0.050)		(0.038)	(0.039)	
Share Catholics × Share New Housing Stock		-0.095	-0.082		-0.144	-0.140		-0.026	-0.039
		(0.095)	(0.093)		(0.092)	(0.089)		(0.075)	(0.073)
Other Controls:									
Share with No Religion			-0.063			-0.045			-0.087
			(0.091)			(0.088)			(0.080)
Share with Other Religions			-0.023			-0.039			-0.067
			(0.053)			(0.052)			(0.052)
Separate School	0.006	0.006	0.006	0.012	0.012	0.012	0.002	0.002	0.001
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)	(0.005)
Share New Housing Stock	-0.034	0.003	-0.002	-0.002	0.055	0.054	-0.027	-0.017	-0.011
	(0.017)	(0.042)	(0.041)	(0.017)	(0.040)	(0.039)	(0.014)	(0.033)	(0.032)
R-Squared	0.129	0.130	0.130	0.097	0.098	0.098	0.117	0.117	0.119
Mean of Dependent Variable		-0.021			-0.020			-0.015	
(Standard Deviation)		(0.123)			(0.121)			(0.103)	

Notes: standard errors in parentheses. Dependent variable is school-average change in test scores for all test takers (between grade 3 students in 2004 and grade 6 students in 2007), minus corresponding average for stayers. Sample includes 1734 schools. All models include controls for average characteristics of students in grade 3 and grade 6 and FSA-level neighborhood characteristics.

Appendix Table 1: Census-Based Characteristics of non-Rural FSA's w/ School Changes

	Mean for FSA's with:			
	No Changes	School Openings	School Closings	Openings &Closings
Number of FSA's	215	77	92	45
Basic FSA Characteristics:				
Total population	24,177	29,832	25,361	30,960
Share of Houses Built Between 1991-2001	16.40%	32.59%	6.74%	14.13%
Presence of Children:				
Share of population age 5-9	6.4%	7.7%	5.8%	6.5%
Share of population age 10-14	6.5%	7.7%	5.8%	6.6%
Family Characteristics:				
Share Single Parent Families	22.95%	17.65%	28.46%	24.59%
Share with 1 Child	42.71%	36.88%	45.62%	42.12%
Share with 2+ Children	39.48%	43.68%	37.77%	40.32%
Education (Adult Population):				
Share with University Degree	23.25%	24.32%	22.23%	19.79%
Share without High School Diploma	27.33%	24.06%	30.63%	28.09%
Language, Nativity and Ethnicity:				
Share that Speak English at Home	90.40%	93.18%	88.16%	94.26%
Share Immigrants	23.79%	32.93%	28.99%	21.23%
Share Southwest Asian Ancestry	4.38%	8.63%	4.33%	2.69%
Share East Asian Ancestry	5.70%	9.88%	8.09%	4.55%
Share North European Ancestry	13.50%	10.14%	11.55%	14.35%
Share South European Ancestry	9.50%	15.26%	13.24%	9.57%
Share East European Ancestry	10.93%	9.92%	11.11%	10.44%
Religious Affiliation:				
Share Catholic	35.27%	38.53%	40.21%	33.21%
Share Protestant	40.96%	35.88%	33.60%	44.70%
Share Other Religions	8.83%	12.16%	10.70%	6.70%
Share No Religion	14.95%	13.42%	15.50%	15.39%

Note: based on FSA-tabulations of 1991-1996-2001-2006 Censuses.
Religious measures, however, are available only for 1991 and 2001 Censuses

Appendix Table 2: Distribution of Affected Schools by Numbers of Opening and Closing Events that Affect the School

	Number of Closings:			
	None	One Closing	Two Closings	Three-Four Closings
Number of Openings:				
None	0	337	101	24
One Opening	272	48	12	9
Two Openings	90	7	1	0
Three Openings	34	0	0	0
Four-Six Openings	18	0	0	0

Note: sample of affected schools includes only non-rural schools.

Appendix Table 3: Coefficients on Closing Measures of Growth Models

	Percentage Change in Enrollment: Grade 1 (t-1) to Grade 1 (t)	Grades 1-5 (t-1) to Grades 2-6 (t)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effects of Nearby Closings (trend shift in following 3 years)							
Own Effects:							
Effect on Public School of Public Closing	4.7	4.8	4.7	4.8	4.8	4.7	4.7
	(1.5)	(0.9)	(0.9)	(0.9)	(0.9)	(0.9)	(0.9)
Effect on Separate School of Separate Closing	7.7	6.1	6.0	6.1	6.1	6.0	6.0
	(3.7)	(1.2)	(1.2)	(1.2)	(1.2)	(1.2)	(1.2)
Cross Effects:							
Effect on Separate School of Public Closing	-0.6	0.5	0.3	0.5	0.5	0.3	0.4
	(1.6)	(0.6)	(0.6)	(0.6)	(0.6)	(0.6)	(0.6)
Effect on Public School of Separate Closing	1.3	-0.8	-0.8	-0.8	-0.8	-0.8	-0.8
	(1.6)	(0.9)	(0.9)	(0.9)	(0.9)	(0.9)	(0.9)
School fixed effects and Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying school characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying local characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Base Opening Measures	Yes	Yes	Yes	Yes	No	No	No
Interaction Opening & Share New Housing	No	No	Yes	No	No	No	No
Interaction Opening & Share Catholic	No	No	No	Yes	Yes	Yes	No
Interaction Opening & Share Catholic*Share New Housing	No	No	No	No	No	Yes	Yes
Number of Observations	11,887	12,007	12,007	12,007	12,007	12,007	12,007
Number of Schools	939	945	945	945	945	945	945

Note: standard errors in parentheses. School characteristics are a dummy for being paired with another school for administrative purposes. Local characteristics are share of enrolled students in the FSA attending public French and private schools, total population in the FSA and shares of population ages 5-9 and 10-14, fraction of FSA residents who are Catholic, fraction who are immigrants, fractions of FSA residents of East Asian, South Asian, and Northern, Southern, and Eastern European ancentry, fraction of population with a university degree, fraction with no high school degree, fraction of single-headed families, fraction of families with 2 or 3 kids, and fraction of adults with home language other than English. British or French ancentry treated as equivalent to "Canadian". Eastern European ancentry groups includes countries formerly affiliated with the U.S.S.R.

Appendix Table 4: Summary Statistics for ALL EQAO Test Takers

	Public Schools		Separate Schools	
	Grade 3	Grade 6	Grade 3	Grade 6
	(1)	(2)	(3)	(4)
Reading Tests				
Number of observations	293,146	327,443	154,565	167,482
Average Score (1-4 Scale)	2.52	2.68	2.52	2.70
(standard deviation)	(0.76)	(0.75)	(0.75)	(0.73)
Share of Students with Missing Score	0.12	0.08	0.11	0.07
Share of Missing Students Identified as Exceptional	0.23	0.12	0.26	0.14
Share Included in Analysis Sample	0.73	0.70	0.96	0.91
Mathematics Tests				
Number of observations	314,614	330,125	160,318	168,228
Average Score (1-4 Scale)	2.73	2.69	2.67	2.68
(standard deviation)	(0.75)	(0.81)	(0.73)	(0.79)
Share of Students with Missing Score	0.09	0.08	0.08	0.06
Share of Missing Students Identified as Exceptional	0.23	0.12	0.28	0.14
Share Included in Analysis Sample	0.73	0.72	0.96	0.92
Writing Tests				
Number of observations	302,282	333,240	158,770	169,743
Average Score (1-4 Scale)	2.66	2.67	2.68	2.71
(standard deviation)	(0.66)	(0.71)	(0.65)	(0.75)
Share of Students with Missing Score	0.10	0.07	0.08	0.06
Share of Missing Students Identified as Exceptional	0.25	0.13	0.29	0.15
Share Included in Analysis Sample	0.73	0.70	0.95	0.91

Notes: based on standardized tests administered in 1998-2005 to students in Grades 3 and 6.

Figure 1: Public and Separate Schools, West of Toronto

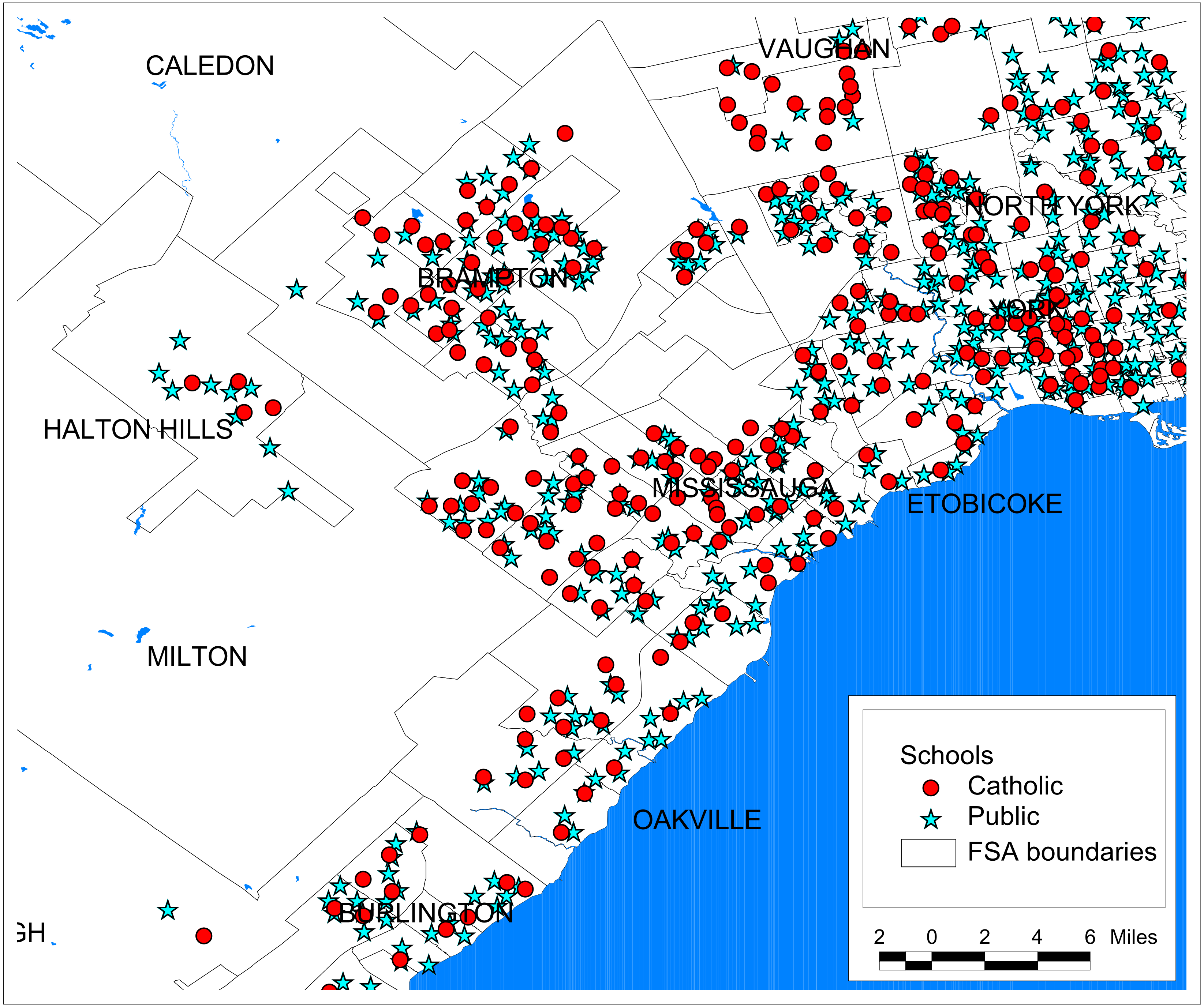


Table 1: Summary Statistics on Opening and Closing Schools

	Total Number of Events (1)	Share of Events 1998 or Later (2)	Share of Events with Nearby Non-Rural School(s) (3)	Mean Enrollment (Schools with Nearby Non-Rural Schools) (4)
Public Opening	252	0.599	0.631	303.9
Separate Opening	169	0.604	0.633	315.5
Public Closing	212	0.717	0.458	228.9
Separate Closing	102	0.765	0.775	247.4

Note: see text for definitions. Enrollment measure used in columns 4-5 is maximum combined enrollment in grades 1-6 observed at opened or closed school during sample period.

Table 2: Summary Statistics on Enrollment Growth Measures

	Public Schools	Separate Schools
Enrollment in Grade 1	50.4	44.3
(Standard Deviation)	(23.3)	(21.0)
Number of School-year Observations	7,554	5,290
Total Enrollment in Grades 1-6	284.8	262.4
(Standard Deviation)	(117.4)	(109.1)
Number of School-year Observations	7,746	5,332
Proportional Change in Grade 1 Enrollment from Previous Year to Current Year (×100)	1.87	2.38
(Standard Deviation)	(26.41)	(30.54)
Number of School-year Observations	6,994	4,893
Proportional Change in Enrollment from Grades 1-5 Previous Year to Grades 2-6 Current Year (×100)	0.13	0.42
(Standard Deviation)	(17.35)	(12.49)
Number of School-year Observations	7,067	4,940

Note: sample includes school-year observations for non-rural elementary schools affected by at least one opening or closing of nearby school over the sample period (1990-2004).

Table 3: Enrollment Growth Models

	Enrollment Growth Measure:						
	Change in 1st Grade Enrollment	Change in Enrollment from Grades 1-5 in Year (t-1) to Grades 2-6 in Year (t)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own-System Effects:							
Effect of Public Opening on Nearby Public School	-7.8	-6.3	-2.9	-6.3	-6.3	-2.9	-2.9
	(1.3)	(0.7)	(1.2)	(0.7)	(0.7)	(1.2)	(1.2)
…Interacted with Share New Housing Stock in FSA	–	–	-13.2	–	–	-12.8	-12.8
			(4.1)			(4.1)	(4.1)
Effect of Separate Opening on Nearby Separate School	-9.7	-9.6	-9.0	-9.5	-9.5	-9.1	-9.1
	(2.0)	(1.3)	(2.3)	(1.3)	(1.3)	(2.3)	(2.3)
...Interacted with Share New Housing Stock in FSA	–	–	-1.4	–	–	-1.0	-1.1
			(5.9)			(5.9)	(5.9)
Cross-System Effects:							
Effect of Public Opening on Nearby Separate School	-2.0	-1.1	1.1	0.1	–	–	–
	(1.3)	(0.6)	(0.9)	(2.2)			
...Interacted with Share New Housing Stock in FSA	–	–	-7.4	–	–	–	–
			(2.5)				
...Interacted with Share of Catholics in FSA	–	–		-3.2	-3.0	2.3	–
				(5.9)	(1.7)	(2.5)	
...Interacted with share of Catholics in FSA × Share New Housing Stock in FSA	–	–	–	–	–	-15.6	-11.1
						(6.8)	(4.4)
Effect of Separate Opening on Nearby Public School	-3.3	-2.9	1.7	-3.0	–	–	–
	(1.5)	(0.9)	(2.0)	(4.2)			
...Interacted with Share New Housing Stock in FSA	–	–	-14.2	–	–	–	–
			(7.3)				
...Interacted with Share of Catholics in FSA	–	–	–	0.4	-7.7	7.5	–
				(12.0)	(2.5)	(5.7)	
...Interacted with share of Catholics in FSA × Share New Housing Stock in FSA	–	–	–	–	–	-42.9	-27.4
						(19.0)	(9.2)
Number of observations	11,887	12,007	12,007	12,007	12,007	12,007	12,007
Number of schools	939	945	945	945	945	945	945

Note: standard errors in parentheses. All models also include the following controls: fixed effects for schools and sample year; indicators for being affected by a nearby closing of a school in the same system or the competing system; a dummy for being paired with another school for administrative purposes; and the following characteristics of the local area (matched by FSA -- see text): share of enrolled students in the FSA attending public French and private schools, total population in the FSA and shares of population ages 5-9 and 10-14, fraction of FSA residents who are Catholic, fraction who are immigrants, fractions of FSA residents of East Asian, South Asian, and Northern, Southern, and Eastern European ancentry, fraction of population with a university degree, fraction with no high school degree, fraction of single-headed families, fraction of families with 2 or 3 kids, and fraction of adults with home language other than English. British or French ancentry treated as equivalent to "Canadian". Eastern European ancentry groups includes countries formerly affiliated with the U.S.S.R.

Table 4: Summary Statistics for EQAO Test Takers

	Public Schools		Separate Schools	
	Grade 3	Grade 6	Grade 3	Grade 6
	(1)	(2)	(3)	(4)
Test-Taker Characteristics for Students with At Least One Test Score:				
Number of observations	323,508	340,259	164,502	172,409
Share Female	0.49	0.49	0.49	0.49
Share ESL Students	0.06	0.05	0.02	0.02
Share Exceptional (Special Needs) Students	0.04	0.04	0.03	0.03
Share Attended Kindergarden	0.86	0.73	0.89	0.75
Share French Immersion Students	0.09	0.06	0.04	0.04
Share Gifted Students	0.01	0.02	0.01	0.01
Share with Scores for All Three Tests	0.64	0.65	0.87	0.86
Reading Tests				
Number of observations	212,761	229,650	147,721	152,638
Average Score (1-4 Scale)	2.52	2.68	2.52	2.70
(standard deviation)	(0.76)	(0.74)	(0.75)	(0.73)
Share of Students with Missing Score	0.13	0.09	0.10	0.07
Mathematics Tests				
Number of observations	230,562	238,153	153,117	154,042
Average Score (1-4 Scale)	2.73	2.71	2.68	2.68
(standard deviation)	(0.75)	(0.80)	(0.73)	(0.79)
Share of Students with Missing Score	0.09	0.08	0.08	0.06
Writing Tests				
Number of observations	219,835	233,759	151,622	154,773
Average Score (1-4 Scale)	2.66	2.67	2.69	2.71
(standard deviation)	(0.66)	(0.71)	(0.65)	(0.69)
Share of Students with Missing Score	0.10	0.07	0.08	0.06

Notes: based on standardized tests administered in 1998-2005 to students in Grades 3 and 6. Student observations are included for school-cohorts that have at least 10 test takers in grade 3 and 3 years later in grade 6, with the ratio of the number of test takers in grade 6 to the number in grade 3 between 0.71 and 1.40.

Table 5: Relationship Between Religion and Education/Earnings Among Ontario Parents

	Dependent Variable = Years Education				Dependent Variable = Log Weekly Wages			
	Mothers		Fathers		Mothers		Fathers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Catholic	0.13	-0.01	0.01	-0.06	0.04	0.02	0.03	0.00
	(0.07)	(0.04)	(0.03)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)
Other Religion (not Catholic or Protestant)	–	-0.20	–	0.08	–	-0.07	–	-0.08
		(0.06)		(0.07)		(0.02)		(0.02)
No Religion	–	-0.44	–	-0.32	–	-0.04	–	-0.06
		(0.05)		(0.05)		(0.02)		(0.01)
Dummies for Country of Origin and Metro Area (CMA)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Variable [standard deviation]	14.16	14.16	14.20	14.20	6.21	6.21	6.75	6.75
	[2.78]	[2.78]	[2.94]	[2.94]	[0.83]	[0.83]	[0.74]	[0.74]
Number of Observations	31,744	31,744	27,988	27,988	22,652	22,652	23,467	23,467

Notes: Sample includes household heads and spouses age 24-64 in households with at least one child age 15 or less living in the province of Ontario in 2001 Canadian Census. Dependent variable in columns 1-4 is estimated years of completed education. Dependent variable in columns 5-8 is log average weekly wage last year. For specifications in odd-numbered columns omitted religious group is all non-Catholics (including those with no religion). For specifications in even-numbered columns omitted religious group is Protestants. Models are fit by unweighted OLS. Standard errors in parentheses.

Table 6: Models for 8th Grade Test Scores and Test Score Gains from 8th to 10th Grade, Students in NELS-88

	All Students				Students in Public Schools in 8th Grade			
	8th Grade Tests		Test Gain: 10th-8th		8th Grade Tests		Test Gain: 10th-8th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Catholic	120.5	33.9	37.4	17.0	102.0	14.4	35.9	14.7
	(18.5)	(20.9)	(10.5)	(11.8)	(21.9)	(24.2)	(12.3)	(13.7)
Other Religion (Not Protestant or Catholic)	–	-39.0	–	-25.3	–	-22.3	–	-26.4
		(22.4)		(12.7)		(24.2)		(13.6)
No Religion	–	-57.2	–	-41.3	–	-41.4	–	-40.8
		(29.3)		(16.6)		(30.7)		(17.3)
Religion Not Reported	–	-365.7	–	-68.9	–	-389.0	–	-66.5
		(23.8)		(16.3)		(25.2)		(17.1)
Mean of Dep. Variable	5081.1		-33.3		4973.6		-38.1	
[standard deviation]	[988.5]		[472.4]		[979.4]		[470.1]	
Number of Observations	13,315		12,037		11,078		9,970	

Notes: Dependent variable in columns 1-2 and 5-6 is composite test score (math and reading) in 8th grade. Dependent variable in columns 3-4 and 7-8 is change in composite test score from 8th to 10th grade. Models are fit by unweighted OLS and do not account for NELS sample design. For specifications in odd-numbered columns, omitted religious group is all other religions (including none and not reported). For specification in even-numbered columns, omitted religious group is any Protestant religion. All models include the following additional controls: dummies for gender, race/ethnicity, urban location and Census division; mother's and father's education (with allocated data for missing cases, and dummies for allocated status); dummy for living with mother and father at 8th grade, and dummies for SES quartile. Standard errors in parentheses.

Table 7: Test Score Growth Models

Dependent Variable: Individual Test Score (4-point scale from 1 to 4, std. dev. ≈ 0.75)	Reading (1)	(2)	(3)	(4)	Mathematics (5)	(6)	(7)	(8)	Writing (9)	(10)	(11)	(12)
Local Competition Measures:												
Share of Catholics × Grade 6	0.15	0.11	–	–	0.23	0.17	–	–	0.13	0.10	–	–
	(0.05)	(0.06)			(0.07)	(0.08)			(0.04)	(0.05)		
Share of Catholics × Share New Housing Stock × Grade 6	–	0.27	0.41	0.34		0.42	0.63	0.46		0.24	0.35	0.26
		(0.16)	(0.14)	(0.15)		(0.23)	(0.20)	(0.21)		(0.12)	(0.11)	(0.12)
Other Controls:												
Share with No Religion × Grade 6	–	–	–	-0.20	–	–	–	-0.53	–	–	–	-0.26
				(0.11)				(0.16)				(0.09)
Share with Other Religions × Grade 6	–	–	–	0.06	–	–	–	0.21	–	–	–	0.06
				(0.07)				(0.10)				(0.05)
Dummy = 1 if Test Taker in Grade 6	0.005	0.02	0.07	0.10	-0.25	-0.23	-0.15	-0.07	-0.12	-0.11	-0.07	-0.03
	(0.04)	(0.04)	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.03)	(0.03)	(0.02)	(0.03)
Separate School × Grade 6	0.02	0.02	0.03	0.03	0.02	0.02	0.03	0.02	0.02	0.02	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Share of New Housing Stock × Grade 6	-0.001	-0.10	-0.15	-0.14	0.06	-0.10	-0.18	-0.15	0.02	-0.07	-0.12	-0.10
	(0.02)	(0.06)	(0.06)	(0.06)	(0.03)	(0.09)	(0.08)	(0.09)	(0.02)	(0.05)	(0.05)	(0.05)
R-Squared	0.16	0.16	0.16	0.16	0.18	0.18	0.18	0.18	0.16	0.16	0.16	0.16
Number of Observations	742,770				775,874				759,989			

Notes: standard errors (clustered by school) are reported in parentheses. Dependent variable is student level test score in reading (columns 1-4) mathematics (columns 5-8) or writing (columns 9-12) in grade 3 or grade 6. All models include individual-level controls, controls for the average characteristics of the students in each school-cohort, and FSA-level controls, as well as school-cohort fixed effects. See text for description of additional controls.

Table 8: Alternative Specifications for Test Score Growth Models

Dependent Variable: Individual Test Score (4-point scale from 1 to 4, std. dev. ≈ 0.75)	Reading			Mathematics			Writing		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Local Competition Measures:									
Share of Catholics × Share New Housing Stock × Grade 6	0.34	0.32		0.46	0.43		0.26	0.24	
	(0.15)	(0.15)		(0.21)	(0.22)		(0.12)	(0.12)	
Share of Catholics × Share New Housing Stock × Grade 6 × Public School			0.48			0.62			0.31
			(0.18)			(0.27)			(0.14)
Share of Catholics × Share New Housing Stock × Grade 6 × Separate School			0.33			0.44			0.25
			(0.15)			(0.22)			(0.12)
Selection Correction Term:									
Local Enrollment Rate in English-speaking Publicly funded Schools		-0.06	-0.06		-0.08	-0.08		-0.05	-0.05
		(0.04)	(0.04)		(0.05)	(0.05)		(0.03)	(0.03)
Controls for Shares of Other Religious Groups:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.16	0.16	0.16	0.18	0.18	0.18	0.16	0.16	0.16
Number of Observations		742,770			775,874			759,989	

Notes: standard errors (clustered by school) are reported in parentheses. See notes to Table 7. All models include individual-level controls, controls for the average characteristics of the students in each school-cohort, and FSA-level controls, as well as school-cohort fixed effects.