How do school 'report cards' affect school choice decisions?

Jane Friesen, Department of Economics, Simon Fraser University

Mohsen Javdani, Simon Fraser University

Justin Smith,Wilfrid Laurier University

Simon Woodcock, Simon Fraser University

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Abstract

We estimate the effect of information about school achievement that is disseminated to the public through websites and school "report cards" on school choice decisions. We find that students are more likely to leave their school when public information reveals poor school-level performance. Some parents' school choice decisions respond to information soon after it becomes available. Others, including non-English-speaking parents, alter their school choice decisions only in response to information that has been disseminated widely and discussed in the media. Parents in low-income neighborhoods are most likely to alter their school choice decisions in response to new information.

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Corresponding author: Jane Friesen, friesen@sfu.ca, 1-778-782-3403

1 Introduction

Economists have long argued that policies designed to increase competition in markets for education can improve educational outcomes by increasing disadvantaged students' access to high quality schools, and by causing underperforming schools to become more effective or to shrink as families "vote with their feet" (Friedman 1955, Becker 1995, Hoxby 2003). Recent evidence shows that providing information about school-level achievement directly to parents can influence school choice (Hastings and Weinstein 2008). However, it is unknown whether publicly disseminating information about school achievement through the media has the same effect.[1] On the one hand, widespread dissemination has the potential to influence the choices of many parents, and may therefore substantially increase the effectiveness of school choice policies. However, a large increase in the demand for high-achieving schools will not increase competitive pressure on weaker schools unless preferred schools can actually accommodate more students. Furthermore, children whose parents have poor access to media, or who are not part of well-informed social networks, may not benefit from public dissemination strategies. In addition, if school achievement measures are subject to substantial sampling variation, then parents could be misled or confused when education authorities update public information about achievement.

Our study addresses these issues by examining the effect of public information about school achievement on school choice behavior in British Columbia (B.C.). Our estimates are based on student-level longitudinal data for multiple cohorts of students that span the introduction of standardized testing and the subsequent wide dissemination of school-level results. We study the propensity of elementary school students to leave their school in response to new information about school-level performance on those standardized tests. We also investigate whether the response to information about school achievement differs among parents who may face higher costs of accessing the information, such as those with low income or those who do not speak English at home.

School-level achievement measures may be correlated with unmeasured characteristics of schools that influence parents' beliefs about school quality and affect student mobility. We identify parents' response to new information using two separate identification strategies that exploit the timing of testing and the release of test results. The first is a difference-in-differences approach that controls for unobserved factors that jointly determine mobility and test scores by comparing the relationship between mobility and lagged test scores before and after the public release of information about test scores. The second is a control function approach (Navarro 2008) that uses current cohort test scores to control for unobserved factors that jointly determine mobility and test scores. Because test scores are not publicly revealed until the school year following the exam, the current cohort's exam results are a valid control for such unobservables, and new information about the achievement of previous cohorts is a conditionally exogenous shock to parent's information about school quality.

We find that publicly disseminated information about school-level achievement has a substantial effect on the inter-school mobility of some public school students. In general, students are more likely to leave their school when they learn that their schoolmates have performed relatively poorly. Families that speak English at home respond strongly to early information releases, and continue to respond to subsequent releases. Families that speak a language other than English respond only to the later, more highly publicized information releases. The response is most pronounced among English-speaking families in low-income neighborhoods. Arguably, these families may have had poor private information about school quality, and hence valued the new public information more highly than families in higher-income neighborhoods. The delayed response of non-English speakers suggests they face high costs of accessing public information.

1.1 Previous literature

Hastings and Weinstein (2008) find that parents of children attending low-achieving schools in a North Carolina school district were more likely to enroll their child in a higher-achieving school when the district provided them with information about school achievement. They also find that simplified information sheets distributed randomly to parents in low- and middle-income schools doubled the estimated preference parameter on school test scores in a school choice model. Using data from the same school district, Hastings et al. (2009) find that test scores play a small role in parents' school choice decisions relative to travel distance and peer composition, and that parents' preferences vary substantially with characteristics such as income.

Unlike these studies, we focus on information that is disseminated to all parents, at all schools, through public media. Public information may have different effects on school choice behavior compared to the private information strategies studied previously, for several reasons. First, newly informed parents, especially those of disadvantaged children, may face less competition for spaces in preferred schools when they are part of a smaller, targeted group. Second, parents of children who attend low-achieving schools may respond differently to new information than those of the broader student population. Third, media dissemination may be a less effective way to inform disadvantaged parents compared to direct communication from schools.

An alternative method for learning about the effect of public information about school-level achievement on school choice is through its effect on housing prices.[2] Figlio and Lucas (2004) and Fiva and Kirkebøen (2010) find that public information about school-level achievement is capitalized into housing prices in Florida and Oslo respectively, but the effect diminishes quickly over time. Kane et al. (2003) find that while housing prices reflect long-run average school-level test scores, they do not respond to year-to-year fluctuations in a given measure of school quality or to the introduction of newly framed test score information.

Housing price studies only capture the effects of information about school achievement on school choice decisions that operate through residential choice. However, the link between residential and school choice decisions in many jurisdictions is weakened by the availability of private schools, charter and magnet schools and/or open enrolment policies. To the extent that information affects school choice decisions along these margins, it will not be reflected in housing prices. Moreover, housing price studies reveal little about the characteristics of the families whose decisions are affected.

Several studies examine the direct effect of public information about achievement on school choice decisions. Mizala and Urquiola (2008) find that, when measures of school achievement are already widely available, receiving a highly publicized *SNED* award has no effect on enrollment levels, tuition fees, or socioeconomic composition of Chilean schools. Hussain (2007) finds that enrollment falls by up to 6 percent in the three years after English schools receive a public "fail" rating, while enrollment increases by up to 2 percent in schools rated "very good."

Finally, publicly disseminated information in the form of "report cards" or rankings like those examined here has been shown to affect consumer decisions in other markets. These markets include health services (Dranove et al. 2003, Dafny and Dranove 2008, Jin and Sorensen 2006) and restaurant patronage (Jin and Leslie 2003).

2 Institutional Background

2.1 School access and funding in B.C.

As in many other jurisdictions, B.C. students are guaranteed access to their neighborhood "catchment" public school. B.C.'s provincial education authority (the Ministry of Education) instituted an official "open boundaries" policy in July 2002 that allows students to attend any public school that has space and facilities available after catchment area students have enrolled.

Provincial legislation requires that school boards give priority to students who reside within the district; boards may elect to give priority to children whose siblings are already enrolled, and must establish policies for allocating spaces among students within a priority category. Entry into most public magnet programs is restricted to students entering Kindergarten or Grade 1, and space in popular programs is often allocated by lottery. Finally, students may choose a private school.

Along with capital funding, the B.C. Ministry of Education provides districts with operating funds in proportion to total district enrolment. Supplementary funding is provided for each student who is Aboriginal, is gifted or disabled, or who qualifies for English as a Second Language (ESL) instruction. Public districts are not authorized to raise their own revenue. Private schools receive per-student operating grants of up to 50% of the base public school rate, and are responsible for teaching the provincial curriculum and meeting various provincial administrative requirements (B.C. Ministry of Education 2005).

2.2 Testing and information

Prior to 1999, the BC Ministry of Education administered standardized Provincial Learning Assessment Program (PLAP) exams, in various subject areas on a rotating schedule, to students in grades 4, 7, and 10. These were replaced by standardized Foundation Skills Assessment (FSA) exams in the 1999/2000 school year. The FSAs are administered in the spring of each year to students in grades 4, 7, and 10 in reading comprehension, writing, and numeracy.[3] Neither the PLAP or FSA exams has any academic consequences, and teachers and schools face no financial incentives related to students' exam performance.

PLAP exam results were never disseminated to parents or the public. The Ministry of Education first released individual and provincial, district, and school-level FSA exam results to schools in fall 2000 (based on the 1999/2000 exam). Schools were instructed to share this information with parents upon request (B.C. Ministry of Education 2000). School-level results of the 1999/2000 and 2000/2001 FSA exams were first posted on the Ministry's website in October 2001 (B.C. Ministry of Education 2001). Subsequent exam results have been posted on the Ministry's website in the following fall of each year. Since 2003, schools have been required to share individual students' exam results with parents prior to September 30. Note that in each case, FSA results are released in the school year following the year of the exam.

The Fraser Institute, an independent research and educational organization (Fraser Institute 2008),[4] began issuing annual "report cards" on B.C.'s elementary schools in June 2003 (Cowley and Easton 2003).[5] These include school scores and rankings based on FSA results. From the outset, the school report cards have received widespread media coverage in the province's print, radio and television media.[6]

3 The effect of new information on school choice

3.1 Basic model

We present a highly simplified model of school choice that accounts for uncertainty about school quality and focuses attention on the effects of new information.[7] Assume parent *i*'s utility (U_{is}) depends on the quality (q_s) of their child's school *s*,

$$U_{is} = q_s + \varepsilon_{is} \tag{1}$$

where ε_{is} is a random taste-shifter with mean zero. We interpret q_s as an index of school characteristics that determine parents' utility, such as teacher experience, peer ability, the state of technology at the school, the quality of sports programs, and class size.

We assume that parents cannot perfectly observe q_s. They consequently form beliefs about each school's quality based on directly observable school characteristics X_s, such as neighbourhood income and the demographic composition of the student body. Their prior beliefs are normally distributed with mean $X_s{}'\beta$ and precision h_{qi}, where β is the vector of weights given to observable characteristics. Parents are assumed to know h_{qi}. Although prior precision is the same for all schools, it varies between parents to reflect the idea that some (e.g., new immigrants) may have less precise beliefs about school quality than others (e.g., a native born individual who has lived in the area for many years).

Absent any additional information and given a set of available schools Σ, a parent chooses school s if

(2) $$E\left[U_{is} \mid X_s'\beta\right] - c_{is} > E\left[U_{ik} \mid X_k'\beta\right] - c_{ik} \quad \forall k \in \Sigma, k \neq s$$

where c_{is} reflects the direct (e.g., tuition) and indirect (e.g., commuting distance) costs of attending school s. Given our assumed prior beliefs and utility function, this implies that the average parent chooses school s if

(3) $$X_s'\beta - c_{is} > X_k'\beta - c_{ik} \quad \forall k \in \Sigma, k \neq s.$$

Now suppose that parents also observe a noisy signal S_s of each school's quality, such as standardized test scores aggregated to the school level,

(4) $$S_s = q_s + \eta_s$$

where $\eta_s \sim N\left(0, h_{\eta i}^{-1}\right)$. The noise component η_s has zero mean, implying that test scores provide unbiased information about school quality. The precision of test scores as a signal of school quality, $h_{\eta i}$, is known but varies across parents. This reflects the possibility that test scores are more informative signals of school quality for some parents than others.

Parents are assumed to update their expectations about each school's quality using Bayes' rule.[8] Their posterior beliefs are normally distributed with mean m_s and precision $h_{qi} + h_{\eta i}$, where

(5) $$m_s \equiv E\left[q_s \mid X_s'\beta, S_s\right] = \frac{h_{qi}}{h_{\eta i} + h_{qi}} X_s'\beta + \frac{h_{\eta i}}{h_{\eta i} + h_{qi}} S_s.$$

Parents' updated expectation of school quality is a precision-weighted average of the signal and their prior expectation. The greater is the precision of test scores relative to prior information, the greater is the weight that parents will place on test scores. If test scores are very noisy, prior information will continue to dominate parents' beliefs about school quality.

Defining $\theta_i = h_{\eta i}\left(h_{\eta i} + h_{qi}\right)^{-1}$ as the weight that parents place on test scores, and rearranging equation (5), we can write the average parent's expected utility from choosing school *s* as

(6) $$E\left[U_{is} \mid X_s'\beta, S_s\right] = \theta_i\left(S_s - X_s'\beta\right) + X_s'\beta.$$

Define $S_s^* = \left(S_s - X_s'\beta\right)$ to be the test score "shock," which represents the new information acquired from the signal. Parents will choose to enroll their child in school *s* if

(7) $$\theta_i\left(S_s^* - S_k^*\right) > \left(X_k' - X_s'\right)\beta - \left(c_{ik} - c_{is}\right) \quad \forall k \in \Sigma, k \neq s.$$

Parents choose school *s* if the information shock is sufficiently good relative to other schools that it outweighs any relative differences in the schools' other characteristics and attendance costs. Conversely, parents will choose school *s* even if they receive a relatively poor information shock, provided its characteristics are good enough and attendance costs low enough to outweigh the "bad news" about school-level test scores. Note also that test score information only affects school choice if it is sufficiently precise to be useful ($\theta_i > 0$). Furthermore, θ_i varies across individuals because of interpersonal differences in the precision of prior beliefs, and differences in the precision of test scores as a signal of school quality.

3.2 Model Dynamics

We now extend the model to accommodate a sequence of signals rather than a one-time event. Suppose that in each period T, parents observe an unbiased signal of school quality S_{sT} that conforms to equation (4), such as the results of an annual standardized test that are revealed to the public in each school year. We define parents' beliefs about school quality recursively. After observing T signals, parents' beliefs are normally distributed with mean m_{sT} and precision $h_{qi} + Th_{\eta i}$, where

$$
\begin{aligned}
m_{sT} = E\left[q_s \mid X_s'\beta, S_{s1}, S_{s2}, \ldots, S_{sT}\right] &= \frac{h_{qi} + (T-1)h_{\eta i}}{h_{qi} + Th_{\eta i}} m_{sT-1} + \frac{h_{\eta i}}{h_{qi} + Th_{\eta i}} S_{sT} \\
&= \frac{h_{qi}}{h_{qi} + Th_{\eta i}} X_s'\beta + \frac{Th_{\eta i}}{h_{qi} + Th_{\eta i}} \bar{S}_{sT}
\end{aligned} \tag{8}
$$

and $\bar{S}_{sT} = T^{-1}\sum_{t=1}^{T} S_{st}$ is the average of all observed signals.

From the first line of equation (8) we see that, as in the case of a single signal, parents' revised expectations about school quality are a precision-weighted average of the new signal (S_{sT}) and expected quality prior to observing the signal (m_{sT-1}). We also see that each new signal receives successively smaller weight in parents' Bayesian update, because the previous $T - 1$ signals have already contributed to the precision of their beliefs, and consequently increased the weight assigned to m_{sT-1}. From the second line of equation (8), however, we see that the combined weight assigned to the average of all signals ($T\theta_{iT}$, where $\theta_{iT} = h_{\eta i}\left(h_{qi} + Th_{\eta i}\right)^{-1}$) increases with T, and the weight assigned to observable school characteristics X_s consequently decreases.

Defining $S_{sT}^{**} = \left(S_{sT} - m_{sT-1}\right)$ to be the information shock embodied in the new signal, and rearranging the first line of equation (8), parents choose school s over school k if

(9) $$\theta_{iT}\left(S_{sT}^{**}-S_{kT}^{**}\right)>\left(m_{kT-1}-m_{sT-1}\right)-\left(c_{ik}-c_{is}\right).$$

The intuition behind this maximization condition is the same as before: parents choose school *s* if the information shock contains sufficient "good news" about school quality relative to what was previously believed and attendance costs.

4 Methodology

4.1 Empirical Model

For tractability and because of data limitations, we treat residential location as exogenous and examine inter-school mobility conditional on residential choice. Specifically, we model the probability that a student separates from their current school after September 30 of year *t* and enrolls in a new school before September 30 of year *t*+1, conditional on their residential location. In terms of our theoretical model, students will separate from their current school *s* if some alternative school satisfies equation (9).

The fundamental identification issue is that school-level achievement measures may be correlated with unmeasured characteristics of schools that influence parents' beliefs about school quality and affect student mobility. Our first estimator addresses this problem by comparing the relationship between lagged school-average test scores and the probability of separating from the current school, conditional on observable characteristics, before and after lagged test scores were first released to the public in fall 2000. Under some identifying assumptions described below, this strategy allows us to estimate the effect of the 1999/2000 test score information on separations in the year it was released.[9] The estimating equation takes the form

(10)

$$y_{ist}=\alpha_0+\alpha_1 S_{st-1}+\alpha_2 d_t^{t=2000}+\alpha_3 S_{St-1}d_t^{t=2000}+Z_{it}'\gamma+X_{st}'\beta+C'_{ist}\delta+\psi_s+\upsilon_{st}+\varepsilon_{ist}$$

where y_{ist} is a binary variable indicating whether student *i* separated from school *s* at the end of school year *t*; S_{st-1} is a measure of lagged test scores; $d_t^{t=2000}$ is a binary indicator for the 2000/2001 school year; Z_{it} is a vector of observable student characteristics; X_{st} is a vector of observable school characteristics; C_{ist} is a vector of proxies for the student's cost of attending school *s*; ψ_s is a fixed school effect, υ_{st} is a random school-by-year effect that captures any unmeasured correlation among students' separation behavior in a given school and year;[10] ε_{ist} is a stochastic error term; and $\alpha_0, \alpha_1, \alpha_2, \alpha_3, \gamma, \beta, \delta$ are coefficients to be estimated. The coefficient of interest, α_3, measures how the relationship between lagged test scores and separations differs before and after the release of test score information to parents.

Since FSA testing began in 1999/2000, there is no lagged FSA score from which to identify the baseline relationship between lagged test scores and separations in 1999/2000. Instead, we use the results of the PLAP reading exam as our measure of pre-policy achievement, and use the FSA reading scores as our measure of post-policy achievement. The use of the PLAP results as a proxy for baseline achievement raises two issues. First, although the exams were not identical, they tested similar skills (Raptis and Fleming 2006: 1204), so we expect that their underlying relationship with mobility behavior would be similar. Second, because the PLAP reading test was not administered in 1998/1999, we use the 1997/1998 PLAP reading results as a proxy for the lagged baseline score. Our identifying assumption therefore is that the relationship between the 2000/2001 separations and the 1999/2000 FSA test scores (written one year previously) would have been the same as the relationship between the 1999/2000 separations and the 1997/1998 PLAP test scores (written two years previously), had information about FSA performance not been released.

We estimate a second specification that allows us to identify the effects of a series of information shocks on inter-school mobility, while separately identifying the effect of new information versus the lagged effect of previously released information, and that does not rely on proxy test score measures. This estimator includes the *current* cohort's average FSA score, S_{st}, as a control for unobserved time-varying school characteristics that jointly affect test scores and separations. We estimate the following specification using data from the 1999/2000 through 2003/2004 school years:

(11)

$$y_{ist} = \alpha_0 + \sum_{j=1999}^{2002} \alpha_3^j S_{sj} d_t^{j=t-1} + \sum_{j=1999}^{2002} \alpha_4^j S_{sj} d_t^{j<t-1} + \alpha_5 S_{st} + Z'_{it}\gamma + X'_{st}\beta_t + C'_{ist}\delta + \tau_t + \psi_s + \upsilon_{st} + \varepsilon_{ist}$$

As before, S_{sj} is a school-level aggregate of test scores. Here, however, we have no need for test scores prior to 1999/2000, and therefore all test scores are aggregates of FSA reading and math results. Each test score measure j is interacted with a pair of binary indicator variables: $d_t^{j=t-1}$ equals one when $j = t - 1$ and zero otherwise; similarly $d_t^{j<t-1}$ equals one when $j < t - 1$. We call α_3^j "news" coefficients because they measure how each information release affected students' separation probability *in the year in which that particular information first became available to parents*. Each information release also takes a separate "old news" coefficient α_4^j that measures its effect on separation probabilities in subsequent school years. As discussed in more detail below, S_{st} is the current cohort's FSA score and takes coefficient α_5; and τ_t is a fixed year effect. All other terms are as previously defined. Note, however, that β_t now varies over time.[11] This reflects our theoretical model's prediction that parents will give less weight to observable school characteristics as more test-based information becomes available to them. As with the difference-in-differences estimator, we include fixed school effects that control for any between-school differences in average separation rates that may be correlated with between-school differences in average achievement.

We include the current cohort's FSA score (S_{st}) in equation (11) to control for unmeasured time-varying school characteristics that have persistent effects on student achievement and separation probabilities. Suppose, for example, that a school hires an unusually bad grade 4 teacher in year *t*-1 who produces lower FSA results. If her continuing presence at the school negatively influences parents' beliefs about school quality in year *t,* this *directly* increases students' year *t* separation probability. Absent an adequate control, the bad teacher's independent effect on year *t*-1 FSA scores and year *t* separations will be confounded with parents' year *t* response to information about year *t*-1 FSA scores. It is reasonable to assume, however, that if the bad teacher influences students' separation probability in *t* then she also influences students' FSA performance in *t*. Because year *t* FSA scores are not observed by parents until year *t*+ 1, they are a valid control for the "bad teacher effect" in year *t,* as well as any other school-level unobservables that jointly affect mobility and performance and have persistent effects over time. Controlling for the current cohort's FSA scores (S_{st}) thus allows us to identify parents' response to *information* from lagged test scores, under the identifying assumption that unobserved time-varying factors that influenced previous cohorts' achievement are only correlated with current-year unmeasured heterogeneity in separations (υ_{st} and ε_{ist}) via their persistent effect on achievement. When this assumption is satisfied, lagged FSA scores are exogenous in the separation equation conditional on current FSA scores. The formal proof of identification is provided in an Appendix.

If parents at low-achieving schools were more constrained by neighborhood enrolment policies than parents in high-achieving schools, then separations from low-achieving schools might have increased relative to separations from high-achieving schools when the open boundaries enrolment policy took effect in 2002/2003. We therefore allow the coefficient on S_{st} to differ before and after 2002/2003. Under our maintained assumption that unobserved time-varying factors that influenced previous cohorts' achievement are only correlated with υ_{st} and ε_{ist} via their persistent effect on achievement, this identifies the effect of information released in 2002/2003 separately from the effect of the change in enrolment policy.

4.2 Data

Our investigation focuses on B.C.'s Lower Mainland region, a large metropolitan area with a population of approximately 2.5 million that includes the city of Vancouver and its suburbs. It encompasses fourteen public school districts with a total annual enrollment of roughly 375,000 students in Kindergarten through Grade 12 (B.C. Ministry of Education 2007:8).[12]

Our student-level data are based on two administrative databases, integrated via a unique student identifier: an enrollment database (collected for each student on September 30 of each year), and an FSA exam database. Our analysis is based on an extract of the enrollment database that includes all students in the Lower Mainland who entered Kindergarten between 1994/1995 and 2003/2004. We restrict our analysis to public school students who made regular progress through the grades and remained in the Lower Mainland through grade 5. Our regression estimates are based on the subset of this population enrolled in grade 4, because the FSA exam is administered in this grade.[13,14] We create our indicator of separations, y_{ist}, by comparing the school at which the student was enrolled on September 30 of their grade 4 year and the school at which they were enrolled on September 30 of the following year. Because FSA results are released in October, y_{ist} measures separations during or at the completion of the grade 4 year, following the release of FSA results.

We augment these data with: (1) school-by-grade average student characteristics; (2) selected characteristics of each student's neighborhood as measured in the Census of Population; (3)

school-average 1997/98 PLAP reading scores; (4) annual Fraser Institute school scores and rankings for the 1999/2000 through 2003/2004 school years, and a three-year average score released in 2003; and (5) geographic coordinates associated with each school's postal code and each student's residential postal code.[15]

The complete set of information shocks and the variables we use to capture them in our control function specification are summarized in Table 1. In each case, these variables are based on school-average performance on the FSA reading and numeracy exams. The first set of FSA results was released by the Ministry at the beginning of the 2000/2001 school year, and a new set was released in each subsequent year. The Fraser Institute released their first scores and rankings in June 2003, based on the FSA exams written in 1999/2000, 2000/2001 and 2001/2002. That release included overall scores (out of 10) for each school in each of the three years, the three-year average score (also out of 10), and school rankings based on the 2001/2002 score and the three-year average score.[16] The three-year average was arguably the most salient measure, since schools were ordered on this measure in the ranking published in local newspapers.[17] The Fraser Institute released an additional set of scores and rankings based on the 2002/2003 FSA exams in spring 2004. We normalize all information shock variables in Table 1 to have mean zero and variance one over schools in each year.

The specific control variables included in our regressions are listed in the table notes and described further in a Technical Appendix available from the CJE online archive at cje.economics.ca..

5 Results

5.1 Descriptive statistics

Our estimation sample consists of 65,180 students who attend 361 public elementary schools.[22] We report sample means and mean separation rates for some key characteristics in Table 2 (sample means for all control variables are reported in Table OA1 in the Online Appendix). Almost five percent of students self-report as Aboriginal, and these students have significantly higher separation rates than average (18.4% vs. 8.9%). Almost one-third of students speak a language at home other than English, and overall these students have a higher than average separation rate. Only a small fraction (3.6%) has been diagnosed with disabilities at this early stage of their education and approximately 7% of students attend a French Immersion program. The separation rate of disabled students is higher than average (13.6%) and that of French Immersion students is lower than average (6.4%).

Table 3 shows school separation rates by grade for the five Kindergarten cohorts that we are able to follow through grade 5. Over 60% of students remain in the same school throughout these grades, and about 30% separate once. The remaining students experience multiple separations between Kindergarten and grade 5. The separation rate is highest following Kindergarten, but is still fairly high (between 8% and 10%) following grade 4.

5.2. Econometric estimates

Our theoretical model contains a number of testable implications. Most fundamentally, when parents observe new information about school quality, they may alter their original school choice if the information shock makes an alternative school appear more attractive. For this to occur, the shock must provide new information about school quality rather than simply confirming what they already know, and the signal cannot be so noisy relative to prior information that parents ignore it.

5.2.1 Difference-in-differences estimates of response to first information shock

We begin by investigating parents' response to the first public release of information about school-level achievement. On the one hand, we expect parents to respond most strongly to this first shock, since our model predicts that parents' response to new releases of test score information gets weaker over time as information accumulates and their beliefs about school quality become increasingly precise. On the other hand, this first information release was not as widely publicized as subsequent releases, so its effect may have been muted if some parents did not absorb and act on this information.

Selected coefficient estimates from our difference-in-differences estimator (eq. 10) are presented in Table 4.[18] Column 1 presents estimates for the full sample of grade 4 students in 1999/2000 and 2000/2001. The estimated coefficient of interest (the lagged test score interacted with an indicator for the first year test score information was publicly released) is negative. This indicates that students' separation probability declined at public schools that received better news (higher school-average test scores), relative to public schools where the news was worse. However the estimate is small and imprecise and we cannot reject the null of no effect at conventional levels.

The remaining columns of Table 4 explore possible heterogeneity in parents' response to the first information release. In columns 2 and 3, we divide the sample into students who report speaking English at home and those who report speaking another language. Language barriers may impede some parents' access to information, reducing the precision of test scores as signals of school quality.[19] Indeed, we find no evidence that non-English speakers responded to the first information release. In contrast, we observe a substantive and statistically significant response among parents who speak English at home: all else equal, a one standard deviation improvement in a school's average test score reduced students' separation probability by a full percentage point on a baseline separation rate of 8.3%.

In the remaining columns of Table 4, we investigate how parents' responses to the first information release varied by neighborhood income. Parents who live in disadvantaged communities may have relatively poor access to private information (i.e., imprecise prior beliefs) or fewer school choice opportunities, both of which could mediate their response to information about school achievement. We consequently break out families who reside in Census EA/DAs in the top (richest) and bottom quartiles of the distribution of average household income. The results in columns 4 and 6 suggest that parents in top quartile neighborhoods do not respond to test score information, regardless of home language. Families in high-income neighborhoods may already have had good access to private information, so that the new public information did not cause them to update their beliefs about schools in any meaningful way. In contrast, English-speaking families in low-income neighborhoods responded quite strongly to new information (column 5). We investigate this further below.

5.2.2 Control function estimates

Our estimates of parents' response to the first release of school-average test scores suggest that either this information did not reach certain groups, or the quantity or quality of the news it contained was not sufficient to alter their school choices. Further insight into the factors that shape parents' heterogeneous responses can be gained by investigating how they responded to subsequent releases of test scores, when this information was more widely disseminated. We consequently turn to estimates for the full series of information shocks, based on our control function specification. This specification allows us to distinguish between the lingering effects of previous information releases versus the effect of newly released information. Recall that our theoretical model predicts that even after new information about school quality is observed, "old news" continues to influence parents' beliefs (via m_{sT-1}) and hence their choices.

The first column of Table 5 presents selected coefficient estimates for the full sample of grade 4 students.[20] We restrict the specification to a single information measure in each year: FSA exam scores in the first two years (when these were the only information measures available) and Fraser Institute scores in the latter two years (since these were published in the media, and therefore are arguably more salient).[21] In three of four years, the coefficient estimates are negative, indicating that students' separation probability declined at schools where the news was better. The exception is the second release of Fraser Institute scores, where the coefficient is effectively zero. The point estimates imply that a one standard deviation increase in the 1999/2000 FSA score relative to other schools reduced students' separation probability by 0.9 percentage points. This is almost identical to the corresponding point estimate from the difference-in-differences estimator. The corresponding figure for the 2000/2001 FSA score is 1.1 percentage points, and 3.5 percentage points for the first release of Fraser Institute scores. Only the latter release had any effect beyond the initial year and, as predicted by our model, the "old news" effect was smaller than the "news" effect (-0.027 versus -0.035). The estimated "old news" effects associated with the other information releases are all statistically insignificant, and the point estimates are small.

The coefficient on the current-year mean FSA score is statistically insignificant, indicating no systematic relationship between current-year achievement and separations.[22] The sign of the point estimate on the interaction between the current-year mean FSA score and an indicator for those years that the open enrolment policy was in effect is positive, indicating that if anything open enrolment *increased* separations from high-achieving schools relative to low-achieving schools. Thus we are confident that the estimated negative effect of the first Fraser Institute release, which coincided with the introduction of open enrolment, captures parents' response to the release of the Fraser Institute report cards, and not unobserved heterogeneity that jointly affects achievement and mobility, or changes in behavior associated with the introduction of open enrolment. Furthermore, the statistically significant response to FSA scores released by the Ministry in 2000 and 2001, which predate the introduction of the open boundaries policy, reinforces the impression that some parents were able to respond to new information about school-level achievement even absent official open enrolment policies.

As with the difference-in-differences estimates, the point estimates in columns 2 and 3 indicate that parents of children who speak English at home responded to the first release of FSA exam scores, while non-English speaking parents did not. The magnitude of the point estimate is slightly larger than from the difference-in-differences estimator, and implies that when a school scored one standard deviation higher in the distribution of published school-average FSA scores, students' separation probability declined by 1.3 percentage points. On a base separation rate of about 8.3% per year, this is quite a large response. Parents in this group responded further to the first release of information by the Fraser Institute in 2002. The magnitude of the point estimate implies that when a school scored one standard deviation higher in the distribution of Fraser Institute scores, students' separation probability declined by 3.1 percentage points. Again, this is quite a large response.

As before, parents of children who report speaking a language other than English at home did not respond to the release of FSA scores in 2000/01 and 2001/02. However, they did respond to the release of Fraser Institute scores. The point estimate implies that a one standard deviation increase in the first Fraser Institute score relative to other schools reduced these students' separation probability by 4.6 percentage points. Such a large response suggests poor access to previously released information, rather than resources or preferences, explains these parents' delayed response to information about school-level achievement.

A potential concern is that language barriers may not be the genuine cause of observed heterogeneity in responses. Rather, heterogeneity could be driven by correlates of language, such as income. In Table 6, we present estimates broken out by quartiles of the distribution of neighborhood income. The results follow essentially the same pattern as the difference-in-difference estimates for the first information release. Parents in top quartile neighborhoods, both in the full sample and in the English-speaking sub-sample, do not respond to test score information. In contrast, those in low-income neighborhoods respond strongly. Public information releases evidently contained substantial news for these parents, leading them to update their beliefs about school quality and respond substantively. Non-English speaking parents do not respond to the first or second release of FSA scores by the Ministry, regardless of neighborhood income. However they do respond to subsequent releases of Fraser Institute scores, although the timing of the response differs by neighborhood income. Overall, it seems clear that access to information about test scores, rather than school choice opportunities, preferences or financial resources, is the essential factor determining how parents respond.

We explore the sensitivity of our results to sample composition and specification in Table OA5 in the Online Appendix. These robustness checks show that English-language parents' estimated response to information is not driven by the behavior of Aboriginal parents or parents of French

Immersion students (as shown in Table 2, both of these groups had unconditional separation rates significantly different from the average student); nor are our estimates sensitive to specifying information measures based on schools' Fraser Institute rankings instead of Fraser Institute scores.

Under our identifying assumptions, our reported estimates can be interpreted as causal. We cannot test these identifying assumptions directly. We therefore look for contradictory evidence via two falsification tests for each specification reported in Table 5. In each, we replace our "news" variables with false information measures based on year t +1 and year t test scores respectively, and correspondingly update the "oldnews" variables. Parents could not directly observe these false news measures at the time they were making school choice decisions, and consequently there should be no systematic relationship between them and separations. Estimates, reported in Tables OA6 and OA7 in the Online Appendix, confirm this to be true.

6 Conclusion

We find that the public release of information about school-level achievement had a substantial effect on the inter-school mobility of some public school students in the Lower Mainland of B.C. A substantial proportion of parents appear to revise their beliefs about the relative quality of their child's school in response to this information, and "vote with their feet" by moving their child to a preferred school. This response is observed primarily among parents who reside in low-income neighborhoods, and occurs the first time that school-level achievement measures are placed in the public domain. While both English and non-English language parents respond strongly to public information about school achievement, non-English parents appear to face higher costs of accessing school achievement information. They respond strongly to school achievement information, but only when the media provided widespread coverage to the Fraser Institute's school report cards. These results suggest that high-profile dissemination can play a crucial role in ensuring access to publicly provided information in environments with culturally and linguistically diverse populations.

Jurisdictions that publicize school-level results typically update this information annually, raising concerns that parents may respond to year-to-year fluctuations that are largely noise (Kane and Staiger 2002, Mizala, Romaguera and Urquiola 2007). Our results show that English-speaking parents in low-income neighborhoods respond immediately to the first release of information, and continue to respond to subsequent releases in later years. Our data provide no way to determine whether these ongoing responses are a series of reactions to noisy information updates, or whether they simply reflect the time it takes for information to reach all members of the community. Likewise, the delayed response of non-English-speaking parents suggests substantial heterogeneity in parents' access to public information. Consequently, annual releases of school achievement information that elicit ongoing media coverage may play an important role in communicating that information to all segments of the community, including recent immigrants.

Our results add to a growing body of evidence that information about school-level achievement affects behavior in ways that may have real consequences for educational outcomes. In addition to ensuring that all parents are able to access the information provided, educational authorities should therefore take care to ensure that widely disseminated information brings competitive pressure to bear on schools that are ineffective, rather than on schools that serve disadvantaged populations. As a growing literature attests, designing meaningful measures of school effectiveness continues to be a challenge (Hægeland et al. 2004, Mizala, Romaguera and Urquiola 2007).

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Table 1: Information Shocks and Information Variables

Information	Date	"News" Variables	"Old News" Variabl
1999/2000 cohort school mean FSA exam results released to parents on request	Oct. 2000	(1999 Mean FSA Score)*(Yr=2000)	(1999 Mean FSA Score)*(Yr>2000)
2000/2001 cohort school mean FSA exam results released on Ministry of Education website	Oct. 2001	(2000 Mean FSA Score)*(Yr=2001)	(2000 Mean FSA Score)*(Yr>2001)
2001/2002 cohort school mean FSA exam results released on Ministry of Education website	Oct. 2002	(2001 Mean FSA Score)*(Yr=2002)	(2001 Mean FSA Score)*(Yr>2002)
1999/2000, 2000/2001 and 2001/2002 cohort Fraser Institute (FI) scores and rankings released	June 2003	(1999-2001 Mean FI Score)*(Yr=2002); (2001 FI Score)*(Yr=2002)	(1999-2001 Mean FI Score)*(Yr>2002); (2001 FI Score)*(Yr>2002)
2002/2003 cohort school mean FSA exam results released on Ministry of Education website	Oct. 2003	(2002 Mean FSA Score)*(Yr=2003)	out of sample
2002/2003 cohort Fraser Institute scores and rankings released	June 2004	(2002 FI Score)*(Yr=2003)	out of sample

Sources: see text.

Note: Calendar years in "News" and "Old news" variable names refer to the calendar year in which the school year began. For instance, "2001 FI Score" refers to the Fraser Institute score based on the FSA exam administered in the 2001/2002 school year. This measure was released in June 2003, and hence could first affect separations at the end of the 2002/2003 school year (Yr=2002).

Table 2: Sample Percentages and School Separation Rates

	Sample Percent	Separation Rate
All	100	8.9
Male	50.6	8.9
Non-English Home Language	32.4	10.1
English Home Language	67.6	8.3
English as a Second Language	30.7	10.1
Aboriginal	4.7	18.4
Disabled	3.6	13.6
Attends French Immersion	6.8	6.4

Source: Authors' calculations based on B.C. Ministry of Education enrollment database.

Table 3: Frequency of Separations by Year of Kindergarten Entry

Kindergarten Entry Year	Never Separated Before Grade 5	Separated Once Before Grade 5	Separated After… Kinder-garten	Grade 1	Grade 2	Grade 3	Grade 4
1995	0.610	0.292	0.151	0.120	0.107	0.094	0.091
1996	0.606	0.294	0.165	0.117	0.103	0.099	0.080
1997	0.618	0.292	0.147	0.111	0.096	0.090	0.097
1998	0.617	0.294	0.141	0.106	0.086	0.100	0.099
1999	0.605	0.305	0.138	0.098	0.109	0.103	0.103

Source: Authors' calculations based on B.C. Ministry of Education enrollment database.

Table 4: Difference-in-Differences Estimates of the Effect of Information about School-level Achievement on Separation Probability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full Sample			English		Non-English	
	All	English	Non-English	Top Quartile	Bottom Quartile	Top Quartile	Bottom Quartile
Lagged Score	-0.003	-0.003	-0.001	0.003	-0.009	-0.016	-0.019
	(0.004)	(0.004)	(0.007)	(0.007)	(0.014)	(0.035)	(0.012)
Lagged Score*(Yr=2000)	-0.007	-0.010**	0.003	0.000	-0.024*	-0.010	0.021*
	(0.004)	(0.005)	(0.008)	(0.009)	(0.014)	(0.033)	(0.013)
Number of Observations	26360	18599	7761	6207	3323	995	2709
Number of Schools	361	361	347	254	306	191	238

Source: Authors' calculations based on B.C. Ministry of Education enrollment database and auxiliary data.
Notes: "Lagged score" refers to the school-average 1998 PLAP reading score in 1999; and the school-average 1999/2000 FSA reading score in 2000. All scores are normalized to have mean zero and variance one over schools in each year. Additional control variables in these regressions are: main effects for student characteristics (non-English home language; Aboriginal; enrolled in ESL program; male; disabled; gifted; changed schools prior to Grade 4; enrolled in French Immersion, own FSA reading and numeracy scores, travel distance to school, and the numbers of public and private schools nearby), school proportions of student characteristics (non-English home language; Aboriginal; enrolled in ESL program; male; disabled; enrolled in French immersion) fully interacted with year dummy, school proportion excused from the FSA reading exam, Census characteristics for the student's EA/DA of residence (mean and dispersion of household income; proportion visible minority; proportion one-parent families; unemployment rate; average dwelling value; proportion of dwellings owned; proportion moved last year and in last 5 years; proportion of household heads with less than grade 9 education, some high school, high school, and bachelor's degree or higher; proportion immigrant), and fixed main effects for year and school. See Table OA3 in the Online Appendix for complete coefficient estimates for specifications in columns 1-3 and additional information. Complete coefficient estimates for all other specifications available from the authors on request. Robust standard errors in parentheses, clustered at the school-by-year level. ***indicates statistically significant at the 1% level, **indicates significant at the 5% level, *indicates significant at the 10% level.

Table 5: Control Function Estimates of Effect of Information about School-level Achievement on Separation Probability

	(1) Full Sample	(2) English	(3) Non-English
"News" Measures			
1999 FSA Score*(Yr=2000)	-0.009**	-0.013***	0.003
	(0.005)	(0.005)	(0.007)
2000 FSA Score*(Yr=2001)	-0.011*	-0.011	-0.015
	(0.006)	(0.007)	(0.010)
1999-2001 FI Score*(Yr=2002)	-0.035***	-0.031***	-0.046***
	(0.010)	(0.011)	(0.015)
2002 FI Score*(Yr=2003)	0.001	0.006	-0.011
	(0.007)	(0.009)	(0.009)
"Old News" Measures			
1999 FSA Score*(Yr>2000)	-0.001	-0.003	0.006
	(0.005)	(0.006)	(0.008)
2000 FSA Score*(Yr>2001)	0.009	0.006	0.016*
	(0.007)	(0.008)	(0.009)
1999-2001 FI Score*(Yr>2002)	-0.027***	-0.024**	-0.032**
	(0.010)	(0.011)	(0.016)
Current FSA Score	-0.005	-0.002	-0.011*
	(0.004)	(0.004)	(0.006)
Current FSA Score*(Yr>2001)	0.009*	0.003	0.020**
	(0.005)	(0.006)	(0.009)
Number of Observations	65180	44077	21103
Number of Schools	361	361	360

Source: Authors' calculations based on B.C. Ministry of Education enrollment database and auxiliary data.

Notes: "FSA Score" refers to the school-average of FSA Reading and Numeracy scores. "FI Score" refers to the Fraser Institute school score. All scores are normalized to have mean zero and variance one over schools in each year. Additional control variables in these regressions are: main effects for student characteristics (as described in notes to Table 4), school proportions of student characteristics (as described in notes to Table 4) interacted with dummies for three information regimes (1999/2000, 2000/2001-2001/2002, 2002/2003-2003/2004), the school proportion excused from each FSA reading and numeracy exam interacted with the same "news" and "old news" year dummies as the corresponding test scores, Census characteristics for the student's EA/DA of residence (as described in notes to Table 4), and fixed main effects for year and school. See Table A1 in the Appendix for complete coefficient estimates for column 1 and Table OA4 in the Online Appendix for complete coefficient estimates for all three specifications. Robust standard errors in parentheses, clustered at the school-by-year level.

***indicates statistically significant at the 1% level, **indicates significant at the 5% level, *indicates significant at the 10% level.

Table 6: Effect of Information about School-level Achievement on Separation Probability, by Home Language and Quartile of Distribution of Neighborhood Income

	(1)		(2)		(3)	
	All		English		Non-English	
	Top Quartile	Bottom Quartile	Top Quartile	Bottom Quartile	Top Quartile	Bottom Quartile
"News" Measures						
1999 FSA Score*(Yr=2000)	-0.003	-0.018**	0.001	-0.038***	-0.022	0.009
	(0.009)	(0.008)	(0.010)	(0.011)	(0.023)	(0.012)
2000 FSA Score*(Yr=2001)	-0.000	-0.024**	-0.002	-0.033**	-0.008	-0.028
	(0.010)	(0.012)	(0.011)	(0.013)	(0.026)	(0.019)
1999-2001 FI Score*(Yr=2002)	-0.002	-0.045***	0.006	-0.078***	-0.046*	-0.015
	(0.010)	(0.013)	(0.010)	(0.018)	(0.027)	(0.018)
2002 FI Score*(Yr=2003)	0.005	-0.031***	0.001	-0.036*	0.022	-0.033**
	(0.011)	(0.012)	(0.011)	(0.018)	(0.031)	(0.014)
"Old News" Measures						
1999 FSA Score*(Yr>2000)	-0.008	0.002	-0.003	-0.006	-0.023	0.012
	(0.009)	(0.010)	(0.010)	(0.014)	(0.023)	(0.013)
2000 FSA Score*(Yr>2001)	-0.008	0.012	-0.018	0.024	0.028	0.001
	(0.011)	(0.011)	(0.013)	(0.017)	(0.027)	(0.014)
1999-2001 FI Score*(Yr>2002)	-0.016	-0.007	-0.001	-0.027	-0.078	0.019
	(0.016)	(0.016)	(0.015)	(0.023)	(0.047)	(0.020)
Current FSA Score	-0.010	0.000	-0.004	0.001	-0.029	0.007
	(0.007)	(0.007)	(0.008)	(0.010)	(0.019)	(0.009)
Current FSA Score*(Yr>2001)	0.009	0.013	0.002	0.021	0.045*	0.002
	(0.009)	(0.009)	(0.009)	(0.013)	(0.025)	(0.013)
Number of Observations	17592	14928	14783	7705	2809	7223
Number of Schools	332	350	303	344	281	289

Source: Authors' calculations based on B.C. Ministry of Education enrollment database and auxiliary data.

Notes: For details of this specification refer to notes to Table 5. Complete estimates of all coefficients are available from the authors on request. Robust standard errors in parentheses, clustered at the school-by-year level. *** indicates statistically significant at the 1% level, ** indicates significant at the 5% level, * indicates significant at the 10% level.

Appendix A: Identification via the Control Function Estimator

For illustrative purposes, consider a simplified version of equation (11):

(A1) $$y_{ist} = X'_{ist}\beta_t + \alpha_3 S_{st-1} + \upsilon_{st} + \varepsilon_{ist}.$$

We have subsumed all observables, including the constant, fixed school and year effects, and student characteristics, into X_{ist}. We have also omitted longer lags of test scores for expositional clarity, and omitted the current test score, S_{st}, to illustrate the potential endogeneity problem. We assume observables are exogenous in the sense that X_{ist} and X_{ist-1} are uncorrelated with υ_{st} and ε_{ist}.

Consider the projection of school-average test scores onto contemporaneous observables:

(A2) $$S_{st} = X'_{ist}\gamma + \chi_{st}$$

where χ_{st} has mean zero and is orthogonal to X_{ist} by construction. It is helpful to think of χ_{st} as a mean-zero "shock" that represents the effect of teachers and other time-varying school-specific unobservables on test scores. Equation (A2) implies $S_{st-1} = X'_{ist-1}\gamma + \chi_{st-1}$. Given exogeneity of X_{ist-1}, lagged test scores are therefore endogenous in (A1) if and only if past shocks to achievement, χ_{st-1}, are correlated with unobserved heterogeneity in separations (υ_{st} or ε_{ist}).

Suppose that shocks to achievement are correlated with unobserved time-varying school-specific heterogeneity in separation probabilities. We represent this via the projection:

(A3) $$\upsilon_{st} = \kappa\chi_{st} + \xi_{st}$$

where $E[\xi_{st}] = E[\chi_{st}\xi_{st}] = 0$ by construction. Suppose further that shocks to achievement are persistent, as represented via the projection:

(A4) $$\chi_{st} = \rho\chi_{st-1} + \zeta_{st}$$

where $\rho \neq 0$ and $E[\zeta_{st}] = E[\chi_{st-1}\zeta_{st}] = 0.$ It is easy to see that lagged test scores are now endogenous in (A1), because $E[\chi_{st-1}\upsilon_{st}] = \kappa\rho Var[\chi_{st-1}] \neq 0.$

Consider the "long" regression, analogous to (11), which includes current test scores as a control:

(A5) $$y_{ist} = X'_{ist}\beta_t + \alpha_3 S_{st-1} + \alpha_4 S_{st} + \eta_{ist}$$

where η_{ist} is the compound statistical error that arises when (A1) is the DGP.

Proposition: Under the identifying assumption $E[\chi_{st-1}\xi_{st} \mid X_{ist}, S_{st}] = E[\chi_{st-1}\varepsilon_{ist} \mid X_{ist}, S_{st}] = 0$, the least squares estimate of α_3 in the long regression (A5) is unbiased.[1] In words, our identifying assumption is that conditional on observables and current test scores, unobserved time-varying school-specific factors that influenced previous cohorts' achievement are only correlated with current-year unmeasured heterogeneity in separations via their persistent effect on achievement.

Proof: *Substituting* (A3) *into* (A1)*, we can write the DGP as:*

[1] For equation (11), where the model also includes longer lags of test scores, a more complete statement of our identifying assumption is $E[\chi_{st-j}\xi_{st} \mid X_{ist}, S_{st}] = E[\chi_{st-j}\varepsilon_{ist} \mid X_{ist}, S_{st}] = 0$ for each included lag of test scores $S_{st-j} = X'_{ist-j}\gamma + \chi_{st-j}$.

(A6) $$y_{ist} = X'_{ist}\beta_t + \alpha_3 S_{st-1} + \kappa\chi_{st} + \xi_{st} + \varepsilon_{ist}.$$

The least squares estimate of α_3 from the long regression (A5) *satisfies:*

$$E[\hat{\alpha}_3] = \alpha_3 + \kappa V^{-1}E[S_{st-1}\chi_{st} \mid X_{ist}, S_{st}] + V^{-1}E[S_{st-1}\xi_{st} \mid X_{ist}, S_{st}] + V^{-1}E[S_{st-1}\varepsilon_{ist} \mid X_{ist}, S_{st}]$$

where $V = Var[S_{st-1} \mid X_{ist}, S_{st}]$. $E[S_{st-1}\chi_{st} \mid X_{ist}, S_{st}]$ is the covariance between S_{st-1} and residuals from the regression of χ_{st} on X_{ist} and S_{st}. These residuals are zero from the definition of χ_{st} in (A2)*, and hence $E[S_{st-1}\chi_{st} \mid X_{ist}, S_{st}] = 0$ also. Similarly,*

$$E[S_{st-1}\xi_{st} \mid X_{ist}, S_{st}] = E[(X'_{ist-1}\gamma + \chi_{st-1})\xi_{st} \mid X_{ist}, S_{st}] = E[\chi_{st-1}\xi_{st} \mid X_{ist}, S_{st}] = 0$$

where the second equality follows from exogeneity of X_{ist-1}, and the final equality is our identifying assumption. An identical argument gives,

$$E[S_{st-1}\varepsilon_{ist} \mid X_{ist}, S_{st}] = E[(X'_{ist-1}\gamma + \chi_{st-1})\varepsilon_{ist} \mid X_{ist}, S_{st}] = E[\chi_{st-1}\varepsilon_{ist} \mid X_{ist}, S_{st}] = 0$$

and hence $E[\hat{\alpha}_3] = \alpha_3$.

The intuition underlying this result is straightforward. Since X_{ist-1} is exogenous, only the "shock" component of lagged test scores, χ_{st-1}, is potentially endogenous. Equation (A2) implies that S_{st} is a valid control function for χ_{st}. Thus the only potential source of endogeneity in the long regression is conditional covariation between χ_{st-1} and unobserved heterogeneity that is orthogonal to current test scores, i.e., ξ_{st} and the component of ε_{ist} that is orthogonal to S_{st}. Our identifying assumption rules this out.

Our identifying assumption would be violated under the following conditions. First, $E[\chi_{st-1}\xi_{st} \mid X_{ist}, S_{st}] \neq 0$ if there are time-varying unobserved school-specific factors (including school-level policy and teacher quality) that are correlated with lagged test scores and current separations, *but uncorrelated with current test scores* (and X_{ist}). Similarly, $E\left[\chi_{st-1}\varepsilon_{ist} \mid X_{ist}, S_{st}\right] \neq 0$ if there are unobserved student characteristics that are correlated with lagged test scores and current separations, *but uncorrelated with current test scores* (and X_{ist}). It is difficult to construct realistic examples where these conditions would arise. We nevertheless implement several falsification tests (see the Results section and online Appendix) to assess the validity of our identifying assumption, and find no systematic evidence to the contrary.

B: Additional Tables

Table A1: Complete Coefficient Estimates, Control Function Estimator, Full Sample

Non-English Home Language	0.009*	Proportion with High School Diploma	0.01
	(0.01)		(0.03)
Aboriginal	0.069***	Proportion with Bachelor’s or Higher	0.062***
	(0.01)		(0.02)
Enrolled in ESL Program	-0.013**	Proportion Immigrant	-0.03
	(0.01)		(0.02)
Male	0.00	Travel Distance to School (km)	0.005***
	(0.00)		(0.00)
Disabled	0.025***	Enrolled in French Immersion	-0.01
	(0.01)		(0.01)
Gifted	0.01	Number of Public Schools Nearby	0.00
	(0.01)		(0.00)
Changed Schools Prior to Grade 4	0.060***	Number of Private Schools Nearby	0.00
	(0.00)		(0.00)
Own FSA Reading Score	-0.004**	Proportion Male	-0.05
	(0.00)		(0.03)
Own FSA Numeracy Score	-0.006***	Proportion Disabled	0.04
	(0.00)		(0.06)
Mean Household Income / $1000	-0.000*	Proportion Non-English	-0.04
	0.00		(0.04)
SE of Mean Household Income / $1000	0.00	Proportion Aboriginal	-0.10
	0.00		(0.07)
Proportion Visible Minority	0.032*	Proportion Enrolled in ESL Program	0.02
	(0.02)		(0.04)
Proportion One-Parent Families	0.00	Prop. Enrolled in French Immersion	0.01
	(0.02)		(0.04)
Proportion Aged 25+ Unemployed	0.00	Proportion Disabled*(Yr ≥2000)	0.05
	(0.03)		(0.08)
Average Value of Dwelling / $1000	0.00	Proportion Non-English*(Yr≥2000)	0.04
	0.00		(0.04)
Proportion of Dwellings Owned	-0.01	Proportion Aboriginal*(Yr≥2000)	0.02
	(0.01)		(0.09)
Proportion Moved Last Year	0.039**	Proportion ESL *(Yr≥2000)	-0.04
	(0.02)		(0.04)
Proportion Moved in Last 5 Years	-0.01	Proportion Male*(Yr≥2000)	0.06
	(0.01)		(0.04)
Proportion Less than Grade 9 Education	0.01	Prop. French Immersion*(Yr≥2000)	0.02
	(0.04)		(0.02)
Proportion Some High School	0.044*	Proportion Non-English*(Yr≥2002)	0.05

	(0.03)		(0.05)
Proportion Aboriginal*(Yr≥2002)	0.18	Current FSA Score*(Yr>2001)	0.009*
	(0.13)		(0.01)
Proportion ESL *(Yr≥2002)	-0.06	Prop. Excused 1999 Read.*(Yr=2000)	0.05
	(0.04)		(0.25)
Proportion Male*(Yr≥2002)	-0.06	Prop. Excused 2000 Read.*(Yr=2001)	0.00
	(0.07)		(0.21)
Proportion French Immersion*(Yr≥2002)	-0.047***	Prop. Excused 2001 Read.*(Yr=2002)	-0.234*
	(0.02)		(0.13)
Proportion Disabled*(Yr≥2002)	-0.04	Prop. Excused 2002 Read.*(Yr=2003)	-0.02
	(0.10)		(0.08)
Year = 2000	-0.01	Prop. Excused 1999 Num.*(Yr=2000)	-0.05
	(0.01)		(0.28)
Year=2001	-0.04	Prop. Excused 2000 Num.*(Yr=2001)	-0.04
	(0.02)		(0.20)
Year=2002	-0.02	Prop. Excused 2001 Num.*(Yr=2002)	0.16
	(0.03)		(0.11)
Year=2003	0.01	Prop. Excused 2002 Num.*(Yr=2003)	-0.08
	(0.04)		(0.11)
1999 FSA Score*(Yr=2000)	-0.009**	Prop. Excused 1999 Read.*(Yr>2000)	0.05
	(0.01)		(0.20)
2000 FSA Score*(Yr=2001)	-0.011*	Prop. Excused 2000 Read.*(Yr>2001)	0.00
	(0.01)		(0.18)
1999-2001 FI Score*(Yr=2002)	-0.035***	Prop. Excused 2001 Read.*(Yr>2002)	-0.14
	(0.01)		(0.12)
2002 FI Score*(Yr=2003)	0.00	Prop. Excused 1999 Num.*(Yr>2000)	0.11
	(0.01)		(0.23)
1999 FSA Score*(Yr>2000)	0.00	Prop. Excused 2000 Num.*(Yr>2001)	-0.06
	(0.01)		(0.18)
2000 FSA Score*(Yr>2001)	0.01	Prop. Excused 2001 Num.*(Yr>2002)	0.164*
	(0.01)		(0.09)
1999-2001 FI Score*(Yr>2002)	-0.027***	Constant	0.088***
	(0.01)		(0.03)
Current FSA Score	-0.01		
	(0.00)		

Source: Authors' calculations based on B.C. Ministry of Education enrollment database and auxiliary data.

Notes: This table reports all coefficient estimates for the specifications reported in column 1 of Table 5 in the main text. "FSA Score" refers to the school-average of FSA reading and numeracy scores. "FI Score" refers to the Fraser Institute school score. All scores are normalized to have mean zero and variance one over schools in each year. "Nearby" is defined as a circle with radius equal to the 75th percentile of travel distance to school. Robust standard errors in parentheses, clustered at the school-by-year level. ***indicates statistically significant at the 1% level, **indicates significant at the 5% level, *indicates significant at the 10% level.

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[1] Information about school achievement is publicly disseminated in jurisdictions including England (West and Pennell 2000), Chile (Urquiola, McEwan and Vegas 2007), New Zealand (Fiske and Ladd 2000), and many U.S. states (Figlio and Lucas 2004) and Canadian provinces (Cowley 2007).

[2] A large body of literature examines the relationship between school-level achievement measures and real estate prices, but most studies do not focus on the effect of *new information* on prices (e.g. Black 1999; Barrow 2002; Bayer et al. 2004; Gibbons and Machin 2003, 2006; Ries and Somerville 2010). These studies typically find that residential property values are higher in neighborhoods with higher-achieving schools.

[3] The grade 10 FSA exams were discontinued after the 2002/2003 school year.

[4] The authors are not affiliated with the Fraser Institute in any way.

[5] The Fraser Institute scores released in 2003 were based on school-average exam results in reading, writing and numeracy in Grades 4 and 7, and the average gap between male

and female scores on the Grade 7 reading and numeracy exams. The scores released in 2004 were constructed using different weights and included the percentage of students that did not "meet expectations" according to provincial standards (see Cowley and Easton (2008, p. 96) for details).

[6] A ProQuest search of Vancouver's two most widely-read daily newspapers (the Vancouver Sun and the Province) returned twelve articles (including editorial content) published about the Fraser Institute's first elementary school report cards in June, 2003.

[7] The notation is adapted from Moretti (2010), where a similar model is used to study peer effects in movie consumption.

[8] For recent applications of Bayesian learning, see Moretti (2010), Ichino and Moretti (2009), Erdem and Keane (1996), Altonji and Pierret (2001), Lange (2007), Chernew et al. (2008), and Woodcock (2010).

[9] According to our theoretical model, FSA scores will continue to affect school choice decisions in subsequent school years, but with a smaller weight. Estimating the effect of information on separations in years after 1999/2000 in the difference-in-differences framework therefore would require us to include higher order lags of test scores in our specification. This approach is not feasible because numeracy and reading exams were not administered regularly during the 1990s.

[10] Stated differently, our reported standard errors account for clustering at the school-by-year level.

[11] Specifically, β_t varies across three information "regimes" that reflect the nature of information available to parents. During the first regime (1999/2000), parents observed no formal measures of school-level achievement. Parents could observe school-average

FSA exam results during the second regime (2000/2001 and 2001/2002), and they could observe both school-average FSA exam results and the Fraser Institute scores and rankings during the third regime (2002/2003 and 2003/2004).

[12] The region is geographically isolated from other populated areas by the U.S. border to the south, the Strait of Georgia to the west, and rugged mountains to the north and east.

[13] Our control function approach relies on current cohort FSA scores to control for unobserved time-varying factors that have persistent effects on FSA scores and separations. This identification strategy is most credible for those students in the current FSA cohort (grade 4). Focusing on grade 4 also allows us to control for students' own FSA scores.

[14] To test the hypothesis that parents' response to new information might be strongest upon initial school entry, we have also used a difference-in-differences specification to estimate how kindergarten enrollment changed in response to the release of FSA test scores. We do not report these estimates for two reasons. First, they were too imprecise for us to be able to draw any reliable conclusions. Second, impacts on enrollment levels (unlike separations) might be muted by capacity constraints, and a full analysis under such constraints is beyond the scope of this paper.

[15] We use these geographic coordinates to calculate measures of distance between the student's home and various schools. Details of how we construct these measures are provided in the Data Appendix available online.

[16] Our estimation sample includes only students who attended schools for which a full set of Fraser Institute scores was released in both 2003 and 2004. See the online Data Appendix for details.

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[17] See, for example, "Elementary school rankings one useful tool for B.C.," in *The Province* newspaper, June 8, 2003: pg. A.20. Also "Elementary schools get their grades, by Janet Steffenhagen in *The Vancouver Sun* newspaper, June 9, 2003: pg. B.1.

[22] Students with missing data are excluded. In particular, note that our sample includes only students who made regular progress through grades K-5 in a Lower Mainland school. This excludes students who separate and move to a school outside of the province. See the online Data Appendix for information about the nature and frequency of missing data.

[18] Complete coefficient estimates are given in Online Appendix Table OA3. All else equal, the probability of separation is higher among disabled students, and is strikingly higher among Aboriginal students. It is lower among high-achieving students and students in English as a Second Language programs. Distance to school has a significant positive effect on separations, but the magnitude is small. Unsurprisingly, prior mobility is a strong predictor of current separation. Students who live in neighborhoods where a greater proportion of household heads have a university degree are substantially more likely to leave their school.

[19] Home language may also proxy for preferences or cultural norms.

[20] Complete coefficient estimates are given in Appendix Table OA4.

[21] Estimates for specifications that include the full set of information measures released in each year (for those years where multiple measures were released, e.g., 2002/2003 when the Ministry published school-average FSA scores via its website and the Fraser Institute released their first set of scores and rankings) are available on request. Because contemporaneous information measures are highly correlated (see Online Appendix

Table OA2), estimates from specifications that include multiple information measures for each year are imprecise and difficult to interpret. Hence we prefer the reported estimates.

[22] In other specifications reported below, the coefficient on current-year FSA scores differs significantly from zero.