

# **Methodological Thoughts on Measuring the Impact of Private Sector Competition on the Educational Marketplace**

Dan D. Goldhaber

The University of Washington and the Urban Institute

Eric R. Eide

RAND

**Abstract** — Numerous researchers have speculated on and attempted to quantify the impact of policies designed to increase school choice and competition between schools, and they have reached widely differing conclusions. In this paper, we provide an overview of the research that focuses on the potential achievement effects of greater private sector competition on K-12 schooling. In particular, we explore the various methodologies that have been used to assess the relative effectiveness of public and private schools. We discuss the strengths and shortcomings of different approaches, and based on this, we ponder what is known and not known about the impact of increased choice and competition. In conclusion, we maintain that although many of the methodologies used to assess the effects of particular interventions, such as educational vouchers, are sound, they likely fail to capture the general equilibrium consequences of enhanced choice. As a result, many of the questions we would want answered to inform policymaking remain unknown.

Goldhaber is a Research Associate Professor at the University of Washington and an Affiliated Scholar of the Urban Institute. Eide is an Economist at RAND. We wish to thank Susanna Loeb, Jane Hannaway, and Meaghan McElroy for valuable comments on an earlier draft. We also thank three anonymous referees for helpful insights. *The views expressed in this paper are solely those of the authors and do not necessarily reflect those of The University of Washington or RAND.*

Contact author: Dan Goldhaber, The University of Washington, 327 Parrington Hall, Box 353060, Seattle, Washington 98195-3060, e-mail: [dgoldhab@u.washington.edu](mailto:dgoldhab@u.washington.edu).



## **I. Introduction: Why Competition and Choice?**

Early support for expanded educational choice was largely based on the idea that choice in education is fundamental to freedom (e.g. Friedman, 1955) — that is, choice in and of itself is a good. More recently, scholars have argued that increased choice and competition is necessary to improve education for both students who actively choose schools and those who do not. Numerous researchers have speculated on and attempted to quantify the impact of policies designed to increase choice and competition among schools, but they have reached widely different conclusions regarding the likely effects.

In this paper, we provide an overview of the research that focuses on the potential effects of greater private school competition in K-12 education. We focus primarily on the various methodologies that have been used to assess the relative effectiveness of public and private schools as well as the impacts of competition on the educational system as a whole. We discuss the strengths and shortcomings of different approaches, and based on this, ponder what we do and do not know about the potential impact of increased choice and competition from private schools. In conclusion, we maintain that although many of the methodologies used to assess the effects of particular interventions, such as educational vouchers, are sound, they likely fail to capture the general equilibrium consequences of enhanced choice. As a result, many of the questions we would want answered to inform policymaking remain unknown.

The paper is laid out as follows. Section II focuses on the methodological complications of assessing private school effectiveness using non-experimental data, and Section III discusses statistical approaches for correcting for biases that may arise from using non-experimental data. The advantages and disadvantages of using quasi-experimental and experimental data to study private school effects are discussed in Section IV. Section V presents evidence on systemic effects of competition and explores equity issues associated with school choice programs. Section VI offers some concluding thoughts.

## **II. Methodological Complications of Assessing Private School Effects Using Non-Experimental Data**

Much of the research on the effect of private school attendance on student outcomes has utilized non-experimental data. Typical datasets consist of a cross-section of students and

contain information on a variety of educational variables, such as whether the student attended a private or public school, measures of student outcomes such as test scores, wages, or educational attainment, and information on students' family backgrounds. These non-experimental data sources, however, contain *ex post* information on private school attendance, meaning all that is observed is whether or not the student attended public or private school. Thus, we do not observe the counterfactual that is truly of interest: How a private school student would have done had he/she attended a public school and how a public school student would have done had he/she attended a private school. Accordingly, there is typically no information about the reasons why a student's parents chose to send the student to a private school. Researchers can estimate the various factors that are correlated with attendance at private schools, such as family income or parental education, but there is generally little *ex ante* information available about the decision. Furthermore, even if *ex ante* information is available, there is still a problem because students are not randomly assigned to attend private and public schools, and private and public school students may differ in ways that are unobservable to the researcher. For example, we cannot directly observe students' motivation and this likely plays a role in explaining students' outcomes.

The primary drawback of non-experimental data is that the individuals undergoing the "treatment" under study, in this case private school attendance, are non-randomly assigned to the "treatment group." As a result, there may be individual differences that are correlated both with the decision to be treated (e.g. sent to a private school) and with the outcome in question. For instance, researchers typically do not attribute the effect of private school attendance on test scores entirely to private schools since some of the effect is likely due to the select nature of the students who choose to attend these schools (e.g. private school students tend to be from more affluent families). Confounding factors, such as student achievement levels, the availability of educational resources in the home, or parental support for education, if not accounted for, can bias the estimates of the effect of the treatment on the outcome. This statistical problem is commonly referred to as "selection bias."

Non-experimental analysis, for example, might consist of comparing the test scores of students who have actively chosen the school they attend (e.g. private school students) to students who remain in an assigned school. The fact that parents freely choose a school (and in

the case of private schooling, freely choose to spend additional money) suggests that they may be quite different from other parents. For example, these parents demonstrate a willingness to support education that could indicate that they also provide an environment in the home that is conducive to educational achievement. Statisticians can account for *observable* differences in characteristics using standard techniques, but, if there are important *unobservable* characteristics of students or their families, which influence achievement and are systematically related to the school sector in which they are enrolled, then ordinary statistical models of student achievement are inadequate.

In the context of comparing public and private school students in non-experimental settings, researchers have attempted to account for unobserved differences between individuals using a variety of statistical techniques, and these different methodological approaches have yielded varying results. The early work analyzing differences in outcomes between public and private school students typically fit a single regression equation for achievement that includes a binary right-hand side variable identifying private school attendance. Differences between public and private school students are accounted for by the independent variables included in the model (e.g. parental income, a student's prior achievement level, etc.). A positive and statistically significant coefficient on the private school variable was taken as evidence that private schools outperform their public school counterparts (when a value of 1 for the binary private school variable signifies attendance at a private school). There are several potential problems with these early studies. First, the methodology used may not adequately deal with the selection bias issue since it does not account for the likelihood that students who choose to attend a private school are different in *unobservable* (at least given available data) ways from public school students.<sup>1</sup> Second, most early studies rely on cross-sectional data so they fail to include a measure of students' initial academic achievement as a control variable, which makes it difficult to assess the degree of educational "value-added" of private schools relative to public schools.

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<sup>1</sup> These early studies attempt to account for systematic differences between private and public school students using a control function approach, that is, by including detailed sets of observable characteristics. However, omitted *observable* variables that determine both attendance choices and outcomes are just as problematic as unobservable variables, so that selection bias is not limited to the case where selection is on unobservable characteristics.

Third, the regression coefficients on the schooling variables in each sector (public or private) are restricted to being equal, despite theoretical arguments for why this might not be the case.<sup>2</sup>

Some of these issues are illustrated by the debate over early findings on differences between students in the private and public sectors. In an influential study by Coleman, Hoffer, and Kilgore (1981), the authors found that private Catholic schools were more effective at educating students than public schools, and that Catholic schools were better at equalizing educational opportunities for students of differing racial, ethnic, and socioeconomic backgrounds. A host of follow-up studies using the same data, found differing results and offered criticisms of the statistical techniques, findings, and conclusions of Coleman et al. For example, Noell (1983) found that adding four additional student background variables -- sex, handicap status, region, and eighth grade college expectations -- to the public-private school regression equation reduced the impact of Catholic school attendance on senior and sophomore cognitive outcomes to statistical insignificance (in most cases). These additional variables are meant to proxy for selection into private schools. Goldberger and Cain (1982) also critiqued Coleman et al for not accounting for selection bias. These authors found that when academic track is included as an explanatory variable to attempt to control for selection bias, the outcomes in favor of private schools disappear.<sup>3</sup> These results suggest that the estimated private school effect can be dramatically altered by the set of variables included on the right hand side of the regression equation, implying that part of the private school effect is accounted for by these additional factors.

In the years following the Coleman et al paper, subsequent researchers have attempted to overcome some of the methodological deficiencies present in the early work. Much of the attention has focused on accounting for selection bias, and researchers have primarily used two statistical techniques to address this: Instrumental Variables (IV) estimation and the two-stage selection bias correction developed by Heckman (1979).<sup>4</sup> We discuss these estimation techniques in the next section.

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<sup>2</sup>For example, if one of the impediments to teaching in the public sector is bureaucracy, we might expect a teacher with a given set of observable characteristics to show a higher return to those characteristics in the private sector.

<sup>3</sup> Even though the private school effect disappeared when academic track was included in the regression model, the academic track variable itself likely suffers from selection bias and thus there is some question as to whether or not it should be included as an explanatory variable after all.

<sup>4</sup> See Wooldridge (2000), chapters 3, 15, and 17 for a full treatment of these issues.

### **III. Statistical Approaches for Correcting for Selection Bias**

#### *A. Instrumental Variables*

Researchers typically employ an Instrumental Variables (IV) technique when estimating a single equation for the effect of private school attendance on some outcome such as test scores, where private school attendance is measured with a binary variable. The purpose of IV estimation is to account for the unobserved differences in student and family backgrounds by “breaking” the correlation between the private school variable and the error term in the regression equation. The idea behind the IV technique is to find a variable that is highly correlated with private school attendance, but that is thought to be uncorrelated with the error term. Using the analogy of experiments, the problem is that treatments and controls are not randomly assigned. However, if there is a variable that makes it more or less likely that students attend private school and this variable does not otherwise affect outcomes, the sample can be split in a new way to compare student outcomes across students with differing values for this variable. Since this variable does not directly affect outcomes, any differences across groups can be attributed to differences in sector choices. Distance from a student’s home to the closest private school is an example of an instrument used in private school studies. The assumption is that the distance would affect the likelihood of attending a private school but would not directly affect a student’s academic achievement. Another common instrument in private school attendance studies is Catholic religion. If Catholic is used as an instrument for private school attendance, the researcher is assuming that outcomes between similar Catholic and non-Catholic students would not differ if they chose to attend the same sets of schools, and differ only because they tend to make different sector choices.

In practice, finding suitable instrumental variables to identify the private school effect can make this estimation approach difficult (variables like distance are often not available). Indeed, using a poor instrument (or set of instruments) can also cause problems. For instance, if the instrument is only weakly correlated with the endogenous variable, then the standard errors

for the estimated private school effect will be large. Moreover, weak instruments also imply that the IV estimates will be biased toward the Ordinary Least Squares (OLS) estimate.<sup>5</sup>

As a more formal treatment of the IV approach, consider the following equation measuring the “true” effect of private school attendance on student achievement:

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + u \quad (1)$$

In this equation,  $y$  is student achievement,  $x_1$  is a binary variable that equals 1 if the student attends a private school and equals 0 otherwise, and  $x_2$  is another variable that affects achievement. The parameters  $\beta_1$  and  $\beta_2$  represent the effect of *ceteris paribus* changes in  $x_1$  on  $y$  and  $x_2$  on  $y$ ,  $\beta_0$  is the intercept, and  $u$  is the error term that contains factors that influence  $y$  other than  $x_1$  and  $x_2$ .

Estimation of equation (1) by Ordinary Least Squares (OLS) would yield unbiased estimates of  $\beta_1$  if data on  $x_2$  were available. However, often data on  $x_2$  is not available, typically because it is unobservable to the researcher. For example,  $x_2$  may represent the degree to which education is stressed in the student’s home, a variable likely important to student achievement but not typically observable to the researcher. When this is the case the achievement equation we actually estimate is

$$y = \beta_0 + \beta_1 x_1 + \beta \quad (2)$$

where  $\beta = \beta_2 x_2 + u$ . In general, estimation of equation (2) by OLS will yield biased and inconsistent estimates of  $\beta_1$  because the private school variable is correlated with the error term, assuming that  $x_1$  and  $x_2$  have nonzero correlation, as is usually the case. This bias is simple to show. The OLS slope estimator from (2) is (we use  $\tilde{\beta}_1$  rather than the usual  $\hat{\beta}_1$  to distinguish between OLS estimation of the observed model in (2) from the true model in (1) and  $n$  denotes the sample size):

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<sup>5</sup> Bound, Jaeger, and Baker (1995) discuss the pitfalls of implementing the IV technique with instruments that are only weakly correlated with the endogenous variable.



$$\tilde{\beta}_1 = \frac{\sum_{i=1}^n (x_{i1} - \bar{x}_1)y_i}{\sum_{i=1}^n (x_{i1} - \bar{x}_1)^2} \quad (3)$$

If we then substitute in the *true* model as given in equation (1) for  $y$  we obtain the following:

$$\tilde{\beta}_1 = \frac{\sum_{i=1}^n (x_{i1} - \bar{x}_1)(\beta_0 + \beta_1 x_{i1} + \beta_2 x_{i2} + u_i)}{\sum_{i=1}^n (x_{i1} - \bar{x}_1)^2} \quad (4)$$

When we simplify this expression and take the expected value we obtain

$$E(\tilde{\beta}_1) = \beta_1 + \beta_2 \frac{\sum_{i=1}^n (x_{i1} - \bar{x}_1)x_{i2}}{\sum_{i=1}^n (x_{i1} - \bar{x}_1)^2} \quad (5)$$

which shows that OLS is biased since the second term is only equal to zero in two special cases. Specifically, OLS will be unbiased if  $\beta_2 = 0$ , or if  $x_1$  and  $x_2$  are uncorrelated. In words this means that unless the unobservable variable, such as the educational environment in the home, is unrelated to student achievement, or unless private school attendance and educational environment in the home are uncorrelated, then OLS will be biased and inconsistent. Moreover, the direction of the bias of the private school effect is a function of the sign of  $\beta_2$  and whether the covariance between  $x_1$  and  $x_2$  is positive or negative (note that

$$\sum_{i=1}^n (x_{i1} - \bar{x}_1)x_{i2} = \sum_{i=1}^n (x_{i1} - \bar{x}_1)(x_{i2} - \bar{x}_2) \text{ and if we divide the numerator and denominator of the}$$

second term in (5) by  $n - 2$  we obtain the sample covariance of  $x_1$  and  $x_2$  over the sample variance of  $x_1$ ).

The problem here is that since  $x_1$ , the private school variable, is correlated with the error term in the achievement equation, OLS yields biased and inconsistent estimates of private school attendance on student achievement. The IV approach is designed to address this problem.

To carry out an IV strategy, we need to find a variable in our data,  $z$ , that satisfies the following properties: 1)  $z$  is *uncorrelated* with the composite error term,  $\epsilon = \beta_2 x_2 + u$ , or  $\text{Cov}(z, \epsilon) = 0$ ; and 2)  $z$  is *correlated* with  $x_1$  so that  $\text{Cov}(z, x_1) \neq 0$ . In words this means we need to find a variable that we believe is uncorrelated with the unobservable factors that affect student achievement and that is correlated with private school attendance. If we can find such a variable,  $z$ , that meets the above criteria, the IV estimator is given by (in the two-variable regression model for simplicity):

$$\hat{\beta}_1 = \frac{\sum_{i=1}^n (z_i - \bar{z})(y_i - \bar{y})}{\sum_{i=1}^n (z_i - \bar{z})(x_{i1} - \bar{x}_1)} \quad (6)$$

This estimator provides a consistent estimate of the private school effect on achievement. To see intuitively why this estimator is consistent, we can rewrite the IV estimator as follows:

$$\hat{\beta}_1 = \frac{\sum_{i=1}^n (z_i - \bar{z})y_i}{\sum_{i=1}^n (z_i - \bar{z})(x_{i1} - \bar{x}_1)} \quad (7)$$

If we then substitute in for  $y$  we obtain the following:

$$\hat{\beta}_1 = \frac{\sum_{i=1}^n (z_i - \bar{z})(\beta_0 + \beta_1 x_{i1} + \epsilon_i)}{\sum_{i=1}^n (z_i - \bar{z})(x_{i1} - \bar{x}_1)} \quad (8)$$

When we simplify this expression we obtain

$$\hat{\beta}_1 = \beta_1 + \frac{\sum_{i=1}^n (z_i - \bar{z}) \beta_1}{\sum_{i=1}^n (z_i - \bar{z})(x_{i1} - \bar{x}_1)} \quad (9)$$

If  $z$  is a valid instrumental variable then the estimator will approach  $\beta_1$  as the sample size gets large [ $\text{Cov}(z, \beta_1)$  approaches 0] and it will therefore be a consistent estimator.

### *B. Heckman Two-Stage Correction*

In some instances measuring the effect of private school attendance on student achievement using OLS is problematic because our sample is not representative of all students. For example, we may wish to estimate the impact of private school attendance on student outcomes. One way to do this is to estimate separate achievement equations for public and private school students, then compare the predicted achievement scores to observe relative performance. This would replace the approach of measuring the private school effect with a binary variable in a model containing the entire sample of private and public school students. A typical motivation for estimating separate regressions for public and private sectors is that it allows the regression coefficients on the explanatory variables to differ between public and private school students, which potentially can be important.

Consider the equation

$$y = x\beta + u \quad (10)$$

where  $y$  is a measure of student achievement,  $x$  is a vector of variables that are assumed to influence student achievement (e.g. family background, per pupil spending, class size, etc.),  $\beta$  is the coefficient vector to be estimated from the data, and  $u$  is the error term. A sample selection problem would occur if equation (10) were estimated separately for private and public school students. The estimates from such an estimation procedure would only be relevant for the particular sector type in the estimation sample; therefore we may not infer anything about the effect of private school attendance among the population of students in general.

The most common technique for correcting this type of selection bias is to use the two-stage method developed by Heckman (1979). In this approach we first model a student's decision to attend a private school, then we use estimates from this first stage regression to

“correct” for the selection bias in the primary equation of interest (equation (10) in our example). The first stage regression modeling a student’s decision to attend a private school may be modeled as follows:

$$P = z\beta + v \quad (11)$$

where  $P$  is a binary indicator that equals 1 if the student attends a private school and equals 0 if the student attends a public school,  $z$  is a vector of variables assumed to affect the decision to attend a private school,  $\beta$  is a vector of coefficients to be estimated and  $v$  is a normally distributed error term. We assume that  $z$  and  $x$  are uncorrelated with  $u$ , that  $x$  is a subset of  $z$ , and that some variables in  $z$  do not appear in  $x$ . That is, all variables assumed to affect achievement also affect private school attendance, but there are some variables that are assumed to affect private school attendance but not achievement. We also assume that  $v$ , the error term from the first stage equation describing the private school attendance decision, is uncorrelated with  $z$  (and therefore  $x$ ), and is jointly normally distributed with  $u$ , the error term from the equation of interest. The sample selection problem arises due to the correlation between  $v$  and  $u$ . In words, this means that the selection bias problem arises because there are unobservable factors that jointly affect both the decision to attend a private school and student achievement. If there were no correlation between the error terms from the two equations, then there would be no selection bias problem and OLS estimation of equation (10) would provide unbiased estimates.

Under the above assumptions, if we take the expected value of  $y$  conditional on our observed data,  $z$ , and attending a private school ( $P = 1$ ) it can be shown that the equation for the private school students is

$$E(y | z, P = 1) = x\beta + \lambda\lambda(z\beta) \quad (12)$$

where  $\lambda(z\beta)$  is called the inverse Mills ratio, and  $\lambda(z\beta) = \frac{\phi(z\beta)}{\Phi(z\beta)}$  where  $\phi(z\beta)$  is the probability density function for the standard normal distribution and  $\Phi(z\beta)$  is the cumulative distribution function for the standard normal distribution, both of which are evaluated at  $z\beta$ . The parameter  $\lambda$  is the coefficient for the inverse Mills ratio, and its value suggests whether there is selection bias in the data: a value of zero suggests no selection bias, while a nonzero

value suggests possible selection bias. Equation (12) shows that if the inverse Mills ratio is added to equation (10) then we obtain consistent estimates based on the private school sample. In effect, the inverse Mills ratio is an omitted variable that accounts for the likelihood that a student attends a private school, and once this variable is added to the regression equation the omitted variable bias is corrected.

In order to implement this estimation approach we need an estimate of the inverse Mills ratio for each observation, or  $\hat{\lambda}_i = \lambda(z_i\lambda)$ . The two-stage selection bias correction can be summarized as follows:

- 1) Estimate equation (11) using a probit regression of P on z to obtain estimates of  $\lambda$ , then calculate  $\hat{\lambda}_i$ .
- 2) Using the private school sample, estimate the OLS regression of y on x and  $\hat{\lambda}_i$ .

Note that a simple hypothesis test for selection bias in private school attendance is a t-test on  $\hat{\lambda}_i$  with the null hypothesis that  $H_0 : \lambda = 0$ .

As noted previously, while x should be a subset of z, there should be some elements of z that are not included in x. That is, the first stage equation should include at least one variable that affects private school attendance but does not directly affect student achievement. It is possible to estimate the Heckman model without an “identifying” variable, but in this case the inverse Mills ratio,  $\hat{\lambda}_i$ , is simply a nonlinear combination of the variables, x, used to predict student outcomes and identification is simply based on functional form. For identification to be based solely on the nonlinearity, one has to believe, from a theoretical perspective, that the variables in x only affect achievement in a linear fashion, which seems somewhat implausible. From a practical perspective, this identification strategy is also problematic since it is still likely that  $\hat{\lambda}_i$  is highly correlated with x, which would lead to high estimated standard errors for the regression coefficients in the achievement equation. Hence, including some variables in the private school attendance equation that are not in the student achievement equation will help identify  $\hat{\lambda}_i$ , that is, provide variation that is independent of the other variables in the achievement equation. In this sense, z is analogous to an instrumental variable since it helps

identify the private school effect, and indeed, the identifying variable is often referred to as an instrument.

Both of the empirical methods described above – instrumental variables and the Heckman two-stage correction – rely on the choice of instruments. As such, this choice may greatly influence one's findings. This is true for the studies that attempt to account for selection bias in private school attendance, where the findings are mixed, and the divergence in findings appears to be due at least in part to the types of identifying variables used by different researchers (Altonji et al., 2000). Evans and Schwab (1995), for instance, use a variable for whether or not the student is Catholic as well as Catholic religion interacted with a variety of religious attendance variables as an instrument for Catholic school attendance. They find that attending a Catholic high school raises the probability of finishing high school and also of entering a four-year college. Neal (1997) uses as instrumental variables for Catholic school attendance a measure for the population density of Catholics in a given locality as well as a measure of the geographic density of Catholic schools. The argument is that these types of variables directly influence the probability of attending a Catholic school but do not directly impact student outcomes. Neal finds that the gains from Catholic schooling are modest for urban whites and are negligible for suburban students, but that there are substantial benefits of Catholic school attendance for urban minorities. Sander and Krautman (1995) use interactions between a Catholic religion variable and a measure of urbanicity, and interactions of urbanicity and region where regions with high concentrations of Catholics were selected. These authors find that after adjusting for self-selection, Catholic schooling reduces the odds that sophomores do not graduate with their class, but they do not find any evidence that Catholic schooling increases educational attainment six years after high school graduation.

It is important to note that the use of religion as an instrumental variable is central to the above studies. Work by Goldhaber (1996) and Figlio and Stone (1997) employ a different set of instruments. Goldhaber employs the Heckman two-stage technique to correct for selection bias when estimating the separate achievement equations for public and private school students. In modeling sectoral choice in the first stage equation, Goldhaber uses information about the cost and availability of private schools, including controls for region and urbanicity, to identify the selection effect. That is, Goldhaber assumes that these variables for the cost and availability of

private schools affect the decision to attend a private school, but do not directly influence achievement. Goldhaber's findings suggest the majority of the mean differential in achievement between the public and private sectors can be attributed to average differences in the characteristics of students and schools rather than the returns to these characteristics as measured by differences in the regression coefficients.

So how does one know if the chosen instruments are appropriate? As was the case with IV estimation, it is important that the instruments have some statistical power to predict the first stage outcome (whether or not a private school is chosen). It is also necessary that the instruments not be contemporaneously correlated with the error term in the second stage (student achievement) equation. If there is more than one available instrumental variable, one way to formally determine whether this is the case is to perform what is known as a test of over-identifying restrictions. This test involves, at an intuitive level, using one of the instruments to get what is assumed to be consistent estimates of the coefficients in the second stage model (the estimates of student achievement), then testing whether the estimated residuals from this model are uncorrelated with the other potential instruments that were not used in the estimation process. If the model is correctly specified and the instrument is appropriate, then the other instruments should be uncorrelated with the residuals (this is the assumption for an instrument). This same procedure is then repeated using other potential instruments.<sup>6</sup>

The limitation of this test is that it relies on a comparison of the error term to at least one unused instrument so it necessarily requires at least two exclusions (instruments) from the second stage of the model. There are several informal tests that are often used to test a single instrument. Consider, for example, the case of using Catholic religion as an instrument as is done in studies such as Evans and Schwab (1995) and Neal (1997). Aside from making arguments in favor of the appropriateness of religion as an instrument, these authors include religion as an independent variable in the second stage outcome model in question. They argue that an insignificant coefficient of the instrument variable – that is, the instrument has little predictive power in the achievement equation – provides “suggestive” evidence that the instrument is appropriate. This, however, is merely an informal test that does not provide definitive proof that the instruments are appropriate, and as some of the authors note (Evans and

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<sup>6</sup> See Wooldridge (2000) for further details.

Schwab, 1995), the results from some models are “sensitive to the choice of instruments” (p. 969).

Figlio and Stone (1997) show that employing two of the most commonly used instruments in the literature – a student’s religion or the percent Catholic in the county – results in unreliable estimates based on their data. Specifically, Figlio and Stone find that Catholic students as a group perform at a significantly higher level on standardized tests than non-Catholic students, even after controlling for a variety of demographic and economic variables in their achievement equations. Hence it appears that being Catholic is correlated directly with student achievement implying that it is an inappropriate instrument. Furthermore, the authors find that Catholic students in Catholic schools outperform Catholic students in public schools, which they believe suggests positive selection of Catholic students into private schools, though one might also infer this to be a private school effect. The bottom line, however, is that using Catholic religion as an instrument likely overstates the treatment effect of private schooling.

Rather than using religion, Figlio and Stone (1997) suggest as instruments binary variables reflecting whether the state has “duty to bargain” or “right-to-work” laws<sup>7</sup>, both of which are variables that attempt to capture the relative power to bargain over contracts between employers (the district or state) and employees (hired teachers); interactions between these variables and the median income in the county; and interactions between the prior instruments and the family’s socio-economic status. They find these variables are highly correlated with sector selection but do not directly affect student performance. Using these instruments, they find positive private school effects for the probability of two years of college attendance and the probability of selective college attendance, but their estimates for more traditional measures of academic performance are mixed. They find that private schools outperform public schools in mathematics only for a few subgroups of students.

In summary, the early studies based on non-experimental data were methodologically flawed, largely because they did not control for self-selection bias. More recent studies which attempt to overcome these weaknesses have found largely mixed results on the effectiveness of private schools, as measured by students’ test scores, with the divergence in results apparently

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<sup>7</sup> Duty to Bargain laws say that districts have to bargain with teachers; Right to Work laws say that teachers are not able to strike.



due to the particular set of instrumental variables used to control for selection bias, the data set used, and the outcomes examined.<sup>8</sup> Of the studies we described above, some found positive private school effects on educational attainment while others found no effect on student achievement. Since student achievement presumably is positively correlated with educational attainment, these findings seem to be at odds. One can speculate on the source of the underlying mechanism, but it is difficult to draw conclusions based on the findings in the papers.

We note that it is often difficult to make value judgments about which studies are most reliable given that the studies follow sound empirical techniques but rely on different data sets and statistical approaches. The general point is that the statistical methodologies we have described rely on assumptions that are not easily tested, which makes it difficult to assess the validity of particular studies and discrepancies across studies. For instance, we never actually know whether a single instrumental variable is valid, as we cannot formally test whether the instrument is uncorrelated with the error term in the regression model. Researchers can test the extent to which a single instrument is correlated with the endogenous variable, and can invoke logic and intuition to support their argument for using a particular instrument, but they cannot prove whether or not the instrument is valid. *The bottom line is that even if the researcher attempts to model selection bias, the reader should nevertheless interpret the results cautiously since in practice it is difficult to adequately model selection into private school.* This is especially true since most researchers base their findings on non-experimental data, which contains only *ex post* outcomes of the private school decision.

#### **IV. Assessing Private School Effects Using Quasi-Experimental and Experimental Data**

One way to assess the public school versus private school debate is by studying the impact of vouchers on student outcomes in voucher programs that allow students to attend private schools. Recent interest in the use of voucher programs to improve the quality of primary and secondary education has resulted in a number of voucher programs, both publicly and privately funded. One of these voucher programs, the Milwaukee Parental Choice Program, has resulted in a unique data set, which has been used by researchers to evaluate the effects of

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<sup>8</sup> Altonji et al (2000) show that it may be possible to infer the potential for bias depending on the extent to which student selection is correlated with observable variables relative to its correlation with unobservable variables (e.g.

private school attendance on student achievement.<sup>9</sup> This program is also useful in illustrating the importance of methodology in influencing research findings since the handful of most-cited studies (which are discussed below) on the impact of the Milwaukee voucher program on student achievement have reached different conclusions regarding its effects.

One issue in assessing this program is defining the appropriate comparison groups. It is quite likely that program “take-up” is non-random — that is, there may be differences between students who were chosen for the choice program and choose to attend, and students who were chosen for the program and ultimately decided not to attend. A second issue is that students who are rejected from the choice program (the “non-selected sample”) may have elected to attend an alternative private school or have moved out of the Milwaukee Public School District, in which case they are not in the comparison sample. Students who are rejected and go back in the public school system are in the sample, but these students may not be representative of the full sample of students who applied to participate in the choice program.<sup>10</sup> There is also the problem of non-random attrition over time from both the choice and the non-selected samples, which consequently can affect comparison of these groups. For example, if students in the choice sample attending private schools do not perform well in these schools, they may drop out of the private schools, leaving the private school sample to consist of relatively high achievers. This means that individuals’ placement in the treatment group (private schools) and the control group (public schools) is no longer truly random.

The studies that have analyzed these data have yielded mixed results. These differences in findings are due to differences in how the researchers define the comparison group to the choice sample, and the statistical estimation techniques employed. Witte and Thorn (1996) examine the type of students who participate in the Milwaukee choice program. They find that choice parents were more likely to be involved with their children’s schooling prior to

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student motivation).

<sup>9</sup> The Milwaukee Voucher experiment began in the fall of 1990. Students living in Milwaukee who came from families with incomes not exceeding 1.75 times the national poverty line were eligible to attend private non-sectarian schools in the district. Further, students enrolling in the choice program could not have been in a private school in the immediate prior year or enrolled in public schools in districts outside Milwaukee (Witte 1997).

<sup>10</sup> The reason for this is that only three of the schools participating in the program are oversubscribed and it is unlikely that the participating private schools to which students apply are random. For instance, parents who are particularly dissatisfied with the current public school are more likely to enroll in the choice program. As a result, it

participating in the choice program, rated their prior public schools lower, and had higher educational expectations for their children than did non-participants. These results suggest that choice participants may differ from non-participants in important ways that may be difficult to empirically quantify.

Witte (1997) compared the students who enrolled in the choice program to a sample of students enrolled in the Milwaukee public school system. He controls for the possibility that students applying to the choice program and attending a choice school have unobserved characteristics that systematically differ from the Milwaukee public school students by employing the Heckman two-step methodology, using distance to present school as an instrument for private school attendance. He finds no case where private schools outperform public schools and, in one specification of the model, public school students outperform the choice students in reading. However, it is again important to note that the accuracy of the results depend crucially on how successfully one accounts for selection.

Greene et al. (1998) compare students who participated in the choice program and attended a private school with those who applied for the choice program, but were rejected through a process of random selection, and thus ended up attending a Milwaukee public school.<sup>11</sup> In theory, the comparison of “selected” private school students and those students who applied for the program but were non-selected based on a lottery (the “non-selects”) avoids the problems associated with selection bias. The hypothesis is that the non-selects do not have unobservable characteristics that are systematically different from those who applied to the program and were accepted.

The authors find little evidence of a private school effect for students in the first two years of the program but a large private school advantage in years three and four. For instance, they estimate a private school advantage on standardized tests of 7 percentile points in math and 6 percentile points in reading in year three of the program.<sup>12</sup> It is important, however, to note that the methodology used by Greene et al. does not allow for inference outside of those students

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may be that oversubscribed schools tend to be located in those areas where dissatisfaction with the Milwaukee public schools is particularly great.

<sup>11</sup> Schools were required to admit choice students without discrimination based on race, ethnicity or prior academic performance, but were not required to admit disabled students, and had to limit choice students to 49% of their enrollment (this figure rose to 65% beginning in the 1994-95 school year) and schools that were oversubscribed were required to accept students based on a *random selection* (Witte 1997).

who applied to participate in the choice program: the effect of attending a private school may be different for the average student and the student who chooses to apply to the choice program. There are also problems with this approach if there exists non-random attrition from the program.

Rouse (1998) utilizes instrumental variable techniques to address the possibility that selection bias exists due to the fact that not all those who are selected to participate in the choice program actually attend. Rouse's instrumental variable is the initial selection into the program. The assumption is that the initial selection is correlated with attendance at a choice school but does not directly affect student achievement. Rouse also controls for time-invariant individual ability by using a "fixed-effects" estimation approach, where individual-specific controls are included in the regression specification. In theory this accounts for the possibility of non-random attrition from either the treatment or control groups. In this specification of the model, Rouse finds that students who attended a choice school scored about 1 to 2 percentage points per year higher in math than students who were not selected, but that the results for reading scores were mixed. While her results appear to be robust to many of the data problems previously discussed, Rouse is careful to point out that there may still be unobserved differences between the choice and comparison groups that may affect the estimation results.<sup>13</sup>

"Quasi-experimental" data like the Milwaukee voucher program have some distinct advantages over non-experimental data, however researchers using such data are clearly still faced with significant obstacles. For this reason, educational experiments, whereby students are randomly assigned to either a treatment group or a control group and differences in outcomes between the two groups are observed, are viewed by many as the "holy grail methodology" used in assessing program effects.

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<sup>12</sup> Both the year three math and reading results are only statistically significant at the 10% level for one-tail tests.

<sup>13</sup> In another paper examining the Milwaukee voucher data, Goldhaber et al. (1999) investigate whether students with unobserved characteristics correlated with achievement are more likely to apply to the voucher program. They first estimate probit models for application to the choice program, and then math and reading achievement models. By comparing students randomly rejected from the program with students who did not apply, they test whether applicants and non-applicants have unobservable characteristics that are correlated both with the choice of school sector and with the included explanatory variables. They do not find strong evidence that students who apply to participate in the Milwaukee choice program have unmeasured characteristics, such as motivation, which systematically differ from non-applicants.

Though there is an intuitive appeal to this methodology, there are also a number of factors associated with the design of an experiment that are central to assessing program impacts. Researchers worry about several processes including 1) setting an appropriate scale of an experiment such that program effects can be detected (at a reasonable confidence level); 2) preventing or accounting for contamination of treatment or control groups; and 3) dealing with differences in response rates between treatment and control groups. These are all important to consider, but we do not focus on design issues here; instead we concentrate on what an experimental methodology can and cannot tell us about the impact of enhanced competition on the educational system.<sup>14</sup>

The strength of using a random assignment methodology to assess program effects is that, if done correctly, there is no need to control for the background characteristics of students in either the control or treatment groups. Even more important, controlled experiments also eliminate the need to account for differences in *unobservable* characteristics. Thus, unlike the majority of the work described above where it was necessary to use statistical methodologies, such as the Heckman two-stage correction, to account for differences in “treated” and “non-treated” samples, observed differences between groups are generally attributed to program effects. As a result, experiments are more readily understood and the findings from them tend to garner wider acceptance and public support.

In education there are relatively few controlled experiments, mainly due to political constraints. However, recently there have been several public-private choice (voucher) experiments, based on privately funded programs in large urban areas, such as New York City and Washington, D.C. (Peterson, Myers, and Howell, 1998; Wolf, Howell, and Peterson, 2000). These are new programs that tend to be relatively small with vouchers valued at modest amounts. The New York City program, which only included elementary grades, started in 1997 and the vouchers were worth up to \$1400. The program in Washington was originally established in 1993, but expanded significantly in 1997. Vouchers worth up to \$1700 for elementary school tuition and worth up to \$2200 for high school tuition were offered. Both of these programs were significantly oversubscribed thereby allowing for random assignment of participants to treatment (private schools) and control (public schools) groups. Research on these programs has focused

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<sup>14</sup> For a comprehensive review of the issues surrounding the design of (voucher) experiments, see Doolittle (1999).

on the impact of 1) the offer of a voucher, and 2) the use of a voucher at a private school and on a number of outcome variables (e.g. various measures of satisfaction, parental involvement in schooling, growth in students' test scores, etc.). Perhaps not surprisingly, there were important differences between the schools that students chose and those that they left, and parents and students in elementary grades who used vouchers were significantly more satisfied with their schools along a number of dimensions.

Though the effect sizes vary from grade to grade, attendance at a private school was found to have had a relatively large positive impact on student test scores for elementary school students, in both the New York and Washington programs. For example, the largest private school effects (which were in math) were on the order of a quarter of a standard deviation. As the authors note, this is roughly comparable to the effects sizes found in the widely cited Tennessee class size reduction study (The Student/Teacher Achievement Ratio Experiment or "STAR") and represent about a quarter of the "black-white test score gap" (Peterson, Myers, and Howell, 1998; Wolf, Howell, and Peterson, 2000). By contrast, the findings for students in grades 6-8 (in Washington, D.C.) who utilized a voucher to attend a private school did not score significantly higher on math tests, and actually scored significantly lower on reading tests (by 8 percentile points). Based on the responses to questions about acclimation to a new school, the authors hypothesize that the lower scores of the older students resulted from the difficulty of making the adjustment to the new school (Wolf, Howell, and Peterson, 2000). Additional empirical work is needed to understand why the effects would vary so much for students from different racial and ethnic backgrounds and from grade to grade.

Even in extremely well designed studies about the subject, there are a number of reasons why one must be careful about drawing very strong conclusions about public and private school differences from social experiments. First, the randomization must take place at some point, which limits the inferences that can be drawn from the experiment. As mentioned above in the context of the Milwaukee program, the reason is that it is inappropriate to assume that the effects of an intervention (e.g. private schooling) will apply to those who do not seek to be treated by the intervention. In the case of public and private school differences, the randomization to determine who gets treated (i.e. who gets a voucher), may for instance, be among those who expressed an interest in attending private schools. Therefore, the estimated impact on this group that desires

to attend private schools is not necessarily the effect on the general population (Angrist, Imbens, and Rubin, 1996; Heckman and Smith, 1993). For instance, those who did not seek a voucher because they are not interested in attending private schools may not be interested in private schooling because they know that they would not do well in that setting.

One might guess that those who are attending low quality public schools are the least satisfied with public schools and are more likely to apply to participate in a voucher program, narrowing the treatment group sample to those coming from ineffective schools. In that situation, the worst public schools would be compared with private schools, yielding results that might overstate the benefits of private education. On the other hand, one might argue that lower quality private schools are the ones with available capacity to accept voucher-using students. This would argue that the results would tend to understate the general effect of private relative to public schools. There are in fact numerous ways the participating private and public school samples may be misrepresentative of public schools or private schools in general on account of what factors influence the students and schools that participate in such programs.

There is also a question as to whether experiments, by their very nature, may influence actions in a way that provides misleading information about the consequences of implementing programs. One of the main arguments made in favor of enhanced competition is that all students, including students who do not actively choose a school, would benefit from the competition. This argument rests on the notion that even those schools not chosen will change in positive ways as a result of the competition. In an experimental setting, however, public schools may not behave as they would if a program were actually implemented because they know the experiment is slated to end at some point. As a result, even if competition would in fact lead to public school improvements, we may not observe these improvements in such a setting. This suggests that social experiments on school choice might understate the overall positive impact of vouchers. The general point is that there are a number of reasons that social experiments, though they may be a preferable research methodology, may not accurately predict general equilibrium effects of competition. This point is discussed in greater detail in the next section.

## **V. Systemic Effects of Competition**

We tend to think about choice and competition in general equilibrium terms -- that is, what happens to the entire school system — but only measure the partial equilibrium impacts of enhanced choice. For example, most research on public-private school choice has focused on the demand side of schooling (students) rather than the supply side (schools).<sup>15</sup> But, to truly understand the general equilibrium impact of vouchers it is necessary to study the effect they would have on potential new entrants into the market. Theory suggests that the most effective suppliers are likely already in the marketplace, implying the quality of future private school entrants would be lower.<sup>16</sup>

Ultimately for enhanced choice to have a truly profound impact on the educational marketplace it would need to lead to positive changes in the public sector, since that is where most students are likely to remain under any new institutional structure. Competition may have important impacts on all schools, and competition among public schools and between public and private schools exists in today's educational marketplace. But, as was the case with studies of public and private schools, it is difficult to isolate the effects of competition from other factors affecting students' achievement. Studies that test for general equilibrium effects of greater competition compare public school performance across areas with differing degrees of competition from other public and private schools, with the assumption that areas with more competition are otherwise the same, and would therefore not have public schools of varying quality for independent reasons.

However, in practice, the formation of public school district boundaries may be related to the characteristics of the community or to the performance of schools in the district. For example, boundaries may be drawn in such a way that they include or exclude students of varying achievement levels. The relationship between public schools and private schools is also potentially complex. As discussed above, when parents leave the public system, they may be less willing to pay taxes to support public schools. As a result we would expect aggregate public

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<sup>15</sup> One exception is a paper by Downes and Greenstein (1996), which examines the supply decisions of private schools.

<sup>16</sup> This does not imply that enhanced competition would not lead to better overall results since it may affect the behavior of public schools in myriad ways.



school spending to drop. However, as Goldhaber (1999) points out, per pupil expenditures could either rise or fall depending on whether the percentage decline in public school students exceeds the percentage decline in tax revenues designated for public schools. Alternatively, if those with strong preferences for private schools choose to locate in areas with low public school expenditure, then we would observe a negative correlation between public school expenditures and private school enrollment rates. For example, families with strong religious preferences may choose private schooling for the religious aspects of schools, irrespective of academic quality.

Hoxby (2001) argues that competition among public schools should lead to greater public school productivity. In turn this will give parents less incentive to send their children to private schools. In a study testing these hypotheses, Hoxby attempts to account for the potential that public school district boundaries are not exogenously determined. To address this issue, Hoxby uses an IV approach and finds that greater choice among public schools leads to greater productivity, improved school quality, better average student performance, and a smaller share of students attending private schools. The instrumental variable in this study is based on the notion that the concentration of public school districts is related to natural boundaries such as rivers.<sup>17</sup>

Several papers (Couch et. al. (1993), Dee (1998), Epple, Figlio, and Romano (2000), Hoxby (1994; 1996), Sander (1999)) investigate the impact of private sector competition on public schools. Most of these studies take steps in their statistical models to account for the possibility that the relationship between public school performance and private sector competition is complicated by the fact that the supply of private schools in a locality may be partially determined by both the quality of the public schools and the characteristics of the community. Sander (1999) finds no evidence that private sector competition affects public school performance, as measured by graduation rates, test score performance, and college-going behavior. Although the studies use slightly different statistical techniques to account for this possibility, all find that, to various degrees, competition positively impacts public school students. However, as was the case with the other studies using an IV approach, the reliability of

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<sup>17</sup> Grosskopf et al. (2000) examine the same issue using a slightly different approach. They first estimate the amount of inefficiency in each district and then determine, using measures of competition similar to Hoxby, whether competition benefits public schools. They find that there is a wide range of efficiencies among school districts in Texas and that there is less inefficiency in areas with greater competition. Borland and Howsen (1992) use an approach similar to Hoxby's and also find that competition has a positive impact on student achievement on standardized tests.

the results ultimately depend on the appropriateness of the instruments. Kane (1996) clearly illustrates this difficulty in a critique of Hoxby's study (1996) by showing the potential problem that the instruments chosen (the historical percentage of Catholics in each locality) might be correlated with unobservable characteristics (e.g. educational attainment, wages, and test scores) likely to affect outcome variables of interest leading to biased results.

Failure to adequately control for unobserved differences in community preferences, such as choice of locality, may lead to biased estimates of the effect of private schooling on public school expenditure and vice versa. For instance, private sector enrollment may be positively correlated with public school expenditures, community affluence, and demand for education. If this proves true a simple statistical model would tend to overestimate the effect of competition on public school quality. Alternatively, private schools may locate in areas where public schools are of low quality. This effect would tend to lead to an underestimate of the impact of competition in simple econometric models. The bottom line is that failure to adequately account for these potentials could result in the misattribution of the effects of competition.

Another important consideration in evaluating the relative performance of private and public schools is to address the question: who is affected by school choice? As Hsieh and Urquiola (2002) suggest, the sorting of students across schools as a result of choice makes it difficult, if not impossible, to determine the impact of the system on achievement solely by examining whether public schools improve in response to competition or whether students perform better in private schools, instead one must examine changes in the whole education system. The effects of school choice also may not be distributed equally across different types of students. For example, private school attendance may change test scores more for students who are lower performing compared to average students. It is therefore useful to employ evaluation techniques that will yield implications and insights about how changes in policy will impact different types of students differently.

At this point, however, there is little evidence on possible heterogeneous effects of school choice policies. In particular, the types of statistical methodologies used do not allow for private school attendance to have different effects on outcomes at different points of the distribution of the outcome of interest. The typical statistical methodologies (such as Ordinary Least Squares) compare *average* outcomes of private school students with *average* outcomes of public school

students. However, there is a *distribution* of outcomes associated with private school students and a *distribution* of outcomes associated with public school students, and hence comparing only average outcomes yields a somewhat narrow set of implications about relative performance of private and public school students.<sup>18</sup>

The studies discussed above suggest the introduction of competition can bring substantial change to both schools and students. When examining the consequences of school choice however, it is important to consider the reasons why parents send their children to private schools, and how the introduction of choice will affect these decisions. For example, one of the reasons parents send their children to private schools is so their children will be able to associate with other children from similar backgrounds. Parents may consider factors such as the income level or religious affiliation of other students at the school, or possible alumni networks that may help their child gain admission to selective colleges. This interest in such characteristics is often referred to as a “peer effect.” While the empirical evidence on the importance of peer effects is mixed, it is still viewed by many parents to be an important consideration in their children’s education.<sup>19</sup>

To assess the effects of school choice, researchers often compare the outcomes of students from different types of schools, and implicitly assume that sending more students to private schools will yield similar outcomes. However, if school choice was introduced and more children moved from public to private schools, then the composition of the student body at private schools might dramatically change. And, if peer effects are important and students who leave public schools to attend private schools are on average lower achievers than the existing private school students, then average test scores at private schools would fall. Parents of students who already attend private schools may perceive the introduction of school choice to be a means through which the average ability of their child’s peers will be diluted, having possible effects on their own child’s performance. This perceived dilution of the private school peers may extend beyond academic ability into other areas mentioned above, such as the percentage of students from high-income families and students of a particular religious denomination. From a public policy perspective we want to control for sample selection in comparing outcomes

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<sup>18</sup> The estimation technique of quantile regression would help address this issue. For examples see Eide and Showalter (1998) and Eide, Showalter, and Sims (2002).

between public and private schools, but to an individual family making schooling decisions, the self-selecting nature of private school attendance is one of the factors that leads them to consider the private school alternative.

## **VI. Conclusion**

In this paper we have set out to illustrate the variety of issues associated with the empirical evaluation of competition in education, particularly the relative effectiveness of private schools compared to public schools. There are a multitude of factors that can affect the results of studies: the particular question asked by the researcher, the type of data used (e.g. non-experimental, quasi-experimental, or experimental), the statistical technique used to correct for selection bias (if the researcher attempts to correct for this problem), and the particular explanatory variables used in the regression model.

We have also emphasized that competition or choice can take a variety of forms. In fact, it is not true that each type of choice that we have discussed has only one outcome. For instance, each voucher program may carry a unique design depending on the purpose for which it is created. Vouchers, both current programs and those proposed, vary from those that target individuals based on income — as in Milwaukee — to programs that target entire school communities based on school performance. Unique programs will produce unique effects.<sup>20</sup>

Because of such differences across programs, researchers should be careful not to over-generalize results. The effects of a new program are determined by the nature of the intervention, the context in which the intervention takes place, and the treatment group. What is the exact nature of the intervention in terms of scope and eligibility for treatment? Does the intervention have different impacts in different settings? Does the intervention have different impacts on different populations? Are there both short-term and long-term effects of the intervention? Answers to all of these questions are central to assessing the full general equilibrium impact of competition in the educational marketplace.

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<sup>19</sup> See Zimmer and Toma (2000) for a recent review and new evidence on peer effects.

<sup>20</sup> It is also worth noting that we have not discussed the issue of the costs associated with interventions designed to enhance choice and competition. This is an important area to address. For instance, Levin and Driver (1997) and Levin (1998) estimate that the public costs of a voucher plan in a representative US context could raise public educational costs by 25 percent or more.

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