**Estimating Parents’ Valuations of Class Size Reductions**

**Using Attrition in the Tennessee STAR Experiment**

Chris Rohlfs and Melanie Zilora[[1]](#footnote-1)

September 2011

Abstract

This study estimates parents’ valuations of small classes by examining the effects of randomly assigned class type on the decision to remove one’s child from the Tennessee STAR experiment, using a new hedonic estimation strategy that estimates the cash payment that would be required to generate the same difference in attrition rates as was observed between treatment and control groups. In 2010 dollars, our preferred estimates indicate that parents on the margin of sending their children to private schools valued small classes at $2,000 to $18,000 per year relative to a cost of $3,000 per student year.

JEL Classifications: I22, I21, H75, D61, C35

Student-teacher ratios are a topic of considerable interest to economists, educators, and policymakers, and class size reductions in elementary school have been shown to increase standardized test scores and college attendance (Angrist and Lavy, 1999; Chetty, *et al.*, forthcoming; Krueger, 1999, 2003; Krueger and Whitmore, 2001). To better evaluate the usefulness and cost-effectiveness of these policies, it is desirable to combine the various benefits of class size reductions into a composite, dollar-denominated measure of their full economic benefits as perceived by parents. Doing so can help to determine the extent to which parents recognize, internalize, and value the various benefits that class size reductions provide. The current study contributes to the literature on class size by applying new hedonic methods to parents’ attrition decisions in the Tennessee Student Teacher Achievement Ratio (STAR) class size experiment to measure the economic value that parents place on class size reductions.

Previous estimates of parental valuations of class size reductions rely upon regression-based comparisons of housing prices across school attendance areas with varying student-teacher ratios (Brasington, 1999; Brasington and Haurin, 1996, 2006). Such approaches are likely to produce biased results, as student-teacher ratios are correlated with key determinants of home value that are difficult to measure, such as neighbor attributes, crime, prices of local goods, and other determinants of school quality such as peer characteristics and teacher quality. Some quasi-experimental studies avoid these neighborhood-level biases by examining discontinuities in housing demand across school district boundaries (Bayer, Ferreira, and McMillan, 2007; Black, 1999). These discontinuity studies estimate parental demand for living in districts with higher average test scores; however, district-level differences in test scores are mainly driven by parental inputs, and these studies do not provide information about the demand for specific school policies (Brasington and Haurin, 2009). Additionally, as Rohlfs (2010b) notes, any unobserved “reverse causal” effects of school test scores on demographics, crime, the prices of local goods, or other neighborhood characteristics could bias such estimates.

Our estimation strategy builds upon the idea of Philipson and Hedges (1998) of examining sample attrition to measure the degree to which experimental subjects regard a treatment as desirable. In doing so, this study contributes to a growing literature that applies the data from the Tennessee STAR experiment in new ways (*e.g.*, Chetty, *et al.*, forthcoming; Dee, 2004; Dee and Keys, 2005; Ding and Lehrer, 2010a, 2010b, 2010c; Sojourner, 2010). Attrition among students in Project STAR is often cited as an important limitation of the experiment (*cf.*, Hanushek, 1999; Krueger, 1999; Sojourner, 2010). However, there is much that can be learned from sample attrition, and in the current study, attrition behavior provides a unique opportunity through which to identify families’ preferences for the different class types.

To illustrate the approach used here, Figure 1 plots hypothetical demand curves for private school among parents whose children were assigned to small and regular sized classes. Transferring to a private school is predicted to be less desirable for those assigned to small classes than for those assigned to regular classes in the public school. The effect of class type on the fraction leaving the school system is shown along the horizontal axis. The vertical difference between the two curves identifies the value placed on class size reductions by families on the margin of sending their children to private school. This value is measured as the quantity change multiplied by the slope of the demand curve and can be viewed as the private school voucher amount that would cause the same number of students to switch to private school as being in regular classes did. The slope of the demand curve is estimated from application rates for private school voucher programs in various cities. In order for this approach to be valid, we require that the slope of the demand for private school is similar between the families in the class size and voucher studies. The econometrics of this procedure are described in Rohlfs (2010b), and applied in Rohlfs (2010a) to measure the cost of the Vietnam draft.

Among students who entered Project STAR in kindergarten, we find, as previous authors do, that those assigned to small classes were three to five percentage points less likely to change schools in the first year than were those assigned to regular-sized classes (Hanushek, 1999; Krueger, 1999; Nye, *et al*., 2000; Schanzenbach, 2006), with much of this attrition due to students leaving the public school system. This response was largest in inner city areas and their suburbs, which had the most private school options. Relative to a lottery between a regular class and a regular class with an aide, we find that inner city parents on the margin of switching to private school valued a year in small class at $7,000 to $20,000 in 2010 dollars. For families living in less urban areas, our estimates are less precise but fall within similar ranges.

In addition to measuring parents’ valuations of small classes, this study illustrates a valuable new methodology for revealed preference estimation. Whenever leaving an experiment involves a costly sacrifice such as changing schools or health care providers, moving, or forgoing a cash payment, this approach can be used to measure subjects’ valuations of the treatments. Unlike previous revealed preference methods, estimation is transparent and straightforward and does not involve restrictive assumptions about market competitiveness, consumers’ utility functions, or the exogeneity of product attributes.

**II. Background and Descriptive Results on Attrition in Project STAR**

A. Overview of Project STAR and the Data

Through the Tennessee STAR experiment, each student in the 1985 kindergarten cohort in participating public schools in Tennessee was randomly assigned to a small class of 13-17 students, a regular sized class of 22-25 students, or a regular sized class with a teacher’s aide. The experiment lasted through third grade. The initial STAR cohort consisted of 6,325 students in 325 classes. Additional waves of students entered the experiment in later grades; however, the randomization of class type does not appear to have been effective for these later entry cohorts, and among the later cohorts, class type is correlated with many student and teacher characteristics. Following some recent studies on Project STAR, we exclude these later cohorts from our analysis (Ding and Lehrer, 2010a, 2010c); analyses including these later entry waves appear in the web appendix. The experiment and the associated data are described extensively in Finn and Achilles (1990, 1999), Grissmer, *et al.* (2006); Hanushek (1999), Krueger (1999), Schanzenbach (2006), and Word, *et al.* (1990) and are provided by Health and Research Operative Services, Inc. (2009).

Table 1 shows descriptive statistics for the kindergarten entry cohort, broken down by initial class type.[[2]](#footnote-2) This table is patterned after two similar tables in Krueger (1999) but presents some additional variables and uses a different set of sample restrictions. The means for students in small, regular, and regular with aide classes appear in columns (1), (2), and (3). Column (4) shows the p-value for an F-test of the null hypothesis that the three means are equal. The number of classes of different types varied across schools due to their numbers of students, and, following Krueger (1999), column (5) shows the p-value for the same F-test after controlling for school fixed effects. Following Chetty, *et al.* (forthcoming), all of the F-tests are adjusted for clustering by school; similar results are obtained without clustering or with clustering by teacher.

Within each column, the sample consists of students with nonmissing values for all of the variables shown; the row labeled “missing data” shows the fraction of observations dropped due to this restriction. Most of the missing values are for “special instruction” and “days absent.” Using all of the available observations has little effect on the F-tests. Unlike with previous studies, the sample is not restricted to students with valid test scores. Of the 79 schools that participated in the experiment, four dropped out after the first or second year; students who entered the program in these schools are excluded from both the “nonmissing” and “total” samples presented here.[[3]](#footnote-3)

The results from Table 1 indicate that the randomization appears to have generally been effective for the kindergarten cohort. We fail to reject the null hypothesis of equality across class types for all of the student and teacher controls, whether or not we control for fixed effects. Additionally, we effects of class type on three outcome variables, with varying degrees of significance: class size (significant with or without school fixed effects), days absent (marginally significant without fixed effects, significant with them), and missing data (significant with no fixed effects, insignificant with them).[[4]](#footnote-4)

B. Attrition in Project STAR

Figure 2 provides detailed evidence on the forms of attrition that occurred in Project STAR and how they varied across class types. Panels A, B, and C correspond to students initially assigned to small, regular, and regular with aide classes. The upper left portion of each pie chart shows the fraction of students who remained in the program in their assigned grades, class types, and schools into third grade. Clockwise from the upper left, the remaining five pieces of the pies show the fractions leaving the program, the student switching to another school in Project STAR, switching to another grade or a Tennessee public school not in the experiment, leaving the public school system, or changing class type in the same school. The entire sample is included in these graphs. Among those students who left Project STAR, the fraction remaining in Tennessee Public Schools is estimated based on which students have school identification numbers in the Tennessee Comprehensive Assessment Program (TCAP) Achievement Test data in 1990 or 1991, the first two years of the TCAP data and the years that most of the STAR cohort were in fourth and fifth grades (Tennessee Department of Education, 1990-1997).[[5]](#footnote-5)

As these pie charts show, 46% of those initially assigned to small classes were still in their assigned schools and class types in third grade; for students assigned to regular and regular with aide classes, this percentage is considerably lower at 18% and 23%. In response to pressure from the parents of students who entered the program in regular and regular with aide classes, these students were given new, randomly assigned class types after the first year. Due to this re-randomization of class type, changes of class type within school occurred for 30% and 24% of those initially assigned to regular and regular with aide classes, as compared with only 5% of those initially assigned to small classes. Additionally, class type appears to have affected switching to other public schools or grades (28% in regular and aide classes versus 27% in small classes) and leaving the public school system (17% in the regular and regular with aide classes versus 15% from the small classes).

**III. Conceptual Framework**

A. Hedonic Framework

The econometric strategy used here to estimate the value of class type is taken from the “matching and audit” procedure in Rohlfs (2010b, pp. 11-13). The demand for private school may vary across parents for many reasons, some of which are unobserved by the researcher. We assume that, among students assigned to regular sized classes, the Cumulative Distribution Function (CDF) for this valuation is linear in the price of private school, as in the upper demand curve in Figure 1. The supply curve for private school may be elastic or inelastic. The horizontal line for the price that is shown in Figure 1 reflects the assumption that the law of one price holds, so that the cost of attending private school does not vary with one’s assigned class type in Project STAR. Being assigned to a small (or a regular with aide) class is predicted to reduce the family’s demand for private school; the amount of this reduction may vary across families. The estimation procedure used in this study identifies the dollar-denominated vertical difference in the demand curve (termed “marginal surplus” in Rohlfs, 2010b) for a family on the margin of sending their child to private school.

The model in Rohlfs (2010b) allows for a very general set of preferences and imposes few restrictions on the supply of private schooling or demand and supply in other markets. To measure the slope of the demand curve, we examine the application rates to six private school voucher programs in urban areas around the U.S. Our key identifying assumptions are that: (1) the randomly assigned class type is uncorrelated with other determinants of the demand for private school, (2) unbiased estimates of the slope of the demand curve for private school can be obtained, and (3) the slope of the demand curve for private school in the voucher programs is similar to the slope of the demand for leaving a Project STAR school.

In the Project STAR data, our key measure of attrition is changing schools; in the voucher samples, our key measure of the demand response is switching to a private school. Hence, switches between public schools (which generally occur through changing residences), both within and between states, are recorded as attrition in the Project STAR data but not in the voucher studies. For some families wishing to leave the experiment, changing residences may have been less costly than switching to private school. To the extent that the treatment affected families’ rates of changing residences, the cost of switching to private school overstates the cost of sample attrition, and our procedure overstates parents’ valuations of class size reductions.

For most families in Project STAR, switching to private school was probably less costly (or at most only slightly more costly) than moving residences, and this bias is probably small. Additionally, the slope of the demand curve for private school is the best available measure of the slope of the demand curve for attrition from the experiment. As Figure 2 showed, the rates of switching within the Tennessee Public School System were slightly lower for kindergarteners in small classes than for those in larger classes. However, the majority of the difference in attrition rates was attributable to students leaving the Tennessee public school system.[[6]](#footnote-6)

B. Estimation Equations

To measure the effect of class type on the fraction leaving the sample, we consider the following linear regression equations:

1. ,
2. ,

where , , and are indicators for whether student changed schools, changed class types in the same school, or was recommended to repeat a grade before grade . and are dummies representing student ’s initially assigned class type of small or regular with aide (so that regular class is the omitted category), and is a vector of control variables. The effects of , , and are allowed to vary by year.

B. Normalized Attrition Response

The benefit of staying in a Project STAR school depended in part on the number of remaining years that one expected to be enrolled in a small class after the first year. Among students who remained in their assigned Project STAR schools through third grade, suppose that the number of years spent in small classes after kindergarten is described as:

1. .

A student who was assigned to a small class in kindergarten could expect to remain in a small class through third grade. Due to the re-randomization of class type, a student initially assigned to a regular or a regular with aide class could expect, after kindergarten, to be switched to any of the other class types. The probabilities of moving to the different class types in first grade were very similar for students in regular and regular with aide classes. Estimates of Equations (1) and (4) that include as a regressor find that it has small and insignificant effects on changing schools or on remaining years in small or aide classes. To construct our estimates of parents’ valuations of class size reductions, we suppose that the effects of are zero.

For a student who chooses to remain in Project STAR, being initially assigned to a small class has associated with it more expected future years in a small class than does an initial assignment in a regular or regular with aide class. Hence, from Equation (1) measures the attrition effect of expecting future years of small class as compared to the alternative of years of a lottery between regular or a regular with aide. In order to measure the value of a change in class type, it is useful to normalize the attrition response from Equation (1) by to produce , the degree to which one future year of small classes affects student attrition. Dividing by the slope of the demand curve for private school (as estimated in appendix A) then produces an estimate of the value parents place on one future year of small classes.

C. Demand for Private School

Our estimates of the slope of the demand curve for private school come from the application rates to three private school voucher experiments (in New York City, Dayton, OH, and Washington, D.C.) and three policy-based natural experiments (in Milwaukee, WI, San Antonio, TX, and Pensacola, FL). In each case, poor families in urban areas were given the opportunity to apply for vouchers. In the San Antonio and Pensacola cases, vouchers were provided with certainty; in the other cases, vouchers were provided to a randomly selected subset of those completing the application process. In each case, the sample is restricted to students in public school at the time of application. We consider a discrete choice model that estimates the slope of the demand curve for private school based upon the fraction who apply to the program and the fraction who would switch to private school in the absence of the program.[[7]](#footnote-7) We consider application costs of $0 and $200 and discount rates (for multi-year voucher programs) of 0% and 50%. We estimate that, for the schools in Project STAR, the fraction who would switch to private school in response to a $1,000 voucher (the reciprocal of the slope in Figure 1 divided by $1,000) ranges from 0.002 to 0.006 for inner cities and their suburbs and 0.0004 to 0.001 for less urban areas. Detailed descriptions of this discrete choice model, our calculations, and a comparison between the voucher and STAR samples appear in the appendix to this paper.

**IV. Results**

A. Effects of Initial Class Type on Changes in School, Class Type, and Grade

Following Equations (1) to (3), results in Table 2 are presented using the three different measures of sample attrition as dependent variables: changes in school (in columns 1, 2, 5, and 6), changes in class type (in columns 3, 4, 7, and 8), and grade repetition (in columns 9 and 10). In columns (1) to (4), the dependent variable indicates whether that form of attrition happened after kindergarten, and in columns (5) to (10), the dependent variable indicates whether that form of attrition happened by third grade. Grade repetition is measured based on whether the child’s teacher recommended promotion to the next grade. Grade repetition data are not available for kindergarteners, as kindergarten was not mandatory in Tennessee Public Schools at the time (Krueger, 1999, pg. 501). The STAR data do not indicate whether students who left the experiment changed schools or grades. For the “changed school” outcome variable, all kindergarteners who left the sample are assumed to have changed schools, as are first and second graders who were recommended for promotion to the next grade and left the sample. Additionally, students who remained in the experiment but changed school ids are counted as having changed schools. In the odd-numbered columns, the regressions include a constant and indicators for small class and regular class with aide. The even-numbered columns control for student and teacher characteristics and school fixed effects as listed in the footnotes to the table. The sample is the same as in Table 1.

The results from Table 2 confirm the general findings from Figure 2. Among students who entered Project STAR in kindergarten, relative to students initially assigned to regular classes, those initially assigned to small classes were 3.3 to 4.8 percentage points less likely to change schools in the first year and 0.8 to 2.4 percentage points less likely to change schools by third grade. Of these four differences, one is statistically significant, and one is marginally significant. Not surprisingly given the re-randomization of class type, we do not observe a consistent pattern of differences in school changing behavior between those initially assigned to regular and those assigned to regular with aide classes. Due to the re-randomization, we observe large and significant negative effects of small class on changing class type in panel A. After the first year, relative to those initially assigned to regular classes, those initially assigned to regular with aide classes were 2.9 to 3.0 percentage points more likely to have changed class types within schools – a difference that is not significant and is partly attributable to the slightly lower fraction of regular with aide students changing schools. This difference reverses by third grade, at which point students initially assigned to regular with aide classes are 6.3 to 7.1 percentage points less likely than those assigned to regular classes to have changed class types – a difference that is statistically significant. This lower rate of class type changes among regular with aide students may reflect a high degree of pressure (or sensitivity to pressure) applied by parents of students who spent multiple years in the regular class type. In columns (9) and (10), we observe a moderate-sized but statistically insignificant negative effect of small class assignment on having a STAR teacher recommend grade repetition by third grade.

*Estimates of Equations (1) and (4) by Level of Urbanization, Omitting Aide as a Regressor*

In order to construct a better match between the estimated attrition effects in Project STAR and the voucher estimates (which are based upon students in large cities), we measure the effects of class type separately by the level of urbanization of the school’s location. The quality of local private and public school options is likely to be an important potential determinant of the willingness to pay to change schools. For example, in Appendix A of this study, we find a considerably less elastic response to school vouchers in Milwaukee, where the choice of private schools was restricted, than in other cities where it was not. Recent data from Tennessee show that, of the 580 private schools grades pre-kindergarten through eight in the state, 48% are located in the most urban parts of the state, with a high concentration near Memphis (Private School Review, 2011).[[8]](#footnote-8)

Table 3 shows estimates of Equations (1) and (4) separately for inner city, suburban, urban not inner city, and rural schools. Panel A shows estimates of Equation (1), where the dependent variable is an indicator for having changed schools after the first year in Project STAR. Panel B shows estimates of Equation (4), where the dependent variable is the number years spent in a small class after kindergarten, where the sample is restricted to students who remained in their assigned schools through grade three. Columns (1) and (2) show estimates for inner city schools (88% of which were located in Memphis; Word, *et al.*, 1990, pg. 5). Columns (3) and (4) show estimates for the suburbs of Knoxville, Nashville, Memphis, and Chattanooga, columns (5) and (6) show estimates for smaller urban areas, and columns (7) and (8) show estimates for rural areas. The odd-numbered columns show specifications with no controls, and the even-numbered columns show specifications in which controls and school fixed effects are included. In each column, Equations (1) and (4) are estimated using Seemingly Unrelated Regression (SUR), so that standard errors for ratios of coefficients can be obtained for our estimates of the attrition response per yearlong change in class type.[[9]](#footnote-9)

In panel A, we find large and significant negative effects of -0.09 for inner city schools smaller and less significant effects ranging from -0.05 to -0.06 for suburban schools, and even smaller and insignificant effects of -0.01 to -0.02 for rural schools. For smaller urban areas, we obtain two positive (though very imprecise) estimates of +0.03 and +0.04. For Equation (4) in panel B, we observe highly significant and stable positive effects indicating that, relative to assignment in a regular or regular with aide class, initial assignment in a small class was associated with 2.3 to 2.6 more years in a small class from grades one to three.

B. The Economic Value of Class Type

Next, in Table 4, we present our estimates of the normalized attrition response to a future year of small classes, and we consider the range of economic valuations implied by these attrition responses using the range of voucher responses estimated in the Appendix A. Estimates are presented separately by the level of urbanization of the school’s location. All eight columns estimate the yearlong response to a year of small class relative to a lottery between regular and regular with aide class (with a slightly higher likelihood of regular with aide). The form of attrition measured in all eight columns is leaving the school after the first year of Project STAR. The normalized attrition response is estimated as , where the estimates of and from Equations (1) and (4) are shown in Table 3. The specifications and samples are the same as in Table 3, with the ratios of coefficients estimated using the delta method.

The rows below the attrition effect per yearlong change in Table 3 present estimates of the economic value of a yearlong change in class type. For the inner city and suburban schools, these estimates are constructed by dividing the attrition effect per yearlong change by $1,000 times the higher and lower estimates of 0.006 and 0.002 of the effect of a $1,000 voucher on the likelihood of switching schools. We use the smaller range of voucher responses from the Milwaukee study (0.001 and 0.0004 per $1,000) for the small urban and rural areas to account for those areas’ smaller selection of private school options. The data sources and calculations for those ranges are discussed in Appendix A.

The estimates from Table 4 indicate that the school switching response is most prevalent in the inner city areas, with statistically significant attrition effects of -0.039 per year of small classes in both specifications. This response is second largest in the suburbs of those areas, with attrition effects of -0.020 to -0.025 per year of small classes; one of these two estimates is marginally significant. Our estimated valuation of a year of small classes ranges for inner city students from $6,528 to $19,702 and for suburban students from $3,381 to $12,393, with average estimates across the four specifications of $13,095 for inner city students and $7,512 for suburban students. The smaller estimates for suburban areas may reflect a smaller price elasticity of demand for private schools (due to better public school options and longer commutes to private schools in those areas), in which case the higher range of estimated valuations would be more appropriate for suburban areas. Our estimates are imprecise for the smaller urban areas, due to the lower number of observations, and we find smaller and statistically insignificant effects for smaller urban and for rural areas. We obtain a negative valuations of -$11,180 to -$37,730 in for small urban, presumably due to the imprecision for that sample, and our valuation estimates for rural range from $3,639 to $19,670.

Krueger (1999, pg. 530) calculates the cost of reducing kindergarten class size from regular to small is roughly $3,000 per student per year in 2010 dollars. For inner city, suburban, and rural families, our estimated benefits to parents all exceed this cutoff for cost-effectiveness. While their estimates are imprecise, Chetty, *et al.* (forthcoming) find that the effects of class size on earnings are close to zero and possibly negative. Hence, our estimates of parents’ valuations of class size reductions may far exceed the true lifetime earnings gains; however, it is not clear how parents’ valuations of small classes compare to their beliefs about the earnings effects.

**V. Conclusion**

This study presents new estimates of the parents’ valuations of class size reductions and teachers’ aides in elementary school classes. A revealed preference approach is used that examines the effects of students’ assigned class types in the Tennessee STAR class size experiment on the decision to change schools or class types within the experiment. These effects are converted into dollar valuations by comparing them with estimates from other studies of the per dollar effect of private school vouchers on the decision to change schools. While imprecise, our preferred estimates indicate that, among students who entered Project STAR in kindergarten, relative to the alternative of a lottery between a year in a regular or a year in a regular with aide class, inner city parents on the margin of sending their children to private school valued one year of a small class at $7,000 to $20,000 in 2010 dollars. While less imprecisely estimated, we obtain similar ranges of valuations for parents from less urban areas. In general, our estimated benefits to parents from class size reductions appear to exceed the $3,000 cost, and in some cases, they exceed this cost by a substantial amount.

**References**

**Achilles, Charles M., 2010.** Personal interview, November 15.

**Angrist, Josh D., and Victor C. Lavy, 1999.** “Using Maimonides’ Rule to Estimate the Effect of Class Size on Scholastic Achievement,” *Quarterly Journal of Economics, 114*(2), pp. 533-75.

**Bayer, Patrick, Fernando Ferreira and Robert McMillan, 2007.** “A Unified Framework for Measuring Preferences for Schools and Neighborhoods,” *Journal of Political Economy, 115*(4), pp. 588-638.

**Black, Sandra, 1999.** “Do Better Schools Matter? Parental Valuation of Elementary Education,” *Quarterly Journal of Economics, 114*(2), pp. 577-99.

**Brasington, David M., 1999.** “Which Measures of School Quality Does the Housing Market Value?” *Journal of Real Estate Research 18*(3), pp. 395-413.

**Brasington, David M., and Donald R. Haurin, 1996.** “The Impact of School Quality on Real House Prices: Interjurisdictional Effects,” *Journal of Housing Economics 5*(4), pp. 351-368.

**Brasington, David M., and Donald R. Haurin, 2006.** “Educational Outcomes and House Values: A Test of the Value-Added Approach,” *Journal of Regional Science 46*(2), p p.245-268.

**Brasington, David M., and Donald R. Haurin, 2009.** “Parents, peers, or school inputs: Which components of school outcomes are capitalized into house value?” *Regional Science and Urban Economics, 39*(5), pp. 523-9.

**Caetano, Gregorio, 2009.** “Identiﬁcation and Estimation of Parental Valuation of School Quality in the U.S.” Unpublished paper.

**Campbell, David E., Martin R. West, and Paul E. Peterson, 2005.** “Participation in a National, Means-Tested School Voucher Program,” *Journal of Policy Analysis and Management, 24*(3), pp. 523-541.

**Card, David and Alan B. Krueger, 1992.** “Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States,” *The Journal of Political Economy, 100*(1), pp. 1-40.

**Chakrabarti, Rajashri, 2005.** “Can Increasing Private School Participation and Monetary Loss in a Voucher Program Affect Public School Performance? Evidence from Milwaukee,” Harvard University.

**Chakrabarti, Rajashri, 2008.** “Impact of Voucher Design on Public School Performance: Evidence from Florida and Milwaukee Voucher Programs,” Staff Reports, Federal Reserve Bank of New York.

**Chetty, Raj, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan, forthcoming.** “How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star,” *Quarterly Journal of Economics*.

**Cowen, Joshua M., 2008.** “School Choice as a Latent Variable: Estimating the ‘Complier Average Causal Effect’ of Vouchers in Charlotte.” *The Policy Studies Journal, 36*(2).

**Dee, Thomas, 2004.** “Teachers, Race, and Student Achievement in a Randomized Experiment,” *Review of Economics and Statistics*, *86*(1), pp. 195-210.

**Dee, Thomas and Benjamin Keys, 2005.** “Does Merit Pay Reward Good Teachers? Evidence from a Randomized Experiment,” *Journal of Policy Analysis and Management*, *23*(3), pp. 471-88.

**Ding, Weili and Steven Lehrer, 2010a.** “Estimating Context-Dependent Treatment Effects in Education Experiments,” Unpublished manuscript.

**Ding, Weili and Steven Lehrer, 2010b.** “Estimating Treatment Effects from Contaminated Multiperiod Education Experiments: The Dynamic Impacts of Class Size Reductions,” *Review of Economics and Statistics*, *92*(1), pp. 31-42.

**Ding, Weili and Steven Lehrer, 2010c.** “Estimating Treatment Effects in the Presence of Psychological Biases in Education Experiments.” Unpublished manuscript.

**Epple, Dennis and Holger Sieg, 1999.** “Estimating Equilibrium Models of Local Jurisdictions,” *Journal of Political Economy,107*(4), pp. 645-81.

**Finn, Jeremy D., and Charles M. Achilles, 1990.** “Answers and Questions about Class

Size: A Statewide Experiment,” *American Educational Research Journal,27*, pp. 557-77.

**Finn, Jeremy D., and Charles M. Achilles, 1999.** “Tennessee’s Class Size Study: Findings, Implications, Misconceptions,” *Educational Evaluation and Policy Analysis,21*(2), pp. 97-109.

**Greene, Jay P., 2000.**“The Effect of School Choice: An Evaluation of the Charlotte Children’s Scholarship Fund Program,” Civic Report No. 12.

**Greene, Jay P., 2001.** “An Evaluation of the Florida A-Plus Accountability and School Choice Program,” Program on Educational Policy and Governance, Harvard University.

**Greene, Jay P. and Daryl Hall, 2001.** “The CEO Horizon Scholarship Program: A Case Study of School Vouchers in the Edgewood Independent School District, San Antonio, Texas,” Mathematica Policy Research, Inc.

**Greene, Jay P., Paul E. Peterson, and Jiangtao Du, 1999.** “Effectiveness of School Choice: the Milwaukee Experiment,” *Education and Urban Society*, *31*(2), pp. 190-213.

**Grissmer, David and Ann Flanagan, 2006.** “Improving the Achievement of Tennessee Students: An Analysis of the National Assessment of Educational Progress,” TR-381-EDU, RAND Corporation.

**Hanushek, Eric A., 1999.** “Some Findings from an Independent Investigation of the Tennessee STAR Experiment and from Other Investigations of Class Size Effects,” *Educational Evaluation and Policy Analysis, 21*(2), pp. 143-63.

**Health and Education Research Operative Services, Inc., 2009.** Project Student-Teacher Achievement Ratio Data. Available at: <http://www.heros-inc.org/data.htm>

**Howell, William G. and Paul E. Peterson, 2000.** “School Choice in Dayton, Ohio: An Evaluation After One Year,” Program on Educational Policy and Governance, Harvard University.

**Howell, William G. and Paul E. Peterson, 2006.** *The Education Gap: Vouchers and Urban Schools.* Washington, D.C.: Brookings Institute Press.

**Howell, William G., Patrick J. Wolf, David E. Campbell, and Paul E. Peterson, 2002.** “School Vouchers and Academic Performance: Evidence from Three Randomized Field Trials,” *Journal of Policy Analysis and Management, 21*(2), pp. 191-217.

**King, Elizabeth M., Peter F. Orazem, and Darin Wohlgemuth, 1998.** “Central Mandates and Local Incentives: The Colombia Education Voucher Program,” *World Bank Economic Review, 13*(3) pp. 467-91.

**Krueger, Alan B., 1999.** “Experimental Estimates of Education Production Functions,” *Quarterly Journal of Economics, 114*(2), pp. 497-532.

**Krueger, Alan B. and Diane M. Whitmore, 2001.** “The Effect of Attending a Small Class in the Early Grades on College-Test Taking and Middle School Test Results: Evidence from Project STAR,”*Economic Journal*, *111*(468), pp. 1-28.

**Mayer, Daniel P., Paul E. Peterson, David E. Myers, Christina C. Tuttle, and William G. Howell, 2002.** “School Choice in New York City After Three Years: An Evaluation of the School Choice Scholarships Program,” Mathematica Policy Research, Inc.

**Merrifield, John D., 2010.** “Education Vouchers Benefit Edgewood Economy.”National Center for Policy Analysis Brief Analysis No. 714.

**Mosteller, Frederick, 1995.** “The Tennessee Study of Class Size in the Early School Grades,” *Critical Issues for Children and Youths, 5*(2), pp. 113-127.

**Nye, Barbara, Larry V. Hedges, and Spyros Konstantopoulos, 2000.** “The Effects of Small Classes on Academic Achievement: The Results of the Tennessee Class Size Experiment,” *American Educational Research Journal, 37*(1), pp. 123-151.

**Peterson, Paul E., Jay P. Greene, William G. Howell, and William McCready, 1998.** “Initial Findings from an Evaluation of School Choice Programs in Washington, D.C. and Dayton, Ohio,” Program on Educational Policy and Governance, Harvard University.

**Peterson, Paul E., David E. Myers, and William G. Howell, 1999.** “An Evaluation of the Horizon Scholarship Program in the Edgewood Independent School District, San Antonio, Texas: The First Year,” Mathematica Policy Research, Inc. and Program on Education Policy and Governance, Harvard University.

**Philipson, Tomas and Larry V. Hedges, 1998.** “Subject Evaluation in Social Experiments,” *Econometrica, 66*(2), pp. 381-408

**Private School Review, 2011.** “Tennessee Private Schools by County.” Available at: <http://www.privateschoolreview.com/state_private_schools/stateid/TN>

**Ritsche, Daniel F., 2006.** “Milwaukee Parental Choice Program.”State of Wisconsin Legislative Reference Bureau Brief No. 06-15.

**Rohlfs, Chris, 2010a.** “The Economic Cost of Conscription and an Upper Bound on the Value of Life: Hedonic Estimates from Two Margins of Response to the Vietnam Draft.”Unpublished paper.

**Rohlfs, Chris, 2010b.** “A Generalized Theory of Hedonic Estimation Using Experimental and Quasi-Experimental Designs,”Unpublished paper.

**Rouse, Cecilia E, 1998.** “Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program,” *The Quarterly Journal of Economics, 113*(2), pp. 553-602.

**Ruggles, Steven J., Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. 2010.** Integrated Public Use Microdata Series: Version 5.0 [Machine-readable database]. Minneapolis: University of Minnesota.

**Schanzenbach, Diane W., 2006.** “What Have Researchers Learned from Project STAR?,” Harris School Working Paper Series 6.06, University of Chicago.

**Sojourner, Aaron, 2010.** “Identification of Peer Effects with Missing Peer Data: Evidence from Project STAR,” Unpublished manuscript.

**Tennessee Department of Education, 1990-1997.** Tennessee Comprehensive Assessment Program (TCAP) Achievement Test Data.

**Texas Education Agency, 1999.** “Snapshot: School District Profiles.” Available at: <http://ritter.tea.state.tx.us/perfreport/snapshot/99/index.html>

**U.S. Census Bureau, 2000.** “Population Estimates for Places (Sorted Alphabetically Within State): Annual Time Series, July 1, 1990 to July, 1, 1999.” Available at: <http://www.census.gov/popest/archives/1990s/SU-99-07.html>

**West, Martin R., Paul E. Peterson, and David E. Campbell, 2001.** “School Choice in Dayton, Ohio after Two Years,” Program on Educational Policy and Governance, Harvard University.

**Witte, John F., 1991.** “First Year Report, Milwaukee Parental Choice Program,” University of Wisconsin-Madison.

**Witte, John F., 1998.** “The Milwaukee Voucher Experiment,” *Educational Evaluation and Policy Analysis, 20*(4), pp. 229-251.

**Wolf, Patrick J., 2008.** “School Voucher Programs: What the Research Says About Parental School Choice,” Brigham Young University Law Review, *2008*(2), pp. 415-446.

**Wolf, Patrick J., William G. Howell, and Paul E. Peterson, 2000.** “School Choice in Washington, D.C.: An Evaluation After One Year,” Program on Educational Policy and Governance, Harvard University.

**Wolf, Patrick J., Paul E. Peterson, and Martin R. West, 2001.** “Results of a School Voucher Experiment: The Case of Washington, D.C. After Two Years,” Annual Meetings of the American Political Science Association.

**Word, Elizabeth, John Johnston, Helen Pate Bain, B. DeWayne Fulton, Jayne Boyd Zaharias, Charles M. Achilles, Martha Nanette Lintz, John Folger, and Carolyn Breda, 1990.** *The State of Tennessee’s Student/Teacher Achievement Ratio (STAR) Project: Technical Report 1985-1990,* Tennessee State Department of Education

**Appendix: The Demand for School Vouchers**

A. Model of Demand for Private School

To translate the decision to leave the Tennessee STAR Experiment into financial terms, we consider how students and parents respond to financial incentives to change schools. Consider a population of poor families with children currently enrolled in public school. To simplify the notation, suppose that each family has one child. Let denote the dollar-denominated surplus that family would obtain from switching its child to private school, where measures family-specific taste and cost factors that are known to the family at the time that it applies for the voucher program, and is a mean zero shock that occurs between the time of application and the time that the enrollment decision is made, possibly due to the admission process at the private school. For simplicity, suppose that and are independent. Let and denote the population density functions for and among families eligible for the vouchers, and let denote the density of . In the absence of any subsidy, the fraction of families switching to private school is .

The ideal experiment for measuring the slope of the demand curve for private school would be to randomly assign vouchers across individuals in the population. Among families in a treatment group receiving a voucher amount , the fraction switching to private school would be , and the per dollar effect of a voucher on the probability of switching, averaged over the domain from to , could be measured as the treatment-control difference in the fraction switching to private school divided by the voucher amount. Unfortunately, a comparison of this form is not possible using the data on available voucher experiments, because previous voucher experiments involved lengthy application processes, and voucher/no voucher designations were randomly assigned among the subset of families who completed applications. Consequently, a comparison of private school attendance between the treated and untreated groups provides an estimate of the demand for private school for a non-random subset of families with especially high demand for private school.

In order to identify the per dollar effect of vouchers on switching schools using the available data, we focus on application rates for the voucher programs. Given a program that provides a voucher of with probability and supposing that the cost of applying is , the expected surplus from applying for the voucher program is for those who would attend in the absence of the voucher and for those who would not. Taking the expectation over , a family that maximizes expected surplus applies for the voucher program iff:

(A1) .

This condition states that the expected value of the surplus from the voucher program, with expectation taken over the probability of winning and the distribution of , equals or exceeds the application cost. Assuming that the voucher provides sufficient value to overcome the application costs, so that , the fraction of eligible families who apply can be written as:

(A2) ,

Hence, the fraction applying to the voucher program is the fraction of families who would switch to private school given a voucher of .

In the context of this discrete choice model, any family that expects to switch to private school even without a voucher (*i.e.*, ) would apply for the program. Among the families who apply and do not win vouchers, those with would still switch to private schools. The fraction of families who apply, do not win vouchers, and still switch to private school can be expressed as:

(A3) .

The average slope of the demand curve for private school over the subsidy range from to can now be expressed as . Using this formula, we can calculate the slope of the demand curve for vouchers based upon aggregate statistics presented in the previous voucher studies – in particular, the application rate, the take-up rate, and features of the program itself, including the application cost, the voucher amount, and the likelihood of the voucher being awarded.

B. Design and Descriptive Statistics for Six Voucher Programs

Our estimated voucher demand effects come from two sources: randomized controlled trials and policy-based natural experiments. Columns (1) to (4) of Table A1 describe the key features of those six voucher programs, including the location, school year, the grades and income levels of students who were eligible, and the voucher amount and number of years of vouchers that an award represented. Columns (5) to (9) describe statistics relevant to the take-up rates of vouchers in these programs, including the number of eligible children, the fraction of eligible children for whom applications were completed, the fraction of applicants who were awarded vouchers, the fraction of voucher winners who used their vouchers, and the fraction (when available) of non-winners who attended private school. It is these statistics that are used to compute the demand curves for private school. To facilitate comparability across programs, for the programs in Milwaukee and San Antonio that were offered in multiple years, data are only presented for the first year.

The voucher experiments listed in rows one to three follow a common structure. In each case, vouchers were introduced in a single year, and students from multiple grades could apply, provided that their families lived below a specified income level (ranging from 185% to 270% of the poverty line). Applying was often a lengthy process, and vouchers were awarded to a randomly selected subset of students for whom applications had been completed. The voucher amounts generally covered a substantial fraction of the cost of private school, and benefits lasted for multiple years. In most cases, students applied for vouchers before they knew whether they had been accepted to private schools, and consequently, some winners were not able to take advantage of the vouchers.

The first of the three policy-based natural experiments, the Milwaukee Parental Choice Program, was enacted by the Wisconsin state government. The program specified that private schools accepting voucher students could not charge tuition above the voucher amounts, and seven of the city’s twenty-two private nonsectarian schools chose to participate in the first year. A student applying for a voucher had to first specify the school to which he or she was applying; when demand for slots in a given private school outpaced supply, the vouchers were randomly assigned across applicants for those slots. In the second of the policy-based natural experiments, a private foundation offered vouchers for any student from grades K-12 in San Antonio’s Edgewood Independent School District who wished to attend private school. Funding was guaranteed for ten years. The third policy-based voucher initiative, through the Florida State Department of Education’s A-Plus Program, provided vouchers to students at underperforming schools (measured as two grades of “F” on statewide exams in the previous four years). Two schools in Pensacola met this criterion after the first two years of the policy, and any student in one of those schools qualified for private school vouchers for every year through the fifth grade.

The voucher programs varied in scale from 850 eligible in the Florida program to 188,312 eligible in the New York voucher experiment. In the Dayton, Washington, D.C., and San Antonio programs, students who had previously attended private schools were eligible for vouchers; in the other three programs, they were not. In all six cases, the statistics in columns (5) to (9) are presented for the subset of students who were previously attending public schools.[[10]](#footnote-10) The voucher packages tend to have been more generous for the policy-based natural experiments than for the randomized trials. The average voucher amount is not available for the Pensacola data, and the maximum is listed here; the maximum in Pensacola was similar to the maximum voucher amount per year in the San Antonio program ($5,351 for high school students and $4,816 for grades K-8).

The application rates range across the six programs from 0.010 for New York to 0.061 in Pensacola. Despite the generous voucher package in Milwaukee, we observe a low application rate for that program, possibly due to the small number of private schools that accepted the voucher. For the other two policy-based voucher programs, we observe higher application rates than for the randomized trials, possibly because of the greater generosity of the voucher packages, and possibly because the programs did not involve lotteries, and vouchers were provided to all applicants. Across the three experiments, the fraction of applicants who did not win vouchers who chose to attend private school ranges from 0.05 to 0.18. The fraction of non-winners who attended private school was not measured for the Milwaukee case and is not applicable for the other two policy-based cases in which all applicants received vouchers.

C. Response per $1,000 Voucher Year

Next, we use Equations (A2) and (A3) and the take-up rates from Table A1 to estimate the slope of the demand curve for private school, as measured in the fraction applying per $1,000 voucher year. To compute these rates, it is first necessary to measure the dollar value of each of the voucher packages, which provide benefits over multiple years. We consider two scenarios: one with zero discounting, and one with an annual discount rate of 0.50. The relatively high discount rate in our second scenario falls within the 0.35 to 0.54 range of estimated discount rates for enlisted military personnel from Warner and Pleeter (2001); while poor parents’ discount rates may be lower than those of enlisted men, we consider the higher range to take into account the high likelihood of losing benefits due to moving or losing eligibility status. For the policy-based natural experiments, which offered benefits up through a specific grade level, the number of program years is estimated for a student entering in the middle year (grade six for the Milwaukee and San Antonio Programs and grade three for the Florida program).

With no discounting, the values of the voucher packages are estimated to be $5,619 for New York, $6,280 for Dayton, $4,710 for Washington, D.C., $29,197 for Milwaukee, $18,725 for San Antonio, and $15,705 for Pensacola, where, due to data limitations, the Pensacola number is based upon the maximum and not the average voucher amount. Assuming 50% discounting, the values of the packages are estimated to be $3,954 for New York, $3,780 for Dayton, $3,314 for Washington, D.C., $11,781 for Milwaukee, $7,810 for San Antonio, and $11,052 for Pensacola.

Table A2 presents our estimated response per $1,000 voucher year for the six different programs. This response is estimated as , with , , and taken from Table A1. In columns (1) to (4), is computed as the fraction of non-winners switching to private school from column (9) of Table A1 multiplied by the fraction of eligible who applied from column (6) of Table A1. In columns (5) to (8) of Table A2, the fraction of non-winners switching to private school is assumed to be zero. In columns (1), (2), (5), and (6), the application cost is assumed to equal zero; in columns (3), (4), (7), and (8), we assume a moderate-sized application cost of $200. In the odd-numbered columns, we assume no discounting of future voucher benefits, and in the even-numbered columns, we assume an annual discount rate of 0.50.

Across the thirty-six estimates presented in Table A2, we obtain an average response per $1,000 voucher of 0.0050 and a median response of 0.0039. Across specifications, the assumption of 50% discounting is the most quantitatively important variation, and the estimates that assume discounting in the even columns are 72% larger on average than the estimates that assume a zero discount rate. Assumptions regarding have a moderate-sized effect on the estimated response; the estimates that use data on this fraction in columns (1) to (4) are on average 19% smaller than their counterparts in columns (5) to (8) that assume that this fraction equals zero. The application cost has relatively little effect, and the estimates that assume a $200 application cost are only 6.5% larger than the estimates that ignore this cost.

D. Comparison to Areas in Tennessee

To construct a range of potential voucher responses to apply to the Tennessee STAR students, we consider how the areas in which the STAR schools were located compare to the voucher program cities. As discussed in the text, private schools in Tennessee are concentrated in inner city areas. Recent data show that, of the 580 private schools in Tennessee, 115 are located in Shelby County where Memphis is located, 72 in Davidson County where Nashville is located, 49 in Knox County where Knoxville is located, and 43 in Hamilton County where Chattanooga is located. The remaining 301 schools are spread across the other 77 counties, with fifteen counties including only one private school each in those grades and one county including none (Private School Review, 2011).

The fractions attending private school in the inner cities in Tennessee are similar to the fractions attending private school among those meeting the eligibility requirements in the voucher cities in non-voucher years. Estimates from the 2000 Census Public Use Microdata Samples (PUMS) indicate that among students in kindergarten in Tennessee, the fractions attending private school were 12.5% in Memphis, 7.9% in Knoxville, 15.4% in Chattanooga, and 6.1% among those not located in central cities.[[11]](#footnote-11) Among students meeting the income and grade requirements for the voucher programs, the percentage enrolled in private school in the 2000 Census was 10.8% for New York, 7.1% for Dayton, 5.7% for Washington, D.C., and in the 1990 Census was 7.9% for Milwaukee. The rates of private school attendance are slightly higher for Tennessee overall, in part because families in the voucher samples are poor relative to families in Tennessee cities. However, inner city students in Project STAR were slightly poorer on average than students from the voucher samples were, and the two groups were otherwise generally similar, as shown in the online appendix.

When averaged across specifications, we obtain responses per $1,000 voucher of 0.0022 for New York, 0.0046 for Dayton, 0.0110 for Washington, D.C., 0.0007 for Milwaukee, 0.0037 for San Antonio, and 0.0048 for Pensacola (though the estimate for Pensacola would be larger if the average voucher amount were used rather than the maximum). The response per $1,000 for the Milwaukee program is relatively small, possibly due to the lack of private school options through that program, and the response to the Washington, D.C. program is especially large, possibly due to dissatisfaction with public schools or the large number of quality private schools in that area. Excluding these extreme cases of the restricted Milwaukee experiment and the unusual set of schools in Washington, D.C., we obtain a range of voucher responses of roughly 0.002 to 0.006. It is this range of values that we use for the inner city STAR schools in our economic value estimates.

While the suburban families had higher incomes than the inner city families did (making private school a more feasible option), they probably faced longer commutes to private school and had higher quality public school options, making private school less attractive. Our main estimates use the same range of 0.002 to 0.006 for students in suburban STAR schools as for those in inner city schools; however, given the longer commutes and the better public schools in the suburbs (both presumably lowering the elasticity of demand for private school), the lower end of this range may be more accurate. For STAR schools in smaller urban areas and in rural areas, we suppose that demand for private school is less elastic due to the smaller number of options available. Consequently, for these areas, we use the 0.0004 to 0.001 range of estimates from the Milwaukee program in which private school options were restricted to seven of the twenty-two local non-sectarian private schools.

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Table 1: Sample Means for Project STAR Students and Classes by Class Type, Kindergarten Entry Cohort | | | | | |
|  | (1) | (2) | (3) | (4) | (5) |
|  | Small | Regular | Regular/Aide | Joint P-Value | Within-school Joint P-value |
| Free lunch | 0.474 | 0.483 | 0.508 | 0.131 | 0.425 |
| White/Asian Student | 0.682 | 0.678 | 0.655 | 0.375 | 0.398 |
| Age on October 1, 1985 | 5.058 | 5.050 | 5.054 | 0.687 | 0.619 |
| Class size in kindergarten | 15.08 | 22.32 | 22.67 | 0.000\*\* | 0.000\*\* |
| Urban school | 0.295 | 0.292 | 0.308 | 0.721 |  |
| White/Asian teacher | 0.862 | 0.817 | 0.842 | 0.663 | 0.564 |
| Teacher has postgraduate degree | 0.305 | 0.384 | 0.374 | 0.396 | 0.338 |
| Teacher experience | 8.993 | 9.100 | 9.893 | 0.494 | 0.365 |
| Special education in kindergarten | 0.036 | 0.032 | 0.028 | 0.480 | 0.619 |
| Special instruction in kindergarten | 0.057 | 0.042 | 0.047 | 0.505 | 0.601 |
| Days present in kindergarten | 156.0 | 156.8 | 155.6 | 0.604 | 0.331 |
| Days absent in kindergarten | 10.00 | 10.52 | 10.95 | 0.059\* | 0.034\*\* |
| Missing data | 0.010 | 0.025 | 0.025 | 0.000\*\* | 0.214 |
|  |  |  |  |  |  |
| Observations (Nonmissing) | 1,788 | 2,028 | 2,058 | 5,874 | 5,874 |
| Observations (Total) | 1,806 | 2,079 | 2,111 | 5,996 | 5,996 |
| Classrooms | 121 | 93 | 93 | 307 | 307 |
| Schools | 75 | 74 | 75 | 75 | 75 |

Notes to Table 1: Data source is Project STAR dataset. Tables are structured after Tables 1 and 2 of Krueger (1999) but with some different variables and sample definitions. Small, regular, and regular/aide indicate the student’s class type in the first year in Project STAR. “Special instruction” indicates that student was pulled out for special instruction in that grade. Urban school and teacher characteristics are measured for the first year the student was in Project STAR. Data on special education, special instruction, days present, and days absent are not available for second grade. Sample includes those observations with non-missing values for the variables shown; sample for the “missing data” dummy includes all observations. Schools that left the program by third grade are excluded from the sample. Most of the missing observations come from missing data on special education or instruction or days present or absent. Column (4) shows the p-value for an F-test of the null hypothesis that the means are equal across the three groups; \*\* indicates that the null hypothesis is rejected at the 5% significance level, and \* indicates that the null hypothesis is rejected at the 10% level. Column (5) shows the p-value for the same F-test after controlling for fixed effects for students’ schools in their first year in the program. For individual student characteristics (free lunch, white/Asian student, special education, special instruction, days present, days absent, and missing data), robust standard errors are used. For urban school, the standard errors adjust for clustering at the school level, and for the classroom-level variables (class size, white/Asian teacher, teacher has postgraduate degree, and teacher experience), the standard errors adjust for clustering by school (the id of the school for the first year in the program).

|  |  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Table 2: OLS Estimates of Effects of Class Type on Attrition, Kindergarten Entry Cohort (Clusters = 75) | | | | | | | | | | |
|  | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Initial Class Type | Changed School in First Year | | Changed Class Type within School in First Year | | Changed School by Third Grade | | Changed Class Type within School by Third Grade | | Recommended to Repeat by Third Grade | |
| Small | -0.033 | -0.048 | -0.316 | -0.315 | -0.008 | -0.024 | -0.269 | -0.263 | -0.017 | -0.012 |
|  | (0.016)\*\* | (0.014)\*\* | (0.022)\*\* | (0.021)\*\* | (0.016) | (0.014)\* | (0.019)\*\* | (0.019)\*\* | (0.010) | (0.010) |
|  |  |  |  |  |  |  |  |  |  |  |
| Regular | -0.006 | -0.017 | 0.029 | 0.030 | 0.020 | 0.007 | -0.071 | -0.063 | 0.000 | 0.001 |
| w/ Aide | (0.015) | (0.015) | (0.025) | (0.025) | (0.017) | (0.014) | (0.015)\*\* | (0.015)\*\* | (0.009) | (0.010) |
|  |  |  |  |  |  |  |  |  |  |  |
| R2 | 0.001 | 0.097 | 0.116 | 0.141 | 0.001 | 0.119 | 0.074 | 0.114 | 0.001 | 0.052 |
|  |  |  |  |  |  |  |  |  |  |  |
| Controls? |  | Yes |  | Yes |  | Yes |  | Yes |  | Yes |
| School Fixed Effects? | | Yes |  | Yes |  | Yes |  | Yes |  | Yes |

Notes to Table 2: Each column shows results from a different linear regression in which the dependent variables are as listed in the column headings and the regressors of interest are indicators for assigned class type in one’s year in Project STAR. Standard errors adjust for clustering by kindergarten school id. Sample excludes students with missing values for the variables shown in Table 1 and those whose schools left Project STAR by third grade. “Changed school” indicates whether the student was recommended for promotion but does not appear in the sample in the same school in the next year. “Recommended to repeat” indicates that the teacher did not recommend promotion to the next grade for some year while in Project STAR. Grade repetition was rare and not measured for kindergarten students. “Controls” include free lunch recipient, white/Asian student, age on October 1, 1985, white/Asian teacher, teacher has postgraduate degree, and teacher experience. Additional details in the text.

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Table 3: OLS Estimates of Equations (1) and (4) Excluding Aide as a Regressor | | | | | | | | |
|  | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Initial Class Type | Panel A: Dependent Variable is Changed School After First Year | | | | | | | |
| Inner City | | Suburban | | Urban, not Inner City | | Rural | |
| Small | -0.089 | -0.088 | -0.047 | -0.057 | 0.036 | 0.026 | -0.009 | -0.020 |
|  | (0.040)\*\* | (0.029)\*\* | (0.040) | (0.033)\* | (0.062) | (0.062) | (0.015) | (0.014) |
|  |  |  |  |  |  |  |  |  |
| R2 | 0.007 | 0.038 | 0.002 | 0.069 | 0.001 | 0.113 | 0.000 | 0.067 |
| N (Students) | 1,318 | | 1,306 | | 436 | | 2,814 | |
| Clusters | 67 | | 68 | | 6 | | 147 | |
|  |  |  |  |  |  |  |  |  |
| Panel B: Dependent Variable is Left Public School System After First Year | | | | | | | | |
| Small | -0.031 | -0.020 | -0.011 | -0.013 | -0.043 | -0.061 | -0.007 | -0.011 |
|  | (0.032) | (0.029) | (0.040) | (0.018) | (0.025)\* | (0.026)\*\* | (0.009) | (0.010) |
|  |  |  |  |  |  |  |  |  |
| R2 | 0.002 | 0.019 | 0.000 | 0.047 | 0.005 | 0.031 | 0.000 | 0.038 |
| N (Students) | 1,318 | | 1,306 | | 436 | | 2,814 | |
| Clusters | 67 | | 68 | | 6 | | 147 | |
|  |  |  |  |  |  |  |  |  |
| Panel C: Dependent Variable is Remaining Years in Small Class, Sample Includes Those Remaining in Project STAR Through Third Grade | | | | | | | | |
| Small | 2.269 | 2.236 | 2.293 | 2.317 | 2.369 | 2.352 | 2.505 | 2.551 |
|  | (0.133)\*\* | (0.134)\*\* | (0.136)\*\* | (0.118)\*\* | (0.210)\*\* | (0.210)\*\* | (0.082)\*\* | (0.068)\*\* |
|  |  |  |  |  |  |  |  |  |
| R2 | 0.619 | 0.648 | 0.593 | 0.637 | 0.628 | 0.669 | 0.702 | 0.740 |
|  |  |  |  |  |  |  |  |  |
| Controls? |  | Yes |  | Yes |  | Yes |  | Yes |
| School Fixed Effects? | | Yes |  | Yes |  | Yes |  | Yes |
| N (Students) | 441 | | 586 | | 205 | | 1,745 | |
| Clusters | 15 | | 68 | | 6 | | 147 | |

Notes to Table 3: This table shows estimates of Equation (1) in panel A and Equation (4) in panel B, both with omitted as a regressor. In panel A, these regressions are the same as in columns (1) and (2) of Table 2 but with “regular with aide” omitted as a regressor and estimates presented separately by level of urbanization. The sample in panel B is restricted to students who remained in Project STAR from kindergarten through third grade, as in Table B1 of the web appendix. Within each column, the pair of equations in panels A and B are estimated simultaneously using seemingly unrelated regression (suest in Stata). Standard errors are clustered by kindergarten teacher id.

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Table 4: Estimated Value of Small Class versus Regular-Aide Lottery, Kindergarten Entry Cohort by Urbanicity of School | | | | | | | | |
|  | (1) | (2) | (5) | (6) | (3) | (4) | (7) | (8) |
| Panel A: Attrition Measured as Changing Schools After First Year | | | | | | | | |
|  | Inner City | | Suburban | | Urban, Not Inner City | | Rural | |
| Normalized attrition effect | -0.039 | -0.039 | -0.020 | -0.025 | 0.015 | 0.011 | -0.004 | -0.008 |
| (0.017)\*\* | (0.013)\*\* | (0.017) | (0.014)\* | (0.027) | (0.027) | (0.006) | (0.005) |
| Value per year assuming. . . |  |  |  |  |  |  |  |  |
| 0.006 response per $1,000 voucher | $6,567 | $6,528 | $3,381 | $4,131 |  |  |  |  |
| 0.002 response per $1,000 voucher | $19,702 | $19,584 | $10,142 | $12,393 |  |  |  |  |
| 0.001 response per $1,000 voucher |  |  |  |  | -$15,092 | -$11,180 | $3,639 | $7,868 |
| 0.0004 response per $1,000 voucher |  |  |  |  | -$37,730 | -$27,949 | $9,098 | $19,670 |
|  |  |  |  |  |  |  |  |  |
| Panel B: Attrition Measured as Leaving Public School System After First Year | | | | | | | | |
| Normalized attrition effect | -0.014 | -0.009 | -0.005 | -0.006 | -0.018 | -0.026 | -0.003 | -0.004 |
| (0.014) | (0.013) | (0.009) | (0.008) | (0.010)\* | (0.011)\*\* | (0.004) | (0.004) |
| Value per year assuming. . . |  |  |  |  |  |  |  |  |
| 0.006 response per $1,000 voucher | $2,275 | $1,526 | $774 | $966 |  |  |  |  |
| 0.002 response per $1,000 voucher | $6,825 | $4,578 | $2,321 | $2,898 |  |  |  |  |
| 0.001 response per $1,000 voucher |  |  |  |  | $18,300 | $25,792 | $2,601 | $4,152 |
| 0.0004 response per $1,000 voucher |  |  |  |  | $45,751 | $64,480 | $6,504 | $10,380 |
|  |  |  |  |  |  |  |  |  |
| Controls? |  | Yes |  | Yes |  | Yes |  | Yes |
| School Fixed Effects? |  | Yes |  | Yes |  | Yes |  | Yes |
| Observations | 1,318 | | 1,306 | | 436 | | 2,814 | |

Notes to Table 4: The first row measures the change in attrition associated with an expected future yearlong change to a small class relative to a lottery between a year of a regular and a year of a regular class with aide, as described in the text. The specifications in each column are the same as in Table 3. The attrition effect per yearlong change is measured as the coefficient on small class from Equation (1) in panel A of Table 3 divided by the coefficient on small class from Equation (4) in panel B of Table 3. The standard errors are calculated by simultaneously estimating the attrition regression and the years in small class regression using seemingly unrelated regression (suest in Stata) and using the delta method (nlcom in Stata) to compute the standard errors for the relevant ratios. All standard errors adjust for clustering at the level of initially assigned classroom. The sample in each case is restricted to students whose kindergarten schools fell into the location types (inner city, suburban, urban not inner city, and rural) listed in the column headings. The value per year in a given class type is computed by multiplying the attrition effect per year of change in class type by -$1,000 and dividing by the estimated effect of a $1,000/year voucher on the fraction attending private school. For inner city and suburban areas, this effect is assumed to range from 0.002 to 0.006, and for urban not inner city and rural areas, this effect is assumed to range from 0.0004 to 0.001, as described in Appendix A. Additional details in the text.

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Table A1: Descriptive Characteristics of Six Private School Voucher Programs | | | | | | | | |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Panel A: Randomized Field Experiments | | | | | | | | |
| Site | School Year | Eligible Population | Voucher Amount in 2010 Dollars and Duration of Benefits | Number Eligible | Fraction of Eligible who Applied | Fraction of Applicants Receiving Vouchers | Fraction of Winners Attending Private School | Fraction of Non-winners Attending Private School |
| New York City | 1997-1998 | Children grades 1 to 4 at 185% poverty or below who had not already attended private school. | Average voucher of $1,873. Vouchers guaranteed for 3 years with possibility of renewal. | 188,312 | 0.010 | 0.765 | 0.760 | 0.050 |
| Dayton, OH | 1998-1999 | Children in grades K to 12 at 200% poverty or below | Average voucher of $1,570. Vouchers guaranteed for 4 years with hopes to renew through high school. | 33,150 | 0.024 | 0.641 | 0.540 | 0.180 |
| Washington, DC | 1998-1999 | Children in grades K to 8 at 270% poverty or below. | Average voucher of $1,570. Vouchers guaranteed for 3 years with possibility of renewal. | 35,019 | 0.045 | 0.513 | 0.530 | 0.110 |
| Panel B: Policy-Based Natural Experiments | | | | | | | | |
| Milwaukee, WI | 1990-1991 | Children in grades K to 12 at 175% poverty or below who had not attended private school in the previous year. | $4,171 for every eligible year through twelfth grade | 50,583 | 0.011 | 0.704 | 0.840 |  |
| San Antonio, TX | 1998-1999 | Children grades K-12 in Edgewood Independent School District | Average of $2,675 per year | 13,889 | 0.041 | 1.000 | 1.000 | -- |
| Pensacola, FL | 1999-2000 | Students at A.A. Dixon and Spencer Bibbs Elementary schools (all grades K-5, all below 270% poverty). | Up to $5,235 per year through 5th grade | 850 | 0.061 | 1.000 | 1.000 | -- |

Notes to Table A1: All dollar amounts are converted to 2010 dollars. For Milwaukee, number eligible is computed based upon number meeting eligibility requirements (living in Milwaukee at 175% poverty or below, in public school, not yet graduated high school, aged 5 to 18) in the 5% Public Use Microdata Samples (PUMS) of the 1990 U.S. Census. For New York, Dayton, Washington, D.C., the corresponding numbers are computed from the PUMS of the 2000 U.S. Census and re-scaled for population growth by multiplying by (city population in program year)/(city population in 2000), also taken from the U.S. Census (Ruggles, *et al.*, 2010; U.S. Census, 2000). Due to differing sets of variables in the two Censuses, the number eligible is calculated in the 2000 Census based upon “grade currently attending” rather than age and educational attainment. For San Antonio, the number eligible is computed as the number of students enrolled in public schools in Edgewood School District in 1997-1998 plus the number who were previously in public schools and received vouchers (Texas Education Agency, 1999). For Pensacola, the number affected is measured as the number of students enrolled in the schools affected by the policy. Students receiving vouchers in Milwaukee remained eligible as long as their incomes did not fall above 220% poverty. Sources on voucher programs includeChakrabarti (2005, 2008), Greene (2001), Greene and Hall (2001), Greene, Peterson, and Du (1999), Howell and Peterson (2000, 2006), Howell, *et al*. (2002), Mayer, *et al.* (2002), Merrifield (2010), Peterson, Myers, and Howell (1999), Ritsche (2006), Rouse (1998), West, Peterson, and Campbell (2001), Witte (1991, 1998), Wolf (2008), Wolf, Peterson, and West (2001), and Wolf, Howell, and Peterson (2000). Additional details in the text.

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Table A2: Estimated Change in Fraction Switching to Private per $1,000 Voucher | | | | | | | | |
|  | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|  | Fraction Switching in the Absence of Vouchers . . . | | | | | | | |
|  | Estimated from the Data | | | | Assumed to be Negligible | | | |
|  | Application cost = $0 | | Application cost = $200 | | Application cost = $0 | | Application cost = $200 | |
| Program Site | Discount rate = 0.00 | Discount rate = 0.50 | Discount rate = 0.00 | Discount rate = 0.50 | Discount rate = 0.00 | Discount rate = 0.50 | Discount rate = 0.00 | Discount rate = 0.50 |
|  |  |  |  |  |  |  |  |  |
| New York City | 0.0017 | 0.0025 | 0.0018 | 0.0026 | 0.0019 | 0.0026 | 0.0019 | 0.0028 |
|  |  |  |  |  |  |  |  |  |
| Dayton, OH | 0.0028 | 0.0046 | 0.0029 | 0.0050 | 0.0039 | 0.0064 | 0.0041 | 0.0070 |
|  |  |  |  |  |  |  |  |  |
| Washington, DC | 0.0075 | 0.0107 | 0.0082 | 0.0121 | 0.0096 | 0.0136 | 0.0105 | 0.0154 |
|  |  |  |  |  |  |  |  |  |
| Milwaukee, WI |  |  |  |  | 0.0004 | 0.0010 | 0.0004 | 0.0010 |
|  |  |  |  |  |  |  |  |  |
| San Antonio, TX |  |  |  |  | 0.0022 | 0.0052 | 0.0022 | 0.0054 |
|  |  |  |  |  |  |  |  |  |
| Pensacola, FL |  |  |  |  | 0.0039 | 0.0055 | 0.0039 | 0.0056 |
|  |  |  |  |  |  |  |  |  |

Notes to Table A2: This table presents estimates of the effect of a $1,000 voucher on the fraction attending private school based upon the descriptive statistics from Table A1 and using the formulas from Equations (A1) and (A3). Additional details in the text Appendix A.

Figure 1: Demand for Private School for Parents in the Tennessee STAR Experiment

Demand for private school if child is in small class

Economic value of class size reduction for parent on the margin of sending child to private school

Tuition

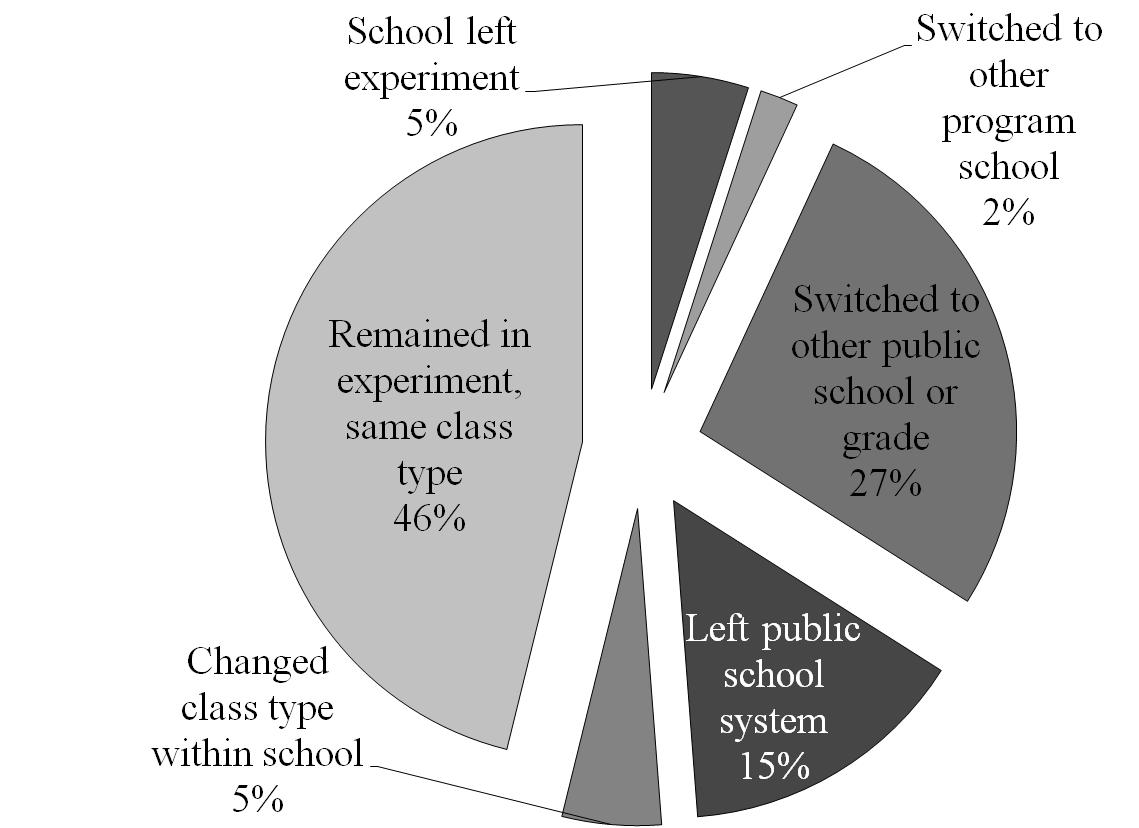
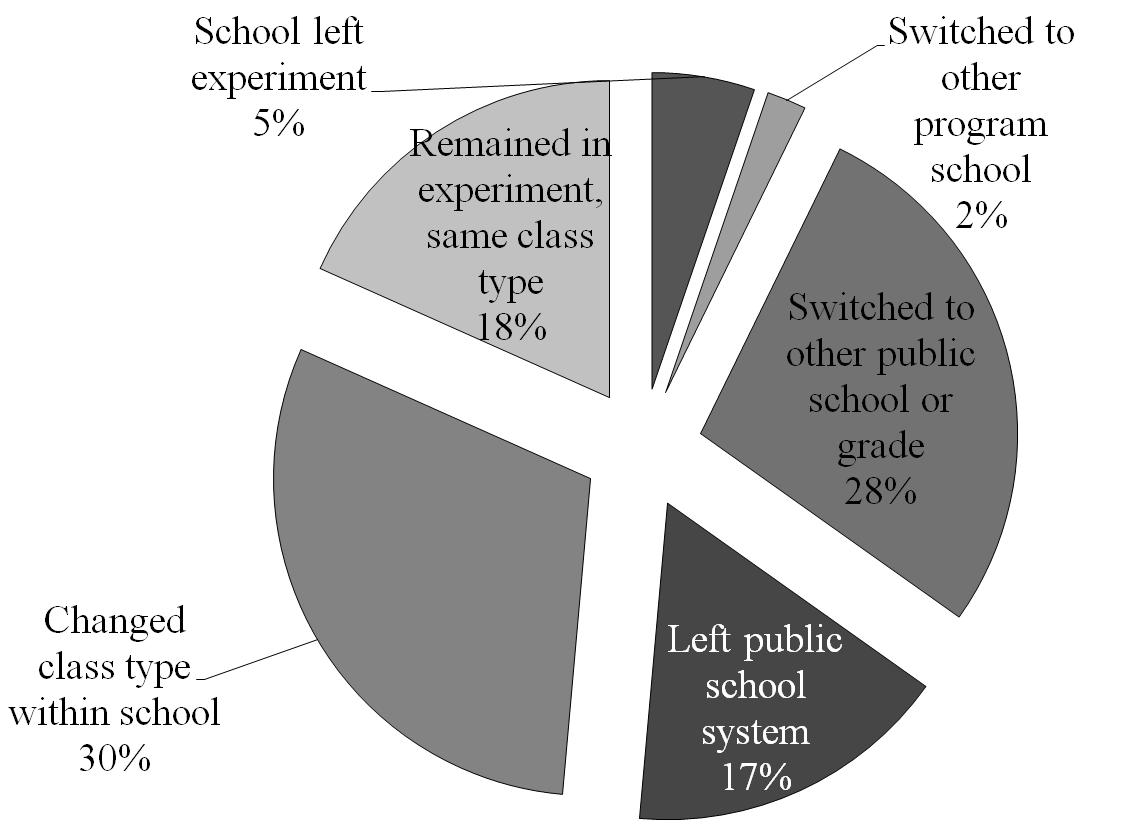
Private school attendance

Observed difference in quantity demanded

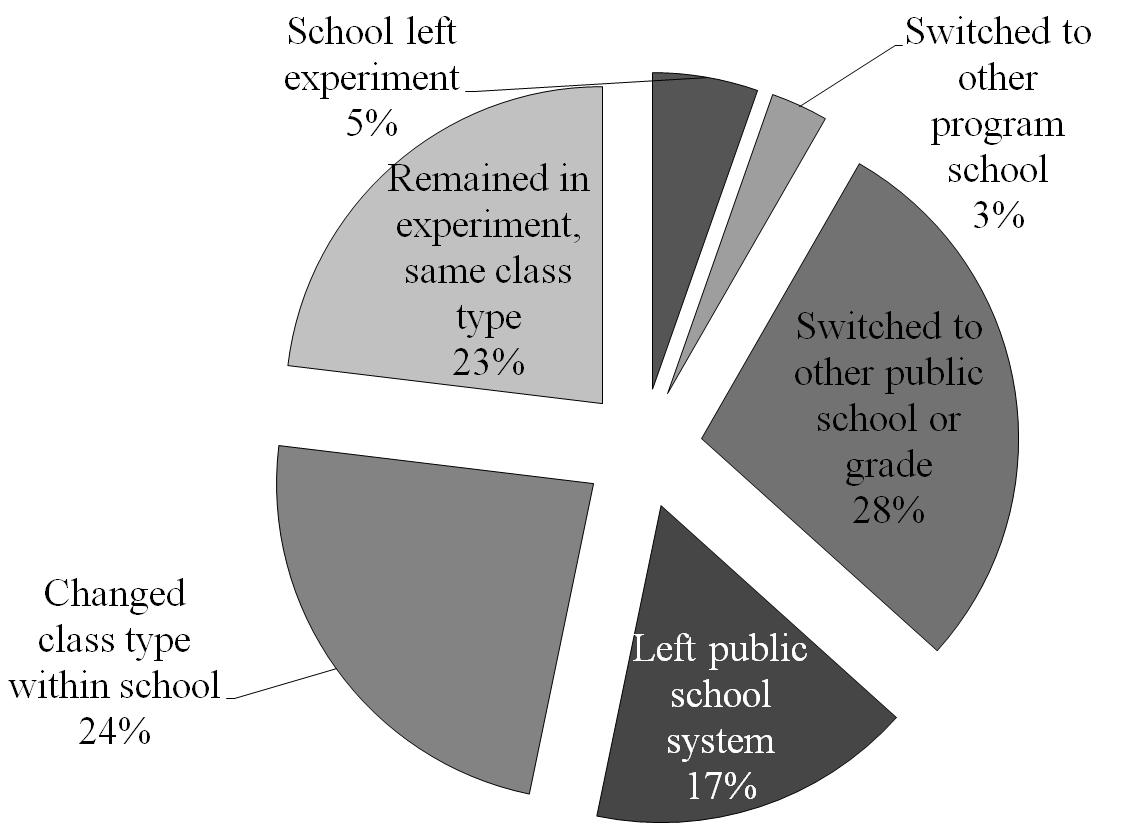
Demand for private school if child is in regular-sized class

Figure 2: Status in Third Grade by Initial Class Assignment in Project STAR, Kindergarten Entry Cohort

Panel A: Initially in Small Class Panel B: Initially in Regular Class

Panel C: Initially in Regular Class w/ Aide



Notes to Figure 2: Students are counted as having left the public school system if they do not appear in Project STAR in third grade and TCAP scores are not available for them in 1990 (the year in which most students from the cohort were in fourth grade). Additional details in the text.

1. Center for Policy Research.426 Eggers Hall.Syracuse University. Syracuse, NY 13244. [carohlfs@maxwell.syr.edu](mailto:carohlfs@maxwell.syr.edu) and [mazilora@syr.edu](mailto:mazilora@syr.edu). Thanks to Charles Achilles, Bob Bifulco, Sandra Black, David Brasington, Christian Bürger, Jay Greene, Jeff Kubik, Len Lopoo, Diane Schanzenbach, Kevin Tsui, Johnny Yinger, two anonymous referees, and seminar participants at Syracuse University for many helpful comments. [↑](#footnote-ref-1)
2. As Krueger (1999) notes, the Project STAR data include class type in the first year, which does not correspond exactly to the initial randomly assigned class type. However, an examination by Krueger of the original class type assignments found that changes in class type between the random assignment and the data collection were rare. [↑](#footnote-ref-2)
3. One of the initial schools (school id 216536) was kindergarten only but fed into an elementary school (school id 216537). For the purposes of the current study, these two schools are treated as a single school. [↑](#footnote-ref-3)
4. Days absent does not exactly equal 180 minus days present due to variation in the length of the school year. [↑](#footnote-ref-4)
5. The 1990 and 1991 TCAP data are both used because not all public school students took the 1990 test. This measure may overstate the number of students still in the public school system, as some children may have switched to private school and returned after third grade, when the experiment was finished. This variable definition does not affect our final estimates, which measure attrition as changing schools, regardless of whether the student remained in the public school system. Thanks to an anonymous referee for alerting us to this discrepancy in the TCAP data. [↑](#footnote-ref-5)
6. Some of this difference may be attributable to changes of residence across states. Given, however, the small effect of treatment on moves within state, it is unlikely that cross-state moves accounted for a substantial fraction of the difference. [↑](#footnote-ref-6)
7. For the voucher experiments, this latter fraction is estimated based upon the fraction who applied, did not receive vouchers, and switched to private school anyway. For all six, alternative specifications are estimated in which this latter fraction is assumed to be negligible. [↑](#footnote-ref-7)
8. Additionally, data from the 1990 and 2000 Census indicate that most of the students attending private school come from urban and suburban areas and that these fractions have been fairly stable over time. [↑](#footnote-ref-8)
9. Because the set of regressors is the same in the two equations, the OLS and SUR estimates are identical. The OLS standard errors are nearly identical to the SUR standard errors shown here. [↑](#footnote-ref-9)
10. Data are not presented for two additional randomized trials, one nationwide and one in Charlotte, North Carolina, because the studies do not track whether applicants were previously attending public school. When computed for all students, regardless of previous private school attendance, the fraction of eligible parents applying was 0.084 for Dayton, 0.202 for Washington, D.C., 0.075 for Charlotte, and 0.065 for the nationwide program (Campbell, West, and Peterson, 2005; Cowen, 2008; Greene, 2000; Howell and Peterson, 2006). Hence, the application rate was high for D.C. but was similar across the other three programs. [↑](#footnote-ref-10)
11. The 2000 Census is used because it includes a measure of “grade currently attending.” In the 1990 Census, among children 5 and older who lived in Tennessee, were in school, and had not yet completed kindergarten, we find very similar fractions attending private school (12.1% in Memphis, 8.4% in Knoxville, 16.0% in Chattanooga, and 6.0% among those not in central cities). [↑](#footnote-ref-11)