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Contributions

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Estimating Parents' Valuations of Class Size Reductions Using Attrition in the Tennessee STAR Experiment

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 Student Teacher Achievement Ratio 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–\$18,000 per year relative to a cost of \$3,000 per student year.

Keywords: educational vouchers, charter schools, class size, attrition, Tennessee STAR
JEL Classifications: I22, I21, H75, D61, C35

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1 Introduction

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. 2011; 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 studies of parental valuations of school quality have focused exclusively on housing prices, comparing housing prices across school attendance areas with varying school characteristics (Bogart and Cromwell 2000; Brasington 1999; Brasington and Haurin 1996; 2006; Caetano 2010; Downes and Zabel 2002; Hayes and Taylor 1996; Leech and Campos 2003), around school district boundaries (Bayer, Ferreira, and McMillan 2007; Black 1999), and before and after desegregation and district-wide or nationwide school assessments (Bogin 2011; Figlio and Lucas 2004; Gill 1983; Kane, Staiger, and Stamms 2003). While these studies aim to answer questions about education policies, their approaches require many restrictive assumptions about the housing market, including known functions for supply and demand, frictionless pricing, and the exogeneity of location-specific attributes such as neighborhood quality. Housing prices are notoriously difficult to predict and depend on many unobservable factors. Student–teacher ratios are known to be correlated with key determinants of home value that are difficult to measure, such as neighborhood attributes, crime, and other determinants of school quality such as peer and teacher characteristics (Brasington and Haurin 2009; Hill 2003; Weimer 2003). Additionally, any "reverse causal" effects of school test scores on demographics, crime, or other neighborhood characteristics could bias estimates based upon the housing market, even if an initial shock to school quality is exogenous. One key advantage of the current study is that the identification strategy uses a limited set of straightforward assumptions regarding school choice and does not rely upon implausible assumptions about an entirely different market.

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. 2011; 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-

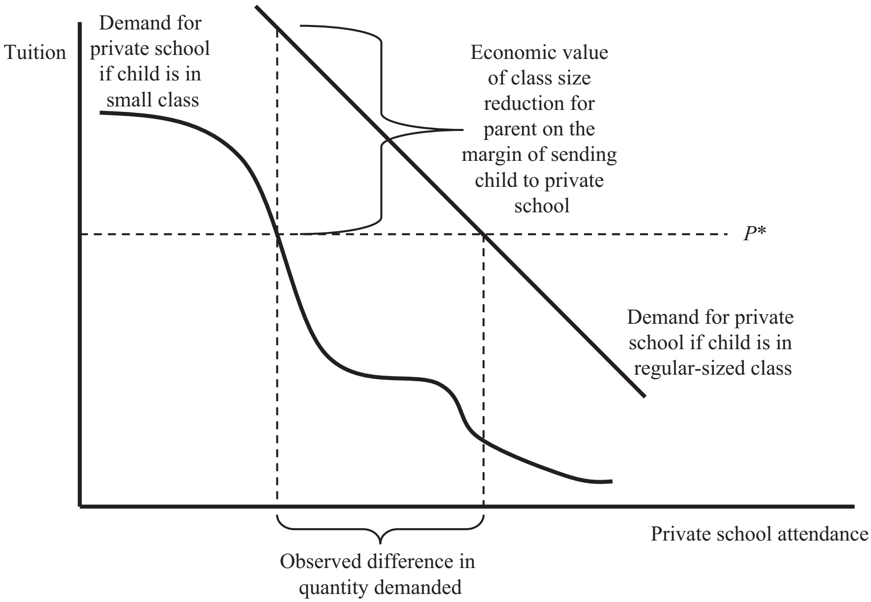

Figure 1: Demand for private school for parents in the Tennessee STAR experiment

and regular-sized classes. The top curve is assumed to be locally linear, but the lower curve may have arbitrary functional form. 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 induce 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 to 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 Kniesner, Rohlfs, and Sullivan (2013) and applied in Rohlfs (2012) to measure the cost of the Vietnam draft.

One difficulty with our approach is that, while vouchers could only be obtained by attending private school, a family could avoid a Project STAR class type by switching to private school or by changing residences to switch public schools. In order to construct the comparison illustrated in Figure 1, it is necessary to know how many of the Project STAR families that changed residences would have switched to private school if private school was the only alternative to staying. The fraction who changed schools at all in response to STAR class type overstates the fraction who would switch to private if private were the only alternative; the fraction who switch to private school understates this fraction.[1] We construct estimates of the parental valuation of class size using both types of attrition as dependent variables; our preferred estimates are averages of these two measures.

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 other class types (Hanushek 1999; Krueger 1999; Nye, Hedges, and Konstantopoulos 2000; Schanzenbach 2006), with much of this attrition due to students leaving the public school system. While imprecise, our preferred estimates indicate that, relative to a lottery between a regular class and a regular class with an aide, parents on the margin of switching to private school valued a year in small class at $2,000–$18,000 in 2010 dollars.

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 other product attributes.

2 Background and descriptive results on Project STAR and voucher programs

2.1 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

1 Some of those leaving the Tennessee Public School System switched to public schools in other states; however, the fraction that were induced to change states as a result of the experiment is probably very small.

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).[2] The experiment and the associated data are described extensively in Finn and Achilles (1990; 1999), Grissmer and Flanagan (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.[3] 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), respectively. 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. (2011), 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 in 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 began in these schools are excluded from both the "non-missing" and "total" samples presented here.[4]

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2 Another difficulty with the later cohorts is that many of those families that were initially assigned to regular-sized classes were able to lobby to change class types.

3 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.

4 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.

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: 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 nonmissing 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, instruction, days present, or days 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. All of the F-tests adjust for clustering by kindergarten school id.

The results from Table 1 indicate that the randomization was generally 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 observe 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).[5]

2.2 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–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 appear in the Tennessee Comprehensive Assessment Program (TCAP) Achievement Test data in 1990 or 1991, the first 2 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).[6]

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%, respectively. 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, respectively, as compared with only 5% of those

5 Days absent does not exactly equal 180 minus days present due to variation in the length of the school year.
6 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. Thanks to an anonymous referee for alerting us to this discrepancy in the TCAP data.

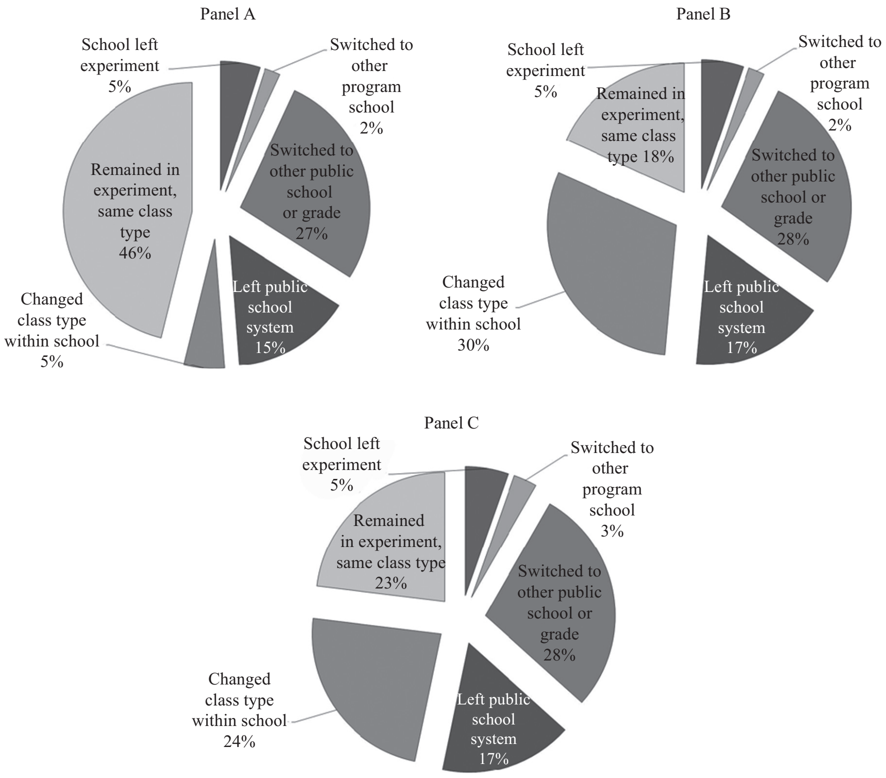

Figure 2: Status in third grade by initial class assignment in Project STAR, kindergarten entry cohort. Panel A: initially in small class, Panel B: initially in regular class, and Panel C: initially in regular class with aide

Notes: 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.

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). This differential attrition from the experiment is the central outcome of interest in this paper, as the switches reflect parental expectation of remaining years of a class type, which then determine the attrition response (e.g. if small classes are desirable, a parent with 2.5 years remaining in small classes is less likely to have their student leave the experiment than a parent with only half a year remaining).

2.3 Design and descriptive statistics for six private school voucher programs

Our estimates of the slope of the demand curve for private school come from application rates for three private school voucher experiments (in New York City, Dayton, OH, and Washington, DC) and three policy-based natural experiments (in Milwaukee, WI, San Antonio, TX, and Pensacola, FL). In the experiments, it is non-experimental variation that is used to identify the slope of the demand curve for private school. In each case, poor families in urban areas were given the opportunity to apply for vouchers. Columns (1)–(4) of Table 2 describe the key features of these 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)–(9) describe statistics relevant to demand for vouchers, 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 switched to private school. It is these statistics that we use 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 non-sectarian 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

Table 2: 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 1–4 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–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–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–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 fifth grade	850	0.061	1.000	1.000	–

Notes: 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 5–18) in the 5% PUMS of the 1990 US Census. For New York, Dayton, and Washington, DC, the corresponding numbers are computed from the PUMS of the 2000 US Census and re-scaled for population growth by multiplying by (city population in program year)/(city population in 2000), also taken from the US Census (Ruggles et al. 2010; US 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 of students 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 include Chakrabarti (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 are given in the text.

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 10 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 state-wide exams in the previous 4 years). Two schools in Pensacola met this criterion after the first 2 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, DC, 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)–(9) are presented for the subset of students who were previously attending public schools.[7] In general, the voucher packages were more generous for the natural experiments than for the randomized trials. The average voucher amount is not available for the Pensacola data, and the maximum is listed; 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 would accept 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

7 In the Dayton experiment, roughly a third of vouchers went to students who were previously attending private school, and all of those students used the vouchers (West, Peterson, and Campbell 2001, 8–9). Data are not presented for two additional randomized trials, one nation-wide and one in Charlotte, NC, because the studies did 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, DC, 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 DC but was similar across the other three programs.

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.

2.4 Comparison of voucher areas to 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. 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 15 counties including only 1 private school each in those grades and 1 county including none (Private School Review 2011).[8]

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.[9] 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, DC, 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 appendix.

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.

9 The 2000 Census is used, because it includes a measure of "grade currently attending." In the 1990 Census, among children aged 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).

The private school markets in both Tennessee STAR and the voucher study cities are similar to those for the nation as a whole. Between 2000 and 2011, the fraction of US students attending private school (in all geographic areas) hovered between 8% and 10%, with urban areas closer to 15% (NCES 2010) The average tuition charged by US private schools is about $10,000 in current dollars, with elementary schools being slightly cheaper and secondary schools being slightly more expensive (NCES 2010). Thus, the frequency of private school attendance and the costs thereof are similar to those for the component studies.

3 Conceptual framework

3.1 Hedonic framework

The econometric strategy that we use to estimate the value of class type is taken from the "matching and audit" procedure in Kniesner, Rohlfs, and Sullivan (2013, 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 for this valuation is linear in the price of private school, as in the upper demand curve in Figure 1 of the current paper. 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 class is predicted to reduce the family's demand for private school by an amount that varies across families. The estimation procedure used in this study identifies the dollar-denominated vertical difference in the demand curve (termed "marginal surplus" in Kniesner, Rohlfs, and Sullivan 2013) for a family on the margin of sending their child to private school.

The model in Kniesner, Rohlfs, and Sullivan (2013) 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. 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 the two measures of attrition from Project STAR.

3.2 Estimation equations

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

$$Left\ School_{it} = \beta_{st}^{LSch} * Small_i + \beta_{at}^{LSch} * Aide_i + \beta_{\mathbf{xt}}^{\mathbf{LSch}} \mathbf{x_i} + u_{it}^{LSch}, \qquad [1]$$

$$Left\ System_{it} = \beta_{st}^{LSys} * Small_i + \beta_{at}^{LSys} * Aide_i + \beta_{\mathbf{xt}}^{\mathbf{LSys}} \mathbf{x_i} + u_{it}^{LSys}, \text{ and} \qquad [2]$$

$$Retained_{it} = \beta_{st}^{R} * Small_i + \beta_{at}^{R} * Aide_i + \beta_{\mathbf{xt}}^{\mathbf{R}} \mathbf{x_i} + u_{it}^{R}, \qquad [3]$$

where $Left\ School_{it}$, $Left\ System_{it}$, and $Retained_{it}$ are indicators for whether student i changed schools, left the public school system, or was recommended to repeat a grade before grade t. $Small_i$ and $Aide_i$ are dummies representing student i's initially assigned class type of small or regular with aide (so that regular class is the omitted category), and $\mathbf{x_i}$ is a vector of control variables. The effects of $Small_i$, $Aide_i$, and $\mathbf{x_i}$ are allowed to vary by year, and we estimate the effects of initial class type on attrition after 1 year and after 3 years; our parental valuation estimates are based entirely on the 1-year attrition figures.

3.3 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, the number of years spent in small classes after kindergarten can be described as:

$$Remaining\ Years\ in\ Small_i = \alpha_s^s * Small_i + \alpha_{\mathbf{x}}^{\mathbf{s}} \mathbf{x_i} + \varepsilon_i^s. \qquad [4]$$

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 after the first year, 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 initially in regular and regular with aide classes. Consequently, estimates of eqs [1], [2], and [4] that include $Aide_i$ as a regressor find that it has small and insignificant effects on changing schools, leaving the public school system, or remaining years in small or aide classes. To construct our estimates of parents' valuations of class size reductions, we suppose that the effects of $Aide_i$ are zero.

For a student who chooses to remain in Project STAR, being initially assigned to a small class has associated with it α_s^s more expected future years in a small class than does an initial assignment in a regular or regular with aide class. Hence, β_{st}^{LSch} from eq. [1] and β_{st}^{LSys} from eq. [2] measure the attrition effect of expecting α_s^s future years of small class as compared to the alternative of α_s^s 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 responses from eqs [1] and [2] by α_s^s to produce $\beta_{st}^{LSch}/\alpha_s^s$ and $\beta_{st}^{LSys}/\alpha_s^s$, the degrees to which one future year of small classes affects student attrition. Dividing $\beta_{st}^{LSch}/\alpha_s^s$ and $\beta_{st}^{LSys}/\alpha_s^s$ by the slope of the demand curve for private school then produces estimates of the value parents place on one future year of small classes.

3.4 Demand for private school

Due to the lengthy application processes that the voucher programs required, only parents with especially high valuations of private school applied for vouchers. Rather than use the experimental variation in voucher receipt among these selected samples, we derive an expression for the slope of the demand curve for a representative sample of parents based upon a discrete choice model of the application decision. Our estimator uses data on the fraction applying for the voucher program and estimates of the fraction who would have switched to private school in the absence of a voucher program.

Consider a population of poor families with children currently enrolled in public school.[10] To simplify the notation, suppose that each family has one child. Let $v_{1i} = v_{0i} + \varepsilon_i$ denote the dollar-denominated surplus that family i would obtain from switching its child to private school, where v_{0i} measures family-specific taste and cost factors that are known to the family at the time that it applies for the voucher program, and ε_i 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. Because ε_i represents new information, we suppose that v_{0i} and ε_i are independent. Let $f_{v_0}(.)$ and $f_\varepsilon(.)$ denote the population density functions for v_{0i} and ε_i among

10 The sample is restricted to poor families because the voucher studies we use were restricted in this way.

families eligible for the vouchers, and let $f_{v_1}(v) = \int_{-\infty}^{\infty} f_{v_0}(v-u) f_\varepsilon(u) du$ denote the density of v_{1i}. In the absence of any subsidy, the fraction of families switching to private school is $1 - F_{v_1}(0) = \int_0^{\infty} f_{v_1}(v) dv$.

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 ξ, the fraction switching to private school would be $1 - F_{v_1}(-\xi)$, and the per dollar effect $[F_{v_1}(-\xi) - F_{v_1}(0)]/\xi$ of a voucher on the probability of switching, averaged over the domain from 0 to ξ, could be measured as the treatment–control difference in the fraction switching to private school divided by the voucher amount. Unfortunately, this comparison is not possible using the data on available voucher experiments, because voucher/no voucher designations were randomly assigned among the selective subset of families who completed applications. Consequently, a comparison of private school attendance between the treated and untreated groups would estimate 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 ξ with probability π and supposing that the (time) cost of applying is c, the expected surplus from applying for the voucher program is $\pi^* \xi$ for those who would attend in the absence of the voucher and $\pi^* (\xi + v_{1i})$ for those who would not, where v_{1i} is negative for the latter group of families. Taking the expectation over ε_i, a family that maximizes expected surplus applies for the voucher program iff:

$$\pi^* \left(\xi + \int_{-\infty}^{\infty} \min\{v_{0i} + u, 0\} f_\varepsilon(u) du \right) \geq c. \qquad [5]$$

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 ε_i, equals or exceeds the application cost. Assuming that the expected voucher amount exceeds the application costs, so that $\xi \geq \frac{c}{\pi}$, the fraction of eligible families who apply can be written as:

$$Pr(Apply) = 1 - F_{v_1}\left(\frac{c}{\pi} - \xi\right), \qquad [6]$$

Hence, the fraction applying to the voucher program is the fraction of families who would switch to private school given a voucher of $\xi - \frac{c}{\pi}$ or greater.

In the context of this discrete choice model, any family that expects to switch to private school even without a voucher (i.e. $E[v_{1i} \mid v_{0i}] \geq 0$) would apply for the program. Among the families who apply and do not win vouchers, those with $v_{1i} \geq 0$ 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 the fraction not winning the voucher times the fraction with $v_{1i} \geq 0$:

$$Pr(Apply\ and\ No\ Voucher\ and\ Switch) = (1-\pi)^{*}(1-F_{v_1}(0)). \qquad [7]$$

Hence, the rate of switching among non-winners can be used to identify $1-F_{v_1}(0)$, the fraction who would switch in the absence of a voucher. The average slope of the demand curve for private school over the subsidy range from 0 to $\xi-\frac{c}{\pi}$ can now be expressed as $\frac{\frac{1}{1-\pi}{}^{*}Pr(Apply\ and\ No\ Voucher\ and\ Switch)-Pr(Apply)}{\xi-\frac{c}{\pi}}$. 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 application cost, the voucher amount, and the likelihood of the voucher being awarded.

4 Results

4.1 Effects of initial class type on changes in school, class type, and grade

Following eqs [1]–[3], results in Table 3 are presented using the three different measures of sample attrition as dependent variables: changing schools (in columns 1, 2, 5, and 6), leaving the Tennessee public school system (in columns 3, 4, 7, and 8), and grade repetition (in columns 9 and 10). In columns (1)–(4), the dependent variable indicates whether that form of attrition happened after kindergarten, and in columns (5)–(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 kindergartners, as kindergarten was not mandatory in Tennessee Public Schools at the time (Krueger 1999, 501). The STAR data do not indicate whether students who left the experiment changed schools or grades. For the "changed school" outcome variable, all kindergartners who left the sample are assumed to have changed schools, as are first and

Table 3: OLS estimates of effects of class type on attrition, kindergarten entry cohort (clusters = 75)

Initial class type	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Changed school in first year		**Left public school system in first year**		**Changed school by third grade**		**Left public school system by third grade**		**Recommended to repeat by third grade**	
Small	−0.033	−0.048	−0.018	−0.022	−0.008	−0.024	−0.019	−0.024	−0.017	−0.012
	(0.016)**	(0.014)**	(0.012)	(0.011)**	(0.016)	(0.014)*	(0.013)	(0.013)*	(0.010)	(0.010)
Regular w/aide	−0.006	−0.017	−0.005	−0.009	0.020	0.007	−0.001	−0.006	0.000	0.001
	(0.015)	(0.015)	(0.010)	(0.010)	(0.017)	(0.014)	(0.013)	(0.012)	(0.009)	(0.010)
R^2	0.001	0.097	0.001	0.044	0.001	0.119	0.001	0.048	0.001	0.052
Controls?		Yes		Yes		Yes		Yes		Yes
School fixed effects?		Yes		Yes		Yes		Yes		Yes

Notes: 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 are given in the text.

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 3 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 3.3–4.8 percentage points less likely to change schools in the first year and 0.8–2.4 percentage points less likely to change schools by third grade. We find that much of this school changing behavior was attributable to students leaving the public school system, which was 1.8–2.2 percentage points more common in the first year and 1.9–2.4 percentage points more common by third grade for those assigned to regular classes than for those assigned to small classes. Without controls or school fixed effects, one of these four attrition effects is significant. All four effects increase when controls and school fixed effects are added, and we find significant effects for attrition after the first year and marginally significant effects for attrition by third grade. 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. 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.

4.1.1 Estimates 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. Table 4 shows estimates of eqs [1], [2], and [4] separately for inner city, suburban, urban not inner city, and rural schools. Panels A and B show estimates of eqs [1] and [2], where the dependent variables

Table 4: OLS estimates of eqs [1] and [4] excluding aide as a regressor

Initial class type	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	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)
R^2	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)	(0.021)	(0.018)	(0.025)*	(0.026)**	(0.009)	(0.010)
R^2	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	

(*continued*)

Table 4: (*Continued*)

Initial class type	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
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)**
R^2	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: This table shows estimates of eq. [1] in panel A, eq. [2] in panel B, and eq. [4] in panel C, all with $Aide_i$ omitted as a regressor. In panels A and B, these regressions are the same as in columns (1)–(4) of Table 3 but with “regular with aide” omitted as a regressor and estimates presented separately by level of urbanization. The sample in panel C is restricted to students who remained in Project STAR from kindergarten through third grade. Within each column, the three equations in panels A–C are estimated simultaneously using SUR (suest in Stata). Standard errors are clustered by kindergarten school id.

are indicators for having changed schools and having left the public school system after the first year in Project STAR. Panel C shows estimates of eq. [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, 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, all three equations 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.[11]

In panel A, we find large and significant negative effects on changing schools 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. Focusing on those leaving the public school system produces considerably smaller effects of −0.03 to −0.02 for inner city schools and −0.01 for suburban schools, and it produces slightly smaller effects of −0.01 for rural schools. All six of these coefficients are statistically insignificant. Surprisingly, for urban schools not in the inner city, the effects change signs in panel B, and we find that small classes reduced attrition by −0.04 to −0.06 percentage points; these effects are marginally significant in one and significant in one of the two specifications. The estimates in panel A are very imprecise; however, for these urban, not inner city families, being in a small class appears to have significantly reduced the fraction switching to private school but increased the fraction changing public schools. If the effects in panel A are interpreted as statistical zeros, one possible explanation for this finding is that being assigned to a small class improved families' opinions of public schools, but only for those families that were already planning to change schools. For eq. [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

11 Because the set of regressors is the same across these three equations, the OLS and SUR estimates are identical. The OLS standard errors are nearly identical to the SUR standard errors shown here.

a small class was associated with 2.3–2.6 more years in a small class from grades one to three.

4.2 Effect of a $1,000 voucher on private school enrollment

Next, we use eqs [6] and [7] and the take-up rates from Table 2 to estimate the reciprocal of 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 0.35–0.54 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, DC, $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, DC, $11,781 for Milwaukee, $7,810 for San Antonio, and $11,052 for Pensacola.

Table 4 presents our estimated response per $1,000 voucher year for the six different programs. This response is estimated as $\frac{\frac{1}{1-\pi}{*}Pr(Apply\ and\ No\ Voucher\ and\ Switch)-Pr(Apply)}{\xi-\frac{c}{\pi}}$, with π, $Pr(Apply\ and\ No\ Voucher\ and\ Switch)$, and $Pr(Apply)$ taken from Table 2. In columns (1)–(4), $Pr(Apply\ and\ No\ Voucher\ and\ Switch)$ is computed as the fraction of non-winners switching to private school from column (9) of Table 2 multiplied by the fraction of eligible who applied from column (6) of Table 2. In columns (5)–(8) of Table 2, the fraction of non-winners switching to private school is assumed to be zero. In columns (1), (2), (5), and (6), the application cost c 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 36 estimates presented in Table 4, we obtain an average response per $1,000 voucher of 0.0048 and a median response of 0.0037. Across specifications, the assumption of 50% discounting is the most quantitatively important variation, and the estimates that assume discounting in the even columns are 56% larger on average than the estimates that assume a zero discount rate. Assumptions regarding $Pr(\textit{Apply and No Voucher and Switch})$ have a moderate-sized effect on the estimated response; the estimates that use data on this fraction in columns (1)–(4) are on average 30% smaller than their counterparts in columns (5)–(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 8.2% larger than the estimates that ignore this cost.

When averaged across specifications, we obtain responses per $1,000 voucher of 0.0021 for New York, 0.0040 for Dayton, 0.0109 for Washington, DC, 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, DC 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, DC, we obtain a range of voucher responses of roughly 0.002–0.006. It is this range of values that we use for the inner city STAR schools in our economic value estimates.

While 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 0.002–0.006 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 0.0004–0.001 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.

4.3 The economic value of class type

Next, in Table 6, 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 Table 5. In panel A, the normalized attrition response is $\beta_{ts}^{LSch}/\alpha_s^s$, where attrition is measured as changing schools after the first year. In panel B, this response is $\beta_{ts}^{LSys}/\alpha_s^s$, where attrition is measured as leaving the public school system after the first year. These ratios of coefficients are computed from the estimates in Table 4, with standard errors estimated using the delta method. As in Table 4, 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 rows below the normalized attrition effect in Table 6 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 normalized attrition effect 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 estimates from Table 6 reiterate and express in dollar terms the findings from Table 5. Ideally, we would know the fraction who would leave their randomly assigned classroom in response to \$1,000 in subsidies. What we have an estimate of is the fraction who would switch to private school in response to \$1,000 in subsidies. This latter fraction is certainly smaller than the fraction who would leave their classrooms, as the number switching to private schools is a subset of the number leaving the classroom at all. In panel A of Table 6, we use the latter fraction as a proxy for the former one and consequently overestimate parents' valuations of class size reductions. In panel B of Table 6, we ignore switching public schools as a potential response and consequently underestimate parents' valuations. Thus, we will average over these two attrition measures in our discussion.

As in Table 4, we obtain moderate-sized attrition effects for inner city families, smaller effects for suburban families, and even smaller effects for rural families. In all three cases, the effects are considerably larger when attrition is measured as changing schools rather than switching to private. When averaged across the two attrition measures, our estimated valuations per year of small classes range from \$4,027 to \$13,263 for inner city parents and from \$2,077

Table 5: Estimated change in fraction switching to private per $1,000 voucher

Program site	(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	
	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	0.0015	0.0021	0.0015	0.0022	0.0019	0.0026	0.0019	0.0028
Dayton, OH	0.0019	0.0032	0.0020	0.0035	0.0039	0.0064	0.0041	0.0070
Washington, DC	0.0074	0.0106	0.0081	0.0120	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: 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 2 and using the formulas from eqs (6) and (7). Additional details in the text.

Table 6: 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: Within each panel, the first row measures the "normalized attrition effect" associated with an expected future yearlong change to a small class relative to a lottery between a year of a regular class and a year of a regular class with aide, as described in the text. In panel A, the normalized attrition effect is measured as the coefficient on small class from eq. [1] in panel A of Table 6 divided by the coefficient on small class from eq. [4] in panel C of Table 6. The normalized attrition effect in panel B is computed in the same way, but with the attrition effects taken from eq. [2] in panel B of Table 6 rather than from panel A. The standard errors are calculated by simultaneously estimating the attrition regression and the years in small class regression using SUR (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 kindergarten school. 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. Additional details are given in the text.

to \$7,656 for suburban parents. 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. For rural parents, we use the smaller range of voucher effects, and when averaged across the two attrition measures, our estimates range from \$3,120 to \$15,024. We obtain the same strange result for urban, not inner city families as in Table 6, with positive (but insignificant and imprecisely estimated) effects of small on changing schools and moderate-sized negative effects on switching to private school. Averaging across the two attrition measures and using the smaller range of voucher effects for urban, not inner city parents, we obtain parental valuations per year of small class ranging from \$1,604 to \$18,270.

5 Conclusions

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, parents on the margin of sending their children to private school valued 1 year of a small class at \$2,000–\$18,000 in 2010 dollars.

Krueger (1999, 530) calculates the cost of reducing kindergarten class size from regular to small is roughly \$3,000 per student per year in 2010 dollars; Project STAR researchers obtain similar cost estimates (Word et al. 1990, part IX, 175). Our ranges of estimated benefits to parents generally exceed this cutoff for cost-effectiveness, but with the lower ends of the ranges falling close to or below this threshold. Because these benefit estimates apply to families on the margin of switching to private school, they probably overstate the value of small classes for the average family, who is probably less sensitive to school quality than the marginal family[12] is. For the average family, the perceived benefits of small classes may be closer to the lower ends of our estimated valuations (and consequently closer to the cost–benefit threshold). While their estimates are imprecise, Chetty et al. (2011) 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 far exceed the true lifetime earnings gains; however, it is not clear how parents' valuations of small classes compare to what they believed the earnings (and non-earnings) effects of small classes to be.

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Appendix: comparison between STAR and voucher samples

To examine the comparability of the Project STAR participants with the students in the voucher programs discussed in the text, Table 7 shows sample means for a handful of characteristics for the STAR data and the populations meeting the eligibility criteria for four of the voucher studies. Panel A shows means when the sample is restricted to kindergartners enrolled in public schools, and panel B shows means for kindergartners in public or private schools. Columns (1)–(4) shows estimates for the different levels of urbanization from the Project STAR data, and columns (5)–(8) show estimates for New York kindergartners at or below 185% of the poverty line, Dayton, OH kindergartners at or below 200% poverty, Washington, DC kindergartners at or below 270% poverty, and Milwaukee kindergartners at or below 175% poverty.

12 Results by race indicate that marginal families are not significantly more likely to be of minority race for their region type. While white/Asian students seem to fuel the result for non-inner city urban students, 90% of the students of our sample of non-inner city urban schools are in fact white. The majority of our inner city effect comes from black students, who comprise 96% of the population there. The suburban effect is fueled by both black and white students, as about 40% of students in our sample of suburban schools are black, and 60% are white.

Table 7: Descriptive statistics, Tennessee STAR and program eligible kindergartners in voucher cities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Tennessee STAR, 1985			Eligible kindergartners in voucher cities (2000 Census)				
Variable	**Inner city**	**Suburban**	**Urban, not inner city**	**Rural**	**New York, NY**	**Dayton, OH**	**Washington, DC**	**Milwaukee, WI**
Panel A: Sample restricted to students attending public schools								
Black	0.976	0.325	0.103	0.060	0.363	0.362	0.812	0.671
	(0.004)	(0.013)	(0.015)	(0.004)	(0.011)	(0.043)	(0.025)	(0.033)
Age on	5.441	5.488	5.537	5.531	5.137	5.582	5.092	5.130
April 1	(0.014)	(0.014)	(0.026)	(0.010)	(0.016)	(0.059)	(0.044)	(0.058)
Urban	1.000	0.000	1.000	0.000	1.000	0.455	1.000	1.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.045)	(0.000)	(0.000)
Eligible for	0.904	0.276	0.424	0.404	0.772	0.667	0.617	0.800
Free lunch	(0.008)	(0.012)	(0.024)	(0.009)	(0.009)	(0.042)	(0.031)	(0.028)
Observations	1,318	1,306	436	2,814	1,977	125	253	204
Panel B: Sample includes students in public and private schools								
Attending private					0.160	0.100	0.041	0.116
School					(0.008)	(0.025)	(0.012)	(0.021)
Observations					2,354	140	278	228

Notes: Sample means presented with standard errors of the sample means in parentheses. Tennessee STAR sample is the same as in panel Table 1 of the main text. Standard errors are computed with not clustering to facilitate comparability with the Census data. Voucher city samples include kindergartners in the April 2000 Census who satisfied the residency and income eligibility requirements for the voucher programs as described in the main text. The 2000 Census is used rather than the 1990 Census for Milwaukee, because the 1990 Census does not include data on "grade currently attending." "Black" is used rather than "white/Asian" because, given the multiple racial categories in the 2000 Census, "white" is difficult to identify in a way that corresponds with the Project STAR data. Means are not shown for San Antonio and Pensacola because those eligible populations are defined based upon school district and school attendance areas not measured in the Census.

In general the inner city students in Project STAR schools are fairly similar to the students in the voucher samples, and the Project STAR samples from suburban, small urban, and rural areas tend to include fewer black students and poor students than we observe in the voucher samples. As the results from Table 7 show, the fractions of students who were black and who were eligible for free lunch is higher, at 0.976 and 0.904, for students from inner city schools in Project STAR than for voucher eligible students in the five cities studied, where the fraction black ranged from 0.362 to 0.812 and the fraction eligible for free lunch ranged from 0.617 to 0.800. Students in Project STAR schools at other levels of urbanization all showed lower fractions black and lower fractions eligible for free lunch than in the voucher samples. At all four levels of urbanization, kindergartners in Project STAR tend to be older (with ages ranging from 5.441 to 5.537) than those in the voucher cities, where the ages ranged from 5.092 to 5.137 plus one particularly high average age of 5.582 for the Dayton sample.

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