Public School Choice:
Charter and Magnet Schools and the Public-Private School Decision

Job Market Paper

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ABSTRACT

This paper examines whether access to public schools of choice influences a household's decision to choose private school for their child. To date, much of the analysis regarding charter and magnet schools focuses on their effects on sorting behavior and student achievement. A developing area of this literature examines the effects of public school choice programs on sector enrollment. This paper adds to the latter. I employ nationally representative, individual-level data on students and their families — for which I have been granted access to restricted geo-code information. I supplement these data by matching students with their respective school districts using geographic information systems (GIS); I then examine whether relative measures of public school choice in a school district influence the household's public-private school decision. I do not find strong evidence that households respond to "choice" in general. I find some evidence to suggest a charter school effect; rather than private sector crowd-out, however, I observe that households in districts with a greater proportion of charter schools are more inclined to choose private school. I find no evidence of a magnet school effect, which implies that households do not treat charter and magnet schools as substitutes when making their public-private school decision.

I. INTRODUCTION

In recent years, primary and secondary schooling options have increased dramatically through the advent or growth of programs generally referred to as *public (sector) school choice* programs that allow parents to choose among multiple public schools rather than have their child's school be determined solely by their place of residence. Rooted in the philosophy of equal opportunity to quality education, public school choice (PSC) is essentially the aggregate of a diverse collection of programs, policies, and institutions that aspire to provide quality education for all students. Public schools of choice (such as magnet and charter schools) and programs (such as enrollment polices) have multiplied and spread across the United States. Even though many parents have access to these public sector alternatives, they may still choose to opt out of the public sector altogether. Private school remains an option for parents who find their neighborhood school inadequate. But as parents choose between public and private schools for their children, to what extent does PSC factor into that decision? Does "choice" itself in the public sector affect the public-private school decision? Furthermore, does such an effect depend on the type of school choice available?

Two forms of PSC are considered in this paper: charter schools and magnet schools.[1] In a general sense, these schools are arguably somewhere *between* traditional public schools and private schools. Charter schools are public schools that are privately owned and operated. They operate under a state charter that determines their degree of autonomy and their evaluation standards; they are not required to adhere to all the guidelines of traditional public schools in their state. Magnet schools are publically owned and operated; their origins lie in the pursuit of racial and socioeconomic diversification, and they adhere to the guidelines of the district they operate within. Both types of schools relax the residential constraint on households, although the extent to which

[1] In addition to magnet and charter schools, open enrollment policies also play an important role in PSC; however, I do not include open enrollment policy measures in this paper.

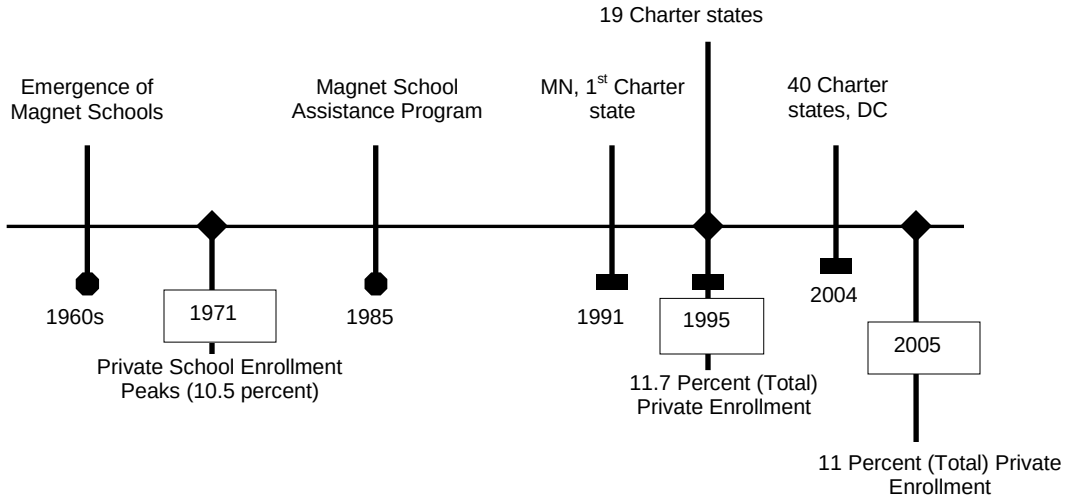
the constraint relaxes can vary.[2] Generally, on residing in a district with a charter or magnet school, a household can apply for their child to attend regardless of where they live in the district. Both magnet and charter schools are also meant to be inherently unique — to draw a diverse group of students by offering superior and/or unique curriculum or teaching styles. Charter schools often focus on nontraditional education techniques or concentration of disciplines. And magnet schools generally tout the excellence and/or originality of their programming in tandem with their commitment to diversity.

Over the years, both charter and magnet schools have boomed in the United States, fueled in part by federal grants. One could argue that such growth among these schools of choice, in combination with federal legislation such as No Child Left Behind (NCLB), reflect a sentiment that parental choice in public school education can be superior to residential assignment. Illustration 1 shows an abbreviated timeline of the progress of charter and magnet schools in the United States, highlighted with a few private school enrollment trends. Although magnet schools date to the 1960s, charter schools got underway only in the 1990s. Over the last 10 to 20 years, both types of schools have come a long way to establish themselves as formidable presences in public elementary and secondary education. In the 2007-2008 academic year, more than 4,000 magnet schools were operating in the United States, serving more than 1.3 million students in 40 states and Washington, D.C., and representing approximately 3 percent of the public school population.[3] In 2008, approximately 4,300 charter schools operated in 40 states and Washington, D.C., serving more than

[2] Some magnet schools reserve a number of placement spots for students residing in the immediate or traditional catchment area. Charter schools often allow cross-district enrollment. Although application procedures and a school's residential requirements would likely affect the degree to which households consider various PSC schools to be a legitimate option, I am not able to observe such qualities in these data and do not attempt to control for them.
[3] Magnet school data for 2007-2008 come from the Center for Education Reform; data available online at: http://www.edreform.com/_upload/CER_charter_numbers.pdf.

1.2 million students.[4] For context, in 2008, approximately 6.1 million primary and elementary students were projected to be enrolled in U.S. private schools.[5]

Illustration 1. PSC Progress in U.S. with Private School Trends



Charter and magnet school data come from the U.S. Department of Education (Nelson, B. et al 2000); Magnet School Assistance: http://www.ed.gov/programs/magnet/index.html), U.S. Charter Schools (http://www.uscharterschools.org/pub/uscs_docs/index.htm, and Magnet Schools of America (http://www.magnet.edu/index.php). Private school data come from National Center for Education Statistics (http://nces.ed.gov/fastfacts/display.asp?id=65).

To estimate the effects of these PSC schools on the household's public-private school decisions, I employ a richly detailed national cross-section dataset, well supplemented to add scope, to look at individual students in elementary school. I further examine whether PSC effects are school-type specific: do households treat charter and magnet schools as substitutes or do the different schools elicit unique responses? I find that conditional on private school access measures, there is little evidence to suggest that households respond to aggregate measures of PSC in their

[4] Charter school data for 2008 come from the Department of Education; data available online at: http://www.ed.gov/nclb/choice/schools/choicefacts.html.

[5] Private school enrollment projection data come from the NCES. Source: http://nces.ed.gov/fastfacts/display.asp?id=65. Because various sources for enrollment data (magnet, charter and private) are used, the reader is cautioned against interpreting these statistics as strict point estimates. The values presented may represent data from various initial sources and may not be directly comparable, so the values are presented only to provide context.

district. Conditional on school type, I find evidence to suggest a charter school effect. Specifically, the percentage of charter schools in a district is positively related to a household's choice of private school. I find no notable magnet school effect, implying that these schools of choice are not treated as substitutes for one another.

In the following section I briefly discuss the most relevant literature that has examined the effects of types of public choice schools on private enrollment, and I explain how this paper contributes to the existing literature. In section 3, I discuss the data and sample used in the analysis. In section 4, I present the empirical model. In sections 5 and 6, I present the results and discuss my findings. Section 7 concludes.

II. Contribution

To date, much of the research on charter schools has focused on how these schools affect student achievement, sorting, and school competition (Bettinger 2005; Bifulco & Ladd 2006, 2007; Booker et al. 2008; Cobb & Glass 1999; Hoxby 2004; Renzulli & Evans 2005; Sass 2006; Wamba & Ascher 2003). This research agenda has been similar across magnet school studies (Archbald 1988; Clewell & Joy 1990; Gamoran 1996; Henig 1995; Saporito 2003; Steel & Levine 1994).

The literature recently began examining the effects of PSC on private school enrollment. Though most research that addresses this issue has focused on charter schools, Dills (2004) finds evidence that suggests high-quality magnet schools may compete, to some extent, with private schools for students. Lander's (2007) report on how charter schools affected Catholic school enrollment in Michigan and Arizona finds mixed outcomes. With rapid growth of charter schools in both states, Catholic school enrollment suffered only in Michigan. Lander stresses that the difference may be explained by Arizona's tax credit programs and Michigan's limited financial assistance for private schools and stagnant school-aged population. Toma et al. (2006) and Roy and Chakrabarti (2007) find evidence that competing charter schools crowd-out the private sector. Toma

et al. (2006) examine charter schools in Michigan in the mid-to-late 1990s, when charter schools were developing significantly in the state. Specifically, the authors estimate that approximately 20 percent of Michigan charter school students previously attended private schools. Roy and Chakrabarti (2007) reexamine the potential charter school crowd-out in Michigan and argue the effects are marginal.

Both Toma et al. (2006) and Roy and Chakrabarti (2007) look at how nearby charter schools affect private school enrollment — measured as either the percent of the school district's student population or the percent of the county's student population. Toma et al., who employ a county-level analysis, measure charter school competition by the fraction of county students enrolled in charter schools. Roy and Chakrabarti, who conduct a school district level analysis, use alternative measures for charter competition: the fraction of district students enrolled in charter schools, the number of charter schools in the district, and the number of charter schools that fall within a particular distance (one, two, or five miles) from private schools. Additionally, the authors, who study a 12-year span, always include an indicator variable denoting the presence of a charter school in a school district in tandem with any particular charter competition measure.

My study closely follows Roy and Chakrabarti's, with some extensions. First, my analysis is national rather than state specific. Although charter schools can vary significantly by state, I control for state effects in my analysis. Second, the unit of observation is the individual student. Each student in my sample is either in private school or public school, so I account for everyone in the dependent variable. Enrollment ratios may be subject to measurement error without a means of controlling for students who are homeschooled or for student mobility over time. In addition, the individual level data I employ contains student and household variables proven to influence the household's public-private school decision, such as race, household size, and income (Goldhaber 1996; Long & Toma 1998). Third, I purposefully examine the extent to which magnet schools affect

the public-private school decision rather than solely concentrating on charter schools (Lander 2007; Roy & Chakrabarti 2007; Toma et al. 2006), and I examine aggregate affects of PSC, which do not distinguish between charter and magnet schools.[6] I further build on Roy and Chakrabarti's work by including an alternative measure of choice school competition — the fraction of schools that are choice in a district.

III. Data Section

The principal component of my dataset is a sample of third-grade students and their families from the Early Childhood Longitudinal Survey — 1998-1999 Kindergarten Cohort (ECLS-K) from the National Center of Educational Statistics (NCES). This nationally representative dataset follows approximately 20,000 students in private and public school from kindergarten through eighth grade, interviewing them approximately every-other spring.[7] For participants, surveys were completed by the student, a parent, teachers, and school administrators. I focus on the parent survey, which provides information on family structure, health, household socioeconomic characteristics, and school participation by the student and parents. In particular, I use the restricted geo-code data of the ECLS-K, which offers two key pieces of information: school and geographic identifiers. The school identifiers match students with their particular school, whether public or private, and the geographic identifiers match students with their residential census tract and/or zip code. Both the school and the geographic identifiers can be further linked to outside data sources: public schools (and school districts) can be matched with the NCES's Common Core of Data (CCD), private

[6] A number of studies have looked at the types of students who attend magnet schools (Archbald 1988; Clewell & Joy 1990; Dills 2004) and the types of students who attend charter schools (Bifulco & Ladd 2007; Hanushek et al. 2007; Renzulli & Evans 2005; Zimmer & Buddin 2006). Evidence suggests that magnet schools attract minorities and students who live nearby; charter schools attract academically concerned households and special groups of students. Magnet schools, on average, possess a stronger commitment to racial diversity; however, schools of choice can vary significantly by whom they attract as well as what type of students they target.

[7] The surveys are administered biannually for kindergarten (1998-1999) and first grade (1999-2000) and then in the spring semesters of the third-, fifth-, and eighth-grade years (2002, 2004, 2007, respectively). Additionally, the dataset is nationally representative of kindergarten students in the 1998-1999 academic year and those in first grade in 1999-2000; the sample is not nationally representative of third-grade students.

schools can be matched with the NCES's Private School Universe Survey (PSS), and the geographic identifiers can be matched with 2000 Decennial Census data.

Supplemental data come from a number of sources. The Census of Governments Survey of Local Government Finances (form F-33) is an annual comprehensive survey by the U.S. Department of Commerce that reports on the financial practices of U.S. school districts. The CCD is another annual comprehensive dataset for U.S. schools that contains data on teacher and student demographics as well as enrollment numbers and student-teacher ratios. The PSS is a biannual comprehensive dataset for U.S. private schools, which includes data on grade range and student demographics. I also supplement with data from the Census 2000 Summary File 3 – Sample Data, a one-in-six U.S. households sample, and Census 2000 Cartographic Boundary Files at the school district and census tract levels. These boundary files are a form of geographic information systems (GIS); they are spatial data represented by maps of geographical areas and borders. These spatial data also include an array of qualities and characteristics that further depict each particular geographic area's population.

As my focus is to examine how PSC affects the household's public-private school decision, it is crucial to control for factors that likely influence this decision at the household level as well as the factors that define and contribute to school choice. In building a dataset to address this research question, I match the geographic identifiers in the ECLS-K data with the subsequent components listed above. Some discussion of this process and the creation of certain variables is warranted.

A. Data Sources

A.i. ECLS-K

As mentioned above, the ECLS-K survey is administered roughly every other year, including the students' expected first-, third-, and fifth-grade years. I use the third-grade sample in my analysis for three main reasons. First, this is the last year in the restricted data that the residential census tract

for the student is reported, and I want to use this finer geographic detail rather than the zip code to control for any underlying heterogeneity within counties or school districts mutually resided in by students in the sample. Additionally, I require the census tract (preferred over the zip code) to match students with a school district based on their place of residence. Second, third grade is generally accepted as an elementary school grade, whereas fifth grade has the potential to be in a middle school or other nontraditional elementary-level school. Using third-grade students allows me to be consistent across school levels in this analysis, and focusing solely on elementary school students provides more choice because elementary schools are more numerous than any other school level. Third, using data on third-grade students rather than first-grade students allows the sample to consist of students who have been in school a few years, have possibly formed opinions along with their parents about where they want to attend school based on their experiences, preferences, and/or abilities (and, of course, their alternatives), and are likely mature enough to be engaged in programs that represent a core aspect of their school.

Of the initial 15,300 students in the third-grade survey sample, I drop students who repeated or skipped grades or those lacking valid school identifiers and residential census tracts. By retaining only third-grade students with valid school and census tract identifiers, I reduce the sample size to 11,130 observations.[8] For these remaining students, I collect student-level and household-level data. The variables collected include the student's gender, age, race, disability status, and an indicator variable taking the value of one if the student changed schools since the last interview (spring of the first-grade year). At the household level, I collect an income measure, the size of the household, parental structure indicators (i.e., no mother or no father), parental employment, mother's age, and urban/rural status. Additional geographic explanatory variables include regional controls and an indicator variable taking the value of *one* if the state of residence was a non-charter state as of 2000.

[8] In compliance with the NCES's restricted data regulations, all sample sizes reported have been rounded to the nearest 10.

The ECLS-K oversamples specific student populations to produce a sample that reflects the universal U.S. kindergarten class of 1998-1999. Additionally, the survey employs different rates of mobility tracking for students who change schools, such as oversampling minority groups. Because of these issues, I use the most appropriate cross-sectional sampling weight for my analysis.

A.ii. 2000 Decennial Census Data

It is important to note that the 2000 Decennial Census data is the only supplemental data that does not correspond with the 2001-2002 academic year of the ECLS-K third-grade sample. These Census data correspond with the 1999-2000 academic year, the first-grade year for students in the sample. The variables I create using the Census data perform two functions: they assign students to a public school district and account for the number of students enrolled in grades K-12 who live in the student's residential census tract. Although these two measures represent data two years premature to the third-grade year for the sample, they are unlikely to have changed substantially by the time the students are in third grade. Occasional anomalies might surface: school district borders may change or schools could open or close between the academic years of 1999-2000 and 2001-2002; however, such occurrences are expected to be minimal and are not further addressed.[9]

I also use the 2000 Decennial Census to match students with public school districts through a GIS matching process. Although the public school students in the sample are already matched with the school district of the school they attend, private school students have no such match. To be consistent across all observations, I match the students in the sample with the school district in which their residential census tract falls. To do this, I employ the Census 2000 Cartographic Boundary Files at the census tract level and the school district level. In this process, I map the school districts within a state and then overlay that state's census tract map using combatable

[9] In the final sample, no observed school district reported a significant change in their border since the last school year (2001-2002 CCD survey data, school district level). However, this condition must be examined also for the previous year to validate the approach of retaining the school district assignment of the 1999-2000 academic year to that of the 2001-2002 academic year.

mapping specifications. When a census tract falls geographically within a given school district, the census tract is assigned, or matched with, that school district. For this analysis I use the 3,770 ECLS-K student observations for which I have matched census tract with school district based on this mapping exercise. For these matches, the entire census tract falls within the school district, meaning the census tract neither crossed nor lay on a school district border. This significantly reduces the observations, so concerns regarding potential sample selection bias are reasonable. I address such concerns in sub-section B, where I discuss the final sample.

The 2000 Decennial Census Cartographic Files at the school district level further include data on a variety of qualities of a district's residents. Once students are matched with a school district, I further supplement this GIS data by including demographic and socioeconomic measures at the school district level to control for various community characteristic associated with the entry of private schools (Barrow 2006) and public schools of choice (Glomm et al. 2005) into an area.

A.iii. Common Core of Data & Form F-33

I use the 2001-2002 CCD data, which corresponds to the academic year of the third-grade ECLS-K sample. Because some variables are reported at the district level and others at the school level, I use both district and school-level data. However, I aggregate school-level data to the district level.

I calculate the school district variables to describe the district itself and to profile the district's average or typical school. To accomplish the former, I collect variables representing the number of schools in a district, the grade-span of the district (for this I employ a dummy variable for elementary-only school districts), the fraction of Title I eligible schools in the district, and an indicator variable denoting whether significant border change occurred since the last school year. Additionally, I supplement from the Census of Governments F-33 survey for the 2001-2002

academic year to create per-student levels of total district expenditures and revenues (disaggregated across federal, state, and local levels).

I also create a set of variables representing school district means using the school-level CCD data. These averages are strictly district averages; they are not weighted or non-weighted school means. Although such alternative measures can and perhaps should be used rather than the simple district mean, preliminary comparisons of the mean values based on the different construction techniques do not reveal strong differences across methods. The remaining district-level mean variables include the proportion of district students who are free-lunch eligible, student-teacher ratio, proportion of students by minority race or ethnicity, and the proportion of female students.

The school-level CCD data is also the source of the public school choice variables. The CCD asks schools to self-report their magnet or charter status in the school-level survey. Adding to these particular measures, I create a variable that does not distinguish between charter or magnet but merely indicates whether a school is a choice school.[10] In the final dataset, 42 schools indicate they are both charter and magnet, so I create a final classification of *both* magnet and charter. Although I control for these "both" measures, I do not report them in the results as they represent a trivial amount of school, and the coefficients are generally insignificant. For each of these four school-level classifications (magnet, charter, choice, and both), I aggregate to the district level and create three main types of PSC measures: number of schools in a district, percent of schools in a district, and percent of district enrollment. This process yields twelve PSC measures.

All school district measures represent the school district assigned to the student by the GIS matching. Thus, the core of the dataset is the ECLS-K sample, matched via the residential census tract to census tract- and school district-level measures from the 2000 Census. The school district

[10] In the data, I cannot distinguish between magnet schools and traditional public schools with magnet programs. Both are classified as a magnet school.

assignments are further supplemented using 2001-2002 CCD data and Census of Governments form F-33. The remaining piece is a measure of private school access.

A.iv. Private School Universe Survey

I use the PSS data from the 2001-2002 academic year to create a measure of private school accessibility. The PSS data is collected at the school level, and it provides the county of each school's mailing address. I use this information to control for the presence of private schools in a student's county of residence. Although a given private school need not limit enrollment policies to its county of operation, I contend that this is a logical first step to measuring the presence of private schools. Though I do not include neighboring counties here, such an extension would be a logical next step and would help address the issue of the cross-county enrollment. For now, however, I focus solely on the student's county of residence. Specifically, I tabulate the number of private schools in the county.

B. The Sample:

The final sample that I use in my analysis consists of 3,770 observations. As mentioned previously, it is notably smaller than the initial 15,300 students in the ECLS-K third-grade survey sample. Dropping non-third-grade students as well as observations with missing census tract data or school identifiers reduces the sample size to 11,130 observations; thus, the biggest contributor to the reduction in sample size comes from the spatial matching of census tract to school district. Recall, only census tracts that fall completely within a given school district are matched in this dataset. Census tracts that either fall along or cross a school district border were not immediately matched to a particular school district using the mapping software and are not included in this analysis.[11]

Focusing on census tracts that fall completely within school districts avoids having to match students that reside in census tracts that cross school district borders — an exercise that is not

[11] The most immediate extension of this work is to match students residing in census tracts that fall along the border of a school district with that school district.

clearly defined in and of itself. In addition, with this matching procedure, I ultimately focus my sample on students who live in more urban areas, which is the most appropriate sample for this study as it is generally urban areas, rather than rural, for which both public schools of choice and private schools are relevant options.

Because the final sample size represents a significant reduction in observations, it is important to compare how the reduction changes the composition of the larger third-grade sample. Table 1 summarizes student and household qualities of the reduced, or matched, sample of 3,770 students and the larger sample of 11,130. Because missing categories for variables are created for the analysis, I do not omit student observations with incomplete information. Thus, the summary statistics below do not represent a balanced panel — the means represent only valid observations, so the number of observations across descriptive variables can vary.

A comparison of the student profiles for the matched sample of 3,770 students versus the sample of 11,130 third-grade students with valid census tract and school identifiers, presented in the top portion of table 1, reveals key differences in the otherwise similar groups. The dependent variable in the analysis, private school enrollment, does not appear sensitive to limiting the data to only census tracts located completely within a school district; the fraction of sample students attending private school is only slightly higher for the matched sample: 12.51 percent versus 12.08 percent. The most apparent difference in observed student characteristics between the matched and larger sample is the differences in the students' racial profiles: students residing in census tracts completely within their matched school district are more likely to be African-American or Hispanic and less likely to be white. Students in the matched sample are also slightly more likely to have changed schools since the last interview (spring of the first-grade year).

Table 1: Student and Household Attributes for Matched Sample and Larger Third-Grade Sample

	Matched Sample (N=3,770)		Third-Grade Sample (N=11,130)	
	Mean	SD	Mean	SD
Student Attributes				
Private	0.1251	(0.3309)	0.1208	(0.3259)
Female	0.4968	(0.5001)	0.4980	(0.5000)
Age	9.27	(0.39)	9.28	(0.38)
African American	0.2220	(0.4157)	0.1516	(0.3587)
White	0.4087	(0.4917)	0.5790	(0.4937)
Hispanic	0.3018	(0.4591)	0.2017	(0.4013)
Other Race	0.0675	(0.2509)	0.0677	(0.2512)
IEP	0.1138	(0.3177)	0.1131	(0.3168)
Disability	0.2620	(0.4398)	0.2714	(0.4447)
Changed Schools	0.2240	(0.4170)	0.2061	(0.4045)
Household Attributes				
Household Size	4.57	(1.44)	4.52	(1.37)
Both Parents Present	0.6671	(0.4713)	0.7160	(0.4509)
No Father Present	0.3076	(0.4616)	0.2579	(0.4375)
Father Employed	0.9261	(0.2616)	0.9347	(0.2470)
Mother Employed	0.7145	(0.4517)	0.7328	(0.4425)
Household Income, Category Median ($K)	55.51	57.78	63.68	(63.54)
Father's Age	39.85	(7.12)	40.21	(7.04)
Mother's Age	37.30	(6.86)	37.51	(6.55)
Father High School	0.2479	(0.4319)	0.2636	(0.4406)
Father Less than College	0.2819	(0.4500)	0.2862	(0.4520)
Father College	0.1631	(0.3696)	0.1716	(0.3771)
Father Professional	0.1193	(0.3242)	0.1362	(0.3431)
Mother High School	0.2680	(0.4430)	0.2603	(0.4388)
Mother Less than College	0.3417	(0.4743)	0.3567	(0.4790)
Mother College	0.1493	(0.3565)	0.1643	(0.3706)
Mother Professional	0.0770	(0.2667)	0.0896	(0.2856)
Poverty (At or Below)	0.2787	(0.4484)	0.2089	(0.4065)
Northeast	0.1362	(0.3430)	0.1808	(0.3848)
Midwest	0.1495	(0.3566)	0.2320	(0.4222)
West	0.2622	(0.4399)	0.2351	(0.4241)
Suburb	0.3730	(0.4837)	0.4461	(0.4971)
Urban	0.6068	(0.4885)	0.3705	(0.4830)

Private: student attends private school; *IEP*: student has an individual educational program at the school; *Disability*: student has a diagnosed disability on record at the school. Summary statistics are weighted. Omitted category for parental education variables is *Less than High School*. Statistics are weighted.

The bottom portion of table 1 comprises a comparison of the household profiles for these samples. The statistics present fairly comparable household structures, with some differences in family composition, socioeconomic status, and notable geographic discrepancies. These differences in geography that are observed across the two groups appear to be consistent with how variation in population density likely impacts the geographical size of school districts. Households in the matched sample of 3,770 students are slightly less likely to have both parents residing in the home

than the larger sample of 11,130; the absence of the father is more prevalent in the sample of households residing in census tracts located completely within a school district. Overall, households in the matched sample reflect a lower socioeconomic status. Both mothers and fathers have slightly lower levels of education, on average, in the matched sample, and the fraction of households who report being at or below the poverty level is also higher — which supports the finding that households reporting lower income levels are more representative of the matched sample. The most notable differences across the limited and larger third-grade samples are geographic in nature: the fraction of households in the South are overrepresented in the smaller sample, and the fraction of households in the Midwest are underrepresented. And as mentioned, the households in the matched sample are far more likely to reside in urban areas compared with the larger sample of 11,130.

Table 2: Private School Access for Matched Sample and Larger Third-Grade Sample

	Matched Sample (N=3,770)		Third Grade Sample (N=11,130)	
	Mean	SD	Mean	SD
Private School Access Measures				
Private Enrollment, Census Tract	0.1228	(0.1461)	0.1174	(0.1077)
Private Schools, County (Number)	228.38	(377.0)	146.26	(294.6)
Average Private School Enrollment, County	224.09	(70.4)	195.13	(72.30)
At least one Catholic School, County	0.9296	(0.2510)	0.8778	(0.3275)

The private school enrollment measure merely reflects the total number of students in private schools reported by the PSS (2001-2002) in the county divided by the number of private schools in the county. Statistics are weighted.

Finally it is important to present any notable differences in private school accessibility across the matched and larger third-grade samples; table 2 presents this comparison. The fraction of students enrolled in private school (grades K-12) who live in the same census tract as a student in the sample is slightly higher for the matched sample, 12.28 percent versus 11.74 percent.[12] The remaining private school access variables are measured at the county level and represent the 2001-2002 school year. For the matched sample, students generally live in counties with more and larger

[12] The fraction of census tract students attending private school comes from data collected by the 2000 Census Summary 3 File; thus, it represents the fraction of private enrollment for the 1999-2000 academic year, the current sample's first-grade year.

private schools. And students in the matched sample are more likely to have at least one Catholic school in their county of residence.

Table A1 of the appendix reports the weighted summary statistics for school district variables and means — including PSC measures — for the final, matched sample. Also presented are weighted summary statistics for qualities of the residents of these school districts.[13] I report mean weighted summary statistics for all 3,770 students for just the private school students, who represent approximately 12.5 percent of the final sample. Discrepancies across these groups can be attributed to differences in the qualities of the school districts residentially assigned to students in private versus public school. I find that, on average, students attending private school live in school districts that have more access to PSC schools, particularly magnet schools; a higher proportion of Title I eligible schools; a smaller proportion of white students; and a higher proportion of students eligible for free lunch.

Among the qualities representing the residents of the school districts, little difference appears across the private school sample and the overall sample of 3,770. In the public school districts where private school students reside, median household income is slightly lower, and high and low levels of educational achievement are slightly more concentrated.[14] Generally, variation in demographics and socioeconomic measures such as income, race, and age, encourage entry of private schools and charter schools — rather than means values (Barrow 2006; Glomm et al. 2005).

[13] Residential data at the school district level comes from the 2000 Census Cartographic Files (school district level) and represent data reflective of the 1999-2000 academic year.

[14] The residential income measure is believed to be median income, but this has not yet been verified. The values do not match exactly with alternative sources (School District Demographics System) that provide district level median income data but are very close.

C. *Data Limitations*

Using the CCD school-level data to generate measures of PSC introduces a significant data limitation: for a number of schools in the 2001-2002 CCD survey, magnet status is missing.[15] This generally tends to be a state-specific anomaly; only students who reside in the states where this is an issue are affected. Of the matched sample of students used in this analysis, 17 different states of residence are represented; of these 17, missing data on magnet schools is a particular issue for four of them.[16] In such cases, the data are generally missing for all school districts observed in the state. To put this in perspective, 950 observations are in the final dataset with missing magnet school information is, or 25 percent of the 3,770 student observations.[17] When creating variables at the school district level, any district with at least one school reported as *missing* causes the entire district to receive a missing value for any magnet or choice school measure (number of schools, fraction of enrollment, etc.).

Additionally, many specific qualities of charter and magnet schools, such as application procedures and school tenure, are not always (if ever) observable in the data. Although such attributes potentially influence the household's public-private school decision, such specific qualities are not included in this analysis.

IV. Empirical Model:

Using the data I have compiled, I examine whether PSC affects whether households send their children to private school. And to the extent that it does, do households treat charter and

[15] All charter school data in the final sample is reported as valid from the CCD (2001-2002) school-level data.
[16] A handful of remaining states have not undergone the GIS matching. For these states, the number of observations in the data was relatively small (usually less than 100 students per state). These matches will be included in later versions of this paper. The names of states of residence for students in the ECLS-K sample are not released in compliance with the NCES's restricted-data regulations.
[17] For some states, such as Arizona, Florida, and Pennsylvania, magnet schools are included in the CCD survey data but are not distinguishable from traditional schools. It is unclear as to whether magnet school data surfaces in a similar fashion for other states that report missing magnet school data.

magnet schools as substitutes for each other, or do the effects of charter and magnet schools tell different stories regarding their influence over the household's public-private school decision?

Reduced Form – Probit Model

To estimate the household school-choice decision, I employ a reduced form probit model represented by equation 1:

$$\textbf{(1)} \qquad \mathbf{Pr}\,[\mathbf{Y}_i=1] = \alpha_0 + \alpha_1\mathbf{H}_i + \alpha_2\mathbf{D}_j + \alpha_3\mathbf{C}_j + \alpha_4\mathbf{P}_c + \alpha_5\mathbf{R}_{tj} + \varepsilon_{itjc}$$

where *i* represents a household or student, j indicates a school district, *t* represents a census tract, and *c* represents a county. In the model, $\mathbf{Y}_i$ is an indicator variable taking the value of one if student *i* attends private school. Looking categorically at the regressors, $\mathbf{H}_i$ is a collection of student- and household-specific qualities; $\mathbf{D}_i$ is a set of variables describing the school district (*j*) matched with the student's residence; $\mathbf{C}_i$ is a measure of PSC in school district *j*; $\mathbf{P}_c$ is a measure of private school accessibility measured at the level of the county (*c*) which corresponds with the student's residence; and **R** is a set of resident characteristics at the school district level (j) and the census tract level (*t*).

The student-specific variables that **H** comprises include the student's gender, age, race, disability status, and an indicator variable for whether the student changed schools since the last interview. The household variables that **H** comprises include an income measure, the size of the household, parental structure indicators (i.e., no mother, no father), parental employment, mother's age, and urban/rural status. Additional geographic explanatory variables include regional controls and an indicator variable taking the value of *one* if the state of residence was a non-charter state as of 2000.

Table 1 presents a more comprehensive set of student and household variables than are discussed here.[18] Fewer controls are included in the analysis because of two issues: one is collinearity

[18] In the final dataset, 17 states are represented; of these, two states were non-charter states as of 2000. Charter status data available online at: http://www.ed.gov/PDFDocs/4yrrpt.pdf.

across regressor and the other is that to include all of the independent variables would create a highly restrictive model for this sample size.

The district-level variables in **D** include a collection of mean values for the district — meant to represent qualities of a typical public school in the district. The set of district mean variables are meant to provide both private and public school students with a fictitious public-school option; these variables include the proportion of students who are free-lunch eligible, student-teacher ratio, proportions of students by minority race or ethnicity, and proportion of female students. Additional district variables included in **D** are the proportion of schools that are Title I eligible, grade-span of the district (for this I employ a dummy variable for elementary-only school districts), and an indicator variable for whether borders changed significantly in the last year, and the number of schools in the district.[19] Finally, per-student levels of total district expenditures and revenues (disaggregated across federal, state, and local levels) are among the district-level variables.

The private school access variable is the number of private schools in the county of the student's residence. Although other private school variables at the county level were constructed for summary statistics purposes (as presented in table 2), they are not included in the actual analysis.

All resident characteristic variables in **R** come from 2000 Census data and thus represent community attributes two years premature to the third-grade sample. The residential qualities controlled for at the school district level are demographic and socioeconomic. I control for the fraction of the district that is minority and its squared value — to account for the nonlinear relationship between private school enrollment and racial diversity. I also control for the median household income, the fraction of the population below the poverty threshold, the fraction of the adult population with less than a high school degree, and the fraction of the adult population with at least a traditional college degree. At the census tract level, I control for the number of students in

[19] The number of schools in a district includes only schools with strictly positive enrollment for the year.

grades K-12 who attend private or public school and reside in the same census tract as a student in the sample.

To quantify PSC options in a school district, I employ a number of alternative measures of PSC: a choice school indicator (or dummy) variable for a school district, the number of choice schools in a district, the proportion of choice schools in a district, and the fraction of district students enrolled in choice schools.

A. Base and Extended Models

Initial estimates are run on a base model that consists of a select set of key student, household, district, and resident characteristic variables. The student and household regressors for the base model are the student's race and a household income variable.[20] The district-level variables include the proportion of district students eligible for free lunch and total expenditures per child. The only school district resident characteristic controlled for in the base model is median income.[21] The base model also includes the private school access measure, regional controls, and an indicator variable that takes the value of *one* for states that had a non-charter status as of 2000. The base model estimates are included to present a complete model with estimates comparable with others in the literature. The reduced set of regressors, which allow for greater variation in the independent variables, is meant to provide an overview of what the data generally reveal about key household and district qualities regarding the household's school-choice decision; when additional variables are then added, a general context has been established for any changes that are observed in the coefficient estimates.[22]

The extended model includes the remaining student, household, district, and resident characteristic variables discussed earlier in this section, with some exceptions. The household

[20] The income variable in the base model is the median value of the household income category measured in thousands.
[21] The income measure for the school district is in 1999 dollars; it has not been CPI adjusted.
[22] The only coefficients reported beyond the base model are PSC and the private school access measure.

income variable in the extended model is an indicator variable for the household's income category rather than the category median as used in the base model. Additionally, total revenues per child, decomposed by federal, state, and local funding sources, are used in the extended model, and expenditures per child is omitted. The extended model is estimated with and without state effects.

B. Modeling Public School Choice

In my analysis, my motivating interest is to examine whether a school district's measure of PSC influences a household's decision to send their child to private school; as stated, I employ a series of specifications to accommodate the different measures of PSC in a district. This is done to exploit the unique effects of the various aspects of PSC. The number of choice schools (magnet and charter) in a district highlights the potential diversity across public sector alternatives. The fraction of students enrolled in PSC schools in a district essentially measures how dominant these types of schools are in the district relative to other public schools; the fraction of district enrollment attending choice schools essentially measures the same thing but with enrollment measures. These measures represent aggregate PSC measures that do not differentiate between charter or magnet school status. I subsequently consider compositional PSC measures that differentiate by school type.

To examine how a school district's measure of PSC affects a household's public-private school decision, I first estimate the model when the sole aggregate PSC control is a dummy variable taking the value of *one* if the district has a choice school (equation 2 below). Alternative specifications then incorporate an additional measure of PSC in tandem with the choice school dummy (equations 3 through 5).

(2) $\mathbf{PSC}_j = \mathbf{Choice\ Dummy}_j$

(3) $\mathbf{PSC}_j = \mathbf{Choice\ Dummy}_j + \mathbf{Number\ Choice}_j$

(4) $\mathbf{PSC}_j = \mathbf{Choice\ Dummy}_j + \mathbf{Fraction\ Choice}_j$

(5) $\mathbf{PSC}_j = \mathbf{Choice\ Dummy}_j + \mathbf{Choice\ Enrollment}_j$

In equation 2, when the only the PSC indicator variable is included, the coefficient captures the effect of simply the presence of a PSC school — regardless of how dominant the PSC schools are in the district. In equations 3 through 5, the additional PSC measures are meant to further control for the magnitude of the district's PSC. Thus, observed effects of the number or fraction of choice schools as well as the fraction of choice enrollment are all conditional on having at least one choice school in the district.

V. Results

A. *Aggregate PSC Measures*

Initial probit estimates do not reveal a strong relationship between aggregate measures of PSC and the household's decision to send their child to private school. Recall, these aggregate measures include a dummy variable indicating the presence of at least one choice school in the district, the number or fraction of choice schools in the district, and the fraction of students enrolled in choice schools. Table 3 presents the probit marginal effect estimates for these measures on the household decision from the base model, which, recall, includes only a basic set of explanatory variables and a private school access measure. In column 1, I report the results from estimating the model when the only aggregate PSC measure included is a dummy variable indicating the presence of a choice school in the district. The coefficient is insignificant. In column 2, the PSC measures include the choice school dummy and the number of choice schools in the district. Again, I find no evidence of a relationship between these aggregate PSC measures and the household's public-private school decision. In column 3, I replace the number of choice schools in the district with the fraction of choice schools in the district. In this case I find a positive and significant coefficient on the fraction of choice schools in the district; again, however, the coefficient on the dummy variable for the presence of a choice school in the district is not statistically related to the household decision.

Finally, in column 4, I include the choice school dummy and the fraction of district enrollment that attends a choice school as the aggregate PSC measures. Both coefficients are insignificant.

Table 3. Aggregate PSC Measures – Base Model, Probit Marginal Effects

	(1)	(2)	(3)	(4)
PSC Measures				
Choice School, Dummy	0.019	0.014	-0.017	0.002
	(1.41)	(0.98)	(0.90)	(0.11)
Choice Schools, Number		0.007		
		(0.62)		
Choice Schools, Fraction			0.158	
			(2.46)*	
Choice Enrollment, Fraction				0.070
				(1.22)
Private School Access				
Private Schools, Number	0.001	0.001	0.001	0.001
	(4.02)**	(3.17)**	(3.10)**	(3.29)**
Additional Model Controls				
African American (Student)	-0.092	-0.092	-0.093	-0.092
	(9.99)**	(10.03)**	(10.18)**	(10.05)**
Hispanic (Student)	-0.107	-0.108	-0.108	-0.108
	(10.57)**	(10.59)**	(10.75)**	(10.65)**
HH Income, Category Median ($K)	0.001	0.001	0.001	0.001
	(9.39)**	(9.39)**	(9.38)**	(9.38)**
Total Expenditures ($K)	-0.003	-0.003	-0.002	-0.003
	(0.87)	(0.77)	(0.59)	(0.77)
Free Lunch, Fraction (District)	0.186	0.180	0.169	0.178
	(4.17)**	(3.98)**	(3.76)**	(3.94)**
Household Income ($K), (District)	-0.001	-0.001	-0.001	-0.001
	(1.56)	(1.61)	(1.85)	(1.65)
Observations	3,770	3,770	3,770	3,770
Absolute value of z statistics in parentheses				
* significant at 5%; ** significant at 1%				

Dependent variable: student attends private school. Number of choice schools measured one-in-100 schools in the district; number of private schools measured one-in-10 schools in the county. Additional controls include "other race" for student, regional controls, non-charter state as of 2000 dummy, and missing data indicator variable. Weighted estimates.

Considering the base model, the estimates from the remaining coefficients generally support economic theory: minority and lower income households are less likely to attend private school, on average, and the proportion of district students eligible for free lunch is positively related to attending private school, ceteris paribus. The coefficients on total expenditures per child and the district's median household income are never significant.

I also estimate the base mode, as described above, with no controls for private school access. Because private schools do not operate under the geographical bounds that constrain public schools, we can argue that technically all students have a private school option. In this context, private school

access simply reduces the costs associated with attending a private school (i.e. time, travel, tuition, etc.). Under this relaxed specification, I find positive and significant coefficients on the number of choice schools in a district, the fraction of choice schools in a district, and the fraction of district enrollment attending choice schools when each of these PSC measures is controlled for in tandem with the choice school dummy. The coefficient on the choice school indicator variable is never significant. The estimates are presented in table A2 of the appendix. Much of this observed PSC effect is absorbed by the private school access measure when the model includes the number of private schools in a child's county of residence, stressing the importance of controlling for a relevant measure of private school accessibility. Thus, for the remainder of my analysis, I include this variable to control for private school access.

In table 4, I report the probit marginal effect estimates when I expand the base model to include the additional explanatory variables listed and described in the previous section. I estimate the extended model with and without state-specific controls. I do not report the estimates when the sole aggregate PSC measure is a choice school dummy variable for the school district; rather I report the estimates when the choice school dummy is included in tandem with another aggregate PSC measure. In effect, table 4 expands the last three columns of table 3.

The three panels of coefficients provided in table 4 present the progression of the marginal effect estimates of the aggregate PSC measures as the model is expanded to include the additional independent variables. The first panel (columns 1 through 3) presents the results when the aggregate PSC measures include the choice school dummy and the number of choice schools in the district; the three specific columns present the probit marginal effect estimates from the base model, extended model, and the extended model with state controls — columns 1, 2, and 3, respectively. The results generally reinforce the findings from the base model estimates; the coefficient on the number of schools in a district that are choice is never significant. Neither is the coefficient for the

Table 4. Aggregate PSC Measures, Base and Extended Model Specifications – Probit Marginal Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Aggregate PSC Measures									
Choice School, Dummy	0.014	0.014	-0.006	-0.017	-0.006	-0.028	0.002	0.011	-0.021
	(0.98)	(0.93)	(0.42)	(0.90)	(0.32)	(1.62)	(0.11)	(0.60)	(1.18)
Choice Schools, Number	0.007	-0.003	0.019						
	(0.62)	(0.25)	(1.27)						
Choice Schools, Fraction				0.158	0.087	0.174			
				(2.46)*	(1.41)	(2.41)*			
Choice Enrollment, Fraction							0.070	0.006	0.120
							(1.22)	(0.10)	(1.79)
Private School Access									
Private Schools, Number	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(3.17)**	(4.57)**	(4.38)**	(3.10)**	(4.46)**	(4.54)**	(3.29)**	(4.60)**	(4.51)**
Additional Model Controls									
Extended Student/Household	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Extended School District, Resident	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3,770	3,770	3,770	3,770	3,770	3,770	3,770	3,770	3,770

Absolute value of z statistics in parentheses
* significant at 5%; ** significant at 1%

Dependent variable: student attends private school. Number of choice schools is measured one-in-100 schools in the district; number of private schools is measured one-in-10 schools in the county. Additional controls for base and extended model are listed in section four. Estimates are weighted.

choice school dummy. The second panel of coefficients (columns 4 through 6) presents the results when the aggregate PSC measures include the choice school dummy and the fraction of choice school in the district. Again the three columns present the estimates from the base and extended models as well as the extended model with state controls — columns 4, 5, and 6, respectively. The coefficient on the fraction of choice schools in a district appears sensitive to the inclusion of additional explanatory variables. When including the extended set of student, household, school district, and resident variables (but not state controls), the coefficient is not significant. However, with the further inclusion of state controls, the coefficient on the fraction of choice schools is again significant, as in the case of the base model. The choice school dummy is persistently insignificant. Finally, the third panel of estimates presents the results when the aggregate PSC measures include the choice school dummy and the fraction of district enrollment attending choice schools; again the three columns present results from the base model, extended model, and extended model with state controls — columns 7, 8, and 9, respectively. Neither coefficient is ever significant.

I also estimate the base model, extended model, and extended model with state controls — as described above and depicted in table 4 — with no controls for private school access. The results are presented in table A3 of the appendix. Again, under this relaxed specification, I find slightly different results for the estimates of the PSC measures. When the PSC measures include the choice school dummy and the number of choice schools in a district, the coefficient on the number of choice schools is positive and significant under the base model and the extended model with state effects (columns 1 and 3, respectively). This finding is not observed when the model controls for private school access. In addition, when the private school access measure is excluded, I find the coefficient for the fraction of choice schools in a district to be positive and significant under the base model and the extended

model with state controls, conditional on the presence of at least one choice school in the district (columns 4 and 6, respectively). This finding reinforces the results of table 4, when a private school access measure is included.[23] Finally, I find that when the PSC measures include the choice school dummy and the fraction of district enrollment that attends a choice school, the latter is positive and significant under the base model and the extended model with state controls (columns 7 and 9, respectively) — a finding not present when private school measures are included.

B. Impacts of Magnet versus Charter Schools on Private School Attendance

To examine how the type of PSC affects the decision to enroll in a private school, I decompose the aggregate PSC variables into their charter and magnet components — the number of choice schools is replaced in the model with the number of magnet schools and the number of charter schools, etc. This approach changes some things in the underlying model. Up to this point, for a school district to have a valid PSC measure has required that both charter and magnet school data in the district be valid, so districts that had valid charter school data but were missing magnet school data were assigned missing values for aggregate PSC measures. The decomposition relaxes this constraint.[24] Thus, the estimates presented here are only comparable with the aggregate measure estimates presented above to the extent that missing data on choice schools is not an issue.

Table 5 presents the probit marginal effect estimates from the extended model with state controls, now with PSC measures decomposed. Generally, the findings are similar to those of the aggregate measures of PSC. In column 1, the only PSC measures included are

[23] For this set of PSC measures, under the extended model with state controls the choice school dummy is negative and significant when one does not control for private school access (column 6 of table A3) — an outcome not observed with the inclusion of the private school access measure.

[24] I control for PSC measures by charter and magnet school classification, and the model is run with the inclusion of indicator variables for missing PSC variables by school type. Recall, roughly 25 percent of the data on magnet schools reflect missing data for the roughly four states. In the current sample, however, all charter school data is reported to be valid.

Table 5. Compositional PSC Measures – Extended Model with State Controls, Probit Marginal Effects

	(1)	(2)	(3)	(4)
PSC Measures				
Magnet School, Dummy	0.010	0.005	-0.014	-0.010
	(0.61)	(0.33)	(0.70)	(0.50)
Charter School, Dummy	0.007	0.004	-0.028	0.011
	(0.36)	(0.22)	(1.26)	(0.48)
Magnet Schools, Number		0.012		
		(0.71)		
Charter Schools, Number		-0.108		
		(0.55)		
Magnet Schools, Fraction			0.138	
			(1.63)	
Charter School, Fraction			0.804	
			(2.13)*	
Magnet Enrollment, Fraction				0.102
				(1.28)
Charter Enrollment, Fraction				-0.268
				(0.55)
PSC Measures				
Private Schools, Number	0.001	0.001	0.001	0.001
	(4.74)**	(3.99)**	(4.64)**	(4.50)**
Observations	3,770	3,770	3,770	3,770

Absolute value of z statistics in parentheses
* significant at 5%; ** significant at 1%

Dependent variable: student attends private school. Number of choice schools is measured one-in-100 schools in the district; number of private schools is measured one-in-10 schools in the county. Explanatory variables include an extended set of student/household, additional school district variables and state controls. In column one, the number of schools in the district is omitted as an explanatory variable. Weighted estimates.

an indicator variable for the presence of at least one magnet school and an indicator variable for at least one charter school in the district. Neither coefficient is significant. In column 2, I present the results when the type-specific dummy variables are included as well as controls for the number of magnet schools in the district and the number of charter schools in the district. Again, no compositional PSC measure is significant. In column 3, I present the results when the type-specific dummies are included as well as the fraction of schools in the district that are magnet and the fraction of district schools that are charter. Here I find that the coefficient on the proportion of charter schools in the district is positive and significant; however, I find insufficient evidence to suggest that households respond to the proportion

of magnet schools in the district when making their public-private school decision. Finally, in column 4, I present the results when the PSC measures include the type-specific dummies as well as the fraction of district enrollment attending magnet schools and the fraction of district enrollment attending charter schools. Again, none of the coefficients is statistically different from zero.

I also estimate the extended model with state controls using PSC measures decomposed by school type — as described above and depicted in table 5 — with no controls for private school access. The results are presented in table A4 of the appendix. Under this relaxed specification, I again find slightly different results for the PSC measures estimates. When the PSC measures include the choice school dummies by type and the fraction of district schools by type (column 3), the coefficient on the fraction of district schools that are magnet is positive and significant, but the coefficient on the fraction of charter schools is insignificant. This is the reverse of the findings for these variables when the model includes a measure of private school access. Otherwise, the findings are generally consistent with those presented in table 5.

VI. Discussion

When considering potential aggregate PSC measure effects on the household's public-private school decision, I find slight evidence to suggest a positive relationship between the fraction of choice schools in a district and a household's choice of private school, conditional on the presence of a choice school in the district and conditional on a private school access measure. To provide a point of reference for the estimates of the PSC measure coefficients, I first discuss the estimates of other key variables from the base model. Consider the estimates presented in the third column of table 3: I find that being African American rather than white reduces the probability a household chooses private school by,

on average, 9.3 percent, ceteris paribus. This finding is consistent across the various aggregate PSC measure specifications in table 3. I also find that being Hispanic rather than white is associated with a reduction in the probability a household chooses private school by, on average, 10.8 percent, holding all else constant. An extra $1,000 in annual family income is associated with an average increase of 0.1 percent in the probability a household chooses private school, ceteris paribus. Additionally, a one percent increase in the percent of district students eligible for free lunch is associated with an average increase in the probability of attending private school of 0.169 percent, all else constant. Lastly, considering the private school measure, the presence of an additional ten private schools in the county is associated with an increase in the probability a household chooses private school of, on average, 0.1 percent, ceteris paribus. Thus, conditional on a basic set of student, household, and school district variables, the most influential factors on the household's public-private school decision appear to be race, arguably followed by the fraction of district students that are eligible for free lunch and a household's income. Given that private school enrollment is not bound by county lines, the magnitude of the marginal effect on the number of private schools in the county of residence is perhaps not surprising.

Within the context of the base model, looking at the aggregate PSC measure coefficients, the only measure that is statistically significant is the fraction of choice schools in the district. The coefficient of 0.158 is the marginal effect estimate for a one-unit increase in the fraction of schools in the district that are choice, implying that a one-unit increase in the *percent* of schools that are choice is associated with an on-average increase of 0.158 percent in the likelihood a household chooses private school. This magnitude is comparable with the effect of a one-unit increase in the percent of district students eligible for free lunch, and it is slightly greater than the average effect of an additional $1,000 in household

income, ceteris paribus. Under the extended model with state controls (refer to table 4), the coefficient on the fraction of choice schools in a district, conditional on the presence of a choice school in the district, is 0.174, just slightly larger than the estimate from the base model. The private school access measure under the extended model with state fixed effects is unchanged from the base model, though with a different unit of measurement (perhaps one-in-100 schools in the county), one might observe more subtle effects.

Considering the marginal effect estimates from the compositional PSC measures, which are presented in table 5, the fraction of charter schools in the school district is the only PCS measure with a statistically significant coefficient. Conditional on the presence of either type of choice school and the fraction of magnet schools in the district, a one-unit increase in the percent of schools in the district that are charter is associated with an average increase of 0.804 percent in the probability a household chooses private school. This effect is roughly eight times larger than the effect associated with the presence of an additional ten private schools in the county, ceteris paribus. Interestingly, Glomm et al. (2005) finds that charter school entry is positively related to the presence of private schools in an area. So the charter school results here might suggest endogeniety concerns; that is, private school enrollment would have been more likely in these school districts even without the charter school option because of the community qualities that attract private schools. The weighted correlation coefficient between the number of private schools in the county and the number of charter schools in the school district for this sample (recall, students are linked to counties and school districts by their census tracts) is 0.604; however the weighted correlation coefficient between the number of private schools in the county and the fraction of charter schools in the district, for this sample, is 0.299. Thus, there is a stronger correlation between the number of private and charter schools than there is between the number of private

schools and the fraction of charter schools.[25] Although I include residential controls at the school district level that have generally been found to be associated with private school (Barrow 2006) and charter school entry (Glomm et al. 2005), it may be necessary to look closer at these controls, expand their geographic specification, or consider an instrumental variable approach.

The coefficient on the fraction of district enrollment attending charter schools is negative but not statistically different from zero. Although my findings do not provide enough evidence to suggest a crowd-out effect from charter schools as seen in the literature (Toma et al. 2006; Roy & Chakrabarti 2007), the sign of the coefficient is consistent with a crowd-out effect.[26] None of the magnet school coefficients is significant when the model includes a measure for private school access, indicating no observable effect of magnet schools on the household's private-public school decision.

VII. Conclusion

In this paper, I examine how PSC affects the household's public-private school decision. I analyze whether a general effect may be observed that can be attributed to the availability of public school alternatives, and I further examine whether effects on the household decision are specific to the type of choice — either magnet or charter. When considering aggregate PSC effects, I find evidence to suggest a slight effect of the proportion of choice schools in a district — conditional on the presence of a choice school in the district and conditional on a private school access measure — whereas alternative PSC measures consistently yield insignificant coefficients. When controlling for the type of choice

[25] The corresponding correlation coefficients for magnet schools are as follows: the weighted correlation coefficient for the number of private and magnet schools is 0.493; the weighted correlation coefficient for the number of private schools and the fraction of magnet schools is 0.260.

[26] In additional model specifications not included here, the fraction of students attending charter schools is negative and significant conditional on the proportion of charter schools in the district.

school, again it is the fraction of schools in a district that is the sole significant PSC measure. Furthermore, the results suggest that households respond to the presence of charter schools in particular, rather than magnet schools, again, conditional on a private school access measure.

The finding that the fraction of choice schools in a district is positively related to the household's decision to choose private school is consistent with the argument that poorer performing public schools inspire entry of schools of choice, implying that households seek private schools because they perceive that their public school options are of lower quality. Additionally, the finding is also consistent with the argument that PSC allows students to be redistributed in the public sector, shuffling the demographic and socioeconomic mix of students. To the extent that this phenomenon contributes to the decision of households to choose private schools would propose a PSC sorting effect that influences the household's public-private school decision. The results presented here and the use of cross-sectional data without school tenure controls do not provide enough information to claim one argument over the other. These are just the most immediate potential arguments consistent with the findings.

One might presume that households would respond to the number of choice schools in a district when making their public-private school decision — the argument being that these schools are meant to be unique, so as they become more numerous so do the unique alternatives to the traditional public school. However, with the exception of estimating the model with no control for private school access, I find no evidence of this effect. It may be that such an effect is only observed conditional on other PSC measures, such as the fraction of district students enrolled or the fraction of schools that are choice. Based on my results, however, I do not observe that households consider the number of PSC schools when they

choose between public and private schools, conditional on having a choice school in the district and conditional on a private school access measure.

Alternatively, one might presume that households would respond to the fraction of district enrollment attending schools of choice. Indeed, the literature provides evidence of private school crowd-out effects from charter school enrollment (Toma et al. 2006; Roy & Chakrabarti 2007). Again, however, my results are mixed. Aggregate PSC measures that account for the fraction of students enrolled in choice schools are only significant when the model is run without private school access controls, and even then they are positive and significant (table A2 of the appendix). When considering the effects of enrollment in choice school by school type, charter school enrollment has a negative coefficient, consistent with a crowd-out effect, and magnet school enrollment has a positive coefficient. These findings persist whether the model is run with or without a private school access measure (table 5 and table A4 of the appendix). However, neither coefficient is statistically different from zero. Thus, based on my results, conditional on having a choice school in the district and conditional on a measure of private school accessibility, I do not find that households respond to the fraction of district students enrolled in choice schools when making their public-private school decision.

I do not control for a number of additional factors that may contribute to the appeal of PSC schools, such as their tenure and application processes. In addition, I do not control for variation across schools within a school district. These concerns, along with the small sample size and the missing data on magnet schools, lead me to caution the reader that the findings presented here are preliminary and should not be presumed estimates for a more general population of students at this point. A subsequent version of this paper will increase the sample size by matching students who reside in census tracts that fall on the border of

school districts and will include measures of variation in school qualities.

Table Appendix:

Table A1: Final Sample (3,770) Summary Statistics, School District Variables, and Means

	Private School		Private & Public	
	Mean	(SD)	Mean	(SD)
Private School (%)			12.5	(33.1)
PSC Measures				
Schools, Number	200.0	(287.1)	168.8	(276.4)
Choice Schools, Number	42.1	(87.5)	28.1	(72.1)
Choice Schools (%)	13.0	(16.2)	9.8	(14)
Choice Enrollment (%)	15.4	(19.1)	11.6	(16.5)
Magnet Schools, Number	37.6	(81.0)	25.0	(66.3)
Magnet Schools (%)	11.6	(15.3)	8.7	(13.3)
Magnet Enrollment (%)	14.7	(18.5)	11.0	(16.1)
Charter Schools, Number	5.2	(10.8)	4.1	(9.6)
Charter Schools (%)	1.8	(2.6)	1.5	(1.6)
Charter Enrollment (%)	0.9	(1.4)	0.8	(1.6)
School District Measures, Means				
Title I Eligible (%)	58.4	(23.3)	55.0	(26.4)
White (%)	37.4	(31.2)	42.0	(30.0)
Black (%)	26.3	(21.1)	23.2	(20.1)
Hispanic (%)	28.7	(25.6)	28.1	(26.5)
Other Race (%)	7.4	(9.8)	6.5	(7.7)
Female (%)	48.8	(0.6)	48.8	(0.6)
Free Lunch Eligible (%)	45.4	(20.4)	42.2	(20.2)
Total Expenditures ($K)	9.0	(2.1)	8.9	(2.2)
Total Revenues ($K)	8.6	(1.6)	8.5	(1.8)
Federal Revenues ($K)	0.8	(0.3)	0.7	(0.3)
Student-Teacher Ratio	17.5	(2.6)	17.5	(2.7)
School District Resident Attributes				
Median Income ($K)	40.66	(9.24)	42.03	(11.05)
Households Below Poverty (%)	13.85	(5.32)	13.40	(5.49)
Minority (%)	45.37	(22.95)	44.98	(23.17)
College Educated (%)	30.73	(6.59)	30.35	(6.95)
Less than High School (%)	24.33	(8.20)	24.12	(9.84)

Private School is the percent of the final sample attending private school. PSC Measures and School District Measures and Means come from 2000-2001 CCD data. School District Resident Attributes comes from the 2000 Census Cartographic Files – school district level. All statistics are weighted.

Table A2. Aggregate PSC Measures Without Private School Measures – Base Model, Probit Marginal Effects

	(1)	(2)	(3)	(4)
PSC Measures				
Choice School, Dummy	0.024	0.007	-0.026	-0.011
	(1.76)	(0.50)	(1.46)	(0.60)
Choice Schools, Number		0.024		
		(2.57)*		
Choice Schools, Fraction			0.219	
			(3.56)**	
Choice Enrollment, Fraction				0.142
				(2.61)**
Additional Model Controls				
African American (Student)	-0.091	-0.092	-0.092	-0.092
	(9.68)**	(9.92)**	(10.03)**	(9.87)**
Hispanic (Student)	-0.105	-0.108	-0.107	-0.107
	(10.21)**	(10.54)**	(10.56)**	(10.47)**
HH Income, Category Median ($K)	0.001	0.001	0.001	0.001
	(9.69)**	(9.59)**	(9.60)**	(9.61)**
Total Expenditures ($K)	-0.004	-0.002	-0.002	-0.003
	(1.02)	(0.66)	(0.61)	(0.80)
Free Lunch, Fraction (District)	0.235	0.200	0.199	0.207
	(5.46)**	(4.45)**	(4.53)**	(4.69)**
Household Income ($K), (District)	-0.001	-0.001	-0.001	-0.001
	(0.91)	(1.31)	(1.49)	(1.25)
Observations	3,770	3,770	3,770	3,770
Absolute value of z statistics in parentheses				
* significant at 5%; ** significant at 1%				

Dependent variable: student attends private school. Number of choice schools measured one-in-100 schools in the district. Additional controls include "other race" for student, regional controls, non-charter state as of 2000 dummy, and missing data indicator variable. Weighted estimates.

Table A3. Aggregate PSC Measures Without Private School Measures, Base, and Extended Model Specifications – Probit Marginal Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Aggregate PSC Measures									
Choice School, Dummy	0.007	0.005	-0.012	-0.026	-0.015	-0.034	-0.011	0.001	-0.028
	(0.50)	(0.36)	(0.85)	(1.46)	(0.83)	(1.97)*	(0.60)	(0.04)	(1.61)
Choice Schools, Number	0.024	0.010	0.033						
	(2.57)*	(0.91)	(2.10)*						
Choice Schools, Fraction				0.219	0.122	0.197			
				(3.56)**	(1.96)	(2.70)**			
Choice Enrollment, Fraction							0.142	0.045	0.149
							(2.61)**	(0.78)	(2.23)*
Additional Model Controls									
Extended Student/Household	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Extended School District, Resident	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3,770	3,770	3,770	3,770	3,770	3,770	3,770	3,770	3,770

Absolute value of z statistics in parentheses
* significant at 5%; ** significant at 1%

Dependent variable: student attends private school. Number of choice schools is measured one-in-100 schools in the district. Additional controls for base model are listed in section four. Estimates are weighted.

Table A4. Compositional PSC Measures Without Private School Measures – Extended Model with State Controls, Probit Marginal Effects

	(1)	(2)	(3)	(4)
PSC Measures				
Magnet School, Dummy	0.005	-0.003	-0.027	-0.024
	(0.35)	(0.19)	(1.42)	(1.23)
Charter School, Dummy	0.006	0.001	-0.026	0.002
	(0.33)	(0.05)	(1.16)	(0.10)
Magnet Schools, Number		0.028		
		(1.65)		
Charter Schools, Number		-0.235		
		(1.19)		
Magnet Schools, Fraction			0.190	
			(2.28)*	
Charter School, Fraction			0.692	
			(1.80)	
Magnet Enrollment, Fraction				0.152
				(1.95)
Charter Enrollment, Fraction				-0.044
				(0.09)
Observations	3,770	3,770	3,770	3,770
Absolute value of z statistics in parentheses				
* significant at 5%; ** significant at 1%				

Dependent variable: student attends private school. Number of choice schools is measured one-in-100 schools in the district. Explanatory variables include an extended set of student/household, additional school district variables and state controls. In column one, the number of schools in the district is omitted as an explanatory variable. Weighted estimates.

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