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ABSTRACT OF DISSERTATION

Kylie M. Goggins

The Graduate School
University of Kentucky
2010

PUBLIC SCHOOL CHOICE AND THE PUBLIC-PRIVATE SCHOOL DECISION

ABSTRACT OF DISSERTATION

A dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy in the
College of Business and Economics
at the University of Kentucky

By

Kylie M. Goggins

Lexington, Kentucky

Director: Dr. William Hoyt, Professor of Economics

Lexington, Kentucky

2010

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ABSTRACT OF DISSERTATION

PUBLIC SCHOOL CHOICE AND THE PUBLIC-PRIVATE SCHOOL DECISION

This dissertation is a compilation of three studies related to public school choice issues. Chapter 2 examines whether access to public schools of choice influences a household's decision to choose private school for their child. I employ a multistate, individual-level data-set 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 (PSC) in a school district influence the household's public-private school decision. I find slight evidence that households respond to general measures of choice, though the implied effects appear to be trivial. Conditional on the presence of either PSC type of school in a district, I find more consistently significant crowd-out effects for competition measures from magnet schools, while charter school measures elicit stronger private-sector crowd-out effects, roughly three times those of the respective magnet school measures.

Chapter 3 examines the statewide educational policies and student, household, and school district-level attributes that influence the demand for interdistrict and intra-district public schools of choice. In the context of a multinomial probit model, I also estimate the demand for private school as a third alternative to attending an assigned school. I find evidence to suggest that households substitute between intra-district and interdistrict schools of choice.. I also find that mobility patterns may significantly increase the probability a household opts out of district.

Chapter 4 is an exploratory analysis that examines the qualities that distinguish school districts as net-losers, net-keepers, or net- gainers of students in their public schools. In particular, I examine how public schools of choice affect the net flow of students across the public sector. I find that charter schools appear to locate in districts that are net-losers of students, where students are opting into private school. I also find evidence to suggest that net-loser districts may signal better quality school districts with more diverse options available to facilitate positive student-school matches.

KEYWORDS: School choice, charter, magnet, open enrollment, private school

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CHAPTER 1: Introduction to Dissertation

Public and private sector school choice has all but captivated Americans over the past 40 years. School choice has a history of promoting equality of academic opportunity – appealing to the American dream of overcoming adversity, beating the odds, and the hope that given the opportunity, today's disadvantaged youth can rewrite their future prospects – perhaps becoming tomorrow's political leaders, great scholars, and successful innovators. Just ask any parent – given the right education for their child, the sky is the limit.

Dating back to the 1960s, school choice has generally been considered a valuable tool for dealing with issues of inequality of educational opportunity. Magnet schools in the south sought to break racial barriers and extend the same quality of educational resources to children regardless of their color. But today's school choice is less and less about racial segregation – while, to be fair, the issue still plays a role. While quality of education may still be the rallying cry of school choice proponents, today's issues regarding school choice are far more complicated than any one concern. Questions arise such as – does school quality in the private sector come at the expense of the public sector? Do policies and institutions meant for equality lead to greater inequality? And the intimate connection between property values and school enrollment continues to dwell on the minds of policy makers.

School choice has a far reaching grasp – over the years it has expanded in options, scope, and complexity. While private school is the classic alternative for households dissatisfied with the public school services available to them, open enrollment policies and magnet schools and programs have also long been avenues for expanding choice – and these alternatives reflect public school options. More recently, voucher programs, charter schools, and the No Child Left Behind Act of 2001 have further expanded the consumer's set of options across and within educational sectors. Indeed the onslaught of private, public, and hybrid schooling alternatives to the classical neighborhood school bring to light the demand for diversity and quality in primary and secondary education. Numerous federal, state, and local policies alike serve to further dissolve the financial and/or institutional barriers to choice across educational providers.

In this dissertation, I explore aspects of public sector school choice. In particular, I examine how households respond to additional schooling options in the public sector in deciding where to enroll their children. The displacement of students outside their

neighborhood schools into private school and public schools of choice affects student composition in the classroom, expenditures and revenues per student, can potentially alter the demand for services and programming across schools and districts, and can theoretically lead to competition for students across schools and districts. Thus with a variety of relevant consequences to explore, I focus my analyses on why and how households appear to prefer one school type over another.

It is perhaps not surprising that private schools and public schools of choice are generally found in the literature to affect sorting, though whether school choice effectively leads to more or less stratification by race, socioeconomic status, and student ability remains a hotly debated topic in the literature. Increased stratification is a logical outcome if one assumes that heterogeneity across household preferences is the catalyst that generates the demand for these schools in the first place. However, many public schools of choice adhere to diversification standards. There are studies that examine the racial and ethnic stratification between public and private schools, such as Clotfelter (1999), supporting the persistence of "white flight" – as white students continue to opt out of racially diverse urban public school districts. And studies which address the stratification effects of PSC typically look at which students from the public sector filter into specific schools of choice – such as charters or magnets. These latter studies often note drops in district enrollments for white students, as they leave the public sector for private school or choose other school districts. A number of studies address the issue of cream skimming effects of private schools – and, to some extent, public schools of choice (Epple and Romano, 2008; Figlio and Stone, 2001; Ladd, 2003). Also, there is some overlap in similarities among the community qualities that generate demand for private schools and public schools of choice (Barrow, 2006; Glomm et al., 2005).

As I look at the effect of PSC on school enrollment decisions in the following chapters, relevance for my findings will rest upon what research tells us about the effects of mobility and stratification on school children. What does it matter how PSC affects a household's enrollment decision for their child? To answer this, I refer to a rich and diverse body of existing literature which both lays a foundation for my work and provides context and inference for any findings. The effects of stratified schools, the influence of one's peers, and the consequences of mobility are all interwoven and directly relate to how PSC affects both the public-private school decision as well as the choice of a particular school or school

type in the public sector. If we consider that, ceteris paribus, students in racially diverse schools generally perform better than those in more racially isolated schools, (Summers and Wolfe, 1977), and schools that are predominantly minority students often lack quality resources, then a finding that PSC draws white students (back) into the public sector could have a positive implication. Likewise, if moderate mobility across schools generally leads to academic gains (Swanson and Schneider, 1999), than a finding that PSC encourages transfers between academic sectors may indicate improved matching of students and schools. And, finally, if the loss of academically superior peers to private school most adversely affects lower achieving classmates (Summers and Wolfe, 1977; Hoxby 2000), and classes with a range of student ability levels benefits all students (Toma and Zimmer, 2000), then the extent to which PSC may increase the diversity of student populations within the public or private sector may reveal social and personal academic benefits for students.

In addition to affecting the distribution of students across public and private school sectors, PSC programs are likely to affect the distribution of students among public schools; Wamba and Asher (2003), Renzulli and Evans (2005), Bifulco and Ladd (2006) find evidence of private sector stratification due to the presence of PSC schools. Where policy is dealt a challenging hand is the tradeoff between letting households pick the best-matched school for their students and where government intervenes on behalf of somewhat disenfranchised students whose higher achieving peers exit. And beyond sorting implications, as schools face mounting competition for their students, they have an incentive to take more active roles in attracting students (or households) and retaining students in their schools or even districts. And policy, as it relates to school administrators and educators, may gain from observing how and why households choose schools other than those that are residentially assigned.

This dissertation is a compilation of studies that explore school choice decisions at the household level. For the bulk of the analysis, I employ a rich, multistate, individual-level dataset on third-grade students and their families. For my analyses 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 how households respond to their supply of public schools of choice as they decide where to enroll their children.

The first of these studies is presented in Chapter 2. Here I examine whether households treat public schools of choice (PSC) – charter and magnet schools particularly –

as reduced-priced substitutes for private schooling. I find evidence to suggest that charter and magnet schools compete with private schools for students; however the effects appear trivial. In particular, I find that, while still negligible, the private sector crowd-out effect of charter schools appears to be roughly three times that of magnet schools. This analysis adds to the literature by examining the effect of charter school competition on private school enrollment using student-level data over a multi-state area; it is also among the first to consider magnet schools as potential private school rivals.

In Chapter 3, I present an analysis that estimates the demand for intra-district public schools of choice, interdistrict public schools, and private schools over residentially assigned public schools. In particular, I examine how a handful of key education policy and student and household level measures influence this household decision. I find evidence to suggest that households treat intra-district and interdistrict school choice as substitutes when states have mandated cross-district open enrollment policies. I also find that households that have recently relocated are more likely to opt out of district rather than attend their residentially assigned school. This analysis adds to the literature by being among the first to pose both interdistrict and intra-district options to households choosing a school. It is also unprecedented in its collective look at intra-district (and interdistrict) choice rather than focusing on one particular policy or type of institution that represents a subset of intra-district (or interdistrict) choice.

In the fourth chapter, I pursue an exploratory analysis examining the attributes of school districts that are observed to, on net, lose, keep, or attract public school students. I employ a unique dataset that allows me to observe students' residentially assigned districts and further matches public school students to the districts they attend and identifies private school students. I estimate how measures of PSC and statewide education policies influence the net flow of students within the public and private school systems. While the findings of Chapter 4's analysis are generally descriptive, I observe that charter schools tend to locate in districts that are losing students and that net-losing school districts – compared with districts that keep or gain students – may very well signal better quality school districts with more diverse options available to facilitate positive student-school matches. This analysis contributes to the literature by examining how PSC affects the retention rate of public school districts.

In the fifth and final chapter of this dissertation, I conclude with my overall findings from the analyses of the preceding chapters. I also discuss how future work will build on the three studies.

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1. CHAPTER 2: Public School Choice: Charter and Magnet Schools and the Public-Private School Decision

1.1 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 open 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, to what extent does PSC factor into that decision? Does "choice" itself in the public sector affect the public-private school decision? Do households view these schools to be reduced-price substitutes for a private school education? Furthermore, does the PSC effect depend on the type of school choice available?

Two main forms of PSC are considered in this chapter: 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 in which they operate. Both types of schools relax the residential

[1] In addition to magnet and charter schools, open enrollment policies also play an important role in PSC. While open enrollment implications are not a main part of this analysis, I do discuss the effects of incorporating open enrollment measures in the discussion section.

constraint on households, although the extent to which 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 concentrations of discipline. 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. Figure 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 made strides 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 1.2 million students.[4] For context, in 2008, approximately 6.1 million primary and secondary students were projected to be enrolled in U.S. private schools.[5]

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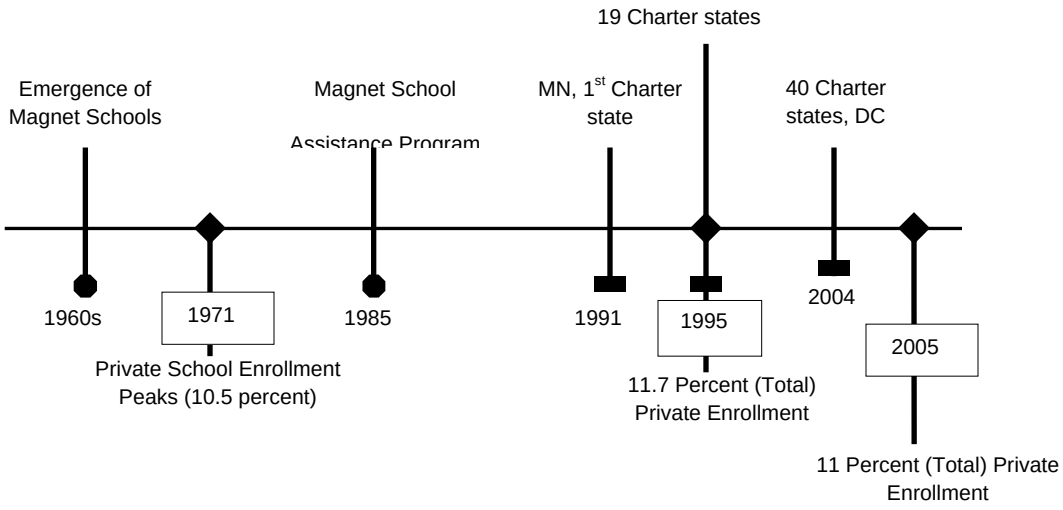
[2] Some magnet schools reserve a number of placement spots for students residing in the immediate or traditional catchment area. Charter schools often allow cross-district enrollment. Although application procedures and a school's residential requirements would likely affect the degree to which households 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.

[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.

Figure 1.1: 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; that is, do households treat charter and magnet schools as substitutes for one another or do the different schools elicit unique responses? I find that conditional on private school supply measures, some evidence suggests that households respond to general measures of PSC in their district. Conditional on school type, I find evidence to suggest both a magnet school effect and a charter school effect that is consistent with private sector crowd out, though the magnitude of the charter school effect is notably stronger.

In particular, I find that some general and type-specific measures of PSC competition in a district tend to be negatively related to a household's choice of private school. The discrepancy in magnitude of effects suggests that charter and magnet schools are not always

treated as substitutes for one another. I also find the presence of a PSC school in a district to be positively related to a household's choosing private school. This measure, which may proxy for school quality or is otherwise correlated with private school entry, also appears to provide more information on PSC demand than supply in this framework. Without better controls for school quality or an instrument for the presence of PSC schools in a district, the effect of the PSC dummy is assumed to be an endogeniety issue that is as of yet unresolved, although the magnitude of the estimated effect may serve to trivialize any endogeniety concerns with respect to this issue.

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 chapter 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.

1.2 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 and Ladd, 2006, 2007; Booker et al., 2008; Cobb and Glass, 1999; Hoxby, 2004; Renzulli and Evans, 2005; Sass 2006; Wamba and Ascher, 2003). This research agenda has been similar across magnet school studies (Archbald 1988; Clewell and Joy 1990; Gamoran 1996; Henig 1995; Saporito 2003; Steel and 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 private sector crowd-out effect of charter schools in Michigan and argue that they 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 school 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 research most closely follows Roy and Chakrabarti's work with some extensions.[6] First, my analysis is multistate rather than state specific. Although charter schools can vary significantly by state, I include state fixed effects. 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 and Toma 1988). 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 and Chakrabarti 2007; Toma et al. 2006), and I examine general affects of PSC, which do not distinguish between charter and magnet schools.[7] I further build on Roy

[6] My research is also closely related to Butler et al. (2009) who also use the restricted geo-code ECLS-K sample to examine how distance influences where a household chooses to enroll their child.

[7] A number of studies have looked at the types of students who attend magnet schools (Archbald 1988; Clewell and Joy 1990; Dills 2004) and the types of students who attend charter schools (Bifulco and Ladd 2007; Hanushek et al. 2007; Renzulli and Evans 2005; Zimmer and Buddin 2006). Evidence suggests that

and Chakrabarti's work by including an alternative measure of choice school competition — the fraction of schools that are choice in a district.

1.3 Economic Theory

In the absence of a public school choice (PSC) option, households choose between either their residentially assigned public school or a private school alternative for which they must pay. I assume that the school quality of private schools is superior to that of nearby public schools. Thus public and private schools are distinguished by quality, tuition and, for some, religious affiliation. If private schools have a religious affiliation, then even if some public schools have educational quality equal or above the private alternatives, some households might still prefer paying a substantial tuition to send their children to the private, religious alternative. Thus the tradeoff of households considering private school rather than their residentially assigned school is whether the improved school quality and/or the religious component of the school are worth the price of tuition. Assume two types of households opt into private school: households that have a strong preference for the religious component of private schools, and household that have a strong preference for the improved school quality that private schools offer.

Now consider the entry of magnet or charter schools into a school district. PSC schools of higher quality than the average public school would certainly prompt students in assigned public schools to transfer into them. But how do the magnet or charter schools compete with the private schools for students? The degree of private sector crowd out depends on the quality of these schools and the type of attachment households have for private sector schools. We first consider the households that prefer private schools for their superior school quality. To induce these households to enroll their students in PSC schools, the school quality of the PSC schools must be superior to the residentially assigned public school, but does not necessarily need to be as high quality as the private school. Now consider households that prefer private school for their religious component. In order for PSC schools to compete with private schools for these students, the level of school quality at

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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.

the PSC schools would have to be very high – perhaps greater than that of the private school.

A priori, the expectation of the effect of PSC schools in a district with heterogeneous households is that these choice schools should reduce the probability of households entering into private school by increasing the household's expected utility of entering the public sector. Additionally, competition from PSC schools may raise the quality of schooling in the district's assigned public schools – again positively affecting the household's expected utility from entering the public sector. As stated, the PSC schools' quality and the religious preference of the households are expected to affect private sector crowd out differentially. In particular, I expect to find a smaller private sector crowd out effect among households that have a religious attachment to the private sector, and I expect to find a stronger private sector crowd out effect among households that are prone to seek out the private sector for superior school quality. Additionally, I expect to find private sector crowd out to be positively related to the level of PSC school competition in a district.

1.4 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.[8] 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

[8]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.

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 schools can be matched with the NCES's Private School Universe Survey (PSS); 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 that includes data on grade range and student demographics. I also supplement with data from the Census 2000 Summary 3 Files as well as the Census 2000 School District Tabulation (STP2), which were procured from the NCES's School District Demographics System; these data-sets provide information about demographics and socioeconomic statuses of residents at the census tract level and school district level, respectively. Finally, I supplement the dataset with 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.

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 factors that define and contribute to the supply of PSC. 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.

1.4.1 *Data Sources*

1.4.1.1 ECLS-K

As mentioned above, the ECLS-K survey is administered about 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. I 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 ensures the largest set of alternatives 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.[9] By retaining only third-grade students with valid school and census tract identifiers, I reduce the sample size to 11,130 observations.[10] 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., presence or absence of mothers and fathers), parental employment and educational attainment, mother's age, and urban/rural status.[11] Additional geographic explanatory variables include regional controls and an indicator variable taking the value of *one* if the state of residence was a charter state as of 2000.[12]

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[9] Attrition and the constraints of following student participants that move effectively reduce the sample size of the third-grade survey from the initial student participants, numbering approximately 20,000.

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

[11] The household income measure in the ECLS-K survey assigns an income category appropriate for the household's income level. There are nearly 15 categories, not all of which have equal ranges. I employ two alternative income measures: (1) the median income of the category (2) the income categories themselves.

[12] Regional controls are consistent with the 2000 Census. I assign states a charter classification as of 2000 according to Nelson et al. (2000) and www.chartschools.org. Charter state classification is lagged one year because new charter states are unlikely to be comparable with charter states that have already had at least a year to develop charter schools.

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. All estimates and summary statistics presented here are weighted.

1.4.1.2 2000 Decennial Census Data

It is important to note that the 2000 Decennial Census data correspond with the 1999-2000 academic year, the first-grade year of this cohort. Although these measures represent data two years premature to the third-grade year of the sample, they are unlikely to have changed substantially by the time the students are in third grade. The variables I create using the census data perform three functions: they assign students to a public school district; they account for the number of students enrolled in grades K-12 who live in the student's residential census tract, and they control for resident heterogeneity at the school district level.

To match students with public school districts, I employ 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 mapping specifications. When a census tract falls geographically within a given school district, the census tract is assigned, or matched with, that school district.[13] For these matches, the entire census tract falls within the school district or borders the district but does not cross a school district border. The final sample consists of 7,630 ECLS-K student observations for which I successfully match census tracts with school districts based on this mapping exercise.[14] As this sample size reflects approximately 68

[13] In the final sample, no school district reported a significant change in its border since the last school year in either the 2001-2002 CCD district survey or the 2000-2001 CCD district survey. Thus the districts assigned residentially using Census 2000 data should be unchanged when extrapolating data from the 2001-2002 school year for the same district.

[14] Though I was able to successfully match approximately 70 percent of the remaining sample to a residentially-based school district, about 70 observations were subsequently dropped because of anomalies across their explanatory variables that prevented successful estimation of the model.

percent of the third grade sample with valid census tract identifiers, I address concerns regarding potential bias due to sample selection in a coming sub-section where I discuss the final sample.[15]

The Census 2000 School District Tabulation (STP2) files, from the NCES's School District Demographics System, provide demographic and socioeconomic resident profiles at the school district level. Once students are matched with a school district, I further supplement the GIS data by including resident characteristics at the school district level to control for various community attributes shown to be associated with the entry of private schools (Barrow 2006) and public schools of choice (Glomm et al. 2005).

1.4.1.3 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. While concern exists for endogeniety by using current year variables, the variables are highly correlated with the one-year lag. Additionally, I employ the 2000-2001 CCD to create the PSC variables.

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), and the fraction of Title I eligible schools in the district. 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 2001-2002 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

[15] Ninety-five percent of the public school students report attending public school in the school district assigned to them by the GIS matching process.

minority race or ethnicity, and the proportion of female students. I also interact student-teacher ratio with the grade-span dummy variable.

As stated above, the school-level 2000-2001 CCD data is the source of the public school choice variables. The PSC variables are also limited to schools that offer third grade. Thus the PSC variables represent a one-year lag of school district variables and are only representative of elementary-inclusive schools. The one-year lag assumes households make the public-private decision cognizant of the PSC available in their school districts the previous year and attempts to address the concern that more-tenured PSC schools tend to have different effects on student enrollment than newer schools that have yet to prove themselves to parents (Booker et al. 2007). By using only elementary-inclusive schools in the PSC measures, I avoid including irrelevant PSC schools – such as a magnet middle school or charter high school – to the sample of third graders.

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 general variable that does not distinguish between charter or magnet but merely indicates whether a school is a choice school.[16] Some schools identify themselves as being both magnet and charter, 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 few schools, 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 competition measures: number of schools in a district, percent of schools in a district, and percent of district enrollment. This process yields twelve PSC measures. I also generate four dummy variables that indicate the presence of at least one PSC school in a district – be it magnet, charter, choice, or both.

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 assignments are further supplemented using 2000-2001 CCD, the 2001-2002 CCD data, and 2001-2002 Census of Governments form F-33. The remaining piece is to measure private school supply.

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

1.4.1.4 Private School Universe Survey

I use the PSS data from the 2001-2002 academic year to create a measure of private school supply. 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 supply 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. And to account for potentially religion-inspired attachment to the private sector, I also control for the proportion of Catholic private schools in the county.

1.4.2 *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 2000-2001 CCD survey, magnet status is missing.[17] This generally tends to be a state-specific anomaly; only students who reside in the states where this is an issue are affected. 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.).[18]

Additionally, many specific qualities of charter and magnet schools, such as application procedures and more explicit measures of PSC school tenure, are not always (if ever) observable in the data.[19] Although such attributes potentially influence the household's public-private school decision, such specific qualities are not included in this analysis.

[17] All charter school identification data in the final sample is reported as valid from the CCD (2000-2001) school-level data.

[18] As a programming note, all missing data is set to zero and missing data dummy variables are included in any analysis to denote observations initially had missing data.

[19] The school level CCD survey started collecting magnet and charter status only in the 1998-1999 academic year. I do not attempt to go back to this year to collect more complete tenure measures, but a researcher could conceivably do so.

1.4.3 *The Sample:*

The final sample that I use in my analysis consists of 7,630 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, a strong contributor to the reduction in sample size comes from the spatial matching of census tract to school district.

Though the final matched sample represents roughly 68 percent of the 11,130 observations with valid census tracts, the composition, or profile, of the sample is greatly unchanged by selecting only households that reside in census tracts that do not cross school district borders. Table 2-1 summarizes student and household qualities before and after the GIS matching. Because missing categories for variables are created for the analysis, I do not omit student observations with incomplete information. Thus, the summary statistics 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 student profiles for the matched sample of 7,630 students and the unmatched sample of 11,130 third-grade students with valid census tract and school identifiers, presented in the top portion of Table 2-1, reveals fairly consistent student depictions across the two samples. Variables denoted with an asterisk represent statistically different means across the sample sizes. Looking at these attributes one sees that in the matched sample 13 percent of the students are enrolled in private school, while 12 percent of students in the unmatched third grade sample are enrolled in private school. Though a statistically significant slightly higher concentration of private school enrollment is found in the final, matched sample, the proportions are comparable across samples and are similar to the national average of 12 percent private school enrollment for prekindergarten through eighth-grade students in 2001.[20] In the final sample, the typical student is just as likely to be male as female, is nine-years-old, and is white. Making up 55 percent of the final sample, white students are slightly less concentrated in the final sample than in the unmatched sample where they constitute 58 percent, and African American students are slightly more

[20] National private school enrollment estimates come from NCES and can be found online at http://nces.ed.gov/fastfacts/display.asp?id=65

concentrated in the final sample than in the larger third-grade sample, at 18 and 15 percent respectively. Hispanics and students who would be classified as *other* race are represented fairly consistently across both sample sizes. Eleven percent of students in the final matched sample have individualized education plans (IEPs), 27 percent have a diagnosed disability on record at their school, and 21 percent of the students have changed school since their last survey, which was administered in the spring of this cohort's first-grade year. These percentages are not statistically different from those of the unmatched sample.

Table 2-1 also contrasts the household attributes of the final, matched sample with those of the larger third-grade sample. As with the student's attributes, the household profile is essentially unchanged by the matching process. Across both samples, the typical household size is four people. Seventy percent of students in the final sample live in a two-parent home, a statistically significant difference from the slightly higher 72 percent of the unmatched sample residing in two-parent homes. And students in the final sample are marginally more likely to live in homes where no father is present compared with students in the unmatched sample, at 27 and 26 percent, respectively. The mean values for the remaining parental qualities are not statistically different across samples. Fathers are employed at a rate of 93 percent; whereas mothers are employed at a rate of 73 percent. Together, they contribute to an average household income that falls in an income category with a median value of $63,000. The average father is 40-years-old and has at least a high school education; 32 percent of fathers have a four-year college education or an advanced degree. The average mother is 37-years-old and has at least a high school education; 27 percent of mothers have a four-year college education or more.

A slightly higher concentration of households is below the poverty line in the final sample, at 22 percent, compared with the unmatched sample, at 21 percent. And the regional and urban specifications represent perhaps the most notable distinctions across samples. At 40 percent, most of the households in the matched sample live in the South; the same holds for the larger third-grade sample, where 35 percent live in the South. The fraction of students living in the South across samples is statistically different. Eighteen percent of the matched sample resides in the Midwest, whereas 23 percent of the larger third-grade sample resides in the Midwest. Although percentages of students residing in the Northeast or West are also statistically different across samples, their values appear to be very close. Perhaps the strongest difference between the two samples sizes is the proportion residing in an urban

area: 43 percent of the final, matched sample report residing in urban areas, while 37 percent of the larger third grade sample report residing in urban areas. Because urban areas, as opposed to rural or suburban areas, generally have more access to both public schools of choice and private schools, a slightly higher concentration of this geographic specification works in favor of this analysis.

Finally it is important to present any notable differences in the supply (and demand) of private schooling options across the matched and unmatched samples; Table 2-2 presents this comparison. The fraction of students enrolled in private school (grades K-12) residing in the same census tract as a student in the sample is slightly higher for the matched sample, 12.22 percent versus 11.74 percent for the unmatched sample; this difference in means is statistically significant.[21] The remaining private school access variables are measured at the county level and represent the 2001-2002 school year. Though the average number of private schools in a student's county of residence is not statistically different across samples, evidence shows that students in the matched sample tend to live in counties with slightly larger private schools and, on average, are slightly more exposed to Catholic private schools in their county of residence than students in the larger third-grade sample.

Table 2-3 reports the weighted summary statistics for school district variables and means — including PSC measures — for the final, matched sample only. Also presented are weighted summary statistics for qualities of the school districts' residents.[22] I report mean weighted summary statistics for all students enrolled in private school, approximately 13 percent of the 7,630 students – representing 310 school districts. I also present summary statistics for all public school students in the final sample – representing 540 school districts.[23] 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; again, variables with asterisks denote that difference in the means across the two samples is statistically not equal to zero.

[21] 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.

[22] Residential data at the school district comes from the 2000 Census and thus represents the 1999-2000 academic year.

[23] In the final sample, 200 districts are mutually resided in by at least one public school student and at least one private school student.

I find that, on average, students attending private school live in larger school districts with more access to PSC schools, particularly magnet schools, compared with those students attending public school. Thirty-four percent of the private school students in the final sample live in a school district with at least one PSC school, versus 29 percent of public school students. [24] Conditional on having at least one PSC school in a residentially assigned school district, the average number of choice schools per district for students attending private school is 41, as opposed to an average of 32 PSC schools in the districts where public school students live. The fraction of PSC schools in the district is also marginally higher where private school students live than in the districts where public school students live, at 18 and 17 percent, respectively. Interestingly, the fraction of district enrollment attending choice schools, conditional on having a choice school in the district, is not statistically different across the two samples.[25] Though magnet schools tend to be more common in districts resided in by private school students than in districts where public school students live, with 27 and 21 percent of the samples having at least one magnet school respectively, no evidence indicates that magnet schools are more numerous or that they make up a larger share of the districts' schools or enrollment when compared with the districts of public school students – conditional on having at least one magnet school in the district. Interestingly, charter schools are also more common among the sample of private school students, with 23 percent of private school students residing in a district with at least one charter school and 19 percent of public school students residing an districts with at least one charter school. But both the fraction of charter schools in the district and the fraction of district enrollment attending charter schools, conditional on the district having a charter school, are marginally higher in the school districts of the public school students. Conditional on having a charter school in a district, the number of charter schools per district is just shy of eight schools across both samples.

Aside from the discrepancies across the PSC measures, Table 2-3 further depicts that the districts of private school students have, on average, a slightly higher proportion of Title

[24] Recall that general PSC measures require that data is valid for both the magnet and charter schools in the district, so it is missing in districts that have valid charter school data but no valid magnet school data. All missing values are set to zero in the data and corresponding indicator variables are included to identify initially missing data.

[25] Recall that for the PSC measures, the fraction of schools is in reference only to schools in the district that are third-grade inclusive, Also, the fraction of enrollment merely reflects the fraction of district enrollment attending schools that are third-grade inclusive.

I eligible schools and a higher proportion of students who are free-lunch eligible compared with the districts resided in by public school students. Additionally, districts resided in by private school students have, on average, a lower proportion of white students enrolled in public school and a higher proportion of African American students compared with the districts of public school students. Slightly higher spending levels are also seen per pupil, on average, in districts represented by the sample of private school students than in the districts of the public school student sample. A difference in federal revenue per student appears to drive this difference. The average student-teacher ratio is 17 students per teacher across all districts.

Because variation in demographics and socioeconomic measures such as income, race, and education, have generally been found to encourage the entry of private schools and charter schools — rather than means values (Barrow 2006; Glomm et al. 2005). I also contrast the demographic profiles of the districts' residents in the private school sample and the public school sample in the bottom portion of Table 2-3. Among these qualities representing the school districts' residents, little difference appears across the private school and the public school student samples.[26] Perhaps not surprisingly, a slightly higher proportion of the districts' residents are a minority race or ethnicity in school districts of private school students compared with the school districts of public school students, at 30 and 27 percent respectively. Across both samples, the average median household income is $44,000, with 12 percent of the districts' households below the poverty level. And finally, across all school districts, roughly a quarter of the residents ages 25 and older have at least a four-year college education, and 48 percent of this age group have at most a high school diploma.

1.5 Empirical Model

Using the data I have compiled, I examine whether households treat PSC schools as reduced-priced substitutes for private school education. And to the extent that households do, are they treating charter and magnet schools as substitutes for one another, or do the effects of charter and magnet schools tell different stories regarding their influence over the household's public-private school decision?

[26] Recall that all residential profiles of at the school district level reflect 2000 Census data, which correspond to the first-grade year of this sample's cohort.

Reduced Form – Probit Model

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

(1) $$\mathbf{Pr\ [Y_i=1] = f(H_i, D_j, C_j, P_c, R_{tj})}$$

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, **H** is a collection of student- and household-specific qualities; **D** is a set of variables describing the school district (*j*) matched with the student's residence; **C** is a measure of PSC in school district *j*; **P** is a measure of private school supply measured at the county-level (*c*) corresponding to the student's residence; **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 charter state as of 2000.

Table 2-1 presents a more comprehensive set of student and household variables than are used in the analysis. Fewer controls are included in the analysis because of two issues: one is collinearity 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), an interaction term of the grade-span and

student-teacher ratio variables, and the number of schools in the district.[27] 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 supply variables include the number of private schools in the county of the student's residence. Additionally, to control for any religious-inspired attachment to the private sector, I also include the proportion of Catholic private schools in the county. Although other private school variables at the county level are constructed for summary statistics purposes (as presented in Table 2-2), they are not included in the actual analysis because of concerns about potential endogeniety.

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 a 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 at most 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 total number of students in 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 competition within a school district, I employ a number of alternative measures of PSC: 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. I also include PSC dummy variables to denote the presence of at least one PSC school in a district.

1.5.1 *Base and Extended Models*

Initial estimates are run on a base model that consists of a handful of key student, household, district, and resident characteristic variables. The student and household

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

regressors for the base model are the student's race and a household income variable.[28] 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.[29] The base model also includes the private school supply measures, regional controls, and an indicator variable that takes the value of *one* for states that had charter legislation 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.[30]

The extended model includes the remaining student, household, district, and resident characteristic variables discussed earlier in this section, with some exceptions. The household 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 dummy variables to control for state fixed effects.

1.5.2 ***Modeling Public School Choice***

In my analysis, my motivating interest is to examine whether households respond to PSC schools as a reduced-price substitute for 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), measured in 100s, in a district highlights the potential diversity across public sector alternatives. The fraction of district schools that are PSC schools essentially measures how dominant these types of schools are in the district relative to other public

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

schools; the fraction of district enrollment attending choice schools essentially measures the same thing but with enrollment measures – likely a much stronger measure as it represents where parents have chosen to entrust their children. General PSC measures do not distinguish between charter or magnet school status; I subsequently consider type-specific PSC measures.

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 general PSC control is a dummy variable taking the value of *one* if the district has a choice school (equation 2 below). Subsequent specifications then also incorporate alternative PSC competition measures (equations 3 through 5).

(2) PSC = Choice Dummy

(3) PSC = Choice Dummy; Number of Choice Schools

(4) PSC = Choice Dummy; Fraction of Choice Schools

(5) PSC = Choice Dummy; Fraction of District Enrollment in Choice Schools

In equation 2, when the only the PSC indicator variable is included, the coefficient captures the effect of the presence of a PSC school — regardless of how dominant the PSC schools are in the district. In equations 3 through 5, the coefficients on the additional PSC measures estimate the effect of relative measures of PSC competition, conditional on a school district having at least one PSC school.

1.6 Results

1.6.1 *General PSC Measures*

Initial probit estimates under a basic set of controls reveal no strong relationship between general measures of PSC and the household's decision to send their child to private school. Recall that these general measures include a dummy variable indicating the presence of at least one choice school in the district and one of three additional competition measure: the number of choice schools in the district, the fraction of choice schools in the district, and the fraction of district students enrolled in choice schools. Also recall that the PSC measures portray district profiles that reflect the previous academic year (2000-2001) and take into account only schools and student enrollment from institutions that were elementary-inclusive in that they offered third grade during the 2000-2001 academic year.

Table 2-4 presents the probit marginal effect estimates for these PSC measures on the household decision from the base model, which, recall, includes only a basic set of explanatory variables and two private school supply measures. In column one, I report the results from estimating the model when the only general PSC measure included is a dummy variable indicating the presence of at least one choice school in the district. The coefficient is insignificant. In column two, the PSC measures include the choice school dummy and the number of choice schools in the district. Here, the coefficient on the dummy variable is positive and significant at the 10 percent level. The coefficient on the number of choice schools is negative and statistically significant at the 5 percent level. In column three, I replace the number of choice schools in the district with the fraction of choice schools in the district. Here I find that both PSC coefficients are insignificant. Finally, in column four, the general PSC measures include the choice school dummy and the fraction of district enrollment that attends a choice school. The coefficient on the PSC dummy variable is positive and significant at the 10 percent level, and the coefficient on the fraction of district enrollment in PSC schools is negative and also statistically significant at the 10 percent level.

In both columns two and four of Table 2-4, the presence of PSC schools in a district is associated with an increased probability of choosing private school, likely because of perceived poor public school quality in areas that attract PSC schools. However, conditional on having at least one PSC school in a district, both PSC competition measures in columns two and four reveal mild private sector crowd-out effects as choice schools take up a larger share of a district. The positive and statistically significant PSC dummy coefficient in column two, where the PSC competition measure is the number of choice schools in the district, implies that residing in a district that has a choice school, rather than in a district that does not, increases the likelihood a household chooses private school, on average, by 4.7 percent, all else equal. In column four, where the PSC competition measure is the proportion of choice school enrollment, the average effect of at least one choice school in the district increases to 6.6 percent. The magnitude of these measured effects further support the hypothesis that the PSC dummy likely captures, in part, the demand for PSC schools rather reflects a measure of PSC supply. This ultimately proves to be a useful control for school quality, allowing me to examine the impact of PSC competition using the remaining PSC measures, conditional on the presence of PSC in a district.

Recall that the coefficients on the PSC competition measures in Table 2-4 are negative, supportive of private sector crowd-out as a result of a greater presence of PSC schools in the district. Specifically in column two, the coefficient of interest is -0.054, implying that an additional 100 PSC schools in the district decreases the likelihood of choosing private school by 5.4 percent, on average, ceteris paribus – likely a very negligible impact. In column four, the coefficient of -0.203 is the marginal effect estimate for a one-unit increase in the fraction of the district's enrollment attending choice schools, implying a one percentage point increase in the percentage of district enrollment attending PSC schools is associated with an average decrease of 0.203 percent in the likelihood a household chooses private school. The implication of this effect is somewhat elusive, as the marginal effects are calculated at the mean values for the independent variables. As there are a number of dummy variables included in the explanatory variables, such a calculation forces those variables to take a value somewhere between 0 and 1, which is not interpretable. Thus in a later section I do a simulation to look more closely at how the supply of PSC in a district affects the probability of private sector crowd out using households with a particular set of characteristics.

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, ceteris paribus. In addition, living in a school district with a higher median household income is negatively related to choosing private school. The coefficients on total expenditures per child and the proportion of district students eligible for free lunch are never significant. To provide a point of reference for the PSC coefficient estimates, I also discuss the coefficient estimates of these remaining household and school district variables. Again, consider the estimates presented in the second column of Table 2-4. I find that being African American rather than white reduces the probability a household chooses private school by, on average, 8.2 percent, ceteris paribus. This finding is consistent across the various PSC measures in Table 2-4. 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, 8.5 percent, holding all else constant. Increasing a household's income category median by $1,000 a year is associated with an average increase of 0.1 percent in the probability a household chooses private school, ceteris paribus. Lastly, an extra $1,000 in the median household income in the school district (two years prior) decreases the probability of

a household opting for private school by, on average, 0.1 percent, all else constant. Of the two private school supply measures, only the number of private schools in the county is significant. An additional ten private schools in the county of residence increases the probability of choosing private school by 0.1 percent, on average, holding all else constant.[31] 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.

Thus, conditional on a basic set of student, household, and school district variables, the factors that tend to elicit the largest effects on the household's public-private school decision appear to be the student's race, then the presence of at least one PSC school in the district, arguably followed a household's income, the number of private schools in the student's county of residence, and the median income of the school district. In this base model, the magnitudes of the PSC competition measures coefficients are among the smallest statistically significant coefficients, and while they are consistent with private sector crowd-out, they imply perhaps trivial impacts on the household public-private decision. Regarding the factors that appear to be the most influential in the base case, in that evidence is strongest that they are related to the household's public-private decision, household income is the most influential factor, followed by the student's race and the number of private schools in the student's county of residence. School district level qualities represent the least influential factors here – these include the district's median household income and district PSC measures.

In Table 2-5, 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 fixed effects.[32] When the PSC

[31] In the main model specifications, private school measures are not restricted to elementary-inclusive schools because of the presumed strong correlation between private schools of secondary and primary grades. However, I also estimate the base model when restricting the private school measures to those that offer third grade during the 2001-2002 school year. On doing so, the sign and statistical significance of the PSC coefficients are unchanged from the main results presented in Table 3.

[32] State dummies are included in the state fixed effect model. For a few states with at most 20 observations each, the students were grouped into one fictitious state. Though the results are not presented here, the model was also run with state fixed effects and no fictitious state, which ultimately reduced the sample size to 7,610. Estimates for this smaller sample were consistent in sign and statistical significance to the coefficients in the base and extended models presented in Table 2-5. However, when the base model is run using this smaller sample, the coefficient on the number of PSC schools is insignificant, conditional on the presence of at least one PSC school in the district.

dummy variable is the sole general PSC measure, its coefficient is never significant, and the results are not present here; rather I report the estimates when the choice school dummy is included in tandem with a PSC competition measure. In effect, Table 2-5 expands the last three columns of Table 2-4.

The three panels of coefficients provided in Table 2-5 present the progression of the marginal effect estimates of the general PSC measures as the model is expanded to include the additional independent variables. The first panel (columns one through three) presents the results when the general 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 fixed effects — columns one, two, and three, 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 consistently negative and statistically significant. The positive coefficient on the PSC dummy, however, is significant under the base model only. It is possible the extended model's additional controls somewhat address the school quality aspects captured by the PSC dummy in the base model as well as introduce more household level qualities – such as parental education, family size, and the student's disability status – that can arguably contribute to a more discriminating set of preferences held by households as they choose schooling for their children.

The second panel of coefficients (columns four through six) presents the results when the general 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 model as well as the extended model with state fixed effects — columns four, five, and six, respectively. The coefficient on the choice dummy is persistently insignificant. While the coefficient on the fraction of choice schools in the district is insignificant under the base model, on inclusion of the extended set of student, household, school district, and resident variables (but not state fixed effects), the coefficient is negative and statistically significant at the 5 percent level. With the further inclusion of state dummies, the coefficient retains its negative sign and the statistical significance falls to the 10 percent level. The PSC competition coefficients variables follow this general trend of increased magnitude and statistical significance as the model extends from its base case; though this is perhaps most evident with the fraction of PSC schools variable. Households appear more responsive to

the supply of PSC in their district when additional factors such as parental education, student disability status, and community attributes are also controlled for, as again these are household and school district qualities that likely affect the public-private decision. The further inclusion of state fixed effects captures state-wide open enrollment policies and their nuances, charter school legislation, voucher programs, and other state-specific education policies that influence a household's public-private school decision.[33] To the extent these state programs and policies are correlated with the entry and expansion of PSC schools in a district would mitigate the effect of PSC competition measures once state fixed effects are included.[34]

Finally, the third panel of estimates presents the results when the general PSC measures include the choice school dummy and the fraction of district enrollment attending choice schools; the three columns present results from the base model, extended model, and extended model with state fixed effects — columns seven, eight, and nine, respectively. As seen with the first panel, the positive coefficient on the PSC dummy variable is initially significant in the base case but is insignificant under the extended models. The coefficient on the fraction of district enrollment attending PSC schools is always negative and statistically significant, though the level of significance fluctuates with the model's set of control variables consistent with the trend described above.

Under the extended model with and without state fixed effects, as depicted in Table 2-5, the coefficients on the number of choice schools in the district is -0.058 and -0.053, respectively, which is consistent with the base model. Thus, conditional on having PSC in the district, an additional 100 PSC schools in a district continue to be associated with an average 5 percent decrease in the probability a household chooses private school, all else constant. Again, this appears a fairly inconsequential effect. With respect to the second competition measure, the fraction of PSC schools in the district, the coefficient under the extended model without state fixed effects is -0.202, implying a one percentage point

[33] I also estimate the base model and the extended model, without state fixed effects, with the inclusion of an indicator variable for whether the child lived in a state with mandatory interdistrict open enrollment polices state-wide in 2000 or in 2001. The results are presented in the discussion section.

[34] The state fixed effects model using the slightly smaller sample of 7,610 and no default group-state for the few sparsely represented states (none of which had a mandatory interdistrict open enrollment policy as of 2001) was run using the extended model and an open enrollment dummy variable. When the results are compared with the extended model with state fixed effects estimated on the same sample of 7,610, no changes occur in PSC coefficient sign or statistical significance. They are also consistent with the results presented in Table 2-5.

increase in the percentage of choice schools in the district is associated with a 0.202 percent reduction in the probability a household opts for private school. With state fixed effects, the magnitude of this coefficient falls to -0.177. Again, the private sector crowd-out effect appears trivial.

Finally, the third PSC competition measure, the fraction of district enrollment attending PSC schools, retains a similar coefficient to the base model when the extended model's set of controls are added, as depicted in Table 2-5. Recall the coefficient under the base model is -0.203. This coefficient under the extended model is -0.250, implying that a one percentage point increase in the percentage of district students enrolled in PSC schools is associated with a reduction in the probability a household opts for private school of 0.250 percent. On the inclusion of state-specific controls, this coefficient remains fairly steady at -0.234.

As in the case of the base model, only one of the two private school supply measures is ever significant in the extended models; this is depicted in Table 2-5. An additional ten private schools in the county of residence increases the probability of choosing private school by 0.1 percent, on average, holding all else constant.[35] This coefficient in sign, magnitude, and statistical significance is persistent across these model specifications.

Overall, on the inclusion of the extended model's set of additional control variables, notable changes as well as affirmations are seen in the coefficients of the PSC variables when compared with the base model's estimates. No longer is the PSC dummy variable significant, although it still retains its positive sign. The inclusion of additional student and household variables appears to allow for more discerning tastes from heterogeneous consumers; the additional school district variables perhaps proxy sufficiently for school quality measures, helping address, in part, the suspected endogeniety of the PSC dummy. The PSC competition measures continue to reveal evidence of private sector crowd out – the two most pervasive crowd-out elicitors appear to be the number of choice schools and the fraction of district enrollment in a PSC school, both of which are significant in the base and extended models.

[35] I also estimate the extended models when restricting the private school measures to those that offer third grade during the 2001-2002 school year. On doing so, the sign and statistical significance of the PSC coefficients are unchanged from main results presented in Table 2-5 with one exception: when running the extended model without state fixed effects, the coefficient on the PSC dummy is positive and statistically significant when the included PSC competition measure is the fraction of district enrollment in PSC schools.

1.6.2 *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 general 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 general PSC measures. The decomposition relaxes this constraint.[36] Thus, the estimates presented here are only comparable with the general measure estimates presented previously to the extent that missing data on choice schools is not an issue.[37]

Table 2-6 presents the probit marginal effect estimates from the extended model with state fixed effects, now with PSC measures decomposed. The findings tend to be similar to those of the general PSC measures. In column one, the only PSC measures included are 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 two, 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, neither of the type-specific dummy variables have significant coefficients; similarly, the coefficient on the number of charter schools in the district is also insignificant. The coefficient on the number of magnet schools in the district, however, is negative and statistically significant. Because charter schools are fewer in number than magnet schools and the potential polices for some charter schools to represent their own district, the lack of variation between the number of charter schools in a district and the charter dummy variable for the district may help explain these results.[38] The coefficients for

[36] 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 that about 25 percent of the data on magnet school classification is missing in the current sample, all data on charter school classification are valid for the 2000-2001 academic year.

[37] I also estimate the type-specific model only decomposing the PSC variables by type if the district had valid identifiers for both charter and magnet schools. The sign and statistical significance for PSC coefficients are consistent with Table 2-6.

[38] In the 2000-2001 CCD data, approximately 23 percent fewer elementary-inclusive charter schools appear than magnet schools. In Michigan charter schools are treated as occupying their own district (Toma 2003).

all the remaining type specific PSC competition measures for both charter and magnet schools, columns three and four of Table 2-6, are statistically significant.

In column three, 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 the coefficient on the fraction of magnet schools in the district is negative and statistically significant at the 1 percent level; the same finding holds for the fraction of charter schools in the district, only its coefficient is significant at the 5 percent level. The discrepancy in statistical significance across the two types of schools perhaps further supports the limited effects of some charter school measures when charter schools are relatively fewer than magnet schools and may be treated in some states as occupying their own district. The coefficient on the magnet school dummy is positive and statistically significant at the 10 percent level, while the charter school dummy is insignificant – though its z-score is not far removed from significance at the 10 percent level. Thus conditional on these measures of PSC composition in a district, as well as the full set of the extended model's control variables and state fixed effects, the presence of magnet schools may be more endogenous to school quality than the presence of charter schools, as charter schools are subject to a state's charter legislation, rather than the discretion of school district officials.

Finally, in column four, 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, the coefficient on the magnet school dummy is positive and statistically significant at the 10 percent level, while the coefficient on the charter school dummy is marginally significant. The coefficients on the fraction of district enrollment in magnet schools and the fraction of district enrollment in charter schools are both negative and statistically significant at the 1 percent level. [39]

Considering the marginal effect estimates from the type-specific PSC measures, presented in Table 2-6, nearly all of the magnet and charter school competition measures have statistically significant coefficients. Regarding the impact of the number of choice

[39] Though not reported here, I also estimate type-specific model presented in Table 2-6 using the smaller sample of 7,610 students and include all state dummies (rather than creating a fictitious state from a few sparsely represented states). The estimates are consistent in sign and statistical significance with those presented in Table 2-6. However, the marginally significant charter school dummy coefficients (columns three and four of Table 2-6) become significant using the smaller sample.

schools in the district, this effect appears to be driven by magnet schools, as the coefficient on the number of charter schools in insignificant.[40] Conditional on the presence of either type of choice school, an additional 100 magnet schools in a district is associated with a reduction in the likelihood of a household choosing private school of, on average, 5.7 percent, all else equal – again, a negligible effect. Regarding the fraction of schools in the district that are either charter or magnet, the magnitude of this PSC effect is substantially greater for charter schools. Conditional on the presence of either type of choice school, a one percentage point increase in the percentage of magnet schools in the district is associated with an average reduction of 0.377 percent in the probability a household chooses private school, all else equal. A similar increase in the percentage of charter schools in the district is associated with a reduction in the probability of a household choosing private school of 1.017 percent, on average, ceteris paribus. Although the magnitude of each effect may appear negligible, the charter school effect is nearly three times larger than the magnet school effect. A similar story is told by looking at the fraction of district enrollment attending either magnet or charter schools in the district. Conditional on the presences of either type of choice school in the district, a one percentage point increase in the percentage of district enrollment attending magnet schools is associated with a decrease in the probability of a household opting for private school of, on average, 0.370 percent, ceteris paribus. However, a similar increase in the percentage of district enrollment attending charter schools is associated with a reduction in the probability of a household choosing private school of 1.215 percent. The magnitude of this impact of charter school enrollment is again roughly three times greater than that of the magnet school enrollment effect.

As in the case of the previous models with general PSC measures, only one of the two private school supply measures is ever significant in the type-specific extended model with state fixed effects, as depicted in Table 2-6. An additional ten private schools in the county of residence increases the probability of choosing private school by 0.1 percent, on average, holding all else constant.[41] This coefficient in sign, magnitude and statistical

[40] Recall that school districts with valid charter school identifiers but without valid magnet school identifiers had their general PSC measures set to missing, then set to zero with the inclusion of an indicator variable for missing PSC measures. Decomposing schools by charter and magnet status does not require both school types to be valid in the data, and thus the samples are configured differently for the general PSC choice measures and the PSC measures that are charter-school specific.

[41] I also estimate the type-specific models when restricting the private school measures to those that offered third grade during the 2001-2002 school year. On doing so, the sign and statistical significance of the PSC

significance is consistent across model specifications with general or type-specific PSC measures.

1.7 Discussion

Looking at the base model as well as the extended model with and without state fixed effects, some evidence among the PSC competition measure coefficients supports private sector crowd-out, though the magnitudes appear to be trivial. Additionally, the coefficients on the PSC dummy variables tend to be either insignificant or positive – the presence of these PSC schools appears to be correlated with the entry of private school enrollment, perhaps because of underlying public school quality concerns in the district. I estimate the base and extended models as presented in Tables 2-4 through 2-6, under a variety of specifications to perform robustness checks on the results presented in the previous section. The various specifications include (1) modifying the PSC measures, (2) including open enrollment measures, and (3) estimating the model with no private school supply measures. I also incorporate interaction terms in an attempt to examine whether there are differential PSC-inspired private sector crowd out effects according to a household's income level or extent of religious services participation. Finally I perform a simulation using the marginal effect estimates presented in Table 2-6 using a particular type of household rather than the mean values of the explanatory variables. Not all of the results corresponding to the following discussion are presented here, but they are available from the author on request.

1.7.1 *Modifying the PSC Measures*

For robustness checks, I vary the construction of the PSC variables and rerun each model. Recall that the main results presented in the previous section reflect PSC measures that are one-year lags and reflect only elementary-inclusive schools. I also construct the PSC variables three additional ways: (1) elementary-inclusive-only schools for the sample's current

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measures are consistent with the main results presented in Table 2-6 with two exceptions: the coefficients on the charter school dummy variable is positive and statistically significant when the included PSC measures are either the type specific fractions of PSC schools or the type specific fractions of district enrollment in PSC schools.

academic year, (2) one-year lag measures without grade restrictions, and (3) current academic year without grade restrictions. These various PSC specifications relax one or both of two key restrictions of the main PSC measures. First, using PSC measures that reflect the current academic year relaxes any attempts to control for tenure of PSC in a given district; such measures may be more prone to endogeniety concerns as households could be responding to the same public school concerns that led to the entry of any recent PSC schools. Second, using PSC measures that are not grade-restrictive hones in on the market share and presence of PSC in a district but does not necessarily identify how much choice in the public sector is available to the students in this sample. Such PSC measures are likely prone to understate the effect of PSC in a district.

In Tables 2-7 – 2-9, I present the results for these additional PSC specifications run on the base model; the results are to be contrasted with the main results presented in Table 2-4. The findings are somewhat robust across PSC specification. The number of PSC schools in a district is consistently negative and statistically significant; the proportion of PSC schools in the district is always negative but never significant, and the fraction of district enrollment attending PSC schools is always negative but is statistically significant only when the PSC measures are restricted to elementary-inclusive schools – though the coefficient for the PSC measures that are not grade restrictive are not far removed from significance at the 10 percent level. As for the dummy variable, it is consistently positive across the PSC specifications, and it tends to be statistically significant only when the number of schools or the fraction of district enrollment are also included in the model as PSC competition measures. Though some evidence supports the potential consequences mentioned above of relaxing key restrictions on the PSC measures, not all coefficients changed in the predicted fashion. When the PSC measures reflect the current academic year and are elementary inclusive, all estimated PSC effects are slightly smaller in magnitude than those of the main results presented in Table 2- 3. This is consistent with households giving more value to more-tenured PSC options, but does not appear to reflect increased concern that the presence of PSC schools in a district is endogenous to public school quality. This observation may also reveal that households both take in information about their set of schooling options and make consumption decisions over time, not immediately; thus a lag in consumption of private or public school may reflect a change in preferences that occurred a

year before, and perhaps that very change in the household's preferences was motivated by a change in schooling options that occurred a year before that.

In Tables 2-10 and 2-11, I present the results for rerunning the extended model and extended model with state fixed effects using the modified PSC measures described above; the estimates are to be contrasted with the main results presented in Table 2-5. When reviewing the extended model in Table 2-10, the coefficient on the dummy variable is generally positive and is never significant. The number of PSC schools in a district is perhaps the most persistently significant PSC competition measure under the extend model across the various PSC specifications, though the PSC enrollment measure is always at least marginally significant. And slightly more evidence indicates a private sector crowd-out from PSC competition measures when they are restricted to elementary-inclusive schools as opposed to when they are not grade-restrictive. The same comments apply to the various PSC specifications with the extended model with state fixed effects, the results for which are presented in Table 2-11, with the exception that notably more evidence shows private sector crowd-out from the PSC competition measures when the PSC measures are restricted to elementary-inclusive schools versus when the PSC measures are not grade restrictive. As in the case of the base model, the coefficients on the PSC measures for both extended models tend to fall slightly in magnitude on relaxing either the tenure constraint (i.e., using the current year PSC measures) or the grade restriction.

Finally, in Tables 2-12 – 2-14, I present the results for rerunning the type-specific extended model with state fixed effects using the modified PSC measures described above; the estimates are to be contrasted with the main results presented in Table 2-6. The sign of the coefficients on the dummy variables is almost always positive, and is predominantly insignificant for magnet schools and always insignificant for charter schools. Under the type-specific model, the number of choice schools offers the least amount of evidence in support of private sector crowd-out across the varied PSC specifications; the number of magnet schools is only significant one time other than the main results: when the PSC measures reflect a one-year lag and are not grade restrictive. The number of charter schools is persistently insignificant across the alternative PSC specifications. The coefficient on the fraction of magnet schools in a district retains its sign and statistical significance on relaxing either the tenure constraint or grade restriction; however, the coefficient on the fraction of charter schools, despite being significant in the main results, is insignificant under every PSC

modification presented in Tables 2-12 – 2-14. The fractions of district enrollment both for magnet schools and charter schools retain their sign and statistical significance on relaxing either the tenure constraint or the grade restriction, but not both – though under such a relaxed specification, the charter school enrollment measure is marginally significant. As with the general PSC measure coefficients discussed above, the coefficients on the type-specific PSC measures for both the extended models generally fall slightly in magnitude on relaxing either the tenure constraint (i.e., using the current year PSC measures) or the grade restriction, with some exceptions.

1.7.2 *Open Enrollment*

I also estimate the base model and extended model (without state fixed effects) with the inclusion of an indicator variable for whether the child lived in a state with statewide mandatory interdistrict open enrollment polices in 2001. It is important to note that a few discrepancies appear in reports on statewide open enrollment for this time period.[42] However, assigning a state's open enrollment status according to any of the various reports does not drastically affect the results; in fact little difference is observed in the coefficient estimates across specification. I ultimately assign open enrollment status according to one report and check the results against running the model according to the other reports' specifications.

Though the results are not reported here, on including an open enrollment dummy variable in the base model, the findings are consistent with Table 2-4. The PSC coefficients retain their sign and statistical significance with one exception. The coefficient on the PSC dummy variable, when the PSC competition measure included is the fraction of choice schools (column three) is positive and statistically significant under one reports open enrollment specification. Additionally, the coefficient on the open enrollment dummy variable is always insignificant in the base case. The findings for the extended model without state fixed effects are also consistent with previous results, those presented in Table 2-5, with the exception that the dummy variable is positive and statistically significant when the PSC

[42] A 2009 report by the Joint Committee on Education out of Jefferson City, MO, listed 13 states with mandatory state-wide interdistrict policies by 2001. A 2003 report by the NCES reported four additional states, for a total of 17, with mandatory interdistrict open enrollment policies in the 2001-2002 school year. Finally an article by Brunner, Cho, and Reback list 13 states as well, but the list is not entirely consistent with that of the Joint Committee on Education Report.

competition measure is the fraction of district enrollment in PSC schools (column five). The coefficient on the open enrollment dummy variable in the extended model is positive and statistically significant, though not under all report-specific open enrollment state classifications. This finding could reflect generally poorly perceived public school quality relative to public schools in states without such mandatory open enrollment policies. Or perhaps the finding suggests that mandatory open enrollment policies in states may ultimately have a tendency to redistribute students across districts such that the discriminating tastes of households are not satisfied within the public school system. It is beyond the scope of this chapter to further explore the potential implications of the open enrollment coefficient.

Overall, the general findings regarding PSC effects are not sensitive to the inclusion of interdistrict open enrollment controls. The PSC coefficients generally retain their magnitude, sign, and statistical significance on the inclusion of an open enrollment dummy variable. Thus no obvious evidence indicates that ignoring mandatory interdistrict open enrollment policies biases the PSC coefficients upward. However, the inclusion of indicator variable here does not account for intra-district policies nor does it capture the further nuances of the open enrollment policies. Thus state fixed effects may be a more thorough means of addressing state-specific open enrollment policies.[43]

1.7.3 *No Private School Supply*

I also estimate the base model, extended models, and type-specific model without controls for private school supply. Because private schools do not operate under the geographical bounds that constrain public schools, one can argue that technically all students have a private school option. In this context, private school supply simply reduces to the unobserved costs associated with attending a private school (i.e., time, travel, tuition, etc.). Estimating the base model under this relaxed specification, I find no evidence of a relationship between the measure of PSC in a district and the household's public-private decision under the base model.

[43] Alternatively, one could further control for the general nuances of a state's open enrollment policy and interacting those with the appropriate community and school district qualities to help expose the effects of open enrollment policies on the household public-private school decision.

That the PSC effects within the base model depend on including private school supply measures suggests that proximity to a supply of private schooling is necessary to observe any influence of PSC over the household's public-private school decision. It suggests that the PSC effects may be limited to or strongest across the radii of equally concentrated sets of private school alternatives. PSC schools may try more to compete with private schools when more private schools exist nearby; implying that without immediate proximity to private schools, the PSC schools themselves behave or are structured in such at way to make themselves less competitive with private schools. This result may also say something about the community residents. Just as the presence of a PSC school is generally tied to the immediate and/or neighboring public school quality, so is the presence of private schools. Areas with higher concentrations of private schools should correspond with heterogeneous demographic and socioeconomic community profiles. Charter and magnet schools are inherently designed to hone in on and appeal to the diverse tastes among consumers of educational services.

Beyond the base model, I also estimate the extended model and extended model with state fixed effects, without controls for private school supply. Again, under this relaxed specification, I find no evidence of a relationship between the measure of PSC in a district and the household's public-private decision under the extended model without state fixed effects. When I estimate the type-specific PSC model — as described above and depicted in Table 2-6 — with no controls for private school supply, I find slightly different results than those presented previously. I find no evidence that households respond to the presence of at least one magnet or charter school in the district, with the exception of a positive and statistically significant coefficient on the charter school dummy when the PSC competition measures include the proportions of schools in the district that are either magnet or charter. All other type-specific dummy coefficients are insignificant. All the type-specific PSC competition measure coefficients, however, are negative and statistically significant, save one; the number of magnet schools in a district is insignificant when the type specific dummies and numbers of PSC schools are included.[44] Also insignificant is the coefficient on

[44] I also estimate the type-specific model without private school measures when only districts with valid charter and magnet school identifiers have their PSC decomposed by type; the results are consistent in coefficient sign and statistical significance with those discussed here.

the magnet school dummy when the PSC competition measures are the type specific numbers of choice schools in the district.[45]

1.7.4 *Interaction Terms*

As previously mentioned, the expectation for the observed effect of the PSC schools in a district was private sector crowd out. However, both public schools and private schools are multi-dimensional. It is not possible to fully account for all the attributes of each type of school or sector of education – even if one could, public sector schools are still limited in their ability to compete with religious private schools. Thus a greater supply of PSC schools in a district is expected to elicit differential crowd out effects across heterogeneous households. Here I address whether households with a religious attachment to the private sector are truly less likely to opt into the private sector due the presence of PSC schools. And I examine whether the evidence suggests that households whose attachment to the private sector is rooted in school quality are more likely to opt into the public sector due to a greater supply of PSC schools.

I estimate the model controlling for a measure of religious participation of the household. This measure, collected from the first grade survey of the ECLS-K asks the parent completing the survey how often he/she attended a religious service during the previous year. I include the religious household measure in the base and extended models and the coefficient is positive and significant, indicating that more frequent participation of the parent(s) in religious services s positively related to private school enrollment for the student.[46] I also interact the religious household measure with type-specific PSC measures – in particular, the PSC dummy variables and the enrollment-based PSC competition measures – to examine whether there is a differential crowd out effect observed by the degree of religious attachment a household may have to the private sector. I estimate the type-specific extended model with state dummies, including the household's religious measures and the

[45] While the evidence is intriguing in support of private sector crowd-out from the type-specific PSC competition measures, the results are sensitive to the inclusion of some of the model's variables. Additional study is warranted before further comment.

[46] The frequency of religious services is not a continuous measure, but rather a categorical measure. Parents pick from among five categories that range from "never or almost never" to "several times a week" to report their level of religious service participation in the previous year. Thus, the coefficient is not directly interpretable.

four religious interaction terms.[47] I present the estimates in Table 2-15**.** The only statistically significant interaction-term coefficient is that of the religious variable interacted with the fraction of district enrollment attending a charter school. The sign is positive, and is consistent with the prediction that households with a religious attachment for the private sector are less prone to opt into the public sector due to the presence of PSC schools. This finding may reflect the tendency of charter schools to locate in areas where private schools already operate (Glomm et al., 2005). I do not observe such a relationship between more religious households and magnet school enrollment.

To address whether there is a differential private sector crowd out effect based on a household's preference for private school's superior quality, I take a similar approach to that discussed above. Because I cannot observe a household's preference for school quality, I use an income measure. I assume that higher income households are more likely to enroll in private school but do not necessarily have a religious attachment to the private sector; thus higher income households can pay for greater levels of school quality. To examine this in the model, I interact income measures with type-specific PSC measures – in particular, I create a dummy variable for high income households that takes the value of one if households make at least $75,000 annually and a mid-to-high income category variable that takes the value of one if households make between $40,000 and $75,000 annually. Both income categories are created with the expectation that high income households will likely not be drawn into the public sector by the availability of PSC schooling, however, mid-to-high income households may be more likely to opt into the public sector due to the availability of PSC alternatives. I interact these income dummy variables with the type-specific PSC dummy variables and the enrollment-based PSC competition measures to examine whether there is a differential crowd out effect observed by the household income level. I estimate the type-specific extended model with state dummies, including the eight income interaction terms. I present the estimates in Table 2-16.

Again, I only observe statistically significant coefficients on the charter school interaction terms. In particular, the coefficient on the interaction term of the mid-to-high income dummy and the charter school dummy variables is positive and statistically

[47] I also estimate the base model with type-specific PSC measures, the religious interaction terms, and the religious measure – as with the extended model. The estimates are very similar to the extended model with state dummies.

significant. Again, this may be due to charter schools tending to locate in areas where private schools already operate (Glomm et al., 2005). Additionally, the coefficient on the interaction term for high income and charter school enrollment is positive and a bit shy of statistical significance at the 10 percent level.[48] The mid-to-high income version of this interaction measure has a negative coefficient, although, the coefficient is neither significant nor marginally significant. These findings suggest increased charter school competition does not draw the high income households into the private sector – as such households appear more likely to pay the premium for superior school quality provided by the private sector. But households with slightly lower income levels may be more inclined to opt into the public sector given a greater supply of PSC schools. Also, the magnet school interaction terms have negative coefficients, but are not statistically significant. Indeed, the estimates never reveal an income group for which PSC competition generates a private sector crowd out effect at a statistically significant level.

1.7.5 *Simulation*

Because the marginal effects are calculated at the mean values of the explanatory variables, I also perform a simulation to examine the private sector crowd out effects of effects of various levels of PSC school competition in a district looking at a particular type of household. This avoids the use of mean values for dichotomous explanatory variables. For the analysis, I use a hypothetical household of size four where the student is white with two parents, both college educated and residing in a suburban area in a southern state. The household makes between $50,000 and $75,000 a year. Additionally, the household lives in a county with 150 private schools, 80 percent of which have a religious affiliation. The school district values generally reflect sample mean values. In Tables 2-17 and 2-18, I present the predicted probabilities that a household sends their child to private school. (The value of one indicates the child attends private school, zero implies the student attends public school.)

[48] I also estimate the base model under these specifications – though I include income category dummy variables rather than the income category median value. Under the base model, the coefficient on the interaction term of high income and charter school enrollment is positive and statistically significant, and the remaining estimates are generally unchanged.

As mentioned, I extrapolate from Table 2-6, which presented the estimates of the type-specific extended model with state fixed effects. I conduct the simulation using these marginal effect estimates. I look at various levels of the total competition of PSC schools as well as solely looking at various levels of one type of PSC school in the absence of the other. In varying the number of PSC schools in a district, which is seen in Table 2-17, I increase the number of PSC schools across the four columns. In the first column I present the probabilities when there is no PSC present in a district. In the second column, I present the probabilities when there is a marginal presence of PSC in the district – merely one magnet and/or charter school. The third column reflects five charter and/or magnet schools in the district; and the last column reflects 25 charter and/or magnet schools in the district.

Looking across the four columns, one can see that private sector crowd out appears to be driven by charter schools rather than magnet schools. Though the presence of only one magnet school or one charter school as the sole measure of PSC is associated with a slight increase in the probability a household opts for private school, subsequent type-specific schools in a district are observed to be associated with a slight decrease in that inflated probability of attending private school. However, it is only observed for charter schools that the probability of private school enrollment eventually falls below the probability of attending private school in the absence of PSC. This private sector crowd out effect does not emerge until the number of charter schools exceeds five. And, again, these results present a trivial private sector crowd out effect – most notably observed here at the level of 25 charter schools in the district.

Looking now at Table 2-18, I keep the number of district schools at 100, but I vary the fraction of district enrollment that attends a PSC school. The first column reflects no PSC schools in a district. In the second column, the total fraction of district enrollment in a type specific PSC school is 1 percent; note that when both magnet and charter schools are and present in a district in this column, each type represents 2.5 percent of the district's enrollment. In the third column, the total fraction of district enrollment in a type specific PSC school is 5 percent. In the fourth column, the fraction of district enrollment in either charter or magnet schools always 25 percent.

Looking across the four columns, one can see the fraction of district enrollment in PSC schools generates a private sector crowd out effect. Consistent with earlier findings, this

occurs most strongly due to the presence of charter schools rather than magnet schools. However, both are associated with a slight private sector crowd out effect.

1.8 Conclusion

In this chapter, 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 schools. When considering general PSC effects, I find that PSC dummy variables tend to proxy for perceived school quality in a district and are generally positively related to households opting out of public school. Of the PSC competition measures, I find evidence to suggest a slight private sector crowd-out effect — conditional on the presence of a choice school in the district and conditional on a measure of private school supply. The two general PSC competition measures that were most consistently found to be associated with private sector crowd-out were the number of PSC schools in the district and the proportion of district enrollment attending a PSC school. All statistically significant general and type specific PSC measures were consistent with private sector crowd-out.

With respect to the type-specific PSC measures, again the dummy variables tend to reveal a correlation between private school enrollment and the entry of PSC schools and may, to some extent, proxy for school quality – though overall the dummies are rarely significant. However, based on the additional PSC competition measure included, the magnet school dummy variable is occasionally positive and statistically significant. Again, all PSC measures – for charter schools and for magnet schools – present coefficients that are consistent with private sector crowd-out. Magnet schools competition measures are perhaps more consistently associated with private sector crowd-out, as all three measures when included yield a statistically significant coefficient. However, the charter school competition measures, though not always significant, generate effects that tend to be nearly three times larger than the respective magnet school effects.

I do not find evidence that public schools of choice compete with private schools for households that have more of a religious attachment to the private sector, as measured by households that report attending more religious services more regularly. In addition I do not observe that public schools of choice compete with private schools for the high income

households that prefer private school for its superior school quality. However, it appears that households with slightly lower income levels may be more prone to substitute PSC schools for private schooling.

1.9 Tables

Table 1-1: Student and Household Attributes for Matched and Larger (or Unmatched) Third-Grade Samples

	Matched Sample (N=7,630)		Third-Grade Sample (N=11,130)	
	Mean	SD	Mean	SD
Student Attributes				
Private (%)*	13.04	(33.67)	12.08	(32.59)
Female (%)	50.8	(50.00)	49.8	(50.00)
Age	9.27	(0.38)	9.28	(0.38)
African American (%)***	17.57	(38.06)	15.16	(35.87)
White (%)***	55.25	(49.73)	57.9	(49.37)
Hispanic (%)	20.53	(40.40)	20.17	(40.13)
Other Race(%)	6.64	(24.91)	6.77	(25.12)
IEP (%)	11.49	(31.89)	11.31	(31.68)
Disability (%)	27.02	(44.41)	27.14	(44.47)
Changed Schools (%)	20.85	(40.63)	20.61	(40.45)
Household Attributes				
Household Size	4.49	(1.36)	4.52	(1.37)
Both Parents Present (%)**	69.9	(45.87)	71.6	(45.09)
No Father Present (%)**	27.38	(44.59)	25.79	(43.75)
Father Employed (%)	93.32	(24.97)	93.47	(24.70)
Mother Employed (%)	73.47	(44.15)	73.28	(44.25)
Household Income, Category Median ($K)	62.73	(62.65)	63.68	(63.54)
Father's Age	40.31	(7.05)	40.21	(7.04)
Mother's Age	37.58	(6.59)	37.51	(6.55)
Father High School (%)	26.41	(44.09)	26.36	(44.06)
Father Less than College (%)	27.3	(44.56)	28.62	(45.20)
Father College (%)	17.69	(38.16)	17.16	(37.71)
Father Professional (%)	14.14	(34.85)	13.62	(34.31)
Mother High School (%)	25.3	(43.48)	26.03	(43.88)
Mother Less than College (%)	35.37	(47.82)	35.67	(47.90)
Mother College (%)	16.73	(37.33)	16.43	(37.06)
Mother Professional (%)	9.63	(29.50)	8.96	(28.56)
Poverty (At or Below) (%)*	21.85	(41.32)	20.89	(40.65)
Northeast (%)***	21.2	(40.88)	18.08	(38.48)
Midwest (%)***	17.5	(38.00)	23.2	(42.22)
West (%)***	21.56	(41.13)	23.51	(42.41)
Suburb (%)**	43.38	(49.56)	44.61	(49.71)
Urban (%)***	43.23	(49.54)	37.05	(48.30)

*Difference in sample means is statistically different from zero at the: * 10% level; ** 5% level; *** 1% level. Private: student attends private school; IEP: student has an individual educational program at the school; Disability: student has a diagnosed disability on record at their school. Omitted category for parental education variables is Less than High School. Statistics are weighted.*

Table 1-2: Private School Supply and Demand Measures for Matched and Larger Third Grade (or Unmatched) Samples

	Matched Sample (N=7,630)		Third Grade Sample (N=11,130)	
	Mean	SD	Mean	SD
Private School Measures				
Private Enrollment, Census Tract (%)***	12.22	(11.53)	11.74	(10.77)
Private Schools, Co. (Number)	148.3	(293.50)	146.3	(294.60)
Average Private School Enrollment, Co.***	204.5	(73.40)	195.1	(72.30)
At Least One Catholic School, Co. (%)***	90.22	(29.71)	87.78	(32.75)

*Difference in sample means is statistically different from zero at the: * 10% level; ** 5% level; *** 1% level. 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.*

Table 1-3: Final Sample (7,630) Summary Statistics – School District Variables, and Means

	Private School (13% of Sample)		Public School	
	Mean	**(SD)**	**Mean**	**(SD)**
PSC Measures				
Choice School, Dummy (%)***	34.06	(47.41)	29.32	(45.53)
Choice Schools, Number†***	41.37	(64.89)	31.56	(56.84)
Choice Schools (%)†*	18.23	(13.90)	16.91	(13.28)
Choice Enrollment (%)†	16.91	(13.65)	16.3	(13.36)
Magnet School, Dummy (%)***	27.33	(44.58)	20.77	(40.57)
Magnet Schools, Number†	44.92	(67.24)	38.54	(61.58)
Magnet Schools (%)†	19.09	(14.41)	19.7	(13.67)
Magnet Enrollment (%)†	18.41	(13.97)	19.61	(13.70)
Charter School, Dummy (%)***	22.7	(41.90)	18.73	(39.02)
Charter Schools, Number†	7.82	(9.91)	7.58	(8.79)
Charter Schools (%)†*	4.4	(2.60)	5.04	(3.05)
Charter Enrollment (%)†*	2.78	(2.71)	3.4	(3.50)
School District Measures, Means				
Schools, Number***	124.3	(226.80)	107.8	(214.10)
Elementary Only School District (%)	0.05	(2.31)	0.23	(4.74)
Title I Eligible (%)*	54.16	(24.14)	52.77	(25.97)
White (%)***	51.29	(32.36)	55.95	(30.61)
Black (%)***	23.32	(23.08)	18.94	(20.14)
Hispanic (%)	19.11	(22.60)	19.15	(22.70)
Other Race (%)	6.24	(9.62)	5.91	(10.13)
Female (%)	48.7	(0.70)	48.69	(0.65)
Free Lunch Eligible (%)***	37.03	(21.93)	34.61	(20.44)
Total Expenditures ($K)***	9.52	(2.28)	9.11	(2.36)
Federal Revenues ($K)***	0.69	(0.36)	0.66	(0.38)
Student-Teacher Ratio	16.8	(2.60)	16.8	(2.70)
School District Resident Attributes				
Median Income ($K)	44.3	(12.10)	44.6	(14.50)
Households Below Poverty (%)	11.79	(5.82)	11.96	(6.17)
Minority (%)*	29.69	(20.37)	27.28	(19.68)
At Most, High School (%)	47.75	(11.85)	47.65	(13.07)
College Educated (%)	25.72	(10.87)	25.21	(12.20)

† *Values are based on School Districts with at least one PSC school of the proper type. 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 1-4: Aggregate PSC Measures – Base Model, Probit Marginal Effect Estimates

	(1)	(2)	(3)	(4)
PSC Measures				
Choice School, Dummy	0.026	0.047	0.053	0.066
	(1.13)	(1.73)*	(1.47)	(1.79)*
Choice Schools, Number		-0.054		
		(2.57)**		
Choice Schools, Fraction			-0.129	
			(1.21)	
Choice Enrollment, Fraction				-0.203
				(1.87)*
Private School Supply				
Private Schools, Number	0.001	0.001	0.001	0.001
	(3.82)***	(4.25)***	(3.87)***	(4.01)***
Proportion Catholic Schools	0.077	0.073	0.075	0.073
	(1.57)	(1.54)	(1.56)	(1.54)
Additional Model Controls				
African American (Student)	-0.083	-0.082	-0.082	-0.081
	(6.12)***	(6.07)***	(5.91)***	(5.88)***
Hispanic (Student)	-0.086	-0.085	-0.086	-0.086
	(7.82)***	(7.89)***	(7.79)***	(7.80)***
HH Income, Category Median ($K)	0.001	0.001	0.001	0.001
	(8.26)***	(8.26)***	(8.28)***	(8.30)***
Total Expenditures ($K)	0.004	0.003	0.004	0.004
	(1.09)	(0.80)	(0.98)	(0.97)
Free Lunch, Fraction (District)	0.078	0.099	0.085	0.088
	(1.24)	(1.56)	(1.37)	(1.42)
Household Income ($K), (District)	-0.002	-0.001	-0.001	-0.001
	(2.30)**	(2.06)**	(2.21)**	(2.20)**
Observations	7630	7630	7630	7630

Absolute value of z statistics in parentheses
* significant at 10%; ** 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 variables. Weighted estimates. Standard errors are clustered by school district.

Table 1-5: Aggregate PSC Measures – Base and Extended Model Specifications, Probit Marginal Effect Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Aggregate PSC Measures									
Choice School, Dummy	0.047	0.022	0.006	0.053	0.043	0.028	0.066	0.052	0.039
	(1.73)*	(0.97)	(0.31)	(1.47)	(1.33)	(0.91)	(1.79)*	(1.58)	(1.23)
Choice Schools, Number	-0.054	-0.058	-0.053						
	(2.57)**	(3.35)***	(2.34)**						
Choice Schools, Fraction				-0.129	-0.202	-0.177			
				(1.21)	(2.22)**	(1.75)*			
Choice Enrollment, Fraction							-0.203	-0.250	-0.234
							(1.87)*	(2.70)***	(2.31)**
Private School Supply									
Private Schools, Number	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(4.25)***	(3.93)***	(2.96)***	(3.87)***	(3.89)***	(3.12)***	(4.01)***	(4.09)***	(3.34)***
Proportion of Catholic Schools	0.073	0.055	-0.025	0.075	0.057	-0.039	0.073	0.056	-0.039
	(1.54)	(1.48)	(0.58)	(1.56)	(1.53)	(0.88)	(1.54)	(1.52)	(0.90)
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	7630	7630	7630	7630	7630	7630	7630	7630	7630

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 10%

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. Clustered by school district

Table 1-6: Type-Specific PSC Measures – Extended Model with State FE, Probit Marginal Effects

	(1)	(2)	(3)	(4)
PSC Measures				
Magnet School, Dummy	0.013	0.022	0.102	0.087
	(0.54)	(0.83)	(1.88)*	(1.76)*
Charter School, Dummy	-0.004	0.030	0.076	0.073
	(0.18)	(0.86)	(1.45)	(1.62)
Magnet Schools, Number		-0.057		
		(2.35)**		
Charter Schools, Number		-0.820		
		(1.37)		
Magnet Schools, Fraction			-0.377	
			(3.11)***	
Charter School, Fraction			-1.017	
			(2.32)**	
Magnet Enrollment, Fraction				-0.370
				(3.32)***
Charter Enrollment, Fraction				-1.215
				(2.91)***
PSC Measures				
Private Schools, Number	0.001	0.001	0.001	0.001
	(3.02)***	(2.88)***	(3.90)***	(4.64)***
Proportion of Catholic Schools	-0.035	-0.024	-0.024	-0.015
	(0.79)	(0.57)	(0.55)	(0.36)
Observations	7630	7630	7630	7630
Absolute value of z statistics in parentheses				
* significant at 10; ** 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. Standard errors clustered by school district..

Table 1-7: Basic Model Probit Marginal Effects with Alternative General PSC Measure: Current Year, Elementary Inclusive

	(1)	(2)	(3)	(4)
PSC Measures (General)				
Choice School, Dummy	0.023	0.038	0.047	0.063
	(1.08)	(1.58)	(1.45)	(1.86)*
Choice School, Number		-0.040		
		(2.51)**		
Choice Schools, Fraction			-0.116	
			(1.28)	
Choice Enrollment, Fraction				-0.191
				(2.09)**
Private School Supply				
Private Schools in County, Number	0.001	0.001	0.001	0.001
	(3.91)***	(4.23)***	(3.93)***	(4.08)***
Proportion of Catholic Private Schools	0.081	0.079	0.080	0.079
	(1.64)	(1.64)	(1.63)	(1.64)
Additional Model Controls				
African American	-0.082	-0.081	-0.081	-0.081
	(6.14)***	(6.09)***	(5.97)***	(5.94)***
Hispanic	-0.086	-0.085	-0.086	-0.085
	(7.85)***	(7.91)***	(7.84)***	(7.86)***
Income, Category Median ($K)	0.001	0.001	0.001	0.001
	(8.23)***	(8.22)***	(8.24)***	(8.26)***
Total Expenditures ($K)	0.004	0.003	0.003	0.003
	(0.94)	(0.67)	(0.78)	(0.71)
Free Lunch, Fraction (District)	0.078	0.095	0.086	0.089
	(1.23)	(1.49)	(1.36)	(1.41)
Med HH inc in SD, ($K)	-0.002	-0.001	-0.001	-0.001
	(2.30)**	(2.07)**	(2.15)**	(2.11)**
Observations	7630	7630	7630	7630
Absolute value of z statistics in parentheses				
* significant at 10%; ** 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. Standard errors clustered by school district.

Table 1-8: Basic Model Probit Marginal Effects with Alternative General PSC Measure: One-year Lags, All Grades

	(1)	(2)	(3)	(4)
PSC Measures (General)				
Choice School, Dummy	0.033	0.055	0.049	0.062
	(1.48)	(2.14)**	(1.59)	(2.12)**
Choice School, Number		-0.042		
		(2.58)***		
Choice Schools, Fraction			-0.079	
			(0.71)	
Choice Enrollment, Fraction				-0.142
				(1.44)
Private School Supply				
Private Schools in County, Number	0.001	0.002	0.001	0.001
	(3.82)***	(4.25)***	(3.63)***	(3.74)***
Proportion of Catholic Private Schools	0.073	0.066	0.071	0.067
	(1.53)	(1.44)	(1.51)	(1.42)
Additional Model Controls				
African American	-0.083	-0.082	-0.082	-0.082
	(6.15)***	(6.18)***	(5.98)***	(6.05)***
Hispanic	-0.085	-0.084	-0.085	-0.085
	(7.74)***	(7.75)***	(7.67)***	(7.66)***
Income, Category Median ($K)	0.001	0.001	0.001	0.001
	(8.28)***	(8.29)***	(8.30)***	(8.31)***
Total Expenditures ($K)	0.005	0.004	0.004	0.004
	(1.15)	(0.91)	(1.12)	(1.08)
Free Lunch, Fraction (District)	0.075	0.097	0.080	0.088
	(1.20)	(1.55)	(1.30)	(1.45)
Med HH inc in SD, ($K)	-0.002	-0.001	-0.002	-0.001
	(2.32)**	(2.12)**	(2.29)**	(2.24)**
Observations	7630	7630	7630	7630
Absolute value of z statistics in parentheses				
* significant at 10%; ** 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. Standard errors clustered by school district.

Table 1-9: Basic Model Probit Marginal Effects with Alternative General PSC Measure: Current Year, All Grades

	(1)	(2)	(3)	(4)
PSC Measures (General)				
Choice School, Dummy	0.025	0.040	0.038	0.050
	(1.24)	(1.78)*	(1.42)	(1.99)**
Choice School, Number		-0.03		
		(2.38)**		
Choice Schools, Fraction			-0.067	
			(0.71)	
Choice Enrollment, Fraction				-0.133
				(1.58)
Private School Supply				
Private Schools in County, Number	0.001	0.002	0.001	0.001
	(3.99)***	(4.26)***	(3.74)***	(3.88)***
Proportion of Catholic Private Schools	0.079	0.076	0.078	0.075
	(1.62)	(1.60)	(1.61)	(1.58)
Additional Model Controls				
African American	-0.082	-0.082	-0.082	-0.081
	(6.17)***	(6.16)***	(6.00)***	(6.04)***
Hispanic	-0.085	-0.084	-0.085	-0.085
	(7.79)***	(7.82)***	(7.74)***	(7.75)***
Income, Category Median ($K)	0.001	0.001	0.001	0.001
	(8.26)***	(8.25)***	(8.27)***	(8.30)***
Total Expenditures ($K)	0.004	0.003	0.004	0.004
	(0.99)	(0.76)	(0.93)	(0.90)
Free Lunch, Fraction (District)	0.074	0.091	0.079	0.086
	(1.16)	(1.41)	(1.25)	(1.37)
Med HH inc in SD, ($K)	-0.002	-0.001	-0.002	-0.002
	(2.35)**	(2.18)**	(2.30)**	(2.29)**
Observations	7630	7630	7630	7630
Absolute value of z statistics in parentheses				
* significant at 10%; ** 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. Standard errors clustered by school district.

Table 1-10: Extended Model (without State FE) Using Alternate General PSC Measures, Probit Marginal Effects

	PSC Measures: Current Year, Elementary Inclusive			PSC Measures: Lags, All Grades			PSC Measures: Current Year, All Grades		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
PSC Measures (General)									
Choice School, Dummy	-0.003	0.013	0.024	0.029	0.038	0.044	0.002	0.009	0.012
	(0.16)	(0.48)	(0.88)	(1.30)	(1.30)	(1.59)	(0.11)	(0.34)	(0.50)
Choice School, Number	-0.039			-0.042			-0.028		
	(2.86)***			(3.12)***			(2.65)***		
Choice Schools, Fraction		-0.141			-0.147			-0.096	
		(1.81)*			(1.55)			(1.18)	
Choice Enrollment, Fraction			-0.194			-0.180			-0.118
			(2.52)**			(2.10)***			(1.59)
Private School Supply									
Private Schools in County, Number	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(3.98)***	(4.05)***	(4.32)***	(3.83)***	(3.59)***	(3.54)***	(3.87)***	(3.74)***	(3.68)***
Proportion of Catholic Private Schools	0.058	0.057	0.056	0.052	0.055	0.051	0.059	0.059	0.059
	(1.52)	(1.52)	(1.49)	(1.40)	(1.50)	(1.38)	(1.55)	(1.57)	(1.56)
Observations	7630	7630	7630	7630	7630	7630	7630	7630	7630

Absolute value of z statistics in parentheses
* significant at 10%; ** 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. Standard errors clustered by school district.

Table 1-11: Extended Model with State FE Using Alternative General PSC Measures, Probit Marginal Effects

	PSC Measures: Current Year, Elementary Inclusive			PSC Measures: Lags, All Grades			PSC Measures: Current Year, All Grades		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
PSC Measures (General)									
Choice School, Dummy	-0.014	-0.002	0.010	0.007	0.011	0.014	-0.018	-0.018	-0.014
	(0.86)	(0.10)	(0.41)	(0.34)	(0.46)	(0.58)	(1.16)	(0.91)	(0.72)
Choice School, Number	-0.036			-0.033			-0.022		
	(1.88)*			(2.00)**			(1.60)		
Choice Schools, Fraction		-0.105			-0.084			-0.037	
		(1.23)			(0.96)			(0.48)	
Choice Enrollment, Fraction			-0.181			-0.105			-0.061
			(2.10)**			(1.29)			(0.84)
Private School Supply									
Private Schools in County, Number	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	(2.86)***	(3.04)***	(3.32)***	(2.85)***	(2.88)***	(2.87)***	(2.70)***	(2.80)***	(2.80)***
Proportion of Catholic Private Schools	-0.021	-0.032	-0.034	-0.026	-0.035	-0.036	-0.018	-0.026	-0.026
	(0.50)	(0.74)	(0.78)	(0.61)	(0.80)	(0.81)	(0.42)	(0.60)	(0.60)
Observations	7630	7630	7630	7630	7630	7630	7630	7630	7630

Absolute value of z statistics in parentheses
* significant at 10%; ** 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. Standard errors clustered by school district.

Table 1-12: Extended Model with State FE Probit Marginal Effects - Alternative Type-Specific PSC Measure: Current Year, Elementary Inclusive

	(1)	(2)	(3)
PSC Measures - Type Specific			
Dummy – Magnet	-0.007	0.033	0.033
	(0.39)	(0.89)	(0.94)
Dummy – Charter	0.016	0.034	0.058
	(0.53)	(0.80)	(1.37)
Magnet, Number	-0.034		
	(1.49)		
Charter, Number	-0.434		
	(0.85)		
Magnet Schools, Fraction		-0.231	
		(2.04)**	
Charter Schools, Fraction		-0.411	
		(0.93)	
Magnet Enrollment			-0.264
			(2.57)**
Charter Enrollment			-0.833
			(1.80)*
Private School Supply			
Private schools in County, Number	0.001	0.001	0.001
	(2.70)***	(3.34)***	(4.28)***
Proportion of Catholic Private Schools	-0.026	-0.028	-0.022
	(0.61)	(0.64)	(0.50)
Observations	7630	7630	7630
Absolute value of z statistics in parentheses			
* significant at 10%; ** 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. Standard errors clustered by school district.

Table 1-13: Extended Model with State FE Probit Marginal Effects - Alternative Type-Specific PSC Measure: One-year Lags, All Grades

	(1)	(2)	(3)
PSC Measures - Type Specific			
Dummy – Magnet	0.025	0.061	0.067
	(1.12)	(1.57)	(1.79)*
Dummy – Charter	0.005	0.001	0.049
	(0.18)	(0.04)	(1.41)
Magnet, Number	-0.039		
	(1.65)		
Charter, Number	-0.135		
	(0.29)		
Magnet Schools, Fraction		-0.200	
		(1.88)	
Charter Schools, Fraction		-0.079	
		(0.19)	
Magnet Enrollment			-0.207
			(2.18)**
Charter Enrollment			-1.790
			(2.87)***
Private School Supply			
Private schools in County, Number	0.001	0.001	0.001
	(3.07)***	(3.44)***	(3.76)***
Proportion of Catholic Private Schools	-0.027	-0.039	-0.023
	(0.65)	(0.90)	(0.54)
Observations	7630	7630	7630
Absolute value of z statistics in parentheses			
* significant at 10%; ** 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. Standard errors clustered by school district.

Table 1-14: Extended Model with State FE Probit Marginal Effects - Alternative Type-Specific PSC Measure: Current Year, All Grades

	(1)	(2)	(3)
PSC Measures - Type Specific			
Dummy – Magnet	-0.007	0.016	0.01
	(0.40)	(0.61)	(0.43)
Dummy – Charter	-0.004	-0.012	0.023
	(0.18)	(0.45)	(0.82)
Magnet, Number	-0.021		
	(1.02)		
Charter, Number	-0.115		
	(0.32)		
Magnet Schools, Fraction		-0.124	
		(1.34)	
Charter Schools, Fraction		0.178	
		(0.45)	
Magnet Enrollment			-0.105
			(1.25)
Charter Enrollment			-1.018
			(1.52)
Private School Supply			
Private schools in County, Number	0.001	0.001	0.001
	(2.71)***	(3.18)***	(3.33)***
Proportion of Catholic Private Schools	-0.023	-0.035	-0.023
	(0.54)	(0.81)	(0.53)
Observations	7630	7630	7630
Absolute value of z statistics in parentheses			
* significant at 10%; ** 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. Standard errors clustered by school district.

Table 1-15: Probit Model Marginal Effects Estimates, Type-Specific PSC Measures, Include HH Religious Participation and Religious Interaction Terms

	Base Model	Extended Model, State FE
PSC Measures - Type Specific		
Dummy – Magnet	0.060	0.064
	(0.87)	(1.02)
Dummy – Charter	0.084	0.076
	(1.12)	(1.21)
Magnet Enrollment, Proportion	-0.135	-0.230
	(0.73)	(1.72)*
Charter Enrollment, Proportion	-2.450	-1.952
	(3.51)***	(3.54)***
Interaction Terms		
Magnet Dummy*Religious	0.001	0.0001
	(0.06)	(0.01)
Charter Dummy*Religious	0.001	-0.003
	(0.05)	(0.31)
Magnet Enroll*Religious	-0.033	-0.021
	(0.85)	(0.74)
Charter Enroll*Religious	0.318	0.295
	(1.77)*	(2.31)**
Religious Participation Measure		
Frequency of Religious Services Attended	0.041	0.030
	(9.26)***	(9.45)***
Private School Supply		
Number of Private schools in Co., (10s)	0.001	0.001
	(3.91)***	(4.57)***
Proportion of Catholic Private Schools	0.083	-0.019
	(1.53)	(0.46)
Observations	7630	7630
Absolute value of z statistics in parentheses		
* significant at 10%; ** significant at 5%; *** significant at 1%		

Dependent variable: student attends private school. Number of private schools is measured in 10s. Explanatory variables for base and extended model with state dummy variables are also included, however, the coefficients are not presented here. Regarding Religious Participation Measure, this is a categorical measure indicating frequency of parental attendance in religious services collected in the first grade survey. Weighted estimates. Clustered by school district.

Table 1-16: Probit Model Marginal Effects Estimates, Type-Specific PSC Measures, Include HH Income Interaction Terms

	Base Model	Extended Model, State FE
PSC Measures - Type Specific		
Dummy – Magnet	0.067	0.100
	(1.49)	(1.65)*
Dummy – Charter	0.034	0.036
	(0.74)	(0.82)
Magnet Enrollment, Proportion	-0.302	-0.297
	(2.18)**	(2.30)**
Charter Enrollment, Proportion	-2.856	-1.588
	(2.79)***	(2.67)***
Interaction Terms		
Magnet Dummy*High Income	0.049	-0.004
	(0.75)	(0.10)
Charter Dummy*High Income	0.051	0.012
	(0.84)	(0.32)
Magnet Enroll*High Income	0.0001	-0.120
	(0.001)	(0.96)
Charter Enroll*High Income	1.766	0.787
	(1.69)*	(1.46)
Magnet Dummy*Mid-High Income	0.033	-0.016
	(0.71)	(0.60)
Charter Dummy*Mid-High Income	0.134	0.123
	(2.15)**	(2.47)**
Magnet Enroll*Mid-High Income	0.007	-0.059
	(0.03)	(0.47)
Charter Enroll*Mid-High Income	0.617	-0.420
	(0.70)	(0.97)
Private School Supply		
Number of Private schools in Co., (10s)	0.001	0.001
	(4.39)***	(4.76)***
Proportion of Catholic Private Schools	0.090	-0.013
	(1.80)*	(0.29)
Observations	7630	7630
Absolute value of z statistics in parentheses		
* significant at 10%; ** significant at 5%; *** significant at 1%		

Dependent variable: student attends private school. Number of private schools is measured in 10s. Explanatory variables for base and extended model with state dummy variables are also included, however, the coefficients are not presented here. Regarding Interaction Terms, "High Income" represents households making at least $75,000 annually. "Mid-High Income" represents households making from $40,000 to $75,000 annually. Weighted estimates. Clustered by school district.

Table 2-17: Predicted Probabilities for Attending Private School. Simulation using Specific Values for the Number of PSC Schools in a District.

	Number of Type-Specific PSC Schools in District			
	0	1	5	25
District PSC Composition				
Districts with both charter and magnet schools*	0.448	0.465	0.451	0.383
Districts with magnet schools only	0.448	0.456	0.455	0.451
Districts with charter schools only	0.448	0.456	0.443	0.380

** For districts with both charter and magnet schools, the total number of PSC schools across the four columns is zero, two, ten and 50. Note that when varying the number of PSC schools in a district, the number of schools in a district is not controlled for. All PSC measures are representative of one-year lagged data and only represent elementary-inclusive schools. A probability of 1 indicates a household sends their child to private school.*

Table 2-18: Predicted Probabilities for Attending Private School. Simulation using Specific Values for the Fraction of District Enrollment Attending a PSC School.

	Fraction of District Enrollment in Type-Specific PSC School			
	0%	1%	5%	25%
District PSC Composition				
Districts with both charter and magnet schools*	0.432	0.493	0.479	0.371
Districts with magnet schools only	0.432	0.465	0.459	0.429
Districts with charter schools only	0.432	0.458	0.436	0.343

** For districts with both charter and magnet schools, the fraction of district enrollment in PSC schools across the four columns is zero, five, ten and 50. Thus in the second column, magnet and charter schools each make up 2.5% of district enrollment (rather than 1%) when both school types are present. The number of schools in the district is always set at 100. All PSC measures are representative of one-year lagged data and only represent elementary-inclusive schools. A probability of 1 indicates a household sends their child to private school.*

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2. CHAPTER 3: Primary School Students and the Demand for Intra-district, Interdistrict, and Private Schools

2.1 Introduction

Although public school students traditionally attend their residentially assigned schools, the advent of charter schools, the progress of open enrollment policies, and the passage of No Child Left Behind Act of 2001 (NCLB) allow students, depending on their special needs or talents, to be assigned elsewhere. Such changes, along with additional official and unofficial means of sending students to public schools outside their catchment areas or even outside their districts, have relaxed the ties between household residence and public school attendance. What factors appear to play a role in the displacement of public school students outside their residentially assigned schools? How do such factors affect the appeal of a household's classic substitute for their neighborhood school – a private education?

Students who opt into schools that are within their district but are not their residentially assigned schools, referred to as *intra-district choice*, generally do so to attend a magnet school or program, a charter school, or another school with an open enrollment policy**.** Also, students with special needs or talents may be more inclined and more frequently permitted to partake in programs offered by another school within the district but not residentially assigned. The phenomenon of households engaging in intra-district school choice potentially affects classroom composition and can possibly alter the demand for services and educational programming across schools within a district. In particular, intra-district open enrollment policies have been found to exacerbate student stratification within a district (Cullen et al. 2005; Hasting et al. 2009; Bifulco et al. 2009), and magnet schools have been shown to have similar effects (Saporito 2003; Weiher and Tedin 2002).

Now consider students enrolled in public schools outside their district lines, referred to as *interdistrict choice*. This is potentially a more extreme and costly public school option than an intra-district alternative, though it need not always be. Households that engage in interdistrict school choice either do so within regulation of transfer policy or outside of

regulation.[49] Both scenarios present interesting implications. First, for students who transfer in accordance with regulation, generally their allotted expenditures are transferred to the receiving district. However, households who opt to circumvent regulation when enrolling their students in another district may ultimately free ride on the publically provided education they consume, straining the school district's resources. Second, households that opt out of district have likely exhausted their public school options within district as well as private school alternatives. The literature has shown that the displacement across district lines because of open enrollment policies further stratifies students across districts, while resident profiles across districts are incentivized to become more heterogeneous (Brunner, Cho, and Reback 2010; Cullen et al. 2005). This outcome, as with that of intra-district choice, tends to give way to a variety of student-school matches: students better fit with programs that meet their needs or gifts, somewhat disenfranchised students left behind by their higher-achieving exiting peers, and students who opt into schools and/or districts where their racial or ethnic likeness is more predominantly reflected by their peers (Armor and Peiser 1997; Bifulco et al. 2009; Cullen et al. 2005; Fossey 1994; Hasting et al. 2009; Holme and Richards 2009; Reback 2005; Welsch et al. 2010).

The phenomenon of students opting out of district affects student composition in the classroom and expenditures and revenues per child. It can potentially alter the demand for services and programming across districts, and can theoretically lead to competition across districts for students. Thus with a variety of relevant consequences to explore, I examine the demand for intra-district and interdistrict enrollment, and I extend the analysis to include the demand for private school to complete the set of institution-based schooling options. (Thus I do not explore the home schooling alternative here.) In particular, what educational policies as well as student, household, and school district-level attributes contribute to the household's decision to opt out of their residentially assigned public school?

Open enrollment policies represent perhaps the most significant source of displacing students in the public sector. Essentially, these policies allow students to choose their desired school, subject to enrollment constraints and other regulatory mandates, either within their school district (intra-district) or outside of their residentially assigned school district

[49] Certainly individual circumstances are bound to arise that do not neatly fit within established interdistrict transfer policies. In such cases the discretion of the relevant districts or some higher party comes into play.

(interdistrict). As of 2010, 46 states had some sort of open enrollment policy.[50] Interdistrict open enrollment policies may be mandatory, requiring districts to participate – subject to constraint, or voluntary, allowing districts to option to participate. Charter schools – public schools of choice that are not subject to all the rules and regulations of traditional public schools – also tend to invite students to cross district borders. Another reason for attending a public school outside the district is that the NCLB has granted options to students who are residentially assigned to persistently under-performing schools: in some cases they may select to transfer out of district. And with respect to intra-district school choice, magnet schools, many open enrollment policies, and also NCLB generally offer students a means of enrolling in an alternative public school while retaining students in their residentially assigned districts.

Certainly other factors play a role in a student's decision to opt out of their residentially assigned public school. Student attributes such as special needs or gifts, parents who are teachers, and recent relocations may prompt some households to enroll their children outside their assigned school, even outside their home district. Furthermore, race, income and parental education classically play roles in decisions related to school choice. Thus, I incorporate the student, household, and district level measures assumed to be related to this household decision and estimate the demand for intra-district enrollment, interdistrict enrollment, and private schooling.

I find evidence to suggest that households substitute between intra-district and interdistrict schools of choice. In states with mandatory interdistrict open enrollment policies, households are 3 percent, on average, less likely to attend a within-district public school of choice rather than their residentially assigned school, and these households are approximately 3 percent, on average, more likely to opt out of district. While I find no effect of residing in a charter state on the household decision to pursue intra-district or interdistrict public school options rather than attend one's assigned public school, I do find a subtle effect suggesting such households are less likely than students in non-charter states to opt into private school by an average of 4 percent, all else equal. I also find that mobility patterns significantly affect this household decision. It should be noted that this model assumes household relocation decisions are exogenous, and are thus independent of their schooling decision. Students whose households have moved out of the census tract they resided in two

[50] Statistic comes from NCES Website state-specific report on open enrollment policies. Available at: http://nces.ed.gov/programs/statereform/tab4_2.asp.

years earlier, as opposed to students who have not moved, are more likely to opt into either a within-district public school of choice or opt out of district rather than attend their residentially assigned school, by averages of 2 and 6 percent, respectively. And this recent mobility reduces the probability that students attend private school by an average of 3 percent. Conditional on the type of move, however, only the relocation effect on the decision to opt out of district rather than attend one's assigned school is statistically significant.

I also find marginally significant effects that suggest that students with a diagnosed disability on record at their schools are slightly more likely to opt out of district rather than attend their residentially assigned school. However, a number of additional appropriate controls regarding the disability of the student (i.e. type and severity) and the services provided at the school or district level not controlled for in the model. And finally, I find that having a parent who teaches in non-postsecondary education affects this household decision; I find marginally significant evidence to suggest that such households are more likely to opt either into a within-district public school of choice or out of district rather than attend their residentially assigned school – at averages of 3 and 2 percent, respectively.

In the following section I briefly discuss the most relevant literature that examines the demand for intra-district and interdistrict public schooling, and I explain how this chapter 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.

2.2 Contribution

The majority of research in the economics literature that addresses why households go outside their district to meet their student's educational needs is rooted in the niche of studies that focus on the effects of interdistrict open enrollment policies (Armor and Peiser 1997; Fossey 1994; Henig 1990; Holme and Richards 2009; Reback 2008; Schneider and Buckley 2002; Welsch et al. 2010). Indeed these policies, theoretically, do the most to expand the breadth of public schooling options beyond a household's residentially assigned district. Perhaps research on charter schools constitutes a secondary strand of the literature that addresses the demand for public educational services outside one's home district. With charter schools, the question is more subtle (Bifulco and Ladd 2007; Hanushek et al. 2007;

Weiher and Tedin 2002). As researchers examine what types of households tend to choose charter schools, that subtlety lies in the extent to which opting for a charter school equates crossing a district border. Other school choice research, in particular research that examines the demand for private schooling (Long and Toma 1988) or magnet schools (Saporito 2003), fall even further short of addressing the research question of why households opt out of district – as private schools operate outside the bounds of a school district, and magnet schools, under the jurisdiction of their districts, are theoretically designed to have more influence on intra-district school choice than on interdistrict school choice.

Studies that focus on intra-district open enrollment policies (Cullen et al. 2005; Hasting et al. 2009; Bifulco et al. 2009) as well as studies that explore magnet school enrollment and effects (Saporito 2003; Weiher and Tedin 2002) most succinctly address the demand for aspects of intra-district enrollment. However, a number of interdistrict options also by default extend themselves to offering intra-district options to students, that is, charter schools located in their district of residence and aspects of NCLB. Indeed quite a variety of factors may contribute to intra-district choice. Traditionally, studies do not look collectively at the various policies and institutions that contribute to intra-district options.

As perhaps an aside, another interesting implication with respect to open enrollment policies worth noting is that by relaxing the constraint on residential location and school quality, households have incentives to move to areas with cheaper costs of living – increasing the rate of students who opt out of their assigned school not by inspiring a change in schools but rather by inspiring a change in residence (Cullen et al. 2005).

It is unlikely that households opting out of their catchment area schools – whether to attend private schools, charter schools, magnet schools, or schools in other districts – reflect a homogenous demand for schooling options across households and a heterogeneous supply of schooling service across the districts and counties where they live; indeed the literature shows no support for that contention. Although the literature provides a great deal of research regarding the demand for various schools of choice, the extent to which this research area addresses the demand for intra-district choice and interdistrict choice in broad terms is limited. For research on cross-district enrollment, the literature is generally limited to open-enrollment policy studies, most of which use aggregate level analyses and concentrate their studies on one particular geographic area. In part, my contribution is to further examine the demand for interdistrict educational services looking beyond the impacts

of a particular education policy and more fully incorporating the attributes of the household and the district in which students live. I also use micro level data to do an individual-level analysis employing a multistate dataset, rather than employing an aggregate level analysis or focusing on one geographic area. Additionally, the economic education literature often focuses on magnet school and intra-district open enrollment policies to explore intra-district choice. My study again contributes to the literature by broadening the scope of intra-district choice beyond one type of school or a specific policy. Also, I know of no other assessment of school choice that incorporates both intra-district and interdistrict schools as independent options.

Of the studies that examine the demand for interdistrict transfers under open enrollment policies, researchers have found that economic status, race, and parental education seem to be among the household variables that influence the decision to opt out of district, and test scores and expenditures per child in the districts students opt into appear to be key district level variables. Recent work by Welsch et al. (2010) and Reback (2008) examine the demand for interdistrict transfers in states with interdistrict open enrollment policies. In a study of Wisconsin school districts, Welsch et al. find that white students in households with higher socioeconomic statuses tend to be more likely to transfer out of district than students in minority and lower socioeconomic status households. Reback finds that households in Minnesota respond more to higher standardized test scores than to the socioeconomic status of the district's residents when transferring into other districts and also finds higher rates of cross-district transfers among students with disabilities and students of lower socioeconomic status. Looking at the role of interdistrict open enrollment policies in the metropolis of Denver, Holme and Richards (2009) also find upper socioeconomic status white households to be more likely to opt out of district; they tend to leave communities that were less affluent to attend schools in better communities. Fossey (1994) reports that among the most commonly cited reasons for opting out of district are academics or family convenience.

Intra-district open enrollment policies in Charlotte, North Carolina, reveal similar findings; students from higher socioeconomic status homes tended to opt into schools with higher test scores, and lower socioeconomic households appeared to prefer schools with higher rates of minority students (Hastings et al. 2009). Bifulco et al. (2009) also find that students from higher socioeconomic status households are more likely to participate in intra-

district open enrollment programs than students from households with poorer socioeconomic statuses. Consistent with other findings, Bifulco et al. also find that among these more affluent and educated households that transfer out of their assigned school, test scores in the schools students opted into appeared to be a motivating factor.[51]

While these studies do not necessarily reflect interdistrict or intra-district choice, charter and magnet school studies tend to find much of the same type of evidence supporting that schools of choice exacerbate student stratification. Studies that examine the demand for charter schools find that students who attend them tend to opt into schools with higher concentrations of their own race or ethnicity compared with their former schools (Bifulco and Ladd 2007; Weiher and Tedin 2002). Studies tend to find heterogeneity across race and ethnic lines regarding the importance of student test scores in charter schools. Bifulco and Ladd (2007) find African Americans to be more likely to choose charter schools that have a higher percentage of African American students, even at the expense of their educational achievement; whereas white students tend to opt into charter schools with higher student test scores. Saporito (2003) finds similar results on racial and socioeconomic stratification facilitated through magnet school intra-district choice. In the conclusion section of this chapter, I reflect on how my findings effectively compare with the literature presented here.

2.3 Data

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.[52] 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

[51] Bifulco, Ladd, and Ross (2009) note that higher intra-district transfer rates may be found among elementary school students than among students in higher grade levels. Typically elementary schools are more numerous within a district than middle or high schools – providing more choice for households within district. Thus one may presume that inter-district transfer rates may be smaller among elementary school students than higher grade levels.

[52] Refer to Chapter Two for further details on the ECLS-K data.

parents. As in Chapter 2, 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. Recall that 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 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.

As with the previous chapter, 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 that includes data on grade range and student demographics. I also supplement with data from the Census 2000 Summary 3 Files as well as the Census 2000 School District Tabulation (STP2), which were procured from the NCES's School District Demographics System; these datasets provide information about demographics and the socioeconomic statuses of residents at the census tract level and school district level, respectively. Finally, I supplement the dataset with Census 2000 Cartographic Boundary Files at the school district and census tract levels. These files are a form of geographic information systems (GIS); they are spatial data represented by maps of geographical areas and borders.

To identify and profile the districts that students reside in as well as the districts that public school students are enrolled in, I match the geographic identifiers in the ECLS-K data with the subsequent components listed above. A brief discussion of this process follows.

2.3.1 *Data Sources*

2.3.1.1 ECLS-K

As mentioned above, the ECLS-K survey is administered about every other year, including the students' expected first-, third-, and fifth-grade years. I use the third-grade

sample; this is the last year in the restricted data that the residential census tract for the student is reported. I drop students who repeated or skipped grades or those lacking valid school identifiers and residential census tracts. The sample is reduced from 15,300 to 11,160 observations.[53] For these remaining students, I collect student-level and household-level data. The variables collected include the student's gender, age, race, disability status, whether the student relocated to a different census tract since the last interview (spring of the first-grade year), whether the student attends a school other than their residentially assigned school, and whether the student attends private school. At the household level, I collect an income measure, the size of the household, parental structure indicators (i.e., two-parent or single-parent households), parental employment, educational attainment, mother's age, and whether either parent is a teacher in non-postsecondary education. Additional geographic explanatory variables include urban/rural status and regional controls. Because the child's state of residence is identified in the restricted data, I supplement the ECLS-K data using outside sources that account for a states' open enrollment policies and charter status for this time period.[54] I control for whether the household resides in a state with mandatory interdistrict open enrollment as of 2001 and whether the student's state of residence was a charter state as of 2000.[55]

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.

2.3.1.2 2000 Decennial Census Data

It is important to note that the 2000 Decennial Census data correspond with the 1999-2000 academic year, the first-grade year for students in the sample. Although these

[53] In compliance with the NCES's restricted data regulations, all sample sizes reported have been rounded to the nearest 10.
[54] I assign states a mandatory interdistrict open enrollment classification as of 2001 according to the Joint Committee on Education Report (2009).
[55] I assign states a charter classification as of 2000 according to Nelson et al. (2000) and www.chartschools.org. Charter state classification is lagged one year because new charter states are unlikely to be comparable with charter states that have already had at least a year to develop charter schools. I also estimate an alternate charter school specification that incorporates any state that passed charter legislation in 2001. I comment on this further in the results section.

measures represent data two years prior to the third-grade academic year of the sample, they are unlikely to have changed substantially by the time the students are in third grade. The variables I create using the Census data perform two functions: they residentially assign students to a public school district and provide additional information at the census tract level. In particular, for summary statistics purposes I collect the fraction of students in grades K-12 living in a given census tract that attend private school.

To match students to a public school district based on their place of residence, I employ a GIS matching process. Using the Census 2000 Cartographic Boundary Files at the census tract level and the school district level, I map the school districts within a state and then overlay that state's census tract map using compatible mapping specifications – just as I did in the preceding chapter. Census tracts that fall geographically within a given school district are residentially assigned to that district. The final sample consists of 7,650 ECLS-K student observations for which I successfully match census tracts with school districts based on this mapping exercise. This sample size reflects approximately 69 percent of the third-grade sample with valid census tract identifiers; I address how this matching affects the composition of the final sample in the next section.

The Census 2000 School District Tabulation (STP2) files, from the NCES's School District Demographics System, provide demographic and socioeconomic resident profiles at the school district level. I further supplement the dataset by including residential characteristics at the school district level to control for various community attributes associated with the entry of private schools (Barrow 2006) and public schools of choice (Glomm et al. 2005).

2.3.1.3 Common Core of Data & Form F-33

I use the 2000-2001 CCD data to create one-year lags for the school district variables, with the exception of the number of schools in the district. This measure reflects the current academic year (2001-2002). Lagging the school district variables is done to avoid any potential endogeniety concerns. All district-level variables are collected and/or calculated for the school district in which the student resides.

I calculate the school district variables to describe the district itself and to profile the district's average, typical school. 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), and the fraction of Title I eligible schools in the district. Additionally, I

supplement from the Census of Governments F-33 survey for the 2000-2001 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 district averages; they are not weighted or non-weighted school means. These 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. Not all of these variables are included in the analysis because the model becomes too restrictive for the sample size. However, they are included in the summary statistics for the purpose of presenting a more complete profile of the students sampled.

2.3.1.4 Private School Universe Survey

Because the supply of private schooling may affect the household's decision to opt out of district, and it most certainly affects the decision to attend private school, I use the PSS data from the 2001-2002 academic year to create measures of private school supply near students' residences. Specifically, I tabulate the number of private schools in the student's county of residence, and I control for the proportion of Catholic private schools in the county. I do not lag the private school supply measures because the data do not include enrollment counts or student qualities that could be construed as endogenous.

2.3.2 *Data Limitations*

An important limitation of my data is that no test scores are available across the school districts. The relevant literature finds households to be influenced by the test scores of students in the districts or schools they opt into (Bifulco and Ladd, 2007; Reback, 2008); thus ideally I would include it as a measure of school quality. To the extent that tests scores are correlated with socioeconomic measures of a district's residents and students, the inclusion of district-level demographic and socioeconomic measures effectively controls for test scores. However, while they are sure to be somewhat correlated, district level affluence measures is not the ideal measure of school quality. Further complicating my effort to tie test scores with the socioeconomic status of school district residents is the previously mentioned open-enrollment policies, which now give households the incentives to move to districts that have lower costs of living and poorer school quality. This compounds the latent

heterogeneity in school quality and student performance within districts. Thus, ideally, I would control for the test scores among the students in the district.

Another data limitation comes from the ECLS-K's variable that identifies the urban, suburban, or rural residential location of the household. In the final dataset, 2 percent of the sample is missing this indicator variable. However, an anomaly surfaces among these observations. While the final sample used will ultimately reflect that 4 percent of households opt out of district, 20 percent of the students that are missing their urban/rural status are observed to opt out of district. To address this, I take a two-step approach to assigning an urban, suburban, or rural status to those households for which it is missing. The first step is to assign these households the rural/urban status of other households that are observed to reside in the same census tract, a geographic unit with approximately 4,000 people. The second step applies to any household for which either no other observed household resides in the same census tract with a valid urban/rural status to compare with, as well as any household for which multiple other households residing in the same census tract report conflicting urban/rural statuses. For these households, I use another method to impute the measure. The 2000 Census Summary 3 files report the urban and rural population for all U.S. census tracts. I assign urban, suburban, and rural status across the remainder of the observations according to the following criteria: households in census tracts that report an urban population of 5,000 or more (and the rural population makes up less than 20 percent of the census tract) are assigned the status of urban; households in census tracts where the urban population is less than 5,000 (and the rural population makes up less than 20 percent of census tract) are assigned the status of suburban; households in census tracts where at least 20 percent of the population is reported to reside in rural areas – regardless of the population size in the urban areas – are assigned the status of rural. When the model is run, I include the imputed urban/rural measure that assigns all observations either their reported or an imputed urban/rural status, and I include a dummy variable to indicate whether the urban/rural status was initially missing in the ECLS-K data.[56]

[56] Estimates occasionally reveal that the coefficient on the indicator variable for missing urban data is statistically significant. Excluding these observations generally does not change the estimates of the remaining coefficients; thus I include all observations in the analysis. Occasionally this occurs with other missing category indicator variables; again, the removal of the observations in which the data were initially missing tends not to affect the model's estimates. Thus I include all observations.

2.3.3 *Construction of Specific Variables*

I use the ECLS-K data in conjunction with the GIS data to construct the dependent variable. In particular, I assign the outcome variable categories as follows. Students who report to attend a private school in the ECLS-K survey are assigned private school as their outcome. Students who report attending public school in a district other than the district in which they are residentially matched to using the GIS matching process are assigned the outcome of opting out of district. While it may very well be that students are assigned to a school in another district, such an outcome is suppressed in the specification of this variable. For the remaining two schooling options, I consider only those students in public school attending schools within their residentially assigned district, and I separate them into two groups. Those that report choosing their school are assigned the outcome of attending a within-district public school of choice. And those that report attending their assigned school are assigned the outcome of attending their residentially assigned, or catchment area, school.[57]

Measurement error is a concern in the construction of this variable. Approximately 10 percent of the households with students attending private school report that their children attend their assigned school rather than a school of choice. Additionally, more than 50 percent of the households observed to attend public school out of district report sending their children to their assigned schools. To address the conflicting responses, which could not be readily reconciled, I prioritized the assignment of the dependent variable as discussed in the preceding paragraph. The implications are that 10 percent of the students observed in the analysis as private school students also report attending an assigned school. Additionally, just more than half of the students observed in the analysis to attend public school out of district report attending their assigned school. Thus I cannot distinguish between households that are sending their children out of district under the radar (households falsely claiming to live in the district where the student attends school) from households who may be assigned schools in other districts for some reason.

[57] Assigned schools can also be a *choice school*. The variable in the ECLS-K survey asks households to identify whether their child attends their (1) assigned school, (2) a choice school, or (3) an assigned school that is also a choice school.

I considered various approaches to address how to correctly account for the dependent variable across observations. I tried reprioritizing the variables to assign students an outcome consistent with whether they reported to attend a chosen school or their assigned school (regardless of whether they were observed to opt out of district) as well as dropping all observations with conflicting responses. Neither of these strategies resulted in a sufficient number of observations across the various outcomes to estimate the model as specified. This is likely because only 230 students are observed to attend public school outside of district; this number drops significantly by either of these alternative methods of construction.

Another variable that required construction is the indicator variable for whether the student recently moved. This variable was constructed by comparing the residential census tract of the student in their third-grade year to that reported in their spring first-grade survey. The census tract identifier includes the state, county, and specific census tract; thus a difference from one census tract to another can be because of a relocation within the same county but into a new census tract, a move to a new county within the same state, or a move to a new state. For any intra-census tract moves, no mobility is observed.

2.3.4 *The Sample*

The final sample that I use in my analysis consists of 7,650 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 11,160 observations; approximately 69 percent of the sample that undergoes the GIS matching is successfully matched to a residential school district. Table 3-1 summarizes the sample before and after the GIS matching. Because missing categories for variables are created for the analysis, I do not omit student observations with incomplete information. Thus, summary statistics do not necessarily represent a balanced panel — the means represent only valid observations, so the number of observations across descriptive variables can vary. Variables denoted with asterisks have statistically unique means across the samples. Slightly fewer controls are included in the analysis than are presented in the summary statistics because of

two issues: one is collinearity across regressors and the other is that to include all of the independent variables would create a highly restrictive model for this sample size.

Table 3-1 presents the weighted summary statistics for student, household, and private school supply (and demand) measures broken down by category. I do not present the summary statistics for most of the outcome categories – the percentages of students attending a within-district school of choice, opting out of district, or attending their residentially assigned school. The construction of these outcome measures depends on identifying the student's residentially assigned school district, and this not possible for unmatched students. However, I can identify private school students regardless of a successful GIS matching process; thus I present this summary statistic at the top of Table 3-1. The percentage of students attending private school in the matched sample is 13 percent, while 12 percent of the unmatched sample is enrolled in private school. Although a statistically significant slightly higher concentration of private school enrollment appears in the final, matched sample, the proportions are comparable across samples and are similar to the national average of 12 percent private school enrollment for pre-kindergarten through eighth-grade students in 2001.[58]

The key variables of interest in this analysis are a few variables expected to displace students in the public sector; these measures are also presented at the top of Table 3-1. They include indicator variables for whether the student's state of residence had a mandatory interdistrict open enrollment policy as of 2001, whether the student resides in a state that had a charter legislation as of 2000, whether the student has moved in the last two years, whether one (or both) of the student's parents is a teacher in non-postsecondary education, and whether the student has a disability on record with the school.[59] Looking at the mean values for these variables across the final sample and the larger, unmatched sample, only the geographic measures are statistically different across the two groups, although the differences appear insubstantial. Twelve percent of the final sample and 14 percent of the larger, unmatched sample reside in a states with mandatory interdistrict open enrollment

[58] National private school enrollment estimates come from NCES and can be found online at http://nces.ed.gov/fastfacts/display.asp?id=65.

[59] Because of conflicting reports and limited data on statewide intra-district open enrollment policies, I do not include controls for intra-district open enrollment policies here, although it would be a welcome addition to the analysis. A contributing factor is that many intra-district policies may not be state-specific but rather district-specific. This increases the challenge of collecting the data to properly control for such policies.

polices. A slightly higher concentration of households residing in charter states appears in the final sample than in the unmatched sample – 85 and 84 percent, respectively. The remaining key variables, which are perhaps more personal than geographical in nature, do not have statistically different means across the two samples; this trend generally holds across the remainder of the variables present in Table 3-1. Of the final and unmatched samples, 21 percent of students have relocated to a new census tract since their last interview two years prior; 6 percent of students have a parent who teaches in non-postsecondary education; 27 percent of students have a diagnosed disability on record at their school.

Regarding the student and household level attributes, again very little is statistically different across the two samples. As depicted in the preceding chapter, the final, matched sample has higher concentrations of minority and lower socioeconomic households, and the largest distinction across samples is geographically inspired. In the final sample, 18 percent of students are African American; 15 percent of the larger third-grade sample are African American. Although 55 percent of students in the final sample are white, 58 percent of the larger third-grade sample are white. Additionally, students in the final sample are slightly more likely to be in single-parent homes and are more prone to being in poverty than students in the larger third-grade sample. Regarding the regional distinctions across samples, higher proportions of students in the final sample reside in the Northeast or South compared with students in the larger, unmatched sample. Lower proportions of students reside in the West or Midwest in the final sample, as opposed to the larger, third-grade sample. The remaining student level and household level attributes are generally equally represented across samples. I discuss these variables at more length later in this section when I decompose the final sample across the outcome categories.

For both the matched and unmatched samples, I also present weighted summary statistics for measures of the supply of private schooling in the student's county of residence as well as a measure that addresses the demand for private schooling in a student's immediate living area – the student's census tract. 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, 12.15 percent versus 11.74 percent; this difference in means is statistically significant. Recall, this measure, collected from the 2000 Census, reflects the 1999-2000 academic year. The remaining private school supply variables are measured at the county level and represent the 2001-2002 school year. Though the average

number of private schools in a student's county of residence is not statistically different across samples, evidence shows that students in the matched sample tend to live in counties with slightly larger private schools and, on average, are slightly more exposed to Catholic private schools in their county of residence than students in the unmatched third-grade sample.

Table 3-2 presents the weighted student and household level summary statistics as well as private school supply (and demand) measures for the final sample broken down by the four outcome categories: (1) the default decision – students attending their assigned, within-district public school, (2) students attending a public school of choice in their residentially assigned district, (3) students attending a public school outside their residentially assigned school district, and (4) students attending private school. The variables denoted with asterisks indicate statistically significant differences in the means compared with the default category. Looking first across the columns, one can see that of the four different outcomes, 70 percent of students attend their residentially assigned public school; 12 percent attend a within-district public school of choice; 4 percent opt out of district; 13 percent attend private school.

With respect to the key variables of interest, students who opt out of district are more likely than students who attend their assigned school to live in a state with a mandatory interdistrict open enrollment policy – perhaps not a surprising statistic. And interestingly, students that attend private school are less likely than students that attend their assigned school to live in a state with charter school legislation. Across the four subsamples, one can see that recent mobility patterns vary across the outcome categories; students who attend a public school alternative are more likely to have recently relocated than students who attend their residentially assigned school; in particular; students who opt out of district are more than twice as likely to have recently moved than students attending their residentially assigned school. Private school students, on the other hand, are less likely than students in any of the public school outcomes to have recently moved. Students who attend their assigned school are less likely than students in any of the alternative outcomes to have a parent who is a teacher in non-postsecondary education. And students attending their neighborhood school are less likely to have a diagnosed disability on record with their school than are students who attend a public school out of district.

With respect to the student-specific qualities, race appears to be a distinguishing feature across the sample of students that opt into a within-district public school of choice – these students are more frequently African American or some other race, and are less likely to be white than students attending their residentially assigned school. Students opting into a within-district public school of choice also appear to be slightly younger, on average, than students who attend their residentially assigned school. On the other hand, the sample of students who attend their assigned school and the sample of students who opt out of district appear very similar – with the exception of a discrepancy in the probability students in these groups recently changed schools. However, this statistic is consistent with the higher rate of residential mobility among the sample of students who opt out of district. And finally with respect to students attending private school, these students are more likely to be female or white, and are less likely to be African American, Hispanic, have an individualized education program (IEP), or have changed schools recently – compared with the students who attend their assigned schools.

Regarding the household attributes, overall, little notable difference is seen across students who opt into a within-district public school of choice and students who attend their residentially assigned school; however students who opt into public schools of choice within their district tend to come from slightly smaller households with younger parents. Additionally, students who opt into a within-district public school of choice tend to be slightly less likely to be in poverty than households who send their children to their assigned school. These differences also hold for students who opt out of district, with the exception of the discrepancy across poverty levels. Additionally, students who attend public school in an alternative district are more likely to come from a single-parent home, have an employed mother, and have a father with some college compared with students who attend their assigned school. Consistent with theory and the economics of education literature, the sample of students in private school are more likely to come from smaller, two-parent households that reflect a stronger socioeconomic profile – in terms of parental education and household income – compared with students attending their residentially assigned school.

The geographic differences across subsamples imply that students attending within-district public schools of choice and students attending private school are more likely to reside in urban areas compared with students attending their assigned school. Students who

opt out of district are more likely to reside in rural areas than students attending their residentially assigned school. Finally, the regional differences are somewhat muddled. Students who attend a within-district public school of choice are more likely to reside in the West and less likely to reside in the South or Northeast compared with students attending their residentially assigned schools; students who opt out of district are also less likely to reside in the Northeast, but are more likely to reside in the South compared with students attending their assigned school; students attending private school are more likely to reside in the Northeast or Midwest and less likely to reside in the South compared with students attending their assigned school.

Recall that the student level information is linked to census tract and county identifiers. Thus, in the bottom section of Table 3-2, I present summary statistics regarding the supply (and demand) of private schooling at the county (and census tract) level for all four subsamples. I find that students who opt into either a within-district public school of choice or attend private school tend to live in census tracts where a higher proportion of primary and secondary students attend private school, and tend to live in counties with more private schools that more frequently have at least one Catholic school compared with students who attend their assigned school. Interestingly, every private school measure is less concentrated among the sample of students who opt out of district compared with students who attend their residentially assigned school.

I also present weighted summary statistics for the school districts represented by the four subsamples, found in Table 3-3. Students attending within-district public schools of choice – as opposed to students who attend their assigned school – are more likely to reside in larger school districts with higher student-teacher ratios, and higher percentages of minority students and students eligible for free lunch. These students' districts also tend to have higher average levels of federal and state revenues per student, while their average local revenues are slightly lower than districts resided in by the sample of students who attend their assigned school. Little is statistically different across the residentially assigned school districts of the sample of students who opt out of district and the sample of students who attend their assigned school. What differences there are appear to be rooted in the size of the district and in student expenditure and revenue patterns. Students who opt out of district are more likely to live in districts with fewer schools compared with students who attend their assigned school – by about half. And students who opt out of district are more likely to live

in districts with smaller levels of per-pupil spending and less local revenue per student than students who attend their assigned school. And perhaps unsurprising, students who attend private school live in larger districts with more minority students and fewer white students; they also have higher percentages of Title I eligible schools and students eligible for free lunch compared with the school districts of students who attend their assigned school. Additionally, private school students generally have higher levels of spending per student, and higher federal and local revenues per student in their residentially assigned districts compared with students who attend their assigned school.

In the bottom portion of Table 3-3, I present a few demographic and socioeconomic measures representative of the residents of these students' districts. Recall that these measures reflect 2000 Census data, two years prior to the academic year of the sample. The average student who attends a within-district public school of choice is more likely to live in a district with lower median income levels, higher poverty rates, and higher minority rates compared with students who attend their assigned school. The typical school district resided in by students who opt out of district is fairly similar to the districts of students who attend their assigned school, though students who opt out of district tend to live in school districts with a lower median household income and fewer adults with higher levels of educational attainment. And overall the typical districts represented by private school students and students attending their assigned school do not appear distinctive, although a slightly higher concentration of minority residents is found among the private school students' school districts.

2.4 Empirical Model

Because estimating the probability of opting out of one's assigned school to attend another public school alternative ignores the decisions made by private school students and potentially introduces a sample selection bias into the model, I instead estimate the probability of opting out of one's assigned school when the option of private school is an allowable alternative. I employ a multinomial probit model that is not restricted by the independence of irrelevant alternatives assumption and does not rank the possible outcomes as would an ordered model.[60] This model essentially assumes that households pursue a

[60] I also estimate this household decision as a multinomial logit. The coefficients – in magnitude and statistical significance - are similar to those of the multinomial probit.

particular school rather than a particular education sector. I estimate the model depicted by equation 1.

[1] $$Pr\,[Y_i=1, 2, 3] = f(H_i, P_c, D_j, R_j)$$

where *i* represents a household or student, *j* indicates a school district, *c* represents a county, and *s* represents a state. In the model, **Y** is an indicator variable taking the value of one if student *i* attends a public school within district that is not their residentially assigned school; **Y** takes the value of two if student *i* attends public school outside of the residentially matched district; **Y** takes the value of three if student *i* attends private school. Looking categorically at the regressors, **H** is a collection of student- and household-specific qualities; **P** is a set of variables that measure the private school supply at the county level (*c*) of the student's residence; **D** is a set of variables describing the school district (*j*) matched with the student's residence; **R** is a set of resident characteristics at the school district level.

The student-specific variables that **H** comprises include the student's gender, age, race, disability status, and an indicator variable for whether the student moved since the last interview. The household variables that **H** comprises include the size of the household, parental structure indicators (i.e., single-parent household or neither parent present), mother's age, mother's education, whether a parent teaches in non-postsecondary education, and an income measure – in particular, the income category variables as they correspond to a household's income level. Additional geographic measures at the household level include suburban/rural status, regional controls, an indicator variable taking the value of *one* if the state of residence was a charter state as of 2000, and an indicator variable for whether the state of residence has a mandatory interdistrict open enrollment policy.

The private school supply measures, **P,** included in the analysis are the number of private schools in the student's county of residence and the proportion of those schools that are Catholic. Although other private school variables at the county level were constructed for summary statistics purposes, they are not included in the actual analysis because of potential endogeniety.

The district-level variables in **D** profile the student's residentially assigned school district, and recall these measures are one-year lags. These variables include the proportion of students who are free-lunch eligible, average student-teacher ratio, and proportions of students by minority race or ethnicity. Additional district variables included in **D** are the proportion of schools that are Title I eligible, the number of schools in the district (recall

that this is the only school district variable not lagged), and an indicator variable taking the value of one if the district is an elementary-only school district. Finally, per-student levels of total expenditures and federal revenues are among the district-level variables.

All resident characteristic variables in **R** come from 2000 Census data and thus represent community attributes two years prior to the current academic year of 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, the median household income, the fraction of the population below the poverty threshold, the fraction of the adult population with at most a high school diploma, and the fraction of the adult population with at least a traditional college degree.

2.5 Results

I begin by estimating a base model that examines how a household's demand for either (1) a public school of choice in their residentially assigned district, (2) a public school outside their residentially assigned school district, or (3) private school over the default decision – their assigned, within-district public school – based on an abbreviated set of explanatory control variables. The key variables of interest are a handful of education policy and student and/or households measures that are expected to contribute to the displacement of public school students outside their residentially assigned schools; these include indicator variables for whether the student's state of residence had a mandatory interdistrict open enrollment policy as of 2001, whether the student resides in a state that had charter legislation as of 2000, whether the student has moved in the last two years, whether one (or both) of the student's parents is a teacher in non-postsecondary education, and whether the student has a disability on record with the student's school. I also include a select set of student, household, and school district variables as well as the two private school supply measures. In the base model, the student and household level variables include the student's race, the parental structure (i.e., single-parent home) in the household, and income category variables. The base model geographic measures include regional dummies and the urban/rural status that correspond with the student's place of residence. Finally, with respect to the school district level variables, the base model controls include the number of schools in the district, the proportion of district students eligible for free lunch, total expenditures per child, and the median household income among district residents (as of 2000). I later

extend the model to incorporate the full set of student and household, school district, and school district resident variables. This is more thoroughly described and depicted below.

2.5.1 *Base Model*

In Table 3-4, I present the marginal effect estimates for the different schooling alternatives generated from the base model; column one presents the marginal effect estimates for opting into a within-district choice school; column two presents the marginal effect estimates for opting out of district; column three presents the marginal effect estimates for opting into private school rather than one's residentially assigned pubic school. I discuss columns one and two at some length and provide brief commentary regarding column three, as the results regarding the private school decision are presented for comparison purposes and are not the main focus of the analysis. On presenting the extended model's estimates, I discuss any notable changes that occur in the coefficients when including additional explanatory variables.

2.5.1.1 Opting for a Choice School, Within District

As stated previously, column one of Table 3-4 presents the marginal effect estimates of the base model's explanatory variables on a household's decision to stay in district but opt into a chosen –rather than assigned – public school. Regarding the key variables of interest, I find that students who reside in states with mandatory interdistrict open enrollment policies – as opposed to students who do not reside in states with such policies – are, on average, 3 percent less likely to be enrolled in a choice school that is within their district rather than attend their assigned school, all else equal. I find insufficient evidence to suggest that the household's decision to choose a within-district public school of choice rather than attend their residentially assigned school is statistically related to whether the student lives in a charter state. I also find that students who have recently moved are, on average, 3 percent more likely to attend a public school of choice in their district rather than attend their assigned school, ceteris paribus, compared with students who have not recently relocated. Students who have parents who teach in non-postsecondary education are, on average, 3.5 percent more likely than students whose parents are not teachers in elementary or secondary education to opt into a within-district public school of choice rather than attend their residentially assigned school, holding all else equal. And finally I find insufficient evidence to

suggest that the household's decision to choose a within-district public school of choice rather than attend their residentially assigned school is statistically related to whether the student has a diagnosed disability on record at the student's school.

With respect to the additional student and household controls, I find very little evidence of individual attributes contributing to the decision to opt into a within-district choice school rather than one's assigned public school. This may very well reflect the heterogeneous nature of schools of choice; they are meant in part to draw from a variety of students with diverse backgrounds. Indeed, in this analysis, I find no evidence that parental structure, race or ethnicity, or household income factor into this decision in a predictable way – with a sole exception. Students in households with an annual income greater than $200,000 – as opposed to students in households with an annual income of less than $5,000 – are, on average, less likely to opt into a choice school within-district rather than attend their residentially assigned public school by 6 percent, ceteris paribus. I find that geography appears to factor into this household decision substantially. Students who reside in suburban or rural areas – versus urban areas – are, on average, 3 percent less likely to opt into within-district public schools of choice rather than attend their residentially assigned schools, ceteris paribus. And I find that students living in the Northeast, Midwest, or the South all tend to be less likely – by approximately 8 percent, on average – than students residing in the West to opt into a public school of choice within district over their assigned schools, ceteris paribus.

With respect to private school supply measures, I find that the number of private schools in the area is negatively related to students opting into a choice school within district rather than their residentially assigned public school. The implied effect, however, appears trivial. An additional 10 private schools in the county of residence reduces the probability a household opts for a within-district public school of choice over their assigned school by less than 1 percent, all else equal. Furthermore, I find marginally significant evidence to suggest that households considering a within-district choice school over their residentially assigned school are influenced by the concentration of Catholic schools in the area; though the coefficient on the Catholic school measure is just shy of significant at the 10 percent level. Again, the implied effect is trivial; a 1 percent increase in the percentage of the county's private schools that are Catholic is associated with an increased probability of opting into a within-district public school of choice rather than attending one's residentially

assigned school by less than 1 percent, on average and holding all else constant. Interestingly, these two measures elicit opposing effects on this educational outcome.

Regarding the public school district variables, I find that households appear to respond to the size of their school district, as measured by the number of schools. In particular, I find that students in larger school districts are marginally more likely than students in smaller districts to opt into a public school of choice within-district rather than attend their residentially assigned school, all else equal.

2.5.1.2 Opting Out of District

As stated previously, column two of Table 3-4 presents the marginal effect estimates of the base model's explanatory variables on a household's decision to opt out of district (for public school) rather than enroll their student in the assigned public school within district. Regarding the key variables of interest, I find insufficient evidence that either living in a state with a mandatory open enrollment policy or living in a charter state influence the household decision to opt out of district rather than attend their residentially assigned school. I find that students who have recently moved are, on average, 5.6 percent more likely to opt out of district rather than attend their assigned school, ceteris paribus, compared with students who have not recently relocated. This relocation effect is twice the magnitude of the relocation effect on the household's decision to opt into a choice school within district, as presented in column one. Though the effect is not statistically significant, I find marginally significant evidence to suggest that students who have parents who teach in non-postsecondary education are, on average, 2 percent more likely than students whose parents are not teachers in elementary or secondary education to opt out of district rather than attend their residentially assigned school, holding all else equal. I also find that students with a diagnosed disability on record at their school – as opposed to students who do not have a diagnosed disability on record with their school – are, on average, 1 percent more likely to opt out of district rather than attend their residentially assigned schools. This disability effect was not evident among households considering opting into a choice school within district rather that attending their assigned school.

Among the student and household level measures, the coefficient on the single parent dummy variable is positive and statistically significant at the 5 percent level. Its coefficient suggests that students who live in a single-parent home are, on average, 1.7 percent more likely to opt out of district rather than attend their assigned school, as opposed

to students who live in a two-parent home, all else equal. And students who live in a further nontraditional household structure – in which neither parent lives at home – are statistically less likely to opt out of district than students in households with two parents, by an average of 1.9 percent, all else equal. This is perhaps unsurprising as one considers the likely constraints on such nontraditional households. For reference, recall approximately 30 percent of the sample represents single-parent households; students with neither parent at home make up less than 3 percent of the sample.

Evidence also shows that a student's race or ethnicity is a factor in the household decision to opt out of district rather than attend their assigned school. In particular, the coefficient on the Hispanic indicator variable is positive and statistically significant. Being Hispanic rather than white increases the probability a student opts out of district by 2 percent, on average, all else equal. This finding may reflect a cultural issue, transient mobility patterns, a language need, or an unobserved socioeconomic constraint; this potential list itself perhaps makes the result unsurprising.[61] I find insufficient evidence to suggest that being either African American or of another race – as opposed to being white – is related to the household's decision to opt out of district rather than attend one's assigned school. I also find insufficient evidence to suggest that household income is statistically related to the decision of a household to opt out of district for public schooling rather than attend their assigned public school within district. Unlike the previous results, the coefficients on the suburban/rural indicator variables are insignificant here – as are the coefficients on the regional dummies – suggesting that these geographic measures are not related to the household decision to opt out of district in predictable ways conditional on the other included variables.

With respect to the supply of private schooling, I find insufficient evidence to suggest households respond to the number of private schools in their county when deciding

[61] Additionally, across the public school students in this sample, Hispanic students are slightly overrepresented even when including the sample weights. Most students in the sample of public school students are white, although at approximately 50 percent white students may be slightly underrepresented when compared with the universal U.S. student data for the academic year (CCD 2001-2002 data), in which white students comprise more than 60 percent. Additionally both Hispanic and African American students make up about 20 percent each of this public school sample. According to CCD 2001-2002 data, Hispanic and African American students should each represent approximately 16 percent of the public school sector. Contributing factors to the overrepresentation of public school minorities are likely geographically inspired – occurring as a result of the GIS matching – as well as reflecting that this survey over-samples minorities and represents the universe of kindergarten students in the 1998-1999 school year. It does not claim to be nationally representative of third-grade students.

whether to opt out of district for their child's public schooling. However, I do find evidence to suggest a relationship between the household's decision to opt out of district and the concentration of Catholic schools in their county of residence. In particular, conditional on the number of private schools in their county, higher concentrations of Catholic schools tend to be associated with a slightly lower probability of students opting out of district. The magnitude of the coefficient on the proportion of Catholic private schools, however, implies that the effect is likely negligible. A one percentage point increase in the percentage of Catholic private schools in the county is associated with a 0.060 percent decrease in the probability a student opts out of district, on average, all else equal. The magnitude of this effect is not far removed from that of this variable's estimated effect on the decision of households to opt into within-district public schools of choice over their assigned schools, though this effect is just less than statistical significance. Interestingly, the sign of the coefficient changes across columns one and two, implying the role of the concentration of Catholic schools is unique across the two public sector alternatives.

Finally, with respect to the additional school district level variables, only the number of schools in a district has a coefficient that is statistically different from zero – a finding that is consistent with previous results. The estimate implies that an additional 100 schools in the district is associated with an average decrease in the probability a household opts out of district of less than 1 percent, ceteris paribus. Though this effect is significant at the 1 percent level, it appears to be negligible. This finding is consistent with students in smaller school districts having fewer public schooling options in their residentially assigned school district, thus having a higher probability of opting out of district compared with students with more options within district.

2.5.1.3 The Private School Alternative

As stated above, the third column of Table 3-4 presents the marginal effect estimates of the model's explanatory variables on a household's decision to opt into private school rather than enroll their student in the student's assigned public school within district. While it is unlikely that many of the variables will have the same effects on the decisions to either opt into a public school of choice that is within district, opt out of district, or opt into private school, it is reasonable to assume that general traits of one's household or school district consistently influence the decision to opt out of one's residentially assigned school. Alternatively, observed unique effects of the various control variables across the three

alternatives to attending the assigned public school help tell a more complete story about the decisions households make regarding educational services.

Regarding the key variables of interest, I again find insufficient evidence that living in a state with a mandatory open enrollment policy influences the household decision to opt into the private sector rather than attend their residentially assigned school. However, I find that students residing in a charter state – as opposed to students residing in states without charter legislation – are 3.6 percent less likely to attend private school rather than attend their residentially assigned school, on average and holding all else equal.[62] This finding is mildly consistent with the assumption of private sector crowd-out from charter school competition.

I again find evidence to suggest a relocation effect. However, this effect is unique from those presented before. Students who have relocated from their reported census tract two years prior are 3.5 percent less likely to opt into private school rather than attend their assigned public school, on average, ceteris paribus, compared with students who have not moved. This variable appears to have the opposite effect on the decision to opt into private school than it does on the two public school alternatives (columns one and two), and one can see that the relocation effect is expected to have the greatest impact, in terms of magnitude of the coefficient, on the household's decision to opt out of district rather than attend one's residentially assigned school. It may be that families have a strong attachment to the previous public school district as well as an increased incentive to move given the greater availability of public schools of choice. Additionally this may reflect that recent movers have not yet acquired enough information on the private schools – or have not yet had time to process and enroll in the private schools – in the areas they move to. It is also important to note that no control is included for the distance involved in the move. Conditional on the

[62] As mentioned in an earlier footnote, I also estimate the base model respecifying the charter state measure to include any state that introduced its charter legislation in 2001. On doing so, the coefficient on the charter state dummy variable is no longer significant with respect to the decision to attend private school rather than attend one's assigned school (and the coefficients are also insignificant for two public school alternatives). Again, this may imply that lagging this variable one year allows states time to incorporate charter schools in their state – allowing these states to be distinguishable from non-charter states with respect to public school alternatives that are attributable the state's charter legislation. Furthermore, I employ only the variable indicating whether states had charter legislations by 2000 in the remainder of this analysis.

type of move – across town, across state, or inter-state moves – may reveal slightly different estimates in this model for the effects of a household's relocation.[63]

The remaining key variables of interest – having a parent teach in non-postsecondary education or a student having a disability on record at the student's school – appear to be unrelated to the household's decision to opt into private school over attending their residentially assigned school, according to this model's specifications.

Regarding the student and household level qualities, the results suggest that students who reside in the fairly nontraditional home where neither parent is present are less likely than students who live in two-parent homes to opt into private school over attending their residentially assigned school – by an average of 8 percent, all else equal. This finding is consistent with the results presented in column two of Table 3-4, and again may be explained by additional constraints faced by such nontraditional households; though this parental structure effect is nearly four times greater as it influences the decision to opt into private school than its influence on the decision to opt into public school in an alternative district. The coefficient on the single parent indicator variable is insignificant.

Whereas household income was not particularly important for households opting into a choice school within district or opting out of district, it certainly plays a consistent role in the decision to opt into private school rather than attending the residentially assigned school. Compared with households with annual income of less than $5,000, students in households making $5,000 –$10,000 are slightly less likely to attend private school; students in households with income categories of $35,000–$40,000, $40,000–$50,000, $50,000–$75,000, $75,000–$100,000, $100,000–$200,000, and greater than $200,000 are all statistically more likely to attend private school, on average, all else constant. All remaining income category variable coefficients are insignificant.

[63] For the sample of households that relocate, 80 percent change census tracts, but remain in the same county; 13 percent change counties but remain in the same state; 7 percent relocate to new states. I also estimate the model controlling for type of move. Specifically, I include a dummy variable indicating whether the student moved within-county – the default category is a move to a new county or state. Under this specification, only the decision to opt out of district rather than attend one's assigned school is influenced by the relocation effect. The extended (and base) model's coefficient on the relocation dummy variable indicates that households are, on average, 12 percent more likely to opt out of district than attend their residentially assigned school, ceteris paribus. And conditional on having moved, households who moved within their county – as opposed to students who moved to another county or another state – are 2.4 percent *less* likely opt out of district rather than attend their own school. The results for the other key variables of interest are generally unaffected by this specification – occasionally coefficients move between marginally significant and statistically significant.

Race and ethnicity also appear to play a larger role on the household decision to opt into private school than they do on the household decision to opt into another district or to stay in district and attend a choice school. Hispanic students are approximately 6 percent less likely than white students to opt into private school, on average, ceteris paribus, than attend their assigned public school. Recall that Hispanics were more likely than white students to opt out of district rather than attend their assigned school. The influence of the Hispanic effect, as measured by the magnitude of the coefficient, is nearly three times larger on the decision to attend private school than its estimated effect on the decision to opt out of district. And while being African American did not have a notable effect on the previously reported public schooling choices, the evidence suggests being African American does influence the decision to opt into the private sector. African Americans are approximately six times less likely than their white counterparts to opt into private school, on average, ceteris paribus, rather than attend their assigned school.

Among the geographical measures, the suburban/rural status of households appears to affect the decision of households to opt into private school. Students in suburban and rural areas – as opposed to students residing in urban areas – are found to generally be less likely to opt into private schools rather than attend their assigned schools, on average and holding all else constant – by 7 and 6 percent, respectively. The magnitude of the coefficients on the suburb and rural dummies in column one imply residing in such areas is less of an influential factor for households considering a choice school within their district than for households considering private school as an alternative to their residentially assigned public schools; recall that the urban/rural status of a student's household does not appear to affect whether households opt out of district. I also find that students residing in the Midwest – as opposed to students residing in the West – are 8.4 percent, on average, more likely to attend private school rather than attend their residentially assigned school.

Consistent with expectation, I find that households are responsive to the supply of private schools when deciding between these schooling alternatives; in particular, both private school supply measures appear to be positively related to households opting for private school over sending their students to their assigned public schools. Again, this estimate tells a slightly different story than the results discussed previously. The number of private schools in the student's county of residence was found to negatively influence the decision to opt into a public school of choice within district, and then was not found to

affect the household's decision to opt out of district; however here evidence suggests that an additional ten private schools in the county of residence increases the probability a student opts into private school over attending their assigned public school by an average of 0.1 percent, on average, holding all else constant. Again, the private school supply measure appears to elicit negligible effects. I find that a 1 percent increase in the percentage of Catholic private schools in the county increases the probability a student opts into private school by less than 1 percent, on average, ceteris paribus.

Among the school district variables, I again find that the size of a district, as measured by the number of schools, appears to be an influential factor as households decide between private school and attending their residentially assigned public schools. Also consistent with previous results is that this effect appears trivial. In addition, I find that median household income across district residents appears to play a role in the decision of households to opt into private school. An additional thousand dollars in median household income decreases the probability a household opts for private school by less than 1 percent, on average, ceteris paribus.

2.5.2 *Extended Model*

In Table 3-5, I again present the results for the multinomial probit in which a household chooses from four possible outcomes for their child's education: (1) a public school of choice in their residentially assigned district, (2) a public school outside their residentially assigned school district, (3) private school, or (4) the default decision – their assigned, within-district public school. In particular, Table 3-5 presents the marginal effect estimates for the different alternatives generated from the extended model; column one presents the marginal effect estimates for opting into a within-district choice school; column two presents the marginal effect estimates for opting out of district; column three presents the marginal effect estimates for opting into private school rather than attending one's assigned school. Only the coefficients of the key variables of interest are presented here, although Table 3-6 presents a more complete list of the extended model's coefficients. Again, I discuss columns one and two at some length and provide brief commentary regarding column three, as the results regarding the private school decision are generally presented for comparison purposes and are not the main focus of the analysis. I also

concentrate my comments on how the coefficient estimates differ from those of the basic model.

The additional student and household level explanatory variables incorporated in the extended model include the student's gender and age, household size, mother's age, and mother's educational attainment.[64] The additional school district level variables include the proportion of Title I eligible schools in the district, the proportions of students by racial or ethnical classification, federal revenues per child, student-teacher ratio, and a dummy variable taking the value of one if the district is an elementary-only school district. Finally, the additional district level resident variables include the fraction of households below the poverty level, the proportion of minority residents and its square, the fraction of adults 25 and older with at most a high school diploma, and the fraction of adults 25 and older with at least a four-year college degree. Again, these variables are intended to address potential heterogeneity of preferences across households in the district.

2.5.2.1 Opting for a Choice School, Within District

As stated previously, column one of Table 3-5 presents the extended model's marginal effect estimates of the key variables of interest on a household's decision to stay in district but opt into a chosen – rather than assigned – public school. On the inclusion of the additional controls, the coefficient on whether the household resides in a state with a mandatory interdistrict open enrollment policy generally retains its sign, magnitude, and statistical significance. I continue to find insufficient evidence to suggest that the household's decision to choose a public school within district rather than attend their residentially assigned school is statistically related to whether the household resides in a charter state or whether the student has a disability on record at the student's school. Also, upon the inclusion of the additional controls, the coefficients on whether the student recently moved generally retains its sign, magnitude, and statistical significance. Interestingly, on including the additional model variables, the coefficient on the measure indicating the student has a parent who teaches in non-postsecondary education becomes only marginally significant, as its coefficient falls just below the 10 percent level of significance.

[64] In alternative specifications, other household level variables were included – such as father's age and parental employment variables – but coefficients were generally not significant or highly correlated with variables already included. Thus to avoid taxing the small sample size with an increasingly restrictive model, these other variables are excluded from extended set of household and student level controls.

The coefficients on the remaining variables of the model, as depicted in Table 3-6, are generally unchanged, with a few notable exceptions. Under the extend model, the sole income category variable that was previously significant and the suburb indicator variable are no longer significant. Also upon the inclusion of the extended model's additional set of controls, the concentration of Catholic schools in the county becomes significant. In particular, a 1 percent increase in the percentage of Catholic private schools in the county increases the probability a household opts into a within-district public school of choice over their residentially assigned school by less than 1 percent, on average, all else equal. Again, the private school supply effects may reflect the correlation between the entry of private schools and expanded public school choice in an area; without better school quality measures or instruments for private schools, these findings and their suggested endogeniety are not immediately reconcilable. A final difference in the models' estimates is that on including the additional variables for the extended model, median household income in the school district becomes negative and statistically significant. In particular, I find that an additional thousand dollars in the median household income among district residents marginally reduces the probability a household opts for a public school of choice within district over their residentially assigned public school by less than 1 percent, on average, all else constant. The remaining variable coefficients, as presented and discussed in the base model, generally retain their sign, magnitude, and statistical significance.

Of the additional set of control variables included in the extended model, I find that households with older mothers are slightly less likely to opt into a public school of choice within district rather than attend their residentially assigned schools. In particular, an additional year of the mother's age is associated with a reduction of less than 1 percent, on average, in the probability a household chooses a school within district that is not residentially assigned, ceteris paribus. I also find that students who reside in elementary-only school districts are 12.8 percent less likely to attend a within-district public school of choice rather than their residentially assigned public school. However, it should be noted that elementary-only school districts represent less than 1 percent of the school districts residentially assigned to the students in this sample.[65] I also observe an effect of the presence of students classified as *another race* in the district – those students who are not classified as

[65] The z-score on the elementary district indicator variable is substantially large – 21.13. Excluding this variable from the model has little effect on the estimates of the remaining coefficients.

white, African American or Hispanic. The coefficient suggests that a 1 percent increase in the percentage of the district's enrollment that are of another race increases the probability a household chooses a within-district public school for their student rather than enrolling the student in the student's residentially assigned school by 0.25 percent, on average. Finally, I also observe that a one-student increase in the average student-teacher ratio in the district slightly reduces the probability that a household chooses a within district public school of choice over their assigned school by less than 1 percent, on average and ceteris paribus.

2.5.2.2 Opting Out of District

As stated previously, column two of Table 3-5 presents the extended model's marginal effect estimates of the key variables of interest on a household's decision to opt out of district (for public school) rather than enroll their student in the student's assigned public school within district. On including the extended models' additional variables, the coefficient on the open enrollment dummy becomes statistically significant. Students who reside in states with mandatory interdistrict open enrollment policies are 3 percent more likely, on average, to opt out of district, ceteris paribus, compared with students who do not live in states with such open enrollment policies. Consistent with the base model's estimates, I continue to find insufficient evidence to suggest that the household's decision to opt out of district is statistically related to whether the household resides in a charter state. I also continue to find marginally significant evidence to suggest that having a parent who teaches in non-postsecondary education is related to the decision to opt out of district. On including the additional controls, the coefficient on whether the student recently moved generally retains its sign, magnitude, and statistical significance. And interestingly, on including the additional model variables, the coefficient on the measure indicating the student has a diagnosed disability on record at the student's school loses its significance, although one might consider it marginally significant as its z-score is not far removed from the 10 percent level of significance.

The coefficients on the remaining variables of the model, which can be found in column two of Table 3-6, are generally unchanged, with one notable exception. Under the extend model, the single-parent indicator variable is no longer significant. The remaining variable coefficients, as presented and discussed in the base model, generally retain their sign, magnitude, and statistical significance.

Of the extended set of control variables, I find that households with older mothers are slightly less likely to opt out of district. In particular, an additional year of the mother's age is associated with a reduction of less than 1 percent, on average, in the probability a household opts out of district, ceteris paribus. Of all the included education measures for the mother – less than high school, high school, some college, college, and professional – two have statistically significant coefficients. The estimates suggest that students whose mothers finish high school, as opposed to students whose mothers do not complete high school, are less likely to opt out of district rather than attend their assigned school by an average of 1.3 percent, ceteris paribus. Additionally, students whose mothers finish college, but do not have an advanced degree – as opposed to students whose mothers do not complete high school – are 1.3 percent less likely to opt out of district, on average, ceteris paribus.

2.5.2.3 The Private School Alternative

As stated above, the third column of Table 3-5 presents the extended model's marginal effect estimates of the key variables of interest on a household's decision to opt into private school rather than enroll their student in their assigned public school within district. Under the extended model, residing in an open enrollment state is marginally negatively related to attending private school rather than attending the assigned public school; however, the z-score falls just shy of the 10 percent level of significance. I find that under the extended model, students who reside in charter states – as opposed to students residing in non-charter states – are 4.3 percent less likely to attend private school rather than their residentially assigned school; this effect is somewhat stronger than the charter school effect estimated in the base model. On including the additional controls, the coefficient on whether the student recently moved retains its sign and statistical significance, though it falls slightly in magnitude. Students who moved since the last interview – as opposed to students who still reside in their previous census tract – are 2.6 percent less likely to attend private school rather than their residentially assigned public school, on average, holding all else equal; this relocation effect was estimated at a reduction in the probability of attending private school of 3.5 percent under the base model. The remaining student level variables that were expected to be influential over the household's decision to opt out of district or into a choice school – having a parent teach in non-postsecondary education, or having a disability on record at the student's school – still appear to be unrelated to the household's decision to opt into private school, according to this model's specifications. However, under

the extended model, the coefficient on the measure indicating a student's parent is a teacher in non-postsecondary educations is negative, whereas under the base model, the coefficient was positive.

The coefficients on the remaining variables of the model, which can be seen in column three of Table 3-6, are generally unchanged, with a few notable exceptions. In addition, the magnitude of the coefficients is subject to slightly more variation across the two model specifications than was seen with the two public school options. Under the extend model, the coefficient on the income category of $35,000–$40,000 is no longer significant, as it was in the base model. Additionally, the Midwest dummy variable loses its significance as well, leaving no regional classification with a statistically significant coefficient. The general trend across the remaining statistically significant income and urban/rural variables is that the magnitudes of their coefficients fall slightly. Also, on including the extended model's additional controls, the number of schools in a school district is no longer significant. The remaining variable coefficients, as presented and discussed in the base model, generally retain their sign, magnitude, and statistical significance.

Of the additional control variables under the extended model, I find female students are marginally more likely to attend private school than their male counterparts, all else constant. And I find that students with older mothers are slightly more likely to attend private school rather than attend their residentially assigned schools. In particular, an additional year of the mother's age is associated with an increase of less than 1 percent, on average, in the probability a household opts into private school, ceteris paribus. Of the included education measures for the mother – less than high school, high school, some college, college, and professional – all have positive and statistically significant coefficients. The estimates suggest that students whose mothers finish high school – as opposed to students whose mothers do not complete high school – are more likely to attend private school than attend their assigned school by an average of 8.0 percent, ceteris paribus; students whose mothers finish high school and complete some college are more likely to attend private school than attend their assigned school by an average of 12.5 percent, ceteris paribus; students with mothers who complete college are more likely to attend private school than attend their residentially assigned school by 21.1 percent, ceteris paribus; students

whose mothers hold a professional degree are, on average, 19.4 percent more likely to attend private school rather than their assigned school, ceteris paribus.

With respect to the extended model's additional school district variables, I find that students who reside in elementary-only school district – as opposed to students who do not live in an elementary-only school district – are 6 percent less likely, on average, to attend a private school rather than attend their residentially assigned public school, all else equal. And regarding the additional school district resident variables, I find residing in districts with higher poverty rates are on average, slightly less likely to attend private school rather than attend their residentially assigned public school. And students who live in districts with greater heterogeneity in the educational attainment of adult residents are, on average, slightly more inclined to attend private school rather than attend their residentially assigned public school.

2.6 Discussion

This analysis of the household's decision to opt into a within-district public school of choice, out of district, or into private school rather than attend their residentially assigned district reveals some interesting results. Across the various choices, the model consistently yields findings that suggest key educational policies as well as student, household, and school district level variables indeed appear to play a role in the household's decision to opt out of their residentially assigned school. Worthy of some discussion are the likely implications of the results presented and briefly discussed in the previous section.

With respect to the key variables of interest – assumed to be likely candidates for displacing students within the public sector – the findings tend to be consistent with expectation and tell an interesting story. First consider the effects of statewide mandatory interdistrict open enrollment policies. Students who live in states with mandatory interdistrict open enrollment polices are, on average, 3 percent less likely to opt into a choice school within their district – all else constant. Such students are also approximately 3 percent more likely to attend a public school outside their district. These findings suggest a substitution effect between intra-district and interdistrict school choice when state policy mandates interdistrict choice. Conditional on having the option to opt out of district, there appears to be a smaller take-up rate of within-district schools of choice and a slightly larger take-up rate of interdistrict choice. It is not known whether the substitution is a supply or

demand issue, however, as I do not have data accounting for the intra-district choice in the school districts this sample of students resides in.

The coefficient on the open enrollment variable is nearly significant at the 10 percent level as it relates to the decision of households to opt for private school rather than their residentially assigned public school. So while it is not a statistically significant finding, students residing in states with mandatory interdistrict open enrollment policies are, an average, approximately 4 percent more likely to opt into private school than attend their residentially assigned school, all else constant. A number of factors may influence the positive correlation between private school enrollment and state mandated open enrollment policies – perhaps the most likely is that both private schools and open enrollment polices surface in areas where public schools are performing poorly or are perceived to be insufficient in some respect. However, without further controls for school quality, it is beyond the scope of this chapter to conjecture further about the implications of this finding.

Turning next to the estimates of the charter state effect, I find insufficient evidence to suggest that residing in a state that has charter legislation is related to the household decision to either opt into a within-district public school of choice or opt out of district. I do, however find evidence to suggest that residing in a charter state is negatively related to households opting for private school. This finding may suggest private sector crowd-out by charter school competition. It is an interesting result as charter schools have been found to locate in areas that have already seen the entry of private schools (Glomm et al. 2005). It could signal increased competition among public schools in charter-states; however, a more thorough explanation for the finding is readily observable.

With respect to the estimates of the household relocation effect across these outcomes, interpreting the effect is done cautiously because the assumption of the model is that the household's decision to move is exogenous to the household's educational choice. While public sector school choice, in theory, certainly relaxes the tie between residential location and where households send their child to school, an undeniable relationship still exists between the two. General equilibrium models that examine this issue can be extremely challenging to estimate as households make decisions about where to live based on available school quality and vice versa – they also make decisions on where to attend school based on their place of residence. Separating one decision from the other has long been a challenge for economists in the education literature. In this model I do not attempt to estimate the

household's mobility and schooling choices as simultaneous decisions; as the sample size, with its limited number of observations in the four outcome categories, would be significantly strained. However, future work will examine this issue.

Thus based on the assumption that the mobility decision of a household is exogenous to the household's consumption of particular types of educational services, the following can be said about the model's estimation results. Recent mobility appears to increase the probability that households send their children to public schools of choice – either within or out of district – rather than sending their children to their residentially assigned schools. And alternatively, having recently moved tends to reduce the probability that households send their children to private school. The magnitudes of the effect suggest that third-grade students who have moved out of the census tract they were in when they were in first grade are 2 percent, on average, more likely to be enrolled in a public school of choice within district rather than attend their assigned public school. Even more, these students are, on average, 6 percent more likely to opt out of district; this effect is significant at the 1 percent level; whereas the relocation effect on the decision to opt into a within-district public school of choice is significant at the 10 percent level. It may be that households who have recently moved with young children have an attachment to the school or school district their children attended before the move; or perhaps school choice policy and availability incentivize relocation to another district. Without additional information matching students to their prior school and school district, it is impossible to extrapolate the implications of these relocation effects; however, they are certainly reasonable findings.

The magnitude of the relocation effect on the private school decision implies that households that have recently moved out of their previous census tracts are approximately 3 percent less likely to send their children to private school rather than their residentially assigned school. This is quite a plausible finding as noted in the previous section; as new residents to an area may be acquiring information about the public and private schools available to them, may still be processing application and enrollment procedures of private schools, or may be substituting for private schools by enrolling their students in public schools of choice, both within and out of district.

As noted in a previous footnote, I also estimate the models controlling for the type of move – specifically, I include a dummy variable indicating whether the student moved within-county, as opposed to moving to a new county or state. I present the marginal effect

estimates of the extended model with this specification in Table 3-7, though, for brevity, I include only the coefficients of the key variables of interest in the table. Under this specification, the relocation effect influences only the decision to opt out of district rather than attend one's assigned school. The extended model's coefficient on the relocation dummy variable indicates that households are, on average, 13 percent more likely to opt out of district, ceteris paribus. And conditional on having moved, households that moved within their county – as opposed to students who moved to another county or another state – are 2.4 percent *less* likely opt out of district. This result implies that having recently moved may incur a larger effect on the decision of households to opt out of district than was previously thought, and that smaller moves around or across town actually slightly reduce the probability households opt out of district compared with bigger moves – out of town or out of state.

With respect to the remaining key variables of interest, I also find subtle effects of a student's disability status as well as whether the student has a parent who teaches in non-postsecondary education. In the extended model, these effects are only marginally significant, as their z-scores fall just less than statistical significance at the 10 percent level. Regarding students with disabilities, I find that students with a diagnosed disability on record at their school are, on average, 1 percent more likely to opt out of district. This may suggest that special needs services and resources are fairly evenly distributed across schools within a given district; thus finding a unique option for publically provided education with better matched special education services to the needs of a household may require households to look outside their district. Or it may imply relaxed constraints on interdistrict enrollment policies for households with students who require special education services. Additionally, it is important to note that the classification or severity of the student's disability is not controlled for. Thus while differential effects are likely across learning, physical, and behavioral disabilities, those effects are suppressed here. Additionally, the model does not control for the level or quality of special needs services provided by the household's residentially assigned school or school district. Thus it is not clear whether the coefficient is likely biased upward or downward toward the true disability effect on this household education decision, although the general finding is certainly reasonable. The disability status does not appear to influence the household decision to opt into either a within-district public school of choice or a private school.

Finally the implied effect of having a parent who teaches in non-postsecondary education also varies across the schooling options. Though the coefficients are only marginally significant, I find that students who have at least one parent who teaches in primary or secondary education are more likely to opt into a public school of choice – either within or out of district – rather than attend their residentially assigned schools, compared with students who do not have parents teaching in non-postsecondary education. Specifically, students with parents who teach in non-postsecondary education are more likely to opt into a within-district public school of choice by 3.4 percent, on average; such students are 2.2 percent, on average, more likely to opt out of district. It is quite plausible that students attend schools in which their parents teach, leading to this outcome; but it may also have to do with increased opportunities to choose across district schools for parents who teach, or perhaps parents who teach have a superior information set regarding school quality, reducing their cost of acquiring information about better schools for their children. I do not find evidence to suggest that having a parent who teaches in non-postsecondary education affects the household decision to opt for private school.

Regarding the student and household level variables, the general finding is that very little in terms of student and parental attributes predictably influence the household decision to opt for a public school of choice (whether intra-district or interdistrict). While certainly these qualities play a role; it is highly likely that what is observed is a heterogeneous set of students opting for a heterogeneous set of schools for a heterogeneous set of reasons – and far more preferences and factors of influence that affect these decisions are unobserved than are observed in the data. An anomaly is that Hispanic students – as opposed to white students – appear to be more likely to opt out of district by an average of 2 percent, all else constant. This could merely reflect the overrepresentation of Hispanics in the sample, or, as mentioned earlier, some underlying Hispanic effect may be correlated with cultural or mobility patterns as well as potential language needs – such as English as a second language (ESL) programming. Race has classically been shown to play a role in the public-private school decision and does not disappoint here, as being a student of any minority is associated with a reduced probability of attending private school over one's residentially assigned public school. Students who live in fairly nontraditional households in which neither parent is present are found to be less likely to either opt out of district or opt into private school. Again, the likely constraints faced by such nontraditional households and the

likely costs associated with these two schooling alternatives – none of which are controlled for in the model – likely explain these findings. I also find that students with slightly younger mothers are more likely to opt for a public sector alternative to their residentially assigned schools; whereas students with slightly older mothers are more likely to adopt the private school alternative. And while income appears to be irrelevant in the household's decision to either opt out of district or attend a within-district public school of choice; certainly higher income households are more inclined to opt for private school.

Regarding the geographic variables of interest, residing in a rural area logically reduces the probability a student either opts for a within-district public school of choice or private school over the residentially assigned school – at averages of 3 and 6 percent, respectively. And while the finding is not statistically significant – as the coefficient is just below statistical significance at the 10 percent level – students residing in rural areas are 3 percent more likely to opt out of district. The results also suggest that of the three alternatives to attending one's residentially assigned public school, living in a particular region of the country tends to impact only households considering opting into a within-district public school of choice. In particular, students residing in the West – as opposed to students living in any other region, are more likely to opt for a within-district public school of choice. This finding may reflect strong public school choice policies in Western states, may be tied to the geographical alignments of school district and county borders in some Western states, may be linked to the Hispanic anomaly mentioned above – as the plurality of Hispanic students in the sample reside in the West – or may be the result of some other phenomena of the data. Without a more thorough depiction of what constitutes intra district choice – such as additional policy and institutional measures – this finding is challenging to justify concretely.

The estimates on the private school measures present fairly predictable and somewhat interesting results. The measures appear to be positively related to students either attending a within-district public school of choice or a private school rather than their residentially assigned public school – with one exception. The concentration of Catholic schools appears to be negatively related to the probability a household opts into a within-district public school of choice. And as previously mentioned, the concern of the positive correlation between private schools and policies regulating the set of intra-district and interdistrict public school options is not controlled for here with school quality measures,

finer policy data, or instruments for private schools. Thus an endogeniety concern is likely regarding complimentary entry into public school of choice and private schools conditional on the supply of private schooling. While this endogeniety concern is not reconciled here, it may very well be inconsequential to the results, as all of the estimated private school supply effects appear to be trivial.

The coefficients on the variables that profile the school district reveal that the number of schools in the district appears to influence students opting for a public school alternative to their residentially assigned school; though the effect is trivial. Consequently, no notable effect appears for district size on the household decision to opt for private school. Students residing in elementary-only school districts, which is rare, are less likely to opt either for a within-district public school of choice or a private school; no noted elementary-only district effect appears on the decision to opt out of district. As elementary-only school districts are uncommon, and represented less than 1 percent of the sample's residentially assigned districts, perhaps a very limited means exists for generalizing this finding over the larger population.

Finally, with respect to the school district resident attributes, higher median income among district residents appears to decrease the probability a household opts either for a within-district public school or for a private school. Only the household decision to opt for private school rather than attend their residentially assigned public school is further influenced by qualities of the district residents. Students residing in school districts with higher poverty rates are slightly less likely to attend private school, all else constant, and greater heterogeneity in the educational achievement of the districts residents is positively related with students opting for private school, ceteris paribus.

2.7 Conclusion

In this chapter, I examine how various statewide education policies as well as student, household, and school district level factors affect the household's decision to enroll their primary grade student in either (1) a public school of choice in their residentially assigned district, (2) a public school outside their residentially assigned school district, or (3) private school over the default decision – their assigned, within-district public school, based on a rich set of student, household, and school district-level control variables. The handful of measures expected to contribute to the displacement of public school students outside

their residentially assigned schools represent the key variables of interest; these include indicator variables for whether the student's state of residence had a mandatory interdistrict open enrollment policy as of 2001, whether the student resides in a charter state, whether the student has moved in the last two years, whether one (or both) of the student's parents is a teacher in non-postsecondary education, and whether the student has a disability on record with the student's school. I estimate a base and extended multinomial probit model to incorporate a number of additional explanatory variables also believed to be potentially influential in the household's decision regarding these four schooling outcomes.

Regarding educational policy, I find evidence to suggest that households substitute between intra-district and interdistrict schools of choice. While I find no effect of residing in a charter state on the household decision to pursue intra-district or interdistrict public school options rather than attend the assigned public school, I do find a subtle effect suggesting such households are less likely to opt into private school, all else equal. I also find that mobility patterns significantly affect this household decision. Students who moved out of the census tract they resided in two years earlier – as opposed to students who have not moved – are more likely to opt for either a within-district public school of choice or opt out of district rather than attend their residentially assigned school. And this recent mobility reduces the probability students attend private school, by an average of 3 percent. The relocation effect, however, appears sensitive to the type of move the household undergoes. Controlling for within-county moves, only the decision to opt out of district is found to be significantly related to a household's recent relocation.

Marginally significant effects suggest that students with a diagnosed disability on record at their schools are slightly more likely to opt out of district. Also, marginally significant effects suggest having a parent who is a teacher in non-postsecondary education slightly increases the probability of opting either for a within-district public school of choice or out of district.

Earlier studies that examine the demand for interdistrict transfers in states with open-enrollment policies find, broadly speaking, that open enrollment can lead to more stratified districts as upper-income white students opt into better school districts and minority students opt for districts with larger proportions of their own race or ethnicity. (Holme and Richards 2009; Reback 2008; Welsch et al. 2010). My findings are somewhat unique – revealing Hispanics to be among the most likely to opt out of district. While I find

some evidence to suggest that students with disabilities are more likely to opt out of district, similar to Reback (2008) this finding is only marginally statistically significant. Additionally, previous studies show that households are more likely to opt out of less affluent districts (Reback, 2008; Welsch et al. 2010); however, I find no statistically significant coefficients among the attributes of the school district residents.

My results are not directly comparable with the literature on intra-district choice, as the majority of this research area addresses variation across schools within a district and my analysis does not (Hastings et al. 2009; Bifulco et al. 2009). Just as these studies tend to find racial and ethnic heterogeneity in the demand for better performing or more racially/ethnically distinct intra-district schools; the results of this analysis also imply that no predictable influence is found in demographic or socioeconomic measures – such as race, education or income – on a household's decision to opt for a within-district public school of choice, other than perhaps having younger parents, all else equal. And as Cullen et al. (2005) find that intra-district choice in the Chicago Public School District likely led to the relocation of households to areas with lower housing values, my analysis reveals /higher rates of residential mobility among students that attend public schools of choice (both within and out of district).

A major contribution of this chapter is to link interdistrict and intra-district choice as substitutes. Indeed many of the same types of individuals pursue either type of open enrollment policy; based on its availability (Holme and Richards 2009; Reback 2008; Welsch et al. 2010; Hastings et al. 2009; Bifulco et al. 2009). The underlying infrastructure and additional policy measures necessary to identify the supply of the intra-district and interdistrict schools of choice are beyond the scope of this data, so it is not known how or why this substitution occurs.

Finally, although the finding is not greatly explored, the observed result that residing in a charter state negatively influences the probability a household will opt for private school rather than their residentially assigned school is complimentary to the literature that looks at the private sector crowd of out of charter school competition (Toma et al. 2006; Roy and Chakrabarti 2007). Especially in light of the tendency of charter schools to locate in areas where private schools are already present (Glomm et al. 2005), I hesitate to put much stock in any interpretation of the implication of the estimated effect without additional data and analysis.

Thus while my results are not directly comparable to previous studies that look at open-enrollment policies, my research question is not to look at the response to a specific educational policy but rather to better define the demand for opting for within-district public schools of choice as well as the demand for interdistrict enrollment. Compared with many previous studies, I incorporate a richer dataset of student and household level data, but include fewer nuances on education policy and infrastructure. Future work will examine some of the more puzzling results uncovered here, such as charter school and residential mobility effects.

2.8 Tables

Table 2-1: Summary Statistics - Final Sample vs. Unmatched, Larger Third Grade Sample

	Final Sample N = 7,650		Unmatched Sample N = 11,160	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Attends Private School*	13.00	(33.63)	12.08	(32.59)
Key Variables				
Open Enrollment State as of 2001 (%)***	12.38	(32.94)	14.33	(35.04)
Charter State as of 2000 (%)***	84.93	(35.78)	84.09	(36.58)
Recently Moved (%)	21.56	(41.12)	21.17	(40.86)
Parent is a Teacher (%)	6.22	(24.16)	6.02	(23.78)
Disability (%)	27.01	(44.40)	27.14	(44.47)
Additional Student Attributes				
Female (%)	50.82	(50.00)	49.80	(50.00)
Age	9.27	(0.38)	9.28	(0.38)
African American (%)**	17.59	(38.07)	15.16	(35.87)
White (%)***	55.44	(49.71)	57.90	(49.37)
Hispanic (%)	20.26	(40.20)	20.17	(40.13)
IEP (%)	11.48	(31.88)	11.31	(31.68)
Changed School Dummy (%)	20.62	(40.46)	20.61	(40.45)
Additional Household Attributes				
Household Size (%)	4.49	(1.36)	4.52	(1.37)
Two-Parent Home (%)***	69.80	(45.92)	71.60	(45.09)
Single Parent Household (%)***	27.96	(44.88)	26.25	(44.00)
Father Not Present (%)***	27.49	(44.65)	25.79	(43.75)
Father Employed (%)	93.32	(24.98)	93.47	(24.70)
Mother Employed (%)	73.48	(44.15)	73.28	(44.25)
Household Income, Category Median ($K)	62.53	(62.54)	63.68	(63.54)
Father's Age (%)	40.30	(7.08)	40.21	(7.04)
Mother's Age (%)	37.58	(6.61)	37.51	(6.55)

*Difference in means is statistically different across samples at the: * 10% level; ** 5% level, *** 1% level. Statistics are weighted. Omitted parental education variable is Less than High School. 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.*

Table 3-1 continued
Summary Statistics - Final Sample vs. Unmatched, Larger Third Grade Sample - *Continued*

Variable	Final Sample N = 7,650 Mean	Std. Dev.	Unmatched Sample N = 11,160 Mean	Std. Dev.
Additional Household Attributes *(Continued)*	13.00	(33.63)	12.08	(32.59)
Father's Education - High School (%)	26.50	(44.13)	26.36	(44.06)
Father's Education - Some College (%)*	27.37	(44.59)	28.62	(45.20)
Father's Education - College (%)	17.63	(38.11)	17.16	(37.71)
Father's Education - Professional (%)	14.09	(34.79)	13.62	(34.31)
Mother's Education - High School (%)	25.46	(43.56)	26.03	(43.88)
Mother's Education - Some College (%)	35.26	(47.78)	35.67	(47.90)
Mother's Education - College (%)	16.71	(37.31)	16.43	(37.06)
Mother's Education - Professional (%)	9.60	(29.46)	8.96	(28.56)
Household is at or Below Poverty Line (%)*	21.95	(41.39)	20.89	(40.65)
Suburb (%)***	43.06	(49.52)	44.61	(49.71)
Rural (%)***	13.60	(34.29)	18.34	(38.71)
Northeast (%)***	21.11	(40.81)	18.10	(38.50)
Midwest (%)***	17.34	(37.87)	23.22	(42.23)
West (%)***	21.73	(41.24)	23.50	(42.40)
Private School Supply & Demand				
K-12 Private School Enrollment in CT (%)**	12.15	(11.48)	11.74	(10.77)
Private Schools, Number in County	146.33	(291.70)	146.26	(294.60)
Average Size of Private Schools in County***	203.31	(73.91)	195.13	(72.30)
At Least One Catholic School in County (%)***	90.35	(29.52)	87.78	(32.75)

*Difference in means is statistically different across samples at the: * 10% level; ** 5% level, *** 1% level. Statistics are weighted. Omitted parental education variable is Less than High School. 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.*

Table 2-2: Summary Statistics for Student, Household, and Private School Supply and Demand Variables by Outcome Category

	Assigned School		Within-District School of Choice			Opts Out of District			Private School		
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**		**Mean**	**Std. Dev.**		**Mean**	**Std. Dev.**	
Percent of the Sample:	**70.27**	**(45.71)**	**12.27**	**(32.82)**		**4.46**	**(20.64)**		**13.00**	**(33.63)**	
Key Variables of Interest											
Open Enrollment, 2001 (%)	12.21	(32.74)	11.96	(32.47)		19.72	(39.88)	***	11.23	(31.59)	
Charter State as of 2000 (%)	85.62	(35.09)	84.72	(36.00)		82.01	(38.50)		82.39	(38.10)	***
Recently Moved (%)	20.62	(40.46)	25.51	(43.62)	***	52.18	(50.07)	***	12.90	(33.53)	***
Parent is a Teacher (%)	5.34	(22.48)	7.41	(26.20)	**	9.11	(28.84)	**	8.92	(28.52)	***
Disability (%)	26.55	(44.16)	28.85	(45.33)		36.04	(48.12)	***	24.66	(43.12)	
Additional Student Attributes											
Female (%)	50.49	(50.00)	50.80	(50.02)		46.97	(50.02)		53.93	(49.86)	**
Age	9.28	(0.38)	9.23	(0.39)	***	9.28	(0.35)		9.28	(0.36)	
African American (%)	18.23	(38.61)	21.21	(40.91)	**	19.54	(39.74)		10.00	(30.00)	***
White (%)	54.17	(49.83)	47.01	(49.94)	***	50.74	(50.10)		71.91	(44.96)	***
Hispanic (%)	21.01	(40.74)	22.89	(42.04)		24.51	(43.11)		12.26	(32.80)	***
IEP (%)	12.66	(33.26)	13.72	(34.43)		16.54	(37.27)		2.59	(15.89)	***
Changed School Dummy (%)	20.11	(40.09)	21.45	(41.07)		41.79	(49.43)	***	15.32	(36.03)	***

*Difference in means is statistically different from the sample of students attending their assigned school at the: * 10% level; ** 5% level, *** 1% level. Statistics are weighted.*

Table 3-2 continued (1)

Summary Statistics for Student, Household, and Private School Supply and Demand Variables by Outcome Category - *Continued*

Variable	Assigned School Mean	Assigned School Std. Dev.	Within-District School of Choice Mean	Within-District School of Choice Std. Dev.		Opts Out of District Mean	Opts Out of District Std. Dev.		Private School Mean	Private School Std. Dev.	
Additional Household Attributes											
Household Size (%)	4.54	(1.38)	4.42	(1.39)	**	4.24	(1.26)	***	4.38	(1.28)	***
Two-Parent Home (%)	68.65	(46.40)	67.53	(46.85)		59.07	(49.28)	***	81.83	(38.57)	***
Single Parent Household (%)	28.68	(45.23)	30.02	(45.86)		40.56	(49.21)	***	17.83	(38.28)	***
Neither Parent at Home (%)	2.67	(16.12)	2.45	(15.46)		0.37	(6.06)	**	0.35	(5.89)	***
Father Absent (%)	28.74	(45.26)	30.05	(45.87)		34.66	(47.69)	*	15.83	(36.51)	***
Father Employed (%)	92.91	(25.67)	92.27	(26.72)		91.62	(27.79)		96.54	(18.28)	***
Mother Employed (%)	72.52	(44.64)	74.82	(43.43)		77.97	(41.54)	*	75.95	(42.75)	***
Household Income, Category Median ($K)	57.96	(58.75)	56.22	(55.36)		51.81	(46.02)		96.88	(80.35)	***
Father's Age (%)	40.17	(7.19)	39.67	(7.07)	*	37.96	(6.81)	***	41.98	(6.25)	***
Mother's Age (%)	37.46	(6.75)	36.81	(6.58)	***	35.63	(6.40)	***	39.65	(5.32)	***

*Difference in means is statistically different from the sample of students attending their assigned school at the: * 10% level; ** 5% level, *** 1% level. Statistics are weighted.*

Table 3-2 continued (2)

Summary Statistics for Student, Household, and Private School Supply and Demand Variables by Outcome Category - *Continued*

Variable	Assigned School Mean	Assigned School Std. Dev.	Within-District School of Choice Mean	Within-District School of Choice Std. Dev.		Opts Out of District Mean	Opts Out of District Std. Dev.		Private School Mean	Private School Std. Dev.	
Additional Household Attributes *(continued)*											
Father's Education - High School (%)	28.44	(45.12)	26.83	(44.34)		22.78	(42.07)		18.14	(38.55)	***
Father's Education - Some College (%)	26.53	(44.15)	28.79	(45.31)		35.02	(47.84)	**	28.13	(44.98)	
Father's Education - College (%)	15.84	(36.52)	17.51	(38.03)		18.02	(38.55)		25.96	(43.86)	***
Father's Education - Professional (%)	12.50	(33.08)	11.14	(31.49)		10.47	(30.71)		24.78	(43.19)	***
Mother's Education - High School (%)	27.20	(44.50)	28.24	(45.04)		22.73	(42.00)		14.33	(35.05)	***
Mother's Education - Some College (%)	34.80	(47.64)	36.07	(48.05)		40.00	(49.10)		35.43	(47.85)	
Mother's Education - College (%)	14.98	(35.70)	13.63	(34.33)		13.82	(34.59)		29.89	(45.79)	***
Mother's Education - Professional (%)	8.09	(27.26)	9.65	(29.55)		8.71	(28.26)		17.97	(38.41)	***
HH is At or Below Poverty Line (%)	25.30	(43.48)	20.18	(40.15)	***	23.61	(42.56)		4.91	(21.62)	***
Suburb (%)	46.65	(49.89)	33.36	(47.18)	***	36.96	(48.39)	***	34.59	(47.58)	***
Rural (%)	13.95	(34.65)	12.79	(33.42)		23.11	(42.26)	***	9.51	(29.34)	***
Northeast (%)	21.90	(41.36)	15.83	(36.52)	***	13.25	(33.97)	***	24.52	(43.03)	**
Midwest (%)	16.40	(37.04)	16.01	(36.69)		16.39	(37.10)		24.02	(42.73)	***
West (%)	20.65	(40.49)	31.47	(46.47)	***	20.29	(40.30)		18.86	(39.13)	
Private School Supply & Demand											
K-12 Private Enrollment in CT (%)	10.66	(9.14)	11.46	(10.13)	**	9.43	(7.46)	**	21.80	(18.42)	***
Private Schools, Number in Co	133.50	(272.73)	168.02	(317.18)	***	86.54	(168.96)	***	215.74	(374.98)	***
Average Size of Private Schools in Co	200.78	(74.17)	204.67	(74.26)		192.07	(73.12)	*	219.46	(70.17)	***
Catholic School Dummy, Co (%)	89.44	(30.74)	93.39	(24.86)	***	79.26	(40.63)	***	96.26	(18.98)	***

*Difference in means is statistically different from the sample of students attending their assigned school at the: * 10% level; ** 5% level, *** 1% level. Statistics are weighted. 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.*

Table 2-3: Summary Statistics for Students' Residentially Assigned School Districts Variables by Outcome Category

Variable	Assigned School Mean	Assigned School Std. Dev.	Within-District School of Choice Mean	Within-District School of Choice Std. Dev.		Opts Out of District Mean	Opts Out of District Std. Dev.		Private School Mean	Private School Std. Dev.	
School District Variables											
Number of Schools	103.87	(214.21)	137.98	(232.14)	***	44.74	(55.79)	***	124.26	(226.82)	***
Student-Teacher Ratio	16.91	(2.67)	17.44	(2.76)	***	16.83	(2.67)		16.98	(2.55)	
Title I Eligible (%)	52.14	(26.62)	53.95	(24.90)		54.08	(26.74)		54.17	(24.86)	**
African American (%)	18.14	(19.75)	20.75	(21.58)	***	17.84	(20.75)		22.80	(23.04)	***
Hispanic (%)	17.43	(22.07)	21.41	(23.04)	***	17.05	(21.93)		18.29	(22.39)	
Other Race (%)	5.48	(9.38)	8.50	(14.44)	***	4.47	(5.23)		6.09	(9.64)	**
Free Lunch Eligible (%)	32.44	(19.91)	38.40	(19.82)	***	31.95	(16.19)		34.65	(21.56)	***
Total Expenditures per Pupil ($K)	8.68	(2.26)	8.66	(2.12)		8.02	(1.78)	***	8.95	(2.03)	***
Federal Revenues per Pupil ($K)	0.58	(0.39)	0.66	(0.31)	***	0.56	(0.27)		0.61	(0.35)	**
State Revenues per Pupil ($K)	4.06	(1.59)	4.47	(1.69)	***	4.16	(1.64)		4.02	(1.62)	
Local Revenues per Pupil ($K)	3.96	(2.28)	3.50	(2.16)	***	3.31	(1.85)	***	4.25	(2.19)	***
School District Resident Attributes											
Median Household Income ($K)	44.90	(14.99)	42.69	(11.99)	***	42.92	(12.16)	*	44.26	(12.12)	
Households Below Poverty Line (%)	11.85	(6.24)	12.86	(5.79)	***	11.69	(5.45)		11.79	(5.82)	
Minority (%)	26.59	(19.60)	31.98	(20.14)	***	24.34	(17.26)		29.69	(20.37)	***
Adults, HS Diploma (%)	47.58	(13.15)	47.35	(12.57)		48.94	(12.73)		47.75	(11.85)	
Adults, College Degree (%)	25.24	(12.39)	25.36	(11.55)		23.14	(10.56)	**	25.72	(10.87)	

*Difference in means is statistically different from the sample of students attending their assigned school at the: * 10% level; ** 5% level, *** 1% level. Statistics are weighted.*

Table 2-4: Multinomial Probit Marginal Effect Estimates - Base Model

	(1)	(2)	(3)
	Within-District	**Out of District**	**Private**
Likely Reasons for Opting Out of District			
Open Enrollment State (2001)	-0.032	0.020	0.020
	(1.79)*	(1.30)	(0.77)
Charter State (2000)	-0.029	0.001	-0.036
	(1.28)	(0.12)	(1.68)*
Recently Moved	0.027	0.056	-0.035
	(2.03)**	(5.67)**	(3.60)***
Parent is a Teacher	0.035	0.020	0.025
	(1.65)*	(1.61)	(1.49)
Disability	0.016	0.010	-0.009
	(1.27)	(1.65)*	(1.07)
Student & Household Attributes			
Single Parent Home	0.007	0.017	0.008
	(0.51)	(2.10)**	(0.79)
Neither Parent Living at Home	-0.002	-0.019	-0.079
	(0.05)	(3.40)***	(6.34)***
African American	0.022	0.005	-0.056
	(1.33)	(0.70)	(4.44)***
Hispanic	-0.005	0.023	-0.061
	(0.36)	(2.22)**	(5.53)***
Other Race	0.018	0.005	-0.021
	(0.81)	(0.49)	(1.48)
Income Category: $5K > X > $10K	-0.055	0.045	-0.061
	(1.40)	(0.84)	(2.12)**
Income Category: $10K > X > $15K	0.011	0.050	0.001
	(0.21)	(0.88)	(0.02)
Income Category: $15K > X > $20K	-0.033	0.120	0.031
	(0.82)	(1.62)	(0.53)
Income Category: $20K > X > $25K	-0.016	0.051	0.06
	(0.37)	(0.99)	(1.04)
Income Category: $25K > X > $30K	0.008	0.083	0.078
	(0.15)	(1.24)	(1.17)
Income Category: $30K > X > $35K	-0.012	0.059	0.099
	(0.24)	(1.04)	(1.57)
Income Category: $35K > X > $40K	0.017	0.052	0.169
	(0.29)	(0.92)	(2.10)**
Income Category: $40K > X > $50K	-0.042	0.091	0.234
	(1.01)	(1.30)	(2.77)***

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors are clustered at the school district level. Omitted income category is Less than $5,000; omitted. Number of private schools in county is measured one-in-10 schools. Additional explanatory variables include indicator variables for observations that initially had missing data. Number of Private schools is measured 1-in-1000. Number of district schools is measured 1-in-100.

Table 3-4 continued

Multinomial Probit Marginal Effect Estimates - Base Model – *Continued*

	(1)	(2)	(3)
	Within-District	**Out of District**	**Private**
Student & Household Attributes (Continued)			
Income Category: $50K > X > $75K	0.002	0.032	0.263
	(0.05)	(0.85)	(3.30)***
Income Category: $75K > X > $100K	-0.049	0.062	0.345
	(1.27)	(1.09)	(3.78)***
Income Category: $100K > X > $200K	-0.052	0.073	0.409
	(1.38)	(1.12)	(4.30)***
Income Category: > $200K	-0.063	0.015	0.517
	(1.70)*	(0.41)	(5.03)***
Suburb	-0.029	-0.002	-0.071
	(2.04)**	(0.21)	(4.68)***
Rural	-0.037	0.018	-0.062
	(1.93)*	(1.42)	(3.89)***
Northeast	-0.098	0.011	0.058
	(6.43)***	(0.85)	(1.43)
Midwest	-0.071	0.020	0.084
	(4.81)***	(1.26)	(2.10)**
South	-0.085	0.010	0.052
	(4.67)***	(1.00)	(1.58)
Private School Supply			
Private Schools in County, Number	-0.051	0.001	0.150
	(2.43)**	(0.10)	(4.91)***
Proportion of Catholic Private Schools in County	0.056	-0.060	0.099
	(1.59)	(2.90)***	(2.02)**
School District Variables			
Number of Schools	0.055	-0.146	-0.047
	(1.75)*	(4.48)***	(1.84)*
Free Lunch Eligible, Proportion	0.086	0.026	0.052
	(1.56)	(1.17)	(0.85)
Total Expenditures Per Child ($K)	0.004	-0.003	0.004
	(1.04)	(1.48)	(0.84)
Median Household Income in SD (S10K)	-0.003	0.001	-0.014
	(0.39)	(0.23)	(2.34)**
Observations	7650	7650	7650

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors are clustered at the school district level. Omitted income category is Less than $5,000; omitted. Number of private schools in county is measured one-in-10 schools. Additional explanatory variables include indicator variables for observations that initially had missing data. Number of Private schools is measured 1-in-1000. Number of district schools is measured 1-in-100.

Table 2-5: Multinomial Probit Marginal Effect Estimates - Extended Model (Abbreviated)

	Within-District	Out of District	Private
Likely Reasons for Opting Out of District			
Open Enrollment State (2001)	-0.033	0.031	0.041
	(1.98)**	(1.66)*	(1.61)
Charter State (2000)	-0.030	0.001	-0.043
	(1.22)	(0.08)	(1.70)*
Recently Moved	0.023	0.055	-0.026
	(1.72)*	(5.61)***	(2.90)***
Parent is a Teacher	0.034	0.022	-0.0002
	(1.61)	(1.62)	(0.02)
Disability	0.017	0.009	-0.010
	(1.40)	(1.50)	(1.43)
Additional Model Controls			
Base & Extended Student, HH	Yes	Yes	Yes
Base & Extended SD & Resident	Yes	Yes	Yes
Private School Supply	Yes	Yes	Yes
Observations	7650	7650	7650
Absolute value of z statistics in parentheses			
* significant at 10%; ** significant at 5%; *** significant at 1%			

Standard errors are clustered at the school district level. Omitted income category is Less than $5,000; omitted. Number of private schools in county is measured one-in-10 schools. Additional explanatory variables include indicator variables for observations that initially had missing data.

Table 2-6: Multinomial Probit Marginal Effect Estimates - Extended Model (Unabbreviated)

	Within-District	Out of District	Private
Likely Reasons for Opting Out of District			
Open Enrollment State (2001)	-0.033	0.031	0.041
	(1.98)**	(1.66)*	(1.61)
Charter State (2000)	-0.030	0.001	-0.043
	(1.22)	(0.08)	(1.70)*
Recently Moved	0.023	0.055	-0.026
	(1.72)*	(5.61)***	(2.90)***
Parent is a Teacher	0.034	0.022	-0.0002
	(1.61)	(1.62)	(0.02)
Disability	0.017	0.009	-0.010
	(1.40)	(1.50)	(1.43)
Student & Household Attributes			
Single Parent Home	-0.006	0.010	-0.005
	(0.48)	(1.37)	(0.52)
Neither Parent Living at Home	0.022	-0.017	-0.078
	(0.56)	(2.72)***	(9.82)***
African American	0.021	0.010	-0.061
	(1.36)	(1.14)	(5.20)***
Hispanic	-0.007	0.022	-0.043
	(0.47)	(2.06)**	(4.13)***
Other Race	-0.003	0.008	-0.027
	(0.18)	(0.68)	(2.12)**
Female	0.001	-0.004	0.018
	(0.16)	(1.10)	(2.69)***
Age	-0.025	0.002	0.011
	(2.08)**	(0.38)	(1.07)
Household Size	-0.009	-0.003	-0.008
	(2.59)***	(1.48)	(2.33)**
Income Category: $5K > X > $10K	-0.056	0.042	-0.050
	(1.50)	(0.82)	(1.87)*
Income Category: $10K > X > $15K	0.008	0.047	-0.013
	(0.16)	(0.83)	(0.31)
Income Category: $15K > X > $20K	-0.038	0.128	0.02
	(1.03)	(1.62)	(0.39)
Income Category: $20K > X > $25K	-0.020	0.045	0.043
	(0.48)	(0.90)	(0.87)
Observations	7650	7650	7650

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors are clustered at the school district level. Omitted income category is Less than $5,000; omitted. Number of private schools in county is measured one-in-10 schools. Additional explanatory variables include indicator variables for observations that initially had missing data. Number of Private schools is measured 1-in-1000. Number of district schools is measured 1-in-100.

Table 3-6 continued (1)

Multinomial Probit Marginal Effect Estimates - Extended Model (Unabbreviated)–*Continued*

	Within-District	Out of District	Private
Student & Household Attributes *(Continued)*			
Income Category: $25K > X > $30K	0.002	0.082	0.050
	(0.04)	(1.18)	(0.93)
Income Category: $30K > X > $35K	-0.020	0.058	0.056
	(0.46)	(1.00)	(1.14)
Income Category: $35K > X > $40K	0.008	0.051	0.109
	(0.15)	(0.91)	(1.72)*
Income Category: $40K > X > $50K	-0.044	0.105	0.143
	(1.17)	(1.32)	(2.06)**
Income Category: $50K > X > $75K	0.007	0.036	0.158
	(0.16)	(0.87)	(2.44)**
Income Category: $75K > X > $100K	-0.042	0.079	0.202
	(1.10)	(1.15)	(2.65)***
Income Category: $100K > X > $200K	-0.041	0.102	0.239
	(1.03)	(1.22)	(2.87)***
Income Category: > $200K	-0.040	0.033	0.334
	(0.95)	(0.62)	(3.34)***
Mother's Education - High School	0.026	-0.013	0.080
	(1.42)	(2.14)**	(3.48)***
Mother's Education - Less than College	0.011	-0.007	0.125
	(0.71)	(1.11)	(5.82)***
Mother's Education - College	-0.006	-0.013	0.211
	(0.32)	(2.08)**	(5.75)***
Mother's Education - Professional	0.024	-0.006	0.194
	(0.93)	(0.72)	(4.84)***
Mother's Age	-0.002	-0.001	0.003
	(2.28)**	(1.77)*	(5.28)***
Suburb	-0.022	-0.001	-0.075
	(1.45)	(0.08)	(4.86)***
Rural	-0.034	0.030	-0.060
	(1.78)**	(1.59)	(3.99)***
Northeast	-0.077	0.028	0.043
	(3.10)***	(1.22)	(0.90)
Midwest	-0.049	0.022	0.059
	(2.00)**	(1.11)	(1.32)
South	-0.057	0.012	0.017
	(2.27)**	(0.84)	(0.46)
Observations	7650	7650	7650
Absolute value of z statistics in parentheses			
* significant at 10%; ** significant at 5%; *** significant at 1%			

Standard errors are clustered at the school district level. Omitted income category is Less than $5,000; omitted. Number of private schools in county is measured one-in-10 schools. Additional explanatory variables include indicator variables for observations that initially had missing data. Number of Private schools is measured 1-in-1000. Number of district schools is measured 1-in-100.

Table 3-6 continued (2)

Multinomial Probit Marginal Effect Estimates - Extended Model (Unabbreviated)– *Continued*

	Within-District	Out of District	Private
Private School Supply			
Private Schools in County, Number	-0.060	-0.003	0.115
	(2.65)***	(0.24)	(3.43)***
Proportion of Catholic Private Schools in County	0.082	-0.052	0.083
	(2.18)**	(2.60)***	(2.00)**
School District Variables			
Number of Schools	0.086	-0.146	-0.0003
	(2.26)**	(5.03)***	(0.01)
Free Lunch Eligible, Proportion	0.099	0.037	-0.023
	(1.64)	(1.10)	(0.37)
Title I Eligible, Proportion	0.024	0.012	-0.028
	(0.68)	(0.68)	(0.77)
African American, Proportion	0.046	-0.027	0.129
	(0.69)	(0.72)	(1.75)*
Hispanic, Proportion	0.036	-0.010	0.050
	(0.65)	(0.32)	(0.89)
Other Race, Proportion	0.252	-0.043	0.092
	(2.77)***	(0.81)	(0.84)
Total Expenditures Per Child ($K)	0.005	-0.002	-0.002
	(1.07)	(0.90)	(0.57)
Federal Revenue per Child ($K)	-0.024	0.002	-0.009
	(1.10)	(0.19)	(0.38)
Elementary District (%)	-0.128	0.009	-0.06
	(21.13)***	(0.21)	(1.98)**
Student-Teacher Ratio	0.006	0.001	-0.005
	(1.82)*	(0.65)	(1.30)
School District Variables			
Median Household Income in SD (SK)	-0.022	-0.002	-0.026
	(1.90)*	(0.40)	(2.60)***
Fraction of Household's Below Poverty Level	-0.140	-0.096	-0.902
	(0.55)	(0.83)	(3.38)***
Minority	-0.084	0.120	0.118
	(0.58)	(1.36)	(0.75)
Minority - Squared	0.005	-0.115	0.050
	(0.03)	(1.31)	(0.28)
Adult Residents with at Most a High School Diploma	0.054	-0.053	0.438
	(0.26)	(0.61)	(2.10)**
Adult Residents with at Least a College Degree	0.198	-0.033	0.270
	(1.03)	(0.38)	(1.32)
Observations	7650	7650	7650

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors are clustered at the school district level. Omitted income category is Less than $5,000; omitted. Number of private schools in county is measured one-in-10 schools. Additional explanatory variables include indicator variables for observations that initially had missing data. Number of Private schools is measured 1-in-1000. Number of district schools is measured 1-in-100.

Table 2-7: Multinomial Probit Marginal Effect Estimates, Include Move Type - Extended Model

	(1) Within-District	(2) Out of District	(3) Private
Likely Reasons for Opting Out of District			
Open Enrollment State (2001)	-0.033	0.030	0.041
	(1.96)**	(1.63)	(1.61)
Charter State (2000)	-0.030	-0.001	-0.044
	(1.20)	(0.07)	(1.71)*
Recently Moved	-0.018	0.127	-0.011
	(0.60)	(5.04)***	(0.54)
Within-Count Move	0.039	-0.024	-0.027
	(0.96)	(4.45)***	(1.39)
Parent is a Teacher	0.033	0.024	-0.0002
	(1.56)	(1.72)*	(0.01)
Disability	0.016	0.009	-0.010
	(1.35)	(1.62)	(1.40)
Additional Model Controls			
Base & Extended Student, HH	Yes	Yes	Yes
Base & Extended SD & Resident	Yes	Yes	Yes
Private School Supply	Yes	Yes	Yes
Observations	7650	7650	7650
Absolute value of z statistics in parentheses			
* significant at 10%; ** significant at 5%; *** significant at 1%			

Standard errors are clustered at the school district level. Omitted income category is Less than $5,000; omitted. Number of private schools in county is measured one-in-10 schools. Additional explanatory variables include indicator variables for observations that initially had missing data.

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3. CHAPTER 4: Winning and Losing School Districts – An Analysis into the Flow of Students Among the Public and Private School Sectors

3.1 Introduction

This chapter is an analysis of the attributes of school districts that are observed to lose, keep, or attract public school students. In light of survey data that provides the rare opportunity to observe students entering and exiting school districts, I take an unconventional approach to examining what qualities appear to distinguish districts as net gainers or net losers of students. In particular, I estimate how measures of public (sector) school choice (PSC) and statewide education policies influence the net flow of students within the public school system.

For years public schools have competed with the private sector for student enrollment. And additional competition within the public sector is also a growing phenomenon; school districts are systems increasingly less closed-off with the advent and growth of interdistrict open enrollment policies and charter schools. However, an incentive exists (varying in degree) across districts to retain their students – even attract students into their borders. Per-student funding is tied to the number of students attending school in a district. Also, as school quality improves, the value added and teacher performance incentive packages of some teachers and administrators are positively impacted. Thus, incentive is strong to retain high performing students in their public schools or districts.

The ultimate irony, however, is that choice that comes in the form of private schooling options, public schools of choice, and even homeschooling, may result in students opting out of district – but to their perceived betterment. The best match for a student may be a private school or a public school in an alternative district; while the school district the student resides in may have benefited from having that student enrolled in the student's public school district. While it is reasonable to expect that districts that keep or attract students – versus districts that lose their students to private schools or other districts – might be better quality districts where residents are content with the offered educational services, my findings suggest this is not the case. I find that school districts that keep their students – as well as school districts that, on net, attract students into their public schools – tend to be schools in poorer areas with a limited supply of schooling options. I do, however,

find some mixed evidence that increased charter school competition in a district may be positively related to the retention rate of a district. Additionally, I do not find that public schools of choice have a positive net effect on the flow of students into the district's schools. In particular, charter schools are more likely to be present in school districts that are net losers of students. Because of the timing of this data, this may reflect that public schools of choice have been well targeted to areas that are losing students to the private sector (Glomm et al., 2005). Perhaps more timely data that reflect the effects of more tenured schools of choice on the net flow of students would reveal a different result.

I find very little evidence that statewide educational polices affect the net flow of students in a district. This may be attributable to the general equilibrium nature of this question. As students move across districts, for any net losing school district in a state, there is potentially a net gaining district. It may also reflect the limited power of this analysis based on the sample size and limited identification of a particular district, which often reduces to a nonrandom sample of a few students.

The findings of this analysis are generally descriptive and are not meant to be generalized over the larger population because the sample is not random nor is it representative of the universe of students in the school districts. However, the findings do suggest reasonable outcomes worth exploring – that PSC schools, charter schools in particular, tend to locate in districts that are losing students and that net-loser school districts, compared with districts that keep their students or net-gaining districts, may very well signal better quality school districts with more diverse options available to facilitate positive student-school matches.

In the following section I briefly discuss the most relevant literature regarding how PSC schools and education policies affect the retention and transfer-in rates of school districts, and I explain how this chapter 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.

3.2 Contribution

Little if any research examines how PSC schools affect the retention rates of school districts. However, a number of studies have examined public sector and private sector school choice, examining – at least partly – how aspects and qualities of a student's school

district contribute to opting either for a charter school, a private school alternative, or an interdistrict open enrollment program.

Studies tend to find that students who opt into charter schools or participate in interdistrict open enrollment policies tend to exit less affluent districts for more affluent districts with higher expenditures per student (Holme and Richards 2009). And some heterogeneity is observed in the literature as to whether students tend to opt into districts where their peers better reflect their race or ethnicity or whether students transfer into districts with higher student test scores (Reback 2008; Welsch et al. 2010). Additionally, private schools tend to locate in areas where school districts have swollen elementary-grade class sizes, poor public school test scores, and heterogeneity in residential income, education, and race (Barrow 2006). Additionally, studies find that open enrollment policies can lead to residential mobility- as households opt to reside in areas with lower school quality and lower costs of living and subsequently enroll their students out of district – in particular, to districts with better performing schools and higher costs of living (Cullen et al. 2005).

The contribution of this analysis is to examine the factors that affect the retention of a school district's public school students – in particular how PSC schools as well as state education policies, on net, affect the retention rates of school districts. The analysis is a descriptive study, but it contributes to the existing literature by collectively looking at districts that lose, keep, or gain students to examine how, and perhaps why, these districts differ. I consider the various institution-based alternatives to the residentially assigned public school district in this analysis and offer a depiction of what the flow of students across the public (and private) sector says about school quality, the availability of alternatives, and the influence of public schools of choice.

3.3 Data

3.3.1 *Building the Dataset*

As previously stated, I utilize a dataset that offers a unique opportunity to observe students entering and exiting districts. This data comes from survey data that matches students to their residentially assigned districts, matches public school students to the districts they attend, and also identifies private school students. Aggregate level datasets that account for district enrollment, such as the Common Core of Data (CCD), do not identify the districts of residence for the students enrolled in a particular district. Thus while district enrollment can be measured to shrink or grow over time, individual districts cannot be identified as losing particular students to other districts (or private school) or attracting students from other districts. Thus I employ the survey data that offers substantially more detail regarding such student mobility across districts and educational sectors to address this question of net-gaining and net-losing districts.

The principal component of my dataset is based on 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.[66] 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. As in Chapter 2, 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. Recall that 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

[66]Refer to Chapter 2 for further details on the ECLS-K data-set.

Data (CCD), and the geographic identifiers can be matched with 2000 Decennial Census data.

As with the previous two chapters, 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. I also supplement with data from the Census 2000 School District Tabulation (STP2), which were procured from the NCES's School District Demographics System; these data provide information about demographics and the socioeconomic statuses of residents at the school district level. Finally, I supplement the dataset with Census 2000 Cartographic Boundary Files at the school district and census tract levels. These files are a form of geographic information system (GIS); they are spatial data represented by maps of geographical areas and borders.

I match the geographic identifiers in the ECLS-K data with the subsequent components listed above according to the process laid out in the preceding chapter. All school district measures from the CCD reflect one-year lagged measures with the exception of the number of schools in the district. Once the sample of third-grade students has been matched to their residentially assigned school districts as well as further identified as either opting out of district for another public school or opting into private school, I then aggregate the data to the school district level. This yields school district level data with the number of students observed to reside in the district (which I also refer to as the number of student observations), the number of students observed to opt out of the district for public school in another district, and the number of students who opt into private school. This aggregation also reveals the number of students observed to opt into a specific district even though they reside in another district. Unfortunately, these samples of students are not representative of the district; they merely reflect the students sampled from particular schools that the survey included when trying to create a nationally representative sample of students in the kindergarten class of 1998-1999. As a result, on initially aggregating the data to the district level, a number of districts are observed to have students enrolled but are not observed to have students living in them; a number of school districts have only one student observed to live in the district, or perhaps a dozen live in the district but all attend private

school. Additionally, no obvious analytical or sampling weights are present to attribute to the school districts included in this analysis. Thus I stress that the results and analysis presented here are not to be generalized and should be cautiously interpreted.

On generating this district-level dataset, I further collect additional variables not used in the previous chapter. To control for school quality, I include the average high school graduation rate of twelfth grade students in the district from the 2000-2001 CCD data.[67] And from the Census 2000 School District Tabulation (STP2) files, via the NCES's School District Demographics System, I also include median property tax rates. Additionally, I collect median housing values and total population counts; however because of collinearity and the very small sample size giving way to a degrees of freedom concern, I provide these latter school district level variables only in the summary statistics.

With respect to open enrollment policies, I assign a school district as being located in a state that has either voluntary or mandatory interdistrict open enrollment policies as of 2001 in accordance with the state classification of open enrollment policies by the NCES (2003).[68] Finally, I also include the type-specific (charter school or magnet school) PSC measures using the 2000-2001 CCD data. These PSC measures are limited to schools that offered third grade in the 2000-2001 school year.[69] Thus the PSC variables represent a one-year lag of school district variables and are representative only of elementary-inclusive schools. The one-year lag assumes households make the public-private decision cognizant of the PSC available in their school districts the previous year and attempts to address the concern that more-tenured PSC schools tend to differently affect student enrollment compared to newer schools that have yet to prove themselves (Booker et al. 2007). By using only elementary-inclusive schools in the PSC measures, I avoid including irrelevant PSC

[67] The NCES's Build a Table tool was used for this. I divided the number of diploma recipients in the district by the number of twelfth grade students in the district for the 2000-2001 academic year. I cap the graduation rate at 100 percent.

[68] This report is not the same one used for open-enrollment policy classification in the preceding chapters . In the previous chapters, I used the report that had the most consistently verified list of states with interdistrict open enrollment policies across various reports. Thus, inconsistencies exist in the states classified as having mandatory interdistrict open enrollment policies across the reports used in the chapters. I also estimate the model when states with mandated inter-district open enrollment policies are assigned according to the Missouri Report (2009), as in the previous chapters, rather than the NCES (2003) report, the main findings are not significantly changed. For the ordered probit, the estimates generally retain their sign and statistical significance. The coefficients, however, tend to double or triple in magnitude. However, this is not necessarily concerning given the lack of expected coefficient precision as point estimates.

[69] A thorough discussion of these variables and their creation is presented in Chapter 2.

schools – such as magnet middle schools or charter high schools – to the sample of third graders.

Regarding PSC measures, I employ only type-specific controls. For both charter school and magnet schools, I create dummy variables that indicate the presence of at least one PSC school in a district. I further include two PSC competition measures: the number of PSC schools in a district and the fraction of district enrollment attending PSC schools. This process yields four type-specific PSC competition measures.

It should be noted that the dependent variable – the classification of a school district as either a net-gaining district or a net-losing district – is based on enrollment decisions reflective of the 2001-2002 school year. The explanatory variables in the model generally reflect the 2000-2001 academic year (for CCD and finance data) and the 1999-2000 academic year (for Census data).

The dependent variable represents net changes in district enrollment and is calculated as follows. If the number of exiting students exceeds the number of students transferring-in, then the district is assigned a net-losing status; if the number of exiting students is equally offset by the number of students transferring in, then the district is assigned a status of no net change or is referred to as keeping its students; if the number of transferring-in students exceeds the number of exiting students, then the district is assigned a net-gaining status. Students that exit districts are represented by students who either attend public school in a school district other than their residentially assigned school district or are students that attend private school. Because a far greater number of students are observed to opt for private school rather than opt out of district for a public school alternative, the identification on net-losing districts is predominantly driven by private school students. Students referred to here as transferring-in are students that report attending a public school in one district but actually reside in a different district. The discrepancy is that their geographically assigned district (procured from the GIS matching process, discussed at length in Chapter 2) is not the same district where the student reports attending public school as recorded by the ECLS-K survey.

3.3.2 *Data Limitations*

The most substantial limitation to the data is that the observed student behavior within a district is not based on a representative sample of students from that district; thus the findings are not generalizable. The findings are generally descriptive and are meant to highlight and identify underlying trends that affect the flow of students in the public sector that can be subsequently explored under more ideal data conditions. Additionally the sample size of students is prone to be very small in any given district. I ultimately pursue the analysis limiting the sample to school districts with at least five students observed to reside in the district, thus reducing the sample size even more. This limitation also impedes the inclusion of more explanatory variables. Thus the model controls only for a few key variables – and those included are meant to maximize explanatory power, as they are often highly correlated with other measures that are not included in the model but are expected to influence the flow of students across the public and private sectors.

Additional limitations in the data are outlined in the previous chapters. Regarding the PSC measures, recall from Chapter 2 that a significant number of schools had missing values for magnet school status in the 2000-2001 CCD survey. In the dataset used in this analysis, approximately 30 percent of school districts are missing magnet school identifiers.

3.3.3 *The Sample*

I conduct my analysis on school districts with at least five student observations – in particular, school districts have data on at least five survey students reported to live in them. Thus I present summary statistics for only these school districts. These descriptive statistics are presented in Table 4-1. I include the number of valid observations per variable. Note that in the analysis missing variables are changed to zero, and the model includes indicator variables to denote initially missing values.

As one can see, 64 percent of the districts are net losers of students, consistent with students opting into private school more frequently than into another school district. And overall, 7 percent of districts are net gainers. Looking a bit closer at the student mobility observed in the sample, the typical district has 20 observed survey students residing in the district and 15 survey students enrolled in the district; the remaining five survey students

observed tend to opt into private school. The average district in the sample has no survey students transferring in or out.

With respect to the PSC measures, while 13 percent of the districts in the sample have at least one magnet school and 13 percent of the sample of districts also have at least one charter school, the number of PSC schools and the fraction of district enrollment are not similar across school types. The typical district that has at least one magnet school has an average of 20 magnet schools, and these schools enroll approximately 19 percent of the district's enrollment. However, the typical district that has at least one charter school has four charter schools, which account for 4 percent of the district's enrollment.[70]

Regarding the statewide education policies, 89 percent of the school districts in the sample are in states that had charter legislation as of the year 2000. And while 17 percent of the school districts are in states that had mandatory interdistrict open enrollment policies as of 2001, 53 percent of school districts are in states that had voluntary interdistrict open enrollment policies as of 2001.

With respect to the school district variables, in a typical district 62 percent of students are white, 17 percent of students are black, and 15 percent of students are Hispanic. Additionally the average student-teacher ratio is 17, and 93 percent of high school seniors graduate from high school in the typical school district in the sample. Additionally, in a typical district 55 percent of schools are Title I eligible and 29 percent of students are free-lunch eligible. The average district has 43 schools and spends an average of $9,000 per student, the majority coming from local revenues per student.

Finally with respect to the residents of the school districts, and recall these measures reflect the 1999-2000 academic year as collected from the 2000 Census, a typical district in the sample has just fewer than 200,000 people and a median household income of $47,000. For the typical district, 48 percent of residents 25 and older have at most a high school diploma, and 26 percent of residents in this age category have at least a four-year college degree. On average, 24 percent of a district's residents are of a minority and 10 percent of households are below the poverty level. Finally, in the typical district, the median housing values are $143,000, and the median real estate tax rate is 1.3 percent.

[70] Recall that all PSC measures are one-year lags that reflect only elementary grade inclusive schools. Thus the percentage of district enrollment refers to the percentage of district enrollment attending any school that offered third grade during the 2000-2001 school year.

3.4 Empirical Model

The primary method of analysis I employ is ordered probit, although I also estimate OLS and probit model specifications for robustness checks. Under the assumption of the ordered probit model, districts rank as the least preferred outcome the outcome of losing more students (to either private schools or public schools in other districts) than they attract from other districts. Then they rank keeping their students – or having no net change in enrollment – as a more preferred outcome. And finally the most preferred outcome is that of attracting more students from other districts than a district loses to either private school or other districts. The simple ordered probit model I estimate is depicted below in equation 1:

[1] $$Pr\,[Y_i=1, 2] = f(C_i, D_i, R_i, E_s)$$

where *i* represents a school district and *s* represents a state. In the model, **Y** is an indicator variable for which the default category is the classification of a net-losing district, where the number of students exiting the district exceeds the number of incoming transfers; **Y** takes the value of one if district *i,* on net, keeps their students, as the number of observed private school students and students opting out of district equally offsets the number of students transferring in; **Y** takes the value of two if district *i,* on net, gains students, as the number of students transferring in exceeds the number of exiting students. Looking categorically at the regressors, **C** is a set of type-specific (either charter school or magnet school) PSC variables measured at the district level. **D** is a set of variables describing the school district; **R** is a set of resident characteristics at the school district level; **E** is a set of state-level education policies.

With respect to the dependent variable, the assumption is rather lofty. Without measures of individual student performance or other socioeconomic and demographic measures, it is a hefty assumption to expect that all transferring-in students and all exiting students are equally attractive to a district. No discretion is allowed for student ability or performance; a lack of such controls equates all students whether they exit a district to opt into an academically competitive private school or a behavioral program in another district – or whether they are transferring into a district as part of a gifted and talented program or as part of a high-risk student outreach program. This again underscores the importance of not generalizing the outcomes of the analysis without additional study.

Recall that the district-level variables in **C** are one-year lags representative of only elementary-inclusive schools. The measures include type-specific PSC dummy variables as well as two type-specific competition measures: the number of PSC schools in a district and the fraction of district enrollment. Refer to Chapter 2 for a more complete description of these variables and how they enter the model as type-specific PSC measures.

The district-level variables in **D** are also one-year lags. These variables include the number of schools in the district (this is the sole school district variable that represents the current academic year of the sample), the proportion of Title I eligible schools, average student-teacher ratio, and the average graduation rate of high school seniors. Additional district-level finance variables also included in **D** are per-student levels of total expenditures and local revenues. Again, additional variables were included in alternative specifications, but because of issues of collinearity and a very small sample size, the variables ultimately included in the analysis were considered the best option. Additionally, measures for race, free lunch eligibility, and federal revenues were highly correlated with school district resident income and minority measures. And these resident variables were less foiled by missing data than the school data from the CCD or the financial data of the Census of Governments (form F33).

Because of the small sample size and the fact that students observed in the data do not necessarily reflect a representative sample of a district's students, I estimate the model at different thresholds of student observations. Initially I estimate the model conditional on districts having at least five survey students observed to reside in a specific district; I then rerun the model at student observation thresholds of 10 and 15. Of course, an obvious tradeoff occurs between increasing the number of survey students representing any one district and the number of districts observed to have these larger counts of student observations. I present only the results of the model estimated using the threshold of five students observed to reside in a district; however, I comment on how the estimates change as that threshold is increased.[71]

[71] I also estimate a model specification in which school districts are weighted by the number of survey students observed to reside in them. The estimates for the key variables of interest do not change substantially from the presented marginal effect estimates of the ordered probit model. Regarding the type-specific PSC measures, the weighted estimates present statistically significant, negative coefficients on the charter school dummy variable across threshold levels. The charter school competition measures are negative and statistically significant in all threshold cases except that of 15 students. The weighted estimates rarely yield significant magnet school measures coefficients. With respect to the education policy

3.5 Results

I estimate an ordered probit model in which I rank the possible net outcomes for a school district as follows – (0) losing students, (1) keeping students, and (2) gaining students. The key variables of interest are a few type-specific charter and magnet school indicator variables and competition measures as well as various statewide education polices: being in a charter state, being in a state with a mandatory interdistrict open enrollment policy, or being in a state with a voluntary interdistrict open enrollment policy.[72] I also include a select set of district level variables profiling a district's infrastructure, financial practices, student enrollment, and resident characteristics. In Tables 4-2 and 4-3, I present the marginal effect estimates for the ordered probit model when the model is run on school districts that have at least five survey students observed to be residing there. Table 4-2 presents the marginal effect estimates as the variables contribute to a district keeping its students (all outgoing students are equally offset by incoming students) rather than being a net-losing district. Table 4-3 presents the marginal effect estimates as the variables contribute to a district being a net-gainer of students as opposed to a being a net-losing district.

3.5.1 *Keeping Students*

Column one in Table 4-2 presents the marginal effect estimates with no PSC measure. Columns two through four present the results when the model includes district-level charter and magnet school measures. Column two presents the estimates when only the type-specific PSC dummy variables are included. And columns three and four subsequently add alternative type-specific PSC competition measures – the numbers of charter and

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measures, the weighted estimates yield some new findings. Marginal effect coefficients examining the probability a school district keeps students rather than loses students are negative and statistically significant on the charter state indicator variable and the voluntary inter-district open enrollment dummy – however, these coefficients are not statistically significant across all threshold levels. The same holds for marginal effect coefficient estimates examining the probability a school district on net gains students rather than loses them, with the exception that the weighted open enrollment measure coefficients are always insignificant.

[72] Just as in the previous chapters, a state's charter school status is as of the year 2000, and open-enrollment policies are attributed to any state that had such policies as of 2001.

magnet school are included in column three, whereas the fractions of district enrollment attending charter and magnet schools are included in column four.

Regarding the PSC measures, only the PSC dummy variable coefficients are statistically significant, and they are not consistently significant across specification. However, evidence suggests that the presence of a charter school in a district is negatively related to a district's keeping its students. I find insufficient evidence to suggest that the presence of a magnet school influences whether a district keeps students, on net, rather than loses students. With respect to the charter school dummy variable, the coefficient moves between an average reduction in the likelihood of having no net change in student enrollment rather than losing students of 17 and 19 percent because of the presence of a charter school, all else constant. Because of the numerous caveats in the data, little trust is put in the coefficients as point estimates; moreover the sign and general strength of the coefficient is of interest here. While the expectation of the PSC measures was to observe a heightened retention rate for the school district, I observe the opposite. Because of the timing of this data – and likely recent inception of charter schools – this may reflect that public schools of choice have been well targeted to areas that are losing students to the private sector (Glomm et al. 2005). Perhaps more timely data that reflect the effects of more-tenured schools of choice on the net flow of students may reveal a different result. Additionally, it would be interesting to observe whether these coefficients are sensitive to the inclusion of measures of private school supply and more finely tuned school quality controls – measures not readily available in the current dataset. I find less evidence to support a magnet school effect; yet I still observe negative coefficient on the magnet school dummy variable in column seven. The charter school dummy measure coefficients are consistent in sign, significance, and magnitude for the model run at the thresholds of 10 and 15 student observations; the magnet school coefficient retains its insignificance across threshold levels,[73]

With respect to the type specific competition measures, while the coefficients are insignificant, the coefficients on the enrollment measures are positive. The implication is that additional competition of charter schools and magnet schools – in terms of district enrollment – may positively affect district retention rates. So while there is insufficient evidence to support this claim, the coefficients present the best un-biased estimate of the

[73] This may reflect the more limited data available on magnet schools rather than an actual reduction in the magnet school effect garnered by re-specifying the sample.

relationship between the supply of PSC in a district and a district's net change in student enrollment. Across the thresholds of 10 and 15 students, these coefficients keep their statistical insignificance but only the charter school measure retains the positive sign.

Looking at the coefficients on the state-level education policy measures presented in Table 4-2, insufficient evidence exists to suggest either residing in a charter state or living in a state that has either voluntary or mandatory interdistrict open enrollment policies effectively contributes to districts having no net-change in student enrollment versus losing students. This may be attributable to the general equilibrium nature of this question. As students are potentially moving across districts, for any net losing school district in a state, there is potentially a net-gaining district. It may also reflect the limited power of this analysis based on the sample size and limited identification of a particular district. On estimating the model at the thresholds of 10 and 15 student observations, the insignificance of these policy measures persists.

Looking now at the remaining model coefficients presented in Table 4-2, evidence suggests that school districts with higher student-teacher ratios, higher median household incomes, and higher poverty rates are more likely to keep their students, or have no net change in enrollment, rather than lose students, on net. These coefficients are generally consistent across threshold levels of 10 and 15 student observations. The student-teacher ratio effect is consistent with limited and/or smaller, more rural districts having larger class sizes and few options outside of the assigned district. Likewise, the income effect, holding constant both the median real estate property tax rate and the average poverty level in the district, is positive. This is consistent with the tendency for school quality to be correlated with the socioeconomic status of district residents; thus the more affluent the median household of the district, the more positively perceived are the public schools in the district. The positive coefficient on the poverty measure is consistent with fewer households in the district being able to afford the private school option or perhaps opt out of district.

I also find some evidence to suggest that the proportion of district residents who are a minority and the median real estate property tax rate are negatively related to districts keeping their students (or having no net change in enrollment) as opposed to districts that are net-losers of students; alternatively, I find some evidence to suggest that total per-student expenditure is positively related with a district's retention rate, although the coefficients are not as consistent as those discussed above because these measures are sensitive to limiting

the sample to school districts with a greater number of student observations.[74] Even though the evidence is mixed, the results are consistent contextually with those presented so far: that the minority proportion of a district's population would adversely affect the retention rate of a district is consistent with white flight to the private sector. Additionally, more average spending per student would likely appeal to households, improving the district's retention rate. The negative coefficient on the real estate tax rate is inconsistent with the *Tiebout hypothesis* (Tiebout 1954), that as mobile consumers, households ultimately pay for their public good of choice, public schooling, by matching tax rates to the desired public school quality. This finding is arguably consistent with an open enrollment effect in which households opt to reside in districts with lower costs of living and send their students to districts with better schools. But the finding here likely reflects the behavior of households with private school students – as the number of private school students substantially exceeds that of students opting for public school out of district. Thus, because identification for net losing districts essentially rests on private school students, it is more likely this property tax effect reflects a correlation between housing tax rates and private school enrollment. I reiterate that this effect is not significant at the threshold levels of 10 or 15 student observations.

3.5.2 ***Gaining Students***

Turning now to Table 4-3, I present the marginal effect estimates when the outcome is being identified as a school district that is a net-gainer of students as opposed to a being a net-losing district. The columns are aligned just as Table 4-2 was laid out. Column one of Table 4-3 presents the marginal effect estimates with no PSC measure. Columns two through four present the results when the model includes type-specific PSC measures. Column two presents the estimates when the only PSC measures are the type-specific dummy variables. And columns three and four subsequently add alternative type-specific PSC competition measures – the number of PSC schools in the district is included in

[74] Under the specification where the threshold is set at 10 students, the coefficient on the average high school graduation rate of seniors in the district is positive and significant – implying that better performing schools in the public sector help retain students in the district However, at this threshold level, the coefficients on total expenditures per student and on the real estate tax rate are insignificant.

column three, whereas the fraction of district enrollment attending PSC schools is included in column four.

Very similar to the estimates presented in Table 4-2, in Table 4-3 only the PSC dummy variable coefficients are statistically significant, and they are not consistently significant across specification. However, evidence suggests that the presence of a charter school in a district is negatively related to a district's gaining students. The coefficient moves slightly between an average reduction in the likelihood of gaining students rather than losing students of 4.5 and 4.9 percent because of the presence of a charter school, all else constant. I again caution against much weight being given to point estimates because of the numerous caveats in the data. Here I find slight evidence to suggest magnet schools effectively influence whether a school district is a net-gainer of students rather than a net-loser. The coefficient on the magnet school dummy variable is negative and significant in column four. The charter school coefficients are consistent in sign and significance – and in magnitude, though, to a lesser extent – for the model run at the thresholds of 10 and 15 student observations; however, the magnet school coefficient is only significant under the threshold of five student observations.[75]

Again, with respect to the type specific competition measures, while the coefficients are insignificant, the coefficients on the enrollment measures are positive. Across the thresholds of 10 and 15 students, these coefficients keep their statistical insignificance and generally retain their positive sign.

Looking at the coefficients on the state-level education policy measures presented in Table 4-3, I again find insufficient evidence to suggest either residing in a charter state or living in a state that has either voluntary or mandatory interdistrict open enrollment policies effectively contributes to districts' being net-gainers rather than net-losers. On estimating the model at the thresholds of 10 and 15 student observations, the insignificance of these policy measures persists.

Looking now at the remaining model coefficients presented in Table 4-3, the findings are similar to those presented in Table 4-2 but offer some interesting differences. Student-teacher ratio is the only measure that is consistently significant across all threshold levels. As before, average student-teacher ratio in the district is positively related to the

[75] Again, this may reflect the more limited data available on magnet schools rather than an actual reduction in the magnet school effect garnered by re-specifying the sample.

outcome of district retention. Specifically, here it appears to contribute to a district's status as a net-gainer rather than a net-loser of students. This finding is again consistent with isolated, rural districts without many alternatives to the residentially assigned public school; special open enrollment policies may also play a role. With respect to the other coefficients, the findings are very similar to those presented in Table 4-2, and the coefficients are not significant across all threshold levels. Average total expenditures per student and median household income in the district are both positively related to the probability a district is a net-gainer of students rather than a net-loser. Again, the coefficients are intuitive – more money spent on students and perceived better school quality because of a higher median household income level both serve to attract households into such public school districts. Resident minority rates in the district and median real estate tax rates are found to be negatively related to school districts, on net, gaining students rather than losing them. Again the findings are consistent with predominant white flight as well as fewer transfer-in rates to high-minority districts. Interestingly, the tax rate coefficient implies that higher tax rates on real estate is associated with a decreased probability that districts will attract students, on net. Again, this outcome is not expected, and the likely motivating factor is a positive correlation between private school enrollment and property tax rates.

3.6 Discussion

Overall, the findings from the ordered probit model yield similar marginal effect coefficients estimating how the model's explanatory variables effectively contribute to a school district either experiencing no net change in enrollment or being classified as a net gainer of students, as opposed to being a net loser of students. Interestingly the main PSC measures to play a role are those for charter schools, and the evidence suggests such schools do not contribute to increased student retention rates for school districts. The ordered probit model estimates failed to provide substantial evidence of magnet school or state-level education policy effects on this ordered ranking of district outcomes. Interestingly, the estimates of the ordered probit model reveal that net-loser school districts, compared with districts that keep their students or net-gaining districts, may very well signal better quality school districts with more diverse options available to facilitate positive student-school matches.

Beyond the robustness checks of running the ordered probit model only on school districts with at least 10 or 15 student observations, I also estimate alternative models. As mentioned previously, I estimate an OLS model addressing the issue of school district retention and attraction, as well as two probit model specifications. While the results are not presented here, I do discuss how these robustness checks serve to validate the findings presented by the ordered probit model. I also note any inconsistencies or new findings from the alternative model estimates.

3.6.1 *OLS*

In estimating the OLS model, the dependent variable is the net change in the number of students attending the district; thus it is equal to the number of students transferring into the district minus the sum of students opting out of district or into private school. Because of the sample I use, and the prevalence of private school enrollment compared with students opting for public school in another district, the dependent variable reveals that most districts are net-losers. Also, because of the caveats of the data, interpreting coefficients that are estimates of actual counts of student entering or exiting a district are highly suspicious. Thus most relevant here are the sign and the statistical significance of the effects.

Among the key variables of interest, I find intermittent cases in which the magnet school dummy variable has a statistically significant negative coefficient. And, interestingly, the charter school measure coefficients are never significant. This finding is opposite of the type specific PSC effects observed in the ordered probit mode. Interestingly, under OLS, I find a slight charter state effect that is more consistently significant than the coefficient on the charter school dummy variable and reflects the opposite sign.

I continue to find mixed results and insignificant coefficients on the type specific PSC competition measures. Across the threshold levels, the number of either charter or magnet schools in a district is insignificant and has a negative sign. However, the coefficients on the fraction of district enrollment attending either a magnet or charter school are positive and insignificant. These findings are generally consistent with the results of the ordered probit.

Under the OLS model, I find a positive coefficient on the charter state dummy, this implies some positive relationship between this policy and districts collectively gaining students. However, the coefficient is not consistently significant across threshold levels and to some extent appears to be offset somewhat by the effect of the actual presence of charter schools in the district. The positive effect of the charter legislation on district retention is an interesting finding as charter schools themselves tend to locate in areas that have private schools already established (Glomm et al. 2005), and thus are likely located in net-losing districts. However, the charter law is at the state level and thus reflects a much broader scope of district profiles than any one district.

Under OLS, I also find slight evidence of a voluntary interdistrict open enrollment policy; though it is not a consistently significant finding and is generally significant only under the higher threshold levels. The coefficient is negative, which is not necessarily surprising. Again, as students transfer across districts in a state, some school districts may be net-gainers while others may be net-losers. Add to that the availability of a private school option that only adds to the reduction of students in any district, and it becomes increasingly more challenging to identify a general trend of net-gaining districts.

I also discuss some of the remaining coefficients to further compare coefficients from the OLS model with those of the ordered probit. Among the measures with coefficients that are significant across all thresholds are the number of schools, the median household income in the district, and average local revenues per student. Interestingly, the OLS model introduces two new measures of potential importance. Both the number of schools in a district and the local revenue per student are found to be negatively related to the change in student enrollment. Thus larger districts and more financially savvy residential areas are less likely to attract students, on net. Again, this is consistent with the argument that districts that either keep or gain students, on net, are in some way limited in schooling alternatives and resources. Consistent with the ordered probit model, the OLS model estimates suggests that higher median income levels in a school district are positively associated with the flow of student enrollment into the district.

Overall, the OLS model supports the findings of the ordered probit model with the exception of the discrepancies in the coefficients on the magnet and charter school dummies. The OLS estimates additionally offer mixed evidence of education policy measures as well as introduces additional measures that may affect the flow of students

across the public sector. The difference in some of the findings may suggest that the ordered probit does not accommodate the more subtle effects of various explanatory variables with its three categories and rather rigid extensive margins. The OLS model with its continuous dependent variable allows for observed changes at the intensive margin.

3.6.2 *Probit*

I also estimate two different probit models. In the first model, the dependent variable classifies a district as a net-gainer, and the default category includes districts that are either net-loser of students or districts that have no net change in enrollment. The second probit model I estimate employs a dependent variable that classifies a district as a net loser, where the default category is being either a net-gaining district or a district that has no net change in enrollment.

Similar to the ordered probit results, the coefficients on the PSC variables for the first probit model discussed tend to be negative if significant; predominantly they are insignificant. One exception holds, however – when the type specific PSC dummies and number of PSC schools per district are included, the coefficient on the charter school is positive and significant, and the charter school competition measure is negative and statistically significant. When considering the qualities that effectively contribute to a district being a net-gainer rather than a keeper or net-loser of students, only the charter school measures have statistically significant coefficients; magnet school measure coefficients are never significant. The charter school dummy variable has a negative and statistically significant coefficient when the type specific PSC dummy variables are the only PSC measures included. I observe negative and statistically significant coefficients on the charter school competition measures once PSC competition measures are included. When the model is run using the threshold level of 10 students, only the charter school dummy variable, when PSC dummy variables are the only PSC measures included, retains its statistical significance. The model cannot be estimated at the higher threshold level of 15 student observations with the successful inclusion of the charter school measures because of a number of perfectly predicted outcomes in the more restrictive, smaller sample. The magnet school measure coefficients, however, retain their insignificance under both subsequent threshold levels.

Otherwise the probit model for net-gaining districts produces fairly similar results to the ordered probit model with some exceptions. None of the education policy measure coefficients are found to be statistically significant. I find fairly consistent evidence that residential poverty rates and income rates are positively related to districts being net gainers as opposed to not being net-gainers. And I also find mixed evidence that the number of schools in a district, the fraction of schools that are Title I eligible, average local revenues per student, real estate property tax rates, and the percentage of minority residents in the district to all be negatively related to districts gaining students.[76] Subsequently mixed evidence appears across the threshold levels that total expenditure per student is positively related to school districts attracting student, on net. These findings are generally consistent with the results of the ordered probit, as well as some of the findings of the OLS estimates.

Finally, I estimate the second probit model in which the dependent variable indicates districts are net losers of students rather than keeping or gaining students, on net. The results are similar to what one might expect for a probit model estimating private school enrollment. With respect to the key variables of interest, I find that the presence of charter schools in the district tend to be positively related to a district being a net loser of students. I find almost no observable magnet school effect, save one occurrence of a statistically significant and positive magnet school dummy coefficient. Nearly all of the PSC competition measures that account for the proportion of student enrollment in the in the district attending either charter or magnet schools (with elementary grades) yield negative coefficients across the estimated threshold levels of five and ten students. Under the threshold level of ten students, the coefficient on the fraction of students enrolled in charter schools is negative and statistically significant, consistent with private sector crowd out as well as increased student retention rates in the district.

Regarding state education policies, I observe intermittent evidence suggestive of a negative effect of being in a charter state on the probability a district loses students, on net. This remains a puzzling finding that may even reflect an anomaly in the data.

[76] The missing data indicator variable for the fraction of district schools that are Title I eligible is also statistically significant under this probit model specification. I also estimate the probability a school district is a net-gaining school district using a probit model on only observations with valid measures of Title I eligibility in the district. The coefficients are similar to those of the main model's estimates, though there is never a statistically significant positive PSC coefficient.

With respect to the remaining explanatory variables for the second probit model, I find district average student-teacher ratios, total expenditures per student, median household income in the district, and poverty rates to be negatively related to the probability a district is a net-loser of students rather than keeping students or being a net-gainer. And I find mixed evidence to suggest that local revenue per student is positively related to school districts being net-losers of students rather than keeping or being net-gainers of students. By and large, the results are consistent with those presented from the other models.

3.7 Conclusion

Overall, the findings of this exploratory analysis do not reveal evidence to suggest that PSC schools contribute to increased student retention rates for school districts, with the exception of some limited, mixed evidence. Indeed, the presence of a charter school is negatively related, on average, to a district keeping or attracting students, on net. The finding, however, may reflect that charter schools are perhaps well targeted to districts that are losing students. Analysis using more tenured measures of charter schools, as well as a more representative sample of students within a district, may yield different results. Additionally, I find that net-loser school districts, compared with districts that keep their students or net-gaining districts, may very well signal better quality school districts with more diverse options available to facilitate positive student-school matches.

3.8 Tables

Table 3-1: School District Summary Statistics

	Obs	Mean	Std. Dev.	Min	Max
Dependent Variable Measures					
Net Losing Districts (%)	260	64.34	(0.48)	0	100
Net Gaining Districts (%)	260	7.36	(0.26)	0	100
Survey Student Observations Per District					
Total Students that Live in District (#)	260	19.71	(19.84)	5	189
Total Students Enrolled in District (#)	260	14.70	(16.06)	0	135
Students Opting for Private School (#)	260	4.84	(7.55)	0	56
Students Transferring Into District (#)	260	0.33	(0.96)	0	8
Students Opting Out of District (#)	260	0.49	(1.41)	0	17
PSC					
Elem Magnet School Dummy, Lag (%)	180	13.19	(0.34)	0	100
Elem Charter School Dummy, Lag (%)	260	13.95	(0.35)	0	100
Elem Magnet Schools (#), Lag†	20	20.42	(43.98)	1	217
Elem Charter Schools (#), Lag†	40	3.75	(4.94)	1	28
Elem Magnet Enrollment, Lag (%)†	20	19.22	(0.15)	1	56
Elem Charter Enrollment, Lag (%)†	40	3.96	(0.04)	0	14
State Education Policies					
Charter State, 2000 (%)	260	88.76	(0.32)	0	100
Mandatory Open Enrollment, 2001 (%)	260	17.05	(0.38)	0	100
Voluntary Open Enrollment, 2001 (%)	260	52.71	(0.50)	0	100
School District Demographics, One-Year Lags					
White (%)	260	62.33	(0.31)	0	99
Black (%)	260	16.85	(0.20)	0	93
Hispanic (%)	260	15.05	(0.21)	0	95
Average Student-Teacher Ratio in SD	260	16.74	(2.85)	9	24
HS Graduation Rate for Seniors (%)	260	93.01	(7.20)	64	100
Title I Eligible Schools (%)	220	54.82	(0.27)	0	100
Free Lunch Eligible Students (%)	230	28.57	(0.20)	0	79

† indicates means only calculated on school districts with at least one of the appropriate PSC school; Observation counts have been rounded to the nearest 10 in compliance with the NCES's restricted data usage policies.

Table 4-1 continued

School District Summary Statistics - *Continued*

	Obs	Mean	Std. Dev.	Min	Max
School District Finances & Infrastructure					
Schools per District	260	43.24	(76.28)	3	663
Total Expenditures per Child - $K, Lag	260	9.13	(2.64)	5	22
State Revenues per Child - $K, Lag	260	4.16	(1.88)	1	13
Local Revenues per Child - $K, Lag	260	4.35	(2.73)	0	17
SD Residents as of 2000					
Total Population (K)	260	194.63	426.39	3	4444
Median Household Income in SD - $K	260	47.62	(15.34)	22	104
Residents with at most a HS Diploma (%)	260	47.04	(0.15)	11	79
Residents with at least a College Degree (%)	260	25.77	(0.14)	5	69
Minority (%)	260	24.11	(0.20)	1	83
HHs Below Poverty Line (%)	260	10.27	(0.06)	1	34
Median Housing Value - $K	260	143.06	74.65	41	461
Median Real Estate Tax Rate (%)	260	1.31	0.69	0	4

† indicates means only calculated on school districts with at least one of the appropriate PSC school; Observation counts have been rounded to the nearest 10 in compliance with the NCES's restricted data usage policies.

Table 3-2: Marginal Effect Estimates from Ordered Probit, Outcome (1) - Keeping Rather than Losing Students

	(1)	(2)	(3)	(4)
PSC Measures				
Charter – Dummy		-0.165	-0.055	-0.192
		(2.34)**	(0.46)	(1.88)*
Magnet – Dummy		-0.096	-0.122	-0.193
		(0.96)	(1.24)	(1.57)
Charter, Number (100s)			-6.108	
			(1.41)	
Magnet, Number (100s)			0.144	
			(0.68)	
Charter, Enrollment Proportion				0.833
				(0.58)
Magnet, Enrollment Proportion				0.660
				(1.13)
Education Policies				
Charter State, 2000	0.053	0.079	0.083	0.076
	(0.82)	(1.23)	(1.30)	(1.18)
Inter-District Mandatory, 2001	-0.064	-0.070	-0.070	-0.073
	(1.03)	(1.05)	(1.05)	(1.10)
Inter-District Voluntary, 2001	-0.003	0.006	0.005	0.010
	(0.07)	(0.12)	(0.10)	(0.18)
SD Measures				
Schools (#)	-0.104	-0.057	-0.030	-0.068
	(1.79)**	(1.05)	(0.43)	(1.12)
Title I Eligible, Proportion	0.024	0.021	0.033	0.009
	(0.27)	(0.24)	(0.36)	(0.10)
Student-Teacher Ratio	0.019	0.022	0.021	0.023
	(2.00)**	(2.24)**	(2.13)**	(2.37)**
Total Exp per Student	0.028	0.031	0.031	0.028
	(2.08)**	(2.18)**	(2.19)**	(1.97)**
Local Rev per Student	-0.018	-0.021	-0.023	-0.019
	(1.19)	(1.39)	(1.48)	(1.23)
Graduation Rate of HS Seniors	0.004	0.003	0.003	0.004
	(1.09)	(0.81)	(0.84)	(1.17)
Observations	260	260	260	260

Absolute value of z statistics in parentheses

* significant at 10%; ** significant at 5%: *** significant at 1%

Additional model controls include missing data indicatory variables.

Table 4-2 continued

Marginal Effect Estimates from Ordered Probit, Outcome (1) - Keeping Rather than Losing Students - *Continued*

	(1)	(2)	(3)	(4)
SD Residents				
Med HH Income ($K)	0.007	0.007	0.008	0.007
	(2.67)***	(2.74)***	(2.83)***	(2.75)***
Poverty (Below), Proportion	1.352	1.281	1.299	1.331
	(1.80)*	(1.67)*	(1.69)*	(1.72)*
Minority, Proportion	-0.539	-0.540	-0.567	-0.520
	(3.00)***	(2.95)***	(2.95)***	(2.83)***
Real Estate Tax Rate	-0.073	-0.079	-0.079	-0.081
	(1.87)*	(1.75)*	(1.76)*	(1.77)*
Observations	260	260	260	260
Absolute value of z statistics in parentheses				
* significant at 10%; ** significant at 5%: *** significant at 1%				

Additional model controls include missing data indicatory variables.

Table 3-3: Marginal Effect Estimates from Ordered Probit, Outcome (2) - Gaining Rather than Losing Students

	(1)	(2)	(3)	(4)
PSC Measures	.			
Charter – Dummy		-0.045	-0.017	-0.049
		(2.51)**	(0.51)	(2.24)**
Magnet – Dummy		-0.028	-0.032	-0.046
		(1.20)	(1.64)	(2.18)**
Charter, Number (100s)			-2.091	
			(1.42)	
Magnet, Number (100s)			0.049	
			(0.69)	
Charter, Enrollment Proportion				0.302
				(0.59)
Magnet, Enrollment Proportion				0.239
				(1.15)
Education Policies				
Charter State, 2000	0.018	0.024	0.024	0.023
	(0.91)	(1.40)	(1.47)	(1.33)
Inter-District Mandatory, 2001	-0.022	-0.022	-0.021	-0.023
	(1.13)	(1.15)	(1.15)	(1.20)
Inter-District Voluntary, 2001	-0.001	0.002	0.002	0.003
	(0.07)	(0.12)	(0.10)	(0.18)
SD Measures				
Schools, Number (100s)	-0.041	-0.021	-0.010	-0.025
	(1.81)*	(1.04)	(0.43)	(1.11)
Title I Eligible, Proportion	0.010	0.008	0.011	0.003
	(0.28)	(0.24)	(0.37)	(0.10)
Student-Teacher Ratio	0.008	0.008	0.007	0.008
	(1.99)**	(2.12)**	(2.01)**	(2.20)**
Total Exp per Student	0.011	0.011	0.011	0.010
	(2.04)**	(2.08)**	(2.08)**	(1.89)*
Local Rev per Student	-0.007	-0.008	-0.008	-0.007
	(1.23)	(1.42)	(1.50)	(1.25)
Graduation Rate of HS Seniors	0.002	0.001	0.001	0.001
	(1.10)	(0.82)	(0.84)	(1.16)
Observations	260	260	260	260

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%: *** significant at 1%

Additional model controls include missing data indicatory variables.

Table 4-3 continued

Marginal Effect Estimates from Ordered Probit, Outcome (2) - Gaining Rather than Losing Students – *Continued*

	(1)	(2)	(3)	(4)
SD Residents				
Med HH Income ($K)	0.003	0.003	0.003	0.003
	(2.55)**	(2.58)***	(2.61)***	(2.56)**
Poverty (Below), Proportion	0.534	0.473	0.445	0.482
	(1.61)	(1.50)	(1.51)	(1.54)
Minority, Proportion	-0.213	-0.199	-0.194	-0.188
	(2.72)***	(2.64)***	(2.60)***	(2.54)**
Real Estate Tax Rate	-0.029	-0.029	-0.027	-0.029
	(1.70)*	(1.63)	(1.63)	(1.64)
Observations	260	260	260	260

Absolute value of z statistics in parentheses

* significant at 10%; ** significant at 5%: *** significant at 1%

Additional model controls include missing data indicatory variables.

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4. CHAPTER 5: Conclusion

This dissertation examines the impact of public school choice, that is, magnet schools, charter schools, and open enrolment policies, on a households schooling decisions – both within the public sector between traditional, neighborhood schools and schools of choice, and between public and private schools. Each chapter explores the household's school choice decision. In Chapter 2, I examine whether households treat public schools of choice (PSC), particularly charter and magnet schools, as substitutes for private schooling. In Chapter 3, I estimate the demand for intra-district public schools of choice, inter-district public schools, and private schools over residentially assigned public schools. In particular, I examine how a few key education policy and student and household level measures influence this household decision. In Chapter 4, I examine the attributes of school districts that are observed to, on net, lose or attract students in their public schools. I employ a unique dataset that allows me to observe students' residentially assigned districts and further matches public school students to the districts they attend and identifies private school students. In particular, I estimate how measures of PSC and statewide education policies influence the net flow of students within the public school system. Each chapter lends itself to influencing today's educational policy.

There is potential concern that the entry of public schools of choice, such as charter and magnet schools, will attract the best students from the public schools, leaving behind the poorer achieving students without their higher performing peers. These higher performing students have proven beneficial to their struggling counterpart in the literature. However, to the extent that these public schools of choice also compete with private schools, there is the potential to draw high performing students back into the private sector. In Chapter 2, I find evidence of a minimal private sector crowd out effect of public schools of choice – with much stronger effects for charter schools than magnet schools. This finding may well reflect the inability of PSC schools to compete with private schools in the area of religious-inspired teaching style or curriculum; as over 80 percent of elementary level private schools have a religious affiliation, this automatically drastically reduces the potential pool of private school

with which PSC schools may meaningfully compete.[77] My results are consistent high income households continuing to pay the premium for quality education in the private sector even in the presence of PSC schools, but households with income levels that are slightly lower appear to be more likely to substitute PSC schools for private schools. Thus, from a policy standpoint, there is the potential to draw in high achieving students from the private sector. As the literature has found that higher achieving students are not necessarily adversely affected by the presence of their lower achieving peers, the finding that PSC schools compete with private schools may be overall beneficial for students in the public sector.

In Chapter 3, I find evidence that households are willing to substitute for the most intra- and interdistrict enrollment in states with mandatory interdistrict open enrollment policies. Thus, it appears that households will substitute towards the most accessible form of public school choice – though, without more precise measures of the supply of both intra- and interdistrict schooling options, it is not possible to discern exactly how or why this substitution occurs. Nevertheless, there may very well be a means of combating student transfers that are taxing a particular set of resources in the public school sector by making available another option to households. If indeed households are engaging in interdistrict transfers that are lead to undesirable outcomes or overtax resources, then incorporating intra-district choice through either an open enrolment policy or by incorporating charter or magnet schools may lead households away from the over taxed recourses or the unwanted inter-district transfers.

The findings of Chapter 4 are most closely related to those of Chapter 2 – again revealing a relationship between charter schools and private school enrollment. It is charter schools, in particular that appear to enter into districts that are losing students to private school. However, private schoolings appear to compete to some extent with charter schools, as a larger share of district enrollment in charter schools is found to positively affect the district's retention rate. Additionally, as districts that are net-keepers or net-gainers of students appear to be districts with larger class sizes, higher poverty rates, and fewer schooling options available, policy may consider plausible means of introducing choice into such districts. Such districts, possibly often rural in setting, may be an ideal setting for introducing technology-based educational services, such as satellite classrooms.

[77] Religiously-affiliated private school statistics come from the Council for American Private Education (CAPE) and are available online at: http://www.capenet.org/facts.html.

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6. Vita

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