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Dual Language Education and Student Achievement

ANDREW BIBLER
University of Alaska Anchorage
Institute for Social and Economic Research
Department of Economics and Public Policy

UAA DEPARTMENT OF ECONOMICS & PUBLIC POLICY
3211 Providence Drive
Rasmuson Hall 302
Anchorage, AK 99508

http://econpapers.uaa.alaska.edu/

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Andrew Bibler*

University of Alaska Anchorage

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Abstract

Dual language classrooms provide English language learners (ELLs) an opportunity to receive instruction in their native language in hopes of easing the transition to English fluency, and provide an opportunity for native English speakers to receive instruction in a second language. For ELLs, learning in their native language could improve achievement by helping them build a stronger foundation in core subjects, but could also have a negative impact through delayed growth in English skills. For native English speakers, communication barriers could hurt achievement, but many argue that mental stimulation from speaking two languages leads to greater cognitive growth. Empirical testing for the effect of dual language education on academic achievement is necessary to inform the debate on the practice of dual language education, and to inform policymakers and practitioners on practices for assimilating students with non-English dominant languages. I examine dual language education and student achievement using school choice lotteries from Charlotte-Mecklenburg School District, finding local average treatment effects on math and reading exam scores of more than 0.06 standard deviations per year for participants who were eligible for English second language (ESL) services or designated limited English proficient (LEP). There is also some evidence that attending a dual language school led to a lower probability of having limited English proficient status starting in third grade. For applicants who were not eligible for ESL services or designated as LEP, attending a dual language school resulted in higher end of grade exam scores of about 0.09 and 0.05 standard deviations per year in math and reading, respectively.

Keywords: language immersion, bilingual education, English language learner, magnet schools, school choice, lottery

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1 Introduction

Dual language classrooms use a non-English language of instruction for a significant amount of the curriculum. They are primarily used to provide instruction to English language learners (ELLs) who might benefit from receiving instruction in their first language, and to promote bilingualism and biculturalism among native English speakers. There are two types of dual language (DL) classrooms.[1] *Two-way* classrooms are one form of *bilingual education*, which broadly refers to programs that are targeted toward ELLs and include some amount of home language instruction. Two-way classrooms typically enroll students from two different language backgrounds and teach curriculum in both languages,[2] so instruction is not generally based on the current English ability of ELL students.[3] There were only about ten such programs in the U.S. in 1980, but that number was almost 250 by 2000 [Howard and Sugarman, 2001]. In contrast, most students in a *one-way* (*immersion*) classroom share a similar language background, but receive instruction in a second language. The number of one-way classrooms increased from fewer than 50 to almost 450 over the last few decades (Center for Applied Linguistics, 2011). Recent expansions in several states have driven these numbers even higher.[4] Despite the growth, there is little causal evidence on the effect of dual language education on student achievement.

For ELLs, dual language education might ease the transition to full English instruction, providing a potential route for improving outcomes of the growing and struggling ELL population. The alternative is often placement in an English-only classroom coupled with English second language (ESL) services, which could mean missing important classroom instruction time and disruption to the student and his or her peers, and ultimately making students more likely to fall behind. On the other hand, placement in an English-only classroom might

[1] Although there are two different types of classrooms studied here, two-way dual language and language immersion, I will refer to both as dual language for simplicity.

[2] Two-way dual language classrooms tend to use the non-English language for a large proportion of instruction (50% or more) throughout elementary school.

[3] Other forms of bilingual education, such as transitional bilingual education and structured English immersion, are more focused on expediting English fluency and do not necessarily group ELLs and non-ELLs in the same classroom. See Valentino and Reardon [2015] for a good description of some of the differences between dual language classrooms and other forms of bilingual education.

[4] Utah passed legislation for funding of dual language programs in 2008, and since then has implemented programs in 118 schools in 22 districts. New York City added or expanded 40 programs in 2015 [Harris, 2015].

expedite the development of English skills, leading to faster re-classification out of ELL status and higher scores on standardized exams that are written in English. Districts also target dual language education to English speaking students, with the primary goal of developing bilingual, biliterate, and bicultural students. Dual language schools, and other specialized programs, allow districts to offer a more diverse set of options and compete with charter and private schools, and the influx of dual language programs seems to be driven in large part by demand from English speaking families[5] [Watanabe, 2011, Parkes, 2008].[6] Dual language programs are often promoted using high test performance of participants as evidence of increased cognitive development [Maxwell, 2012, Maxwell, 2014]. However, lack of formal training in English could slow progress as measured by scores on standardized exams. It is unclear whether participating in a dual language program would increase or decrease test scores for either group, English dominant students or ELLs, so empirical testing is necessary to determine the direction and magnitude of any causal effects.

Drawing conclusions from previous literature is complicated by the fact that bilingual education can take several forms, and the goals and degree to which home language instruction is used varies within and across program types. The biggest technical difficulty in estimating causal effects of dual language education comes from concerns about self-selection into the programs. Some prior research addresses self-selection by matching on pretest scores [Cazabon et al., 1999] or other observable characterisitcs [Cobb et al., 2009], often finding that dual language participants outscore their peers on math, reading, and writing exams. Several meta-analyses focus on estimating effects of different types of bilingual education, but the conclusions are sometimes contradicting with some suggesting that certain types of programs rarely have a positive impact on achievement [Rossell and Baker, 1996],[7] while

[5] About 70 percent of the estimation sample in this study are students who were never identified as English language learners or limited English proficient in the data.

[6] In a survey of families with children enrolled in DL education, about 25% of parents indicated they were English dominant, and 40% indicated they were bilingual [Parkes, 2008]. From the same survey, more than 80% of parents cited having their child learn to speak, read, and write in two languages as motivation for choosing a DL education, and more than 50% cited success in school, and success in a global society.

[7] Rossell and Baker [1996] focus on transitional bilingual education, which teaches reading in the native language in early grades, but moves to complete English instruction as early as second grade, and structured English immersion, which uses English instrucution with a classroom madeup of only English learners and moves at a classroom dependent pace. They deemed 25% of the studies they considered to be methodologically

others find positive effects of bilingual education across subjects and in different types of programs [Greene, 1998, Slavin and Cheung, 2005, Willig, 1985, Collier and Thomas, 2004].[8][9] Recent research has focused on causal identification of the effects of bilingual education programs on achievement [Slavin et al., 2011, Guo and Koretz, 2013, Steele et al., 2016, Valentino and Reardon, 2015].[10] Valentino and Reardon [2015] use data on student preferences from a large urban district to compare achievement across program types conditional on the type of program that the student preferred.[11] They find that dual language students progress faster in math and English language arts after second grade, leading to better long-run performance than those in English immersion[12] [Valentino and Reardon, 2015]. Similar to this study, Steele et al. [2016] exploit random assignment from admissions lotteries into dual language programs in Portland, Oregon. They report mostly positive estimates for the impact of dual language instruction on reading and math exam scores. However, this study differs from Steele et al. in at least two important ways. First, Steele et al. pool two subgroups of students - ELLs and non-ELLs - together. However, treatment differs between these two groups, and the resulting effects could be of different signs and magnitudes, which would have important policy implications.[13] Another important difference is that the dual

acceptable, and found that transitional education students outperformed their peers who received regular classroom instruction in a limited number of cases, but that they never outperformed their peers who received structured English immersion instruction.

[8]There are a handful of studies included in these meta-analyses that used random assignment to estimate causal effects. However, these studies were generally based on small samples (e.g. less than 175 students) and from nearly 30 years ago [Greene, 1998].

[9]Collier and Thomas [2004] summarize 18 years of results on one- and two-way programs from 23 different school districts. Students in both program types close at least 70% of the ELL test score gap by the end of fifth grade.

[10]Slavin et al. [2011] find that students randomly assigned to a transitional classroom scored lower on English reading exams than their peers in Enlish immerions classrooms in early grades, but there were no statistically significant differences by fourth grade. Using a difference-in-differences estimation strategy, Guo and Koretz [2013] find that a Massachusetts policy that shifted the early elementary education for ELLs from a several year transitional bilingual model to a one-year sheltered (or structured) English immersion model (a shift away from home language instruction) had no effect (or a small positive effect) on fourth grade English reading scores. I refrain from discussing these two studies in detail, because they focus on forms of bilingual education that are not two-way dual language.

[11]Assignment is quasi-random, but they do not use knowledge of the assignment mechanism to completely exploit the random assignment.

[12]The dual language and English immersion comparison is the most relevant for this study. Valentino and Reardon [2015] also consider the performance of students in transitional bilingual and developmental bilingual classrooms.

[13]About nine percent of students are ELLs at the time of application and fifteen percent have a non-English home language [Steele et al., 2016].

language programs in Portland Public Schools (PPS) are strand programs, meaning that they only make up a portion of the school. All of the dual language programs in CMS are housed in three schools, where every classroom in the school is a dual language classroom.

For native English speakers, parents may be concerned that enrolling their child in a dual language school could promote bilingualism at the expense of achievement on standardized tests, which are written in English. Learning in a second language could create confusion or frustration that would negatively impact achievement, especially in the short-run. On the other hand, the mental juggling involved with thinking in two languages might promote cognitive development [Baddeley and Hitch, 1974, Baddeley, 2003, Alloway, 2010, Adesope et al., 2010, Barac et al., 2014]. However, empirical evidence directly supporting a causal link between second language acquisition and cognitive development is sparse.[14] Some prior research has pointed out the generally positive (or null) effects for English dominant students in dual language programs by comparing participants and non-participants [Thomas and Collier, 2009, Thomas et al., 2010],[15] or matching on observables [Cobb et al., 2009, Cazabon et al., 1999].[16] Using quasi-random assignment from admissions lotteries, Steele et al. [2016] estimate generally positive effects of dual language instruction on math and reading scores in a sample comprised mostly of native English speakers.

This paper adds to the literature on estimating causal effects of dual language education on student achievement by exploiting quasi-random assignment from oversubscribed admissions lotteries. I focus on students who applied through the Charlotte-Mecklenburg school choice lottery for their kindergarten year, and specified a dual language school as their first choice. I use assignment to a dual language school through the lottery as an instrument

[14] Some research focuses on a theoretical connection related to working memory, which is used to store and process information and execute related tasks and can be considered a measure of ability to learn [Baddeley and Hitch, 1974, Baddeley, 2003, Alloway, 2010]. It is strongly correlated with academic outcomes and much of the growth in working memory capacity takes place before adolescence [Alloway, 2010]. There is also some empirical evidence that bilingualism is correlated with working memory [Adesope et al., 2010], as well as other measures of cognitive ability [Barac et al., 2014].

[15] English speaking participants of two-way programs in North Carolina score higher than their peers on end-of-grade exams and have better attendance [Thomas and Collier, 2009, Thomas et al., 2010]. See [Bialystok, 2016] for further review on this topic more broadly.

[16] Learning a portion of curriculum in a second language does not hinder progress for English dominant students, and might be associated with positive effects on achievement in reading [Cobb et al., 2009, Cazabon et al., 1999].

for dual language school attendance to identify the local average treatment effect of dual language schooling on standardized math and reading exam scores. Treatment differs by whether or not the student uses English as a home language. For a native English speaker, the treatment is to receive instruction in a second language and the alternative to attending a dual language school is receiving instruction in their home language. For ELLs, the typical alternative to attending a dual language school is to receive instruction in English (not their home language) accompanied by other ESL services. Because of this divide in treatment, I estimate effects separately for two subgroups using a proxy for whether or not the student was proficient in English when they entered school.

The first group is made up of students who were eligible for English second language (ESL) services or were designated as limited English proficient (LEP).[17] I will refer to this group of students as the "ESL/LEP" sample. In the ESL/LEP sample, I find that attending a dual language school leads to increased scores on math and reading exams of more than 0.06 standard deviations per year. The second subgroup is made up of students who were never eligible for ESL services or designated LEP. I will refer to this group as the "English" or the "non-ESL/LEP" sample. Among this group, I estimate that attending a dual language school leads to 0.09 standard deviations higher achievement in math per year, and 0.05 standard deviations higher achievement in reading per year. The estimates are statistically significant and generally robust to a number of alternate specifications. As these results are inclusive of all contributing factors, the biggest limitation of the analysis is the inability to disentangle the influences of the mechanisms that facilitate positive gains or mitigate the measured effects.

This paper also adds to the literature on the causal effect of dual language education on reclassification out of LEP status for language minority students. While test scores are one

[17]The term limited English proficient (LEP) is generally considered less favorable, because it has negative connotation and marginalizes the population that it refers to [Webster and Lu, 2012]. North Carolina uses the term LEP to refer to students who do not use English as a primary language in their home, and score below a specified cutoff on an English skills test. The term English language learner (ELL) is often used in place of [Webster and Lu, 2012], or interchangeably with LEP. I use the term LEP when referring to the designation given to students in North Carolina because that is the term the state uses. In addition, consistent use of the terms helps to acknowledge the use of information from multiple data sources and clarifies the distinction between the two pieces of information.

important outcome, districts may also care about the duration of LEP classification for this group of students.[18] Duration of LEP classification is another measure of student progress that differs from math and reading exam scores. In addition, offering ESL services is costly, so districts benefit from programs that expedite reclassification, all else equal. Umansky and Reardon [2014] find that dual language participants in a large urban district are reclassified out of LEP status at a slower rate in early grades, but have higher total reclassification and English proficiency than students from English immersion classrooms by the end of high school. Similarly, Steele et al. [2016] report slower reclassification out of ELL status for dual language participants throughout elementary and middle school. When estimating treatment effects though, they find that attending a dual language classroom led to a higher probability of exiting ELL status starting in fifth grade. I use assignment lotteries to estimate the effect of dual language education on LEP classification among students in the ESL/LEP sample, finding some evidence that attending a dual language school has led to a lower probability of being designated as LEP in grades three through six.

The positive results of the programs studied here, for both ELLs and English proficient students, suggest that effective implementation of dual language programs would be a promising route for public schools to improve their ability to foster assimilation of students with non-English home languages. They provide an option for the growing population of ELLs to improve their academic standing in comparison to native English speakers, and simultaneously alleviate concerns about potentially negative peer effects in traditional classrooms [Chin et al., 2013, Cho, 2012, Geay et al., 2013, Diette and Oyelere, 2014].[19] In addi-

[18] When a student enrolls in a district in North Carolina, their parent takes a survey that asks about the languages the student uses at home. The district uses that survey, and possibly interviews with the parents and/or student, to determine the home language of the student. If the home language of the student is not English, then the student must take a test that determines LEP status and eligibility for ESL services. When a student is identified as LEP based on the score of the placement test, they are required to continue testing annually until they are re-classified out of LEP status.

[19] Chin et al. [2013] use district level variation in the number of LEP students in Texas to study whether having a bilingual education option improves achievement for LEPs and their non-LEP peers. They identify the treatment effects using the discontinuity generated by a Texas rule that districts with at least twenty LEP students who share a common language in a specific grade must offer a bilingual education option to those students. They do not find significant increases in the test scores of LEP students from districts that offer bilingual education, but do find an increase in the scores of non-LEPs in districts that offer bilingual programs. Their findings suggest that offering bilingual education resulted in positive spillover effects to non-LEP students. Estimating peer effects directly has rarely been done in this setting and with mixed

tion, English speaking students who opt in score higher then their peers in other classroom types and presumably acquire some level of second language skills.

The next section provides details on the dual language schools in CMS. Section 3 discusses the data and some descriptives. In section 4 the empirical strategy used for the main results is discussed. Section 5 presents the empirical results and section 6 concludes.

2 Dual Language Schools in CMS

All three dual language schools in CMS are full magnet schools, meaning that admission requires a lottery application and every student in the school participates in the dual language program. Collinswood Language Academy and Oaklawn Language Academy offer two-way English-Spanish classrooms, and Waddell Language Academy (formerly Smith Language Academy) offers full immersion strands in Mandarin, French, German and Japanese. Collinswood started in 1997 and now houses grades K-8. In kindergarten, 90% of instructional time is in Spanish [Thomas and Collier, 2009]. In grades one through five, half of the content is taught in each language. Oaklawn is a newer program, started in 2004, but follows a similar model to that of Collinswood. The curriculum is taught 90% in Spanish in kindergarten, 75% in first grade, and 50% in grades two through five.[20] Spanish is by far the most common non-English language among students in CMS, and the two-way programs are targeted toward native speakers of both languages. The German and French one-way immersion classes offered at Waddell have complete foreign language instruction in grades K-2, whereas the Mandarin and Japanese classes teach one hour in English per day in grades K-2 [Thomas and Collier, 2009]. All four programs at Waddell target 90% of instructional time in the non-English language in grades 3 – 5. The one-way programs primarily target English speaking students, but they admit ELLs who speak the partner language or another language altogether.

In this specific setting, students apply for entry into a DL school for their kindergarten

evidence [Geay et al., 2013, Cho, 2012, Diette and Oyelere, 2014].

[20] Both schools use team teaching, divided by language of instruction.

year but don't take their first high stakes exam until third grade, so students and teachers have some time to overcome any initial difficulties in adjusting to the new language. The gap in time between school assignment and testing allows teachers and administrators to commit to teaching in the second language in the first few years of school when there are no high stakes exams looming. If working in two languages can boost cognitive development, then one might expect it to show up in this environment. The lag in time between entry and testing could also be beneficial for language minority students. Dual language education is not focused on expediting reclassification, so there is less immediate pressure on teachers and administrators to prepare ELLs for testing in English in early grades.

Two detailed reports on six districts in North Carolina, including CMS, find that ELLs in two-way programs score higher than students in English-only classrooms on end-of-grade exams [Thomas and Collier, 2009, Thomas et al., 2010]. Collier and Thomas [2009, 2010] also point out the positive achievement gap for English dominant students in dual language programs. English speaking participants of two-way programs in North Carolina score higher than their peers on end-of-grade exams and have better attendance [Thomas and Collier, 2009, Thomas et al., 2010]. One concern with interpreting the differences in outcomes for dual language school participants and non-participants is that the two groups might differ on unobservable characterstics in important ways. This study builds on prior research by exploiting the random assignment created by the lottery in CMS to estimate causal effects of the dual language education programs.

3 Data

The data use here were provided by CMS and the North Carolina Education Research Data Center (NCERDC). CMS provided eight years (2006 – 2013) of lottery results with assignment into the three dual language schools. NCERDC linked the lottery data from CMS with statewide data. The following analysis will focus on end-of-grade exam scores in math and reading, which begin in third grade. Linking the lottery data with statewide data provides information on end-of-grade exam scores, and allows for the tracking of students who leave the district but stay in the North Carolina public school system. Since lottery results could impact school attendance decisions, this helps to mitigate attrition issues [Rouse, 1998, Steele et al., 2016].

The analysis sample includes students entering kindergarten from the 2006-2007 through 2010-2011 school years who submitted an application for a dual language school in the CMS school choice lottery, and were linked from the CMS data to the NCERDC data.[21] I start with the 2006-2007 school year because of changes implemented that year to the lottery system, including how the priority groups were determined. End-of-grade exams start in third grade, so the most recent exam scores used here are from the 2013-2014 school year. The empirical strategy used in this paper makes use of exogenous variation from oversubscribed lotteries, so it is useful to describe how the lottery operates. The next subsection provides a brief summary of the lottery process.[22]

3.1 Lottery

Every student enrolled in Charlotte-Mecklenburg School District is assigned to a neighborhood school based on geographic boundaries. CMS students who wish to opt out of their neighborhood school can submit up to three programs in order of preference through a centralized lottery. Non-guaranteed[23] seats are assigned in three rounds. In the first round, only first choices are considered. If there are fewer applicants than seats available to a given program, then all of the applicants to that program will be assigned to their first choice. Identification comes from comparing winners and losers from the same lottery, so estimates are driven by oversubscribed lotteries. When the number of applicants is greater than the number of available seats (the choice is oversubscribed), seats are awarded quasi-randomly. Seat assignment is not completely random, because the probability of winning for a particular student depends on the priority group that the student is assigned to. Priority groups refer

[21] NCERDC was able to link between 93% and 97% of all observations from the CMS data in each year. Among observations of rising kindergarteners in the CMS data who chose a dual language school first in the lottery over the sample period, 93.5% were matched with the NCERDC data.

[22] Appendix B contains more details on the lottery.

[23] Students with an older sibling in a school are guaranteed a seat in that school by making it their first choice.

to sets of students that meet (or do not meet) some pre-specified criteria. In CMS, over the sample period they are based on geographic location and whether the student's neighborhood school is a Title I *choice*[24] school. I use lottery (program of application by year by priority group) fixed effects to exploit the fact that winners should be randomly chosen within these groups.[25] The identification strategy relies on comparing students who met a specific priority and won with students who met that priority and did not win. After going through all first choices, second and third choices are considered in a similar fashion. If a student's choice is already full from a previous round of assignments, then they remain unassigned in that round. All students are assigned to a default neighborhood school based on pre-determined geographic zones if not otherwise assigned in the lottery. In the following analysis I restrict to students who made a dual language school their first choice. Since the lottery considers student choices in order, students are most likely to win a choice by picking it first, and more seats are awarded in the first round than in the second or third. The treatment assignment variable is a dummy variable for winning their first choice, which should be random within lottery.

I use available information to construct lottery fixed effects. The data contain up to three choices for every student in order of preference, as well as sibling placement, Title I choice placement, FRPL status, and transportation zone.[26] I start with the sample of all applicants without a guaranteed seat and proceed in the following way to generate lottery fixed effects.

1. Proxy Title I choice school using whether or not any student from their neighborhood school was placed under the Title I choice option that year.

[24]Title I schools are those with a high percentage of students eligible for free and reduced price lunch (FRPL). A Title I school becomes a Title I *choice* school if they fail to meet adequate yearly progress in the same subject for two consecutive years. No Child Left Behind (NCLB) requires that the district allow students assigned to Title I choice schools the opportunity to attend a non-Title I choice school, but it does not require the district to allow students to choose the school they are offered.

[25]In addition to priority groups, all applicants are ordered based on randomly assigned numbers. When a choice is oversubscribed, the combination of priority groups and randomly assigned numbers determine who wins the lottery. The next subsection discusses the priority groups, and gives more detail on how lottery winners are determined during and after the first round.

[26]CMS stopped reporting FRPL after 2010. For 2011 I proxy for FRPL at the time of application using FRPL from the NCERDC data.

2. Generate priority groups using FRPL, transportation zone, and Title I choice proxy.

3. Lottery fixed effects are priority-year-program of application combinations.

Since the lottery fixed effects are generated, they are a proxy to the true lottery fixed effects. The assignment, conditional on lottery, provides the exogenous variation used to estimate the treatment effects.

3.2 Data Summary

Since estimation relies on applicants with non-guaranteed seats in oversubscribed lotteries, there are a couple of things worth noting. From the first row of Table 1, between 20 and 30 percent of the seats in each school were awarded to students with a sibling guarantee.

Table 1: Application Numbers from CMS Lottery

	One-Way School	**Two-Way Schools**		**Other Applicants**
	Waddell	Collinswood	Oaklawn	
	[1]	[2]	[3]	[4]
Applicants				
Sibling Placement	0.296	0.232	0.218	0.230
Won First Choice	0.782	0.561	0.951	0.589
DL Applications	2.081	1.248	1.283	0.093
N	1,147	1,112	533	13,071

*Notes: This table displays the type and number of applications submitted for all those submitting applications in the CMS school choice lottery.

Those students are dropped from the estimation sample. The second row of Table 1 shows the percentage of applicants to each school that won their first choice. Only 56 percent of applicants who listed Collinswood as their first choice won their first choice, and 78 percent of first choice applicants to Waddell won their first choice.[27] The CMS assignment mechanism only considers first choices in the first round of seat allocation. If a program fills up in the first round, then second and third choice applicants to that program will not win a seat there. Table 2 shows application numbers for students who chose one of the dual language schools

[27]These percentages include students with guaranteed seats, so they are overestimates of the percentage of winners among those with non-guaranteed seats. About 43 percent of non-guaranteed applicants to Collinswood won and 69 percent of non-guaranteed applicants to Waddell won.

as their second or third choice, but not as their first choice. From column 1, 49 percent of students who made a dual language school their second choice, won their first choice to a non-dual language school. About 14 percent of students who chose a dual language school with their second choice but not their first, won that choice, but only 10 percent attended a dual language school.

Table 2: Second and Third Choices

	Second Choice DL [1]	**Third Choice DL** [2]
Attend DL School	0.105	0.087
Second Choice DL	1.000	0.219
Third Choice DL	0.203	1.000
Assignment		
Collinswood	0.000	0.000
Waddell	0.080	0.072
Oaklawn	0.077	0.075
Any DL Choice	0.157	0.147
Choice		
Collinswood	0.339	0.245
Smith	0.332	0.475
Oaklawn	0.329	0.279
Won		
First Choice	0.490	0.426
Second Choice	0.143	0.094
Third Choice	0.070	0.117
Any Choice	0.703	0.638
Observations	286	265

Figures 1-2 compare average standardized math and reading scores for dual language and non-dual language students.[28] These descriptive comparisions of DL and non-DL students represent a good starting point, but ignore useful information on lottery fixed effects and sibling placement. Figure 1 graphs the comparison for non-LEP students. Non-LEP, dual language students score well above the state average in reading in every grade, and there

[28]Figures 1-2 graph average standardized (by year and grade across the state) residualized scores. They are residuals from linear regressions of standardized exam scores on grade dummies, year dummies, sex, FRPL status, and exceptionality. For the purposes of Figures 1-2, dual language students are all students who attended a dual language school in any grade, 3 - 8. Figure 1 uses all students who were not identified as limited English proficient in any grade, 3 - 8, and Figure 2 uses all students who were identified as limited English proficient in at least one of those grades.

is a divergence between dual language students and the rest of the district from grades three through eight. In seventh and eighth grade the dual language students score more than 0.3 standard deviations above the state average in reading. Non-LEP, dual language students also score well above the state average in math, but the gap between DL students and the rest of the district displays a slight downward trend with grade. The dual language, non-LEP students score about 0.3 standard deviations above the mean in grades 4 and 5, but about 0.2 standard deviations above the mean in eighth grade.

Figure 1

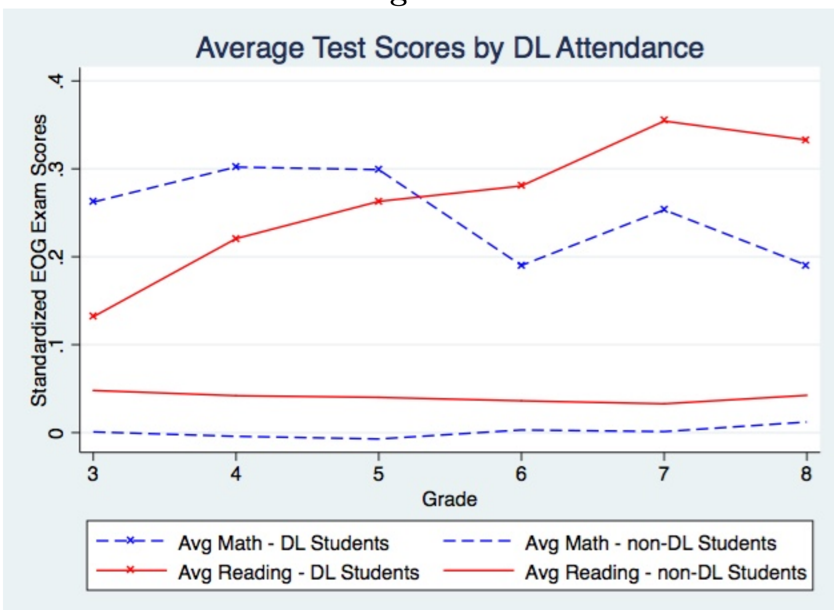


Figure 2

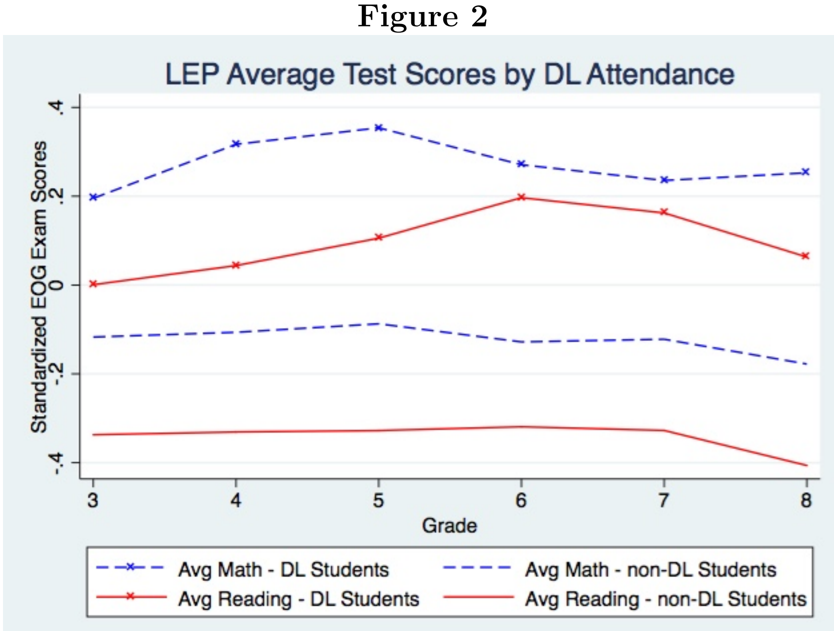
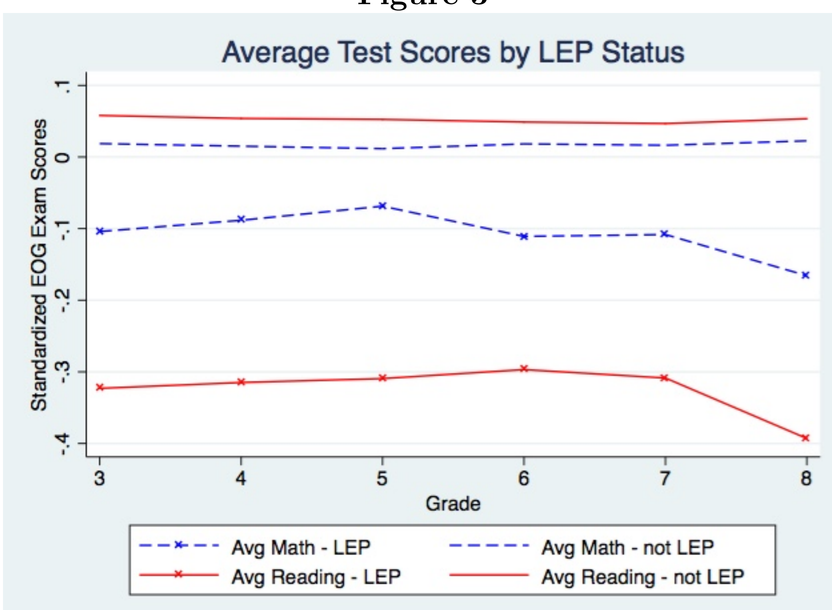


Figure 2 displays the analogous comparison for students who were identified as LEP in at least one grade, three through eight. Non-dual language, LEP students score below the state average in math and reading in every grade. On the other hand, LEP students who attend dual language schools score about 0.2 standard deviations above the state average in math in third grade, and more than 0.2 above the average in every grade after that. For comparison, Figure 3 compares average residualized test scores for students classified as LEP with scores for non-LEP students. LEP students score about 0.1 to 0.2 standard deviations below their non-LEP peers on math scores after accounting for some baseline characteristics, a gap which is substantially smaller than the DL advantage in math for LEP shown in Figure 2. Reading scores follow a similar pattern, for which DL students classified as LEP score at the state average in reading in third grade, and above it in every grade thereafter. Figure 2 shows a DL advantage of 0.3 - 0.4 standard deviations in reading for LEP students, and the LEP reading test score gap shown in Figure 3 is between 0.3 and 0.4 standard deviations in grades three through seven.

Figure 3



Tables 3 and 4 describe the lottery winners and losers.[29] Columns 1 - 3 describe the application sample, which includes all applicants regardless of whether valid test scores are

[29]Tables 3 and 4 exclude applicants who received sibling placements.

observed. Columns 4 - 6 describe the estimation samples, which are restricted to applicants with valid test scores. Average math and reading scores on their first exam are displayed for lottery winners in column 4 and those who lost the lottery in column 5. The differences in these scores give the raw test score gaps after restricting to the estimation sample. From Table 3, among the non-ESL/LEP students, lottery winners scored about 0.24 standard deviations (0.52 - 0.28) higher than lottery losers on their first end-of-grade math exam.[30] The analogous differences between winners and losers in the ESL/LEP sample are shown in Table 4. Lottery winners scored 0.08 standard deviations above the state mean on their first math exam, and lottery losers scored about 0.2 standard deviations below the state mean. That is a difference of about 0.28 standard deviations in favor of lottery winners on their first end-of-grade math exam. Similarly, lottery winners in the ESL/LEP sample scored about 0.24 standard deviations higher than lottery losers on their first end-of-grade reading exam, although both groups scored below the state average.

Tables 3 and 4 also show how frequently students comply with their initial lottery assignment. From Table 3, 89.9 percent of first choice lottery winners and 37.5 percent of first choice lottery losers from the non-ESL/LEP estimation subsample attend a dual language school, meaning that there is non-compliance among winners and losers. The first row of Table 4 gives the analogous figures for the ESL/LEP subsample. Ninety-three percent of lottery winners from the ESL/LEP estimation sample attend a dual language school and 29 percent of lottery losers attend a dual language school. There are several possible sources of non-compliance. First, a student who lost the lottery for their first choice could win a seat to a different dual language program with their second or third choice in the lottery. This is somewhat unlikely since they typically fill up with first choice applicants, but it does happen. From Table 3, about 31 (21) percent of lottery losers in the non-ESL/LEP estimation sample chose a dual language school with their second (third) choice. More than 11 percent of the lottery losers in that sample won a seat in a dual language program with their second or third choice. Table 4 shows that lottery losers from the ESL/LEP sample were less likely to choose

[30] The scores included are from the first exam score available for each student, which is typically the third grade score.

Table 3: Summary and Balance - English Proficient Sample

	Application Sample			Estimation Sample		
	Won	Lost	Test	Won	Lost	Test
	[1]	[2]	[3]	[4]	[5]	[6]
Attended (K/First)	0.870	0.358	0.509***	0.899	0.375	0.521***
	(0.337)	(0.480)	(0.042)	(0.302)	(0.485)	(0.050)
Won Any Choice	1.000	0.364	0.645***	1.000	0.367	0.649***
	(0.000)	(0.482)	(0.050)	(0.000)	(0.483)	(0.049)
Won Any DL Choice	1.000	0.113	0.880***	1.000	0.117	0.879***
	(0.000)	(0.317)	(0.036)	(0.000)	(0.322)	(0.036)
Female	0.546	0.513	0.012	0.533	0.508	0.003
	(0.499)	(0.501)	(0.045)	(0.500)	(0.501)	(0.050)
Black	0.304	0.354	-0.043	0.303	0.387	-0.075*
	(0.461)	(0.479)	(0.042)	(0.460)	(0.488)	(0.041)
White	0.372	0.268	0.017	0.373	0.262	0.027
	(0.484)	(0.444)	(0.043)	(0.484)	(0.441)	(0.047)
Hispanic	0.112	0.215	-0.038	0.111	0.206	-0.048
	(0.316)	(0.412)	(0.039)	(0.315)	(0.405)	(0.041)
Second Choice DL	0.407	0.328	0.074	0.408	0.315	0.081
	(0.492)	(0.470)	(0.044)	(0.492)	(0.465)	(0.049)
Third Choice DL	0.245	0.212	0.054	0.247	0.210	0.037
	(0.431)	(0.409)	(0.036)	(0.432)	(0.408)	(0.049)
Non-missing Test Scores	0.847	0.821	0.000	1.000	1.000	
	(0.361)	(0.384)	(0.035)	(0.000)	(0.000)	
FRPL	0.181	0.373		0.174	0.351	
	(0.374)	(0.467)		(0.380)	(0.478)	
EOG Math Score				0.524	0.281	0.269**
				(1.004)	(0.979)	(0.110)
EOG Reading Score				0.452	0.249	0.136
				(0.941)	(0.897)	(0.102)
Lottery FE			X			X
FRPL-Year Dummies			X			X
Neighborhood School FE			X			X
Observations	339	302	641	287	248	535
Number of Clusters			46			44

Table 4: Summary and Balance - ESL/LEP Sample

	Application Sample			Estimation Sample		
	Won	Lost	Test	Won	Lost	Test
	[1]	[2]	[3]	[4]	[5]	[6]
Attended (K/First)	0.891	0.275	0.642***	0.929	0.290	0.669***
	(0.312)	(0.448)	(0.067)	(0.258)	(0.455)	(0.068)
Won Any Choice	1.000	0.298	0.735***	1.000	0.297	0.711***
	(0.000)	(0.459)	(0.061)	(0.000)	(0.458)	(0.060)
Won Any DL Choice	1.000	0.023	0.976***	1.000	0.021	0.987***
	(0.000)	(0.152)	(0.013)	(0.000)	(0.143)	(0.013)
Female	0.457	0.444	-0.046	0.442	0.455	-0.052
	(0.500)	(0.498)	(0.074)	(0.499)	(0.500)	(0.096)
Black	0.054	0.053	0.016	0.062	0.055	0.026
	(0.227)	(0.224)	(0.027)	(0.242)	(0.229)	(0.025)
White	0.054	0.029	0.040	0.044	0.034	0.030
	(0.227)	(0.169)	(0.030)	(0.207)	(0.183)	(0.031)
Hispanic	0.829	0.895	-0.099**	0.823	0.890	-0.105**
	(0.378)	(0.308)	(0.043)	(0.383)	(0.314)	(0.046)
Second Choice DL	0.202	0.205	-0.107*	0.195	0.234	-0.136**
	(0.403)	(0.405)	(0.058)	(0.398)	(0.425)	(0.065)
Third Choice DL	0.147	0.111	0.003	0.142	0.117	-0.028
	(0.356)	(0.315)	(0.034)	(0.350)	(0.323)	(0.036)
Non-missing Test Scores	0.876	0.848	0.055	1.000	1.000	
	(0.331)	(0.360)	(0.046)	(0.000)	(0.000)	
FRPL	0.679	0.726		0.690	0.724	
	(0.462)	(0.441)		(0.464)	(0.448)	
EOG Math Score				0.082	-0.199	0.154
				(0.802)	(0.888)	(0.141)
EOG Reading Score				-0.074	-0.313	0.223
				(0.833)	(0.797)	(0.141)
Lottery FE			X			X
FRPL-Year Dummies			X			X
Neighborhood School FE			X			X
Observations	113	145	300	113	145	258
Number of Clusters			39			36

a dual language school for their second (third) choice, as only 23 (12) percent did, and only 2 percent of them won a seat in a dual language school. Second, students who do not win their first choice are placed on a waiting list for that school, which is accessed if seats become available. If a lottery winner chooses not to take the seat offered to them, the seat is offered to the next student on the waiting list. This is likely a major source of non-compliance from lottery losers. From the non-ESL/LEP estimation sample in Table 3, 10.1 percent of winners do not end up attending a dual language school. From Table 4, 7.1 percent of lottery winners in the ESL/LEP estimation sample do not attend a dual language school. Even if a winning student enrolls in the dual language school and attends that school, but eventually exits, that seat can be offered to another student. The waiting list can be accessed all the way through the first academic quarter of the school year. Lastly, students can reapply in the school choice lottery for the subsequent year and win a seat.[31] As discussed in detail in the next section, non-compliance does not invalidate the empirical strategy used here.

Tables 3 and 4 also contain a number of tests for differences in means between the lottery winners and losers. I test for differences in DL attendance, conditional on lottery fixed effects, by regressing the dummy variable for attending a DL school on a dummy for winning and lottery fixed effects. The results are shown in the first row of Tables 3 and 4. Rejecting the null hypothesis of no effect indicates that winning is correlated with attendance. The test in column 6 of Table 3 suggests that for students in the non-ESL/LEP estimation sample winning the lottery increases the probability of attending a DL school by about 0.52. Column 6 of Table 4 shows that in the ESL/LEP estimation sample winning increases the probability of attending a DL school by almost 0.67. Both estimates are strongly statistically significant, suggesting that winning the lottery is a good predictor for attending a DL school.

The remaining tests, found in columns three and six of Tables 3 and 4, give some indication whether the lottery results are truly random. Assignment is random within lottery groups, so the generated lottery fixed effects are included in each test. Since lottery groups depend on geographic location and free and reduced price lunch status, we shouldn't expect

[31] There is also a second lottery mainly for students who enrolled in CMS after the deadline for the first lottery, but second lottery applicants are placed at the end of the waitlist for oversubscribed programs.

fixed characteristics of applicants to be unrelated to winning the lottery unconditionally. A rejection of the null hypothesis suggests that winning the lottery might be related to that characteristic in some non-random way and generally gives cause for concern about the identification strategy proposed below. Tests in column 3 of Tables 3 and 4 are for the application sample, which is the sample that the randomization actually took place in. None of the tests in the non-ESL/LEP application sample reject the null hypothesis. After restricting to the estimation sample, the only rejection in column 6 of Table 3 is on the coefficient in the regression of a dummy variable for black on winning the lottery. From Table 4, there is a rejection of the null hypothesis for the dummy variable for Hispanic in both the application and estimation samples. There are at least two reasons why a test might reject even if the initial assignment is random. The first could be from non-random attirition from the sample. Since I am estimating effects on math and reading scores, students who do not remain in the sample long enough to observe test outcomes must be dropped for estimation. Even though assignment is random at the time of application, it is not necessarily random when restricting to the applicants that remain in the sample through third grade. Staying in the district could be related to winning the lottery and the resulting attrition would lead to selection bias [Rouse, 1998, Steele et al., 2016].[32] While this would not explain the rejection in the ESL/LEP application sample, it could explain the rejection in the non-ESL/LEP estimation sample. I include initial tests in Tables 3 and 4 for non-random attrition, which are from OLS regressions of having at least one available set of test scores on winning the lottery. Both tests fail to reject the null hypothesis, suggesting that non-random attrition is not an issue.[33] Another possible explanation is that assigment is actually random, and the rejection of the null hypothesis is an artifact of measurement error in the constructed proxies used for lottery fixed effects. Since priority groups depend on free and reduced price lunch status and characteristics of the student's neighborhood school, I control for this flexibly by including free and reduced lunch by cohort dummy variables and neighborhood school fixed

[32] Attrition is likely higher because of the lag between application and testing and the focus on students entering kindergarten. I have at least one set of exam scores for about eighty-five percent of applicants (see the rows labeled "Non-missing Test Scores" in Tables 3 and 4).

[33] I include a more formal discussion of non-random attrition below, as well as a discussion of estimates weighted for attrition.

effects, in addition to the lottery fixed effects. To further alleviate concerns of endogeneity and non-random attrition, I include a number of robustness checks including using weights based on the probability of remaining in the sample.

4 Empirical Strategy

The counterfactuals in Figure 1, lines for the non-DL students, include all non-DL students in the district, most of whom had no interest in attending a dual language school. Since there are likely systematic differences between DL applicants and non-applicants, estimates generated from this sort of analysis should not be considered causal. Once again, Figure 2 provides evidence that LEP, DL students score above their non-DL peers in math and reading on average, but the differences should not be interpreted as causal effects. For causal evidence, I turn to the randomization created by the oversubscribed lotteries, for which assignment must be a significant predictor of attendance and must be exogenous conditional on lottery fixed effects to have a causal interpretation. If students perfectly complied with the lottery assignment, then assignment would be synonomous with attendance and the causal effect could be estimated using OLS regressions of achievement on dual language school assignment/attendance as shown in equation 1.

$$Y_{i,j,g,s} = \gamma \cdot 1[DualLanguage]_{i,j} + \beta \cdot X_{i,j,g,s} + \Omega_j + N_s + \varepsilon_{i,j,g,s} \tag{1}$$

Where $Y_{i,j,g,s}$ represents an end-of-grade math or reading exam score of student i in grade g who applied to lottery j from neighborhood school s. The variable of interest, $1[DualLanguage]_{i,j}$, is a dummy variable that is equal to one if the student attended a dual language school.[34] Lottery fixed effects, Ω_j, are included because winning the lottery is not

[34]Using enrollment in the year of the exam is one way to measure participation. That leaves a lot of time between application and when enrollment is measured. One might worry that this could bias estimates since students have time to apply to other schools or simply withdraw from the dual language program, both of which are likely non-random. For this reason, I prefer using enrollment in kindergarten as the participation measure. In one year of the data (2007), the school of attendance in kindergarten is missing for a non-trivial portion of applicants, many of whom show up in a dual language school in first grade. For this reason, I actually measure attendance as showing up in a dual language school in either kindergarten or first grade.

unconditionally random, but students are drawn randomly within lottery. Fixed effects for the neighborhood school[35] that the student was assigned to at the time of application, N_s, and student level covariates, $X_{i,j,g,s}$, are also included. Grades are pooled for estimation, so grade of exam dummy variables are included in $X_{i,j,g,s}$. One concern with this approach is that, although the assignment is random conditional on lottery fixed effects, compliance with initial assignment may not be random, leading to a biased and inconsistent estimator for the average treatment effect. Compliance might be non-random for a couple of reasons. In particular, over 30 percent of lottery losers end up attending a dual language school. Students who attend a dual language school, despite losing the lottery for their first choice, might be systematically different from the students who lost and did not end up attending a dual language school. For example, students who chose a dual language program with their second and/or third choice are more likely to attend a dual language school relative to those who did not specify a dual language school with their second and/or third choice. Non-compliance could represent strength of preferences for dual language schooling or for their neighborhood school, or the ability of parents to maneuver their way into their first choice school. Since OLS estimators are biased and inconsistent if attending a dual language school is non-random, I focus on estimating the intention-to-treat and local average treatment effects which are consistent when assignment is random conditional on lottery fixed effects.

I follow a standard approach for estimating treatment effects using applicants for over-subscribed lotteries [Deming et al., 2014, Rouse, 1998]. The intention-to-treat effect is estimated by regressing end-of-grade math and reading scores on a dummy for winning the lottery and a set of covariates in the sample of lottery applicants, as shown in equation 2.

$$Y_{i,j,g,s} = \gamma^{ITT} \cdot 1[LotteryWinner]_{i,j} + \beta^{ITT} \cdot X_{i,j,g,s} + \Omega_j^{ITT} + N_s^{ITT} + \varepsilon_{i,j,g,s}^{ITT} \tag{2}$$

Where $1[LotteryWinner]_{i,j}$ indicates whether student i was a winner of lottery j. The

[35] Neighborhood school refers to the school that the student was assigned to at the time of the lottery. This is the school that the student would be assigned to attend in kindergarten unless the student either opts out during the lottery, enrolls in a charter or private school, or changes address.

difference between equations 1 and 2 is that equation 2 replaces the attendance variable, $1[DualLanguage]_{i,j}$, with the assignment variable, $1[LotteryWinner]_{i,j}$. Now the estimated treatment effect, $\hat{\gamma}^{ITT}$, is for the intention-to-treat [Imbens and Angrist, 1994]. The estimators from equations 1 and 2 are not estimating the same parameter, but $\hat{\gamma}^{ITT}$ is consistent under the assumption that assignment is random. Whereas, consistency of $\hat{\gamma}$ requires the less plausible assumption that attending a dual language school is random. Both the intention-to-treat and local average treatment effect estimators share this advantage over the OLS estimator from equation 1.

Equations 3 and 4 describe a two-stage estimation strategy using the dummy for winning the lottery as an instrument for attending a dual language school. Now $\hat{\gamma}^{LATE}$ is an estimate of the local average treatment effect, the effect for those who are induced to participate by winning the lottery [Imbens and Angrist, 1994]. In the main specification, the effects are actually estimated by pooling grades and interacting the treatment dummy with years of treatment (grade of exam plus one). Dummy variables are included for grade of exam, leading to a per-year of participation intepretation.

$$1[DualLanguage]_{i,j} = \gamma^{DL} \cdot 1[LotteryWinner]_{i,j} + \beta^{DL} \cdot X_{i,j,g,s} + \Omega_j^{DL} + N_s^{DL} + \varepsilon_{i,j,g,s}^{DL} \quad (3)$$

$$Y_{i,j,g,s} = \gamma^{LATE} \cdot \hat{1}[DualLanguage]_{i,j} + \beta^{LATE} \cdot X_{i,j,g,s} + \Omega_j^{LATE} + N_s^{LATE} + \varepsilon_{i,j,g,s}^{LATE} \quad (4)$$

I perform specification checks to alleviate any concerns from exogeneity of the treatment or non-random attrition, including using weights based on the estimated probability of remaining in the sample.[36] Weighting the regressions adjusts for attrition related to observable characteristics. Including neighborhood school fixed effects in all of the main estimates also

[36]Remaining in the sample means that the student has valid end-of-grade exam scores for at least one grade. Weights are based on estimated probabilities from logit regressions of an indicator for staying in the sample on race, gender, FRPL, and a dummy for winning the lottery.

further restricts the comparisons to help with any concerns about misspecifying the lottery fixed effects. Neighborhood school is defined as the school that the student would have been assigned to if they did not win any seat in the lottery, change address, or enroll in a charter or private school. Having the same neighborhood school means that the students live in the same geographic area and have the same outside schooling option. For comparison, I also show estimates from an alternative specification that does not include neighborhood school fixed effects.

In addition to estimating the effect of attendance on achievement, I estimate the effect of attending a dual language school on limited English proficiency status. I interact the treatment and attendance variables with each grade (three through six), and estimate the effect on having limited English proficient status in each grade on the ESL/LEP sample. Prior research suggests that dual language participants re-classify at a slower rate in early grades, but eventually surpass their non-dual-language-schooled peers [Umansky and Reardon, 2014]. This is a good point of reference, although we should not necessarily expect these results to be the same. These are two different contexts, and I focus on estimating effects in a select subsample, unlike the district wide analysis by Umansky and Reardon [2014].

5 Results

5.1 Main Results

I begin by providing first stage ($\hat{\gamma}^{DL}$ from equation 3) and treatment effect ($\hat{\gamma}^{ITT}$ from equation 2 and $\hat{\gamma}^{LATE}$ from equation 4) estimates from the main specification in Table 5. Panel A of Table 5 shows estimates for the non-ESL/LEP sample of applicants, students who were never identified as eligible for English second language services or as limited English proficient.

The estimated effect on math scores in column 5 suggests that among compliers, attending a dual language school led to an increase in math scores of 0.089 standard deviations. This can be interpreted as a per-year gain in achievement. Column 7 shows an effect on reading

Table 5: Impact of Attending a Dual Language School on Achievement

Panel A: English Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.464***	0.042*		0.024	
			(0.064)	(0.022)		(0.015)	
Attend DL School	-0.004	-0.022*			0.089*		0.053*
	(0.018)	(0.011)			(0.047)		(0.032)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	1,472	1,472	1,472	1,472	1,472	1,472	1,472
Number of Clusters	44	44	44	44	44	44	44

Panel B: ESL/LEP Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.667***	0.052**		0.042*	
			(0.069)	(0.024)		(0.024)	
Attend DL School	0.052***	0.065***			0.078**		0.064**
	(0.019)	(0.019)			(0.034)		(0.032)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	809	809	809	809	809	809	809
Number of Clusters	36	36	36	36	36	36	36

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1
*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of exam, year of exam, and neighborhood school fixed effects. Attendance is measured by whether the student attended a DL school in kindergarten or first grade and interacted with years of treatment (grade plus one). Standard errors are clustered by lottery.

scores for this sample of 0.053 standard deviations per year. Both estimates are statistically significant at the ten percent level. These estimates are promising for the growing practice of dual language education for native English speakers. At least in CMS, the dual language schools have been successful in delivering instruction in a second language, and increasing math and reading exam scores for English proficient students. Although these estimates do not separate out the mechanisms through which achievement gains are operating, they show that it is possible to successfully promote bilingualism and increase academic achievement.

Panel B of Table 5 shows estimated treatment effects for the ESL/LEP sample. The estimated effect of attending a dual language school on math scores in column 5 is 0.078. From column 7, the estimated effect on reading scores in this sample is 0.064. Both estimates are statistically significant at the five percent level. While these estimates are large, they are in line with the fact that treatment is muti-year and begins at a young age. The estimates suggest that dual language education can be an effective teaching method for ELLs and can help to reduce achievement gaps in math and reading. Consider the achievement gaps between LEP and non-LEP students, which are displayed in Figure 3. The district average math and reading scores for LEP students are below the state averages in every grade, and below the district non-LEP averages in every grade. The largest gap in math scores is about 0.2 standard deviations, so the estimate of 0.078 standard deviations per year is large enough to more than close that gap by third grade. The largest disparity in reading scores is a little more than 0.4 standard deviations. The estimated effect on reading scores of 0.064 standard deviations is enough to close the gap in test scores by the end of elementary school.

As shown in Table 6, the estimates are not sensitive to the omission of neighborhood school fixed effects. The estimate on reading scores for the non-ESL/LEP sample without neighborhood school fixed effects, shown in column 7 of Table 6, is 0.057 and statistically significant at the ten percent level. The estimated impact on math scores in that sample, reported in column 5, is 0.086, and is statistically significant at the five percent level. Estimated effects in the ESL/LEP sample without neighborhood school fixed effects are reported in Panel B of Table 6. The estimated effect on math scores is reported in column 5. It is a little smaller than in the main specification, now 0.063, but still statistically significant at the five percent level. The estimated effect on reading scores, reported in column 7, increases from 0.064 to 0.069, and is still statistically significant at the five percent level. The exclusion of neighborhood school fixed effects makes very little difference. All four of the estimates remain positive, similar in magnitude to the initial estimates, and statistically significant.

Table 6: Impact of Attending a Dual Language School on Achievement

Panel A: English Sample

	OLS			**Math**		**Reading**	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.471***	0.040*		0.027*	
			(0.081)	(0.020)		(0.014)	
Attend DL School	0.011	-0.010			0.086**		0.057*
	(0.017)	(0.014)			(0.043)		(0.032)
Observations	1,472	1,472	1,472	1,472	1,472	1,472	1,472
Number of Clusters	44	44	44	44	44	44	44

Panel B: ESL/LEP Sample

	OLS			**Math**		**Reading**	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.635***	0.040**		0.044**	
			(0.084)	(0.019)		(0.020)	
Attend DL School	0.039**	0.065***			0.063**		0.069**
	(0.019)	(0.023)			(0.031)		(0.030)
Observations	809	809	809	809	809	809	809
Number of Clusters	36	36	36	36	36	36	36

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1
*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of exam, and year of exam. Attendance is measured by whether the student attended a DL school in kindergarten or first grade and interacted with years of treatment (grade plus one). Standard errors are clustered by lottery.

Since non-random attrition would lead to inconsistent estimators and test scores are missing for a non-trivial portion of applicants, I include estimates that are weighted by the inverse of the estimated probability of having test scores in the data. I estimate the probability of remaining in the sample long enough to have valid test scores using logit regressions on dummy variables for race/ethnicity, gender, FRPL, and winning the lottery, then use the inverse of the estimated probabilities as weights in the estimation. The estimated probabilities are summarized in Panel A of Table 7.

Table 7: Attrition and Weighting

Panel A: Summary of Probabilities of Staying

	Full Sample		English Sample		ESL/LEP Sample	
	Winners	Losers	Winners	Losers	Winners	Losers
	[1]	[2]	[3]	[4]	[5]	[6]
Average Pr(Stay)	0.855	0.831	0.847	0.821	0.876	0.848
SD Pr(Stay)	0.031	0.038	0.043	0.060	0.040	0.047
APE	0.024		0.015		0.019	
(SE)	(0.024)		(0.030)		(0.042)	
N	468	473	339	302	129	171

Panel B: Non-Random Attrition

	Coefficient on Indicator for Winning		
	Full Sample	English	ESL/LEP
	[1]	[2]	[3]
No Controls	0.024	0.026	0.028
(SE)	(0.024)	(0.030)	(0.040)
Controls + Lottery FE	0.019	-0.003	0.047
(SE)	(0.024)	(0.029)	(0.036)
+ Neighborhood School FE	0.025	-0.002	0.052
(SE)	(0.029)	(0.038)	(0.049)
N	941	641	300

*Notes: Panel A summarizes estimated probabilites of having at least one set of test scores available. The estimates are based on logit regressions including gender, race, frpl-year, and an indicator for winning the lottery. Panel B shows estimated coefficients from OLS regressions of having at least one set of test scores available on winning the lottery. The baseline controls are gender, race, and frpl-year. The second set of OLS estimates also conditional on the lottery fixed effects, and the third set include neighborhood school fixed effects.

From column 3 in Table 7, the estimated probability of having a set of test scores in the data among those who won in the non-ESL/LEP subsample is almost 85 percent, and the estimated probability for those who lost the lottery is only slightly lower at 82 percent. The estimated average partial effect of winning on remaining the sample is 0.015, and not statistically different from zero. Similarly, in the ESL/LEP subsample, the estimated average partial effects of winning on remaining in the sample is 0.019 with a standard error of 0.042. The estimates suggest that winning the lottery is not a strong predictor of remaining in the sample, alleviating concerns about non-random attrition. Panel B shows linear tests for non-random attrition, including tests that condition on lottery fixed effects and neighborhood school fixed effects. From column 2 in Panel B of Table 7, winning the lottery does not appear to be strongly correlated with remaining in the sample of English proficient students. The estimated coefficient on winning is -0.002 with a standard error of 0.038. Similarly, among the sample of ESL/LEP students, the estimated coefficient on winning is positive, 0.052, but statistically insignificant with a standard error of 0.049. These linear tests provide further evidence that non-random attrition is not an issue, because despite attrition rates of around fifteen percent on average, attrition is not strongly correlated with winning the lottery.[37] Despite the apparent lack of correlation betwen winning the lottery and remaining in the sample, weighted estimates are reported in Table 8 to show that the estimates are not sensitive to weighting. Panel A shows the inverse probability weighted estimates for the non-ESL/LEP sample. From colmun 5 of Panel A, the estimated treatment effect for math scores is now 0.089, the same as the initial estimate, and significant at the ten percent level. The weighted estimate for reading in that sample, from column 7, is 0.052, almost identical to the intial estimate. The weighted estimates on math and reading scores in the non-ESL/LEP sample are the same as the estimates from the initial specification, suggesting that non-random attrition is not likely to be a significant factor. Estimates for the ESL/LEP sample are shown in Panel B of Table 8. The estimated average treatment effects on math and reading scores are the same as the initial estimates, and both are still statistically significant at the five percent level. Weighting has almost no impact on the point estimates,

[37]The estimates in Table 7 are based on a single observation for each individual applicant, and a dummy variable indicating whether the student has valid test scores in any grade. Another way to investigate non-random attrition would be to expand that data to include an observation for each individual for each grade that they could have tested in. The results are not sensitive to this alternative method. See Table A5 for

Table 8: Impact of Attending a Dual Language School on Achievement - Weighted

Panel A: English Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.468***	0.042*		0.024	
			(0.065)	(0.022)		(0.015)	
Attend DL School	-0.004	-0.022*			0.089*		0.052*
	(0.018)	(0.012)			(0.046)		(0.031)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	1,472	1,472	1,472	1,472	1,472	1,472	1,472
Number of Clusters	44	44	44	44	44	44	44

Panel B: ESL/LEP Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.673***	0.052**		0.043*	
			(0.071)	(0.025)		(0.025)	
Attend DL School	0.053***	0.066***			0.078**		0.064**
	(0.019)	(0.020)			(0.033)		(0.032)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	809	809	809	809	809	809	809
Number of Clusters	36	36	36	36	36	36	36

Robust standard errors in parentheses

***p<0.01, **p<0.05, *p<0.1

*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of exam, year of exam, and neighborhood school fixed effects. Attendance is measured by whether the student attended a DL school in kindergarten or first grade and interacted with years of treatment (grade plus one). Regression are weighted by the inverse probability of having test scores available in the data. Weights were generated from logit regressions. Standard errors are clustered by lottery.

which suggests that non-random attrition is probably not inflating the estimates much, if at all.

5.2 Heterogeneity

I report differential effects by gender (columns 1-2), program type (columns 3-4), and race/ethnicity (columns 5-7) in Table 9. Heterogeneous treatment effects are estimated by interacting dummy variables indicating mutually exclusive sets of students with the attendance variable, and using the same dummy variables interacted with the assignment variable as instruments. Estimates for the non-ESL/LEP sample are reported in Panel A. Columns 1 and 2 of Panel A show that effects on math scores for females, 0.106, are stronger than for males, 0.068. Similary, the estimated effect on reading for females is 0.069 and statistically significant at the five percent level, and the effect for males is a statistically insignificant 0.030. On the other hand, columns 1 and 2 in Panel B suggest that the effect is stronger for males in the ESL/LEP subsample. The difference in heterogeneous effects by gender between samples is somewhat striking, and may reflect the difference in treatments. A big part of the treatment for students in the ESL/LEP subsample is likely that they receive some instruction in their home language as opposed to English immersion coupled with ESL services. On the other hand, treatment in the non-ESL/LEP sample is typically receiving instruction in a second language as opposed to English immersion. The differences in heterogeneity could result from differing treatments and potentially different mechanisms facilitating the effects.

Effects for one-way and two-way programs are reported in columns 3 and 4. The difference comes down to which school the student applied to since Waddell contains all of the one-way programs and the other two schools, Collinswood and Oaklawn, house two-way programs only. The size of the estimated effects are similar by program type for the non-ESL/LEP sample, but the estimates on effects for one-way programs have much larger standard errors. For example, the estimated effect on math scores for one-way programs in that sample is 0.081, but the standard error is 0.127. The estimate on math for two-way

more evidence.

Table 9: Heterogeneous Effects

Panel A: English Sample

	Gender		School Type		Race/Ethnicity		
	Female	Male	One-Way	Two-Way	White	Black	Hispanic
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Math	0.106**	0.068	0.081	0.090*	0.190**	0.046	0.090*
	(0.054)	(0.046)	(0.127)	(0.046)	(0.076)	(0.053)	(0.048)
Reading	0.069**	0.030	0.032	0.054**	0.084	0.034	0.115***
	(0.034)	(0.043)	(0.104)	(0.028)	(0.055)	(0.034)	(0.037)
Neighborhood School FE	X		X		X		
Observations	1,472		1,472		1,472		
Number of Clusters	44		44		44		

Panel B: ESL/LEP Sample

	Gender		School Type		Race/Ethnicity		
	Female	Male	One-Way	Two-Way	White	Black	Hispanic
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Math	0.051	0.090**	-0.018	0.079**	-	-	0.083***
	(0.048)	(0.038)	(0.176)	(0.034)	-	-	(0.031)
Reading	0.011	0.087**	-0.055	0.065**	-	-	0.062**
	(0.045)	(0.039)	(0.157)	(0.031)	-	-	(0.031)
Neighborhood School FE	X		X		X		
Observations	809		809		809		
Number of Clusters	36		36		36		

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1
*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of exam, year of exam, and neighborhood school fixed effects. Attendance is measured by whether the student attended a DL school in kindergarten or first grade and interacted with years of treatment (grade plus one). Heterogeneous effects are estimated using interactions with the assignment variable and instrumenting for interactions with the attendance variable. Standard errors are clustered by lottery.

programs is 0.090 with a standard error of 0.046. There is also a statistically significant estimated effect for two-way applicants of 0.054 on reading scores, but the estimated effect for one-way applicants is smaller and statistically insignificant. Panel B in Table 9 reports estimated treatment effects for students in the ESL/LEP sample for one-way and two-way programs. Similar to the non-ESL/LEP sample, estimates for one-way programs are very noisy. The estimate on math scores for one-way programs is -0.018 in this sample, but the standard error is 0.176. The estimated effects for two-way applicants in the ESL/LEP sample are 0.079 and 0.065 on math and reading scores, respectively. Both estimates are statistically significant at the five percent level.

Finally, I estimate heterogeneous effects by race/ethnicity in the non-ESL/LEP sample in columns 5, 6, and 7 of Panel A in Table 9. The estimated impact on math scores is largest in the white subsample, but estimates for the black and Hispanic subsamples are also positive and the estimate for the Hispanic subsample is statistically significant at the ten percent level. The estimated treatment effect on math scores for the white subsample is 0.190, which is large relative to most other estimated effects, and is significant at the five percent level. The estimated effect on the black subsample is 0.046 but it is statistically insignificant. The estimated treatment effect on math scores in the Hispanic subsample is 0.090 and significant at the ten percent level. Estimated effects on reading scores are relatively similar across the white, and Hispanic subsamples. From Panel A of Table 9, the estimated effect on reading scores in the black subsample, 0.034, is less than half the size of that estimate in the white subsample, 0.084, but both estimates are statistically insignificant. The only significant effect on reading scores is on the Hispanic subsample, 0.115, and it is significant at the one percent level.

I do not estimate effects for each race/ethnicity in the ESL/LEP sample, because 85% of the students in that sample are Hispanic. Any estimate for other races would be unreliable. However, restricting to the Hispanic subsample using dummy interactions shows that the main finding is robust in this subsample. Column 7 in Panel B of Table 9 shows the estimated effects on math and reading for the Hispanic students in the ESL/LEP sample. The estimated effects on math and reading scores are 0.083 and 0.062, respectively. Both estimates are statistically significant.

Table 10 shows estimates by grade. These are estimated by interacting the DL attendance variable and/or the indicator for winning the lottery with each exam grade. The estimated impact on math exam scores for the non-ESL/LEP sample are shown in column 5 of Panel A. The estimated effect for math scores on the third grade interaction is 0.374, and significant at the five percent level. The estimated effect on math scores for sixth grade is 0.721 and significant at the five percent level. This estimate is only identified from two of the cohorts, leading to a relatively large standard error of 0.346. The estimates for the effect on reading scores in this sample are shown in column 6 of Panel A in Table 10, and they also appear to exhibit an increasing pattern with grade. The estimates on the third and fourth grade interactions are 0.215 and 0.151, respectively. Neither of them are statistically significant. The largest estimate is on the fifth grade term, 0.431, and it is significant at the five percent level.

Estimated effects are also reported by grade for the ESL/LEP sample in Table 10. The estimated effects are stronger in the ESL/LEP sample, but the estimated effects for math scores do not exhibit quite as strong of an increasing pattern with grade. The effect on math scores on the third grade interaction from column 5 in Panel B of Table 10 is 0.393 and significant at the five percent level. The estimated coefficient on the sixth grade interaction is 0.542 and is also significant at the five percent level. The largest of all of the estimated effects on math scores for the ESL/LEP sample is on the fourth grade interaction. That estimate is 0.657 and significant at the five percent level. The largest estimated effect on reading in the ESL/LEP sample is on the sixth grade interaction, an effect of 0.531 and significant at the one percent level.

Table 10: Effects by Grade

Panel A: English Sample

	OLS		ITT		LATE	
	Math	Reading	Math	Reading	Math	Reading
	[1]	[2]	[3]	[4]	[5]	[6]
Grade						
Third	-0.011	-0.152*	0.189**	0.108	0.374**	0.215
	(0.120)	(0.082)	(0.092)	(0.079)	(0.171)	(0.147)
Fourth	0.042	-0.086	0.211	0.070	0.446	0.151
	(0.112)	(0.078)	(0.143)	(0.093)	(0.284)	(0.188)
Fifth	-0.009	-0.115	0.139	0.216**	0.264	0.431**
	(0.138)	(0.094)	(0.150)	(0.096)	(0.276)	(0.205)
Sixth	-0.107	-0.107	0.327**	0.136	0.721**	0.298
	(0.121)	(0.091)	(0.141)	(0.135)	(0.346)	(0.295)
Neighborhood School FE	X	X	X	X	X	X
Observations	1,472	1,472	1,472	1,472	1,472	1,472
Number of Clusters	44	44	44	44	44	44

Panel B: ESL/LEP Sample

	OLS		ITT		LATE	
	Math	Reading	Math	Reading	Math	Reading
	[1]	[2]	[3]	[4]	[5]	[6]
Grade						
Third	0.055	0.312***	0.244**	0.244*	0.393**	0.390**
	(0.097)	(0.101)	(0.109)	(0.128)	(0.174)	(0.187)
Fourth	0.374**	0.254*	0.413**	0.153	0.657**	0.238
	(0.138)	(0.134)	(0.171)	(0.167)	(0.274)	(0.248)
Fifth	0.397***	0.370***	0.320*	0.208	0.471**	0.302
	(0.131)	(0.100)	(0.163)	(0.137)	(0.229)	(0.188)
Sixth	0.417***	0.408***	0.367**	0.364**	0.542**	0.531***
	(0.134)	(0.145)	(0.171)	(0.140)	(0.231)	(0.183)
Neighborhood School FE	X	X	X	X	X	X
Observations	809	809	809	809	809	809
Number of Clusters	36	36	36	36	36	36

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1
*Notes: Each regression includes lottery fixed effects and controls for female, race, frpl-year, exceptionality, grade of exam, year of exam, and neighborhood school fixed effects. Attendance is measured by whether the student attended a DL school in kindergarten or first grade and intearacted with each exam grade. Instruments are interactions between grade of exam and the indicator for winning the lottery. Standard errors are clustered by lottery.

5.3 Mechanisms

The CMS data also contain the neighborhood school that each student is assigned to, which helps to describe the outside options that students are foregoing to enter a dual language school. Characteristics of the neighborhood schools of the applicants are informative for thinking about the counterfactual. Language of instruction is not the only thing that changes for the student when they opt out of their neighborhood school and into a dual language school. Specifically, there could be changes in peer quality and composition of the student body that might influence achievement [Hoxby, 2000, Whitmore, 2005, Sacerdote, 2011, Imberman et al., 2012, Billings et al., 2014]. Mean characteristics of the schools that applicants are opting out of are displayed in Table 11. Applicants to Oaklawn come from schools that have a relatively high proportion of minorities (12 percent white), 76 percent of students on free and reduced price lunch, and score 0.3 standard deviations below the state average on end-of-grade math and reading exams. They come from neighborhood schools that score worse than the average for all applicants. On the other hand, applicants to Waddell and Collinswood come from neighborhood schools with a smaller percentage of FRPL students (57 percent and 65 percent, respectively), but still score below the state average on end-of-grade math and reading exams.

Table 11: Neighborhood School Characteristics of Lottery Applicants

	One-Way School	Two-Way Schools		Other Applicants
	Waddell	Collinswood	Oaklawn	
	[1]	[2]	[3]	[4]
Neighborhood School				
White	0.315	0.250	0.119	0.197
Black	0.383	0.389	0.607	0.501
Hispanic	0.216	0.281	0.194	0.220
FRPL	0.574	0.652	0.760	0.688
LEP	0.180	0.232	0.175	0.194
EOG Exam Scores				
Math	-0.010	-0.080	-0.318	-0.186
Reading	-0.032	-0.136	-0.357	-0.224
N	1,147	1,112	533	13,071

*Notes: This table displays mean characteristics of the neighborhood schools that applicants are assigned to weighted by the number of applicants from each school. Everything is based on first choice school.

Self-selection and peer effects could play a significant role in the high performance of DL schools,[38] but there are other features that might hurt their performance relative to neighborhood schools. Specifically, DL schools experience higher teacher turnover and begin with larger classes in kindergarten. Dual language classrooms need teachers who are fluent in the language of instruction, so the schools in CMS often recruit teachers from abroad. The teachers are permitted to work in the U.S. for a limited amount of time, leading to higher turnover. This is particularly true in Collinswood and Oaklawn. Table 12 shows that over 50 percent of the teachers in each of those schools has zero to 3 years of experience, compared to about 30 percent in the neighboring schools[39] and other magnets. Not all teachers and staff members in dual language schools come from abroad, nor are they necessarily fluent in a second language. Since they often implement team teaching, in most grades there is at least one English speaking teacher. Table 12 shows that the dual language schools do have highly experienced teachers, although they have a smaller proportion than the neighboring schools and other magnets. From column seven, 37 percent of teachers at other magnets have 11 or more years of experience, but that number is only 25 percent at Collinswood (column 4) and 27 percent at Waddell (column 2). Since students can not enroll in a dual language school after kindergarten (or first grade) without meeting a minimum language requirement, the schools start with larger class sizes, anticipating some attrition throughout elementary school. The average kindergarten class has 21.4 students at Collinswood and 22.3 at Waddell, as seen in columns 2 and 4 of Table 12.[40] That is 3 more students than the other schools in their respective areas. From column 7 of Table 12, other magnet schools have and average of 18.7 students in a kindergarten class. Although there are several differences between dual language schools and the typical neighborhood school, greater teacher turnover [Ronfeldt et al., 2013] and larger early elementary school class size [Krueger, 1999, Angrist and Lavy, 1999] are two

[38]For example, Table 12 shows that over 75 percent of students at Oaklawn are at grade level in reading, but only 50 percent of the students at schools in the area near Oaklawn are at grade level in reading.

[39]I refer to schools in their area as the neighborhood school zone that the dual language school is in as well as all of the school zones contiguous to that zone. Since all of the dual language schools are full magnet schools, no students are automatically assigned to them. Instead each student has a neighborhood school assignment that they attend unless they opt out through the lottery, change address, or enroll in a charter or private school.

[40]Figure A1 shows the proportion of lottery winners and losers attending a DL school in grades 1 - 5 for the 2007 - 2009 cohorts, which suggests that the class sizes decrease throughout elementary school.

characterstics of DL schools that could lead to lower achievement.

Table 12: Dual Language and Neighborhood School Characteristics

	Waddell		Collinswood		Oaklawn		
	Area	Waddell	Area	Collinswood	Area	Oaklawn	Other Magnets
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Teaching Experience							
0 - 3 Years	0.329	0.320	0.294	0.501	0.298	0.542	0.299
11+ Years	0.300	0.272	0.367	0.255	0.359	0.195	0.374
FRPL	0.806	0.334	0.759	0.573	0.900	0.688	0.568
AYP Targets	0.869	0.986	0.871	1.000	0.779	1.000	0.873
Pct at Grade Level							
Reading	0.612	0.822	0.612	0.880	0.496	0.758	0.718
Math	0.698	0.878	0.705	0.944	0.531	0.753	0.733
KG Class Size	18.057	21.400	19.049	22.333	18.200	19.333	18.736

*Note: Average characteristics at each dual language school, for the neighborhood schools with zones contiguous to each dual language school, and all other magnet schools.

5.4 LEP Classification

In addition to estimating treatment effects on math and reading scores, I estimate the effect of attending a dual language school on LEP classification among the sample of students ever eligible for ESL services or considered LEP. I estimate the effects by regressing a dummy variable for being considered LEP in a given year on DL attendance by grade interactions. I instrument for attendance by grade interactions using a dummy for winning the lottery interacted with each grade. OLS estimates by grade are shown in columns 1 and 2 of Table 13. Column 1 shows estimates without neighborhood school fixed effects. Every estimate in column 1 is negative, meaning that students who attend DL schools are less likely to be considered limited English proficient in each grade. The largest in absolute value is the -0.210 estimate on the sixth grade interaction and it is significant at the one percent level. The analogous treatment effects are shown in column 3. They are all negative, but only the estimate on the sixth grade interaction, -0.168, is statistically significant. These estimates are in line with the higher English reading scores, but seems to counter some results in the prior literature [Umansky and Reardon, 2014] yet agree with others [Steele et al., 2016].

These results are not necessarily comparable with prior literature on re-classification since estimates are specific to a set of students who applied for dual language schools in CMS. Furthermore, all of the estimates on LEP classification are noisy and most of them are not significantly different from zero. In general though, they suggest that movements forcing English immersion on ESL/LEP students might be misguided. In this setting, students attending DL schools not only score higher on math and reading exams, but they are also less likely to be considered LEP in grades 3-6.

Table 13: Impact of Dual Language Schooling on LEP Status

	Limited English Proficient			
	OLS		LATE	
	[1]	[2]	[3]	[4]
Attend DL School				
Grade 3	-0.025	0.021	-0.032	0.045
	(0.047)	(0.055)	(0.084)	(0.101)
Grade 4	-0.148**	-0.120	-0.159	-0.107
	(0.071)	(0.080)	(0.137)	(0.160)
Grade 5	-0.192**	-0.176*	-0.071	-0.051
	(0.079)	(0.093)	(0.118)	(0.144)
Grade 6	-0.210***	-0.196**	-0.168*	-0.141
	(0.073)	(0.076)	(0.086)	(0.111)
Neighborhood School FE		X		X
Observations	809	809	809	809
Number of Clusters	36	36	36	36

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1
*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of observation, and year of observation. Attendance is measured by whether the student attended a DL school in kindergarten or first grade and interacted with grade dummy variables. Estimates are from OLS and 2SLS interaction terms. Standard errors are clustered by lottery.

6 Conclusion

Dual language schools offer an alternative option for students to learn curriculum in a non-English language. Using random assignment from school choice lotteries, I estimate that attending a dual language school lead to increases of 0.06 and 0.08 standard deviations per year on math and reading exam scores, respectively, among students who were ever eligible for ESL services or considered LEP. The estimated effects are large enough to close the LEP - non-LEP achievement gap in math and reading if applied to an average LEP student in CMS. I find further evidence that among students in this sample, those who attend a dual language school are less likely to be considered LEP in grades three through six, although the differences are generally statistically insignificant. English first language applicants also experience large gains estimated at 0.09 and 0.05 standard deviations per year in math and reading, respectively.

The widespread benefits of dual language education have important implications for education policy in the United States. They suggest that DL education would be an effective route for educating and assimilating non-English dominant students, a population which are costly to serve and have historically underperformed relative to their English dominant peers. The estimated effects suggest that DL education is a way to boost achievement and decrease reliance on English Second Language services, as they re-classify sooner and learn a portion of curriculum in their native language. Furthermore, for English first language students, it appears that the dual language schools in CMS provide a good opportunity for them to become bilingual and biliterate while increasing achievement in math and reading. A program that accomplishes all of these goals simultaneously through pairing non-English dominant students with English dominant students rather than separating them, should be uniquely desirable to a broad set of families, administrators, and policymakers.

Future research should aim to disentangle the mechanisms that facilite the achievement gains realized by the dual language and immersion students. Although the lottery winners in both subsamples in this study have been successful at learning in a non-English language and outperforming their peers who lost the lotteries, the current study does not specify the mechanisms through which gains were realized, and therefore can not distinguish an effect of learning in a second language itself from potential differences in peer and teacher quality, among other things. Separating out these mechanisms could point to, or rule out, specific aspects of the CMS schools that are critical to the achievement gains, and should be a primary goal of future research on the topic.

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Appendices

(not intended for publication)

A Supplemental Results

Figure A1

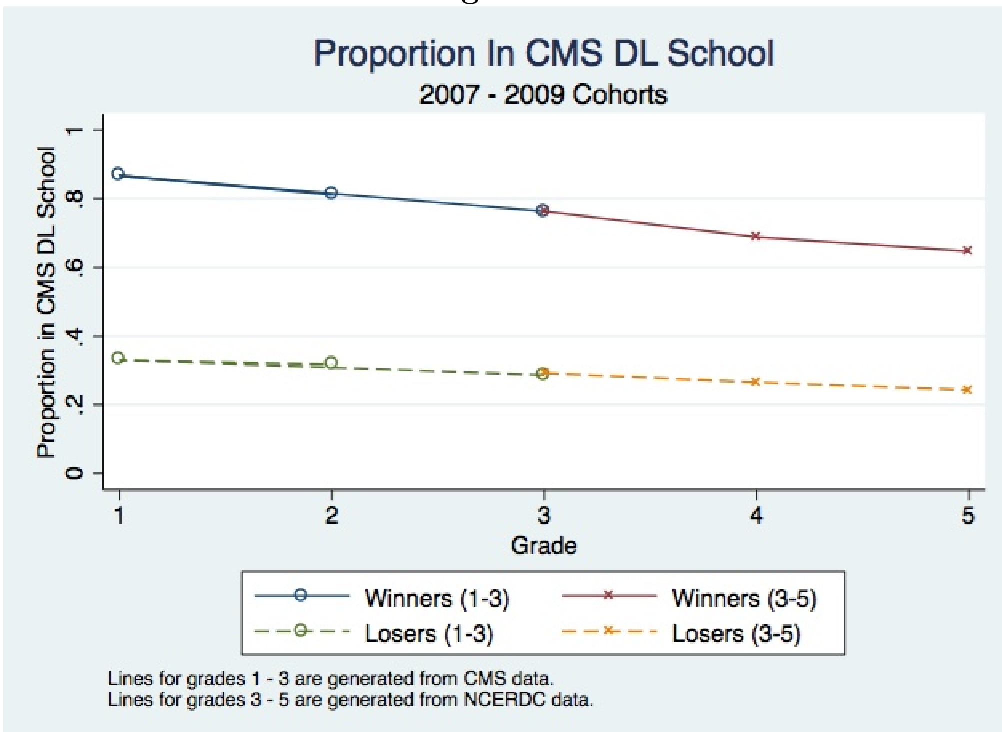

Table A1: Impact of Dual Language Education - Constant Effect

Panel A: English Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.492***	0.209*		0.120	
			(0.053)	(0.115)		(0.077)	
Attended (K/First)	-0.016	-0.127*			0.426*		0.245
	(0.100)	(0.063)			(0.231)		(0.155)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	1,472	1,472	1,472	1,472	1,472	1,472	1,472
Number of lotfe	44	44	44	44	44	44	44

Panel B: ESL/LEP Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.653***	0.296**		0.226	
			(0.063)	(0.136)		(0.138)	
Attended (K/First)	0.266**	0.348***			0.453**		0.346*
	(0.111)	(0.109)			(0.199)		(0.193)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	809	809	809	809	809	809	809
Number of Clusters	36	36	36	36	36	36	36

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1
*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of exam, year of exam, and neighborhood school fixed effects. Attendance is measured by whether the student attended a DL school in kindergarten or first grade. The treatment and attendance variables are not interacted with years of treatment in this specification. Standard errors are clustered by lottery.

Table A2: Impact of Dual Language Education - 3rd Grade Attendance Measure

Panel A: English Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.293***	0.209*		0.120	
			(0.062)	(0.115)		(0.077)	
Attended (3rd)	0.043	-0.012			0.715*		0.411
	(0.142)	(0.094)			(0.414)		(0.274)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	1,472	1,472	1,472	1,472	1,472	1,472	1,472
Number of lotfe	44	44	44	44	44	44	44

Panel B: ESL/LEP Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.560***	0.296**		0.226	
			(0.067)	(0.136)		(0.138)	
Attended (3rd)	0.278**	0.347**			0.528**		0.403*
	(0.136)	(0.163)			(0.248)		(0.233)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	809	809	809	809	809	809	809
Number of clusters	36	36	36	36	36	36	36

Robust standard errors in parentheses

***p<0.01, **p<0.05, *p<0.1

*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of exam, year of exam, and neighborhood school fixed effects. Attendance is measured by whether the student attended a DL school in third grade. The treatment and attendance variables are not interacted with years of treatment in this specification. Standard errors are clustered by lottery.

Table A3: Grades Three Through Five Only

Panel A: English Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.503***	0.034		0.023*	
			(0.054)	(0.024)		(0.014)	
Attend DL School	-0.001	-0.023			0.068		0.046*
	(0.022)	(0.013)			(0.045)		(0.027)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	1,172	1,172	1,172	1,172	1,172	1,172	1,172
Number of Clusters	44	44	44	44	44	44	44

Panel B: ESL/LEP Sample

	OLS			Math		Reading	
	Math	Reading	First Stage	ITT	LATE	ITT	LATE
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Won First Choice			0.652***	0.055*		0.028	
			(0.061)	(0.031)		(0.030)	
Attend DL School	0.049**	0.058**			0.084*		0.043
	(0.023)	(0.022)			(0.047)		(0.043)
Neighborhood School FE	X	X	X	X	X	X	X
Observations	623	623	623	623	623	623	623
Number of Clusters	36	36	36	36	36	36	36

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1
*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of exam, year of exam, and neighborhood school fixed effects. Attendance is measured by whether the student attended a DL school in kindergarten or first grade and interacted with years of treatment (grade plus one). Standard errors are clustered by lottery.

Table A4: Cohort Interactions

	English Sample		ESL/LEP Sample	
	Math	Reading	Math	Reading
	[1]	[2]	[3]	[4]
Attend DL School				
2007 Cohort	0.401**	0.200*	0.105***	0.133***
	(0.157)	(0.116)	(0.038)	(0.031)
2008 Cohort	-0.009	-0.014	0.048	-0.012
	(0.035)	(0.030)	(0.063)	(0.028)
2009 Cohort	0.074	0.132**	0.032	0.045
	(0.109)	(0.058)	(0.050)	(0.086)
2010 Cohort	0.151**	0.153***	0.286	0.239
	(0.069)	(0.040)	(0.197)	(0.166)
2011 Cohort	0.131***	0.029	0.067	0.061
	(0.044)	(0.034)	(0.086)	(0.096)
Neighborhood School FE	X	X	X	X
Observations	1,471	1,471	809	809
Number of Lottery FE	44	44	36	36

Robust standard errors in parentheses
***p<0.01, **p<0.05, *p<0.1
*Notes: Each regression includes lottery fixed effects (priority-year-program) as well as controls for female, race, frpl-year, exceptionality, grade of exam, year of exam, and neighborhood school fixed effects. Reported coefficients are on interactions between the attendance variable and cohort. Attendance is measured by whether the student attended a DL school in kindergarten or first grade and interacted with years of treatment (grade plus one). Standard errors are clustered by lottery.

Table A5: Attrition and Weighting (Panel)

Panel A: Summary of Probabilities of Testing

	Full Sample		**English Sample**		**ESL/LEP Sample**	
	Winners	Losers	Winners	Losers	Winners	Losers
	[1]	[2]	[3]	[4]	[5]	[6]
Average Pr(Test)	0.853	0.832	0.843	0.825	0.878	0.841
SD Pr(Test)	0.022	0.029	0.032	0.047	0.035	0.045
APE	0.019		0.011		0.027	
(SE)	(0.029)		(0.033)		(0.038)	
N	1532	1433	1105	816	427	617

Panel B: Non-Random Attrition

	Coefficient on Indicator for Winning		
	Full Sample	English	ESL/LEP
	[1]	[2]	[3]
No Controls	0.021	0.018	0.037
(SE)	(0.022)	(0.033)	(0.032)
Controls + Lottery FE	0.021	-0.009	0.049
(SE)	(0.031)	(0.038)	(0.038)
+ Neighborhood School FE	0.030	-0.003	0.057
(SE)	(0.032)	(0.046)	(0.042)
N	2965	1921	1044

*Notes: Panel A summarizes estimated probabilites of having test scores available. This table uses an expanded dataset, relative to Table 5, where each student has an observation for each grade that they could have tested in if they passed each grade. The estimates are based on logit regressions including gender, race, frpl-year, and an indicator for winning the lottery. Panel B shows estimated coefficients from OLS regressions of having at least one set of test scores available on winning the lottery. The baseline controls are gender, race, and frpl-year. The second set of OLS estimates also conditional on the lottery fixed effects, and the third set include neighborhood school fixed effects.

B CMS Lottery Details

Every student enrolled in Charlotte-Mecklenburg School District is assigned to a neighborhood school based on geographic zones. The district uses a school choice lottery to allocate seats for students who wish to opt out of their neighborhood school. The empirical strategy used in this paper makes use of exogenous variation created from oversubscribed lotteries, so it is useful to describe how the lottery operates and why it facilitates the identification of treatment effects. This section provides details on the lottery.

B.1 Magnet Programs and Priority Groups

All CMS students can submit up to three programs in order of preference through a centralized lottery. All students with an older sibling in a school are guaranteed a seat in that school by making it their first choice.[41] Then non-guaranteed seats are assigned in three rounds. In the first round, only first choices are considered. If there are fewer applicants than seats available to a given program, then all of the applicants to that program will be assigned to their first choice. Identification comes from comparing winners and losers from the same lottery, so estimates are driven by oversubscribed lotteries. When the number of applicants is greater than the number of available seats (the choice is oversubscribed), seats are awarded quasi-randomly. Seat assignment is not completely random, because the probability of winning for a particular student depends on the priority group that the student is assigned to. Priority groups refer to sets of students that meet (or do not meet) some pre-specified criteria. In CMS, over the sample period they are based on geographic location and whether the student's neighborhood school is a Title I choice school.

With that in mind, the district gives priority with a couple of apparent goals. First, they care about transportation costs and allowing students to attend schools that are close to home. Students who live within close proximity to a full magnet school are given priority.

[41] Students who meet admission criteria and have a twin or older sibling assigned to a magnet program receive guaranteed admission to that program. The applying student must specifiy it as their first choice in order to be guaranteed admission through sibling preference. The sibling guarantee requires that the students have the same residence and at least one common parent or guardian.

In addition, the district is split into four geographic zones. Magnet schools offer transportation to at least one, and up to four of the zones, leading the zones to be referred to as transportation zones. Students who live in a zone served by a magnet are given priority for admission to that school over students who live in a zone that is not served by that school. Students outside of the zone can still apply, but living outside of the school's transportation zone means they have a lower probability of winning, all else equal. They are also required to provide their own transportation. The district also cares about equity. They show this by offering priority to students who are assigned to Title I *choice* schools. Title I schools are those with a high percentage of students eligible for free and reduced price lunch (FRPL). A Title I school becomes a Title I *choice* school if they fail to meet adequate yearly progress in the same subject for two consecutive years. No Child Left Behind (NCLB) requires that the district allow students assigned to Title I choice schools the opportunity to attend a non-Title I choice school, but it does not require the district to allow students to choose the school they are offered. In fact, they could be offered a school that they did not apply for in the lottery.

Assigning students to priority groups alters the probabilities of winning, and means that assignment is not unconditionally random. I use lottery (program of application by year by priority group) fixed effects to exploit the fact that winners should be randomly chosen within these groups. In addition to priority groups, all applicants are ordered based on randomly assigned numbers. When a choice is oversubscribed, the combination of priority groups and randomly assigned numbers determine who wins the lottery. The next subsection discusses the priority groups, and gives more detail on how lottery winners are determined during and after the first round.

B.1.1 Priority Groups

Seats are allocated based on priority group and lottery number. The top priority for applicants to full magnet schools in CMS is given to students who live within one-third mile of the school, but only twenty percent of seats can be assigned through that priority.[42] For example, if there are ten seats available to a specific full magnet school and more than two applicants live within one-third mile of the school, then the students with the first two numbers win under that priority. Then they move to the second priority group, students with Title I choice neighborhood schools.[43] Only ten percent of available seats can be assigned through this priority. Continuing with the example, the student with the first number who meets the second priority is assigned a seat, but the rest of the students assigned to Title I choice schools remain unassigned. Finally, they move to the third priority, all students who live in transportation zones served by the magnet school.[44] There is no limit on the number of seats assigned through this priority, so in this example, students with the next seven numbers who live in the transportation zone are admitted. The last priority is for students from transportation zones not served by the magnet school.[45] In this example, if more than two students meet priority one, then that priority group is oversubscribed. The identification strategy relies on comparing students who met a specific priority and won with students who met that priority and did not win. Similarly, if more than one student meets the second priority, then that lottery is oversubscribed as well. Finally, if more than seven students meet priority three, then that lottery is oversubscribed. In such a case, students from all three of those priority groups contribute to the estimates. In contrast, consider what happens to the students in the last priority group, those from outside of the transportation zone. Since no students in the last priority group won a seat, those students do not directly contribute to the estimates.

After going through all first choices, second choices are considered. If a student's second choice is already full from the first round of assignments, then they remain unassigned in the second round. Then third choices are considered. All students are assigned to a default

[42]Students with this priority are still subject to the lottery if demand from this priority exceeds 20% of seats available. The area can be extended beyond 1/3 of a mile by the superintendent if the number of students enrolled meeting this criteria for a specific grade is less than 15.

[43]The first seats awarded are for students who qualify for FRPL and those below their grade level in reading. For kindergarten students, below grade level in reading is defined as having a personalized education plan.

[44]This priority first limits the number of seats from any particular neighborhood school assignment zone to be proportional to the potential number of applicants to the school. Then priority opens up to all students in the transportation zone. This restriction does not seem to be practically important.

[45]Applicants from outside of the transportation zone must provide their own transportation.

neighborhood school based on pre-determined geographic zones if not otherwise assigned in the lottery. Since the lottery considers student choices in order, students are most likely to win a choice by picking it first, and more seats are awarded in the first round than in the second or third. In the following analysis I restrict to students who made a dual language school their first choice. The treatment assignment variable is a dummy variable for winning their first choice, which should be random within lottery.

B.1.2 Creating Lottery Fixed Effects

Although lottery fixed effects are not explicitly given in the data, I use available information to construct fixed effects. The data contain up to three choices for every student in order of preference, as well as sibling placement, Title I choice placement, FRPL status, and transportation zone.[46] I start with the sample of all applicants without a guaranteed seat and proceed in the following way to generate lottery fixed effects.

1. Proxy Title I choice school using whether or not any student from their neighborhood school was placed under the Title I choice option that year.

2. Generate priority groups using FRPL, transportation zone, and Title I choice proxy.

3. Lottery fixed effects are priority-year-program of application combinations.

Since the lottery fixed effects are generated, they are a proxy to the true lottery fixed effects. The assignment, conditional on lottery, provides the exogenous variation used to estimate causal effects.

[46] CMS stopped reporting FRPL after 2010. For 2011 I proxy for FRPL at the time of application using FRPL from the NCERDC data.