ABSTRACT

Title of dissertation: ESSAYS ON THE ECONOMICS OF EDUCATION

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This dissertation explores several facets of the economics of education. The first chapter explores the impact of intradistrict public school choice on the relationship between house prices and elementary school test scores. Previous research has established a positive relationship between house prices and school quality in settings where students do not have opportunities for intra-district public school choice. I examine a program of intra-district school choice in Jefferson County, Kentucky. Using a boundary regression discontinuity approach, I estimate that the presence of school choice weakens the relationship between house prices and local school test scores. My estimates suggest that a five percent increase in a local school's test scores would lead to no more than two to three tenths of a percent increase in house prices. This response is concentrated in areas where houses are farther from the schools available in their choice set. This chapter also explores parents' preferences in a case where intradistrict school choice is available. I find that parents care about

both their proximity to schools and the test scores of schools.

In chapter 2, I investigate the impact of high school music classes on student academic achievement. Other researchers have documented a positive correlation between participation in music and academic achievement. However, there is a strong possibility that this correlation is driven by selection into music. Using propensity score matching, I estimate the causal impact of high school music classes on several academic outcomes. The results indicate that taking at least one music class in high school leads to increases in enrollment at a postsecondary school, increases in enrollment at a four-year college, increases in high school test scores, and small increases in students' high school academic GPA. The largest effects are found for students who participate in high school band.

ESSAYS ON THE ECONOMICS OF EDUCATION

by

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Dedication

I dedicate this dissertation to my parents, Keith and Gail Moody. Thanks for always believing in me.

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I owe my gratitude to all the people who have made this thesis possible. It could not have been done without the help of many individuals.

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Chapter 1: Introduction

The topic of education has been a part of many areas of research in economics for many years. In the most traditional sense, economists measure education by years of schooling. However, this measurement overlooks all of the complex intricacies of education.

Different schools offer a different quality of education. Different classes lead to different experiences. Education is also directly tied to non-educational activities and decisions. Many schools offer extracurricular activities, which add another component of complexity to the idea of what education is. Additionally, educational choices can impact household decisions, such as where to live.

This dissertation explores several facets of the economics of education. The second chapter explores the impact of intradistrict public school choice on the relationship between house prices and elementary school test scores. Research has established that, in the absence of school choice, house prices are very responsive to school test scores. Because one's address typically determines one's public school, the choice of where to live also typically determines which school one's children will attend. Then, it makes sense that parents will be willing to pay a premium in order to send their child to a better school.

In chapter 2, I examine how the relationship between house prices and elementary school test scores are affected by the way that students are assigned to schools. In Jefferson County, Kentucky, elementary schools students are assigned to a cluster of schools and are not required to attend their local school. Students and parents may rank up to four elementary schools that are within their cluster. The school district then uses these rankings to assign students to schools.

In my main empirical approach, I explore the relationship between house prices and school test scores in Jefferson County, KY using a boundary fixed effects approach that has been widely used in the literature. This strategy is advantageous compared to a traditional Ordinary Least Squares approach because it allows me to control for unobserved neighborhood characteristics shared by houses near the same catchment-area boundary. The first step in the analysis is to restrict the sample to houses very close to schools' catchment area boundaries. In this way, I can restrict comparison to houses that are geographically close together. Houses that are geographically close should share similar neighborhood characteristics. Therefore, conditional on house characteristics, I can assume that the only difference between houses on the opposite side of a shared boundary is the quality of the schools available to each house. Then, by including a set of boundary fixed effects, I can control for unobserved neighborhood characteristics shared by houses near the same boundary.

The results of this approach demonstrate a very small relationship between local school test scores and house prices in Jefferson County. The estimated coefficient on school test scores is about ten times smaller than what has been estimated in school districts that do not offer any type of school choice.

As an extension of the boundary fixed effects approach, I also explore the impact of cluster schools' characteristics on house prices. This exploration is an important contribution to the literature, because it is not theoretically obvious what parents prefer in their child's school. I investigate whether house prices are responsive to changes in the test scores of non-local cluster schools. I also explore the importance of schools' proximity to one's house.

This results indicate that parents' preferences in Jefferson County are difficult to understand. The system of school choice is complicated, so it is not surprising that the results are not definitive. The results suggest that parents care about the proximity to the school that their child will attend and that parents also care some about the quality of the cluster schools.

Using these results, I further extend my analysis to explore how accessibility to school choice affects the relationship between house prices and school test scores. Because of the design of the elementary choice program, the accessibility of school choice varies throughout the district. For example, in areas where the available set of choice schools are, on average, geographically closer to one's home, school choice is a more accessible option than when the choice set of schools are, on average, far from one's home.

I examine how the relationship between house prices and school quality differs for houses that have access to many schools versus houses that do not. My results indicate that in areas where houses have more access to choice, house prices are less responsive to changes in local school test scores. The results are more ambiguous for whether the relationship between house prices and the quality of available schools is affected by accessibility to school choice.

In the third chapter, I study the impact of high school music classes on students' academic outcomes. This topic is of interest because there has been much debate in recent years over the importance of music education. Some individuals argue that music education can improve performance in other academic areas, such as math and science. Other individuals argue that time spent in music education takes away from time that could be spent on more math and science courses. I examine whether participation in high school music classes leads to changes in ACT and SAT test score, academic GPA, whether a student attends college, and whether a student drops out.

To empirically investigate these effects, I utilize two empirical approaches: OLS with school fixed effects and propensity score matching. Each approach has advantages and disadvantages. The advantage of OLS with school fixed effects is the ability to include fixed effects. The disadvantages of OLS are its restrictive functional form and the fact that it generally restricts the average treatment effect on the treatment to be equal to the average treatment effect (though this may not be true once fixed effects are estimated). Propensity score matching does not restrict the average treatment effect on the treated to be equal to the average treatment effect, and it allows for a less restrictive functional form. However, propensity score matching is unable to accomodate school fixed effects; therefore, the matching estimates may be biased upward.

The data used is the Education Longitudinal Study of 2002 from the National Center for Education Statistics, which surveys approximately 15,000 high school students and follows these student for several years. These data are ideal for propensity score matching because there are many variables that affect both the likelihood of taking a music class and the outcome variables.

The results demonstrate that music classes have large effects on academic outcomes. I estimate that taking at least one music class in high school leads to .8 percentage point decrease in dropping out of high school, which is a fifteen percent decline. Taking at least one music class also leads to large increases in enrollment at a postsecondary school, increases in enrollment at a four-year college, and small increases in students' academic GPA. Taking at least one music class also leads to approximately two percent improvement in ACT or SAT test scores.

The largest effects are found for students who participate in high school band. High school band students see a five to eight percentage point increase in post-secondary school attendance and an approximately five percentage point increase in four-year college attendance. Band students also see statistically significant increases in SAT and ACT scores, scoring approximately three percent higher on both tests. Participating in high school band also leads to increases of about two tenths of a points in students' academic GPA.

Chapter 2: The Impact of Intradistrict School Choice on the Relationship between House Prices and Elementary School Quality

2.1 Introduction

In recent years, a large body of research has estimated the impact of school choice on academic outcomes. However, much less is known about the impact of school choice on non-academic outcomes. One such important outcome is house prices. In this paper, I investigate the effect of intradistrict public school choice on the relationship between school quality and house prices.

Previous research has established a positive relationship between house prices and school quality in school districts where there is no school choice (Black (1999); Downes and Zabel (2002); Fack and Grenet (2010); Figlio and Lucas (2004); Kane et al. (2006) are a few examples[1]). The intuition behind this relationship is that local school quality is an important determinant of one's residential location decision. Parents prefer to live in a location that offers a higher quality education for their children. In a school district where one's address determines one's school, home buyers bid up the price of a house near a better school. Thus, the desirability of the

[1] For an extensive review of the literature on the relationship betweeen house prices and school quality, see Black and Machin (2010).)

local school is capitalized into house prices.

In this chapter, I investigate how the relationship between house prices and school quality is affected by a change in the relationship between home address and school assignment. Specifically, if parents are allowed to choose from among several public schools within a district, instead of being assigned to a single school based on their address, how will this change the relationship between house prices and school quality?

Additionally, this chapter explores the parents' preferences in the presence of intadistrict school choice. When parents can choose a school instead of being assigned to one, I expect that additional factors beyond local school quality will be capitalized in house prices. Parents presumably place value on the quality of the available non-local schools. They also may place value on the distance of their house to potential schools. The extent to which parents value each of these factors is not obvious. I will use several different measures of quality and distance to explore parents' preferences.

In order to explore these questions, I examine an extensive program of public school choice in Jefferson County, Kentucky. The Jefferson County Public School District (JCPS) contains the city of Louisville and the entirety of Jefferson County. It is the 28th largest school district in the United States, with approximately 98,000 students (Snyder and Dillow, 2011)[2]. As I will describe in detail later, elementary, middle, and high school students all have the option of exercising school choice. I will focus on elementary schools for two reasons. First, there are many more elementary schools than middle or high schools. Second, the choice program at the elementary level is structured slightly differently from the choice program in middle

[2]This statistic is based on Fall 2008 enrollment figures. JCPS enrollment numbers are comparable to Detroit City Public Schools (97,000) and Baltimore County Public Schools (103,000).

and high schools and is more suitable for my empirical approach.

I hypothesize that house prices will be less responsive to local school quality (as measured by test scores) when students have the opportunity to exercise choice, compared to a situation where students cannot exercise choice. In a scenario where there is school choice, parents are no longer required to live in a specific geographical area in order to send their children to their preferred school. As a consequence, I expect that parents' willingness-to-pay for a home near a high quality local school will be less than their willingness-to-pay if the district did not offer choice.

As a practical matter, the hypothesis that school choice will weaken the relationship between house prices and school quality is difficult to test. First, I must address the endogeneity of house characteristics and school quality. In a traditional hedonic price regression, a consumer's choice over a good such as housing amounts to choosing the preferred bundle of house and neighborhood characteristics (e.g. square footage, income of neighbors, school quality). In the hedonic regression, school quality may be correlated with unobserved characteristics of a house's neighborhood. For example, better schools tend to be located in wealthier neighborhoods. If individuals are willing to pay a premium not just for a better school but also to live in a neighborhood with these unobserved characteristics, then this will bias the effect of school quality on house prices.

To address this issue, I adopt a regression discontinuity approach developed by Black (1999). This approach has been utilized extensively in the literature (See Black and Machin (2010) for a review of the literature.). The first step in the analysis is to restrict the sample to houses very close to schools' catchment area boundaries. In this way, I can restrict comparison to houses that are geographically close together. Houses that are geographically close should share similar neighborhood characteristics. Therefore, conditional on house characteristics, I can assume that the only difference between houses on opposite sides of a shared boundary is the quality of the schools available to each house. Then, by including a set of boundary fixed effects, I can control for unobserved neighborhood characteristics shared by houses near the same boundary.[3]

In addition to the endogeneity of school quality, another empirical difficulty will be how to measure school choice. The school choice program is in place for the entirety of the time period for which I have data; therefore, I need to use another source of variation in the availability of school choice within the district. Because of the design of the elementary choice program, the accessibility of school choice varies throughout the district, causing some areas to have more choice and some to have less choice. For example, in areas where the available set of school choices are, on average, geographically closer to one's home, school choice is a more accessible option than when the school choices are, on average, farther from one's home.

In areas where choice is more accessible, I expect house prices to be less responsive to local school quality changes than in previous studies, because parents can more easily send their child to an alternative school. My results support this hypothesis. The coefficient estimate of the impact of local school quality changes on house prices is about 2.5 times smaller in areas with more choice compared to areas with less choice.

Additionally, I expect house prices to be responsive to changes in other school quality measures (in addition to one's local school quality), once school choice is available. I find that when a house is, on average, far from any available school choices, house prices are more responsive to changes in local school quality and less responsive to changes in cluster school quality. This finding suggests that par-

[3] I will eliminate boundaries where this assumption is unlikely to hold, such as school boundaries that are also natural boundaries, like rivers or major highways.

ents value a smaller distance from their house to their child's school. I also find that when a house is far from the best schools, house prices are more responsive to changes in local school quality and less responsive to changes in cluster school quality. This suggests that parents value the quality of the available schools in addition to proximity to these schools.

One point that should be kept in mind is that this school choice program is unique to Jefferson County, Kentucky. The way students are assigned to schools and the way they choose schools is complicated and unlike other school districts in the United States. For this reason, the results may not be generalizable to other school districts in the US. This is simply an exploration of the relationship between house prices and test scores within this very interesting school district.

The remainder of the chapter is structured as follows: Section 2.2 provides background on the JCPS choice program. Section 2.3 presents a conceptual framework. Section 2.4 describes the data. Sections 2.5 through 2.8 present my empirial strategies and main results. Section 2.9 contains extensions and robustness checks. Section 2.10 concludes.

2.2 Background

The Jefferson County Public School District is the 28th largest school district in the United States. To my knowledge, it is one of the few school districts in the United States that offer school choice to all of its students.[4] The type of school choice available is intradistrict public school choice, such that students may choose from among public schools within their district.[5]

[4]Chicago Public Schools has a system of public school choice that is most comparable to that in Jefferson County.

[5]Other types of school choice are interdistrict, where some students may have the option of attending a school that is not in their school district. The term school choice can also refer to

In 1992, JCPS introduced the school choice plan that I study.[6] At the elementary level, students may apply to any elementary school that is within their cluster. A cluster is defined as a group of 5 to 12 elementary schools, where a student who lives in the catchment area of any one of the cluster schools may apply to attend any school that is in the same cluster. There are a total of twelve clusters in JCPS. Figures 2.1 and 2.2 display the division of clusters, with 6 clusters shown per map to make the distinctions between clusters more visible.

One will notice that the clusters are mostly non-contiguous. Cluster 9 is the only completely contiguous cluster within the county. By contiguous, I mean that every school within the cluster is bordered by at least one other school in the same cluster, and there are no geographic holes in the cluster. In this way, Cluster 9 is very different from the other clusters. Many school catchment areas do not share any part of their border with a school area in the same cluster (e.g. most schools in cluster 11). The clusters were structured in this way in order to have a more diverse population of students in each cluster.[7] Though the cluster schools can be far apart, each student is guaranteed free transportation to any school within his or her cluster.

Students may apply to any of the schools within their assigned cluster. The process is illustrated in Figure 2.3 and works as follows. First, the student is tentatively assigned to a residence-based school based on the student's home address.

the availability of charter schools or private schools. Jefferson County does not have any charter schools. It also does not allow for interdistrict school choice.

[6]The school choice program was a modification of a busing system that was implemented in 1975 as a result of a court order to desegregate (Kleber, 2001) The school choice plan that I study was again modified in 2010.

[7]Because JCPS includes both the city of Louisville and the rest of Jefferson County, the district varies from very urban to very rural. Like many cities in the US, the inner-city area is mostly black, and the county becomes more white as one moves away from the city center. In order to increase racial diversity, the school district requires that at least one school from a mostly black neighborhood be included in every cluster. Thus, most clusters contain both urban and rural schools.

I call this the local school. In Figure 2.3, a student who lives within the hatched area is assigned to school A as his or her local school school. The hatched area is what is typically referred to as the catchment area for a school. In a district where there is no school choice, students who live within the catchment area of a school are definitively assigned to that school. Thus, the first step of the assignment process in JCPS is identical to the process for assigning students to a residence-based school in a district where there is no choice, except that the assignment in JCPS is not mandatory, nor does the assignment guarantee the student a spot at the local school.

As a result of the residence-based school assignment, a student also knows the cluster to which he or she belongs. All school areas that are shaded gray in Figure 2.3 belong to the same cluster as the hatched school. The student who lives in the hatched area may apply to school A and/or any of the schools in the gray areas. Similarly, if a student lives in a gray area, he or she may apply to school A or any other schools in the gray area.

To apply to a school, a student must submit a choice application. Approximately fifty percent of JCPS students submit such an application. On the application, students may rank up to four school choices within their cluster.[8] The school district reviews the applications and assigns each student to a school.

The assignment process is based soley on three criteria: the address of the student, the student's race, and the school's capacity constraints. Students who

[8]Transfers between clusters are allowed. However, transfers are considered on a case by case basis, and parents must provide documentation for why they would like their child to attend a school in a different cluster. The most common reasons for transfers are if a student's sibling attends a school in a different cluster, a student having behavioral problems that they believe can be addressed in a different cluster, or if a student needs special attention (such as a learning disability) and there is a school in a different cluster better suited to handle that student. Approximately 10% of students attend a cluster that is not their assigned cluster. This information was obtained by speaking directly with the school district.

live in the catchment area of a school are given first priority to attend that school. Students who do not submit a choice appliation are assumed to prefer the local school but may be sent to other schools if the local school faces capacity constraints. If there are more applicants than space permits, a student will be somewhat randomly selected to attend the school (I describe this process with two scenarios below). Additionally, schools must have a demographic make-up of at least fifteen percent, but no more than fifty percent, black students. Where the school lies between those racial guidelines is up to the principal of the school. The applications are divided into four groups: black males, black females, white males, and white females. A computer randomly sorts each list. The principal then selects a number of students from each list that satisfies the racial guidelines and the capacity constraints. The application data are discarded each year after the assignment process is completed and thus are not available to me. What the reader should take away from this process is that applications to schools are not merit-based. Additionally, a student has the best chance of getting a spot at a school if he or she lives in the catchment area of that school.

To illustrate this point, I describe two scenarios below to illustrate the ways in which students may be assigned to a school such that the racial and capacity constraints are satisfied.

Scenario 1

Suppose there are 100 seats available at new school A. 200 students apply. Seventy-five of these students are from school A's assignment area. Of these students 25 are black, and 50 are white. Of the 125 students who applied from outside A's assignment area, 55 are black, and 70 are white. School A will admit all 75 of the students from its assignment area. It is then up to the principal to fill the remaining spots. The students who applied from outside A's assignment area will be divided into four groups: white males, white females, black males, and black females. A computer will place the names within each group in a random order. The principal will then choose how many students to accept from each group. While he or she may choose the race and gender of the remaining students, the principal must start at the top of each list and work down, such that selection within each list is random.

Even in this simple case, the eventual outcome is uncertain. Because the school is already 25% black, the principal could potentially choose only white students to fill the remaining seats. Following the same argument, the principal could also choose only black students without violating the racial guidelines. According to anecdotal evidence, the choice is up to the preferences of the principal and the needs of other schools within the cluster. If several other schools have too many black applicants, for instance, then it is likely that school A will accept more black students and send the remaining white applicants to other schools in the cluster. See the next scenario for this possibility.

Scenario 2

Suppose there are 100 seats available at new school B. Two-hundred students apply. 150 of these students are from school B's assignment area. Of these students 125 are black, and 25 are white. Of the 50 students who applied from outside B's assignment area, 40 are black, and 10 are white. The school must be no more than 50% black. Therefore, all white applicants will be accepted. But this brings the school to only 35% white. Additional white applicants must come from students who listed the school as second, third, or fourth place, or from students who did not send in a choice application at all. Relating this scenario to scenario 1, some white students who listed school A as a first choice may be sent to school B in order to increase the percentage or white students at school B.

Additional scenarios could be discussed, but one can already see that the assignment process has the potential to be complex. The racial guidelines, combined with priority to local residents, makes assignment and application strategic in an unpredictable way to the econometrician. Because my unit of observation is the house, I do not know the race of any household members, nor do I know how they ranked schools in the application process. What I do know is that a student has the best chance of getting a spot at a school if he or she lives in the catchment area of that school. However, because of the choice options, parents do not need to live near their preferred school in order to send their child to that school. For this reason, I hypothesize that the prices for houses near the best schools will not be bid as high as they would have been in the absence of school choice.

In the next section, I describe my conceptual framework for examining the relationship between local and/or neighborhood school quality and house prices.

2.3 Conceptual Framework

There is a large body of research examining the relationship between house prices and school quality. My analysis is based on the hedonic price model developed by Rosen (1974). In this model, the price of a house can be described as a function of the house's observable characteristics (number of bedrooms, square footage, etc.) and its neighborhood characteristics. For instance, if a house is located near a very nice public park, we might expect to see a higher price for that house, which would reflect the homeowner's value of the park. If we assume that the house and neighborhood characteristics enter the equation linearly, we can write this model as:

$$ln(price)_{i,n} = \beta_0 + X_i\beta_1 + N_n\beta_2 + \epsilon_{i,n}, \tag{2.1}$$

where X_i is a vector of house characteristics, which includes school quality, and N_n is a vector of neighborhood characteristics. The coefficients on these vectors describe a household's marginal willingness-to-pay for each characteristic.

Given that we expect that parents prefer higher quality education for their children, we would expect that houses located in the catchment area of better schools to fetch a higher price than houses located in the catchment area of lesser quality schools. The literature has found that when school quality is measured using test scores, houses in the catchment area of schools with higher test scores sell for higher prices (Black (1999); Downes and Zabel (2002); Fack and Grenet (2010); Kane et al. (2006)). Black (1999) and Fack and Grenet (2010) find that a five percent increase in test scores leads to an increase of house prices of between 1.5 and 2.5 percent. The smallest estimates in the literature come from Bayer et al. (2007) who find that a five percent increase in school quality leads to a one percent increase in house prices.

Parents in Jefferson County may not be willing to pay as much of a premium for living in the catchment area of a good school as they would have paid in the absence of school choice. Machin and Salvanes (2010) utilize a difference-in-differences strategy before and after the implementation of school choice reform in Norway to identify the impact of school quality on house prices. They find that the house price premium for local school quality falls by at least fifty percent when school choice is introduced.

Second, parents will now care about characteristics of many schools, not just the local school. Without school choice, parents need not care about the quality of other schools in the district. With school choice, there is a non-zero probability that a child may attend a school other than the local school. Then, even if the local school is the parents' preferred school, parents may pay more to have better alternatives to the local school as insurance against the possibility that their child may not be given a spot in the local school.

The relationship between house prices and non-local school characteristics is relatively understudied. (Machin and Salvanes (2010), Hastings et al. (2005), and Reback (2005) are the studies that I am aware of.) For this reason, it is not obvious what parents' preferences are in this case. Parents may prefer to live in a cluster where the average school quality is higher. They may prefer a cluster where one or two schools are very good, and not care about the quality of the other schools. They may be very risk averse and care about the quality of the worst possible school that their child could attend.

Alternatively, parents may care more about the distance from their house to their child's school. Hastings et al. (2005) examine the implementation of an intra-district school choice program in Mecklenburg County, North Carolina. Using parents' school choice applications, they find that parents value both quality and proximity highly. There is reason to believe that parents in Jefferson County may value proximity highly. According to records kept by JCPS, it is not uncommon for children to travel more than an hour each way between their house and their school. This is due to students attending cluster schools that is far from their homes. Knowing this and knowing the results from Hastings et al. (2005), I expect that parents may pay more to live in a cluster where the available schools are close by. I use a variety of independent variables in my analysis to investigate parents' preferences with regard to the cluster schools.

2.4 Data

The data for this chapter come from two primary sources. House price data were obtained from the Jefferson County Property Value Administrator. These data contain information about all residential arms-length sales in Jefferson County between 2000 and 2006. For each house, I know its street address, sale date, year built, number of stories, square footage, acreage, number of bathrooms, whether it has a basement, whether it has a garage, and whether it has central air-conditioning.

Table 2.1 displays summary statistics for houses in my sample. Column 1 contains all houses from all years. Column 2 restricts the sample to houses that are within 2000 feet of a school area boundary, which is a standard distance used in the literature. Column 3 uses houses that are on the “high” side of a boundary. By high, I mean that the test score of the house’s local school is higher than the test score of the local school on the opposide side of the boundary. Similary, column 4 uses houses that are on the “low” side of a boundary.

Moving from the full sample to the sample of houses near the boundary, one will notice that the overall quality of houses diminishes. Houses are slightly smaller, are on smaller lots, have few stories, etc. However, these differences are not statistically significant. The decline in quality is due to the fact that the density of housing is much lower in rural areas. I lose more of my sample in the rural areas than in the inner-city, poorer areas. This drives the average quality down.

When comparing houses on the high side of a boundary to houses on the low side of a boundary, prices are higher on the high side. However, other house characteristics are similar on both sides of the boundary.

School information was obtained from the Kentucky Department of Education school report cards and JCPS Data Books. For each school, I have information about its test scores, student demographics, attendance and disciplinary records, as well as teacher characteristics. I primarily make use of the test score information.

As is common in the literature, I use test scores as my measure of school quality. Each year, in January or February, JCPS publishes a report card for each school. During the time period I study, these report cards were mailed annually to every student's home. The report card displays the results from several Kentucky testing measures, a national test, and other school characteristics such as teachers' education levels and attendance rates. In addition to these report cards, JCPS publishes an annual Data Book, which contains information about all schools. One can view the test scores of all schools in one central location. Anecdoctal evidence suggests that real estate agents utilize both the report cards and the Data Book when working with a home buyer. Thus, school quality measures are easily available and are provided to parents searching for homes.

My primary measure of school quality is the school's average percentile ranking on the nationally standardized Comprehensive Test of Basic Skills (CTBS/5). All third graders in KY took the CTBS5 in each year in my sample. The schools report scores in reading, math, and language arts. My regressions primarily make use of the composite score, since results from individual tests are not qualitatively different.[9] Table 2.2 displays school summary statistics. Columns 1 through 12 correspond to cluster 1 through 12. Column 13 contains the summary statistics for all clusters.

There is some variation in the average quality of the clusters; however, most

[9]While test scores for Kentucky tests are also available, I choose not to use these scores for a variety of reasons. First, the test itself has changed many times. Second, there is anecdotal evidence that many individuals do not support the Kentucky testing system. Third, the tests consist primarily of open-response type questions, which makes the scoring system subjective and also makes year-to-year comparisons quite difficult.

clusters are close to the mean test score percentile of around fifty-one percent. The last row shows the average across all years of the test score of the best school in the cluster. There is more variation in the test score of the best school than in the average of all cluster schools. On average, schools with higher test scores tend to have teachers with more years of experience and more teachers with master's degrees. Schools with higher test scores also have lower per-pupil spending. The school tax rate in Jefferson County is a flat rate throughout the county, and the money from the entire county is pooled before being distributed to each school. JCPS tends to increase spending in schools that are not performing as well.

I combine school and house data using ArcGIS, a geographic information system for working with maps and geographic information. JCPS provided me with the exact boundaries for each elementary school area. Additionally, I have the exact street address for every house. By geocoding the house addresses, I am able to assign each house to a residential-area school and also to a cluster. I can also calculate the distance from each house to every school in its cluster.

I make further use of ArcGIS when restricting my sample to houses near boundaries. I am able to calculate the distance from each house to its nearest boundary, where a boundary is a segment of a total school area boundary that separates only two schools. I then draw a buffer zone 2000 feet around each boundary.[10] I select only houses within this buffer zone for my analysis. Houses that are near two boundaries are assigned to the closest boundary.[11] Houses near boundaries that are geographic dividers, such as rivers or parks, are dropped from the sample.

[10] 2000 feet is the standard distance used in the literature. I also do the analysis using a distance of 1000 feet. The results are qualitatively similar.

[11] Excluding these houses does little to affect the estimates. Because catchment areas can be small within the city, there did not seem to be a standard way to choose a distance within which houses would be excluded from the sample. The previous literature has also assigned houses to their closest boundary when a house is close to two boundaries.

2.5 Estimating the Impact of Local School Quality on House Prices

To begin, I utilize an empirical approach first used by Black (1999). This approach has been widely used in the literature that estimates the impact of school quality on house prices. Without boundary fixed effects, the estimating equation in this case is

$$ln(price)_{i,s,n,t} = \alpha + \beta Quality_{s,t} + X_i\delta + Z_n\gamma + W_{s,t}\phi + \lambda_t + \epsilon_{i,s,n,t}, \tag{2.2}$$

where $ln(price)_{i,s,n,t}$ is the natural log of the price per square foot of house i assigned to local school s in neighborhood n at time t, $Quality_{s,t}$ is some measure of school quality available to house i, X_i is a vector of house characteristics (number of stories, year built, square footage, etc), Z_n is a vector of neighborhood characteristics (racial composition, percent below the poverty line, percent homeowners, etc), and $W_{s,t}$, a vector of school characteristics (such as per-pupil spending, student-teacher ratio, and attendance rate).

This approach will lead to biased coefficients if there are unobserved house and neighborhood characteristics that are correlated with school quality. To reduce this bias, Black (1999) proposed a boundary fixed effects approach (also referred to as a boundary discontinuity approach), which I use as my baseline empirical strategy.

First, I restrict my sample to include only houses that are within 2000 feet of a catchment-area boundary. Figure 4 displays the houses that will be selected. Houses near the same boundary should have similar neighborhood characteristics. A 2000 foot buffer zone is drawn around each boundary. Houses that lie within this buffer zone are included in my sample. In addition to restricting the sample in this way, I also estimate a full set of boundary fixed effects. The identification assumption is that the only difference between houses that share a boundary is the quality of the local school and potentially also the quality of the cluster (conditional on house characteristics). Equation 2.2 then becomes:

$$ln(price)_{i,s,b,t} = \alpha + \beta Quality_{s,t} + X_i\delta + \theta_b + \lambda_t + \epsilon_{i,s,b,t}. \tag{2.3}$$

θ_b is a vector of boundary fixed effects, which should account for any unobserved characteristics shared by houses on either side of a particular boundary. Notice that neighborhood characteristics no longer need to be included, as the key assumption for this strategy is that houses near the same boundary share the same neighborhood characteristics. I estimate equation 2.3 for houses less than 2000 feet from a school area boundary. Standard errors are clustered at the school level.[12] I expect β to be positive.

Before presenting the results, one should consider the possibility that test scores are a proxy for other school characteristics. Due to the racial capacity constraints in schools, it is possible that test scores are a proxy for other characteristics, such as the racial composition of a school. Alternatively, test scores might be a proxy for student/teacher ratio or the quality of teachers. Bayer et al. (2007) show that households prefer to self-segregate by race. Parents then may also prefer to send their child to a school where he or she is in the majority race. I do not directly test for this, but it is a possibility.

Table 2.3 presents results using the local school's CTBS total score as the independent variable. Column 1 displays the results from estimating equation 2.2 without any controls, a baseline OLS regression of price on test scores. The co-

[12]One could also cluster at the boundary level, or the cluster level. It is not obvious what the appropriate level of clustering is. The standard procedure in the literature is to cluster at the school level.

efficient on the test score implies that a five percent increase in local school test score is associated with a two percent increase in house prices. Column 2 presents the results from estimating equation 1 with controls for house characteristics and year effects, the standard hedonic model. Though the estimates from Columns 1 and 2 are biased, it is important to note that both coefficients are positive and demonstrate a strong correlation between house prices and local school test scores.

Column 3 presents results from estimating equation 2.2 on the sample of houses that are less than 2000 feet from a school area boundary. This column shows that the later results are not driven by the restricted sample. Column 4 displays my preferred estimation results. Here, I estimate equation 2.3, the boundary fixed effects approach. The coefficient on school quality is much smaller than in previous columns. Comparing column 3 to column 4, the magnitude of the coefficient falls by about eighty percent. This drop is consistent with the literature that uses a boundary fixed effects approach. Other researchers have found that the addition of boundary fixed effects causes the coefficient on school quality to fall by at least fifty percent (Bayer et al., 2007; Fack and Grenet, 2010).

Estimates obtained when the sample is restricted to houses less than 1000 feet from a catchment area boundary are qualitatively similar and are presented in Appendix Table 2.1.

A comparison of the magnitudes of my coefficient estimates to those from the literature shows that these estimates are much smaller than those from the literature that examines the responsiveness of house prices to school quality in the absence of school choice. Black (1999) found that parents are willing to pay two and a half percent more in house prices for a five percent increase in test scores. Based on a mean composite test score percentile of fifty percent, a five percent increase in the local school's composite test scores scores in Jefferson County would lead to between two and three tenths of a percent increase in house prices. These estimates are approximately ten times smaller than those found previously in the literature in settings with no school choice. This is a substantial difference.

This difference is consistent with the existing research that focuses on districts with schools choice. Reback (2005) examines the impact of the adoption of several school choice programs on house prices in Minnesota. He finds that properties appreciate significantly in school districts that students may transfer out of. In school districts that accept transfers, property values declined. Machin and Salvanes (2010) find that the house price premium for local school quality falls by at least fifty percent when school choice is introduced in a school district in Norway. Thus, both this chapter and previous research suggest that the presence of school choice doesn't just slightly diminish the effect of school test scores on house prices. The presence of school choice substantially mitigates the impact of local school quality on house prices. In Jefferson County, the impact of school test scores on house prices is very small.

These results could alternatively imply that preferences in Jefferson County are dramatically different from preferences in other school districts. Or, it might be the case that school quality is being mismeasured, because I have included only local school test scores in the analysis so far. This possibility leads to a natural follow-up. Are other schools' test scores capitalized into house prices instead of the local school's test score?

2.6 Estimating the Impact of Cluster School Quality on House Prices

In the previous section, I found that house prices respond less to local school quality in a district with school choice than they do in the absence of school choice.

I expect that characteristics of other available schools will also be capitalized into house prices. However, it is not obvious which characteristics of available schools will be important to parents and students. Will the quality of the closest schools be capitalized into house prices? Or will it be the quality of the best schools? The distance to the best schools?

Hastings et al. (2005) examine the implementation of an intra-district school choice program in Mecklenburg County, North Carolina. Using parents' school choice applications, they find that parents value both quality and proximity highly. Based on these findings, I also examine the extent to which parents value cluster school quality and proximity to cluster schools in JCPS. To estimate the relationship between house prices and these characteristics, I explore the relationship between house prices and several different measures of cluster school characteristics.

First, I regress house prices on a measure of cluster school quality. The first measure is simply an average of the CTBS composite scores of the top three highest scoring schools in one's cluster[13]:

$$\frac{(CTBS_1 + CTBS_2 + CTBS_3)}{3}$$

For the second measure, I include each of the top three cluster schools' test scores separately to allow more flexibility in the estimation. Third, I measure cluster quality as a weighted average of the CTBS scores of the top three highest scoring schools in one's cluster, where the weights are the inverse distance from the house to each school:

$$\sum_{i=1}^{3} \frac{CTBS_i * (\frac{1}{\text{Distance to } S_i})}{\sum_{i=1}^{3} \frac{1}{\text{Distance to } S_i}}$$

[13] The local school is included in the set of available cluster schools.

I expect the coefficient on each measure of cluster quality to be positive.

Table 2.4 presents results from regressing house prices on the three measures of cluster school quality. All columns include estimates of year effects and controls for house characteristics. The sample is restricted to houses less than 2000 feet from a catchment-area boundary. Columns 1-3 do not include boundary fixed effects. Columns 4-6 do include boundary fixed effects.

As expected, there is a positive relationship between average cluster school quality and house prices when fixed effects are not estimated. When fixed effects are included, the magnitudes of the coefficients drop substantially and are sometimes the wrong sign. When each school's test scores enter into the regression separately, the coefficients on the top two schools' scores are very small, and the coefficient on the first and second best schools are actually negative. Most of the effect is captured in the third best school's test score. This suggests that parents may care about the worst possible schools that their child could attend. This would make sense in Jefferson County, as clusters seem to be designed such that each cluster has at least one of the district's better schools.[14] Thus, every cluster has at least one or two good options. Where the clusters begin to differ is in how many good schools they have and how many bad schools they have. Recall that parents may rank up to four schools on their application. If parents are worried that their child may end up attending their third choice school, it makes sense for the quality of the third best school to be capitalized into house prices.

Regarding the negative coefficient on the first and second best schools, it is not clear what is driving this result. Without boundary fixed effects, the coefficients are positive. It is not until boundary fixed effects are estimated that the coefficient

[14] Technically, the clusters are designed to have several schools with different racial compositions. Because the racial composition is correlated with test score, this effectively means that each cluster has schools with varying levels of test scores.

becomes negative.

Comparing the magnitude of the coefficient on cluster schools' test scores to the coefficient on local school test score, the results are inconclusive as to whether house prices are more responsive to the quality of cluster schools or local schools. Without boundary fixed effects, the coefficient on the third best cluster school is slightly higher than the coefficient on the local school test score. However, when boundary fixed effects are included, the coefficient on local school test score is about twice this size of the coefficient on the third best cluster school. It makes sense that parents would pay more attention to the test scores of the local school, as this is the school that their child is most likely to attend.

With regard to boundary fixed effects, recall that we know that the coefficient on local school quality is biased without the inclusion of boundary fixed effects, because it is positively correlated with unobserved neighborhood characteristics. In Table 2.3, I included a full set of boundary fixed effects to account for this bias. When the current specification is estimated with a full set of controls and boundary fixed effects, the coefficient on cluster quality becomes very small, occasionally the wrong sign, and of less statistical significance. This is true for all measures of cluster quality. It is unclear exactly what is driving these results. It could be that the fixed effects are exacerbating measurement error. It may be that cluster quality is unimportant or has unexpected effects on house prices. Furthermore, it is plausible that the boundary fixed effects approach is inappropriate and/or insufficient in this context.[15]

[15] It is not obvious how cluster school quality is correlated with unobserved neighborhood quality. Because cluster schools are not necessarily near one's house, there need not be a positive correlation between one's own neighborhood quality and cluster school test scores. In fact, it could be that because better schools are located in better neighborhoods, the impact of a high-scoring cluster school's neighborhood on house prices would be negative, as one's own neighborhood becomes relatively less desirable. It is unclear how to account for this bias, and it is also likely that boundary fixed effects do not properly overcome this bias.

Even though I cannot definitively determine the extent to which cluster school quality is capitalized into house prices, as the coefficients vary in both magnitude and sign, the results provide additional support for the estimate of local school quality's impact on house prices. The estimate of the impact of local school quality on house prices is similar to what was estimated in Table 2.3 without boundary fixed effects. Estimates with the boundary fixed effects are similar to column 4 of Table 2.3.

2.7 Estimating the Importance of Proximity

Next, I estimate the importance of proximity. First, I regress house prices on the test scores of the closest cluster schools. Second, I regress house prices on both test scores and the distance from one's house to available schools. I estimate variants of the following equation:

$$ln(price)_{i,s,b,t} = \alpha + \beta_1 Q_{s,t} + \beta_2 Distance_{i,s} + \beta_3 (Q_{s,t} \times Distance_{i,s}) +$$
$$\sum_{c=1}^{3} [\phi_{1,c} Q_{c,t} + \phi_{2,c} Distance_{i,c} + \phi_{3,c}(Q_{c,t} \times Distance_{i,c})] + X_i \delta + \theta_b + \lambda_t + \epsilon_{i,s,b,t}, \tag{2.4}$$

where $Distance_{i,s}$ is the distance (in thousands of feet) from house i to the local school s, and $Distance_{i,c}$ is the distance from house i to cluster school c.

Table 2.5 presents the results from regressing house prices on the test scores of

While approximately thirty percent of students attend a school that is not their local school, those students are fairly evenly distributed amongst the cluster schools. On average, five percent of students from a given school area attend the top cluster school. Whether this is because parents did not choose this school or because the school was oversubscribed, I cannot know. Access to the school choice applications would be necessary to conduct further investigations of the relationship between cluster school quality and house prices. Unfortunately, those records are not available from the school district.

the three cluster schools closest to each house. Distance measures are not included in this preliminary regression. The magnitude of the coefficient on test scores of cluster schools is positive and statistically significant, except for the third closest cluster school.

One will also notice that when test scores of each school enter the regression equation separately, the test score of the local school (which is also typically the closest school) has the largest impact on house prices. As schools become progressively farther away, the magnitude of the coefficients declines, indicating that house prices become less responsive to changes in the school's test scores as the available school gets farther away. This suggests that both test scores and the proximity of one's available schools are important, confirming Hastings et al. (2005)'s result that proximity is an important decision when parents apply to schools.

In the next two tables, I estimate the full specification of equation (2.4). I will estimate this equation twice. The first time, I rank the cluster schools by test scores, meaning cluster school 1 will be the highest scoring cluster school, and cluster schools 2 and 3 will be the next two highest scoring cluster schools. The second time, I rank the cluster schools by proximity, meaning cluster school 1 will be the closest cluster school, and cluster schools 2 and 3 will be the next two closest schools.

Table 2.6 show results from estimating the impact of distance on house prices using the highest scoring cluster schools. For local schools, the coefficient on distance is negative when the interaction term is not included, implying that as you move farther from the school, house prices decline. When the interaction between test score and distance is included, the coefficient on distance is positive, and the coefficient on the interaction is negative. Though the coefficients are not statistically significant, the signs are as expected. The coefficients imply that when test scores are low, house prices decrease as one moves closer to the school. But when test scores are high, house prices increase as one moves closer to the school.

For the cluster schools, the results are more ambiguous. The coefficients on cluster distances and the interactions are very small, statistically insignificant and mostly the wrong sign. Given that the system of school choice in Jefferson County is very complicated, it is not surprising that the results are unclear. Because many cluster are, on average, very similar, and because the distance between schools varies dramatically both within and across clusters, it is difficult to infer the degree to which parents value distance from and quality of cluster schools. To further explore the importance of proximity, I next estimate a similar relationship; however, the cluster schools will now be ranked by proximity, rather than by test scores.

The columns of Table 2.7 estimate the same relationships as columns 3-6 of Table 2.6, except that the cluster schools are ranked by their distance from the house of interest, instead of by test score. All coefficients have a higher statistical significance than in Table 2.7. But, again, the magnitudes and the signs of the coefficients are still occassionally the wrong sign.

In general, Tables 2.6 and 2.7 suggest that, for cluster schools, distance is slightly more important than the quality of the school. This makes sense for Jefferson County. The distances between cluster schools can be large. Some parents report that their student rides the school bus for more than one hour each way, and some students must ride more than one bus to get to school. With such large distances to travel, it is not surprising that parents place a lot of weight on how close they might be to potential schools.

Beyond this preference for proximity, the results also suggest that determining the preferences of parents living in Jefferson County is not a simple matter. Because of the non-contiguity of clusters, parents must carefully examine their location with regard to a number of factors: the possibile distances from potential schools to their house, the potential quality of potential schools, and the likelihood of acceptance at each school.

Though Tables 2.6 and 2.7 provide less clear results, Tables 2.4 and 2.5 suggest that parents care about both proximity and the test scores of potential schools. In the next section, I use these results to develop a strategy to test the impact of varied degrees of school choice on the relationship between house prices and school quality.

2.8 Estimating the Impact of Access to School Choice

To directly estimate the impact of school choice on the relationship between house prices and school quality, I make use of parents' preferences for particular cluster school characteristics and exploit the design of the cluster system in JCPS. Recall that most clusters in JCPS are non-contiguous, meaning that school catchment areas generally do not share a catchment area boundary with other schools in the same cluster. Because of this non-contiguity, some houses are far away from the available cluster schools and other houses are relatively close to their available cluster schools. Additionally, there are differences in the quality of nearby or far away cluster schools.

Because of these differences, the accessibility of choice varies throughout both the school district and one's cluster. To illustrate this point, consider two ficticious clusters, A and Z. Cluster A contains schools A, B, C, and D. Cluster Z contains schools W, X, Y, and Z. Suppose the average test score in each cluster is the same. Also suppose that school A and school Z have the same test score. Now, consider two families, one that lives in school A's catchment area and one that lives in school Z's catchment area. Based on test scores, the clusters are essentially identical. However, suppose that schools A, B, C, and D are all less than five miles from the house in school A's catchment area. And suppose that schools W, X, and Y are all more than fifteen miles from the house in school Z's catchment area. Even though both houses have the option of choosing a school other than their local school, that option is more accessible to the family in school A's catchment area because the house is very close to all of the available schools.

The previous section revealed several results regarding parents' preferences. First, parents care about proximity. Second, parents care about the test scores of cluster schools. Third, parents may care about the worst possible outcome for their child and thus care about the marginal cluster school that is ranked near last but could still be attended. Based on these observations, I make several assumptions about the accessibility of choice. I claim that when a house is very close to its cluster schools, choice is very accessible, and when a house is very far away from its cluster schools, choice is much less accessible. I also claim that when a house is very close to the best cluster schools, choice is more accessible compared to houses far from the best cluster schools.

Based on these assumptions, I can now test whether house prices are more or less responsive to changes in school quality when there is more or less school choice. I expect that the coefficient on local school test scores will be higher for houses in areas where choice is less accessible compared to houses in areas where choice in more accessible. I expect that the coefficients on cluster school test scores will be lower in areas where choice is less accessible comapred to houses in areas where choice is more accessible.

I test whether house prices are more or less responsive to changes in school quality when there is more or less school choice by dividing my sample based on the accessibility of choice and then estimating the relationship between house prices and school quality for each group in the sample. To do this, I first construct several measures of school choice accessibility. Below, I describe each measure of accessibility. For each measure, choice is most accessible when the value of the measure is smallest.

1. The average of the distances between a house and its three closest cluster schools.
2. The average of the distances between a house and all cluster schools.
3. The distance between a house and the best cluster school.
4. The average distance between a house and the top three cluster schools.

After constructing the measures, I divide the sample into quartiles of each measure, such that the lowest quartile has the most choice accessibility and the highest quartile has the least choice accessibility. For each measure, I then estimate a new regression equation, where I interact the indicators of choice accessbility quartile with the school quality variables to test whether houses are more or less responsive to changes in school quality when the accessibility of choice is different. The estimating equation is

$$ln(price)_{i,s,b,t} = \alpha + \sum_{q=1}^{4}[\beta_q(Quality_{s,t} \times Q_q)] + X_i\delta + \lambda_t + \epsilon_{i,s,b,t}, \quad (2.5)$$

where $Quality_{s,t}$ is the test score of the local school and Q_q are dummy variables indicating that the house belongs to the qth quartile of choice accessibility. Q_q can correspond to any of the four accessibility measures described previously. Each dummy variable is also included in the estimating equation. Equation 2.5 will also be modified to include measures of cluster quality interacted with choice quartile:

$$ln(price)_{i,s,b,t} = \alpha + \sum_{q=1}^{4}[\beta_q(Quality_{s,t} \times Q_q) + \gamma_q(Quality_{c,t} \times Q_q)] + X_i\delta + \lambda_t + \epsilon_{i,s,b,t}, \tag{2.6}$$

where $Quality_{c,t}$ is first the weighted average of top three cluster schools' test scores and then the average of the closest three schools' test scores.

Tables 2.8 - 2.11 show the results from estimating equation 2.6. Each table uses a different measure of choice accessibility to divide the sample into quartiles. Each set of rows uses a different measure of cluster school availability to divide the sample into quartiles. Column 1 of each table estimates the effect of the local schools' test score interacted with each quartile. In columns 2 though 5, cluster schools' test scores interacted with quartile are also included. In all columns, I also include controls for each quartile, year effects, and house characteristics. Though I have not been displaying the results that do not include boundary fixed effects, I do include these estimates in the following tables. I do this because the results change so dramatically with the inclusion of boundary fixed effects that it is important to see this change. I will first discuss the estimated coefficients on local school test scores, and then I will discuss the estimated coefficients on cluster school test scores.

2.8.1 Local School Quality Effects

In table 2.8, the school choice accessibility index is measured as the average distance to all cluster schools. This means that houses in quartile 1 are, on average, closest to their cluster schools. Houses in quartile 4 are, on average, farthest from their cluster schools. The impact of local school quality on house prices is largest for households that are, on average, farther from cluster schools. This makes sense;

if one is far from the available cluster schools, then we would expect parents to care more about the local school, which is nearby. In columns 2 and 3, the coefficient on local school quality for houses far from cluster schools is three to four times higher than this coefficient for houses close to cluster schools. However, one should not rely on the estimates from columns 2 and 4 because they do not use boundary fixed effects. The pattern of the coefficients is still present in columns 1, 3, and 5, though the coefficients are not as dramatically different.

In table 2.9, the school choice accessibility index is measured as the average distance to the closest three cluster schools. I chose three schools because parents are allowed to rank up to four schools. These results echo the results of table 2.8. Even with fixed effects included, the effect of local school test scores on house prices is largest for houses that are farthest from the closest three cluster schools.

Tables 2.9 and 2.10 incorporate both distance and quality into the choice accessibility index. In table 2.9, the index is measured as the average distance to the highest scoring three cluster schools. As in tables 2.8 and 2.9, one can see that the largest effects of local school test scores on house prices are found for houses farthest from the best cluster schools. This result is even more pronounced than in tables 2.8 and 2.9. In table 2.10, the choice index is distance to the single best school in the cluster. Again, when a house is far from the high scoring cluster school, house prices are more responsive to changes in local school test scores.

2.8.2 Cluster School Quality Effects

For all measures of school choice accessibility, houses that have less access to choice because they are farther away from the available schools are the most responsive to local school quality. Houses that have the most access to choice are not as responsive to changes in their local school quality.

The results are not so clear-cut with respect to cluster school test scores. When examining the impact of cluster school test scores on house prices, an interesting pattern emerges that is present is all of tables 2.8 through 2.11. Without fixed effects, house prices are much more responsive to cluster school test scores when they are very close to the cluster schools, compared to when houses are far from the cluster schools. However, when fixed effects are estimated, this pattern usually disappears, and occasionally the reverse effect emerges, such that house prices are most responsive to cluster school test scores when they are farthest from cluster schools. This is true whether cluster test scores are measured as simply the average test score of all cluster schools or a weighted average of test scores, where the weights are the distance from each house to its cluster schools.

Theoretically, it is difficult to know what we expect to happen with respect to cluster school test scores. The most obvious expectation is that when choice is less accessible, parents care less about the cluster schools and more about the local school. However, it could be the case that when choice is more accessible, parents care less, in general, about the quality of each school. If they are close to many schools, then one of the desirable factors of a school has been met - its proximity; quality may then play less of a role in choosing a school.

As before, there are several difficulties arise when estimating the impact of cluster "quality" on house prices. The question of what measure of quality to use is not easily answered. Do parents see the quality as the average test score of the cluster schools? Do they look at the best test score? Do they look at some combination of test scores and distance to one's house? The results from the previous section indicated that parents care about both test scores and proximity. However, the initial estimates of the relationship between cluster school test scores and proximity are not precise enough to let one know for sure exactly what measures of cluster "quality" are important. This is most certainly and area where more research can and needs to be done.

2.9 Extensions and Robustness Checks

My primary results support the hypothesis that school choice mitigates the responsiveness of house prices to changes in local school quality. In this section, I extend my analysis to show that my results are robust to different methodologies. First, I examine whether my identification assumption is likely to hold.

2.9.1 Exploring Identification

My identification strategy rests on the assumption that houses that are geographically close have similar unobservable characteristics. Ultimately, my identification strategy cannot be tested. However, I can test the extent to which houses close to a boundary have more similar *observable* characteristics than houses farther away from a boundary.

What I want to test is whether houses located in a better school's area are observably better houses in terms of square footage, number of stories, etc. In particular, for houses that are near a boundary and in a better school's area, are these houses better than houses that are near the same boundary but in the lower-scoring school's area? To answer this question, I first assign each house to either the "low" or the "high" side of the boundary. A house is on the high side if its residential-area school has higher test scores than the school on the opposite side of the boundary[16].

[16] I compare schools' CTBS total scores to determine high or low status.

I then regress each house characteristic on boundary fixed effects and a set of distance dummy variables. Each distance dummy variable measures the distance from the house to the boundary, in .05 mile increments. I include only houses for which the gap between low and high is at least as large as the median gap.

Figure 5 presents the results in a series of graphs. The graphs are constructed by plotting the coefficients on the distance dummy variables from the above regressions. A negative distance means that the house is on the low side of the boundary.

The first panel presents the results for CTBS reading percentile. By construction, there should be a jump at the boundary; this jump is indeed pronounced. The next panel presents the results for the natural log of price (inflation adjusted). Interestingly, there is not a large jump in price at the boundary. However, prices are on average higher on the high side of the boundary.

The remaining graphs display the coefficients on other characteristics. Notice that the distance from the boundary is allowed to reach up to one mile. The relevant area for my identification strategy is to compare the coefficients less than 2000 feet (or approximately one half mile) away from the boundary. We expect to see more of a trend as the distance gets larger.

The coefficient on acreage is very stable across boundaries. For square footage, houses tend to to have slightly higher square footage on the high side of the boundary. Stories and home age are rather variable but they appear to be reasonably continuous on either side of the boundary.

The statistical tests for the presence of discontinuities at the boundary are presented in Table 2.12. The tests reinforce what the graphs suggest. Test scores and house prices change discontinuously at catchment area boundaries. However, other home characteristics change continuously. This finding lends support to my identification strategy. Because there are no sharp discontinuities in observable characteristics when one moves from the high side of the boundary to the low side, this suggests that there will not be discontinuities in unobservables across the boundary.

2.9.2 Fack and Grenet (2010)

An extension of Black's strategy was used by Fack and Grenet (2010) to estimate the relationship between house prices and school quality for middle school students in Paris, France. This approach improves upon the boundary discontinuity approach in some ways. One problem with the boundary discontinuity approach is its inability to deal with houses that are close to the same boundary but still geographically far apart. For instance, I restrict my sample to houses that are 2000 feet from a school assignment boundary. However, if the boundary is long, say 5,000 feet, then two houses on opposite sides of the boundary and opposite ends of the boundary could be much more than 5,000 feet apart. In this case, the identification assumption of the boundary fixed effect method may fail. In response to this issue, Fack and Grenet (2010) proposed an extension of Black's strategy. As I show, the method used by Fack and Grenet also introduces a lot of noise into the measurements. Because of this shortcoming, it is not ideal for this context, where the estimates are already very small. Additionally, Fack and Grenet do not prove that their estimates are consistent or that the standard errors are correct. Because of this, one should be cautious in interpreting the results from using their method.

The method works as follows. For each house, I construct a counterfactual sale price, which is based on home sales that occurred near the home of interest but on the opposite side of the boundary. The difference in price between the sale of interest and the counterfactual sale is the dependent variable.

To be more specific, the counterfactual price is a weighted average of the sale price of houses located within a specified radius (2000 feet) of the house of interest, but on the opposite side of the school boundary. The weights are the inverse of the distance from the house of interest to each house included in the counterfactual sale price calculation. Hence, the counterfactual price is calculated as:

$$\ln price_{i',s',b,t} = \sum_{j=1}^{J} \frac{\frac{1}{d_{i,j}}}{\sum_{j=1}^{J} \frac{1}{d_{i,j}}} \ln price_{j,s',b,t}, \tag{2.7}$$

where $d_{i,j}$ is the distance from house i, the house of interest, to house j on the opposite side of the boundary. Figure 6 compares the process for selecting houses for this procedure compared to the boundary fixed affects approach. With this method, I draw a circle of radius 2000 feet around a house that is within 2000 feet of the boundary. The houses that fall into this circle and are on the opposite side of the boundary will be used to construct the counterfactual sale.

The regression equation is then as follows:

$$\ln price_{i,s,b,t} - \ln price_{i',s',b,t} = \beta(Quality_{s,t} - Quality_{s',t}) + (X_i - X_{i'})\delta + (\epsilon_{i,s,t} - \epsilon_{i',s',t})^{17} \tag{2.8}$$

I estimate this equation using OLS. My identifying assumption is that neighborhood characteristics change continuously over space, while school quality changes discontinuously. Standard errors are clustered at the school level.

The results of this procedure are presented in Table 2.13. Column 1 displays the coefficient estimates from estimating equation (6) without control variables. Column 2 includes these controls. One can compare column 2 to the estimates in column 4 of Table 2.3. The results are very similar. The coefficient estimates from

[17] I contruct counterfactual house characteristics the same way that I construct counterfactual sales. This differs from Fack and Grenet, who use regression-adjusted house prices to construct the counterfactual house prices. This eliminates the need for house characteristics in the main estimating equation. However, it introduces more measurement error.

the current method are slightly larger than those from the boundary fixed effects approach. Columns 3 and 4 include estimates of the effect of cluster school quality on house prices. The estimates are slightly smaller than those obtained from the boundary fixed effects approach, but they are qualitatively similar.

2.9.3 Other Extensions

As an additional robustness check, I utilize the approach of Bayer, Ferreira, and McMillan (2007). They show that the inclusion of both boundary fixed effects and neighborhood characteristics drives the coefficient on local school quality down even further. Though they are ultimately able to make use of restricted Census data in much of their analysis, they also estimate a boundary fixed effects regression with Census block-group level neighborhood characteristics. The characteristics used are percent of block group that is black, percent of block group with at least a college degree, and the average block group income.

There are 556 Census block groups in Jefferson County. I assign a house to a block group by matching the geocoded house addresses to Census shapefiles. There will naturally be some measurement error in the neighborhood characteristics. Sometimes houses that share a boundary will be in the same block group. Other times they will be in different block groups. There may be several block groups per boundary. However, I follow Bayer et al. (2007) as closely as possible and compare my results.

The results are presented in Table 2.14. One of the findings of Bayer et al. is that the inclusion of neighborhood characteristics in addition to boundary fixed effects causes the estimate of the house price premium for school quality to fall by about fifty percent. Column 2 presents my results with boundary fixed effects but without neighborhood characteristics. Column 4 presents results with both boundary fixed effects and neighborhood characteristics. Consistent with Bayer et. al., the coefficient on local school quality does fall. It does not fall by fifty percent, only by about twenty. However, this is a decline from an already small coefficient. This finding lends more support to their conclusions that the boundary fixed effects approach does not fully account for unobserved neighborhood characteristics. In this research, it further supports the hypothesis that school choice mitigates the impact of local school quality changes on house prices. With a mean test score of fifty, a five percent increase in test scores implies an increase in house prices of less than two-tenths of a percent.

2.10 Conclusion

In this chapter, I investigate the relationship between school quality and house prices in the context of intradistrict school choice. I find that the estimated impact of local school quality on house prices is much smaller than what has been estimated in school districts without school choice. The literature has found that a five percent increase in local school quality leads to a one to three percent increase in house prices. I estimate that a five percent increase in the local school's test score percentile would lead to between one and three tenths of a percent increase in house prices.

This effect is primarily observed in houses that have the least opportunities to exercise choice in this school district. Using the distance to cluster schools as a way to measure school choice accessibility, I find that as the proximity to cluster schools increases, the house price premium for local school quality declines. For houses that are, on average, close to their cluster schools, the coefficient on local school quality is up to four times smaller than the coefficient on local school quality for houses farthest from their cluster school.

Additionally, as the proximity to cluster schools increases, the house price premium for cluster school quality increases. Together with the previous result, this finding is consistent with previous research that finds that parents care about the distance between their house and their child's school.

Overall, this chapter shows that, under school choice, the impact of local school quality on house prices is smaller. I also show that parents prefer high scoring schools and schools that are close to their home. Further exploration using parents' choice applications in addition to the empirical approach used here could shed more light on the impact of school choice on parents' preferences.

Table 2.1: House Summary Statistics

	All Houses	<2000ft From Boundary	High Side	Low Side
Price	$152,112	$144,547	$148,793	$140,430
	(90,204)	(79,820)	(82,100)	(77,325)
Sq. Footage	1,465	1,428	1,457	1,400
	(615.7)	(579.8)	(595.9)	(562.4)
Stories	1.323	1.320	1.324	1.316
	(0.386)	(0.383)	(0.393)	(0.373)
Acreage	0.249	0.235	0.236	0.235
	(0.343)	(0.259)	(0.235)	(0.279)
Home Age	43.38	44.24	42.93	45.52
	(24.51)	(24.75)	(24.49)	(24.93)
Assessed Value	151,653	143,403	147,811	139,129
	(94,316)	(84,214)	(87,153)	(81,037)
No. Bathrooms	1.723	1.682	1.717	1.649
	(0.740)	(0.718)	(0.725)	(0.710)
Air Conditioning	0.894	0.884	0.892	0.875
	(0.308)	(0.321)	(0.310)	(0.330)
Attached Garage	0.267	0.254	0.266	0.242
	(0.442)	(0.435)	(0.442)	(0.428)
Detached Garage	0.365	0.374	0.369	0.379
	(0.482)	(0.484)	(0.483)	(0.485)
Garage Sq. Footage	290.4	284.7	289.9	279.6
	(274.7)	(270.2)	(270.2)	(270.1)
Basement	0.623	0.615	0.621	0.610
	(0.485)	(0.487)	(0.485)	(0.488)
% of Students in Catchment Area Attending:				
Local School	0.696	0.696	0.694	0.699
	(0.129)	(0.133)	(0.133)	(0.133)
#1 School	0.0514	0.0531	0.0482	0.0578
	(0.0563)	(0.0568)	(0.0517)	(0.0609)
#2 School	0.0427	0.0435	0.0399	0.0469
	(0.0499)	(0.0501)	(0.0461)	(0.0535)
#3 School	0.0454	0.0464	0.0477	0.0452
	(0.0531)	(0.0535)	(0.0527)	(0.0543)
Distance (in 1,000's of feet) to:				
Local School	0.900	0.927	0.957	0.898
	(0.618)	(0.594)	(0.608)	(0.578)
#1 Cluster School	5.570	5.479	5.594	5.368

	(3.164)	(3.155)	(3.132)	(3.174)
#2 Cluster School	5.482	5.377	5.325	5.428
	(2.997)	(2.983)	(2.847)	(3.109)
#3 Cluster School	5.618	5.574	5.521	5.625
	(3.162)	(3.176)	(3.191)	(3.161)
Closest Cluster School	2.366	2.327	2.288	2.364
	(1.509)	(1.526)	(1.411)	(1.628)
2nd Closest Cluster School	3.692	3.624	3.516	3.728
	(1.850)	(1.833)	(1.692)	(1.954)
3rd Closest Cluster School	5.261	5.228	5.069	5.383
	(2.384)	(2.378)	(2.280)	(2.459)
Observations	36,348	28,807	14,183	14,624

Table 2.2: Local School Summary Statistics, Clusters 1-6

Cluster:	(1)	(2)	(3)	(4)	(5)	(6)
Local School Reading Score	49.23	56.1	43.07	59.37	42.51	48.67
	(12.60)	(12.92)	(9.92)	(15.44)	(12.63)	(11.23)
Local School Math Score	50.44	56.78	45.21	59.9	43.9	49.98
	(13.44)	(12.63)	(11.86)	(16.24)	(14.65)	(10.55)
Local School Lang. Arts Score	48.94	55.7	43.12	59.08	42.53	48.48
	(12.90)	(12.94)	(11.13)	(15.81)	(13.79)	(10.58)
Local School Composite Score	50.16	57.52	43.86	61.02	42.96	49.5
	(13.92)	(13.82)	(11.63)	(17.09)	(14.73)	(11.48)
Composite Score of Best Cluster School	74.1	54.6	76.9	61.3	63.5	61.6
(Avg. of All Years)	(5.68)	(5.12)	(3.39)	(9.73)	(10.60)	(4.86)
Observations	551	69	42	49	49	42

Table 2.2: Local School Summary Statistics, Clusters 7-12

Cluster:	(7)	(8)	(9)	(10)	(11)	(12)	All
Local School Reading Score	45.14	44.08	43.93	55.29	48.88	48.5	48.59
	(13.08)	(9.14)	(7.40)	(7.78)	(9.19)	(14.77)	(9.02)
Local School Math Score	45.57	44.73	44.62	56.16	51.71	50.62	49.44
	(13.28)	(11.96)	(7.41)	(8.79)	(11.95)	(15.47)	(11.00)
Local School Lang. Arts Score	44	44.98	43.21	54.69	47.9	49.14	47.88
	(12.31)	(11.38)	(6.95)	(7.79)	(9.96)	(14.36)	(10.55)
Local School Composite Score	45.22	44.6	44	56.57	50	50.17	49.22
	(14.02)	(11.35)	(7.42)	(8.61)	(10.91)	(16.03)	(10.62)
Composite Score of Best Cluster School	61.0	51.3	69.1	64.5	71.2	60.5	
(Avg. of All Years)	(11.8)	(4.21)	(3.50)	(10.97)	(7.51)	(6.69)	
Observations	37	40	42	49	49	42	41

Table 2.3: Estimates of the Impact of Local School Test Scores on House Prices

	All Houses		Sample Restricted to Houses ≤2000ft from Boundary			
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	ln(price/sqft)	ln(price/sqft)	ln(price/sqft)	ln(price/sqft)	ln(price)	Price
Local School Test Score	0.00822***	0.00488***	0.00435***	0.00092***	0.00129***	151.12**
	(0.00010)	(0.00094)	(0.00093)	(0.00032)	(0.00035)	(73.739)
Year Effects		X	X	X	X	X
House Characteristics		X	X	X	X	X
Boundary Fixed Effects				X	X	X
Observations	36,348	36,348	28,807	28,807	28,807	28,807
R-squared	0.14502	0.33952	0.36063	0.59073	0.86500	0.82420

(1) Standard errors clustered at the school level in parentheses
(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$
(3) House characteristics include acreage, number of stories, number of bathrooms, whether the house has air conditioning, whether basement, whether garage, and home age.
(4) Local School Test Score is the CTBS test score percentile of the house's local school.

Table 2.4: Estimates of the Impact of High Scoring Cluster Schools' Test Scores on House Prices

	No Boundary Fixed Effects			Boundary Fixed Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
Cluster Avg. of Composite Test Scores (Best Schools)	0.00436***			-0.00019		
	(0.00018)			(0.00022)		
Weighted Cluster Avg. of Composite Test Scores		0.00400***			-0.00015	
		(0.00018)			(0.00021)	
Composite Test Score (#1 Cluster School)			0.00062**			-0.00050**
			(0.00028)			(0.00025)
Composite Test Score (#2 Cluster School)			0.00036			-0.00020
			(0.00038)			(0.00033)
Composite Test Score (#3 Cluster School)			0.00323***			0.00046*
			(0.00029)			(0.00028)
Local Composite Test Score	0.00323***	0.00330***	0.00319***	0.00093***	0.00093***	0.00092***
	(0.00013)	(0.00013)	(0.00013)	(0.00014)	(0.00014)	(0.00014)
Year Effects	X	X	X	X	X	X
House Characteristics	X	X	X	X	X	X

(1) Clustered standard errors in parentheses
(2) *** p<0.01, ** p<0.05, * p<0.1
(3) All columns include controls for house characteristics and year effects.
(4) Sample is restricted to houses within 2000ft of a catchment area boundary.

Table 2.5: Estimates of the Impact of Cluster School Quality on House Prices

	(1)	(2)	(3)	(4)
Composite Test Score (Closest Cluster School)		0.00136***		0.00120***
		(0.00012)		(0.00012)
Composite Test Score (2nd Closest Cluster School)		0.00112***		0.00110***
		(0.00012)		(0.00012)
Composite Test Score (3rd Closest Cluster School)		0.00041***		-0.00013
		(0.00012)		(0.00013)
Composite Test Score Cluster Avg. (Closest Schools)	0.00291***		0.00224***	
	(0.00016)		(0.00016)	
Local Composite Test Score			0.00223***	0.00239***
			(0.00013)	(0.00013)

(1) Standard errors in parentheses
(2) *** p<0.01, ** p<0.05, * p<0.1
(3) Boundary fixed effects estimated in all columns

Table 2.6: Estimates of the Importance of Proximity, Cluster Schools Ranked by Test Scores

	(1)	(2)	(3)
Local Composite Test Score	0.00268***	0.00330***	0.00247***
	(0.00023)	(0.00048)	(0.00055)
Distance to Local School	-0.00331*	0.00433	0.00521
	(0.00171)	(0.00572)	(0.00581)
Local Composite Test Score × Distance		-0.00014	-0.00015
		(0.00009)	(0.00009)
Composite Test Score (#1 Cluster School)			0.00102**
			(0.00041)
Composite Test Score (#2 Cluster School)			0.00070
			(0.00075)
Composite Test Score (#3 Cluster School)			0.00025
			(0.00058)
Distance to #1 Cluster School			-0.00064
			(0.00083)
Distance to #2 Cluster School			0.00000
			(0.00079)
Distance to #3 Cluster School			-0.00074
			(0.00058)
Cluster Comp. Test Score × Dist. (#1 School)			0.00001
			(0.00001)
Cluster Comp. Test Score × Dist. (#2 School)			0.00000
			(0.00001)
Cluster Comp. Test Score × Dist. (#3 School)			0.00002
			(0.00001)

(1) Clustered standard errors in parentheses

(2) *** p<0.01, ** p<0.05, * p<0.1

(3) All columns include controls for house characteristics and year effects.

(4) All columns include boundary fixed effects.

(5) Sample is restricted to houses within 2000ft of a catchment area boundary.

Table 2.7: Estimates of the Importance of Proximity, Cluster Schools Ranked by Distance

	(1)	(2)	(3)
Local Composite Test Score	0.00268***	0.00330***	0.00327***
	(0.00023)	(0.00048)	(0.00049)
Distance to Local School	-0.00331*	0.00433	0.00722
	(0.00171)	(0.00572)	(0.00554)
Local Composite Test Score × Distance		-0.00014	-0.00019**
		(0.00009)	(0.00009)
Composite Test Score			0.00129***
(Closest Cluster School)			(0.00040)
Composite Test Score			0.00145***
(2nd Closest Cluster School)			(0.00050)
Composite Test Score			-0.00188***
(3rd Closest Cluster School)			(0.00056)
Distance to Closest Cluster School			-0.00025
			(0.00169)
Distance to 2nd Closest Cluster School			0.00147
			(0.00151)
Distance to 3rd Closest Cluster School			-0.00316***
			(0.00097)
Cluster Comp. Test Score ×			0.00000
Distance (Closest School)			(0.00003)
Cluster Comp. Test Score ×			-0.00001
Distance (2nd Closest School)			(0.00003)
Cluster Comp. Test Score ×			0.00007***
Distance (3rd Closest School)			(0.00002)

(1) Clustered standard errors in parentheses

(2) *** p<0.01, ** p<0.05, * p<0.1

(3) All columns include controls for house characteristics and year effects.

(4) Sample is restricted to houses within 2000ft of a catchment area boundary.

Table 2.8: Estimates of the Impact of School Test Scores on House Prices by Quartile of School Choice Accessibility, where Accessibility is Measure by the Average Distance to All Cluster Schools

	(1)	(2)	(3)	(4)	(5)
Composite Test Score x Q1	0.00247***	0.00198**	0.00175***	0.00177**	0.00173***
	(0.00043)	(0.00082)	(0.00043)	(0.00070)	(0.00036)
Composite Test Score x Q2	0.00254***	0.00188	0.00154***	0.00195	0.00216***
	(0.00039)	(0.00150)	(0.00047)	(0.00143)	(0.00055)
Composite Test Score x Q3	0.00265***	0.00275	0.00197***	0.00379	0.00267***
	(0.00055)	(0.00203)	(0.00057)	(0.00246)	(0.00058)
Composite Test Score x Q4	0.00306***	0.00764***	0.00204***	0.00682**	0.00236***
	(0.00053)	(0.00278)	(0.00055)	(0.00281)	(0.00056)
Weighted Cluster Avg. of Test Score x Q1		0.00377***	0.00299***		
		(0.00116)	(0.00046)		
Weighted Cluster Avg. of Test Score x Q2		0.00841***	0.00219***		
		(0.00218)	(0.00063)		
Weighted Cluster Avg. of Test Score x Q3		0.00487***	0.00313***		
		(0.00145)	(0.00042)		
Weighted Cluster Avg. of Test Score x Q4		-0.00200	0.00353***		
		(0.00357)	(0.00070)		
Avg. Test Score of Cluster Schools x Q1				0.00399***	0.00297***
				(0.00086)	(0.00037)
Avg. Test Score of Cluster Schools x Q2				0.00658***	0.00051
				(0.00168)	(0.00067)
Avg. Test Score of Cluster Schools x Q3				0.00384*	0.00289***
				(0.00230)	(0.00049)
Avg. Test Score of Cluster Schools x Q4				0.00118	0.00371***
				(0.00410)	(0.00077)

Boundary Fixed Effects	X	X	X

(1) Clustered standard errors in parentheses
(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$
(3) All columns include controls for house characteristics and year effects.
(4) Sample is restricted to houses within 2000ft of a catchment area boundary.
(5) Composite Test Score Weighted Cluster Average is the average of cluster school test scores, weighted by distance to each house.
(6) Dummy variables for each quartile are included in all columns.

Table 2.9: Estimates of the Impact of School Test Scores on House Prices by Quartile of School Choice Accessibility, where Accessibility is Measure by the Average Distance to the Closest Three Cluster Schools

	(1)	(2)	(3)	(4)	(5)
Composite Test Score x Q1	0.00228***	0.00180**	0.00171***	0.00122	0.00155***
	(0.00037)	(0.00089)	(0.00041)	(0.00078)	(0.00035)
Composite Test Score x Q2	0.00175***	0.00293	0.00115***	0.00278	0.00127***
	(0.00041)	(0.00204)	(0.00043)	(0.00167)	(0.00043)
Composite Test Score x Q3	0.00299***	0.00378	0.00170***	0.00370	0.00239***
	(0.00044)	(0.00245)	(0.00055)	(0.00236)	(0.00056)
Composite Test Score x Q4	0.00316***	0.00417**	0.00252***	0.00499**	0.00304***
	(0.00044)	(0.00162)	(0.00054)	(0.00202)	(0.00045)
Weighted Cluster Avg. of Test Score x Q1		0.00471***	0.00232***		
		(0.00147)	(0.00051)		
Weighted Cluster Avg. of Test Score x Q2		0.00933***	0.00370***		
		(0.00206)	(0.00074)		
Weighted Cluster Avg. of Test Score x Q3		0.00328	0.00296***		
		(0.00259)	(0.00038)		
Weighted Cluster Avg. of Test Score x Q4		0.00326	0.00407***		
		(0.00268)	(0.00062)		
Avg. Test Score of Cluster Schools x Q1				0.00515***	0.00226***
				(0.00119)	(0.00056)
Avg. Test Score of Cluster Schools x Q2				0.00548**	0.00215***
				(0.00215)	(0.00061)
Avg. Test Score of Cluster Schools x Q3				0.00406**	0.00276***
				(0.00161)	(0.00053)
Avg. Test Score of Cluster Schools x Q4				0.00520	0.00307***
				(0.00334)	(0.00074)

Boundary Fixed Effects	X	X	X

(1) Clustered standard errors in parentheses

(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

(3) All columns include controls for house characteristics and year effects.

(4) Sample is restricted to houses within 2000ft of a catchment area boundary.

(5) Composite Test Score Weighted Cluster Average is the average of cluster school test scores, weighted by distance to each house.

(6) Dummy variables for each quartile are included in all columns.

Table 2.10: Estimates of the Impact of School Test Scores on House Prices by Quartile of School Choice Accessibility, where Accessibility is Measure by the Average Distance to the Best Three Cluster Schools

	(1)	(2)	(3)	(4)	(5)
Composite Test Score x Q1	0.00144***	0.00154**	0.00109**	0.00152**	0.00121***
	(0.00036)	(0.00063)	(0.00041)	(0.00058)	(0.00042)
Composite Test Score x Q2	0.00246***	0.00083	0.00105***	0.00055	0.00127***
	(0.00035)	(0.00169)	(0.00037)	(0.00168)	(0.00037)
Composite Test Score x Q3	0.00256***	0.00203**	0.00176***	0.00214***	0.00208***
	(0.00037)	(0.00080)	(0.00037)	(0.00066)	(0.00030)
Composite Test Score x Q4	0.00354***	0.00689***	0.00274***	0.00663***	0.00308***
	(0.00041)	(0.00200)	(0.00052)	(0.00203)	(0.00047)
Weighted Cluster Avg. of Test Score x Q1		0.00554***	0.00208***		
		(0.00160)	(0.00042)		
Weighted Cluster Avg. of Test Score x Q2		0.00467***	0.00277***		
		(0.00111)	(0.00041)		
Weighted Cluster Avg. of Test Score x Q3		0.00809***	0.00365***		
		(0.00234)	(0.00051)		
Weighted Cluster Avg. of Test Score x Q4		0.00218	0.00315***		
		(0.00282)	(0.00062)		
Avg. Test Score of Cluster Schools x Q1				0.00521***	0.00187***
				(0.00138)	(0.00053)
Avg. Test Score of Cluster Schools x Q2				0.00428***	0.00210***
				(0.00092)	(0.00036)
Avg. Test Score of Cluster Schools x Q3				0.00827***	0.00335***
				(0.00220)	(0.00050)
Avg. Test Score of Cluster Schools x Q4				0.00646*	0.00366***
				(0.00328)	(0.00064)

Boundary Fixed Effects	X	X	X

(1) Clustered standard errors in parentheses
(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$
(3) All columns include controls for house characteristics and year effects.
(4) Sample is restricted to houses within 2000ft of a catchment area boundary.
(5) Composite Test Score Weighted Cluster Average is the average of cluster school test scores, weighted by distance to each house.
(6) Dummy variables for each quartile are included in all columns.

Table 2.11: Estimates of the Impact of School Test Scores on House Prices by Quartile of School Choice Accessibility, where Accessibility is Measure by the Distance to the Best Cluster School

	(1)	(2)	(3)	(4)	(5)
Composite Test Score x Q1	0.00230***	0.00303***	0.00190***	0.00303***	0.00203***
	(0.00031)	(0.00113)	(0.00031)	(0.00111)	(0.00033)
Composite Test Score x Q2	0.00199***	0.00283*	0.00039	0.00252**	0.00111**
	(0.00043)	(0.00155)	(0.00043)	(0.00126)	(0.00048)
Composite Test Score x Q3	0.00249***	0.00101	0.00160***	0.00096	0.00182***
	(0.00044)	(0.00116)	(0.00058)	(0.00091)	(0.00053)
Composite Test Score x Q4	0.00356***	0.00822***	0.00251***	0.00782***	0.00316***
	(0.00032)	(0.00243)	(0.00040)	(0.00201)	(0.00034)
Weighted Cluster Avg. of Test Score x Q1		0.00411***	0.00226***		
		(0.00140)	(0.00037)		
Weighted Cluster Avg. of Test Score x Q2		0.00649***	0.00394***		
		(0.00174)	(0.00052)		
Weighted Cluster Avg. of Test Score x Q3		0.00600***	0.00313***		
		(0.00165)	(0.00060)		
Weighted Cluster Avg. of Test Score x Q4		0.00120	0.00288***		
		(0.00314)	(0.00061)		
Avg. Test Score of Cluster Schools x Q1				0.00434***	0.00208***
				(0.00149)	(0.00045)
Avg. Test Score of Cluster Schools x Q2				0.00620***	0.00258***
				(0.00127)	(0.00055)
Avg. Test Score of Cluster Schools x Q3				0.00532***	0.00284***
				(0.00102)	(0.00047)
Avg. Test Score of Cluster Schools x Q4				0.00397	0.00250***
				(0.00294)	(0.00058)

Boundary Fixed Effects	X	X	X

(1) Clustered standard errors in parentheses
(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$
(3) All columns include controls for house characteristics and year effects.
(4) Sample is restricted to houses within 2000ft of a catchment area boundary.
(5) Composite Test Score Weighted Cluster Average is the average of cluster school test scores, weighted by distance to each house.
(6) Dummy variables for each quartile are included in all columns.

Table 2.12: Testing the Difference in Means Across Boundaries

	High Test Score Side Mean	Low Test Score Side Mean	Difference in Means	Test of Difference t-statistic
Local School Test Score	60.71	43.63	17.08	14.84
Price	148,793	140,430	8,363	3.13
Sq. Footage	1,457	1,400	57	1.58
Stories	1.324	1.316	0.008	1.69
Acreage	0.236	0.235	0.001	-0.44
Home Age	42.93	45.52	-2.59	-0.95

(1) Column 4 reports the t-statistic for a test of the hypothesis that the mean of the house characteristic does not vary across school catchment area boundaries. The test conditions on boundary fixed effects and adjusts for clustering at the catchment area level.
(2) Sample is restricted to houses within 2000ft of a catchment area boundary.

Table 2.13. Estimates of the Impact of Local and Cluster School Test Scores on House Prices, Replicating Fack and Grenet (2010)

	(1)	(2)	(3)	(4)
(Local Composite Test Score	0.00105***	0.00102***		0.00071
- CF Composite Test Score)	(0.00012)	(0.00033)		(0.00044)
(Cluster Composite Test Score Avg			0.00167***	0.00082
- CF Cluster Composite Test Score Avg)			(0.00060)	(0.00071)
House Characteristics		X	X	X
Year Effects	X	X	X	X

(1) Clustered standard errors in parentheses

(2) *** p<0.01, ** p<0.05, * p<0.1

(3) Independent variable is the difference between the local (or cluster) school's test score and the test score of the school located in the adjacent catchment area (across the boundary).

Table 2.14. Estimates of the Impact of Local School Test Score on House Prices with Controls for Neighborhood Characteristics

	(1)	(2)	(3)	(4)
Local Composite Test Score	0.00436***	0.00092***	0.00024	0.00074**
	(0.00092)	(0.00032)	(0.00062)	(0.00029)
%Black			-0.00419***	-0.00178***
			(0.00037)	(0.00061)
Median Family Income			0.00120	0.01240***
			(0.00369)	(0.00314)
At Least College			0.00546***	0.00193**
			(0.00069)	(0.00078)
House Characteristics	X	X	X	X
Year Effects	X	X	X	X
Restricted Sample	X	X	X	X
Boundary Fixed Effects		X		X
Observations	28,807	28,807	28,807	28,807
R-squared	0.36147	0.59075	0.52966	0.60024

(1) Standard errors clustered at the school level in parentheses.

(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

(3) Dependent variable is ln(price per square foot) in all regressions.

(4) House characteristics include acreage, number of stories, number of bathrooms, whether the house has airconditioning, whether basement, whether garage and whether attached or detached garage, and home age.

(5) Restricted sample uses only houses less than 2000 ft from boundary.

Figure 2.1: JCPS Clusters 1 - 6

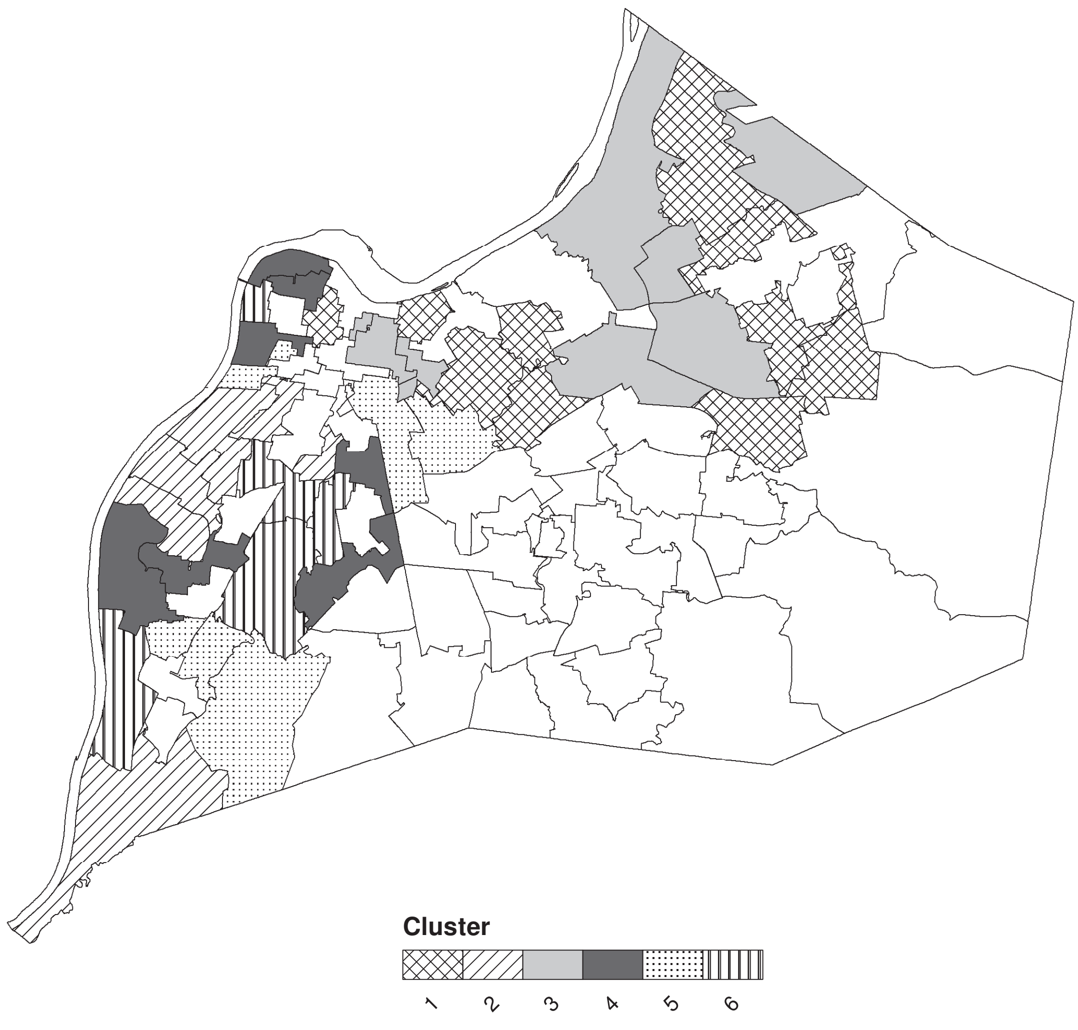


Figure 2.2: JCPS Clusters 7 - 12

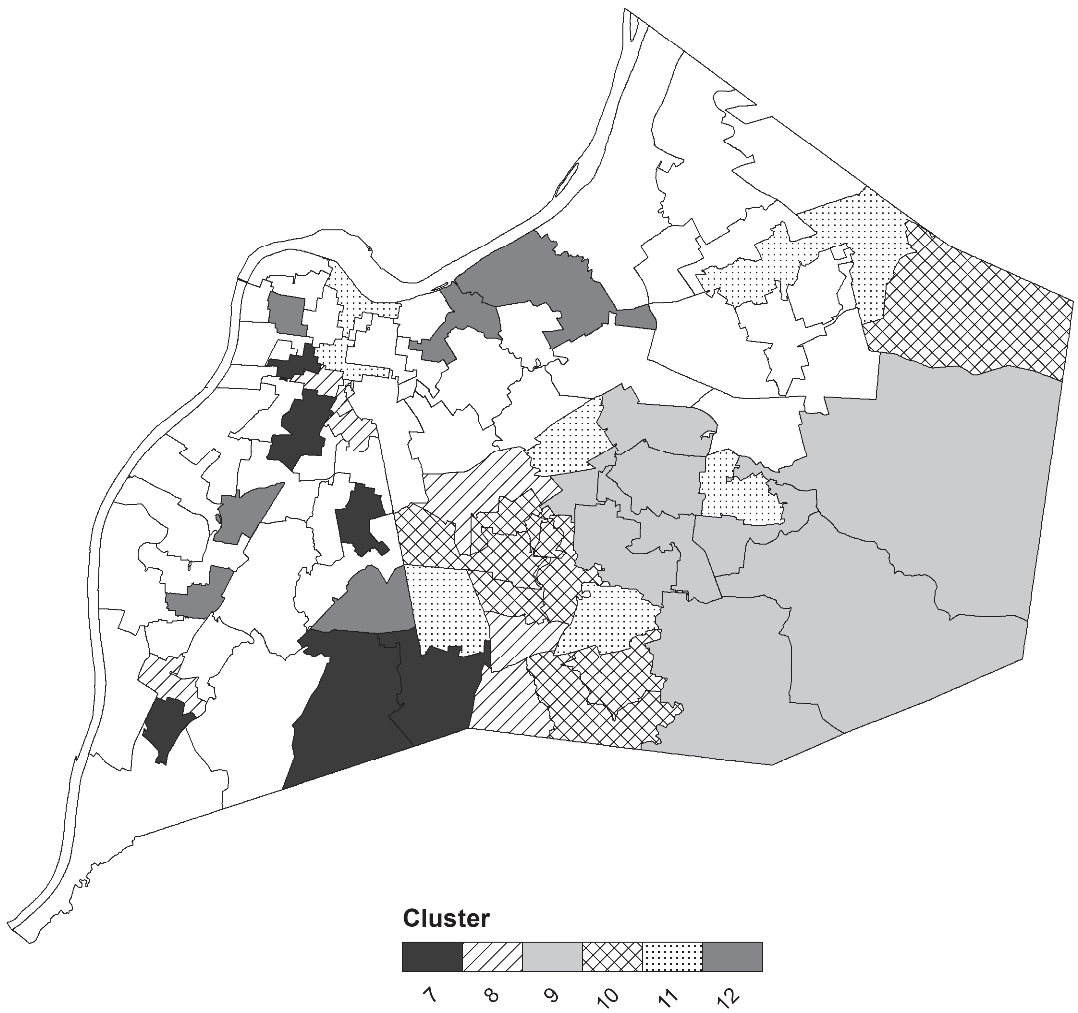


Figure 2.3: Assignment of Clusters

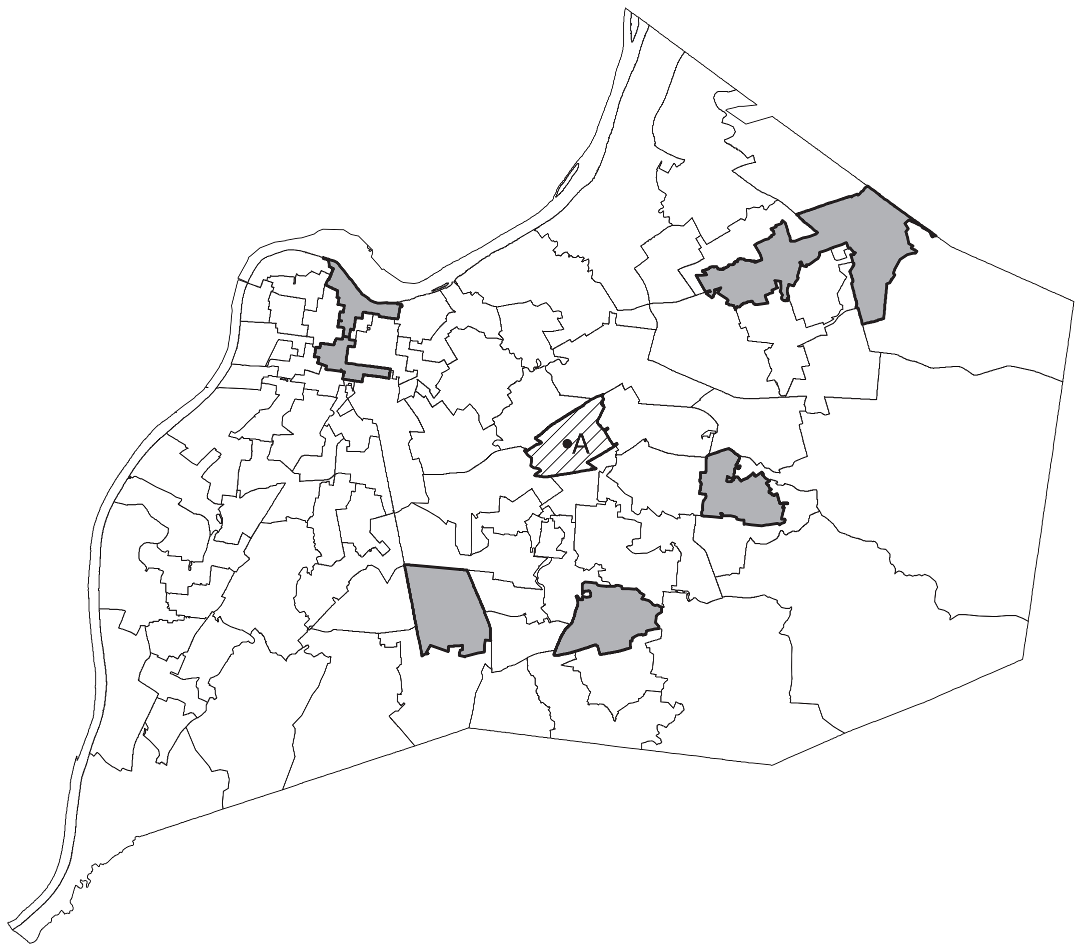


Figure 2.4: Selecting Houses within 2000 Feet of a Boundary

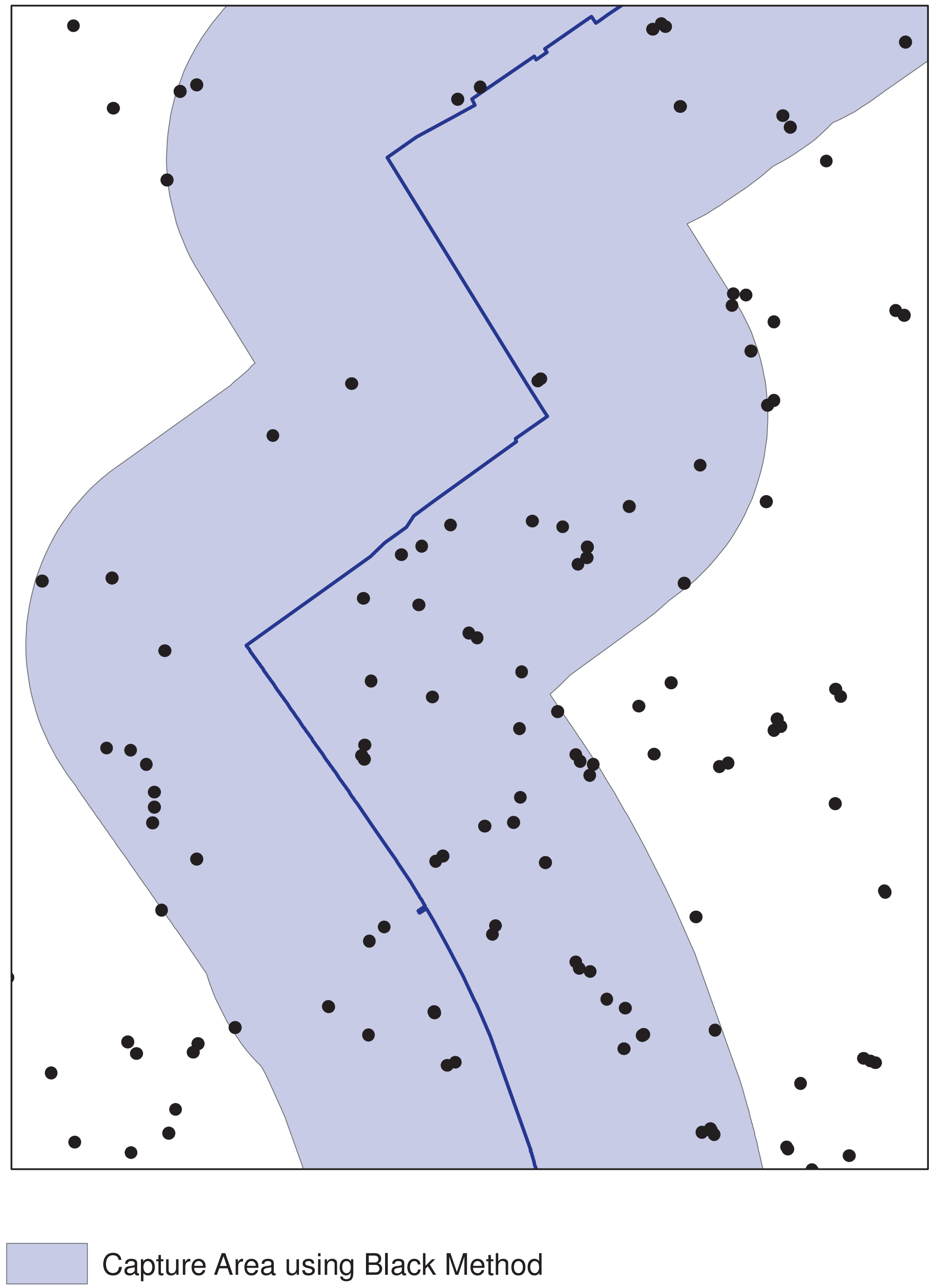
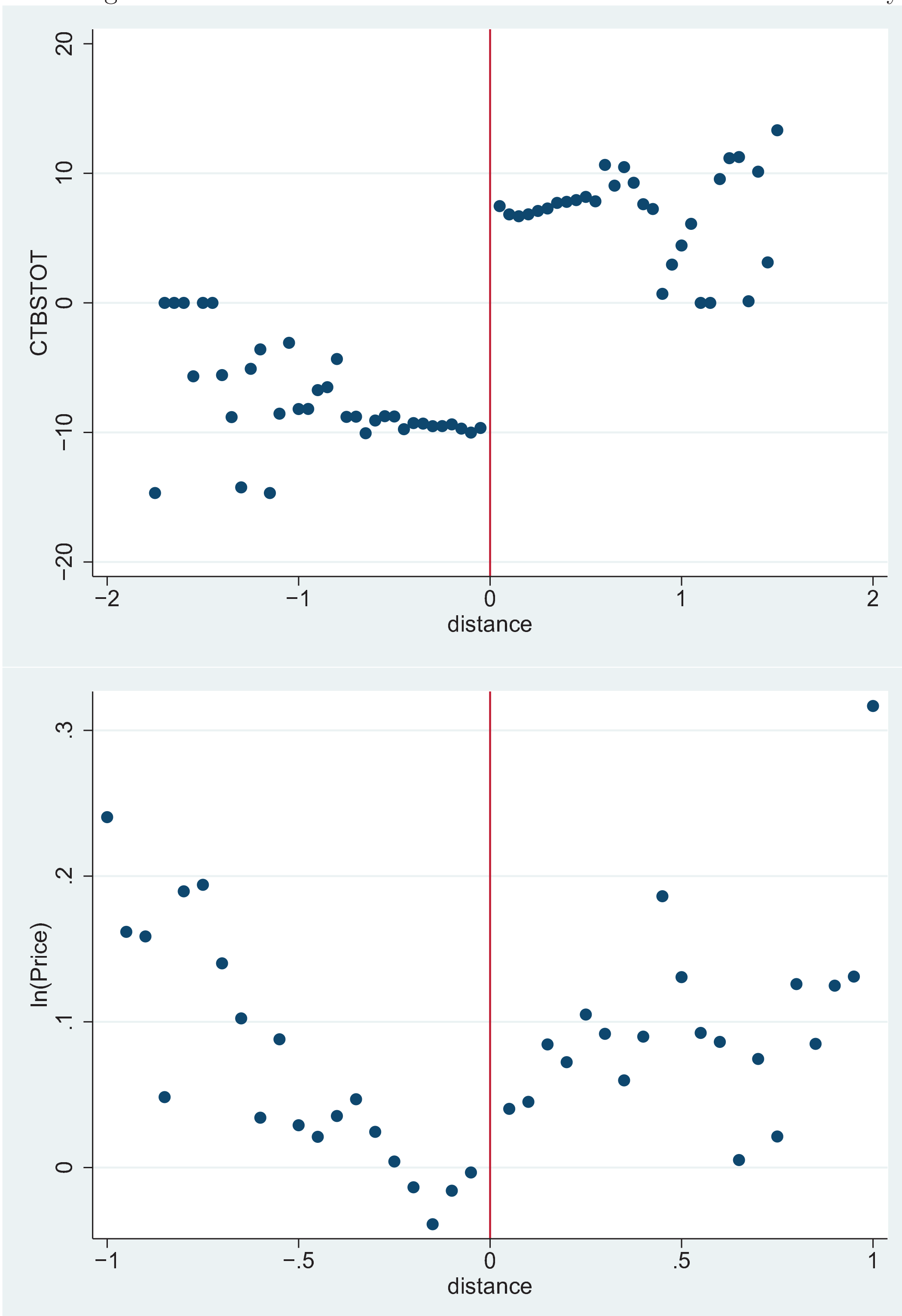
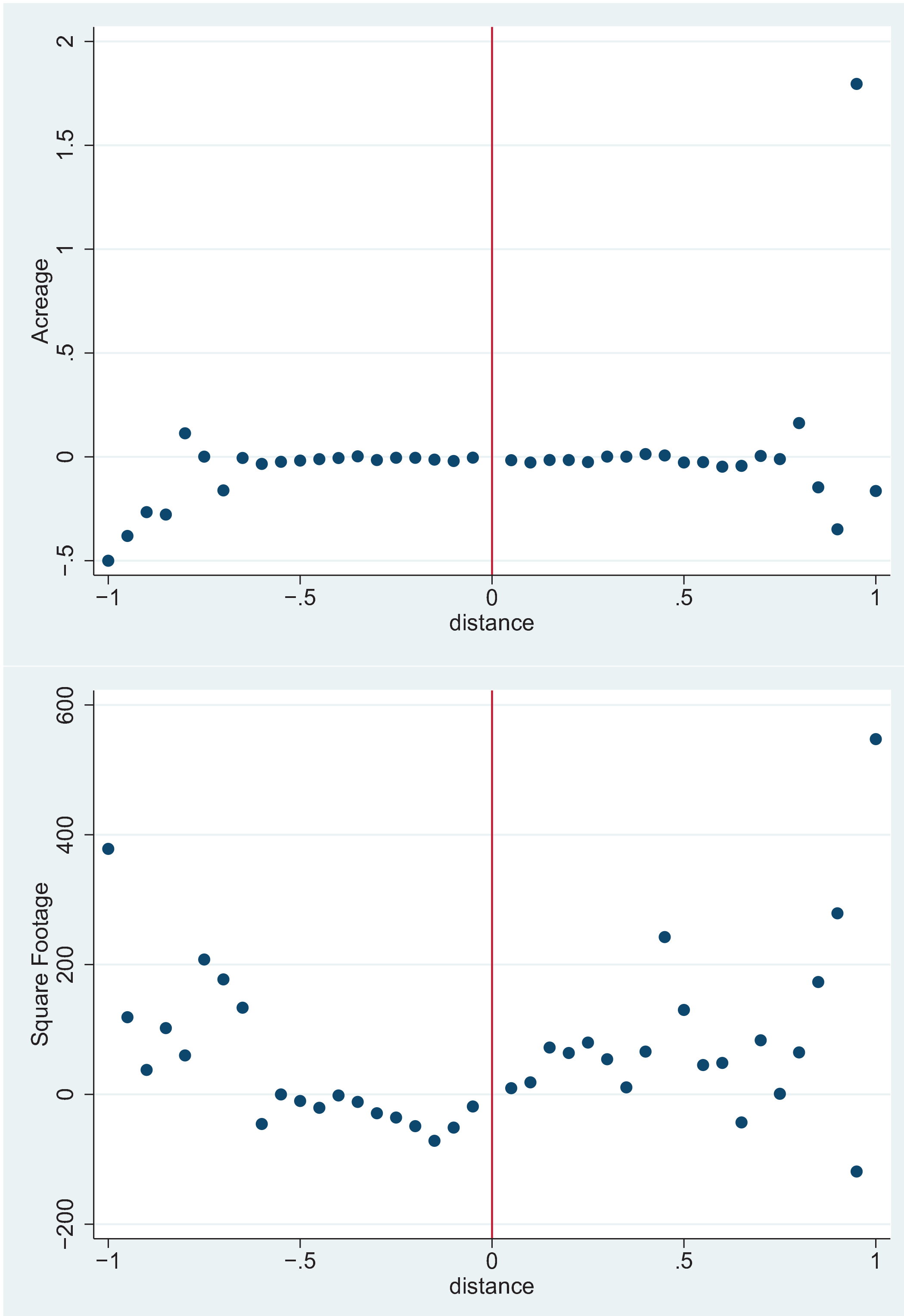


Figure 2.5: Test Scores and House Characteristics Near the Boundary





Acreage
2
1.5
1
.5
0
−.5
−1
−.5
0
.5
1
distance
Square Footage
600
400
200
0
−200
−1
−.5
0
.5
1
distance

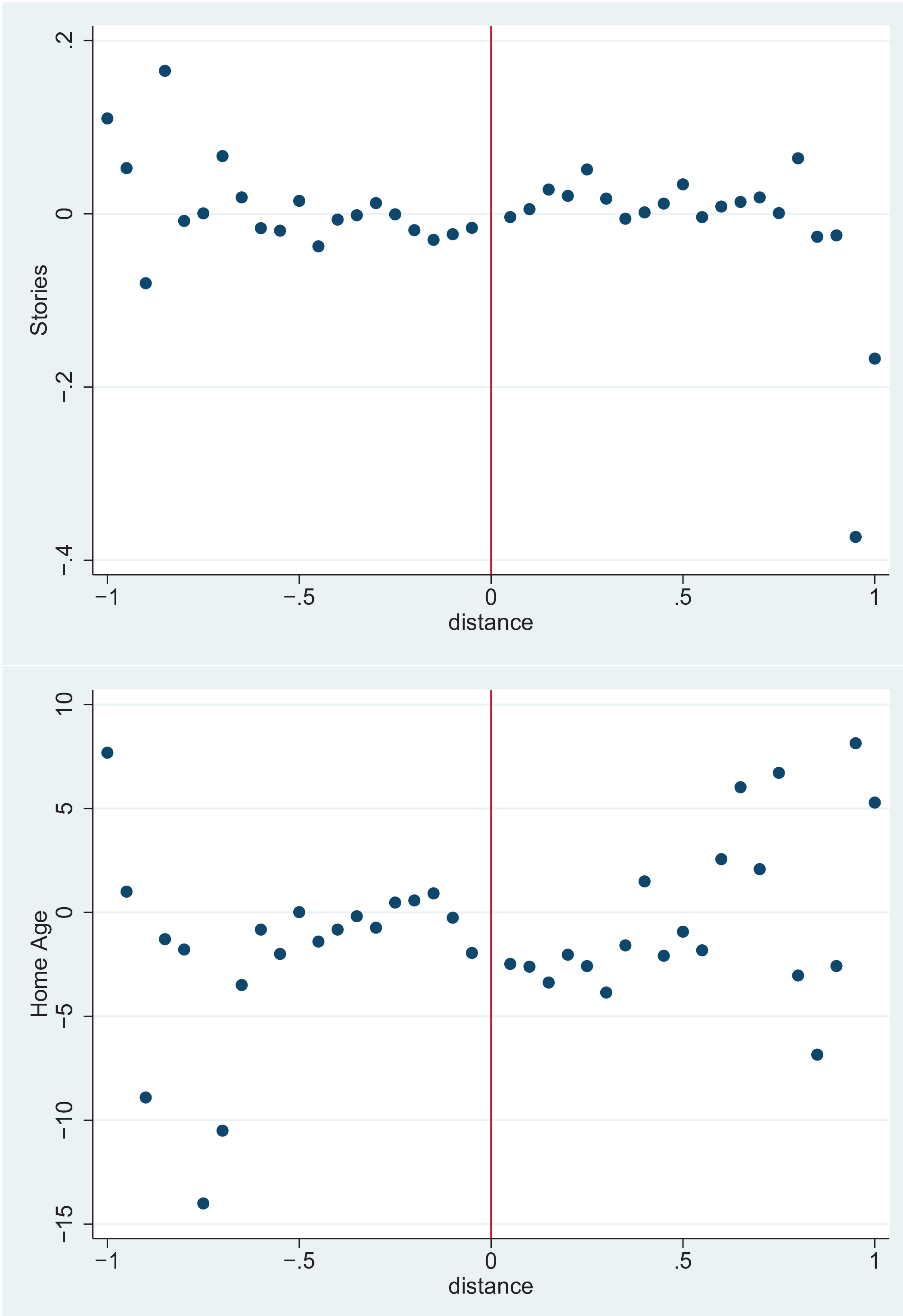
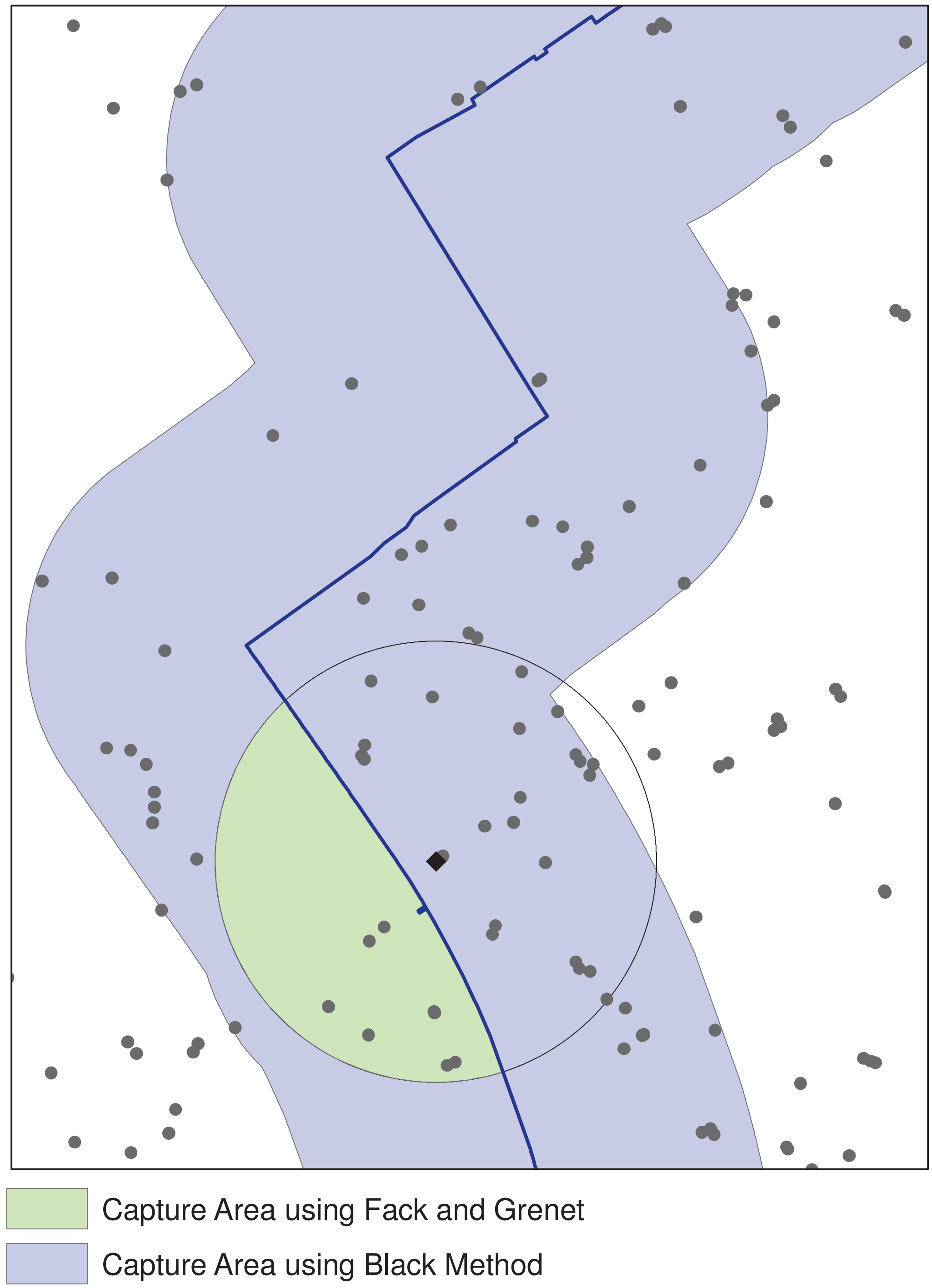

.2
0
−.2
−.4
Stories
−1
−.5
0
.5
1
distance
10
5
0
−5
−10
−15
Home Age
−1
−.5
0
.5
1
distance

Figure 2.6: Selecting Houses to Create a Counterfactual Sale



Chapter 3: Do Music Classes Lead to Improved Student Outcomes?

3.1 Introduction

In recent years, with many school districts facing sharp budget cuts, the importance of music education has been hotly debated. Often, music education is one of the first place schools districts look to make cuts in expenses. However, many argue that music has an important place in our children's education. Some contend that participation in music can lead to improved academic achievement. This paper investigates the relationship between high school music participation and educational outcomes. Specifically, I examine whether participation in high school music classes affects high school graduation, college attendance, high school GPA, SAT scores, and ACT scores. The results suggest that participating in music classes improves student outcomes along all of these dimensions. The largest effects are found for students who participate in band.

Theoretically, participation in high school band or chorus could have positive or negative effects on student outcomes. It is possible that, in the absence of music classes, students would be taking more math, science, or other academic courses. If more of these courses lead to improved academic acheivement, then music classes could distract students from more important subjects and consequently lowering their outcomes.

Other the other hand, music may promote creativity, work ethic, and abstract thinking. To the extent that these qualities lead to improved academic outcomes, then students who take music classes may be more likely than their non-music peers to graduate from high school and to attend college. Additionally, music classes could expose some students to high-achieving peers whom they otherwise might not interact with. If having friends who go to college increases one's own likelihood of attending college, then participating in music classes could improve outcomes through peer effects.

A variety of research examines the impact of music on academic achievement. Perhaps the most well-known is that which documents the "Mozart effect" (Rauscher and Ky, 1994). The authors find that after listening to a Mozart sonata for ten minutes, college students perform better on a spatial reasoning test. However, the students do not perform significantly better on any of the other tests administered during the experience. Additionally, despite many attempts to replicate these results, most studies are unable to find a significant effect of listening Mozart on more than one specific spatial reasoning test (see Steele et al. (1999) for a meta-analysis). In another experiment, Rauscher et al. (1997) find that children who received piano lessons scored significantly higher than the non-music group on an object assembly task.

Many other researchers have documented a positive correlation between participation in music and academic achievement. However, there is a strong possibility that this correlation is driven by selection into music. Music lessons are costly, and students from poor families often cannot afford these lessons. In general, students who participate in music tend to come from families at a higher socio-economic status (Elpus and Abril, 2011; Kinney, 2010). Because students from affluent families are also more likely to attend college or to receive higher test scores, a positive correlation between music participation and academic achievement does not necessarily imply a causal relationship.

To my knowledge, there has been no research on the relationship between music participation and academic achievement in the economics literature. In general, there is a very small literature that estimates the causal impact of music classes on academic achievement. I investigate the impact of music participation on student outcomes using both OLS with school fixed effects and propensity score matching.

There are advantages and disadvantages to both strategies. One strength of OLS is that I can estimate school fixed effects using this method, while propensity score matching cannot accommodate fixed effects. By estimating school fixed effects, I can control for the effect of each school on students' academic outcomes. However, one disadvantage of OLS is that it compares treated observations to the average of all untreated observations (or, to untreated individuals within the same school in the case of school fixed effects). Therefore, one might be hesitant to conclude that OLS is estimating a causal relationship. The advantage of propensity score matching over OLS is that matching will match treated students with only the comparison students who share propensity scores.

Additionally, one should be cautious when comparing the OLS with school fixed effects results to the propensity score matching results. OLS without school fixed effects restricts the average treatment effect on the treated (ATT) to be equal to the average treatment effect (ATE). In this case, the ATT is the average treatment effect of music courses on students who take music courses. Though it is not obvious whether OLS with school fixed effects still restricts ATT to be equal to ATE, OLS estimates the ATE and one must assume that the ATT is equal to the ATE in order to recover the ATT. Propensity score matching directly estimates the ATT. If the ATT is not equal to the ATE when using OLS with fixed effects, then the propensity score matching effects and OLS effects may not be comparable.

My primary outcome measures are whether the student graduates from high school, whether the student attends college, academic GPA, SAT score, and ACT score. Then, using college attendance as an example, the ATT is the difference in college attendance of a student who participates in a high school music class and what that student's likelihood of college attendance would have been if he or she had not participated in a high school music class.

In the next section, I describe the related literature. Section 3.3 describes the Data. Section 3.4 will present my empirical strategy. Section 3.5 discusses my results, and in Section 3.6, I conclude.

3.2 Literature

Though the relationship between music and academic achievement has not received much attention in the economics literature, it has not been ignored in other fields. Among the supporters of the music, there are two major lines of reasoning for why music education is important. The first group argues that music helps students become better at other subjects like reading and math. The second group argues that, whether or not music leads to improved perfomance in other academic subjects, music is important as its own subject. They argue that music is not to be justified by what it can do for reading and mathematics; instead, music education is justified by teaching what no other subject can teach. See Mark (2002, 2005) for a discussion of music education advocacy in the United States.

It is important to think about these two claims when researching this area. If music is not found to improve academic achievement, would this imply that music classes are not worthwhile? Furthermore, there is still a debate over what constitutes academic acheivement. I have carefully considered the dependent variables of interest for this paper with these claims in mind. When evaluating education programs, a common outcome variable is a student's test score.

The College Board in 2010 reported that students who take four years of arts and music classes while in high school score 102 points higher on the SAT than their non-music counterparts (Americans for the Arts, 2010). Elpus (2013) estimates the impact of high school music classes on SAT scores using ordinary least squares with school fixed effects. He finds that music classes have no impact on SAT scores.[1] Following this line of research, I estimate the impact of music participation on SAT scores, ACT scores, and students' academic GPA. But, even if music classes do not improve test scores, the question of whether music classes are important to education is still unanswered.

It is still unclear to researchers what some test scores are actually measuring. Whether they are a good measure of achievement is debatable. However, what economists and policy-makers generally agree on is that a desirable goal for all young adults is to increase educational attainment. Completion of high school improves labor market and earning opportunities in individuals' adult lives. Completion of college further improves these outcomes (Card, 1999). For these reasons, I also test whether participation in high school music classes leads to fewer high school dropouts and increases in matriculation to college.

There is a vast literature outside of economics supporting a positive relationship between music and several different academic outcomes. However, the extent to which studies are able to identify a causal relationship and not just a correlation varies dramatically (Babo, 2004; Cheek and Smith, 1999; Costa-Giomi,

[1]One should note that some of Elpus's control variables are likely to be endogenous. In particular, he defines treatment as taking a music class during any year of high school. However, the control variables for prior academic achievement are measured in 9th and tenth grade, meaning they could be affected by music participation. He also includes the amount of time spent watching TV and playing video games each week.

2004; Graziano et al., 1999; Helmrich, 2010; Legg, 2009; Schellenberg, 2004, 2006). Many researchers have noted that the positive relationship between music and academic achievement may be caused by selection bias (Kinney, 2008, 2010; Winner and Cooper, 2000). These researchers show that there are many systematic differences between music and non-music students prior to the students' participation in music classes. For example, students who select into music classes are more likely to be from wealthier families. They are also more likely to be from two-parent homes. Studies that do not account for these differences will suffer from selection bias.

The first research that, to my knowledge, uses econometric techniques to estimate a causal relationship is Elpus (2013). Elpus estimates the impact of participation in high school music classes on SAT scores using a regression approach with school fixed effects. He finds no effect of music classes as a whole on SAT scores. However, he does find a small effect of participation in band on SAT scores. I use this paper as a model for my own research. Like Elpus, I use the Education Longitudinal Survey of 2002. I also begin my analysis with a regression approach using school fixed effects. I extend Elpus's research by examining additional dependent variables, including college attendance, academic GPA, ACT scores, and high school completion. I additionally modify his empirical strategy by eliminating some of his control variables which are likey to be endogenous. Specifically, I eliminate tenth grade test scores as a control variable, because this is observed post-baseline. I also eliminate variables that indicate how many hours each week are spent watching TV, playing video games, or working.

I further extend the literature by using propensity score matching. Propensity score matching can be used to correct for selection bias, making it well-suited for answering this question. As compared to OLS with school fixed effects, propensity score matching has a less restrictive functional form. However, it must also satisfy several of its own strict assumptions. I will discuss these strengths and limitations in section 3.4. In the next section, I describe the data used for this study.

3.3 Data

The data for this research come from the Education Longitudinal Study of 2002 (ELS 2002). The purpose of the survey was to monitor the transition of a national sample of high school student as they progressed from tenth grade through high school and on to postsecondary school and/or work. It is a longitudinal study that follows the same students throughout their high school career and beyond.

Approximately 15,000 tenth grade students from 750 schools across the United States were surveyed in 2002. In the baseline survey, students were tested in reading and mathematics to provide a baseline achievement level. Students were also asked a variety of questions about their attitudes and experiences, such as their attitude toward school, whether they participated in extra-curricular activites, how much time they spend on homework, whether they had taken an Advanced Placement course, etc.

Students were surveyed again in 2004 and 2006. In the 2004 survey, information was gathered about colleges applied to, whether the student enrolled in a postsecondary school, and whether the student completed high school. Students were also asked about their employment and earnings. This survey provides the outcome measures for my analysis. My main outcomes of interest are whether the student completed high school, whether the student ever attended college, SAT score, ACT score, and four-year academic GPA.

In addition to the student information, parents were also surveyed in the base year to obtain additional information. I have information about mother's education, father's education, family income, family structure, socio-economic status, and a variety of other variables from the parent survey.

I also have access to students' transcript information, which lists every course in which the student was enrolled and the student's grade in the course. I make use of the transcript data in order to identify which students are music students. I use several definitions for a music student. The most general definition identifies music students as those students who earned credit for one or more music courses during high school. However, one may argue that the baseline controls variables could coincide with treatment if this definition is used. If a student participates in music in the 9th grade, then some control variables would not truly be "pre-treatment" variables. In order to ensure that all control variables are observed pre-treatment, I define treatment as participating in music in 10th, 11th, or 12th grade.

Unfortunately, this definition creates another difficulty. Defining treatment as participation in a music class in 10th, 11th, or 12th grades opens the possibility that students who took music classes in 9th grade will be in the comparison group. However, this definition is better than the alternative, because I am not explicitly using concurrently observed treatment and control variables. Additionally, while having 9th grade music students in the comparison group is not ideal, this should bias the estimates downward, which isn't a large concern. Selection bias should lead to estimates that are biased upward, so it is not too concerning to have a factor that biases the estimates downward.

An alternative to including 9th grade music students in the comparison group is to drop these students from the sample. I will do this in section 3.6. However, as I will show, it is not the ideal way to define the treatment and comparison groups.

The inability to obtain clearly defined treatment and control groups is certainly a drawback of this research. While the ELS data contain a great deal of information, surveys such as this could be improved by asking questions about past achievement and activities. A richer set of baseline variables could improve causal estimates. However, even with the addition of richer baseline variables, there would likely still be concerns about estimating a causal effect. If high school music affects academic achievement, then it's possible that elementary and middle school music also affects academic achievement. If this is true, then this creates the possibility that OLS has omitted variable bias and that the conditional independenc assumption is violated in propensity score matching. Unfortunately, these are not things I can test for with the data available. But it is something to keep in mind when interpreting the results.

In addition to estimating the effect of music participation on academic outcomes, I also separately estimate the effect of band or choir on academic outcomes. Elpus (2013) found different effects of music classes on SAT scores for band students versus choir students, and so I continue to extend his research by estimating the impact of band and choir on several additional outcomes. One might expect to see different results for band versus choir students. Band students are required to read music, whereas not all choir students need to have this skill. If reading music is similar to learning a language, then having this ability might cause the band students to think differently.

I make use of one final definition of a music student in this research. I examine students who earned one or more credits of music in 10th, 11th, or 12th grade. I use this definition of music to investigate whether the impact of music is primarily through students who take many music classes or if the effect is present for students who take any music classes.

In order to define music in so many different ways, I had to very carefully examine the ELS transcript data. With the help of an expert in both music education and the ELS 2002 data, I reviewed the transcript data to ensure that the information was properly coded. First, any courses coded as arts courses were reviewed to ensure that no music courses were incorrectly listed as a visual art, dance, or theater class. Second, within the music classes, each course had to be properly identified as a band, chorus, or "other music" course. Other music includes music theory, piano courses, general music courses, etc. See Elpus (2013) for a full review of the transcript data cleaning process.

3.3.1 Summary Statistics

Table 3.1 displays summary statistics from the data. Column 1 shows summary statistics for all students. Column 2 contains students who enrolled in at least one music class during any grade in high school, and column 3 shows student who did not participate in a music class. The music indicator is broken down into type of music student. I examine band students, choir students, students who earned more than 1 credit of music, and students who participated in music in at least the 10th, 11th, or 12th grade. Approximately thirty-two percent of students participated in at least one music class during high school. Elevent percent of students participate in band, and sixteen percent of students participate in chorus. Of the music students, only about half earn at least one full credit of music. Eighty percent of music students take at least one music class in 10th, 11th, or 12th grade.

Comparing columns 2 and 3, one can see that students who participated in music are more likely to attend college and more likely to attend a four-year college. Music students are less likely to drop out of high school. These students are also more likely to have received an academic honor and are less likely to have taken a remedial math or reading class. The music students' parents are more educated, have a higher family income, and are more likely to participate in a parent-teacher organization.

When one compares the characteristics of schools that music students attend, one sees that music students are more likely to attend a private school than a public school. They are also more likely to attend urban schools than rural or suburban schools.

The summary statistics are consistent with the patterns that exist in the literature. We see that music students are more likely to have higher academic achievement. However, given their advantage in socio-economic status and other areas, a careful analysis is required to determine whether there is a causal relationship between music participation and student outcomes. The next section describes my empirical approach for testing this relationship.

3.4 Empirical Strategy

I use propensity score matching to estimate the impact of high school band or chorus participation on student outcomes.

Define treatment as participation in a music class at the baseline. Let M_i=1 if a student is in the treatment group and 0 otherwise. Let Y_i represent the student outcome of interest. For ease of exposition, I consider dropping out of high school to be the main outcome of interest when I discuss the empirical methodology. $Y_{i,1}$ indicates whether student i dropped out of high school and received treatment, and $Y_{i,0}$ indicates whether she dropped out of high school and she did not receive treatment. I estimate the average treatment effect on the treated (ATT). While the average treatment effect (ATE) may be interesting, it represents the effect of treatment on an individual drawn randomly from the sample and is not easily estimated. The goal of this paper is not to say what would occur if all students were to take a music class. Instead, I am interested in the effect of music classes on those students who take them. Thus, the ATT is more suitable. The ATT can be expressed as follows:

$$ATT = E[Y_{i,1} - Y_{i,0}|M_i = 1] = E[Y_{i,1}|M_i = 1] - E[Y_{i,0}|M_i = 1]. \tag{3.1}$$

The right-hand side of equation (3.1) is the difference between whether a student drops out of high school if she participates in a high school music class and whether she drops out of high school if she does not participate in a high school music class.

The first term, $E[Y_{i,1}|M_i = 1]$, can be obtained from the data. However, the second term, $E[Y_{i,0}|M_i = 1]$, is not observable. This term represents whether the student would have dropped out of high school if we could go back in time and have her not participate in a music class. Though we cannot observe this term, matching allows me to estimate the term using the outcomes of students in the sample who did not participate in music classes. In essence, I match treated individuals to untreated individuals who are very similar on observable characteristics. Once individuals are matched, the students in the comparison group are as similar as possible to the treated group along observable characteristics, and treatment can be considered a random event. This requires that the conditional independence assumption be satisfied, which I explain in Section 3.4.1.

In practice, matching individuals based on observable characteristics can be problematic when there are a large number of characteristics on which to match. In order to match individuals, I would divide the data into subgroups based on observed characteristics. Each subgroup containing a treated individual would also need to contain an untreated individual. As the number of variables increases, the number of subgroups increases, and it becomes increasingly difficult to find match for each of the treated individuals.

To overcome this problem, Rosenbaum and Rubin (1983) introduced the propensity score. The propensity score, $p(Z)$, is defined as the conditional probability of treatment given a vector of observable characteristics, Z:

$$p(Z) = Prob(M_i = 1|Z)$$

.

The basic strategy of propensity score matching is to match individuals based on their propensity score, rather than on their observed characteristics. In this way, the dimensionality problem can be reduced to a single index.

Rosenbaum and Rubin show that, if $Y_{i,0}$ is independent of treatment status given Z, then $Y_{i,0}$ is also independent of treatment status given $p(Z) = Prob(M_i = 1|Z)$. As a result, matching can be performed on a single index $p(Z)$ instead of on all of the variable in Z.

3.4.1 Propensity Score Matching Methodology

3.4.2 Identification

Two identification assumptions are used for matching. The first is the conditional independence assumption (CIA). This assumption states that, conditional on observable characteristics, Z, the distribution of the outcome variable of interest for the treated group in the absence of treatment is the same as the distribution of the outcome variable of interest for the comparison group. It can be expressed as follows:

$$\text{CIA: } Y_{i,0} \perp M_i | Z.$$

Given the previous Rosenbaum and Rubin result, the CIA can be rewritten as

$$Y_{i,0} \perp M_i | p(Z).$$

The conditional independence assumption allows the counterfactual outcome for the treatment group to be inferred from the outcomes of the comparison group, and then any differences between the two groups can be attributed to treatment. For this assumption to be plausible, one must have available a large amount of variables that affect both treatment and the outcome variable. The ELS 2002 contains many variables that determine whether students participates in high school music classes, whether they graduate from high school, whether they attend college, and their test scores. This richness makes it well-suited for using matching techniques.

The second identification assumption is the common support condition. This condition states that there must be substantial overlap between the treatment and comparison group in the value of the propensity score. If, for example, the treatment group is very likely to receive treatment, then there may not be an adequate comparison group for the treated group. For estimating the ATT, I need a positive probability of observing untreated individuals at each value of Z, $P(M = 1|Z) < 1$. If $P(M = 1|Z = z_0) = 1$, then at $Z = z_0$, I would observe only treated individuals. Because there are many students who do not participate in high school music classes, the common support condition is not an issue in this context.

3.4.3 Implementation

To implement propensity score matching, I break the process down into two steps. The first step is to estimate the propensity score. I estimate the propensity score using a probit model. I select the conditioning variables to control for factors expected to affect both the decision to participate in high school band or chorus and whether the student graduates from high school and attends college.

The propensity score specification varies slightly, depending on the definition of treatment, so that the balancing test (which I will describe in detail later) is satisfied. In general, the specification will include age, race, gender, whether the student had a computer at home, the region in which the school is located (Northeast, South, West, or Midwest), and the student's 9th grade GPA. I also include characteristics of the student's family, such as the mother's education, father's education, family income, and family's socio-economic status. Additionally, I include several characteristics of the student's school. These include whether the school is public or private; whether the school is urban, suburban, or rural; the percent of teachers who are certified to teach in their field; the percentage of students who receive free or reduced lunch; and the percentage of student who have limited English proficiency.

The next step in the estimation procedure is to match individuals based on their propensity score and estimate the ATT. Applied economists have used many different matching estimators. The choice of a matching estimators determines two factors. It determines how an individual is matched to a group of comparison individuals. It also determines the weighting approach for how the weighted outcome of the comparison group will be computed and then assigned to a treated individual.

I use kernel matching.

$$\hat{y}_i = \frac{\displaystyle\sum_{j \in \{M=0\}} K(\frac{p_i - p_j}{h}) y_j}{\sum_{j \in \{M=0\}} K(\frac{p_i - p_j}{h})},$$

where p_i is the treated individual's propensity score, p_j is a comparison individual's propensity score, $K()$ is the kernel used, and h is the bandwidth. Treated individuals are matched only with comparison individuals where $p_i - p_j$ falls within this bandwidth. Then, within the bandwidth, comparison individuals whose propensity score is closer to that of a treated individual will receive higher weight. I use the Epanechikov kernel in the matching procedure. The bandwidth is 0.1.[2] After matching the individuals, the estimates of $\hat{y}_i$ will be used to estimate the effect of high school band or chorus on student outcomes.

3.5 Results

3.5.1 OLS Model

I begin by presenting results of an Ordinary Least Squares regression of the outcome variables of interest on a student's participation in high school music classes in 10th, 11th, or 12th grade. The regression takes the form

$$Y_i = \beta M_i + \delta Z_i \delta + \epsilon_i, \tag{3.2}$$

where M_i is a dummy variable that indicates whether the student participated in a high school music class in 10th, 11th, or 12th grade, Z_i is a matrix of individual and family characteristics, and Y_i is a dummy outcome variable. For high school drop

[2] I estimated the results using bandwidths varying from 0.05 to 0.15, and the results were not sensitive to choice of bandwidth.

outs, Y_i is 1 if the student dropped out of high school and 0 otherwise.[3] For college attendance, Y_i is 1 if the student ever attended college and 0 otherwise. Other dependent variables include the student's SAT score, ACT score, and academic GPA.

Table 3.2 displays the results from estimating equation (3.2). Panel A displays a baseline estimation of the effect of music classes in 10th, 11th, or 12th grade on various outcome variables; no control variables are included in Panel A. Panel B esimates this relationship controlling for individuals and family characteristics. Panel C includes controls for 9th grade GPA in addition to individual and family characteristics. Panel D further adds school fixed effects. Panel E is the same as Panel D, except that school characteristics are included as control variables in place of the school fixed effects. Propensity score matching will not allow for school fixed effects due to dimensionality constraints. Instead, I can only include school characteristics. Panel E should be compared to Panel D to see how the estimates differ when school characteristics are used as control variables in place of school fixed effects.

Finally, Panel F includes control variables for hours spent watching television or playing video games each week. This panel is included as a comparison to Elpus (2013)'s research. In his regression, his preferred estimates include these additional variables. Though these variables are likely endogenous, I include them as a comparison to his previous work.

For all estimates, standard errors are clustered at the school level. Each cell in the table represents a separate regression.

[3]One may be concerned about using dropping out as an outcome variable if many students are dropping out of high school very early on. Fortunately, only approximately fifteen percent of students who drop out are doing so prior to their junior year. I will eliminate from my sample those students who drop out prior to 11th grade. This eliminates 130 students.

The outcome variables are whether the student drops out of high school, whether the student attends a post-secondary institution, whether the student attends a four-year college, the student's SAT score, the student's ACT score, and the student's academic GPA. The academic GPA is defined as the students GPA in only academic courses. Thus, grades in music classes and/or other non-academic courses have been removed from the calculation.

Panel A shows a strong correlation between participating in high school music classes and all of the outcome variables. Student who participate in music are eleven percentage points more likely to attend college. Students who take music classes are approximately four percentage points less likely to drop out of high school. The correlation is strongest for band students and for students who earned at least one full music credit.

In Panel B, when individual and family control variables are included, the magnitudes of the coefficients decrease. In Panel C, when control for 9th grade GPA is included, the magnitudes of the coefficients decline dramatically. Most of the coefficients are at least five times smaller than in the baseline estimation. These results are all consistent with the idea that the correlation between music and academic achievement is driven partly by selection.

Panel D builds off of Panel C by additionally estimating school fixed effects. This panel contains my preferred OLS specification. This panel contains the richest set of controls without including potentially endogenous variables. Additionally, the school fixed effects should control for the effect of a student's school on both music participation and academic achievement. The coefficient on music participation is smaller for nearly every definition of music student and for nearly every outcome variable than in the baseline correlation. The results indicate that participating in music increases the likelihood of attending a postsecondary institution and improves academic GPA. Music classes also appear to affect whether a student will drop out of high school. Based on the averages of the variables, the results suggest that taking a music class leads to a four percent increase in attending a postsecondary institution and improve academic GPA by two percent.

The relationship between music and academic achievement is strongest for band students. The results indicate that participating in band increases the likelihood of attending a postsecondary school by six percent and increases the likelihood of attending a four-year college by seven percent. Band students also see increases in both SAT and ACT scores of approximately two percent.

Panel E replaces the school fixed effects in Panel D with school characteristics. These variables are whether the school is public or private; whether the school is urban, suburban, or rural; the percent of teachers who are certified to teach in their field; the percentage of students who receive free or reduced lunch; and the percentage of student who have limited English proficiency. Although in theory, these characteristics can vary over time, in practice they do not vary enough to be included in the regression along with school fixed effects. Therefore, a regression with school fixed effects does a better job than a regression with this set of school characteristics at capturing variation across schools in factors that may affect achievement and that are correlated with taking music classes. But because I am not able to estimate school fixed effects with the propensity score matching approach, it is useful to explore the differences that arise when school characteristics are included in the regression in place of school fixed effects.

Overall, the estimates that use school characteristics as controls are fairly similar to the estimates that arise from using school fixed effects. The most similar estimates are those where the dependent variable is attending a postsecondary institution, attending a four-year college, and academic GPA. When the dependent variable is dropping out, the estimates with school fixed effects are slightly larger, though the estimates that use school characteristics are generally not statistically significant. The most notable discrepancy between the estimates is in the estimated effect of music on ACT and SAT scores. Without school fixed effects, the estimated treatment effects are much larger than when these effects are estimated with school fixed effects. Because of this discrepancy, I am cautious in concluding that propensity score matching is truly estimating the causal impact of music classes on ACT or SAT scores.

Panel F further includes time-use control variables, which are hours per week spent watching television and hours per week spent playing video games. Though these variables are likely endogenous, I have included them because they are used in the research done by Elpus (2013). The specification used in Panel E is nearly identical to that used by Elpus. His only outcome variable is SAT score, and my results are very similar to his results when SAT score is the dependent variable. Whether these variables are endogenous or not, they have little impact on the results. I exclude these controls from future regressions.

Though I have included many control variables and a full set of school fixed effects, one might still be wary of the results. Based on the summary statistics, we know that students who participate in high school music also tend to be from more educated and more affluent families. Since students who participate in music differ from non-music students along observable characteristics, it is also likely that they differ along unobservable characteristics. OLS is restrictive in its functional form and, without fixed effects, requires that the Average Treatment Effect on the Treated be equal to the Average Treatment Effect. Because I am estimating school fixed effects, it is not clear exactly what treatment effect is being estimated and if ATT is still equal to ATE in this case. In order to address these restrictions, I now use a different method of estimating the causal effect of music classes on academic achievement. The next section discusses the results of propensity score matching.

3.5.2 Propensity Score Matching Results

The first step of propensity score matching is to estimate the propensity score. Table 3.3 presents the probit marginal effects estimates of the propensity score for each definition of music student. Compared to non-music students, music students are more likely to be female, more likely to be black, have a higher 9th grade standardized test score, and are more likely to live in a two-parent home. In addition, music students' parents are more educated, though they are not likely to have a higher income.

Among students who did not participate in music, some have a predicted probability of participating in music that is lower than the predicted probability of music participationg among students in the treatment group. I enforce common support by excluding students with propensity scores lower than the minimum of the treatment group. This excludes approximately 40-50 students, depending on the definition of music student. I also exclude students who attend a school that does not offer music classes.

Tables 3.4 through 3.6 present results of the balancing test of the hypothesis that the mean of each variable is equal between the treatment and comparison groups. The definition of treatment is different in each table. The balancing test ensures that, on average, students in each group have similar observable characteristics. For all variables, the results of a paired t-test indicate that the means of each variable for the treatment and comparison groups are not statistically different. The balancing condition is safisfied for all definitions of music student.

Table 3.7 displays the results of the propensity score matching estimates of the effect of 10th, 11th, or 12th grade music participation (also sub-divided into band or choir participation) on six measures of academic achievement. Each cell in the table represents a separate estimation procedure. Thus, cell one shows the estimate of the impact of taking at least one music class on whether a student drops out of high school. Refer to Table 3.3 to see the full list of covariates for each specification. Standard errors are block bootstrapped by school using 800 replications. The bandwidth is 0.1. Results using a bandwidth of 0.05 or 0.15 are similar but not displayed. Neither the coefficient magnitudes nor the level of statistical significance is very sensitive to choice of bandwidth.

Recall that the OLS model without fixed effects estimates the average treatment effect and restricts the average treatment effect to be equal to the average treatment effect on the treated. It compares the average high school drop out rate and college attendance rate for music students and non-music students.[4] Propensity score matching identifies a slightly different effect, the average treatment effect on the treated. The ATT is an estimate of the effect of participating in band or chorus on high school outcomes among students who participated in band or chorus. Unlike OLS, the propensity score matching method eliminates non-music students who are too different from the music students.

For students who took at least one music class in 10th, 11th, or 12th grade, the estimated treatment effect is generally slightly larger than what was estimated with both OLS with school fixed effects and OLS with school characteristics in place of fixed effects. This finding could have a few different explanation.

It could be that the treatment effect is in fact larger than what the OLS es-

[4]With fixed effects, it is not clear exactly what treatment effect is being estimated. I have estimated the effects both with and without school fixed effects. Only for the estimations without fixed effects am I definitively estimating the ATE.

timates suggested. If one wants to infer the ATT from OLS estimates, one must assume that the ATT is equal to the ATE. Propensity score matching does not force such a restriction, which could allow the ATT to be closer to its true value. Alternatively, it might be the case that school fixed effects are necessary to estimate a causal effect and that, without them, propensity score estimates are still biased upward. Unfortunately, it is not possible to know for sure which is true. However, even though propensity score matching yields somewhat higher estimates, they are not dramatically higher, which suggests that the estimates may be close to estimating the causal effect.

The results suggest that taking at least one music class lowers the likehood of dropping out of high school by about eight tenths of a percentage point. The mean drop-out rate is 5.2%, suggesting that participation in at least one high school music class lowers the likelihood of dropping out of high school by fifteen percent.

Students who took at least one music class are also three percentage points more likely to attend a postsecondary institution (which is similar to the treatment effect with OLS) and three percentage points more likely to attend a four-year college. Based on the mean levels of these variable, this implies a four percent increase in postsecondary school attendance and a seven percent increase in matriculation to a four-year college. Taking a music class also improves academic GPA by nearly a tenth of a point, which is a three percent increase.

The effect of music classes on ACT and SAT scores is very similar to what was estimated using OLS with school fixed effects. This is true for music classes in general and for band students. For choir students, the estimated coefficients are much larger when using propensity score matching. It is not obvious why this would be the case.

The largest effects are found for students who participated in band. Participation in band has a statistically significant effect on every academic achievement outcome. Band students are nearly eight percentage points more likely to attend a postsecondary institution. This effect is twice the size of the impact of at least one music class on attendance at a postsecondary institution. Band also improves SAT scores by more that thirty points and improves ACT scores by more than half of a point. Additionally, participating in band improves one's academic GPA by almost two tenths of a point.

There are several reasons why one might expect to see the largest effects for band students. First, participation in band requires discipine. Student must practice their instrument on their own. They must pay attention during rehearsals. They may also be required to attend activities outside of the school day. Also, band requires a special set of skills. Unlike students in other music classes, band students must read music. While reading music is helpful for choir, it is not necessary. It is possible that the ability to read music improves students' academic abilities.

Both the propensity score matching estimates and the OLS estimates suggest that music classes have large effects on academic outcomes. In the next section, I use two different definitions of treatment to further explore this relationship.

3.6 Alternative Definitions of Treatment

3.6.1 Music in 10th, 11th, or 12th Grade Only

For both OLS and for propensity score matching, it is important that any control variables be observed prior to treatment. Therefore, I defined treatment to be taking a music class in 10th, 11th, or 12th grade, so that any variables observed in 9th grade could be used as control variables.

An alternative way to define treatment would be to restrict treatment to those who participated in music in 10th, 11th, or 12th grade *only*. The advantage of this definition is that the treatment group very clearly did not receive treatment prior to the baseline. However, this definition of treatment also has several problems.

First, by defining treatment as taking a music class in *only* 10th, 11th, or 12th grade (and specifically *not* in 9th grade), I am putting any student who took a music class in 9th grade in the comparison group. This is far from an ideal division of treatment and comparison groups. When music participation is defined as taking a music class in *only* 10th, 11th, or 12th grade, seventy percent of students that took at least one music class at any time in high school are considered to be in the comparison group. Said another way, seventy percent of students who take a music class in high school take a music class in 9th grade.

Additionally, students who take a music class in 9th grade actually take more music classes on average than the treatment group. Forty percent of students who took a music class in 9th grade earn three or more credits of music. Fifteen percent earned 4.5 credits or more. Among students who took at least one music class in any grade, only twenty-five percent earned three or more credits in music, and only ten percent earned 4.5 credits or more. Only five percent of students who took music in 10th, 11th, or 12th grade only earned three or more credits of music. Additionally, the sample of students who only took a music class in 10th, 11th, or 12th grade is fairly small compared to the number of student who took a music class in any grade. While thirty-two percent of all students took at least one music class, only eleven percent of students took a music class in 10th, 11th, or 12th grade only.

In addition, students who take music in 10th, 11th, or 12th grade are not representative of music students in general. Table 3.8 displays summary statistics for all students, music students, non-music students, students who took a music class in 10th, 11th, or 12th grade only, and for students who either did not take music or did not take music in only 10th, 11th, or 12th grade (which would be students who took music classes in at least 9th grade).

Along individual and family characteristics, the students who took music only in 10th, 11th, and 12th grade are often more similar to the general student population than they are to students who took at least one music class. Additionally, twenty-five percent of students who did not take music in only 10th, 11th, or 12th grade did take at least one music class. Thus, there are many treated students in the comparison group.

To address the issue of treated students being included in the control group, I can exclude from the sample any student who took a music class in 9th grade. This would exclude about twenty-two percent of the total students and approximately seventy percent of the students who took at least one music class. The results from this estimation are displayed in Table 3.9. The results from the full sample are presented as a comparison. The results from the restricted sample are surprisingly similar to the results from the full sample. This suggests that the results may not be driven by the 9th grade students who are taking a lot of music classes. One might expect that eliminating students who take the most music classes (9th grade music students) from the sample would diminish the magnitude of the coefficients. Because this does not occur, a natural question is whether more music classes lead to more improved outcomes or whether the full effect of music classes on academic achievement can be captured by just one music class.

3.6.2 Students Earning One or More Music Credit

In order to explore whether the intensity of music participation affects the relationship between music classes and academic outcomes, I now define treatment as earning one or more full credit in music during one's high school career. One credit of courses is equivalent to one year. Therefore, these students have either taken at least one full year of music or have taken multiple courses that add up to one credit of music. Approximately fifty-five percent of students who take at least one music class earn one or more full credit of music. By defining treatment in this way, I can explore whether the effect of music classes on academic achievement is greater for students who take more music classes.

Table 3.10 displays the results of this estimation, using both OLS and propensity score matching. The estimate of the impact of any high school music class on academic outcomes is also shown as a comparison. Looking at the OLS results, the magnitudes of the coefficients are larger when the treatment is taking one or more music credit compared to taking any music class. This finding suggests that more music classes lead to even better outcomes. The results are similar when using propensity score matching. With the exception of attending a four-year college, taking one or more credits of music has a larger impact on academic outcomes than does taking at least one music class.

I can directly test this hypothesis by regressing academic outcomes on the number of music credits earned in high school. Table 3.11 displays these results. In addition to number of music credits, I also include music credits squared to investigate whether the effect changes depending on how many credits one has.

The results indicate that earning more music credits improves academic outcomes. Whether this effect dimiinishes with the number of credits earned is unclear. The coefficient on music credits squared is not statistically significant in any regression, but it is consistent in some cases with a dimishing treatment effect.

3.7 Extension to the Effect of Participating in Extracurricular Activities

A natural question is whether the approaches taken here could be used to measure the effect of participating in extracurricular activities on academic achievement. Consider the standard matching estimators to estimate the treatment on the treated of extracurricular activities. There are two problems in applying matching here. First, eighty-four percent of students partake in extracurricular activities, leaving only sixteen percent of the students as the comparison group, and it is well known that matching estimators do poorly in this situation (see, e.g., Ham et al. (2011)). Secondly my view is that those not partaking in any extracurricular activities will make up a very select group, i.e. much more select than taking or not taking music classes, and I only have 9th grade GPA to deal with the selection. Thus, I decided not to pursue matching estimators.

Standard OLS estimates of the the effect of partaking in extracurricular activities can only control for selection through school fixed effects, and again this approach will provide unconvincing estimates. Perhaps the best approach is to use a regression which conditions on both 9th grade GPA and school fixed effects. As in the current chapter, this can be thought of as a simple matching estimator. I ran this regression and found the effect of partaking in extracurricular activities to have positive effects on academic achievement. Parcipation in at least one extracurricular activity is estimated to lead to a nine percentage point decrease in dropping out of high school, a nineteen percentage point increase in attendance at a postsecondary institution, a tenth of a point increase in academic GPA, and a half point increase in ACT scores. However, I am not convinced that this approach sufficiently deals with selection, and thus have chosen not to make this analysis into a separate chapter.

3.8 Conclusion

This research investigates the impact of high school music classes on student academic achievement. Very little research has been able to estimate a causal relationship between music classes and academic outcomes. I find evidence that while selection bias is driving some of the positive correlation between music and academic achievement, there seems to be a strong causal impact of music class on academic outcomes.

I utilize two empirical approaches to estimate a the causal relationship between music classes and academic outcomes. First, I use OLS with school fixed effects. Second, I use propensity score matches. Each approach has advantages and disadvantages. For both approaches, a drawback is that the data disclose nothing about a student's prior musical activities. I must assume that the treatment effect estimates the impact of high school music classes on academic outcomes. However, it is likely that students who take high school music classes also took middle school music classes. And if these middle school music classes also have a positive impact on academic outcomes, then this research will overstate the treatment effect.

When comparing OLS with school fixed effects and propensity score matching, the advantage of OLS with school fixed effects is the ability to include fixed effects. When I replace fixed effects with school characteristics, the estimated treatment effects are slightly higher, suggesting that school characteristics will not capture all of the effect of individuals' school on academic outcome. The disadvantages of OLS are its restrictive functional form and the fact that it generally restricts the average treatment effect on the treatment to be equal to the average treatment effect (though this may not be true once fixed effects are estimated). Propensity score matching does not restrict the average treatment effect on the treated to be equal to the average treatment effect, and it allows for a less restrictive functional form. However, propensity score matching is unable to accomodate school fixed effects. Because of this, the matching estimates may be biased upward.

The results suggest that taking at least one music class in high school leads to a fifteen percent decrease in the likelihood of dropping out of high school. Taking at least one music class also leads to large increases in enrollment at a postsecondary school, increases in enrollment at a four-year college, and small increases in students' academic GPA. Taking at least one music class also leads to approximately two percent improvement in ACT or SAT test scores.

The largest effects are found for students who participate in high school band. High school band students are between five and eight percentage points more likely to attend a postsecondary school and approximately five percentage points more likely to attend a four year college. Band students also see statistically significant increases in SAT and ACT scores, scoring approximately three percent higher on both tests. Participating in high school band also leads to increases of about two-tenths of a points in students' academic GPA. There are several reasons why the band students might see larger results. One reason is because band students are required to read music. Like speaking another language, reading music requires individuals to think in different ways. Another reason that band students might see more improvements over other music students is that band students will generally need to practice outside of school. This requires discipline and a more intense study of music. Both of these factors could lead to improved outcomes.

The propensity score matching results are similar in magnitude to the OLS results, though the estimated treatment effects are slightly larger when using propensity score matching. This finding suggests that with a large number of control variables, OLS with school fixed effects does quite well in estimating the effect of music classes on student academic outcomes. It also suggests that propensity score matching may overestimate the treatment effect, as it cannot be estimated with school fixed effects.

In addition to the positive impact of taking any music class on academic outcomes, I find that taking more music classes leads to even more improved outcomes. Each credit of music earned increases the likelihood of attending college by one percentage point. Earning more music credits effects similar gains on the other academic outcomes.

This paper demonstrates a strong positive effect of taking music classes on improving academic outcomes. The results are robust to different specifications, different samples, and different definitions of music student. Further research should further explore the types of music that are most beneficial. Additional work exploring the impact of the intensity of music study would also be beneficial.

Table 3.1: Summary Statistics

	(1) All Students	(2) Music Students	(3) Non-Music Students
Music Variables:			
Music Student	0.324	1.000	
	(0.46785)		
Choir Student	0.156	0.482	
	(0.36276)	(0.49972)	
Band Student	0.111	0.343	
	(0.31414)	(0.47478)	
At Least 1 Music Credit	0.177	0.546	
	(0.38138)	(0.49793)	
Music in 10th, 11th or 12th Grade	0.258	0.798	
	(0.43760)	(0.40175)	
Academic Outcomes:			
ACT	21.5	22.1	21.2
	(5.05)	(5.05)	(5.01)
SAT	1005	1027	991
	(207.26)	(205.72)	(207.02)
Dropout	0.052	0.026	0.064
	(0.22136)	(0.15868)	(0.24488)
Attend Postsecondary School	0.753	0.829	0.715
	(0.43136)	(0.37698)	(0.45138)
Attend 4-yr College	0.451	0.529	0.413
	(0.49761)	(0.49921)	(0.49232)
Individual Characteristics:			
Female	0.502	0.584	0.463
	(0.50001)	(0.49299)	(0.49865)
Black	0.158	0.151	0.161
	(0.36479)	(0.35830)	(0.36782)
Asian	0.116	0.111	0.119
	(0.32063)	(0.31366)	(0.32389)
Other Race	0.121	0.085	0.139
	(0.32631)	(0.27870)	(0.34544)
Hispanic	0.144	0.102	0.165
	(0.35135)	(0.30208)	(0.37089)
English Native Lang.	0.831	0.873	0.811
	(0.37447)	(0.33310)	(0.39140)
Academic Honor	0.353	0.407	0.326
	(0.47783)	(0.49138)	(0.46894)
Club Participation	0.238	0.282	0.216

	(0.42572)	(0.44987)	(0.41177)
Ever took Remedial Course	0.115	0.109	0.118
	(0.31874)	(0.31142)	(0.32218)
Family Characteristics:			
Two Parents	0.749	0.764	0.741
	(0.43384)	(0.42482)	(0.43793)
Mother HS	0.268	0.252	0.276
	(0.44303)	(0.43395)	(0.44711)
Mother Some College	0.329	0.345	0.322
	(0.46999)	(0.47531)	(0.46727)
Mother College	0.183	0.208	0.172
	(0.38707)	(0.40572)	(0.37729)
Mother $\geq$College	0.089	0.105	0.082
	(0.28513)	(0.30706)	(0.27371)
Father HS	0.281	0.278	0.283
	(0.44963)	(0.44811)	(0.45037)
Father Some College	0.264	0.269	0.261
	(0.44058)	(0.44341)	(0.43922)
Father College	0.178	0.192	0.171
	(0.38259)	(0.39426)	(0.37670)
Father $\geq$College	0.140	0.166	0.127
	(0.34667)	(0.37239)	(0.33294)
Parent in PTO	0.262	0.302	0.241
	(0.43953)	(0.45917)	(0.42765)
Mother Native US	0.779	0.819	0.760
	(0.41472)	(0.38547)	(0.42734)
Father Native US	0.780	0.815	0.762
	(0.41419)	(0.38800)	(0.42566)
Family has Computer	0.888	0.909	0.878
	(0.31506)	(0.28737)	(0.32748)
\$25k $\leq$Fam. Inc. $\leq$50k	0.305	0.295	0.309
	(0.46032)	(0.45618)	(0.46224)
\$50k $\leq$Fam. Inc. $\leq$75k	0.204	0.214	0.199
	(0.40291)	(0.41028)	(0.39927)
Family Inc.$\geq$75k	0.281	0.308	0.269
	(0.44969)	(0.46182)	(0.44322)
SES	0.042	0.144	-0.007
	(0.74301)	(0.72596)	(0.74614)
School Characteristics:			
Private	0.204	0.258	0.195
	(0.4028)	(0.4377)	(0.3964)
Urban	0.346	0.387	0.339
	(0.4756)	(0.4873)	(0.4734)

Rural	0.173	0.161	0.175
	(0.3781)	(0.3679)	(0.3797)
Suburban	0.482	0.451	0.486
	(0.4997)	(0.4978)	(0.4998)
% Ltd English Proficiency	4.28	3.31	4.44
	(8.34)	(7.56)	(8.45)
% Free Lunch	24.34	25.42	24.17
	(25.57)	(27.29)	(25.29)
% Teachers Certified	91.97	90.76	92.15
	(18.28)	(20.20)	(17.96)
Region:			
Northeast	0.180	0.188	0.176
	(0.38390)	(0.39076)	(0.38052)
South	0.367	0.331	0.385
	(0.48210)	(0.47054)	(0.48658)
West	0.199	0.177	0.210
	(0.39962)	(0.38133)	(0.40764)
N	15,370	4,970	10,400

Table 3.2: OLS Estimation of the Relationship between Music Classes and Academic Outcomes

Dependent Variable:	Dropout	Attends College	Attends 4-year College	Academic GPA	ACT	SAT
Panel A: Baseline Estimates with No Control Variables						
Music in 10th, 11th or 12th Grade	-0.03866***	0.11127***	0.11234***	0.30273***	0.90553***	37.15787***
	(0.00408)	(0.00838)	(0.00957)	(0.01537)	(0.11354)	(4.66418)
Choir in 10th, 11th or 12th Grade	-0.03285***	0.08419***	0.09718***	0.27274***	0.54042***	22.50513***
	(0.00540)	(0.01107)	(0.01267)	(0.02029)	(0.14775)	(6.06945)
Band in 10th, 11th or 12th Grade	-0.03672***	0.14228***	0.15130***	0.42664***	1.51376***	61.95682***
	(0.00636)	(0.01289)	(0.01472)	(0.02372)	(0.16507)	(6.78114)
Panel B: Panel A + Individual and Family Control Variables						
Music in 10th, 11th or 12th Grade	-0.01785***	0.05379***	0.05934***	0.16919***	0.73950***	30.12774***
	(0.00372)	(0.00878)	(0.01174)	(0.01685)	(0.13011)	(5.25966)
Choir in 10th, 11th or 12th Grade	-0.01439***	0.03099***	0.05246***	0.10903***	0.34418**	14.19000**
	(0.00417)	(0.01157)	(0.01534)	(0.02164)	(0.15662)	(6.29309)
Band in 10th, 11th or 12th Grade	-0.01588***	0.07710***	0.08093***	0.27752***	1.19126***	48.59593***
	(0.00480)	(0.01052)	(0.01667)	(0.02221)	(0.17065)	(6.85798)

Panel C: Panel B + Prior Academics

Music in 10th, 11th or 12th Grade	-0.00844**	0.03135***	0.02315**	0.07449***	0.59890***	24.23151***
	(0.00359)	(0.00868)	(0.01164)	(0.00793)	(0.11709)	(4.71930)
Choir in 10th, 11th or 12th Grade	-0.00833**	0.02145*	0.03442**	0.06517***	0.28969**	11.81617**
	(0.00415)	(0.01115)	(0.01467)	(0.00997)	(0.13813)	(5.50795)
Band in 10th, 11th or 12th Grade	-0.00091	0.03380***	0.01537	0.08536***	0.68074***	27.66940***
	(0.00479)	(0.01023)	(0.01634)	(0.01080)	(0.15361)	(6.14033)
Panel D: Panel C + School Fixed Effects						
Music in 10th, 11th or 12th Grade	-0.00850**	0.03143***	0.01554	0.07284***	0.43018***	17.23253***
	(0.00409)	(0.00969)	(0.01103)	(0.00839)	(0.11943)	(4.81707)
Choir in 10th, 11th or 12th Grade	-0.00954**	0.01422	0.02540*	0.05826***	0.03765	0.92225
	(0.00483)	(0.01254)	(0.01413)	(0.01077)	(0.14073)	(5.61882)
Band in 10th, 11th or 12th Grade	0.00089	0.04377***	0.03191**	0.08985***	0.85376***	34.28036***
	(0.00546)	(0.01165)	(0.01612)	(0.01100)	(0.16579)	(6.64681)
Panel E: Panel C + School Characteristics						
Music in 10th, 11th or 12th Grade	-0.00678*	0.03098***	0.02620**	0.07051***	0.59982***	24.35752***
	(0.00393)	(0.00931)	(0.01176)	(0.00835)	(0.12608)	(5.07875)
Choir in 10th, 11th or 12th Grade	-0.00602	0.01409	0.02856*	0.05504***	0.23957	9.93506*
	(0.00458)	(0.01198)	(0.01507)	(0.01029)	(0.15000)	(5.99733)
Band in 10th, 11th or 12th Grade	-0.00223	0.04736***	0.03800**	0.08716***	0.92630***	37.93483***

	(0.00521)	(0.01120)	(0.01695)	(0.01155)	(0.16110)	(6.44785)
Panel F: Panel D + Time-Use Variables						
Music in 10th, 11th or 12th Grade	-0.00795*	0.02951***	0.00822	0.07065***	0.38460***	15.41918***
	(0.00413)	(0.01009)	(0.01144)	(0.00876)	(0.11942)	(4.81298)
Choir in 10th, 11th or 12th Grade	-0.00712	0.01391	0.01894	0.05397***	-0.01944	-1.27368
	(0.00508)	(0.01283)	(0.01445)	(0.01104)	(0.14535)	(5.79656)
Band in 10th, 11th or 12th Grade	-0.00225	0.04659***	0.02831*	0.08977***	0.84580***	33.95100***
	(0.00514)	(0.01199)	(0.01680)	(0.01151)	(0.16734)	(6.71208)

(1) *** p<0.01, ** p<0.05, * p<0.1

(2) Standard errors are clustered at the school level.

(3) Individuals and family control variables are: gender, race, Hispanic indicator, native English speaker, whether lives in a two parent home, father's education, mother's education, family income, family socio-economics status, whether family has a computer, whether parents are a part of the parent teacher organization, and region.

(4) Prior academic control variable is ninth grade GPA.

(5) Time-Use variables are: hours per week spend watching TV, hours per week spent playing video games

(6) Treatment is taking a music/band/choir class in 10th, 11th, or 12th grade.

Table 3.3: Propensity Score Estimation Results

	(1) Music Student	(2) Band Student	(3) Choir Student
9th Grade GPA	0.04623***	0.04339***	0.02587***
	(0.00685)	(0.00444)	(0.00789)
Female	0.08044***	0.00396	0.16166***
	(0.00964)	(0.00607)	(0.02848)
Black	0.04325**	0.02010*	0.02111
	(0.01791)	(0.01119)	(0.01389)
Asian	0.02813	0.02790*	0.01016
	(0.02247)	(0.01652)	(0.01715)
Other Race	-0.01597	-0.01535	0.00292
	(0.01955)	(0.01177)	(0.01546)
Hispanic	-0.03697*	0.02352	-0.03461**
	(0.02003)	(0.01512)	(0.01379)
English Native Lang.	0.03316	0.04129***	0.00577
	(0.02022)	(0.01045)	(0.01581)
Two Parents	0.00574	0.00247	0.00211
	(0.01257)	(0.00790)	(0.00920)
Mother HS	-0.03086	-0.00412	-0.01089
	(0.02028)	(0.01403)	(0.01519)
Mother Some College	0.00549	0.00932	0.00981
	(0.02246)	(0.01542)	(0.01701)
Mother College	0.03306	0.02158	0.01557
	(0.02760)	(0.01940)	(0.02079)
Mother >College	0.02697	0.02373	0.00398
	(0.03333)	(0.02382)	(0.02421)
Father HS	0.06181***	0.03248**	0.04390***
	(0.02046)	(0.01528)	(0.01633)
Father Some College	0.06347***	0.04525***	0.02856*
	(0.02230)	(0.01692)	(0.01728)
Father College	0.06404**	0.03651*	0.04998**
	(0.02686)	(0.01992)	(0.02198)
Father >College	0.08108**	0.04126*	0.06322**
	(0.03165)	(0.02339)	(0.02664)
Mother Native US	0.04773**	0.01744	0.04373***
	(0.02106)	(0.01294)	(0.01435)
Father Native US	-0.03910*	-0.01024	-0.00597
	(0.02237)	(0.01417)	(0.01660)
Fam. Computer	0.01843	0.01777*	0.00598
	(0.01731)	(0.01058)	(0.01295)
25K $\leq$ Family Inc. $\leq$ 75K	-0.01685	0.00398	0.00279

	(0.01610)	(0.01041)	(0.01194)
Fam. Inc.≥ $75K	-0.06397***	-0.02836**	-0.00519
	(0.02025)	(0.01232)	(0.01535)
SES	0.01528	0.01845*	-0.01583
	(0.01786)	(0.01121)	(0.01480)
Northeast	-0.02561	-0.03344***	-0.04334***
	(0.01618)	(0.00773)	(0.00992)
South	-0.09316***	-0.02515***	-0.08576***
	(0.01195)	(0.00718)	(0.00793)
West	-0.04474***	-0.03197***	-0.04700***
	(0.01476)	(0.00816)	(0.00931)
Private	0.00851	-0.01199	-0.00494
	(0.01678)	(0.00914)	(0.01172)
% Free Lunch	0.00073***	0.00069***	-0.00077***
	(0.00026)	(0.00016)	(0.00020)
Urban	-0.00975	-0.01467**	-0.01115
	(0.01161)	(0.00722)	(0.00840)
Rural	0.06371***	0.04993***	0.01432
	(0.01374)	(0.00939)	(0.01519)
% Teachers Certified	-0.00046	0.00046**	-0.00102***
	(0.00029)	(0.00020)	(0.00020)
% Ltd English	-0.00351***	-0.00122**	-0.00069
	(0.00076)	(0.00050)	(0.00056)
Northeast × Private	-0.01512		0.03764
	(0.02814)		(0.02334)
Black × Private	-0.02987		-0.00382
	(0.03735)		(0.02670)
Female × SES			0.00657
			(0.01084)
Female × GPA			-0.02404**
			(0.00987)
Female × Rural			0.03408*
			(0.02011)

(1) *** p<0.01, ** p<0.05, * p<0.1
(2) Standard errors in parentheses
(3) Coefficients are marginal effects from probit estimation.

Table 3.4: Balancing Test. Treatment = Music in 10th, 11th, or 12th Grade

	Difference	Paired t-statistic
Ninth Grade GPA	-0.0033	-0.16
Female	0.00796	0.57
Black	0.00439	0.45
Asian	0.0016	0.18
Other Race	-0.00184	-0.25
Hispanic	-0.00348	-0.45
English is Native Lang.	0.00127	0.14
Two Parents	0.00116	0.1
Mother HS	-0.00184	-0.15
Mother Some College	0.00556	0.41
Mother College	-0.00175	-0.15
Mother $\geq$ College	-0.00169	-0.19
Father HS	-0.00243	-0.19
Father Some College	0.00492	0.38
Father College	0.0029	0.25
Father $\geq$ College	-0.00314	-0.29
Mother US Native	-0.00134	-0.13
Father US Native	-0.00198	-0.19
Family has computer	0.00102	0.13
\$25k $\leq$ Family Income $\leq$ \$50k	0.00223	0.16
\$50k $\leq$ Family Income $\leq$ \$75k	-0.00256	-0.19
Family Income $\geq$ \$75k	-0.00256	-0.19
SES	0.00059	0.03
Northeast	-0.00031	-0.03
South	0.00099	0.07
West	-0.00247	-0.23
Private	-0.00571	-0.46
% Free Lunch	0.158	0.24
Urban	-0.00304	-0.23
Rural	0.01064	0.88
% Certified	-0.019	-0.03
% Ltd English	-0.0311	-0.17
Northeast $\times$ Private	-0.00107	-0.18
Black $\times$ Private	0.00038	0.1

Table 3.5: Balancing Test. Treatment = Choir Student

Ninth Grade GPA	0.0089	0.29
Female	0.01377	0.74
Black	0.00091	0.07
Asian	0.00286	0.25
Other Race	0.0006	0.06
Hispanic	-0.003	-0.3
English is Native Lang.	0.0044	0.37
Two Parents	0.00168	0.1
Mother HS	-0.00251	-0.14
Mother Some College	0.00387	0.2
Mother College	-0.00359	-0.21
Mother $\geq$ College	0.0029	0.24
Father HS	0.00492	0.27
Father Some College	0.00067	0.04
Father College	0.00296	0.18
Father $\geq$ College	-0.00327	-0.21
Mother US Native	0.00053	0.04
Father US Native	0.00288	0.21
Family has computer	9E-05	0.01
\$25k $\leq$ Family Income $\leq$ \$50k	0.00166	0.08
\$50k $\leq$ Family Income $\leq$ \$75k	-0.00324	-0.17
Family Income $\geq$ \$75k	-0.00324	-0.17
SES	0.00253	0.09
Northeast	0.00416	0.26
South	-0.01033	-0.57
West	0.01177	0.79
Private	-0.0005	-0.03
% Free Lunch	-0.045	-0.06
Urban	-0.01217	-0.67
Rural	0.01442	0.83
% Certified	-0.257	-0.28
% Ltd English	0.0632	0.25
Northeast $\times$ Private	-0.00138	-0.14
Black $\times$ Private	0.00273	0.43
Female $\times$ SES	-0.00144	-0.06
Female $\times$ GPA	0.0402	0.65
Female $\times$ Rural	0.01013	0.65

Table 3.6: Balancing Test. Treatment = Band Student

	Difference	Paired t-statistic
Ninth Grade GPA	0.0238	0.74
Female	0.00903	0.39
Black	0.00302	0.19
Asian	0.00128	0.09
Other Race	-0.00598	-0.53
Hispanic	-0.00936	-0.74
English is Native Lang.	0.00685	0.54
Two Parents	0.00219	0.12
Mother HS	-0.00087	-0.04
Mother Some College	0.0037	0.16
Mother College	0.00049	0.02
Mother $\geq$ College	0.00107	0.07
Father HS	-0.00218	-0.11
Father Some College	0.0093	0.43
Father College	0.00024	0.01
Father $\geq$ College	-0.0019	-0.11
Mother US Native	0.00569	0.35
Father US Native	0.00496	0.3
Family has computer	0.00503	0.44
\$25k $\leq$ Family Income $\leq$ \$50k	0.00862	0.37
\$50k $\leq$ Family Income $\leq$ \$75k	-0.00313	-0.14
Family Income $\geq$ \$75k	-0.00313	-0.14
SES	0.01036	0.33
Northeast	-1E-05	0
South	-0.0011	-0.05
West	-0.00871	-0.51
Private	-0.00915	-0.49
% Free Lunch	0.287	0.27
Urban	-0.00492	-0.24
Rural	0.01395	0.65
% Certified	0.36	0.49
% Ltd English	-0.0823	-0.28

Table 3.7 Propensity Matching Estimates of the Effect of Music Classes in 10th, 11th, or 12th Grade on Academic Outcomes

Dependent Variable:	(1) Drop Out	(2) Attend Postsecondary	(3) Attend 4 Year College	(4) Academic GPA	(5) ACT	(6) SAT
Propensity Score Matching						
Music in 10th, 11th or 12th Grade	-0.00862**	0.03306***	0.03248**	0.09825***	0.44014***	17.748**
	(0.00369)	(0.01006)	(0.01499)	(0.01842)	(0.16278)	(7.6917)
Choir in 10th, 11th or 12th Grade	-0.01068**	0.01783	0.03692*	0.09275***	0.21554	9.0387
	(0.00429)	(0.01324)	(0.02022)	(0.02689)	(0.22979)	(9.2397)
Band in 10th, 11th or 12th Grade	-0.00756	0.06344***	0.05910***	0.17370***	0.84440***	34.232***
	(0.00506)	(0.01058)	(0.02079)	(0.02482)	(0.23387)	(9.4302)
OLS with School Fixed Effects						
Music in 10th, 11th or 12th Grade	-0.00850**	0.03143***	0.01554	0.07284***	0.43018***	17.23253***
	(0.00409)	(0.00969)	(0.01103)	(0.00839)	(0.11943)	(4.81707)
Choir in 10th, 11th or 12th Grade	-0.00954**	0.01422	0.02540*	0.05826***	0.03765	0.92225
	(0.00483)	(0.01254)	(0.01413)	(0.01077)	(0.14073)	(5.61882)
Band in 10th, 11th or 12th Grade	0.00089	0.04377***	0.03191**	0.08985***	0.85376***	34.28036***
	(0.00546)	(0.01165)	(0.01612)	(0.01100)	(0.16579)	(6.64681)

OLS with School Characteristics

Music in 10th, 11th or 12th Grade	-0.00678*	0.03098***	0.02620**	0.07051***	0.59982***	24.35752***
	(0.00393)	(0.00931)	(0.01176)	(0.00835)	(0.12608)	(5.07875)
Choir in 10th, 11th or 12th Grade	-0.00602	0.01409	0.02856*	0.05504***	0.23957	9.93506*
	(0.00458)	(0.01198)	(0.01507)	(0.01029)	(0.15000)	(5.99733)
Band in 10th, 11th or 12th Grade	-0.00223	0.04736***	0.03800**	0.08716***	0.92630***	37.93483***
	(0.00521)	(0.01120)	(0.01695)	(0.01155)	(0.16110)	(6.44785)

(1) Standard errors in parentheses.

(2) *** p<0.01, ** p<0.05, * p<0.1

(3) Variables used in the estimation of the propensity score vary by definiton of treatment. See table 4 for a full list.

(4) Standard errors in Panel A are block bootstrapped by school using 800 replications.

(5) Standard errors are clustered at the school level in Panel B.

(6) Control variables in panels B include: gender, race, Hispanic indicator, native English speaker, whether lives in a two parent home, father's education, mother's education, family income, family socio-economics status, whether family has a computer, whether parents are a part of the parent teacher organization, and region.

Table 3.8: Summary Statistics for Students Who Participated in Music in 10th, 11th, or 12th Grade Only

	All Students	Music Students	Non-Music Students	Music in 10th, 11th, or 12th Grade Only	Either No Music or Music in 9th Grade
Music Variables:					
Music Student	0.324	1.000		1.000	0.243
	(0.46785)				(0.42906)
Choir Student	0.156	0.482		0.442	0.122
	(0.36276)	(0.49972)		(0.49678)	(0.32720)
Band Student	0.111	0.343		0.135	0.108
	(0.31414)	(0.47478)		(0.34171)	(0.31059)
$\geq$ 1 Music Credit	0.177	0.546		0.280	0.164
	(0.38138)	(0.49793)		(0.44923)	(0.37061)
Music in 10th, 11th or 12th Grade Only	0.106	0.328		1.000	
	(0.30800)	(0.46952)			
Academic Outcomes:					
ACT	21.5	22.1	21.2	21.2	21.6
	(5.05)	(5.05)	(5.01)	(5.05)	(5.04)
SAT	1005	1027	991	991	1007
	(207.26)	(205.72)	(207.02)	(208.19)	(207.09)
Dropout	0.052	0.026	0.064	0.027	0.055
	(0.22136)	(0.15868)	(0.24488)	(0.16060)	(0.22733)
Attend College	0.753	0.829	0.715	0.790	0.748
	(0.43136)	(0.37698)	(0.45138)	(0.40756)	(0.43393)
Attend 4-yr College	0.451	0.529	0.413	0.485	0.447
	(0.49761)	(0.49921)	(0.49232)	(0.49995)	(0.49720)
Individual Characteristics:					
Female	0.502	0.584	0.463	0.518	0.500

	(0.50001)	(0.49299)	(0.49865)	(0.49985)	(0.50002)
Black	0.158	0.151	0.161	0.202	0.153
	(0.36479)	(0.35830)	(0.36782)	(0.40140)	(0.35986)
Asian	0.116	0.111	0.119	0.129	0.115
	(0.32063)	(0.31366)	(0.32389)	(0.33503)	(0.31886)
Other Race	0.121	0.085	0.139	0.117	0.122
	(0.32631)	(0.27870)	(0.34544)	(0.32091)	(0.32695)
Hispanic	0.144	0.102	0.165	0.131	0.146
	(0.35135)	(0.30208)	(0.37089)	(0.33773)	(0.35291)
English Native Lang.	0.831	0.873	0.811	0.827	0.832
	(0.37447)	(0.33310)	(0.39140)	(0.37814)	(0.37404)
Academic Honor	0.353	0.407	0.326	0.356	0.352
	(0.47783)	(0.49138)	(0.46894)	(0.47892)	(0.47772)
Club Participation	0.238	0.282	0.216	0.255	0.236
	(0.42572)	(0.44987)	(0.41177)	(0.43574)	(0.42447)
Remedial Course	0.115	0.109	0.118	0.122	0.114
	(0.31874)	(0.31142)	(0.32218)	(0.32776)	(0.31766)
Family Characteristics:					
Two Parents	0.749	0.764	0.741	0.7311	0.7507
	(0.43384)	(0.42482)	(0.43793)	(0.44351)	(0.43265)
Mother HS	0.268	0.252	0.276	0.2452	0.2709
	(0.44303)	(0.43395)	(0.44711)	(0.43037)	(0.44444)
Mother Some College	0.329	0.345	0.322	0.3305	0.3292
	(0.46999)	(0.47531)	(0.46727)	(0.47053)	(0.46994)
Mother College	0.183	0.208	0.172	0.1895	0.1828
	(0.38707)	(0.40572)	(0.37729)	(0.39199)	(0.38649)
Mother $\geq$ College	0.089	0.105	0.082	0.0987	0.0881
	(0.28513)	(0.30706)	(0.27371)	(0.29837)	(0.28351)
Father HS	0.281	0.278	0.283	0.2839	0.2810
	(0.44963)	(0.44811)	(0.45037)	(0.45102)	(0.44948)

Father Some College	0.264	0.269	0.261	0.2563	0.2644
	(0.44058)	(0.44341)	(0.43922)	(0.43671)	(0.44105)
Father College	0.178	0.192	0.171	0.1882	0.1769
	(0.38259)	(0.39426)	(0.37670)	(0.39101)	(0.38157)
Father $\geq$ College	0.140	0.166	0.127	0.1471	0.1388
	(0.34667)	(0.37239)	(0.33294)	(0.35436)	(0.34575)
Parent in PTO	0.262	0.302	0.241	0.2504	0.2630
	(0.43953)	(0.45917)	(0.42765)	(0.43340)	(0.44026)
Mother Native US	0.779	0.819	0.760	0.7699	0.7805
	(0.41472)	(0.38547)	(0.42734)	(0.42102)	(0.41395)
Father Native US	0.780	0.815	0.762	0.7580	0.7827
	(0.41419)	(0.38800)	(0.42566)	(0.42844)	(0.41240)
Fam. Computer	0.888	0.909	0.878	0.8925	0.8878
	(0.31506)	(0.28737)	(0.32748)	(0.30988)	(0.31568)
\$25k $\leq$ Fam. Inc. $\leq$\$50k	0.305	0.295	0.309	0.2894	0.3066
	(0.46032)	(0.45618)	(0.46224)	(0.45362)	(0.46109)
\$50k $\leq$Fam. Inc. $\leq$\$75k	0.204	0.214	0.199	0.1895	0.2056
	(0.40291)	(0.41028)	(0.39927)	(0.39199)	(0.40417)
Fam. Inc.$\geq$\$75k	0.281	0.308	0.269	0.2765	0.2820
	(0.44969)	(0.46182)	(0.44322)	(0.44741)	(0.44998)
SES	0.042	0.144	-0.007	0.0405	0.0420
	(0.74301)	(0.72596)	(0.74614)	(0.75924)	(0.74108)
Region:					
Northeast	0.180	0.188	0.176	0.1919	0.1782
	(0.38390)	(0.39076)	(0.38052)	(0.39392)	(0.38268)
South	0.367	0.331	0.385	0.3323	0.3715
	(0.48210)	(0.47054)	(0.48658)	(0.47119)	(0.48322)
West	0.199	0.177	0.210	0.2195	0.1971
	(0.39962)	(0.38133)	(0.40764)	(0.41403)	(0.39783)

Table 3.9: Propensity Matching Estimates of the Effect of Music Classes in 10th, 11th, or 12th Grade on Academic Outcomes, Restricted Sample

Dependent Variable:	(1) Drop Out	(2) Attend Postsecondary	(3) Attend 4 Year College	(4) Academic GPA	(5) ACT	(6) SAT
Sample excludes students who took music classes in ninth grade:						
Propensity Score Matching:						
Music in 10th, 11th or 12th Grade Only	-0.01245*	0.02569	0.04274*	.05246*	-0.03197	-1.8531
	(0.00653)	(0.01789)	(0.02323)	(0.02985)	(0.28869)	(11.864)
OLS with School Fixed Effects:						
Music in 10th, 11th or 12th Grade Only	-0.01619**	0.01480	0.00545	-7.89162	-0.18535	0.04811***
	(0.00658)	(0.01681)	(0.01873)	(6.01868)	(0.14836)	(0.01528)
OLS with School Characteristics:						
Music in 10th, 11th or 12th Grade Only	-0.00931	0.02761*	0.04151**	0.05601***	0.33033*	13.02200
	(0.00650)	(0.01436)	(0.01753)	(0.01384)	(0.19820)	(8.08149)
Sample includes students who took music classes in ninth grade:						
Propensity Score Matching:						
Music in 10th, 11th or 12th Grade Only	-0.00926	0.00641	0.02116	-0.00383	-0.30864	-13.237
	(0.00608)	(0.01610)	(0.02273)	(0.02613)	(0.27360)	(11.188)

OLS with School Fixed Effects:						
Music in 10th, 11th or 12th Grade Only	-0.01671**	0.01133	0.01108	-12.70743**	-0.32069**	0.03788***
	(.00643)	(0.01529)	(0.01513)	(5.31786)	(0.13153)	(.01388)
OLS with School Characteristics:						
Music in 10th, 11th or 12th Grade Only	-0.00678*	0.03098***	0.02620**	0.07051***	0.59982***	24.35752***
	(0.00393)	(0.00931)	(0.01176)	(0.00835)	(0.12608)	(5.07875)

(1) Standard errors in parentheses.

(2) *** p<0.01, ** p<0.05, * p<0.1

(3) Variables used in the estimation of the propensity score vary by definiton of treatment. See table 4 for a full list.

(4) Standard errors for propensity score matching are block bootstrapped by school using 800 replications.

(5) Standard errors are clustered at the school level for OLS estimates.

(6) Control variables in OLS include: gender, race, Hispanic indicator, native English speaker, whether lives in a two parent home, father's education, mother's education, family income, family socio-economics status, whether family has a computer, whether parents are a part of the parent teacher organization, and region.

Table 3.10: Estimates of the Impact of Earning One or More Credits of Music on Academic Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Drop Out	Attend Postsecondary	Attend 4 Year College	Academic GPA	ACT	SAT
Sample excludes students with less than one music credit but more than zero music credits:						
Propensity Score Matching:						
$\geq$1 Music Credit	-0.12052***	0.04568***	0.02654*	0.12562***	0.51369***	20.981***
	(0.00314)	(0.00876)	(0.01422)	(0.01638)	(0.16586)	(6.664)
OLS with School Fixed Effects:						
$\geq$1 Music Credit	-0.00915**	0.03350***	0.03281***	0.08005***	0.26593***	10.915***
	(0.00404)	(0.01088)	(0.01110)	(0.00848)	(0.10037)	(4.0302)
Sample includes students with less than one music credit but more than zero music credits:						
Propensity Score Matching:						
$\geq$1 Music Credit Class	-0.01202***	0.04957***	0.03362**	0.11779***	0.48694***	20.122***
	(0.00340)	(0.00968)	(0.01668)	(0.01870)	(0.17073)	(6.8644)
OLS with School Fixed Effects:						
$\geq$1 Music Credit Class	-0.00736*	0.02741***	0.02705**	0.05495***	0.10791	4.1306
	(0.00420)	(0.01037)	(0.01079)	(0.00837)	(0.08531)	(3.4250)

(1) Standard errors in parentheses.

(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

(3) Variables used in the estimation of the propensity score vary by definiton of treatment. See table 4 for a full list.

(4) Standard errors for propensity score matching are block bootstrapped by school using 800 replications.

(5) Standard errors are clustered at the school level for OLS estimates.

(6) Control variables in OLS include: gender, race, Hispanic indicator, native English speaker, whether lives in a two parent home, father's education, mother's education, family income, family socio-economics status, whether family has a computer, whether parents are a part of the parent teacher organization, and region.

Table 3.11: Estimates of the Relationship Between Music Credits and Academic Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Drop Out	Attend Postsecondary	Attend 4 Year College	Academic GPA	ACT	SAT
Music Credits	-0.00223***	0.01053***	0.00659**	0.02423***	0.08340***	3.3138***
	(0.00085)	(0.00266)	(0.00332)	(0.00195)	(0.02470)	(0.98490)
Music Credits	-0.00472**	0.01387**	0.00051	0.02422***	0.05550	2.14467
	(0.00239)	(0.00630)	(0.00743)	(0.00500)	(0.05980)	(2.38774)
Music Credits Squared	0.00044	-0.00059	0.00108	0.00000	0.00480	0.20100
	(0.00031)	(0.00097)	(0.00121)	(0.00076)	(0.00968)	(0.38845)

(1) Standard errors in parentheses.

(2) *** p<0.01, ** p<0.05, * p<0.1

(3) Standard errors are clustered at the school level.

(4) Control variables include: gender, race, Hispanic indicator, native English speaker, whether lives in a two parent home, father's education, mother's education, family income, family socio-economics status, whether family has a computer, whether parents are a part of the parent teacher organization, and region.

Appendix Table 2.1: Estimates of the Impact of School Test Scores on House Prices Using Houses less than 1000 feet from a Boundary

	(1)	(2)	(3)
Local Composite Test Score	0.00145***	0.00115***	0.00141***
	(0.00023)	(0.00020)	(0.00020)
Composite Test Score (#1 Cluster School)		-0.00012	
		(0.00037)	
Composite Test Score (#2 Cluster School)		0.00125***	
		(0.00048)	
Composite Test Score (#3 Cluster School)		0.00042	
		(0.00039)	
Composite Test Score (Closest Cluster School)			0.00063***
			(0.00017)
Composite Test Score (2nd Closest Cluster School)			0.00079***
			(0.00018)
Composite Test Score (3rd Closest Cluster School)			-0.00008
			(0.00018)
Year Effects	X	X	X
House Characteristics	X	X	X

(1) Standard errors clustered at the school level in parentheses.

(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

(3) Dependent variable is ln(price per square foot) in all regressions.

(3) House characteristics include acreage, number of stories, number of bathrooms, whether the house has air conditioning, whether basement, whether garage, and home age.

(5) Local Composite Test Score is the CTBS test score percentile of the house's local school.

Appendix Table 2.2: Estimates of the Impact of Nearby Cluster Schools' Test Scores on House Prices, including Boundary Fixed Effects

	(1)	(2)	(3)	(4)
Composite Test Score (Closest Cluster School)		0.00112***		0.00110***
		(0.00012)		(0.00012)
Composite Test Score (2nd Closest Cluster School)		0.00136***		0.00120***
		(0.00012)		(0.00012)
Composite Test Score (3rd Closest Cluster School)		0.00041***		-0.00013
		(0.00012)		(0.00013)
Cluster Avg. of Composite Test Scores (Closest Schools)	0.00291***		0.00224***	
	(0.00016)		(0.00016)	
Local Composite Test Score			0.00223***	0.00239***
			(0.00013)	(0.00013)

(1) Clustered standard errors in parentheses
(2) *** p<0.01, ** p<0.05, * p<0.1
(3) All columns include controls for house characteristics and year effects.
(4) Sample is restricted to houses within 2000ft of a catchment area boundary.
(5) A full set of boundary fixed effects is estimated in every column.

Appendix Table 2.3: Estimates of the Impact of Reading Test Scores on House Prices

	(1)	(2)	(3)
Local Reading	0.00302***	0.00052	0.00049
	(0.00028)	(0.00055)	(0.00055)
Reading (#1 Cluster School)			0.00076***
		(0.00024)	
Reading (#2 Cluster School)		0.00147***	
		(0.00028)	
Reading (#3 Cluster School)		0.00063**	
		(0.00025)	
Reading (Closest Cluster School)			0.00132***
			(0.00013)
Reading (2nd Closest Cluster School)			0.00115***
			(0.00013)
Reading (3rd Closest Cluster School)			-0.00008
			(0.00014)
Year Effects	X	X	X
House Characteristics	X	X	X
≤2000ft from Boundary	X	X	X
Boundary Fixed Effects	X		

(1) Standard errors clustered at the school level in parentheses
(2) *** p<0.01, ** p<0.05, * p<0.1
(3) Dependent variable is ln(price per square foot) in all regressions.
(4) House characteristics include acreage, number of stories, number of bathrooms, whether the house has airconditioning, whether basement, whether attached garage, whether detached garage, and home age.
(5) Boundary fixed effects estimated in all columns.

Appendix Table 2.4: Estimates of the Impact of Math Test Scores on House Prices

	(1)	(2)	(3)
Local Math	0.00262***	-0.00046	-0.00061
	(0.00027)	(0.00045)	(0.00045)
Math (#1 Cluster School)		0.00100***	
		(0.00018)	
Math (#2 Cluster School)		0.00113***	
		(0.00025)	
Math (#3 Cluster School)		0.00085***	
		(0.00023)	
Math (Closest Cluster School)			0.00101***
			(0.00012)
Math (2nd Closest Cluster School)			0.00119***
			(0.00012)
Math (2nd Closest Cluster School)			0.00012
			(0.00012)
Year Effects	X	X	X
House Characteristics	X	X	X
$\leq$2000ft from Boundary	X	X	X
Boundary Fixed Effects	X		

(1) Standard errors clustered at the school level in parentheses
(2) *** $p<0.01$, ** $p<0.05$, * $p<0.1$
(3) Dependent variable is ln(price per square foot) in all regressions.
(4) House characteristics include acreage, number of stories, number of bathrooms, whether the house has airconditioning, whether basement, whether attached garage, whether detached garage, and home age.
(5) Boundary fixed effects estimated in all columns.

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