THE EFFECTS OF EXPANDING SCHOOL CHOICE IN THE SAN FRANCISCO UNIFIED SCHOOL DISTRICT

by

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ABSTRACT

This thesis represents the first empirical examination of the 2001 transition from a neighborhood-based assignment process to a choice-based assignment process in the San Francisco Unified School District (SFUSD). The SFUSD is unique in that it coupled its expansion of school choice with a diversity-index selection mechanism for over-subscribed schools, intended to counter the homogenizing effects of having total school choice. I evaluate two facets of the SFUSD school choice system: whether the policy succeeds in its goal of offering school choice without sacrificing diversity, and the effects of the 2001 legislation on the capitalization of neighborhood school quality into home prices in San Francisco.

My results show that in the schools that are more likely to be in high demand—those with high test scores and low percentages of students receiving free lunch—diversity is increasing as a response to the 2001 policy, whereas the reverse holds true for low-performing schools with a high percentage of students participating in the lunch program. With regard to capitalization I find strong evidence that test scores are capitalized in the San Francisco housing market but find mixed evidence on the effect of school choice on the extent of this capitalization.

CHAPTER 1: INTRODUCTION

School districts around the country are concerned with providing a better education for their students. School choice is advocated by many economists as a way for school districts to increase educational efficiency with minimal additional costs. Choice is thought to increase school quality by placing competitive pressure on schools and enabling parents to choose the best schools for their children (see Hoxby 2000). However, in urban areas where the differences between schools are dramatic, school choice raises concerns about educational equality and about the possibility that students attending low-performing schools will be made worse off. In addition, policymakers are often concerned about ensuring school diversity while weighing the concerns of parents who want better options in public education.

The San Francisco Unified School District (SFUSD) provides an especially interesting situation to study school choice because it has an extensive school choice policy which was radically expanded in 2001 in response to a lawsuit initiated in 1994 by Chinese American families. The new policy gives parents complete school choice; that is, they can request to send their child to any school in the SFUSD. Furthermore, the new policy eliminates neighborhood schools; a student who does not request a specific school will simply be assigned to a school with available spots and not necessarily the school closest to his or her home.

To prevent schools from becoming too homogeneous as a result of self-selection, schools with more requests than spots available use a diversity index, discussed below, to ration available spots. In accordance with the 1994 settlement, the selection process is forbidden to use a student's ethnicity as a factor in assigning students, so the diversity index is comprised of six race-neutral factors thought to be correlated with student achievement.

In this thesis, I evaluate two facets of the SFUSD school choice system: whether the policy succeeds in its goal of offering school choice without sacrificing diversity, and the effects of the 2001 legislation on the capitalization of neighborhood school quality into home prices in San Francisco.

1.1 School Choice and Ethnic Diversity

Economic theory on the effect of choice on the school diversity in a district gives ambiguous predictions. In the absence of school choice, the extent of within-school diversity is determined by the extent of residential segregation across neighborhoods. If neighborhoods are completely segregated, then school choice would be expected to increase diversity because it would allow students from any neighborhood equal access to high-achieving schools. If neighborhoods are not at all segregated, one might expect diversity to decrease as students tend to self-select into schools with student populations similar to themselves. This raises the concern that highly motivated students will self-select into high-performing schools, leaving less motivated students in the lower-achieving schools. While the high-achieving students might benefit from being surrounded by high-achieving peers, the lower-achieving students may be adversely affected by the absence of high-achieving peers. With respect to diversity, one might expect a similar pattern of self-selection, resulting in the clustering of ethnicities across schools and decreased ethnic diversity within schools.

A main criticism against the new system in San Francisco is that because the diversity index is only applied to the schools that receive more applications than they can accommodate, lower-performing or less popular schools have no mechanism to enhance diversity and are thus becoming "re-segregated" by school choice. Furthermore, many people doubt the ability of the diversity index to enhance ethnic diversity even when it is applied because it does not use race as a factor in assigning students. Another concern with the new policy is that by opening up schools to all students, some students unable to attend the high-achieving school of their choice will move out of the district or attend private schools.

To assess the validity of these concerns, I measure diversity within grades at SFUSD elementary schools and compare the diversity of cohorts who were affected by both the 2001 expansion of choice and the diversity index (those who entered kindergarten 2002 and later) with those who were only affected by the expansion of choice (those who entered kindergarten in 2001) as well as with those who were not affected by either policy (students who entered kindergarten before 2001). To see if different types of schools were affected differently by the legislation I relate grade-level diversity to the percentage of students receiving free or reduced-price lunches in 1997, a proxy for the socioeconomic status of a school's students. As a second test I use standardized 1997 test-score quintiles interacted with the policy dummy as an explanatory variable to test whether diversity is decreasing at low-achieving schools. My estimates indicate that the 2001 policy helped to increase diversity overall (by a 0.0574 increase in the Herfindahl index measure of diversity), but that a ten percent increase in the school's percentage of students receiving free or reduced lunch in 1997 is associated with a 0.0112 decrease in the Herfindahl index. These results indicate that while schools with less than fifty percent of students receiving free or reduced-price lunch in 1997 were made more diverse, schools with more than fifty percent of students receiving free or reduced-price lunch were made *less* diverse by the policy. Furthermore, results from the regressions that use pre-reform test- and school lunch-quintile dummies rather than percentages indicate that the segregating effects of increased choice are concentrated in a handful of low-achieving and disadvantaged schools.

An examination of the diversity index selection mechanism shows that after its implementation in 2002 schools with less than 70% of students receiving free lunch were made more diverse, which accounts for approximately half of the elementary schools in the SFUSD. Schools with more than 70% of students receiving free lunch were found to be more segregated in the presence of school choice. This is a profound improvement in diversity, as without the diversity index it appears that approximately ninety percent of schools were being made less diverse by school choice. When the diversity index is applied as a selection mechanism, only fifty percent of schools are being made less diverse by having complete choice.

1.2 School Choice and Home Prices

In the second part of my study, I examine the effect of the 2001 legislation on home prices in the SFUSD. Numerous studies have shown that homes located within higher-performing school districts are priced higher than comparable homes in lower-performing districts (see, for example Black 1999, Kane, Staiger and Reigg 2005) as parents are willing to pay a premium for perceived increases in educational quality. Because the policy enacted in 2001 virtually eliminated neighborhood schools, one would expect the values of homes near highly-demanded neighborhood schools to decrease, while the values of homes near lower-performing schools would increase due to the improved educational opportunities associated with those homes.

The economic theory underlying school quality capitalization, and hedonic regressions of property values in general, assumes the supply of homes in a given district to be inelastic (Kanemoto 1988). An increase in demand for a public good provided by a particular district (such as education) will therefore be reflected by an increase in the price of homes in that district.

The presence of school choice, which extends access to a school beyond its attendance district, effectively nullifies the assumption that home supply is inelastic. The price-premium for homes in high-performing school districts would therefore be reduced in the presence of school choice. In the case of San Francisco after 2001, one would expect that the school quality of San Francisco public schools as a whole would be capitalized into the price of homes instead of the quality of the neighborhood schools within the district.

In this thesis I find strong evidence that test scores are capitalized in the price of San Francisco homes. I find mixed evidence on the effects of school choice on capitalization, though it appears that the policy may have reduced capitalization in single-family homes. My results underscore the importance of controlling for neighborhood effects as both the magnitude and sign of multiple coefficients vary significantly depending on how restrictive the neighborhood controls are.

Chapter 2: Background Information on the SFUSD

Few school districts north of the Mason-Dixon Line have had as much difficulty in complying with the desegregation orders of Brown v. Board of Education as the San Francisco Unified School District (SFUSD). Known for its ethnically diverse population, the city has been struggling with its desegregation policies since 1978 when the NAACP filed a lawsuit claiming that African-American and Latino students were not getting equal treatment because of the de-facto "re-segregation" in the city's neighborhood schools. In a 1982 settlement, the district agreed to an aggressive desegregation policy under which no neighborhood school could have more than 45% of one ethnicity and that at least four ethnic groups be represented in each school. The policy was defeated in 1994 by a group of Chinese-American parents who claimed their children were being unfairly denied admittance to top-performing schools on the basis of race. The resulting settlement forbade the use of race in the school-assignment process for any San Francisco public school and necessitated yet another overhaul of the district's school assignment process.

Ethnic diversity is not the only concern facing policy makers in the SFUSD. The city has a long history of providing parents with a choice of schooling options for their children. In addition to neighborhood schools, there are a number of charter schools, nonselective alternative schools and two selective alternative high schools. The availability of private schooling also contributes to school choice as the fraction of students attending private school in San Francisco is also among the highest in the country. In the 2005-2006 school year almost 30 percent of San Francisco students attended private school, compared to 10 percent of students nationwide and 8.7 percent of students statewide (San Francisco Chronicle, May 2006). Issues relating to school choice in the SFUSD are inextricably linked to school diversity and educational equity as policymakers know that steps taken to restrict choice and increase educational equity would potentially result in the departure of high achieving students to private schools or neighboring public school districts.

From 1983-2001, the SFUSD employed an assignment process known as the Optional Enrollment Process. Under this system, students were automatically assigned to their attendance area (or neighborhood) school unless they requested another school through an Optional Enrollment Form. If a school was oversubscribed, a random computerized lottery was used to award spots, though it is important to note that a child was guaranteed admission to his or her neighborhood school whether or not his or her parents participated in the Optional Enrollment Process. In compliance with the aforementioned 1982 desegregation settlement with the NAACP, no neighborhood school could have more than 45% of its student body be of the same ethnicity and no "alternative" school could have one ethnicity make up more than 40% of its student body. This policy of placing caps on certain ethnicities was the target of much controversy as the district saw dramatic increases in the number of Chinese students during the 1980s and 1990s. In particular, maintaining these ethnic caps at the selective alternative school Lowell High School meant that Chinese students had to score much higher than students of any other ethnicity on entrance exams in order to be admitted. The 1999 settlement of a lawsuit initiated in 1994 by a group of Chinese parents against the use of ethnic caps declared any use of race as a deciding factor in admissions decisions to be unconstitutional.

In 2001, the SFUSD abandoned the ethnic caps and adopted a controversial new school assignment system intended to give parents more choice without sacrificing school diversity. Beginning in the 2001-2002 school year, students were no longer guaranteed admission to their attendance area schools and all parents were required to fill out the formerly optional form indicating school preferences. On the form, all parents of incoming kindergarteners, 6^{th} graders and 9^{th} graders rank up to seven schools for their child to attend. For the 2001-02 school year, attendance to oversubscribed schools was decided purely by random lottery and race was prohibited from being used to assign students. In the next school year (2002-03) the SFUSD employed the diversity index instead of the random lottery for the first time to assign students to oversubscribed schools.

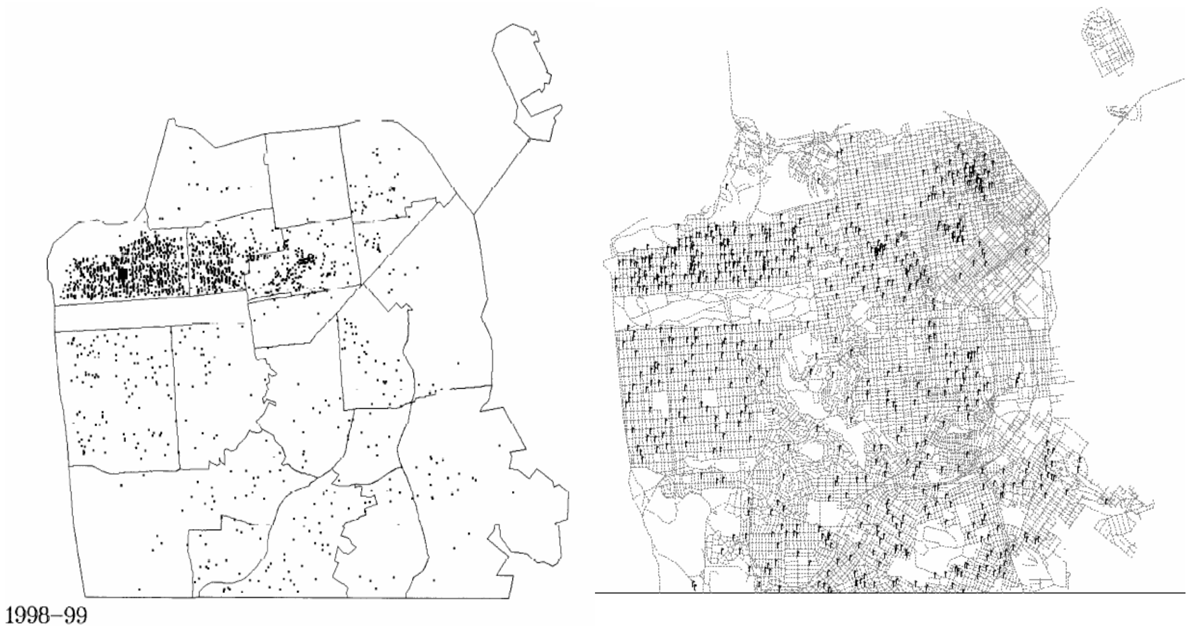
The diversity index was presumably intended to counter the “re-segregating” effects of expanding school choice. Because of the 1999 lawsuit, the SFUSD needed a way to diversify public schools without using race. The Diversity Index is thus made up of six race-neutral factors that are believed to be correlated with student achievement (defined in detail below), and diversity is maximized along these factors. Beginning in 2002, a school that receives more attendance requests than it can accommodate selects students that will maximize diversity along the six race-neutral characteristics defined in the diversity index. If a student is admitted to more than one school out of his or her seven choices, the student is assigned to the school that is highest on his or her list, so it is in the student’s best interest to list seven ranked preferences. According to the SFUSD website (SFUSD 2007), in the 2003-2004 school year 67% of incoming kindergartners were placed in their first-choice school and 87% of incoming kindergartners were placed in one of their seven choices. There were a minority of students who did not get placed in any of their choices and were randomly assigned to a school with available spots. The enrollment process has multiple rounds, so students have the ability to reapply if they are dissatisfied with their first assignment. Home address and transportation infrastructure are considered in the assignment process, but the student will not necessarily be placed in the school closest to his or her home.

Aside from providing parents with more choice, breaking the link between neighborhood and school assignment is a crucial step in reversing de-facto segregation. The residential segregation across San Francisco neighborhoods was certainly a driving force behind the de-facto segregation in SFUSD schools. Appendix Figure A.1 shows the geographic distribution of SFUSD students by ethnicity. Note especially the high concentrations of African-American and Latino students and on the east side of the city, while White and Chinese students mainly come from the west side.

Figure 2.1 illustrates the dramatic redistributive effects of the new assignment process on Washington High School, the city's most requested neighborhood high school according to first-choice request data published on the SFUSD website (SFUSD 2007). Each dot represents the address of a Washington High School student. In the 1998-99 school year, the dots are primarily clustered around the school's location (not plotted) in the northwest portion of the city. In 2004, however, the dots are more evenly distributed throughout the city. Especially significant is the increase in students coming from the southeast portion of the city, notable for high levels of poverty and some of the worst public schools in the district.

Figure 2.1: Washington High School Attendance Offers 1998-2004

1998-1999 School Year 2004-2005 School Year



Maps obtained from http://orb.sfusd.edu/pinmap/misc/pinmp98.pdf

2.1 The Diversity Index

The SFUSD's diversity index is comprised of six characteristics (five for incoming kindergartners), each of which can be expressed binomially. The SFUSD chose these characteristics because they are thought to be correlated with academic performance. The six characteristics are: Academic Achievement Score (= 1 if the student scored above the 30th national percentile on standardized tests or attended preschool), Mother's Educational Background (= 1 if the student's mother graduated from high school), Socioeconomic Status (= 1 if the student participates in free/ reduced lunch, CalWORKS, or lives in public housing), Language Proficiency (= 1 if the student is enrolled in ESL classes), Home Language (= 1 if English is the student's home language), and (for non-kindergartners) the API Rank of Sending School (= 1 if the student's sending school is ranked above the 4th decile on the statewide Academic Profile Index). These characteristics taken together are hereafter referred to as the student's "diversity profile."

There is no ideal diversity profile and thus no ideal score on the diversity index, as the index is a formula for calculating the probability that in a given grade two randomly chosen students will be different from each other along the race-neutral factors listed above. Students are admitted to an oversubscribed school only if they will improve the school's diversity along the six characteristics in the index. Students with an older sibling attending their first-choice school and those with special program needs are not subject to the diversity index, are pre-assigned to their school and their diversity profiles thus used to calculate the school's "base profile." After these students are identified the school will admit students who request admittance on the mandatory enrollment form. If there are more attendance requests than remaining spots, the applicants are split into two groups: those students living within the school's attendance area and those living outside the attendance area. The selection process involves first calculating diversity profiles for students within the attendance area, and admitting the student(s) with the profile that is most different from the base profile. The base profile is then recalculated to include the profile of the student(s) just added and the process is repeated until students from the attendance area no longer contribute to the diversity of the grade, at which point all applicants are considered and the process is repeated until there are no more spots available. A critical point to note is that while students living within a school's attendance area are more likely to be offered admittance, they are never guaranteed a spot based on residence alone. As Figure 4.1 illustrates, in 1998 Washington High School was attended primarily by students living within its attendance area in the northwest quadrant of the city. In 2004, after the expansion of school choice, Washington High School appears to have a substantially higher concentration of students commuting from the east side of the city.

CHAPTER 3: THEORETICAL FRAMEWORK AND REVIEW OF RELEVANT LITERATURE

3.1 "Re-segregation" since Brown v. Topeka Board of Education

The years immediately following the landmark *Brown v. Board of Education* decision saw widespread racial integration of schools nationwide. Maintaining these levels of integration over the long term has proved to be a more challenging task. Putting an end to segregation immediately after the *Brown* decision meant changing enrollment policies that explicitly assigned students of different races to separate schools. In the years following *Brown,* however, policymakers have had to contend with re-segregation caused not by explicit policies but by changing demographics and segregated housing markets. Racial integration has caused "white flight"—the disproportionate departure of white students in response to increases in nonwhite enrollment—in some districts.

Since the *Brown* decision there have also been subsequent decisions that freed districts formerly under strict desegregation orders as well as made it more difficult for districts that were not under court order to operate voluntary desegregation programs (Lutz 2005). To study whether this subsequent court action had any impact on resegregation, Clotfelter, Vigdor and Ladd (2006) study racial isolation and racial imbalance in the 100 largest Southern school districts between 1994 and 2004. They document a significant resegregating trend in southern schools. Clotfelter, Vigdor and Ladd conclude that the end of court-ordered desegregation played a role in resegregating southern schools, but they also find that demographic shifts were a major culprit behind resegregation.

Also related to the findings of this thesis is the work done by Hanushek, Kain, and Rivkin in 2002. Hanushek, Kain, and Rivkin studied the relationship between racial composition and student achievement in Texas elementary schools in the years following desegregation orders. They find achievement for African American students to have a negative relationship to African American enrollment share, but not the share of other minorities. Furthermore, they find these effects to be most detrimental to high-achieving African American students.

3.2 Effects of School Choice

The existing literature on school choice focuses on two controversial points: whether increased choice causes schools to be more productive and the extent to which students are made better off (worse off) as a result of attending school with higher (lower) achieving peers.

One problem faced by many researchers trying to study the effects of school choice on school productivity is the fact that the degree of choice in a district tends to be endogenous to student achievement. This endogeneity could be the result of a situation where parents who prefer to live in areas with high degrees of school choice also happen to have high-achieving children. The test scores in high-choice districts are therefore more reflective of the type of families who live there rather than the quality of local schools.

Empirical work in this area gives mixed results. Hoxby (2000) studies the effects of school choice at the district level using natural boundaries such as streams as an instrument for the degree of Tiebout (residential) choice. She concludes that students in cities with higher degrees of Tiebout choice display increased school productivity measured as higher scores on standardized tests, and lower per-pupil spending. Hoxby concludes that Tiebout choice can explain up to one-half a standard deviation in test-score difference between districts with no choice, like Miami-Dade, and districts with a high degree of choice such as Boston. On a national scale, however, she finds Tiebout choice only accounts for a small percentage of the differences in student achievement. Hsieh and Urquiola (2005) exploit a policy change that expanded the use of school vouchers in Chile to identify the effects of expanded school choice and find no positive effects of the expanded use of vouchers on student achievement. In fact, they observe significant decreases in educational quality in districts that had the biggest increase in private school enrollment due to vouchers. In their study of the lottery winners and losers in the Chicago Public School system, Cullen, Jacob and Levitt (2006) find no correlation between exercising school choice and positive student outcomes. The second concern with expanding school choice is the possible self-selection of students into homogenous groups. High achieving students are more likely than low-achieving students to exercise school choice (Cullen, Jacob and Levitt 2005), so there is concern that students who remain in less popular schools will be adversely affected by the departure of higher-achieving students. Dills (2004) examines this issue directly and finds that the addition of a selective magnet school in Fairfax County, VA had adverse effects on students remaining in regular public schools. She finds that when the top 1% of high-scoring students depart for the magnet school, the percentage of remaining students that score in the bottom national quartile increases by approximately 9%. She does not test whether it was the departing students' test scores, or characteristics such as race or socioeconomic status that are associated with higher test scores, that caused the adverse peer effects for remaining students.

3.3 School Quality and Capitalization Effects

While the fact that school quality is capitalized in home prices is well-documented, studies testing the extent of school quality capitalization must overcome several obstacles. First, the researcher must identify school characteristics that would make access to that school more valuable to homebuyers. Many studies, such as Black (1999) and Kane, Staiger and Reigg (2005) have used standardized test scores because they are widely available under the No Child Left Behind legislation. Clapp, Nanda and Ross (2005), however, find that the demographic characteristics of a school such as percentage African American or Latino students and percentage of students receiving free lunch matter more to homeowners in Connecticut than test scores as reflected in home price data.

The second obstacle is separating the effect of school quality from neighborhood effects in determining home prices. Black (1999) uses proximity to primary school attendance boundaries in Massachusetts to identify similar homes that differ only in primary school assignment, and finds that parents are willing to pay an additional 2.5 percent for a 5 percent increase in standardized test scores. Black also finds larger test-score capitalization effects in homes with three or more bedrooms, as there is increased likelihood that such homes would be inhabited by families with children. She argues that because the inhabitants of homes with more than three or more bedrooms are more likely to have children that they should be willing to pay more for good schools. Kane, Staiger and Reigg (2005) observe even more home price sensitivity to school quality when they exploit the redistricting in Mecklenberg County, North Carolina to identify changes in school quality in a given neighborhood. Kane, Staiger and Reigg find that North

Carolina families would be willing to pay roughly ten percent more for homes where the neighborhood elementary school scores at the 25th percentile than homes with neighborhood elementary schools that score at the 75th percentile. Furthermore, they find significant population changes around attendance boundaries after the redistricting, indicating that the population living within certain school boundaries are a function of school boundaries. Using data from a midsize Florida school district, Figlio and Lucas (2000) analyze the effects of better information on school quality capitalization. They find evidence that the marginal willingness to pay for a home in a better school district increases when information on school quality is made more available through the use of school report cards in the state of Florida—suggesting that the extent to which school quality is capitalized in home prices can change to reflect policy changes and the structure of a school district.

Previous studies provide evidence that the extent of this capitalization would be sensitive to an increase in school choice. Eric Brunner and Jon Sonstelie (2004) show that voters consider this when voting on school choice initiatives. They survey potential voters on California's 2000 voucher initiative, which lost in the general election by a 2-1 margin, and found that homeowners without children were less likely to vote for the voucher initiative if they lived in a high-performing school district. Their findings indicate that California homeowners recognize the value added to their home by the quality of local schools could be diminished if school choice is expanded. Finally Randall Reback's 2005 study of Minnesota's statewide inter-district choice program finds significant reductions in home values in districts that received students, and significant increases in home values in districts that sent more students to other districts than it received. He finds that a one-standard deviation increase in initial outgoing transfer rates is associated with an increase in house prices of three percent, and that a one-standard deviation increase in initial incoming transfer rates is associated with a decrease in house prices of about three percent. These results indicate that homebuyers do indeed consider the quality of public education associated with a home in determining their willingness to pay for that home.

3.4 Contributions of this thesis to the existing knowledge

This paper is unique in that it is the only empirical examination of San Francisco's transition to having total school choice. Following Hsieh and Urquiola's (2005) methodology I use the 2001 expansion of school choice in the SFUSD as my source of identification and use grade-level data for my analysis on the policy's effect on diversity within schools.

As the first empirical study of San Francisco's diversity index assignment mechanism, this thesis addresses the extent to which school districts can influence the racial composition of its schools through indirect means. This is a very relevant question considering that the constitutionality of using a student's race in deciding school assignments has been under attack in courts nationwide. Furthermore, this study is among the first to study short-term resegregation in an urban school district. Though the results presented in this paper only reflect the short-term effects of the San Francisco policy, a similar study of this program that covers a longer time frame could be valuable in studying the long-term effects of indirect desegregation policy.

My study does not directly consider changes in student achievement caused by the SFUSD's expansion of school choice. However, findings that indicate the policy caused certain schools to have larger concentrations of students more likely to score poorly on standardized tests would imply that student achievement at these schools would be adversely affected by the disproportionate departure of high-achieving students.

The second part of my project focuses on whether increased school choice had an effect on the extent to which neighborhood school quality is capitalized in the price of San Francisco homes. Because it is no longer guaranteed that one's child will be assigned to the nearest "attendance area" school, one would expect school quality capitalization to be equalized across all neighborhoods. That is, the price of a San Francisco home would reflect the quality of the district overall rather than the quality of the local schools in the neighborhood.

Following Reback, I use the SFUSD expansion of school choice to test the predictions of economic theory concerning the capitalization of public school quality in home prices. Because the 2001 policy gives all San Francisco residents equal access to all of its public schools, home prices in neighborhoods with highly-demanded public schools should have experienced a decline while home prices in neighborhoods with less popular schools should experience an increase due to better public schooling options. Just as Reback compared districts that received students to districts that sent students, I can determine which schools are highly demanded by parents by looking at the number of attendance requests received by schools.

This paper augments the existing literature on school-quality capitalization in that it measures these effects in an urban school district where a significant portion of students attend private school. Additionally, parents also had a significant degree of choice among public schools in the district before the 2001 policy change. Previous studies that measure school-quality capitalization have taken place in districts with little or no school choice; in fact Black (1999) deliberately excludes districts with school choice programs for her estimates of school-quality capitalization. The fact that capitalization is observed at all in a district with as much educational choice as the SFUSD prior to 2001 underscores the importance of neighborhood schools to all residents, not just those who plan to use the public school system to educate their children.

CHAPTER 4: EFFECTS ON DIVERSITY

4.1 School Data

In accordance with California's accountability practices, annual profiles documenting various school-level statistics are published annually on the SFUSD website. The test-score data reported in these school profiles are also published annually in the San Francisco Chronicle. For grades 2 through 11, Math and Reading national percentile and "national curve equivalent" scores are published in school profiles. Both range in value from 1-99 and because no explanation is given about the difference between the national percentile score and national curve equivalent score, the two were averaged across all grades in a school to generate a mean reading score and mean math score for each school during each year of the sample period.

For years 2000-2003, the SFUSD provides grade-level and school-level data on 10 ethnic categories: African American, Latino, Chinese, Japanese, Korean, Filipino, American Indian, other white, other non-white and "declined to state". The "declined to state" category consisted of 2,771 students or 2.9% of the entire sample and was excluded from the diversity calculations. For this phase of the project, one observation is equal to one school-grade-year.

Time constraints required that the scope of first part of the study be limited to years 2000-2003. Thus the total sample used in the diversity regressions consists of all students in San Francisco's 53 elementary schools with attendance areas[1]. In addition to these 53 schools, there are 15 nonselective alternative schools that were excluded from

[1] Since 2003, three of the elementary schools studied (Cabrillo, DeAvila, and Golden Gate) have been closed.

the study[2]. Observations for nine cohorts are reported in this paper, of which three cohorts were affected by the expanded school choice (those entering kindergarten after 2000-01) and two cohorts were subject to the diversity index (those entering kindergarten after 2001-02). To account for the within-group correlation of errors caused by the fact that the same cohorts are observed multiple times during the time period of the study, I have clustered the errors by school and cohort.

Table 4.1 shows relevant summary statistics of the variables used to estimate changes in grade-level diversity. As indicated by the summary statistics, the SFUSD is quite diverse and the vast majority of students attending the included schools received free or reduced-price lunches.

Summary Table 4.1: Summary of Key Education Variables

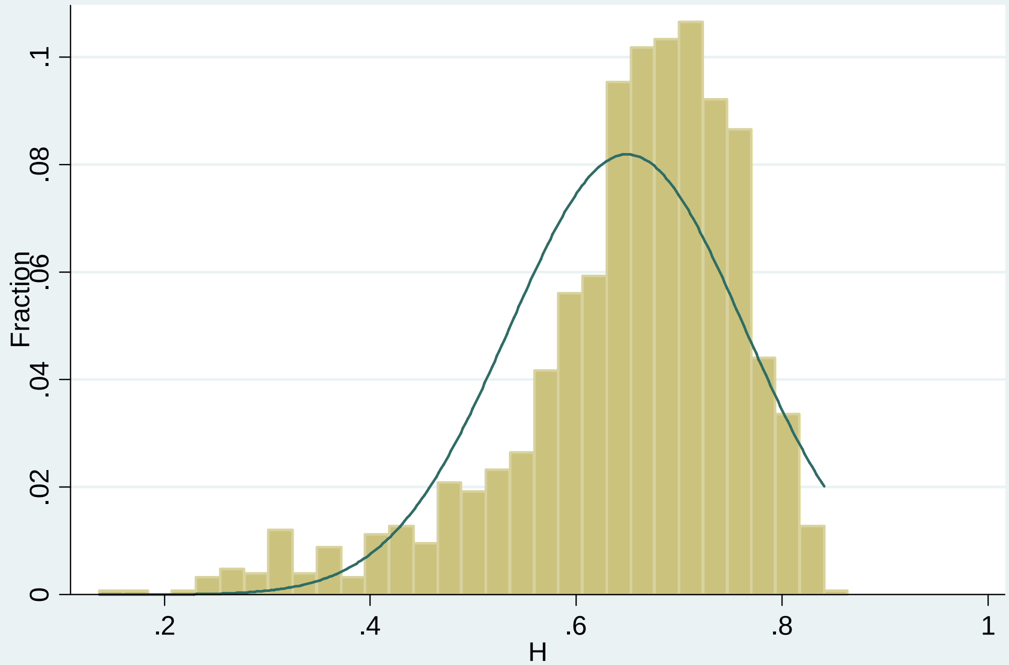
Variable	Mean	Standard Deviation	Range
H (diversity measure)	0.65	0.11	0.137-0.84
Percent of Elementary students receiving free or reduced lunch in 1997	0.70	0.17	0.258-0.941
1997 Elementary Math National Percentile	55.8	15.6	21-88

Motivating the implementation of the diversity index was ostensibly the wide range of H across schools. Figure 1 presents a histogram of H values across all grades over all years covered in this study. The long tail toward the left of the distribution shows that while the majority of schools are quite diverse with H values between 0.6 and 0.8, there are a minority of extremely segregated schools with H values of less than .25.

[2] "Attendance area" schools are those that were formerly neighborhood schools. In addition, the SFUSD has 15 nonselective "alternative" elementary schools and several charter schools. Neither alternative nor charter schools have attendance areas

These schools tended to be located on the southeast portion of the city and were mostly attended by African-American students.

Figure 4.1 -- Histogram of H values across SFUSD grades K-5



4.2 Estimating Changes in Diversity

To measure the extent of segregation caused by the expansion of choice, I measure diversity using a Herfindahl index for each grade based on the ethnic shares of nine ethnicities recognized by the SFUSD. The Herfindahl index consists of the sum of the squared shares of each ethnicity represented in the grade, and has values ranging from 0 to 1. For my regressions, I define H as one minus the Herfindahl index such that 0 indicates perfect homogeneity and values approaching 1 indicate increasing ethnic diversity within a grade:

$$(4.1) \qquad H_{ijt} = 1 - \sum_{e=1}^{n} s_{eijt}^{2}$$

where s_e is the share of all students within grade i at school j in year t that are of the e[th] ethnicity. As defined in my regressions, H_{ijt} can also be interpreted as the probability that two students chosen at random will be of different ethnicities (Hoxby 2000).

To begin my investigation of the policy's effect on grade-level diversity within schools I estimate an ordinary least squares regression of the following form:

$$(4.2) \qquad H_{ijt} = \beta_1 D_{policy\,2001} + \beta_2 (Year_t) + \varepsilon$$

where the policy dummy, D_{policy}, is equal to 1 for all cohorts affected by the 2001 policy change and is equal to zero for cohorts not affected (those who entered kindergarten before 2001). β_1 is therefore interpreted to be the change in H_{ijt} caused by the expansion of school choice. β_2 controls for a linear time trend to account for the diminishing diversity in the public school system as a whole due to a declining white student population and increasing Asian and Latino populations and ε is a random error term.

To test the prediction that the policy had different effects depending on the socioeconomic status of its students, I test a specification in which the policy dummy is interacted with the percentage of students at the school who received free or reduced price lunches in 1997:

$$(4.3) \qquad H_{ijt} = \beta_1 D_{policy\,2001} + \beta_2 (Year_t) + \beta_3 (lunch97_j * D_{policy\,2001}) + \varepsilon$$

The percentage of students receiving free or reduced-priced lunches at the school is fixed at the 1997 level so as to avoid reverse-causality in the case that diversity and socioeconomic status of students are related. β_1 is now interpreted to be the effect of the 2001 policy on a school with zero students receiving free or reduced-price lunches, while β_2 is interpreted to be the marginal change in H_{ijt} associated with a 100 percent increase in free-lunch students after the expansion of choice.

To ensure that I do not over-estimate β_2 by capturing the effect of having higher or lower concentrations of free-lunch students, I also test a specification that controls for the 1997 percentage of students receiving free or reduced-price lunch at the school:

$$(4.4) \quad H_{ijt} = \beta_1 D_{policy2001} + \beta_2 (Year_t) + \beta_3 (lunch97_j * D_{policy2001}) + \beta_4 (lunch97_j) + \varepsilon$$

Finally, I test a more restrictive specification that includes school fixed effects to control for unobservable differences between schools that might influence how grade-level diversity was affected by school choice:

$$(4.5) \quad H_{ijt} = \beta_1 D_{policy2001} + \beta_2 (Year_t) + \beta_3 (lunch97_j * D_{policy2001}) + \beta_4 (SchoolDum_j) + \varepsilon$$

It is possible that all of the spurious variation in H_{ijt} is due to changes captured by the lunch97 variable and that using school dummies instead of lunch97 only eliminates variation in the data due to unobservable factors. In this scenario one would expect the use of school dummies to reduce standard errors but not the actual coefficients of the policy dummy or policy interaction term. The coefficient on the linear time trend should not change with the addition of school fixed effects, as it is a measure reflecting city-wide trends in diversity.

To look at the combined effects of school choice and the diversity index selection mechanism, I test specifications where D_{policy} is equal to one for all cohorts affected by the 2002 implementation of the diversity index selection mechanism, or those who entered kindergarten after 2002:

$$(4.6) \quad H_{ijt} = \beta_1 D_{policy2002} + \beta_2 (Year_t) + \varepsilon$$

$$(4.7) \quad H_{ijt} = \beta_1 D_{policy2002} + \beta_2 (Year_t) + \beta_3 (lunch97_j * D_{policy2002}) + \varepsilon$$

$$(4.8) \quad H_{ijt} = \beta_1 D_{policy2002} + \beta_2 (Year_t) + \beta_3 (lunch97_j * D_{policy2001}) + \beta_4 (lunch97_j) + \varepsilon$$

$$(4.9) \quad H_{ijt} = \beta_1 D_{policy2002} + \beta_2 (Year_t) + \beta_3 (lunch97_j * D_{policy2002}) + \beta_4 (SchoolDum_j) + \varepsilon$$

In these models, β_1 is now interpreted to be the effect of the policy combination on grade-level diversity at a school with no free-lunch students. β_2 is now interpreted to be the marginal change in H_{ijt} associated with a 100 percent increase in free-lunch students after the implementation of both policies. For the regressions that estimate the combined effects of the two policies, I exclude the cohort that was affected by school choice but not by the diversity index selection mechanism (those who entered kindergarten in 2001), leaving 6 cohorts that were affected by neither policy and 2 cohorts that were affected by both policies.

I also attempt to separate the effects of the two policies using the following specifications:

$$(4.10)\quad H_{ijt} = \beta_1 D_{policy2001} + \beta_2 D_{policy2002} + \beta_3 (Year) + \varepsilon$$

$$(4.11)\quad \begin{aligned} H_{ijt} = {} & \beta_1 D_{policy2001} + \beta_2 D_{policy2002} + \beta_3 (Year) + \beta_4 (lunch97_j * D_{policy2001}) \\ & + \beta_5 (lunch97_j * D_{policy2002}) + \varepsilon \end{aligned}$$

$$(4.12)\quad \begin{aligned} H_{ijt} = {} & \beta_1 D_{policy2001} + \beta_2 D_{policy2002} + \beta_3 (Year) + \beta_4 (lunch97_j * D_{policy2001}) \\ & + \beta_5 (lunch97_j * D_{policy2002}) + \beta_6 (Lunch97_j) + \varepsilon \end{aligned}$$

$$(4.13)\quad \begin{aligned} H_{ijt} = {} & \beta_1 D_{policy2001} + \beta_2 D_{policy2002} + \beta_3 (Year) + \beta_4 (lunch97_j * D_{policy2001}) \\ & + \beta_5 (lunch97_j * D_{policy2002}) + \beta_6 (SchoolDum_j) + \varepsilon \end{aligned}$$

β_1 (β_2) now represents the effect of only the 2001 expansion of school choice (2002 implementation of the diversity index selection mechanism) on grade-level diversity at a school with no students receiving free or reduced-price lunches. The 2001 and 2002 policy*free-lunch interactions now only represent the marginal change in H_{ijt} associated with a 100 percent increase in free-lunch students after the implementation of their respective policies.

Finally, to test whether the effects of the 2001 school choice policy are concentrated at the extremes of the performance distribution, I run the following OLS regression using test-score and free-lunch quintiles as explanatory variables:

$$(4.14)\quad H_{ijt} = \beta_1(LunchQuintile * D_{policy2001}) + \beta_2(Year) + \beta_3(SchoolDum_j) + \varepsilon$$

$$(4.15)\quad H_{ijt} = \beta_1(TestQuintile * D_{policy2001}) + \beta_2(Year) + \beta_3(SchoolDum_j) + \varepsilon$$

Equations (4.14) and (4.15) respectively enable one to see specifically how the most disadvantaged schools and lowest-achieving schools were affected by expanding school choice. Again, the values used to calculate test-score and free-lunch quintiles are fixed at 1997 values so as to avoid reverse causality.

Summary Table 4.1: Description of Variables Used in Diversity Regressions

Variable Name	**Explanation**
H (dependant variable)	1 – Herfindahl index of grade level; 0 indicates perfect homogeneity
policydum	= 1 for cohorts affected by the 2001 policy
Lunch97	Percent of students at the school receiving free or reduced price lunch in 1997
Lunch97interation	Interacts the policy dummy with the percentage of students receiving free or reduced lunch at the school in 1997
LunchQuintile	Interacts policy dummy with which quintile the school falls in with respect to percentage of students receiving free lunch in 1997 1st quintile corresponds with schools with the highest percentage of students receiving free or reduced-price lunches
TestQuintile	Interacts policy dummy with 1997 Math-score Quintiles (calculated at the city-wide, not nationwide level) 1st quintile corresponds with highest-scoring schools
Year	Controls for a linear time trend and is equal to the year of observation
SchoolDum	Controls for school fixed effects

4.3 Grade-level Diversity Results

Table 4.1 presents my estimates of the effect of the 2001 school choice expansion in the SFUSD. Column (1) of Table 4.1 presents the results when equation (4.2) is estimated using the grade-level data. Though insignificant, the policy coefficient is negative, indicating that on average, within-school grades were made less diverse by the policy. The coefficient on the linear time trend if negative and significant, indicating that within-school grades in the district are getting less diverse each year. The negative time trend in diversity is robust to all specifications tested in this thesis.

Table 4.1: The Effect of SFUSD School Choice on Grade-level Diversity

Independent variable	(1)	(2)	(3)	(4)	(5)
policydum2001	-0.0213	**0.214*****	0.0574	**-0.0213*****	**0.0574****
	(0.014)	(0.043)	(0.047)	(0.0062)	(0.025)
year	**-0.00999*****	**-0.00999*****	**-0.00999*****	**-0.00999*****	**-0.00999*****
	(0.0021)	(0.0021)	(0.0020)	(0.0015)	(0.0015)
lunchinteraction2001		**-0.336*****	-0.112		**-0.112*****
		(0.065)	(0.072)		(0.037)
lunch97			**-0.224*****		
			(0.032)		
School fixed effects	No	No	No	Yes	Yes
Observations	1248	1248	1248	1248	1248
R-squared	0.02	0.08	0.16	0.73	0.73

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Column (2) presents my estimates of the effect of school choice once the effect of the policy is allowed to vary by the percentage of students receiving free or reduced-price lunches. Once socioeconomic status is controlled for, the policy appears to have had a positive effect on diversity. The coefficient on the interaction between the percent in the lunch program and the policy is negative and indicates that a school with 100 percent of students receiving free or reduced-priced lunches would have experienced a .336 decline in its Herfindahl measure—a decrease of about three standard deviations. Dividing the policy coefficient by the coefficient on the interaction term gives a value of .63, which is interpreted to be the share of free-lunch students at which the policy would have resulted in no change in diversity.

Column (3) presents my estimates once a school's socioeconomic status is controlled for using its 1997 percentage of students receiving free or reduced-price lunch. Though still retaining their signs from column (2) both the policy and interaction term coefficients become smaller in magnitude insignificant, indicating that they were overestimated before controlling for socioeconomic status. The positive and significant coefficient on the lunch97 term indicates that schools with less-advantaged students tend also to be less diverse, as is typical in many urban school districts.

Columns (4) and (5) are the same as columns (1) and (2) but with addition of school dummies so as to control completely for unobservable differences between schools. The addition of school dummies does not change my estimates of the policy coefficient or the coefficient on the interaction term, but it lowers standard errors sufficiently to make those estimates statistically significant. This result suggests that the addition of school dummies eliminates unexplained variation in H and makes it easier to isolate and identify the effects of the policy. Looking at the estimates in columns (4) and (5), it is again the case that school choice appears to have a negative aggregate effect on diversity before allowing the policy's effect to vary with a school's free-lunch percentage. Dividing the policy coefficient by the coefficient on the interaction term in column (5) gives a value of 0.51, indicating that a school with 51 percent of students receiving free or reduced-price lunches would have experienced no change in diversity after the implementation of school choice. A school with more than 51 percent would have experienced a decrease in diversity and a school with less than 51 percent would have experienced an increase in diversity after the expansion of choice.

As Summary Table 4.1 indicates, the minimum value of lunch97 was .258. Column 5 (Table 4.1) implies that the 2001 policy caused an elementary school with the minimum percentage to have experienced approximately a .03 increase in its H measure of diversity. My results imply that the policy would cause a school with the mean percentage of students receiving free or reduced price lunches in 1997 to experience a decrease in the diversity measure by almost 2 standard deviations. On average, the results indicate that schools with less than 50% of students receiving free or reduced-price lunches were made more diverse by the policy. However, of the elementary schools in my sample only about ten percent had less than 50% of students receiving free or reduced-price lunches.

The 2002 implementation of the diversity index, however, appears to have softened the segregating effects of choice. Table 4.2 presents my estimates of the combined effects of the two policies:

Table 4.2: The Effect of the SFUSD Diversity Index on Grade-level Diversity

Independent variable	(1)	(2)	(3)	(4)	(5)
policydum2002	-0.0229	**0.231*** **	0.0746	**-0.0229*** **	**0.0746** **
	(0.017)	(0.050)	(0.054)	(0.0086)	(0.032)
year	**-0.0101*** **	**-0.0101*** **	**-0.0101*** **	**-0.0101*** **	**-0.0101*** **
	(0.0022)	(0.0022)	(0.0021)	(0.0015)	(0.0015)
lunchinteraction2002		**-0.363*** **	-0.139		**-0.139*** **
		(0.079)	(0.085)		(0.048)
lunch97			**-0.224*** **		
			(0.032)		
School fixed effects	No	No	No	Yes	Yes
Observations	1092	1092	1092	1092	1092
R-squared	0.02	0.07	0.16	0.71	0.72

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Columns (1) through (5) in Table 4.2 follow the same pattern as the results reported above in Table 4.1. It is important to note that the cohort that entered kindergarten in 2001 was excluded from these estimates as they were affected by the expansion of choice but not the diversity index. As presented in column (5) in Table 4.2, the diversity index increased the H measure for a school with no free lunch students by

0.0746, but the lunch interaction term remains similar at -0.139. The higher value for the policydum2002 coefficient, however, has profound implications for elementary school districts. Table 4.1 implies that schools with more than 50% of its students receiving free or reduced lunches were made less diverse by expanding school choice. Table 4.2 implies that with the implementation of the 2002 diversity index, schools with 70% or more students receiving free of reduced lunches were made less diverse. Therefore while only 10 percent of schools were made more diverse by the 2001 policy, approximately 50 percent of schools in the SFUSD were made more diverse after the diversity index was implemented.

The fact that the two policies were employed in consecutive years makes it difficult to isolate the effects of the diversity index separately from choice. Results table 4.3 presents the results of a model which incorporates both the 2001 expansion of choice and the 2002 diversity index separately

Results Table 4.3: Separating the Effects School Choice and the Diversity Index on Grade-level Diversity

Independent variable	(1)	(2)	(3)	(4)
policydum2001	-0.0196	0.0403	**-0.0196****	0.0403
	(0.020)	(0.070)	(0.0081)	(0.034)
policydum2002	-0.00361	0.0340	-0.00361	0.0340
	(0.024)	(0.083)	(0.011)	(0.042)
year	**-0.00984*****	**-0.00984*****	**-0.00984*****	**-0.00984*****
	(0.0020)	(0.0019)	(0.0014)	(0.0014)
lunchinteraction2001		-0.0855		**-0.0855***
		(0.11)		(0.050)
lunchinteraction2002		-0.0538		-0.0538
		(0.13)		(0.063)
lunch97		**-0.224*****		
		(0.032)		
School Dummies	No	No	Yes	Yes
Observations	1248	1248	1248	1248
R-squared	0.02	0.16	0.73	0.73

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Though neither of the policydum coefficients is statistically significant, they take on the expected signs and relative magnitudes. Recall that the coefficient on the 2002 policy dummy is still interpreted to be the sum effect of the two policies, as all cohorts who entered kindergarten in 2002 and later were affected by both the expansion of choice and the diversity index selection mechanism. Both policies appear to decrease overall diversity in columns (1) and (3), before the free-lunch interaction term is included. While the 2001 policy initially decreased the Herfindahl measure by .0086 with every 10 percent of students receiving free or reduced lunch, the 2002 diversity index appears to have lessened this effect somewhat, evidenced by the 2002 policy coefficient's smaller magnitude. The 2002 coefficients are not statistically significant, however, which is not surprising since the policies were implemented in consecutive years, making it difficult to isolate the separate effect of each policy on grade-level diversity.

Results Table 4.4 shows the results of the regressions that use lunch quintiles (as opposed to percents) and math-score quintiles as explanatory variables. I specifically used math test scores for these regressions (instead of reading scores) to avoid endogeneity, since a school's average reading scores are largely reflective of the level of English-proficiency which is also closely related to that school's racial composition.

Results Table 4.4: Including Free Lunch Quintile and Math Score Quintile Interaction Terms

Independent variable	(1)	(2)
year	**-0.00980***	**-0.0108***
	(0.0015)	(0.0015)
LunchQuintile1 interaction	**-0.0607***	
	(0.015)	
LunchQuintile2 interaction	**-0.0334**	
	(0.014)	
LunchQuintile3 interaction	-0.00254	
	(0.013)	
LunchQuintile4 interaction	-0.0180	
	(0.011)	
LunchQuintile5 interaction	Dropped	Dropped
TestQuintile1 interaction	Dropped	Dropped
TestQuintile2 interaction		-0.0217
		(0.014)
TestQuintile3 interaction		0.00760
		(0.013)
TestQuintile4 interaction		-0.0144
		(0.0100)
TestQuintile5 interaction		**-0.0504***
		(0.016)
School Dummies	Yes	Yes
Observations	1196	1196
R-squared	0.73	0.73

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

For the LunchQuintile variable (column 1), quintile one corresponds to schools with the most students receiving free lunch. For the TestQuintile variable (column 2), quintile one corresponds to the schools scoring highest in math in 1997. Not surprisingly, the policy had very similar impacts on both the highest lunch-quintile schools and the lowest testquintile schools. Schools in the top lunch-quintile appear to exhibit the most extreme decline in diversity in response to the policy, implying that parents are less willing to send their children to schools with high levels of poverty. The fact that only the top two quintiles in the lunch category and only the bottom quintile for test scores had significant coefficients suggests that a handful of schools are being severely affected, while schools toward the middle of the distribution are less affected by choice.

The fact that the year coefficient is negative and significant at 99% confidence in all models provides evidence that the SFUSD as a whole is getting less diverse each year. This appears to be at least partially attributable to the declining white student population, as from 1998 to 2003 the percentage of white students attending a San Francisco public school declined from 11 percent to 8.4 percent[3]. The declining overall diversity also speaks to the limits of public school policy in promoting diversity, as more privileged students will always have the option of leaving the district altogether.

My results indicate that the 2001 policy combination of expanded school choice coupled with the diversity index selection mechanism appears indeed to have had different effects on different types of schools. While higher achieving and more affluent schools were made more diverse by the two policies, lower achieving and poorer schools were made significantly less diverse. The homogenizing effects of school choice appear to be countered somewhat by the 2002 implementation of the diversity index as a selection mechanism for oversubscribed schools, but it is difficult to identify the effect of each policy separately as they were applied consecutively. I also find strong evidence that the district as a whole is getting less diverse, likely a result of the growing Asian and

[3] These figures were obtained from the school profiles available at www.sfusd.edu (accessed January 2007). It is important to note that these percentages reflect all schools in San Francisco, including those excluded from my sample.

Latino student populations and declining White student population. It is also important to reiterate that these results are only indicative of the short-term effects of the two policies. Because students are still given priority admission to the school their older sibling attends, one could also assume that the redistributive effects of the new assignment policies will intensify with time.

CHAPTER 5: EFFECTS ON CAPITALIZATION

Having established that school choice had a significant impact on the ethnic composition of elementary schools in the SFUSD, I now begin my discussion of the policy's effect on property values. The fact that student populations were significantly affected by choice clearly shows that schools are attracting different populations of students than they were before the expansion of school choice. As discussed above, numerous studies have documented that in a neighborhood-based school-assignment system school quality has a significant impact on the values of homes in the area. Switching from a neighborhood-based system to a choice-based system should therefore have a significant on the housing market as school-quality capitalization in home prices would no longer reflect the quality of local neighborhood schools but rather reflect the quality of schools in the SFUSD as a whole.

5.1San Francisco Home Sales Data

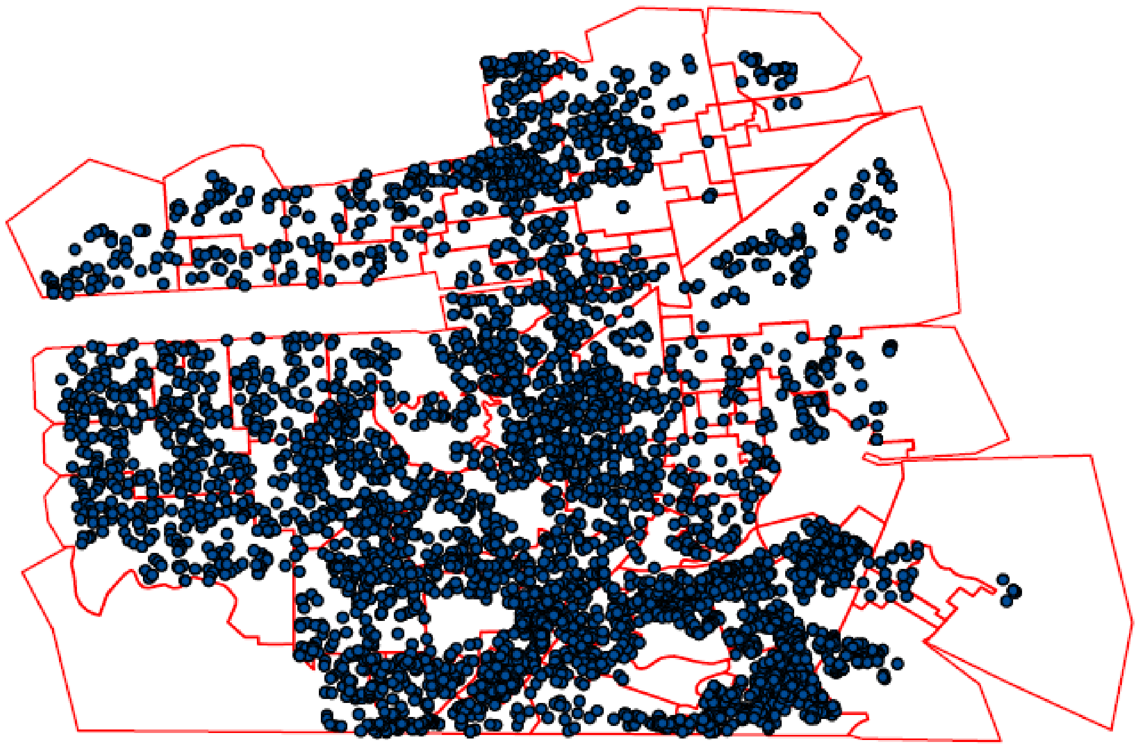
The typical approach to studying capitalization of school quality into house prices, estimation of a hedonic model, requires data on house prices and characteristics. Previous studies that use house price capitalization as a means to judge the value of public education have used publicly available property tax data (see Barrow and Rouse 2003, Black 1999, Figlio and Lucas 2000 and Kane, Staiger and Reigg 2005). In San Francisco, however, data from the tax assessor list a property's assessed value rather than its actual sales price. Under California Proposition 13, homes are reappraised when there is a change in ownership or upon completion of new construction on the property. While assessed value data are available for the city of San Francisco, it is unclear whether the data differentiate between homes that were reappraised due to change in ownership (where the assessed value would simply be equal to the sales price) or reappraised due to new construction (where the assessed value would reflect the physical characteristics of the addition and not necessarily the capitalization of local public goods) tax data are not used in this paper.

Data on home sales were therefore obtained from the Multiple Listing Service (MLS) database. The complete home sales data obtained from the MLS cover the period 1997-2004 and include 31,420 sales. The MLS is a database that allows real estate agents representing sellers under a listing contract to post information about properties that is widely available to other real estate agents representing potential buyers. The MLS database for San Francisco properties is owned by the San Francisco Association of Realtors (SFAR) and its use is restricted to member real estate agents. However, information from the MLS database may be shared at the discretion of SFAR members and the historical data that will be used in this paper were provided courtesy of a member agent.

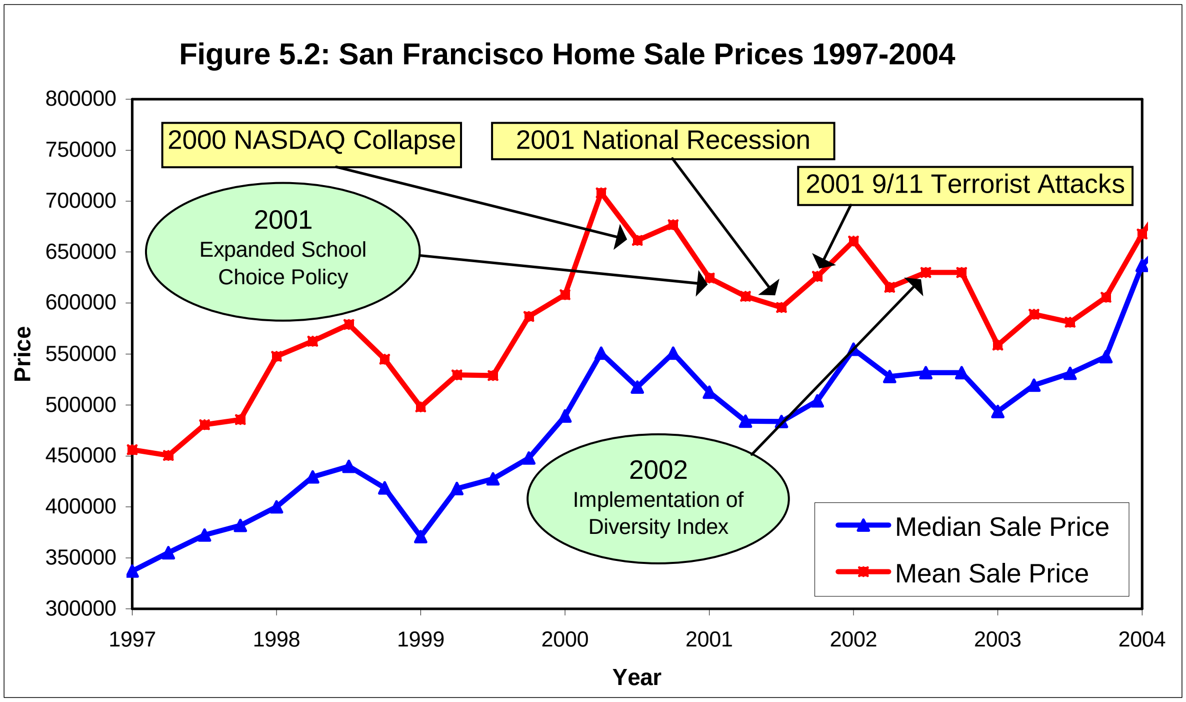
MLS data are preferable to tax data because the MLS database lists actual sales prices and addresses for all single-family houses and condos sold through a broker in San Francisco, though it excludes for sale by owner transactions. In addition to sale prices and sale dates it also lists property value determinants including number of bedrooms, number of bathrooms, square footage and the number of parking spaces on the property. A central obstacle in measuring school quality capitalization is being able to isolate increases in home price associated with better schools from the price premium associated with living in certain neighborhoods. This is another advantage of using MLS data, as the SFAR divides the city into 10 real estate districts and 84 sub-districts, allowing me to control for fixed neighborhood effects of varying specificity. These districts and sub-districts are drawn by the SFAR to reflect neighborhoods and are independent of school attendance boundaries. The data also include 3,325 properties that were sold multiple times during the time period studied, enabling a specification that focuses on repeat sales and controls for address-level fixed effects. As shown in Figure 5.1, repeat sales appear to be evenly distributed throughout the city.

The MLS listing does not include the lot size associated with the property, however it is a reasonable assumption that controlling for square footage and parking spaces will also control for a large amount of the variation in lot size, considering the homes are all located within San Francisco city limits and thus the lot sizes rarely exceed the square footage of the house and parking spaces by a significant amount. The MLS data provide the addresses, sale prices and property characteristic, but provide no Geographic Information System (GIS) coordinates or any other geographic data. Addresses were thus plotted on a street map of San Francisco using GIS software, described below.

Figure 5.1: All repeat sales in sample plotted on SFUSD Elementary School boundary map



A central difficulty in measuring capitalization rates in the SFUSD is controlling for the macroeconomic activity that coincided with the school choice legislation in 2001. It is important to note that 1997-2004 was a period of rapid growth and sudden economic recession around the nation and especially in the San Francisco Bay Area. Figure 2 illustrates the short-term fluctuations in mean and median property values during the time period for the study as well as the some major events that are likely responsible for the trends for the city as a whole. Property values increased rapidly through the late 1990s until growth stagnated with the 2001 recession, which hit the San Francisco Bay Area particularly hard as it was in the center of the rapid information technology expansion that characterized the late 1990s.



Source: Author's calculation from SFAR MLS data

5.2 School Data

For the test-score capitalization estimates, I augment the school data used in the diversity estimates to include from 1998-99 school year through 2002-03. I also expand the data to include 13 neighborhood middle schools and 7 neighborhood high schools[4].

To generate a measure for the local school-quality associated with a particular address, I averaged all test scores (National Percentile and National Curve Equivalent scores from grades 2 through 11) to generate an average score for each school. After matching addresses to three sets of attendance boundaries (discussed below) I average these scores to obtain one average test score measure for each home. In my regressions I

[4] In addition to these neighborhood middle and high schools, there are 5 alternative middle schools and 13 alternative high schools that were excluded from the study because alternative schools have never guaranteed admission to students living within attendance boundaries. Additionally, two middle schools included in the study (Luther Burbank MS and Ben Franklin MS) have closed since 2003. No information was available on whether these closures were related to having low-demand under the expanded choice policy. One high school, McAteer HS, was closed due to low performance in the spring of 2002—one year after the SFUSD assignment eliminated assignment boundaries. As such, there is no test score data available for McAteer after 2002 and the AvgTestScore variable for homes within McAteer's attendance boundary is calculated using only elementary and middle school scores.

used the natural log of this average, denoted lnTestScore, to estimate the elasticity of San Francisco home prices to standardized test scores.

5.3 Combining School data with MLS Data

The estimates in this thesis are generated from a unique set of data that were compiled by combining the data obtained from the SFUSD annual school profiles and real estate transaction data obtained from the MLS database.

Using addresses provided by the MLS, I used GIS software to plot each address on an ESRI street map of San Francisco. Using detailed maps of attendance boundaries obtained from the SFUSD website, I was able to place the plotted homes within three different sets of attendance boundaries. The most recent attendance area maps were drawn in 1999, at which time there were 81 elementary school attendance areas for the district's 53 non-alternative elementary schools, 26 middle school attendance areas for the district's 13 non-alternative middle schools and 9 attendance areas for the district's 7 non-alternative high schools. The addresses that were successfully placed on the map (approximately 95% of the addresses) were then matched to Elementary, Middle, and High school attendance areas using polygons drawn based on detailed attendance boundary maps obtained from the SFUSD website (www.sfusd.edu).

As long as a property could be matched to at least one school, it was included in the data set. Due to random errors in the geocoding and attendance boundary-drawing process, 1,636 of the homes were not matched to an elementary school, 2,393 were not matched to a middle school and 78 were not matched to a high school. Average test scores for these observations thus represent the mean published test score of as many schools as were successfully matched to the property. There were also twelve attendance areas for which two elementary schools were listed as being the neighborhood school. For the 7,383 properties in the sample located within one of these districts it was unclear which school a child at that address would be assigned. In these cases the average test score was calculated using both elementary schools—in all such districts the two elementary schools in question were of similar quality. Additionally, some of the successfully matched properties were missing data for square footage and those properties were necessarily excluded from regressions that used square footage as an explanatory variable.

Though the new school choice policy took effect in the 2001-2002 school year, it is unclear when the details of the new policy became common knowledge and would thus affect capitalization rates. The SFUSD enrollment process takes place during October through March. Parents participating in the enrollment process for the 2001-2002 school year would have thus been aware of the new policy at some point during this process. Additionally, those involved with the school board or school board deliberations might have known about the new assignment system beginning in early 2001. Widespread coverage of the new assignment system in the San Francisco Chronicle, however, did not appear until late 2001—with the start of the classes for the first cohort affected by the policy. Because potential homebuyers could have been made aware of the policy at any point during 2001, all transactions that took place during 2001 are dropped from the regressions.

The total dataset used in this paper consists of 18,683 properties that were successfully matched with the neighborhood elementary, middle and high schools a student living at that address would have been assigned to before the policy change in

2001. The mean sale price of successfully matched homes in the sample was $614,474 and the median price was $520,000.

Table 5.1: Summary of Variables Used in Capitalization Estimates

Variable	Mean	Standard Deviation	Range
Sale price	$614,474	$451,360	$1000-$16,000,000
Elementary School average published test score 1997-2003 (all grades, math and reading)	49.5	10.5	33-71
Middle School average published test score 1997-2003	48.3	12.3	33-69
High School average published test score 1997-2003	41.7	12.5	29-58
AvgTestScore (average of all published scores matched to an address)	47.8	9.3	27.5- 67.4

5.4 Estimating Changes in Capitalization of School Quality in House Prices

I estimate San Francisco home prices using a standard hedonic regression. Hedonic regression is based on the idea that a good can be considered as a bundle of characteristics. Each characteristic is assumed to have its own market and its price is determined separately from the other components. A hedonic regression thus incorporates the assumption that the total price of a good, such a house, can be decomposed into the sum of the prices of its characteristics. Public school quality is assumed to be a public good associated with particular homes, thus using hedonic regressions one can ascertain the market value placed on school quality (as measured by test scores).

For the capitalization estimates, the only significant policy change is the 2001 expansion of school choice. One would expect school choice to have a negative impact on the extent of capitalization of neighborhood schools as one's address no longer guarantees access to local schools. To estimate the change in marginal willingness to pay (MWTP) to live near schools with higher test scores, I estimate home prices using the following standard hedonic regression:

$$\begin{aligned} \ln SalePrice_{ktm} &= \beta_1 BD_k + \beta_2 BA_k + \beta_3 PK_k + \beta_4 \ln SqFt_k + \beta_5 House_k \\ &+ \beta_6 \ln AvgTestScore_{kt} + \beta_7 \ln TestInteraction_{kt} + \beta_8 MonthDum_t + \varepsilon \end{aligned} \tag{5.1}$$

where ln$SalePrice_{ktm}$ is the natural log of the sale price of a home k in year t and month m. BD is equal to the number of bedrooms, BA the number of bathrooms, PK is the number of parking spaces on the property, SqFt is the reported square footage and House is a dummy variable that equals one if the property is a single family home and zero if the property is a condominium, duplex or apartment unit. BD, BA, PK, SqFt and House were all obtained directly from the MLS data.

B_8 controls for monthly fixed effects for the month and year of the property's sale date. The 2001 expansion of school choice in the SFUSD coincided with dramatic increases in property values due to the information technology boom in the late 1990s and the stagnation of property values during the recession that followed. It is likely that this "dot-com" boom and bust resulted in spurious non-linear variation in property values during the time period of the study. As demonstrated in Figure 4.2, the mean price of a home in San Francisco was increasing steeply from 1997-2000 and then stagnates from 2001-2004. If unaccounted for, these non-linear trends in home prices could bias estimates of the 2001 policy's effects on test-score capitalization. Incorporating monthly dummies into my model enables me to control for monthly fluctuations in price for the properties in my sample.

Log prices are used to prevent my estimates from being skewed by properties at the very top or bottom of the price distribution. Such outlying properties may contaminate the sample in two ways. First, being that San Francisco has one of the highest private school enrollment rates in the nation, families purchasing in the more expensive areas are less likely to consider the quality of local public schools in determining their marginal willingness to pay for a home. Second, it is possible that extremely high or low prices might reflect an unobservable characteristic such as being built by a famous architect or having prominent former owners.

As an alternative to using monthly fixed effects, I also estimate an alternative specification that adjusts the sale price using the Office of Federal Housing Enterprise Oversight (OFHEO) house price index for the San Francisco Metropolitan Statistical Area (MSA). The index is updated quarterly and is a measure designed to capture changes in the market for single family homes. San Francisco is included as one of the MSAs covered by the program, so the index can be used specifically to adjust the sale prices to reflect macroeconomic changes in the market for single-family homes in San Francisco. Appendix Table A.3 plots the values of Northern California OFHEO index values from 1975-2007 and demonstrates the fluctuations in the housing market that coincided with my study. The indexed sale price, denoted as IndexPrice in my regressions, is therefore the sale price divided by the San Francisco OFHEO index. Because the OFHEO index is updated quarterly, I replace the monthly fixed effects with year fixed effects in these specifications to avoid multicollinearity[5].

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[5] I also test a third specification that takes the log of the sale price adjusted using the CPI shelter index as the dependent variable.

$lnAvgTestScore_{kt}$ is the natural log of the mean value of the most recent published test scores for the local neighborhood elementary, middle and high schools.[6] Policy2001 is a dummy variable that is equal to 1 if the home was sold after 2001 and zero if the home was sold prior to 2001. Policy2001 was not included in the actual regressions because it is collinear with the year fixed effects, but it was used to calculate the TestInteraction term. TestInteraction is an interaction term which is equal to the policy dummy times the natural log of average test score for the neighborhood schools associated with the property.

Of primary interest are the estimated values of β_6 and β_7. β_6 is interpreted as the elasticity of home price to test scores in San Francisco and is my measure of school-quality capitalization. B_7 is interpreted as the change in this elasticity resulting from the 2001 expansion of school choice. If a policy of total school choice completely eliminates the perceived benefits of living in the attendance area of high-performing schools, then one would expect β_7 to be of the same magnitude but opposite sign of β_6, completely wiping out any capitalization effects in San Francisco. However, as discussed above there is still some benefit of living near a desirable school. When admitting applicants a school first considers the diversity profiles of the students within its attendance boundary, so proximity is still somewhat important in the school assignment process. Additionally, living with a school's attendance boundary means that if a child is admitted to that school he or she will have a shorter commute. For these reasons I do not expect the effects of choice to completely wipe out capitalization effects.

[6] By "local neighborhood school" I am referring to the schools associated with the three attendance areas matched to the address during the geocoding process.

Black (1999) finds hedonic regressions to be susceptible to omitted variable bias and to bias upward estimates of school quality capitalization when neighborhood effects are not accounted for. Omitted variables in this regression might be average income or crime rate in the surrounding area. Such unobserved neighborhood effects would overstate the extent to which school quality is capitalized if higher-performing schools are located in neighborhoods regarded as being better for reasons unrelated to educational quality.

Transactions recorded in the MLS database are coded by real-estate district and sub-district. The 10 districts and 84 sub-districts are defined by the San Francisco Association of Realtors and are indicative of distinct neighborhoods and are drawn independently of school-attendance boundaries. The neighborhood identifiers provided in the data enable me to test the model by controlling for neighborhood fixed effects, where the neighborhoods are defined more broadly and more narrowly. As such I am able to run two additional specifications to control for district and sub-district fixed effects:

$$\begin{aligned}(5.2)\quad &\ln SalePrice_{ktm} = \beta_1 BD_k + \beta_2 BA_k + \beta_3 PK_k + \beta_4 \ln SqFt_k + \beta_5 House_k \\ &+\beta_6 \ln AvgTestScore_{kt} + \beta_7 \ln TestInteraction_{kt} + \beta_8 MonthDum_t + \beta_9 DistrictDum_t + \varepsilon\end{aligned}$$

$$\begin{aligned}(5.3)\quad &\ln SalePrice_{kq} = \beta_1 BD_k + \beta_2 BA_k + \beta_3 PK_k + \beta_4 \ln SqFt_k \\ &+\beta_5 House_k + \beta_6 \ln AvgTestScore_{kt} + \beta_7 \ln TestInteraction_{kt} \\ &+\beta_8 MonthDum_t + \beta_9 SubdistrictDum + \varepsilon\end{aligned}$$

Though these regressions help eliminate bias due to omitted neighborhood effects, they also restrict the variation in school quality used to estimate capitalization. As the neighborhood controls get more restrictive, the model becomes increasingly reliant on within-school rather than across-school variations in school quality.

It is also possible that the adoption of complete school choice may have had some effect on neighborhood quality. Such a scenario would be possible if merely having higher concentrations of families with school-aged children was particularly desirable or undesirable to homebuyers. To investigate the extent to which neighborhood effects were influenced over time by factors unrelated to school quality, I test a specification that includes an interaction between the neighborhood and policy dummies to control for changes in neighborhood quality over time:

$$\begin{aligned} &\ln SalePrice_{ktm} = \beta_1 BD_k + \beta_2 BA_k + \beta_3 PK_k + \beta_4 \ln SqFt_k \\ (5.4)\quad &+\beta_5 House_k + \beta_6 \ln AvgTestScore_{kt} + \beta_7 \ln TestInteraction_{kt} \\ &+\beta_8 YearDum_t + \beta_8 Subdistrict * Year_t + \varepsilon \end{aligned}$$

This specification would also control for any gentrification occurring in response to macroeconomic activity.

The sample also includes 3,325 properties that were sold more than once during the time period, so I also estimate a specification controlling for unit-level fixed-effects[7]:

$$\begin{aligned} (5.5)\quad &\ln SalePrice_{ktm} = \beta_1 BD_k + \beta_2 BA_k + \beta_3 PK_k + \beta_4 \ln SqFt_k + \beta_5 House_k \\ &+\beta_6 \ln AvgTestScore_{kt} + \beta_7 \ln TestInteraction_{kt} + \beta_8 YearDum_t + \beta_8 Unit + \varepsilon \end{aligned}$$

Controlling for fixed-effects at the unit-level helps control for unobservable characteristics about a property such as having a view or fancy banisters.

One weakness inherent to restricting my model with increasingly specific neighborhood or unit fixed effects is that the more restrictive model relies on within-school rather than across-school variations in test scores. A model that controls for unit fixed effects is completely reliant on within-school variation in order to estimate

[7] "Units" refer to specific units such as houses or individual units in a multi-family dwelling. That is, two units in the same building that have the same property characteristics (BD, BA, PK, Sqft) are not considered to be the same unit. The variation in these capitalization estimates thus comes from multiple sales of the same property during the observation period.

capitalization as attendance boundaries are fixed over the time period studied. Estimates that rely on within-school rather than across-school variation might understate the extent of capitalization if within-school test score changes over a short term were not perceived as permanent changes in school quality. In addition, test score variation over the short time period in my study may reflect noise rather than true variations in school quality (see Kane and Staiger 2002), which would also be less likely to affect the degree of capitalization.

While the absence of cross-school variation should not bias estimates of the change in capitalization observed after the expansion of choice, as the expansion of school choice effectively changes the public schooling options available to a given household.

5.5 Test Score Capitalization Results

Tables 5.2 and 5.3 present the capitalization estimates obtained when my specifications are tested using the entire sample of homes and condominiums.

Table 5.2: Hedonic regressions using entire sample

Dependent variable	Log of sale price			
Independent variables				
	(1)	(2)	(3)	(4)
Bedrooms	**-0.0379*** **	**-0.0156*** **	0.000282	-0.000313
	(0.0031)	(0.0025)	(0.0023)	(0.0022)
Bathrooms	**0.0870*** **	**0.0734*** **	**0.0663*** **	**0.0625*** **
	(0.0035)	(0.0028)	(0.0025)	(0.0025)
Parking spaces	**0.0327*** **	**0.0505*** **	**0.0453*** **	**0.0463*** **
	(0.0031)	(0.0024)	(0.0022)	(0.0021)
lnSqft	**0.733*** **	**0.601*** **	**0.525*** **	**0.522*** **
	(0.0084)	(0.0068)	(0.0062)	0.0060
House	**-0.0605*** **	**0.171*** **	**0.219*** **	**0.201*** **
	(0.0057)	(0.0052)	(0.0053)	(0.0055)
lnTestScore	**0.103*** **	**0.195*** **	**0.0579*** **	**0.077*** **
	(0.015)	(0.016)	(0.019)	0.025
lnTestInteraction	**0.145*** **	-0.00225	0.0203	-0.0182
	(0.021)	(0.017)	(0.015)	(0.036)
Neighborhood fixed effects	None	District	Sub-district	Sub-district * Year
Time fixed effects	Month Dummies	Month Dummies	Month Dummies	Month dummies
Observations	18683	18683	18683	18683
R-squared	0.66	0.79	0.84	0.85

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

As presented in Tables 5.2 and 5.3, the estimates do not change significantly when the log of the OFHEO index-adjusted price is used instead of the log of the sale price[8]. Because they produce very similar estimates of most coefficients, I will discuss the specifications presented in Tables 5.2 and 5.3 together.

[8] A specification was also tested which took as a dependent variable the sale price divided by the CPI shelter index for San Francisco. The coefficients estimated by this specification follow the same pattern of sign and significance as those reported in Tables 5.1 and 5.2 and are not reported in this thesis.

Table 5.3: Hedonic Regressions Using Entire Sample

Dependent variable	Log of sale price adjusted using SF OFHEO Index			
Independent variables				
	(1)	(2)	(3)	(4)
Bedrooms	**-0.0377*** **	**-0.0155*** **	0.0000912	-0.000475
	(0.0048)	(0.0034)	(0.0032)	(0.0032)
Bathrooms	**0.0875*** **	**0.0740*** **	**0.0672*** **	**0.0632*** **
	(0.0054)	(0.0044)	(0.0039)	(0.0039)
Parking spaces	**0.0325*** **	**0.0502*** **	**0.0451*** **	**0.0463*** **
	(0.0035)	(0.0029)	(0.0025)	(0.0024)
lnSqft	**0.732*** **	**0.600*** **	**0.524*** **	**0.521*** **
	(0.015)	(0.012)	(0.012)	(0.011)
House	**-0.0613*** **	**0.170*** **	**0.217*** **	**0.200*** **
	(0.0066)	(0.0061)	(0.0065)	(0.007)
lnTestScore	**0.100*** **	**0.195*** **	**0.0626*** **	**0.0849*** **
	(0.016)	(0.018)	(0.020)	(0.025)
lnTestInteraction	**0.150*** **	-0.000573	0.0229	-0.0188
	(0.019)	(0.017)	(0.015)	0.035
Neighborhood fixed effects	None	District	Sub-district	Sub-district * Year
Time fixed effects	Year dummies	Year dummies	Year dummies	Year dummies
Observations	18683	18683	18683	18683
R-squared	0.58	0.74	0.80	0.82

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Column (1) in Tables 5.2 through 5.6 present the capitalization estimates from my most basic specification which includes no neighborhood effects. When neighborhood effects are excluded, I consistently estimate large and statistically significant coefficients for lnTestScore, my measure for school quality. However, as Black (1999) finds in her analysis of test-score capitalization in Massachusetts, these estimates are likely biased upward by omitted neighborhood effects. If homes in the attendance areas for high-performing schools also have other desirable characteristics that are not observed in this paper (such as a low crime rate) then the coefficient on lnTestScore would reflect the value of these qualities as well as the value of living near good schools. The lnTestInteraction term is also subject to be biased upward by unobserved neighborhood effects. If neighborhoods with high-performing schools appreciated disproportionately after 2001 for reasons unrelated to school quality then this would account for the positive and significant policy interaction term.

My results also provide evidence that models that neglect neighborhood give biased estimates of other home characteristics. One way to increase the supply of homes in a good neighborhood with fixed acreage is to break up large buildings into condominium units. As a result one might expect higher concentrations of condominiums in good neighborhoods. A model that does not take neighborhood effects into account will therefore underestimate the value buyers place on owning a home rather than a condo. Indeed, column (1) of Tables 5.2 and 5.3 imply that designating a property as a house decreases the sale price by about six percent. A similar argument can be made for other characteristics related to lot size, such as the number of parking spaces on a property.

Column (2) of Tables 5.2 and 5.3 present the estimates obtained when district fixed-effects are included. The ten districts are depicted in Figure A.2 in the appendix, outlined in red. Using districts provides a relatively broad neighborhood control, so there are still unobserved differences in quality between areas within districts. An advantage of this specification, however, is that it controls for some neighborhood effects while still allowing for across-school variation in test scores, as there are 9 high school attendance areas, 26 middle school attendance areas and 81 elementary school attendance areas. This specification gives the largest estimates of home price elasticity to test scores, implying that a one standard-deviation increase in average test scores would result in a 1.8 percent increase in the price of a home associated with those schools. This result is roughly consistent with Black's (1999) capitalization estimates, though smaller in magnitude. This estimate may still be biased to some degree by unobserved neighborhood effects within districts, especially if one expects that school choice diminishes the extent to which school quality is capitalized as the SFUSD has substantially more choice than the Boston-area schools studied by Black. Controlling for neighborhood even at the district level produces a negative though statistically insignificant estimate for the effect of school choice on test-score capitalization. These estimates indicate that school choice decreased capitalization rates by about 1 percent.

Because each address from the MLS was also coded by sub-district, I am able to employ a more restrictive model that controls for fixed effects at the sub-district level. These results are presented in column (3) in tables 5.2 and 5.3. There are a total of 84 sub-districts, so these neighborhood effects are much more restrictive that the previous specification. A weakness with this specification is that with 84 sub-districts, the model is increasingly reliant on within-school rather than across-school variation[9]. The capitalization estimates produced by this model are much smaller than when district-level fixed effects are used, though are still positive and statistically significant at 99% percent confidence. Column (3) of both tables 5.2 and 5.3 estimate the elasticity of San Francisco house prices to test scores to be approximately 0.06, implying that a one-standard deviation increase in test scores would result in a 0.56 percent increase in house prices.

Though insignificant, the policy interaction term takes a positive sign in this specification, implying that school choice increased the value of living near high-

[9] Recall that there are 81 elementary school boundaries, 26 middle school attendance boundaries and 7 high school attendance boundaries

performing schools. This contrasts with my theoretical predictions about the effects of choice on capitalization. One potential reason for this result is that buyers did not perceive the expansion of choice as a permanent policy change, as there have been many reforms in the SFUSD school-assignment process over the years. It is also possible that capitalization changes occurred in the long term but were not immediately apparent in the housing market. This is a plausible explanation given that younger siblings still have access to the school their older sibling attends.

It is also possible that the policy did have an effect on test-score capitalization that is not identified in this specification due to neighborhood changes such as gentrification. This model assumes neighborhood effects to be constant over time, but that assumption might not have been true in San Francisco during the late nineties. As discussed above, there was a surge in the San Francisco housing market in the late 1990s, and then growth stagnated in response to the economic recession and dot-com bust of 2001. If certain neighborhoods were more attractive to "dot-comers" then those neighborhoods would have experienced different growth patterns in response to the economic boom and bust.

The results presented in column (4) of tables 5.2 and 5.3 are generated when the sub-district fixed effects are interacted with year dummies and allowed to change over time. This controls for the possibility that neighborhood effects were changing with time due to gentrification. Using this specification I get a similar estimate of price elasticity to test score (0.07-0.08) but the interaction term takes on the expected negative sign. These estimates indicate that the policy decreased test-score capitalization by about 23 percent. Though the interaction term is negative, it is not statistically significant. This is possibly due to the fact that the sub-district*year controls are also controlling for some of the useful variation in home prices caused by the policy.

To further control for any possible biases in my capitalization estimates due to unobserved neighborhood effects, I test my model on the subset of homes that were sold multiple times during the observation period, enabling me to control for unit-level fixed effects. The results of the repeat-sales analysis are presented in Table 5.4. Columns (1) through (4) replicate the specifications used above in table 5.2, so as to ensure that the repeat-sales sample does not systematically differ from the complete sample. The fact that the signs and significance of the coefficients in columns (1) through (4) follow roughly the same pattern as those presented in Table 5.2 and 5.3 suggests that this is the case.

Table 5.4: Hedonic Regressions Using Repeat Sales Sample

Dependent variable	Log of sale price				
Independent variable	(1)	(2)	(3)	(4)	(5)
Bedrooms	**-0.0629*** **	**-0.0188*** **	-0.00186	0.00140	**0.0231*** **
	(0.0079)	(0.0062)	(0.0056)	(0.0057)	(0.0088)
Bathrooms	**0.0968*** **	**0.0726*** **	**0.0641*** **	**0.0618*** **	**0.0981*** **
	(0.0087)	(0.0067)	(0.0060)	(0.0062)	(0.011)
Parking spaces	**0.0382*** **	**0.0523*** **	**0.0442*** **	**0.0461*** **	**0.0316*** **
	(0.0074)	(0.0056)	(0.0050)	(0.0461)	(0.0077)
lnSqft	**0.823*** **	**0.645*** **	**0.565*** **	**0.562*** **	**0.350*** **
	(0.022)	(0.017)	(0.015)	(0.015)	(0.033)
House	-0.0221	**0.188*** **	**0.242*** **	**0.231*** **	0.0174
	(0.014)	(0.012)	(0.012)	(0.013)	(0.053)
lnTestScore	0.0499	**0.234*** **	**0.0937** **	**0.102* **	-0.135
	(0.035)	(0.037)	(0.043)	(0.055)	(0.090)
lnTestInteraction	**0.129** **	-0.0493	0.00873	0.0332	**0.0694** **
	(0.053)	(0.040)	(0.035)	(0.088)	(0.032)
Neighborhood fixed effects	None	District	Sub-district	Sub-district * Year	Unit
Time fixed effects	Month dummies	Month dummies	Month dummies	Month dummies	Month dummies
Observations	3325	3325	3325	3325	3325
R-squared	0.69	0.82	0.87	0.89	0.98

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The estimates presented in column (5) control for fixed-effects at the unit level. When unit-level fixed effects are controlled for, the capitalization estimate is negative and not statistically significant, contrary to my theoretical predictions. The interaction term is positive and weakly significant, also contrasting with theoretical predictions.

The perverse sign of the interaction term might be the result of a small sample size. Though there were 3,325 properties that were sold more than once during the observation period only 548 of these were sold both before and after the 2001 expansion of choice, so the majority of these sales did not contribute to the estimation of the lnTestInteraction coefficient. Also, as discussed above, specifications that employ fixed effects at the unit-level rely completely on within-school test-score variation to generation estimates of capitalization. This can be problematic, especially when dealing with such a short time frame. It is possible that a school's attractiveness to parents is simply not sensitive to short-term test-score fluctuations within schools.

All of the specifications reported above rely on the assumption that the prices for both types of properties in the sample (single-family homes and condominiums) have the same elasticity to the characteristics defined in the model. However, because families with children are more likely to reside in single-family homes, I expect that all house-buyers should be willing to pay more for increases in school quality[10]. As a final test, I eliminate the restriction that the prices of single-family homes and condos respond the same way to variation in the characteristics and allow the coefficients to vary across the two property types by splitting the sample into single-family home sales and condo sales and testing my model on the two datasets separately. Additionally, one could perform a

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[10] Black (1999) performs a similar test in her paper by comparing one and two-bedroom homes sales with the sales of homes with three or more bedrooms and finds school quality to be capitalized more in larger homes.

similar analysis that differentiated properties based on other measures of size such as square footage or the number of bedrooms[11]. For the simplicity purposes I only consider the difference between single-family homes and condos.

The results presented in Tables 5.4 and 5.5 indicate that in San Francisco, house prices do not respond to changes in test scores the same way that condominium prices do.

The results presented Table 5.5 indicate that test scores are capitalized in single-family homes and that school choice had a negative and statistically significant effect on the degree of capitalization. If the magnitudes reported in Table 5.5 are assumed to be correct market valuations of local test scores, this would imply that a one standard-deviation increase in test scores would result in a 0.8 - 3.6 percent increase in home prices, depending on the restrictiveness of neighborhood controls.

The coefficient on the test-score*policy term is negative and significant in columns (2) and (3), though it is larger in magnitude than the capitalization estimate in both specifications. This appears to be the result of either an under-estimate of capitalization due to a lack of cross-school variation or an over-estimate of the policy resulting from exogenous macroeconomic activity. Thus, these magnitudes should not be taken as the true estimates of capitalization as the models tested on these data are still subject to the same weaknesses as discussed above.

Though still subject to bias, the results presented in Tables 5.5 and 5.6 provide valuable insight into the differences between single-family homes and condos in the sample that may have been responsible for the patterns of results presented above in the models that include both condos and homes without letting their coefficients differ. As

[11] Though time constraints prohibited me from performing this analysis, the single-family homes in my sample tended to have more bedrooms than units that were designated as condos. The average number of bedrooms in a single-family home in my sample is 2.8, compared to 1.9 for condos.

expected, columns (2) through (4) of both tables indicate that single-family home prices are more sensitive to test-score variation than are condominium prices. Table 5.5 implies that a one-standard deviation increase in average standardized tests scores would result in a 0.85 - 3.8 percent increase in the price of a single family home, depending on the specificity of neighborhood controls. The corresponding coefficients from Table 5.6 imply that a one-standard deviation increase in test scores to be associated with only a 0.1 - 0.46 percent increase in the price of a condominium, but these values are not significantly different from zero at the 90 percent confidence level.

Table 5.5: Hedonic regressions - Single-Family Homes

Dependent Variable	Log of sale price			
Independent variable	(1)	(2)	(3)	(4)
Bedrooms	**-0.0610*** **	**-0.0202*** **	-0.000779	-0.00176
	(0.0039)	(0.0028)	(0.0023)	(0.0023)
Bathrooms	**0.120*** **	**0.0751*** **	**0.0633*** **	**0.0624*** **
	(0.0043)	(0.0031)	(0.0026)	(0.00256)
Parking spaces	**0.0322*** **	**0.0456*** **	**0.0410*** **	**0.0417*** **
	(0.0034)	(0.0024)	(0.0020)	(0.0020)
lnSqft	**0.691*** **	**0.492*** **	**0.385*** **	**0.385*** **
	(0.010)	(0.0076)	(0.0066)	(0.0065)
lnTestScore	**0.105*** **	**0.391*** **	**0.0874*** **	**0.0865*** **
	(0.017)	(0.018)	(0.020)	(0.026)
lnTestInteraction	**0.0674** **	**-0.130*** **	**-0.139*** **	0.0299
	(0.027)	(0.020)	(0.017)	(0.041)
Neighborhood fixed effects	None	District	Sub-district	Sub-district * Year
Time fixed effects	Month dummies	Month dummies	Month dummies	Month dummies
Observations	12754	12754	12754	12754
R-squared	0.67	0.84	0.89	0.90

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5.6: Hedonic regressions – Condominiums

Dependent Variable	Log of sale price			
Independent variable				
	(1)	(2)	(3)	(4)
Bedrooms	**-0.0107****	**0.0139*****	**0.0114*****	**0.0117*****
	(0.0052)	(0.0047)	(0.0044)	(0.0044)
Bathrooms	**0.0367*****	**0.0650*****	**0.0801*****	**0.0803*****
	(0.0062)	(0.0055)	(0.0051)	(0.0051)
Parking spaces	**0.0482*****	**0.0723*****	**0.0709*****	**0.0700*****
	(0.0071)	(0.0061)	(0.0056)	(0.0056)
lnSqft	**0.794*****	**0.688*****	**0.662*****	**0.663*****
	(0.014)	(0.012)	(0.011)	(0.011)
lnTestScore	**0.186*****	0.0107	0.0469	**0.133*****
	(0.030)	(0.032)	(0.035)	(0.049)
lnTestInteraction	0.0373	**0.0549***	**0.0528***	**-0.131****
	(0.039)	(0.033)	(0.030)	(0.064)
Neighborhood fixed effects	None	District	Sub-district	Sub-district * Year
Time fixed effects	Month dummies	Month dummies	Month dummies	Month dummies
Observations	5929	5929	5929	5929
R-squared	0.64	0.74	0.79	0.80

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Comparing the results presented in Tables 5.5 and 5.6, it appears that aside from having different sensitivities to test scores, prices for the two types of properties also respond differently to marginal changes in physical property characteristics. The estimated elasticity of condo prices to square footage, for instance, is almost twice that of the estimated elasticity for single-family homes. Notice also that while the number of bedrooms become negative and insignificant determinants on house prices once sub-district fixed effects are controlled for, it remains positive and significant across all four specifications for condominiums.

Though the magnitudes of these estimates are subject to the same weaknesses as the results presented earlier, the signs and statistical significance of the coefficients provide useful insight into the capitalization effects of school choice. The difference in capitalization patterns across home types suggests that models which assume the two types of properties to have the same pricing structure were too restrictive. This provides an alternative explanation for the lack of significant policy effects in Tables 5.2 – 5.4.

As mentioned in the description of the school data used in the capitalization estimates, McAteer High School was closed in 2002 due to low performance. For the purposes of studying the changes in capitalization caused by new assignment policies, test-score averages for homes within McAteer's attendance boundary after 2002 are calculated using only the test scores from their respective elementary and middle schools. To test whether the missing values had any effect on the results, I reran the model after substituting McAteer's 2001-02 test scores for the two years after the school closed. Incorporating McAteer's 2001-02 test scores into the average test score calculations for these homes had no significant effect on the coefficients so those results are not reported in this thesis.

I find strong evidence that test scores are capitalized in the city of San Francisco, and this finding is robust across all models tested. I also find some evidence that the prices of single-family homes appear to be more sensitive than condominium prices to changes in average test scores. Consistent with Black's (1999) findings, I also find evidence that capitalization effects are overestimated when neighborhood effects are not adequately controlled for. I find mixed evidence on the effects of school choice on capitalization, but results indicate that it had a negative effect on test-score capitalization in single-family homes.

Chapter 6: Conclusions and Discussion

6.1 Effects of School Choice on Ethnic Diversity

My results show that in the schools that are more likely to be in high demand—those with high test scores and low levels of students receiving free lunch—diversity is increasing as a response to the 2001 policy change despite a decrease in diversity for the city overall. More specifically, school choice as implemented in the SFUSD increases diversity at schools where less than 70 percent of students receive free or reduced-price lunch (about fifty percent of schools) and school choice decreases diversity in schools where more than 70 percent of the student population receives free or reduced-price lunches. These results indicate that while the SFUSD indirect desegregation policy is increasing diversity at high-achieving schools, it is less successful in preventing less popular schools from becoming re-segregated by choice. The validity of the latter claim is ambiguous, however, in the absence of a larger control group, as I was unable to completely isolate the effects of the two policies.

One major weakness with my identification strategy in the diversity regressions is that I am capturing the effects of two near-simultaneous policy changes: the expansion of school choice (expected to decrease grade-level diversity) and the utilization of the diversity index as a selection mechanism (expected to increase grade-level diversity). It is unclear what the pattern of "re-segregation" would have been in the absence of the diversity index. It is possible that a more progressive selection mechanism, such as a diversity index that takes race or neighborhood into account, might prevent the re-segregation of less popular schools more effectively. It is also important to note that the observation period in this part of the analysis only spans 2000-2003, preventing me from drawing conclusions about the long-term effects of having total school choice on diversity. As the results of my study are only indicative of short-term effects, it would be useful to extend this research to study diversity patterns over the long term in response to increases in school choice.

6.2 Effects on School Quality Capitalization

This paper provides strong evidence that neighborhood test scores are capitalized in the price of San Francisco homes. This result is interesting in and of itself considering that families were allowed quite a bit of school choice prior to 2001. This result also underscores the importance of public school quality for all home owners, not just those who have children and plan on sending them to public schools.

I find mixed evidence, however, on whether these capitalization effects changed in response to the 2001 expansion of school choice, which effectively broke the link between neighborhood and school assignment. When my specifications are estimated on the entire sample, I find no statistically significant effects of choice on the extent of capitalization. It is only when I allow house and condo characteristics to vary independently that I find the policy to have had a statistically significant impact. Even then the results are dubious as they imply that the policy not only decreased capitalization but reversed it.

Consistent with Black's (1999) findings I find evidence that capitalization estimates are biased when neighborhood effects are not adequately controlled for. However, because I use pre-defined district and sub-districts to control for neighborhood effects rather than only looking at homes on either side of attendance boundaries my estimates rely increasingly on within-school rather than across-school variations in quality. If home prices are more sensitive to across-school variation, my estimates that employ sub-district fixed effects are likely biased downward. As a result I encountered a trade-off between controlling for relevant neighborhood effects and maintaining adequate levels of cross-school variation to generate unbiased capitalization estimates. Future studies on capitalization in urban districts may therefore wish to consider using boundary fixed-effects similar to those used by Black (1999) rather than neighborhood fixed-effects. Using boundary fixed-effects would have allowed for greater flexibility in determining the specificity of neighborhood controls.

Eliminating the link between neighborhood and school-assignment is a transfer of public goods from the residents of high-performing attendance zones to the residents of low-performing zones. If school choice in fact generalizes capitalization effects in a city such that property values reflect the educational quality of the city as a whole rather than the quality of local schools it could have quite significant redistributive implications. It provides all property owners in a school district with a direct monetary incentive to take measures to improve all public schools. In this case it would be in the interest of any school district concerned with educational equality to break the link between neighborhood and school-assignments.

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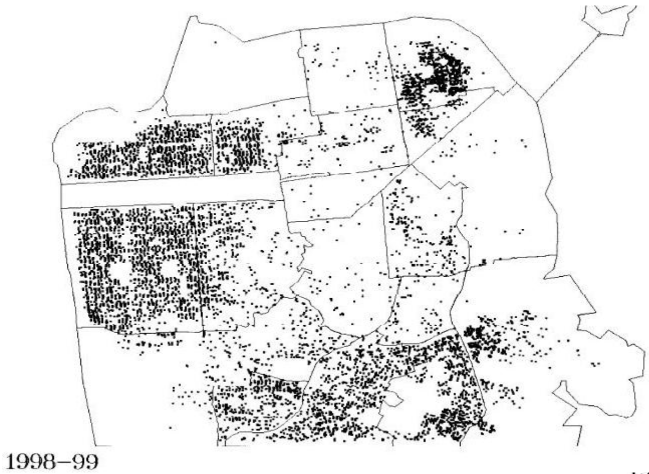
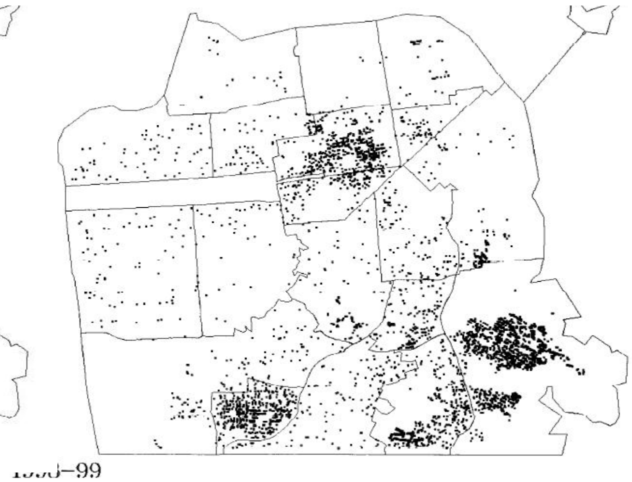
APPENDIX

Figure A.1: Geographic Distributions of SFUSD Students, 1998-99

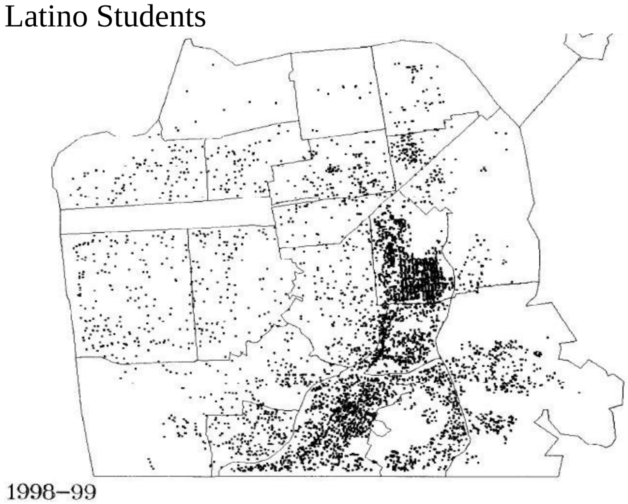
Chinese Students



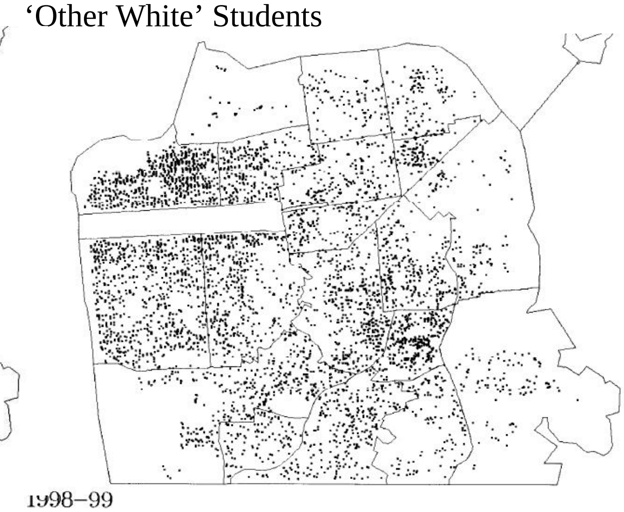
African-American Students



Latino Students

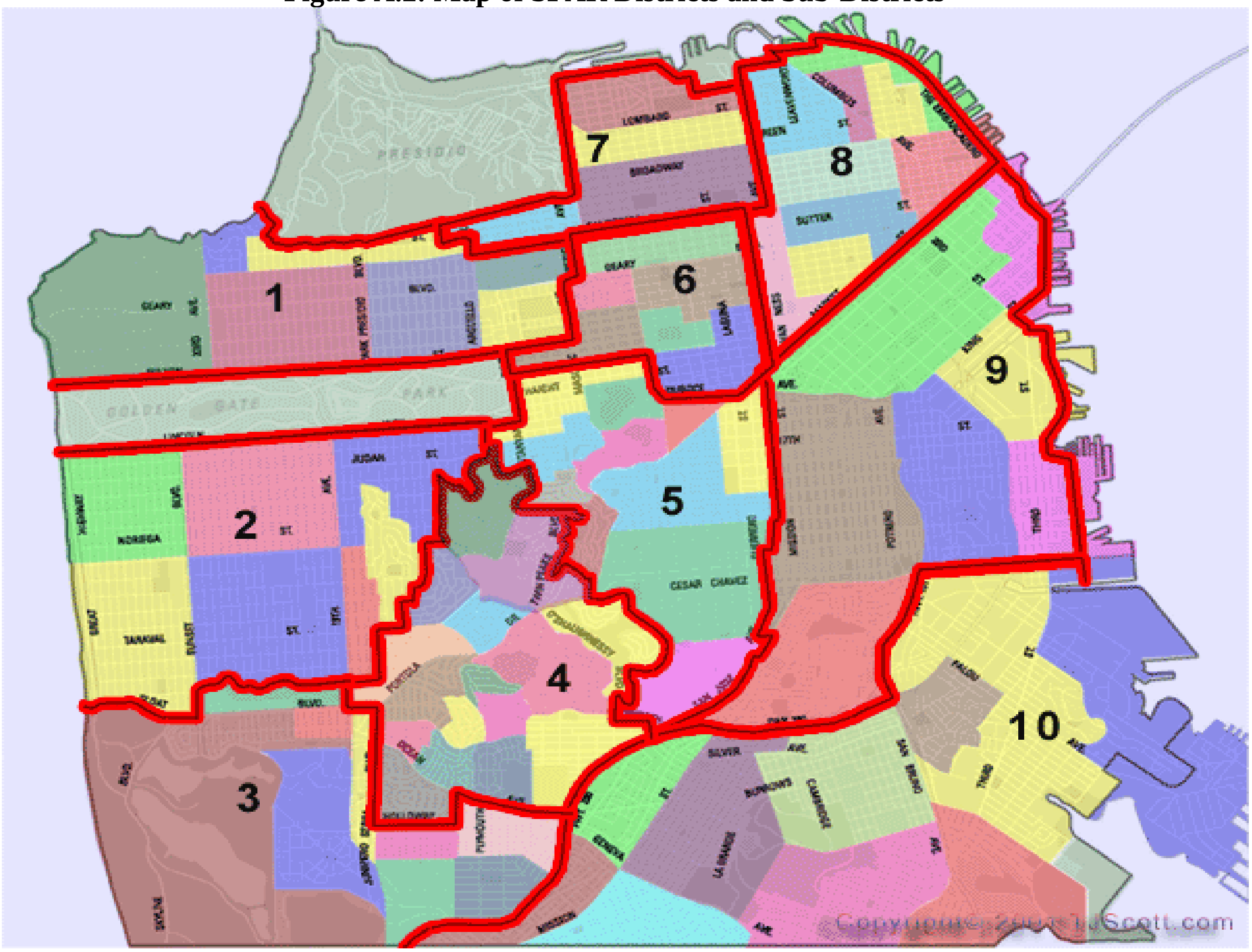


'Other White' Students



Maps obtained from
http://orb.sfusd.edu/pinmap/misc/pinmp98.pdf

Figure A.2: Map of SFAR Districts and Sub-Districts



Map obtained from the TJ Scott Group, http://www.tjscott.com/SF%20Base%20Map.htm

Figure A.3: Northern California OFHEO House Price Indexes, 1975-2007

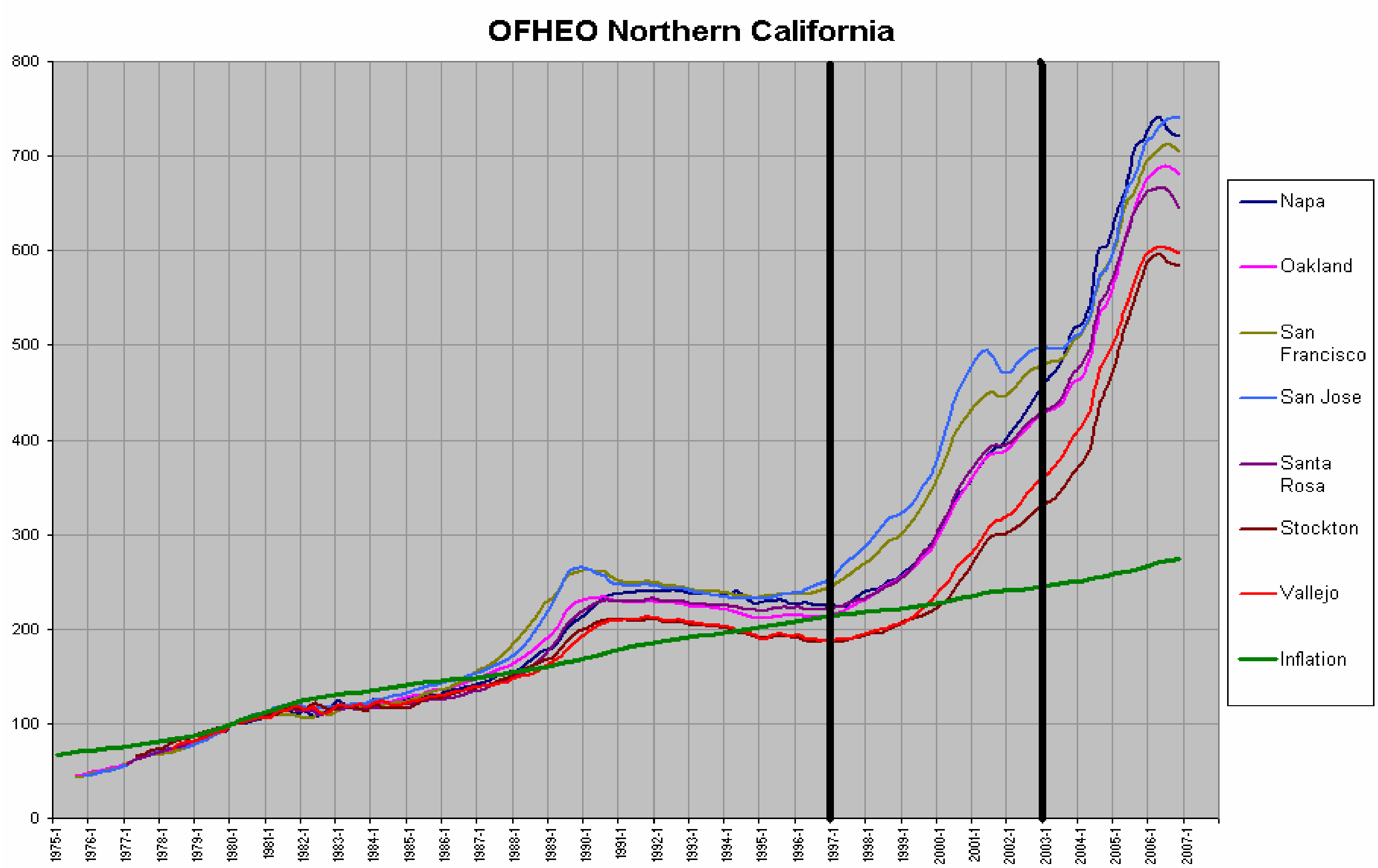


Figure obtained from http://www.housingbubblebust.com/OFHEO/Major/NorCal.html based on OFHEO house price index data. Vertical black lines were added by author to indicate the time period of this study.