# GENTRIFICATION AND FAILING SCHOOLS: THE UNINTENDED CONSEQUENCES OF SCHOOL CHOICE UNDER NCLB

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*Abstract*—We examine the housing market and residential mobility changes that occur soon after a Title 1 school fails to achieve adequate yearly progress (AYP) in Charlotte, North Carolina. Students within attendance zones of failing schools are given priority in lotteries for oversubscribed schools, potentially increasing the attractiveness of living in a failing school attendance zone. We find that housing prices, home buyer income, and the probability of attending a nonassigned school increase in the highest-quality neighborhoods within failing school attendance zones. Our results are driven largely by the behavior of new residents.

## I. Introduction

THE 2002 No Child Left Behind (NCLB) Act represents one of the most far-reaching federally mandated educational reforms in history. NCLB required states to administer standardized tests to students in all schools and identify schools that fail to meet state established standards overall or in any specified subgroup. Schools that fail to meet standards are monitored in order to establish whether they achieve Adequate Yearly Progress (AYP) toward the state standards. For schools that receive Title 1 funds, a significant sanction associated with failure to achieve AYP for two consecutive years is that students attending these low-performing schools must be provided the opportunity to attend a nonfailing school. In districts with extensive school choice opportunities, the school choice sanction is often implemented by providing students with improved odds in lotteries for spots at oversubscribed schools.

A growing literature finds that state and federal accountability policies such as NCLB may have positive effects on student achievement (Carnoy & Loeb, 2002; Hanushek & Raymond, 2005; Jacob, 2005; Figlio & Rouse, 2006; West & Peterson, 2006; Reback, 2008; Reback, Rockoff, & Schwartz, 2014; Rockoff & Turner, 2010; Dee & Jacob, 2011; Chakrabarti, 2013, 2014; Ahn & Vigdor, 2014). At the same time, a parallel literature documents the many unintended consequences of school accountability policies on the behavior of school administrators and teachers. For example, Cullen & Reback (2006), Jacob (2005), Figlio (2006), and Figlio and Getzler (2006) find that schools attempt to strategically manipulate the composition of the test-taking pool, while Reback (2008) and Neal and Schanzenhach (2010) find that accountability standards induce teachers to focus on students near the current proficiency standard.[1]

What has largely been overlooked in the literature, however, is how accountability mandates may affect the residential location decisions of families. Specifically, an unintended consequence of NCLB and other large-scale accountability programs with significant school choice provisions is that they may create an incentive for households with strong preferences for school choice or school quality to move into the attendance zones of failing schools in order to improve their likelihood of being admitted into high-performing schools.

Ferreyra (2007) and Nechyba (2000) provide theoretical evidence consistent with that notion. Using structural and computable general equilibrium models, they demonstrate that the introduction of private school vouchers, targeted to low-performing school districts, induces relatively high-income households to move into low-performing districts in order to take advantage of lower housing values and the ability to use school vouchers. These higher-income households purchase homes in the "nicest" neighborhoods within low-performing districts, driving up property values and inducing neighborhood gentrification.

This paper provides the first direct empirical evidence on how NCLB school choice provisions affect housing markets and the residential location decisions of families. Following Ferreyra (2007) and Nechyba (2000), we hypothesize that households with strong tastes for school quality may strategically move into the best neighborhoods in attendance zones of Title 1 schools that fail to meet AYP standards for two consecutive years in order to improve their likelihood of being admitted into high-performing, oversubscribed schools. To test that hypothesis, we use data from 2003 to 2011 in the Charlotte-Mecklenburg county school district in North Carolina to examine how such failures affect housing prices, the income of individuals buying homes, and the school choice decisions of students.

To identify the effect of Title 1 AYP failure, we focus on neighborhoods (Census block groups) that are bisected by recently redrawn (2002–2003) school attendance zone boundaries. We conduct difference-in-differences analyses by comparing the outcome changes (housing prices, home buyer income, and student choices) that occur on the side of an attendance zone boundary where a failure occurs, to the outcome changes that occur on the other side of the attendance zone boundary where a second failure did not take place. Our model is therefore identified by comparing deviations from neighborhood trends on either side of a school attendance zone boundary after one school fails to meet AYP standards. We examine these changes separately by pre-NCLB neighborhood housing value terciles in order to test whether the effect of AYP failure differs between the highest- and lowest-priced neighborhoods.

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[1] Also see Jacob (2005) and Reback et al. (2014) on subject matter focus and Jacob and Levitt (2003) on teacher cheating.

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In our student-level analyses, we begin by assigning students to their 2002–2003 residential locations in order to mitigate concerns over the impact of nonrandom sorting across attendance zone boundaries. However, this restriction impedes our ability to examine an outcome of primary interest: the impact of AYP failure on residential mobility and the school choice decisions of families. Thus, we also estimate specifications that use contemporaneous residential locations and include student fixed effects to mitigate concerns over the potential correlation between student unobervables and residence in attendance zones of Title 1 AYP failing schools.

In specifications where we assign every student to his or her original 2002–2003 residential location, we find that housing prices, home buyer income, and the probability of attending a nonassigned magnet school rise in the highest-quality neighborhoods in failing school attendance zones in comparison to locations on the other side of the attendance zone boundary. Further analyses, based on specifications that utilize contemporaneous residential locations, reveal that new residents who move into the highest-quality neighborhoods of failing schools are significantly more likely to attend a nonassigned school, an effect that is absent for original residents.

Our work makes several important contributions to the literature. First, as noted previously, our paper is the first to provide direct empirical evidence on how NCLB choice provisions affect housing values and residential location decisions. More generally, understanding how school choice policies affect residential location decisions and therefore neighborhood composition has become increasingly important as states and cities across the country continue to experiment with school choice (Jordan & Gallagher, 2015).[2] Yet despite that fact, there is almost no empirical evidence on how households respond to increased school choice.[3] While several studies provide important theoretical insights into how school choice policies are likely to affect housing values and residential location decisions (Nechyba, 2000, 2003; Ferreyra, 2007; Epple & Romano, 2003), those studies must make strong and empirically untested assumptions about household preferences.

Second, our work contributes to a growing literature on how households respond to school choice programs. Hastings, Kane, and Staiger (2009) find that low-income families place less weight on academics when selecting schools and exert less pressure on low-performing schools to improve performance, while Jacob and Lefgren (2007) find that low-income and minority parents are less likely to actively select a teacher. Consistent with these studies, our results suggest that higher-income families are more likely to strategically move into the attendance zones of failing schools in order to gain access to better school choice options.[4]

## II. Institutional Details

Prior to the beginning of the 2002–2003 academic year, Charlotte-Mecklenburg Public School District (CMS) operated under a court-ordered desegregation plan that used busing to achieve racial integration in schools. In 1997, a CMS parent whose child was denied entrance to a magnet school program based on race filed a lawsuit against the district (*Capacchione* v. *Charlotte-Mecklenburg Schools*). This case escalated into a larger challenge of Charlotte's race-based busing policy and led to the end of court-ordered busing in summer 2002.

In order to adapt to the court order to end race-based busing, CMS dramatically redrew school attendance boundaries. Starting with the 2002–2003 academic year, school attendance boundaries were based on school capacity and the geographical concentration of students around a school. Students were assigned to a neighborhood school by default, but the school system provided a number of magnet schools and allowed enrollment at any school, with a lottery determining enrollment at oversubscribed schools. The end of court-based busing led to approximately 50% of students being reassigned to a new school over summer 2002.

Layered on top of the end of court-ordered busing and the establishment of new neighborhood school attendance zones during summer 2002 was the enactment of the NCLB Act in January 2002. As part of NCLB, a school is subject to sanctions if it fails to meet AYP standards for two consecutive years and it is classified as a Title 1 school (a school where 75% or more of the students qualify for federal lunch subsidies). We refer to these schools throughout the paper simply as AYP failing schools. As part of meeting its obligations to students in AYP failing schools, students within the attendance zones of these schools are given priority in lotteries to attend schools that are not AYP failing (both magnet schools and traditional nonassigned schools).[5] The first year of high-stakes NCLB testing was 2002–2003, but the AYP standard in that initial year was set lower in order to ease the transition into the new testing regime. The first year of high AYP standards was 2003–4 and just under half

[2] For example, students attending NCLB failing schools in Georgia are given priority in the statewide intradistrict choice program, and in Florida and Oregon, they are given priority in charter school lottery admissions. Other examples of cities that provide students at NCLB failing schools with priority at gaining access to schools of choice are Albuquerque, New Mexico; Milwaukee, Wisconsin; and Houston to name a few.

[3] To our knowledge, Brunner et al. (2012) provide the only empirical evidence on the impact of expanded school choice on residential location decisions by showing that the introduction of interdistrict choice programs is associated with higher housing prices and increasing incomes in districts with nearby, attractive, out-of-district schooling options.

[4] Our results also complement Cullen, Long, and Reback (2013), who find that households strategically move to neighborhoods located in lower-performing school attendance zones in order to improve their odds of qualifying for the Texas "Top Ten Percent Plan." Similarly, Cortes and Friedson (2014) find that the Texas Top Ten Percent Plan increased housing values within the attendance zones of the lowest-performing high schools.

[5] Students who do not gain admission to a school of their choice through the lottery process are guaranteed admission to another non–Title I Choice School. Students enrolled in a nonassigned school can remain in that school through the last grade offered by the school, even if their assigned school passes AYP at a later date.

of our twice-failing Title 1 schools are classified as failing to make AYP in 2004–5.[6]

The Charlotte-Mecklenberg County school district provides several major advantages for studying the effects of the 2002 NCLB Act. First, the district maintains a high-quality longitudinal student database that contains information on each student's residential location as well as school attended. Second, the district is a county-wide school district containing a major southern city that encompasses both very poor urban neighborhoods and relatively affluent suburban and urban neighborhoods, similar to other large school districts in many southern states. Finally, the 2002–3 redistricting of attendance zones in CMS following the end of court-ordered desegregation created a relatively exogenous distribution of individuals and housing stock across attendance zones.[7]

Two concerns exist with exploiting the variation across these attendance zones. First, since families may sort in response to a failing designation, it is problematic to use contemporaneous addresses to determine a student's school assignment. To address this issue, in our primary specification we assign every parcel and student to his or her 2002–03 school attendance zone. The 2002–3 school year represents the first year after the school attendance boundaries were redrawn in response to a court order to cease busing for racial integration. It also represents the first school year after CMS allowed for district-wide school choice following redistricting and thus allows very little time for students to sort into new neighborhoods.

Second, although school assignment boundaries were relatively stable after 2002–3, approximately 12% of parcels were reassigned to at least one new school between 2004 and 2011. Most of this reassignment was due to the introduction of new schools, but one may be concerned that failing schools may be subject to a larger number of boundary changes due to the loss of students. Since most boundary changes are related to school capacity issues, the redrawing of school attendance zones may be related to failing designation. We therefore fix parcels and students who live in those housing units to their assigned school attendance zones based on the zone definitions just after those reorganizations but prior to the implementation of NCLB choice sanctions beginning in 2004–5.[8]

## III. Methodology

In order to implement our difference-in-differences analysis, we estimate a model that controls for both school assignment based on attendance zone and neighborhood-by-year fixed effects. The neighborhood-by-year fixed effects imply that any effect of AYP failure is identified by neighborhoods that are bisected by attendance zone boundaries, and the school assignment fixed effects allow for initial across-attendance zone differences in housing prices, income, and student choices.[9] We then test whether the estimated relationship between changes in student or housing market outcomes and failure to achieve annual progress varies by neighborhood price tercile. While the schools that fail AYP are very different from nearby schools that pass AYP, the maintained assumption is that the population within a block group on a new boundary between two school attendance zones is randomly distributed prior to that boundary being drawn because households had no information in advance in terms of where that boundary would lie.[10]

The resulting empirical model for our key housing transaction and student outcomes is

$$y_{ijst} = \gamma_1 F_{ts} \times Z_{1j} + \gamma_2 F_{ts} \times Z_{2j} + \gamma_3 F_{ts} \times Z_{3j} + \beta X_{st-1} + \delta_{jt} + \theta_s + \varepsilon_{ijst}, \quad (1)$$

where $y_{ijst}$, represents an outcome of interest for observation $i$ (housing unit or student) in neighborhood $j$, school assignment $s$, and year $t$; $F_{ts}$, is an indicator variable for whether one of the schools (elementary or middle) to which the housing unit or student was assigned in 2002–3 (base year) failed to achieve AYP in both years $t_{-1}$, and $t_{-2}$,;[11] $Z_{1j}$, $Z_{2j}$ and $Z_{3j}$, are indicator variables that take the value of unity if a neighborhood is in the first, second, or third housing price tercile, respectively; $X_{st-1}$ is a vector of lagged school test score outcomes based on assignment to school $s$ in 2002–3;[12] $\delta_{jt}$ is a vector of block group-by-year fixed effects allowing for nonparametric trends in neighborhood circumstances over time; $\theta_s$ is a vector of fixed effects

[6] See appendix A for more information on the distribution of failing schools.

[7] See Billings, Deming, and Rockoff (2014) for a discussion of the exogeneity of redistricting boundaries at this time and for details on the effects of the court-ordered end to desegregation policies in CMS on a variety of student outcomes.

[8] This approach minimizes potential sorting bias but does increase measurement error since some students and parcels were reassigned to a new school after 2003 and other students changed residence and moved to an alternative school assignment zone.

[9] This difference-in-differences strategy is very similar to the strategy used in Dhar and Ross (2012) except that in our case, the across-time variation is driven by a specific event, the second failure under NCLB. Further, most of the boundaries in Dhar and Ross (2012) had been stable for several years, while CMS boundaries were redistricted just prior to our sample period, leaving little time for systematic residential sorting across boundaries prior the implementation of NCLB. Consistent with that notion, Billings et al. (2014) find no evidence of residential relocation in 2001–2 or 2002–3 in response to redistricting.

[10] Figure 1A in the appendix provides an example of the variation we exploit in our identification strategy. Also, see Bayer, Ross, and Topa (2008) for evidence that residential location within block groups is relatively random even though households show strong evidence of sorting into block groups.

[11] The vast majority of schools that fail for a second time continue to fail AYP in the following years of our sample. Our analyses are robust to dropping schools from the sample that fail a second time and then subsequently pass at a later date.

[12] These controls include the lagged test scores of the assigned elementary, middle, and high school. While a formal regression discontinuity is not possible because we do not observe a single continuous variable that identifies failure, annual yearly progress is based on school test scores so these controls can be viewed informally as approximating a running variable.

associated with the geographically assigned school in 2002–3; and $\varepsilon_{ijst}$ is a random disturbance term.[13]

The coefficients of primary interest in equation (1) are the $\gamma$'s, the coefficients on the interactions between the indicator for whether a school fails to meet AYP standards and the neighborhood quality tercile indicators. Specifically, $\gamma_1$, $\gamma_2$, and $\gamma_3$ are the difference-in-differences (DD) estimates of the effect of treatment (failure to meet AYP standards) on our outcomes of interest for the first, second, and third terciles of neighborhoods quality, respectively. Furthermore, based on the literature, we expect the response to the school choice opportunities brought about by AYP failure to be strongest among higher-resource households, which typically reside in higher-quality neighborhoods. Consequently, we focus primarily on $\gamma_3$, the difference-in-differences estimate for the highest-quality neighborhoods. We also report the $p$-value associated with the $F$-test of the null hypothesis that $\gamma_3 - \gamma_1 = 0$.

We also conduct a balancing test to provide further evidence that the estimates from equation (1) have a causal interpretation. Using the cross-sectional variation in the sample, we regress an indicator variable for whether a school ever had a second consecutive failure during our sample period (i.e., the treatment) on predetermined student and housing attributes $W_{ijs}$:

$$F^E_{ijs} = \lambda W_{ijs} + \delta_j + \tau S_s + \varepsilon_{ijs}, \tag{2}$$

where $F^E_{ijs}$ equals 1 if $F_{ts} = 1$ for any $t \geq t_0$ and 0 otherwise, and $\delta_j$ is a set of block group fixed effects. Our balancing test is designed to examine whether predetermined student and housing attributes appear to "cause" the treatment conditional on the controls that should render the treatment exogenous (i.e., block group fixed effects). Since we are testing if 2003 attributes predict whether a school ever fails, we are limited to only cross-sectional variation and thus include a vector of observable pre-NCLB school characteristics, $S_s$, in equation (2) rather than the school fixed effects in equation (1).

We are also directly interested in whether failure to meet AYP standards causes some families to move into the neighborhoods of failing schools in order to take advantage of the school choice preferences. Thus, we examine the outcomes for all students residing in a neighborhood after a failure (movers and stayers) using the student's current neighborhood $k$ and current school attendance zone $n$ at time $t$ and controlling for student fixed effects. Specifically,

$$y_{iknt} = \gamma_1 F_{tn} \times Z_{1k} + \gamma_2 F_{tn} \times Z_{2k} + \gamma_3 F_{tn} \times Z_{3k} + \beta X_{nt-1} + \delta_{kt} + \theta_n + \pi_i + \varepsilon_{iknt}, \tag{3}$$

where student fixed effects $\pi_i$ ensure that the model is identified by observing changes in outcomes for students who currently live in an attendance zone of an AYP failing school.[14]

Finally, the model specification in equation (3) can be used to examine how AYP failure affects residential location choice. Specifically, the dependent variable in the resulting model is whether the student moved into the block group during our sample period, as opposed to belonging to the original resident sample. The resulting model tests whether new residents are more or less likely to be on the failing school side of the block group after the AYP failure.

## IV. Data

We combine a number of administrative databases in order to track students as well as property values and home buyer income for our study area of Charlotte. All of our data sets are assigned to individual addresses, thus allowing us to define neighborhoods and outcomes based on small spatial units as well as school attendance boundaries. We obtained parcel-level data on the structural characteristics of properties as well as complete records of any sales transaction for all parcels located in Mecklenburg County from 1994 through 2010 from the Mecklenburg County assessor's office. We limit our analysis of property valuation to single-family homes and include 106,736 transacted sales of single-family homes between 2003 and 2010.

In order to examine neighborhood income and price trends, we acquired the population of mortgage deeds of trust in Mecklenburg County from 2004 to 2010. The mortgage deeds data provide parcel-level information on every home buyer who acquired a mortgage in the purchase of a home, including the home buyer's name, the mortgage amounts (including the loan amount), the name of mortgage lender, and the exact address of each parcel. These mortgage deeds are then subsequently linked to Home Mortgage Disclosure Act (HMDA) data in order to assign individual mortgages to a home buyer's mortgage application stated income.[15]

To examine residential mobility and school choice trends, we use administrative records from CMS for all individual students who attended public school for any school year between 2002–3 and 2010–11 and enrolled in

[13] Models of test score capitalization tend to find quite modest price effects of 2% to 4% for a 1 standard deviation increase in test scores (Black, 1999; Fack & Grenet, 2010; Dhar & Ross, 2012). However, such capitalization effects are likely smaller than effects associated with the phenomena studied in this paper for several reasons. First, the estimated capitalization effects are partial equilibrium controlling for neighborhood quality while our estimates allow households to value the higher neighborhood incomes arising from enhanced choice options. Second, our estimated effects arise from all attributes over which families might base their school choice decisions, not just standardized test scores. Finally, households may place intrinsic value on the increased choice option possibly because it may increase flexibility to respond to unforeseen events.

[14] In these specifications, a student's assigned school is not held constant and may change due to residential mobility or redistricting. The resulting analysis retains the DD structure, but now the differences across boundaries do not have the exogeneity provided by using the residential locations immediately following the post-busing redistricting.

[15] See appendix A for details on how we merge the HMDA data with the mortgage deeds data.

Table 1.—Summary Statistics

| | All | Tercile 1 | Tercile 2 | Tercile 3 | Higher-Income Block Groups |
|---|---|---|---|---|---|
| *Average sales price (1998–2002)* | | \$31,466–\$93,877 | \$93,930–\$149,790 | \$150,235–\$389,217 | \$391,362–\$813,331 |
| Sales price | 149,907 | 63,993 | 116,188 | 216,302 | 546,698 |
| | (112,215) | (18,518) | (15,244) | (52,372) | (117,891) |
| Single family parcels | 683.9 | 386.3 | 807.4 | 921.0 | 404.3 |
| | (720.0) | (313.7) | (699.9) | (933.2) | (267.0) |
| Multifamily parcels | 76.78 | 31.93 | 73.63 | 121.5 | 88.75 |
| | (164.2) | (59.37) | (128.5) | (243.1) | (109.6) |
| Annual property sales | 41.06 | 17.33 | 48.24 | 61.21 | 26.0 |
| | (57.70) | (15.85) | (54.84) | (78.55) | (27.43) |
| Mortgage income | 91.73 | 49.06 | 62.12 | 135.0 | 308.6 |
| | (73.86) | (19.22) | (22.56) | (53.63) | (135.0) |
| Total students per year (K–8) | 173.4 | 129.8 | 217.6 | 185.9 | 79.92 |
| | (175.4) | (91.63) | (184.6) | (220.9) | (57.13) |
| Attend nonassigned schools (%) | 0.335 | 0.402 | 0.323 | 0.285 | 0.295 |
| | (0.151) | (0.0927) | (0.139) | (0.180) | (0.178) |
| *Magnet (%)* | 0.0834 | 0.0810 | 0.0820 | 0.0913 | 0.0651 |
| | (0.0638) | (0.0345) | (0.0685) | (0.0825) | (0.0309) |
| *Nonmagnet (%)* | 0.231 | 0.293 | 0.220 | 0.181 | 0.222 |
| | (0.106) | (0.0759) | (0.0836) | (0.112) | (0.162) |
| Failing (%) | 0.291 | 0.621 | 0.223 | 0.060 | 0.0 |
| | (0.365) | (0.319) | (0.314) | (0.197) | (0.0) |
| Average elementary school test scores | -0.0894 | -0.475 | -0.147 | 0.293 | 0.457 |
| | (0.446) | (0.245) | (0.309) | (0.357) | (0.160) |
| Average middle school test scores | −0.129 | −0.509 | −0.178 | 0.241 | 0.430 |
| | (0.456) | (0.254) | (0.328) | (0.388) | (0.197) |
| Average high school test scores | −0.242 | −0.473 | −0.273 | −0.0270 | 0.0710 |
| | (0.284) | (0.204) | (0.229) | (0.216) | (0.106) |
| Number of failing schools | 32 | 32 | 16 | 11 | 0 |
| Number of schools | 97 | 70 | 74 | 69 | 14 |
| CBGs w/ failing school | 186 | 107 | 56 | 19 | 0 |
| CBGs | 367 | 118 | 117 | 117 | 15 |

Summary statistics based on aggregating housing and student outcomes to 2000 Census block group definitions using data from the 2004–2011 school years. Cells indicate means for each variable in row headings with standard deviation in parentheses. Number of schools indicates the total number of unique middle plus elementary schools assigned to CBG neighborhoods defined by each column. Number of failing schools based on a school ever failing between 2004 and 2011.

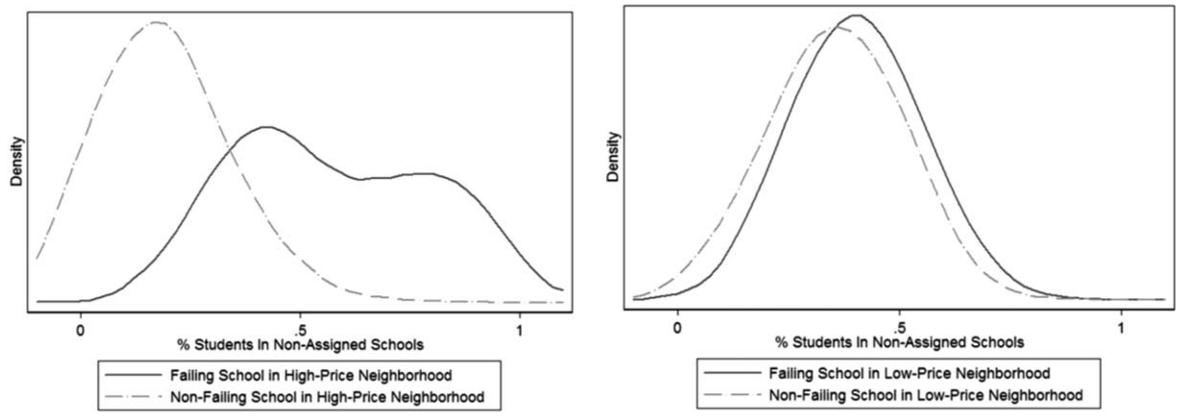
grade 8 or lower. This unbalanced panel allows us to characterize initial entry into the school system as well as transfers among schools within CMS. Student data include information on gender, race, and yearly end-of-grade (EOG) test scores for grades 3 through 8 in math and reading. All EOG tests were standardized and administered across the state of North Carolina, and corresponding test scores are normalized to mean 0 with standard deviation of 1 for the entire state. We also create variables for whether the student attends a nonmagnet school to which he or she is not assigned or attends a magnet school. The student-level administrative records also include the exact address of residence in every year for every student in CMS. Of our initial sample, 2% have missing or invalid address information, which leaves us with 88,984 unique students in CMS during the 2002–3 school year. Student-level geographical information allows us to determine a student's assigned school for each year and match each student to a unique neighborhood.

We focus on the 2002–3 through 2010–11 school years given the introduction of NCLB in 2002 and associated designation of failing schools beginning in the 2003–4 school year. We define AYP failing schools as Title 1 schools that failed to meet AYP for two consecutive years. To meet AYP standards, a school must satisfy statewide proficiency goals in math and reading for ten subgroups of students. If a school misses the proficiency goal for just one subgroup, it does not make AYP. Of the 116 elementary and 48 middle schools in operation between 2003 and 2011, 24 Title 1 elementary schools and 9 Title 1 middle schools received a failing designation over this time period and no high schools were designated as failing.

Neighborhoods (block groups) are organized into terciles using pre-NCLB average housing sales prices as an indicator of the overall quality of the neighborhood and housing stock. The highest housing value block group that contains at least part of an attendance zone for a school that experienced two consecutive AYP failures had an average sales price during the preperiod of \$389,217. All 352 block groups with average transaction prices lower than this amount are ordered by average price and divided into terciles. The remaining 15 block groups with higher average price are placed in a higher category and do not influence the estimates on the effect of AYP failure. Note that the housing price terciles are absolute in nature based on a block group's position in the entire sample.

Table 1 provides descriptive statistics across our sample of 367 block groups or neighborhoods. The first column presents means and standard deviations based on the entire sample of block groups. Columns 2, 3, and 4 present summary statistics based on terciles of mean block group housing prices where mean housing prices are based on the

FIGURE 1.—DISTRIBUTION OF STUDENTS TAKING ADVANTAGE OF SCHOOL CHOICE OPPORTUNITIES




Figures provide the distribution of Census block groups by school attendance boundary neighborhoods based on the portion of students attending a school that differs from their residential-based school assignment. We do this analysis separately for failing and nonfailing schools over the period 2004 to 2011. The housing price terciles used in these figures are the same as those shown in the top row of table 1.

pre-NCLB transaction sale price of homes between 1998 and 2002. These housing prices are adjusted to an average price level between 1998 and 2000 using a simple hedonic-based price index estimated from the same transaction data. The house price terciles were constructed using all the block groups where the mean housing price is at or below the highest mean housing price observed for the subsample of block groups that contain an attendance zone boundary associated with a failing school sometime during our sample period. The top row of table 1 gives the minimum and maximum average housing price for each tercile. Finally, column 5 presents summary statistics for the subsample of the highest housing price block groups (those with average housing prices of \$390,000 or more) that do not contain an attendance zone associated with a failing school. Most school choice is associated with nonmagnet schools, and while selection into magnet schools is positively associated with neighborhood income, selection into nonmagnet schools actually decreases with neighborhood income. As expected, sales price, mortgage stated income, and assigned school test scores increase with neighborhood quality.

While table 1 indicates that overall use of school choice falls with neighborhood quality, figure 1 provides some initial evidence that choice is primarily exercised in higher-quality neighborhoods following an AYP failure. The left panel of figure 1 plots the distribution of the share of students who opt out of assigned schools for each residential high-quality neighborhood, while the right panel plots the same information for low-quality neighborhoods. Consistent with our empirical specifications, we define neighborhoods in terms of census block groups and 2002–3 school attendance zones. Figure 1 provides evidence that a failing designation leads to greater opt-out of assigned schools only in higher-quality neighborhoods. Specifically, the left panel shows clear evidence of a rightward shift in the distribution of students taking advantage of school choice options in higher-quality neighborhoods. In contrast, the right panel shows no evidence of a rightward shift in the distribution of students taking advantage of school choice options in lower-quality neighborhoods.

With that in mind, we now turn to the results from the balancing tests specified in equation (2). Recall that our balancing tests are designed to examine whether predetermined housing and student attributes appear to "cause" the treatment conditional on the controls that should render the treatment exogenous. Thus, to implement our test, we regress an indicator variable for whether a school ever failed to meet AYP standards during our sample time frame on predetermined student and housing attributes. We then test whether any of the predetermined attributes have a statistically significant effect on the probability of failure and whether all the estimated coefficients are jointly equal to 0. The estimates reported in table 2 are linear probability model estimates with standard errors clustered at the census block group.

In column 1 of table 2, which provides estimates that do not control for school attributes, the only coefficient that is statistically significant is the coefficient on reading test scores. Furthermore, as shown in the lower panel of column 1, based on an $F$-test, we fail to reject the null hypothesis that all the estimated coefficients are jointly equal to 0. The use of an $F$-test for the balancing attributes avoids concerns about multiple testing bias because a single test statistic is used to assess balance over all exogenous attributes. The specification presented in column 1 is designed to provide the best evidence on the quasi-randomness of school assignment since it provides a very strong test for the exogeneity of residential location to assigned school as of 2002–03. Thus, the fact that we fail to reject the null hypothesis that all the coefficients are jointly equal to 0 is quite encouraging. In column 2, which includes the school controls, none of the coefficients are statistically significant, and we once again fail to reject the null hypothesis that all the estimated coefficients are jointly equal to 0.

The last three columns present the balancing tests by tercile, and again the resulting $F$-statistics do not suggest any

TABLE 2.—BALANCING TEST

| Variable | All (1) | All (2) | Tercile 1 (3) | Tercile 2 (4) | Tercile 3 (5) |
|---|---|---|---|---|---|
| Male | 0.0010 | 0.0011 | 0.0001 | −0.0002 | 0.0012** |
| | (0.0010) | (0.0009) | (0.0022) | (0.0014) | (0.0006) |
| Black | −0.0070 | −0.0089 | −0.0143 | −0.0037 | 0.0004 |
| | (0.0073) | (0.0066) | (0.0171) | (0.0076) | (0.0026) |
| Hispanic | −0.0023 | −0.0054 | 0.0059 | −0.0114* | −0.0047 |
| | (0.0071) | (0.0059) | (0.0149) | (0.0064) | (0.0030) |
| Reading Test Score | −0.0031** | −0.0017 | −0.0040 | −0.0003 | 0.0007 |
| | (0.0016) | (0.0015) | (0.0036) | (0.0019) | (0.0009) |
| Math Test Score | 0.0010 | 0.0013 | 0.0028 | 0.0003 | −0.0005 |
| | (0.0014) | (0.0013) | (0.0039) | (0.0019) | (0.0005) |
| Student Noncompliance with School Assignment | 0.0020 | 0.0023 | 0.0012 | −0.0004 | 0.0025 |
| | (0.0033) | (0.0030) | (0.0059) | (0.0047) | (0.0018) |
| SF Parcel | −0.0202 | −0.0212 | −0.0565* | −0.0147 | 0.0040 |
| | (0.0158) | (0.0143) | (0.0314) | (0.0183) | (0.0028) |
| Neighborhood Housing Prices (98–02) ($000s) | −0.0003 | 0.0000 | −0.0056 | 0.0000 | 0.0001 |
| | (0.0003) | (0.0003) | (0.0041) | (0.0015) | (0.0001) |
| Change in Housing Prices, 1998 to 2002 ($000s) | 0.0001 | −0.0000 | 0.0030* | −0.0006 | −0.0000 |
| | (0.0003) | (0.0003) | (0.0016) | (0.0009) | (0.0002) |
| CBG fixed effects | X | X | X | X | X |
| School-level test score variables | | X | X | X | X |
| $F$-statistics $p$-value (all individual covariates = 0) | 0.18 | 0.49 | 0.10 | 0.90 | 0.60 |
| Observations | 88,984 | 88,984 | 26,041 | 33,563 | 27,059 |

All covariates based on 2002–3 school year, and dependent variable is a dummy for a neighborhood ever being designated a failing school. All models include grade fixed effects and dummy if missing test scores. We include all K–8 grade students in CMS in 2003. Neighborhood housing prices computed using the average or change in average sales prices for each neighborhood defined as a CBG by school attendance boundary area. Standard errors are clustered at the Census block group level.

TABLE 3.—IMPACT OF FAILING DESIGNATION ON HOUSING MARKET OUTCOMES

| | (1) Log Price | (2) Log Income | (3) Log Income |
|---|---|---|---|
| NBHD T1 × Fail | −0.054 | −0.040 | −0.047 |
| | (0.055) | (0.045) | (0.069) |
| NBHD T2 × Fail | 0.116* | −0.019 | −0.041 |
| | (0.064) | (0.048) | (0.066) |
| NBHD T3 × Fail | 0.084* | 0.202*** | 0.131** |
| | (0.049) | (0.052) | (0.060) |
| $p$-value (T3 × Fail = T1 × Fail) | 0.02 | 0.00 | 0.01 |
| Observations | 157,955 | 52,666 | 37,472 |

All boundaries based on 2002–3 school year. Observations include 2004–2011. All terciles of CBGs based on average CBG housing prices for all transacted sales between 1998 and 2002. Terciles for CBG prices are restricted to CBGs where prices fall within the range of any CBG that contains a failing neighborhood. All models include CBG by year fixed effects, as well as fixed effects for each unique combination of assigned elementary, middle, and high school in 2002–3, quarter by year fixed effects as well as lagged average school test scores for assigned elementary, middle, and high schools. The price model in column 1 also includes 47 indicators for unique structural attributes and measures of proximity to downtown Charlotte and the interstate. Column 2 indicates our main mortgage income model; column 3 removes parcels that may not be owner occupied based on parcel records (mailing versus physical address for ownership records). Standard errors clustered by CBG. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

statistical relationship between exposure to failure and student attributes. We observe one rejection of the null at the 5% level with a $t$-statistic of 2.0 and two rejections at the 10% level, which is about what we would expect based on type 1 error and 27 hypothesis tests. One of the three $F$-tests is just barely significant at the 10% level, but given that we are conducting three $F$-tests, the application of a standard Bonferroni correction implies a significance level of 0.30 outside any reasonable standard for a balancing test.

## V. Results

### A. *Effects of Failure Using 2002–3 Residential Location*

Having provided preliminary evidence that estimates based on our identification strategy have a causal interpretation, we now turn to our key findings regarding the effect of failure to meet AYP on housing market and student outcomes. Results based on the estimation of equation (1) for our key housing transaction outcomes, housing prices, and home buyer income are presented in table 3.[16] The standard errors reported in table 3 and all subsequent tables are clustered at the block group. In the interest of brevity, we report only the estimated coefficients on the interaction terms between the indicator variable for failing schools (i.e., failed to meet AYP for two consecutive years) and the neighborhood-quality tercile indicators. Furthermore, in the second to last row of table 3 and all subsequent tables, we

[16] Note that the number of failures in the highest-price tercile is relatively low, and so our parameter of interest is based on 14 top tercile block groups that are bisected by an attendance zone of a school with an AYP failure during our sample period, as compared to 39 and 62 block groups with a failure for the middle- and lowest-price terciles. Although this has no impact on the validity of our analyses, it may affect generalizability. We therefore present further evidence on the generalizability of our results in appendix C. Also see appendix A for more details on the distribution of failing block groups by pre-NCLB sale prices.

TABLE 4.—IMPACT OF FAILING DESIGNATION ON ATTENDANCE AT NONASSIGNED SCHOOL BASED ON ORIGINAL RESIDENCE IN 2002–2003

| | (1) Attend Nonassigned School | (2) Attend Nonassigned, Nonmagnet School | (3) Attend Magnet School | (4) Attend Nonassigned School | (5) Attend Nonassigned, Nonmagnet School | (6) Attend Magnet School |
|---|---|---|---|---|---|---|
| NBHD T1 × Fail | 0.045** | 0.035* | 0.010 | 0.059** | 0.045* | 0.015 |
| | (0.021) | (0.018) | (0.007) | (0.029) | (0.026) | (0.009) |
| NBHD T2 × Fail | 0.047** | 0.053** | −0.006 | 0.055 | 0.058 | −0.002 |
| | (0.030) | (0.023) | (0.013) | (0.049) | (0.044) | (0.019) |
| NBHD T3 × Fail | 0.150*** | 0.076* | 0.075** | 0.084 | −0.022 | 0.107** |
| | (0.060) | (0.044) | (0.031) | (0.070) | (0.039) | (0.053) |
| Student fixed effects | | | | X | X | X |
| $p$-value (T3 × Fail = T1 × Fail) | 0.09 | 0.38 | 0.03 | 0.73 | 0.10 | 0.09 |
| Observations | 303,374 | 303,374 | 303,374 | 303,374 | 303,374 | 303,374 |

See the notes to table 3 for sample definitions. All regressions include CBG by year fixed effects and assigned school fixed effects, as well as lagged average school test scores for assigned elementary, middle, and high schools. Includes only students in grade 8 or lower since we have no failing high schools in our data set. Column headings indicate dependent variables, which are dummies for attending nonassigned schools, nonassigned nonmagnet schools, and magnet schools. Specifications in columns 1 to 3 include controls for race, gender, grade, and first test scores in CMS. Specifications in columns 4 to 6 replace student characteristics with student fixed effects. Standard errors clustered by CBG. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

also present the $p$-value associated with the $F$-test of the null hypothesis that the coefficient on the interaction term for the third tercile equals the coefficient on the interaction term for the first tercile ($\gamma_3 - \gamma_1 = 0$). We present the results from this hypothesis test since we are directly interested in whether we observe stronger responses among households living in the highest-quality neighborhoods.

Column 1 presents results where the dependent variable is the natural log of the sale price of residential homes.[17] We begin by noting that the estimated coefficient on the interaction term for the first tercile of neighborhood quality is negative in column 1 but statistically insignificant. Specifically, our results suggest that in the lowest-quality neighborhoods, failure to meet AYP standards reduces property values, but the estimate lacks statistical precision. Our finding that failing designation reduces home values is consistent with the results of Figlio and Lucas (2004), who find that housing markets respond to the assignment of letter grades for school quality even after controlling for test scores.

Turning to the estimated coefficients on the interaction terms for the higher-quality neighborhoods (T2 and T3), we note that both are positive and statistically significant. In terms of magnitude, our estimates imply that the highest-quality neighborhoods within the attendance zones of schools that fail to meet AYP experience between 11.6% and 8.4% increases in housing values relative to neighborhoods on the other side of the attendance zone boundary. These results are consistent with the notion that relative housing demand increases in the best neighborhoods in attendance zones of failing schools due to the improved likelihood of being admitted into higher-performing, oversubscribed schools.

Columns 2 and 3 of table 3 present results where the dependent variable is the natural log of home buyer income. Column 2 is for the full sample of owner-occupied transactions from the HMDA data, while column 3 restricts the sample by eliminating observations where mailing and physical addresses differ. Similar to the housing value results, the estimated coefficient on the interaction term for the lowest-quality neighborhoods (T1) is negative in columns 2 and 3 but statistically insignificant. However, for the highest-quality neighborhoods (T3), income increases with AYP failure by between 13.1% and 20.2%: the nicest neighborhoods attract higher-income borrowers after the NCLB failure occurs as compared to the housing in the same neighborhood but the attendance zone for a nonfailing school. Finally, we note that in all three columns of table 3, we easily reject the null hypothesis that the estimated coefficient on the third tercile interaction equals the coefficient on the first tercile interaction at the 5% level or better.

In table 4 we turn our attention from housing transaction outcomes to student outcomes and use equation (1) to ask how failure to meet AYP standards affects student participation in choice programs in the sample of original residents. The dependent variables in table 4 are indicator variables that take the value of unity if a student attends a nonassigned school, a nonassigned, nonmagnet school, or a magnet school, respectively. The estimates in columns 1 to 3 show that among original residents, the use of nonmagnet school choice increases for all neighborhoods, but for magnet schools, the likelihood of a student attending such a school increases only in the highest-quality neighborhoods. Columns 4 to 6 add student fixed effects to the specifications in columns 1 to 3. In column 6 (attend magnet school), the estimated coefficient on the third tercile interaction increases in magnitude with the inclusion of student fixed effects, while in column 5 (attend nonassigned, nonmagnet school), the estimated coefficient on the third tercile interaction declines in magnitude and becomes statistically insignificant.[18] Thus, our specifications with student fixed effects suggest that after AYP failure, families in the highest-quality

[17] Note that the sale price specification also includes 47 indicators for unique structural attributes and measures of proximity to downtown Charlotte and the Interstate.

[18] Note that the estimates reported in table 4 represent intent to treat (ITT) estimates, while the estimates reported in table 3 and all subsequent tables represent treatment on the treated (TOT) estimates.

TABLE 5.—IMPACT OF FAILING DESIGNATION ON ATTENDANCE AT NONASSIGNED SCHOOL BASED ON CURRENT RESIDENCE

| | (1) Attend Nonassigned School | (2) Attend Nonassigned, Nonmagnet School | (3) Attend Magnet School |
|---|---|---|---|
| NBHD T1 × Fail | 0.066*** | 0.043 | 0.022* |
| | (0.023) | (0.028) | (0.013) |
| NBHD T2 × Fail | 0.060 | 0.041 | 0.020 |
| | (0.038) | (0.037) | (0.013) |
| NBHD T3 × Fail | 0.382*** | 0.278*** | 0.103* |
| | (0.071) | (0.091) | (0.059) |
| *p*-value (T3 × Fail = T1 × Fail) | 0.00 | 0.01 | 0.17 |
| Observations | 306,651 | 306,651 | 306,651 |

See the note to table 3 for sample definitions. All regressions include CBG by year fixed effects and assigned school fixed effects as well as lagged average school test scores for assigned elementary, middle, and high schools. Includes only students in grade 8 or lower since we have no failing high schools in our data set. Column headings indicate dependent variables, which are dummies for attending nonassigned schools, nonassigned, nonmagnet schools, and magnet schools. All specifications include student fixed effects. Standard errors clustered by CBG. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.10$.

neighborhoods exercise their school choice option mainly by enrolling their children in magnet schools.

Our finding that use of magnet schools increases in the highest-quality neighborhoods after a school receives a failing designation suggests that households located in these neighborhoods value the expanded school choice options that come with a failing designation. Specifically, while a failing designation provides information to parents that their child's assigned school is in need of improvement, it also provides those same parents with increased odds of gaining admission to an oversubscribed, high-performing school if they remain in their current school attendance zone.[19] The fact that parents located in the highest-quality neighborhoods within a failing school zone enroll in magnet schools at higher frequencies suggests that it is higher-income households that are most likely to take advantage of the NCLB choice options. That interpretation is consistent with the results of Hasting et al. (2009), who find that higher-SES parents are more likely to utilize school choice options to send their children to higher-performing schools.[20]

### B. *Effects of Failure using Current Residential Location*

Table 5 presents estimates from equation (3), where we allow the use of school choice to depend on each student's current residential location. We continue to restrict the sample to students who are in CMS in the 2002–03 school year and use the 2002–03 attendance zone boundaries. All of these specifications include student fixed effects in order to minimize bias from selection into schools and neighborhoods. The inclusion of student fixed effects implies that our estimates are identified by the decision of individual students to attend choice schools following a failure. The estimates for attending a nonassigned school or a nonassigned, nonmagnet school change dramatically relative to the results reported in table 4 with students in the highest-quality neighborhoods now being 27.8 percentage points more likely to attend such schools. The estimates for magnet school attendance are relatively unchanged.

The results reported in table 5 suggest that recent movers play a significant role in the effect NCLB failure has on attendance at a nonassigned school. In order to examine this directly, we estimate our use of school choice models from equation (3) separately for students who live in a different neighborhood (movers) and for students who live in the same neighborhood (stayers) as compared to our base year of 2002–3. These results are shown in table 6. Columns 1 to 3 present results for the sample of movers, while columns 4 to 6 present the same information for stayers. We find that movers into the highest-quality neighborhoods are 66 percentage points more likely to attend a nonassigned, nonmagnet school. Again, these results are consistent with the effect of AYP failure on nonassigned school attendance being driven largely by movers.

Table 7 presents a final exercise designed to isolate decisions about school choice that are most likely to be related to failure. Since the children of families that move are entitled to remain at their current schools, some families that changed residence and moved to a new school attendance zone may continue to send their child to the school associated with their previous residence. Indeed, this behavior may become much more likely when the school associated with their new residential location fails to meet AYP. The specifications reported in table 7 mitigate this concern by recoding the dependent variable (attend a nonassigned school) to 0 if a student remains in his or her original 2002–3 school after moving. As the results reported in table 7 reveal, recoding students that remain in their original 2002–3 school after moving as not attending a nonassigned school has a dramatic effect on the results: the estimated coefficients for movers into the highest-quality neighborhoods fall substantially in magnitude but remain statistically significant at the 1% level. These results suggest that a significant fraction of the families that move into a failing school zone exercise school choice by continuing to send their child to their original 2002–3 school after moving. Nevertheless, the results reported in column 2 suggest that movers

[19] Magnet schools tend to be higher performing than traditional public schools in terms of average test scores. Specifically, from 2004 to 2011, the average standardized reading and math test scores were 0.18 and 0.15 for magnet schools, −0.57 and −0.55 for failing schools, and 0.11 and 0.12 for nonfailing, nonmagnet schools.

[20] We also examine the effects of AYP failure on student test scores, but in general the estimates are too noisy to be informative. See appendix B.

TABLE 6.—SEPARATE ESTIMATES OF IMPACT OF FAILING DESIGNATION ON ATTENDANCE AT NONASSIGNED SCHOOL FOR MOVERS AND STAYERS

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Movers | | | Stayers | | |
| | Attend Nonassigned School | Attend Nonassigned, Nonmagnet School | Attend Magnet School | Attend Nonassigned School | Attend Nonassigned, Nonmagnet School | Attend Magnet School |
| NBHD T1 × Fail | 0.032 | 0.008 | 0.024 | 0.089*** | 0.072* | 0.018 |
| | (0.028) | (0.034) | (0.015) | (0.033) | (0.037) | (0.017) |
| NBHD T2 × Fail | 0.019 | 0.009 | 0.009 | 0.116* | 0.100* | 0.016 |
| | (0.024) | (0.023) | (0.012) | (0.063) | (0.061) | (0.027) |
| NBHD T3 × Fail | 0.694*** | 0.664*** | 0.029 | 0.148 | 0.009 | 0.139 |
| | (0.138) | −0.101 | (0.071) | (0.133) | (0.057) | (0.095) |
| $p$-value (T3 × Fail = T1 × Fail) | 0.00 | 0.00 | 0.94 | 0.67 | 0.36 | 0.21 |
| Observations | 120,369 | 120,369 | 120,369 | 186,282 | 186,282 | 186,282 |

All boundaries based on 2002–3 school year. Sample restricted to students in grade 8 or lower for 2004–2011 school years. All terciles of CBGs based on average CBG housing prices for all transacted sales between 1998 and 2002. All regressions include CBG by year fixed effects, assigned school fixed effects, and lagged average school test scores for assigned elementary, middle, and high schools. Column headings indicate dependent variables, which are dummies for attending nonassigned schools, nonassigned, nonmagnet schools, and magnet schools. All specifications include student fixed effects. Columns 1 to 3 present results for students living in a different neighborhood than they did in 2003. Columns 4 to 6 present results for students living in the same neighborhood as they did in 2003. Standard errors clustered by CBG. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

TABLE 7.—ALTERNATIVE SPECIFICATIONS OF IMPACT OF FAILING DESIGNATION ON ATTENDANCE AT NONASSIGNED SCHOOL FOR MOVERS

| | (1) | (2) | (3) |
|---|---|---|---|
| Variable | Attend New Nonassigned School | Attend New Nonassigned, Nonmagnet School | Attend New Magnet School |
| NBHD T1 × Fail | 0.020 | 0.008 | 0.011 |
| | (0.024) | (0.026) | (0.013) |
| NBHD T2 × Fail | 0.018 | 0.010 | 0.008 |
| | (0.023) | (0.022) | (0.010) |
| NBHD T3 × Fail | 0.236** | 0.268*** | −0.033 |
| | (0.101) | (0.097) | (0.040) |
| $p$-value (T3 × Fail = T1 × Fail) | 0.03 | 0.08 | 0.26 |
| Observations | 120,369 | 120,369 | 120,369 |

Results based on specifications that recode the dependent variable to 0 if a student remained in his or her original 2002–3 school after moving. Column headings indicate dependent variables, which are dummies for attending nonassigned schools, nonassigned, nonmagnet schools, and magnet schools. All boundaries based on 2002–3 school year. Sample restricted to students in grade 8 or lower for 2004–2011 school years. All terciles of CBGs based on average CBG housing prices for all transacted sales between 1998 and 2002. Terciles for CBG prices are restricted to CBGs where prices fall within the range of any CBG that contains a failing neighborhood. All regressions include CBG by year fixed effects and assigned school fixed effects, as well as lagged average school test scores for assigned elementary, middle, and high schools. All specifications include student fixed effects. Standard errors clustered by CBG. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

TABLE 8.—IMPACT OF FAILING DESIGNATION OF STUDENT RESIDENTIAL MOBILITY BASED ON CURRENT RESIDENCE

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Any Failing | −0.001 | | 0.001 | |
| | (0.011) | | (0.014) | |
| NBHD T1 × Fail | | 0.015 | | 0.016 |
| | | (0.017) | | (0.018) |
| NBHD T2 × Fail | | −0.029** | | −0.027 |
| | | (0.014) | | (0.020) |
| NBHD T3 × Fail | | 0.169*** | | 0.227*** |
| | | (0.047) | | (0.070) |
| Student fixed effects | | | X | X |
| $p$-value (T3 × Fail = T1 × Fail) | | 0.00 | | 0.00 |
| Observations | 306,142 | 306,142 | 306,142 | 306,142 |

The dependent variable is an indicator variable that equals 1 if a student moved into the neighborhood in the previous year and is based on current residence. Columns 3 and 4 include student fixed effects. All boundaries based on 2002–3 school year. Observations include 2004–2011 school years. All terciles of CBGs based on average CBG housing prices for all transacted sales between 1998 and 2002. Terciles for CBG prices are restricted to CBGs where prices fall within the range of any CBG that contains a failing neighborhood. All regressions include CBG by year fixed effects and assigned school fixed effects, as well as lagged average school test scores for assigned elementary, middle, and high schools. Lose 509 observations due to incomplete addresses. Standard errors clustered by CBG. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

into the highest-quality neighborhoods who do not continue to send their child to their original 2002–3 school after moving are approximately 27 percentage points more likely to attend a nonassigned, nonmagnet school.

## C. *Examining the Location Choices of Movers*

Table 8 presents estimates from equation (3) where we examine how AYP failure affects residential location choice within each block group. The dependent variable in table 8 is an indicator that equals unity if a student moved into the neighborhood in the previous year. This allows us to examine whether recent movers to a block group are more likely to move to the side of the block group that is within the attendance zone of a failing school. Column 1 presents results for the average effect of an AYP failure, while column 2 adds housing price tercile interactions. In the lowest-price neighborhoods, failure has no impact on

Table 9.—Falsification Tests

| | (1) Log Price | (2) Log Income | (3) Attend Nonassigned School | (4) Moved into Home (since last year) |
|---|---|---|---|---|
| Falsification tests based on random attendance boundary shifts | | | | |
| NBHD T1 × Fail | 0.0407 | 0.0181 | −0.0053 | 0.0005 |
| | (0.0540) | (0.0364) | (0.0309) | (0.0130) |
| NBHD T2 × Fail | 0.0059 | −0.0083 | −0.0085 | −0.0103 |
| | (0.0279) | (0.0380) | (0.0133) | (0.0153) |
| NBHD T3 × Fail | 0.0341 | −0.0561 | 0.0032 | 0.0129 |
| | (0.0460) | (0.123) | (0.0487) | (0.0463) |
| *p*-value (T3 × Fail = T1 × Fail) | 0.92 | 0.66 | 0.89 | 0.78 |
| Falsification tests Based on Non–Title 1 failing schools | | | | |
| NBHD T1 × Fail | −0.053 | −0.128 | 0.022 | 0.051 |
| | (0.205) | (0.111) | (0.037) | (0.050) |
| NBHD T2 × Fail | 0.023 | −0.064 | −0.037 | 0.021 |
| | (0.030) | (0.039) | (0.044) | (0.031) |
| NBHD T3 × Fail | 0.019 | 0.055 | −0.027 | 0.005 |
| | (0.019) | (0.036) | (0.030) | (0.020) |
| *p*-value (T3 × Fail = T1 × Fail) | 0.73 | 0.11 | 0.28 | 0.38 |
| Observations | 134,283 | 47,112 | 250,155 | 249,773 |

The table presents a series of falsifications tests for our main results. The top panel presents falsification tests based on random attendance boundary shifts. For each column, we estimate 100 regressions based on our original models and samples, but randomly shift our school attendance boundaries and treat homes and students as being assigned to schools based on those new (pseudo) boundaries. Cells indicate mean coefficients and standard deviations of those 100 regressions for the models based on column headings. Observations for each regression are smaller than main models and vary with each boundary shift due to the loss of some parcels when boundaries shift outside the school district boundaries. The bottom panel presents falsification test based on non–Title 1 failing schools. For each model given in column headings, we drop all observations with any failing schools and then assign schools as pseudo-failing in a year if the school missed AYP in the same subject for the previous two years and is not a Title 1 school. The row headings indicate pseudo-fails, which we limit to our three terciles of failing neighborhoods. Columns 1 and 2 are based on the first two models presented in table 3 for housing market outcomes, and columns 3 and 4 focus on current resident models with individual fixed effects. Standard errors clustered by CBG. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

the likelihood of being a mover into the neighborhood. In contrast, in the highest-quality neighborhoods (T3), residents on the failing side of the block group become significantly more likely to be recent movers into the block group. As shown in columns 3 and 4, these results are robust to adding student fixed effects. The results reported in table 8 are therefore consistent with families moving into the best neighborhoods of the attendance zone of a struggling school soon after that school has an AYP failure.

### D. *Falsification Tests*

In this section, we present falsification tests for our core housing market and school choice models. In the first falsification test, we reestimate our model 100 times while randomly shifting the attendance zone boundaries by between one and two times the average diameter of a census block group, which is 3,590 feet in our sample.[21] This falsification test compares students on either side of a fake attendance zone boundary. If our results were driven by spatial patterns running through our data, such as schools and neighborhoods both becoming worse as one moves toward the south side of the city, then our results should also arise when the boundaries have been shifted so that the students on either side are actually assigned to the same school.

For the second falsification test, we drop any Title 1 school that experienced two failures and then assign schools as pseudo-failing in a year if the school missed AYP in the same subject for the previous two years and is not a Title 1 school. Since non–Title 1 schools are not subject to the choice sanctions associated with NCLB, there should be no incentive for families to strategically move into the best neighborhoods in the attendance zones of these schools.

Table 9 presents the falsification tests for our key models. Columns 1 and 2 reestimate the housing price model and the income model for the full sample of mortgages from table 3, column 3 reestimates the model for current residents attending a nonassigned school from table 4, and column 4 reestimates the moved-into-neighborhood/block group model from table 8. The top panel of table 9 reports results based on specifications where we randomly shift the attendance zone boundaries, while the bottom panel reports results where we assign schools as pseudo-failing in a year if the school missed AYP in the same subject for the previous two years and is not a Title 1 school. As table 9 reveals, in all four columns, the estimates from these falsification tests are statistically insignificant.

We conduct two further falsification tests that are presented in tables 1A and 2A of the appendix. The first falsification test involves treating schools that failed twice in a given year as if they failed AYP two years earlier. For this test, we add two years of observations prior to the beginning of AYP testing and drop all observations following the actual AYP failure. In the second test, we drop any Title 1 school that experienced two failures and treat schools that experienced a single failure as failing AYP in the year following that failure. In all but one case, we continue to find no positive and statistically significant relationship between AYP failure and any of our outcomes for the third tercile. The one case where we do find a positive and statistically significant coefficient is in the home buyer income model reported in table 2A. However, even in that case, the estimated coefficient is quite small in magnitude relative to the estimate reported in table 3.

[21] The random shifts are based on shifting the entire school district map of school attendance boundaries, and both direction and the distance of the shift are randomly determined. Results are robust to different distances of boundary shifts, as well as different directions.

Table 10.—Impact of Failing Designation on Housing Market, Mobility, and Attendance at Nonassigned School by Early- versus Later-Failing Schools

| Variable | (1) Log Price | (2) Log Income | (3) Attend Nonassigned School | (4) Moved into Home (since last year) |
|---|---|---|---|---|
| NBHD T1 × Fail | 0.024 | −0.090 | 0.092*** | 0.011 |
| | (0.054) | (0.059) | (0.018) | (0.022) |
| NBHD T2 × Fail | 0.483 | 0.123 | 0.039 | −0.044 |
| | (0.305) | (0.132) | (0.055) | (0.037) |
| NBHD T3 × Fail | 0.203** | 0.108 | 0.448*** | 0.230*** |
| | (0.080) | (0.76) | (0.69) | (0.036) |
| NBHD T1 × Fail × Later Fail | −0.133 | 0.127* | −0.046 | 0.013 |
| | (0.088) | (0.067) | (0.043) | (0.032) |
| NBHD T2 × Fail × Later Fail | −0.408 | −0.147 | 0.033 | 0.018 |
| | (0.302) | (0.144) | (0.075) | (0.044) |
| NBHD T3 × Fail × Later Fail | −0.180 | 0.128 | −0.213** | −0.213*** |
| | (0.119) | (0.98) | (0.104) | (0.044) |
| *p*-value (T3 × Fail = T1 × Fail) | 0.03 | 0.02 | 0.00 | 0.00 |
| *p*-value (T3 LaterFail = T1 LaterFail) | 0.21 | 0.99 | 0.10 | 0.00 |
| Observations | 157,955 | 52,666 | 306,651 | 306,142 |

All boundaries based on 2002–3 school year. Observations include 2004–2011 school years. Later Fail is a dummy for schools that obtained failing status in 2007 or later. All models include CBG by year fixed effects, as well as fixed effects for each unique combination of assigned elementary, middle, and high school in 2002–03, quarter-by-year fixed effects, as well as lagged average school test scores for assigned elementary, middle, and high schools. The price model includes the same additional controls as in table 3. Column 2 indicates our main mortgage income model. Columns 3 and 4 focus on current resident models with student fixed effects. Standard errors clustered by CBG. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.10$.

The results from the balancing tests presented in table 2 tend to rule out any systematic differences between individuals residing on either side of the boundary prior to redistricting. The falsification tests reported in the top panel of table 9 are designed to further rule out the possibility that our results are driven by systematic spatial variation in residential composition across the entire school district. Similarly, another set of falsification tests reported in the appendix attempts to address variation over time by moving the failure two years earlier in time, but these tests are imperfect in part because AYP failure and the declines in school quality that precede failure can have direct (although probably negative) effects on the observed outcomes. Therefore, we cannot entirely rule out the possibility that our primary results are driven by systematic variation over time that arises as schools and residential populations adjust to the new boundaries that were drawn shortly before the implementation of NCLB.

However, we anticipate that any potential bias from these adjustments over time would be larger as AYP failure events get further away from the time of redistricting. Therefore, in addition to the falsification tests reported in the appendix, we also estimate a model where we interact the tercile interactions with whether the AYP failure occurred three or more years after the full implementation of AYP standards. The level estimates on the interaction terms provide the best estimates of our hypothesized effect since those estimates are based on events that happened very close in time to the redistricting. As shown in table 10, our results are robust in terms of significance and magnitude when based on only the schools that have an immediate AYP failure. The estimated effects for the third tercile interactions are smaller using the schools that experience a later fail, but positive effects remain for all key outcomes except for whether the individual moved into his or her home in the past year. The smaller estimates are consistent with potential bias toward 0 from changes over time along the redistricted boundaries.

## VI. Conclusion

In this paper, we examine the housing market, school choice, and residential mobility changes that occur soon after a school fails to achieve AYP (for the second time) in the Charlotte, North Carolina, school district. We hypothesize that households with strong tastes for school quality may strategically move into the best neighborhoods in attendance zones of schools that fail to meet AYP standards for two consecutive years in order to improve their likelihood of being admitted into high-performing, oversubscribed schools.

Consistent with this hypothesis, we find that after a school receives a failing designation, residential property values and new home buyer income increase in the highest-quality neighborhoods within attendance zones of failing schools in comparison to portions of the neighborhood just outside the attendance zone. Our results also indicate that the probability of attending a nonassigned traditional school or magnet school increases in these high-quality neighborhoods. When we split our sample to examine the school choice decisions of families that remain in their original neighborhood after their assigned school fails to meet AYP (stayers) and the school choice decisions of families that move into the attendance zone after AYP failure (movers), we find that our results regarding attendance at a nonassigned school are being driven largely by movers. Specifically, families that move into the highest-quality neighborhoods in attendance zones of failing schools are 66 percentage points more likely to send their child to a nonassigned school and 27% more likely to send their child to a new, nonassigned school. Finally, among movers, families moving to the highest-quality neighborhoods are more likely to select the attendance zone associated with the AYP failing school.

From a policy perspective, our findings that incomes and housing prices rise in the nicer neighborhoods within the attendance zones of AYP failing schools suggest that expanded school choice opportunities may reduce residential income stratification and induce gentrification effects. In that sense, our results are consistent with the findings of theoretical studies that examine the general equilibrium effects of expanded choice (e.g., Nechyba, 2000; Epple & Romano, 2003; Ferreyra, 2007).

Furthermore, our finding that families with strong tastes for school quality strategically move into the attendance zones of failing schools in order to gain access to expanded school choice also points to an unintended consequence of NCLB and other large-scale accountability programs with significant school choice provisions: that the incentives created by these programs may lead to the benefits of the programs mainly accruing to households for which they were not intended. While the NCLB school choice provisions were designed to benefit the current students of AYP failing schools, households that move into the highest-quality neighborhoods within the attendance zones of failing schools are substantially more likely to send their children to a nonassigned school than the original residents.

Finally, because families that place the highest value on education are more likely to use school choice, the school choice provisions of NCLB most likely generate a downward spiral in a school's progress toward ever leaving AYP failing status. The fact that only a handful of schools ever exit failing status supports this possibility and probably contributed to North Carolina being granted a waiver from some of the mandated provisions of NCLB in 2012.

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