New Affordable Housing and School Quality Measures in Florida

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Abstract
This paper examines the impact of introducing new affordable housing on the school quality, as defined by standardized test scores, of nearby schools. The results serve to allay the concerns of opponents of affordable housing who may fear the quality of their schools would deteriorate, as well as inform policy makers considering where to place new affordable housing. I utilize data on affordable housing and schools from the state of Florida. I use an event-study model to compare the standardized test scores of schools before and after the introduction of affordable housing nearby. Nearby “untreated” neighbors who do not receive new affordable housing serve as the matched control. I find no statistically significant general impact from the introduction of affordable housing, with a slightly negative general impact on test scores as the size of the new affordable housing increases. However, I find that the effect is dependent on socioeconomic status as measured by Free and Reduced Lunch Program eligibility. I show that standardized test scores increase in middle schools with low rates of Free and Reduced Price Lunch eligibility after the introduction of new affordable housing. While further study is needed, policymakers should take note that introducing new affordable housing near schools of a higher socioeconomic status causes the average achievement of the entire school to increase.

Acknowledgements
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Introduction

Property and school quality are inextricably linked by the limitations of geography. The educational quality of neighborhood public schools plays a major role in the decisions of home-searching middle to upper income parents. Understanding this understudied connection between geographical location and educational outcomes, proponents of affordable housing have sought to increase access to high quality education for lower income families by increasing the feasibility of living in well-resourced neighborhoods. Yet opposition to affordable housing has limited its expansion, with its availability on the verge of shrinking in the Palo Alto community. In particular, at the Buena Vista Mobile Home Park only a few miles away from Stanford campus, affordable housing residents whom I have had the privilege to know and work with are facing eviction from the homes they have lived in for, in some cases, decades. They are only a small part of the human story in the Bay Area of newly displaced families who can no longer afford their rents and are forced to leave their children’s schools behind and move as far as the Central Valley.

Meanwhile, many opponents of affordable housing support it in practice yet oppose it in their own local community. A review of the literature surrounding affordable housing opposition indicates that the concerns are influenced by stereotyping of the poor, racial minorities and immigrants (Tighe 2010). When officially surveyed, opponents of increased affordable housing cite fears of decreased property values, increased crime and decreased school quality as their main concerns (The Campaign for Affordable Housing, 2004).

Motivated by an interest to support affordable housing expansion efforts and to further analyze the connection between geographical location and education, I seek to investigate the validity of their concern regarding the impact of affordable housing on school quality. In so
doing, I contribute not only to a sparse literature on the broader neighborhood impact of affordable housing but also to a present policy debate. Namely, does expanding or introducing affordable housing significantly impact the academic results of the community’s nearby schools? Only one previous study has addressed the issue with an econometric and macro analysis rather than an anecdotal case study examination. I seek to answer the question by examining the impact introducing new affordable housing units had on school outcomes in the entirety of the state of Florida from 2002 until 2013.

As a result of the continuous introduction of new affordable housing units throughout the time period and the inherent unobservable uniqueness of each school, I employ a long-horizon event study with neighboring schools serving as the matched control. The event study methodology originates in the financial economics literature, typically used by researchers to compare expected returns of companies or securities in stock markets with the abnormal returns caused by some event, such as an oil spill or scandal. In this case, the introduction of affordable housing into the proximity of a school serves as the “event” and the “treatment” faced by the school. Furthermore, here returns are educational outcomes in the form of standardized test scores, and are compared against the average score of their “untreated” neighbors who did not receive affordable housing nearby.

My first model looks only at the introduction of housing, disregarding its size. I find a statistically insignificant relationship between the introduction of affordable housing and standardized test scores. The second model looks at the impact of the size of the housing relative to the size of the school. At first glance, larger housing developments appear to have a slightly negative but statistically significant impact on standardized reading test scores in elementary schools. However, when I examine in more detail how the socioeconomic status of the school
(measured by Free and Reduced Price Lunch eligibility) affects the school’s response to the introduction of affordable housing. I find larger housing relative to the size of the school in higher socioeconomic status schools has a statistically significant positive impact on standardized test scores, with the impact decreasing as the Free and Reduced Price Lunch eligibility rate of the school increases (schools of lower socioeconomic status).

The first section reviews relevant literature. Studies on affordable housing’s impact on the student performance of residents generally show a positive educational benefit for individuals living in affordable housing. Studies on affordable housing’s impact on property values and crime rates represents the most developed literature detailing affordable housing’s impact on neighborhood outcomes. I then briefly discuss the sparse literature on affordable housing’s impact on school quality. The second section explains and summarizes the four data sources I compiled to answer the question, as well as highlighting relevant data trends. The third section reports on a validity check used to test the key assumption of an event study that treated and untreated schools have similar trends in test scores over time before the introduction of affordable housing. The third section also outlines the broader empirical strategy, presenting the two models used. The fourth section presents the results while the fifth section interprets them and discusses possible mechanisms by which affordable housing could impact school quality. The sixth section discusses the limitations of the empirical method used and outlines potential lines of further study. The seventh section concludes.

**Literature Review**

The literature around affordable housing studies two distinct effects: its impact on affordable housing residents as individuals and its impact on the surrounding neighborhood. The literature on the impact on residents as individuals examines the potential outcomes on a wide
variety of life outcomes. The literature on the impact of affordable housing on the broader neighborhood or community focuses largely on its impact on nearby property values and crime rates. The literature on its impact on overall school quality is a noted gap in the literature.

*The Beneficial Impact for Residents of Living in Affordable Housing*

Social scientists and advocates of affordable housing have studied its impact on residents for decades. Below I discuss major examples from the literature, specifically in the context of affordable housing’s impact on resident educational outcomes. For a considerable time since its inception in the 1950s, affordable housing in the United States was synonymous with large scale public housing projects of poor quality that were cheap and easy to construct. However, society came to see them as concentrators of poverty. More recent affordable housing efforts to improve resident welfare have focused either on low-density public housing or on relocation and voucher programs. Voucher programs present prospective buyers with discounted rents at cooperating properties, with the federal Section 8 voucher the most commonly used. However, despite this negative image of old-fashioned affordable housing, when controlling for socioeconomic factors, traditional high-density public housing may have had a positive or at least neutral impact on the educational outcomes of residents, specifically reducing the likelihood of failing a grade (Currie and Yelowitz 2000). Additionally, Brian Jacob tracked the educational outcomes of individual residents who had formerly lived in some of the worst public housing in the nation in Chicago before and after the demolition of their decaying residences. From the comparison of their educational performance before and after the demolition he interpreted his findings as showing that despite the horrible reputation of Chicago’s affordable housing, it had no direct negative effect on the educational outcomes of its former residents (2003). Worries over negative impacts of traditional high-density public housing projects on educational outcomes appear overblown.
In any case, it is more relevant to focus on present and recent programs better resembling current affordable housing policies. Researchers typically focus their efforts on evaluating particular programs due to the complexity of isolating the impact of moving into affordable housing. In 1995, James E. Rosenbaum investigated the Gautreaux family relocations program in Chicago in the late 70s, one of the most famous family relocation efforts. He found children whose families utilized the affordable housing voucher program to move to suburban schools had lower dropout rates, higher rates of college attendance and improved employment outcomes (Rosenbaum 1995). However, to see a positive effect on educational outcomes of individuals, successful relocation programs must ensure residents move to low-poverty areas, such as the Move to Opportunity program in Baltimore (Ladd and Ludwig, 1997).

The hypothesized list of mechanisms by which affordable housing can have the above found positive impact on educational outcomes is long and include such causes as increased familial residential stability, improved housing quality, and improved access to educational opportunity (Mueller and Tighe, 2007). Both a broader literature review and meta-analysis of 26 prior studies suggest frequent moves originating from housing instability, not family choice, reduces academic performance (Scanlon and Devine, 2001; Mehana and Reynolds, 2004). Affordable housing has the potential to prevent such residential and school upheaval. The general evidence is mixed on whether relocation specifically placing students in higher quality schools has an overall beneficial impact on educational outcomes, as found by Rosenbaum and discussed above (Turner and Acevedo-Garcia, 2005). Quality, family-based housing has been found to reduce overcrowding in domiciles (Mills et al. 2006), a stressor repeatedly found detrimental to childhood development (Evans et al. 1998). Quality housing may also improve education by improving health.
A more general review of the literature found that studies connecting housing stability and education show a strong correlation between decreased residential upheaval and improved student performance, but, despite the voluminous literature, suffer from a host of methodological issues in order to prove causation (Mueller and Tighe, 2007; Ellen and Turner, 1997). Despite the intuitive and demonstrated link between quality housing and education, more generalizable sophisticated studies in the literature, specifically focused on the magnitude of impacts, are needed.

*Affordable Housing’s Impact on Property Values and Crime*

I review other literature on affordable housing’s general neighborhood impact to draw parallels and trends that may extend to affordable housing’s impact on schools. A developed literature exists about affordable housing’s impact on property values, and to a lesser extent, crime rates. The large and decaying apartment projects of the past surrounded by urban neglect often received the title “hotbeds of crime” from affordable housing opponents. Yet a study by John E. Farley titled “Has Public Housing Gotten a Bum Rap?” suggests even large, notorious developments, specifically in St. Louis, do not suffer from higher crime rates than the rest of the city in comparison to the expected rates given neighborhood composition (1982). The general distribution of severity and type of crime also did not differ from the rest of city (Farley 1982, 470, 475). However, assessing crime in public and affordable housing has proven incredibly difficult from a methodological perspective due to potential underreporting by residents, or, conversely, over reporting by a heavy police presence (Holzman 1996, Farley 1982).

Partly as a result of their “bum rap” and a lack of public knowledge about public housing (Holzman 1996), such projects are no longer built. As mentioned earlier, in an effort to reform housing assistance, affordable housing now comprises of low-density units, voucher systems and
mixed-income housing. The intention of such programs is often to avoid concentrations of poverty and place individuals in more well-resourced communities. As regards crime, the evidence suggests the strategy of scattering affordable housing placements throughout a city appears to help further lower crime (California Round Table 1993). Similarly, the federal mixed-income and voucher program of the 1990s titled HOPE VI has brought numerous benefits to affordable housing residents, including but not limited to broader neighborhood revitalization and drastically lower crime rates, by replacing decaying public housing with new high-quality affordable and mixed-income housing (A Decade of Hope 2004, 58).

The concern around affordable housing and property values is the most well studied potential neighborhood impact of housing. The California Department of Housing and Community Development in 1988 reviewed studies of housing projects in the previous 26 years and found 13 of 14 studies concluded no negative impact on property values results from affordable housing (California Round Table 1993). Building off their work, Mai Thai Nguyen examined the broader literature to derive a more nuanced narrative on the impact of affordable housing and property values. Studies on clustered low-quality housing defined by poor architectural design and incompetent management in already declining neighborhoods has shown some negative impact on property values resulting from shoddy affordable housing (Nguyen 2005). However, by any measure, a study in Minnesota sums up the impact of affordable housing by stating “The maintenance and condition of the private housing stock in a neighborhood is more significant in determining property values than is the presence of publicly subsidized housing.” Negative findings are rare as neutral to no impact from affordable housing is the norm in studying affordable housing and property values, but when studies have found negative
impacts, the impact of affordable housing is small in comparison to other factors impacting property values (Nguyen 2005).

Unfortunately, I could not borrow any empirical strategies from either the literature on property values or crime as the present literature most commonly treats new affordable housing development as the epicenter of an impact, and measure how property values or crime rates change with distance from the epicenter. With both property values and crime, the impact of affordable housing is found to be nuanced but routinely small if even significantly positive or negative.

**Affordable Housing’s Impact on School Quality**

If schooling is in anyway similar, I expect to find a small to nonexistent impact dependent in part on the size and physical characteristics of the affordable housing. Contrary to the literature on affordable housing’s impact on property values and crime rates, the lack of empirical research of its impact on school quality represents a glaring hole in the present literature. As a 2007 review on the affordable housing literature by Mueller and Tighe remarked,

“One oft-expressed fear relating to affordable housing development is that the new housing will create a burden on a community’s school system by bringing large numbers of new students into already overcrowded schools. While claims abound, few studies systematically examine this issue. The few that do find that new housing, especially new affordable housing, has much less of an impact on municipal school costs than anticipated (2007, p. 376).”

Even among the small number of studies, the focus lies on costs rather than educational quality. A privately commissioned study measuring the impact of a low income housing tax
credit on the local schools demonstrated no difference in the number of children per unit in low-income housing compared to regular housing types (Danter). A study commissioned by the Citizen’s Housing and Planning Association in Boston took a similar approach with similar results (Sanborn et al., 2003). Both interpret their findings on the number of expected children to project that new affordable housing would have no impact on local schools as they would not be overburdened by unexpected increases in student population.

Yet the above studies may be wrong to assume that their focus on the impact of affordable housing on student population or school resources enables them to make any statement or projection on the impact on school quality. Even if the literature had found an unexpectedly high number of children occupying new affordable housing resulted in a negative impact on school resources, the education literature would not predict an automatic negative impact on school quality. Rather, in a review of the literature on school resources, Eric Hanushek finds over 400 studies show no strong or consistent relationship linking academic achievement and school resources (1997, Hanushek 2003).

One study, however, does attempt to directly address the link between affordable housing and education. A review commissioned by the Dallas Federal Reserve Bank of the impact of Low Income Housing Tax Credits (LIHTC) on the academic performance of local and public elementary schools examines the question by using longitudinal student data and LIHTC administrative data in Texas. Using a first-differenced ordered probit model, they find no overall adverse effects on school accountability and student tests scores resulting from the introduction of affordable housing. In other words, increased number of LIHTC units had no general impact on the probability of an individual average school receiving positive accountability grades from the state of Texas, given out by the government as grades on school quality. Yet they do find a
slight negative impact of introducing new LIHTC properties on the probability schools in high-minority or lower socioeconomic status neighborhoods receive better accountability grades (Di and Murdoch, 2010). But interestingly, they actually find a positive impact resulting from introducing new LIHTC properties in higher-income neighborhood schools. One potential interpretation of their finding suggests the introduction of affordable housing and therefore an increased number of lower-income students attending the school improves the academic performance of all students.

Though their study required assumptions about student attendance to their assigned school and focused solely on elementary schools, their findings provide more incentive to strategically place new affordable housing into higher-income neighborhoods (Di and Murdoch, 2010). As a result of their finding, any study of affordable housing’s impact on school quality, including mine, must consider how schools at different levels of socioeconomic status may respond differently to new affordable housing.

**Summary**

Despite the need for further research, the literature suggests a positive potential of affordable housing to improve resident’s educational outcomes. This adds further importance to investigating the claims of those opposed to affordable housing. Research addressing worries about increased crime and decreased property values suggest little to no negative impact caused by affordable housing. Research on how school quality may be impacted is decidedly missing. I look to build off the work of Di and Murdoch. In the hope of contributing positively to the present literature and conversations around public housing, I will expand beyond solely examining elementary schools and one kind of housing and focus on Florida to complement their study of Texas.
Data

To answer if expanding or introducing new affordable housing significantly impacts the academic results of the community’s nearby schools I required data on both schools and affordable housing. Florida, conveniently, has the most comprehensive information on affordable housing at the state level in the nation. Ideally, available information would detail where students in a particular affordable housing location attend school, called mandatory attendance zones. However, information on mandatory attendance zones, historical or present, does not exist at the statewide level. Additionally, similar school choice issues arise as in the Texas analysis by Di and Murdoch, weakening the connection between geographical location and school attendance. Former Florida Governor Jeb Bush’s expanded school choice options in the early 2000s by allowing parents to easily move their student’s to charter schools and permitting districts to establish complete free choice for parents to select any public school. As a result of the school choice expansion and data limitation, I utilize proximity of affordable housing as a proxy for direct knowledge of where new affordable housing residents attend school. Utilizing proximity as an imperfect proxy rests on the assumption that families live close to the schools their children attend.

The diverse sources of funding for affordable housing at all levels of government disperses the relevant data to a broad number of organizations. Most affordable housing data in the United States, including California, is primarily managed by local public housing authorities and has not been collected at a statewide level. Luckily, in Florida the Florida Housing Data Clearinghouse (FHDC) has collected information from all the local housing authorities, as well as realtor and finance organizations, the University of Florida and U.S. Federal Departments like the Department of Housing and Urban Development. The FHDC data on the construction of new
affordable housing dates back as far as the middle of the 20th century, when government funded affordable housing first began in the United States.

I included all permutations resulting from the sources of funders or types of housing, including everything from government constructed public housing to government funded vouchers for private housing. I limited the information only to housing designated for families rather than, for example, the elderly. Most importantly for my analysis, the data contains information on the geographic location of affordable housing, the year a property began operating as affordable\(^1\) and the number of affordable housing units within the property.

\[^1\text{Within the data compiled by the FHDC, the various sources of funding for affordable housing report affordability start date in two distinct ways not at first available in the dataset. When reporting the affordability start date, some programs count the year when the property was ready for occupancy while others report the year of the issuance of the funds for the property. Therefore it would be possible that a property had an appraised construction date later than their affordability start date. I create my own affordability start date variable by taking the more recent of either the appraised construction date or the reported affordability start date. As an additional check I do not analyze properties listed as “Not Ready for Occupancy”.}\]
From 1998 to 2014, Florida constructed or funded 692 new affordable housing properties within the state, for a total of 106,192 units of housing. As seen in Table 1, the average number of units per property was 153.5. The largest property had 428 units while the smallest had only one. As seen in Figure 1, the average units of new affordable housing dedicated for families in Florida trends downwards since the first half of the 2000s, as new properties have grown smaller with time. This is in line with the trends towards smaller and smaller affordable housing properties described in the literature review. Figure 2 also demonstrates a slight decrease in the number of properties with time. The two trends combine to significantly reduce the number of total new units available over time. However, the graphs on decreasing affordable housing construction do not depict the nearly 100 properties funded in 2012 to 2014 but not yet constructed by 2014, suggesting the construction of affordable housing may yet rebound in Florida post-recession.

I obtain school data from a number of different sources and systems all within the Florida Department of Education. Florida began its Florida Comprehensive Assessment Test (FCAT) in 1998, with math tested in the 4th and 8th grades and reading tested in the 4th, 8th and 10th grades. From 1998 to 2011, Florida continued to administer the FCAT to an ever expanding number of grades and subjects, but I focus my analysis on the five aforementioned grades continuously tested by Florida. In 2011, Florida introduced the FCAT 2.0, with a slightly different scoring scale. The change to the FCAT 2.0 and its new scoring scale significantly reduced the average total score for 4th grade reading from around 300 to around 210, with the standard deviation now one third of the former test. The impact on the mean and standard deviations from introducing the FCAT 2.0 and the general change in average test scores from year to year is visible in Table 2. I treat the change to the FCAT 2.0 as I treat every year to year variation in school scoring, by
normalizing the scores for each year and each test type. Normalizing the scores of the test scores by year and test type is a standard way to account for changes in the structure of tests from year to year in the educational literature. With normalized scores, the outcome of interest becomes how a school’s average score shifted in respect to the mean, rather than the shift in raw numerical score, after the introduction of affordable housing.

Additionally, beginning in 2002, the Florida Department of Education began to compile data on school demographics. I primarily utilize demographic data on the percentage of students qualifying for Free and Reduced Price Lunch as a socioeconomic indicator of school wealth. Eligibility for Free and Reduced Price Lunch is determined directly from reported family income and household size, making it representative of the income determination of socioeconomic status.

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In a given year, no score observations existed for schools who tested fewer than 10 students out of privacy concerns for the students. If School X tested 9 students in the 4th grade in 2003 but 15 in 2004, I had observations for one year but not the other. The missing year of observations prevented me from including School X as either a treated or control school in the proximity of 2003, but it could potentially serve as either in an event window spanning from 2004 to 2008 if no other missing observations existed.
Lastly, I received school coordinates and information on the type of school (public, charter, etc.) from greatschools.org, a non-profit that produces an online service where individuals can input a mailing address and receive a list of schools associated with their address, parent reviews of the school and a rating determined by greatschools. Since I only have school coordinate information for the present, I have to assume schools do not change location since 2002. I focus only on public schools in my analysis.

I use the information on school location and affordable housing to determine what schools are considered treated and what serve as the control in my analysis. Specifically, schools that received affordable housing within a two mile radius are considered treated. I then employ matching to compare the treated schools to some form of control. The intention of matching is to use a perfect untreated counterfactual for the treated school. Ideally, the match is so similar that the only discrepancy in their outcomes after some treatment, the introduction of affordable housing for example, can be isolated as the impact of the treatment. I use the average performance of the neighboring schools that did not receive affordable housing as the match for my treated schools. Proximity will hopefully account for unobservable characteristics that nearby schools share, such as similar district policies or economic trends. I avoid matching on the basis of covariates, or observed similarities in school type, due to lack of observed information on a sufficiently large number of characteristics. Imagine if you sought to find the perfect match for a school. With data on every detail of the schools and the surrounding neighborhoods you may find good pairs of schools that are similar to each other in every way aside from one of each pair being treated and the other being untreated. If the schools are this similar, the assumption is that

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3 I conducted almost all of my data merging and analysis within the data analysis program Stata. However, I had to translate my data into R for merging by coordinates and proximity. With much gratitude to the Social Science Data Software consulting, I wrote a code in R to merge the school and housing data on the basis of geographical distance between schools and housing.
the treatment was an essentially random chance to happen to one school and not the other. This sort of one-to-one matching is the most common. However, in the absence of such detailed information, I can utilize proximity to make a similar assumption on the randomness of affordable housing placement. I assume the decision to place affordable housing closer to one school than to its nearby neighboring schools was an essentially random decision. I specifically determine the neighboring average in the following way. Their neighbors, which serve as their matched control, are the schools that were not treated (as defined above) and lie within 5 miles of the treated school. The average scores of their neighbors serves as the match or control to the treated school.

Unfortunately, to properly isolate the impact of affordable housing as starting in one specific year, I have to restrict my analysis to schools who did not receive housing twice within a concentrated amount of time. Specifically, I analyze an event window of two years either side of the introduction of affordable housing. The below summary statistics in Table 3 represent differences between the treated schools and their control, given this restriction.

<table>
<thead>
<tr>
<th>Variables</th>
<th>Count</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min</th>
<th>Max</th>
<th>Count</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Year</td>
<td>2,519</td>
<td>2006.3</td>
<td>3.13</td>
<td>2002</td>
<td>2013</td>
<td>2,519</td>
<td>2006.3</td>
<td>3.13</td>
<td>2002</td>
<td>2013</td>
</tr>
<tr>
<td>Units of Housing</td>
<td>2,519</td>
<td>32.41</td>
<td>76.82</td>
<td>0</td>
<td>730</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
</tr>
<tr>
<td>Avg. Students Tested</td>
<td>2,519</td>
<td>138.87</td>
<td>139.92</td>
<td>12.77</td>
<td>985</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
</tr>
<tr>
<td>Free and Reduced Price Lunch</td>
<td>2,519</td>
<td>62.1</td>
<td>23.02</td>
<td>4</td>
<td>100</td>
<td>2,519</td>
<td>54.29</td>
<td>20.33</td>
<td>0</td>
<td>92</td>
</tr>
<tr>
<td>Standardized 4th Grade Reading</td>
<td>1,639</td>
<td>-0.20</td>
<td>0.87</td>
<td>-5.54</td>
<td>2.48</td>
<td>1,607</td>
<td>-0.05</td>
<td>0.53</td>
<td>-1.90</td>
<td>1.46</td>
</tr>
<tr>
<td>Standardized 8th Grade Reading</td>
<td>541</td>
<td>0.04</td>
<td>0.70</td>
<td>-1.81</td>
<td>2.42</td>
<td>467</td>
<td>-0.06</td>
<td>0.62</td>
<td>-2.73</td>
<td>1.85</td>
</tr>
<tr>
<td>Standardized 10th Grade Reading</td>
<td>394</td>
<td>0.10</td>
<td>0.72</td>
<td>-2.66</td>
<td>2.65</td>
<td>323</td>
<td>0.22</td>
<td>0.70</td>
<td>-3.63</td>
<td>2.13</td>
</tr>
<tr>
<td>Standardized 5th Grade Math</td>
<td>1,658</td>
<td>-0.16</td>
<td>0.85</td>
<td>-2.98</td>
<td>2.64</td>
<td>1,606</td>
<td>-0.03</td>
<td>0.50</td>
<td>-2.56</td>
<td>1.52</td>
</tr>
<tr>
<td>Standardized 8th Grade Math</td>
<td>541</td>
<td>0.05</td>
<td>0.67</td>
<td>-1.76</td>
<td>2.15</td>
<td>467</td>
<td>-0.06</td>
<td>0.64</td>
<td>-3.19</td>
<td>1.62</td>
</tr>
</tbody>
</table>

The 2,519 observations come from the measurement of 510 introductions of affordable housing (events), with an observation for each school for the five years which compromise the event window. The not applicable observations (“N/A”) in the control schools reflect the fact
that they by design received no affordable housing. The average number of units of housing faced by treated schools appear much smaller than the overall average of all new affordable housing found earlier of 153.5 because the average includes observations of two years either side of the introduction of affordable housing where no affordable housing was introduced. The control and treated schools differ slightly in Free and Reduced Lunch Program rates, with treated schools averaging a Free and Reduced Lunch Program eligibility rate of 62.1% and control schools at 54.29%. Of note, the standardized test scores in all grades between the control and the treated appear nearly identical. For example, the average score for reading in the 4th grade is .15 standard deviations less for treated schools than control schools. As long as the small differences in control and treated school scores remain the same through time, this difference will not affect the analysis. In other words, I can go on with the analysis if the treated and control schools have similar trends in grades over time. I conduct a formal check of this difference in trend in the beginning of the empirical strategy section after framing the overall strategy.

**Empirical Strategy**

I seek to determine the empirical impact that the introduction of affordable housing has on the quality of nearby schools. I hypothesize the ideal experiment to better demonstrate how my empirical strategy adapts the ideal to fit the limitations of reality. The ideal experiment would in one specific year place new affordable housing in the mandatory attendance zone of “treated” schools with no new housing in the control schools. No differences would exist between control and treated schools prior to the placement of affordable housing. However, as a result of experimental implausibility, I measure the impact on schools by the introduction of affordable housing with a long-horizon event study.
I utilize an event study methodology to compare the standardized testing scores of treated schools before and after the introduction of affordable housing. The event study model is typically utilized in finance economics to measure the impact of an event, such as an unexpected board resignation, on the stock market. In the simplest event studies, researchers compare the returns faced by firms before and after the event, utilizing the results before the event to create an estimation of expected results after the event. Event studies are used to test the difference between the results occurring after the event and the expected results had the event never occurred, and this versatility makes it easy to apply to the case of affordable housing.

Event studies all have unique aspects, but contain the following general structure (MacKinlay, 1997). First one must define the event of interest, in this case the introduction of affordable housing. Second, one customarily studies the impact of the event over an event window spanning longer than the length of the event. If the introduction of affordable housing is measured as occurring in an event year, the event window in which I compare whether treated schools differ from control schools would therefore last at least two years. Then one must determine who is considered “treated” or impacted by the event, in this case schools within two miles of the introduction of affordable housing. An event study analysis must then have a metric by which to measure the abnormal outcome resulting from the introduction of affordable housing. I use standardized test scores as my outcome, and test the difference between the results of the treated schools and the results of their matched control observations after the introduction of affordable housing. Lastly, matching is recommended by Khotari and Warner as a way to strengthen long-horizon event studies by improving the accuracy of the control observations with which one compares the treated observations (2004).
I conduct an observational study in Florida that follows educational outcomes and the introduction of affordable housing from 2002 to 2013. Most previous studies examining the connection between affordable housing and school quality have focused on case studies. Only Di and Murdoch have run a similar statewide analysis of the impact of affordable housing on school quality.

Defining the “Event”

When conducting an event study, one must first define the “event”, or the “treatment.” As discussed in the data section, I consider the introduction of affordable housing within a two mile radius of a school as an event for that school and that year. The school is then “treated” for the length of the event window. I initially utilize an event window of two years. As mentioned above, unfortunately numerous schools face affordable housing in continuous years, such as in both 2003 and 2004. When the introduction of a nearby second or third affordable housing property causes event windows to overlap, I have to omit the entirety of that series of years from my analysis. Therefore, if I were to expand the event window to longer than two years either side of the “event”, the increase in event window length would lead to fewer “events” available for analysis and a weaker analysis.

Ideally, the residents of each new housing would be matched exactly to the school which its residents attend. I substitute proximity as measured by miles as an imperfect proxy for nonexistent data on mandatory attendance zones due to school choice issues as well as imperfect historical data on attendance zones, specifically considering as treated those schools within two miles of new affordable housing. In the absence of research on the average distance travelled to school in Florida or in the United States more broadly, I arbitrarily choose two miles as my determinant of proximity. Consequently, the results of my analysis can only be interpreted as
identifying the impact of introducing affordable housing within a two mile radius of a school. My analysis cannot reveal anything about the impact on school quality resulting from the attendance of new affordable housing residents.

I normalize the years relative to the year of new affordable housing to account for the continuous introduction of affordable housing throughout the entire time period. If school $i$ receives new affordable housing in 2005, for that school only the year 2005 is normalized to equal zero. For another school who receives new affordable housing in 2009, for that school only the year 2009 is normalized to equal zero. I am using a pre-event and post-event window of only two years. I therefore normalize all the years for each individual treated school such that every year in the analysis equals -2, -1, 0, 1 or 2. All other years are not relevant and are dropped for the analysis. For the aforementioned school $i$ who received affordable housing in 2005, 2003 becomes -2, 2004 becomes -1, and so on.

\[ Pre-Event \quad \quad \quad \quad Event \quad \quad \quad \quad Post-Event \]
\[ -2 \quad -1 \quad 0 \quad 1 \quad 2 \]
\[(Normalized \ Years)]

Comparing treated and untreated: Pre-event trends

As mentioned briefly in the data section, overall static differences in the standardized scores between the treated schools and control schools are not an issue if the dynamic trends are similar over time. I run the following regression on the pre-event normalized years -2 through 0, to identify whether or not the trends differ. I consider the year zero as a pre-event year,
effectively assuming that affordable housing has no impact in its year of construction. If the pre-event trends significantly differ, I cannot conduct an accurate event study.

\[
\text{Score}_{i,t} = \alpha_0 + \alpha_1 Treated_{i,t} + \alpha_2 \text{NormYears}_t + \alpha_3 Treated_{i,t} \times \text{NormYears}_t \\
+ \alpha_4 \text{FRPL}_{i,t} + \mu_t + \pi_i
\]

In this regression dedicated only to the pre-event normalized years -2 through 0, the variable \( Treated_{i,t} \) equals 1 if the city ever receives housing, and zero if it represents a school’s control (the average of neighbors). In this regression \( i \) represents a school (if treated) or the collected average of neighboring schools (if control). The \( t \) represents a specific normalized year. The variable \( \text{NormYears}_t \) represents the normalized year of either -2, -1, or 0. The variable \( \text{FRPL}_{i,t} \) measures the Free and Reduced Price Lunch eligibility. Figure 3 in the appendix demonstrates the growing difference over time in the academic performance between higher socioeconomic status schools and lower socioeconomic in Florida. We saw in the data section that treated and control schools have different average levels of Free and Reduced Price Lunch Program eligibility. If I do not add the variable \( \text{FRPL}_{i,t} \) to control for this growing dynamic difference in performance over time, the pre-trends might appear to diverge solely for this phenomenon in increasingly disparate academic performance which deserves lengthy study in of itself. The \( \mu_t \) is a year fixed effect that takes into account any otherwise inherent characteristics of each real year, since a normalized year of -1 could be 2000 for one school and 2010 for another. The variable \( Treated_{i,t} \times \text{NormYears}_t \) is an indicator variable of the \( Treated_{i,t} \) variable multiplied by the \( \text{NormYears}_t \) variable. The coefficient on \( Treated_{i,t} \times \text{NormYears}_t \), \( \alpha_3 \), is the coefficient of interest because it indicates the difference of the trends over time. For example, when \( Treated_{i,t} = 1 \), \( \alpha_1 \) will equal the static difference between
treated and control, \( \alpha_2 \) will equal the slope of the trend in scores for the averages of the neighbors (the control), and \( \alpha_3 \) will equal the difference between the slope of the scores of the control and the slope of the treated schools. Ideally \( \alpha_3 \) would equal zero, or \( H_0: \alpha_3 = 0 \), indicating no difference in slope between the treated and control, therefore assuring the dynamic trends are similar over time and we can continue with the analysis. The table below contains the results for all five different types of tests measured, with the coefficient of interest highlighted by the box.

\[
\begin{array}{lccccc}
\text{VARIABLES} & \text{Reading 4th} & \text{Reading 8th} & \text{Reading 10th} & \text{Math 5th} & \text{Math 8th} \\
\hline
\text{Treated} & 0.124*** & 0.106* & -0.144** & 0.0839** & 0.127** \\
 & (0.000280) & (0.0553) & (0.0416) & (0.0175) & (0.0232) \\
\text{Treated} \times \text{Years} & -0.00102 & -0.0259 & -0.00651 & -0.0319 & -0.0164 \\
 & (0.970) & (0.568) & (0.911) & (0.262) & (0.720) \\
\text{Normalized Year} & 0.00334 & 0.00709 & 0.00472 & 0.0124 & -0.00891 \\
 & (0.847) & (0.718) & (0.833) & (0.487) & (0.653) \\
\text{FRPL} & -0.0210*** & -0.0145*** & -0.0124*** & -0.0183*** & -0.0140*** \\
 & (0) & (0) & (0) & (0) & (0) \\
\text{Constant} & 1.081*** & 0.762*** & 0.717*** & 0.945*** & 0.709*** \\
 & (0) & (0) & (0) & (0) & (0) \\
\text{Observations} & 2,287 & 1,587 & 1,443 & 2,287 & 1,586 \\
\text{R-squared} & 0.417 & 0.233 & 0.174 & 0.339 & 0.228 \\
\hline
\end{array}
\]

*pval in parentheses

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

The coefficient on \( \text{Treated}_{it} \times \text{NormYears}_{it} \), \( \alpha_3 \), was not statistically significantly different from zero. We cannot reject our null hypothesis for any of the standardized tests,
allowing us to proceed with the analysis. In fact, the P-values for all of the tests, in parentheses, are far higher than what is typically required for statistical significance! The P-value for reading in the 4th grade, for example, equaled 0.97 when statistical significance by convention requires a p-value less than 0.05. This indicates that there is not even close to a hint of a significant pre-event divergence in trend between treated and control schools and we can progress with the analysis. The statistically significant results for the coefficients of the \( Treated_{i,t} \) variable in all of the tests demonstrates a difference in scale in terms of standardized test scores between the treated and control schools, as seen informally previously in the summary statistics in Table 3. However, in the models described below we can account for the static differences in scale by creating a relationship between the “treated” schools and their controls. For example, if a treated school’s scores are always 80% of its control before housing is introduced, we can still measure the change (or lack thereof) caused by housing. For example, if the treated school’s scores are now 85% of the control schools scores after affordable housing is introduced, we can attribute that 5% change to the introduction of affordable housing. Our validity check confirms no change in slope, allowing us to proceed with our planned regression models described below.

**Model with Binary Housing Indicator**

To reiterate, the event-study model compares the difference between a school’s results relative to its untreated neighbors, before and after the introduction of affordable housing. The untreated neighbors, discussed earlier, serve as a matched control for the treated schools. Comparing the treated schools with their untreated neighbors should then allow us to reveal the impact of affordable housing.

For each school \( i \) in the year \( t \) where \( t \) is the normalized years in the event window, I calculate the average normalized test scores of its nearby control schools, \( AC_{i,t} \). I then regress the
average normalized test score of the treated school, $AT_{i,t}$, on $AC_{i,t}$ as well as a fixed effect $\mu_t$ for each year and a binary variable for the introduction of housing. The fixed effect for year controls for unobserved general changes overtime. For example, it would control for statewide changes felt in a particular year, such as a change in school legislation. I attempt to control for changes in the assessments from year to year by normalizing the test scores to each year. The binary variable for the introduction of housing, $Housing_{i,t}$, takes a value of zero until affordable housing has been introduced, and takes a value of 1 for the following two years of the estimation window to reflect that the school is “treated”, that the school has received affordable housing nearby in a previous year. All this is designed to try and isolate the impact of affordable housing on the standardized test scores of nearby schools.

\[
AT_{i,t} = \beta_0 + \beta_1 AC_{i,t} + \beta_2 Housing_{i,t} + \beta_3 FRPL_{i,t} + \mu_t + \epsilon_i
\]

(1)

Since we are trying to identify the impact of affordable housing, the coefficient $\beta_2$ is the key coefficient of interest. I test a standard null hypothesis is $H_0: \beta_2 = 0$, or that the introduction of affordable housing had no impact on expected test scores. Failure to reject the null hypothesis would be consistent with the theorized result in the Literature Review section that affordable housing would have little to no impact on school quality.

Model with Continuous Ratio of Housing Size to School Size

However, a yes or no binary variable may not capture all of the impact of affordable housing. New affordable housing comes in a wide distribution of different sizes, from 1 unit (or bedroom) to 400 new units. Schools also vary wildly in size. A school testing only 20 students in a grade facing an influx of 400 nearby units of affordable housing might arguably have a
different response to new affordable housing compared to a school testing 200 students in a grade with only 10 new units of affordable housing. Therefore, in the second regression I replace the binary indicator of housing with a continuous variable representing the number of new units divided by the average number of students tested in a given year, \( \frac{\text{NewUnits}}{\text{AvgTesters}} \). This variable will measure the impact of introducing new affordable housing as well as the impact of increased housing size relative to school size. The equation takes a similar form to Model 1:

\[
AT_{i,t} = \gamma_0 + \gamma_1 AC_{i,t} + \gamma_2 \frac{\text{NewUnits}}{\text{AvgTesters}_{i,t}} + \gamma_3 FRPL_{i,t} + \mu_t + \epsilon_t
\]

(2)

We again try to isolate the impact of affordable housing by controlling with a variable representing Free and Reduced Price Lunch eligibility, year fixed effects and the average score of nearby neighbors (the main control). I am now interested in the coefficient on the new continuous variable, which increases in value as the number of new units grows in proportion to the maximum number of students tested by the school in one grade. However, I maintain a similar standard null hypothesis of \( H_0: \gamma_2 = 0 \), expecting the increase of units relative to the size of the school to have no impact on the difference between the expected test scores and the actual test scores for school \( i \) in the post-event window.

**Results**

*Table 5* contains the results from Model 1. Our coefficient of interest was the coefficient on housing, \( \beta_2 \), highlighted by the box in *Table 5* below with standard error in parentheses.
Table 5

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Reading 4th</strong></td>
<td>0.102*** (0.0288)</td>
<td>-0.0764*** (0.0292)</td>
<td>0.0705 (0.0512)</td>
<td>0.100*** (0.0325)</td>
<td>-0.0367 (0.0318)</td>
</tr>
<tr>
<td><strong>Reading 8th</strong></td>
<td>0.0262 (0.0366)</td>
<td>0.0677 (0.0681)</td>
<td>0.0152 (0.0312)</td>
<td>0.0391 (0.0413)</td>
<td></td>
</tr>
<tr>
<td><strong>Reading 10th</strong></td>
<td>0.0677 (0.0512)</td>
<td>0.0681 (0.0512)</td>
<td>0.0312 (0.0312)</td>
<td>0.0413 (0.0413)</td>
<td></td>
</tr>
<tr>
<td><strong>Math 5th</strong></td>
<td>0.100*** (0.0325)</td>
<td>-0.0367 (0.0318)</td>
<td>0.0391 (0.0413)</td>
<td>0.0413 (0.0413)</td>
<td></td>
</tr>
<tr>
<td><strong>Math 8th</strong></td>
<td>-0.0367 (0.0318)</td>
<td>0.0391 (0.0413)</td>
<td>0.0413 (0.0413)</td>
<td>0.0413 (0.0413)</td>
<td></td>
</tr>
</tbody>
</table>

FRPL

<table>
<thead>
<tr>
<th><strong>FRPL</strong></th>
<th>-0.0312*** (0.000705)</th>
<th>-0.0311*** (0.000908)</th>
<th>-0.0284*** (0.00216)</th>
<th>-0.0277*** (0.000761)</th>
<th>-0.0277*** (0.00102)</th>
</tr>
</thead>
</table>

Constant

<table>
<thead>
<tr>
<th><strong>Constant</strong></th>
<th>1.908*** (0.0507)</th>
<th>1.919*** (0.0608)</th>
<th>1.229*** (0.107)</th>
<th>1.692*** (0.0548)</th>
<th>1.730*** (0.0685)</th>
</tr>
</thead>
</table>

Observations

<table>
<thead>
<tr>
<th><strong>Observations</strong></th>
<th>1,607</th>
<th>467</th>
<th>323</th>
<th>1,606</th>
<th>467</th>
</tr>
</thead>
</table>

R-squared

<table>
<thead>
<tr>
<th><strong>R-squared</strong></th>
<th>0.600</th>
<th>0.728</th>
<th>0.423</th>
<th>0.494</th>
<th>0.626</th>
</tr>
</thead>
</table>

The null hypothesis of housing having no impact on standardized test scores, \( H_0: \beta_2 = 0 \), cannot be rejected for any combination of grade or subject. The introduction of new affordable housing had no significant impact on raising or lowering standardized test scores for schools within a 2 mile radius. Standardized 4th grade reading scores, for example, had a coefficient on housing value of -0.0114 and statistically indistinguishable from zero. Of our three main controls, the average score of nearby neighbors as well as the Free and Reduced Price Lunch eligibility rate are both represented in Table 5, while the fixed effect for years is not visibly shown but still present in the underlying regression. The Free and Reduced Price Lunch control and year controls are both statistically significant, with Free and Reduced Price Lunch resoundingly negative. For example, standardized 4th grade reading scores decrease by -0.0312 standard deviations for each one percentage increase in students eligible for Free and Reduced Price Lunch. Our matched control, the average of the neighbors’ scores, is statistically
significantly non-zero for 4th and 8th grade reading and 5th grade math, providing an extra guarantee for those tests in particular that the neighbors serve as a strong match for the treated schools.

When we introduce the continuous treatment variable in Model 2, the coefficient on \( \frac{\text{NewUnits}}{\text{AvgTesters}} \), \( \gamma_2 \), becomes the coefficient of interest and is highlighted by the box in Table 6 below.

![Table 6](image)

The null hypothesis of the introduction of new affordable housing having no impact regardless of the size of the affordable housing relative to the size of the school, \( H_0: \gamma_2 = 0 \), can be rejected for all of the combinations of grades and subjects except for 4th grade reading. The statistical significance and values of the coefficients of the controls barely vary from Model 1.
The result for 4th grade reading is statistically significantly non-zero at a p-value of 0.05, suggesting the introduction of new affordable housing can have slightly negative impact on elementary schools when the size of the new affordable housing is large relative to the size of the school. Since the variable of interest is a ratio, the size of the affordable housing relative to the size of the school, the interpretation of the coefficient value of -0.0222 suggests that when the number of new affordable housing units is equal to the average number of students tested by the school in a given year, the normalized test scores of the “treated” schools is 0.0222 standard deviations less than would be expected had affordable housing never been introduced within 2 miles of the school. The average treated school had a ratio of new affordable housing units to average number of students tested of 0.85, causing a standard deviation fall of 0.0187 relative to the test score had the average treated school never received nearby affordable housing. Aside from their potential use in visualizing the impact, raw test points are not especially informative metrics for changes in test scores as they are not comparable across tests. For this reason the educational literature discusses impact in terms of standard deviations.

**Discussion of Results**

Contrary to opponents of affordable housing, the results do not suggest any large negative impact from the introduction of affordable housing. The majority of the results suggest no impact. The only statistically significant result suggests very large affordable housing close to small schools may have a small negative impact. The small impact of -0.0222 standard deviations would be considered a minute change in the general education literature. However, the only previous similar analysis by Di and Murdoch found similarly slightly negative results for lower socioeconomic status schools and actually found larger positive results for higher socioeconomic status schools. Higher socioeconomic schools may have the resources required to
smoothly incorporate new students from affordable housing and actually benefit from the increased socioeconomic, racial and linguistic diversity in the classroom which an increasing body of research suggests improves the academic achievement of all students.

To see how “treated” schools at different socioeconomic levels respond to the introduction of affordable housing, in my models I introduce a new interacting variable where I multiply my housing variables by the Free and Reduced Price Lunch eligibility rate,

\[ Housing_{i,t} \times FRPL_{i,t} \] for the Model 1 and \[ \frac{NewUnits}{AvgTesters_{i,t}} \times FRPL_{i,t} \] Model 2. The coefficient on interactive variables show how two variables may simultaneously affect a third variable. In this case, the interacting variable will reveal how the relationship between housing and a school’s Free and Reduced Price Lunch eligibility rate simultaneously work together to impact the standardized test scores of treated schools. The equations for Models 1 and 2, unchanged aside from the additional interacting variable, now look as follows:

\[
AT_{i,t} = \delta_0 + \delta_1 AC_{i,t} + \delta_2 Housing_{i,t} + \delta_3 FRPL_{i,t} + \delta_4 Housing_{i,t} \times FRPL_{i,t} + \mu_t + \epsilon_i
\] (1.1)

\[
AT_{i,t} = \varphi_0 + \varphi_1 AC_{i,t} + \varphi_2 \frac{NewUnits}{AvgTesters_{i,t}} + \varphi_3 FRPL_{i,t} + \varphi_4 \frac{NewUnits}{AvgTesters_{i,t}} \times FRPL_{i,t} + \mu_t + \epsilon_i
\] (2.1)

The specific coefficient of interest is the coefficient on the interactive variables, \( \delta_4 \) in equation 1.1 and \( \varphi_4 \) in equation 2.1. If they are zero, then schools at all socioeconomic levels respond identically to new affordable housing. In other words, the impact of affordable housing on scores does not change even if a school has a 100% Free and Reduced Price Lunch eligibility rate or a 0% eligibility rate. When running equation 1.1 that hypothesis looks unlikely though the
results are all still statistically insignificant. When running equation 2.1, we can reject that hypothesis. In other words, as the results below demonstrate, housing looks to have a positive impact on standardized test scores that decreases as the Free and Reduced Price Lunch eligibility rate of the school increases.

Table 7

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Neighbors Avg.</td>
<td>0.102***</td>
<td>-0.0778***</td>
<td>0.0705</td>
<td>0.0994***</td>
<td>-0.0377</td>
</tr>
<tr>
<td>(0.0289)</td>
<td>(0.0292)</td>
<td>(0.0513)</td>
<td>(0.0325)</td>
<td>(0.0319)</td>
<td></td>
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<tr>
<td>Housing</td>
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<td>0.127</td>
<td>0.0848</td>
<td>0.130</td>
</tr>
<tr>
<td>(0.0908)</td>
<td>(0.110)</td>
<td>(0.162)</td>
<td>(0.0996)</td>
<td>(0.123)</td>
<td></td>
</tr>
<tr>
<td>FRPL</td>
<td>-0.0311***</td>
<td>-0.0302***</td>
<td>-0.0276***</td>
<td>-0.0272***</td>
<td>-0.0270***</td>
</tr>
<tr>
<td>(0.000952)</td>
<td>(0.00123)</td>
<td>(0.00283)</td>
<td>(0.00103)</td>
<td>(0.00139)</td>
<td></td>
</tr>
<tr>
<td>FRPL * Housing</td>
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<td>-0.00190</td>
<td>-0.00143</td>
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<td>-0.00149</td>
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<td>(0.00129)</td>
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<tr>
<td>Constant</td>
<td>1.901***</td>
<td>1.863***</td>
<td>1.199***</td>
<td>1.657***</td>
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<td>(0.0660)</td>
<td>(0.0788)</td>
<td>(0.130)</td>
<td>(0.0718)</td>
<td>(0.0888)</td>
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</tr>
<tr>
<td>Observations</td>
<td>1,607</td>
<td>467</td>
<td>323</td>
<td>1,606</td>
<td>467</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.600</td>
<td>0.729</td>
<td>0.423</td>
<td>0.495</td>
<td>0.627</td>
</tr>
</tbody>
</table>

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table 8

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Neighbors’ Avg.</td>
<td>0.101*** (0.0288)</td>
<td>-0.0729** (0.0290)</td>
<td>0.0662 (0.0513)</td>
<td>0.102*** (0.0325)</td>
<td>-0.0318 (0.0316)</td>
</tr>
<tr>
<td>Reading 4th</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reading 8th</td>
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<tr>
<td>Reading 10th</td>
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<tr>
<td>Math 5th</td>
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</tr>
<tr>
<td>Math 8th</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Units/Avg. Testers</td>
<td>-0.00921 (0.0365)</td>
<td>0.185** (0.0783)</td>
<td>0.00552 (0.131)</td>
<td>-0.0114 (0.0401)</td>
<td>0.160* (0.0882)</td>
</tr>
<tr>
<td>FRPL</td>
<td>-0.0310*** (0.000841)</td>
<td>-0.0298*** (0.00103)</td>
<td>-0.0292*** (0.00236)</td>
<td>-0.0275*** (0.000913)</td>
<td>-0.0264*** (0.00116)</td>
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<tr>
<td>FRPL * Units/Avg. Testers</td>
<td>-0.000188 (0.000504)</td>
<td>-0.00308*** (0.00117)</td>
<td>0.000611 (0.00184)</td>
<td>-0.000125 (0.000553)</td>
<td>-0.00311** (0.00131)</td>
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<tr>
<td>Constant</td>
<td>1.907*** (0.0586)</td>
<td>1.851*** (0.0668)</td>
<td>1.285*** (0.111)</td>
<td>1.704*** (0.0637)</td>
<td>1.677*** (0.0752)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,607</td>
<td>467</td>
<td>323</td>
<td>1,606</td>
<td>467</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.601</td>
<td>0.732</td>
<td>0.423</td>
<td>0.495</td>
<td>0.631</td>
</tr>
</tbody>
</table>

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

For equation 1.1, the coefficient on housing becomes more positive than in Table 5 (where we ran the original Model 1) while the coefficient on the indicator variable is negative for all grade and subject combinations. Though, again, the results are not statistically significant.

However, when running equation 2.2 with the new indicator variable, the original coefficient of interest on \( \frac{\text{NewUnits}}{\text{AvgTesters}_{i,t}} \) becomes statistically significant and positive for both Math and Reading in the 8th grade, while the coefficient on the interactive variable \( \frac{\text{NewUnits}}{\text{AvgTesters}_{i,t}} \ast FRPL_{i,t} \) is statistically significantly negative. Taking 8th grade reading as an example, the results in Table 8 show that for schools with no Free and Reduced Price Lunch eligible students (likely very wealthy schools), the introduction of affordable housing causes standardized test scores to increase by 0.185 standard deviations if the school size is equal to the number of new affordable
housing units (if $\frac{\text{NewUnits}}{\text{AvgTesters}} = 1$). If the size of the new affordable housing is even larger relative to the size of the school (if $\frac{\text{NewUnits}}{\text{AvgTesters}} > 1$, the positive impact on standardized test scores would be larger than 0.185 standard deviations. As the Free and Reduced Price Lunch eligibility rates of a school increase, the impact of new affordable housing grows smaller by -0.00308 for each percentage point increase in eligibility. A school with 50% Free and Reduced Price Lunch eligibility and school size equal to the number of new affordable housing units would have a 0.031 standard deviation increase in 8th grade reading scores as a result of new affordable housing ($0.185 \times 1 - 0.00308 \times 50\% = 0.031$).

Interpreting the coefficients indicates that for high socioeconomic status schools, the introduction of new affordable housing, even large affordable housing relative to the size of the school, has a statistically significant positive impact for middle schools. The coefficients suggest that for middle schools that receive new affordable housing units equal in number to the average number of testers in the school, the effect of affordable housing remains positive until the Free and Reduced Price Lunch eligibility rate surpasses 60% for reading scores and 51% for math scores. The results second the findings of Di and Murdoch that higher socioeconomic status schools actually see a small positive impact from the introduction of new affordable housing, and explain in more detail the slightly negative impact of new affordable housing found initially. In addition to the validity check, confirming the findings of Di and Murdoch lends further credence to the model design and data.

I also run a number of robustness checks and see no significant change in the results. For example, it is not entirely clear that affordable housing’s impact would only start in the year after the introduction of affordable housing, normalized year one, as I assumed. I run a regression
including the normalized year zero in both of the models with the interactive variable included to test if results change if I relax my assumption. The additional number of treated observations causes the coefficient measuring the introduction of new affordable housing’s impact on 8th grade readings to remain positive but also become statistically significant in Model 1. Meanwhile, the previously statistically significant observations on middle schools in Model 2 become more muted. The muting impact of including the year zero as a “treated” year implies affordable housing does not have a strong impact in its year of introduction, lending support to my earlier assumption.

In sum, insignificant results in both models at the high school level are not surprising given the relatively low number of measurable instances of new affordable housing. Insignificance for all grades and subjects in Model 1 indicates the binary measurement of “treatment” does not capture the full impact of affordable housing. In all possible interpretations of the results, however, it is clear that new affordable housing does not result in rapidly deteriorating school quality. If anything, opponents of affordable housing in wealthier communities could actually see an increase in average school test scores resulting from the introduction of new affordable housing.

Discussion of Limitations

First, in estimating only the impact on the average standardized test score of each individual school, the analysis is insufficient to make any broad statement about the impact on school quality, however it may be defined. Florida’s government measures school quality through accountability grades given to each school, a similar metric to the one measured by Di and Murdoch in Texas. Further analysis could undertake an ordered probit model measuring the impact of affordable housing on the likelihood of a change in the categorical measurement of
school grades. However, in calculating the accountability grades, the Florida Department of Education depends mostly on the very standardized test scores already measured here.\footnote{The manner of determining accountability grades has changed over the time period of interest, introducing an added complication. In 2014, grades were determined from 9 components, all but one of which came either directly or indirectly (through learning gains measurements) from FCAT 2.0 test scores in reading, math, science and social studies.} Therefore, such an analysis would likely return similar results.

Second, just as the analysis can only speak to the impact on average test scores for the entirety of the school, it can also only narrowly speak to the impact of introducing affordable housing within geographical proximity of a school. Geographic proximity, does not predict where affordable housing residents will attend school. For one, new residents to the neighborhood may try to have their students continue attending their previous school if possible. For another, families in Florida have significant choice in school, either through charter school programs or, in some districts, choice of any public school in the district.

Third, further analysis should utilize more data to investigate how various socioeconomic indicators at the neighborhood level influence the impact of affordable housing on a broader set of school quality measures. Free and Reduced Price Lunch program eligibility served as an able proxy for socioeconomic status in this analysis, but policy makers and proponents and opponents of affordable housing may have more interest in neighborhood or census block data of life surrounding the school. However, given the already strong and statistically significant coefficient on the Free and Reduced Price Lunch variable, and the values of R-squared above 0.5 in all of the models estimated above, new socioeconomic variables may not add much to the analysis.

Fourthly, the analysis only examines the impact of one new affordable housing unit. Many schools faced affordable housing in continuous years, observations which had to be
discarded because of the nature of the event study model. Therefore the results speak to the impact of new affordable housing units introduced in a specific year, but not to the effects of cumulatively adding more and more affordable housing to a neighborhood.

Lastly, I determine causality by utilizing the average scores of the neighbors as a form of matched control. The literature on event studies questions is still in the process of developing methods to enhance the consistency of estimates in long-horizon event studies, but currently champions matching as one such way to improve the study. A more sophisticated analysis may incorporate a more detailed form of matching beyond geographical proximity, such as propensity score matching. Such a characteristic-based matching approach is a standard approach in the event study literature to minimize the misspecification of expected outcomes (Khotari and Warner, 2004). Propensity-score matching would require a much broader swathe of covariates than I had available upon which to match treated schools with control schools, and matches schools of a similar likelihood to be treated given their characteristics.

However, using geographical proximity has its own benefits. Resting on the assumption that geographically nearby schools are equally likely to receive affordable housing when their disparate rates of Free and Reduced Price Lunch program eligibility are controlled for, geographic proximity may control for inherently unobservable variables such as regional characteristics which characteristic-based propensity score matching may not. After all, as seen in the coefficients on Neighbors’ Average score in both models in Table 4 and Table 5, the matching process of choice in this analysis already does a fairly good job of predicting the average scores of treated individuals in all but the tests on 10th grade reading and 8th grade math. In the end, the largest limitation of this approach is that in using Free and Reduced Price Lunch as a control in the matching process and analysis more generally, I cannot capture any impact
affordable housing may have had on school test scores via potentially altering the rates of Free and Reduced Price Lunch eligibility rates in the school.

In sum, the model in its present form suffers from a number of data limitations and can only offer narrow interpretations of the impact of affordable housing on the standardized test scores of nearby schools. However, as regards the possible limitations resulting from the model’s design, the analysis in its present form has drawbacks as well as positives.

Conclusion

In conclusion, schools who receive affordable housing within a two mile radius do not by any measure witness a large change in their standardized test scores. In general, however, as the size of the affordable housing increases relative to the size of the school, scores trend ever so slightly downward in elementary school reading. Upon further examination of the general case, I find that middle schools with lower rates of Free and Reduced Price Lunch eligibility (a proxy for socioeconomic status) actually receive a slightly positive increase in average standardized test scores even as the size of the affordable housing increases. As the Free and Reduced Price Lunch eligibility rate increases the impact grows smaller and eventually negative. Wealthy opponents of affordable housing have no basis to fear that the nearby introduction of affordable housing will decrease the quality of their public schools. If anything, policymakers considering affordable housing should actively pursue placing new properties in the proximity of wealthier schools.
Bibliography

California Planning Roundtable. 1993. Myths and facts about affordable housing and high density housing. Sacramento: California Department of Housing and Community Development.


The Campaign for Affordable Housing, http://www.tcah.org/pdf/Public_Attitudes.pdf


Appendix

Figure 3

Difference Over Time Between Schools with Low and High FRPL Participation
4th Grade Standardized Reading Scores

Year

Standardized 4th Grade Reading Score

Above 75%  Below 25%