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Promise Scholarship Programs as Place-Making Policy: Evidence from School Enrollment and Housing Prices

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ABSTRACT

Following the example of the Kalamazoo Promise initiated in 2005, place-based "Promise" scholarship programs have proliferated over the past 8 years. These programs guarantee money towards the costs of attendance at selected colleges and universities provided that a student has resided and attended school within a particular public school district continuously for at least the four years prior to graduation. While some early programs have been studied in isolation, the impact of such programs in general is not well understood. In addition, although there is substantial and controversial variation from the original program's design, there is no direct evidence on how outcomes vary along with these design choices. We use a difference-in-difference approach to compare the evolution of both school enrollments and residential real estate prices around the announcement of these programs within the affected Promise zone and in the surrounding area. Taken together, our estimates suggest that these scholarships have important distributional effects that bear further examination. In particular, while estimates indicate that public school enrollments increase in Promise zones relative to their surrounding areas following Promise announcements, schools associated with merit-based programs experience increases in white enrollment and decreases in non-white enrollment. Furthermore, housing price effects are larger in neighborhoods with high quality schools and in the upper half of the housing price distribution, suggesting higher valuation by high-income households. These patterns lead us to conclude that such scholarships are primarily affecting the behavior of households living above the median income for whom they present the greatest value and that merit-based versions disproportionately impact white households.

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1. Introduction

In late 2005, the Kalamazoo Public School District announced a novel scholarship program. Generously funded by anonymous donors, the Kalamazoo Promise offers up to four years of tuition and mandatory fees to all high school graduates from the Kalamazoo Public Schools, provided that they both resided within the school district boundaries and attended public school continuously since at least 9th grade. The Kalamazoo Promise is intended to be a catalyst for development in a flagging region, encouraging human capital investment and offering incentives for households to remain in or relocate to the area. In the first eight years of the Kalamazoo Promise, research has documented a number of encouraging results, including increased public school enrollment, increased academic achievement, reductions in behavioral issues, and increased rates of post-secondary attendance.¹

Encouraged by these early returns, many organizations have implemented similar programs modeled after the Kalamazoo Promise in urban school districts across the U.S. Still, most programs do not adhere exactly to the Kalamazoo archetype. Each iteration of the place-based “Promise” model varies in its features, including the restrictiveness of eligibility requirements, the list of eligible colleges and universities, and the generosity of the scholarship award itself. While research has been conducted on the Kalamazoo program and its impact on various outcomes of interest, this extant work only describes one particular intervention. As a result, we still know very little about the impact that such programs have on their communities. With hundreds of millions of dollars being invested in these human capital development initiatives, understanding their true impact is an important task for policy research.

This paper broadens the scope of our understanding of Promise programs by evaluating the impact of a broad cross-section of Promise programs on two targeted development outcomes: K-12 public school enrollment and home prices. In addition to providing the first estimates of the impacts over a set of multiple Promise programs, we document the

¹See [Bartik et al. \(2010\)](#); [Bartik and Lachowska \(2012\)](#); [Miller-Adams and Timmeney \(2013\)](#); [Miron et al. \(2011\)](#); [Miller \(2010\)](#); [Andrews et al. \(2010\)](#); [Miller-Adams \(2009, 2006\)](#); [Miron and Evergreen \(2008a,b\)](#); [Miron et al. \(2008\)](#); [Miron and Cullen \(2008\)](#); [Jones et al. \(2008\)](#); [Miron et al. \(2009\)](#); [Tornquist et al. \(2010\)](#) for some evaluations of the impact of the Kalamazoo Promise.

significant heterogeneity of these effects across different constellations of program features. While the effect of regional policy on both public school populations and housing markets is of interest itself, including housing markets in the analysis allows us to speak to the valuation of this program across different groups by examining the variation in the capitalization effects across different neighborhoods and across the housing price distribution. Such patterns have important implications for the distribution of economic benefits from Promise programs.

First, we find that, on average, the announcement of a Promise program in a school district increases total public school enrollment. When analyzed by grade level, announcement leads to immediate increases in enrollment in primary schools (K-4) in particular. Since it is common in Promise programs to offer escalating benefits for students beginning their continuous enrollment at earlier grade levels, this pattern lends credence to a causal interpretation of our results. Dividing programs along prominent differences in design, we find that programs which offer scholarships usable at a wide range of schools provide the largest immediate boosts in total enrollment. In addition, some features of Promise programs have significant effects on the composition of affected schools. We find that merit requirements have differential effects across white and non-white enrollment decisions, leading to large increases in white enrollment and decreases in non-white enrollment, potentially exacerbating existing racial inequality in educational attainment.

In addition, within 3 years of the announcement of a Promise program residential properties within selected Promise zones experiences a 7% to 12% increase *on average* in housing prices relative to the region immediately surrounding the Promise zone, reflecting capitalization of the scholarship into housing prices.² This increase in real estate prices is primarily due to increases in the upper half of the distribution. These results suggest that the value of Promise scholarship programs is greater for higher-income families while simultaneously suggesting that the welfare effects across the distribution are ambiguous. While higher-income households seem to place a higher value on access to these scholarships, they also appear to be paying a higher premium for housing as a result.

Finally, for two Promise programs located in major metropolitan areas— Pittsburgh

²Housing market data were not available for all Promise program locations. A sample of 8 Promise programs were utilized in this analysis.

and Denver— we observe sufficient housing market transactions over the relevant time period to analyze the heterogeneity of housing market effects across schools within the Promise-eligible school districts. After linking housing transactions data to school attendance boundaries, we compare capitalization effects across the distribution of school quality within each city. Appreciation in housing prices is concentrated in Pittsburgh and Denver neighborhoods that feed into high quality schools (as measured by state standardized test scores). Since the previous evidence suggests that the increased demand is driven by high-income households, it is natural that it should be focused on areas with already high-achieving schools. However, this could have the effect of contributing to further inequality in educational outcomes if the high-income households attracted by Promise programs are exclusively attending already high-quality schools.

These results should guide those looking to establish new Promise programs or to tailor existing Promise programs. While place-based scholarships certainly can impact regional development, the basic features of the scholarship matter. Allowing students to use scholarships at a wide range of schools seems to be of first-order importance for total enrollment, with more flexible scholarships generating larger increases in total enrollment. The decision to impose merit requirements has important compositional effects on affected schools, leading to larger relative increases of white students in schools with merit-based programs. When combined with the distribution of capitalization effects, the evidence clearly suggests that Promise scholarships are having the largest impact on households in the middle- and upper-class. It is possible, however, that the change in peer composition and the increased tax base that result from increased demand amongst high-income, white households may have significant spillover effects on low-income and minority students in Promise districts. More research is needed to pin down the relative importance of these effects.

The following section will describe the relevant literature as well as the general structure of the Promise programs being analyzed, including the dimensions along which they vary. Section 3 will describe the data and the empirical methodology that will be used to estimate the impact of the program on public school enrollment and housing prices. Section 4 will be divided into three subsections, the first of which will present the results of the enrollment analysis on the entire sample of Promise programs. The remainder

of section 4 will be devoted to housing market analysis, first using a pooled sample of local housing markets in the second subsection and subsequently focusing on two of the larger urban areas in the final subsection. Finally, section 5 will discuss the results and conclude.

2. Background

In addition to their policy ramifications, our findings contribute to two different strands of literature. First is the already substantial body of work regarding the provision of financial aid. There is a large literature addressing the impact of financial aid on postsecondary educational attainment.³ Surveying contributions too numerous to cite individually, [Dynarski \(2002\)](#) reviews the recent quasi-experimental literature on the topic and concludes that financial aid significantly increases the likelihood that an individual attends college. Her estimates indicate that lowering the costs of college attendance by \$1,000 increases attendance by roughly 4 percentage points. She further concludes that the distributional implications of aid are ambiguous. Estimates of the relationship between the impact of aid and income are evenly divided, with half indicating that the impact of aid rises with income. The studies she surveys focus exclusively on how financial aid affects the college attendance decision and choice of college. While our contribution will not address this question directly, we nevertheless provide important results on a recent development in the financial aid landscape. In particular, the implementation of Promise programs may either contribute to or mitigate inequality in educational attainment across racial groups, depending on the program design. We provide preliminary and indirect evidence that merit-based Promise scholarships in particular may favor white students in the distribution of benefits. In addition, our capitalization results suggest that high-income households are willing to pay more for access to Promise scholarships, although the true incidence of the subsidy remains unclear due to the effects of housing price capitalization.

The second strand of literature to which we contribute concerns research into place-based policies. Recently reviewed by [Gottlieb and Glaeser \(2008\)](#), these studies focus on

³See [Leslie and Brinkman \(1988\)](#) for a review of early studies.

outcomes such as regional employment, wages, population, and housing markets. The authors demonstrate significant agglomeration effects on these outcomes, suggesting the potential for policies aimed at redistributing population across space to have aggregate welfare implications. The caveat is that if agglomeration elasticity is constant across locations, redistribution can not have any overall effect. Any place-based policy aiming to capitalize on agglomeration externalities must rely on nonlinearities in the externality, otherwise the gains from population increases in one place will simply be offset by the loss of population in another. Indeed, the research on specific place-based interventions such as the Appalachian Regional Commission, Enterprise and Empowerment Zones, the Model Cities program, and urban renewal spending yield primarily negative results. The authors withhold comment on whether these projects were simply underfunded or such policies are ineffective in general, but the picture painted is not optimistic for the efficacy of such programs. Contributing further to this pessimism are [Kline and Moretti \(2011\)](#), who examine one of the more ambitious place-based policies in U.S. history: the Tennessee Valley Authority (TVA). The authors show that the TVA led to large, persistent gains in manufacturing employment which led to welfare gains through long term improvements manufacturing productivity. However, the productivity gains were exclusively the result of huge infrastructure investments; the indirect agglomeration effects of the policy were negligible. The central message is that, while large place-based interventions can bolster one locality at the expense of another, any gains will evaporate with the termination of the policy and persistent net welfare gains are rare. We find that place-based Promise scholarship programs do in fact increase public school populations and housing prices, which is plausibly explained by the scholarship increasing the willingness to pay for housing in these areas. The existing literature suggests that these effects would evaporate upon the withdrawal of the scholarship program from the area, unless the Promise intervention is to human capital what a program like the TVA is to physical capital. In that case, the direct productivity effects of Promise scholarships may have lasting effects, although the indirect agglomeration effects on productivity are likely to be minimal.

The overlap of financial aid and place-based policy did not begin with the Kalamazoo Promise, but until recently place-based financial aid had been the domain of state education agencies. The Georgia HOPE scholarship has been in place since 1993, awarding

scholarships to Georgia high school graduates who satisfy GPA requirements and enroll at a Georgia college or university. Like the Kalamazoo Promise, many states used the HOPE scholarship as a model when introducing statewide merit-based scholarships of their own. Several studies have thoroughly examined the impact of the HOPE scholarship program on outcomes such as student performance in high school (Henry and Rubenstein, 2002), college enrollment (Dynarski, 2000; Cornwell et al., 2006), college persistence (Henry et al., 2004), and degree completion (Dynarski, 2008). To summarize the findings, the HOPE scholarship has led to overall improvements in K-12 education in Georgia as well as reductions in racial disparities. In addition, college enrollments increased among middle- and high-income students, but income inequality in college enrollments widened and college persistence was not necessarily increased. While evaluating place-based policies, it is notable that most of the research on these programs has focused on the outcomes typically associated with the financial aid literature— i.e. impact on college attendance, degree completion, and the impact of merit scholarships on educational inequality. Because of the statewide nature of these programs, outcomes on a smaller spatial scale that would interest place-based policy researchers— i.e. impact on regional development outcomes, population, public school enrollments, and housing markets— have been largely ignored.

The unexpected introduction of place-based Promise scholarship programs in school districts across the U.S. provides a series of natural experiments similar to those provided by statewide scholarships. However, the smaller geographic scale allows us to study local outcomes for the first time, using the immediate geographic vicinity of a Promise school district as a plausible counterfactual. With an ever-expanding sample of Promise programs implemented at different times in different regions, we can now assess the impact of providing place-based scholarships on a number of relevant but hitherto ignored outcomes, as well as how these impacts vary with the design of the program.

2.1. Promise Scholarship Programs

According to the W.E. Upjohn Institute for Employment Research, a Promise-type scholarship program is a “universal or near-universal, place-based scholarship program.” Upjohn has identified a list of 23 such programs (plus the Kalamazoo Promise itself). These programs are listed in Table 1 along with some other details of the programs

themselves.^{4,5}

[Table 1 about here]

In practice, the place-based nature of these scholarships is dictated by the requirement that a student maintain continuous enrollment in a particular school district (or other small collection of schools) for several years prior to graduation to receive any benefit.⁶ Although the continuous enrollment requirement alone constitutes a restriction on residential location for most U.S. households, many programs pair this with an explicit requirement for continuous residence in the district itself.

Although the Kalamazoo Promise was universal within its Promise zone as can be seen in Table 1, many Promise programs have other eligibility requirements. Minimum GPA requirements, minimum attendance requirements, and community service requirements are common. Previous work has called attention to the variation in eligibility requirements as an important element in program design, but to date no research has empirically investigated the impact of universal vs. merit-based eligibility on program effectiveness in the context of Promise programs. Miller-Adams (2011) documents the successes of the Kalamazoo Program and attributes some results to its universal eligibility. In particular, the Kalamazoo Public Schools experienced increases in enrollment without significant changes in the ethnic, racial, or socioeconomic composition of its schools. This pattern is attributed to the universality of the Kalamazoo Promise. Without an accompanying analysis of near-universal programs, however, it is unclear whether similar results could be obtained from very different interventions. In addition, some districts' goals may include modifying the demographic composition of area schools. For example, Schwartz (2010) indicates that relocating disadvantaged children to low-poverty schools has large and lasting effects on their educational achievement. The analysis to date provides dis-

⁴The majority of the list of Promise-type scholarship programs was obtained from <http://www.upjohninst.org/Research/SpecialTopics/KalamazooPromise>. Further research revealed an additional Promise program in Buffalo, NY, which has been added to the list. All other information is based on a review of each program's website.

⁵Of the programs detailed in Table 1, a number are excluded for data availability or other reasons. Of particular interest is the intervention located in Detroit, MI which is excluded from the analysis because the precipitous decline of Detroit in the years surrounding the Promise is likely to overshadow the relatively insignificant intervention, as discussed in detail in the following section.

⁶While not always defined in terms of school districts, we will use the terms "Promise district", "Promise area", and "Promise zone" interchangeably to refer to the geographical boundaries of a Promise program.

districts looking to capitalize on such effects with no guidance regarding what program design choices best suit their goals.

[Bangs et al. \(2011\)](#) review existing research on the effects of merit and universal place-based scholarship programs on K-12 enrollment, student achievement, college attainment, and inequality. Relative to merit aid, the universal scholarships they study are more effective at increasing school district enrollment and reducing poverty and racial disparities in educational attainment. However, the authors include only the Kalamazoo Promise and the Pittsburgh Promise from the class of Promise programs. In addition, direct evidence of the impact of the Pittsburgh Promise is scant; most comparisons are made between Kalamazoo and statewide programs such as the Georgia HOPE scholarship. Using data from over 20 Promise-type programs announced to date, many of which include a merit eligibility requirement, we present direct evidence on the contrast between merit-based and universal programs, specifically in the context of place-based Promise scholarship programs.

Eligibility requirements are scarcely the only source of heterogeneity in program design; the scholarship award itself varies across programs. By way of example, the maximum award for the Jackson Legacy scholarship is \$600 per year for two years, whereas the Pittsburgh Promise recently increased their maximum scholarship award from \$5,000 to \$10,000 per year for up to four years. The maximum scholarship duration varies as well from one year (Ventura College Promise) to five years (El Dorado Promise and Denver Scholarship Foundation). However, the exact degree of variation in benefits is obfuscated by two common features of Promise scholarships. First, scholarships are often stated in percentage terms of tuition, which makes the value dependent on the choice of postsecondary institution. Second, many Promise programs award benefits on a sliding scale based on the grade at which the student first enrolled in a Promise zone school. As an example of both, the Kalamazoo Promise benefit ranges from 65% (enrolled grades 9-12) to 100% (enrolled grades K-12) of tuition and mandatory fees at a Michigan public college or university. As a result, the expected benefit of a Promise scholarship varies across locations in a way that is difficult to quantify, but is nevertheless significant.

The last major feature we will address is the list of colleges and universities towards which the scholarship applies. Most programs require enrollment at an accredited post-

secondary institution located within the same state as the Promise zone. Some limit that further to public institutions, while many scholarships are only usable at a short list of local colleges. This aspect of the program has a substantial impact on both the value of the scholarship in absolute terms and the distribution of its benefits across groups. For instance, some programs have flexible scholarships that allow use at a large list of institutions including trade schools as well as nationally-ranked four-year universities. Naturally, scholarships that allow use at more expensive schools are potentially more valuable to their recipients. In addition, the variation in price points and selectivity within the list of eligible schools makes the scholarship valuable to both low-income and high-income households alike. Programs with inflexible scholarships typically allow use only at local junior and community colleges. This restriction not only caps the benefit of the scholarship to full tuition at one particular school, but also presents less value to high-income graduates focused on four-year programs.

As the oldest program in its class, a considerable amount of research has evaluated the impact of the Kalamazoo Promise on the outcomes of students in the Kalamazoo Public School District.⁷ A series of working papers from Western Michigan University’s Department of Education outline the mechanism for community development in principle, with the Promise generating increased attendance in secondary school leading to better classroom performance and graduation rates and ultimately increased college attendance in the region. Their research to date culminated in [Miron et al. \(2011\)](#) which presents quantitative and qualitative evidence documenting a significant improvement in school climate following the announcement of the Promise.⁸ In addition, the W.E. Upjohn Institute for Employment Research has taken a leading role in research surrounding the Kalamazoo Promise. Researchers there have determined that the Kalamazoo Promise has successfully increased enrollment ([Hershbein, 2013](#); [Bartik et al., 2010](#)), improved academic achievement ([Bartik and Lachowska, 2012](#)), and increased college attendance in

⁷We have found sources that indicate Pinal County’s “Promise for the Future” program started as early as 2001. It is perhaps more accurate to say that the Kalamazoo Promise is the oldest widely-recognized program in this class.

⁸See [Miron and Evergreen \(2008a\)](#), [Miron and Evergreen \(2008b\)](#), [Miron et al. \(2008\)](#), [Miron and Cullen \(2008\)](#), [Jones et al. \(2008\)](#), [Miron et al. \(2009\)](#), and [Tornquist et al. \(2010\)](#) for more evidence from their evaluation of the Kalamazoo Promise program.

certain groups ([Miller-Adams and Timmeney, 2013](#)). Finally, [Miller \(2010\)](#) confirms the documented positive effects on public school enrollment, achievement, and behavioral issues. She also adds a preliminary analysis of home values, finding that the announcement of the Promise had no impact on home prices in Kalamazoo relative to the surrounding area.

Apart from these studies of the Kalamazoo Promise, however, little research has been conducted on Promise programs in order to generalize the findings. [Gonzalez et al. \(2011\)](#) study the early progress of Pittsburgh’s Promise program and find that it stabilized the previously declining public school enrollment in the Pittsburgh public schools. The study also presents survey-based and qualitative evidence that the Pittsburgh Promise’s merit-based eligibility requirements motivate students to achieve and that the Promise was influential in the decisions of many parents to move their children to city public schools. Additionally, some programs’ websites present internal research intended to promote the program’s progress. Importantly, all studies to date have been limited in scope to an individual Promise location. Also, with the exception of some work regarding Kalamazoo, the research has been primarily qualitative or descriptive in nature. In the remainder of the paper, we will present the first research which utilizes data from a broad array of Promise-type programs. We present direct evidence on the effectiveness of Promise scholarships in increasing public school enrollments, as well as document patterns in enrollment across different programs which are clearly related to program details such as eligibility requirements and award amounts. In addition, we present the first analysis confirming the influence of Promise scholarship programs on property values, the results of which also have interesting implications for future program design.

3. Data and Methodology

Our estimation strategy for measuring the impact of the Promise hinges on treating the announcement of a Promise program in a region as a natural experiment, relying on the assumption that the announcement in each area was unexpected. To justify this assumption, we conducted substantial research into the timing of program announcements in each area that we study. The date of the announcement that we use in our analysis corresponds to the earliest mention we could find of the program’s existence. Typically,

this corresponds to the date of a press release announcing the program. In cases where press releases were unavailable, we used the Internet Archive at <http://www.archive.org> to find the earliest iteration of the program’s own home page, using the archival date as the announcement date. We were able to determine the approximate date of announcement for 22 of the 25 known programs in Table 1; the remaining 3 were excluded from the analysis.⁹ It is likely that some of our announcement dates will be subject to measurement error. This problem is mitigated somewhat in the public school analysis, as enrollment data evaluated on an annual basis. In addition, any bias resulting from measurement error should serve to attenuate our estimates of the true effect of these programs.

In addition to those programs mentioned above, the Detroit College Promise was also excluded from the analysis. The reasons for this exclusion are two-fold. First, the intervention in Detroit was very small. The maximum scholarship attainable under the Detroit Promise is \$500 per year, and that only for the initial two cohorts of graduates from a particular high school; most other students are entitled to a maximum award of \$500 total.¹⁰ This small award is due to the lack of sponsorship for the Detroit Promise; as of June 13, 2013, there was only one donor to the Detroit Promise that contributed over \$50,000. Contrasted with the 35 such donors to the Pittsburgh Promise, it is obvious why the Detroit Promise is not capable of offering larger scholarships to its graduates. Second, we believe the precipitous decline of a city on the verge of bankruptcy is likely to overshadow any small positive impact on house prices that may have been generated by the Detroit Promise. In the year following the announcement of the Detroit Promise, two of the so-called “Big 3” automakers based in and around Detroit filed for bankruptcy, followed by the city itself filing for bankruptcy in 2013. From 2000 to 2010, Detroit experienced a 25% decline in population—the largest percentage decrease in population for a U.S. city aside from the exodus out of New Orleans after Hurricane Katrina in 2005. Because of these non-Promise related factors, we believe Detroit to be non-representative

⁹The excluded programs were the Educate and Grow Scholarship (Blountville, TN), the Muskegon Opportunity Scholarship (Muskegon, MI), and School Counts! (Hopkins County, KY).

¹⁰The exception to this is the graduating class of 2013, who it was recently announced will receive \$600 scholarships from the Detroit Promise.

of the typical Promise program and we exclude it from all results below.

There are two main outcomes that we will be interested in studying in relation to Promise Scholarship programs: K-12 public school enrollments and housing prices. Naturally, identifying and estimating the impact of the Promise presents a unique set of empirical challenges for each outcome of interest. We will first present a description of the data and empirical strategy used to analyze the impact of Promise programs on K-12 enrollment, followed by a similar section devoted to the data and methodological concerns related to our housing market analysis.

3.1. Public School Enrollment

Our data source for public school enrollments is the National Center for Education Statistics' Common Core of Data (CCD). The CCD surveys the universe of public schools in the United States every year. Among the data collected in the survey are the names and locations of all schools, the operational status code as of the survey year, the instructional level of the school (primary, middle, high), student enrollment counts by grade and by race/ethnicity, and staff counts. As all Promise programs were announced after the year 2000, we retrieved CCD records dating from the 1999-2000 survey year up to the most recently available 2010-2011 survey year.¹¹ This yielded a total of 1.2 million school-year observations. This data was then combined with information on which schools' students were eligible for Promise scholarships and the year the programs were announced.

Ultimately, the goal is to estimate the change in enrollments resulting from the announcement of the 21 Promise programs observed. For causal inference, however, it is not sufficient to compare student counts in Promise districts prior to the announcement with student counts after the announcement. We require an appropriate counterfactual to account for the possibility that similar (or proximate) schools unaffected by the Promise may have also experienced increases or decreases in enrollment as a result of some unobserved common shock. The interpretation of an increase in Promise school enrollment

¹¹Five programs— Say Yes Buffalo (Buffalo, NY), the Sparkman Promise (Sparkman, AR), the Arkadelphia Promise (Arkadelphia, AR), the New Haven Promise (New Haven, CT), and the Great River Promise (Phillips and Arkansas Counties, AR)— were announced recently enough that no post-announcement data is yet available. However, the pre-announcement data for these Promise Zones and their surrounding areas is included in our analysis to help estimate nuisance parameters more precisely. Importantly, the exclusion of these observations does not qualitatively change our estimates.

counts changes substantially if similar but unaffected schools experienced increases just as large, for example. As such, we use a difference-in-differences approach to identify the causal impact of Promise program announcement. We estimate variations of the following fixed-effects regression

$$Y_{it} = \alpha + \beta Post_{it} \cdot Promise_i + \mathbf{X}'_{it} \cdot \gamma + \eta_{it} + \delta_i + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the natural log of enrollment in school i in year t , $Post_{it}$ is an indicator for surveys occurring after the announcement of the Promise program relevant to school i , $Promise_i$ is an indicator for schools located in Promise zones, \mathbf{X}_{it} is a vector of characteristics school i in year t , η_{it} is a vector of region-by-year and urbanicity-by-year fixed effects, and δ_i are school fixed effects. Standard errors in all specifications are clustered at the school level to allow for correlation in ε_{it} within schools over time.

In addition, some results will be presented that modify equation 1 as follows

$$Y_{it} = \alpha + \sum_{J \in \{M, NM\}} \sum_{K \in \{W, NW\}} \beta_{JK} Post_{it} \cdot J_i \cdot K_i + \mathbf{X}'_{it} \cdot \gamma + \eta_{it} + \delta_i + \varepsilon_{it} \quad (2)$$

yielding four coefficients— β_{MW} , β_{NMW} , β_{MNW} , and β_{NMNW} — where M_i indicates a Promise program with a merit-based eligibility requirement, NM_i indicates a universal Promise program, W_i indicates a Promise program with a broad (more than three) list of eligible postsecondary institutions, and NW_i indicates a Promise program with a narrow (no more than three) list of eligible postsecondary institutions. This specification allows us to answer questions regarding how the impact of Promise programs varies along prominent design dimensions.

The coefficients of interest in the above equation estimate the impact of Promise announcement on school outcomes— or average treatment effect— provided that the chosen control schools act as an appropriate counterfactual for the evolution of K-12 enrollment in the absence of treatment. Our estimation strategy will use geographically proximate schools as our control group for schools located in Promise zones. As a result, we limit our attention to schools that were located in the county or counties surrounding the treated schools. The intuition for this control group is that schools in the same county or neighboring counties will be affected by the same regional shocks to K-12 enrollment

as their treated counterparts, such as broad regional migration or demographic patterns. In addition, we only include surveys conducted within 4 years of the announcement date of the Promise program relevant to the school in question. Finally, we only include observations from schools which reported total student counts and student counts by race/ethnicity in every available survey within the estimation window.¹² This restriction results in our baseline estimation sample of 47,600 school-year observations across 74 U.S. counties and 947 school districts. Table 2 presents the summary statistics for the sample of treated and untreated schools across all years in the sample.

[Table 2 about here]

The schools initiating Promise scholarship programs are statistically different from those in the geographically proximate control group. Schools in Promise zones have fewer students overall and fewer white students as a fraction of the total students (although this difference, while statistically significant, is very small). In addition, the Promise schools are much more likely to be located in urban areas, naturally making the nearby schools in the control group much more likely to be in suburban areas. Differences in the distribution of schools across levels are very similar, although the more urban Promise districts tend to have fewer schools designated as middle schools in the CCD.

Bear in mind, our empirical strategy does not explicitly rely on Promise schools being similar to comparison schools. Provided that Promise schools and non-Promise schools are not becoming more or less dissimilar over the period prior to the Promise announcement, our estimates should identify the causal impact of the Promise announcement. Specifically, identification of the causal effect of the Promise announcement requires that the outcomes of interest would follow parallel trends (conditional on observable covariates) in the absence of any intervention, such that any difference in the period following announcement can be attributed to the treatment itself. Importantly, this assumption can not be explicitly tested as we do not observe the true counterfactual. In the next section, however, we will present graphical evidence in support of this assumption. Specifically, we will demonstrate that the evolution of enrollment in the periods immediately prior to Promise announcement was similar between Promise zone schools and control

¹²Relaxing this restriction only slightly changes the estimated coefficients.

schools. This requirement also implicitly assumes that no other major changes are occurring in one group and not the other at approximately the same time as the treatment is occurring. While we can not rule this out, due to the time variation in the announcements of the geographically diverse set of programs it is unlikely that any shock other than the Promise program announcement would have occurred in all Promise zones at the time of announcements, especially a shock that would differentially impact Promise zones relative to their immediate surroundings.

3.2. Housing Prices

Our housing price data comes primarily from DataQuick Information Systems, under a license agreement with the vendor. These data contain transactions histories and characteristics for properties in a large number of U.S. counties. Included in the data collected are sales of newly constructed homes, re-sales, mortgage refinances and other equity transactions, timeshare sales, and subdivision sales. The transaction related data includes the date of the transfer, nominal price of the sale, and whether or not the transaction was arms-length. In addition, every building in the data has characteristics as recorded from the property's most recent tax assessment. These variables include floor area, year built, number of bedrooms, number of bathrooms, and lot size.¹³ Finally, the latitude and longitude of each property is also included.

The location of the property is crucial to the analysis. Locating the property within a Census tract allows us to combine property characteristics with neighborhood demographic data from the U.S. Census and also allows us to control for unobserved neighborhood characteristics through the use of fixed effects. We require a fixed geographical definition of a neighborhood for the latter, but Census tract definitions change over time. Fortunately, the Longitudinal Tract Database (LTDB) has developed tools to estimate any tract-level data from the 1970 onward for 2010 Census tract definitions. So, properties were allocated to 2010 Census tracts and historical neighborhood demographic data was

¹³Note that not all variables are reliably recorded across all jurisdictions. Most jurisdictions reliably record floor area and year built, but other details are often unreliably encoded (i.e. missing values, unrealistic quantities, no variation in codes, etc.). As a result, any analysis that pools data from all markets only includes floor area (in square feet) and a quartic in building age in specifications where structural characteristics are included. These characteristics were the only variables that were reliably recorded across all jurisdictions studied.

estimated based on these tools, interpolating between years when necessary. These demographic data include median income, racial composition, age distribution, educational attainment, unemployment rates, fraction in poverty, fraction of family households, and private school attendance. Also, geographical data allows us to match properties to school districts, counties, or Census places using U.S. Census TIGER files. As Promise eligibility is ultimately determined by location within these boundaries, this is crucial for determining which properties are eligible to receive Promise scholarships.

Unfortunately, not all counties that are home to Promise programs are covered by DataQuick. As a result, the housing market analysis necessarily focuses on a subset of eight Promise zones due to data limitations.¹⁴

As with demand for public schools, there is reason to believe that the announcement of a Promise program will increase demand for housing within the Promise zone. However, unlike with K-12 enrollment data, housing market data gives us an indication of the value of the announcement of the Promise to households. Since we observe the transaction price associated with the residential location decision, we can draw inference on the household's willingness to pay for access to the program. Assuming that housing supply is fixed in the short-run, any increase in the average household's willingness to pay must be capitalized into prices. As a result, by identifying the change in housing prices attributable to the announcement of a Promise program, we will recover the capitalization of program announcement into housing prices, providing a signal of the average household's marginal willingness to pay for access to the program.¹⁵

¹⁴For only six of these does the data originate from DataQuick. For two Promise programs— Say Yes Syracuse (Onondaga County, NY) and the Kalamazoo Promise (Kalamazoo County, MI)— real estate transaction and assessment data was pulled from public records on the internet. For Onondaga County, parcel information and transaction histories were obtained from the Office of Real Property Services (ORPS) websites at <http://ocfintax.ongov.net/Imate/search.aspx> (for Onondaga County) and <http://ocfintax.ongov.net/ImateSyr/search.aspx> (for City of Syracuse). For Kalamazoo and neighboring Van Buren county, parcel information and transaction histories for each property were gathered from the BS&A Software portal for Kalamazoo and Van Buren Counties at <https://is.bsasoftware.com/bsa.is/>. In terms of the scope of content, the data acquired in this way is comparable to those supplied by DataQuick.

¹⁵Kuminoff and Pope (2009) demonstrate that capitalization is equivalent to marginal willingness to pay only if the hedonic price function is constant over time and with respect to the shock being analyzed or if the shock is uncorrelated with remaining housing attributes. Neither condition is likely to be satisfied here and consequently our estimates are not directly interpretable as marginal willingness to pay. However, we present results that identify capitalization from repeat sales data which has been shown in Monte Carlo experiments to drastically reduce so-called “capitalization bias” over pooled OLS

In practice, however, identifying the causal impact of a change in a local amenity like access to a Promise scholarship is not trivial. In this paper, we use the hedonic method to model a property’s price.¹⁶ In general, the hedonic method expresses the transaction price of a property as a function of the characteristics of that property. The implicit price of a characteristic is then recovered by estimating the hedonic price function via regression. In addition, [Parmeter and Pope \(2009\)](#) demonstrate how combining this technique with quasi-experimental methods allows the researcher to exploit temporal as well as cross-sectional variation in amenity levels. Recent studies have used quasi-experimental hedonic methods to recover the value of school quality ([Black, 1999](#); [Barrow and Rouse, 2004](#); [Figlio and Lucas, 2004](#)), air quality ([Chay and Greenstone, 2005](#)), airport noise ([Pope, 2008a](#)), toxic releases ([Bui and Mayer, 2003](#); [Gayer et al., 2000](#)), flood risk reduction ([Hallstrom and Smith, 2005](#); [Pope, 2008b](#)), crime reduction ([Linden and Rockoff, 2008](#); [Pope, 2008c](#)), and mortgage foreclosures ([Cui and Walsh, 2013](#)). We adopt this technique as well in our estimation of the causal impact of Promise programs on housing prices.

As above, our estimation strategy will employ a difference-in-differences approach to identify the causal impact of Promise program announcement, which is fairly standard in the quasi-experimental hedonic valuation literature. Our baseline estimating equation is written as follows:

$$Price_{imdt} = \alpha + \beta Post_{mt} \cdot Promise_d + \mathbf{X}'_{it} \cdot \gamma + \eta_{mt} + \delta_d + \varepsilon_{imdt}, \quad (3)$$

where $Price_{imdt}$ is the natural log of the transaction price for property i in market m and school district d at time t , $Post_{mt}$ is an indicator for transactions occurring after the announcement of the Promise program relevant to housing market d , $Promise_d$ is an indicator for properties located in Promise zones, \mathbf{X}_{it} is a vector of building and neighborhood characteristics of property i at time t , η_{mt} are market-by-year-by-quarter fixed effects, and δ_d are school district fixed effects. Market-by-year-by-quarter fixed effects account for regional shocks in housing prices in a given period, while district fixed effects control for static differences between neighborhoods over time. We also

([Kuminoff et al., 2010](#)).

¹⁶For a thorough review of the hedonic method, [Bartik and Smith \(1987\)](#), [Taylor \(2003\)](#), and [Palmquist \(2005\)](#).

estimate variations on the above equation, where school district fixed effects are replaced by 2010 Census tract fixed effects and, finally, property fixed effects. The property fixed effects specifications yield our preferred estimates of the treatment effect, identifying the impact of treatment from repeat sales only and thus controlling for any time-invariant unobservables associated with an individual property. Standard errors in property fixed effects regressions are clustered at the property level to allow for correlation in ε_{imdt} for the same property over time; all other specifications cluster standard errors at the 2010 census tract level. Again, β identifies the impact of Promise announcement on housing prices provided that the prices of control properties would have evolved similarly over time in the absence of treatment.

For several reasons, we expect that the value of most Promise programs may increase with household income. [Light and Strayer \(2000\)](#) find that family income and mother’s education level increase both the likelihood of college attendance as well as the selectivity of the chosen school, thus making the Promise scholarship more valuable to higher-income, higher-educated households. In addition, many Promise scholarships are “middle-dollar” or “last-dollar” aid, ultimately applied towards unmet need at your institution of choice after the application of federal, state, and institutional aid. Importantly, while Promise aid is typically *not* need-based, these other sources of aid are typically dependent on the expected family contribution (EFC) as calculated by the household’s Free Application for Federal Student Aid (FAFSA) form, with lower income families expected to contribute less than higher income families. As a result, for an identical institution, higher income families are likely to receive less aid than lower income families from these other sources, leaving a larger amount of unmet need. For these reasons, the value of the Promise should be greatest for families with higher incomes. As it is reasonable to expect these higher income families to occupy higher priced domiciles, we would like to test this hypothesis by allowing the treatment effect to vary across the housing price distribution. As such, we perform a two-step procedure that first defines where properties lie on the *pre-Promise* distribution of housing prices— even for properties sold after the Promise— and subsequently estimates treatment effects both above and below the median of said distribution via OLS.

The first step is accomplished by restricting attention to the pre-Promise period in

each housing market and estimating a standard hedonic price function which includes all observable property-specific characteristics, i.e. structural and neighborhood features, and controls flexibly for time through quarterly fixed effects. The coefficient estimates from this regression are then used to predict the sale price of each property observed in the sample— including those sold after Promise announcement— as if it had been sold in the first quarter of the year prior to the announcement. The resulting number provides a measure of the component of housing value that is unaffected by the treatment by construction. All transactions are then sorted on this statistic and grouped into observations above and below the median. This exercise tells us where a property would have fallen in the housing price distribution for that particular housing market if the transaction had taken place prior to the announcement of the Promise.¹⁷

The second step simply repeats the DD analysis specified in equation 3, but separately for properties above and below the median of the distribution generated by the first step. Each β then estimates the treatment effect of the Promise announcement within each half of the housing price distribution.

It is worthwhile to briefly discuss the functional form assumption implicit in equation 3. The semi-log functional form, with the natural log of price as the dependent variable, is fairly standard in the hedonic literature and has been justified by Monte Carlo simulations performed initially by Cropper et al. (1988) and more recently by Kuminoff et al. (2010). However, we will also present estimates using a fully linear functional form with deflated transactions prices as the dependent variable. As all Promise scholarships are per-student subsidies and not a per-housing-unit subsidies, there is reason to suspect that the causal effect of the program is better interpreted in levels and not logs. For example, consider two identical families each with one child, one moving into a 2 bedroom house and one moving into a 10 bedroom house in the same neighborhood in a Promise zone. Both families will be willing to pay more for the house after the announcement of the Promise as their child will receive the scholarship with some positive probability. Yet, the expected

¹⁷As discussed below, in some specifications the estimation sample will be restricted either geographically or as a function of observable characteristics. A property's rank in this distribution is based on the widest definition of the housing market and will not depend on the estimation sample. As a result, the above and below median sample will not necessarily contain an equal number of observations when estimation samples are restricted in this way.

value of the benefit is the same even though the 10 bedroom house is undoubtedly priced higher than the 2 bedroom house. As such, we would not expect both families to be willing to pay the same *percentage premium* after the announcement of the Promise, which is what would be captured by a DD estimate in logs.

Another important consideration in any hedonic model is the spatial definition of the relevant housing market. The trade-off between using a large geographic housing market and a small geographic housing market is one between internal validity of the estimates and the precision with which they are estimated (Parmeter and Pope, 2009). As such, we take a flexible approach by estimating our equation on a number of different samples, each representing a different housing market definition.

After determining the geographic extent of each of the eight Promise programs, two estimation samples were constructed: one representing a relatively large housing market definition and one representing a small housing market definition. The large housing market is constructed by including all transactions within Promise zones as well as all transactions occurring within 10 miles of the geographic boundary of the Promise zone. The small sample is constructed by only using transactions within a 1 mile bandwidth along both sides of the Promise zone boundary. Figure 1 depicts an example, using the housing markets constructed around the Pittsburgh Promise treatment area.

[Figure 1 about here]

The large sample affords us many observations of market transactions and thus provides precise estimates. However, the concern in a large sample is that the estimate of the treatment effect will be biased if either the scholarship is not relevant to households in the periphery of the sample or they are simply unaware of the program. The small housing sample mitigates this bias by constructing a sample over which we can be relatively sure that all households will be informed of the scholarship and consider it relevant. The variance of the estimate, however, increases due to the smaller number of observations from which to draw inference. The goal in estimating our hedonic model on both samples is to evaluate the sensitivity of the measured treatment effect to the choice of housing market definition.

In addition to the two geographically defined markets, we also construct a housing market that, while bounded geographically, is defined in statistical terms. Even in the

small housing markets defined above, it is possible that properties on either side of the treatment boundary can vary significantly and discontinuously in terms of observable characteristics, calling into question their use as a counterfactual for houses within the treatment area. By means of example, figure 2 depicts the Promise zone in New Haven, CT (outlined in red) along with its corresponding large housing market (outlined in black). The area is subdivided into census tracts and color coded by racial composition according to the 2000 U.S. Census. As can plainly be seen, neighborhoods vary considerably across the border defining the Promise zone. While this difference in observables can be controlled for econometrically, it raises the question of variation in unobservables and, more importantly, the validity of the parallel trends assumption required for causal interpretation of DD estimates.

[Figure 2 about here]

In econometric terms, our concern is with limited overlap in observables between treatment and control groups which can cause “substantial bias, large variances, as well as considerable sensitivity to the exact specification of the treatment effect regression functions.” (Crump et al., 2009). As such, we would like to define a sample that reduces these concerns by trimming some observations in the non-overlapping region of the support, while simultaneously minimizing the variance inflation that accompanies the reduction in observations.

After pooling all large housing markets defined above, we follow Crump et al. (2009) to define what the authors refer to as the optimal subpopulation. We estimate the following logit model to predict the probability that a transaction occurs within a Promise zone based on pre-Promise property characteristics:

$$\text{Prob}(Promise_d | \mathbf{X}_i) = \frac{1}{1 + e^{\alpha + \mathbf{X}_i' \gamma}}, \quad (4)$$

where \mathbf{X}_i is a vector of time-invariant characteristics of property i including floor area (in sq. feet), a quadratic in building age, and available 2000 U.S. Census demographic information at the tract level.¹⁸ Recovering the associated parameters, we go on calculate

¹⁸As all Promise programs were announced after the year 2000, there is no endogeneity concern introduced by using Census demographics. Building age is similarly unaffected by endogeneity concerns as it is constructed as the difference between year built and year of transaction. Unfortunately, we do

the predicted value of $Promise_d$, obtaining propensity scores for all properties in the large housing market sample. We then trim the sample to observations with intermediate propensity scores.¹⁹ Equation 3 is then estimated on this sample, producing the Optimal Subpopulation Average Treatment Effect (OSATE).

Finally, we wanted to document any heterogeneity in capitalization effects across the distribution of school quality. It is well-known that the residential location decisions of households with children are heavily influenced by school quality. If the intention of these programs is in part to encourage the migration of households into Promise districts from nearby areas with higher quality schools, it stands to reason that increases in demand for housing should be concentrated in Promise area neighborhoods with access to relatively high quality schools. For two major metropolitan Promise zones—Pittsburgh and Denver—we were also able to obtain school attendance boundaries from the Minnesota Population Center’s School Attendance Boundary Information System (SABINS). After matching properties to schools and obtaining standardized test scores at the school level from each state’s education agency, we were able to generate standardized pre-Promise measures of primary school and high school quality for each property in the Pittsburgh and Denver samples. First, we divide the universe of schools on the basis of the highest tested grade level, with schools testing only 8th graders and lower being labeled primary schools and schools testing any students higher than 8th grade being labeled high schools. Then, we calculate the percentage of tested students scoring proficient or better on standardized tests (math and reading) in the universe of public schools in Colorado and Pennsylvania for the year 2005. Finally, within each state by school level cell we standardize this measure such that the resulting variable is a Z-score distributed with mean zero and unit standard deviation.

Pooling these two markets, we directly estimate how Promise capitalization varies with school quality by estimating variations of the following equation in each market

not observe variation in other building characteristics, so for each property we do not know whether we observe post-Promise floor area (which could potentially be endogenous to Promise announcement) or pre-Promise floor area (which would necessarily be exogenous to Promise announcement) of each property. However, over our short estimation window, it seems unlikely that floor area would respond to Promise announcement in any systematic or meaningful way.

¹⁹The optimal bounds of the propensity score distribution were calculated according to [Crump et al. \(2009\)](#). We thank Oscar Mitnik for sharing the code for the procedure on his website.

definition:

$$Price_{imdt} = \alpha + \beta Quality_i \cdot Promise_d \cdot Post_{dt} + \mathbf{X}'_{it} \cdot \gamma + \eta_t + \delta_d + \varepsilon_{imdt}, \quad (5)$$

where $Quality_{it}$ is one of four standardized pre-Promise measures school quality for property i — primary school math Z-score, primary school reading Z-score, high school math Z-score, or high school reading Z-score. The resulting estimate of β tells us how the capitalization effect of the Promise varies across neighborhoods with access to different quality schools.

For each selected housing market definition, we restrict our attention to transactions occurring within three calendar years of the program announcement date, yielding seven calendar years of transactions for each housing market. We limit transactions to arms length sales or resales of owner-occupied, single-family units. Houses with missing transaction prices, transaction dates, and spatial coordinates are dropped, as were houses with a building age of less than -1. Then, as the coverage and reliability of data varies significantly across jurisdictions, we eliminate outlying observations on a market by market basis. This process typically removed observations with unreasonable (i.e. floor area of 0 square feet) or extreme covariate values (i.e. floor area more than 5,000 square feet, more than 11 bedrooms, more than 10 bathrooms, etc.), taking care that the observations removed constituted a small percentage of observations (1% or less). Finally, we eliminate transactions occurring at prices less than \$1,000 or greater than \$5,000,000

Table 3 presents the summary statistics for the sample of treated and untreated properties for each housing market definition.

[Table 3 about here]

As with public school data, our housing market data reveals that the neighborhoods receiving Promise programs are different from those outside of Promise zones along several dimensions. Using a large housing market definition, the housing stock in Promise zones covered by our housing data smaller in size and typically older than that in the outlying areas. The Promise zones represented in the housing sample— Denver, CO; Kalamazoo, MI; New Haven, CT; Pittsburgh, PA; Peoria, IL; Syracuse, NY; Hammond, IN; and Pinal County, AZ— are mostly urban areas. The exceptions are Hammond and Pinal County, both of which lie very close to urban areas (Chicago and Phoenix, respec-

tively). As such, this could be an artifact of the availability of data through DataQuick, with rural areas being lower priority. This urban differential also reveals itself in the demographic characteristics; Promise neighborhoods typically contain more black residents, fewer children, and fewer college educated individuals. In addition, unemployment and poverty are more prevalent, leading to lower median incomes. Finally, Promise residents are more likely to enroll K-12 children in private schools. Many of these gaps are reduced or even reversed when considering our smaller geographic housing market or our propensity score screened optimal subpopulation, although differences remain significant. It is important to note that neither of the more selective samples dominates the other in terms of matching observables across groups. For example, the floor area of Promise properties matches more closely to the control properties in the small geographic market than in the optimal subpopulation, while the reverse is true for the percentage of black residents in the neighborhood. Due to the way the optimal subpopulation is constructed, the two groups in that sample should be matched closely on the covariates that are important for residential location decisions. In addition, the small geographic market definition yields fewer observations and estimates will be less precise as a result. We present results from both samples in what follows, but we believe the optimal subpopulation represents the best trade off between reducing bias from unbalanced observables and increasing the variance of the resulting estimates.

4. Results

We first address the results from the K-12 enrollment data, which apply to a broad sample of Promise scholarship programs. We follow that with evidence of the impact of selected Promise scholarship programs on local housing markets. Finally, we present a more detailed housing market analysis for two large metropolitan Promise zones—Pittsburgh and Denver.

4.1. Public School Enrollment Estimates

Figure 3 provides graphical evidence, both towards the validity of the parallel trends assumption and of the effect of the Promise on K-12 enrollment. We divide the baseline sample into geographic areas, each composed of one or two Promise zones and the surrounding counties. Within a geographic area, years were normalized such that the

year that the relevant Promise was announced was set equal to zero.²⁰ We then regress log-transformed student counts on a full set of area-by-year fixed effects and plotted the yearly average residuals for treated schools and untreated schools along with a linear fit.

[Figure 3 about here]

The graph depicts the variation in total student enrollment that is orthogonal to region-wide shocks in the years leading up to and immediately following the announcement of a Promise program. While there are substantial differences in levels between the groups, the trends in enrollment were not substantially different between groups prior to treatment. After the announcement of a Promise program, however, the control group continues on its pre-existing trend, while the Promise schools display a jump in enrollment as well as a sharp upturn in their enrollment trend. We attribute this convergence to increased demand for public schools following the announcement of a Promise program.

Table 4 displays the results of our fixed-effects estimates of school-level outcomes from equation 1 in Panel A and equation 2 in Panel B.

[Table 4 about here]

As predicted, when enrollment in a particular set of schools gains a student access to a potentially meaningful scholarship award, more students will enroll in those schools. The announcement of a Promise program leads to an increase in overall enrollment of roughly 4%. On average, increases in total enrollment are similar across racial groups, although the effects are not significant when decomposed in this way.

It is typical for Promise programs to scale up scholarship awards with the length of continuous enrollment at graduation. This feature makes the scholarship more valuable to students who begin their enrollment at early grade levels. Also, students who begin their enrollment spell past grade 9 or 10 are excluded from most Promise scholarships. As a result, we would expect much of the enrollment increases over the initial years of a Promise program to occur in the earlier grade levels especially in those programs that feature this sliding scale. Figure 4 depicts the treatment effect as estimated for each grade level separately.

[Figure 4 about here]

²⁰If two Promise programs were announced in the same year and were located close enough that there was significant overlap in the adjacent counties, they were pooled into one area.

The estimated increases in enrollment in Promise districts match this pattern almost precisely, with significant increases in enrollment at the lower grade levels (1-4), followed by no detectable changes through most of the higher grades (5-11), and finally decreases in enrollment in grade 12. Furthermore, this pattern is much more pronounced amongst those programs featuring a sliding scale relative to those which lack this feature. This match between the enrollment incentives provided by Promise scholarships and the estimated treatment effects gives us confidence that the identified overall effect is causal.

Turning our attention to the heterogeneity across program features, in panel B of Table 4 the effects of Promise programs are decomposed into those generated by programs of different classes. This exercise reveals that estimated overall effect is masking heterogeneity across programs. In addition, the variation is consistent with the expected effect of program features on the scholarship's prospective value. We would expect universal programs that allow use at a wide range of schools should present the most value to the widest range of households. Either imposing a merit requirement or restricting the list of schools should decrease the attractiveness of the program, although which restriction matters more is ambiguous. Finally, offering a merit-based scholarship usable only at a small list of schools should present the least value for the fewest households. Our estimates follow that profile exactly, with universal, wide-list programs generating the largest enrollment increases (8%) followed by merit-based, wide-list programs and universal, narrow-list programs (4%). Programs offering merit-based scholarships usable at a small list of schools seem to have no effect on overall enrollment.

There are also racial disparities in the response to these programs that vary by program feature as indicated by columns 2 and 3 in Panel B. In particular, programs featuring merit requirements prompt increases in white enrollment while leading to significant decreases in non-white enrollment. The racial pattern is likely explained by the existing racial achievement gap in U.S. public schools (Murnane, 2013). As award receipt in these programs is conditioned explicitly on success in high school, the value for the average non-white student is diminished. Universal programs with large lists of eligible schools seem to have no effect on relative enrollment across racial groups, consistent with the analysis of the Kalamazoo Promise which belongs to this class. Finally, the small decrease in total enrollment in schools offering merit-based scholarships usable at a small

list of schools is driven by a significant decrease in the enrollment of non-white students. Again, this conforms to our expectations regarding the incentives implied by different scholarship features and how they interact with racial groups.

Overall, offering a Promise scholarship tied to enrollment in a particular public school district is effective in drawing students into that school district, especially if graduates are able to use the scholarship at a wide range of institutions or there are no merit requirements for eligibility. However, Promise programs also have an important impact on the demographic composition of schools. Program administrators should note that scholarships with merit requirements will primarily attract white students and may lead to decreases in non-white enrollment, potentially contributing to racial inequality in educational attainment.

4.2. Pooled Housing Market Estimates

Our enrollment estimates suggest that demand for public schools increases in areas where it is a pre-requisite for Promise scholarship receipt. As public school enrollment is tied to residential location, this would imply an increase in housing demand as well. If we assume that housing supply is fixed in the short run, any increase in housing demand must be capitalized into housing prices. In figure 5, we repeat the graphical exercise conducted on the K-12 enrollment data, but using instead the housing market data and plotting separately for each market definition. Log housing prices for our eight Promise-related housing markets were regressed on a full set of market-by-year-by-quarter fixed effects and the monthly average residuals for treated properties and untreated properties are plotted along with a local linear fit on either side of the announcement date.

[Figure 5 about here]

Clearly in the context of the large housing market definition, any impact of program announcement on housing prices in Promise areas is hard to detect. While the difference between groups narrows after the program announcement, the series diverge again to pre-Promise levels within about 2 years. As mentioned previously, however, this estimate is subject to significant bias due to the composition of the sample. The large market definition includes properties in the periphery who may not be affected by the Promise as well as properties in the center of the Promise zone that may not be considered by the

marginal household when making their residential location decision. Inclusion of both groups biases the estimate of the effect towards zero.

When restricting attention to the smaller geographic housing market definition, the impact of the Promise is more noticeable, but qualitatively similar. There is a convergence between the series immediately after the program announcement, followed by slight divergence after about two years. It is hard to discern from the graph if there was or was not a lasting impact of the Promise announcement on housing prices in the sample. Using the optimal subpopulation yields a different story, however. After the announcement of the Promise, there is a noticeable and discrete increase in prices occurring in Promise zones which persists through the 2.5 years following the announcement.

Table 5 presents the results from our estimation of equation 3. Each panel corresponds to a different definition of a housing market. The specification in Column 1 includes only school district and market-specific time fixed effects. Of the difference in difference estimators, this specification is the most similar to the graphical analysis and is also subject to the most omitted variables bias, as it identifies the effect through temporal variation of prices at the school district level. Column 2 adds controls for various building and neighborhood characteristics of the property and exchanges school district fixed effects for the more spatially explicit Census tract fixed effects. Finally, column 3 includes property fixed effects, identifying the impact of the program from repeat sales of identical properties in Promise zones vs. outside. These same estimates are repeated in Table 6 using price in constant 1990 dollars as the dependent variable

[Tables 5 and 6 about here]

The simplest DD specification yields inconsistent and imprecise capitalization estimates. This may indicate why previous studies using such a specification, but lacking access to rich real estate data across several programs have been unable to uncover a significant treatment effect. After controlling for property covariates and neighborhood fixed effects, the magnitude of estimates increases and the variance decreases across all samples, suggesting capitalization effects on the order of 4% to 6% of home values, or between \$5,500 and \$8,000. Our preferred specifications use either the small geographic housing market or propensity score screened optimal subpopulation and include property level fixed effects, identifying the effect from repeat sales. These specifications provide

very precise treatment effects of between 6% and 12% of home values or \$14,000 and \$20,500.

Our analysis of public school enrollment suggested that Promise programs have different impacts on different populations, particularly on different racial groups. As such, we would like to document any such heterogeneity in the housing market as well. Our housing market data provides no information on the characteristics of the individuals participating in the transactions. However, we do observe the transaction price of the house, which should be correlated with income and, as a result, race.

To investigate the heterogeneity of the capitalization of Promise scholarships with respect to income, we divide each housing market in half according to the distribution of housing values implied by the pre-announcement hedonic price function. As described in the previous section, we estimate the hedonic price function over the pre-Promise period in each housing market, recover the coefficient estimates, and then use them to predict the sale price of *all* transactions observed in the sample as if each had occurred in the first quarter of the year prior to the relevant Promise announcement. We then repeat the DD analysis above, but separately for the samples of properties above the median and below the median of the distribution generated by the first step. We report the estimates from the tract-level fixed effects specification (equivalent to column 2 in Table 5) only. The results are depicted in Figure 6. Across estimation samples, the capitalization of Promise programs into housing prices increases across the housing price distribution. Capitalization effects in the 1st quintile range from 2.8% to 5.5% compared to capitalization in the top quintile of between 6.8% and 8.9%.

[Figure 6 about here]

There are several reasons why higher income households may be willing to pay more to gain access to Promise scholarship programs. As mentioned in the previous section, students from higher income households are more likely to attend college and the value of access to Promise scholarships is ultimately conditional on college attendance. Even conditional on college attendance and the quality of the institution, most Promise scholarships only apply to unmet need, which should be greater for high income households due to a larger expected family contribution. As it is reasonable to expect these higher income families to occupy higher priced domiciles, the results from our regressions pro-

vide more evidence in support of the claim that higher income households are willing to pay more for access to Promise scholarship programs.

4.3. Large Urban Promise Zone Estimates

The pattern of capitalization across the housing distribution suggests that higher-income households place more value on access to Promise scholarships. As a result, one might also expect there to be a similar pattern of capitalization across the distribution of school quality. In order to verify such a pattern, we must link properties to school-level data on performance, such as state standardized test scores. Unfortunately, neither school attendance boundaries nor standardized test performance data is readily available for all of the Promise zones included in our housing market analysis.

For the two Promise programs in our housing market data based in large metropolitan areas—the Pittsburgh Promise and the Denver Scholarship Foundation—we obtained school attendance boundary maps through SABINS. In addition, we acquired school-level data on standardized test scores from the Pennsylvania and Colorado state education agencies. This data allows us to link properties in our housing market data to objective measures of pre-Promise school quality. Before presenting those results, however, we verify that the results from the pooled housing market sample also hold in both Pittsburgh and Denver. Table 7 reports estimates of the treatment effect within each market, identifying from repeat-sales as in column 3 of Table 5.

[Table 7 about here]

Both programs display large treatment effects across all samples, ranging from 15% to 22% in the Pittsburgh market and 5% to 11% in the Denver market. Estimates from specifications using price in constant dollars as the dependent variable are provided for comparison purposes; the implied capitalization amounts are roughly in line with the magnitude of award amounts.

Our final set of results attempts to correlate the capitalization effects of these Promise programs with the quality of schools. Our hypothesis is that capitalization will be concentrated in neighborhoods with higher quality schools. This is because the higher income households on the margin will likely be choosing between higher quality suburban neighborhoods (and no access to Promise aid) and lower quality urban schools (with access

to Promise aid). As such, the households that relocate will aim first to minimize the associated loss in school quality.

In order to quantify school quality, we first calculated the percentage of students in each Pennsylvania or Colorado public school that scored “proficient” or better in math and reading standardized tests in 2005, prior to the announcement of either program. Then, we standardize this measure of quality such that within each state by school level cell the distribution has a zero mean and unit standard deviation. Table 8 contains the results from estimating equation 5.

[Table 8 about here]

With the exception of the measure of high school quality in the large housing market definition, all of our school quality metrics are associated with larger capitalization effects of Promise program announcement. Across Pittsburgh and Denver, a one standard deviation increase in the quality of the neighborhood high school leads to an increase in the capitalization effect of the Promise of between 1% and 5% (or \$2,500 and \$6,000). Estimates using primary school quality are uniformly larger; a one standard deviation increase in the quality of the neighborhood primary school leads to an increase in the capitalization effect of the Promise of between 5% and 10% (or \$8,800 and \$16,000). We expect that the magnitude of the primary school quality effect relative to the high school quality effect is due to a combination of factors. First, the incentives provided by many Promise programs (including the Pittsburgh Promise) are strongest for primary school students as the scholarship amount scales with years of continuous enrollment. As a result, primary school quality should be focal for the households most likely to be influenced by the program. Also, due to the presence of school choice programs in Pittsburgh and Denver, residential location is not always the sole determinant of school quality and the strength of this link varies across grade levels. In Pittsburgh in 2010, 62% of the public elementary school students attended their neighborhood school compared to only 52% of public high school students. The situation in Denver is similar; in 2013, 57% of K-5 public school students attended their neighborhood school compared to 39% of public high school students (9-12). As a result, the quality of the neighborhood high school may be less relevant to the residential location decision than the quality of the neighborhood primary school for which fewer feasible alternatives exist.

Furthermore, in results not presented here we estimated the capitalization effects by individual high school neighborhoods and found that after the announcement of Promise programs in Pittsburgh and Denver, housing prices increased in the neighborhoods associated with top performing high schools in the district (top 3 in Pittsburgh, top 4 in Denver). In addition to these high-performing schools, large capitalization effects are also estimated for the neighborhood associated with the school that ranked at the bottom of each city’s high schools— Peabody High School in Pittsburgh and North High School in Denver. Neighborhood level data, however, shows that school attendance rates of resident public school students are among the lowest in each district for these lower-quality schools. On this measure, Peabody ranked 45 out of 48 traditional schools in Pittsburgh in 2010 and North ranked 97 out of 103 traditional schools in Denver in 2013. As a result, some high-income households seem to have located in these Promise-eligible neighborhoods associated with poor quality schools, while utilizing the school choice systems in Pittsburgh and Denver to send children to high quality public secondary schools.²¹

5. Conclusion

Place-based “Promise” scholarship programs have proliferated in recent years. Typically implemented at the school district level and financed privately, they guarantee financial aid to eligible high school graduates from a particular school district, provided they have continuously resided in the district for a number of years. In this study, we measure the impact of a cross-section of Promise scholarships on a range of policy-relevant outcomes, including public school enrollment and housing prices. In addition, we provide the first direct evidence of how enrollment effects vary with features, such as eligibility requirements and scholarship flexibility.

Using a difference-in-differences approach, we conclude that the initiation of a Promise program leads to an increase in public school enrollment in affected schools and an increase in housing prices of between 6% and 12%, with capitalization effects most dramatic amongst Promise zone properties in the upper half of the house price distribution. Even so, there is substantial variation in these effects according to the features of the programs.

²¹All data on neighborhood school attendance rates was provided by Pittsburgh Public Schools and Denver Public Schools.

Scholarships that are usable at a wide range of institutions are effective at increasing total public school enrollment, although this is mitigated by the imposition of merit requirements. However, the effects on school composition vary, with merit requirements providing strong incentives for white enrollment at the expense of non-white enrollment. Furthermore, focusing on Pittsburgh and Denver specifically, the capitalization effect of scholarship programs into housing prices increases with the quality of the neighborhood public school. Taken together, this evidence suggests that these scholarships have important distributional effects that bear further examination.

These results provide strong guidance to future program designers. First and foremost, place-based scholarship programs are capable of having an impact on important regional development outcomes, such as population, school enrollment, and property values. Making the scholarship usable at a wide range of schools is essential in attracting households to the scholarship area. Unfortunately, since minority students are less likely to satisfy them, adding merit requirements could increase educational inequality. Further contributing to inequality, we find that the increase in housing demand resulting from the announcement of the Promise is most pronounced in high-priced neighborhoods with high-quality schools. As a result, the potential for peer effects to play a role in the mitigation of inequality is greatly reduced as the high-quality students attracted by the Promise seem to be settling into already high quality schools.

Still, these same capitalization effects are evidence that high-income households are paying a premium for housing in the wake of a Promise scholarship program, while low-income households do not face the same increase in housing costs to the extent that they own instead of rent. As such, while low-income students will likely utilize these scholarships less often than high-income students, they may benefit more net of this house price effect, although a complementary analysis of rental rates would be necessary to confirm this intuition. In addition, the increase in the tax base that may result from the increase in home values leaves open the potential for more disadvantaged students to benefit. If high-income households are contributing more to Promise school districts in the form of property taxes, low-income students stand to benefit through that channel as well. As a result, the impact of Promise scholarships on educational equity remains somewhat ambiguous and is an area for future research.

There are many other avenues for future research into Promise scholarship programs. Broader real estate transactions data would allow for an extension of the housing market analysis conducted here to the remaining Promise programs. Such research would be important in generalizing the house price effects of Promise programs beyond our sample of eight programs, which offer little variation in program features. We also hope to increase the scope of our evaluation to a wider range of outcomes. Any impact of Promise scholarships on school quality and test scores is important in answering questions related to the effect on educational inequality. Retaining high-income families has the potential to substantially change the composition and performance of urban schools, leading to spillover effects for low-income students.

Extending the analysis to the postsecondary education market would also be fruitful. Some individual Promise programs have studied their effects on college choice and attendance with success. However, typically such studies are conducted through arrangements with school districts, which often have student level records of college applications and enrollments. As a result, data availability is a concern. The same is true for the impact of Promise scholarships on cost of attendance. Recent studies have shown that if students are likely to receive aid from other sources and their chosen college or university can easily quantify the amount of aid, the institution will increase its effective price by reducing the amount of institutional aid provided (Turner, 2011, 2012). Knowing that a student comes from a Promise district is a fairly strong signal to a post-secondary institution that the student may be receiving Promise aid. As a result, some of the value of the scholarship may well be captured in the market for post-secondary education. In addition, if the signal is stronger for high-income students than low-income students, perhaps due to uncertainty surrounding additional merit requirements, or demand is more elastic among low-income students, documenting such an effect would have distributional implications as well.

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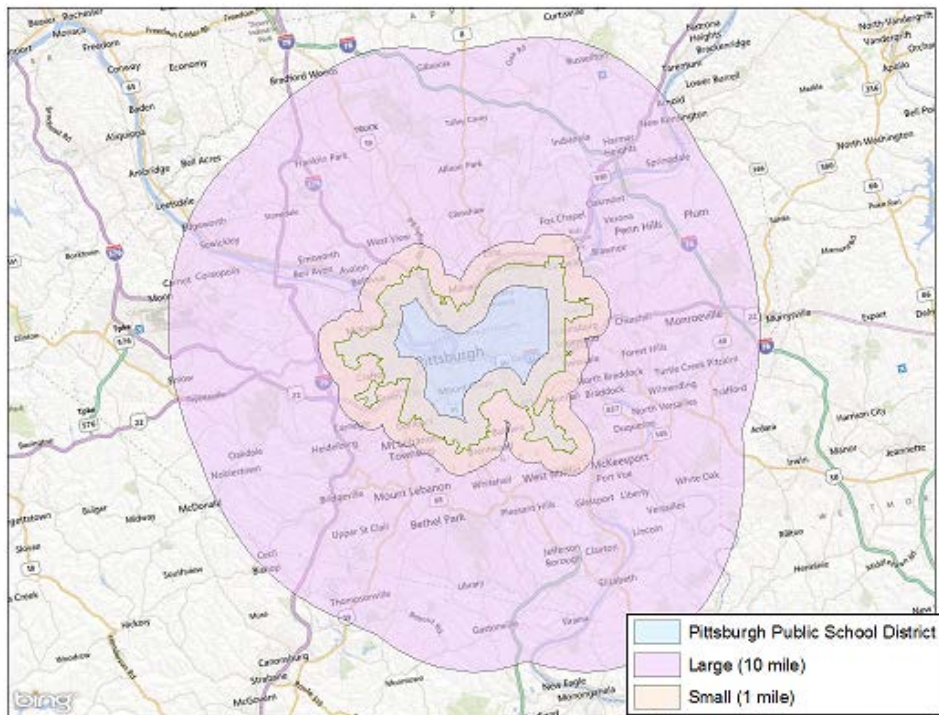


Figure 1: Large (10 mile) and Small (1 mile) Housing Markets in Pittsburgh, PA

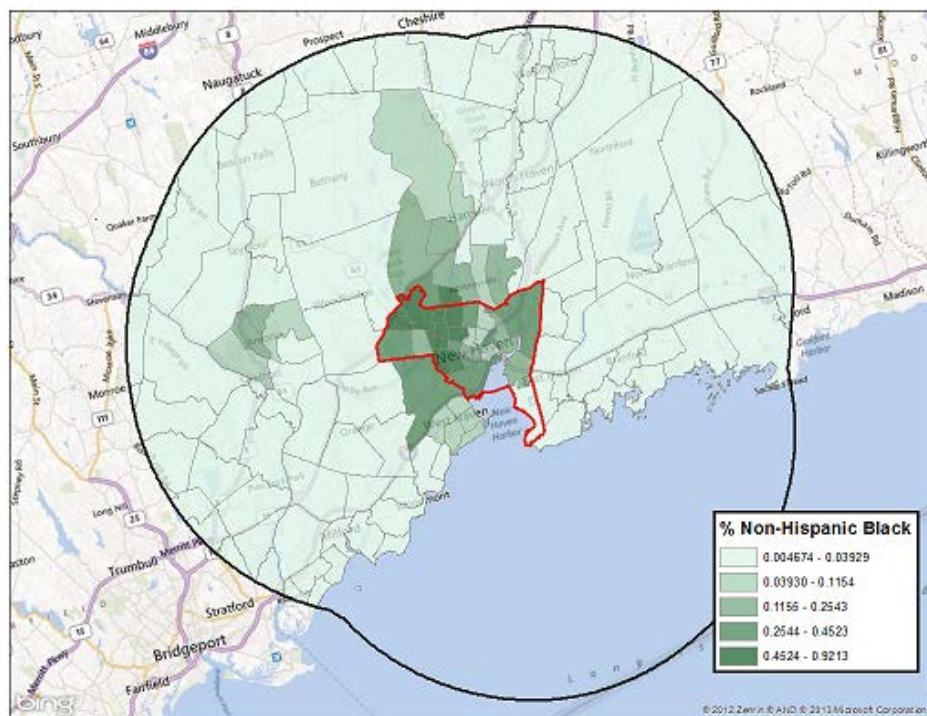


Figure 2: Percent Non-Hispanic Black (2000) by Census Tract

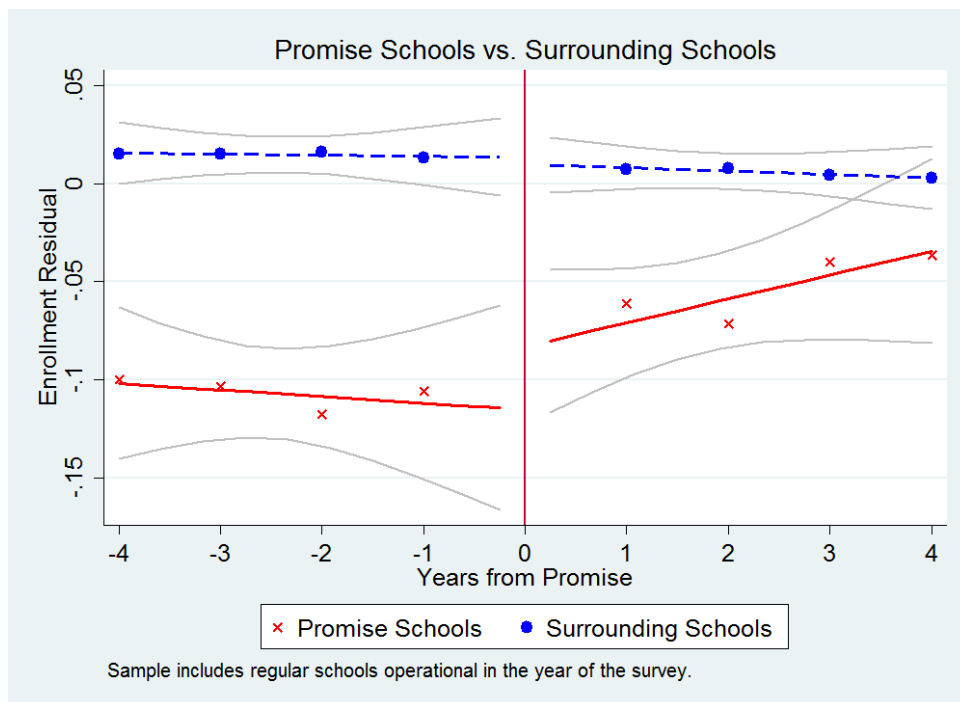


Figure 3: Total Enrollment Residual by Year

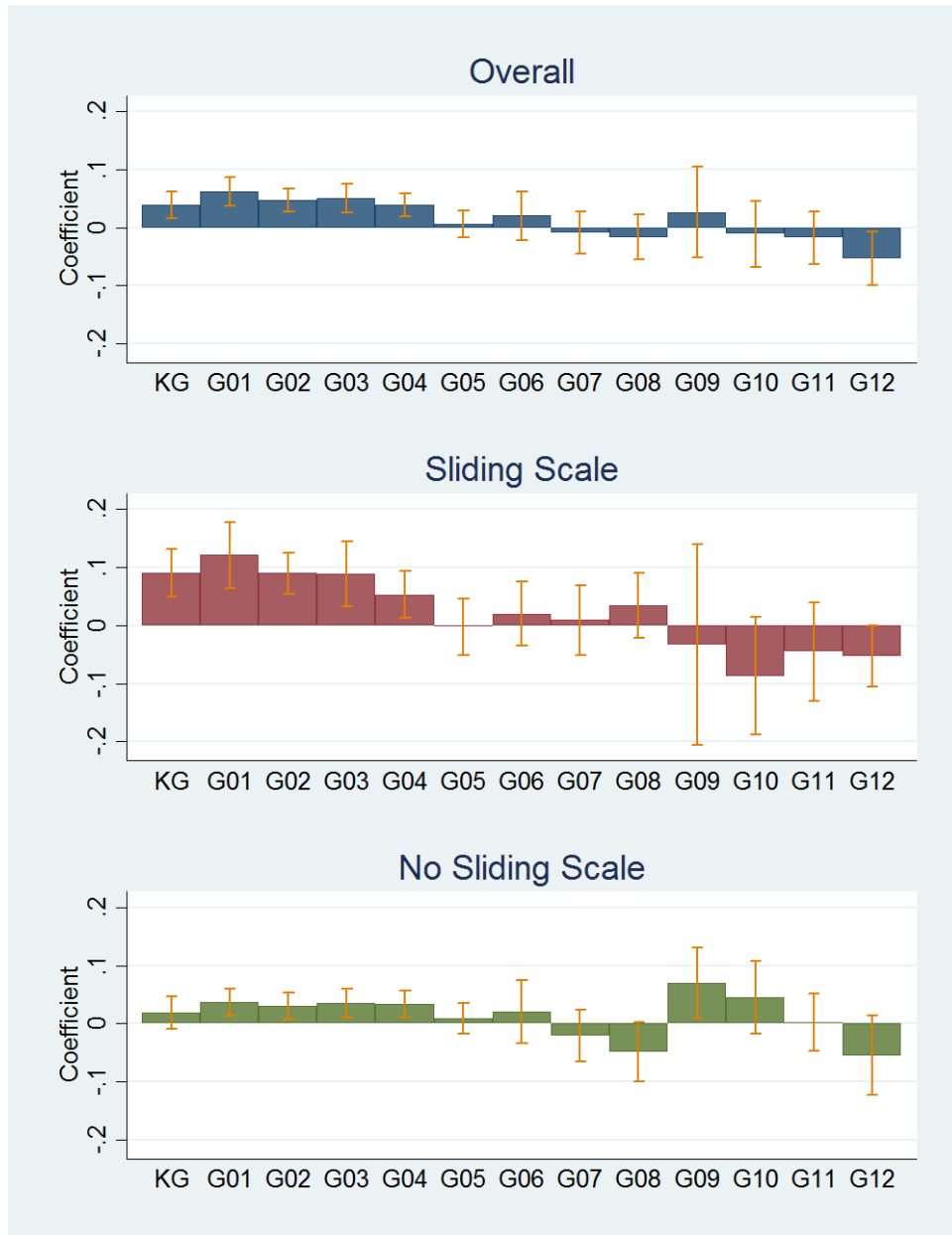


Figure 4: Treatment Effect by Grade Level

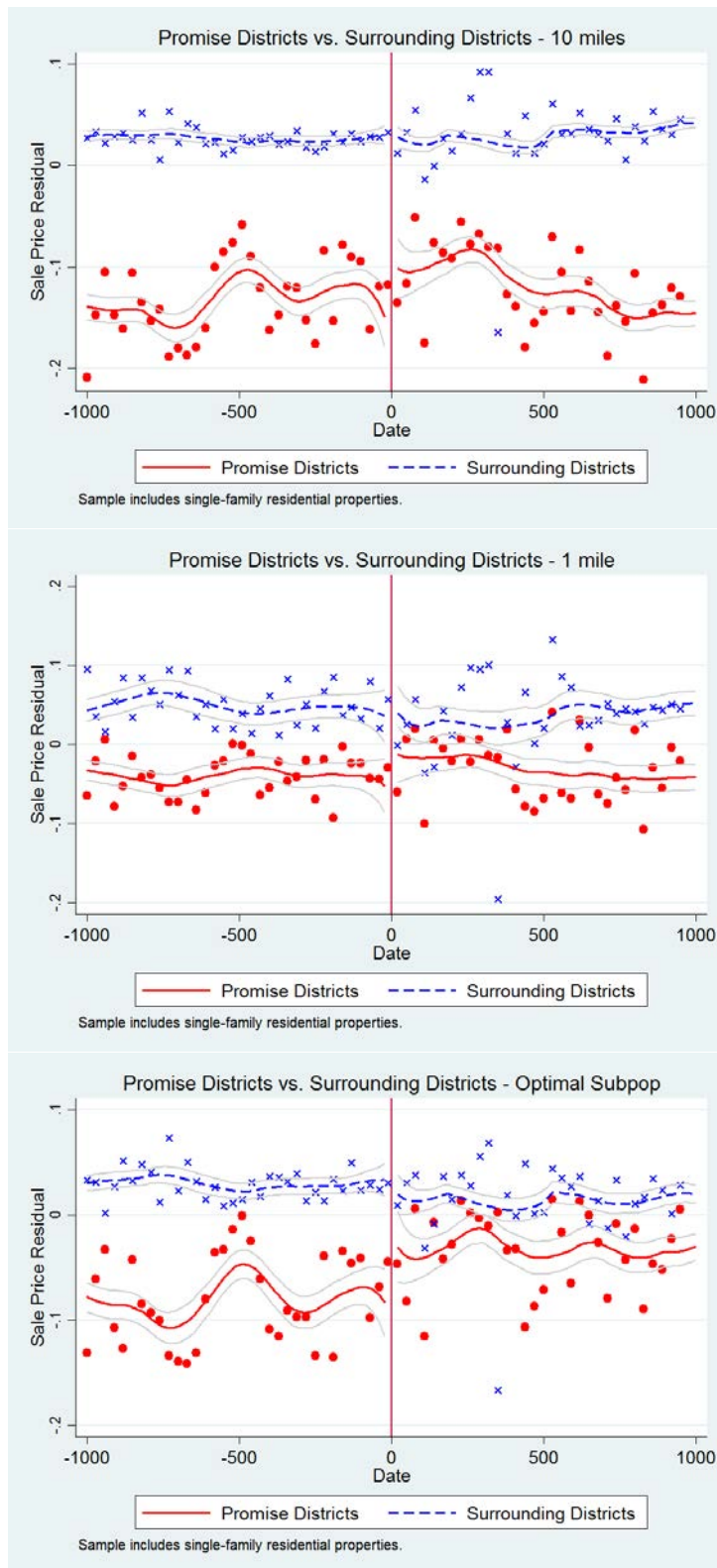


Figure 5: Sale Price Residuals by Date

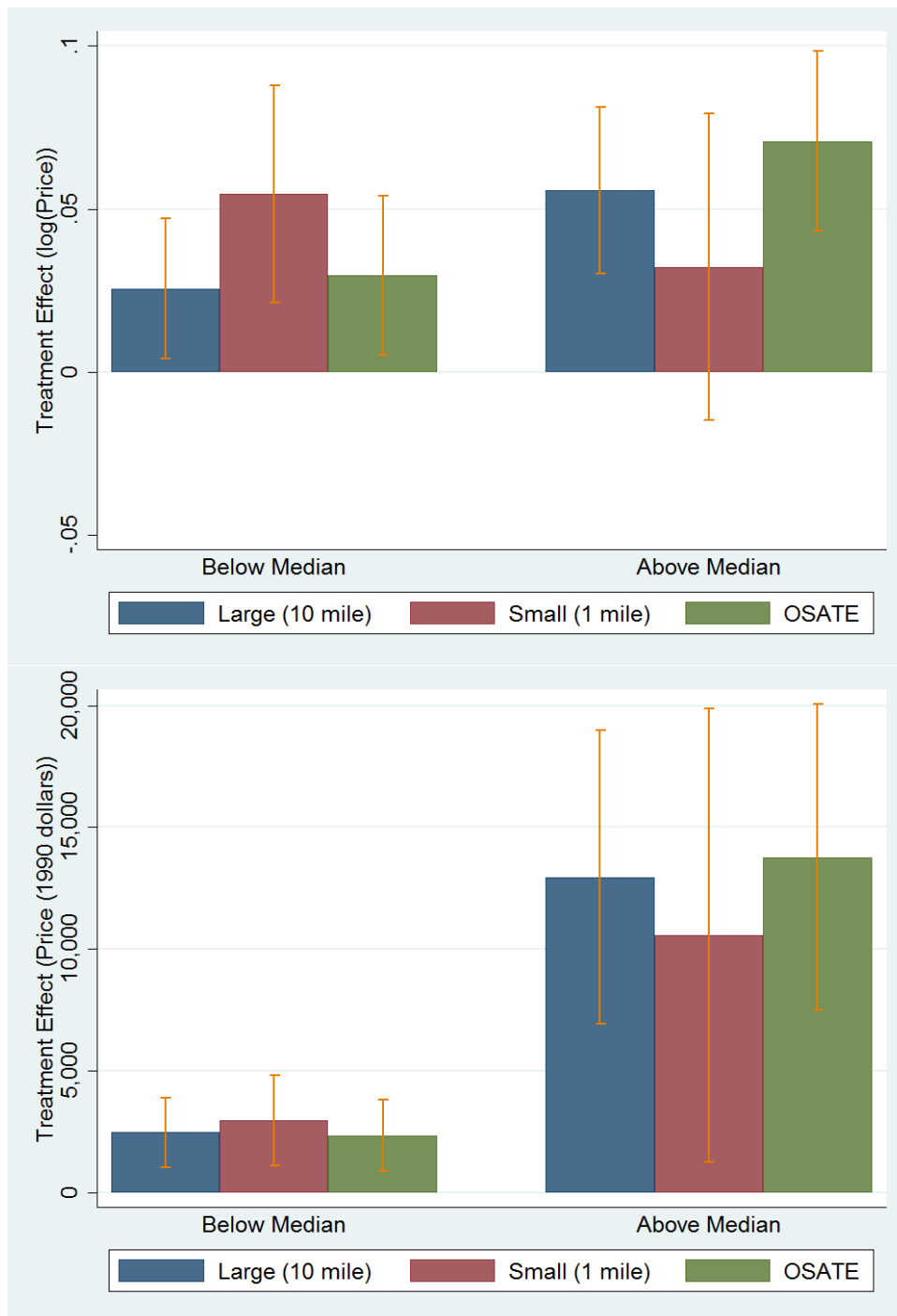


Figure 6: Treatment Effect by Above/Below Median

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Arkadelphia Promise	Arkadelphia, AR	2010	<ul style="list-style-type: none"> • Graduate from Arkadelphia HS • Continuous enrollment since 9th grade • 2.5 GPA or 19 ACT • Receive AR Lottery scholarship • Apply for 2 other scholarships 	Sliding scale; 65% to 100% of unmet need per year; Max: highest tuition at Arkansas public PSI.	Any accredited PSI in the U.S.
Baldwin Promise	Baldwin, MI	2010	<ul style="list-style-type: none"> • Reside within Baldwin Community SD • Graduate from any HS within zone • Continuous residency since 9th grade. 	Sliding scale; \$500 to \$5,000 per year	Any accredited PSI in the Michigan

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Bay Commitment	Bay, MI	2006	<ul style="list-style-type: none"> ● Graduate from Bay County HS ● Continuous enrollment since 9th grade ● Continuous residency for 6 years ● First-generation college student 	\$2,000 per year	Delta College or Saginaw Valley State University
College Bound Scholarship Program	Hammond, IN	2006	<ul style="list-style-type: none"> ● Continuous residency within Hammond City for 3 years ● Graduate from any HS in Hammond City ● 3.0 GPA OR ● 2.5 GPA with 1000 SAT (math and verbal) OR ● 2.5 GPA with 1400 SAT 	Sliding scale; 60% to 100% of unmet need per year; Max: tuition at Indiana Univ. Bloomington.	Any accredited PSI in Indiana

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Denver Scholarship Foundation	Denver, CO	2006	<ul style="list-style-type: none"> • Graduate from Denver Public HS • Continuous enrollment since 9th grade • 2.0 GPA • Demonstrate financial need (EFC < 2x Pell limit) 	\$250 to \$3,400 per year depending on PSI and EFC	39 PSIs in Colorado
Detroit College Promise	Detroit, MI	2008	<ul style="list-style-type: none"> • Graduate from traditional Detroit Public HS • Continuous enrollment since 9th grade • Continuous residency since 9th grade 	\$150 to \$600 for one semester	43 public PSIs in Michigan
Educate and Grow Scholarship	Blountville, TN	Unknown	<ul style="list-style-type: none"> • Continuous residency within selected counties for 12 mos. prior to graduation • Graduate from any HS 	Full tuition (4 semesters)	Northeast State Community College

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
El Dorado Promise	El Dorado, AR	2007	<ul style="list-style-type: none"> • Graduate from El Dorado Public Schools • Continuous enrollment since 9th grade • Continuous residency since 9th grade. 	Sliding scale; 65% to 100% of unmet need per year; Max: highest tuition at Arkansas public PSI.	Any accredited PSI in the U.S.
Great River Promise	Phillips County, AR	2010	<ul style="list-style-type: none"> • Graduate from Arkansas or Phillips County HS • Continuous enrollment since 9th grade • Achieve high school attendance requirements. 	Full tuition (4 semesters)	Phillips Community College of the University of Arkansas
Hopkinsville Rotary Scholars	Hopkinsville, KY	2005	<ul style="list-style-type: none"> • Graduate from selected high schools • 2.5 GPA • 95% attendance 	Full tuition (4 semesters)	Hopkinsville Community College

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Jackson Legacy	Jackson County, MI	2006	<ul style="list-style-type: none"> • Graduate from Jackson County HS • Continuous enrollment since 10th grade • Community service 	Sliding scale; \$150 to \$600 per year for two years	Jackson Community College, Spring Arbor University, Baker College of Jackson
Kalamazoo Promise	Kalamazoo, MI	2005	<ul style="list-style-type: none"> • Graduate from Kalamazoo Public Schools • Continuous enrollment since 9th grade • Continuous residency since 9th grade. 	Sliding scale; 65% to 100% of tuition per year	Any public PSI in Michigan
Legacy Scholars	Battle Creek, MI	2005	<ul style="list-style-type: none"> • Graduate from Battle Creek or Lakeview SD • Continuous enrollment since 10th grade 	Sliding scale; 31 to 62 credit hours	Kellogg Community College

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Leopard Challenge	Norphlet, AR	2007	<ul style="list-style-type: none"> • Graduate from Norphlet HS • Continuous enrollment since 9th grade • Continuous residency since 9th grade • 2.25 GPA 	Sliding scale; \$2,600 to \$4,000 per year	Any accredited PSI in the U.S.
Muskegon Opportunity	Muskegon, MI	2009 ^a	TBD	TBD	TBD
New Haven Promise	New Haven, CT	2010	<ul style="list-style-type: none"> • Graduate from New Haven Public Schools • Reside in New Haven • 3.0 GPA • 90% attendance • Community service 	Sliding scale; 65% to 100% of unmet need per year at public; Up to \$2,500 at private	Any accredited PSI in Connecticut

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Northport Promise	Northport, MI	2007	<ul style="list-style-type: none"> • Graduate from Northport HS • Continuous enrollment since 9th grade • Participate in fundraising activities 	Sliding scale; Amount determined each year	Any public PSI in Michigan
Peoria Promise	Peoria, IL	2008	<ul style="list-style-type: none"> • Graduate from public school in Peoria • Continuous enrollment since 10th grade • Continuous residency since 10th grade. 	Sliding scale; 50% to 100% of tuition for up to 64 credit hours	Illinois Central College
Pittsburgh Promise	Pittsburgh, PA	2006	<ul style="list-style-type: none"> • Graduate from Pittsburgh Public Schools • Continuous enrollment since 9th grade • Continuous residency since 9th grade • 2.5 GPA • 90% attendance 	Sliding scale; \$1,000 to \$10,000 per year	Any accredited PSI in Pennsylvania

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Promise for the Future	Pinal County, AZ	2001 ^b	<ul style="list-style-type: none"> • Graduate from Pinal County HS • Continuous enrollment since 8th grade • 2.75 GPA 	Full tuition (4 semesters)	Central Arizona College
Say Yes Buffalo	Buffalo, NY	2012	<ul style="list-style-type: none"> • Graduate from Buffalo Public Schools • Continuous enrollment since 9th grade • Continuous residency since 9th grade 	Sliding scale; 65% to 100% unmet need	Any State University of New York or City University of New York campus. ^c
Say Yes Syracuse	Syracuse, NY	2009	<ul style="list-style-type: none"> • Graduate from Syracuse Public Schools • Continuous enrollment since 10th grade • Continuous residency since 10th grade. 	100% unmet need	Any State University of New York or City University of New York campus. ^c

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
School Counts Program	Hopkins County, KY	Unknown	<ul style="list-style-type: none"> • Graduate from Hopkins County HS in 8 consecutive semesters • Continuous enrollment since 9th grade • Continuous residency since 9th grade • 2.5 GPA yearly • 95% attendance • Exceed graduation credit requirements. 	\$1,000 per semester for 4 semesters	Madisonville Community College
Sparkman Promise	Sparkman, AR	2011	<ul style="list-style-type: none"> • Graduate from Sparkman Public Schools • Continuous enrollment since 9th grade • 2.5 GPA or 19 ACT • Receive AR Lottery scholarship • Apply for 2 other scholarships 	Sliding scale; 65% to 100% of unmet need per year; Max: highest tuition at Arkansas public PSI.	Any accredited PSI in the U.S.

Table 1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Ventura College Promise	Ventura County, CA	2006	<ul style="list-style-type: none"> • Graduate from Ventura County HS • Continuous enrollment since 9th grade • 2.5 GPA or 19 ACT • Receive AR Lottery scholarship • Apply for 2 other scholarships 	Enrollment costs for 1 year	Ventura College

Source: <http://www.upjohn.org/Research/SpecialTopics/KalamazooPromise/PromiseTypeScholarshipPrograms>, Gonzalez et al. (2011), and authors' research. Program details have changed over time; for brevity, all details reported represent current program configurations.

^a Announced in 2009, but no details of eligibility or amount have been provided to date. Due to the high degree of uncertainty, was not included in analysis.

^b While the Kalamazoo Promise is often referred to as the first in this class, we have found a source dating the start of the Promise for the Future back to 2001 (“Deadline to enroll in *Promise for the Future* Scholarship approaching” *The Superior Sun*. April 15, 2009.). Historical program details were not found during our research.

^c There are other “Say Yes” partner schools, but additional restrictions apply.

Table 2: K-12 Public School Summary Statistics

		Promise Schools	Control Schools	t-stat
Total Enrollment	mean	599.70	745.36	24.71
	(s.d.)	(431.00)	(615.82)	
	Obs.	5,287	42,313	
% White	mean	0.44	0.44	1.84
	(s.d.)	(0.31)	(0.35)	
	Obs.	5,287	42,313	
Primary	mean	0.67	0.68	0.92
	(s.d.)	(0.47)	(0.47)	
	Obs.	5,287	42,313	
Middle	mean	0.17	0.16	1.81
	(s.d.)	(0.38)	(0.37)	
	Obs.	5,287	42,313	
High	mean	0.14	0.14	1.57
	(s.d.)	(0.35)	(0.35)	
	Obs.	5,287	42,313	
City	mean	0.57	0.39	-28.92
	(s.d.)	(0.50)	(0.49)	
	Obs.	5,287	42,313	
Suburb	mean	0.25	0.45	38.82
	(s.d.)	(0.43)	(0.50)	
	Obs.	5,287	42,313	
Town	mean	0.05	0.04	0.80
	(s.d.)	(0.21)	(0.20)	
	Obs.	5,287	42,313	
Rural	mean	0.14	0.12	-4.06
	(s.d.)	(0.35)	(0.32)	
	Obs.	5,287	42,313	

Notes: T-statistic from a two-sided t-test with unequal variance.

Table 3: Housing Market Summary Statistics

		Large (10 mile)			Small (1 mile)			Optimal Subpop.		
		Promise	Control	t-stat	Promise	Control	t-stat	Promise	Control	t-stat
Transaction price	mean	220,026	219,754	-0.42	214,049	189,897	-21.55	216,271	189,513	-35.08
	(s.d.)	(190,684)	(143,416)		(190,952)	(161,839)		(191,359)	(136,338)	
	Obs.	95,954	418,440		55,279	43,933		77,059	174,262	
Price (1990 dollars)	mean	131,961	134,164	5.68	126,971	114,554	-18.63	130,026	114,016	-35.46
	(s.d.)	(112,909)	(85,987)		(113,360)	(96,472)		(113,213)	(80,906)	
	Obs.	95,954	418,440		55,279	43,933		77,059	174,262	
Building age	mean	48.38	26.12	-175.48	45.00	38.72	-31.16	51.65	37.95	-94.43
	(s.d.)	(36.85)	(26.81)		(32.80)	(30.11)		(35.27)	(29.24)	
	Obs.	94,955	401,715		54,867	43,088		77,059	174,262	
Floor area (sq. feet)	mean	1,595.62	1,820.44	87.41	1,573.41	1,598.54	5.56	1,540.07	1,578.04	13.00
	(s.d.)	(710.96)	(750.93)		(723.14)	(693.64)		(689.00)	(642.62)	
	Obs.	95,954	418,440		55,279	43,933		77,059	174,262	
% Black	mean	0.14	0.11	-49.37	0.16	0.09	-65.87	0.12	0.15	33.34
	(s.d.)	(0.17)	(0.22)		(0.17)	(0.14)		(0.17)	(0.25)	
	Obs.	94,751	413,571		54,088	42,424		77,059	174,262	
% under 15	mean	0.20	0.24	128.24	0.21	0.20	-24.57	0.20	0.21	46.59
	(s.d.)	(0.07)	(0.06)		(0.07)	(0.07)		(0.06)	(0.05)	
	Obs.	94,751	413,571		54,088	42,424		77,059	174,262	
% over 60	mean	0.17	0.16	-29.86	0.16	0.21	65.77	0.19	0.19	3.50
	(s.d.)	(0.11)	(0.11)		(0.09)	(0.15)		(0.10)	(0.10)	
	Obs.	94,751	413,571		54,088	42,424		77,059	174,262	

Table 3: Housing Market Summary Statistics

		Large (10 mile)			Small (1 mile)			Optimal Subpop.		
		Promise	Control	t-stat	Promise	Control	t-stat	Promise	Control	t-stat
% Households with children	mean	0.32	0.40	172.03	0.34	0.32	-22.03	0.31	0.34	75.77
	(s.d.)	(0.13)	(0.11)		(0.13)	(0.12)		(0.11)	(0.10)	
	Obs.	94,751	413,571		54,088	42,424		77,059	174,262	
% HS diploma	mean	0.40	0.34	-86.73	0.42	0.41	-3.33	0.40	0.43	29.88
	(s.d.)	(0.19)	(0.16)		(0.18)	(0.16)		(0.20)	(0.16)	
	Obs.	95,056	414,961		54,386	42,424		77,053	174,262	
% College	mean	0.34	0.34	4.47	0.32	0.29	-25.87	0.34	0.28	-71.54
	(s.d.)	(0.21)	(0.17)		(0.20)	(0.16)		(0.21)	(0.16)	
	Obs.	95,056	414,961		54,386	42,424		77,053	174,262	
% unemployed	mean	0.08	0.06	-87.68	0.08	0.07	-23.53	0.08	0.08	6.73
	(s.d.)	(0.04)	(0.05)		(0.04)	(0.04)		(0.05)	(0.05)	
	Obs.	94,154	414,961		53,484	42,424		77,053	174,262	
% in poverty	mean	0.16	0.08	-215.38	0.15	0.11	-65.11	0.16	0.12	-89.99
	(s.d.)	(0.11)	(0.08)		(0.10)	(0.09)		(0.10)	(0.09)	
	Obs.	94,154	414,961		53,484	42,424		77,053	174,262	
% K-12 private	mean	0.18	0.13	-99.90	0.18	0.14	-51.44	0.18	0.14	-59.60
	(s.d.)	(0.17)	(0.09)		(0.16)	(0.11)		(0.16)	(0.12)	
	Obs.	94,513	413,695		54,381	41,751		76,515	173,715	
Median income	mean	51,491	68,328	207.35	52,493	54,967	16.01	50,615	53,512	34.07
	(s.d.)	(21,829)	(25,221)		(22,956)	(24,406)		(20,398)	(17,851)	
	Obs.	94,152	414,961		53,482	42,424		77,052	174,262	

Notes: Prices were deflated to January 1990 dollars using the “All Urban Consumers-Owner’s Equivalent Rent of Primary Residence CPI” from the Bureau of Labor Statistics. T-statistic from a two-sided t-test with unequal variance.

Table 4: K-12 Public School Enrollment Effects of Promise Programs

Dependent Variable:	log(Total)	log(White)	log(Non-white)
<i>Panel A: Overall effects</i>			
PromiseXPost	0.037*** (0.007)	0.023 (0.016)	0.021 (0.012)
<i>Panel B: Effects by type</i>			
No Merit & Wide (117 schools)	0.080*** (0.023)	-0.010 (0.042)	0.001 (0.038)
Merit & Wide (203 schools)	0.040** (0.017)	0.110*** (0.038)	-0.039** (0.020)
No Merit & No Wide (327 schools)	0.039*** (0.009)	-0.020 (0.019)	0.076*** (0.017)
Merit & No Wide (66 schools)	-0.031 (0.026)	0.054 (0.033)	-0.129*** (0.033)
Observations	47,600	47,600	47,600
Clusters (Schools)	6,337	6,337	6,337
R-squared	0.97	0.98	0.98

Notes: Standard errors clustered at the school level in parentheses. Sample includes open, regular schools located in Promise zones and neighboring counties that reported student counts by race in all available surveys conducted within 4 years of the region-relevant Promise announcement. Fixed effects at the region-by-year, locale-by-year, and school level are included in all specifications. Controls include school level (primary, middle, high, other) and locale (city, suburb, town, rural).

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 5: Capitalization Effects of Promise Programs

Dependent Variable: log(Price)	(1)	(2)	(3)
<i>Panel A: Large (10 mile)</i>			
PromiseXPost	-0.003 (0.017)	0.039*** (0.012)	0.083*** (0.007)
Observations	514,394	487,930	505,604
Clusters	2,055	2,008	393,570
R-squared	0.38	0.69	0.92
<i>Panel B: Small (1 mile)</i>			
PromiseXPost	-0.006 (0.022)	0.045*** (0.017)	0.066*** (0.013)
Observations	99,212	93,711	94,925
Clusters	607	595	72,656
R-squared	0.41	0.72	0.93
<i>Panel C: Optimal subpopulation</i>			
PromiseXPost	0.032* (0.018)	0.061*** (0.013)	0.123*** (0.009)
Observations	251,321	250,229	250,229
Clusters	1,465	1,461	196,877
R-squared	0.38	0.67	0.92
Building Controls	NO	YES	NO
Census Controls	NO	YES	YES
Market-Year-Qtr FE	YES	YES	YES
School District FE	YES	NO	NO
Neighborhood (Tract) FE	NO	YES	NO
Property FE	NO	NO	YES

Notes: Standard errors clustered at the school level (in columns 1 and 2) or the property level (column 3) in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. All controls are interacted with housing market indicators. Building controls in column 2 include square footage and a quadratic in building age. Census controls include the following tract-level statistics interpolated from the 1990, 2000, and 2010 Census full-count data as well as the 2006-2010 American Community Survey: % of pop. black, % of pop. under 15/over 60, % of households with children under 18, % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Optimal subpopulation includes sales with propensity scores in the interval [.075,.925].

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 6: Capitalization Effects of Promise Programs

Dependent Variable: Price (\$1990)	(1)	(2)	(3)
<i>Panel A: Large (10 mile)</i>			
PromiseXPost	445.5 (2,244)	7,335*** (1,678)	17,966*** (1,029)
Observations	514,394	487,930	505,604
Clusters	2,055	2,008	393,570
R-squared	0.25	0.72	0.94
<i>Panel B: Small (1 mile)</i>			
PromiseXPost	-2,451 (2,904)	5,504*** (1,732)	14,244*** (1,748)
Observations	99,212	93,711	94,925
Clusters	607	595	72,656
R-squared	0.24	0.75	0.94
<i>Panel C: Optimal subpopulation</i>			
PromiseXPost	3,018 (2,308)	8,214*** (1,595)	20,440*** (1,110)
Observations	251,321	250,229	250,229
Clusters	1,465	1,461	196,877
R-squared	0.27	0.71	0.95
Building Controls	NO	YES	NO
Census Controls	NO	YES	YES
Market-Year-Qtr FE	YES	YES	YES
School District FE	YES	NO	NO
Neighborhood (Tract) FE	NO	YES	NO
Property FE	NO	NO	YES

Notes: Standard errors clustered at the school level (in columns 1 and 2) or the property level (column 3) in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. All controls are interacted with housing market indicators. Building controls in column 2 include square footage and a quadratic in building age. Census controls include the following tract-level statistics interpolated from the 1990, 2000, and 2010 Census full-count data as well as the 2006-2010 American Community Survey: % of pop. black, % of pop. under 15/over 60, % of households with children under 18, % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Optimal subpopulation includes sales with propensity scores in the interval [.075,.925].

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 7: Large Metropolitan Promise Programs

	Pittsburgh		Denver	
	log(Price)	Price (\$1990)	log(Price)	Price (\$1990)
<i>Panel A: Large (10 mile)</i>				
PromiseXPost	0.218*** (0.046)	13,508*** (2,619)	0.105*** (0.006)	24,784*** (1,326)
Observations	52,716	52,716	221,198	221,198
Clusters	46,573	46,573	160,455	160,455
R-squared	0.91	0.95	0.89	0.94
<i>Panel B: Small (1 mile)</i>				
PromiseXPost	0.147** (0.075)	8,701*** (2,763)	0.069*** (0.013)	17,841*** (2,363)
Observations	14,474	14,474	49,445	49,445
Clusters	12,762	12,762	34,241	34,241
R-squared	0.90	0.96	0.9	0.93
<i>Panel C: Optimal subpopulation</i>				
PromiseXPost	0.155** (0.077)	8,096*** (2,424)	0.046*** (0.012)	4,783*** (1,273)
Observations	13,517	13,517	36,104	36,104
Clusters	11,903	11,903	24,748	24,748
R-squared	0.91	0.97	0.87	0.94
Census Controls	YES	YES	YES	YES
Market-Year-Qtr FE	YES	YES	YES	YES
Property FE	YES	YES	YES	YES

Notes: Standard errors clustered at the property level in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. Census controls include the following tract-level statistics interpolated from the 1990, 2000 and 2010 Census full-count data: % of pop. black, % of pop. under 15/over 60, % of households with children under 18. In addition, the following block tract-level statistics are interpolated between the 1990 and 2000 Census sample files and the 2006-2010 American Community Survey: % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Full count statistics interpolated between 1990-2010 with years after 2010 held constant at 2010 values. Sample statistics interpolated between 1990-2006 with years after 2006 held constant at 2006 values. Optimal subpopulation includes sales with propensity scores in the interval [.091,.909] for Pittsburgh and [.076,.924] for Denver.

** Significant at the 5% level

*** Significant at the 1% level

Table 8: Large Metropolitan Promise Programs by School Quality

	High School Quality				Primary School Quality			
	Math		Reading		Math		Reading	
	log(Price)	\$1990	log(Price)	\$1990	log(Price)	\$1990	log(Price)	\$1990
<i>Panel A: Large (10 mile)</i>								
Promise x Post	-0.011*	-5,397***	0.004	-3,081***	0.084***	14,083***	0.068***	10,798***
x Quality	(0.006)	(716.8)	(0.004)	(514.1)	(0.005)	(978.6)	(0.005)	-812.6
N (Clusters)	195,412 (144,002)				179,567 (131,872)			
R-squared	0.91	0.95	0.91	0.95	0.91	0.95	0.91	0.95
<i>Panel B: Small (1 mile)</i>								
Promise x Post	0.027**	4,535***	0.046***	5,859***	0.092***	15,867***	0.080***	14,172***
x Quality	(0.011)	(1,420)	(0.008)	(1,002)	(0.008)	(1,696)	(0.007)	(1,528)
N (Clusters)	52,925 (37,750)				49,749 (35,495)			
R-squared	0.93	0.94	0.93	0.94	0.93	0.94	0.93	0.94
<i>Panel C: Optimal subpopulation</i>								
Promise x Post	0.014*	2,579***	0.023***	2,967***	0.060***	9,960***	0.052***	8,885***
x Quality	(0.008)	(919.6)	(0.006)	(642.6)	(0.007)	(1,306)	(0.006)	(1,110)
N (Clusters)	67,663 (47,838)				61,787 (43,451)			
R-squared	0.92	0.94	0.92	0.94	0.91	0.95	0.91	0.95
Census Controls	YES	YES	YES	YES	YES	YES	YES	YES
City-Year-Qtr	YES	YES	YES	YES	YES	YES	YES	YES
Property FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Standard errors clustered at the property level in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. Raw school quality in 2005 is measured as the percentage of students that score proficient or advanced on state standardized tests. This raw measure is then standardized within state-school level cells such that the resulting standardized measure has mean

zero and standard deviation 1 within each cell. All controls are interacted with housing market indicators. Census controls include the following tract-level statistics interpolated from the 1990, 2000 and 2010 Census full-count data: % of pop. black, % of pop. under 15/over 60, % of households with children under 18. In addition, the following block tract-level statistics are interpolated between the 1990 and 2000 Census sample files and the 2006-2010 American Community Survey: % of pop. with high school diploma or less, % of pop. with some college, % unemployed, % of pop. in poverty, % of K-12 children enrolled in private schools, and median income. Full count statistics interpolated between 1990-2010 with years after 2010 held constant at 2010 values. Sample statistics interpolated between 1990-2006 with years after 2006 held constant at 2006 values. Optimal subpopulation includes sales with propensity scores in the interval [.078,.922].

** Significant at the 5% level

*** Significant at the 1% level