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 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 has been substantial (and controversial) variation from the original program's design, there is no direct evidence on how outcomes vary along with these design choices. Using data from multiple Promise sites, we adopt a difference-in-difference approach to compare the evolution of both school enrollment and residential real estate prices around the announcement of these programs within affected Promise zones and in surrounding areas. Taken together, our estimates suggest that these scholarships have important distributional effects that bear further examination. In particular, while estimates indicate that public school enrollment increases in Promise zones

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relative to 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 primary 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 already advantaged 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 (Miron and Evergreen, 2008a). 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]

[1]See 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

Encouraged by these early returns, many organizations have implemented similar programs in 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 amount of the scholarship award itself. While research on the Kalamazoo program has described its impact on various outcomes of interest, this work applies to 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 from multiple Promise programs, we also begin to document the heterogeneity of Promise 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 also 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.

We find that, on average, the announcement of a Promise program in a school

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.

district increases total public school enrollment by roughly 4%. In addition, this increase is driven almost entirely by primary school enrollment. Since it is common in Promise programs to offer escalating benefits for students first enrolling 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 the least restrictive programs— offering scholarships usable at a wide range of schools with no achievement requirements— provide the largest immediate boosts in total enrollment. In addition, certain features of Promise programs have differential effects across racial subgroups. We find that attaching merit requirements to a Promise scholarship yields 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 experienced a 7% to 12% increase *on average* in housing prices relative to the region immediately surrounding the Promise zone, reflecting capitalization into housing prices of the scholarship and its associated effects on the community.[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. It is also that the change in peer composition and the increased tax base that result from increased demand amongst high-income, white households may have

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

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.

Finally, for two Promise programs located in major metropolitan areas— Pittsburgh 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 primary 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 not surprising 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.

The following section will describe the relevant literature as well as the general structure of the Promise programs being analyzed. Section 3 will describe the data and the empirical methodology used to estimate the impact of the program on public school enrollment and housing prices. Section 4 will be divided in to 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 informing policy, our findings contribute to two different strands of literature. First among these is the substantial body of work regarding the provision of financial aid.[3] 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 also finds that existing estimates of the relationship between income and the impact of aid 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 outcomes such as regional employment, wages, population, and housing markets. The authors demonstrate significant agglomeration effects on these out-

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

comes, suggesting the potential for policies aimed at redistributing population across space to have aggregate welfare implications. The caveat is that 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 overall message is not optimistic. 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. It is notable that most of the research on these place-based 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 leveraged by researchers studying 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

The W.E. Upjohn Institute for Employment Research has identified 23 "Promise-type" scholarships (plus the Kalamazoo Promise itself), which are characterized as "universal or near-universal, place-based scholarship program[s]."[4] These programs are listed in Appendix Table A1 along with some other details of the programs themselves.[5]

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 collection of schools) for several years prior to graduation in order 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, many Promise programs have additional eligibility requirements. Minimum GPA requirements, minimum attendance requirements, and community service requirements are

[4]See http://www.upjohninst.org/Research/SpecialTopics/KalamazooPromise. Further research revealed an additional Promise program in Buffalo, NY.

[5]All information is based on a review of each program's website. Of the programs detailed in Appendix Table A1, two are excluded from the subsequent analysis. The reasons for these exclusions are discussed in detail in the following section.

[6]While 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.

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 Pubilc Schools experienced increases in enrollment without significant changes in the ethnic, racial, or socioeconomic composition of its schools. Without an accompanying analysis of non-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 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 from one year (Ventura College Promise) to five years (El Dorado Promise and Denver Scholarship Foundation). 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 postsecondary 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. Naturally, scholarships that allow use at schools with higher tuitions are potentially more valuable to their recipients, whereas scholarships that allow use only at local junior and community colleges cap the benefit of the scholarship to full tuition at one particular school. In addition, variation in price points and selectivity within the list of eligible schools make the distribution of potential benefits more equal across low-income and high-income households.

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 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.

[7]We 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.

[8]See 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.

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. Due to their promotional nature, however, these reports may be less than objective. 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 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 treats the announcement of a Promise program in a region as a natural experiment, relying on the assumption that each announcement was unexpected. To support this assumption, we conducted substantial research into the timing of program announcements in each area that we study. For every program included in the analysis, we were able to contact staff within the organization responsible to establish an announcement date. We also conducted independent online research aimed at finding announcement press releases, which were used to corroborate the dates provided by the organizations. Still, announcement dates may be measured with error. Provided the error is distributed symmetrically around the true announcement date, any bias resulting from measurement error should serve to attenuate our estimates of the true effect of these programs.

Only two programs listed in Appendix Table A1 were excluded from the analysis: the Muskegon Opportunity Scholarship (Muskegon, MI) and the Detroit College Promise (Detroit, MI). The Muskegon Opportunity Scholarship was eliminated because, although the program has been announced, it was still in the preliminary planning phase as of the time of this writing. As a result, there is considerable uncertainty regarding when funding will become available for students.

The reasons for the exclusion of the Detroit College Promise 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.[9] This small award is due to the lack of sponsorship for the Detroit Promise; as of June 13, 2013, there was only one donor that contributed over $50,000 to the Detroit Promise. 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

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

city on the verge of bankruptcy is likely to overshadow any small positive impact on enrollment or 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 decadal 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 unrelated factors, we believe Detroit to be non-representative 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 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 2011-2012 survey year.[10] This yielded a total of 1.3 million school-year observations. This data was then combined with information on which schools' students were eligible for Promise scholarships and the years in which the programs were announced.

Ultimately, the goal is to estimate the change in enrollments resulting from the announcement of the 23 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 counts changes substantially if similar but unaffected schools experienced increases just as large, for example. As such, we use a difference-in-differences (DD) approach to identify the causal impact of Promise program announcement. We estimate variations of the following fixed-effects regression

$$Y_{it} = \alpha + \beta Post_{it} \cdot Promise_i + \eta_{it} + \delta_i + \varepsilon_{it}, \tag{1}$$

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, η_{it} is a vector of region-by-year and urbanicity-by-year fixed effects, and δ_i are school fixed effects.

[10]Only one program— Say Yes Buffalo (Buffalo, NY)— was announced recently enough that no post-announcement data is yet available, although several programs have only one year of post-announcement data. The pre-announcement data for all 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.

Standard errors in all specifications are clustered at the school district level to allow for correlation in ε_{it} within school districts over time.

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

$$Y_{it} = \alpha + \sum_{J \in \{M,NM\}} \sum_{K \in \{W,NW\}} \beta_{JK} Post_{it} \cdot J_i \cdot K_i + \eta_{it} + \delta_i + \varepsilon_{it} \tag{2}$$

yielding four coefficients— $\beta_{MW}, \beta_{NMW}, \beta_{MNW}$, 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 wide (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 equations 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 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.[11] This restriction results in our baseline estimation sample of 52,163 school-year observations across 98 U.S. counties and 994 school districts. Table 1 presents the summary statistics for the sample of treated and untreated schools across all years in the sample.

[Table 1 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 more white students as a fraction of the total students. Differences in the distribution of schools across levels are very similar, although 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.

Bear in mind, our empirical strategy does not explicitly rely on Promise schools being similar to comparison schools. Given some initial level of dissimilarity, 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 still 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. 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 show that the evolution of enrollment in the periods immediately prior to Promise

[11] Relaxing this restriction only slightly changes the estimated coefficients.

announcement was similar between Promise zone schools and control 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, the time variation in announcements and the geographic spread of the programs makes it unlikely that any shock would have occurred in all Promise zones at the time of announcements, much less a shock that would differentially impact Promise zones relative to their immediate surroundings.

3.2. Housing Prices

Our housing price data come 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.[12] Finally, the latitude and longitude of each property is also included.

The exact 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

[12]Note that not all variables are recorded across all jurisdictions. Most jurisdictions 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 quadratic in building age in specifications where structural characteristics are included. These characteristics were the only variables that were reliably recorded across all jurisdictions studied.

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 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 permits the identification of properties that 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.[13]

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

[13]For only six of these eight sites 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 were 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/. The data acquired in this way are comparable to those supplied by DataQuick in terms of the scope of content.

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.[14]

In practice, however, identifying the causal impact on housing prices 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.[15] 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,

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

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

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 DD 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:

$$Price_{imdt} = \alpha + \beta Post_{mt} \cdot Promise_d + \sum_{m \in M} \mathbf{X'_{it}} \cdot \gamma_{\mathbf{m}} + \eta_{mt} + \delta_d + \varepsilon_{imdt}, \qquad (3)$$

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 m, $Promise_d$ 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. The implicit prices of structural characteristics and neighborhood demographics are allowed to vary across markets, as reflected by $\gamma_{\mathbf{m}}$. 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 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 are clustered at the 2010 Census tract level to allow for correlation in ε_{imdt} for properties within the same neighborhood over time. Here, 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 will 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 usually 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 by construction unaffected by the treatment. 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.[16]

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 lin-

[16] As 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.

ear functional form with appropriately deflated transactions prices as the dependent variable. As all Promise scholarships are per-student subsidies and not 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 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 attenuated 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 concern 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 statistically. 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:

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

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.[17] Recovering the associated parameters, we go on calculate 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.[18] Equation 3 is then estimated

[17]As 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 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, is seems unlikely that floor area would respond to Promise announcement in any systematic or meaningful way.

[18]The optimal bounds of the propensity score distribution were calculated according to Crump

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 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

et al. (2009). We thank Oscar Mitnik for sharing the code for the procedure on his website.

with school quality by estimating variations of the following equation in each market definition:

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

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

[Table 2 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, respectively). 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 in the housing sample 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, residents of Promise districts are more likely to enroll K-12 students 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. 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.[19] 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]

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, there is a slight but clear convergence in enrollment between groups, primarily driven by an upturn in the Promise group's enrollment trend. We attribute this convergence to increased demand for public schools following the announcement of a Promise program. The gradual nature of the convergence is understandable given the nature of the market for public

[19] If 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.

schooling. A household anticipating the future enrollment of a kindergarten or first grade student might respond immediately, but we would not see a corresponding increase in enrollment until their child became school-aged. In what follows, we present some evidence consistent with this view; specifically, that much of the enrollment response is driven by primary school enrollments and in a related market without this feature (housing) we see a more pronounced immediate response.

Table 3 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 3 about here]

As predicted, when enrollment in a particular set of schools gains a student access to a scholarship, more students will enroll in those schools. The announcement of a Promise program leads to an increase in overall enrollment of roughly 3.9%. Across racial groups, increases in total enrollment seem to be driven by increases in white enrollment, although the effects are not significant when decomposed in this way.[20]

It is typical for Promise programs to scale up scholarship amounts with the length of continuous enrollment at graduation making the scholarship more valuable to students who begin their enrollment at early grade levels.[21] Also, students who first enroll past grade 9 or 10 are excluded from most Promise scholarships. As a result, we would expect much of the new enrollment over the initial years of a Promise program to occur in the earlier grade levels, especially in those programs that feature a sliding scale. Table 4 depicts the treatment effect as estimated for each school level (primary, middle, and high) separately.

[20] Analysis by income group was attempted, but we discovered that tallies of free- and reduced-price lunch eligible students were less reliably reported than tallies by race/ethnicity.

[21] Note that this statement pertains to the amount *when received* not the present value when an enrollment decision is made. In particular, it does not account for the time lag in use of the scholarship or the appreciation of tuition prices over time.

[Table 4 about here]

The estimated increases in enrollment in Promise districts match the predicted pattern, with significant increases in enrollment at the primary grade levels (K-5), followed by smaller (and statistically insignificant) increases through middle (6-8) and high school (9-12). The exception is in 9th grade where enrollment also increases, most likely because this is the latest one can first enroll and still remain eligible for Promise funding in most programs. Furthermore, as shown in panel B, the pattern is more pronounced amongst those programs featuring a sliding scale relative to those which lack this feature. The match between the enrollment incentives provided by Promise scholarships and the estimated 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 3 the effects of Promise programs are analyzed by sub-group. The overall effect is masks heterogeneity across programs of different types. In addition, the variation is consistent with the expected effect of program features on the scholarship's prospective value. 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 *ex ante*. 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 (9.7%) followed by merit-based, wide-list programs (4.9%) and universal, narrow-list progams (3.5%). Programs offering merit-based scholarships usable at a small list of schools have no statistically significant 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 decreases in non-white enrollment, although the decomposed effects are not always individually significant. 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. Finally, the increase in total enrollment in schools offering universal scholarships usable at a small list of schools favors the enrollment of non-white students.

Although addressed in previous work on the Kalamazoo Promise (Hershbein, 2013), it remains to be seen whether the increases in Promise district enrollment reported here are driven primarily by sorting of public school students across districts or sorting of students between private and public schools. The aforementioned paper utilized microdata which contained the originating school of new Kalamazoo Public School students in order to determine that only a small percentage of new enrollees after the Promise announcement were coming from private schools while the bulk were coming from nearby public schools. Our DD analysis is uninformative on this question, as it only shows that public schools inside Promise zones are gaining students *relative to* public schools nearby. This result is almost certainly driven to some extent by the migration of public school students across the Promise zone border, causing an increase in Promise district enrollments and a corresponding decrease outside the Promise district.

If the Promise generates significant migration across the border, adjacent school districts should be thought of as "treated" with an opposing intervention. This suggests an alternative approach that uses districts within the same *state* as a Promise program (but plausibly outside of the program's sphere of influence) as a control group for both the Promise district and the districts immediately adjacent. As these schools are further from the Promise zone, they are less suited to serve as a counterfactual according to Tobler's first law of geography: "Everything is related to everything else, but near things are more related than distant things."(Tobler, 1970) Still, this exercise may assuage these concerns of "double-counting" the positive and negative treatment effects raised by the above DD estimates. In table 5, we report the results of this alternative approach.

[Table 5 about here]

It is clear that areas nearby Promise programs suffered losses in overall enrollment that were *not* suffered by the Promise districts themselves. Across grade levels, the story remains qualitatively similar: Promise school districts either lost fewer or gained more students than adjacent school districts relative to unaffected school districts. Of course, this information was already contained in the DD estimates reported in tables 3 and 4. In fact, the differences in the coefficients in table 3, although not directly analogous, conform roughly to the estimates reported previously. Given that baseline enrollment in the exterior schools in this sample is higher, the flows out of the nearby public schools more than account for the increase in enrollment in the Promise districts themselves, although this hardly means that the documented net inflows in Promise districts were entirely driven by sorting across these district borders. As our data are insufficient for the task, we leave an accounting exercise specifically designed to track and decompose student flows to future research.[22]

[22]While we attempted to investigate the question of private school enrollment, we ran into a number of issues in the analysis. First, while there is a NCES product similar to the Common Core of Data for private schools, it is biennial and not annual, leading to a much smaller sample.

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 4, 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 non-parametric, local linear fit on either side of the announcement date.

[Figure 4 about here]

Clearly in the context of the large housing market definition, any impact of program announcement on housing prices in Promise areas is difficult to detect. While the difference between groups narrows slightly after the program announcement, the two series diverge again to pre-Promise levels within about 2 years. As mentioned previously, however, inference from this sample is subject to attenuation bias due to the inclusion of properties in the periphery that may not be affected by the Promise.

Further, in constructing a DD analysis of private school enrollments, it is unclear which schools to use as a control group, as there is no "bright line" border separating treated schools from untreated schools. We report the results of two exercises in Appendix Table A2. The first leverages only time-series variation in enrollments at private schools close to the 23 studied Promise zones, while the second mimics the DD design used on the public school data using private schools physically located within the Promise district as the treated group and those outside as the control group. The lack of consistency and precision in the coefficients could be indicative that the effect is small, consistent with the findings of Hershbein (2013). However, it is just as likely that the results are due to the insufficiency of the data to address the question and the lack of an ideal design.

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 6 presents the results from our estimation of equation 3. Each panel corresponds to a different housing market definition. The specification in column 1 includes only school district and market-specific time fixed effects. Of the DD 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 (where coefficients are allowed to vary by housing market) 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 7 using price in constant 1990 dollars as the dependent variable

[Tables 6 and 7 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 $5,700 and $8,000. Our preferred specifications use either the small geographic housing market or propensity score screened optimal subpopulation and include property fixed effects. These specifications provide very precise treatment effects of between 6.8% and 12.1% of home values or $15,000 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.

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 as if each had occurred prior to the relevant Promise announcement. We then repeat the DD analysis above, but separately for the properties above the median and below the median of the distribution generated by the first step. The estimates from the property fixed effects specification (equivalent to column 3 in Table 6) are depicted in figure 5. Across estimation samples, the capitalization of Promise programs into housing prices increases across the housing price distribution. Capitalization effects below the median range from 3.4% to 4.5% compared to capitalization above the median of between 10.2%

and 16.1%.

[Figure 5 about here]

There are several reasons why high-income households may be willing to pay more to gain access to Promise scholarship programs. Students from higher income households are more likely to attend college. 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 high-income families to occupy higher priced homes, the results from our regressions provide 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 high-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.

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 8 reports estimates of the treatment effect within each market, identifying from repeat-sales as in column 3 of table 6.

[Table 8 about here]

Both programs display large treatment effects across all samples, ranging from 15% to 22% in the Pittsburgh market and 6% to 10% 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 correlates 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 since 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 9 contains the results from estimating equation 5.

[Table 9 about here]

It appears that, while high school quality is not associated with larger capitalization effects in these citites, primary school quality is strongly associated with Promise program capitalization. Across Pittsburgh and Denver, 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 3% and 8% (or $6,000 and $14,000).

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, as mentioned previously, the incentives provided by many Promise programs (including the Pittsburgh Promise) are strongest for primary school students, making primary school quality 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.[23]

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.

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

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. 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, if the increase in home values means 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 a fertile 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 to the remaining Promise programs, generalizing the house price effects of Promise programs beyond our sample of eight programs and adding variation in program features to the housing market analysis. 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 (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. If the signal is stronger for high-income students than low-income students— perhaps due to uncertainty surrounding additional merit requirements or variation in demand elasticity across income groups— documenting such an effect would have distributional implications as well.

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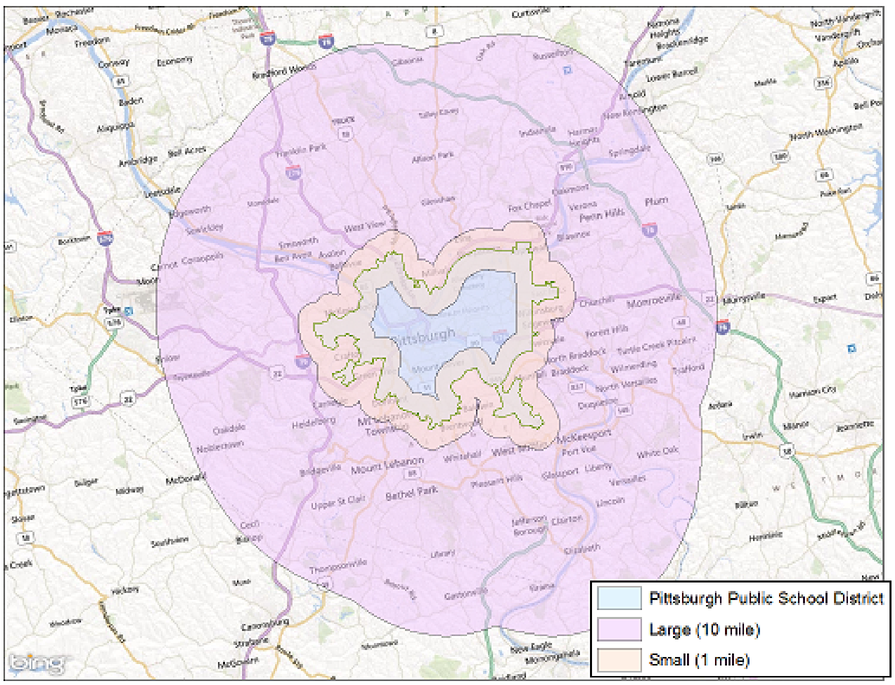

Figure 1: Large (10 mile) and Small (1 mile) Housing Markets in Pittsburgh, PA

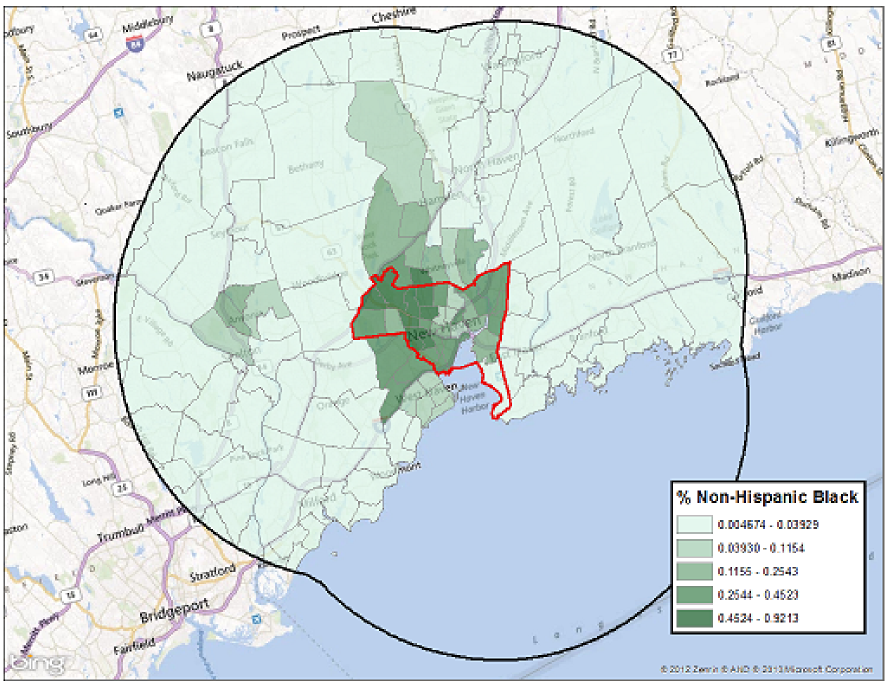

Figure 2: Percent Non-Hispanic Black (2000) by Census Tract

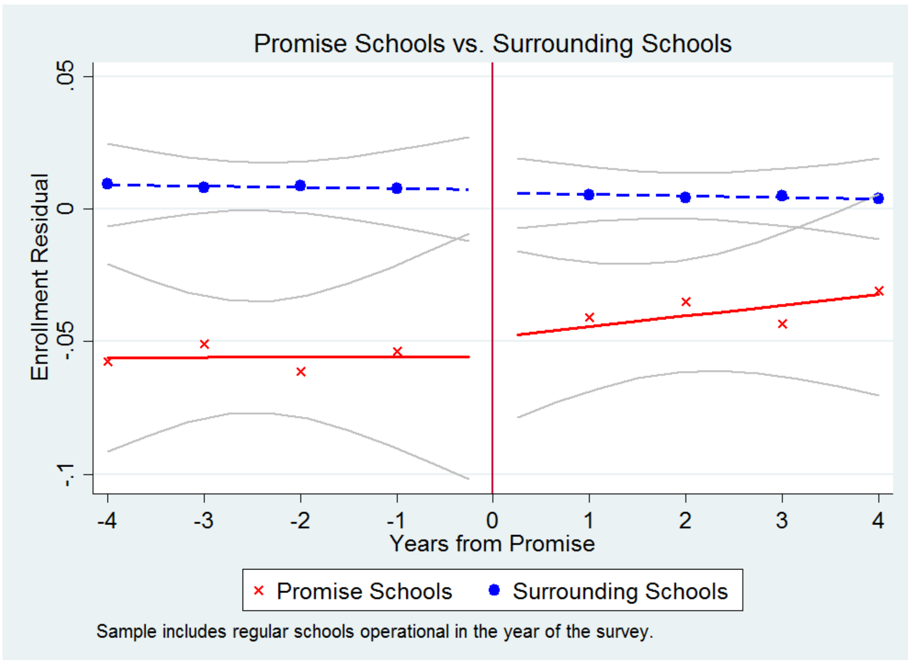

Figure 3: Total Enrollment Residual by Year

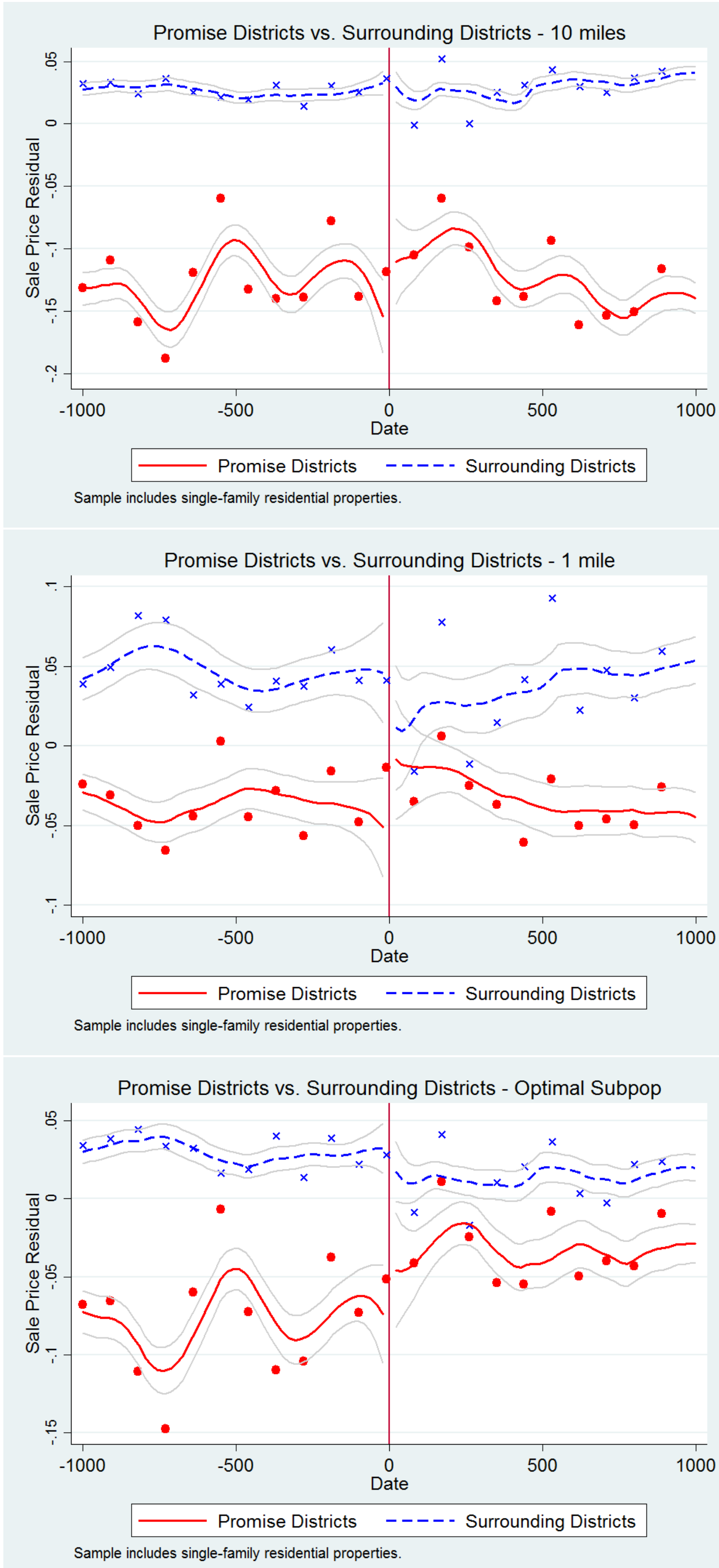

Figure 4: Sale Price Residuals by Date

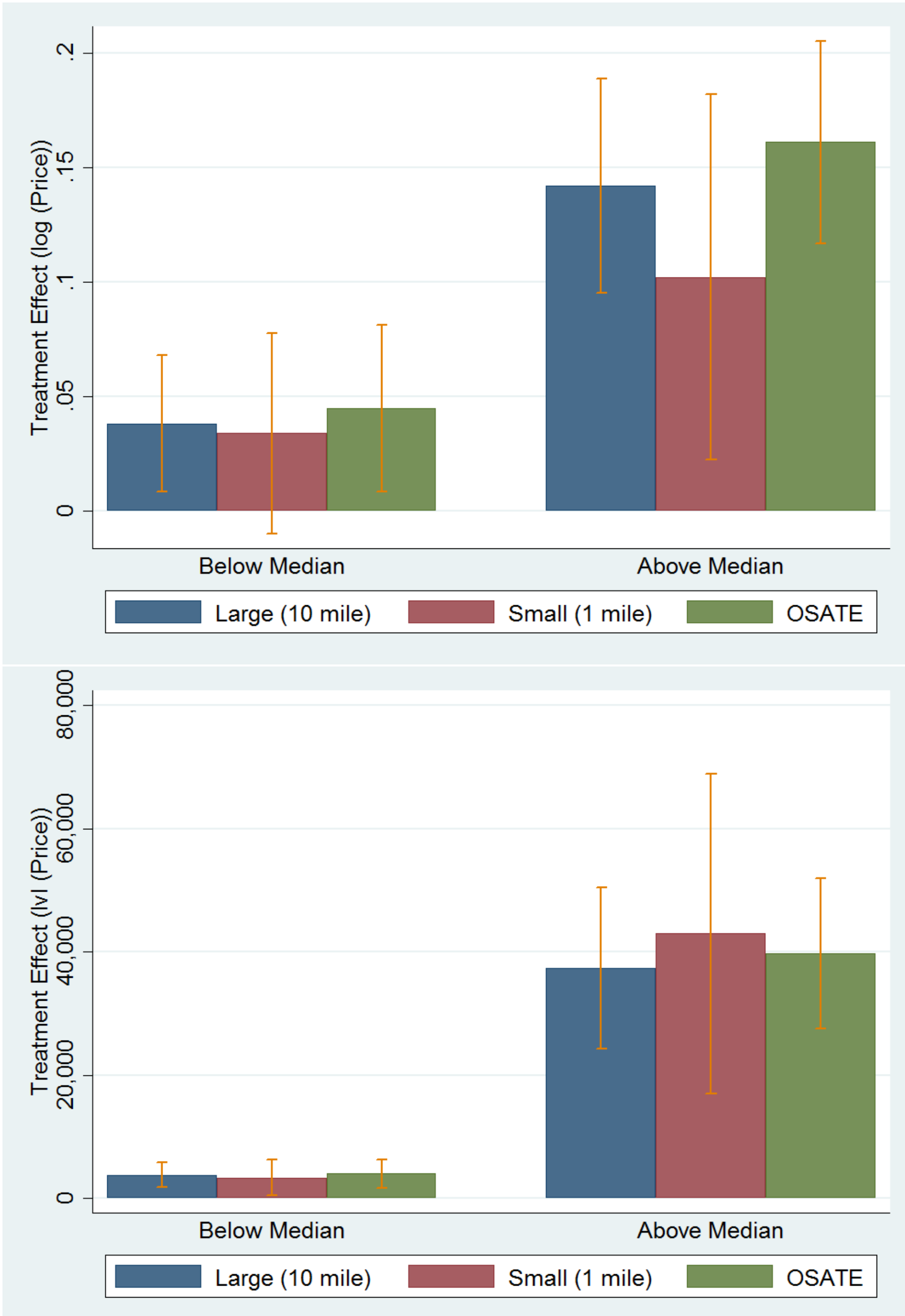

Figure 5: Treatment Effect by Above/Below Median

Table 1: K-12 Public School Summary Statistics

		Promise Schools	Control Schools	t-stat
Total Enrollment	mean	587,04	729.95	24.02
	(s.d.)	(415.92)	(606.72)	
% White	mean	0.49	0.46	-7.35
	(s.d.)	(0.33)	(0.36)	
Primary	mean	0.66	0.67	1.68
	(s.d.)	(0.47)	(0.47)	
Middle	mean	0.17	0.16	-1.04
	(s.d.)	(0.38)	(0.37)	
High	mean	0.15	0.14	-0.68
	(s.d.)	(0.35)	(0.35)	
City	mean	0.53	0.37	-24.02
	(s.d.)	(0.50)	(0.48)	
Suburb	mean	0.24	0.46	36.03
	(s.d.)	(0.43)	(0.50)	
Town	mean	0.06	0.04	-5.68
	(s.d.)	(0.24)	(0.20)	
Rural	mean	0.16	0.13	-6.89
	(s.d.)	(0.37)	(0.34)	
Obs.		6,323	45,840	

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

Table 2: 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	220,145	219,979	-0.25	214,259	189,931	-21.70	216,387	189,518	-35.22
	(s.d.)	(190,736)	(143,537)		(191,047)	(161,858)		(191,405)	(136,332)	
	Obs.	95,976	418,105		55,301	43,927		77,078	174,287	
Price (1990 dollars)	mean	132,004	134,260	5.82	127,049	114,564	-18.73	130,065	114,014	-35.55
	(s.d.)	(112,920)	(86,055)		(113,382)	(96,480)		(113,219)	(80,900)	
	Obs.	95,976	418,105		55,301	43,927		77,078	174,287	
Building age	mean	48.37	26.12	-175.41	45.00	38.72	-31.08	51.64	37.95	-94.38
	(s.d.)	(36.85)	(26.81)		(32.80)	(30.11)		(35.26)	(29.25)	
	Obs.	94,978	401,725		54,890	43,084		77,078	174,287	
Floor area (sq. feet)	mean	1,595.88	1,820.93	87.50	1,573.91	1,598.72	5.49	1,540.34	1,578.01	12.90
	(s.d.)	(710.96)	(750.85)		(723.15)	(693.57)		(689.00)	(642.57)	
	Obs.	95,976	418,105		55,301	43,927		77,078	174,287	
% Black	mean	0.14	0.11	-49.21	0.16	0.09	-65.83	0.12	0.15	33.35
	(s.d.)	(0.17)	(0.22)		(0.17)	(0.14)		(0.17)	(0.25)	
	Obs.	94,773	413,236		54,110	42,418		77,078	174,287	
% under 15	mean	0.20	0.24	128.25	0.21	0.20	-24.63	0.20	0.21	46.59
	(s.d.)	(0.07)	(0.06)		(0.07)	(0.07)		(0.06)	(0.05)	
	Obs.	94,773	413,236		54,110	42,418		77,078	174,287	
% over 60	mean	0.17	0.16	-29.88	0.16	0.21	65.76	0.19	0.19	3.48
	(s.d.)	(0.11)	(0.11)		(0.09)	(0.15)		(0.10)	(0.10)	
	Obs.	94,773	413,236		54,110	42,418		77,078	174,287	
% Households with children	mean	0.32	0.40	172.02	0.34	0.32	-22.07	0.31	0.34	75.78
	(s.d.)	(0.13)	(0.11)		(0.13)	(0.12)		(0.11)	(0.10)	
	Obs.	94,773	413,236		54,110	42,418		77,078	174,287	

Table 2: Housing Market Summary Statistics

		Large (10 mile)			Small (1 mile)			Optimal Subpop.		
		Promise	Control	t-stat	Promise	Control	t-stat	Promise	Control	t-stat
% HS diploma	mean	0.40	0.34	-86.98	0.42	0.41	-3.22	0.40	0.43	29.98
	(s.d.)	(0.19)	(0.16)		(0.18)	(0.16)		(0.20)	(0.16)	
	Obs.	95,078	414,626		54,408	42,418		77,072	174,287	
% College	mean	0.34	0.34	4.68	0.32	0.29	-25.98	0.34	0.28	-71.65
	(s.d.)	(0.21)	(0.17)		(0.20)	(0.16)		(0.21)	(0.16)	
	Obs.	95,078	414,626		54,408	42,418		77,072	174,287	
% unemployed	mean	0.08	0.06	-87.50	0.08	0.07	-23.49	0.08	0.08	6.72
	(s.d.)	(0.04)	(0.05)		(0.04)	(0.04)		(0.05)	(0.05)	
	Obs.	94,176	414,626		53,506	42,418		77,072	174,287	
% in poverty	mean	0.16	0.08	-215.27	0.15	0.11	-65.03	0.16	0.12	-89.97
	(s.d.)	(0.11)	(0.08)		(0.10)	(0.09)		(0.10)	(0.09)	
	Obs.	94,176	414,626		53,506	42,418		77,072	174,287	
% K-12 private	mean	0.18	0.13	-99.77	0.18	0.14	-51.44	0.18	0.14	-59.64
	(s.d.)	(0.17)	(0.09)		(0.16)	(0.11)		(0.16)	(0.12)	
	Obs.	94,535	413,360		54,403	41,745		76,534	173,740	
Median income	mean	51,507	68,359	207.47	52,522	54,969	15.82	50,633	53,513	33.86
	(s.d.)	(21,839)	(25,216)		(22,972)	(24,405)		(20,411)	(17,849)	
	Obs.	94,174	414,626		53,504	42,418		77,071	174,287	

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 3: K-12 Public School Enrollment Effects of Promise Programs

Dependent Variable:	log(Total)	log(White)	log(Non-white)
Panel A: Overall effects			
PromiseXPost	0.039***	0.030	-0.005
	(0.011)	(0.037)	(0.026)
Panel B: Effects by type			
No Merit & Wide	0.097***	0.008	0.008
(113 schools)	(0.021)	(0.040)	(0.045)
Merit & Wide	0.049***	0.134	-0.057**
(200 schools)	(0.010)	(0.090)	(0.030)
No Merit & No Wide	0.035**	-0.018	0.055*
(420 schools)	(0.015)	(0.035)	(0.033)
Merit & No Wide	-0.011	0.053*	-0.228***
(87 schools)	(0.026)	(0.032)	(0.079)
Observations	52,163	52,163	52,163
Clusters (Districts)	994	994	994
R-squared	0.97	0.98	0.98

Notes: Standard errors clustered at the school district 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.

[*] Significant at the 10% level

[**] Significant at the 5% level

[***] Significant at the 1% level

Table 4: K-12 Public School Enrollment Effects of Promise Programs, by Grade Level

Dependent Variable:	log(Primary)	log(Middle)	log(High)	log(9th)
Panel A: Overall				
PromiseXPost	0.037***	0.018	0.010	0.055*
	(0.012)	(0.020)	(0.021)	(0.031)
Panel B: Sliding Scale vs. Static				
SlidingXPost	0.060**	0.040	-0.005	0.053
	(0.025)	(0.032)	(0.040)	(0.061)
StaticXPost	0.028**	0.009	0.019	0.056*
	(0.012)	(0.025)	(0.022)	(0.031)
Test: Sliding - Static = 0	0.033	0.031	-0.024	-0.002
	(0.027)	(0.040)	(0.045)	(0.066)
Observations	36,976	25,613	8,712	8,474
Clusters (Districts)	902	920	635	630
R-squared	0.96	0.97	0.99	0.97

Notes: Primary (Middle) [High] School enrollment is equal to the sum of enrollments in grades K-5 (6-8) [9-12]. Standard errors clustered at the school district 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.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 5: Interior vs. Exterior Enrollment Effects of Promise Programs

Dependent Variable:	log(Total)	log(Primary)	log(Middle)	log(High)	log(9th)
InteriorXPost	0.009	0.016	-0.133	0.019	0.050
	(0.008)	(0.011)	(0.118)	(0.019)	(0.035)
ExteriorXPost	-0.022**	-0.010	-0.153	0.014	0.002
	(0.011)	(0.014)	(0.119)	(0.023)	(0.040)
Test: Diff = 0	0.031***	0.026**	0.020	0.005	0.048
	(0.010)	(0.012)	(0.020)	(0.022)	(0.030)
Observations	229,236	153,472	107,196	46,853	45,543
Clusters (Districts)	5,337	5,002	5,094	4,051	4,025
R-squared	0.97	0.97	0.97	0.99	0.97

Notes: Primary (Middle) [High] School enrollment is equal to the sum of enrollments in grades K-5 (6-8) [9-12]. Standard errors clustered at the school district level in parentheses. Sample includes open, regular schools located in states affected by Promise programs that reported student counts by race in all available surveys conducted within 4 years of the relevant Promise announcement. Fixed effects at the region-by-year, locale-by-year, and school level are included in all specifications.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table 6: Capitalization Effects of Promise Programs

Dependent Variable: log(Price)	(1)	(2)	(3)
Panel A: Large (10 mile)			
PromiseXPost	-0.005	0.038***	0.083***
	(0.017)	(0.012)	(0.022)
Observations	514,081	487,963	505,291
Clusters	2,053	2,007	2,037
R-squared	0.38	0.69	0.92
Panel B: Small (1 mile)			
PromiseXPost	-0.007	0.043***	0.068***
	(0.021)	(0.016)	(0.025)
Observations	99,228	93,730	94,941
Clusters	606	594	598
R-squared	0.41	0.72	0.93
Panel C: Optimal subpopulation			
PromiseXPost	-0.029	0.060***	0.121***
	(0.018)	(0.013)	(0.020)
Observations	251,365	250,273	250,273
Clusters	1,464	1,460	1,460
R-squared	0.38	0.67	0.92
Controls	NO	ALL	DEMO
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 2010 Census tract level 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 demographic controls include the following tract-level statistics interpolated from the Census full-count data or the American Community Survey: % black, % under 15/over 60, % of households with children under 18, % with high school diploma or less, % with some college, % unemployed, % 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: Capitalization Effects of Promise Programs

Dependent Variable: Price ($1990)	(1)	(2)	(3)
Panel A: Large (10 mile)			
PromiseXPost	-118.9	7,453***	18,108***
	(2,218)	(1,725)	(4,112)
Observations	514,081	487,963	505,291
Clusters	2,053	2,007	2,037
R-squared	0.25	0.72	0.94
Panel B: Small (1 mile)			
PromiseXPost	-2,119	5,707***	15,098***
	(2,805)	(1,805)	(4,251)
Observations	99,228	93,730	94,941
Clusters	606	594	598
R-squared	0.24	0.75	0.94
Panel C: Optimal subpopulation			
PromiseXPost	-2,604	8,272***	20,575***
	(2,285)	(1,649)	(3,319)
Observations	251,365	250,273	250,273
Clusters	1,464	1,460	1,460
R-squared	0.27	0.71	0.95
Controls	NO	ALL	DEMO
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 2010 Census tract level 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 demographic controls include the following tract-level statistics interpolated from the Census full-count data or the American Community Survey: % black, % under 15/over 60, % of households with children under 18, % with high school diploma or less, % with some college, % unemployed, % 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 8: Large Metropolitan Promise Programs

	Pittsburgh		Denver	
	log(Price)	Price ($1990)	log(Price)	Price ($1990)
Panel A: Large (10 mile)				
PromiseXPost	0.218***	13,457***	0.104***	25,016***
	(0.052)	(3,112)	(0.028)	(5,555)
Observations	52,716	52,716	221,198	221,198
Clusters	378	378	531	531
R-squared	0.91	0.95	0.89	0.94
Panel B: Small (1 mile)				
PromiseXPost	0.147**	8,641**	0.071**	18,940***
	(0.067)	(3,590)	(0.032)	(5,902)
Observations	14,474	14,474	49,445	49,445
Clusters	164	164	172	172
R-squared	0.90	0.96	0.90	0.93
Panel C: Optimal sub.				
PromiseXPost	0.155**	8,062***	0.062**	6,407***
	(0.074)	(2,703)	(0.030)	(4,352)
Observations	13,517	13,517	48,690	48,690
Clusters	191	191	253	253
R-squared	0.91	0.97	0.86	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 2010 Census tract level in parentheses. Sample includes arms-length transactions of owner-occupied single family homes. Census demographic controls include the following tract-level statistics interpolated from the Census full-count data or the American Community Survey: % black, % under 15/over 60, % of households with children under 18, % with high school diploma or less, % with some college, % unemployed, % in poverty, % of K-12 children enrolled in private schools, and median income. Optimal subpopulation includes sales with propensity scores in the interval [.091,.909] for Pittsburgh and [.085,.915] for Denver.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 9: Large Metropolitan Promise Programs by School Quality

	HS Math		HS Reading		Prim. Math		Prim. Reading	
	log(Price)	$1990	log(Price)	$1990	log(Price)	$1990	log(Price)	$1990
Panel A: Large (10 mile)								
Promise x Post x	-0.035*	-8,859***	-0.017	-6,002***	0.074***	13,089***	0.053***	8,723***
Quality	(0.021)	(3,364)	(0.015)	(2,434)	(0.016)	(3,228)	(0.013)	(2,265)
N (Clusters)	273,914 (909)				269,647 (903)			
R-squared	0.92	0.95	0.92	0.95	0.92	0.95	0.92	0.95
Panel B: Small (1 mile)								
Promise x Post x	0.016	1,506	0.035*	3,365	0.084***	14,434***	0.071***	12,463***
Quality	(0.022)	(3,486)	(0.018)	(2,549)	(0.017)	(3,051)	(0.015)	(2,678)
N (Clusters)	63,919 (336)				60,988 (335)			
R-squared	0.93	0.94	0.93	0.94	0.93	0.94	0.93	0.94
Panel C: Optimal subpopulation								
Promise x Post x	-0.016	-1,131	-0.001	-14	0.039**	7,394***	0.030**	6,069***
Quality	(0.020)	(2,487)	(0.014)	(1,696)	(0.018)	(2,555)	(0.015)	(2,239)
N (Clusters)	95,453 (652)				91,881 (645)			
R-squared	0.92	0.95	0.92	0.95	0.92	0.95	0.92	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 2010 Census tract 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 demographic controls include the following tract-level statistics interpolated from the Census full-count data or the American Community Survey: % black, % under 15/over 60, % of households with children under 18, % with high school diploma or less, % with some college, % unemployed, % in poverty, % of K-12 children enrolled in private schools, and median income. Optimal subpopulation includes sales with propensity scores in the interval [.078,.922].

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Arkadelphia Promise	Arkadelphia, AR	2010	• 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	2009	• 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 A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Bay Commitment	Bay, MI	2007	• 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	• 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 A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Denver Scholarship Foundation	Denver, CO	2006	• 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	• 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	2011	• Continuous residency within selected counties for 12 mos. prior to graduation • Graduate from any HS	Full tuition (4 semesters)	Northeast State Community College

Table A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
El Dorado Promise	El Dorado, AR	2007	• 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	• 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	• Graduate from selected high schools • 2.5 GPA • 95% attendance	Full tuition (4 semesters)	Hopkinsville Community College

Table A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Jackson Legacy	Jackson County, MI	2006	• 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	• 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	• Graduate from Battle Creek or Lakeview SD • Continuous enrollment since 10th grade	Sliding scale; 31 to 62 credit hours	Kellogg Community College

Table A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Leopard Challenge	Norphlet, AR	2007	• 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	• 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 A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Northport Promise	Northport, MI	2007	• 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	2007	• 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	• 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 A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Promise for the Future	Pinal County, AZ	2001[b]	• 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	2011	• 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	• 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 A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
School Counts Program	Hopkins County, KY	2004	• 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	• 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 A1: List of Promise Type Programs

Name of Program	Location	Announced	Requirements	Award	Eligible Schools
Ventura College Promise	Ventura County, CA	2006	• 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 A2: K-12 Private School Enrollment Effects of Promise Programs, by Grade Level

Dependent Variable:	log(Total)	log(Prim)	log(Middle)	log(High)	log(9th)
Time-series model					
Post-Announcement	0.028	-0.002	0.107	0.103*	0.161*
	(0.021)	(0.033)	(0.101)	(0.061)	(0.071)
Difference-in-differences model					
InteriorXPost	-0.001	0.003	0.016	0.065*	0.103
	(0.025)	(0.027)	(0.041)	(0.039)	(0.063)
Observations	4,788	4,114	4,037	1,251	1,228
Clusters (Districts)	407	388	385	178	176
R-squared	0.97	0.96	0.95	0.98	0.97

Notes: Primary (Middle) [High] School enrollment is equal to the sum of enrollments in grades K-5 (6-8) [9-12]. Standard errors clustered at the (public) school district 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.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level