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Promise scholarship programs as place-making policy: Evidence from school enrollment and housing prices☆

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ABSTRACT

Place-based "Promise" scholarship programs—which guarantee financial aid for qualifying graduates of a school district—have proliferated in recent years. Using data from multiple sites, we compare the evolution of school enrollment and residential real estate prices around program announcement dates within Promise-eligible and surrounding areas. While our estimates indicate that enrollment increased following Promise announcements, merit-based programs generated relative increases in white enrollment. Housing prices respond strongly in neighborhoods with better primary schools and in the upper half of the housing price distribution. We conclude that these programs have important and under-studied distributional considerations.

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 several years of the Kalamazoo Promise, researchers documented a number of encouraging results, including increased public school enrollment, increased academic achievement, reductions in behavioral issues, and increased rates of post-secondary attendance.[1]

Encouraged by these early returns, many organizations have implemented similar programs 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, the amount of the scholarship award itself, and whether or not the aid is provided on a first- or last-dollar basis. While research on the Kalamazoo program has described its impact on various outcomes of interest, this extant 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. One drawback of this broad, multi-program approach is the limits it places on the outcomes we can study. Educational outcomes—high school persistence, high school achievement, postsecondary attendance,

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[1] See Bartik et al. (2010), Bartik and Lachowska (2013), Miller-Adams and Timmeney (2013), Miron et al. (2011), Miller (2017), Andrews et al. (2010), Miller-Adams (2015), Miller-Adams (2009), Miller-Adams (2006), Miron and Evergreen (2008a), Miron and Evergreen (2008b), 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.

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and degree completion—would require the cooperation of many disparate school districts or colleges, as national survey samples are typically underpowered to identify effects in specific regions.[2] Some achievement data is available in the form of standardized test results, but comparing these outcomes across states and over time requires the combination and harmonization of data from dozens of sources. While these efforts are underway, however, we believe that providing estimates of regional development effects has independent value. Early research and media coverage of the Kalamazoo Promise suggests that it is first and foremost a place-making policy, meant to attract and retain high human capital individuals in order to boost the economy. In addition, including housing markets in the analysis allows us to speak to the valuation of this program across different groups by examining the variation in the capitalization effects across different neighborhoods and across the housing price distribution. Such patterns have important implications for the distribution of economic benefits from Promise programs.

We find that, on average, the announcement of a Promise program in a school 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 the percentage of white students and decreases in percentages of black and Hispanic students, 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-eligible areas experienced a 7–12% increase in average housing prices relative to the region immediately surrounding the Promise-eligible area, reflecting capitalization into housing prices of the scholarship and its associated effects on the community.[3] This increase in real estate prices is primarily due to increases in the upper half of the housing quality distribution. These results suggest that the monetized 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. Also, there will be changes in the peer composition and the property tax base in both the Promise district as well as the neighborhoods from which the higher-income, white households move. These may result in significant spillover effects on low-income and minority students in urban Promise districts. Whether such spillovers offset the corresponding adverse peer composition and property tax effects in the suburban districts is unknown; more research is needed to pin down the relative importance of these welfare 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 price effects are focused in areas with already high-achieving schools. This outcome 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, as well as contributing to segregation by income and/or race.

The following section describes the relevant literature as well as the general structure of the Promise programs being analyzed. Section 2 describes the data and the empirical methodology used to estimate the impact of the program on public school enrollment and housing prices. Section 3 is divided into three subsections, the first of which presents the results of the enrollment analysis on the entire sample of Promise programs. The remainder of Section 3 is 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 4 discusses the results and concludes.

1. Background

1.1. Related literature

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. 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 $1000 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.

The second strand of literature to which we contribute concerns research into place-based policies. According to a review 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 outcomes, 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. 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. That said, while there may be some increased productivity due to increased educa-

[2] Many of these outcomes have been studied in the context of particular programs where the data requirements are less onerous. These results are surveyed in the following section.

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

tional attainment, the agglomeration effects on overall welfare are likely to be minimal for the reasons expressed above.

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 college enrollment and degree completion. To summarize the findings, college enrollments increased among middle- and high-income students, but income inequality in college enrollments widened and college persistence was not necessarily increased.[4] It is notable that most of the research the HOPE scholarship and similar 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, since the immediate geographic vicinity of a Promise school district is a more plausible counterfactual than the neighboring states of a statewide scholarship program like the Georgia HOPE scholarship.[5] 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.

1.2. Promise scholarship programs

At the time of this writing, the W.E. Upjohn Institute for Employment Research had identified 23 "Promise-type" scholarships (plus the Kalamazoo Promise itself), which are characterized as "universal or near-universal, place-based scholarship program[s]."[6] These programs are listed in online Appendix A along with additional details of the programs themselves.[7]

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.[8]

As a high profile program in this 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. A series of working papers from both Western Michigan University's Department of Education and the W.E. Upjohn Institute demonstrate the community development potential of these programs. The research to date has documented significant improvement in school climate (Miron et al., 2011), increased enrollment (Hershbein, 2013; Bartik et al., 2010), improved academic achievement (Bartik and Lachowska, 2013), and increased college attendance in certain groups (Miller-Adams and Timmeney, 2013; Bartik et al., 2015).[9] In independent work, Miller (2017) adds a preliminary analysis of home values, finding that the announcement of the Promise had no impact on home prices in Kalamazoo relative to the surrounding area.

Apart from these studies of the Kalamazoo Promise, however, little research has been conducted on Promise programs in order to generalize the findings. Bartik and Sotherland (2015) and Gonzalez et al. (2011) both find evidence that Promise programs reverse outmigration in previously declining school districts. Gonzalez et al. (2011) 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. Also, with a few notable exceptions, the research has been qualitative or descriptive in nature and no work has provided direct evidence of how the impact of a Promise program varies according to its features.

Although the Kalamazoo Promise was universal within its Promise-eligible boundary, many Promise programs have additional eligibility requirements. Minimum GPA requirements, minimum attendance requirements, and community service requirements are common. Previous work has called attention to the variation in eligibility requirements as an important element in program design, but to date no research has empirically investigated the impact of universal vs. merit-based eligibility on program effectiveness in the context of Promise programs. Michelle Miller-Adams' "Promise Nation" makes a strong case for universal eligibility, highlighting some of the attractive attributes of its design, including simplicity and portability as well as its important impacts on the college-going culture of both school and community. She also documents the Kalamazoo Program's success at increasing in enrollment without causing significant changes in the ethnic, racial, or socioeconomic composition of its schools and points to a lack of evidence that merit-based programs are delivering on outcomes that motivate their restrictive requirements, namely improved K-12 achievement and college preparedness. Without a true comparative analysis of universal and merit-based programs, however, it remains unclear what outcomes are attributable to eligibility requirements as opposed to other idiosyncratic features. In addition, some districts' goals may include modifying the demographic composition of area schools, contra the results generated by the Kalamazoo program. The analysis to date provides districts with no guidance regarding what program design choices best suit their goals.

[4] See Deming and Dynarski (2010) for more details on the HOPE scholarships and other similar programs.

[5] As evidenced by Tables 1 and 2, Promise-eligible areas tend to be located in urban school districts, which are often coterminous with city limits and demographically different than the surrounding suburban districts. Our estimation strategy described in Section 2 below tries to account for this both by controlling for observable demographics as well as restricting the sample to units that are similar on observables.

[6] The original W.E. Upjohn Institute website dedicated to Promise-type scholarship programs (dated February 27, 2013) listed 23 such programs. Upjohn researchers continue to identify newly-created Promise-type programs and document previously existing programs. Their database currently includes 85 programs across the United States. See http://upjohn.org/research/education/kalamazoo-promise-place-based-scholarships for details.

[7] All information is based on a review of each program's website which was conducted in 2013. As a result, some details may have changed. Of the programs detailed in the online Appendix, two are excluded from the subsequent analysis. The reasons for these exclusions are discussed in detail in the following section.

[8] While not always defined in terms of school districts, we will use the terms "Promise district" and "Promise-eligible area" interchangeably to refer to the geographical boundaries of a Promise program.

[9] 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 the Kalamazoo Promise program.

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, in period under study the maximum award for the Jackson Legacy scholarship was $600 per year for two years, whereas the maximum scholarship for the Pittsburgh Promise was $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 terms of percentage 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-eligible school. As an example of both, the 2013 Kalamazoo Promise benefit ranged from 65% (first enrolled by 9th grade) to 100% (first enrolled in Kindergarten) 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 program. Some limit that further to public institutions, and many scholarships are only usable at a short list of local colleges. 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.

2. 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 we discovered 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.[10] Second, and more importantly, we believe the precipitous decline of a 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. 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.[11]

We study two main outcomes in relation to Promise Scholarship programs: K-12 public school enrollments and housing prices. Identifying and estimating the impact of the Promise presents a unique set of empirical challenges for each outcome of interest. We 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.

2.1. 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.[12] 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.

Our goal is to estimate the change in enrollments resulting from the announcement of the 23 observed Promise programs. For causal inference, 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

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

[11] The Detroit College Promise is not to be confused with the Detroit Scholarship Fund or the Detroit Promise Zone, which offer last-dollar scholarships to Detroit-area community colleges. These programs are better funded and more generous, but were not announced until April 2013 and started awarding benefits with the class of 2014. As a result, these programs were not included in our analysis.

[12] 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-eligible areas 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.

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 as well. Thus, 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

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

where Y_{it} is some enrollment outcome of interest 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 Promise-eligible schools, η_{it} is a vector of region-by-year and urbanicity-by-year fixed effects, and δ_i are school fixed effects. Standard errors in all specifications are clustered at the school district level to allow for correlation in ε_{it} within school districts over time.

In addition, some results are presented that modify Eq. (1) as follows

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

yielding four coefficients—β_{MW}, β_{NMW}, β_{MNW}, and β_{NMNW}—where M_i indicates a Promise program with a merit-based eligibility requirement, NM_i indicates a universal Promise program, W_i indicates a Promise program with a 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 capture 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 Promise-eligible schools. 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.[13] 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.

Schools initiating Promise scholarship programs are statistically different from those in the geographically proximate control group. Promise-eligible schools in have fewer students overall and more white students as a fraction of the total students. The distribution of schools across levels are very similar, although Promise schools are more likely to be located in urban areas, naturally making the nearby schools in the control group more likely to be in suburban areas.

Of course, 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 cannot be explicitly tested as we do not observe the true counterfactual. However, we will present graphical evidence in support of this assumption. In the following section, we will show that the evolution of enrollment in the periods immediately prior to Promise announcement was similar between Promise-eligible 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 cannot formally 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 districts at the time of announcements, much less a shock that would differentially impact Promise districts relative to their immediate surroundings.

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	

Note: *t*-Statistic from a two-sided *t*-test with unequal variance.

2.2. Housing prices

Housing price data comes primarily from DataQuick Information Systems (now CoreLogic), under a license agreement with the vendor. These data contain transactions histories and characteristics for properties in a large number of U.S. counties. 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.[14] Finally, the latitude and longitude

[13] Relaxing this restriction has no qualitative impact on our results.

[14] 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.

of each property is also included, which allows us to combine property characteristics with tract-level neighborhood demographic data from the U.S. Census and unobserved neighborhood characteristics through the use of fixed effects.

We require a fixed geographical definition of a neighborhood to include neighborhood fixed effects, 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. 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 programs due to data limitations.[15]

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-eligible area. However, unlike with K-12 enrollment data, housing market data gives us an indication of the value of the announcement of the Promise to households. Since we observe the transaction price associated with the residential location decision, we can draw inference on the household's willingness to pay for access to the program. Any increase in housing demand will be capitalized into housing prices, with the degree of the capitalization dictated by the elasticity of the housing supply. In the extreme case of a fixed housing supply in the short run, the increase in demand will be capitalized completely.[16]

We use the hedonic method to model a property's price.[17] 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 (2013) demonstrate how combining this technique with quasi-experimental methods allows the researcher to exploit temporal as well as cross-sectional variation in amenity levels. As above, our estimation strategy employs a DD approach to identify the causal impact of Promise program announcement, which is standard in the quasi-experimental hedonic valuation literature. Our baseline estimating equation is written as follows:

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

where $Price_{imdt}$ is the natural log of the transaction price for property i in market m and school district d at time t, $Post_{mt}$ is an indicator for transactions occurring after the announcement of the Promise program relevant to housing market m, $Promise_d$ is an indicator for properties located in Promise-eligible areas, $\mathbf{X_{it}}$ 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.[18] The implicit prices of structural characteristics and neighborhood demographics are allowed to vary across markets, as reflected by $\boldsymbol{\gamma}_{\mathbf{m}}$. 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. 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 individual properties. Standard errors are clustered at the 2010 Census tract level. 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 Promise programs may increase with household income. Light and Strayer (2000) find that family income and mother's education level increase both the likelihood of college attendance as well as the selectivity of the chosen school, thus making each dollar of the Promise scholarship potentially more valuable to higher-income, higher-educated households. In addition, almost all 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 may be greatest for families with higher incomes. As it is reasonable to expect these higher income families to occupy higher priced domiciles, we test this hypothesis by allowing the treatment effect to vary across the housing price distribution. 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.

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 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

[15] For only six of these eight sites the data originates 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.

[16] Kuminoff and Pope (2014) 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).

[17] For a thorough review of the hedonic method, see Palmquist (2005).

[18] In online Appendix B, we discuss the functional form assumption implicit in Eq. (3) and replicate all housing price estimates using a fully linear functional form. The main results are qualitatively unchanged.

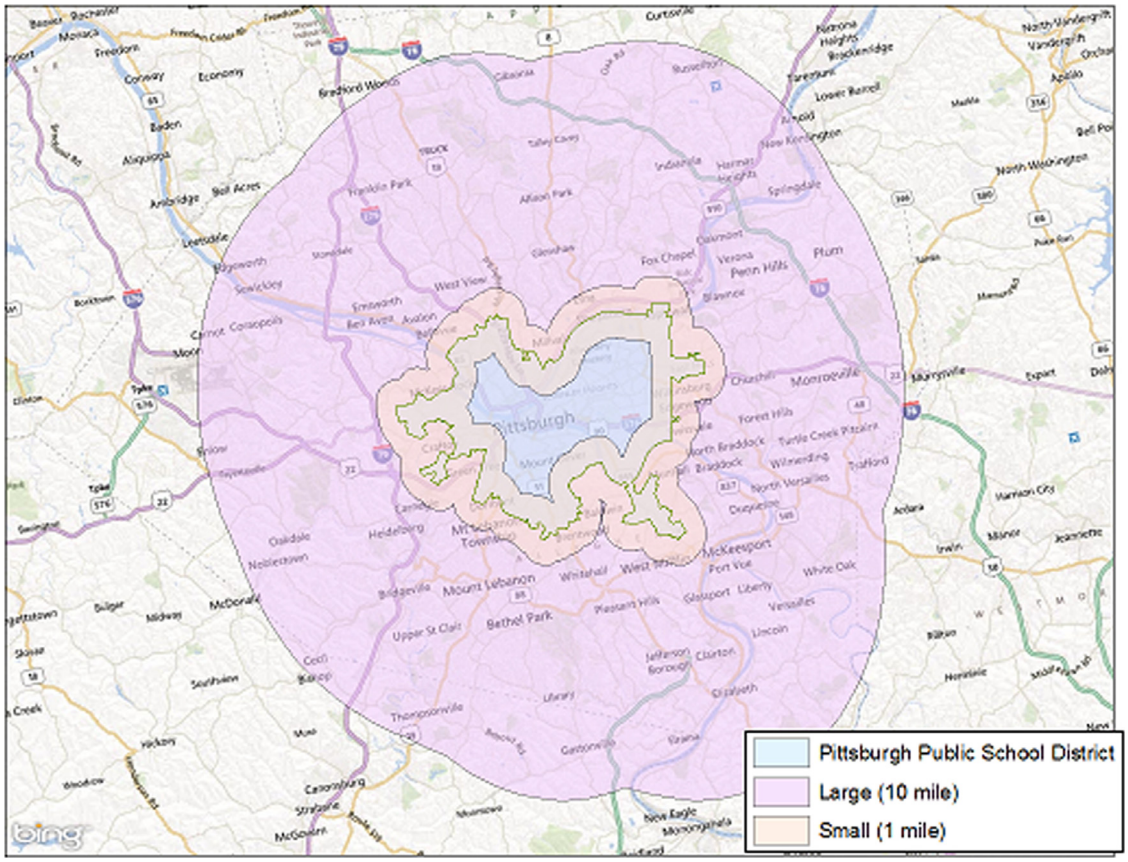

Fig. 1. Large (10 mile) and small (1 mile) housing markets in Pittsburgh, PA.

announcement—as if it had been sold in the first quarter of the year prior to the announcement. This procedure provides a measure of the component of housing value that is by construction unaffected by treatment. All transactions are then sorted on this statistic and grouped into observations above and below the median.[19] The second step repeats the DD analysis specified in Eq. (3) separately for properties above and below the median of the distribution generated by the first step.

Another important consideration is the spatial definition of the relevant housing market. The trade-off between a large geographic market definition and a small geographic market definition is between internal validity of the estimates and the precision with which they are estimated (Parmeter and Pope, 2013). We take a flexible approach by estimating our equation on a number of different samples, each representing a different housing market definition.

Based on 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 market includes all transactions within the Promise-eligible area as well as all transactions occurring within 10 miles of the geographic eligibility boundary of the Promise program. The small market only includes transactions within a 1 mile buffer along both sides of the Promise-eligibility boundary. Fig. 1 depicts an example, using the housing markets constructed around the Pittsburgh Public School District.

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, properties on either side of the treatment boundary may vary significantly in terms of observable characteristics, calling into question their use as a counterfactual for houses within the treatment area and creating a potential lack of overlap in the support of the observable characteristics in control and treatment regions (Crump et al., 2009). By means of example, Fig. 2 depicts the Promise-eligible area 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. Neighborhoods vary considerably across the Promise eligibility border. While this difference in observables can be controlled for econometrically, it raises the question of variation in unobservables.

Thus, our goal for the statistically defined sample is to identify treatment and control groups in a way that reduces these concerns by trimming 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-eligible area based on pre-Promise property characteristics:

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

[19] 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.

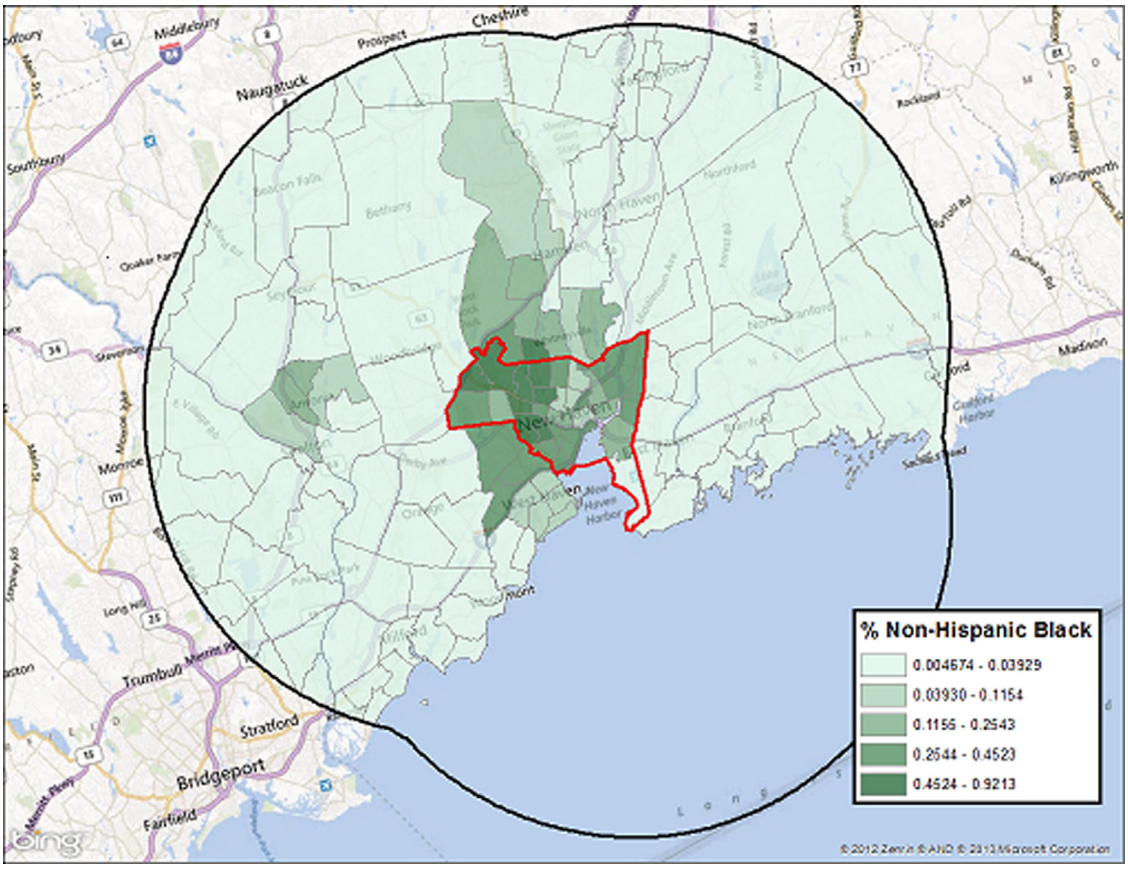

Fig. 2. Percent non-Hispanic Black (2000) by Census tract.

where $\mathbf{X_i}$ is a vector of time-invariant characteristics of property i including floor area (in square feet), year built, and available 2000 U.S. Census demographic information at the tract level.[20] Recovering the associated parameters, we 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.[21] Eq. (3) is then estimated on this sample, producing the Optimal Subpopulation Average Treatment Effect (OSATE).

Finally, we seek to document any heterogeneity in capitalization effects across different types of households and different neighborhoods, to the extent our data allow. As Promise scholarships subsidize post-secondary education, their benefits should be concentrated amongst households with school-aged children. While our transaction data do not include the size of the household, in some markets we have reliable information on the number of bedrooms in the house, which we expect to be correlated with the presence of children. For these markets, we run additional regressions interacting the main effect with an indicator for the presence of 3 or more bedrooms in order to confirm our hypothesis that capitalization effects will be concentrated in such households.

In addition, 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 from nearby areas with high quality schools into Promise districts with lower quality schools, it stands to reason that increases in demand for housing will be concentrated in Promise-eligible neighborhoods associated with the highest quality schools in the Promise district. For two major metropolitan Promise programs—Pittsburgh and Denver—we 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 generate standardized pre-Promise measures of primary school and high school quality for each property in the Pittsburgh and Denver samples. First, we divide the universe of schools on the basis of the highest tested grade level, with schools testing only 8th graders and lower being labeled primary schools and schools testing any students higher than 8th grade being labeled high schools. Then, we calculate the percentage of tested students scoring proficient or better on standardized tests (math and reading) in the universe of public schools in Colorado and Pennsylvania for the year 2005. Finally, within each state by school level cell we standardize this measure such that the resulting variable is a Z-score distributed with mean zero and unit standard deviation.

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

$$Price_{imdt} = \alpha + \beta Quality_i \cdot Promise_d \cdot Post_{dt} + \mathbf{X'_{it}} \cdot \gamma$$

[20] As all Promise programs were announced after the year 2000, there is no endogeneity concern introduced by using Census demographics. For a very small number of observations, floor area varies slightly over time. In these cases, the first observed floor area is used. For all other properties, 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.

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

$$+\eta_t + \delta_d + \varepsilon_{imdt}, \quad (5)$$

where $Quality_{it}$ is one of four standardized pre-Promise measures of 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 up to 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. 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, transaction date more than 1 year prior to building construction) or extreme covariate values (i.e. floor area more than 5000 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 $1000 or greater than $5,000,000.[22]

Table 2 presents the summary statistics for the sample of treated and untreated properties for each housing market definition. As with public school data, our housing market data reveals that the neighborhoods receiving Promise programs differ from those outside along several dimensions. Using a large housing market definition, the housing stock in Promise-eligible areas covered by our housing data is smaller in size and typically older than that in the outlying areas. The Promise programs represented in the housing sample are mostly located urban areas. This urban differential also reveals itself in the demographic characteristics; Promise neighborhoods in the housing sample typically contain more black residents and fewer children. 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 other samples, although differences remain significant. We note that neither of the two more selective samples dominates the other in terms of matching observables across groups. 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.

3. Results

We first address the results from the K-12 enrollment data, which apply to a broad sample of Promise scholarship programs. We then follow 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 programs—Pittsburgh and Denver.

3.1. Public school enrollment estimates

Fig. 3 provides graphical evidence of 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 programs 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.[23] 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 through the residuals and the corresponding 95% confidence interval.

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 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 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 Eq. (1) in Panel A and Eq. (2) in Panel B. As predicted, when a Promise program is enacted in a particular set of schools, more students enroll in those schools. The announcement of a Promise program leads to an increase in overall enrollment of roughly 3.9%, although there seem to be no significant effects on the racial composition of schools on average.[24]

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 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 this 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 the percentage of a school's students who are white and corresponding decreases in black and Hispanic percentages, although the decomposed effects are not always individually significant. Programs without such requirements experience reductions in the share of students who are white, with mixed effects on black and Hispanic percentages. The racial

[22] The main results remain robust to increasing the lower bound from $1000 to $25,000 or $50,000.

[23] 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.

[24] 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.

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	219,981	−0.25	214,259	189,946	−21.69	216,348	189,771	−34.80
	(s.d)	(190,736)	(143,537)		(191,047)	(161,863)		(191,463)	(136,590)	
	Obs.	95,976	418,096		55,301	43,921		76,992	174,148	
Price ($2012)	Mean	250,035	254,312	5.82	240,650	217,019	−18.71	246,334	216,047	−35.37
	(s.d)	(213,887)	(163,001)		(214,762)	(182,752)		(214,538)	(153,472)	
	Obs.	95,976	418,096		55,301	43,921		76,992	174,148	
Building age	Mean	48.37	26.12	−175.41	44.98	38.71	−31.10	51.57	38.11	−92.90
	(s.d)	(36.85)	(26.81)		(32.80)	(30.11)		(35.20)	(29.23)	
	Obs.	94,978	401,716		54,890	43,078		76,992	174,148	
Floor area (square feet)	Mean	1595.88	1820.94	87.50	1573.91	1598.83	5.52	1540.09	1577.63	12.85
	(s.d)	(710.96)	(750.85)		(723.15)	(693.54)		(688.77)	(642.84)	
	Obs.	95,976	418,096		55,301	43,921		76,992	174,148	
% Black	Mean	0.14	0.11	−49.20	0.16	0.09	−65.82	0.12	0.14	32.66
	(s.d)	(0.17)	(0.22)		(0.17)	(0.14)		(0.17)	(0.25)	
	Obs.	94,773	413,230		54,110	42,415		76,992	174,148	
% Under 15	Mean	0.20	0.24	128.25	0.21	0.20	−24.62	0.20	0.21	45.74
	(s.d)	(0.07)	(0.06)		(0.07)	(0.07)		(0.06)	(0.05)	
	Obs.	94,773	413,230		54,110	42,415		76,992	174,148	
% Over 60	Mean	0.17	0.16	−29.88	0.16	0.21	65.75	0.19	0.19	3.78
	(s.d)	(0.11)	(0.11)		(0.09)	(0.15)		(0.10)	(0.10)	
	Obs.	94,773	413,230		54,110	42,415		76,992	174,148	
% HH with children	Mean	0.32	0.40	172.03	0.34	0.32	−22.07	0.31	0.34	75.01
	(s.d)	(0.13)	(0.11)		(0.13)	(0.12)		(0.11)	(0.10)	
	Obs.	94,773	413,230		54,110	42,415		76,992	174,148	
% HS diploma	Mean	0.40	0.34	−86.98	0.42	0.41	−3.22	0.40	0.43	30.49
	(s.d)	(0.19)	(0.16)		(0.18)	(0.16)		(0.20)	(0.16)	
	Obs.	95,078	414,620		54,408	42,415		76,986	174,148	
% College	Mean	0.34	0.34	4.68	0.32	0.29	−25.98	0.34	0.28	−71.70
	(s.d)	(0.21)	(0.17)		(0.20)	(0.16)		(0.21)	(0.15)	
	Obs.	95,078	414,620		54,408	42,415		76,986	174,148	
% Unemployed	Mean	0.08	0.06	−87.50	0.08	0.07	−23.50	0.08	0.08	7.26
	(s.d)	(0.04)	(0.05)		(0.04)	(0.04)		(0.05)	(0.05)	
	Obs.	94,176	414,620		53,506	42,415		76,986	174,148	
% in poverty	Mean	0.16	0.08	−215.27	0.15	0.11	−65.03	0.16	0.12	−89.29
	(s.d)	(0.11)	(0.08)		(0.10)	(0.09)		(0.10)	(0.09)	
	Obs.	94,176	414,620		53,506	42,415		76,986	174,148	
% K-12 private	Mean	0.18	0.13	−99.77	0.18	0.14	−51.44	0.18	0.14	−59.50
	(s.d)	(0.17)	(0.09)		(0.16)	(0.11)		(0.16)	(0.12)	
	Obs.	94,535	413,354		54,403	41,742		76,448	173,601	
Median income	Mean	51,507	68,359	207.47	52,522	54,970	15.83	50,640	53,538	34.04
	(s.d)	(21,839)	(25,216)		(22,972)	(24,406)		(20,421)	(17,865)	
	Obs.	94,174	414,620		53,504	42,415		76,985	174,148	

Note: Prices were adjusted to constant December 2012 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.

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.

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.[25] Also, students who first enroll after 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.

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. This increase partially results from 9th grade being the last year one can first enroll and remain eligible for Promise funding in most programs but also the prevalence of private primary schools that terminate at 8th grade.[26] 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

[25] 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.

[26] Thank you to an anonymous referee for pointing out the contribution of private schools.

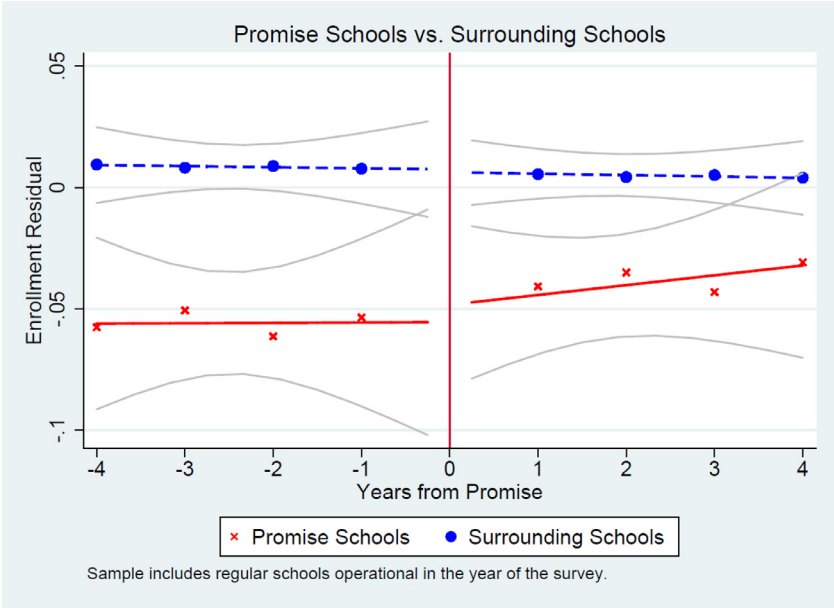

Fig. 3. Total enrollment residual by year.

Table 3
K-12 public school enrollment effects of Promise programs.

Dependent variable:	log(Total)	% White	% Black	% Hispanic
Panel A: Overall effects				
PromiseXPost	0.039***	−0.006	−0.002	0.005
	(0.011)	(0.008)	(0.003)	(0.005)
Panel B: Effects by type				
No merit and wide	0.097***	−0.013	−0.010**	0.009
(113 schools)	(0.021)	(0.009)	(0.005)	(0.007)
Merit and wide	0.049***	0.017	−0.010	−0.006
(200 schools)	(0.010)	(0.019)	(0.007)	(0.011)
No merit and no wide	0.035**	−0.020***	0.003	0.013***
(420 schools)	(0.015)	(0.007)	(0.003)	(0.004)
Merit and no wide	−0.011	0.020***	−0.003	−0.013*
(87 schools)	(0.026)	(0.007)	(0.003)	(0.007)
Observations	52,163	52,163	52,163	52,163
Clusters (districts)	994	994	994	994
R-squared	0.970	0.990	0.990	0.990

Note: Standard errors clustered at the school district level in parentheses. Models using % White/Black/Hispanic as the dependent variable are estimated by OLS. Sample includes open, regular schools located in Promise-eligible areas 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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				
StaticXPost	0.028**	0.009	0.019	0.056*
	(0.012)	(0.025)	(0.022)	(0.031)
SlidingXPost	0.060**	0.040	−0.005	0.053
	(0.025)	(0.032)	(0.040)	(0.061)
Test: $\Delta = 0$	0.033	0.031	−0.024	−0.002
	(0.0266)	(0.0404)	(0.0447)	(0.0662)
Observations	36,976	25,613	8712	8474
Clusters (districts)	902	920	635	630
R-squared	0.961	0.965	0.985	0.969

Note: 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-eligible areas 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Promise scholarships and the estimated effects gives us confidence that the identified overall effect is causal.[27]

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 Promise-eligible public schools are gaining students *relative to* public schools nearby. With our data, it is impossible to say to what extent the documented net inflows in Promise districts were driven by sorting across district borders versus between private and public schools. As our data are insufficient for the task, we leave an accounting exercise specifically designed to track and decompose student flows to future research.[28]

3.2. 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, to the extent that these students are coming

[27] We have repeated the enrollment analysis on just those areas included in the housing market sample analyzed in the subsequent section and the pattern of results remains qualitatively similar. In particular, the overall enrollment increase from Promise programs over this subset is almost identical to the analogous estimate in Table 3.

[28] We present some preliminary findings on this question in online Appendix C.

Table 5
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	514,072	487,957	505,285
Clusters (tracts)	2094	2047	2077
R-squared	0.383	0.694	0.918
Panel B: Small (1 mile)			
PromiseXPost	−0.007	0.043***	0.068***
	(0.022)	(0.016)	(0.025)
Observations	99,222	93,727	94,938
Clusters (tracts)	636	623	627
R-squared	0.408	0.719	0.931
Panel C: Optimal subpopulation			
PromiseXPost	0.028	0.060***	0.121***
	(0.018)	(0.012)	(0.020)
Observations	251,140	250,048	250,048
Clusters (tracts)	1491	1487	1487
R-squared	0.383	0.673	0.922
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

Note: 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 [0.075,0.925].
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

from other public schools this implies an increase in housing demand and, in turn, housing prices. In Fig. 4, we perform as similar graphical exercise to the one conducted on the K-12 enrollment data, but using 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 through the residuals on either side of the announcement date and the corresponding 95% confidence interval.

In the context of the large housing market definition, any impact of program announcement on housing prices in Promise-eligible 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. 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-eligible areas which persists through the 2.5 years following the announcement.

Table 5 presents the results from our estimation of Eq. (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-eligible areas vs. outside.[29]

The simplest DD specification yields imprecise capitalization estimates that are negative and small in magnitude. This may indicate why previous studies using such a specification have failed 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. 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.[30]

Our analysis of public school enrollment suggested that Promise programs have different impacts on different populations. Data considerations in the pooled sample limit our ability to assess the heterogeneity of effects, although we explore interactions with household size and public school quality on a smaller sample with higher quality data in the following section. In Table 6, we investigate the heterogeneity of the capitalization of Promise scholarships with respect to income, by dividing 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 separately for properties above the median and below the median of the housing distribution. Estimates analogous to the property fixed effects specification in column 3 in Table 5 are presented. For all samples, the capitalization of Promise programs into housing prices increases across the housing price distribution. Capitalization effects below the median range from 3.3% to 4.5% compared to capitalization above the median of between 10.2% and 16.2%. As it is reasonable to expect high-income families to occupy higher priced homes, the results from our regressions provide further evidence in support of the claim that higher income households are willing to pay more for access to Promise scholarship programs.

3.3. Large urban promise program estimates

Absent spillover effects, Promise programs are only valuable to households with school-aged children. Furthermore, that value is mediated by the quality of the public schools available in the Promise district. We hypothesize that capitalization will be

[29] The number of observations listed for the property fixed effects models includes singletons—properties which only sold once over the period—that are used to estimate the model's constant. Removing these observations does not change the coefficients reported and increases standard errors only slightly.

[30] In online Appendix D, we attempt to put these estimates into context by comparing capitalization effects in Pittsburgh to the net present value of the Pittsburgh Promise scholarship award under various assumptions. We find that the estimated capitalization effects are close to the high end of the range of possible values of the scholarship award. We interpret the similarity between capitalization effects and scholarship values as further reinforcement that the price increases are causal capitalization effects of the associated Promise programs.

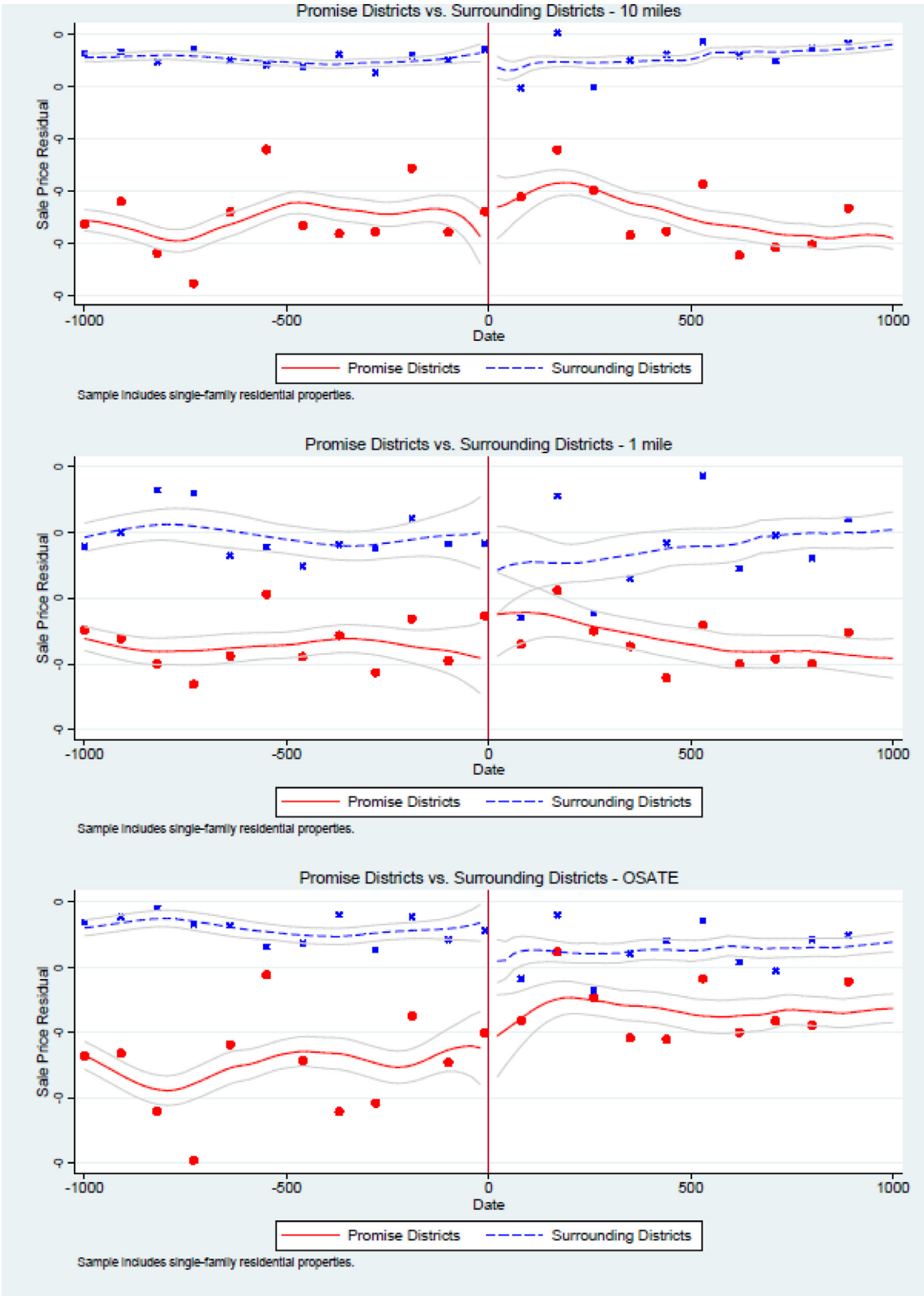

Fig. 4. Sale price residuals by date.

Table 6
Capitalization effects of Promise programs by above/below median.

Dependent variable: log(Price)	Above median	Below median
Panel A: Large (10 mile)		
PromiseXPost	0.142***	0.038**
	(0.024)	(0.015)
Observations	243,969	243,988
Clusters (tracts)	1634	1702
R-squared	0.914	0.891
Panel B: Small (1 mile)		
PromiseXPost	0.102**	0.033
	(0.040)	(0.022)
Observations	30,238	63,489
Clusters (tracts)	436	530
R-squared	0.934	0.908
Panel C: Optimal subpopulation		
PromiseXPost	0.162***	0.045**
	(0.022)	(0.018)
Observations	102,417	147,631
Clusters (tracts)	1106	1240
R-squared	0.918	0.894
Controls	Demo	Demo
Market-year-Qtr FE	Yes	Yes
Property FE	Yes	Yes

Note: 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. 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 [0.075,0.925].
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7
Large metropolitan Promise programs.

	Pittsburgh		Denver	
Dependent variable: log (Price)	(1)	(2)	(3)	(4)
Panel A: Large (10 mile)				
PromiseXPost	0.218***		0.104***	
	(0.052)		(0.029)	
< 3 BedroomsXPromiseXPost		0.148*		0.087***
		(0.087)		(0.027)
3+ BedroomsXPromiseXPost		0.233***		0.115***
		(0.058)		(0.034)
Observations	52,716	52,716	221,198	221,198
Clusters (tracts)	379	379	531	531
R-squared	0.907	0.907	0.886	0.886
Panel B: Small (1 mile)				
PromiseXPost	0.147**		0.071**	
	(0.067)		(0.032)	
< 3 BedroomsXPromiseXPost		0.166		0.018
		(0.107)		(0.033)
3+ BedroomsXPromiseXPost		0.143*		0.098***
		(0.077)		(0.036)
Observations	14474	14474	49445	49445
Clusters (tracts)	165	165	172	172
R-squared	0.900	0.900	0.898	0.898
Panel C: Optimal subpopulation				
PromiseXPost	0.179**		0.067***	
	(0.076)		(0.026)	
< 3 BedroomsXPromiseXPost		0.106		0.035
		(0.109)		(0.030)
3+ BedroomsXPromiseXPost		0.199**		0.085***
		(0.083)		(0.027)
Observations	13512	13512	48441	48441
Clusters (tracts)	191	191	252	252
R-squared	0.912	0.912	0.852	0.852
Controls	Demo	Demo	Demo	Demo
Market-year-Qtr FE	Yes	Yes	Yes	Yes
Property FE	Yes	Yes	Yes	Yes

Note: 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 [0.091,0.909] for Pittsburgh and [0.086,0.914] for Denver.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

concentrated amongst households with children under 18 and 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 mentioned previously, we do not observe whether a particular household has children and, while we observe the location of the property, tying that location to the quality of the neighborhood school is challenging. For what follows, we restrict attention to two Promise programs in our housing market data based in large metropolitan areas—the Pittsburgh Promise and the Denver Scholarship Foundation—for two reasons.[31] First, the housing transaction data for both markets contains the number of bedrooms in the house, which we will use as a proxy for the presence of children. As in Black (1999), if families with school-aged children live exclusively in houses with three or more bedrooms and all individuals without children lived in houses with one or two bedrooms then the Promise program should have little value for the residents of houses with one or two bedrooms. Without school-aged children, these households cannot receive scholarships and would not benefit from school peer effects, so their benefits are limited to other spillovers and possible confounding treatments. As households with children receive the direct benefits of the scholarship in addition to these other benefits, interacting the $Post_{mt}$ and $Promise_d$ indicators with an indicator for the presence of 3 or more bedrooms serves as a test of whether it is the direct benefits of the scholarship that are driving the capitalization.

Second, both markets have school attendance boundary maps available through the School Attendance Boundary Information System (SABINS) as well as school-level data on standardized test scores from their respective state education agencies, allowing us to link properties to the quality of the neighborhood school. In order to quantify school quality, we first calculate 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. Finally, we link properties in the transactions data to this objective measure of pre-Promise school quality via attendance boundaries.

Table 7 presents the results from estimating Eq. (5) in both markets individually, as well as the triple interaction with the three or more bedroom dummy. The first thing to note is that, qualitatively, the results from Table 5 hold individually in both Pittsburgh and Denver. Second, most of the effect is concentrated in homes with more than 3 bedrooms, reinforcing the idea that the results are driven by something that affects households with more children differentially and not a secular boom/bust phenomenon.[32]

[31] Importantly, the Denver Scholarship Foundation programs is one of the very few Promise programs with a need-based eligibility requirement. As such, it is not directly comparable to the Pittsburgh Promise program, nor to the universal programs mentioned above.

[32] In online Appendix D, we attempt to put these estimates into context by comparing capitalization effects in Pittsburgh to the net present value of the Pittsburgh

Table 8
Large metropolitan Promise programs by school quality.

Dependent variable: log(Price)	High school		Primary	
	Math	Reading	Math	Reading
Panel A: Large (10 mile)				
PromiseXPostXQuality	−0.035*	−0.017	0.074***	0.053***
	(0.021)	(0.015)	(0.016)	(0.012)
Observations	273,914	273,914	269,647	269,647
Clusters (tracts)	910	910	904	904
R-squared	0.916	0.916	0.917	0.917
Panel B: Small (1 mile)				
PromiseXPostXQuality	0.016	0.035*	0.084***	0.071***
	(0.022)	(0.018)	(0.017)	(0.015)
Observations	63,919	63,919	60,988	60,988
Clusters (tracts)	337	337	336	336
R-squared	0.927	0.928	0.928	0.928
Panel C: Optimal subpopulation				
PromiseXPostXQuality	−0.020	−0.004	0.039**	0.030**
	(0.020)	(0.015)	(0.019)	(0.015)
Observations	95,448	95,448	91,875	91,875
Clusters (tracts)	651	651	644	644
R-squared	0.916	0.915	0.915	0.915
Controls	Demo	Demo	Demo	Demo
Market-year-Qtr FE	Yes	Yes	Yes	Yes
Property FE	Yes	Yes	Yes	Yes

Note: 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 [0.078,0.922].
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8 presents the results from estimating Eq. (5). We find no evidence that high school quality is associated with larger capitalization effects in these cities. However, our analysis suggests that 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 a predicted increase in the capitalization effect of the Promise of between 3% and 8%. 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 discussed above, 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. Second, 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.[33]

4. 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. While many programs were announced around the peak of the recent housing bubble, our confidence that these effects are causal is bolstered by variation in timing of announcements as well as patterns in the results that would be predicted by the institutional features. First, enrollment increases are concentrated in primary schools, especially in programs where benefits increase with earlier enrollment. Second, the capitalization effects are concentrated in houses with 3 or more bedrooms, suggesting that whatever mechanism is driving the results differentially impacts households with children. Finally, capitalization effects are much stronger in areas with high quality primary schools, which would not necessarily be consistent with a secular boom/bust explanation of the increases in house prices.

These results provide potential 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 appears 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. There is plenty of evidence that the socioeconomic integration of schools can lead to large improvements in the achievement of disadvantaged students (Kahlenberg, 2012). So if Promise programs are exacerbating socioeconomic segregation at the school level as high-quality students attracted by the Promise settle into high quality schools, they may be contributing to inequality via this mechanism as well.

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 in the single-family housing segment. Thus, 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

Promise scholarship award under various assumptions. We find that the estimated capitalization effects are close to the high end of the range of possible values of the scholarship award. We interpret the similarity between capitalization effects and scholarship values as further reinforcement that the price increases are causal capitalization effects of the associated Promise programs.

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

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.

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, 2012; 2014). 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.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.jue.2017.06.001.

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