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ABSTRACT

Place-based college scholarships, such as the Kalamazoo Promise, provide students who live in a particular place, and/or who attend a particular school district, with generous college scholarships. An important potential benefit from such "Promise programs" is their short-term effects on local economic development. Generous Promise scholarships provide an incentive for families to locate in a particular place, which may change migration patterns, and potentially boost local employment and housing prices. Using data from the American Community Survey, this paper estimates the average effects of eight relatively generous Promise programs on migration rates and housing prices in their local labor market. The paper finds evidence that Promise programs lead to significantly reduced out-migration rates for at least three years after a Promise program is announced. These reductions in out-migration rates are larger for households with children, and are also larger when we focus on smaller areas around the Promise-eligible zone rather than the entire local labor market. These out-migration effects are large, implying that Promise programs lead to a 1.7% increase in overall population of the local labor market.

JEL Classification Codes: I22, I25, I28, J61, R23

Key Words: College scholarships, Kalamazoo Promise, local economic development, migration

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INTRODUCTION

Place-based college scholarship programs—often called “Promise programs”—have proliferated, with over 50 programs created since the 2005 start of the Kalamazoo Promise (Miller-Adams 2015). What unifies Promise programs is their targeting by location: college scholarship eligibility is restricted to K-12 graduates who live in a particular place and/or have attended and graduated from a particular school district.

A common goal of Promise programs is local economic development. In the long-term, local economic development may be enhanced by increasing the post-secondary attainment of local students. But local economic development may also be enhanced in the short-term. As soon as Promise programs are announced, parents have an additional reason to move to or remain in Promise communities. An increased local population of school-age families will boost local economic development, by increasing local labor supply and demand for local goods and services. Both increased local labor supply and increased local demand will encourage employers to add local jobs. This theory about short-term economic benefits of Promise programs was apparently believed by the anonymous Kalamazoo Promise donors. According to Dr. Janice Brown, the Kalamazoo Public Schools superintendent who mediated the Kalamazoo Promise’s creation, the donors believe that “equal access to higher education for all creates a powerful incentive that will bring people and employers back to Kalamazoo” (Miller-Adams 2009, p. 7 quoting Boudette 2006).

These short-term effects of Promise programs on local economic development are estimated in the current paper. The paper focuses on eight Promise programs, chosen because they are large enough and generous enough to potentially significantly affect local migration and the local economy, and because they have been in existence long enough to have some post-Promise evidence. Promise effects are estimated for out-migration, in-migration, and housing prices. Out-migration and in-migration effects are estimated both for all households, and for households with children. Data on migration and housing prices is taken from the American Community Survey (ACS), 2005–2013. Promise effects on migration and housing prices are estimated for two types of spatial areas: “Commuting Zones,” and “Migration Public Use Microdata Areas” (Migration PUMAs). Commuting zones are groups of counties intended to constitute local labor markets. Migration PUMAs are smaller than Commuting Zones, and are the smallest geographic areas with public-use Census data on migration.

With only eight Promise areas, and relatively few post-Promise years, this paper's estimation faces some challenges. One challenge is that the small number of Promise areas and post-Promise years limits the estimates' precision. A second challenge is that, according to recent research (Conley and Taber 2011), standard statistical methodologies may understate the imprecision of estimates when there are few “treatment units”. This paper uses procedures that deal with this problem and make accurate statistical inferences.

Based on this paper's estimates, Promise programs significantly reduce out-migration. This reduced out-migration is greater for the Migration PUMA area immediately surrounding the Promise-eligible area, and for households with children. However, reduced out-migration also occurs for the entire local labor market.

In contrast, the paper does not provide strong direct evidence of Promise effects on in-migration or housing prices. In-migration effects hop around over time. Some post-Promise effects occur for housing prices, but these effects may be due to pre-existing housing price trends.

The estimated out-migration effects of Promise programs are large. For example, the estimates imply that Promise programs increase the total population of the overall Commuting Zone by almost 2 percent, even though the Promise-eligible area on average only includes one-seventh of the Commuting Zone's population. A 2 percent increase in Commuting Zone population would be predicted to increase the Commuting Zone's housing prices and employment enough to imply sizable benefits relative to scholarship costs.

The next section analyzes what economic development effects would be expected from Promise programs, based on economic theory. We then review previous empirical research that estimates Promise effects on variables related to local economic development. The estimation model is then summarized, and we discuss how we overcome some estimation challenges. Following that, we describe this study's data. Estimation results are presented for how Promise programs affect out-migration, in-migration, and housing prices. The conclusion argues that these estimation results imply large effects of Promise programs.

THEORY

Promise programs would be expected to attract households with children. What effect would this attraction have on local economic development? How would this attraction be expected to affect out-migration rates, in-migration rates, and housing prices?

For local economic development to be increased by Promise programs, the direct attraction of households must lead to "spillovers." Spillovers are any indirect effects due to the direct attraction of households with children, such as effects on local employment, households without children, local demand, housing prices, employment rates and wealth. Such spillovers may be positive or negative, either increasing or decreasing local economic development. For example, some spillovers may attract households without children to the local economy, whereas others may repel households without children. Spillover effects on the entire local labor market level are first considered, before considering spillover effects within the local labor market, between the Promise area and the rest of the local labor market.

At the local labor market level, attraction of households with children would yield some positive spillovers on local economic development. First, a greater population of households with children would increase local employment, in several ways. Greater labor supply from these households would encourage employers to locate in or expand in the local labor market, by making it easier for employers to find additional workers. A greater population of households with children will increase local demand for goods and services, due to effects on consumption, government services, and investment. The additional households bring with them non-labor income and government assistance that increase demand for local consumption goods and services. Because many intergovernmental aid formulas are based in part on local population, the additional households will lead to increased aid from the federal and state government to local governments, which will increase public services spending.[1] Additional households will also lead to the need for additional housing and infrastructure, which will cause at least a short-term burst of local spending related to housing and infrastructure.

Second, this increase in local employment would also attract households without children. The new jobs in local retailers, local governments, and the local housing sector provide opportunities for all workers.

On the other hand, attracting households with children reduces local economic development with some negative spillovers. The most important negative spillover is the

[1] Instead of increasing local public spending, increased intergovernmental aid from federal and state governments to local governments may lead to local tax cuts, which would increase local consumption demand.

potential for increased housing prices. More population and more employment will increase local housing prices, land prices, and property values. The magnitude of housing price increases (and the interrelated increases in land prices and property values) depends upon how "elastically" local housing supply responds to increased local demand. The local housing supply elasticity will be affected by the availability of properties for new housing development or redevelopment, which will be altered by local geographic features, local zoning rules, and state and local housing codes.

Increased local housing prices will repel some households. This reduction in local labor supply, as well as increased land prices, will have some depressing effects on local employment.[2]

The net outcome for local economic development from these positive and negative spillovers cannot be determined a priori on theoretical grounds. The net outcome depends on empirical factors, such as the elasticity of local housing supply, and how much intergovernmental aid and non-labor income go up due to additional households with children. These empirical factors vary across diverse local labor markets. However, previous research on local labor markets suggests that, on average, positive spillovers from local population attraction at least match negative spillovers. For example, previous research finds that when local labor markets experience increases in local labor supply due to in-migration, this increased local labor supply is matched by employment growth, with little adverse effects on the labor market fortunes of the local area's original residents (Greenwood and Hunt 1984; Muth 1971).

[2] In addition, the increase in local housing prices may have some consumption effects due to the resulting transfer of wealth from local renters to local property owners. One would suspect that this transfer would reduce local demand, as it seems likely that renters tend to have a higher propensity to spend their resources on local goods and services.

How is this analysis altered because Promise programs only target a portion of the local labor market, that is the school district or city that has Promise availability (the Promise "zone")? This limited Promise coverage of local labor markets does not qualitatively alter overall labor market effects, but may quantitatively reduce the magnitude of local labor market effects, as well as creating some effects within local labor markets. First, tying the Promise award to a smaller geographic area would be expected to reduce the attractive effects of Promise programs, because it ties Promise availability to a more limited set of neighborhood choices and school district choices. Second, the Promise zone would be expected to encourage some geographic redistribution of households with children within the local labor market, from the rest of the local labor market to the Promise zone. This geographic redistribution would put some upward pressure on housing prices in the Promise zone relative to the rest of the local area.

But this geographic redistribution of households with children and housing prices does not mean that there are no overall local labor market effects. For example, if housing supply was perfectly elastic in the rest of the local labor market, then housing prices in the rest of the local labor market would stay the same. Furthermore, we would expect there to be a very elastic supply of households from the rest of the U.S. to this specific local labor market. When households with children are redistributed from the rest of the local labor market to the Promise zone, this opens up housing units in the rest of the local labor market for new households to move into the local labor market (or alternatively, for households to stay who otherwise would have left the local labor market). Both households with children and without children would be attracted to the local labor market.

Based on this analysis, the overall local development effects of Promise programs should not be judged solely by such intuitively appealing statistics as what percentage of any increased school enrollment in Promise-eligible schools comes from households new to the local labor market, as opposed to households moving into the Promise-eligible schools from elsewhere in the local labor market. Even if the entire increased enrollment in Promise-eligible schools came from households who previously lived elsewhere in the local labor market, their previous housing units may be filled by new households who moved in from outside the local labor market, or by households who otherwise would have moved out of the local labor market. The chain of housing vacancies causes the immediate apparent effects of Promise programs to differ from the true ultimate general equilibrium effects.

How would the overall attractive effects of Promise programs for households with children, and the subsequent spillover effects, be expected to be manifested over time in in-migration rates, out-migration rates, and housing prices? First, we expect Promise programs to result in a one-time temporary spike in in-migration rates, but in more persistent reductions in out-migration rates. This pattern is expected because most Promise programs make the Promise scholarship more generous the longer a student has been enrolled in the school district.[3] Therefore, for any household with children who is considering moving into a Promise zone, it makes sense to move in as soon as possible, rather than waiting. We would expect to see a one-time increase in in-migration immediately after the Promise announcement, assuming the Promise announcement is understood by households and believed.

On the other hand, Promise programs create persistent incentives for out-migration to be lower. Because of the availability of Promise benefits, households with children have another

[3] For example, for the Kalamazoo Promise, the award is zero if a student starts continuous enrollment in Kalamazoo Public Schools in 10th grade, 65 percent if the student starts in 9th grade, 70 percent in 8th grade, and then goes up by 5 percent for each earlier starting grade, until it is frozen at 95 percent at grades 1 through 3, before going up to 100 percent for KPS students who have been continuously enrolled in KPS since kindergarten. (This continuous enrollment must also have been accompanied by the family's continuous residency in the school district.) For the eight Promise programs considered in the current study, all but Syracuse condition the Promise award's magnitude on the length of enrollment in the Promise zone.

reason to hesitate before moving out in response to any changes in their personal circumstances (e.g. a new job offer) or due to any dissatisfaction with the Promise-eligible school district. Out-migration rates would be expected to be persistently lower, although perhaps not permanently lower.

This expected pattern of in-migration and out-migration effects is consistent with research evidence on how Promise programs increase Promise school districts' enrollment. For the Kalamazoo Promise, research shows that the Promise resulted in a one-time increase in new students entering the district, in the year just after the Promise announcement, but more persistent reductions in the rate at which students exited the district (Bartik et al. 2010; Hershbein 2013).

As for housing prices, Promise programs would be expected to cause some persistent increase in housing price levels. This housing price effect is due to the expected increase in the area's population, and the reality that local housing supply is unlikely to be infinitely elastic with respect to housing prices. The research literature suggests that a 1 percent increase in population increases local housing prices on average by somewhere between 0.5 percent and 1 percent. In addition, we would expect some increase in the relative housing price differential between the Promise zone and the rest of the local labor market.

The timing of the housing price increase depends on public expectations about the Promise program. In theory, if everyone fully believes that an announced Promise program would be fully implemented and would last, housing prices should immediately increase after the program's announcement. If a Promise program's funding and implementation is more uncertain, housing prices may only gradually increase, as the public sees that scholarships are actually awarded.

REVIEW OF RELEVANT PROMISE RESEARCH LITERATURE

A growing research literature on Promise programs estimates a wide variety of program effects, including effects on student success in high school and college (Bartik and Lachowska 2013; Bartik, Hershbein, and Lachowska 2015). But for this paper, this review section focuses on narrower Promise effects, those more directly related to short-run local economic development.[4] This narrower focus includes studies that estimate Promise effects on student enrollment and housing prices.

For enrollment, a cross-site analysis finds that the average Promise program increases student enrollment by 4 percent (LeGower and Walsh 2014). More universal Promise programs with broad college choices increase student enrollment by 8 percent. Other site-specific Promise studies have found evidence of enrollment effects in El Dorado, Buffalo, Syracuse, and Kalamazoo (Bartik et al. 2010; Hershbein 2013; PromiseNet 2015). Kalamazoo studies suggest that the Kalamazoo Promise increased enrollment in Kalamazoo Public Schools by about 30 percent, compared to what KPS enrollment otherwise would be (Bartik, Eberts and Huang 2010; Hershbein 2013; PromiseNet 2015).

What do such enrollment effects imply for overall local economic development? This depends primarily on two factors: the percentage the Promise districts are of the overall local labor market; assumptions about the spillover effects of the additional households enrolling in Promise districts on other types of households in the local labor market. Hershbein did some simulations of how Kalamazoo Public Schools' enrollment increases affected Kalamazoo area

[4] Broader reviews of the research literature are found in Bartik, Hershbein, and Lachowska (2015) and Miller-Adams (2015).

local economic development. Based on relatively conservative assumptions, the Kalamazoo Promise is estimated to increase gross regional product by 0.7 percent.[5]

For housing prices, the cross-site analysis by LeGower and Walsh estimated that Promise programs increase housing prices in Promise zones by 6 percent to 12 percent. Other site-specific Promise studies do not consistently find Promise price effects. For example, Miller's (2010) study did not find evidence of positive effects of the Kalamazoo Promise on housing prices.

How these Promise zone housing price effects are reflected in overall local area housing prices depends upon how one models the overall local housing market. If prices only go up in the Promise zone, and remain unchanged in the rest of the local area (for example, if housing supply is very elastic in the rest of the local area), then the overall area housing price effect would obviously be the Promise zone effect times the proportion of the area in the Promise zone.

ESTIMATION MODEL AND ISSUES

In the current study, Promise program effects are examined using panel data. These panel data are on a cross section of local areas, observed over different years, for which we have annual observations. The dependent variables are means for area/year cells for migration rate and housing price variables. The local areas in the sample include some areas with Promise zones, along with matched comparison areas. The years examined include years before the Promise announcement, and years after the Promise announcement. The estimation model seeks to determine how migration rates and housing prices varied before and after the Promise

[5] Hershbein's estimates used the Upjohn Institute's REMI model of the Kalamazoo economy. He assumed that one-third of the newly enrolled households came from outside the school districts. The displacement effects of additional labor supply were assumed to be large: every two additional workers in the Kalamazoo economy displaced one existing worker from a job, and only added one job to the local economy. If there is instead no displacement—added labor supply leads one-for-one to increased employment, as some studies suggest—then presumably economic effects would be twice as high, at around a 1.4 percent boost to regional product.

announcement, relative to matched comparison communities, and controlling both for area fixed effects and year fixed effects.

The underlying data on migration rates and housing prices come from the American Community Survey, all years from 2005–2013. The data sources and derivations will be described in more detail in the next section.

The estimation model can be written as:

(1) $$Y_{it} = B_0 + F_i + F_t + \sum_{j=-t0}^{j=t1} Bj{*}D_{ijt} + \varepsilon_{it}\ .$$

The areas are indexed by *i*. The areas considered are in one set of estimations, "Commuting Zones," which are local labor market areas, to be discussed further below. In another set of estimations, the areas considered are "Migration Public Use Microdata Areas," or "Migration PUMAs," which are smaller areas, created by the Census, that are the smallest geographic unit for which it is possible in public use data to determine in and out-migration rates. The years are indexed by *t*. The dependent variables Y_{it} are in-migration rates, out-migration rates, and the natural logarithm of housing prices. The migration rates are the migration rates from last year to this year, and represent the rate of in-migration or out-migration into or out of this area, as a percent of the relevant group's population in the area (either this year's population for in-migration, or last year's population for out-migration). Migration rates are calculated both for the population in all households and for the population in households that include at least one child under age 18.

The right-hand side of the equation includes a constant term, B_0, as well as two sets of fixed effects, a set of fixed effects for each area *i* (F_i), and a set of fixed effects for year *t* (F_t). The model also includes a disturbance term, ε_{it} .

On the right hand side, the main policy variables of interest are a complete set of dummies, each allowed to have its own coefficient, for all leads and lags relative to the Promise announcement for any area that at any time period includes an active Promise zone. This formulation is represented by the expression $\sum_{j=-t0}^{j=t1} \mathrm{Bj} * D_{ijt}$. A particular dummy is defined for a particular lead or lag (designated by the *j* subscript) relative to the number of years before or after a Promise program is announced in a particular area. Such a dummy is equal to 1 if the area ever contains a Promise zone, and if the particular year, for that Promise zone, is some particular number of years either before or after the Promise announcement. This dummy will be 0 for every year for all areas that never contain a Promise zone. For areas that at some point contain a Promise zone, this dummy will be 0 for years not at that particular lead or lag relative to the announcement.[6]

Because Promise zones in our sample are created in years ranging from 2005 to 2011, a particular lead or lag relative to the Promise zone announcement will not correspond to the same calendar year for all areas that contain Promise zones. Because the model includes a complete set of area fixed effect dummies, one of the Promise leads or lags has to be omitted to avoid perfect collinearity. We omit the year just prior to the Promise being announced, so all estimated Promise zone effects in a particular year are relative to the year just before the Promise announcement. Because our data runs from 2005 through 2013, and Promise programs in our sample are announced from 2005 through 2011, we have some observations on lags and leads relative to the Promise announcement from six years before the Promise announcement (2005 is six years before a Promise announcement date of 2011), to eight years after the Promise

[6] In addition, our model for migration rates from last year to this year for Commuting Zones includes an instrument that predicts employment growth based on the Commuting Zone's base-year industry mix and national growth in each industry (Bartik 1991). For the housing price dependent variable, we include a control for predicted ln(employment) in the Commuting Zone in year *t*, based on the Commuting Zone's industry mix in 2000, and national industry growth from 2000 to year *t*.

announcement (2013 is eight years after a Promise announcement date of 2005). Therefore, we have up to 14 dummy variables for lags and leads relative to the Promise announcement, from six years before until eight years after, with the first year before the Promise omitted.[7]

In our estimation model, the included Promise areas are: Kalamazoo, Hammond, El Dorado, Pittsburgh, Syracuse, Arkadelphia, New Haven, and Buffalo. These eight areas are selected because these eight Promise programs are relatively generous, and therefore more likely to show an effect on local economic development, and because each of these eight programs was announced in 2011 or earlier, so we have at least two years of follow-up data. (More information on the areas and their Promise programs will be provided later.) Each Promise area is matched to 15 comparison areas, for a total of 120 comparison areas. The matching is done to reduce the pre-existing differences between "treatment" areas, and comparison areas. More detail on the matching procedures and their results are given below.

A key econometric challenge of this estimation approach is that the number of Promise programs in the estimation is small. Research on panel data estimation suggests that if the number of treatment areas is small, the usual t-statistics for the treatment effects may be misleading, because they will overstate the statistical significance of the estimated treatment effects. The intuition is that the usual calculated t-statistics will actually only follow the usual t-distribution asymptotically, as the number of treatment groups approaches infinity. In a sample in which the number of treatment groups is "small," unobserved time-varying shocks to the treatment units, shocks that possibly will be correlated over time, can lead to large estimated treatment effects that are not as unlikely in a finite sample as the calculated t-statistics suggests. Based on analyses by Conley and Taber (2011), this problem is particularly severe if the number

[7] As explained below, our Migration PUMA data ends in 2011, so we only have data and therefore dummies for up to six years after the Promise announcement, and hence include only 12 dummies for Promise lags and leads in those estimating equations.

of treatment groups is only one or two. However, some over-statement of statistical significance occurs when there are only 10 treatment areas.

In this paper's model, some of the far leads and lags for Promise zone effects on the overall area essentially only have one or two treatment areas that are identifying the treatment effect, as not all of the eight Promise areas were early or late enough for data to be available the appropriate number of years before or after the Promise announcement. For the Promise effects in the year of Promise announcement, and one or two years after, we have eight areas with Promise zones that are identifying the Promise effects at that time interval.

To help identify the appropriate statistical inferences from the estimates, we rely on a methodology recently proposed by MacKinnon and Webb (2015). A similar approach has been used by Conti, Heckman, and Pinto (2015). The basic idea is the following: randomly exchange treatment areas for comparison areas, recover the t-statistics from this random reassignment, repeat this process many times, and then see what the distribution of t-statistics is in these randomly assigned treatment group samples. The actual t-statistic in the original model with the real treatment areas is compared with the t-statistic distribution in these many randomly reassigned treatment status models, and it is seen how probable it would be to see a t-statistic of that absolute value.[8]

To implement this procedure in this paper's model, for each set of 1 treatment area with its 15 matched comparison areas, one comparison area is randomly selected for us to regard as being the treatment area, with the Promise announcement occurring in this imaginary treatment

[8] As MacKinnon and Webb (2015) point out, this t-statistic randomization inference procedure is an alternative to the coefficient randomization inference procedure that was proposed in Conley and Taber (2011). MacKinnon and Webb (2015) present Monte Carlo evidence that t-statistic randomization inference, compared to coefficient randomization inference, gives more accurate inferences when the number of treatment groups is small but greater than one. In particular, it seems to work well when the number of treatment groups is six or more. In cases where the number of treatment groups is very small, say one or two, any of these inference procedures depends upon the treatment groups being similar in sample size and error variances to the control groups. The Mahlanbois matching procedure done later in this paper helps increase the similarity of treatment to control groups.

area in the same year as it did for the true treatment area. The other 15 areas (the true treatment area, and the other 14 comparison areas) are considered to be the comparison areas in this randomly chosen reassignment of treatment status. This same random reassignment of treatment status is done in turn for each of the other 7 Promise areas and their 15 comparison areas each. Using this imaginary reassignment of treatment status to eight areas, areas that in the real world are NOT Promise areas, and the assignment of comparison area status to the other areas, including the true Promise areas, the model is re-estimated. The t-statistics on all the Promise dummies for various leads and lags are recovered from this re-estimate. Under the null hypothesis that the true treatment has no effect, the effect of this imaginary reassignment of treatment status should be zero, for all leads and lags on the Promise dummies. The t-statistic on a given Promise lead or lag dummy from this re-estimation using "fake" treatment assignment is one draw from the t-statistic produced in this model, under the null hypothesis that the true treatment effect is zero. This imaginary reassignment of eight treatment areas is done 10,000 times, each time randomly changing which eight areas are chosen to be regarded as imaginary treatment areas. T-statistics for all leads and lags on the Promise dummies are recovered from each of these 10,000 estimations. For each Promise lead or lag dummy, the distribution of these 10,000 t-statistics represents the true distribution of the t-statistics of this model with a small number of treatment groups, with the effective number of treatment groups varying with the lead or lag, under the null hypothesis that the treatment effect is zero. For each of the Promise leads and lags in the original true model, we look at the estimate's t-statistic. For that t-statistic, an inference of its probability can be derived by seeing what the probability would be of a t-statistic of that absolute size in the 10,000 fake estimates.

Figures 1 and 2 provide illustrations of the results from such resimulations. Figure 1 shows the actual distribution of t-statistics from the 10,000 simulations for the dummy variable for eight years after the Promise announcement, for the Commuting Zone regression where the dependent variable is the out-migration rate for the population in households with children under 18. This estimate is only identified by one treatment area, and in fact by an observation on one area/year cell, as we don't have information on eight years after the Promise announcement for seven of the eight Promise areas. The actual distribution of t-statistics in the 10,000 random simulations is compared with a standard normal distribution, which is the distribution we would expect a t-distribution with so many nominal degrees of freedom to approximately follow. As can be seen, the actual distribution of t-statistics does not resemble the standard normal distribution. The simulated probability of having t-stats greater than 5 in absolute value is far greater than would be predicted based on the standard normal distribution.

Figure 2 provides a contrast. This figure also shows the actual distribution of t-statistics from 10,000 random resimulations of the model with fake treatment areas, but this time the t-statistics are for the dummy for one year after the Promise announcement, again for the Commuting Zone regression where the dependent variable is the out-migration rate for the population in households with children under age 18. This dummy is identified from the experience of eight treatment areas for which we have such data for one year after the Promise announcement. In this case, the match between the simulated t-statistic distribution, and the standard normal distribution, is much closer. The tails are a bit thicker than the standard normal, but the difference is obviously not nearly as great as for the previous figure, which rested on only one treatment area.

As a result, the usual t-tail probabilities are likely to be most seriously misleading for the far leads and lags, whose estimation rests on fewer treatment areas. However, in the later reported estimates, we correct all the two-tail probabilities for this bias due to having a finite number of treatment areas. The corrected two-tail probabilities, based on the distribution of t-statistics inferred from 10,000 simulations, will give improved statistical inference for whether a particular coefficient on a treatment dummy lead or lag is significantly different from zero.

DATA

The dependent variables are the natural logarithm of median home values, and out-migration and in-migration rates for the entire population and for the population in households with children under 18, from last year to this year, for two types of geographic areas: Commuting Zones and Migration Public Use Microdata Areas (Migration PUMAs). A Commuting Zone is a group of contiguous counties that has sufficient county-to-county commuting rates to be categorized as being in the same local labor market. In urban areas, Commuting Zones are similar to metropolitan areas. But unlike metropolitan areas, Commuting Zones have the advantage of encompassing the entire United States, thus allowing rural areas to have defined local labor market areas.[9] These Commuting Zone definitions divide the U.S. into 741commuting zones.

Public Use Microdata Areas (PUMAs) are Census-designated geographic areas, with a minimum population of 100,000 and usually not many more than a population of 100,000, which are the smallest area for which the Census Bureau will in public-use microdata bases identify a

[9] Allowing rural as well as urban areas to have defined local labor markets was the purpose of the creation of Commuting Zone definitions, by researchers associated with the U.S. Department of Agriculture (Tolbert and Sizer 1996).

household's geographic location. Migration PUMAs are one or more contiguous PUMAs that, for households that move, are the smallest geographic area for which in public-use microdata the Census will report the geographic location the previous year. In other words, the Census sometimes provides less geographic detail on location the previous year than on location the current year.

The Commuting Zone and Migration PUMA estimates of housing prices and migration rates are calculated from the American Community Survey (ACS), 2005–2013. The ACS does not directly identify Commuting Zone of residence. As mentioned, the ACS only identifies PUMA of current residence, and for movers, the Migration PUMA of residence the previous year. However, weights are available that tell what percentage of the population of a given PUMA (or Migration PUMA) is in one or more counties, which allows us to say what percentage is in one or more Commuting Zones. These weights are used in a complex but logical procedure to estimate median home values and in-migration rates and out-migration rates for various populations by Commuting Zone.

For median housing values, we first discard observations with Census-imputed values (e.g., the household actually did not answer this question, and the Census made a guess as to the correct value). Each household observation has a sampling weight that can be seen as being proportional to the number of households it represents, given the stratified nature of the Census-sampling procedure, and patterns of households not responding to any Census questions. If the household's PUMA is entirely in one Commuting Zone, then that sampling weight alone is used in calculating statistics for that Commuting Zone. But if a household's PUMA is allocated across two or more Commuting Zones, then the household is treated as if it is divided among those zones. For a given Commuting Zone that the household might live in, a weight is assigned to the household, equal to the household's original sampling weight, times the proportion of that PUMA in that Commuting Zone. In calculating the median home value for that Commuting Zone, all observations with non-zero weights for that Commuting Zone are included, both households whose PUMA lies totally within the Commuting Zone, and households whose PUMA lies partly in that Commuting Zone, but the weights are adjusted to reflect that some households only have a probability of less than one of being in that Commuting Zone.

For migration variables, we use the ACS data on persons combined with the data on the person's household. We first disregard person observations for which PUMA of residence, migration, or Migration PUMA of residence last year are Census-imputed. We also disregard persons who are ineligible for having answers for the question of migration since the previous year, that is persons less than one year of age. For both persons in all households, and households with children less than 18, we calculate similar types of statistics to determine migration rates. For each type of person (all households, and household with children under 18), we have a Census-assigned sampling weight. There is a response for all persons one year of age or older for whether the person changed houses from a year ago. If the person did so (e.g., was a mover), there is a response for where the person lived a year ago, and the Census reports publicly what Migration PUMA the person lived in a year ago. We use these data to calculate four population numbers, for the population age one and over, and for the population one and over living in households with children under 18: the population this year in each Commuting Zone; the population moving into each Commuting Zone from last year to this year; the population moving out of each Commuting Zone from last year to this year; the population last year in each Commuting Zone excluding persons who died or moved out of the U.S. since last year. The in-migration rate is then the number of in-migrants to each Commuting Zone divided by this year's

Commuting Zone population, and the out-migration rate is the number of out-migrants from each Commuting Zone divided by last year's Commuting Zone population excluding subsequent deaths and out-of-country moves. For all migration variables, the population denominators used correspond to the migrant numerator, e.g., the overall population is used when looking at all migrants, and the population in households with children under age 18 is used when looking at migrants in such households.

The calculation of these population statistics involves using the Census-assigned sampling weights along with the proportion of each PUMA or Migration PUMA assigned to one or more Commuting Zones. We treat each person as having particular probabilities of being in one or more Commuting Zones this year, and as having particular probabilities of being in one or more Commuting Zones the previous year. Each combination of probabilities is treated as if it is a separate person, with a weight equal to the original sampling weight times the probability of being in Commuting Zone x this year and Commuting Zone y the previous year. The four population totals (population this year, migrants to *x*, migrants from *y*, population this year) are calculated by using some combination of these weights for certain categories of persons. The population this year calculates the population total using the sampling weight times the probability of being in Commuting Zone *x* this year and in any Commuting Zone last year. The population in-migrating to Commuting Zone *x* this year uses the sampling weight times the probability of being in Commuting Zone *x* this year for any Commuting Zone *y* last year that is not Commuting Zone *x*. The population out-migrating from Commuting Zone *y* last year uses the sampling weight times the probability of being in Commuting Zone *y* last year for any Commuting Zone *x* this year that is not Commuting Zone *y*. Finally, the population in

Commuting Zone *y* last year uses the sampling weight times the probability of being in Commuting Zone *y* last year regardless of what Commuting Zone *x* the person lives in this year.

Because of the nature of the ACS data, which is based on responses only for persons who in the current year were alive and in the United States, these population totals for last year and for the number of out-migrants necessarily omit persons who died between last year and this year, and persons who moved out of the United States between last year and this year. Therefore, out-migration rates are the out-migration rates from a Commuting Zone to another Commuting Zone in the U.S., excluding out-migrants from the U.S. and persons who died from both the numerator and denominator. On the other hand, the in-migration rates do include persons who moved into a Commuting Zone from outside the U.S. However, these in-migration rates do not include persons born within the last year.

A similar set of procedures is used to calculate the in-migration and out-migration rates for each Migration PUMA in the U.S., both for persons one and over in all households, and persons one and over in households with children less than 18 years old. Because of changes in Census definitions, it is only possible to calculate in-migration rates and out-migration rates for the years 2005–2011. In 2012 and subsequent years, the Census switched to an updated set of PUMA definitions (and Migration PUMA definitions). For Commuting Zones, we can map both the old and new set of PUMA and Migration PUMA definitions into the same Commuting Zones reasonably well. Therefore, we can calculate reasonably consistent in-migration rates and out-migration rates by Commuting Zone for the entire period 2005–2013. But for PUMAs and Migration PUMAs, the change in definition means we cannot reliably assign in-migration and out-migration status using a consistent set of Migration PUMA definitions.[10]

[10] There is a population allocation mapping from new to old PUMAs. In theory, this could be used in 2012 and 2013 to assign probabilities to each person of what Migration PUMAs they were in this year and last year using

The estimated housing price and migration rate effects are for Commuting Zones or Migration PUMAs that include Promise program areas, relative to comparison CZs or Migration PUMAs. For these estimates, eight Promise programs are chosen. The Promise programs chosen meet two criteria: the scholarships awarded are relatively generous, both in dollar value of scholarships provided and in allowing for a broad range of college choices; the programs have been around long enough to have some post-Promise data by 2013. Other things equal, looking at more Promise programs should improve estimation precision, which would argue for including more Promise programs. However, Promise effects are more likely for more generous programs, which argue for restricting the estimation to fewer Promise programs, those that are likely to actually have detectable effects. Balancing this tradeoff led to a choice of eight Promise programs.

Table 1 lists the eight Promise programs. These Promise programs were undertaken in a wide variety of regions and areas around the country. What these programs have in common is that all of them potentially provide families with tens of thousands of dollars for a child's college tuition, at a wide variety of colleges. In looking at the areas, migration PUMAs and Commuting Zones, for which we can actually measure migration rates and housing prices, the eight Promise program areas are modest but non-negligible portions of such areas. Across the eight areas, the Promise areas range from 12 percent to 80 percent of the surrounding Migration PUMA area,

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the 2005–2011 PUMA definitions. The problem is that this probability assignment will tend to overestimate in-migration and out-migration rates. For example, in the common case where a person moves but stays within the same Migration PUMA using 2012–2013 definition, and the 2005–2011 Migration PUMA definitions differ from the 2012–2013 definitions, such an algorithm will assign too high a probability that the person will switch Migration PUMAs under the 2005–2011 definitions, as it uses the overall population allocation from 2012–2013 PUMAs to 2005–2011 PUMAs to allocate mover locations, but in the real world shorter moves are more likely. We tried this procedure, and saw in practice that reassigning new PUMAs to old PUMAs artificially pushed up Migration PUMA migration rates above previous rates using the old definitions, or above current rates using the new PUMA definitions.

with a simple average of 30 percent. As a percent of the surrounding Commuting Zone, the Promise areas range from 4 percent to 32 percent, with a simple average of 14 percent.

As mentioned previously, and as shown in Table 1, these Promise programs were announced in various years, ranging from 2005 to 2011. For Commuting Zones, the ACS data only allow us to calculate the housing price and migration dependent variables from 2005 to 2013. For Migration PUMAs, the ACS data only allow us to calculate these dependent variables from 2005 to 2011. The combination of these announcement dates and CZ/MP data availability leads to varying number of Promise program observations being available to show the effects of Promise programs at various years, relative to the Promise announcement. Table 2 describes the resulting pattern of what years for what Promise areas have surrounding Commuting Zone and Migration PUMA data as of various time periods before or after the year in which that Promise program was announced. Table 2 also adds up how many Promise areas identify a given lead or lag effect relative to the Promise announcement year.

In this paper's regressions, we compare the migration and housing price trends in Commuting Zones or Migration PUMAs containing Promise areas, to other Commuting Zones or Migration PUMAs. As mentioned in the previous section, we control for area fixed effects. However, we seek to also choose comparison areas to increase pre-existing similarities between the CZs and MPUMAs containing Promise programs, and the comparison areas. To do so, we do a matching with replacement between each of the Promise-containing CZs and MPUMAs, and other CZs and MPUMAs. The matching is based on migration trends, housing prices, and other CZ/MPUMA characteristics, all measured prior to the estimation period that begins in 2005. Each CZ/MPUMA is matched to 15 CZ/MPUMAs, using Mahlanbois smallest distance matching. The matching is done with replacement in that a given CZ/MPUMA containing a

Promise program can be matched with a CZ/MPUMA that was already used to match to another CZ/MPUMA that contains a Promise program. The MPUMA matching is constrained to MPUMAs that are within the CZs that are matched to that particular Promise program area.

Why use Mahlanbois smallest distance matching rather than propensity score matching, which is often used in matching procedures? The main rationale is that it is not obvious that Promise programs have a single model that determines which areas are selected for Promise programs. Propensity score matching would seek to match the overall eight Promise zone areas to other areas that resemble them in having similar probabilities of being selected as Promise areas, under some model of what determines Promise zone selection. But it is not obvious that there are similar reasons why, for example, Kalamazoo, Pittsburgh, New Haven, and El Dorado, AK were selected to have Promise programs. Propensity score matching would seek comparison areas with a high probability of being selected as Promise areas based on what may be a mistaken assumption that there is a unified model of such selection. Under propensity score matching, there is no guarantee that each Promise area will have similar non-Promise areas among the comparison areas. In contrast, Mahlanbois matching ensures that each of the eight Promise areas have comparison areas selected that resemble them in pre-Promise characteristics. The matching is done with replacement so that each of the eight Promise areas has a set of 15 comparison areas that are as similar as possible.[11]

For the Migration PUMA matching, the matching is constrained to operate so that for each Migration PUMA containing a Promise programs, the only eligible Migration PUMA for matches are Migration PUMAs that are located within the 15 Commuting Zones that were

[11] This matching may also help the t-statistic randomization inference procedure work better for the coefficients identified by fewer than six treatment groups. As discussed by MacKinnon and Webb (2015), some Monte Carlo evidence suggests that t-statistic randomization inference works in general when the number of treatment groups is six or greater, and works well even with fewer treatment groups if the treatment groups are similar in size and error variance to the control groups.

chosen as matches for that particular Promise program. This ensures that the matched Migration PUMAs not only are good matches in having similar Migration PUMA pre-existing characteristics, but also are good matches in having similar characteristics of the overlaying Commuting Zone.

The Commuting Zone matching uses the characteristics listed in Table 3. These characteristics include values as of 2000 of various migration rates and housing prices, along with variables measuring the size of the Commuting Zone, the Commuting Zone's education level, percent black, and percent poor, and several variables measuring recent employment growth trends and predicted employment growth trends. The rationale is to match on pre-existing versions of the dependent variables,[12] and also to match on other variables that might be correlated with a Commuting Zone's future economic development, such as its size, past growth trends, predicted growth trends, and several CZ demographic characteristics. The hope is that the CZs containing Promise areas, and their matches, will be similar in some important variables that might affect the CZ's migration trends and housing price trends from 2005 to 2013.

As can be seen in Table 3, Panel 3A, the matching significantly reduces the differences between the Promise Commuting Zones and the matched Commuting Zones, compared to all other Commuting Zones. In particular, the matched areas are made more similar to Promise areas by choosing areas with lower historical migration rates, somewhat larger size, and somewhat lower prior employment growth. The average absolute value of the t-statistic of the difference between Promise areas and matched areas, versus Promise areas and all other areas, is reduced by three-fourths, from an average t-statistic absolute value of a little over four to an average

[12] Because of the way that the Census reports migration data in the 2000 PUMS, the migration rates are five-year migration rates, which obviously are not exactly the same as the one-year migration rate dependent variables in the estimated regressions.

absolute value of t-statistics of a little under one. The average of the normalized differences between variables, a measure advocated by Imbens (2014), is reduced by two-thirds.[13]

The Promise Commuting Zones still tend to be somewhat larger areas, with somewhat lower historical growth, and lower in-migration rates and out-migration rates. These remaining differences are controlled for with Commuting Zone fixed effects. In addition, in interpreting the results, we will examine the pattern of the pre- and post-Promise announcement dummies and see whether there are signs of pre-existing trends.

Panel 3B shows the Migration PUMA matching. In this case, the original set of all other Migration PUMAs is not so drastically different from the Migration PUMAs with Promise areas. Therefore, the scope of the matching for lowering pre-existing differentials is narrowed, and the matching doesn't make as much difference to these Migration PUMA-specific variables. However, because this Migration PUMA matching takes place within the matched Commuting Zones, we know the matched Migration PUMAs will also have similar Commuting Zone characteristics to the Promise areas.

The regressions are done using the matched datasets. Table 4 presents descriptive statistics for the matched databases for the dependent variables. The descriptive statistics seem reasonable. As shown in Table 4, in- and out-migration rates tend to average around 4 percent. These are annual migration rates from last year to this year, averaged over the time period from 2004–2005 to 2012–2013 for Commuting Zones, and through 2010–2011 for Migration PUMAs.

[13] As described in Imbens (2014, p. 17), the normalized difference is equal to the difference between the means in the two groups, divided by the square root of the average of the sum of the squared standard deviation in each group. (This standard deviation is the standard deviation across the Commuting Zone or Migration PUMA means for each CZ or MPUMA.) So this difference is the mean difference scaled in standard deviation units. The t-statistic for the difference divides each squared standard deviation by the sample size in each group, which, as Imbens points out, can mean large t-statistic differences may reflect modest differentials in terms of the difference in means compared to the average variable standard deviation across units of observation.

Median home values in this sample of Commuting Zones average around $120,000 for this time period.

The migration rates don't differ much between CZs and Migration PUMAs. This may reflect that the Migration PUMAs in this database tend to be large compared to the Commuting Zones. For example, among the eight Promise areas, about 57 percent of the population in the typical Promise-area Commuting Zone lives in the Migration PUMA we are examining.

There is a fair amount of variation in the sample in migration rates and housing prices. For example, looking at the 10th and 90th percentile of the distributions, migration rates vary by a factor of 2-to-1 to 3-to-1. Similar variation occurs for housing prices.

RESULTS

This section will present the results, grouped by whether the dependent variable is an out-migration rate, an in-migration rate, or housing prices. As discussed above, if Promise programs have effects, we expect them to be most persistent for out-migration and housing prices, and stronger for Migration PUMAs than Commuting Zones, and for households with children than for all households.

Out-migration

To summarize the results for out-migration, the out-migration estimates suggest that Promise programs persistently cause out-migration rates to decline, for at least three years after a Promise program is announced. This inference is supported by the out-migration results' pattern across different groups.

As shown in Table 5, after the Promise program's announcement, there are some statistically significant effects for one or more years in causing out-migration for all groups to decline. (Statistical significance is judged by a 10 percent test, using the simulated 2-tail probabilities for the estimated t-statistics.) These negative out-migration effects of Promise programs are stronger for the Migration PUMA area that is more immediately around the Promise zone, than it is for the larger Commuting Zone. Negative out-migration effects of Promise programs are also strongest for the population in households with children, compared to the overall population.

If we look at out-migration's time pattern of Promise effects, before and after the announcement (Table 5 and Figure 3), there is no pre-existing trend prior to the Promise announcement towards reduced out-migration. If anything, the estimates suggest that in Migration PUMAs surrounding Promise programs, the pre-existing trend might have been towards out-migration increasing. Perhaps increased out-migration around Promise areas might be part of why some areas have adopted Promise programs, as a response that aims to reverse this increased out-migration trend. If so, the empirical evidence suggests that Promise programs in fact do reverse the pre-existing trend towards increased out-migration, and lead to some sustained reduction in out-migration.

Summing-up over the three years after a Promise announcement, the estimates suggest Promise program effects on out-migration that are sizable enough to yield substantively large effects on an area's population. For example, the cumulative effect on out-migration over these three years is sufficient to increase overall Commuting Zone population by 1.7 percent and population in the Migration PUMA by 2.7 percent. For households with children, the implied population effect summed over three years is even larger, increasing the population of households with children by 2.5 percent in the overall Commuting Zone and 6.0 percent for the Migration PUMA.

The average population share of Migration PUMAs in the overall Commuting Zone, and of households with children in the overall population, can be used to look at the population of different groups, both with and without children, and inside and outside the Migration PUMA immediately around the Promise program. These calculations are reported in Table 6.

As shown in Table 6, over the entire Commuting Zone, positive spillovers seem to predominate for households without children. The direct effects of Promise programs in reducing out-migration for households with children are accompanied by some reductions of out-migration for households without children, although at a lesser rate than for households with children. Within the Commuting Zone, the out-migration estimates imply that Promise programs redistribute households with children so that more of them live within the surrounding Migration PUMA, and fewer live outside the surrounding Migration PUMA. However, on net the overall Commuting Zone population of households with children increases. Furthermore, the reduced population of households with children in the "remainder of the Commuting Zone"—that area outside the Migration PUMA surrounding the Promise area—is more than offset by increases in this remainder area for the population of households without children. Even in the Migration PUMA immediately around the Promise program, the estimates imply that Promise programs lead to some increase in population not only for households with children but for households without children.

In-Migration

Overall, the in-migration results do not provide much support for effects of Promise programs. In the year of the Promise announcement, positive in-migration effects are found, relative to the year before the Promise announcement, for the overall Commuting Zone.

However, in-migration effects are not found for the Migration PUMA immediately around the Promise program, where one would expect any true in-migration effects to be larger.

Furthermore, in-migration effects move up and down with not much of a clear sustained pattern, both before and after the Promise announcement. This is shown not only in Table 7, but in Figure 4. From looking at Figure 4, speculation might imagine that prior to the Promise announcement, there was a more pronounced tendency for declining in-migration, which seems during the post-Promise period to be stabilized. But these estimates are too imprecise to allow for firm conclusions.

Housing Prices

Pre-existing trends make it difficult to conclude anything definitive about how Promise programs affect housing prices. As shown in Table 8, the estimates for Migration PUMAs suggest a statistically significant and large effect of Promise programs on housing prices, as of four years after the Promise programs' announcement. However, Table 8 and Figure 5 also show that housing prices in Migration PUMAs were trending already, prior to the Promise program announcement. There is no sign that the Promise announcement led to this upward trend accelerating. For Commuting Zone housing prices, pre-existing trends are less evident, and the point estimates suggest that after the Promise program announcement, housing prices increased. However, none of the Commuting Zone housing price estimates are close to being statistically significant. The housing price estimates are noisy enough that even large effects are not statistically distinguishable from zero.

CONCLUSION

This paper has analyzed the effects of place-based scholarship programs on out-migration, in-migration, and housing prices. The results suggest that these “Promise” programs have significant and sustained effects in reducing out-migration, and thereby increase a local area’s population. These out-migration reduction effects are particularly concentrated among households with children, and in the areas that surround the Promise program. In contrast, this paper’s results provide no strong evidence for or against Promise program effects in increasing in-migration or housing prices.

These estimated out-migration effects of Promise programs are substantively large, in that they are of sufficient magnitude to make a difference to Promise program’s benefits versus costs. For example, the annual scholarship costs of the Kalamazoo Promise are around $11 million. The estimates here suggest that Promise programs might increase the population of a local area such as Kalamazoo by about 1.7 percent. The previous research literature suggests that an increase in a local area’s population by 1 percent might increase local housing prices by 0.6 percent (Bartik 1991). Therefore, we might expect the reduced out-migration due to a Promise program to increase housing prices in Kalamazoo County by 1 percent. Such an increase would be perfectly consistent with the direct estimates of housing price effects in the current paper, as these estimates are imprecise.

A 1 percent boost to Kalamazoo County housing prices would increase the county’s property values by about $168 million. (Based on property values reported for 2015 by the Michigan Department of Treasury, at http://www.michigan.gov/treasury/0,1607,7-121-1751_2228_21957_45818---,00.html. We include all property in our calculations.) This is insufficient to allow the increased property taxes on these increased property values to finance the Kalamazoo Promise’s cost. There is no miracle public service version of a Laffer curve here. However, the increased property wealth of $168 million is of similar present value to the present value of $11 million in annual costs. For example, at a social discount rate of 3 percent, $11 million in annual costs has a present value of around $367 million. Therefore, the property value increases alone due to the Kalamazoo Promise might be over 45 percent of the program’s costs. And a complete benefit-cost analysis would obviously consider other benefits, such as the increased earnings of any increased educational attainment due to Promise programs (Bartik, Hershbein, and Lachowska 2015).

A 1.7 percent boost to local population might also be expected to lead to a local employment increase of similar size. This expectation is based on research literature suggesting that shocks to local population from migration do not significantly affect local employment to population ratios or wages (Greenwood and Hunt 1984; Muth 1971). For Kalamazoo County, this would correspond to the creation of about 1,900 permanent jobs. The annual cost per job-year would then be around $6,000 (= $11 million / 1,900). This compares quite favorably with many economic development incentives, which often will have annual costs per job-year created that might average around $20,000 (Bartik 2016). Therefore, from an economic development perspective, Promise programs might be reasonably cost-effective ways of creating local jobs.

The main limitation of this study is its imprecision. Because currently there are only relatively few Promise programs that are sufficiently generous and have a sufficient number of post-Promise years to allow for estimation, this study’s estimates are necessarily imprecise. If Promise programs continue to spread around the U.S., future research may be able to pin down their local effects with more precision. In addition, future research may have a sufficient sample size to allow for analysis of how Promise program effects vary with program design.

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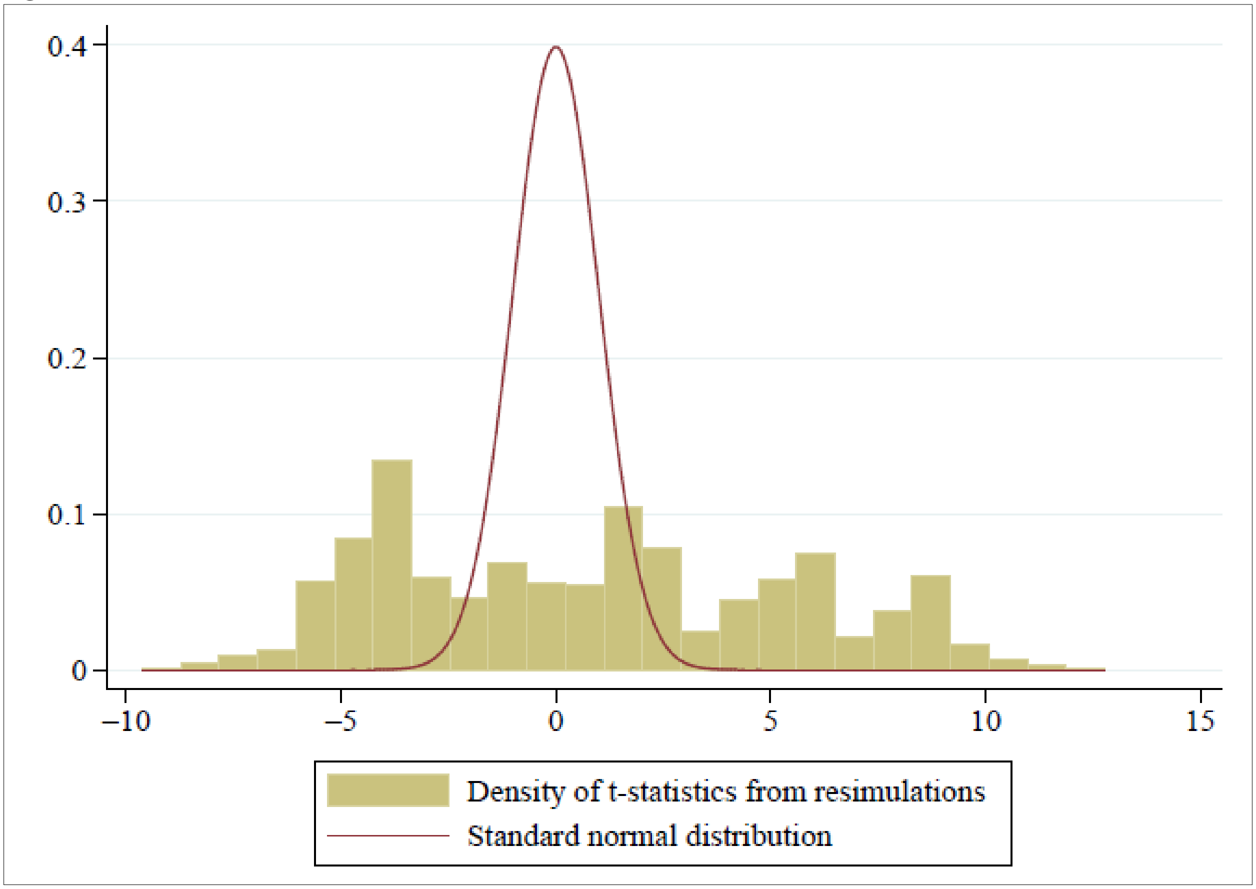
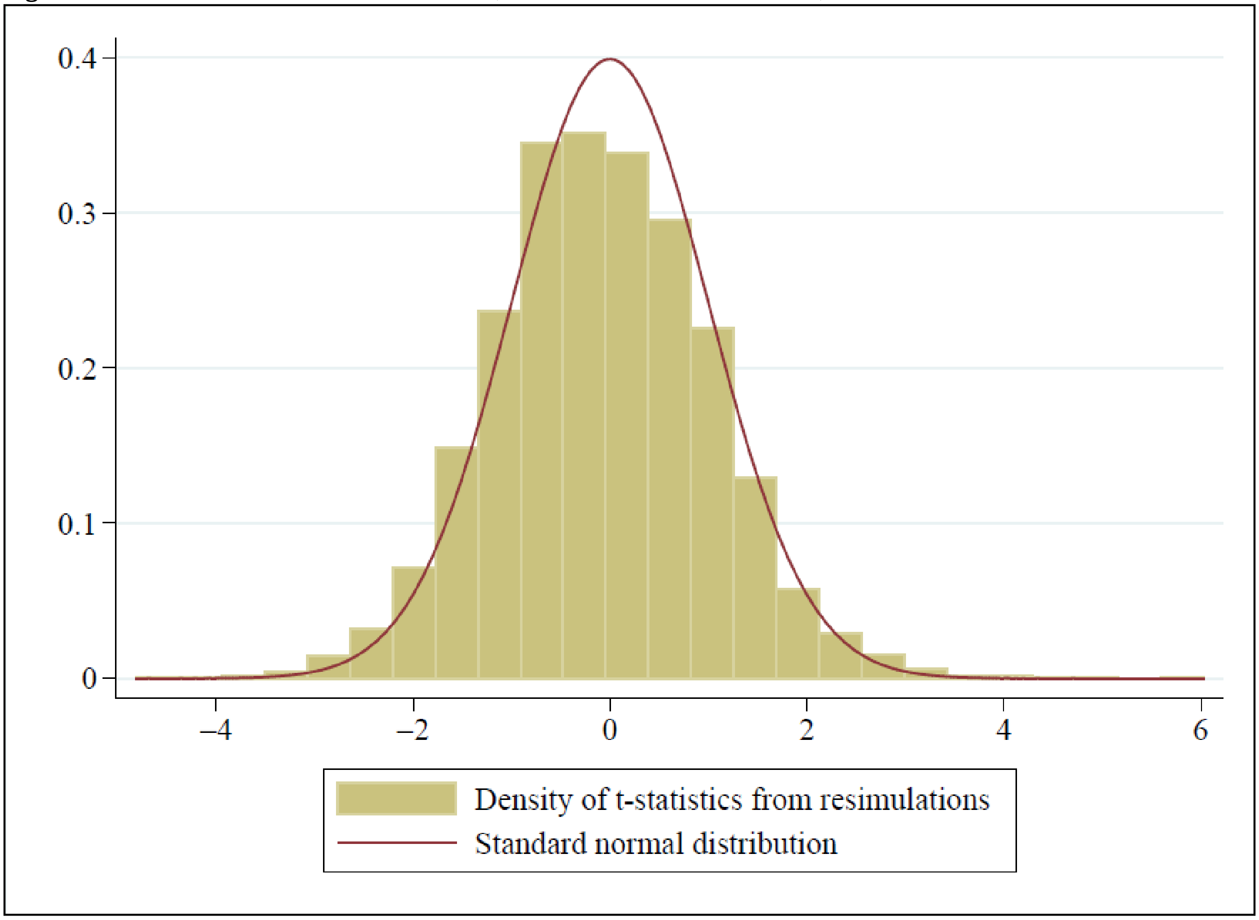
Figure 1 Distribution of t-statistics from 10,000 Resimulations of Model, Lead 8



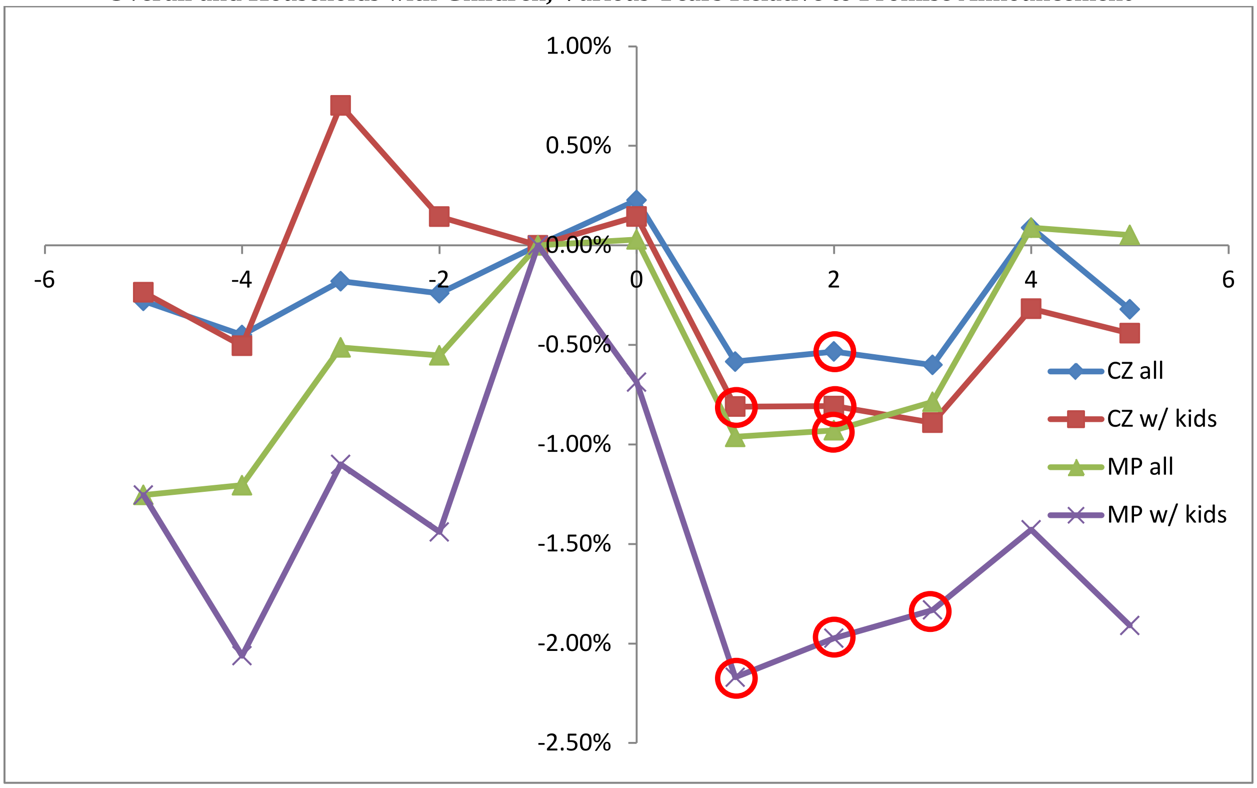
NOTE: These are estimates where the dependent variable is the out-migration variable for the population in households with children under age 18 for the Commuting Zone. The reported t-statistics are for the 8th lead, that is for the dummy variable for eight years after the Promise announcement. This is essentially identified by only one treatment area, that is from the group of comparison areas for Kalamazoo.

Figure 2 Distribution of t-statistics from 10,000 Resimulations of Model, Lead 1



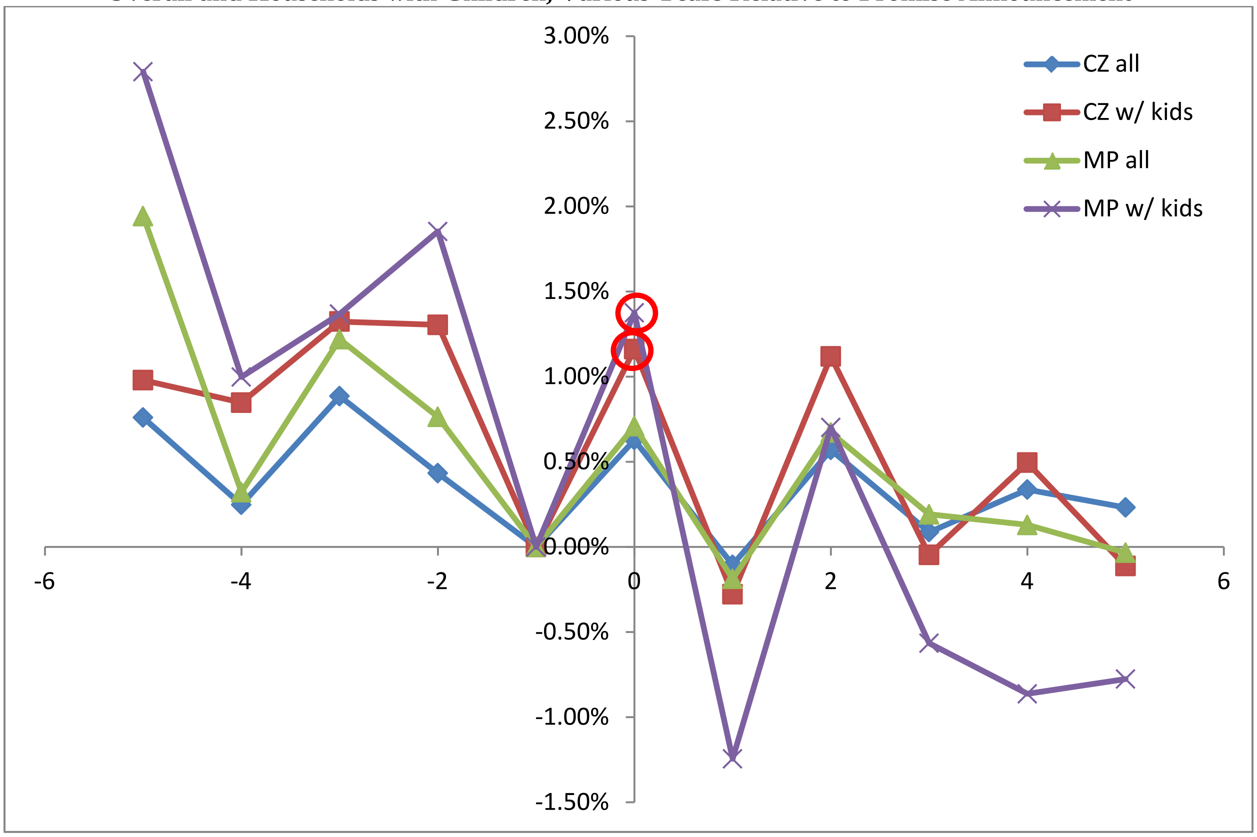
NOTE: These are estimates where the dependent variable is the out-migration variable for the population in households with children under age 18 for the Commuting Zone. The reported t-statistics are for the first lead, that is for the dummy variable for one year after the Promise announcement. This is identified by all eight treatment areas, that is the resimulations potentially select any of the 120 comparison areas.

Figure 3 Out-Migration Effects of Promise Programs, Trends in Commuting Zones and Migration PUMAs, Overall and Households with Children, Various Years Relative to Promise Announcement



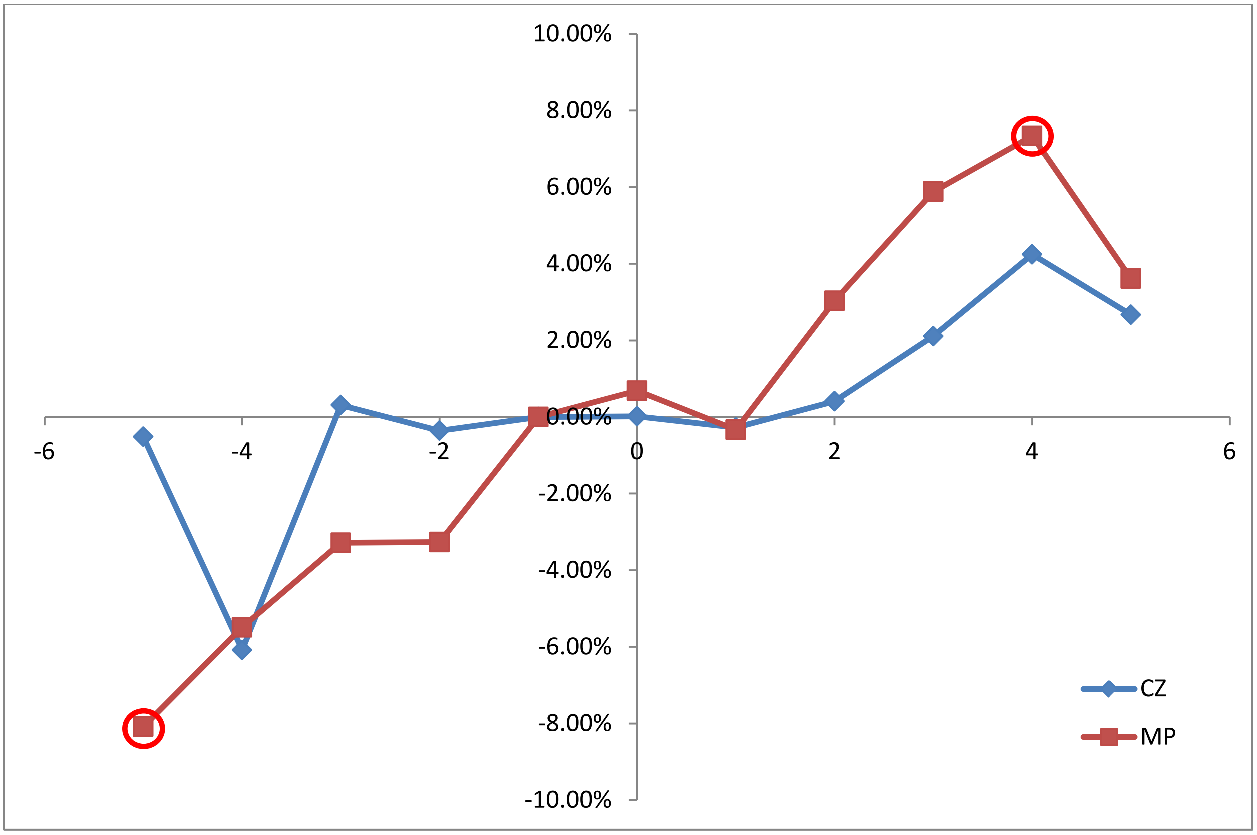
SOURCE: See Table 5.
NOTE: Estimates that are significantly different from zero at 10% level are circled.

Figure 4 In-Migration Effects of Promise Programs, Trends in Commuting Zones and Migration PUMAs, Overall and Households with Children, Various Years Relative to Promise Announcement



SOURCE: See Table 7.
NOTE: Estimates that are significantly different from zero at 10% level are circled.

Figure 5 Housing Price Effects of Promise Programs, for Commuting Zones and Migration PUMAs, Various Periods Relative to Promise Announcement



SOURCE: See Table 8.
NOTE: Estimates that are significantly different from zero at 10% level are circled.

Table 1 Descriptive Information for Promise Programs and Areas

Program	Year Announced	Program Characteristics	Promise area population	Promise area % of Migration PUMA	Promise area % of Commuting Zone
Kalamazoo	2005	School district, universal, expansive, 65-100% of tuition	107,907	32.9%	20.7%
Hammond	2006	City, targeted based on merit & homeownership, expansive, up to $10.5K/yr.	80,830	16.3%	11.4%
Pittsburgh	2007	City, merit-targeted, expansive, up to $5K-$10K/yr varying over time	305,704	24.9%	12.3%
El Dorado, AK	2007	School district, universal, expansive, 65-100% of tuition	26,332	24.8%	32.4%
Syracuse	2009	City, universal, expansive, 100% of unmet need.	145,170	23.4%	13.3%
Arkadekphia, AK	2010	School district, merit-targeted, expansive, 65-100% of unmet need.	16,268	12.5%	7.5%
New Haven	2010	City, merit-targeted, expansive, 65-100% of unmet need up to $10K/yr.	129,779	79.6%	3.6%
Buffalo	2011	City, universal, expansive, 65-100% of unmet need.	261,310	28.5%	11.1%

NOTE: “Universal” refers to not-targeted based on merit or need, although all are targeted based on location and usually on length of residence at location. All programs are “expansive” in that many colleges trigger eligibility for assistance, which is not always the case for Promise-style programs. Also, as noted, all programs pay amounts that are likely to be large percentages of tuition for up to 4 years.

Table 2 Number of Promise Areas Identifying Promise Effects at Various Years Relative to Promise Announcement, Commuting Zone Data and Migration PUMA Data

	Years since announcement														
Area	−6	−5	−4	−3	−2	−1	0	1	2	3	4	5	6	7	8
CZ data															
Kalamazoo							2005	2006	2007	2008	2009	2010	2011	2012	2013
Hammond						2005	2006	2007	2008	2009	2010	2011	2012	2013	
Pittsburgh					2005	2006	2007	2008	2009	2010	2011	2012	2013		
El Dorado					2005	2006	2007	2008	2009	2010	2011	2012	2013		
Syracuse			2005	2006	2007	2008	2009	2010	2011	2012	2013				
Arkadelphia		2005	2006	2007	2008	2009	2010	2011	2012	2013					
New Haven		2005	2006	2007	2008	2009	2010	2011	2012	2013					
Buffalo	2005	2006	2007	2008	2009	2010	2011	2012	2013						
# of Promise areas in CZ data	1	3	4	4	6	7	8	8	8	7	5	4	4	2	1
MP data															
Kalamazoo							2005	2006	2007	2008	2009	2010	2011		
Hammond						2005	2006	2007	2008	2009	2010	2011			
Pittsburgh					2005	2006	2007	2008	2009	2010	2011				
El Dorado					2005	2006	2007	2008	2009	2010	2011				
Syracuse			2005	2006	2007	2008	2009	2010	2011						
Arkadelphia		2005	2006	2007	2008	2009	2010	2011							
New Haven		2005	2006	2007	2008	2009	2010	2011							
Buffalo	2005	2006	2007	2008	2009	2010	2011								
# of Promise areas in MP data	1	3	4	4	6	7	8	7	5	4	4	2	1		

Table 3 Matching of Promise-Including Commuting Zones and Migration PUMAs, with Comparison Areas

Matching variable	Mean: Promise	Mean: Matched	Mean: All non-Promise areas	t-test for significance of difference: Promise and matched	t-test for significance of difference: Promise and all non-Promise	Normalized difference: Promise and matched	Normalized difference: Promise and all non-Promise
PANEL 3A: Commuting Zones							
Out-migration rate (overall)	0.134	0.155	0.269	−1.36	−9.23	−0.442	−1.725
Out-migration rate (HHs w/ children)	0.143	0.168	0.308	−1.23	−8.18	−0.418	−1.721
In-migration rate (overall)	0.117	0.150	0.269	−1.68	−7.92	−0.607	−1.903
In-migration rate (HHs w/ children)	0.139	0.174	0.314	−1.48	−7.47	−0.531	−1.806
ln(median home value)	11.352	11.390	11.279	−0.28	0.54	−0.116	0.200
Poverty rate	0.121	0.118	0.139	0.20	−1.46	0.073	−0.426
Proportion black	0.138	0.118	0.078	0.56	1.72	0.204	0.556
Proportion college-educated, ages 25 & up	0.221	0.210	0.188	0.50	1.56	0.190	0.554
ln(CZ employment)	12.976	12.680	10.980	0.61	4.20	0.237	1.351
Change in ln(employment), 1989–2000	0.094	0.150	0.186	−1.88	−3.09	−0.774	−0.889
Predicted change in ln(employment) based on industry mix, 2000–2005	0.016	0.015	0.022	0.22	−1.23	0.055	−0.160
Average of absolute value of t-stats and normalized differences, all variables				0.91	4.24	0.332	1.026
Panel 3B: Migration PUMAs							
Out-migration rate (overall)	0.159	0.147	0.171	−0.51	0.52	−0.237	0.204
Out-migration rate (pop in HHs w/ children)	0.168	0.154	0.182	−0.53	0.57	−0.242	0.216
In-migration rate (overall)	0.135	0.140	0.178	0.23	1.94	0.104	0.696
In-migration rate (pop in HHs with children)	0.159	0.172	0.211	0.57	2.24	0.246	0.779
ln(median home value)	11.339	11.370	11.480	0.29	1.36	0.109	0.389
Poverty rate	0.142	0.117	0.121	−1.47	−1.23	−0.578	−0.405
Proportion black	0.197	0.108	0.100	−1.84	−2.05	−0.725	−0.739
Proportion college educated, ages 25 and over	0.232	0.200	0.218	−1.49	−0.68	−0.534	−0.181
Average of absolute value of t-stats and normalized differences, all variables				0.87	1.32	0.347	0.451

NOTE: All the migration rate and home value and area demographics are from the 2000 Census PUMS data. Migration rates represent the 5-year migration rate, as that is the only migration information available. CZ employment is as of 2000. This variable and the change in employment from 1989 and 2000 are taken from REIS data from BEA. The predicted change in ln(employment) is based on the Upjohn Institute's WholeData series of detailed industry data based on overcoming suppression in County Business Patterns data. These data are based on a model developed originally by Isserman and Westerveldt (2006). This industry mix prediction is based on year 2000 industry mix in each Commuting Zone, and national industry growth between 2000 and 2005, a methodology described in Bartik (1991). This industry mix prediction proxies for changes in demand for a Commuting Zone's export-base industries. For the matching, there are originally 741 Commuting Zones and 1,024 Migration PUMAs. Therefore, the original comparisons are between 8 Promise areas and either 733 Commuting Zones, or 1016 Migration PUMAs. The matching creates 120 matched Commuting Zones and Migration PUMAs. Because the matches are selected with replacement across different Promise areas (e.g., a given non-Promise CZ or MPUMA could be matched to more than one Promise CZ or MP), for Commuting Zones, there are actually only 82 distinct matched Commuting Zones. For Migration PUMAs, there actually are only 97 distinct matched Migration PUMAs. This matching with replacement may also affect standard errors, which should also be reflected in the t-statistic simulations using random reassignment of treatment status.

Table 4 Descriptive Statistics for Dependent Variables

Dependent variable	mean	sd	min	max	p10	p25	p50	p75	p90
PANEL 4A: Commuting Zones									
Out-migration rate (overall) (%)	4.47	2.05	1.66	15.88	2.44	2.98	3.94	5.53	7.08
Out-migration rate (HHs w/ children) (%)	4.26	2.54	0.64	19.70	1.84	2.46	3.59	5.46	7.38
In-migration rate (overall) (%)	4.41	2.11	1.37	16.08	2.22	2.89	3.98	5.53	7.15
In-migration rate (households with children) (%)	4.35	2.53	1.01	20.91	1.80	2.59	3.81	5.41	7.50
ln(median home value)	11.732	0.359	10.7144	13.0170	11.2898	11.5129	11.6953	11.9184	12.1548
Delogged home value ($)	124,434		45,000	449,999	80,000	100,000	120,000	150,000	190,000
PANEL 4B: Migration PUMAs									
Out-migration rate (overall) (%)	4.24	1.35	0.94	12.31	2.74	3.34	4.09	4.92	5.93
Out-migration rate (HHs w/ children) (%)	3.77	2.02	0.00	14.51	1.68	2.40	3.41	4.65	6.44
In-migration rate (overall) (%)	4.28	1.53	1.12	12.43	2.60	3.19	4.02	5.09	6.34
In-migration rate (households with children) (%)	4.00	2.06	0.00	19.87	1.79	2.55	3.63	5.03	6.72
ln(median home value)	11.744	0.360	10.9151	12.7657	11.2898	11.5129	11.6953	11.8845	12.2061
Delogged home value ($)	125,965		55,000	350,001	80,000	100,000	120,000	145,000	199,999

NOTE: These are descriptive statistics for the dependent variables for overall Commuting Zone and Migration PUMA samples. These descriptive statistics are calculated over 128 areas—8 Promise areas and 120 Comparison Areas. The home values are unadjusted for inflation, which is dealt with through the inclusion of year dummies. P10 means 10th percentile, etc. The percentiles are of the unweighted distribution of values across the areas in the estimation sample.

Table 5 Outmigration Effects of Promise Programs, for Commuting Zones and Migration PUMAs, and for Overall Population and Households with Children, Various Years Relative to Promise Announcement

Year relative to Promise announcement	−6	−5	−4	−3	−2	−1	0	1	2	3	4	5	6	7	8
PANEL 5A: Commuting Zones (Overall) (Mean=4.47%)															
Effect relative to year before Promise announcement (%)	0.00	−0.28	−0.45	−0.18	−0.24	0.00	0.23	−0.58	**−0.53**	−0.60	0.09	−0.32	−0.75	−0.70	0.59
Standard error (%)	0.21	0.20	0.28	0.37	0.22		0.23	0.35	0.29	0.37	0.20	0.56	0.46	0.34	0.34
t-statistic	0.00	−1.39	−1.62	−0.48	−1.12		1.00	−1.68	−1.85	−1.63	0.43	−0.57	−1.63	−2.08	1.75
Usual 2-tail prob	0.996	0.164	0.105	0.628	0.264		0.318	0.093	0.065	0.103	0.665	0.567	0.103	0.038	0.080
Simulated 2-tail prob.	0.999	0.315	0.154	0.693	0.347		0.350	0.124	**0.098**	0.144	0.753	0.638	0.180	0.226	0.727
PANEL 5B: Commuting Zones (Population in Households with Children under 18) (Mean=4.26%)															
Effect relative to year before Promise announcement (%)	0.05	−0.24	−0.50	0.70	0.14	0.00	0.14	**−0.81**	**−0.81**	−0.89	−0.32	−0.44	−0.87	−0.38	0.41
Standard error (%)	0.28	0.34	0.45	0.85	0.40		0.41	0.44	0.41	0.63	0.68	0.34	0.74	0.82	0.50
t-statistic	0.17	−0.70	−1.11	0.82	0.36		0.35	−1.84	−1.97	−1.42	−0.47	−1.31	−1.18	−0.46	0.81
Usual 2-tail prob	0.866	0.482	0.266	0.410	0.720		0.726	0.065	0.049	0.155	0.640	0.192	0.239	0.645	0.416
Simulated 2-tail prob.	0.928	0.664	0.363	0.531	0.760		0.773	**0.085**	**0.073**	0.209	0.717	0.274	0.356	0.807	0.912
PANEL 5C: Migration PUMAs (Overall) (Mean=4.24%)															
Effect relative to year before Promise announcement (%)	−1.11	**−1.25**	−1.21	−0.51	−0.55	0.00	0.03	−0.96	**−0.93**	−0.79	0.09	0.05	−1.10		
Standard error (%)	0.42	0.45	0.82	0.34	0.49		0.43	0.68	0.42	0.57	0.59	0.81	0.56		
t-statistic	−2.64	−2.78	−1.47	−1.53	−1.13		0.07	−1.42	−2.19	−1.37	0.15	0.06	−1.97		
Usual 2-tail prob	0.008	0.006	0.142	0.127	0.258		0.945	0.157	0.028	0.170	0.881	0.949	0.050		
Simulated 2-tail prob.	0.693	**0.069**	0.224	0.220	0.315		0.948	0.202	**0.074**	0.261	0.892	0.981	0.537		
PANEL 5D: Migration PUMAs (Population in Households with Children Under 18) (Mean=3.77%)															
Effect relative to year before Promise announcement (%)	−1.80	−1.25	−2.06	−1.10	−1.44	0.00	−0.69	**−2.17**	**−1.97**	**−1.83**	−1.43	−1.91	−1.84		
Standard error (%)	0.83	0.58	1.02	1.45	1.51		0.96	0.90	0.84	0.74	1.42	0.86	0.90		
t-statistic	−2.18	−2.15	−2.03	−0.76	−0.95		−0.71	−2.41	−2.36	−2.47	−1.01	−2.22	−2.04		
Usual 2-tail prob	0.030	0.032	0.043	0.448	0.341		0.476	0.016	0.019	0.014	0.314	0.027	0.042		
Simulated 2-tail prob.	0.670	0.142	0.132	0.560	0.379		0.483	**0.042**	**0.074**	**0.077**	0.424	0.205	0.503		

NOTE: Model is derived from regression of out-migration rate from last year to this year for this geographic unit and population group, which is explained by geographic dummy fixed effects, year fixed effects, predicted employment growth from last year to this year based on CZ industry mix and national industry growth (for Commuting Zone models only), and set of dummies equal to one for year relative to Promise announcement. Estimated effects are bolded when the probability of a t-statistic of that size, from the 10,000 simulations of the model, is less than 10%.

Table 6 Implications of Out-Migration Estimates for Population Effects of Promise Programs After Three Years, Various Geographic Areas and Groups

	Overall CZ	Part of CZ in MP surrounding Promise area	Remainder of CZ
PANEL 6A: As % of population for that group and geographic area			
Overall pop (%)	**1.72**	**2.68**	0.45
HHs with children (%)	**2.51**	**5.97**	−2.13
Other households (%)	1.17	0.39	2.20
PANEL 6B: As % of overall CZ population			
Overall pop (%)	1.72	1.52	0.19
HHs with children (%)	1.02	1.39	−0.37
Other households (%)	0.70	0.13	0.57

NOTE: All figures are implied population effects due to out-migration effects summed over three years after Promise is announced. Figures in bold in Panel 6A are directly taken from Table 5, and simply sum these three years of effects. The remaining effects are derived from some simple averages of relationships between population sizes of various groups in the sample. Specifically, the simple average over all eight Promise programs included in this study are as follows: 56.93% of the CZ's population is in the Migration PUMA; 40.73% of the CZ's population is in households with children less than 18 years of age; 40.97% of the Migration PUMA's population is in households with children less than 18. From this, one can derive the percentage of the sample in all these various subgroups. This is used to calculate the non-bolded numbers in Panel 6A, and the numbers in Panel 6B. Specifically, Panel 6B is derived by dividing the percentage effect by what proportion each sub-group is of the overall CZ population; interested readers can reverse the process to get these proportions.

Table 7 In-migration Effects of Promise Programs, for Commuting Zones and Migration PUMAs, and for Overall Population and Households with Children, Various Years Relative to Promise Announcement

Year relative to Promise announcement	−6	−5	−4	−3	−2	−1	0	1	2	3	4	5	6	7	8
PANEL 7A: Commuting Zones (Overall) (Mean=4.41%)															
Effect relative to year before Promise announcement (%)	0.04	0.76	0.25	0.89	0.43	0.00	**0.63**	−0.11	0.57	0.09	0.34	0.23	0.25	0.18	1.37
Standard error (%)	0.41	0.73	0.47	0.85	0.48		0.26	0.32	0.46	0.27	0.39	0.48	0.37	0.58	0.33
t-statistic	0.09	1.04	0.53	1.04	0.91		2.39	−0.33	1.24	0.32	0.86	0.48	0.68	0.31	4.18
Usual 2-tail prob	0.925	0.300	0.600	0.298	0.363		0.017	0.742	0.217	0.747	0.388	0.630	0.495	0.760	0.000
Simulated 2-tail prob.	0.982	0.415	0.610	0.509	0.459		**0.035**	0.768	0.275	0.754	0.471	0.744	0.621	0.853	0.437
PANEL 7B: Commuting Zones (Population in Households with Children under 18) (Mean=4.35%)															
Effect relative to year before Promise announcement (%)	0.30	0.98	0.85	1.32	1.30	0.00	**1.16**	−0.28	1.12	−0.05	0.50	−0.11	−0.06	0.24	2.03
Standard error (%)	0.62	0.64	1.02	1.23	1.20		0.46	0.36	0.81	0.53	0.54	0.71	0.57	0.75	0.41
t-statistic	0.47	1.54	0.83	1.07	1.09		2.55	−0.76	1.39	−0.09	0.93	−0.16	−0.10	0.32	4.91
Usual 2-tail prob	0.636	0.123	0.408	0.283	0.277		0.011	0.447	0.165	0.929	0.355	0.875	0.922	0.748	0.000
Simulated 2-tail prob.	0.980	0.253	0.443	0.398	0.346		**0.025**	0.497	0.194	0.948	0.451	0.924	0.938	0.840	0.396
PANEL 7C: Migration PUMAs (Overall) (Mean=4.28%)															
Effect relative to year before Promise announcement (%)	0.28	1.94	0.32	1.22	0.76	0.00	0.71	−0.19	0.67	0.19	0.13	−0.03	0.91		
Standard error (%)	0.56	1.05	0.90	1.06	0.55		0.46	0.59	0.75	0.45	0.74	1.13	0.49		
t-statistic	0.50	1.84	0.36	1.15	1.39		1.55	−0.32	0.89	0.42	0.17	−0.03	1.85		
Usual 2-tail prob	0.616	0.066	0.723	0.250	0.166		0.122	0.752	0.373	0.673	0.862	0.976	0.065		
Simulated 2-tail prob.	0.937	0.197	0.748	0.356	0.244		0.143	0.776	0.413	0.717	0.878	0.983	0.430		
PANEL 7D: Migration PUMAs (Population in Households with Children Under 18) (Mean=4.00%)															
Effect relative to year before Promise announcement (%)	0.93	2.79	1.00	1.37	1.85	0.00	1.37	−1.24	0.70	−0.57	−0.86	−0.78	1.08		
Standard error (%)	0.74	1.10	1.45	1.17	1.24		0.79	0.94	0.92	1.45	1.26	1.00	0.76		
t-statistic	1.26	2.54	0.69	1.17	1.49		1.73	−1.33	0.76	−0.39	−0.68	−0.78	1.43		
Usual 2-tail prob	0.208	0.011	0.492	0.242	0.136		0.083	0.184	0.445	0.696	0.495	0.436	0.153		
Simulated 2-tail prob.	0.791	0.086	0.548	0.307	0.206		0.108	0.236	0.497	0.721	0.557	0.612	0.642		

NOTE: Model is derived from regression of in-migration rate from last year to this year for this geographic unit and population group, which is explained by geographic dummy fixed effects, year fixed effects, predicted employment growth from last year to this year based on CZ industry mix and national industry growth (for Commuting Zone models only), and set of dummies equal to one for year relative to Promise announcement. Estimated effects are bolded when the probability of a t-statistic of that size, from the 10,000 simulations of the model, is less than 10%.

Table 8: Home Value Effects of Promise Programs, for Commuting Zones and Migration PUMAs, Various Years Relative to Promise Announcement

Year relative to Promise announcement	−6	−5	−4	−3	−2	−1	0	1	2	3	4	5	6	7	8
PANEL 8A: Commuting Zones															
Effect relative to year before Promise announcement (%)	3.02	−0.52	−6.08	0.31	−0.36	0.00	0.02	−0.27	0.41	2.11	4.24	2.67	2.15	−4.58	−10.58
Standard error (%)	2.26	4.67	3.61	7.08	2.82		2.34	2.24	2.28	4.07	3.82	4.21	5.25	3.95	2.96
t-statistic	1.34	−0.11	−1.68	0.04	−0.13		0.01	−0.12	0.18	0.52	1.11	0.63	0.41	−1.16	−3.57
Usual 2-tail prob	0.181	0.912	0.093	0.965	0.900		0.993	0.903	0.857	0.604	0.267	0.526	0.682	0.247	0.000
Simulated 2-tail prob.	0.789	0.926	0.157	0.971	0.905		0.996	0.904	0.855	0.590	0.318	0.538	0.691	0.431	0.209
PANEL 8B: Migration PUMAs															
Effect relative to year before Promise announcement (%)	−0.47	**−8.08**	−5.49	−3.28	−3.26	0.00	0.68	−0.33	3.03	5.88	**7.33**	3.61	−2.76		
Standard error (%)	2.75	2.33	5.65	2.05	4.64		3.12	2.71	3.01	4.94	3.59	2.85	2.89		
t-statistic	−0.17	−3.47	−0.97	−1.60	−0.70		0.22	−0.12	1.01	1.19	2.04	1.27	−0.95		
Usual 2-tail prob	0.863	0.001	0.332	0.110	0.482		0.826	0.903	0.315	0.234	0.041	0.206	0.341		
Simulated 2-tail prob.	0.992	**0.010**	0.345	0.176	0.490		0.831	0.880	0.328	0.222	**0.071**	0.391	0.780		

NOTE: Model is derived from regression of ln(home value) for this year for this geographic unit, which is explained by geographic dummy fixed effects, year fixed effects, predicted logarithmic employment level for this year based on CZ industry mix in 2000 and national industry growth (for Commuting Zone models only), and set of dummies equal to one for year relative to Promise announcement. Estimated effects are bolded when the probability of a t-statistic of that size, from the 10,000 simulations of the model, is less than 10%.