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DISCUSSION PAPER SERIES

IZA DP No. 3320

Empowerment Zones, Neighborhood Change and Owner Occupied Housing

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January 2008

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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IZA Discussion Paper No. 3320
January 2008

ABSTRACT

Empowerment Zones, Neighborhood Change and Owner Occupied Housing*

This paper examines the effects of a generous, spatially-targeted economic development policy (the federal Empowerment Zone program) on local neighborhood characteristics and on the neighborhood quality of life, taking into account the interactions amongst the policy, changes in neighborhood demographics and neighborhood housing stock. Urban economic theory posits that housing prices in a small area should increase as quality of life increases, because people will be more willing to pay to live in the area, but these changes in prices and quality of life will also affect the demographics of the population through sorting and the housing stock through reinvestment. Using census block-group-level data, we examine how housing prices respond to the Empowerment Zone policy intervention. Changes in the other dimensions of neighborhood quality (demographics and housing stock characteristics) will also help determine the total, or full effect on housing values of the policy intervention. This paper estimates these direct and indirect effects in a simultaneous equations setting, compares indirect and full effects, and examines the robustness of the effects to alternate estimation strategies. We find strong evidence for substantively large and highly significant direct price effects, while results suggest that the indirect effects are substantively small or even negative.

JEL Classification: R0, R21, R31, R38, R58

Keywords: economic development, empowerment zones, porperty values, household mobility, sorting

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* The authors would like to thank John Winters for valuable, reliable research assistance, participants in sessions at the annual APPAM meetings in 2006, the AREUEA annual meetings in 2008, and participants in the Lincoln Institute of Land Policy “Impact of Public Policy on Land Values” workshop. This research benefited from the support of the Lincoln Institute of Land Policy. This document contains demographic data from Geolytics, Inc, East Brunswick, NJ.

I. Introduction

Spatially targeted economic development policy has been a popular tool for addressing the problem of entrenched concentrations of poverty in urban areas. Such spatially targeted programs usually consist of tax incentives and other off-the-books expenditures. Over the 1980's many states created such programs, generically referred to as enterprise zones,[1] which provide economic incentives (usually through tax abatements) for companies that create jobs in these depressed areas. While the popularity of such programs is irrefutable, the efficacy of spatially targeted development incentives is not well understood. While early case-study research suggested that the programs were effective, more recent research has cast this early consensus into considerable doubt.

During the Clinton administration, the Federal Government created a similar program, called Empowerment Zones (henceforth EZs), which coupled tax incentives and wage credits with large amounts of federal funding for community development. This program was continued during the early years of the Bush administration. At present, the EZ initiative covers over 700 census tracts with a combined population of over 3 million individuals in 31 communities (Greenbaum and Bondonio 2004). Although the generosity of the program has varied over time, total incentives and grant expenditures are valued at over 5 billion dollars, according to the HUD website. Despite, the extent of the program, the literature on the effects of the EZ program is relatively undeveloped, even compared to the more extensive literature on state enterprise zone programs.

[1] Terminology in this field is unfortunately problematic, because the state-level programs have various names. In this paper, we use the term enterprise zone to signify any of the various state programs and Empowerment Zone (or EZ) to refer to the more generous federal program. A federal program called enterprise communities also exists, but this program is more similar to the state programs than the federal Empowerment Zone program.

In this paper, we examine the effects of the federal program over a wide variety of neighborhood-level indicators. We focus on the total effect of the Empowerment Zone intervention, which likely includes not only direct effects, but several types of indirect effects. This approach conceives of neighborhood outcomes as the result of a complicated interplay between economic, demographic and housing market forces. Recent researchers have had trouble finding significant direct effects of spatially targeted economic development programs. By identifying both the direct effects and the indirect effects, our approach offers EZ status its "best chance" to show some positive effect on neighborhood quality.

Our results show that for our preferred measure of neighborhood quality (housing values) EZ status appears to have had statistically significant and substantial positive effects. The effects of EZ status on other neighborhood characteristics was more mixed. The indirect effects vary somewhat depending on specification and estimation method, but are generally either small or negative.

The rest of the paper is organized as follows. Section II reviews the literature on state and federal spatially targeted economic development incentives. Section III lays out a conceptual foundation for our empirical section, discusses the empirical specification, and describes the data. Section IV presents and discusses the results. Section V concludes.

II. Literature

Winnick (1966) lays out a very strong case against place-based policy. The primary justification for spatially targeted economic development programs lies in the persistence of concentrations of poverty, mainly in urban areas.. Kain (1968) framed the problem in terms of the spatial mismatch hypothesis (SMH), which posited that blacks were prevented from commuting or moving to the suburbs, where their labor was demanded, and that low-skill jobs were prevented from moving into the central city, where the low-skill black population lived. The spatial mismatch of low-skill labor supply and low-skill labor demand causes the location-constrained inner-city residents to experience adverse labor market outcomes. Since that seminal paper, spatially-targeted policies have become popular at many levels of government. While the SMH enjoyed several decades of empirical support, more recent work taking into account the endogeneity of residence choice has cast some doubt on the causal relationship between spatial mismatch and poor central city labor market outcomes.[2] Whether the SMH holds or not, it is widely accepted by policy-makers, and spatially targeted economic incentives can be seen as an attempt to correct for the cost differentials that keep businesses from locating in the inner city.

Even in the absence of a causal effect of spatial mismatch, local jurisdictions may wish to spur development within their boundaries to increase tax receipts. It is not far fetched to believe that localized tax incentives could be beneficial for local jurisdictions, even if they had no effect on the indigenous population. Bartik (1991) reviewed the literature on the effects of local taxes on business activity and found that the elasticity of business activity with respect to local tax rates lay somewhere between -1 and -3. If this is true, decreasing local taxes (even in a small section of the jurisdiction) could be

[2] Gurmu *et al.*(2006) uses panel data to control for individual-specific fixed effects, finding that access to employment has little effect on employment outcomes for their sample of Atlanta-area TANF recipients. Kling *et al.* (2004) use the random assignment of neighborhood achieved in the Moving To Opportunity experiments to look at the effects of job access, and find that the experimental group (who were encouraged to move to low-poverty neighborhoods) did not have better labor market outcomes.

revenue-enhancing for local governments.[3] These large elasticities suggest that the effects of local tax incentives may be large, and that enterprise zones may be an effective policy tool from a local perspective.

Research examining the effects of spatially targeted incentives has concentrated on the various state programs. While many studies have found that enterprise zones have faired well in terms of employment, Boarnet (2001) points to the many methodological pitfalls inherent in straight comparisons of zones to non-zone areas. More rigorous evaluations of the state programs have not been lacking. An extensive review of this literature can be found in Peters and Fisher (2002). They find that while early econometric studies of the effects of state enterprise zones usually found positive results,[4] more recent results have been much less favorable.[5] Peters and Fisher offer several possible explanations for this set of findings. They suggest that the tax incentives are not generous enough to overcome the substantial disadvantages associated with the targeted areas. They also suggest that the administration of zones, which often put conditions on the incentives that exist, may reduce their attractiveness. Bondonio and Greenbaum (2007) suggests that the insignificant net effects mask countervailing positive effects on

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[3] These elasticity figures pertain to changes in business activity *within a metropolitan area*. Elasticities are of much smaller magnitude (between -0.1 and -0.6) when comparing changes in business activity across large areas. This implies that any tax advantages a jurisdiction might expect are coming primarily from other near-by jurisdictions, not through the attraction of business from other parts of the country. Of course, in the case of targeted incentives, the lower taxes may be drawing businesses away from other parts of the *same* jurisdiction. Such possibilities complicate any kind of cost/benefit analysis of such programs. In this paper we focus only on the local effects of the program, not the measurement of the benefits.

[4] Erickson and Friedman (1990), Papke (1993), Papke (1994) are examples.

[5] Boarnet and Bogart (1996), Greenbaum (1998) Greenbaum and Engberg (2000), Engberg and Greenbaum (1999), Bondonio and Engberg (2000) and Peters and Fisher's (2002) own analysis all point towards this conclusion. Elvery (2004) is another very careful analysis that finds insignificant results of enterprise zone status.

new firms and negative effects on existing firms (who exit the zone), along with a number of other interesting results.

The literature examining the effects of the federal Empowerment Zone program is much less developed. Wallace (2003) examines the probability of an EZ applicant being selected, while Greenbaum and Bondonio (2004) examine how the selection of federal EZs has changed over the three rounds of the program. Oakley and Tsao (2006, 2007a, b) use propensity score matching, as in much of the recent literature on the state programs, to examine the effect of Chicago's and some other Empowerment Zones on a variety of socio-economic neighborhood outcomes. While they find some localized effects (*e.g.* on poverty and related variables in the case of Chicago's zone), they characterize the effects as underwhelming. When pooling the four zones[6], the intervention had no significant effects on poverty, unemployment or average household income.

While most of the studies mentioned above examine job creation or employment outcomes, our primary variable of interest will be the value of owner-occupied housing in a neighborhood. While we will also be examining the effect of EZ status on employment outcomes of neighborhood residents, this more traditional variable takes a secondary position in our analysis. This is because the empowerment zone program is supposed to improve neighborhoods along a variety of dimensions (McCarthy 1998), not just improve employment outcomes. As such, the general quality of life in a neighborhood should be improved by the program. If the program is successful in making a neighborhood more attractive, the price of housing should increase (Rosen 1974, Bartik and Smith 1987).

[6] The other three zones were located in Baltimore, Detroit and New York City. The analysis of all four zones is carried out in Oakley and Tsao (2006). The other two papers focus exclusively on Chicago.

Our empirical approach will allow us to examine the effects of EZ status on other variables of more traditional concern (employment outcomes, poverty, *etc.*), but housing values will be the main variable of interest.

III. Empirical model and data

A. Empirical model

The empirical model used here follows closely on Noonan *et al.* (2007). We refer the reader to that paper for the details of the model, and focus here on its highlights. The hedonic approach generally uses cross-sectional data to predict housing prices. A national database of individual home prices would be required to analyze a national program such as Empowerment Zones in this way. Such a data base is not available, so we are forced to use neighborhood averages as proxies for these individual values. The use of aggregated data, even at the neighborhood level, limits our ability to infer price effects at the individual level. Nonetheless, some hedonic research has shown that estimates using aggregate data produce reasonably accurate results (Freeman 1979, Nelson 1979, O'Byrne *et al.* 1985).[7] Noonan *et al*. (2007) also find generally plausible implicit prices in OLS estimations using aggregated data. Moreover, the median housing value in a neighborhood is of considerable policy import. Learning more about the effects of a policy on this neighborhood measure is informative, even if it does not recover the true underlying hedonic price. The results based on such aggregate measures

[7] See Shultz and King (2001) for additional review of the use of aggregated Census data in hedonics. Greenstone and Gallagher (2005) use a similar data set for their analysis of superfund designation, although they use the larger geography of the census tract.

can be viewed in an epidemiological light; the effects of average policy exposure on average outcomes, while perhaps not the ideal, are nonetheless interesting.

An advantage of our data is that these neighborhood averages can be observed over time. One potential problem with a simple OLS approach to the hedonic equation in levels is that some neighborhood characteristics will be unobserved and correlated with the other variables of interest. This may be especially important in the context of EZs, since EZ designation was not randomly distributed, but was targeted at distressed neighborhoods (Greenbaum and Bondonio 2004). To mitigate this problem, we estimate the model in first differences. This strategy purges our parameter estimate of bias from the omission of time-invariant variables (Mendelsohn *et al.* 1992, Zabel 1999), and we thus identify the parameters from within-neighborhood changes in neighborhood quality, neighborhood demographic conditions, and housing structural characteristics. Our primary equation of interest can be expressed as in equation 1,

$$(1) \qquad \dot{P}_{it} = \beta_0 + \beta_{EZ}\dot{E}Z_{it} + \beta_S \dot{S}_{it} + \beta_N \dot{N}_{it} + \beta_M \dot{M}_{it} + \dot{\varepsilon}_{1it},$$

where t indexes time, i indexes neighborhoods, $\dot{X}_{it} = X_{i,t} - X_{i,t-1}$, P is the median house value value, S is a vector of structural characteristics of the neighborhood housing stock, N is a set of neighborhood demographic characteristics and M is a vector of municipal characteristics such as public services and taxes that may vary with time. Differenced out of this equation are any time-invariant geographic factors that affect price (such as distance to the CBD, metropolitan-wide factors, views or unobserved quality of the neighborhood).[8] Finally, the *EZ* variable allows the designation of a neighborhood as an

[8] In the results reported below, we allow for some of these geographic factors to affect median price appreciation.

Empowerment Zone to have an independent effect on neighborhood attractiveness. Such an effect is possible if EZ tax incentives increase employment in the area, or the federal funds are used to improve neighborhood quality, or lower taxes.

It is likely that many of the variables in equation (1) are set simultaneously with price, however, so that equation (1) is part of a larger system. If changes in neighborhood quality also affect the types of housing and demographic characteristics, it will be important to control for the simultaneity bias when estimating the direct and indirect effects of federal intervention on home values, as is the goal of this paper. We model the neighborhood housing stock as a partial adjustment process, with current levels a function of lagged levels and other variables. For comparability to equation (1) and to avoid problems associated with unobserved effects being correlated with independent variables, we run all our regressions in first differences, as in equation (2):

(2) $$\dot{S}_{it} = \gamma_S \dot{S}_{it-1} + \gamma_{0t} + \gamma_{EZ} \dot{EZ}_{it} + \gamma_N \dot{N}_{it} + \gamma_P \dot{P}_{it} + \gamma_M \dot{M}_{it} + \dot{\varepsilon}_{2it} .$$

Here, the housing stock depends on its past levels, Empowerment Zone status, neighborhood demographics, price and other considerations. The kind of housing built in a neighborhood depends upon past levels because housing is a very durable asset, and changes in the housing stock (at the aggregate level) will be gradual. These structural characteristics might also depend on EZ designation if program funds are used to clear abandoned housing or to subsidize construction of new housing. The housing stock may also depend on the neighborhood demographics (if rich people demand different kinds of housing than poor people), municipal-level variables (zoning restrictions, tax treatment) and geographic variables (which are differenced out of equation (2)). Finally, the price of housing may affect the kind of housing built because housing is produced using land and capital. Production theory suggests that if land becomes more expensive, some substitution towards more capital would be expected. Since the value of a housing unit (our price variable) includes the value of the land on which it sits, some effect should be expected, although the sign depends on substitution elasticities in the production and consumption of housing services.

We apply similar logic to the modeling of neighborhood demographic characteristics. Neighborhood demographics follow a partial adjustment process, and we difference the equation to control for unobserved fixed effects.

(3) $$\dot{N}_{it} = \delta_N \dot{N}_{it-1} + \delta_0 + \delta_{EZ} \dot{E}Z_{it} + \delta_S \dot{S}_{it} + \delta_P \dot{P}_{it} + \delta_M \dot{M}_{it} + \dot{\varepsilon}_{3it}.$$

In equation (3), *N* follows a partial adjustment process, where lagged changes in demographics are persistent because housing market frictions prevent neighborhood demographics from reaching their equilibrium levels between periods. Demographic groups' differing demands for neighborhood quality may cause them to sort into neighborhoods being improved by EZ programs according to their willingness to pay for these attributes (Diamond and Tolley, 1982). Similar sorting according to municipal characteristics would be expected. Similarly, changes in housing stock may attract different types of residents, at least when the capital stock is somewhat inelastic. Finally, the price level in a neighborhood could affect neighborhood demographics if certain demographics are "priced out" of a neighborhood when prices increase.

The system of first-differenced equations (1)-(3) can be represented in matrix notation as in equation (4).

(4) $$\begin{bmatrix} 1 & -\beta_S & -\beta_N \\ -\gamma_P & 1 & -\gamma_N \\ -\delta_P & -\delta_S & 1 \end{bmatrix} \begin{bmatrix} \dot{P}_{it} \\ \dot{S}_{it} \\ \dot{N}_{it} \end{bmatrix} = \begin{bmatrix} \beta_{EZ} \\ \gamma_{EZ} \\ \delta_{EZ} \end{bmatrix} \dot{E}Z_{it} + \begin{bmatrix} 0 \\ \gamma_S \dot{S}_{t-1} \\ \delta_N \dot{N}_{t-1} \end{bmatrix} + \begin{bmatrix} \beta_M \\ \gamma_M \\ \delta_M \end{bmatrix} \dot{M}_{it} + \begin{bmatrix} \dot{\varepsilon}_1 \\ \dot{\varepsilon}_2 \\ \dot{\varepsilon}_3 \end{bmatrix}.$$

In this paper, we are specifically interested in the total effect of the EZ policy intervention. System of equations (4) shows us that these effects depend on its direct effect (β_{EZ}), *and also* on its indirect effects. Totally differentiating and dividing through by $d\dot{E}Z$ yields:

(5) $$\begin{bmatrix} 1 & -\beta_S & -\beta_N \\ -\gamma_P & 1 & -\gamma_N \\ -\delta_P & -\delta_S & 1 \end{bmatrix} \begin{bmatrix} d\dot{P}/d\dot{E}Z \\ d\dot{S}/d\dot{E}Z \\ d\dot{N}/d\dot{E}Z \end{bmatrix} = \begin{bmatrix} \beta_{EZ} \\ \gamma_{EZ} \\ \delta_{EZ} \end{bmatrix}.$$

The total effect in neighborhood housing prices due to the implementation of the Empowerment Zone policy is thus available through the application of Cramer's Rule:

(6) $$\frac{d\dot{P}}{d\dot{E}Z} = \frac{\beta_{EZ} + \beta_S\gamma_{EZ} + \beta_N\delta_{EZ} + \beta_S\gamma_N\delta_{EZ} + \beta_N\delta_S\gamma_{EZ} - \beta_{EZ}\gamma_N\delta_S}{1 - \beta_S\left(\gamma_N\delta_P + \gamma_P\right) - \beta_N\left(\delta_S\gamma_P + \delta_P\right) - \gamma_N\delta_S}.$$

The direct effect on price is captured by the first term in the numerator. The next two terms are the first-order indirect effects: $\dot{E}Z$'s effect on $\dot{P}$ through $\dot{S}$ and $\dot{N}$. The third and fourth terms are the second-order indirect effects: $\dot{E}Z$'s effect on $\dot{P}$ through $\dot{S}$'s effect on $\dot{N}$ and $\dot{N}$'s effect on $\dot{S}$. The negative term corrects for double counting. The denominator accounts for the bidirectional effects of $\dot{P}$ on $\dot{S}$ and $\dot{N}$ and their effects back on $\dot{P}$. If there is no simultaneity in equation (4) this total derivative reduces to the first three terms in the numerator.

As in Noonan *et al.* (2007), the system of equations is considerably more complex because *S, N* and *M* are vectors. Hence, we assume that each variable in *S* depends on its own lag; the vectors *EZ, N*, *M* and *G*; *and* the contemporaneous values of the other variables in *S*. Likewise, each *N* variable depends on its own lag; the vectors *EZ*, *S*, *M* and *G*; *and* the contemporaneous values of the other variables in *N*. The system in equation (4) thus models each $\dot{S}$ and $\dot{N}$ equation as dependent on that variable's own lagged difference, $\dot{E}Z$, $\dot{M}$ and the rest of the endogenous variables.

Intuitively, the suite of policies represented by the designation of an area as an urban EZ is meant to have several effects on a neighborhood. On the one hand, if the money is spent on beautification, increased police patrols, or improved social services, there could be a direct improvement in the attractiveness of the neighborhood. Such improvements would increase the demand for housing in the neighborhood, and increase the price of housing there. On the other hand, a stated goal of the EZ program is to improve the employment situation for zone residents. If the program is successful, unemployment or poverty in the area may decrease. If high unemployment or poverty decreases property values,[9] then the EZ policy would have this indirect effect on housing values through neighborhood composition. If program money is used to improve the housing stock (demolition of abandoned properties), and that improvement effects prices in the neighborhood (because the least valuable houses were demolished, or because the abandoned houses had been driving prices down), then there will be an indirect effect of the program on prices through improvements in the housing stock.

This paper tries to disentangle both the direct and indirect effects of EZ program participation. To this end, the system of equations represented in (4) is estimated simultaneously. To do so, we require at least one exogenous variable for each endogenous variable in each equation. The partial adjustment theory used to generate the empirical equations suggests the twice-lagged levels of each variable will be both

[9] This effect could be either direct (people having a direct preference to live near more affluent people) or indirect (decreased poverty leads to lower crime, which makes the neighborhood more attractive).

exogenous and excludable in the context above. These excluded variables will be sufficient to identify the system and allow estimation.[10]

A simpler method for obtaining indirect effects of the policy is available. In estimating equation (1) with OLS, the coefficient β_{EZ} represents the partial or direct effect of the Empowerment Zone policy intervention on prices, holding other endogenous variables constant. However, if equation (2) were estimated constraining β_S and β_N to be zero (equivalently, omitting the endogenous variables from the regression), the returned coefficient on the policy variable *EZ* will represent the effect of the policy intervention holding *nothing* constant. In other words, estimation of a price equation containing only the exogenous variables and *EZ* will return an unbiased estimate of the full effect computed in equation (6). The difference between the direct and full effects is the indirect effect. While this approach to the indirect effects makes it impossible to trace the avenues by which the indirect effects are generated (through *S* or through *N*), it is simple and probably more robust to misspecification than the systems approach. For that reason, in this paper we will compute indirect and full effect by both methods.

B. Exogeneity of *EZ*

Up to this point, we have assumed that the designation of a neighborhood for EZ status is exogenous. This is a dubious assumption. Greenbaum and Bondonio (2004) show that EZs are less populated; are poorer; have more minorities, unemployment and

[10] Specifically, in the basic model, there is one price variable *P*, seven demographic variables in *N*, and four structure variables in *S*, leaving 12 equations in the system. Each *N* and *S* equation includes a lag of the dependent variable in the partial adjustment model. Thus each *S* and *N* equation has 12 endogenous variables, while the *P* equation has 11. Twice-lagged levels of *P*, *N* and *S* are available as excluded instruments for each equation, leaving the *S* and *N* equations just-identified. The *EZ*, *M* and *G* vectors serve as their own instruments.

renters; and have depressed housing values. Wallace (2003) shows that – conditional on applying to become an EZ – an area was more likely to be designated an EZ if it was closer to the urban center, had higher poverty or was in a state with less experience with enterprise zones or more experience applying for federal funds.

This non-random selection of EZs has been an important problem for researchers studying their effects. In the context of state programs, Greenbaum and Engberg (2000) use propensity scores to select a comparable sample of zip codes for comparison of the effects of targeted incentives and compare the effects of actual zone selection versus zip code characteristics. They find that on average, enterprise zones became worse, relative to non-zones, over the 1980's, but that once you control for area characteristics, the effects of being in a zone were mostly insignificant. Elvery (2004) uses propensity score matching and, after considerable effort, is able to get the estimated effect of being in the Florida or California state enterprise zone programs back to insignificant. (More naïve estimates suggested negative effects.) The possibility that program administrators are less likely to spend valuable resources on areas that are likely to have substantial rejuvenation in the absence of the program suggests that any estimate of the EZ program's effects will produce downward biased effects if the special nature of the treatment group is ignored. In this analysis, we address this problem in two ways.

The first way that the problem is addressed is through the differencing outlined above. If unobserved area characteristics are causing housing prices to be lower, residents to be poorer and less employable, and housing to be less well-maintained, the first-differencing of all the equations and the resultant focus on *changes* will get rid of these effects.

If EZ status is granted to areas where unobserved factors are causing a relative stagnation in a neighborhood, however, then even the first-difference coefficients will be biased down. If there is something about the EZ neighborhoods that is causing them to become worse, degrading the housing stock and impoverishing the residents, then leaving this factor out will bias our results. Obviously, in a study that is national in scope, it is impossible to directly control for all these factors. Our second tactic is to seek out a comparison group that could reasonably be assumed to share trends in most of these unobserved factors, and compare the EZ group to this comparison group.

To this end, we use the timing of the EZ program to identify such a group. Neighborhoods were granted EZ status in three waves: Round 1 in 1994, Round 2 in 1998 and Round 3 in 2001. It is reasonable to assume that the neighborhoods that entered the EZ program in these three waves are similar in the unobservable qualities that may negatively impact property values, neighborhood demographics and the upkeep of the neighborhood housing stock. However, our data period ends in April 2000, when the 2000 census was conducted. It is unreasonable to expect that selection into the 2001 round of EZ designation would have any causal effect on 1990-2000 trends in housing values, neighborhood demographics or housing stock.[11] Thus, we take the experiences of the Round 3 EZ neighborhoods as representing the counterfactual of what would have happened to the Round 1 EZ neighborhoods had the policy intervention not occurred, conditional on observables and time-invariant unobservables. Our empirical equations all include controls for Round 2 EZ status and Round 3 EZ status so that the coefficient on

[11] The legislation enabling the third round of Empowerment Zones did not pass the legislature until Dec. 21st, 2000. Workshops for interested applicant jurisdictions occurred in June, 2001. Selection occurred on the last day of 2001, with the designation becoming effective the next day. Given this timeline, it is unlikely that even expectation effects could have increased prices in early 2000 in Round 3 EZs.

Round 1 ("EZ1") status will reproduce an estimate of the direct effect of EZ selection on each of the neighborhood indicators we examine.

The validity of this approach rests on the equivalence of the "unobserved effect" for the Round 1 and Round 3 EZ neighborhoods.[12] The approach is valid whether HUD administrators attempted to use the program to help especially distressed neighborhoods, or whether they attempted to pick neighborhoods that were likely to rebound on their own to make the program look successful. If the decision rule (concerning the unobserved factors) changed between 1994 and 2001, the approach will fail to control for policy endogeneity. Obviously, this cannot be directly tested. Greenbaum and Bondonio (2004) compare the tracts selected in the three rounds and find that Round 3 and Round 1 census tracts are not significantly different in median income and in value of owner-occupied housing, although they differ in many other (observable) respects.[13] They also show that the relationship between the probability of selection into an EZ and various observable characteristics differed between rounds, and that in the later rounds selection appears to depend less on observable characteristics.[14] In our data, across the 17 neighborhood-level variables we examine, the difference in the *changes* experienced over the 1980's for Round 1 and Round 3 neighborhoods are statistically different from one another for nine variables, it was substantively large in eight, and it was both statistically and

[12] It bears emphasis that if unobserved factors *in levels* differ, they will be differenced out. Only difference in the *changes* in unobserved factors will affect our results.

[13] It should be stressed that while the statistical significance of these difference is often extreme, the substantive differences are less extreme except in the case of population density. Third round EZ neighborhoods still have relatively high unemployment, poverty and minority rates, and low education, rental and ownership rates.

[14] This last result could be arising because the selection process was becoming more focused on the unobservables, or because the selection process was becoming more random.

substantively significant in seven cases.[15] In our empirical section, the control variables will take care of the half of the variables that differ in observable ways between Round 1 and Round 3 EZs. We will have to hope that any remaining unobserved "distressed neighborhood" effect is time-invariant and washed away in the first-differencing or that the time-varying unobservable effect is the same for Round 1 and Round 3 EZs.

C. Data

We use neighborhood aggregate data to estimate the system of equations described in part A. We use block-group level census data for the census years 1980, 1990 and 2000 from Geolytics®, Inc., which processes the data into constant census 2000 geographies. The constant geographies allow us to take the neighborhood (block group) as the unit of observation, and observe developing neighborhood outcomes as time elapses.

There are four types of variables in the empirical model sketched in part A: *P*, *N*, *S* and *M*. *P* is measured with the log of the median housing value as reported in the census long form. In our baseline models, the neighborhood composition variables, *N*, include the *percent* of families with at least one worker, the proportion of households with incomes at or below 150 *percent* of the poverty line, the log of median household income, the proportion of people reported as being white and non-Hispanic, the average commute time for workers and the population density. These variables were chosen either because they have been shown in hedonic studies to affect housing values, or

[15] Statistical significance is measured at the .1 confidence level. Substantive significance here means that the difference in means was greater than 50% the pooled average of the means for each round. The sign of the mean was always the same across rounds.

because they are variables of special interest in the local economic development literature.[16] In addition, five more neighborhood composition variables are included in the "extended" models. These include the proportion employed in manufacturing industries, the proportion of households who rent their property, the proportion of the population that lived in the same home five years prior to the census, the proportion of households that have children, and the proportion of the population aged over 25 with a college degree. All these variables are measured as changes from 1990 to 2000.

The housing stock variables, *S*, include the vacancy rate of neighborhood housing units, the median year built, the proportion of neighborhood housing units that are detached, single family units, the average number of rooms and the average number of bedrooms. These are most of the relevant variables included in the census long form. In some equations, we also add the proportion of neighborhood housing units with complete plumbing. All these variables are measured as changes from 1990 to 2000.

The variables discussed thus far are all endogenous: they are part of the system of equations. We also include several exogenous variables. The municipal-level variables, *M*, include measures for the median income, housing value and rental rate for the census-defined place that contains the neighborhood; the proportion of families that have children and families that are "traditional families" with children; and the number of

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[16] Population Density and the proportion white are examples of the former. The family labor market variable is an example of the latter. The inclusion of the poverty and income variables could be justified by either rationale. The commute time variable is also justified by both rationale: standard urban economic theory posits that house values should be declining as commuting time increases (all else equal), while the spatial mismatch hypothesis implies that workers in distressed inner-city neighborhoods will need to commute longer to find employment.

households in the place.[17] These variables are meant to capture the municipality's tax-base (income, housing values, rents and household count) and service provisions (the family variables). We also include a variable derived from the National Center for Education Statistics (NCES) School District Demographic System (SDDS) and the 1992 and 2002 Census of Governments measuring per pupil expenditure in the elementary or unified school district that contains the centroid of the block group as a measure of public service quality. These variables are measured as changes from 1990 to 2000.[18] In all the results reported for the price equation, we also include a battery of location characteristics, including a county-level amenity score, MSA-specific fixed effects and distance to the nearest historic city center interacted with metropolitan dummy variables. As time-invariant geographic attributes, these variables enter the structural model interacted with time, thus relaxing the assumption of constant hedonic prices over time for these characteristics. In general, the addition of these variables did not affect the model appreciably.

Finally, our variable of interest, *EZ*, includes three dummy variables. These variables indicate in which round of the program the block group was included, and are mutually exclusive. The specification of these variables are two dummy variables (*EZ1* and *EZ2*) equaling one if the block group fell into the first or second round, and a third dummy variable (*EZever*) that equals one if the block group was ever in an EZ, including the post-2000 census Round 3. *EZ1* and *EZ2* can be interpreted as changes in EZ status

[17] The place is the census's closest approximation of the municipal or jurisdictional geography. For areas falling outside any place, the county-level values are used, since such areas will presumably get their services from a county government instead of a municipal government.

[18] These place-level variables are considered exogenous on the logic that any one block group will make up a small-enough proportion of the place that it has negligible effect on place-level averages.

over the course of the 1990's, while the variable *EZever* is merely a control variable, as discussed above. The interpretations of the coefficients on *EZ1* and *EZ2* is thus the effect these variables have on the dependent variables, controlling for the fact that they have the unobserved "distressed" characteristic, as represented by the *EZever* variable.

The empirical model also includes the lagged differences of many of these variables (the changes in all the *S* and *N* variables from 1980 to 1990). These variables will also be endogenous given our assumptions. Therefore, consistent estimation requires that each of these lagged differences be included as a separate equation in our system of equations. The twice-lagged levels of all the variables serve as instruments for these lagged differences *and* for the contemporaneous differences in our equations of interest (equation (4)). While the coefficients on the twice-lagged levels will not be consistent in the unreported regressions, we only need the fitted values they produce to be orthogonal to the error terms in the equations represented in (4). Our theory suggests that this will be the case.

Table 1 presents the average changes for all the variables for which we report results. This table presents averages for the full sample of block groups (N≈196,000), the sample of block groups within MSA's (N≈107,000), and the sub-sample average changes for block groups in each round of EZs. To control for the possibility of persistent area-wide changes, we run all regressions on differences from metropolitan averages. This is equivalent to including a set of metropolitan-area dummies in every regression in that all coefficients are identified off of variation *within* the metropolitan area, not across. It is worth noting the strong appreciation of Round 1 Empowerment Zones and the weak appreciation of Round 3 Empowerment Zones. This is a pattern that is preserved in the regression results reported below.

IV. Results

The results are reported in Tables 2, 3 and 4. Tables 2a and 2b report the results from OLS regressions for the full sample of block groups and the sub-sample of metropolitan block groups, respectively. Tables 3a and 3b report coefficients for the endogenous variables in the price equation and the *EZ1* variable's coefficient in the other equations for the two samples. Finally, Table 4 reports direct, full and indirect effects of the policy intervention on block group housing prices, as computed from the various specifications and models.

Turning first to the results in Tables 2a and 2b, most of the coefficients are fairly robust to the inclusion of additional control variables. The primary variable of interest (*EZ1*) is positive, significant and substantively large: whether the additional demographic variables are included or not, the results indicate that EZ designation leads to an eventual increase in median home value of about 25% in the full sample and 27% in the MSA sample. These are extremely large effects. This in itself is a novel finding, as Greenbaum and Engberg (2000) find no statistically significant effects on land values for several state enterprise zone programs. These results may be the product of a bias from some unobserved effect making selection into the EZ program in the first round more likely. However, two considerations lead us to believe that upward bias is not the cause of these large coefficients. First, most previous research on state programs has found a downward bias that must be eliminated through more sophisticated techniques.

Furthermore, Greenbaum and Bondonio (2004) show that, in terms of *observables*, third round EZs were generally less distressed than first round EZs. If first round EZs are more distressed in *trends* as well as levels, our identification strategy would lead us to underestimate the effect of the EZ program. As we will see below, the significance of this coefficient is extremely robust.

There are at least four reasons why these effects appear so much larger than those in Greenbaum and Engberg (2000) and other appraisals of state enterprise zone programs. First, and most obviously, the EZ program differed from most state programs in that it offered not only tax incentives but substantial grants. The influx of federal dollars and the community improvements that can be achieved with that spending may have a larger effect on land values (or other variables) than tax incentives alone. Second, the fact that the EZ program is federal, included federal tax incentives (usually along with state and local incentives), probably means that the tax incentives were stronger than in state enterprise zones. Also, the federal EZ program may have gotten better publicity in local media, helping businesses become aware of the potential benefits. Finally, there are measurement issues that suggest the difference between these results and previous results may be (slightly) over-stated. Most previous studies have been forced to use zip codes for information on employment, or other neighborhood outcomes. Zip code boundaries do not match well with state enterprise zone boundaries. Even if a study uses census tract or block group data, state enterprise zones were not drawn according to census geographies. Researchers are forced to assign zone status to partially designated zip codes (or census tracts) according to some decision rule. However justifiable that decision rule is, it will mean that the independent variable “enterprise zone” is measured with error, biasing the coefficient towards zero. With the federal *Empowerment* Zone program, zone boundaries were drawn to match very closely to 1990 census geographies. Thus, the measure of program status we are able to obtain for the federal program is relatively error free. This eliminates a downward bias that is present in most state enterprise zone studies.

The coefficient on *EZever* is also of interest. The negative, significant coefficient on this variable implies that if the Round 1 EZ block groups had not gotten the EZ policy intervention they would have been witness to substantial declines in home values over the 1990s. This is picking up the "distressed neighborhood" effect that would bias down the coefficient on *EZ1* if *EZever* was not also included. These broad conclusions – that the direct effect of the empowerment zone program is significant and positive and that the "distressed neighborhood" proxy is significant and negative – are robust to sets of control variables, sample composition and estimation method.

Tables 3a and 3b present results from the system of equations estimation. These more complete results show the effect of *EZ1* in all the other equations of interest.[19] Turning first to the coefficients in the price equation, the direct price effect of the *EZ1* variable stays above 20% and gets even larger in the metropolitan sample as more variables and equations are added to the system. The strong, significant negative effects of *EZever* remain and are particularly strong in the metropolitan sample.

The coefficients of the other endogenous variables in the price equation bear some discussion. First, many of the coefficients are large in the extended models. This is largely a result of the small range of values of these variables. The coefficients represent

[19] Recall that there are also another set of equations predicting the lagged differences of each of the endogenous variables, save price. However, it is not instructive to look at these equations.

the effect of a one unit change in the independent variable, but a one unit change in most of these variables would represent a wild extrapolation from the limited range of these aggregate variables. More troubling is the robustness of these coefficients across models. Comparing coefficients from the full sample and the metropolitan sample, many variables' effects seem to change sign, significantly. Of the 17 control variables, nine of them change from increasing price to decreasing prices or *vice versa* when we purge rural neighborhoods from the sample. Only five seem to significantly affect prices in the same direction in both samples. We do not have strong intuitive explanations for most of these sign switches, and even some of the consistent estimates are hard to square with intuition (*e.g.* more whites hurts property values, but more renters helps them). Similar instability can be seen in observing the changes in coefficients as the plumbing variable is added. This addition had little effect in the OLS regressions in Tables 2a and 2b. However, the magnitudes of several variables (*avg. rooms* and *% Solo Units* in the full sample; *avg. rooms* and *bedrooms* and *percent college* in the MSA sample) change considerably when it is added in the system. While these changes are not as large as those between the full and MSA sample, they nonetheless highlight that the coefficients of the endogenous variables are not extremely robust.

The lower panels of Tables 3a and 3b report the coefficients of the *EZ1* variable in all the endogenous equations of the system. These results are somewhat more consistent across samples, although there is somewhat more variation within sample, across specifications. First, it should be noted that the program effect is often significant, although not always in the direction program managers may have hoped. Of the non-price effects that we can be fairly sure of, it appears that the Empowerment Zone policy intervention increased neighborhood poverty, vacancies and the average number of bedrooms, while it decreased population density (and thus population), the proportion renters, the proportion long-term residents, the proportion of stand-alone structures, the average number of rooms and the proportion of workers employed in industrial sectors. Focusing on the MSA sample (which we believe to be the relevant one), it also seems likely that *EZ1* increased the proportion of families that worked, the proportion of the population that is white and the proportion of families with children under 18 years of age. It also appears to have decreased commutes and the proportion of the population aged over 25 with a college degree. One might be willing to hazard a guess about the program encouraging new construction (*EZ1* has a consistently positive coefficient in the year built equations, but is never significant). We would characterize these effects as mixed, based on our understanding of the goals of the policy. Whether these non-price effects are beneficial on net depends on how the effected characteristics are valued in the housing market.

With these results in hand, and the empirical model laid out in Section III.A, it is possible to calculate the full effects of Empowerment Zone status in two ways. First, by comparing the coefficient on *EZ1* in the last column of Tables 2a and 2b (which represent the "full effect") to the direct effect as estimated in the first three columns of those tables (β_{EZ}), we get six estimates of the indirect effect of the program on property values. Second, by plugging the coefficients from the price equation and the other equations into an expanded equation (6), we can calculate the full effect and compare it to the direct effect for each of the 6 estimates of system of equations (4). The results of these exercises are reported in Table 4. We find these results striking in light of the goals and rationale of the policy. Empowerment Zones were not billed as property value enhancement programs. Instead, they were understood as attempts to affect neighborhoods for the better across a number of dimensions. One would thus expect that the direct effects of Empowerment Zone interventions would be minimal, but that the indirect effects would be large. To the extent that the results in Table 4 tell a story, it is the exact opposite one. Across all models, the direct effects are very large. On the other hand, the indirect effects are either quite small by comparison (estimated in OLS), or actually negative (estimated in the systems of equations). While some of the estimates may strain credibility, the consistency of this story is striking. There are no results pointing towards large *positive* indirect effects. Disregarding the extreme results, we could peg the indirect effect of the Empowerment Zone program at somewhere between 1% and -10%.

V. Conclusions

This paper has examined the effects of a very generous economic development policy: the federal Empowerment Zone program. This program offers the best chance to find positive effects of spatially targeted economic development policies because on top of the state and local tax incentives, federal tax incentives and direct federal investment is added.

This paper contributes to the literature because it is one of the first attempts to explicitly account for the complex, interacting processes which generate neighborhood measures like home values, demographic characteristics and housing stock characteristics. While the equations we estimate are admittedly reduced form, the system of equations approach allows for a much richer picture of neighborhood outcomes to emerge.

Although studies of state enterprise zones have struggled to find significant effects, we find a sizeable and significant positive effect on home values, and varying effects on other outcomes of interest. The significance and size of these effects are probably explained by the generosity of the federal program, along with better measurement of program status because of the close matching of EZ boundaries with census geographies. The indirect effects of EZ status on home prices through the other endogenous variables appear to be either extremely small or actually perverse. These results fit well into the existing literature on spatially targeted economic development programs. The recent literature on state programs (*e.g.* Bondonio and Greenbaum 2007), which rely completely on tax incentives, has shown them on net to be ineffective. Moreover, the developing literature on the effects of the federal program (Oakley and Tsao 2006, 2007a, b) finds generally unimpressive effects of the intervention on neighborhood indicators other than price. The results in this paper suggest that the federal grants are able to affect local quality of life with complex and not generally positive net effects on other neighborhood attributes.

We believe that these results raise questions about what the federal Empowerment Zone program has accomplished and how. The strong positive direct effect suggests the program is working, perhaps through improved amenities (lower crime, better infrastructure or better access to employment). Another possibility is that the positive increase in price represents a composition effect. Density decreases in these neighborhoods. A possible interpretation is that federal money is being spent to knock down low-value homes, increasing the median value in a neighborhood. Such an intervention would provide little beneficial neighborhood revitalization, and so we see the non-price effects of the program are extremely mixed. While this is possible, the sheer size of the *EZ1* effect makes it unlikely that this is the only explanation.

Another aspect worth examining is program heterogeneity. While this paper has concentrated on the average effect of the policy intervention, Oakley and Tsao (2006) show that there is some heterogeneity across Empowerment Zones in terms of the non-price effects of the program. This is to be expected since the actual policy intervention in each zone would differ according to the zone's administration, goals and strategies. Whether differences in policy outcomes are correlated with differences in the policy implementation in a sensible way is an interesting question. The identification strategy used here would not be appropriate for such an examination. The possibility of positive or negative spatial spillovers from Empowerment Zones is also worth consideration. Oakley and Tsao (2007a) show some suggestive evidence that the Chicago Empowerment Zone had positive spillovers in terms of poverty reduction. Chicago was also the Empowerment Zone with the strongest in-zone effects on poverty. Whether such spillover effects also pertain to these strong price effects remains to be seen. Examination of such effects could aid in our understanding of how the price effect arises. An effect on prices which arises solely through a composition effect, for instance, should not have strong spillover effects.

Further work might also be done on the indirect effects of EZ status on the other variables. Two variables of interest are the family labor market variable and the college education variable. The first is a direct target of the program, while both have been hypothesized to have positive externality effects in neighborhoods. At least in urban areas, the Empowerment Zone program seemed to have conflicting effects on these variables (increasing working families but decreasing college attainment). However, the price of housing *did* affect these variables. To the extent that these coefficients represent causal relationships, the positive effect of home prices in the labor market equation combined with the positive effect of EZ status in the home price equation opens at least one avenue through which EZ status could indirectly affect labor-market out comes in an area.

Spatially-targeted economic development programs are an important feature in the landscape of social policy in America. Because much of the cost of these programs is off the books, they are popular. The suite of policies at local state and federal levels create considerable variation in the intensiveness of these interventions. Considerable effort has been and will continue to be directed towards understanding the effects of these policies, and what works. To that end, this paper can be seen as adding to the literature in examining the effects of a very generous program. At the same time, these policy-induced variations in taxes and expenditures represent an opportunity to examine the forces affecting neighborhood change along a host of measurable dimensions. From that perspective, the differences in results across programs (state, federal) and across dimensions (price, non-price) offer insight into neighborhood dynamics and the workings of the various housing sub-markets in metropolitan areas.

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Table 1: Descriptive statistics for 1990's changes, main variables.

	Variable Change 1990-2000 of:	Sample				
		Full	MSA Only	Round 1	Round 2	Round 3
P	ln(median Home Value)	0.3603	0.3899	0.5306	0.3403	0.2451
		0.3236	0.3208	0.6013	0.4356	0.3761
N	Family Worker	0.0021	0.0046	0.0769	0.0361	0.0392
		0.0937	0.0956	0.1831	0.1451	0.1439
	% Poor	-0.0028	0.0006	-0.0302	-0.0123	-0.0163
		0.1049	0.1058	0.1942	0.1384	0.1688
	ln(median Income)	0.3339	0.3341	0.4037	0.3337	0.3702
		0.2470	0.2493	0.4851	0.3223	0.3909
	% White	-0.0680	-0.0818	-0.0286	-0.0585	-0.0699
		0.1133	0.1202	0.1174	0.1210	0.1364
	Commute (minutes)	2.1018	1.7819	2.6457	2.2904	1.6862
		5.1606	4.9910	10.9184	6.8534	6.5260
	Pop. Density	275.275	280.656	11.396	-610.596	-186.470
		3222.56	2373.90	10635.08	7410.43	2147.08
Nx	% Industrial	-0.0624	-0.0624	-0.0791	-0.0662	-0.0488
		0.0901	0.0893	0.1600	0.1044	0.1155
	% Renter	-0.0053	-0.0069	-0.0245	-0.0055	0.0102
		0.1010	0.1086	0.1569	0.1062	0.1384
	% In Same House	0.0168	0.0240	0.0134	-0.0019	0.0071
		0.1272	0.1334	0.1745	0.1363	0.1474
	% w/ Kids	-0.0020	-0.0013	0.0011	-0.0021	0.0001
		0.0648	0.0666	0.1097	0.0808	0.0938
	% College	0.0495	0.0514	0.0319	0.0340	0.0247
		0.0846	0.0891	0.0946	0.0953	0.0813
S	% Vacant	-0.0101	-0.0128	0.0029	-0.0055	-0.0174
		0.0686	0.0699	0.1382	0.0971	0.1148
	median Year Built	3.7079	3.6992	3.4684	7.5591	3.4443
		29.4367	30.4202	51.0575	82.2143	18.4519
	% Solo Unit	0.0030	0.0045	0.0216	0.0189	0.0154
		0.0967	0.1074	0.1426	0.1139	0.1216
	avg. # Rooms	0.0980	0.0641	-0.0064	0.0408	0.0674
		0.4815	0.5038	0.7387	0.5379	0.5870
	avg. # Bedrooms	0.0055	0.0097	-0.0312	0.0012	-0.0147
		0.2597	0.2722	0.4482	0.3055	0.3391
	% Complete Plumbing	-0.0008	0.0001	-0.0160	-0.0076	-0.0060
		-0.0309	0.0350	0.0753	0.0432	0.0372
M	Place: Households	17957.1	20186.8	49308.3	7763.3	29712.0
		42112.0	35171.0	82244.1	19169.1	22133.8
	Place: med. Value	34780.8	40357.3	33526.1	25502.9	20528.9
		34425.6	37917.3	26645.8	16029.5	8753.4
	Place: med. Rent	42.1022	47.7118	41.0000	30.3126	22.1155
		80.4618	80.8110	56.9921	40.6058	40.7479
	Place: med. Income	10379.7	10720.2	7411.5	7488.9	7675.3
		4849.06	5175.64	3101.73	2510.01	2500.91
	Place: % w/ Kids	-0.0097	-0.0069	0.0074	-0.0073	-0.0048
		0.0289	0.0285	0.0183	0.0294	0.0162
	Place: % Trad. Fam.	-0.0241	-0.0203	-0.0019	-0.0203	-0.0235
		0.0284	0.0278	0.0162	0.0276	0.0140

	District: expend./pupil	3.6552 2.0704	3.9901 2.2658	4.4081 1.0727	4.2345 2.0203	3.5027 1.4945
EZ	*EZ1*	0.0075 0.0862	0.0084 0.0912	1	0	0
	EZ2	0.0065 0.0804	0.0086 0.0925	0	1	0
	EZ3	0.0035 0.0594	0.0057 0.0755	0	0	1

Table 2a: OLS results Full Sample

Model	Basic		Extended		Extended'		Full effect	
EZ1	0.2406	***	0.2424	***	0.2455	***	0.2517	***
EZ2	0.0608	***	0.0585	***	0.0595	***	0.0523	**
EZever	-0.0293	*	-0.0254		-0.0248		-0.0054	
WorkingFam	0.0210	*	0.0017		0.0028			
Poverty	-0.0872	***	-0.1025	***	-0.0997	***		
Ln(Median Income)	0.1684	***	0.1528	***	0.1525	***		
% White	0.1761	***	0.1701	***	0.1677	***		
Avg Commute	-0.0004	**	-0.0006	***	-0.0006	***		
Pop. Density	-4.2E-07		-5. 9E-07		-6.5E-07			
% Industrial Emp.			0.0333	***	0.0339	***		
% Renters			0.0098		0.0110			
% in same house			-0.1738	***	-0.1728	***		
% w/ Children			-0.0451	***	-0.0452	***		
% College			0.1647	***	0.1648	***		
% Vacant	-0.0086		-0.0187		-0.0120			
Median Year Built	0.0014	***	0.0014	***	0.0014	***		
% Solo Units	-0.2131	***	-0.1674	***	-0.1632	***		
Avg. Rooms	0.1072	***	0.1061	***	0.1069	***		
Avg. Bedrooms	-0.0440	***	-0.0398	***	-0.0433	***		
% w/ Plumbing					0.2446	***		
Place: Households	-3.6E-07	***	-3.6E-07	***	-3.6E-07	***	-4.4E-07	***
Place: Med. Value	2.7E-06	***	2.6E-06	***	2.6E-06	***	2.8E-06	***
Place: Med. Rent	3.0E-05	***	1.5E-05		1.5E-05		-2.8E-05	**
Place: Med. Income	-4. 8E-06	***	-4.6E-06	***	-4.6E-06	***	-1.9E-06	***
Place: % Children	-1.1741	***	-1.1267	***	-1.1278	***	-1.7093	***
Place: % Trad. Fam.	0.8248	***	0.7118	***	0.7200	***	1.1824	***
District Exp./Pupil	-0.0178	***	-0.0180	***	-0.0179	***	-0.0201	***
County: Amenity	-0.0104	***	-0.0100	***	-0.0101	***	-0.0108	***
N	197310		195962		195962		197310	
R^2	0.232		0.239		0.239		0.160	

Table 2b: OLS results MSA Sample

Model	Basic		Extended		Extended'		Full Effect	
EZ1	0.2729	***	0.2734	***	0.2748	***	0.2835	***
EZ2	0.0665	***	0.0631	**	0.0647	***	0.0555	***
EZever	-0.0362	**	-0.0288	*	-0.0278	*	-0.0083	
WorkingFam	0.0371	***	0.0165		0.0175			
Poverty	-0.0263	**	-0.0434	***	-0.0414	***		
lnMed.Income	0.1659	***	0.1420	***	0.1421	***		
% White	0.1766	***	0.1553	***	0.1531	***		
Avg Commute	0.0000		-0.0002		-0.0002			
Pop. Density	2.5E-06	***	2.2E-06	***	2.1E-06	***		
% Industrial Emp			-0.0224	*	-0.0223	*		
% Renters			0.0131		0.0143			
% in same house			-0.1549	***	-0.1542	***		
% w/ Children			-0.0140		-0.0143			
% College			0.2862	***	0.2853	***		
% Vacant	-0.0086		-0.0218		-0.0160			
Median Year Built	0.0015	***	0.0015	***	0.0015	***		
% Solo Units	-0.1933	***	-0.1348	***	-0.1317	***		
Avg. Rooms	0.1075	***	0.1012	***	0.1016	***		
Avg. Bedrooms	-0.0297	***	-0.0261	***	-0.0283	***		
% w/ Plumbing					0.2844	***		
Place: Households	-3.6E-08		-5.7E-08	**	-5.8E-08	**	8.1E-08	***
Place: Med. Value	1.3E-06	***	1.2E-06	***	1.2E-06	***	1.3E-06	***
Place: Med. Rent	-2.9E-05	**	-4.9E-05	***	-4.9E-05	***	-6.5E-05	***
Place: Med. Income	-1.9E-06	***	-1.7E-06	***	-1.7E-06	***	1.0E-06	***
Place: % Children	-1.0345	***	-0.9544	***	-0.9650	***	-1.4999	***
Place: % Trad. Fam.	1.1414	***	0.9874	***	1.0033	***	1.5535	***
District Exp./Pupil	-0.0025	***	-0.0028	***	-0.0028	***	-0.0028	***
County: Amenity	-0.0013		-0.0028	***	-0.0028	***	-0.0002	
N	107730		107031		107031		107730	
R^2	0.147		0.160		0.161		0.049	

Table 3a: 3SLS full sample

Price	Simple		Extended		Extended'	
EZ1	**0.2160**	***	**0.3422**	***	**0.2711**	***
EZ2	**0.0788**	***	**-0.2445**	***	**-0.3747**	***
EZever	**-0.0158**		**-0.1621**	***	**-0.2023**	***
WorkingFam	**-3.5726**	***	**11.3906**	***	**13.0496**	***
Poverty	**2.1295**	***	**-16.5620**	***	**-20.9433**	***
lnMed.Income	**2.2403**	***	**-8.0209**	***	**-10.0910**	***
% White	**-1.5308**	***	**-7.2008**	***	**-9.1066**	***
Avg Commute	**0.1649**	***	**-0.3816**	***	**-0.5027**	***
Pop. Density	**-0.0001**	***	**-0.0005**	***	**-0.0007**	***
% Vacant	**-2.4090**	***	**-28.1120**	***	**-32.9693**	***
% Industrial Emp			**-6.6994**	***	**-9.3463**	***
% Renters			**10.0990**	***	**11.9005**	***
% in same house			**-8.4519**	***	**-12.0999**	***
% w/ Children			**-11.0225**	***	**-15.8295**	***
% College			**6.3956**	***	**8.5921**	***
Median Year Built	**-0.0017**	***	**0.0357**	***	**0.0480**	***
% Solo Units	**-1.3031**	***	**0.7718**	***	**1.7678**	***
Avg. Rooms	**-0.1235**	***	**0.0951**	*	**0.1647**	**
Avg. Bedrooms	**-0.3929**	***	**-5.0659**	***	**-5.2273**	***
% w/ Plumbing					**-8.4891**	***
Off-Price						
WorkingFam	**-0.0312**	***	**0.0022**		**-0.0001**	
Poverty	**0.0316**	***	**0.0220**	***	**0.0115**	**
lnMed.Income	**0.0278**	***	**0.0209**	*	**0.0165**	
% White	**-0.0353**	***	**0.0001**		**-0.0078**	
Avg Commute	**1.9079**	***	**-0.6849**	**	**-0.3125**	
Pop. Density	**-68.9543**		**-55.9165**		**-16.4487**	
% Vacant	**-0.0066**		**0.0160**	***	**0.0170**	***
% Industrial Emp			**-0.0231**	***	**-0.0313**	***
% Renters			**-0.0518**	***	**-0.0480**	***
% in same house			**-0.0417**	***	**-0.0460**	***
% w/ Children			**0.0017**		**-0.0016**	
% College			**-0.0024**		**0.0135**	**
Median Year Built	**1.2598**		**-3.7970**	***	**-3.2551**	***
% Solo Units	**-0.0096**		**-0.0019**		**-0.0050**	
Avg. Rooms	**-0.1804**	***	**-0.0294**		**-0.0091**	
Avg. Bedrooms	**0.0725**	***	**0.0656**	***	**0.0707**	***
% w/ Plumbing					**-0.0080**	***

Table 3b: 3SLS MSA sample

Price	Simple		Extended		Extended'	
EZ1	**0.2485**	***	**0.4651**	***	**0.4779**	***
EZ2	**-0.0500**		**0.3063**	***	**0.3787**	***
EZever	**-0.1188**	***	**-0.2511**	***	**-0.2367**	***
WorkingFam	**-8.7933**	***	**-6.4868**	***	**-7.0209**	***
Poverty	**7.1189**	***	**0.9146**	***	**2.3729**	***
lnMed.Income	**5.6769**	***	**3.9366**	***	**4.7370**	***
%White	**-1.4135**	***	**-0.7718**	***	**-0.3220**	***
Avg Commute	**0.1327**	***	**0.0704**	***	**0.1155**	***
Pop. Density	**-0.0004**	***	**-0.0001**	***	**-0.0002**	***
%Vacant	**6.3792**	***	**5.0647**	***	**5.8866**	***
% Industrial Emp			**-0.5103**	***	**0.1122**	
% Renters			**3.8106**	***	**3.7122**	***
% in same house			**3.9499**	***	**5.6772**	***
% w/ Children			**7.6550**	***	**10.9100**	***
% College			**-0.3087**	*	**-1.4349**	***
Median Year Built	**-0.0117**	***	**-0.0146**	***	**-0.0150**	***
% Solo Units	**7.9449**	***	**6.7368**	***	**8.2648**	***
Avg. Rooms	**-0.0302**		**0.2843**	***	**0.0647**	***
Avg. Bedrooms	**2.7865**	***	**-0.4026**	**	**-0.1929**	
% w/ Plumbing					**15.5491**	
Off-Price						
WorkingFam	**0.0131**	*	**0.0429**	***	**0.0348**	***
Poverty	**0.0146**	***	**0.0276**	***	**0.0268**	***
lnMed.Income	**0.0105**		**-0.0033**		**0.0051**	
% White	**-0.0163**		**0.0409**	***	**0.0398**	***
Avg Commute	**0.2556**		**-0.6868**	**	**-0.5621**	*
Pop. Density	**-454.5628**	***	**-626.6654**	***	**-636.8403**	***
%Vacant	**0.0211**	***	**0.0196**	***	**0.0216**	***
% Industrial Emp			**-0.0083**		**-0.0103**	
% Renters			**-0.0603**	***	**-0.0627**	***
% in same house			**-0.0249**	***	**-0.0173**	*
% w/ Children			**0.0143**	***	**0.0180**	***
% College			**-0.0392**	***	**-0.0369**	***
Median Year Built	**1.2283**		**1.0933**		**0.5060**	
% Solo Units	**0.0065**		**-0.0259**	***	**-0.0206**	***
Avg. Rooms	**-0.0338**		**-0.0842**	***	**-0.0862**	***
Avg. Bedrooms	**0.0473**	***	**0.0782**	***	**0.0777**	***
% w/ Plumbing					**-0.0008**	

Table 4: Partial, Full and Indirect Effects, various models

Sample	Estimation	Model	Partial	Full	Indirect
Full	OLS	Simple	0.241	0.252	0.011
		Extended	0.242	0.252	0.009
		Extended'	0.246	0.252	0.006
	3SLS	Simple	0.216	0.173	-0.043
		Extended	0.342	-0.136	-0.478
		Extended'	0.271	-0.145	-0.417
MSA	OLS	Simple	0.273	0.283	0.011
		Extended	0.273	0.283	0.010
		Extended'	0.275	0.283	0.009
	3SLS	Simple	0.248	0.236	-0.013
		Extended	0.465	0.387	-0.078
		Extended'	0.478	0.385	-0.093