Crowd Out Effects of Place-Based Subsidized Rental Housing
New Evidence from the LIHTC Program

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Abstract

Since its inception in 1987, the Low-Income Housing Tax Credit (LIHTC) program has ballooned into the largest ever source of subsidized construction of low-income housing in the United States, accounting for one-third of all recent multi-family rental construction. This paper examines the crowd out effects of this increasingly important source of low-moderate income housing. To do so, we analyze the impact of LIHTC construction at three different levels of geography, MSA, county, and 10-mile radius circles. This allows us to employ increasingly extensive geographic fixed effects that help to difference away unobserved factors. Political variables are also used as instruments to further facilitate identification.

In all of our models, IV estimates yield substantially greater crowd out than OLS, confirming the endogenous attraction of LIHTC development to areas ripe for new construction. Our most robust IV estimates indicate that nearly 100 percent of LIHTC development is offset by a reduction in the number of newly built unsubsidized rental units, although the confidence band around this point estimate allows for less dramatic assessments. Additional estimates suggest that LIHTC development has a much more moderate impact on construction of owner-occupied housing, but these estimates are imprecise. Overall, while LIHTC development may well affect the *location* of low-moderate income rental housing opportunities, our estimates strongly suggest that the impact of the program on the *number* of newly developed rental housing units appears to be small.

Key Words: Crowd Out, Subsidized Housing, LIHTC
JEL Codes: H7, H42, R21, R31

"I rise today to introduce the Affordable Housing Tax Credit Enhancement Act of 2005. … the bill would double the current LIHTC [annual allocations], which would yield twice the number of affordable units annually. … Today, the LIHTC program is widely regarded as the nation's most successful housing production program resulting in the construction and rehabilitation of more than 1.3 million housing units for lower income households. …"

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1. Introduction

The manner in which housing assistance to the poor is provided remains subject to considerable and even heated debate: should government invest in people through demand side voucher type programs (e.g. Section 8 vouchers) or in places through supply side construction subsidies such as public and Low Income Housing Tax Credit (LIHTC) housing? Against that backdrop, this paper examines the rapidly growing LIHTC program and highlights the extent to which construction of LIHTC housing displaces or crowds out unsubsidized private development of rental housing. Some further context will help to put the LIHTC program in perspective.

Between the late 1930s and the mid-1980s, the federal government built over one million low-income units through the public housing program. During the 1980s, construction of new public housing units ended and was replaced in 1986 by the Low-Income Housing Tax Credit (LIHTC) program. Under different variants of the LIHTC program between 30 to 91 percent of project construction costs are subsidized. Those subsidies are financed by the Federal government in the form of tax credits administered through the Internal Revenue Service (IRS). In each year, Congress sets the total number of credits to be granted nationwide, and allocates those credits across states in proportion to their population. State housing authorities then reallocate credits to developers who agree to adhere to rent ceilings and tenant income limits for at least 15 years.

In Table 1, notice that from 1987 to 2006, roughly 1.6 million LIHTC units were built accounting for roughly one-third of all recent multi-family rental housing constructed. This makes the LIHTC program the largest supply-side housing subsidy program in the nation's history. Figure 1 further illustrates this point. The figure displays the level of public housing and LIHTC development by decade over the last 60 years.[1] The recent boom in LIHTC development is evident. Also apparent, in Table 2 note that while the cost of the LIHTC program may appear modest in relation to the housing voucher program, the absolute cost of the LIHTC program is large. In 2006, housing voucher programs cost nearly $21 billion. In comparison, lost federal tax revenue associated with the LIHTC program totaled $4.9 billion. That cost, however, is expected to increase sharply in the next few years in response to the 40 percent increase in credits allocated beginning in 2001.[2] Moreover, as recently as May, 2005, there have been calls in congress to substantially increase the size of the LIHTC program.

It is also worth highlighting that LIHTC development is not exclusively a low-income neighborhood event. As shown in Figure 2a, as of 2000, 16 percent of LIHTC units were located in census tracts in the upper third of their MSA income distribution. Another 28 percent were situated in middle-income communities, with the remaining 56 percent in lower-third income neighborhoods. The extension of LIHTC housing into middle- and higher-income neighborhoods differs markedly from public housing. In Figure 2b, observe that 77 percent of public housing units are in low-income locations with most of the rest in middle-income communities. Although our primary focus in this paper is to consider the extent to which LIHTC development displaces unsubsidized rental housing construction, it is important to recognize that even in the event of full crowd out, the patterns in Figure 2 are suggestive that LIHTC development may affect the location of rental housing opportunities.

To assess the crowd out effects of LIHTC development we combine census tract data from 1990 with information on LIHTC development between 1990 and 2000. Our basic strategy is to run cross-section regressions of private sector housing construction in the 1990s on LIHTC development over the same period, controlling for other drivers of housing starts as articulated in prior literature (e.g. Mayer

[1]The public housing data used to create Figure 1 were obtained from analysts at HUD and with assistance from MacArthur and Abt Associates. These data differ from the publicly available series found at www.huduser.org in two ways. First, our data contain the year each "project" was placed-in-service from 1937 to 2000. This allows us to represent new construction of public housing in each decade. Additionally, our data also includes information on public housing demolitions in the 1990's. The LIHTC data were obtained from HUD at http://lihtc.huduser.org.

[2]Those increases will occur even if current proposals seeking to expand the LIHTC program are not born out. Additional details on the cost of various forms of low-income housing support are provided in a report by the U.S. Congress, Joint Committee on Taxation (2005).

and Somerville, 2000). An alternative approach, emphasized by Murray (1999) in his analysis of aggregate time series of U.S. housing stocks, would be to assess the impact of subsidized housing construction on the total stock of housing. We argue later in the paper that our focus on new development yields closely related results that are more reliable given the largely cross-sectional nature of our data. In particular, we condition all of our models on lagged levels of housing stocks. This helps to control for unobserved local factors that might otherwise bias our estimates. This also causes our dependent variable to reflect the change in housing stocks between 1990 and 2000 regardless of whether the dependent variable is specified as the change in housing stock or as the year-2000 level of housing stock.[3] It should also be noted that change in stock over time is equal to new construction net of demolitions. Our data provide direct observations on new construction but demolitions are not reported and can only be inferred. Accordingly, we believe that our data on new construction is more reliable and for that reason we focus our analysis on the impact of LIHTC development on unsubsidized housing starts. Nevertheless, two empirical challenges remain that play a prominent role in our effort to identify crowd out effects. The first is to choose the level of geography over which LIHTC crowd out effects are analyzed. The second is to control for the possibility that LIHTC development may be endogenous. We consider each of these issues briefly in turn below, with further details to follow later in the paper.

For several reasons, estimates of LIHTC crowd out effects are likely to be sensitive to the level of geography at which LIHTC development is analyzed (e.g. city block, county, MSA, state, etc.). First, from a conceptual standpoint, neighborhoods perceived as close substitutes by potential residents of low/moderate income housing belong to a common housing market. For such linked communities, LIHTC development in one neighborhood will tend to reduce equilibrium house prices throughout the common market. Because crowd out occurs when subsidized activity depresses market prices, this suggests that a full accounting of the crowd out effects associated with LIHTC development requires that

[3]Note, for example, if we subtract y_{t-1} from both sides of $y_t = b_o + b_1 y_{t-1} + b_2 x_t$ only the coefficient on the lagged dependent variable is affected while the coefficient on x remains unchanged. In addition, whereas Murray (1999) seeks to analyze the impact of subsidized housing construction on equilibrium stocks of housing, Murray (1983) alternatively focuses on the impact of subsidized housing development on housing starts.

the geographic unit of analysis be large enough to allow for meaningful interactions across neighborhoods. On the other hand, increasing the geographic scale of the unit of analysis reduces the number of locations available for study (e.g., there are fewer states than counties). This causes variation in the data to diminish. Measuring LIHTC development at a broader geographic scale also limits opportunities to use geographic fixed effects to strip away potentially confounding effects of unobserved factors (e.g., MSA fixed effects can be used in a state-level analysis, but county fixed effects can be used in an MSA-level analysis). In the empirical work to follow, we attempt to balance these offsetting factors by conducting our analysis three times measuring LIHTC development at different geographic levels: MSA plus state-specific rural areas, counties, and 10-mile radius circles drawn around the geographic centroids of the year-2000 census tracts. Each of these levels of geography strike us as large enough to allow for substantive interactions across neighborhoods. However, as will become apparent, we favor the 10-mile circle approach as that level of geography provides the greatest degree of variation in the data, and also allows for inclusion of the most precise geographic fixed effects

The need to control for unobserved drivers of unsubsidized housing development is closely related to the question of whether our measures of LIHTC development are endogenous, our second dominant empirical concern. On the one hand, developers recognize that the potential for capital gains varies with location and this affects the anticipated returns on a LIHTC project. In addition, sharp financial penalties are imposed on LIHTC investors if a project fails to meet federal and state guidelines requiring a minimum share of project units to be leased out to low-income families (Eriksen, 2009).[4] The risk of incurring such penalties is sensitive to fluctuations in demand for low-moderate income rental housing and is likely to differ across locations. For these reasons and others, it is plausible that developers will seek to locate LIHTC projects in areas perceived to be ripe for future growth and new housing construction. That positive correlation would cause ordinary least squares estimates of the causal impact of LIHTC development on private unsubsidized construction to be biased towards a more positive

[4]LIHTC developers are obliged to fill 40 percent or more of their units with low-income families and are required to charge rents below a specified ceiling. Non-compliance with these terms results in forfeiture of future tax credits and also repayment of one-third of previously received allocations plus interest.

value, understating the crowd out effects of LIHTC projects. On the other hand, it is also possible that state officials overseeing within-state allocations of LIHTC credits may pressure developers to site projects in hard pressed communities where little development might otherwise be anticipated. This could cause the OLS bias to go in the opposite direction. Together, these considerations suggest that failure to control for the possible endogenous location of LIHTC projects could bias estimates of LIHTC crowd out effects, although the direction of bias is uncertain, *a priori*.

To control for the endogenous placement of LIHTC units, we instrument for LIHTC development in a two-stage least squares procedure using instruments motivated by the political process that governs the allocation of LIHTC credits. Federal law instructs the IRS to allocate LIHTC credits across states in proportion to each state's share of the U.S. population, with the total number of credits nationwide set by congress. Treating state population share as given, we assume that the total allocation of credits to each state in the 1990s is also exogenous. Credits are then reallocated within state using whatever procedures each state deems appropriate in a given year (subject to federal guidelines for the overall program). Those procedures differ across states and over time, and we do not have direct data on individual state/year allocation procedures. Instead, we assume that states allocate their credits at least in part though a political process that mimics the federal government. Specifically, we assume that the allocation of credits within state between 1990 and 2000 is based partly on a given local area's (e.g., county) share of state population in 1990. Multiplying 1990 local population shares by the state allocation of LIHTC credits yields our first instrument for the number of LIHTC units in a given location.

As a second instrument, we allow for the possibility that cronyism may also influence within state allocations of the valuable LIHTC tax credits. Accordingly, for each county we code a dummy variable equal to 1 based on whether that county voted for the sitting Governor in 1989. Multiplying this measure by the first instrument described above allows for the possibility that communities that tend to vote for the winning gubernatorial candidate receive a greater than average share of state LIHTC credits relative to their share of state population.

Additional details on the manner in which these two instruments are constructed are provided later in the paper. For now, it is sufficient to indicate that diagnostic tests indicate that the instruments are strongly correlated with LIHTC development (e.g. Stock and Yogo, 2005; Murray, 2006). In addition, tests of the overidentifying restrictions support the validity of the two instruments, although we view such tests with caution given their weak power and known tendency to yield false positives and false negatives.

Our primary results indicate that at both the county level and for 10-mile radius circles, crowd out arising from LIHTC development is nearly 100 percent, although the standard errors on these estimates are large enough to allow for more moderate assessments. We also find that crowd out effects of LIHTC development occur primarily in the rental sector of the market, and not the owner-occupied segment. On balance, our estimates suggest that while LIHTC development may affect the *location* of rental housing opportunities – encouraging some lower income families to gravitate to LIHTC enclaves within their broader communities or cities – the overall impact of the program on the total stock of rental housing is estimated to be quite small. In certain respects, the high degree of crowd out should not be surprising. Numerous studies in the literature indicate that housing demand is inelastic while new housing supply is quite elastic (e.g. Hanushek and Quigley, 1980).[5] A simple model outlined later in the paper demonstrates that under such market conditions, high rates of crowd out will occur. Advocates of LIHTC development, therefore, need to look beyond mere expansion of the total stock of rental housing to justify the program.

To clarify our results, the paper proceeds as follows. The following section provides further detail on the institutional context of the LIHTC program, and also prior estimates of crowd out associated with place-based subsidized rental housing. Section 3 describes a simple conceptual model that guides our analysis. Section 4 describes our data and develops the empirical model used for the regression analysis. Section 5 presents the results, and Section 6 concludes.

[5]We review prior estimates of the elasticities of demand and supply for housing later in the paper.

2. Institutional Context and Prior Estimates of Crowd Out

This section provides a brief overview of some additional institutional details of the LIHTC housing program. Also included in this section is a review of previous research on crowd out associated with place-based subsidized housing.[6]

2.1 Institutional Context

Between 1937 and the late 1970's, low-income housing support was provided almost exclusively through public ownership and operation of housing built exclusively for low-income families. We refer here to such housing as "traditional public housing."[7] Typically, occupancy in these projects has been limited to families near or below poverty levels with tenants paying 30 percent of their gross income towards rent (Olsen, 2003).

By the 1980s, two concerns about the public housing program had gained attention. The first was simply that the government builds, owns, and operates these projects: there are basic questions as to whether at least some portion of such activity is best left to the private market. In addition, because public housing projects were spatially concentrated and restricted occupancy to very low-income residents, they created dense clusters of poverty. Facilitated in part by several horrifying newspapers accounts, the public came to associate public housing with neighborhood decline, violent crime, and drug-use (Currie and Yelowitz, 2000). Concerns about adverse peer and neighborhood effects on children growing up in the projects also gained attention, both in academic and policy circles (see, for example, Currie and Yelowitz (2000) or Jencks and Mayer (1990)). Primarily for these reasons, the government stopped virtually all construction of traditional public housing in the early 1980's and began to demolish

[6]Exhaustive reviews of the LIHTC and other related housing programs are in Quigley (2000) and Olsen (2003).

[7]Olsen (2003) points out that there are at least 29 different public housing programs.

the worst performing projects during the 1990s.[8] Figure 1 illustrates the construction of traditional public housing units since 1935 along with demolitions in the 1990s.

The LIHTC program was created as a part of the Tax Reform Act of 1986, both as an alternative to public housing and to offset the reform's removal of other tax benefits for owners of rental housing (U.S. Joint Committee on Taxation, 1987). As noted earlier, and different from other federal low-income housing programs, the LIHTC program is administered by the Internal Revenue Service under Section 42 of the U.S. Tax code.

The premise of the LIHTC program is to create a public-private partnership where the Federal Government subsidizes between 30 to 91 percent of non-land construction costs for private developers. In all cases, the subsidy is provided to developers through a 10-year stream of annual nonrefundable federal tax credits – dollar-for-dollar reductions in federal tax liability (Eriksen, 2007). In exchange for the subsidy, developers agree to set rents below specified ceilings and to lease a minimum specified share of the project's units to low-income families for at least 30 years. In practice, most projects contain 100 percent low-income occupancy owing to how the subsidy is structured and administered (see Eriksen (2007) for additional details).

It is also important to note that rent ceilings imposed on LIHTC units occupied by low-income families are effectively set at 18 percent of MSA median household income as determined by HUD.[9] That amount is sufficiently high and the tax credits are sufficiently generous (30 to 91 percent), that LIHTC units are generally of high quality relative to most other low-income housing (Cummings and DiPasquale, 1999). It is also important to note that LIHTC occupants typically have a higher income than residents of traditional public housing (Wallace, 1995).[10]

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[8]In some instances, such as under the HOPE VI program, the structures were remodeled, but in most cases tenants were usually issued housing vouchers and told to seek housing privately (Jacob, 2004).

[9]The 18 percent limit is derived from two conditions imposed by the program. First, low-income families must earn incomes below 60 percent of MSA median income, and second, rent must not exceed 30 percent of that value.

[10]For example, Wallace (1995) estimates that only 28 percent of LIHTC residents earn below 50 percent of an area's median income compared to 81 percent of those who reside in traditional public housing.

These features of the public and LIHTC programs have implications for interpretation of our results to follow. Traditional public housing has suffered a particularly negative reputation over the years. This likely reduces demand for sites in close proximity to public housing projects, lowering property values. Such effects would tend to further discourage new development beyond impacts associated with crowd out. LIHTC developments, on the other hand, are high quality in comparison to other low-income housing, as noted above. In addition, many LIHTC developments "blend in" architecturally with their local communities (Buron et al., 2000). These features likely greatly reduce or even eliminate stigma effects associated with the presence of nearby LIHTC projects. As a result, our estimates of LIHTC crowd out will more closely reflect pure displacement effects as opposed to reductions in private housing construction arising from inward shifts in demand.

2.2 Previous estimates of crowd out from place-based subsidized housing

The possibility of crowd out arises any time government provides goods and services that are also offered through the private sector. This has been examined in a variety of markets, including health insurance, radio, and charitable giving (Culter and Gruber, 1996; Berry and Waldfogel, 1999; Andreoni and Payne, 2003). Several recent studies have also begun to examine crowd out arising from place-based subsidized housing, although most do not consider the LIHTC program. The first of these by Murray (1983, 1999) utilizes national-level aggregate time series data from 1935 to the mid-1980s. These data pre-date the LIHTC program and are used to assess the impact of public and other earlier forms of subsidized rental housing construction on unsubsidized housing construction (Murray, 1983) and the equilibrium stock of housing (Murray, 1999). The general strategy of each paper is to examine whether construction of subsidized rental housing increases the total starts or stocks of housing on less than a one-for-one basis.[11] Evidence of such effects would be indicative of crowd out. Murray (1999) finds that subsidized rental housing programs that target very low income families generate only a small amount of

[11]More precisely, Murray (1999) estimates the cointegrating relationship between the equilibrium stock of subsidized and unsubsidized housing stocks over the 1935 to 1987 period.

crowd out. This is consistent with stylized facts that private market developers build little unsubsidized housing for very low-income families: for crowd out to occur, private markets must first be willing to provide the product. In contrast, Murray (1999) also estimates that between one-third to 100 percent of subsidized "moderate-income" place-based housing is offset by crowd out of unsubsidized construction. This is consistent with the idea that in the absence of construction subsidies, the private market would build at least some moderate income housing.

Given that LIHTC housing is approximately a moderate income housing program, this is important. But it should also be emphasized that Murray's work differs from ours in two very important respects. First, our empirical design is based on cross-section data and relies on geographic and especially instrumental variable methods for identification. Murray's work is based on aggregate time series data. Moreover, as noted above, Murray's data reflect the impact of housing programs that pre-date the LIHTC program. Because those earlier programs have very different institutional arrangements than LIHTC development, there is no guarantee that Murray's results would carry over to the LIHTC program.

More recently, and closer in structure to this paper, Sinai and Waldfogel (2005) examine crowd out effects of place-based subsidized rental housing programs on per capita occupied housing units in 1990. They report OLS crowd out estimates of approximately 70 percent from place-based subsidized rental housing when using data aggregated to the census place level.[12] When the data are instead aggregated to the MSA level, their point estimate of crowd out falls to roughly 30 percent. For both levels of geography, LIHTC housing is grouped with other forms of place-based subsidized rental housing. This is important because the data used by Sinai and Waldfogel (obtained from HUD's 1996 Picture of Subsidized Housing file) includes roughly 2.8 million place-based subsidized units. Of these, only 332,085 are LIHTC units, most of which were not present in 1990, the period associated with their

[12]Sinai and Waldfogel (2005) attempt to instrument for subsidized housing construction using the number of housing units per capita built before 1940 and also occupied public housing units per capita in 1980. However, results from the IV models yield quite different estimates of crowd out depending on which of the instruments are included. Partly for that reason, Sinai and Waldfogel tend to emphasize their non-IV estimates of crowd out.

dependent variable.[13] As with Murray (1983, 1999), therefore, crowd out effects identified by Sinai and Waldfogel (2005) reflect primarily crowd the impact of place-based programs that pre-date the LIHTC program.

Malpezzi and Vandell (2002) do consider directly the crowd out effects of LIHTC development. They analyze the impact of 1987-2001 state-level LIHTC allocations on the per capita stock of housing as measured in 2000 (based on the year-2000 census). Their point estimate implies full crowd out, although their sample is limited to just 51 state-level observations (including Washington, D.C.) with controls for 14 indicators of demand and supply. As a result, and as recognized by the authors, the standard error on their estimate of LIHTC effects is several times larger than their corresponding estimated crowd out effect. They are, therefore, unable to shed much light on the degree of crowd out associated with the LIHTC program.

Most recently, Baum-Snow and Marion (2009) examine the impact on LIHTC development given a census tract's qualified census tract (QCT) status as defined by HUD. QCT tracts are eligible for additional subsidies through the LIHTC program making these tracts especially attractive for LIHTC development, all else equal.[14] Baum-Snow and Marion (2009) provide evidence that developers respond to a tract's QCT status by shifting development from adjacent tracts to the more heavily subsidized location. Although Baum-Snow and Marion discuss implications of their work for LIHTC crowd out of unsubsidized development, their primary focus is on the border region associated with QCTs and the response of developers to the more generous subsidy in those areas. Their finding underscores the tendency of developers to substitute capital across nearby neighborhoods in response to competing development opportunities. This further reinforces our argument in the Introduction that crowd out effects are most clearly identified at a relatively broad level of geography.

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[13]Place-based subsidized units in the 1996 Picture files include Public (1,326,224 units) Section 8 Moderate Rehabilitation (105,845 units), Section 8 New Construction (897,160 units), Section 236 (447,382 units), and other placed-based subsidized units (292,237 units). Additionally, the 1996 picture files only report roughly half of the LIHTC units allocated between 1987 and 1996 (Malpezzi and Vandell, 2002).

[14]LIHTC development in a QCT is eligible for a 30 percent increase in subsidy as compared to those not located in such a tract.

3. Conceptual Model

Consider now Figure 3a which portrays the market for the stock of rental housing at a given point in time. Abstracting from some of the details of the LIHTC program, the program subsidizes construction up to an exogenously given state-level allocation. This implies an outward shift in aggregate supply of rental housing, causing equilibrium rents to fall and housing stocks to increase.[15]

In Figure 3a, notice that the aggregate stock of rental housing increases by less than the level of LIHTC construction. In particular, whereas H_3-H_1 equals the number of LIHTC units built, aggregate supply expands by only H_2-H_1. The difference, H_3-H_2, represents unsubsidized private construction that is "crowded out" by the LIHTC program. Moreover, Figure 3b illustrates that as the demand function becomes more inelastic crowd out becomes more pronounced. Indeed, the figures make clear that only when housing demand is perfectly elastic or supply of newly constructed housing is perfectly inelastic would H_3-H_2 equal zero and crowd out not occur.

To put this in perspective, Hanushek and Quigley (1980) use data from the housing allowance experiments of the 1970s to estimate the elasticity of demand for rental housing. For Pittsburgh and Phoenix their estimates are -0.36 and -0.41, respectively. For owner-occupied units, Rosen (1979) estimates a price elasticity of -0.99 for a random sample of owner-occupiers, while Rosenthal, Duca, and Gabriel (1991) obtain an estimate of -0.5 for FHA homebuyers, a group that is much closer in income to that of the typical renter. These estimates confirm that housing demand is far from perfectly elastic.

[15]Figures 3a and 3b suggest that the LIHTC program causes the entire supply function to shift out. This is a simplification that captures the dominant impact of the LIHTC program that is relevant to this paper while streamlining the discussion. An alternative and more accurate portrayal is that the LIHTC program flattens the slope of the lower most section of the supply curve up to the maximum number of LIHTC units allocated to a given location. Beyond that level of construction, the supply function steepens because additional investment is unsubsidized. Such a specification would imply that developers choose to invest first in LIHTC developments before unsubsidized construction. Moreover, to the extent that LIHTC development draws low-cost factor inputs away from the unsubsidized sector, input costs in the unsubsidized sector will be higher than in the absence of the LIHTC program. This would result in a further inward rotation of the unsubsidized segment of the supply function. See Olsen (2007) for a further discussion of this issue.

On the supply side, Mayer and Somerville (2000) estimate that the supply elasticity of newly built housing of all types is roughly 6. This is close to DiPasquale and Wheaton (1992) who estimate a supply elasticity for newly built multi-family rental housing of 6.8. Other estimates of the elasticity of supply of newly built housing are smaller, but generally well above 1 (e.g. DiPasquale, 1999; Rosenthal, 1999). While the range of supply elasticities is generally greater than on the demand side, both conceptual arguments and evidence in the literature confirm that when considering expansion of the housing stock, supply is far from perfectly inelastic.[16]

Summarizing, only when housing demand is perfectly elastic or the supply of housing is perfectly inelastic would crowd out not occur. However, estimates in the literature strongly suggest that neither of these conditions hold. This suggests that on a qualitative basis, advocates and opponents of LIHTC development alike should anticipate crowd out from the program. The question then is, how much?

4. Data and Empirical Model

4.1 Data

Before developing our regression model it is useful to first highlight certain key features of the data. We use decennial census data aggregated to the census tract level as the root data source for our control variables. These data were obtained from the Geolytics, Inc. Neighborhood Change Database file for 1990 and 2000.[17] Geolytics re-codes data from each of these years to year-2000 census tract boundaries. These data were combined with information on LIHTC projects placed into service up through 2000. The LIHTC data were obtained from HUD over the web.[18] Information on the LIHTC

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[16]As a further perspective, suppose that developable land is plentiful, either as raw land or as previously developed land suitable for redevelopment. Suppose also that long run costs for building materials and labor are constant in real terms. Then home building is approximately constant returns to scale and new housing supply should be quite elastic (e.g. Rosenthal and Helsley, 1994). See also Glaeser and Gyourko (2005) for a discussion of the relative elasticity of supply with respect to expansion versus contraction of the stock of housing.

[17]See www.geolytics.com.

[18]See http://lihtc.huduser.org .

database includes the year placed in service and the year-2000 census tract. Our data include 17,774 LIHTC projects containing 877,972 individual units.

Given these data, we conduct three sets of analyses. In the first, all of the data are aggregated up to the MSA level, a geographic unit sufficiently large that most interactions across neighborhoods in the manner described in the Introduction are likely taken into account. For this portion of the analysis, we drop non-MSA areas from the sample. In our second approach, we instead aggregate the data to the county level. While not as large geographically as MSAs, there are many more counties and this increases variation in the data, aiding in identification of LIHTC crowd out effects. In addition, whereas we include state fixed effects in the MSA-level analysis, for the county-level regressions we are able to use MSA and state-specific non-MSA fixed effects. To the extent that counties are still large enough to allow for most interactions across neighborhoods, this model brings more data and power to bear to identify crowd effects associated with LIHTC development.

For our final approach, we reorganize the data into uniform 10-mile radius circular units using Geographic Information Systems (GIS) software with each circle drawn around the geographic centroid of the individual year-2000 census tracts, i (i = 1, …, n).[19] The circle-based measures are produced for all of the dependent and independent variables, as well as the population variables used in constructing our instruments. This ensures that the geography used to measure variables on both sides of the regression equation is the same. It is worth noting that the typical county covers less area than a 10-mile radius circle. Partly with this in mind, we feel that the 10-mile circle measures do at least as good a job as counties in allowing for cross-neighborhood interactions. Moreover, because the circles are drawn around the geographic centroids of the underlying census tracts, many different circles are centered within a given county. This allows us to control for county fixed effects, and further strengthens identification. The overlapping nature of the circles and implied repetition of some of the information in the dependent variables is dealt with by clustering standard errors in the circle regressions at the county level in a

[19]MapInfo and MapBasic were used to manipulate the geographic features of the data. When drawing circles around the census tract centroids, proportional sum measures were used to calculate the various count variables.

manner to be clarified later. For these reasons, we feel that the 10-mile circle regressions provide our most robust estimates, a point we will return to later. Finally, note that summary statistics of state-level, county-level, and 10-mile circle data are provided in Table 3.

4.2 Housing starts model

The conceptual model outlined above portrayed the impact of LIHTC development on the overall stock of housing. In that context, full crowd out would imply that LIHTC construction has no impact on the total number of housing units in the local economy. Sinai and Waldfogel (2005) draw on that intuition and specify their model so as to highlight the impact of subsidized rental housing programs on the total stock of occupied housing present at a given point in time. As noted in the Introduction, in our empirical work to follow, we take a different but closely related approach. Specifically, we condition on the lagged level of housing stock and assess the impact of LIHTC development on changes in the stock of housing between 1990 and 2000. In this context, full crowd out would imply that every LIHTC unit built would reduce the change in private unsubsidized housing stocks by one unit. While these two perspectives on crowd out are closely related, a focus on flows conditional on lagged stocks offers advantages given the nature of our data as outlined in the Introduction.

Specification of our model is guided by previous work on housing starts, and especially that of Mayer and Somerville (2000).[20] Mayer and Somerville emphasize that new housing construction is a flow, and for this reason, is best expressed as a function of changes in housing prices and costs rather than as a function of the levels of those factors. They also recognize that deterioration of the existing housing stock provides a further motivation for new housing development.

Mayer and Somerville estimate their model using quarterly aggregate time series data for the U.S.. Housing starts between periods t and t-1 are expressed as a function of change in quality adjusted house prices, change in the real interest rate, change in the cost of building materials, the lagged median

[20]Other important housing supply models include the stock adjustment model of Topel and Rosen (1988) and related work by DiPasquale and Wheaton (1994).

time on the market among homes for sale, and the lagged level of the existing housing stock.[21] The interpretation of these control measures is mostly straight forward. Rising prices encourage developers to tap into increasingly expensive factor inputs and increase supply, while rising interest rates and building costs increase the cost of development and have the opposite effect. Longer time on the market for unsold homes signals an increase in inventories that would likely discourage developers from starting new homes. A larger initial stock of housing is likely associated with a greater number of older homes that have become dilapidated or stylistically obsolete, and as such are ripe for replacement. Larger initial stock levels may also reflect the influence of other unobserved factors that drive housing demand and supply.

Our goal is to estimate a model that takes account of the features above, customized to the nature of our data and the timing of the LIHTC program. Adding LIHTC development to the model will then allow us to assess the crowd out effects of the program. As will become apparent, this requires that we address two difficult empirical challenges. The first challenge is that several key drivers of housing starts are difficult if not impossible to directly measure, while the second is that key control measures, including LIHTC development, may be endogenous. These issues will each be addressed in turn.

As a starting point, we express housing starts between 1990 and 2000 (the time period of our data) in a given location as follows,

$$\begin{aligned} s_d^{rental,unsubsidized} &= b_1 \Delta p_d^{Q-adjusted} + b_2 S_{d,1990} + b_3 \Delta rate \\ &\quad + b_4 \Delta q_r^{Non-LandInputs} + b_5 S_{d,1990}^{rental,vacant} + \varepsilon_d \end{aligned} \quad . \tag{3.1}$$

In (3.1), the subscript *d* denotes the location in question, while *r* is the broader geographic region in which *d* is situated. This distinction allows us to use region fixed effects to control for some variables, as alluded to earlier and will be clarified shortly. Recall also, that we code geography in three different ways. Initially, we let *d* denote a given MSA. In this instance, we treat the state in which the county is

[21]Mayer and Somerville (2000) also allow for an AR(1) term and a time trend that help to soak up the influence of unobserved factors. As will become apparent below, we also allow for serial correlation for similar reasons. Our focus on a single cross-section eliminates any issue of time trends.

located as the broader region, denoted by *r*. For MSAs that cross state borders, we divide the MSA into pieces corresponding to those portions in the constituent states and treat each piece as a separate observation. In our second approach, we let *d* denote a given county. In this instance, we treat the MSA in which the county is located as the broader region provided the county is in an MSA. For counties not in MSAs, *r* is coded as the state in which the county is situated. In our final approach, *d* is set to a circle of radius 10 miles with *r* set to the county in which the geographic center of the circle is located.

Defining *d* and *r* in this manner allows us to control for the underlying drivers of housing starts noted above. To clarify, note first that the dependent variable, $s_d^{rental,unsubsidized}$, is the number of unsubsidized rental housing starts between 1990 and 2000 and clearly varies with location, *d*. The term $S_{d,1990}$ is the lagged total number of housing units (rental plus owner-occupied) in location *d* in 1990. As noted earlier, lagged housing stock levels likely control for a host of unobserved local factors that might otherwise bias our estimates of LIHTC effects. Moreover, by conditioning on lagged housing stocks, our dependent variable necessarily reflects the change in housing stocks between periods. The term $S_{d,1990}^{rental,vacant}$, meanwhile, is the number of vacant rental units in 1990 and allows for possible disequilibrium conditions in that year. These terms all vary with *d.* Moreover, all three of the variables just noted can be readily measured for each treatment of geography using the root census tract data described earlier.

To further enrich our specification we decompose $S_{d,1990}$ into separate components for housing built in the 1980s, housing built in the 1970s, and housing built prior to 1970. Moreover, we include separate measures for of each of these terms for the rental and owner-occupied stocks of housing. Accordingly, we rewrite (3.1) as,

$$\begin{aligned} s_d^{rental,unsubsidized} &= b_1 \Delta p_d^{Q-adjusted} \\ &\quad + b_{2,1}^{rent} S_{d,1990}^{rent,80to90} + b_{2,2}^{rent} S_{d,1990}^{rent,70to80} + b_{2,3}^{rent} S_{d,1990}^{rent,pre70} \\ &\quad + b_{2,1}^{own} S_{d,1990}^{own,80to90} + b_{2,2}^{own} S_{d,1990}^{own,70to80} + b_{2,3}^{own} S_{d,1990}^{own,pre70} \\ &\quad + b_3 \Delta rate + b_4 \Delta q_r^{Non-LandInputs} + b_5 S_{d,1990}^{rental,vacant} + \varepsilon_d \end{aligned} \quad . \tag{3.2}$$

Including the 1990 age distribution of the housing stock for both rental and owner-occupied housing has several advantages over the specification in (3.1). To the extent that unobserved local drivers of housing starts are serially correlated, the terms related to housing developed in the 1980s will tend to soak up such effects (e.g. $S_{d,1990}^{rent,80to90}$). In effect, we allow for an AR(1) process as in Mayer and Somerville (2000). At the same time, older housing stocks present in 1990 (e.g. housing built prior to 1970, such as $S_{d,1990}^{rent,pre70}$) would be subject to increasing deterioration as the 1990s wore on. As a result, these stocks would be increasingly ripe for replacement and their presence might lead to more housing starts in the 1990s. To the extent that rental and owner-occupied markets are close substitutes, distinguishing between these two sets of housing stocks would have little impact. But a more likely pattern is that rental and owner-occupied housing are only weak substitutes, in which case we would expect much stronger effects associated with the lagged rental as opposed to owner-occupied stocks.

Of the remaining terms in (3.1), $\Delta rate$, the change in the real interest rates, is the simplest to take into account. This term is assumed to be common across all locations during the 1990s. As we estimate using a single cross section, $\Delta rate$ becomes a constant in (3.2). In a similar vein, the term $\Delta q_r^{Non-LandInputs}$ represents the change in the price of non-land factor inputs (materials and labor). Importantly, we assume that this term varies across regions but is constant within a given *r*. This seems reasonable as an approximation. Given this assumption, we can remove the influence of $\Delta q_r^{Non-LandInputs}$ by including region fixed effects in the model.

That leaves just $\Delta p_d^{Q-adjusted}$, a term that represents the change in quality-adjusted house prices between 1990 and 2000 in location *d*. If we are willing to assume that the growth in house prices is constant across locations within a given region, then including region fixed effects in the model would difference away this term as well. For two reasons, however, this is not an appealing assumption. First, demand shocks could easily differ across locations even within the defined broader regions. This seems especially possible when *d* is small relative to *r* (e.g. when *d* is set to an MSA and *r* to its state). Second, different locations may be in different states of housing market disequilibrium in 1990 relative to the long run as reflected in differing vacancy rates. This would further contribute to differences at the local level in the degree to which house prices might change over the 1990s. We resolve this problem by proxying for $\Delta p_d^{Q-adjusted}$ as follows,

$$\begin{aligned}\Delta p_d^{Q-adjusted} \approx\ & a_1 \Delta Pop_d + a_2 \Delta MedInc_d \\ & + a_3 \Delta Pop_d \cdot S_{d,1990}^{rental,vacant} + a_3 \Delta MedInc_d \cdot S_{d,1990}^{rental,vacant} \\ & + a_4 D_d + \delta_r\end{aligned} \quad . \tag{3.3}$$

In (3.3), the region fixed effect, δ_r, captures the region wide change in quality-adjusted house prices. The remaining terms reflect deviations from the mean effect for individual locations. The term ΔPop_d is the change in population in location *d* between 1990 and 2000. Similarly, $\Delta MedInc_d$ is the change in median family income in location *d*. These two terms are fundamental drivers of shifts in demand and should have a positive impact on changes in price at the local level. However, the degree to which that occurs is likely sensitive to the number of vacant units in 1990. When large numbers of vacant units are present, a positive demand shock can be at least partly accommodated by filling up vacant units, and this will mitigate upward pressure on price. For this reason, the interactive terms in (3.3) should have a negative impact on price. Demand shocks likely also vary systematically with a given location's distance from the city center. This is because cities tend to develop and subsequently redevelop from the center outwards over time (e.g. Brueckner and Rosenthal (2009)). To allow for this pattern, the term D_d denotes the distance to the city center in models where geographic units are measured as 10-mile radius circles, and density (number of housing units divided by land area) when we instead use MSA-level or county-level data.[22]

Substituting (3.3) into (3.2) and reordering some of the variables to facilitate review we obtain,

[22] In the circle regressions, we restrict the analysis to just those core census tracts in MSAs, and define the city center as the geographic centroid of the census tract with the highest population density in 2000.

$$
\begin{aligned}
s_d^{rental,unsubsidized} &= b_1^{rent} S_{d,1990}^{rent,80to90} + b_2^{rent} S_{d,1990}^{rent,70to80} + b_3^{rent} S_{d,1990}^{rent,pre70} \\
&\quad + b_3^{own} S_{d,1990}^{own,80to90} + b_4^{own} S_{d,1990}^{own,70to80} + b_5^{own} S_{d,1990}^{own,pre70} \\
&\quad + b_6 S_{d,1990}^{rental,vacant} + b_7 \Delta Pop_d + b_8 \Delta MedInc_d \\
&\quad + b_9 \Delta Pop_d \cdot S_{d,1990}^{rental,vacant} + b_{10} \Delta MedInc_d \cdot S_{d,1990}^{rental,vacant} \\
&\quad + b_{11} D_d + \lambda_r + \varepsilon_d
\end{aligned}
\quad . \qquad (3.4)
$$

Expression (3.4) captures the primary features associated with housing starts models in the literature.[23] Augmenting this equation with the number of LIHTC units built between 1990 and 2000 (denoted s_d^{LIHTC}) yields our primary estimating equation,

$$
\begin{aligned}
s_d^{rental,unsubsidized} &= \theta s_d^{LIHTC} + b_1^{rent} S_{d,1990}^{rent,80to90} + b_2^{rent} S_{d,1990}^{rent,70to80} + b_3^{rent} S_{d,1990}^{rent,pre70} \\
&\quad + b_3^{own} S_{d,1990}^{own,80to90} + b_4^{own} S_{d,1990}^{own,70to80} + b_5^{own} S_{d,1990}^{own,pre70} \\
&\quad + b_6 S_{d,1990}^{rental,vacant} + b_7 \Delta Pop_d + b_8 \Delta MedInc_d \\
&\quad + b_9 \Delta Pop_d \cdot S_{d,1990}^{rental,vacant} + b_{10} \Delta MedInc_d \cdot S_{d,1990}^{rental,vacant} \\
&\quad + b_{11} D_d + \lambda_r + \varepsilon_d
\end{aligned}
\quad . \qquad (3.5)
$$

In this expression, θ is the primary variable of interest. If its coefficient equals 0 that would indicate that construction of LIHTC units has no effect on the number of private, unsubsidized rental housing units built between 1990 and 2000. If instead θ equals -1, that would imply complete crowd out and indicate that LIHTC construction does little to increase the overall stock of rental housing.

4.3 Endogenous variables

Two sets of variables in (3.4) seem especially prone to being endogenous. The first is the key control variable, LIHTC housing development. The second are the controls for the change in population and median income in location *d* between 1990 and 2000. We consider these latter variables first.

New housing construction in a given location has the potential to *attract* families, in addition to serving the needs of households choosing the location for exogenous reasons. Similarly, construction of rental housing may attract lower income families seeking such housing opportunities. For both reasons, it

[23] Note that λ_r captures the influence of all region-specific effects, including changes in real interest rates, changes in non-land factor price inputs, and changes to the common component to price movements (δ_r from (3.3)).

is possible that the change in population and median income in a given location could be endogenous to new housing development. To address this concern, we measure the change in population (median income) in location *d* by multiplying the 1990 population (median income) level in *d* by the percentage growth in population (median income) for a broader geographic area. When setting *d* to MSA-level geography, we use the state-level percentage change in population and median income to scale the state-specific portion of the MSA measures from 1990. When setting *d* to county-level geography, we use the state-level percentage change in population and median income to scale the county-level measures from 1990. When setting *d* to circle-level geography, we use the MSA-level percentage change in population and median income for the MSA in which the circle center is located. In all three cases, we make two assumptions: (i) the 1990 population and median income level in location *d* is exogenous to housing development between 1990 and 2000, and (ii) the percentage change in population and median income at the broader region level is exogenous to construction of new housing in the 1990s in the corresponding area of focus, as denoted by *d*. The first assumption is really no different than assuming that the stock of housing in 1990 is exogenous, an assumption already implicit in the housing starts model, (3.1). The second assumption is equivalent roughly to arguing that development in a small geographic unit would not noticeably affect the overall rate of population and income growth for a much larger geographic area.

A different strategy is employed to control for the possibly endogenous placement of LIHTC units. We estimate our models by two-stage least squares and instrument LIHTC development in the manner described in the Introduction. Specifically, recall that federal law instructs the IRS to allocate LIHTC credits across states in proportion to each state's share of the U.S. population, with the total number of credits nationwide set by congress. Treating state population share as given, we assume that the total allocation of credits to each state in the 1990s is also exogenous. It seems likely that states allocate their credits at least in part though a political process that mimics the federal government. Accordingly, as noted earlier, we assume that the allocation of credits within a state between 1990 and 2000 is based partly on a given local area's (e.g., county) share of state population in 1990. Multiplying

1990 local population shares by the state allocation of LIHTC credits yields our first instrument for the number of LIHTC units in a given location.

Also as noted earlier, for our second instrument we allow for the possibility that cronyism may further influence within state allocations of the valuable LIHTC subsidies. Accordingly, in the MSA-level regressions, for each state-specific portion of a given MSA we code a dummy variable equal to 1 based on whether that area voted for the sitting Governor in 1989. In the county-level regressions, for each county we code a dummy variable equal to 1 based on whether the county voted for the sitting Governor in 1989.[24] For the 10-mile radius circle regressions, we use this same county-level voting outcome in coding the dummy variable as corresponds to the county in which the geographic centroid of the circle is located. Multiplying this measure by the first instrument described above allows for the possibility that communities with a history of voting for the winning gubernatorial candidate during the initial years of the LIHTC program received a greater share of tax credits throughout the 1990s.

4.4 Overlapping circles

A final empirical issue concerns the overlapping nature of the circles when *d* is set to 10-mile radius circle areas. Recall that the circle measures are drawn around the geographic centroids of the underlying census tracts. This implies that nearby circles will often overlap. For the independent variables such overlap presents no special problems. But for the dependent variables overlapping circles cause the underlying census tract information to be repeated across observations. Failing to address that issue would cause the standard errors from the model to be biased downwards but would not bias the coefficient estimates.

A convenient solution to this problem is available. Specifically, when measuring our variables in circles, we cluster the standard errors at the county level, consistent with our inclusion of county-level

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[24]County-level election results for Governor from 1985-1988 were obtained from the data series "General Election Data for the United States, 1950-1990" produced by Inter-University Consortium for Political and Social Research (ICPSR).

fixed effects. This allows for repetition of information across dependent variables for observations centered in the same county. In instances where circle overlap is limited, clustering in this manner likely overcompensates for the problem. This would cause our estimation procedure to be inefficient but would not affect the consistency of the crowd out estimates.

5. Results

5.1 Rental Housing

Table 4 presents OLS and 2SLS estimates for three sets of crowd out regressions based on MSA, county, and 10-mile circle geography as described earlier. In all cases, our dependent variable is the number of private rental units constructed between 1990 and 2000. Differing levels of fixed effects are included in the regressions based on the underlying geographic level of analysis. For the MSA-level regressions, state-level fixed effects are included. For the county-level regressions, MSA or state-specific rural area fixed effects are included. For the 10-mile circle regressions, county-level fixed effects are included. It should also be noted that for the county-level regressions we drop counties in MSAs made up entirely of that single county;[25] for the 10-mile circle regressions we restrict our sample to circle units whose centroids are located in MSAs.[26] In all cases, standard errors for the model estimates are clustered at the same geographic level as used for the fixed effects. As noted earlier, for the circle-based regressions, clustering the standard errors at the county level addresses possible concerns about the overlapping nature of the circle observations.

Consider now the coefficients in Table 4 on the terms other than LIHTC development. Reading across the columns, the first point to recognize is that the coefficients on these variables differ little regardless of whether LIHTC is treated as exogenous (using OLS) or endogenous (using 2SLS). Even

[25] It should be emphasized that in all cases, counties are drawn from throughout the U.S. yielding a sample of just over 3,000 observations. Dropping the non-MSA counties had little impact on the results and for that reason results from those models are not reported.

[26] We restrict the sample to locations within MSAs for two reasons. First, the underlying census tracts are smaller in MSAs and this helps to reduce measurement error when recoding the data to circles. Second, MSAs have well defined centers allowing us to include distance to the MSA center as a control variable.

more striking, the coefficients on the lagged stocks of rental and owner-occupied housing are not particularly sensitive to the underlying geographic unit of analysis. The most extreme instance is for rental housing built in the 1980s, for which the coefficient is between 0.3 and 0.4 across all of the different models. This pattern is suggestive that two conditions hold as one reduces the geographic scale of the unit of observation: (i) the dependent and independent variables are reduced in equal proportions (approximately), and (ii), the relationship between lagged housing stocks and new construction are largely similar across different sized geographic units.

A closer examination of the lagged housing stock variables suggests a strong pattern of first-order serial correlation in rental housing starts. Consider, for example, the estimates for the 10-mile circle regressions as presented in the second to last column from the right in Table 4. Among rental units present in 1990, the coefficient on those built in the 1980s is 0.39 with a t-ratio of 6.55. For rental units built in the 1970s and also prior to 1970, the corresponding coefficients are close to zero and statistically insignificant. Also observe that the corresponding estimates on the 1990 stock of owner-occupied housing are smaller: the coefficient on units built in the 1980s, for example, is just 0.16 with a t-ratio of 4.83. These patterns suggest that unobserved trends that contribute to housing construction in the 1980s tend to persist into the 1990s. In addition, as might be anticipated, rental housing construction is far more sensitive to existing rental housing stocks as compared to existing stocks of owner-occupied housing. This is consistent with the idea that housing markets are heavily segmented between the rental and owner-occupied sectors. This further suggests that the impact of LIHTC construction on unsubsidized development is likely to be most pronounced in the rental as opposed to owner-occupied sector of the housing market, an idea we will return to later in the discussion.

Observe next that change in population and median income are both associated with positive effects on rental housing construction in the 1990s. For the 10-mile OLS model, in the case of population, the coefficient is 0.0219 with a t-ratio of 1.39; for the change in median income, the coefficient is 0.93 with a t-ratio of 3.68. Moreover, whereas the interaction between the population variable and rental vacancies is clearly insignificant (a t-ratio of 0.79), the interaction for the income variable is negative and significant, with a t-ratio of -3.65. These results indicate that, at least in the case of income, growth increases rental housing construction, but less so to the extent that there are a greater number of vacant rental units present in 1990.[27] This is consistent with priors: income and population push demand up and increase construction, but to a lesser extent if vacant units are present.

Consider now the primary coefficients of interest, the impact of LIHTC development on unsubsidized construction in the 1990s.[28] We begin with the OLS estimates. For the MSA, county, and 10-mile circle regressions, the OLS coefficients on LIHTC development are positive 1.2 (with a t-ratio of 4.90) , -0.05 (with a t-ratio -0.16), and -0.199 (with a t-ratio of (-1.10). Taken at face value, OLS fails to provide evidence of a LIHTC crowd out effect. It is noteworthy, however, that the OLS coefficients on LIHTC development become more negative as the level of geography and underlying fixed effects included in the model become more precise: positive 1.2 for the MSA-level regression with state-level fixed effects, -0.05 for the county-level regression with MSA/State-Rural fixed effects, and -0.199 for the 10-mile circle regression with county fixed effects. This indicates that failing to adequately control for unobserved drivers of construction – many of which are locally based – biases the LIHTC coefficient towards a more positive number. This is consistent with the idea that, conditional on the other model controls, unobserved local factors conducive to new construction tend to attract both subsidized LIHTC and unsubsidized development.

Comparing OLS to 2SLS estimates of the LIHTC coefficients reinforces this view. For each level of geography, 2SLS yields a much more negative estimate of the LIHTC coefficient: -0.35 (with a t-ratio of -0.51) for the MSA-level regression, -0.98 (with a t-ratio of -1.78) for the county-level regression,

---

[27] The coefficients on the change in population and median income in the 1990s, and their interactions with 1990 vacancy rates, do differ somewhat with the geographic level of analysis. However, the broad patterns are robust. Specifically, the direct effects of growth in population and income are always positive, and those effects but those effects are moderated in the presence of higher 1990 vacancy rates.

[28] The remaining coefficients on the non-LIHTC variables are largely insignificant and are not emphasized for that reason. Note, for example, that although vacancies have a further direct negative effect in the OLS model (a coefficient of -0.158 with a t-ratio of 1.24), the corresponding coefficient is nearly equal to zero when treating LIHTC housing as endogenous. In addition, there is no evidence in any of the models in Table 4 that density has a discernible effect.

and -1.07 (with a t-ratio of -2.31) for the 10-mile circle regression. Relative to OLS, these estimates confirm the upward (more positive) bias of the OLS model. This further indicates that developers tend to locate LIHTC projects in growing areas in which unsubsidized development is already taking place. While this pattern is not a necessary condition for crowd out to occur, it is certainly consistent with what might be anticipated. Moreover, and perhaps more obvious, these patterns indicate once again that failing to control for unobserved drivers of new construction – in this case as addressed by treating LIHTC construction as endogenous – would cause one to underestimate the crowd out effects of LIHTC development.

Finally, it is important to take note of the magnitude of the estimated crowd out effects. For this we emphasize the 10-mile circle model which we feel is the most robust specification for reasons described above (e.g. the more precise geographic fixed effects). For this model, our estimates indicate that LIHTC development is fully offset by a corresponding reduction in unsubsidized development of new rental housing units. Before elaborating on the seemingly stark implications of this estimate, some further discussion of robustness is in order.

5.2 Instrument strength and validity

Two concerns arise in any instrumental variable procedure, instrument strength and instrument validity (exogeneity). The bottom of Table 4 reports diagnostic statistics on the instruments along with the first stage instrument coefficients. The complete first-stage regressions are reported in Table A-1 of the appendix. Given our preference for the 10-mile circle regression, we emphasize diagnostics for that specification. Although some difference in diagnostics arises across models based on different underlying geographic units of analysis, for the most part the patterns are similar.

For the 10-mile circle regressions, notice that first stage coefficients on the included instruments are positive and individually significant, with t-ratios of 1.95 and 1.84, respectively. The positive coefficients are as anticipated: more populous locations and counties that voted in favor of the winning governor candidate receive a greater allocation of LIHTC credits and related construction. The statistical significance of the instrument coefficients also indicates that the model is at least identified. Importantly, as a pair, the two instruments appear to be strongly correlated with the endogenous variable, as indicated by a Kleibergen-Paap F statistic of 11.24. That value is above the "10" often used to assess whether weak instrument bias is a serious problem (e.g. Stock and Yogo (2005); Murray (2006)). Overall, we conclude that our instruments appear to have the anticipated signs and also that our estimates are unlikely to suffer from weak instrument bias.[29]

In principle, it is also possible to test whether the overidentifying restrictions in the fourth column can be rejected. Evidence that the overidentifying restrictions are invalid would be indicative of model misspecification, including possibly that the instruments are endogenous. For the 10-mile circle regressions, results from a Sargan test indicate a P-value of 0.1283. On the surface, this suggests that we cannot reject the null that the model and instruments are properly specified. However, we caution that the Sargan test is known to be sensitive to model specification and to have weak power. This is especially true when the mechanism for why the instruments are correlated with the endogenous variable is similar, as is clearly the case here given that both instruments draw on population shares as noted (e.g. Davidson and McKinnon (2004); Cameron and Trivedi (2006); Murray (2006)).

The validity of the instruments is further supported in regressions using each instrument separately. Provided all of the instruments are valid, then estimation using instrument subsets should yield asymptotically similar estimates. Table 5 explores this idea by presenting four sets of estimates for the 10-mile circle regressions. Columns 1 and 4 repeat the OLS and 2SLS estimates provided in Table 4. Column 2 provides 2SLS estimates using only population share as an instrument, while column 3 uses only the cronyism variable as an instrument.

For the middle two columns of Table 5, observe that the LIHTC coefficient equals -0.67 (with a t-ratio of 1.20) when population share is used as the instrument and -1.47 (with a t-ratio of 3.02) when the

[29]Formal critical values for weak instrument tests have yet to be developed for the case where the standard errors are allowed to be heteroscedastic, as is the situation here in that we cluster the standard errors (see, for example, Stock and Yogo (2005)). Nevertheless, the evidence at the bottom of Table 4 is quite suggestive that weak instrument bias is not a problem.

cronyism variable is used as the instrument. In comparison, when both instruments are included in the first stage, the LIHTC coefficient equals -1.07 as previously reported. Moreover, it is clear that when the two instruments are used alone in the middle two columns, each has a positive coefficient that is highly significant and therefore strongly correlated with the endogenous variable (note also the Kleibergen-Paap F statistics well above 10). More generally, while there are clearly differences in the LIHTC coefficient estimates across models, all three 2SLS models point in the same direction: evidence continues to indicate that LIHTC development is largely offset by displacement of unsubsidized private rental construction.

5.3 Owner-occupied construction

Earlier in the discussion, we noted that evidence in Table 4 suggested that unsubsidized rental housing construction is much more closely tied to the composition of the existing stock of rental units in a given area as opposed to the existing stock of owner-occupied homes. This suggested but did not confirm that subsidized rental housing construction – such as LIHTC development – would exert a greater displacement effect on the rental side of the market. We consider this question in Table 6.

Table 6 presents LIHTC crowd out estimates for three sets of OLS and 2SLS 10-mile circle regressions. The first are the estimates of private rental construction from Table 4 which are repeated once again to facilitate comparison. The second use construction of owner-occupied housing units in the 1990s as the dependent variable. The third use the sum of private rental plus owner-occupied construction in the 1990s as the dependent variable. For these latter two models the vacancy measures in the regressions are based on owner-occupied and rental plus owner-occupied vacancies, respectively. All other features of the model are as before but the other model coefficients are suppressed in Table 6 to focus attention on the LIHTC coefficients. The complete set of estimates for these models are provided in Table A-2 of the appendix.

In Table 6, notice that for each set of regressions, OLS yields substantially more positive estimates than 2SLS: for the owner-occupied and rental-plus-owner-occupied models, the OLS coefficients are positive 0.49 (with a t-ratio of 1.90) and positive 0.34 (with a t-ratio of 0.84), respectively. The corresponding 2SLS estimates for these models are -0.6877 and -1.4, respectively (with respective t-values of -0.80 and -1.52). Once again, evidence indicates the importance of controlling for unobserved drivers of LIHTC development that would otherwise bias the LIHTC coefficient towards a more positive value.

Observe also that the 2SLS LIHTC coefficient is smaller and less precise for the owner-occupied sector as compared to the rental sector: -0.68 with a standard error of 0.86 (and a t-ratio of -0.8) for the owner-occupied sector versus -1.07 with a standard error of 0.46 (and a t-ratio of -2.31) for the rental sector. When combining the two sectors, the LIHTC point estimate is larger in magnitude (-1.406), but the corresponding standard error is also quite large (0.93) resulting in a t-value of -1.52. Overall, these results provide at most limited evidence that LIHTC development might displace new construction of owner-occupied units. Instead, consistent with patterns in Table 4, the evidence here suggests that crowd out effects of LIHTC construction likely arise primarily through displacement of unsubsidized rental housing construction.

6. Conclusion

The recent dramatic growth of the Low-Income Housing Tax Credit (LIHTC) program has caused an old question to gain new importance. Should government provide low-income housing support through tenant- or placed-based programs? In that context, the LIHTC program has ballooned since its inception in 1987, and is now the largest subsidized rental housing construction program in U.S. history. The program subsidizes 30 to 91 percent of construction costs for eligible projects, 44 percent of which are located in middle- and higher-income neighborhoods, and has accounted for one-third of all multi-family rental housing construction in recent years. Moreover, recent proposals in Congress have sought to double the size of the program. Nevertheless, little is known about the efficacy of this increasingly important and expensive program. This paper has sought to fill part of that gap.

Our most important finding is that displacement of private rental housing construction as a result of the LIHTC program is substantial. Our most robust estimates suggest that nearly all LIHTC development is offset by crowd out resulting in a corresponding reduction in unsubsidized construction of rental housing units. Further analysis fails to provide convincing evidence that LIHTC development affects construction of owner-occupied units, indicating that as might be anticipated, LIHTC displacement effects arise primarily in the rental sector of the housing market.

These findings suggest that proponents of the LIHTC program need to look beyond simple expansion of the overall stock of rental housing to justify the program's continuance. One possibility is that LIHTC development may affect the *location* of where low-moderate rental housing opportunities are found. Summary measures, for example, indicate that 16 percent of LIHTC projects have been built in neighborhoods in the upper third of their MSA's income distribution, and another 28 percent in communities in the middle third of the income distribution. This is radically different from past public housing programs and raises the possibility that LIHTC development may help low and moderate income families gain access to higher quality local schools and other local public services. We leave this as an area for further research.

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Figure 1. Place-Based Subsidized Construction and Demolitions of Housing Units

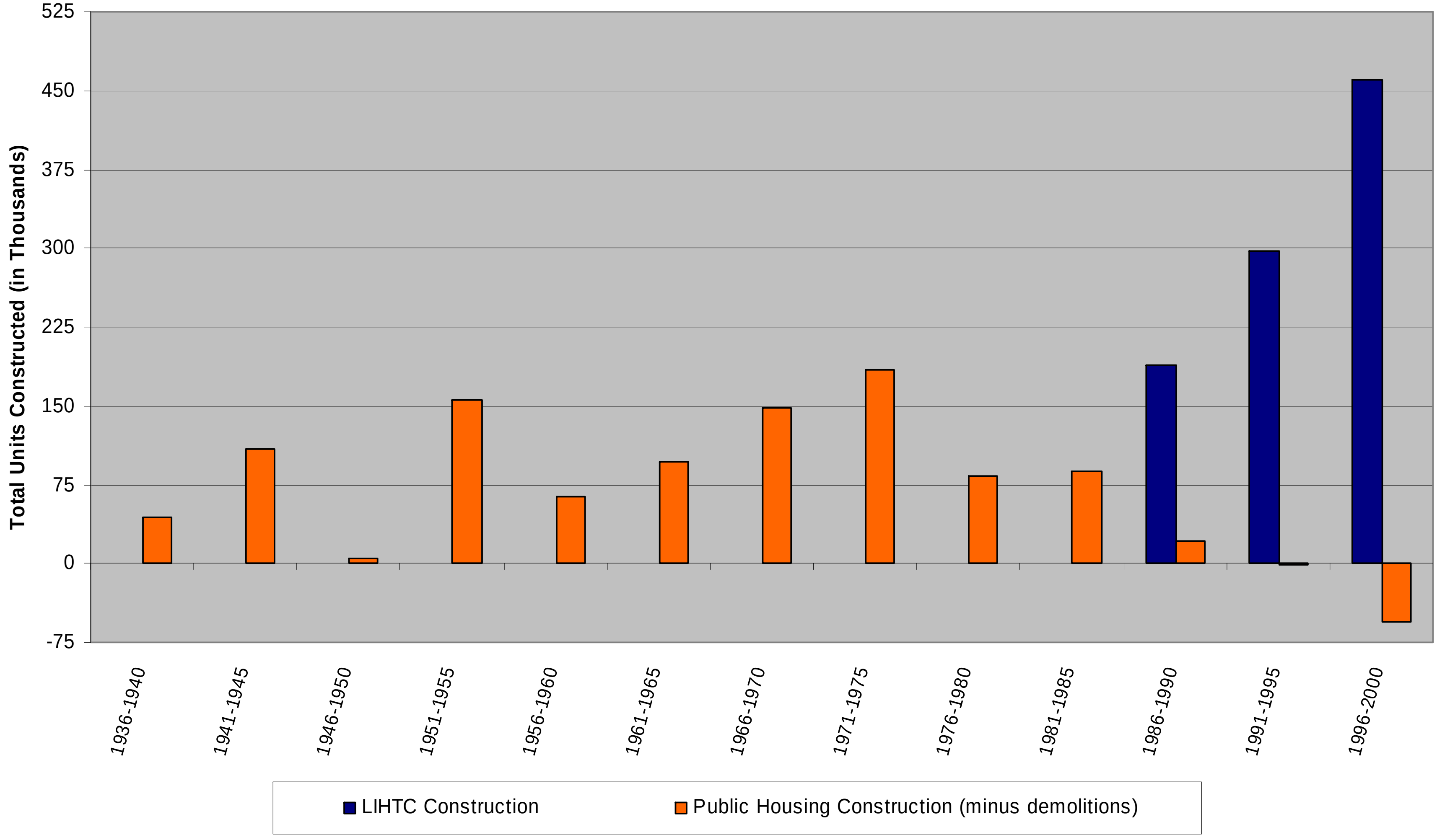

Figure 2a. Location of Low-Income Housing Tax Credit Units by 2000 Neighborhood Income Status

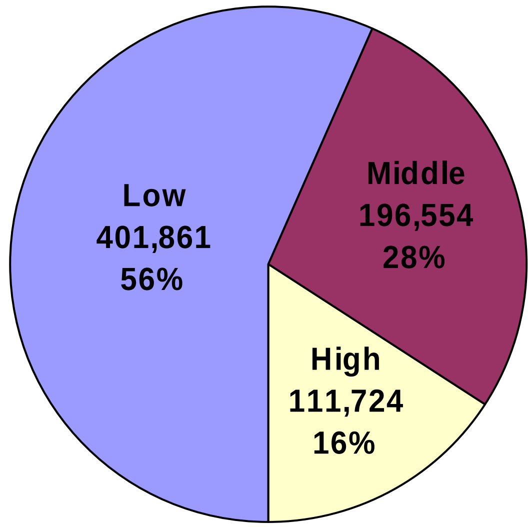

Figure 2b. Location of Traditional Public Housing Units by 2000 Neighborhood Income Status

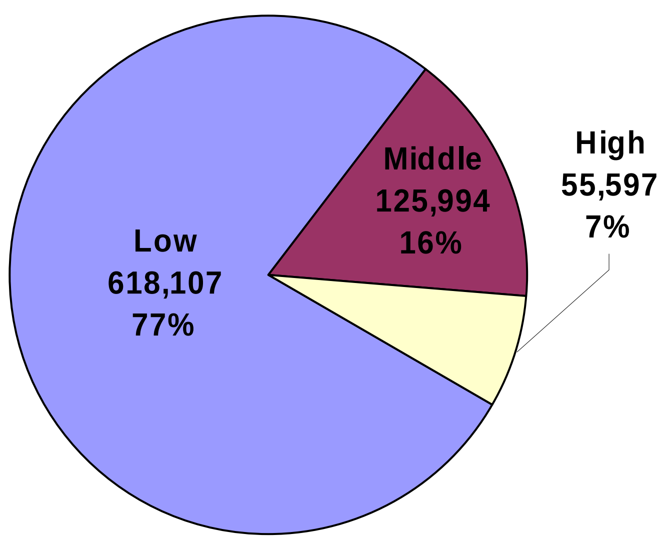

Figure 3a: Crowd Out of Rental Housing With Elastic Demand

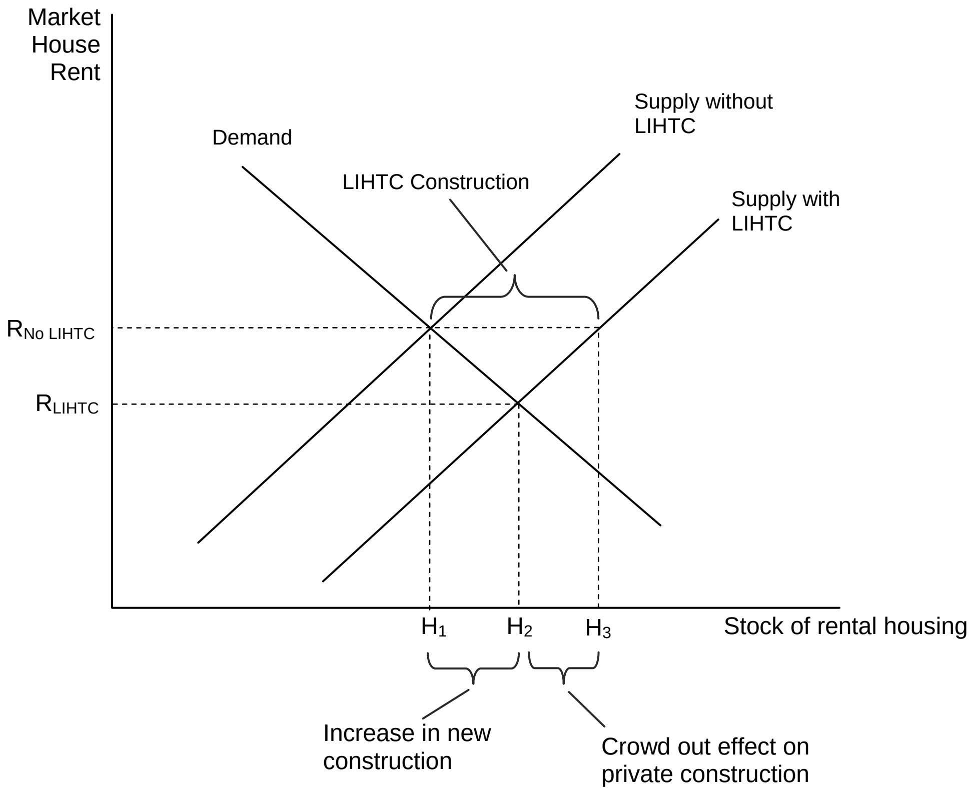

Figure 3b: Crowd Out of Rental Housing With Inelastic Demand

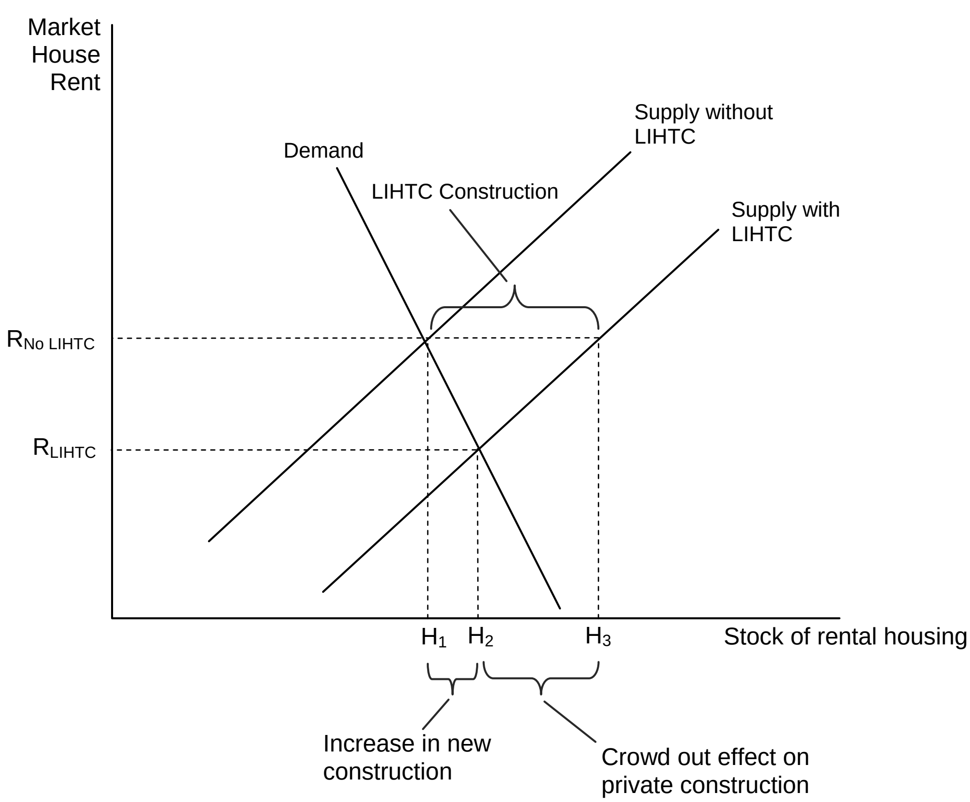

Table 1. National LIHTC Summary Statistics[a]

	Total Annual Allocations ($)[b]	Number of Subsidized Units[c]
1987	980,533,493	34,491
1988	3,140,987,971	81,408
1989	4,387,952,511	126,200
1990	2,888,647,156	74,029
1991	5,207,469,242	111,970
1992	4,255,013,370	91,300
1993	5,205,992,598	103,756
1994	5,915,192,114	117,099
1995	4,892,206,044	86,343
1996	4,277,723,133	77,003
1997	4,225,625,522	70,453
1998	3,999,808,231	67,822
1999	3,983,473,499	62,240
2000	3,895,882,268	59,601
2001	4,624,992,306	67,261
2002	5,162,994,677	69,310
2003	5,507,541,467	73,877
2004	5,680,347,051	75,600
2005	5,556,042,690	70,630
2006	6,668,538,964	74,278
Total	**90,456,964,308**	**1,594,671**

[a]Data Compiled by the National Council of State Housing Authorities.

[b]Calculation made assuming 3 percent inflation and that allocated tax credits would be claimed for the 10 years immediately after allocation.

[c]Does not include unsubsidized, market-rate units sometimes included in LIHTC subsidized properties.

Table 2. Federal Government Low-Income Housing Expenditures (2005-2006)[a]
(in Millions of Dollars)

	2005	2006
Internal Revenue Service[b]		
Low-Income Housing Tax Credit	4,700	4,900
Preferential Depreciation Allowance	3,800	4,200
State-issued Tax Exempt Financing for Rental Housing	300	300
Department of Housing and Urban Development[c]		
Housing Choice Vouchers (HCV)	20,064	20,917
Public Housing	5,017	5,734
Other HUD Programs	8,734	4,559
Department of Agriculture[c]		
Rural Housing Administration	1,369	1,029
Total	43,984	41,639

[a]LIHTC costs reflect foregone tax revenues associated with the 10-years of tax credit allocations. Those costs are expected to increase sharply in the next several years given an increase in LIHTC allocations since 2001. Public housing operating costs are likely to decline in coming years as increasing numbers of public housing units are demolished.

[b]Tax Expenditures as Estimated by Joint Committee on Taxation (2005).

[c]Budget of the United States, Office of Management and Budget (2006).

Table 3. Sample Means of Regression Variables

	MSA	County	10 Mile Circle
Private Construction of Rental Housing 1990 - 2000	8582.9	944.0	9371.8
LIHTC Construction 1990 - 2000	1601.9	187.2	2158.6
# of Rental Units Constructed 1980-1989	15799.3	1704.8	19027.8
# of Rental Units Constructed 1970-1979	16786.5	1828.5	22174.7
# of Rental Units Constructed Prior to 1970	42474.2	4798.4	96303.6
# of Owner-Occupied Units Constructed 1980-1989	27004.7	3085.2	19160.1
# of Owner-Occupied Units Constructed 1970-1979	29625.9	3363.2	21629.0
# of Owner-Occupied Units Constructed Prior to 1970	81365.0	9604.7	111190.7
Vacant Rental Units in 1990	7089.0	816.1	10524.7
Vacant Owner-Occupied Units in 1990	2799.5	324.3	3059.7
Change in Population 1990 to 2000 (ΔPop)	77639.0	8429.2	102821.9
Change in Median Inc 1990 to 2000 (ΔInc)	952.4	111.1	577.59
Observations	427	3,052	49,794

Table 4: Private Rental Construction 1990 to 2000
(t-ratios in parentheses)

	MSA		County		10-Mile Circle	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
LIHTC Construction 1990-2000	1.2168	-0.3474	-0.0513	-0.9811	-0.1995	-1.0692
	(4.90)	(-0.51)	(-0.16)	(-1.78)	(-1.10)	(-2.31)
Rental Stock in 1990						
Constructed 1980-1989	0.3757	0.3898	0.3018	0.3285	0.3912	0.3499
	(4.48)	(3.69)	(3.23)	(3.38)	(6.55)	(6.67)
Constructed 1970-1979	-0.1834	-0.0758	0.0053	0.0549	0.0016	0.0197
	(-3.14)	(-0.82)	(0.08)	(0.74)	(0.03)	(0.41)
Constructed Prior to 1970	0.0136	0.0118	0.0016	-0.0032	0.0045	0.0091
	(2.12)	(1.56)	(0.12)	(-0.21)	(0.83)	(1.62)
Owner-Occupied Stock in 1990						
Constructed 1980-1989	-0.0257	0.0403	0.0690	0.0823	0.1557	0.1816
	(-0.44)	(0.50)	(1.16)	(1.44)	(4.83)	(4.88)
Constructed 1970-1979	0.0680	-0.0014	-0.0838	-0.1260	-0.1050	-0.1290
	(1.56)	(-0.02)	(-1.46)	(-1.78)	(-2.97)	(-3.24)
Constructed Prior to 1970	-0.0259	-0.0293	-0.0288	-0.0316	-0.0143	-0.0149
	(-2.93)	(-2.83)	(-2.11)	(-2.12)	(-2.76)	(-3.12)
Vacant Rental Units in 1990 (Vac)	-0.0395	0.0467	-0.1581	-0.0067	-0.0223	0.1184
	(-0.37)	(0.33)	(-1.24)	(-0.04)	(-0.26)	(1.21)
Change in Population 1990 to 2000 (ΔPop)	0.0435	0.0422	0.0804	0.0803	0.0219	0.0267
	(2.85)	(2.49)	(3.19)	(3.20)	(1.39)	(1.59)
Change in Median Inc 1990 to 2000 (ΔInc)	1.0788	2.1464	2.3844	2.7056	0.9275	0.9976
	(1.01)	(1.23)	(1.60)	(1.58)	(3.68)	(3.84)
Vac*ΔPop	-1.81E-07	-3.66E-07	-5.82E-07	-8.37E-07	8.25E-08	2.84E-08
	(-0.72)	(-1.24)	(-2.10)	(-2.66)	(0.79)	(0.26)
Vac*ΔInc	-4.17E-06	-1.32E-06	5.12E-06	16.8E-06	-1.49E-05	-1.53E-05
	(-0.33)	(-0.08)	(0.36)	(1.12)	(-3.65)	(-3.71)
Density (Pop/Sq mile)	1.4371	1.1882	-0.0172	0.0391	-	-
	(2.28)	(2.04)	(-0.24)	(0.44)	-	-
Distance (miles) to the CBD	-	-	-	-	-8.9016	-10.1778
	-	-	-	-	(-0.68)	(-0.71)
Observations	426	426	3,052	3,052	49,794	49,794
Fixed Effects	State	State	MSA/StRural	MSA/StRural	County	County
Cluster	State	State	MSA/StRural	MSA/StRural	County	County
First Stage: StateAlloc*MSAPopShare	-	0.9081	-	-	-	-
	-	(5.19)	-	-	-	-
First Stage: StateAlloc*MSAPopShare*MSAWin	-	-0.0019	-	-	-	-
	-	(-0.02)	-	-	-	-
First Stage: StateAlloc*CntyPopShare	-	-	-	0.7219	-	-
	-	-	-	(3.82)	-	-
First Stage: StateAlloc*CntyPopShare*CntyWin	-	-	-	0.5588	-	-
	-	-	-	(4.26)	-	-
First Stage: StateAlloc*CirclePopShare	-	-	-	-	-	0.4570
	-	-	-	-	-	(1.95)
First Stage: StateAlloc*CirclePopShare*CntyWin	-	-	-	-	-	0.3521
	-	-	-	-	-	(1.84)
Kleibergen-Paap F-Statistic	-	18.28	-	18.76	-	11.24
Sargan OverID P-Value	-	0.4902	-	0.6208	-	0.1283
R-squared	0.9334	0.9183	0.9038	0.8948	0.86	0.84
Root MSE	3420	3787	841	879	1973	2061

Table 5: Private Rental Construction 1990 to 2000 at the 10-Mile Circle Level
With Different Instrument Combinations
(t-ratios in parentheses)

	OLS	**IV With Population Share**	**IV With "Cronyism"**	**IV With Population Share and "Cronyism"**
LIHTC Construction 1990-2000[a]	-0.1995	-0.6714	-1.4710	-1.0692
	(-1.10)	(-1.20)	(-3.02)	(-2.31)
Observations	49,794	49,794	49,794	49,794
Fixed Effects and Clusters	County	County	County	County
First Stage Instruments and Diagnostics				
StateAlloc*CirclePopShare	-	0.7542	-	0.4569
	-	(4.52)	-	(1.95)
StateAlloc*CirclePopShare*CntyWin	-	-	0.5850	0.3582
	-	-	(4.52)	(1.84)
Kleibergen-Paap F-Statistic	-	20.78	20.57	11.24
Sargan OverID P-Value	-	-	-	0.1283
R-squared	0.86	0.85	0.83	0.84
Root MSE	1973	1999	2157	2061

[a]Other control variables are as in Table 4 but are not reported to conserve space.

Table 6: Crowd Out Effects at the 10 Mile Circle Level for Different Market Segments
(t-ratios in parentheses)

	Rental		Owner-Occupied		Rental + Owner-Occupied	
	OLS	**2SLS**	**OLS**	**2SLS**	**OLS**	**2SLS**
LIHTC Construction 1990-2000[a]	-0.1995	-1.0692	0.4922	-0.6877	0.3426	-1.406
	(-1.10)	(-2.31)	(1.90)	(-0.80)	(0.84	(-1.52)
Observations	49,794	49,794	49,794	49,794	49,794	49,794
Fixed Effects and Clusters	County	County	County	County	County	County
First Stage Instruments and Diagnostics						
StateAlloc*CirclePopShare	-	0.4569	-	0.7287	-	0.4226
	-	(1.95)	-	(3.24)	-	(1.77)
StateAlloc*CirclePopShare*CntyWin	-	0.3582	-	0.2291	-	0.4686
	-	(1.84)	-	(1.41)	-	(2.31)
Kleibergen-Paap F-Statistic	-	11.24	-	9.773	-	10.79
Sargan OverID P-Value	-	0.1283	-	0.0412	-	0.0866
R-squared	0.86	0.84	0.69	0.68	0.7605	0.7474
Root MSE	1973	2061	3962	4056	5221	5362

[a]Other control variables are as in Table 4. The complete regressions are reported in Table A-2 of the Appendix.

Table A-1: First-Stage Estimates of LIHTC Construction
(Absolute value of t-ratios in parentheses)

	MSA-Level Geography	**County-Level Geography**	**10-Mile Circle Geography**
StateAlloc*XPopShare[a]	0.9081	0.7219	0.4569
	(5.19)	(3.82)	(1.95)
StateAlloc*XPopShare*XWin[a]	-0.0019	0.5588	0.3521
	(0.02)	(4.26)	(1.84)
Rental Stock in 1990			
Constructed 1980-1989	0.0164	0.0252	-0.0614
	(0.59)	(1.00)	(4.06)
Constructed 1970-1979	0.0302	0.0452	0.0235
	(1.15)	(2.43)	(1.14)
Constructed Prior to 1970	-0.0001	-0.0059	0.0027
	(0.03)	(1.42)	(0.88)
Owner-Occupied Stock in 1990			
Constructed 1980-1989	0.0275	-0.0001	0.0297
	(1.82)	(0.01)	(2.62)
Constructed 1970-1979	-0.0405	-0.0485	-0.0304
	(3.93)	(3.25)	(2.13)
Constructed Prior to 1970	-0.0077	-0.0104	-0.0064
	(3.42)	(2.85)	(2.51)
Vacant Rental Units in 1990 (Vac)	0.0688	0.1389	0.1544
	(1.35)	(3.17)	(5.12)
Change in Population 1990 to 2000 (ΔPop)	-0.0004	-0.0046	0.0022
	(1.24)	(1.52)	(0.87)
Change in Median Inc 1990 to 2000 (ΔInc)	0.4927	0.0997	0.0501
	(1.66)	(0.32)	(0.52)
Vac*ΔPop	-1.29E-06	-1.69E-07	-1.95E-08
	(2.09)	(3.10)	(0.47)
Vac*ΔInc	4.19E-06	1.06E-05	-1.84E-08
	(1.13)	(2.45)	(0.01)
Density (Pop/Sq mile)	-0.0971	0.0579	-1.5563
	(0.55)	(2.76)	(0.47)
Observations	426	3,052	49,794
Fixed Effects	State	MSA/StRural	County
Cluster	State	MSA/StRural	County
Shea Partial R-squared	0.1975	0.2236	0.0785
Kleibergen-Paap F-Statistic	18.28	18.76	11.24
Sargan OverID P-Value	0.4902	0.6208	0.1283

[a]X denotes the geography over which the instruments are measured. For the MSA and county level regressions, population share and the voting outcome (Win) are measured at the MSA and county levels, respectively. For the 10-mile circle regresisons, circle population share and county voting are used.

Table A-2: Housing Construction in Alternative Market Segments 1990 to 2000
(t-ratios in parentheses)

	Owner-Occupied		**Private Rental + Owner-Occupied**	
	OLS	**IV**	**OLS**	**IV**
LIHTC Construction 1990-2000	0.4922	-0.6877	0.3426	-1.4062
	(1.90)	(0.80)	(0.84)	(1.52)
Rental Stock in 1990				
Constructed 1980-1989	0.0098	-0.0289	0.4192	0.3561
	(0.21)	(0.68)	(5.09)	(4.64)
Constructed 1970-1979	-0.1291	-0.0863	-0.1503	-0.0950
	(1.70)	(1.06)	(1.21)	(0.78)
Constructed Prior to 1970	0.0316	0.0436	0.0352	0.0454
	(2.79)	(4.01)	(2.48)	(3.59)
Owner-Occupied Stock in 1990				
Constructed 1980-1989	0.9839	0.9914	1.0906	1.1297
	(9.04)	(8.76)	(8.45)	(8.47)
Constructed 1970-1979	-0.0662	-0.1112	-0.1762	-0.2376
	(0.82)	(1.27)	(1.62)	(1.98)
Constructed Prior to 1970	-0.0260	-0.0301	-0.0473	-0.0515
	(3.23)	(3.54)	(3.87)	(4.38)
Vacant Owner-Occupied Units in 1990 (Vac)	-0.7785	-0.3329	-	-
	(3.01)	(0.85)	-	-
Vacant Owner-Occupied Plus Rental in 1990 (Vac)	-	-	0.0148	0.1033
	-	-	(0.21)	(1.36)
Change in Population 1990 to 2000 (ΔPop)	0.0159	0.0301	0.0392	0.0530
	(0.53)	(0.87)	(0.86)	(1.11)
Change in Median Inc 1990 to 2000 (ΔInc)	0.4542	0.9469	1.3475	1.4701
	(1.24)	(1.70)	(2.01)	(1.87)
Vac*ΔPop	-8.31E-07	-1.56E-06	-1.40E-07	-2.03E-07
	(1.10)	(1.70)	(0.95)	(1.41)
Vac*ΔInc	-2.58E-05	-5.08E-05	-1.08E-05	-1.06E-05
	(1.25)	(1.68)	(1.89)	(1.57)
Distance (miles) to the CBD	-36.0609	-35.7539	-44.8622	-51.0482
	(1.30)	(1.21)	(1.16)	(1.26)
Observations	49,794	49,794	49,794	49,794
Fixed Effects amd Clusters	County	County	County	County
First Stage: StateAlloc*CirclePopShare	-	0.7287	-	0.4226
	-	(3.24)	-	(1.77)
First Stage: StateAlloc*CirclePopShare*CntyWin	-	0.2291	-	0.4686
	-	(1.41)	-	(2.31)
Kleibergen-Paap F-Statistic	-	9.773	-	10.79
Sargan OverID P-Value	-	0.0412	-	0.0866
R-squared	0.6913	0.6765	0.7605	0.7474
Root MSE	3962.07	4055.56	5221.67	5362.08