Journal of Public Economics 94 (2010) 953–966

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Crowd out effects of place-based subsidized rental housing: New evidence from the LIHTC program[☆]

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ARTICLE INFO

Article history:
Received 10 August 2007
Received in revised form 1 July 2010
Accepted 6 July 2010
Available online 23 July 2010

Keywords:
Crowd out
Subsidized housing
LIHTC

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% 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 suggest that the impact of the program on the *number* of newly developed rental housing units appears to be small.

"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. …"

Statements Submitted to Congressional Record: May 26, 2005 By Rep. William Jefferson (D-LA)

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 LIHTC construction crowds out unsubsidized 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 housing units through "traditional" public housing programs. Importantly, those programs typically limit occupancy to families near or below poverty levels (Olsen, 2003).[2] By the 1980s, on at least two fronts, concerns had begun to erode support for any further expansion of public housing. The first was that government builds, owns, and operates public housing projects. There are basic questions as to whether some of that activity is best left to the private

☆ Funding for this project from the John D. and Catherine T. MacArthur Foundation, the Ford Foundation and the Department of Housing and Urban Development is gratefully acknowledged. This paper has benefited from helpful comments from many individuals. We thank two anonymous referees, Dennis Epple (the editor), Denise DiPasquale, Gary Engelhardt, Jeffrey Kubik, Edgar Olsen, Erika Poethig, Steve Ross, Michael Stegman, Bruce Weinberg, Johnny Yinger, and seminar participants at the January 2007 AREUEA meetings and Ohio State University for helpful comments. Any remaining errors are ours alone.

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2 Olsen (2003) points out that there are at least 29 different public housing programs. Families in these projects typically pay 30% of their gross income towards rent.

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doi:10.1016/j.jpubeco.2010.07.002

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% 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.

sector. The second was that public housing projects created dense clusters of poverty, contributing to concerns about crime, neighborhood decline, and adverse effects on children growing up in the projects (see, for example, Currie and Yelowitz (2000) or Jencks and Mayer (1990)). Primarily for these reasons, construction of public housing came to an end in the early 1980s. Demolition of the worst performing projects began in the 1990s.[3]

With the Tax Reform Act of 1986 (TRA86), the Low Income Housing Tax Credit (LIHTC) program came into being as an alternative to public housing and also to offset the reform's removal of other tax benefits for owners of rental housing (U.S. Congress, Joint Committee on Taxation, 1987).[4] The premise of the LIHTC program differs markedly from public housing and is based on a partnership between government and for-profit developers. Under LIHTC, private market developers receive deep subsidies for non-land construction costs, the generosity of which increases with the share of LIHTC units reserved for tenants with income below limits set by the Department of Housing and Urban Development (HUD).[5] Moreover, developers agree to set rents on targeted units below specified ceilings for at least 15 years, after which market rents are allowed. Given these provisions, LIHTC is in some respects a form of targeted rent control in which eligibility rules restrict occupancy.

LIHTC has quickly overtaken all previous place-based subsidized rental programs to become the largest such program in the nation's history. In Table 1, note 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. Fig. 1 further illustrates this point. The figure displays construction of public housing and LIHTC development by decade over the last 60 years.[6] 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% increase in credits allocated beginning in 2001.[7]

It is evident that LIHTC is an expensive program. It is also clear that LIHTC is a form of targeted rent control. What is less obvious is that LIHTC actually targets *moderate* as opposed to *low*-income tenants. This is especially important when considering the crowd out effects of the LIHTC program, both in general and in relation to public housing. Although the private sector builds little unsubsidized housing for families near or below the poverty line, it regularly builds moderate income rental housing. This suggests that while construction of public housing competes only indirectly with unsubsidized private development, LIHTC projects compete directly with the unsubsidized construction. Because crowd out arises when government competes for market share with the private sector, the targeting of moderate income families increases the likelihood that LIHTC projects displace unsubsidized development. Given the importance of this feature of the LIHTC program, we provide four pieces of evidence below that confirm the tendency of LIHTC to target families with income well above those of traditional public housing.

A first point to consider is that LIHTC rent ceilings on subsidized units are set at relatively high levels. To be precise, rent ceilings are set at 18% of area median household income or AMI (i.e. MSA median income) and are specified annually by HUD.[8] Although the rent ceilings vary by city, they approximate HUD-specified fair market rents (Cummings and DiPasquale, 1999) that are used in regulating rents paid by Section 8 voucher recipients. Those rents are typically between the 40th and 50th percentile of private market contract rents in the prior year. Rents at that level are unaffordable for many low-income families.

A second and related point is that income limits governing tenant eligibility in subsidized LIHTC units are also set at relatively high levels. Specifically, income eligibility in LIHTC subsidized units is set by HUD at 60% of AMI. That limit is well above income limits that govern eligibility for occupants of traditional public housing developments.[9]

A third point concerns the quality of construction in LIHTC units. Targeting of higher income tenants should prompt developers to provide higher quality units (since housing is a normal good). Eriksen

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

[4] The LIHTC program is administered by the Internal Revenue Service under Section 42 of the U.S. Tax code.

[5] The Federal Government subsidizes between 30 and 91% of non-land construction costs for private developers through a 10-year stream of annual nonrefundable federal income tax credits—dollar-for-dollar reductions in their federal income tax liability (Eriksen, 2009). Because the generosity of the subsidy increases with the share of project units allocated to low-income residents, most developers have responded by filling all of their units with income-eligible tenants. Non-compliance with LIHTC rules of operation results in developer forfeiture of future tax credits and also repayment of one-third of previously received allocations plus interest.

[6] The public housing data used to create Fig. 1 were obtained from analysts at HUD and with assistance from the John D. and Catherine T. MacArthur and Abt Associates. These data differ from the publicly available series found at http://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 1990s. The LIHTC data were obtained from HUD at http://www.lihtc.huduser.org.

[7] Those increases will occur even if recent 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).

[8] The 18% AMI income threshold arises because occupants of subsidized units must have incomes below 60% of AMI, and rents charged to individuals in subsidized units must not exceed 30% of that limit.

[9] As an example, for the Washington DC MSA in 2000, the LIHTC income limit specified by HUD for a 3-person family was $43,500 (approximately equal to 60% of median household income for that size family). The maximum allowable rent that a LIHTC project owner could charge such a family in that year was $1088 per month. In comparison, the median rent among all rental units (unsubsidized plus subsidized units) in the Washington DC MSA in 2000 was $840 per month. See the Department for Housing and Urban Development website at http://www.huduser.org.

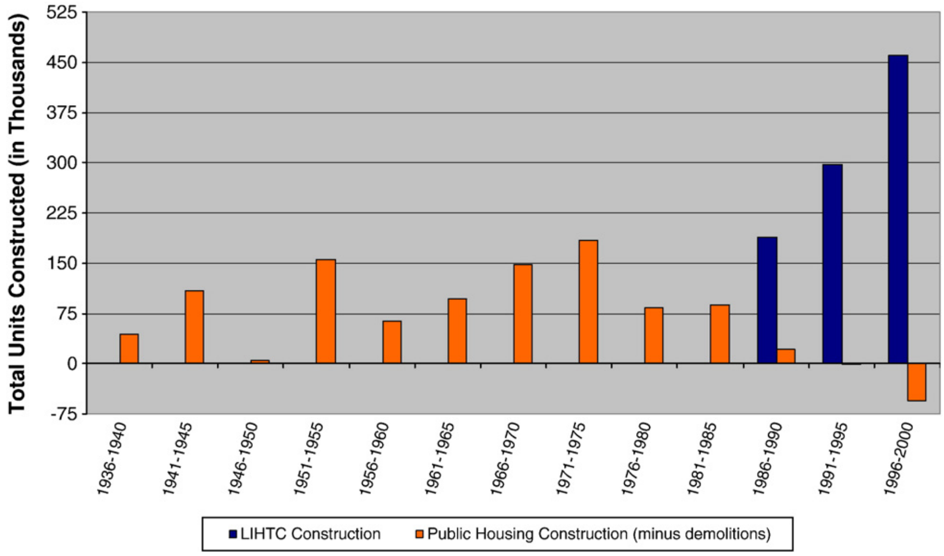

Fig. 1. Place-based subsidized construction and demolitions of housing units.

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	4700	4900
Preferential depreciation allowance	3800	4200
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	5017	5734
Other HUD programs	8734	4559
Department of Agriculture[c]		
Rural housing administration	1369	1029
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 U.S. Congress, Joint Committee on Taxation (2005).
[c] Budget of the United States, Office of Management and Budget (2006).

(2009) reports evidence consistent with that outcome using detailed data on LIHTC developments in California over the 1999 to 2005 period.[10] Eriksen (2009) finds that the median non-land cost of construction for LIHTC projects was \$128 per square foot. Eriksen (2009) further reports that this was 21% higher than the median non-land costs of construction for unsubsidized rental housing development in California over the same period.

It is evident that LIHTC developments are subject to relatively high rent ceilings, high income limits governing eligibility, and are of high quality of construction. Together, these features suggest that LIHTC tenants are likely to be of notably higher income than those served by public housing. Consistent with that premise, Wallace (1995) finds that only 28% of LIHTC residents had income below 50% of AMI (a benchmark used by HUD to define "very low-income" families). In comparison, Wallace (1995) reports that 81% of residents of traditional public housing developments were of very low-income status. Given this evidence and the programmatic features described above, it is clear that LIHTC targets moderate but not low-income families. For that reason, LIHTC is also likely to compete directly with unsubsidized development and this increases the likelihood that LIHTC projects will displace unsubsidized construction.

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 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 (along with other control variables to be described). This 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.[11] Two related empirical challenges then 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. LIHTC development in one neighborhood, therefore, will tend to reduce equilibrium house prices in all

[10] Eriksen (2009) also emphasizes that because LIHTC subsidizes the non-land costs of construction but not the land itself, factor price substitution effects should further encourage developers to increase the quality of construction.

[11] 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) focuses on the impact of subsidized housing development on housing starts.

neighborhoods in the common market, ceteris paribus. Crowd out occurs when subsidized activity depresses market prices, pushing developers of unsubsidized units back down along their supply functions (details are provided later in the paper). This suggests that a full accounting of LIHTC crowd out requires that the geographic unit of analysis be large enough to allow for substitution and related price effects across neighborhoods.

From an empirical perspective, however, increasing the geographic scope 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 making identification of crowd out effects difficult. In the empirical work to follow, we attempt to balance these offsetting factors by estimating our models separately for three different geographic units of analysis: 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. Each also differs in sample size and variation, and each also offers different opportunities to control for location specific factors. For example, we include state fixed effects in the MSA-level models, but use county fixed effects in the 10-mile circle models. The latter are much more rigorous and go further in controlling for unobserved factors that might otherwise bias our estimates of crowd out.

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, as noted earlier, sharp financial penalties are imposed on LIHTC investors if they fail to lease out the minimum required share of project units to families whose incomes are below limits specified by HUD (Eriksen, 2009). The risk of incurring such penalties is sensitive to fluctuations in demand for 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 would cause ordinary least squares estimates of the impact of LIHTC development on private unsubsidized construction to be biased towards a more positive 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 states 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 subsidies. 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.[12]

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%, 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. 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).[13] 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. In the concluding section of the paper, we suggest that the *location* of rental housing opportunities may provide one such motive.

The paper proceeds as follows. The following section provides further detail on 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.

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 studies have also examined 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 crowd out effects 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). Evidence that subsidized rental housing construction increases total housing starts or stocks of housing on less than a one-for-one basis would be indicative of crowd out.[14] Murray (1999) finds that subsidized rental housing programs that target very low-income families generate only a small amount of crowd out. This is

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

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

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

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 and 100% 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 does build moderate income housing.

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% from place-based subsidized rental housing when using data aggregated to the census place level.[15] When the data are instead aggregated to the MSA-level, their point estimate of crowd out falls to roughly 30%. For both levels of geography, LIHTC housing is grouped together 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) include 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 dependent variable.[16] As with Murray (1983, 1999), crowd out effects identified by Sinai and Waldfogel (2005) reflect primarily the impact of 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 crowd out is several times larger than the point estimate.

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. LIHTC developments in QCT tracts are eligible for 30% higher subsidies through the LIHTC program making these tracts especially attractive for LIHTC development, all else equal. 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.

3. Conceptual model

Consider now Fig. 2a 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.[17]

In Fig. 2a, notice that the LIHTC development depresses market rent causing aggregate stocks of rental housing to increase by less than the level of LIHTC construction ($H_2 - H_1 < H_3 - H_1$). The difference, $H_3 - H_2$, represents unsubsidized private construction that is "crowded out" by the LIHTC program. Moreover, Fig. 2b illustrates that as the demand function becomes more inelastic crowd out becomes more pronounced. Indeed, 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 et al. (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.

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 previously estimated supply elasticities is more varied than on the demand side, supply appears to be far from perfectly inelastic when considering expansion of the housing stock.[18]

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. Along with the documented tendency of the LIHTC program to target moderate income households, 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

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.[19] Geolytics re-codes data from each of these years to year-2000 census tract boundaries. These data were combined with

[15] 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.

[16] 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).

[17] Fig. 2a and b suggests 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. A 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 an inward rotation of the unsubsidized segment of the supply function. See Olsen (2007) for a further discussion of this issue.

[18] As a further perspective, if developable land is plentiful and real building costs are constant, then new housing supply should be perfectly elastic (e.g. Rosenthal and Helsley, 1994). This is in contrast to a contraction in the housing stock, for which the durability of housing implies that supply is quite inelastic (e.g. Glaeser and Gyourko (2005)).

[19] See http://www.geolytics.com.

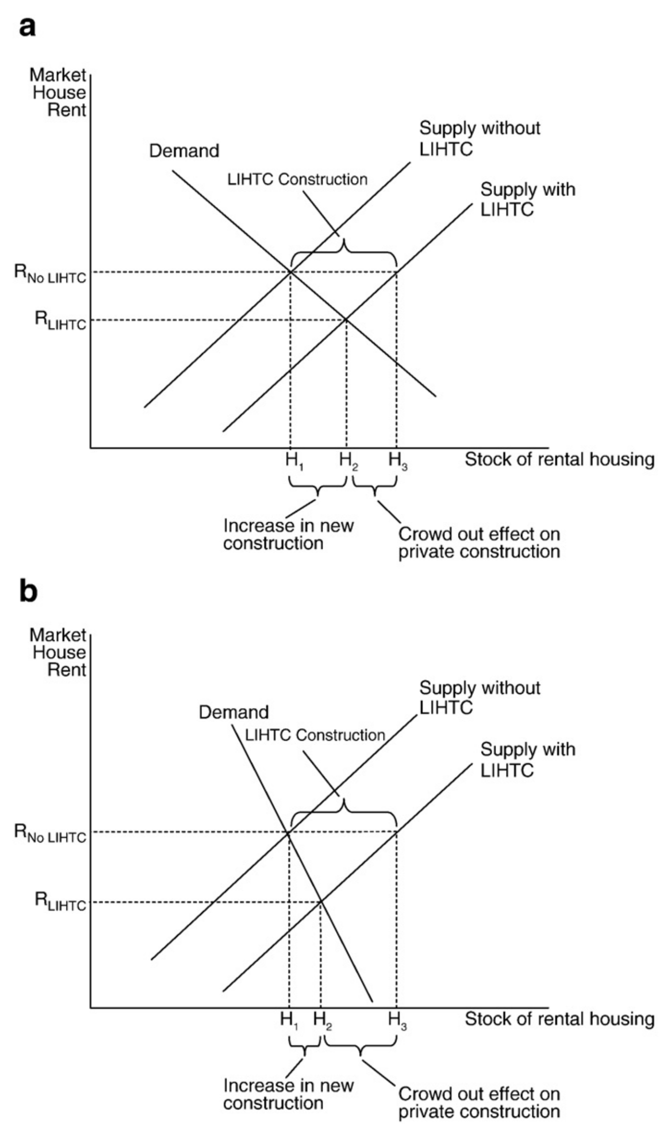

Fig. 2. a: Crowd out of rental housing with elastic demand. b: Crowd out of rental housing with inelastic demand.

information on LIHTC projects placed into service up through 2000. The LIHTC data were obtained from HUD over the Internet.[20] Information on the LIHTC 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 as noted earlier. 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 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.

For our final approach, we reorganize the data into uniform 10-mile radius circular units using Geographic Information Systems (GIS)

[20] See http://www.lihtc.huduser.org.

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	3052	49,794

software with each circle drawn around the geographic centroid of the individual year-2000 census tracts, i ($i=1$, ..., n).[21] 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 for that reason, 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. To allow for the overlapping nature of the circles and implied repetition of some of the information in the dependent variable, we cluster the standard errors in the circle regressions at the county-level. 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 earlier 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 a model that focuses on the total stock of occupied housing at a given point in time. 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. We take this approach in part because it allows us to draw upon the cross-sectional strengths of our data.

Specification of our model is guided by previous work on housing starts, and especially that of Mayer and Somerville (2000).[22] 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.

[21] 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.
[22] See Topel and Rosen (1988) and DiPasquale and Wheaton (1994) for related work.

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 time on the market among homes for sale, and the lagged level of the existing housing stock.[23] The interpretation of these control measures is mostly straight forward. Rising prices encourage developers to 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 a starting point, we express housing starts between 1990 and 2000 (the time period of our data) in a given location as follows,

$$s_d^{rental,unsubsidized} = b_1 \Delta p_d^{Q-adjusted} + b_2 S_{d,1990} + b_3 \Delta rate + b_4 \Delta q_r^{Non-LandInputs} + b_5 S_{d,1990}^{rental,vacant} + \varepsilon_d. \tag{4.1}$$

In Eq. (4.1), 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 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 mi 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, the dependent variable, $s_d^{rental,\,unsubsidized}$, is the number of unsubsidized rental housing starts between 1990 and 2000, while $S_{d,\,1990}$ is the lagged total number of housing units (rental plus owner-occupied) in 1990. As noted earlier, conditioning on $S_{d,\,1990}$ controls for a host of unobserved factors and also ensures that our dependent variable 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, and can all 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, the 1970s, and prior to 1970, and also include separate measures for the rental and owner-occupied stocks of housing. Accordingly, we rewrite Eq. (4.1) as,

$$\begin{aligned} s_d^{rental,unsubsidized} &= b_1 \Delta p_d^{Q-adjusted} + b_{2,1}^{rent} S_{d,1990}^{rent,80to90} + b_{2,2}^{rent} S_{d,1990}^{rent,70to80} \\ &\quad + b_{2,3}^{rent} S_{d,1990}^{rent,pre70} + b_{2,1}^{own} S_{d,1990}^{own,80to90} + b_{2,2}^{own} S_{d,1990}^{own,70to80} \\ &\quad + b_{2,3}^{own} S_{d,1990}^{own,pre70} + b_3 \Delta rate + b_4 \Delta q_r^{Non-LandInputs} \\ &\quad + b_5 S_{d,1990}^{rental,vacant} + \varepsilon_d \end{aligned} \tag{4.2}$$

Including the 1990 age distribution of the housing stock for both rental and owner-occupied housing has several advantages over the specification in Eq. (4.1). 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 have suffered a greater degree of deterioration. These stocks are more likely candidates for replacement and their presence might lead to more housing starts in the 1990s. It is also likely 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 Eq. (4.1), $\Delta rate$, 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 Eq. (4.2). The term $\Delta q_r^{Non-LandInputs}$ represents the change in the price of non-land factor inputs. 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 $\Delta p_d^{Q-adjusted}$, the 1990–2000 change in quality-adjusted house prices in d. If house price growth is similar throughout 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, and especially 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 short run disequilibrium in 1990 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,

$$\begin{aligned} \Delta p_d^{Q-adjusted} \approx\, & a_1 \Delta Pop_d + a_2 \Delta MedInc_d + a_3 \Delta Pop_d \cdot S_{d,1990}^{rental,vacant} \\ & + a_3 \Delta MedInc_d \cdot S_{d,1990}^{rental,vacant}. + a_4 D_d + \delta_r \end{aligned} \tag{4.3}$$

In Eq. (4.3), 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 terms drive 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 Eq. (4.3) 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

[23] 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.

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.[24]

Substituting Eq. (4.3) into Eq. (4.2) and reordering some of the variables to facilitate review we obtain,

$$\begin{aligned} s_d^{rental,unsubsidized} = {} & b_1^{rent} S_{d,1990}^{rent,80to90} + b_2^{rent} S_{d,1990}^{rent,70to80} + b_3^{rent} S_{d,1990}^{rent,pre70} \\ & + b_3^{own} S_{d,1990}^{own,80to90} + b_4^{own} S_{d,1990}^{own,70to80} + b_5^{own} S_{d,1990}^{own,pre70} \\ & + b_6 S_{d,1990}^{rental,vacant} + b_7 \Delta Pop_d + b_8 \Delta MedInc_d \\ & + b_9 \Delta Pop_d \cdot S_{d,1990}^{rental,vacant} + b_{10} \Delta MedInc_d \cdot S_{d,1990}^{rental,vacant} \\ & + b_{11} D_d + \lambda_r + \varepsilon_d \end{aligned} \tag{4.4}$$

Expression (4.4) captures the primary features associated with a typical housing starts model.[25] Adding the number of LIHTC units built between 1990 and 2000 (denoted s_d^{LIHTC}) yields our estimating equation,

$$\begin{aligned} s_d^{rental,unsubsidized} = {} & \theta s_d^{LIHTC} + b_1^{rent} S_{d,1990}^{rent,80to90} + b_2^{rent} S_{d,1990}^{rent,70to80} \\ & + b_3^{rent} S_{d,1990}^{rent,pre70} + b_3^{own} S_{d,1990}^{own,80to90} + b_4^{own} S_{d,1990}^{own,70to80} \\ & + b_5^{own} S_{d,1990}^{own,pre70} + b_6 S_{d,1990}^{rental,vacant} + b_7 \Delta Pop_d \\ & + b_8 \Delta MedInc_d + b_9 \Delta Pop_d \cdot S_{d,1990}^{rental,vacant} \\ & + b_{10} \Delta MedInc_d \cdot S_{d,1990}^{rental,vacant} + b_{11} D_d + \lambda_r + \varepsilon_d \end{aligned} \tag{4.5}$$

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 Eq. (4.4) 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, while construction of rental housing may attract lower income families in particular. For both reasons, it 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: for the MSA- and county-level models, we multiply by the state-level percentage change in population and median income, while for the 10-mile circle model we multiply by the MSA-level percentage change in population and median income. In each case, 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 new housing construction in location d in the 1990s. 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, (4.1). The second assumption is equivalent 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. As described in the Introduction, we estimate our models by two-stage least squares using instruments for LIHTC development that are motivated by the political process that governs the allocation of LIHTC credits. Our first instrument is obtained by multiplying a local area's (e.g. county) 1990 share of its state's population by the state's allocation of LIHTC credits in the 1990s. This mimics the federal allocation of credits across states which depends on a state's share of the nation's population. Our second instrument interacts the first with a 1–0 dummy variable based on whether a given local area voted for the sitting Governor in 1989.[26] For the MSA-level regressions, the 1–0 voting dummy is measured separately for each state-specific portion of a given MSA; for the county-level regressions, the voting dummy is coded based on county-level voting patterns; for the 10-mile radius circle regressions, we also use county-level voting outcomes corresponding to the county in which the geographic centroid of the circle is located. Coding our second instrument in this fashion allows for the possibility that cronyism may further influence within state allocations of the valuable LIHTC subsidies.

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. Because the circle measures are drawn around the geographic centroids of the underlying census tracts, nearby circles will often overlap. This suggests that our dependent variables draw on overlapping information and are not independent. Failing to address this issue would cause the model standard errors to be biased downwards but would not bias the coefficient estimates. To address this issue, when measuring our variables in circles, we cluster the standard errors at the county-level.

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. Fixed effects differ with the underlying geographic level of analysis as described earlier and are noted at the bottom of the table. Note also that for the county-level regressions we drop counties in MSAs made up entirely of that single county; for the 10-mile circle regressions we restrict our sample to circle units whose centroids are located in MSAs.[27] In all cases, standard errors for the model estimates are clustered at the same geographic level as used for the fixed effects.

Consider first the coefficients in Table 4 on the terms other than LIHTC development. Reading across the columns, the coefficients on these variables differ little regardless of whether LIHTC is treated as

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

[25] 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 Eq. (4.3)).

[26] Election results for Governor from 1985 to 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).

[27] 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. Dropping the non-MSA counties had little impact on the results.

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	3052	3052	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

exogenous (using OLS) or endogenous (using 2SLS). Even 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 as one changes the geographic unit of analysis: (i) the dependent and independent variables change in roughly equal proportions, and (ii) the relationship between lagged housing stocks and new construction is 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 those built 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 changes 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

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.

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.[28] 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 impact of LIHTC development on unsubsidized construction in the 1990s.[29] We begin with the OLS estimates. For the MSA, county, and 10-mile circle regressions, the 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 estimates fail 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 local unobserved drivers of construction biases the LIHTC coefficient towards a more positive number. That is consistent with the idea that LIHTC and unsubsidized development are both attracted to areas with profitable unobserved attributes.

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, 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.

Finally, it is important to take note of the magnitude of the estimated crowd out effects. For this purpose 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 point estimate indicates that LIHTC development is fully offset by a corresponding reduction in unsubsidized development of new rental housing units, although the confidence band is wide enough to allow for more moderate effects. Before elaborating, some further discussion of robustness is in order.

5.2. Instrument strength and validity

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 the 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 are 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.[30]

In principle, it is also possible to test whether the overidentifying restrictions in the fourth column can be rejected. Such evidence 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, which fails to 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 (Cameron and Trivedi (2006); Murray (2006)).

[28] 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.

[29] 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 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.

[30] Critical values for weak instrument tests have yet to be developed for the case where the standard errors are allowed to be heteroscedastic, as with clustered standard errors (see, for example, Stock and Yogo (2005)). Nevertheless, the evidence at the bottom of Table 4 is suggestive that weak instrument bias is not a problem.

Table 6
Crowd out effects at the 10 mi 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.

Provided all of the instruments are valid, then estimation using instrument subsets should yield asymptotically similar estimates. Table 5 explores this idea. Columns 1 and 4 repeat the OLS and 2SLS estimates provided in Table 4 to facilitate comparison, while Column 2 provides 2SLS estimates using only population share as an instrument, and 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 cronyism variable is instead used as the instrument. 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 are 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 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. 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 with the other model coefficients suppressed in Table 6 to focus attention on the LIHTC coefficients. The complete set of estimates for these additional models is 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. 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

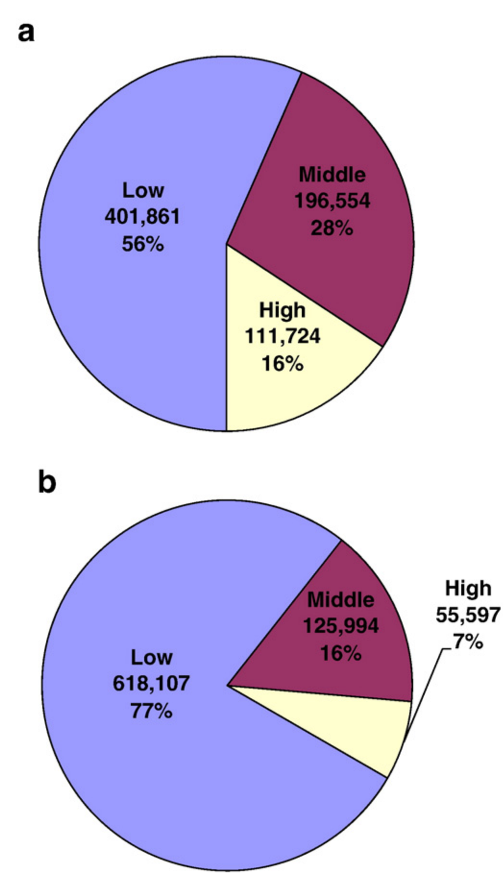

Fig. 3. a. Location of low-income housing tax credit units by 2000 neighborhood income status. b. Location of traditional public housing units by 2000 neighborhood income status.

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 limited evidence that LIHTC development might displace 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% of construction costs for eligible projects 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 point estimates suggest that nearly all LIHTC development is offset by crowd out of unsubsidized rental housing construction, although the confidence band around this estimate allows for more moderate effects. Further analysis fails to provide convincing evidence that LIHTC development affects construction of owner-occupied units. This seems to confirm that 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 in Fig. 3a, for example indicate that as of 2000, 77% of public housing units were in lower-third income locations with most of the rest in middle-third income communities. In sharp contrast, Fig. 3b indicates that as of 2000, 16% of LIHTC units were located in census tracts in the upper third of their MSA income distribution while another 28% were situated in middle-income communities (with the remaining 56% were in lower-third income neighborhoods). The extension of LIHTC housing into middle-third and higher-third income neighborhoods is radically different from past public housing programs. It also 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.

Appendix A

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	3052	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 and 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

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