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Does the low-income housing tax credit increase the supply of housing?☆

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Abstract

The low-income housing tax credit (LIHTC) was originated in conjunction with the Tax Reform Act of 1986 (TRA 86) to provide incentives for private sector production of low-income housing. In this note we examine whether these units have added to the existing stock or merely substituted for unsubsidized units that otherwise would have been built. We explicitly control for effects of the number of other supply-side (e.g., public housing, Section 8 New Construction, Section 236 housing) and demand-side (vouchers and Section 8 Certificates) subsidies. From estimations of a simple cross-state model of the determinants of the stock of housing per 1000 population, we find no significant relationship between the number of LIHTC units (and other subsidized units) built in a given state and the size of the current housing stock, suggesting a high rate of substitution. However, our test is not sufficiently powerful to reject some alternative null hypotheses that suggest a lower rate of substitution, and we make some suggestions for future research.

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1. Introduction

The low-income housing tax credit (LIHTC, also known as Section 42 after the relevant portion of the IRS code) was originated in conjunction with the Tax Reform Act of 1986 to provide incentives for private sector production of low-income housing. The credits provide a mechanism for funding a wide range of projects including new construction, substantial rehabilitation, moderate rehabilitation, acquisition, and repair by existing owners.[1] Over the initial 15 years of the program, roughly 1.2 million units have been built, with roughly $50 billion in tax credits allocated.

Advocates for the LIHTC argue that the program "provide(s) the only means to finance the creation of new affordable apartments" (NCSHA, 2002). Advocates for supply-side programs generally argue that they increase the supply of housing in general (Apgar, 1990). The purpose of this paper is to focus on a particular question related to these assertions, namely, does the LIHTC program add to the stock of housing, or do Section 42 units merely substitute for units that would have been produced with other finance sources?

Table 1 presents the basic time series data on low-income housing tax credit activity from the program's inception in 1987 through 2001. Over this fifteen-year period, approximately 1.2 million Section 42 units have been approved. This represents about a 28% share of the total number of multifamily units approved over the period, or about 6 percent of all housing permits

[1] The low-income housing tax credit provides a "present value" tax credit of 70% of the cost of new construction or 30% of the cost of acquisition of existing low income housing in return for limits on rents charged. The credits are allocated over a ten-year period based on the "Applicable Federal Rate" (AFR). Nominally the value of the credit is 9% annually for the 70% credit and 4% annually for the 30% credit. For acquisition of existing rental housing, the applicable credit is also 4 percent. The developer must decide between two options for the unit. Either 20% of available rental units must be rented to households with income less than 50% of the county median income (adjusted for family size), or 40% of the units must be set aside for households with income less than 60% of the county median income. (The rent can be adjusted in future years as median incomes change). The maximum gross rent, including utilities, paid by households in qualifying units may not exceed 30% of maximum qualifying income. Of course, the credit applies only to those units in the project meeting the income and rent restrictions. The number of Section 42 units in each state is limited each year. Until recently, each year each state has been permitted to allocate $1.25 in tax credits for each resident. In 2001 this ceiling was increased to $1.50 per capita, and in 2001 to $1.75; going forward, the ceiling will be inflation-indexed. Unused credits can be carried forward one year. There is a national "pool" of unused credits turned back by states; states over their limit can draw on this pool. The tax credit can be combined with other housing subsidies. While precise data are hard to find, many LIHTC tenants have also received Section 8 tenant-based assistance.

Table 1
Multifamily housing, and low income housing tax credit units, by year

	LIHTC units approved	Multifamily (5 or more units) permits	Percent of multifamily permits with LIHTC (%)	Thousands of total permits	Percent of total permits with LIHTC (%)
1987	34,491	421,100	8.2	1534	2.2
1988	81,408	386,100	21.1	1456	5.6
1989	126,200	339,800	37.1	1339	9.4
1990	74,029	262,600	28.2	1111	6.7
1991	111,970	152,100	73.6	949	11.8
1992	91,300	138,400	66.0	1095	8.3
1993	68,105	160,200	42.5	1199	5.7
1994	113,845	241,000	47.2	1372	8.3
1995	86,343	271,500	31.8	1333	6.5
1996	79,503	290,000	27.4	1426	5.6
1997	70,220	310,300	22.6	1441	4.9
1998	69,091	355,500	19.4	1612	4.3
1999	62,420	351,100	17.8	1664	3.8
2000	60,348	329,300	18.3	1592	3.8
2001	67,215	321,100	20.9	1612	4.2
Total, 1987–2001	1,196,488	4,330,100	27.6	20,734	5.8

Source. National Council of State Housing Authorities, Census.

during the period.[2] Table 1 also shows that there is substantial variation in Section 42 activity by year; ranging from about 35,000 units in the first year but with several years over 100,000 units (1989, 1991, and 1994). In recent years, the number of units approved has actually declined, partly reflecting the fact that the dollar value of credits per capita permitted per state has been constant until very recently, while construction costs have risen at a more rapid rate than population. In some years, particularly during the real estate recession in 1991–1992, tax credit units were the majority of multifamily units approved and built. In recent years, this proportion has shrunk to about 20%, which is still significant.

Still, the size of the program should be kept in perspective. A large number of units are subsidized in any given year, but compared to many other programs the subsidy is fairly shallow (small on a per-unit basis). For rough comparison, as measured by the level of tax expenditures, currently less than

[2] Not all units that receive permit approval are actually built, but the vast majority is built. Permits are highly correlated with housing starts and housing completions, albeit with a time lag. Data on permits provide the most geographic detail, and in fact provide the sampling framework for the much less comprehensive data collection on permits and completions. They are also the most consistent data with low-income housing tax credit approvals.

five billion dollars is "spent" each year on the LIHTC program, compared to roughly \$20 billion per year in federal on-budget housing assistance (mostly to low-income families, mostly renters) and roughly \$85 billion per year in off-budget housing-related tax expenditures (mostly to upper-middle and upper income families, and mostly homeowners, through mortgage-interest and property-tax deductions).

Given the scope of the program, there has been surprisingly little economic analysis of its performance. A partial explanation is that "traditional" sources of research on housing policies and programs often focus on "traditional" programs out of HUD such as public housing, vouchers, and Section 8. Tax credits are public/private partnerships administered by state agencies, and overseen by the Treasury (who enforces the income caps and other regulations). State agencies and the IRS have little expertise in housing program evaluation, and little incentive to develop such analytic capacity. The current paper is intended to take an initial step to filling this void.

The rest of this paper is organized as follows. First, we briefly review the notion of crowding out and relate it to the price elasticity of supply of housing. Next, the relevant literature is reviewed on the LIHTC program, on supply elasticities, and on crowding out. Then we discuss our model and data sources. Estimation results are presented for two main variants of our model, and other specifications are discussed. Finally, we summarize and present some suggestions for future research.

2. Market effects of housing policies, including "crowding out"

The efficiency and equity issues discussed above are usually framed in terms of the effects of a program or policy on a typical participant household, or in some cases a typical housing unit or project. However, when undertaken at scale, housing programs and policies have the potential to affect the entire housing market, or significant portions of it. It is these latter market-wide effects that we consider in this paper.

Consider a prototypical supply-side program. Suppose, for example, that the government decides to develop a public housing project in a certain city; or alternatively approve a LIHTC development. In either case, at least in the short run, the supply of low-cost housing increases in that city. But what are the long-run effects of such a development on the rest of the market?

It is well known that the answer depends critically on the price elasticity of supply of housing.[3] If the market is more or less unresponsive to changes in price, and if the number of low-income households is largely unaffected

[3] See Green and Malpezzi (2002) or O'Sullivan (1995) for graphical analyses of this dynamic.

by the presence or absence of this additional subsidized housing, then the additional LIHTC housing will have two salutary effects from the point of view of low-income households. First, the LIHTC housing itself will be available for rent, presumably below ex ante market rents, so households who participate will presumably benefit from lower rents and possibly better housing conditions. Second, under these assumptions, the price of housing will fall in the rest of the market, as demand for that fixed stock falls. Thus renters of the unsubsidized stock will also benefit as prices fall; landlords and homeowners will bear the cost of such reductions, or pecuniary externalities.

On the other hand, if the supply of housing is perfectly elastic (and the assumption of no change in number of households is maintained), an initial fall in the price of housing will lead to a reduction in its supply. Such a reduction will take place until the ex ante market price is restored. The total stock of housing will be unaffected ex post; but the new LIHTC units will crowd out an equivalent quantity of unsubsidized housing. Neglecting for the moment any costs of moving or size, quality, or other material differences between unsubsidized and LIHTC units, the participants in LIHTC housing will benefit from lower rents; no pecuniary externalities will be generated; and owners of some pre-existing unsubsidized stock will remove their units from the stock prematurely.

The market effects of demand-side programs will generally be quite different for given assumptions about supply responsiveness. Continuing to hold to the assumption of no household formation effects, consider the effects of a large demand-side program under inelastic supply. A significant number of consumers would have additional purchasing power, but no additional supply would be forthcoming. Such conditions could lead to increases in the price of housing (negative pecuniary externalities) under one of two sets of conditions. First, if the income elasticity of demand for housing is high, and the price elasticity of demand is low, then households newly armed with vouchers will use significant fractions of those vouchers to seek improved housing, and will not be strongly deterred from spending more on housing as they observe its price rising in response to the effects of a large number of households with vouchers. Second, if the vouchers came with binding and enforceable constraints on, say, housing quality, they could cause households to consume more housing than they would otherwise.[4] Generally, if the conditions in this paragraph are met, participant households are better off (though the benefit is partly eroded by higher prices); non-participant

[4] In the sense of larger units. Of course, on the margin such standards will also cause some households to simply choose not to participate in the voucher program. Generally the stricter the standard, the lower will be the participation rate.

renters will generally face higher prices for housing; and owners of existing housing will benefit.

If supply is highly responsive (perfectly elastic), then obviously vouchers will lead to more housing being produced at current market prices. How much depends on the income elasticity of demand, and the existence and severity of standards, of course. If income elasticities are zero (unlikely) and no quality standards are in place, households would not consume any more housing, so market effects would be small or negligible. Participants would in effect benefit from being able to shift their other income from expenditure on housing to expenditure on other goods and services. To the extent income elasticities are positive, and/or standards are binding, more housing would be produced at the market price.

Now consider changes in the other critical assumption, that the presence of new LIHTC housing has no effect on the number of households in general, or low-income households in particular. New low-cost supply could in principle increase the number of households for two reasons. First, if housing is available at low cost, new households could form from the city's existing population, as larger households split up and "doubling up" decreased. Second, households could migrate to the city if they had previously been discouraged by the cost and availability of housing elsewhere. If such household formation effects are significant, they would obviously erode the available pecuniary externality in the inelastic case. If new households completely eroded the pecuniary externality (i.e., if there were very high rates of new household formation and migration in response to the new housing) it must still be reckoned that these new households would be better off (why else would they move?), and that other spillovers would be logically possible, e.g., lessened crowding in their previous units, conceivably even lower rents in the cities from whence they moved. In the case of elastic supply, for a given behavioral pattern of household formation and migration, the effects would be lessened since there is *ex post* no change in total supply of housing or its price

3. Previous research

3.1. Previous research on the LIHTC program

As we noted above, the Section 42 program has been understudied relative to its importance. Many practitioner-oriented articles have been written. Several descriptive analyses have been undertaken by government agencies or their consultants such as ICF (1991), GAO (1993, 1990, 1989), CBO (1992), and Cummings and DiPasquale (1999). These have focused on such issues as the relative contributions of for-profit vs. non-profit developers, how deals are structured by syndicators, the extent of subsidy "layering," and the like.[5] None of these analyses estimate the "crowding out" phenomenon discussed above.

3.2. Previous research on the price elasticity of supply

Evidence on the market effects of housing programs is of two kinds: direct and indirect. Direct evidence stems primarily from the Experimental Housing Allowance Program (EHAP) Supply Experiment. Indirect evidence stems from several research papers on the price elasticity of supply, or the overall responsiveness of the housing market.

The EHAP Supply Experiment (Barnett and Lowry, 1979; Lowry, 1983; Mills and Sullivan, 1981) found that rents in the subject markets rose more or less in line with annual averages for US cities and for subsets of comparative cities. This lack of price responsiveness was attributable to two factors: (1) a fairly low-income elasticity of demand by low income households (implying that fairly large increases in purchasing power from an allowances resulted in only modestly increased spending on housing) and (2) responsiveness of the market on the supply-side through vacancy adjustments (see Rydell, 1982).

There are two empirical literatures providing indirect evidence on the supply elasticity of housing, one from the existing stock and one from new construction. Papers estimated using stock or cross-sectional data include De Leeuw and Ekanem (1971), Rydell (1976), Ozanne and Struyk (1978), and Stover (1986). The basic result of these studies is that in the short run the price elasticity of supply from the existing stock is fairly low, generally much less than one, but in the long run the elasticity of total supply is for all practical purposes determined by the construction elasticity.[6] On the new construction side, the first analysis was by Muth (1960), who found no significant relationship between price and quantity, consistent with perfectly elastic supply.[7] Follain (1979) found qualitatively similar results. Olsen (1987) criticized both Muth's and Follain's studies on the grounds of misspecification but did not undertake corrected estimates in his review article.

Several other papers, which are not usually thought of as "supply elasticity papers," including Huang (1973), Kearl (1979), Poterba (1991), and Topel and Rosen (1988), contain explicit or implicit estimates of such a

[5] Cummings and DiPasquale find that, although non-profit-developed projects are on average about 20% more expensive than privately-developed projects, there has been a clear reduction over time in the returns being expected by equity investors in the projects. The projects are found to be quite costly though, at an average per-unit subsidy of $43,545.

[6] Econometric problems in the estimation of these elasticities bias the results downward, according to the authors of the studies themselves. But the extent of the bias is unknown.

[7] It should be pointed out that Muth limited his investigation to the interwar years 1919–1934.

parameter. Most of these have found or implied low elasticities.[8] Malpezzi and Maclennan (2001) show that the differences between Muth/Follain findings of a highly elastic market and the Poterba/Topel and Rosen findings of a less elastic market can be explained by the fact that cycles in housing prices are very long, and studies that use only 10–20 years of data can be affected by the particular period chosen for study.[9] Malpezzi and Maclennan also present their own estimates of price elasticities of supply from a simple flow model of the housing market, finding it to be relatively high, and even higher in the postwar period.[10]

Finally, we note that all the above studies of supply responsiveness are based on aggregate data. Green et al. (2000) present evidence that the responsiveness of the market also varies from place to place, depending among other things on the regulatory environment and geographic constraints. They find evidence that prices vary with both natural and regulatory restraint, but that over at least part of the range of measured regulation, regulatory constraint dominates.

We are left with the distinct impression that the degree of housing price elasticity is highly variable over both time and geography. It is certainly higher in the long run and begins to reflect the long-term price elasticity of new construction.

3.3. Previous research on crowding out

Perhaps the most relevant literature for our study is that of "crowding out," or the replacement of private units by subsidized units. Swan (1973) and Murray (1983), building on the Foundation of the Fair (1972) model, suggest that additional public housing units built are eventually reflected in a smaller supply of private units, but that the degree of crowding out varies with type of program. Murray makes four methodological contributions

[8] Kearl reported an elasticity of 1.6 for new construction, and Huang, 2.0 for starts. Topel and Rosen's research on starts found a long run elasticity of 3.0 using quarterly data from 1963–1983. Poterba also presented data that seemed to indicate a rising supply price. In general, this set of papers is characterized by models and data that constrain "long run" adjustment to a few quarters or years. Also the particular years chosen were in at least the last two cases periods real housing prices were rising. Had they extended their estimation forwards or backwards in time they would have included declining prices. Put another way, these estimates put lower bounds on the true long run elasticity, but say nothing about how close to the bound the true parameter might be.

[9] The periods chosen by Muth and Follain happened to coincide with periods of flat or declining prices. Both Poterba and Topel and Rosen happened to choose years of rising prices; had they extended their analysis to include earlier or later years their estimated elasticity would have increased.

[10] According to Malpezzi and Maclennan, in the prewar US the price elasticity is between 4 and 10, postwar it is between 6 and 13. In the prewar UK the estimated price elasticity is between 1 and 4, postwar it is between 0 and 1.

beyond those of previous efforts: he (1) handles disequilibium in a longitudinal model, (2) accounts for serial correlation using the recommendations of Granger and Newbold (1974) and "prewhitens" the data, taking first differences, and testing for degrees of higher-order auto-correlation, (3) uses instrumental variables and two-stage least squares to counter simultaneity biases, and (4) imposes several inequality constraints implied by theoretical considerations.[11]

Recently, with more disaggregate data and a longer observation period, Murray (1999) has revisited the crowding-out hypothesis. He finds that public housing and conventionally-financed subsidized housing are not substitutes for each other, while conventionally financed subsidized housing crowds out other housing one-for-one. However, public housing does not exhibit a discernable crowding-out effect. Murray's evidence suggests, in contrast to his earlier results, that in the long run unsubsidized housing and public housing grow together.[12]

Both of Murray's models make use of time-series data; however, Murray's second model, in contrast to his first, makes use of stock, not flow data. Longitudinal models using starts data tend to suffer from specification problems. They are inherently disequilibrium models, and the difficulty in determining appropriate adjustment mechanisms, given the nature of the data available, is challenging.

Sinai and Waldfogel (2002) recognize this difficulty. They adopt a cross-sectional stock equilibrium approach, using HUD data for Census places, providing almost 23,000 observations. Their reduced-form equation is specified using total occupied housing stock per capita as the dependent variable. They regress this against a number of demand shifters, including racial composition of local markets, income and age distribution, median family income, a measure of "pressure" on the public housing stock, and state dummies controlling for unobserved heterogeneity. Their results suggest that government-financed units raise the total number of units in a local market, but that on average three government-subsidized units displace two units that would otherwise have been built by the private market.

[11] Lovell and Prescott (1970) show that in the context of OLS, least-squares estimators that incorporate inequality constraints are maximum-likelihood and provide lower MSE than unconstrained least squares for normally distributed errors.

[12] Murray provides some additional interpretation of the finding that public housing does not substitute fully for unsubsidized housing. He notes that the demand for public housing is not specifically the tenants' demand but society's demand (since society pays the bulk of the bill). "The evidence here indicates that the poor do not respond to society's largess by simply reducing their demand for unsubsidized housing one for one, but rather use the public housing program to reduce their household sizes. Such behavior is by public housing beneficiaries is constant with the common finding (e.g., Bane and Ellwood, 1986) that welfare payments to single parents induce them to form separate households more often than they otherwise would" (p. 117).

Contrary to prior expectations, demand-side programs seem to be more effective than supply-side programs in creating net new additions to the stock. There is no separate evaluation of the LIHTC program.[13]

4. A simple reduced form model of state housing markets

Our model is constructed in the vein of Sinai and Waldfogel. It is cross-sectional and includes controls in the form of a number of demand and supply shifters. It uses state-level data that allows detailed consideration of LIHTC activity, but implies fewer degrees of freedom than Sinai and Waldfogel had with their Census place data. The model we will use is a reduced form of the equilibrium stock demand and supply of housing units, and of Section 42 and other subsidized housing activity.

Consider first the demand for and supply of LIHTC units:

$$L_{\mathrm{D}} = f(Y, N, D, V), \tag{1}$$

$$L_{\mathrm{S}} = f(N, C, R, H), \tag{2}$$

$$L_{\mathrm{S}} = L_0, \tag{3}$$

where L is the number of low-income housing units built. Without loss of generality, we can consider L as a vector of different supply-side programs, including not only LIHTC but also public housing, Section 236, etc., as we will elaborate on below. L_{D} is the demand for LIHTC and other low-income output, and L_{S} is the output offered by suppliers; but supply is constrained by government decision to L_0. Y is a vector of income and poverty measures, D is a demographic vector, V is the supply of demand-side subsidies such as housing vouchers or Section 8 Existing certificates, C is a construction cost vector, R is a measure of the regulatory environment, and H is a vector of housing market conditions (e.g., vacancy rates).

The variable N represents population.[14] Ordinarily population is thought of as a demand-side variable only. It is in our supply equation for two reasons. First, we are estimating *across* markets (states), so we need a scaling factor. Second, N is a determinant of the constraint on L_0, based on the \$1.25(recently \$1.75) per capita limit.

Now consider the demand and supply of units outside the program, denoted Q:

$$Q_{\mathrm{D}} = f(Y, N, D, V; L), \tag{4}$$

[13] Indeed, their 1996 data from HUD includes LIHTC units, but because reporting is not required for the program, information on only 332,000 units relative to some 600,000 allocations is provided.

[14] Subsumed in vector D in the demand-side, but written separately on the supply-side.

$$Q_S = f(C, R, H; L), \tag{5}$$

$$Q_D = Q_S. \tag{6}$$

Here it is reasonable to assume $Q_D = Q_S$, that is, an equilibrium model. The maintained hypothesis of the equilibrium condition $Q_D = Q_S$ bears elaboration. After all, a number of papers such as Fair and Jaffee (1972) emphasize that housing markets are often in disequilibrium and the supply of L is potentially a large exogenous shock. But in our model we estimate relationships among the stock of housing circa 2000. Most low-income supply-side programs, except for the LIHTC, have not produced significant numbers of units for the last decade or longer. Demand-side programs have increased, but rather slowly in recent years. Thus the great majority of the stock of units subsidized under these programs has been around long enough to be incorporated into producers' and consumers' calculus. The LIHTC has been, in relative terms, more active of late. But even in this program, (1) most units were produced several years ago, or earlier; and (2) given the ceilings on production, recent production in recent years has been relatively smooth and predictable. Furthermore, previous studies found that for the entire housing market the equilibrium assumption makes little difference in the results. Both Swan (1973) and Murray (1983) allowed for short run disequilibrium in total housing markets, using a variant of Fair's (1972) model. But they found that it made little difference in the actual results.

Estimable forms of the models can be derived as follows. Write the reduced form for Q, and the reduced form for L. Then substitute the latter into the former. Divide total output Q, supply-side program output L, and demand-side subsidies V by N to scale them, and we obtain the reduced form equation:

$$Q/N = f(Y, D, V, C, R, H; L/N, V/N). \tag{7}$$

Now, as argued above, Q may or may not depend on L (as will be tested). It is also clearly possible that if, despite the arguments made above, L depends partly on the state of the unsubsidized market, L may itself depend on Q. Because of L's possible endogeneity, we will estimate the model using both OLS and the method of instrumental variables.

Many hypotheses could be investigated, including such oft-examined propositions as, for example, $\partial L/\partial N > 0$ and $\partial Q/\partial Y > 0$. But our hypothesis of particular interest is whether Section 42 activity is a net addition to the stock, or whether it merely substitutes for other construction. In our notation, the hypothesis to be tested is

$$\frac{\partial Q}{\partial L} < -1; \text{ equivalently, } \partial Q < -\partial L. \tag{8}$$

5. Data

The basic data source on tax credit activity is the annual report of the National Council of State Housing Agencies (NCSHA, 2002). We obtained NCSHA data on how many LIHTC units were authorized each year from 1987 (effectively the first year of the program) to 2001, for each of the 50 states and the District of Columbia.

Other data were collected from public sources. Housing stock, vacancy rates, demographic data, and poverty rates are from the Census. Per-capita income and population are from the Bureau of Economic Analysis. Other subsidized housing data are from HUD for 1998. We assume that 1998 non-LIHTC subsidized housing stock levels are little changed by 2000.

6. Results: statewise descriptive analysis

Table 2 presents our data on the stock of housing, in total and by program, by state in 2000. The first column is a simple calculation, namely the total stock of housing units (vacant and occupied) for each state divided by its population, multiplied by 1000. This ranges from a low of approximately 359 housing units per 1000 people in California to a high of 513 units per 1000 people in Maine.[15] The second column is a similar calculation but for multifamily stock, after subtracting the subsidized housing stock. Column 3 presents the number of LIHTC units per 1000 population. This column was calculated using the National Council of State Housing Authority's data on each state's approvals per year, assuming approved units were built, and that all units built remain in the stock. Since the first units were built in 1987, this is a reasonable supposition.[16]

[15] Note that we use the measure of total housing units per capita, as contrasted to occupied housing units in Sinai and Waldfogel. We believe total housing units available for occupation is a more appropriate measure for housing supply. Adding controls for the vacancy rate, growth rate of the state, and total state population provides very similar results those from Sinai and Waldfogel's specification, a finding that they also observe.

[16] Given the ceilings, why do we observe statewise differences in LIHTC output? There are two reasons. First, when the program originally was created in 1986, with the first projects allocated in 1987, a few states were quick off the mark; but most states were slow to get started. In 1987, half the states used 15% or less of their first year's application. Only a little over a quarter of the states used more than a fourth of their application. At one extreme, North Dakota, Rhode Island, and Hawaii used none of their allocation in the first year. At the top end Colorado managed to use 96% of its allocation, Nevada 94%, and the District of Columbia 89%. Second, and probably more importantly, construction and development costs vary by state, and the dollar ceilings do not. Low-cost states will be able to produce more units than high cost states, given this constraint.

Table 2
Selected housing indicators of US states

	Total stock per 1000 population	Market constructed multifamily stock per 1000 population	LIHTC stock per 1000 population	Non-LIHTC supply-side program stock per 1000 population	Demand-side program stock per 1000 population	LIHTC share of total permits 1987–2001 (%)
Alabama	429.2	20.8	4.9	15.4	4.6	9
Alaska	396.3	32.2	2.5	15.1	4.3	7
Arizona	460.0	64.3	3.5	7.1	3.3	3
Arkansas	446.3	16.0	5.4	11.8	7.4	11
California	**358.5**	66.0	2.7	5.9	6.3	4
Colorado	452.8	65.2	3.6	8.4	4.4	3
Connecticut	414.3	38.1	2.7	16.7	7.5	5
Delaware	473.2	34.3	6.6	11.6	3.7	6
District of Columbia	498.8	**147.9**	7.0	**51.9**	**11.4**	**71**
Florida	472.7	65.3	4.3	7.3	4.1	3
Georgia	416.0	41.7	5.4	12.3	4.1	4
Hawaii	372.7	66.2	**2.1**	9.8	6.1	3
Idaho	426.3	17.8	4.5	5.3	4.0	5
Illinois	401.0	50.1	3.6	13.3	3.5	6
Indiana	428.0	33.2	4.4	11.0	4.3	5
Iowa	426.1	32.9	5.2	7.6	4.8	10
Kansas	422.5	29.2	6.1	9.7	2.9	9
Kentucky	439.4	23.3	5.3	14.2	5.3	9
Louisiana	416.7	17.7	**8.3**	13.3	5.4	21
Maine	**512.5**	22.9	3.8	12.4	7.6	6
Maryland	415.7	52.0	5.0	12.9	4.4	6
Massachusetts	423.5	45.9	4.1	17.7	6.7	9
Michigan	424.4	33.5	4.7	11.2	2.9	7
Minnesota	430.7	48.3	4.6	13.3	5.3	5
Mississippi	417.7	11.6	7.1	13.3	5.0	15
Missouri	441.8	29.0	4.9	11.0	6.1	8
Montana	465.7	13.9	3.5	16.4	4.5	10
Nebraska	398.3	44.7	4.3	10.3	6.1	7
Nevada	458.4	97.7	3.9	7.3	3.3	**2**
New Hampshire	457.1	41.2	2.5	10.0	5.2	3
New Jersey	393.9	44.6	2.5	12.9	4.9	5
New Mexico	450.4	24.2	5.1	9.2	5.9	7
New York	411.2	85.0	3.1	18.9	7.8	10
North Carolina	466.2	32.1	4.0	10.1	5.2	3
North Dakota	452.7	57.0	5.9	15.2	8.3	11
Ohio	417.6	36.3	5.4	13.5	4.9	9
Oklahoma	439.1	25.0	6.5	13.2	5.4	17
Oregon	442.0	52.0	4.1	6.3	6.8	4
Pennsylvania	435.7	28.0	3.4	12.9	4.1	7
Rhode Island	433.3	25.5	5.0	27.4	7.2	11
South Carolina	454.0	25.3	5.1	10.8	4.3	5

Table 2 (*continued*)

	Total stock per 1000 population	Market constructed multifamily stock per 1000 population	LIHTC stock per 1000 population	Non-LIHTC supply-side program stock per 1000 population	Demand-side program stock per 1000 population	LIHTC share of total permits 1987–2001 (%)
South Dakota	432.0	24.9	6.2	20.8	4.5	9
Tennessee	431.4	31.0	4.3	15.2	4.2	5
Texas	394.4	60.4	6.2	7.5	4.8	10
Utah	358.6	35.6	4.6	**3.8**	3.1	5
Vermont	484.7	18.9	5.7	9.7	7.3	9
Virginia	412.3	45.6	5.1	10.6	3.8	5
Washington	429.8	59.7	3.6	7.5	4.4	4
West Virginia	460.8	**11.4**	4.2	11.2	6.9	17
Wisconsin	433.0	42.1	5.0	10.1	4.1	6
Wyoming	466.6	17.0	4.8	10.5	**2.8**	12
Unweighted average	433.3	40.9	4.6	12.6	5.2	9
Median	431.4	34.3	4.6	11.2	4.8	7
Third Quartile	453.4	51.0	5.3	13.4	6.1	10
First Quartile	416.4	25.0	3.7	9.7	4.1	5
Max	512.5	147.9	8.3	51.9	11.4	71
Min	358.5	11.4	2.1	**3.8**	2.8	2
Coefficient of Variation	0.072	0.595	0.281	0.556	0.322	1.129

Data sources. Census Bureau, National Council of State Housing Authorities, US Department of Housing and Urban Development.

The next two columns present calculations for older HUD-sponsored housing programs. The data are from a data set that was collected by HUD as of 1998, but virtually no additional supply-side units and relatively few vouchers or other demand-side programs were added in the past several years, so the 1998 figures are good approximations for the current stock. Column 4 presents the number of non-tax credit supply side units per 1000 population, incorporating programs such as Indian housing, public housing, Section 8 new construction and moderate rehabilitation, and Section 236 rental assistance. The fifth column is a similar calculation for Section 8 existing certificates and housing vouchers. The final column in Table 3 presents the tax credit program's share of total permits over the entire program period.[17]

[17] Notice that the unweighted average of individual states' shares, at 9 percent, is substantially higher than the overall national figure from Table 1 of approximately 6%. That is largely because there is a tendency for larger states to have smaller shares and these averages are simple unweighted calculations from the 50 states and the District of Columbia.

Table 3
OLS regression model of the determinants of the housing stock dependent variable: housing units per 1000 population

	Coefficient	SE	t-Statistic	Prob. > $\|t\|$	Standardized coefficient
Intercept	63.53	450.28	0.14	0.8886	
LIHTC stock per 1000 population	−1.62	3.76	−0.43	0.6697	−0.067
Non-LIHTC supply-stock per 1000 population	−0.01	1.01	−0.01	0.9938	−0.002
Demand-side stock per 1000 population	0.60	2.71	0.22	0.8249	0.032
Vacancy rate	64.42	147.03	0.44	0.6638	0.066
Percent urban	−85.78	36.85	−2.33	0.0255	−0.417
Percent of population Asian	−114.83	44.57	−2.58	0.0141	−0.315
Percent of population Black	49.05	56.54	0.87	0.3913	0.190
Percent of population 65 or older	819.79	206.12	3.98	0.0003	0.536
Log state per capita income	54.63	46.31	1.18	0.2457	0.280
Poverty rate	110.63	154.99	0.71	0.4799	0.151
State population	−13.74	5.10	−2.69	0.0106	−0.455
Growth in PC income 1980–1990	−475.64	562.17	−0.85	0.4029	−0.132
Growth in population	94.94	39.40	2.41	0.0211	0.350
Adjusted R^2		0.50			
F test: supply-side subsidy programs		0.09			
Prob. > F		0.9117			
F test: all subsidy programs		0.08			
Prob. > F		0.9714			

Notice that there is substantial variation in the housing stock from the low-income tax credit program, ranging from 2.1 units per thousand in Hawaii to a high of 8.3 units per thousand in Louisiana. However, the variance in tax credit units is substantially less than in the other programs, particularly the older traditional supply-side programs. The number of older supply-side units ranges from 3.8 per thousand in Utah to 51.9 per thousand in the District of Columbia. Similarly, there is a large variation in demand-side programs, ranging from 2.8 units per thousand in Wyoming to a high of 11.4 per thousand in DC.

There are correlations between the density of the LIHTC stock, the traditional supply-side program stock, and the demand-side program stock, but the correlations are modest. That tells us that there is probably some underlying common relationship, no doubt due to demand for subsidized housing (related to poverty rates and other factors), but the fact that the correlations are loose tells us that something is affecting the choice of one program over another, a fact we will return to below.

7. Regression results

Table 3 presents our first regression results, using OLS. The dependent variable is the number of housing units per thousand in 2000, that is the first column of data from Table 2, above. The first variables on the right-hand side are the numbers of Section 42 tax credit units per thousand population, traditional supply-side units per thousand population, and demand-side subsidized units per thousand population (columns 3–5 of Table 2, above). The other variables represent various demand and supply shifters and other controls, including the housing market vacancy rate, the percent of the state housing market that is urban (a proxy for cost), the race and age composition of the state, the per capita state income level, the poverty rate, the state population, and the growth in population and income.

The key result for our present purpose is the following: none of our three program-related right-hand side variables are significantly different from zero, nor are they particularly large in magnitude. At the bottom of Table 3 we present two F tests: the first for the joint effects of the two supply-side programs (LIHTC and older traditional supply-side programs), the second a joint test for the null for all three categories, supply, and demand-sides. It is quite clear from the insignificance of these tests that whether one looks at individual program effects or the joint effects of all programs, there is no evidence in this data that housing programs have a long-run effect on the stock of housing in individual states.

Table 3 presents a representative model but we also tested several variants, adding and subtracting reasonable variables Among other variables tested which do not appear here are a cost index we constructed from R.S. Means data, various measures of climate, and additional racial and demographic variables. In none of the models we tested were any of our programmatic variables significant, nor were the joint tests significant. This result also held with more parsimonious specifications, including one that included only our three programmatic variables.[18] Analysis of residuals and diagnostics following Belsley et al. (1980) found little evidence of problems except that the state of Hawaii was especially influential in some respects because of its high Asian population. Estimating the model with and without Hawaii, however, did not change the basic result.

At the end of the day, while we believe this model and similar models estimated but not reported here fail to show that the housing stock is affected by either supply-side or demand-side programs, we must of course emphasize that failing to reject a null does not prove it. We take these results as suggestive rather than as authoritative. This is especially important because

[18] It is well known that we could drive almost any coefficient to insignificance by simply adding enough variables to reduce degrees of freedom sufficiently.

the careful reader may have already noticed that the standard errors of our three policy variables (for LIHTC, supply-side, and demand-side units per 1000 population) are so large that we cannot reject an alternative null hypothesis that the true coefficient is one.[19] Our interpretation of such large standard errors is that with such large "noise" it is difficult to argue that the programs add to the housing stock because errors are so large we can reject either null. On the other hand, we cannot safely claim that a future, more powerful test would be unlikely to present a contrary finding. Future research along the lines suggested by Murray (1999), which would estimate a more complete dynamic stock-flow model with an error correction adjustment mechanism, might give us additional insights into this issue.

Our results differ somewhat from Murray's (1999) longitudinal analysis of aggregate US data, as well as from the recent cross-sectional work using Census places by Sinai and Waldfogel (2002). Both those studies found modest but positive effects of selected housing programs on housing stock, albeit using different units of observation and data sets than our own. We have disaggregated somewhat by state level, and included explicit analysis of the newer low-income housing tax credit program, while Murray analyzed older programs subsidy programs to 1987. Similarly, Sinai and Waldfogel, while going well beyond our effort in terms of geographic detail, were limited in the information they had on the tax credit program. They also considered demand-side and supply-side programs together for most of their results and, in the one instance in which they separately considered these programs,[20] obtained anomalous results suggesting greater crowding out by public housing than by tenant-based assistance.

The above results are from OLS regressions. Earlier we argued that the supply of program units might reasonably be assumed exogenous, given the long history of the traditional supply-side and demand-side programs, and the legislative constraints on the supply of LIHTC units by state. In this section we present results from an alternative model that is a natural alternative specification to the extent the density of provision of LIHTC (or other programmatic) units is dependent upon some of the same conditions that affect the density of housing units overall. Serious bias can result from including a simultaneously-determined variable on the right-hand side of an equation, but including LIHTC activity on the RHS is essential to testing one of our central hypotheses. The solution is to construct an instrumental variable for LIHTC output. That is, in a first-stage regression we regress the LIHTC output against the full set of exogenous variables (independent

[19] We reiterate that these high standard errors are robust with respect to specification, including very parsimonious ones. There is collinearity among our three policy variables, of course, but it is not extreme. The highest R^2 from regressing one policy variable against the other two is 0.37.

[20] See their Table 8.

Table 4
Instrumental variables regression model of the determinants of the housing stock dependent variable: housing units per 1000 population, 2000

	Coefficient	SE	*t*-Statistic	Prob. > $\|t\|$	Standardized coefficient
Intercept	251.85	544.13	0.46	0.6462	
IV for LIHTC stock per 1000 population	4.76	8.72	0.55	0.5886	0.164
IV for non-LIHTC supply-side stock per 1000 population	2.78	2.36	1.18	0.2462	0.573
IV for demand-side stock per 1000 population	2.25	5.22	0.43	0.6688	0.101
Vacancy rate	81.86	142.20	0.58	0.5683	0.084
Percent urban	−132.14	46.57	−2.84	0.0073	−0.642
Percent of population Asian	−56.19	61.23	−0.92	0.3647	−0.154
Percent of population Black	−32.67	90.36	−0.36	0.7198	−0.126
Percent of population 65 or older	634.95	218.28	2.91	0.0061	0.416
Log state per capita income	17.78	54.37	0.33	0.7455	0.091
Poverty rate	−85.12	170.34	−0.5	0.6203	−0.116
State population	−0.85	8.79	−0.1	0.9239	−0.028
Growth in PC income 1980–1990	−681.21	560.39	−1.22	0.2318	−0.189
Growth in population	161.75	55.14	2.93	0.0057	0.597
Adjusted R^2		0.54			
F test: supply-side subsidy programs		0.73			
Prob. > F		0.4874			
F test: all subsidy programs		1.24			
Prob. > F		0.3085			

variables determined outside the model, like population, income, and so on), plus some identifying variables that would affect LIHTC output, but not total housing output. We chose several measures of what we term the "policy environment" of a state: state and local government expenditures per capita, voting patterns in the 1992 election, the maximum dollar AFDC payment circa the early 1990s, and two dummy variables for outliers.[21] It can be shown that the *predicted* LIHTC output from such an equation is correlated with actual LIHTC output but is "purged" of the component that gives rise to the simultaneity problem. Furthermore, we also instrument both non-LIHTC supply-side program output and demand-side program output.

[21] After preliminary regressions were run, it was clear Hawaii and the District of Columbia were outliers and excessively influential, in the Belsley-Kuh-Welsch sense, so these were dummied out. Instrumental variable regression fits were quite reasonable for such cross-section data, namely 0.68 for the LIHTC Units Per 1000 Population variable, 0.85 for the "traditional" Supply-side Units Per 1000 variable, and 0.70 for the Demand-side Units Per 1000 variable.

Table 4 presents the reduced form equation using these instrumental variables. While coefficients change, suggesting there may be some endogeneity, we again find that we cannot reject the null for any or all of our three programmatic variables. We also find, again, that our estimates of the policy variables are extremely noisy. These results are again robust over a variety of specifications.

8. Conclusions

Our simple cross-sectional reduced-form state-level model does a reasonable job of explaining variations in housing unit densities. Urbanization, the age structure of the population and population growth, in particular, explain much of the variation in housing supply. We cannot, however, reject – either individually or jointly – the proposition that demand-side or supply-side (including specifically the LIHTC program) housing subsidy programs substitute one-for-one for the provision of private sector housing units. While inability to reject the null (i.e., the assumption of 100% crowding out) is a weaker form of statistical support than we would like, it is roughly consistent with previous studies (Murray, 1999; and Sinai and Waldfogel, 2002) that found a relatively high rate of substitution.

On the other hand, as we note that our estimates are so noisy that one cannot reject other interesting null hypotheses such as the null that the coefficients are one. Philosophically, when standard errors are so large, we find it hard to interpret the evidence as suggesting net additions to the stock. But we cannot deem it unlikely that future tests would find a contrary result.

We recognize, of course, that even if 100% crowding out by the LIHTC program (and other government housing subsidy programs) were definitively confirmed, this would not necessarily imply the inefficiency of these programs. There are other performance measures besides the "tightness" or "looseness" of the market that must also be considered, including rent/price levels, housing quality levels, and the quantity of housing consumed by tenants in subsidized housing and the rest of the market. In addition, the potential for pecuniary externalities on the benefit side must be examined. Such evaluations await other efforts.

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