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Rent vouchers and the price of low-income housing

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Abstract

Since the early 1980s, low-income housing subsidies have increasingly shifted towards vouchers which allow recipients to rent in the private market. By 1993, vouchers subsidized as many households as lived in traditional housing projects, although most low-income households did not receive any subsidies. This study investigates whether this policy has raised rents for *unsubsidized* poor households, as many analysts predicted when the program was conceived. The main finding is that low-income households in metropolitan areas with more vouchers have experienced faster rent increases than those where vouchers are less abundant. In the 90 biggest metropolitan areas, vouchers have raised rents by 16 percent on average, a large effect consistent with a low supply elasticity in the low quality rental housing market. Considered as a transfer program, this result implies that vouchers have caused a \$8.2 billion increase in the total rent paid by low-income non-recipients, while only providing a subsidy of \$5.8 billion to recipients, resulting in a net loss of \$2.4 billion to low-income households. © 2002 Elsevier Science B.V. All rights reserved.

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1. Introduction

1.1. Overview

Since its origins in 1937, subsidized housing has traditionally consisted of government funded construction known as public housing projects. However, since the Reagan administration, there has been a dramatic shift in the allocation of housing subsidies. New federal dollars no longer subsidize much new construction. In recent years, two-thirds¹ of new subsidized housing units for the poor have been funded by vouchers (also called ‘certificates’), which are used to rent in the private market.² By 1993, over 1.3 million households received vouchers, about the same number as lived in traditional public housing projects.

Recent proposals could dramatically expand the use of vouchers. The Clinton administration put forth plans which would essentially privatize traditional housing projects. All subsidies going to projects would be turned into vouchers, which the tenants would be free to spend elsewhere (Yeager, 1996). Although this proposal has not become law, the Department of Housing and Urban Development (HUD) has begun to demolish housing projects, giving vouchers to the displaced tenants. From 1993 to 1998, the demolition of 76,000 units was authorized, about six percent of the stock (HUD, 1999).

However, there are some reasons for being cautious about privatizing or leveling housing projects. This paper investigates one possible side effect of vouchers: their potential to bid up market rents. The reasoning here is simple. Subsidies to tenants shift the demand curve up, as the subsidized choose more expensive housing. Further, since housing assistance is not an entitlement, but is instead rationed via a waiting list, subsidized renters compete with a large group of income-eligible non-recipients. In fact, about 70 percent of those with incomes low enough to be eligible do not receive vouchers, live in housing projects, or receive any other housing subsidy. These non-recipients will be hurt by vouchers if the increased demand raises market rents.³

The main finding of this study is that the voucher program has already caused a large increase in the price of housing for the poor in the 90 metropolitan areas examined here. The most robust estimate presented here suggests that the voucher program has raised the rent paid by unsubsidized poor households in the average metropolitan area by 16 percent. These are first-difference estimates, which control

¹This figure is for 1990–1996 from the Green Book (1998), and includes newly constructed units funded by programs that mostly serve the poor. It excludes units that are subsidized solely through the Low Income Housing Tax Credit, since LIHTC units are occupied by families that are considerably wealthier than those living in housing projects or receiving rent vouchers.

²There are two major demand-side subsidy programs, called the Section 8 Voucher program and the Section 8 Certificate program. As discussed below, the rules differ somewhat between the two programs. However, this paper will refer generically to both the programs simply as ‘vouchers’.

³Apgar (1990) argues forcefully for supply-side subsidies on these grounds.

for metropolitan area effects which are fixed over long periods of time. Given the size of the program, this is a large effect, consistent with a low supply elasticity in the low-income rental housing market. It also suggests considerable insulation between lower and higher income markets, since the ability to move easily between markets, or substitute towards higher-quality housing, should mitigate the price rise. Consistent with expectations, the first-difference specification implies that vouchers have very little effect on the middle- or upper-income groups. This sensible pattern of results should increase confidence that there are not important variables omitted from the equation, at least not ones that affect lower and higher income housing markets similarly.

An upward sloping supply curve also has the familiar implication that vouchers are not simply a transfer to those who receive them, but also to landlords. Considered as a transfer program, the estimated 16 percent increase in rent implies that vouchers have caused an \$8.2 billion increase in the total rent paid by low-income non-recipients, while only providing a subsidy of \$5.8 billion to recipients, resulting in a net loss of \$2.4 billion to low-income households.

1.2. The Section 8 program

There are actually two programs with different subsidy schemes: Section 8 Vouchers and Section 8 Certificates. The Certificate program, which is far larger, subsidizes rent in privately provided units, requiring tenants to pay no more than 30 percent of their income.⁴ Tenants must rent units that have been approved by local public housing authorities as meeting minimum habitability standards, and that rent for less than the 'Fair Market Rent' (FMR). Fair Market Rent is defined as the 45th percentile of rents in an MSA.⁵ However, the calculation is done only for units occupied by recent movers, that also meet minimum quality standards. So the standard is closer to the median rent, and is sometimes above it (HUD, 1995a). Tenants have no incentive to find units that rent for less than the FMR, since they do not keep the savings.

The main difference between the voucher and certificate programs is that vouchers allow tenants to keep the savings if they rent units that are cheaper than the FMR. Voucher recipients can also choose to rent units that are more expensive. Thus, vouchers are basically a lump-sum income transfer (for tenants of apartments renting for more than 30 percent of the recipient's income). In fact, the programs may not be as different as the rules seem to suggest. A careful study commissioned by HUD found, surprisingly, that the average rent paid by both voucher and certificate recipients was almost the same, and very close to the Fair

⁴In 1989, the mix was 12 percent vouchers and 88 percent certificates (Bartsch, 1990). By 1995, certificates' share had fallen to 77 percent (HUD, 1995b).

⁵FMRs are adjusted for the number of bedrooms. For voucher recipients, household size determines the number of bedrooms that they are entitled to.

Market Rent (Leger and Kennedy, 1990).⁶ It may be that the quality standards, which apply to both programs, are enforced rigorously enough to be binding.

One fact suggesting that quality standards are strictly enforced is that 39 percent of recipients were not able to find a unit that met HUD's standards within the 2 to 4 months allowed for search (Leger and Kennedy, 1990). Since a voucher is large enough to pay for half the units in an MSA, it is pretty surprising that so many people had to return their subsidy checks for the use of another family. This also suggests a slow supply adjustment in low-income rental markets.

This study will treat both vouchers and certificates as if they were simply an order to pay the FMR for an apartment. This is a fairly accurate description of the certificate program, which is by far the larger program. In practice, if not on paper, the voucher program appears to be quite similar. Also, I will continue to use the term 'vouchers' generically, to refer to both programs.

1.3. The size of the voucher program

The demand increase induced by vouchers depends on the size of the population served (relative to the size of the market), the amount of the subsidy received by each household, and the extent to which the subsidy is spent on housing. The amount of the subsidy is easiest to evaluate: housing subsidies are fairly generous, as we might expect from a program that provides welfare recipients and other very low-income households with the median rental housing available. For example, the 1997 Fair Market Rent for a two bedroom apartment in the Oakland MSA was \$794, which is about equal to welfare and food stamps benefits combined.⁷ A 1989 General Accounting Office report describes units available to Section 8 recipients in the Houston area: "Most complexes had swimming pools . . . one had tennis courts, and a few had covered parking for tenant vehicles." Houston was not typical, it was chosen because the GAO thought that the FMRs were likely to be too high, but it may not have been that unusual.

Leger and Kennedy compared the rents paid by voucher recipients before and after they received vouchers. They found that tenants increased their housing expenditure by an average of 59 percent (from \$274 to \$437) after receiving certificates. This figure is a lower bound on the long run increase in rent expenditures, since in the long run all tenants have the opportunity to move (two-thirds of tenants moved immediately).

The before and after comparison will be flawed if recipients' previous housing expenditures reflected a temporary situation. Simple cross-section comparisons,

⁶Certificate rents were tightly clustered around the FMR, while the variance of voucher rents was much higher.

⁷In 1997, the maximum available TANF and food stamps benefits for a three person, single parent family amounted to a total of \$826/month in California (Green Book, 1998).

however, suggest that this is not a severe problem. Cage (1994) studied 1988–1990 Consumer Expenditure Survey data. He found that voucher recipients lived in units that rented for \$527 a month, on average. Income-eligible non-recipients paid only \$337. However, this 56 percent difference is probably understated, since the comparison group (of eligible non-recipients) are generally better off. Voucher recipients have 20 percent less income, for example. Another simple estimate can be calculated by assuming that voucher recipients would have spent 42 percent of their income on rent, as the unsubsidized group does. This suggests that vouchers caused recipients to spend \$527 a month rather than \$270, which is a 95 percent increase.⁸

To qualify for a voucher, households must meet HUD's 'very low income' eligibility standard. Families earning less than 50 percent of the median income in a metropolitan area (MSA) are defined as very low income. However, housing assistance is not an entitlement; instead, vouchers are rationed through a waiting list and a system of preferences.⁹ In 1995, about 1.3 million households received a rent voucher, about the same number as lived in public housing projects. Compared to all 97.7 million U.S. households, this is quite a small number. For this to be a reasonable comparison, however, assumes that all homes are close substitutes, that low-income households can easily switch between owning and renting, or between poorer and wealthier neighborhoods. Compared to the 14.7 million poor households or the 8.7 million poor renter households, vouchers loom somewhat larger.¹⁰ Table 1 shows the distribution of vouchers across the 90 large metropolitan areas studied in this paper. For the median MSA in the data, there are enough vouchers for 11 percent of poor households, for 16 percent of poor renter households, or for 3.6 percent of all renter households. Hence, the voucher program is quite small relative to a broad definition of a housing market, and is of moderate size relative to a narrower definition. Discussed below are some theoretical and empirical reasons to think that housing markets are segmented into higher and lower quality portions, consistent with a narrower definition of the relevant market. For a given stock of vouchers, the smaller is the relevant market, the more likely it is that substantial effects of vouchers on rents will occur.

⁸These figures are unadjusted means. Cage also regresses out-of-pocket rent expenditures on demographics and dwelling characteristics. His results suggest that vouchers raise total rent expenditures by well over 100 percent, but are flawed by the inclusion of endogenous variables such as the number of bedrooms.

⁹Waiting times average about 18 months (Painter, 1996). Priority is given to the homeless, those in substandard housing, and those with rent burdens (rent as a percentage of income) of 50 percent and higher (Nelson and Khadduri, 1992). Another channel through which vouchers can raise rents is by reducing mobility. Potential recipients may be reluctant to move and lose their place on the waiting list. I thank an anonymous referee for this point.

¹⁰These figures are from the *American Housing Survey for the United States in 1995* except for the voucher figure which is from tabulations included in the *Picture of Subsidized Housing* documentation.

Table 1
Vouchers relative to size of MSA housing market, 1995^a

	Representative metropolitan area	Vouchers per:		
		Household in poverty	Renter hhd. in poverty	Renter household
<i>Quantile</i>				
Min	Chattanooga, TN	0.043	0.070	0.018
5	Austin, TX	0.058	0.081	0.021
10	Wichita, KS	0.069	0.103	0.023
25	Kansas City, KS–MO	0.086	0.123	0.028
50	Las Vegas, NV	0.109	0.158	0.036
75	Newark, NJ	0.149	0.202	0.042
90	San Francisco, CA	0.220	0.294	0.051
95	Hartford, CT	0.243	0.316	0.060
Max	Ventura County, CA	0.372	0.556	0.067
<i>Mean</i>		0.124	0.175	0.037

^a Note: the table reports the ratio of vouchers to various measures of the size of the MSA housing market. The representative MSAs have approximately the same fraction of vouchers per poor households as the indicated quantile. Since the quantiles are rounded (e.g., the median is the average of the 45th and 46th MSA), quantiles may not exactly match the actual MSA figures. The sample size is 90 MSAs.

1.4. Literature review

Most of what we know today about low-income housing markets is a result of the housing allowance experiments, which generated a large, but now somewhat dated, literature in the 1970s. One of these studies, the Housing Assistance Supply Experiment, is often cited as finding little effect on market rents. HASE, conducted from 1975 to 1980, was something of a trial run for the current Section 8 voucher program. In two small Midwestern cities, all residents who met income standards were eligible for voucher-based subsidies.

HASE analysts reported that rents in the two experimental sites increased by about the same amount as rents nationwide, and as landlord's costs (Lowry, 1983; Rydell et al., 1982). However, the Housing Allowance experiments only raised recipients' total rental housing expenditures (i.e. expenditures including the subsidy) by about 8 percent (Lowry, 1983, p. 154), probably because the subsidies were very close to being lump-sum transfers, with minimal habitability standards. Most recipients simply remained in place, in sharp contrast to the Section 8 voucher program, where most recipients move immediately, and report large increases in total housing expenditures. Since HASE did little to increase demand, it is not surprising that rents were not bid up, and the experiment ultimately does not tell us much about the elasticity of supply. Also, the experiment was not well designed to capture small effects. Only two cities were studied, and there was no

natural comparison group of cities. Finally, analysts were mainly concerned with effects on the whole local housing market, not merely the low-income submarket.¹¹

The housing allowance experiments also funded the development of an elaborate housing market model by the Urban Institute (de Leeuw and Struyk, 1975). de Leeuw and Struyk used the model to simulate a full scale voucher program that serves many times more people than does the actual program, but that induced much smaller increases in demand for each household. They warned that housing prices could increase by 40 percent in the worst case scenario.¹²

Vouchers will drive up rents if they fail to stimulate a supply response: inducing construction, reducing demolition, or increasing maintenance. It has not generally been recognized that without a supply response, housing subsidies cannot improve the housing conditions of the poor.¹³ If the effect on supply is small, vouchers will mainly redistribute the stock of housing from one group to another. In the extreme case, where the stock of housing is fixed, voucher recipients will trade places with the unsubsidized, and there will be no net benefit from the program. In fact, the results presented below suggest that the elasticity of supply is very close to zero, that vouchers do very little to increase the size or quality of the low-income housing stock.

There has been a fair amount of study of the supply of newly constructed housing (see DiPasquale, 1999, for a review). However, there has been very little examination of the housing supply mechanisms that are probably most relevant to the market served by vouchers, such as demolition or the maintenance of rental housing. Rydell (1982), the more recent of the two studies of maintenance cited in DiPasquale's survey, found that the elasticity of repair expenditures with respect to rent is quite low (about 0.2).¹⁴

Because little is known about the supply response to vouchers, little can be said about their cost-effectiveness. Vouchers have lower budgetary costs, and are often promoted as a cheaper solution than construction subsidies.¹⁵ Knowing that a voucher costs the government, say, \$400 a month while subsidizing construction costs \$600 a month tells nothing about how many new (or better) units are supplied by the subsidies. Although vouchers may induce landlords to maintain their buildings better, and stave off demolition, it is quite possible that construction subsidies, which target the marginal unit of the housing stock, are a more efficient way of supplying new housing. Ultimately, the magnitude of the supply response

¹¹Rosen (1985) offers a similar analysis.

¹²Barnett (1979) discusses a number of similar predictions.

¹³An exception is Galster (1997), who argues for vouchers on the grounds that they induce a supply response. Although such a response is possible, it has not been documented.

¹⁴But see the strong criticisms of Rydell in Olsen (1987). O'Flaherty (1996) examined demolitions, and Weicher and Thibodeau (1988) studied the stock of substandard housing, but neither directly examine the effect of demand or prices on a measure of the quantity of existing housing.

¹⁵Building a new unit costs 58 percent more than a voucher on an annualized basis according to Aggar (1990). Such costs figure prominently in policy debates. See, for example, Burman (1992).

in low-income housing markets is an empirical issue, which this study will examine. Construction subsidies may increase the size of the housing stock, or they may simply crowd out private construction.¹⁶ By the same token, vouchers may induce new construction, or they may simply bid up rents and redistribute the existing housing stock.

1.5. Effect on neighborhood quality

Galster et al. (1999) studied the effect of vouchers on the price of owner-occupied homes in the Baltimore suburbs. They compared prices of homes within 500, 1000, and 2000 feet of voucher households to homes outside these rings, controlling for census tract fixed effects and a few structural characteristics of the home. This method is well-suited to identifying effects on neighborhood quality, such as improved facades, undesirable behaviors of voucher recipients, or neighbors' prejudices about recipients. These effects are likely to be highly localized, since by moving a few blocks away households lose the benefits or escape the costs of voucher units. In contrast, the pure price effects that are the subject of the present study are likely to be felt throughout the housing market. Voucher recipients cannot bid up prices in a 500 foot ring: if homes outside the ring are close substitutes, people will simply move there until prices are equalized.

Although Galster et al. studied a quite different question than that addressed here, their results are still of some relevance. A potential difficulty with the present study is that data limitations make it difficult to distinguish between the effect of vouchers on prices and the effect on neighborhood quality. However, the Galster et al. results suggest that, in practice, this is not a terribly important problem. Galster et al. found that vouchers raise prices by 3 percent within a 500 foot ring of a recipient, and lower them by about 1 percent in the two outer rings ($t = 3.1, 1.7,$ and $1.6,$ respectively). Since the outer ring has three-fourths of the total area, an area-weighted average of these coefficients suggests that house prices fall by 1 percent within 2000 feet of the unit after a voucher recipient moves in. The total effect would be smaller if the areas outside the rings are included (assuming the effect is zero there).¹⁷ In any event, the Galster et al. results suggest that whether the effect of vouchers on neighborhood quality is positive or negative, it is quite small relative to the effect of vouchers on rents found in this study.¹⁸

¹⁶Murray (1999) finds that low-income public housing has not crowded out private construction, but that moderate-income subsidies have.

¹⁷Galster et al. do not report how much of the Baltimore suburbs fall within a 2000 foot ring.

¹⁸Galster et al. use their rich set of results to argue that vouchers improve the quality of neighborhoods in some circumstances and lower it in others.

1.6. Filtering models

Although empirical research on low-income housing markets is fairly sparse, there is an extensive literature on ‘filtering’ models, which provides a useful way of approaching the issue.¹⁹ Filtering models assume a range of housing qualities, with the poorer consumers occupying the lower quality housing. The production function for houses exhibits diminishing returns to quality, but constant returns to scale. Once built, houses deteriorate unless maintained. If construction costs rise more rapidly with quality than do maintenance costs, then above a certain point homes will be built and maintained forever. Since the production function has constant returns to scale, the supply of higher quality housing is infinitely elastic. At lower levels of quality, however, it is cheaper to build medium quality homes for middle-income people and allow them to deteriorate until they are affordable by the poor (call this the ‘build-and-deteriorate’ level of quality). Thus a crucial determinant of the supply of low-income housing is the ratio of middle-income people to the poor. More middle-income people in a city means more new housing, housing that will eventually ‘filter down’ to the poor.

In a filtering model, there are three cases where vouchers can end up raising the price of low-income housing. First, vouchers can move people from very low in the quality distribution to some point near the middle, but below the new construction level. This is the case that most closely corresponds to an increase in demand for low-income housing. Second, vouchers could induce doubled-up households to separate.²⁰ If the number of poor households increases, the price of low-income housing should rise (since new construction only responds to an increase in the middle class). Third, since by law vouchers can only be used in well-maintained units, they could interfere with the filtering process. Vouchers could remove some apartments from the filtering chain, causing them to be maintained forever at higher qualities.

Vouchers could also lower the price of low-income housing. If vouchers are generous enough, they move voucher recipients to the high-income, build-and-maintain-forever interval. The price of housing for the rich will not change, but the price of low-quality housing will fall since there are now effectively fewer poor people for each middle-income person. Slightly less generous vouchers could move recipients to new construction at the build-and-deteriorate quality. This will lower rent for the poor for the same reason, and for an additional one: they will increase the amount of new housing being built that will filter down.

¹⁹The filtering model was first developed by Sweeney (1974a,b). This section closely follows the analysis in O’Flaherty (1995).

²⁰One-third of voucher recipients were doubled-up before receiving a voucher (Leger and Kennedy, 1990).

1.7. Neighborhoods of voucher recipients

As discussed in more detail below, this study will operationalize filtering models by splitting the rental housing market into high-, middle-, and low-income segments ('treciles'), and examining the effect of vouchers on rents in each of the three markets. Since the amount that a voucher is worth (the FMR) is set around the median rent, a natural assumption is that voucher recipients move into the middle-trecile market. If so, they will have the largest effect on rents in this market, and, as filtering models suggest, could end up lowering rents in the low-income market. On the other hand, if there is a stigma attached to vouchers, or if voucher recipients are considered undesirable tenants, then voucher recipients may find themselves trapped in lower income neighborhoods, and may bid up rents there.

In fact, tabulations from the American Housing Survey Neighbor file suggest that the typical voucher recipient lives in the bottom third of the rental housing market. For a subsample of AHS respondents, the AHS questionnaire is administered to up to 10 closest neighbors (on average, about eight households are interviewed per cluster). This data can be used as a measure of the type of neighborhoods voucher recipients live in. Thirty percent of voucher recipients have no private-market neighbors at all, living in 'Section 8 Ghettos.' After excluding subsidized households, recipients' private-market neighbors have an average income of \$24,215, which is quite close to the \$24,303 average incomes in neighborhoods where lower-trecile renters live. The neighbors of middle-trecile private market renters have an average income of \$30,527. Private-market neighbors of voucher recipients pay \$467 a month in rent, which is a fairly close to the \$442 average rent in lower-trecile neighborhoods, and below the \$511 average in middle-trecile neighborhoods. Voucher recipients themselves pay \$512. A natural interpretation of these results is that stigma confines voucher recipients to low-income neighborhoods, but vouchers are generous enough to allow them to rent high-quality (perhaps spacious) housing in those neighborhoods. These estimates are likely to be conservative, to overstate the average neighborhood quality of recipients, since some of the lowest income voucher recipients (with no private-market neighbors) are excluded from the sample. There is some evidence, then, that we live in a world where the voucher program is likely to bid up rent for the unsubsidized poor.

2. Theoretical model

The model assumes that each market has a supply and demand curve for housing services, with constant elasticities. Formally:

$$\ln Q_j^D = X_j - \varepsilon_j^D \ln P_j + \eta_j^D \text{ constant elasticity housing demand,} \quad (1)$$

$$\ln Q_j^S = Z_j + \varepsilon_j^S \ln P_j + \eta_j^S \text{ constant elasticity housing supply.} \quad (2)$$

Here markets are subscripted by j , and should be thought of as an MSA/income class combination, such as the low-income housing market in the Oakland MSA. Q_j^D and Q_j^S are the quantity of housing demanded and supplied in a market; P_j is the price; Z_j is an index of supply shifters; and X_j is an index of demand shifters. The elasticity of demand is denoted ε_j^D , and the elasticity of supply is ε_j^S . The error terms, assumed to be uncorrelated with X_j and Z_j , are denoted η_j^D and η_j^S . In all the equations below, the housing market subscripts will be suppressed.

In the long run, $Q^D = Q^S$, so the equations can be solved to generate the reduced form:

$$\ln P^* = \frac{X - Z}{\varepsilon^S + \varepsilon^D} + \eta^P, \quad (3)$$

where the error term (η^P) is a combination of η^S , η^D , ε^S , and ε^D . With some modifications, Eq. (3) will be the equation estimated in this paper.

Now we need to determine the appropriate measure of voucher-induced demand to be included in X . To derive this, consider the demand curve for an unsubsidized family:

$$\ln q_i = x_i - \varepsilon^D \ln P,$$

where i indexes families or households.

In a market without vouchers, market demand equals the sum of the individual demands:

$$Q^D = \sum_{i \in N} q_i,$$

where N indicates the number of households in a market (and the analogous set). In a market with vouchers, market demand can be written as the sum of the demand without vouchers plus the change due to vouchers:

$$Q^D = \sum_{i \in N} q_i + \sum_{i \in N^V} (q_i^v - q_i^b),$$

where N^V indicates the number of (set of) subsidized households (i.e. with a voucher). The quantity demanded by a voucher recipient is denoted q_i^v , while the quantity demanded by a voucher recipient before receiving the subsidy is denoted q_i^b . Taking logs and making a first-order Taylor series approximation about the first term yields

$$\ln Q^D = \ln(Nq) + \frac{N^V}{N} \frac{q^v - q^b}{q},$$

where the suppressed index indicates the average; for example, q indicates the average of q_i . Take $(q^v - q^b)/q$, the subsidy generosity, to be a constant, denoted θ .

For example, if voucher recipients spend \$200 a month before receiving a voucher and \$400 after, while the average tenant in the market spends \$300, then $\theta = 2/3$ and a voucher will induce demand equal to an extra $2/3$ of an average person. This is a substantive assumption, discussed below. Substituting θ , and rearranging, yields the extended demand equation:

$$\ln Q^D = \ln q + \ln N + \theta \frac{N^v}{N} = X - \varepsilon^D \ln P + \ln N + \theta \frac{N^v}{N}, \quad (4)$$

where $X = (1/N)\sum_{i \in N} \exp(x_i)$. That is, X is the average of some demand shifter, for example, average family income. Eq. (4) shows that the appropriate measure of the demand induced by vouchers is the fraction of households with a voucher, N^v/N , multiplied by the generosity of each voucher.

Combining this with supply Eq. (2) yields the reduced form:

$$\ln P^* = \frac{1}{\varepsilon^S + \varepsilon^D} [X + \ln N + \theta V - Z], \quad (5)$$

where $V = N^v/N$. This is the equation estimated in the paper, and so we have an interpretation of the estimated voucher coefficient. The regression coefficient on V will be $\theta/(\varepsilon^S + \varepsilon^D)$. Vouchers will cause a large increase in price when the subsidy generosity is high, and when the supply and demand curves are inelastic.

The main assumption here is that vouchers do not change the demand elasticity. In fact, assuming that θ is a constant is equivalent to assuming that vouchers act by shifting the intercept, but not twisting the demand curve. This is not as strong an assumption as it seems. Recall that voucher recipients' housing expenditures are determined by the Fair Market Rent (FMR), which in turn is simply set to allow recipients to buy approximately the median quality housing. This can be written $q^v = q^m$, where q^m is the housing demanded by a median household. Then, $\ln q^v = x^m - \varepsilon^D \ln P^m$, which shows that HUD's process for determining the FMR forces voucher recipients to spend as if they were the median household.²¹

To see this, consider two MSAs, one with rent in the median market 10 percent higher than the other. If the demand elasticity is, say, 0.6, then the unsubsidized will consume 6 percent less housing in the more expensive city. HUD observes the median rent and adjusts the FMR so that the subsidized households also consume

²¹The result that θ is a constant depends on two assumptions. First, assume that the ratio of the voucher recipients' housing expenditures to the housing expenditures of the average person is a constant. Write this as $q = k_1 q^b$, for some constant k_1 . Further, assume that $q^v = k_2 q^b$, for some constant k_2 . This says that vouchers shift only the intercept of the demand curve, but do not change the elasticity. To see this, take logs, yielding $\ln q^v = \ln k_2 + \ln q^b = \ln k_2 + x^b - \varepsilon^D \ln P$. These two assumptions imply

$$\frac{q^v - q^b}{q} = \frac{k_2 - 1}{k_1},$$

which is a constant.

6 percent less housing. That is, HUD raises the FMR, but not by enough to fully offset the price increase. Thus, by setting the FMR based on the housing consumption of the unsubsidized, voucher households are forced to behave as if they had the same demand elasticity.

In general, voucher recipients may not face the median price, since they may well end up renting in the low-income market. However, if the low- and middle-income prices move together, then the result that the demand elasticity is unchanged still holds. Some price shocks are market-wide (changes in construction costs may be an example), but this assumption should be regarded as an approximation.²²

Although the model to be estimated is a reduced form, the voucher coefficient has a direct interpretation that suffices to answer the main question. It reveals how much a percentage point increase in the voucher stock increases the price of housing. In addition, if some of the parameters are assumed to be known from past research, then the supply elasticity can be backed out. This is of interest in itself, and provides a useful check on the results.

3. Data and variable construction

The model can be estimated in the following manner. First, using data on individual households, estimate a hedonic regression in order to obtain the price of low-income housing in each MSA. That is, regress (log) rent on dwelling

²²To see when subsidy rules which provide voucher recipients with the median quality housing will result in an unchanged demand elasticity, consider three cases. First, recall that the demand curve for an unsubsidized household (such as a voucher recipient before receiving a voucher) is $\ln q^b = x^b - \varepsilon^D \ln P$. A voucher recipient will consume the median quality housing, so $q^v = q^m$. This implies that $\ln q^v = x^m - \varepsilon^D \ln P^m$. If voucher recipients were in the median market before receiving a subsidy, then $P^m = P$, and the demand elasticity will be unchanged.

Now suppose that voucher recipients were in another market (presumably a lower one), but prices in the two markets move together, so that $P^m = \gamma P$. Then $\ln q^v = x^m - \varepsilon^D \ln P + \varepsilon^D \ln \gamma$. Again the demand elasticity is unchanged, since $d \ln q^v / d \ln P = -\varepsilon^D$. Here, we also have to assume that unsubsidized households in both markets have the same demand elasticity.

An alternative possibility is prices in the low- and middle-income markets do not move together, so that HUD does not adjust the Fair Market Rent in response to a price shock to the low-income market. In this case, the demand elasticity may actually become larger than that of unsubsidized households. If the FMR is fixed, then voucher recipients will consume $q^v = \text{FMR}/P$, which implies that $\ln q^v = \ln \text{FMR} - \ln P$, which is simply a demand curve with an elasticity of one. However, I do not make this assumption for several reasons. First, past research suggests that low-income renters have demand elasticities less than one (Hanushek and Quigley, 1980). Thus, assuming that voucher recipients have unit demand elasticities amounts to assuming that vouchers raise the demand elasticity, which seems implausible. Instead, it seems more likely that vouchers may lower the demand elasticity (perhaps because recipients searching for housing are limited to units that accept vouchers). In addition, allowing voucher recipients to have a different demand elasticity would lead to a much less tractable model.

characteristics (e.g., the number of bedrooms) and a set of dummy variables for each metropolitan area/income group interaction. The coefficient on, for example, the Oakland/low-income dummy will be interpreted as ‘the price of housing for the poor in Oakland,’ and is labeled $\ln P^*$ in Eq. (5). Now take the prices and regress them on population, population growth, median income, and the percent subsidized in each MSA.

No supply shifters enter the model because in practice it is very hard to identify ones which are reasonably well measured. Construction costs are an obvious possibility. However, a large component of construction costs are wages, which affect both demand and supply: high wages attract migrants, which is demand. Most studies of housing supply have found no effect of costs on the volume of construction, probably because costs are measured so poorly (DiPasquale, 1999). Somerville (1999) has documented numerous deficiencies in the commonly used Boeckh and RS Means construction cost indexes.²³ Excluding supply shifters will result in bias if construction costs, for example, are correlated with V . However, there is no obvious reason why voucher allocations should depend on construction costs, or vice versa.

3.1. Data

The main dataset is the American Housing Survey (AHS) national file, which will be used to estimate the price of low-, middle-, and high-income housing in each MSA. Conducted every 2 years, the AHS contains detailed information on dwelling units and their occupants. Crucially, it includes information on subsidy status. The sample used for the hedonic regression consists of unsubsidized renter households only, since the goal is to estimate the effect of vouchers on the private-market rent for the poor. Since subsidized households, such as residents of housing projects, are insulated from market forces, they are not included in the analysis.²⁴ In addition, observations with imputed rent, or who paid rent only once or twice a year were dropped.²⁵

The year 1993 is chosen because few new vouchers have been issued since then, making any effects harder to detect. The 1993 AHS contains usable observations on 6526 households occupying rental housing in 108 MSAs. The main drawback

²³For example, both indexes rely on union wage rates to measure construction wages, even though residential construction is predominantly non-union.

²⁴Arguably, voucher households should be included in the analysis, since they are presumably renting in the private market. This is done in the sensitivity analysis, but not in the body of the paper.

²⁵Most households that reported paying rent once a year in 1993 paid extremely low rents. When I examined rents for the same units in 1991, I found that most of the households had reported paying reasonable rents, on a monthly basis. Typically, the 1991 rent was about 12 times as high as the 1993 rent. For example, a household might have reported paying \$400 a month in 1991, but \$400 a year for the same unit in 1993. Very likely, the 1993 data is a result of a data entry clerk entering a ‘1,’ meaning ‘once a year,’ rather than ‘12,’ meaning ‘12 times a year’.

of this dataset is that the number of observations in each MSA is not large, with the median MSA having 33 observations.²⁶ This does not bias the results, and weighted least squares will insure the small MSAs get the appropriate amount of weight. However, these sample sizes limit the precision of the estimates. In addition, 1974 Annual Housing Survey data is used to calculate similar price variables in the year that the voucher program began.²⁷ Finally, rents in the AHS are topcoded at \$1000 in 1993, at approximately the 95th percentile of my sample. Based on the distribution of 1974 rents, which are not topcoded, I recoded the 1993 rents from \$1000 to \$1147.²⁸

The AHS also contains a ‘neighbor sample,’ which is used in certain auxiliary results. For a subset of AHS sample members, the AHS questionnaire was administered to the 10 closest neighboring households. There are usable clusters of 608 neighbors in the 1993 AHS data, containing a total of 4396 observations (including both renters and owners). If the neighbor sample were big enough, it could be used to directly estimate a hedonic equation that includes dummy variables indicating the average income of each sample member’s neighbors. The neighbor sample is too small and clustered to allow this. However, the neighbor sample can be used to develop an index that relates the income of households to the average incomes of their neighborhoods. As discussed below, this index plays an important role in the hedonic estimates of housing prices. In addition, it can be used to examine the type of neighborhoods that voucher recipients live in.²⁹

MSA characteristics, such as median family income and population, are needed in the second stage regressions. These come from various years of the decennial census. The percent subsidized is calculated using the HUD administrative data released in the Picture of Subsidized Housing dataset. Vouchers are mostly administered by several thousand local public housing authorities (PHAs), which typically cover a city (e.g., the Berkeley PHA). Counting vouchers in each MSA required the creation of a PHA to MSA concordance, a time-consuming task which took many weeks. Some vouchers are administered by state agencies. Fortunately, the PSH dataset also contains detailed information on the location of state agency

²⁶MSAs with fewer than 10 observations are excluded from the sample, and the maximum is 670.

²⁷The Annual Housing Survey was renamed the American Housing Survey in 1985, and a new panel of housing units was drawn. The sample size used in the 1974 hedonic regressions is 8476.

²⁸This figure (1147) is an estimate of $E(\text{Rent} | \text{Rent} \geq 1000)$. This was calculated by regressing the 94 non-topcoded centiles of 1993 log rents on the lower 94 centiles of 1974 log rents and a constant. The regression procedure relates mean and variance of the two distributions as $E(R_{93}) = a + bE(R_{74})$ and $V(R_{93}) = b^2V(R_{74})$. Then I extended the estimated regression line and calculated the expected value of topcoded rents as $E(R_{93} | R_{93} > R_{93}^{(94ile)}) = a + bE(R_{74} | R_{74} > R_{74}^{(94ile)})$. Graphing the quantiles of two distributions is a graphical method of assessing their equality or relationship (Wilks and Gnanadesikan, 1968; Gerson, 1975). The first 94 centiles of the two distributions appeared to be well-described by a linear relationship (the R^2 of the regression is 0.994). In addition, the sensitivity analysis, below, shows that alternative corrections for topcoding make little difference.

²⁹I restrict the AHS neighbor sample to the set of MSAs used in the national sample.

voucher households, and so a complete count of vouchers in each MSA can be constructed.³⁰

3.2. Hedonic regression

As in filtering models, think of housing markets as being split into different quality levels, occupied by different income groups. Thus a patchwork of low-income neighborhoods spread out across an MSA may form a single housing market. In a recent study, Goetzmann and Spiegel (1997) provide some important empirical justification for this concept. Goetzmann and Spiegel studied the pattern of housing price movements among different neighborhoods (defined by zip codes) in a large metropolitan area. They found that housing prices in neighborhoods with similar income levels (or similar demographics) tend to move together. This result holds even for widely separated neighborhoods. In fact, they found very little spatial correlation in price movements. Demand or supply shocks in one poor neighborhood seem to affect the price of housing for the poor in the whole MSA. The model estimated here operationalizes this conception of a housing market by measuring the rents paid by low-, middle-, and high-income people in each metropolitan area.³¹

Ideally, average neighborhood income would be used to split metropolitan areas into income groups. Since this variable is not available in the National AHS file, individual characteristics are used to impute neighborhood income. Specifically, the sample is classified using an index based on household income as a percent of the poverty level, education, and indicators for the receipt of welfare or capital income. The AHS Neighbor Sample (which is too small and clustered to use for the analysis of rents) is used to construct the weights for each element of this index by regressing neighborhood income on the individual characteristics listed in the bottom panel of Table 2. These weights are then applied to the AHS National Sample used in the main analysis.³² Table 2 shows the characteristics of the AHS national sample when split into terciles using the index, labeled ‘Terciles of imputed neighborhood income.’ Rent sharply increases with income, from \$443 in

³⁰An exception is the state of Michigan, where many vouchers are administered by state agencies, but the PSH lacks geographic information on most of these vouchers. For this reason, no Michigan MSAs are used in this study.

³¹There is also a long tradition of splitting up the housing market into quality tiers, using various procedures. Some authors have used a single hedonic equation to calculate the predicted price of two or three housing bundles (Poterba, 1991; Thibodeau, 1989, 1995; Gyourko and Linneman, 1993). Others have examined housing price quantiles (Mayer, 1993; Case and Shiller, 1994), or some combination of the two (Pollakowski et al., 1991; de Leeuw and Ekanem, 1971; Rothenberg et al., 1991). Jencks (1994) has estimated hedonic price indexes for low-income households.

³²Another advantage of this procedure is that it captures the notion of permanent income (upon which housing decisions depend) by including measures such as education which change little from year to year.

Table 2
 Renter household characteristics, by tercile and reciprocity status^a

	Treciles of imputed neighborhood income			Voucher recipients
	Lower	Middle	Upper	
Age of hhd head	41.2	39.6	40.2	46.4
White	0.631	0.722	0.834	0.526
Black	0.259	0.188	0.096	0.398
Other	0.109	0.091	0.070	0.076
Hispanic	0.304	0.128	0.057	0.167
Married	0.356	0.312	0.294	0.134
Single female	0.440	0.374	0.329	0.729
Rent	443	521	637	537
Household income	14,543	24,821	50,475	9,579
HHd income as % of poverty level	121	244	529	93
Education	10.5	13.4	15.3	11.2
Interest, dividend or rental income	0.089	0.355	0.687	0.103
AFDC, SSI or other welfare income	0.246	0.006	0.001	0.471
Assets >\$25K and income <\$25K	0.001	0.012	0.036	0.006
Sample size	2175	2176	2175	329

^a Source: author's tabulations, 1993 American Housing Survey National File. Note: imputed Neighborhood Income terciles are based on an index computed from household income as a fraction of the poverty line, education, and the receipt of welfare or capital income. See text. Subsidized households are included only in the last column.

the lowest tercile to \$637 in the highest. As expected, the lowest tercile is also much poorer than the highest with an average income of 121 percent of the poverty line, compared to 529 percent in the highest level. A quarter of the lowest tercile receive some sort of welfare payments.

Rents will be adjusted using hedonic regression techniques, which control for a long list of housing characteristics, such as the number of bedrooms, and 108 × 3 MSA/neighborhood income dummies. Except for these dummies, the specification follows (with some slight modification) Thibodeau (1995).³³ Formally, the model can be written

$$\ln R_{ij} = M_j' P_j + X_{ij}' \beta, \quad (6)$$

³³ Complete regression results are available from the author upon request. I added extra categories (i.e. dummy variables) to two measures: bathrooms (0 bathrooms added) and respondents' opinion of neighborhood ('no neighborhood' added). I also changed the built '1940 or before' dummy to 'built 1920 or before'.

where R_{ij} indicates rent for unit i in market j , M_j is a vector of indicators for each housing market (i.e. each MSA/trecile), P_j is a coefficient vector of the prices in each market, X_{ij} is a vector of housing characteristics, and β is a vector of coefficients.³⁴ The coefficients on the MSA/income dummies (the P_j s) will be interpreted as estimates of the price of low- (middle-, and high-) income housing services in each MSA.³⁵

The AHS data used for the hedonic regression contains a long list of dwelling characteristics (ranging from the number of bedrooms and the frequency of equipment breakdowns), race, two contract conditions (crowding and length of tenure), and a short list of neighborhood characteristics. The neighborhood characteristics are very limited, including only a few opinion questions,³⁶ and one objective measure: the presence of nearby abandoned buildings. The opinion questions do not matter much. None are statistically significant at the 5 percent level and the most precisely measured one (about crime) lowers rent by only 2.4 percent (with a t -statistic of 1.94). The presence of abandoned buildings does lower rent by 12 percent, however (with a t -statistic of 6.95).

The income trecile dummies will proxy for any housing quality variables that are omitted from the regression (and that are normal goods). Conspicuously absent from the list of explanatory variables are many important locational characteristics such as educational quality, crime rates, access to transportation and employment, and local amenities such as parks. Other potentially relevant variables are omitted from the equation as well. Although a number of measures of dwelling size are included, square feet is not. Although dummies indicate whether the dwelling is detached, or part of an apartment complex, there is no specific information which floor a unit is on. The hypothesis being tested, that vouchers bid up the ‘price of low-income housing,’ is equivalent to testing whether vouchers reduce the discount that tenants receive for living in a low-quality neighborhood.³⁷

This procedure constrains the characteristic prices (β) to be the same in all neighborhoods, using a shift in the intercept to model the price of housing in a different market. An alternative method relaxes this constraint and estimates

³⁴Quigley (1995), among many others, uses a similar setup (interpreting time dummy variable coefficients as the price of housing at a particular time). An alternate interpretation, which changes nothing of substance, would call the P_j s, ‘The price of neighborhood amenity service flows’.

³⁵A desirable property of these estimates is that the top trecile rent should be higher than the bottom trecile. To check this, the difference between upper and lower trecile rent was calculated for each MSA. In the median MSA, the upper trecile rent is 24 percent higher than the lower. There is some noise in the data: in about 10 percent of the MSAs the bottom trecile rent is higher than the top.

³⁶These are: (1) overall quality of neighborhood on a 1–10 scale, and (2) whether crime or street noise is so bad that the respondent would like to move.

³⁷Alternatively, the test is whether vouchers reduce the discount for living in a unit with low unmeasured quality, such as a basement apartment.

separate regressions for each market. Several authors have compared the two methods, and found that the less constrained method does little to improve the resulting price indexes. Schnare and Struyk (1976), using data for a metropolitan area, estimated a hedonic regression that includes only dwelling characteristics, a single location dummy variable, and average neighborhood income (intended to proxy for neighborhood service flows). They compared this model to a procedure that estimated separate regressions for each of several neighborhoods (for example, the low-income tracts in the outer suburbs). They found that price indexes calculated from the unconstrained model had only a marginally tighter fit, and endorsed the single-regression model. Rothenberg et al. (1991) estimated a hedonic price index with national data using dwelling characteristics and MSA dummies. They found that estimating separate models for each MSA did not lead to a statistically significant improvement in fit (i.e. in the price indexes). Thus, a pooled regression appears to be an adequate model.

4. Empirical results

4.1. Identification

Simply regressing price on vouchers will lead to biased estimates if HUD targets high-rent MSAs when allocating vouchers. To the extent that vouchers are sent to ‘needy’ areas, identifying the causal effect of vouchers on rents is difficult: do vouchers raise rents or do high rents lead to voucher allocations? Fortunately, a good deal is known about the methods HUD used to allocate vouchers.³⁸

A substantial portion of vouchers are allocated using a formula. For each HUD ‘allocation area’ in the country (usually an MSA), the formula determines what percentage of new vouchers will go to that area. For example, the Oakland MSA is an allocation area which receives 1.2 percent of all vouchers allocated by formula in any given year. In recent years, vouchers have come with a 5 year funding commitment from HUD. However, expiring vouchers have always been renewed, so it is reasonable to assume that once a voucher is handed out, it is handed out forever. The voucher allocation formula is based on 1980 census data, even as late as 1993.³⁹ This census data is used to calculate a weighted average of six factors

³⁸One recent analysis of the voucher allocation across metropolitan areas finds that vouchers are not well targeted to need. Kingsley and Tatian (1999) concluded that idiosyncratic local institutional factors dominated, while census-based measures of need had little explanatory power: “Some localities have been much more aggressive in seeking HUD assistance than others”.

³⁹See Belsky (1992) and Apgar and Herbert (1994) for some cogent criticism of this practice.

such as the percent of the nation's poor renters that are in a particular metropolitan area. Full details are given in Appendix B.⁴⁰

From 1988 to 1992, 65 percent of all vouchers were allocated using the formula. Although figures for earlier years are not available, an examination of federal regulations governing the voucher program (discussed in Appendix B) suggests that the magnitude must have been roughly similar.⁴¹ The correlation between the fraction of the poor with a voucher in each MSA and the fraction with a formula-allocated voucher is 0.68.

So formula allocation is quite important in determining how many vouchers go to each MSA. One cause for concern is that the formula appears to favor older cities on the coasts, which tend to have high rents. Probably the most serious concern is that all the formula elements are calculated for renter-occupied housing only. Thus, the formula favors cities with low ownership rates, since cities with a large percentage of renters also have a large percentage of poor renters, renters living in crowded housing, and so on. Holding income constant, low ownership rates are associated with higher rents. For this reason, it is important to control for the formula, in a manner that will be discussed below. In addition, metropolitan fixed effects should help to reduce any bias, since the formula is itself fixed over time.

In 1993, but not for earlier years, a detailed breakdown of allocation methods is available. Approximately 47,000 new vouchers were given out in 1993, of which 61 percent were allocated by formula, and the rest were allocated on a discretionary basis. The discretionary vouchers include 26 percent which were allocated on a competitive basis akin to applying for a grant (often for demonstration projects), 7 percent which were given to families displaced by rehabilitation or demolition of government projects, 5 percent which were used for disaster relief, and a small number were allocated by direct congressional mandate.⁴²

If 1993 is typical, then most discretionary (non-formula) vouchers went to housing authorities who successfully competed for them. If a 'housing crisis' prompted local authorities to apply, then the results could be biased. Note, though, that a temporary housing crisis will not necessarily bias the results, since the

⁴⁰The six formula factors are (1) households, (2) poor households, (3) poor households in old housing, (4) crowded households, (5) households with high rent burdens, and (6) two measures of the vacancy shortage. All formula elements refer to renter households only. A similar formula was used from the beginning of the program in 1974 until the 1980 census data became available (conversation with HUD Economist Ray Kahn, March 6, 1997). See the *Code of Federal Regulations*, 1995 (24 CFR 791.402) for the legal definition of the formula, and Apgar and Herbert (1994) for a detailed explanation.

⁴¹These figures have only been published since 1990. The figures for 1988 and 1989 are from unpublished tabulations supplied by HUD.

⁴²These figures are from the *Code of Federal Register*, 1990 (58 FR 38813 and 58 FR 36808). In theory, old vouchers can also be reallocated among MSAs, if housing authorities are not able to use them, or if recipients move. I suspect both factors are minor, but there is little information available.

vouchers stick after the crisis is over. Disaster relief vouchers should not present a problem, since they are only funded temporarily, and will therefore make up a very small percentage of the voucher stock.

Vouchers allocated to families displaced by renovation or demolition are potentially worrisome, since they are associated with a change in the housing stock. However, during the period covered by the data, demolition of subsidized housing was very rare. The policy of ‘vouchering-out’ severely distressed housing projects began only around 1993. Previously, regulations required that demolished public housing be replaced one-for-one with newly constructed units.⁴³ Prior to 1993, renovation was much more common. Since housing stock is only temporarily lost during a renovation, this situation is much less worrisome.

So, although neither the discretionary nor the formula allocation methods are as clean as a random, experimental, allocation method would be, the discretionary method approaches this ideal more closely. It is not obvious that local public housing authorities are much concerned with local housing market conditions when they apply for extra vouchers. Even if they are, the conditions that led PHAs to apply may have long vanished by 1993, the main year examined here, while the vouchers are likely to have remained.

With these considerations in mind, a useful diagnostic would be to estimate

$$\ln P = Xa + b_D V_D + b_F V_F + \eta, \quad (7)$$

where V_F indicates vouchers allocated by formula (as a percentage of poor households) and V_D indicates discretionary vouchers. ‘Formula vouchers’ and ‘discretionary vouchers’ are identical from the point of view of recipients, differing only in the method of allocation to metropolitan areas. Hence, they should have the same effect on housing markets. Testing $b_D = b_F$, therefore, is a test of whether allocation is endogenous, whether vouchers chase rents.

Eq. (7) cannot be directly estimated, since V_F is observed only up to a constant. For example, the data indicate that Oakland received 1.2 percent of all formula vouchers, but not the total number.⁴⁴ So V_F is calculated by assuming that one million formula vouchers have been allocated, multiplying by each MSA’s share, and then dividing by the number of households in poverty.⁴⁵ In addition, the total number of vouchers in each MSA is observed ($V = V_D + V_F$), while V_D is not

⁴³HUD could waive one-for-one replacement in some circumstances, but rarely did. “Only under certain limited conditions, may public housing units be replaced with demand-oriented subsidies . . . At present, virtually no replacement housing plans have been approved that consist primarily of demand-oriented subsidies” (Schill, 1993, pp. 541–542).

⁴⁴Since the formula remained constant until 1993, the formula percentages can be calculated using census data. I have obtained the unpublished 1980 Census tabulations HUD used to calculate the formula from Professor Apgar of Harvard University.

⁴⁵One million is a convenient number, which puts V and V_F in the same scale, since it turns out that this is almost exactly the same as assuming that all vouchers were allocated using the formula.

observed directly. This turns out to be enough information to estimate b_D , and to test $b_D = b_F$. Eq. (7) can be rewritten in terms of observables by noting that

$$\ln P = Xa + b_D(V_D + V_F) + (b_F - b_D)V_F + \eta,$$

which implies

$$\ln P = Xa + b_D V + \frac{b_F - b_D}{\alpha} \alpha V_F + \eta, \quad (8)$$

where α is an unknown constant, and αV_F is observed. Thus, $(b_F - b_D)/\alpha = 0$ tests $\hat{b}_D = \hat{b}_F$ with observables. If the results suggest $b_D = b_F$, this is reassuring, since this can only occur if the allocation method is not biased, or if both allocation methods are biased in exactly the same way. In the preferred estimate below, the hypothesis that $b_D = b_F$ cannot be rejected by the data.

The case where $b_D \neq b_F$ is also worth discussing, since this occurs in the specification tests. In this case, it is likely that b_F is not a valid estimate, because the formula reflects housing market conditions, such as the ownership rate, while b_D , which is only loosely tied to market conditions, would appear to be a more plausible estimate.

Further, it can be shown that, under certain circumstances, bias in b_F will not be transmitted to b_D , even under the rather pessimistic assumption that V_F is correlated with the error term. This result requires, first of all, that V_D could be used as a valid instrument for V , if it were observed (i.e. that V_D is not correlated with the error term). V_D is not observed, but including V and V_F in the regression produces the same coefficient on V as if V_D were observed and substituted for V . The cost of not observing V_D , however, is that the assumption that V_D and V_F are not correlated, conditional on X , is also necessary to insure a consistent estimate in this setting. In particular, Appendix A shows that if $\text{Cov}(V_D, \eta) = 0$ and $\text{Cov}(V_D, V_F | X) = 0$, then $\text{plim } \hat{b}_D = b_D$. One way in which these assumptions could be violated is if HUD uses discretionary vouchers to ‘compensate’ for formula voucher allocations that are too small or too large. In order to do this, HUD would need to know V (i.e. the voucher stock in various localities, relative to some measure of need such as the population in poverty). Since HUD did not produce this information until recently, it would have been difficult for them to second guess their formula allocations. In addition, ‘compensation’ is not one of the allocation criteria in the announcements of voucher competitions that I have examined.

Some intuition for this result comes from partitioned regression. Recall that b_D can be estimated by first regressing V on V_F and X , taking the residuals, and then regressing $\ln P$ on those residuals. In creating the residuals, V_F , the endogenous part of V is stripped away.⁴⁶

⁴⁶This conclusion is closely related to the result that bias due to measurement error is not transmitted to variables that are uncorrelated with the error-ridden variable (Griliches, 1986, p. 1479).

4.2. Empirical model

The main regression results are presented in Tables 4, 6 and 7. The means of the regression variables are reported in Table 3. The dependent variable in all the tables is the price of housing for the lower, middle, and upper terciles, as measured by the neighborhood poverty index, from the first stage hedonic. Henceforth, this will be called simply lower (middle, and upper) tercile rent. The rents for each MSA are then regressed on vouchers and formula vouchers per household in poverty, as well as the log of population, 10-year population growth, median family income, and, in some specifications, a set of regional dummies.⁴⁷

Note that the number of households in poverty (both renters and owners) is chosen for the denominator of the voucher variables, rather than one-third of

Table 3
Means of regression variables^a

	Mean	Std. Dev.
<i>Rent, 1993</i>		
Lower tercile	5.94	0.286
Middle tercile	6.07	0.262
Upper tercile	6.18	0.257
<i>Rent, 1974</i>		
Lower tercile	4.69	0.236
Middle tercile	4.81	0.196
Upper tercile	4.89	0.177
Median family income, 1990	3.62	0.156
Population, 1990	13.8	0.774
Population growth, 80–90	0.129	0.125
Median family income, 70–90 change	1.32	0.080
Population, 70–90 change	0.277	0.261
Decade's population growth, 70–90 change	−0.068	0.112
Vouchers, 1995	0.124	0.058
Updated formula vouchers	0.125	0.063
Formula vouchers	0.110	0.051
<i>N</i>	90	

^a Note: all variables are in logs, except for the voucher variables, which are expressed as a fraction of households in poverty. 'Tercile' refers to the neighborhood poverty index terciles.

⁴⁷The regional dummies represent the nine census divisions. They should not be confused with the four census regions, which are never used in this paper.

unsubsidized renters, which would match the dependent variable, and which would seem more appropriate in light of the theoretical model. This choice was made because the homeownership rate is correlated with rents and it seemed unwise to confound the fraction with vouchers with the fraction of renters. Of course, the chosen measure still confounds the fraction with vouchers with the fraction of poor households, and poverty rates are also correlated with rents. Including poverty in the denominator was thought to be the lesser of two evils, because income is also included in the regression. These issues are explored in the sensitivity analysis below.

Since the number of individual observations in each housing market is not constant in the first stage hedonic, the second stage error term will be extremely heteroskedastic. To correct for this, the second stage estimation technique is weighted least squares, with the inverse of the standard errors from the first stage hedonic price dummies used as weights.⁴⁸ This model could be estimated using a one-step procedure by replacing the MSA/income group interactions in the first stage with a series of MSA/income group/MSA characteristic interactions. However, the two-step procedure greatly facilitates analysis since the appropriate conceptual unit here is the housing market, not the individual.

One way to address concerns about the voucher allocation process is to estimate the model in difference form (i.e. the change in price, rather than simply the price). Differencing the data controls for any unobserved ‘metropolitan effects.’ Since the formula was fixed for over a decade, and since its elements change only slowly, controlling for MSA fixed effects should go a long way towards alleviating concerns about the endogeneity of allocation. The difficulty with differencing the data is that only 1 year of voucher stock data is available, so the change in vouchers cannot be calculated. Even if time series data were available, it seems unlikely that there is much within-MSA variance in vouchers over time, given the fact that voucher allocations are cumulative. However, since the voucher stock was

⁴⁸Card and Krueger (1992) and Hanushek et al. (1996) estimate such two stage models and discuss the econometric issues involved. The simplest WLS model assumes that the variance of the error term in the second stage is inversely proportional to the sample size in each housing market: $\sigma_m^2 = \delta(1/N_m)$. There are no covariance terms to consider, since the hedonic price dummies are orthogonal by definition. A better model assumes that $\sigma_m^2 = \delta\xi_m^2$, where ξ_m^2 is the variance of the prices in each market, estimated in the first stage regression. This is the model estimated here.

I considered the model $\sigma_m^2 = \kappa^2 + \delta\xi_m^2$, where κ^2 is the variance that would remain if N_m were large. κ^2 might be thought of as the ‘substantive’ variance due to omitted MSA characteristics, while ξ_m^2 is the sampling variance due to varying numbers of observations in different markets (Dickens, 1990; Hanushek et al., 1996). This variance components model could be estimated using GLS. I ran some tests to determine if the WLS model is adequate (i.e. if the weighting had transformed the model to homoskedasticity). In every case, I was unable to reject the null hypothesis of homoskedasticity at anything approaching conventional levels of significance. In addition, estimates of κ^2 were extremely imprecise, suggesting that a less constrained GLS model could well be worse. In general, WLS will be an adequate model when the sample size (of individuals in each housing market) is small, and varies significantly between markets. That is certainly the case here.

zero in 1974, when the program was started, the model can be estimated using 20 year price changes.

The first-difference results will only be reasonable if there are MSA characteristics that are fixed over the course of 20 years. Although this is a long time, many underlying conditions are essentially permanent. San Francisco's stock of developable land has been constrained by the Pacific Ocean for a long time and its climate has always been foggy, for example. Fig. 1 plots the 1993 lower tercile rent index against the equivalent for 1974. Clearly, there is a high correlation, even across two decades. In general, MSAs with the highest rents in 1974 still had the highest rents in 1993. The R^2 from the regression of the 1993 index on the 1974 index is 0.53 (implying a correlation coefficient of 0.73). This is consistent with fixed effects. It does not rule out MSA-specific time trends, but it does suggest such time trends cannot be too large.⁴⁹

Another way to address these concerns about the endogeneity of allocation is to estimate a series of specification tests. A classic test from the training literature is applicable to the cross-section results. Evaluations of job training programs frequently test for an effect on *pre*-program earnings. If the trained had higher wages *before* they underwent training, then the estimation method is likely biased (due to omitted variables or selection on unobservables). Similarly, if vouchers are found to raise rents in 1974, the year before the program began, then voucher allocations are not exogenous. Failing this specification test suggests that voucher

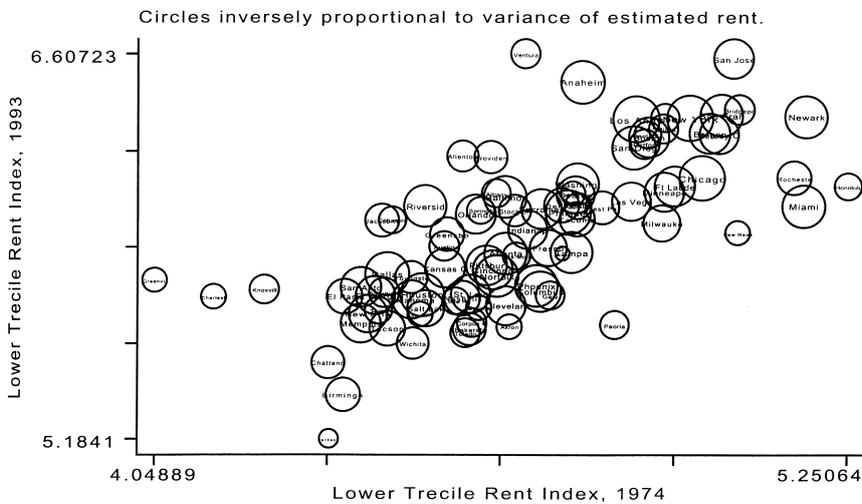


Fig. 1. MSA rent over two decades.

⁴⁹Poterba (1991) also notes this pattern in house prices, which are much more volatile than rents. Similar plots for the middle and upper terciles show higher correlations (of 0.79 and 0.80, respectively).

allocations are determined by some fixed (or at least slowly-evolving) effect that is omitted from the equation.

Another test examines the effect of vouchers on rent for the middle and upper terciles. The idea is that there should be a decreasing pattern to the coefficients. Vouchers should affect wealthier housing markets less than they affect poor markets. If the specification test is failed, a natural assumption is that there is an omitted variable which is positively correlated with both vouchers and rents. Since this variable is unknown, a clear concern is that this variable enters both the equation for the lower and upper tercile, biasing both.⁵⁰

A priori, to the extent that the lower tercile results are big, the middle and upper tercile results should be small. The issue hinges on the cross-price elasticities between the three markets. At one extreme, lower tercile tenants may be unable to move from one market to another. Then, vouchers will have a large effect on lower tercile rent, and none on the other terciles. At the other extreme, all three terciles may be perfect substitutes. In that case vouchers will have moderate effects on all markets. In between, vouchers will presumably have a larger effect on the lower tercile than on the middle, which will be larger than the effect on the upper tercile. A filtering model also leads to the same conclusion. If vouchers have a large effect on lower tercile rent, then it must be that few recipients are moving to the high-quality market, where rent is simply a function of construction cost.

4.3. Cross-section results

Table 4 shows the effect of vouchers on all three rent terciles. The sample is restricted to include only MSAs where 1974 rent information is available. This sample is used for the rest of the results in the paper. All the regressions in this table include regional dummies, which were always jointly significant at the 1 percent level or better.

The lower tercile voucher coefficient is 0.762 (*t*-statistic 1.57). To interpret this figure, recall that, on average, there are enough vouchers for 12.4 percent of the poor (Table 3). This implies that eliminating the voucher program would lower rents by 9.4 percent ($= 0.124 \times 0.762$).⁵¹ This is a fairly large effect, though not implausible. I return to this point below. The middle and upper tercile results are quite reasonable too, suggesting that vouchers raise rents by about 2 percent in the other two markets, which cannot be distinguished from zero.⁵²

⁵⁰Note, though, that the omitted variable may only affect the upper tercile equation. An example of such a variable might be 75th percentile income.

⁵¹Besides representing a useful thought experiment, this figure is also the calculation of the elasticity of rent with respect to the fraction with vouchers. This elasticity is, therefore, 0.094 in this specification.

⁵²It is worth noting that this calculation extrapolates beyond the range of the data, since there are no cities with less than 4 percent subsidized (Table 1).

Table 4
WLS cross-section regressions. Effect of vouchers on rent terciles^a

	Lower	Middle	Upper
Vouchers per poor household, 1995	0.762 (0.486)	0.200 (0.404)	0.142 (0.410)
Median family income, 1990	0.0912 (0.158)	0.120 (0.142)	0.0857 (0.146)
Population, 1990	0.0169 (0.0206)	0.0275 (0.0178)	0.0420 (0.0180)
Population growth, 80–90	0.577 (0.172)	0.445 (0.148)	0.454 (0.156)
Formula vouchers per poor hhd	2.77 (0.579)	3.14 (0.513)	3.31 (0.525)
Regional dummies	Yes	Yes	Yes
R^2	0.837	0.858	0.860
N	90	90	88

^a Note: dependent variable is a hedonic index of unsubsidized (log) rent for the lower, middle, and upper income terciles in 1993. Income and population are in logs. Both voucher measures indicate vouchers as a fraction of households in poverty. All regressions include a constant and dummies for the nine census divisions. ‘Formula vouchers’ indicate the number of vouchers allocated using a formula based on census data. See text. Coefficients are weighted least squares estimates, with the weights proportional to the inverse of the standard error of the first stage hedonic rent estimates. Standard errors in parentheses.

In all three specifications, the formula voucher coefficients are large and statistically significant. As discussed in the sensitivity analysis below (Table 8), if the formula variable is removed from the equation, the effect of vouchers more than doubles, and the specification test is failed, since the middle and upper tercile results also become large and statistically significant. Because the formula coefficient is greater than zero, the results imply that formula vouchers have larger effects than do discretionary vouchers. This can only happen if one of the allocation methods is endogenous, and determined in part by rents. Above, it was argued that the details of the formula allocation suggest that it is tied to long run trends in housing market conditions. The remaining discretionary vouchers, which identify the effect of the voucher coefficient, appear to be less tied to metropolitan conditions.

4.4. Pre-program rents specification test

Another specification test asks if 1995 vouchers ‘affect’ 1974 rents. Since the voucher program did not exist in 1974, finding significant effects suggests that vouchers were allocated to cities with high rents, rather than causing high rents.

Table 5
Specification test: effect on 1974 rent terciles^a

	Lower tercile		Middle tercile		Upper tercile	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Vouchers per poor household, 1995	-0.902 (0.605)	0.707 (0.525)	0.373 (0.460)	1.68 (0.418)	-0.287 (0.424)	1.06 (0.390)
Median family income, 1970	0.212 (0.258)	0.669 (0.260)	-0.126 (0.196)	0.270 (0.200)	-0.111 (0.177)	0.307 (0.182)
Population, 1970	-0.0139 (0.0235)	0.0170 (0.0248)	0.00463 (0.0182)	0.0321 (0.0195)	0.0149 (0.0164)	0.0432 (0.0179)
Population growth, 60–70	0.192 (0.182)	0.0403 (0.198)	0.198 (0.131)	0.101 (0.146)	0.141 (0.115)	0.0552 (0.132)
Formula vouchers per poor hhd	2.85 (0.666)		2.47 (0.520)		2.47 (0.476)	
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.657	0.574	0.700	0.612	0.745	0.653
N	90	90	90	90	88	88

^a Note: dependent variable is a hedonic index of unsubsidized (log) rent for the lower, middle, and upper income terciles in 1974. Income and population are in logs. Both voucher measures indicate vouchers as a fraction of households in poverty. All regressions include a constant and dummies for the nine census divisions. 'Formula vouchers' indicate the number of vouchers allocated using a formula based on census data. See text. Coefficients are weighted least squares estimates, with the weights proportional to the inverse of the standard error of the first stage hedonic rent estimates. Standard errors in parentheses.

Table 5 reports the results. The middle and upper terciles pass the test, with point estimates that are fairly small and are not statistically significant. The lower tercile estimate, however, is -0.9 (t -statistic 1.49), which is much larger in absolute value than the results for the other terciles. Thus, more discretionary vouchers were allocated to metropolitan areas with low rents in 1974. The 1993 cross-section results, then, may be biased towards finding smaller effects on lower tercile rent than are actually caused by the voucher program.

This table also highlights the importance of including the formula in the equation. This table is an equally valid specification test for formula-allocated vouchers. In every specification, though, the formula coefficient is large and significantly different from zero, often at high levels of confidence. In addition, when the formula is omitted from the equation, only the lower tercile specification test is passed, and quite weakly, with a coefficient of 0.71 and a t -statistic of 1.35. It is quite clear, then, that the formula allocates vouchers endogenously. Thus, excluding the formula from the regression would certainly lead to biased results in a cross-section.

Table 6
Cross-section WLS regressions with lagged rent. Effect of vouchers on rent treciles^a

	Lower	Middle	Upper
Vouchers per poor household, 1995	0.862 (0.471)	-0.0382 (0.370)	0.153 (0.390)
Median family income, 1990	0.0898 (0.152)	0.136 (0.128)	0.157 (0.141)
Population, 1990	0.0159 (0.0199)	0.0240 (0.0161)	0.0382 (0.0171)
Population growth, 80–90	0.508 (0.168)	0.381 (0.135)	0.432 (0.149)
Formula vouchers per poor hhd	2.12 (0.615)	2.26 (0.508)	2.36 (0.592)
Hedonic rent, 1974	0.223 (0.0877)	0.375 (0.0886)	0.351 (0.118)
Regional dummies	Yes	Yes	Yes
R^2	0.850	0.885	0.875
N	90	90	88

^a Note: dependent variable is a hedonic index of unsubsidized (log) rent for the lower, middle, and upper income treciles in 1993. Income and population are in logs. Both voucher measures indicate vouchers as a fraction of households in poverty. All regressions include a constant and dummies for the nine census divisions. ‘Formula vouchers’ indicate the number of vouchers allocated using a formula based on census data. See text. Coefficients are weighted least squares estimates, with the weights proportional to the inverse of the standard error of the first stage hedonic rent estimates. Standard errors in parentheses.

4.5. Results with lagged rent and 1974–1993 differences

Table 6 reports results for cross-section regressions that include 1974 rent as an additional explanatory variable. Again, the regional effects are highly statistically significant, so results will only be reported that include them.⁵³ In all specifications, lagged rent is highly significant, with t -statistics ranging from 2.5 to 4. Adding lagged rent increases the lower trecile coefficient somewhat to 0.862 (t -statistic 1.83), and it becomes statistically significant at the 10 percent level. The middle and upper trecile coefficients remain statistically insignificant and close to zero. All the formula coefficients fall as well, although they remain large and statistically significant. Overall, the results are similar to the cross-section results.

⁵³ The results in Tables 4 and 6 were re-estimated without regional dummies. In every case the voucher coefficient became bigger, increasing to 1.16, 0.45, and 0.39 for the lower, middle, and upper treciles, respectively, in Table 4 and increasing to similar values in Table 6.

Table 7
First-difference WLS regressions. Effect of vouchers on 1974–1993 change in rent treciles^a

	Lower	Middle	Upper
Vouchers per poor household, 1995	1.31 (0.544)	0.259 (0.453)	0.0655 (0.411)
Median family income, 70–90 change	0.766 (0.262)	0.0509 (0.232)	0.590 (0.213)
Population, 70–90 change	0.0640 (0.0785)	0.0674 (0.0683)	0.130 (0.0620)
Decade's population growth, 70–90 change	0.0959 (0.159)	0.203 (0.142)	0.137 (0.129)
Formula vouchers per poor hhd	-0.112 (0.615)	1.56 (0.521)	1.63 (0.463)
Regional dummies	No	No	No
R^2	0.324	0.284	0.444
N	90	90	88

^a Note: dependent variable is 1974–1993 change in a hedonic index of unsubsidized (log) rent for the lower, middle, and upper income treciles. Income and population are in logs. Both voucher measures indicate vouchers as a fraction of households in poverty. All regressions include a constant. 'Formula vouchers' indicate the number of vouchers allocated using a formula based on census data. See text. Coefficients are weighted least squares estimates, with the weights proportional to the inverse of the standard error of the first stage hedonic rent estimates. Standard errors in parentheses.

Table 7 reports the first-difference results, with all variables defined as 20-year differences. So the dependent variable is the 1974–1993 rent change, and the right hand side variables include the 1970–1990 change in median family income, and so on. The voucher measures are unchanged from the cross-section specifications. 1974 vouchers (and formula vouchers) were zero so these variables are already implicitly differenced. Since regional effects are differenced out, they are not included in these specifications. Also, tests for the joint significance of the regional effects do not reject zero in two out of the three treciles, confirming that the dummies do not belong in the first-difference equation.⁵⁴

The lower trecile voucher coefficient increases to 1.31 (t -statistic 2.41) in the differenced specification. The middle and upper trecile results remain close to zero. In this model, eliminating the voucher program (reducing the fraction with vouchers from 0.124 to zero) would lower rent for the poor by 16 percent, the middle by 3.2 percent, and the top by 0.08 percent, which is a very sensible pattern of results. Further, the null hypothesis that vouchers have no effect on rent for the

⁵⁴The P -values for the F -tests of the regional dummies were 0.35, 0.004, and 0.13 for the lower, middle, and upper treciles, respectively. In specifications that include the regional dummies, the lower trecile voucher coefficient falls to 0.80 (with a t -statistic of 1.27). The other two voucher coefficients fall as well, becoming negative.

middle and upper terciles cannot be rejected. The fact that the middle and upper tercile coefficients are consistently close to zero is heartening. However, given the size of the standard errors, concern about the power of the specification test is quite sensible. Although zero cannot be rejected, the 95 percent confidence interval for the upper tercile coefficient reaches 0.87, which is substantively large.

Another important result is that the formula coefficient falls almost to zero in the lower tercile results. This should reduce concerns about the endogeneity of allocation. In addition, omitting the formula from the equation has little effect on the lower tercile results (Table 8). The upper tercile results, however, are affected by the inclusion of the formula variable. The formula coefficient in the upper tercile results remains large, although they are smaller than those in earlier tables, and omitting the formula causes the specification test to be failed.

4.6. Sensitivity analysis

To check for the robustness of the results, the cross-section and first-difference results were replicated using different measures of rents, of vouchers and formula vouchers, and additional explanatory variables. The top panel of Table 8 reports the voucher coefficient from regressions with alternative measures of rent. For comparison, the baseline result from Tables 4 and 7 are in the first row.

One potential concern is that changes in the dependent variable, which can be interpreted as ‘the price of neighborhood quality’ could be due to changes either in price or in quality itself. That is, vouchers could have externalities that improve neighborhoods. Although this concern is somewhat alleviated by the Galster et al. (1999) results, additional evidence is also of interest. An additional source of evidence can be found by estimating the effect of vouchers on the price of an observable housing characteristic.⁵⁵ Since vouchers are conditioned on the recipient occupying a unit with enough bedrooms to house her family (program rules map family composition to bedrooms), bedrooms are the obvious choice for this housing characteristic. The price of a bedroom (in each market) should reflect only differing supply and demand conditions, rather than unobservable neighborhood quality.

The price of a bedroom is measured by modifying Eq. (6), and adding the number of bedrooms interacted with the MSA effects:

$$\ln R_{ij} = B_{ij}M_j'b_j + M_j'P_j + X_{ij}'\beta, \quad (9)$$

where B_{ij} is the number of bedrooms of individual i in market j , M_j is a vector of indicators for each housing market, and b_j is a vector of the price of a bedroom in each market, and the other variables are defined as before. This equation estimates a separate price of a bedroom in each market, as well as separate housing market

⁵⁵I thank an anonymous referee for this suggestion.

Table 8
Sensitivity analysis. Voucher coefficients from WLS regressions^a

Rent tercile:	Cross-section			First difference		
	Lower	Middle	Upper	Lower	Middle	Upper
Baseline, from Tables 4 and 7	0.762 (0.486)	0.200 (0.404)	0.142 (0.410)	1.31 (0.544)	0.259 (0.453)	0.0655 (0.411)
<i>Alternate rent terciles</i>						
Price of a bedroom, 1993 (No formula)	0.610 (0.305)	-0.370 (0.308)	-0.198 (0.278)			
No top-coding correction	0.757 (0.482)	0.180 (0.390)	0.0731 (0.401)	1.32 (0.542)	0.259 (0.446)	0.0126 (0.403)
Tobit	0.761 (0.487)	0.214 (0.407)	0.207 (0.419)	1.31 (0.544)	0.275 (0.454)	0.144 (0.420)
Simple poverty terciles	0.704 (0.497)	0.218 (0.399)	0.162 (0.383)	1.55 (0.595)	0.204 (0.434)	-0.143 (0.378)
Voucher recipients in sample	0.654 (0.468)	0.239 (0.364)	0.156 (0.391)	1.37 (0.526)	0.284 (0.444)	0.0314 (0.388)
<i>Alternate specifications</i>						
Formula updated from 1990 Census	1.19 (0.480)	0.615 (0.395)	0.724 (0.431)	1.11 (0.531)	0.200 (0.427)	0.246 (0.404)
Per capita variables as instruments	0.478 (0.555)	0.0520 (0.461)	0.0301 (0.471)	1.18 (0.603)	-0.144 (0.508)	-0.501 (0.485)
No formula vouchers	1.85 (0.486)	1.28 (0.441)	1.26 (0.456)	1.24 (0.385)	1.19 (0.346)	1.06 (0.318)
No formula vouchers and per capita IV	2.09 (0.616)	1.77 (0.566)	1.66 (0.589)	1.32 (0.530)	0.679 (0.481)	0.203 (0.476)
Vouchers per renter/3	0.276 (0.630)	-0.161 (0.529)	-0.178 (0.555)	1.38 (0.708)	-0.392 (0.639)	-0.636 (0.564)
Segregation on RHS	0.820 (0.493)	0.304 (0.383)	0.122 (0.419)	1.09 (0.546)	0.00251 (0.428)	-0.0288 (0.422)
Suburbanization on RHS	0.759 (0.489)	0.187 (0.407)	0.157 (0.414)	1.18 (0.561)	0.0383 (0.451)	-0.0542 (0.421)
<i>N</i>	90	90	88	90	90	88

^a Note: table entries are voucher coefficients from weighted least squares regression, with the weights proportional to the inverse of the standard error of the first stage hedonic rent estimates. Standard errors in parentheses. Cross-section regressions follow the same specification as Table 4 and the first-difference regressions follow the same specification as Table 7, except where noted.

intercepts (P_j). The price of a bedroom can then be regressed on the fraction with vouchers in an MSA and the other X variables, as in the main results.

Table 8 reports estimates of the effect of vouchers on the price of a bedroom. Because Eq. (9) contains MSA/income class effects, there is no need to estimate first-difference models in order to control for such effects. For that same reason, there is no need to include formula vouchers in the equation to control for characteristics of the MSA used in allocating vouchers. That is, although there is some reason to be concerned that vouchers are allocated to high-rent areas, there is

much less reason to be concerned that they are allocated to areas where the price of a large unit is high relative to smaller units.⁵⁶

The results suggest that vouchers bid up the price of a bedroom in the low-income market by 7.6 percentage points ($t = 2.0$), multiplying the coefficient by the average fraction with a voucher, as before. The total effect of vouchers in this model includes the effect on the price of a bedroom and the effect on the housing market intercepts. Comparing the average MSA to an MSA with no vouchers, as before, yields an effect of 22 percent, which is larger than the baseline results, although the effect on the intercepts (not reported in the table) is not statistically significant in this model.⁵⁷ These results are consistent with the baseline findings, and begin to suggest one of the channels through which vouchers affect rents.

The next two rows of the table investigate topcoding. Recall that rents in the 1993 AHS are topcoded at \$1000, and the baseline results recode this to \$1147. The second row of this panel shows the voucher coefficient when topcoded rents are left at 1000. In the third row, a tobit is used instead of imputation. Unsurprisingly, topcoding has no effect at all on the lower and middle terciles. The upper tercile coefficient is slightly larger in the baseline specification compared to the specifications with no correction, and slightly smaller than the tobit specification. In no case, however, does the top-coding adjustment (or lack of one) change the overall conclusions.

The next row substitutes terciles based only on household income as a percent of the poverty line for the imputed neighborhood income index, based on a longer list of income and wealth measures, used in the body of the paper. The poverty terciles have almost no effect in the cross-section specifications. In the difference specification, somewhat surprisingly, the lower tercile voucher coefficient becomes bigger, and the upper and middle coefficients become smaller. The poverty terciles are likely to produce more classification errors than the more complete neighborhood income terciles (moving some of the truly rich into the poor category, and vice versa). This should have resulted in a compression of the coefficients, just as they compress the distribution of mean rents across terciles. However, although this result is surprising, it does not alter the main conclusion that vouchers increase

⁵⁶In models where they were included, the coefficient on formula vouchers was always statistically insignificant, and was negative in two of the three income groups. I also estimated specification tests of the effects of vouchers on pre-program (1974) bedroom prices, like those reported in Table 5. There was no evidence that vouchers were allocated to MSAs where the price of a bedroom was high in 1974 for two of the three income groups, including the crucial low-income group. The specification test was failed by the middle-income group, however.

⁵⁷This is calculated as the effect of vouchers on the price of a bedroom (0.610) multiplied by the average number of bedrooms in the low-income market (1.86) plus the effect of vouchers on the intercepts (0.635), all multiplied by the average fraction with a voucher [$= (0.610 \times 1.86 + 0.635) \times 0.124$]. The effect of vouchers on the intercepts refers to discretionary vouchers, for the same reason that the baseline results control for the formula, and is not statistically significant ($t = 0.7$).

rents for those in the lower tercile. It does offer some evidence that the effect may be bigger than the baseline results suggest.

Finally, the last row of the panel uses the baseline definition of rents, but includes voucher recipients in the sample. Voucher recipients were dropped from the main results because of suspicion about the accuracy of the rent data they report, and confidence in the hedonic to avoid sample selection bias. In this table, rent for voucher recipients is measured as the rent received by the landlords (rather than the rent paid by the tenant after the subsidy).⁵⁸ Adding voucher recipients has almost no effect on the voucher coefficient, although the standard errors become uniformly smaller, reflecting the larger sample size.

The first row of the bottom panel of Table 8 reports results when the formula, which is based on 1980 Census data, is updated using 1990 Census data. Earlier drafts of this paper focussed on results using the updated formula. These results allow identification to come not only from discretionary vouchers, but also from 'erroneous' formula vouchers. Because this specification uses more of the variation in vouchers, the standard errors are slightly smaller. Ultimately, however, the slight gain in precision was far outweighed by the added complication and greater difficulty of interpreting the updated formula results. Whether the actual or the updated formula is used, both sets of first-difference results are quite similar, which is one reason to place the greatest confidence in this set of results. Using the updated formula, the first-difference coefficient falls to 1.11, which is, if anything, a more plausible result than the baseline estimates. The cross-section coefficients show the same general pattern as the baseline results, although they are all larger. When the updated formula is used, the lower tercile coefficient is quite close in size to the first-difference coefficient, and is statistically significant. The middle and upper tercile results become larger as well, and only weakly pass the specification test.

One potential problem with the specification is the possibility that the results are driven by the poverty counts in the denominator of the voucher measures rather than the numerator. That is, it could be that poverty is negatively correlated with rents. Vouchers/poverty, then, would be positively associated with rents, even if there was no true effect of vouchers. To explore this, the equations were re-estimated using vouchers (and formula vouchers) per capita as instruments for vouchers (formula vouchers) per household in poverty. This had little effect on the results. Although the cross-section lower tercile falls, the corresponding first difference coefficient is little affected.

Another alternative specification removes formula vouchers from the equations entirely. As discussed earlier, the voucher coefficients become bigger and specification tests are failed, when the total effect of vouchers is estimated. The

⁵⁸Sample members who did not report this information were dropped. In all, 329 voucher recipients were added to the 6526 observations already in the 1993 hedonic regression.

one exception is the crucial lower tercile first-difference coefficient, which is almost unchanged. The fourth row combines the previous two, removing formula vouchers from the model, and using vouchers per capita as an instrument for vouchers per poor household. The cross-section results are no more plausible than the results in the previous row, which use neither formula voucher nor instruments. However, the first-difference results are quite similar to the baseline, and the lower tercile coefficient is almost the same as the baseline. The middle and upper tercile coefficients follow a sensible decreasing pattern, although the middle tercile specification test is only weakly passed.

The next row of the table modifies the measure vouchers per household in poverty by replacing the denominator with one-third of renters (i.e. the number of renters divided by three), which more closely matches the dependent variable, and the theoretical model. Just as vouchers per household in poverty confounds the fraction poor with the fraction with vouchers, so too does vouchers per lower tercile renter confound voucher availability with ownership rates. The table suggests that this issue is quite important in the cross section, causing the coefficients to become small for all three terciles.⁵⁹ However, the first-difference results are much more robust, with the lower tercile coefficient almost unchanged. Presumably, differencing controls for the homeownership rate, which is fairly constant over time.⁶⁰

Because of a limited sample size, and a concern about data mining, the baseline specification is relatively parsimonious. At the suggestion of a referee, two additional models were estimated, one with a measure of MSA segregation, and one with a measure of the fraction of the MSA population in the suburbs. As reported in the last row of Table 8, adding these variables had little effect on the results.⁶¹

Overall, none of the changes explored in the sensitivity analysis do much to alter the overall conclusions from the baseline results. Alternative measures of rents make very little difference to any of the results. Changes in the explanatory

⁵⁹This is to be expected, since high rents imply a large fraction of renters, which implies a low rate of vouchers to renters, which biases the coefficients downwards.

⁶⁰It is reasonable to compare coefficients because the scale of the two variables is, coincidentally, about the same: the average city has enough vouchers for 11.1 percent of lower tercile renters and 12.4 of poor households (both figures from Table 1). However, it is not coincidence that the effect of vouchers on rent in the average city in the first-difference specification is 15 percent ($= 0.111 \times 1.38$), which is almost the same as the baseline result. It is a substantive result that would remain the same if the voucher denominator were replaced with number of lower quartile renters or some other fraction.

⁶¹The measure of segregation used is the common ‘dissimilarity’ index, as calculated by Cutler et al. (1999). I also tried using the ‘isolation’ index, but this measure was never statistically significant and had little effect on the results. The suburbanization variable itself (not reported in the table) was statistically significant in only one of the six specifications (the middle tercile first-difference results), while the segregation measure was statistically significant in the lower and middle tercile first-difference results.

variables and estimation technique do make some difference in the cross-section model. The first-difference results, however, are quite robust to changes in specification.

5. Conclusion

Overall, the baseline results consistently show that the effect of formula vouchers on rent follows an implausible pattern, while the discretionary voucher results are much more reasonable. Formula vouchers were found to substantially raise rents not only for the lower tercile, but for the middle and upper groups as well. In addition, formula vouchers have large effects in 1974, before the voucher program began. The discretionary voucher results, however, pass all the specification tests. The point estimates in all specifications show almost no effect on middle and upper tercile rent. Nor are discretionary vouchers associated with higher 1974 rents. The only anomaly is the negative effect of vouchers on lower tercile rent in 1974 (*t*-statistics 1.5), which suggests that cross-section results may actually understate the effect of vouchers. In addition, in the difference specification, the hypothesis that formula and discretionary vouchers have the same effect on lower tercile rent cannot be rejected. This suggests that the difference specification is sufficient to control for the targeting of vouchers. Finally, the first-difference results were found to be quite robust to changes in the rent and voucher measures.

5.1. Elasticity of supply

The supply and demand model shows that the voucher coefficient equals $\theta/(\varepsilon^S + \varepsilon^D)$, where θ is the subsidy generosity and ε^S and ε^D are the elasticities of supply and demand. If vouchers allow the subsidized to purchase 75 percent more housing than they otherwise would (relative to non-recipients), so that $\theta = 0.75$, then a coefficient of 1.31 implies that $\varepsilon^S + \varepsilon^D = 0.57$.⁶² This suggests quite small elasticities, but not out of the realm of possibility.

An estimate of the elasticity of demand can be found in Hanushek and Quigley (1980), who estimated it to be around 0.6 in the long run. This is a convenient estimate for my purposes, since they are estimated using experimental data from the Housing Allowance Demand Experiments, which tested a voucher style

⁶²Calculation of θ uses figures from Cage (1994), who found that recipients spent \$527 a month on rent, while income-eligible non-recipients spent \$337. Voucher recipients would have spent \$270 a month on rent, were they unsubsidized, assuming that they would have spent the same proportion of their income on rent (42 percent) that unsubsidized income-eligible households do. Then $\theta = (527 - 270)/337 = 0.76$.

subsidy program for low-income renters.⁶³ If the elasticity of demand is 0.6, then the model implies that the elasticity of supply is -0.03 , i.e. just about zero.

Now, all this is predicated on the point estimate of 1.31. The 95 percent confidence interval is (0.22, 2.40), and so we cannot rule out larger supply elasticities. For example, if the true coefficient were 0.77 (one standard deviation below the point estimate), the elasticity of supply would be 0.38, which is still quite small. At the extremes of the 95 percent confidence interval, however, the supply elasticity runs from -0.29 to 2.8.

5.2. Discussion

These calculations clarify two points. First, the small elasticity implied by the results suggests that, despite its moderate size, the effect of the voucher program on rents is surprisingly large. The calculations also imply considerable insulation between lower and higher income housing markets, since the ability to move easily between markets, or substitute towards higher quality housing, should mitigate the price rise. Since the lower tercile coefficient is about as big as could be expected from a theoretical analysis that assumes that movement between markets is impossible, it follows that the other coefficients must be small. Hence the data tell a consistent story, since the estimated effect of vouchers on middle and upper tercile rents is close to zero, as expected.

Another way to characterize the size of the results is to calculate the redistributive effect of vouchers; the ‘leakiness of the bucket,’ to borrow Arthur Okun’s metaphor. Some simple calculations, reported in Table 9, suggest that vouchers do little to redistribute, in the aggregate. Specifically, vouchers cover about two-thirds of recipients’ rent, costing \$5.8 billion dollars in total (excluding

Table 9
How leaky is the bucket?^a

	Lower-income unsubsidized households	Voucher recipients
Monthly rent	\$443	\$537
Transfer/rent	0.16	0.69
Households	9.6 million	1.3 million
Annual total	\$8.2 billion	\$5.8 billion

^a Source: monthly rent: Table 3. Transfer/rent: 0.16 from text; 0.69 from author’s tabulations, 1993 American Housing Survey. Households: one-third of unsubsidized renters from *American Housing Survey for the United States in 1995*, Table 4-12, and Voucher recipients from *A Picture of Subsidized Housing* (HUD, 1997).

⁶³Hanushek and Quigley found that the short run elasticity of demand was about 0.15, and extrapolated using only 2 years of data. This is a drawback, but these are still the best available estimates.

administrative costs). There are about 9.6 million households in the lower third of the private rental market, whose rents have been increased by 16 percent as a result of the voucher program, according to the results presented here. In total, therefore, while vouchers transfer \$5.8 billion to recipients, they cost similarly impoverished non-recipients \$8.2 billion dollars. The net transfer is \$2.4 billion, which goes from poor households to landlords.⁶⁴

The conclusion that vouchers raise rents depends only on the correct specification of the reduced form empirical model. Given a number of strong assumptions, the theoretical model also allows us to extrapolate from the results that the elasticity of supply must be close to zero. Although this study does not attempt to distinguish which particular feature of low-income housing markets inhibits their adjustment to demand shocks, it is worth thinking about what could cause these results.

First, the lack of free exit could cause low supply elasticities. An apartment building yielding low rents cannot be removed from the market except through demolition. There may be many landlords in poor neighborhoods who are earning enough to cover their operating costs, and thus they do not abandon the building, but are not covering the fixed costs associated with creating new housing. Increased rents would then raise profits, but do nothing to increase entry (or decrease demolition). Consider a city like Detroit, or the Woodlawn neighborhood of Chicago discussed in Wilson (1996), where large numbers of fleeing residents have left behind abandoned buildings and vacant lots. It seems quite possible that in such a neighborhood, vouchers could raise rents, but not by enough to induce landlords to repair the roof, or engage in other costly maintenance projects that would stave off decay.⁶⁵

Local policies may also restrict the creation of low-income housing, reducing the elasticity of supply. Examples include habitability laws, building codes, and zoning restrictions like minimum lot sizes and bans on the conversion of single family housing into multiple occupancy units.⁶⁶

An important topic for future research is studies that distinguish among these various explanations for low elasticities. Low elasticities also have important implications for tax and subsidy policy. In particular, they suggest that construc-

⁶⁴Note that a calculation of the welfare loss due to vouchers would be still more gloomy. The calculations in the text neglect the fact that in-kind transfers are not valued at par by recipients, and the distortions imposed on taxpayers, low-income non-recipients, and landlords.

⁶⁵In the extreme case, when housing lasts forever without maintenance, there is a kink in the supply curve. Once housing is built, upward demand shocks will raise prices and increase construction. But below the current quantity of housing, the supply curve will be vertical and downward shocks will lower prices without affecting the quantity supplied. Then, in neighborhoods that have suffered unanticipated falls in demand, landlords might find themselves on the vertical section of the supply curve.

⁶⁶See, e.g., Downs (1991) and Advisory Commission on Regulatory Barriers to Affordable Housing (1991).

tion subsidies may do more to improve the housing conditions of the poor than do demand side subsidies like vouchers.

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Appendix A. Consistency proof

This section shows that even if V_F is correlated with the error term, the bias will not be transmitted to b , the coefficient on V , if V_D and X are uncorrelated with the error term, and if V_D and V_F are uncorrelated, conditional on X . The proof is for the case with a single X variable, hence all the variables should be interpreted as deviations from means.

First, recall that

$$\ln P = aX + bV_D + cV_F + \eta,$$

which is a version of Eq. (7).

We are concerned that V_F is correlated with the error term. The bias can be written as

$$\text{plim } \hat{\beta} = \beta + \text{plim}(Z'Z/N)^{-1}Z'\eta/N,$$

where $Z = [V_D \ V_F \ X]$, β , the coefficient vector, is

$$\begin{bmatrix} a \\ b \\ c \end{bmatrix},$$

and N is the sample size.

Examining the second part of this expression, we have

$$\text{plim } Z'\eta/N = \text{plim} \begin{bmatrix} V_D'\eta/N \\ V_F'\eta/N \\ X'\eta/N \end{bmatrix} = \begin{bmatrix} 0 \\ \text{plim } V_F'\eta/N \\ 0 \end{bmatrix},$$

since η is uncorrelated with X and V_D .

Therefore, $\text{plim } \hat{b} = b + \text{plim } z_{12}V_F'\eta/N$, where z_{ij} is the (i,j) th element of $(Z'Z)^{-1}$. Using the cofactor method to invert $(Z'Z)$ yields

$$z_{12} = \frac{-1}{|Z'Z|} \begin{vmatrix} V_D'V_F & V_D'X \\ X'V_F & X'X \end{vmatrix},$$

where the vertical bars indicate the determinant.⁶⁷

Taking the determinant and multiplying by N/N yields

$$z_{12} = \frac{-1}{|Z'Z|/N} (V_D'V_F X'X - V_D'XX'V_F)/N.$$

Now, we assume we can decompose V_D into a part that is correlated with X and a part that is not: $V_D = pX + e_D$. Similarly, decompose V_F into $V_F = qX + r\eta + e_F$. Here, it is assumed that $\text{Cov}(V_D, \eta) = 0$ and therefore $\text{Cov}(V_D, V_F|X) = 0$. That is, conditional on X , V_D and V_F are uncorrelated.⁶⁸ Then

$$\begin{aligned} \text{plim } z_{12} &= [\text{Cov}(V_D, V_F) \text{Var}(X) - \text{Cov}(V_D, X) \text{Cov}(X, V_F)] \text{plim} \frac{-1}{|Z'Z|/N} \\ &= [pq \text{Var}(X) \text{Var}(X) - p \text{Var}(X)q \text{Var}(X)] \text{plim} \frac{-1}{|Z'Z|/N} \\ &= 0, \end{aligned}$$

since $\text{Cov}(X, \eta) = 0$, and because e_D and e_F are uncorrelated with each other and all the other variables. Hence, $\text{plim } \hat{b} = b$, and the coefficient on V_D is consistent.

Finally, recall the result from the text that the same \hat{b} will be estimated when the equation is rewritten in terms of observables:

$$\ln P = aX + bV + (b + c)V_F + \eta \leftrightarrow \ln P = aX + bV_D + cV_F + \eta,$$

since $V = V_D + V_F$. Hence, the coefficient on V will also be consistent.

⁶⁷This is the only part of the proof where the assumption that X is a vector matters. If X were a matrix, we would need to use another method to invert $Z'Z$.

⁶⁸Linearity is also assumed. This would not be a particularly restrictive assumption if the proof were generalized to the case of multiple X s. We also need the technical assumption here that all the terms have finite variances in the limit.

Appendix B. Formula allocation of housing subsidies

HUD uses a formula to allocate vouchers to different areas of the country. Allocation areas are generally MSAs (although sometimes MSAs are split at state lines, and sometimes several MSAs are grouped together). In 1990, there were 151 allocation areas. Within an allocation area, local public housing authorities compete for the area's funds, through a process akin to applying for a grant. Since the unit of observation in this study is the MSA, the details of the competitive process are not of much concern. A separate process is used to allocate vouchers to rural, non-MSA areas.⁶⁹

The voucher allocation formula is based on 1980 census data, even as late as 1993. For each formula element, HUD calculates the percent of the metropolitan total in each MSA (in 1980). For example, 1.0 percent of all the poor renter-occupied households in the metropolitan U.S. lived in the Oakland MSA in 1980. HUD then adds up these factors using the weights shown in Table 10. Using census data, these formula percentages can be calculated for all the MSAs in the sample. Until 1989, the HUD was legally mandated to allocate 85 percent of new vouchers by formula, while HUD retained the rest in a 'headquarters reserve fund.'⁷⁰ In the wake of a series of scandals, the HUD Reform Act of 1989 lowered the reserve fund to 5 percent, in order to reduce the possibility of political manipulation. The HUD Reform Act also mandated that HUD provide increased documentation of the formula-allocation process. Specifically, HUD was mandated

Table 10
Formula elements

Formula measure (refers to renter-occupied housing)	Weight
Households	0.2
Households in poverty	0.2
Poor households in pre-1940 housing	0.2
Crowded households (with more than one person per room)	0.1
Households with high rent burdens (greater than 30 percent of income)	0.2
The vacancy shortage and the long-term (2+ months) vacancy shortage (the number of vacancies required to raise the vacancy rates to 7.0 percent and 3.4 percent, respectively)	0.5/0.5

⁶⁹Legally, 20–25 percent of new vouchers must go to rural areas (24 CFR 791.403, 1995).

⁷⁰See, e.g., 24 CFR 791.403, 1988.

Table 11
Voucher stocks and flows^a

Year	Vouchers		Allocated by formula (1000s)	Allocated by formula (percent)
	Stock (1000s)	Flow (1000s)		
1988	956	65.3	48.0	74
1989	1025	68.9	46.4	67
1990	1090	61.3	49.4	81
1991	1137	55.9	41.1	74
1992	1166	62.6	18.9	30
Total	–	314	204	65

^a Sources: voucher stock and flow: Green Book, 1994. Formula allocations: 1988, 1989, unpublished HUD data supplied by Gerry Benoit; 1990, Federal Register, 55 FR 23684; 1991, Federal Register, 56 FR 2754 (also 56 FR 24290); 1992, Federal Register, 57 FR 60223 (also 57 FR 33606).

to publish the allocations annually in the Federal Register. Table 11 displays the available information on formula allocations. Note that even in 1988 and 1989, 3/4 and 2/3 of vouchers were allocated by formula, in spite of the 85 percent legal requirement. The reason is that the budget passed by Congress every year often overrides the law, and sets aside a number of vouchers for a specific purpose. Since the HUD Reform Act, formula allocation has actually become less important. During the years covered by the table, 65 percent of the vouchers were allocated by formula.

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