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Chapter Author: Harvey S. Rosen

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2 Housing Behavior and the Experimental Housing-Allowance Program: What Have We Learned?

Harvey S. Rosen

2.1 Introduction

In the United States and many other countries, attempts have been made to augment the real incomes of the poor by increasing their consumption of housing. Such schemes have taken a number of forms, for example, provision of public housing, construction subsidies, and so forth. It has been suggested that a better method than most might be to give poor people financial allowances that could be used to upgrade their housing standards. The success of such a program would depend upon the answers to several questions. Two of the most important are: Would low-income families respond to financial incentives intended to increase their housing consumption? To the extent that they do respond, would housing prices simply be driven up, resulting in windfall gains for landlords?[1]

To obtain answers to these important questions, the Department of Housing and Urban Development in 1970 authorized a social experiment, the Experimental Housing-Allowance Program (EHAP). The first part of EHAP, the "demand experiment," was designed to predict households' responses to housing allowances. In this experiment members of a random sample of low-income households were granted housing allowances and their behavior compared to a control group without allowances. The second part, the "supply experiment," was designed to

Harvey S. Rosen is professor of economics, Princeton University, and research associate, National Bureau of Economic Research.

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1. A more fundamental question, perhaps, is why housing allowances should be considered at all when direct income transfers would probably be preferable from the point of view of the poor. We will take it as given, however, that the public policy goal is to increase their welfare in some manner tied to housing consumption.

examine market effects of housing allowances. All low-income families in two communities were eligible to receive allowances, and the response of the overall level of prices in each community was carefully monitored. (The precise provisions of the programs are discussed in greater detail below.)

EHAP was not instituted in an intellectual vacuum. For years prior to the experiment, housing markets received considerable attention from economists. The purpose of this paper is to discuss what new insights EHAP has provided concerning probable responses to various types of housing allowances. Specifically, I intend to focus on what experimental data have taught us about these responses that could not have been learned from more traditional sources. This is admittedly a narrow focus, because EHAP produced a number of "serendipitous findings that had nothing to do with the research objectives used to justify them" (Aaron 1979, 48). For example, much of value appears to have been learned concerning efficient techniques for administering welfare programs. Nevertheless, the prediction of behavioral responses lies at the heart of EHAP, and it is on the basis of new knowledge about them that the experiment must be judged.

The existence of numerous studies that have used conventional data to answer questions similar to those studied in EHAP suggests a natural way to organize this paper. I will review the major problems that confronted previous investigators and for each problem discuss whether or not it has been mitigated by the availability of experimental data. I should emphasize that it is not my intention to suggest that the EHAP investigators were unaware of the fact that for some problems, experimental observations offer no particular advantage. Rather, their work has shown keen sensitivity to the limitations of their data.

The demand experiment is discussed in section 2.2 and supply in section 2.3. Section 2.4 contains the conclusions.

2.2 The Demand Experiment

The main purpose of the demand experiments was to obtain predictions of households' responses to various types of housing allowances. In this section I shall describe the experiment's structure and then discuss the problems that users of conventional data have faced in analyzing housing behavior, and the extent to which experimental data can alleviate these problems.

2.2.1 Description of the Demand Experiment

In the demand experiment,[2] a set of randomly selected low-income households received allowances, while members of a control group did

2. This subsection is based upon Allen, Fitts, and Glatt (1981), especially pages 28–30.

not. There were two basic types of allowances. Under the first, the payment received was the difference between the cost of "adequate" housing established for the program (C) and some fraction (b) of household income (Y):[3]

$$M = C - bY, \tag{1}$$

where M is the size of the payment. (C was determined by a panel of housing experts, which considered both household size and the site in making its decision.) Equation (1) is referred to as the "housing-gap formula." Under the second scheme, known as the "percent-of-rent formula," the payment was some fraction (α) of the gross rent (R) paid by the family:

$$M = \alpha R. \tag{2}$$

Essentially, the demand experiment consisted of confronting different families with various values of α, b, and C, and then comparing their housing decisions to those of the control group. In addition, some of the housing-gap households were told that their apartments had to satisfy certain minimum standards before they would be eligible for payment. For example, plumbing and kitchen facilities had to meet certain specifications; roofs, ceilings, and walls had to be in good repair, etc. (Friedman and Weinberg 1978, A-31).

In practice, values for b of 0.15, 0.25, and 0.35 were employed; the parameter α took on values that started at 0.2 and were incremented by 0.1 until they reached 0.6. C varied between 20 percent below and 20 percent above the levels set by the experts. The experiment was conducted for three years beginning in 1973 at two sites—Pittsburgh, Pennsylvania and Phoenix, Arizona. At each site about one thousand low-income families participated in the experiment,[4] somewhat under half of which were included in the control group. Only renters were eligible.

2.2.2 Problems in Predicting the Demand Response to Housing Allowances

Presumably, by appropriately comparing the responses of the control and treatment groups, one can infer the impact of the various types of allowances upon housing behavior. However, suppose for the moment that experimental data were not available and that an investigator was asked to predict the effect that allowances would have upon housing behavior. Most likely the investigator would begin by noting that the housing-gap formula is essentially an increase in income, and the percent-of-rent formula represents a change in the price of housing services from

3. The definition of "household income" was essentially posttax income less a $300 deduction for each worker in the family.

4. For example, in 1973 a Phoenix family with three or four members would be eligible only if its income were less than $8,150; for Pittsburgh, the limit was $6,250.

some price P to $(1 - \alpha)P$. Therefore, given income and price elasticities of housing demand, one can predict an individual's response to the housing allowance.[5] These considerations suggest the following strategy: Employ multiple-regression techniques (or some variant thereof) to estimate the demand for housing services, utilizing either cross-sectional or time-series data. This strategy yields a set of the relevant elasticities. Then, assuming that people would react to the price and income differences generated by a housing-allowance program in the same way as those generated "naturally," use the elasticities to estimate the program's impact on housing demand.

I shall now discuss some problems that face the investigator who wants to implement this strategy and whether or not the problems are eliminated when experimental data are available.

Specification of a Model

Users of conventional data typically begin by specifying a model that relates the quantity of housing services demanded for the i^{th} observation (Q_i^D), to some function $f(\cdot)$ of price (P_i), income (Y_i), and a vector of demographic variables Z_i, which theoretical considerations suggest might be relevant:

$$Q_i^D = f(P_i, Y_i, Z_i) . \tag{3}$$

In some cases $f(\cdot)$ is specified in an ad hoc but convenient form such as log linear (e.g., Polinsky and Ellwood 1979, Rosen 1979b), while other times it is derived from maximization of an explicit utility function (Abbott and Ashenfelter 1976).

Equation (3) is deterministic, so the next step is to assume that even observations with identical right-hand-side variables may have different Q^Ds because of random errors. Usually, an error term is appended additively. (For an exception see King 1980.) Now, given a set of observations on Q_i, P_i, Y_i, and Z_i and the stochastic specification, the model's parameters can be estimated using a variety of econometric techniques. The parameter estimates can then be used to compute behavioral elasticities;[6] indeed, in the case of log-linear demand curves, the parameter values themselves are the elasticities.

There are several major drawbacks with this standard procedure. First, economic theory puts very few constraints on the form of $f(\cdot)$, so the investigator must make an essentially ad hoc choice with respect to the specification of either the demand or utility function. Second, it must be

5. This assumes that individuals' choices are unconstrained by quality standards.

6. For example, the price elasticity of demand is $\partial f/\partial p \; P/Q$ where P and Q are (usually) evaluated at their mean values.

assumed that $f(\cdot)$ is identical across individuals.[7] (When time-series data are used, the analogous assumption is that $f(\cdot)$ does not change over time.) Finally, and perhaps most crucially, it must be assumed that the fitted relationship will continue to apply when a right-hand-side variable for a given observation changes. For example, if the investigator finds that $(\partial Q^D / \partial Y)(Y / (Q^D)$ is less than one, it does not imply that increasing a particular family's income 10 percent will increase its housing consumption by a smaller percentage. All one has really learned is that in the data, poorer families devote a larger fraction of their income to housing than richer families, ceteris paribus. Only by *assuming* that poor families would act like the richer ones if their incomes were increased, and vice versa, can one give any behavioral significance to elasticity estimates from regressions.[8]

In contrast, the situation facing the investigator with experimental data appears simple. There is no need to specify $f(\cdot)$, or to make possibly invalid behavioral assumptions. As Hausman and Wise (this volume) note, provided that the experiment is designed properly, all that is necessary is to compare the behavior of individuals in different treatment groups with each other, and with the control group. Indeed, EHAP investigators Friedman and Weinberg (1978) do exactly this. In a series of tables they exhibit information on housing expenditures for both the experimental and control groups at the time of enrollment and at two years after enrollment (see, for example, pages 8, 13, 14, A-54, A-55). Interestingly, however, only a small portion of Friedman and Weinberg's lengthy (and excellent) report on the demand experiment is devoted to discussion of such results.[9] Most of the document concerns the specification of models like equation (3) and their estimation with data from the experiment. But as Hanushek and Quigley observe, such "regression estimates . . . do not arise from experimental payments of income, but rather from the 'natural' experiment arising because 'otherwise identical' households of [e.g.] varying income are observed to have made different choices" (1979b, 20). In short, the experimental nature of the data is ignored, so that all the model-specification problems associated with conventional data must be confronted.

7. Note that this need not imply that the elasticities be identical across individuals; such will be the case only for the very simple Cobb-Douglas specification. One can also specify a random-coefficients model, which allows for a distribution of elasticities across people. See Hausman and Wise (1980).

8. This point is further developed in Mosteller and Mosteller (1979).

9. Friedman and Weinberg of Abt associates bring together a wealth of information on the demand experiment: the economic theory behind it, sample design issues, statistical analysis of the data, and more. Unfortunately, no similar major report has been issued by the Rand Corporation for the supply experiment.

Why is this the case? The main reason is the possibility that some of the key parameters that govern housing behavior depend upon variables that can change over time. For example, there is some evidence that the price elasticity of demand for housing is a function of income (Rosen 1979a). Thus, to the extent the economic environment changes, the value of simple comparisons between control and experimental groups will be diminished.[10] In contrast, a properly estimated structural model would allow an investigator to deal with such a situation.

Additional reasons are provided by Stafford's (this volume) discussion of the general circumstances under which experimental results are likely to be more useful than those from structural models. First, there must be reasonable certainty that the programs examined in the experiment are the ones that will eventually be considered by policy makers. This is because by its nature, an experiment can generate information only about the specific treatments being examined (or interpolations between them). Second, there must be some agreement on the relevant time horizon. Otherwise the experiment may not be long enough for one to observe all its effects upon the population.

The application of Stafford's criteria suggests that in the case of housing allowances, a structural approach is required. A multitude of housing programs have been considered in the past (see Aaron 1972); there is no reason to believe that society has settled into a consensus on the particular programs and parameters studied in EHAP. Furthermore, housing decisions are evidently made by families within a long-run framework, but the precise amount of time required is not known. As noted below, the problem of estimating lag lengths is not easy in structural models, but at least some interesting results have been obtained.

For all these reasons, it is almost inevitable that Friedman and Weinberg, as well as other investigators using the experimental data,[11] eventually turn to models of the kind used in the analysis of conventional data. Of course, it may be the case that there are other features of experimental data that make such models especially useful, an issue to be discussed below. But in an area like housing, they do not relieve investigators of the burden of constructing theoretical and statistical models.

Definition of Housing Services

Given that analyses of both experimental and conventional data require the construction of models, the important question becomes whether or not the experimental data better facilitate their implementa-

10. One can rescue the experimental approach from this criticism by building income-price interactions into the experimental design. However, as Hausman and Wise (this volume) point out, the more treatment groups, the less convincing are the results, ceteris paribus.

11. See, e.g., Hanushek and Quigley (1979a), Mills and Sullivan (1981), or Hausman and Wise (1980).

tion. Consider, for example, the problem of making operational the left-hand-side variable of the equation, "housing services." Housing is intrinsically a multidimensional commodity—a dwelling is characterized by its number of rooms, their size, the quality of construction and plumbing, etc. It is therefore very difficult to summarize in a single number the quantity of housing services generated by a given dwelling. Usually it is assumed that the amount of housing services is proportional to the rent paid, or, in the case of an owner-occupied dwelling, to the value of the house. (See, e.g., Polinsky and Ellwood 1979.) The difficulty here is that the rental value of a dwelling at a given time may reflect characteristics of the market that have nothing to do with the quantity of housing services actually generated. As King (1980) points out, for example, the special income-tax treatment of rental income will generally influence market values.

An alternative tack would be to abandon the possibility of summarizing housing services in a single variable, and instead to estimate a series of demand functions for various housing attributes. An immediate problem is the absence of observable market prices for attributes. Recently, Witte, Sumka, and Erekson (1979) have implemented the suggestion of Rosen (1974) that attribute-demand equations be estimated in a two-step process: (1) estimate the implicit attribute prices from a hedonic price equation for housing,[12] and (2) use these prices as explanatory variables in regressions with attribute quantities as the dependent variables. However, Brown and Rosen (1982) have shown that major statistical pitfalls are present in this procedure and that the validity of Witte, Sumka, and Erekson's results is therefore in question. Although some progress is being made in dealing with these problems (see Quigley 1982), the approach that continues to predominate is the use of rent as the single measure of the quantity of housing services.

Do the EHAP data allow the construction of more meaningful measures of housing services? The simple answer is no. Friedman and Weinberg, for example, struggle with the problem of measuring housing services in very much the same way as users of nonexperimental data (1978, 92–94). Similarly, Hanushek and Quigley's (1979a) analysis of EHAP data uses housing expenditures as the dependent variable in the demand equations. Experimental data do not remove this important stumbling block.

Price of Housing Services

Imagine an investigator with (nonexperimental) cross-sectional observations on a group of renters, all of whom come from a particular

12. A regression of the price of a commodity R on its characteristics (a vector X) is the basis of an hedonic price index for the commodity. The implicit price of the i^{th} characteristics if $\partial R/\partial X_i$. See Rosen (1974).

community. If the housing market is competitive, it seems reasonable to assume that all individuals face the same price of housing services. However, in the absence of any price variation, it is impossible to estimate the price elasticity of demand. Investigators with conventional data therefore often analyze observations across cities. Of course, the problem of measuring intercity housing-price variation still remains. Because the price of housing services is housing expenditures divided by the quantity of housing services, the above noted difficulties in measuring the latter are bound to create problems in measuring price. Several possible solutions are found in the literature. A popular approach is to estimate hedonic price equations for different cities and use them as the bases for a housing price index. However, Alexander (1975) has pointed out several problems with this approach. One of the most important is that the selection of a set of attributes to be included in the hedonic price index must be decided on ad hoc grounds, but the substantive implications of the estimates often depend upon the choice made.

The user of EHAP data has an advantage in dealing with the problem of measuring price differences across observations. Recall that in a community the effective price of housing facing the individual, P_i, is

$$P_i = (1 - \alpha_i)P_0, \tag{4}$$

where P_0 is the pretreatment price of housing and α_i is the EHAP subsidy rate (equal to zero for members of the control group). Because of the variance generated in P_i by the α_i, the fact that P_0 is identical across individuals in the community no longer precludes estimation of a price response. P_0 can be normalized at an arbitrary value and then equation (4) used as the price term. This approach is used by Friedman and Weinberg (1978) and Hanushek and Quigley (1980).

A potential problem is the possibility that the before-treatment price of rental housing may not be constant within a city. Polinsky and Ellwood (1979, 199) show that even if the market is competitive, variation in land prices within the community will lead to differences in the price of housing services.[13] However, Hanushek and Quigley (1980) argue convincingly that such differences in P_0 are unlikely to be of much importance in the EHAP samples. It seems safe to conclude, then, that the experimental data confer distinct benefits in estimating the price elasticity of demand for rental housing.[14] Ironically, the price elasticity per se is

13. If housing services include accessibility to the work place and the usual competitive assumptions hold, then the before-treatment price of housing services would be constant. But in this case, the dependent variable should be housing expenditures plus commuting costs. Note also that if owner-occupied housing were being considered (as it is in the supply experiment), an additional complication would arise because the effective price of housing services depends upon the individual's marginal federal-income-tax rate. see Rosen (1979a) or King (1980).

14. However, the value of these benefits is lessened to the extent that the program-induced price reductions are perceived as transitory.

unlikely to be of much use in designing a housing-allowance program. A percent-of-rent formula offers such attractive opportunities for mutually beneficial fraud on the part of landlords and renters that is hard to imagine it ever being implemented.

Shift Variables

Consider now the shift (i.e., nonprice) variables of equation (3). Standard theoretical considerations suggest that for income, Y, a permanent rather than annual measure should be used. Previous investigators have dealt with the problem of computing permanent income in various ways. Carliner (1973) and Rosen (1979a), analyzing longitudinal data, take an average of several years' income. Polinsky and Ellwood (1979), using Federal Housing Administration (FHA) data, assume that the FHA's estimate of "effective income" is a proxy for permanent income. Struyk (1976) uses the fitted value of a regression of income on a set of personal characteristics as his permanent-income measure.[15]

Turning now to the vector Z of other shift variables, note that investigators with conventional data have to make arbitrary decisions with respect to which ones to choose, their measurement, and how they interact with the other variables. Typical candidates for inclusion are race, sex of head of household, age, number of children, etc.

In an experimental framework, proper randomization removes the need for specifying the shift variables (Hausman and Wise, this volume). However, to the extent that structural models are required to obtain useful results, users of EHAP data are at no particular advantage when it comes to choosing shift variables and defining them appropriately. For example, Friedman and Weinberg's permanent-income measure (p. 54) is constructed using the same kind of averaging discussed above.[16] Similarly, their selection of demographic variables is made on an ad hoc basis (p. 81).

Disequilibrium

Most of the studies using cross-sectional data to examine housing demand implicitly or explicitly assume that all agents are in equilibrium.[17] Were this not the case, then a regression of housing services on price, income, and demographic variables could not be interpreted as a demand equation. On the other hand, analyses of longitudinal and time-series data often allow for the possibility that at a given point in time, house-

15. Of course, neither the necessity of using a permanent-income measure nor the types of solutions just mentioned are unique to the study of housing; they appear throughout the literature on the estimation of demand functions.

16. An additional problem arises because it is not clear how to convert the monthly EHAP payments, which are known to be temporary, into changes in permanent income.

17. An important exception is the work of King (1980), who considers rationing between different tenure modes in the United Kingdom.

holds may not be at their long-run-equilibrium positions because adjustment costs make it prohibitively expensive to respond immediately to changes in economic environment. It is usually assumed that such a disequilibrium is eliminated over time as households move gradually to their equilibrium positions (e.g., Rosen and Rosen 1980).[18] It is well-known that such models lack a strong choice-theoretic foundation, but a tractable alternative is lacking.

The equilibrium assumption is just as crucial to the analysis of EHAP data as to conventional data. Even simple comparisons of the behavior of the control and treatment groups are less meaningful unless both groups are observed in equilibrium positions. It is for this reason that Friedman and Weinberg (1978, 71) devote a considerable amount of time to separate analysis of those households that changed dwellings during the course of the experiment—movers are assumed more likely to be in equilibrium than stayers. (This, however, creates an important self-selection problem that is discussed in the next section.)

In addition, Friedman and Weinberg utilize the typical partial-adjustment model to study dynamic behavior,[19] and they find rather rapid adjustments in housing behavior (p. 125). Hanushek and Quigley (1979a) present an innovative method to estimate adjustment lags in the EHAP data, but their technique could just as well have been implemented using a conventional set of longitudinal data. Contrary to Friedman and Weinberg, they find rather sluggish adjustments: only about one-fifth to one-third of the gap between desired and actual housing consumption is closed in each year.

One aspect of the EHAP makes proper modeling of disequilibria especially important. For some treatment groups, individuals were ineligible for housing allowances unless their housing met certain quality standards. In other words, individuals were constrained to consume minimum amounts of certain housing attributes. To the extent that any of these constraints were binding, then demand functions for other attributes of the housing bundle would depend not only on prices of the attributes, but on the quantities of the constrained attributes. Estimation of attribute-demand functions in the presence of quantity constraints is clearly a complicated matter. Unfortunately, given the paucity of work on estimating attribute demands in the relatively simple unconstrained case, one cannot expect that the more complicated disequilibrium problem will be solved soon. Such work may provide an interesting use for EHAP data in the future.

18. This differs from the use of "disequilibrium" in much macroeconomics literature, where the term refers to a situation in which markets fail to clear because of some constraint(s). See, e.g., Barro and Grossman (1971).

19. Unfortunately, as Friedman and Weinberg (1978, 127) note, dynamic patterns might be affected by the limited duration of the experiment.

Selectivity Bias

In recent years econometricians have devoted a substantial amount of effort to the study of statistical problems that arise when the sample used in a regression analysis is nonrandom (see Heckman 1979). It has been shown that if selection into a sample is nonrandom, then, unless certain corrective measures are taken, parameter estimates may be inconsistent. For example, it is common to estimate separate demand equations for renters and homeowners. However, since individuals self-select into their tenure modes, the sample-selection process is not random, and inconsistent coefficients may result (Rosen 1979a). Similarly, if separate regressions are estimated for movers and stayers, sample-selection bias is a threat.

As Friedman and Weinberg (1979, 130) point out, although a random sample of low-income households was offered enrollment in the percent-of-rent plans, the demand functions were estimated from a nonrandom subsample; thus, "households that accepted the enrollment offer, were verified to be within the income eligibility limit, remained in the experiment, and moved sometime between enrollment and two years after enrollment." Each of these criteria introduces the possibility of sample-selection bias. Of course, users of EHAP data can take advantage of various statistical techniques to determine whether or not selectivity bias is present, and if so, to correct for it (Hausman and Wise, this volume). In experimental data, then, selectivity bias is not eliminated—it merely appears in new forms.

Participation in and Perception of the Program

To predict the aggregate response to a housing-allowance program, one needs to know the number of eligible families and the proportion of those who would choose to participate. Presumably at least rough information on the first item could be obtained from census or similar figures on income distribution. It is hard to imagine how nonexperimental data could be used to illuminate the participation issue. Although some conventional data sets have information on participation rates in existing welfare programs (e.g., food stamps), probably one cannot reliably infer from that data what the patterns of participation in a quite different program would be.

A related question concerns individuals' perceptions of the program. In order to use results from conventional data to predict the effect of housing allowances, one must first of all assume that people would understand the program. Furthermore, it must be assumed that percent-of-rent and (unconstrained) housing-gap payments are perceived as equivalent to price and income changes, respectively. Although one can test for rational perception of the provisions of existing welfare programs

(e.g., Williams 1975), there is no reason necessarily to expect such results to carry over to the housing-allowance case.

With respect to both the participation and perception questions, the experimental data provide interesting insights, but no definite conclusions. Clearly, EHAP investigators can observe whether or not individuals participate in the experiment and correlate participation with various economic and demographic variables. The main problem is that the results may be affected by the individuals' knowledge that they are involved in an experiment, the "Hawthorne effect." To the extent that people act differently when they know that their behavior is being observed as part of an experiment, it will confound attempts to predict participation under a universal regime.[20] An additional difficulty is that participation rates may be affected by the knowledge that the program is only temporary.[21]

Friedman and Weinberg (1978) attempted direct investigation of the perception issue. Families in the percent-of-rent experiments were asked in what direction their housing allowances would move if their rent were increased by $10. Only about a half understood that their allowance would increase. However, when separate demand functions for both those who understood and those who did not were estimated, the hypothesis that their parameters were the same could not be rejected. Friedman and Weinberg (1979, 139) conclude that, even for persons who answered the question incorrectly, "their response to the allowance payment can be analyzed *as if* they understood."

A more convincing test would have been possible if there was variation in the pretreatment price of housing services. Suppose that the effective price P_i appears in logarithmic form on the right-hand side of the demand equation. Note that

$$\ln P_i = \ln(1 - \alpha_i) + \ln P_{0i},$$

where P_{0i} is the pretreatment price and α_i is as defined above. Thus, if $\ln(1 - \alpha_i)$ and $\ln P_{0i}$ are entered separately into the regression, a natural way to confirm correct perception is to test whether or not their coefficients are equal. Equality would suggest that individuals perceive treatment-induced changes in price the same way as those "naturally" induced. The advantage of such a test is that it does not rely on a direct question addressed to the participants. Unfortunately, as noted above, in the EHAP samples there is probably not enough variation in the pretreatment prices to make an attempt to calculate them worthwhile.

20. Of course, Hawthorne effects can be used to bring into question the results generated by all social experiments.

21. Participation was probably also influenced by the existence of minimum-housing standards. Some critics of EHAP have claimed greater variation in these standards would have provided useful information on the extent to which they influenced participation. See Downs and Bradbury (1981).

Another way to examine the perception issue would be to compare parameter estimates of structural models generated by data from different programs in the experiment (and the control group). If selection into the various groups were random and if individuals perceive program parameters correctly, then the underlying behavioral parameters should be about the same. Of course, to the extent that the particular specification of the structural model influences the results, they are rendered inconclusive.

2.3 The Supply Experiment

In most analyses of housing demand using both conventional cross-sectional and EHAP data, it is assumed that the pretreatment price of housing is constant. In effect, each household faces a perfectly elastic supply of housing services. From an econometric point of view, this assumption is justified because each household is sufficiently small to be regarded as a price taker.[22] However, sole reliance on such demand estimates to predict the overall behavioral response to housing allowances is potentially hazardous. If a considerable number of program participants increase their demand for housing services, then to the extent the supply of housing services to the community slopes upward, the pretreatment price will rise.

Considerations such as these led to the so-called supply experiment. In two communities, all individuals who met certain income qualifications were made eligible for housing allowances. The idea was to see whether or not the allowances would induce increases in prices or any other important disruptions in the housing market.[23]

In this section I shall summarize the provisions of the supply experiment and then, as before, discuss whether or not EHAP data provide substantial improvement over those from conventional sources. As might be expected, many of the issues that were important on the demand side are also present here. Such issues therefore receive only cursory discussion.

2.3.1 Description of the Supply Experiment

The supply experiment began in 1973–74, with a planned duration of ten years.[24] In the two sites chosen, Green Bay, Wisconsin and South Bend, Indiana, enrollment in the program was open to every eligible household. All payments were made according to the housing-gap formula, equation (1), with b, the implicit tax rate on income, set at 25

22. For many homeowners, the federal income tax generates an endogenous price for housing services.

23. Barnett and Lowry (1979, 10) discuss some predictions of the market effects of housing allowances that were made prior to EHAP.

24. This subsection is based upon Allen, Fitts, and Glatt (1981).

percent. In order to qualify for the payments, housing had to meet certain minimum standards. Unlike the demand experiment, homeowners as well as renters were allowed to participate. Perhaps the key methodological difference between the demand and supply experiments is that for the latter, there was no control group.

After four years of observation at both sites, it became clear that "the experimental program . . . had virtually no effect on housing prices, either marketwide or in the market sectors most heavily populated by program participants" (Barnett and Lowry 1979, 1). There are two principal explanations for this phenomenon: (1) because the income elasticity of demand for housing services apparently is quite low for program participants (about 0.3 for renters, according to Mulford (1979, 31),[25] the housing allowance did not shift the market-demand curve very much; and (2) the demand changes that did take place were spread out over time due to adjustment lags. Since both of these phenomena were observed in the demand experiment, some critics (Downs and Bradbury 1981) have argued that the supply experiment should not have commenced until the demand results were in. Nevertheless, it is useful to assess the benefits that the availability of experimental data will confer upon future researchers of housing supply.[26]

2.3.2 Problems in Predicting the Supply Response to Housing Allowances

Specification of a Model

Investigators who want to estimate housing-supply functions generally begin by trying to use economic theory to specify an estimable model. A popular approach is to assume some housing-production function, estimate its parameters, and use them to infer the shape of the supply function.[27] For example, Ingram and Oron assume that housing services are a constant elasticity of substitution (CES) function of "quality capital" and "operation inputs" (1977, 284). Polinsky and Ellwood (1979) also posit a CES production function, but assume that its arguments are land and capital. Field (n.d.) uses a transcendental logarithm production function with three inputs—land, capital, and labor. Poterba eschews selection of a specific form for the production function, and instead starts by postulating a supply function that is log linear in the price of housing, input costs, and credit availability (1980, 10). (Of course, duality consid-

25. In addition, only about half the eligible renters and 30 percent of the eligible homeowners had enrolled after four years (Allen, Fitts, and Glatt 1981).

26. Several researchers have used data from the supply experiment to estimate demand for housing schedules, e.g., Mulford (1979). These will not be discussed here.

27. Given the production function and input prices, one can derive the marginal-cost schedule which, under competition, is the supply curve.

erations suggest that one can work backward from the supply curve to the underlying production function.)

The specification of the underlying technology can sometimes predetermine substantive results. For example, since Ellwood and Polinsky assume constant returns to scale (1979, 201), the implied long-run supply curve of housing services is perfectly elastic, regardless of parameter estimates.[28] Postulating such a technology, then, guarantees the result that housing allowances will have no effect on the pretreatment price of housing, at least as long as input prices remain unchanged. The interesting questions then become: How high do prices rise in the short run? How much time is required to reach long-run equilibrium? These issues are discussed below in the section on dynamics; they are mentioned here to emphasize once again the importance that model specification plays in analyses of conventional data.

The presence of the supply "experimental" data does not remove the necessity for some kind of modeling, particularly since there is no control group. Barnett, for example, provides some simple comparisons of the increase in rents in the test sites relative to those in other U.S. cities (1979, 13). Even such relatively straightforward comparisons, however, require an implicit model of the determinants of housing costs, so that "other" costs can be subtracted out to find the "pure" housing-allowance effect. Rydell (1979) constructs a rather involved model of monopolistic competition in housing markets in order to assess the market impact of allowances. He simulates the model with experimental data, but this could have been done just as well with numbers from conventional sources.

Defining Housing Services and Their Price

The problem in defining housing services and their price are of course as central to supply as demand. Those studying the supply of housing with conventional data have made exactly the same sort of assumptions in constructing their price and quantity variables. (See Poterba 1980, Ingram and Oron 1977, or Rothenberg 1977.)

In this regard, the numbers generated by the supply experiment are no better than conventional data. Indeed, the difficulties associated with the multidimensional nature of housing are particularly vexing here, because one of EHAP's mandates was to find out what combination of rehabilitation of existing units, construction of new units, and improvement of neighborhood quality would be induced by housing allowances (Allen, Fitts, and Glatt 1981). To answer this question, one would need to

28. The assumption of a horizontal supply curve is quite common, e.g., see de Leeuw and Struyk (1975, 15). Of course, to the extent that input prices change with the size of the housing industry, the long-run supply curve will have a nonzero slope.

quantify these attributes, compute their implicit prices, and then estimate supply curves for each. As noted above, researchers have still not solved completely the problems associated with estimating demand and supply schedules for characteristics, and nothing about experimental data per se makes this task any easier.

Shift Variables

In a competitive model, the supply of housing services depends not only upon their own price, but upon input prices as well, so these are important shift variables. Housing studies using conventional data face serious difficulties in obtaining operational measures of housing-input costs. For example, Poterba (1980) uses the Boeckh index of the price of inputs for a new one-family structure to measure construction costs. Although this index is commonly used, it is well known to be deficient because fixed weights are used in its computation. Ingram and Oron (1977) use the fuel component of the consumer price index to account for the price of all operating inputs, but as Rothenberg (1977) points out, it is not clear that this index captures all the needed information.

With respect to measuring the prices of housing inputs, the experimental data provide no particular advantage. For example, Rydell (1979, 36) must make calculations regarding the costs of components of gross rent similar to those who use conventional data. It should be noted, however, that these computations appear to be some of the most careful available.

Disequilibrium and Dynamic Issues

As suggested above, many models of housing supply begin with a production function that exhibits constant returns to scale in the inputs. Given this specification, and assuming constant input prices, the long-run supply of housing services is infinitely elastic. Thus, any demand shift induced by a housing allowance will leave unchanged the long-run price of housing services. However, the question of supply response is still interesting, because the production function does not indicate the length of time required to reach long-run equilibrium or the path of prices during the transition. To understand the supply response, it is crucial to model both the process of adjustment to the new equilibrium and the presence of any factors that might impede the market from achieving equilibrium.

Thus, for example, in one of their models Ingram and Oron (1977, 292) assume that the most a landlord can invest each period is limited to the amount of cash generated by the existing investment, even if this amount is insufficient to close the gap between the desired and actual housing stock. Poterba (1980) argues that conditions in the credit market may affect the supply of housing, and he proxies these by the flow of savings

deposits received by savings and loan associations. Poterba also assumes a delayed supply response to changes in all right-hand-side variables that are entered in polynomial distributed lags (p. 10).

The designers of the supply experiment clearly were aware of the importance of lags in the housing-supply process, as witnessed by the fact that the experiment was given a ten-year duration (although only five years' worth of data were collected). Because there was no control group, however, no simple comparisons can be made in order to learn how movements toward the final equilibrium take place. My guess is that even if there had been a control group (call it "South Bend Prime"), structural models would still be more useful than experimental comparisons for determining the lag structure. By the time a decade had lapsed, it is possible that a number of variables that influence adjustment patterns would have changed, so comparisons of South Bend and South Bend Price would not be very informative.

Market Environment

In the demand experiment it was unnecessary to study market environment, since the key question was how micro-units reacted to exogenous changes in their budget constraints. But to understand overall effects, the question of market structure is crucial—the impact of the housing allowances on pretreatment price clearly will depend mutatis mutandis upon the degree of competitiveness in the market, the amount of slack existing when the program is initiated, the extent of housing-market segmentation, etc.

The standard assumption is that competition prevails. As de Leeuw and Struyk (1975) and Poterba (1980) note, however, even given competition, complications arise because two markets have to be equilibrated by the price of housing services: the market for existing houses and the market for new construction. The situation is even more complicated when one takes into account the multiplicity of tenure modes. Each type of housing is traded in its own submarket, and each of these (interrelated) markets has a market clearing price. If the housing market is noncompetitive, the question of supply effects is even more difficult because of the absence of a generally accepted theory of price determination. Theoretically, one can imagine examining a group of cities that are identical except for housing-market structure and comparing the results when they are subjected to housing allowances. (Indeed, something of this notion was behind the selection of Green Bay and South Bend as the experimental sites.) In practice, such a course would be prohibitively expensive, even if it were possible to find an appropriate group of cities. Again, construction of structural models appears to be the more viable methodology. For example, using data from the supply experiment, Rydell (1979) attempts to explain the insensitivity of housing prices to

apparent variations in market tightness by recourse to a theory of monopolistic competition. This approach is interesting, but the availability of experimental data provides no special advantage when it comes to testing its validity.

2.4 Conclusion

The Experimental Housing Allowance Program has generated a rich and valuable set of data on the housing behavior of lower-income Americans. These data appear to have been analyzed carefully and creatively by the EHAP investigators, although doubtless their conclusions will be challenged as the numbers are studied by other investigators.[29] The issue discussed in this paper is the extent to which the experimental nature of these data per se enhances their value. Specifically, are the problems faced by investigators, who have used conventional data to predict behavioral response to housing allowances, in any way mitigated by the availability of experimental data?

With the possible exception of experimentally induced variations in housing prices, it seems that the experimental data offer no particular advantages. Fundamentally, this is because housing behavior is so complex and the policy environment so uncertain that simple comparisons of experimental and control groups are unlikely to be of much interest. Rather, the data must be interpreted with the help of theoretical and statistical models. Thus, if the goal was to obtain new and improved estimates of the behavioral response to housing allowances, a social experiment was not necessary. The money would have been better spent on augmenting conventional data sources.[30]

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29. For example, Mills and Sullivan (1981) have suggested that problems with econometric technique lead the EHAP investigators to underestimate income elasticities from the demand experiment.

30. A similar conclusion is reached by Hanushek and Quigley (1979b, 68).

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Comment

John M. Quigley

Introduction

Harvey Rosen's paper provides a compact overview of the Housing Allowance Demand (HADE) and Supply (HASE) experiments; it focuses upon the extent to which the experimental nature of the longitudinal data collected enhances their value in scientific research. The Rosen analysis is well written and argued, and it is hard to disagree with his principal conclusions. I will begain by summarizing the conclusions of the Rosen paper, perhaps emphasizing his points in a slightly different way. I will then extend the discussion to include several important issues that the author chooses to ignore.

Rosen's review considers six of the seventeen treatment groups in the demand experiment and the single treatment group that comprises the supply experiment.

One of these groups—the unconstrained-housing-gap treatment—is simply a negative income tax. Imposition of this program nationally would adjust individual payments to reflect the cost of "standard" housing in each market, a good proxy for regional variations in the cost of living.

The other five HADE groups—the percent-of-rent treatments—are pure price reductions of various percentages.

The HASE treatment is a negative-income-tax schedule offered conditional upon the physical characteristics of housing chosen by recipients.

The principal conclusions of the author are three:

1. The experimental feature of the data obtained from the HADE unconstrained-housing-gap treatment group provides no additional evidence on the income elasticity of rental-housing demand.
2. The experimental features of the data generated by the HADE percent-of-rent groups "confer distinct benefits" in estimating the price elasticity of demand for rental housing.
3. The data obtained from the HASE treatments provide no particular advantages for the analysis of housing supply.

John M. Quigley is professor of economics and public policy, University of California, Berkeley.

The Experimental Data

HADE households in the unconstrained-housing-gap treatment group received transfers of varying amounts of additional income according to a single negative-income-tax formula for three years; households in the five percent-of-rent treatment groups received rent rebates of varying fractions (0.2, 0.3, 0.4, 0.5, and 0.6) for the same period. A large body of information was initially gathered about each household and its housing consumption; each household was re-interviewed and an identical body of data was gathered after six months, after one year, and after two years of program operation.

The Unconstrained-Housing-Gap Treatment

First, consider the problem of estimating the income elasticity of housing demand. Ever since Margaret Reid's (1962) revisionist analysis of the relationship between housing and income, the appropriate definition of income has been problematic. Clearly, the choice of a particular dwelling unit (an owned or a rental unit) is based upon a time horizon for consumption which is "long." The search, moving, and transactions costs associated with residential choices are large; presumably, these frictions play the same role as those associated with the purchase and sale of other durable goods. It is not helpful merely to state that these consumption choices are made on the basis of "permanent" income, at least not when confronting observations on individual households.

A number of researchers have used different, plausible, but essentially ad hoc methods to compute "permanent" income from data on individual households so that the income elasticity of demand can be estimated in a subsequent step.[1]

Consider those households receiving cash transfers; they receive thirty-six monthly payments after which their income reverts to its unsubsidized level. How is this stream to be converted into a change in permanent income so that it can be related to housing consumption?

If the problem were merely to establish the partial relationship between reported monthly income and contemporaneous housing consumption, the body of experimental information would suffice.[2] Note, however, that any cross section of households without any experimental payments would be equally adequate.

To take housing allowances seriously as a national program, however, knowledge of this partial relationship would never do. Some estimate of the long-run effect of these payments upon the housing consumption of

1. Many of these are reviewed in Quigley (1979).

2. Subject, as Rosen notes, to an assumed specification, an assumed functional form, and an assumption that within each market unit, price of housing services is constant.

poor households is required. Ironically, in the absence of a specific theory of permanent income, observations on a particular stream of known transitory payments might be a disadvantage in estimating the permanent response to permanent housing allowances.[3]

Joseph Friedman, one of the principal Abt researchers analyzing the demand experiments, estimates that the additional cost of providing lifetime guarantees of transfers to the (lucky) participants in HADE would have been about $10 million (or roughly 6 percent of EHAP program costs).[4] Had these transfers been truly permanent, it would have been possible, at least in principle, to isolate the housing-consumption response to a change in permanent income.

The Percent-of-Rent Treatments

Second, consider the problem of estimating the price elasticity of housing demand, where market observations on housing are in price-times-quantity units. Analysts have pooled observations on households across markets (SMSAs) and used SMSA housing price indexes (i.e., BLS average prices) in regressions to estimate the price elasticity of consumption. Unfortunately this procedure leads to biased estimates of the price-elasticity term (see Polinsky 1977 for details). Other analysts (Muth 1971; Polinsky and Elwood 1979) have employed an ingenious, if highly suspicious, method to derive unit prices for housing which vary within a single market. The method involves estimating the parameters of a housing production function (CES) and using exogenous information on the variation in unit prices and quantities of land to infer variations in unit costs, and hence output prices, for housing. Estimated housing prices are then used in a subsequent analysis to infer the price elasticity of demand.

Neither of these procedures is very satisfactory, but observations generated by the market do not isolate variation in housing prices, only in housing expenditures.

Thus observations generated by the HADE percent-of-rent experiment are extremely valuable. Each household faces housing prices of $P_0(1-\alpha)$, where P_0 is the initial market price and α is the fraction of rent forgiven by the experimental treatment. If P_0, the initial price of housing, is constant but unobserved, then experimental variation in α permits estimation of the partial relationship between housing prices and housing consumption. This body of experimental data is the only evidence available that includes direct information on variations in the price of housing facing individual decision units. The most important problem in utilizing

3. At least researchers have analyzed empirically the problems of inference when income variations arise "naturally."

4. From a discussion of the EHAP research design at the Brookings Conference on Housing Allowances, November 1979.

this information is the transitory nature of the price reduction—rebate offers of α percent of monthly rent are made for only thirty-six months. Again, for (allegedly) small additional resources, this offer could have been extended indefinitely.

The Supply Experiment

Consider the data gathered from the supply experiment. A single tied-subsidy schedule is offered to each household that meets specific income criteria in two metropolitan areas. Detailed longitudinal observations are gathered on a sample of dwelling units, their occupants, and the behavior of their landlords.

What information do these data provide about the supply elasticity of housing services? The analytical problem is to identify the impact of an experimentally induced injection of demand upon the price of housing in the metropolitan area. To infer the effect of housing allowances upon housing supply requires that the experimental treatment be isolated from other factors—net inmigration, household formation, changes in input prices—that affect the supply of housing services according to some model or set of maintained behavioral hypotheses. Since these factors operate at the market level, it is difficult to see how any ingenious estimation strategy would disentangle them when observations are gathered from only two markets.

As Rosen reports, after four years of observing housing prices at both sites, there was little or no change in the relative price of housing at either site. From this fact, the HASE researchers infer "the experimental program . . . had virtually no effect on housing prices." Perhaps. But suppose that the relative price of housing had declined at either site—a possibility not logically excluded by anything in the experiment. What inference would be drawn by the HASE analysts?

In my view, the design of the HASE "experiment" and the organization of its principal reports makes it apparent that HASE can best be considered a "demonstration" from which a large body of behavioral data were gathered. The operation of a "real" housing-allowance program in two metropolitan areas provides a wealth of information useful in designing a national program, in estimating the likely administrative and monitoring costs, and so forth. (Some of the useful results of the demonstration are noted by Aaron 1981.)

In considering the political economy of a housing-allowance program, it is clear that if average housing prices had risen appreciably in Green Bay and South Bend, a universal housing-allowance program would be "dead." However, even if housing prices had risen at the HASE sites, it would not have been possible to conclude that the relevant housing-supply curve would be inelastic if a universal program were adopted—at least not on the basis of any model presented by the HASE researchers.

Inferences from Longitudinal Data

As a body of data on microeconomic behavior, the information generated by the EHAP program is unprecedented. The demand experiment provides multiple observations on individual households receiving transfers under seventeen different subsidy schedules, as well as a control group of households unaffected by the program. The supply experiment provides a rich body of information about particular dwelling units, their landlords and occupants over time. No other body of information contains longitudinal observations of demanders and suppliers of housing services within any single market.

The unique scientific advantage of these data lies as much in its panel design as in its experimental emphasis. The very existence of this rich body of information raises fundamental questions about the short-run dynamic behavior of economic agents.

As noted previously, decisions about changes in housing consumption are subject to substantial transaction costs—the time and out-of-pocket costs of searching and evaluating alternative units, the costs of moving household possessions, and the psychic costs of relocation. All these factors suggest that the quantity of housing services consumed by any household at any instant may deviate substantially from its "equilibrium" level—at least if equilibrium is defined as the quantity chosen given current prices, demographic characteristics, and income.

The concept of permanent income as applied to housing consumption is consistent with the instanteous "disequilibrium" between the current consumption and the current characteristics of households in the local market.

Theory says very little about the magnitude of adjustment lags in this (or any other) market or about the pattern of dynamic adjustment to changed circumstances. The dynamics of microbehavior in this market are, however, of real importance in interpreting the experiments and in evaluating their results.

Consider the percent-of-rent households. In response to a (permanent) reduction in housing prices, it is reasonable to presume that households desire to consume *some* additional quantity of housing services. Alternatively, the "disequilibrium" between current and desired housing services is increased when the price reduction is offered. Additional consumption implies moving, and only when the capitalized utility difference between current and desired housing consumption exceeds the utility costs of moving will an adjustment in consumption be made. Even then, there is likely to be some time lag before any adjustment is actually observed. The percent-of-rent experiment provides information on households at four points in time during twenty-four months of experimental treatment. Is it reasonable to presume that the entire effect of a price reduction is observed after two years? If it is, then the simple cross

tabs or the more elaborate analyses of Friedman and Weinberg (1978) suggest a price elasticity on the order of -0.2. Is it reasonable to estimate the dynamic response pattern and the long-run price responsiveness simultaneously? If it is, then the simple stock-adjustment model of Hanushek and Quigley (1980) yields estimates that are, in the long run, two to three times larger numerically.

This is not to argue that one or the other of these results is preferred on methodological grounds. The different results do indicate, however, that the interpretation of the experimental "facts" is heavily dependent upon a model of the short-run dynamics of the market. With only a short time series and with at most only four observations on each individual household, it is difficult to estimate the asymptote of any assumed dynamic adjustment pattern with confidence. In fact, it does not take a very complex assumption about market dynamics to render the problem logically intractable.

The same difficulties in interpretation apply to each of the other treatment groups in the experiments.

The Constrained Households

Rosen's paper emphasizes the utility of the experimental evidence to estimate three parameters familiar to economists: a price and income elasticity of demand and a price elasticity of supply.

If households are highly sensitive to housing-price incomes in their consumption choices, a national percent-of-rent (housing-gap) program would increase the housing consumption of selected households. If households are relatively insensitive to prices and incomes, either program would reduce the high "rent burdens" that they face.

Clearly much of the popular and political appeal of housing allowances focuses on the provision of inducements to households to consume "adequate" housing. Indeed, as Rosen notes, it is unlikely that any percent-of-rent program would be implemented nationally—landlord-tenant collusion would then be profitable.[5]

Concerns about adequate housing (whether they arise from so-called externalities or simply paternalism) are reflected in the design of the HASE and HADE programs. All of the payments offered in the supply experiment were conditional upon verification that the recipient household was living in "adequate" housing, where adequate is defined programmatically. For five of the HADE treatment groups, housing-gap (i.e., negative-income-tax) payments were conditinal upon similar verification (but according to slightly different program definitions); six other HADE treatment groups offered payments conditional upon some mini-

5. It should be noted, however, that similar incentives exist in the food stamp program. In addition, there was no evidence of collusion among the eight hundred households and landlords participating in the percent-of-rent experiments.

mum rental expenditure. Overall 53 percent of the experimental households at both HADE sites waere assigned to treatments where subsidies were conditional upon verification of the physical condition of dwellings or minimum rental payments; 41 percent were assigned to unconstrained percent-of-rent treatments and only 6 percent to unconstrained-housing-gap treatments (see Allen, Fitts, and Glatt 1981).

The Ambiguous Evidence

In the absence of very rich models of consumer behavior (and, as noted below, in the presence of very small sample sizes), it is difficult to draw strong inferences from the housing-gap experiments where payments were tied to verification of physical standards.

For those households offered transfers subject to minimum rental payments, the sample sizes, after two years of the experiment, are reasonably large. Using comparisons with households in the control group, the HADE researchers estimate the effect of transfers-conditional-upon-rent- payments upon household rental expenditures. Further, in a series of complex analyses (Friedman and Weinberg 1979, chapter 6), they conclude that recipients of these transfers may have shopped less efficiently for housing services than control households.

The analysis of households receiving housing-gap transfers, according to different schedules, conditional upon the physical characteristics of their housing, is much more problematic.[6] In part, the HADE analysis is constrained by small sample sizes and low participation rates. For example, in Pittsburgh, 87 households in the minimum-standards treatment group met those standards after two years. However, 38 of those households met the standards initially, leaving 49 who were induced to meet the standards, at least in part, by the experimental payment offer. However, these offers were made under five different payment schedules. The difference in payments between the least-generous and the most-generous payment schedule is 183 percent.

More important than small numbers in interpreting the HADE analysis is the behavioral reason for small samples of program participants under the constrained treatment groups. Many of the comparisons reported are between participants and control households, or between moving households and movers in the control group. Presumably the analytical samples are selected on the basis of stronger tastes for housing consumption. This selectivity bias is clearly recognized by the researchers, and heroic efforts are made to "triangulate" on the "true" treatment effects by presenting parameter estimates for many different subsamples of households.

Nevertheless, it is difficult to interpret these estimates in the absence of a unified theory of household participation and household consumption.

6. This issue is discussed in detail in Hanushek and Quigley (1981).

Some Behavioral Models

For 53 percent of the households in the HADE treatment groups, payment offers were tied to specific adjustments in housing consumption—adjustments that had not been made in the absence of the experimental bribe. It is clear that in many circumstances the rational household may maximize its welfare by declining the payment and its associated strings. Unfortunately, neither Rosen's discussion nor any of the analyses of the constrained households pays much attention to the welfare economics of the choice problem solved by these low-income households. In response to the experimental offer, households choose program participation and levels of housing consumption; an economic model of behavior must address jointly the program-participation decision and the housing-consumption decision of those invited to enroll in the constrained treatment groups.

Each of the tied offers presents the potential recipient with a distorted and more complex opportunity set or budget constraint. If it is assumed that there exists a single index of housing services and that initial housing prices are constant in the market, then the minimum-rent treatment groups face a linear, but discontinuous budget constraint. If it is assumed that separate indexes exist for "housing-standards goods" and "other housing goods," and that initial prices are constant, then the minimum-standards treatment groups face a budget plane (in their choice of standards, other housing goods, and nonhousing goods) that is discontinuous. It is clear, moreover, that in the absence of these convenient assumptions, the budget constraint need not be linear in its traces; but it surely is discontinuous.

A microeconomic model of the participation and housing-consumption decision must use market information somehow to trace the shape of the indifference curve between housing and nonhousing goods or between standards components, other housing, and nonhousing goods.

Hausman and Wise (1980) have analyzed the choice problem when households face a budget constraint that may be nonlinear and discontinuous, but that can be represented by pricewise continuous functions; their methodology has been applied to the data from the HADE percent-of-rent households gathered after one year of experimental treatment. This methodology estimates the curvature of utility functions between "housing services" and other goods from the revealed participation and consumption decisions of the experimental households.

Assume a single linear price index of housing services, normalized to a value of one, so that rental expenditures R measure service consumption. Assume that household preferences can be represented by:

$$U = (Y - R)^{1-\beta} R^{\beta}, \tag{1}$$

where Y is income and β is a function of household sociodemographic characteristics X and a random error n, i.e.,

$$\beta = X\delta + n. \tag{2}$$

If income is not a determinant of β, then equation (1) is merely Cobb-Douglas in two goods.

More generally, maximizing utility implies

$$R = \beta Y + \epsilon = (X\delta)\ Y + \mathrm{n}Y + \epsilon \tag{3}$$

where ϵ is a stochastic error.

Consider a household assigned to the minimum-rent treatment, i.e., a household i offered a cash transfer Δ conditional upon rental expenditures of R^*.

The household will accept the transfer and will participate in the program if its taste for housing is large enough, that is, if its β_i is greater than the highest value, $\bar{\beta}$, at which it would be indifferent.

$$(Y_i + \Delta - R^*)^{1-\beta_i} R^{*\beta_i} > (Y_i - R)^{1-\beta_i} R^{\beta_i},\ \beta_i > \bar{\beta}. \tag{4}$$

Participating households will have housing expenditures of

$$\tilde{R} = \beta_i\ Y_i + \epsilon_i > R^*. \tag{5}$$

Analogously, a household will decline the transfer and will not participate if its taste β_i is less than the smallest value, $\underline{\beta}$, at which it would be indifferent.

$$(Y_i - R)^{1-\beta_i} R^{\beta_i} > (Y_i + \Delta - R^*)_{1-}{}^{\beta_i} R^{*\beta_i},\ \beta_i < \underline{\beta}. \tag{6}$$

Nonparticipants incur housing expenditures of

$$\tilde{R} = \beta_i\ Y_i + \epsilon_i < R^*. \tag{7}$$

Thus for a sample of households in the minimum-rent treatment group, the likelihood function for the rental expenditures observed depends upon ϵ_i and β_i (for example, we will observe $\tilde{R} > R^*$ if $\beta_i > \bar{\beta}$ and $\epsilon_i = \tilde{R} - \beta_i\,[Y + \Delta]$).

Assumptions about the distribution of ϵ_i and about the distribution of β_i permit the function to be maximized and the parameters δ to be estimated.

Estimates of δ indicate the distribution of housing preferences across sociodemographic groups and the full response of individual households to the minimum-rent treatment.

The procedure is general enough to address choices made in response to any discontinuous monotonic budget constraint regardless of its shape. Indeed, it was first applied to the nonlinear case (Burtless and Hausman 1978).

The Hausman-Wise model may not be appropriate, however, for analyzing the responses to treatments tied to consumption of housing standards. At least two other approaches may be employed to estimate the curvature of indifference surfaces and hence to analyze jointly participation and consumption decisions. The first emphasizes the nonlinearity of prices for continuous-housing attributes. The second, and better known, emphasizes the discrete nature of housing choices.

First, assume that the m characteristics of housing standards (Z) and the n other housing characteristics (H) are priced jointly according to some hedonic rule,

$$R = f(H,Z), \tag{8}$$

Where H and Z are continuous and differentiable. Households of income Y are assumed to have preferences over these components of housing as well as other goods, X, whose price is normalized to one.

Maximizing utility

$$U = g(H,Z,Y-R), \tag{9}$$

subject to the nonlinear budget constraint

$$Y = f(H,Z) + X, \tag{10}$$

means that housing prices are jointly chosen with housing commodities. Nevertheless, utility maximization implies a set of $i = 1,2,\ldots,m+n$ first-order conditions of the form

$$f_i(H,Z) = U_i(H,\ Z,\ Y-R). \tag{11}$$

Joint estimation of the set of first-order conditions is possible as long as f is nonlinear and exogenous, at least for many forms of the utility function. Empirically, estimation may proceed by determining the parameters of the hedonic function, using Box-Cox or some other best-fitting procedure. The derivatives of the estimated function, evaluated at the (H,Z) chosen by each individual, become the dependent variables in the estimation of the parameters of utility functions.[7] These parameters would permit inferences to be drawn about the participation and consumption decisions of households offered tied subsidies.

The second technique would estimate the parameters of a stochastic utility function of the form

$$U = g(H,Z,Y-R) + \epsilon. \tag{12}$$

These parameters could be estimated directly from observations on the dwelling unit chosen by each household in the treatment group and on a

7. For example, assuming the utility function is GCES (Murray 1975), Quigley (1982) provides an example.

sample of the rejected alternatives using the general model suggested by McFadden (1977).[8]

Conclusion

Inferences about the behavior of households offered subsidies tied to minimum rental payments or tied to the consumption of housing meeting minimum standards must address the decision to participate in the program as well as the housing-consumption decision, given participation. Since the opportunity sets of experimental subjects are distorted in peculiar ways by these treatments, an analytical technique that investigates the curvature of indifference surfaces seems necessary. Using somewhat different assumptions, the three strategies outlined above integrate the consumption and participation decision.

The first strategy has been employed by Hausman and Wise to analyze the minimum-rent households. The second strategy was discussed by Murray (1978) in the context of housing allowances. The third strategy is, in principle, a straightforward application of models successfully applied in other markets.

It should be noted, however, that even if the form of the utility function were known with certainty (on the basis of these or other investigations), the short duration of the experiment would still make inferences about the "long run" problematic. The effect of long-term tied subsidies upon the long-term consumption behavior of low-income households is difficult to infer from a very short time series.

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8. The most straightforward approach would be to assume that g is linear in its parameters and the ϵ's are i.i.d. weibull and to employ a sampling rule with the so-called uniform conditioning property to generate observations on rejected alternatives for each household. See McFadden (1974, 1977).

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Comment

Gregory K. Ingram

In his paper Harvey Rosen focuses on whether or not experimental data from the housing-allowance experiments have helped investigators resolve a number of analytic and empirical difficulties that plague the analysis of both the supply and demand sides of the housing market. He concludes that the experimental data have not been particularly helpful. His is a good summary of these issues, and I concur with his views. Rather than review his points directly, I shall take an approach, which I hope will be complementary to his, that focuses on a slightly different question: What have we learned from the housing-allowance experiments that can help us predict what the costs and effects of a full-scale national housing-allowance program would be, and could we have learned these things from nonexperimental data? The discussion of this question is organized around three subheadings: What would a national program cost? How would it affect housing consumption? How would it affect housing markets?

Program Cost

The total cost of a national housing-allowance program would be one of the crucial determinants of its adoption. Calculating these costs requires three pieces of information: the transfer payment and administrative cost per type of eligible household; the participation rate of each type of eligible household; and the total number of each type of eligible household (Khadduri and Struyk 1980). The payment for each type of eligible household would depend on the payment formula, household characteristics, and perhaps local market parameters. Data from housing-allowance experiments would not be particularly relevant. The experiments also provide estimates of administrative costs; these could arguably have come from nonexperimental sources.

The participation rate of households in the program, which is a crucial determinant of program costs, would be heavily dependent on data from the experiments. The experiments have revealed that the participation rates of eligible households in the experimental program were much lower (at 30 to 50 percent) than those of the AFDC programs (around 80 percent) that seemed to have been used as guidelines for the design of the housing-allowance experiments. Average participation rates for renters were 27 percent for the demand experiments, 42 percent for the supply experiments, and 53 percent for the administrative agency experiments, while they were 33 percent for owners in the supply experiment (Struyk and Bendick 1981, chap. 4). In fact, as Rosen states, these unexpectedly

Gregory K. Ingram is director, development research department, the World Bank, Washington, D.C.

low participation rates have been one of the factors that have compromised the success of the supply experiments. The experiments suggest that the variation in participation rates depends on several factors including the magnitude of benefits offered to eligible households (higher benefits raise participation); the level of standards used to inspect and approve dwelling units (stricter standards lower participation); the amount of "outreach" used (more promotion increases participation); tenure (owners participate less than renters); and market conditions (tighter markets may have lower participation). Although these factors seem to affect participation, it is not clear that the experiments have produced reliable models that can be used to predict participation rates as a function of the foregoing factors. Moreover, if a "treatment" other than one used in the experiments were to be evaluated, some kind of model or interpolation procedure would have to be used to predict participation rates.

Could we have learned about participation rates using nonexperimental data? Certainly many persons involved in designing the experiments were surprised at the low participation rates. Perhaps the experimental rates are biased downward. It is possible to argue that the true participation rates of a national program would be higher than those experimentally observed simply due to the temporary nature of the experiments. However, the results of the supply experiment, with its ten-year pay-out period, seem to mitigate this objection. A national program also might have higher participation rates because it would become more widely known than the experiments did, and more networks for "diffusion" of the program might become operative. Hence, although the participation rates from the experiments may be lower bounds for true rates, participation is an important determinant of total program costs that would be difficult to learn about without an experiment.

The total number of eligible households of each type, the last determinant of costs, might not be independent of the design of a housing-allowance program. It is possible that a housing-allowance program might alter rates of household formation or dissolution. For example, a housing-allowance program might encourage the formation of low-income, one-person households and thereby increase the number of eligible households. The housing-allowance experiments cast no light on this matter. Empirical studies, however, tend to find only weak relations between housing-market conditions and household formation (Williams 1978).

Housing Consumption

Two major formulas, the housing-gap and percent-of-rent specifications, were tested in the housing-allowance experiments, but it appears that the income-gap formula would be the most likely one to be incorpo-

rated into a national housing-allowance program. Although the demand experiments found no evidence for it, apparently there is a lingering suspicion that a percent-of-rent formula in a national program would be a prime candidate for fraudulent cooperation between landlords and tenants: it is feared that landlords would raise rents and split additional allowance payments with tenants (Struyk and Bendick 1981, 175). If the income-gap formula is the prime candidate policy, then the income elasticity of the demand for housing is the prime behavioral parameter of interest. This parameter has been widely estimated using cross-sectional data, and it does not appear that the housing-allowance experiments were necessary for its estimation.

The relatively low, income elasticities of demand obtained from the housing-allowance experiments are plausible and consistent with those obtained from other studies. They are certainly at the low range of cross-sectional estimates, but this is believable given the short time frame of the demand experiments and the low income levels of participants and control groups. Some analysts disagree with this characterization of the elasticities, and they believe that the "true" elasticities are higher. They argue that slow speeds of adjustment, model specification, and the omission of tenure change are biasing downward the experimental results (Hanushek and Quigley 1979). Change in tenure was possible in the supply experiments, but only about 1 percent of the participating households changed tenure (Struyk and Bendick 1981), 292). Many of these arguments are doubtless applicable to procedures used with traditional household survey data. In any case, it seems to me that the housing-allowance experiments were not necessary for the estimation of the required income-elasticity parameters.

As Rosen points out in his paper, data from the housing-allowance experiments do have a comparative advantage in the estimation of price elasticities because prices are not observed directly in housing markets. If a housing-gap formula were used in a national program, however, the price elasticity would be relevant only if housing prices changed due to the program. This price change would then have a secondary impact on housing consumption. Hence the price-elasticity estimates are interesting but not necessarily relevant to the consumption effects of a national housing program.

In addition to providing information about overall housing consumption parameters, several additional insights were gained about the consumption and choice behavior of households in housing markets and the usefulness of certain analytic approaches. Three of these are worth mentioning here.

First, several versions of the demand experiment and all of the supply and administrative-agency experiments required participating households to live in dwelling units that passed certain minimum physical

standards. Where dwelling units failed these standards, the units could be upgraded or the household could move. Whichever response was used by a household, it appears that they adjusted their dwelling unit so that it passed the specific constraints imposed. There was usually no significant improvement beyond that mandated by the standards. Given this type of narrow and specific meeting of required standards, it appears that the standards used would be significant determinants of the outcomes associated with a national housing-allowance program. In particular, much thought needs to be given to the setting of standards in a national program. This specificity of response is a significant result that apparently could only have come from the experiments.

Second, a great deal of data were collected about household behavior in housing markets in the course of the demand experiments, and this data has been used to analyze empirically the search behavior exhibited by households in housing markets. It appears, for example, that search behavior does pay off in terms of finding dwellings with particular attributes desired by the households or in terms of finding dwellings that may be bargains (Dunson 1979). This analysis of search behavior has not exploited the experimental nature of the data, however, and in principle could have been done with traditional survey data that interviewed households during or just before their household moves. The existence of the allowance experiments obviously make the identification of this time interval straightforward.

Third, a number of results of the housing-allowance experiments were obtained by using hedonic indexes to control for variations in the price or quantity of housing. For example, it has been reported that households faced with minimum-rent standards tended to pay more per unit of housing quantity than households faced with minimum physical standards (Struyk and Bendick 1981, 140–43). Results of this type are based on hedonic indexes estimated for Phoenix and Pittsburgh. Although Rosen has already mentioned the shortcomings of these indexes in the analysis of housing-market outcomes, it is worth pointing out the particular properties of the Pittsburgh and Phoenix indexes. Using excellent data and sufficient resources for experimentation with alternative specifications, the hedonic equations for Pittsburgh and Phoenix had R^2 of 0.66 and 0.80, respectively (Merrill 1977). These are reasonably high levels of explanatory power. However, an analysis of the residuals from these equations indicates that the average absolute magnitude of the residuals was 22 percent of the average rent paid in Pittsburgh and 20 percent of the average rent paid in Phoenix (Dunson 1979, 208). Since many of the calculations done on price and quantity variation employ residuals from these hendonic equations, the large magnitude of the residuals is troubling. It suggests that there is a specification problem with the underlying indexes, or that hedonic indexes are operationally flawed when used in

housing markets, even with the best data one could hope for. The magnitudes of the Pittsburgh and Phoenix residuals do not give one cause to be optimistic about the use of hedonic indexes to measure the small changes in price or quantity of the sort associated with the experiments. Again, experimental data was not necessary to demonstrate this.

Market Effects

The supply experiments tested for market-wide effects in two urban areas using a housing-gap-allowance formula. Since the housing-gap formula works through the income elasticity of demand, which is low, in combination with a low participation rate, the supply experiments increased housing demand very little. As a consequence, there has been no measurable market-wide effect of the supply experiment. This result is significant because one of the major objections to a national housing-allowance program has been its possible inflationary consequences. Do we really want to believe that a national housing-allowance program would have a similarly small market-wide effect? To answer this question, it is useful to do some simple sensitivity analyses on major-program parameters.

A number of different income limits have been used in the supply, demand, and administration-agency experiments carried out, but they all seem to yield an income-level cutoff that equals about one-half of median household income. About 20 percent of households typically have incomes below this level. In both the demand and supply experiments about one-half of income-eligible households enrolled in the program. Not all of those who enrolled decided to become participants. As noted earlier, in the demand experiment 27 percent of income-eligible households participated while in the supply experiments the participation rate was 42 percent for renters and 33 percent for owners. Thus participants ranged from about 6 percent to 8 percent of all households. Moreover, of those who participated, it is reported that about three-fourths stayed in their pre-enrollment dwellings (Struyk and Bendick 1981, 223). Hence, from 1.5 to 2 percent of all households moved because of, or in conjunction with, their participation in the experiment. This is not a large fraction of the roughly 20 percent of households that move each year. Using the percentage of all households whose moves are associated with allowances to proxy-demand increases suggests that slightly higher participation rates would not have much of an impact on market-wide housing-market outcomes. However, market-wide impacts might be observed if the percent of all households eligible for allowances was increased markedly, say from 20 percent to 40 percent.

One final number also merits reporting: three-fourths of those households whose original dwelling unit failed inspection in the demand experiment, but which eventually passed the standards, obtained a passing unit

by moving (Struyk and Bendick 1981, 109). Given that three-fourths of participants stayed in their pre-enrollment dwelling unit, this implies that the majority of households participating in the demand experiments had pre-enrollment dwellings that passed the standards. The percent of households in the supply experiments that met the standards by moving after first failing the standards was much smaller, about 25 percent, but still over half of the participants in the supply experiments had pre-enrollment units that met the standards. Given this fact, it is not surprising that the demand increase stimulated by the housing-allowance experiments—and particularly the three-year demand experiment—was not large.

The basic behavior pattern implicit in these numbers seems to be as follows; households living in units that pass the minimum standards are likely to participate in the program and to stay in their original units. Households living in units that fail the standards originally are much less likely to participate in the program; those who do participate may move, especially if they are renters. Since the overall moving rates of participating households are similar to the moving rates of control households, and adjustments are made in housing bundles that narrowly and specifically meet the standards, it seems reasonable to conclude that a national housing-allowance program similar to the one tested in the experiments would not have significant market-wide effects. This conclusion depends heavily on the patterns of household behavior (e.g., participation, mobility, adjustment to standards) revealed by the experiments, and it would be difficult to characterize this behavior without the experimental data. However, most of the relevant behavior was observed in the demand experiments. We may yet learn something about specific landlord or supplier behavior from the supply experiments, but it will be difficult. For example, only 217 of the 3,720 monitored dwellings in Green Bay are occupied by program participants (Struyk and Bendick 1981, 301).

Conclusion

I agree with Rosen's view that the experimental data arising from the housing-allowance experiments have not helped us to solve many of the problems of measurement that underly the analysis of housing markets. However, I also believe that some of the experimental findings are crucial if we are to evaluate the case for a national housing-allowance program. The information on participation rates and the specific response of households to minimum standards are two results important to the assessment of a national program. Both would be difficult to obtain using traditional household interview data.

Does this mean that the housing-allowance experiments were worthwhile? The cost of the experiments, shown in table C2.1, are very high. So high, in fact, that one is forced to think there must be a better way to

Table C2.1 Estimated Cost of Housing-Allowance Experiment (in millions)

Experiment	Household Payments	Administration	Research and Monitoring	Total
Demand	$ 3.6	$ 2.0	$25.6	$ 31.2
Supply	42.5	18.5	41.7	102.7
Administrative Agency	9.8	3.4	9.2	22.4
Overall design and analysis	0.	0.	6.8	6.8
TOTAL	$55.9	$23.9	$83.3	$163.3

Source: Struyk and Bendick (1981, 297). Estimates by HUD, October (1979), of projected total costs.

learn what must be learned—or at least a cheaper experiment. With the benefit of hindsight (a crucial preamble), there was a cheaper experiment: the housing-allowance experiments without the supply experiment. But I do not believe there is a better way to learn what must be learned. Social experiments clearly have a place in the social scientist's tool kit.

Some would use the housing-allowance experiments to argue this conclusion more forcefully. First, they would state that a social experiment is much more credible to a decision maker (read congressman) than are empirically estimated equations or a simulation model. Second, they would invoke what I call the "Christopher Columbus principle of attribution" (one is judged by one's findings, not by success in discovering what was originally set out for), and claim that one year's savings from the improvements in administrative efficiency of HUD's existing programs stemming from results of the housing-allowance experiments will pay for the experiments (Struyk and Bendick 1981, 308). Finally, they would dispeal fears about fundamental flaws in social experiments by pointing out that the "Hawthorne effect," the hypothesis that people alter their behavior when under study, no longer has a strong empirical base. A new analysis (Lagerfeld 1979; Franke and Kaul 1978) of the original data collected at Western Electric's Hawthorne plant in Chicago does not support the study's original findings.

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