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HOUSING BEHAVIOR AND THE EXPERIMENTAL HOUSING ALLOWANCE PROGRAM: WHAT HAVE WE LEARNED?

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ABSTRACT

The purpose of this paper is to evaluate the Experimental Housing Allowance Program (EHAP). My focus is on what the experimental data have taught us that could not have been learned from more traditional sources of information. I review the major problems that confronted investigators using non-experimental data, and for each problem discuss whether or not it was mitigated by the availability of EHAP data. I conclude that if the goal was to obtain improved estimates of the behavioral response to housing allowances, a social experiment was not necessary.

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I. 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, etc. It has been suggested that a better method would be to give poor people financial allowances which 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 to increase their housing consumption? To the extent they do, would housing prices simply be driven up, resulting in windfall gains for landlords?[1]

To obtain answers to these important questions, in 1970 the Department of Housing and Urban Development 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 examine market effects of housing allowances. <u>All</u> 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, p.43). 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 fundamentally on the basis of new knowledge about them that the experiment must be judged.

The existence of numerous studies which 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 II, supply in Section III. Section IV contains the conclusions.

II. The Demand Experiment

The main purpose of the demand experiments was to obtain predictions of households' responses to various types of housing allowances. I begin this section by describing the experiment's structure. This is followed by a discussion of problems that users of conventional data have faced in analyzing housing behavior, and the extent to which experimental data alleviate these problems.

A. Description of the Demand Experiment[2]

In the demand experiment, a set of randomly selected low income households received allowances, while members of a control group did 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[3] (Y):

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

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:

$$M = \alpha R$$

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, p. 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% below and 20% 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[4] families participated in the experiment, somewhat under half of which were included in the control group. Only renters were eligible.

B. 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 an investigator were 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 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 allowances.[5] These considerations suggest the following strategy: employ multiple regression techniques (or some variant thereof) to estimate the demand for housing services, employing either cross-sectional or time series data. This 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 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.

1. 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 that theoretical considerations suggest might be relevant:

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

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^D's 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 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 $\frac{\partial Q^D}{\partial Y}\frac{Y}{Q^D}$ is less than one, it does not imply that increasing a particular family's income ten percent will increase its housing consumption by a smaller percentage. All that 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 [1981] 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)[9] report on the demand experiment is devoted to discussion of such results. Most of the document concerns the specification of models like (3), and their estimation with data from the experiment. But as Hanushek and Quigley [1979b] 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" (p. 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.

Why is this the case? The main reason, I think, 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 [1981] 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 which 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, using decisions are evidently made by families within a long run framerk, but the precise amount of time required is not known. As noted in

Section 5, 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 them 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.

2. 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 implementation. 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, et. al [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 an hedonic price equation[12] for housing, and (2) use these prices as explanatory variables in regressions with attribute quantities as the dependent variables. However, Brown and Rosen [1980] have shown that major statistical pitfalls are present in this procedure, and that the validity of Witte, et al's results is therefore in question. Although some progress is being made in dealing with these problems (see Quigley [1980]), 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 [1978], for example, struggle with the problem of measuring housing services in very much the same way as users of non-experimental data (pp. 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.

3. Price of Housing Services

Imagine an investigator with (non-experimental) cross-sectional observations on a group of renters all of whom come from a particular 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 inter-city 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

(4)
$$P_i = (1-\alpha_i)P_o \quad ,$$

where P_o is the pre-treatment 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 σ_i , the fact that P_o is identical across individuals in the community no longer precludes estimation of a price response. P_o can be normalized at an arbitrary value and then (4) used as the price term. This approach is used by Friedman and Weinberg [1978], and Hanuskek 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, p. 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, Hanuskek and Quigley [1980] argue convincingly that such differences in P_o 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 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.

4. Shift Variables

Consider now the shift (i.e., non-price) 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 year's worth of annual 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 [1981]). However, to the extent that structural models are required to obtain useful results (see Section B.1 above), 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).

5. 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, households 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[18] (e.g., Rosen and Rosen [1980]). 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, p. 71] devote a considerable amount of time to separate analysis of those households which changed dwelling 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 which is discussed in the next section.)

In addition, Friedman and Weinberg utilize the typical partial adjustment model to study dynamic behavior.[19] 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 modelling of disequilibria especially important. For some treatment groups, individuals were ineligible for housing allowances unless their housing met certain quality standards. (See

Section II.A, above.) 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 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 (see Section II.B.2 above), 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.

6. 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 non-random (see Heckman [1979].) It has been shown that if selection into a sample is non-random, 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, p. 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 non-random subsample; viz., "...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 [1981]). In experimental data, then, selectivity bias is not eliminated -- it merely appears in new forms.

7. 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 the income distribution. It is hard to imagine how non-experimental 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., foodstamps), probably one cannot reliably infer from them 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 allowances 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, p. 139] conclude that, even for the people 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 were variation in the pre-treatment 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

$$\ell n P_i = \ell n(1-\alpha_i) + \ell n P_{oi} ,$$

where P_{oi} is the pre-treatment price and α_i is as defined above. Thus, if $\ell n(1-\alpha_i)$ and $\ell n P_{oi}$ 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 in Section II.B.3 above, in the EHAP samples there is probably not enough variation in the pre-treatment prices to make an attempt to calculate them worthwhile.

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.

III. The Supply Experiment

In most analyses of housing demand using both conventional cross-sectional and EHAP data, it is assumed that the pre-treatment 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 pre-treatment 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 begin by summarizing 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.

A. Description of the Supply Experiment[24]

The supply experiment began in 1973-74, with a planned duration of 10 years. 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%. 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, p. 1].) There are two principal explanations for this phenomenon: (a) Because the income elasticity of demand for housing services apparently is quite low for program participants,[25] (about 0.3 for renters, according to Mulford [1979, p. 31]) the housing allowances did not shift the market demand curve very much, and (b) 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 [Brookings, 1979] 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[26] will confer upon future researchers of housing supply.

B. Problems in Predicting the Supply Response to Housing Allowances

1. 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 [1977] assume that housing services are a constant elasticity of substitution (C.E.S.) function of "quality capital" and "operation inputs" (p. 284). Polinsky and Ellwood [1979] also posit a C.E.S. production function, but assume that its arguments are land and capital. Field [undated] uses a transcendental logarithm production function with three inputs, land, capital and labor. Poterba [1980] 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 (p. 10). (Of course, duality considerations 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 [1979] assume constant returns to scale (p. 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 pre-treatment 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, and 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 modelling, particularly since there is no control group. Barnett [1979], for example, provides some simple comparisons of the increase in rents in the test sites relative to those in other U.S. cities (p. 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.

2,3 Defining Housing Services and Their Price

The problems 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, et. al, [1979, p. 14]). To answer this question, one would need to 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.

4. 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 is a commonly used index, it is well-known that it is deficient because fixed weights are used in its computation. Ingram and Oron [1977] use the fuel component of the consumer's 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, p. 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 appear to be some of the most careful computations available.

5,6 Disequilibrium and Dynamic Issues

As suggested above, many models of housing supply begin with a production function which 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 which might impede the market from achieving equilibrium.

Thus, for example, in one of their models Ingram and Oron [1977, p. 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 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, which 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, there are no simple comparisons that one can make 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 quite possible that a number of variables which influence adjustment patterns would have changed, so comparisons of South Bend and "South Bend Prime" would not be very informative.

7. 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 pre-treatment 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 (inter-related) markets has a market clearing price. If the housing market is non-competitive, 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 is an interesting approach, but the availability of experimental data provides no special advantage when it comes to testing its validity.

IV. 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 it is doubtless that 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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Footnotes

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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.

2. This sub-section is based upon Allen, et al. [1979], especially pages 28-30.

3. The definition of "household income" was essentially post tax 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 $8150; for Pittsburgh, the limit was $6250.

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

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

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. It is a pity that no similar major report has been issued by the Rand Corporation for the supply experiment.

10. One can rescue the experimental approach from this criticism by building income-price interactions into the experimental design. However, as Hausman and Wise [1981] 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 [1979], or Hausman and Wise [1980].

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} characteristic is $\partial R/\partial X_i$. See Rosen [1974].

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.

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.

18. This differs from the use of the term "disequilibrium" in much of the macroeconomics literature, where it 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, p. 127] note, dynamic patterns might be affected by the limited duration of the experiment.

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 Brookings Institution [1979, p. 10].

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

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

24. This subsection is based upon Allen, et. al., [1979],

25. In addition, only about half the eligible renters and 30% of the eligible homeowners had enrolled after four years (Allen, et. al., [1979, p. 35]).

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.

28. The assumption of a horizontal supply curve is quite common, e.g., see DeLeeuw and Struyk [1975, p. 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 non-zero slope.

29. For example, Mills and Sullivan [1979] 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, p. 68].