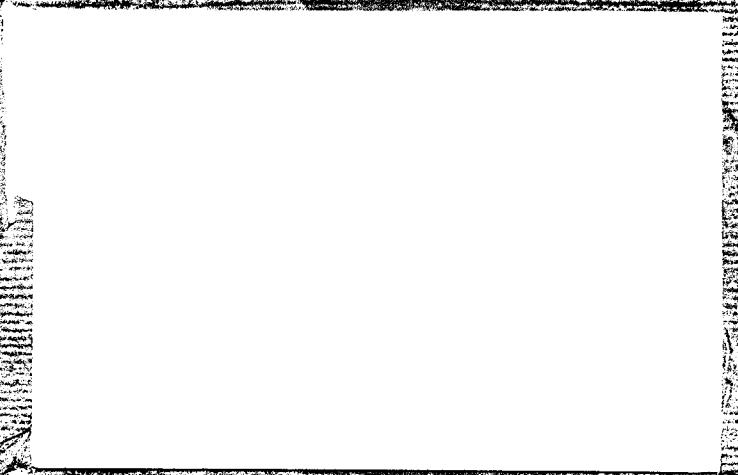

HOUSING ASSISTANCE SUPPLY EXPERIMENT

A WORKING NOTE

This Note was prepared for the DEPARTMENT OF HOUSING AND URBAN DEVELOPMENT, under Contract No. H-1789. It is intended to facilitate communication of preliminary research results. Views or conclusions expressed herein may be tentative and do not represent the official opinion of the sponsoring agency.

Rand
SANTA MONICA, CA 90406

WN-8198-HUD

GENERAL DESIGN REPORT: FIRST DRAFT

Edited by Ira S. Lowry

May 1973

Rand
SANTA MONICA, CA. 90406

PREFACE

This Working Note was prepared for the Office of Policy Development and Research, U.S. Department of Housing and Urban Development, as an update of our general design for the Housing Assistance Supply Experiment. It supersedes WN-7866-HUD, *Preliminary Design for the Housing Assistance Supply Experiment*, June 1972. Component sections and appendixes of the present report have been published as separate Working Notes over the past five months; these have been revised and are incorporated here into a single document.*

The *General Design Report* has thus been assembled in draft form for a review by a panel of experts selected jointly by Rand and HUD. Following this review, a revised draft will be submitted to HUD for final review and comment. Although the present document reflects a number of earlier comments and suggestions made by the Director of HUD's Experimental Housing Assistance Program and his staff, its contents have not yet been accepted by HUD as an approved design for the Supply Experiment.

This Working Note was prepared pursuant to HUD Contract H-1789 and partially fulfills requirements set forth in Sec. II.B, Task 1, of that contract.

---

*In addition to the topics covered herein, HUD has requested that the *General Design Report* include an analysis plan for measuring supply response contingent on unexpectedly high rates of refusal in our proposed Survey of Landlords. This plan has been completed and is now being prepared for publication as a separate Working Note (WN-8268-HUD), *Compensating for Landlord Nonresponse in the Housing Assistance Supply Experiment*, by Adele Massell). Its contents will be summarized in subsequent drafts of the *General Design Report*.

ACKNOWLEDGMENTS

This report summarizes the work of many individuals, performed over a period of 20 months beginning in October 1971, when Rand was invited by HUD (through the Urban Institute) to explore ways of testing the supply response to a housing allowance program.

The basic concept of the experiment and the principal methods of analysis to be pursued were developed in two months of intensive work by Ira S. Lowry, C. Peter Rydell, and David de Ferranti.* Subsequent discussions with HUD led in April 1972 to a contract with Rand for design development drawing on this initial study but broadening the explicit research objectives to include analysis of the behavior of market intermediaries and indirect suppliers, residential mobility, and neighborhood change, and effects on nonparticipants.

The broadened agenda and a decision by HUD as to the general scale of the experiment (metropolitanwide allowance programs in two small metropolitan areas) were reflected in a preliminary design report prepared for HUD in June 1972, supplemented by additional design papers in July.**

These documents set the framework for the experimental design that was elaborated and refined over the ensuing months. As the number of participants in the design process increased and as ideas evolved through joint efforts and staff discussion, individual contributions became less clear-cut. Appendix F lists some 35 Working Notes prepared during this period; their titles and authorship provide a guide to many of the individual contributions but slight others

*See *Testing the Supply Response to Housing Allowances: An Experimental Design*, The Rand Corporation, WN-7711-UI, December 1971.

**Ira S. Lowry, *Preliminary Design for the Housing Assistance Supply Experiment*, The Rand Corporation, WN-7866-HUD, June 1972; and Housing Assistance Supply Experiment Staff, *Supplemental Design Papers for the Housing Assistance Supply Experiment*, The Rand Corporation, WN-7982-HUD, July 1972. Contributors to the latter collection included Ira S. Lowry, David B. Lewis, Timothy M. Corcoran, Michael Shanley, Therman Britt, and Sandra Berry.

whose work is not embodied in a specific document. Below, we try to identify these others as well, grouping contributors to each major element of work leading to the present report. In addition to full-time members of the HASE staff, the listing includes other Rand employees and consultants who made substantial contributions and the principal members of our subcontractor's staff. Within each group, names are ordered alphabetically.

The overall design effort and the preparation of this report were supervised by Ira S. Lowry, assisted for two critical months by Harrison S. Campbell and Malcolm Palmatier. Most of the manuscript and tables were typed by Geraldine Jaimovich. Graphics were prepared by Doris Dong. Janet DeLand edited the typescript and supervised production of final copy.

PROGRAM MANAGEMENT

Program Director

Charles E. Nelson

Manager
Design and Analysis Group

Ira S. Lowry

Manager
Field and Program Operations Group

Robert Dubinsky

Manager
Data Systems Group

Gerald Levitt

Acting Manager
Survey Group

Sandra Berry

Manager
Editorial Group

Janet DeLand

SITE SELECTION

Screening

Annette Bonner
Therman Britt
Alan F. Greenwald
David B. Lewis
Richard B. Rainey
Albert H. Rosenthal
Michael Shanley

Field Reports

Therman Britt
Earl Carter
Robert Dubinsky
Alan F. Greenwald
Michael Shanley

HOUSING ALLOWANCE PROGRAM

General Analysis

Ira S. Lowry
Charles Noland
M. Mack Ott

Program Design

Alan F. Greenwald
David B. Lewis
Ira S. Lowry

Impact Estimates

Tiina Repnau
Barbara M. Woodfill

MONITORING PROGRAM

Sample Design and Selection Procedures

Timothy M. Corcoran
Eugene C. Poggio
Tiina Repnau
Seymour Sudman

Survey Instruments

Robert P. Althauser
Sandra Berry
Therman Britt
Zahavah Blum-Doering
William P. Grigsby
Deborah Hensler

Neighborhood Definition

Bryan Ellickson

Resident Observer

Robert Dubinsky
William P. Grigsby

ANALYSIS PLANS

Supply Response

David de Ferranti
Ira S. Lowry
Adele Massell
Charles Noland
M. Mack Ott
C. Peter Rydell

Residential Mobility

Robert P. Althauser
Zahavah Blum-Doering
Harrison S. Campbell
Ira S. Lowry

Effects on Nonparticipants

Robert P. Althauser
Harrison S. Campbell
William P. Grigsby

Market Intermediaries

William P. Grigsby

DATA MANAGEMENT PLAN

Colleen M. Dodd
Misako Fujisaki
Gerald Levitt

FIELD SURVEY SUBCONTRACTOR
(Mathematica, Inc.)

Director
Urban Opinion Surveys

David Kershaw

Project Director

Mary Scowcroft

Instrument Development

Cheri Marshall
Michael Wills

CONTENTS

I. INTRODUCTION

This report describes and explains the principal features of the Housing Assistance Supply Experiment sponsored by the Office of Policy Development and Research, U.S. Department of Housing and Urban Development (HUD). The Supply Experiment is one element of a broader Housing Assistance Research Program which is intended to help the agency decide whether a national program of housing allowances for low-income families would be preferable to existing programs of housing subsidies for the same general target population; and if so, what form housing allowances should take.

Most existing programs of housing assistance for low-income families channel public funds directly to the suppliers of housing, on condition that the housing be occupied by low-income tenants. There is a contractual relationship between the public agency and the supplier which usually regulates both the housing services to be provided to the tenant and the price the tenant may be required to pay for these services.

A housing allowance program would operate differently. Public funds would be granted directly to low-income families, who would then use their increased resources to buy housing services in the local housing market. The intent of such a program would be to enable recipient families to substantially increase their housing consumption, without depriving themselves of a reasonable standard of living in other respects.

It is thus a matter of some importance to anticipate how recipients would respond to the opportunity afforded them by a housing allowance. For most recipients, the allowance would be intended as a rent supplement, the recipient also contributing toward the cost of his housing. Depending on the form of the allowance (cash grant, rent certificate) and its terms (percent of actual rent, percent of income), and on the restrictions placed on the housing the recipient may occupy (rent level, quality level), the public contribution could be made nonfungible, partially fungible, or entirely fungible with the remainder of the recipient's resources, and he would be given more or less discretion in choosing his level of housing expenditures.

To explore this unknown terrain of "demand" response to housing allowances, HUD is sponsoring a Housing Assistance Demand Experiment. Briefly, the plan for this experiment calls for selection of a thin sample of low-income families in one or more large metropolitan areas for enrollment in a housing allowance program. Subsamples of the enrollees will be given allowances on different terms, as suggested above, and their housing choices and budgetary decisions will be monitored for a period of several years.

Because the number of allowance recipients will be small relative to the total population--or even to the total low-income population--of the housing markets in which the Demand Experiment is conducted, these markets will not be noticeably perturbed by the allowance program. Neither the suppliers of housing services, nor market intermediaries, nor nonrecipient families are likely to be aware of or significantly affected by the efforts of allowance recipients as a group to obtain better housing. In this respect, the Demand Experiment will be very different from a national program of housing allowances which would enroll all low-income families who chose to participate.

The Supply Experiment is intended to fill this gap, testing the market's response to a *large-scale* allowance program. For this purpose, two small metropolitan areas (up to 250,000 population each) with different market characteristics will be selected; in each area, housing allowances will be offered to most low-income families who would probably be elible under a national housing allowance program; under the standards of the experimental program, we expect that 15 to 20 percent of all households in the market area will enroll.* Then, the local housing market will be monitored to see what happens when program participants try to turn their augmented resources into a higher level of housing consumption.

---

*Naturally, the results of both the Demand and Supply Experiments are likely to modify a priori judgments as to who should be eligible for housing allowances under a national program. The point here is simply that enrollment in the Supply Experiment will be a substantial fraction of the metropolitan population and will include most of those who, under any reasonable standard, would be declared eligible under a national program.

Metropolitan areas will be selected as sites for the Supply Experiment because it is important that the experimental allowance program encompass an entire local housing market, both central city and adjoining suburbs, if it is to reflect the consequences of a national program for such a local market. Of course, low-income populations tend to cluster in particular locations within a metropolitan area, so enrollment will be high in some neighborhoods, low in others. But allowance recipients will not be restricted in their search for better housing to the neighborhoods in which they live at the time of enrollment. Indeed, one of the purposes of the experiment is to determine whether, given augmented resources, they will look for and be able to obtain housing improvements in their present neighborhoods, or whether they will prefer or find it necessary to search further afield.

Compared to most existing programs of housing assistance for low-income families, the target of the housing allowance program is both modest and ambitious. Rather than subsidizing the occupancy of expensive new housing for a small number of low-income families, it is intended to enable all, or nearly all, low-income families to afford decently maintained older housing. A major premise of such a program is that older housing, now deteriorating because of the inability of its tenants to pay the full costs of upkeep, could be improved at modest cost to an acceptable standard. One purpose of the experiment is to determine whether and how quickly the owners of such housing will respond to the increased purchasing power of low-income households by upgrading their properties.

The costs of supplying housing of a given quality vary considerably from locality to locality, and local costs will be taken into account in setting allowance levels for the Supply Experiment. However, where direct comparisons have been made, it appears that older housing (e.g., housing built prior to World War II) can be upgraded and kept in good condition at about half the annualized cost of new public housing. Preliminary estimates indicate that a housing allowance averaging $500 to $700 a year, matched by a reasonable contribution from the tenants' other resources, would enable all low-income families in most communities to afford decent housing, so defined. If tenant contributions

ran as high as they do in Federal public housing, a given total Federal contribution would enable five to eight times as many households to obtain decent housing under such an allowance program.

As indicated, these estimates cannot be made precise without analysis of local patterns of income and housing costs. However, it is helpful to draw from them at least a rough notion of the fiscal scale of the Housing Assistance Supply Experiment. For an SMSA* of 200,000 population (about 60,000 households) with an enrollment rate of 20 percent and an average annual allowance payment of $600, allowance payments would total $7.2 million annually. Given the variations in size of metropolitan areas under consideration and present uncertainties about enrollment rates and average costs, it still seems safe to estimate annual allowance payments of $5 million to $10 million per site.

The fraction of total allowance payments that would reappear as a net addition to housing expenditures depends very much on the form of payment and the restrictions imposed on the housing choices of allowance recipients. Assuming that at least half of all allowance payments are devoted to increasing housing expenditures (as opposed to substituting for preallowance housing expenditures), $10 million in annual allowance payments implies an increase in metropolitanwide housing expenditures (by both homeowners and renters) of 10 percent at most in an SMSA of 200,000 population. While this is a large enough increase to perturb the market, it is well within the range of experience with "natural" market shifts occurring over a period of two or three years. Thus, there is no reason to anticipate a severe metropolitanwide dislocation of the housing market. The effects would be focused on that sector of the market that supplies housing to families of low to moderate incomes, where housing expenditures might increase by as much as 30 percent, or even more in very low-income neighborhoods.

The Housing Assistance Supply Experiment is designed to reveal the dynamics of market response to such an increment of low-income housing demand by actually "injecting" the demand increment and monitoring the market thereafter. An experiment is appropriate because there is substantial uncertainty and even fundamental disagreement among housing

---

*Standard Metropolitan Statistical Area.

economists and other interested parties as to the probable effects of a national housing allowance program with the characteristics described above.

Section II of this report lists four clusters of critical questions about the effects of such a program that we believe can be reliably answered by the proposed experiment.* Briefly, they include questions about the effectiveness of the program as a means of inducing housing improvements and the related possibilities for inflation in housing prices; about the behavior of market intermediaries and others in an allowance-stimulated market; about residential mobility induced by the program and the consequent redistribution of local populations; and about the effects of the program on nonrecipients and their attitudes toward it. Then, Sec. II describes the general strategy of the Supply Experiment in seeking empirical answers to these questions. Finally, it provides a chronological overview of the contemplated experiment, from site selection through termination.

The proposed design of the experimental allowance program is presented in Sec. III. This design, as the basis for disbursement of large amounts of public funds, necessarily reflects compromises among conflicting requirements: different views of the feasible and the desirable characteristics of a national program, constraints imposed by available sources of funding, practical problems of creating a local organization to administer the program, and features inappropriate to a national program but helpful for an experiment whose purpose is to produce information.

Section IV describes our proposed monitoring program, which relies partly on administrative records of the allowance program but principally on an annual cycle of field surveys addressed to a large sample of residential properties, their owners, and their occupants. The sample is a longitudinal panel, designed so that successive annual observations will enable us to track changes in the housing market as they occur over

---

*Answers to other questions bearing on the preferred program design and probable consequences of a national program will be sought in the Demand Experiment, or in a third Administrative Experiment which tests various mechanisms for delivering housing allowances.

time. The field-survey agenda is technically complex and in some respects risky; if successful, it will provide data of a kind and quality without precedent in housing research.

The central purpose of these field operations is to produce systematic data concerning the effects of the allowance program and related events on the local housing market. Section V describes our plans for assembling these data into permanent machine-readable files whose overall organization permits data to be abstracted, linked to data from different sources or different points in time, and manipulated to serve a wide range of analytical requirements. The section closes with a very brief prospectus for each of the major analyses to be undertaken, showing its dependence on data from each of the major files.

The following four sections, VI through IX, enlarge upon these analytical prospectuses. Each section presents a plan for analyses related to one of the major research topics with which the Supply Experiment is charged. In each case, we describe the policy issues as we understand them and pose a set of specific research questions whose answers should assist in policy determination. Then we describe the sources of data and the forms of analysis we propose to use in seeking answers to these questions. The level of exposition here is nontechnical; our purpose is to enable the general reader as well as the specialist to judge whether the analytical approach is reasonable in its broad framework, not to present detailed statistical models.

In Sec. X, we evaluate the probable success of the Supply Experiment in terms of the reliability and credibility of the evidence it supplies bearing on the effects of a national housing allowance program. Our discussion here focuses on the measurement of allowance-induced changes in the price and quantity of housing services supplied at each experimental site and on inferences from this evidence to the corresponding effects of a national program.

The emphasis in Sec. X on supply response to the experimental allowance program does not imply lack of interest in the other three research topics included in our charter; rather, it reflects our conviction that those topics present lesser analytical and inferential challenges. The difficulties in measuring supply response in each

experimental site and extrapolating from these findings are sufficiently impressive that we have included in this report a series of technical appendixes designed to demonstrate that we have given careful thought to these problems and see our way to their solution.

These appendixes include a mathematical model of housing deterioration under alternative maintenance policies (Appendix A); a detailed specification of the accounting system by which we propose to measure supply response (Appendix B); a method for estimating parameters of the production function for housing services (Appendix C); a method for measuring changes in the prices of factors used in the production of housing services (Appendix D); and a technique for combining data from the Demand and Supply Experiments to estimate the effects of housing allowances of various kinds in local housing markets other than the experimental sites (Appendix E).

This report does not represent the end of the process of experimental design, or even the full extent of the work so far completed. Technical documentation of analysis plans for each of the four research topics will continue, with next priority given to mobility analysis. Other Working Notes, listed in Appendix F, provide details on many subjects here treated only briefly or not at all.* The material selected for the present report is adequate, we think, to provide the reader with the information he needs to understand the experimental strategy and to evaluate it in relation to the experimental purposes.

The report contains our proposal to HUD for the design of the Supply Experiment, not a design already approved by HUD. As we have articulated the details of this proposal over the past year, we have consulted frequently with the Director of the Experimental Housing Assistance Program and his staff. These consultations have led to innumerable modifications and improvements; however, there have also emerged issues on which Rand and HUD have been unable to reach agreement. Throughout this report, we try to flag those issues for the reader's special attention, by footnote or other means.

---

*These Working Notes, published over the course of the preceding year, frequently present views on experimental design that were subsequently modified. Where they conflict with material in this report, it should be assumed that they are in that respect obsolete.

II. AN OVERVIEW OF THE SUPPLY EXPERIMENT

All of the experiments planned as part of the Experimental Housing Assistance Program are intended to provide information bearing both on the optimal design of a national program of housing allowances and on the merits and demerits of such a program as a means of improving the housing conditions of low-income families. HUD's decision to mount *separate* Demand, Supply, and Administrative Experiments is motivated by considerations of efficiency. Each experiment is designed to answer specific questions and to capture specific kinds of information; the various findings are to be integrated analytically.

EXPERIMENTAL OBJECTIVES

The mission assigned to the Supply Experiment is to provide reliable and credible answers to four clusters of questions about the effects of a national housing allowance program:

1. Supply responsiveness. How will the suppliers of housing services--landlords, developers, and homeowners--respond to the attempts of allowance recipients to increase their housing consumption? Specifically, what mix of price increases and housing improvements will result? How long will these responses take to work themselves out to a "steady state"? How will these responses differ by market sector?
2. Behavior of market intermediaries and indirect suppliers. How will mortgage lenders, insurance companies, and real-estate brokers respond to an allowance program? Will their policies facilitate or inhibit the attempts of allowance recipients to obtain better housing and those of landlords to improve their properties? What happens to the availability, price, and quality of building services and repair and remodeling services? What seem to be the reasons for any observed changes in institutional or industrial policies?

3. Residential mobility and neighborhood change. In their attempts to find better housing (or better neighborhoods), will many allowance recipients relocate within the metropolitan area? What factors influence the decision to move or to stay? What types of neighborhoods will the movers seek and succeed in entering? Do moves by allowance recipients set in motion a chain of moves by nonrecipients--either into neighborhoods vacated by recipients or out of neighborhoods into which recipients have moved?
4. Effects on nonparticipants. How will households not receiving housing allowances--particularly those whose incomes are within or just above the range of eligibility--by affected by the program? Specifically, will the increased housing demands of allowance recipients cause an increase in housing prices for nonrecipients? Whether or not such price increases occur, will nonrecipients perceive personal hardships or benefits from the program? How will they perceive and react to allowance-stimulated neighborhood changes?

The answers to these questions are interdependent. Whether a landlord chooses to raise rents, and whether he also chooses to offer his tenants improved housing, depends on his perceptions of changes in market demand and of the alternatives available to his tenants. If he wishes to undertake capital improvements, he must usually seek outside mortgage financing. The mortgagee must judge that the future stream of revenues will be adequate for debt service, that foreclosure would not result in capital loss, and that the property is and will continue to be insurable against physical damage or destruction. The extent to which their present landlords raise rents and/or improve physical facilities and services will affect the allowance recipients' decisions to stay or to seek other quarters better suited to their augmented budgets and housing preferences. If they seek better housing elsewhere, they are likely to be competing with nonrecipients for housing previously beyond their means.

Furthermore, the answers are likely to change over time. Those initially enrolled in a housing allowance program are unlikely to react immediately or simultaneously to their augmented housing budgets, so

the demand signals to landlords and developers will be delayed and at first unclear. The landlords in turn will need time to implement their responses--whether rent increases or housing improvements--and as market signals clarify, these responses may change. The actions of landlords and developers may in turn modify the perceptions and policies of market intermediaries and financial institutions. All these events, in time, may perceptibly change the alternatives open to allowance recipients and the consequences of their choices for others (e.g., nonrecipients).

Finally, different groups within the relevant populations of landlords, financial institutions, allowance recipients, and nonrecipients are likely to respond differently to a given stimulus, so that an "average" response may conceal important information. The structure of the local housing market and its initial conditions may also influence response patterns. A market initially characterized by excess demand would respond differently from one characterized by excess supply. The incidence of rental tenure or multiple dwellings or ethnic minorities may condition responses in ways that reflect more than simply a different mix of responses by, say, renters and owners or blacks and whites.

Thus, though the questions can be phrased simply, the answers are likely to be both complex and highly dependent on local circumstances. No feasible set of experiments can embrace all plausible variations in circumstances or trace out all consequences. Yet if a national program of housing allowances is a serious possibility, some information about its possible consequences is manifestly better than none, and limited empirical evidence can be extended analytically to predict the unobserved.

EXPERIMENTAL STRATEGY

In our view, the most difficult issues to be resolved by the Housing Assistance Supply Experiment are the questions of supply responsiveness: how the suppliers of housing services would respond to the increased effective demand for housing by low-income families, demand that would be generated by a national housing allowance program.

In general, when the demand for a commodity increases, suppliers respond with some combination of increased output and higher prices.

Also, in general, the short-run response differs from the long-run response. The movements in prices and quantities over time would ordinarily reflect the magnitude of the demand shift, the initial supply conditions (e.g., the size of the unsold inventory), and the costs encountered by producers when they attempt to increase output. Our task is to design an experiment which shows how these general principles apply in a low-income housing market when demand is stimulated by housing allowances.

As we see it, the experimental design must cope with six basic problems:

1. Because a central feature of the contemplated housing allowance program is its dependence on market processes, the experiment must create, on a small scale, the essentials of the market process: buyers and sellers of housing services reaching mutual accommodation through voluntary action in response to market signals.
2. The increment of effective demand resulting from experimental housing allowance payments must be sufficiently focused geographically and sufficiently stable over time that the resulting market signals will be perceptible to suppliers and will not be discounted by them as purely transient phenomena.
3. Changes in the flow of housing services are difficult to quantify; most measures that seem operationally feasible confound price and quantity changes, a result which would defeat a primary purpose of this experiment.
4. Market responses must be observed for a long enough period of time to detect not only short-run behavior--which may be critical to the political success of a housing allowance program--but enough of the longer-run trend to permit strong inferences as to the durable consequences of such a program.
5. Experimental controls must be adequate to distinguish consequences of the housing allowance payments from consequences of independent background events--things that "would have

happened anyway." Some of these background events may be national forces that would impinge generally on a national program; others may be purely local factors that impinge only on the experiment.

6. The results of the experiment must be intelligible and credible not only to professional analysts and housing experts but also to the broader constituency whose support would be essential to passage and implementation of a national program.

This focus on creating experimental conditions that will enable us to measure housing supply response--price and quantity changes attributable to the allowance program--does not imply neglect of the other three clusters of questions to which the Supply Experiment is addressed. One of those, measuring the impact of the program on nonrecipients, is manifestly dependent on our ability to measure housing price and quantity changes in general. Another, the behavior of indirect suppliers and market intermediaries, is of interest primarily because their behavior will help to explain the observed pattern of supply responses. While the residential mobility of allowance recipients can be recorded without accurate information about housing price and quantity changes, it is reasonable to expect that such movements will both reflect and affect supply responses in ways we will want to comprehend. Our point is simply that an understanding of the dynamics of supply response is crucial to the evaluation of housing allowances as a national program, and that of all the kinds of information to be gathered by the Supply Experiment, reliable measurements of housing price and quantity changes will be the most difficult to obtain. They thus become the key to the experimental design.

Our strategies for dealing with the six problems listed above are spelled out in the remainder of this report. However, a brief preview at this point may help to orient the reader.

Creating a Market Context

There are several existing housing programs that entail direct negotiations for housing improvements between a public agency and

private landlords in return for guaranteed rent payments for some term of years. Careful study of such transactions, or an experiment designed along similar lines, would provide evidence of supply responsiveness under conditions of certainty about future revenues and under bilateral bargaining. We do not think that such evidence would be very pertinent to the outcome of a national housing allowance program, which is critically dependent on normal market processes that entail both uncertainty and multilateral bargaining. To discover how the market responds to housing allowances, we think it is necessary to mount an experimental allowance program and monitor the market response. The difficulty lies in finding a way to do this on an economically small scale.

In our initial experimental design,* we proposed the selection of small urban neighborhoods (about 5,000 housing units) as sites for the Supply Experiment. These were to contain predominantly rental housing and a population whose income distribution was such that roughly half the households would be eligible under the standards of a national program. Alternatively, only residents of these neighborhoods might be enrolled in the experimental allowance program, or enrollment might be extended to the entire metropolitan (i.e., housing market) area.

To limit enrollment to the selected neighborhoods would create certain difficulties both in managing the experiment and in interpreting its results; this alternative was proposed because we were uncertain about the fiscal resources available for the experiment. Since HUD has now assured us that funds are available to enroll all eligible households in each of two small (under 250,000 population) SMSAs, the present design is based on metropolitanwide enrollment.

Providing a Perceptible and Stable Demand Stimulus

However, our reasons for singling out high-enrollment neighborhoods for special attention persist. The demand stimulus provided by our experimental allowance program will not affect all sectors of the housing

*Ira S. Lowry, C. Peter Rydell, and David de Ferranti, *Testing the Supply Response to Housing Allowances: An Experimental Design*, The Rand Corporation, WN-7711-UI, December 1971.

market equally; it is unlikely, for example, that the owners of luxurious apartment houses or expensive single-family homes will perceive any demand changes related to the allowance program. Where there is no demand stimulus, there can be no supply response. Consequently, we propose to concentrate our monitoring resources on sectors of the housing market where we expect the allowance program to have an impact. This strategy leads to a somewhat unusual survey sample design.

To achieve the stability of expectations that would be associated with a permanent national housing allowance program, we propose public commitment to a ten-year experimental allowance program at each site even though we expect to monitor their housing markets only for a shorter period. This commitment does not entail an allowance guarantee for that period to individual households, who may become ineligible because of increased income or for other reasons; nor does it entail any guarantee to specific landlords, all of whom must compete in the marketplace for allowance-receiving tenants. It only guarantees a fairly stable increment to low-income housing demand in that housing market for the ten-year period.

Allowances will be portable within the metropolitan area of each site. Recipients who leave the area will lose their entitlement. To safeguard against allowance-stimulated immigration, only those living in the area at the beginning of enrollment will ever be eligible; but enrollment will be open to those who meet this residency requirement whenever they become eligible.

Measuring Changes in the Flow and Price of Housing Services

To measure supply response, we propose to track a panel of residential properties over the term of the experiment, surveying each property annually to learn about changes in rental revenues, physical changes in the structure itself, changes in the services and maintenance provided by the owner, and changes in the levels of satisfaction expressed by the tenants.

We have devised a method for accounting for annual changes in the flow of real factor inputs used in the production of housing services

that we believe will enable us to distinguish in policy-relevant detail between rent increases and price increases--the former including payments for additional housing services, the latter being the inflationary effects of the allowance program and other factors.

It is also important to note that these annual surveys are not confined to housing occupied by allowance recipients. A very important feature of housing allowances is their portability; no owner of residential property can be sure that he will be able to capture or hold allowance recipients as tenants, and some may not even wish to try. We expect that some substandard housing, not easily improvable, may be withdrawn from the market because it is no longer marketable to those who have become allowance recipients and there are no other customers in sight. In sectors of the market where allowance recipients are actively seeking housing, rents and sales prices are likely to increase for recipients and nonrecipients alike. If our experiment is to capture all these effects, we must monitor the housing *market*, not just housing units occupied by allowance recipients.

Duration of the Monitoring Program

By analogy to experience with other markets subjected to sudden increases in demand, we expect the strongest inflationary pressures to appear early in the experimental allowance program, moderating over time as suppliers of housing services perceive the profitability of increasing their outputs and actually do so. Thus, the least encouraging information about the effects of a housing allowance program will be the first information obtained. We think it is important to monitor the local housing markets long enough to observe the more durable consequences of the program.

We have little solid evidence to assist us now in estimating response lags in the housing market. We have constructed a scenario whose elements seem consistent with related experience; it suggests to us that, in the absence of other disturbances, a local housing market ought to adapt fully to a permanent increment of demand in about five years.* We therefore propose five years as the appropriate duration of the monitoring program at each experimental site; however, no

*Ibid., Sec. IV.

immutable decision on this score need be made now, and evidence from the experiment itself will help us to judge whether the monitoring period should be curtailed or extended.

Experimental Controls

Since a metropolitan housing market is the subject and unit of observation for the most important issues to be explored by the Supply Experiment, classical methods of experimental control (matching groups of treated and untreated subjects, or conducting the experiments in a rigorously controlled environment) are either hopelessly expensive or institutionally infeasible. To distinguish consequences of the experimental housing allowance program from consequences of independent background events, we must rely primarily upon before-and-after comparisons, on comparisons of events in market sectors which differ with respect to participation by allowance recipients, and on direct measurement of background forces whose effects can be formally modeled along with those of the allowance program.

Absent a major natural disaster or a powerful exogenous shock to the local economy of our experimental sites, we believe that analysis along the lines suggested above will be adequate to distinguish the role of the experimental allowance program in shaping observed events: supply response, behavior of market intermediaries, residential mobility, and effects on nonparticipants. While we do foresee limits to our ability to quantify the relative responsibilities of the allowance program and of other forces for certain kinds of events, we feel confident that we can narrow the range of ambiguity at least enough to support a fortiori conclusions that are adequate for policy analysis.

Credibility of Experimental Findings

If the Supply Experiment is to influence national housing policy, it is essential that experimental findings be understood and believed both by technical specialists who are able to follow the details of experimental and analytical methods and by a broader audience who will rely upon common sense to interpret and qualify the reported results.

By matching the experimental allowance program to the scale and essential features of a national housing allowance program, and by encompassing the entire metropolitan housing market in the allowance program and in our monitoring plan, we think we will achieve the basic requirements of experimental credibility. Our monitoring program is both broad in scope and intensive in detail; the major contingency that might threaten its credibility is a wholesale failure of cooperation by landlords, tenants, or allowance recipients. Our analysis plans have been designed with a view to yielding findings that are intelligible to a lay audience while embodying the methodological rigor needed to satisfy professional analysts.

These points bear on the credibility and reliability of our account of the consequences of the experimental allowance program conducted in each of two small metropolitan areas. A further question is whether this evidence will support generalizations about the effects of a national housing allowance program. There, the ground is more treacherous.

Manifestly, a sample of two cases provides no basis for statistical inference to the nation as a whole. However, by choosing our sites carefully so that they differ in ways that are generally acknowledged to be critical features of housing markets, we can strengthen the basis for judgmental inference. * The sturdiness of such inference will depend in part upon whether experimental outcomes in the two sites are similar or radically different.

Finally, our analysis plan provides for the estimation of behavioral parameters (e.g., the price elasticity of the supply of housing

---

*The decision to mount the experiment in two small metropolitan areas does limit the kinds of housing markets available as sites. Some market configurations appear only in large metropolitan areas (e.g., spatially extensive ethnic ghettos, high incidence of rental tenure, high incidence of multiple dwellings). Whether these features would have a major effect upon the outcome of a housing allowance program is an open question. Because big-city housing problems are the focus of so much policy attention, we think it is important to buttress our small-city findings by mounting the Supply Experiment in a third site, a low-income neighborhood in a very large metropolitan area. Here, we think the issues to be investigated could be served satisfactorily by an allowance program restricted to residents of that neighborhood.

services, by market sector) that have a certain portability. Combining these parameters with similar ones from the Demand Experiment (e.g., the price and income elasticity of the demand for housing services), it is possible to model the general effects of a housing allowance program in housing markets other than those that served as experimental sites.

IMPLEMENTING THE EXPERIMENT

Above, we have tried to provide the reader with a clear sense of our experimental objectives and of our general strategy for achieving these objectives. Here, we offer another view of the overall framework of the experiment, an account of its implementation that places the major elements of design in chronological perspective. Other sections of this report, organized topically, fill out the tactical details of experimental design and methods of analysis.

Site Selection

As stated earlier, HUD has agreed to fund the Supply Experiment in two small metropolitan areas, with metropolitanwide enrollment of low-income families in the experimental housing allowance program. Most of the potential participants in a national housing allowance program live in large metropolitan areas; in 1970, three-fourths of the nation's metropolitan population and one-half of its total population lived in SMSAs with 500,000 or more inhabitants. For budgetary reasons, the Supply Experiment must select its sites among smaller SMSAs, with the upper limit set at 250,000 inhabitants; however, we would like the selected sites to have as much structural resemblance as possible to the larger SMSAs.

Pursuant to this objective, we classified the nation's SMSAs in terms of those characteristics that seemed pertinent to the effects of a housing allowance program and examined the resulting distributions, both in terms of numbers of SMSAs and total population.* We concluded

---

*For details, see Housing Assistance Supply Experiment Staff, *Site Selection for the Housing Assistance Supply Experiment: Phase I*, The Rand Corporation, WN-7833-HUD, May 1972.

that the two most critical variables, aside from size, were the economic vitality of the central city and the incidence of ethnic minorities in the central-city population. Using the limited data then available from the 1970 Census of Population, we measured the first variable by the intercensal rate of population growth in the central city, and the second by the percentage of blacks in the central-city population.

Table 2.1 shows how the universe of SMSAs is distributed in these terms. While the number of SMSAs in each of the four cells of the table is approximately the same, the greatest share of metropolitan population falls in the slow-growth/high-black category. We therefore proposed to select one site from among the metropolitan areas of this group.

Of the three remaining categories, none is powerfully dominant either in number of cases or total population. We concluded that the greatest contrast to our first choice would be obtained by selecting the second site from among those SMSAs in the fast-growth/low-black category.

Within each of these two categories, we examined the variation of other housing and population characteristics with size of place. The characteristics included tenure, incidence of multiple dwellings, vacancy rate, median housing rents and values, incomes, unemployment rate, and incidence of welfare recipiency. Only tenure and the incidence of multiple dwellings showed a strong correlation with size of place; we concluded that, with these exceptions, SMSAs of under 250,000 population provided a fairly representative assortment of market configurations.

The remaining steps focused on the small SMSAs in each group: 18 in the slow-growth/high-black category and 37 in the fast-growth/low-black category. Systematic screening procedures, each step reflecting information gained from increasingly detailed investigation, eliminated places that did not really fit their assigned categories (e.g., intercensal growth rate distorted by annexation), that would be administratively cumbersome (e.g., SMSAs that straddle state lines), or that were unsuitable for the experiment because of some unusual characteristic (e.g., a large military or college population).

In the most important step, we rated each place not only on the two major dimensions of difference (central-city growth rate and incidence of ethnic minorities) but on the other housing and population

Table 2.1

DISTRIBUTION OF 1970 SMSAs BY SIZE, CENTRAL-CITY GROWTH RATE, AND BLACK POPULATION

SMSA Population (000)	Slow Central-City Growth Rate (6.9% or less)		Fast Central-City Growth Rate (more than 6.9%)		Total SMSAs	Total Population (000)
	Number of SMSAs	Total Population (000)	Number of SMSAs	Total Population (000)		
Central city with low percentage of blacks (10.8% or less)						
1,000 - 1,999	4	5,472	4	4,986	8	10,458
500 - 999	7	4,448	6	4,422	13	8,870
250 - 499	14	4,439	17	5,570	31	10,009
100 - 249	24	3,948	26	4,314	50	8,262
50 - 99	4	335	11	907	15	1,242
Total	53	18,642	64	20,199	117	38,841
Central city with high percentage of blacks (more than 10.8%)						
2,000 or more	11	45,770	1	7,032	12	52,802
1,000 - 1,999	8	11,043	5	6,931	13	17,974
500 - 999	11	7,999	11	7,306	22	15,305
250 - 499	15	5,155	13	4,286	28	9,441
100 - 249	17	2,748	17	2,710	34	5,458
50 - 99	1	94	4	330	5	424
Total	63	72,809	51	28,595	114	101,404
Totals	116	91,451	115	48,794	231	140,245

SOURCE: U.S. Census of Population, 1970, Series PHC (2).

NOTE: Growth rate and ethnic categories are divided at median values for all 1970 SMSAs.

characteristics mentioned above. A scoring system was designed to award the best scores to places in each group whose characteristics were close to the median values for all SMSAs (not just small SMSAs) in that group. Those with low scores were eliminated from further consideration.

In the end, our procedures led us to two lists of three SMSAs each for field evaluation. With one exception,* each of these places was visited by a team of Rand and HUD personnel to gather additional data on its suitability for the experiment and to appraise the level of local interest in participating in the experiment.** The information thus gathered was reviewed jointly by Rand and HUD, and the candidate sites on each list were ranked in order of suitability. At present, negotiations are under way with the highest-ranking places on each list.

To summarize, our site-selection procedure is designed to select one site whose central city has a slow or negative rate of population growth and contains a large black or brown minority, and another site whose central city is growing rapidly but whose population is virtually all white. Within these two categories, we seek places that are typical of their groups in other respects, avoiding extreme or unusual cases. Thus we hope to obtain powerful contrasts in the housing market and population configurations of our two sites, bracketing the modal characteristics of the nation's cities, despite the limit imposed on SMSA size.

While the SMSA is the nominal unit in our search for suitable sites, the actual geographical boundaries of allowance-program enrollment need not be the SMSA boundaries. Instead, they should reflect the spatial extent of a housing market whose core is the central city of the SMSA, also taking into account the administrative conven-

---

*Eliminated by HUD for administrative reasons.

**See Housing Assistance Supply Experiment Staff, *Site Selection for the Housing Assistance Supply Experiment: SMSAs Proposed for Site Visits (A Briefing)*, The Rand Corporation, WN-7907-HUD, August 1972; and R. Dubinsky, *Collected Site Selection Documents: Housing Assistance Supply Experiment*, The Rand Corporation, WN-8034-HUD, January 1973.

iences of operating the experiment within fewer rather than more local jurisdictions.

Baseline Surveys

For each experimental site, we plan a program of field surveys to gather systematic data on preallowance conditions in the local housing market. For this purpose, we will select a stratified random sample of residential structures, including both rental and owner-occupied housing. The stratification (by tenure, size of structure, rent or value, and neighborhood density) distinguishes sectors of the housing market that we expect either to be differently affected by the allowance program or to respond differently to allowance-stimulated demand. Sampling rates will be highest in those sectors of the market within which we expect the most activity by allowance recipients.*

We propose to record data from tax records, examine the physical features of each structure included in the sample, and compile a systematic description of the neighborhood in which it is located. (These data, supplemented by information from the landlord and tenants, will be used to estimate the market value of each residential property at baseline.) Then, we will interview the owner and tenants of the structure.

We will seek data from the owner on his personal and occupational characteristics, his other dealings in real estate, the history and prospects of the subject property, its current physical facilities, its mortgage financing, and (for the preceding year) its rental revenues and its maintenance and operating expenses. We will also inquire about tenant-selection policies and the owner's views of his tenants.

From each tenant household, we will seek information on family composition, income and employment, and life-style; on characteristics and condition of the housing unit, tenant-landlord relationships, contract rent, and any additional housing expenses incurred during the preceding year; and on attitudes toward the housing unit, the landlord,

*See Timothy M. Corcoran, Eugene C. Poggio, and Tiina Repnau, *Sample Design for the Housing Assistance Supply Experiment*, The Rand Corporation, WN-8029-HUD, November 1972.

the neighbors, and the neighborhood. Finally, we plan to compile a history of each tenant's residential mobility, with coordinate data on family composition, income, employment, and housing characteristics.

Since the probability of selecting each sample element will be known from the sampling procedure, we will be able to estimate from the sample data the incidence of observed characteristics of housing, landlords, and tenants in the entire metropolitan area. Of particular importance, we will be able to estimate the annual revenue from and cost of production of housing services within the experimental site, by market sector and for the market as a whole.

Enrollment in the Allowance Program

As soon as the baseline survey work is complete, the experimental housing allowance program will be opened to enrollment. Determination of eligibility and allowance entitlement will require detailed information from each applicant about household composition and income. Additional information, paralleling some items included in the baseline tenant survey, can be obtained from applicants not covered in that survey. This should include the applicant's current housing expenditures, and a physical description of his housing.

The Experimental Housing Allowance Program

At the time of enrollment, HUD will explain the purposes of the experimental allowance program and will publicly commit funds for its support over a ten-year period.* Housing allowances will be offered to eligible families in amounts that increase with household size and decrease with disposable income from all other sources. The program will be open to both renters and homeowners who meet the income requirements (including imputed income from owner-occupied homes). Eligibility will not depend on prior housing conditions or prior housing expenditures. Any household residing in the metropolitan area when

*Because of legislative restrictions on the funds available for the experimental allowance program, the commitment must be complex. The terms and qualifications are discussed in Sec. III. The allowance program at each site will be administered by a nonprofit corporation funded by HUD under Sec. 23 of the Housing Act of 1937, as amended. Initially, the directors of these corporations will be appointed by Rand.

the enrollment period opens may be enrolled at any time thereafter, provided it meets the eligibility criteria at the time of application. To guard against allowance-motivated migration into the metropolitan area, those who arrive after the date at which enrollment is opened will be ineligible. Participants who leave the metropolitan area will be dropped from the rolls.

The amount of the allowance will be set so as to enable the recipient to afford the local cost of modest but well-maintained housing adequate for his household size. Generally, the recipient will have to supplement the allowance with other funds to cover the costs of such housing on the local market, so that the marginal dollar of housing expenditures will come out of his income from other sources.

Allowances will be paid only to otherwise eligible households that occupy housing units of a quality approved by the disbursing agency.* In other words, the program will not subsidize the occupancy of substandard housing but will provide clear signals to the enrollee and to landlords that occupancy of standard housing will be subsidized. At the landlord's request, the disbursing agency will inspect and certify a building for occupancy by allowance recipients. The recipient may also request approval of a housing unit that he has located, and prompt inspection service will be provided.

These specifications limit the fungibility of the housing allowance, ensuring that those now paying low rents for substandard housing will be impelled to seek better housing in order to benefit from the program. Although nondirective counseling will be offered to each enrollee, searching and bargaining for housing are left to the enrollee, impeded as little as possible by official presence.

For homeowners, we propose only slightly different arrangements. First, disposable income will include imputed income from their equities in their homes. Those who qualify for housing allowances under the income-and-family-size standards and whose homes meet the specified quality standards will receive allowances according to the same schedule offered to renters. Those whose housing is substandard may undertake

---

*For a national program, we would recommend instead that participation by a local jurisdiction in the allowance program be conditional on its meeting specified performance standards for housing-code enforcement.

privately financed home improvements in order to qualify for allowance payments; their certificates of eligibility for housing allowances will help to establish their creditworthiness.

The final element of the allowance program will be assistance to low-income home buyers. Both renters and homeowners who have enrolled in the allowance program and who wish to purchase homes will be assisted in obtaining mortgage interest subsidies under Sec. 235 of the National Housing Act, in lieu of housing allowances. (These subsidies, which endure for the life of the mortgage, reduce the effective mortgage interest rate to as little as 1 percent.) In these transactions, the usual FHA underwriting standards for Sec. 235 will apply, eliminating households whose incomes are very low and homes that do not meet FHA specifications.

Recertification

Allowance recipients will be required to appear in person at six-month intervals following enrollment, for eligibility review and recertification.* Allowance payments for the following six months will be based on income reported for the prior six months. Between recertification dates, those experiencing sharp drops in income may apply for an upward adjustment in allowance payments. Housing occupied by allowance recipients will be reinspected annually; continued certifiability is a condition for continuation of allowance payments.

Postenrollment Surveys

We plan to monitor the local housing market and the activities of allowance recipients for a period of five years following the commencement of enrollment in the allowance program. There are several elements to our monitoring plan. First, we will follow the panel of residential properties described above by means of an annual cycle of field surveys addressed to the structure itself, to the owner, and to the current tenants. Second, we will follow each allowance recipient by means of administrative records, possibly supplemented by questionnaires administered on a sample basis. Third, we plan several small-scale surveys or interview schedules to pursue special issues. Fourth, a resident

---

*HUD prefers annual recertification.

observer at each experimental site will be charged with informal monitoring of public events and attitudes bearing on the experiment.

The Panel of Residential Structures

Each year, fieldworkers will visit each residential structure for which a baseline record was compiled. In each case, four survey instruments will be administered:

1. Annual neighborhood survey. The baseline neighborhood evaluation will be repeated for each neighborhood containing one or more sampled structures. The emphasis in these resurveys will be on detection of changes in neighborhood characteristics and the quality of the residential environment.
2. Annual building survey. The baseline building survey will be repeated, with emphasis on detecting changes since the preceding year in the physical condition of the structure, the incidence of vacancies, conversions to and from nonresidential uses, new residential construction, and residential demolitions or abandonments.

   In addition to residential structures surveyed at baseline, we plan to follow a baseline probability sample of vacant parcels and parcels in nonresidential use within the urban portion of the site in order to capture evidence of residential construction or conversion to residential use. Outside the urbanized area, we plan to sample residential building permits annually for the same reason. Once a residential use is evident on a parcel, it joins our panel, to be fully monitored thereafter.
3. Annual landlord survey. We propose to reinterview owners or managers of all rental properties in our sample each year, to obtain a record of rental revenues and outlays for building maintenance and operations that is comparable to the data gathered at baseline. We will also inquire about capital improvements made during the year, and their cost. Finally, we will repeat our inquiries about sources and terms of mortgage financing and insurance, difficulties with tenants and vandals, etc.

The financial data to be gathered in these annual surveys are designed to enable us to estimate for each property the annual changes since baseline in rental revenues and in total costs of production. Deflating the latter by means of factor-price indexes, we expect to be able to estimate changes over time in real factor inputs used to produce housing services, and to compare these with concurrent or lagged changes in revenue. Thus we arrive at measures of the supply response to changing demand conditions in the marketplace.

Other aspects of this survey are aimed at enlarging our understanding of housing investment and operating policies and perceptions by owners of the changes that are occurring in the housing market as a result of the allowance program and of other factors.

4. Annual tenant/homeowner survey. We plan annual interviews of the current tenants of each housing unit included in the baseline sample. In many instances, these will be the same households interviewed in previous years. They will include both allowance recipients and nonrecipients.

In reinterviews, the emphasis of our surveys will be on changes in household composition, income, and employment; on changes in the characteristics or condition of the dwelling unit, contract rent, and other housing-related expenditures; and on changes in attitudes toward the housing unit, the landlord, the neighbors, and the neighborhood. Where we encounter a new tenant, the survey instrument will seek the full range of information captured at baseline. For homeowners, the annual survey will also seek a year's record of housing operating and maintenance costs and mortgage and insurance data similar to those sought from owners of rental property.

In these surveys, our principal purposes are to obtain the data about household budgets needed to estimate how income changes within the experimental site may be affecting housing expenditures; to supplement the neighborhood, building, and landlord surveys with additional information from

actual residents bearing on changes in the neighborhood and in the respondents' housing; and to learn how the supply responses of landlords are conditioned by tenant characteristics and attitudes and how tenants respond to housing improvements and rent increases. We are especially interested in tenant turnover and its relationship to housing characteristics and management policies, and in patterns of household mobility after the commencement of the allowance program. For this latter purpose, we may find it worthwhile to follow baseline respondents who subsequently move from sampled structures.

An important feature of the panel survey program described above is the opportunity it provides for linking housing characteristics, landlord characteristics, and tenant characteristics within individual structures at each survey date; and for following these linked relationships over time. Such microdata, with both cross-sectional and longitudinal dimensions, are extremely rare in social science research; we expect them to be useful for analysis both of housing-allowance issues and of more general questions relating to the dynamics of local housing markets.

Tracking Allowance Recipients

The disbursing agency for the experimental housing allowance program will maintain continuous records on all participants in the program and on their housing circumstances. Every six months, each recipient will be required to come to the allowance office for eligibility review and recertification, at which time any changes in income or family composition will be recorded. When an allowance recipient moves, he must, of course, report his new address, but he must also request an inspection and certification of his new quarters. The agency will also maintain a current record of the landlord's name and address and of the recipient's contract rent.

Thus, administrative requirements alone provide a substantial source of data for following allowance recipients over time. From these data, we can analyze changes in their housing characteristics and housing expenditures and determine their patterns of residential mobility. We may also find it useful to supplement administrative

records by a questionnaire to be administered to a sample of allowance recipients at the time of their semiannual eligibility review; however, since substantial numbers of recipients will be included in the panel survey of tenants and homeowners, we are not now certain that the added information from a direct sample of recipients will be needed.

Data on the pattern of residential location of allowance recipients will also feed into our analysis of landlord and tenant attitudes and actions; we wish to examine their perceptions of neighborhood changes that are possibly related to the allowance program in the light of our direct knowledge of the extent and nature of these changes.

Other Surveys

We foresee the need for a number of special-purpose data-gathering efforts during the course of the experiment, some of which may entail small-scale surveys. For instance, we plan annual interviews of the major institutions providing residential mortgage financing within each of the sites, to learn from them as much as we can about their policies and their perceptions of the allowance-stimulated market. Firms writing property insurance will be surveyed in a similar manner. We may undertake special inquiries among building-trade contractors or other participants in the local housing market.

Other groups of special interest are low-income households encountered in our panel survey who appear to be eligible for housing allowances but who fail to apply for them, and those who enroll but fail to find certifiable housing. We expect to explore their circumstances with special survey instruments, possibly administered separately from the annual tenant/homeowner survey.

For the years after baseline we may also find it advantageous to select a special sample of nonparticipant households as subjects of an annual survey of attitudes toward the allowance program. A special sample may be needed because our sample of residential properties is designed so that most of the tenants interviewed there will be households of low-to-moderate incomes; additional representation of the politically more influential upper-income population may prove desirable.

Informal Monitoring

We plan to place a full-time resident observer at each experimental site for the duration of the monitoring period. His principal assignment will be to gather informal intelligence about community reactions to the allowance program and to provide early warnings of possible difficulties. The monitor will spend much of his time on the street and attending meetings of civic and other local interest groups, reading local newspapers, and following events in City Hall.

Provided with technical support and staff assistance, he may also be asked to conduct some of the less formal small-scale surveys described above, or to search out local data from public records or other sources that bear on issues of experimental interest.

Termination of the Experiment

Although we propose allowance commitments for a period of ten years, we believe that the information returns to the experiment will drop off sharply in the later years of that commitment. The ten-year commitment is needed to provide stable expectations. Allowance recipients must be convinced that their budgetary resources will be augmented for a long enough time to commit their families to a higher level of housing consumption, which may require a lease at higher rent, change of residence, or a change from rental to ownership tenure. The suppliers of housing must believe that the housing demand stimulated by the allowance program will last long enough to provide a continuing market for housing at rents that will amortize capital improvements.

If these *expectations* are provided by a ten-year program commitment, we believe that most of the dynamics of market response and realignment will have become evident at the end of five years, and the apparatus for monitoring the experiment can be dismantled. The allowance program can then be turned over to local control for continued operation under HUD funding.

III. THE EXPERIMENTAL HOUSING ALLOWANCE PROGRAM

The general purpose of the Supply Experiment is to provide information about the probable effects of a national housing allowance program. As outlined in Sec. II, our strategy for obtaining this information is to mount an experimental allowance program in each of two small metropolitan areas, matching on a geographically small scale the essential features of a national program.

Since a national housing allowance program does not exist, we must in many respects invent the model that we propose to copy in the Supply Experiment. However, there seems to be a consensus on the basic features of a national housing allowance program that distinguish it from existing methods for delivering housing assistance to low-income households and which, taken together, form a coherent alternative to these methods. Thus, we postulate the following essential features of the hypothetical national program to be emulated in our experimental program:

1. The purpose of the program is to enable and persuade low-income households to live in housing that meets specifiable minimum standards of health, safety, and decency for family life. In general, these standards can be met by a well-maintained older housing unit whose size is appropriate for the number of persons in a recipient's household.
2. The housing allowance strategy for attaining this purpose entails direct financial assistance to those who are judged unable to afford the market price of housing that meets these standards. Unlike the case with existing programs, enrollment is not limited by a predetermined number of "places" but is open to all who meet the personal and financial criteria for eligibility.
3. This assistance will be in some way earmarked for housing expenditures; while the amount of assistance provided must take into account the recipient's ability to pay for housing, housing allowances are not intended as general budgetary supplements.

4. Having provided the necessary purchasing power to low-income households, the administering agency will limit its intervention in the process of housing choice to nondirective counseling; and in the relationships between tenants and landlords to the minimum consistent with fair housing laws and with the agency's accountability for public funds. It is assumed that a recipient will work through ordinary market channels to find acceptable housing and that he is capable of negotiating on his own behalf concerning rent and conditions of occupancy. The administering agency will have neither a current nor a contingent obligation to the seller of housing services.

Working within these principles, members of Rand's staff have developed a model for a national housing allowance program which includes specific eligibility criteria, an allowance formula, a method of payment, and earmarking provisions.* For the Supply Experiment, Rand has proposed a modified version of that model. The modifications reflect in part the local nature of the experimental program and in part specific experimental needs not present in a national program. More importantly, they reflect constraints imposed by existing legislation and administrative regulations on the use of HUD funds for housing allowances.

We hasten to add that neither our model for a national program nor the modified version proposed by us for the Supply Experiment are uniquely consistent with the four principles cited above. Within their

---

*This model grew out of research into the housing problems of New York City, conducted by The New York City-Rand Institute under contract to the City's Housing and Development Administration. See Ira S. Lowry, Joseph S. De Salvo, and Barbara M. Woodfill, *Rental Housing in New York City*, Vol. II, *The Demand for Shelter*, The New York City-Rand Institute, R-649-NYC, June 1971, Sec. VII and Appendix F; the logic and design for a national program is developed in Ira S. Lowry, "Housing Assistance to Low-Income Urban Families: A Fresh Approach," in *Papers Submitted to Subcommittee on Housing. Panels on Housing Production, Housing Demand, and Developing a Suitable Living Environment*, Committee on Banking and Currency, U.S. House of Representatives, 92nd Congress, First Session, U.S. Government Printing Office, Washington, D.C., 1971, Part I, pp. 489-524. The latter paper has also been reprinted by The Rand Corporation (P-4645, May 1971).

framework, other variants are possible. A number of such variants will be tried in the Demand and Administrative Experiments. Our preferences reflect our own judgments on many details of design whose programmatic virtues are, in the absence of experimental evidence, arguable.

HUD has provisionally and informally accepted a number of important features of our proposal: the use of disposable income, family size, and the local cost of standard housing as the principal factors entering determination of need for assistance; rent certificates as the principal means of payment; a housing-quality standard for allowance recipients as the principal means of earmarking; portability of allowances within the boundaries of the experimental site; and minimization of direct relationships between the administering agency and landlords whose tenants are allowance recipients.

However, HUD has rejected our proposed income standards for eligibility and a related formula for determining allowance payments, citing reasons both of policy and administrative difficulty. Although we are not convinced that either obstacle is insurmountable, we have agreed to respect HUD's preferences on these issues in designing the allowance program for the Supply Experiment.*

In the following pages, therefore, we describe an experimental housing allowance program based on our earlier work but modified to

---

*The allowance formula originally proposed by Rand for the Supply Experiment is, we believe, better attuned to the budgetary problems of households of different incomes and sizes than the formula chosen by HUD (which is presented below). Taking explicit account of budget standards for nonhousing consumption as well as the costs of adequate housing, the Rand formula generally provides larger benefits at low incomes than does the HUD formula, the difference decreasing as income rises; and Rand's upper income limit for eligibility is lower for small households but higher for large households than HUD's.

For a systematic analysis and comparison of the properties of these and other allowance formulas and earmarking provisions, their effects on recipient housing choices and related behavior, and comparable estimates of allowance benefits, see Ira S. Lowry, Mack Ott, and Charles Noland, *Housing Allowances and Household Behavior*, The Rand Corporation, WN-8028-HUD, January 1973. For comparisons of program costs under various assumptions, see Barbara M. Woodfill and Tiina Repnau, *Estimates of Eligibility and Allowance Entitlement Under Alternative Housing Allowance Programs*, WN-7974-HUD, September 1972; and Tiina Repnau and Barbara M. Woodfill, *Additional Estimates of Enrollment and Allowance Payments Under a National Housing Allowance Program*, WN-8167-HUD, March 1973.

meet HUD's views with respect to the income standard and allowance formula. However, it is important to note that this description is still a design *proposal*, yet to be formally accepted by HUD.

GENERAL PROGRAM STANDARDS

The structure of our experimental housing allowance program is embodied in program standards of four types: those relating to eligibility for assistance, those that determine each participant's allowance entitlement, special conditions governing receipt or use of allowances, and commitments as to the duration of allowance entitlement. Below, we specify standards of each type, distinguishing those that reflect allowance-policy decisions, those that must be added because of the experimental nature of the program, and those that reflect legislative constraints on the experimental program.

Eligibility for Assistance

The basic principle of eligibility under a national housing allowance program is that assistance should be available to all households whose income from other sources does not enable them to afford the market price of housing that meets a specified standard of adequacy. We interpret this principle to mean that eligibility is independent of tenure, prior expenditures for housing, or occupancy of a particular housing unit. Rather, it depends on a comparison of household income with the market price of adequate housing, taking into account nonhousing consumption needs in calculating a household's ability to contribute toward its own housing expenses.

Income Limits for Eligibility. Following HUD's views, we propose an income limit for eligibility that varies with household size, of the following form:

$$Y_d^* = 4R^* \,, \tag{3.1}$$

where Y_d^* = maximum disposable income for an eligible household of n persons; and

R^* = standard cost of housing, including utilities, that meets specified quality standards and whose size is adequate for n persons.

We propose to relate eligibility (and the amount of allowance entitlement) to *disposable income* rather than *gross income*. For program purposes, we define disposable income as gross income less Federal, state, and local income taxes; compulsory social insurance payments; and a standard allowance for the work-related expenses of employed members of the household.

Existing Federal programs of housing assistance all express income limits in terms of *adjusted gross income*; the adjustments vary from program to program, usually providing deductions for minor children or excluding their earnings, and in some cases allowing deduction of Social Security taxes and union dues. In no case do the adjustments go far enough to equalize the disparities in disposable income between households dependent on current earnings (subject to income and Social Security taxes and accompanied by unavoidable work-related expenses) and those dependent on pensions or transfer payments (generally nontaxable). Households supported by the employment of one or more members are invariably penalized.

At the same time, we propose to count as part of gross income an imputed income from assets that do not yield a cash flow. In particular, we have in mind equities in owner-occupied housing, the return from which takes the form of a flow of housing services. Here again we depart from past Federal practice in housing assistance and other transfer programs; these usually impose a separate limit on the market value of assets as well as a limit on adjusted gross income.

The standard cost of adequate housing, R^*, will be determined from data gathered at each experimental site prior to the opening of enrollment. In the course of screening residential structures for inclusion in the baseline panel, we plan a brief survey of the residents of nearly 10,000 housing units at each site. For each rental unit, we will record current contract rent, division of responsibility for utility bills, number of rooms, and a series of items bearing on quality and code compliance. From these data, we will calculate gross rents and select the lowest gross-rent level for each size of unit within which a specified proportion (e.g., one-half to three-fourths) of all units pass the quality test, thus determining R^* for each size of unit. We will

also determine the number of rooms or bedrooms needed to accommodate households of each size, thus fixing R^* for each size of household.*

These parameters serve to determine eligibility and, as will be shown below, to fix the amount of allowance entitlement. In a national program, we would expect to set values for R^* to reflect local or regional differences in housing costs, though perhaps not by the procedures here described; over time, we would expect these values to be adjusted to reflect changes in the market price of housing. For the experimental program, the values of R^* will be set in advance of the opening of enrollment; neither HUD nor the local agency administering the allowance** will be committed to adjusting these values in the event of price changes, although this may be done at some point during the life of the program if it appears that background inflation, unrelated to the allowance program, is unduly depreciating the value of scheduled allowances.

Definition of Household. For purposes of determining eligibility and allowance entitlement, a household is defined as one person living alone, or two or more persons living together all of whom contribute jointly to housing expenses unless legally dependent on a member who does contribute. Ordinarily, a household consists of persons related by blood or marriage, but unrelated individuals may be included, provided their income is counted in the amount used to determine eligibility. An eligible household may even consist entirely of unrelated individuals, provided it does not exceed a specified size; under some circumstances, unrelated full-time students may be included in such households.

The legislative authority under which the experimental program will be funded requires some qualification of this definition. Persons under

---

*For a more complete account of these plans, see David B. Lewis and Ira S. Lowry, *Estimating the Standard Cost of Adequate Housing*, The Rand Corporation, WN-8105-HUD, March 1973.

**The latter is a Housing Allowance Office (HAO) to be established at each experimental site. Its structure and functions are described briefly later in this section.

age 62 who live alone will be ineligible for assistance unless handicapped or residentially displaced by a natural disaster or by actions taken under specified Federal programs such as urban renewal.

Residency Requirement. In a national housing allowance program, there would be no residency requirement. Housing allowances would be available across the country, and program participants who moved could reestablish eligibility in their new place of residence, subject to the standards in effect there.

For the experimental program, special rules must be devised both to limit the Federal commitment to the amounts needed for experimental purposes and to forestall disruption of the local housing markets owing to movement into the experimental sites by low-income households eager to share in allowance benefits. We therefore propose two residency requirements, one for initial and the other for continued eligibility.

For a household ever to be eligible for an allowance under the experimental program at a given site, at least one adult member of that household must have resided within the boundaries of that site on a specified date prior to the date on which enrollment is opened at that site. Thus, later inmigrants will be ineligible for assistance, whatever their incomes. Households meeting the residency requirement, however, may later be admitted to the program even though they were ineligible for other reasons at the time of its inception.

Once enrolled in the experimental program, a household will continue to be eligible for assistance as long as its income does not exceed the limit for its size and as long as it continues to live within the boundaries of the experimental site. Outmigrants will lose their allowances for the period of their absence but will be able to reenroll upon returning to the site if still otherwise eligible.

These rules provide the Housing Allowance Office (HAO) with considerable control over the magnitude of the program and should lead to a relatively stable flow of allowance payments throughout its term. Attrition through outmigration and death of recipients will be partly offset by a natural increase in the membership of recipient households and by enrollment of households who become eligible as they withdraw from the labor force because of age or illness. We anticipate a small

net decrease over time in the number of eligible households; if this decrease were in fact large enough to substantially reduce the flow of allowance payments into the local housing market, we would propose opening enrollment to recent (but not future) inmigrants. Program rules should never enable a prospective inmigrant to count on housing assistance.

Amount of Assistance

The size of the allowance to which a household is entitled will be calculated by means of a formula that takes into account differences in housing needs and other consumption requirements among households of different sizes, and which reflects the current relationship between a household's disposable income and the cost of living, including the cost of adequate housing. It is designed so as to fit into the existing mosaic of public-assistance programs (Social Security, unemployment compensation, welfare) without providing windfalls for their beneficiaries, and to adapt housing-assistance payments automatically to future changes in benefits provided by these other assistance programs--or for that matter, to changes in income-tax laws which alter the amount retained by recipients out of a given gross income.

The formula provides for housing assistance equal to the difference between the standard cost of adequate housing (varying with household size) and a specified percentage of the disposable income of the assisted household. It is thus a member of the "housing-gap" family of allowance formulas whose general form is

$$A = \alpha R^* - \beta Y_d \,, \tag{3.2}$$

where A = amount of allowance entitlement;

R^* = standard cost of adequate housing;

Y_d = disposable income from other sources; and

α and β = policy parameters.

For the Supply Experiment, HUD wishes to set $\alpha = 1$ and $\beta = .25$,* so that Eq. (3.2) reduces to

$$A = R^* - .25Y_d \ . \tag{3.3}$$

In principle, at least, a household with no income from other sources would be entitled to an allowance of R^*. Such an extreme case is unlikely inasmuch as all states provide public assistance to nearly all individuals and families who are without other means of support; and in calculating allowance entitlement, welfare payments and net benefits from the Federal Food-Stamp program will be counted as disposable income.

As disposable income increases from zero under this formula, the amount of the allowance entitlement decreases at the rate of 25 cents per dollar of additional disposable income.** When disposable income reaches $4R^*$, the allowance entitlement drops to zero; thus, as noted earlier, $Y_d^* = 4R^*$ is the upper income limit for eligibility.

As explained earlier, the standard cost of adequate housing, R^*, will be determined for each size of household from local data at each experimental site. However, to give the reader a general indication of the levels of allowance payments contemplated by this formula, we have prepared crude estimates of R^* for each size of household, shown in Table 3.1. These estimates are averages for the urban population of the United States, based on rents current in 1969*** for well-maintained but modest housing units, the number of rooms in each case

---

*Other values of β are to be tested in the Demand Experiment; although α will not be explicitly varied from unity in the demand Experiment, the procedures used there to determine R^* seem to be the equivalent of setting $\alpha > 1$ for R^* as defined above. HUD has urged that these same procedures be followed in the Supply Experiment.

**Over the full range of eligibility, the formula thus imposes a "tax" on income from other sources, the amount of the tax depending on the sources of other income. See Lowry, Ott, and Noland, op cit., WN-8028-HUD, pp. 81-92 and Appendix G, for analysis of the implied marginal tax rate on earned income under this and other allowance formulas.

***We used 1969 data for these estimates to facilitate computation of the number of eligible households from household distributions by size and income compiled from the 1970 Census of Population. Incomes reported by the 1970 Census are for calendar year 1969.

Table 3.1

ILLUSTRATIVE ESTIMATES OF ALLOWANCE-PROGRAM STANDARDS, BY SIZE OF HOUSEHOLD: U.S. URBAN HOUSEHOLDS, 1969

Household Size	Number of Rooms[a]	Annual Amount in 1969 Dollars		
		Standard Cost of Adequate Housing (R^*)	Upper Limit of Disposable Income (Y_d^*)	Corresponding Gross Income from Earnings[b]
1	2	1,033	4,132	5,703
2	3	1,162	4,648	6,089
3	4	1,292	5,168	6,609
4	5	1,377	5,508	6,846
5	5	1,464	5,856	7,155
6	6	1,549	6,196	7,450
7	6	1,592	6,368	7,498

SOURCE: Woodfill and Repnau, op. cit., WN-7974-HUD, Secs. III and IV.

NOTE: The estimate of R^* for four-person households is based on housing-expenditure data compiled by the U.S. Bureau of Labor Statistics for four-person families living in rental housing units meeting standards set by the American Public Health Association; it is essentially the median gross rent paid by such families. Comparable estimates for other household sizes were developed by Rand from a study of the variation in housing costs with size of unit.

[a]Room count excludes bathrooms, hallways, and unfinished basements or attics. Kitchenettes and dinettes are counted as half-rooms.

[b]Amount of gross income from earnings of one employed person required to yield indicated disposable income after deduction of income taxes, social insurance, and an allowance of $350 for work-related expenses.

reflecting standards suggested by the American Public Health Association. The values range from $1,033 annually for a two-room apartment for a single person to $1,592 for a six-room house for a seven-person household. On a monthly basis, these values are equivalent to gross rents (inclusive of utilities) of $86 to $133. (Gross monthly rents for equivalent housing in 1973 would have ranged from about $100 to $150, an increase of about 13.3 percent over 1969.)

Table 3.1 also shows upper income limits for eligibility, based on these housing cost standards. The limit expressed in disposable income (gross income less income taxes, social insurance, and an allowance of $350 for work-related expenses of employed persons) is $4R^*$ under our formula. The last column of the table gives an estimate of the corresponding gross income from earnings for a household with only one employed member; the amounts range from $5,703 for a single employed person to $7,498 for a household of seven persons.

In Table 3.2, we show a schedule of allowance entitlements based on the program standards just described. For each size of household, the amount of the allowance decreases from R^* to zero as disposable income rises from zero to Y_d^*. A casual glance at this schedule probably leaves an impression of higher average allowance payments than is actually the case, since the first few rows are for levels of incomes whose incidence in the population is low, and the last few columns are for household sizes whose incidence is also low. We estimate that the average annual entitlement for the national population of households eligible under those standards in 1970 would have been about $480. If single persons under 62 years of age are excluded from the program, the average allowance entitlement would be higher, about $530.

In general, our analyses indicate that the allowance schedules implied by the proposed formula conform also to constraints imposed by the legislative restrictions under which the experimental program is to be funded. The major legislative constraint is the Brooke Amendment, which limits the housing expenditure that may be required of an assisted household to 25 percent of its adjusted gross income. The rules of the experimental housing allowance program do not require any

Table 3.2

ILLUSTRATIVE SCHEDULE OF ALLOWANCE ENTITLEMENT, BY INCOME BY SIZE OF HOUSEHOLD: U.S. URBAN HOUSEHOLDS, 1969

Disposable Income (Y_d in $)	Annual Entitlement in 1969 Dollars, by Household Size						
	1	2	3	4	5	6	7
1,000	783	912	1,042	1,127	1,214	1,299	1,342
1,500	658	787	917	1,002	1,089	1,174	1,217
2,000	533	662	792	877	964	1,049	1,092
2,500	408	537	667	752	839	924	967
3,000	283	412	542	627	714	799	842
3,500	158	287	417	502	589	674	717
4,000	33	162	292	377	464	549	592
4,500	0	37	167	252	339	424	467
5,000	0	0	42	127	214	299	342
5,500	0	0	0	2	89	174	217
6,000	0	0	0	0	0	49	92
6,500	0	0	0	0	0	0	0

SOURCE: Computed from program standards in Table 3.1.

specific contribution by a participating household, but the Federal contribution will amount to standard housing cost less 25 percent of the recipient's disposable income, an amount which is almost always less than 25 percent of his adjusted gross income.

Conditions of Assistance

To distinguish a housing allowance program from general income transfers, some method of earmarking allowances for housing expenditures is needed. While various methods are possible, all require the recipient either to spend at least a specified amount for housing or to occupy housing that meets specified standards of space and quality. Because of its more direct linkage to program objectives, we have chosen the latter course despite the greater administrative burden it entails.*

*See Lowry, Ott, and Noland, op. cit., WN-8028-HUD, for a discussion of alternative earmarking schemes and the incentives for housing expenditure, housing choices, and fraudulent practice that each brings into play.

Under the experimental allowance program, an applicant will be enrolled as soon as his income and household size have been verified. However, allowance payments will begin only when the enrollee is able to show that his housing meets certain standards for quality and condition of structure adapted from local and national model housing codes, and that it conforms to specified standards of minimum space per person. These standards may be more elaborate than, but essentially the same as, the standards used to select local values for R^*, the market price of adequate housing at each experimental site. In general, well-maintained older housing will qualify.

At each experimental site, certification of housing units for occupancy by allowance recipients will be accomplished by inspections conducted by the HAO.* In some cases, the housing occupied at the time of enrollment may qualify. If it does not, the enrollee must improve the property if he is a homeowner, or, if he is a renter, persuade the landlord to improve it; or in either case, he may move to housing that does meet the standard.

Because of the procedure used to determine R^*, we are assured that certifiable housing is generally available in the community for rents in the vicinity of R^*; and we at least presume that additional units can be brought up to the standard of certifiability and profitably marketed at rents or annual ownership costs in the same vicinity. (One of the purposes of the experimental program is to test this presumption.) The allowance schedule is constructed to ensure that all enrollees, of whatever household size and income, can afford to spend R^* for housing, combining the allowance with some amount taken from their other resources. Thus, the quality standard compels enrollees to pay for adequate housing as a condition of receiving the allowance that enables them to do so.

---

*For a national housing allowance program, we would not recommend such a special inspection system. Rather, we would recommend that any municipal jurisdiction's participation in the program be conditional on its adopting a model housing code and meeting specified standards of enforcement.

This is not to say that program regulations will require any recipient to spend exactly or at least R^*. Within the market, certifiable housing will vary in cost; some may find such housing at a rent or annual homeowner cost less than R^*, while others may pay more, perhaps for special features that they find attractive. Since the amount of the allowance does not vary with actual expenditures, there is an obvious incentive to seek the best bargain available that meets program standards of quality.

Once in a certified housing unit, an enrollee would be entitled to allowance payments as long as the unit continued to meet program standards. Each such unit would be reinspected annually at the instance of HAO, although other events might lead to interim inspection and decertification.* After notifying the landlord and allowance recipient of decertification, the HAO would allow reasonable time for corrective action and recertification. If the dwelling is not brought into compliance with program standards by the end of that period, allowance payments would ' case until either the unit is recertified by inspection or the enrollee moves to another, certified unit.

Duration of Assistance

Once approved and funded by the Congress, a national housing allowance program would presumably be regarded by the public as a permanent feature of local housing markets, even though it could be repealed by a subsequent Congress and even though its funding would require annual appropriations. This sense of permanence would have an important influence on the behavior of both allowance-eligibles and suppliers of housing services.

Without the expectation that allowance payments would continue for a long period of time, those eligible to participate in the program would, we think, hesitate to move, to alter their present housing arrangements, or to enter into financial commitments beyond their preallowance means. If landlords, developers, and speculative rehabilitators of residential property did not believe that the allowance-stimulated demand for housing services would persist for a long time, they would be reluctant to commit themselves to capital improvements or long-term investments.

---

*HUD has expressed opposition to recertification inspections, preferring to rely on annual recertification reports completed by the allowance recipient.

It thus becomes a matter of importance to design the experimental allowance program so that it evokes the sense of permanence associated with a national program. To create the appropriate climate of expectations, we believe that HUD must make public at each experimental site its commitment to fund the experimental allowance program for a period of ten years.

To obtain better housing, those eligible for assistance may have to sign leases at higher rents than they could support without allowances, move to new quarters, undertake capital improvements on homes that they own, or shift from rental to ownership tenure with a long-term mortgage commitment. For renters, our judgment is that a five-year allowance commitment would yield nearly the same behavior as a ten-year commitment; longer leases are not likely and moving costs amortized over five years become insignificant relative to allowance benefits. For homeowners contemplating capital improvements, the critical issue is likely to be the need for a home-improvement loan to be amortized over five to ten years. For those considering a shift from rental tenure to ownership, an allowance commitment matching the required mortgage commitment in its duration seems appropriate both to properly influence expectations and to avoid placing HUD in the politically and ethically awkward situation of encouraging low-income households to assume obligations that they cannot meet.

In the rental market, we think that the landlord's expectations are critical. Neither the experimental nor a national housing allowance program offers the landlord any guarantee that he will benefit from the allowance payments. He will observe a general increase in low-income housing demand, a share of which he may be able to capture in competition with other landlords. The risks he is willing to take to capture a share of this demand (benefiting from a higher occupancy rate or higher rents or both) clearly will depend on his perception of its durability. Whatever the mix of allowance recipients' preferences between increased services and capital improvements, a short-term allowance commitment would bias a landlord's response toward service improvements, as compared with his reaction to a permanent allowance program, and toward less total increase in factor inputs.

We do not think that even a ten-year allowance commitment would create for landlords and speculative rehabilitators of residential property all of the expectations of a permanent program. Structural rehabilitation on 20-year mortgage financing is not uncommon; some Federally subsidized rehabilitation mortgages have terms of 40 years. As we understand such investments, cash flow rather than equity accumulation is usually the dominant consideration--if not to the borrower, then to the lender. If so, every dollar increase in annual debt service must be matched by a dollar increase in expected annual revenue to provide a horizon-neutral investment incentive.

Assume that with an unlimited horizon for the allowance-stimulated demand increase, a landlord could get 21-year financing for rehabilitation at 8 percent and concluded that such an investment would be marginally profitable. With a ten-year or five-year allowance commitment, the investment would be less interesting to both the landlord and the lender. To amortize the investment over ten years would require annual debt-service charges that are 1.6 times the charges for 21-year amortization; a five-year amortization schedule raises the annual charge to 2.5 times the 21-year charge.*

If we also assume a balanced program of housing improvement with proportional increases in all inputs, over a typical initial mix of inputs, these alternative horizons yield different price elasticities of supply. If the 21-year horizon has an elasticity of unity, we estimate that the ten-year horizon would have an elasticity of .96 and that the five-year horizon would have an elasticity of .90. Different but reasonable assumptions would yield slightly different answers, but we believe that these illustrative calculations reflect as well as any others the probable biases in supply responsiveness associated with short-term allowance commitments. In our judgment, the 4-percent bias associated with a ten-year term would not seriously degrade the usefulness of our experimental findings, especially since the direction

---

*The computations leading to these conclusions can be found in Lowry, Rydell, and de Ferranti, op. cit., WN-7711-UI, pp. 55-56. Also see C. Peter Rydell, *The Landlord Reinvestment Model: A Computer Based Method of Evaluating the Financial Feasibility of Alternative Treatments for Problem Buildings*, The Rand Corporation, P-4477, October 1970.

of bias is clear. The 10-percent bias associated with a five-year commitment is, we think, larger than is consistent with experimental purposes and credibility.

Thus it is that we have asked that HUD make a public commitment to continue the experimental allowance program for ten years for renters and homeowners, and, where home purchase is involved, for the life of the mortgage. Of course, any individual recipient may lose his eligibility if his income increases beyond program limits or if his housing ceases to meet program standards. What is guaranteed is the continued existence of the allowance program.

The commitment is somewhat qualified, however, by the expectation that midway during its term, monitoring activities will cease and the HAO will be turned over to local control. Under these circumstances, HUD is not prepared to guarantee continuation of the allowance program in exactly the same forms as during the monitoring period.

The exact forms of assistance to be offered during the postexperimental portion of the ten-year period will be determined by HUD and the HAO prior to the conclusion of monitoring activities. However, it is agreed that households participating in the program at the end of the monitored phase will be eligible for continued assistance during the second phase and will be informed of any steps they must take to requalify. The HAO may elect to continue assistance to recipients without change in form, provided it can do so within the constraints of legislation then in effect. The most probable change is termination of the portability of allowances: Recipients may lose their entitlement if they move during the postexperimental phase.

In the event that unforeseen circumstances require the experimental aspects of the housing allowance program to be terminated sooner than is now planned, the HAO will nonetheless continue to provide the promised assistance for the full ten-year period.

PROGRAM ADMINISTRATION

The experimental housing allowance programs at each site will be principally funded under Sec. 23 of the United States Housing Act of 1937, the "leased public housing program." Special administrative regulations promulgated by HUD enable modification of the usual form

of this program to fit the requirements of the Supply Experiment.*

Ordinarily, housing programs funded under Sec. 23 are administered by a duly constituted Local Housing Authority (LHA), and the existence of such a body is a legislative requirement for Sec. 23 assistance. Local Housing Authorities, either municipal or countywide in jurisdiction, exist in each metropolitan area under consideration as a site for the Supply Experiment. However, these bodies now administer programs that are small relative to the anticipated size of the experimental allowance programs.

In order to provide administrative capacity for rapid enrollment in and subsequent management of the large-scale experimental program, we propose to establish a nonprofit corporation at each site, which will be the HAO. HUD will fund the program by entering into an Annual Contributions Contract with a selected LHA, preferably one with jurisdiction over the entire experimental site; by prior agreement, the LHA will in turn contract with the HAO to administer the program and to disburse the funds.

Each HAO will be governed by a Board of Trustees whose members are appointed by an officer of The Rand Corporation. From the beginning, the Board will include both Rand-affiliated members and representatives of the local community; over time, Rand members will be replaced by community members so that when the experiment terminates (about halfway through the term of the allowance program), control of the HAO will be vested in community representatives.

Soon after its establishment, the Board of Trustees will appoint a Director for the HAO, who will have responsibility for staffing the office and administration of the experimental allowance program. The HAO's functions will include provision of public information about the allowance program, outreach and eligibility screening, certification and recertification of eligibility, enrollment of eligibles, determination of allowance entitlement, provision of housing information and equal opportunities counseling to enrollees, disbursement of allowance payments, inspection and certification of housing units occupied by recipients, and responding to grievances submitted by program applicants or participants.

---

*See "Program Funding and Forms of Payment," below.

Technical support and assistance in staffing will be provided to the HAO at each site by The Rand Corporation. Our general objectives are (1) to create organizations capable of managing the heavy workload of the experimental program, (2) to ensure that administrative procedures conform to experimental needs both substantively and with respect to record-keeping, and (3) to build an institution with the staff resources needed to operate the allowance program after Rand's direct involvement terminates.

PROGRAM FUNDING AND FORMS OF PAYMENT

Presumably, a national housing allowance program would operate under specific legislation that authorizes expenditures of appropriated funds as needed to achieve program purposes. Typically, such legislation allows considerable administrative discretion in program design.

The Supply Experiment lacks such freedom. Funds for the experimental allowance program must be drawn from appropriations for existing housing assistance programs, and their use must conform to legislative restrictions and administrative regulations conceived in a different context. Our task is to construct an allowance format for housing assistance out of these materials.

For a national program, we would propose disbursement of housing allowances in the form of monthly certificates redeemable through commercial banks specified by the disbursing agency. For recipients who are renters, the certificates would be endorsed to their landlords in partial payment of contract rent; they would not be otherwise negotiable. For recipients who are homeowners or home buyers, the certificates would be endorsed to the mortgagee; or, in the rare instances of allowance entitlement in excess of debt-service obligations, into a reserve account jointly administered with a trustee, from which payments could be made for insurance, taxes, maintenance, and operating expenses. In no case would the disbursing agency have a contingent obligation beyond the current amount of allowance entitlement.

Funds for the Supply Experiment's allowance program will come primarily from the leased public-housing program authorized under Sec. 23, as amended; and secondarily from the mortgage insurance and interest

subsidy program authorized for low-income home buyers under Sec. 235 of the National Housing Act, as amended. The first of these sources is much more flexible than the second in the program formats permitted by law, and HUD has provided special administrative regulations for the use of Sec. 23 funds in a housing allowance format.*

To approximate housing allowance formats as nearly as possible under these constraints, we have found it necessary to devise three distinct programs: one for renters, one for homeowners, and one for home buyers. In the first two cases, the approximation is close. In the third, it is less so; our proposal does not substantially differ from existing practice.

Allowance Payments to Renters

In the experimental allowance program, funds needed to provide assistance to eligible renter households will be provided under the authority of Sec. 23, but contractual relationships and the form of payment will differ from the usual arrangements.

Under this legislation, HUD normally enters into an Annual Contributions Contract with an LHA, guaranteeing funds to provide housing assistance for a specified number of low-income households living in privately owned housing units. Ordinarily, the LHA searches the local housing market for appropriate units, leasing them from their owners at a negotiated rent (presumably fair market rent) for a minimum term of one year; sometimes, advance leasing commitments are offered to developers for units yet to be built or rehabilitated. Then, acting as landlord, the LHA sublets these units to eligible households at rents below the market, usually determined by comparison with rent schedules in public-housing units owned by the LHA. The amount of the subsidy, covered by the Annual Contributions Contract, is the difference between the lease rent and the sublease rent on each unit. The basic contractual relationships are shown in Fig. 3.1.

---

*At this writing, HUD has provided such regulations only for what is described below as "allowance payments to renters." The program proposed for homeowners has not been endorsed by HUD; however, it is based on a HUD-approved special program operated by the Housing Authority of Contra Costa County, California.

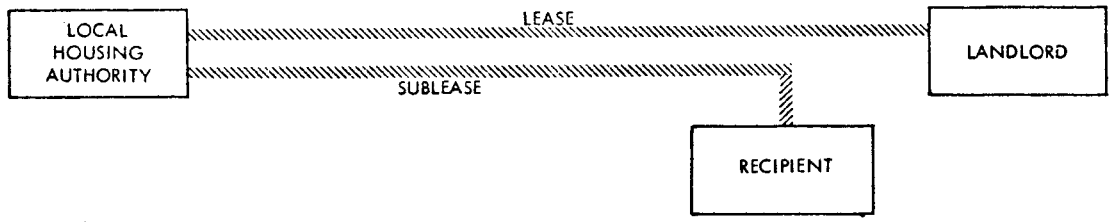

Fig. 3.1 – Standard contractual relationships: Sec. 23 leased housing program

The households subsidized under this program are selected by the LHA from among those who apply for the limited number of available "places." Income limits for enrollment are determined by the LHA, subject to HUD approval and, like the participant's rent obligation, usually follow the precedent of the local public-housing program. Under the recently enacted Brooke Amendment, no participating household can be required to pay more than 25 percent of its adjusted gross income as contract rent to the LHA; special Federal contributions are available as supplementary subsidies for cases in which the permissible tenant contribution is less than the scheduled sublease rent.

Special HUD regulations governing the use of Sec. 23 funds in the Experimental Housing Assistance Program will make it possible to alter these procedures in ways that yield a close approximation to our national model for a housing allowance program for renters. In the Supply Experiment, it is contemplated that HUD will enter into an Annual Contributions Contract with an LHA (or group of LHAs) whose jurisdiction embraces the metropolitan housing market of each experimental site. The LHA in turn will contract with the HAO, described above, for administration of the experimental program funded under the Annual Contributions Contract (see Fig. 3.2).

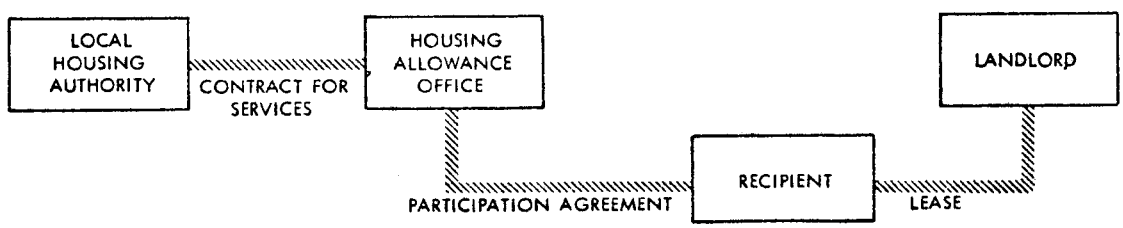

Fig. 3.2 – Special contractual relationships: Sec. 23 experimental housing allowance program for renters

Applying standards described earlier in this section, the HAO will then enroll eligible low-income households and calculate the amount of allowance to which each is entitled. The relationship between the HAO and the enrollee will be formalized by a participation agreement which ensures the enrollee of his allowance entitlement once he occupies a certified housing unit. The enrollee then applies for certification either of his present housing or of some other unit. When the unit has been inspected and certified, the enrollee negotiates a lease with its owner; the HAO is not a party to this lease. When a copy of the lease is submitted to the HAO and approved as to form, the HAO will begin to issue monthly rent certificates to the enrollee.

The enrollee is fully responsible for meeting his obligations under his lease agreement. He pays his rent directly to the landlord by endorsing the rent certificate to the latter and adding from his own resources whatever cash contribution is needed to make up the balance of the rent. The landlord then endorses the certificate and deposits it to his account as he would an ordinary bank check; his bank redeems the certificate through the HAO's bank.

The HAO will continue to provide monthly rent certificates to each enrolled renter household for the duration of the experimental monitoring period, provided that the recipient, his landlord, and the housing unit continue to meet all HAO program requirements. The rent assistance program will continue for ten years, but its format is subject to change in the latter half of that period, after monitoring activities cease.

A detailed statement of procedures for funding and operating this version of the experimental allowance program has been drafted by HUD in the form of a special Sec. 23 Program Circular to be used in conjunction with the Administrative Agency Allowance Experiment. To the extent feasible, this circular will guide the funding and operation of Supply Experiment rent assistance.

Assistance Payments to Homeowners

Preliminary estimates for candidate experimental sites indicate that at least half of all households eligible for assistance under the

income standards we have proposed would be homeowners.* While we were at first surprised by this finding, we now think it would also hold true for the nation as a whole. Apparently, low-income households are predominantly renters only in large cities.

Public discussion of the housing allowance concept has for the most part assumed that the beneficiaries would be renters, perhaps reflecting the customary focus on big-city housing problems. However, our field visits to the small metropolitan areas under consideration as experimental sites revealed as much public concern there for the housing and budgetary problems of low-income homeowners as for those of renters. Our own calculations, as yet crude, support the view that homeownership is by no means evidence of lack of need for housing assistance, even when disposable income includes an imputed income from the homeowner's equity.

Thus, we think it is important in terms of both fairness and program purposes to provide assistance for homeowners as part of the experimental allowance program. We also think that it is essential to do so in order to provide an appropriately brisk stimulus to the local housing markets of our two experimental sites. Finally, we wish to avoid by this means the confusing consequence of a general incentive for otherwise eligible homeowners to sell their homes in order to qualify under the rental program described above.

We propose, therefore, to adapt the Sec. 23 program to serve homeowners as well as renters. The device that makes this possible is taken from a program now operated under Sec. 23 by the Housing Authority of Contra Costa County, California. It enables the HAO to provide assistance to homeowners on the same terms as for renters, without transfer of title or refinancing of existing mortgages, and without contingent liability of the LHA or HAO in the event of mortgage default. Eligible households may own their homes either subject to mortgage liens or in fee simple.

---

*These estimates are as yet crude, because, among other reasons, imputed income from a homeowner's equity is to be included in our measure of disposable income, and systematic data on homeowner equities are hard to obtain. However, we are reasonably sure that the outcome of more refined calculations will be similar in respect to the mix of renters and owners among the eligible population.

Figure 3.3 shows the basic contractual relationships, which introduce a new party into the usual Sec. 23 transactions. The eligible homeowner must execute a limited power-of-attorney in favor of a neutral and trustworthy individual, partnership, or corporation, giving that party (attorney-in-fact) the right to manage the property consisting of the homeowner's residence. An appropriate attorney-in-fact might be a practicing attorney, a bank, a title company, a philanthropic institution, or even a relative or friend. The attorney-in-fact may be compensated for services rendered.

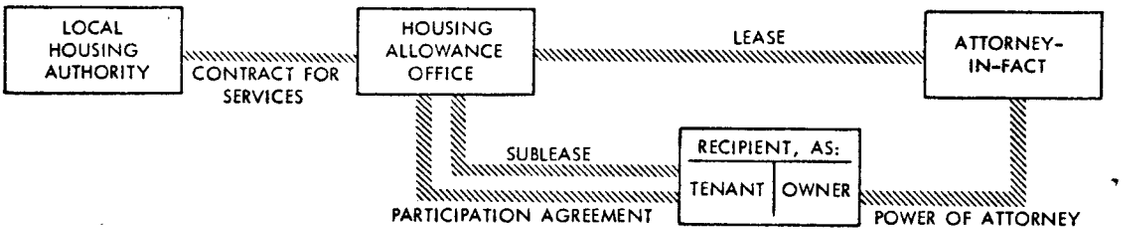

Fig. 3.3 – Special contractual relationships: Sec. 23 experimental housing allowance program for homeowners

This agreement permits the attorney-in-fact to act as landlord of the home in question, in which capacity he can lease the property to the HAO. The lease agreement would acknowledge that the attorney-in-fact is not the titleholder; Sec. 23 permits such a contractual relationship.

Having negotiated a Sec. 23 lease agreement with the attorney-in-fact, the HAO would then sublet the property to the same eligible homeowner for a lesser amount equal to the lease rent minus the homeowner's allowance entitlement. The homeowner, as an assisted tenant, would also execute a participation agreement with the HAO establishing his rights and obligations under the experimental allowance program.

Fig. 3.4 shows the flow of funds under this arrangement when the eligible homeowner holds title subject to an outstanding mortgage. As a tenant of the HAO, he pays his monthly sublease rent, to which the HAO adds the amount of his allowance entitlement. This sum is paid by the HAO to the attorney-in-fact under its Sec. 23 lease agreement. The attorney-in-fact then pays the scheduled monthly debt service to the mortgagee.

In the unlikely event that the attorney-in-fact's receipts under the Sec. 23 lease were inadequate to cover the monthly mortgage payment, the

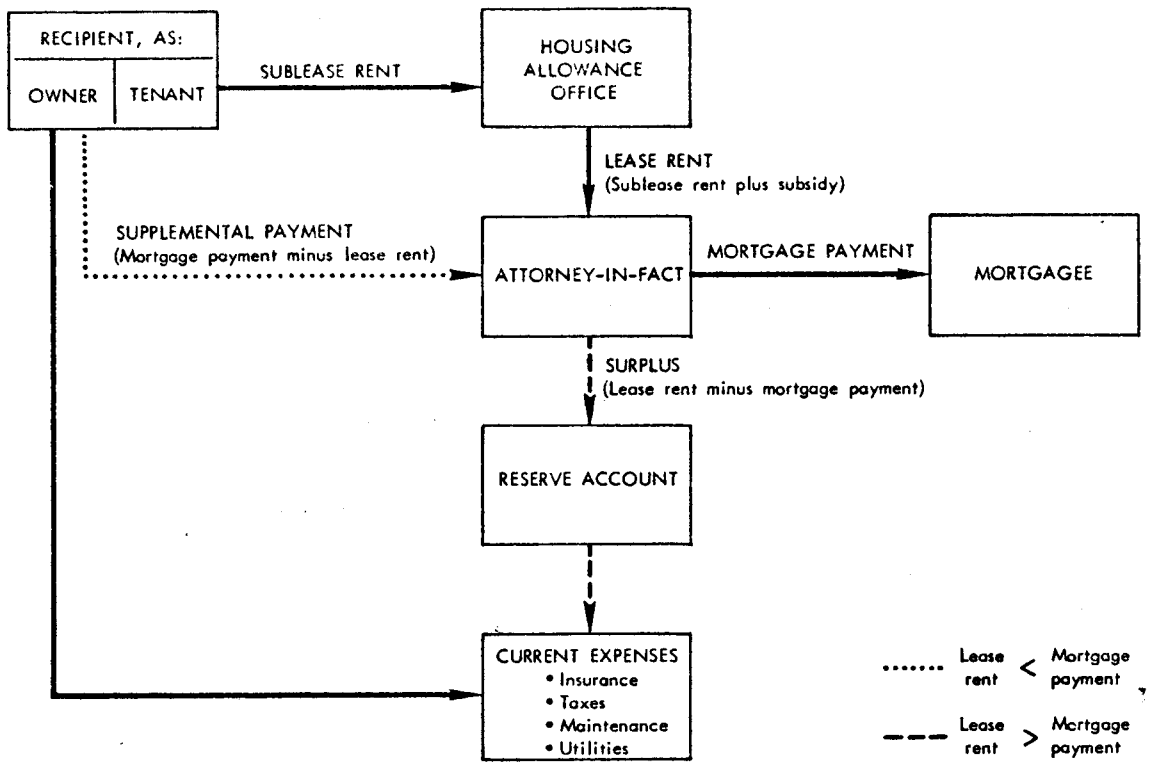

Fig. 3.4 – Flow of funds: Sec. 23 experimental housing allowance program for homeowners with outstanding mortgage debt

the recipient, in his capacity as homeowner, would pay the balance due; he is in any case directly liable under his mortgage for the full amount, since the attorney-in-fact is only his agent. The homeowner also is responsible for other current expenses, such as real-estate taxes or repairs; however, if the attorney-in-fact's receipts exceeded the mortgage payment, the balance would be deposited in a reserve account to be drawn upon for current expenses.

Both the power-of-attorney agreement and the Sec. 23 lease would be designed to terminate automatically if the homeowner became ineligible for further participation in the allowance program, or if he voluntarily withdrew from it. At that time, any funds accumulated in the reserve account would be released to him, and his legal and practical position with respect to his property and his mortgage obligations would revert to status quo ante.

In the event that an eligible homeowner has no outstanding mortgage obligations, the flow of funds under our proposed arrangement would be as illustrated in Fig. 3.5; the attorney-in-fact would deposit all his receipts under the Sec. 23 lease into a reserve account that could be drawn upon for current expenses.

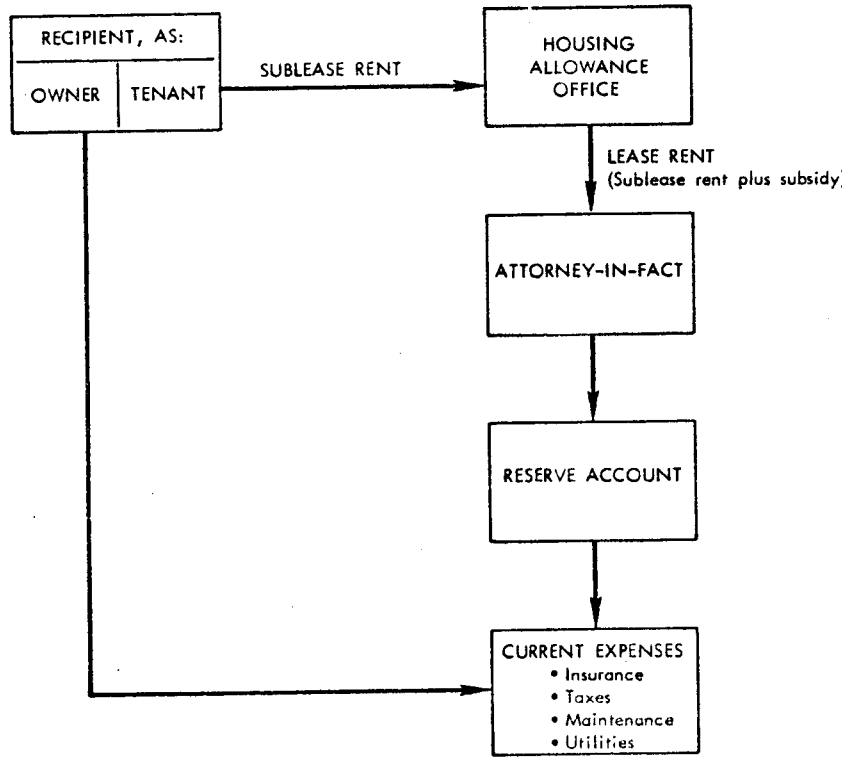

Fig. 3.5 – Flow of funds: Sec. 23 experimental housing allowance program for homeowners without mortgage debt

In principle, the accumulated reserve remaining after payment of current expenses should then equal the total imputed income from the homeowner's equity in his property during the period of participation in the program. This identity assumes that the Sec. 23 lease reflects fair market rent and that current expenses are "normal." Even if these conditions are not precisely met, reserve accumulation could be substantial, on the order of $500 to $1,000 annually for an owner in fee simple. On the other hand, this imputed income would be counted in determining eligibility and allowance entitlement; we would expect the situation illustrated in Fig. 3.5 to be unusual inasmuch as few homeowners in fee simple would qualify for substantial allowances.*

*In its review of this proposal, HUD has expressed reluctance to pay housing allowances to homeowners in excess of "actual homeownership payments plus utility payments plus a maintenance allowance." Our preference, as indicated, is to offer allowances to homeowners on the same terms as to renters, including imputed income from ownership equity as an element of disposable income in determining allowance entitlement.

A more likely situation is one in which a homeowner qualifies on the basis of his low income, but his home fails to meet program standards. Armed with a certificate of eligibility indicating his allowance entitlement, he could negotiate with a private lending institution for a home-improvement loan, using the proceeds of the loan to bring his home up to program standards. Once the property was certified for occupancy by an allowance recipient, the arrangement illustrated in Fig. 3.4 could be established; the attorney-in-fact would repay the home-improvement loan out of his receipts under the Sec. 23 lease, depositing any balance in the reserve account for current expenses.

Assistance for Home Buyers

Under a permanent national housing allowance program, eligible households might in some circumstances wish to shift from rental tenure to homeownership or to sell one home and buy another. Their allowance entitlement would then be viewed by private mortgage-lending institutions as an element of income to be counted in determining creditworthiness; indeed, it would be superior to earned income for this purpose because, in the event of loss of earning capacity, the allowance would ordinarily be increased to partially offset the drop in nonallowance income.

Our standards for allowance entitlement generally imply that homes affordable by allowance recipients would at best be well-maintained older housing, a limitation that we expect would be enforced by private mortgage lenders; they would limit the loan to an amount that the borrower was clearly able to repay from his allowance-augmented income. We do not suppose that all allowance-eligible households would be able to obtain mortgage credit of any kind; those with very low incomes, especially, would have to settle for rental tenure.

The housing-allowance format is harder to apply to home purchase under an experimental program. Mortgage obligations customarily run from 20 to 40 years, longer than it is feasible to guarantee continuance of "experimental" assistance. However, we would like to keep the home-purchase option open because we think it has implications for supply response relating to single-family homes: If there is an allowance-stimulated demand for certifiable single-family housing, we would expect

to find some amount of speculative purchase and rehabilitation of older homes for resale to allowance recipients; under some circumstances, this could amount to a substantial factor in housing improvement.

The most feasible way to provide a home-purchase option for allowance-eligible households appears to be through an existing home-purchase subsidy program, operated by the Federal Housing Administration (FHA) under Sec. 235 of the National Housing Act. Under this program, the FHA insures the lender against losses due to default by the borrower and also pays a substantial portion of the annual debt-service charges. The mortgage insurance thus provided costs the lender .5 percent of the outstanding mortgage balance, a charge usually incorporated by the lender into the interest rate on the mortgage loan. The subsidy payment under Sec. 235 is the amount necessary to reduce the effective interest rate to the borrower to a predetermined level, as low as 1.0 percent; thus, if the market rate on insured loans is 7.5 percent, the borrower might pay as little as 1.0 percent, in which case the FHA would pay 6.5 percent to the lender; the lender in turn would pay the FHA an insurance fee of .5 percent. This arrangement would continue for the life of the loan unless the borrower's income rose above the limit of eligibility for Sec. 235 interest subsidies. If this occurred, the subsidy would terminate, but the mortgage insurance would remain in effect; the borrower would then be liable to the lender for the full amount of scheduled debt service under the mortgage.

To be eligible for assistance under Sec. 235, a low-income home purchaser must meet FHA standards of creditworthiness, designed to protect the agency, as insurer of the loan, against the risk of default. In addition, the home to be purchased must be inspected by the FHA and approved as adequate collateral against the amount of the mortgage loan. The effect of specific income tests applied by the FHA to determine creditworthiness is to exclude from Sec. 235 assistance many households who, because of their low incomes, would by our standards be eligible for housing assistance. As far as we can judge, these tests are reasonable in protecting the borrower from incurring obligations he is unlikely to be able to meet, as well as protecting

the FHA from its liabilities in the event of default.*

For the Supply Experiment, HUD has agreed to reserve adequate Sec. 235 funding for mortgage interest subsidies to be paid on behalf of allowance-eligible households who wish to purchase homes and who qualify for assistance under FHA standards. Figure 3.6 shows the contractual relationships that are contemplated. They differ from conventional Sec. 235 practice only in that the HAO and the local office of the FHA will execute a memorandum of agreement concerning the process of referral by the HAO.

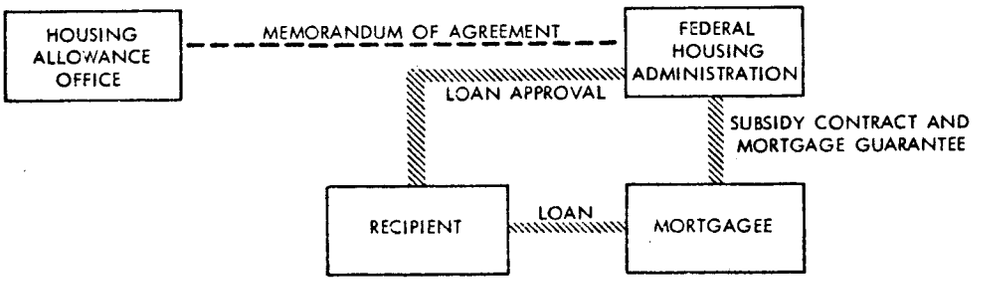

Fig. 3.6 – Special contractual relationships: Sec. 235 experimental housing allowance program for home buyers

If an enrollee in the experimental housing allowance program wishes to investigate home purchase, he will be provided with informational counseling to help him determine whether he might be eligible for assistance under Sec. 235. The HAO will explain to him current FHA income and creditworthiness tests and help him through the necessary computations. Similarly, the HAO will advise the enrollee as to whether the home he contemplates purchasing appears to meet FHA program requirements. If the enrollee, after this counseling, decides to apply to the FHA, the HAO will assist him with the necessary procedures. However, final determination of eligibility and approval of the mortgage loan will rest with the FHA.

---

*See Mack Ott, *Funding Housing Allowances for Homeowners Under Sec. 235*, The Rand Corporation, WN-8025-HUD, November 1972, for comparison of HASE eligibility and assistance standards with Sec. 235 standards.

An enrollee in the experimental allowance program who succeeds in obtaining assistance under Sec. 235 will of course thereupon lose his allowance entitlement. An enrollee refused assistance under Sec. 235 may still elect one of the other options under the experimental allowance program: rent assistance if he is a renter, homeowner assistance if he is already a homeowner.

The standards applied in determining eligibility for the home-purchase option and the amount of assistance granted are different from those governing the other forms of experimental housing assistance. The HAO's role in obtaining assistance for home buyers is limited to counseling and referral. Perhaps the most valuable feature of the arrangement is HUD's reservation of Sec. 235 funds specifically for allowance-program enrollees, guaranteeing access to a form of assistance that might otherwise be unavailable.

PHASING ENROLLMENT AND DISBURSEMENT

For a national housing allowance program, a major consideration governing the pace of enrollment and initial disbursement of allowance payments is certain to be administrative efficiency in staffing and managing a large-scale but decentralized program. Depending on the findings of the Supply Experiment, an additional consideration might be the desire to meter enrollment over time so as to avoid overburdening an unresponsive market with excess housing demand.

Although much can be learned from the Supply Experiment about the administrative problems of a national housing allowance program, the experiment is not intended, as are the Administrative Agency Experiments, to focus on these problems. In mounting the experimental allowance program, we will instead consider primarily the need to produce the appropriate climate of market expectations, deploying all the administrative resources we can muster to implement the allowance program smoothly and speedily. Certainly, we will not attempt to simulate the limitations as to quantity and quality of staff, availability of office equipment and supplies, etc., that would inevitably constrain the effectiveness and speed of implementation of a national program. While we

cannot escape all such limitations, our primary consideration in phasing enrollment and disbursement will be the effects we wish to produce in the housing market.

Because the experiment is of limited duration (and its extension would be expensive) and also because experimental findings will be less pertinent to policymaking the longer they are delayed, we think that the experimental allowance program should be brought to full-scale operation within each experimental site as promptly as feasible. This probably entails a more rapid buildup of allowance-stimulated housing demand than would occur under a national program; but if there is to be any error in metering market impact, there is more policy value to testing the upper limit of the market's ability to "handle" excess demand than to experiment by understatement.*

By rapid enrollment, we can provide the market with clear signals of the change in housing demand attributable to the experimental allowance program, thus prompting, we expect, faster reactions from housing suppliers with respect to both price changes and output changes, and a shorter time-path to the "long-run" market accommodation. The need for such clear signals of change in demand is all the greater in that the experimental program will have a greater barrier of skepticism to overcome than would a fully funded national program.

Planning for a rapid and orderly implementation of the experimental allowance program entails a number of operational considerations and is hampered in many respects by lack of precedent. The following considerations will loom large in designing administrative procedures for the HAO:

1. The rate at which applications are submitted to the HAO is substantially beyond its control. It will be necessary to develop a queuing system to modulate extreme variations and avoid backlogs in subsequent processing.
2. The HAO's outreach program must be attuned to the queuing system. As the queue grows, the outreach message must not

---

*HUD disagrees with this view, preferring to phase enrollment at a pace reflecting national experience with "Medicaid, Medicare, Welfare Reform planning, and other related experience."

create false hopes of instant enrollment and certification for allowance payments.

3. A heavy enrollment workload will undoubtedly persist beyond the date at which income recertification is due for the first enrollees (six months after their enrollment). The HAO must be staffed to continue enrollment concurrently with recertification.
4. The housing-inspection staff must be able to match the pace of enrollment. Once an enrollee has found a housing unit that he believes is appropriate, delays in its inspection would greatly impede the private negotiation between enrollee and landlord that is essential to our scheme.

If it is administratively feasible, we will try to substantially complete enrollment at each experimental site within a period of a year. Thereafter, we would expect a trickle of new applications from households who were at first unconvinced of the program's benefits or who were not at first eligible. Once enrolled, each participant must locate certifiable housing before his allowance payments commence; in our judgment, about half of all enrollees will probably find certifiable units within a few weeks after enrollment; the remainder will find it necessary either to wait for landlord-initiated improvements, or if they are homeowners, to undertake these improvements themselves. How long this process will take is unknown; it is one of the central research questions of the Supply Experiment. Certainly it would be optimistic to suppose that a plateau of allowance disbursement will be reached in less than 18 months from the opening of enrollment. For planning purposes, we have placed this plateau at 18 to 24 months from the opening of enrollment.

ESTIMATES OF ENROLLMENT AND ALLOWANCE COSTS

Estimating enrollment and allowance costs for the Supply Experiment is not a single act but a continual process, for four reasons. First, until general program standards are firmly fixed, assumptions as to eligibility rules and allowance schedules are subject to change

in ways that can substantially affect the outcome for a given population.* Second, until the experimental sites are firmly in hand, our estimates must be based on "illustrative" populations, which may differ from the real ones. Third, generally available data for "illustrative" populations are scanty; a considerable expenditure of staff time and money is needed to compile detailed population descriptions and adjust them to reflect current conditions. Fourth, our standard housing cost, R^*, will not be finally determined until the screening survey has been conducted in each site and its findings have been analyzed.

In this section, we have proposed program standards whose implications for enrollment and allowance costs are estimable, and we have in fact made such estimates for the metropolitan areas of our two leading candidate sites. Here, we present the results only in enough detail to give the reader an idea of the scope and cost of the experimental allowance program. The figures undoubtedly will change as we achieve greater closure with HUD on program standards and as additional data become available on the populations for which the estimates are made.

The calculations are based on 1969 incomes and household sizes reported by the 1970 Census of Population for each of our experimental sites. The parameters of the allowance formula were estimated from housing-expenditure data (Intermediate Standard) published by the Bureau of Labor Statistics (BLS) for each site.** Separate estimates were prepared for renters and homeowners; for the latter, crude estimates of imputed incomes from ownership equities were included in disposable income for eligibility and allowance calculations. Finally, the following assumptions about participation rates among eligible households, based on experience with Aid to Families with Dependent Children and Old Age Assistance, were applied:

---

*Indeed, in the interchange between Rand and HUD, allowance program standards have been progressively refined by iteration with their estimated enrollment and fiscal implications. See Barbara M. Woodfill, *Preliminary Estimates of Enrollment Rates and Allowance Costs*, WN-7901-HUD, July 1972; Woodfill and Repnau, op. cit., WN-7974-HUD; and Tiina Repnau and Barbara M. Woodfill, *Additional Estimates of Enrollment and Allowance Payments Under a National Housing Allowance Program*, The Rand Corporation, WN-8167-HUD, March 1973.

**See Woodfill and Repnau, op. cit., WN-7974-HUD, Sec. III.

Gross Annual Income ($)	Renter Households		Homeowners
	1 Person	2+ Persons	
Under 3,000	.80	.95	.80
3,000-3,999	.80	.95	.80
4,000-4,999	.60	.75	.60
5,000-5,999	.40	.55	.40
6,000-6,999	.30	.45	.30
7,000-9,999	.20	.35	.20

Although the two candidate sites differ substantially in their income distributions and housing costs, the differences are nearly offsetting. At least, applying the same estimating procedures to each, we found little difference in the incidence of eligibility, hence of program participation. Of all renter households in each site, we estimate that 27 to 30 percent would have been participants in such a program in 1969. For homeowners, the corresponding range is 14 to 15 percent. Overall, we estimate that 17 to 18 percent of all households in these small metropolitan areas would then have been participants in our experimental housing allowance program.

These estimates are based on data for 1969-1970. Subsequent general changes in incomes and consumer prices probably have not much altered the incidence of eligibility under our program standards; however, the subsequent increase in Social Security benefits (by about 50 percent, in three stages) undoubtedly has reduced the number of eligible elderly single persons and couples.

Average allowance payments differed considerably between the two sites, reflecting primarily differences in housing costs. In 1969 dollars, the average payment would have been about $450 in one site and $600 in the other. Generally, payments to renters were higher than payments to homeowners, especially in the second site, where the difference in average payments by tenure was about 15 percent.

While prospective eligibility and enrollment has probably decreased with the passage of time since 1969-1970, average allowance costs have almost certainly increased. Very crude calculations suggest that, in 1973 dollars, average allowance payments would be about $510 in one

site and $680 in the other. On this basis, with estimated 1969-1970 enrollment, the allowance budget for 1973, excluding administrative costs, would total about $11.8 million for the two sites combined.

We wish again to emphasize both the crude assumptions and the uncertainties reflected in these estimates. As issues regarding eligibility standards and the allowance formula are resolved, and as we come nearer to final site selection, we will be increasingly able to refine these calculations. We expect that more detailed and current data on the populations of our sites and on their incomes will result in a net reduction in the estimated numbers of eligible households; we are unsure of the effects of better current estimates of the standard costs of adequate housing. Perhaps the best way to express our current understanding is to say that under our proposed program standards, we would expect to enroll between 15 and 20 percent of all resident households at an annual allowance cost of $10 to $15 million of 1973 purchasing power for the two sites combined.

IV. MONITORING THE EXPERIMENT

The experimental allowance program, described in the preceding section, is designed to stimulate low-income demands for housing services in ways that are comparable to the effects of a permanent national program of housing allowances. Having thus provided the experimental stimulus, our second task is to monitor the response. This section explains how we propose to gather the data that will, when properly analyzed, lead us to conclusions about the effects of the allowance program on the supply and price of housing services in the local market and on the behavior and attitudes of the various participants in that market: landlords, homeowners, real-estate investors, market intermediaries, factor suppliers, and consumers of housing services, both program participants and others.

Some of the information we seek will be routinely generated by the HAO as a by-product of its administrative processes. Some will be gathered through a set of sequenced and related field surveys designed especially for our purposes and administered under our supervision. Local public records and general sources such as the U.S. Censuses of Population and Housing will also be tapped. Finally, we propose to maintain on each site a resident observer whose principal function is to fill in the gaps of our knowledge by special-purpose inquiries and to assist our interpretation of systematic data by wide-ranging informal observation.

Below, we describe our plans for each kind of data-gathering activity, emphasizing both method and content. In Sec. V, we explain how the data thus acquired will be organized into research files and, in general terms, how they relate to each of the major research questions addressed by the Supply Experiment. Subsequently, in Secs. VI through IX, we address each major research question in turn, indicating the data and explaining the methods of analysis that we expect to use in seeking answers.

MONITORING THE ALLOWANCE PROGRAM

An important source of experimental data will be the administrative records of the HAO. Of these, two kinds especially concern us: enrollment and disbursement records, and housing-inspection records.

Enrollment and Disbursement Records

As explained in Sec. III, each applicant for housing assistance will be required to supply information on the size and composition of the household on whose behalf assistance is sought, on the amount and sources of income received by its members during the preceding year, and on its place of residence at the date fixed for the residency requirement. All of this information must be documented or verified to the satisfaction of the HAO, inasmuch as it is the basis for determining eligibility and the amount of allowance entitlement.

At this writing, enrollment forms for the allowance program have not been prepared, but it requires little imagination to see that they will inevitably entail circumstantial details beyond the bare minimum described above: for instance, the identity of the applicant, his current address, his current housing circumstances, labor-force status of household members, their occupations and places of employment, asset holdings (including homeownership), income from pensions and public assistance, etc. By careful design of the form, and without turning the enrollment interview into a general fishing expedition, we expect to be able to characterize enrolled households in terms highly relevant to analysis of their behavior as well as to administrative necessity.

There is one subject that we would particularly like to pursue on this occasion: the applicant's housing circumstances prior to enrollment. The questions here must be brief, and the interview will be conducted in the HAO, not the applicant's home; but we can at least obtain a basic description of the housing unit, the applicant's tenure, and his explicit housing expenditures. For renters, the last item would include, at least, contract rent and allocation of responsibility for utility bills; for homeowners, it must, for administrative reasons,

include an appraisal of the market value of the house and an account of its encumbrances.*

Subsequent to enrollment, each enrollee must return to the HAO every six months for recertification of eligibility and recomputation of allowance entitlement. This procedure will entail systematic updating of the information on his basic enrollment record. In the interim, the HAO must maintain a current address register and a record of monthly allowance payments. Thus, for the duration of its enrollment, each household participating in the program will be represented by a semiannually updated file, with a cumulative history for each data item.

This rich research material will be regularly transmitted by the HAO to Rand, where it will be organized into research files, as described below in Sec. V. These files will enable us to track all recipients as they move within the metropolitan area of the experimental site, and to correlate these moves and associated changes in housing circumstances with changes in their personal circumstances, whether due to allowance benefits or to other causes.

Housing-Inspection Records

Administrative requirements of the allowance program will result in a second rich source of experimental data in the form of housing-inspection records. Once enrolled, a household will not be entitled to allowance payments until it occupies a housing unit that has been inspected by the HAO and certified as to size and quality. If the recipient remains in residence, the unit will be reinspected annually; if the recipient moves, his allowance will be discontinued unless his new home is also inspected and certified.

Thus, for every enrollee, the HAO will compile a series of housing-inspection reports, perhaps irregular in interval (but with a maximum

*The urgency of securing this kind of housing information as part of the enrollment interview is lessened by two considerations: First, about 15 percent of all subsequent enrollees will be interviewed at home in the preenrollment survey of tenants and homeowners, described below. And second, many of those enrolled will immediately request inspection of their homes to determine whether their housing is certifiable for occupancy by allowance recipients.

interval of one year), and shifting in their reference as the recipient moves. The reporting form has yet to be devised, but it will probably consist of a checklist covering physical features of both the residential structure and, if a multiple dwelling, of the individual housing unit. Beyond the bare identification of the owner, it will not include an inquiry into his finances or his policies. Rather, it will focus on features of the dwelling that bear on the health, safety, and comfort of its tenants, generally following the guidance of a model housing code. Deficiencies that must be corrected to bring the unit to certifiable condition will be systematically identified, so that reinspections, if requested, can readily check whether appropriate improvements have been made.

These inspection reports will be regularly transmitted by the HAO to Rand, where they will be organized into research files that are linked to enrollment and disbursement records. Thus, for each allowance recipient, we will have regular and thorough reports on the quality and condition of housing, as judged by professional inspectors from the perspective of code compliance.

Other Records

While enrollment and disbursement records and housing-inspection reports are the principal sources of research data to be obtained from the HAO, a variety of other records may also prove useful. In particular, data from the counseling program for enrollees and records of grievance procedures may alert us to features of the program or to circumstances in the housing market that call for further investigation. Additional housing information for renters may be garnered from lease agreements that recipients must submit to the HAO for approval; the format of the allowance program for homeowners clearly will entail submissions that illuminate the nature of encumbrances and possibly the details of other housing expenses. Whether or not these records are systematically abstracted from the beginning for analytical purposes, the HAO record system will be designed to prevent their loss; they will therefore be available for retrospective study.

MONITORING THE HOUSING MARKET

As explained above, records of the allowance program will provide information about the characteristics of all allowance recipients, their movements within the experimental site, their ability to find certifiable housing, the characteristics of the units they occupy, and their housing expenditures. However, the issues addressed by the Supply Experiment cannot be resolved solely by attention to allowance recipients and their housing. We must cast a wider net in order to understand how the allowance program affects the local housing market and its various participants, only a small fraction of whom are allowance recipients.

As our principal basis for marketwide observation, we propose to select a panel of residential properties--both single-family homes and multiple dwellings--and monitor each property, its owner, and its tenants for the duration of the experiment, systematically recording data of interest by means of an annual cycle of field surveys. As compared to independently selected annual samples, a longitudinal panel has the immense advantage that it enables us to measure changes over time for individual elements of the sample and to relate the specific circumstances of a sample property at one point in time to subsequent events involving the same property. By jointly rather than separately selecting the properties, the owners, and the tenants to be surveyed, we greatly enrich the data obtained for each of the three elements by cross reference to data obtained for its associated elements; to put the case differently, our monitoring plan gives us multiple perspectives on the system of interactions among the housing provided by a particular property, its tenants, and its owner, each element affecting and affected by the others.

The thread of continuity in this monitoring plan is provided by keying observations to tax parcels--sample elements that can be precisely defined, are stable over time and easily located from one year to the next, and into which the entire experimental universe can be divided. Our panel will be selected from all such parcels within the boundaries of the experimental site; for the most part, it will consist of parcels in residential use at the beginning of the monitoring

program; however, we have also provided for systematic capture of data on parcels that shift to residential use during the course of the monitoring program.*

Sample Design for a Panel of Residential Properties

In choosing the residential properties to be thus monitored, we are guided by four general considerations, whose sampling implications are broadly indicated below:

1. From our sample observations, we want to be able to generalize about events in the local housing market as a whole. Therefore, we must include residential properties representative of all sectors of the market, each sample element having a known probability of selection.
2. For different sectors of the local housing market, we anticipate different degrees of stimulation from the allowance program and also different patterns of response. We want to be sure that each sector of interest is well enough represented so that we can generalize about its particular response to the allowance program.
3. Since we are particularly interested in the effects of the allowance program, we want our generalizations to be especially reliable for those sectors in which the program has major effects. To the extent that we can identify those sectors a priori, we should concentrate our monitoring resources on them.
4. Although generalization about the effects of a housing allowance program on housing markets other than our two experimental sites will necessarily entail large elements of judgment, transferability of our findings will be enhanced by their

*Within the urbanized portion of our site, we will draw a small sample of vacant and nonresidential parcels, checking them each year to discover whether they have been converted to residential use. In rural areas, this strategy would be ineffective because residential development would be so rare an event; so we will instead sample residential building permits annually and add each year's sample to our panel.

sectoral disaggregation. We want to define our market sectors so that corresponding sectors can be identified elsewhere from generally available data, and to pay particular attention to sectors that may be unimportant at our experimental sites but are prominent elsewhere.

Our resolution of these issues is embodied in a "modified impact-gradient" sampling plan, entailing random sampling within each of 16 well-defined market sectors.* The sectors are defined by four dimensions of stratification, selected to capture (a) probable differences in the degree to which housing demand will change due to the allowance program, and (b) probable differences in the supply response to a given demand change (e.g., in the price elasticity of the supply of housing services). The stratification, by tenure, housing-market density,** size of structure, and rent or value, is detailed in Fig. 4.1.

The number of sample elements to be selected within each stratum will reflect a resource-constrained decision as to overall sample size, a system of "relative reliability targets" for stratum-specific parameter estimates, and estimates of panel attrition over time. We have chosen the price elasticity of the supply of housing services as the parameter whose estimated sampling distribution will guide the setting of relative reliability targets; estimates of other parameters will differ in absolute reliability, but the same general pattern of relative reliability will apply to them also. Panel attrition will result

---

*For a detailed treatment of the sampling plan, see Corcoran, Poggio, and Repnau, op. cit., WN-8029-HUD. There, 32 strata are proposed; these have been subsequently reduced to 16 by changing from four-way to three-way stratification on rent (rental structures) and value (owner-occupied structures) in the urbanized area and even further simplifying the rural stratification.

**Housing-market density is intended to differentiate urban from rural neighborhoods; for various reasons of operational convenience, we propose to approximate this distinction by dividing the SMSA into its urbanized area and its rural area.

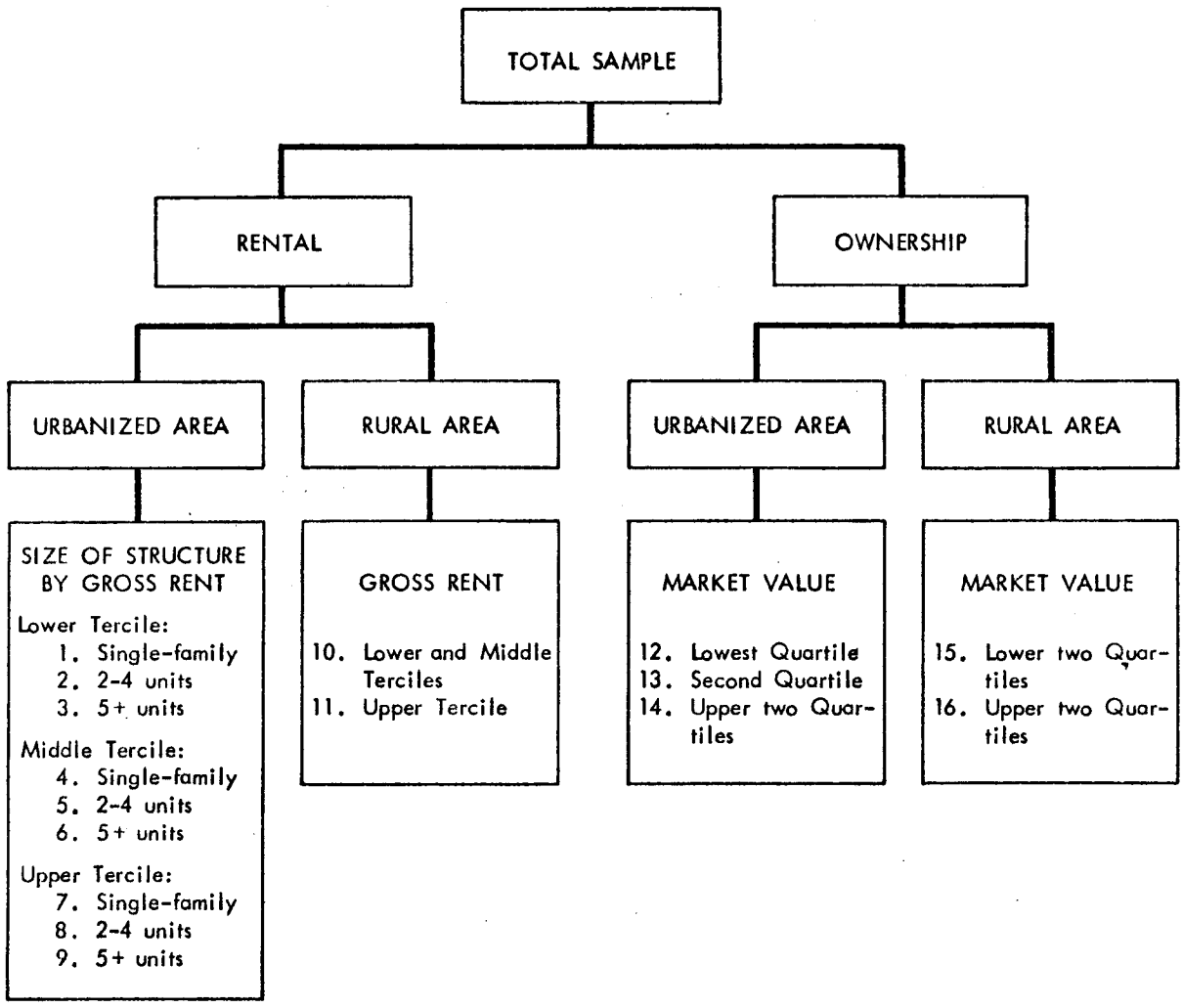

Fig. 4.1—Stratification of residential properties for modified impact-gradient sampling

from failure of landlords, tenants, and homeowners to respond adequately (or at all) to interviews over the planned five annual survey cycles; thus, we must work backward from the desired numbers of complete records at the end of five years to the size of the initial panel.

Allowing for panel attrition over time, we conclude that our baseline sample at each experimental site should contain about 2,250 residential properties for each of which all of the relevant baseline survey instruments are complete.* Figure 4.2 shows how we think these should

*The survey instruments applicable to each type of property are described below; since estimates of panel attrition reflect rates of

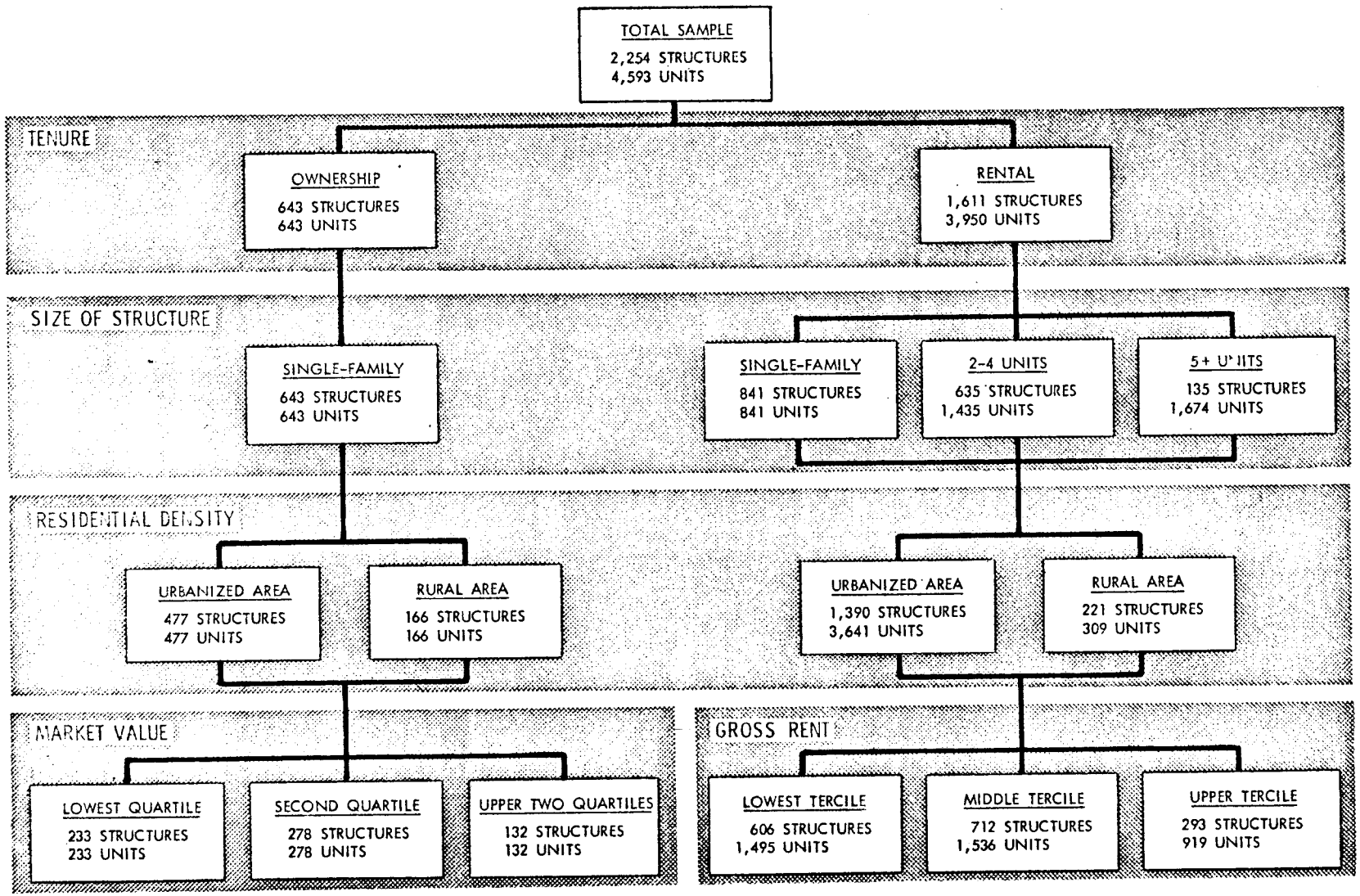

Fig. 4.2—Proposed composition of baseline panel of residential properties, by major dimensions of stratification

be allocated within each dimension of stratification, that being the level of detail most helpful for the present discussion.

We propose to select 2.5 times as many rental structures as owner-occupied homes, to reflect the greater diversity of rental housing and its greater accessibility to allowance recipients. Among rental properties, the number of large multiple dwellings in the panel is limited by their scarcity at our candidate experimental sites; because this kind of housing is important in other markets, we will aim at (but doubtless fall short of) complete enumeration, probably capturing baseline data on about 135 structures. Although we seek more information on small multiple dwellings than on single-family rental houses, we also expect to lose substantially more of the latter due to nonresponses in annual survey cycles; thus, we shall begin with a larger number of single-family houses.

We propose to draw about 83 percent of the panel from among residential properties located in the urbanized area, reflecting our interest in the greater complexities of housing-market interactions in an urban setting.

For rental housing, we propose to draw about 82 percent of the baseline panel from the two lowest terciles of baseline rents, this being the portion of the market within which we expect allowance recipients to be active. For owner-occupied housing, we propose to draw about 80 percent from the two lowest quartiles of the value distribution, for the same reason.*

A panel of 2,250 residential properties, divided among single-family houses and multiple dwellings as shown in Fig. 4.2, would contain about 4,600 housing units. However, we do not propose to try

---

nonresponse to these instruments, our discussion of this subject and our estimate of the number of properties for which complete five-year records will be obtained follows the discussion of the instruments themselves.

*Prosperous households have a greater affinity for homeownership than for renting, and this is reflected in the values of their homes. Thus, a smaller fraction of the total distribution of owner-occupied homes than of rental units is likely to be accessible to allowance recipients, and our sample allocation should reflect this fact.

to interview all tenants in larger multiple dwellings,* so the total number of housing units for which individual data will be sought should approximate 3,700. In our candidate experimental sites, the numbers of households and housing units range from about 50,000 to about 60,000, so that our panel will comprise 6 to 9 percent of the total.

Sample Selection

Selection of the baseline panel of residential properties entails a complex sequence of steps that may differ in detail in the two experimental sites because of differences in the kinds of data available to assist the process. In general, we plan a multistage sampling procedure which is designed to limit the effort expended on acquiring data for properties that do not end up in the final panel, yet without violating the rigorous canons of probability sampling.**

The sampling frame is essentially a list of all tax parcels within the boundaries of the experimental site. With the aid of tax records and local directories, these will be stratified by location (neighborhood density), and by residential or nonresidential use. The residential properties will be further stratified by tenure, number of units, and assessed value per unit (which serves as a proxy for both value and rent). These strata will then be sampled at roughly twice the rates required to obtain the desired number of elements in the baseline panel. Each parcel thus selected will be entered on a *screening list*.

The second stage of sample selection entails a screening survey. Each property on the screening list will be visited by fieldworkers, who will confirm or amend the stratification data obtained from tax records and directories and who will conduct a brief interview with the residents of each housing unit on the property. This survey not only aids in sample selection, it also prepares the way for the baseline surveys to come, by providing information on the ownership of the

---

*See "Survey of Tenants and Homeowners," below.

**See Eugene C. Poggio, *Sample-Selection Procedures for Site I*, The Rand Corporation, WN-8201-HUD, March 1973.

property and on the composition of resident households--information that will aid in scheduling baseline interviews and in selecting appropriate baseline instruments for each property. Finally, it provides early data on housing characteristics and rent for a large number of rental units; these data will be used by the HAO to determine appropriate standards for housing assistance.* The contents of the screening instrument are summarized in Table 4.1.

The screening list, as amended by the reports from the screening survey, will then again be sampled to obtain a smaller list of properties in each stratum, the *baseline survey list.* Each property on this list will be scheduled for administration of the appropriate baseline survey instruments, described below. When the lengthy and expensive process of baseline interviewing is complete, the records will be reviewed for critical elements of nonresponse, as well as for misclassifications that survived earlier screens. The last step will be to select from the baseline survey list the final *panel of residential properties,* for each of which an adequate baseline record has been obtained, and each of which will then be monitored by annual resurveys over a period of five years.

Baseline Surveys

For each member of our panel of residential properties, we will undertake a set of field surveys to gather systematic data on conditions immediately prior to the commencement of the allowance program: descriptions of the property and the neighborhood in which it is located; characteristics and attitudes of the households that inhabit it; characteristics, management policies, and plans of its owner; and a full account of property revenues and expenses. These data will serve as a baseline or point of reference against which subsequent events involving each property can be assessed.

Altogether, we plan to administer five different survey instruments in this preenrollment cycle of fieldwork, though not all of them will be administered to each member of the panel. These five instruments are described below.

---

*See Sec. III, above.

Table 4.1

CONTENTS OF THE SCREENING SURVEY INSTRUMENT

General Topic and Number of Questions[a]	Information Sought
Composition of household (15)	Number, identity, and family relationships of persons in the households, including separate identification of family subunits.
Housing characteristics and condition (18)	Type of structure, number and identification of separate housing units, and characteristics of subject's housing unit: number of rooms and bedrooms, plumbing and kitchen facilities, electrical and heating facilities, safety features.
Tenure, value and rent (14)	Tenure of occupants. For owners, estimated market value. For renters, contract rent and division of responsibility for utility expenses; special considerations affecting contract rent.
Identification of baseline respondents (24)	In the subject household, the name of the principal respondent, information that will help to identify and locate him at baseline (address, telephone, plans to move), and information that will guide future interviewing tactics (attitude toward interview, socioeconomic status, age, sex, ethnicity, approximate income). If owner is nonresident, name and address of owner or his representative.

NOTE: This table is based on the pretest version of the screening survey instrument, dated March 1973. Details of the final instrument may differ as a result of pretest experience and other factors.

[a]Grid items are counted as separate questions; repetitions for each individual are not.

Neighborhood Survey. The territory of each experimental site will be divided into mutually exclusive neighborhoods, and data on the characteristics of each such neighborhood will be compiled from public records and by field observation. Within the urbanized portion of each site, neighborhoods will be small in area and laid out so as to contain relatively homogeneous populations and housing stocks; rural neighborhoods will be larger in area and less homogeneous. We will attempt to make their boundaries conform both to areas defined for statistical purposes by the 1970 Censuses of Population and Housing and by local planning agencies. Neighborhood populations will vary in size, usually ranging between 1,000 and 3,000 households. The information sought will relate to such topics as land use, general type and condition of residential buildings, availability of various public facilities and municipal services, and the general social and economic characteristics of the neighborhood population (see Table 4.2).

Survey of Residential Buildings. Each property selected for inclusion in our baseline panel will be visited by a fieldworker, who will supplement and amend previously obtained tax-record data by direct observation of the physical features and condition of the residential buildings on the property. This inspection will be restricted to exterior aspects of the residential buildings and, for multiple dwellings, to public areas of the interior. No attempt will be made to inspect the interiors of individual housing units, on which some data will have already been gathered by the screening survey and on which more will be gathered by the survey of tenants and homeowners, described below. The items covered by the survey of residential buildings are summarized in Table 4.3.

Survey of Landlords. For each rental property in our baseline sample, we will seek out the owner or his representative for an extensive interview whose purpose is fourfold. First, we seek data on the characteristics and circumstances of the owner and his activities in the real-estate market; second, we will try to elicit his perceptions of the neighborhood housing market and of the prospects of his property there as an investment; third, we will seek information on his management policies and on his relationship with his tenants and suppliers of supporting services; finally, we will seek a detailed account of his

Table 4.2

CONTENTS OF THE NEIGHBORHOOD SURVEY INSTRUMENT

General Topic and Number of Questions[a]	Information Sought
Land use patterns (4)	Neighborhood density (urban, suburban, rural); distribution of land uses by type; changes over past five years and reasons for change.
Characteristics of residential buildings (5)	General distribution by size, age, condition, and type of construction.
Availability of facilities and services (16)	Availability of public and private utility services, distances to public transportation stops, schools, retail facilities, police and fire stations, etc.
Characteristics of residents (3)	General racial composition of the neighborhood, dominant nationality and religion.
Quality of life (15)	Incidence of abandoned residential structures, abandoned automobiles, trends in residential property values, frequency of fires and crimes. Condition of streets and traffic control, appearance of vacant lots and structures.

NOTE: This table is based on the fourth draft of the neighborhood survey instrument, dated 20 December 1972. Details of the final instrument may differ as a result of pretest experience or other factors.

[a]Grid items are counted as separate questions.

Table 4.3

CONTENTS OF THE RESIDENTIAL-BUILDING SURVEY INSTRUMENT

General Topic and Number of Questions[a]	Information Sought
Nature of use and tenancy (8)	Number of housing units in the structure, presence of commercial or industrial uses, observable vacancies, evidence of marketability for vacant buildings.
Physical characteristics of building (13)	Type and layout of structure, placement on lot, principal construction materials, quality of construction.
Related tenant facilities (8)	Availability of garage, carport, on- or off-street parking; quality of landscaping, presence of swimming pool, quality of view.
Exterior condition of building (19)	Presence or condition of 16 exterior items (roof, wall surfaces, doors, windows and screens, porches, foundations, paving, etc.) and three evaluations (state of repair, cleanliness, overall condition).
Interior condition of public areas[b] (15)	Presence or condition of 11 interior items (doors, floors, walls and woodwork, windows, ceilings, lighting fixtures, mailboxes, stairways, elevator, door locks, fire alarms and extinguishers) and two general evaluations (state of repair, cleanliness).
Characteristics of immediate neighborhood (16)	Land uses, vehicular traffic, street lighting, pedestrian walkways, street maintenance, litter, abandoned automobiles, abandoned buildings. For other residential buildings, characteristic types, comparative size, age, and landscaping. Beneficial and detrimental features of neighborhood (noise, odors, physical hazards, parks, ponds, woodlands, etc.).

NOTE: This table is based on the pretest version of the residential building survey instrument, dated March 1973. Details of the final instrument may differ as a result of pretest experience or other factors.

[a]Grid items are counted as separate questions.

[b]Applicable only to multiple dwellings.

mortgage financing and his income and expenses from the property during the preceding year.

For convenience in administration, the survey instrument is divided into modules generally differentiated by subject matter, although other considerations prevent strict application of this principle. Table 4.4 summarizes the contents of each module of the current draft of this instrument.

Survey of Tenants and Homeowners. The households occupying our panel of residential properties, already briefly interviewed for screening purposes, will be revisited for more extensive baseline interviews. We will attempt to interview every household occupying a single-family house or small multiple dwelling; in multiple dwellings of five units or more, we will randomly select an average of six units whose tenants are to be interviewed.

The purposes and contents of this interview differ for rental tenants and for homeowners. In either case, we will seek a careful account of household membership and relationships, amount and sources of income, and the nature and location of employment for all working members. We will also try to determine the respondent's self-identification with ethnic and religious groups and the degree of household involvement in the activities of formal organizations, in extended-family relationships, and in neighborhood social life.

Having thus "placed" the household in the social and economic systems of the community, we propose to pursue three main lines of questioning. One is a detailed history of residential mobility and associated changes in family composition, employment, income, and housing circumstances. The second, differing for owners and tenants, is a thorough exploration of current housing and neighborhood characteristics and the respondent's view of his situation: his attitudes toward his housing, his landlord, and his neighborhood. Finally, we will try to obtain detailed information on housing expenses during the preceding year: contract rent for renters; mortgage financing, taxes, and insurance for homeowners; and for both, other outlays such as utility payments and expenses (or own efforts) for maintenance, repairs, and improvements.

Table 4.4

CONTENTS OF THE LANDLORD SURVEY INSTRUMENT

General Topic and Number of Questions[a]	Information Sought
Acquisition and ownership (8)	Manner in which property was acquired. Reason for acquisition. Number and relationship of owners.
Experience and activity in real estate (46)	Length of time the property owner (landlord or firm) has been active in real estate, the fraction of his income deriving from rent, nature of other business involvement, participation in property owners' or real estate organization; knowledge of tenant organizations.
Property description and revenues (50)	Type of property, past and present use. Age of principal building, number of buildings, number and size of residential units, number of commercial units on property. Mobile homes. Rental income from property. Losses from bad debts and vacancies.
Management, maintenance, and operating costs (170)	Labor. Types of employees, wages, rent discounts. Use of management firms, brokers, collection agents, lawyers, other professional assistance. Utility and heating costs.
Repairs, improvements, and other costs (109)	Expenditures on remodeling, interior decorating, floor work, appliance repair or replacement, repair work of all kinds. Number of repairs, materials and labor costs, perception of repair as operating or capital expenditure. Perception of recent trends in repair and improvement costs.
Mortgage, taxes, and insurance (85)	Purchase price. Costs and terms associated with all loans, taxes, and property insurance.
Perception of neighborhood (18)	Occurrence of new construction, conversions, demolitions. Changes in traffic density, vandalism, property values; changes in income and ethnic composition, perception of neighborhood effects on property value.
Landlord-tenant relationships (33)	Description of tenants, their care of property, rent collection problems, turnover rates, evictions, lease policy, tenant complaints. Ways of locating tenants, willingness to rent to various tenant categories.
Plans for property (37)	Plans for capital improvements, increased maintenance. Cost estimates, effect on rents. Current investment strategy. Knowledge of housing allowance program, its effects on plans for property.

NOTE: This table is based on the fifth draft of the landlord survey instrument, dated 2 April 1973. Details of the final instrument may differ as a result of pretest experience and other factors.

[a]Grid items are counted as separate questions.

This survey instrument is also divided into modules for ease of administration. Table 4.5 summarizes the contents of each module of the current draft.

Survey of Vacant and Nonresidential Properties. The final baseline instrument might be described as a "catch-all," designed to deal with properties that do not fit well into the general survey plan. These include residential properties that are completely uninhabited at the time of the survey and a small sample of nonresidential properties that are carried on the books of the monitoring program until they show signs of residential use, whereupon other instruments will become applicable.

For a vacant residential property, we will seek out the owner and try to learn from him his view of the status and probable future of the property. We want to know whether it has been withdrawn from the residential market and is awaiting demolition or conversion, whether it is undergoing a change in ownership or tenure, whether the vacancy is of long duration or merely reflects normal turnover, etc.

If a vacant residential property is available for rent or sale, we will seek much the same information from its owner as for occupied properties, including an account of the prior year's income and expenses. Special formats will be needed for specific situations--e.g., the case of a homeowner who has moved from his former residence and is holding it for sale, or who plans to convert it to rental use.

For nonresidential properties, the agenda is simpler. We will seek only to describe the property, determine who the owner is, and find out whether he has current plans to convert the property to residential use.

As yet, the contents and format of this instrument have not been determined in detail. Table 4.6 summarizes its probable coverage.

Taken together, these five surveys comprise the baseline for our panel of residential properties, and we will require that all applicable surveys be substantially complete in order for a property to be accepted into the panel. Table 4.7 shows which survey instruments will be used

Table 4.5

CONTENTS OF THE TENANT AND HOMEOWNER SURVEY INSTRUMENT

General Topic and Number of Questions[a]	Information Sought
Household composition, tenure and rent (64)	Basic characteristics of the family unit; respondent's relationship to the dwelling (tenure, rental rates, services provided in return for rent reduction); knowledge and use of the housing market and housing search.
Homeowner mortgages, taxes, and insurance (28)	For homeowners only. Housing expenses, details and manner of payment of mortgages, taxes, and property insurance.
Characteristics and condition of housing unit (31)	Description of residence, number and type of rooms, automobile storage facilities, yard space, appliances, utilities. Condition of unit. Respondent's level of satisfaction with various aspects of the dwelling.
Tenant-landlord relationships (18)	For renter households only. Relationships with landlord, building managers; building rules and their enforcement; experience with tenant organizations, satisfaction with maintenance and management practices.
Perception of neighborhood (23)	Respondent's perception of such neighborhood characteristics as traffic density, safety, cleanliness, municipal services, racial changes, distance to schools.
Housing expenses other than contract rent (37)	Expenses for utilities, services, insurance, exterminators. Major remodeling or improvements. Tenant contributions and resultant rent reductions.
Housing demand (3)	Possible responses to an allowance program.
Mobility and housing history (73)	Dates of previous residence, its location and housing characteristics. Respondent's attitude toward housing, landlord, and neighborhood; the characteristics of the household, and reasons for moving. Information will be sought for three previous years and for three previous residences.
Income, occupation, and industry (143)	Income, occupation, and industry for respondent and spouse for three previous years. Time, distance, and mode of journey to work.
Social integration (28)	Organizational membership; location of and interaction with relatives and friends. Recreational activities.
Social identification (11)	Extent of respondent's identification with national groups; religious preference, marital status.

NOTE: This table is based on the fourth draft of the tenant and homeowner survey instrument, dated 8 February 1973. Details of the final instrument may differ as a result of pretest experience and other factors.

[a]Grid items are treated as separate questions; repetitions for each individual are not.

Table 4.6

CONTENTS OF THE VACANT AND NONRESIDENTIAL PROPERTIES INSTRUMENT

General Topic	Information Sought
Property description and current use	Type of property, past and present use. Age of principal buildings, number of buildings, number and sizes of residential units, number and type of nonresidential uses on property. If vacant, date of last occupancy, availability for rent or sale.
Nonresidential property: revenue and prospects	Whether revenue-producing or not; if not, reason for holding. Plans for improvements or conversion to residential use. Events likely to prompt change in plans.
Vacant residential property: reason for vacancy	Reason for vacancy: tenant turnover, preliminary to sale, preliminary to conversion of use or remodeling, vacate order, inability to rent or sell, abandonment of active interest.
Vacant residential property: revenue and expenses	If occupied or available for rent or sale during the preceding year, revenue and expenses as per landlord or homeowner instrument.
Vacant residential property: prospects and plans	Owner's perception of economic prospects for property. Plans for improvements, alterations, conversion to nonresidential use. Plans to rent or sell property. Events likely to prompt change in plans.

NOTE: This table is based on preliminary plans for the vacant and nonresidential property instrument; its scope, contents, and organization are subject to change. Since actual questions have not been drafted, the table does not indicate number of questions per topic.

Table 4.7

SCHEDULE OF SURVEY INSTRUMENTS TO BE ADMINISTERED TO ELEMENTS OF STRATIFIED SAMPLE OF PROPERTIES, BY TYPE AND STATUS OF PROPERTY

Type and Status of Property	Survey Instruments or Modules to be Administered					
	Residential Buildings	Landlords	Tenants[a]	Homeowners[a]	Vacant Residential[b]	Nonresidential Property[b]
Single-family house:						
Owner-occupied	X			X		
Renter-occupied	X	X	X			
Vacant:						
Formerly owner-occupied	X				X	
Formerly renter-occupied	X				X	
Converted or demolished						X
Multiple dwelling:						
Renter-occupied	X	X	X			
Vacant (all units)	X				X	
Converted or demolished						X
Nonresidential use						X

[a]Different modules of tenant and homeowner survey instrument.

[b]Different modules of vacant and nonresidential property survey instrument.

in which situations, both at baseline and for the annual cycle of post-enrollment surveys discussed below.

Postenrollment Surveys

The baseline surveys just described will require several months for completion and will be followed immediately by the opening of enrollment in the experimental allowance program. Although households meeting residence requirements may become otherwise eligible at any time during the first five years of this program, and those eligible at the beginning may not immediately enroll, we expect the bulk of enrollment to be completed within the first year. Thus, by the anniversary of the baseline surveys, the experimental allowance program should be in full swing.

On that anniversary, and each year thereafter for a total of five years,* we propose to resurvey each property in our baseline panel, using modified versions of the five survey instruments described above. These instruments will cover much the same ground as their baseline counterparts, but with emphasis on detecting changes that have occurred since the prior survey. In addition, no doubt, experience with each instrument will provide us with reasons to modify the wording or the tactics of some lines of questioning, and we will discover information needs not now foreseen.

Survey of Neighborhoods. We will repeat this survey each year for each neighborhood defined at baseline, looking for changes in land use and traffic patterns; for evidence of residential construction, improvements to existing properties, conversions, demolitions, or abandonment; for changes in the availability or adequacy of public facilities and municipal services; and for changes in the general social and economic characteristics of the population.

Survey of Residential Buildings. Using the previous record on each property as a guide, we will search for changes in use or occupancy and in the physical characteristics and condition of each building.

---

*The duration of the monitoring program is not firmly fixed; it will depend in part on the observed course of events at each site. Five years is our estimate for planning purposes.

This information, gathered prior to interviewing the owner, will help guide the interviewer in probing for financial data on repairs or improvements, and will also provide direct if partially judgmental evidence of housing improvement or deterioration over time. This survey will also cover an annual sample of newly constructed housing for the rural portion of the site, drawn from building-permit data; these will be added to our panel and thereafter subjected to all appropriate surveys, depending on the nature of the residential use (e.g., rental vs. ownership). Within the urban portion of the site, our baseline sample of nonresidential parcels will be checked to see if they have been converted to residential use; if so, this and other appropriate instruments will be administered.

Survey of Landlords. Here, our primary interest lies in obtaining from the owner an accounting of income and expenses for the preceding year, an accounting that can be compared, item by item, to the corresponding records from previous survey cycles. If there has been a change in ownership during the preceding year, we will not only try to obtain "baseline" data on the new owner, we may also seek out the prior owner in order to complete the record of income and expenses.

We also want especially to capture evidence of changes in occupancy, rent levels, and kinds of tenants; and to note changes in the owner's perception of the neighborhood housing market, and the specific prospects for his property--changes which may affect his investment strategy or maintenance policy.

Survey of Tenants and Homeowners. We anticipate frequent changes of tenancy, particularly in rental housing. When we encounter new residents, the scope of the survey will be about the same as at baseline, including the mobility history. Households that have been interviewed in previous cycles will be reinterviewed to check for changes in household composition, income, and employment. In either case, we will seek evidence of changes in the interior physical characteristics, facilities, and condition of the housing unit and a record of housing expenses for the preceding year. Finally, we will again probe for each respondent's attitudes toward his housing, his landlord, and his neighborhood.

Survey of Vacant and Nonresidential Properties. Not infrequently (perhaps in 5 percent of the cases), single-family homes in our panel will be vacant at the time of annual resurvey, so that no tenant or homeowner interview will be possible. In these cases, we will seek out the owner and administer appropriate modules of this survey, described earlier. Occasionally we will find that a formerly residential property has been withdrawn from the housing market or converted to other uses, and we will try to find out why. Our baseline panel will include a certain number of urban nonresidential properties; if they are still in nonresidential use, we will briefly interview the owner to check whether his plans for the property have changed since the preceding year.

Panel Attrition

Because our panel of residential properties is to be followed over time, it must initially be large enough to allow for attrition. Unlike the case with panels of individuals or households, the elements of this sample cannot die, disperse, or leave the area; while residential buildings can be altered, demolished, or converted to nonresidential use, these are phenomena we positively want to observe. Rather than "losing" such cases from our panel, we will continue to follow them, albeit with different survey instruments.

For our purposes, attrition is defined as loss of information due to our inability to complete the set of survey instruments applicable to a given property in a given survey cycle. Usually, more than one instrument is applicable, and even with the best scheduling and interviewing techniques some of the subjects will not be located, will refuse to talk to the interviewer, or will decline to answer some questions. Thus, for any particular property, the proportion of information sought but not obtained in a given survey cycle may be anywhere from trivial to total.

Fieldwork quality-control procedures will entail establishing standards for "substantial" completion of each survey instrument, specifying appropriate follow-up steps when the initial interview fails to result in a completed instrument. Failure to complete all the

appropriate instruments will impinge most directly on the selection of the baseline panel, for each member of which all essential data must be obtained. Thereafter, while exerting best efforts to obtain high response rates, we will have no choice but to suffer the statistical consequences of nonresponse.

Working from estimates of nonresponse rates to individual surveys provided by our fieldwork contractor (an experienced survey research organization), we have calculated the cumulative effects in terms of information loss over a five-year period.* The effects differ with the kind of residential property, depending on the applicable survey instruments; and the applicability of the various instruments changes when the status of the property changes--e.g., from occupied to vacant or from owner-occupied to rental. Finally, expected response rates in a given year are contingent on the nature of the response in the prior year.

Although other assumptions are also required, the key estimates of response rates are those for the landlord and the tenant and homeowner interviews, shown in Tables 4.8 and 4.9 by size of structure and experience in the prior year. For multiple dwellings, we require only one completed tenant interview (although more will be sought).

Calculations from these assumptions (using Markov chains of conditional probabilities) lead us to the results shown in Table 4.10. At the end of five annual survey cycles, we anticipate having complete survey records for about one-third of the single-family rental properties in the baseline panel, one-half of the small multiple dwellings, and two-thirds of the large multiple dwellings. For single-family owner-occupied structures, we expect complete data for about 44 percent of those in the baseline panel.

To be sure, incomplete records will not be total losses from an analytical point of view: Nonresponse in Year 2 may be partly remedied by full response in Year 3, and nonresponse by a tenant is less serious than nonresponse by both tenant and landlord. As Table 4.10 shows, an

---

*See Timothy M. Corcoran, *The Effects of Nonresponse on Record Completion in a Panel of Residential Properties*, The Rand Corporation, WN-8174-HUD, April 1973.

Table 4.8

LANDLORD INSTRUMENT COMPLETION RATES BY INTERVIEW STATUS IN PRECEDING YEAR, BY SIZE OF STRUCTURE

Size of Structure	Completion Rate, by Status in Preceding Year		
	Interviewed Different Owner[a]	Interviewed Same Owner	
		Instrument Completed	Instrument Not Completed
Single-family	.50	.90	.25
2-4 units	.60	.92	.30
5+ units	.70	.95	.35

SOURCE: Estimates by Mathematica and HASE staffs.

[a]In applying these rates, we assume that 5 percent of all rental structures undergo a change of ownership each year.

Table 4.9

TENANT AND HOMEOWNER INSTRUMENT COMPLETION RATES BY INTERVIEW STATUS IN PRECEDING YEAR, BY SIZE OF STRUCTURE

Tenure and Size of Structure	Completion Rate, by Status in Preceding Year		
	Interviewed Different Household[a]	Interviewed Same Household	
		Instrument Completed	Instrument Not Completed
Homeowners			
Single-family	.50	.95	.40
Renters			
Single-family	.85	.95[b]	.40
2-4 units	1.00[b]	1.00[b]	(c)
5+ units	1.00[b]	1.00[b]	(c)

SOURCE: Estimates by Mathematica and HASE staffs.

[a]In applying these rates, we assume that 20 percent of all households move each year.

[b]Approximate probability of completing at least one tenant instrument when interviews are attempted for all tenants in structures of 2 to 4 units and six interviews are attempted in structures of 5+ units. For two-family structures, a more exact estimate is .98; for three-family structures, .997.

[c]Probability of completing no tenant instruments is negligible.

Table 4.10

DISTRIBUTION OF SURVEY RECORDS FOR PANEL OF RESIDENTIAL STRUCTURES BY COMPLETENESS OF TERMINAL RECORD BY TYPE OF STRUCTURE

Completeness of Terminal Record	Percentage of Baseline Sample, by Type of Structure			
		Rental		Homeowner
	Single-Family	2-4 Units	5+ Units	(Single-Family)
Complete data (baseline + 5 years)	34	55	67	44
Incomplete data				
1 year missing	19	16	12	21
2 years missing	16	10	7	15
3+ years missing	26	12	7	14
Structure removed from inventory	5	6	6	5
Total	100	100	100	100

SOURCE: Calculations by HASE staff.

NOTE: A complete record for a rental property is one for which a residential building instrument, a landlord instrument, and at least one tenant instrument (or a vacant property instrument) is completed at baseline and annually thereafter; for an ownership property, a complete record is one for which a homeowner or vacant property instrument is completed at baseline and annually thereafter.

Percentage distributions by completeness of terminal record are based on the number of properties for which baseline instruments were completed.

additional 12 to 21 percent of each class of structures will lack complete data for one year only.

Characteristics of the Terminal Panel

The baseline sample and its allocation among housing-market strata, illustrated earlier in Fig. 4.2, was inflated in size to allow for the panel attrition rates reflected in Table 4.10. The size and composition of the corresponding terminal panel, consisting of all members of the baseline panel for which we expect to obtain complete five-year survey records, is illustrated in Fig. 4.3.

Given a baseline panel of 2,250 residential properties allocated shown earlier, we would expect to end the monitoring period with complete survey records on about 1,000 properties, about 44 percent of the

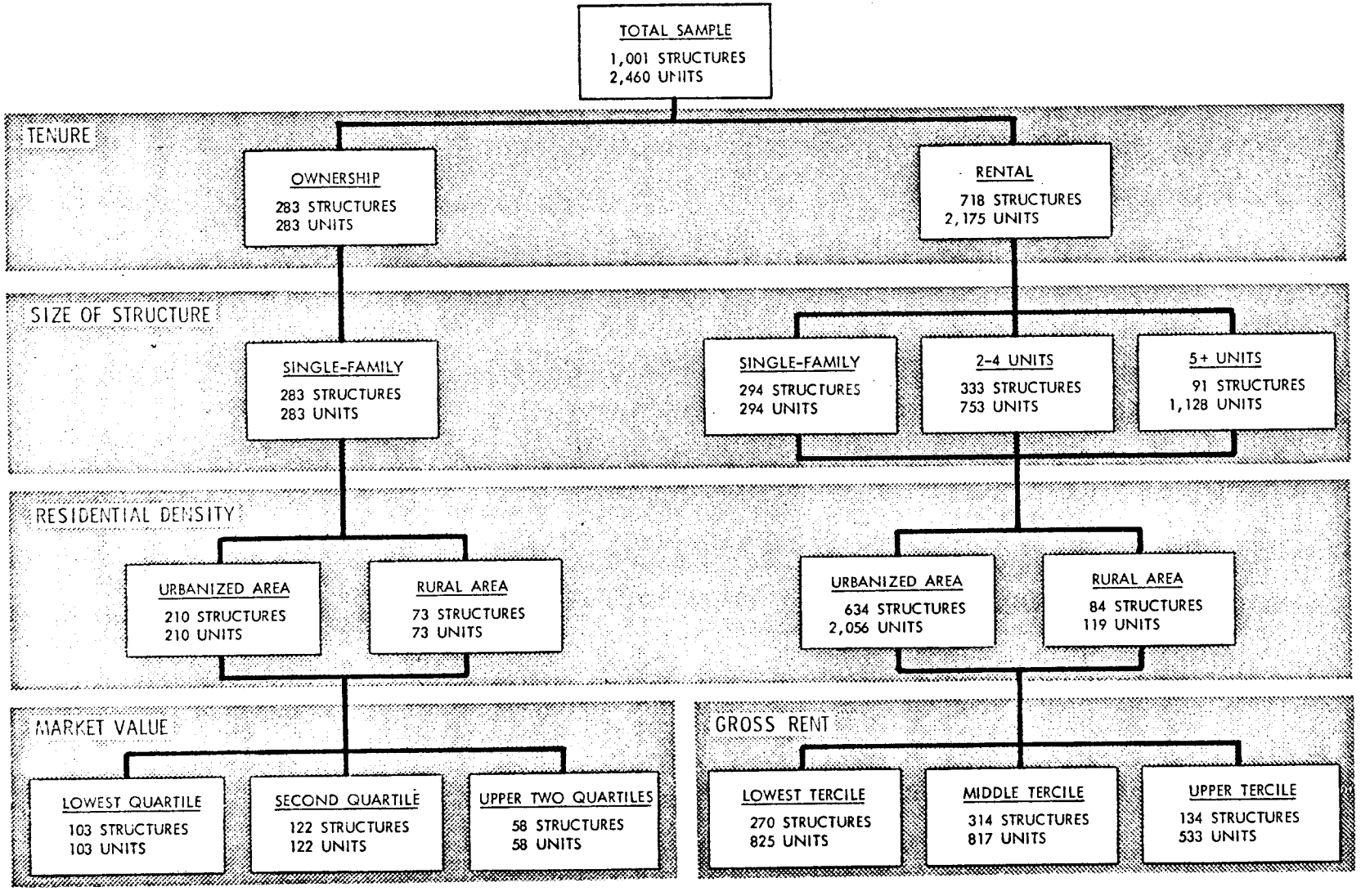

Fig. 4.3—Expected composition of terminal panel of residential properties after attrition due to nonresponse, by major dimensions of stratification

original total.* The rate of loss by stratum varies, for the reasons we have indicated. As reflected in this condensed display, the most notable change in interstratum proportions is between single-family rental units and small multiple dwellings. Beginning with 18 percent more of the former than of the latter, we end with 31 percent fewer.

As nearly as we can determine on a priori grounds, the statistical properties of this terminal panel are such as to enable us to carry out those aspects of our analysis plans that rely on panel data with reasonable confidence in the results. The issues entailed in this assessment are too complex for easy summary; here, we wish only to leave the reader with a general understanding of our panel's size and composition, of the monitoring program addressed to it, and of the end-product.**

Controlling for Nonresponse Bias

The procedure described above for selecting the panel of residential properties to be monitored over time is carefully designed so that the properties selected for field screening will be a random sample of all properties in their respective strata. However, if a property is to be included in the panel, we must obtain baseline data from both the owner and the tenant. If those who are willing to be interviewed differ in other important respects from those who are unwilling, the panel will not be representative of the population from which it is drawn.

Nonresponse bias is always a danger in field surveys; moreover, since the relevant characteristics of nonrespondents can seldom be ascertained, the extent of the bias and its effect on subsequent analysis of the data are difficult to determine. In the present case, how-

---

*This figure does not include accession to the panel of residential properties either from the small baseline sample of urban nonresidential properties that were subsequently converted to residential use or from the annual sample of newly constructed rural homes.

**See Corcoran, Poggio, and Repnau, op. cit., WN-8029-HUD, for an assessment of the statistical properties of the sample of residential properties; and Adele Massell (ed.), *The Role of Household Survey Data in the Supply Experiment*, WN-8218-HUD, March 1973, for an assessment of the statistical properties of the sample of households occupying monitored residential properties. The latter is discussed further below.

ever, we see several ways of evaluating nonresponse bias as it affects our panel of residential properties, their owners, and their occupants.

Exclusion of a property for reasons of nonresponse can occur at either of two stages: following the screening survey or following the baseline survey. In the former case, exclusion occurs because no residents of the property respond to the screening instrument; in the latter case, because either the landlord or all tenants declined to respond to baseline interviews. (The landlord attempt will generally come first, with tenant interviews following only if the landlord responds.) For owner-occupied single-family houses, of course, only one instrument is attempted, and a baseline nonresponse excludes the property and its owner-occupant from the monitoring program.

For properties excluded at screening time, we will have very little information--only the tax record and whatever is gleaned from incomplete screening instruments. However, our main concern is properties excluded at baseline because the owner fails to respond to the landlord or homeowner instrument. For these cases, we have more information about both the property and the landlord--from tax records, from completed screening instruments, from the residential-building instrument (which does not require the owner's cooperation), and finally from the attempted interview with the owner. From these sources, we should be able to construct a basic profile of excluded properties, their owners, and their tenants that will enable us to evaluate the likelihood of serious biases in our panel of residential properties.

As a further aid in assessing these possible biases, especially as they affect our data over time, we propose to draw a small sample of rental properties that were excluded from the panel because of landlord nonresponse. We will then survey the tenants of each such property at baseline and annually thereafter. Comparing tenant characteristics in this sample with the characteristics of tenants in the rental properties included in the panel, we can determine whether there are significant differences between the two groups. Comparing the two samples in terms of tenant-supplied data on housing characteristics and their dealings with their landlords, we can learn much more about differences between the two groups in terms of housing characteristics and landlords. We will be able to make such comparisons not only at baseline, but in subsequent years.

Our preliminary estimate of the number of properties needed for this "bias panel" is about 120, containing about 200 housing units whose occupants will be interviewed annually.

The Household Sample

The monitoring program described above includes annual interviews with the occupants of our panel of residential properties, about 3,700 households at each site. The households in this sample are chosen on the basis of characteristics of their residences rather than on the basis of their own characteristics; but from prior knowledge of the general distribution of households at each site by household characteristics by type of housing, we are able to estimate the composition of the baseline household sample even though we do not strictly control it by our sampling procedures.*

Table 4.11 summarizes our general expectations about the composition of the baseline panel of households at each site, by income and relationship to the housing allowance program. The figures are based

Table 4.11

APPROXIMATE COMPOSITION OF BASELINE SAMPLE OF HOUSEHOLDS OCCUPYING MONITORED HOUSING UNITS, BY INCOME AND ALLOWANCE PROGRAM STATUS

Income and Allowance Program Status	Number of Households	Percent of Total
Under $7,000		
Participants	939	25.2
Nonparticipants[a]	824	22.1
$7,000-9,999	941	25.3
$10,000 or more	1,014	27.3
Total	3,720	100.0

SOURCE: Estimates by HASE staff from data reported by the U.S. Censuses of Population and Housing, 1970, for one of the candidate experimental sites. See Massell, op. cit., WN-8218-HUD, Table 3 and related text.

[a]Both eligible and ineligible.

*HUD strongly questions both the proposed size of our household sample and the procedure for its selection. The issues raised by HUD are systematically addressed in Massell, op. cit., WN-8218-HUD.

on data for one of the candidate sites and do not include households to be interviewed in unmonitored structures--the "bias panel" described above. If these were included, the household counts given in the table would be increased by about 5 percent.

Inasmuch as the panel of monitored properties is designed as a stratified random sample of all residential properties within the boundaries of the experimental site, its occupants constitute a stratified random sample of all households residing within those boundaries--with the qualification that bias could be introduced into the household sample as well as into the property and landlord samples when properties are excluded from the panel because of landlord nonresponse to the baseline instrument. However, our "bias panel," consisting of tenants of unresponsive landlords, should enable us to judge the nature and extent of bias.

When this sample of households is viewed in terms of general experimental purposes--i.e., analyzing housing-market behavior in the context of a housing allowance program--its composition can be seen to have substantial merits. It is divided almost equally among four groups that stand in different relation to the allowance program. One is the group of those who will actually be enrolled in the allowance program. The second is the group of nonparticipants with annual incomes under $7,000 who, prior to the program, most directly competed with potential enrollees for housing. The third is the group with incomes between $7,000 and $10,000 who, after the program is under way, will compete most directly with enrollees (now able to spend more) for housing. The fourth is the group with incomes of $10,000 or more, who will be affected by the allowance program only indirectly if at all.* Naturally, our income intervals do not cleanly separate these groups, but the general configuration is clear.

Because of tenant turnover in our panel of residential properties, the elements of this household sample will not be identical from year

---

*This group (households with incomes of $10,000 or more) comprises about half of the household population in our candidate sites; thus, we devote about one-fourth of our household monitoring resources to households in the upper half of the income distribution.

to year. When we encounter a new occupant in a monitored housing unit we plan to administer the baseline survey instrument, which includes an extensive retrospective on residential mobility and associated circumstances of housing, family size and composition, income, employment, and place of work. Despite tenant turnover, the sample of households interviewed each year will still constitute a stratified random sample of all households.

Since the allowance program may affect residential mobility, we cannot confidently predict the rate of tenant turnover from preallowance data. Generally, we expect that 15 to 25 percent of the households interviewed in any given year will have moved before the next annual survey cycle. About one-third of the movers will have left the metropolitan area; the others will have relocated within the boundaries of our experimental sites.

Those who make local moves could be followed to their new homes by our field interviewers. This step is not essential to getting information on postallowance moving behavior and its housing consequences, inasmuch as those who move into our panel of monitored housing units will constitute a stratified random sample of all movers. However, examination of the sample sizes required for the mobility analyses described in Sec. VIII suggests that we are likely to need data on those who move out as well as on those who move in.* How many and which of the local movers should be tracked to their next residence each year is a decision that should be postponed until the end of the year's survey cycle, when we will know the number and the characteristics of those who moved out of monitored units during the preceding year.

OTHER MONITORING ACTIVITIES

In addition to monitoring the records of the HAO and the buildings, owners, and tenants of our panel of residential properties, we now plan several special-purpose surveys on a much smaller scale. We

---

*See Massell, op. cit., WN-8218-HUD, Sec. IV.

also expect that developments at each experimental site will prompt additional special-purpose surveys whose nature cannot now be anticipated. Finally, in addition to systematic field surveys, we propose to assign a resident observer to each site to provide us with continuous informal observation of housing-market activity, community attitudes toward the allowance program, and related political developments.

Special-Purpose Surveys

We presently see needs for small field surveys relating to two special groups: housing-market intermediaries and households who do not participate in the allowance program.

Market Intermediaries. Section VII of this report describes our plans for analyzing the effects of the housing allowance program on indirect suppliers and market intermediaries. These plans call for annual interviews with all major mortgage-lending institutions at each experimental site (perhaps 20 to 30 institutions) to monitor changes in their lending policies and to obtain data on delinquencies, foreclosures, and voluntary surrenders. In addition, a sample of firms writing residential-property insurance in the community will be interviewed annually to find out about changes in underwriting policy and trends in losses. Finally, we may find it useful to interview samples of real-estate brokers, management firms, or rental agents to investigate ways in which the allowance program has affected their policies and activities.

In all of these cases, the numbers of interviewees is small and the interviews can best be conducted by a single individual who is conversant with the research issues and the nature of the respondent's business. We expect to use members of Rand's professional staff (or consultants) as interviewers. While some quantitative data will be sought, open-ended probes into policies, operating procedures, and views of the housing market are expected to be more enlightening than the quantitative data. Thus, we do not plan elaborate survey instruments.

Nonparticipating Households. A particularly interesting group within the general population of households are those who are eligible for housing allowances but either fail to apply or drop out of the program. We propose to survey a sample of such households, in part to learn their reasons for not enrolling and in part to learn enough about their housing and budgetary circumstances to judge whether it is important from the standpoint of program objectives to recruit them into the program--and if so, what program or procedural changes would be likely to have this effect.

A sample of such households can be obtained from among occupants of our panel of residential properties at the time of the first annual survey cycle following the commencement of the allowance program. From baseline survey questions on income and household composition, we will be able to judge eligibility, and from allowance-program records we can determine which of the eligible households have not enrolled. These, or some subset of them, can then be approached for interviews with instruments that include special modules covering the matters described above.

Preliminary estimates of participation rates for eligibles indicate that our household sample is likely to include about 250 to 300 eligible but nonparticipating households. We think this is a large enough sample for our purposes.

Section IX of this report describes our plans for monitoring the attitudes of nonparticipants generally toward the allowance program. These plans do not imply any special survey work at baseline, prior to the commencement of program operations. In subsequent years, our survey instruments for landlords, tenants, and homeowners in our panel of properties could incorporate modules designed to elicit such attitudinal data; on the other hand, there are arguments for seeking such data from a separate sample of nonparticipants, chosen independently each year. At present, we are not certain which course, on balance, is preferable.

Ad Hoc Surveys. It is not hard to imagine experimental developments that will raise questions of fact, perhaps concerning the supply of eligible households, the representativeness of one of our samples,

the vacancy rates for housing of particular kinds, the conversion of properties from one use to another--there are many possibilities. To answer such questions, we may need to design and implement field or telephone surveys on short notice.

We propose to maintain at each site a limited capability for such survey work, under the direction of the resident observer whose other functions are discussed below. This capability is most apt to be useful in the event that the experiment has an unanticipated but significant effect on some particular sector of the housing market or on some particular community or group. Rather than wait for an annual survey cycle, we will want to inform ourselves quickly about the measurable aspects of the effect so that plans can be adjusted accordingly.

Informal Monitoring

We propose to assign a resident observer to each experimental site to provide a continuous flow of information on events not captured or not capturable by formal survey methods. It will be his responsibility to follow local economic and political developments and community sentiments about the housing allowance program and related matters. He will spend much of his time attending meetings of civic and other local-interest groups, reading local newspapers, following events at City Hall, and talking informally to individuals active in the local housing market as suppliers, intermediaries, and consumers of housing.

While the means by which the resident observer gathers information will be informal, his reporting will be systematic. His reports will serve to alert our research staff (and our local site manager) to developments requiring programmatic or research responses. We expect him to be an active contributor to the ongoing review and evaluation of the experiment.

The resident observer will be supported by a part-time staff that may be employed for special projects--ad hoc surveys, abstracting public records, etc.

OTHER DATA SOURCES

Not all the data needed for our analysis will be obtained by direct fieldwork or from HAO records. We expect to make considerable use of secondary data, gathered by others for other purposes, including local public records, local planning and economic studies, corporate records, and various kinds of data gathered by state and Federal statistical systems and private firms. We cannot now catalogue these sources exhaustively for each site, but the likeliest possibilities are indicated briefly below.

Public Records and Local Planning Studies. We expect to rely on public records for a variety of data, including locations of public facilities (schools, shopping centers, police and fire stations, hospitals, parks and recreational facilities, transit routes); information on the types of public utility or service available in each neighborhood (electricity and gas distribution, water and fire hydrants, sanitary and storm sewers, garbage collection); land-use and zoning maps; and various social indicators such as small-area crime and fire statistics. These data will be assembled at baseline and updated annually.

In addition, local tax-assessment records will be used in selecting and screening our panel of residential properties; each year after baseline, building permits and demolition permits will be analyzed to guide panel modification. Local planning-office studies of neighborhoods or larger areas may provide helpful special-purpose information.

U.S. Censuses of Population and Housing. Although the most recent census (1970) precedes by nearly four years the baseline surveys at our first site, it is nonetheless the most comprehensive and detailed source of information available on local population and housing characteristics. We have already made extensive use of these data in designing our sample of residential properties and in estimating allowance-program eligibility. We anticipate continued use of these data, primarily for independent checks on estimates made from our

field surveys. While discrepancies are to be expected because of the increasing obsolescence of the census data, large discrepancies in numbers or composition of households or housing-stock estimates should be the occasion for special investigation to account for the differences.

Bureau of Labor Statistics and Related Sources. Appendix D of this report explains our plan for constructing price indexes for services, labor, and materials used in the production of housing services at each experimental site. We expect to make use of the BLS's Area Wage Survey and its Wholesale Price Index. Data on construction costs and mortgage interest rates, however, will come from local and regional indexes compiled by private firms (the Boeckh Building Cost Modifier, Dodge Building Cost Index, Roy Wenzlick Research Corporation).

V. ORGANIZING THE DATA FOR ANALYSIS

In Sec. II, we listed the four major research topics that motivated the design of the Supply Experiment, and we outlined the proposed sequence of events at each experimental site, including monitoring activities designed to provide data bearing on each topic. Section IV described that monitoring program in greater detail, explaining the principal sources of data and the methods planned for data collection. Here, we describe our plans for organizing these data, showing how they will be compiled into systematic files that can be manipulated in various ways for research purposes.

As indicated in Sec. IV, the monitoring program will acquire data from the administrative records of the HAO; from field surveys addressed to a panel of residential properties, their neighborhoods, their owners, and their occupants; from smaller special-purpose surveys; and from reports by the resident observer. Data from the first two sources will be large in volume, regular in format, and interconnected in reference; they are the most amenable to statistical analysis and, at the same time, they present the most difficult management problems. It is with these data that this section principally deals, although brief attention is given to the smaller special-purpose surveys. Reports of the resident observer may be equally important as a source of insights and as signals of phenomena that have escaped more systematic data-collection procedures, but data from this source pose no particular problems of file management and are not discussed here. Neither do we discuss data from secondary sources, such as the U.S. Census of Population.

Our concern here is not with the mechanics of data-processing but with the principles we will use to organize the large data files to serve research purposes.* We first explain the content and organization of each master file that we plan to create, then show how these master

*For a more technical description of the data-management system, see C. M. Dodd, M. C. Fujisaki, and G. Levitt, *Data Management System for the Housing Assistance Supply Experiment*, The Rand Corporation, WN-8054-HUD, November 1972.

files can be used separately and jointly to create research files for particular purposes. Finally, we show how the data contained in each file relate to each of the major research topics to be addressed by the Supply Experiment. This last step serves as a bridge to the ensuing four sections, which present our current plans for analyses relating to each of these topics.

CREATING AND LINKING THE MASTER FILES

Presently, we expect to create about twelve separate master files, each designed to receive periodic data from a particular source, storing the data cumulatively for permanent reference. Three of these master files will be compiled from administrative records of the HAO; four will be compiled from annual surveys relating to our panel of residential properties. These two groups constitute our major files, presenting the most complex file-management problems. The remaining five minor files are either temporary in purpose (the screening survey used to select our panel of properties) or small in size, and present less of a management problem.

The HAO Files

Table 5.1 describes the three files that we expect to create from HAO records. Although the HAO record system has yet to be designed, certain kinds of data will clearly be needed for administrative purposes and are likely to be organized as indicated here. In any case, the data will be periodically transferred to Rand for research purposes and can then be reorganized if necessary.

Each of these files will be divided into unit records, one for each household ever enrolled in the program. All are longitudinal files; new entries will be added periodically to each unit record until the subject household is separated from the program, at which time its record is (at least temporarily) closed but remains in the file.

The first of these is an enrollment file, containing a basic description of each enrolled household, including its home address, size, composition, and income, and other information bearing on

Table 5.1

ORGANIZATION OF HAO FILES, BY SOURCE OF DATA

Source of Data	Population Covered	Description of Unit Record	Approximate Number of Records	Frequency of File Entries	
				Initial	Update
HAO enrollment records	All households ever enrolled in allowance program	1 per household	5,000-10,000	Upon enrollment in program	Semiannually from date of enrollment till separation
HAO disbursement records	All enrollees occupying certified housing	1 per household	5,000-10,000	Upon certification of housing unit	Monthly, as allowance payments are made
HAO housing-inspection records	All housing units currently occupied by allowance recipients[a]	1 per household	5,000-10,000	Upon request by enrollee	Each change of residence and annually thereafter

SOURCE: HASE staff, preliminary concepts of HAO record system and program procedures.

[a]The file may also contain some units certified at the landlord's request.

eligibility. Recertification procedures will provide semiannual updates of household size and income for each unit record so long as the household remains enrolled. Changes of address will be entered as they occur, and other administrative actions may result in entries at irregular intervals. Updating in this and all other files described below will be done by adding entries to the appropriate unit record, not by replacing previous entries.

A separate file will probably be maintained by the HAO to record disbursement of allowance payments to enrollees whose housing has been certified as meeting program standards. Each record in this file will include the name and current address of the authorized recipient, the name and address of his landlord, the certification status and contract rent of his housing unit, and the amount of the allowance payment. Records will be updated monthly by the HAO to reflect administrative actions bearing on allowance entitlement, changes of address, etc. At less frequent intervals, copies of the updated files will be transferred to Rand for analysis.

A third file will be constructed from HAO housing-inspection records. These inspections provide the basis for housing certification, a prerequisite to actual disbursement of allowance payments. The housing unit must be reinspected and recertified annually; if the allowance recipient moves, the new unit must be inspected and certified. Thus, this file will contain a continuous history of each recipient's housing for the duration of his enrollment, with periodic information on the condition of the unit and of the structure of which it is a part.

Each of these three files will contain a unit record for each household ever enrolled, the identity of the household defined by the person who heads it. We expect the size of each file to increase over time as new households join the program; at the end of five years, there may be as many as 10,000 unit records in each.

Figure 5.1 is a schematic representation of the cross-sectional and temporal links among these three files. Unlike the panel survey files discussed below, the timing of entries in the different files is not synchronized, so that a cross-sectional link at a given point in time may relate entries that are nearly current with entries that are

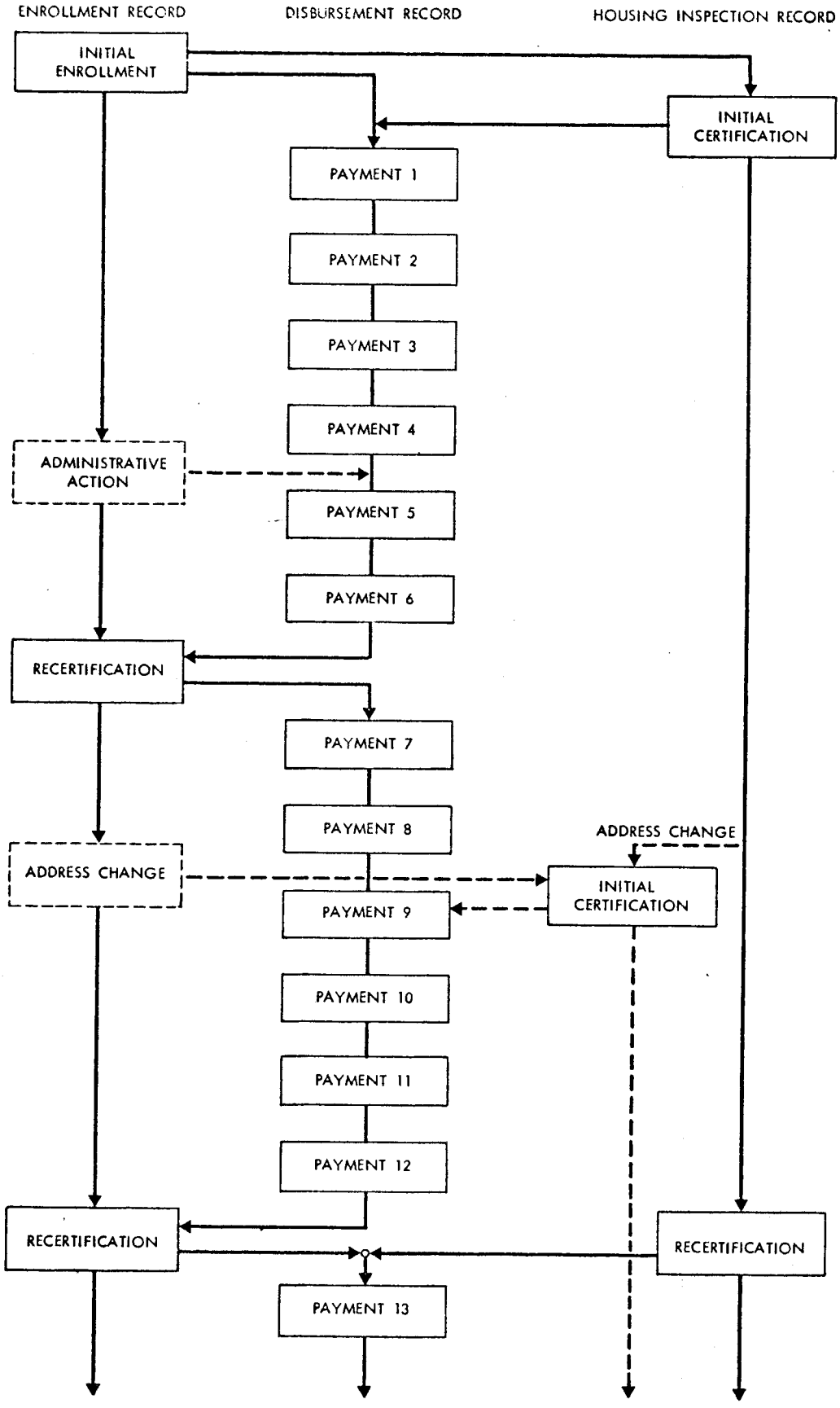

Fig. 5.1 – Cross-sectional and temporal linking of HAO records for one enrolled household

up to a year old. Moreover, the cyclical entries on individual unit records are not synchronized even within a given file: The initial enrollment dates, hence annual recertification dates, will differ for each enrollee; the same is true of housing-certification entries.

Panel Survey Files

Table 5.2 describes the four major files we expect to create from annual surveys related to the panel of residential properties. The unit records of each file will be opened at baseline, with new data pertaining to the same unit added annually thereafter.

The smallest of these files is the one compiled from the survey of neighborhoods. The entire area of each experimental site will be divided into 50 to 100 bounded neighborhoods; a unit record will be opened at baseline for each neighborhood and updated annually by a mixture of direct field observation and data drawn from public records.

From the survey of residential buildings, a unit record will be opened for each residential property selected at baseline as an element of our panel. The unit record may include data for more than one building, when more than one is present on a selected property (tax parcel). In subsequent years, the configuration of buildings may change, or existing residential buildings may be demolished or converted to nonresidential use; however, the unit record will be maintained by annual entries from the survey of vacant and nonresidential properties (V/NRP).

Unit records will also be maintained in this file for the small baseline sample of urban nonresidential properties, likewise monitored annually by means of the V/NRP instrument. Finally, after baseline, we contemplate an annual sample of rural residential building permits, and new unit records will be opened for each property selected from this source.

Altogether, we expect this file to contain fewer than 2,500 unit records at the end of the five-year monitoring program--all those in the baseline panel plus the newly constructed rural homes added later.

The third file in this group will be compiled from the annual survey of landlords, with a unit record opened for the owner of each

Table 5.2

ORGANIZATION OF PANEL SURVEY FILES, BY SOURCE OF DATA

Source of Data	Population Covered	Description of Unit Record	Approximate Number of Records	Frequency of File Entries	
				Initial	Update
Survey of neighborhoods	Entire site	1 per neighborhood	50-100	Baseline	Annually
Survey of residential buildings[a]	Stratified random sample of all residential properties	1 per property	2,000-2,500	Baseline	Annually
Survey of landlords[b]	Current owners of all rental properties in sample	1 per property	1,800-2,200	Baseline	Annually
Survey of tenants and homeowners[c]	Current residents of all properties in sample[d]	1 per housing unit	3,500-4,000	Baseline	Annually

SOURCE: HASE staff, survey designs and sampling plans.

[a]File includes approximately 200 records for nonresidential properties, with file entries at baseline and annually thereafter; if such a property is converted to residential use, it is thereafter included in the survey of residential buildings.

[b]File includes special entries for vacant properties, from the survey of vacant and nonresidential properties.

[c]For vacant properties, formerly owner-occupied, the file contains special entries from the survey of vacant and nonresidential properties.

[d]For sample structures with four or fewer housing units, occupants of all units will be interviewed; for larger structures, a maximum of six units will be surveyed.

rental property in our baseline panel. In the event of a change of ownership, data on the new owner will be entered in the same unit record. If a single-family rental house is sold to an owner-occupant, the landlord record will be closed and a new unit record opened in the tenant-and-homeowner file, described below; of course, appropriate identification of both records will enable them to be linked for analysis.

If the property has no tenants in residence at the time of an annual survey cycle, the vacant-property modules of the V/NRP instrument will be the source of the year's entries for this file.

We expect this file to contain no more than 2,200 unit records at the end of the five-year monitoring period. All unit records will have a complete baseline entry, this being a condition of sample selection; but in some subsequent years, we may be unable to locate the landlord, or he may decline to provide the information sought. Thus, some unit records will be incomplete.

The last file of this group will be compiled from data gathered in the annual survey of tenants and homeowners. A unit record will be opened at baseline for each housing unit that is to be monitored. For single-family houses, whether renter-occupied or owner-occupied, there will of course be a single unit record for each residential property. For multiple dwellings containing fewer than five housing units, there will be a unit record for each housing unit on the property. For larger multiple dwellings, there will be up to six unit records, each for a specific housing unit selected at baseline. Unit records for tenants and for homeowners will differ in format; for the latter, survey data to be collected include information parallel to that sought for landlords of rental properties.

In subsequent years, some residential properties will be altered in ways which may either increase or reduce the number of housing units on the property; thus, new unit records may be opened or existing ones closed. In ambiguous cases, as when two units are merged, the identity of the surviving unit will be that of the baseline unit containing the same kitchen; undoubtedly, situations will arise that require ad hoc decisions as to which unit records to continue and which to close.

The identity of a household occupying a monitored housing unit will be keyed to a particular individual designated on first interview as the household head; if that individual moves out or dies, the occupants will be treated as a different household, even though there may be common members of the old and new households. More frequent will be the case in which an entire household moves out to be replaced by another.

Whichever is the case, the unit record pertaining to the monitored housing unit will accept entries reflecting the change in its tenancy; first interviews with a given household will generally be broader in scope than subsequent interviews.

For owner-occupied homes and single-family rental houses, all unit records will have a baseline entry unless the housing units to which they refer were vacant at baseline; multiple dwellings will be accepted into the panel at baseline as long as at least one occupant responds to the tenant survey, so some unit records may lack baseline entries. In subsequent years, unit records for all classes of properties will sometimes lack entries, either because a housing unit is vacant or because the occupant fails to respond to the survey. For vacant single-family houses that were last occupied by their owners, we will try to find the current owner and administer the vacant-property modules of the V/NRP instrument. If a single-family home transfers from ownership to rental tenure or vice versa, the unit record will be continued for subsequent occupants.

We expect this file, including both tenants and homeowners, to contain fewer than 4,000 unit records at the end of the five-year monitoring program.

We plan to make considerable joint use of these four files in the analyses described in later sections. They will therefore be designed for easy cross-sectional linking, as illustrated in Fig. 5.2. Each property, landlord, and occupant record will include a neighborhood identification code, linking it to a specific neighborhood record. Each landlord and occupant record will also include a property identification code, linking them to the property and to each other. Thus, properties, landlords, and occupants can separately or jointly

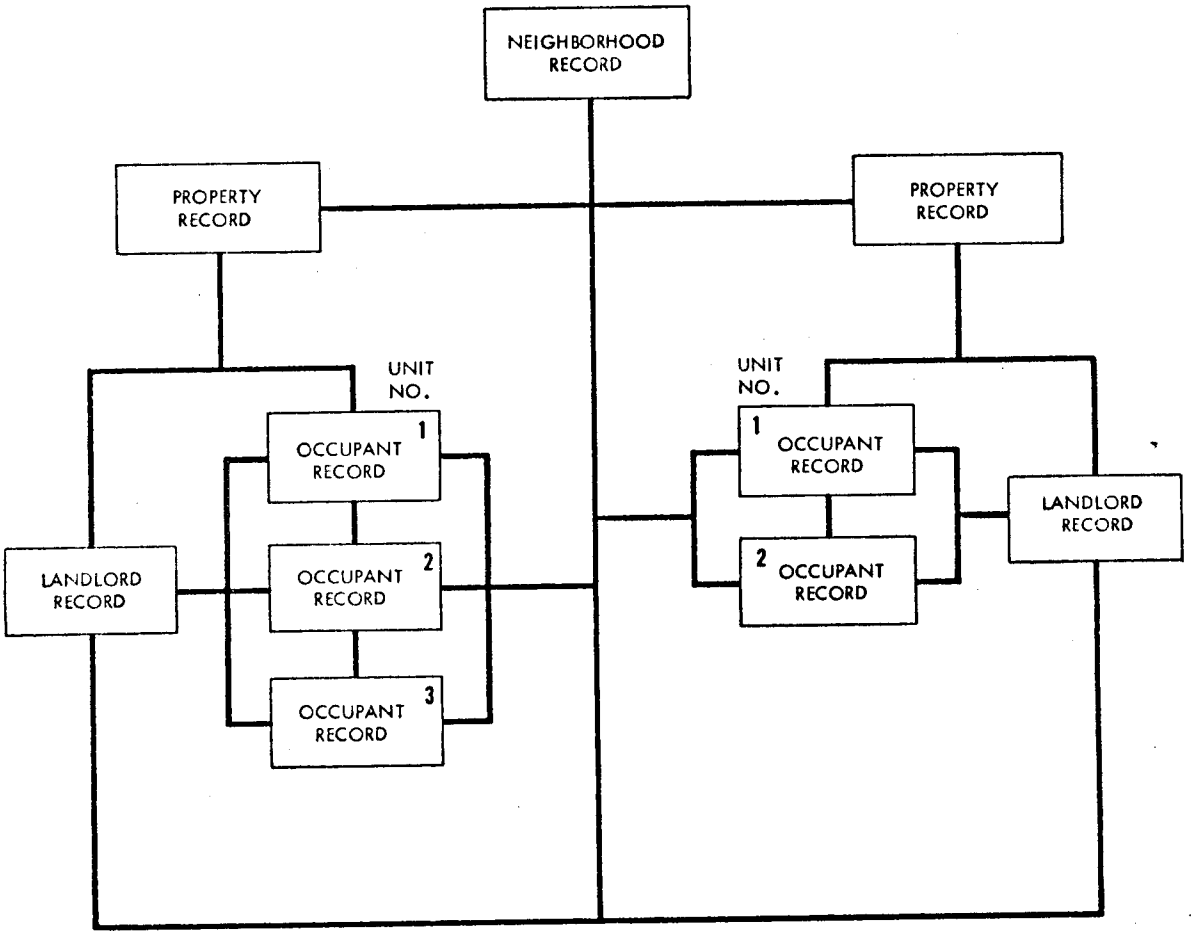

Fig. 5.2 – Cross-sectional linking of panel survey records for one neighborhood

be grouped for analysis by neighborhood; or properties can be grouped by type of property, and a special working file created which abstracts pertinent parts of the neighborhood, landlord, or occupant file. Similar special files can be created for landlords or for tenants, each carrying selected information from the other files.

As illustrated in Fig. 5.3, these cross-sectional links can also be extended over time. Each unit record in each file will have annual entries, all synchronized within the time needed to complete the fieldwork for a given survey, so temporal linkage--e.g., for measuring annual changes in some variable, record by record--is built into the file structure. But it will also be possible by means of mutual identification codes to link a landlord-record entry for Time 1 to an occupant entry for Time 2, etc. This feature of the file opens the way for analysis of lagged interactions among the actors and events relating to a particular property or neighborhood.

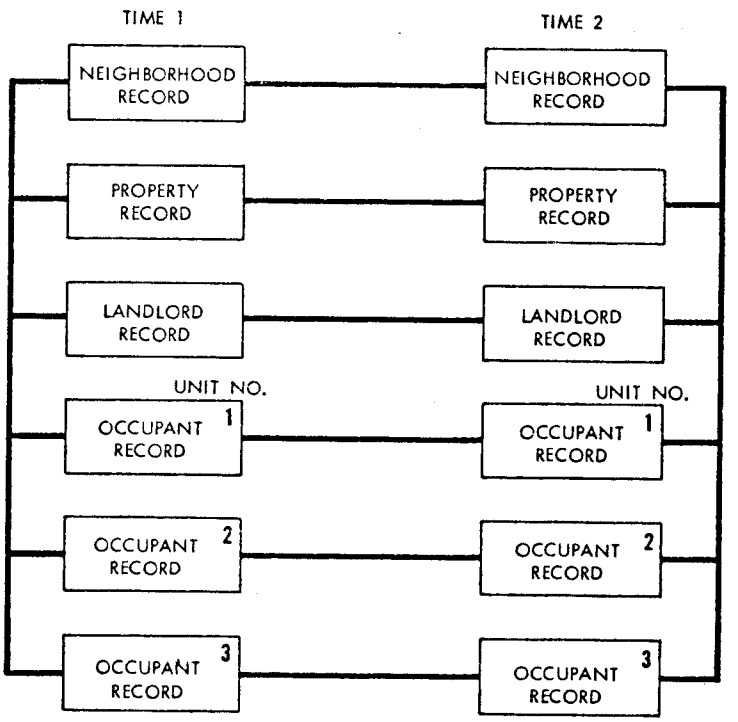

Fig. 5.3 – Temporal linking of panel survey records for one residential property

Linking HAO and Panel Survey Files

In addition to the links among the three HAO files and among the four panel survey files, we also expect to be able to link HAO records to panel survey records in a way that will enrich the analytical possibilities of both files.

When the HAO records are transferred to Rand, one step in our processing will be to compare the name and address of each allowance recipient and his landlord with the current occupant, landlord, and property records compiled from our panel surveys. When a match is found, we will add appropriate codes to the HAO records, creating permanent links between the two sets of files. In most such cases, the allowance recipient will be the occupant of a monitored housing unit, and we will have data on his household from the survey of tenants and homeowners. Occasionally, the allowance recipient will be the occupant of an unmonitored housing unit in a monitored multiple dwelling; then, we will have only a landlord and building record to match with HAO records.

As illustrated in Fig. 5.4, we expect that about 25 percent of our panel survey unit records can be linked in any given year to HAO records; or viewed from the other perspective, about 15 percent of all HAO records in any given year can be linked to panel survey records. The number of linked records should be in the vicinity of 800 to 1,000.

The analytical possibilities created by this overlap of HAO records and panel survey records are numerous. We can, for instance, compare HAO housing-inspection reports with our own survey of residential buildings and with housing-unit characteristics reported by the survey of tenants and homeowners. We can count the number of allowance recipients occupying a monitored multiple dwelling and track changes in this number over time, or tabulate sequences of recipients and nonrecipients in particular housing units or buildings. Most important, by stratifying on variables that are present in both HAO and panel survey records, we can generalize about allowance recipients and their housing circumstances in terms of variables recorded by one source, using the other source to estimate incidence of occurrence in the relevant population.

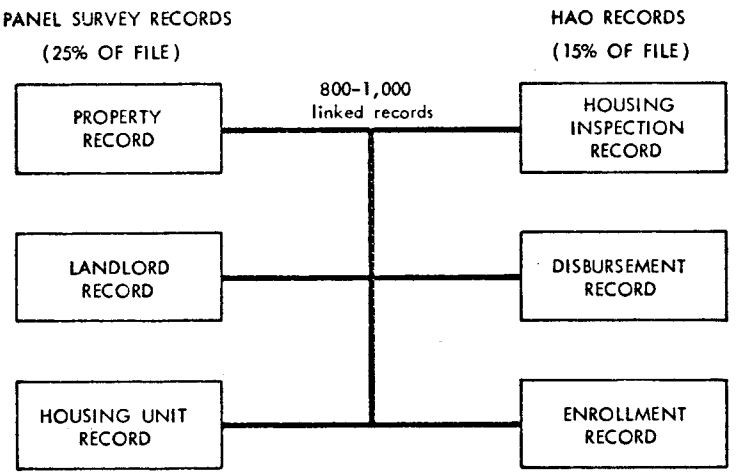

Fig. 5.4 – Cross-sectional linking of panel survey and HAO records for one enrolled household

Minor Files

Table 5.3 describes five minor files that we expect to create from special-purpose surveys. Only the first of these, the screening survey, has been designed in detail. Of the other four, three relate to market intermediaries; data-collection efforts will begin at baseline, with annual updates thereafter. The survey of movers will not be conducted until the first annual survey cycle after baseline.

The largest of these files, and the first to be opened, will be the one compiled from the screening survey. There will be a unit record on each household residing in the presample of all residential properties. This data file will be used for the final selection of properties for the panel surveys; information from this file will also be used in setting allowance-program standards. This file will not be updated; it will contain only the data from the screening survey. The file will be organized so that it will be possible to link it with the panel survey files by comparing housing unit and property identifiers. Since not all screened properties were selected for the baseline panel, only a portion of the records in the screening survey file can be linked to the panel survey files.

Table 5.3

ORGANIZATION OF MINOR DATA FILES, BY SOURCE OF DATA

Source of Data	Population Covered	Description of Unit Records	Approximate Number of Records	Frequency of File Entries	
				Initial	Update
Screening survey	Current residents of presample of all residential properties	1 per housing unit	8,000-10,000	Prebaseline	---
Survey of mortgage lenders[a]	All major mortgage lending institutions active in site	1 per institution	20-30	Baseline	Annually
Survey of property insurers[a]	All major underwriters active in site	1 per company	10-20	Baseline	Annually
Survey of real-estate brokers[a]	Sample of realtors active in site	1 per firm	40-50	Baseline	Annually
Survey of movers[a]	Households moving out of sample of residential properties	1 per household	1,500-2,000[b]	Survey cycle following moveout	New sample annually

SOURCE: HASE staff, survey designs and sampling plans.

[a]Instruments not yet designed.

[b]Maximum five-year accumulation, tracking each local mover to next residence.

The surveys of mortgage lenders, property insurers, and real-estate brokers will include unstructured and qualitative information. While we posit automated data files on each of these, it is not clear that all, or even most, of the information from these surveys will be amenable to the data-control and processing methods planned for the other surveys. The files are small, with from 10 to 50 unit records in each. Unit records will be opened at baseline and updated annually thereafter.

After baseline, we will maintain a file on households that move out of our panel of monitored housing units. This file will be compiled from the survey of movers and will contain data pertinent to the analysis of mobility. The format and sample design for this survey are not yet established, nor will they be required until a year after baseline. We estimate that some 1,500 to 2,000 unit records would accumulate over five years if all local movers were tracked to their next residence. This file can be linked with the tenant-and-homeowner file through the household identifiers. Thus data on each household in the movers file can be linked with data on these same households for the period of their residence in monitored structures.

ORGANIZING THE DATA FOR ANALYSIS

From the master files described above, we expect to create many temporary working files, each designed to serve some specific analytical purpose. Some of these working files may consist of data from a single master file; others will include data from several master files, linked by common identifiers such as household, housing unit, property, or neighborhood codes. Some working files will be strictly cross-sectional, containing only data referring to a particular point or period of time; others will be longitudinal, following particular units of observation over time. As we have tried to show, the structure of our master files is such as to allow considerable flexibility in organizing the data for analysis.

Sections VI through IX, below, describe our plans for analyzing these data. Each section deals with one of the four major research

topics to which the Supply Experiment is addressed: (1) supply responsiveness, (2) behavior of market intermediaries and indirect suppliers, (3) residential mobility, and (4) effects on nonparticipants. Each of these analysis plans relies on data from several of our proposed master files. Before proceeding to these topical discussions, we will try to give the reader an overview of the various uses to which data from each file will be put.

To provide this general perspective, it is, of course, necessary to suppress detail. Unit records in our major files may eventually contain several thousand distinguishable items of data; in the two largest files (the survey of landlords and the survey of tenants and homeowners), the entries for the baseline year alone can number up to 500 for each unit record. Furthermore, each survey instrument is likely to be modified for postbaseline survey cycles, so precise descriptions of unit-record entries for future years cannot now be given.

Our purposes here will be better served by grouping the data topically, as was done in Sec. IV for discussion of the major survey instruments.* Such topics are listed in the stubs of Table 5.4 (for files based on HAO records), Table 5.5 (for files based on the four panel surveys), and Tables 5.6 to 5.8 (for files based on several special-purpose surveys).

Tables 5.4 and 5.5 each have four columns, one for each of our major research topics. In each column, we have attempted to indicate how data from each file will be brought to bear on this research topic, either as measures *(M)* of experimental results that are of direct interest, or as variables that help to explain *(E)* the experimental result in question. The format of Tables 5.6 to 5.8 is the same, except that only one research topic is listed for each. In other words, for each major research topic, we present a set of hypotheses whose general form is $M = f(E_i)$. The individual tables are *not* self-contained; the hypotheses "run" across all three tables, which must therefore be examined jointly.

---

*See Tables 4.1 to 4.6 of Sec. IV, where the kinds of data included under each topic are described systematically.

Table 5.4

RELATIONSHIP OF DATA FROM HAO RECORDS TO MAJOR RESEARCH TOPICS

Source and Description of Data	Relationship to Research Topic			
	Supply Response	Market Intermediaries	Allowance-Induced Mobility	Effects on Nonparticipants
HAO enrollment records	(a)	(a)	(a)	(a)
1. Identification of enrollee	E	—	E	E
2. Household composition	E	—	E	E
3. Income, by source	—	—	E	—
4. Employment and place of work	E	—	E	E
5. Allowance entitlement	E	E	E	E
6. Allowance program option	E	M	—	—
7. Terms of HAO lease (homeowners)	E	—	M	E
8. Place and characteristics of residence	—	—	—	—
9. Record of administrative actions				
HAO disbursement records	(a)	(a)	(a)	(a)
1. Identification of recipient	(a)	(a)	(a)	(a)
2. Identification of landlord or payee	E	—	E	—
3. Certification status of housing unit	M	—	E	E
4. Amount of contract rent	E	—	E	E
5. Amount of allowance payment				
HAO housing-inspection records	(a)	(a)	(a)	(a)
1. Identification of enrollee	(a)	(a)	(a)	(a)
2. Identification of housing unit	(a)	(a)	(a)	(a)
3. Identification of owner	M	—	E	E
4. Characteristics and condition of structure	M	—	E	E
5. Characteristics and condition of unit	M	—	E	E
6. Certification/decertification actions				

SOURCE: HASE staff, preliminary concept of HAO records.

NOTE: Entries in the table indicate nature of relationship: M = measures of dependent variables, E = explanatory variables.

[a]For enrollees, housing units, and landlords that are included in panel surveys, these identification codes provide links to panel survey data.

Table 5.5

RELATIONSHIP OF DATA FROM MAJOR PANEL SURVEYS TO MAJOR RESEARCH TOPICS

Source and Description of Data	Relationship to Research Topic			
	Supply Response	Market Intermediaries	Allowance-Induced Mobility	Effects on Nonparticipants[a]
Survey of neighborhoods				
1. Land-use patterns	E	E	E	E
2. Characteristics of residential buildings	E	E	E	E
3. Availability of facilities and services	E	E	E	E
4. Characteristics of residents	E	E	E	E
5. Quality of life	E	E	E	E
Survey of residential buildings				
1. Nature of use and tenancy	E	E	E	E
2. Physical characteristics of building	M	E	E	E
3. Related tenant facilities	M	E	E	E
4. Exterior condition of building	M	E	E	M,E
5. Interior condition of public areas	M	E	E	M,E
6. Characteristics of immediate neighborhood	E	E	E	M,E
Survey of landlords				
1. Acquisition and ownership of property	E	E	E	E
2. Experience and activity in real estate	E	E	E	E
3. Property description and revenues	M	E	—	E
4. Management, maintenance, and operating costs	M	M	E	M,E
5. Repairs, improvements, and other costs	M	M	E	M,E
6. Mortgages, taxes, and insurance	M	M	—	—
7. Perception of neighborhood	E	—	—	—
8. Landlord-tenant relationships	E	—	E	M,E
9. Plans for property	E	E	E	E
Survey of tenants and homeowners				
1. Household composition, tenure, and rent	M,E	E	E	M,E
2. Homeowner mortgages, taxes, and insurance	M	M	E	M
3. Characteristics and condition of housing unit	M	E	E	M,E
4. Tenant-landlord relationships	E	—	E	M,E
5. Perception of neighborhood	E	—	E	M,E
6. Housing expenses other than contract rent	M	M	E	M,E
7. Housing demand	E	—	E	E
8. Mobility and housing history	—	M	M,E	M
9. Income, and employment history	E	E	E	E
10. Social integration	E	E	E	M,E
11. Social identification	—	—	E	E

SOURCE: Fourth and fifth drafts of survey instruments, various dates from December 1972 to April 1973. Details may change in later drafts.

NOTE: Entries in the table indicate nature of relationship: M = measures of dependent variables, E = explanatory variables.

[a]Because this research question was broadly framed to include both actual effects (e.g., changes in housing quality or rent, frequency or geographic pattern of moves) and effects on nonparticipant attitudes toward the allowance program, a given variable may appear simultaneously as a measure (M) of one effect and as an explanation (E) of another.

Table 5.6

RELATIONSHIP OF DATA FROM SURVEYS OF MARKET INTERMEDIARIES TO INTERMEDIARY ANALYSIS

Source and Description of Data	Relationship to Intermediary Analysis
Survey of mortgage lenders	
1. Description of current portfolio	M
2. Recent lending activity	M
3. Secondary market activity	M
4. Current mortgage terms	M
5. Mortgage insurance	M
6. Lending policies	M
7. Delinquency and foreclosure experience	M
8. Effects of allowance program	E
Survey of property insurers	
1. Description of coverage in force	M
2. Recent underwriting activity	M
3. Reinsurance activity	M
4. Premium rates and terms	M
5. Preferred and assigned risks	M
6. Claims experience	M
7. Effects of allowance program	E
Survey of real-estate brokers	
1. Mix and volume of business	M
2. Structure of commissions and fees	M
3. Perceptions of market trends	M
4. Knowledge of speculative activity	M
5. Special submarket conditions	M
6. Effects of allowance program	E

SOURCE: HASE staff, preliminary concepts of surveys of market intermediaries.

NOTE: While the table indicates probable topics, the instrument formats for these surveys are not yet established.

Entries in the table indicate the nature of the relationship: M = measures of dependent variables, E = explanatory variables.

Table 5.7

RELATIONSHIP OF DATA FROM SURVEY OF MOVERS TO MOBILITY ANALYSIS

Source and Description of Data	Relationship to Mobility Analysis
Survey of movers	
1. Origin and destination of move	M
2. Household composition, tenure, rent	E
3. Homeowner mortgages, taxes, insurance	E
4. Characteristics and condition of housing unit	E
5. Tenant-landlord relationships	E
6. Perception of neighborhood	E
7. Housing expenses other than contract rent	E
8. Current income, employment, place of work	E
9. Social integration	E
10. Reasons for moving	E
11. Methods of residential search	E

SOURCE: HASE staff, preliminary concept of survey of movers.

NOTE: Survey-of-movers instrument is to be administered in each postbaseline survey cycle to a sample of households moving from monitored housing units to other local addresses. Thus, each mover record can be linked to a tenant/homeowner record for the preceding year.

Entries in the table indicate nature of relationship: M = measures of dependent variables, E = explanatory variables.

Table 5.8

RELATIONSHIP OF DATA FROM POSTBASELINE SPECIAL MODULES OF SURVEY OF TENANTS AND HOMEOWNERS TO ATTITUDE ANALYSIS

Source and Description of Data	Relationship to Attitude Analysis
Survey of program participants	
1. Knowledge of program	E
2. Contacts with HAO	E
3. Contacts with other participants	E
4. Dealings with landlords	E
5. Housing certifiability problems	E
6. Attitudes towards program	M
Survey of eligible nonparticipants	
1. Knowledge of program	E
2. Contacts with HAO	E
3. Contacts with participants	E
4. Dealings with landlords	E
5. Housing certifiability problems	E
6. Reasons for nonparticipation	E
7. Attitudes towards program	M
Survey of ineligible nonparticipants	
1. Knowledge of program	E
2. Contacts with HAO	E
3. Contacts with participants	E
4. Dealings with landlords	E
5. Attitudes towards allowance program	M

SOURCE: HASE staff, preliminary concepts of postbaseline "attitude" modules, survey of tenants and homeowners.

NOTE: Modules similar to those indicated will be administered to indicated subsamples of all households living in monitored housing units in each postbaseline survey cycle. Topics covered must be considered in conjunction with "regular" modules of the survey of tenants and homeowners, shown in Table 5.5 for baseline instrument.

Entries in the table indicate the nature of the relationship: M = measures of dependent variables, E = explanatory variables.

Our analysis plans for each major research topic entail seeking answers to several distinct, if related, questions. Thus, there is not a unique *M* for each major research topic; the several *M*s shown in each column in some cases indicate different kinds or sources of data that will be combined into a single measure of an experimental result and in other cases indicate measures that are individually of interest. Similarly, in addressing different questions within a major research topic, different groups of indicated explanatory variables will be appropriate. Finally, some kinds of data appear as measures *(M)* in one context but as explanatory variables *(E)* in another.

Although perusal of these entries will provide the reader with a general idea of the logic of our analyses--i.e., what is to be explained by what--the explicit hypotheses cannot be adequately represented in this summary fashion. For a clearer understanding of the specific research questions to be addressed, the variables entailed in each, and the technique of analysis, we refer the reader to the four analysis plans presented in subsequent sections of this report. For more detail as to the specific data to be used in these analyses, the survey instruments themselves must be consulted. Here, we only want to show the extent to which each analysis plan draws on the different data files, and the extent to which a given class of data serves multiple analytical purposes.

Analysis of Supply Response

Our analysis of supply response to the experimental housing allowance program will draw both on HAO records (Table 5.4) and on records of the four panel surveys (Table 5.5).

To *measure* supply response, we rely primarily on the survey of landlords for rental properties and the survey of homeowners for owner-occupied properties to provide data that reflect changes over time in expenses related to the provision of housing services (to the market or to the owner-occupant). These measures *(M)* are supplemented by direct observation (survey of residential buildings) and occupant reports (survey of tenants and homeowners) of changes in the physical characteristics and condition of panel properties; for housing occupied by

allowance-program participants, similar information can be obtained from HAO housing-inspection records, some of which can be linked to panel survey records.

Explaining supply response entails two general lines of analysis: (1) determining the market stimulus provided by the allowance program and estimating the impact of this stimulus on market rents and rental revenues; and (2) explaining variations in supply responses among residential properties with different physical characteristics or locations or with different kinds of tenants or owners. Analysis of the first kind draws on HAO enrollment and disbursement records for data on the stimulus *(E)* provided by the allowance program generally and its specific incidence within the market; and on the landlord and tenant surveys for data on changes in market rents and rental revenues in various sectors of the market. Analysis of the second kind draws on all four panel surveys for neighborhood, building, landlord, and tenant characteristics *(E)* that may affect supply responses.

Analysis of Market Intermediaries and Indirect Suppliers

Here, we are interested generally in allowance-related changes in the policies and activities of those who supply mortgage capital, insurance, management or brokerage services, and residential repairs and improvements; and in impediments to the success of the allowance program resulting from shortages of these services or to the inefficiency or restrictive policies of those who provide them. However, our research is not guided by strong prior hypotheses; we think these will emerge only as we monitor the allowance-stimulated market.

For baseline, at least, our analytical objectives are essentially descriptive, following two lines: (1) to learn how the services of market intermediaries and indirect suppliers are used by different sectors of the housing market, and (2) to learn how the industries themselves are organized and how their decisionmakers view the market.

The first purpose will be served principally by data drawn from the panel surveys (Table 5.5), where data *(M)* will be collected on mortgage financing and insurance, the use of management firms, rental agents, and real-estate brokers, and the use of contract services for

maintenance, operations, repairs, and improvements; variations in utilization can be related to characteristics *(E)* of the neighborhood, the building, the landlord, and the occupants.

The second purpose will be served by small-scale and informal (but systematic) surveys of members of the relevant industries. Table 5.6 describes the tentative contents of three such surveys--of mortgage lenders, property insurers, and real-estate brokers. We expect to assemble data on other such industries (e.g., home-repair contractors) from secondary sources and to monitor them informally; however, we may at some point determine a need for systematic field surveys.

Analysis of Residential Mobility and Neighborhood Change

Our interest here relates to changes in the frequency and pattern of movement that result from the allowance program and its long-run implications for the residential distribution of program participants and others.

For allowance-program participants, HAO records (Table 5.4) provide us with a complete account of their residential distribution at any point in time and of their moves over time *(M)*, as well as considerable information about participants and their housing, which may partly explain *(E)* observed mobility or lack of it. And a subset of these records can be linked to the four panel surveys (Table 5.5), providing a bridge to broader analyses of marketwide mobility patterns.

In the latter table, the survey of tenants and homeowners includes a mobility retrospective to capture preallowance residential choices and mobility data *(M)* for all occupants of monitored structures, some of whom will (after baseline) enroll in the allowance program. We will seek to explain *(E)* their behavior by a coordinate history of household characteristics, income, and employment, and neighborhood characteristics of their successive residences. Their current housing and neighborhood circumstances, of course, play a role in explaining both their most recent moves and their next moves, the latter to be observed in subsequent survey cycles.

Table 5.7 describes the tentative contents of a survey instrument to be administered in postbaseline years to a sample of households moving from monitored housing units to other local addresses. In addition, those who replace these movers in our panel of monitored housing units will provide mobility histories for both preallowance and postallowance years.

Analysis of Effects on Nonparticipants

Although our research charter indicates a special interest in the effects of the allowance program on nonparticipants' housing circumstances and on their attitudes toward the allowance program, we (and, presumably, HUD) have an equal interest in these same issues in the case of program participants. In general, program effects on nonparticipants will be mediated or reflected by supply response in relevant market sectors; by the terms and policies of market intermediaries with whom nonparticipants must deal; and by allowance-induced mobility and related changes in patterns of residential location. Those issues can best be analyzed (for both participants and nonparticipants) by disaggregation of data already discussed under those headings. The most distinctive element of this portion of our charter is the interest in attitudes toward the allowance program.

Entries in the last column of Table 5.4 reflect our presumption that the characteristics, housing and locational choices, and housing expenditures of allowance-program participants will help to explain (*E*) both the effects of the program on the housing circumstances of nonparticipants and nonparticipants' attitudes toward the program. In Table 5.5, we have endeavored, awkwardly, to reflect the double role of most of the panel survey data, as measures (*M*) and as explanations (*E*) of either effects or attitudes.

Table 5.8 details the tentative contents of modules to be added in later years to the household survey instrument (survey of tenants and homeowners) whose baseline contents are described in Table 5.5. Here, we have divided respondents into three classes: program participants, eligible nonparticipants, and ineligible nonparticipants.

For each group we expect to probe for attitudes toward the allowance program *(M)* after inquiring into factors *(E)* that we expect will have a bearing on attitude formation. These factors, of course, include material covered by the "regular" modules of the instrument, described in Table 5.5.*

Summary

We suspect that, for most readers, the preceding sketch of relationships between our data files and the analyses we plan to conduct raises nearly as many questions as it resolves--partly because the contents of each file are so laconically described, and partly because the research topics and methods of analysis are so briefly treated. In constructing Tables 5.4 to 5.8, we were not infrequently forced to make arguable decisions--whether on balance a particular kind of data could more intelligibly be represented as measures of effects *(M)*, or as explanatory variables *(E)*, or as both. We suggest that individual entries in these tables should not be taken too seriously; our purpose here is only to show *generally* how our various sources of data will be brought together in analysis.

Sections VI through IX, following, describe in more detail the analysis plans for each major research topic, elaborating on the specific questions to be addressed, on the data pertinent to each, and on the appropriate analytical methods. Readers of these sections may perhaps profit by referring back to the tables and figures in this section and in Sec. IV to confirm whether provision has been made for gathering and storing the necessary data, whether appropriate links can be made between data for different entities, data from different sources, and data gathered at different points in time, and whether the sampling frames and sample sizes are appropriate to the issues.

---

*We assume here that our panel survey of tenants and homeowners will include enough members of each group to provide an adequate sample for attitude analysis. As noted in Sec. IV, we may find it expedient in postbaseline years to separate attitude surveys from the panel survey of tenants and homeowners, or to supplement the panel sample where it is thin.

VI. ANALYZING SUPPLY RESPONSE TO HOUSING ALLOWANCES

POLICY ISSUES AND RESEARCH QUESTIONS

As its name suggests, a principal aim of the Supply Experiment is to determine how the suppliers of housing services respond to an increase in effective demand--specifically, to an increase in the effective demands of low-income households brought about by conferring on them some form of housing allowance.

The purpose of such an allowance would be to enable low-income households to consume a larger quantity of housing services without decreasing their consumption of other commodities. At least from the perspective of housing objectives, the overall measure of program efficiency would be the increase in the quantity of housing services consumed per dollar of housing allowance. Maximum efficiency would be achieved if (1) housing expenditures increased by the full amount of the allowance, and (2) there were no increase in the price per unit of housing services.

Finding a way to approach the first type of efficiency goal is the central purpose of the Demand Experiment, which will test various allowance formulas and various earmarking provisions to discover how housing expenditures change as a result of the allowance. The Supply Experiment is concerned with the second goal. Many observers, including some who support the principle of subsidized housing for low-income families, have argued that a national program of housing allowances would be an inefficient, perhaps even ineffective, means of achieving better housing for the recipients--that increased spending by low-income families for housing services would force the price of these services upward, so that the benefits conferred on the assisted families would be in part or wholly wasted. Moreover, if such price increases also affected the housing services purchased by unsubsidized families, whatever gains were made by the assisted low-income families would be substantially at the expense of the unsubsidized families--particularly those whose incomes were just above the level of eligibility for housing allowances.

To address these concerns, the Supply Experiment should be designed

to yield reliable estimates of the changes over time in the prices and quantities of housing services consumed by both subsidized and unsubsidized households in an allowance-stimulated market.* Manifestly, these changes will reflect characteristics of the allowance program that serves as a stimulus to the market: the number of participating households, their characteristics, the amounts of their allowances, and the restrictions imposed on their use of the allowances. But whatever the details of the allowance program, its pertinent consequence will be a known increase in housing expenditures within the experimental site. Our task is to determine how these increased expenditures translate into price changes and changes in the quantity of housing services provided by the market.

It is obviously necessary to choose a specific allowance program for the Supply Experiment. It would be preferable, but obviously impossible, to choose the as-yet-undesigned national allowance program. In lieu of that inaccessible alternative, we propose one which would be plausible in the light of the objectives of a national housing allowance program and which also has desirable characteristics from an experimental point of view. Chief among the latter is that the proposed experimental allowance program is designed to cause a substantial increase in housing expenditures by a large number of recipient households, both renters and owners.**

Our measurement objectives are twofold: First, we want to measure the price and quantity changes that actually occur at our experimental sites, distinguishing as well as we can between changes attributable to the experimental allowance program and changes attributable to background events that are independent of the allowance program. Second, we want to estimate typical responses of suppliers in different sectors of the market to changes in demand from whatever source. These

---

*Our general analysis of supply response, presented in this section, relates both to allowance recipients and to nonrecipients. Effects of the allowance program on nonrecipients are also discussed in Sec. IX.

**See Sec. III for a description of the experimental allowance program.

response parameters can then be linked analytically to other configurations of demand changes, in order to estimate the supply responses at our experimental sites to allowance programs that differ from the one actually employed there; and also to estimate supply responses to specified demand stimuli in housing markets other than our experimental sites.

We think that the first of these two objectives has prior importance. Unless we are able to provide a convincing account of supply response at the experimental sites, analytical generalizations from our data will carry little weight. The latter, in any case, are subject to important qualifications, discussed in Sec. X.

The analysis plan described below, therefore, addresses four responses to events at our two experimental sites:

1. The amount by which the supply of housing services increases following the introduction of the housing allowance program.
2. The amount by which the average price per unit of housing services increases following the introduction of the housing allowance program.
3. The extent to which these changes are attributable to the allowance program, as distinguished from other factors.
4. The response of suppliers in different sectors of the market to changes in the demand for housing services in that sector.

THE EXPERIMENTAL STIMULUS

The experimental housing allowance program described in Sec. III will alter the housing demands of recipients by (a) increasing their incomes and (b) compelling most of them to increase their consumption of housing services. From administrative records of the allowance program, we will be able to determine with precision how much is disbursed in the form of housing allowances. Enrollees will also be required to report their housing expenditures at the time of enrollment, and periodically thereafter. Thus, except for misreporting, we will be able to determine how much the allowance program has directly added to aggregate housing expenditures within the experimental site.

Housing expenditures by those not enrolled in the allowance program may also change during the course of the experiment. Our program of household surveys will enable us to estimate reasonably well the aggregate changes in the numbers of nonrecipient households and the aggregate changes in housing expenditures by nonrecipients. These estimates will not be as accurate as those for recipients because they will be based on sample data, and sampling rates in some sectors of the housing market will be low; on the other hand, we have no a priori reason to expect substantial changes in nonrecipient expenditures.

Thus, from allowance-program records and from our sample survey of households, we will be able to estimate the aggregate change in housing expenditures within the experimental site at annual intervals following introduction of the housing allowance program; we will also be able to decompose this total into the amount attributable to allowance recipients and the amount attributable to nonrecipients.

These observed changes in housing expenditures are not the same as changes in the demand for housing services. Even with no change in demand, events on the supply side of the market could result in a change in the price of housing services that would in turn lead consumers to spend either more or less for housing. The distinction between a change in expenditure and a change in demand is important when we attempt the analytical integration of data from the Supply and Demand Experiments. But for present purposes we do not need to know whose demand schedules have shifted, or why; we only need to know by how much housing expenditures have changed, and what part of the total change is directly attributable to allowance recipients.

MEASURING SUPPLY RESPONSE TO A CHANGE IN HOUSING EXPENDITURES

The central technical problem of measuring supply response is to find a way to disentangle changes in the price of housing services from changes in the delivered quantity of such services. Even in a controlled experimental context, the only readily observable magnitude pertinent to this problem is the change in housing expenditures. By definition,

$$R = PQ \ , \tag{6.1}$$

where R = housing expenditures (e.g., rental payments);

P = price per unit of housing services; and

Q = number of units of housing services delivered.

An increase in housing expenditures is an expectable and intended consequence of a housing allowance program and does not in itself cast any light on whether housing consumption or housing prices have increased. However, if we know $\Delta R/R$ and can find a way to measure $\Delta Q/Q$ directly, then $\Delta P/P$ can be observed as a residual.

Alternative Measures of Quantity Changes

There are three general approaches to the measurement of $\Delta Q/Q$, each suffering from both conceptual and practical difficulties:

1. Observing changes in the quantities of physical inputs to the production of housing services.
2. Observing changes in the quantities of physical outputs from this production process.
3. Observing changes in tenants' satisfaction with the housing services they consume.

The second of these methods would fit most neatly into the machinery of market analysis, which is designed around the concept that the output of a production process is a tangible physical commodity which can be divided into homogeneous, easily counted units. Housing services do not fit this description very well; although we can easily count the number of separate living accommodations, the number of separate rooms, or even the number of square feet of floor space in a housing inventory, these measures clearly do not capture all that we mean by "housing services." Obviously, a tenant would not be indifferent between two apartments that were identical in these respects but differed in design, decoration, level of maintenance, building services provided, or location.

At the very least, measuring changes in the output of housing services would require a multidimensional scorecard, one that would almost certainly be incomplete in the number of dimensions. Furthermore, even among well-defined dimensions, some cannot readily be measured on a

cardinal scale (e.g., "amount" of interior decoration), and there is no readily available weighting scheme to aggregate changes along the various dimensions. At best, one could select a subset of housing characteristics that were readily measurable on cardinal scales and assume that the remainder were collinear with those measured.

Indeed, any serious attempt to measure the flow of housing services leads inevitably to either the first or third approach. Thus, counting housing units or rooms is really counting the structural capital which contributes, along with other physical inputs, to the current flow of services. Attempting to aggregate across output dimensions leads to weighting schemes (such as hedonic indexes) based on consumer preferences expressed in the marketplace. For our purposes, such weighting schemes have the conceptual defect that there are alternative sets of weights, depending on whose preferences are being considered, and the operational defect that construction of hedonic indexes that are sensitive enough to distinguish any except gross changes in price from concurrent changes in the flow of housing services has never been achieved and may not be achievable.

The third approach, directly measuring consumer satisfaction, suffers from several difficulties. To be sure, tenants may be interviewed and asked whether their housing has improved, either along specific dimensions, or in general. When the same tenant living in the same housing unit responds differently at two successive interviews, that is prima facie evidence of an ordinal change in his housing conditions. It would be much more difficult to devise an interview technique that yielded the cardinal measures ("How *much* has your housing improved?") that would be needed for analysis of any subtlety; and given the diversity of tastes among consumers, the commensurability of their responses (e.g., responses of successive tenants of a given housing unit) is very much in doubt.

For the Supply Experiment, we propose measurements both of changes in the physical characteristics of our sample of residential structures and of changes in the housing satisfaction of their tenants. Prior to the commencement of the allowance program, each residential structure in the sample will be surveyed; its basic physical characteristics,

appliances and equipment, and interior and exterior condition will be recorded by a combination of direct observation and tenant interviews; annual resurveys will record the same information by the same methods. Similarly, tenant attitudes toward the housing unit, the structure, the landlord, and the neighborhood will be recorded at baseline and annually thereafter.

While these data should enable us to detect changes in specific features of a structure or dwelling unit, they do not readily combine into an overall measure of the change in total quantity of housing services delivered by the structure or unit. Without such an overall measure, it is obviously difficult to reach unequivocal conclusions about accompanying price changes except under special circumstances.* For that purpose, we must turn to the first approach suggested above, observing changes in the quantities of factors used in the production of housing services.

Measuring Changes in Factor Inputs

This approach has several advantages. First, we can come much closer to comprehensive measurement of inputs than of outputs or of consumer satisfaction. Second, cardinal measurements are possible. Third, different kinds of factor inputs can be combined into a single aggregate measure by means of a deflated-price weighting scheme whose properties are well understood. Finally, if measuring changes in inputs does not tell us precisely how much output changed, it at least tells us how hard the producers tried to change outputs.

Briefly, the measurement scheme would work as follows:

1. Inventory all expenditures (explicit and implicit) for factor inputs during the year preceding the beginning of housing allowance payments (V).

*Where annual resurveys show only trivial changes in physical characteristics, appliances and equipment, and interior and exterior condition, a change in rent is manifestly a change in price. But we have no assurance that this special case will occur with enough frequency to serve as a basis for statistical inference.

2. During the experiment, record changes in expenditures for factor inputs measured in current dollars, cumulating these over a comparable time period, and deflating the totals to base-year dollars by factor-specific price indexes (ΔV).
3. Calculate the ratio of the change to the base ($\Delta V/V$); take this change as a measure of the change in output ($\Delta Q/Q = \Delta V/V$).
4. Record rental revenue (R) for the year preceding the beginning of allowance payments.
5. During the experiment, record annual changes in rental revenue (ΔR).
6. Using the relationship

$$\frac{\Delta R}{R} = \frac{\Delta P}{P} + \frac{\Delta Q}{Q} + \frac{\Delta P \Delta Q}{PQ} , \qquad (6.2)$$

estimate the relative change in the price of a unit of housing services as a residual.

An alternative to Step 2 would be to reinventory all factor inputs and derive the change by differencing against the base-year inventory. The objection to this alternative is that major items in base-year factor expenditures are the annual costs of land and structural improvements, i.e., their market values multiplied by the current market rate of interest. These market values may change in the course of the experiment for various reasons, such as a shift in the demand for housing services, and we have no price index to deflate them. Furthermore, differencing two magnitudes neither of which can be measured with great precision yields an extremely imprecise estimate of the change if the difference is small relative to the base.

On the other hand, it appears to us that nearly all the significant changes in capital and current inputs can be captured by event recording. These changes include land and existing buildings converted to or withdrawn from residential use, valued at base-year appraisals; new construction and alterations to existing structures, valued at cost of production and deflated to baseline values by a construction cost

index; and current maintenance and operating outlays, deflated by factor-specific cost indexes. The explicit procedures are detailed in Appendix B.

The Relationship Between Input Changes and Output Changes

The most serious objection to our procedure is the possibility that it may overestimate changes in the quantity of housing services produced and underestimate changes in the price of those services. The reasoning behind this objection is simple and forceful: For many production processes in which output is readily and directly measurable, a short-run change in the level of output can be shown to reduce the technical efficiency of the production process. Contrary to our assumption above, the change in output is less than proportional to the change in factor inputs ($\Delta Q/Q < \Delta V/V$).

If this rule applies to the production of housing services, we would like to know about it, yet it cannot be directly tested without an independent, reliable measure of the physical quantity of output--which, we have argued, is inaccessible. But all is not lost. We have devised a method for analyzing changes in factor inputs and rental revenues for individual buildings that yields an estimate of the typical elasticity of output to changes in inputs that occur during the experiment.* The method relies in part on the existence of variations among buildings with respect to factor-input changes, and in part on the assumption that both factor prices and output prices are more or less uniform throughout the universe of buildings covered by the analysis.

If this analysis succeeds, its findings can be used to adjust our estimates of relative changes in the price and quantity of housing services delivered during the course of the experiment. We suspect, however, that the initial circumstances of the housing stock in the modular neighborhoods are such that our assumption ($\Delta Q/Q = \Delta V/V$) will be approximately correct.**

---

*See Appendix C.

**Our reasoning is presented in Appendix B.

Estimating Changes in the Price of Housing Services

Having directly observed relative changes in housing expenditures ($\Delta R/R$), and having estimated relative changes in the quantity of housing services supplied to the market ($\Delta Q/Q$) by the procedure described above, it is then a simple matter to calculate the relative change in the price of a unit of housing services between the base and test years; rearranging Eq. (6.2), we have

$$\frac{\Delta P}{P} = \left(\frac{\Delta R}{R} - \frac{\Delta Q}{Q}\right)\left(1 + \frac{\Delta Q}{Q}\right)^{-1}. \qquad (6.3)$$

The Unit of Analysis

The analytical plan described above relies on measurements taken on a sample of individual residential properties. For each such property, our annual field surveys will compile the data needed to estimate annual rental revenue for rental structures and annual housing expenditures by homeowners.* For each such property, these surveys will also provide the data needed to estimate the quantity of factor inputs required to supply these housing services. The calculations summarized in Eqs. (6.1), (6.2), and (6.3) can thus be performed for each of the structures in our sample, leading to estimates for each structure of the relative changes in rental revenue for that structure, in the quantity of housing services it supplies, and in the price per unit of those services.

We do not expect these changes to be uniform throughout the sample. Different sectors of the housing stock will be differently affected by the allowance program, and the owners of individual buildings will respond differently to market signals which they may also perceive differently. Rental revenues in some structures may increase, while in others they decrease. Some landlords may improve their buildings,

---

*The analysis in the case of homeowners differs in important respects from that for rental property. The differences are discussed later in this section.

others may withdraw them from the market.* These differences--in impact, by market sector, and in response, by characteristics of structure or of landlord--are of considerable interest and are discussed later in this section. Here, however, we are concerned with the overall effects of the housing allowance program on the flow and price of housing services in the experimental sites.

To estimate the aggregate change in the supply of housing services between baseline and each annual survey cycle, we must generalize from our sample. Although the sampling rate will vary by market sector, it will be known for each sector, and we can attach weights to each observation reflecting its sampling rate. Summing the weighted observations of revenues and factor inputs across the sample, we can estimate interperiod changes in total housing expenditures and decompose them into changes in quantity and changes in price, using the same technique as for an individual property.

As noted in Sec. IV, our baseline survey will include a sample of parcels of urban land not then in residential use; if some of these parcels are subsequently converted to residential use, our annual surveys will capture them as additions to the supply of housing. Outside the urbanized area, we propose to sample residential building permits annually to estimate additions to supply from new construction. Also, among our original panel of residential structures, some will be withdrawn from residential use in the course of the experiment; these events too will be captured in our annual surveys.

Thus, within the limits of sampling error, we expect to be able to estimate aggregate changes in the flow of housing services and average changes in the price per unit of housing services. If we succeed in this task, the remaining problem is to determine what parts of these changes are attributable to the housing allowance program and what parts to other factors.

---

*For a scenario of market response, see Lowry, Rydell, and de Ferranti, op. cit., WN-7711-UI, Sec. IV.

Decomposing Changes in Housing Expenditures

Appendix B presents a method for decomposing an observed change in housing expenditures into seven component parts. We are able to identify shares of the total increase in housing expenditures (or rental revenues) attributable to changes in

a. Real-estate taxes;
b. The quantity of factor inputs;
c. The market rate of interest;
d. Prices of capital-improvement inputs;
e. Prices of maintenance and repair inputs;
f. Prices of building-service inputs; and
g. Producers' markup on factor costs.

This decomposition is an extremely important part of the analysis of the results of the Supply Experiment. It enables us to distinguish the share of the change in total expenditures that reflects a change in the flow of housing services (or at least of real factor inputs) from the share that reflects only price changes; and among the components of price change, it enables us to distinguish those that benefit the supplier from those that are simply passed on by him to the consumer.

Thus, the direct beneficiary of Share (a) is the municipal fisc; in a national program of housing allowances, the resulting increase in real-estate taxes would be considerable--on the order of 15 to 25 percent of the increase in housing expenditures--unless effective tax rates on market value were lowered.

Share (b) is the only direct benefit that accrues to tenants. Strictly speaking, it is the increase in the quantity of resources used to produce housing services; subject to the qualifications presented earlier, it also measures the increase in the flow of housing services during the course of the experiment.

Share (c) in our accounting scheme accrues to the owners of residential property until such time as they refinance their mortgages or incur new indebtedness. Changes in the market rate of interest may be either positive or negative, depending more on events in a much broader

capital market than on the housing allowance experiment. Under a national program of housing allowances, it is possible that increased demand for mortgage funds would drive up the market interest rate. The experiment will enable us to estimate the magnitude of this increased demand for mortgage funds; others, versed in the lore of national money markets, could do better at estimating the effects on the interest rate.

Shares (d), (e), and (f) benefit neither the producer nor the consumer of housing services. Depending on the scale of our experiment, these price changes may be purely extraneous events--"background inflation" in response to regional or national influences--or they may be partly caused by the experiment itself.

Our a priori calculations lend little support to the notion that the increased factor demand due to the experimental allowance program will be large enough to influence factor prices perceptibly, even though the allowance program embraces an entire metropolitan area. However, we propose to monitor local factor prices and compare their changes with corresponding changes reported in regional and national indexes.*

Finally, Share (g) is a residual, the difference between the change in rental revenues and the change in the cost of production of the housing services delivered. This residual, which may be either positive or negative, accrues to the owners of residential property. Under conditions of increased demand, we would expect Share (g) to be positive. With qualifications, it may be described as *the payment needed to persuade producers to increase factor inputs by the observed amount.*

Overall, the responsiveness of housing supply to the observed increase in housing expenditures can be judged simply by the magnitude of Share (b)--i.e., by the percentage of the incremental housing expenditure that went for real increments of output. A more discriminating judgment, however, would certaintly net out background inflation and fiscal recapture through real-estate taxes--leaving only demand-caused inflation in factor prices and changes in owners' profits as relevant inefficiencies.

---

*See Appendix D for details.

Netting Out Background Effects

Above, we have discussed a logically tight method for decomposing the observed changes in housing expenditures into shares attributable to real-estate taxes, payments for increased housing services, several kinds of factor-price changes, and changes in the producer's markup on his costs. This decomposition brings us considerably closer to an understanding of the supply response to the experimental allowance program, but there remains a problem of imputing observed price changes to specific causes. This cannot be done with precision, but we think it can be done adequately for our purposes, unless the experiment is conducted in a very unstable market environment.

Items (c) through (f) of our decomposition are factor-price changes--changes in interest rates and in the costs of capital improvements, maintenance and repairs, and building services. Although such changes are most likely to reflect market forces on a regional or national level, they could also result from local forces: an allowance-induced increase in demand for housing services, changes in housing demand by nonrecipients, or even in some cases, competing demands by other users of these factors of production (e.g., commercial construction). Item (g), the housing producer's markup on factor costs, is unlikely to be affected by forces other than those in the local housing market, allowance-induced and otherwise.

Accounting for nonlocal inflationary forces is easiest. We plan to construct local price indexes for factors used to produce housing services; these can be compared to corresponding price indexes for the region within which our experimental site is located.* We seriously doubt that after discounting the nonlocal effects there will be much left in the way of factor-price changes to account for, unless the experimental housing allowance program coincides with a local building boom.

Sorting out allowance-induced and other local demand pressures on factor prices and (more important) on producers' markups is more complicated. The experimental allowance program is a deliberate demand

*See Appendix D.

stimulus, controlled in amount and measurable in its effects on housing expenditures. As noted earlier, our monitoring program will provide us with the basis for fairly good estimates of changes in nonrecipient housing expenditures, especially in those sectors of the housing market where nonrecipients compete with recipients. We will also be able to estimate changes for nonrecipients in the principal factors affecting housing demand, i.e., numbers of households and household incomes.

The influence of these independent* changes on the demand for housing services within the experimental site can be estimated, provided that the price and income elasticity of the demand for housing is known. Similarly, the supply response to these nonrecipient demand changes can be estimated if the price elasticity of supply and the amount of any exogenous factor-price changes is known.

In principle, the necessary data and parameters will be available to calculate the price and quantity of housing services that would have cleared the market in the absence of the allowance program. Our monitoring program measures the forces affecting demand and also the nonlocal factor-price changes. The Demand Experiment is designed to provide estimates of the income and price elasticities of the demand for housing services. As described later in this section, the Supply Experiment is designed to provide estimates of the price elasticity of the supply of housing services. The mathematics of the general solution to our problem are developed in Appendix E.

Though the data needed for such an analysis are all scheduled to be produced in some form within the general framework of the Experimental Housing Assistance Program, their uneven quality and the complexity of the implied econometric modeling do not encourage us to think of this procedure as a means of netting out second-order background effects. If our annual surveys reveal only minor changes in the numbers and incomes of nonrecipient households, we doubt that going

---

*Changes in the number and incomes of nonrecipient households will not be entirely independent of the allowance program. Between allowance payments and local expenditures for monitoring, the experiment will pump $5 million to $10 million annually into the local economy.

through such an analysis would add much to our understanding of the effects of the experimental allowance program on the local housing market.

At the other extreme, there are possibilities of major shifts in nonrecipient housing demand due to rapid growth or decline in the local population or drastic changes in income, events that might occur because of a severe dislocation in the metropolitan economy. Should such dramatic events occur, producing a change in nonrecipient housing demand that is large relative to the allowance-induced change, we doubt that any formal analysis would salvage a credible interpretation of the effect of the allowance program on the local housing market.

The useful analytical possibilities lie between these extremes, for experimental outcomes in which changes in nonrecipients' housing demands appear to be comparable in magnitude to the changes induced by the allowance program. We believe that our monitoring program will provide us with the data needed to assess the approximate responsibility of the allowance program for observed increases in the price of housing services and in the prices of the factors used to produce those services.

Is Vacant Housing Part of the Supply?

At any given time, some housing units within our experimental site will be unoccupied--probably 3 to 7 percent of the total. Over time, the number of vacancies and the particular units that are vacant will change. Our plan for measuring changes in the flow and price of housing services must deal with these vacant units: Should they be counted as emitting housing services? If so, who pays for these services?

It is possible to construct alternative models of a housing market that handle these issues differently. We think that the purpose of the present analysis--measuring the marketwide supply response to a permanent change in housing demand--is best served by including vacant units as part of the supply, so long as they are available for sale or rent. And in calculating the price of housing services, we propose to count only actual receipts from the sale of housing services, net of vacancy and collection losses.*

*The special problems of pricing owner-occupied units are discussed later in this section. The discussion that follows presents the issues in terms of rental housing.

This system of accounts is designed to reflect the supplier's rather than the consumer's view of the housing market. A tenant pays a contract rent, in return for which he receives a bundle of services and amenities. His view of the price of housing services is formed by comparing the rent he pays to his perception of the "size" of that bundle, its elements weighted in terms of his preferences.*

The landlord, however, measures price differently. He receives a flow of rental revenue in return for providing a bundle of factor inputs. The amount of revenue he receives depends not only on contract rent, but on vacancy rates and collection losses. His unit of operation is the structure, which may contain more than one housing unit. In the event of vacancies or delayed rent payments, he can make only minor adjustments in his inputs, even for a single-unit structure; for a multiple dwelling, he is even more tightly constrained. To him, inputs are commensurable in terms of the prices he must pay for them.

We argue then that the landlord's supply decisions are motivated not by contract rent but by expected net rental revenue, allowing for vacancy and collection losses; and that supply decisions are decisions to increase or decrease factor inputs, taking into account their prices. Presumably, the landlord believes that a judiciously chosen mixture of added inputs will enhance the marketability of his property, leading to an increase in net rental revenue, either through higher occupancy rates or higher rents, or both. His calculation at the margin is a comparison of the expected increase in net rental revenue with the expected increase in total factor cost.

If these are the terms in which supply decisions are made, it seems to us that these are the appropriate measuring sticks for supply responsiveness. Our proposed data-gathering plan and analysis scheme follow this principle, except that we must settle for after-the-fact

---

*Note, however, that vacant units in a housing market are a floating benefit to tenants, providing accessible alternatives to their present quarters, alternatives to be used in the event of dissatisfaction with their housing, disagreements with their landlords, changes in income or family composition, etc. Over the long run, moreover, the tenants pay for this benefit: The total housing inventory, including vacancies, is supported by the revenues from occupied units.

observations of his revenue and his inputs, as distinguished from his ex ante expectations.*

The scheme summarized earlier for measuring changes in the quantity of housing services is, as we have made clear, really a scheme for measuring the quantity of factors used to produce these services. For vacant units, some factor inputs are discontinued; but unless an entire structure is vacant, the owner must continue nearly all of his outlays because of their indivisibility. Our measurement scheme would capture any reduction in outlays associated with vacancies but would not distinguish inputs to vacant units from inputs to occupied units. If a vacant building was withdrawn from the market--by boarding up, manifest abandonment, condemnation, or demolition--we would cease to count it as part of the housing supply.

For the housing market as a whole, an increase in the vacancy rate would thus imply no more than a small decrease in the quantity of housing services supplied. The decrease in rental revenue would be greater, relative to its base, than the decrease in quantity. It follows, therefore, that a rising vacancy rate, in our accounting scheme, would be accompanied by a decrease in the average unit price of housing services even if rents for occupied units were unchanged. By the same token, a declining vacancy rate would be accompanied by an increase in the average unit price of housing services.

Both the theory of markets and common observation lead us to expect that such changes in vacancy rates would also influence the rents for occupied units. When the vacancy rate rises substantially, landlords are usually compelled to offer rent concessions to hold the tenants they have; when the vacancy rate falls to a low level, they can usually raise rents. Thus, the short-run workings of the market would intensify the direct effects on the average unit price of housing services noted in the preceding paragraph. Over the longer run, however, producers should respond to price changes by altering output in more fundamental ways: withdrawing housing from the market if prices are

---

*Quite possibly, we will find that our analysis is improved by comparing response variables for Period 2 with stimulus variables for Period 1, etc.

too low, adding to the stock if they are profitably high. In the absence of further disturbances, both the price of housing services and the vacancy rate should in time return to "normal" levels.

Thus a market equilibrium--which we expect will be approached within five years after the introduction of the allowance program, barring major exogenous disturbances--is not defined as a circumstance in which all housing units are occupied, or one in which all families are housed, but rather as a level of revenues from occupied housing units such that suppliers are content to neither increase nor decrease the stock of housing units or the level of their current inputs. We would expect the equilibrium vacancy rate to be greater than zero.

Many housing analysts find it useful to view a metropolitan housing market as a set of overlapping submarkets, within each of which special forces affect supply and demand relationships, but always with some spillover into other submarkets. Thus, vacancy rates are seldom uniform across the housing market but vary by neighborhood, rent level, type of structure, etc.

We expect to find such variations within our experimental sites, and we expect the impact of the allowance program also to be uneven. We can enrich our understanding of market dynamics by disaggregating our housing accounts by neighborhood or market sector and relating the observed changes in housing prices and quantities to the initial and subsequent distribution of allowance recipients among these submarkets.*

Price and Quantity Changes for Owner-Occupied Housing

The analysis plan described in the preceding pages has emphasized supply response in the rental housing market, where tenants and landlords are distinct persons. However, our allowance program also includes assistance to owner-occupants, cases in which the consumers of housing services are also the producers. Here, we explain how our data-gathering plans and methods of analysis deal with this situation.

---

*See Sec. VIII for an account of our plans for analyzing the movements of allowance recipients and others within the experimental site.

Our sample of residential structures will include owner-occupied housing units, usually single-family homes but sometimes units in multiple dwellings with resident owners. We propose to treat the latter as rental properties, gathering data on the value of factor inputs for the structure as a whole and on rental revenues from each unit; for the owner-occupied unit, we will accept the owner's estimate of its market rental value. Usually, this estimate should be accurate, since his unit is likely to resemble the rental units in the structure.

Owner-occupied single-family houses are a different matter. Our sample will include both allowance recipients and nonrecipients who are homeowners. How the former respond to housing allowances is indeed an interesting question, but it is more nearly a demand question than a supply question. In the absence of factor-price changes or an increase in real-estate taxes, any increase in housing expenditures by an owner-occupant will be exactly matched by an increase in the flow of housing services as defined by our measurement system. In other words, unlike the case of rental housing, there is no producer's markup entailed in the supply decision of the owner-occupant, and it is this markup which we perceive to be the element of housing price change that is most pertinent to the policy issues of supply response to housing allowances.

Our techniques for measuring changes in factor inputs over time are equally applicable to rental and owner-occupied structures, so for our sample of the latter we will be able to measure changes in the flow of housing services. But for owner-occupied single-family houses, there is no continuous market test of value that corresponds to rental revenue. Rather, we are restricted to sporadic measures, on the occasions when such properties are sold. Then, we are able to identify something corresponding to producer's markup, i.e., capital gain or loss. For properties that change hands after baseline, we can compare the sales price with the baseline appraisal of market value to determine the interim change in the market value of the property. To the extent that this amount differs from the value of recorded capital

improvements less depreciation over the same period, we can conclude that the price of owner-occupied housing services has changed.*

We anticipate that about 10 percent of the owner-occupied single-family homes in our sample will change hands annually; over the five-year monitoring period, perhaps 40 percent will have been sold at least once. With the contemplated sample size,** these transaction rates imply somewhere between 100 and 150 observed sales within our sample during the five-year monitoring period--enough to support some general conclusions about price changes, but too few for much disaggregation.

In principle, these data will enable us to construct a price index for owner-occupied homes, using a variation of the Wyngarten method.*** For every such property sold during the course of the experiment, we will have data on (1) acquisition price, (2) baseline appraised value, and (3) subsequent sales price. Adjusting between baseline and subsequent sale for capital improvements and depreciation, we can construct price-relatives for each case, and these in turn readily combine into a general price index for the group, or for its subsets.

With less accuracy, this method can be extended to owner-occupied housing not in our sample of monitored properties that that is sold twice during the term of the experiment, and this may prove to be a desirable special study. Accuracy will be lower outside of our sample because we will lack evidence of capital improvements other than those recorded by alterations permits issued by local authorities; in addition, we will have less evidence on which to sort out "arm's length" transactions from other types of formal conveyances for which recorded prices are misleading indicators of market value.

---

*To be sure, we could periodically reappraise owner-occupied single-family homes, using the same techniques as for the baseline appraisal; but the major variable in such a reappraisal would be the sales price of the subject property or similar properties, so that going through the motions of reappraisal really adds little more to our knowledge than we have in any case from observations of sales transactions.

**Not yet finally determined.

***Herman Wyngarten, "An Index of Local Real Estate Prices," *Michigan Business Studies*, University of Michigan, January 1972. For a more complete explanation of the method, see David M. Blank, "Relationship Between an Index of Housing Prices and Building Costs," *Journal of the American Statistical Association*, Vol. 49, 1956, pp. 67-68.

Transactions in owner-occupied single-family houses can take a variety of forms. Of greatest interest in an allowance-stimulated market are those involving speculative intermediaries--individuals or firms who buy single-family houses, improve them to program standards, and resell them to allowance recipients. Doubtless, our sample of structures will include some cases of this sort, but we will have little difficulty in identifying all such cases through records of the allowance program. Section VII discusses our plans for retrospective analysis of these cases.

ESTIMATING PRICE ELASTICITIES OF SUPPLY

So far in this section, we have explained our plans for measuring the changes in the price and quantity of housing services that occur at our experimental sites, distinguishing as well as we can between changes attributable to the experimental allowance program and changes attributable to background events that are independent of the allowance program. We believe that the techniques we have proposed will enable us to document convincingly the policy-relevant consequences of the experimental housing allowance program at each site, barring other major disturbances of the housing market.

To the extent that experimental findings affect policy decisions concerning a national housing allowance program, we think that this level of reporting will be the most important. However, it does not exhaust the analytical possibilities of our data. Whereas our measures of price and quantity changes tell us directly what happened as a consequence of a specific change in low-income housing demand, they also can be used to estimate what would have happened in response to other untested demand changes.

The analytical machinery needed for such estimates is complicated and relies on data from the Demand Experiment as well as the Supply Experiment. It is discussed in Sec. X of this report, with a mathematical elaboration in Appendix E. For present purposes, we need only note that the critical ingredients from the Demand Experiment are estimates of the price and income elasticities of the demand for housing services, specific to categories of households that are readily identifiable from

commonly available data. From the Supply Experiment, the critical ingredients are estimates of the price elasticity of the supply of housing services, specific to sectors of the housing market that are likewise readily identifiable from commonly available data. In other words, findings from both experiments must be reduced to "portable" parameters, applicable to housing markets that differ in important respects from those in which the data were obtained.

Defining the Price Elasticity of Supply

The formal definition of the price elasticity of supply for an individual producer of a commodity is: *The amount by which he would be willing to change his output, given a specified small change in the price at which he expected to be able to sell his product, each change expressed as a fraction of its current base.* As a ratio of two ratios, it is easier to grasp in algebraic form than in prose:

$$\varepsilon(S,P) = \frac{\Delta S/S}{\Delta P/P}, \qquad (6.4)$$

where $\varepsilon(S,P)$ = the price elasticity of supply;
S = the quantity of output per unit period of time; and
P = the expected price per unit of quantity.

Several things should be noted about this definition. First, it describes a hypothetical response to a hypothetical circumstance. Second, it presumes that the only price change taken into account by the producer is a change in the price of output; factor prices are assumed to be unchanged. Furthermore, there is an implicit time horizon entailed in the measure; the producer's willingness to alter his rate of output depends on when he expects the price to change and how long he expects the change to persist. Finally, there is no a priori reason to assume that $\varepsilon(S,P)$ is a single-valued function; it is more likely to vary with the values of S and P, and there is also likely to be some threshold value of ΔP below which ΔS is zero.

The definition given above was adopted by economists because of its analytical rather than its empirical convenience. All attempts

to measure supply elasticity with which we are familiar fail in one or more respects to meet the rigorous requirements of this definition. This does not make empirical measures valueless; it does mean that if they are used in conjunction with standard market models, due consideration must be given to the ways in which the empirical measure differs from the concept with which it is identified.

Measuring Price Elasticity in the Supply Experiment

As explained earlier in this section, our annual surveys of residential structures will provide data from which we can estimate for each structure separately the annual change in real factor inputs and the annual change in the revenue received by the producer for the resulting housing services. We also show how the revenue change can be decomposed into components reflecting changes in real-estate taxes, changes in the flow of housing services, changes in factor prices, and changes in the producer's markup.

From these data, we can calculate a variety of price elasticities, each with special analytical properties but all based on ex post observation of the outcome of the producer's decision rather than ex ante observation of his assumptions about the future. In each case, the numerator of the measure is the same: the relative change in real factor inputs as measured by our scheme. The denominator varies according to which components of the total price change (relative change in revenue per unit of real factor input) we wish to allow as having influenced the producer's decision.

The most general measure of price elasticity includes all components of price change in its denominator. The producer's response to a change in the market price of his output is here assumed to reflect foreknowledge of or adaptation to the actual changes in real-estate taxes and factor prices that in retrospect affected his costs of production. The "portability" of this parameter is correspondingly limited to market contexts in which the same pattern of tax changes and factor-price changes is anticipated.

Portability can be increased by a series of adjustments to the measured total price change, subtracting out the components that reflect

the changes in real-estate taxes and factor prices. Essentially, the procedure assumes that the producer discounts an increase in the market price of output to the extent that it is matched by an increase in factor prices. It leads to a calculated elasticity that is an estimate of how he would have responded to a change in the market price of output in the absence of some or all of the actual factor-price changes.

The reader can see that a long chain of assumptions is entailed in estimating "portable" supply elasticities, and the final conclusions will be correspondingly crude. Nonetheless, they will represent a considerable improvement on our present understanding of supply behavior in the housing market.

Stratifying Supply Response

Our panel of residential structures at each site will be stratified by tenure, size of structure, value per unit, and neighborhood density (urban, rural), for a total of 16 strata. This system of stratification was chosen to distinguish sectors of the housing market that we think are likely to be differently affected by the experimental housing allowance program.*

The allowance program is designed to increase the housing demands of low-income families. Given the allowance schedule and terms of payment and the characteristics of recipients, we expect that their increased housing demands will be more focused in some sectors of the market than in others. Thus, the demand for modest but certifiable apartments and single-family houses is likely to increase substantially, while the demand for luxury apartments or expensive suburban houses is unlikely to be significantly affected. These differences will be reflected in the marketplace by different rates of increase in housing expenditures by market sector.

Not only will program impacts vary by market sector, but the characteristics of supply response may also vary. For some sectors, output may increase substantially in response to a small increase in price;

*See Corcoran, Poggio, and Repnau, op. cit., WN-8029-HUD. This document proposes 32 strata, since reduced to 16.

for others, prices may rise sharply with little change in output. In any given sector, the mix of price increases and output increases is likely to change over time.

Using the data on price and quantity changes secured from our stratified sample of residential structures, we propose to estimate the average price elasticity of supply for producers in each sector for alternative periods of time--i.e., for intervals ranging from one to five years from baseline, as our annual cycle of surveys extends the time series. These parameters will be the most portable findings from our study of supply response in our two experimental sites--that is, they will be the information most readily adaptable to analytical integration of our findings with those of the Demand Experiment, and to modeling the results of hypothetical allowance programs in other housing markets.

We should note, however, that the reliability of the estimates will not be uniform across market sectors, for two mutually reinforcing reasons. First, we have chosen to concentrate our survey resources in those sectors of the market that we think are most likely to be affected by the allowance program. Those sectors where only small price changes are likely will be represented in our sample by relatively few cases. At the same time, these are the cases most subject to measurement error. The price elasticity of supply is a ratio of two percentage changes; if either is very small--e.g., if the price change is insignificant--a small measurement error will make a large difference in the value of the ratio.

To put the case more generally, it is not possible to obtain an accurate measure of supply responsiveness in the absence of a substantial stimulus. Consequently, we do not expect to learn much about supply responsiveness in sectors of the market that are not affected by the allowance program.*

---

*In the first part of this section, we discussed the problem of distinguishing allowance-generated demand changes from exogenous demand changes when we attempt to trace the effects of the allowance program on the housing market at the experimental site. Here, we are concerned with estimating supply response to price changes resulting from any demand stimulus. Thus, there may be increases in the demand

Thus, even our portable parameters will be limited in their usefulness to the analysis of housing markets in which the demand stimulus occurs at the lower end. But since this portion of the market is the locus of policy interest, we can live with this limitation.

---

for luxury housing, quite unrelated to the allowance program, large enough to provide the data needed for reliable estimates of price elasticity in that sector of the market. But since we are not specifically creating the stimulus, we cannot count on its occurrence.

VII. THE ROLES OF INDIRECT SUPPLIERS AND MARKET INTERMEDIARIES

Investors in residential property rely in varying degrees on external resources to enable them to acquire their properties and to produce and market housing services. In any given case, these resources may include some or all of the following:

1. Long-term credit. The real-estate market, more than any other sector of the private economy, depends for its efficient functioning on the ready availability of long-term debt capital for financing the creation, improvement, and transfer of real assets. The principal institutional suppliers of such capital in the residential sphere are savings and loan associations, mutual savings banks, commercial banks, and insurance companies. In addition, the Federal government and several state governments have become important suppliers of funds. In low-income neighborhoods, when loans are not available from these sources, capital is frequently advanced by an individual, often the seller of the property that requires financing.

2. Insurance. Property insurance is one foundation of an orderly real-estate market. By enabling the investor to protect himself against large losses, it gives liquidity to real-estate investments and broadens their attractiveness to the investment community. Yet, such insurance often is difficult to obtain at reasonable rates in low-income neighborhoods. Insurers, by necessity, are large corporations.

3. Brokerage and speculation. Real-estate brokers are usually called upon for help in buying or selling residential property. In this role, they may be instrumental in implementing informal covenants for residential segregation. In declining neighborhoods or in neighborhoods experiencing racial change, brokers may be unable to find ordinary investors and home buyers for properties that are listed with them for sale. When this situation arises, the broker may either purchase a property on his own account with the intention of reselling, or he may arrange a sale to another short-term investor. This form of speculation provides needed liquidity for the former owner, though usually at substantial cost.

4. Management. Most low-income properties are managed by their owners. Some owners, however, contract this function to a professional firm, usually paying a percentage of rental revenue for the service. In either case, the property may be listed with a rental agent who locates and screens potential tenants. His fee is usually paid by the tenant.

5. Building services, repairs, and improvements. Owners usually contract out at least some maintenance and operating functions: heavy cleaning, window washing, heating-system maintenance, pest extermination, etc. In the low-income housing market, with which we are primarily concerned, much of this work is done by the owner or his direct employees.

Major structural repairs or improvements, including electrical, plumbing, or heating-system renovation, are usually contracted out. Homeowners occasionally undertake some of these functions themselves.

POLICY ISSUES AND RESEARCH QUESTIONS

The availability, cost, and quality of the services provided by these market intermediaries and indirect suppliers powerfully influence the ability of the private market to deliver housing services to low-income families. Their policies and practices directly shape the operating decisions of owners and to some extent determine who the owners will be. One of the objectives of the Supply Experiment, therefore, is to learn whether the stimulus provided by a housing allowance program will substantially alter the attitudes and policies of the firms supplying these resources. It is equally important to learn how the results of the experiment have been shaped by these attitudes and policies.

Questions about the policies of indirect suppliers and market intermediaries have a common feature: Even in an experimental setting, it is extremely difficult to devise robust tests of the changes induced by the allowance program. Some of the individuals and institutions concerned may be reluctant to reveal their practices frankly to an interviewer--all the more when these practices may be either illegal or generally disapproved. Yet a certain amount of systematic data can be

obtained. Combined with informed but informal observation, these data should lead to credible judgments. In the remainder of this section we outline for each of the indirect suppliers and market intermediaries who supply the external resources listed above the major research questions which will be explored, our data-collection plans, and the types of analysis we intend to pursue. Table 7.1 summarizes our plans for data collection related to these analyses.

FINANCIAL INTERMEDIARIES

With the flight of institutional sources of mortgage funds from low-income neighborhoods, real-estate transfers and structural repairs and improvements must be financed by purchase-money mortgages, land installment contracts, loans that are secured by other property, and unsecured personal loans. Since even these noninstitutional and indirect sources may be closed to certain classes of investors, transactions may not be consummated and improvements may not be made for lack of long-term credit. One measure of the efficacy of housing allowances will be the extent to which they are able to restore liquidity to the market by attracting debt capital back into sections of the city that have been shunned by institutional lenders. Equally, a barrier to the success of an allowance program could be the unwillingness of institutions to provide loans for transfers and improvements even where equity investors are prepared to commit capital. For these reasons, the mortgage market will be carefully monitored, both as part of the annual financial survey of owners and by direct interviews with lenders, and where necessary, by analysis of public records. Although an early change in lending policies is not anticipated, observable shifts should occur by the end of the fifth year if they are to occur at all.

In the financial survey, owners will be asked about the characteristics of any loans obtained during the year, as well as about any attempted sales or improvements which were thwarted by unavailability of financing. Where financial data about properties in the sample cannot be obtained from owners, public mortgage records will be examined.

All institutional lenders at the two sites will be interviewed at the beginning of the experiment and annually thereafter. A semi-structured questionnaire will be used. Lenders will be queried concerning the financing they are willing to make available to different types

Table 7.1

SOURCES AND TYPES OF DATA TO BE USED FOR THE ANALYSIS OF INDIRECT SUPPLIERS AND MARKET INTERMEDIARIES

FINANCIAL INTERMEDIARIES

1. Annual Financial Survey of Landlords and Homeowners
   a. Characteristics of mortgage loans on property at beginning of experiment
   b. Characteristics of new mortgage loans obtained during the experiment
   c. Perceived difficulties in obtaining mortgage loans for improvements or sale
   d. Perceived difficulties in making payments
2. Public Records

   Mortgage data on properties for which incomplete data are obtained from owners, if the missing data are deemed essential to the analysis
3. Surveys of Institutional Mortgage Lenders
   a. Number of residential mortgage loans outstanding in various parts of the SMSA, by type of property and borrower
   b. Number of residential loans that are delinquent or in default in various parts of the SMSA, by type of property and borrower
   c. Number of residential foreclosures during the previous year in various parts of the SMSA, by type of property and borrower
   d. Number and characteristics of residential mortgage loans made during the previous year in various parts of the SMSA, by type of property and borrower
   e. Official lending policy toward various parts of the SMSA, various types of borrowers, and various types of residential property
   f. Perceived circumstances under which loans with specified characteristics would be made in various parts of the SMSA
   g. Attitude toward specific low-income neighborhoods
   h. Perceived circumstances under which participation in various Federal mortgage insurance programs might be possible
   i. Suggestions for lending programs to complement allowance program
4. Informal Monitoring

Table 7.1 (continued)

INSURANCE COMPANIES

1. Annual Survey of a Sample of Companies
   a. Premiums charged for various types of policies in various parts of the SMSA
   b. Number of policies actually written in various parts of the SMSA during the previous year
   c. Aggregate dollar value of claims paid in various parts of the SMSA during the previous year
   d. Attitude toward specific low-income neighborhoods
   e. Characteristics of pooled-risk funds, if any such funds exist
2. Interviews with State Insurance Commissioner
   a. Verification of information obtained from insurance companies
   b. Contemplated or existing state programs for high-risk neighborhoods
3. Annual Financial Survey of Landlords and Homeowners
   a. Cost of insurance
   b. Inability to obtain insurance
   c. Cancellation of insurance

REAL-ESTATE BROKERS AND SPECULATORS

1. Administrative Records of Allowance Program
   a. Patterns of residential redistribution of allowance recipients as clues to residential segregation
   b. Reports from allowance recipients of discriminatory practices
2. Administrative Records of FHA Office
   a. Condition of structure prior to sale to allowance recipient and amount of improvements required by FHA
   b. Reports of subsequently discovered structural defects or misrepresentations
3. Informal Monitoring

MANAGEMENT FIRMS AND RENTAL AGENTS

1. Annual Survey of Landlords
   a. Use of services of management firms and rental agents
   b. Tenant-selection policies
2. Informal Monitoring

MAINTENANCE AND REMODELING CONTRACTORS AND TRADESMEN

1. Annual Financial Survey of Landlords and Homeowners
   a. Prices of factor inputs
   b. Opinions of owners as to quality of services supplied
2. Informal Monitoring
   a. Opinions of contractors and others concerning availability of skilled tradesmen, productivity of workers, and quality of work
   b. Opinions of building inspectors as to quality of completed work

of borrowers in various parts of the city. The extent to which these policies are determined by the rules of regulatory agencies will be explored. Information on delinquencies, foreclosures, and voluntary surrenders will also be obtained. Both owners and lenders will be asked about any dealings they may have had with the FHA in connection with the allowance program and whether the FHA created any problems for them.

Although the data on financial intermediaries will not be subjected to rigorous mathematical analysis, it should be possible with the information that will be gathered on sources of financing, types of loans, applications refused, interest rates, amortization periods, etc., to obtain a clear picture of the changing mortgage finance situation in the lower reaches of the market over the life of the experiment.

INSURANCE COMPANIES

The attractiveness of residential real estate as an investment depends in large part on its mortgageability, which in turn depends very much on its insurability. Even for the few equity investors who do not rely on mortgage capital to finance their acquisitions, ability to obtain insurance is an important prerequisite to any purchase decision. Thus, unavailability of insurance is certain to contribute to declining market values. It may lead as well to deliberate disinvestment on the part of existing owners who see the market values of their assets eroding and who do not wish to maintain a long-term investment position in unprotected properties.

It is well documented that owners of residential property in low-income neighborhoods are often unable to obtain insurance coverage. Even with assigned-risk pools, insurance in the older areas of some cities cannot be obtained at rates which owners regard as reasonable, and many owners are unprotected. Increased revenues due to housing allowances will partially compensate for the high premiums, thus somewhat alleviating financial pressures on the owner. Whether allowances can also create an environment in which risks, hence insurance premiums, are reduced is a relevant research question. It may, however, be a partially unanswerable question, since insurance reforms, as well as various social programs which could affect underlying risks, are already

under way and will very likely be proceeding independently but alongside the experiment.

Indications of both the initial seriousness of and changes in the insurance situation will be sought from three sources: (a) the annual financial survey of landlords, (b) the state insurance commissioners, and (c) a sample of companies writing policies at the sites. From the landlords we will learn whether the cost of insuring their buildings has moved upward or downward during the year, and also whether any difficulties have been encountered in obtaining insurance. From the insurance commissioners and the insurance companies, we will try to learn what the actual losses in various parts of the metropolitan area have been. In addition, the views of the commissioners and the companies will be sought concerning trends in losses following the introduction of housing allowances. Their judgments and explanations will help us assess whether the allowance program has had any effect on loss experience. Except for the data on losses, all of the information supplied by the commissioners and the companies will be obtained through unstructured interviews. The general reasoning underlying this approach, as it relates to both insurance companies and other market intermediaries and indirect suppliers, is outlined at the end of this section.

REAL-ESTATE BROKERS AND SPECULATORS

Two benefits which some persons hope will accrue from an allowance program are: (a) the dispersion of low-income families into better neighborhoods, and (b) a transition for many of these families from rental tenure to homeownership. These hopes are matched by concern that dispersion may be thwarted by discrimination against allowance recipients, especially those who are members of minority groups; and also that homeownership may be thwarted by exploitation of allowance recipients, as it has been in many cities where low-income households purchased homes with the aid of Sec. 235 subsidies. From the standpoint of the experiment it is important to determine the extent to which the residential choices of recipients are constrained by discrimination on the part of investor-owners or their agents, and also whether exploitation of the low-income home buyer by speculators is likely to represent a major threat to a full-scale allowance program.

Discrimination Against Allowance Recipients

Allowance recipients could be discriminated against in the real-estate market either because they belong to ethnic minorities,* because of their family characteristics, or simply because they are allowance recipients. Discrimination would be indicated by refusal to sell or rent housing to allowance recipients who were able and willing to pay the price at which it was offered to others. In today's legal and political climate, the refusal is unlikely to be overt; rather it would consist of evasions, deceptions, and withholding of information.

Real-estate brokers in many communities are known to play an important role in "managing" housing segregation; by informal agreement, some neighborhoods are declared out-of-bounds for some classes of home buyers. We see no reason to expect that the allowance program, per se, will alter these informal restrictive practices, but it may place additional pressure on them by increasing the financial ability of low-income ethnic minorites to seek homes in neighborhoods from which they are systematically excluded. It is also possible that community groups will seize the opportunity to establish a fair-housing organization or invigorate an existing one.

In our judgment, the number of minority-group recipients--both buyers and renters--who will try to find accommodations in all-white neighborhoods is almost certain to be insignificant even if the allowances which they receive are quite generous. Several studies suggest that the vast majority of low-income blacks prefer to live in predominantly black neighborhoods, and the same is probably true of other low-income minorities as well. Moreover, few families enjoy the role of housing pioneer, and the role is usually played by those who are well up on the socioeconomic ladder. It should be expected, therefore, that only a small number of minority-group recipients will search for homes in white areas that lie well beyond the edge of existing ghettos.

---

* Only one of our two experimental sites will contain a substantial ethnic (black) minority; this is a deliberate choice, to test how the results of the allowance program will differ between ethnically homogeneous and ethnically disparate communities.

As noted in Sec. VIII, we propose to track and map all changes of residence by allowance recipients. This information, combined with the expected feedback from the recipient counseling program, seems to us to promise insights into both the pattern of residential choices by recipients and the role of discrimination in constraining that pattern. If evidence emerges of discrimination enforced by market intermediaries, the problem and the agents involved can be investigated more systematically. We do not, however, see much value in preplanning a particular style of analysis.

Exploitation of Home Buyers

The possibility exists that a house many be sold to an allowance recipient at a price which is inflated relative to nearby values or to the quality and condition of the structure. With respect to the experiment, two questions must be addressed. First, is this situation likely to occur frequently? Second, can it be adequately monitored by the proposed measurement procedures?

The lessons learned from recent Sec. 235 scandals make it extremely unlikely that excessive markups would escape the attention of the local FHA office, which will administer such loans for the experimental program. Some "excess" profits are likely despite FHA appraisals, since the measurement of property values is imprecise.

Analytically, the profiteering seller is no different from the landlord who raises his rents by amounts that are out of proportion to improvements that he has made in the property. The difference between the two situations lies only in the fact that the landlord's action may be easier to detect, if he is cooperating in providing data for the experiment. However, since owners who sell their properties to allowance recipients must dispose of these properties under an FHA program, it should be easy to obtain data on building condition and on improvements that were made prior to sale. The existence of structural problems that were not apparent to the FHA appraiser can be ascertained by a program of follow-up interviews by the resident observer. Trends in the number and severity of these problems will be analyzed as the experiment proceeds.

The discussion above suggests that a program of housing allowances may engender a certain amount of speculative activity that could thwart

the goals of the program. While this is not improbable, the opposite result should also be anticipated. The role of speculators may become less important as confidence in the future of low-income neighborhoods among potential and existing investors, homeowners, and mortgage lenders grows. If this indeed proves to be the case, the configuration of owners should change over the life of the experiment, as should the nature and volume of transactions. These possibilities will be explored in the annual survey of owners.

MANAGEMENT FIRMS AND RENTAL AGENTS

During the course of the experiment it is possible that the extent to which owners make use of professional managers and rental agents may change. It is also possible that the quality of the firms which supply these services may change or that the quality of the services themselves may change even though new firms do not enter the field in significant numbers. Although we are uncertain as to what sorts of shifts to expect, our annual landlord financial surveys will include questions about such services and thus enable us to detect any significant trends that may be attributable to the allowance program.

Earlier, we discussed the role of real-estate brokers in enforcing residential segregation in the homeownership market. There is a parallel in the rental market, where landlords may accept or refuse tenants on grounds other than ability to pay and genuine evidence of their qualities as tenants. Especially in multiple dwellings, such policies may be implemented by a management firm or rental agent. Our survey of landlords and their agents includes a series of questions on tenant-selection policies, the answers to which can be compared with the characteristics of tenants, directly observed in the course of our household interviews. We should emphasize, however, that we do not expect rental agents to formulate tenant-selection policies; rather, we suppose that they will implement the owner's policies.

MAINTENANCE AND REMODELING CONTRACTORS AND TRADESMEN

Since the experimental allowance program will pay allowances only to those whose housing meets specified standards of quality, the

program may create a sharp increase in demand, at least temporarily, for the services of individuals and firms engaged in maintenance, repair, and rehabilitation. This, in turn, could result in a rise in the price or deterioration in the quality of the services provided. If the latter were to occur, measured "Q" in the supply-response formula for investors and owner-occupants would contain a hidden "P" component reflecting the behavior of the suppliers with whom they deal. Elsewhere in this report, we have outlined a method of measuring changes in the prices of factor inputs.* Since the method will not detect shifts in the quality of services, but only their prices, we propose that *major* changes in the performance levels of suppliers be ascertained as part of the annual landlord and owner-occupant financial surveys. In addition, the informal monitoring described at the end of this section should enable us to discover whether slipshod performance has become a significant problem for the suppliers themselves.

Although our design has anticipated the possibility of price and quality effects in the maintenance, repair, and rehabilitation sectors, it may be useful to explain at this point why such effects are expected to be small. Initially, the allowance program is almost certainly not going to persuade owners to make large investments in their structures. Accustomed to a market where investment horizons are only about three or four years, owners who would have to make large investments in their structures in order to participate in the program will conclude that the annualized cost of improvements is greater than the increase in rents that they could anticipate. So at the beginning of the experiment, we expect rehabilitation outlays to be quite modest, probably averaging no more than $500 to $700 per dwelling unit and rarely exceeding $1,000.**

---

*See Appendix D.

**Fortunately, expenditures of this general magnitude should be sufficient to upgrade most of the stock to code level. A study by the New York City-Rand Institute in 1969 indicated that the median expenditure required to bring a substandard dwelling into compliance with the New York housing code at that time was $500. (See Lowry, op. cit., P-4645.) A similar study in Baltimore in the same year by the Institute for Environmental Studies, University of Pennsylvania, found that 60 percent of the substandard inventory could be brought *above* code

The probable impact of expenditures of this magnitude can be estimated with the aid of a few additional assumptions. If, at a site of 60,000 households, one-fifth (12,000) of the residents qualify for an allowance; and if four-fifths (9,600) of those who qualify actually sign up for assistance; and if, of those who sign up, one-fourth (2,400) are already adequately housed, one-half are in modestly substandard units, and one-quarter are in units requiring intermediate or extensive treatment; then the maximum demand for additional rehabilitation services would, in the short run, be confined to only 4,800 units or about 8 percent of the stock. If we assume generously that all 4,800 units are upgraded over an 18-month period at an average expenditure of $700 per unit, the total investment for the first year of the program would be on the order of $2,000,000. This figure is equivalent to about 100 units of new construction and roughly 100 man-years of site labor. These are magnitudes which imply little stress on the supply side, unless the allowance program were introduced in the midst of a major building boom. To the extent that shortages did emerge, they would impact primarily on small investors and homeowners, who do not have well-established connections with the building trades. Because they do not have these connections, however, they could be expected to do some of the necessary work themselves, thereby reducing the possibilities of stress even further.*

Such stress on prices and quality as does emerge during the first year or two could possibly be counteracted by the sort of technical assistance that is available in the Sec. 312/115 program. The question of whether to supply such assistance, however, is primarily a program, not a design, issue.

---

for a cost of less than $1,000 per dwelling. See William Grigsby, et al., *Housing and Poverty*, Institute for Environmental Studies, University of Pennsylvania, 1971.

*In neighborhoods where the prices of factor inputs are already quite high because of the extra risks of theft and bodily harm, it is not inconceivable that a program of allowances could reduce risks and prices. Such neighborhoods do not exist at the experimental sites, however.

LEARNING FROM INFORMAL OBSERVATION

Statistical analysis of the systematic data described above should go part way toward answering our questions about the responses of indirect suppliers and market intermediaries to a housing allowance program. Such analysis, however, will have a strong tendency to fragment data into its smallest manageable units, good for testing hypotheses but poor as a method of synthesis. Yet a major part of our research problem is to discern coherent, clearly motivated patterns of behavior. For this purpose we think that informal monitoring is an effective tool. It will permit us to piece together a large array of disparate pieces of information gathered as part of the more formal surveys. It is not difficult to find local lenders, landlords, real-estate brokers, and public officials who will talk freely about "what is going on" in the real-estate market. Much of the information thus obtained may be unreliable, consisting of either unwarranted generalizations from a few incidents, biased accounts of personal dealings, or hearsay. But usually there is a germ of truth in even the unreliable accounts. An observer who can tell a hawk from a handsaw can detect these pieces of the truth, rationalize seemingly conflicting points of view, and formulate hypotheses. He can pursue these hypotheses for additional evidence, unconstrained by the structured framework of a systematic survey. His conclusions may not rest on statistical evidence, but they are likely to be vivid in the sense of capturing complex, partly irrational patterns of behavior. If the issues raised by his conclusions are of critical importance, they may be further pursued by well-targeted, limited-purpose survey work or analysis of public records.

In this part of the experiment, therefore, we propose to count what can be easily counted, then rely on our resident observers to guide any further systematic investigations into the behavior of indirect suppliers and market intermediaries. This procedure will lead us to ask new questions throughout the course of the experiment rather than force us to formulate all questions a priori and possibly miss a number of pertinent questions in the process. Given the length and complexity of the experiment, we regard this approach as essential to the success of the overall effort.

VIII. RESIDENTIAL MOBILITY AND NEIGHBORHOOD CHANGE

Each year, about 20 percent of all households in the United States change their places of residence; among renters and among low-income households, the proportions are higher. About two-thirds of all moves are local, to a new address within the same city or county; the remaining third are, in descending order of frequency, intercounty, interstate, or interregional. Here, we are concerned primarily with local moves and how their frequency and spatial patterns may be affected by a housing allowance program.

POLICY ISSUES AND RESEARCH QUESTIONS

Local population movements due to low-income housing assistance programs have become a matter of increasing policy concern. The largest such program, public housing, was originally intended to clear existing slums, rehousing the former residents at the same sites. For a variety of reasons--the long redevelopment cycle, deliberate reduction in residential density, and tenant selection policies that excluded many slum residents--this was seldom the result. Instead, most of the former population of a redeveloped site usually dispersed into nearby neighborhoods where housing was cheap, reestablishing slum conditions not very different from those of their former location.

Since World War II, Federal and local policymakers have experimented with a variety of alternatives designed to escape this dilemma: instead of demolishing slum housing, rehabilitating it without displacing its low-income tenants; building public-housing projects in middle-income neighborhoods to avoid the wholesale displacement of a slum population; building small-scale projects in scattered sites to minimize social impact on the "host" neighborhood; dispersing low-income families into subsidized private rental housing, often selected units within buildings the bulk of whose tenants were unsubsidized; and providing heavy mortgage subsidies to enable poor families to become owners of homes scattered through middle-class neighborhoods.

In some respects, the housing allowance concept is the logical culmination of this line of social experimentation. It entails no forced displacement of slum dwellers and no directed relocation of assisted families. Because the subsidies would be attached to eligible households rather than to particular housing units, recipients of housing allowances would have a wider range of choice as to type, tenure, and location of housing than under any previous program, constrained only by what the private market offers that is within reach of an allowance-augmented budget. But if these choices are to be left to allowance recipients, it becomes important to anticipate the residential redistribution and neighborhood changes that would ensue from a housing allowance program.

In the Supply Experiment, the range of choice permitted by the experimental allowance program designed within the constraints of Sec. 23 and Sec. 235 is only slightly less than would be the case under a "pure" housing allowance scheme. Households may be assisted as renters, as homeowners, or as home buyers; they may shift from one tenure to another and from one residence to another within the boundaries of the experimental site without loss of eligibility. For renters and homeowners, benefits under Sec. 23 will be identical, and the only restriction on housing choice is that the unit must meet minimum standards of quality and size; but for home buyers, conditions of eligibility for mortgage subsidies under Sec. 235 are more stringent and benefits generally greater than those for renters and homeowners under Sec. 23, and this subsidy is not automatically transferable to a different house if the assisted household should later decide to move.

For those enrollees who are dissatisfied with the housing they occupy or with the neighborhoods in which they live at the time of enrollment, the allowance program provides an *opportunity* to move. If their housing is substandard, the program provides an *incentive* to move, since allowance benefits are restricted to those living in standard housing; enrollees must either forgo the allowance, persuade their present landlords to make the improvements needed to meet code standards, or move to housing that is already in acceptable condition.

It is thus reasonable to anticipate that the experimental allowance

program will generate pressure on building owners (through their tenants) to make improvements and will also produce changes of residence by enrollees. We expect the amount of movement by enrollees to depend both on the speed with which landlords respond to this new incentive and on the vacancy rates* in standard units, both in the enrollees' neighborhoods and elsewhere in the metropolitan area.

Somewhat more problematical will be the effect of the allowance program on the mobility of nonparticipating households. Some of these households may move because of rent increases traceable to the allowance program; others may move because they object to allowance recipients who appear in their buildings or neighborhoods; others who wish to move for housing reasons may be restricted in their choices by more vigorous competition for standard housing.

There is little question, then, that a housing allowance program *could* result in rapid spatial redistribution of program participants; they would have the means, the motive, and, presumably, the opportunity to relocate. But whether a housing allowance program *would* have this result is another matter, about which well-qualified observers disagree.

Some of the disagreement may be traceable to the lack of a clear assumption about the level of assistance provided by an allowance program. While such a program, as we conceive it, does not place a ceiling on the amount that an assisted family may pay for rent or ownership, what is presently known about the income elasticity of housing demand (less than unity) indicates that some form of earmarking would be needed to increase the recipient's housing expenditures by as much as the amount of the allowance. Thus the program's target level of housing expenditure is an upper bound. No one, to our knowledge, has seriously proposed an allowance formula that would enable recipients to afford new

---

*It is easy but erroneous to suppose that the number of vacant standard units sets an upper limit on the number of moves by program participants. It sets an upper limit only on the number of substandard units that can be abandoned by program participants without displacing nonparticipants from standard housing. High turnover rates are found in urban areas with both high and low vacancy rates. Thus, New York City had a rental vacancy rate of 1.2 percent in 1968, but 15 percent of all renters moved during the preceding year, not counting those who left the city.

housing, or even expensive existing housing. At best, the target would be the median rent in the community, and well below the median annual cost of owned homes.

Thus, allowance recipients might be able to afford the costs of perhaps one-third of the community's housing stock. The housing within their reach would be concentrated in the older neighborhoods; by and large, the suburban developments of the 1950s and later would be out of reach for all recipients except those who placed an especially high value on housing, as opposed to other forms of consumption.

If this portion of the stock contained, as we expect, too few available vacant standard units to accommodate all allowance eligibles, there could follow competition between them and the ineligible occupants of such housing. The aim of the housing allowance program is to match supply and demand by inducing improvements in below-standard housing and forestalling the present process of deterioration to which standard units are subject when their tenants are unable to pay the cost of adequate maintenance.

In sum, we do not expect wholesale relocation of program participants from their preenrollment neighborhoods to distant parts of the central city or metropolitan area. Most moves by participating households, we think, will be within their preenrollment neighborhoods or to the fringes of them, in the classic pattern of ghetto expansion.

Nonetheless, it is important to test the a priori reasoning explained above. Any substantial relocation or redistribution of households within the metropolitan area, if it occurred, would be a result the detailed nature of which would be extremely important to understand.

From one standpoint, residential redistribution resulting from an allowance program is a potential side effect of considerable political and social importance; we will want to know in detail what to expect and why it occurs. From another standpoint, residential mobility is part of the process by which the housing market matches supply and demand. What role does it play in the context of a housing allowance program? Can we infer that impeding or encouraging mobility would have a positive effect on the supply response? Our information on the dynamics of the market response to an increase in low-income housing demand

is scant. The pattern of mobility can tell us much about the reaction of both buyers and sellers in this situation.

Additionally, the allowance program is likely to affect the mobility of participants and nonparticipants differently. These differences, too, are important subjects for analysis. Section IX deals with the general question of measuring impacts on nonparticipants (considering the experiment as a whole), but we are concerned with them here as well.

These policy interests require investigation of a number of specific questions, which can be grouped as follows:

The amount of moving. Will the amount of moving, or the frequency of moves, change when the allowance program is implemented? Will we observe a temporary increase in moving, followed by a resumption of the "normal" rate, or will we find the moving rate to be at a permanently higher level? Will participants and nonparticipants be affected alike with respect to frequency?

Spatial aspects of moving. Next to frequency, the spatial pattern of movement is the most significant aspect of increased residential mobility. We will be concerned with how program participants distribute themselves in the metropolitan area over time, and with the geographical origins and destinations of participants and nonparticipants, before as well as after program implementation.

Causes of moving. Who moves, and under what circumstances? Will the allowance program affect the propensities to move of both participants and nonparticipants? What characteristics of the "old" housing or neighborhood and what characteristics of the "new" affect moving decisions most? Will the type of households or the housing variables change as a result of the allowance program? If an eligible household must move to obtain certifiable housing, will it do so, or will it decline the allowance?

Results of moving. Is mobility a significant factor in upgrading housing? Do participating households typically upgrade their housing to certification quality by moving, or by obtaining improvements in the units they occupy? Does tenant turnover decline in buildings to which improvements are made? Do participating households tend to displace

nonparticipants from certified or certifiable units? Will increased moving activity lead households to adopt new procedures for locating available units?

SOURCES AND CHARACTERISTICS OF MOBILITY DATA

Our present monitoring plans provide us with two major sources of data that can be used to address these questions: administrative records of the allowance program, and the annual survey of households living in our sample of residential structures. To supplement the latter, follow-up surveys of movers may be needed in years subsequent to baseline.

The enrollment and recertification records of the housing allowance program will contain detailed information concerning the household characteristics of enrollees and their housing, necessary for determining eligibility and allowance payments. Their changes of residence can be continuously tracked for the entire period of enrollment.

The survey of sample structures and their residents is described in Sec. IV. For households included in the baseline sample, we will obtain retrospective data on places of residence and reasons for moving, as well as household characteristics at the time of each move. Our most detailed data on housing and household characteristics will relate, of course, to the respondents' circumstances at the time of the survey.

This sample will include both program participants and nonparticipants, with the latter predominating. While participants, whether in this sample or not, can be tracked by administrative records, a special effort would have to be made to track nonparticipants. Our annual cycle of household interviews will encompass only the original panel of housing units, so that we are sure of reinterviewing only households that do not move. For those that move, we will know only the date of departure, unless they happen to move into another monitored housing unit. Households that were not included in the baseline survey but later move into a monitored structure will be interviewed by the next annual survey, and their housing and mobility experiences can then be captured.

Some of the analyses described below will be incomplete unless nonparticipants interviewed at baseline are followed when they move. However,

we do not think it is essential to commit ourselves firmly to tracking all of them to their new residences, since the decision has no implications for planning the baseline surveys. Experience in other surveys indicates that such follow-up operations are time-consuming and expensive; we think the decision should be deferred.

MOBILITY PATTERNS FOR PROGRAM PARTICIPANTS

As noted above, the administrative procedures of enrollment and disbursement and housing inspection will enable us to track the residential locations of all enrollees as long as they continue to participate in the program, providing a complete residential history extending from the date of enrollment to the end of the five-year monitoring period or to the enrollee's separation from the program, whichever comes first. The analytical task is to discern and document patterns in these linked records. We propose to search for several kinds of patterns:

Spatial redistribution of program participants. We will compile an annual inventory of the residential locations of all program participants by small areas (e.g., census tracts). Differencing these annual inventories will tell us where in the metropolitan area the allowance-receiving population is growing and where it is decreasing.

Origin/destination of moves. We propose to construct annually a matrix of moves by small-area origin and destination. This matrix will display the flows whose net results are recorded in the analysis described in the preceding paragraph. Examination of the large flows will lead to specific hypotheses that can be statistically tested. Since these hypotheses will concern both movers and nonmovers, the discussion that follows applies to both, i.e., a nonmover is one whose origin and destination are identical.

Characteristics of origin and destination neighborhoods. One hypothesis to be tested is that the rate of flow depends on neighborhood characteristics at origin and destination. A basic delineation of neighborhood characteristics will come from our neighborhood survey: land-use patterns, characteristics of residential buildings, availability

of services and facilities, characteristics of residents, and "social indicators" of the quality of life.

Characteristics of origin and destination housing. Our data file will show, for each mover who is an allowance recipient, the general characteristics of the structure and the housing unit he occupies before and after each move, tenure, and rent. Careful application of multivariate analytical techniques should enable us to distinguish housing factors from neighborhood factors in explaining patterns of movement. This analysis will be integrated with a substantially similar analysis of the household-survey data, which covers nonrecipients. The analysis and underlying regression model are discussed at greater length below.

Characteristics of movers. We would expect some subgroups within the population of allowance recipients to have a greater propensity to move than others; subgroup moves will also differ in origin and destination characteristics. Again, multivariate analysis should enable us to relate mover characteristics to both neighborhood and housing characteristics, and again, the analysis will parallel, and be integrated with, the analysis of the household-survey data.

EFFECTS OF THE ALLOWANCE PROGRAM ON MOBILITY

In the preceding subsection we described a group of analyses designed to reveal the pattern of movement by program participants and their consequent redistribution within the metropolitan area. This information is directly responsive to concerns that have been voiced about residential redistribution associated with a housing allowance program--either that program participants (especially ethnic minorities) will move away from neighborhoods in which they are now concentrated, or that they will not do so.

However, these descriptive analyses fall short of a policy-relevant explanation of the events observed. They do not enable us to answer, except intuitively, the question of whether the same or a different pattern of movement would have occurred in the absence of an allowance program. We propose to seek more objective answers to this question

through a four-way comparison of moving behavior: program participants versus nonparticipants, before and after the commencement of the allowance program.

1. Preallowance mobility patterns. What characteristics of households, housing, or neighborhoods influence the frequency of movement and the origin/destination of moves before the commencement of the allowance program? Classifying households according to whether they later participated in the program, were preallowance mobility patterns different for participants and nonparticipants? What characteristics seem to account for the differences?

2. Postallowance mobility patterns. What characteristics of households, housing, or neighborhoods influence the frequency of movement and the origin/destination of moves after the allowance program commences? Classifying households according to whether they are participants or nonparticipants, how do mobility patterns differ? What characteristics other than participation status seem to account for the differences?

3. Changes in participant mobility patterns. Comparing the preallowance and postallowance moving behavior of those who enroll in the allowance program, do mobility patterns change in terms of frequency or origin/destination of moves? Can the observed changes in mobility be accounted for by factors operative prior to the allowance program--for instance, by changes in household composition or nonallowance income?

4. Changes in nonparticipant mobility patterns. Comparing the preallowance and postallowance moving behavior of those not enrolled in the allowance program, do mobility patterns change in terms of frequency or origin/destination? Can the observed changes be accounted for by factors operative prior to the allowance program?

These comparisons can be made at different levels of statistical sophistication, but an attractive approach is an adaptation of the demographer's technique of standardization based on regression analysis.

Thus, for frequency of movement, this would entail fitting equations separately for each of the four strata of households (preallowance participants,* preallowance nonparticipants, postallowance participants, postallowance nonparticipants); in each equation, the probability of moving within a specified period of time would be expressed as a function of selected household characteristics (such as age, sex, and race of head, marital status, size of household, income), selected housing characteristics (such as rooms per person, rent/income ratio), and selected neighborhood characteristics (distance to place of work, ratio of respondent's rent to neighborhood median, congruence of respondent's characteristics with those of the neighborhood's population).**

By exchanging coefficients of the fitted regressions among the four strata, it is possible to decompose the observed differences in frequency of movement into differences in the characteristics of the population represented in each stratum and differences associated with the stratifying variable, i.e., relationship to the allowance program. Short of a classical experiment with matched control and treatment groups, this technique comes as close as possible to isolating the effects of the allowance program on both participants and nonparticipants.

A similar approach is applicable to origin/destination of move, though additional reservations are in order here. To use such a model, the relationship of origin and destination must be expressed as one continuous variable (e.g., distance between origin and destination, ratio of (or difference between) a quantitative characteristic of the origin to the same characteristic of the destination).

---

*That is, data for the preallowance period on households that later enrolled in the allowance program.

**The reader will note that the examples given of housing and neighborhood characteristics are expressed relative to some characteristic of the respondent's household. While nonrelational variables might also be used, this form seems to us to have special promise, as a way of indicating the degree to which the household is satisfactorily integrated with its residential environment.

An alternative technique that is perhaps more appropriate to this problem is to classify origins and destinations into a manageably small number of categories of research or policy interest (for neighborhoods, perhaps by ethnic mix or median value of housing units) and represent the reported moves by respondents in each stratum as transition matrices linking these origin and destination vectors. Exchanging transition matrices among the four strata, we can decompose the differences in the destinations of each group into differences in their origins and differences in patterns of movement for each stratum from given origins.

These analyses would be easiest to perform if nonparticipants as well as participants were tracked when they moved from monitored structures. A less sturdy but practicable approach could be developed even without this feature, by retrospective questioning of nonparticipants encountered in the monitored structures for the first time in one of the postenrollment surveys.

MOBILITY AND HOUSING IMPROVEMENT

Households move for many reasons, only one of which is to improve their housing. Those eligible for housing allowances, however, have a particular incentive to move for this reason. Below, we describe several topical analyses that will help us to understand the role of mobility in securing housing improvements.

Housing Improvements for Movers and Nonmovers

When the allowance program opens for enrollment, some eligible households will be living in housing that is certifiable under program standards. In order to qualify for allowance payments, they will need only to enroll and have their housing inspected and certified.

Most eligible households, however, will be found in noncertifiable housing. To qualify for allowance payments, they will have to either persuade their present landlords to upgrade the property to program standards, or move to another unit that is certifiable. Which course they follow is a matter of considerable policy interest and

is indicative of landlord responsiveness to allowance-created demand for housing improvement.

Data bearing on this issue are available both from administrative records of the allowance program and from the sample survey of households in monitored structures. From the former source, we can obtain a list of applicants to the allowance program who are (a) declared eligible and (b) told that their present housing is unsatisfactory. Following these households over time, we can tabulate the proportions who (a) move into certifiable housing, (b) subsequently obtain certification of their original housing, and (c) do neither, thus losing their allowance entitlement. Within the first two groups, we can further tabulate cases by elapsed time between enrollment and certification. These data form a basis for reasonably strong inferences about the effectiveness for allowance recipients of each course and the frustrations they may encounter.

We can also expect the size of the available allowance to affect the program applicant's incentive and the leverage he will have in dealing with his current landlord or with landlords of buildings to which he may consider moving. Whether or not larger allowances will be associated with an increased tendency to move is problematic but is a question which we wish to examine. Accordingly, we will also tabulate the proportions of eligible applicants in the three groups above--those who move into certifiable housing, those who get their original housing certified, and the others--by the amount of the allowance they receive or for which they are eligible. This can be done at semiannual intervals from the enrollment and recertification records.

One weakness of these data is that they exclude eligible households who never apply for the allowance program. There may be many reasons for not applying, but one is that the household concludes from its understanding of program rules that its present housing is uncertifiable and the household is unwilling to move. From records of the baseline survey, we expect to be able to classify households as eligible or ineligible for enrollment, and their housing as certifiable or uncertifiable--not with the rigor required for program

administration, but accurately enough for research purposes. We propose a follow-up by the resident observer of a sample of eligible nonapplicants to ascertain why these households failed to apply. Findings from this follow-up study may modify conclusions reached from the analysis of program records described above.

Housing Improvements and Tenant Turnover. Another perspective on these issues is provided by tracking the histories of individual structures with respect to housing improvements and relating these events to tenant turnover and vacancy rates. Thus, we can determine whether a landlord strategy of housing improvement in an allowance-stimulated market is rewarded by less turnover and higher occupancy rates. Possibly, the data from the landlord financial survey will be precise enough on a building basis to test whether there is a systematic relationship between the owner's markup rate on factor costs, as reflected in his rent schedule, and the actual profitability of his operation, taking into account rent losses from turnover and vacancies.

Housing Succession: Participants and Nonparticipants. As we have noted, the allowance program will put participants on a competitive footing with nonparticipants for housing previously beyond the means of the former group. We will be able to determine who succeeds whom in the occupancy of specific units for our sample panel of structures. Sorting these findings by structure characteristics and rent levels should tell us a good deal about where in the spectrum of housing types and costs to anticipate the greatest competitive pressure from allowance recipients in a national program.

Triggering Events. Many of the moves by those enrolled in the housing allowance program will manifestly be triggered by the need to find certifiable housing so as to become eligible for allowance payments. However, not all moves, whether by program participants or others, are solely motivated by a desire for better housing or even better neighborhoods. And whatever a household's general preferences, a decision to move is likely to reflect some recent change in its circumstances that causes the members of the household to reevaluate alternatives.

Frequently, moves will reflect changes in household size, changes in job location, rent increases, or quarrels with the landlord. In our household surveys, we will record the reasons supplied for moving, although experience indicates that it is not easy to get an unequivocal explanation of the reason for a move from the respondent. We propose, therefore, to analyze the timing of moves relative to recorded events in the respondent's history (such as a change of jobs or of income or household size) and to compare housing characteristics before and after the move. Such an analysis, we think, will clarify the relationship of housing satisfaction and dissatisfaction to family life and will cast at least some light on a household's success in remedying its housing problems by moving.

HOUSING SEARCH PROCEDURES

The experimental housing allowance program is designed to enable low-income families to afford better housing than they generally occupy, but it relies on their individual efforts in the marketplace to obtain certifiable units, either by moving or by inducing their present landlords to provide the needed improvements. The portability of their allowances is essential to their bargaining power in the marketplace; but this feature could be nullified by lack of initiative, skill, or information in exploring alternatives to their present quarters.

Sociologists have often commented on the limited horizons of urban dwellers, particularly the poor. We suspect that lack of information about housing opportunities outside their immediate neighborhoods, combined with timidity about exposing this ignorance, is a substantial factor in the cohesiveness and persistence of segregated low-income neighborhoods, at least third in importance behind racial discrimination and lack of means.

Participants in the experimental housing allowance program will be offered counseling and guidance in how to look for housing, in how to judge its quality and appropriateness to their family circumstances, and in their rights and obligations as tenants or home buyers. These features of the program could prove to be as important as the allowance in helping enrollees to achieve their objectives. It is not intended,

however, that the counselor act as real-estate agent on behalf of the enrollee; given good advice, the enrollee will still have to conduct his own search, make his own choice, and negotiate his own terms with the landlord.

Our program of household surveys offers us the opportunity to evaluate the effectiveness of the counseling program in several respects; here, we focus on its contribution to skills and initiatives in searching the market for housing alternatives. At baseline, each respondent will be asked to describe the methods he used to find his present housing and the duration of search. Similar questions will be asked on subsequent annual surveys. Thus, we will be able to associate search methods with household characteristics and learn how the patterns change over time.

The formal method for analyzing these data parallels in many respects the technique proposed for determining the effects of the allowance program on mobility. Stratifying baseline respondents into those who later become program participants and those who do not, and similarly stratifying respondents to postallowance surveys into participants and nonparticipants, we can construct four strata for parallel analysis. Within each stratum, we can statistically associate search techniques not only with household characteristics, but also with the respondent's satisfaction with the housing he finds. Interstratum comparisons will enable us to see how these relationships change for program participants before and after enrollment and counseling; and for nonparticipants before and after the housing market is perturbed by the allowance program.

The latter point conceivably could be important. If the allowance program creates excess demand for particular kinds of housing, or for housing in general, search procedures that were effective in a looser market may fail to serve as well. How quickly our respondents master more appropriate techniques, and what types of respondents are most adaptable, could significantly affect the way in which housing is redistributed in an allowance-stimulated market.

IX. EFFECTS OF HOUSING ALLOWANCES ON NONPARTICIPANTS

The direct and indirect effects of housing allowances may extend considerably beyond participating families and those who supply these families with shelter. In every neighborhood in which participants reside at the beginning of the experiment, there will be a number of nonparticipants. Even within a single residential block, it is unlikely that the incidence of allowance-eligible households will ever rise above two-thirds of the total; and for larger neighborhoods, one-half seems a likely limit. Moreover, as some of the participants disperse to new neighborhoods, the spatial intermingling of the participants and nonparticipants will become, if anything, even more pronounced. For a fairly large portion of the total market, they will compete for the same housing and will mutually shape the same environment.

POLICY ISSUES AND RESEARCH QUESTIONS

Nonparticipating families may thus be affected by an allowance program in both obvious and subtle ways. Some may experience rent increases as a result of added housing demand on the part of participating households. Others may find their neighborhoods being "invaded" by a socioeconomic or racial group that they would like to avoid. Others may observe improvements in their social and physical environments as the additional income of participating families is translated into home repairs, increased owner-occupancy, better landlord-tenant relations, less involuntary mobility, and more permanent interest in the neighborhood. Finally, at least a few nonparticipants may watch their neighborhoods deteriorate as participant families move on to better areas and are not replaced by other households.

The political acceptability of a housing allowance program will depend as much on its actual and perceived effects on nonparticipants as on its benefits to participants. An important goal of the experiment, therefore, is to measure the various ways in which allowances alter the housing choices and neighborhood environments of nonparticipants; another objective is to ascertain their reactions to the program.

Since our general research concern with nonparticipating households is the same as that for participating families and their housing, most of the conceptual and measurement issues that were discussed in Secs. VI and VIII apply here. This section focuses on the ways in which nonparticipants will receive special treatment in our research and analysis.

The discussion is divided into two parts. First, we review the ways in which the allowance program may affect the housing choices and neighborhood environments of nonparticipants, and how those effects can be measured. Second, we consider how nonparticipants may perceive the allowance program's effects, and how changes in their attitudes can be measured.

HOUSING CHOICES AND NEIGHBORHOOD ENVIRONMENTS

We foresee three ways in which the lives of nonparticipants may be significantly affected by the experimental housing allowance program: (1) through changes in the cost or availability of suitable housing, (2) through changes in the physical characteristics or social milieu of the neighborhoods in which nonparticipants live, and (3) through residential relocation in response to changed housing or neighborhood conditions. Because these changes are interactive, we discuss them together below.

Availability and Cost of Housing

A frequently expressed reservation about the wisdom of undertaking a national program of housing allowances is the potentially adverse effect of such a program on the housing consumption of nonparticipants, especially those whose incomes are not far above the upper limits of allowance eligibility. Those who voice this concern usually point out that large-scale disbursement of such allowances to low-income families would substantially increase the demand for better housing without guaranteeing any increase in its supply. The likely consequence, they argue, is inflation in housing rents and prices due to excess demand. Nonparticipants would have to either reduce their housing consumption or reduce other expenditures to cover the increase in their housing costs.

If the allowances are generous, participants may even be able to outbid nonparticipants for the limited supply of better housing units. Not only does this seem an inequitable redistribution of housing resources, it entails little net improvement in communitywide housing conditions.

This scenario assumes a very inelastic supply function for housing services in the sector of the housing market serving families of low-to-moderate incomes. Its inarticulated premise seems to be that a significant increase in the supply of housing services occurs only through new construction. Even with their augmented resources, program participants will seldom be able to afford new housing; therefore, the allowance program will not stimulate an increase in the supply of housing services to match the increased demand.*

What the argument overlooks or dismisses is the possibility of preventive maintenance and capital improvements in the existing inventory. Such improvements were common during the 1950s, a decade of dramatic increase in real income and housing demand. Nationally, the Bureau of the Census estimates that some 5 million housing units out of the 17 million that were substandard in 1950 had been upgraded to standard condition by 1959.** Although the Bureau has not made comparable national estimates for the decade 1960-1969 and did not make comparable appraisals of housing quality in the 1970 Census of Housing, indirect evidence and special studies indicate that the pace of housing deterioration increased during the decade, particularly in large central cities.***

---

*Cf. Henry B. Schecter and Marion K. Schlefer, "Housing Needs and National Goals," *Papers Submitted to Subcommittee on Housing Panels*, Committee on Banking and Currency, U.S. House of Representatives, 92nd Congress, First Session, U.S. Government Printing Office, June 1971, pp. 37-38.

**U.S. *Census of Housing, 1960*, Vol. IV, *Components of Inventory Change*, HC(4), Part IA, No. 1. The Bureau estimates the reverse flow, standard to substandard, at 1.7 million units, including substandard units created by conversion and related means. In addition, 1.2 million units constructed during the decade were substandard in 1959.

***See National Urban League, *National Survey of Housing Abandonment*, New York, April 1971; and Lowry, "Housing Assistance for Low-Income Families: A Fresh Approach," *Papers Submitted to Subcommittee on Housing Panels*, op. cit., pp. 489-524.

Our point is that there is substantial room for housing improvement without resorting to new construction, through upgrading the existing inventory and--equally important--by applying a greater maintenance effort to forestall deterioration. Elsewhere, we have argued that housing deterioration is usually attributable to lack of effective demand, i.e., to lack of tenants who are able and willing to pay the full costs of adequate maintenance.* We will not repeat that argument here; suffice it to say that the Housing Assistance Supply Experiment will provide a test of this hypothesis. If it proves correct, allowance recipients will be able to obtain better housing without seriously discommoding nonrecipients. Both groups, able to afford decent housing, will be able to get it through normal market channels.

Section VI above describes our plans for measuring the responsiveness of housing suppliers to the experimental allowance program. For each of the monitored residential structures, we have proposed careful measurement of the inputs employed by the owner to produce housing services before and after the allowance program is initiated. At the same time that we measure changes in the flow of housing services, we will measure changes in rents paid by residents. We expect rents to increase as a result of the demand pressure created by the allowance program. The critical question is the extent to which the increase in rents will be accompanied by an increase in the flow of housing services.

Market theory tells us that the impact of the allowance program on the housing of nonrecipients should be reflected in these aggregate changes, because price changes tend to diffuse through the market. If the unit price of housing services rises sharply, nonrecipients will be discommoded thereby; if the increased spending by allowance recipients is nearly all reflected in increased services, the program's price effects on nonrecipients will be negligible.

Our analysis plan will provide a clear measure of the annual average change in the price of housing services within the experimental site, but this may not be the whole story. Housing services come in packages, by dwelling units, and consumers differentiate among these

---

*Lowry, ibid., pp. 490-496.

packages in the same way that they differentiate among makes of automobiles. Demand pressures focused on a particular type of housing may cause its price to rise more than the prices of other types, without a corresponding change in factor inputs. The presence in the market of less-favored but cheaper alternatives serves to limit price differences of this type, but it does not eliminate them.

Here, we are interested in the possibility that those who do not receive housing allowances will encounter price increases that are different--greater or less--from those encountered by allowance recipients. The structures in our sample can be divided into subsets, and the housing accounts described in Sec. VI can be compiled separately for each subset. We propose to compare accounts for the subset of structures most of whose occupants are program participants with accounts for subsets most of whose occupants are nonparticipants, simultaneously controlling on the neighborhood incidence of program participants. This procedure should reveal with reasonable accuracy whether the price effects of the experimental allowance program are different for program participants and for various categories of nonparticipants.*

We have argued (in Secs. VI and VIII) that neighborhoods with a high incidence of program participants will be the loci of the highest demand pressures. Absent the perfect market of textbook fame, housing prices in these neighborhoods should rise before prices in the others. We can again turn to the housing accounts to test this hypothesis. One approach is to classify each structure into one of several groups according to the incidence of participants in the neighborhood in which it is located, then compare the average price increases for the several groups. Alternatively, we can use the neighborhood as the unit of observation and regress average price change against the proportion of households that are program participants. Comparing these results for successive years will provide insights into the pace

---

*However, we should note that the reliability of our price-and-quantity-change measures decreases as the number of structures in a subset decreases. In particular, disaggregation can lead to aberrant results because of the lumpy effects of chance vacancies.

and completeness with which allowance-created demand pressures are propagated into market sectors in which direct effects (as evidenced by the concentration of recipients) are small.

In both of the analyses described above, various groupings of structures would be followed through time, and changes in the price and quality of housing services yielded by each would be measured by the method described in Sec. VI. Differential effects of these changes on participants and nonparticipants would be estimated on the basis of the changing incidence of each class of household in the structure or its neighborhood.

An alternative approach to measuring these housing effects on nonparticipants is to track individual nonparticipating families, rather than structures, noting the changes over time in their housing consumption and in the prices they pay for housing. For those who remain in the same structure, the two approaches are identical. For those who move from one monitored structure to another, price/quantity relationships for origin and destination housing can be compared, but it is a cumbersome procedure suited only to detecting gross differences. Finally, for those nonrecipients who move to unmonitored structures, no comparisons are possible.

Although tracing the fortunes of individual families of nonparticipants has considerable intuitive appeal as a way of determining how they are affected by the allowance program, it is neither essential nor, we think, as productive for this purpose as the first two modes of analysis proposed above. These address the question of how nonparticipants as a group are affected, with relatively good measures of the effects. The alternative addresses the question of how individual nonparticipating households are affected, with weaker measures of the effects.

Neighborhood Changes

Whether or not the experimental allowance program significantly affects the cost of nonparticipants' housing, it may affect their

neighborhood environments. In some neighborhoods, the ethnic and social characteristics of the resident population may change as program participants move in or move out, thus altering the social milieu for nonparticipating residents. Housing improvements made by landlords and homeowners to comply with program requirements will often be externally visible and conceivably will be frequent enough to change the appearance of a neighborhood from shabby to spruce; other structures, left behind by program participants and not worth rehabilitating, may be boarded up or abandoned. Operating jointly, population changes and housing investment (or disinvestment) may be reflected in the cleanliness, safety, and social ambience of streets and public areas, neighborhood shopping facilities, etc.

Analysis of the effects on nonparticipants of neighborhood changes induced by the allowance program entails a series of difficulties. First, neighborhood changes must be observed systematically. Second, they must be attributed to the allowance program or else classified as independent events. Third, they must be shown to be matters of interest to nonparticipants. For some of the changes discussed above, all three steps seem feasible; for others, some steps are easy, others difficult or impossible. Below, we discuss the prospects as we see them.

Characteristics of Neighbors. Administrative records of the experimental allowance program will enable us to determine with precision how many households in a given neighborhood at a given time are program participants, how many moved there after enrollment, and how many left after enrollment. The same records will enable us to describe these households in terms of ethnicity, family composition, and income. Thus, we will be able to determine which neighborhoods experience an influx or an outflow of specified kinds of households as a direct consequence of the allowance program, and in which neighborhoods nonparticipants have more or less contact with such households than they had prior to the program.

Other population changes, involving only nonparticipants, will be less easy to measure. Our panel of monitored structures is not geographically stratified by neighborhood, and the tenants of the structures that are located in a particular neighborhood are not necessarily

a representative cross section of the neighborhood population. It is true that the sampling rate will probably be high in all neighborhoods containing substantial numbers of program participants at any time during the experiment; and it is also true that the sample of monitored structures and their tenants in a given neighborhood can be expanded, stratum by stratum, to represent the total neighborhood population of structures and tenants. Thus, in principle, we can use our sample data from successive annual surveys to estimate the net changes in the nonparticipating as well as the participating population. But we hesitate to rely generally on estimates prepared by this method.

In this situation, it seems most realistic to limit our plans. We can determine whether monitored nonparticipants in a given neighborhood have encountered more or fewer participants as neighbors, and how the characteristics of these participants differ from those of the monitored nonparticipants. These observations then become variables potentially influencing nonparticipant attitudes toward the allowance program, the analysis of which is discussed later in this section.

Housing Characteristics and Property Values. As explained above, records of the experimental allowance program will enable us to classify neighborhoods according to the incidence of program participants and changes in that incidence over time. If we are also able to observe neighborhoodwide changes in characteristics of the housing stock or in property values, these can be statistically associated with the changing incidence of program participants. If regular associations--either positive or negative--are visible in the data, it is reasonable to infer that the changes in housing characteristics and property values are effects of the allowance program.

Evidence of changes in land use, property values, and the characteristics and quality of the housing stock by neighborhood is available from three sources. First, our annual neighborhood surveys are designed to capture by direct observation the obvious changes in land use and in housing characteristics of each neighborhood, though not with precision. Second, our landlord (and to a lesser extent, our tenant and homeowner) surveys include a series of questions designed to elicit the respondents' perceptions of changing land uses, property

values, and housing characteristics. Third, the detailed data on structural improvements gathered in the landlord and homeowner surveys will give us good estimates of the amount and kinds of housing improvements that have occurred within each neighborhood's panel of monitored structures.

This last kind of information, while the most precise, is also the one whose generalization to the neighborhood level is most qualified by the sampling problems described above. Obviously, the case for neighborhood housing changes is strongest if all three data sources agree, and obviously agreement is least likely when improvements are few in number and scale. However, it is reasonable to anticipate that five years of monitoring will reveal unmistakable changes in some neighborhoods, for better or worse. As noted, we can connect these changes to the allowance program only by examining their covariance with the incidence of program participants. If that connection is clear in the data, we can then reasonably conclude that for nonparticipants, the program has changed their neighborhood residential environment in specified ways. How such changes may affect their attitudes toward the allowance program is explored later in this section.

Public Areas and Public Facilities. Changes in neighborhood populations combined with housing investment or disinvestment are often reflected as well in the ambience of public areas. However, the network of causation is complicated. Some changes in street life follow directly from population turnover--e.g., more or fewer children at play, different habits of waste disposal, carelessness or respect for public and private property, etc. Others are mediated by the public sector, which may respond to neighborhood changes with compensating or supportive changes in the level and type of services provided--e.g., more frequent trash collection, police patrols, capital improvements or neglect of neighborhood schools, street repairs, etc.

Many such changes will be observed in the course of the Supply Experiment. Our neighborhood survey requires the fieldworker to rate a variety of public facilities and services, and our household survey elicits the perceptions of respondents about neighborhood cleanliness, the quality of schools, public safety, etc. As with housing changes,

these observations can be statistically associated with changing incidence of allowance recipients, and thus with the allowance program. However, where a change in the level or type of public services is involved, the responsibility of the allowance program for the change is less clear.

Consequently, while we may be able to show that nonparticipants have been exposed to neighborhood changes of specified kinds, it is less likely that we can show that the allowance program is clearly responsible for those changes. However, the attitudes of nonparticipants toward the allowance program may be nonetheless affected by their experiences with neighborhood change if they make (correctly or incorrectly) the causal inferences about which we feel cautious.

Nonparticipant Mobility

In the preceding pages, we have discussed ways in which the availability and cost of housing for nonparticipants may be affected by the allowance program, and how their neighborhoods may change as a consequence of relocation by participants and associated changes in the housing stock. In response to such events, nonparticipants may readjust their lives without changing residence, or they may themselves move to different neighborhoods to escape what they regard as undesirable changes in their previous housing or neighborhoods.

At one end of the spectrum of possibilities, inflation in the price of housing could force some nonparticipants into worse housing or less desirable neighborhoods. At the other end, some may decide that shifting from rental tenure to homeownership in a different neighborhood, even at greater total cost, is preferable. It is also conceivable that allowance-induced changes in the housing market may persuade some nonparticipants who might otherwise have moved to stay where they are. At this point, we can only speculate about the relative weights of various push/pull factors and how they might affect the redistribution of nonparticipants by neighborhood, tenure, and general housing circumstances.

In Sec. VIII, we explain how our program of annual field surveys addressed to the tenants of monitored structures will provide data

enabling us to assess the effects of the allowance program on patterns of movement by both participants and nonparticipants. Combining retrospective and current data on mobility and on the circumstances associated with changes of residence, we expect to be able to show how the allowance program affects the frequency and directions of movement and at least some of the relationships between the characteristics of origin and destination housing and neighborhoods. As we note in Sec. VIII, the analysis is improved by following nonparticipants when they move, but data on postenrollment moves by nonparticipants can, if necessary, be obtained by retrospective questioning of those who move into monitored structures.

Effects on Special Groups

There are several groups within the nonparticipating population whose fortunes under the allowance program are of special policy concern. Chief among these are the elderly, the poor, and ethnic minorities--groups generally conceded to be least able to cope with price inflation, dislocations in the housing market, or undesirable changes in their residential neighborhoods.

We expect these groups of nonparticipants to include many individuals and households whose lack of resources would justify housing assistance under our program standards. Some, like unrelated individuals under 62 years of age, are statutorily excluded. Others who are eligible may fail to apply or decline to enroll when they understand what is required of them. Thus, an elderly couple owning a dilapidated home and living on Social Security would be entitled to assistance only if they sold their home and moved to certifiable quarters, a step they may be unwilling to take. Still others may have incomes that enable them to afford adequate housing, but they may be particularly immobile (e.g., the elderly) or may face particularly limited opportunities in the housing market (e.g., blacks).

Thus, in pursuing the various impact analyses described in the preceding pages, we plan wherever possible to isolate these groups for special analysis, contrasting their experiences with those of the general run of nonparticipants with respect to changes in housing costs

and availability, neighborhood environments, and mobility patterns. The principal limitation on such analyses will be sample sizes, which may be too small to allow separate treatment of all the special groups of interest.

ATTITUDES OF NONPARTICIPANTS

The design of the experiment provides for measurement and analysis of various effects of the allowance program on nonparticipating households. Even if those households are not substantially affected, however, they may well believe their situation has changed as a result of the program, either because some other forces have been operative at the same time, or because their subjective assessment has misled them.

Consider the probable reaction to any increase in the price of housing during the course of the allowance program. We can expect housing costs to rise during the course of the experiment from background inflation, if for no other reason, but we also expect some allowance-caused price inflation in the early stages of the experiment. We are prepared to separate out the allowance-caused portion by analysis of the experimental results. However, a valid finding that the price rise was primarily due to background inflation could well get a skeptical reception from those accustomed to relying on their own interpretation of events or those who resent the allowance program for other reasons.

A similar point can be made about population movements. We expect some moving to result from the experiment as households attempt to qualify for the allowance by locating new quarters of certifiable quality. However, much moving occurs anyway, particularly in the low-income segment of the population. This background moving may well have a pattern that attracts notice, and nonparticipants may erroneously attribute it to the experiment. Again, our analytical techniques will enable us to separate the two components, but we can only guess at how the population at large will interpret such developments.

Such opinions and attitudes will constitute a particular sort of appraisal of the experiment. We need to be informed of these opinions

and attitudes, both because we wish the experimental findings to meet the arguments of such informal assessments and because the attitudes and feelings of the population involved may guide us to features of the experiment that are producing some gratuitous impression or irritation. We can identify four major areas of interest: what nonparticipants think about the allowance program; the views of nonparticipating eligibles toward the program and why they failed to enroll; how nonparticipant attitudes toward the poor, minority groups, landlords, the government, etc., change during and as a result of the program; and what kind of overt action or expression toward the program or the issues can be expected--if any. Treatment of these topics entails survey work beyond that contemplated by the experimental design, up to this point. We will consider the importance and the difficulties of each of the areas in turn, after addressing some methodological questions associated with attitude surveys.

Methodological Issues

A basic problem with the measurement of attitudes (in comparison with economic or demographic data) is the serious risk of erroneous measurement resulting from the measurement process itself. The more sensitive the respondent feels a topic to be, the more obtrusive these problems become. The list of hazards is well known: social desirability effects, halo effects, response sets, and equivocal answers given by the respondent because of his perception of the interviewer's expectations. There is also a serious risk of creating attitudes toward objects or situations about which the respondent lacks knowledge.

Such considerations lead to several conclusions about our approach and analysis:

1. The best way to measure sensitive attitudes is by indirection, that is, by seeking reactions to statements that do not obviously require the respondent to express his prejudices about such factors as race. One particularly appealing method uses information tests to diagnose attitudes. For example, attitudes

toward the recipient population might be probed by a multiple-choice question about the average amount of the monthly housing allowance payment for a female-headed household with two children. At least one choice would be decidedly too low, and another would be too high, relative to the correct amount; the correct amount would also be offered as a choice. The direction of the respondent's error in response to such a question is taken as an indicator of his attitude toward allowance recipients.

2. If we are to interpret the attitudinal data we obtain, we must relate attitudes to other variables such as the following: education, level and type of organizational activity, income, and the racial and class composition of the block in which the respondent resides; the amount of his contact with program participants; and his relationship to them (neighbors, close friends, relatives).
3. In addition, we need to find out what nonparticipants know about the program in order to interpret their attitudes toward it as a joint function of their knowledge and their exposure to its effects.

Attitudes of Nonparticipants Toward the Allowance Program

Of highest priority in our concern about nonparticipants is our need to sense and weigh the development of local opposition to the allowance program. Nonparticipants will not directly benefit from the program and, partly because they will realize such schemes are tax-financed, may easily perceive disbenefits for them. That they do not receive direct benefits does not mean that they will not, eventually, realize some indirect ones in the form of generally upgraded housing and related community improvements. Whether these are felt at all by the nonparticipants will have to be discovered by asking them.

Attitude and reality can be related in several ways. The respondent may perceive the effect of the allowance program on his interests, favorably or otherwise, more or less correctly, or he may make either of two errors in perception, loosely analogous to the Type I and Type

II errors of statistical inference. The program might in fact have a detrimental effect on nonparticipants' housing costs or environment, but respondents might not attribute the effect to the program; alternatively, the program may have no adverse effects (or may even confer benefits), while nonparticipants have the impression that it does affect them adversely.* The latter possibility may be more likely, and it is the one that motivated our initial concern with attitudes. The other is also potentially important. We could not count on nonparticipants in future programs to similarly overlook the programs' drawbacks to them, and we thus might considerably underestimate future implementation difficulties.

We wish to know what attitudes nonparticipants hold toward the program, how attitudinal differences vary with the respondent's situation, and how these attitudes were formed. Therefore, using appropriate multivariate methods, we shall attempt to relate our attitudinal information to the major socioeconomic characteristics of the respondent (including neighborhood and housing); the amount of exposure he has had to the allowance program, particularly his contact with participants as evidenced by their incidence in his neighborhood; his more general fund of knowledge about the program; his fund of knowledge about housing--rent control, landlords' costs, property values; and his exposure to events and circumstances--neighborhood change, housing difficulties, price inflation--that may not be related to the program but could be so interpreted.

Various groupings of nonparticipants may prove to be of analytical importance. For example, attitudes may vary according to the housing submarket in which the respondent is found: those who competed for housing with eligible households before the allowance program began; those in the same submarket with program participants after the program began; those who do not compete in the same market but may feel an induced effect on rents or prices; and those who are not, as housing consumers, affected at all by the program. Equally, groups of

---

*A view of the program impact that has the proper sign but exaggerates the magnitude is another possibility, of course.

special policy concern, such as the elderly, unrelated poor persons under 62 years of age (who will not be eligible for allowances), and racial and ethnic minorities are important to examine separately.

Survey Tactics

There is clearly a strong case for gathering data on attitudes toward the allowance program after it is in operation. We see much less to be gained by pursuing this issue in the preenrollment period, when respondents can only react to advance publicity about the experiment. Consequently, we have made no provision for such questions in our baseline survey instruments, other than some general inquiries about how expenditure patterns would be affected by specified changes in income.

There are two ways of acquiring the necessary information in the postenrollment period: We can incorporate attitudinal questions in the survey of households living in the monitored structures, or we can administer an independent survey to a random sample of other households. The former procedure would avoid the cost of designing the sample and administering a separate survey, and it would take advantage of some relevant questions already included (chiefly, those asking for socioeconomic information from the responding households).

An independent survey would also have significant strong points. It would avoid loading more topics into our already rather lengthy survey instrument. It would avoid, or at least permit controlling for, the (probably small) possibility that attitudes toward the allowance program might be contaminated by repeated surveying or by residence in a monitored structure.* Finally, an independent survey would give us a free hand in designing an appropriate sample. Our sample of structures is intentionally weighted toward sectors of the market that are likely to contain program participants, with very low sampling rates in large sectors of the market within which we expect to find

*A partial counter to this objection is that new nonparticipants will presumably appear in the sample structures as the experiment proceeds. However, we should be cautious about assuming that new nonparticipant move-ins will be as numerous as we would like for this purpose.

only nonparticipants. While this sample directs our attention to nonparticipants who are most directly affected by the program, it tends to ignore those more likely to be influential in community affairs.

We are not now sure which arguments should prevail. Because no information must be obtained at baseline, a final decision need not be made until well into the first year.

We might also note that the resident observer can play an important role in sensing and interpreting attitudinal developments. Aware of the context of local and national events in which these developments take place, and the character of the channels through which they are articulated, a sensitized individual will be able to discern, and diagnose the impact of, shifts in public attitude in ways that cannot be done with survey data alone.

Attitudes of Those Eligible but Not Participating

The eligibles who choose not to enroll comprise a subgroup of nonparticipants of particular interest. Do these respondents have an accurate understanding of the program? Does the program have drawbacks for them that are not now foreseen? Will we find significant attitudinal or social barriers to participation? Do respondents feel that enrollment would be stigmatizing or threatening to their personal liberty? Would acceptance of the allowance involve reducing other expenditures, by virtue of the higher cost of certificated housing (after all, some are now receiving housing free or in exchange for services)? Are the benefits simply not worth the trouble?

The variety of possible explanations, all conjectural at this stage, underscore the importance of exploring the subject of nonparticipation. By use of the baseline survey, we will be able to tell, with sufficient accuracy for present purposes, which households in our monitored structures are eligible for the allowance program. Reference to program enrollment records will, in turn, reveal which ones did not sign up for the allowance. These households constitute an entirely suitable sample panel to whom the relevant questions about their situations and attitudes can be addressed. We can then assess the relative

importance of attitudinal factors and other circumstances--housing, economic, or informational--to the choice not to participate.

No additional baseline data will be required for this analysis.

Attitudes Affected by the Allowance Program

In addition to learning about public reactions to the allowance program, we might also try to discover how the allowance program may affect other social attitudes, e.g., attitudes toward the poor, the welfare system, racial minorities, landlords, the government. If such effects occurred and could be traced to the allowance program with fair confidence, useful purposes--even important ones--might be served.

However, we have no particular reason to expect the program to be so prominent as to greatly affect peoples' views about these related (or relatable) matters. We are concerned, moreover, about the feasibility of establishing causality between contact with the allowance program and shifts in attitudes about other things, even related things. Attitude formation is a complex process, and attitudes will be modified, slowly or however, by a variety of forces. Since we do not think the allowance program is likely to have a very large effect on peoples' views of social issues, we conclude that the odds are low that analysis of the connection between the allowance program and changes in these views would be fruitful.

Furthermore, an interest in comparing attitudes before and after the allowance would require prompt action to mount the appropriate survey before or at the start of the program. These considerations together lead us to recommend that this topic not be included in the survey.

Not including such questions in the survey does not mean that we must lose all interest in the subject, however. As indicated above, the resident observer will be alert to attitudinal developments. It seems unlikely that a major reaction of the type that concerns us in this subsection could completely escape his notice.

Attitudes and Action

We are also concerned about the nature of antiprogram activity that may develop in the community. The effectiveness of citing systematic surveys for "representative" attitudes in the face of an outspoken reaction can be blunted if even a tiny minority with negative attitudes can mobilize organizational or influential support. Alternatively, even a substantial minority of nonparticipants having negative attitudes toward the program may lack a constructive outlet for their reactions. In such an event, it may be in the interest of the program to supply one.

One useful relationship to explore is that between attitudes toward the allowance program and organizational affiliations. The latter provide clues to nonparticipants' potential leverage in influencing community opinion. Such information will therefore be sought as part of the postenrollment surveys of nonparticipant attitudes that we recommend.

In addition to (and in between) these surveys, our resident observer will make it his business to talk informally with community leaders and members of influential organizations comprised of nonparticipants, and to attend meetings of civic or fraternal organizations where the housing allowance program might be discussed. His role will not be to defend the program, but to gather intelligence. If this is done with skill and diligence, we will be alerted not only to changes in the tone of public opinion, but to the emergence of organized opposition.

In sum, even if the effect of the program on community housing standards is favorable, there may develop organized opposition either to the experiment (with all its field surveys) or to the allowance program. As managers of the experiment, we shall have to deal with such opposition as best we can, seeking to pacify it without misrepresentation. As advisors to HUD, it will be our responsibility to pinpoint, if we can, specific features of the experimental allowance program that provoke opposition, and to suggest, if we can, modifications that would lessen these difficulties in the case of a national program. The study of nonparticipant attitudes is part of the machinery for doing this.

X. INFERENCE FROM EXPERIMENTAL FINDINGS

In the preceding sections of this report, we have set forth our recommendations for the design of an experiment whose purpose is to gain information about the probable consequences of a national program of housing allowances. This information is needed both to shape the design of a legislative proposal for such a program and to inform the judgment of those who must decide whether, on balance, the proposal is in the public interest. As noted in Sec. II, the results of this experiment must be intelligible and convincing not only to professional economists and housing experts, but to the broader constituency whose support would be essential to passage and implementation of a national program.

Here, we shall try to evaluate the reliability and credibility of inferences drawn from the proposed experiment as guides to the effects of a national program. We shall also try to show how the findings of the Supply Experiment can be analytically linked to those of the Demand Experiment, and we discuss the problems of inference from the joint results.

GENERALIZING FROM EXPERIMENTAL EVIDENCE

Basically, the plan of the Supply Experiment is to mount a full-scale housing allowance program in each of two small metropolitan areas and monitor its consequences: changes in the price and quantity of housing services supplied by the market, the behavior of indirect suppliers and market intermediaries, residential mobility and neighborhood changes, effects on and attitudes of nonparticipants. The most intensive monitoring efforts are planned for a sample of residential properties, their landlords, and their tenants, concentrated in those sectors of the housing market most likely to be affected by the program; and for program participants, who will be followed as long as they are enrolled. Monitoring is scheduled to last for five years after commencement of the allowance program.

Can we reliably infer from the evidence thus produced what would happen in response to a national program of housing allowances? It is helpful to distinguish six reasons why this experimental evidence might lead to erroneous general conclusions:

1. The estimates and measurements made during the course of the experiment in these two metropolitan sites may be inaccurate.
2. Consequences attributed to the experimental allowance program may be due to other factors, or the reverse.
3. The consequences of an experimental allowance program may differ from the consequences of a "real" program of the same design.
4. The consequences observed in the two metropolitan sites may be unrepresentative of those that would occur in other metropolitan (or nonmetropolitan) areas.
5. The term of the experiment may be too short to observe important long-run consequences of an allowance program.
6. Consequences may be so closely linked to the specific provisions of a housing allowance formula that tests based on any one formula would have little relevance to other formulas that might be chosen for a national program.

We have thought a good deal about these issues, with the result that many specific features of our proposed experimental design were included to avoid or reduce the likelihood of erroneous inferences about matters of importance. On most of these issues, we think either that the experiment will be self-validating--i.e., that analysis of experimental findings will enable us to judge the reliability of inferences from them--or that the path to greater reliability is clear, the principal obstacle being its incremental costs. Below, we offer our conclusions on each issue.

Errors of Estimation and Measurement

Our monitoring plan includes field surveys to gather quantitative data on residential properties, their owners, and their tenants; the

use of administrative procedures to gather data on program participants; and informal monitoring of local attitudes and events bearing on the allowance program. Data gathered by all three means are subject to error.

Our field surveys will follow a sample of residential properties for the duration of the experiment. The sampling frame is designed so that properties moving into or out of residential use during the monitoring period will also be captured. The sample is stratified by housing characteristics that differentiate major sectors of the housing market; the sampling rates vary among the strata, with the bulk of the sample elements drawn from sectors of the market that we think are likely to be directly affected by the experimental housing allowance program. The probability that any given residential property will be included in the sample can be calculated, and all such probabilities are greater than zero.

By applying inverse probability weights to observations made on the elements of our sample, we can estimate the incidence of an observed characteristic in the universe from which the sample was drawn--i.e., in the housing stock of the geographically bounded experimental site. Such estimates are possible not only for the physical and financial characteristics of the residential properties that are surveyed, but also for the characteristics of their owners and their tenants.

However, such estimates are subject to sampling errors whose magnitudes cannot be determined precisely in advance, since they depend in part on the actual distribution of values of the characteristic for which the estimate is made. Our sample design is keyed to the problems of estimating stratum mean values of a particularly slippery parameter, the price elasticity of the supply of housing services. Our a priori calculations indicate that for those strata of the housing market likely to be substantially affected by the allowance program, sampling error will be small enough so that we can tolerate equal amounts of error from other sources without destroying the usefulness of the estimates for policy analysis. For less complex measures--e.g., the number of resident landlords, the average fuel expenditure per unit, the number of female-headed households, the percentage of households dissatisfied

with their housing--sampling errors should present no serious obstacles to a reliable description of the universe, or at least of that portion of the universe which is significantly affected by the allowance program.

We do need to worry about nonresponse bias and about erroneous or misleading responses in field surveys. The household surveys pose no novel problems in these respects, and there are well-developed quality-control techniques to deal with the problems we do anticipate.

We anticipate more difficulty with the field survey of building characteristics and condition, which relies on direct observation by the fieldworkers. Here, our task is to devise a field instrument that will reliably capture changes in building condition from one annual survey to the next; we have little hope of devising one that will enable us to make rigorous comparisons among buildings. Because of these anticipated difficulties, our analysis plan does not rely heavily on the building survey.

We think that the key to the success of the Supply Experiment will be our ability to obtain from landlords honest and accurate reports on their maintenance and operating outlays and the costs of capital improvements. Experience with this type of survey is limited. We know of several instances in which public-housing authorities, nonprofit-housing sponsors, or building-management firms have been able and willing to supply researchers with well-documented financial records on individual structures or housing projects. But we know of only one survey, conducted in New York City, that was addressed to a large number of small holders and sought the kind of financial data we must collect.*

Considering that this survey was conducted under the sponsorship of the Office of Rent Control in a tense political environment, it was surprisingly successful. We have reviewed the field instruments and operating procedures, studied the incidence of nonresponse, and worked with the reported financial data, comparing them with audited data from

---

*The survey and its findings are reported in George Sternlieb, *The Urban Housing Dilemma: The Dynamics of New York City's Rent Controlled Housing*, issued in draft form by the Department of Rent and Housing Maintenance, Housing and Development Administration, City of New York, May 1970.

nearly the same universe. We think we have a better-than-fighting chance of obtaining the information we need to measure supply responses within tolerable error limits.

But we have gone to considerable pains to design the experiment and plan the data analysis so as to allow latitude for measurement error. Here, we rely on three principles: powerful experimental "treatments," aggregation, and redundancy. Our use of these principles is described below.

On purely definitional grounds, it is clear that the supply response to a housing allowance program cannot be greater than the incremental housing expenditures that result from the program, i.e., that

$$\frac{\Delta R}{R} = \frac{\Delta P}{P} + \frac{\Delta Q}{Q} + \frac{\Delta P \Delta Q}{PQ} .$$

If $\Delta R/R$ is large, we can be sure that either $\Delta P/P$ or $\Delta Q/Q$ or both will also be large. With a given error of measurement, the relative accuracy of our estimates of price and quantity changes increases with the magnitude of the changes. It is for this reason that we insist on focusing our monitoring on sectors of the market with a high incidence of allowance recipients, where we can be sure that $\Delta R/R$ will be large.

We have planned our analysis of supply responsiveness so that the principal conclusions are derived from aggregate values for market sectors rather than from values for individual structures. Such aggregate values are relatively insensitive to randomly distributed measurement errors for individual structures. But we have also planned the analysis in terms of relative changes, which means that its conclusions are insensitive to biased errors of measurement as long as the bias is consistent over time.

Finally, we propose to supplement our central analysis of supply responsiveness based on landlord financial reports with two less rigorous collateral measures: changes in directly observed building condition, and changes in tenants' evaluations of their housing. If these collateral measures seem to contradict the evidence of our financial

measures, the credibility of the latter is obviously weakened. In a five-year monitoring program, there is time for corrective feedback from such signals of trouble.

Netting Out Background Effects

A potential difficulty in interpreting our experimental findings is that of distinguishing events caused by the experimental allowance program from those that would have happened in its absence. There are two classical methods for isolating the effects of an experimental treatment. One is to match the treated population with an untreated "control" population, ascribing the differences in the subsequent histories of the two populations to the treatment. Another is to apply the treatment to a system that is in equilibrium, sheltering it from any other exogenous influences during the term of the experiment. Unfortunately, neither of these methods can be applied to our experiment.

The first method will be used in the Demand Experiment, where the experimental unit is the household. But in the Supply Experiment, the experimental unit is a metropolitan housing market. While it is true that individual households are "treated" by giving them housing allowances, our interest lies less in their individual responses to this treatment than in the effect on the metropolitan housing market of a large infusion of low-income housing demand. To make a statistical comparison of treatment and control groups, we would need to mount the housing allowance program in a randomly selected subset of all metropolitan housing markets and monitor a similar subset as a control group. Considerations of cost, if nothing else, rule out this approach.

It would not even be very helpful to select two metropolitan housing markets, each matched to one of our experimental sites, as control cases. For a control case to be matched with a treated case implies not only congruence of baseline characteristics, but congruence of demographic and economic forces (other than the allowance program) operating on each site throughout the term of the experiment. Achieving congruence of the first kind would be difficult enough; guaranteeing congruence of the second kind, before the fact, would be impossible.

The second classical method of experimental control is equally difficult to implement for our experiment, but we will nonetheless have to rely upon it in part. In selecting sites for the Supply Experiment, we have tried to avoid metropolitan areas whose housing markets have suffered recent severe dislocations, and those in which such events seem likely over the term of the experiment. In other words, we seek to conduct the experiment in a semiequilibrated environment, where the "natural" forces affecting the supply and demand for housing are changing according to some regular pattern that can be extrapolated with reasonable confidence.*

But whatever our success in choosing housing markets with these properties as experimental sites, we are unable to guarantee that they will be free of exogenous shocks during the term of the experiment. We will have some control, with respect to Federal housing and urban-renewal programs; but we cannot prevent natural disasters or large-scale private actions that would dislocate the housing market (such as the opening or closing of a large manufacturing plant).

We think that it is important to recognize the real possibility that a major shock to the metropolitan economy could play havoc with our experiment. By careful site selection, we can reduce the possibility of such an event; with luck, we will escape its occurrence; finally, with two experimental sites, the odds are very high that at least one will not experience such a dislocation.

Within the context of such a semiequilibrated housing market, we must and can reasonably rely on analysis to distinguish the effects of the allowance program from the effects of other factors. The techniques of analysis and the assumptions they entail vary with the effect to be analyzed; but generally we rely on one or more of three methods:

---

*Technically, this situation can be described as a *dynamic equilibrium*, in which rates of change for the variables of interest are stable over time--e.g., 2-percent annual increase in population, 5-percent annual increase in income per capita. It contrasts with a *static equilibrium*, in which rates of change over time are zero.

1. Comparing circumstances and behavior during the experiment with corresponding circumstances and behavior prior to the experiment.
2. Comparing the circumstances and behavior of those most directly affected by the allowance program with the circumstances and behavior of those for whom the connection to the allowance program is remote.
3. Modeling events in the housing market with and without allowances, using the same behavioral parameters for both cases.

Thus, our proposed analysis of the effects of the allowance program on residential mobility relies on Methods 1 and 2, with a four-way comparison of the behavior of program participants and of nonparticipants, before and after the commencement of the allowance program. Our analysis of the effects of the allowance program on the price and quantity of housing services relies on Methods 2 and 3, comparing sectors of the market in which allowance recipients participate with those from which they are absent; and modeling the aggregate effects on supply of income and housing demand shifts for participants and nonparticipants separately.

Finally, to the extent that we are unable to disentangle the effects of the allowance program from other factors at work in the local housing market, we should be able at least to reach a fortiori conclusions of a type that have considerable power in policy analysis. That is, we can conclude that the effect of the allowance program on Variable *X* was *at least (or at most)* such-and-such. So long as there is continuity with preallowance time in the behavior of background forces, the inseparable background changes should be small relative to allowance-induced changes in the housing market.

Experimental Response Versus Program Response

The Housing Assistance Supply Experiment differs from a national housing allowance program in several ways. First, the experiment will enroll only those eligibles who live in the two selected metropolitan areas at the time enrollment is opened, and those who leave these areas

will be dropped from the rolls. Second, the experimental allowance program is of limited duration; we have proposed a ten-year commitment. Third, participants in the local housing market will be aware that their behavior is being monitored, and that the future of a national housing allowance program may well depend on the monitors' conclusions.

In an earlier version of the experimental design, when we proposed to limit enrollment to residents of selected low-income neighborhoods, the first of the differences noted above loomed large. The implied restrictions on the local mobility of experimental allowance recipients were seriously inconsistent with the probable rules of a national program, and the results of the experiment could thus be misleading. The present design, with enrollment expanded to include all eligible residents of the metropolitan area, quiets this concern. Within the limits of their allowance-augmented budgets, participants may select their housing from the full variety offered by the local housing market. Restricting eligibility to those in residence at the beginning of the program precludes allowance-motivated migration into the metropolitan area; in a national program, there would be no such motive for inter-metropolitan movement.

The one remaining difficulty is that under these rules the number of program participants may decline over time due to outmigration without replacement. This could be a serious problem, since the normal rate of low-income outmigration from a single-county SMSA is about 7 percent annually; however, the experimental allowance program may reduce this flow, since recipients who leave will lose their benefits. If we do encounter serious depletion of our panel of recipients, we have the option of opening enrollment to recent arrivals--without promising to accommodate future arrivals.

As long as we select metropolitan sites for the Supply Experiment that are "free-standing"--i.e., separated from other population concentrations by an expanse of thinly populated territory--it is manifest that cross-elasticities of housing demand between our sites and other local housing markets will be trivial: We do not need the background of a national housing allowance program to produce its demand consequences

at a local level. Cross elasticities of supply are more problematical, since some of the resources required to produce housing services are intraregionally mobile. If we observe a large influx of outside resources (e.g., remodeling contractors) in response to local demands for housing improvements, we ought to be cautious in our inferences from the experiment as to the inflationary effects of a national program; factor-price inflation observed at the experimental site is likely to be an underestimate for a national program. But unless the experimental program's demand impact is at least twice as great as we anticipate, significant intraregional factor movements seem to us highly unlikely.

The second point on which the experimental program contrasts with a national program is its expected duration. This is important because present behavior of both housing consumers and suppliers is governed by their expectations about the future. An allowance-eligible household would hesitate to move, or to sign a long-term lease at a higher rent, or to shift from rental to ownership tenure if it expected the allowance to be withdrawn soon after its action. A building owner would hesitate to make capital improvements whose amortization depended on allowance-stimulated revenue increases if he expected the allowance program to terminate soon thereafter.

Although we propose to monitor the experiment for only five years, *we cannot overstress the importance of a longer commitment for the allowance program: We think ten years is the minimum that should be considered.* For eligible renters who wish to purchase a home, we have proposed life-of-the-mortgage assistance commitments under Sec. 235. For homeowners who must make capital improvements to qualify for assistance and for renters who are considering allowance-assisted moves or rent increases on their present units, we feel certain that a ten-year assistance guarantee (contingent on continued eligibility) would be adequate to motivate the behavior that would accompany the permanent guarantee of a national program. For landlords, we think a ten-year guarantee is the minimum.*

*We are not proposing to guarantee each landlord a specific increase in revenue for ten years--only that the allowance program will

The mathematics of debt service tell us that if a landlord must amortize capital improvements over five years versus their expected life of 21 years, the annual cost to him is nearly tripled. We estimate that to cover the increased debt service costs, averaged in with other housing inputs, he would typically have to raise rents by 12 percent more than he would under a 21-year amortization program. If amortization were reduced from 21 to 10 years, he would need only 4 percent more rental revenue to cover his added costs.*

The third problem of experimental bias is the so-called Hawthorne effect. Participants in the Supply Experiment may behave differently than they would under a national housing allowance program simply because they are aware that it is an experiment. Tenants and landlords in our sample of residential properties will not only be aware that the *program* is experimental, they will be aware that their own actions are being closely monitored in a way that would not be characteristic of a national program.

The problem here is particularly acute for landlords whose operations are monitored. Annual surveys of building condition, maintenance and operating outlays, capital-improvement costs, and rental revenues will make the landlord aware that the monitors of the experiment can estimate whether he is profiting unduly from the allowance program. The survey instruments and field procedures will be designed to muffle the issue of landlord profits--e.g., we will not inquire directly about them--and those who are doing poorly will not hesitate to tell us so. But those who are doing well are likely to be troubled by the implications for them of revealing this fact.

We would be surprised to discover that actual property-management decisions were much influenced by the monitoring program. More likely in our view would be misrepresentation or refusal to cooperate with

---

continue to generate effective demand for that period. It is then up to the landlord to capture his share by making whatever improvements will attract allowance recipients.

*This brief exposition is less than satisfactory. See Lowry, Rydell, and de Ferranti, op. cit., WN-7711-UI, pp. 55-57, for a more adequate account of the calculations that lead to these conclusions.

interviewers.* Thus, while the real outcome of the experiment would not be much affected, our comprehension of that outcome could be. Our main protection is careful survey procedures and cross-checking of data to alert us to inaccurate responses.

We should note that periodic interviews with allowance recipients and certification of eligible structures are planned as administrative procedures that would be necessary elements of a national program; in the experiment, the interviews and building inspections may be a bit more elaborate than would be necessary and probably more careful than would be typical of a national program. We see no reason to anticipate serious experimental bias from this source.

Uniqueness of Experimental Sites

When the Supply Experiment is considered as a basis for generalization about the probable consequences of a national housing allowance program, the unit of observation is not the individual property or household but the local housing market. Within each experimental site, we may be able to detect characteristically different response patterns for classes of individuals, but all responses may be significantly influenced by the configuration of the market: its size, the compositional characteristics of its household population and its housing inventory, its cultural and economic geography, and its history.

Metropolitan areas of the United States vary considerably in these respects. Given a sample of only two such areas for the Supply Experiment, what can we say about the probable effects of a national program in other places?

Clearly, the usual methods of statistical inference are not helpful here. But we see two ways of guarding against unwarranted generalization: careful selection of the experimental sites, and structural analysis of the experimental findings.

Our plans for site selection are detailed in a separate report.** The screening procedures are complex, but basically we seek two

---

*See Adele Massell, *Compensating for Landlord Nonresponse in the Housing Assistance Supply Experiment*, The Rand Corporation, WN-8268-HUD (forthcoming).

**Housing Assistance Supply Experiment Staff, op. cit., WN-7833-HUD.

metropolitan areas that differ in ways that are likely to affect the outcome of the Supply Experiment. The characteristics that we think are critically important are central-city growth rate and the ethnic composition of the central-city population. Second-order characteristics are the incidence of rental tenure and multiple dwellings in the central-city housing inventory, and the central-city vacancy rate. Third-order characteristics are unemployment rates, welfare incidence, and poverty incidence.

Our focus on central-city characteristics reflects their greater variability among SMSAs. The particular characteristics we have selected and their weighting reflect educated judgment and the availability of 1970 Census data. Roughly speaking, our screening procedures lead us to select upper- or lower-quartile values for the characteristics of interest, bracketing the center of the SMSA distribution. Thus, our two experimental sites should be distinctively different, each representing a well-populated subset of the universe of metropolitan areas.

If experimental outcomes in these two sites are similar in all respects that are important for the evaluation of a national housing allowance policy, we think most people would agree that generalization from the experiment is reasonable despite the inapplicability of statistical tests. If the experimental outcomes differ substantially, we must be more cautious in our generalizations.

In the latter case, structural analysis may help. Given different outcomes, can we account for the differences in terms of the characteristics of our sites? This is a difficult assignment because of covariance among the characteristics. But by combining housing-market theory with our observations of the dynamics of the experiment at each site, we should be able to develop reasonably sturdy hypotheses about the critical factors at work. Then, by examining the incidence of these critical factors in the universe of SMSAs, we can estimate which are likely to respond in the pattern of Site A and which in the pattern of Site B.

The technique is helpful but not foolproof. There may be relevant market response patterns not encountered in *either* of the two experimental

sites. Our structural analysis is not subject to independent validation, though partial checks may be possible. But it seems clear to us that the information gathered from even two case studies is much better than no information at all. At a minimum, we can cast light on the prospects of a national allowance program in places that resemble the two experimental sites, and these will be numerous.

We are least comfortable about the issue of SMSA size. For reasons of economy, the experiment is to be mounted in small SMSAs, with fewer than 250,000 inhabitants. Yet the most critical housing problems are generally acknowledged to be in large SMSAs, where ethnic ghettos are extensive and the incidence of rental tenure in multiple dwellings is high. We recommend to HUD that it consider a third experimental site, consisting of a low-income neighborhood in a large metropolitan area, with enrollment in the allowance program restricted to that neighborhood.*

Duration of the Experiment

Earlier, we discussed the effects of limited duration on the expectations of allowance recipients and property owners. Here, we are concerned with a different issue: whether the supply response to a sudden increase in effective demand would work itself out to a new and stable equilibrium within the five years allowed for its observation.

There is little systematic information about response lags in the housing market, but informed observation leads us to think that a sudden change in demand is generally reflected in rents within about a year, and in output changes within two years, except for new construction and major capital improvements. We think that five years is certainly adequate to observe most of the changes that would occur.**

Moreover, we think that the policy relevance of information gained from a longer monitoring period would be slight. Within five years, either a national housing allowance program will have been proposed and

---

*See Lowry, Rydell, and de Ferranti, op. cit., WN-7711-UI, for discussion of the uses and limitations of this type of experiment, especially pp. 22-43.

**See Lowry, Rydell, and de Ferranti, op. cit., WN-7711-UI, pp. 48-53, for our scenario of the dynamics of an allowance-stimulated market.

passed into law, or it will have been decisively rejected. Insofar as the design and decision are based on experimental evidence at all, it will be on the evidence from the first few years of the experiment. Later findings may come in time to modify such a program if its principle has been accepted, but they would be unlikely to revive it if its principle has been rejected.

In any case, a firm decision about the duration of the monitoring program need not be made a priori. As data from the early years of the experiment are analyzed, we will acquire a better grasp of market response than we now have. We will also discover new issues pertinent to a national housing allowance program, for which no monitoring provisions have been made. Clearly, as we gain experience, we should reconsider not only the duration but the content of our monitoring program.

Supply Response Under Different Allowance Programs

We have designed an allowance program for the Supply Experiment, and we plan to apply the same rules to all participants at both sites. Briefly, we propose to issue rent certificates to all eligible renters, for use in partial payment of rents in certified structures; to provide equivalent mortgage interest subsidies to eligible home buyers; and to assist homeowners by monthly contributions channeled through a third party. Except for special restrictions on eligibility that may be imposed by law on the funds used for the experimental program, eligibility and the amount of the allowance are determined by the same factors: the applicant's disposable income, the size of his household, and the local cost of a defined level of housing consumption. The details are presented in Sec. III.

The allowance formula operates on the "housing-gap" principle; that is, the amount of the allowance for each household is set so as to enable the household to afford a specified level of housing consumption without unduly restricting other consumption.

We have no guarantee that a national program of housing allowances would follow either our rent-certificate format or our particular

variant of a housing-gap allowance formula. Alternatives to both are under active consideration. Can the results of the Supply Experiment, based on a specific allowance program, be generalized to apply to other variants of the housing allowance concept?

It is helpful to divide the issues. With respect to the *demand* response to housing allowances, the terms and conditions of the specific allowance program are manifestly critical. This issue is to be explored by the Demand Experiment. With respect to the *supply* response, the specific allowance formula is less critical; what counts is the result--i.e., the amount of the increase in housing expenditures, and variations in this increase among households. Finally, any special restrictions imposed on the use of allowances could be important if they focused allowance recipients' demands on a particular class of structures.

We think that the issue of the dependence of supply response on the specifics of the allowance formula can easily be overinflated. Most of the variants in housing allowance formulas are matters of more or fewer eligible households, higher or lower levels of payments, and more or less freedom for households in allocating assistance payments between housing and other goods. For a given experimental site, different allowance programs would lead to somewhat greater or somewhat smaller increments in demand for housing, and some variation in the distribution of this incremental demand among households of various sizes and incomes.

So long as it is possible to estimate, for a given allowance formula, its effects on housing demand, it should also be possible to estimate from the data gathered in the Supply Experiment how the market will respond to increased spending by allowance recipients. To be sure, the more drastic the differences between the allowance formula actually used in the Supply Experiment and the formula whose hypothetical supply consequences are to be examined, the less direct are the inferences from experimental outcomes; more reliance must be placed on analytical modeling that uses context-free parameters drawn from both the Supply and Demand Experiments. The issues entailed in such modeling are discussed below.

INTEGRATING THE DEMAND AND SUPPLY EXPERIMENTS

In the Demand Experiment, a thin sample of low-income families living in one or more large metropolitan areas will be selected for enrollment in a housing allowance program. Subsamples of the enrollees will be given allowances on different terms, and their housing choices and budgetary decisions will be monitored for a period of several years. Because the number of allowance recipients will be small relative to the population of the housing market, increased housing expenditures by allowance recipients will not add up to significant demand pressure. No measurable supply response, in the sense of a change in the price of housing services or in the quantity supplied, can be anticipated.

The Supply Experiment works the other side of the street. Using a single allowance formula, it enrolls all eligibles in two selected metropolitan areas, creating demand pressure comparable to that anticipated from a national program. It then tracks the dynamics of supply response over a period of several years, measuring changes in the price of housing services and the quantity supplied.

By analytically combining the findings of the two experiments, we can estimate how suppliers would respond to variants of housing allowance formulas not tested in the Supply Experiment in market contexts not explored by the Demand Experiment. The analytical link between these two kinds of experiment is provided by a standard market model, in which the market price of housing services is varied until a balance is struck between the quantity of housing services demanded (which decreases with price) and the quantity supplied (which increases with price).

To perform such an analysis, it is necessary first to reduce the principal quantitative findings of both the Demand and Supply Experiments to forms which are at least quasi-independent of the particular market contexts of each experiment. In the Demand Experiment, two general types of allowance formulas will be tested: formulas that aid the recipient according to his income (housing-gap principle) and formulas that aid the recipient according to the amount of rent he pays (housing-discount principle). By careful selection of recipients to cover a range of household characteristics and incomes, and by varying the parameters of both formulas, the monitors of the Demand Experiment expect to be able to map both the income and price elasticities of

housing demand by low-income households, under a variety of special restrictions on the use of allowance payments. Because the Demand Experiment will be conducted in several housing markets, the dependence of these parameters on background conditions can also be tested.

In the Supply Experiment, we propose to estimate the price elasticity of housing supply for each of 16 sectors of the housing market, distinguished by tenure, rent or value, size of structure, and neighborhood density (urban vs. rural).* These elasticities will be estimated independently in each of two housing markets selected for their structural differences, but both subjected to comparable demand stimuli. Thus, we will have a limited but powerful test of the dependence of these parameters on market configuration.

If we are able to convince ourselves that the parameters drawn from both the Demand and Supply Experiments are indeed reasonably independent of the context in which their values were estimated, they can be used to map demand and supply functions in actual or hypothetical housing markets whose structural characteristics (numbers and types of households and their incomes, numbers and types of housing units and factor costs) can be specified. The mathematical structure of such an analysis is presented in Appendix E; it could be implemented at various levels of sophistication, depending on the amount of detailed information about the pertinent elasticities obtained from the experiments, and the amount of detailed information obtainable about the structure of the housing market to be analyzed.

Such analytical integration of the findings of the Demand and Supply Experiments does not hinge on the use of the same allowance formula in both experiments--a manifest impossibility if the Demand Experiment is to test variations in allowance formulas. The market model described in Appendix E is adaptable on the demand side to either housing-gap or housing-discount allowance formulas; the supply function formally depends on factor prices and output prices, not on the allowance-modified demand function.

---

*These are the dimensions of stratification selected on a priori grounds, subject to three considerations: functional significance, sampling convenience, and reproducibility from commonly available data. Doubtless, analysis of the data will lead us to combine some of these strata and to create new strata.

We do think that it would be helpful for the Supply Experiment's allowance formula to belong to a *family* that is represented in the Demand Experiment--preferably a family that has a high probability of eventual use as the basis for a national program. The housing-gap allowance formula fits this description.* The variant of it that will be used in the Supply Experiment is scheduled for test in the Demand Experiment.

Our main reservations about the analytical integration of experimental findings do not pertain to "mixing" allowance formulas, but to the extension of supply response parameters to housing markets whose sizes and structures differ radically from those of the two experimental sites selected for the Supply Experiment. As noted earlier in this section, we are least comfortable with the issue of SMSA size, both because of untested market scale effects and because market structure varies with size, particularly with respect to the incidence of rental tenure and of multiple dwellings. While the sites under consideration for the Supply Experiment are close to national averages in these respects, they are very different from the large metropolitan areas whose housing problems preoccupy public attention. It is for this reason that we have recommended to HUD a third experimental site, consisting of a low-income neighborhood in a large metropolitan area, with enrollment in the allowance program restricted to that neighborhood. While an experiment of this type would have limited relevance to some of our research objectives (e.g., effects of the allowance program on mobility), it would enable us to gather data on supply responsiveness of the owners of deteriorated multiple dwellings in a market in which low-income renters predominate.

---

*Housing-discount formulas will also be tested in the Demand Experiment, as a necessary vehicle for measuring price elasticities of demand. Our analysis of recipient behavior under alternative allowance formulas leads us to hope that the housing-discount principle is not a serious contender for a "real" allowance program; we think it would be a mischievous incitement to price inflation and (in its pure form) would badly distribute the benefits of the allowance program. See Lowry, Ott, and Noland, op. cit., WN-8028-HUD.

Appendix A

A DYNAMIC MODEL OF THE PRODUCTION FUNCTION FOR HOUSING SERVICES

Discussions of housing policy usually reflect implicit assumptions about the characteristics of the production function for housing services. Since the assumptions are not articulated, they can and often do conflict within the course of a few pages of exposition. The greatest confusion concerns the relationship among the output of housing services, maintenance expenditures, and the pattern of housing deterioration over time. This appendix attempts to sort out these issues by means of a dynamic model of the production function for housing services.

Our production function takes the following form:

$$Q_t = Q(L_t, K_t, M_t, S_t) , \tag{A-1}$$

where Q_t = the flow of housing services at time t;
L_t = the stock of land in residential use at time t;
K_t = the stock of residential capital improvement at time t;
M_t = the flow of housing-maintenance inputs at time t; and
S_t = the flow of building-service inputs at time t.

Output and all inputs are measured in real terms (i.e., constant-dollar values). L_o and K_o are valued at acquisition cost, which for K_o is the cost of production.

These factors of production are distinguished because they differ with respect to the time lag between acquisition of the factor and its transformation into output. Empirically, they form a spectrum from the most durable (land) to the most fleeting (services of an elevator operator), so that the limits assigned to each category are necessarily arbitrary.

There is, however, a special relationship between two of these factors, the stock of capital and the flow of maintenance inputs: The productivity and longevity of housing capital are powerfully affected

by the level of maintenance it receives. Indeed, the distinction between what one chooses to call "capital" and what one chooses to call "maintenance" is largely a matter of the level of aggregation.

A paradigm will probably be more helpful than abstract exposition. Consider the heating system of a multiple dwelling. The original installation--furnace, fuel storage tanks, distribution ducts--is a capital item, K_o, which deteriorates over time but may be renovated by maintenance inputs, the two processes determining its current state, K_t. Regular cleaning of furnace filters, oiling of blowers, etc., are maintenance activities, M_t, that sustain the efficiency of the heating system and extend its useful life; occasional repairs and replacement of minor parts serve the same purposes. Eventually, it may become necessary or economical to replace a major element such as the furnace, though probably not the distribution ducts; whether this is treated as a logical extension of maintenance, M_t, or as a lumpy capital input, ΔK_t, is arbitrary.

As one aggregates over such functional systems within a building, and over groups of buildings whose replacement cycles are uncorrelated, the lumpiness of replacement items smooths out, and it is convenient to include all repairs and less-than-complete replacements in M_t, along with daily or weekly chores.

At the other end of the spectrum, we wish to distinguish service inputs, S_t. For our heating system, these would include fuel, whose consumption is immediately and fully reflected in the interior temperature of the building; the amount of heat provided (and fuel consumed) can be increased or decreased without substantially affecting the efficiency or longevity of the heating system.

The dynamics of our production function result from the interaction of K and M over time. We argue that for any given K_o there exists a level of maintenance, M_1^*, that is needed to maintain a steady flow of output, Q; this relationship is defined by

$$M_1^* = \beta K_o \ . \qquad \text{(A-2)}$$

We will call M_t^* the "good-as-new" level of maintenance associated with K_t, the amount of capital at time t. In a study of 588 project-years of data for 56 public housing projects in New York City, Rydell has shown that M_t^* increases over time, reflecting irreversible deterioration of the capital stock as it ages.* His data indicate the following functional form:

$$M_t^* = M_1^* t^{\alpha} , \qquad \alpha \simeq .1 . \tag{A-3}$$

Reflection on Eqs. (A-2) and (A-3) suggests a definition of real depreciation of capital due to age: If current inputs must be increasingly substituted for capital to maintain a given level of output, then the amount of capital lost, ∂K_t^*, can be measured by the value of the additional maintenance needed, ∂M_t^*, capitalized at the appropriate rate of interest, i:

$$\frac{\partial K_t^*}{\partial t} = - \frac{\partial M_t^*}{i \partial t} = - \frac{M_1^* \alpha t^{\alpha-1}}{i} . \tag{A-4}$$

If actual maintenance inputs, M_t, are less than M_t^*, there will be depreciation of the capital stock in addition to the depreciation that is due to age. In this case also, the value of the capital stock thus lost is measurable by the capitalized value of the expenditures on current account that would have been needed to prevent deterioration and maintain a steady flow of output. Representing cumulative depreciation due to undermaintenance as K_t^{**} and the current amount of undermaintenance as M_t^{**}, we have

$$\frac{\partial K_t^{**}}{\partial t} = - \frac{M_t^{**}}{i} = - \frac{(M_t^* - M_t)}{i} = - \frac{(M_1^* t^{\alpha} - M_t)}{i} . \tag{A-5}$$

---

*C. Peter Rydell, *Factors Affecting Maintenance and Operating Costs in Federal Public Housing Projects*, The New York City-Rand Institute, R-634-NYC, December 1970, pp. 15-27. The official (and, Rydell judges,

Combining these two forms of depreciation, we conclude that

$$\frac{dK_t}{dt} = -\frac{M_1^* \alpha t^{\alpha-1}}{i} - \frac{(M_1^* t^{\alpha} - M_t)}{i} = \frac{M_t - M_1^* \left(\frac{\alpha+t}{t^{1-\alpha}}\right)}{i} . \tag{A-6}$$

The expression in Eq. (A-6) involving α and t is not immediately recognizable; but if α is small (e.g., $\alpha \simeq .1$), the expression simplifies as t becomes large, approaching t^{α} as a limit. Using this limiting value, we can express the depreciation rate (with K_t as a base) as

$$\frac{dK_t}{K_t dt} = \frac{M_t - M_1^* t^{\alpha}}{iK_t} = \frac{M_t - M_t^*}{iK_t} . \tag{A-7}$$

We can find the value of the stock of capital remaining at time t by integrating Eq. (A-6):

$$K_t = K_o - \frac{M_t^*}{i} - \int_o^t \frac{(M_t^* - M_t)dt}{i} . \tag{A-8}$$

Finally, the useful life of the original capital, K_o, ends when it becomes cheaper to replace what remains than to continue to operate it. This will occur at some point even if there is no undermaintenance, simply due to the age effect. The maximum useful life of K_o is defined by

$$K_o - \frac{M_1^* t^{\alpha}}{i} = 0 \quad \text{or} \quad t_{max} = \left[\frac{iK_o}{M_1^*}\right]^{\alpha^{-1}} = \left[\frac{i}{\beta}\right]^{\alpha^{-1}} . \tag{A-9}$$

---

actual) policy of the New York City Housing Authority is "good-as-new" maintenance; thus an increase in maintenance expenditures over the life of a given project, when adjusted for factor-price inflation, is a change in M_t^* as defined above.

The minimum useful life of K_o would result from a policy of zero maintenance; from Eq. (A-8), with $M_t = 0$, we have

$$K_o - \frac{M_1^* t^\alpha}{i} - \int_o^t \frac{(M_1^* t^\alpha)dt}{i} = 0 \ . \qquad \text{(A-10)}$$

Unfortunately, this equation cannot be solved analytically for t; it reduces only as far as

$$t^\alpha(\alpha + 1 + t) = (\alpha + 1)\frac{i}{\beta} \ , \qquad \text{(A-11)}$$

which can, however, be solved by numerical approximation when values for the other variables and for α and β are known.

Our model includes two technological parameters. One is β, which relates the amount of maintenance needed to offset current depreciation to the original value of the capital stock. The other is α, which relates the growth of good-as-new maintenance inputs to the passage of time. The second of these parameters has been evaluated ($\alpha = .1$) by Rydell, op. cit., in a context that gives reasonable grounds for generalization. With less assurance, we can also estimate the value of β.

The difficulty is primarily that expense data are insufficiently detailed to permit us to separate thoroughly outlays which effect the capital stock, M_t, from those used for building services, S_t. Rydell's data show the following expense distribution for his standard case, a 21-year-old project in 1968 with 1,000 units, 10 buildings, and 800 square feet per unit.*

Major Expense Category	Expense per Unit (1968 $)	Percentage Change per Year of Project Age
Services	376	0.6
Repairs	125	3.4
Painting	81	5.4
Utilities	142	0
Management & other	138	0
Total	862	1.1

*Rydell, op. cit., pp. 54-55.

Utilities and management expenses revealed no significant tendency to increase with project age and may be presumed to be components of S_t. Service expenses (which include janitorial, security, grounds, and plumbing services; general supervision of the maintenance and service staff; and contributions to the pension fund) show a small tendency to increase as projects age; they are manifestly a mixture of M_t and S_t. Repair and painting expenses are the most powerfully influenced by project age, obviously belonging in M_t.

The combined total of repair and painting expenses for the standard case, $206, is thus probably on the low side of M_t^*; it will serve at least as a rough estimate for the standard case, $t = 21$. We can calculate from Eq. (A-3) that

$$M_1^* = \frac{M_{21}^*}{21^{\alpha}} = \frac{\$206}{1.356} = \$152 \ , \qquad \text{(A-12)}$$

which is about 1 percent of construction costs per unit ($14,500 in 1968 dollars).*

Another New York City data base leads to roughly the same conclusions. Audited expense data on 311 rent-controlled (pre-1947) multiple dwellings, free of housing-code violations, yield an average annual expenditure (in 1965-1967 dollars) of $135 per unit for maintenance, repairs, replacements, and improvements.** The average building in the sample was 38 years of age, consisted of 56 units with an average of 3.55 rooms, and had an estimated market value of $7,650 per unit. Its violation-free status suggests that current maintenance, at least, was adequate. If we assume, precariously, that the building has a history of adequate maintenance,

$$K_t = K_o - \frac{M_t^*}{i} \quad \text{or} \quad K_o = \$7,650 + \frac{\$135}{i} \ . \qquad \text{(A-13)}$$

---

*Rydell, op. cit., p. 57.

**Karen M. Eisenstadt, *Factors Affecting Maintenance and Operating Costs in Private Rental Housing*, The New York City-Rand Institute, R-1055-NYC, August 1972, Table 8.

Capitalizing at $i = .10$, we obtain $K_o = \$9{,}000$; and repeating the calculation of Eq. (A-12), we have

$$M_1^* = \frac{M_{38}^*}{38^{\alpha}} = \frac{\$135}{1.439} = \$94 \ , \qquad \text{(A-14)}$$

which is about 1 percent of K_o.

To summarize, then, we have estimates of our technological parameters,

$$\alpha = .1 \quad \text{and} \quad \beta = \frac{M_1^*}{K_o} = .01 \ .$$

Applying these parameters to Eqs. (A-9) and (A-11), and assuming an interest rate of 10 percent, we can solve for the maximum and minimum useful lives of residential capital, K_o. It turns out that

$$t_{max} = 10^{10} \ years \ ,$$

and

$$t_{min} \simeq 8 \ years \ .$$

In other words, a well-maintained building will last indefinitely, and a poorly maintained building can be run into the ground in only a few years.

These formulations of the maximum and minimum lives of residential capital assume that M_t^* is a function only of K_o and t. It could alternatively be argued that as the capital stock diminishes due to undermaintenance, the amount of maintenance needed to prevent further depreciation (except that part due to the passage of time) also decreases. Thus, if M_t^* is defined as the "hold-the-line" level of maintenance, a function of K_t and t, the rate of capital loss for a regime of undermaintenance would be less than the calculations above indicate.

Thus, if we replace

$$\beta = \frac{M_t^*}{t^{\alpha}K_o} = .01 \quad \text{with} \quad \beta = \frac{M_t^*}{t^{\alpha}K_t} = .01 \ ,$$

we are led to the following result* for a regime of zero maintenance:

$$K_t = K_o \left[1 - \frac{\beta t^{\alpha}}{i}\right] \exp \left[-\frac{\beta t^{1+\alpha}}{i(1+\alpha)}\right] . \qquad \text{(A-15)}$$

Because Eq. (A-15) is exponential in form, it does not lead to a minimum value for t. However, we can compare the rate of decrease in the capital stock under the two alternative formulations of β, i.e., using Eq. (A-8) and Eq. (A-15), respectively. In the table on the following page, we make this comparison and also show the behavior of the capital stock under a policy of good-as-new maintenance.

As the first column of the table shows, the effects of age on the capital stock are most pronounced in its first year of life, when it diminishes by 10.0 percent. In 10 years, age-related depreciation reduces the capital stock by 12.6 percent, and in 100 years by 15.8 percent. Under a good-as-new maintenance policy, capital is replaced in the production function by increased current outlays, so that the flow of output is constant.

If something less than good-as-new maintenance is pursued as a policy, the capital stock diminishes more rapidly and output declines as well. The second and third columns of the table show capital depreciation under a zero-maintenance policy. The second column assumes that the amount of maintenance required to prevent further deterioration increases over time even though the capital stock is shrinking. The third column assumes that the amount of maintenance required to prevent further deterioration decreases as the capital stock shrinks. Under either assumption, it is clear that a policy of zero maintenance leads

*The results given in Eq. (A-15) are an approximate solution of Eq. (A-6), using the revised concept of β. The exact solution leads to a more complicated exponential term in the equation but does not give appreciably different numerical results.

to the loss of most of a building's real value in a relatively short time.

Of course, residential-property owners seldom follow either a policy of good-as-new maintenance or a policy of zero maintenance. The policies they do choose, we think, reflect their expectations about the demand for output. Faced with declining demand in the foreseeable future, they can adjust output by decreasing maintenance outlays, losing capital but saving on current account.

Years Since Construction	Percent of Capital Stock Remaining		
	With Good-as-New Maintenance	With Zero Maintenance	
		Eq. (8)	Eq. (15)
0	100.0	100.0	100.0
1	90.0	80.9	82.2
2	89.3	69.8	73.5
3	88.8	58.4	65.5
4	88.5	46.7	58.3
5	88.2	34.9	51.7
6	88.0	22.8	45.8
7	87.8	10.5	40.6
8	87.7	--	35.8
9	87.5	--	31.6
10	87.4	--	27.8
20	86.5	--	7.4
30	85.9	--	1.9
40	85.5	--	.4
50	85.2	--	.1
100	84.2	--	--

Appendix B

ACCOUNTING FOR SUPPLY RESPONSES

Among the four major research objectives of the Supply Experiment, we expect that measuring supply responses to the experimental allowance program will be the most difficult, both in terms of data collection and in terms of analysis. To serve this objective, we propose a program of field surveys to gather longitudinal data on a sample of residential properties in each experimental site.

In order to obtain systematic evidence of additions to and deletions from the stock of housing, as well as changes in the characteristics of the existing inventory, the sample has three components:

1. A stratified random sample of tax parcels in residential use at the time of the baseline survey, prior to the commencement of the allowance program; this sample is drawn from the entire area of the experimental site.
2. A random sample of tax parcels not in residential use, but which may be converted to residential use during the experiment; this sample will be selected at baseline and is confined to the urbanized portion of the experimental site.
3. Outside the urbanized portion of the site, a sample of residential building permits that will be drawn each year after baseline, to capture evidence of additions to the housing supply in territory where they are too spotty to be efficiently sampled by the method described under Item 2, above.

Each element of the combined sample will be surveyed annually for that portion of the experimental period during which it is in residential use. The surveys will gather data on the physical characteristics of the residential structure, on the income and expenses of its operation, and on the characteristics of its owner and tenants. We expect to observe by these means a representative sample of all changes in the supply of housing services that occur during the experiment.

Presumably, the major cause of such changes will be an increase in effective demand for housing services by low-income households, generated by the allowance program. General public knowledge of the program, combined with attempts by allowance-eligible households to obtain certifiable housing units and thus qualify for allowance payments, should prompt various responses from the owners of residential property in those sectors of the market in which allowance recipients are active. Our task is to measure these responses, not merely for those housing units actually occupied by allowance recipients, but throughout the affected sectors of the market.

We emphasize that supply responses to the allowance program will not be confined to housing units actually occupied by allowance recipients. Since the allowance is attached to the eligible household rather than to any particular housing unit, owners of deteriorating residential property may seek to attract recipients by speculative housing improvements, or they may lose tenants by failure to make such improvements. Owners of well-maintained housing, on the other hand, may be able to raise rents without making improvements because of increased demand from allowance-eligible households for such units. In fact, the suppliers of housing services will not always be able to distinguish the allowance program as the source of market signals that influence their production and pricing policies, perceiving only that the demand for some kinds of housing has increased and the demand for other kinds of housing has decreased.

Changes in housing demand that occur during the experiment will be reflected in changes in housing expenditures, readily measurable for our sample of residential structures. By definition, a change in housing expenditures is accountable on the supply side to some combination of (a) changes in the flow of housing services produced and (b) changes in the price per unit of these housing services. To evaluate the results of the experimental allowance program, both for allowance recipients and others, we must distinguish these two aspects of supply response.

In Sec. VI, we argued that since changes in the flow of housing services cannot be measured directly, the next best alternative is to

measure changes in the flow of factors used in the production of housing services. This appendix provides a rigorous (i.e., algebraic) explanation of the method by which we propose to measure these input changes and shows how we can then estimate the magnitude of concurrent price changes, distinguished according to their causes and their relevance to the interests of the experiment. The empirical procedures required for each step are indicated but not detailed.

For the reader's convenience, the notation used in this appendix is summarized on the following page.

MEASURING CHANGES IN FACTOR INPUTS

For each residential structure in our sample, we propose to measure the flow of factor inputs for the year preceding the commencement of the allowance program, and to measure changes in this flow for each succeeding year. Our primary concern is to devise a measurement scheme that is sensitive to aggregate changes in factor inputs for sectors of the housing market that are likely to be substantially affected by the allowance program, changes that can be related to corresponding changes in housing expenditures within these same market sectors. The scheme will allow tracking of the fates of individual structures, and analysis of these fates may cast additional valuable light on the processes of supply response; but our emphasis is on *aggregate* supply response to market signals rather than on individual buildings or transactions between individual landlords and individual tenants.

During the base year, we propose an inventory and appraisal of all residential properties included in the sample, with land and capital improvements appraised separately. We also propose a field survey of property owners, designed to yield a full accounting of rental revenues received, maintenance and operating expenses during the base year, and real-estate taxes paid. Although information about mortgage financing will be gathered at this time, it is not used in the measurements described below.

$$V_f = P_f Q_f = i\left\{V_l + V_k\right\} + V_m + V_s \ . \qquad \text{(B-1)}$$

NOTATION

Principal Variables

V = market value of factor inputs or housing-service outputs, measured at base-year prices

P = price per unit of factor input or housing-service output

Q = number of units of factor inputs or housing-service outputs

Z = market value of land or existing capital improvements added to or removed from residential use, measured at base-year prices

W = quantity weights used in the construction of factor-price indexes

R = rental revenue received by the producers of housing services

Π = producers' markup, the ratio of sales price to cost of production

Subscripts on Principal Variables

f = generalized factors used in the production of housing services (a mixture of l, k, c, m, and s)

l = residential land

k = capital improvements in place

c = current capital improvements (new construction or alterations)

m = current inputs for building maintenance and repairs

s = current inputs for building services and operations

h = housing services

t = real-estate taxes

$g = h + t$

Parameters

i = market rate of interest on residential mortgages

r = real rate of depreciation of capital improvements

Dating the Variables

To simplify notation, variables have not been explicitly dated. Instead, base-year values are written without superscripts (e.g., P_m); test-year values are distinguished by prime superscripts (e.g., P'_m); and differences between test-year and base-year values are indicated by a preceding delta (e.g., $P'_m - P_m = \Delta P_m$).

In Eq. (B-1), we sum the market values of factors used in the production of housing services during the base year. The first term on the right-hand side measures the opportunity cost of residential land and capital in use during the base year, assuming a midyear appraisal.* Opportunity cost is defined as appraised value multiplied by the market rate of interest on residential mortgages; it is the amount that the suppliers of housing must pay annually for the use of this land and capital.**

The remaining two terms on the right-hand side of Eq. (B-1) measure outlays during the base year for maintenance and repairs, V_m, and building services, V_s. They are distinguished for reasons that will become apparent below.

The sum of these three terms, V_f, is the base against which we wish to measure changes in the flow of factor inputs during the years of the experiment. Insofar as possible, we propose to measure these changes by event recording. By so doing, we are less likely to overlook small but significant changes, and we avoid the inconsistencies that would result from direct reappraisal of capital assets under changed demand conditions.

$$V'_f = P_f Q'_f = i\left\{V'_l + V'_k\right\} + V'_m + V'_s \ . \tag{B-2}$$

Equation (B-2) sums the values, in base-year dollars, of factor inputs during the first test year. Although Eq. (B-2) is parallel in form to Eq. (B-1), the measuring procedures are more complicated. The value of each factor input must be either measured in or adjusted to base-year prices so that V'_f is comparable to V_f in "real" terms. Item-by-item, the procedures are as follows:

$$V'_l = V_l + Z_l \ . \tag{B-3}$$

$$V'_k = (1 - r)(V_k + Z_k) + V_c \ . \tag{B-4}$$

---

*This assumption simplifies the algebra, but the method is adaptable to any schedule of fieldwork.

**This point is discussed further at the end of this section.

Residential land and capital improvements are accounted for in the middle of the test year. In Eq. (B-3), Z_l represents parcels of land added to (+) or withdrawn from (-) active residential use since the base-year inventory, valued at their base-year appraisals. In Eq. (B-4), Z_k is similar, representing existing structural improvements whose use has shifted from residential to nonresidential, or the reverse. Thus if a residential building was boarded up, destroyed by fire, or converted to offices, its base-year appraised value would be deducted from V_k, and the value of its site would be deducted from V_l. (Note, however, that a vacant or partially vacant residential building, if available for rent, remains in V_k and V_l.) The last term of Eq. (B-4) records the value of residential construction and alterations completed subsequent to the base-year inventory.

Data on these occurrences could be obtained from municipal permits, required for most of the events here described. However, we also plan annual field inspection and interviews with owners and tenants to verify the completeness and accuracy of permit data.

In Eq. (B-4), the value of capital improvements, net of conversions, is adjusted for depreciation since the base-year appraisal. In Appendix A, we related the rate of capital depreciation to the age of the capital and the level of current maintenance. For mature housing, we concluded there (Eq. (A-7)) that the relationship could be approximated well by

$$\frac{dK_t}{K_t dt} = \frac{M_t - M_t^*}{iK_t} .$$

Changing to the notation used in the present appendix, and shifting from continuous time to one-year intervals, we can define the base-year rate of capital depreciation as a positive fraction, r:

$$r = \frac{V_m^* - V_m}{iV_k} . \qquad \text{(B-5)}$$

Here, V_m^* is the outlay required for "good-as-new" maintenance of the base-year capital stock, V_k. The rate of depreciation of V_k thus depends on the difference between V_m^* and actual maintenance outlays, V_m. The cost of good-as-new maintenance amounts to about 1 percent of V_k for mature buildings; it can be estimated more precisely from that part of the expense-survey data that pertains to buildings which are manifestly in good condition.*

$$V_m' = \frac{P_m}{P_m'} P_m'Q_m' = P_mQ_m' \ . \tag{B-6}$$

$$V_s' = \frac{P_s}{P_s'} P_s'Q_s' = P_sQ_s' \ . \tag{B-7}$$

The remaining terms of Eq. (B-2) are elaborated in Eqs. (B-6) and (B-7), above. Each year during the course of the experiment, the field survey of building revenues and expenses must be repeated. Maintenance and service outlays for the test years are taken from these surveys and deflated to base-year prices by appropriate price indexes (see Appendix D).

As noted in Appendix A, ordinary real-estate accounting practices do not always neatly distinguish outlays for maintenance and repairs, V_m, from outlays for building services, V_s. However, the distinction is pertinent only to our estimate of r, which is derived by differencing V_m^* and V_m. So long as the accounting is consistent over time, the classification of marginal items as V_m or V_s will have little effect on $(V_m^* - V_m)$.

This completes our expansion of Eq. (B-2), which measures the value of factor inputs during the first test year, but in base-year prices. The procedures for subsequent years are analogous. By differencing Eqs. (B-1) and (B-2), we obtain the change in factor inputs between the base and test years:

---

*See Appendix A.

$$\Delta V_f = V_f' - V_f = P_f \Delta Q_f \ . \qquad \text{(B-8)}$$

This difference may be conveniently generalized by expressing it as a ratio to the base-year value:

$$\frac{\Delta V_f}{V_f} = \frac{P_f \Delta Q_f}{P_f Q_f} = \frac{\Delta Q_f}{Q_f} \ . \qquad \text{(B-9)}$$

Thus, without ever having to specify exactly in what units of account we measure factor inputs, we have derived *the relative change in real factor inputs for the first year of the experiment*. We can thus determine by what percentage total factor inputs have increased or decreased as a result of the allowance program and/or concurrent background events.

Three features of this accounting for factor inputs should be emphasized because they bear directly on the interpretation of the results.

First, the contribution of residential land and structural improvements is measured by their opportunity cost: the market rate of interest on residential mortgages multiplied by the base-year appraised value of residential real estate. This opportunity cost is analogous to explicit payments for other factor inputs in the sense that the market rate of interest is what an investor must pay for the use of residential land and capital improvements. Of course, the actual rates of interest on existing mortgage debt may be higher or lower than the current rate, depending on money-market conditions at the time the debt was incurred. If so, building owners will have capital gains or losses that would be realized if they sold their properties on the current market. We are not concerned with that history, only with future events; so we need not take into account the terms of existing mortgage instruments or the fraction of appraised value that is covered by outstanding mortgage debt. These considerations are irrelevant to our attempt to measure "real" factor inputs.

Second, it has been suggested to us that Eq. (B-1) should contain an additional term, rV_k, measuring capital consumption during the base year and treating it as a factor input:

$$V_f = P_f Q_f = i\left\{V_l + V_k\right\} + rV_k + V_m + V_s \ . \qquad \text{(B-10)}$$

Instead, we treat depreciation of capital improvements only as disinvestment which reduces opportunity cost in subsequent years (see Eq. (B-2)).

The accounting for capital consumption suggested in Eq. (B-10) has a surface plausibility, but it confuses the measurement of factor inputs with cash-flow accounting. The easiest way to demonstrate the illogic of treating rV_k as a current factor input is to explore the implications of this method of accounting. As we have shown in Appendix A, the rate of depreciation is a function of the age of capital and the level of its maintenance; these propositions are reflected in Eq. (B-5), repeated below in slightly different form:

$$rV_k = \frac{V_m^* - V_m}{i} \ . \qquad \text{(B-11)}$$

From this equation, we can see that there is a tradeoff between V_m and rV_k which is independent of other terms in Eq. (B-10). If $V_m = V_m^*$, then $rV_k = 0$; at the other extreme, if $V_m = 0$, $rV_k = V_m^*/i$. We can simplify by combining V_m and rV_k into a single expression:

$$rV_k + V_m = \frac{V_m^* - V_m}{i} + V_m = \frac{V_m^* - (1 - i)V_m}{i} \ . \qquad \text{(B-12)}$$

Substituting this last expression into Eq. (B-10) shows that under such an accounting system, an *increase* in current maintenance outlays, V_m, would be counted as a *decrease* in total factor inputs--a nonsensical result. *Capital consumption is not a real factor input on current account.*

The final point to be made about our accounting for changes over time in the flow of factor inputs is that the choice and staging of time intervals is necessarily arbitrary. In our scheme, stock accounts (land and capital improvements) are compiled at midyear; flow accounts (maintenance and service inputs, depreciation allowances) are compiled

at year-ends. This particular choice simplifies the algebra and poses no special difficulties in scheduling fieldwork; but the accounting could be adapted to any reasonable alternative that provided more or less concurrent data on capital and current accounts.

RELATING FACTOR INPUTS TO HOUSING-SERVICE OUTPUTS

In Sec. II of this report, we argue that the output of housing services cannot be rigorously defined or cardinally measured in physical units. If this is so, the relationship between factor inputs and housing-service outputs is necessarily ambiguous. Given that we can measure changes in "real" factor inputs, can we say anything specific about the resulting changes in the flow of housing services?

As a point of departure, we may take the conventional model of the firm in long-run equilibrium. Given the demand for output and the prices of the several factors of production, a firm which has achieved equilibrium will be using the most efficient combination of factors for the desired level of output. If such a firm then attempts to change the level of output in response to altered demand conditions, there will be a period of time in which the factor mix is less than optimal for the new level of output because not all factors are instantaneously variable. Furthermore, the new equilibrium position, when achieved, may be either more or less efficient than the old one.

If, as we expect, the allowance program primarily affects market sectors in which deteriorating housing is the norm, this implies that most producers of housing services in these sectors are *not* in equilibrium during the base year, but have been systematically reducing their levels of output from those for which their buildings were initially designed. They will have accomplished this by reducing those factor inputs that are easy to vary--building services and maintenance outlays--and by abstaining from capital replacement as the building deteriorates.

If, during the base year, our producers are operating with a suboptimal factor mix in their attempt to adapt to a lower level of output, an increase in the demand for output will enable them to raise their sights, moving toward the optimal factor mix of the original design.

In this phase of upward readjustment, the efficiency of the production process should increase.

With a large increase in demand, producers might seek to increase output above the level that is optimal for the existing stock of capital. To do so, they could in the short run add building services, presumably subject to declining marginal product. *In this phase of upward readjustment, the efficiency of the production process would decrease.* Given more time, the producers could add capital improvements as well as maintenance and service inputs, achieving something like a proportional increase in all factors. *As compared with the short-run readjustment, the efficiency of the production process would increase as a more nearly optimal factor mix was achieved.*

If these three phases of readjustment to increased demand occur strictly in sequence, they imply first declining, then rising, then declining average costs. But they need not occur in strict sequence; certainly the first two could be telescoped in time, given a sharp increase in demand at the beginning of the experiment. Without knowing a good deal more than we do about the technology of housing deterioration and repair, we cannot judge which effect would dominate at any particular time.

It is possible that the experiment itself will enable us to learn something about the relationship between real input changes and real output changes. In Appendix C, we show how regression techniques might be used to estimate the parameter γ in a production function of the form $Q_h = (Q_f)^{\gamma}$. In the meantime, however, our best assumption seems to be

$$\frac{\Delta Q_h}{Q_h} = \frac{\Delta Q_f}{Q_f} . \qquad \text{(B-13)}$$

In other words, we provisionally propose that the flow of housing services changes proportionally to the flow of factor inputs. This assumption is used below in estimating changes in the market price of housing services during the course of the housing allowance experiment.

MEASURING CHANGES IN THE PRICE OF HOUSING SERVICES

In the first section of this appendix, we presented a method for estimating changes in the flow of factor inputs between the base year and each subsequent year of the housing allowance experiment. Given those estimates, and the identity postulated in Eq. (B-13) above, we can also estimate the concurrent change in the price of housing services.

$$R_h = R_g - R_t = P_h Q_h \ . \tag{B-14}$$

Gross rental revenue, R_g, can be divided into a payment for housing services, R_h, and a tax, R_t, that the landlord must deduct from his gross revenue. While the tax supports some services that the consumer associates with housing, the amount of the tax bears only a tenuous relationship to the amount of service delivered; the supplier cannot purchase more or less public services by offering to pay more or less taxes.

Also, the payment for housing services whose production is under landlord control, R_h, can be defined as $P_h Q_h$. We propose to observe both rental revenue and real-estate tax payments in the base year and the test year; these data enable us to estimate R_h and R_h' as residuals of rental revenue after deduction of taxes. Differencing them gives us ΔR_h, the change in aggregate payment for housing services, which we decompose as follows:

$$\Delta R_h = \Delta R_g - \Delta R_t = \Delta P_h Q_h + \Delta P_h \Delta Q_h + P_h \Delta Q_h \ . \tag{B-15}$$

Dividing Eq. (B-15) by Eq. (B-14), we obtain

$$\frac{\Delta R_h}{R_h} = \frac{\Delta P_h Q_h}{P_h Q_h} + \frac{\Delta P_h \Delta Q_h}{P_h Q_h} + \frac{P_h \Delta Q_h}{P_h Q_h} = \frac{\Delta P_h}{P_h}\left[1 + \frac{\Delta Q_h}{Q_h}\right] + \frac{\Delta Q_h}{Q_h} \ . \tag{B-16}$$

Observing values for ΔR_h and R_h, and using the identity postulated in Eq. (B-13), we can solve for the relative increase in the price of housing

services between the base and test years:

$$\frac{\Delta P_h}{P_h} = \left(\frac{\Delta R_h}{R_h} - \frac{\Delta Q_h}{Q_h}\right)\left(1 + \frac{\Delta Q_h}{Q_h}\right)^{-1} . \qquad \text{(B-17)}$$

We have noted that this estimate depends on Eq. (B-13), which asserts that the percentage change in the output of housing services is identical with the percentage change in factor inputs. If, in fact, the attempt to increase output encountered diminishing returns, i.e., if

$$\frac{\Delta Q_h}{Q_h} < \frac{\Delta Q_f}{Q_f} ,$$

then our accounting method would lead us to underestimate $\Delta P_h/P_h$. If increasing returns were encountered, we would overestimate this relative change in the price of housing services. Appendix C proposes a supplementary analysis that would detect these conditions. Such errors, however, would have no effect on the further accounting procedures presented below, or on the interpretation of their results.

ACCOUNTING FOR PRICE CHANGES

The price changes measured by Eq. (B-17) could come from any of several sources. They may reflect changes in the prices of external factors of production purchased currently by the suppliers of housing services (ΔP_m, ΔP_s, or ΔP_c); changes in the opportunity cost of capital as indicated by the market rate of interest (Δi); or changes in the owners' profits, net of the opportunity cost of their stock of capital ($\Delta \Pi$). For the housing allowance experiment, it is important to distinguish among these several price changes because they have different implications for the costs of a national program. The total price change can be decomposed as follows:

$$\frac{\Delta P_f}{P_f} = \frac{\Delta i}{i}\left\{\frac{i(V_l' + V_k')}{V_f'}\right\} + \frac{\Delta P_c}{P_c}\left\{\frac{iV_c'}{V_f'}\right\} + \frac{\Delta P_m}{P_m}\left\{\frac{V_m'}{V_f'}\right\} + \frac{\Delta P_s}{P_s}\left\{\frac{V_s'}{V_f'}\right\} . \qquad \text{(B-18)}$$

Each term on the right-hand side of Eq. (B-18) accounts for a factor-price change (relative to the base-year factor price), weighted by that factor's share in total factor costs (current quantities in base-year prices). The weights, segregated by brackets, are all available from previous calculations; excluding the second term, the numerators of the weights sum to the common denominator, V'_f.

The second term is needed to distinguish the effects of rising construction costs for capital improvements from the effects of rising interest rates. The reader will recall from Eq. (B-4) that capital improvements enter V'_k at the end of the year in which they occur, valued in base-year rather than current prices. Consequently, a separate term is needed to capture changes in construction costs. Actually, complete accounting would require a third term of the form

$$\frac{\Delta i}{i} \cdot \frac{\Delta P_c}{P_c}\left\{\frac{i(V'_c P'_c Q'_c)}{(V'_f)2}\right\},$$

which we here neglect on the grounds that it would be very small. For similar reasons, we neglect the minor disparity in the weighting scheme noted above; it could, however, be remedied by rescaling the first two bracketed weights.

Once factor-price changes are accounted for, there remains a possible residual, the change in owners' rate of profit. It is convenient to measure the rate of profit as a markup on the cost of production; e.g., for the base year,

$$\Pi = \frac{R_h}{V_f} = \frac{P_h Q_h}{P_f Q_f} . \qquad \text{(B-19)}$$

With Π so defined, we can write

$$\frac{\Delta P_h}{P_h} = \frac{\Delta P_f}{P_f} + \frac{\Delta \Pi}{\Pi} + \frac{\Delta P_f \Delta \Pi}{P_f \Pi} . \qquad \text{(B-20)}$$

The last term will be small and can be neglected. We can then estimate the change in markup rate by subtraction, i.e.,

$$\frac{\Delta\Pi}{\Pi} = \frac{\Delta P_h}{P_h} - \frac{\Delta P_f}{P_f} . \qquad \text{(B-21)}$$

If $\Delta\Pi/\Pi$ is positive, this indicates that part of the measured increase in the price of housing services is attributable to an increase in the producers' markup on factor inputs. If it is negative, the producers are absorbing part of the inflation in factor prices. Since the change in the markup rate is observed ex post, we cannot tell for sure whether the outcome reflects a planned change in the markup rate or simply miscalculation of costs or of demand for output. But if the change is large, we can reasonably infer that it was planned.

If the base-year situation were one in which the production of housing services was decreasing (i.e., owners were reducing services and disinvesting through undermaintenance), this is prima facie evidence that Π was then too low to compensate for entrepreneurial risk; it might even have been less than unity. In this case, we would expect a threshold effect in supply responsiveness: Not until the anticipated value of Π exceeded some minimum value, at least greater than unity, would owners decide to increase their production of housing services. Thus, for housing that has been allowed to deteriorate, we would not expect to find an increase in output without an accompanying increase in the markup rate. The question of interest is, How large will this increase be?

DECOMPOSING CHANGES IN RENTAL REVENUE

We are now in a position to determine how the increased rental revenue resulting from our housing allowance experiment divides into payments for increased housing services, for increased real-estate taxes, for factor-price changes, and for owners' profits. We begin with the observed change in gross rental revenue, obtained by differencing reported rent receipts in the base and test years, which we

divide into three components:

$$\Delta R_g = \Delta R_t + \Delta V_f + \Delta P_h Q'_h \ . \qquad \text{(B-22)}$$

The first term on the right-hand side, ΔR_t, is the change in real-estate taxes, obtained directly from fieldwork (by differencing reported real-estate tax liabilities in the base and test years). The value of the second term, ΔV_f, was calculated in Eq. (B-8); it measures the increase in real factor inputs over the same interval. The last term, $\Delta P_h Q'_h$, can then be derived as a residual; it is the portion of the rent increase absorbed by the various price increases that occurred between the base and test years.

This last term can be further decomposed with the aid of Eqs. (B-18) and (B-21). Simplifying the notation of Eq. (B-18) by defining the bracketed factor-price weights as W_i, W_c, W_m, and W_s, respectively, and substituting for $\Delta P_f/P_f$ from Eq. (B-21), we have

$$\frac{\Delta P_h}{P_h} = \frac{\Delta i}{i} W_i + \frac{\Delta P_c}{P_c} W_c + \frac{\Delta P_m}{P_m} W_m + \frac{\Delta P_s}{P_s} W_s + \frac{\Delta \Pi}{\Pi} \ . \qquad \text{(B-23)}$$

This equation can be normalized, multiplying through by $P_h/\Delta P_h$, so that each term on the right is expressed as a fraction of the left-hand term. The normalized version can then be used to decompose the last term of Eq. (B-22):

$$\Delta P_h Q'_h = \Delta P_h Q'_h \frac{P_h}{\Delta P_h} \left\{ \frac{\Delta i}{i} W_i + \frac{\Delta P_c}{P_c} W_c + \frac{\Delta P_m}{P_m} W_m + \frac{\Delta P_s}{P_s} + \frac{\Delta \Pi}{\Pi} \right\} . \qquad \text{(B-24)}$$

The multiplicative term preceding the bracket reduces to $P_h Q'_h = V'_h$. Distributing V'_h, and substituting into Eq. (B-22), we have the complete decomposition of the increase in rental revenue:

(a) (b) (c) (d) (e) (f) (g)

$$\Delta R_g = \Delta R_t + \Delta V_f + \frac{\Delta i}{i} W_i V'_h + \frac{\Delta P_c}{P_c} W_c V'_h + \frac{\Delta P_m}{P_m} W_m V'_h + \frac{\Delta P_s}{P_s} W_s V'_h + \frac{\Delta \Pi}{\Pi} V'_h \ . \qquad \text{(B-25)}$$

The terms on the right-hand side have been labeled for ease of reference. If Eq. (B-25) is normalized by multiplying through by $(100/\Delta R_g)$, they may then be interpreted as percentages of the total increase in rental revenues (expenditures) attributable to changes in

a. Real-estate taxes;
b. The quantity of factor inputs;
c. The market rate of interest;
d. Prices of capital-improvement inputs;
e. Prices of maintenance and repair inputs;
f. Prices of building-service inputs; and
g. Producers' markup on factor costs.

Thus, the housing allowance experiment can be designed to reveal how the benefits of an added dollar of housing expenditures are divided up by market processes.

The direct beneficiary of Share (a) is the municipal fisc; in a national program of housing allowances, the resulting increase in real-estate taxes would be considerable--on the order of 15 to 25 percent of the increase in housing expenditures--unless effective tax rates on market value were lowered.

Share (b) is the only direct benefit that accrues to tenants. Strictly speaking, it is the increase in the quantity of resources used to produce housing services; whether the physical output of housing services increases commensurately is, we have argued, a metaphysical question.

Share (c) is received by the owners of residential property but would be passed on by them to suppliers of mortgage funds when existing mortgages were refinanced or new indebtedness was incurred. Under our experimental conditions, changes in the market rate of interest would be determined mostly by events in a much broader capital market; under a national program of housing allowances, it is possible that increased demand for mortgage funds would drive up the market interest rate.

Shares (d), (e), and (f), in the experimental context, reflect inflationary forces in the economy in general. Depending on the scale of the experiment, they might also include inflation directly attributable to the housing allowance payments; the other side of this effect, however, would be an increase in consumer incomes which should in some degree lessen the need for housing allowances.

Share (g) is also received by the owners of residential property. It is their compensation for undertaking to increase the output of housing services. We expect this share to rise sharply at the beginning of the experiment, then gradually decline. In a demand-stabilized market, the producers' markup rate, Π, should eventually settle at a level adequate to compensate for normal risks.

Overall, the responsiveness of housing supply to an increase in effective demand can be judged by the magnitude of Share (b), i.e., by the percentage of the incremental expenditure for housing that went for real increments of output. A more discriminating judgment, however, would certainly net out fiscal recapture through real-estate taxes and background inflation, leaving only program-caused inflation in factor prices and changes in owners' profits as relevant inefficiencies.

Appendix D describes the sources of data and the procedures we plan to use to construct factor-price indexes for each experimental site. Most of the data are collected by existing statistical systems, not only for the candidate experimental sites, but for a number of other metropolitan areas within the same geographic region. For each factor used to produce housing services, the average price change within the larger region is a measure of background inflation, unrelated to the market disturbances created by the experimental allowance program.

Thus, netting out nonlocal background inflation in factor prices enables us to focus on site-specific inflation of factor prices as a possibly adverse effect of the allowance program. In principle, site-specific inflation, if any, will reflect some combination of (1) allowance-induced changes in housing demand; (2) changes in housing demand that are independent of the allowance program; (3) changes in demand within other sectors of the local economy that use the same factors of production; and (4) possibly even local changes in supply

conditions two or more steps removed from the production of housing services.

In fact, we do not expect either the allowance program or these other events to have a significant influence on local factor prices; we expect that the needs of policy analysis will be quite adequately served by a fortiori argument, i.e., that the total observed site-specific inflation is the maximum that could have been caused by the allowance program. However, in the event that the amount of site-specific factor-price inflation is large, we see at least limited possibilities for further analysis to distinguish the direct responsibility of the allowance program. The general approach is described in Appendix D.

Appendix C

ESTIMATING PARAMETERS OF THE PRODUCTION FUNCTION FOR HOUSING SERVICES

In Appendix B, we proposed a method for measuring changes in the price of a unit of housing services that entails a strong assumption about the characteristics of the production function--i.e., that the flow of housing services changes proportionally with the flow of factor inputs, or

$$\frac{\Delta Q_h}{Q_h} = \frac{\Delta Q_f}{Q_f} . \qquad \text{(C-1)}$$

An assumption was necessary because we do not think that the flow of housing services can be rigorously defined or directly measured in physical units; the particular assumption of constant returns to scale was chosen because it seems likely to be approximately correct for the sectors of the housing market with which we are principally concerned.*

It is possible, though not certain, that the experiment itself will enable us to estimate the relationship between real input changes and real output changes in the range of policy interest. The estimating procedure is based on three simple ideas: First, if for some buildings at our experimental site we observe no change in the quantity of factor inputs, we can reasonably conclude that the quantity of output has not changed; for these buildings, the relative change in the price per unit of output would then be equal to the relative

---

*See Appendix B, "Relating Factor Inputs to Housing Service Outputs." In any event, the most important results of that analysis, the decomposition of changes in rental revenue into producers' costs and profits, are independent of this assumption.

change in rental revenue, or

$$\frac{\Delta P_h}{P_h} = \frac{\Delta R_h}{R_h} \quad \text{if} \quad \frac{\Delta Q_h}{Q_h} = 0 \ . \tag{C-2}$$

Second, if the price change thus measured applies equally to buildings whose factor inputs *were* observed to have changed, we could use this information to estimate the relative change in output for these buildings; from the identity $R = PQ$, we can derive

$$\frac{\Delta Q_h}{Q_h} = \left\{\frac{\Delta R_h}{R_h} - \frac{\Delta P_h}{P_h}\right\} \left\{1 + \frac{\Delta P_h}{P_h}\right\}^{-1} \ . \tag{C-3}$$

Finally, we could compare these estimates of output changes with measured changes in factor inputs for each building to estimate the ratio (γ) of output changes to input changes:

$$\frac{\Delta Q_h}{Q_h} = \gamma \frac{\Delta Q_f}{Q_f} \ . \tag{C-4}$$

This estimate could then replace our assumption in Appendix B of constant returns to scale ($\gamma = 1$).

Since it is unlikely that there will be many buildings whose measured quantities of inputs will remain constant during the experiment, the method of estimating the relationship between real input changes and real output changes cannot be quite as simple as the paradigm above suggests. However, we can nonetheless use buildings with only small changes in factor inputs to inform us about price changes, and buildings with large changes in inputs to inform us about the characteristics of the production function. Regression techniques enable us to do both steps simultaneously and obviate the need for a set of buildings for which only price changes occur.

Below, we sketch a procedure for estimating the parameters of a log-linear production function whose variables are the four classes of

factor inputs defined for the accounting system described in Appendix B. The procedure also provides an estimate of the relative change in the price per unit of output (housing services). The data required by this procedure are the same as those required by the accounting system presented in Appendix B.

NOTATION

R_h = rental revenue received by producers of housing services, net of real-estate taxes.

P_h = price per unit of housing-service output, net of real-estate taxes.

Q_h = number of units of housing-service output.

Q_f = number of units of (generalized) factor-inputs.

Q_c = number of units of capital inputs.*

Q_l = number of units of land inputs.

Q_m = number of units of maintenance inputs.

Q_s = number of units of building-service inputs.

γ, α, β, λ, δ = parameters of housing-service production function.

As in Appendix B, the quantities of factor inputs are measured for each building by deflating reported and imputed expenditures for each factor by an appropriate index of factor prices.

THE PRODUCTION FUNCTION

In our introductory exposition, as in Appendix B, we postulated a very simple production function whose implicit form is

$$Q_h = (Q_f)^{\gamma} = (Q_c + Q_l + Q_m + Q_s)^{\gamma} . \qquad \text{(C-5)}$$

With this production function, an increase in output requires

*Equivalent to $(Q_k + Q_c)$ as defined in Appendix B.

$$Q_h + \Delta Q_h = (Q_f + \Delta Q_f)^{\gamma} = (Q_f)^{\gamma} + \gamma \Delta Q_f (Q_f)^{\gamma - 1} + \ldots . \qquad \text{(C-6)}$$

Collecting the small second-order terms of the binomial expansion of the right-hand side of Eq. (C-6) into a single variable X, dividing by Eq. (C-5), and subtracting 1 from each side, we arrive at the operationally convenient form,

$$\frac{\Delta Q_h}{Q_h} = \gamma \frac{\Delta Q_f}{Q_f} + X \ . \qquad \text{(C-7)}$$

The parameter γ in Eq. (C-7) could readily be estimated by the procedures described below, and a single-parameter production function may turn out to be adequate for practical purposes. However, using the same data, we may be able to estimate the parameters of a more detailed production function, one offering greater insight into the production process:

$$Q_h = Q_c^{\alpha} \, Q_l^{\beta} \, Q_m^{\lambda} \, Q_s^{\delta} \ . \qquad \text{(C-8)}$$

The production function given by Eq. (C-5) implies perfect substitutability of the several factors, which may be approximately correct over a limited range of factor mixes but clearly cannot be true for extreme cases. The production function given by Eq. (C-8) implies that factors are imperfectly substitutable, with diminishing returns to any one factor when the others are held constant. With a production function of this form, relative changes in outputs and inputs are given by

$$\frac{\Delta Q_h}{Q_h} = \alpha \frac{\Delta Q_c}{Q_c} + \beta \frac{\Delta Q_l}{Q_l} + \lambda \frac{\Delta Q_m}{Q_m} + \delta \frac{\Delta Q_s}{Q_s} + Z \ , \qquad \text{(C-9)}$$

where Z represents a collection of small second-order terms of the binomial expansion of each $(Q_i + \Delta Q_i)$ and small cross-product terms involving more than one ΔQ_i .

If we can estimate the parameters of Eq. (C-9), we are then able to describe the separate contributions of changes in each factor-input.

ESTIMATING THE PARAMETERS

Starting from the definition of net rental revenue,

$$R_h = P_h Q_h \ , \tag{C-10}$$

it can be shown that a change in rental revenue is equivalent to:

$$\frac{\Delta R_h}{R_h} = \frac{P_h \Delta Q_h + \Delta P_h Q_h + \Delta P_h \Delta Q_h}{P_h Q_h}$$

$$= \frac{\Delta P_h}{P_h} + \left\{1 + \frac{\Delta P_h}{P_h}\right\} \frac{\Delta Q_h}{Q_h} \ . \tag{C-11}$$

The production relationship given by Eq. (C-9) enables us to substitute inputs for outputs in Eq. (C-11) to obtain

$$\frac{\Delta R_h}{R_h} = \frac{\Delta P_h}{P_h} + \left\{1 + \frac{\Delta P_h}{P_h}\right\} \left\{\alpha \frac{\Delta Q_c}{Q_c} + \beta \frac{\Delta Q_l}{Q_l} + \lambda \frac{\Delta Q_m}{Q_m} + \delta \frac{\Delta Q_s}{Q_s} + Z\right\} . \tag{C-12}$$

This notation may be simplified by letting

$$r = \frac{\Delta R_h}{R} \ ,$$

$$q_i = \frac{\Delta Q_i}{Q_i} \ ,$$

$$a_0 = \frac{\Delta P_h}{P_h} \ ,$$

$$a_1 = (1 + a_0)\alpha \ ,$$

$$a_2 = (1 + a_0)\beta \ ,$$

$$a_3 = (1 + a_0)\gamma \text{ , and}$$
$$a_4 = (1 + a_0)\delta \text{ .}$$

Then Eq. (C-12) can be written in a form suitable for estimation by multiple linear regression,

$$r = a_0 + a_1 q_c + a_2 q_l + a_3 q_m + a_4 q_s + \varepsilon, \qquad \text{(C-13)}$$

where r and the q_i are observations for individual buildings, and ε is the error term, including Z.

If the regression enables us to estimate the intercept a_0 and coefficients a_i of Eq. (C-13) with reasonable precision, they can be used in turn to estimate the parameters of the production function given by Eq. (C-8):

$$\hat{\alpha} = \frac{\hat{a}_1}{1+\hat{a}_0} \text{ ,}$$
$$\hat{\beta} = \frac{\hat{a}_2}{1+\hat{a}_0} \text{ ,}$$
$$\hat{\gamma} = \frac{\hat{a}_3}{1+\hat{a}_0} \text{ , and} \qquad \text{(C-14)}$$
$$\hat{\delta} = \frac{\hat{a}_4}{1+\hat{a}_0} \text{ .}$$

The procedure also yields an estimate $\hat{a}_0$ of the average change in the price per unit of housing services across the sample.

While this procedure promises far better estimates of the parameters of the housing-service production function than are now available, we should also note several possible sources of inconsistency or bias in these estimators.

First, there may be some bias in the estimators of the intercept $\hat{a}_0$ and coefficients $\hat{a}_i$. The error term of Eq. (C-13) is known to include Z, the collection of second-order terms of the binomial expansion

whose result is given in Eq. (C-9), and of the small cross-product terms involving more than one ΔQ_i from the same operation. As a consequence, the error term may be correlated with the independent variables, a condition that leads to inconsistent* estimators.

Second, even if the $\hat{a}_0$ and $\hat{a}_i$ are unbiased maximum-likelihood estimates of the true parameters a_0 and a_i, our estimators of input elasticities ($\hat{\alpha}$, $\hat{\beta}$, $\hat{\lambda}$ and $\hat{\delta}$) are likely to be biased because each is a nonlinear function of two of the regression parameters. The nonlinear transformations entailed in Eqs. (C-14) would give rise to bias even if only one regression parameter were involved; when the transformation involves two regression parameters, their possible covariance is also a problem.

Finally, some of our colleagues argue that the estimators of input elasticities obtained by this procedure (or even by a procedure based on a strictly cross-sectional model such as a log transformation of Eq. (C-8)) are subject to simultaneous-equations bias; others are unconvinced, and the conclusions below do not reflect this argument.

CONCLUSIONS

To implement the procedure described above, we need only data on relative changes in rental revenue and relative changes in real factor-inputs (factor-expenditures in constant dollars) for individual properties. However, our data-set must have the following characteristics:

1. The relative change in rental revenue, $\Delta R_h/R_h$, must vary substantially among properties.
2. The relative changes in each factor input, $\Delta Q_i/Q_i$, must vary substantially among properties.
3. The ratios of relative changes among factor inputs must differ substantially among properties.

If the third condition does not prevail--that is, if producers vary all inputs proportionally--the multiple-factor production function is

*An inconsistent estimator is one whose sampling distribution does not converge on the parameter being estimated as sample sizes are increased.

not estimable even if it is appropriate; but we can still estimate the parameter γ of the single-factor production function given by Eq. (C-5). In this case, the second condition is simply that relative changes in total factor inputs, $\Delta Q_f/Q_f$, must vary substantially among properties.

We expect the Supply Experiment to produce data satisfying the first two conditions. At the beginning of the experiment, we know that residential properties will vary widely in the amounts of revenue they receive, in physical condition, and in quantities of factor inputs they use. The experimental allowance program will establish a minimum standard for certifiability of housing units; since allowance payments are conditional on the occupancy of certified housing, the program should increase the demand for certifiable housing and decrease the demand for housing below the level of certifiability. Owners of the latter type of housing can expect declining revenues (more vacancies) unless they improve their properties to the standard of certifiability.

For properties already above the standard, no improvements will be needed to enable them to participate in the allowance-stimulated market; and the increase in demand for such housing will enable them to raise their rents to match the increase in market price, i.e.,

$$\frac{\Delta R_h}{R_h} = \frac{\Delta P_h}{P_h} . \qquad \text{(C-15)}$$

For properties below the standard, the owners will be able to obtain the higher price per unit of services, $(P_h + \Delta P_h)$, only if they also increase their outputs. The necessary increase in output, ΔQ_h, depends on how far below standard a property is initially, so it should vary among properties whose owners elect to enter the allowance-supported market from below. The revenue increase each thus obtains will also vary, since it will reflect quantity changes as well as the price change that motivated the housing improvement:

$$\frac{\Delta R_h}{R_h} = \frac{\Delta P_h}{P_h} + \left\{1 + \frac{\Delta P_h}{P_h}\right\} \frac{\Delta Q_h}{Q_h} . \qquad \text{(C-16)}$$

Finally, if these varying amounts of output change are undertaken, they will certainly require varying amounts of input change. Thus, both our first and second conditions for implementing the production-function analysis should be satisfied by data from our sample of monitored properties.

However, the scenario we have just described suggests that the best sample of properties for this purpose would consist of (1) those beginning the experiment just above the standard of certifiability (price change, no output change); and (2) those beginning the experiment below standard but reaching the standard during the period of observation (both price and output change). Properties initially below standard that never reach the standard are never permitted to participate in the submarket in which the price per unit of services is $(P_h + \Delta P_h)$; indeed, in their submarket, price is more likely to decline. Properties initially well above standard are unlikely to attract allowance-stimulated demand, because the minimum quantity of housing services offered to any one consumer is large; even at the preallowance price, and even with an allowance-augmented budget, few program participants will seek such housing, and output in this sector of the market is unlikely to change.

With the more limited sample described above, we have a good theoretical basis for assuming that $(P_h + \Delta P_h)$ is indeed the relevant submarket price for a unit of housing services, so that individual households in that submarket who spend different amounts for housing are in fact buying different quantities. Of course, a "market price" is always an abstract concept with no precise empirical correlative; our measure of its change, $(a_0 = \Delta P_h/P_h)$, is as close as anyone is likely to get.

Finally, we should comment on the characteristics of the production function for housing services whose parameters are to be estimated by the procedure described above. The result of that analysis

will, of course, be an "average" production function for the population from which our sample of properties is drawn and will be pertinent only to production possibilities that can be implemented within the time span of our period of observation. Assuming that all producers in our sample face the same set of factor prices, the technical possibilities for increasing output faced by each producer will undoubtedly differ from those of his colleagues.

This does not necessarily mean that their production functions differ in the sense that $(\alpha, \beta, \lambda, \delta)$ take on radically different values in each case. It may mean simply that each producer, because of past irreversible decisions, is confined to a subspace of the production function. If he begins the period of observation with $Q_c = X$, in the short run he is foreclosed from production methods that use either $2X$ or $X/2$ to achieve the newly desired quantity of output. It is precisely the likelihood of this circumstance that leads us to hope that our data set may conform to Condition 3, above. The same circumstance leads us to expect that producers' rates of profit, as defined in Appendix B, will vary because their accessible techniques for producing a given amount of housing services will vary in efficiency.

Appendix D

ADJUSTING FOR REGIONAL AND LOCAL PRICE CHANGES

Section VI discusses in broad outline the way in which factor-price changes--changes in interest rates and in the prices paid for building improvements, maintenance inputs, and building services--will be accommodated in the analysis of supply response. Even if no other forces were at work, background inflation over the five-year period of the experiment could, if not properly accounted for, confound our interpretation of the experimental results.

In this appendix we present a three-step plan for measuring changes in the price of factor inputs, and for separating local from regional effects and allowance-induced from other local effects. First, we must measure the overall price change at each experimental site, so that expenditures for inputs during the experiment can be calculated in base-year dollars. Second, we must construct price indexes by which changes in regional, or background, price levels can be separated from the changes observed in local price levels. Third, we must devise a method for decomposing, with as much precision as possible, the local change--after accounting for background inflation--into allowance-induced and non-allowance-induced components.

MEASURING CHANGES IN THE PRICE OF FACTOR INPUTS AT THE EXPERIMENTAL SITES

As explained in Appendix B, it will be necessary to measure the overall change in the price of input factors used by each supplier of housing services at each experimental site. For analytical convenience, we propose to divide factor-price changes into four components: interest rates (the opportunity cost of capital), the price of capital improvements, the price of maintenance inputs, and the price of building services, each weighted by its share in total factor costs at baseline.* The weights are shown in brackets in the following equation:

*See Eq. (B-18) in Appendix B and its associated text.

$$\frac{\Delta P_f}{P_f} = \frac{\Delta i}{i}\left\{\frac{i(V'_\ell + V'_k)}{V'_f}\right\} + \frac{\Delta P_c}{P_c}\left\{\frac{iV'_c}{V'_f}\right\} + \frac{\Delta P_m}{P_m}\left\{\frac{V'_m}{V'_f}\right\} + \frac{\Delta P_s}{P_s}\left\{\frac{V'_s}{V'_f}\right\} \qquad \text{(D-1)}$$

Below, we estimate typical weights for each component for some standard cases derived from recent data on factor costs for single-family houses and multiple dwellings. These estimates of the appropriate weights for each type of factor input enable us to judge the sensitivity of an overall factor-price index to changes in component factor prices, thus indicating the relative care with which each component should be price indexed.

Tables D-1 and D-2 show some of the data used in constructing our standard cases. They are empirical estimates of annual "occupancy costs" for single-family houses and units in multiple dwellings in the Northeast Corridor in 1968. Additional data for multiple dwellings in large U.S. cities in 1970, published by the Institute of Real Estate Management, were also consulted.

We used these data to construct the eight standard cases shown in Table D-3. The first four cases deal with single-family homes. Case 1 considers a new home that costs $20,000; Case 2, a well-maintained older home valued at $16,000;* Case 3, an old home valued at $15,000 with minor rehabilitation of $1,000;** and Case 4, a similar old house undergoing major rehabilitation costing $5,000.

The remaining cases represent units in multiple dwellings. Case 5 concerns newly constructed units at a cost of $15,000 per unit; Case 6, older, well-maintained units valued at $11,000 per unit. Case 7 introduces minor rehabilitation of $1,000 per unit; Case 8, major rehabilitation of $5,000 per unit, on older units valued at $10,000.

The estimated values of each of the weighting factors in brackets in Eq. (D-1) are shown for each case. The first two terms on the right-hand side of the equation represent interest charges on the value base

---

*For convenience, all land costs are assumed to be included as part of the initial capital cost.

**This figure is the expected upper bound on allowance-induced rehabilitation (see Sec. VII). Because rehabilitation could be more extensive, we have included Case 4.

Table D-1

DISTRIBUTION OF ANNUAL OCCUPANCY COSTS FOR SINGLE-FAMILY HOUSING UNITS, BY MAJOR EXPENSE CATEGORY: NORTHEAST CORRIDOR, 1968

Major Expense Category	Development-Built Units		Prefabricated Units	
	Amount ($)	Percentage	Amount ($)	Percentage
Debt retirement	1,104	52.9	1,008	51.9
Taxes	540	25.8	492	25.3
Utilities	348	16.7	348	17.9
Maintenance and repairs	96	4.6	96	4.9
Total costs	2,088	100.0	1,944	100.0

SOURCE: McGraw-Hill Information Systems Company, "A Study of Comparative Time and Cost for Building Five Selected Types of Low-Cost Housing," *The Report of the President's Committee on Urban Housing*, Vol. II, U.S. Government Printing Office, Washington, D.C., 1968, p. 8.

NOTE: Figures are for typical low-income housing units in the Boston-Washington corridor.

Table D-2

DISTRIBUTION OF ANNUAL OCCUPANCY COSTS FOR HOUSING UNITS IN MULTIPLE DWELLINGS, BY MAJOR EXPENSE CATEGORY: NORTHEAST CORRIDOR, 1968

Major Expense Category	Rehabilitated Units		New Units	
	Amount ($)	%	Amount ($)	%
Payroll, management, and administrative expenses	204	10.0	300	11.0
Utilities	180	8.9	240	8.8
Decorating, maintenance, and repairs	180	8.9	168	6.1
Taxes	288	14.2	372	13.6
Insurance	48	2.3	60	2.2
Debt retirement	768	37.9	1,164	42.5
Vacancies and bad debts	216	10.6	252	9.2
Profit and reserves	144	7.1	180	6.6
Total costs	2,028	100.0	2,736	100.0

SOURCE: McGraw-Hill Information Systems Company, op. cit., p. 9.

NOTE: Figures are for typical low-income housing units in the Boston-Washington corridor.

Table D-3

ESTIMATED COMPONENT WEIGHTS OF A HOUSING-SERVICE FACTOR-PRICE INDEX, BY TYPE OF STRUCTURE: EIGHT STANDARD CASES

Case Number	Case Description	Fraction of Total Annual Factor Costs				
		Interest Charges on				
		Value Base	Capital Improvements	Maintenance and Repairs	Building Services	Total
	Single-Family Homes					
1	Newly constructed	.783	0	.047	.170	1.00
2	Well-maintained old structure	.743	0	.056	.202	1.00
3	Old structure, minor rehabilitation	.696	.046	.056	.202	1.00
4	Old structure, major rehabilitation	.587	.196	.047	.170	1.00
	Multiple Dwellings					
5	Newly constructed	.610	0	.085	.305	1.00
6	Well-maintained old structure	.565	0	.092	.343	1.00
7	Old structure, minor rehabilitation	.536	.054	.121	.290	1.00
8	Old structure, major rehabilitation	.442	.221	.099	.238	1.00

SOURCE: Tables D-1 and D-2, and Institute of Real Estate Management, *Apartment Building Income/Expense Analysis*, Chicago, 1971.

NOTE: Fractions may not add exactly to 1.00 because of rounding.

and on capital improvements. The last two terms represent maintenance and repairs and building-services costs, respectively.

To determine which component price changes most affect overall factor prices, we can calculate the overall change for a 10-percent change in each of the component prices. Practically, this requires only moving the decimal point one place to the right for each entry in Table D-3.*

In all eight cases, the most important component is the value of initial capital and land, which determines the weight of the first component of Eq. (D-1), the interest rate. A 10-percent rise in i will result in an overall increase in factor costs ranging from 4 to 8 percent among the eight cases. Clearly, accounting for interest-rate changes will be a very important part of the price-indexing scheme.

Changes in the price of capital improvements are much less important. In the two (more probable) cases dealing with rehabilitation expenditures of $1,000, a 10-percent price increase in the cost of capital improvements causes an overall increase in factor costs of only 0.46 and 0.54 percent, respectively. Even where rehabilitation expenditures are $5,000, a 10-percent price increase causes overall factor costs to increase by only 1.96 and 2.21 percent, respectively.

The last two components, maintenance and service, which are part of maintenance and operating costs, are indexed together. Although less important than the interest rate, they are considerably more important than the cost of capital improvements. In this case, a 10-percent change in the maintenance and service components together causes an overall increase in factor costs ranging from 2.17 to 4.35 percent, depending on the type of housing unit in question. Thus, in our price-indexing scheme, priority is given to interest rates and maintenance and operating costs. Capital-improvement costs, although of much less

---

*For example, consider a 10-percent change in the interest rate. Since all other prices are constant, $\Delta P_c = \Delta P_m = \Delta P_s = 0$. All of the right-hand terms except the first drop out of Eq. (D-1). The bracketed expression (corresponding to an entry in Table D-3) is multiplied first by .10, since $\Delta i/i = .10$, and then by 100 to convert it to percent. This is equivalent to multiplying the bracketed expression by 10, thus moving the decimal point one place to the right.

importance than the other costs, are indexed to cover cases where there are large expenditures on rehabilitation.

Price Index for Interest Rates

Appendix B explains why the current rate of interest determines the opportunity cost of land and capital used for housing. The relevant interest rate here is that on residential mortgages. Since neither of our sites will include large cities, the number of financial institutions in the mortgage market is likely to be small. Hence, it should be feasible for Rand to monitor directly the interest rates on conventional first mortgages at each site. Some survey work will be necessary unless conventional mortgage rates are published locally.

Price Index for Capital Expenditures

The capital price index is less problematic because the experimental results are relatively insensitive to changes in capital prices. For input measurement, the capital values of the monitored properties will be appraised only in the base year; thereafter, the costs of capital inputs made during the course of the experiment (alterations, replacements, improvements) will be measured and must be deflated to base-year equivalents. As we have shown, the results of our analysis are relatively insensitive to changes in the prices of this group of factor inputs.

Therefore, it does not seem necessary to construct an index that measures changes in the specific costs of rehabilitating or altering residential structures. Rather, we can use an available price index of building construction costs. The Boeckh *Building Cost Modifier*[*] is published bimonthly for over 200 cities in the United States and Canada.[**]

---

[*] Published by the American Appraisal Company, Inc., Milwaukee, Wisconsin.

[**] It is very likely that the experimental sites will be covered by the Boeckh index. However, in the event that either (or both) is not, it appears that we will be able to contract with McGraw-Hill, Inc., to include our sites in their *Dodge Building Cost Index* (a comparable, although less residentially oriented construction cost index) for a moderate sum. Because of their method of data collection, American Appraisal is unwilling to contract for cities not already included in the Boeckh index.

The index is constructed from wage rates in 19 building trades and from prices for 64 types of materials. It is broken down into 11 different types of construction, including two types of residential construction. In our judgment, the latter are satisfactory deflators for our purposes.

Price Index for Maintenance and Operating Expenditures

An appropriate index for maintenance and operating (M&O) input prices is crucial to the correct interpretation of the experimental findings. Background inflation is likely to raise the price of M&O inputs by an average of 3 to 5 percent per year, more than enough to bias any conclusions about changes in housing costs resulting from the allowance experiment. Unfortunately, except for New York City, current M&O price indexes are not available and must be constructed when needed.

Basically, constructing an M&O index requires two steps: First, some way must be found for dividing M&O expenses into component categories, so that their relative weights in total M&O expense can be established for the type of housing being studied. Second, price changes in these categories must be estimated, or measured, and given weights. These weighted price changes are used to construct an index of price changes for overall M&O costs.

The first step, defining M&O expense categories and their relative importance, will be a by-product of surveys conducted during the experiment to monitor M&O inputs to housing production. Table D-4 illustrates major M&O expenses and their fraction of total M&O expense for a typical rental building. These data are based on national averages collected by the Institute of Real Estate Management for 1,483 rental buildings in 1971.* Additional category breakdowns were made using data collected by the New York City-Rand Institute.** The M&O indexes constructed for the experiment will use percentage weights derived from site data.

The expenses in Table D-4 are grouped into categories of relatively homogeneous goods and services. Within these categories, price changes

---

*Institute of Real Estate Management, op. cit.

**C. Peter Rydell, *Testing the Maximum Base Rent Formula on Sternlieb's Data*, The Rand Corporation, WN-7417-NYC, May 1971, p. 28.

Table D-4

DISTRIBUTION OF MAINTENANCE AND OPERATING COSTS FOR HOUSING UNITS IN MULTIPLE DWELLINGS, BY DETAILED EXPENSE CATEGORY: MAJOR U.S. CITIES, 1970

Detailed Expense Category	Percentage Distribution
Utilities	
Electricity	8.16
Gas	2.80
Water	4.47
Heating fuel	11.04
Insurance	4.77
Painting	7.57
Management	12.83
Other administration	3.77
Maintenance and repairs	
Miscellaneous repairs	6.52[a]
Plumbing and heating	4.46[a]
Roofing work	.75[a]
Elevator work	1.13[a]
Glass work	.37[a]
Masonry work	.37[a]
Floor work	.18[a]
Air-conditioning work	[b]
Carpentry	[b]
Plastering	[b]
General structure work	[b]
Other wages	21.10
Supplies and services	
Janitorial	4.02[a]
Exterminator	.73[a]
Rubbish and garbage removal	.37[a]
Grounds	.37[a]
Other	4.22
Total	100.00

SOURCE: Institute of Real Estate Management, op. cit., and C. Peter Rydell, *Testing the Maximum Base Rent Formula on Sternlieb's Data*, The Rand Corporation, WN-7417-NYC, May 1971, Table 14.

[a]Category breakdowns from Rydell, op. cit.

[b]Categories for which data will be available from experimental surveys but which were not included in the above sources.

for component goods and services ought to be of comparable magnitude. Moreover, no price data are available for categories finer than those shown in the table.

There are three alternative methods for estimating M&O category price changes: (1) *direct pricing*, (2) *specification pricing*, and (3) finding a usable published index for the category. The *direct-pricing* method involves surveying the prices actually paid for inputs, for a sample of buildings. This method ensures that only strictly relevant evidence is being used, but it is vulnerable to error if changes occur in the quantity or quality of inputs purchased. *Specification pricing* seeks to overcome the quantity/quality-control problem by tightly specifying characteristics of the input under analysis and then estimating the supply price of a unit of input. Input prices may be estimated by surveying contractor estimates (e.g., for painting) or by calculating supply price from supplier formulas (e.g., for heating fuel).

The third method means, in effect, using information published by the BLS. Certain components of the Area Wage Survey (published annually for 1970 areas in the United States) and the Wholesale Price Index (published monthly for the nation as a whole) closely approximate our M&O expense categories. The relevant BLS index can then be used as a measure of price changes in each category. Since the third method of estimation does not involve survey costs, it is preferred for the purposes of the Supply Experiment. Inaccuracies introduced by matching BLS series components with M&O expense categories appear to be tolerable.

So far as we have been able to determine, however, there is no existing price index (BLS or other) for the categories of utilities and insurance. Local indexes covering these items may exist for the site cities, but a regional index is needed to net out background inflation.* The best choice would therefore seem to be a combination of methods (2) and (3), above. We will use existing BLS indexes where possible and draw upon specification pricing to index the remaining categories.

---

*A discussion of the background-inflation problem is given below, on pp. 278-281.

Table D-5 repeats the M&O expense categories of Table D-4, adding the source index and the component that can be used in constructing our overall M&O index. Those categories in Table D-5 that list only the Area Wage Survey (AWS) are either chiefly labor inputs, which can be indexed with the appropriate AWS wage category, or inputs consisting of a significant amount of labor for which no corresponding commodity index exists. For example, management costs can be indexed by using the "Office Clerical Wage Index" of the AWS. Likewise, elevator repairs can be indexed by using the "Median Hourly Earnings Index" for maintenance mechanics, part of the AWS.*

Categories that are indexed from both the Wholesale Price Index (WPI) and the AWS involve significant amounts of materials as well as labor. Table D-5 notes the source indexes as appropriate and indicates how their data should be weighted. A more comprehensive and detailed description of this method of indexing is provided in Sternlieb's study of rental housing costs and revenues in New York City.**

Utilities, insurance, and several other services will be indexed by specification pricing. These categories are easy to price and involve only a small number of suppliers. The BLS publishes an index of operating costs for apartments in New York which illustrates how specification pricing is used and provides a useful list of specifications for utilities.*** Table D-6 gives an example of the BLS index for

---

*The AWS is not currently performed for every site of possible interest. In the event that the experimental sites are not covered by the AWS, we have two options: We can have the BLS survey the site(s) under special contract, or we can negotiate with the American Appraisal Company to purchase the wage and price data collected in the course of constructing the Boeckh index and use these data to construct an M&O index in its entirety.

**Sternlieb, op. cit.

***_1972 Price Index of Operating Costs for Rent Stabilized Apartment Houses in New York City_, Middle Atlantic Regional Office, Bureau of Labor Statistics, U.S. Department of Labor, Regional Report 28, July 1972.

Table D-5

SOURCES OF DATA FOR PRICE INDEXING MAINTENANCE AND OPERATING EXPENSES IN THE SUPPLY EXPERIMENT

Expense Category	Proposed Sources of Data	
	General Indexes and Weighting Factors	Subindexes and Weighting Factors
Utilities		
Electricity	Specification pricing	
Gas	Specification pricing	
Water	Specification pricing	
Heating fuel	Specification pricing	
Insurance	Specification pricing	
Painting	AWS (80%)	Painters
	WPI (20%)	Prepared paint
Management	AWS	Office clerical wage index
Other administration	AWS	Office clerical wage index
Maintenance and repairs		
Miscellaneous repairs	AWS	Janitors, porters, cleaners (50%) Helpers, maintenance trades (50%)
Plumbing and heating	AWS (60%)	Stationary engineers (10%) Firemen, boilers (20%) Plumbers (70%)
	WPI (40%)	Hardware (10%) Heating equipment (20%) Plumbing fixtures (70%)
Roofing work	AWS (60%)	Roofer and sheet metal workers
	WPI (40%)	Prepared asphalt roofing
Elevator work	AWS	Maintenance mechanic
Glass work	AWS (75%)	Helpers, maintenance trades
	WPI (25%)	Concrete products
Masonry work	AWS (75%)	Helpers maintenance trades
	WPI (25%)	Concrete products
Floor work	[a]	
Air conditioning work	[a]	
Carpentry	AWS	Carpenters
Plastering	[a]	
General structure work	[a]	
Wages	AWS	Janitors, porters, cleaners
Supplies and services		
Janitorial	AWS	Janitors, porters, cleaners
Exterminator	Specification pricing	
Rubbish and garbage	Specification pricing	
Grounds	AWS	Janitors, porters, cleaners (50%) Helpers, Maintenance trades (50%)
Other	Weighted average of all previously indexed categories	

SOURCE: Table D-4 and George Sternlieb, *The Urban Housing Dilemma: The Dynamics of New York City's Rent Controlled Housing*, issued in draft form by the Department of Rent and Housing Maintenance, Housing and Development Administration, City of New York, May 1970.

NOTE: Percentage breakdowns are those given in Sternlieb.

[a]Not included in the above sources. Some minor survey work will be necessary to determine the subdivision of these components and their weighting.

Table D-6

RELATIVE IMPORTANCE OF COMPONENTS OF THE PRICE INDEX OF OPERATING COSTS FOR RENT-STABILIZED APARTMENT HOUSES IN NEW YORK CITY: 1972

Group	Relative Importance (%)
Taxes, fees, and permits	41.5
Labor	17.2
Fuel and utilities	16.4
Contractor services	11.4
Administrative	7.5
Insurance	3.2
Parts and supplies	1.8
Replacement costs	1.0
All items	100.0

SOURCE: Middle Atlantic Regional Office, Bureau of Labor Statistics, op. cit., Table 3.

rent-stabilized apartments in New York City for 1972. Within the eight groups listed, the BLS used data from over 5,000 price quotations, based on a sample of about 60 specifically defined commodities and services, to compute price changes. Among the items priced are fuel oil, electricity, repainting a one-bedroom apartment, roof repairs, ledger paper, and light bulbs.

Table D-7 provides a historical perspective of M&O price changes. It lists the Boeckh index, wage-rate indexes, and the WPI construction-materials index for selected years since 1955. Residential-construction prices have increased by an average of 8 percent per year since 1967, wages by 11 percent per year, and construction-material prices by 5 percent per year.

Table D-7

INDEXES OF DWELLING UNIT CONSTRUCTION COSTS, UNION HOURLY WAGE RATES FOR SELECTED BUILDING TRADES, AND WHOLESALE PRICES OF CONSTRUCTION MATERIALS: SELECTED YEARS

(1967 = 100)

Year	Residential Construction Index			Union Hourly Wage Rates Index								Wholesale Price Index All Construction Materials
	Total	Frame	Brick	All Trades	Bricklayers	Carpenters	Electricians	Painters	Plasterers	Plumbers	Building Laborers	
1955	72.5	73.4	71.7	60.0	65.3	59.8	60.3	60.9	66.7	60.3	56.1	90.4
1960	81.8	82.1	81.5	75.4	78.8	75.0	76.4	74.9	79.6	75.3	73.8	95.5
1965	90.4	90.4	90.6	90.9	91.8	90.7	91.5	90.9	92.1	91.4	90.5	95.8
1967	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
1968	107.3	107.4	107.3	106.6	106.8	107.0	106.5	106.3	105.1	106.8	106.5	105.6
1969	116.2	116.6	116.2	115.4	115.0	115.8	117.1	115.1	113.3	115.9	114.8	111.9
1970	122.4	122.3	122.3	128.8	127.7	128.9	130.4	126.6	126.0	130.5	129.3	112.5
1971	132.8	132.1	132.9	144.0	144.9	141.5	148.4	139.5	140.9	145.8	144.4	119.5

SOURCE: *1971 HUD Statistical Yearbook*, U.S. Department of Housing and Urban Development, U.S. Government Printing Office, Washington, D.C., 1972, pp. 330-332.

NOTE: Dwelling-unit construction costs are from the Boeckh index, American Appraisal Company; hourly wage rates are from *Union Wages and Hours: Building Trades, July 1, 1971*, BLS Bulletin No. 1747; and construction materials are from *Wholesale Prices and Price Indexes*, Bureau of Labor Statistics.

SEPARATING THE EFFECTS OF BACKGROUND INFLATION FROM OBSERVED LOCAL PRICE CHANGES

Some of the price changes measured by the method described above will reflect background inflation--inflation occurring throughout the economy and thus not specific to the experimental sites. The background-inflation component can be factored out by comparing local factor-price changes with those for a larger surrounding area. This requires regional indexes comparable to our local indexes. We must start by defining the larger area to be covered by the regional index. It should bear some relation to the market area of the site, yet be large enough to escape significant influence by forces operating in local markets.

In the case of interest rates, we need an index of mortgage rates for the region. The Roy Wenzlick Research Corporation compiles local and regional data on interest rates gathered by surveying firms involved in the mortgage market. The compilations, which give the prevailing rate of interest on conventional residential mortgages on both a regional and a metropolitan level, are published annually in the *Real Estate Analyst*. The data are compiled separately for six regions and for many of the metropolitan areas within those regions.

We could either use an existing Wenzlick regional index or we could weight the rates for the various metropolitan areas within a specified region. The weighting scheme would be based on each city's share of the total lending volume in the region. Having both a local and a regional index of interest rates, we could subtract regional (background-inflation) changes from total local changes to obtain a net index of local interest-rate changes.

Construction of a regional index covering capital prices is relatively easy. Again, we must define the region to be covered by the index. If the Boeckh index is used as a capital-price index for the experimental site,* a regional index can be constructed by taking a population-weighted average of the Boeckh indexes for the cities within the region. The resulting index of background inflation could be subtracted from the local index of capital prices, giving an index of local price changes net of background inflation.

*The procedure would be unchanged if the Dodge index were used.

The procedure for preparing the M&O index is similar. Components from the AWS index can be population-weighted for cities within the region. Specification pricing would be done for the same cities used to weight the AWS indexes, applying the population-weighting procedure used there. Because the WPI is a national index, any regional index would be identical with our site index. This presents little cause for concern, since the materials indexed by the WPI are generally supplied in a national market. A local increase in demand for these materials would probably have no effect on their prices (although there might be temporary shortages in the site city).

Using the WPI and the population-weighted AWS and specification-price indexes, we can construct a regional index similar to that for capital inputs. This index could then be used to net out background inflation in the region.

As an example, consider the (likely) possibility that both experimental sites will be located in the north central portion of the United States. A map of this region, indicating the area to be covered by a regional index, is shown in Fig. D-1. The various shaded areas represent seven major trading areas as determined by Rand McNally.* We have combined these areas to form a region comprising most of Michigan, Wisconsin, Illinois, Indiana, and Ohio. The availability of the Boeckh index and the AWS for various cities within the region is indicated in Fig. D-1 and Table D-8. In each case the data are sufficient to construct a population-weighted regional index. As indicated in Table D-8, Wenzlick provides interest-rate data for 28 cities in the region, which we could use should we decide to construct a regional interest-rate index.

---

*1972 *Commercial Atlas and Marketing Guide*, Rand McNally and Company, Chicago, 1972. The trading areas as defined in the atlas were "determined after an intensive study of such factors as physiography, population, newspaper circulation, economic activities, highway facilities, railroad services, suburban transportation, and field reports of experienced sales analysts" (p. 65).

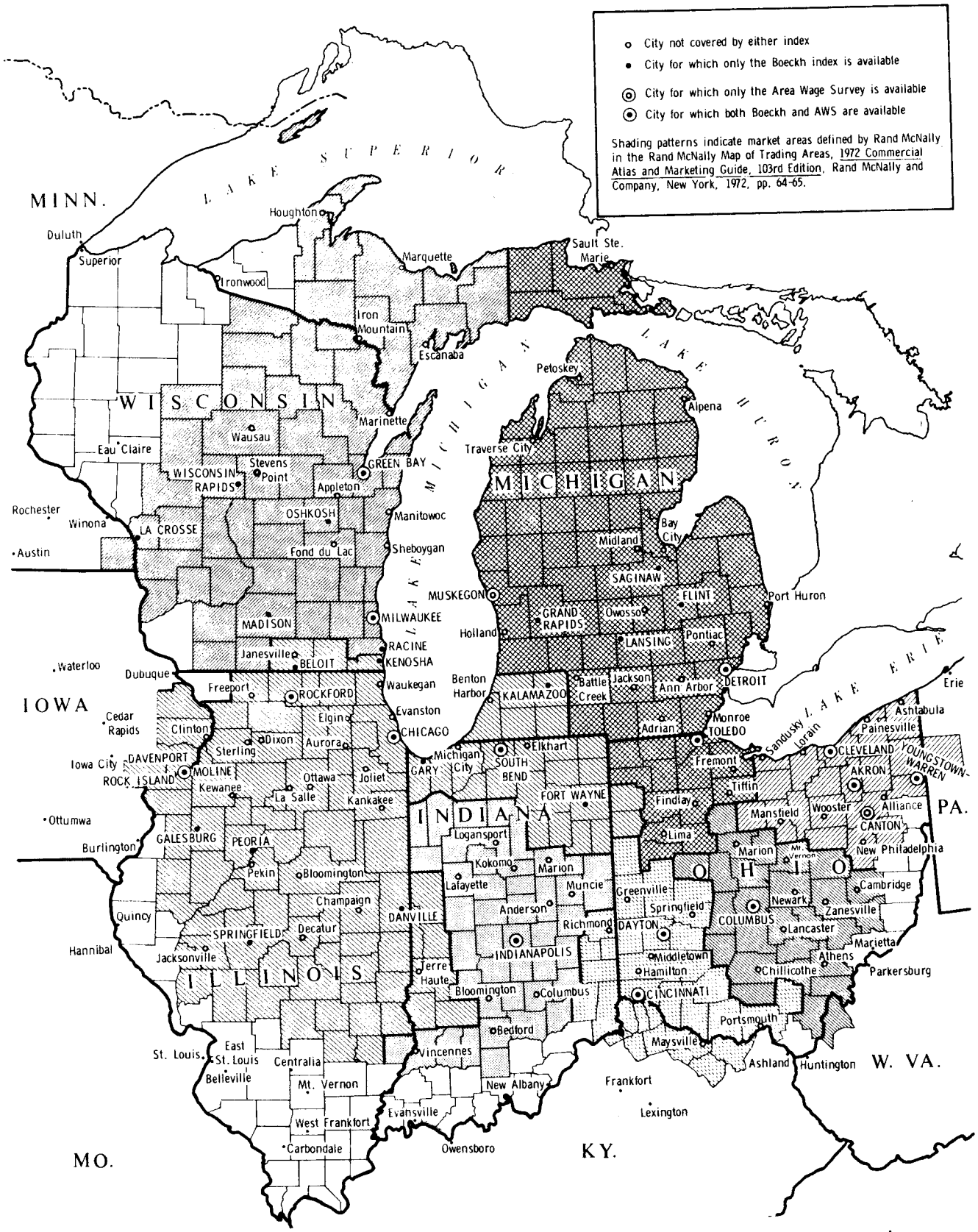

Fig. D-1—Availability of Boeckh and AWS indexes for market areas in the north central region of the United States

Table D-8

AVAILABILITY OF LOCAL WAGE AND PRICE INDEXES TO BE USED TO CONSTRUCT A REGIONAL INDEX: NORTH CENTRAL UNITED STATES

City	Index Availability (x = available annually)		
	Boeckh Building Cost Modifier	Area Wage Survey	Wenzlick Mortgage Interest Rates
Wisconsin			
Beloit	x		
Green Bay	x	x	
Kenosha	x		
La Crosse	x		
Madison	x		x
Milwaukee	x	x	x
Oshkosh	x		
Racine	x		x
Wisconsin Rapids	x		
Michigan			
Detroit	x	x	x
Flint	x		x
Grand Rapids	x		x
Kalamazoo	x		
Lansing	x		x
Muskegon		x	
Saginaw	x		x
Ohio			
Akron	x	x	x
Canton		x	x
Cincinnati	x	x	x
Cleveland	x	x	x
Columbus	x	x	x
Dayton	x	x	x
Hamilton-Middletown			x
Springfield			x
Toledo	x	x	x
Youngstown	x	x	x
Indiana			
Fort Wayne	x		x
Gary	x		x
Indianapolis	x	x	x
South Bend	x	x	x
Illinois			
Chicago	x	x	x
Danville	x		
Decatur			x
Galesburg	x		
Peoria	x		x
Rockford	x	x	x
Rock Island	x	x	x
Springfield	x		x
Total	33	17	28

SOURCE: *Boeckh Building Cost Modifier*, The American Appraisal Company, Inc., Milwaukee, Wisconsin, and *Area Wage Surveys*, U.S. Department of Labor, Bureau of Labor Statistics, Bulletin 1725-3, Washington, D.C., July 1971.

SEPARATING ALLOWANCE-INDUCED FROM OTHER LOCAL PRICE CHANGES

Within the local housing market at each site, factor prices will be determined by derived or direct demand from three distinguishable sources: the demand for housing by allowance recipients, the demand for housing by nonrecipients, and the direct demand for these same input factors for entirely nonhousing objectives. For analytical purposes we may assume that the demand for housing by nonrecipients is independent of the allowance program. This assumption is not strictly correct, since the allowance program will have the side effect of raising incomes in the site, thus somewhat increasing all expenditures (including those on housing). However, this effect will be sufficiently small to be neglected in the analysis.

We must devise some means of measuring the price inflation caused by the experimental allowance program itself. It is possible that forces exogenous to the experiment will cause an increase in the demand for housing by nonrecipients, or in the demand for housing factors of production through the influence of some other, nonhousing market. Disentangling the three types of demand shifts that can take place is conceptually straightforward, though empirically laborious.

For the moment, let us focus on the market for housing by itself. Since nonrecipients and recipients compete for housing in the market, any increase in demand by nonrecipients will drive the price of housing up. This increase will be measured by the price-indexing scheme for the experiment, but it cannot rightly be attributed to the experiment (assuming, as we argue above, that allowance-program and nonrecipient housing demand are essentially independent).

Thus we need some method for removing the local inflation of factor prices caused by nonrecipient housing demand. Figure D-2 illustrates the supply of and demand for housing services. The quantity of housing services is measured along the horizontal axis and the price per unit of housing services along the vertical. The total demand curve for housing is broken down into demand by allowance recipients (D_1) and demand by nonrecipients (D_2).*

*Note that D is just the horizontal summation of D_1 and D_2.

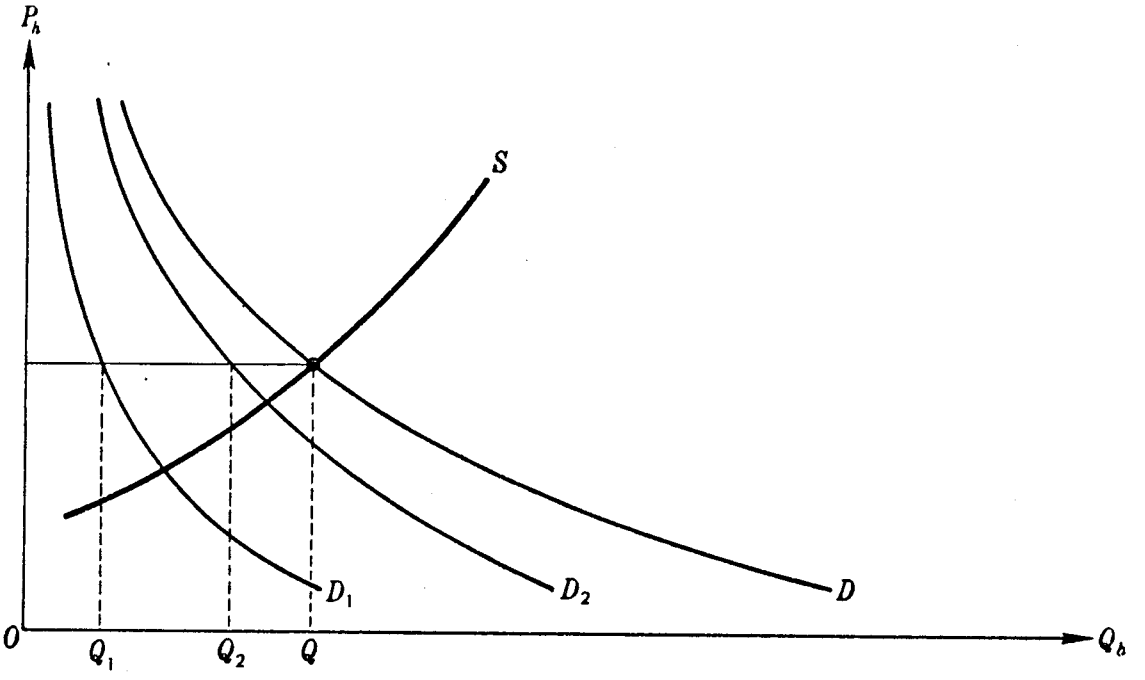

Fig. D-2—Recipient and nonrecipient demand for housing

Figure D-3 shows the situation when demand for housing by both recipients and nonrecipients increases (with a resulting increase in total demand). In each case, the demand curve representing the situation after the shift is marked D'. Conceptually, the relative contribution to price inflation of the two components of the demand increase is measured by the relative distance by which these curves shift to the right. If there has been an increase in demand, more units of a good will be purchased at a given price; the difference in the amounts purchased is a measure of the demand shift.

Since the experiment will monitor actual market situations, we will obtain data that determine points a, b, and c (the quantities purchased at price P_1 before the demand shift) and points g, h, and i (the quantities purchased at the new price, P_2, after the shift). However, the changes in quantities purchased, implicit in these points, will not give us information about the magnitude of the demand shift, since these quantity changes are not measured holding price constant. However, if we know the price elasticity of demand for housing and can

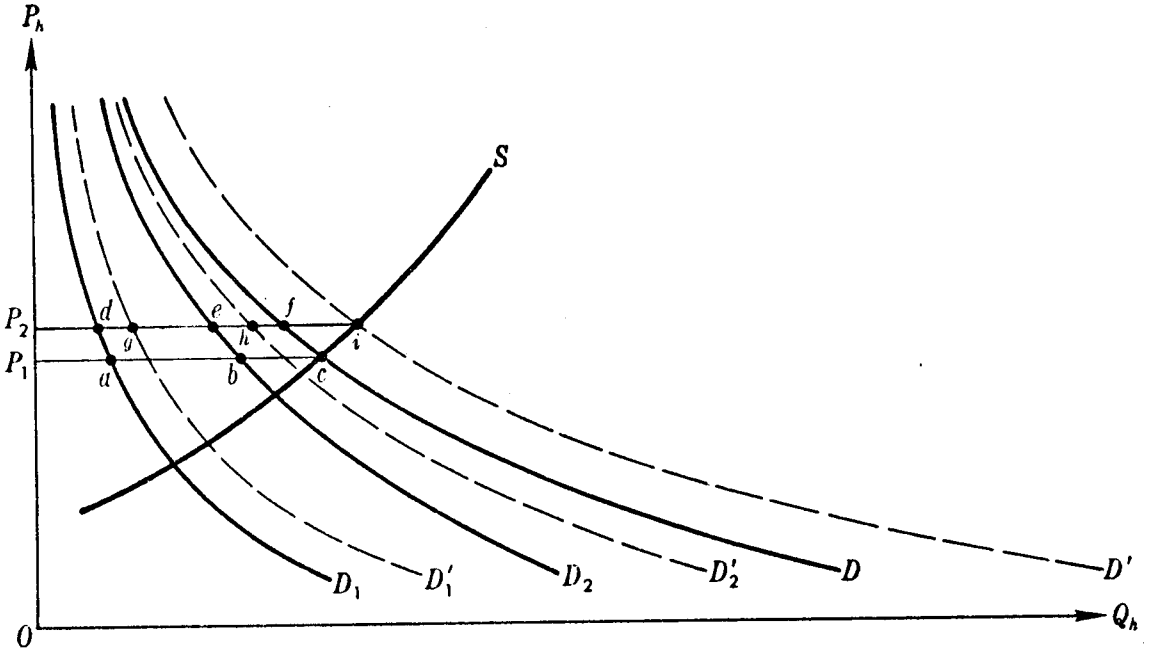

Fig. D-3—Shifts in recipient and nonrecipient demand for housing

assume that it is constant in the range with which we are dealing, we should be able to determine the size of the demand shifts. We will know points a, b, and c in Fig. D-3 as well as the price increase from P_1 to P_2; the price elasticity of demand will then allow us to determine points d, e, and f.*

We now have the information required to estimate the ratio of the increase in demand by allowance recipients to the increase in total demand. According to the definition given earlier, the total demand

*As an illustration, let point a in Fig. D-2 represent a quantity of housing denoted by Q_o. Define the arc elasticity of demand with respect to price as

$$\varepsilon(Q,P) = \left(\frac{Q_o - Q_1}{Q_o + Q_1}\right)\left(\frac{P_o + P_1}{P_o - P_1}\right).$$

We know $\varepsilon(Q,p)$, Q_o, P_o, and P_1, so it is only a matter of algebra to solve for Q_1. Then P_1 and Q_1 determine point d in Fig. D-2.

increase shown in Fig. D-3 is represented by the *distance fi*. The demand increase of recipients is represented by the *distance dg*. Thus, the ratio *distance dg/distance fi* yields the proportion of total demand increase caused by allowance recipients.

Let us call local factor-price inflation ΔP_f (in reality, a different value for each factor f); and call ϕ the proportion of the total increase in demand for housing caused by allowance recipients.* Then the amount of the factor-price inflation that can be attributed to a demand increase by allowance recipients can be estimated as $\phi(\Delta P_f)$, where $0 \leq \phi(\Delta P_f) \leq \Delta P_f$.**

Actually, $\phi(\Delta P_f)$ is an upper bound on the amount of factor-price inflation caused by the experiment. The analysis above explicitly assumes that the only demand for factor inputs comes from the residential housing market. Thus, the entire price increase is apportioned to recipient and nonrecipient demanders of residential housing. Other economic sectors, of course, use the factors for which we are constructing price indexes; it is possible that some of the price increase can be accounted for within those sectors.

The possible alternative uses of factor inputs at the test sites, and the intensity of their employment, are not known at present. Table D-4 suggests that in most cases either nonhousing consumption is relatively small or the factor price in question is not sensitive to local market prices. Approximately 20 percent of M&O expenses concern utilities and insurance, the prices of which are determined by regulatory bodies. Heating fuel, which accounts for 11 percent, while not directly price-regulated, is sold in a larger regional market and is unlikely to

---

*Note that ϕ is a fraction whose value lies between 0 and 1 inclusive.

**In a strict sense, this statement may not be correct. It is based on the fact that the demand for factor inputs is derived from the demand for housing services. Only if the factors are used in fixed proportions (which is probably less true in the short run than in the long run) can the demand for the final product (housing) be apportioned to the various inputs. If factors are not fixed and if the apportionment changes, pressures on the prices of different factors will vary. In view of possible measurement errors in our other data, however, we think that the calculation is sufficiently precise to yield a good approximation.

fluctuate in price appreciably with a shift in nonhousing demand at the sites. Management costs are nearly 13 percent of the total, but the suppliers are probably specialized to the housing market. The categories of miscellaneous repairs, plumbing and heating, elevator service, and janitorial service may not be wholly specialized to housing; these amount to about 16 percent of total M&O cost and are probably the factors most sensitive to nonhousing demand shifts. We conclude that vigorous and unlikely shifts would have to take place in nonhousing-market sectors before factor prices would be significantly affected in the aggregate.

Hence, we believe that it is not worth the additional effort to isolate demand shifts in nonhousing sectors. We will instead attribute all locally caused price changes to shifts in housing demand. A fortiori, the allowance-induced price rise will not be greater than this estimate.

In terms of Fig. D-3, $\Delta P_{f.h}$, the amount of factor-price inflation caused by the housing market, will be less than ΔP_f; thus $\phi(\Delta P_{f.h}) \leq \phi(\Delta P_f)$. Since $\phi(\Delta P_{f.h})$ is what we are really trying to measure, $\phi(\Delta P_f)$ will be an upper bound on the amount of inflation caused by the experiment.

To complete the above analysis we will need certain data, most of which will be generated in the course of the experiment. The necessary data are the prices of housing services, changes in the total quantity of housing services purchased and separation of these changes into changes in the amounts purchased by allowance recipients and nonrecipients, and the price elasticity of the demand for housing services. The prices of housing services will be measured as explained in Appendix B.

The total quantity of housing services purchased can be estimated from survey data obtained for our panel of residential properties, which will be a probability sample of the entire housing stock. HAO administrative records will give exact figures on expenditure changes by allowance recipients. Deflating expenditure changes by the previously indexed price changes will yield the changes in quantity purchased. Subtracting changes in quantities purchased by recipients from total quantity changes gives changes in quantities purchased by nonrecipients.

The price elasticity of the demand for housing services will be mapped by the Demand Experiment. We propose to use the elasticity function thus estimated in this analysis despite the fact that the estimate will be based on data for cities other than our sites. To the extent that we question the exact appropriateness of these "borrowed" functions, we will resort to a fortiori analysis, showing results for approximations that are most favorable and least favorable to the allowance program.

Appendix E

ANALYTICAL INTEGRATION OF DATA FROM THE DEMAND AND SUPPLY EXPERIMENTS

Although conducting the Demand and Supply Experiments as separate operations makes good sense to us, and conducting them at different sites is necessary to the success of each, one of the possible penalties is that they might develop design features that interfere with subsequent analytical integration of their findings.

In designing the Supply Experiment, our main consideration has been to devise an experimental setting from which credible evidence about the effects of a housing allowance program could be obtained. To the extent that the experimental evidence plays a part in future legislative processes, we think that the critical question that will be addressed to the monitors of the Supply Experiment is, What happened in City X and City Y as a consequence of the experimental programs mounted there? It is no easy task to devise monitoring procedures that will enable us to answer this limited question in a convincing way. If our experimental design accomplishes that much, it will have accomplished a great deal.

The broader question is, What would happen in other cities or in the nation if a national housing allowance program were implemented? We would in fact expect our audiences, in both the executive and legislative branches, to draw their own general conclusions from the evidence presented for the experimental sites, whatever arcane extrapolations were offered by analysts.

Nonetheless, there is a smaller audience, perhaps equally important, that is concerned about scientific techniques for generalizing from limited experimental evidence. The exposition below is addressed to this audience. Here, we try to show how the data from the Demand and Supply Experiments could be combined analytically to permit estimation of:

1. The effects of tested allowance programs on the market price of housing services in housing markets other than the experimental sites,

2. The effects of variations on the tested allowance programs in such markets, and
3. The effects of the estimated price changes on the quantities of housing services consumed by allowance recipients and nonrecipients.

The basic structure of the analytical model here proposed can be summarized as follows: The data gathered in the Demand Experiment should enable us to estimate individual parameters of demand response to price that vary with observable household characteristics, including household income and allowance entitlement. The data gathered in the Supply Experiment should enable us to estimate individual parameters of supply response to price that vary with the characteristics of the supplier and his property. If these parameters of individual behavior are expressed in terms that are reasonably independent of the particular housing-market context in which they were observed, they should have broader applicability to the behavior of similar individuals elsewhere; in other words, the behavioral parameters will be more or less "portable."

Carrying these behavioral parameters to a local housing market other than those that served as sites for the Demand and Supply Experiments, we can apply the appropriate demand parameters to each household in that market. The effects of a hypothetical allowance program on each individual's housing demand can be estimated from program rules as to eligibility and allowance entitlement. For each household in the new site, whether an allowance recipient or not, we can thus construct a schedule that describes the quantity of housing services it would demand at alternative prices. These individual schedules can be aggregated to form a market demand schedule.

A similar procedure can be followed for suppliers at the new site, constructing for each a schedule that describes the quantity of housing services it would supply at alternative prices. These individual schedules can be aggregated to form a market supply schedule.

Comparing the market demand schedule and the market supply schedule, we can estimate the price for housing services that would clear

the market--i.e., the price at which the quantity demanded would equal the quantity supplied. Once this price is determined, it can be substituted into individual demand and supply schedules to estimate the quantities of housing services that would be demanded or supplied by individuals with specified characteristics, given the market-clearing price.

The general framework of this analysis is thus easily described. Working out its details in the context of the data we expect to obtain from the Demand and Supply Experiments is another matter. Below, we give a provisional sketch of these details, one which falls short of resolving either the conceptual or the operational problems, but which at least suggests strategies that might be employed. We try to show, in principle, what could be done, given "clean" and comprehensive data from the experiments. Actually, the data will be both "dirty" and incomplete; at best, we can hope for a crude approximation to the data needed to implement the analysis here described. But the data requirements of the analytical model will at least serve as a target for experimental design; and, if HUD wishes to pursue analytical integration of data from the two experiments, the sketch given here of the analytical model will serve as a point of departure for its systematic development and articulation.

THE DEMAND EXPERIMENT

In the Demand Experiment, a thin sample of low-income families living in one or more large metropolitan areas will be selected for enrollment in a housing allowance program. Subsamples of enrollees will be given allowances on different terms, and their housing choices and budgetary decisions will be monitored for a period of three years. Because the number of allowance recipients will be small relative to the population of the housing market, increased housing expenditures by allowance recipients will not add up to significant demand pressure. No measurable supply response, in the sense of a change in the price of housing services or in the quantity supplied, can be anticipated as a consequence of the experiment.

As we understand the plans for the Demand Experiment, treatment variations will include two basically different allowance formulas:

1. Housing-gap formula. Participants receive allowances equal to a specified percentage of their disposable incomes.
2. Housing-discount formula. Participants receive allowances equal to a specified percentage of their contract rents.

Under the first formula, allowance payments may be earmarked directly or indirectly for housing expenditure. Under both plans, the percentage contributions by the government will be varied.

The assumed lack of significant supply response makes it possible for the Demand Experiment to capture two vital kinds of information: estimates of the income elasticity of housing demand and of the price elasticity of housing demand. Allowance recipients will be able to purchase housing services at the going market price, and their purchases will not significantly affect that price. Thus, any increase in their housing expenditures subsequent to enrollment can be interpreted as an increase in the quantity of housing services consumed.*

An allowance program may affect the recipient's income, or the price that he pays for a unit of housing services, or both. Under a housing-gap allowance plan, his income is increased by αY, but the price he pays for housing services is the market price. Under a housing-discount plan, he gets a discount on the market price of housing services, paying only βP per unit of service; simultaneously, he gets an increase in income equal to the public contribution to his rent, $(1 - \beta)R$. For different subjects in each treatment class, α and β will be varied.

By careful selection of subjects and by systematic variations in treatments, the Demand Experiment should be able to obtain the data needed to estimate the parameters of two household response functions whose general forms are shown below:

$$\left(\frac{\partial Q/Q}{\partial Y/Y}\right)_t = f(H,\ A,\ Y,\ \beta P) \qquad \text{(E-1)}$$

and

$$\left(\frac{\partial Q/Q}{\partial P/P}\right)_t = f(H,\ Y,\ \beta P) \qquad \text{(E-2)}$$

*This interpretation assumes that there is no change, over time, in the market price of a unit of housing services due to factors other than the experimental program, so a requirement for unbiased estimation of the elasticities discussed below is a system for deflating market rents to a constant-dollar basis. For similar reasons, the incomes of allowance recipients must be adjusted to take into account changes in the consumer price index.

where Q = quantity of housing services demanded by the household;

H = a vector of household characteristics;

A = a vector of dummy variables representing alternative earmarking provisions;

t = time since enrollment;

Y = disposable income; and

βP = the unit price of housing services to the allowance recipient; P is arbitrarily set equal to \$1.

We assume that these functions would be fit by multiple regression on cross-sectional data for each of a series of postenrollment years. With their aid, it is possible to map the demand response over time to changes in either Y or P, or both, for any household whose characteristics, H, are known. If changes in Y are induced by a housing-gap allowance program, we would also need to know the earmarking provisions. If the allowance program is a housing-discount plan, we would of course need to know the discount rate, $(1 - \beta)$. In general, Eqs. (E-1) and (E-2) can be combined and transformed into a household demand function, with time (T) in the argument:

$$Q_d = D(H, A, T, Y, P) . \qquad \text{(E-3)}$$

Furthermore, if we can assume mutual independence of household preference functions, we can aggregate housing demand as a function of price over any set of households for each of which H and Y are known. Hereafter, we will let Eq. (E-3) stand for this aggregate, rather than individual, demand function; a more accurate notation would use summation signs to achieve this result.

THE SUPPLY EXPERIMENT

In the Supply Experiment, two metropolitan areas will be selected as sites. In each, a full-scale housing allowance program will be mounted, enrolling all those likely to be eligible in a national program of this type. At both sites, the same allowance formula will be used; it will be a housing-gap formula, and allowances can be used only in housing certified as meeting minimum standards.

Within these sites, we propose to monitor changes in rental revenues and changes in the flow of housing services for a period of five years. Separate accounts will be carried for individual structures whose owners' characteristics will be identified, as well as the characteristics of the tenants and the neighborhoods in which the structures are located. We propose to concentrate our sampling resources on structures whose rents (or market values) are within reach of allowance-augmented budgets, i.e., structures for which changes in rental revenues due to the allowance program are probable.

We have devised a method for estimating changes in the flow of housing services from these structures, based on systematic accounting for changes in real factor inputs and certain assumptions about the characteristics of the production function.* With its aid, we can decompose observed changes in rental revenues, as follows:

$$\frac{\Delta R}{R} = \frac{\Delta P}{P} + \frac{\Delta Q}{Q} + \frac{\Delta P \Delta Q}{PQ} , \qquad \text{(E-4)}$$

where R = base-year rental revenue;

Q = base-year quantity of housing services supplied to the market; and

P = base-year price per unit of housing services.

We are further able to decompose the estimated change in the price of housing services into components reflecting observed changes in factor-costs per unit of output (including the opportunity cost of capital inputs) and changes in the owner's rate of return on cost of production:

$$\frac{\Delta P}{P} = \frac{\Delta F}{F} + \frac{\Delta \Pi}{\Pi} + \frac{\Delta F \Delta \Pi}{F \Pi} , \qquad \text{(E-5)}$$

---

*See Appendix B for details of the accounting system; it assumes that changes in output are proportional to changes in real factor inputs. Appendix C proposes a method for testing this assumption and adjusting our results if it proves substantially incorrect.

Below, we generally follow the notation of Appendix B, but we have simplified it to avoid excessive subscripting and to suppress distinctions irrelevant to the present discussion.

where F = factor costs per unit of output; and

$\Pi = P/F$ = producer's markup on factor costs.

Since Q, our measure of the quantity of housing services supplied, includes vacant available units as well as occupied ones, and R is actual net rental revenue for the structure, P is a buildingwide average price for housing services that will vary not only with rent levels but with vacancy rates. So our observations on individual structures should give us plenty of variation in $\Delta P/P$, with which we can associate observed changes in output, $\Delta Q/Q$.

We wish to use these data to estimate a supply response function corresponding in form to Eq. (E-2). In principle, this function should describe the amount by which a producer would change his output for a given change in its expected market price, all other things equal. With a certain amount of behavioral modeling, our data will serve.

First, we need an estimate of the producer's supply response to an *expected* change in its market price, whereas our observations of $\Delta Q/Q$ and $\Delta P/P$ are after-the-fact. Landlords who misinterpret market signals may be either pleasantly surprised or disappointed by the consequences of their production decisions. A plausible behavioral model is that output in year t reflects decisions made in year $(t - 1)$, based on prices then prevailing; in estimating our response function, we should lag P one period behind Q or perhaps use a distributed lag.

Second, we seek a partial derivative of supply with respect to price, all other things equal. Most pertinently, these "other things" include the available technologies of production and the costs of the various factors of production, which together determine the producer's costs at various levels of output. Our observed responses do reflect price changes, but they may also reflect changes either in methods of production or in factor prices, limiting their relevance to contexts in which changes in the market price of output are accompanied by the same set of technological possibilities and the same set of factor costs.

We do not think that technological innovations during the five years of the Supply Experiment are likely to much alter the options of

producers of housing services, constrained as they will be by the technology embodied in the existing stock of housing; but production alternatives, hence supply responses to price changes, are likely to vary among producers, depending on the characteristics of their existing structures. So baseline structure characteristics should enter as explicit variables in estimates of supply responsiveness for individual producers, either as regression variables or as stratification variables.

Factor prices, on the other hand, are likely to change over time during the experiment, but not to vary among producers at a given time.* In order to estimate how the producer would respond to price changes alone, we must adjust our observed ΔP, discounting for the changes in factor-costs that occur over the period in question.** As suggested by Eq. (E-5), this can be accomplished by substituting $\Delta\Pi$ for ΔP in the supply function, noting that in the absence of changes in factor prices, $\Delta\Pi = \Delta P$.

Finally, we presume that the producer's skill, his expectations, and his resources may affect his response to changes in the market price of his output for other reasons than those already indicated. So the argument of our supply response function should contain

---

*Cross-sectional variations are possible, either in the form of lower prices for bulk purchases, or in the form of price discrimination in an imperfect factor market. In either case, we would expect cross-sectional variations in factor prices to be regularly associated (in our experimental sites and elsewhere) with structure, landlord, and neighborhood characteristics; if so, building these variations into estimates of supply responsiveness that are specific to housing-market sectors does not much reduce the generality of the response function.

**As explained in Appendix D, factor prices will be indexed at each site and site-specific components of change will be distinguished from regional or national components of change; to the extent possible, site-specific changes due to the allowance program will be distinguished from those attributable to other events in local markets.

For present purposes, the total observed change in factor prices at the experimental site is the appropriate discount, whatever the components of change. We do not expect the increment of housing-service production caused by the allowance program to have a significant impact on factor prices; if it does, the market model described below would require an additional equation to estimate the effect of changes in the level of housing-service production on factor prices.

variables describing the landlord, his building, and its neighborhood that (1) are statistically associated with response differences among producers, and (2) whose values can be estimated for places other than the experimental sites.

The considerations reviewed above lead us to a functional form that is reasonably consistent with our purposes and whose parameters can be estimated by multiple-regression analysis of cross-sectional data for individual properties monitored in the Supply Experiment:

$$\frac{\dfrac{Q_t - Q_o}{Q_o}}{\dfrac{\Pi_{t-1} - \Pi_o}{\Pi_o}} = f(B_o,\ L_o,\ N_o,\ \Pi_o) \qquad \text{(E-6)}$$

where Q_t = output of housing services during year t;

$\Pi_t = P_t/F_t$ = producer's actual* markup on factor costs during year t;

P_t = average price received per unit of output during year t;

F_t = average factor cost per unit of output during year t;

B_t = a vector of building characteristics as of year t;

L_t = a vector of landlord characteristics as of year t; and

N_t = a vector of neighborhood characteristics as of year t.

The expression on the left-hand side of Eq. (E-6) is the observed supply response of one producer for year t. Its numerator measures the percentage by which his output increased between the base year and

---

*As distinguished from his *expected* markup, which would be based on *expected* prices for output and factors of production, i.e.,

$$E\left[\Pi_t\right] = \frac{E\left[P_t\right]}{E\left[F_t\right]} .$$

year t; its denominator measures the percentage by which this profit margin increased between the base year and year $(t - 1)$. We thus assume that his most recent experience with market conditions governs his production plans for year t.

The right-hand side of the equation offers the hypothesis that a producer's output response to a change in profit margin is conditional on the variables there specified: the base-year characteristics of the building, the landlord, and the neighborhood; the base-year level of output; and the base-year markup rate. The purpose of the proposed regression analysis is to estimate the parameters of the partial relationships between observed supply response and each variable in the argument of the function.

If these parameters can be estimated with reasonable precision on a sample of properties varying with respect to B_o, L_o, N_o and Π_o, for periods $(t = 1, 2, 3, \ldots)$ of varying distance from baseline $(t = o)$, they will help us to model a "behavioral" supply function for an individual producer. The general form of such a function would be:

$$S_t = f\left(B_o,\ L_o,\ N_o,\ P_o,\ F_o,\ E\left[P_t\right],\ E\left[F_t\right]\right). \qquad \text{(E-7)}$$

Here, S_t is the planned level of output for year t, which is related explicitly to the base-year circumstances by the producer and to expected prices for output and expected costs of production in year t. The derivation of Eq. (E-7) from a fitted version of Eq. (E-6) entails both mathematical manipulation of the terms of Eq. (E-6), the nature of which depend on its actual functional form, and the assumption that $E\left[\Pi_t\right] = \Pi_{t-1}$. While it is not hard to imagine practical complications in going from Eq. (E-6) to Eq. (E-7), their seriousness cannot be easily assessed prior to specifying the functional form of Eq. (E-6).

If we are further willing to assume mutual independence of landlords' responses to market signals, we can aggregate over any set of landlords for each of whom B, L, and N are known in any local market for which factor costs F are known; these known quantities will define the relationship between the preferred level of output and the price of housing services for each producer, and these individual supply

schedules can be added to obtain an aggregate supply schedule as a function of price. Hereafter, we will let Eq. (E-7) stand for such an aggregate supply function; for convenience in exposition, we will also drop the subscript detail, relying on the reader's memory to fill out the generalized relation,

$$Q_S = S(B, L, N, T, F, P) \qquad \text{(E-8)}$$

ANALYTICAL INTEGRATION

Suppose we now consider a local housing market other than those in which the Demand and Supply Experiments were conducted. Assume that we are able to describe the structure of that market, both with respect to the characteristics of housing consumers *(H)*, their incomes *(Y)*, and their total housing expenditures *(R)*; and with respect to the characteristics of its housing inventory *(B)*, its landlords *(L)*, and its neighborhoods *(N)*. We wish to estimate the effects of a housing allowance program--either a housing-gap allowance or a housing-discount allowance--on the price and quantity of housing services in the local market.

We may begin by describing the state of that market before the hypothetical allowance program is installed. From Eqs. (E-3) and (E-8) we can form a market-clearing equation of the following form:

$$D(H, Y, P) = S(B, L, N, F, P) \ , \qquad \text{(E-9)}$$

where $D = S = Q = R/P$ is the quantity of housing services that clears the market--not in the sense of eliminating all vacancies, but in the sense that landlords continue to offer the vacant units for rent rather than withdrawing them from the market.

Although the Demand and Supply Experiments provide us with estimates of parameters for all of the variables in Eq. (E-9), the units of account for some of these variables were arbitrarily defined at each experimental site. To combine them in Eq. (E-9), and to apply Eq. (E-9) to housing markets other than the experimental sites, we must adjust all such variables to common units of account. The problem involves

Y and P in the demand function and F and P in the supply function and is resolvable with the aid of price indexes.

First, we will need a consumer price index to transform Y_n as measured at the new site to dollars whose purchasing power is equivalent to the dollars in which Y_d was measured in the Demand Experiment. Where ϕ is the appropriate deflator, we can then use ϕY_n as an argument in the demand function whose unit of account is consistent with the fitted parameters.

Second, we will need an index relating factor prices in both the Demand and Supply Experiments to those of the new site. Such an index will enable us to adjust both F and P to common units of account, as explained below.

To fit the parameters of our demand function, we first arbitrarily set the baseline market price of housing services, $P_d = \$1$, so that the quantity of housing services demanded could be measured by

$$Q_d = \frac{R_d}{P_d} \quad , \tag{E-10}$$

where R_d is total housing expenditures in current dollars. Once the unit of measurement for Q_d was thus defined, the variations in price needed to measure demand response were achieved by charging allowance recipients only βP_d, varying β; covariation of Q_d with βP_d could then be observed and is the analytical equivalent of varying P_d.

In the Supply Experiment, our system of measurement, as explained in Appendix B, is generally designed to avoid the necessity of specifying the unit of account for housing services; it deals in relative changes only. Implicitly, however, that unit of account is defined by

$$Q_s = \frac{V_f}{P_f} \, , \tag{E-11}$$

where V_f is total expenditures for factors of production at baseline and P_f is the unit price of a composite factor of production, arbitrarily setting $P_f = \$1$. With Q_s thus defined, it follows that

$$P_s = \frac{R_s}{Q_s} \quad , \tag{E-12}$$

where R_s is total revenue from the sale of housing services. As factor prices change over time, we index them $(P'_f = \lambda P_f)$; the index λ is then used to deflate total factor expenditures in current dollars, preserving the same unit of account for housing services:

$$Q'_s = \frac{V'_f}{P'_f} = \frac{V'_f}{\lambda P_f} \quad , \tag{E-13}$$

where the prime symbols indicate observations at a later date, and remembering that $P_f = \$1$.

Under certain assumptions, this technique of indexing factor prices enables us to establish a common unit of account for Q_d, Q_s, and Q_n, where Q_n is the quantity of housing services that clears the market at our "new" site. The assumptions are as follows:

1. At all three sites, households with the same specified characteristics (H, Y) have the same demand for housing services relative to their price.
2. At all three sites, landlords with the same specified characteristics (B, L, N) are willing to supply the same amount of housing services, given a specified market price and a specified factor cost per unit of output.
3. At all three sites, landlords with the same specified characteristics (B, L, N) face the same production function.

First, we adjust for factor-price differences among the three sites, just as in Eq. (E-13) we adjusted for factor-price changes over time. Letting $F = P_f Q_f$ for a fixed bundle of factors Q_f, we can directly measure P_f at each site and calculate deflators γ and λ such that:

$$F_n = \gamma F_d = \lambda F_s \quad . \tag{E-14}$$

Then, if we take the output quantities Q_d and Q_s as originally measured at each site, it follows that equivalent values at the new site are defined by

$$Q_n = \gamma Q_d = \lambda Q_s \ ; \tag{E-15}$$

then, from Eq. (E-12), equivalent prices for housing services are defined by

$$P_n = \gamma^{-1} P_d = \lambda^{-1} P_s \ . \tag{E-16}$$

Making the appropriate substitutions in the arguments of our demand and supply functions, our market-clearing equation becomes

$$\gamma D(H_n, \ \phi Y_n, \ \gamma P_n) = \lambda S(B_n, \ L_n, \ N_n, \ F_n, \ \lambda P_n) \ . \tag{E-17}$$

However, to simplify notation once again, we will assume that all the appropriate adjustments in units of account have been made and return to the unsubscripted notation of Eq. (E-9).

MARKET EFFECTS OF AN ALLOWANCE PROGRAM

To estimate the effects of a housing allowance program on the price and quantity of housing services in the local housing market described above, we must alter the arguments in our demand function to reflect the specifications of the allowance program, then find the price at which the demand for and supply of housing services would be equal. The procedure differs for a housing-gap allowance program and a housing-discount allowance program.

Housing-Gap Allowance Program

A housing-gap allowance formula provides for allowance payments equal to a specified percentage of disposable income, the percentage dropping to zero as income increases; if it is to be other than a general income supplement, it also must earmark some or all of the allowance payment for housing expenditures.

For a given household population whose incomes are known, we can estimate the change in income (ΔY) that would result from a specified housing-gap allowance program; together with the earmarking provision (A), this change in income will determine how demand responds to the market price of housing services (P):

$$Q_d = D(H, A, Y + \Delta Y, P) \quad . \tag{E-18}$$

The allowance program has no effect on the argument of our supply function:

$$Q_s = S(B, L, N, F, P) \quad . \tag{E-19}$$

For the specific local market, we know the baseline characteristics of the housing stock, landlords, and neighborhoods (B, L, and N); the unknowns are factor prices (F) and the price of housing services (P). Finally, we need a market-clearing equation,

$$Q_d = Q_s \quad . \tag{E-20}$$

We would propose to solve this system of three equations in four unknowns by predicting a value for F, the solution then being conditional on the accuracy of that prediction. As noted earlier, a priori analysis of incremental factor-demands associated with plausible levels of allowance payments lead us to doubt whether factor prices will be much affected by a local allowance program;* and even in the case of a national allowance program, we would expect program effects to be swamped by independent events in national factor markets--particularly, in capital markets. We doubt that much reliability would be gained by modeling

$$P_f = f(Q_s) \tag{E-21}$$

*See Appendix D.

from data gathered by the Supply Experiment, but it remains a possibility if we find evidence of substantial allowance-induced inflation in factor prices.

In reduced form, our market-clearing equation is solved by finding ΔP such that

$$D(H,\ A, \Delta Y+\ Y,\ P+\Delta P) = S(B,\ L,\ N,\ F+\Delta F,\ P+\Delta P)\ . \qquad \text{(E-22)}$$

Comparing the solution values of $D = S = Q$ for this case to those of Eq. (E-9), with preallowance values, we can measure the relative change in the flow of housing services $(\Delta Q/Q)$ resulting from the allowance program and the associated change in the average price of housing services $(\Delta P/P)$. Using the marketwide solution value of $(P + \Delta P)$ as an argument in the demand function, we can also estimate how housing consumption would change for particular classes of households (e.g., recipients vs. nonrecipients).

Housing-Discount Allowance Program

For an allowance program employing the housing-discount formula, the solution is more complicated. First, the effective unit price of housing services to allowance recipients differs from the unit price to nonrecipients and to suppliers of housing services. Second, the amount of the income change for allowance recipients cannot be computed a priori; the income change consists of a refund on their housing expenditures, and the amount of these expenditures cannot be deduced solely from program regulations.

As noted earlier, under a housing-discount formula, the price of a unit of housing services to the recipient is βP, $\beta < 1$; for nonrecipients, $\beta = 1$. Therefore, in constructing our marketwide demand function, we must aggregate individual demand functions of the form

$$Q_d = D(H,\ Y+\Delta Y,\ \beta P)\ , \qquad \text{(E-23)}$$

in which both ΔY and P are variables. Therefore, we need an additional equation,

$$\Delta Y = (1-\beta)PD \qquad \text{(E-24)}$$

which says that the change in income as a result of the allowance program is equal to the amount of the refund on actual housing expenditures by allowance recipients.

Our reduced-form market-clearing equation is then

$$D\left[H,\ Y+(1-\beta)(P+\Delta P)(D+\Delta D),\ \beta(P+\Delta P)\right] = S\left[B,\ L,\ N,\ F+\Delta F,\ P+\Delta P\right], \qquad \text{(E-25)}$$

which can be solved for ΔP as before, conditional on a predicted value for ΔF.

CONCLUSIONS

In principle, the data from separately conducted Demand and Supply Experiments can be combined analytically to estimate the consequences of either a housing-gap or a housing-discount allowance program applied to a housing market other than those that served as experimental sites. The application of the principle, however, is extremely complicated; the exposition above bristles with unresolved technical issues and, as we have discovered on each review, with hidden assumptions. We offer it only as a general analytical plan that could be pursued at various levels of sophistication. Its critical features can be simply stated:

1. Household responses to the allowance program, observed in the Demand Experiment, can be made reasonably portable by relating them to household characteristics, income, and the price of housing services.
2. Supplier responses to the allowance program, observed in the Supply Experiment, can be made reasonably portable by relating them to the baseline circumstances of the individual producer, the price of housing services, and the cost of producing them.
3. To estimate the effects of an allowance program in any given local housing market, these portable parameters of demand and

supply responses can be applied to the local population of demanders and suppliers, if their relevant characteristics are known.

4. Given local factor prices, the market-clearing price and quantity of housing services can be estimated by aggregating demand and supply responses to price changes over the relevant local populations of demanders and suppliers.

Our principal reservations about this method for analytical integration and extension of site-specific findings can also be summarized briefly:

- o On both the demand side and the supply side of the market, it must be assumed that each participant responds independently to market signals, so that individual supply and demand functions can be aggregated.
- o It must be assumed that the arguments of the demand and supply functions include all the relevant variables. Those that are likely to be omitted are "background" variables, general characteristics of the experimental sites. In both experiments, the number of sites is too small to provide enough variation in these variables to allow them to be used for parameter estimation, but sites are being selected deliberately for such differences, so that there will at least be some evidence that our response parameters are either stable or unstable under different background conditions.
- o To combine the fitted demand and supply functions into a single analytical model and apply them to a new site, it is necessary to establish a common unit of account for incomes and for housing services, so that fitted parameters are consistent with the data for the new site. This can be done with the aid of a consumer price index, a factor-price index, and some strong assumptions.

- o The range of observations on which the demand and supply functions are estimated limits the applicability of the fitted functions to new sites, where the data may fall outside that range.
- o While the experiments provide data that can be used in conjunction with untested public contribution rates for either housing-gap or housing-discount formulas, earmarking provisions must have been tested. If they are of a kind that renders certain building types ineligible, or that requires a minimum level of housing services, they must be tested in the Supply as well as the Demand Experiment, or else their effects on suppliers in those market sectors must be modeled.

If this scheme for analytical integration of data from the Demand and Supply Experiments survives general scrutiny, the designers of both experiments should keep in mind its data requirements. It does not appear to call for any drastic revision of present plans. In crude form, at least, it can be implemented and would be useful to HUD and to others concerned with housing allowances. However, we suspect that the analytical extensions of experimental findings described here will have much less influence on thinking about housing allowances than the more directly observable outcomes of the experimental programs at the sites where they are mounted.

Appendix F

WORKING NOTES PREPARED FOR THE HOUSING ASSISTANCE SUPPLY EXPERIMENT

WN-7711-UI — *Testing the Supply Response to Housing Allowances: An Experimental Design*, Ira S. Lowry, C. Peter Rydell, and David de Ferranti, December 1971.

WN-7833-HUD — *Site Selection for the Housing Assistance Supply Experiment: Stage I*, Housing Assistance Supply Experiment Staff, May 1972.

WN-7866-HUD — *Preliminary Design for the Housing Assistance Supply Experiment*, I. S. Lowry, June 1972.

WN-7883-HUD — *Preliminary Description of Survey Instruments*, Housing Assistance Supply Experiment Staff, June 1972.

WN-7885-HUD — *Data Management System: Part I, Fieldwork Data and Data Transfer Specifications*, G. Levitt, July 1972.

WN-7888-HUD — *Phase II: Price Controls and the Housing Assistance Supply Experiment*, D. B. Lewis, July 1972.

WN-7895-HUD — *Failure Mode Analysis for the Housing Allowance Program*, Robert A. Levine, July 1972.

WN-7901-HUD — *Preliminary Estimates of Enrollment Rates and Allowance Costs*, Barbara M. Woodfill, July 1972.

WN-7907-HUD — *Site Selection for the Housing Assistance Supply Experiment: SMSAs Proposed for Site Visits (A Briefing)*, Housing Assistance Supply Experiment Staff, August 1972.

WN-7953-HUD — *Data Management System: Part II, The Management of Data for Analysis*, G. Levitt, August 1972.

WN-7974-HUD — *Estimates of Eligibility and Allowance Entitlement Under Alternative Housing Allowance Programs*, Barbara M. Woodfill and Tiina Repnau, September 1972.

WN-7980-HUD — *Contingency Planning for the Supply Experiment*, Ira S. Lowry, October 1972.

WN-7982-HUD — *Supplemental Design Papers for the Housing Assistance Supply Experiment*, Housing Assistance Supply Experiment Staff, July 1972.

WN-8025-HUD *Funding Housing Allowances for Homeowners Under Sec. 235*, Mack Ott, November 1972.

WN-8028-HUD *Housing Allowances and Household Behavior*, Ira S. Lowry, Mack Ott, and Charles Noland, January 1973.

WN-8029-HUD *Sample Design for the Housing Assistance Supply Experiment*, Timothy M. Corcoran, Eugene C. Poggio, and Tiina Repnau, November 1972.

WN-8034-HUD *Collected Site Selection Documents: Housing Assistance Supply Experiment*, R. Dubinsky, January 1973.

WN-8043-HUD *Five Site Evaluation Papers*, Housing Assistance Supply Experiment Staff, February 1973.

WN-8054-HUD *Data Management System for the Housing Assistance Supply Experiment*, C. M. Dodd, M. C. Fujisaki, and G. Levitt, November 1972.

WN-8100-HUD *Analysis Plan for Indirect Suppliers and Market Intermediaries*, William Grigsby, January 1973.

WN-8101-HUD *Preliminary Description of Sample-Selection Procedure*, Eugene Poggio, January 1973.

WN-8102-HUD *Analysis Plan for Residential Mobility*, Robert P. Althauser, Harrison S. Campbell, and Ira S. Lowry, January 1973.

WN-8105-HUD *Estimating the Standard Cost of Adequate Housing* David B. Lewis and Ira S. Lowry, March 1973.

WN-8110-HUD *Analysis Plan for Measuring Supply Responsiveness*, Ira S. Lowry, January 1973.

WN-8111-HUD *Analysis Plan for Measuring Effects on Nonrecipients*, Robert P. Althauser, William Grigsby, Harrison S. Campbell, and Ira S. Lowry, January 1973.

WN-8124-HUD *Generalizing from the Supply Experiment*, Ira S. Lowry, January 1973.

WN-8133-HUD *A Dynamic Model of the Production Function for Housing Services*, Ira S. Lowry, C. Peter Rydell, and David de Ferranti, February 1973.

WN-8134-HUD *Accounting for Supply Responses*, Ira S. Lowry, February 1973.

WN-8157-HUD *An Overview of the Supply Experiment*, Ira S. Lowry, February 1973.

WN-8167-HUD *Additional Estimates of Enrollment and Allowance Payments Under a National Housing Allowance Program*, Tiina Repnau and Barbara M. Woodfill, March 1973.

WN-8171-HUD *The Experimental Housing Allowance Program*, David B. Lewis, Alan Greenwald, and Ira S. Lowry, February 1973.

WN-8174-HUD *The Effects of Nonresponse on Record Completion in a Panel of Residential Properties*, Timothy M. Corcoran, April 1973.

WN-8201-HUD *Sample-Selection Procedures for Site I*, Eugene C. Poggio, March 1973.

WN-8209-HUD *The Housing Allowance Office: Functions and Procedures*, Alan Greenwald and David B. Lewis, March 1973.

WN-8218-HUD *The Role of Household Survey Data in the Supply Experiment*, Adele Massell (ed.), March 1973.

WN-8220-HUD *Adjusting for Regional and Local Price Changes*, Charles Noland and Ira S. Lowry, March 1973.

WN-8243-HUD *Monitoring the Supply Experiment*, Ira S. Lowry, Harrison S. Campbell, and Malcolm A. Palmatier, April 1973.

WN-8257-HUD *Organizing the Data for Analysis*, Ira S. Lowry, Harrison S. Campbell, and Tiina Repnau, May 1973.

WN-8268-HUD *Compensating for Landlord Nonresponse in the Housing Assistance Supply Experiment*, Adele Massell (forthcoming).

RELATED BACKGROUND STUDIES

P-4645 *Housing Assistance for Low-Income Urban Families: A Fresh Approach*, I. S. Lowry, May 1971.