WN-8396-HUD

PROCEEDINGS OF THE GENERAL DESIGN REVIEW OF THE HOUSING ASSISTANCE SUPPLY EXPERIMENT

Held June 25-27, 1973, at
The Rand Corporation
2100 M Street
Washington, D.C.

October 1973

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

PREFACE

This Working Note was prepared for the Office of Policy Development and Research, U.S. Department of Housing and Urban Development. It is a summary of the proceedings of a three-day meeting convened by Rand to review the design of the Housing Assistance Supply Experiment. The Review Panel consisted of twelve scholars, chosen jointly by Rand and HUD for their technical competence and experience in policy-related research. Other participants included representatives of the Office of Policy Development and Research and related offices of HUD, representatives of the Urban Institute's staff for the Experimental Housing Assistance Program, Rand's staff for the Housing Assistance Supply Experiment, and representatives of Mathematica, Inc., field work subcontractors for the experiment.

This Note includes both a summary of the discussion during the three-day review and supplementary memoranda provided by some of the panelists. During the sessions, notes of the discussion were taken by Tiina Repnau and Charles Noland; aided by Adele P. Massell, they prepared a draft of the Proceedings which was circulated to all participants for correction and amplification. The present version incorporates these corrections. Janet DeLand edited the typescript and supervised production of final copy.

Although the panelists were provided with all of the relevant Working Notes pertaining to the Housing Assistance Supply Experiment, the principal basis for the reviewers' deliberations was Rand's *General Design Report: First Draft.*[*] Rand's response to this peer review is reported in a Supplement to that report,[**] prepared in the weeks immediately following the three-day session herein described. Because of its timing, the Supplement cites passages of an earlier draft edition of the Proceedings, some of which were subsequently altered in language

---

[*] Ira S. Lowry (ed.), *General Design Report: First Draft*, The Rand Corporation, WN-8198-HUD, May 1973.

[**] Ira S. Lowry (ed.), *General Design Report: Supplement*, The Rand Corporation, WN-8364-HUD, August 1973.

(though not in intent) by the participants; thus, readers of the Supplement may in a few instances be unable to identify passages of the review proceedings cited there.

This Working Note was prepared pursuant to HUD Contract H-1789, as amended in June 1973. It documents the fulfillment of requirements set forth in Task 2.1 for a peer review of the experimental design.

CONTENTS

INTRODUCTION

This Working Note reports the proceedings of a review of Rand's proposed design for the Housing Assistance Supply Experiment, as described in the *General Design Report: First Draft.*[*] The Review Panel consisted of the following twelve scholars, chosen jointly by Rand and HUD for their technical competence and experience in policy-related research:

Henry Aaron	Senior Fellow, The Brookings Institution
David M. Austin	Research Associate, Harvard-M.I.T. Joint Center for Urban Studies
Lee Bawden	Department of Economics, University of Wisconsin
Robert Crane	Department of Social Relations, The Johns Hopkins University
Frederick O'R. Hayes	Fund for the City of New York
Raymond J. Jessen	Graduate School of Management, University of California, Los Angeles
John F. Kain	Department of Economics, Harvard University
Edwin S. Mills	Department of Economics, Princeton University
Alexander M. Mood	Director, Public Policy Research Organization, University of California, Irvine
Richard Muth	Department of Economics, Stanford University
Alice M. Rivlin	Senior Fellow, The Brookings Institution; Chairman, Design Review Panel
Harold M. Watts	Director, Institute for Research on Poverty, University of Wisconsin
John Wilson	North Star Research and Development Institute, Minneapolis

At Rand's invitation, eleven of these panelists[**] convened in Washington, D.C., for a three-day review of the experimental design. Prior to the meeting, each had been provided with copies of the *General*

---

[*] The Rand Corporation, WN-8198-HUD, May 1973.

[**] Mr. Watts was unable to attend.

Design Report and a number of other documents detailing various aspects of experimental design. The scope of the review proposed to the panelists was as follows:

1. The research objectives of the experiment as they relate to policy issues likely to be raised in connection with proposals for a national program of housing allowances for low-income families.
2. The appropriateness of the proposed experimental allowance program as a vehicle for testing the effects of a national program.
3. The appropriateness of the criteria employed for selection of experimental sites.
4. The reliability and efficiency of the proposed monitoring program, including the sample design and the techniques of data collection and data handling.
5. The appropriateness of the analyses proposed for seeking answers to the research questions to be addressed, and the adequacy of the data base for performing these analyses.
6. The degree to which the experiment is exposed to contingencies that might prevent its completion, invalidate the proposed analyses, or damage the credibility of experimental findings.
7. Whether data needed for evaluation of the effects of a national housing allowance program could be procured by alternative means at substantially less expense.

In addition, the panelists were requested to give particular attention to specific features of experimental design concerning which questions had previously been raised by HUD or other parties to the Experimental Housing Assistance Program:

1. Is the proposed sample of tax parcels appropriate to provide results generalizable to the population of households at the experimental sites or to significant subpopulations, e.g., recipients, nonrecipients, minorities, elderly?

2. What are the advantages and disadvantages of linking data by tax parcel and how important are they in answering the research questions?
3. Will the procedure proposed to compensate for landlord nonresponse provide an adequate backup measure of supply elasticity if nonresponse among landlords is very high? Is it an efficient way to provide the backup capability?
4. Is the proposed measurement framework and analysis for measuring the supply response in the homeownership sector appropriate and efficient?
5. How and to what extent can information obtained from two sites be used in predicting responses to alternative housing allowance programs in the rest of the United States? What additional steps could be taken in the Supply Experiment or in related research efforts to enhance the applicability of the Supply Experiment findings?
6. How is the population value of housing supply elasticity defined? If it is a function of aggregated supply changes and aggregated revenue changes, is the proposed estimator, which is a sample average of individual elasticities, appropriate? If a different estimator is desirable, is the proposed sample design an efficient one?
7. In what ways should the housing allowance program design, the sampling framework for household and housing-unit data, and the data collected in the Supply Experiment be consistent with those employed in the Demand and the Administrative Agency Experiments?

The proceedings were organized into topical sessions, generally following the outline of the *General Design Report*. At the beginning of each session, a member of Rand's staff for the Supply Experiment briefly reviewed the relevant design proposals and the reasoning behind them; thereupon, the floor was opened to questions and discussion by the panelists and other participants.

No attempt was made to obtain a consensus report from the panelists. Instead, at the final session, each was asked in turn to summarize his views and recommendations. In addition, all were invited to supplement their remarks by written memoranda; six such memoranda were received and are appended to this record of the Proceedings.

In addition to the designated review panelists, the participants in the three-day conference included representatives of the Office of Policy Development and Research and related offices of HUD; representatives of the Urban Institute's staff for HUD's Experimental Housing Assistance Program; Rand's staff for the Housing Assistance Supply Experiment; and representatives of Mathematica, Inc., fieldwork subcontractors for the Supply Experiment. Observers from Abt Associates, contractors for the Housing Assistance Demand Experiment and Administrative Agency Experiment, were also present. These participants and observers are listed below, alphabetically by organization:

Abt Associates, Inc.

S. Kennedy
J. Wallace

Department of Housing and Urban Development

G. Allen
K. Alles*
T. Connell*
C. Field*
J. Fitts*
E. Glatt*
A. Unger*

Mathematica, Inc.

J. Dixon
D. Kershaw*
M. Scowcroft

The Urban Institute

G. Buchanan*
J. Heinberg*
R. Melton
L. Ozanne*
G. Taher
C. Thomas*

New York City-Rand Institute

P. Rydell*

The Rand Corporation

D. Alesch*
B. Askin
S. Berry
Z. Blum-Doering*
T. Britt
T. Corcoran*
R. Dubinsky*
B. Ellickson*
A. Greenwald*
W. Grigsby*
D. Hensler*
R. Levien*
G. Levitt
D. Lewis*
I. Lowry*
A. Massell*
C. Nelson*
C. Noland
M. Ott*
G. Poggio*
T. Repnau*
B. Woodfill

*On record in the Proceedings.

In the following pages, we summarize the discussion of each design topic. The summary is based on notes taken by Rand staff members during the proceedings. Subsequently, all participants on record were invited to correct or amplify the draft record; such changes are incorporated here.

MONDAY, 25 JUNE 1973

9:15 a.m.: CONFERENCE ARRANGEMENTS AND AGENDA

The session opened with remarks by Rivlin. Field then described the background of the Housing Experiment from HUD's point of view, describing the policy issues leading to the Demand, Administrative, and Supply Experiments. He also gave the history and chronology of Rand's involvement in the Supply Experiment. Lowry then expressed Rand's need for outside review to identify problems in the experimental design, emphasizing two major questions: (1) Does the general experimental strategy make sense? (2) What can we do now, given the work that has already been done and the time schedule set for the Experiment?

9:30 a.m.: POLICY ISSUES AND RESEARCH OBJECTIVES

Lowry presented a briefing on Policy Issues (summarized in the following charts). Mills responded to the briefing by stating his view that the key issues are the benefits and problems of the allowance program. He questioned what one gets from direct regulation that would be lacking if certification requirements were dropped. He suggested using two metropolitan areas, dropping direct regulation in one of them, to examine the differences and to see in which city the low-income people obtain better housing.

Wilson stated that classical controls would be desirable, but that exogenous factors may swamp the comparative results. Assuming that the experiment can achieve internal and external validity, Wilson asked what the implications are for a national housing program. He stated that finding these implications requires (1) the ability to integrate the results of the Demand and Supply Experiments, (2) generalization from HASE results to the nation as a whole, and (3) the use of experimental treatments that are not totally dissimilar to those of a prospective national program. He expressed concern about using Appendix E of the Design Report* for Point 1 above, since Rand slides over the problem

---

*Ira S. Lowry (ed.), *General Design Report: First Draft*, The Rand Corporation, WN-8198-HUD, May 1973.

PRINCIPLES OF A HOUSING ALLOWANCE PROGRAM

PURPOSE:

TO ENABLE LOW-INCOME HOUSEHOLDS TO OCCUPY MINIMUM STANDARD HOUSING

STRATEGY:

1. DIRECT FINANCIAL ASSISTANCE TO ELIGIBLE HOUSEHOLDS
2. OPEN ENROLLMENT FOR ELIGIBLE HOUSEHOLDS
3. ASSISTANCE EARMARKED FOR HOUSING
4. HOUSING OBTAINED BY RECIPIENTS THROUGH ORDINARY MARKET CHANNELS

GENERAL POLICY ISSUES

- ARE HOUSING ALLOWANCES COST-EFFECTIVE COMPARED TO ALTERNATIVES ?
- IS A FULL-SCALE PROGRAM ADMINISTRATIVELY FEASIBLE ?
- HOW MUCH WOULD IT COST ?
- ARE SIDE-EFFECTS EITHER BENEFICIAL OR TOLERABLE ?

RESEARCH QUESTIONS ADDRESSED BY SUPPLY EXPERIMENT

1. SUPPLY RESPONSIVENESS TO HOUSING ALLOWANCES

2. BEHAVIOR OF MARKET INTERMEDIARIES AND INDIRECT SUPPLIERS

3. RESIDENTIAL MOBILITY AND NEIGHBORHOOD CHANGE

4. EFFECTS ON NONPARTICIPANTS

EXPERIMENTAL STRATEGY

1. CREATING A MARKET CONTEXT

2. PROVIDING A PERCEPTIBLE AND STABLE DEMAND STIMULUS

3. MEASURING CHANGES IN THE FLOW AND PRICE OF HOUSING SERVICES

4. DURATION OF THE MONITORING PROGRAM

5. EXPERIMENTAL CONTROLS

6. CREDIBILITY OF EXPERIMENTAL FINDINGS

after presenting the mathematical model. He wants HUD to assign the responsibility for integration to one of the two major contractors. Wilson feels that without such integration, we cannot go from experimental results to recommendations for national policy. He did not suggest changing the Supply Experiment to analyze different things; rather, he wants to make one contractor responsible for integration.

Kain then suggested that a control city be included in the Experiment, i.e., Rand should monitor a similar metropolitan market to help determine which results are attributable to the Experiment and which are due to exogenous forces. He expressed the fear that the HASE may be blamed for anything that goes wrong--for example, if the Green Bay Packers lose 10 games.

The discussion proceeded as follows:

Lowry: Rand has thought about the experimental-control problem at length. We've considered a natural experiment, i.e., monitoring an area in which the poor are getting richer and demand is increasing, then analyzing the data ex post; or we could use an actual control city for our experiment. There is a stumbling block: You need about the same level of design and modeling for such controls in order to observe the proper factors and excerpt exogeneous effects from applicable ones. If you have enough cities, you can randomize the effects out.

Kain: I am still concerned about background inflation effects.

Lowry: This issue has been addressed; we plan to use site-specific and regional price indexes to deal with the problem.

Kain: What are the implications of rapid background inflation?

Lowry: We would need many controls to say anything about cause/effect relations.

Bawden: You [Lowry] seem to feel that we don't need any controls, and I disagree. The fact is, we don't know much about the housing market. Therefore a control site is necessary.

Crane: I agree that a control site should be used. And how about the political context of the sites? What about inferences

to a national program? I am also concerned about the input accounting technique: We may increase inputs substantially and not get much improvement relative to another city that does not have allowances.

Mood: Housing allowances would simply inflate prices in the ghetto (in a tight housing market). How would the HASE address or help such areas?

Lowry: Rand has chosen two sites, one of which is 25 percent black, with a ghetto area. Extreme price inflation in such an area is a conceivable result, one which we wish to test in Saginaw and compare with Green Bay.

Mood: I would like to see one of the sites chosen in the South. The situation there is subtly different from that of black ghettoes elsewhere. Real-estate practices in the South are somewhat different from those in the North.

Field: As a point of clarification, the Demand and Administrative Experiments will cover other areas of the nation. One of the Demand Experiment sites, Phoenix/Maricopa County, Ariz., has a large Mexican-American population; the other site, Pittsburgh/Allegheny County, Pa., has a substantial black population. The Administrative Experiment sites are Salem, Ore., Springfield, Mass., Peoria, Ill., Jacksonville, Fla., San Bernardino County, Cal., Tulsa, Okla., the Bismarck area, N. Dak., and the Durham area, N.C.

Rivlin: But neither the Demand nor the Supply Experiment will be in the South. Can Mood's question be answered?

Field: I don't think so.

Wilson: What about the Rand proposal to go into a neighborhood in a large city? Why not go into a southern city?

Lowry: This issue has been postponed by Rand and HUD, in agreement.

Hayes: The sociology of a large urban ghetto in the Northeast is different from that in the experimental sites. This creates problems that we will not find in Green Bay or Saginaw. I agree with Mood's point about the South. Moreover, Rand must agree if they propose a miniexperiment in a large-city

neighborhood. Could you use mini-Supply Experiments which are neighborhood-oriented, for example, in the Northeast, South, and Southwest, to address the situations not found in Green Bay and Saginaw?

Lowry: We agree with the general point, and it's made clear in the Design Report that we would like to monitor a neighborhood in a large metropolitan area. The South fell out of our list of possible sites for a metropolitanwide experiment because most of the potential candidates there (in terms of size) seem to differ from each other and from cities in other parts of the country.

Rivlin: The recommendations so far imply that HUD should spend two to four times as much money on the Experiment.

Aaron: There are many other dimensions along which markets differ and we would want to see a control site for each one. I'm not sure that the benefits of additional sites outweigh the costs. Rather, I would like to see the experiment run in a larger metropolitan site. A *neighborhood* experiment in a large SMSA would probably not be worth the money, since it would be so different from an operating national program.

Austin: A simple extension to a large city is not desirable without a design change. A program which is limited to one type of housing-assistance policy would not deal with problems found in large cities. Another point concerns controls: There are other income-transfer programs. Some of them will be increasing benefits. Would such programs result in the same changes as a housing allowance program? We need a control to sort out the effects from increasing benefits of other transfer programs, particularly those affecting the elderly. I suggest monitoring other cities in the same state.

Bawden: Has any thought been given to having different minimum standards in the two sites?

Lowry: No, the specification of minimum standards is important in determining what happens. The level determines the results, but it's hard to make a small differentiation; we would need large differences in standards to be able to tell differences. Our standards are based on model national housing codes and the APHA standards, but the appropriate standard is really a political question. To pick any particular standard, one must guess at the political consensus. We focused on different market configurations in selecting our sites.

Rivlin: Back to the point made by Mills: Perhaps there should be no certification requirements.

Aaron: But that would just be income maintenance.

Mills: That's not correct. We could see what happens if building codes are enforced.

Aaron: Then it would be a code-enforcement experiment. Without some form of earmarking, a housing allowance is just a negative income tax under a false label.

Muth: The costs of a national program based on your standards would be too large. You would have to enforce a lower standard for all participants or a higher standard for only part of the population. Rand should vary the minimum standard in the two sites to analyze the results and determine the implications for a national program.

Lowry: If we are constrained to two sites, I would rather model differences in minimum standards than have to model differences in market structure.

Wilson: Muth is getting at the feasibility of a national program of this size. If the cost is $5 billion per year, do you drop the standards? Does Rand have estimates of the cost of a national program based upon its minimum standards?

Lowry: Yes, both Rand and HUD have made estimates.

Wilson: What are the implications of such estimates for program design? Is Rand looking at a national program that is feasible, or should it look at standards that are half as high?

Lowry: Our standard is the minimum which will have any significant impact on the market or make the allowance large enough that it would be worth the administrative costs. HUD is actually pushing for higher standards.

Mills: The Design Report indicates to me that the goal of the experiment is not to estimate the effects of a politically feasible national plan, but to estimate theoretical parameters--for example, of the supply curve.

Kain: Two points: (1) It would be better to scrap one site and substitute a control site, which would be less expensive because it can be monitored without allowance program costs; this would provide more information for generalization and inference. (2) The basic concept of the Supply Experiment is wrong. Rather than running an experiment that employs a sudden shock, we should be conducting a pilot demonstration program to see if a housing allowance program could be implemented without generating undesirable side effects. The differences are in the design of the implementation. The program should have a feedback control system that will allow the rate of enrollment to be varied to allow supply to keep up with demand. Over time, the demand increase will be accommodated by the market. The question is how much time will this take. A big shock is risky and may have undesirable side effects, possibly discrediting the program.

Bawden: Another method is to advertise rising standards over time.

Kain: But it is still essential to minimize the bad side effects.

Rivlin: This is a different question; this is not, Do we want an allowance, but How can we make it work?

Kain: Yes, but can we have an allowance program without undesirable side effects?

Nelson: What about the analytics of a demonstration project? What about a control site--would we need one then?

Kain: The stress on a control site is related to the objectives of the HASE. I would prefer a demonstration project, but if not, a control site should be used. Rand's analysis is

weak on dynamics; this is all the more reason to design a program that has maximum flexibility in responding to market signals.

Aaron: If the purpose is to test the impact of increases in demand from a feasible national program, the HASE gets bad results. Two times the stimulus may not produce two times the response. I think either Bawden's approach or graduated enrollment might be the solution. Also constraints must be determined--including their use and size--to distinguish the HASE from other income-maintenance programs.

Wilson: I agree that rapid expansion would cause problems. How about the possibility of a gradual increase in allowance standards? It's desirable to lessen the shock, and one possible way is to increase standards gradually. This makes more sense in the context of a national program. You can't have limited enrollment in a national program.

Austin: The eligibility pool for housing allowances should be different from the pool for other welfare programs. You should keep a fairly high level of allowances to distinguish the program from public assistance. Unless there is a substantial level of benefits from the beginning, there is little rationale for separate housing allowances, as contrasted to an increase in public-assistance benefits and coverage.

Wilson: I disagree. Earmarking makes the difference rather than the level.

Austin: At low income levels, the size of the allowance payment does make a difference. Earmarking has little effect at low income levels, since most households will be paying at or above the level of earmarked transfer anyway.

Lowry: In response to the suggestions for gradually phasing in the allowance program, I would like to note that it was a conscious design decision to speed up the clock. We want not only to study the short-run responses of the market to the introduction of an allowance program, but to estimate the long-run equilibrium adjustment of the market to

a full-scale permanent program. While the short-run market response has important political implications, the long-run market adjustment has greater relevance to the effectiveness of housing allowances as a tool of Federal housing policy. Because both the experimental allowance program and the monitoring program will have a limited duration, we want to get to full-scale operation as soon as is feasible. Gradual enrollment would extend the necessary monitoring period.

Bawden: A two-year phase-in only increases the length by one year.

Kain: The possible disastrous effects of fast enrollment could be great. Congress will probably not wait five years for results, anyway. Demonstrating that a large-scale program could be geared up without catastrophe would probably have considerable political impact.

Lowry: I would not recommend a housing allowance program on the basis of a demonstration merely that catastrophe was avoided. We also want to know if an allowance program accomplishes its housing goals.

Kershaw: If you go easy, Congress might not buy it, since this is not the way the real world works.

Hayes: I agree with Kain about the speed of start-up. If we begin to see bad results, we would want to modify the course of the experiment. And I'm worried about inmigrants not being covered. High attrition of participants may lessen the demand shift, thus affecting the measured supply response.

Lowry: We don't specifically exclude inmigrants. We just do not promise them anything. Based on observed attrition, we may reopen the rolls to inmigrants. The reason is that we do not want to encourage people to migrate to our two sites just to take advantage of the allowances.

Wilson: That is a sound design.

Muth: If you do have a constant level of infusion of expenditure, it is hard to distinguish between long- and short-run effects. Thus you need to include inmigrants as necessary.

Crane: I don't believe that inmigration would be affected by the allowance, which will be relatively small for each family.

Lowry: That is probably right, but we need to be able to place an upper limit on the budget.

Bawden: Do you have any estimates of the average annual cost of monitoring a control site?

Kershaw: It would cost approximately $1/2 million to $1 million per year.

Rivlin summarized the discussion, as outlined below:

1. Design Issues
   - Control sites
   - Number and location of experimental sites
   - Coverage of inmigrants
2. Experimental and Policy Issues
   - Certification requirements
   - Varying the level of the allowance
   - Demonstration vs. "shock" experiment

Fitts commented on the importance of integrated analysis, saying that HUD is aware of it. The Urban Institute is charged with integrating the results of the Demand and Supply Experiments.

11:30 a.m.: EXPERIMENTAL ALLOWANCE PROGRAM

Woodfill presented a briefing on Participation and Cost Estimates, and Dubinsky presented a briefing on Housing Allowance Office Procedures (summarized in the following charts). The discussion after the briefings went as follows:

Kain: How does the homeowner relationship work over time, say with respect to background inflation?

Dubinsky: R^* is the same for homeowners and renters.

Lowry: To protect the allowance agency, we don't want to commit ourselves to raising R^* for inflation.

ESTIMATES OF PARTICIPATION AND ENTITLEMENT:
GREEN BAY, WISCONSIN SMSA
SAGINAW, MICHIGAN SMSA
1970

HOUSEHOLD CHARACTERISTICS, 1970

	Green Bay, SMSA			Saginaw, SMSA		
	Renter	Owner	Total	Renter	Owner	Total
No. of Households	11,671	31,889	43,560	14,036	49,107	63,143
Median Household Size	2.3	3.6	3.1	2.4	3.3	3.1
Median Household Income, 1969 ($)	6,900	10,300	9,300	7,300	10,900	10,500

ALLOWANCE FORMULA

$$A = R^* - .25\ Y_d$$

DISPOSABLE INCOME, Y_d

$$Y_d = Y_g - T - S - W + I$$

EXCLUSIONS

SINGLE PERSONS UNDER 62 YEARS OF AGE
ARE INELIGIBLE

ESTIMATED NUMBERS ELIGIBLE AND COSTS

Item	Green Bay, SMSA			Saginaw, SMSA		
	Renter	Owner	Total	Renter	Owner	Total
HOUSEHOLDS ELIGIBLE	4,007	6,368	10,375	5,547	11,609	17,156
% of all households	34.3	20.0	23.8	39.5	23.6	27.2
TOTAL PAYMENT COSTS ($000)	1,778	2,477	4,255	3,181	5,659	8,840
AVERAGE PAYMENT COST ($)	444	389	410	573	487	515

ESTIMATED PARTICIPANTS AND COSTS

Item	Green Bay, SMSA			Saginaw, SMSA		
	Renter	Owner	Total	Renter	Owner	Total
HOUSEHOLDS PARTICIPATING	3,141	4,321	7,462	4,241	7,345	11,586
% of all households	26.9	13.6	17.1	30.2	15.0	18.3
% of all eligible	78.4	67.9	71.9	76.4	63.2	67.5
TOTAL PAYMENT COSTS ($000)	1,512	1,872	3,384	2,795	4,150	6,945
AVERAGE PAYMENT COST ($)	481	433	453	659	565	600

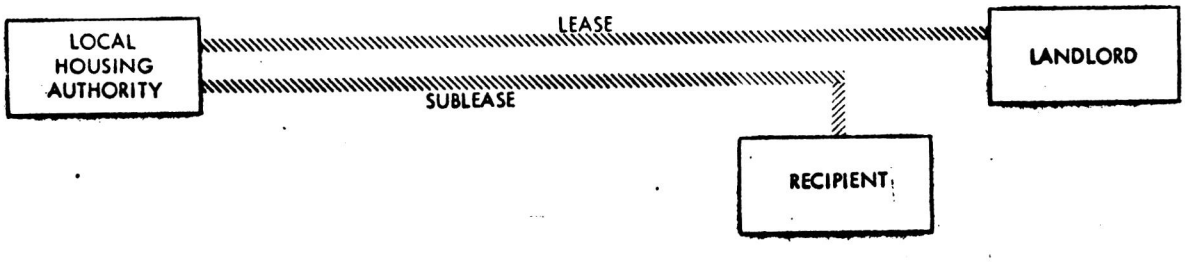
ESTIMATED COSTS IN 1973

	GREEN BAY SMSA	SAGINAW SMSA
HOUSEHOLDS PARTICIPATING	7,462	11,586
TOTAL PAYMENTS ($000) . .	3,806	7,878
AVERAGE ALLOWANCE ($) . .	510	680

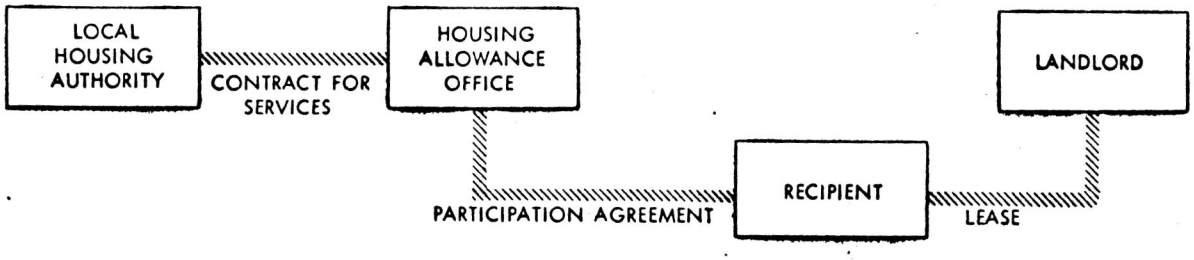
PROGRAM ADMINISTRATION

1. SOURCES OF FUNDS
2. HOUSING ALLOWANCE OFFICE
3. FORMS OF PAYMENT
   - RENTERS
   - HOMEOWNERS
   - HOMEBUYERS
4. ENROLLMENT PROCEDURES
5. DISBURSEMENT PROCEDURES
6. HOUSING CERTIFICATION

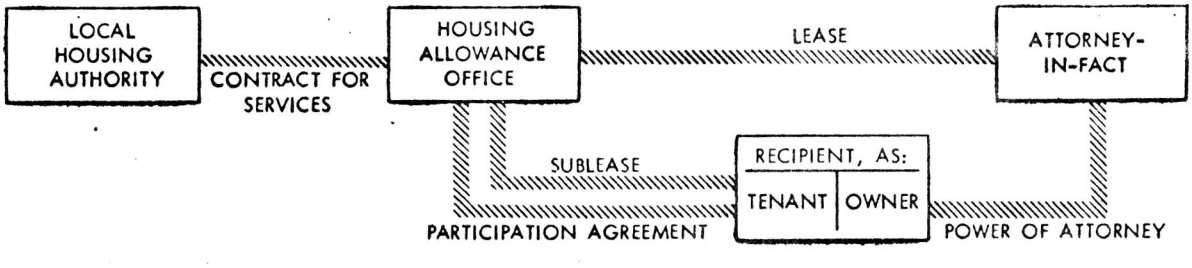
STANDARD CONTRACTUAL RELATIONSHIPS: SEC. 23 LEASED HOUSING PROGRAM



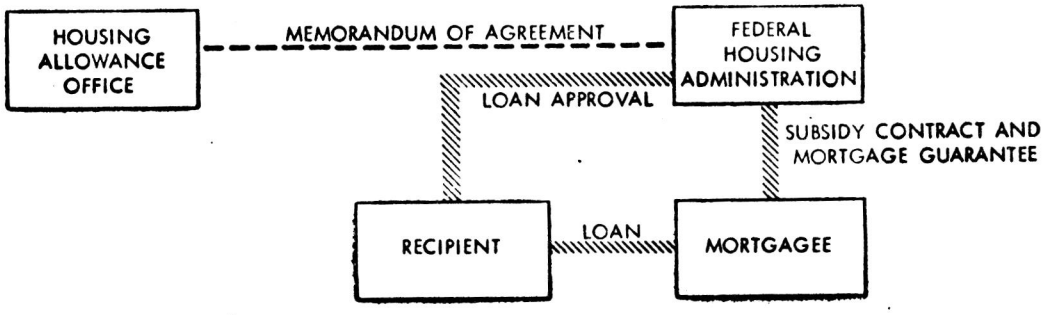
SPECIAL CONTRACTUAL RELATIONSHIPS: SEC. 23 EXPERIMENTAL HOUSING ALLOWANCE PROGRAM FOR RENTERS



SPECIAL CONTRACTUAL RELATIONSHIPS: SEC. 23 EXPERIMENTAL HOUSING ALLOWANCE PROGRAM FOR HOMEOWNERS



SPECIAL CONTRACTUAL RELATIONSHIPS: SEC. 235 EXPERIMENTAL HOUSING ALLOWANCE PROGRAM FOR HOME BUYERS



Wilson: Will the homeowner allowance exceed the cost of actually maintaining the home?

Lowry: Homeowner allowance payments are based on R^* and disposable income, which includes imputed income from equity in the home. Hence the allowance may exceed out-of-pocket payments, but it is unlikely.

Kain: How are other assets treated?

Lowry: Nothing specifically has been decided on this issue. Equity in homes is the only asset which is felt to be significant. Possibly cars and boats might be considered, but probably not. We do count explicit income from all assets. It is not clear that homeowners would get a higher allowance if they refinanced their homes, since any cash thus obtained would be an asset the income from which would be counted.

Bawden: Will Rand collect information on other assets to see if the Rand hypothesis is correct?

Lowry: Yes.

[Anon.]: The homeownership element of the program is very complex. Can't HUD provide a simpler method?

Field: We did go to Congress to try to get the law liberalized, and Congress did not do so. So we are using the Contra Costa method for treating homeowners under Sec. 23. Section 235 generally has higher eligibility standards and provides higher benefits than Sec. 23 for eligible participants.

Kain: Can people purchase homes under the HASE?

Lowry: Yes.

Kain: How many eligibles might want to become homeowners but don't qualify for Sec. 235 funds?

Lowry: We don't know. We guess not many. Households which don't qualify for Sec. 235 funds would have to obtain private financing. We think the allowance would be better for credit than equivalent earned income, but we do not know how mortgage lenders will react.

Kain: Have you estimated the number of eligibles who will take the home-purchase option?

Lowry: No.

Austin: What provisions have been made for newly formed households, and what about residency requirements?

Lowry: If the head of household meets the grandfather clause on residency in the site, the household will be eligible.

Muth: I have misgivings about using Sec. 235 funds in the experiment. Section 235 is a subsidy to a specific input. Why not allow people to buy a new home in the same way that homeowners are treated?

Lowry: We would like to, but we can't figure out how to do it.

Kain: Then you are confusing several effects. This changes the relative price of inputs (capital versus maintenance), and you are testing Sec. 235 in conjunction with a housing allowance.

Dubinsky: The number of families assisted under Sec. 235 will be small, but they cannot be excluded.

Aaron: Why can't you use experimental funds to get purchasers through the transaction period, and then apply Sec. 23 in the same way as for homeowners?

Lowry: We don't want to encourage people to take on 30-year mortgage commitments when we are giving only a 10-year allowance commitment.

Aaron: After 10 years a household will have built up enough equity in a home to let them get out safely.

Lowry: This may be so, but it is hard to predict. There are dangers in terms of political and moral implications of encouraging people to take on long-term financial obligations and then pulling the rug out from under them.

Kain: I would suspect that the number of households wanting to purchase homes would be larger than Rand is anticipating.

Lowry: We will have problems convincing homeowners to participate at all in the program.

Kershaw: The income-maintenance experiment in New Jersey was a short-term experiment in which a significant number of families purchased houses. Thus there exists a precedent for pulling the rug out from under owners. We want to do a follow-up on what happens to these owners.

Bawden: Imputing equity to some groups--such as farmers, where the house is a small part of the investment--presents problems. Requiring income recertification twice a year may also be a problem.

* * * * * *

BREAK FOR LUNCH

1:37 p.m.: CONTINUATION OF DISCUSSION

Hayes: I am concerned about the nature of the allowance formula. There are demands on income other than housing. Requiring households, regardless of size, to contribute 25 percent of their income for housing disregards these other demands.

Lowry: We did propose another formula (Housing Gap II), which comes closer to serving the purposes of a housing allowance program. HUD has disagreed on the grounds of (1) policy (low-income households get all their housing expenses paid, and HUD felt this was a bad idea) and (2) administration (we are restrained by the Brooke Amendment). HUD wanted us to use the same formula as the Demand Experiment is using.

Field: Twenty-five percent of adjusted income is approximately 20 percent of gross income. The Demand Experiment will vary the percentage of income--15 to 35 percent--which households must contribute to investigate the implications. To ensure continued funding we must use Sec. 23 funds. Thus we are constrained by the Brooke Amendment.

Hayes: The Brooke requirement is an average requirement. It does not preclude differential treatment of recipients according to income.

Field: But to integrate the Demand and Supply Experiments they must use the same formula, and the Demand Experiment is not using the Housing Gap II formula.

Hayes: A uniform percentage treatment (e.g., 25 percent) favors smaller households. While R^* does take some account of household size, it takes no account of higher food and clothing demands.

1:45 p.m.: SUPPLY RESPONSE

Lowry presented a briefing on the Supply-Response Analysis Plan (summarized in the following charts). Discussion followed:

POLICY ISSUES

IN AN ALLOWANCE-STIMULATED MARKET ....

- ARE ALLOWANCE-RECIPIENTS ABLE TO OBTAIN BETTER HOUSING?
- ARE ACCOMPANYING PRICE INCREASES TOLERABLE?

RESEARCH OBJECTIVES

- MEASURE CHANGES IN MARKETWIDE SUPPLY OF HOUSING SERVICES
- MEASURE CHANGES IN PRICE OF HOUSING SERVICES
- DISTINGUISH PROGRAM EFFECTS FROM BACKGROUND EFFECTS
- EXPLAIN VARIATIONS BY MARKET SECTOR

MEASURING SUPPLY RESPONSE

1. COMPONENTS OF CHANGE IN TOTAL HOUSING EXPENDITURES:
   - REAL ESTATE TAXES
   - REAL FACTOR INPUTS
   - FACTOR PRICES
     - Interest rates
     - Capital improvements
     - Maintenance and repairs
     - Building services
   - PRODUCER's MARKUP ON FACTOR COSTS
2. PRICE ELASTICITY OF SUPPLY OF HOUSING SERVICES
3. VARIATIONS BY MARKET SECTOR

PROBLEMS OF MEASUREMENT AND INTERPRETATION

- BASIC IDENTITY:

$$\frac{\Delta R}{R} = \frac{\Delta P}{P} + \frac{\Delta Q}{Q} + \frac{\Delta P \Delta Q}{PQ}$$

- MEASURING REAL FACTOR INPUTS:

  INDEXING FACTOR PRICES

- ESTIMATING INPUT-OUTPUT RELATIONSHIPS:

$$Q_h = f(Q_f)$$

- GROSS VS. NET OUTPUT:

  TREATMENT OF VACANCIES

- OWNER-OCCUPIED HOMES:

  MEASURING PRODUCER's MARKUP

ESTIMATING BEHAVIORAL PARAMETERS

- PRICE ELASTICITY OF SUPPLY OF HOUSING SERVICES
  - FOR INDIVIDUAL SUPPLIERS
  - AS FUNCTION OF SPECIFIED VARIABLES (B, L, N)
  - AVERAGES, BY MARKET SECTOR

- EMPIRICAL APPROXIMATION

$$E(S,P) = \left[\frac{Q_t - Q_0}{Q_0}\right] \left[\frac{\pi_{t-1} - \pi_0}{\pi_0}\right]^{-1}$$

EXPLAINING VARIATIONS IN SUPPLY RESPONSE

- FOR INDIVIDUAL PRODUCERS
  - PERCEPTION OF DEMAND CHANGES
  - ENTREPRENEURIAL CHARACTERISTICS
  - TENANT CHARACTERISTICS
  - BUILDING CHARACTERISTICS
  - NEIGHBORHOOD CHARACTERISTICS

- BY MARKET SECTOR
  - SECTOR-SPECIFIC DEMAND CHANGES
  - SECTOR-SPECIFIC PRODUCTION FUNCTIONS
  - SECTOR-SPECIFIC RISK FACTORS

Kain: Do you have any estimates of income, eligibility, and the cost of housing over the entire decade?

Lowry: We have no information on inflation effects, so it is difficult, but we expect R^* (in real dollars) to remain constant. We also expect the producers' markup to go up and real incomes to remain constant over the decade.

Muth: Historical data do not support these expectations. Real income has grown since World War II, and income has risen faster than housing costs.

Lowry: We must distinguish between decades. Growth in the 1950s was much higher than in the 1960s. We don't know whether this trend will continue, so we are using the assumption that rent changes will equal income changes for the decade.

Kain: If incomes do rise faster than housing costs, the number of eligible households will decrease. Has Rand considered what factors will affect real R^* in the next decade? This is important for generalizing to a different market and in assessing the macroeconomic forces which will affect prices.

Lowry: Mortgage interest and labor costs in the construction trades are the major component costs of housing expenses. We do not predict high rates of inflation for these prices in the next decade.

Kain: If this is the case, then incomes will rise faster than housing costs.

Aaron: Rand is treating interest-rate changes as bringing capital gains or losses to people with mortgages. This does not seem to be realistic.

Lowry: Rand proposes to use the current market rate of interest to impute income from capital. But we also will collect information on contract rates of interest on mortgages from our surveys, which could be used in the analyses. We propose to measure the change in rent which, in some sense, is a capital gain.

Kain: Capital gains should be measured with respect to alternate investment possibilities rather than by using the market-specific rate of interest.

Lowry: We appraise market value at baseline and then measure additions to, or deletions from, that value in real terms. We also measure real changes in factor inputs.

Hayes: How will rental properties be appraised?

Lowry: Our first notion was to use a fee appraisal, but we have been persuaded that it is cheaper to use multiple-regression techniques for a mass appraisal. For multiunit dwellings, we will base our appraisal on rent rolls. For smaller structures, we will look at physical characteristics. We do not reappraise the value each year.

Mills: How are you going to depreciate this investment?

Lowry: We are going to use the model presented in Appendix A.

Aaron: There is a persistent inconsistency between the exposition in the appendix and that in the chapter on supply response: In the chapter you imply that the percentage change in quality equals the percentage change in inputs. But if good-as-new maintenance (which implies constant quality) requires increasing expenditures for factor inputs, then this cannot be true. Concerning the handling of vacancies, in Rand's model a decline in the vacancy rate is treated as an increase in housing prices to the extent that it is not associated with an increase in factor inputs. But vacancies can also be viewed from the output side. A decrease in vacancies implies that people are getting more direct housing services, but flexibility is reduced. I expect that the former effect will dominate. Will housing costs in the HASE include a payment for some equilibrium vacancy rate? The role of vacancies is more complicated than its treatment in the model suggests.

Lowry: The level of vacancies is powerfully reflected in producers' revenues, and we capture it by measuring those revenues.

Aaron: Removal of multiunit buildings from the housing stock is generally preceded by rising vacancy rates.

Lowry: Isn't that reflected in revenues?

Muth: There are similarities between vacancies in the housing market and unemployment in the labor market. As demand for labor increases, unemployment decreases.

Aaron: Are decreases in vacancies treated as an increase in the amount of housing services produced?

Lowry: Not unless there is an increase in the amount of inputs.

Kain: Suppose an exogenous force causes a 10-percent increase in all rents in Green Bay. How does this fit into the analysis?

Lowry: That implies there is a 10-percent increase in revenues. If there is no change in inputs, the supply elasticity is zero. If there is an increase in inputs, then there has been an increase in the output of housing services and we assume that the supply curve has a positive but finite elasticity.

Kain: But the profitability of investing in standard housing relies on the relative prices of standard and nonstandard housing, and if there is a 10-percent increase in all rents, then it is not clear that producers will increase the output of standard housing.

Mills: Are you saying that such an increase in rents will not call forth more housing?

Kain: The market is segmented and not enough housing is produced at the bottom. Such a uniform increase implies that the relative returns to high-cost and low-cost housing do not change.

Mills: That is not the way it works.

Kain: I am considering a segmented market again, with each segment affected uniformly.

Lowry: Then in each segment, entrepreneurs will enter the market and increase the supply.

Mills: I think Kain is talking about pure inflation, i.e., the case in which there is no change in the equilibrium quantity produced in response to an increase in demand.

Kain: What if the uniform increase all shows up as land rent?

Lowry: Then the numerator does not change. If factor prices increase, we capture this in the elasticity calculations.

Muth: You don't need a measure of output, except to decompose revenue into price and quantity changes. There are, however, other methods for estimating price changes, for example, hedonic indices. By this method, rent is regressed on a vector of housing characteristics. Such a procedure has been used by various people in this room. Indeed, Rand is proposing to use a similar procedure to appraise properties at baseline, and it involves the very same problems that they attribute to hedonic indices. I feel that it would be no more difficult to convince nonprofessionals of the validity of results obtained by using hedonic indices. Moreover, Rand is worried about the problem of nonresponse, and a hedonic index would be less sensitive to this problem.

Mills: The analysis could be done both ways.

Lowry: We have thought about the alternatives and are familiar with the problems and results of hedonic indices. Our accounting system is designed to deemphasize any errors introduced by using the hedonic approach for baseline appraisal.

Muth: We can do the same thing in applying hedonic indices to estimating quality. You could estimate the importance of various physical characteristics at baseline and then in following years apply the baseline regression coefficients to changes in housing characteristics.

Lowry: It is a complex task to construct a hedonic index for quality which will be sensitive to changes over time. If we take this approach, it would require fairly detailed interior inspection of housing units, and this would probably result in a nonresponse problem with households.

Aaron: A hedonic index will probably not scratch the policy itch. We are primarily interested in the changes in the landlord

profit caused by the allowance. Rand's input analysis is the best way of solving this problem. However, the HASE will not provide any measure of the change in the quality of housing, a question in which it would seem HUD would be very interested.

Kain: What if housing turns out to be a good capital investment even without the allowance program?

Lowry: Then the housing allowance will not have much effect.

Kain: But suppose all other landlords earn 50 percent and HASE landlords earn 51 percent. Will price indexing solve this problem? The elasticity model is partial-equilibrium in nature and does not accommodate general equilibrium capital-gain/loss considerations.

Crane: The analysis should be done in both ways. A hedonic index can solve some of the problems and could be used instead of a control site.

Austin: Should neighborhood and environmental characteristics be used in a hedonic index? There exist data which indicate that environmental characteristics do influence people's decisions.

Lowry: We do not think that hedonic indexes are a suitable primary instrument for addressing supply questions, but they are useful and we are investigating their applicability to our analyses. We hope to construct them on the basis of the data which will be gathered from our surveys, but we will not choose our sample for the purpose of obtaining data for hedonic indices. Rather, our sample is focused in sectors of the market where the housing is relatively homogeneous and most likely to be impacted by an allowance.

Hayes: Input data will indicate where landlords do spend their money even if the quality improvement is not visible and thus would not show up in a hedonic index measure. If nothing else can be obtained, we would want income/expense data from the landlord.

Bawden: You are not disaggregating enough with respect to expenditures on factor inputs.

Lowry: We will disaggregate by class of factor input. The total change of factor inputs is the *highest*, not the only, level of aggregation of factor inputs we will use.

Bawden: I am concerned about the role of vacancies, and how this role may confound the analysis. The vacancy rate may not be at the equilibrium rate. Thus any change in demand may first affect vacancies without affecting prices.

Lowry: With regard to the treatment of vacancies, we will have data on factor inputs, contract rent, and vacancies from the landlord survey; so we will have these data to analyze as necessary.

Aaron: I am concerned about the use of minimum standards, which implies a cutoff level for certifiable housing. This makes the last unit of quality required to obtain certification very important in the way it affects the suppliers' response to the stimulus.

Lowry: Yes.

Field: I would like to hear any thoughts about the impact of interior unit inspection on nonresponse.

Crane: If you have well-trained interviewers, once they get in the door they can obtain almost any kind of information.

Bawden: Perhaps if money were offered to interviewees, we would have little problem with nonresponse. If the level of compensation were too low, however, people still might not respond.

Kershaw: The level of compensation necessary to persuade people to respond is too high to make this solution feasible. If you get in the door, you can get almost any kind of data. The important variable in determining nonresponse is the length of the interview, not the substance of the questions.

Hayes: Income data is the only thing you're likely to get refusals on.

Crane: If income data are difficult or impossible to obtain, we would want to get data on the interior of the unit by inspection.

Lowry: It's not the length of the interview that is important, but the willingness of the lady of the house to let you look at her bathroom. This is very different from a living-room interview. You can pretest such an interview once, but cooperation on repeat interviews is uncertain.

Rivlin: What about landlords? The landlord questionnaire is horrendous. Are you going to pay them?

Lowry: We thought of offering them a lottery ticket on a free trip to Europe.

Rivlin: The pretest on the landlord instrument is critical in obtaining information on the landlord nonresponse problem.

Kershaw: It is in the field, being pretested.

Bawden: If you calculated the value of the data lost per landlord nonresponse, you might find that you could afford to pay $100 to each landlord and obtain something like a 90-percent response rate.

Lowry: This is possible, but it is something we will know more about after the pretest.

Hayes: Does the landlord questionnaire have to be as detailed as it is?

Lowry: We expect the pretest will reveal questions for which we do not get much detail and which can be dropped. Moreover, the format of the questionnaire is deceptive. Many of the items will not be asked of most of the respondents. The pretest will tell us which parts are too elaborate and how long it takes to administer.

Rivilin summarized the major points of the preceding discussion:

1. Input versus hedonic-index approach
2. Disaggregation
3. Problem with vacancies

4. Question of control sites
5. Response rates

3:45 p.m.: CONTINUATION OF DISCUSSION

Crane: Should we ask people to describe the quality and condition of their unit, or should we actually look around? The quality of data you get by asking should be adequate.

Heinberg: Similar information for the Demand Experiment will be obtained through ratings by trained housing evaluators, so obtaining the data through a household survey would involve problems of comparability for an integrated analysis.

Kershaw: As we said before, though, once you get in the door, you can ask almost anything.

Mills: That may be so for tenants, but landlords are a different kettle of fish.

Austin: The refusal rate depends in part upon general attitudes toward research and governmental programs. There may be neighborhoods where you get 100 percent refusals.

Crane: There is a high correlation between a woman's telling you that her toilet works and the fact that it does.

Mood: What is the effect of change in construction technology on the supply curve? For example, prefabrication.

Lowry: We don't know the answer to this, but the experiment should reveal it. We expect the main supply response to be an improvement of existing structures, though we may be wrong. If technology gets the jump on us, we would expect to observe old houses being junked and new ones being built.

Kain: Can people take their allowance and buy a mobile home?

Lowry: Yes, they can buy a mobile home, but it must meet certification requirements. The mobile home has been a problem in designing survey instruments.

Kain: What fraction of mobile homes meet the standards for certification?

Lowry: We don't know. Heating and ventilation may be problems, especially in cold regions.

Austin: It is more likely that zoning for mobile-home parks will be a problem.

Alesch: Insulating the base of mobile homes solves some of the heating problems in the Green Bay climate. Rural areas in Brown County appear to be permissive about allowing mobile homes. Prefabricated houses and mobile homes are becoming more numerous.

Austin: But mobile homes are not really a viable option for residents of the city who do not have access to land.

Alesch: Plans exist for large trailer parks.

Connell: Do Secs. 23 and 235 cover mobile homes?

Lowry: So far as we know, the regulations do not forbid using such funds to purchase mobile homes.

Field: There is nothing in the legislation to preclude it, as far as I know.

Mood: Mobile homes represent a different technology and may require a different method for the separation of price and quantity.

Lowry: Mixing conventional and more efficient production methods poses a problem. But mobile homes and conventional housing are distinguishable in the field and our data are disaggregated enough to enable us to analyze them separately.

Mills: Factor-price indexes rise faster than housing costs, and mobile-home prices are high. As for vacancies, it takes a certain amount of resources to supply services in vacant units, although not as much as for occupied units. If a vacant unit becomes occupied, this does not represent inflation or a change in factor prices.

Lowry: A change from vacant to occupied status represents an increase in the landlord's take and is a motivating force in landlord supply. If a landlord finds he has a higher profit due to an increasing occupancy rate, the question is how this affects his subsequent output.

Aaron: The vacancy-rate problem may imply that the market is in disequilibrium both at the beginning and at the end of the

experiment. If this is so, what kind of comparisons can you make?

Lowry: We expect some analytical "noise" of this nature but have selected sites whose recent housing market history, at least, shows a fairly stable trend, reflecting a moving equilibrium in demand forces.

Mills: You may find an increase in abandonment if program participants decide that housing at the bottom of the market is not good enough. This seems to have been a result of public-housing programs in large cities in recent years.

Lowry: This phenomenon is due to a combination of downward filtration of the housing stock and decreasing central-city populations.

Hayes: The data to be gathered will meet the needs of various analyses. Indeed, once you see the data, you may want to change to another form of analysis.

Bawden: There is some confusion with respect to the proposed analysis of supply response. The Design Report says little about the value of the housing allowance. It does address the question of how much shock will occur and how long it will take to reach equilibrium. Twenty percent of all households receiving an allowance of $400 per household will not cause an extreme shock. So the question is, How long will it take to get over this mild shock? But the Report is not couched in these terms.

Mills: I disagree. If after five to ten years there is no measurable change in supply, HUD and Congress will be disappointed and unlikely to lend support to a national program. We want to know if we get more housing or just higher prices.

Aaron: Mills and Bawden are making different points. If the experiment is to provide political answers, it must present results interesting to Congressmen. Bawden is right. A housing allowance must include constraints to distinguish it from income-maintenance programs. The experiment should test the types of constraints which are politically realistic.

Lowry: [To Bawden] Your interest appears to be not in a measure of supply response but, rather, you want to test earmarking decisions, constraints, and their impact on demand.

Bawden: The experiment is designed to test the shock and the time required to get over it. There is, however, interest in what happens in the first year or two during the initial shock state.

Lowry: There are two classes of problems: (1) the shock problem--this is a short-run question--and (2) the questions of the long-term benefits. We want to know the time pattern for price and quantity changes. If the experiment runs for five years, with a prompt start, we will get a good idea of the long-run equilibrium adjustments. If, however, the implementation of the program is blocked by short-term problems, we will be interested but would still want to know something about possible long-term benefits.

Bawden: Is the degree of initial shock truly a policy question?

Lowry: Is there any way to get to an actual national program by taking small increments? Over the last 35 years we have seen incremental housing programs implemented with little result. The history of housing is different from that of Medicare due to the speed of initiation, so it is of interest to find out the effects of a sudden shift in housing demand because that is the way a housing program would be implemented nationally.

Field: A national program would be a sizable jump in HUD's present budget. It is quite plausible that a national program would be implemented by steps.

Austin: It is my guess that most states have not improved Medicaid programs much since their inception, although the original intention had been to increase the range of services. There is little political precedent for starting small and building a program up by steps where redistribution is part of the objective.

4:28 p.m.: MARKET INTERMEDIARIES

Grigsby presented a briefing on Market Intermediaries (summarized in the following charts). Discussion followed:

Mills: Is any special enforcement of open-housing laws planned for the experimental sites?

Glatt: In the Demand and Administrative Experiments there will be considerably more such enforcement than there is elsewhere in the nation. Participants in these programs will be provided with information on these laws--and legal resources, should such be needed.

Mills: I agree that such enforcement is a good idea. Also it might be good to keep in mind its effects on improving recipients' opportunities to get better housing.

Wilson: In regard to differences from the national norm in the sites, I am concerned that much more emphasis is being placed on the users of the market than on comparing the structures of the markets for intermediaries.

Alesch: Green Bay is literally wired to Milwaukee. Many Green Bay banks use computer terminals wired to Milwaukee bank computers. There is no problem with the flow of capital into the area; it appears to have sufficient capital to meet any of our needs.

Crane: There is no discussion in the Report about the local government. It seems that you would have to get a local ordinance passed in order that tenants could complain without fear of eviction. This would probably employ some sort of rent escrow mechanism.

Lowry: This problem would be typical of a national housing allowance program.

Crane: Is the experiment designed to test a national allowance program or to compute some ideal theoretical index of supply response? What kind of money will Green Bay and Brown County get from the experimental program? If the program is just a headache to administer, we may not get much local cooperation.

MARKET INTERMEDIARIES AND INDIRECT SUPPLIERS

1. MORTGAGE LENDERS

2. INSURANCE UNDERWRITERS

3. MANAGEMENT FIRMS

4. CONTRACT BUILDING SERVICES

5. REPAIR AND IMPROVEMENT CONTRACTORS

POLICY ISSUES

- DO INTERMEDIARIES WITH CONTINGENT LIABILITIES RESPOND FAVORABLY TO ALLOWANCE PROGRAM?

- IS REAL ESTATE SPECULATION A HELP OR A HINDRANCE?

- WHAT EFFECT DO ALLOWANCES HAVE ON DISCRIMINATORY PRACTICES?

- DO FACTOR PRICES RISE IN AN ALLOWANCE-STIMULATED MARKET?

RESEARCH OBJECTIVES

- OBSERVE CHANGES OVER TIME IN ....
  - USE OF INTERMEDIARIES AND CONTRACT SUPPLIERS
  - INDUSTRIAL ORGANIZATION
  - INDUSTRIAL POLICIES AND TERMS OF TRADE
- DISTINGUISH SITE-SPECIFIC FROM GENERAL FACTOR-PRICE CHANGES
- FOLLOW UP EVIDENCE OF SALIENT PROBLEMS
  - SPECULATION AND FRAUD
  - DISCRIMINATION

DATA SOURCES

- ANNUAL SURVEYS OF ....
  - MORTGAGE LENDERS
  - INSURANCE UNDERWRITERS
  - REAL ESTATE BROKERS
- SURVEYS OF LANDLORDS, TENANTS, HOMEOWNERS
- RESIDENT OBSERVER

Lowry: First, we are interested in any institutional changes which take place and will watch these through the resident observer. Second, in Saginaw, if no strong fair-housing agencies currently exist, they are very likely to appear along with the housing allowance program. And third, in regard to rent withholding and the legal basis for such action, we are interested in seeing if there will be pressure on municipal authorities to make such ordinances.

Crane: You should have an outreach program which will provide as much information as people will get in a national allowance program. Otherwise, Green Bay and Saginaw might sit there for five years and do nothing.

Alesch: The problem now is to keep a low profile in Green Bay. Entrepreneurs may already be withholding investments. There are indications that some potential eligibles were already beginning to make tradeoffs between Sec. 23 and 235 allowances within the context of the allowance program when it was first announced. Already landlords are inquiring about standards for the program.

Aaron: What does this do to your baseline data?

Lowry: It probably biases the data.

Alesch: We are trying to keep people in Green Bay from acting or not acting in anticipation of the program.

Mills: What will you learn from the study of speculation?

Lowry: For example, we may discover that people are buying single-family houses, improving them, and then selling them.

Grigsby: The soft indicators which we will be monitoring may appear before some of the harder data. These soft indicators might provide some early indication of a turnaround of the market.

Field: People generally pay more for houses in areas with better public services. Should we measure whether public services improve in response to the allowance?

Aaron: This would change market rents.

Lowry: We plan to study neighborhoods and their characteristics and can use our data to detect any changes caused by the allowance and any impact this may have on supply.

* * * * * *

5:00 p.m.: ADJOURNMENT FOR THE DAY

TUESDAY, 26 JUNE 1973

9:18 a.m.: OPENING REMARKS

Rivlin opened the session by summarizing the major issues that emerged in the previous day's sessions:

1. The speed at which the experiment should be started up
2. Control sites
3. Varying the certification
4. Hedonic indexing

9:23 a.m.: RESIDENTIAL MOBILITY

Ott presented a briefing on Residential Mobility (summarized in the following charts). Discussion followed:

Aaron: I have a question about the proposed income-stratification scheme: Why not choose a sample from the ineligible groups just below and just above the income threshold for eligibility?

Ott: The mobility sample fell out of the sample design for the other panel surveys. It was not drawn up specifically for the mobility analysis.

Aaron: The summary table does not indicate whether or not the sample allows for changing the income level of eligibility.

Lowry: Our sample of households falls out of the sample of residential properties. We had to estimate the composition of that household sample from Census data, and it appears to give us a usable sample.

Austin: There is the question of how to treat income transfers for analytical purposes. If you do not impute an equivalent value before taxes for such transfers, gross incomes for families receiving transfers would be understated. Account should be taken of this fact in using the income variable in analyses.

POLICY ISSUES

- WILL ALLOWANCE PROGRAM RESULT IN LOCAL POPULATION REDISTRIBUTION ?

- DOES PORTABILITY OF ALLOWANCES CONTRIBUTE TO HOUSING IMPROVEMENT ?

RESEARCH QUESTIONS

EFFECTS OF THE ALLOWANCE PROGRAM ON:

- AMOUNT OF MOVING
- PATTERNS OF MOVEMENT
- CAUSES OF MOVING
- RESULTS OF MOVING
- HOUSING SEARCH PROCEDURES

DATA SOURCES

- HAO ADMINISTRATIVE RECORDS
  - LIMITED TO PROGRAM PARTICIPANTS
  - LIMITED TO POST-ALLOWANCE TIME
  - LIMITED DATA ON HOUSEHOLDS AND HOUSING
- SAMPLE SURVEYS, TENANTS OF MONITORED-HOUSING UNITS
  - BOTH PARTICIPANTS AND NONPARTICIPANTS
  - PRE-ALLOWANCE MOBILITY AND HOUSING HISTORY
  - DATA ON ATTITUDES AND PERCEPTIONS
- OTHER PANEL SURVEYS
  - DATA ON BUILDING, LANDLORD, AND NEIGHBORHOOD
  - LINKED TO INDIVIDUAL TENANTS
- FOLLOW-UP ON MOVERS

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 ... (BOTH ELIGIBLE AND INELIGIBLE)	824	22.1
$7,000 to $9,999	941	25.3
$10,000 OR MORE	1,014	27.3
TOTAL	3,720	100.0

STRUCTURE OF COMPARATIVE ANALYSIS

ROLE IN HOUSING MARKET	PRE-ALLOWANCE TIME	POST-ALLOWANCE TIME
PROGRAM PARTICIPANT	A	B
COMPETING NONPARTICIPANT	C	D
NONCOMPETING NONPARTICIPANT	E	F

ANALYTICAL METHODS

- REGRESSION ANALYSIS OF FACTORS AFFECTING MOBILITY
  - WORKS BEST FOR CONTINUOUS VARIABLES
  - USE PARAMETERS IN COMPARATIVE ANALYSIS
- ANALYSIS OF TRANSITION MATRICES
  - WORKS BEST FOR DISCONTINUOUS VARIABLES
  - USE PARAMETERS IN COMPARATIVE ANALYSIS

Wilson: What is Rand doing about income transfers? How does this affect eligibility?

Lowry: Rand has lobbied vigorously for a distinction between gross and disposable income. We propose to use disposable income. For analytical purposes, we intend to count all cash transfer payments as income. Transfers in kind have a different impact on household budgets. This is a problem which has not yet been resolved. We do not want to treat Medicare as a cash transfer.

Rivlin: You don't want to leave it out of the analysis either. Its effect is a hypothesis to be tested.

Aaron: It is a common practice to impute some sort of actuarial value to these services.

Lowry: Gross earnings and transfer payments impact family spending decisions differently. We will have to model this to choose the best form of accounting.

Aaron: How big is the income tax on eligibles?

Lowry: About 14 percent and up on earned income.

Muth: If you give someone a transfer in kind, it is not worth the market price, but it is worth something. You need some sort of concept of consumer surplus for estimating the value of transfer payments.

Lowry: There are two concepts of income here--one for analytical purposes and one for determining eligibility.

Wilson: Field says you can't use food stamps to determine disposable income for eligibility purposes, but I assume you will collect information on such transfers to be used in the analyses.

Lowry: Yes. Income is an explanatory variable in our analyses.

Wilson: How do you treat the imputed income for, say, farmers who grow their own food?

Lowry: Farmers are in a different situation from other people; we can stratify them and just count cash income in analyzing their response.

Heinberg: Most of these sorts of issues will be addressed in the Demand Experiment. Sections 23 and 235 restrict the income

definition possible for the use of funds for the Supply Experiment. The Demand Experiment will provide the flexibility for assessment of different definitions of income.

Hayes: Are you going to use data about your neighborhoods in the mobility analyses?

Lowry: Yes. [A brief explanation of the map delineating neighborhoods in Green Bay followed.]

Austin: It is important to decide on a definition of disposable income. The definition used makes a difference in analyzing the changing incomes of neighborhoods; for example, the effective income of a neighborhood may not decrease even though there is an influx of poor people, if these people are receiving substantial welfare payments.

Greenwald: The Demand Experiment can test different formulas. Section 23 legislation does not preclude Rand's definition of disposable income.

Mood: There will be few households in the mobility sample which fall in the upper income bracket.

Lowry: About one-fourth of the sample is estimated to fall in the greater-than-$10,000 income group, as compared to about half the total population falling in that bracket.

Mood: There is a probability that mobility will be restricted because the shock will result in the filling of vacancies so there would be no place to move.

Lowry: Households in the $10,000-and-more group come primarily from the middle and upper rent/value strata. The sampling rate for the middle stratum is about twice that for the upper stratum; so we think we will have enough households in the income range likely to be affected by housing competition from allowance recipients.

Mills: I question whether we will find much mobility at all.

Kain: The argument for control is particularly strong in the mobility analysis. The control may take the form of a control city or a variety of secondary data permitting mobility analysis in other similar housing markets. It

is important to compare the behavior of recipients and nonrecipients, but it is also important to compare the residents in the housing allowance site with those living elsewhere.

Blum-Doering: We are protected in two ways: (1) In the panel of households, we will collect retrospective information on moves. This will allow us to analyze household mobility in the site before the allowance program was introduced. Since mobility in the U.S. has been reasonably flat in recent years, we can infer that such behavior would have remained reasonably stable in the site in the absence of the allowance program. (2) We will keep close tabs on mobility at the national level and on other studies to see if mobility rates change elsewhere. The cost of monitoring a second site in order to obtain control data on mobility is not worth it. However, should a control site be employed, we would want mobility data as well as other data.

Lowry: Mobility studies have mainly collected data on long-distance movement. There is little available on local moves. While rates of movement and some gross patterns are known, there are no detailed data to relate these to household characteristics.

Kain: Abandonment and vacancies in cities probably indicate that there have been changes in mobility recently. Moreover, it is important to disentangle experimental effects from general background. I would give up an experimental site in favor of a control to accomplish this. But the analysis should at least employ secondary data for comparison.

Lowry: The two sites chosen for the HASE offer interesting contrasts with respect to central-city growth and movement into or out of the central city. In Green Bay the hinterland is emptying and the central city is growing. In Saginaw, just the opposite is occurring.

Heinberg: In the integrated analyses of supply and demand, mobility comparisons will be made.

Kain: Yes, but this can only be done for recipients.

Field: Is the tracking of movers important, or is the panel sample sufficient?

Hayes: For participants, you will have HAO [Housing Allowance Office] data on mobility, so tracking is important only for nonparticipants.

Kain: Tracking would be very expensive.

Lowry: Field is trying to focus attention on the issue of getting mobility data from the panel of households rather than following the households when they move out of the sample. When a household moves into a monitored property, we will get retrospective information about its earlier moves.

Austin: The neighborhood concept you have defined is not useful for analytic purposes. The unit for analysis should be the block on which the person lives. Average or aggregate descriptions for a neighborhood are misleading; the area is too large. These do not coincide with people's perceptions of their actual neighborhoods. Starting with block characteristics and aggregating upward is more flexible. The question of what are useful descriptive variables for neighborhoods is not addressed in the Design Report. You should try for a few characteristics and get good data on these rather than attempting to collect many variables.

Unger: Your sample of households is not a probability sample but, rather, a convenient sample. How do you treat this problem in making inferences?

Lowry: If we start with a random sample of properties, this will yield a probability sample of households. Our household sample will be biased only if the households' movements are affected by the monitoring.

Mills: We really know very little about mobility, and this is a question of great academic interest. But what import does mobility have for a housing allowance program for the nation? Is this aspect of the analysis really that important or should it be dropped?

Lowry: What if 100 percent of the residents of the black ghetto in Saginaw move to other parts of the city?

Mills: You don't really need a sophisticated analysis to discover that effect.

Field: There are two policy issues: (1) the redistribution of the population, and (2) the portability of the allowance. What are the policy implications of possible outcomes?

Lowry: Rand has thought about that problem. With respect to population redistribution, we will address the question of whether the housing allowance affects patterns of racial segregation, but we don't really know how this will affect the allowance program or the policy implications because the policy relating to residential integration is ambiguous.

Kain: We can say something. If substantial integration results and no one minds, this is clearly a program benefit. However, if the Governor loses the next election, there would be significant political repercussions.

Field: These are obvious things. Returning to the policy implications, the question is, Is there a learning process that we should be going through during the experiment? If families start to get in trouble, what can we learn about those problems which would help us restructure a national allowance program?

Rivlin: If we employ Kain's suggestion, we would be trying methods for correcting such problems as the program goes along.

Field: One can sit back and do nothing but observe, or one can adapt and change the program as problems are found. This is a subtle difference between a demonstration and an experiment.

Kain: In suggesting a demonstration program, I did not have in mind one which would require that Rand prove that a national housing allowance program would work. The objective would be rather to implement a full-scale program in such a way as to minimize disruptions as the program is introduced. I did not mean that Rand should not carry out the proposed

analyses to determine if housing allowances are more or less cost-effective than alternative housing programs. Rand's principal concern appears to be to estimate how the housing market responds to a demand shock. Instead, I believe the experiment should be designed to minimize the shock and to minimize undesirable side effects. The demonstration mode was not meant as an assault on Rand's scientific purity.

Lowry: Worries about pressure to produce certain kinds of results is a reasonable area of concern to Rand. Such pressures inevitably appear from all kinds of sources. The implications of the demonstration mode would be a change in what kinds of information to collect.

Kain: It would not change the data collection.

Lowry: But it would change the events on which data are collected.

Kain: But do we have any real interest in learning what the adjustment process should be if the program were introduced in a way which many people would consider incorrect?

Hayes: Questions of the effects of housing allowances on neighborhoods and mobility have a lot to do with long-term housing policy.

Kain: The market's adaptation may depend crucially on the strategy employed to start up the program--programs with fast and slow start-ups would give different results. The experimental start-up should attempt to represent the way in which a national program would probably be implemented.

Wilson: A national program would probably not be phased in slowly. Congress makes a decision and wants it implemented immediately. The phase-in approach could be accomplished by adjusting benefits or by introducing it in different geographical areas at different times.

Rivlin: You could phase in by bringing different groups in at different times.

Kain: A housing allowance program differs from welfare reform. Each urban area has a different housing market. The least

likely outcome is that a housing allowance program that replaced all existing types of housing assistance would be passed overnight. Rather, it would be only part of a package of legislation that provided a variety of housing assistance programs.

Wilson: I don't agree. While the housing allowance will be part of a package, Congress can take a given amount of money and offer it to everyone eligible at a low benefit level.

Aaron: This is an ahistorical approach. Existing housing legislation is a large number of very heterogeneous programs, with much discretion left to local officials.

Wilson: I think that is right, but an allowance program will be nationwide.

Kershaw: The speed of start-up is critical to the results obtained. We should think about this and pass our thoughts along to Abt. The Supply Experiment, however, does not have sufficient sites to attempt varying start-up speed between them.

Mills: The job of this group is not to preguess Congress, but to give them advice on what they should do. We should conduct the experiment so as to learn the most. To do so, we may want to shock the market deliberately to see what happens.

Field: What if increasing the probability of measuring the supply change implies that we will increase the probability of obtaining negative results, i.e., creating a disaster?

Muth: This is precisely what we want to find out.

Field: This could be very detrimental to an allowance program. If Congress sees this result, they may well decide not to have a national program.

Lowry: There is the issue of judgment in regard to what types of risks can or should be taken to get what kind of information. We would prefer to infer from the results of a large shock what would be the results of a small shock. If we knew the optimal enrollment rate we would use it. We prefer to err on the high side, but not too high. This is

more a question of risk of damage to the local community than a question of recommendations to Congress.

Aaron: The purpose of the HASE is political rather than scientific. A phase-in would have fewer price effects and more quality effects than a shock approach. This is a critical thing at which decisionmakers will look. We want a scientifically honest experiment, but one which would satisfy policy needs.

10:53 a.m.: EFFECTS ON NONPARTICIPANTS

Lowry presented a briefing on Nonparticipants Analysis (summarized in the following charts). Discussion followed:

Mood: How will the sample panel be updated from year to year?

Poggio: It will not be updated. We will draw a stratified random sample of parcels at baseline that we will keep for the duration of the experiment.

Field: Is this true for both households and structures?

Lowry: The baseline panel is selected from a frame of all residential parcels. It includes a sample of unimproved properties, and we will capture the additions to the housing stock as these properties are improved or converted. Demolitions are captured in the field. In the rural area we will sample building permits so as to add new construction to the panel.

Aaron: To measure the impact of the allowance on nonparticipants, we must know what would have happened if the allowance had not been implemented. Neighborhoods are known to change quite rapidly--for example, with respect to racial composition. How will you sort out allowance-induced effects from other such effects?

Lowry: We will have a description of all neighborhoods in the site. From the HAO records we will have data on the influx of allowance recipients to neighborhoods. Other neighborhood effects that are exogeneous will hopefully be sortable

POLICY ISSUES

- DOES ALLOWANCE PROGRAM RAISE HOUSING COSTS FOR NONPARTICIPANTS?
- DOES ALLOWANCE-STIMULATED MOBILITY DESTABILIZE RESIDENTIAL NEIGHBORHOODS?
- DO NONPARTICIPANTS MEASURABLY BENEFIT FROM THE PROGRAM?
- DO NONPARTICIPANTS OBJECT TO THE PROGRAM? WHY?
- WHY DO ELIGIBLES DECLINE TO PARTICIPATE?

RESEARCH OBJECTIVES

- MEASURE CHANGES IN AVAILABILITY AND COST OF HOUSING FOR NONPARTICIPANTS
- TRACK NEIGHBORHOOD TURNOVER ASSOCIATED WITH MOVES BY PROGRAM PARTICIPANTS
- TRACK AND ANALYZE NONPARTICIPANT ATTITUDES
  - BY SOCIO-ECONOMIC STATUS
  - BY EXPOSURE TO PROGRAM EFFECTS
  - BY KNOWLEDGE OF PROGRAM

DATA SOURCES

- HAO RECORDS
  - NEIGHBORHOOD INCIDENCE OF PARTICIPANTS
  - PATTERNS OF RELOCATION BY PARTICIPANTS
- PANEL SURVEYS
  - SUPPLY RESPONSE ANALYSIS
  - MOBILITY ANALYSIS
- ATTITUDE SURVEYS
- RESIDENT OBSERVER

through regression analysis. The Resident Observer will notice systematic happenings, but this neighborhood question is a weak point in the analysis.

Bawden: What percent of the sample are eligible nonparticipants?

Lowry: There will be about 400 eligible nonparticipating households living in monitored structures. There will be about the same number of ineligible nonparticipating households who are very similar to the former, being excluded by categorical rules. Eligible nonparticipants are self-excluded, probably heavily weighted toward aged homeowners. The more homogeneous the group, the fewer we need in the sample to describe their behavior.

Bawden: What about those households that apply for the program but never follow up?

Lowry: We expect that the sample would capture some such households as well as other groups of interest, so we don't

think it will be necessary to make a special effort to locate these eligible nonparticipants. However, if we don't get enough of them, we can go out and get some more.

Wilson: It would make sense to go out and get them.

Lowry: The question is whether we have to make a special effort. The decision can be postponed until after baseline.

Field: Is it critical as to when we do the follow-up interview on those who have chosen not to participate?

Bawden: I'm not sure there is an analogy with the Income Maintenance Experiment. We went back only to those who accepted the offer, so I can give no indication as to when you should go back to interview nonparticipants.

Lowry: With open enrollment, it is difficult to determine the stage at which we can say that people have chosen not to participate.

Blum-Doering: We will also have data from HAO records about these first contacts who choose not to participate. So if at Year 1 we find our sample does not contain a sufficient number of them, we could sample some of these HAO first contacts.

Hayes: There are two types of nonparticipants: (1) the first contact who never comes back, and (2) the household that never even makes the first contact.

Connell: Is it possible that some households might not participate because their rents were lowered?

Lowry: Yes, that is a good hypothesis.

Austin: The crucial group is those who start and then drop out of the program. A major factor determining nonparticipation is the level of benefits, i.e., those households receiving a low level of benefits are more likely to drop out.

Hayes: There is a problem in measuring neighborhood changes which would take place in the absence of an allowance program. Indeed, measurement of such changes might be impossible even with a control site. You might try to use past developments in Green Bay to build a model and use it to

predict what would happen if you had a continuation of the last ten years of developments in Green Bay. Is Rand planning anything like this?

Lowry: We plan to write a history of the Green Bay housing market with as much detail on small areas as we can get. We will get neighborhood projections from the City Planning Commission.

Wilson: Eligible nonparticipants are politically important. Participation rates, reasons for nonparticipation, and the dynamics of the flux in and out of the program should be analyzed. Concerning a demonstration versus an experiment: The analysis of administration should not be left to the Administrative or Demand Experiments. In the Income Maintenance Experiment we never addressed the question of how to administer a social-welfare program, but the data which facilitated such an analysis were collected anyway. Don't leave this up to the Administrative Experiment, but consider a plan for collecting detail on the administration of the experiment for such an analysis.

Austin: Returning to the definition of neighborhoods, I would urge employing blocks as geographical units instead, so as to obtain a more precise measure and description of neighborhood effects. The fixed neighborhood definition is almost completely worthless--no two people would agree on the boundaries or descriptive characteristics. I suggest using the two facing sides of the block in which the respondent lives as an operational definition of a unit for analysis.

Lowry: We will be gathering data on neighborhoods at two levels: (1) We will use data on the fixed, large neighborhoods defined by Ellickson; and (2) for each property we will have observations and respondent perceptions of the immediate environment.

Ellickson: When attempting to measure demand changes, the use of blocks would indeed be more appropriate. However, if we are interested in supply changes and the housing market,

we need a larger area than the block. Our definition is a compromise between some sort of ideal neighborhood and having respondents define for us what they consider their neighborhood to be.

Kain: An alternative would be to work with smaller units such as blocks which can then be aggregated into a neighborhood according to whatever definition you decide upon later. This will allow you to vary neighborhood boundaries for the analysis.

Austin: When asking about the immediate environment in your surveys, you should specify what area you are talking about, e.g., facing sides of the street in the same block.

Kershaw: We want to know about the potential eligibles who move into the area after the eligibility deadline for the program. How many of these households are there and what are their problems?

Levien: What are the feelings about the ethical aspects and negative consequences of the program with respect to ineligible low-income households and other nonparticipants?

Lowry: We never promise move-ins that they will receive an allowance, but if we have enough money we can open the program to them. We would do this for experimental reasons, but there may be ethical considerations as well.

Levien: I am concerned about ineligibles whose prices go up.

Mills: What else can we do?

Levien: We could decide not to run the experiment, or we could employ the ramp buildup rather than the step-function buildup.

Kershaw: Local residents frequently want assurances that such programs won't result in a huge influx of people. The decision about letting move-ins participate may be out of Rand's hands.

Field: If there is a potential for a bad impact, should we have a more gradual phase-in or some provision for an abort if we have a crisis?

Lowry: A good test of the importance of the impact of the program on nonparticipants is the noise we get. We can imagine that there may be occurrences such that aborting the program would be necessary. The program, however, involves informed consent at two levels: (1) Program enrollees are informed on what they can expect from the program and what we expect of them; (2) the communities are told of the possibilities of unpleasant consequences of the program.

Wilson: What is the political process you must go through to initiate the program?

Alles: We must obtain memoranda of understanding from the local governments. In effect, each local community must agree to the program. HUD is committed to leave the community, should they be requested to do so. The question of whether or not the City Council could actually abort the experiment is unresolved. But they could, for example, just not sign the ACC [Annual Contributions Contract], or they could put pressure on the LHA [Local Housing Authority]. We feel, however, that once we get into a community they will let us do what we consider necessary, until we cause problems. Major problems will probably result in our being asked to leave. The experiment is committed to pull out in such an event, but Sec. 23 funds would probably continue.

Lowry: We must keep ourselves updated on what is happening in the community and be prepared to counter anecdotal evidence of problems.

Levien: I am concerned about Rand's reputation. People will react to this experiment, and Rand has an obligation to consider the negative consequences of the experiment. Social science research should have a concern for the ethical.

Kershaw: There will be people who will say it is unethical to treat people differently. The British would rather not obtain experimental results than have to apply a program which may have bad effects.

Kain: A saturation income-maintenance program would have inflationary effects on local housing markets.

Lowry: Our calculations suggest that we will increase housing expenditures in both sites by about 10 percent. The program will affect different groups differently, but it is not too different from the type of impacts which have occurred in communities in the past. The demand stimulus we will create is not so much different from those to which housing markets have adjusted in the past--for example, boom towns. Treating people unequally is relevant only at the level of Green Bay and Saginaw. Inside Green Bay, some people will be helped and some harmed; but we don't know who, and by how much. We have an option and an obligation to shut down the program if the harm is too great.

Field: We do not push the experiment down the throats of local officials. It should be noted that we do not go into an area until the program has been accepted. We must maintain a monitoring system to obtain early warning of potential harm, which can then be discussed with local officials.

Austin: There is a difference between events which just happen and those which result from deliberate public policy. If we know there are problems of equity, do we do anything about them? You might develop some sort of contingency plans--for example, you could compensate families encountering rent increases greater than 10 percent.

Rydell: We are actually correcting what is already an inequitable situation. What you are discussing is the marginal inequities to people resulting from our correction.

Levien: By what right can we take the decision to make such a correction upon ourselves?

Kain: One possible outcome of the experiment is that the owners of substandard properties may suffer substantial capital losses. You may wish to consider the possibility of buying out the equity of this group.

Lowry: This class of owners has been declared by local legislation as not fit for consideration. Housing codes indicate that their housing is substandard.

Kershaw: We must do everything possible to identify groups that may be hurt by the program.

Wilson: I would prefer to design and carry out a well-structured experiment which announced the possible inequities and adverse results in advance and obtained community consent to proceed.

Alles: One option we don't really have is abortion. Although we can discontinue the experiment, Sec. 23 funds will probably have to continue.

Alesch: Our design includes provision for a site office as well as the HAO, a Resident Observer, etc., in order to learn as early as possible of any adverse and unanticipated consequences of the experiment. We can deal with them as they arise. It is difficult to plan for unanticipated consequences.

Fitts: We should direct our attention to determining the important indicators which will tell us when to discontinue the experiment.

Wilson: Have you given any thought to (1) announcing the conditions under which an abort will take place and how it will take place, (2) when you plan to release your analyses, or (3) how to counter if the GAO [Government Accounting Office] enters the picture? Do you have any friends in Congress who know about and understand the experiment and who will help you?

Fitts: The Congressmen from Green Bay and Saginaw know about the experiment but are not involved to any great extent. The GAO has been interested in the experiment from the very beginning.

* * * * * *

BREAK FOR LUNCH

1:31 p.m.: SITE SELECTION

Lewis presented a briefing on Site Selection (summarized in the following charts). Discussion followed:

Mood: Can you compare your sites with the locations where the other experiments will be run?

Lewis: The Demand Experiment sites are Pittsburgh/Allegheny County, Pa., and Phoenix/Maricopa County, Ariz. The Administrative Experiment sites are Salem, Oreg., Springfield, Mass., Peoria, Ill., Jacksonville, Fla., San Bernardino County, Calif., Tulsa, Okla., Bismarck area, N. Dak., Durham area, N.C. They will give us no analytical market data that will be helpful.

Field: What should we look for?

Mood: I don't see how you are going to do any interpolation from just two sites which differ in so many characteristics.

Muth: I think you could build a better rationale for your site selections based upon the growth rate. This is very important in determining the supply curve.

Lewis: [Explanation of how potential sites were screened with respect to rates of growth.]

Muth: If you have just two sites, you can vary only one thing. Many factors vary within the sites, but you must choose the sites in order to vary the growth rate, since this will not vary within a site. The Demand Experiment cannot do this, since it will not provide enough of a demand stimulus to look at supply response.

Kain: You have only two sites and have let two market characteristics vary across them. It will be difficult to disentangle the effects of varying growth rates from those of differing racial patterns.

Lowry: There is a high correlation between central-city growth rate and the percent of the population which is black in the universe of cities to which the allowance program would be applied.

PRINCIPLES OF SITE SELECTION

1. NATURALLY BOUNDED HOUSING MARKET

2. LIMIT ON SIZE: UNDER 250,000

3. CONTRASTING MARKET STRUCTURES

4. REPRESENTATIVE MARKET STRUCTURES

DEMOGRAPHIC CHARACTERISTICS, 1970

CHARACTERISTIC	GREEN BAY SMSA			SAGINAW SMSA		
	CENTRAL CITY	OTHER SMSA	TOTAL	CENTRAL CITY	OTHER SMSA	TOTAL
TOTAL POPULATION (000)	87.8	70.4	158.2	91.8	127.9	219.7
PERCENT OF TOTAL:						
Black	.1	.4	.2	24.3	3.6	12.2
Chicano	.4	.4	.4	6.8	2.5	4.3
PERCENTAGE CHANGE, 1960 - 1970						
Net change	39.6	13.2	26.4	-6.5	38.3	15.1
Natural increase	17.8	19.1	18.5	13.5	19.2	16.2
Net migration	21.8	-5.9	8.1	-20.0	19.1	-1.1

EMPLOYMENT AND INCOME CHARACTERISTICS, 1970

CHARACTERISTIC	GREEN BAY SMSA			SAGINAW SMSA		
	CENTRAL CITY	OTHER SMSA	TOTAL	CENTRAL CITY	OTHER SMSA	TOTAL
MEDIAN INCOME ($)						
Families	9,975	10,737	10,300	9,995	11,427	10,878
Unrelated individuals	3,026	1,821	2,591	3,263	3,135	3,221
PERCENT BELOW POVERTY LEVEL:						
Families	5.5	7.0	6.1	10.9	5.4	7.7
Unrelated individuals	32.1	38.8	34.0	34.6	32.6	33.9
PERCENT ON WELFARE:						
Families	2.5	2.0	2.3	7.9	2.7	4.9
Unrelated individuals	3.1	2.0	2.8	4.4	5.0	4.6
UNEMPLOYMENT RATE:						
Male	3.6	2.5	3.1	5.1	3.6	4.2
Female	5.5	5.3	5.4	7.0	5.3	6.1

HOUSING CHARACTERISTICS, 1970

CHARACTERISTIC	GREEN BAY SMSA			SAGINAW SMSA		
	CENTRAL CITY	OTHER SMSA	TOTAL	CENTRAL CITY	OTHER SMSA	TOTAL
NUMBER OF UNITS (000)	27.1	17.7	44.8	29.8	35.9	65.6
PERCENT OF TOTAL:						
Rental tenure	33.9	17.3	27.3	33.1	14.2	22.8
In multiple dwellings	32.7	12.1	24.5	27.3	7.2	16.3
VACANCY RATE:						
Rental units	4.3	6.6	4.9	7.1	3.8	6.0
Ownership units	.6	.9	.7	1.2	.8	.9
RENT OR VALUE:						
Median contract rent ($)	86	87	86	87	119	94
Median value ($000)	15.8	19.0	16.9	13.7	18.9	16.3

Kain: But this is not true for SMSA growth. Perhaps you should have chosen two sites with varying rates of such growth, but both with a high percentage of black residents.

Lowry: The success of our site-selection strategy depends upon our findings. If our results are similar for such dissimilar sites, we will have a much better basis from which to extrapolate.

Mood: In the Administrative Experiment, is it possible to look at one individual building as a mini-Supply Experiment?

Lowry: In the Administrative Experiment, the tenants don't have to remain in the original building, and in fact their behavior will depend on what the landlord does.

Muth: There is good reason to suspect that the supply response will be much greater in Saginaw than in Green Bay, due to the outmigration from the central city of Saginaw.

Ozanne: Do your growth rates account for differences in central-city boundaries between the 1960 and 1970 Censuses?

Lowry: Boundary changes were taken into account in our calculations.

Bawden: You should consider the sociology of the black ghetto. To what extent is Saginaw's black population representative of black areas in major cities?

Lewis: [Description of the process by which the initial list of possible sites was narrowed down to the final candidate sites.]

Field: We're pretty well locked into our two sites unless there is a major problem with them.

Kain: Is it anything other than an accident which dictated your going into Green Bay first?

Field: Green Bay seemed easier than Saginaw, and we took the easy case first.

Kain: What is the time lag between start-up of the two experiments?

Lowry: We are planning about a three-month lag between starts.

Kain: I was getting at the point that if you determined that you could do only one site, it should be Saginaw rather than Green Bay.

Lowry: If we could do only Green Bay, I'd rather drop the entire experiment.

Mills: What about the shelter allowance in Wisconsin?

Alesch: We are investigating the implications of the shelter allowance for our experiment. It is something we will have to cope with, but we do not see it as a major problem. About 12 to 15 percent of the households potentially eligible for our housing allowance are also eligible for a welfare shelter allowance.

Dubinsky: The key things about the shelter allowance are that it is quite high and that it is not tied to standard housing in any way.

Austin: Wisconsin is not the only state that has a shelter allowance, and its allowance is not the highest in the country. Each state has a different system, so Green Bay is not really generalizable. However, this is a problem that you would face no matter what state your sites were in. Neither site is generalizable to a large metropolitan area. Your experiment can be generalized to a range of cities of the size of Green Bay, but not to cities with populations over 500,000. If you were going to do the experiment in the neighborhood of a large metropolitan area, you would need a different design.

Crane: Saginaw is peculiar among midwestern cities in that its school system is highly segregated. It is not representative even of cities in its size category.

Mills: I'm not so pessimistic about the possibility of generalizing to other cities. The generalizability depends upon what results you find.

Mood: The supply of housing for minorities depends upon the constraints to which they are subject. Your site selection should not have been governed by the size of the black community, but by its characteristics and the racial climate.

Field: We considered cities in the South as possible sites. However, it is almost impossible to run this type of experiment in a Southern city.

Alesch: The Mason-Dixon line is not really a physical thing. Many of the racial attitudes in Green Bay and Saginaw are as extreme as those found in the South.

2:25 p.m.: SURVEY SAMPLE DESIGN

Corcoran presented a briefing on Sample Design (summarized in the following charts). Discussion followed:

Muth: How sensitive is the sample allocation to the estimated supply elasticities?

Corcoran: The sample allocation was performed under a constraint on total sample size; thus a change in our assumptions about the mean elasticities (assuming all stratum means are changed proportionally) would have no effect on the sample size or allocation. We have performed sensitivity analyses where the stratum mean and the variance assumptions were changed; on the whole, within the ranges tested, these parameters had little effect. The major factors which do influence the sample size in any given stratum are massive increases or decreases in the total sample size constraint, the number of strata chosen, and the reliability targets assigned to the strata.

Poggio: For a fixed level of reliability, the square root of the sample size is inversely proportional to the mean elasticity. Hence, if a mean elasticity were twice what we have estimated, the sample size would need be only one-quarter as large.

Kain: What do you estimate will be the ex post distribution of the sample with respect to housing costs, say, if the supply response equals the increased income? Would there be any units left in the bottom stratum?

Corcoran: We will not restratify the sample in postbaseline years.

Lowry: We would expect to see many improved structures or vacant buildings in the lowest-rent-tercile stratum.

Kain: What are the dollar cutoff values for the terciles?

MONITORING PROGRAM

1. MONITORING THE ALLOWANCE PROGRAM
    - ENROLLMENT AND DISBURSEMENT RECORDS
    - HOUSING INSPECTION RECORDS
2. MONITORING THE HOUSING MARKET
    - PANEL OF RESIDENTIAL PROPERTIES
    - ANNUAL PANEL SURVEYS
        - NEIGHBORHOODS
        - RESIDENTIAL BUILDINGS
        - LANDLORDS
        - TENANTS AND HOMEOWNERS
    - OTHER SURVEYS
        - MARKET INTERMEDIARIES
        - MOVERS
    - RESIDENT OBSERVER

PURPOSES OF MARKETWIDE MONITORING PROGRAM

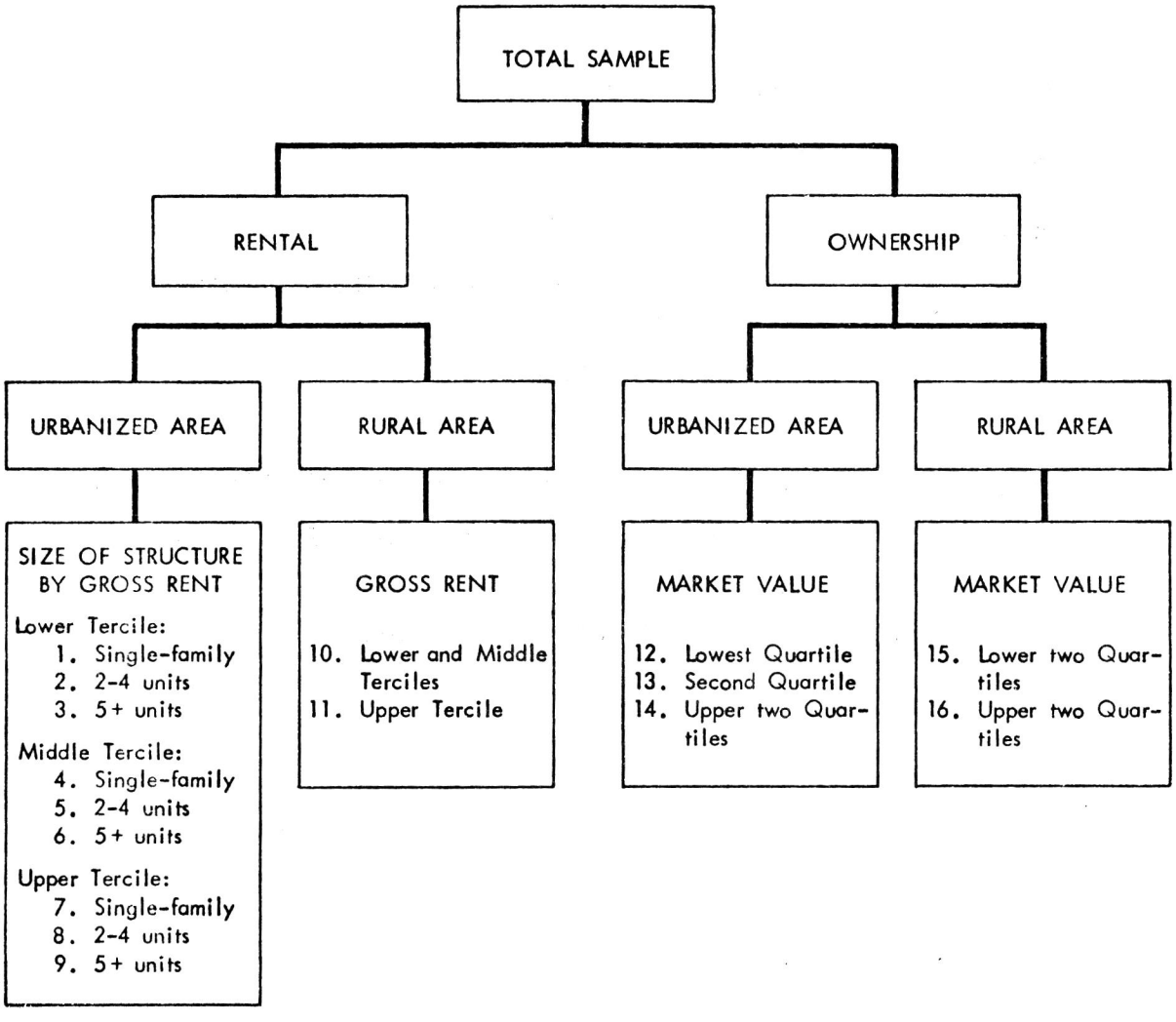
- MEASURE AND EXPLAIN EFFECTS OF ALLOWANCE PROGRAM AT EACH SITE

- PROVIDE BASIS FOR GENERALIZATION (EFFECTS OF NATIONAL PROGRAM)

SURVEY SAMPLING STRATEGY

- LONGITUDINAL PANEL SAMPLE
- STABLE SAMPLING UNIT
- MODIFIED IMPACT-GRADIENT PLAN
  - CONCENTRATES RESOURCES ON MOST-AFFECTED MARKET SECTORS
  - OVERSAMPLES SECTORS UNDERREPRESENTED IN LOCAL POPULATION
- SURVEY CHARACTERISTICS AFFECTING SAMPLE SIZE
  - BASELINE DATA NOT SUBSEQUENTLY CAPTURABLE
  - SECTORAL DETAIL IMPORTANT
  - SURVEY COSTS INDEPENDENT OF AND SMALLER THAN ALLOWANCE COSTS

STRATIFICATION OF RESIDENTIAL PROPERTIES FOR MODIFIED IMPACT-GRADIENT SAMPLING



SAMPLE ALLOCATION PRINCIPLES

- PARAMETER OF INTEREST: $E(S,P)$
  - PRICE ELASTICITY OF SUPPLY OF HOUSING SERVICES
- METHODS OF ESTIMATION
  - MULTIPLE REGRESSION
  - STRATUM MEAN VALUES
- RELATIVE RELIABILITY TARGETS (W)

- HIGH-INTEREST STRATA	1.00
- MODERATE-INTEREST STRATA	.50
- LOW-INTEREST STRATA	.25

SAMPLE ALLOCATION PROCEDURE

- ESTIMATE μ AND σ FOR EACH STRATUM i
- DEFINE RELIABILITY MEASURE FOR ESTIMATE OF μ

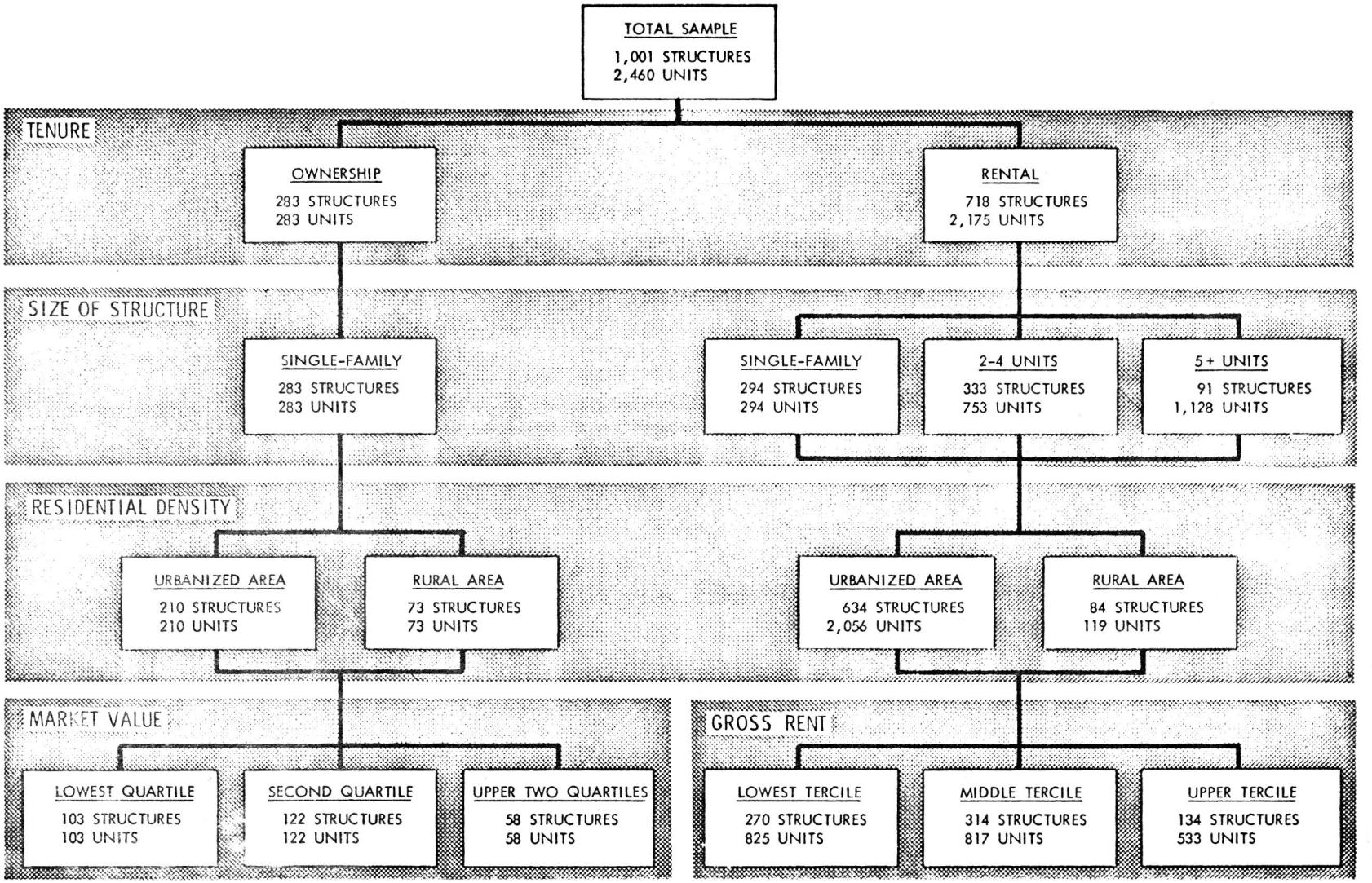
$$R = \frac{2\sigma}{\mu\sqrt{n}}$$

- CHOOSE STRATUM SAMPLE SIZES (n_i) SUCH THAT

$$\frac{1}{R_i} \sim W_i$$

$$\sum_i n_i = 1{,}000$$

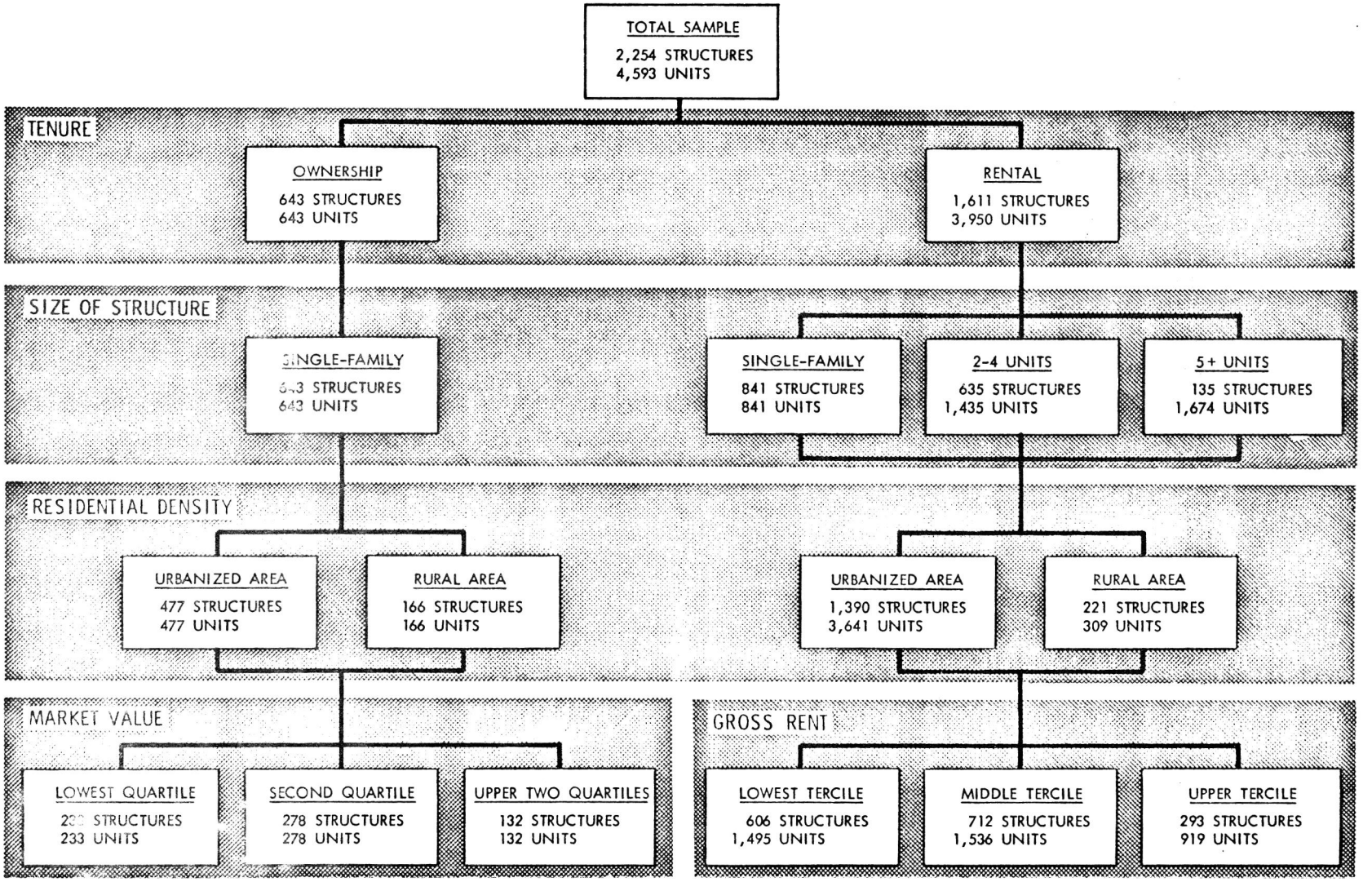
EXPECTED COMPOSITION OF TERMINAL PANEL OF RESIDENTIAL PROPERTIES AFTER ATTRITION DUE TO NONRESPONSE, BY MAJOR DIMENSIONS OF STRATIFICATION



PANEL ATTRITION DUE TO NONRESPONSE

TYPE OF PROPERTY	5-YEAR COMPLETION RATE
RENTAL PROPERTIES:	
● SINGLE-FAMILY	.34
● 2 – 4 UNITS	.55
● 5 + UNITS	.67
OWNERSHIP PROPERTIES	.44

PROPOSED COMPOSITION OF BASELINE PANEL OF RESIDENTIAL PROPERTIES, BY MAJOR DIMENSIONS OF STRATIFICATION



EXPECTED RELIABILITY OF STRATUM MEAN VALUES OF E(S,P) IN YEAR 3

TYPE OF PROPERTY	URBAN		RURAL	
	ABSOLUTE VALUE	PERCENT OF MEAN	ABSOLUTE VALUE	PERCENT OF MEAN
RENTAL PROPERTIES				
UNDER 5 UNITS				
Low or middle rent	.04	6	.06	11
High rent	.07	12	.09	23
5 + UNITS				
Low or middle rent	.09*	15-20*	—	—
High rent	.09*	24*	—	—
OWNERSHIP PROPERTIES				
Low value	.04	6	.05	12
High value	.06	12	.07	23

* Based on sampling from an infinite population

Corcoran: We won't know until the screener is completed.

Muth: What percent of the housing units in the sample are occupied by landlords?

Repnau: In Green Bay, about 20 percent of the units in multiunit buildings are owner-occupied.

Mills: Lots of slum landlords reside in their buildings.

Unger: The sample is sensitive to what you want to estimate. The allocation would be quite different if you were to study a different behavioral parameter. Your analysis does not address this problem at all.

Lowry: You are right. We will try to come up with sampling distributions for changes in the quantity of housing services. This is a tougher job than for elasticities. However, we don't expect, given our resource constraints and strata definitions, that any other sample allocations would be significantly different.

Unger: You may discover that your sample will yield no reliable estimates for total housing services. Your analysis is based on the assumption that you will calculate the elasticity for each property and average the elasticities for the sector. Your average will be unweighted by the size of the property. The need for such weighting should be considered.

Lowry: We are aiming for an estimate of the central tendency for a sector, which we will do by measuring the average elasticity. Unger wants to aggregate properties, then compute an aggregate elasticity.

Jessen: What information do you have on the parcels?

Poggio: [Description of sample-selection procedures.]

Jessen: Why don't you use blocks? Did you consider first drawing a sample of blocks, then stratifying? You have all sorts of data available from the Census on block characteristics.

Lowry: We were more interested in the characteristics of individual buildings than of blocks.

Field: [To Jessen] Are you questioning the efficiency or the appropriateness of the sample selection?

Jessen: The efficiency. I would also argue for a uniform procedure in the segregation by rental value. You should use terciles in all cases or quartiles in all cases.

Lowry: Terciles are applicable to renters, quartiles to homeowners. They are not based on the same unit of measurement--rent and home value are not comparable. We combined terciles in the rural rental strata because our sample was so small that we would not have been able to use the terciles separately for analysis purposes.

Jessen: The estimate of the price elasticity of supply may be seriously biased if the numerator and denominator are independent.

Lowry: Economic theory tells us that the numerator and denominator are highly correlated.

Jessen: But the estimator will be biased if they are random variables.

Connell: The sample allocation is based on the elasticity for single structures. Would Rand be willing to change the allocation if this scheme turned out to be completely wrong?

Lowry: Differences in the means and variances have very little effect on our allocation. Given our stratum definitions and a constraint on total sample size, the allocation will not change much.

Jessen: Your design is not optimal for any one variable. However, you seem to capture most things very well, and, given lots of data, you can do various other analyses.

Unger: Without weighting properties by their share of the market, how will the average elasticity be portable?

Heinberg: I am troubled by the fact that you are looking at the responses of individual suppliers, rather than aggregate response in the different strata. You have discussed the sampling errors involved, but what about the measurement error? One method generally used to reduce measurement error is to aggregate individual observations.

Field: I have a question to the economists on this panel: What is the appropriate population parameter that we should be looking at--the average or the aggregate elasticity?

Muth: I think the parameter Rand is estimating is the correct one. The aggregate elasticity is just the weighted sum of the individual elasticities. Moreover, there is less variability within strata than across strata.

Lowry: We are not interested in supply elasticity only as a measure of what happened in Green Bay. If we want portability of that measure, as a behavioral parameter, I would argue for the average.

Kain: If supply elasticity is different for structures of different size, then you need to consider this in the weighting scheme.

Aaron: But each stratum has a Green Bay-specific weight anyway.

Field: Are you people comfortable with the weighting scheme proposed?

Connell: What about infinite elasticity?

Aaron: What kind of elasticity are you talking about?

Lowry: Price elasticity.

Mills: If you find an infinite elasticity, this implies that there has been an increase in the quantity of housing services with no price increase, and that is good. The larger the elasticity, the better.

Unger: The aggregate response is not simply derived from the weighted sum of individual elasticities. There is the question of what is price. The weighting scheme would work only if the price is the same for each supplier. So you must also weight the prices and consider the problem of the average price being different from the individual prices.

Lowry: The question of what is price and how to measure it is very difficult. The market price of a unit of housing services is not observable but rather is a theoretical concept. If a general market price existed, the producer could be expected not only to act on that price but also to base his actions on what happens across the street, etc. We may discover in the course of this experiment how far out the producer's horizon extends. Our accounting system looks at returns to individual producers. We will pursue ways of aggregating across producers, but this procedure may give large variances.

Kain: What do you do about properties that change hands, specifically with respect to measuring returns to individual producers?

Lowry: We have not fully worked out the accounting problems. However, we will interview the new owner and get as much income/revenue data from him as possible. If this does not cover an entire year, then we will try to interview the previous owner as well.

Kain: What about the price of capital inputs?

Lowry: The value of a structure is estimated at baseline.

Kain: This reflects the return to the original owner of the property. Does the second owner base his decisions on the value of the structure to the previous owner?

Lowry: His decisions do not depend on the historic value of the property or on its purchase price. Rather, they depend on the opportunity cost of capital.

Kain: Is that the way you do the analysis?

Lowry: Yes. But even if you do not agree, we will have the data to calculate opportunity cost in another manner.

Hayes: The new purchase price of a structure is immaterial. The current account figures are what are important; that is the elasticity we want to measure.

Lowry: By sticking to the baseline evaluation and measuring changes in the supplier's capital stock, we measure the flow of factor inputs. The question of who captures changes in price--the present or the previous owner--is not central to a policy-relevant analysis.

Kain: Doesn't the nature of the capital actually change? The allowance supposedly leads to a better utilization of the existing stock of capital. If the allowance program changes landlords' expectations about the profitability of the use of a particular type of capital--for example, this may be reflected by an increase in the present value (opportunity cost) of capital--then the landlord would pursue a different strategy, for example, in the maintenance of his capital (property).

Hayes: Distinguish current cash flow from return on capital. Capital values are picked up best by appraisal.

Lowry: We will be doing appraisals only at baseline. We are trying to develop a quantity accounting scheme.... For example, suppose we have two buildings with equal total capital and cash flow currently, but the owners have completely different expectations of future returns.

Kain: That is part of the problem, but there are measurement difficulties too. One structure uses capital much more

rapidly than the other one. The experiment may result in a more efficient utilization of the capital stock. In this case the change in housing output would be larger than that captured by your accounting scheme.

Levien: Let's return to the topic of sample design.

Ozanne: Are there any problems involved in generalizing from a sample of households selected from tax parcels? I also have a question on the backup analysis. How did you determine the number of parcels which you would use for this? (You've cited a figure of 120 parcels.)

Lowry: We have some information about landlords who don't respond, from information on the tax parcel, from field observations of the building, and from whatever information we can glean from the tenants. The size of the bias panel is my guess about what is needed to say something about the mean and variance of the characteristics of nonresponding landlords.

Ozanne: Are you assuming that the bias is the same for all nonresponding landlords?

Lowry: Currently yes, but we will have quite a bit of information on the nature of the bias before the backup panel is chosen. The present size of the panel does not represent any arcane calculations.

Bawden: Returning to the suggestion of using payments to reduce attrition: I made some quick calculations which would indicate that if you paid owners $50 and tenants $25 and you got up to 70-percent response rates, you would get less bias from nonresponse, and you would still save money. Additional interviews would entail overhead costs which you would not have with the payments to respondents.

Lowry: The idea sounds well worth investigating further. We had considered paying landlords, but, in our judgment, the price required to influence their behavior was too high. We had also considered the possibility that payments may make the interviewers feel better about taking up the respondents' time.

Kershaw: If you are considering payments, you should probably try varying them in the pretest.

Mood: Payments would certainly raise response rates. An alternative method is to go to local real-estate agents, pay them, and let them fill out most of the questionnaire.

Ozanne: There is still the question of the appropriateness of using the tax parcel as the source for selecting the sample of households.

Lowry: Our sample is defined by selecting residential parcels. Within those parcels, we fix the housing units as well. We interview the occupants of these housing units and not a fixed panel of households. We have no control over the decisions of households which bring them into or take them out of our sample over the course of the experiment.

Kain: But the program is designed to change the characteristics of the sampling frame.

Lowry: The characteristics of property changes in the sample reflect changes in Green Bay. Households in Green Bay have to live somewhere. This should give us as good a cross section of households as any.

Buchanan: There is a weighting problem in converting the given sample into the desired sample. For example, if we were interested in looking at satisfaction, we would want a different sample from that which is useful in measuring supply response.

Heinberg: The household sample might be weighted one way in Year 1 and another way in Year 2. The complications these differing weights would have in the analysis of, say, changes in household responses over time is problematic. It is not at all clear how data on changes in household attitudes and behavior in such a sampling framework could be appropriately weighted in, say, a regression analysis.

Aaron: Returning to the lower tercile: The minimum standards operate to upgrade part of this tercile. Thus, that stratum will no longer be representative.

Lowry: Of what?

Aaron: Of people.

Lowry: It never was.

Poggio: Using the original stratification of the panel is legitimate if the strata are properly weighted.

Unger: Forget the weighting problem. The original sample is a probability sample of households. To be a probability sample a year later, it must be a probability sample under every contingency. For example, if no one moves out, then inmigrants do not have the same probability of being in the sample as everyone else in the sample in Year 1. This would be the case if no one moved out of your sample in the first year. [Unger later withdrew this argument.]

Mills: The issue is, Do people moving into the SMSA have a different mobility pattern than those who lived in the SMSA at baseline?

Lowry: [To Unger] How do you draw a random sample of anything? It is a question of whether this is a serious issue. The circumstances under which we do not have a probability sample are extremely unlikely to obtain.

Levien: I would infer from Unger's sample that no one in the SMSA moved.

Jessen: We should look at the sample as consisting of housing units. Your sample of people then consists of the households you find living in those units. Your household sample is random because your household units were random, and it will remain so as long as you do not treat the sample units differently from other units.

Poggio: Then maybe we had better not use the suggestion of paying tenants that we interview.

Field: If we don't track movers, we will lose only longitudinal current data. So, if we are willing to use retrospective information, there seems to be no problem.

Bawden: The method you have proposed by which your sample would capture new construction may not be representative because of the paucity of such construction. This might be a problem, especially in rural areas.

Lowry: We will be looking at building permits in rural areas. While our present plans do not call for this approach in urban areas, should the number of newly constructed units and conversions that we capture be too small, we could use the same approach in the urban area as in the rural area.

Field: Regarding compensation for landlord nonresponse: If there is a serious nonresponse problem, is the proposed compensatory analysis adequate?

Jessen: Every effort should be made to complete the landlord questionnaire, even to the extent of going to realtors.

Hayes: There is a good example from the New York Rent Stabilization Project. Good data were obtained on rents and income from banks and other institutions. You might be able to use the same types of sources for your income and expenditure data requirements if the landlord gives permission.

4:10 p.m.: SURVEY INSTRUMENTS

Hensler presented a briefing on Survey Instruments (summarized in the following charts). Discussion followed:

Austin: Has the neighborhood survey been pretested for reliability? That is, do different people have the same perceptions of the neighborhood?

Hensler: The survey of residential buildings has been quite thoroughly pretested, and the degree of reliability among observers is very high. The neighborhood survey is not as far along.

Austin: It might be pretty hard to get consistency on neighborhood data. I would still recommend using blocks rather than neighborhoods, and if you keep the neighborhood survey, restrict your questions to those for which you can get consistent data.

Hensler: We plan to use multiple sources of information for many of the data items, and thus we can verify observations in part.

SURVEY OF NEIGHBORHOODS

- LAND USE PATTERNS
- CHARACTERISTICS OF RESIDENTIAL BUILDINGS
- AVAILABILITY OF FACILITIES AND SERVICES
- CHARACTERISTICS OF RESIDENTS
- QUALITY OF LIFE

SURVEY OF RESIDENTIAL BUILDINGS

- NATURE OF USE AND TENANCY
- PHYSICAL CHARACTERISTICS
- TENANT FACILITIES
- EXTERIOR CONDITION
- INTERIOR CONDITION (PUBLIC AREAS)
- CHARACTERISTICS OF IMMEDIATE NEIGHBORHOOD

SURVEY OF LANDLORDS

- ACQUISITION AND OWNERSHIP
- EXPERIENCE AND ACTIVITY IN REAL ESTATE
- PROPERTY DESCRIPTION AND REVENUE
- MANAGEMENT, MAINTENANCE, AND OPERATING COSTS
- REPAIRS AND IMPROVEMENTS
- MORTGAGES, TAXES, INSURANCE
- PERCEPTION OF NEIGHBORHOOD
- LANDLORD-TENANT RELATIONSHIPS
- PLANS FOR PROPERTY

SURVEY OF TENANTS AND HOMEOWNERS

- HOUSEHOLD COMPOSITION, TENURE, AND SOCIAL BACKGROUND
- CHARACTERISTICS AND CONDITION OF HOUSING UNIT
- TENANT-LANDLORD RELATIONSHIPS
- PERCEPTION OF NEIGHBORHOOD AND SOCIAL PARTICIPATION
- HOUSING EXPENSES
- MOBILITY AND HOUSING HISTORY
- INCOME AND OCCUPATIONAL HISTORY

Hayes: Some of the interviewed tenants will be program participants on whom you have other sources of data. Will the verification procedures employ these other sources? For example, the HAO records will contain some income and expenditure data for low-income households--items which are generally very difficult to get.

Lowry: The HAO records will be linked with our other surveys. We estimate that about 25 percent of our interviewed households will have HAO records, and that about 15 percent of the HAO records will pertain to households living in our sampled housing units.

Hensler: There are ways to encourage people to give income data.

Hayes: Could you please explain why you do not sample all units in multiunit structures?

Lowry: It was a question of efficiency. Housing units (and also households) are pretty much alike in a large building. So we concluded that the marginal gains to be achieved from using more than six units are not worth the effort and expense.

Poggio: We are being careful to sample housing units randomly within structures.

Hayes: For landlords owning more than one parcel, do you administer the questionnaire for all of the parcels or only for those which fall in your sample?

Lowry: Only for the sample parcels.

Jessen: But that landlord is a supplier of services, and you only ask him about a part of his supply, so the meaning you attach to elasticity is strange. The marginal cost of getting the information on all of his properties is small.

Lowry: We expect the landlord to base his decisions for a given property on the characteristics of that property rather than on his entire holdings. We do ask the landlord about the extent of his real-estate activity and use it as an explanatory variable.

Hensler: I am not sure that the cost of the additional data-gathering for the other parcels is that small.

Aaron: But the response rate for large landlords is higher.

Lowry: For one property perhaps, but he may not continue to respond if you ask about all of his holdings.

Levien: Still, you could get the data on all of his properties even if you don't use it.

Mood: There are different kinds of property owners. Large owners frequently invest in property as a tax shelter; the small owner usually earns his living from his labor rather than from the investment. The transformation of inputs into quantity of housing produced is quite different in the two cases.

Lowry: That point is well taken; we are doing our best to distinguish between different types of owners and to capture information about inputs that do not pass through the market.

Mood: You may need to do more than just estimate the hours that a landlord devotes to his property.

Lowry: We will try to attach a value to the owner's time and to other inputs for which we do not have a market valuation.

Levien: Do you get ethnic data on the landlords?

Hensler: Only race, by observation.

Mood: You also need data on tenant labor that is essentially an input to housing.

Lowry: We have addressed that problem in the tenant questionnaire. We ask him how much time he spent on it and how much it would have cost him to have it done.

Bawden: Concerning the fluctuation of income over time: Annual interviewing is not frequent enough to capture these changes.

Crane: Concerning the format of the landlord questionnaire: Have you thought of setting up the section on income and expenditures like a set of books?

Hensler: Not all landlords keep their books in the same way. We anticipate encountering everything from official ledgers to shoe boxes, and we need to be ready to deal with all

cases. We are keeping track of the types of records we encounter while pretesting the instrument.

Hayes: Many states, Wisconsin for example, provide various tax exemptions for owner-occupants. Do you collect data on that?

Lowry: No, but we should. We would like to find out how prevalent this practice is and how large the rebates are.

Aaron: If property taxes exceed a certain percent of income, elderly owner-occupants can actually receive a rebate.

Bawden: Wisconsin has extended this policy to renters as well.

Lowry: We have to decide whether this is a reduction in housing costs or an income change. If it is a general program, we will probably treat it as a reduction in housing costs; if it affects different people differently, we will consider it a change in income.

Field: Are the sample sizes too small, too large, or just right for the proposed analyses?

Kain: To answer this, we would need information on average and marginal cost of the surveys.

Aaron: It would also be helpful to see other budget items. Since there appear to be budget constraints, we would need to know alternative costs.

Jessen: Are you making estimates of supply response separately for each site?

Lowry: Yes, we are not pooling the data from the two sites for analysis. We hope to find key parameters that are constant across both sites.

Thomas: There is considerable overlap of information between the tenant and the landlord surveys. If you beef up the amount and quality of information from the landlord survey and take mobility data from the HAO records, could you do without the tenant survey?

Lowry: I don't think so. The HAO records give data only for recipients; the landlord survey gives minimal information about individual units, and it is not a good source for more detail.

Thomas: If a tenant moves out and is replaced by another tenant, the second may describe the same dwelling differently. You have no way of knowing whether the unit has changed, or whether two different tenants merely perceived the unit in different ways. It would be preferable to have a trained observer rate features of the dwelling unit, like the Demand Experiment is doing.

Hensler: We have tried to ask questions mainly about objective characteristics rather than those that require subjective interpretation.

Poggio: There may be a bias between a tenant's perceptions at move-in and at move-out.

Austin: Tax rebates for the elderly and Social Security benefits may soon be increased. You may find that 95 percent of the elderly residents in Green Bay would not be eligible for housing allowances. If the percentage is that high, you should know it in advance. You need to sit down with state officials to understand welfare rules and to see how your allowance is going to fit with them.

Lowry: That is a good point. External events may be so important that they will indeed muddy the waters.

Heinberg: I would like to hear some discussion on the role of homeowners in the experiment, considering policies available for funding and homeowners' relation to supply response and the question of monitoring. It seems that the central measurement process was developed for renters. Does it make sense for homeowners? For example, use of Sec. 23 funds may restrict mobility, in that mortgages are not portable. The whole role of homeownership in the experiment may be very unrepresentative of that in a national program. So what resources should be devoted to the monitoring and analysis of homeowners?

Kain: Is it possible to obtain legislative changes that would allow you to treat renters and homeowners in the same way?

Lowry: We would be delighted to treat them in the same manner, but we cannot plan on it.

Kain: Would a change of this kind, say in Year 2, affect the experiment?

Field: There's no apparent rush at the moment to adopt a new housing bill.

Kain: You expressed concern that few homeowners will participate in the program. Is this due to the mechanism by which they are funded, or is it just that home buying is not popular?

Lowry: Home buyers have to go through Sec. 235, which puts strong constraints on them. We are stuck with FHA standards.

Aaron: Why not use research money to cover the initial transaction for a home buyer and put him in a position where he can be covered under Sec. 23 as a homeowner?

Kain: Can't we still hope for legislative changes? If you slow down enrollment, any legislative change would come earlier in the program. Would Rand and HUD put more energy into trying to obtain the necessary legislative changes if they knew that they would still have much of the enrollment process to go through a year from now?

Lowry: Any legislative change would affect the experiment, but we are not sure how. It does not make sense to decide the pace of enrollment on the basis of expectations about legislative change.

Kain: True, but if your pace of enrollment were slower, you might spend more energy in trying to obtain the necessary legislative change.

Field: We tried to have an amendment passed, but it hinged on the success of the housing bill. The probability of a legislative change is small. We would rather not base the experiment on any expectations about future legislation.

Austin: You also have to worry about extending Sec. 235 money to cover housing repairs. This is a complex issue and requires higher-level support.

* * * * * *

5:10 p.m.: ADJOURNMENT FOR THE DAY

WEDNESDAY, 27 JUNE 1973

9:15 a.m.: SURVEY COSTS

Lowry presented a briefing on Marginal Costs of Changes in Sample Size (summarized in the following chart). Discussion followed:

Aaron: What is the marginal cost of an entire site?

Lowry: We don't know. Kershaw indicated that the marginal field costs for a site lie between $1/2 million and $1 million.

Aaron: What is the average marginal cost?

Lowry: Somewhere between those of single-family and 2- to 4-unit buildings.

Crane: How important is it to have a complete five-year record? How many holes in that record could you tolerate?

Lowry: We need the baseline and the fifth year to tell total change. But this would not give us much information about the dynamics of the process.

Field: How many years would you need to get a picture of the dynamic process? If attrition becomes worse than you assumed, what additional efforts could you make to complete a five-year record?

Lowry: Our worst problem will be the high probability of missing data for the fifth year.

Field: What are the feelings of the panel as to data requirements for suitable analysis? Would one year of missing data be acceptable? Two years?

Kain: This is a complicated issue.

Jessen: Couldn't you use all combinations of pairs of years?

Lowry: Not if we are interested in the length of time it takes for the allowance program to take effect.

Field: The question is, What are the tradeoffs if we want to cut back on the sample size?

Wilson: That's a complicated question. It depends on distributions, analysis plans, etc. In some cases, you can compensate for missing data with regression.

MARGINAL MONITORING COSTS:
PANEL OF RESIDENTIAL PROPERTIES

Unit of Account	Marginal Cost ($)			
	One Site		Two Sites	
	Baseline	5-Year	Baseline	5-Year
One Residential Property				
Owner-Occupied	108	563	217	1,125
Rental:				
Single-Family	233	1,312	466	2,624
2-4 Units	288	1,582	576	3,163
5+ Units	714	3,762	1,428	7,523
One Complete 5-Year Record				
Owner-Occupied	246	1,224	492	2,447
Rental:				
Single-Family	685	3,774	1,371	7,547
2-4 Units	524	2,583	1,048	5,165
5+ Units	1,065	5,589	2,131	11,178

Hayes: Anyway, we still have the attrition problem, about which we can only guess.

Lowry: If we start with a large enough sample at baseline, we can drop some parcels later. However, we cannot do the reverse, since we would not be able to recapture baseline data.

Aaron: Judging from your marginal-cost numbers, differential bribery rates would be most efficient.

Kain: You could almost put the owners of 5-or-more-unit properties on the payroll.

Blum-Doering: We envision a high baseline and first annual response rate, but we can't anticipate what will happen beyond that, so no parcels in the baseline panel should be dropped from the survey prior to the second year.

Lowry: We must consider these survey costs in the context of the total budget. Allowance costs will run about $12 million

Mills: The objection seems to be a geographical one. In a small city, where landlords are not clustered, there would be no savings in a geographical approach. The savings from cutting the sample size may also offset the costs of batch interviewing.

Crane: What is involved in interview costs?

Hensler: It's not just transportation, it's administrative. We have to carry out landlord, tenant, neighborhood, and building surveys on the same property. As for interviewer time and cost, these are based on about 7 to 8 hours of interviewer time and cost for completing a questionnaire; this includes time for the interview itself and editing afterwards.

Mills: This is worth some further thought. I don't think clustering is a problem.

Lowry: The landlord completion rates can be factored to estimate the number of attempts required to get a completion at baseline. Our marginal-cost estimates include these attempts and the failures. If our estimated response rates are low, then the estimates of marginal costs are high.

Levien: In summary, then, you are suggesting that the sample size should be reexamined; Rand should take into account the possibility of reducing sample size by using bribery and adapting to the response rates discovered in the field.

Field: But field operations and sample-selection procedures may preclude adaptive measures.

Crane: The calculations are conservative, but I would not recommend cutting the sample at baseline. Rather, I would suggest undersampling during selected postbaseline years, since you don't need five points for every parcel.

Mills: Such a skipping procedure creates problems for analyses of supply response.

10:00 a.m.: GENERALIZING FROM EXPERIMENTAL FINDINGS

Lowry presented a briefing on Inference from Experimental Findings (summarized in the following chart). Discussion followed:

per year. In addition, there will be the administration costs and research costs estimated at about $4-1/2 million to $5 million per year. Of this last, field survey costs will run about $1 million.

Wilson: What's your view on paying landlords?

Lowry: Small payments will have little effect. With, say, $100 payments, costs would equal the costs of administration and coding of a landlord questionnaire; $100 is the break-even point.

Wilson: But the opportunity cost is greater than $100, due to non-response bias.

Crane: The refusal rates for a landlord who replies for four years must be low.

Bawden: To get 1,000 landlords at the end, you could start with a smaller sample if you bribe.

Field: How much could the nonresponse rate be improved beyond the 90- to 95-percent response rates for landlords who have completed baseline interviews?

Lowry: We assume responding landlords will continue to respond.

Bawden: I think your estimates of response rates are conservative.

Mills: If the attrition turns out to be less than you assumed, say, if you get 80 percent response, then you can cut back on your fieldwork.

Lowry: At baseline, we keep going until we reach the target number of parcels; we can always do more.

Poggio: There are sampling problems involved in terminating baseline surveys before completion of the selected random sample of parcels if the target is indeed reached early because of higher-than-anticipated response rates.

Hensler: To stop sooner presents operational difficulties as well. We are interviewing by geographical area, not in random batches, since the latter adds to the expense. Our gut feeling is that the random-batch approach is not suitable for our type of experiment.

INFERENCE FROM EXPERIMENTAL FINDINGS

1. SITE-SPECIFIC EFFECTS OF EXPERIMENTAL PROGRAM

   - Reliability of Sample Data
   - Duration of Monitoring Program

2. MODELING THE EFFECTS OF A NATIONAL PROGRAM

   - Estimating "Portable" Behavioral Parameters
     - Income Elasticity of Demand
     - Price Elasticity of Demand
     - Price Elasticity of Supply
   - Constructing a Market Model
     - Aggregate Demand Function
     - Aggregate Supply Function
     - Market-Clearing Price of Housing Services
   - Mix Effects vs. Configuration Effects
     - Comparing Behavioral Parameters Among Sites
     - Proposed: A Third Big-City Site

Mills: Work has been done on these issues and should be considered. You should do your analysis, but take this previous work into account in presenting your results. The effects of income on housing consumption are fairly well known; less is known about the effects of price changes. You may encounter some difficulties due to rent controls in the future.

Lowry: This is a serious issue. Even if such controls are not effective, they would undercut our credibility. Phases I, II, and III were not binding in the markets we're interested in. This is comforting.

Mills: The least is known about supply response. I'm concerned about the imposition of standards and their effects on

the results. You are constraining the supplier to specific points on his supply curve, rather than permitting free movement along that curve.

Lowry: Our results may not be generalizable to other housing markets, but they will be pertinent to housing markets under an allowance program.

Mills: If that program is the same as your experiment.

Heinberg: The most likely national program would have standards. With imperfect enforcement, such standards already exist. We may be interested in the political/legal reaction in a marketwide context to the enforcement of these standards.

Austin: There is possibly a bad contamination factor. There is no evidence that the mandating of such standards by Congress correlates with their enforcement. In a tight market, controls are relaxed. If you require a consistently administered enforcement, you would obscure whether the Supply Experiment demonstrates that rising incomes cause quality to increase. It is only partially a test of the supply function. It would also test how well codes are enforced, which is really an administrative experiment.

Dubinsky: In Wisconsin, the state code is minimal. Only the city of Green Bay, of the local governments in our site, has its own code. The enforcement of existing codes in our sites is probably better than in larger SMSAs.

Field: Should we direct our analysis more toward the case-study approach or toward a modeling effort and inference to other sites?

Mills: That depends on what you find from your data. If the elasticity is very high, it will be easy to extrapolate.

Field: You would do many more different things in a case study, but I'm not sure what.

Crane: City officials are less interested in economic models and more in the benefits and disbenefits of the program for their property taxes and payrolls. You should place more stress on the political atmosphere.

Lowry: The clearest visible benefit to the city governments is that property appraisals go up, making tax revenues increase. We are also actively concerned about reaching agreement with the city housing-inspection agency so that we don't interfere with each other.

Field: We're probably creating the largest bureaucracy in Green Bay. The local officials will probably struggle less to kick it out than to control it.

Alesch: There is a feeling of pride in the city bureaucracy that Green Bay was picked as a site. This is a positive benefit to them. They also see money being pumped into the local economy. There are too many local governments around to "pay officials off" individually in terms of larger bureaucracies or any special attention. The boost to the economy and the tax base is the primary payoff to the county.

Field: The city governments must agree to let the experiment come into the area.

Alesch: The payoff to the site is in the input into the local economy in the form of jobs, prestige, and the impact on their economy.

Wilson: I feel it would be a mistake to abandon the attempt at generalization. The case study would provide a fallback. It would be hard to justify a national program to Congress just on the basis of a case study.

Bawden: The extent of generalizability depends on the standards. Are the minimum standards the same all over the U.S.?

Lowry: There is more variation in the enforcement than in the intent of the standards.

Aaron: Is there any evidence to suggest that minimum standards significantly raise housing quality?

Lowry: We have no evidence. The people enforcing the standards have their heads screwed on; they won't force people out in the street.

Alesch: There are two elements to the proposed standards: (1) the characteristics of the unit, and (2) the size of the family relative to that of the unit.

Alles: This may be the first time that there are *positive* incentives for landlords to meet standards, such as that provided by the allowance payments.

Mills: It's hard to separate the effects of the increase in demand from the effects of code enforcement.

Lowry: Rand will be responsible for disbursing allowance payments; we want to inspect for housing quality in any case to protect the Housing Allowance Office.

Kain: I would have done things differently, but since the experiment is so far along, most of my proposals must be add-ons rather than tradeoffs. In what spirit should I propose substantial changes?

Lowry: You propose; let us provide the spirit.

Kain: The experiment ought to be less concerned with the experimental approach.

Levien: Could you hold off on this until, perhaps, this afternoon?

Heinberg: There is some confusion about whether this is an income-maintenance or a housing allowance program. You need some earmarking of the subsidy payment to housing to make a distinction, and the use of standards is one possibility. If you're going to use Sec. 23 funding, you are constrained by the requirements therein, and standards are one of them.

Aaron: There are several forms of earmarking: price subsidy, quality constraint, or payment constraint. In any case, it is critical to evaluate how much more people choose to spend on housing than they would if given unconstrained cash. You need to know the extent of upgrading to meet standards. Rand doesn't know how much an allowance would impact demand above what an income-maintenance program would do.

Hayes: I have the same concerns as Aaron about the standards and their relation to the inventory in the two sites. Making

the existing codes work is not good enough. The choice of standards must be an explicit decision, since they are a measure of the quality of housing and HUD ought to test what it wants to achieve. We don't know how codes relate to consumer utilities. We need to know something about the cost of attaining the standards. If they are too high to be met with the funds available, there will be problems. There are also difficulties with the supply function; the use of factor inputs and accounting for exogenous price increases should be clarified.

Alles: Section 23 constrains us to use some standards. We are in contact with and have made presentations to local officials in Green Bay. It is important to measure the impact of the program on the local economy.

Austin: We must consider both the physical condition of the unit and its crowdedness. There is the question of whether or not you are trying to enforce the crowdedness standard. If so, the landlord may not be able to meet the standard due to the size of the family living in a unit. We do not know anything about the tradeoffs that people are willing to make between crowdedness and the quality of the dwelling unit. We should let the market deal with this problem.

Ozanne: Because standards are so important in affecting outcomes from housing allowances, you should have the same standards in the Demand and Supply Experiments in order to integrate the results.

Field: The Federal government is moving away from the practice of setting and enforcing standards. A national program of housing allowances would probably involve local standards which are locally enforced, so there is no need to be concerned about whether or not to adopt local codes.

Hayes: We should look at housing codes and their relation to quality. I doubt that housing codes have very much to do with housing quality as perceived by the renter or purchaser. Reliance on local enforcement is a second question

and it can vitiate the purposes of the experiment by variations among jurisdictions and among inspectors.

Field: It's not necessary to set standards which are different from local standards. If the standards differ, landlords may raise an outcry about not being certifiable.

Ozanne: As we have said, it is necessary to have comparable standards for integration of supply and demand.

Field: That is why I raised the question of a case study versus generalization to different sites from your statistical analyses.

Hayes: Concerning standards: Physical standards have less to do with quality than do service standards, but maybe I'm wrong.

Field: You mean like heat and running water?

Levien: Concerning the portability of the supply curves: You're not measuring a pure supply response, but one which is constrained by standards. This will affect its generalizability to other sites or to a national program.

Kain: Those problems are among the least important. The supply elasticity is very much dependent upon the way in which the demand shock is introduced.

Bawden: One way to solve this problem is to raise the minimum standards over time. For example, you could offer recipients one-half of the allowance for the first year regardless of whether or not they lived in standard housing. At the end of the first year they would have to have occupied standard housing or they would lose the allowance entirely.

Field: There is a problem in that to use program funds for the allowances, we must state and adhere to the minimum standards. Perhaps research funds will be available to subsidize households in the above manner until they can find certifiable housing.

Bawden: To ease the initial shock on the market, you should give people a longer time to adjust to the minimum standard.

Lowry: You could achieve the same effect if you just told people that they could not have their allowance until they occupied housing which satisfied the minimum standards.

11:15 a.m.: CONTINGENCIES

Massell presented a briefing on Contingencies (summarized in the following charts), which was followed by a discussion of budget contingencies: Section 23 and 235 funds are committed for the length of the program, but monitoring funds could be cut.

Wilson: Congress could tell HUD that they could not use any of their money for the allowance experiment.

Field: This issue will be cleared up shortly. HUD has asked Congress to budget about $160 million to $170 million for the allowance program. Once allocated, this money cannot be recalled.

Hayes: You have made no commitment for updating R^*. Why is this?

Lowry: This would cause problems. It is rather a self-fulfilling prophesy. We would be willing to make such a commitment if HUD is willing, but we don't see the necessity of such a commitment.

Kain: Then you expect to increase R^* so that it will keep pace with the cost of living, but you are not formally committing yourself to such a policy?

Lowry: That is right.

Kain: Aren't you worried about how the lack of such a commitment will affect landlord behavior in the face of substantial inflation?

Lowry: We want to make the experiment comparable to a probable national program. Would Congress make such a commitment?

Kain: Yes.

Mills: Congress would not make such a commitment in advance, although they might increase the allowance over time to keep pace with inflation.

Kain: Landlords would view a national housing program as permanent, but they will view the experiment as temporary. That is the reason for the ten-year time horizon for the

CONTINGENCIES PREVENTING ACHIEVEMENT OF EXPERIMENTAL OBJECTIVES

- PROBLEMS WITH ALLOWANCE PROGRAM
- PROBLEMS WITH MONITORING PROGRAM
- INADEQUATE ANALYTICAL METHODS
- INADEQUATE BUDGET

PROBLEMS WITH ALLOWANCE PROGRAM

- ALLOWANCE FORMULA UNLIKE NATIONAL PLAN
- FORMS OF PAYMENT UNLIKE NATIONAL PLAN
- MANAGEMENT UNLIKE NATIONAL PLAN
- PARTICIPATION UNLIKE NATIONAL PLAN
- PROGRAM MODIFICATIONS DUE TO PUBLIC ANTAGONISM

PROBLEMS WITH MONITORING PROGRAM

- OMISSION OF IMPORTANT VARIABLES
- INSUFFICIENT SAMPLE SIZES
- UNRELIABLE SURVEY DATA
- DATA-MANAGEMENT FAILURE

PROBLEMS WITH ANALYTICAL METHODS

- AMBIGUITY OF VARIABLES
- INADEQUATE EXPERIMENTAL CONTROLS
- BIAS IN PARAMETER ESTIMATES
- HEAVY DEPENDENCE ON MODELING
- HAWTHORNE EFFECTS
- COMPATIBILITY OF SUPPLY AND DEMAND DATA

BUDGET CONTINGENCIES

- UNEXPECTED BUDGET OVERRUNS
  - ALLOWANCE PROGRAM
  - MONITORING PROGRAM
- INSECURE LONG-TERM FUNDING

experiment, but a guarantee for ten years at current prices will not necessarily convince landlords to act as if the experiment were a permanent program.

Mills: You could account for this by allowing, say, a 5 percent per year increase. You can take this problem into account if you model landlords' behavior in the face of inflation, treating the inflation in the same manner as lowering the allowance.

Kain: If Rand fails to take the problem of the effects of inflation on landlord expectations into account, they may determine that the supply response is less than it actually would have been in the absence of the inflation, or if there had been a commitment to increase R^* to keep pace with the cost of living.

Lowry: But if an actual national allowance program does not make such a commitment, our prediction of supply response would be correct for a market facing inflation.

Ozanne: But you are not really designing a scaled national program because of the magnitude and rapidity of the demand shock you are introducing.

Rydell: You are placing too much emphasis on what a landlord will do when he sees the level of R^*. Landlords base their supply decisions on the actions of tenants. The only assurance they need is that the program will continue, and thus that the demand increase will continue.

Kain: Landlords have a view of the market based on the past ten to twenty years of experience. The experiment reduces the old demand for low-income housing by enforcing a minimum standard. You must overcome landlords' expectations which are based on their past experience. If you guarantee them this year's rent for the next ten years, this will have a significant impact on the way in which they respond to the program. You should tie R^* to the Consumer Price Index in some manner.

Mood: The ten-year experimental horizon pushes expenditures toward maintenance and repairs and away from capital expenditures. Without attaching R^* to the Consumer Price Index, this causes serious problems.

Lowry: Capital expenditures are in current dollars. They are immune to future inflation.

Alesch: The price of materials (lumber) in Green Bay is increasing rapidly. I am more concerned about exogenous inflation, such as changes in the price of materials, than in program-induced inflation.

Aaron: This is all too refined. Landlords perceive tenants facing a conditional offer. They only worry about R^* as it affects tenants. There is no clear link between R^* and the quality of housing or improvements in it. Landlords respond to tenant demand, and they are only worried about how many tenants will demand standard housing as a result of the program. There is no link between refined changes in R^* (i.e., the small changes which would be necessary for R^* to keep pace with the Consumer Price Index) and landlord response. This result is a serious implication of the minimum-standards approach.

Kain: It depends on where the notch--that is, the minimum-standard cutoff--is.

Aaron: If the minimum-quality standard is not effective, the allowance payment is just an income-maintenance payment.

A discussion followed about how Rand treats changes in recipient income, how it affects allowance payments, and the problems entailed by this.

Lowry: We can recertify recipient income at other than six-month intervals. We have discussed with HUD the frequency of income recertification. At the moment, we plan an annual recertification requiring an office visit, and half-year self-certifications by mail.

Bawden: Will the allowance be based on past income or your expectations about the recipient's future income? Concerning female heads of household: Is the relevant income history hers or somebody else's (e.g., a spouse)?

Wilson: Concerning the dynamics of income and the importance of your income-reporting period: The period of time between recertification makes a significant difference in the level of allowance payments.

Bawden: Also, the provision of allowing voluntary interim reporting will significantly affect costs. Families experiencing declining incomes and/or increases in family size will report and get larger payments; families experiencing rising incomes or smaller family size will probably not report. Thus the overall cost of the program will be larger than if voluntary interim reporting is not allowed.

Alesch: Currently we are planning minirecertifications at six-month intervals and random checks as well to detect overpayment. We plan to use income histories as a basis for projecting what is likely to happen to a recipient's future income, which will in turn direct our random detection policy. More emphasis will be given to income projecting than to

averaging past incomes. But the dynamics of incomes are more relevant to operations than to the analytical design.

Bawden: Basing payments on income forecasts rather than on past income will significantly affect transfer costs (because families will have an incentive to underestimate future income). Moreover, if future income is more than the forecast indicates, will the amount of overpayments be recovered?

Alesch: Estimating the number of people who show up for allowances is more critical to determining the ACC [Annual Contributions Contract] than the problem of overpayments.

Wilson: But the number of people that qualify depends on how you deal with income.

Kain: What if every unit presently in Green Bay qualifies as standard?

Austin: You need to specify in advance a detailed system of rules concerning eligibility. This includes procedures for income recertification. You cannot have administrative personnel making random checks on eligibility and judgmental decisions based on intuitive assessments of "need."

Lowry: We have given much thought to grievance procedures.

Hayes: Concerning the budget: Does Rand's budget allow for increase in R^*?

Lowry: The only budget estimates have been made for full-scale participation at 1969 prices and incomes. Further projections require that we take into account both income and price increases, and this is difficult.

Levien: A question about funding: Do you have to estimate the contract cost for the full ten years?

Field: Yes. It is possible that R^* can go up, but the contract authority does not give us open-endedness. We have to take into account the number of participants, the amount of payments, and the length of the program to estimate the cost for the entire contract.

Heinberg: Concerning the backup analysis: Are there any reactions to it?

Wilson: I have no problems with what is in the paper, but the analysis is a negative approach. I would suggest that a more positive approach is to try to prevent a catastrophe. This could be done by paying landlords to respond to the surveys, making political friends, etc.

Aaron: I feel that the instrument for obtaining landlord data from the tenant is rather blunt and will result in very low R^2, and that it introduces the problem of bias. I am not happy about the proposed plan, but I have no alternative solution.

Bawden: A better method would be to cost improvements on the basis of descriptions obtained from the tenant.

Lowry: That is in effect what we will be doing. We worked with this type of landlord data in New York City and were struck by the small variance in the patterns of the data once account was taken of easily distinguishable characteristics which make for different production functions (size of building, etc.).

Wilson: Are you going to try to price out the improvement information that you get?

Lowry: We plan to take data from both the landlord and the tenant questionnaires at baseline and see how well the data on improvements fit. With regard to the surveys: The interviewers will inspect the public areas of structures but will not do a room-by-room inspection. We will ask the landlords and tenants to describe any improvements that have been made.

Hayes: Due to the minimum-quality requirement, you will have good data on improvements made in participants' units.

Lowry: The way in which we deal with this problem depends on what we learn from the comparison of landlord and tenant data on improvements at baseline.

Kain: I would think that your fieldworkers would become pretty adept at inspecting units. You could probably enrich the quality of your data by allowing your interviewers to

inspect the housing units, especially the bathrooms and kitchens.

Ozanne: Since you will be using regressions anyway, why not use a hedonic index and regress rent on improvements rather than using expenditures for improvements?

Massell: That is an alternative approach which we are considering. Here, however, we are trying for something else, i.e., to augment landlord data that we expect to be available.

Field: How important is it to actually observe improvements? Is a tenant's description close enough? What are the problems with such an inspection?

Kain: Some improvements can be made in a wide variety of ways. For major renovations, a fieldworker should be able to evaluate them adequately, and you would probably get better data in this way. I do not think such an inspection would present major problems. In most cases I would expect the respondent to invite the interviewer to inspect any improvements that had been made.

Field: What about the implications of such an approach for refusal rates? I would think there is a tradeoff involved: There is a gain due to improved data and a loss due to increased refusal rates.

Kain: The refusal rate would depend on the skill of the interviewer.

Ellickson: There is a difference between asking to inspect improvements made in a person's unit and asking to check the bathroom for deficiencies.

Rydell: We want information on the actual components of the improvement, not just on the end result. An inspection by a fieldworker will not tell us what was involved in the repair. It is difficult to tell by looking at the end result what was there before and how much the improvement involved. It would be preferable just to ask the tenant to describe the improvement and then to evaluate it ourselves.

* * * * * *

BREAK FOR LUNCH

1:30 p.m.: SUMMARY REPORTS

Aaron: The change in demand attributable to housing allowances will occur against a background of increasing demand. Rand does not really worry about this. Also, there may be problems because the introduction of allowances will be sudden; the purpose of the Supply Experiment seems to be to see if the effects of this are serious. I endorse Kain's position, favoring as gradual as possible a phase-in of allowances to avoid what would not and should not occur under a national program. If program initiation is sudden and there are distortions, it will be hard to convince decisionmakers that this would not occur if introduction was slow. If introduction is slow, Rand and HUD will be in a better position, since they could point to the (presumed) lack of adjustment problems under gradual introduction and warn of potential problems from fast introduction.

My concerns center on one aspect of the experimental design--the minimum standards requirement. Most of the experimental design strikes me as highly professional and competent. I am doubly disturbed, therefore, that problems stemming from the use of the minimum-standards requirements are likely to undermine the experiment.

The only differences between a housing allowance and a negative income tax is that the former contains certain incentives to encourage or to compel recipients to spend more on housing than they would voluntarily elect, while the latter does not. There are three such incentives: (1) a price subsidy, expressed in housing allowances through a percentage-of-rent formula; (2) denial of the subsidy to potential recipients whose housing does not meet basic standards; and (3) denial or reduction of benefits to potential recipients who pay less than a specified rent for housing (or equivalently, who spend, in addition to the allowance, less than a specified amount from their own resources in housing).

Rand has elected to use the second kind of incentive, the minimum-standards constraint. Unfortunately, the Design Report contains no discussion of these standards. The report indicates that most allowance recipients will live in housing initially below these standards; this implies that the standards will be set to achieve this end.

Vagueness on this point is, quite frankly, appalling for at least two reasons. First, vagueness stems from the genuine impossibility of knowing in advance what standards for which housing characteristics will guarantee that any fraction of allowance recipients reside in below-standard housing. Data simply are unavailable to make this determination before all units of all potential recipients are inspected. Second, and far more important, the nature of the standards--how many and how stringent--will determine how much owners must spend to bring their units up to standard. Thus even if the characteristics of every unit were known in advance, it would be impossible in the absence of specific standards to know the cost of meeting them.

In other words, it would be impossible to know the size of the stimulus to housing improvement the allowance would create. Note that the size of the stimulus hinges far more on the nature and level of these standards than on the size of R^*, α, or β. These parameters determine the size of the allowance a household gets. Whether a household chooses to accept the allowance depends jointly on the allowance amount, the increase in expenditure required to obtain standard housing (the household automatically participates if it already occupies standard housing), and the utility of bringing housing quality up to standard. The fact that the household has accepted the allowance tells one only that the household either occupied standard housing initially or upgraded its housing by some undetermined amount. Thus, the size of the allowance is not clearly related to the increase in housing demand. All of the

allowance paid to households already occupying standard housing and some unknown part of the allowance paid to families who initially occupied below-standard housing is fungible and can be used freely for purposes other than housing.

But there are more basic shortcomings in using minimum standards to upgrade housing under the allowance. These shortcomings stem from the fact that minimum standards can never cover more than a small part of the relevant attributes of a housing unit. They can cover plumbing fixtures (but not whether they are promptly repaired), kitchen appliances (but not their age or adequacy), the heating system (but not insulation and drafts), and so on. They cannot stipulate that hallways be kept clean or when carpets should be replaced or floors sanded. They cannot specify how often walls should be repainted or papered or anything else the quality of which depends on tenant behavior. They cannot specify how good neighborhood schools should be or what the crime rate on the block must be. In short, housing standards must omit many, perhaps most, of the attributes that determine how much people are willing to pay for an apartment. In addition, minimum standards must be chosen for administrative simplicity. That means that they must apply to important, objectively measurable attributes. To minimize the judgment required for inspectors, the standards must be simple on-off indices, e.g., private toilet or not, hot and cold running water or not, etc. Thus, whether a unit fails all standards massively or passes all but one with distinction is irrelevant--it fails in either case.

The inevitable incompleteness of any set of minimum standards creates a dilemma. The standards may be set low, so that only the most egregious hovels fail. In that case, the housing allowance is really unrestricted cash assistance for most recipients. It is a housing allowance in

name only. While such a course may be good politics--a clever way to get a negative income tax past Congress--it clearly is not a housing allowance, and an experiment will yield almost no information about the impact of an increase in demand keyed to housing.

Alternatively, the housing standards may be set high so that a sizable fraction of the units will fail to meet them. Then, however, the standards must be arbitrary. The number of units that fail one or even several standards by a small amount while being quite adequate in all other respects will be vastly increased. If one requires only that the apartment have a private kitchen, the few units that fail will almost certainly be poor indeed. If one requires that the kitchen contain specified appliances, cabinets, and floor space, so that many units must be improved to meet the standard, inevitably a number of them will be quite adequate in other respects.

The use of minimum standards also can create enormous incentives for irrational investments. If a unit renting for $100 per month is below standard and can be upgraded at an investment cost of $2,000 into a standard apartment that will rent for $120 per month, the rational landlord will not undertake the investment if his interest rate is more than 3.5 percent (assuming the improvement will last ten years). The allowance may confront the owner with the prospect of a 50-percent vacancy rate if he does not make the improvement, making the investment seem worthwhile if his interest rate is less than 20 percent.

In summary, the minimum-standards approach suffers from two rather disturbing faults: It expresses a silly policy, and it will make the experience virtually useless.

What should be done? The proper course, I think, is to adopt one or another form of the minimum-rent requirement. Personally, I favor setting the minimum contribution each household must make from its own resources. This

forced contribution rate can be made a function of income or held constant at all income levels. As a practical matter, it seems undesirable to disallow the subsidy completely for households that pay less than minimum rent or make less than the forced contribution. Instead, the allowance could be reduced by some amount, say half, of the shortfall of actual below-minimum rent or of actual household contribution below forced contribution. Also, as a practical matter, it would be necessary to set some very basic, low-level, minimum standards to prevent public monies from being used to pay for hovels. The great advantage of the minimum-rent requirement is that one can determine directly and beforehand how large an increase in demand the experiment is causing. It also permits recipients to choose the mix of housing services they prefer, a major advantage of housing allowances over existing programs that the minimum-standards approach negates. The chief shortcoming of the minimum-rent approach, even when softened by only a partial loss of benefits for failure to meet the requirement, is that it may engender collusion between landlords and tenants. In view of the total unacceptability of the minimum-standards approach both for policy and for the experiment, it seems necessary to determine whether this problem is serious.

On another issue, the Design Report was confusing about the relationship between price, inputs, and quality of housing. To measure quality, you would have to use a hedonic index or other index; input accounting is not vital for such quality measurements. However, quality measurement is not and should not be the central focus of the Experiment. The HASE tends to treat background effects too casually. Personal income rises at 8 to 10 percent per year; over the entire experiment, this would be a 60- to 70-percent increase of incomes just from background effects. It would be hard to sort out the effects of allowances from background effects.

The impact of allowances on residential mobility requires modeling; we recognize that these models are not well developed generally, but we cannot say much without them. I am very concerned about the ability to sort out these effects.

Whether this set of arguments points to control sites or not is a question of experimental cost. I feel that the above arguments do point in that direction.

Austin: I am concerned with the operational structure. Rand seems to be preoccupied with the idea that this is a supply experiment only, but it is also a demand demonstration and an administrative demonstration. Congress will raise the question of work-incentive effects regardless of the intent of the experiment.

The study should focus on spillover and interaction effects. The HASE will have greater local visibility than other experiments. You should consider ways of gathering data comparable to those of the other experiments, and you should specify administrative details, keeping a diary of the political atmosphere.

I would like to see an early analysis of the impact of increases in other transfer programs on the eligible population (e.g., increases in Social Security benefits). You should have a specialist in income-transfer programs and you should encourage families to make full use of other programs, with the housing allowance as a supplement. You should reexamine the concept of neighborhood. The present neighborhood-designation scheme is just a stratification (disaggregation) of citywide differences; it may not be a relevant variable for the households in the panel sample. You should ask if the variables used to determine neighborhoods are reliable.

The problems of generalizing to large cities should also be considered. I suggest that Rand conceive of a procedure that focuses on design preparation for a large city

rather than operation. You could use the Green Bay experience to develop economic and sociological models to describe (model) city experience rather than operationalizing in a large city.

Bawden: I am in complete agreement with Aaron. Some additional points:

1. Rent certificates are stigmatizing--analogous to food stamps. You should consider dropping them for part of the population to see the effects on participation.

2. There is much to be learned about participation behavior. This should be explored in the Administrative Experiment, but it won't be; and it can't be in the Demand Experiment. You should consider varying the method of enrollment. For example, offer, say, three months of enrollment stimulated only by mass-media advertising; two-thirds of the sample might be selected to receive brochures explaining the program. Three months later, half of this two-thirds might be selected for personal contact by an outreach worker. In this way, the cost and benefits (in terms of increased participation) can be assessed. Also, I would recommend interviewing a sample of those who inquire about the program, are eligible, but never enroll. It is important to know *why* they never enrolled.

3. I am very interested in whether recipients must move to get housing improvements or whether they can induce their landlords to make improvements.

4. The analysis of intermediaries is important for mobility and search. The HASE seems confused between the role of quantification and statistical inference. You should try to quantify information from intermediaries, rather than just talking to them, even if the sample is too small for inference.

5. I agree with Rand that homeowners who have paid off their mortgages should not be penalized.

6. Sample sizes are hard to assess without detailed calculations, but I would like to see rural homeowners flagged. Presumably, the reason for stratifying for this group is that we expect differences in the difficulties of improvements for them; if so, a sample size of 27 seems too small to tell anything.

7. Rand should do a paper on how to run a political demonstration. The effects of anecdotal information may swamp research results. If Rand would need a year to get information to refute this, it won't work. You should try to anticipate what kind of information reporters will try to dig out, and get there first.

8. A control site is important. It would not be necessary to measure everything, though; you would only need to know expenditures for inputs for improvements.

Crane: I agree with Aaron: I don't understand the removal of exogenous effects through a complex model. According to the Design Report, the output of the program is the change in the quality of the percentage of a person's life affected by housing. You should think of how the reported results will be received by noneconomists. Also a control group should be used as a backup evaluation procedure to support economic analysis. I suggest a simple before-and-after survey of the quality of housing: a survey of about 3,000 households across ten small midwestern SMSAs that bracket Green Bay--stratifying the sample on race and income--and a survey at baseline and at the end of monitoring period. Then, at the end of the experiment, you could say whether the quality of housing improved by a measurable degree in Saginaw and Green Bay.

Hayes: I agree with much that has been said, particularly with Austin; we can learn about demand and administrative aspects of allowances because of the scale of the Supply Experiment. More attention should be paid to the process of adjustment in supply. The lag in making improvements

is very long. You ought to model this process to sharpen the acuteness of observation; then you should estimate the time taken for these steps and evaluate what can be done to accelerate quality change.

I question the character of supply response in terms of current expenses versus capital inputs. In Green Bay, 50 percent of the participants will be homeowners, and in this case, almost all expenditures will be for capital improvements. You have to consider the division between capital and current expenditures for rental properties and the division between improvements prior to certification and improvements afterwards. We should know about the extent to which allowances will subsidize capital improvements as opposed to current costs.

A study of intermediaries is also very important: whether loans will be made in adequate amounts; what will happen to the repair and rehabilitation industry; how long adjustments will take. There may be a substantial lag for this purpose. The whole area of investment is important.

Regarding landlord reaction to recipient tenants, how will improvements be split in multiple-unit dwellings?

I suggest that the payment formula be reevaluated. The suspicion is that it will have greater impact for smaller families than for large ones. Could the contribution rate be altered by family size?

On the issue of control sites, I agree with Crane's comments. Rand should also look at what might happen without a program, using some simple model, which might include changes in income, rent, and migration. Rand's expectations of what might happen to rents under the program should be made more explicit.

I agree with Austin about the use of a large city. Rand's proposed sites will not yield good inferences for some areas of, say, Harlem. Maybe a less than full-scale experiment would be sufficient; only a few hundred buildings in such an area could be surveyed.

Jessen: I am not particularly happy with the logic used in the choice of sites; however, I have no simple alternative to suggest. I am particularly concerned with the absence of a large metropolitan area. If this undertaking is to be regarded as an experiment, then Rand should seriously consider control sites. Otherwise, the project should be labeled a demonstration or pilot. Concerning what to do in the sites, I feel that sample planning has been done competently, though there may be more efficient ways of doing it. A particular problem may be landlord attrition; the landlord sample could be rotated to minimize the problem of nonresponse.

Kain: I am also in agreement with Aaron, particularly on the slow phase-in and on the use of a minimum-rent instead of a minimum-quality standard. I have two sets of comments: Thinkable and Unthinkable. The Unthinkable: The study design suffers from tension between experimental/scientific objectives to permit generalization to a national program and a demonstration to show the feasibility of a national program. I suggest a sharper separation of these two objectives. I would first conceive of the experiment as a pilot or demonstration program to test the feasibility of a program of this kind. In this spirit, I would choose as the test site the toughest and biggest metropolitan area possible within the budget constraint. The primary objective of this supply demonstration would be to find out if a housing allowance program could be implemented without a disaster. A secondary focus would be on analysis of the effects of the allowances on the housing market. The Thinkable: I suggest monitoring and modeling the housing markets in a number of other sites and doing the studies necessary to make inferences about other types of housing allowance programs. In particular, I would recommend a large-scale demonstration in a hard area. Green Bay should be scrapped and Saginaw retained; the Green Bay money should

be used to do the series of controls proposed above. One control site should be as much like Saginaw as possible. These controls would show whether the fears of adverse consequences are real, would indicate the best way of phasing in a program, and would provide more information on the operation of the private housing market.

Mills: Rand has done a good and thorough job of research, but it is not what I would have done. I have a great desire to simplify government programs, and I would have designed a program in which the government pays a fraction of rent, with the fraction falling as the recipient's income increases. The program should be undertaken in two sites--both with large black ghettoes, because the problems are not serious elsewhere. I would change the formula in the second area, as well as the amount of physical intervention. Concerning the speed of start-up: It is important that the thing not blow up in the first year, but this seems an unlikely problem, particularly in Green Bay, even with a fast start. You might ask, What do you lose by a slow start-up? There is the danger that you won't be able to measure some price increase; if you don't observe some price increase, you will wonder if you measured the right thing. This argues for fast start-up. In Saginaw, you may get a price rise even with a slow start because of discrimination. Fast start-up means taking a risk; but with a slow start, the risk is that after five years, prices would have increased entirely as a consequence of general inflation--and people might then say you measured the wrong thing. I suggest that standards of housing not be used in one of the metropolitan areas. If this can't be done, then Rand must try to learn how important the imposition of standards is to improving housing.

The arrangements for assistance to homeowners are cumbersome; it's not clear that anything can be done, however, since these arrangements seem to be constrained by HUD and the law.

Mood: I would like Rand to start over in selecting sites. I think the experiment should be done in a site where the situation is tough--a big-city site. You should keep Saginaw but replace Green Bay with a city ten times as large--call this larger city Green Blocks. Select 10 percent of the blocks in the city as your experimental population, and offer allowances only to residents of dwelling units on these blocks. You would lose saturation pressure, but you get that in Saginaw. At the bottom of the rent distribution, you would get less pressure because households have alternatives in nonmonitored blocks. The point is, to make generalization to the country as a whole possible, you should collect data in a way that minimizes the judgmental leaps necessary for generalizing.

Mills: What can you learn about the supply side from this?

Mood: A lot of suppliers will respond. Response will be the same for all but the bottom echelon of suppliers.

Mills: I take exception to this. You would get little real improvement in housing, and the experiment would not test the housing supply.

Muth: [Prepared comments read by Lowry. See pp. 151-160.]

Wilson: We should consider the possibilities within the current constraints. Green Bay and Saginaw are given. Besides, this is not that big a problem; half the poor live in rural areas and small towns.

A control site is not worthwhile. Instead, you should model and monitor behavior in three areas: a rural, a southern, and a large-city ghetto. There is no need for a formal control site.

This is the largest social experiment ever. The outcome could determine the future of U.S. social experimentation. You should expand the analysis to include experience in administering the program, because this information would be better than that from the Administrative Experiment. You also should cover the participation behavior of

eligibles because this is important to projecting the budget for a national program. The experiment should include an analysis of the dynamics of income on eligibility, enrollment, and cost.

I vote for a gradual start-up.

Rand should give more forethought to the political context of social experimentation to avoid disaster: (1) You should consider how to deal with local politicians and Congress. (2) A research timing plan is needed to say something about the impact of the experiment before it is over--what will Rand be able to say at the end of the first year? (3) You should consider the relation to the GAO and others who will want to help run the program. (4) You should think about what could be learned from the Demand Experiment if Congress cut off the money for the Supply Experiment.

Not enough attention has been paid to integrating the results of the Demand and Supply Experiments; I would like to see Rand do more on this, working with the Urban Institute.

ADDITIONAL COMMENTS

Aaron: I agree with Wilson concerning Congressional relations. You should announce that you will have an interim report at the end of the first year. You should indicate generally what will and what will not be included; this would satisfy many and would protect Rand and HUD.

Bawden: Crane's suggestion for control assumes that no reports will be forthcoming until the end of the five years. Also, Rand should consider how the program will look if homeowners are told their properties are not certifiable unless they add a foot of counter space.

Lowry: Yes, but the case is different if we are telling them they must have indoor plumbing. The nature of the standards is important and should relate to health, safety, and decency.

Rydell: I want to make three points: (1) In resolving the debate about how gradual a start-up is optimal for the experiment, it seems to me that the dangers to the experiment of no impact (not having any response dynamics to study) are greater than the dangers of too much impact (uncomfortably high short-run price inflation). In other words, the decision on start-up time should err on the side of being more rapid than optimal, in order to magnify the treatment effect. (2) On the use of a hedonic index to measure quantity of housing services, instead of the inputs approach: The defense of the hedonic approach based on use of hedonic measures of base-year amount of capital misses a difference in the two problems. In measuring the amount of capital we are measuring a stock at a point in time. In attempting to measure changes in the quantity of housing services we would need to measure the quantity of housing services at two points in time (say, Year 0 and Year 3) and take a difference. The problem is that any measurement uncertainties get considerably magnified when two uncertain measures get differenced. My conclusion is that hedonic measures may not be good enough to produce reliable estimates of differences, even though they may well be adequate for estimating capital stock. Note that the inputs approach to measuring changes in quantity of housing services gets direct measures of additional inputs and so does not suffer from the statistical problem that the hedonic-measure approach would. (3) Finally, concerning Kain's suggestion of a control site: If there is no shock (allowance treatment) to a control site, you may not be able to measure the dynamics of the housing market because there will be no dynamic events to measure.

Aaron: If demand increases, the effect must go into price increase or quantity increase; if you say there's no effect, you mean there's no price change--but then output must have changed.

Lowry: There seems to be some consensus on the ramp approach. What is the time horizon? Rand has said one year. Is the panel saying two years?

Hayes: At least two; administrative details are so messy, you probably couldn't do it much faster anyway. Think about half of the eligibles being homeowners and the impact of this on rate of movement.

Kain: I'm not going to buy the two-year time horizon. It should be done as rapidly as possible without side effects. It is not bad to take five years, if you can show that there are no bad effects, that recipients obtained more and better housing, and that the program is cost-effective compared to other alternatives. If you could accomplish these goals with a two-year phase-in, that would be fine. What is important is to have a monitoring system and feedback controls on the rate of enrollment to minimize price inflation and other undesirable side effects.

Aaron: What would you do if after six months of enrollment (covering one-fourth of the eligibles) you find a shortage of two-bedroom apartments and people are unhappy? Do you go ahead, or can you stop enrollment until things cool off?

Mills: You may not be able to stop enrollment.

Bawden: You need a plan to slow the rate of enrollment--not by stopping it but by giving recipients money with a period of time to find standard housing.

Field: But then you have the political reporting problem.

Kain: There are two ways to slow the start. The way chosen depends on the view of the program.

Lowry: We very much appreciate your help. Thank you.

SUPPLEMENTAL MEMORANDA

(Additional memoranda submitted by the panelists upon completion of the General Design Review)

COMMENTS ON INPUT ANALYSIS IN THE SUPPLY EXPERIMENT

Henry Aaron

Assume that the following relations obtain:

$$R = PQ \ , \tag{1}$$

where R is gross rent, P is price per unit of housing services, and Q is the number of housing units consumed.

Then

$$\dot{R} = P\dot{Q} + Q\dot{P} \ , \tag{2}$$

where the dot indicates the time derivative of each variable.

Assume that

$$Q = f(K,S,N) \ , \tag{3}$$

where K is the capital stock, S is current service inputs, and N is neighborhood amenities. S is distinguished from M, or maintenance expenditures, which do not affect Q except as they affect K. Clearly, the distinction between S and M is not sharp in practice, but it will be assumed to be sharp here. For simplicity, assume that Eq. (3) is linear in the vicinity of the observed values of K, S, and N. Then Eq. (3) becomes (3'):

$$Q = a_o + a_1K + a_2S + a_3N \ . \tag{3'}$$

Taking time derivatives of Eq. (3') yields Eq. (4):

$$\dot{Q} = a_1\dot{K} + a_2\dot{S} + a_3\dot{N} \ . \tag{4}$$

Following the Rand analysis,

$$P = g(u,s,m,\pi) \ , \tag{5}$$

where u is the user cost of capital, K; s is the cost of S; m is the cost of M; and π is the producer's markup. Converting Eq. (5) to linear form and taking time derivatives yields Eqs. (5') and (6):

$$P = b_o + b_1 u + b_2 s + b_3 m + b_4 \pi \ , \tag{5'}$$

$$\dot{P} = b_1 \dot{u} + b_2 \dot{s} + b_3 \dot{m} + b_4 \dot{\pi} \ . \tag{6}$$

Presumably b_1, b_2, b_3, and b_4 are approximately equal to 1, and b_o approximately equals 0.

Denote depreciation $\dot{K}$. Then, following the Rand description and plausible assumptions,

$$\dot{K} = h(A,M,B) \ , \tag{7}$$

where A is the age of the housing unit, M is as noted, and B is the behavior of resident households and neighbors (e.g., vandalism).

Again, linearizing yields

$$\dot{K} = c_o + c_1 A + c_2 M + c_3 B \ . \tag{7'}$$

Substituting Eq. (7') into Eq. (4) yields

$$\dot{Q} = a_1 [c_o + c_1 A + c_2 M + c_3 B] + a_2 \dot{S} + a_3 \dot{N} \tag{4'}$$

$$= d_o + d_1 A + d_2 M + d_3 B + a_2 \dot{S} + a_3 \dot{N} \ .$$

I take it that V as defined in the experiment includes both M and S. If so, Eq. (4') makes Q a function of both V and $\dot{V}$. It is, therefore, not the case that $\frac{\Delta V}{V} = \frac{\Delta Q}{Q}$.

Moreover, Eq. (4') states that in order to understand $\dot{Q}$ you have to know how neighborhood amenities affect $\dot{Q}$, which raises serious problems of commensurability--how is $\dot{N}$ measured except in terms of what people pay for a neighborhood? Similarly with B.

Substituting into Eq. (2) yields

$$\dot{R} = (b_o + b_1 u + b_2 s + b_3 m + b_4 \pi)(d_o + d_1 A + d_2 M + d_3 B + a_2 \dot{S} + a_3 \dot{N}) + (a_o + a_1 K + a_2 S + a_3 N)(b_1 \dot{u} + b_2 \dot{s} + b_3 \dot{m} + b_4 \dot{\pi}) \quad (2')$$

Now, this equation is a monster because of the interaction terms, to say nothing of the problems of measuring time derivatives (viz., one base year may be unrepresentative) and of handling such imponderables as N and B.

I think it would be useful to make explicit the assumptions you have to make to get from my Eq. (2') to the corresponding equation in your research design. Do you think these assumptions are tenable? Note that I have ignored all the second-order interaction terms corresponding to your $\frac{\Delta P \Delta Q}{PQ}$ which would make Eq. (2') appalling. I have also ignored property taxes to the extent that they do not enter the user cost of capital.

COMMENTS ON THE SUPPLY EXPERIMENT

Frederick O'R. Hayes

One of the participants last Wednesday commented on the high quality of Rand's work in preparing the design for the experiment and its evaluation. I would go beyond that. The quality and detail are beyond anything I have seen preparatory to launching any experiment or program. It is all a bit overwhelming. I think this was borne out by the discussion which concentrated very heavily on peripheral issues rather than on the guts of the basic design. Most (but not all) of my comments fit this pattern. They relate, in large measure, to aspects of the experimental learning process upon which HUD placed little or no emphasis in its charge to Rand.

THE ELASTICITY QUESTION

I understand the argument for using an average of individual elasticities, but I suspect that I would also want a calculation of aggregate supply and price elasticities, if only as a yardstick against which to assess the average of the individual computations. It seemed to me that the limited discussion on this point was unsatisfactory--and there was no answer to Heinberg's argument on the problems of sample design or of weighting in calculating aggregate elasticities.

BIG-CITY NEIGHBORHOOD

I support your argument for the inclusion of a segment of a large-city ghetto in the experiment even though the inclusion of an interdependent small sector of a metropolitan housing market raises serious design problems. The basic issues are genuine supply-response issues

on which Green Bay and even Saginaw are unlikely to offer answers applicable to the large metropolitan areas of the Northeast.

I would not dismiss the Mood-Jessen Green Block proposal out of hand, but I suspect that you can learn most of what you need to know without something that complex. The key phenomenon is how landlords will react in neighborhoods characterized by the following:

1. Very high densities.
2. Large buildings (by Green Bay and Saginaw standards--say, 16 to 30 units).
3. Very substantial building obsolescence and deterioration.
4. Predominantly black low-income population.
5. High neighborhood pathology as measured by crime and delinquency, disease, fires, nonintact families, etc.

The basic question: In such neighborhoods, is the downward momentum or the inertial force against upgrading so great as to significantly dampen landlord investment response to prospective increases in cash flow from housing allowances? Will the lenders provide funds?

The ghetto neighborhoods have the highest priority, but there would also be some value in the same explanation of investment response in threatened or transitional working-class white neighborhoods.

Green Bay and Saginaw are, in fact, supply *and demand* experiments emphasizing the evaluation of supply response. In the ghetto experiment, I believe that demand could be far more constrained without significant loss of its major values.

The experiment should be testing the deterrent effect upon housing investment of three factors:

1. Neighborhood pathology (presumably at a value well above any for Green Bay or Saginaw).
2. High population density.
3. High average number of units per structure.

(Needless to say, a single limited experiment will not provide an adequate basis for separating the effects of the three factors.)

THE PROCESS

John Wilson mentioned the value in the initial Trenton income-maintenance experiment of David Kershaw's detailed reporting on the administrative aspects of the program. It may be even more important that the administrative and program process be monitored and analyzed in the housing allowance experiment.

I advocate, in any complex program, an effort before program initiation to lay out a "best-judgment" schedule or forecast of action, using PERT, CPM, or a simpler milestone format. This initial effort will almost certainly uncover some otherwise unanticipated problems in managing the experiment itself. It will definitely identify areas where additional information is needed. But most important, it sharpens the analytic perspective of the process and improves the quality of the later ex post evaluation.

This would help provide an empirical basis for the administrative design of an eventual national housing allowance program--perhaps more

valuable than the administrative experiment itself because it encompasses scale-related problems. It also would permit realistic estimates of the speed of program implementation and participation rates over time.

Note that the rate of program progress may be not unrelated to its price and quality effects. If administrative process and participant response tend to spread out the impact upon demand, the price effects would tend, other things unchanged, to be less than if the demand were concentrated. We may have John Kain's "ramp" effect not by conscious design but from unintended delays in processing and lags in both household and investor response.

All of this is obvious. I insert only the argument that the evaluation of complex administrative and program processes requires advance thinking and design almost as much as the evaluation of economic impact.

OPERATING VS. INVESTMENT COSTS

The participating landlord must meet two requirements:

1. The threshold requirement--the certification that the unit he seeks to rent is standard;
2. The demand requirement--the quality of the housing in terms of structure, facilities, services, and neighborhood is attractive to the tenant at the rent charged.

The owner-occupant must meet the first of these requirements and, implicitly, the second.

For some property owners, both conditions may be satisfied without incremental expenditures. At the other extreme, there will be housing units which cannot be certified as standard without an investment in improvements and housing which cannot be rented at R^* levels without an increase in maintenance and operating expenditures. And some will require an incremental investment in improvements over and above certification levels to be rentable at these levels.

These variations in possible response are extremely significant in the economic impact of the allowance-induced expenditures in several important respects:

1. Timing. If the principal response is the investment required to bring the housing unit up to certification standard, the major impact upon suppliers of housing inputs will have taken place before the relevant housing allowances are paid.
2. Effects of Loan Financing. If either certification or tenant demands results primarily in major investment, there is a strong likelihood of loan financing up to the limit supportable by increased rents. In the extreme case, if total housing allowance payments resulted in an identical increase in rents and housing expenses and the entire amount were used to amortize a long-term investment in new housing and housing improvements, the total investment would be about ten times the amount of the annual aggregate of housing allowance payments. This is obviously absurd, but

it indicates that loan financing might multiply early impact by smaller factors, say two to three times the allowance amount. Would this involve short-term pressure on lending resources or supply factors?

I have two concerns. The first is anticipation--understanding what is likely to happen in the experiment itself. The missing ingredient at the present time is a decision on standards and a clear notion as to the investment per unit likely to be needed to secure certification. I would favor a small survey designed to estimate the investment required to meet alternative standards criteria and the continuing maintenance and operations costs required to sustain certificate standards.

My second concern is the need for an analytic perspective that will provide the most valid basis for determining the applicability of its results to the remainder of the country. The landlord survey document and, I assume, the homeowner survey provide adequate information to do this. This means differentiation among (a) expenditures made to secure certification and other expenditures; (b) expenditures for major improvements and those for maintenance, operating, and expensable improvements; and (c) borrowing-financed and cash-financed expenditures. These will be meaningful, of course, only for different categories of properties, probably with several splits by size in the multiple-dwelling category.

Another aspect of these questions is the impact upon the start-up program. A high precertification investment, for example, would

suggest a long lead between family enrollment and dwelling-unit certification.

CONTROLS

I was initially skeptical on the idea of a control site and became increasingly negative as our discussion continued. It seems to me that the control question is best answered by statistical analysis and data gathering aimed at data and estimates on two questions:

1. What would have happened in Green Bay and Saginaw if the experiment had not taken place? The answer must be a projection based upon what was happening in Green Bay and Saginaw prior to the experiment.
2. What is happening during the experiment in the relevant and comparable part of the rest of the world? The suggestion for a limited ten-city survey is one solution but there are others, probably including some not dependent on new, direct data gathering.

Regardless of the specific form, it should be possible to produce statistical measures against which the results of the analysis and monitoring programs in Green Bay and Saginaw can be calculated.

THE FORMULA

One result of the experiment should be an evaluation of the housing allowance formula. I say this because I believe it is oversimplified, with a strong bias toward smaller households.

MODELING

Modeling is one of my consumer preferences, an insanity that I have shared with Rand. To my mind, modeling is one of the best means of divining and examining the implications of complex sequential processes. It is a substitute for control groups which are often not possible, and it is a means of thinking through problems that is often better than any other method.

I use the term broadly, and I believe useful models can often be crude, coarse-grained, and oversimplified. I would use a crude model of sorts, for example, simply to relate inmigration, average per capita income, and price level to changes in housing expenditures for housing over the last decade to suggest the likely impact of the various different parameters on housing expenditures. I would like to see housing allowances fitted into and measured against other independent variables. Similarly, administrative process modeling through milestone data is certainly no big thing--but it may make a major contribution toward understanding the lags that should be reflected in supply-response models.

COMMENTS ON THE HOUSING SUPPLY STUDY

R. J. Jessen

Here, very sketchily perhaps, are some of the impressions and ideas that occurred to me during the reading of the working reports, and during discussions in Washington the past few days.

GENERAL

To keep this as short as possible (since I'm trying to leave on vacation tomorrow) I shall not mention all the good things about the reports and the discussions. These remarks are centered on those things that bother me.

The proposed study (call it "experiment," "trial," "test," "case study," "demonstration," or whatnot, but I like to concentrate on the learning opportunities it offers) is expensive and unconventional, and the findings may have an effect on me as well as all others in the U.S., so I feel particularly concerned about its scientific quality. In fact, *validity* and *soundness* are matters that far outweigh *efficiency* in deciding where to concentrate my attention. In view of this, I think the "treatment"--that is, the housing program under consideration--should be the best candidate available and it should be administered as it would likely be administered if a national program is adopted. If bad side effects are present, the trial should reveal them. If they can be anticipated, then why not attempt to remove them now? If they are non-reasonable, then let us accept them as such. If variations in treatment (including methods of application) are of keen interest, then perhaps the overall design could be adapted to accommodate them. This may be very difficult but some thinking may do it.

The complexity of the overall study is such that many conventional approaches and procedures seem to be quite inadequate. There are many opportunities for the unconventional--both unthinkable as well as thinkable--here. The issues here are too important for the conventional alone, even where the conventional is carried out with utmost competency.

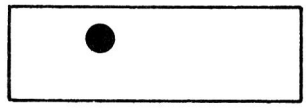
THE NUMBER AND SELECTION OF SITES

The number of sites depends on various considerations of which costs in this case seem to dominate. However, costs also depend on site definition or concept. Suppose, to illustrate principles, we are limited to one site. How is it to be chosen? Suppose the chosen site is to be "representative" of the population of sites in which we have an interest. Then we may proceed in order of sophistication as shown in Fig. 1 where factors (variables) may be size, percentage black, etc.--whatever appeals most. Most of us would feel happiest if the chosen site is "representative," that is, possesses the population "average," on every factor (characteristic) of relevance; hence we would prefer Case 4 because it has this "balance."

Suppose now we consider three sites. A possible alternative is given in Fig. 2. Here I believe most people would prefer Case 1 to Case 2. Generally, I would. To apply these simple principles in the present case we could classify all 3,000 counties in the U.S. into a three-factor, three-level cube where the MOS (measure of size, e.g., counties, could be classified by, say, total "HHs," or total "poverty families," etc.) such that each factor level cuts off terciles of MOS rather than numbers of counties. Anyway, approaches like this could be considered where rural as well as metropolitan counties are considered

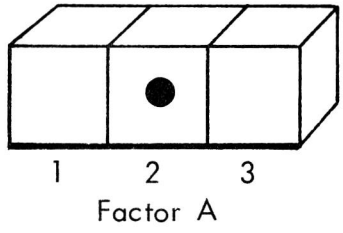
CASE 1

Classification by one factor, 0-level: select site at random.



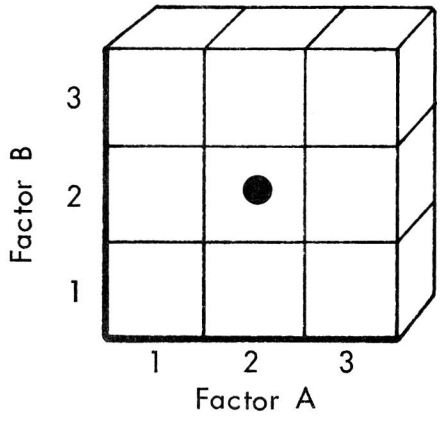
CASE 2

Classification by one factor, three levels: select site falling in middle level.



CASE 3

Classification by two factors, three levels each: select site falling in middle level of each factor.



CASE 4

Classification by three factors, three levels each: select site falling in middle levels of all factors.

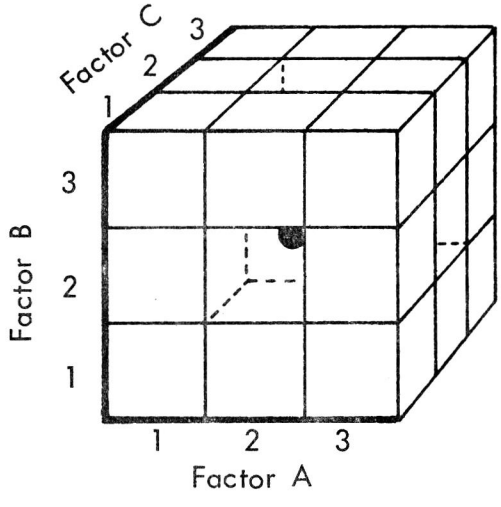
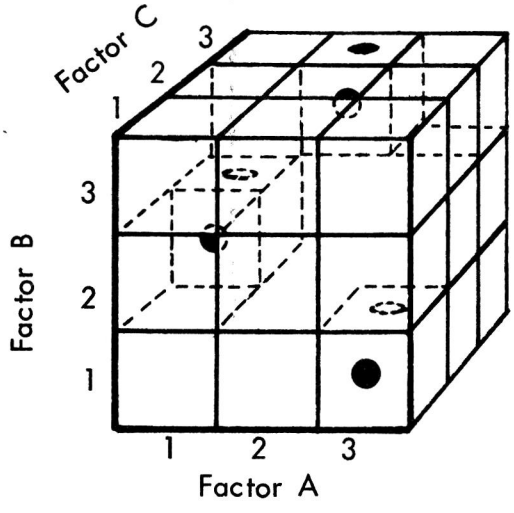


Fig. 1 — Choosing a single "representative" site: Four classification methods with appropriate selection strategies

CASE 1

Sites spread to represent a spectrum of counties.



CASE 2

Sites clustered to represent an "average" county

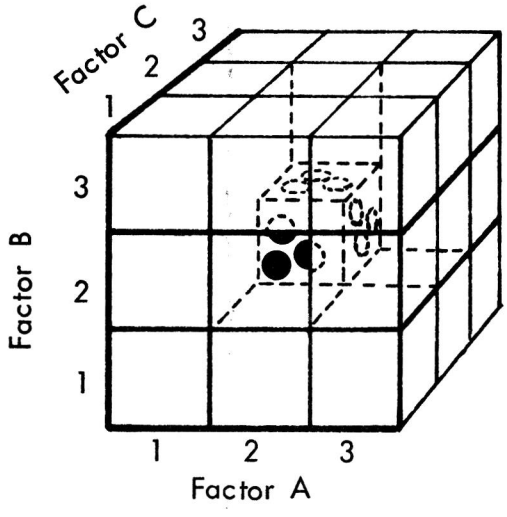


Fig. 2 — Choosing three experimental sites: Alternative selection strategies for a 3-factor, 3-level classification of U.S. counties

and where the classifying factors might be degree of urbanity, population of county, percentage black, etc. (Actually four or more factors can be used even where $n = 3$.)

Any three counties (metropolitan or whatever) chosen to be "representative" of conditions in the U.S. would require heroic efforts to justify. But in the case of Saginaw and Green Bay, it will take superheroic efforts.

In order to accommodate more than two sites, an alteration of site concept would be helpful to cut costs. Alex Mood and I propose seriously the checkerboard scheme presented at the meeting and briefly described below.

REDUCTION OF SITES BY SAMPLING (THE GREEN-BLOCK SCHEME)

The case has been made that big counties (metros) cannot be accommodated because of heavy costs. The case for large metros is that that is where serious housing problems exist and that they are different in nature from those of smaller population concentrations. To deal with this problem we suggest that a large metro be selected (e.g., Chicago, Detroit, New York, etc.) and cut into a number of blocks, tracts, neighborhoods, or areas of some sort. A sample of those areas will be selected (e.g., by multiple stratification) such that they contain what is regarded as a suitable number of HHs (e.g., 50,000 HHs). These areas will represent all important aspects of the metro in a microcosm. They as a group would be Green Block, the "experimental city." The size of the experimental city can be scaled up or down to meet expected costs and problems of simulating the treatment (program) properly. There would be special problems of public relations, but at the moment nothing here appears to be insurmountable.

If only two sites are taken, we propose Saginaw and Green Block. I believe for the same budget we could take three sites by scaling down the cost of Green Block and including a "rural" county, too.

SELECTION OF PARCELS (WHENCE LANDLORDS)

Here I suggest that all parcels in the sites be classified by number of housing units (3 levels), tenure (2 levels), assessed value per housing unit (3 levels), and zone, that is, rural versus urban (2 levels), resulting in a 3 x 3 x 2 x 2 cell frame as an improvement over the current somewhat disjointed scheme. Actually, another zone--say, "suburban"--could be created in a manner to have some meaning on housing practices. This would improve sampling precision whether there is any demand for it as an analytical unit or not.

Allocation of sampling resources to the cells might be improved by taking a hard look at how analysis might be done. Here I would make no hard ties between this frame for sampling (where cells are "strata") and another frame for analysis (where cells may be called "domains"). For example, the strata cells are based on nonsurvey data. The domains may be defined on survey data where one is interested in actual rent levels rather than "imputed," etc.

Updating

New parcels (subdivisions of old parcels) can be identified in the sample parcels, but it may be wise to keep a watch on new housing developments, etc., in the site. This information can be used as a second sampling frame for which samples can be drawn. Information on new developments and subdivisions may be available from the U.S. Bureau of

the Census (in its CPS program). The technique of sampling that is useful here is known sometimes as "multiple frame" sampling.

Multiple-Parcel Landlords

I would guess that if there are very many of these, including them with all their parcels will lead to increased accuracy. (Weights must be employed, however.)

Use of Census Data

Alternative procedures, mostly conventional, could be used to select the sample of parcels using Census block data (or value, color, etc.). Or the data could be used in stratification of parcels within the 36 cells. It appears that something could be done to improve things here, but I have not explored them.

SELECTION OF HOUSING UNITS

Since this area doesn't appear to be likely to offer problems, I have not explored it much. There are some problems in using appropriate stratification methods in large structures, but they are of relatively minor importance. Methods of dealing with contraction and expansion of housing units in multiple-unit structures may offer a minor problem.

ERRORS OF RESPONSE AND OF NONRESPONSE

These are always-present problems of any survey. I presume that at least the usual precautions are taken to minimize errors of response and to employ some measures to detect and measure them.

Nonresponse seems to be a possible serious problem. The "payment-you-can't-refuse" proposed for landlords seems worth exploring. For further ideas, see the next section.

ESTIMATION PROBLEMS

During the meetings, I suggested that the nature of estimating price elasticity be looked into and possibly some estimators like

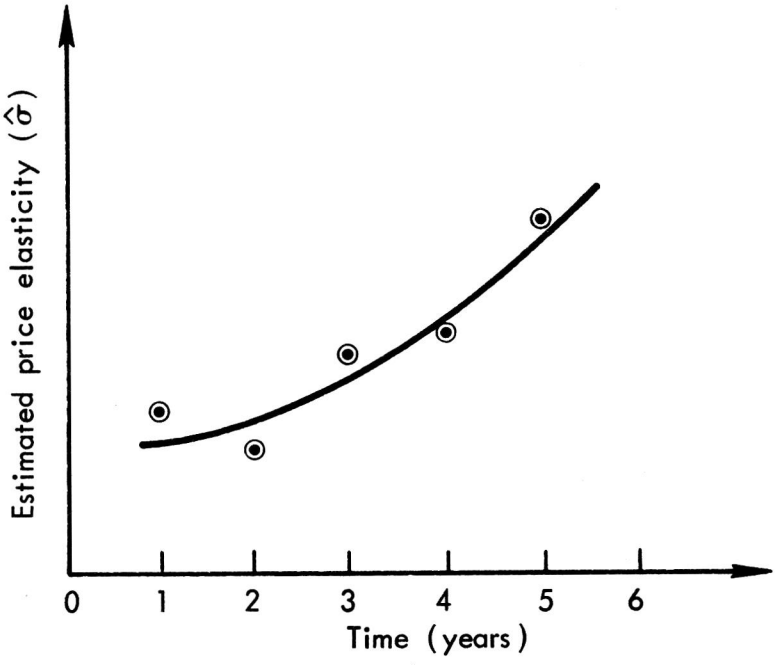
$$\hat{\sigma} = \frac{\Sigma \Delta P/P}{\Sigma \Delta Q/Q}$$

would be preferable to, say,

$$\hat{\sigma} = \frac{1}{n} \Sigma \frac{\Delta P/P}{\Delta Q/Q} \quad .$$

Perhaps neither is appropriate; I don't know.

If estimates are to be made for each year, say, like



then one may wish to estimate σ for each time interval. In this case one might wish to sample the landlords on a somewhat different basis than every year. A possible approach here is to take a very large sample in

Year 0 (perhaps even a census) and then take a *different* sample of landlords (parcels) each year to determine ΔP and ΔQ. In this case only two contacts are required and nonresponse may be comfortably small. Of course this presumes that ΔP and ΔQ are measurable by two instants in time. Having the same panel of landlords in this scheme is certainly okay but is not necessary.

If inputs are required for each year in the interval, then this scheme is no good.

Another possibility is to estimate the ΔPs by taking one or more random intervals of the five over the period. If these are taken at random, each landlord's ΔP (over the five-year interval) will be estimated unbiasedly and $\hat{\sigma}$ will be estimated essentially unbiasedly.

Another possibility is a rotation scheme of bringing in landlords and dropping them in a planned scheme. This would be appropriate if one wanted to get the best estimates of the σ for each one-year period during the five-year run.

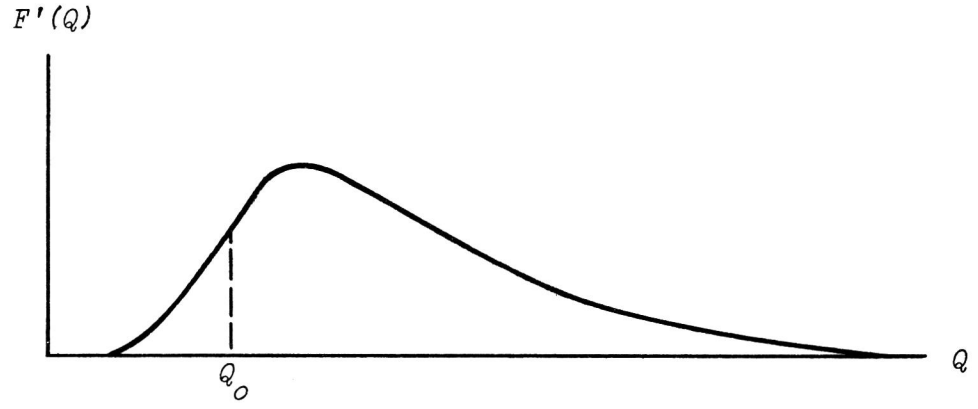
Or a combination could be considered.

DESIGN FOR EXTRAPOLATION

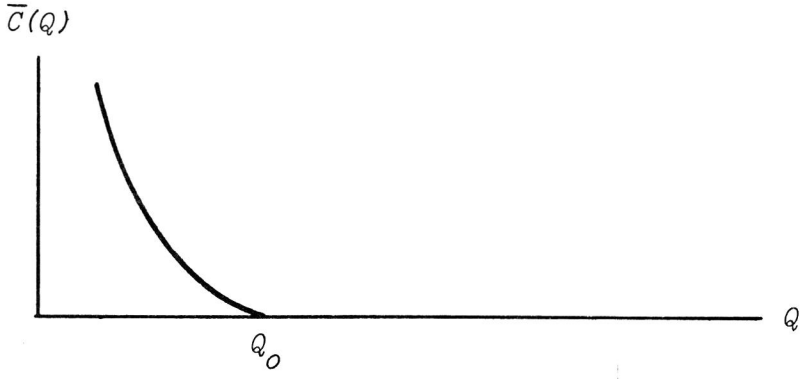
Alexander M. Mood and Raymond J. Jessen

We do not know how Rand plans to extrapolate from the experimental data to the nation as a whole, but we presume that a reasonable procedure might focus on the parameters that surround the decision of the individual landlord and would depend on the statistical distributions associated with those parameters. For illustration, we shall outline a simple extrapolation process which any of us could improve given a little time to think about it.

In the first place we think of a statistical distribution of units with respect to quality with some proportion, $F(Q_o)$, being below an acceptable standard, Q_o:



For our present purpose we shall assume that any given structure falls entirely on one side or the other of Q_o; there will be exceptions in the real world, and it will be no problem to handle them by minor modification of the extrapolation we are describing. For any $Q < Q_o$ there will be a distribution of costs $C(Q)$ of bringing the units of that quality up to standard, and the averages $\overline{C}(Q)$ of those distributions might plot as shown below.



The cost for any structure will consist of two parts, C_c and C_u, the first being associated with the grounds and common areas of the structure and the second with the individual unit. These two parts will doubtless be highly correlated, but we shall need their joint distribution.

Now let's turn to some calculations that the owner of a substandard structure would make at the onset of a housing allowance program. Suppose his structure contains N units, with N_1 of them occupied by households eligible for the allowance.

Assuming that he would bring all units up to standard (modifications for other cases are obvious), this cost would be

$$D = C_c + NC_u - \frac{N_1 r_1 + N_2 r_2}{i} ,$$

where D = net cost associated with improvements;

C_c = improvement costs associated with common areas;

C_u = improvement costs associated with individual housing units;

N_1 = number of units occupied by eligible tenants;

N_2 = number of units occupied by ineligible tenants;

$N = N_1 + N_2$;

r_1 = average additional revenue from eligibles;

r_2 = average additional revenue from ineligibles; and

i = a discount rate for capitalizing the additional revenue.

Very crudely, the cost of not bringing the structure up to standard would be

$$E = N_1 C_r + N_1 R f + \frac{N r_3}{i} ,$$

where E = net cost associated with no improvements;

C_r = immediate cost of a vacancy (cleaning, painting, repairs);

R = annual rent per unit;

f = average fraction of a year that a vacant unit must wait to find a tenant; and

r_3 = average amount that rents must be reduced in order that the structure can compete in a diminished market.

This formula exaggerates E by assuming that all eligibles move out at once, whereas in reality their departures will be scattered out over time. Some owners may also weigh the possibility of converting the property to a distinctly higher residential class or to another use, but that will be a realistic option for a relatively small minority and we will forget about it for the present purpose except to denote the cost of conversion to the next best nonresidential use by A and conversion to distinctly higher residential use by B.

Let's extrapolate to the nation from the experimental sites by the following simple procedure: (1) Use the data to fit four probabilities

(which sum to unity) as functions of A, B, D, E, Q', i.e.,

$p_1(A,B,D,E)$ of improving a substandard structure to Q_o;

$p_2(A,B,D,E)$ of not improving it but keeping it in residential use;

$p_3(A,B,D,E)$ of taking it out of residential use; and

$p_4(A,B,D,E,Q')$ of improving it to Q' above Q_o.

In a fancier model, some other variables, e.g., ethnicity of households, might be added to A, B, D, E, Q'. (2) Survey samples of properties in a sample of cities to obtain information about the statistical distribution of Q and of A, B, D, E in the nation. (3) Apply the probabilities to the distribution of A, B, D, E to estimate the new nationwide distribution of Q as a result of the allowance program. The big leap in this procedure is, of course, the assumption that the $p(A,B,D,E,Q')$ determined from a study of one or two locations are appropriate for the rest of the nation. It can be argued with at least some conviction that this is not a wild assumption because it mainly implies that reasonable financial judgment cannot be very different at different locations.

Now let us turn to what appears to us to be the essential difference between doing the experiment in a small city, S, as opposed to a checkerboard slice, L, of a large city. The difference is that the owners' decisions will be accelerated in L and hence that equilibrium will be reached more quickly in L. The reason that owners of substandard units must act more quickly is that their eligible tenants can move more quickly; those tenants will move into existing vacancies with

$Q > Q_o$ in L thus causing demand pressure which, however, cannot build up because it will be dissipated into areas of the city not in L. Thus nonrecipients in L will tend to move out of L and make room for recipients in L. In S, on the other hand, there is no way for the demand pressure on units with $Q > Q_o$ to be dissipated and there will be little opportunity for eligibles to move; hence owners of substandard units can be more deliberate about making their decisions and about implementing them.

There are other advantages of L over S: (1) There will not be excessive demand in L on contractors and suppliers of services to landlords, hence the decisions of owners can be implemented at normal costs. (2) In S the demand pressures will initially exert great price pressure on units having Q at and not greatly above Q_o. The long-run market adjustment will bring those inflated prices back down, but the early and intermediate data from S will be quite misleading whereas the early and intermediate data from L will be quite useful because owners will have made their decisions early and because the market will not have been subjected to the price distortion. (3) A control site is hard to find for S; L has a natural control site in the remainder of the city of which it is a slice. (4) Finally, and most important, experimental data gathered in L will be representative of the heart of the housing problem which lies mainly in the cores of our large cities. Extrapolation from such a set of data will generate considerably more confidence than will extrapolation from a set obtained in S.

What about the fact that the experiment in L will tell us nothing about the initial price surge that a nationwide allowance program would bring about? The answer is that it would be stupid for the nation to institute a program that would cause such a surge. A sensible program

would avoid the price surge by some device such as moving Q_o up in increments over a period of several years.

To summarize, we would characterize the experiment as hanging everything on a single sledge-hammer blow which will hopefully correctly integrate the various forces which influence suppliers of housing. We would like to see the experiment also try to isolate those forces and measure their effects on suppliers. To this end we would prefer to see the experimental funds divided into two parts with the larger part used to carry out a few experiments, much as Rand has devised, in slices of a few cities. The smaller part would be used to survey samples of parcels (and their owners) in a sizable number of cities in order to estimate the statistical distributions of Q, A, B, D, and E in the U.S.

COMMENTS ON *GENERAL DESIGN REPORT: FIRST DRAFT*

Richard F. Muth

Because I will be unable to deliver my summary comments in person on Wednesday afternoon, I have been asked by Jack Lowry to write them out. Regardless of their mode of presentation, by themselves these comments might seem wholly critical of the experimental design. Let me then try to correct this wholly erroneous impression. On the whole I find the *Design Report* a fine one, and I am quite impressed with the care and professional expertise that went into its preparation. I would hope that any suggestions of mine, except perhaps on the question of measuring the price component of rental increases, would make, at most, a small incremental improvement to an already very fine design.

As I stated Monday afternoon, I have serious misgivings over the proposed measurement of housing output by inputs in order to remove the quantity-change component from rental change to isolate the pure price-change effect. Therefore, I would strongly urge that quantity change also be estimated directly by using so-called hedonic indexes. By the latter I mean regression estimates of dwelling-unit rentals as a function of the characteristics of the dwellings themselves and the "neighborhoods" in which they are located. There are at least three broad classes of reasons for this suggestion. As a professional I feel that the problems of estimating output change this way are less severe than those of indirect measurement through measuring factor inputs. Most of my comments below will be directed to this first point.

The hedonic index would also, I feel, be more believable to non-professionals. Though I am not at all expert on survey problems, it

would seem to me that nonresponse of tenants would provide a much lower potential for disaster than would nonresponse by landlords. I am not, however, arguing that estimation of input changes shouldn't be attempted. This estimation would provide very important information for understanding sources of inelastic supply response. It would also provide very valuable data for measuring housing production functions, to say nothing of providing an alternative means of measuring output changes.

It is my understanding that baseline capital values are to be estimated by regression analyses in much the same manner that rental values of dwellings would be. Consequently, almost any criticism that might be made of hedonic rental equations applies equally to the proposed baseline estimates of capital values. Two criticisms have been made explicitly about hedonic rental indexes. First it is argued that rental values depend upon a whole host of different factors, some of which are difficult to measure in a cardinal sense. If true, the criticism applies equally to baseline capital-value estimates, since the latter are merely appropriately discounted values of rentals. The same whole list of variables necessary to explain rental values would be necessary to explain capital values; explaining the latter would also require taking appropriate account of any factors leading to differences in discount rates and other cost differences among properties. Difficulty of cardinal measurement can be avoided, though at a cost in degrees of freedom used up in estimation, by using dummy variables for attributes. This would have to be done also in explaining baseline capital values and could be more of a problem here, for reasons stated below.

The other major objection to hedonic rental indexes is that the appropriate weights for different housing characteristics might change as a result of the experiment. To suggest a possible example, the rental value attributable to a dwelling's meeting the housing standard necessary for allowance payments to be made could well rise relative to the rental value attributable to an additional bedroom or 100 square feet of floor space. If so, however, the discounted future stream of rental payments, or capital values, associated with different kinds of dwellings would change. This merely reflects the fact that housing services and the residential real estate which produce them, like scotch and wheat, are not literally homogeneous commodities. They are, rather, convenient abstractions from a complicated reality which are useful for analytical purposes. But measuring them involves index number problems. To deal adequately with the latter, one would need indexes using both base and current period weights, whether these were indexes of housing services or capital assets themselves.

There is one factor, however, which makes estimation of hedonic indexes of housing rentals easier than hedonic indexes of residential real-estate values. Market data on rental rates, though possessing some problems, are always available for rental properties. Market data on values are only available at infrequent intervals when properties are sold. In a city the size of Green Bay, there might well be very few sales of certain kinds of rental properties in any time interval. If this were the case, estimation of capital values via regression analysis would be much more difficult than estimation of the corresponding rental values if it were necessary to include a larger number of

attribute variables in the regression equation. Yet in measuring output via factor inputs, I would judge that about seven-tenths of the value of factor inputs is associated with capital stocks.

Estimating output by factor inputs not only involves as severe measurement problems as direct measurement of output but has several other problems associated with it. Output is related to factor input either via approximations valid for "small" changes or via what are called production functions. I regard the General Design's proposed measurement as essentially a first-order approximation. Where

p = price of housing services,

x = output of housing services,

p_i = price of the i^{th} factor service,

a_i = quantity of the i^{th} factor input,

and the production function is

$$x = x(a_1, \ldots, a_n) ,$$

linear homogeneity of the last implies $px = \sum_{i=1}^{n} p_i a_i$ and $p'x' = \sum_{i=1}^{n} p_i' a_i'$ (as in the General Design, primed variables refer to current-year values, unprimed ones to base-year values). Deflating current-year factor expenditures by factor-price indexes and summing yields

$$\sum_{i=1}^{n} \left[\frac{p_i' a_i'}{(p_i'/p_i)} \right] = \sum_{i=1}^{n} p_i a_i' .$$

Subtracting out base-year expenditures then yields $\sum_{i=1}^{n} p_i (a_i' - a_i)$. This is, essentially, a first-order approximation to pdx provided the marginal product of the i^{th} factor $x_i = p_i/p$. As such, it is valid whether returns to scale are decreasing, constant, or increasing.

But, does $px' = \sum_{i=1}^{n} p_i a_i'$, as the General Design claims? I think not. Linear homogeneity implies $x' = \sum_{i=1}^{n} x_i' a_i'$, so if the General Design's assertion were true,

$$x' = \sum_{i=1}^{n} \left(\frac{p_i}{p}\right) a_i' = \sum_{i=1}^{n} x_i a_i' = \sum_{i=1}^{n} x_i' a_i' .$$

That is, the sum of current-year factor inputs in physical terms would be the same when weighted by base-year as by current-year marginal products. Though I can't say this couldn't happen--obviously it could if relative prices remained the same in the current year as in the base year--I would find a demonstration that it is always true as shocking as a demonstration that the world is flat.

Thus, I would view the General Design's output measurement as nothing but a first-order approximation. A somewhat better approximation based upon market data alone can be developed from a second-order Taylor's expansion of x about base-year values.* It is

$$\Delta x \simeq \left(1 - \frac{1}{2}\frac{dp}{p}\right) \sum_{i=1}^{n} \left(\frac{p_i + \frac{1}{2} dp_i}{p}\right) da_i .$$

Since $\left(1 - \frac{1}{2}\frac{dp}{p}\right) \simeq \left(1 + \frac{1}{2}\frac{dp}{p}\right)^{-1}$, the above merely suggests inserting the arithmetic mean of base- and current-year prices for base-year prices in the first-order approximation. Doing so, however, requires knowing dp, for which Δx is wanted.

The only alternatives to such approximations are using housing production functions themselves to relate inputs to output. Rather

*See pp. 158-159 for derivation.

little is known about them. Enough is known, however, that neither $x = \left(\sum_{i=1}^{n} a_i \right)^{\gamma}$ nor $x = \prod_{i=1}^{n} a_i^{\alpha_i}$, which the General Design suggests, are very good approximations for housing. Indeed, I would argue that partial-substitution elasticities are probably smaller than unity and differ from each other. To estimate such substitution elasticities would require data on inputs in situations where differences in relative factor prices exist. Such differences aren't likely to occur in a single cross section in a city like Green Bay except, perhaps, for land rentals. Consequently, one would either need observations for a number of time periods or would need to use outside data as well to estimate these production functions. Either alternative suggests difficulties. Though I would hope the experiment could generate data useful for estimating housing production functions, I'd hate to have to pin understanding the experiment's outcome on these hopes.

The other major problem in relating output to factor inputs is in proper measurement of the appropriate price of capital services. From a model of maximization over time, either of utilities for owner-occupants or incomes of landlords, one finds that the price of capital services to which owners or landlords adjust is

$$R(t) = \left[k(t) + r(t) \left\{ 1 + \frac{t\dot{r}(t)}{r(t)} \right\} - \frac{\dot{P}(t)}{P(t)} \right] P(t) ,$$

where $P(t)$ = capital-asset price;

$k(t)$ = the depreciation rate, assumed a constant proportion of capital stock;

$r(t)$ = the discount rate;

t = time;

dots represent time derivatives; and taxes are neglected. As in the General Design's formulation, capital-service prices depend upon depreciation and interest charges. They also depend upon capital gains or losses, which are zero only if asset prices are unchanging. Yet it is anticipated that the experiment will cause asset prices to change over time. One is almost in the position that to measure the price of capital services correctly, one needs to know what the experiment is seeking to measure. Furthermore, interest changes are properly represented by current discount rates only if these are unchanging over time. Only under stationary conditions is

$$R(t) = \left[k(t) + r(t)\right]P(t) \ .$$

A further question arises as to whether current mortgage interest rates are the same as the rates at which housing producers discount the future. A plausible case can be made for the proposition that they are for corporations, which can issue equity as well as debt instruments. For unincorporated producers, however, who are certainly an important part of all housing producers, the making of a larger mortgage can't be accompanied by issuing more equities. Hence, larger mortgages are riskier to lenders and carry higher interest rates. For the unincorporated producer, the appropriate discount rate may thus be the marginal rather than average rate of interest on borrowed funds.

Finally, the General Design provides a plausible depreciation analysis--one with some but not much empirical support. I find the hypothesis that residential structures depreciate at a constant relative rate over time to be equally tenable, however. Regardless, relatively little is know about depreciation, yet it accounts for

perhaps 20 percent of the cost of housing services. For all of these reasons, appropriate measurement of the price of capital services, I feel, is far more difficult than the General Design suggests.

To sum up this rather long comment, which some may think a diatribe, it seems to me that measuring output via factor inputs involves all the problems that direct measurement via hedonic rental indexes does and some very sticky ones of its own. These latter include expressing changes in output as an appropriate function of changes in factor inputs and proper measurement of the price of capital services. For these reasons, I strongly recommend that measurement of the output component of dwelling-rental changes be based primarily upon direct rather than indirect measurement.

SECOND-ORDER APPROXIMATION FOR MEASURING OUTPUT

$$x = f(a_1, \ldots, a_n),$$

$$f_i = \frac{p_i}{p}, \quad df_i = \sum_{j=1}^{n} f_{ij} da_j = \frac{dp_i}{p} - \frac{p_i}{p}\frac{dp}{p}$$

$$\Delta x \simeq \sum_{i=1}^{n} f_i da_i + \frac{1}{2} \sum_{i,j=1}^{n} f_{ij} da_i da_j$$

$$= \sum_{i=1}^{n} \frac{p_i}{p} da_i + \frac{1}{2} \sum_{i=1}^{n} \left(\frac{dp_i}{p} - \frac{p_i}{p}\frac{dp}{p} \right) da_i$$

$$= \left(1 - \frac{1}{2}\frac{dp}{p} \right) \sum_{i=1}^{n} \left(\frac{p_i + \frac{1}{2} dp_i}{p} \right) da_i + \frac{1}{4}\frac{dp}{p} \sum_{i=1}^{n} \frac{dp_i da_i}{p} .$$

Thus, neglecting the third-order term,

$$\Delta x \simeq \left(1 - \frac{1}{2}\frac{dp}{p}\right) \sum_{i=1}^{n} \left(\frac{p_i + \frac{1}{2}dp_i}{p}\right) da_i \ .$$

To a first-order approximation

$$\left(1 - \frac{1}{2}\frac{dp}{p}\right) \simeq \left(1 + \frac{1}{2}\frac{dp}{p}\right)^{-1} ,$$

so the above suggests using the mean of base- and current-year prices rather than base-year prices for weighting input changes to obtain output changes (this may require iteration with dp).

C.E.S. PRODUCTION FUNCTION

$$X = [\alpha_1 A_1^{-\beta} + \alpha_2 A_2^{-\beta}]^{-1/\beta} \ , \ \sigma = 1/(1+\beta) \ ,$$

$$X_{A_1} = \left(-\frac{1}{\beta}\left[\alpha_1 A_1^{-\beta} + \alpha_2 A_x^{-\beta}\right]^{-1/\beta-1}\right)\left(-\beta\alpha_1 A_1^{-\beta-1}\right) ,$$

$$= \alpha_1 X^{(1+\beta)} A_1^{-(1+\beta)} = p_1/p \ ,$$

$$\frac{X}{A_1} = \alpha_1^{-\sigma} (p_1/p)^{\sigma} \ ,$$

and similarly for A_2 ;

$$p = p_1 \frac{A_1}{X} + p_2 \frac{A_2}{X}$$

$$= p_1 \alpha_1^{\sigma} (p_1/p)^{-\sigma} + p_2 \alpha_2^{\sigma} (p_2/p)^{-\sigma} .$$

Thus, $$p = \left[\alpha_1^{\sigma} p_1^{1-\sigma} + \alpha_2^{\sigma} p_2^{1-\sigma}\right]^{\left(\frac{1}{1-\sigma}\right)} .$$

Estimating α's and σ:

$$ln\left(\frac{p_1 A_1}{pX}\right) = \sigma\ ln\ \alpha_1 + (1 - \sigma)\ ln\left(\frac{p_1}{p}\right) ,$$

$$ln\left(\frac{p_1 A_1}{p_2 A_2}\right) = \sigma\ ln\ (\alpha_1/\alpha_2) + (1 - \sigma)\ ln\ (p_1/p_2) .$$

If we use the latter because we don't know p, then we normalize by taking $p = 1$. That is, use

$$1 = \left[\left(\frac{\alpha_1}{\alpha_2}\right)^{\sigma} p_1^{1-\sigma} + p_2^{1-\sigma}\right] \alpha_2^{\sigma}$$

to solve for α_2, hence α_1.

COMMENTS ON THE HOUSING ASSISTANCE SUPPLY EXPERIMENT

John Oliver Wilson

POLICY ISSUES AND RESEARCH OBJECTIVES

To what extent will the results from the housing experiment enable Congress and the Executive to make a better policy decision regarding the adoption of a national housing allowance program? Assume that both the Demand and Supply Experiments achieve internal and external validity, then the question is, To what extent can we use the results of the experiment to assist us in making policy decisions? This will depend upon at least the following three issues:

1. The ability to integrate the Demand and Supply Experiments so that information concerning the effects of a national program can be answered.
2. The extent to which the supply-response results for the 16 stratified cells of residential properties in the Supply Experiment can be used in national projections.
3. The existence of experimental treatments that are not totally dissimilar to those that might be recommended in a national program.

If housing allowances are proposed as a public policy, some of the questions that HUD could well be asked are:

1. What would be the cost of a national housing allowance program for alternative levels of payments?
2. What proportion of the total eligible population will participate in the first year, the second year, and the equilibrium

year when full knowledge and acceptance of the program have occurred?

3. What proportion of the total cost of the program will go for increased housing services as opposed to higher prices?
4. Would this program cost less per unit of housing service than the existing HUD housing programs?

I have some concern that at the conclusion of these two [Demand and Supply] experiments we will not be able to answer such questions as those given above. There are two experiments, each being run by different parties, and the most important policy questions can only be answered by integrating the results of both.

My concern is heightened when I read the following statements in the Design Report:

> ... 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 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 [p. 291].
>
> 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 [p. 305].
>
> 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 [p. 307].

After having spent $100 million to $150 million over a ten-year period, I doubt that Congress will be pleased if HUD officials say that they cannot really answer the questions listed above. We cannot expect Congress to do our analytical work for us. We must be willing to consider the use of the results in assessing the expected impact of a national housing allowance program. This will require a much greater effort at integrating the two than is evident in Appendix E of the Design Report.

I would strongly urge the following:

1. That HUD assign the responsibility for integrating the two experiments to one of the two parties.
2. That the lead party be given, and assume, the power to ensure that both experiments can be effectively and easily integrated.
3. That a great deal more attention be given to the problems of integrating the two than has been evidenced to date.
4. That the lead party be responsible for making estimates of a national program under various alternative formulas for payments. (This will ensure that the experiments actually consider these issues and do not leave the final analysis to Congress or HUD.)

Once having obtained supply-response data for the 16 stratified cells in the Supply Experiment, are national data available against

which these experimental results could be applied? To what extent can we generalize from the results of the experiment?

On p. 56 of the Design Report, the question is raised as to whether homeowners should receive (a) an allowance that is equal to or less than actual homeownership payments plus utility payments plus a maintenance allowance (HUD's position) or (b) the housing allowance, irrespective of its relationship to housing costs (the Rand position).

The reasons for allowing homeowners to accumulate equity from the program, as they would if the home were owned in fee simple, is to maintain some idea of "integrity" in the experimental design. That is, we do not want to differentiate between a homeowner who has paid off his mortgage and one who has not. If we did, there would presumably be an incentive in the program for the homeowner in fee simple to sell his house, buy another house of presumably higher quality, and assume a higher mortgage. He would thus receive the entire housing allowance.

It would seem to me that this position is not as important as the reasons for limiting the housing allowance to actual expenses of maintaining a residence: (1) The program is basically a housing program. By allowing equity to accumulate, you are really approaching an income-maintenance program. I would think that a national program would have to be limited to actual housing costs, for political reasons alone. It would be difficult to explain to the American public why a private homeowner should be allowed to accumulate capital at the taxpayers' expense. But politics aside, I think that the objective of the program warrants this restriction. (2) It would become a much more difficult program to administer if for every homeowner in fee simple we had to set up the proposed elaborate mechanism for holding his equity. What

would be the process in a national program? In the experiment, we propose to refund the equity at the conclusion of the experiment. What would happen to the equity in a national program? This would either have to be returned to the government--in which case there is no need to give it in the first place--or given directly to the homeowner as an income transfer--in which case we have created both a housing allowance program and an income-transfer program. (3) It can be argued that although by restricting the allowance to actual housing expenditures we would be creating an incentive for a homeowner to sell and buy another house, this incentive is not different from that for a renter or homeowner with a mortgage. The incentive of the entire program is to upgrade the quality of housing more effectively than is currently being accomplished through various conditional grant programs.

Therefore, based on the evidence thus far presented, I tend to favor restricting the housing allowance to actual costs incurred by the homeowner.

CONTINGENCIES AFFECTING EXPERIMENTAL RESULTS

Compensating for Landlord Nonresponse

Working Note WN-8268-HUD* outlines quite clearly the various statistical methods that will be used. I can find no fault with these techniques; but I would like to see some discussion of the approach that will be taken in the field to reduce the possibility of a catastrophic event. For example:

---

*Adele P. Massell, *Compensating for Landlord Nonresponse in the Housing Assistance Supply Experiment*, The Rand Corporation, WN-8268-HUD, June 1973.

1. To what extent will individual Congressmen and Senators from the areas where the experiment is to be undertaken be included in gaining local public support? You could plan briefings for these leaders and then get them to send letters or correspond directly to local property owners.
2. To what extent will local public officials be actively used in eliciting support?
3. How do you plan to introduce the experiment to local property owners?

Has any consideration been given to paying the property owners for completing the yearly forms? There are to be 1,600 rental properties in the experiment and it is estimated that complete five-year records will be obtained for 45 percent of these. Assume that 75 percent of the total possible interviews (1,600 × 5) are actually completed during the five-year period. (The information in the report is insufficient to calculate this, but it is a relatively simple matter.) Then a total of 6,000 separate interviews will be conducted after the baseline interview. If you paid the property owners $100 to complete the questionnaire, the total cost would be $600,000. This might be cheap insurance for a program that will cost between $100 million and $150 million to conduct.

In what form do you propose to collect data in making estimates of property-owner expenditures based on tenant answers? On p. 54 of the Design Report, you state that yes and no answers can be obtained for various types of property improvements, but you then point out the

problems with this type of data. You then suggest that you can apply average cost figures to such improvements. Do you intend to do this?

What Are the Plans for Releasing Analyses of the Experiment?

Suppose that two years after the experiment is under way, a national housing voucher program is introduced into Congress. HUD or the GAO requests that you analyze the results to date. What is your answer? Do you have a schedule of proposed analyses, what they might contain, and when they might be released to the public?

Has any thought been given to the possibility that the GAO may request all of the data? They did so with the New Jersey Income Maintenance Experiment and Performance Contracting in Education from OEO. I just noticed that the GAO is seeking subpoena power from Congress. This gives them the real tool to get data.

What Are the Plans for Dealing with the Press?

To what extent will the experiment be publicized in the areas where it is to be undertaken? What are your plans to present to the press an unbiased view of the experiment? How do you intend to protect individual participants from press interviews, TV appearances, etc.?

I guess I am really asking what nonstatistical efforts will be undertaken to minimize the risk of a catastrophic event. I think it may be worthwhile to give some thought to these.

Rand Corporation.
Proceedings of the general design review of the Housing Assistance Supply Experiment

DATE	ISSUED TO
1/19/19	E. Olsen 8136/5/59001
	1/9/79
10/12-84	ILL to GAO

U.S.DEPARTMENT OF HOUSING AND URBAN DEVELOPMENT
LIBRARY BOOK CARD
HUD—750 (5—70)

DATE	ISSUED TO