Mincome Manitoba

Manitoba Basic Annual Income Experiment

An Evaluation of the Experimental Sample of Mincome Manitoba

Mordecai Kurz

Technical Report No. 5

1979

AN EVALUATION OF THE EXPERIMENTAL SAMPLE OF MINCOME MANITOBA

by

Mordecai Kurz

Stanford University

April, 1977

FOREWORD

The Manitoba Basic Annual Income Experiment is designed to evaluate the economic and social consequences of a guaranteed annual income program based on the concept of negative income tax. Of particular research interest is the labour supply response of individuals and families containing non-aged, able-bodied members. The Experiment is a jointly-funded project of the governments of Canada and Manitoba and was collectively designed by researchers and officials of Mincome Manitoba, the Department of Health and Social Development, Manitoba, and the Policy Research and Long Range Planning Branch of the Department of National Health and Welfare, Ottawa. Mincome Manitoba is the agency established to administer the project and is solely responsible for all experimental operations. Seventy-five percent of the cost of the Experiment is funded by the Government of Canada; twenty-five percent is funded by the Province of Manitoba.

AN EVALUATION OF THE EXPERIMENTAL SAMPLE OF MINCOME MANITOBA

by

Mordecai Kurz*
Stanford University

I. INTRODUCTION

The merits and potential scientific utility of the data base generated by the Manitoba Basic Annual Income Experiment (Mincome Manitoba) is a topic of major concern. The purpose of this report[1] is to provide an analysis of the development of the Mincome sample, the problems encountered, and the methods employed to solve them. The ultimate objective, however, is to arrive at some conclusions regarding the scientific merit of the data at hand and the problems which may be encountered by future researchers and policy makers who may wish to employ this information. The data base is expected to be used in the study of the characteristics of Canada's labor markets and their implications for the formulation of national policies in labor markets in general and welfare reform in particular.

---

* In preparing this paper I have benefited greatly from the help and comments of Derek Hum, Edward Ketchum, and Don Sabourin. All the data regarding Mincome was prepared with great care by Don Sabourin and a great deal of the analysis of Mincome's operations was aided by the work of Derek Hum and Don Sabourin.

1 I have been looking into the problem since June 1976 following a request by the Government of Canada and the Province of Manitoba. The terms of my examination were to lead to an evaluation of the entire sample at the time this report is completed, April 1977.

It is impossible to evaluate the characteristics of Mincome's experience on any absolute scale since "ideal" experiments do not exist. Thus, the evaluation must be made relative to a reasonable and realistic set of criteria which are either scientific in nature, related to optimal design and execution, or empirical in nature, related to what others accomplished in similar circumstances. The basic question can be summarized as follows:

(1) To what extent was the sample design conceived and carried out in a scientifically sound manner?

(2) To what extent were the instruments used to collect data appropriate to the stated objectives of the data base development?

(3) To what extent did the performance of the Mincome organization, including researchers, interviewers, etc., have any biasing influence on the quality of the data and the scientific merit of the data base?

(4) How does the data base and Mincome's performance compare with similar data bases and experiments conducted in the U.S.?

Clearly, some items such as the scientific merit of the sample design, the nature of the instruments, the procedures employed by the interviewers, etc., can be evaluated directly. However, it is not easy to ascertain if a scientific parameter like the rate of "non-completions" or the rate of "refusals" to interviews is "too high" or "reasonable". When confronted with such questions the experiences of other experiments will be introduced to obtain a better perspective on the problems considered. Such

comparisons are not simple since data from the different experiments is not easily comparable due to different procedures employed in the collection of data or the definition of various variables.

This paper will start with a general methodological section and will consider the instruments involved one at a time: the Screener, the Baseline, the Enrollment, and the Periodics.

II. ON TRUNCATION AND ATTRITION IN ALL SOCIAL EXPERIMENTS

The micro data banks created by the various social experiments in recent years do not contain random samples. Moreover, the non-random aspect of the sampling was due partly to the design of these experiments and due partly to the nature of the process involved. Thus, some of the problems of Mincome Manitoba are the same as those in other experiments, and I will return to the specific problems of Mincome after the relevant data has been investigated.

The sample used was not random but rather stratified. This means that certain key independent variables like age, sex, marital status, race, etc., were selected to define target subpopulations from which random samples were to be drawn. Thus, as is usual with stratification, the sample as a whole was not intended to be a random drawing but rather, the subsamples should have been random samples from the subpopulations. Unfortunately this has not been the case in any experimental data bank for three fundamental reasons.

(i) Truncation of the Data Sets. In all experiments there has been a well defined eligibility rule stating a cut-off point for income eligibility. It is generally agreed that an income-maintenance program will usually not be relevant to a family of four with annual income above $25,000. But what about income levels between the cut-off and $25,000? A specified subpopulation defined by race, age, family structure and wage rates of the heads with a cut-off point of $10,000 has a "truncation" problem. Among all identical families where the head earns $5 an hour, only those working less than 2,000 hours a year would be eligible and included, while the harder working people who work more than 2,000 hours a year will be excluded. This is true for all subpopulations. Holding everything constant, around the cut-off point the harder working people stand a higher chance of being excluded from the sample. This is characteristic of all experiments and of all social programs which have income cut-offs. This non-random element in sample selection appears to create a serious bias in all of the micro data banks. But in practice, the effective cut-off points in all of the experiments have been selected sufficiently high[1] so as to lend the empirical conclusions concerning the target population of the working poor a relatively high degree of validity. Also, for those researchers who may be interested in

---

[1] See, for example, Pechman-Timpane (1975) for New Yersey, Kurz-Spiegelman (1972) for SIME/DIME, and Hum and Laub (1977) for Mincome Manitoba.

broader investigations of the labor markets and not only in the working poor, the issue of truncation can be handled by utilizing advanced statistical methods which help overcome this problem.[1]

(ii) Self Selection in Enrollment. In all experiments it has been found that a large number of eligible members of the target, stratified subpopulations refused to enroll and to respond to questionnaires. Theoretically and from the available empirical evidence, the frequency of refusals is decreased by the degree to which the family expects to benefit from the experimental treatments. However, since a negative income tax program associates payment with income and income is related to work effort, it follows that refusals are related to the work effort of members of the subpopulations. Other reasons for refusals and non-enrollment are under intensive study. It is the current belief that in order to carry out the evaluation of the experimental effects, the tendency to refuse treatment should be incorporated as one possible behavioral response to eliminate the bias caused by refusals in the estimated parameters of the labor supply response surface.

(iii) Attrition During the Experiment. The third category of bias-creating behavior is the non-random process of attrition. There

---

[1] See, for example, Heckman (1976) and Hausman-Wise (1976a) and (1976b).

is ample evidence that attrition is inversely related to the financial compensation received.[1] Here again, as in the case of "refusals", it is clear that attrition tends to render the subsamples non-random and perhaps vitiates any research work based on the data base. This means that all of the studies to date of the effects of income maintenance on various aspects of family behaviour, all of which ignored the issue of refusals and attrition, contain an unknown element of bias which should have been taken into account. As methodology for dealing with refusals and attrition is available, most researchers to date have felt that the effect of attrition was not significant. However, attrition as such cannot be viewed as a sufficient reason to reject any micro data bank created by a social experiment, on the grounds that attrition renders the data invalid. The pattern of attrition must be established and show conclusively that the surviving sample does not contain sufficient information to enable proper testing of the hypotheses in need of testing.

These three problems are common to all data bases developed by social experiments which have been initiated in recent years. The problems were both expected and incorporated into the design of the experiments and correspondingly, overcoming these difficulties is one of

---

[1] In the volume on the New Jersey experiment by Pechman and Timpane (eds.) (1975), most papers and comments refer to the possible bias creating effects of attrition. This relation is also examined by Rossi and Lyall (1976) and Kershaw-Fair (1976), Table 7.5.

the essential elements in any proper study evaluating the effects of an experiment. The surprise of researchers and their disappointment at the magnitude of these problems is also common to all the experiments. The Canadian team was not spared this surprise.

As to the validity of the Canadian data base, it is clear that there is no way in which the data base can be declared free of the problems specified above. Thus, a mixture of criteria such as those proposed in the Introduction of this paper must be used.

III. SAMPLE DESIGN

By the time it was decided to implement Mincome, a significant amount of work in sample design had already been accomplished, and it was possible for Mincome's planners to capitalize on this work. The material regarding the design and selection procedures of the sample, has been extensively reviewed, particularly the sample allocation model designed by Charles Metcalf and various documents by Michael Laub and Derek Hum regarding the procedures for selecting families. This closely followed the procedure suggested by Conlisk and Watts[1] and used in New Jersey and in Seattle-Denver.[2] Apart from a slight deficiency of sample points resulting in a discrepancy between the actual and the theoretical assignment, all the procedures are perfectly sound. Each problem dealt with was solved correctly and in my opinion the outcome of this phase of the

---

[1] See Conlisk and Watts (1969) and Conlisk (1973).

[2] See Conlisk and Kurz (1972).

work was at least as good as any comparable work in the American experiments.[1]

In spite of the correct methods of sample selection, Mincome experienced a substantial amount of non-completions, refusals and attritions. These phenomena progressed to a point where, in the middle of the first year of payments, a significant portion of the initial sample was not included in the survey Panel and serious questions began to be raised as to the viability of the Experiment. In addition, Mincome discovered what all other experiments experienced: that large amounts of other transfer payments received by members of the sample put a larger than expected fraction of the sample above the breakeven level, resulting in their receiving minimum payments only. As a result, late in 1975 a decision was made to enroll a "supplementary sample" of 293 families in the Winnipeg segment, bringing the total sample, including the Controls and the Saturation segment, to over 1700 families by early 1976.

The procedures used for the sample supplementation will be reviewed separately below. The supplementary sample was drawn from special subpopulations and thus, the performance characteristics (in terms of non-completions, refusals, moves, etc.) are not easily comparable with the rest of the sample. Thus, the characteristics of the Screening, the Baseline and the Enrollment interviews of the original sample will be reviewed, the supplementary sample will be evaluated,

---

[1] The reader may consult a recent paper by Hum, Laub, Metcalf and Sabourin (1977) that provides a detailed description of the history of the sample development in general and the assignment procedure in particular.

and finally the characteristics of the entire merged sample as it developed during the Periodic interviews will be considered.

IV. THE SCREENER AND BASELINE INTERVIEWS

One method of assessing the quality of the surveys operation is to examine the gross "non-completion" rate which is defined as the number of incomplete interviews expressed as a percentage of total interviews attempted. Non-completion is caused by refusals, lack of communication between interviewers and respondents, and a host of other reasons. However, the definition of a "non-completion" was subject to significant variations across experiments. For example, if a family was found to be ineligible, the interview would either be terminated or cancelled; in some experiments this was recorded as "non-completion", in others it was eliminated from the set of families for which a completion was attempted. Non-completion due to ineligibility reflects inefficiency in the selection procedure of the families. In general, an aggregate measure of non-completion provides only a general idea of the degree of efficiency and general ability of the organization to carry out the experimental task. The non-completion rates in Manitoba will be compared below with similar rates in the U.S. The procedures used in both Seattle and New Jersey were rather rudimentary, and because of the high initial non-completion rates in Seattle, a second phase of sampling for Screening and Baseline interviewing took place. The comparative table on the next page gives the data for the two phases separately. On the other hand, the procedures used in Denver were very satisfactory.

TABLE 1

Non-Completion Rates in Manitoba,* Denver and Seattle**
(Screener and Baseline)

	Manitoba (Excluding Supplementary Sample)			Denver			Seattle			New Jersey		
Interview	Available	Not Completed	% Not Completed	Available	Not Completed	% Not Completed	Available	Not Completed	% Not Completed	Available	Not Completed	% Not Completed
Screener	29,948	8,290	27.7%	53,581	13,913	26.0%	24,168	16,180	66.9%	43,722	26,223	60.0%
Baseline	6,372	2,567	40.3%	7,350	2,816	39.3%	4,843	2,292	47.3%	2,593	612	21.7%
Screener and Baseline (Seattle Phase II)	-	-	-	-	-	-	11,856	10,201	86.0%	-	-	-

Sources: For Manitoba: Mincome Manitoba files
For SIME/DIME: My own estimation.
For New Jersey: Kershaw-Fair (1976), Chapter 7. Also see Rossi-Lyall (1976), Table 3.1.

* Recall that I am considering here only the original Winnipeg and Rural samples, excluding the supplementary sample which was enrolled later. However, I count as completions and non-completions those observations which were re-contacted as part of the effort due to the supplementary sample. This procedure is justified in view of the great efforts used in the other experiments to recontact refusals and trace the movers.

** No comparable data for the U.S. Rural Experiment is available. Only refusal rates were available and these are given in Table 2.

The experience of Seattle led to a great improvement in interview techniques and the planning stage was sufficiently long to enable adequate preparation. The comparative results are presented in Table 1.

The Screener interview was similar for all projects and the Manitoba 27.7% non-completion rate is the same as that of Denver and much lower than that of Seattle and New Jersey. The Baseline instrument of Manitoba was similar to that of Denver but much more demanding and complex[1] than the one in Seattle or New Jersey. It is evident from Table 1 that the gross non-completion rate of Manitoba is similar to that of Denver and Seattle while that of New Jersey is much lower (i.e., 21.7%) than all the others. This illusion is due to the manner in which the rate was compiled in New Jersey. There are many categories of individuals who passed the Screener and were available for Baseline but when such an interview was attempted, the family was not eligible or could not be interviewed because of language difficulties, etc. Manitoba, Denver and Seattle count these as terminated "non-completions" while the above figures for New Jersey simply excluded them from the category of "available" for interviewing, resulting in an artificially lower "non-completion" rate.

A more important comparison would examine the "refusal rates" of the different experiments. Again here, the refusal rate of New Jersey is expected to be lower since the Baseline interview in that experiment is much simpler and less provocative than Mincome's or Denver's. The

---

[1] N.B. To be explained below.

comparison is presented in Table 2.

Table 2

Refusal Rates: Screener and Baseline

	Screener	Baseline
Seattle	12.5%	9.1%
Denver	7.6%	17.1%
Manitoba	13.2%	16.1%
New Jersey	18.2%	6.9%
Rural U.S.	12.9%	11.8%

SOURCES

For SIME/DIME: My own estimates.

For Manitoba: Mincome Manitoba files.

For New Jersey: Kershaw-Fair (1976) and Rossi-Lyall (1976), Table 3.1.

For Rural U.S.: Bawden-Harrar (1976).

Note that the Screener interview was essentially the same in all sites and Mincome's record is comparable to Seattle's and the Rural U.S. experiment while all three are behind Denver. The Baseline interviews in New Jersey and Seattle were simpler and less demanding than those in Denver and Manitoba, and not surprisingly, Manitoba's performance is similar to that of Denver for this instrument.

A more complete picture of the breakdown of non-completions in

Manitoba is provided in Table 3. Note the extremely high percentage of the Manitoba population which moved between the time of the Screener and the time of the Baseline. This is partially explained by the length of time between the administration of these two instruments. The mobility rate itself is very high and appears later as a major cause of the high attrition rate of Mincome; this is due to the mobile nature of the population in the Canadian Prairie. It is also possible that the mobile and individualistic nature of this population is one additional factor in explaining the refusal rate. Such a factor should be taken as exogenous to the experiment, reflecting the distinct nature of the population and making the data so much more valuable.

Table 3

Analysis of Non-Completion:
Screener and Baseline in Manitoba

	Screener		Baseline	
	Sample Points	%	Sample Points	%
Available	29,948	-	6,372	-
Refused	3,954	13.2%	1,027	16.1%
Moved	-	-	852	13.4%
Other Terminations	4,336	14.5%	688	10.8%
Total Not Completed	8,290	27.7%	2,567	40.3%

My conclusion here is that both the Screener and Baseline interviews were executed in an adequate manner and the data in these two interviews can probably be regarded as comparable to any of the data bases available. The general performance characteristics are remarkably similar to the sample in Denver.

Before turning to the Enrollment interview, let me return to the Baseline instrument which I defined earlier as "demanding and complex". The three important aspects of this interview are:

(1) Its length; the interview tended to last 2-3 hours since it was the first "continuous time" interview in which members of the household were requested to provide a continuous record of their labor market activities for a complete year prior to the date of the interview.

(2) The Baseline interview asked a great many attitudinal questions, some rather private in nature, relating to the way households function. Such questions tend to generate hostility on the part of the family members.

(3) The inexperienced nature of the interviewers: this was the first extensive survey instrument to be administered and unfamiliar interviewers could have easily failed to handle it with care.

The effects of interviewing in general, and complex instruments in particular, are not easily established. Instruments can be so demanding and the information required so private or complex that some interviews may be terminated at the home or contain ambiguities which cannot be accepted

by quality control. A succession of complex demanding instruments and continuously poor performance by the interviewers will influence the refusal and non-completion rates of a whole sequence of future interviews. The converse is also true -- in order to stop the deteriorating effects of a sequence of interviews, it will take more than one good performance to reverse the trend. Given the difficulties encountered in the Baseline interview, the remarks here show that the outcome of these difficulties should be sought in the Enrollment and Periodic interviews.

In actual field operations Mincome made a distinction between three kinds of refusals;

(1) Refusal before an interview starts. This is recorded as "Refusal".

(2) Refusal to continue an interview in process. This is recorded as "Split Refusal".

(3) Refusal indicated at the end of a completed current interview in which the refusal refers to the next anticipated interview.

By studying this pattern of refusals, quantitative measures of the cumulative effects and, perhaps, the lagged effects of demanding instruments can be obtained. Due to the complexity of this data, its detailed presentation will be omitted; some information is given in Table 17 below.

V. THE ENROLLMENT INTERVIEW

V.1 The Sample as a Whole Excluding the Supplementary Sample

The non-completion rates in three sites are compared in Table 4. It is clear that the non-completion rate in Manitoba lies between the extremely low rate of Denver and the very high rate of Seattle. The higher rate was not only a result of the greater inefficiency in that site, it was also a result of a long list of special circumstances that made the Seattle interview and sample different from that of Denver. Thus, the question is "Does the discrepancy in non-completion rates between Manitoba and Denver represent a significant bias in the Manitoba data base or does it reflect the special circumstances of the two sites?".

Table 4

Non-Completion Rates for the Enrollment Interview

	Available	Not Completed	% Not Completed
Manitoba *(original sample)	2394	529	22.1%
Denver	3211	361	11.2%
Seattle	2942	899	30.5%

SOURCES

For Manitoba: Mincome's files.
For SIME/DIME: My estimates.

*No easily available record was kept in Seattle with regard to the attempted enrollment of the second phase enrollment. Also, both New Jersey and the Rural U.S. data are not included since no comparable data is available for these experiments.

This question cannot be answered by examining the data itself but rather by comparing the instruments and the procedures used to gather the data in the field.

In order to evaluate the nature of the Enrollment interview, the analysis would have to be divided into two parts: first, the lagged cumulative effects of the Baseline interviews and second, the circumstances of the Enrollment interview itself.

Starting with the Baseline, in Manitoba this instrument was long and demanding in its details and privacy, and was conducted by inexperienced interviewers. The Baseline in Seattle was relatively shorter and simpler because it did not require continuous time data. The Baseline in Denver was long due to the continuous time data requirement but less demanding in terms of the attitudinal questions. Moreover, the organization in Denver was built very carefully, and the Baseline interview was conducted with great professional skill. The Baseline instruments in Denver and Manitoba were similar (although Manitoba's was more demanding), but in Denver the interviewing was conducted under very good conditions. On the other hand, in Manitoba the conditions were far from ideal: investigation of the conditions and procedures employed in the different sites at Baseline reveal the following facts which may have contributed to a negative attitude on the part of the sample families towards the experiment.

(1) In Denver the interviewers paid the interview fee in cash at the time of the interview; in Manitoba the families were promised money. Also the payments may have been too small.

(2) In Denver the interviewers introduced themselves as representing a private firm conducting research for the improvement of conditions in Denver. In Manitoba the interviewers introduced themselves as representing the government, and many families may have been unsure "why the government does not simply look up the financial data at the local office of Revenue Canada (income tax files).

(3) Out of the 70 active interviewers, 50 were temporary employees and of these, 10 were summer students. Although their training may have been adequate, the interviewers were inexperienced. Moreover, due to their temporary employment status, they might have been less dedicated and less responsive to the pressure at that time.

(4) The office space of Mincome Manitoba was very small and the air conditioning broke down during the summer, preventing the interviewers from spending adequate time together and comparing notes among themselves and with the quality-control people.

The relatively adverse conditions at Baseline may have contributed to the non-completion rate at Enrollment. However, before turning to the analysis of the Enrollment interview itself, note that all the above factors tending to increase the refusal rate do not seem to have any systematic pattern which would produce the complete disappearance of any specific subpopulation. Moreover, the forces operating in Mincome can be viewed as the intensification of the same forces which produce widespread attrition

throughout the sample and in all social experiments with all the normal problems discussed in Section I above.

The Enrollment interview itself was very long; tending to last 2-3 hours and very demanding in terms of details and information on family attitudes. In this respect the Manitoba Enrollment was similar to the Enrollment interview in Seattle, and this contributed to the non-completion rate in both sites. A long and concentrated interview has significant advantages as a large amount of data is collected in a short time, thereby minimizing the problem of "stretching out" the interview over an excessively long calendar period relative to the period covered in the interview. On the other hand, long and demanding interviews increase the hostility of the family, thereby increasing the probability of ultimate attrition. In Denver a satisfactory balance was obtained with regard to the Enrollment interview by dividing it into two physically distinct instruments which were given at two different points in time with a few weeks in between: the "Enrollment" interview itself, which covered only the "Core" and other items needed to initiate payment, followed a few weeks later by the "Post-Enrollment" interview which collected the rest of the data. Although the material collected in Denver and Manitoba was the same, the methods used in Denver were less harmful to the dynamics of the sample. Also, other considerations must be kept in mind as potential contributors to the non-completion rate of the Manitoba Enrollment.

(1) Items (1), (2), (3) and (4), mentioned as influential at Baseline were still operating at the time of Enrollment and contributing to the dissatisfaction of the families and the frustration of the interviewers.

(2) Due to lack of integration within Mincome, there was insufficient contact between the interviewers and the research staff. This had two consequences: first, the basic lack of understanding by the interviewers of some of the more subtle issues of their work was not corrected, and they only "learned by doing". Second, in response to natural problems which arise in field operations of this sort, the field decisions were being made on purely administrative-programmatic grounds rather than upon the strictly scientific value of the experiment and the need to keep the sample intact. Therefore, no major effort was made to initiate an anti-attrition program within Mincome at the time.

(3) The data processing systems and quality control did not keep up with the interviewing pace. More specifically, for efficient collection of continuous time data the interviewer requires the end-point information of the Baseline interview prior to proceeding with the Enrollment. In many cases this data was not made available and the interviewer had to either obtain this information from other sources or proceed without it.

(4) The problem of "Moves" remained significant at the time of Enrollment as the following table, giving the components of non-completion, shows:

Table 5

Breakdown of Non-Completion in Manitoba: Enrollment

	Sample Points	%
Available	2,394	-
Refused	312	13.0%
Moved	185	7.7%
Other Terminations	32	1.3%
Total Not Completed	529	22.1%

Clearly, the refusal rate is high. Note, however, the extremely high rate of mobility of the Canadian population which accounts for 7.7 percentage points out of the 22.1% non-completions of Enrollment in Manitoba. This rate is comparable with Seattle's but much higher than that of either Denver or New Jersey where non-completion rates of Enrollment interviews due to mobility was between 2.5% and 5%. A contributing factor to the loss due to mobility was the decision by Mincome not to follow any family whose residence was further than 200 miles from the nearest Mincome office, even when its address was available.

(5) Canada, in comparison with the U.S., may have had two additional specialized problems in addition to the mobility mentioned above. First, a language problem may have reduced the completion rate. Second, Americans are much more exposed to public opinion polls, political surveys, and interviewing

for commercial purposes. Thus, the average Canadian may be more surprised and perhaps more antagonistic when faced with such questioning.

The refusal rate at Mincome will now be compared to the rest of the experiments. Table 6 shows that the refusal rate at Enrollment in Manitoba was significantly higher than in New Jersey and Seattle but only moderately higher than in Denver. I am isolating Denver here since Mincome and Denver had similar Baselines and required the same extensive information except that, at the Denver Enrollment, the interviewing process was divided into two meetings. Yet, the refusal rate at Manitoba was only moderately higher than Denver. It should be emphasized that the special circumstances at Manitoba combined with the specifics of the instrument itself would seem to provide adequate explanation for the refusal and non-completion rates at Mincome's Enrollment.

The refusal rate at Mincome's Enrollment was a bit higher than one would wish, but far below any level that would render the sample invalid. Although both the non-completion and refusal rates at Enrollment in Mincome are high, they are not excessively high in relation to comparable experiments. Consequently, the most important observation is related to the long list of problems that the experiment had at Baseline and Enrollment. They all result either from a special set of circumstances or are the consequences of inexperience and inefficient organization. However, there is nothing to indicate any systematic bias that would make the surviving sample unusable; on the contrary, these factors are nothing but the amplification of the common causes of attrition in all experiments and

will need to be treated in accordance with the discussion in Section I above. Although it is paradoxical, it appears that exactly because Mincome was inexperienced, it committed only those typical errors which tend to distribute the attrition in its usual way rather than concentrating it in any specific subpopulation. Let us look at observations regarding the reasons given for the refusal to enroll.

Table 6

Refusal Rates: Enrollment	
New Jersey	4.7%
Manitoba	13.0%
Seattle	4.3%
Denver	8.0%

SOURCES

For New Jersey: Kershaw-Fair (1976), Ch. 7 and Rossi-Lyall (1976), Table 3.1.

For SIME/DIME: My own estimates.

V.2 Preliminary Analysis of the Refusal to Enroll

At the end of Section V.1 above, it was pointed out that there was no clear bias-producing pattern of attrition and refusals that would result from any specific procedure adopted by Mincome. To support this, I present here some preliminary tabulations of the respondents' reasons for refusals to the Enrollment interview and payment as recorded by the

interviewers. The "reasons" given below are not mutually exclusive, thus a person may not only say that "he is not interested" but also that "the interview is too long". Table 7 gives only a first impression of the issues involved. Clearly the largest category of refusals are those who "are not interested". I note that category 2 reasons, indicating that the individuals "do not wish to participate", are based on a deeper knowledge than reason type 1, since individuals giving this reason probably decided not to participate only after receiving some information and thinking about their options. What is also striking is that a great number of reasons exist for individuals to refuse to enroll in a program which aims to provide them with financial support. Consider the distribution of reasons given by individuals for not receiving payments. Table 8.A gives the distribution of reasons for refusal to be interviewed in surveys (which will remove the family's eligibility for payments). This is the most common refusal and many were people who never initiated payments due to their failure to complete the appropriate payment enrollment package. Table 8.B gives the distribution of reasons provided by individuals who had earlier enrolled and received some payments but subsequently decided to withdraw from payments. Questionnaires sent out by Mincome to those who refused to initiate the payment package indicate again a multitude of reasons for refusal but a distinct segment indicated that the IRF was too complicated. Due to a later recontact a large fraction of these refusals were changed and the families enrolled.

It is clear from the tables below that a large fraction of the refusing families also will not provide an explanation for the refusal to accept payments even when they are entitled to receive them. On the other

Table 7

Distribution of Reasons Given for the Refusal to Complete the Enrollment Interview (Winnipeg and Rural Dispersed Sites)

		Frequency	%
1.	Not interested or "don't want to be bothered"	66	23.8%
2.	Don't want to participate	33	11.9%
3.	One respondent (usually male) not interested	30	10.8%
4.	No time for interview or interview too long	30	10.8%
5.	Feel their income is too high	21	7.6%
6.	Don't like interviews	19	6.9%
7.	Completely disagree with program	13	4.7%
8.	Information too personal	11	4.0%
9.	Dislike government (and their program)	10	3.6%
10.	$10 not enough for all that information	8	2.9%
11.	Respondent thinks he can do better on welfare	8	2.9%
12.	Moving in near future	6	2.2%
13.	Doesn't like "hand-outs"	6	2.2%
14.	Other reasons	16	5.7%
	TOTAL	277	100.0%

Table 8.A

Distribution of Reasons Given
for the Refusal to Continue Survey Interviews (Winnipeg Site)

	Reasons Cited	Frequency	%
1.	No longer interested	73	30.0
2.	Interviews are a bother--take too much time	72	29.6
3.	Payment forms much too complicated	53	21.8
4.	Too personal	18	7.4
5.	Welfare is better	6	2.5
6.	Sibling refusal (refusal by a non-head)	6	2.5
7.	Respondent separated	4	1.7
8.	No particular reason	11	4.5
	TOTAL	243	100.0%

Table 8.B

Distribution of Reasons Given
for the Refusal to Continue Payments (Winnipeg Site)

	Reasons Cited	Frequency	%
1.	Don't like interviews--too personal	12	8.3
2.	Just not interested in Mincome	11	7.6
3.	No time to do reports	10	6.9
4.	Not enough money received for participation	9	6.2
5.	Forms ask too much information	5	3.4
6.	New spouse refuses to cooperate	3	2.1
7.	Personal reasons	3	2.1
8.	Sibling refusal (refusal by a non-head)	1	.7
9.	Family problems	1	.7
10.	IRF too complicated	1	.7
11.	Don't trust Mincome	1	.7
12.	No reason given	88	60.6
	TOTAL	145	100.0%

hand, a great many reasons are given and these need to be studied with great care.

The discussion suggests that even if a national program is initiated, not all eligible households will participate and take advantage of its provisions. The information presented here gives only a glimpse of the issues involved, and additional studies are needed before firm conclusions can be drawn.

V.3 The Problem of Refusal to Enroll in the Dauphin Saturation Site

The experimental survey in Dauphin represents a special interest since it has two fundamental properties that no other experiment has. First, the Dauphin segment is a saturation experiment meaning that all 4,196 households in the target area are eligible to receive both negative tax payments and positive tax reimbursement. Second, being an ongoing program, the eligibility of each resident is maintained throughout and, if a household were missed or initially refused, it may change its mind later on and participate.

These two properties give the Dauphin segment some qualities of interest which extend beyond the experimental setting itself and are capable of providing direct implications for a national program. Therefore, in evaluating the Dauphin experience, we should concentrate not only on the specific experimental meaning of the situation but also, we must examine the national implications as well.

Starting with a record of interview completions, Table 9 shows that the non-completion rate in Dauphin has been uniformly high. The

Table 9

Analysis of Non-Completion: Dauphin

Total Screened				4,196
	Refused	752	17.9%	
	Other Terminations	178	4.2%	
	Completed	3,266	77.9%	
			100.0%	
Total Baseline				1,162
	Refused	280	24.1%	
	Moved	84	7.2%	
	Other Terminations	34	2.9%	
	Completed	764	65.8%	
			100.0%	
Total Enrollment				595
	Refused	38	6.4%	
	Moved	39	6.6%	
	Other Terminations	2	.3%	
	Completed	516	86.7%	
			100.0%	
Total Periodic 1				547
	Refused	75	13.7%	
	Moved	4	.7%	
	Completed	468	85.6%	
			100.0%	
Total Periodic 2				493
	Refused	35	7.1%	
	Moved	6	1.2%	
	Completed	452	91.7%	
			100.0%	
Total Periodic 3				464
	Refused	23	5.0%	
	Moved	9	1.9%	
	Completed	432	93.1%	
			100.0%	
Total Periodic 4*				358
	Refused	9	2.5%	
	Moved	4	1.1%	
	Completed	345	96.4%	
			100.0%	
Total Periodic 5				350
	Refused	9	2.6%	
	Moved	4	1.1%	
	Completed	337	96.3%	
			100.0%	
Total Periodic 6				339
	Refused	2	.6%	
	Moved	9	2.7%	
	Completed	328	96.7%	
			100.0%	

* 84 farmers were inactivated before the fourth periodic went into the field.

refusal rates are 17.9% for the Screener, 24.1% for the Baseline and 6.4% for the Enrollment. Although the rate rises to 13.7% in Periodic 1, it falls continuously afterwards as follows:

Periodic 2 - refusal rate of 7.1%

Periodic 3 - refusal rate of 5.0%

Periodic 4 - refusal rate of 2.5%

Periodic 5 - refusal rate of 2.6%

Periodic 6 - refusal rate of .6%

Furthermore, we may compare the non-completion and refusal rates of Dauphin and Winnipeg. The results are presented in Table 10. In Table 10 note that since Dauphin is a saturation site, the Screener was in fact a complete survey which was intended to identify the eligibles. Due to the intensity of the Screener, the refusal rate of Baseline among the higher-income families was higher in Dauphin than in Winnipeg. However, the sum of refusals to Baseline and Enrollment is essentially the same, indicating that the lower refusal rate at Enrollment in Dauphin is simply the lagged effect of the earlier high refusal rate. This lower refusal rate is also due to the fact that some of the causes for refusal present in Winnipeg were not present in Dauphin. The higher non-completion rate in Winnipeg simply reflects the higher mobility rate of the population in that site. Otherwise, the two sites appear reasonably similar in their performance characteristics. However, this does not alter the earlier observation made in Section V.1 that the non-completion and refusal rates for the Enrollment interview were a bit higher than one would have wished them to be.

Table 10

Refusal and Non-Completion Rates: Winnipeg vis. Dauphin

	Non-Completion Rate		Refusal Rate	
	Dauphin	Winnipeg	Dauphin	Winnipeg
Screener	22.1%	29.0%	17.9%	13.1%
Baseline	34.3%	38.5%	24.1%	14.0%
Enrollment	13.3%	25.2%	6.4%	15.1%

Of particular interest is the fact that out of the 516 households in Dauphin that completed the Enrollment interview and were clearly eligible for some payment, only 322 returned the completed payment package that would have initiated payment. Out of the 516 families that Mincome wanted to include in the survey panel, thus expressing the thought that the program could offer to them some potential benefits, only 62% agreed and enrolled to receive payments. This, however, is a complicated matter since the selection of families was based on the Baseline data. At the time of enrollment some of the information was out of date, resulting in an attempt by Mincome to enroll relatively high-income people in payments who then refused to initiate payments. On the other hand, Mincome's support level was relatively low. Accordingly, by the end of the sixth month some 70% of the sample enrolled for payments were receiving minimum payments. This suggests that some of the people who refused to enroll might have made accurate estimates of the amounts of money they would have received if they had enrolled, and this may explain their decision

not to enroll.

Yet, a different approach leads to contrasting estimates. First, recall that in order to control the tax rate, Mincome's rules require the experiment to reimburse the positive taxes paid by the household. Thus, due to the special phase-out provisions of the experiment,[1] the effective cut-off for negative tax payments and/or positive tax reimbursements is about $12,000-$13,000[2] (for a family of 4 comprising two adults and two children). This means that every household whose income is less than this cut-off level is eligible to receive at least some positive tax reimbursement even if negative tax payments are zero. From the limited information about the 4,196 households in Dauphin, it is safe to assume that at least 1/3 of the households have income less than this range, implying that there are at least 1400 households in Dauphin eligible for some payments. It is probable that at least 15% of the Dauphin households in 1975 had income of less than $8,000 (for the standard 4 person family), and thus at least 630 families were eligible for non-minimum negative tax payments as well as positive tax reimbursement.

Obviously, many of these were either terminated because they are non-interviewable or because they refused the Screener, Baseline, or Enrollment interviews. Mincome discovered 516 of those who may have been eligible for negative payments who completed all three stage of interviewing, but then 37.6% of these refused to initiate payment. Further

---

[1] See Hum and Laub (1977) for further details.

[2] Depending upon the cost of living adjustment.

examination of those 194 households in Dauphin who refused to initiate payments shows that:

93 of the families ultimately refused to be interviewed
6 moved out of the area
15 were farmers who were inactivated later
80 are still active in surveys

It was estimated that at least 1400 households in Dauphin were eligible for some payments, but Mincome identified only 516 such potential households. This large gap is a result of the special procedure used in Dauphin: although the rules allow any household to walk into Mincome's office and request payments, it was publicly announced that "only those with income below $8,000 (for the standard 4 person family) are expected to benefit from the program". In Screening the population, Mincome sought to establish an interviewing panel of those receiving negative tax payments, and the procedure concentrated on finding these. The resulting panel of interviews has a significant variance in actual income, including a significant number of enrolled families with income above the effective cut-off level making them ineligible for either negative tax payments or for positive tax reimbursement. Therefore, there is a significant number of families in Dauphin who are eligible to receive negative tax payments, yet they are not enrolled; but also, there is a large number of households which could have received positive tax reimbursements but are not enrolled for such payments.

What is perplexing is the fact that in order to receive any payment a family does not need to be selected by Mincome; it can simply walk into Mincome's office and demand to receive what it is entitled to. There is evidence that some families have begun to take this course of action, but the question still remains why such demands have not been made (or made sooner), and to what extent this fact is a reflection of ignorance, fear, political conviction, or rational decisions made by the households. The reasons are simply not known to us at this point.

A preliminary idea may be obtained from a consideration of the three distributions of reasons given for the refusal to complete the Enrollment interview, the refusal to continue payment (after being enrolled for payments), and finally the refusal to be interviewed, thus causing a discontinuance of payment (for those enrolled in payments). Recall that a family may select multiple reasons for refusal. In Table 11.A, note that the distribution is given for households which Mincome wished to include in the Panel because the family was thought to be part of the target population. Table 11.B, presents the reasons given by families who initially enrolled in payments but later decided to refuse any further payments. The expectation of low level of payments and too much "trouble" in getting it explain some refusals, but the bulk of the families (60.3%) provide no reason.

Table 11.C provides some information on the reasons for the refusal to be interviewed. For people who were enrolled in payments, this means a discontinuation of payments, but for a large number of

Table 11.A

Distribution of Reasons
Given for the Refusal to Complete the Enrollment Interview (Dauphin)

	Reasons Cited	Frequency	%
1.	Feel their income is too high	6	14.3
2.	Don't like "hand-outs"	6	14.3
3.	Information too personal	5	11.9
4.	Not interested or "don't want to be bothered"	5	11.9
5.	No time for interview or interview too long	4	9.5
6.	Completely disagree with program	3	7.1
7.	Don't like interviews (fill out all that stuff)	2	4.8
8.	Don't want to participate	1	2.4
9.	One respondent (usually male) not interested	1	2.4
10.	Dislike government (and their programs)	1	2.4
11.	Other reasons	8	19.0
	TOTAL	42	100.0%

Table 11.B

Distribution of Reasons
Given for the Refusal to Continue Payments (Dauphin)

	Reasons Cited	Frequency	%
1.	Not enough money received for participation	14	18.0
2.	Forms ask too much information	6	7.7
3.	Just not interested in Mincome	4	5.1
4.	No time to do reports	3	3.8
5.	Don't like interviews--too personal	3	3.8
6.	I.R.F. too complicated	1	1.3
7.	No reason given	47	60.3
	TOTAL	78	100.0%

families it does not, since they had not enrolled in payments. In fact, these families constitute a significant portion of the total. Otherwise, the reasons are systematic and in accordance with expectations; low payments, dissatisfaction with the interviewing requirements, and a general sense of violation of family privacy. Finally, there is the question if the refusal to enroll in payments after agreeing to be interviewed is the same in all sites. Table 12 indicates that the Dauphin saturation site is more problematic.

A reaction to this information is to reject the validity of the data collected and to conclude that the administration of the experiment failed to reach its target population. This would be a serious error in judgement. The phenomena at hand reflect only to a limited degree the administrative and research abilities of Mincome. The facts observed above have not been caused by any bias producing procedure adopted by Mincome but rather by a self selection process which is operating in all such programs and will operate in any real national program when enacted. These facts are also consistent with similar observations reported in Tables 7-8 above with regard to Winnipeg and the rest of the rural segment. This implies that in spite of the fact that this extensive degree of self selection has deep implications for the analysis of the experimental results, it is equally clear that this problem is quantitatively the same as the problem that all experiments face when significant refusals and attrition occur. In the analysis of response to income maintenance, it will be necessary to include as one possible response the refusal to accept financial support from the experiment even when the individual is

Table 11.C

Distribution of Reasons Given
for the Refusal to Continue to be Interviewed by Surveys (Dauphin)

Reasons Cited	Frequency	%
1. Payments too small	40	26.5
2. Interviews are a bother; take too much time	27	17.9
3. No longer interviewed	16	10.6
4. Too personal	15	9.9
5. Opposed to negative income tax	8	5.3
6. Sibling refusal	5	3.3
7. Welfare is better	4	2.6
8. Payments forms much too complicated	1	.7
9. No particular reason	35	23.2
TOTAL	151	100.0%

Table 12

Comparison of the Refusals to Accept Payments

	Winnipeg	Dauphin	Rural Dispersed
Total Number of Families Enrolled for Interviews	1079	516	270
Families Enrolled for Interviews but who Have Not Initiated Payments	113	194	66
% of Those Interviewed Who Refused to Initiate Payments	10%	38%	24%

entitled to it. What is also implied here, however, is that if a national program were initiated, a large number of households would also refuse to participate for an extensive and complex set of reasons ranging from "dislike of government" to optimistic expectations of future income which prevent a family from identifying itself as "needy", and thereby refusing. The refusals to submit to questionnaires or receive payments are an important element in program development and therefore should be studied as intensively as possible.

The unique feature of Dauphin is the fact that the above questions can be studied. Such a study can be initiated since the eligibility of the family is not terminated at enrollment time. Thus, the refusal of the 194 (out of 516 who completed Enrollment) households to claim their payments from Mincome at enrollment time should be viewed as an ongoing refusal by these households (and many other eligible households who refused earlier interviews) to lay claims against Mincome. What we know is that 122 households visited Mincome's offices during the period of operation and submitted applications to establish their eligibility to receive payments. Of these, 84 were deemed to be of research interest and were included in the survey panel. However, this number is much smaller than the number of true eligibles. The recommended study should focus both on the estimation of the frequency of those eligible for payments and the distribution of their potential payments, as well as the reasons for their refusal to be interviewed and collect payments after being interviewed.[1]

---

[1] To help familiarize the reader with some further details, I include in Appendix A some additional information regarding the enrollment process in Dauphin.

VI. THE SUPPLEMENTARY SAMPLE AND THE ISSUES OF SAMPLE SIZE

VI.1 Background

The supplementary sampling was initiated a year after the enrollment of the initial sample and resulted in the enrollment of 293 additional families. Internal discussion within Mincome during the second half of 1975 revealed that:

(1) It was thought that an "unacceptably small number of families" were below the breakeven level and received non-minimum payments. This was viewed as a problem of having an unbalanced sample with an insufficient number of sample points in the "target" population.

(2) The high attrition rate aggrevated the above problem and reduced the number of low income families in the sample, thus reducing the value of the data for research purposes.

(3) The overall size of the Mincome sample was too small, thus raising questions of the reliability of the results based on the analysis of the Mincome data.

In reviewing the problems related to the development of the sample, I have already discussed the issue of attrition. The question of sample size will be discussed at the end of this section. The question to be answered now is to what extent the supplementary sample represents a genuine increment to the data base, and if there are any systematic biases in the selection process that would reduce the value of this sub-sample.

The fact that the supplementary sample was enrolled a full year later than the original does not, by itself, represent a serious experimental defect. It certainly makes the analysis more difficult since the experimental and the calendar "clocks" of the different samples do not match. However, with adequate information on the overall economic climate in Manitoba, like unemployment rates, investment and growth in the target areas, etc., the analysts can easily correct for the differences in the calendar "clocks". Thus, the important issue to be examined relates to the procedures for the selection of the supplementary sample.

VI.2 Procedures Used for the Selection of the Supplementary Sample

The supplementary sample was selected from four basic segments.[1]

The first segment originated from the discovery that during the original screening, a procedure was established by which any home occupied at that time by elderly people (head over 57 years of age) was to be regarded an "ineligible". However, due to the dwelling and mobility patterns of the population, it is clear that this same address could, at the time of Screening, be occupied by an eligible family which should have been surveyed. Due to this error, 16% of the dwelling units were incorrectly excluded from selection. This group constituted the first segment of resurveyed dwelling units and yielded 56 new eligible families which were added to the Panel. The decision to resurvey these addresses corrected an earlier error.

---

[1] The matter is also discussed in Hum, Laub, Metcalf and Sabourin (1977).

The second segment of the supplementary sample was obtained by recontacting families who did not complete the Baseline instrument because they were recorded as "refusals" or "moves". This segment yielded an additional 72 eligible families who were enrolled. Recontacting of refusals or moves is a correct procedure to reduce attrition. Similar procedures have been extensively used by all other experiments and, it is inappropriate to label these families as belonging to the "supplementary sample". They are part of the original sample that was viewed as eligible but was not completed due to all the reasons discussed earlier in this paper.

The third segment of the sample comes from further random drawings from the file from which the original sample was drawn. This segment yielded 71 additional eligible families which were added to the Panel. This procedure is clearly correct.

The fourth segment does represent a problem. With the intent of increasing the representation of low income families, Mincome obtained from municipal sources in Winnipeg a list of families who had earlier applied for welfare or had previously been on welfare, but were not on welfare at the time the list was compiled. A sample of these were contacted yielding an additional 94 families who were added to the Panel.

Given the criteria for selecting the "welfare segment" of the supplementary sample, it is not clear how this subsample fits the requirements of the file from which the original sample was drawn. Given the possible bias introduced here, on strictly theoretical gounds, this segment could be excluded from the Panel and future researchers may in

fact do so. I have tried to help by examining the difference in some characteristics between the "welfare segment" of the supplementary sample and those families included in the original sample who were "past-welfare" families in the sense that they received some welfare support between January 1, 1972 and June 30, 1973. The hypothesis was that the two subsamples came from the same population and thus may be merged. Due to the limited amount of information available at this time, the characteristics tested were age distribution, family size, and pre-experimental income. The results indicate that the welfare segment of the supplementary sample:

(i) tends to be younger and more concentrated in the 25-34 age group and less in the 45-57 age group, compared to the welfare segment of the initial sample;

(ii) tends to contain fewer households with a small number of children; and

(iii) tends to have higher pre-experimental income.

I view the above differences as insufficient evidence to reject the hypothesis that the samples can be merged. More detailed information is provided in Appendix B.

On the basis of the above, one may safely conclude that the additional 293 families enrolled as a result of the supplementary sample provided significant additional information to the Mincome sample. The supplementary sample significantly strengthens the value of the data base.

VI.3 The Issue of Sample Size

As I noted earlier, one of the motivations for the supplementary sample was the desire to increase the sample size in order to increase the reliability of future research results, which will be derived from the Mincome data base. The question now is whether the sample size is sufficiently large for the conduct of serious scientific research on Canadá's labor markets.

Compare the Mincome sample with those of the two U.S. experiments which have been completed and from which significant conclusions have already been drawn. Table 13 below includes all surviving families for New Jersey and the Rural U.S. Experiment, although these numbers

Table 13

Sample Size Comparisons

	Treatment	Controls	Total
New Jersey: Final Number of Families	612	472	1084
Rural U.S.: Final Number of Families	511	583	1094
Manitoba: No. of Families to Date (3/77)			
Winnipeg and Rural Dispersed	806	388	
Dauphin Saturated	328	-	
Total Mincome	1134	388	1522

SOURCES For New Jersey: Kershaw-Fair (1976), Table 7.4, p. 110.
For U.S. Rural: Bawden-Harrar (1976), Table 6, p. 24.
For Manitoba: Internal Mincome data.

include various complex families arising from family composition changes. The analysis of New Jersey and the Rural U.S. data has been conducted, so far, with respect to the "intact families" only which means, in their context, "continuous husband and wife present". This is a subsample of the groups reported in Table 13.

In reviewing Table 13, one needs to keep in mind the following considerations which influence the sample requirements:

(i) Racial Composition. In New Jersey there were three separate racial groups present: Whites, Blacks and Puerto-Ricans. In the Rural U.S. Experiment, only Blacks and Whites were present. Mincome does not have any racial composition problem and this is a significant advantage.

(ii) Plan Variations. Since the guarantee level of .5 of poverty was not effective, both New Jersey and the Rural U.S. experiments had guarantee levels of .75 and 1.00 of poverty with an additional single group of a guarantee of 1.25 of poverty and 50% tax. The variations in Mincome's plans are much more significant. First, the low plans have been ignored due to the presence of welfare, thus if $3,600 is the January 1975 poverty line in Canada, then the Mincome plans were

1.06 of the poverty level with 35% tax
1.28 of the poverty level with 35% tax
1.06 of the poverty level with 50% tax
1.28 of the poverty level with 50% tax
1.50 of the poverty level with 50% tax
1.06 of the poverty level with 75% tax
1.28 of the poverty level with 75% tax
1.50 of the poverty level with 75% tax

Mincome's variations in the guarantees and tax rates are greater than those of the two U.S. experiments mentioned above. Thus, if the elasticity of response to treatment is not zero, then the larger variations in treatments are likely to induce larger absolute responses to treatment, resulting in greater reliability of the estimates compared with the U.S. experiments.

(iii) New Jersey covered five cities but they were closely related to each other; the Rural U.S. Experiment was administered in Iowa and North Carolina and these are very different sites. Mincome Manitoba operates within one province but the Urban and Rural segments are probably distinct.

(iv) Questions have been raised about the value of the data for the 270 self-employed farmers who were included in the Rural U.S. Experiment. By contrast, the current Mincome sample size of 1522 excludes over 100 originally enrolled farmers who were dropped from the Panel at the end of 1975.

(v) The New Jersey file concentrated on two-headed households and thus only some 690 observations belong to the "intact families". The rest resulted from endogenous changes in the family composition. Both the Rural and U.S. Experiment and Mincome Manitoba aimed from the start to obtain samples for both two and one-headed households.

(vi) Mincome has one saturation site in Dauphin which represents a segment that was not present in the U.S. experiments.

It is clear that, apart from the sample size itself, Mincome has some distinct advantages compared with the U.S. experiment, and therefore, if in fact the U.S. experiments did provide valuable information regarding the U.S. labor market, then the Mincome sample size as such, which is expected to be 30% larger than New Jersey's and the Rural U.S. sample, will be adequate to support similar conclusions with regard to the Canadian labor market. Mincome has, however, additional advantages.

As the American experiments unfold, the accuracy of the estimates of the effects of income maintenance on labor market behavior will increase. With this increased accuracy will grow a certain "common knowledge" consensus which will be part of the basic information used by the policy maker. The analysis of the Canadian sample will therefore start from a different informational base than that available to the earlier American investigators. This means that the Canadian researchers will be able to concentrate on those issues of labor market behavior in which the Canadian results are different from the American results and thus, for the same level of reliability, the Canadian sample requirements will be smaller than the earlier American studies. Given the information gathered in the U.S. experiments to date, the Canadian sample, which is larger than those of the above mentioned two U.S. experiments, can be expected to yield more reliable estimates than the U.S. results.

Finally, it is obvious that no absolute scale exists to determine if the Mincome sample is "too small" or "too large". The limited experience we have had, so far, in analyzing Panel data of this sort suggests that the available sample will detect with reasonable reliability responses

which are important from the economic point of view, and it will do so at least as successfully as the two U.S. experiments which have already been partly analyzed.

VII. THE PERIODIC INTERVIEWS

In the periodic interviews, comparisons with the U.S. experiments become much more complicated. This complexity is related to two facts. First, Seattle and Denver kept attrition records as a matter of rates per unit of time (1 year). Since different interviews required different lengths of time to complete, the comparison of completion rates per interview is not completely meaningful. Second, Seattle and Denver did not record the "moved out of area" as non-completions since extensive effort was made to follow them up. The record for New Jersey is as follows:

Table 14

Annual Attrition Rates in New Jersey*

First year	9.2%
Second year	7.4%
Third year	5.0%

Source

Kershaw-Fair (1976), p. 106

* No final attrition rates for the other experiments are as yet published or easily available.

SIME/DIME seems to have experienced lower attrition rates (5% - 7%) during the first two years of operation. As a benchmark, assume that an attrition rate of 6% - 9% in the first year and 5% - 7% in the second year is not unreasonable.

Consider now the experience at Mincome.

Table 15

Non-Completion Rates for Periodics (All Sites)

Interview	Available	Not Completed	Refusals	% Refusals	Moves	% Moves
Periodic 1	1946	216	191	9.8%	22	1.1%
Periodic 2	1773	123	96	5.4%	27	1.5%
Periodic 3	1700	102	73	4.3%	27	1.6%
Periodic 4	1779	80	59	3.3%	19	1.1%
Periodic 5	1704	86	57	3.3%	27	1.6%
Periodic 6	1626	80	30	1.8%	49	3.0%

Although the data presented in Table 15 is not easily comparable with our above stated benchmarks, it is obvious that the Manitoba attrition rates are rather high. However, to make the comparison with the benchmark clearer, an estimate of the annualized attrition rates in each quarter of the first eight quarters of Mincome's operations appears below in Table 16.

Table 16

Estimated Annualized Attrition Rates (All Sites)

	Refusals	+	Moves	+	Other	=	Total
Months 1-12	17.6%		3.3%		.2%		21.1%
Months 4-16	10.5%		3.0%		.2%		13.7%
Months 8-20	9.2%		3.0%		.3%		12.5%
Months 12-24	8.1%		4.1%		.3%		12.5%

These rates are much too high compared with the true annual attrition rates of the proposed benchmarks. Moreover, they are much too high for any experiment to survive. It is now clear that it was this high attrition rate during the Periodic interviews that led Mincome Manitoba to seek the supplementary sample although, as Table 15 shows, the true attrition rate was declining very rapidly and by the end of the second year would have been at the 8% - 10% annual rate.

We must keep in mind that the Periodic instruments used in Manitoba are very similar to those used in SIME/DIME. Their design is very sound, they have been pre-tested and field-tested in the U.S. and are not known to generate any bias in response. Although the interviews themselves are very long and demanding, they have been used in the U.S. repeatedly and have not caused drastic attrition. My view is that the explanations of the attrition rates cannot be related to the Periodic instruments themselves. There are four classes of problems which caused the level and structure of the attrition rates as reported above.

VII.1 Initial Attrition and the Payment Package

All social experiments have had the experience of high initial attrition which results from the heterogeneity of the sample. Some people "just do not like" to be interviewed, some do not like to reveal financial information, some were disappointed at the level of payment received, etc. Mincome Manitoba added to this problem by requiring of families essentially a "self enrollment" procedure in payment. Each family had to complete on its own a complex payment package and if the package was not returned, no payments were made. In other experiments enrollment to payment was initiated by an interviewer who visited the home and instructed the family on how to initiate the payment procedure. Due to the complexity of the IRF and the payment package, there were a significant number of families who were enrolled in surveys and not in payments. These obviously constituted an extensive source of high initial attrition, and the record shows that such families had a higher attrition probability than those who were enrolled in payments.

The reasons above explain why the initial attrition rates of Mincome were high (and perhaps higher than other experiments), but other reasons must be provided for the fact that the rates remained high for a long period of time.

VII.2 The Lagged Effect of Earlier Problems

One of the main reasons for my extensive study of the Baseline and the Enrollment interviews is the fact that the problems of Mincome during the enrollment process had a lagged effect on attrition during the early periodics. Consider Table 17 for the Winnipeg site, splitting the refusals

between those taking place before the interview and those taking place during or at the end of the interview.

Table 17

Refusals Before, During and After an Interview (Winnipeg Only)

	Number of Refusals Before the Interview	Upper Bound for the Number of Refusals During or at the End of the Interview
Enrollment	209	9
Periodic 1	71	23
Periodic 2	26	10
Periodic 3	32	13
Periodic 4	20	10
Periodic 5	10	6
Periodic 6	12	6

Unfortunately, the numbers in Table 17 are not accurate (thus the term "upper bound") since a refusal during or at the end of each interview was recontacted if the family was not classified as a "hostile". If such a recontact resulted in a second, outright refusal, it was classified as a refusal "before" the next interview. Thus, the numbers in the first column of Table 17 are overstated while the numbers in the second column are understated. Fortunately, this distortion does not appear to be very large. If one assumes that the refusal before the interview is influenced by the nature of the instrument at hand, it follows that the "lagged effect"

continued to operate until the fourth periodic where the refusals before the interview fall to the 10-20 range. The refusals during or at the end of the interview, which would indirectly and partly indicate the reaction to the instrument being used, drops to approximately 10 at the second periodic.

VII.3 The Mobility Problem

In earlier discussion it was pointed out that the fraction of attrition due to "Moves" is much too high at Mincome compared with other experiments. In general, it appears that the percentage contribution of "Moves" to attrition in the U.S experiments has been running in the 1% - 2% per year, while at Mincome it has been running at an annual rate of 3.0% - 4.1% per year. I believe an important explanation of this is the fact that the mobility rate in the Canadian Prairie is higher than comparable rates in the sites of the American experiments. However, an additional important reason is due to the rule that Mincome adopted according to which "Moves" are interviewed only if they are traced to be living within 200 miles of the nearest Mincome office. This is an unwise rule, given that the long-distance movers have considerable importance for policy makers. The figures in Table 18 indicate the dimensions of the problem. Table 18 reports the status at the time of this report of those declared as moves, but it must be assumed that some of these will change. What is not likely to change is the fact that out of the 171 moves reported here, 66.1% are due to moves out of the area, and a large fraction of these lost sample

points results from the "200 mile" rule. In my opinion this rule should be abolished and replaced by a rigorous program of interviewing remote movers. In addition, quick and more efficient tracing procedures should be instituted to reduce the volume of untraced moves and the consequent attrition from this source.

Table 18

Composition of Moves to Date (All Sites)

Interview	Weeks of Field Administration	Untraced Moves	Moves Out of Area (Address Known)
Periodic 1	102 weeks	5	17
Periodic 2	88 weeks	6	21
Periodic 3	74 weeks	6	21
Periodic 4	54 weeks	4	15
Periodic 5	42 weeks	13	14
Periodic 6	24 weeks	24	25
TOTAL TO DATE		58	113

VII.4 The Organization of Research and Operations.

Even if we cut down the attrition due to "Moves" from the 3.0% to 4.1% per year level, the total attrition rate during the first 16 months of the experiment would remain very high. Given the negative effects of the Baseline and Enrollment interviews, no major anti-attrition measures were taken by Mincome until mid-1975 when support

levels were substantially increased and a recontact program introduced. Also, later that year, a supplementary sample was added. Thus, some important explanation for the high attrition must be found in the way the organization itself functioned.

This being a scientific experiment, all major decisions should have been dictated by research considerations. During the life of a large scale experiment such as this one, hundreds of operational and procedural decisions had to be made, many of which should be made by researchers keeping in mind the statistical properties of the samples and the basic theory underlying the design. Within the organization there should exist a basic process by which the research team can give the operational body both continuous guidance to the resolution of specific operational issues as well as a broad sense of direction and priorities.

This was not the case in the Manitoba experiment. In the federal/provincial agreement, which established the general framework for conducting the experiment, research and operational responsibilities were divided. The federal and provincial governments shared responsibility for experimental design and research, while operations were solely under the authority of the province. Thus, the federal government and its research team had no control over the operational activities which were to have an impact on research aspirations. Also, the fact that the provincial organizational structure did not provide provincial researchers with direct control and responsibility for the day to day operational decisions which had research design and scientific implications, did not

facilitate the integration of research and operational activities. Thus, these less than optimum structures which left the research team without the necessary authority over operational activities that affected research plans, led to a somewhat programmatic orientation whereby early decisions were being made on operational grounds rather than following from design and research considerations.

In my opinion, Mincome Manitoba has experienced a very high early attrition rate because the "program" nature of the project gave more attention to administrative rather than research issues. If the organization had allowed stronger research direction with the power to evaluate the statistical and experimental implications of each procedure employed and every programmatic decision made, perhaps the non-completion would not have gone so far.

These observations relate to the past. There have recently been improvements in the experiment's procedures and organization of the research and operational affairs.

My remarks are intended to emphasize the fact that the four factors discussed here (the payment package, the lagged effects of the enrollment process, the "Moves" problem and the organization of Mincome) all have induced high attrition rates but have had no clear bias-creating distributional distortions on the sample.

VIII. CONCLUSIONS

In this paper I have tried to examine those aspects of the development of the Mincome sample which may be important to future researchers utilizing the Mincome data. Important problems were brought up which Mincome experienced in its early stages. However, we must remember that each one of the American experiments had its own problems and the critical book by Rossi and Lyall (1976), in addition to the current, critical discussions of this matter in the U.S., should provide a certain perspective. In relation to the questions which I raised in the Introduction, I conclude that the experiment was designed by the two governments in a sound manner and that proper survey instruments were developed and used.

We are left with the critical question of the scientific merits of the surviving sample. I have tried to study the attrition problem to find out if the attrition was caused by any systematic factor which would have eliminated certain groups related to some endogenous variable. Such a systematic attrition would have made it impossible to reconstruct the response surface from the surviving sample. I did not find such systematic factors. Instead, if appears that although the attrition problem has been severe and critical, the surviving data base is sufficient to enable the reconstruction of the response surface to treatment utilizing advanced statistical methods. This is the challenge of the next phase.

April 1977
Stanford University

Appendix A A Brief Account of the Enrollment Process in Dauphin

In order to initiate enrollment an extensive Screening interview was administered to all dwelling units in Dauphin. This interview was conducted between January and May of 1974. The list of addresses used was obtained by having surveys staff go along each street, avenue, etc., in Dauphin and make a list. The Screener interview was presented as an interview given by the Department of Health & Social Development, and did not mention Mincome Manitoba nor that there was to be a guaranteed annual income experiment. Because the Screener interview was administered before July 1, 1974, all of the respondents to the interview met the residency requirement for Dauphin.

The second interview was Baseline which was administered in the period from May to August, 1974 to all those households which completed the Screener interview and had a total annual normal income that was less than $9,000 and where both heads were not older than 63 years as of July 1, 1974. The information obtained by the Screener interview was used to determine if these conditions were met.

Because the information on the Baseline interview was more detailed than that on the Screener interview, the income requirement excluded some of the households that had completed the Baseline interview from the Enrollment interview.

The next step in the enrollment process was to send a payments enrollment process package to the appropriate reporting units. In the period from January to March, 1975 this package was sent to all of the

reporting units that completed the Enrollment interview. In March of 1975 the package was also sent to all of the reporting units who completed the Baseline, but not the Enrollment interview, for whom there was a complete address on file.

Included in the information that Mincome Manitoba made available during the enrollment process in Dauphin was a table of income values by family size where the income values were twice the support level rounded upward to the nearest $500, and where it was stated that the plan would be of interest to families with up to the income amounts given in the table reproduced below:

Family Size	Support Level	Ceiling
1	1,444	3,000
2	2,698	5,500
3	3,344	7,000
4	3,800	8,000
5	4,180	8,500
6	4,560	9,000

In conjunction with the Enrollment interview in Dauphin, Mincome Manitoba circulated an informational brochure on the Project. This brochure included the table of income values mentioned above, and mentioned that the net worth of the family was used in determining the payment and that the plan involved the reimbursement of positive income tax such that a family that was not entitled to Mincome payments could

still receive a partial reimbursement of their positive income tax. It was also stated that both the income and the net worth of a family must not exceed the stated ceiling if the unit was to be eligible for payments under the rules.

The payments enrollment package included a "General Information Handbook" which was plan-specific with a separate book for Dauphin. This handbook included the table of income values previously described. The text also mentioned the positive tax reimbursement and stated that a family could benefit from the plan until its income was approximately three times the support level.

The income level at which a family will cease to receive any payment from Mincome Manitoba is not the breakeven level but rather the tax equivalence level. The following table gives these levels for both support levels used in Dauphin in 1975.

Support Level	Breakeven	Tax Equivalence	Period When Used
3,800	7,600	9,736	75/01 - 75/06
4,400	8,800	11,664	75/07 - 75/12

This table applies to a family of four with two heads and with two children, both under sixteen years of age. To construct the table it was assumed that the only income is wage income received by one of the heads and that the net worth of the family was less than the basic exemption of $3,000. In addition, it was assumed that the head who paid the income tax claimed only the standard exemption and deductions.

The difference between the breakeven income level that was publicized by Mincome and the tax equivalence level below which units are still eligible for tax reimbursement may have led to a self-selection bias.

Some information on the monthly payments statistics for Dauphin in 1975 may be useful. This information gives the percentage of the reporting units with factor income (not total income) above tax equivalence, between tax equivalence and breakeven, and below breakeven. The monthly values have been combined into averages for the period from 75/01 to 75/06 and from 75/07 to 75/12 as these periods correspond to the periods when the two different support levels were used.

Period	# of Units	% above Tax Equivalence	% between Tax & Breakeven	% below Breakeven
		Not Self-Employed		
75/01-75/06	230	21	12	67
75/07-75/12	249	20	12	68
		Self-Employed		
75/01-75/06	96	15	2	83
75/07-75/12	103	15	2	83

In addition to the process of enrollment described above, the Dauphin site also allowed a process of walk-in enrollments under which a family or an individual can contact the Mincome payments office and request to be enrolled in the payments program. The applicant is required to complete a "Preliminary Application for Mincome Payments" form, which

obtains the family composition, the town of residence of each member as of July 1, 1974, and the addresses at which the family has been resident since January 1, 1974.

When the application form is complete and is received by the payments office, the reported residency in Dauphin on July 1, 1974 is verified by the payments staff by checking the 1974 voters list and the 1974 telephone directory. If these lists do not verify the residency, the applicant is requested to bring in two pieces of documentation that establish residency in Dauphin. If the residency requirement is not met, the applicant is not eligible for payments.

After a unit who initiates contact with Payments is deemed eligible with respect to residency and citizenship requirements, he is cross-checked against the master file. If the family had previously been enrolled, it is considered a recontact and must agree to complete surveys. Only if the unit had never been previously enrolled is it considered a walk-in.

If the unit is a walk-in, it is sent the enrollment package. When the first IRF is returned, the information is forwarded to the Payments Records Co-ordinator and a request for a decision on whether or not the unit should be added to the surveys panel is sent to Research. The unit will be added to the panel unless it is of no interest to research or if either head is over age 57. If the unit is added to the Panel, it must complete the required surveys if it is to remain eligible for payments.

Appendix B Further Information on the "Welfare Subgroups" In the Initial and Supplementary Samples

A portion of the supplementary sample consisted of households from "closed" welfare files. We wished to determine whether these households had a significantly different distribution of socio-demographic characteristics than did the households in the original sample with similar welfare experience. If they should prove to have a similar distribution of socio-economic characteristics, we would be more confident in accepting the hypothesis that these households came from the same population, inasmuch as the above socio-economic characteristics are highly correlated with other variables (whether observable or not).

The Variables for the Tests

The following four design variables were chosen to test for similarity in distribution of the two samples. Inasmuch as these are the four variables hypothesized (in the design model) to have the most significant effect on the response of interest (labor supply response, etc.), they also provide an indirect test of possible biases that could have been introduced by using a welfare subgroup as part of the supplementary sample (whose purpose was to supplement the original sample).

(1) Age of male head.

(2) Family size.

(3) Pre-experimental wage income (during the year prior to enrollment).

(4) Normal income cell (as assigned by research), and adjusted for a family size of four.

The Two Samples for the Test

The "welfare subgroup" of the supplementary sample consisted of all double-headed households who went off welfare in Winnipeg between January 1, 1974 and May 15, 1975. All of the above households selected for enrollment in the supplementary sample made up the first sample for the tests.

The "welfare subgroup" of the original sample consisted of all double-headed households who had been on welfare between January 1, 1972 and June 30, 1973. All of the above households selected for enrollment in the original sample made up the second sample for the tests.

The tests are thus based on 145 households from the original sample and 90 households from the supplementary sample.

Identical tests were also conducted on the portion of the above two groups who were actually enrolled in surveys (i.e., 127 for the original sample and 79 for the supplementary sample). The results of these tests from this portion were virtually identical and thus are not included here.

The Test

χ^2 tests of significance on contingency tables were used, which verify the hypothesis that the two samples came from the same population (i.e., column percentages are equal for each of the two samples). A significant χ^2 value for this test implies that the column percentages are significantly different and, therefore, that the two samples come from different populations with respect to that variable.

The following four tables give the frequency distribution of the above variables by the two welfare samples as well as the χ^2 values associated with each of the tables.

Comments on the Above Tests

(1) Table 1: The distribution of households in the supplementary sample "welfare subgroup" appear to be centered more heavily in the age category (25 - 34) and less in the age category (45 - 57) relative to the distribution of households in the original sample "welfare subgroup". However, the two distributions are not significantly different at the 1% level.

(2) Table 2: The supplementary sample "welfare subgroup" appears to contain fewer households with no children. The two distributions are, however, not significantly different even at the 50% level.

(3) Table 3: The supplementary sample "welfare subgroup" appears to have fewer households in the income range (0 - 2999) and more households in the income range (5000 - 6999). The two distributions are, however, not significantly different even at the 50% level.

(4) Table 4: The normal income cells (1 - 5) correspond to the same income ranges as those in Table 3 above (i.e., normal income cell 1 corresponds to a normal income level of \$0 - \$2999, etc.). The two tables differ for the following reasons:

(i) Table 4 is adjusted for a family size of 4, while Table 3 is not adjusted for family size.

Table 1

Age by Welfare Sample

Sample	Original Sample			Supplementary Sample		
Age	#	%	Cum %	#	%	Cum %
24	30	20.7	20.7	19	21.1	21.1
25-34	45	31.0	51.7	44	48.9	70.0
35-44	40	27.6	79.3	19	21.1	91.1
45-47	30	20.7	100.0	8	8.0	100.0
TOTAL	145			90		

χ^2 = 10.39 with 3 degrees of freedom, which is not significant at the 1% level.

Table 2

Family Size by Welfare Sample

Sample	Original Sample			Supplementary Sample		
Family Size	#	%	Cum %	#	%	Cum %
2	19	13.1	13.1	6	6.7	6.7
3	20	13.8	26.9	12	13.3	20.0
4	41	28.3	55.2	27	30.0	50.0
5	25	17.2	72.4	15	16.7	66.7
6	15	10.4	82.8	12	13.3	80.0
7	25	17.2	100.0	18	20.0	100.0
TOTAL	145			90		

χ^2 = 2.90 with 5 degrees of freedom, which is not significant at the 50% level.

Table 3

Pre-Experimental Yearly Income by Welfare Sample

Sample	Original Sample			Supplementary Sample		
Income Category	#	%	Cum %	#	%	Cum %
0-2999	49	33.7	33.7	25	27.8	27.8
3000-4999	22	15.2	48.9	15	16.7	44.5
5000-6999	28	19.3	68.2	23	25.6	70.1
7000-8999	23	15.9	84.1	17	18.8	88.9
9000-12999	23	15.9	100.0	10	11.1	100.0
TOTAL	145			90		

χ^2 = 2.91 with 4 degrees of freedom, which is not significant at the 50% level.

Table 4

Normal Income Cell by Welfare Sample

Sample	Original Sample			Supplementary Sample		
Normal Income Cell	#	%	Cum %	#	%	Cum %
1	50	34.5	34.5	4	4.5	4.5
2	30	20.7	55.2	17	18.9	23.4
3	30	20.7	75.9	49	54.4	77.8
4	30	20.7	96.6	20	22.2	100.0
5	5	3.4	100.0	0	0	0
TOTAL	145			90		

χ^2 = 43.88 with 4 degrees of freedom, which is highly significant.

(ii) The normal income cells theoretically reflect a more stable measure of pre-experimental income with the transitory components removed (i.e., concept of normal income).

(iii) Since the assignment of "normal income" has a significantly arbitrary component, it seems that the high significance of the χ^2 test should not be taken too seriously since it may be a result of different procedures of assigning normal income at the time of initial enrollment compared with the procedures employed at the time of enrollment of the supplementary case. For this reason the issue of comparing "normal income" of the two groups was not mentioned in the test. This matter should be further studied with some care.

References

Bawden, D. L. and W. S. Harrar [1976], "The Rural Income Maintenance Experiment: Purpose, Design and Operation," mimeograph draft, October 14, 1976.

Conlisk, J. [1973], "Choice of Response Functional Form in Designing Subsidy Experiments," Econometrica, Vol I. 41, No. 4, pp. 643-656.

Conlisk, J. and M. Kurz [1972], "The Assignment Model of the Seattle and Denver Income Maintenance Experiments," Research Memorandum No. 15, Stanford Research Institute, July.

Conlisk, J. and H. Watts [1969], "A Model for Optimizing Experimental Designs for Estimating Response Surfaces," American Statistical Association Proceedings, Social Statistics Section, pp. 150-156.

Hausman, Jerry A. and David A. Wise (1976a), "Social Experimentation, Truncated Distributions, and Efficient Estimation," Mathematica Policy Research, Technical Analysis Series, No. 4, November.

Hausman, Jerry A. and David A. Wise (1976b), "The Evaluation of Results from Truncated Samples: the New Jersey Income Maintenance Experiment," Mathematica Policy Research, Technical Analysis Series No. 6, December.

Heckman, J. [1976], "The Common Structure of Statistical Models of Truncation, Sample Selection and Limited Dependent Variables," Mimeo, University of Chicago.

Hum, D. and M. Laub [1977], "The Objectives and Design of the Manitoba Basic Annual Income Experiment," Mincome Manitoba, March.

Hum, D., M. Laub, C. Metcalf and D. Sabourin [1977], "The Sample Design and Assignment Model of the Manitoba Basic Annual Income Maintenance Experiment," Technical Report No. 1, Mincome Manitoba, Winnipeg, Manitoba, February.

Kershaw, D. and J. Fair [1976], The New Jersey Income Maintenance Experiment: Operations Surveys and Administration, Academic Press, New York.

Kurz, M. and R. G. Spiegelman [1972], "The Design of the Seattle and Denver Income Maintenance Experiment," Research Memorandum No. 18, Stanford Research Institute, May.

Peck, Jon [1974], "The Problem of Attrition," Vol. 2, Part C, Chapter 1 of Watts, H.W. and A. Rees (ed.), Final Report of the New Jersey Graduated Work Incentive Experiment, Institute for Research on Poverty, University of Wisconsin, Madison.

Pechman, J. A. and P. M. Timpane (eds.) [1975], Work Incentives and Income Guarantees, Brookings Studies in Social Experimentation, Washington, D.C.

Rossi, P. H. and K. C. Lyall [1976], Reforming Public Welfare: A Critique of the Negative Income Tax Experiments, The Russell Sage Foundation, New York.