

Longitudinal Designs for Housing Research

Institute for Social and Economic Research

**Faculty of Arts
The University of Manitoba
Winnipeg, Manitoba R3T 2N2**

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Greg Mason

Institute for Social and Economic Research
University of Manitoba
Winnipeg, Manitoba
R3T 2N2
(204) 474-9710

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INTRODUCTION

This report evaluates the use of longitudinal data analysis for housing research, particularly from the demand side. Of course, supply-side questions may also be addressed using longitudinal designs and the discussion below is pertinent in that context, but the focus here is the demand-side.

The report proceeds in the following sequence. First, longitudinal research is reviewed from the conceptual, data collection and analysis perspectives. The various types of data common to socio-economic analysis are considered with special emphasis on how such information supports causal analysis and policy design.

Second, particular problems in designing and collecting longitudinal information are reviewed. Special attention is paid to the design and administration of survey instruments (questionnaires), field operations and data base design and management. Respondent tracking and follow-up are also reviewed.

Third, the Manitoba Basic Annual Income Experiment (MINCOME) forms the basis for a case study. This research project ran from 1974 to 1979 and collected detailed socio-economic information from households in Winnipeg and Manitoba. Although the focus of MINCOME was on labour supply and work behaviour, data were also collected on housing. This case study provides insight into the problems and potential for longitudinal research in housing. The administrative experience of MINCOME, especially with respect to the definition of key concepts such as "dwelling unit" and "household", is examined.

The final section presents an overview of a research program in housing featuring longitudinal data. Significant advances have been made in the last decade in terms of concept and administration of longitudinal data and such a project is both technically feasible, policy relevant, and fiscally defensible.

LONGITUDINAL RESEARCHOverview

This section of the report explores the notion of causality which underlies all policy research. A major reason why longitudinal data are attractive for policy analysis is that they can support more sophisticated causal analysis than conventional cross-sectional data sets. It is important to stress that causal relations in social research can rarely be tested directly in the same way that a laboratory experiment with extensive controls can isolate treatments and effects. Rather the procedure is to create statistical models supported by assumptions which reflect a set of hypotheses. The statistical models then are used to evaluate the plausibility of these hypotheses within the context of the assumptions underlying the model and statistical tests. The ability to evaluate causal relations and draw inferences about cause and effect is a function of the number of untested assumptions which must be maintained by the researcher. Models which have fewer assumptions have greater power in identifying probable cause and effect. No model, even the randomized experiment, is completely free of these assumptions, although it has far fewer than models which are based on cross-sectional data. This is the basic rationale for the extra effort (and expense) of creating longitudinal data in the social and policy sciences and will be a constant theme throughout this report.

Typology of Data Designs

Quantitative data designs in the social sciences fall into a number of distinct classifications. The causal power of each design simply means the ability of the data, as defined, structured and collected to support causal inference. This is not the place to speculate on the nature of causality. Philosophers have provided much reflection on that point. For the purposes here, a simple notion of causality will suffice such as provided by Cook and Campbell (1979). Termed by them as a "critical realist perspective" this view asserts that causal relationships exist in the "real" world, but human perspectives are always flawed. The essence of causality is simply the notion that changes in the presumed cause will produce changes in the presumed effect. The causal relation is a construct of the human mind; evidence is employed to test competing theories. Causal chains can be convoluted (the immediate cause is frequently not the most relevant) and experimentation is the most powerful empirical method for evaluating cause.

Experiments versus Observation

A basic distinction is between experimental data in which the researcher usually separates subjects into a control group and one or more treatment groups and observational data. In the experiment, after an initial test to determine a benchmark, explicit changes are introduced into the treatment group and certain indicators are measured. The treatments are narrowly

than the generation preceding may seem to support a causal prediction - namely, that parties to the left of centre could encounter greater difficulty in obtaining political support over the next decade but this is a risky form of inference.

To begin examination of how various types of data can support causal testing, imagine a data set in which a dependent variable "Y" is associated with several independent variables "X(i)." In a linear relation (regression) the following statistical conditions must hold for cross-sectional data to allow causal inference from X(i) to Y:

- no effect of Y on any of the X(i);
- no omitted variables from the specification;
- the relationship among the variables is linear;
- the independent variables are measured without error.

This is a tall order and never fulfilled in practical terms.

Another assumption in using cross-sectional data to support causality is that differences between cases are the same as changes in levels of the variables. Thus, for a housing study using cross-sectional data, a causal model requires the assumption that the difference between two respondents' income and differences in housing consumption reflect what would happen if the lower income household were to have an increase in income.²

Longitudinal studies using observational data (successive interview of cohorts or panels) have several features which enhance their power to provide causal insight compared to cross-

² Cross-sectional demand studies produce estimates of long run elasticities.

defined, and in laboratory setting, the treatment and control groups are identical except for the specific changes in cause (such as drug level). Experiments usually endure over time and so by definition are longitudinal.

Observational studies may not be longitudinal; indeed, most are not. The most common social science data are cross-sectional. Typified by the opinion poll, these data represent individual or group attributes at a point in time.¹

Cross-sectional data have little causal explanatory power at all and have description as their sole rationale. Even though the description can be quite sophisticated and employ multivariate techniques, it remains nothing more than description.

Some analyses on cross-sectional data attempt to derive causal insight by probing for retrospective data, or by comparing "similar" people at different ages. For example, one might compare the attitudes of young adults with those several years older in an attempt to measure some change in attitudes. Thus the frequently reported phenomena that people tend to become more conservative over time have been supported by noting differing political attitudes on a cross-section poll. A recent trend showing that the young are now more likely to be conservative

¹ Technically there is no such thing as a pure cross-section. All opinion polls endure for at least several hours since everyone is not interviewed at the instant. A minor point for a short poll over the phone, this is a major problem for large-scale surveys in which interviewing can occur over days, weeks or even months. Aside from the fact that respondents may change in the face of a changed environment, interviewers usually do a different (not necessarily better) job after a 100 interviews.

sectional data.

- Dated variables (e.g., income) allow the assumption that change is unidirectional such that a change in income in period cannot influence home purchases in period T-1;
- The effect of omitted variables can be tested without actually having to measure them;³
- Change is measured from case to case, rather than inferred from difference in levels among cases at a point in time.

The assumptions of linearity are still required and time-lags do not always ensure causal direction. The latter is seen when using expectations and attitudes as part of the causal model; it can be difficult to separate the expectation of event from their realization such as in investment models.

An intermediate step in causal explanation is the non-equivalent control group longitudinal design. Popularized by Cook and Campbell (1979) is the "quasi-experiment" or the "pre-post design" where one or more of the independent variables are manipulated over time and resultant changes in the dependent variable are measured.⁴

Finally there is the full, randomized experiment which surmounts most threats to causal attribution. The laboratory experiment is the most common example and is a "pure" experiment in that only a single independent variable is manipulated - all others are presumably controlled. In this situation, changes in the

³ Not all models of omitted variables can be tested. For a discussion of this see Dwyer (1985) pp.328 - 330.

⁴ A very useful reference is by Trochim (1985).

independent variable are easily linked to changes in the dependent variable. The key maintained assumption is that all interventions aside from the treatment have been controlled. This may not be the case and it is rarely feasible to either test or ensure this assumption. Therefore it must be maintained without explicit test.

Less "pure" is the social experiment such as MINCOME where there is the standard division treatment and controls, but without the laboratory setting. Therefore the researcher must rely upon a statistical test to incorporate control over the experiment. Presumably, changes in the environment impact upon members of the treatment and control groups, but this assumption can never be made. As a result, social experiments must collect vast numbers of variables in addition to the measures of main interest. These are incorporated into statistical testing to provide a basis for uniquely assigning a measure of the effect of X on Y. MINCOME exemplifies the most complex design possible in several researches (aside from small group designing in psychology). An experimental design is combined with longitudinal panels to monitor the treatment effects.

A longitudinal design in a social setting requires that all relevant variables are:

- specified in the theory;
- measured.

Of course, this is required of all theory, but the fact that models are specified over time requires that specific change components are included. This is discussed in further detail below.

Causal Testing and the Passive Longitudinal Design

It is unlikely that many social experiments will be mounted in the future. The expense is considerable and there are ethical issues limiting the extent of manipulation.⁵ Academic researchers are unlikely to have the resources.

The observational longitudinal study is likely to be used and more frequently. Dissatisfaction with cross-sectional designs and the increasing access to administrative data sets (such as the Unemployment Insurance Commission data set) is also prompting heightened interest in panel designs.

There is a danger of exaggerating the power of the observational longitudinal study. While it represents considerable enhancement over cross-sectional data, it does not approach the fully randomized true experiment in terms of causal attribution. Any retreat from the full randomized experiment requires additional assumptions be explicitly introduced before causal modelling is possible. That is, the researcher is required to carry "maintained (untested) hypotheses" in order to test relations of interest. These untested assumptions are less stringent than enumerated above for cross-sectional data, nevertheless, they must be made explicitly. Typical of such assumptions are:

- the form of the function (linear vs non-linear);
- the nature of feedback, if any;

⁵ A universal result from all the social experiments is that the policy (treatment) variations were too small to produce effects of any magnitude in the three-four year program. The result is that all experimental effects are subtle and may easily be mashed by survey errors and other non-treatment effects.

(Note feedback is a social situation which only makes sense with temporal data.)

- no panel conditioning and absence of sample selection (especially attrition).

Note that these assumptions also pertain to cross-sectional data but the existence of feedback is simply not at issue in cross-sectional data since the variables are not dated. Therefore that assumption remains untested and untestable in the context of that type of data. This is apparent when structural models involving feedback are imposed on cross-sectional data.

Pooled cross-section, time-series data can evaluate feedback effects with greater plausibility. In the United States where administrative boundaries permit the distinction of 50 cross-sections (states) and a decade or two of time-series data may be available in each state, the data can support quite complex models which produce estimates that lie somewhere between those produced by a pure time-series and a pure cross-section. In many instances such estimates are more useful for policy purposes such as developing estimates for various demand elasticities. In Canada, administrative realities can preclude developing such information. Aside from the small number of cross-sections (provinces), variations in data collection practice often subvert analysis and produce inconsistent data among provinces.

What observational longitudinal data allow is for certain models to be specified and tested. The process of postulating an explicit model and then testing it against the data is a much

less powerful method for assigning causality than the explicit exercise of measuring the correlation between a truly randomized independent variable and the measured effect. Dwyer(1983) cites one important early example of mistaking the power of longitudinal designs to isolate causal relations. Termed "cross-lagged" correlations, Figure 1 below shows a typical two-wave panel design. In an early perspective Kenny (1975) argued that if there was a difference between the cross-lagged correlations ($\text{Corr } X(1), Y(2)$ and $\text{Corr } Y(1), X(2)$), this was sufficient to assign a causal relationship between X and Y - in particular, a lagged causal relationship between X and Y. In fact, this only eliminates a few spurious correlational models; many others remain plausible.

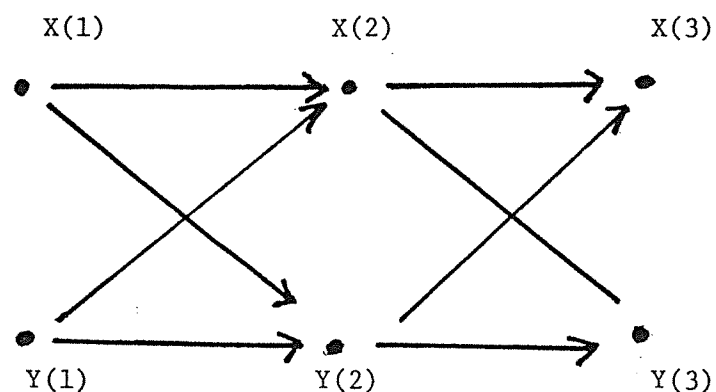


Figure 1: Cross-lagged Correlation

As Dwyer(1983) stresses, there are many competing hypotheses which are plausible with most longitudinal data and only by specifying a causal (structural) model in detail is it possible to initiate a process of systematically eliminating false models. The term "causal" model has assumed a distinct meaning in social science. Combining elements of psychometrics and econometrics, causal models are based on the work of Joreskog and Sorbom (1977). The model structure is divided into two general components - a measurement model (factor model) and a structural model (a system of simultaneous equations). Econometricians are familiar with the structural model while psychometricians are familiar with the measurement model. The key to the contribution of Joreskog is to unify a factor analytical framework where latent variables (such as consumer confidence) are related to measurable variables (such as responses to questions regarding the desirability of purchasing a house) with a structural model which could include the latent variable (in this case expectations) would be an independent or dependent variable. The entire system is specified and estimated using maximum likelihood techniques. Key to this type of analysis is the notion of change in a longitudinal context. When is a change in the dependent variable, the result of changes in the independent variables, and when is it the result of changes in unmeasured variables?

Measuring Change

A simple approach to analyzing longitudinal data is to measure change-over time and correlate such variation in the dependent variable with changes in the independent variables. This can produce problems. For example, if there is no measured change in the dependent variable, this does not necessarily imply stability. There are a number of "forces" bearing on longitudinal social data. These must be carefully analyzed and isolated from any policy intervention.

1. Temporal Stability

One feature of some social processes is stability, where an attribute remains stable throughout the measurement period. For example, many households would exhibit a constant rate of increase in networth. While some would not follow the pattern, most would, and models using this variable may find correlation between changes in networth and housing demand.

2. Temporal Drift

Finally, it is common for some socio-economic variables to systematically increase (decrease) over time. For example, in the MINCOME data set there is a systematic tendency for respondents in the sample to become better off through time. This gradual increase in absolute economic welfare is demonstrated by a larger number of homeowners after 11

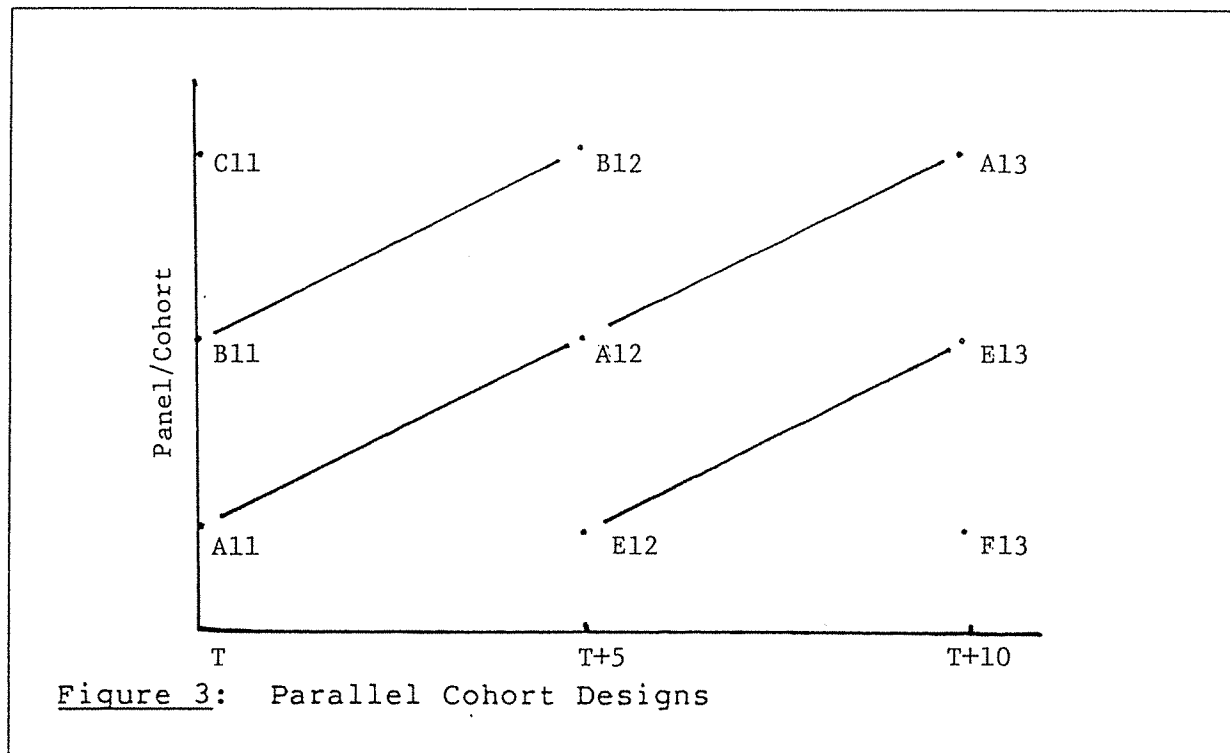
surveys than were measured initially.⁶ An essential component of all causal models, especially those involving change over time, is a stochastic component. Often the dependent variable (e.g., economic welfare, or home purchase) may change without commensurate change in the dependent variables. An error term is essential for modelling this "drift" and not misassigning it to any change in an independent variable. Dwyer (1983:328) has a useful review of this particular point.

Typology Data Designs in the Policy Sciences

By way of summary, it is useful to visualize the types of data encountered in the social sciences. In some cases, the models would not apply to housing research - in other situations and as stressed in this report, possibilities may exist in data structures not commonly employed.

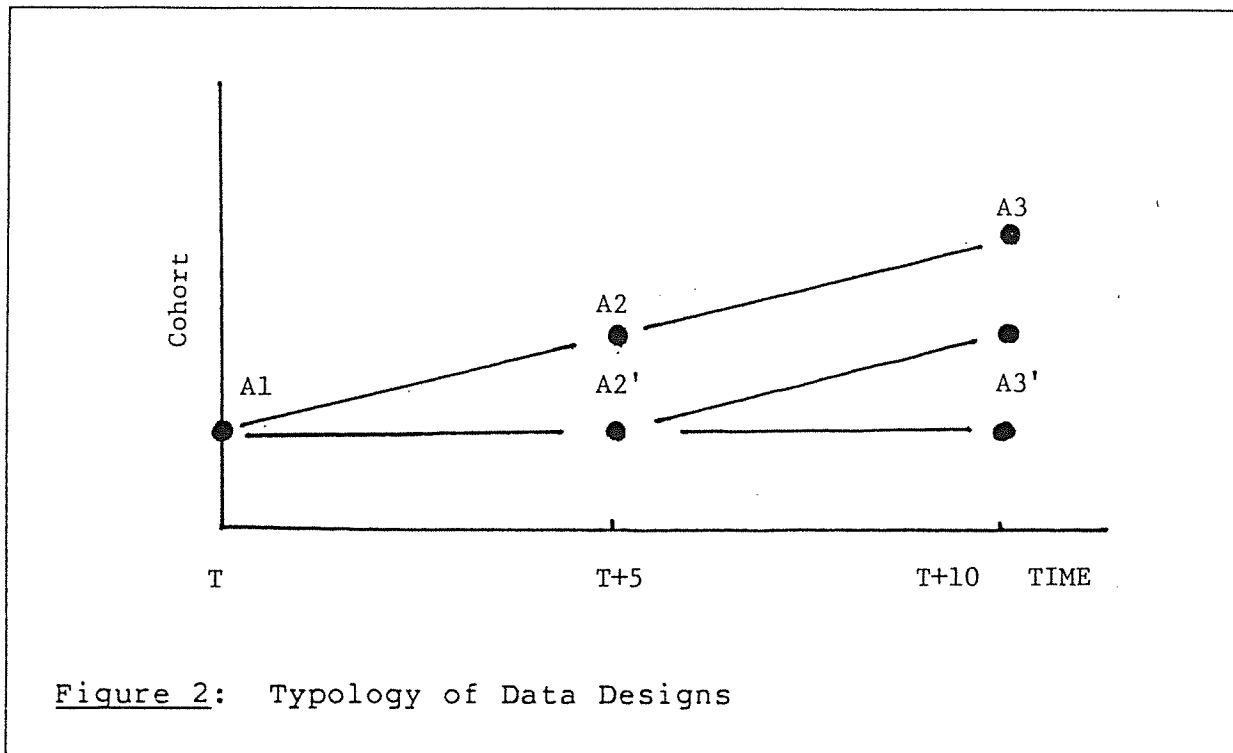
Consider Figure 2 below. An initial cross-sectional survey is specified as A1. This could be random sample of the general population, or it might be a specific subsample such as 15 year olds or renters. At this point (time = T), the data are representative of most surveys done in housing.

⁶ A useful recent issue of the Journal Of Econometrics (vol. 18, no. 1, January 1982) which is devoted to the analysis of longitudinal data. Heise (1975) is a basic reference and Janson (1981) is a recent review of techniques. Markus (1984) also provides an analytical perspective on panel data. Dwyer(1983) remains the most pertinent and accessible account for the policy analyst.



In this study, it is possible to undertake comparisons between two groups, say A17 and B17 which would compare 12 and 17 year olds at a point in time or new home owners at different stages in their life-cycle (say 5 years apart in the age of heads). This type of paired cross-section provides some insight into causal processes, under the assumption that differences between the two groups are a function of intrinsic processes (life-cycle consumption) and not extrinsic forces (interest rates).

If the same respondents are resurveyed then a "panel" design is obtained. This recontact of identical respondents provides a superior basis for specifying causal models, as long as panel conditioning and sample selectivity do not distort the sample.



Also imagine that this initial group consists of a particular age group.⁷ If this population is not to be followed and a resampling is to occur at a subsequent time period, the design is referred to as a "cohort" model. For a general population survey the term cohort is a bit too rigorous, but for a sample of 15 year olds, the concept is very useful. Reinterviewing this "cohort" is a very common form of longitudinal survey. A major concern is the fact that not all respondents will be retained in the sample: some will have died, others will move out of the country and still others will refuse to continue in the study. This "attrition" is a basic issue in longitudinal designs and is

⁷ Imagining that there are distinct age groups is necessary to demonstrate the difference between panel and cohort studies.

examined in the next section.

Continuing the cohort study will produce further samples in year $T + 5$ when the participants are 19 and in year $T + 10$ when the participants are 24. This type of cohort study is very common in health research which seeks to discover the impact of diet, exercise and other long-term factors on the incidence of disease. The cohort of specific age groups "ages" naturally which is reflected in the upward slope. It is important to stress that the cohorts identified as A2 and A3 are resamples of the general population which identify 19 and 24 year old respondents. Often, those who were previously surveyed are excluded.

General samples which are drawn from the general population would not follow this cohort "slope" and are portrayed on the horizontal axis as A1', A2', etc. This is known as pooled, cross-section, time-series data and is common in economic research.

One of the defects of the cohort study is that considerable time must pass before "results" are available. This is unavoidable in research which examines human development or disease processes which may take decades to emerge. For policy studies this time frame is unacceptable. One device to increase early research results is to undertake two or more cohorts which run parallel. This is shown below in Figure 3.

This typology applies to both panel and cohort studies. More precisely a cohort study involves tracking members of a group such as homebuyers between the ages of 25 and 35. Each wave involves a resampling.⁸

A panel study involves tracking the same people over time. This is what is normally considered when using the term "longitudinal analysis". The next section of the report reviews longitudinal data in some greater detail, with emphasis on issues such as attrition, panel conditioning and procedures to recontact respondents.

Summary

Causal analysis, the basis for policy research, can be supported to varying extents by all social science data set, provided the models are supported by assumptions. Beginning with the most restrictive data sets, cross-sectional information, the assumptions required to support causal testing in the context of a linear regression model are as follows:

1. linearity of functional form;
2. feedback effects are non-existent (instantaneous diffusion);
3. independent variables are measured without error;
4. changes between levels of the dependent variable (i.e., differences between cases) are equivalent to changes over time;

⁸ If the population is small and there is sufficient time between surveys (i.e., more than a year), this exclusion is often dropped.

LOGISTICAL ISSUES IN LONGITUDINAL RESEARCH

Overview

All social research requires attention to logistical details. In cross-sectional polls, factors such as interviewer effect, close monitoring of interviewers, pre-testing questionnaires, random selection of respondents, etc., must all be executed. In longitudinal designs, all the usual care and attention common to cross-sectional research must be present, but some unique problems emerge. In addition, other features of the research process undergo a transformation and factors which, while present in cross-sectional research, become even more important to control and monitor in a longitudinal context.

This chapter briefly reviews some of the major logistical issues in survey research. Next, the changes imposed by longitudinal designs are considered. Finally, issues unique to longitudinal research are considered.

Logistical Issues in Survey Research

Many texts and references which provide guidance on logistical issues in survey research⁹ are available. Of particular importance are the following:

1. Interviewer Effects

Interviewers vary considerably in ability to elicit information which is unbiased. Needless to say, all survey projects must provide sufficient training to ensure

⁹ Anderson, Rossi and Wright (1984), Labaw (1981), Dillman (1979) Turner and Martin (1986) and (1982) are recommended.

5. all unobserved variables do not mediate the relationship between independent and dependent variables.

Panel designs allow relaxation of some of these assumptions. In particular, assumptions 2 and 5 can be incorporated into the model and tested directly. Especially useful for policy purposes is the ability to allow diffusion to occur (assumption 2). In an important sense, assumption 6 is also modified. In a social process, it is unfeasible to obtain data on all aspects of decision making or human behaviour. By following the same individuals over time, unmeasured (and possibly unmeasurable) variables such as genetic inheritance or early cultural environment are better controlled. With pooled cross-section, time-series data, it is possible to test for feedback effects. Changes in levels can be observed through time, but unmeasured attributes of the units of analysis are not well controlled. It is for these reasons that panel data are finding wider acceptance in social and market research.

The next section considers some logistical aspects of survey panel data.

that once fieldwork begins interviewers obtain information in similar ways. For example, instructing interviewers in neutral and informative probes is timeconsuming and difficult. In obtaining information about housetype, it is important that interviewers all clarify in the same way. The response "one storey" (if provided over the phone) needs to be clarified, but on many surveys, some interviewers will probe for a structure type, others may not be able to obtain the response within the correct frame. On occupation, respondents will frequently indicate place of work and not type of work. Again, interviewers may vary considerably in their ability to elicit correct information.

The absence of interviewer effects is a major advantage for mailout surveys in which respondents all must react to the question as written. Of course, while interviewer effects are removed, mailout surveys require literacy. Also, text in French and English may vary considerably in true meaning which may be clarified by skilled interviewers. Mailout surveys often gloss over semantic differences which exist in the two official languages.

2. Questionnaire Design

Perhaps the most important activity in survey research is the questionnaire design. The usual steps in developing a questionnaire are:

- Specification of Themes

This step begins with the itemization of central points in the questionnaire. Usually this is the listing of descriptive requirements although, in some instances, there is a theoretical basis which will inform the question formulation process. If the survey is purely explorative, then the relation to theory is weak. As the theoretical basis strengthens, so does the way in which the questionnaire "maps" into theory.

- Question Formulation

Frequently the initial questionnaire is developed by a single author, but eventually the pre-testing process requires that others examine the structure and content of the survey. Essentially at this stage, the questionnaire will move through an expanding set of expertise. These experts are useful in ensuring that the questionnaire develops the themes required, but two problems usually begin to emerge. First, the questionnaire process becomes circular as phrasings are honed and enhanced. Second, experts are useful in developing the general content of surveys, but are frequently poor at developing the exact phrasing.

Other important problems emerge in this formulation stage. Not only must the phrasing be refined, context effects must be examined. A common practice is to start with demographic questions. This can work, but

it is better to initiate with some interesting and easy questions. Demographic questions can be viewed as personal by the respondent (e.g., income), so these are usually best left until the end of the survey or tackled throughout the questionnaire.

Some respondents are reluctant to begin a process which they feel unqualified for and so it is useful to begin with simple questions to encourage participation. This is especially true for seniors and immigrants.¹⁰

- Pre-testing

Most surveys are pre-tested under actual field conditions. This tends to consist of administering a small number of (25 - 50) questionnaires to the target population. This will usually reveal clumsy wording and difficulty with interviewing, but will usually not reveal problems in misinterpretation on the part of the respondent. Belson (1984) has presented a number of case studies in which respondents were queried after completing a survey. Specifically they were asked in detail about their interpretation to the question and its content. The important aspect of Belson's research is that about 30% of respondents typically fail to understand the intent of the question. In some cases, 55% had not comprehended the question. This is a sur-

¹⁰ Immigrants can also be fearful of surveys and may either refuse altogether or provide responses too swayed by social desirability.

prising finding, especially in light of the apparent simplicity of the questions. Indeed, many experts are amazed to learn that the general public has a completely different frame of reference on issues.

The typical survey budget rarely allows for complete pre-testing in which respondents are debriefed through the process of questionnaire design. Allowing for this full debriefing would be a major improvement in survey practice.

3. Sampling

Many survey research texts concentrate upon sampling. In addition to simple random sampling and systematic random sampling, stratified and cluster and even more complex designs are common to many surveys. It is useful to emphasize that most surveys employ a simple random sample or a systematic sample. Complex designs are used to provide "point" estimates such as the unemployment rate, but for policy work these sampling approaches have some limitations.

Stratified and cluster samples can assist in allowing more efficient¹¹ estimation, but usually at the expense of complicating inference and testing complex nested hypotheses typical of causal models. Even the estimation of sample means and variances is made more complex by stratifica-

¹¹ Efficiency translates into fewer sample points for a given level of precision (variance) or lower variance for a given sample size.

tion and imputing population attributes from a sample can be difficult. The application of multivariate procedures is often very difficult, yet in policy research this is precisely what is required.¹²

The simple conclusion is that complex sample designs can and usually does complicate analysis, especially that which seeks to develop causal links. The example of MINCOME provided below is especially instructive.

4. Time Lapse

Most cross-sectional surveys are presented as if the data were all collected at an instantaneous point in time. This is never the case. In a telephone poll of 5 minutes, good field operations permit up to 600 interviews in an evening (between 5:30 and 9:30 in the evening), and for a survey of 1200 respondents there will usually be nothing to interrupt the validity of response over the two days.

Experience and research have indicated that day-time interviewing is less productive (i.e., fewer completions per contact) than evening interviewing. There is little difference in productivity during the support hour compared to later in the evening. Interviewing can also be under-

¹² The explanation for this is that stratification and cluster samples can introduce non-orthogonalities in the data. When the object of the analysis is simply to derive estimates of means and variances this presents no serious problem. In multivariate analysis, derivation of regression coefficients or factor loadings, to cite just two examples can be quite difficult. In fact, many analysts ignore this feature of stratified samples and treat them as if they were derived from a simple random process. This ought not to obscure the fact that this is usually wrong.

taken every evening and during the days on weekends with no substantial differences in response rates.

Cross-sectional surveys which endure for longer periods than a few days can encounter two threats to internal validity. First, the quality of field operations can change throughout the course of interviewing. Typically interviewers get better and are able to present the survey more effectively. Against this, there is a tendency for supervision to become a little more "slack" when the survey is "in the bag." That is, once it is evident that sample size targets are going to be met, it is tempting to ease back on quality control. Also, interviewers can become fatigued on a long survey, especially if the work is a second job.

Second, external events may intercede to change attitudes. For example, surveys seeking public attitudes on current policy can be influenced by events such as a political speech. Rather than invalidating the research and provided there is enough sample prior to the change, it is often possible to model the effect of this unplanned intervention through a pre-post design. Such an intervention is serendipitous and survey research with sound field management can take advantage of the situation.

At times interventions are difficult to place. Even a major political event only slowly seeps into public consciousness over several days. In a national housing survey

where the field operations endure for several months, changes in mortgage rates could alter purchasing intentions throughout the course of the survey. Again, such events may be serendipitous but in this instance, where the "intervention" is spread through time, modelling its effect is difficult.

5. Refusals

All surveys encounter refusals. The pattern of refusal is often believed to be conditional on the form of the survey. Thus, mail questionnaires are frequently downgraded because many have had poor results with return rates as low as 15% being common. However, as Dillman (1979) has shown, return rates of 75% and higher are possible with a mail survey if proper design is used. It is unwarranted to compare the results of a mail questionnaire with one mailing and an in-person interview in which there may be as many as 6 callbacks. The underlying response pattern is probably the same regardless of survey format, with differences reflecting only slight biases in the sample frame.¹³ As an example, consider the role of follow-up. In mail surveys, response rates of 70% are normal using three follow-up contacts. Telephone surveys typically encounter 15-35% refusal rates depending on content and interviewer capability. The in-home interview will usually feature a letter prior

¹³ For example, mail surveys presume literacy, in-person interviews tend to fall off in quality in inner-city areas and telephone surveys are confined to those with phones.

to contact and also has a refusal rate of 20%, depending on survey content. Refusal rates can be reduced by follow-up, respondent contact in advance of the actual survey and attempts to sell the validity of the survey both to potential respondents and to "soft" refusals.¹⁴

There have been many logistical advances in survey research during the past decade. Some methods, previously thought weak (such as mailout questionnaires) are now very attractive in terms of validity and cost especially for housing research.

Logistical Issues in Longitudinal Surveys

While cross-sectional surveys have logistical requirements as outlined above, longitudinal designs typically impose greater restrictions or raise unique problems altogether. This section outlines the types of problems typically found in designing and conducting longitudinal surveys. These issues are important, for failure to appreciate the special requirements of longitudinal research frequently means that budgets are not comprehended or that unreasonable expectations are placed upon researchers to deliver research products.

Self-Selection:

All surveys suffer from self-selection and this general problem is prevalent in longitudinal designs. Composed of the simple total refusal to the entire survey, item refusals to specific

¹⁴ A "soft" refusal is a respondent who disqualifies because of perceived lack of knowledge or time. A "hard" refusal is usually expressed as complete indifference or hostility.

questions, or attrition, where respondents are not retained in the survey after several panels, self-selection is an insidious force which erodes the inferential basis for panel data.

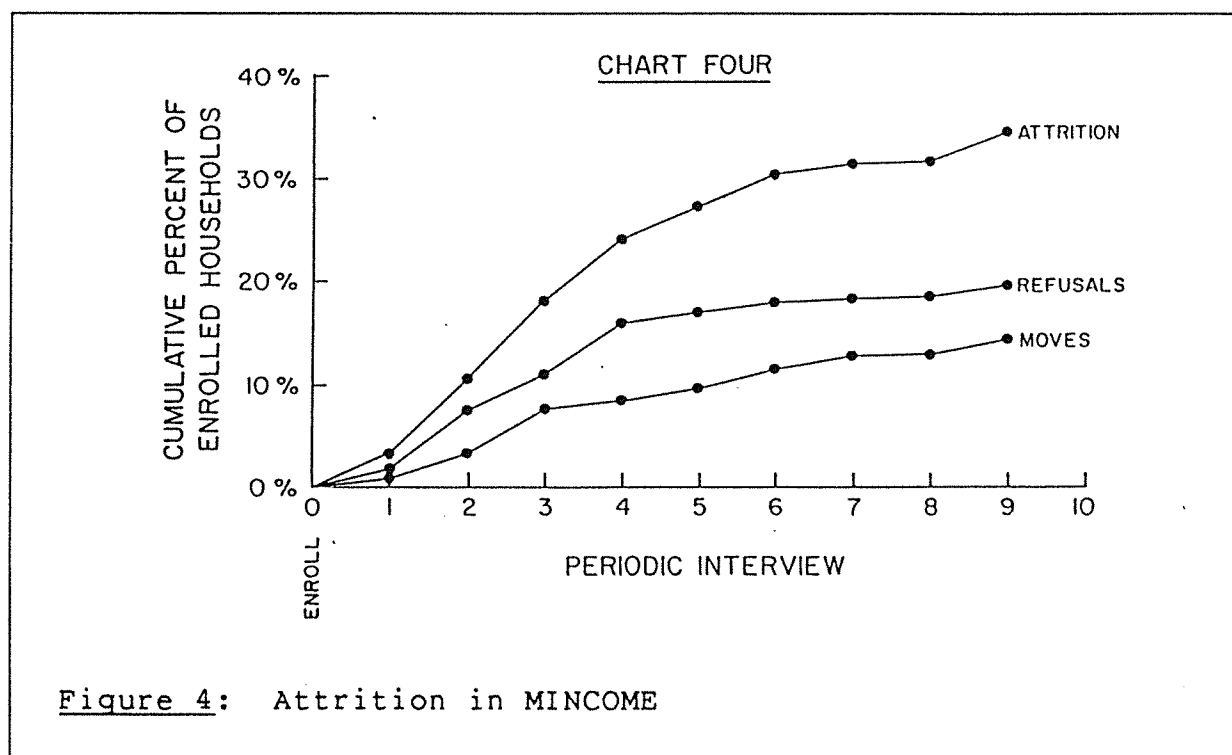
The initial refusal to the entire survey is always serious, but it is particularly important for longitudinal designs. Self-selection is non-random; that is, those who refuse to participate are usually different from those who agree to be interviewed. Typically on general polls, men refuse more than women, and urban residents more than rural residents. For a panel study the initial self-selectivity may be reinforced at each wave. For example, if a survey encourages respondents from a particular socio-economic group not to participate, after several waves the panel may be completely irrelevant to the target population as this group is repeatedly underrepresented.

Despite the fact that respondents were initially selected randomly, refusals destroy this randomness and the sample loses its ability to support inferential analysis. This feature of all surveys is insufficiently stressed either by academic researchers or professional pollsters, yet it is possibly the most serious shortcoming in the technique.

Another form of self-selection is item refusal in which a respondent declines to provide information to a specific question. At times this may be indicated explicitly such as "I do not want to answer that" and at other times respondents indicate they "Don't Know." This second form of item refusal is more important and damaging than the honest and explicit refusal to

respond. Item refusals are common to all surveys, but in longitudinal work the damage can decline over time. A respondent may grow to trust the interviewer and be prepared to provide responses after several surveys. In some cases, such as income, this information can be imputed to previous panels and the problems with item refusal may be surmounted. Questions relating to personal attitudes cannot be recovered so easily.

One form of self-selectivity of major concern to panel studies is attrition. Not all panel respondents are maintained in a study. An example of attrition is shown below in Figure 5 which depicts the attrition in the MINCOME study.



Several aspects of the attrition pattern are interesting. First, most of the attrition occurs in the first three waves. It is typical that attrition is initially high, then it tapers. The frequency of recontact is an important variable in determining the attrition pattern. A recontact every five years is likely to produce much more serious attrition patterns than panel designs which follow respondents every six months. Second, attrition is non-random. That is, those leaving the experiment are different from those who remain. For example, in the MINCOME experiment, those leaving the experiment prematurely tended to be responding to employment opportunities in western Canada and also tended to respond to labour market inducements. They were also less willing to be reinterviewed every three or four months. Those remaining in the experiment had stronger family ties, were reasonably secured in their employment or were willing to be reinterviewed.

Self-selection bias is a major problem in panel research, however, it is merely an aspect of all survey research. It tends to be more prominent in longitudinal work because attrition can be quite startling to the uninitiated, and also because most surveys are a little too discreet about refusal bias in general. Recent advances in statistical modelling of attrition bias offer significant hope that causal modelling and policy analysis can successfully proceed in the context of attrition and other sample selectivity problems.¹⁵

¹⁵ See Heckman (1979) for the standard treatment of the issue.

Panel Conditioning

Respondents who are recontacted repeatedly may begin to provide socially desirable responses or adopt socially desirable behaviour. This can be caused by interviewer effects, or because the context of the survey encourages a modification of behaviour. In panel designs stretching over several years, the published results of the research can influence behaviour. Finally, many participants in panel research come to view themselves as an elite and modify their behaviour accordingly. All of these effects are known as panel conditioning. Respondents who are repeatedly asked about family budgets could start to balance their cheque books or develop a regular budget where no such planning was undertaken previously. This increased budgeting may also change consumption patterns and soon the survey is conditioning the respondent. A survey of renters probing for home purchase intention may well encourage such behaviour before it otherwise would have occurred.

Such panel conditioning is an important form of bias in longitudinal research. For this reason, researchers often argue that aside from attrition, it is a sound idea to replace respondents and "refresh" the sample. A design which replaces a number of the panel respondents with each survey is known as a rotating panel and is the basic design employed by the labour force survey.

Another method for controlling panel conditioning is to conduct cross-sectional surveys and compare selected variables with

those obtained from the the longitudinal sample. This provides a basis for comparison and permits the researcher to evaluate the extent of conditioning.

Data Base Design

The easiest way to view a longitudinal design is as a series of linked cross-sectional surveys which happen to use the same respondents. It is tempting to believe that issues of archiving the data and developing a framework for analysis can be delayed until the surveys are designed and in the field. This is a crucial mistake. Data base design is integral to all phases of survey research and must be undertaken at the outset. There are several reasons for this, not all are based in scientific principles. Some general issues in the role of data bases in research include:

1. First, all surveys require a process of verification and validation. Verification is the process whereby the collected data are compared to the respondent data. This occurs first by randomly reverifying responses prior to data entry and then randomly comparing a sample of the machine readable data with the information recorded on the actual survey instruments. If some predetermined percentage of responses have errors (usually less than 2%), a substantial error correction process must be undertaken. A data base framework is essential in the verification process. It allows fast record retrieval and correction. In

most cases it is wise to have comprehensive error detection and verification process.

Validation is the process of detecting inconsistent data. This may or may not be caused by errors in recording by interviewers or data entry, or incorrect reporting by the respondent. Validation arises in three general contexts:

- out of range values;
- cross-sectional consistency in which records must logically relate to each other at an instantaneous point;
- longitudinal consistency in which data must logically relate over time.

Cross-sectional consistency exists when the wage earned by a welder is not greater than that earned by a surgeon. Longitudinal consistency ensures that the recorded age of respondents over time increases in step with the time sequence of panels.

2. Data base frameworks are essential for monitoring field operations. Technology now provides the capability of entering verifying and validating data soon after collection.
3. Data base frameworks also allow closer monitoring of field operations. Often interviewer effects or systematic mistakes can be detected if data are tabulated and analyzed while most of the survey is still in the field.
4. A data base framework allows initial results to be available throughout the field operations. This allows

researchers to detect errors but more importantly it permits the sponsor to obtain early insight into the results. A common mistake made by longitudinal research programs is failure to provide preliminary release of results. The perceived delay in the product of longitudinal research is a major obstacle to securing support for this type of data collection and analysis.

Data base systems have become increasingly sophisticated. It is important to distinguish between the data base needs of a bureaucracy, such as a hospital, and the needs of research. The hospital bureaucracy makes frequent access of the data for single pieces of information. The researcher makes infrequent but quite large requests in which a group of records are extracted and created as a "flat" file for statistical processing.¹⁶ Relational data bases, ideal for extracting information in small "bits" from a complex and interrelated structure, are often unsuited to the preparation of flat research type files. Although this may seem to be a trivial point, it is possible to overdesign a data base framework and produce a product which simply does not fulfill verification/validation or research needs. This was a common mistake in the income maintenance experiments and an important factor in the delay experienced by MINCOME in releasing research results and verifying data.¹⁷

¹⁶ A flat file is the conventional data matrix in which variables are in columns and cases in rows.

¹⁷ A detailed explanation of data entry and quality control operations of MINCOME is found in Rasmussen, Anderson, Wright and Sang (1983)

Tracking Respondents

Recent refinements in respondent follow-up have greatly enhanced the integrity of longitudinal panel studies. In one context, the panel study is nothing more than a very long survey interview. Maintaining respondent contact is a process of ensuring respondent allegiance to the aims of the study in a somewhat analagous form as ensuring respondent attention in a conventional survey.¹⁸

Of course, during the intervening periods between panels, even the most committed respondent will have other things on his/her mind. Therefore, the basic dicta for respondent retention are quite straightforward.

These basic principles are:

1. Simplicity

The tracking task is confined solely to that function and is not burdened with data collection. That is, do not require the "tracker" to also conduct interviews or undertake data entry functions. Focus on the tracking tasks will tend to producing superior results than splitting this function among the research group.

2. Multiplicity

For large national studies, multiple (regional) tracking centres should be used since they cut down on costs and those with "local" knowledge are likely to have more suc-

¹⁸ A recent work by Call et. al. (1982) provides a useful overview of tracking methods from which this section draws heavily.

cess than a national office.

3. Diversity

More tracking devices or approaches will increase success. One useful analogy is of sifting sand where finer screens are employed to approach more remote respondents.

4. Automation

A parallel data base system for maintaining respondents is essential. Although clearly linked to the main data base, this respondent data base must stand alone and must be "relational."¹⁹

The initiation of a tracking operation requires very careful planning. An essential requirement is that respondents must provide names of immediate family, close friends and current associations. This last source of information is especially useful if respondents are professionals, or engaged in activities such as amateur athletics.

In addition, respondents who are enrolled in the panel should be "sold" on the importance of the study. Periodic recontacts and requests to provide new addresses will then tend to be honoured more often and reduce the tracking effort.

Three approaches to tracking are: mail, telephone and community visit. The first is cheapest, but tends to produce the lowest recovery rate, especially for respondents who have been missing

¹⁹ The discussion before has stressed the virtues of a data base management system which produces flat files. For respondent tracking where a key is to attach to each respondent, a comprehensive vector of significant "others" (friends, family, etc.), a relational data base strategy is superior.

for some time. The community visit is the most expensive but works best for smaller samples where the extent of the interview requires detailed information. That is, the data requirements of the study are high and the value of information compensates for the cost.

Tracking by phone is a good compromise, but is limited to published directory sources, or telephones of friends and relatives which may be wrong if the tracking is done two or more years after the last contact. It is not uncommon for the tracking costs to equal the interviewing costs implying that longitudinal research can easily cost 50% more than conventional surveys. If one takes \$100 per interview as a norm for a local survey with a 45 minute interview, a longitudinal wave can easily average to \$150 per interview and will go higher as the panel "ages."

Information sources for tracking respondents are crucial. Some such as friends and relatives have been mentioned. Others include department of motor vehicles, credit bureaus, voting lists, schools, etc. The tracking strategy needs to be well defined at an early stage in the study since much of the information for these tasks will emanate from the initial contact. Not only must the respondent be "sold" on the participation, but so must friends and relatives. Later, if contact with the respondent is lost, these people need to feel comfortable in providing the "tracker" with information on the whereabouts of the respondent.

Finally, tracking requires great patience and ingenuity. It is a certain type of person who enjoys the "hunt" and combines that instinct with a sensitivity to the ethical issues in locating respondents. Special care is required in creating the "tracking group" and constantly empowering them to employ all data sources. Access to data bases and protection of respondent privacy are central issues to all survey research, but the tracker who accesses friends, family and other sources of information must also be very careful about ethical boundaries.

Application of modern tracking methods, and ensuring that this function is well funded, results in maintenance of over 90% of respondents over a prolonged period of time. Some follow-up studies have been able to locate 100% of respondents interviewed 10 years previously. In many cases a certain amount of luck is needed. The basic rule is simply to have repeated and frequent contact with respondents and to organize the tracking function with military precision.

Summary

Longitudinal survey research requires the usual attention to sound research design. It places added stress on questionnaire design and validity as well as the need to have a refined data base framework. Of special importance are attrition and panel conditioning. Recent advances in respondent tracking have greatly enhanced the viability of panel studies over relatively long time periods.

SELECTIVE REVIEW OF HOUSING RESEARCH USING
LONGITUDINAL PANEL DATA.

Overview

This section presents an overview of housing research using longitudinal data with the intent to indicate possibilities for analysis. The review is not exhaustive and concentrates upon the income maintenance experiments and the Panel Study of Income Dynamics (PSID) sponsored by the Institute for Social Research at the University of Michigan. After reviewing the structure of the experiments and PSID, the major studies emanating from these data sources are summarized.

Income Maintenance Experiments

In the late sixties a number of income maintenance experiments were undertaken in the United States to evaluate the probable impact of various adjustments to social welfare, in particular, the negative income tax (generally synonymous with the term "guaranteed annual income). These experiments were funded at several locations in the U.S. such as Gary, New Jersey, Iowa, Seattle and Denver (these last two sites were amalgamated into one experiment referred to as the Seattle and Denver Income Maintenance Experiments or SIME/DIME).

The primary focus of the income maintenance experiments was on the impact that a negative income tax has upon work incentives and the administrative feasibility of such a scheme. Housing issues were peripheral although to varying extents wealth data

were collected. The typical time period during which data were collected was 3-5 years - an admittedly limited time for studying housing consumption.

The number of housing related studies emanating from the income maintenance experiments is small. In general, sample sizes were truncated to represent the working poor, so house purchase decisions are poorly captured. Also, the short period of time and the fact that the experimental design complicates analysis mean that changes in housing consumption are subtle and difficult to disentangle from background statistical noise.²⁰

One study which did detect changes in housing consumption among the treatment and control groups is by Kalzuny (1979). A dichotomous choice model (logit) was employed to evaluate differences in rental (upgrading) and house purchase as a function of income, education, stratification (treatment versus controls), etc. The probability of rental upgrading or home purchase was slightly higher among those in the more generous treatment cells compared to the control group.

Another set of studies are available in connection with the housing allowance experiments conducted in the United States. Described in Bradbury and Downs (1981) and Friedman and Weinberg (1983) these studies explore various results emerging from housing allowance experiments undertaken in the seventies. Although a follow-up panel design was employed, these studies are focussed on the experimental question of whether housing allowances trans-

²⁰ These issues are addressed in further detail elsewhere in the report.

late into improved housing and as such are limited to those issues. Also, since policy experiments in the social sciences are unlikely to receive funding in the next decade, they are not relevant to the more general issue of how longitudinal research can inform housing policy.

Panel Study on Income Dynamics

This is the major North American longitudinal study which was initiated in 1969 under U.S. Federal government funding. This data base has provided insight on a variety of issues ranging from poverty, unemployment, intergenerational transfers of wealth, gas deregulation, impact of inflation on attitudes, childcare, mobility, taxation and equity, and of course, housing. As a general instrument within which to analyze changes in policy plus the impact of long-term extrinsic changes (e.g., high interest rates), this data source has no equal.

The housing research as reported annually in a series of volumes from research papers in mimeo form, to articles in journals. It is likely that some of the papers in mimeo form referenced below are also available in journals, however, they have not all been identified. It is possible to obtain assistance in securing material from the Institute.²¹

²¹ Survey Research Center, Institute for Social and Economic Research, University of Michigan, Ann Arbor, Michigan.

Several studies have been undertaken on housing consumption.

1. Housing and Homeownership (E. Roistacher) (Vol.II)²²

This is largely a descriptive piece based on the first five years of panel data. To truly determine changes in the consumption of housing, this amount of time must pass to permit economic and demographic change to have an effect on panel data.²³

The study found that the most important predictors of ownership are age of head(s) and family size. Many of the other conclusions are familiar to those who have worked with large cross-sectional data sets: the important aspect of this work is the analysis of changes which occurred over the first five years of the sampling. About 37% moved during this period and of these 44% moved more than once. During this period, for the movers, there was a 28% increase in real housing expenditures. The percentage increase in housing expenditures is directly related to income, change in family size, and percent change in income.²⁴ Other important explanators are 1968 family size (baseline) and education of the head.

²² The Volume number refers to the volume number of Five Thousand American Families: Patterns of Economic Progress. The full reference is by author in the bibliography.

²³ The panel can easily be put to short term policy use and also provides a useful cross-sectional base in the early years thereby allowing funders to "see" some results early in the project.

²⁴ The Beta values are .53, .21 and .31 respectively.

The longitudinal data echo cross-sectional results by indicating that changes in housing consumption rise less than proportionately to income, implying that income maintenance is not effective in raising housing consumption. The important role of the panel data is to show that short and long run elasticities (as measured by panel and cross-sectional data) are close in value. The actual changes in income and family size are good predictors of changes in housing consumption.

2. Race and Home Ownership (E. Roistacher) Vol. IV

This study finds a considerable reduction in differentials of homeownership in 24 of the largest metropolitan areas. This contrasts with other studies which suggest blacks are not increasing their economic welfare. This study demonstrates that panel results may be at considerable variance with cross-sectional results.

3. Permanent Income Hypotheses of Housing Consumption
(T.H. Lee and C.M. Kong) Vol. IV

Conventional estimates of permanent income elasticity of demand using cross-sectional data range from .5 to 2.1 (with the majority of estimates being well over 1). Panel data permit a more refined estimate of the concept of permanent income and the conclusion reached by the authors is that the permanent income elasticity is less than one. This reinforces the notion that housing is an inferior good (in the economic sense). Again the panel conclusion differs from most other evidence.

4. Residential Problems, Dissatisfaction and Mobility

(S. Newman and G. Duncan) Vol. VI

A major advantage of panel data is that intentions can be modelled and verified in a subsequent survey. In this research, the relation between housing satisfaction, moving intentions and subsequent action is modelled. The policy issue concerns the appropriate action for individuals who are in poor housing: should they be assisted to move, or should the problems cited with the house be fixed? The research found that about 14% of all respondents reported various problems with the house or location. An important finding is that the concept of housing satisfaction does not relate to cited incidences of housing problems or subsequent mobility. This suggests that the use of general concepts such as satisfaction with the residence to predict mobility should be re-examined. It also was concluded that many of the problems cited by owners could be remedied by rehabilitation.

5. Poverty, Housing Deprivation, and Housing Assistance

(S. Newman, R. Struyk and D. Manson) Vol. X

This research examines the nature of housing consumed by the permanently poor and the transiently poor. By examining income over time, it is possible to identify households who are permanently poor and those which are only temporarily in difficulty. This type of research parallels work done using longitudinal data which identifies transiently unemployed and those who are chronically out of work.

Summary

This brief section has reviewed some possibilities for housing research using longitudinal data. The income maintenance experiments and the panel study on income dynamics were reviewed. In general, the experimental data sets (and this includes the housing allowance experiment) cannot be effectively used for analysis which deviates from the objectives of the experiment. However, a general longitudinal economic and social survey which included housing issues at regular intervals would have great policy utility.

The next section examines the major experimental data set in Canada, the Manitoba Basic Annual Income Experiment (MINCOME).

MINCOME HOUSING VARIABLES: OVERVIEWIntroduction

This section of the report presents a brief overview of MINCOME with special references to the housing related data. The purpose here is not to present a detailed view of MINCOME - that is available in separate documentation - but rather highlights those features which bear upon housing issues. In particular, the definition of central concepts such as family, household, network and family composition is reviewed. Also, those aspects of field operations bearing on the quality of housing related data are reviewed. The nature of MINCOME data definition and collection stands in juxtaposition to the discussion in the previous section. In hindsight it is apparent that many features of MINCOME failed to conform to what is now considered optimal scientific practice. This is not the place to evaluate MINCOME, but it is worth stressing that retrospective criticism is always facile. Much of what is cited above as desirable features of longitudinal research is informed by MINCOME practice and experience.

Overview of MINCOME

As a "social experiment" MINCOME undertook to identify the "working poor" and then invite some to participate in an experiment which had the following general features:

- No participant would have a reduced income from participation;
- Based upon an income qualification (i.e., income could not exceed \$13,000 as measured in 1974), the participant family (household) would qualify for one of several "treatment" groups or a control group.
- Those in treatment groups would be eligible for a negative income tax program which would qualify them for a basic annual income and a tax program which would tax back earned income in excess of this basic amount.²⁵
- MINCOME assumed all tax reporting and collecting functions on their behalf, and also eliminated conventional welfare, although Unemployment Insurance eligibility was retained by the participant. In the event that benefits exceeded income guarantee this "surplus" would be taxed (but this was rare and also controlled).
- Participating households were required to complete a detailed interview about every three months at which time detailed information on work behaviour (hours worked, wages, job search, etc.) was completed by all members over 15. In addition, changes in certain types of consumption, especially of durables, housing, cars and furniture were recorded. Of course, family composition was also recorded at each survey.

²⁵ Clearly, participants who earned, say \$12,000, would not be enrolled in a treatment which provided them with \$5,000 annual income and taxed back the excess at 50%. Households were "allocated" to the experimental cell in ways to avoid this type of situation.

- The formal experiment endured over 11 surveys and from 1974 - 1978.²⁶
- In addition to participating in the surveys, households were placed into a guaranteed annual income and payments made to bring income up to the support level, or tax back portions of income earned in excess of this basic support level.²⁷

Housing Related Concepts in MINCOME

Although MINCOME was an income maintenance experiment, it did consider related social outcomes to a negative income tax. One central concern was the effect, if any, a guaranteed annual income (negative income tax) would have upon housing consumption both for owners and renters.

At the outset it should be noted that many researchers doubt that a limited duration experiment will have measureable effects on the consumption of major durables. Indeed, it is likely that effects will be subtle at best. Yet, some researchers have

²⁶ In 1975, MINCOME believed that a major omission existed in their original sampling. Also, it was feared that attrition far exceeded norms for counterpart experiments in the United States. At Survey 3, a secondary sample was drawn, primarily from the welfare rolls in Winnipeg to enrich the main sample. In effect, this supplementary sample parallels the main sample and was intended to be integrated with the main sample. No guidance was provided on how this integration was to be accomplished and to this date no satisfactory solution to this integration problem exists. Accordingly, the Institute for Social and Economic Research has decided not to formally support the supplementary sample, and it is provided only on special request. This is why reference is occasionally made to 14 surveys which endured from 1974 to 1979.

²⁷ Details of all aspects of MINCOME can be found in related documentation, the most useful of which is the MINCOME User Manual, available from the Institute for Social and Economic Research.

reported such effects from other experiments.²⁸ In general, these studies deal with the outcome of an experimental design, which was imposed on a longitudinal study. There has been no observational panel study of sufficient duration which examines consumption patterns by households except for the Panel Study on Income Dynamics, for which the most accessible account is by Morgan (1973).

Unit of Analysis:

Housing researchers make a distinction between the family, economic family, and household. Demand is based upon household formation. Doubling and undoubling are common occurrence with respect to housing markets. In addition, most housing research incorporates simultaneously in the process where the supply of affordable dwellings determines, in part, the formation of households.

For MINCOME the unit of analysis was both the adult individual and the family. In functional terms, a MINCOME family is similar to what is conventionally termed the "nuclear" family. Data were collected from all adults.²⁹ For example, it is well known that job search and holding by secondary workers (at the time of MINCOME usually women and young adults) are conditioned by the employment status of the primary worker. It is also not uncommon for the economic family to obtain "remittances" from family mem-

²⁸ See Kalzuny (1979) for analysis of the New Jersey Data. Other related studies are Poirier (1978) and Wooldridge (1978).

²⁹ MINCOME was one of the first surveys in Canada to treat male and female heads equally with respect to reporting labour market and other activity.

bers who do not happen to live in the dwelling.

The implication of this focus is that when there was a divorce, marriage or some other change in the family which might have occasioned a change in the demand for dwelling units, these changes were frequently not tracked. In many cases, a divorce would result in one member leaving MINCOME, not because of the specific regulations governing the experiment (since, all members of the family continued to be eligible even if there was a divorce), but because one of the divorcing heads (usually the man) would not be interested in maintaining contact with MINCOME. Thus, changes in the demand for housing due to family composition changes are poorly recorded in the MINCOME data. Again, the focus was on labour market, not housing market outcomes.

Despite this orientation, MINCOME often treated the concept of family and household as interchangeable, resulting in an ambiguity with respect to housing related research. Therefore, when consulting various documentation, especially that which originated with the experiment, it is useful to remember that the pertinent concept is the "nuclear" family.³⁰

³⁰ The incidence of unrelated individuals residing together is very rare in the experiment and does not persist beyond a few surveys. If an individual was selected for inclusion in the experiment and they happened to live with other unrelated adults (i.e., roommates) these other individuals would be invited to participate.

Four family structures were incorporated into the experiment:

- double-headed, multiple earners;
- double-headed, single earners;
- single-headed;
- single individual.³¹

Financial Concepts:

The key financial measures, both stock and flow, were carefully measured by MINCOME.

1. Income

Since MINCOME acted on behalf of the participant with respect to Revenue Canada, it ostensibly had access to most sources of income. Wage income was verified through paystubs and other documentation, and where feasible, all income was recorded. Aside from wage income (UIC payments were considered to be wage income) other sources of income could include some aspects of welfare payments and incidental income.³²

Income was recorded in a monthly income reporting form and also at the surveys. The survey based information was collected for research purposes while the monthly data were

³¹ The MINCOME User Manual and other documentation which detail the sample characteristics are available. The reader is encouraged to consult these sources to obtain a more detailed view of the MINCOME sample and data.

³² After the Baseline interview, those selected for enrollment were invited to participate. A family could not be on regular welfare payments and MINCOME. It was possible to remain on full welfare and act as a "welfare control", in which case the family would participate by filing regular income reports for which they were paid. Participants eligible for welfare could elect either MINCOME or welfare and switch without penalty.

collected for auditing and taxation purposes.

2. Networth

Detailed information was collected on networth, including real property, financial assets, cars and furniture. The data are quite complete, but their complexity can be illusory. For example, those participants owning real property usually just owned their principal residence. Information on other property, both residential and commercial, was also collected but this applies to very few participants. Often these participants initially qualified due to a temporary lull in income and were later dropped because their income rose beyond the cut-off.

The financial assets data are also quite complete, however, this information is somewhat more error prone than income data. This is because the current market value of some assets had to be estimated or because documentation (e.g., mortgage information) was not readily available. In the sample there are wealthy families with low income. It is difficult at this stage to verify whether these are valid observations or data errors.

Demographic Change:

Key to housing research is change in family composition. MINCOME recorded family size and structure in great detail. All payments were geared to a family size index which fluctuated as the number of adults and children changed in the family.



For families which remained "stable", (no divorces or death of one of the heads), recording changes in the number of children or related adults functioning as part of the family were easy and well executed. Tracking families in which there was a divorce (split) or a marriage (join) proved much more complicated.

In general, it is easy to measure family composition over time, but information on "splits" and "joins" is much more variable. Often families undergoing such change would drop from the experiment. For example, a double-headed household which divorced would usually only retain one head (usually the female); the other head would usually not remain in the experiment. Alternatively a single individual who married and now had a family income in excess of the cut-off would no longer be eligible for payments. Thus, changes in family composition could well cause a participant to drop from the sample. For this reason, changes in family composition must be carefully analyzed.

Field Operations

At its peak, MINCOME had over 200 staff, many of whom were interviewers and data entry personnel. Detailed analyses of field operations are available elsewhere³³ and here only those aspects pertinent to housing data are considered. While the unit of analysis is clear, once field operations commence many details intercede to amend, occasionally in major ways, the operational definitions of key concepts.

³³ See Rasmussen, Anderson and Wright (1982).

Reporting Unit:

For any large scale survey, designation of the reporting unit is vital. Most survey researchers note a bias toward female respondents since they traditionally have had time at home to participate in surveys.³⁴ In MINCOME each family had at least one reporting unit, some had more. For example, in a house where a mother, father, daughter, son, son's wife and their child resided, there would be two reporting units. One would have two heads and a dependent daughter, the other would have two heads and a dependent child. For a housing study designation of such complex reporting units may be needed, especially where household formation, doubling and undoubling are important foci. Multiple reporting units in a household increase the complexity of the data base considerably and are major factors in the field operations costs.

Survey Instrumentation:

MINCOME sought to identify the work behaviour of all adult family members. In addition, various family attributes such as assets were required. A modular approach to instrumentation was employed whereby each working head was administered an employment module designed to record work histories and other job related behaviour in detail. In addition, a family attributes module was administered to a designated reporter within the family. Accordingly, a typical family might complete several work modules and the attributes module every three months. Occasionally other

³⁴ This is becoming less true as female participation rates in the labour market approach that of men.

modules were added to the survey.³⁵

Ideally, on a panel study, survey instrumentation should remain stable. In the MINCOME experiment this stability did not occur until the third or fourth survey. This is a major problem in the data in that variables were generated using varying question texts. For many housing variables, such as value of the home, this is not a problem. However, for many of the labour market data, there is considerable variation which could invalidate a variable such as earnings.

Researcher discretion is advised, and when in doubt, original question texts in the Surveys Catalogue or on the micro-fiches should be consulted.

Sample Allocation:

A major issue in the MINCOME data is sample allocation. As mentioned above, stratification or clustering can impose real problems on the analysis. In the MINCOME data, not only were the initial data at Survey 1 generated, using a multiphase technique (designed to identify the working poor), but upon enrollment (Survey 2) respondents who were qualified to participate were allocated to the treatment and control groups, not randomly as would be expected in a normal experiment, but based upon the expected information they would provide on labour supply. This

³⁵ There were "youth" and "marital" modules added at various points to evaluate attitudes and perceptions of young people and married couples (who were interviewed separately on the quality of their marriage).

produced some important non-random disturbances in the data.³⁶

At this time no really satisfactory solution to analyzing data allocated in this fashion has been developed, although the consensus is that the effects of sample allocation can be captured by including both intercept and slope dummy variables in any multivariate regression. The nature of the sample allocation need not be laboured with respect to housing data, but it is important to note that analysis must incorporate these effects into the statistical modelling.

Summary

MINCOME did not have housing policy as its primary focus. Accordingly some of the definitions and practices do not conform to those required for housing research. In particular, the definition of the "nuclear" family and adult individuals within this unit, as the unit of analysis, is more limiting than the definition of household required for housing research. Question texts also tended to vary considerably in the initial surveys, especially on key concepts such as work behaviour, but it does not appear to have distorted housing related variables.

The upshot is that MINCOME provides a useful longitudinal sample of "working poor" with respect to housing policy. As an experimental sample, featuring non-random allocation to treatment and control groups, researchers must exercise caution in using multivariate analysis as if the sample were simple random to

³⁶ The sampling design and its rationale is described in Hum and Basilevsky (1979).

measure treatment effects.

OUTLINE FOR A LONGITUDINAL STUDYOverview

This section of the report sketches three longitudinal designs for housing policy and issues. In utilizing a longitudinal design for housing problems, the general advantages from longitudinal designs cited in section 1 are all relevant. However, the key requirement for a longitudinal study, as in a research, is a set of clear objectives which will remain stable. Annual macroeconomic changes that influence housing market dynamics will probably not be revealed in a longitudinal design. For example, increases in nominal and real interest rates will be quickly reflected in successive cross-sectional surveys and changes in demand easily imputed to these transitory events.

A longitudinal design is more useful than a cross-sectional survey in delineating developmental changes such as changing demand for homeownership by the "baby boom" population, household formation among young adults, and other "life-cycle" issues in consumption.

The proposals below present several possible variations for a longitudinal research program in housing issues. They have been developed in view of other housing data collected in the last decade.³⁷

³⁷ The review by Streich (1984) summarizes the various data sources developed for housing over the past decade.

Housing Consumption by Renters

This study would use a simple five-year follow-up procedure to analyze the housing consumption and demand by current tenants. The main features of the survey are:

- Sample frame would be telephone numbers and national in scope;
- The initial telephone interview would serve to identify tenants and homeowners and obtain initial information. Respondents would then be invited to participate in a mailout questionnaire. Completions would be paid by a nominal sum (\$5.00 - \$10.00);
- The initial telephone survey, if conducted by professional interviewers would probably have reasonable initial success (about 20% refusal) with the mailout acceptance of 90% of those who completed the telephone interview. Most of those who agreed to complete the mail survey would return the questionnaire, especially if paid;
- A national sample of 1500 would be sufficient to identify a representative group of tenants;
- Once the mailout was completed, semi-annual follow-ups would be used to maintain contact and keep attrition low. By using the forwarding facilities of Canada Post and the multi-trait tracking procedures outlined above, maintaining contact with 90% of the sample is within current recontacting standards;
- Annual surveys would be used to reinventory the group and measure changes in housing consumption, family structure, economic welfare, etc.;

- Specific questions which this data could answer are:
 - estimating the probability of various groups (young couples) of ownership/up or down sizing;
 - evaluation of the impediments to homeownership;
 - evaluation of attractiveness of alternatives (condominium, co-op housing, etc.);
 - estimating the impact of changes in various household attributes on housing consumption.

Variations on this theme are possible. By using multi-phase sampling³⁸ it will be possible to modify the purpose to specifically examine housing problems among particular populations.

In terms of cost, the annual expenditures for such a project, assuming a sample size of 1500, would be about \$40,000.00 - \$50,000.00. This includes mailout costs, initial telephone sample, data entry and analysis as well as compensation for completing the annual survey.

This type of follow-up research is malleable and can be amended over time to shift to other target populations. Once the basic logistics had been settled, the survey would become quite routine. There is also no reason why results would not be available within 2 months of the conclusion of field operations each year. Typically, results could be returned even earlier, thereby overcoming a defect cited by Streich in her survey of housing data.

³⁸ This is just a fancy way of saying that the sample is qualified on the basis of attributes reported over the phone.

Ownership Survey

In most respects this survey is similar to the tenants survey. Indeed both could be developed from the basic sample frame. Again, mailout contacts would be used every 6 months with the annual contact presenting the respondent with a comprehensive mailout questionnaire. Like the tenants survey, the sample is malleable and can be developed with specific sub-groups in mind.

The annual cost is similar and the objectives also would be to examine demand-side dynamics.

Full Longitudinal Design

To focus the issue, imagine a housing study within the following constraints:

1. National sample of 2000 continuously in the panel;
2. Rotating design which keeps respondents within the survey for three interview periods;
3. Concentration of the sample at specific age groups to identify life-cycle decision bases;
4. Major surveys every 5 years with semi-annual contacts and periodic up-date surveys;
5. Mixed mode survey comprised of in-person interviews, mail-out and telephone recontacts;
6. The unit of analysis is the "nuclear" family and the object is to evaluate the demand for housing, and not inventory of the housing stock.

From review by Streich (1984) it is apparent that an objective of housing surveys in the last decade or so has been to develop detailed regional "facts" about housing. Thus, there has been concentration on ensuring that regions are well represented in an effort to ensure external validity.

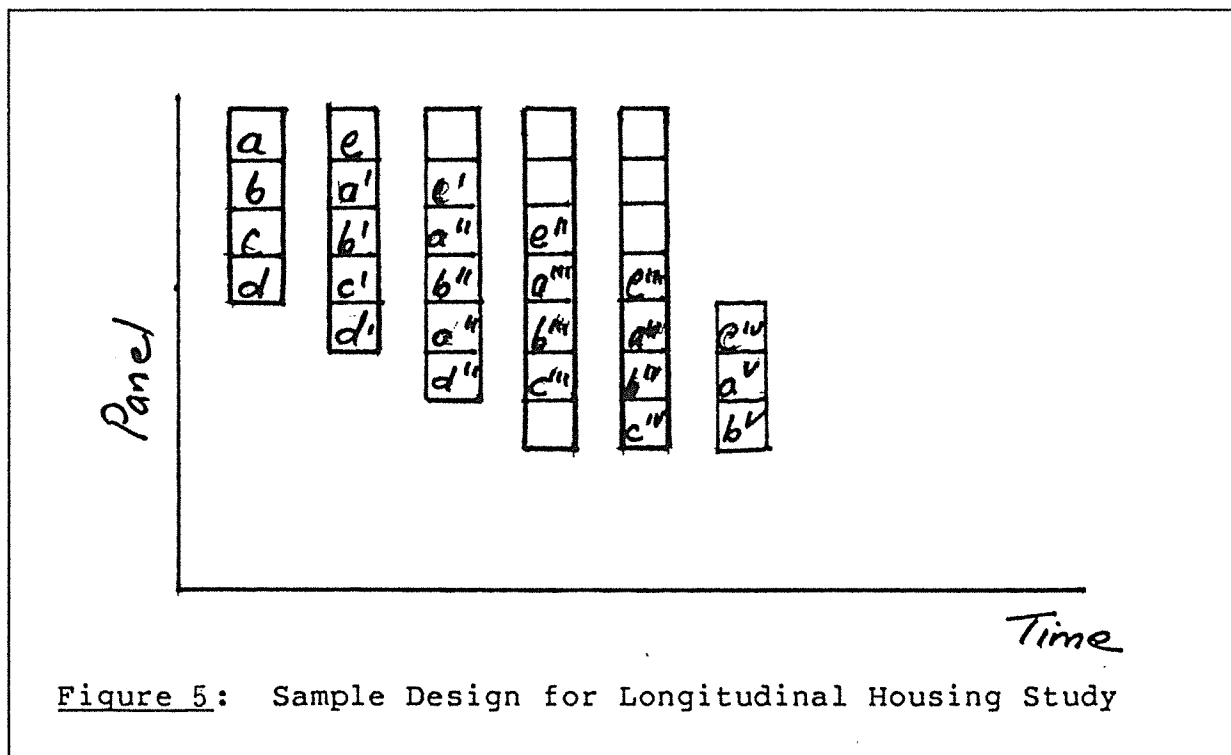
However, replicating the 74000 sample points in the 1974 Survey of Housing Units is simply unfeasible. To even replicate it as a cross-section is unviable. The labour force survey and the family expenditure surveys also are required to develop complex samples to obtain point estimates for key economic data. As such, they too feature very large samples which cannot be replicated in a survey devoted strictly to housing.³⁹

The important issue is whether a sample of 2000 is viable. First, such a sample size could not be spread across the population. Any gain from regional representation would be lost from the increased administrative costs of conducting such a dispersed sample. It must be emphasized that longitudinal panel data gain validity by replicating sample points through time, so perceived losses from smaller cross-sections are compensated by greater temporal depth and improved causal inference.

³⁹ A major advantage is that the housing modules can be linked into the socio-economic data. Important disadvantages are that the questions must not disturb the main purposes of the survey, and also Statistics Canada's reluctance to administer anything but factual questions. For a developmental study examining market trends and other aspects of long-term demand these are serious shortcomings.

Second, a survey which seeks to identify the basis for demand and longer run dynamics in housing markets must concentrate upon a wider range of data that is common to the existing data sources. This implies that additional variables will be required and that interviews and questionnaires will be longer and more complex.

Third, a continuous sample of 2000 in a rotating panel actually means that in any cross-section more respondents will be interviewed. This can be seen from the figure below.



This model makes no allowance for attrition, which would require either a strategy to replenish the sample through time (i.e., the addition of about 500 respondents initially, plus raising the

annual incremental sample from 500 to about 600). As proposed, the model shows that a panel of 2000 can be tracked for over four interviewing periods. Groups a - d in the figure are the initial panel, which is retained until time $t+8$, at which time group d''' is dropped. Group e enters at time $t+2$ and is retained for up to seven periods. Likewise, group a is retained in this design for seven periods. For the sake of clarifying potential costs, imagine a standard panel study, composed only of groups a - b through time and followed for a period of 8 years. In Figure 5 above this would be the successive panels marked a - d, a' - d', etc. In addition, assume that each year there is some replacement because of attrition, say, about 500 replacements. Even with this replacement special attention would have to be paid to selectivity bias.

Assume also that the initial surveys would be a comprehensive in-home interview which would cover demographics, financial data, expenditure patterns, and expectations, somewhat analogously to the Survey of Family Expenditures. Thereafter there would be annual mail surveys designed to up-date basic demographic data and a semi-annual mailing to maintain contact with the sample. As outlined above frequent contact is essential to maintain high panel integrity.

The initial sample frame would be drawn from a telephone survey which would enroll respondents into the panel. Other approaches might be to use a subsample of various existing surveys such as FAMEX or the LFS.

Thereafter the survey would proceed in much the same way as the first two designs. The major difference lies in the scope of the questionnaire and the length of time over which the study would persist. Considerably more resources will be required to maintain the integrity of the sample over an 8-year period.

The general costs for such a project at the national level and with a sample size of 2000 would be as follows:

1. Instrument Design/Pretest	\$100,000
2. Data Base Development	\$ 60,000
3. Field Operations Design	\$ 75,000
4. Baseline Survey	\$150,000
Total	\$385,000

II Annual

1. Field Operations	\$100,000
2. Recontact	\$ 50,000
3. Analysis/Reporting	\$ 60,000
Total	\$210,000

Annual costs will rise slightly each year as recontacting is required to track the sample. Every five years, attrition will reduce the sample to about 60-70% of the original size. One can have a policy of adding replacements each year to compensate, or to add only every five years (or some other interval).

The planning horizon required for such a project is stringent. A major factor in most panel research thus far has been the problems caused by overly optimistic projections on the time required to design a stable instrument. A minimum of a year is needed to undertake this work prior to the baseline survey. The need to work in both official languages also requires that this design phase be very careful.

It is reasonable to expect initial results from the first panel (baseline) 18 months after initial commitment to the project. Thereafter, annual results can be expected.

This estimate represents a typical budget for panel research. It is more expensive than a single survey, and since the instrument is used over time, more design effort (expense) is essential.

CONCLUSION

This paper has sketched the main features of longitudinal research without doing justice to the potential of this design for policy research. The basic rationale for such an approach is that longitudinal designs allow more precise evaluation of causal relationships. Policies implemented within a longitudinal framework are also required to develop a more complete program logic than is required for descriptive cross-sectional surveys. This more complete program logic eventually will find expression in program evaluation tasks and will assist in imposing a more complete evaluation framework on the program activities. p. Longitudinal designs are more complicated and costly than single surveys, however, their advantages are significant. It is likely that in times of restraint, joint sponsorship of such survey designs will prove to be an attractive method for undertaking such research.

The main advantages of panel data over conventional cross-sectional or time-series information are:

1. measurement of change is more precise compared to using recall or retrospection (sampling error on a mean difference is lower than the difference of two means);
2. information improves as respondents begin to reveal more detail;
3. expectations, intentions and actualization become measurable and available for evaluation;

4. reinterviews assist in reducing the "noise" common to attitudinal data in that respondents' true attitudes and beliefs are revealed over period of time;
5. causal (policy) analysis is more reliable;
6. the face validity of the information is greater to non-technical audiences.

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Appendix A

VARIABLE DEFINITIONS FROM MINCOME

These definitions reflect the complexity of housing data, the longitudinal nature of the project (requiring consistency over time as well as within the cross-section) and the experimental nature of MINCOME.

The listing below is only partial and designed to illustrate the above dimensions with variables relevant to housing.

1. Child

... means the son or daughter (including a step-son or step daughter) of any age, unless the contrary is specified, of an adult member and includes a child by nature or by law but does not include a foster child for whom maintenance is received. "Child" also means the Dependent, as defined below (see "Dependent") of an adult member and may include a child who is being adopted if no maintenance payment is being received for the child.

2. Dependent

... includes only persons under 18 years of age who are related to an adult member (anyone over 15) in one of the following ways: brother, sister, nephew, niece, grandchild, aunt or uncle, and who are dependent upon the adult member for financial support.

3. Dwelling

... means a house, an apartment, a mobile unit or a single room occupied or intended for occupancy as separate living quarters; separate living quarters are those in which the occupants do not live and eat with persons in the structure and are quarters which have direct access from the outside of the building through a common hall; the occupants may be a single family, one person living alone, two or more families living together, or any group of related or unrelated persons who share living arrangements; under special circumstances the Director (of MINCOME) may determine whether or not a structure or part of a structure or a set of structures constitutes a Dwelling.

4. Lives or Living With

... shall be said to apply to a person's occupation of a Dwelling if that person spends more non-working, non-instructional, non-institutionalized time in that Dwelling than any other Dwelling during the filing period;⁴⁰ or, normally satisfied this condition, but is in one of the following circumstances:

- absent for the purposes of working or looking for work and is expected to return upon completion of that work or upon success or failure of finding a job;

⁴⁰ Filing period referred to an accounting period used to identify payments provide MINCOME participants.

- in prison, jail, lock-up, detention centre, or a penal or correctional institution and has not yet been sentenced, and in any case can be reasonably expected to be released within the ensuing filing period;
- in a hospital or mental institution or tuberculosis sanitarium and can reasonably be expected to be released during the ensuing filing period;
- absent for the purpose of training or rehabilitation in connection with employment opportunities, and is expected to return upon completion of the specified training or rehabilitation period;
- absent for any other reason for a period not expected to exceed the ensuing filing period.

5. Rent Free or Subsidized Housing

... means any housing the Unit lives in without paying rent or any housing arrangement where the rent is reduced from the normal market rent, unless the reduction can be shown to be the result of a promotion scheme by the property management.

Appendix B

VARIABLES IN MINCOME

The following is a complete review of variables as defined by MINCOME. These definitions required a full year of refinement with a cost of about two full-time staff. In part, the complexity was due to the need to define experimental units very carefully and to ensure that treatment and control groups were well identified.

In general, however, this variable definition is common to all survey research with somewhat greater attention required for panel research.

Throughout the MINCOME field operations, many exceptions were encountered which required disposition on the spot, usually by the Director of Research. These dispositions and rulings are contained in three large binders. Again, the basic message is clear - panel research requires great care and somewhat higher expenses than is common to standard cross-sectional research. It is a mistake to simply multiply the cost per cross-sectional survey times the number of waves to obtain an estimate of the final cost.

Appendix C

30/10/74

A. Scope

1. A Reporting Unit's Net Worth is reported (Form 2) at least annually (See Schedule 4) to Mincome Manitoba and must include the Net Worth of all members of that Unit.
2. The Filer must submit additional Net Worth Reports to report any change in Net Worth between annual reports which exceeds \$5,000.
3. Additional Net Worth Reports may be requested by the Director if the size or make up of a Reporting Unit changes between annual reports.
4. If a married couple separates, the Director may divide the last reported Net Worth equally between each partner until new reports are filed, or a legal declaration properly divides their assets.
5. A Net Worth Report (Form 2) must be made out by the Filer at the time of Enrollment.
6. The percentage of Net Worth counted by Mincome Manitoba for payments (See Payments V.D.3) causes a dollar for dollar reduction in the Mincome Payment.

K. J. Lickel

30/10/74

B. How to Determine Net Worth

1. Net Worth is the difference between the market value (See Definitions, VII) of the Reporting Unit's assets and the amount owing on them. This difference is called the "equity" held in the asset(s). Costs of keeping up and repairing an asset are not deductible and therefore not included in the equity held in an asset.
2. A Unit's assets are of three types: Personal Property (See III.B.3 below) Financial and Real Estate Property (See III.B.4), and Business Property (See III.B.5). Unsecured Liabilities (See III.B.6) can be subtracted from the value of assets, but Net Worth can go no lower than zero.
3. The following Personal Property results in a Net Worth increase equal to the amount that equity in each item or "collection" is more than \$1,000; and they must be listed on the Net Worth Report if the market value of each, or of any "collection", is more than \$1,000:
 - (a) Any vehicle not covered as Business Property in III.B.5., including a car, truck, bus, boat, aircraft, trailer, mobile home, (except where this is the Principal Residence), motorcycle, tractor, snowmobile, minibike, and the like..

R. J. Lukid

3. (b) Any furniture, household appliance, clothing, tool, or other equipment not used in work that is not covered as Business Property.
- (c) Any "collection" of antiques, art, jewellery, books, stamps, coins, and the like.
- (d) Anything else the Director determines is Personal Property, including property held in trust if it is to be given within the calendar year to a member of the Reporting Unit, or if it is left up to a member of the Unit when to receive it.
4. Financial and Real Estate Property results in a Net Worth increase only when there is equity in a category as listed below, and the Net Worth Report must show all items in these categories regardless of the amount of equity.
- (a) All cash-on-hand not covered as a Business Property in III.B.5.
- (b) All money in chequing, savings, and current accounts, including department store deposit accounts, not covered as Business Property in III.B.5.
- (c) Anything over \$1,000 in total cash surrender value of life insurance policies, pensions, retirement savings plans and annuities, except those from which income or benefit now is being received. (Employee pension plans are not included here if participation in them is compulsory.)
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4. (d) The current value of all stocks, shares and bonds not counted in (c) above nor as Business Property in III.B.5.
- (e) The outstanding amount of any debt owed to a member of the Reporting Unit not covered in (c) above or as a Business Property in III.B.5. The outstanding amount is the principal owed on the date of the net worth assessment plus back interest still owed, but not current or future interest. A bad debt (see Definitions VII) is not counted.
- (f) Any dwelling occupied by the Reporting Unit (except mobile homes reported in III.B.3.(a)) even if only occupied part of the time, such as a summer cottage. When living and work or business areas are together, the business part of the equity is reported in III.B.5., and the rest here.
- (g) All other real estate, except revenue real estate and real estate that is used in the operation of a business (including farming) operated by a member of the Unit (in which case, it is reported in III.B.5.(c)).
5. Business Property results in a Net Worth increase whenever there is equity in a category as listed below, and the Net Worth Report must show all items in the categories regardless of the amount of equity.

R. J. K. K.

III. "NET WORTH" AS IT APPLIES TO MINCOME PAYMENTS. B....

30/10/74

5. (a) All cash-on-hand for business.
- (b) All money in Business chequing, savings and current accounts, or on deposit with suppliers as a credit against future purchases, and any other financial assets (including accounts receivable) held in the name of the Business. Only the outstanding amount of debts are counted: that is, the principal owed on the date of the Net Worth assessment plus back interest still owed but not current or future interest.
- (c) All real estate, (except real estate reported in III.B.4) including revenue real estate and farm property and any real estate used in the Business operation of a member of the unit.
- (d) Inventories such as merchandise, supplies, finished goods, components, feed grain, fertilizer, and the like.
- (e) Tools, machines, store fixtures, office furniture and all other business equipment or furnishings.
- (f) Livestock, including breeding animals.
- (g) Any vehicle used in Business and not covered in III.B.3.(a).

J. J. L. L. L.

30/10/74

6. Unsecured liabilities to be subtracted from assets include:

- (a) The total amount of any debts or liabilities associated with the operation of a business of a member of the Reporting Unit and not secured by any particular asset.
- (b) Any excess over \$1,000 of the total amount of any debts or liabilities which are not associated with the operation of a business of a member of the Reporting Unit and not secured by any particular asset.

(Note: The allowance for debts and liabilities secured by particular assets is provided for in the calculation of the equity held in assets.)

Q. L. L. L.

FORMS

1. Participants' Agreement.
2. Income Report Form with the following schedules:
 - (1) Reporting Unit Composition
 - (2) Salaries, Wages, Fees, Commissions
 - (3) Rental Income from own Dwelling and Personal Property
 - (4) Monthly Business Income Report including Farming and Fishing
 - (5) Capital Gains and Losses
 - (6) All other Income subject to the Normal Reduction Rate
 - (7) Low Rental or Assisted Housing
 - (8) Tax Credits
 - (9) All other Government Payments subject to 100% Reduction Rate
 - (10) Expenses/ Provision of Alimony or Support
 - (11) Net Worth Report for Non-Business Property
 - (12) Annual Business Income Report including Farming and Fishing
 - (13) Estimate of Annual Business Income
3. Payment Worksheets

SCHEDULES

1. Sites
2. Support Levels, Reduction Rates, Wealth Tax Rates, Exemption Levels, etc.
3. Reporting Unit Size Index
4. Calendar of Reporting Dates

SCHEDULE 1

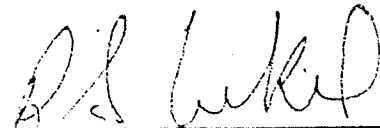
Sites for Enrollment and Payments:

A. DISPERSED SITES

Dufferin (RM)
Carman (Town)
Langford (RM)
Neepawa (Town)
Minto (RM)
Minnedosa (Town)
Morris (Town and RM)
Odanah (RM)
Portage (Town and RM)
Stonewall (Town)
Rosedale (RM)
Winnipeg
Stanley (RM)
Morden (Town)
Swan River (Town and RM)

B. SATURATION SITE

- Dauphin (Town and RM)



SCHEDULE 2:

A. Annual Support Levels, for Reporting Unit Size 4:

For Reporting Units with size other than 4, the Annual Support Level is calculated as follows:

(Support Level, Size 4) X (R.U.S.I.) , where

R.U.S.I. is the Reporting Unit Size Index, as defined

in Schedule 3.

B. Normal Reduction Rates (r):

$$r = .35$$

$$r = .50$$

$$r = .75$$

C. General Exemptions on Net Worth:

All Reporting Units are given a \$3,000 exemption on Net Worth. Farm Operations are given an additional \$20,000 on Net Worth. The Farm Operation must be operated by the Reporting Unit and must potentially provide more than 50% of the family's non-transfer income.

D. Wealth Tax Rates (g_1 , g_2 , g_3):

$$g_1 = 4\%$$

$$g_2 = 8\%$$

$$g_3 = 16\%$$

Paul L. L. L.

30/10/74

E. Points of Inflection for calculating Net Worth assessment
(X_1, X_2):

X_1 = \$10,000 of non-exempt Net Worth

X_2 = \$30,000 of non-exempt Net Worth

F. Factor which sets the Tax Integration Rate (h):
 $h = 1.0$

G. Point where tax integration rate becomes applicable is θB ,
where $\theta = 1.2$.

R. S. L. K. P.

SCHEDULE 3

Reporting Unit Size Index (R.U.S.I.)

Number of Adults
OTHER THAN FAMILY HEADS*

<u>Reporting Unit Size</u>	<u>0</u>	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	<u>5</u>	<u>6+</u>
1	.38						
2	.71	.71					
3	.88	.97	.97				
4	1.00	1.14	1.23	1.23			
5	1.10	1.26	1.40	1.49	1.49		
6	1.20	1.36	1.52	1.66	1.75	1.75	
7	1.30	1.46	1.62	1.78	1.92	2.01	2.01
8	1.40	1.56	1.72	1.88	2.04	2.18	2.27
9	1.45	1.61	1.77	1.93	2.09	2.23	2.32
10	1.50	1.66	1.82	1.98	2.14	2.28	2.37
11	1.55	1.71	1.87	2.03	2.19	2.33	2.42
12+	1.60	1.76	1.92	2.08	2.24	2.38	2.47

* For each adult other than family heads - the R.U.S.I. is increased by .26 subject to the constraint that the addition of the second member of the reporting unit always increases the R.U.S.I. by .33 in a single-adult-member unit.

A Head is a person who satisfies the following conditions:

- (i) He or she is the major earner or the spouse of the major earner in the Reporting Unit.
- (ii) And he or she is an Adult Member of the Reporting Unit.

R. A. Wickel

30/10/74

Dates for Income Reporting Monthly:

1975

DEC
(1974)

	JAN	FEB	MAR	APR	MAY	JUN	JUL	AUG	SEP	OCT	NOV	DEC
Due Date	JAN 2	FEB 3	MAR 3	APR 1	MAY 1	JUN 2	JUL 2	AUG 1	SEP 2	OCT 1	NOV 3	DEC 1 (1976)
Payment Delay Warning	JAN 9	FEB 10	MAR 10	APR 8	MAY 8	JUN 9	JUL 9	AUG 8	SEP 9	OCT 8	NOV 10	DEC 8
Overdue -- \$5 penalty	JAN 13	FEB 13	MAR 13	APR 11	MAY 13	JUN 12	JUL 14	AUG 13	SEP 12	OCT 13	NOV 13	DEC 11
Overdue loss-of-minimum- payment	JAN 22	FEB 24	MAR 24	APR 23	MAY 23	JUN 24	JUL 23	AUG 25	SEP 23	OCT 24	NOV 24	DEC 29
												JAN 23

Special:

Net Worth Schedules are required with the Income Report Form for December income.

Where applicable, when an Annual Business Income Report (Schedule 12) cannot be completed to accompany the Income Report Form for December income, an estimate of Annual Business Income on Schedule 13 must accompany the Report. In any case, Schedule 12 must be completed by the time of the Income Report Form for the month of April.

- A. In these rules, except where the context otherwise requires:
1. "Active Unit" means any Reporting Unit which continues to receive any type of payment from Mincome Manitoba.
 2. "Adjusted Income" means an amount of income or money as described in V.C.2.b.
 3. "Adult" means a person 18 years of age or older.
 4. "Adult Member" means a person who, given that the other criteria for eligibility in Part II are met, is eligible to be a filer by reason that he or she meets the conditions of I.B.1.
 5. "Age" of an individual means the age on the last day of the reporting period, and includes the birthday should this occur on the last day of the reporting period.
 6. "Asset" means, any item or property of value as defined in III.
 7. "Bad debt" means a debt that is uncollectible or unlikely to be paid; debts which have been turned over to a collection agency or on which no payment has been made for 24 months are counted within the meaning of "bad debts" for Mincome Manitoba.

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8. "Break-even Level" means an amount of money as described in V.D.3.

9. "Business" means the following:

- (a) Any enterprise that provides those who own it with an income other than from their own labour.
- (b) Any person or enterprise claiming deductions from income from self-employment on their Income Tax returns for Revenue Canada Taxation.
- (c) Or any enterprise which the Director finds to be a business.

Wherever the word "Business" appears in the rules it is to include farm and fishing operations, and professional enterprises.

10. "Business Income" includes any income from a business covered by VII.A.9. above, but does not include income from an office or employment.

11. "Business Property" means any item, asset or property of value owned by the business as defined in III.B.5.

12. "Capital Gain" means the amount of the profit from the disposal of an asset which is not the principal residence or an item of business inventory. As the case requires, one of the following applies in the calculation of the Capital Gain:

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12. (a) In the case of Financial and Real Estate Property (See III.B.4), the value of a realized Capital Gain, less the verified costs of making the gain, is counted according to the following provisions:

- (i) When an asset is disposed of, the Capital Gain is the difference between the proceeds of disposition and the market value as reported on the last regular Annual Net Worth Report in which the asset was listed.
- (ii) When an asset is disposed of which has not appeared on the last regular Annual Net Worth Report, the Capital Gain is the difference between the purchase price and the proceeds of disposition.
- (iii) For Capital Gains on Real Estate, the cost of improvements and additions, but not normal maintenance and repairs, are deductible over the shortest of these three periods:
 - (aa) The period since the Real Estate was bought.
 - (bb) Since it last was reported on a regular Annual Net Worth Report.
 - (cc) Or since the start of the Mincome Manitoba program.



12. (b) In the case of Personal Property (See III.B.3), the value of a realized Capital Gain, less the verified costs of making the gain, is counted according to the following provisions:

- (i) A Capital Gain is counted only in cases where the proceeds of disposition exceed \$1000.
- (ii) The Capital Gain is calculated in the same manner as a Capital Gain on Financial or Real Estate Property (see (a) above) except that, in cases where the item has not appeared in a previous Net Worth Report, the Capital Gain is equal to the proceeds of disposition minus the greater of the purchase price of \$1000.
- (iii) Costs of improvements and additions are allowed as in (a) (iii) above.

(c) In the case of Business Property (See III.B.5), the value of a realized Capital Gain, less verified costs of making the gain, is counted according to the following provisions:

- (i) The Capital Gain is calculated in the same manner as a Capital Gain on Financial or Real Estate Property (See (a) above).
- (ii) Costs of improvements and additions are allowed as in (a) (iii) above.

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13. "Capital Loss" means the amount of the loss from the disposal of an asset which is not the principal residence or an item of business inventory. As the case requires, one of the following applies in the calculation of the Capital Loss:

(a) In the case of Financial and Real Estate Property (See III.B.4) a Capital Loss is counted only when it is deductible from Capital Gains on Personal Property and Financial and Real Estate Property. Capital Losses are deductible according to the following provisions:

(i) Capital Losses are deductible only if the asset was listed on the last Net Worth Report, or was bought after the last Report was filed.

(ii) When an asset is disposed of, the Capital Loss is the difference between the proceeds of disposition and the market value as reported on the last Net Worth Report in which the asset was listed, or the purchase price, if the item was purchased after the last Net Worth Report was filed.

(b) In the case of Personal Property (See III.B.3), a Capital Loss is counted only when it is deductible from Gains on Personal Property and Financial and Real Estate Property. Capital Losses are deductible according to the following provisions:

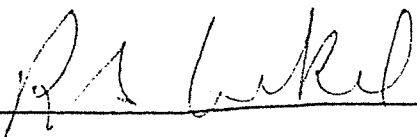
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13. (b) (i) A Capital Loss is counted only in cases where the proceeds of disposition exceed \$1,000.
- (ii) Capital Losses are deductible only if the asset was listed on the last Net Worth Report, or was bought after the last Report was filed.
- (iii) When an asset is disposed of, the Capital Loss is the difference between the proceeds of disposition and the market value as reported on the last Net Worth Report in which the asset was listed or the purchase price, if the item was purchased after the last Net Worth Report was filed.
- (c) In the case of Business Property (See III.B.5.) Capital Losses are deductible from Capital Gains, but the net value may never be less than zero. Capital Losses are deductible according to the following provisions:
- (i) Capital Losses are deductible only if the asset was listed on a previous Net Worth Report, or was bought after the last Report was filed.

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13. (c) (ii) When an asset is disposed of the Capital Loss is the value listed on the last Report on which the asset appeared, minus the proceeds of disposition.
14. "Child" means the son or daughter (including a step-son or step-daughter) of any age, unless the contrary is specified, of an adult member and includes a child by nature or by law but does not include a foster child for whom a maintenance payment is received. "Child" also means the Dependent, as defined in VII.A.16, below, of an Adult Member and may include a child who is being adopted if no maintenance payment is being received for the child.
15. "Control Unit" means individuals or groups of persons organized as if they were Reporting Units but not receiving Mincome Payments. The Units will receive Participation Payments in recognition of their service.



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16. "Dependent" includes only persons under 18 years of age who are related to an Adult Member in one of the following ways: brother, sister, nephew, niece, grand-child, aunt or uncle, and who are dependent upon that Adult Member for financial support.
17. "Depreciation" means the amount allowed as an expense because of the decrease in value of Capital Goods as defined in IV.B.3.(b).VI. It includes all the Allowance Classes as defined by Income Tax but it does not include any allowance for the depreciation of Real Estate.
18. "Designated" means named by the Director.
19. "Designated site" means one of the sites or areas described in II.B.
20. "Director" or "Director of Mincome Manitoba", as the case may be, means the person appointed as Experiment Director by the Ministers under the provisions of the Social Services Administration Act.
21. "Dispersed Module" means a part of the experimental design which calls for selecting a sample of Reporting Units on a partially random basis from a large population within a number of geographic areas.

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22. "Disposal" (of an asset) includes loss of possession of the asset in any of the following ways: selling, trading, losing, theft, and destruction.
23. "Dwelling" means a house, an apartment, a mobile unit or a single room occupied or intended for occupancy as separate living quarters; separate living quarters are those in which the occupants do not live and eat with other persons in the structure and are quarters which have direct access from the outside of the building through a common hall; the occupants may be a single family, one person living alone, two or more families living together, or any group of related or unrelated persons who share living arrangements; under special circumstances the Director may determine whether or not a structure or a part of a structure of a set of structures constitutes a Dwelling.
24. "Enrollment Plan" means the payment plan to which a Unit has been assigned. The plans have different Support Levels and Normal Reduction Rates.
25. "Equity" means the amount described in III.B.1.

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26. "Experiment" means the Manitoba Basic Annual Income Experiment.
27. "Farm operation" means any agriculture enterprise that provides those who own it with an income other than for their own labour. (Note: Farm operations qualifying for special exemption as farms are further specified in Schedule 2).
28. "Filer" means an Adult Member described by I.C. and accepting the responsibilities of I.G.
29. "Filing Period" means the time covered by the regular Income Report Forms (one month).
30. "Financial and Real Estate Property" means any item, asset, or property of value as defined in III.B.4.
31. "Gross rent" means the aggregate of all amounts of rent from Real Estate received in a taxation year.
32. "Income" means income as described in IV.
33. "Income-in-kind" means the value of goods and services rendered, when goods or services are received instead of money.
34. "Income-tested program" means a program that gives grants, or subsidies, where the amount of the grant or subsidy is determined in whole or in part by the income of the applicant.

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35. "Inventory" means a description of supplies on hand, other than depreciable property. Supplies on hand include the following:
- (a) Supplies held as stock in trade.
 - (b) Goods being manufactured or processed for sale.
 - (c) Supplies to be consumed in the rendering of services.
 - (d) Any other supplies the cost of value of which is reflected as an asset in the business balance sheet.
36. "Lives" or "living" shall be said to apply to a person's occupation of a Dwelling if that person spends more non-working, non-instructional, non-institutionalized time in that Dwelling than any other Dwelling during the Filing Period; or, normally satisfies this condition, but is in one of the following circumstances:
- (a) absent for the purposes of working or looking for work and is expected to return upon completion of that work or upon success or failure of finding a job;
 - (b) in prison, gaol, lock-up, detention centre, or a penal or correctional institution and has not yet been sentenced and in any case can reasonably be expected to be released within the ensuing Filing period;
 - (c) in a hospital or mental institution or tuberculosis sanitarium and can reasonably be expected to be released during the ensuing filing period;

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36. (d) absent for the purpose of training or rehabilitation in connection with employment opportunities, and is expected to return upon the completion of the specified training or rehabilitation period;
- (e) absent for any other reason for a period not expected to exceed the ensuing Filing Period.
37. "Mandatory Member" means a person described in I.E. who,
- (a) if he or she is an Adult Member, must agree to be a Filer or must agree to have someone file on his or her behalf as described in I.C.b., as a condition of that Reporting Unit receiving payments; or
- (b) if he or she is not an Adult Member, is the spouse or child of a person who must agree to be a Filer or must agree to have someone file on his or her behalf as described in I.C.b., as a condition of that Reporting Unit receiving payments.
38. "Market value" means the value of an asset in terms of what it can be sold for at the time the assessment is made.

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39. "Married" means united in marriage in any way recognized under the Statutes of the province in which they live.
40. "Means-tested program" means a program that gives grants or subsidies, where the amount of the grant or subsidy is determined in whole or in part by the wealth of the applicant.
41. "Member" includes anyone who belongs to a Reporting Unit who is either
- (a) An adult member;
 - (b) Or a person unable by reason of age to be an Adult Member.
42. "Mincome Payment" means the monthly payment as described in V.A.2.
43. "Minimum payment" means the minimum monthly Mincome Payment as described in V.A.2.
44. "Needs-tested program" means a program that gives grants or subsidies, where the amount of the grant or subsidy is determined in whole or in part by an assessment of the needs of the applicant relative to a scale of needs.
45. "Net Worth" means Net Worth as described in III.
46. "Non-adult" means a person under 18 years of age.
47. "Non-casual employee" means any person who has signed a contract of employment with Mincome Manitoba.

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VII. DEFINITIONS.

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48. "Normal Reduction Rate" means the percentage determined by the Enrollment Plan, by which adjusted income is reduced before it is deducted from the Support Level.
49. "Original Reporting Unit" means, in the case where a member has left a Reporting Unit, the Unit which was left. In the case of a series of composition changes, the Director may determine which Unit is the Original Reporting Unit.
50. "Participant" means as the context requires, a person who
- (a) is a member of a Reporting Unit receiving Mincome Payments; or,
 - (b) is a member of a Reporting Unit receiving Participation Payments; or,
 - (c) is an individual receiving Participation Payments.
51. "Participation Payment" means a payment to a person or to a Reporting Unit in recognition of service, as described in V.A.1.
52. "Partnership" means any business arrangement (other than incorporation) for sharing operating expenses and profits. The partnership can be legal or informal.
53. "Personal Property" means any asset or property of value owned by an individual as defined in III.B.3.

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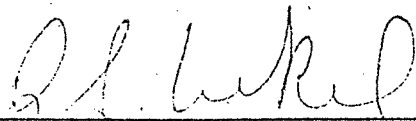
54. "Principal Residence" means "Principal Residence" as defined for the purposes of the Income Tax Act, Canada.
55. "Proceeds of disposition" means an amount received as the result of disposing of an asset and includes amounts received as a selling price or a trade-in value. In the case of an asset being destroyed where the owner of the asset receives no remuneration, the proceeds of disposition are zero.
56. "Project" means the Manitoba Basic Annual Income Experiment.
57. "Real Estate" means an estate or property consisting of lands and all buildings on the lands.
58. "Reconciliation Amount" means under the terms of the Participants' Agreement the amount owed to Mincome Manitoba or the amount owed by Mincome Manitoba as calculated in V.D.6. and defined in V.C.4.
59. "Reduction Rate" means the percentage by which income is reduced before it is subtracted from the Support Level.
60. "Rent free or subsidized housing" means any housing the Unit lives in without paying rent or any housing arrangement where the rent is reduced from the normal market rent, unless the reduction can be shown to be the result of a promotion scheme by the property management.

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61. "Reporting Unit" means an individual or a group of persons as described in I,
- (a) actually participating in Mincome Manitoba and receiving Mincome Payments;
 - (b) intending to participate in Mincome Manitoba and to receive Mincome Payments subject to a determination by Mincome Manitoba as to eligibility according to the criteria of II;
 - (c) or who have participated and received Mincome Payments in the past but no longer do.
62. "Reside" means live on a continuous basis in a Dwelling or a series of Dwellings within the boundaries of the Saturation Site. For the purposes of establishing eligibility (See II.B.2.), the required period of continuity is bounded by 1 July 1974 and the date Mincome Manitoba receives the signed Participants' Agreement.
63. "Saturation Module" means a part of the experimental design which calls for designating a particular geographic area within which Reporting Units qualify for Mincome Payments with respect to initial eligibility but not, necessarily, with respect to any other criteria.

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64. "Saturation Site" means the geographic area designated for conducting the saturation module of the experiment.
65. "Self-employed" means earning one's income from one's own enterprise in the form of business, professional, commission, farming or fishing income as counted in the Individual Income Tax Return for Revenue Canada Taxation.
66. "Spouse" means a partner in marriage in any way recognized under the Statutes of the province in which a Participant lives.
67. "Support Level" means an amount of money as described in V.B.
68. "Unit" means Reporting Unit.
69. "Unit composition" means the membership of Reporting Unit.

A handwritten signature in dark ink, appearing to read 'D. L. L. L.', is written over a horizontal line.