

I. INTRODUCTION

This manual reviews the Manitoba Basic Annual Income Experiment (MINCOME) which was conducted from 1974-1979 under joint sponsorship of the federal and provincial governments, and surveys the data base as presently installed at the Institute for Social and Economic Research. The basic objective of the experiment was to test the hypothesis that a guaranteed annual income, or negative income tax, would have an adverse impact upon the work behaviour of recipients. In addition, a host of other hypotheses relating to the impact this form of welfare has upon family stability, consumer durables purchases, investments in additional education and housing, etc., were to be analyzed.

In the experiment, a sample of low income households was selected for a control group, while others were assigned to a complex of eight different treatments consisting of various combinations of basic annual income support and tax rates. Each family unit who agreed to be part of the experiment (control and treatment groups) was periodically surveyed over the three-year field phase of the study, and the information archived in a number of data sets.

Prior to research on the main question being undertaken, the experiment was halted, and the data archived. The Institute for Social and Economic Research was created in 1981, with a major responsibility to execute a feasibility study of the data, and to prepare it for analysis by qualified researchers. This feasibility stage is well underway, and research using most of the data is now possible.

This manual provides an overview of the experiment (Section 1), describes the range and nature of the variables collected by the surveys which monitored the experiment (Section 2), surveys some important technical issues associated with hypothesis tests based upon MINCOME data (Section 3), and summarizes the technical reports available from the Institute which provide more detail on various aspects of the information contained in the data (Section 4).

The final part of the manual (Section 5), explains how research using the data may be executed. This section reviews the steps whereby researchers may request extracts of the data for processing. It is vital that researchers interested in using these data thoroughly familiarize themselves with MINCOME. This manual attempts to reduce these learning costs to a minimum.

Overview of MINCOME: Objectives and Experimental Design

MINCOME was a major experiment designed to evaluate the consequences of an alternative type of public welfare based on the concept of a negative income tax (NIT), or guaranteed annual income (GAI). The idea of a negative income tax is based on two related principles. First, a family is guaranteed that their annual income will be at least some specified amount. Based on monthly reports of family income, a cheque is issued to bring the family up to the guarantee level for the month. Second, income earned in addition to the basic support is taxed at less than 100 per cent. By implication, a family is always able to keep a portion of every earned dollar. A combination of a guarantee level and a tax rate represents a NIT welfare plan.

MINCOME collected data to assess the impact of a NIT on a variety of social and economic outcomes, including labour supply, earnings, job search, consumer durables spending, marital satisfaction and marital stability, use of leisure time, investments in human capital, housing, various social and psychological factors (perceived quality of life, job satisfaction, feelings of self-adequacy, optimism/pessimism, and so on), community participation, mobility and others. (See Section II for more details.) The experiment took place in Manitoba, primarily in Winnipeg, beginning in 1975. Approximately 1,700 families were assigned to various control and experimental treatment conditions. The control group was composed of households (roughly, the poor and working poor) who remained eligible for existing welfare assistance throughout the course of the experiment. This group was considered to be the reference group against which the various NIT plans were to be compared. The experimental groups, 8 in number (later 7), were composed of different combinations of tax and guarantee, as shown below.

TABLE 1

Tax Rate

	35 Per Cent	50 Per Cent	75 Per Cent
Guarantee \$ 3800	Plan 1	Plan 3	*
at \$ 4800	Plan 2	Plan 4	Plan 7
Enrollment \$ 5480	X	Plan 5	Plan 8

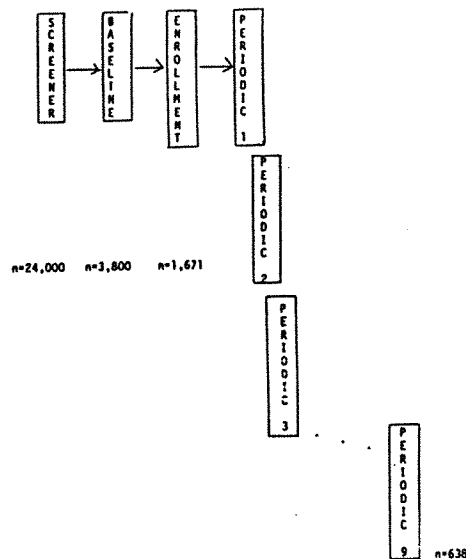
Plan 9 = Control Group

*Plan 6 was collapsed into Plan 7 due to sample attrition problems.

In a conventional factorial experimental design, the families would be randomly assigned to each cell, with equal or at least proportional allocation. Other things equal, the result would be data that would permit a straightforward partition of variation of a dependent variable into those components due to the experimental variables and random er-

FIGURE 1

Periodic Timing in the MINCOME Experiment



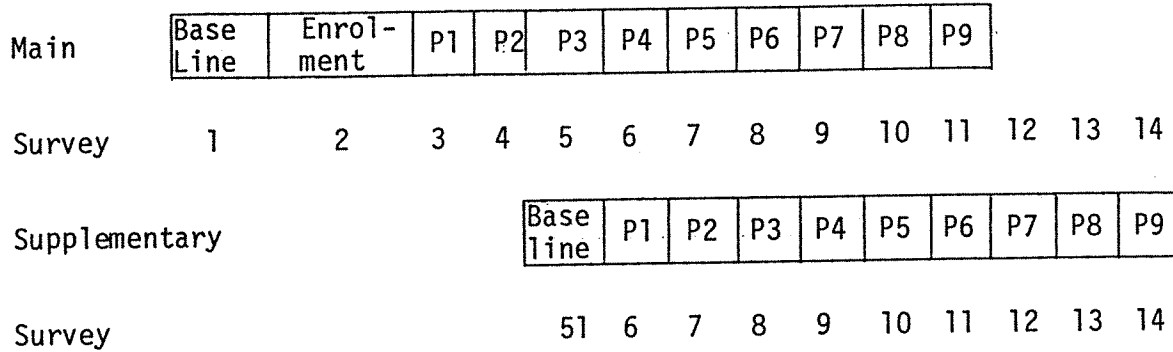
The sampling procedure was such that families interviewed early in the Baseline were positioned early in all subsequent surveys. In this way, there is roughly a four month interval between each family's response.

The MINCOME experiment may now be graphically illustrated as in Figure 2, where each of the surveys is shown. In addition, the approximate sample sizes for intact families (i.e., those with no change in headship) throughout the experiment for the three sites (Winnipeg, Dauphin, and Rural-Dispersed) are indicated. Finally, the Supplementary Sample is also shown in Figure 2.

In early 1976, it was feared that attrition within the experiment was endangering the entire exercise. As a result a Supplementary Sample was drawn, and these new families incorporated into the experiment. This "Supplementary Sample" is in effect a parallel experiment that lags the main sample by about 12 months.

FIGURE 2

The MINCOME Experiment - Main and Supplementary Samples



P=Periodic

This overviews the essentials of the MINCOME experiment, but several cautions and caveats are important.

1. Except for the Baseline, the MINCOME data cannot be treated as probability based survey research data. The sample up to the enrollment stage was selected using multi-phase random selection, but after the enrollment, the experiment became a panel study. The various surveys were used to monitor the outcomes of that experiment. This has very important implications for the appropriate statistics to be used in hypothesis testing using these data, some of which are explored later in this manual.

2. The support levels illustrated in Table 1 were not applied to all families, but pertained only to a double-headed, single earner family of four. Other household types had different levels of support.

3. Support levels were indexed by a yearly cost-of-living adjustment to reflect the then emerging fact of inflation (which peaked at 12.5 per cent in 1975).

4. In order to keep a family unit at the assigned tax rate (35 per cent, 50 per cent, or 75 per cent), MINCOME prepared all tax returns for families in the experiment (excluding controls) and either paid or rebated taxes to Revenue Canada to keep families within the prescribed tax range.

5. Parallel to the Surveys data shown in Figure 2, there is a Payments data base which represents three years of monthly data obtained from the income report forms required of each participant family.

The next section of the manual surveys the range of data contained in MINCOME.

II. VARIABLES IN MINCOME DATA BASE

Since the primary purpose of MINCOME was to measure the labour supply response to the various NIT plans, extensive data were gathered on hours worked, wage rates and related matters. Beyond the labour supply data, a variety of social, economic, psychological and behavioural variables were measured. Some of these were thought to be variables that might potentially be affected by a NIT program. Others were thought to be intervening or conditioning variables useful in understanding the direct effects of NIT on labour supply.

The MINCOME index in Appendix 1 categorizes questions under 22 major topics. These topical categories are shown below in Table 2, along with some subcategories. The number of titles refers, roughly, to the number of separate and distinct questions asked during the course of MINCOME, some 624 in all.

TABLE 2

Variable Categories in the MINCOME Data Base

1. Assets - 14 titles
2. Debts/Liabilities - 33 titles
3. Disposal of Property - 13 titles
4. Durables Owned - 19 titles
5. Earnings - 83 titles
6. Employment - 33 titles

Current Employment Status
Employment Status at Last Interview
Job Responsibility Changes
Labour Union Membership
Supervision of Employees
Termination of Job

7. Family Composition - 24 titles

Family I. D. Codes
Interview Codes
Members Moving Into Family Unit
Members Moving Out of Family Unit
Respondent Identifiers

8. Geographical Mobility - 20 titles
9. Housing Owned - 38 titles
10. Housing Satisfaction - 11 titles
11. Interviewer Comments - 20 titles
12. Job Attitudes - 16 titles

Occupational Prestige Ratings
Satisfaction with Job Ratings

13. Job Searching - 32 titles
14. MINCOME Knowledge Questions - 15 titles
15. Ownership of Non-Dwelling Property - 26 titles
16. Rented Housing - 19 titles
17. Respondent Characteristics and Attitudes - 86 titles
18. Standard of Living Attitudes - 26 titles
19. Unearned Income - 23 titles
20. Unemployment Benefits - 34 titles

Disability Status
Manpower Training Allowance Benefits
Unemployment Insurance Commission Benefits
Unemployment Status
Worker's Compensation Benefits

21. Vehicles Owned - 9 titles
22. Work Hours - 30 titles

III. USING MINCOME DATA

The variables listed in Section II demonstrate the wide range of topics on which data were obtained. The most obvious use of MINCOME data is in assessing the consequences to a NIT program. Beyond that, MINCOME data can serve a variety of research interests having nothing to do with guaranteed annual income policy. The sample is large, data were gathered over a four year period and a wide range of social, economic and psychological variables were measured. For some research purposes, the exact nature and amount of available welfare support may be relatively unimportant. For such research, the full sample may be used. For research questions that may be affected by the NIT program, there remains a set of some 300 control group families that are "uncontaminated" by a NIT program.

To use the data intelligently, it is important to become thoroughly familiar with the experiment. At least some of the technical documents should be read in detail. Income maintenance experimentation has some complexities and problems that are not immediately obvious. This section reviews some of the more important issues which have presented problems in the analysis of data from other NIT experiments; all are present in the MINCOME data. Certainly, not every problem exists for every piece of research, but many analysts will have to grapple with one or more of these confounding issues. It would be inappropriate to suggest solutions to each problem; these are matters best handled by the individual researcher in light of his or her own research interests. The Institute for Social and Economic Research maintains an advisory service to better acquaint potential researchers with these procedures and to provide methodological assistance.

*inconsistent. how
can one become
thoroughly familiar
by reading only
some...*

Researchers are strongly encouraged to exploit the Baseline survey, prior to attempting to research a particular question using longitudinal data.

*longitudinal extract/analyses
services offered?*

1. The Sample Allocation Problem

The data do not lend themselves to the traditional analysis of variance statistical models usually associated with experimental designs. Section IV summarizes the technical report dealing with this problem and Technical Report 2 examines the matter in detail, but a brief overview of the problem is in order.

In short, the sample was not assigned randomly to treatment conditions. The cells are not of equal size nor are they proportional, implying that the tax rate and guarantee levels are not orthogonal. Moreover, the allocation model assigned families of different types to varying plans with different probabilities. Families were stratified on the basis of family composition type and by "normal income." The allocation model, based on the Watts-Conlisk model used in most of the previous income maintenance experiments, took into account the expected form of the response surface, the expected cost of different sample points, the policy interests and other factors in an attempt to get the

least expensive allocation of families in the design to commensurate with the scientific and policy goals of the experiment. The rationale is presented in Technical Report 2 and the references cited in that document. The important point here is that there were differences between the various experimental groups prior to the experiment. These differences are due to the way families were allocated to the cells and not to the experimental effects. Any attempt to assess experimental effects must control for these pre-experimental differences. Most analysts of the U.S. NIT experiments did this with multiple regression models that used the stratification variables (income and household type) in some form as control variables. There are other approaches as well.

2. The Nature of the Treatment

A first reading of materials covering the experimental design used in MINCOME may leave the impression that the design is relatively straightforward. There are two experimental variables, tax rate and guarantee level, each having three levels thus producing 9 possible treatments plus a control group. The only complication seems to be in the two empty cells; however, this is not the whole story.

For much research, these complexities are unimportant. But in those research efforts concerned with the nature of the treatment and its consequences, several points must be kept in mind. First, the tax rates and guarantee levels presented in the design tables do not hold uniformly for all families in each cell. In particular, guarantee levels are indexed by family type and size. The tabled values (see Table 1 above) are for a family of four composed of two adults and two children. Moreover, during the course of the experiment, the guarantee levels were indexed so as to keep the guarantee from being eroded by inflation.

Second, families having the same size and composition may not receive identical payments for the same month. This point is an obvious consequence of the nature of NIT experimentation, but it is worth keeping in mind that families in a particular group are homogeneous only with respect to the rules governing the calculation of their monthly support cheque, and not necessarily with respect to the amount of the cheque issued. Various factors discussed in Technical Report 1 influence the amount of the cheque: assets, earnings, tax rebates, year-end reconciliations, etc. For some research, it will be enough to treat families in the same cell as equivalent because they are under the same set of rules (the treatment condition). For other efforts, it may be necessary to understand what was actually given to the family and for these projects it will be necessary to use the more complex version of the "treatment."

Third, treatments under income maintenance experiments are more dynamic in some respects than is usual in experimental designs. Variations in treatment occur for the reasons given above, but for other reasons as well. An experimental family earning enough to put them above the break-even point (the point at which, theoretically, the subsidy goes to zero) faces new rules. In particular, their tax rate becomes the positive tax holding for the general populous. The guarantee still exists for them, but it exists rather as a floor below which they know

they will not fall. It does not serve as a mechanism for producing income as a function of earnings; the income has become zero. This complexity may be safely ignored for most research. But, for projects attempting to include a set of variables reflecting at any given point the expected return for a dollar earned, the representation of the treatment in effect at each point in time will be more complicated than may be anticipated from brief descriptions of the experiment.

Finally, the nature of the control group should be considered. At one level, it may make sense to think of this as a single reference group, all members of which have in common their eligibility for public welfare as determined by the laws of Canada and Manitoba during the course of the experiment. However, for other purposes the picture is not so clear. Consider, for example, an analysis aimed at assessing the effect of the tax rate on hours worked. For the experimental families, the tax rate -- except for problems mentioned above -- is the assigned level. What tax rate is assigned for the control group? Clearly, the tax rate is not zero. They face the usual progressive tax schedule faced by other people in Manitoba at the time of the project. That rate increases with earnings, other things being equal. Stating labour supply as a function of the tax rate, among other things, means that some value or values must be used for the tax rate faced by controls. It is not impossible to arrive at such a figure, but it is not a value immediately available from the design specifications. Of course, not all research requires a tax rate variable for the controls. For those that do, though, the problem of a changing endogenous tax rate for controls is important to be grappled with.

There are other aspects of the design that present problems, but these have been the major issues in prior research. A careful reading of the relevant technical documents, particularly numbers 1 and 2, should equip a researcher to anticipate and resolve most of the more serious issues.

3. The Supplementary Sample

MINCOME elected to enroll an additional group of participants approximately one year into the experiment because of a perceived under representation in some experimental cells. This group of subjects will have to be considered separately for some types of research. Any research concerned with a phenomenon sensitive to secular trends in the economy, for example, will want to examine the supplementary sample separately because the first experimental quarter of this group does not correspond to the first experimental quarter of the main sample. This can be accomplished by creating a dummy variable reflecting membership in the Supplementary Sample. Other differences are found in the survey questionnaire. By the time the Supplementary Sample was enrolled, the instrument was relatively stable and, therefore, early Periodics may not be compared directly with the same data on the main sample.² Such

² Unfortunately it was not until Periodic 3 of the main sample that the survey instrument was stabilized.

comparisons can be made, but only after some additional work. In addition, the Supplementary Sample was not allocated to all the cells used in the main sample. These and other issues are discussed in Technical Reports 1, 2, and 6.

4. Changes in the Design

The summary of Technical Report 1 in Section IV discusses the collapsing of plan 6. Because this plan was not producing enough useful data points and because of the deterioration of the sample size, a decision was made to re-allocate this cell's participants to other plans. The justification is given in Technical Report 1. Research aimed at assessing experimental impact must take the change into account.

5. Unit Problems

The primary unit for experimental purposes was the family. The family was the unit on the basis of which guarantees were calculated and cheques issued. However, the definitions of just who is in a family, who is head of household and related issues must be considered. Technical Report 12 reviews the definitions. Users of the data should become familiar with the definitions because they do not in all cases correspond to definitions used in other income support programs. Moreover, a family unit could change: births, deaths, marriages, divorces, moves into and out of the household, all have the effect of changing the family. To make matters more complex, an individual may at different times be part of different participating families. These problems are likely to be encountered at the point of requesting data. The suggestion entered here is simply that the researcher be aware that the unit may undergo change, quite substantial in some instances, and that the data request must make allowance for that change. It will not do to ascertain family composition at the beginning of the experiment and to assume it is constant throughout. Even such obvious changes as a switch in household head must be considered.

Another aspect of the unit problem arises when a researcher is concerned with individuals rather than families. The MINCOME data permits the analysis of individual data of various sorts. For these endeavors, it will be useful to consider much of the family data as applying to the individual members. That is, family assets or income may be considered as a characteristic of the member individuals. While the distinction may seem a bit overdrawn, it is useful to bear in mind in defining data sets. Finally, individuals are also dynamic; they move into and out of families and even into and out of the experiment. Researchers should give some thought to how to handle individuals whose membership is intermittent and/or dynamic.

6. Attrition

The issue of attrition is discussed in detail in Technical Documents 5, 6 and 7. (These documents also consider the related matter of selection bias). The problem is that attrition is non-random and consequent-

ly it can be confounded with experimental effects. Some types of families were more likely to leave the program and different cells in the experiment have different attrition rates. Methods for adjusting for this vary greatly. It is beyond the scope of this section to do more than indicate that the problem exists. Selecting a method for dealing with the matter is best left to individual researchers. Once again, the Institute for Social and Economic Research will provide technical assistance on this matter.

7. Changes in the Instrument

Technical Report 12 discusses the way in which the questionnaire changed through time. After the first year, the instrument was relatively stable, but before that point the main sample had been asked about some topics in a way that differs substantially from the methods used after that point. Reasons for the change are discussed in Report 12. Researchers should check the surveys catalogue carefully to see how the question changed over time and to take the change into account in formulating data requests.

8. Payments Data

The survey data form the major part of the archive. However, there is an additional data source that is available. The Payments data includes all the information on the basis of which cheques were issued, along with the cheque amounts and certain accounting information regarding tax rebates and year end reconciliation. Researchers needing cheque amounts will have to use this file, but there are one or two quirks involved. The Payments office was relatively independent of the survey operation. Therefore, there are some important discrepancies between the two data sets. The periods of time covered in the two are not equivalent. A particular IRF (an income report form used by the Payments office to calculate cheque amounts) may cover a month that is covered by the survey operation in two separate interviews. In addition to problems to make the two synchronous, there may be discrepancies in the recorded household composition. Household composition changes were picked up only when people were interviewed or when the family sent in IRF's. Since the IRF's were done monthly and the interviews were done three times a year, changes in family composition would not be picked up at the same time.

These are not the only problems in using MINCOME data and all of these are not of equal importance. Some research projects may be relatively untroubled by even the more serious of the above list. They are offered here because experience has shown them to be frequently encountered in income maintenance experimentation or because of problems unique to MINCOME. This list, no doubt, will grow as experience in analyzing the data increases.

It is important to stress that despite these aspects of the data, a variety of statistical procedures do exist to permit these data to be analyzed using modern social science techniques. A major responsibility of the Institute for Social and Economic Research is to provide the research community with comprehensive support to analysis of MINCOME data.

Also, the Baseline survey is unhampered by most of the above problems. It is a very detailed micro survey of low income families, and by itself is a powerful addition to the information base for social and economic analysis in Canada.

The next section provides a synopsis of the technical documentation presently available to assist in understanding the MINCOME experiment and the data.

IV. TECHNICAL DOCUMENTATION

1.0 Introduction

Technical details of the MINCOME experiment are documented in a series of thirteen Technical Reports. Depending on research interests, one or more of these reports will be essential reading for any secondary user of the data. The following provides brief summaries of each of the technical reports, highlighting the more important contents and excerpting relevant sections. The expectation is that potential users of the data will use these summaries to make informed decisions about which Technical Reports to acquire for more detailed study. The summaries that follow, in other words, are adequate as a guide to the contents of the Technical Report series, but not as a substitute for them.

2.0 The Object and Design of MINCOME (Technical Report No. 1)

In both objective and design, MINCOME is similar to the several negative income tax experiments undertaken in the United States. The main research objective is to estimate the behavioural response of families and individuals to a guaranteed annual income support plan. The income support plan tested is a negative income tax scheme, whereby families below a designated income floor receive payments tied to their earned income, so as to guarantee a basic minimum income level. In general, NIT has been conceived, both in Canada and the United States, as an alternative to conventional welfare systems. The presumed advantages are two-fold. First, the administrative overhead for a national NIT program should be substantially less than the overhead incurred in conventional welfare support programs. Secondly, and more critical to the purposes of the experiment, by allowing families to keep some share of their earned income (rather than reducing payments dollar for dollar against earnings), NIT programs are thought to avoid the presumed work disincentive effects inherent in conventional welfare programs. The general idea of NIT is fully described elsewhere. See, for example, Peter H. Rossi and Katharine C. Lyall, Reforming Public Welfare (New York: Russell Sage Foundation, 1976), Ch. 1.

An NIT plan is defined by two parameters: the income guarantee level (G) and the tax rate (t). Typically, G is adjusted for family size and composition (the guarantee being larger for larger families). The tax rate, t, is the amount by which payments are reduced for each dollar of

earned income. The MINCOME experiment was designed to test for "optimal" combinations of G and t, that is, to determine experimentally which combinations of G and t produce the most positive behavioural outcomes within a feasible cost range. The MINCOME treatment conditions, or "plans," are therefore defined by discrete combinations of these two experimental parameters.

In the MINCOME experiment, no less than in the U.S. NIT experiments, the overriding experimental concern is with the labour supply response. This includes the labour force participation decisions of primary and secondary earners, the number of hours worked, the job search process, job satisfaction, and investments in human capital, among others. Data collection, therefore, was focused on (but not restricted to) labour supply variables. Secondary research issues concerned the effects of NIT on family formation and dissolution, the behavioural response of youth, community involvement and participation, consumption decisions (for example, housing), and geographical mobility. Most standard social and demographic variables, and some social psychological variables, are also contained within the data base.

As in any classic experiment, participating households are allocated either to treatment or control conditions. In MINCOME, the experimental treatment is the set of conditions governing the payment made to any particular family. (Control families, of course, received no payments other than a small sum paid for agreeing to be interviewed and for filing the monthly Income Report Forms [IRF]). Payments, P, are determined according to the following formula:

$$P = G - t.Y - r.W$$

where P = the payment under the NIT program
 G = the support level for a representative family
 t = the offset tax rate
 Y = family income
 r = the net worth tax rate
 W = family net worth (assets minus liabilities)

Of the several parameters in the above equation, only two are experimentally determined, G and t, as described below. Family income, Y, and family net worth, W, are, of course, determined by the economic behaviour of the family unit. The tax rate on net worth, r, was constant for all experimental families (0 per cent on the first \$3,000.00 of net worth, 4 per cent from \$3,000.00 to \$10,000.00, 8 per cent from \$10,000.00 to \$30,000.00, and 16 per cent thereafter, with an additional \$20,000.00 exemption for farmers).

Three guarantee levels were experimentally tested. In 1975, when payments to experimental families commenced, the guarantee levels were \$3,800.00, \$4,800.00, and \$5,800.00 for a family of four composed of two adults and two children. The actual support guarantee was adjusted

above or below these figures according to a Family Size Index (FSI): for example, the guarantee level for a single-person household (one adult, no children) was 38 per cent of the above values, whereas the guarantee level for the largest possible family was 247 per cent of the above values. These support levels were further adjusted periodically to maintain approximately constant real value over the three year duration of the experiment. The most generous possible plan (high guarantee, low tax rate) was ruled out in the design stage on grounds of cost. Households in the least generous possible plan (Plan 6, combining the lowest guarantee and the highest tax rate) were transferred to Plan 7 after start-up, to minimize attrition. The experiment thus consists, over its full run, of 7 treatment groups and 1 control.

Every treatment plan has a break-even point, B, which (assuming zero net worth) is defined as G/t . B is the point at which the family's earned income is sufficiently high that NIT Payments cease. Above that point, a family would be gradually phased into the normal, or positive, income tax (PIT) system. Thus, the integration between the experimental NIT and official PIT taxation systems is critical to the integrity of the experimental treatments. To maintain full experimental control over the true tax rate, any taxes owed (other than those determined by the tax rate for the experimental plan) were simply rebated to families beneath break-even. When a family exceeded break-even, the tax rebate was gradually phased out, reaching zero at the tax equivalence point. Note, then, that families above break-even could still receive some payments.

There is a similar problem in integrating the experimental NIT plan with the existing welfare system. Again, in order to maintain experimental control over the true tax and guarantee levels, MINCOME participants that were otherwise eligible for other welfare programs had to choose between MINCOME and welfare (with some exceptions). In essence, this amounts to taxing welfare benefits at a 100 per cent rate.

The experiment ran for three years; as in other short-run NIT experiments, the "time horizon" is an important design issue (in short, whether three years is "enough" time for labour supply adjustments to be made). Families filed monthly IRF's and received monthly payments according to the Payments formula appropriate to their plan. Year-end adjustments against the actual entitlement were made.

Sample Design and Assignment (Technical Report No. 2)

MINCOME was jointly funded by the Federal Government of Canada and the Province of Manitoba; the experiment was confined to Manitoba. All told, the experiment was run in three sites, each differing substantially from the other. The "urban-dispersed sample" was drawn from the population of the city of Winnipeg (which contains over half the total provincial population). The "rural-dispersed sample" was drawn from the population of 18 small rural communities in Manitoba. The "saturation sample" was drawn from the city of Dauphin.

The two dispersed samples are stratified probability samples of income-eligible households (as determined through pre-enrollment surveys).

It was recognized in the U.S. NIT experiments that samples of this sort, while ideal from a research viewpoint, do not represent the conditions that would obtain in a fully implemented national NIT policy, under which eligibility would be universal. And certainly, it is possible that the behavioural response under universal eligibility would be sharply different than the responses observed in a more tightly-controlled experimental condition. To approximate the condition of universal eligibility, the "saturation" site of Dauphin was added, where the entire community was declared eligible to participate in the experiment and receive payments. The inclusion of the saturation site therefore greatly enhances the policy relevance of the experiment. Note, however, that there is no obvious control group with which the Dauphin results may be compared, although controls in the rural sample may serve that purpose under certain assumptions.

Payment plans differed by site. The full set of experimental plans shown in the previous section were implemented only in the urban-dispersed sample. In the saturation site, and in the rural-dispersed sites, all experimental families were on a single plan: the low guarantee and middle tax rate plan (Plan 3). The saturation site was restricted to a single plan because it makes no sense to have more or less generous plans universally available.

Experimental participants in the dispersed samples were located through large-scale Screening interviews in the chosen sites. (In Dauphin, a Screener was attempted on all residents, but families could also participate on a "walk-in" basis. In all 29,948 families were contacted during the Screening phase, and complete Screening data were obtained on 21,658 (72.4 per cent). Among the families for whom Screening data were obtained, 6,372 (about 29 per cent of the total) were determined to be potentially eligible for the income maintenance program. These families were then approached for the Baseline survey (Survey 1), on whose basis final eligibility would be determined. Data from Screening and Baseline surveys were also used for purposes of sample stratification and allocation to the various treatment and control conditions, as described below.

Complete Baseline data were obtained on 3,819 of the 6,372 potentially eligible reporting units (59.9 per cent). Another 1,002 (15.7 per cent) refused to participate in the Baseline survey, 863 units (13.2 per cent) had moved since the Screener and could not be located, and there were 688 (10.7 per cent) other non-completions, due mostly to "not at homes." (Later in the experiment, attempts were made to contact families who refused the Baseline survey; these and other aspects of the field operations are outlined in the summary of Report No. 12).

Of the 3,819 families for whom complete Baseline data were gathered, 2,394 (63 per cent) were determined to be eligible (according to income and family composition considerations) for experimental participation and were approached for Survey 2, the Enrollment survey. At Survey 2, families were told that they were eligible to receive income support payments and that they would later receive an enrollment package. Of the 2,394 families approached for Survey 2, 1,865 (78 per cent) completed the Enrollment survey.

Experimental families were allocated to one of the various treatment conditions, or to the control group, according to a rather complex optimum allocation model. A full discussion of the allocation model employed is beyond the scope of this summary. Since labour supply response was the central outcome of interest to the experiment, certain segments of the population were automatically excluded from participation, for example, the aged, the institutionalized, the disabled, etc. The remaining families were stratified according to family structure and sampled at different fractions, on grounds that the labour supply response would vary by family structure. Family units were further stratified according to an estimate of "normal earned income" (earned income from which an estimate of the transitory component is removed), again on the grounds that the labour supply response might vary according to the family's permanent income.

The allocation problem is as follows. Given an expectation that labour supply response would vary by family composition and earned income, and that either or both of these factors might interact with the experimental variables themselves, each treatment condition must contain enough families of each composition and normal income level to estimate the appropriate interaction effects. To illustrate, in the case of the Winnipeg, or urban-dispersed sample, there were 18 "family type by income level" cells that were determined to be of interest to the experiment. There are, in addition, 9 treatment conditions (8 - then later 7 - payment plans, and one control group). The urban-dispersed sample must therefore be allocated over $18 \times 9 = 162$ distinct cells. The most efficient possible allocation of the sample depends on the assumed functional form of the response surface, the variance in response across cells, and the cost of each observation within any particular cell. The latter is an especially important consideration in NIT experiments, since the treatments consist of fairly sizeable payments to participating families: as such, the cost of an observation in the most expensive plan is about 24 times that in a less generous plan (or in the control group). Again, the details of the allocation process are beyond the scope of this summary. Final results for the Winnipeg site allocated just under 40 per cent of the cases to the control condition, with the remainder of the sample dispersed across the various treatment conditions. In all, 1079 families were enrolled in the experiment from the Winnipeg site: 370 of them were allocated to the control group, and the remainder to one of the eight (later, seven) treatment plans. Sample sizes varied across treatment plans from $N = 58$ to $N = 133$.

Sample allocation in the rural-dispersed site was greatly simplified because there was to be only one payment plan, and thus, only one treatment and one control condition. As in the urban-dispersed segment, there were again 16 "family type by income level" cells of interest, which, times two experimental conditions, gives 32 cells across which the sample was allocated. In the end, the results of the theoretical allocation model for the rural-dispersed sample had to be modified because there were insufficient sample points available in some strata.

Shortly after Payments began, it became apparent that non-participation and attrition were serious problems: the number of families actu-

ally receiving payments were much lower than expected, and if attrition out of the experiment continued at the early rate, there would not be enough cases left at the end of the experiment to sustain the intended data analyses. (This problem is discussed more fully in the summary of Technical Report No. 6.) As a result of these and other considerations, a Supplementary Sample was enrolled. The Supplementary Sample was restricted to the urban-dispersed site and confined to those household types and income strata for which the then-current sample sizes were thought to be inadequate. As with the main sample, Supplementary sample payments continued for three years, but began one year after the start-up of the experiment (and thus continued for a year after close-out for the main sample). In all, 293 additional families were initially added to the experiment in the Supplementary Sample, all of them double-headed (i.e., two-adult) households. This sample was allocated to various treatment (or the control) conditions mainly according to where the need for additional sample size was greatest.

In sum, there are four distinct portions of the MINCOME sample. Among the four, the Dauphin, or "saturation," site is unique and is discussed more fully later (in the summary of Technical Report No. 8). The remaining three portions constitute the analytic sample. The distribution of the sample in each portion according to treatment and control conditions is shown in the following table:

TABLE 4

	<u>Sample Sizes at Enrollment</u>	
	<u>Treatment Group</u>	<u>Control Group</u>
Urban Dispersed	704	370
Rural-Dispersed	103	166
Supplementary Sample	196	97
TOTALS	1,003	633

Source: The Sample Design and Assignment Model Technical Report No.2, MINCOME Appendices A.5, A.6, A.7.

Thus, excluding the saturation site, there were initially 1,637 families who participated in the experiment, some 61 per cent of whom were in the various treatment groups. Final sample sizes for the data base are somewhat different than the above numbers, because of various family "splits" and "merges"; see the summary of Technical Report No. 12, below. Of course, throughout the experiment, attrition eliminated many respondents.

The Payments System: Design and Administration
(Technical Reports No. 3 and No. 4)

The MINCOME experimental treatment is a monthly income transfer payment made to participating families. Since the size of the payment var-

ies not only by plan but also by earned income, any NIT Payments system will necessarily include a method of measuring the income or need of the recipients on a periodic basis, a system of calculating the payments, and a method of delivering the cash transfers to the recipients. Collectively, the above constitutes the MINCOME Payments system.

To the extent possible, the MINCOME Payments system and the ongoing survey operation were kept distinct, mainly to approximate the "real world" condition. To this end, a separate agency, MINCOME Manitoba, was established to administer the Payments system. Two Payments offices were set up: one in Winnipeg, which administered all payments to both the Winnipeg and rural dispersed samples, and the second in Dauphin, the saturation site.

Families designated as reporting units (according to the Payments formula given earlier), received income-conditioned payments through the Payments system. Payment amounts were based upon an annual accounting period, the calendar year, and a monthly payment and reporting period. Reporting units filed monthly income statements on Income Report Forms (IRF's), which were used as the basis for calculating payments. The system also included a carry over provision, the reimbursement of positive income tax withholdings on a monthly basis, a system of annual reconciliation to close out each calendar year accounting period, and self-reporting of income. In addition, there were formal provisions for payment adjustments to correct any over or underpayments that occurred. As well, procedures also existed to reset carry over and outstanding payment amounts whenever changes in family composition made such adjustments necessary. Consult Technical Report No. 9 for details on each of the above.

The administrative procedures followed by MINCOME Manitoba are, as one would expect, rather complex and detailed. This summary focuses on the broad outlines of these procedures and does not discuss the scores of specific rules and regulations followed. The latter are contained in the Rules of Operation, which must be consulted for administrative details. (Several major changes in the Rules of Operation were instituted in July, 1975; these are described in Technical Report No. 4, pp. 24-30.)

To be eligible to participate in the experiment in either Winnipeg or the rural-dispersed sites, a person must have been selected by MINCOME Manitoba as part of the final experimental panel, or have resided with a person so selected and become a "mandatory member" of the "reporting unit." (See below for definitions.) To be eligible in the saturation site (Dauphin), a person must have been residing there as of July 1, 1974, as well as when he/she applied for enrollment in the Payments program. In all sites, eligibility was restricted to persons who were Canadian citizens or landed immigrants.

Members present when the reporting unit was initially selected for enrollment in Payments were eligible, should they subsequently leave the original unit, to form their own unit, provided they were an adult member or the spouse of an adult member. Members who joined a reporting

unit as mandatory members after enrollment, and remained with the enrolled unit for six months, also became eligible to subsequently form a new unit under the same conditions as those applied to members of the original unit.

The basis of the Payments system of MINCOME Manitoba was the "reporting unit." The definition of "reporting unit" centered upon an adult member and the family related to and living with that adult member. "Reporting units" could contain more than one adult member. However, one adult member was designated as the "filer" and was responsible for submitting all required reports.

(Certain members of the household were "mandatory members." "Mandatory members" included the spouse and children of the adult member (or spouse), unless the children were either married or had children of their own. Unmarried adult members of the household were included as "mandatory members" if they were parents of other "mandatory members" or if they were married, non-parent children of the parents in the household).

As would be anticipated from the above comments, the number of families actually receiving payments varied substantially from month to month. For example, a family whose earned income was above break-even in any month would receive no NIT payment for that month. A family was also ineligible to receive payments if it refused to participate in any of the Periodic surveys. Families could also simply refuse the transfer payment, even if the Periodics were completed. Finally, as noted above, under certain conditions, family members could take their payment eligibility with them if they split from the initial reporting unit. Thus, the size of the Payments sample continually fluctuated. The following table shows the total number of reporting units ever enrolled in the Payments program, by site and experimental condition:

TABLE 5

Total Number of Reporting Units Ever Enrolled

	<u>Winnipeg</u>		<u>Dauphin</u>	<u>Rural</u>	<u>Total</u>
	<u>Original</u>	<u>Supple- mentary</u>		<u>Dispersed</u>	
Treatment	725	246	586	99	1656
IRF Control	183	34	-	65	282
PC Control	200	46	-	89	335
Total	1108	326	586	243	2273

Note that the table gives figures separately for "IRF Controls" and "PC Controls." IRF controls filed monthly Income Report Forms, just like the families actually receiving payments. This was to control for the possibly contaminating effects of the income reporting requirements. Other control families (the PC controls) gave only a monthly post card report of their addresses and family composition.

The table on the following page shows the average monthly sample available to the experiment, by site, year, and by treatment or control conditions. The table also shows the number of reporting units who actually received a transfer payment, the percentage of them that received the minimum payment, and the average payment made to those receiving the non-minimum payment.

TABLE 6

Average Monthly Sample by Site/Year

	Treatment, Postcard and IRF Controls		Treatment Group			Control Group	
	No. of Units Paid	Average Payment	No. of Units Paid	% Receiv- ing Non- Minimum Payment	Average Non- Minimum Payment	No. of IRF Control Paid **	No. of Postcard Control Paid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<u>Winnipeg*</u>							
1975	805	\$ 50.33	490	44.64%	\$158.85	167	148
1976	1116	84.65	719	55.53	218.99	203	194
1977	1018	81.04	673	49.15	229.16	172	173
1978	225	138.93	163	62.84	293.47	26	36
<u>Rural Dispersed</u>							
1975	159	21.74	53	34.28	113.08	50	56
1976	181	27.65	62	38.45	143.27	51	68
1977	177	24.33	60	30.33	149.85	50	67
<u>Dauphin</u>							
1975	351	67.63	336	45.11	141.94	15	-
1976	410	92.71	386	55.92	169.13	24	-
1977	428	88.58	405	49.99	176.50	23	-

* Winnipeg includes both the Original Sample (1975-1977) and the Supplementary Sample (1976-1978)

** IRF Controls include Welfare Convert Units (Treatment units converted to IRF Control because they receive welfare payments)

with a separate employer. Other normal reduction rate income included almost all other income received from private sources as well as certain types of income from government sources. Also included was income-in-kind received from an employer where such receipts were not designed to cover employment expenses incurred by the employee. Two types of income excluded from other normal reduction rate income were inheritances and lump-sum life insurance benefits. These were not counted as income but were treated as an increase in the net worth of the reporting unit (see below).

Income subject to the one hundred per cent reduction rate included a number of government transfer payments, such as unemployment insurance benefits, Canada Pension Plan benefits, Old Age Security benefits, War Veterans Allowances and Civilian War Allowances, and the market value of the income-in-kind of rent-free or subsidized housing provided by a government source.

Income totally exempt from any reduction rate included special needs and medical needs funds received from a means tested welfare program by a MINCOME Manitoba participant previously on that particular program. Normal maintenance payments received from a welfare agency before the initial MINCOME Manitoba payments were also exempt, but thereafter were subject to the one hundred per cent reduction rate. Also exempt from any reduction rate application were payments received for the maintenance of a foster child and day-care subsidies.

The net worth of a reporting unit as well as its income, was considered in the payment calculation, and equalled the difference between the market value of the assets and the amount owing on the assets. All assets of the unit except for exemptions specifically allowed in the Rules of Operation were included. The definition of assets included the following three types -- personal property, financial and real estate property, and business property. In calculating the net worth amount, unsecured liabilities were subtracted from the value of the assets. However, the final net worth amount was not allowed to be less than zero. A dwelling occupied as a principal residence by a reporting unit was excluded from net worth after 1975, although it was included for the 1975 calendar year. If an individual owned a business, the net worth of the business was equal to assets minus liabilities, and where an individual owned shares in a business the value counted was the market value of the shares. Net worth (for non-businesses) was reported annually rather than monthly.

Evaluation of the Experimental Sample (Technical Report No. 5)

In 1977, the MINCOME staff requested an independent evaluation of the quality of the MINCOME data, which was prepared by Professor Mordecai Kurz of Stanford University. Kurz's assessment of the experimental sample is contained in Technical Report No. 5.

In general, there are no absolute standards by which the "worth" of scientific data can be judged. The strategy followed in the present

case was to compare MINCOME results with those obtained in the various U.S. NIT experiments. Attention is focused on problems of attrition from the experiment, sample allocation, and the non-participation rates for the Screener, Baseline, Enrollment, and Periodic interviews.

Some rather serious problems ,to which MINCOME was certainly no exception, are inherent in all large scale social experiments. One problem is that of truncation of the sample according to an income criterion. Near the cutoff point for eligibility (wherever it happens to be), harder working people stand a higher chance of being excluded from the sample, which would have an obvious implication for the labour supply response. Refusal to participate in Payments is also a problem in all NIT experiments and tends to be correlated with generosity of plan.

Other problems vary in seriousness from one experiment to the next. Concerning the optimum allocation model used to allocate the sample to treatment and control conditions, Kurz notes that the procedures followed were very similar to those employed in the U.S. experiments, were implemented correctly, and were perfectly sound.

Despite the theoretical soundness of the sample design, non-completions, refusals, and attrition out of the experiment were all substantial. As noted earlier, the Supplementary Sample was enrolled in 1975 as a response to this precise problem.

The MINCOME non-completion rates for the Screener and Baseline surveys were similar to those experienced in the U. S. experiments, once allowance is made for the generally higher rate of mobility found in the Canadian Prairie.

Non-completion in the Enrollment interview was higher than observed in the Denver experiment, but lower than that observed in Seattle. Kurz advances several possible explanations for these differences, of which the very long interview schedule and generally inexperienced interviewers are most important.

Annualized attrition rates from the MINCOME Periodics are definitely higher than the rates experienced in the U. S. experiments, although the rates tend to fall with time. There is, however, no clear evidence that attrition during Periodics was non-random.

In general, Kurz's conclusion is that the scientific calibre of the MINCOME experiment is similar to that of the comparable U. S. experiments. None of the problems reviewed is serious enough to vitiate the scientific usefulness of the data base.

Sample Development Over Time:

Participation and Attrition (Technical Report No. 6)

As noted before, the MINCOME experimental sample is comprised four segments: the Winnipeg (urban-dispersed) site, the rural-dispersed sites, the Dauphin (saturation) site, and the Supplementary Sample. Each segment of the sample evolved over time, through attrition, non-participation, and changes in family status and composition.

All segments except the Supplementary Sample (there was no Enrollment survey for the Supplementary Sample) were interviewed 12 times: twice prior to enrollment (the Screener and Baseline surveys), once coterminous with enrollment (the Enrollment survey), and every four months thereafter for the three year duration of the experiment (the 9 Periodic surveys). The Screener was a very short interview designed to locate potentially eligible families. Families deemed potentially eligible in the Screener were then approached for the Baseline survey, which was a lengthy, in-depth survey gathering data on earnings, wage rate, net worth, employment history, and so on. Families deemed eligible for program participation from the Baseline survey were approached for the Enrollment interview. Completion of the Enrollment interview was a necessary requirement for enrollment in the Payments program. Once enrolled, the reporting unit was then required to complete a Periodic survey every four months, with payments continuation conditional on having completed the most recent Periodic. Obviously, sample points could be lost at any of the above stages through attrition, non-participation, dissolution of the reporting unit, geographic mobility, etc.

Some families were, of course, ineligible because they were not of direct interest to the experimental purposes of the research, for example, the aged, the disabled, the affluent, etc. Following is the list of criteria on whose basis ineligibility for participation was determined:

1. Households with either head over 57 years of age at time of enrollment;
2. Households with an average 1971/1973 yearly income, adjusted to a family size of 4, in excess of \$13,000;
3. Mentally incompetent households;
4. Households with a language barrier to answering in English;
5. Households with one or more heads in the armed forces;
6. Households with disabled adult members;
7. Members of a religious order;
8. Institutionalized households;
9. Employees of MINCOME Manitoba;
10. Households comprised of more than 5 roommates living in the same dwelling.

The eligibility criteria for the rural dispersed sites and the Dauphin Site were similar, except that in the Dauphin site, one head could exceed 57 years of age (but had to be less than 63), if the other head

Again, the largest share of losses between Screening and Baseline was in the "refusal" category. Moves were also important, especially in Winnipeg. In part, the high rate of loss from moves is due to the long time which elapsed between the Screener and Baseline surveys. In any case, the completion rate in the Baseline survey was just under 60 per cent. (Some of the initial "refusers" were, of course, enrolled during the Recontact Program discussed earlier.)

Since there are Screener data available for reporting units who did not complete the Baseline survey, some assessment of the nature of the sample losses at the stage of Baseline can be made. The patterns differ by type of non-completion (refusals vs. moves vs. others) and also differ by site. In general, single-headed households had the highest completion rate, single individuals had the highest move rate, and double-headed households had the highest refusal rate. Refusals were less common among welfare recipients than others, but moves were more common. Refusals also tended to increase with the number of earners in the unit, and so on. Correlated sample biases introduced by non-completion at Baseline, in other words, are potentially quite important and should be carefully examined by any potential user of these data.

Complete Baseline data were eventually obtained for 3,819 reporting units. Of these, 2,394 were determined to be eligible for the experiment and were approached for the Enrollment survey. (It must be emphasized that families were not actually enrolled in the Enrollment survey; rather, completion of the Enrollment survey was a necessary pre-condition for self-enrollment in Payments later, once an enrollment package had been sent.) The following table shows the disposition of the 2,394 Enrollment survey attempts:

TABLE 9

Segment	Completions	Refusals	Moves	Other Non Completions	Total
Winnipeg:	1079(74.8%)	218(15.1%)	124(8.6%)	21(1.5%)	1442
Rural Dispersed:					
Non-Farm	182(72.5%)	38(15.1%)	22(8.8%)	9(3.6%)	251
Farm	88(83.0%)	18(17.0%)	-----	-----	106
Dauphin:					
Non-Farm	410(85.0%)	31(6.4%)	39(8.1%)	2(0.5%)	482
Farm	106(93.8%)	7(6.2%)	-----	-----	113
TOTALS	1865(78.0%)	312(13.0%)	185(8.0%)	32(1.0%)	2394

Note first: Enrollment survey results in the rural-dispersed and Dauphin sites are given separately for farm and non-farm households. The farm portion of these two sample segments was problematic throughout the MINCOME experiment. At the stage of the Enrollment survey, the key problem was one of sample allocation. As discussed earlier, sample allocation was based on an estimate of "normal income," earned income from which the transitory component is removed. Assigning a "normal income" to farm households is, for obvious reasons, a very complex undertaking. Therefore, farm households were classified simply as "eligible" or "not eligible" for program participation. (In Dauphin, "eligible" farm households were assigned either to treatments or controls. In the rural-dispersed sites, all eligible farm households were assigned to the control group.) In addition to the sample allocation problem, the refusal rate at Payments enrollment was also noticeably higher among farm than non-farm families (in both Dauphin and rural-dispersed sites). Given the enrollment in the Payments program, non-completion of the Periodics was also higher among farm households than others. For these and other reasons, interviewing of farm households was discontinued after the Third Periodic.

Of the 2,394 households approached for the Enrollment survey, completions were achieved with 1,865, a completion rate of about 78 per cent. As before, most non-completions were due to refusals and moves, the former the more important source of loss by far. Analysis of non-completions in the Enrollment survey suggests no very sharp patterns. Single individuals tend to have higher move rates, but lower refusal rates, in all sites. In general, the completion rate for the Enrollment survey was about the same across all experimental conditions.

Households completing the Enrollment interview were invited to enroll by mail in the Payments program. Households were not permitted to enroll in the Payments program without completing the Enrollment interview. On the other hand, households completing the Enrollment interview might still refuse to enroll in the Payments program. Following is the distribution of enrollment in the Payments program for the various segments:

TABLE 10

Segment	Enrolled in Payments Program	Never Enrolled in Payments Program	Total Enrolled in Surveys
Winnipeg	9667(89.5%)	113(10.5%)	1079(100.0%)
Rural-Dispersed (non-farm)	143(78.5%)	39(21.4%)	182(100.0%)
Rural-Dispersed (farm)	61(69.3%)	27(30.7%)	88(100.0%)
Dauphin (non-farm)	259(61.0%)	160(39.0%)	410(100.0%)
Dauphin (farm)	72(67.9%)	34(32.1%)	106(100.0%)
Orig. Sample	1492(80.0%)	373(20.0%)	1865(100.0%)

The initial response rate to the Payments enrollment invitation (by mail) in the Winnipeg site was 74.5 per cent. This is similar to that experienced in the rural-dispersed sites. MINCOME Manitoba subsequently conducted two major recontact efforts in the Winnipeg site in an attempt to increase the proportion of households enrolled in the Payments program. The first recontact effort was conducted in early 1975 and resulted in 40 new completions, while the second effort, in June, July and August of 1975, resulted in 122 conversions for a total enrollment rate in payments of 89.5 per cent.

Non-participation in the Payments program has been analyzed in some detail. In general, non-participation was higher in the less generous plans and in the control group than in the more generous plans, especially in Winnipeg. The most common reason given for not enrolling in the Payments program was that the income reporting form was too complicated. Therefore, a simpler form was used in the recontact efforts, and in the enrollment of the Supplementary Sample.

Reporting units who completed the Enrollment survey were also asked to complete a Periodic survey every four months. A household could refuse these interviews at any time, but would forfeit their payments (if any) in so doing. Some households would refuse a Periodic (and forego payments) at one time, and then agree to "re-enter" at a later time. Thus, the size of the sample constantly fluctuated from one Periodic to the next.

In Winnipeg, the cumulative attrition rate over the nine Periodics was 35.6 per cent, for an annualized attrition (subsequent to Enrollment) of 11.9 per cent. About 70 per cent of the attrition was due to

refusals, the rest to moves. Attrition was much higher in the earlier Periodics and stabilized during the later stages. Attrition was also higher than average in the less generous plans.

In the rural-dispersed and Dauphin sites (non-farm sample only), the patterning of attrition was similar to that shown above for Winnipeg. Overall, about a third of the families initially enrolled underwent attrition sometime before the 9th Periodic, with the highest losses coming early in the experiment. Interestingly, attrition in the rural-dispersed sites was higher among treatments than controls.

Although refusals were the major component of attrition at all stages of data collection, including the Periodics, moves constituted an important secondary source of sample loss. MINCOME classified moves into four categories, each treated differently:

1. "Move traces." A family changed residence, but all subsequent efforts to find them failed. Move traces, of course, were never recontacted and were permanently lost to the panel.

2. "Moves final." These designate moves to a destination outside of Canada. No effort to contact "moves final" was made.

3. "Near moves." These are moves made within 200 miles of the original site. "Near move" families were interviewed by the normal survey process, assuming they could be found.

4. "Remote moves." These are moves to a distance of more than 200 miles from the original site.

Initially MINCOME Manitoba did not interview remote moves. Since remote moves were eligible for payments, procedures were established whereby they could be re-interviewed if they moved back within a 200 mile radius of one of the interviewing sites. In spite of this change, a large number of households remained outside the 200 mile radius. Subsequently, it was decided to survey these households. This project resulted in 116 completions, of which 74 were intact households, and 42 were splits from original households who formed their own units. The 74 intact households are disaggregated below by site of enrollment (total number of intact household moves for the site in parentheses).

Winnipeg Site	44 (94)
Rural-Dispersed Sites	4 (9)
Dauphin Site	8 (25)
Dauphin Walk-ins	3 (12)
Supplementary Sample	15 (35)
Total Completions	<hr/> 74

In early 1975, shortly after payments began, monitoring of the Payments program revealed the following:

1. After the fifth month of payments, only 63 per cent of the treatment households completing the enrollment interview had actually enrolled in the Payments program, with the corresponding figure for controls being 79 per cent. Many households who enrolled also subsequently quit the Payments program.

2. Of households enrolled in the Payments program, too small a number of households were receiving non-minimal payments. This was not acceptable for research purposes. In June 1975, 532 Winnipeg households received payments, but only 200 of these were non-minimal payments.

3. A "hole" was discovered in the sample frame whereby 16 per cent of the addresses had been systematically excluded from selection for the Screener interview.

4. About 80 per cent of the households in the least generous treatment plan were receiving minimum payments. This rendered the treatment plan very inefficient.

Accordingly, a major recontact effort was set up to enroll households who had not returned their income reporting forms. Eventually about 84 per cent of these treatment households and 70 per cent of the control households were successfully enrolled in the Payments program. In spite of this, it was still felt that the sample size and the proportion of households receiving non-minimum payments were too small, and for these reasons, a decision was made to enroll a Supplementary Sample. The Supplementary Sample was drawn from four distinct groups: (1) the Baseline recontact group, comprised of households selected for the Baseline interview in Winnipeg, but who did not complete the interview because of a refusal, move, or other reason; (2) the "Mobility Hole" group, comprised of approximately 16 per cent of the households in Winnipeg that had been incorrectly excluded from the initial sampling frame. The sampling was initially done from an address list, and had excluded addresses where the household head was over 57 years old. But, owing to mobility, many of these addresses would, at the time of actual interviewing, be occupied by an eligible household; (3) the "Original Frame" group, that is, new households selected by a further draw from the initial Baseline sampling frame; and (4) the "Welfare" group, drawn from a list of households who had been on provincial or municipal welfare during the previous three years, but not currently on welfare.

All told, the above four groups contributed several thousand potential households to the sample. Of the several thousand new households baselined, 344 met the appropriate eligibility criteria and were asked to enroll in the experiment, and 293 actually completed enrollment in Payments. Unlike the original sample, the Supplementary Sample households were given the Screening and Baseline interviews simultaneously, and the eligible households did not have to complete an Enrollment interview. Enrollment coincided with agreement to enroll in Payments.

The Supplementary Sample was allocated to experimental plans mainly on the basis of where the need for additional sample size was felt to be

greatest. Payments to the Supplementary Sample began one year later than payments to the original sample, thus, the Periodics continued for the Supplementary Sample one year beyond that of the original sample. Attrition in the sample during the Periodics was similar to that observed for the original sample. Over the three years, total attrition was 34.7 per cent, and again was higher in the earlier Periodics. As in the original sample, attrition was related to the generosity of the plan.

An Analysis of Non-Response (Technical Report No. 7)

As we have just seen, sample losses occurred at each of four discrete stages: (1) non-completion of the first Screening interview, (2) non-completion of the Baseline interview, (3) non-completion of the Enrollment survey, and (4) attrition during the Periodics. Technical Report No. 7 provides a detailed analysis of the nature of the losses at stages (2) and (3).

The Baseline survey made no mention of the impending NIT experiment itself but rather was presented as a general social and economic survey of the area. It was a fairly complex and demanding interview and usually required some two or three hours to administer. Non-participation at Baseline would therefore reflect factors associated with the interviewing process itself. In contrast, the Enrollment survey emphasized that participating households would be eligible for an income maintenance program, and indeed, that completion of the Enrollment survey was a precondition for enrolling in that program. Non-response at Enrollment would therefore reflect both factors associated with the interviewing process and any hostilities households may have felt towards the negative income tax program being offered.

Analyses were undertaken separately by site and family type (double- vs. single-headed families vs. single individuals). Summarizing briefly, the results for non-response to Baseline were as follows:

Winnipeg, double-headed families: Refusals increased with the age of head and with the earnings of the male head. Moves were lower for homeowners and the self-employed, and higher for the lower income groups. Overall, non-responders at Baseline were households with older heads, high male head earnings but low total household income, and small family size.

Dauphin, double-headed families: Non-responders were generally self-employed and young.

Rural-Dispersed, double-headed families: Non-responders were generally households with high male head earnings but low total income, and renters.

Winnipeg, single-headed families: Nothing predicted refusals; moves were highest among households with younger heads.

Dauphin, single-headed families: None of the equations were significant.

Rural-Dispersed, single-headed families: Non-responders were generally older than responders, and more likely to rent.

Winnipeg, single persons: Refusers were generally older; movers were usually young, male, low earners, and renters.

Dauphin, single persons: Refusers were generally homeowners; movers were disproportionately young.

Rural-Dispersed, single individuals: None of the equations were significant.

Non-response to Enrollment (original sample only) was patterned rather differently. In Winnipeg, the only significant equation was that for refusals among double-headed families: refusals were positively correlated with age of head and earnings of the male head, and negatively correlated with the amount of welfare payments received in 1974. In Dauphin, the only significant equations were for single-headed households, but these results must be treated with great caution because there were only 9 non-responders (4 of them refusals) in the Dauphin, single-headed households group. In the rural-dispersed sites, the only significant equation was for non-response among double-headed households, and the only significant predictor in the equation was job satisfaction (non-response higher among the more satisfied).

Non-response to Enrollment in the Supplementary Sample shows still a third pattern. Nothing was significant in predicting non-response in the Supplementary Sample, at least partly because the completion rate in the Supplementary Sample was much higher than in any of the three original sample segments.

No easy summary of the findings on non-participation is possible. In general, the age of the head, income, family size, and home ownership appear to have been most important in conditioning the patterns of non-response.

Program Participation in Dauphin (Technical Report No. 8)

As noted previously, the Dauphin or "saturation" site was included in the MINCOME design in order to approximate the real-world condition of universal eligibility that one would obtain in a fully implemented national NIT plan. Thus, participation data from Dauphin gave some preliminary indication of the likely participation rate (and therefore, the likely costs) of a national NIT program.

Although any household in Dauphin meeting age and income criteria was eligible for payments, the participation rate was, nonetheless,

rather low. In 1976, the Dauphin caseload varied around 350 families; in contrast, preliminary estimates were that some 650-900 families were probably eligible to receive payments. These latter estimates, however, were very uncertain, and thus, it was not possible to firm up an estimate of the participation rate unless a better estimate of the total number of eligible families could be obtained.

To this end, MINCOME drew a probability sample of 700 addresses from the Dauphin telephone directory, to be interviewed in order to ascertain program eligibility. 139 of these addresses were excluded because of the age of the head, 146 of the contacted households refused to participate in the interview, and others from the list of 700 were lost for other reasons. In the end, complete interviews were obtained with 302 households. Of these, 94 (or 31.1 per cent) were found to be eligible for some kind of MINCOME payment: 63 of them were eligible for NIT payments, and 31 were eligible for tax rebates. Of the 94 eligibles located in this survey, 32 (or 34 per cent) participated in the MINCOME Payments program sometime during 1976, which seems unusually low for what amounts to the second year of a well-publicized income transfer program.

Generalizations regarding a national NIT program, of course, remain problematic. Although MINCOME was very well-publicized in Dauphin, a national NIT program would doubtlessly be better publicized still. Also, program participation required co-operation with the Periodic surveys -- vastly more intrusive than the reporting requirements of a national NIT program would be. It can also be noted that the tax rebate provision of MINCOME was not well-publicized, and this may have lowered participation among relatively higher income families (families above break-even but below the tax equivalent point).

Questions posed to the Dauphin sample (and to a supplemental group of known non-participants) provide additional details on the nature of non-participation. It appears that somewhere between 10 and 20 per cent of the eligible non-participants were simply ignorant of the Payments program. About half thought they were ineligible, even though they were not. (Most of these said they would participate if they thought they were eligible to do so). A few (about 5 per cent) were satisfied with their current welfare support. Most of the remaining eligible non-participants expressed some hostility to the idea of government welfare programs or reported a sense of independence that would not incline them to participate.

Retrospective vs. Prospective Income Reporting:
An Experiment (Technical Report No. 9)

MINCOME income guarantee levels were set up on an annual basis, but payments were made monthly and participants filed monthly income report forms. The actual payment in month T was therefore calculated on the basis of earnings reported in month T-1. If income flows were perfectly constant through time, the one-month lag would be immaterial. If, in contrast, a family's income fluctuated considerably month to month, then the payment made in any one month might not correspond very closely to the actual income needs of the family in that month. For this reason,

it has been suggested that a prospective income reporting system might produce a more responsive NIT program: in this case, families would report their expected income for the coming month, with the payment for that "coming month" then determined by the income estimate.

In order to compare the advantages and disadvantages of retrospective vs. prospective income reporting, participants in the Dauphin site were randomly divided into two groups. In the first group, payments in month T were calculated on the basis of reported income in month T - 1 (the retrospective method used in the remainder of the experimental sample). In the second group, payments were calculated on the basis of the family's estimate (in month T - 1) of what the income in month T would be. In general, the retrospective method proves superior. Both methods give non-zero errors relative to the actual income in Month T, but errors under the retrospective system are considerably smaller. Given the prospective method, it appears that families systematically under-predict true incomes.

The Accuracy of Income Reporting (Technical Report No. 10)

MINCOME participants were required, as a condition of eligibility, to submit their annual Revenue Canada income tax forms for each year of the experiment. Taking reported incomes as the "true income" of each reporting unit, it is therefore possible to compare the monthly IRF income reports with the Revenue Canada report, and in the process to assess the accuracy of income reporting among MINCOME participants. Of particular importance is the possibility that treatment families under-reported their income in order to qualify for larger monthly NIT payments.

This analysis is restricted to calendar year 1976 and to reporting units that (a) remained with MINCOME for the entire year, and (b) remained intact during the period. In all, there are 1,012 reporting units that fill these criteria, 860 treatment families and 152 controls.

Results for the pooled sample show that, on average, MINCOME participants tended to under-report their incomes by a few hundred dollars, especially wage and tax deduction income. In spite of this, the correlation between the MINCOME and Revenue Canada incomes is in the order of .99. Comparisons between treatment and control families revealed no sharp or substantively interesting differences. In general, the small tendency to under-report income was about the same in both treatment and control conditions. There is no evidence that treatment families systematically under-reported income in order to get larger NIT payments.

Administrative Issues (Technical Report No. 11)

MINCOME was (in the first instance) an experiment to ascertain the labour supply response of households to a guaranteed annual income. But it was also a "trial run" in the administration of a "real world" NIT program, and the administrative experience gathered in the experiment should therefore be of some interest. Technical Report No. 11 discusses three of the more important administrative issues: the process of annu-

al reconciliation (between the payments made and the actual entitlement), the payments audit function, and the issue of month-to-month income fluctuations.

In calendar year 1976, the middle year of the experiment, the annual close-out reconciliation showed that 13 per cent of the reporting units were underpaid, 11 per cent were paid precisely the correct amount, and 76 per cent were overpaid. The most common reason for overpayment was that the unit's income increased towards the end of the year. The most common reason for underpayment was MINCOME's neglect of statutory deductions.

External auditing of the work of the Payments office was performed for a subset of cases. From March 1976 through December 1977, for example, 690 reporting units' records were subject to some kind of audit. A sample of these cases (N = 258) was selected and analyzed. These 258 units produced a total of 317 distinct audits, of which 216 (68 per cent) uncovered no problem. Most of the "problem" cases involved income misreporting.

Monthly income fluctuations produce a serious administrative problem: since a family's income needs will likewise vary month to month, a responsive NIT program has to make monthly adjustments to the size of the transfer payments. An analysis of the monthly income fluctuation in the MINCOME sample is contained in the third part of Technical Report No. 11.

Field Operations (Technical Report No. 12)

MINCOME was a complex, multi-wave panel survey. The sample consists of four main segments (described earlier), each of which changed over time. Each segment was interviewed repeatedly over the course of the experiment, at roughly four-month intervals. The contents of the Periodic surveys also changed over time. An overview of the field operations is provided in Technical Report No. 12.

The research design called for three interviews to be conducted each year, over each of the three experimental years, giving a total of nine experimental measurements for each reporting unit. In addition, data on a full year of pre-experimental behaviour were gathered in the first two survey interviews. The largest part of each interview was devoted to participants' experiences and behaviour in the four-month period preceding the interview. Occasionally, however, owing to moves and changes in family composition, the reporting period covered in any particular interview would be shorter or longer than four months, as explained in more detail below.

Rather than concentrating the interviews for all families into three relatively short periods in any year, interviewers were kept in the field throughout the year, mainly for reasons of efficiency. Thus, for example, the first interview with any particular family in 1976 could have occurred at any time between January and April of that year. Interviews were conducted in four-month cycles or waves. The sample was

accordingly divided into waves based on the date at which Survey 1 was completed. Reporting units retained their wave designation throughout the course of the experiment and, consequently, the position of any one reporting unit in the interviewing cycle remained relatively stable. Thus, the elapsed time between interviews was roughly constant, at about four months, for all reporting units in the experiment.

The MINCOME sample consists of "reporting units," which are roughly comparable to what is conventionally known as the nuclear residential family. The first step in defining the reporting unit is to designate the head or heads of the household. To qualify as a head of household, an individual had to meet one of three criteria:

1. - be a single individual at least eighteen years of age; OR
2. - be at least sixteen years of age, married, and living with his or her spouse; OR
3. - be at least sixteen years of age and a parent living with his or her children.

Given a designated head, the household was then considered to include (a) the spouse of the head (who was also treated for interviewing purposes as a head of the household), (b) all children of the household heads, and (c) all dependents of the head other than his or her children, unless these other dependents were age eighteen or over at the time of the interview. Non-child dependents over age 18 were not included in the head's reporting unit, but rather were treated as heads of their own reporting unit. All members of a household had to share the same dwelling unit.

Common-law spouses are relatively frequent among the poor and were handled according to the following criteria:

1. If the household head were cohabitating with a common-law spouse and declared so at the time of Payments enrollment, that spouse was regarded as a member of the reporting unit.
2. However, after Payments enrollment, only common-law spouses with whom children were shared were considered to be members of the reporting unit. If children were not shared, then the common-law spouse was treated as an optional member of the reporting unit for survey purposes, and could thus refuse an interview without causing the reporting unit to be dropped from the experiment. (In contrast, "mandatory" members of the unit could not refuse an interview without jeopardizing their Payments status). Common-law spouses who were treated as optional members of the unit were not considered eligible for receipt of the guaranteed income payments.

According to the rules enumerated above, all members of a reporting unit had to fall into one of three categories: (a) a head of the household, (b) a non-head adult in the household, or (c) a child. These distinctions are important because each category was treated differently in

the experiment. A non-head adult was defined as any non-head member of the reporting unit who was at least 15 years old at the time of the interview. If they remained with the unit, they were interviewed as part of the unit, and if they split from the unit during the course of the experiment, they were traced. In contrast, members younger than 15 were considered children and were neither interviewed as part of the unit nor traced if they split from the unit.

The contents of the survey questionnaires changed from wave to wave. Some question sequences were asked in every interview, others were asked once and only once, and still others were asked intermittently throughout the experiment. Certain basic labour supply and related economic data were gathered at every Periodic interview. These data are referred to as the "economic core" data and comprise Modules 1, 2, and 5 of the survey instrumentation.

Module 1 gathered economic core data from the heads of reporting units. Module 5 collected similar economic core data from other non-head adults in the unit. Module 2, administered only to family heads (or to the person they declared to be the most knowledgeable about the relevant information), obtained data on net worth and non-employment income for the reporting unit.

Major changes in the survey instruments, affecting both Modules 1 and 5, were introduced at Survey 4. In particular, the job section of both modules was substantially revised, as follows:

Prior to Survey 4, experimental interest was focused on labour supply over a one-week period. For persons currently employed at the time of the interview, the one-week period of interest was the week immediately preceeding the interview. For persons not employed at the time of the interview, the one-week reference period was the last week worked by the respondent since the previous interview, no matter when in the interview cycle that week was worked. At Survey 4, it was decided to gather data on continuous labour supply, rather than take the one-week snapshot. Thus, beginning at Survey 4, all job data were gathered for each week that had elapsed between the previous and current interview.

Module 1, the economic core Module for heads of households, consists of three main components: the job core (jobs, hours, and wages), a job search section, and a socio-demographic section. The job core section is in turn divided into two subparts: the section on hours worked, which was changed as described above at Survey 4, and the section on wage rates, which remained relatively stable throughout the experiment.

Although the major focus in the first three surveys was on a one-week snapshot of labour supply, all three surveys do contain a measure of continuous labour supply as well. In Surveys 1 and 2, respondents were asked to give the average hours worked per week for each month since the previous interview. Survey 3 combined elements from both approaches. At Survey 3, interviewers were instructed to prompt respondents into supplying accurate average weekly hours per month through the use of a weekly hours calendar. Thus, the continuous labour supply data con-

tained in Survey 3 are roughly comparable to those data contained in Survey 4 and afterwards.

Continuous labour supply was not the only variable in the economic core whose measurement was changed over the course of the experiment. The measurement of job search behaviour was also changed, particularly at Survey 6, where meaningful search activity was defined more precisely. Since the definition of "job search" is more precise from Survey 6 forward, there is an apparent (but entirely artifactual) decline in "job search activity" among the sample occurring at Survey 6. It is, of course, critical that this artifact not be mistaken as an experimental effect.

The final section of the Module 1 economic core is the socio-demographic section, and the questions in this sequence were relatively stable throughout the duration of the experiment.

Module 2 was administered only to heads of reporting units and gathered data on net worth and a few other matters. As with Module 1, some (relatively minor) changes were made in Module 2 during the course of the experiment. Module 2 contains four major sections: a properties section, a financial assets section, a section on outstanding debt, and a section on durable goods. All four sections were revised slightly in the course of the experiment, mainly to reduce the number of questions that had to be asked. The only significant change, again occurring at Survey 4, occurred in the durable goods section and involved the procedure by which durable goods data were collected.

There were three additional modules designed to meet other objectives of the research: the farm, business, and family life modules. The first two of these were designed to explore in some additional depth the labour supply response of the self-employed (farmers and independent businessmen), and the third was designed to assess the possible effects of income maintenance on selected aspects of family life. None of these three modules were administered according to schedule, and the farm and business modules were significantly changed midway through the course of the experiment, reflecting a change in research priorities. For these reasons, the data from these three modules must be used with caution. See Technical Report No. 12 for relevant details.

As noted previously, the experimental sample also evolved over time, through refusal to participate, geographical mobility, family composition changes, etc. Each of these threats to the integrity of the panel posed its own problems for the MINCOME surveys operation, as described below:

1. The loss of reporting units due to mobility was felt early in the course of the experiment and persisted throughout, and it therefore became necessary to develop methods by which movers could be traced and recontacted.

All available sources of information were used in the attempt to contact geographically mobile families. Beginning at Survey 1, the change of address information kept by the Post Office and the Family Allowance office was made available to the experiment, and this liaison continued through to the end. As a matter of course, a move that could not be successfully traced by MINCOME would be traced through both the above organizations. Other sources employed in the trace effort included the address change record provided in the survey instrument, current occupants of the unit's previous residence, the telephone company, Henderson's Directory, and the address record maintained by the Payments office. All trace mechanisms were exhausted twice before a unit was classified as a "move final" and dropped from the experiment.

These trace procedures, however, were not used uniformly for all reporting units. For example, in the Survey 1 and 2 interviews, only moves of intact units were traced. In addition, tracing was limited exclusively to the experimental sites. (That is, prior to enrollment, a mobile family would be traced and recontacted only if they had moved from one experimental site to another, or from one to another part of a single experimental site.) After Payments enrollment, the trace procedures were employed more widely and aggressively. All moves of participating units were traced after Payments enrollment, regardless of site. And further, all eligible members of the unit were traced. Thus, after Payments enrollments, all moves of all eligible participants in all experimental families were traced, no matter to where in Canada the move occurred.

2. As noted previously, refusal to participate was another major source of attrition. The first explicit effort to counteract the impact of refusals took place during the fielding of Surveys 1 and 2, when an attempt was made to recontact people who had refused to participate in previous surveys.

The Survey 1 recontact attempt was restricted to the Winnipeg sample. All double-headed, multiple earner families with annual (family size adjusted) incomes below \$7,000.00 who had initially refused were recontacted. In addition, fifty per cent of all families with annual (adjusted) incomes above \$7,000.00 were recontacted. (Information on incomes and family size, of course, was obtained through the Screening survey. No attempts were ever made to recontact persons who had refused to participate in the Screening survey.) Seven highly productive members of the interviewing staff were assigned to the recontact task. Altogether, 177 recontacts were achieved, and of these, 83 were persuaded to participate in the experiment.

It should be noted that the Survey 1 recontact attempt was limited to the Winnipeg sample because the initial completion rates in the other sites were deemed acceptable by the MINCOME staff.

Survey 2 also had a relatively high rate of non-response due to refusal, although the rate was lower than for Survey 1. Again, all non-hostile refusals among the Winnipeg sample at Survey 2 were recontacted, ~~and 20 of 94 initial refusals who were recontacted were successfully enrolled in the experiment.~~

3. Changes in the composition of reporting units also posed special fieldwork problems. In general, a family can change by either gaining or losing members. Call these "joins" and "splits," respectively. For the purposes of MINCOME, neither of these was deemed to have occurred unless the split or join exceeded four weeks. Thus, a three-week marital separation, followed by reconciliation, was not considered to be a change in family composition.

Normal interviewing procedures were used for splitting members by asking one of the remaining heads to supply information on the split member up to the date of the split. Information on the splitting member so obtained was retained as a part of that member's record, unless it could be updated or improved by the splitting member himself or herself through a successful recontact.

Procedures for joining members were somewhat different. Pre-experimental information on work history and income for one year prior to the date of the join was gathered by administering a "new Member" interview module to the joined member. Further, information covering the period from the date of the join to the date of the interview was also obtained as a part of the then-current interview.

Prior to Survey 5, pre-experimental data on joiners were obtained simply by extending the reference period covered in the then-current survey from the previous four months to the previous year. Thus, in the case of Survey 3 (the first experimental survey), the pre-experimental data for joiners extends to 12 months, whereas in the case of Survey 4, it extends for only 8 months. (In both cases, the elapsed months refer to the date of the interview, not the date of the join). At and beyond Survey 5, of course, the pre-experimental data are gathered in "New Member" modules. (Strictly speaking, Survey 5 is the transition survey, and the "New Member" modules were jointly fully employed after Survey 5).

As might be anticipated, the treatment of splits and joins was most complicated when it was accompanied by geographical mobility of the base reporting unit and consequent loss of contact with that unit for some extended period. In cases where the unit had been lost for less than a year, normal practices concerning splitting and joining members were followed. In contrast, special procedures were adopted when, after having lost contact with a unit for more than a year, it was learned on recontact that a split or join had occurred in the interim. A split that occurred more than a year in the past was traced, but the trace mechanisms were used only once. (Thus, one attempt, but only one, was made to establish recontact with the split member.) If a join had occurred during the loss of contact, surveys normally administered to the unit were given to the joint member (that is, multiple interviews, as described above, were administered to joiners). However, in this case, no "New member" module would be administered, as the recall period would have exceeded one year.

The procedures instituted to deal with family splits and joins were designed to serve two purposes. First, the eligibility rules imposed on

splitting members were designed to insure that the financial advantages of family dissolution inherent in the Payments program were not overtly manipulated by artificial formations or break-ups of family units. And secondly, the procedures developed for joiners were designed to acquire pre-experimental data from new members that would be comparable to the data obtained on initial enrollees.

Data Quality (Technical Report No. 13)

Interview protocols returned from the field were sent to quality control and data entry, where they were coded, checked, cleaned, and entered into the master data base. The details of MINCOME's quality control and data entry procedures are described in Technical Report No. 13.

Initially, MINCOME attempted to enter data via optical scanning devices (OPSCAN). Interviewers were trained to record data in a particular way on OPSCAN sheets which would then be read by an optical scanner and, if "acceptable," entered directly onto the electronic file. This system proved unworkable for several reasons. At that time, the state of the OPSCAN art (or at least the version of it available to MINCOME) was considerably less than it is today. Problems occurred in teaching the proper method of printing to interviewers. The OPSCAN sheets themselves were frequently not up to specification and would often be rejected by the scanner. The amount of consistency and range checking that could be done at the point of data entry was limited.

The primary problem with the OPSCAN system, however, was logistical. Errors detected at data entry would be sent to an error correction office, and an "error correction sheet" would be prepared. This sheet would then have to be entered into the system, and it too could encounter errors and be sent back to the error correction station for yet another iteration. Indeed, multiple passes for a single interview record were often required. Coupled with the rapid rate new data were being collected (interviews on over 1,500 families every four months), and inexperience on the part of the data entry staff, this deficiency in the system led to an immense backlog in the entry process. There was also much confusion about just what state of disrepair any given interview was in at a given moment, as the files were so difficult to track and monitor. Thus, the OPSCAN system threatened to bring data entry to a virtual standstill.

Because of these problems, the OPSCAN approach was discarded in favour of a direct, key-to-disk, data entry system. There were numerous advantages to this system, not the least being that many additional more complex data quality checks could be performed at the point of data entry. But still, many serious problems remained. For a variety of reasons, the interview schedules underwent numerous changes, some minor and others quite extensive. Some questions were dropped entirely, others were asked in new and different ways, and response options were also sometimes revised. The online data entry and QC system as it was initially developed did not anticipate this degree of change in the interview documents, and as a result, changes in the interview documents often led to (sometimes large and costly) changes in the entry-QC

programs. It was therefore necessary to transform the entry-QC process into a table-driven system that allowed additions, deletions, and other changes without requiring major changes in the underlying software.

Some general data-entry lessons can be advanced on the basis of the MINCOME experience. First, any high-technology data entry system should be extensively tested before adoption. This, for example, would have prevented the false start with an OPSCAN system. Secondly, there are many good reasons to resist changes in the survey instrumentation over time, and the ensuing complication in entry and quality control is certainly among them. Third, on the assumption that some change over time in the instruments is inevitable, it is important to assure that the entry-QC system selected is sufficiently generalized that such changes in the underlying instrument are easily accommodated.

V. DATA STRUCTURE AND EXTRACTS

At this time (September, 1982), each of the surveys of the main sample (Baseline, Enrollment, Periodics 1 - 9), and the Baseline/Enrollment and Periodics (4 - 12) of the Supplementary Sample, is available on a "pseudo" cross-sectional basis. Since each of these surveys took several months to administer, and since the time covered by the Periodics overlap, no survey represents a genuine "instantaneous point in time."

It is strongly recommended that researchers acquire experience with the Baseline survey, before proceeding to other parts of the experiment. The Baseline is by far the most extensive survey, has the highest participation, and is very useful for acquiring some familiarity with the definition of the variables and the structure of the data. For this reason, a summary Baseline tape is available for a nominal charge. Although the information on this tape is useful primarily for descriptive purposes, it does represent a convenient way to "see" the data, and should provide considerable stimulus for research ideas.

1.0 Question Number, Unique Questions Number (UQN) and Unique Response Number (URN)

In the interests of interviewing efficiency and overall management of the field operations, a special coding scheme was used to track responses to questions which may have been asked of the same respondent several times, but for different jobs, assets, or educational activities. Since MINCOME attempted to identify all possible contact with the labour market during the survey period (time between two surveys), it was necessary to allow for the possibility that a respondent had more than one job. The same set of questions would be asked for each job. Accordingly, sets of questions are recycled to cover multiple job holding activity.

Each question is assigned an alphanumeric code on the survey form. In addition, a unique question number (UQN) provides a numerical code to identify each question. As a question is repeated, for example, for a

second job held by the respondent, a third numerical code, the unique response number (URN), was required to separate questions pertaining to job 1 from questions pertaining to job 2. This triple identification code is used throughout the experiment, and all extracts must completely identify the question number (as it appears on the survey form) the unique question number and the unique response number. In Table 11, Question Number, UQN and URN sequences are shown for a part of the Base-line survey (Survey 1).

TABLE 11

Question Numbers, Unique Question Numbers
and Unique Response Numbers

Question	Question Number	UQN	URN
Were you employed last week?	1	28	28
Please tell me the name of each employer you had last week (skipped if the response to Q1 was negative)	2	29	29
What kind of business is (NAME OF EMPLOYER) in?	3	30	30
Job #1 (Questions 4 - 37B pertain to job #1)			
(If there was no first job, these responses would be coded as missing data)	.	.	.
How much is this/are these benefit(s) worth to you in dollars per month?	37B	90	90
What kind of business is (NAME OF EMPLOYER) in?	3	30	92
Job #2 (Questions 4 - 37B pertain to Job #2)			
(If there was no second job, these responses would be coded as missing data)	.	.	.
How much is this/are these benefit(s) worth to you in dollars per month?	37B	90	152

What kind of business is 3 30 154
NAME OF EMPLOYER) in?

Job #3 (Questions 4 - 37B pertain to Job #3)

(If there was no third . . .
job, these responses would . . .
be coded as missing data) . . .

How much is this/are 37B 90 214
these benefit(s) worth
to you in dollars per
month?

Notice how the question number recycles for each job. The UQN also re-
cycles but is in a purely numeric code, while the URN increases with
each question asked, regardless of actual question text, as long as it
pertains to a different job.

All variables on a survey are identified by this Question Number, UQN
and URN code. The URN serves to uniquely identify the response to any
question, but can be cumbersome to use. Since most users will specify
the extract they wish from the subject catalogue and/or the actual sur-
vey forms, extracts can be specified by identifying the relevant ques-
tion numbers desired.

2.0 Surveys and Modules

On each survey, the questions are grouped by topic, and type of re-
spondent within the family unit. Each such grouping or module is an es-
sential parameter to specify in an extract request. Table 12 shows the
modules which are currently available.

TABLE 12

Modules and Respondent Categories

<u>Module</u>	<u>Subject</u>	<u>Respondent</u>
1	Employment, Wages, Hours worked, etc.)	Head(s) (up to 2)
2	Assets, Non-employment Income, Attitudes, and Family Composition	Head #1
3	Farm Income	Head #1
4	Business Income	Head #1
5	Employment Wages, Hours worked etc.	Adult, Non-Head Members

6	Marital Satisfaction (Surveys 1, 6, 10)	Wives
7	Marital Satisfaction (Surveys 1, 6, 12)	Husbands
8	Youth Attitudes	Adult Non-Head

* Adult is defined as anyone over 15 years old.

The Surveys catalogue currently contains detailed information on Modules 1, 2 and 5, which represents well over 95 per cent of the MINCOME data from the surveys. Most likely, those who would like to use Modules 6 and 7, would like the entire set of questions, and for this reason there is little point in incorporating this information explicitly in the catalogue. It is much easier to indicate that the entire module is desired. This is also true for Modules 3 and 4.

Two points are critical to understanding the structure of the data.

1. The amount of information varies with the structure of the household and their socio-economic activity. Module 1, for example, will be completed by each head. A single individual would complete one Module 1, while double-headed household will provide two Module 1s per survey. Since the data have been forced into a fixed format (although they are inherently composed of variable length fields), typical records may contain large numbers of fields composed of missing modules.

The range of URNs generated by the reporting unit varies from under 500 to over 5,000, each of which must be considered a separate variable.

2. Within a module, say Module 1, the presence of data is a function of economic activity. An unemployed head would skip past vast blocks of questions, while a head that had held several jobs during the survey period would generate many URNs.

3.0 Specifying an Extract Request

The steps required to specify an extract from the MINCOME data base are relatively straightforward. First, the Subject Catalogue is consulted to identify the survey and modules which are relevant to the study. Next, a set of question numbers are prepared, qualified by survey and module. A complete extract must specify the survey, module and question number for each variable desired.

Survey	Module	Question Number
1	1	1
1	1	2
.	.	.
.	.	.
.	.	.
1	1	35a
1	2	12
1	2	13
.	.	.
.	.	.
.	.	.
1	2	41
1	5	1
.	.	.
.	.	.
.	.	.
1	5	15

At this time, it is preferable to access only separate surveys, preferably and initially the Baseline. Once experience has been obtained with the Baseline, extracts for the remaining surveys can be attempted.

A final word is needed on the level of aggregation in typical extracts. It has been decided by the Institute for Social and Economic Research not to make any strong assumptions regarding the construction of certain socio-economic variables. For example, nowhere in the surveys were respondents explicitly asked for their annual family income. This variable is constructed from employment income, tips, bonuses commissions, welfare, unemployment insurance benefits, etc. Different research projects will require different definitions of income, and to construct a measure of family income could well submerge subtleties in definition which are best left to the researchers to evaluate. Assistance is available from the Institute with respect to the appropriate construction of such variables. The Baseline summary tape does contain a variable and is explained in the documentation provided with this tape. It is likely that for many research projects, especially those