

CONFIDENTIAL

PROVINCE OF MANITOBA

**a PROPOSAL for a
GUARANTEED ANNUAL INCOME
EXPERIMENT**

To: THE HONOURABLE MARC LALONDE
Minister of National Health and Welfare
House of Commons
Ottawa, Canada

From: THE HONOURABLE EDWARD R. SCHREYER
Premier and Chairman of Planning and Priorities Committee of Cabinet
Province of Manitoba

Prepared by:

THE MANITOBA MINIMUM
ANNUAL INCOME PROJECT

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I. INTRODUCTION AND OVERVIEW

A. Background to the Proposal

In November, 1970, the Department of National Health and Welfare published its white paper, Income Security for Canadians. This document expressed uncertainty about the suitability of a universal guaranteed income program in Canada (p. 25).

An overall guaranteed income program for the whole population that is worthy of consideration is one that offers a substantial level of benefit to people who are normally in the labour market. Therefore a great deal of further study and investigation, like the experiments now under way in New Jersey and Seattle in the United States, is needed to find out what effects such a program would have on peoples' motivation, on their incentives to work and save. Until these questions are answered, the fear of its impact on productivity will be the main deterrent to the introduction of a general overall guaranteed income plan.

The white paper then suggested a research strategy to answer these basic questions.

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provinces would develop quickly.

The policy relevance of guaranteed annual income experimentation was reinforced two months later when the Special Senate Committee on Poverty, in its report, called (p. 175) for a national guaranteed income plan:

The provision of a Guaranteed Annual Income to all Canadians is more than an anti-poverty measure: it is an idea whose time has come. If properly designed, implemented, and operated, such a plan will restore to decency and dignity those Canadians who, through no fault of their own, have been stigmatized and demeaned because they are unable to earn an income adequate for themselves and their families. It will also protect all Canadians from loss of income through accident, illness, technological progress, and the many events that can deprive any of us of our income and make us poor. *stop*

Most recently, two events have demonstrated the continued national importance of putting GAI to the test. The first of these was an enunciation, in the Speech from the Throne opening the 29th Parliament, of principles the federal government sees as the basis for the future development of Canada's social security system. Certainly, extending a guaranteed income to additional categories of those defined as unable to work, a concern with work incentives among those who can work; and an interest in the maintenance of equity between workers on low wages, those who cannot work, and the temporarily unemployed, points toward an experimental examination of a guaranteed annual income program.

The Honourable Marc Lalonde's Commons address on January 11, 1973 expanded on the principles presented in the Throne Speech, and made the policy significance of a guaranteed income test unmistakably certain. This was especially true of his remark that

"... there must be a link between the social security system and the taxation system. The basic hypothesis related to the possibility for single people and families of paying taxes on one hand and to their need for a

supported income on the other should be considered as components of the same problem. Efforts should be made in order to set up income security programs reaching the objectives within a relationship of interdependence with the taxation system itself." (emphasis added).

B. Principles of the Manitoba Experiment

Yet, unfortunately, and paradoxically, given the extent of official concern, the guaranteed annual income approach remains untested in Canada. Now, however, the Province of Manitoba has developed an overall design for an experiment, which it plans to implement in 1973. This design has emerged from a continuous series of meetings beginning in July, 1972, between officials from the Manitoba Minimum Annual Income Project and the Guaranteed Income Experimental Division, National Department of Health and Welfare. This design represents a joint planning effort, drafted in accord with the Guidelines for the Financing of Guaranteed Income Experimentation written by the G.I.E.D. staff. It is intended to be a basic framework within which a heavy schedule of theoretical elaboration, plus pre-experimental data generation and analysis will take place over the next few months. This work is necessary both to operationalize the design and to develop a comprehensive body of baseline data on potential recipient families and communities. Inevitably, during this process, certain aspects of the design will come to be seen in a new light. Thus, while this next, operational planning phase should carry the experiment into implementation, it will also provide an opportunity for a rigorous review and testing of the design parameters themselves. We invite Canada to continue and to expand its participant role in this phase of the project.

There is no need, here, to repeat the arguments supporting the relevance of the guaranteed annual income approach. These have been adequately covered in the documents noted above. But a few words about the assumptions and goals underlying this particular model are appropriate.

1. A Policy-Relevant Experiment

First and foremost, this is a proposal for a social science experiment which is intended to yield policy-relevant conclusions.

This is not a "demonstrative project", for we do not know the optimal plan to be demonstrated. However, this project is taking place against a background of six years of GAI experimentation in the United States, and by the time our project is completed these studies will be finished and much of their data will be available. Generally speaking, these experiments have been more "purely scientific" and less suited to the resolution of policy questions than we feel is now necessary. As the number of completed experiments increases, the body of pure data grows and the focus of research can be narrowed.

Therefore, certain tentative assumptions have been made about the broad ranges within which Canadian policy options exist (e.g., a GAI reduction rate of not more than 70%, nor less than 30%; and support levels which are neither significantly higher nor significantly lower than current welfare rates). Within these ranges of policy options, we seek to create knowledge about the costs, and behavioural consequences of various combinations of possible program parameters, including administrative ones.

If a label is required for the experiment, it is an "experimental and developmental project", i.e. "... a type of project in which the merits of new ideas with programmatic implications are assessed in terms of outcomes in a setting corresponding at least in part to actual field operating conditions."¹ To make this assessment of outcomes, we propose to develop two distinct but linked modes of experimentation, one a dispersed sample test of GAI, and the other a saturation test of GAI.

The dispersed module of the experiment

A dispersed sample experiment will be conducted in Metropolitan Winnipeg and the rural area immediately around it. In this module of the experiment three tax rates and three support levels will be examined,

1. Walter Williams, Social Policy Research and Analysis, Elsevier, New York, 1971, pp. 53-54.

with seven of the nine possible combinations of financial treatments being tested. This test will involve a treatment group (eligible for, or receiving GAI payments) of 600 families, and a control group (no payments) of 400 families, chosen at random from that portion of the population deemed most relevant to a test of GAI (the working and welfare poor). (See Section III.B. below). Thus the dispersed module is a controlled experiment, in a simplified social setting, characterized by a deliberate limitation in the number of variables treated as significant, in order to facilitate a more precise measurement of certain relationships regarded as crucial in predicting behavioural responses.

This scientific rigour would be limited, however, and purchased at the price of some policy relevance, were we to concentrate only on the behaviour of isolated families. Therefore, the dispersed module of the experiment will be linked to a saturation test of GAI.

The saturation module of the experiment

A saturation experiment dealing with the effects of GAI will be conducted in a small Manitoba community and the surrounding rural area. In this segment of the experiment, an entire community will be eligible for payments, and the behavioural responses of those individuals and families receiving benefits will be systematically studied and compared both to the dispersed sample and to a control group chosen from outside the saturation site (see section III.C.). Below it is argued that a saturation module, because it best represents the social milieu likely to be found under a universal GAI, provides us with a more externally valid setting for the testing of the effects of GAI. At the same time, it allows us to gain valuable knowledge about the operational feasibility and probable administrative cost of a GAI. Together, the two experimental modules — dispersed and saturation — are viewed as providing an overall experimental design which is optimally satisfactory both from the perspectives of experimental methodology and policy relevance.

Both modules will commence payments by January 1, 1974 and continue with full payments until December 31, 1976. There will then follow a one-year phase-out period, necessitated in the first instance by the economic and social disruption that would result were the experiment to be abruptly terminated in the saturation site.

2. A Socially Inclusive Experimental Program

The second basic orientation shaping this proposal is a view that the interests of policy-makers are best served by designing an experiment which is, in two different senses, universal.

First, we have not a priori excluded any potential recipient group from the experiment. It was thought preferable to provide information on the cost and consequences of a socially inclusive program. Thus, subsequent to the experiment, and on the basis of hard data, policy-makers can decide how selective or universal a program they wish to adopt, or how to implement GAI in segments. But if only limited information were available, neither a universal program, nor one more inclusive than that anticipated in a selective design, could be established with any certainty as to probable consequences. Limiting the experimental sample would be throwing away information, which, because of the marginal cost of adding groups once the basic costs are incurred, is relatively cheap to purchase. Thus, the experimental program is open, in principle, to single individuals of both sexes over the age of 18, as well as to families, without any ceiling on their size, limited in all cases only by income levels.

Second, the experiment is designed to ascertain the maximum capacity of GAI to replace existing provincial and federal income maintenance and support programs. The extent to which this is possible is itself subject to wide debate (Income Security for Canadians, pp. 24-29; Welfare Policy in Manitoba, pp. 56-57), and is a question of considerable policy interest. Therefore the program is open, in principle, to individuals on social assistance, as well as the "working poor", and those on other income security programs.

In the experiment itself, this universality of the experimental program will be best reflected in the saturation module. In the saturation module the program to be tested will be effectively universal inasmuch as all residents of the community studied will be potentially eligible for benefits. In the dispersed module the program is tested on a carefully chosen sample (See Section III.B.3.) but one which is intended to maximize the power of the experimental design, and not to preclude a priori any particular group.

3. Focus on Work Incentives/Disincentives

The basic policy question about guaranteed annual income, as has been suggested in the white paper on income security, is the impact of guaranteed income payments on the willingness of recipients to work. The erosion of work incentives inherent in existing social support programs is obvious. Will GAI itself cause a decline in work effort? Perhaps as important is the question of whether it will stimulate increased labour? These questions are not identical. No one knows to what degree able-bodied people presently receiving social assistance or pension payments would prefer, or how they would respond to, a GAI program offering the same level or slightly less support, but having a built-in work incentive. It is important, then that the primary research focus of the Manitoba experiment be on work effort and labour supply. This is a dominant consideration in the articulation of the design set forth in Section III.

In summary, the design set forth below calls for an experimental-developmental project, implemented in the Province of Manitoba with the active and continuing collaboration of Canada, to gain data on the economic costs of, and social responses to various formulations of the guaranteed income program. With this data, policy-makers at both the Federal and Provincial levels can more effectively evaluate the desirability of establishing a guaranteed income program, and be guided to the construction of an optimum model, should there be a positive

decision. The Project generating this data will be the first large-scale social science field experiment ever conducted in Canada.

C. Plan of the Proposal

Research foci are described in Section II. While the Project's concern with labour supply and administrative variables has been fully developed, further work is now being conducted on the specification and clarification of secondary areas in which investigation, within the context of the experiment, will be most fruitful, and supportive of the labour supply analysis.

Section III describes the parameters of the experiment; it is the core of this proposal. It is followed by a discussion in Section IV of the main data collection activity — the baseline and periodic surveys. A secondary community data collection operation is defined in Section V. The overall plan for processing, storing and maintaining the confidentiality of the data is presented in Section VI.

We then move from the research to the operational activities. First, the basic rules of operation are given. These will subsequently be operationalized, and published in an operations manual. Then we turn to a description of the standard operational procedures of the Project, including enrollment and payments. The next section deals with the major problem of community relations. This is followed by the final section, which deals with the administration of the Manitoba Project. The budget figures given here, incidentally, are subject to further refinement.

The proposal concludes with a series of appendices which expand on key points in the body of the proposal.

II RESEARCH FOCI OF THE MANITOBA GAI EXPERIMENT

Introduction of a guaranteed annual income on a universal scale is a step bound to have an impact on the entire fabric of society. Thus it is essential that an experiment dealing with the effects of GAI be able to address itself to the likelihood of a broad range of outcomes having direct and indirect economic cost and social cost implications. Thus we assume that in appraising GAI, policy makers are concerned with the extent to which GAI experimentation can resolve questions regarding cost of GAI, administrative feasibility, and effects of GAI on work incentives and (by implication) national economic productivity. At the same time, we assume that policy makers are deeply concerned with the possible effects of a guaranteed annual income on family life, individual development, and the "quality of life" in general. Questions relevant to each variable domain must be carefully formulated and answered if policy makers are to have information which allows for design of a GAI which is maximally beneficial to society. To pursue these goals, it is necessary to delineate the research areas and research questions of primary interest, develop specific hypotheses, and create suitable indicators for the testing of those hypotheses. A number of basic research objectives will be considered, but these are to be sharply defined to ensure that available research resources are not dissipated.

The primary focus of the experiment is on labour supply and the relations of individuals and families to the labour market.¹ This focus dominates many features of the overall experimental design (see Section III below) and it encompasses such phenomena as effects of GAI

1. This designation of the primary research focus is consistent with that deemed centrally important by the Guaranteed Income Experiments Division of the Department of National Health and Welfare as set forth in Guidelines for Financing of Guaranteed Income Experimentation: "The Department has established as the primary objective of experimentation to analyze the effect of guaranteed income plans on the relationship of individuals to the labour market. Specifically, this will entail analysis of the effect of guaranteed income plans on: (a) the desired and actual labour supply of individuals; (b) choice regarding investment in human capital; and (c) mobility of individuals across occupations and geographic location." (p.6)

on the labour supply of primary and secondary earners, the duration of job search, occupational mobility, and retirement decisions. The relevance of this focus is clear, since changes in work patterns induced by a universal GAI might significantly affect both the financial cost of a GAI program and the real economic cost in terms of foregone production.²

Secondary research foci extend the breadth of the study to encompass seven areas of importance to policy makers' appraisal of GAI. They are: (a) geographic mobility, especially effects of GAI on rural out-migration; (b) family behaviour, especially as relating to effects of GAI on family composition and nuclear family interactions; (c) community involvement and participation, including effects of GAI on levels of participation, cooperative behaviour and community-related values and attitudes; (d) individual self-development, including effects of GAI on aspirations, investment in education, investment in job-related training, and feelings of individual competence and self-worth; (e) demand for social services, including effects of GAI on awareness of, and actual utilization of, such services; (f) community economic effects of GAI (to be monitored in the saturation module and control community only) including effects on wages, level of unemployment, rent and economic development; (g) administrative parameters, including such matters as enrollment operations, income reporting and so on. We will outline our objectives regarding the primary research focus and briefly consider each of the secondary research foci in the sections below.

A. Primary Focus-Labour Supply and the Relation of Individuals and Families to the Labour Market

1. Basic hypotheses regarding effects of GAI on labour supply

Concern about possible effects of GAI on labour supply has arisen from the standard static analysis of individual labour supply

2. It should be noted, however, that a modest shift in national levels of labour supply (measured as per capita hours worked) need not produce a corresponding drop in national income. This is because output is dependent upon the intensity and quality of work effort as well as on the number of hours worked. It follows (and social-psychological findings regarding well being and security make it plausible) that a national GAI could result in significant reductions in labour supply without a corresponding decline in output. The importance of this question is considerable, such that models for estimating effects of income maintenance programs on productivity should be developed in the immediate future.

decisions. Attention has largely been focused on the behaviour of potential GAI recipients, i.e., people below the break-even level of income. Here the introduction of a GAI simultaneously increases non-earned income and decreases the effective take-home wage rate through the imposition of an offset tax. Since the net transfer payment is always positive, the income effect of a GAI increases the demand for leisure (thereby reducing labour supply) as long as leisure is a normal good. The offset tax rate reduces the effective wage rate (which can be regarded as the price of leisure). This may further increase the demand for leisure (and thereby further reduce work effort).

Assuming that leisure is a normal good, the static theory of labour supply indicates, as a result of GAI, a probable decrease in the number of hours of work the individual offers at the existing wage rate. This result is not uniformly reflected in correlational analyses which have been used to predict effects of income maintenance on labour supply.³ Nor is this prediction clearly confirmed by preliminary results of American income maintenance experiments. Atkinson et al.'s discussion of findings in this area is useful as a summary:

³ Atkinson et al. comment as follows regarding cross-sectional analyses:

"More interesting than studies of limited, existing income support plans are the various attempts to investigate the response to a negative income tax. Three simulation studies have been completed and one actual social experiment conducted for which both preliminary and mid-experiment results are available. The labour supply response coefficients in the negative rate study by Galloway were estimated from the response of the aged to OASDHI payments, may thus be considered of limited usefulness, and, further, appear to depend significantly on the region of the country used for investigation. The study by Jane Leuthold utilized information on a cross-section sample of low-income workers and, on the basis of certain assumptions about the structure of worker preferences, drew conclusions about the relation of work effort and income. The findings of the study were that the average change in work effort over the entire population would be likely to be small, but in a slightly positive direction, although workers with low wages and/or large families might be expected to reduce work effort substantially. These rather puzzling conclusions may be a consequence of the particular preference function which is used in the model, or may be related to data limitations.

The Green and Tella study utilized U.S. Current Population Survey data for 1965-1966 - both high employment years - to investigate the differences in labour supply of families with and without non-employment

The preliminary results of the New Jersey experiment were based on returns from 509 out of the 1,359 participating families from August, 1968, to October, 1969, Trenton, Paterson and Passaic, New Jersey: the results are based on the experiences of 364 families receiving various levels of support payments and a control group of 145 families not receiving payments. From this limited sample the conclusion arrived at was that "there is no evidence that work effort declined among those receiving income support payments..... On the contrary, there is an indication that the work effort of participants receiving payments increased relative to the work effort of those not receiving payments." The adjusted preliminary results maintained the optimistic view: "... to the extent that differences appear between control and experimental families they are generally in favour of greater work effort for experimentals. Hence, anyone who seeks to support an argument of drastic disincentive effects cannot expect to find even weak support in the data so far." The mid-experiment report, reflecting a more serious analysis of full sample data for the first year of the experiment, revealed differential labour supply response of control and experimental families: "...the results indicate a continuation of the earlier findings on earning change, i.e., no significant difference between control and experimental families. There are significant differences, however, in two alternative indicators of labour supply: (1) persons employed per family, and (2) hours worked per family. These differences indicate fewer workers or hours for the experimental families as static labour-supply theory would predict. There is also a differential in average hourly earnings that reconciles the different indications given by earnings and hours." A preliminary explanation for these unanticipated results is that "the experimental treatment provides the security to enable earners to get better jobs." (pp. 120, 121).

Our mode of proceeding will be to assume that the overall impact of GAI will be to reduce labour supply (i.e., statistically significant differences will be found between control and treatment groups). The effects of GAI program parameters will be in accordance with labour supply theory, and that effects on specific categories of individuals and families will be highly variable. It should however, be noted at this point, that the formulation of our hypotheses do not necessarily reflect our views of the likely outcomes, but are often adopted for working purposes. Our caution in this respect derives from the somewhat contradictory results indicated by labour supply theory and by results of the New Jersey Experiment.

3. cont.
 income work less on the average (i.e., are less likely to be full-time full-year workers) than persons without non-employment income. Specifically, the study indicated that the substitution effect of a NIT is likely to be stronger than the income effect, i.e., that the impact on work effort of a NIT scheme may be more dependent on the NIT offset rate than on the magnitude of the basic income guarantee.

Knowledge of the magnitude of the effects of changing guarantee levels and tax rates is necessary to determine the sensitivity of program cost to modifications in design. Accordingly the Manitoba experiment is designed so that these effects can be separated and independently measured (see Section III below).⁴ Beyond this, the standard static model referred to above is too simplistic to provide more than initial guidance. Therefore, the experimental design proceeds on the basis of a preliminary conceptualization of the following hypotheses.

- H₁ GAI will increase the rate of successful job search (measured in terms of both wage rate and attractiveness) because of the reduced opportunity-cost of search time. Thus we expect that individuals will be able to devote more time and more resources to job search activities. This hypothesis is derived from findings of the New Jersey Experiment.
- H₂ GAI will result in a shift in labour supply towards seasonal and/or high-unemployment-risk jobs. This should occur because GAI removes much of the risk, while the wage rates for these jobs will continue to reflect their original high risk or seasonal nature, until such time as the wage rate structure has adjusted to the new institutional environment.
- H₃ GAI will result in an increase in the rate of job turnover. At least initially, GAI should result in a temporary increase in job turn-over as a result of the decrease in the opportunity cost of job search which encourages workers presently employed in low paying and/or unattractive jobs to seek more attractive jobs, as a result of the decreased supply of labour in these areas.
- H₄ GAI will have the specific effect of increasing withdrawal (full or partial) of secondary workers (particularly wives of employed household heads) from the labour force.

4. In other words, the basic hypothesis to be tested is that work effort decreases as guarantee levels and/or tax rates increase, but the design is such that effects of these variables may be assessed independently or in various combinations

This is a result of the reduced opportunity cost to the family of the withdrawal.⁵ It should be noted here that this reduction in marketed work effort may be almost completely offset by non-marketed productive activities in the home. Accordingly, care must be taken not to measure the marketed productive losses resulting from these labour force withdrawals as net social losses. The distinction between market and non-market productive activities is often arbitrary (e.g., baking bread in a bakery vs. baking at home).

- H₅ GAI will encourage withdrawal from moonlighting and encourage reduction in over-time, etc. because of the reduced return from incremental hours worked.

In the dispersed module of the experiment:

- H₆ Labour supply will be negatively related to the size of the GAI guarantee (annual benefit payable when earned and imputed income together equal \$0.00). Thus we anticipate that as the guarantee increases, the supply of labour (measured primarily in terms of hours worked) will decline.
- H₇ Labour supply will be negatively related to the size of the GAI tax rate (the rate at which benefits are reduced as earned and imputed income increases). Thus we anticipate that as the rate increases, individual labour supply will decline.

In comparisons of the saturation and dispersed modules of the experiment:

- H₈ Secondary wage earners will withdraw from the work force and reduce their labour supply more rapidly in the saturation module than in the dispersed module.
- H₉ Individuals who are "moonlighting" will reduce their labour supply more rapidly in the saturation than in the dispersed module of the experiment.

5. For a discussion of related findings from the New Jersey Experiment see: Kershaw, op. cit.; Harold Watts, "Mid-Experiment Report on Basic Labour Supply Response, mimeo, University of Wisconsin, 1971; and David Elesh et al. "After 15 Month's Preliminary Results from the Urban Negative Income Tax Experiment", mimeo, University of Wisconsin, 1971.

2. Additional Concerns regarding Labour Supply

Beyond testing these specific hypotheses, the focus on labour supply will extend to a number of important questions regarding which more explicit hypotheses are not presented at this time.

One such concern will be with extending our understanding of the manner in which individuals calculate preferences, and families combine preferences to determine their actual level of labour supply, pattern and amount of job search and so on. This is an area in which little is known.⁶ To expand our knowledge in this area, we must on the one hand pursue the development of social-psychological models of individual economic decision-making processes, and on the other develop models of family decision making which are pertinent to family labour supply decisions. Exploration of this area raises a number of questions of theoretical import — for example: regarding the role of perceived marginal tax rates (as opposed to actual tax rates⁷ in the individual's calculation of labour supply), and regarding the nature of income distribution within families. Understanding of such phenomena is essential to development of our understanding of labour supply responses (and

⁶ See, for example: George Katona, et al., Aspirations and Affluence, McGraw-Hill, 1971; George Katona, Psychological Analysis of Economic Behaviour, McGraw-Hill, 1951.

⁷ Research regarding perception of marginal tax rates will be important in another, more immediately policy-relevant manner. The researchers posit that adequacy of understanding of the program — especially accurate perception of marginal tax rates will be one of the crucial factors differentiating short-term and long-term adjustments to a GAI. Thus it is anticipated that careful study of the effects of perception-misperception of marginal tax rates will (along with findings pertaining to the time-horizon effect, from the Seattle-Denver experiments, and along with the analysis of youth responses indicated below) allow more accurate estimation of the long-run effects of GAI on labour supply.

Regarding analysis of time horizon effects in the Seattle-Denver experiment see: M. Kurz and R.G. Spiegelman, "The Design of the Seattle and Denver Income Maintenance Experiments", Stanford Research Institute, 1972. For a related discussion of short-term and long-term effects of income maintenance experiments see Atkinson et al., pp. 50-52, and 214-229.

therefore to the development of policy recommendations in this area). Therefore, special emphasis will be given to these and related problems in the specification and collection of data, and in the analysis of labour supply effects.

A second area for attention will be retirement decisions and retirement orientations. Questions regarding retirement behaviour are clearly significant for appraisal of GAI: "Will a GAI speed up the trend towards early retirement?" "What will be the macro-economic implications of increased tendency towards earlier individual retirement?" To address these questions, a special effort will be made in the experiment to assess retirement orientations and retirement behaviour. Because mounting of an experimental module explicitly focused on retirement is not viewed as advantageous at this time, this effort will be focused primarily upon: (1) the study of attitudes towards retirement in each module of the experiment; and (2) the explicit study of retirement behaviour in the saturation module of the experiment. Retirement behaviour can be assessed among a significant group of near-retirement-age (55-64) persons in the saturation community. About 1000 persons are in this group. Their behaviour will be carefully monitored and compared to control populations so that we may estimate the effects if any which a GAI might have on retirement.

A third area for analysis will be the response of young people to GAI. Again, as with retirement decisions, it is not thought desirable at this time to mount an experimental module dealing specifically with the responses of young people to GAI. Yet the questions of interest here are similarly compelling: Will young people, who have in most cases little or no work history or work experience, respond to GAI in the same manner as more general populations? Or will their responses be very different, and perhaps indicative of major shifts in popular responses to a universal GAI which might be expected among new cohorts of young adults? Analysis of the response of young people to GAI may give us clearer understanding of the probable long-run changes which might occur in labour supply and productivity. Therefore, a special study will also be made of the effects of GAI on labour supply, entry into the work force, and work-related values and attitudes, for young people resident in the saturation site. It is estimated that about 1800 young persons (ages 14 to 21) reside in the saturation site, so that an adequate supply of subjects for such an analysis is clearly available.

A fourth area for analysis will concern the possible effects of GAI on work-related values and attitudes. We will be concerned, for example, with the effects of GAI upon such factors as: job satisfaction; occupational aspirations; the value which individuals place on work per se; and attitudes towards the receipt of government transfers such as social assistance or GAI. It seems reasonable to posit that effects of a GAI on work related attitudes and values will be evident before corresponding effects on actual labour supply. This seems reasonable inasmuch as theorists generally anticipate that predispositions will usually change before behaviour, and inasmuch as social/contractual constraints limit the rate at which individuals may adjust their labour supply. A related argument is presented by James Morgan in his discussion of income maintenance experiments.⁸ There Morgan outlines a simple behaviour-sequence model of the following form

Change in Awareness Interpretation Behavioral Resulting changes
the rules → and → and attitude → changes → in earnings or
Credibility response birth rate

and comments that it would be a mistake

to focus on the mechanics of the rules (the levels of support and marginal tax rates) at one end and the ultimate outcome in work or pregnancy at the other, to the neglect of the intervening variables. There are two main reasons why this is a mistake. First, the intermediate changes occur earlier in time, and second, they are less subject to the distorting influences of outside events. It is possible for a family given better economic security to start using family planning and to look harder for work, but the first methods they may try may not work, or the job market may deteriorate for their particular skills or location. And change in work effort will not correlate perfectly with change in earnings, so finding that their behaviour has changed is really more convincing than the finding that they are earning more, or less, or having children or not.

8 James Morgan, "Discussion of 'Current Status of Income Maintenance Experiments'", American Economic Review (May, 1971).

9 Morgan, op. cit., pp. 39-40.

Thus a special effort will be made to monitor rates of change on key psychological dimensions which underlie much of the labour supply decision-making process. This will be important input to the study of individual and family decision-making processes outlined above, and also an important check on inferences to be made regarding short-term and long-term adjustments to a GAI.

B. Secondary Research Foci

1. Geographic Mobility

A prime concern of policy makers in appraising Guaranteed Annual Income will be its effect upon mobility patterns, particularly any effects of GAI on rural out-migration and provincial out-migration. Will a GAI hasten the trend towards relative depopulation of rural farm areas? Or will a GAI enhance the "stay option", encouraging individuals and families to stay in such areas? ¹⁰ The likely answers to these questions are not clear. On the one hand a GAI might hasten rural depopulation by providing security against short-term financial hardships normally involved in significant moves. On the other hand, a GAI might, (through its general redistributive effect, and through the security guaranteed to individuals and families against cyclical income disruption), diminish the relative attraction of urban settings, and thus diminish rural out-migration. Bonner's comment on anticipated general migratory tendencies of the poor under an income maintenance programme is similar:

Expected changes in the propensity to migrate in the event of a national income maintenance program are not entirely clear. On the one hand, it is conceivable that the availability of subsistence income in their present area of residence may permit some residents to stay who might otherwise have migrated. This would appear to be a logical reaction on the part of large families, where income transfers may be relatively significant and for which migration can be especially onerous. It may also be the reaction of certain population groups who labour under a kind of 'place inertia'

10 For related discussions see: M.V. George, Internal Migration in Canada, Dominion Bureau of Statistics, 1970; Damian Hannan, Rural Exodus: a study of the forces influencing the large-scale

On the other hand, the availability of a subsistence income, without reference to residence, could permit migration which otherwise would not take place. It might happen that those members of high-propensity-to-migrate cohorts ... who would not ordinarily choose to migrate would, upon assurance of a reasonably steady income ... decide to emulate those more 'adventurous' members of the cohort so long as a lesser risk of failure would attend their migration. Also, increased return migration appears plausible, particularly if the initial move was less than successful economically and ties to the old community remain strong. (pp 160-161).

To estimate the character of effects of GAI on rural-urban and out-of-province migration, a special analysis of mobility behaviour and orientations will be mounted in the saturation module of the experiment. Rates of migration and mobility orientations will be carefully studied in the saturation community and the main control community. ¹¹ Rates of migration will also be compared at the aggregate level for the saturation community and other areas of the province using time-series data available from the Manitoba Health Services Commission data bank.

The study of mobility patterns in the dispersed module will, because of the essentially urban character of the sample studied, focus upon patterns of migration to other cities in Manitoba and other provinces.

These analyses will require careful attention to factors which demographers have noted as affecting migration, among them: factors associated with the area migrated from (push factors); factors associated with the area migrated to (pull factors); factors representing obstacles to movements between points; and personal factors which may heighten or suppress the tendency to migrate or otherwise mediate push and pull factors. ¹²

10 migration of Irish rural Youth, London, 1970; Ernest Bonner, "The influence of a National Income Maintenance Program on Migration of the Poor", in L.L. Orr et al., (eds.) Income Maintenance: Interdisciplinary Approaches to Research, Markham, 1971, and M.C. Barth, "Migration and Income Maintenance", Technical Studies, the President's Commission on Income Maintenance Programs, Washington, 1970.

11 Attention will have to be paid to migration from the rural to the town component of the saturation site, as well as to migration from the saturation community to larger urban areas.

12 See Bonner, op. cit., for a related discussion.

2. Family Behaviour

Effects of GAI on family behaviour are of interest to the experiment for two reasons. First, because a GAI is structured to deliver differing guarantees to families of different size, it is reasonable to suppose that these guarantees will (to the extent that they represent subjectively significant differences) have some effects on family formation and family disintegration. Such family effects are of immediate interest because of their direct implications for program cost. If, for example, a GAI resulted in large-scale "splitting" of families this would result in noteworthy cost increases, because of the somewhat higher per capita guarantees usually set for smaller family units.¹³ Similarly, a tendency for GAI to accelerate family formation and suppress family "splitting" would have obviously favourable cost implications.

Our second concern relates to the social cost implications of effects of GAI on family life. We anticipate that policy makers will require a firm fix on the question as to whether or not effects of the GAI on families will be favourable, benign or harmful before a universal program could be considered. Therefore, this question will be approached from a number of perspectives. Here, key questions of the following sort concern us: "What are the effects of GAI upon family stability and family splitting?" "How does GAI affect levels of family integration-disintegration?" "How does GAI affect harmony within families?" "How does GAI affect family decision-making patterns regarding expenditures, work effort and other matters?"

Allocation of money within a family (by whom and to whom and for what purpose) has long been recognized as a major source of conflict.

13. To estimate the impact of family splitting on transfer payments, both economic family and census family definitions were used. Using a standard set of guarantees, a 67% tax, and an income distribution suitable for rural Manitoba, we found that family splitting could possibly increase the per capita annual transfer payment cost from \$105.50 to as much as \$133.50.

While we anticipate that our design of support levels and administrative regulations will minimize these effects, the extent to which these measures are effective can only be appraised in the light of behavioural responses to the experiment.

Further, it has been noted above that such intra-family allocation of money will be of considerable interest vis a vis determination of labour supply responses. For this reason, particular attention will be paid to matters of intra-family income distribution, and any redistributive effects of GAI within families.

These and related behavioural responses among families included in the experiment will be closely monitored to determine what if any effects may be attributed to GAI. Primarily, this will be done using data collected in the periodic surveys, but some reliance will also be placed on community level data collected in both experimental sites.

3. Community Involvement and Participation

Effects of GAI on community involvement and participation present an additional important area for analysis. Considerable research has suggested that moderate to high levels of citizen involvement are essential for the effective functioning of the political system.¹⁴ In a broader sense, involvement in community decision-making and activities has become a key goal of governments attempting to minimize impersonalized decision making in our society. It would appear in fact that community involvement and participation have over the past few years gradually emerged as social indicators of considerable importance. Effects of any social program on these phenomena are therefore of interest.

Good reasons exist, however, for expecting an impact of GAI in particular on levels of community involvement. Most importantly, a wealth of behavioural research has illustrated over the past few decades that income patterns are closely related to levels of social participation. This research has shown that when community participation and involvement are quantified for a population, the statistical distribution of participation for that population will be almost perfectly described by the previously-established income distribution for that population. Generally speaking

14. For a review of this research see: Carol Pateman, Political Participation, Cambridge University Press, 1971.

political and social involvement (and especially effective involvement) are the province of those who are better off, and are denied to the poor.

It follows that a GAI, because of its effects upon overall levels of well-being and income distribution, may have noteworthy effects upon the level, style and distribution of social participation. It is important, therefore, that we assess effects of the GAI on the distribution of both amounts and kinds of participation within the various strata of our society (by whatever criterion one chooses to stratify). This means that pre-treatment and post-treatment assessments of community related behaviour will be essential. In quantification of involvement careful attention must be paid to the task of achieving accurate measurement. That is, indicators must be relevant to the site, they must be concrete, and we must have knowledge of the individual's state of awareness with regard to possible options. To this end, data collected on participation opportunities of both experimental sites will be used as a factual basis for the formation of indicators.

Analysis of effects of GAI on community involvement will be focused upon the following research questions: "Does GAI have any significant impact on political and social participation?" "Does GAI have any significant effect on modes of participation as they vary across income groups?" "Does GAI significantly affect positive evaluations of the community and the political system?" Answers to these questions -- and a clear appraisal of their implications for the public interest -- are essential if policy makers are to design an income maintenance system which has maximally favourable effects on the structure of social participation and the political system itself.

4. Individual Self Development

Individual self development is of noteworthy interest primarily because of the anticipated impact of GAI on human capital investment, and upon important personal perspectives pertinent to the determination

of labour supply. These include such factors as occupational and mobility aspirations, orientations towards job training and educational investment, and feelings of competence and self-worth. Each of these (and similar phenomena) can be viewed as important in the overall determination of the individual's relation to the labour market. But such phenomena also describe important effects of poverty in general. The chronically poor are usually described in terms of just such dimensions, as lacking in aspirations or possibilities, as disinclined to risk valuable resources on ventures such as job training or education, as directed by feelings of helplessness and incompetence and a socially defined role as inadequate.

The latter effects represent one of the major areas, in fact, where a positive impact of a GAI is expected by its proponents. Stigmatization of the poor, it is argued, can be minimized or resolved under a universal GAI, whereas the impact of current welfare systems on individuals can only be destructive — both in terms of impact on the individual and in terms of social cost. It is hoped that the validity of this claim will be clarified by the experiment.

Testing for effects of GAI on such aspects of individual self development will therefore be a major objective of the experiment. Individuals in each site will be surveyed in accordance with this interest.

5. Demand for Social Services and Community Economic Effects

Good reasons exist for anticipating significant effects of GAI upon the level of demand for, and use of, social services such as day care, counselling, medical and dental and other health care, legal services, etc. Mainly this is because increases in disposable income among the poor may increase their exposure to information and capability to seek and demand such services. The probability of substantial shifts in demand patterns will be evaluated through appraisal of community level data (in the saturation site) as well as through periodic surveys. The policy significance of any large-scale community economic effects of

GAI are obvious. The major operation here, of course, involves work effort; however, there are a cluster of other possible effects such as the impact on consumer demand, wages, prices and inflation which, while they are more difficult to measure, are still critical. The Project will monitor a range of possible changes (see Section V) and will seek to develop hypotheses about the relationship between GAI and specific community effects. We anticipate that there will be substantial and continuing public concern about the community economic effects of a GAI; therefore, the analysis of these factors is also necessary as a means of meeting public desires for comprehensive information about the economic consequences of the program.

6. Administrative Parameters

The primary source of uncertainty about the cost of a universal guaranteed annual income program derives from the unknown impact of guaranteed income on labour supply; the greater the decline in family earned income, the greater the cost of transfer payments. However, there is an area of concern which has cost implications of potentially similar magnitude, as well as touching on the question of the practical feasibility of a guaranteed income: the accuracy of the periodic self-reporting of income. Therefore, we propose to make four related administrative variables the second set of primary response foci. These are:

1. Income reporting form
 - a. simple
 - b. complex
2. Frequency of reporting
 - a. four weeks
 - b. twelve weeks
3. Documentation of income
 - a. nil
 - b. basic
 - c. full
4. Audit type
 - a. random
 - b. targeted

The basic problem is not simply one of deliberate under-reporting. The average low income person has commonly obtained what formal education he has in an inferior school. Further, the incomes of the poor are frequently characterized by great monthly fluctuations. Others, with higher education and perhaps a more even flow of income, face a less challenging task when filling out their monthly income reporting forms. Conceivably this could result in systematic under- or over-payments to particular groups of filers. We wish not only to learn something about the extent of systematic misreporting, but also something about the major contributing factors, such as education, so that appropriate measures can

be developed.

This approach does not distinguish between deliberate and unintentional misreporting; we do not believe this to be the most significant distinction to be made. Under-reporting, of whatever motivation, still increases costs, and administrative mechanisms must be developed to counter this. Over-reporting, and thus under-payment, is a distinct problem which must be dealt with using a different strategy.

In order to avoid any confounding of the labour supply analysis in the dispersed site, the administrative variations will be limited primarily to recipients in the saturation mode. Following is a more detailed description of the rationale for including such variations.

a. Income Reporting

i. To what extent is the accuracy of income reporting a function of the length and complexity of the income reporting form? To generate data on this question a small, randomly drawn subsample of recipients will be selected at the time of enrollment to receive a more complex income reporting form. The experiment will commence with a periodic interview, and will coincide with a series of interviews. In these, persons receiving the more complex form and a sample using the standard form will be asked a set of specific questions designed to measure the accuracy of their income reports. The income reporting accuracy from these groups will be compared to determine the correlations between accuracy and form type. The length of this experiment will probably be one year.

ii. A similar experiment will be undertaken with a twelve week (as opposed to the standard four week) income reporting period. This will test the effect of recall time on reporting.

iii. Documentation of gross earned income, in the form of pay stubs, will normally be required. However, a small subsample will

be allowed to claim payments without such supporting evidence (testing whether reliance on the comparison of income data with that reported to National Revenue etc. is sufficient). A second group will be required to present more elaborate documentation of income and expenses. The same interview and analysis strategy as described above will be used to determine the impact of increased, and nil documentation on reporting accuracy.

iv. Because of the typical irregularity of their income over time, the variety of its sources, the general difficulty of labour supply and income measurement associated with those who do not work a fixed number of hours for specified wages, and the increased opportunities for under-reporting, the self-employed will be over-represented in these experiments. The subsample required to send in extended documentation on business operations and claimed losses that affect monthly payments will be drawn from here.

v. We also will experiment with randomized and targeted audits, and compare the rate of misreporting detected by each. [It should be emphasized that the potential cost of misreporting could well be the difference between a manageable program and a nightmare]. The frequency of auditing, as well as variations in the accounting procedures employed, will be examined.

b. Interaction Effects

A question of major policy importance is the effectiveness of various combinations of administrative procedures. For example, what is the accuracy loss with a three month reporting period, a complex reporting form and heavy documentation, relative to a four week reporting period, the standard report form and nil or standard documentation. We will test for interaction effects between the administrative variables, e.g. combining standard documentation with both the simple and complex income reporting form, over both the four week and twelve week reporting periods.

c. Administrative Cost

Each of the above administrative procedures (and the various possible combinations) will be analyzed to determine the per capita cost. From this it should be possible to develop an administrative system which is not only optimal from the viewpoint of informational accuracy and behavioural impact, but also in terms of cost.

III. DESIGN OF THE MANITOBA GAI EXPERIMENT

A. Overview of the Experimental Design

Social experimentation must be undertaken with a view towards achieving maximal policy relevance, while maintaining optimal methodological rigour. For this reason, the Manitoba Minimum Annual Income Project encompasses two experimental modules, each of which extends the scope of the project's policy relevance, and improves the methodological rigour of the experiment. The two modules are: (1) a dispersed sample test of the impact of various specific program parameters on individuals and families; and (2) a saturation community study of the impact of GAI on individuals and families. Since the two modules are complementary, it is important to outline both their unique and common characteristics with some care.

1. Overview of the Dispersed Sample Module

The dispersed sample module is similar to income maintenance experiments which have been fielded in the United States, in New Jersey-Pennsylvania, North Carolina-Iowa, Gary, and Seattle-Denver. As such, it is a "controlled experiment", modelled after the pure experiment of the laboratory sciences. At the same time it is a type of field experiment, since it is conducted in the real world. Certain manipulations, however, (most importantly, the degree of imposed dispersion which isolates program recipients from one another in a larger population which is unaware of and ineligible for GAI benefits) gives the dispersed sample experiment a more artificial character than can be attributed to the saturation module discussed below.

The dispersed sample experiment involves the following:

- a. testing of seven financial treatments. Each treatment involves a different combination of GAI support levels (the amount guaranteed to an individual or family having no other source of income) and GAI tax or offset rates (the rate at which GAI benefits are reduced as other income increases);
- b. selection of subjects (to be assigned to different financial treatment and control groups) from a large population to which

one wishes to generalize. (This will be the population of Metropolitan Winnipeg and the rural area immediately surrounding it); and

c. assessment of behavioural responses of subjects on such key dimensions as: labour supply, family behaviour, migration, self-development, and community participation.

Knowledge regarding behavioural responses provides policy makers with more precise guidelines for setting parameters of a universal GAI so that it will achieve maximal intended effects with minimal undesirable effects. Thus it is crucial, for example, that the experiment determine whether and how a GAI can be implemented without creating unacceptable levels of work disincentive -- a goal that can only be satisfied if a range of policy recommendations regarding appropriate support-level/tax-level combinations can be derived from the research. At the same time, it is important that our understanding of the effects of varying tax-level/support-level combinations on social behaviour be articulated in such a way that the potential of such treatments for creating secondary (aside from work disincentive) economic and social costs is well understood. These considerations taken together with a desire to generalize to larger populations, lead directly to the conclusion that the overall experiment must rely heavily on a dispersed sample module.

A natural consequence of random selection and random assignment of a small number of subjects from a large population area is that participants in the program are likely to be largely unaware of and unknown to other program participants. In almost all cases, GAI recipients will continue to move in a social world where neighbours, friends and relatives are not eligible for GAI benefits.

It is this relative isolation of subjects from one another which makes it possible to vary treatments within the dispersed module, but at the same time, this relative isolation creates a social environment quite unlike that which would obtain under a universal program. This artificiality of the social setting in which GAI benefits are introduced reflects the

major weakness of the otherwise extraordinarily sound experimental design of the dispersed module. Internal validity and control are largely guaranteed by an elaborate design; external validity is unclear, however, because of the artificial environment (and demands) created by the dispersed experiment.¹ [Somewhat more correctly we may state that the dispersed experiment has not altered the total environment as would be the case in the real world where a universal GAI were being implemented.] It is important that the saturation experiment offsets certain of these weaknesses, so that the two modules taken together allow for an optimally generalizable experiment.

2. Overview of the Saturation Experiment

The saturation experiment involves selection of an entire community and the surrounding rural area, and introduction of a universal GAI for members of that community. Although a control community will also be studied (providing a control group for the saturation experiment (see III.C.4. below)) the saturation study is methodologically distinct from the more pure experiment of the dispersed sample. It is an elegant field experiment — one which can provide important input into the decision-making process regarding GAI social-behavioural effects and GAI administrative feasibility which probably could not be derived from the more artificial environment of the dispersed module.

The major departures of the saturation module from the dispersed model are as follows. First, in the saturation experiment only one tax rate and one support level are utilized. This is because program recipients are highly visible to one another and to non-recipients in the saturation site. A second major difference is the mode of extension of eligibility

1. Generalizability to a larger referent population can be assured by adequate attention to (stratified) random sampling. The question of generalizability from the experimental mode to the real world is much less clear. For related discussions, see: D.T. Campbell and J.C. Stanley, Experimental and Quasi-Experimental Designs for Research, Rand McNally, 1963; N. Friedman, The Social Nature of Psychological Research: The Psychological Experiment as a Social Interaction, Basic Books, 1967; and A. Rosenthal, Experimenter Effects in Behavioural Research, Appleton-Century-Crofts, 1966.

within the saturation experiment. Every family or individual who resides² within the saturation community is eligible for GAI, with actual benefits payable only to families or individuals with income below the break-even point.³ It is important to remember then, that all members of the saturation community are potentially eligible for GAI, and all are considered treatment subjects in the overall appraisal of the experimental design. Therefore, any control subjects must be taken from one or more communities outside of the saturation site.

These departures from the model of the dispersed experiment are necessary for an effective saturation experiment. In such a setting, the need for horizontal equity of treatment for all community members is highly important. More important, the use of a single set of program parameters is essential to focus community attention on, and to allow experimenters to be able to attribute any reactions to, a single GAI program.⁴ The crucial advantage of the saturation experiment is that it allows for a more natural emergence of responses to the GAI — primarily because of the interaction of a single GAI program with the total social network of the community. Where social interaction is relatively open, and where exposure to GAI becomes a shared, community experience, it seems likely that responses will more clearly resemble those to be obtained under a universal program. This seems likely as regards individual behavioural responses (such as those relating to labour supply), and such matters as rent and wage changes, and as regards general questions of administrative

2. See Section III.C.5. below for a discussion of residence and the treatment of residence requirements.

3. The only exception to this rule is that individuals who are permanently institutionalized will not be eligible for GAI benefits. Such persons may, however, be dependent members of families which are eligible. See Section VIII for a discussion of the definition of income. This designation of eligibility criteria is generally consistent with the experimental eligibility rules set forth by Atkinson et al., A Programme of Experimentation for Evaluating Income Maintenance Policy in Canada, pp. 66-67.

4. Atkinson et al., op. cit., pp. 193-194.

feasibility.⁵ Only where GAI can be subjected to, and respond to the needs, demands and criticisms of an entire community, can we begin to determine the degree to which GAI is administratively feasible and publically acceptable. Such an "acid test" is clearly not encompassed by a dispersed sample experiment.

3. Interconnections between the Dispersed and Saturation Modules of the Experiment

The major hypothesis underlying our connection of the dispersed and saturation modules of the experiment is that behavioural responses in the saturation module will be intensified by social interaction, peer group influence, and the altered administrative operations of a "universal type" GAI program. To facilitate the testing of this hypothesis, equivalent treatments must be utilized in the saturation module and in a portion of the dispersed module. For this reason, tax and support levels in the saturation module will be identical to one financial treatment cell of the dispersed experiment implemented in Metropolitan Winnipeg. Thus the design allows for direct comparisons between treatment subjects in the saturation module, and subjects assigned to the corresponding treatment cell of the (Winnipeg) dispersed module. Additionally, development of comparisons between saturation and dispersed module effects requires the implementation of more stringent control procedures, with a two-component control group developed in the control community. In the first instance, this will consist of a control group matched on key social and economic characteristics to the treatment group located in the saturation community. In the second case, we have a 'treatment' group which serves from a methodological point of view as a control group on effects of saturation-dispersion (see Section III.C.4. for a more elaborate discussion of these two control groups).

5. Certain administrative experiments will be undertaken in the saturation site which could not be undertaken in a dispersed sample experiment. A major administrative experiment to be implemented in the saturation module will deal with mode of enrollment (see III.C.6. below). Other experiments dealing with payments and with field operations are to be developed between now and the proposed implementation date (but see Section II.C. for an overview of these).

These two control groups are to be established within a community which is designated as the main control community. Together, these two control operations allow for highly effective integration of results from the two experimental modules.

Making comparisons between the two experimental modules also requires that survey and related data collection procedures for each be as similar as possible. For this reason, the focus on labour supply will be identical in each module, and roughly equivalent attention will be focused upon related behavioural responses. It is obvious that the saturation module allows measurement of aggregated social and economic effects of the GAI which could not be obtained in a dispersed sample experiment. It is obvious as well, that the saturation module provides an excellent opportunity to observe the effects of GAI on the "microcosm" of society found in any complete community and its environs. These opportunities will, of course, be exploited to the fullest extent and their treatment is explicit in the design of the experiment as a whole. Aside from a concern with aggregated effects, however, and aside from a somewhat heightened concern with administrative experimentation in the saturation module (it should be noted, however, that some administrative testing will be undertaken in the dispersed module as well) the two modules of the experiment share common treatments, common data collection procedures and common research foci.

4. Duration of the Experiment

Determining the duration of the experiment is an important step in development of the design, since the apparent permanence or impermanence of the experimental program will determine the nature of behavioural responses. More importantly, we can say with fair confidence that the ideal experiment should have a duration approximating that of a universal program if our inferences regarding the effects of a universal program are to be reliable. This caution is pertinent both to analyses of labour supply responses and social-behavioural responses to a GAI. The implications of this issue are outlined by Atkinson et al.:

In describing the U.S. income maintenance experiments, we argued that the validity of the results generated by them could be seriously questioned because of the limited duration of those experiments. Experimental treatments which are clearly perceived as temporary, as the NIT treatments were, will produce a reaction that may be quite different than that following the introduction of a permanent program. In particular, the behavioural responses of the recipients would, in most cases, be more conservative when the program was considered temporary. We would not expect a recipient to withdraw from a low paying but secure job when he or she knew that they will be left without the NIT guarantee in less than three years. The net effect of the bias produced by the short-term experiments would be to overestimate the work effort response of the population and underestimate the cost of a national or provincial program.⁶

A similar view is presented by designers of the Seattle-Denver income maintenance experiment.

Milton Friedman, among others, has hypothesized that responses to transitory income differ from those to so-called permanent income with regard to effects on consumption. He theorizes that transitory income will be mostly saved. In terms of work response, it may be anticipated that an increase in transitory nonwork income will have less of an effect on an individual's work effort than a similar increase in permanent income because of the expectation that, when payments cease, the individual will have to return to work. The risks associated with leaving a job in terms of ability to return, loss of seniority, and loss of efficiency will all influence his decision. Also, there may be an interaction between the effects of transitory income on consumption and on work effort. If a person tends to save transitory income and tends to regard earnings from work as being correlated with the maintenance of his standard of living, then he will be less likely to reduce work effort on the receipt of transitory income.⁷

6. Atkinson *et al.*, *op. cit.*, p. 214. See Charles Metcalf, "Making Inferences from Controlled Income Maintenance Experiments", mimeo, University of Wisconsin, September 1971, for a related discussion of this problem.

7. M. Kurz and R. G. Spiegelman, "The Design of the Seattle and Denver Income Maintenance Experiments", Stanford Research Institute, May, 1972, p. 10.

Determination of an ideal length for the Manitoba experiment is complicated by the linking of our two modes of experimentation — saturation and dispersed. It seems probable that the ideal duration for each kind of module can be determined independently, and that the length of saturation experiment (because it must treat an entire community, and because it allows for the appraisal of macro-level effects as well as micro-level effects) should be somewhat greater than the length of the dispersed experiment. The two modules are closely linked together in our experiment, however. Comparison of saturation and dispersed experimental results requires a similar duration for each module.

We have settled upon three full years of payments exclusive of start-up and shut-down as optimal from the point of view of policy and experimental demands. Manitoba has indicated a strong interest in early appraisal of experimental outcomes, which position argues for a shorter duration. There is a consensus, however, among those conducting income maintenance experiments that three years represents an absolute minimum duration for deriving adequate generalizations. Therefore a period of three years of operations after the program reaches full enrollment is envisioned, followed by a period of phasing-out (see Section III.E. below). This decision does not, however, resolve the question as to how a particular limited duration of the experiment will affect the power of generalizations.

Designers of the Seattle-Denver experiment provide an excellent solution to the problem, by building duration of the experiment into the Seattle-Denver experiment as a particular treatment factor. As the designers suggest:

The "long run" may be too long to evaluate during the life of any reasonable experiment. The alternative to directly measuring responses over the long run is to evaluate the effect of the pure time dimension on the responses of families. Therefore, the length of the experimental guarantee is to be regarded as a treatment variable. Some families will be placed on three-year programs, while others are on identical programs for five years.

The families will be told at the beginning of the experiment the length of the guaranteed payment period. We will estimate the difference in response as a function of the difference in program length. On the basis of such an estimate, we hope to be able to extrapolate the results to provide an estimate of the long term response.⁸

While the validity of certain details of this method are unclear (it is uncertain, for example, whether a test for linear effects only, as allowed by the two-duration treatment, is suitable for determining the effects of time), it presents in general a sophisticated response to the problem posed.

Only implementation of a variable duration treatment such as that utilized in the Seattle-Denver experiments can resolve this question, but such a treatment (unless limited to small samples) complicates the experiment considerably. This is a matter of utmost importance to the overall validity of the experiment. Therefore we propose to examine the results of the Seattle-Denver time-horizon treatment as they become available, in order to determine their implications for the experimental design set forth below. This will not, however, result in modification of the basic proposed duration set forth above.

B. Design of the Dispersed Module of the Experiment

1. Purpose of the Dispersed Module

The primary purpose of the dispersed module of the experiment is to test the impact of GAI on labour supply and related behavioural responses, and to test the particular effects of varied financial treatments. For this reason it is necessary to utilize a policy-relevant set of GAI support levels and GAI tax rates, and to test their general and specific effects on a representative sample of the population thought likely to be affected by GAI.

The broader population to be generalized to is the population of the Province of Manitoba. To maximize generalizability while simultaneously

8. Kurz and Spiegelman, op. cit., p. 12.

maximizing administrative economies, however, the dispersed module of the experiment is focused upon Metropolitan Winnipeg and its rural environs. Thus, in the immediate sense, the dispersed module will allow generalizations to the Winnipeg area only; this allows inferences regarding 55% of the Province's population. This geographic focus is settled upon as best complementing the focus of the saturation module. At the same time, the dispersed module encompasses an additional subsample of rural families to further complement the saturation module.⁹ Together, it is anticipated that the dispersed module and the saturation module (including the subjects residing in the main control community) will allow for confident generalizations regarding the population of the Province as a whole.

2. Financial Treatments in the Dispersed Module

To achieve maximal policy relevance, the dispersed sample experiment will utilize three tax rates, and three support levels (expressed as a percentage of the "poverty line"¹⁰). Together these will comprise seven distinct financial treatments. They are illustrated in

9. Atkinson *et al.*, *op. cit.*, argue for a stratified random sample of an entire Province in their discussion of their "core" experiment. They take this position primarily because of concerns with variations in response to GAI across different labour markets. The design set forth here, however, does not attempt to encompass the relatively expensive task of explicitly studying the effects of variation in demand for labour (although labour market effects will be monitored). This difference, along with a desire to achieve administrative economies, has favoured selection of a smaller geographic area as the site for the dispersed sample module of the experiment.

10. This is a modification of the Senate Committee's poverty line. It is adapted by the Senate Committee on Poverty from the Economic Council of Canada, further adjusted by us for approximately 4% annual increase in the cost of living, and also adjusted to allow a somewhat different allocation across family size. Support levels corresponding approximately to 50%, 65% and 80% of the poverty line are \$2,900, \$3,700 and \$4,500 respectively for a family of four. (See Appendix A for details regarding support levels). Poverty line income is given by family size below:

<u>Unit Size</u>	<u>Poverty line income (January, 1973)</u>
1	\$2645
2	\$4512
3	\$5134
4	\$5757
5	\$6379
6	\$7002
7	\$7624
8	\$8246

Table III. 1.

Table III.1. Financial treatments of the Manitoba Minimum Annual Income Project. Treatments to be utilized are numbered 1 through 7.

GAI tax rate	Support level as approximate percentage of adjusted poverty line		
	50%	65%	80%
35%	1	2	
50%	3	4	5
65%		6	7

Three treatment and three tax levels are utilized because use of three or more treatment levels for each program parameter can, in principle, allow the extrapolation of non-linear relationships in the development of policy recommendations.

It will be noted that tax rates and support levels tend to cover a somewhat narrower range than similar experiments undertaken in the United States. These tax levels and support levels were chosen to bracket the areas thought to be of greatest interest to policy makers. Tax rates which are examined vary from about the lowest rate imaginable for a national GAI program to a high rate which is only slightly less confiscatory than that proposed by the Senate Committee on Poverty.¹¹ Support levels also tend to bracket the area of greatest policy relevance, inasmuch as current welfare rates both for Manitoba and the nation as a whole tend to lie well to the centre or lower end of the range set off by 50% and 80% of the poverty line.¹² (See Appendix A for a clarification

11. The meaning of these tax rates is clarified in our discussion of mode of integration of positive and negative taxes, Section III.D. below.

12. Table 24 (p. 69) of the Senate Committee Report Poverty in Canada presents data regarding welfare support levels by provinces, for a hypothetical family of four persons. Although the rates presented do not in each case include certain amounts which vary widely on a family to family basis (e.g. rents), they are nonetheless illustrative. The rates noted above for our treatments indicate support levels of \$2,900, \$3,700 and \$4,500 respectively for our low, medium and high support level. No Province provided general support levels higher than the \$4,500 rate, and only New Brunswick, Saskatchewan and British Columbia fell more than \$200 below the \$2,900 support rate.

of the calculation of support levels). This, of course, reflects the view that (barring severe manipulation of the tax rate) it is unlikely that any universal GAI would be implemented with support levels significantly lower or significantly higher than current average welfare rates. Break-even levels (the point at which earned income is such that GAI benefits cease) for each of seven financial treatments for a family of four, are given in Table III.2.

It will be noted that two cells are not used. These are the high support level/35% tax cell, and the low support level/65% tax cell. These are omitted in the experiment primarily because of their marginal relevance for policy. The first case is discarded because it is viewed as too generous to be politically or financially feasible. With a \$4,500 support level for a family of four and a 35% tax, GAI benefits would be paid to a break-even point of \$12,857 and such a program could cover as much as 75% to 85% of the national population. The second case is discarded because its low support level and high tax rate render it too confiscatory. The break-even point for this cell is \$4,461 making relevance of the treatment to income redistribution quite marginal. Together, the seven treatments chosen allow for a careful examination of the impact of variations in GAI tax and support levels. Not only is the design suitable for the detection of non-linear effects, it is suitable for the detection of interaction effects as well.¹³

Table III.2. Break-even levels (B) for family unit size 4, for the 7 treatment groups, where $B = \frac{\text{GAI support level}}{\text{GAI tax rate}}$

GAI tax rate	Support level as approximate percentage of poverty line		
	50%	65%	80%
35%	B = \$8,286	B = \$10,571	
50%	B = \$5,800	B = \$7,400	B = \$9,000
65%		B = \$5,692	B = \$6,923

13. For a related discussion of the several criteria guiding the design of the financial treatments of the dispersed sample module, see Atkinson *et al.*, *op. cit.*, pp. 64-76.

3. The Assignment Process in the Dispersed Sample Experiment

An assignment model adapted from Conlisk and Watts¹⁴ will be used to determine the final allocation of experimental subjects among the seven treatment cells and the control cells. It will have the following characteristics. It minimizes the trace of a weighted covariance matrix of a vector linear function of the coefficient vector in a regression model, subject to linear constraints. The weights are determined by the priority which is given to obtaining statistical significance for a particular parameter. The assignment procedure can be viewed as a non-linear programming problem with a convex objective function.

Present computer programs permit an approximate solution to this problem by first solving it as if the integer variables (number of families in each cell) were real, then integerizing them using a rounding procedure. Investigation of the possibility of obtaining an exact solution in terms of integer variables by solving a sequence of integer linear programming problems should be considered if the objective function appears to be sensitive to the chosen rounding procedure.

Design of the assignment model for the Manitoba Project is not yet complete. Nor has a computer program been adapted for it. To allow an interim assignment across the seven treatment cells, the following simplified model has been used. (These results will be displaced by final results of the more complex assignment model as those results become available).

One-way analysis of variance [Conlisk and Watts, page 152, formula (10)] is used, which is a particular case of the model explicated in Conlisk and Watts.

The number of families assigned to a cell is:

- a. proportional to the policy weight attached to the financial treatment for this cell [in formula (10) of Conlisk and Watts, this corresponds to $\sqrt{W_i}$] and is
- b. inversely proportional to the square root of the cost per year per family allocated to this cell.

14. John Conlisk and Harold Watts, "A Model for Optimizing Experimental Designs for Estimating Response Surfaces", 1969 Proceedings of the Social Statistics Section, American Statistical Association.

Further constraints are that

a. the number of families corresponding to a cell (i.e. a financial treatment) should be greater than, or equal to, 40. This constraint is introduced in order to obtain sufficiently precise estimates of the coefficients of the linear regression model relative to each cell, and is derived from the assumption that the number of observations in each cell should be greater than or equal to 1.5 x the number of unknown coefficients in the linear regression model.

b. the total number of families in the experimental group is 600.

The following table presents policy weight assigned to each of the seven financial treatments used in the experiment. The seven policy weights add up to 100. When divided by 100, a policy weight represents the estimated probability that the corresponding financial treatment will be chosen for general application in a universal program, relatively to the six other financial treatments.

Table III.3. Policy weights assigned to the nine possible financial treatments

GAI tax rate	Support level as approximate percentage of adjusted poverty line		
	50%	65%	80%
35%	10.5	8	0
50%	15	35	8
65%	0	13	10.5

The mean survey cost per family per year for a family of size less than or equal to seven has been estimated at \$189.75¹⁵ (no family of

15. The mean survey cost per family per year has been calculated in the following way:

Let S_i be the real survey cost per year for a family size i ; $i = 1, \dots, 7$.

Assume that only families of size less than or equal to 7 would be included in the survey.

We have, from Harvey (Ted Harvey, Intra-Project Memo, addressed to Norval Campbell, Manitoba Project, dated January 9, 1973, paragraph number 7)

size eight or more would be included in the dispersed sample experiment).

The mean transfer payments per family to be included in the dispersed survey, per year, are given in the following table.

Table III.4. Average projected transfer payments per year for each of seven financial treatments

GAI tax rate	Support level as approximate percentage of adjusted poverty line		
	50%	65%	80%
35%	\$500	\$1,100	
50%	\$500	\$1,000	\$1,400
65%		\$1,000	\$1,500

15. (continued)

$$S_1 = \$129$$

$$S_2 = \$174$$

$$S_3 = \$186$$

$$S_4 = \$198$$

$$S_5 = \$210$$

$$S_6 = \$225$$

$$S_7 = \$246$$

The proportions P_i of families size i (among all families) in Manitoba are for $i = 1, \dots, 7$:

$$P_1 = 13.20$$

$$P_2 = 16.20$$

$$P_3 = 14.56$$

$$P_4 = 19.08$$

$$P_5 = 15.04$$

$$P_6 = 9.70$$

$$P_7 = 5.48$$

The mean survey cost C per family per year is the weighted sum of the S_i and is given by:

$$C = \frac{\sum_{i=1}^7 P_i S_i}{\sum_{i=1}^7 P_i}$$

Using the numerical values of P_i and S_i we find that the mean survey cost per family per year is \$189.75.

It is anticipated that average transfer payments will be slightly lower than those corresponding to the New Jersey and RIME experiments because we will be including as income, a return to net worth (see Section VII.D. below).

Let r_i be the policy weight for cell number i ; $i = 1, \dots, 7$. (See numeration in Table III.1.) Let τ_i be the average transfer payment per year for cell i ; $i = 1, \dots, 7$ and let C be the mean survey cost per family per year. The r_i 's are given by Table III.3. and the τ_i 's by Table III.4. $C = \$189.75$. Let C_i be the cost per year per family allocated to cell i . We have

$$C_i = \tau_i + C \quad ; \quad i = 1, \dots, 7 \quad (1)$$

Let n_i for $i = 1, \dots, 7$ be the number of families to be assigned to cell i . From conditions a. and b. we may write:

$$\frac{\frac{n_1}{\tau_1}}{\sqrt{C_1}} = \frac{\frac{n_2}{\tau_2}}{\sqrt{C_2}} = \frac{\frac{n_3}{\tau_3}}{\sqrt{C_3}} = \dots = \frac{\frac{n_7}{\tau_7}}{\sqrt{C_7}} \quad (2)$$

As

$$n_1 + n_2 + n_3 + \dots + n_7 = 600 \quad (3)$$

we obtain from (2)

$$n_i = \frac{\tau_i}{\sqrt{C_i}} \frac{600}{\sum_{m=1}^{m=7} \frac{\tau_m}{\sqrt{C_m}}} \quad ; \quad i = 1, \dots, 7 \quad (4)$$

If some of the n_i 's (say n_1 and n_7) are lower than 40 we would set n_1 and n_7 to 40 and we would compute the five remaining numbers n_2, n_3, \dots, n_6 by the formula (4) modified thus:

$$n_i = \frac{r_i}{\sqrt{C_i}} \frac{(600 - 40 - 40)}{\sum_{m=2}^{m=6} \frac{r_m}{\sqrt{C_m}}} ; i=2, \dots, 6 \quad (5)$$

The n_i 's would be made integer so that the numbers assigned to all of the cells would add up to 600. Applying this procedure the following table is obtained in which the number corresponding to a given tax rate and a given support level is the number of families which should be assigned to that financial treatment.

Table III.5. Number of families per cell in preliminary assignment

GAI tax rate	Support level as approximate percentage of adjusted poverty line		
	50%	65%	80%
35%	79	44	
50%	113	200	40
65%		74	50

4. Control Group in the Dispersed Module

A control group is used to determine the degree to which changes in the treatment group can be attributed to chance or non-treatment effects which are related spuriously to the period of treatment, or alternatively to the impact of the experimental treatments themselves. It is anticipated that the control group will be 2/3 the size of the treatment group. Thus we have a major control group of $N = 400$. This control group will

(like complementary control panels discussed in Section IV below) be created as an integral part of the operation used for assignment of subjects to the various financial treatments. In effect, individuals should be assigned in an equivalent and random manner to the treatment and control groups. These two groups should, therefore, be as similar as possible before the experimental treatments are applied.

5. Stratification Variables in the Dispersed Module

Both the treatment and control cell populations will be stratified according to estimated normal income, family unit size, and relation of members to the work force. This assignment is treated as an integral component of the assignment model. In the first case, normal income will be estimated using a regression model based on current and historical income data and related family characteristics data. Stratification by income will be designed to standardize pre-experimental income across treatment and control cells, with overall stratification reflecting the national population distribution of income between \$0 and 2.0 of the poverty line. A specific income cut-off will be adopted for each treatment cell, as a function of the pertinent break-even level, and structured to include a "reasonable" range of income above that level. (See footnote 10 in this section for a discussion of the poverty line used herein).

Regarding family unit size and relation to the work force, an attempt will be made to apportion the dispersed module sample between: (1) intact families (families having a husband and wife) where only one person works; (2) intact families where both husband and wife work or where there are other secondary earners; and (3) single individuals. Presently it is anticipated that the overall allocation of subjects will be as in Table III.6.

Table III.6. Stratification by family type and relationship to work force

	Percentage	Number
1. intact families with a primary earner only	35%	N = 210 treatment, 140 controls
2. intact families with primary and secondary earners	35%	N = 210 treatment, 140 controls
3. single individuals ¹⁶	30%	N = 180 treatment, 120 controls

A heavy weight (70%) has been assigned to intact families (families with husbands and wives and (usually) with dependent children) because of the dominant role such groups play in the work force. Within this group, families with secondary earners have been heavily weighted, both because of the significant role of women in the labour force today, and because of the more noticeable effects of GAI which might be predicted for secondary earners on the basis of results of the New Jersey experiment.¹⁷ Single individuals are given the relatively heavy weight of 30%, largely because of the higher incidence of poverty among this group.

Stratification by income, family unit size, and relation of members to the work force indicates, of course, our interest in concentrating our analysis of labour supply on a sample defined with respect to these characteristics. We intend to pursue this further by studying the behaviour of specific subgroups within our sample, categorized with respect to the stratification variables. Further, we will examine the behaviour of groups defined with reference to age, work history, occupation, previous welfare status, and ethnicity, although these factors will not define strata for purposes of the assignment process.

16. This can include persons with dependent children who are, however, capable of being employed.

17. See Kershaw, op. cit.; Watts, op. cit.; Elesh et al., op. cit.

6. Eligibility and Enrollment in the Dispersed Module

Only individuals 18 or over and below 56 or families with a head age 18 or over and below 56 will be eligible for initial enrollment in the dispersed module of the experiment.¹⁸ Most of the subjects will for the most part have incomes less than 2.5 of the adjusted poverty line.

Enrollment will be preceded by an extensive screening operation including selection of target areas using on-site observation, Manitoba Health Services Commission and Census data, a screening survey, and a pre-enrollment survey. The screening survey is designed to identify families suitable for assignment to the experiment. The pre-enrollment survey will determine final eligibility for payments and will provide baseline data for the sample assigned to the treatment and control cells. (See Section IV below for a more detailed discussion of these surveys).

Enrollment itself will be carried out in the following manner.¹⁹ Enrollment will be conducted by Project field personnel who have been trained to explain each of the major features of the program.²⁰ This will be done in a personal interview — preferably one where every adult and every working member of the given family is present. The field

18. Eligibility is restricted to individuals and families with heads under age 56 primarily in order to prevent confounding of the experiment by retirement of experimental subjects. Having a few individuals near retirement age would confound the analysis of labour supply responses, while at the same time a small number of such cases would provide an inadequate basis for generalizations regarding the effects of GAI in retirement decisions. As we have noted elsewhere, more elaborate attention will be paid to retirement behaviour in the saturation module.

19. This operation corresponds closely to enrollment procedures used in the American income maintenance experiments. See, for example: David Kershaw, "Administrative Issues in Income Maintenance Experimentation", in Orr *et al.*, (eds.) Income Maintenance, pp. 281-283; Rural Income Maintenance Experiment, "July 1969 to July 1970, Annual Progress Report", pp. 34-50.

20. It is probable that an advance notice regarding selection of the participant family would be delivered by mail several days before appearance of the field worker, and an appointment arranged by telephone. This should ensure better co-operation and easier scheduling of enrollment interviews.

worker will (a) explain the general purpose of the program; (b) attempt to enroll the family in the program; and, if the family will let itself be enrolled, (c) present the family with its first cheque, if any; (d) explain the family's benefits and responsibilities, including their support level, tax rate, and the mode of income reporting; and (e) work through sample operations of filling out income report and other forms.

Although direct contact of recipients with field personnel is to be held to a minimum during the course of the experiment, it is important that enrollment workers be capable and well trained.²¹ Because of its importance to the experiment, unusual care will be required in the development of the enrollment operation.

C. Design of the Saturation Module of the Experiment

1. Purpose of the Saturation Module

The primary purpose of the saturation experiment is to examine the impact of GAI on the individuals and families of an entire community and the surrounding rural area, for key research interests such as labour supply, family behaviour, migration, self-development and community participation. At the same time, a saturation module provides a better opportunity than does the dispersed module to study the social effects of a GAI, and some of the important macro-economic effects of GAI which, of course, can only be studied in a saturation experiment. Thus the saturation module serves to strengthen, in a consequential manner, the generalizability of the dispersed sample module, while extending the capability of the experiment to allow comment on the social-economic responses of individuals as well as aggregated or macro-level effects. In this

21. Presently it is anticipated that enrollment workers will be drawn from three sources: (a) regular Project field office personnel; (b) payments personnel; and (where necessary) (c) interviewers. We should note that use of interviewers is contingent upon satisfactory evidence that this is not significantly contrary to the goal, outlined in Section IV.E., of separating survey and administrative operations.

connection Atkinson et al., argue:

We want to be able to assess as broadly as possible the costs and benefits of the program and to do so requires an analysis of its effects on the community, and the individual in the community, as well as the isolated recipient family. The behaviour of individuals is affected by the perceived expectations and norms of others. The dispersed sample experiments may have affected the expectations of the members of recipient families but they left untouched those of the recipients' friends, neighbours and community. We do not know the relationship between an isolated individual's response to an NIT program and his response to it in an environment where his friends could also be recipients and where the community attitudes toward the program were well developed. We might reasonably expect, for instance, to see little change in the work effort of recipients in the dispersed sample because all of their friends are still required to work when possible, reducing their availability for leisure activities and increasing their hostility toward someone who works less because of a guaranteed income. Once the program is generally available both constraints will be removed and the work effort response will probably be much different. We are arguing that dispersed sample experiments have inherent shortcomings in that they cannot assess the effects of an NIT program on the community or of a community where the program is generally available to the recipients. Both effects are of great importance in assessing the costs and benefits of a program.²²

It is important to remember that the primary focus of the saturation module is on the behavioural responses of individuals and families, not on such effects as might be encompassed by changes in institutions and changes in the economy. It is in fact uncertain whether a rigorous study of macro-level effects could be effectively carried out, given the current state of theory regarding such macro-level phenomena, and the cost implications of the notion that such studies should focus on a large number of communities.²³ Our approach is less ambitious, with the anticipated analysis of the saturation module focusing

22. A Program of Experimentation for Evaluating Income Maintenance Policy in Canada, pp. 189-190.

23. Myron J. Lefcowitz, "Community Effects", in Orr et al., (eds.) Income Maintenance.

primarily on behavioural changes. This is not to say, however, that macro-level changes are to be ignored. While we will not attempt to systematically explain their occurrence, we will attempt to control for them in our analysis by the collection of various categories of community data. This will involve, for instance, the measurement of the overall conditions of labour markets, and the inclusion of such measurements in the analysis of responses to the program parameters.

It is important to note that the saturation module extends the power and policy relevance of the total experiment in a number of ways beyond its creation of a more realistic social laboratory, and its addition of capacity to allow comment on macro-level effects. Firstly, the saturation module of the experiment allows us to study several important individual-behavioural effects which cannot be readily studied in the dispersed sample experiment. The most salient of these are effects which may appear among non-poor, middle income groups, and effects on relations between the poor and the non-poor.

Under a universal GAI it is likely that those who are not currently eligible for GAI benefits may react in unpredictable ways to the program. Behaviourally, we may anticipate some possible response of the non-poor to the income security features of the program, i.e. work effort may shift in focus; job search behaviour, aspirations, social outlooks etc. may all shift because of increased feelings of security offered by the GAI program.

More importantly, however, the labour supply response of the non-poor to a universal GAI can probably be best appraised in a saturation study. We expect that this will be the case simply because saturation will provide a setting for learning about program parameters which could be more conducive to learning than that provided in a dispersed sample experiment. In the dispersed module, we would expect individuals above break-even to learn very slowly about the options open to them — especially as compared to similar individuals in the saturation

module. This is particularly important inasmuch as the labour supply response of middle income groups to GAI is unknown (and also untested in American income maintenance experiments).²⁴

In addition to allowing the study of labour supply and social-behavioural responses, the saturation module allows us to monitor two factors which are crucial to assessing the social-political feasibility of a GAI: (1) attitudes of middle income groups towards the GAI program and concept; and (2) interaction between GAI recipients and non-recipients. In the first case, we have an opportunity to study the reaction to GAI of middle income groups which may be expected to carry the major burden of financing a GAI (should, as seems likely, any additional cost be involved beyond existing transfers), and a group which (significantly) at the same time has the political power on either a provincial or national scale to influence whether or not there actually will be a universal GAI. This concern is related to our concern with recipient-non-recipient interactions. Such interactions can only be studied in a saturation site, and they allow us to determine whether and to what degree (a) GAI is a source of tension between recipients and non-recipients, as is the case with the current welfare system; (b) whether or not GAI has the effect often attributed to it of reducing the level of social stigma attached to the poor generally, and recipients of transfer payments in particular; and (c) the impact of GAI on both the

24. American experiments seem to have been designed with the assumption that NIT programs would be implemented in a more categorical fashion, for categorically definable segments of the poor, or that they would be relevant because of their relative unattractiveness to the very poor only. The logic of this second argument is simply that the attractiveness of the guarantee to be provided under any feasible GAI is bound to be minimal for persons already earning incomes over the break-even. Whether this is in fact the case is an unresolved question with important cost implications. It seems reasonable to posit on the contrary, that under a universal GAI certain categories of individuals above break-even may reduce their labour supply in response to a universal GAI, simply because of the high marginal positive tax rates already faced. Such effects may have important implications for national economic productivity (because of the important role of middle income groups in the economy) as well as for program cost. Therefore, exploration of the magnitude of these effects is viewed as a research task with considerable policy relevance.

objective and perceived present inequities between those on low wages, those who can work but are temporarily unemployed and in receipt of compensating transfer payments, and those receiving income maintenance because they are unable to work.

The saturation module also extends the policy relevance of the total experiment by allowing careful attention to a number of particular research questions which cannot be economically dealt with in the dispersed module. Most important in this respect is the opportunity to study labour supply and other responses of the near-retirement-age population and the corresponding responses of young people to a GAI. In each case, the responses under consideration are of noteworthy policy relevance (for a discussion of labour supply responses of these groups, see Section II.B.1.b. above), both because of the size of these groups in the general population and because of the importance of their labour supply decisions to determination of overall labour market conditions and overall levels of labour supply. As was noted earlier in the proposal, a useful study of responses of these groups can be undertaken in the saturation module with no increase in overall transfer costs, whereas mounting of specific sub-experiments to deal with these responses would at the present time be prohibitively expensive.

Another area in which the policy relevance of the experiment is extended relates to rural, and especially farm populations. At the same time that effects of saturation are studied, we will simultaneously (and at little cost) direct our attention to the responses of rural and farm families to GAI. (See Section II.B.2. for a related discussion of GAI and rural-out-migration). It is anticipated that the effects of GAI on rural populations and the small farm sector of the economy will be of noteworthy interest to policy makers considering implementation of a universal GAI, especially at the provincial level.

2. Selection of the Site for the Saturation Module of the Experiment

Selection of a site for the saturation module of the experiment is a centrally important activity of the design process. We have approached this task by appraising all communities in Manitoba in terms of

such key criteria as size, character of labour market, integration with other economic centres, and socio-economic composition of the population.²⁵ Our goal has been to identify a community which allows both sound analysis of labour supply and other behavioural responses to GAI, and a sound basis for formulating generalizations regarding broader populations. These objectives are clearly difficult to achieve in a jurisdiction characterized by such social and economic heterogeneity as obtains for Manitoba. Therefore, although we have tentatively selected a site for the saturation module of the experiment, we have also designated two alternative sites. We further anticipate that analysis of the suitability of these and other sites will continue during the next several months, and up to the time when intensive community contacts are initiated.

a. The Tentative Saturation Site

The tentative choice for the saturation site is the Town of Dauphin (population 8,891 according to the 1971 Census), as well as the Rural Municipality of Dauphin (population 3,166 according to the 1971 Census).²⁶ Contingency saturation sites are: (1) Portage la Prairie, (2) Swan River, and (3) Morden-Winkler. In selecting the saturation and contingency sites in Manitoba, there were nine criteria each of which is discussed below.

(1) Size

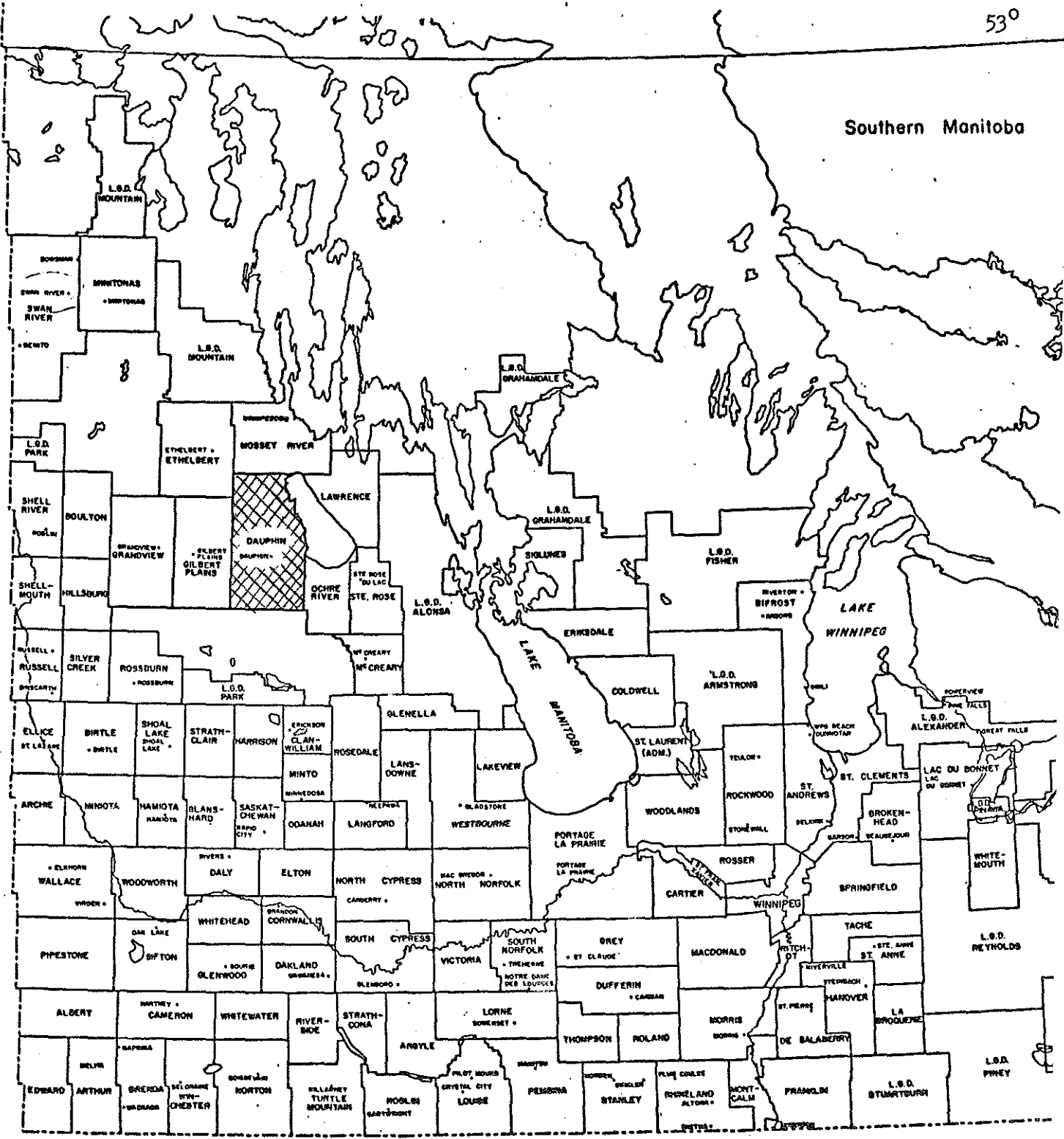
Combined urban-rural areas with a population of more than 15,000 persons were excluded as potential sites because of the cost of transfer payments. Communities of less than 5,000 were eliminated because they would be too subject to external shocks to their economy.

25. See Atkinson et al., op. cit., for a related discussion of problems in choosing sites for saturation experiments.

26. In analyzing the available community data, the choice of Dauphin emerged clearly; it was the unanimous preference of those involved in this proposal. Discussions with Mr. Donald E. Vernon, Assistant Deputy Minister for Community Operations, Health and Social Development, suggest that integration problems between the Project and the Dauphin Regional Welfare Office will be less than with other possible sites.

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Southern Manitoba



TOWN AND RURAL MUNICIPALITY OF DAUPHIN



Also, in a very small community we would expect the results of the introduction of a GAI to be potentially more variable or unstable. The combined population of the Town and Rural Municipality of Dauphin, in 1971, was 12,057. The percentage increase for the Town from 1961 - 1971 has been 20.2% (1,486). Further, the population of the Town and the Rural Municipality has been quite stable, having an aggregate increase of 6.08% since 1961.

(2) Labour Market Diversity

Because labour supply is the primary response variable, it was viewed as essential that the saturation site be a community offering a range of job opportunities. Among sites in Manitoba within the acceptable population ranges, Dauphin best satisfies this criterion, having significant proportions of the labour force in three different sectors: transportation (19.1%), trade (24.5%), and community service (26.3%). This diversity is unmatched by any community of similar size in Manitoba.²⁷

(3) Distance from Predominant Centres

In order to test the impact of GAI on economic behaviour in the purest possible way, it was thought desirable to select a community that was not under the direct economic influence of any other centre, i.e. a site at least 50 miles from a place of equal or greater population. Dauphin meets this criterion. The nearest competing centre is Brandon (1971 population 31,150), 104 miles to the south.

(4) Regional Centre

To test urban-rural interaction, including migration, a site was sought which serves as the centre of a clearly definable rural area, not directly served by any competing centres, and which was itself more or less typical of rural Manitoba. Dauphin is located at the centre of the Rural Municipality of Dauphin; and it is the economic hub of the Rural Municipality and the surrounding Parkland Region (see map, p.55).

27. DREE Report, Urban Environment, Ottawa, 1971, pp. 848-851.

The Rural Municipality has grain farming, special crops, forage crops, and livestock.²⁸

(5) Generalizability

A community and related rural area were sought that would more or less mirror the provincial mean for a series of indicators, including the rate of population increase, participation rate, family size, urban/rural division and average income (so that the percentage of the population eligible for payments would approximate the provincial mean). Dauphin is viewed as the community which best satisfies these multiple criteria. (See Table III.7., p. 58 for selected indicators).

(6) Unemployment

An area with a more or less average unemployment level was required. In an atypically tight labour market, the labour supply response would be unrepresentative. Available figures indicate that recently Dauphin has tended to have an unemployment level approximating the provincial mean. Canada Manpower figures for 1972 indicate that the Dauphin region has a "moderate" unemployment level (defined as 3 - 4%).

(7) Presence of a Canada Manpower Office

Because of the Project's concern with labour market conditions, the presence of a Manpower Centre, with which data generation and analysis could be co-ordinated was thought to be desirable. There is a Centre in Dauphin.

(8) Adequate Population of Low Income Families

As a program directed towards both the welfare and the working poor, it is important that the saturation test of GAI be conducted in a community with a significant population eligible for benefits. Census and income tax data indicate that there are about 1,700 families and single individuals eligible for GAI benefits in Dauphin. This eligible

28. Parkland Profile: An Economic Inventory of Manitoba's Parkland Region, Parkland Regional Development Corporation, 1971; and Urban Environment, Department of Regional Economic Expansion, Ottawa, 1971.

population should generally satisfy the requirements of the experiment.

(9) Comparability of Welfare Population

Analysis of the welfare caseload in Dauphin for January and August, 1972, indicates there is a total of 199 cases eligible for GAI, of a total of 300 in the Town and Rural Municipality. The average monthly payments for these cases was quite close to the average monthly cost per case for the Province as a whole. The latest available figures indicate a Provincial average of \$130, compared to a Dauphin average of \$116. Costs for the Dauphin Regional Office are closer to the Provincial mean than that of any other office outside of Winnipeg and below the 53rd parallel.

b. Alternative Sites

Other communities have either been rejected as possible sites or retained as contingency sites only. Reasons for rejecting other possible saturation sites are summarized below. The potential sites discussed include all Manitoba communities falling inside the acceptable population range (5,000 - 15,000), according to the 1971 Census.²⁹

(1) Steinbach (population 5,197)

Steinbach is viewed as unsuitable because of the atypical ethnic composition of its population (64% Dutch, 27% German), because of its close economic ties (through the retail trade sector) with Winnipeg, and

29. The three Manitoba communities with 1971 populations above 15,000 are Thompson (population 19,001), Brandon (population 31,150) and Winnipeg (population 540,262). Thompson has the same liabilities as Flin Flon and The Pas; Brandon and Winnipeg are obviously beyond consideration for saturation. None of the communities in Manitoba below 5,000 in population exceeds 3,500. A possible site would be Swan River (population 3,522) and the surrounding Rural Municipality (population 3,630). But its relatively small size and limited economy makes it less desirable than either Dauphin or Portage la Prairie. Swan River is, however, designated as the second contingency saturation site after Portage la Prairie. The other possible saturation site is Morden (population 3,266) and Winkler (population 2,983), but they are too small and not dissimilar enough to justify being studied separately. Treating them as a single community could involve substantial operational and theoretical problems.

because of its atypically tight labour market.

(2) The Pas (population 6,062)

The Pas is rejected because of its economic dependence on a single employer, making the labour supply study vulnerable to distortion, and because of the absence of a farming hinterland.

(3) Flin Flon (population 9,344)

Essentially Flin Flon is rejected for the same reasons as The Pas. The problems here are complicated by the fact that a portion of Flin Flon's population actually lies in Saskatchewan. Also, Flin Flon consistently has one of the lowest unemployment rates in Canada. The average for 1972 was "low", meaning 1% or less.

(4) Selkirk (population 9,331)

Selkirk is viewed as only a marginally suitable site because of its proximity to Winnipeg (25 miles), and the significance in its economy of primary manufacturing (steel), which is atypical for non-Winnipeg Manitoba. Its suitability is further diminished by the fact that it is centred between not one, but two rural municipalities which between them have a population of about 10,000. Thus Selkirk and its natural rural hinterland constitute a population which is too large for saturation.

(5) Portage la Prairie (population 12,950)

As with Selkirk, Portage la Prairie is rejected because its size would make the inclusion of a rural dimension to the saturation module financially impossible. The population of the City and the Rural Municipality of Portage la Prairie is 20,464. However, because of the similarity of Portage la Prairie to Dauphin, Portage has been selected as a control community, and designated as the primary alternative saturation site (excluding the Rural Municipality of Portage la Prairie).

3. Financial Treatment for the Saturation Module

As was noted in the overview of the design, a single financial treatment is required for the saturation module of the experiment. The treatment chosen is that with the guarantee set at approximately 65% of

the adjusted poverty line (\$3,700 a year for a family of four) and with the GAI tax set at 65%. This treatment corresponds to treatment "6" in the dispersed experiment.

This treatment is used in the saturation site for several reasons. Most importantly, the treatment seems (because of its moderate parameters) to be one which is highly relevant in terms of policy. Secondly, a less generous plan would have poor domination of welfare. [The 65% of poverty line support level dominates payments made to about 75% of the current Manitoba Welfare population, while the domination rates for the 50% and 80% support levels are about 30% and 100% respectively]. Thirdly, a more generous plan would not seem to be fiscally possible on a universal basis without radical adjustments of the positive tax system.³⁰ Table III.8. below illustrates the support level by family size, and the break-even level for the saturation module of the experiment.

Table III.8. Guarantee levels and break-even points by size for the saturation experiment

Family or Support Unit Size	Guarantee*	Break-even Point**
1	\$1,700	\$2,615
2	\$2,900	\$4,462
3	\$3,300	\$5,077
4	\$3,700	\$5,692
5	\$4,100	\$6,308
6	\$4,500	\$6,923
7	\$4,900	\$7,538
8	\$5,300	\$8,154

* These guarantee levels can vary slightly depending upon the presence of "other adults". See Appendix A.

** Based on the formula: Break-even point = $\frac{\text{Guarantee}}{\text{GAI tax rate}}$, where GAI tax rate = 65%.

30. Atkinson et al., op. cit., (p. 194) rely on a similar set of criteria in recommending a treatment for saturation studies. They recommend a guarantee of 100% of their "poverty line" (\$4,000 per year for a family of four) and a tax rate of 70%, primarily to test a treatment likely to produce maximal work disincentive, and thus provide the harshest possible saturation test of GAI. The treatment adopted here is stringent, but somewhat less harsh than that recommended by Atkinson et al.

4. Control Procedures for the Saturation Module

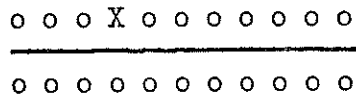
The question of control procedures for the saturation site has two dimensions. The first relates to controls upon the program itself, i.e. controls having approximately the same relation to saturation experimental subjects, as controls do to dispersed experimental subjects in the dispersed module. The second dimension pertains to control over the saturation-dispersed/rural-urban duality of the experiment, and is essential to integration of the two modules of the experiment.

a. Controls for the Saturation Module

While it is recognized that certain strengths accrue to the design of the saturation module of the experiment simply because of its time series nature, it is nonetheless desirable to develop a formal control group.³¹ Only if a control group exists can we have any confidence in our interpretations regarding effects of GAI in the saturation site. Such controls cannot, of course, be perfect, mainly because any control group must be developed in one or more control communities, a condition which prevents overall random assignment. Yet a control group can improve this design considerably.

Campbell and Stanley argue generally for the improvement of imperfect or quasi-experimental designs as a rational approach to the maximization of control and reliability in situations not suited to the implementation of laboratory-like models of the pure experiment. Their comments regarding the "multiple time-series design" are pertinent to appraisal of the control procedures outlined here:

In studies of major administrative change by time-series data, the researcher would be wise to seek out a similar institution not undergoing the X [treatment], from which to collect a similar "control" time series ideally with X assigned randomly):



31. See D. T. Campbell and J. C. Stanley, Experimental and Quasi-Experimental Design for Research, pp. 37 to 57, for discussions of time series experiments with controls.

characteristics to the major survey panel of the saturation site (see Section IV.B.3 below). Because it is developed through direct matching, this control group can only be developed subsequent to fielding of the saturation module.

b. Control link between saturation and dispersed modules:
control on saturation

In order to make clear inferences regarding the effects of saturation as opposed to dispersed GAI treatments, and in order to allow full integration of the experimental results from both modules, it is essential that the experiment include a control group which serves as a control on saturation. The logic of this argument is as follows. First, effects of saturation GAI cannot be readily compared to effects of dispersed GAI if in the first case implementation is for rural subjects only, and if in the second case implementation is for urban subjects only. This is because the degree of saturation-dispersion and rural-urban location are confounded in the design: If the design were to stand as outlined to this point it would be difficult to be certain whether or not any different responses found in the saturation site were to be attributed to social-interaction effects of the saturation module, or to different responses of rural as opposed to urban populations. This weakness can be significantly mitigated through the use of matching procedures (across the two modules), and multivariate analysis. This approach is not as satisfactory, however, as one which deals directly with the design problem indicated.

To deal with this in the most appropriate manner feasible a treatment group (approximately N = 125) will be established in the main control community, Portage la Prairie. The group for this sub-experiment will be chosen in the same manner as the control group for Portage la Prairie: i.e., it will be matched to correspond to key characteristics of the major survey panel in the saturation community and will be developed after the saturation experiment is underway.³⁶ This treatment group will receive the same financial treatment as the saturation

³⁶An additional advantage of this is that the control panel discussed immediately above serves simultaneously as a control on the saturation treatment group, and as a control on the Portage la Prairie sub-experiment treatment group. Random assignment, of course, is clearly feasible across the Portage la Prairie treatment and control groups.

module — 65% of poverty line guarantee, and 65% GAI tax rate. But it is important to remember that this treatment group serves a clear control function in the overall design. Since Portage la Prairie and the saturation site are fairly similar (rural-urban differences are clearly controlled for) and since the several samples will be matched on key economic and social characteristics, the researchers will be able to attribute differences between the two to mode of implementation (saturation versus dispersed) with reasonable confidence.

Using the control procedures set forth above, integration of the two experimental modules becomes fairly complete, and the overall design is strengthened considerably.

5. Eligibility for the saturation module

In the broad sense eligibility in the saturation site is universal, as would be anticipated in a universal program. The only qualification of this rule are: (1) individuals (unless married) must be 18 years of age or over to be eligible for benefits; (2) individuals who are permanently institutionalized can only be eligible for benefits as dependents of another individual or family; (3) actual receipt of payment is based upon passage of an income test.

One difficulty regarding eligibility of persons for the program derives from the fact that the program operates in an environment where GAI-type benefits are not generally available to persons living outside the saturation site. In order to prevent extreme and selective in-migration, therefore, it will be necessary to impose a residence requirement. Tentatively, it is anticipated that this requirement will be one year. This limit is subject to adjustment, however, on the basis of discussions with community leaders. The rationale for such a limitation is found (a) in the fact that significant in-migration has not normally characterized population changes in the community in recent years, e.g., 1961 to 1971, and (b) in the understanding that a key operational and experimental goal is to create as little extraneous disturbance as possible in the saturation community so that the results of the experiment are not clouded. (See Section VII.B. below for a more general discussion of residence.)

6. Enrollment Procedures in the Saturation Module

It is not clear that procedures for GAI enrollment in the saturation site should be exactly identical to those utilized in the dispersed module of the experiment. This is true for several reasons. Firstly, the dispersed experiment enrollment procedure (unless modified) calls for a much more labour-intensive operation (this mode will be referred to as "targeted enrollment") than would be attractive for a national program. As we pointed out in III.B.6, enrollment in the dispersed module calls for personal contact between an enrollment worker and the potential recipient family. Further, it must be noted that such procedures require competent and well-trained personnel if families are to be taught the major rules and procedures of the program in a short interview.

Secondly, the targeted model requires a staggering of enrollment which might result in a lack of responsiveness to families in immediate need. One conclusion which follows from this point is that even if the targeted enrollment model were adopted for the saturation site, the GAI program would at the same time require a supplementary enrollment procedure for those wishing to volunteer themselves into the GAI program, including persons now on social assistance.

Thirdly, much has been made of the question as to whether utilization of GAI should be voluntary or self-initiated per se. It has been argued that a totally voluntary national program (this will be referred to as "open enrollment") will be advantageous from a cost point of view, mainly because of a greater degree of underutilization. Against this one may pose the argument that the redistributive impact of a GAI would be severely undermined by a totally voluntary enrollment system, mainly because of the disadvantaged position of those who are relatively more needy (the very poor, the elderly and the under-educated) in "working" bureaucratic systems.

Setting aside the fact that this issue may eventually be resolved by policy decisions merging GAI (as a negative tax) with the positive tax system — the fact remains that mode of enrollment emerges

as a highly important administrative parameter in a GAI experiment. Our approach to this question is to develop the mode of enrollment in the saturation site as a major administrative experiment of the Manitoba GAI experiment.

This experiment will have the following general characteristics. Firstly, two enrollment treatments are tested: (a) open enrollment, where enrollment is at the initiative of the potential recipient only, and where the only stimulation provided by GAI is a public education program held at a level of intensity likely to characterize the introduction of any universal program; and (b) targeted enrollment (modified), where individuals are contacted in a planned frame of targeted enrollment activity, (but individuals and families in this category would not be prevented from volunteering). Secondly, individuals are assigned to treatment "a" or "b" by random assignment. Thirdly, (a) and (b) are conducted with open enrollment beginning first, and targeted enrollment following as soon as administratively feasible, i.e., perhaps within four to six weeks. This experiment can be conducted with minimal impact upon the project goal of gradual phasing-in of enrollment and payments, and with no visible impact on horizontal equity of treatment in the saturation community. It is anticipated that data from the enrollment experiment will allow the researchers to address questions pertinent to the effects of the mode of enrollment on each of the areas of: cost and level of underutilization; redistributational efficiency; and behavioural responses.³⁷

³⁷For details see: Appendix B: A Design for an Administrative Experiment Focused on Mode of Enrollment.

D. Integration of the Positive and Negative (GAI) Taxes

Integration of positive and negative (GAI) taxes presents a significant problem in the design of income maintenance experiments which seek to test the behavioural effects of a negative income tax. Essentially, this is true because such experiments, because they are experiments, must be conducted in an unchanged legal-institutional context. One implication of this is that many persons eligible for GAI, especially in the saturation site, will also be positive tax payers. Given these conditions, the crucial questions are: "How may positive tax payments affect the technical merit of the experiment?" "How can the positive tax system be treated so that the financial effects derived from the integration of the two tax systems can be added to our financial treatments, with minimal negative impact upon the generalizability and policy relevance of the experiment?"

Atkinson *et al.* outline five alternative modes of tax integration: "All," they indicate, "require the payment of those taxes by the recipient family followed by:

1. Reimbursement of positive taxes on earned income up to and beyond the break-even point. This procedure produces a situation in which all income below break-even is subjected only to the negative tax and all income above break-even is subjected to no tax.
2. Reimbursement of positive tax on earnings at or below break-even but no reimbursement above that point. This produces a negative tax only on earned income below and positive tax on that income above break-even. This procedure results in a significant drop in after tax income -- a notch effect -- as the individual's income passes break-even.
3. Reimbursement of positive taxes to the break-even point and partial and decreasing reimbursement beyond that point. This procedure eliminates the notch effect of the preceding option by phasing the recipient into the positive tax system.

4. Reimbursement of all positive taxes on transfer payments but not on earnings. This would in effect raise the family exemptions depending on the guarantee level and tax rate of the NIT treatments.
5. No reimbursement of positive taxes above or below the break-even point. The transfer would be included as taxable income." 38

For our purposes, we have considered only three methods. The first of these is the system involving the rebate of all positive taxes to the break-even, with further rebate across the "notch" created by the initial rebates. (method #3 above). This system is utilized in all of the American experiments. The second system is one where tax rebates are avoided and recipients pay positive taxes on total taxable income, with GAI benefits treated in the same manner as earned income (method #5 above). The third system is one where all positive taxes on GAI transfer payments, but not on earnings, are reimbursed (corresponding to method #4 above).

Appraising the tax rebate model (method #3), we will consider first the argument presented on its behalf, and then several of its major weaknesses. The major claim for the tax rebate model is that it provides control over variations in the real marginal tax rate facing individuals in the space between \$0 earned income and the break-even point (the guarantee level divided by the GAI tax rate). It is argued that such control over the marginal tax rate is essential if analyses of labour supply responses are to be productive.

Eight disadvantages are inherent in the tax-rebate model of integration.^{38.1} First, the objective of complete control over marginal tax rates facing individuals cannot be exactly achieved.

38 Atkinson et al., op. cit., pp 77-78

38.1 "Disadvantages" of a rebate model, however, may only be stated relative to some alternative. The model and the alternative, (which we will take as being option #5, above) further, must be fully specified (such as with respect to tax rates and guarantee levels) for the discussion to be meaningful. The following list, therefore, is given for general guidance only. It is noted that some of the points, especially the third, fourth and fifth, vary in significance, depending upon the particular program parameters specified.

This is true for all individuals in multiple-earner families. There such individuals face a marginal tax on their own earnings which depends in part upon the division of the GAI transfer within the family. Further, the individual average tax rate depends, in part, on the behaviour of the other earners. These latter circumstances can also detract from the accuracy of the labour supply analyses. It can, in addition, be anticipated that the average tax rate as perceived by the recipient will vary as a result of treatment of net worth and such accounting operations as the "carry forward" procedure (see Section VII.B below). These fluctuations would (especially the first) be of noteworthy interest to the experiment. Second, the elimination of positive taxes for the bulk of recipients is likely to bias individual responses in the direction of greater work effort. Because positive tax payments are effectively eliminated, recipients are more likely to increase their labour supply than they would be under a similar program without rebates. Therefore, the validity of the work disincentive test is weakened by that approach.³⁹ Third, consequential "leakage" of transfer payments to families over break-even occurs with the tax rebate model, undermining the income redistribution power which should, in principle, be maximized under a NIT. This is because the rebate system results in a "notch effect" at the break-even point, and because the method of resolving this problem is to rebate taxes further across the notch, and because the notch can only be "crossed" (or the disposable income differences phased out) very gradually.^{39.1} Therefore, looking at our modal support level and modal tax rate, we see that tax rebates must be made to a very large portion of the population, including families earning as much as \$15,000 or \$20,000. Computer analyses have, in fact, indicated that

³⁹ Additionally, we must consider the real possibility that individuals receiving tax rebates may perceive their marginal tax rate as zero, because they are in effect paying no "taxes" in the sense normally meant. This expectation is based on the assumption that recipients are likely to view positive tax and GAI tax as quite distinct.

^{39.1} See Appendix "C", section 1.(b) for a diagrammatic exposition. Computer tests indicate, for example, that the natural "cut off point" for a family of five at the not otherwise unreasonable \$4,765 guarantee, 50% tax treatment is \$40,000.

much higher "cut off" (termination of rebate) points may be required if severe "kinks" (programmatically induced changes in slope) in the disposable income curve are to be avoided. Aside from cost consideration and "leakage" questions, this also means that a much larger administrative burden (directly proportional to numbers of recipients) must be borne by the GAI program. Fourth, because natural cut off points cannot be used to phase in the two systems (because of cost) artificial "kinks" appear in the curve representing disposable income. These discontinuities, because they represent a softened notch effect, might result in additional work disincentives. Further, the existence of these "kinks" limits the utility of the rebate system for controlling the marginal total tax rate to the income range below the break-even level. This is a disadvantage from the point of view of studying the behaviour of those with incomes in the vicinity of the break-even point. Fifth, the volume of tax rebates is such that overall program costs can be increased by 40% to 80% over the basic cost projected for GAI benefits. Sixth, the tax rebate model is less relevant to the implementation of a universal GAI because it corresponds to a severe recasting of the positive tax system, which would be unlikely in the real world. Further this "marries" GAI to a basic tax reform, limiting the options of policy makers. Seventh, a tax rebate model, (a) may appear to result in a tax holiday for the saturation community and (b) involves implementation of a costly and more highly visible experiment which would present significant financial problems in the real world. Given these features, use of the rebate model involves a risk of adverse general public reaction to the experiment. Eighth, any system which stabilizes the marginal family total tax rate must also create a situation where the marginal tax rates faced by primary and secondary workers within the family are equalized. This "distorts" the decision as to the division of work between primary and secondary workers, and creates an environment for this decision which may not be comparable to that which will exist under a universal program.

The emphasis behind points three, five, six, seven and eight is that the mode of tax integration put forward is neither policy relevant nor experimentally interesting, simply because it represents a model which could not be implemented as public policy in the foreseeable future. Atkinson et al. present a similar view regarding each of the rebate-integration models they discuss.

"It seems obvious that the likelihood of a government taking on an expensive new social welfare program while decreasing its tax revenues through increase in exempt income is almost nil. Experimental treatments, coupled with the positive tax rebate condition, produce a situation which will not be duplicated in any national program. The external validity of results derived from experiments using such procedures is, therefore, highly questionable unless the outlined modification of the tax system will accompany the NIT program. The results of U.S. experiments which utilize the third procedure, i.e. rebate taxes to the break-even point and phase-in to positive taxes, are of limited utility on these grounds."⁴⁰

The major criticisms and advantages of the no-rebate model, (method #5), closely parallel those discussed above regarding the rebate model. Experimentalists indicate two major weaknesses of such a model: (a) they argue that the control lost over marginal tax rates weakens the rigour of labour supply analyses which may be derived from the experiment; (b) they argue that not rebating positive taxes, thus placing positive taxes "on top" of the GAI offset tax must result in extraordinarily high marginal tax rates.⁴¹ Briefly, we will indicate how our analyses lead us to believe that these criticisms may be imprecise.

First, regarding the argument that control is lost over marginal tax rates, it must be pointed out that it is an empirical question as to whether or not the difference between the two plans is consequential. As noted above, some fluctuation of the

⁴⁰ Atkinson et al., op. cit., p. 86.

⁴¹ For an algebraic exposition of the additive qualities of tax rates, see Appendix "A", section 2, note 3.

⁴² This stands to reason given the following:

If: (r is the GAI tax rate
(t is the positive tax rate, a function of income ($t=f(y)$)),
then the marginal total tax rate is $r+t-rt$ for the income range under consideration. Under a rebate scheme, this rate would be constant at r, hence $t-rt$ measures deviation under a no rebate system. If $F=t-rt$, then $\frac{dF}{dy}=(1-r)\frac{dt}{dy}$. That is, the "disturbance"

marginal tax rate faced by individuals will occur under the rebate model in any case. This was the first of the disadvantages of the rebate model noted above. The magnitude of these fluctuations is not known to us as we have seen no studies pertaining to this. More importantly, a computer simulation analysis of fluctuations in marginal tax rates indicates that the range over which actual tax rates for the no-rebate model fluctuate (with \$200 increments in income) is extraordinarily small between the point where recipients begin to pay positive taxes and the point where recipients cease to receive GAI benefits (the break-even point).⁴² Whereas econometric theory indicates that any fluctuation is "harmful", it does not instruct us as to the seriousness of that harm. This uncertainty may lead us to accept no risks or to minimize risk, but general econometric practice has been to display tolerance for problems of this nature when they are present in a "small" way. This, together with the fact that variable (progressive variations as well as variations produced by varying deductions of individuals) marginal tax rates are inherent in the present tax system, and the fact that control subjects as well face variable marginal tax rates, leads us toward the conclusion that criticism of the no-rebate model on the grounds that it loses control over the marginal tax, while well-founded, may not be serious enough to disqualify the model.

The second criticism -- that pertaining to high marginal tax rates -- is less relevant to the no rebate model considered here. Mainly this is because of our treatment of GAI benefits themselves as taxable. This means, as stated above, that the total marginal tax rate faced is not the simple sum of the two tax rates, but rather their sum minus their product. Tax rates are less with this integration model than they would be if the two tax rates were simply added one on top of the other. Using this method, the marginal tax rate faced by an individual is on the average only 11% more than the GAI reduction rate. The tendency of this tax integration model to result in higher marginal tax rates can be controlled by setting the GAI reduction rate at a constant, lower level.

42. (continued) introduced by the positive tax system is mediated by the term $(1 - r)$.

With identical GAI offset rates, advantages of the no-rebate model are located on each of the dimensions of criticism raised vis a vis the rebate model: [1] fluctuations of marginal tax rates facing individuals are reasonably well controlled; [2] work incentives are not exaggerated for the experimental subjects; [3] no payments are made to individuals over breakeven; "leakage" is reduced, and administrative load is minimized by significantly reducing the numbers of individuals receiving payments; [4] no "notch" results from the integration plan; [5] program costs are minimized; [6] no recasting of the tax system is required for the no-rebate plan, making the experiment more policy-relevant; [7] no tax holiday is required for the saturation community, and the experiment as a whole reflects appreciation of cost and feasibility limitations; [8] marginal tax rates faced by primary and secondary earners are not equalized. These considerations, taken together, suggest noteworthy advantages from both experimental and policy perspectives for use of the "no-rebate" model.

The possible system presented as method #4 above is to be discounted, since it displays, in a more serious form, some of the problems inherent in method #5. That is, under method #4, marginal tax rates will fluctuate "greatly" as a function of income and be universally higher than those under method #5.

It should be noted at this point that a system of tax rebates (such as in method #3) can be explained to recipients in a variety of ways, which differ in the extent to which it is apparent that tax is in fact being rebated. Under any tax rebate system, participants still go through the motions of paying their taxes. The difference occurs when the GAI authority taxes them less as their positive taxes increase. An attempt could be made to educate recipients to the fact that they will under all circumstances face a uniform and relatively high tax rate.

Our inclination at the present time is to leave the question of mode of tax integration open for further discussion during the next several months, because of the important arguments which can be made regarding each of the major systems discussed herein. Nonetheless, we should note that administrative programs are, at present being developed for a system corresponding to the rebate modal, in which effective control over marginal tax rates is maximized. Thus, the tentative decision for the experimental design is to rebate taxes, but this decision will be subject to deliberate examination in the next few months.

E. Procedures for Phasing out the Experiment

Contingencies for phasing out the experiment modules are outlined below. Such contingency planning is essential in the event that the experiment either fails to recommend continued operation of the program; or in the event that any period of time elapses between the conclusion of the experiment and implementation of a universal GAI. Either development requires the application of procedures to moderate the impact of the withdrawal of payments.

The experiment involves extensive intervention in and impact on the lives of persons in the experiment, and because these effects may be magnified in the saturation module, and in the cases of certain enrollees in the dispersed module, procedures for softening the impact of "shut-down" (in particular the termination of direct transfer payments) will be necessary. This is important not only because of the particular families involved, but also because of its relevance to the responsibilities of social experimentation. The future of social experimentation clearly hinges on an approach which emphasises both concern for, and effective protection of, the well-being of participants. Additionally, a careful approach to the matter of shut-down protects the long-run integrity of experimental results. Results from a GAI experiment cannot be rationally appraised if the mode of termination of the experiment had unusual or damaging effects. Protection, both of the well-being of the community chosen for the experiment and the well-being of individual participants is essential if the experiment as a whole (and the concept of GAI by implication) is to be objectively assessed by the public.

Shut-down will require approximately 9 months for the dispersed experiment and control modules and 1 year for the community saturation module. Shut-down will involve systematized attempts to minimize hardship or administrative tangles resulting from the termination of the program, as well as efforts to determine post-experimental behaviour changes in both the saturation and dispersed sites.⁴³ Additionally, this period should allow for a relatively gradual transition of GAI field staff to other employment, preventing undesirable disruption of field operations as the termination date approaches.

⁴³ This will involve the fielding of two additional post-experimental surveys during the shut-down phase.

The following specific shut-down procedures are planned:

(1) Families Moving above their Cut-off Point.

From the last day of the third year of payments, no new recipients will be admitted to the program. Further, from this date, those who, by virtue of an increase in their level of earned income, are no longer eligible for payments will not be re-admitted to the program should their income subsequently fall below break-even. Thus, there will be a natural attrition process which, by definition, will remove from the program all those persons who no longer require payments. More specifically, persons who (for whatever reason) cease to apply for payments in any month, from the date specified above, will not be eligible for re-admission.

(2) Gradual Reduction of Payments.

In the saturation and dispersed experiment sites, a plan will be adopted for the gradual reduction of payments. Several alternatives are currently being considered, each of which would involve the progressive reduction of the total volume of payments over the course of the phase-out year. At present it is anticipated that this will involve the gradual reduction of payments, with initial termination for those receiving bi-weekly payments under \$5. Gradual reduction of payments is a responsible position, both regarding individuals and the saturation community. In the first case, it will allow individuals sufficient time to make careful personal decisions regarding alternative modes of employment, public support, etc. (At this stage a shrinking field staff will be devoting much of its energies to assisting recipients in making connections with other agencies, employment opportunities, etc.). In the second case, the total inflow of money to the saturation site will be reduced gradually, so that marked economic shocks do not accompany phasing out of the program.

(3) Families Remaining at the Basic Support Level.

Payments families participating in the experiment, and having no other income will be counselled as to their eligibility for welfare, and gradually terminated throughout the course of phase-out. The drop in each family's payments will also be phased over the shut down period. Thus there will be no sudden drop in disposable income for families with no earned income and no earners. Care will be taken to ensure that the transition of families back on to the welfare rolls will be spread over the phase out period, to avoid an abrupt demand on the welfare systems, both provincial and municipal.

(4) Job Creation.

To prevent marked "reduction" in the economic well-being of the saturation site, it is recommended that the federal-provincial agreement on the conduct of the experiment include provision for the supply of alternative job opportunities to replace any jobs lost to the community which disappear as a result of the withdrawal of the experiment group, or as a result of diminishing money supplies in the community. This is viewed as a responsible position, consistent with the general assumption that the experiment itself should not damage or appear to damage a community which it has used. Such post-experimental job replacement should be viewed an integral part of the saturation experiment phase-out, and thus should be funded under the overall Federal-Provincial cost-sharing formula set forward for GAI experimentation.

The sum effect of these several measures is to assure that neither individuals nor the community involved would be damaged in any way by the experiment. On a large scale, these measures correspond to procedures usually followed in smaller-scale social science experiments to protect and compensate experimental participants.

IV. SURVEY OPERATIONS AND SURVEY DATA QUALITY CONTROL

A. Introduction

1. Overview of survey work in the saturation and dispersed modules

The main data collection task requires development of a strategy for the use of survey research operations to collect high quality data on the effects of the Manitoba experiment on the three main target groups — subjects of the saturation, dispersed and control modules of the GAI experiment. A secondary and, of course, interrelated goal is to devise ways in which the impact of this mode of data collection can be ameliorated and/or contained within tolerable and measureable bounds.

Survey operations for the experiment will require careful attention to the manner in which both communities and individuals are approached. On the one hand, a large dispersed experiment sample must first be selected through extensive screening surveys, assessed in a pre-treatment baseline survey, and subsequently monitored through the life of the experiment and beyond. On the other hand, a small rural community chosen as the saturation site will be subjected to survey operations which will be both more extensive and intensive than any yet applied to any one community. The saturation module of the experiment must, for reasons of economic feasibility, take place in a relatively small population centre, and yet the statistical analyses of social-economic responses necessary for evaluation of the experiment will require periodic surveying of a relatively large panel of families.¹ As a consequence of these two conditions, the survey work anticipated will encompass a relatively large proportion of the community's population, and this large group will be monitored following what will be in effect a census combining tasks of 1) screening for eligibility for GAI support and 2) collection of pre-experiment baseline data. Minimizing the direct effects of the surveys

¹The term "panel" in survey research is usually applied to any group of persons interviewed and reinterviewed on a regular basis. The term does not encompass any a priori assumptions as to the mode in which the panel is constructed. All panels referred to in this section will be created either by use of an assignment model (see Section III.B.3) or by use of stratified random sampling, or both.

themselves (particularly in the saturation module) becomes therefore a prime goal in mounting the survey operations.

2. Mode of interviewing

The surveys must provide high quality data relating to labour supply, work effort and social behaviour for individuals and families included in the experiment. Because the data collected in the experiment must be of high quality to allow precise estimation of program costs and effects, the survey research operations and interviewing must be of corresponding quality. This indicates that the survey work should be based on personal interviews conducted by well-trained survey personnel. The use of personal interviews will not, of course, preclude the utilization of self-administered survey instruments where cost and/or quality of subsequent data suggests the use of such instruments, but reliance in the first instance will always be on personal interviews.

3. Controlling for Reactivity

Regarding reactivity of research in general, Webb et al. and Phillips² suggest that alternative, unobtrusive modes of observation should always be considered in lieu of, or as adjuncts to, survey research. From this perspective it is argued that survey research operations themselves may have an impact on subjects (in this case our experimental and control families) which exceeds the impact of ex-post-facto-observed independent variables, or (in our case) the impact of the experimental treatments themselves.

Notwithstanding the strength of this argument, unobtrusive measures seem unsuited to the collection of primary data regarding effects of the experiment for several reasons. Firstly, a very large number of experimental and control subjects are necessarily involved to produce the economic-social behavioural data necessary to determine program costs and effects. In particular we note that unobtrusive modes of observation

²See: Eugene Webb et al., Unobtrusive Measures: Nonreactive Research in the Social Sciences, Rand McNally, 1966; and Derek Phillips, Knowledge from What? Rand McNally, 1971. Phillips' suggestions, however, extend as well to an elaborate consideration of techniques for the elimination of such bias from survey operations.

have not been developed for dealing suitably with a problem of the magnitude which will be encountered in the saturation site. Observation implemented on a massive scale becomes, by definition, overt and reactive. Existing records, moreover, will not provide the information we require either as relates to labour supply or as relates to social-behavioural phenomena. Secondly, certain of the data required regarding labour supply and secondary areas of interest involve attitudes and subjective estimates of probabilities which can only be obtained from the respondents themselves (for example: subjective readiness to assume employment; job search; attitude towards work; perception of marginal tax rates; mobility, etc.).

Unobtrusive measures can not be utilized to replace survey operations for the above reasons and cost prevents us from pursuing complementary experiments using alternative data collection procedures (although from a methodological point of view this would be highly desirable). The problem of reactivity will, however, be taken into account in the following manner: Firstly, a systematic attempt will be made to determine the response to interviewing in the saturation site through observation and unobtrusive data collection. One of the key tasks of any "on site" researchers will be to collect information and record their systematic observations regarding attitudes towards the interviews and towards the survey operations in general. For this purpose it is anticipated that a qualified social scientist will be located "on site" (in the saturation site) for the duration of the experiment, to act as a participant observer. As necessary, additional personnel may also be assigned to this task. Secondly, intensity of interviewing will be incorporated as a treatment of the experimental design in the saturation experiment, and as a specific treatment for supplementary control panels in the dispersed experiment as well. This will be done in the saturation site by selecting two additional panels (beyond that which is to be reinterviewed periodically) which panels will be reinterviewed annually and at the conclusion of the experiment respectively. In the dispersed sample similar control panels will be

established (See IV. D. below). This treatment should allow us to determine the impact of the intensity of interviewing per se on treatments and controls. This strategy is a somewhat less than perfect control on intensity of interviewing because of the correlation between "saturation" and application of the interviewing treatment to experimental subjects. Nonetheless it provides a noteworthy improvement over the basic design by giving a fairly strong degree of control over the effects of repeated interviewing.³ Additionally, the strategy automatically extends the breadth of the data base for the experiment (particularly in the saturation module), at relatively low cost.⁴

3. Concern with the reliability of data obtained through surveys, and concern with the effects of repeated interviewing have not been very evident in the American income maintenance experiments. Attempts to measure the effects of repeated interviewing are indicated only in the New Jersey and Gary experiments, and no attempt is made in either of those experiments to determine the effects of repeated interviewing upon subjects in receipt of NIT payments. This is important, inasmuch as lack of control in this area means that differential behaviour of control and treatment groups may be attributed to interactions of experimental (including null) treatments and survey operations.

4. In the example noted above, and outlined in greater detail below, the data base for an annual time-series may be doubled with only a 33% increase in cost. More participant families may be studied in this way, less obtrusively, and at less cost, simply by overlaying time-series of different length. This simply reflects the fact that the most expensive data are those required for the most elaborate time-series.

B. Survey Operations

The following series of surveys will be required in order to implement the saturation and dispersed modules of the experiment respectively:

1. Saturation Module

The saturation module must be initiated with a baseline survey to provide data on eligibility, work history and related variables and to provide data on secondary research areas as well (family and community behaviour, social integration, etc.). All families in the saturation site would be interviewed (excluding whatever percentage refused to be interviewed), with the baseline survey being implemented in two phases. Two phases will be utilized to implement the baseline survey, in order to minimize the impact of that survey's census-like character. The first phase will involve a survey of one-third to one-half of the family units. The second phase will involve a survey of all remaining family units, and will be implemented as field conditions permit after phase one. The first periodic survey would begin about four months after the baseline survey, with a stratified random sample of recipients and non-recipients (see Section IV. D below) comprising the main panel to receive the periodic interviews.

2. Dispersed Module

The dispersed module must be initiated by a screening survey designed to locate families suitable (in terms of income, composition, etc.) for allocation to any of the experimental treatment or control groups. Preliminary work for the screening survey will include designation of priority target areas within the dispersed site, and complete listing of households in those areas. (Depending upon results of aggregate data analyses of the 1971 census, six to ten times as many families will have to be screened as will be chosen for the final assignment to the experiment).⁵ Subsequently a baseline survey will be

5. Complementary use of Manitoba Health Services Commission data files is also being considered for the delineation of target areas for the screening survey.

conducted with a much smaller sample of potentially eligible families. This survey will provide more complete data for determining eligibility, and periodic baseline data for families subsequently enrolled in the experiment. The first periodic survey would begin about four months after the pre-enrollment survey, with all experimental families and all families in the major control panel (see IV.D below) being interviewed.

3. Control Community

The notion of control is important to the saturation study, even though the only controls which are available may not be perfect. Significantly, no control community exists which is identical to the proposed saturation site on all (or even most) of the dimensions of size, social-demographic composition, and economic characteristics. Therefore, it is intended to develop a control panel matched on key social-economic characteristics, which is drawn from as similar a community as possible, and which taken in the aggregate may be comparable to the population of the experimental site. This control group would have to be developed subsequent to the implementation of the saturation experiment. As presently anticipated, the control panel would include 300 to 400 families chosen from Portage la Prairie and the rural area surrounding that community (see Section III.C.4).

C. Periodic Interviews

Interviews will be collected on a periodic basis from members of each family designated -- the period between interviews to be four months. Periodic interviews are required to provide the elaborate time-series data necessary to detect changes of behaviour in the primary and secondary areas of interest.

1. Frequency of the Periodic Interviewing

Ideally, the experiment would require the assessment of responses to the program at a fairly high frequency, for example on a weekly or monthly basis. Such data would allow for optimally satisfactory time-series analysis, as decisions relating to labour supply are for most segments of the population, made on a weekly, monthly, or

irregular basis. Unfortunately, this goal, like that of totally non-reactive data collection, can not be attained in the real world. Rather, it is necessary to balance the demand for frequent interviewing which is necessary for the development of reliable time-series data with the demand for socially acceptable and economically feasible interviewing. Both cost (which is high) and the task of maintaining reasonable cooperation from respondents indicate better payoffs from less frequent interviewing. Problems of reactivity and "learning effects" (of even greater importance with a panel survey) are also minimized with less frequent interviewing.

For these reasons, we have settled upon a four month interval, so that periodic interviews are conducted three times a year. This decision is based on: (1) the desire to use a relatively natural time interval, so respondents know when to expect interviewers and so the periodic interview corresponds regularly with seasonal changes; (2) American experience which has suggested that quarterly interviews are somewhat too frequent and irritating to respondents; and (3) an appraisal of two interviews per year (six month intervals) as inadequate both in terms of probable reliability of recall information, and in terms of the suitability of the resulting time series. Three interviews per year for our primary survey panels is settled on as a suitable compromise between requirements for more reliable observations and requirements for minimum disturbance of respondents' normal lives by survey operations. Periodic surveys would be conducted on an approximately continuous basis, a procedure which allows for economy in survey operations and also for considerable flexibility in the area of disaggregating survey data into more complex time series.

D. Composition of Panels

As was noted under IV.A., regular periodic interviews are applied to the major experimental and control panels only. Separate control panels in the saturation and dispersed sites are assigned to the annual and end of experiment control panels and all other individuals are ignored by survey operations (barring contingencies). Panels which

are reinterviewed during each regular periodic interview are comprised as follows:

1. The Saturation Module

In the saturation site the major panel is a random sample⁶ of the community's families stratified by income, rural-urban location, and family type, N = 2000 family units. Random sampling with stratification by income and family type assures adequate representation of recipient and non-recipient types as well as all major family types. Oversampling of GAI recipients in the major panel can be accomplished by stratification by income to include a satisfactory proportion of the eligible population. Secondary panels in the saturation site include 500 families assigned to the panel reinterviewed once annually, and an additional 500 families to be reinterviewed at the end of the experiment only. Assignment to each of these three panels is random so that mode of stratification is roughly identical across panels, and all assignment is based on baseline data collected in the original baseline survey. It is anticipated that an assignment model will be developed and applied to the task of achieving optimal statistical composition of the

⁶Random sampling here makes optimal sense, since (a) non-random sampling (selection of recipients only, for example) is likely to require program development information (especially delineation of the basic recipient population) which will not be available until well after field operations are launched; (b) the non-random sampling model (again for illustration purposes: selection of recipients only) will be untenable where the recipient population is changing; (c) non-random sampling becomes a potential source of stigma for interviewees who are more likely to be publicly identified (because of our survey presence) as recipients; (d) responses of middle and higher income groups are of noteworthy interest.

It is important to note that a major panel of N = 2000 families could encompass about 45 percent of the saturation population and that the supplementary panels outlined below bring the total survey "catch" to about 70% of the population.

stratified random sample.

2. The Dispersed Module

In the dispersed module of the experiment the major panel is comprised of all families assigned to any of the GAI treatments $N = 600$, and a control group of $N = 400$. This panel is interviewed periodically, three times each year for the duration of the experiment. Composition of the panel is predetermined from composition of the experimental and control groups. Secondary panels in the dispersed experiment include 200 control families assigned to the panel interviewed once annually, and an additional 200 control families to be interviewed at the end of the experiment only. Assignment to the major panel control group, and to the two additional panels is random, with the same mode of stratification applying in each. No treatment involving variation in the intensity of interviewing is applied to subjects who are in receipt of, or eligible for, GAI payments.⁷

E. Implementation and Scheduling of Surveys

1. Implementation of Surveys

It is presently anticipated that the survey operations for the Manitoba GAI experiment will be undertaken in collaboration with the Manitoba Bureau of Statistics, and with a broad participatory/consulting role allocated to Statistics Canada.

This organizational approach has been adopted for the following reasons. First, it is viewed as essential that the Manitoba GAI program itself (the administrative operation directly paying and otherwise affecting program participants) be separated in participants' minds from the survey operations connected with the experiment. The rationale for this organizational bifurcation lies in the need to minimize sources of error in the survey data. This position has been well-reflected in American experiences with income maintenance experiments.

7. This is primarily because of the cost of experimental families in the dispersed experiment. It is not thought desirable from a cost point of view to forego time series data on experimental (treatment) families in the dispersed module.

It has been the practice in all American income maintenance experiments to separate administrative and survey operations — partly to minimize response biases, but also to avoid potential administrative tangles that might ensue in triadic encounters between program participants, administrators, and interviewers. Problems encountered may include the following:

- i. respondents may exhibit heightened tendencies towards social desirability responding as a reaction to (incorrectly) perceived power of the interviewer to affect GAI benefits.
- ii. a similar distortion, which might fall under the heading of "demand characteristics" could result from the general link between the program and Project survey operations. This danger becomes greater, the closer the interviewers are to the researchers and to some greater empathetic understanding of the researchers' own hypotheses, expectations and biases. This problem is minimized by separating the survey force as much as possible from central administrative and research operations.
- iii. individuals who are overly anxious about relations with government programs (for example, individuals who are worrying about consistency of survey responses with income reports and other reports to the Project) are likely to react unfavourably and cooperate less with interviewers who may be seen as "caseworkers" or "spies". Refusal and attrition rates are bound to be higher in the case where the survey organization is a visible arm of the program.
- iv. attrition among control groups (and unpredictable kinds of bias) are likely to be heightened if the survey operations themselves baldly announce to controls that they are participants in (on?) the short end of a guaranteed annual income experiment.⁸
- v. it is desirable for us to learn some things about experimental subjects' behaviour which they could not report at all to an interviewer directly connected to the program, for example, under-reporting of income. Thus we must be able to guarantee

⁸This incidentally has happened anyway in the American experiments, although no published reports have indicated the extent or kind of side-effects which resulted.

that survey responses will not be made available to the administrative program. This is difficult at best, but clearly impossible for a survey organization directly connected to the program's administrative office.

- vi. if the administrative and survey organizations are not distinct, interviewers can spend a lot of time fruitlessly disentangling themselves from responsibility for program actions (such as changes in payments or other complaints) regarding which they cannot be responsible.
- vii. non-recipient controls are likely to respond with hostility to project-affiliated interviewers, simply because the interviewers are likely to be seen as representatives of a welfare-like program. This is particularly true regarding individuals who are over the breakeven-point in the saturation study.

Various alternatives for survey implementation have been examined. These included various commercial contractors⁹ as well as the alternative of the Project itself developing a survey organization. Various commercial firms have been tentatively rejected for reasons relating to cost and to quality control. Development of a survey organization by the Project alone has been tentatively rejected as unnecessarily duplicating functions of other agencies. The Manitoba Bureau of Statistics/Statistics Canada option is presently viewed as the optimal alternative for implementing the surveys. It is anticipated that this solution will provide the best organizational support for effectively undertaking and completing the surveys. Simultaneously it is anticipated that this arrangement will allow GAI survey personnel and other researchers the best possible control over the character and quality of the interviewing. Finally, this option provides important advantages vis a vis data confidentiality inasmuch as all personnel will be covered by the Manitoba Statistics Act.

2. Scheduling of Surveys

It is anticipated that the following schedule can be met for

⁹Estimates and operational proposals were sought from a large number of commercial and academic survey organizations, and detailed submissions were received from six such organizations between October 1972 and January 1973.

survey and survey-related operations. These estimates are based on the assumptions that all survey personnel are in place by March-April, 1973.

Survey and Enrollment Schedule

Saturation Experiment

(i) baseline surveys to collect primary data (May, June, 1973 and September to November, 1973)

(ii) screening and baseline survey in the main control community (July and September to November, 1973)

(iii) initiation of enrollment (January 1974)

(iv) periodic interviewing, at four month intervals, of a stratified random sample of saturation recipients and non-recipients and control community panels. (begins January, 1974)

Dispersed Experiment

(i) screening surveys and related operations to locate suitable families (May, June, 1973)

(ii) baseline survey (June, July, 1973)

(iii) enrollment (September, 1973)

(iv) periodic interview of recipients and control group (begins January, 1974)

F. Screening and Training of Interviewers

As was noted in IV.A. it is essential that the surveys be conducted by well-trained, competent survey personnel. This is especially important as: (1) all interviews will deal with sensitive information regarding income, net worth etc.; and (2) baseline and periodic interviews will probe in a number of other areas which are usually thought to be highly demanding of skilled interview delivery (i.e., such areas as family and community behaviour which may be sensitive to mode of interviewer presentation, and such general areas as attitudes, values and other subjective orientations which must be carefully presented and explained). This means that considerable care must be taken in the recruitment and training of interviewers and their supervisors.

To attend to this task, a special group will be jointly established by the Manitoba Minimum Annual Income Project and the Manitoba Bureau of Statistics, which group will be responsible for designing and supervising the implementation of interviewer recruitment, screening, and training procedures. It is anticipated that screening and training of survey personnel will be an on-going activity. Nonetheless, it may be supposed that initial activities in this area will be developed

on the basis of the following outline.

Interviewers will be recruited from occupational strata likely to provide high quality interviewers.¹⁰ This will be done in such a way that interviewers are drawn wherever possible from the same areas in which the surveys are to be conducted, or alternatively from areas as similar as possible to those target areas. This means that interviewers for the dispersed module of the experiment will be drawn almost entirely from Winnipeg and the immediate surrounding rural area. Interviewers for the saturation module of the experiment will be drawn almost exclusively from areas near the saturation site, and from similar communities in rural Manitoba.

Each potential interviewer will be required to perform satisfactorily in a personal interview designed to appraise probable suitability for survey work, and each potential interviewer will be required to administer a "trial" interview. Individuals performing in a satisfactory manner through these screening operations will be allowed to attend interviewer training sessions and be paid during their training.

Initial training for interviewers will focus on basic techniques of survey research and interviewing. Particular attention will also be paid to such matters as: the role of the surveys in the project as a whole; confidentiality of survey data; and field and payroll procedures. Subsequent training will alternate between small group sessions (including simulated interviewing) and real interviewing in field situations. Emphasis here is to be placed on improvement of interviewing techniques. In this connection, continuous assessment would be made of each interviewer's performance.

Such extensive and varied training will be essential, since only a minority of the persons recruited can be expected to have previous interviewing experience. It is anticipated that this training will take 60 to 80 hours, of which it is important that 15 to 20 hours are devoted to actual interviewing. Special training of much shorter duration will be required to introduce the interviewers to each of the

¹⁰It is anticipated that cautious use also will be made of research findings pertinent to the selection of high quality interviewers, particularly such findings as deal with the characteristics of high quality interviewers. (See, for example: Seymour Sudman, Reducing the Cost of Surveys, Chapter 8, "Cost and Quality of Interviewers", pp. 100-153.)

major surveys implemented.¹¹

G. Survey Quality Control

The goal of obtaining high quality data requires considerable attention to the minimization of error in survey responses. This goal is approached in two ways, first through the use of extensive pre-testing to develop reliable indicators, second through careful attention to survey quality control.

Survey quality control will be reinforced by each of the following. (1) Each survey returned from the field will be edited in detail by an interviewer supervisor or other quality control person. This will provide a check on such problems as incorrect skip-logic, and missing or illegible answers. (2) A random sample of each interviewer's work will be checked and validated by telephone or personal contact. This should eliminate gross attempts at fraud or falsification of interviews. (3) Interviewers will be randomly shifted from family to family through time, so that interviewers will generally be able to interview a different set of families during each periodic survey. (4) Interviewers will almost certainly be placed on a pay scale which (among other things) allocates a small financial penalty for each error in an individual interviewer's work. This procedure should result in greater attention and care in the interview work — thus reducing errors and improving survey quality generally. (5) Interview data will be cross-validated from the current periodic survey to previous periodic surveys to detect suspiciously radical changes in situation or behaviour indicative of probable errors. (6) The internal logic of each individual survey will be checked by a computer-editing program before the data is accepted for permanent storage.

11. In addition to normal on-going assessment of interviewer performance, a special study will be undertaken to determine the effects of interviewers on survey response patterns (For a discussion of this problem, see: Derek Phillips, Knowledge from What? 1971, pp. 78-123)

H. Security of family interview data

Security of family interview data is of the highest importance. Only if families and individuals can be secure in their belief that their responses are absolutely confidential can the researchers expect cooperation. For this reason, several security procedures will be followed.

First, all interviewers and all persons handling survey materials will operate under closely scrutinized check-out and check-in rules. This should minimize possibilities of survey materials going astray. Consistent with this procedure, an attempt will be made to keep all survey data-processing activities under one roof.

Secondly, all interview materials will be "masked" as soon as they are returned from the field. At the point where a given questionnaire has passed field editing, identifying information regarding the specific family and individual will be removed. This is easily done if identifying information is compartmentalized and if interview schedules are number-coded.

Thirdly, all interviewers, coders and other persons having access to individual survey data will be sworn under the Manitoba Statistics Act. This will reinforce the above security procedures significantly inasmuch as severe penalties may be levied under that Act for violations involving confidential data.

V. COMMUNITY DATA COLLECTION

A. Introduction

Elsewhere in this design are indicated experimental variables concerning individual and family behaviour, for which data will be systematically collected. But a basic rationale for the saturation module is the opportunity it affords to examine external factors which might be related to individual behaviour, and thus bear upon the interpretation of experimental results. Therefore, a major research activity will be the systematic collection of general economic, social and institutional data on community factors, especially in the saturation and control sites which are basic to the structural situations within which this behaviour will occur.

There is little theoretical guidance concerning the appropriate categories for which data should be collected. Items are included on the list because they will provide general background information. Unlike the variables specified in the design section, the designation of these factors is not informed by hypothesis about their dependent or independent relationship to key response variables. Rather, this data collection activity will involve the monitoring of a series of community indicators which may prove useful in the future development of hypotheses.* We are also considering collecting data on various kinds of unobtrusive measures (see section III.A.3).

Data collection prior to the start of payments will be heavily emphasized. This will give a history against which the subsequent modifications can be assessed. Also, it will yield a finer measurement of the natural (pre-payments) similarities and differences between the saturation and control sites. Data will also be collected, for a scaled-down set of categories, on the dispersed site. Using this data, community profiles will be constructed on all three experimental communities. An economic research assistant is being employed to work full time on the compilation of the data now available from governmental sources. Community data collection will be the responsibility of researchers in the saturation, control and dispersed sites. This activity will be a major part of the responsibilities of the senior field researcher stationed in the saturation community.

*See III.C for a brief discussion of how this relates to the overall survey design

B. Sources of Data

1. Experimental Site History

Information on local history will be developed through library and archival research, newspapers, and other locally published materials.

2. Governmental Agencies

The Province of Manitoba's Regional Analysis Program (Department of Industry and Commerce) involved the generation and analysis of a significant range of economic, social and demographic data on all possible experimental sites. Other Federal and provincial agencies which regularly collect relevant community data have been or are being approached with requests. These include:

- a. Manitoba Department of Health and Social Development, Community Operations Division;
- b. Manitoba Department of Agriculture;
- c. Manitoba Department of Labour;
- d. Canada Manpower;
- e. National Revenue;
- f. Unemployment Insurance Commission;
- g. Statistics Canada (Census data, etc.).

3. Non-Governmental Agencies

Such bodies as trade unions, farmers' organizations, cultural associations, trade and commerce groups, and professional associations are additional possible sources of data.

4. Survey Data

Initial discussions have been held with Statistics Canada on expanding the samples of existing surveys, such as the Monthly Labor Force Survey, Job Vacancies, Prices, Consumer Finances, etc. The collection of additional data not available through existing sources will be performed by the Project's baseline and periodic surveys.

5. Community Indicators

There are a number of social and economic indicators, such as the annual value of cheques cashed, that will be systematically collected by Project staff.

C. Types of Data to be Collected

While the categories are subject to considerable revision, the following list does represent the kinds of desirable community data, and it does indicate the magnitude of the community data monitoring task which is planned.

1. Standard Census Information such as ethnic composition, age distribution, marriage, birth, death and divorce rates, population growth rate, density, in-out migration, etc.

2. Community Development (patterns of settlement, establishment of community institutions, etc.).

3. Economic Development

a. Agricultural

- i. History of agriculture in the area;
- ii. Price trends and fluctuations for different crops and livestock;
- iii. Yield trends and fluctuations for different crops and livestock;
- iv. Historical trends in size and types of farms;
- v. Value of farms;
- vi. Farm incomes by type;
- vii. Rate of sale and abandonment of farms;
- viii. Governmental agricultural programs;
- ix. Farm organizations - existence and membership.

b. Industrial

- i. Number, size and type of manufacturing facilities;
- ii. Openings and closings of major facilities;
- iii. Major expansions or contractions of existing facilities;
- iv. Value of output by industrial category;
- v. Productivity.

c. Provincial and Federal Economic Development Programs

4. Employment/Unemployment

- a. Composition of labour force by job type, part and full time;
- b. Wage rates for job types;
- c. Unemployment rates by occupation;
- d. Seasonality of employment;
- e. Unionization rate;
- f. Participation rate;
- g. Job vacancy rate;
- h. Retirement patterns;
- i. Underemployment.

5. Housing

- a. Rental rates;
- b. Vacancy rates;
- c. Housing starts by type;
- d. Assessed valuation;
- e. Availability of low-income and subsidized housing;
- f. Mortgage rates.

6. Retail Trade

- a. Types of existing stores and services;
- b. Opening of new facilities;
- c. Value of retail trade, by types;
- d. Annual value of cheques cashed;
- e. Bankruptcy rate;
- f. Rate of purchase of automobiles and other consumer durables;
- g. Price levels of selected items.

7. Credit Structure

- a. Number and type of financial institutions;
- b. Interest rates;
- c. Value and purpose of business and personal loans;
- d. Amount of savings;
- e. Change in the number and type of financial institutions;
- f. Average number of days sales outstanding (collections);
- g. Government loans.

8. Municipal Financial History

- a. Size of annual budget;
- b. Revenues raised;
- c. Patterns of expenditures.

9. Health Services

- a. Number and type of doctors and dentists;
- b. Hospital size and facilities;
- c. Public health programs;
- d. Utilization rate of these services.

10. Schools

- a. Dropout rates;
- b. University and college attendance;
- c. Utilization of Manpower and other training courses.

11. Recreational Activity

- a. Admissions to National and Provincial Parks;
- b. Purchase of recreational vehicles;
- c. Expansion or contraction of facilities;
- d. Attendance at sporting, cultural, etc. events.

12. Nature and Utilization of Social Assistance and Transfer Programs

- a. Municipal welfare programs;
- b. Provincial programs, such as social assistance, provincial home repair program, the health card, etc.
- c. Federal programs, e.g. O.F.Y., F.I.S.P., U.I.C., etc.

VI DATA PROCESSING AND CONTROL

A. Storage and Processing

1. Introduction

A primary objective is to maintain survey data files and administrative data files as completely separate entities. This is essential, inasmuch as the two data sets will, to some extent, pertain to differing universes of individuals. Not all the recipients will be surveyed; therefore the administrative file will reference individuals and families not found in the survey data file. Similarly, many of those surveyed will not be recipients and therefore they will not be referenced on the administrative file. Additionally, it will be important that each data file allow for: (1) aggregation and disaggregation of family data over time; (2) simultaneous tracking of individuals or families through time; (3) economical data retrieval; and (4) integration with the other file, where possible, and where necessary, for research purposes.

The data processing system for administrative purposes (payments) will be designed to function as an operational program potentially suitable for a universal GAI, as well as part of the experiment.¹

There will be a procedure for matching files, by using the Social Insurance Number (SIN) and/or the Family Number fields. The family number field will allow for analysis of the break-up of families over the duration of the experiment. A unique number will be assigned to each family (support unit). If one filer in a support unit (see Section VII.A, Definition of the Support Unit) were to take up new residence, then a new number would be assigned, but his record would also contain his previous family number to allow matching back to his former family. The 'SIN' number will be the primary identifying field for each individual. Where a 'SIN' number is not available or not applicable, a dummy number will be assigned. The combination of these two fields will allow tracing of separate individuals, and individuals within families.

¹The Project's computer analyst and financial controller who are jointly responsible for the development of the Project's administrative systems, have visited Ames, Iowa to talk with the RIME data processing manager. Copies of input forms and computer output were obtained and will be an important base for our own systems design considerations.

2. Storage Medium and Hardware Requirements

The primary storage medium will be punched cards, which will be converted to magnetic tape for all data processing operations. Survey file data will be retained on magnetic tape, but may be written onto disk depending on data retrieval which is desired. The hardware used for these operations will be the Provincial Computer Centre's IBM Model 370 - 155 with 512 K memory. Peripheral equipment includes 9 - 9 track 2400 tape drives and 40 - 2314 disk drives.

3. Administrative File

This will be a tape file with a record for each filer. A copy of the proposed record may be found on pp 103-4. Current transactions will be applied to this file every two weeks. These transactions may include correction records, addition of new filers and income reports from filers. The file update process will include printing of error lists.

Filers will be required to complete an income report form every four weeks for mailing to the relevant field office. Employees of the field office will check reports for errors, omissions, etc. and take steps to correct information received. The reports will then be sent to the Project's head office for coding and keypunching. Blank income reports will be mailed out from Winnipeg to filers based on a list printed out as a by-product of the updating of the Master Payment File.

Cheques will be printed every two weeks, and as a by-product, statistical reports will be printed summarizing characteristics of the recipient population. These may include characterization of GAI recipients by (a) occupation type; (b) age; (c) sex; (d) family size; (e) marital status; (f) hours worked; (g) residence type; (h) education; and (i) ethnicity.

It is desirable to run a weekly edit to provide reasonably good turn-around from the time a person enrolls until he can expect his first cheque. It is also essential in order to re-introduce corrected records that have been corrected from the previous week's error listing.

The following does not constitute a complete list of the items to be edited; it is intended only to be representative of the checks to

(See Section IV .G. for a discussion of survey quality control procedures).

There will be a separate tape file for each of the periodic surveys as well as the baseline data. As survey information is obtained, the data will be coded and keypunched. The cards will be read onto magnetic tape for processing. The basic processing will be an editing function to ensure, to the greatest degree possible, that the information obtained is reasonably accurate. Each new period's data would be compared to information obtained in the last survey period in order to compare values and thereby establish an automatic survey quality control check. It may be desirable to transfer the information from the tape files to disk storage for specific retrieval in the future.

5. Summary

The above is a preliminary outline of the data processing system. The first consideration is to design the system in a fashion that will allow the greatest probability of success in the time available. Therefore, simplicity has been emphasized. The system will be made increasingly sophisticated over the course of the next three years. Since data processing is fundamental to the operation of a universal GAI, several different systems design ideas will be examined in the course of the experiment. Our ultimate objective will be to reduce the reporting inconvenience on the part of the recipient and minimize the cost of the coding and data transcribing function (keypunching). Such options are being considered as pre-punched cards, mark-senses cards, optical character readers (full sheets), and remote direct input and retrieval of data.

B. Automated Auditing Procedures

As a system, the guaranteed income can operate only if it is administratively simple, involving a minimum of contact between support units and Project personnel. At the same time, however, the Project must have mechanisms for ensuring that filers are receiving payments to which they are entitled. To serve both ends, the Project is investigating means of enforcement by comparing information supplied to the Project, with

that supplied by the same filer to other government agencies. What we are looking for are not minor, but gross deviations. When such discrepancies are found, a check will be made with the filer to determine, if possible, the cause of the conflict. In the experiment, the purpose of this will be to learn about the extent of mis-reporting which could be anticipated with a universal program and to develop automated auditing procedures for coping with this. In the experiment itself, only the most gross abuse will result in administrative action, i.e., separation from the program. The following data sources will form the basis for developing these automated auditing procedures.

1. Manitoba Health Services Commission (MHSC) File

A data processing file is now maintained containing a record for each person in Manitoba. The Project will submit a file of new GAI applicants to MHSC every month to allow the existence of dependents claimed by filers to be checked against the MHSC file. It is also possible to determine location code, address, birth date, social insurance number and MHSC registration number. Changes in the number of dependents claimed may also be compared to the MHSC file.

2. Manitoba Income Tax File

Permission will be requested from the Federal Government to allow the Project to use the Manitoba Income Tax file to compare the income declared for tax purposes to income declared to the Manitoba Project. As the file is over a year old (e.g., the 1971 information is not available until the spring of 1973), this is a long term control mechanism. Other data that can be compared are birth date, social insurance number, location code and address. Filers will, of course, be informed that these comparisons are to be made.

3. Manitoba Provincial Welfare File

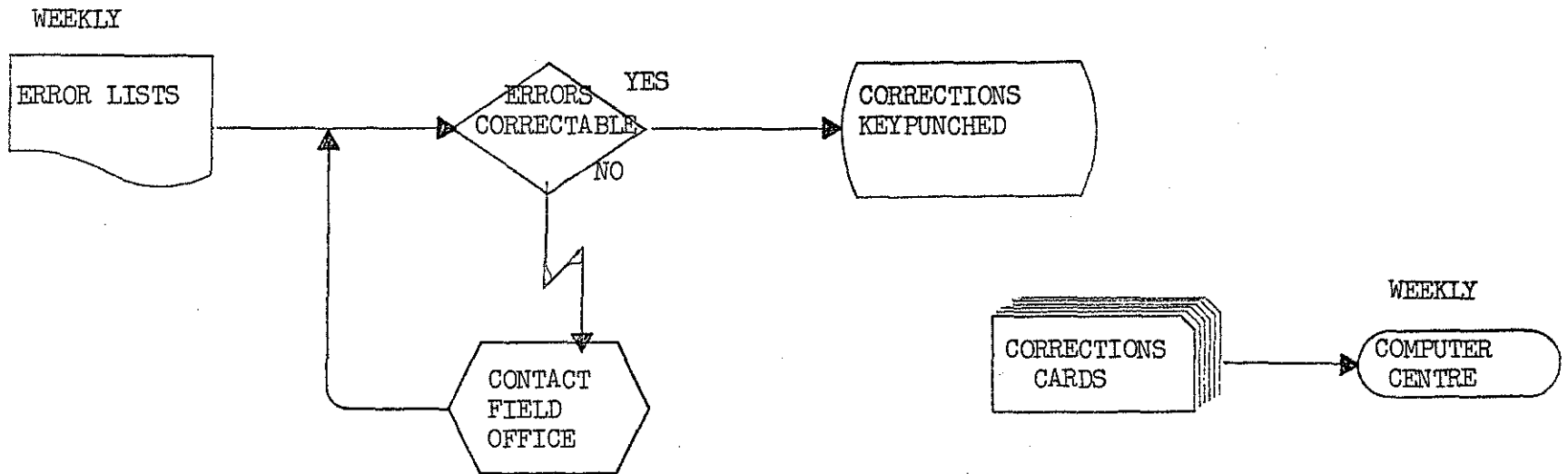
The Project's master file will be matched to the welfare file on a quarterly basis and a listing prepared of those people who received payments from both GAI and welfare in any one month. Other data that may be compared are birth date, location code, dependents, income, and address.

MASTER PAYMENT RECORD

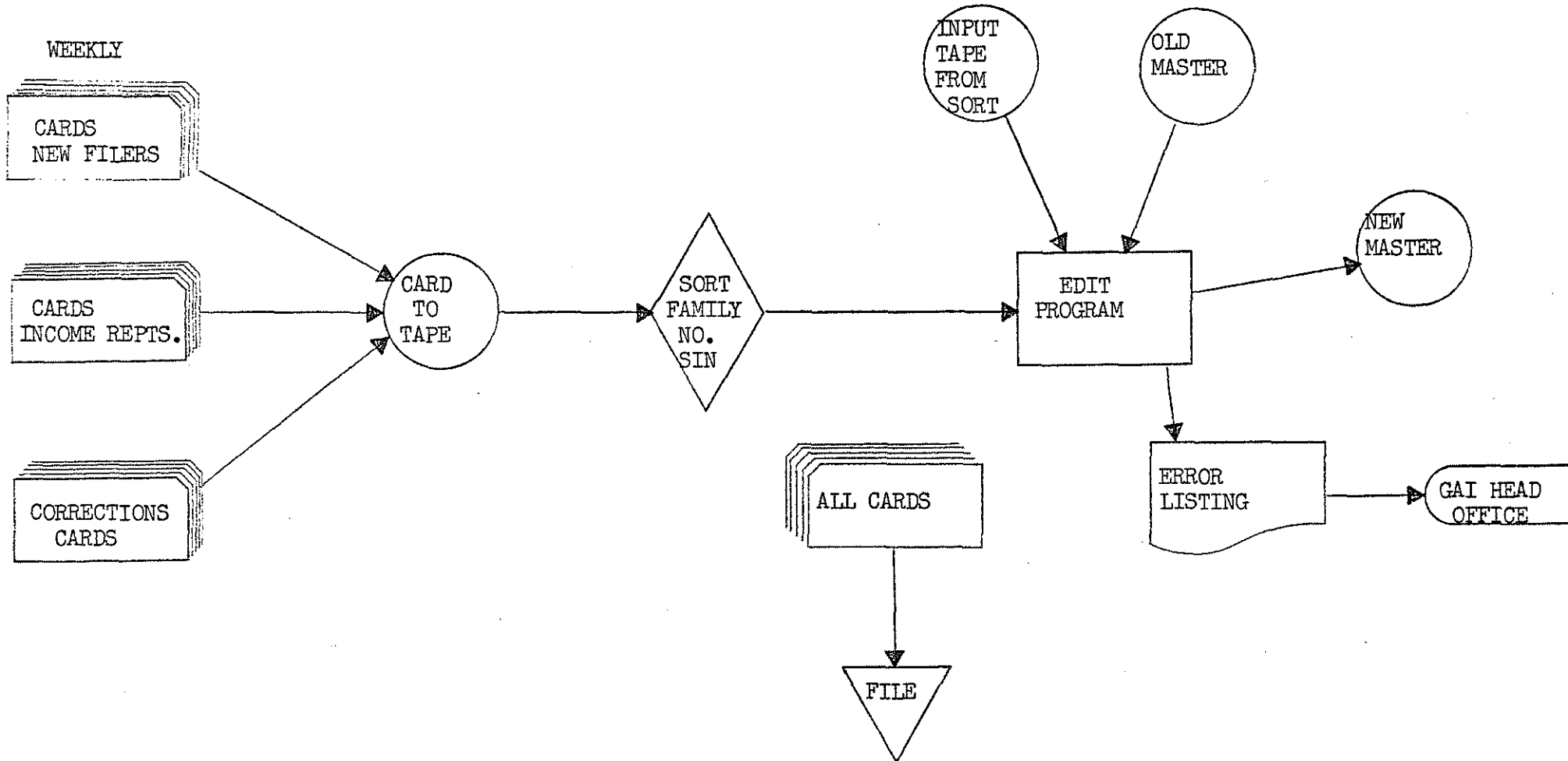
FIELD NAME	SIZE	EXPLANATION
Social Insurance Number	9	
Surname	20	
First Name	10	
Street Address	20	
Post Office/City Address	20	
Municipal Code	3	
MHSC Registration Number	7	
Birth Day	2	
Month	2	
Year	2	
Sex	1	1 Male 2 Female
Marital Status	1	0 Missing 1 Married 2 Widowed/Widower 3 Divorced 4 Separated 5 Single
Number of Dependents	2	
Occupation Code	2	(Tax Codes 00-82)
Residence Code	2	1 Renter Apt. Suite 2 Renter House Suite 3 Renter House 4 Home Owner 5 Boarder/Lodger etc.
Last Payment Period	2	
Last Income Reported Period	2	
Date of Manitoba Residence	6	Day Month Year
Date of Current Residence	6	Day Month Year
Filer Code	2	01 Family Head 02 Spouse 03 Related Other Adult 04 Non-Related Other Adult 05 Unattached Individual
Hours Worked	3	Major Source of Income
Carry Forward	5	

FIELD NAME	SIZE	EXPLANATION
Ethnic/Race Code	2	
Total Government Income	5	Welfare U.I.C. Children's Allowance O.A.S. G.I.S. Workmen's Compensation
Treatment Code	1	1 Tax Rate 1 2 Tax Rate 2 3 Tax Rate 3
Net Worth Amount	6	
Net Worth Deduction	5	
Family Number 1	5	Code to allow analysis of integration- disintegration over time
2	5	
3	5	
4	5	
5	5	
Education Code	2	Highest level of formal education obtained
Payment Amount - Period 1-2	5	
3-4	5	
5-6	5	
7-8	5	
9-10	5	
11-12	5	
13-14	5	
15-16	5	
17-18	5	
19-20	5	
21-22	5	
23-24	5	
25-26	5	
Income Reported - Period		
1-2	5	
3-4	5	
5-6	5	
7-8	5	
9-10	5	
11-12	5	
13-14	5	
15-16	5	
17-18	5	
19-20	5	
21-22	5	
23-24	5	
25-26	5	

HEAD OFFICE - ERROR LIST

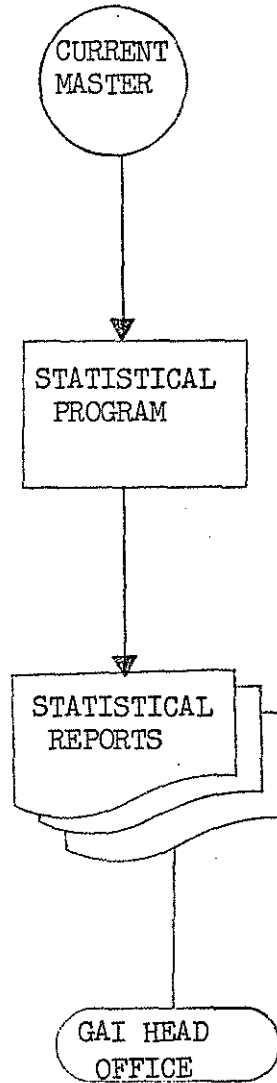


COMPUTER CENTRE - WEEKLY EDIT



COMPUTER CENTRE - MONTHLY

MONTHLY



C. Security of Data

To conduct this experiment, the Project must acquire and store a large volume of confidential data. We must be able to assure all participants that we can guarantee the security of this data. This obligation, which is a normal aspect of social science experimentation, is reinforced in this instance because the experiment is being conducted by the Province of Manitoba, with the support of Canada. Limitations also apply in this project, to the public release of analyses of the data. To ensure maximum confidentiality and security, these procedures will be followed:-

1. Every full-time member of the project will take the Oath of Allegiance to the Queen, which includes a confidentiality clause.
2. Each member of the project staff, and all part-time employees will be administered the Oath of Secrecy of the Manitoba Statistics Act. An offence against the Act makes the offender liable to a conviction and a fine not to exceed \$5000.
3. Use of the Act also provides direct protection to respondents because any information collected by interviews, etc. is privileged and cannot be used as evidence in any legal proceeding; nor can anyone be legally required to produce in Court or for any other body, information obtained under the authority of the Act.
4. In no case will data on any identifiable individual, family or business be released to any person or agency; nor will published results contain data on identifiable individuals, families, or businesses.
5. Only persons sworn under the Act will be permitted to examine any individual survey return, or other raw data.
6. All data will be stored at the Provincial Computer Centre. Only the project's computer analyst can authorize access to these files. To ensure responsibility, the analyst has been designated as Project Data Security Officer.

7. Release of aggregated data and the project's analysis of it, will be governed by the provisions of the Act and will also require the written approval of the Minister of Health and Social Development.

VII RULES OF OPERATION

A. The Support Unit

1. Definition of the Support Unit

A support unit will always consist of at least one filer, plus any other adults, and any dependents of the filer, and any dependents of the other adults.

Each of the following will normally be treated as a multi-member support unit. Any unit falling under one or more of these definitions will be treated as a multi-member support unit.

(Note: The concept multi-member support unit always includes families as we usually think of them and as defined in American NIT experiment rules of operation. Thus, for example, the concept encompasses nuclear, census and economic families. But not all multi-member support units are families).

- (a) two or more persons 18 or over (and their dependents), normally living together, and related by blood, marriage or adoption;
- (b) two or more persons 18 or over (and all dependents), normally living together, who regularly pool their income;
- (c) a single parent living with at least one natural or adopted child;
- (d) two or more persons living together with at least one child (that child related by blood or adoption to at least one of the adults).

Any person 18 years of age and over who is not a member of a multi-member support unit as defined above, or a dependent of a member of a multi-member support unit as defined above, can be treated as a single-member support unit. Anyone less than 18 years of age is ineligible for treatment as a single-member support unit.

A husband and wife normally living together will (along with their dependents) always constitute a single support unit.

A person can not belong to more than one support unit in any one month.

2. Members of the Support Unit

Members of the support unit include the filer (or filers), other adults and dependents. The following pertain to each type of member:

The filer(s) are responsible for filing the income report form for the support unit. The filer(s) in a support unit will always be the head(s) of that support unit. The head(s) of the support unit will be identified as follows:

- (a) where a family unit contains a husband and wife, both are heads of the family unit.
- (b) if the unit does not contain a husband and wife, then the head is to be a person who is a parent or guardian of a dependent child in the unit.
- (c) any eligible person who is co-habiting with a head of an eligible family unit, will be considered a head of that unit.
- (d) where the above definitions do not result in identification of one or more heads for the support unit, the head will be the oldest employed person or the oldest person, if no members of the unit are employed. The head(s) identified in this way will be the filer(s) for the unit, but the members may designate one filer if they wish.

Other adults in a multi-member support unit will always be 18 years of age or older, or persons having other dependents. Unrelated persons living in a household and paying rent will not normally be viewed as "other adult" members of the support unit comprising the household.

Dependents will always include dependents normally claimed by the filer and by any other members of the support unit for positive income tax purposes. Additionally, dependents may include:

- (a) natural or adopted children less than 18 years of age normally living with the eligible unit;
- (b) non-related children normally living with the eligible

unit, and normally receiving more than 50% or his/her/their support from the eligible unit (excludes foster children);

- (c) natural or adopted children not resident with the support unit, but receiving more than 50% or his/her/their support from the support unit;
- (d) other related persons over 18 if the support unit provides for more than 50% of his/her/their support.

The following also apply regarding dependents:

- (a) if a person over 18 is declared a dependent, this declaration must be agreed to by said dependent in writing.
- (b) no person may be a dependent for more than one support unit.

B. Residence Requirements

Because we are conducting an experiment which is not a universal program, we anticipate that there may be some problem with people moving into the experimental sites in anticipation of receipt of payments. This in-migration response to the program could be a particularly serious problem in the saturation site. For this reason, it is necessary to establish basic residence qualifications.

Thus we will require that the primary filer in each support unit be (a), a resident of the Province of Manitoba, and (b), a resident of the experimental site on the date of the first public announcement of the experimental site. For those subsequently applying for enrollment in the saturation site, the primary filers must have been residing in the community for one year prior to application.

For purposes of enrollment we will require that each primary filer have: (a) A Social Insurance Number card and (b) A Manitoba Health Services Commission (MHSC) card. This latter card shows name, address and date registered. The MHSC card can be taken as prima facie evidence of residence, and date of residence. Other documentary evidence may also be acceptable, such as: voters' lists, tax rolls, etc.

Since there are problems inherent in any definition of residence, we will allow rulings on this and other qualifications to be appealed to the Review Board. (See Section VII.F.)

C. The Accounting Method.

We begin from the assumption that a guaranteed annual income program should respond to current need, while taking into account the factors of net worth and income history, i.e. we will weigh current need against the ability to meet that need. Therefore, it is intended that payments will reflect a filer's income history, present income and present net worth. The length of time which will elapse before transfer payments increase to reach full support will be determined by these factors.

1. Accounting Period

Current need will be calculated by averaging income over the two complete calendar periods prior to application (a calendar period consists of four weeks). Any filer whose average net income (ANI) for the last two periods prior to application was either a loss or less than the breakeven for the appropriate family size, can qualify for a transfer payment in the period of application, and so on, always using family income for the past two periods.

The two period accounting plan is a compromise which will give us a reasonably quick response to income changes, while at the same time affording averaging capability which can smooth out incomes which fluctuate above and below breakeven. The two period accounting plan narrows the fluctuations and has the advantage of reducing the potential for overpayments. By contrast, a one period plan would be more responsive, but would also tend to produce more overpayments which could only be recovered over a longer future time span, and a three or more period accounting plan is insufficiently sensitive to current need.

The ANI can be calculated in different ways, some of which are:

- (a) i Previous period's actual income in total
- ii Previous period's actual income up to B/E - the excess being assigned to Carry Forward (CF).
- (b) i A 2 or 3 period moving average of actual income in total
- ii 2 or 3 period moving average of actual income up to B/E with the individual period's excess over B/E being assigned to CF.
- iii 2 or 3 period moving average of actual income with the excess of the average beyond B/E assigned to CF.

The last method (b)(iii) is the most consistent with the considerations which lead us to opt for the two period accounting plan. The ANI may,

however, be affected by imputing an amount of carry forward and net worth from the filer's previous income history.

2. Calculation of Carry Forward.

Carry forward is the mechanism by which recognition is given to income activity over a longer span than two periods. Carry forward can be positive, nil, or negative. If positive, it represents that portion of the average net income which is in excess of breakeven; if negative, it represents a loss incurred over the carry forward period. These excess incomes or losses are set aside and used to moderate future income fluctuations. The carry forward mechanism acts like a savings account if positive, and a loan, if negative.

The Manitoba experiment will determine carry forward as follows. Upon enrollment we will consider a full year's income prior to the period of enrollment, taking the current calendar year first. Assume, for example, an enrollment during the month of September -

Current calendar year, income Jan. - Aug. :	\$ 5300
Previous calendar year, total income: (\$9600)	
Sept. - Dec. estimate (\$9600 x 4/12)	<u>3200</u>
	\$ 8500
B/E level/yr:	<u>7400</u>
Carry forward:	<u>\$ 1100 (see "illustration" # 1)</u>

The previous calendar year "estimate" calculation is used because we do not anticipate that monthly recall of the previous year will be possible. Given this we must accept the undue influence accorded the early part of the previous year by the calculation.

The carry forward balance at any given time will be the net sum of:

- (a) initial carry forward
- (b) plus subsequent positive CF
- (c) minus subsequent negative CF
- (d) minus any imputation (or consumption) of carry forward

3. Imputation of Carry Forward.

The key in income maintenance is the rate at which we take excess income (carry forward) into account in calculating current transfer payments, and for how long after this excess was earned. The "20% of diminishing

ACCOUNTING PLAN
2 periods/20% of reducing
balance

Income History	
Last 2 periods	Last year
\$900.00	8500

B/E	Period	Year
S	\$570. 285.	7400. 3700.

Period	Actual Net Income	Past 2 periods Average Net Income (ANI)	Carry Forward imputation(+) set aside (-) (C.F.)	Net Worth imputation (N.W.)	Total Deemed income (T.D.I.)	GAI Transfer Payment	Carry Forward Balance (1)	8500 7400 1100
	500							
	400							
	500	450	120(3)		570	0		980
	600	450	120		570	0		860
	800	550	20		570	0		840
	400	700	-130(7)		570	0(9)		970
	200	600	-30		570	0		1000(2)
	1200	300	200(2)		500	35(10)		800
	300	700	-130		570	0		930
	-900	750	-180		570	0		1110
	-500	-300	300(4)		0	285(11)		810
	-300	-700	700(5)		0	285	(5)	110
	100	-400	400(6)		0	285	(6)	-290
	800	-100	100		0	285		-390
	300	450	-78(8)		372	99		-312
	-600	550	-62		488	41		-250
	1200	-300	300		0	285		-550

CALCULATION OF PAYMENT

Support Level(S) - (Total Deemed Income x Tax Rate)

Example item (9)	285	-	(570 x .50) = 0
	285	-	285 = 0
(10)	285	-	500 x .50 = 250
	285	-	250 = 35
(11)	285	-	0 x .50 = 0
	285	-	0 = 285

balance" method is the one to be used in the Manitoba experiment. It has the following desirable characteristics:

- (a) It has an open time horizon - the consumption of carry forward is dependent upon current income first, which means that we can store carry forward for several years, if we so desire. Of course, we also have the option of wiping it out in extenuating circumstances.
- (b) Consumption of carry forward is heavier during the earlier periods after the high income was earned. This results in a support pattern which continues to gradually increase until it reaches full support after several periods.
- (c) This method has long been used for income tax depreciation and is familiar to many people.
- (d) Normal imputation (or consumption) will be at the rate of 20% per period of the existing CF balance from the previous period [see "illustration" item (2)].
- (e) The imputation amount may be less than 20% (3) so that current (average) income and imputation of CF will be = to B/E. Support payment will be nil at this point.
- (f) Conversely, should current (average) income be a loss, then imputation may be more than 20% (4) in order to liquidate this loss, bringing combined current income up to '0'. In this situation support payment will be made in full.

At this juncture it might be useful to point out that neither the carry forward "set aside" nor "imputation" are necessarily independent "flows". For example, it is presumed that a family cannot live on less than '0' income, so if average net income (ANI) is a loss, sufficient funds must flow in to liquidate this loss. This is accomplished by the family either drawing from existing savings (imputation) (5) or by going into new debt (negative "set aside") (6).

Conversely, a family is presumed to live on average net income (ANI) within B/E. Any excess over B/E is presumed to be saved (positive "set aside") (7). It is further presumed that a family makes some payments (negative "imputation") (8) towards debt as long as there is some current income (ANI) to pay from.

The net of average net income (ANI) + carry forward flows = total deemed income (T.D.I.) each period. This is the total income that the family is deemed to have available to live on. This is the sum on which GAI transfer payments are based. Using the standard negative income tax approach, this earned income (TDI) is 'taxed' at a given rate (35%, 50% or 65% in Manitoba). The remainder after tax is subtracted from the support level (S) to determine GAI payment.

Thus: Average net income (ANI)	450	^{see} illustration (8)
Less: carry forward (CF)	<u>-78</u>	
= Total deemed income (TDI)	<u>372</u>	
which is 'taxed' at 50%		
leaving, after tax, income of	\$186	
on which GAI transfer is based	99	

'Taxing' is simply a means of determining how much the transfer payment is reduced for every additional dollar of natural earnings (Total Deemed Income). e.g.

	<u>Earnings A</u>	<u>Earnings B</u>
Total deemed income (1)	<u>372</u>	<u>472</u>
Taxed @ 50%		
Leaving (2)	186	236
GAI transfer (3)	<u>99</u>	<u>49</u>
Total support level (4)	<u>285</u>	<u>285</u>
Combined income (1)+(3)	<u>471</u>	<u>521</u>

It should be noted that:

- (a) As the family's income increased by \$100, their GAI transfer was reduced by \$50.
- (b) The combined income always increases as earnings increase.

4. Types of Income Patterns and Overpayments

A sensitive aspect of any GAI scheme is the possibility of overpayments. To place the whole topic in perspective it is useful to categorize possible types of overpayments.

Given the various types of possible GAI payment schemes, we make the following observations of earned income patterns.

1. People who are consistently in poverty due to no earned income (probably on some welfare program).

STATIC SITUATIONS

2. People who are consistently in the low income range (0 - B/E).
3. (a) People whose incomes are generally increasing in normal increments year by year.
(b) People (salesmen) whose incomes fluctuate above and below B/E (slightly) from one year to the next.
4. (a) Seasonal workers whose annual income is between 0 - B/E but have some Nil or low periods.

PREDICTABLE ANNUAL CYCLICAL OR COMMON FLUCTUATIONS

- (b) Seasonal workers whose annual income is above B/E but have some Nil or low periods.
5. (a) Farmers or business men whose annual incomes are between 0 - B/E with some low, nil or loss periods.
(b) Farmers or business men whose annual incomes are above B/E with some low, nil or loss periods.

NOT PREDICTABLE DISRUPTIONS

6. (a) Short term
(b) Long term

The prepared system would cope with the above patterns in the following ways.

- 1 and 2 - They should never get overpaid since we will be able to correct as we go (these filers are always entitled to new transfers from which to recover).
- 3 (a) As people "graduate" from our program by achieving natural incomes beyond B/E, they would surely not be expected to repay, since that would constitute a penalty for leaving the program. Such overpayments quite properly constitute phasing out costs, and in any event cannot be extremely high.
- 3 (b), 4 (b), 5 (b) After the first year, the carry forward will put a big dent in any potential overpayments.
- 4 (a) No problem
- 5 (a) No problem
- 6 (a) No problem - a basic forgivable allowance of 2 periods may be provided for the short term.
- 6 (b) Also bear in mind that "disruptions" has the connotation of a significant drop in income. Carry forward would come into play.

D. Definition of Income and the Application of the Offset Tax Rates.

1. Income to be Exempt from Offset (i.e. tax rate = 0)

All income, either money or in kind, derived from a Federal, Provincial or Municipal needs-tested welfare program will be exempt from offset, provided that the calculation of benefits for each case under the welfare program fully reflects the GAI payments received. The introduction of GAI into the saturation site will relieve the existing Provincial and Municipal social assistance systems of a great deal of the pressure they now bear. This in turn should allow social assistance personnel to expand their capacity to react quickly to changes in the resources and/or needs of their clients. It is essential that effective communication exist between the Project and social assistance administrators, as a check on abuse by people receiving benefits under both programs. The view here is that GAI would be the basic or foundation program that would be adequate for most "normal" families, while social assistance would be a supplementary program providing a much more sensitive service tailored to special individual and family needs.

2. Income to be Fully Offset (i.e. tax rate = 100%)

Since they can be considered as substitutes for GAI, all other government transfer payments will be subject to 100% offset in the period received.

The 100% offset rate significantly reduces the financial cost of GAI, yet has no detrimental effect on incentives since participation in social insurance schemes is compulsory. In fact, applying an offset tax rate of less than 100% to programs like unemployment insurance would actually reduce work incentives. This is illustrated in the following example:

Consider a family eligible for GAI where the head can work, but is currently on unemployment insurance. If G is the GAI guarantee, and say, 50% is the offset tax rate used for both earned income (Y_e) and un-

employment insurance (.75Y_e), then the total family income (Y_t) will be:

- a. no job, with unemployment insurance:

$$\begin{aligned} Y_t &= G - .5(.75Y_e) + .75Y_e \\ &= G + .375Y_e \end{aligned}$$

b. if the family head is offered and accepts a job at his regular wage (Y_e), family income will rise to:

$$\begin{aligned} Y_t &= G - .5Y_e + Y_e \\ &= G + .5Y_e \end{aligned}$$

The increase in income gained by searching out and obtaining the job is only 12.5% of his earned income (and an even smaller percentage increase in his total income). Hence, this individual effectively faces a marginal tax rate of 87.5% which significantly reduces the financial incentive to get a job.

There are three problems to be resolved in the area of transfer payments.

a. Lump sum payments. When a lump sum payment is received as compensation for lost income and/or injury (eg. Workman's Compensation) from a compulsory government program, it will be treated as a regular transfer payment offset @ 100% (see carry-forward provision below). Where the payment is made from a voluntary government program, such as a crop insurance payment, it will be treated the same as a private insurance payment and would be subject to the prevailing GAI offset rate (Section 3 below).

b. Low cost public housing. For horizontal equity, GAI recipients living in subsidized public housing should be credited with an imputed transfer payment equal to the rental subsidy. If this were not done, the result would not only be inequitable, but would also affect the marginal effective tax rate faced by such recipients. Since rents in Manitoba for public housing are income-tested in a way that imposes an implicit "rental tax", the GAI recipient in public housing will face a marginal tax rate equal to the sum of the GAI offset rate and the

implicit rental tax rate. If the rents in low cost housing are a proportion of income with the proportion rising as income rises, rather high marginal "rental tax" rates could be reached (over 30%) raising the total effective tax rate to over 80%. The above arguments also apply to other income-tested programs.

c. Carry-forward provisions. Given that transfer income is treated differently for offset purposes, how should it be treated for carry-forward purposes? Here, administrative simplicity dictates that we carry forward all income over break-even¹ (regardless of source) and offset all carried forward income in future periods at the normal GAI rate.

3. Income Subject to the Prevailing GAI Offset Rate

All other sources of income not covered under 1. and 2. will be subject to the normal offset rate after the deductions to be noted later. Income included in gross income shall be the following:

- a. earned income, e.g. wages, salaries, bonuses, fees and commissions, and other compensation for services
- b. investment income in all forms
- c. net income from self-employment (with limitations on allowable expenses as noted below)
- d. gifts in cash or kind from those outside the support unit
- e. inheritances (net of taxes) and trust fund payments
- f. all insurance payments including those representing reimbursement for loss of physical assets, income and injury (except where used to offset premiums)
- g. income in kind (only where easily enforced e.g. room and board, company car, etc.)
- h. interest and royalties
- i. rental receipts
- j. strike pay
- k. alimony, support payments and support in kind
- l. prizes, awards and scholarships
- m. private pension, annuities, etc.
- n. net yield from the sale of assets

The last item should be commented on. The yield from the sale of assets net of the direct costs of the transaction is included since this

1. Break-even to be calculated on the basis of the normal offset tax rate.

revenue reflects liquid resources available for consumption. These revenues should be fully included in income exempt to the extent they are used to pay off existing debts. This revenue would include realized capital gains but ignore capital losses. However, it should be noted that this approach discourages the sale of assets since the receipts are taxed at a minimum of 40% while their retention would only involve the net worth consumption tax of 15%.

4. Deductions

The following items will be deducted from the calculation of total deemed income (see p.118):

- a. for work-related expenses, it would be desirable to allow only justifiable expenses. However, obvious enforcement problems suggest an arbitrary rule - e.g. deduction = 3% of labour income up to maximum of \$150/year.
- b. alimony payments (or support) if legally required and actually paid.
- c. intra-family transfers - only deductible if another unit on GAI pays the offset tax upon receiving the transfer.
- d. no depreciation expenses allowed as deductions from business, revenue property, or farm income on the grounds that the GAI program is not trying to encourage investment in physical assets. The primary purpose of this exclusion being to avoid the extensive and obvious possibilities for abuse.
- e. day-care expenditures - actual costs can be deducted (receipts required), up to \$500 per child, \$2,000 per support unit, per year.

5. Carry-Over of Business/Farm Losses

As long as depreciation is not an allowable expense, any net loss arising from the operation of a business, farm, or rental property will be deducted from other income. Where total net income for the period is negative, this negative entry will be carried over in the regular fashion and used to reduce future income.

E. Net Worth Imputation.

Net worth imputation refers to an attempt to pragmatically impute income to non-income generating assets, such as owner-occupied dwellings, as well as to assets that generate most of their return in the form of capital appreciation rather than income. The approach suggested applies an imputed rate equal to the three year moving average for provincial borrowing rates (r) to the GAI recipient's net worth (W , defined later) and subtracts actual net income generated by the assets:

$$\text{Net Imputation} = r \cdot W - \text{actual asset income}$$

Where business or farm assets are involved, there is a serious problem in trying to separate returns to labour from returns to capital. Possibilities being examined here are to use the provincial minimum wage with an assumed 40 hour week, or to accept the applicants' division subject to some arbitrary constraints.

The net imputation component of income is designed to broaden the income base subject to tax so that it more fully reflects the income generating potential of physical and financial assets. However, a support unit with significant positive net worth not only has an enhanced capacity to generate income, it also has the capacity to raise its standard of living through dis-saving. The extent to which a support unit should be expected to consume assets for self-maintenance is a policy decision. This decision will have a significant impact on program costs. A computer based cost model developed by the Project indicates that omitting a net worth consumption rate from the calculation of benefits would double the cost of transfer payments relative to a 15% imputation rate. Increasing the net worth imputation rate beyond 15% does not result in equivalent savings; the basic cost sensitivity is to the first 10% or 15% of imputation.

In the long run net worth imputation may well affect the incentive to save, the incentive to invest (particularly in housing), and the viability of small owner-operated businesses and farms. It is possible

to have different rates of net worth consumption applied to different types of assets reflecting differential degrees of liquidity as an attempt to encourage socially desirable forms of investment, but the administrative problems are severe.

We are designating a net worth consumption rate of 15%, to be applied to total net worth subject to the deductions noted below. Net worth is defined to include the current market value of all real and financial assets excluding only personal possessions (such as furniture, clothes, appliances, etc. as long as no individual item exceeds \$1500 in value) and automobiles. Where maxima above are exceeded, the excess value must be included in net worth. Where there are pensions, annuities, etc., the values are not capitalized but the payments are treated as income when received. The only exception here will be the case where the filer had control over the amount of payments. Here it must be ensured that the income from this asset is not being postponed in order to increase current GAI benefits. This could be done by imputing a minimum income to this asset if the actual yield fell below the rate of return used in the net imputation component of income.

All debts and liabilities that can be substantiated would be deducted from the total value of assets to obtain total net worth.

For administrative simplicity, basic exemptions of \$2000 per year for a single member support unit and \$4000 per year for a multiple member support unit will be allowed in the net worth calculation. In addition, all farmers and self-employed businessmen will be allowed a limited exemption for their business assets to ensure that the operation's long run viability is not jeopardized because of a short-run financial crisis. This will be a \$15,000 per year maximum exemption for business and farm assets.

While the initial enforcement problems here will be substantiated, the equity and cost implications certainly justify a serious attempt to incorporate net worth considerations. The Project's experience with the net worth concept will indicate whether it is a feasible dimension of a universal program.

F. Appeal Procedure

Any person who is a resident of Manitoba and a potential filer in the dispersed, saturation or control site can appeal if he feels that he has received unfair treatment because:

- a. He was not allowed to apply.
- b. His request for payments was not responded to within a reasonable time.
- c. His application was denied.
- d. His payments were cancelled or varied in a manner seen as contrary to the regulations of the Project.

In the first instance, the appeal should be directed, in writing, to the relevant field manager. The field manager will render a decision in consultation with the Project Director (or his designate) and the Financial Control Officer. At the request of either the filer, or a Project officer, a personal interview with the filer may be a part of this process. A decision will be required within 14 days from the date of receipt of the formal appeal.

If this decision is unsatisfactory to the filer, he or she may appeal to a Review Board. Said Board is to be composed of five persons, appointed by order of the Lieutenant-Governor-in-Council. The chairman and any two members shall constitute a quorum. The Board shall hold a hearing on the appeal, for the purposes of which it shall secure the required and available information from both the filer and the Project Director. It shall be the policy of the Board to make a decision as soon as possible, and to immediately notify those concerned of this decision.

VIII REGULAR OPERATIONS

A. Enrollment

Field office enrollment teams will be set up in advance. These teams will be trained in sessions which will include familiarization with enrollment forms and procedures, in a simulated field setting.

Enrollment will take place through personal interviews, in either field offices or homes. The enrollment site will be a variable in the saturation enrollment experiment (see pp 158-65). All dispersed site enrollment will be in the filer's home. Enrollment officers will fill in the application forms while interviewing potential recipients.

For dispersed enrollers the classroom training will be supplemented by participation in a pilot enrollment project mounted during July and August, 1973. Here, up to 100 control and payments support units will be enrolled, in advance of the general enrollment which will commence on September 4, 1973. This pilot enrollment will be conducted as a training exercise, with special attention also being paid to the adequacy of the administrative system.

When it is completed, the personnel required for the once-and-for-all enrollment in the dispersed site will then be transferred to the saturation site, where there will be continuous enrollment. Most of the enrollment personnel required for the saturation site will already have been trained by conducting enrollments in the dispersed site. Because enrollment in the saturation site will be a continuous process, there will be two full-time persons permanently assigned to this task, one of whom will be designated the overall enrollment supervisor. It is anticipated that two persons will be sufficient to handle enrollment once the initial phase is completed.

During the initial saturation enrollment phase, however, when the enrollment load is at its peak, the enrollment staff will be supplemented by the six enrollee contact personnel (who, when full enrollment is reached, will act as payments auditors responsible for checking the monthly income report forms). We assume that six persons will be sufficient to handle the processing of the IRF's at full enrollment, but until this has been reached a portion of this force will

be available for enrollment duties. As the demand on enrollment staff decreases, the demand on the payments staff will proportionally increase (as support units come "in pay") and payments personnel can be gradually shifted over to their regular duties. If necessary, the enrollment force will be supplemented by interviewers who have demonstrated particular skill and sensitivity in their relations with the public.

Because enrollers will assist filers in filling out their first monthly income report form, having enrollers experienced with payments regulations and procedures will be a considerable advantage. Also these people can be shifted back to enrollment on an ad hoc basis as required by the load on the system. Giving interviewers this experience should also improve their competence. In general, the Project will encourage this cross-training for all personnel.

At enrollment, where possible each support unit will be assigned a contact officer by name, and instructed to call on him for future assistance. The use of contact officers as enrollers in the saturation site will mean that many support units can be assigned to the contact officer who enrolled them, thus establishing the basis for a personal relationship.

Completed application forms will be reviewed by the enrollment supervisor and the field manager, and submitted daily to head office for coding. Control lists will be prepared by the field office prior to submission. A copy will be retained in the field and another will be submitted to head office with the application forms.

At the head office the enrollment forms will be processed by coders performing three operations: (1) they will generally peruse the application forms to make sure that the data is complete; (2) they will generally examine the data to see that it is internally consistent and logical; and (3) they will then apply codes to prepare the data for keypunching. The coded application forms will then be key punched and verified, and made ready for computer input.

B. Processing of Income Report Forms (IRF)

At the time of enrollment, filers will be instructed in the filling out of the IRF's, and the first one actually completed. The forms will be examined by the enrollment officer for completeness and accuracy.

Once the filer's initial enrollment is processed, subsequent income report forms will be automatically mailed from the head office to established recipients. These forms will be generated by the computer after each payment, with certain information already filled in. If the filer goes over breakeven, no form will be generated. Instead, a standard letter will be sent explaining why the filer is ineligible, and giving information on the appeal procedure.

Filers will complete the IRF's at their home and mail them to their field office. If a filer has problems, he may bring the IRF into the office, or call, to receive assistance from the enrollee contact officer assigned to him.

The IRF can either be mailed or personally delivered to the field office. Filers will be required to include supporting documentation with their IRF's; for most filers this will take the form of a pay stub. Contact officers will scan the IRF's and the supporting documentation. If there are questions, the contact officer will initiate an enquiry, most of which can probably be handled by phone. A copy of the IRF will be returned to the filer, along with his supporting documentation. Another copy will be retained in the field office. The original IRF's are sent to the head office daily for processing.

In preparation for key-punching, head office coders will re-check the validity of income data and code the reports. The bulk of the editing will be generated by a computer program. Batch tapes will be taken for control purposes prior to key-punching.

Income report forms will be kept at the head office, along with the computer-produced "calculation of payments". There will be a separate file for each filer, giving us a complete record by support units.

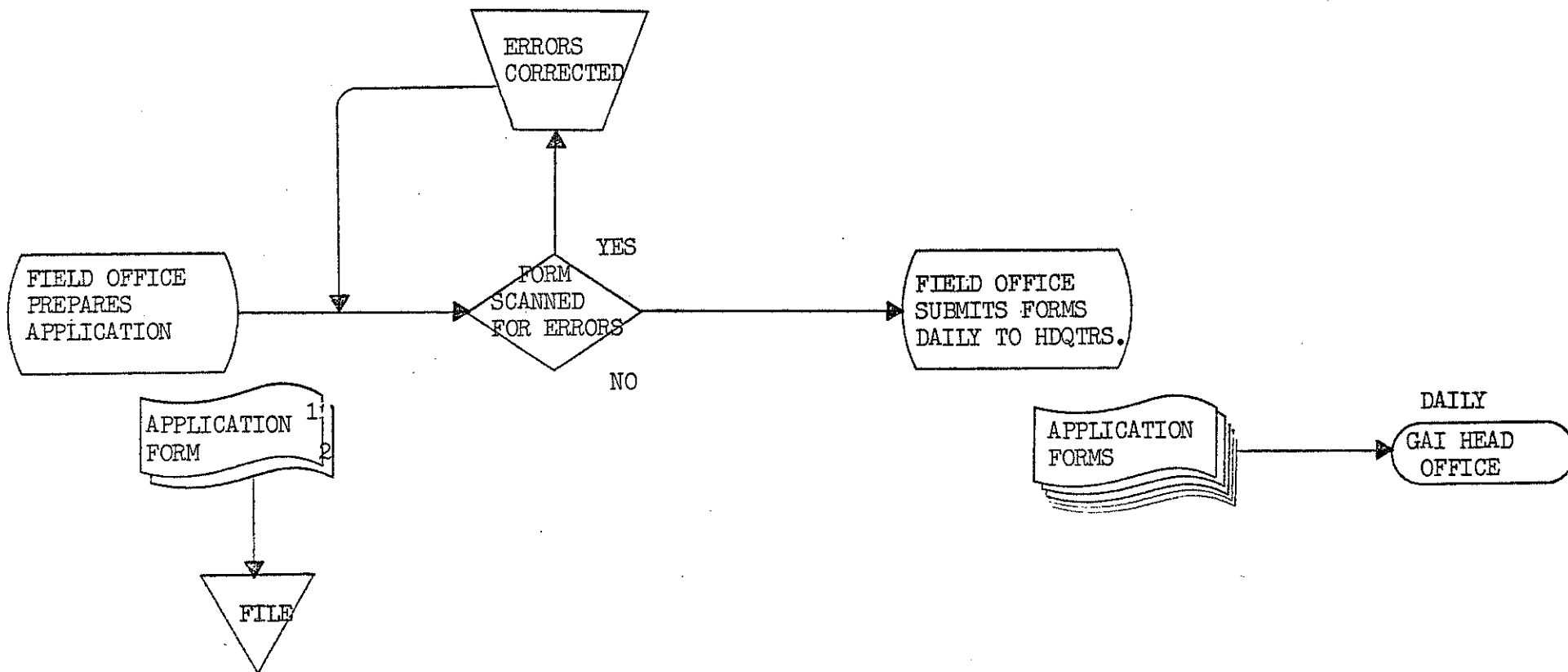
C. Payments operation

Cheques will be printed every two weeks, and mailed to recipients, with an explanation sheet of how the amount was calculated. The cheque form will allow for the computer printing of both the cheque and the explanation sheet during the same processing run. This will eliminate collating two separated printed forms and the

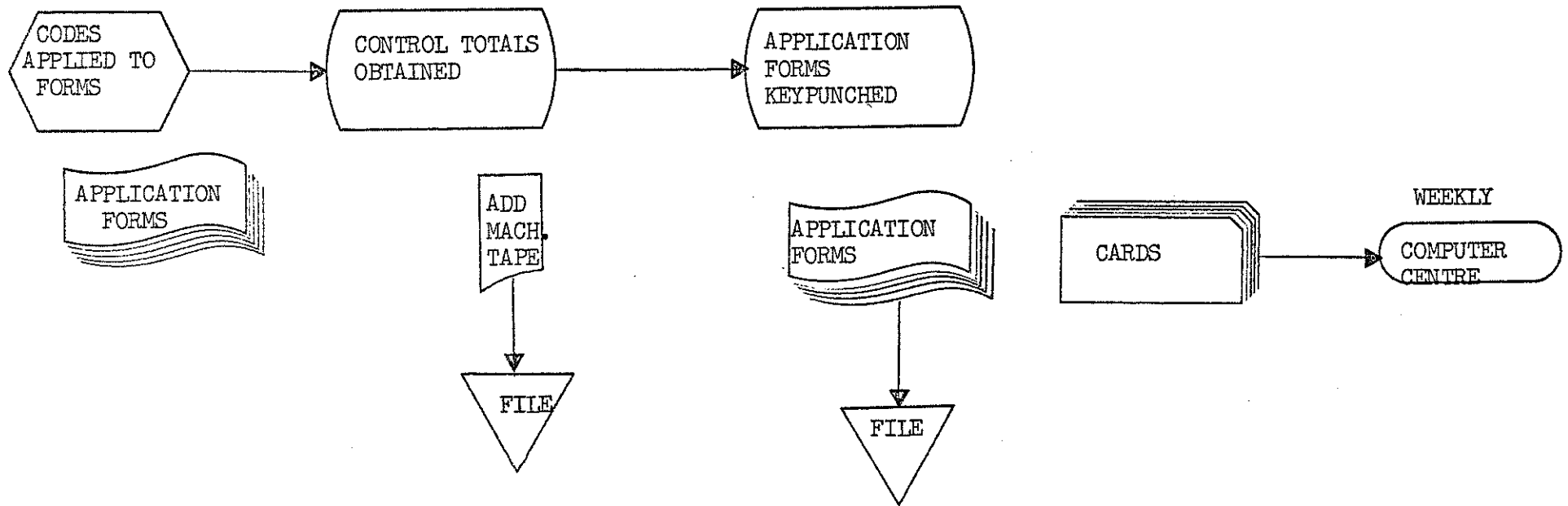
necessity of mailing the two documents (cheque/explanation sheet) in separate envelopes. We are investigating the feasibility of punched cards with the filer's family number, being generated in the same run. This card could be sent out with the monthly mailing of the IRF's. These report forms may be a computer printed sheet, produced as a separate computer run; various designs are being studied.

Finally, The Queen's Printer, Province of Manitoba, will be responsible for inserting the forms and the cheques in envelopes for mailing to recipients.

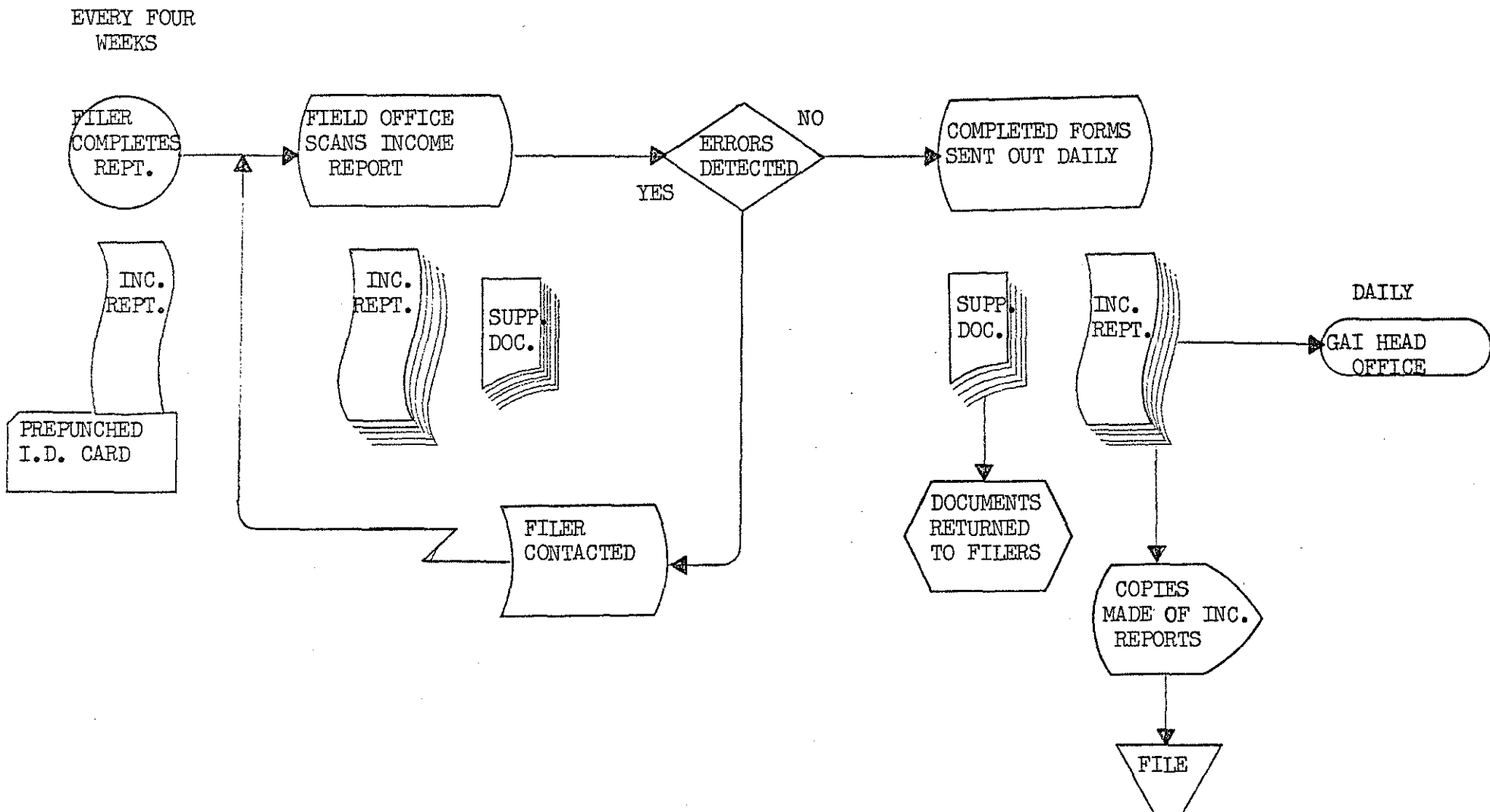
FIELD OFFICE - ENROLLMENTS



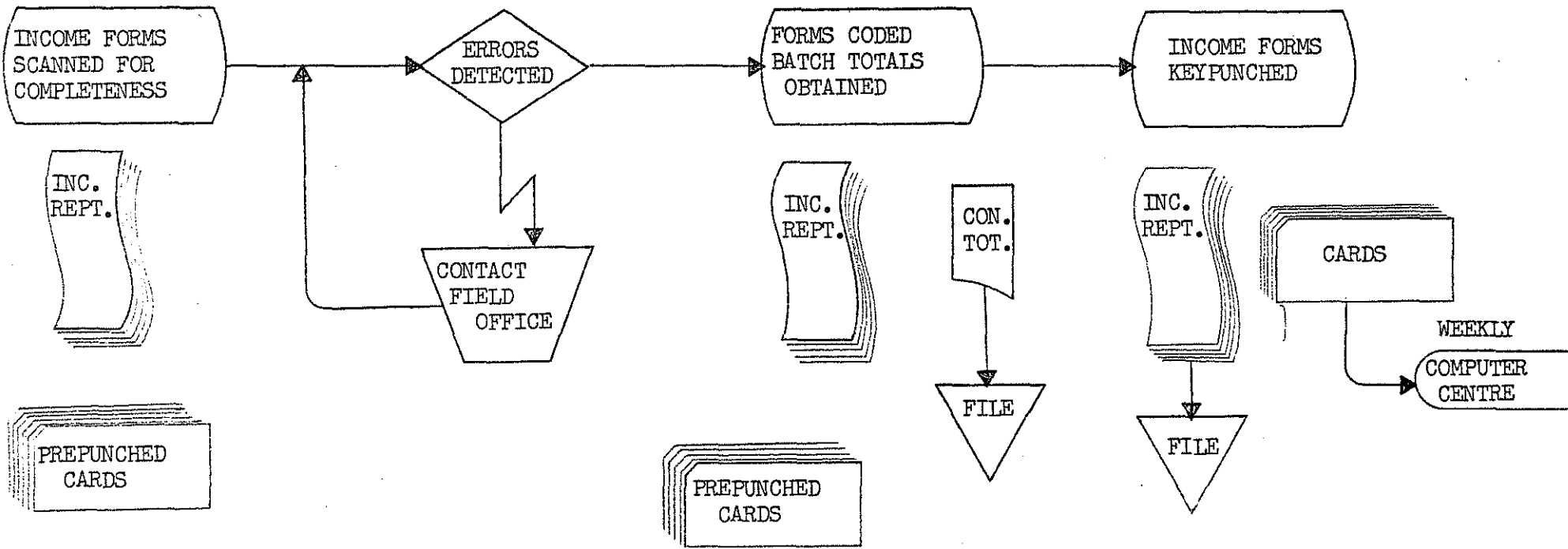
HEAD OFFICE - ENROLLMENTS



FIELD OFFICE - INCOME REPORTS

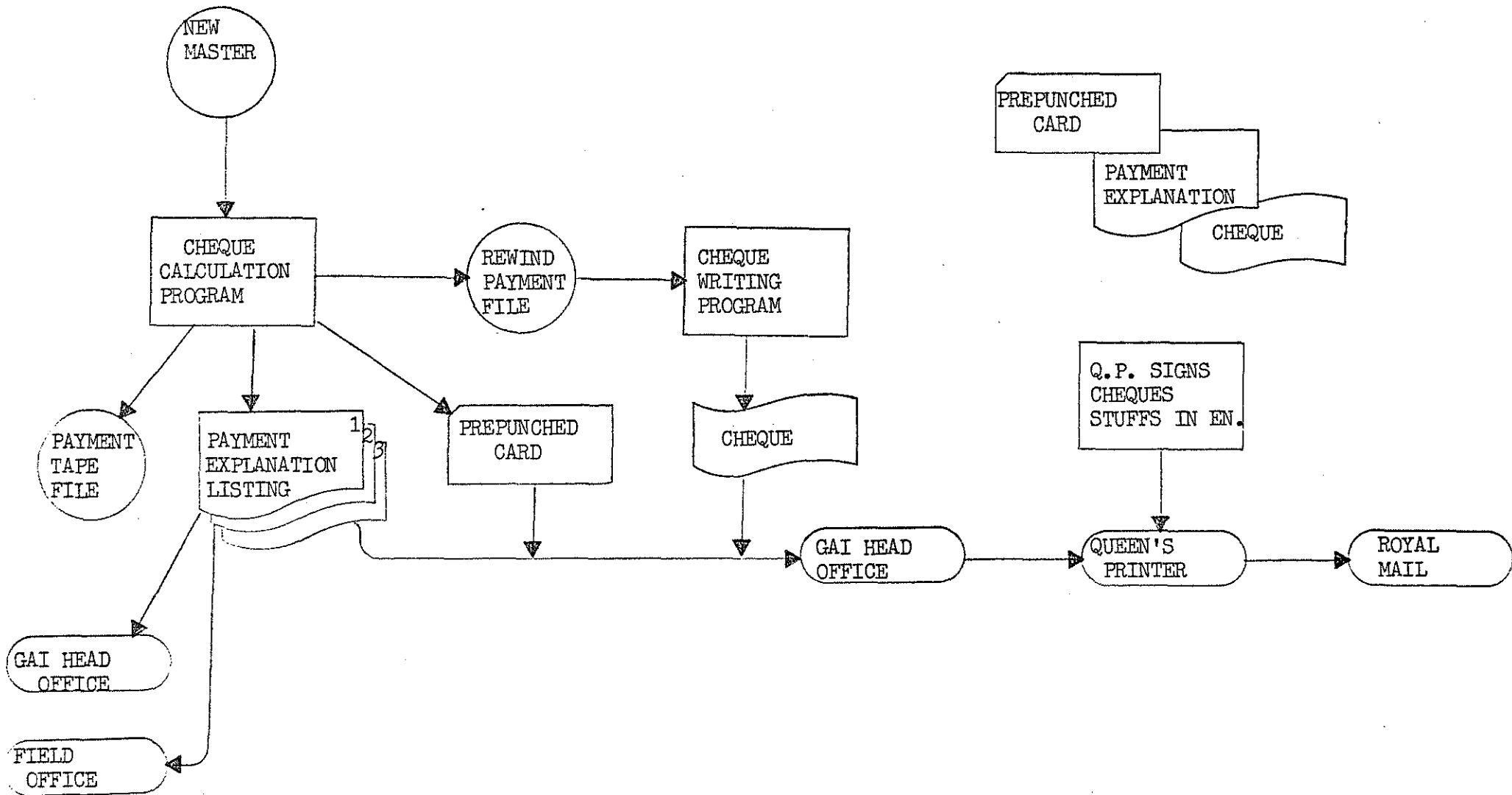


HEAD OFFICE - INCOME REPORTS



COMPUTER CENTRE - CHEQUE WRITING

EVERY TWO WEEKS



IX. COMMUNITY, ACADEMIC AND MEDIA RELATIONS

The primary objective of the public relations program is to gain and maintain the voluntary co-operation of payments and control families. However, this task is complicated by the fact that the highly visible and somewhat unorthodox nature of the project will stimulate considerable public comment. The most significant dimension of this is its possible effect on participants' behaviour. For example, the tenor of discussion and debate could conceivably affect attrition rates. In other words, we must be concerned both with the initial willingness of persons to join the Project, and with their reactions to the evaluation of the Project by others. This requires us to ensure that the initial response by the public and by professional groups, as well as subsequent assessments, are based on an accurate and adequate comprehension of the Project's nature and purpose. We must create an appreciation of the Project as a serious attempt at the scientific evaluation of the guaranteed annual income concept. This is important, inasmuch as the public acceptability of the Project and the findings will ultimately be determined by its reputation for scientific integrity. The Project, moreover, must be seen to be regarded by social scientists (and others in Canada and elsewhere who are in a position to assess the Project) as a significant undertaking.

The target populations for our public relations program are: (a) the enrolled residents of experimental sites, (b) non-participant residents of experimental sites, (c) newsmen and commentators, (d) interested members of private social agencies, academics, and government officials. We will first cover the procedure for introducing the Project to the residents of the experimental sites, then deal specifically with programs aimed at Project participants and non-participants in these sites. Following this are sections outlining programs for the news media and interested professionals.

A. Introduction of the Project

1. Saturation and Control Sites

Obviously, our first public relations task is to gain support for the conduct of the test in the preferred saturation, control and dispersed sample sites. Because the saturation and, to a lesser degree, the control sites, do involve the community and not just individual families, the gaining of community acceptance must be based upon a comprehensive process of community consultation. This process, which will not be required to the same extent in the dispersed site, is outlined below.

a. Phase One

Community contact will be initiated with a series of discussions on a very informal basis in homes rather than offices, with a list of local notables including the Mayor, other municipal officials, and business and community leaders. A Federal official should be present at all meetings to underline Canada's interest in this experiment and to participate in subsequent evaluations. These early discussions will stress that a particular community is one of several being considered for the experiment. It will be emphasized that the project is a scientific test whose outcome will strongly influence the decision about whether to proceed with a universal program.

Four points will be covered in these meetings:

- i. an explanation of the guaranteed income concept.
- ii. a brief description of the Project staff, including their training, etc. and introduction to the analysis techniques to be employed in the site. The purpose of this is to create an appreciation for the scientific nature of the Project.
- iii. an explanation of why the community has been selected for consideration as a potential site for part of the experiment, and a discussion of its possible role.
- iv. a description of how the program will affect - and benefit - the community.

Community leaders must be told that sufficient Federal and Provincial resources will be available to ensure that no community would suffer from being used for the test and, whatever the outcome, would benefit from participation. The financial impact on each community will be explored in detail.

Five important advantages to the saturation community will be emphasized:

- i. Over \$1.5 million in transfer payments will be paid out for each of three years. All of this will go to those with low incomes, and most of this money will enter the local economy and circulate.
- ii. There will be a number of part-time jobs created by the Project, especially for housewives, and students during the summers. These salaries will also enter the local economy.
- iii. The field interviewing staff will be recruited from surrounding areas. These people will be a part-time force of about 30 - 40, mostly retired school teachers, farmers' wives, municipal officials, etc., who will be eating a lot of meals, buying a lot of gas, and spending nights in local motels, etc. A large part of their salaries will go back into the community. These salaries alone could in a year amount to \$200,000. The project will also have a full-time field office staff of about 12 persons living in the community, whose salaries will further add to the local economy.
- iv. There will be a one-year phase-out period. For this we will consider the necessity of developing job-creation projects to ease any disruption of the local economy. This will constitute a large part of the guarantee that we will not damage the local economy.
- v. During its life, the test will virtually abolish the costs of municipal welfare, a considerable saving to the community and all local ratepayers.

It can be anticipated that community leaders will perceive certain drawbacks to having such an experiment conducted in their community. Such concerns will have to be dealt with satisfactorily during these initial meetings. When these discussions are completed, the results will be assessed. If, in the opinion of the Federal and Provincial officials participating, there is a high probability of sustained and widespread negative reaction to the test, then activities would shift to the contingency saturation site. [In the very unlikely event that no saturation site could be developed, contingency plans call for the creation of an expanded dispersed sample throughout the province, with near-saturation clustering of this sample in selected urban and rural areas, on the lines of the Gary, Indiana experiment.] Assuming it is successful, this first phase, which should not last more than a week to ten days, will lead to a second one.

b. Phase Two

This phase would last, say, a month, the purpose being to ensure that the general population first learns about the Project from the Project's members or Project materials.

Having come this far, we can risk a low-key public announcement that the community is "under active consideration", a fact which would in any case elude the press for only a few days at most after the start of Phase One. Larger, more formal meetings open to all those interested can then be held, with a presentation of the concept, a listing of local advantages, (see above) and the introduction of key Project staff. Suitable press releases covering the same ground would be sent out. There will be an explanatory booklet, and other materials mailed to every home in the community during this period. A major purpose of this will be to convey a sense of the scientific purpose and nature of the Project. By creating some appreciation of why survey interviewing is necessary, and by providing general explanations about why certain kinds of questions will be asked, we hope to make surveying acceptable. An advertising and a

public relations firm have already been contacted and given a general indication of the problem. They are preparing the explanatory booklet and related materials.

c. Phase Three

In each stage the number of people consulted is increased. In this third stage we carry the question directly to the people.

At this point we should have enough community support to begin the general baseline survey, still on the basis of gaining more information and studying suitability. The survey itself will be an effective mechanism for creating awareness of the scientific methodology of the Project. The survey will contain questions about people's reaction to their town being used for the test so it becomes, in effect, a chance for every person in town to have a say. If the responses to the experiment-related questions are satisfactory, the decision to proceed to full implementation can probably be safely made.

d. Phase Four

Following the successful completion of the baseline survey and a preliminary analysis of the results, a final decision about the actual saturation site can be made by Federal and Provincial officials, and then announced. At the same time, the names of seven or so local residents who have agreed to serve on a community advisory committee would be announced, along with the composition of an outside review board, including a person from the community. Also, a date would be announced for the start of applications for payment. A payments office would then be opened. It is only at this point that the extent of the publicly acknowledged commitment would make subsequent cancellation an acute embarrassment.

The operative assumption in this approach is that community consent is required if we are to proceed. When we have explained the program, residents must have a clear chance to respond, for it is better to measure the extent of any opposition while we are still in a position to gracefully go elsewhere. In any particular community we could cancel

the process at the end of any of the first three stages and explain that it was no longer one of the several sites being considered. Of course, the further along this process proceeds the more expensive cancellation becomes. But segmenting the process of extending a commitment does give us maximum possible protection.

The other general approach would be to hold a few local talks and then make an announcement "out of the blue". This is obviously unacceptable.

2. Control Site

Phases one, three and four will be repeated here, but with a smaller scale and shorter duration. There will be no need to hold general community meetings as only a small percentage of the population will be involved. Discussions here will not start until after the process in the saturation site is well along.

3. Dispersed Site

As the recommended site is Winnipeg and the sample will be approximately 1,000 families, there is no need to secure the same degree of community assent and co-operation as is necessary in the saturation and control sites. Introduction in the dispersed site will involve program liaison with the Winnipeg offices of the Community Operations Division, Department of Health and Social Development, the Welfare Department of the City of Winnipeg, and interested private bodies such as the Council of Self-Help Groups and the Community Welfare Planning Council.

B. Relations with the Residents of the Experimental Community

1. Project Administration

There are three ways in which the support of participants -- both in the payments and control sites -- is vital. The first, of course, involves their own co-operation in meeting the demands placed on them by the experiment. Second, unless we are able to educate recipients so that they comprehend the program, the validity of the experiment is seriously weakened. Third, given their numbers in the saturation

site, the enrollees as the "consumers" of the program will have a decisive impact on the general community response. Adequate comprehension is the basis for a favourable orientation by recipients, and their informed support is the sine que non of sound community relations. However, we regard program comprehension as an experimental necessity which will not be easily attained. Therefore, we are anticipating development of a continuous program of enrollee education.

In the first instance, explaining the program to recipients (aside from whatever they may have picked up from the press, etc.) will be the responsibility of the enrollment officer. Upon being signed up, each person or family (hereafter support unit) will be given a standard briefing keyed to an explanatory booklet which can be retained for future reference. Once the support unit is enrolled, it will be assigned to a specific enrollee contact officer who functions in both a payments control and program information capacity. Each contact officer, in his meeting with the support unit, will pay particular attention to the level of comprehension demonstrated, and will respond to this with direct explanations. Records will be kept of all enrollee contacts with the Project, whether by phone or in person. Feedback from contact personnel will indicate areas of common misunderstanding, and brief explanations of program operations will be regularly mailed to recipients in the same envelope in which their pay stubs or other documentation is returned. Also, each cheque will contain a summary sheet explaining how the monthly payment was arrived at. The level of comprehension thus attained will be tested in each periodic interview. The intensity and focus of subsequent informational activities can then be suitably adjusted.

2. Non Participants

Assuming a favourable community attitude to the initiation of the test, the obvious objective is to retain this attitude. Relations with those persons not in receipt of payments will be the task of two full-time community relations officers, one in the saturation site, another in the dispersed site. In both cases they will be responsible to their res-

pective field managers. Because of the small size of the sample, and the generally low visibility of the Project in the control community, it is unnecessary to make the function into a distinct position in that site.

Community relations (CR) officers will be responsible for handling public enquiries about the program. More importantly, they will be responsible for developing an active program of community education. This will include media relations (see below) as a part of this program. The CR officers will seek out invitations to speak to interested groups, and will initiate contacts with a range of local officials, etc. Contacts will be developed and maintained with those community organizations which have a particular concern with the test, the aim in all these activities being to develop general support and, equally important, to pick up feedback about the Project, with a particular interest in 'folk knowledge' and rumors pertinent to the Project. They will make regular reports to their respective field managers about these public attitudes, and where necessary recommend specific responses.

C. Media Relations

We anticipate a substantial media interest in the Project, especially in its early stages. Therefore a media relations campaign must be developed prior to the commencement of payments.

As noted above, this will be the operational responsibility of the two community relations officers. Because of his somewhat lighter community duties, the Winnipeg officer will serve as media co-ordinator with primary responsibility for arranging press conferences, tours, monitoring of the national and local press, radio and television, and for developing press releases, radio spot news reports, etc. The saturation site officer will be responsible for local media, and for the local role in plans developed by the Winnipeg office. Each officer will be expected to develop contacts with editors, reporters, columnists, public affairs program producers, etc. By centering the press functions in Winnipeg,

the CR officer will be able to work closely with Project experimentors in the development and release of data, especially that serving to refute biased reports concerning the behaviour of participants, etc. The media program will be developed in close connection with the Province of Manitoba's Communications and Information Services Branch.

D. Relations with Academics, Private Social Agencies and Governments

Responses to outside non-media requests for data on recipient responses, Project organization, etc., will be the personal responsibility of the Deputy Project Director. Generally speaking, no behavioural data will be released prior to the termination of the experiment. Any release of information will be cleared with both levels of government. However, it will be desirable from time to time for Project members to make public progress reports to academic and social science conferences, governments, etc. both to develop contacts with interested persons who might be useful in the analysis of the data, and to lay the foundation for the future evaluation of the Project by others. The Project is developing a program of reviewing research proposals made to it by interested academics who see the experiment as having relevance to work they are, or will be, conducting. We will consider requests to generate data on specific questions; the release of this data is, of course, subject to the same rules as all other data.

X. PROJECT ADMINISTRATION

A. Organizational Structure

The organization responsible for conducting the guaranteed annual income experiment in Manitoba is known as the Manitoba Minimum Annual Income Project. This name was chosen in order to avoid further public confusion, because of the similarity between the guaranteed annual income and the Guaranteed Income Supplement.

The Project Director reports in the first instance to the Honorable Edward Schreyer, chairman of the Planning and Priorities Committee of Cabinet. He reports, in all matters touching on the administration of the Project, to the Honorable Rene E. Toupin, Minister of Health and Social Development. Funds are appropriated through this Department.

The Project has a permanent head office at 238 Portage Avenue, Winnipeg. Field offices will be opened in Winnipeg, and in the saturation and control sites.

The organizational structure of the Project is presented on p. 147 . All Project personnel will be recruited on term contracts, i.e. they will be outside the Manitoba Civil Service. This is being done to allow rapid recruiting, to accommodate positions that have no precise equivalent in the existing civil service structure, and to allow greater flexibility in salaries and permissible outside activities. A listing of the positions, with their salaries for fiscal 1973 - 1974 can be found on p. 149 .

B. Present Full Time Project Staff

1. R.S. Hikel, Project Director, is on leave from the Department of Political Science, University of Winnipeg, where he is an Assistant Professor of Political Science.
2. Ted G. Harvey, Deputy Director and Survey Director, is a political sociologist on leave from the faculty of the University of Western Ontario. He is the author of numerous published papers in his field.
3. John S. Rybuck, payments officer, is a Chartered Accountant, who served for five years before coming to GAI as controller with Data Business Forms, Toronto.
4. Marius Lewin, Statistician, came to the Project from the staff of Bell Northern Research Laboratory, Ottawa. He holds masters degrees in both applied and pure mathematics from the University of Paris, and has done post-graduate study in probability theory and statistics. He is the author of four published papers.
5. Norval Campbell, Computer Analyst, was a systems analyst with the Metro Corporation of Winnipeg for six years. Before coming to the Project he was programming education co-ordinator with Phoenix Data Corporation, Winnipeg.
6. Stephen Miller, research economist, has a first class honours degree (economics) from McGill. He has done post-graduate study and has been a teaching assistant in economics at the University of Wisconsin, and is a Ph.D. candidate, Department of Economics, McGill. His dissertation is titled: "Canadian Personal Income Distribution and the Guaranteed Minimum Income".
7. Research Assistants:
 Roger Brunet, BA (Honours, Political Science), University of Winnipeg
 Nancy Moorhouse, BA (Political Science), University of Winnipeg
 Jane Sampson, Social Welfare Services Certificate, Assiniboine Community College, formerly on the staff of the Manitoba Health Services Commission.

Consultants:

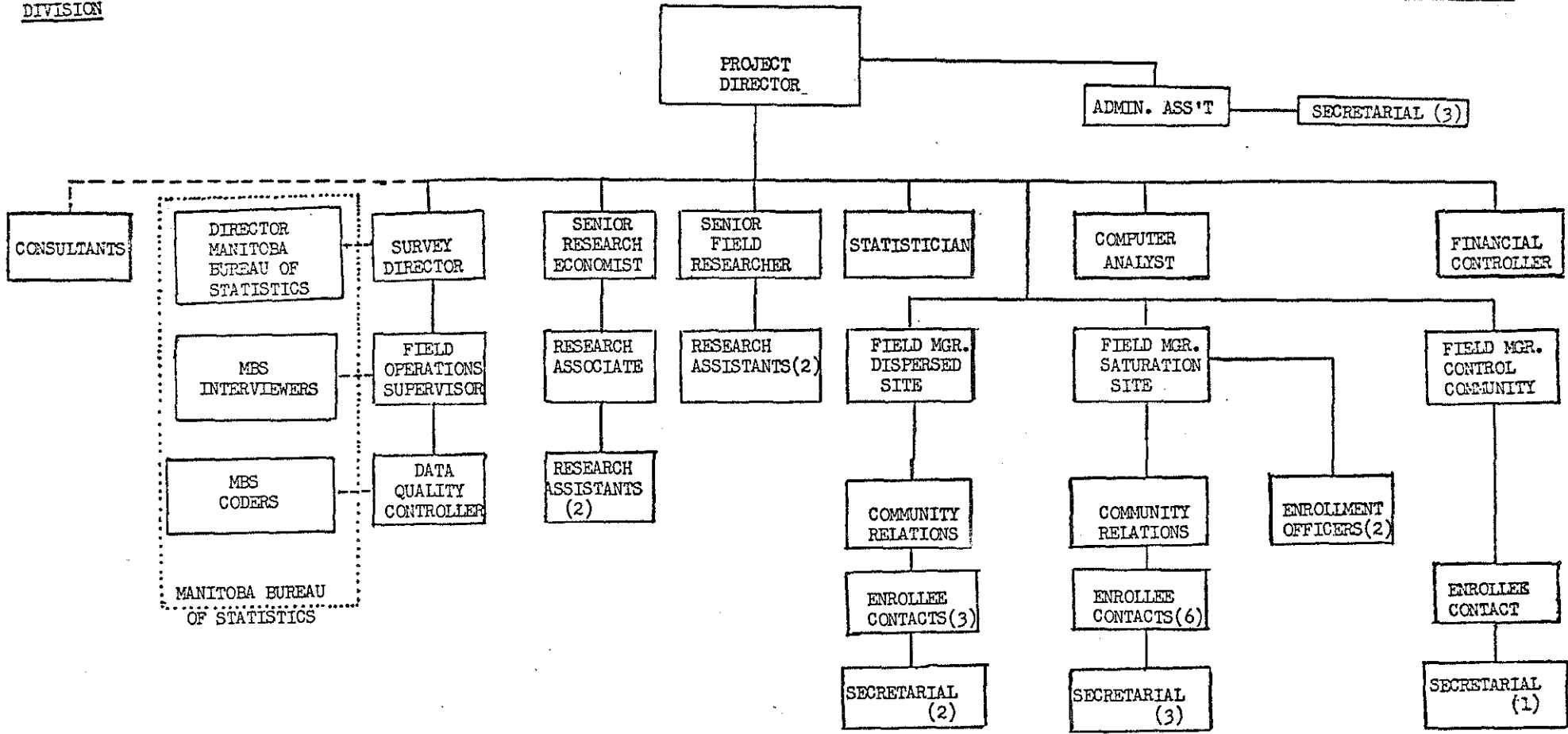
Professor Michael Laub, Department of Economics, University of Manitoba
 Professor John Hofley, Chairman, Department of Sociology, University of Winnipeg
 Professor Robert Swadinsky, Department of Economics, University of Guelph
 Professor Lee Bawden, Department of Economics, University of Wisconsin
 Professor Sharon Sutherland, University of Essex, United Kingdom, and
 The Social Science Research Council.

C. ORGANIZATIONAL CHART

 MANITOBA MINIMUM ANNUAL INCOME PROJECT
 ORGANIZATION CHART

RESEARCH
 DIVISION

ADMINISTRATIVE
 DIVISION



———— FULL TIME PERSONNEL
 - - - - PART TIME PERSONNEL

D. Program Budget1. Overview

Design costs to date, fiscal 1972-73, \$70,000; 75% cost recoverable from Canada.

(i)	1st year 1973-1974 (see attached budget for final design, all start ₁ up costs, and seven months of operations):	\$2353.
(ii)	1974-1975, (full year of operation)	4034.
(iii)	1975-1976 (full year of operation)	4277.
(iv)	1976-1977 ² (9 months of full operation)	4122.
(v)	1977-1978 ³ (phase out and reports)	2571.
	Estimated Total Cost	<u>\$17,357.</u>

2. Budget Request for 1973-1974

	<u>Amount</u>	<u>Staff</u>
Administration (See Schedule 1 for details)	\$ 520.6	42
Research and Operations (See Schedule 2 for details)	922.5	
Transfer Payments - net (See Schedule 3 for details)	910.0	
	<u>\$2353.1</u>	

Notes

1. These estimates are preliminary and are being refined by further analyses as tax, census and other data become available. Transfer payments are net of existing social assistance payments presently being made to some people who will subsequently be enrolled in the Project. Above figures do not include any amount for lost income tax revenue.
2. Reflects savings from reduction of transfer payments commencing on January 1, 1977.
3. Reflects savings because of reduction of transfer payments during calendar year 1977, and their termination on December 31, 1977, and because of the gradual phase-out of operations.

1973-74 1974-75

OTHER ADMINISTRATIVE EXPENDITURES:

Office rentals (3-4 locations)	\$ 20.0	\$ 25.0
Office Equipment	20.0	25.0
Postage	15.0	30.0
Design and printing of forms and explanatory material	50.0	35.0
Staff travel	25.0	30.0
Community relations	5.0	5.0
Office operations:		
telephone (incl. computer terminals)	25.0	30.0
supplies and furnishings	20.0	20.0
services	5.0	5.0
	<u>\$ 185.0</u>	<u>\$ 205.0</u>
 Total Administration	 <u>\$ 520.6</u>	 <u>\$ 678.1</u>

Schedule 2: Research and Operations

Consulting fees and services	25.0	25.0
Computer time, programming and systems development	50.0	50.0
Research materials	7.5	7.5
Data coding	180.0	150.0
Interviewer travel and maintenance	60.0	60.0
Survey design, development and administration (see note below)	600.0	400.0
	<u>\$ 922.5</u>	<u>\$ 692.5</u>

1973-74 1974-75

Schedule 3: Transfer Payments (gross)

Gross Payments to recipients		
Dispersed site 7 months	\$510.0	
full year		\$875.0
Saturation site 3 months	400.0	
full year		1560.0
	<u>\$910.0</u>	<u>\$2435.0</u>

Summary of Costs:

Administration - Schedule 1	520.6	678.1
Research operations - Schedule 2	922.5	692.5
Transfer payments (gross) - Schedule 3	<u>910.0</u>	<u>2435.0</u>
Total costs 1973-74	<u>\$2353.1</u>	<u>\$3805.6</u>

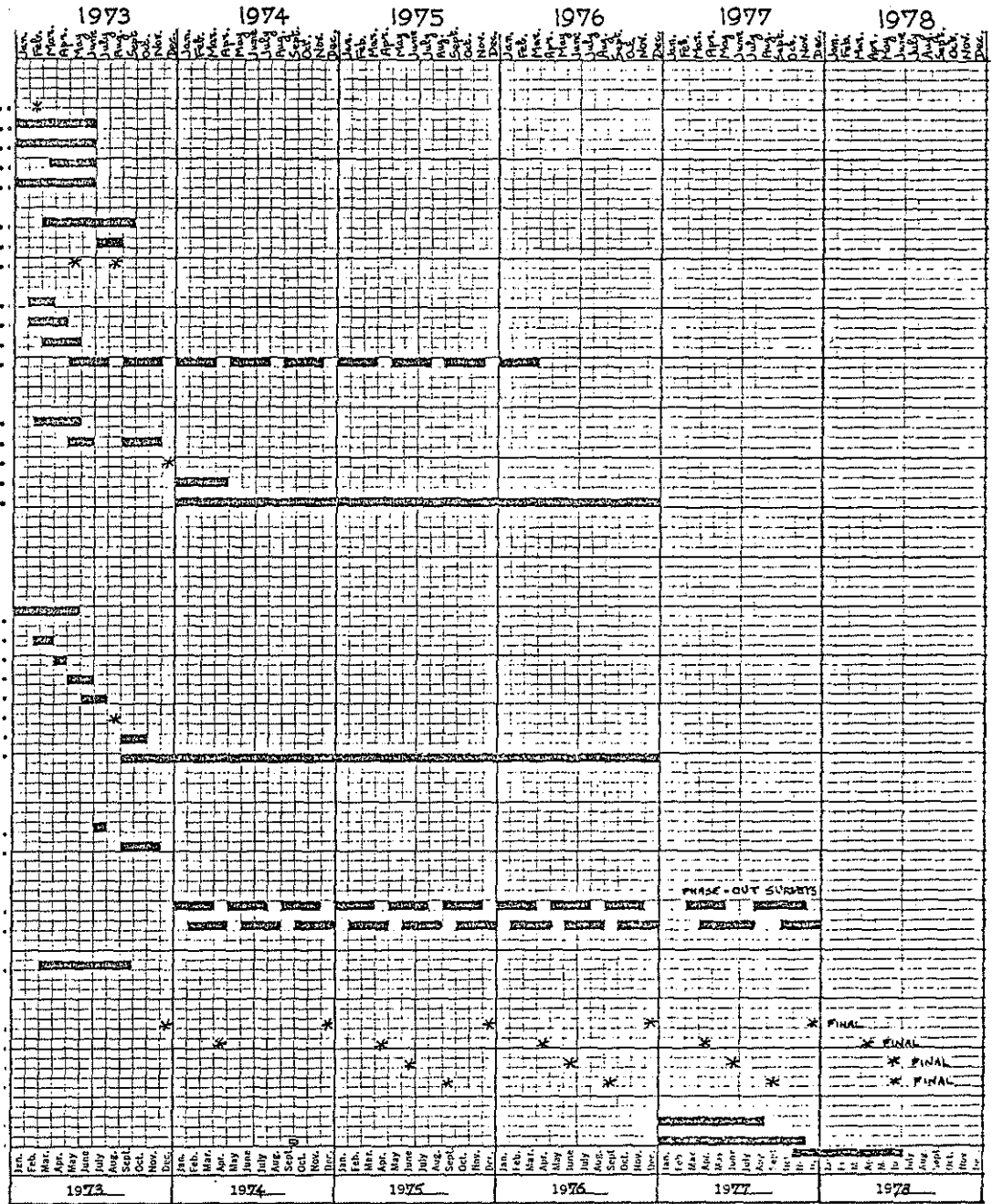
* Add: Cost increases		<u>227.7</u>
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Total costs 1974-75		<u>\$4033.3</u>
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* All costs are based on 1973 price levels. An estimate of 6% per year is being used to reflect inflation.

Figure 1. Schedule of Activities for the Manitoba G.A.I. Project.
Given in real time as represented by solid blocks

1. General Activities:
 - Submission of proposal.....
 - Staffing and organizational development.....
 - Development of accounting computer programmes.....
 - Development of payment procedures and forms.....
 - Development of rules of operation.....
 - Development of community relations programme and materials.....
 - Pilot enrollment project.....
 - Public announcement of project.....
2. Questionnaire development:
 - Design of the screening survey.....
 - Design of the baseline survey.....
 - Pretesting, questionnaire and scale development...
 - Development and pretesting of periodic interviews.
3. Implementation of the saturation module:
 - Initial community contact.....
 - Saturation baseline surveys.....
 - Opening of payments field office.....
 - Initiation of saturation enrollment.....
 - Payments.....
4. Implementation of the dispersed module:
 - Development of assignment model and cell assignment.....
 - Preliminary study of target areas.....
 - Listing of households.....
 - Dispersed screening operation.....
 - Dispersed baseline survey.....
 - Opening of payments field office.....
 - Initiation of dispersed enrollment.....
 - Payments.....
5. Implementation of the saturation control module:
 - Screening operation.....
 - Baseline survey.....
6. Periodic interviewing:
 - Implementation of periodic surveys (including phase-out surveys).....
 - Survey coding and data reduction.....
 - Development of computer programs for survey data processing and data retrieval.....
7. Reports (* indicates presentation):
 - Annual reports on operations.....
 - Reports on administrative feasibility.....
 - Reports on labour supply.....
 - Reports on secondary response variables.....
8. Phasing out operations:
 - Phasing out of payments.....
 - Phasing out of field organization.....
 - Phasing out of research organization.....
 - (data processing as well as social/economic research personnel)



APPENDIX A. SUPPORT LEVELS FOR THE MANITOBA EXPERIMENT

As was noted in the section of the proposal dealing with experimental design, three distinct support levels are utilized in the experiment. These correspond to approximately 50%, 65% and 80% of the project poverty line. (See Section III above, footnote 7). Specific support levels for families or support units size 1 to 8 are set forth in Table A.1. There, for purposes of presentation, we assume that members 3 through 8 of units size 3 to 8 are dependent children.

Table A.1 Basic Support Levels for Families with No Income

Unit Size	GAI Poverty Line	Low Support Level	Medium Support Level	High Support Level
1	\$2645	\$1332	\$1700	\$2067
2	\$4512	\$2273	\$2900	\$3527
3	\$5134	\$2586 ⁺	\$3300 ⁺	\$4013 ⁺
4	\$5757	\$2900	\$3700	\$4500
5	\$6379	\$3213	\$4100	\$4986
6	\$7002	\$3527	\$4500	\$5473
7	\$7624	\$3840	\$4900	\$5959
8	\$8246	\$4154 [*]	\$5300 [*]	\$6446 [*]

+ other adults (see Section VII.A are attributed an increment of \$770, \$1000 and \$1231 respectively for the three support levels.

* additional children entitle a support unit to additional support increments of \$313, \$400 and \$487 respectively for the three support levels.

For all individuals and for most families, support levels are readily determined. Single individuals would be entitled to guarantees of \$1332, \$1700, and \$2067 respectively at the low, medium and high support levels. Typical families of four are guaranteed

\$2900, \$3700 and \$4500 at the 3 support levels. Increments for additional children would normally be \$313, \$400 and \$487 at the three support levels designated. Rules for calculation of benefits would vary somewhat however, where only one parent was present in a family with dependent children. In such cases, the family's guarantee would be determined by family size rather than by composition per se. Break-even points for the several specific financial treatments used in the experiment are given in Table A.2.

Table A.2 Break-even Points for Three Tax Rates by Family Size

	Unit Size	Low Support Level	Medium Support Level	High Support Level
35% GAI Tax	1	\$3806	\$4857	
	2	\$6494	\$8286	
	3	\$7389	\$9429	
	4	\$8286	\$10,571	
	5	\$9180	\$11,714	
	6	\$10,077	\$12,857	
	7	\$10,971	\$14,000	
	8	\$11,869	\$15,143	
50% GAI Tax	1	\$2664	\$3400	\$4134
	2	\$4546	\$5800	\$7054
	3	\$5172	\$6600	\$8026
	4	\$5800	\$7400	\$9000
	5	\$6426	\$8200	\$9972
	6	\$7054	\$9000	\$10,946
	7	\$7680	\$9800	\$11,918
	8	\$8308	\$10,600	\$12,892
65% GAI Tax	1		\$2615	\$3180
	2		\$4462	\$5426
	3		\$5077	\$6174
	4		\$5692	\$6923
	5		\$6308	\$7670
	6		\$6923	\$8420
	7		\$7538	\$9168
	8		\$8154	\$9917

The support levels presented have been developed with careful attention to questions of real need and economies of scale

attributed to families and households of different size. Thus we have been concerned with the way in which guarantees are prorated across family size¹ as well as with the extent to which our guarantees dominate welfare. In the first case, we have conducted extensive examinations of welfare rates across Canada and of support rates used in American income maintenance experiments. Examination of rates in those cases has led us to the formulation of the rates presented above. In our view these support rates vary in such a way that need should be handled in a reasonably equitable manner, while incentive to split or enlarge family units is minimally affected. Regarding domination of welfare, preliminary analyses of welfare populations allow us to conclude that our modal support levels provide excellent domination over welfare (See Table A.3.)

Table A.3 Welfare Domination by Support Level.²
The percentage domination indicates the proportion of current welfare populations which would receive GAI benefits equivalent to or higher than previous welfare payments.

Unit Size	Low Support Level	Medium Support Level	High Support Level
1	46.1%	77.2%	96.2%
2	51.8%	95.7%	100.0%
3	33.6%	92.1%	100.0%
4	26.3%	88.9%	100.0%
5	19.2%	77.3%	100.0%
6	18.0%	76.5%	100.0%
7	17.7%	76.4%	100.0%
8	12.2%	56.0%	92.5%

1. A Family Size Index (FSI) has been used to determine proration of support levels across family size in a standardized manner. The product of the FSI and the basic guarantee for a family of size four is the support level for that particular size for that support level. Values of the FSI by unit size are: size 1 = .4594; size 2 = .7837; size 3 = .8918; size 4 = 1.0; size 5 = 1.1081; size 6 = 1.2162; size 7 = 1.3243; size 8 = 1.4324.
2. Percentage domination of Provincial social assistance cases in pay, at March 31, 1972, excluding recipients of institutional care, those in aged and infirmed care homes, homeowners, and payments for care of foster children.

Determination of support levels has been closely connected to the problem of "splitting" in families. "Splitting" is what happens where one or more members of a family break off from that family or where a single family breaks up into two or more families. Significantly, GAI support levels may have a noteworthy impact on splitting, should those support levels create a financial incentive for splitting. Such an incentive is created whenever a family or support unit might receive a larger benefit after splitting up. The imputation of any economy of scale to larger families requires (almost by definition) the setting of guarantees which may increase subjective predisposition to split. One example of this would be a system which allowed a guarantee of \$2000 for a single person, but only \$3000 for a husband and wife. Under such a system, husbands and wives would be encouraged to split, because they could increase their total income to \$4000.

We have attempted to minimize incentives to split through manipulation of support levels.² In the case of husbands and wives, we have attempted to manipulate guarantees so that the return from a split will be small relative to the cost of maintaining separate households, etc. Thus, in the case of our modal support level, a husband and wife gain only \$500 in total income by separating: their guarantee is \$3400 if they are apart and \$2900 if they are together. In the case of other adults in households, we have similarly attempted to minimize the incentive to split. This is done by introducing a separate support level for "other adults" into our calculation of family support levels. These support levels for other adults are calculated on the basis of increments of \$770, \$1000, and \$1231 for the three support levels. Thus the N'th adult in a family at the modal support level is worth a support increment of at least \$1000 to the family, rather than a child's allotment of \$400. To take an

² This tactic stems in part from an expectation that administrative manipulation — through rules of operation, definition of the family eligibility, etc. — will be generally ineffective in controlling splitting. See W.A. Klein, "Problems in Choosing Family Unit Rules for a Negative Income Tax" mimeo University of Wisconsin, Madison, January, 1970.

example of a husband and wife with the parents of either living with them, such a family would be entitled (at the modal support level) to a guarantee of \$4900. This guarantee would come to \$5800 if the family split in two, but use of the "other adult" benefit minimizes the incentive to split considerably. Where such a family might gain \$900 under the plan set forth above, it could gain \$2100 by splitting if no allowance were made for higher benefits for "other adults".

Needless to say, we cannot be certain what effects the structure of guarantees set forth here will have on family structure until the experiment is well under way. Nonetheless, the researchers are confident that the structure of guarantees set forth here minimizes considerably the possible effects of guarantees on family splitting, while at the same time accounting for variations in real needs across family size.

APPENDIX B. A DESIGN FOR AN ADMINISTRATIVE EXPERIMENT FOCUSED ON ENROLLMENT PROCEDURES

Mode of enrollment is an intriguing program parameter affecting the nature of a guaranteed annual income experiment and also affecting in very basic ways the nature of a guaranteed annual income program. This is naturally so, since enrollment procedures may influence the distribution of utilization, de facto eligibility, and the efficiency of guaranteed annual income as an income redistribution mechanism.

1. Mode of Enrollment

Enrollment can be taken as a static program parameter and it can be assumed that the ideal procedure is one similar to that generally utilized in the American negative income tax experiments.¹ Following that example, (1) a population of interest is specified; (2) a suitable sample representative of that population is located; and (3) individuals are enrolled or recruited into the program. Essentially, this model of enrollment requires intensive program-initiated enrollment of participants. The enrollment procedure to be utilized with the dispersed module will closely correspond to that procedure used in the American experiments (see Section III.B.6). Whether the same procedure can be or should be applied to the saturation module is not so clear. From an overall perspective, continuity of rules of operation and mode of relation to participants should obtain in both modules of the experiment. This is advantageous from the point of view of experimental design (where experimental operations and analyses are to be integrated in a number of ways across sites). But this perspective need not preclude certain adjustments to the particular research/operational requirements or opportunities presented by a saturation study.

¹See, for example: David Kershaw, "Administrative Issues in Income Maintenance Experimentation", in R. Orr, et al., (eds.) Income Maintenance, pp. 281-283.

The key question regarding enrollment for the saturation module is: "To what extent shall enrollment be initiated by the potential recipient and to what extent shall enrollment be initiated by the operators of the program themselves?"² On the one hand enrollment may depend on the initiative of the potential recipient only (or nearly so). Such enrollment is in this sense open and undirected. On the other hand, enrollment may be targeted on a certain eligible population. These will be referred to as the "open" and "targeted" enrollment models respectively.

Each of these models seems to have certain advantages and disadvantages. It is clear for example that the open model is likely to result in somewhat greater absolute underutilization.³ It can be further hypothesized that the open model will maximize the likelihood of misutilization of the program and inefficiency in the redistribution of income. This is because political-social sophistication itself is an inequitably distributed resource. Thus, extraction of benefits will be a spontaneously-conceived and realizable goal for those who are likely to be least in need of them, i.e., students, the self-employed and the clever generally, while those most in need of supplementary income — the old and the marginally intelligent — are likely to be least able to "work" the system. Under an open enrollment system adequacy of income redistribution power (measured, for example, as the ratio of dollars-transferred-to-those-in-greatest-need to dollars-transferred-to-those-least in need)⁴ is bound to suffer. A number of other advantages supposedly attributable to each mode of operation could undoubtedly be catalogued. Rather than pursuing a

²This question is independent of the question of "Voluntary Participation" as outlined in the Federal Guidelines (p. 4). All participation covered in the experiments (and referred to in this discussion) must, of course, be voluntary in the sense that participants agree of their own accord to be enrolled in the program.

³It remains, of course, an empirical question as to how great a difference is likely to obtain between underutilization resulting from open enrollment and underutilization resulting from targeted enrollment.

⁴This indicator is presented for illustrative purposes only. In the enrollment experiment it is probable that a number of alternative indices of redistributive power will be examined.

speculative inventory however,⁵ a strategy will be outlined which attempts to (a) maximize the relevance of the enrollment procedures used in the saturation experiment to alternative policy choices; and (b) results in minimal discontinuity between the procedures of the saturation and dispersed modules of the experiment. (The prime consideration here is thought to be the simultaneous attainment of "full" and "normal" enrollment in the dispersed and saturation modules respectively.)

The following discussion outlines a strategy which allows optimal satisfaction of both goals.

2. An Enrollment Experiment

If the goal is to determine various effects of open as opposed to targeted enrollment, two strategies are possible. First, open and targeted enrollment may be staged sequentially. This involves a period of open enrollment of some set length, followed by a period of targeted enrollment also of a fixed length. Second, open

⁵Beyond those points noted in the discussion above, differential effects of enrollment mode may plausibly be anticipated in several areas. It may be hypothesized that a program involving an open enrollment system would be more likely to generate a heightened attachment of stigma to the individuals receiving benefits, simply because the act of self-initiated enrollment is likely to be viewed as more indicative of personal inadequacy or need. An important, and related question has to do with interactions between mode of enrollment and subsequent responses (such as labour supply responses) to other key program parameters such as the support level and tax rate. Overall impact of the program is bound to be heightened under a "targeted" enrollment system — which heightened impact could have both good and bad effects as was suggested above for the case of trade-off between redistributive efficiency and overall cost of the program. Regarding administrative efficiency, it is not immediately clear which mode of enrollment should be most satisfactory — mainly because there are several competing criteria of efficiency. Fiscally, legislation of a universal guaranteed annual income is perhaps more likely to emerge as an extension of general tax policy than through other avenues (see: Courchene, "The Poverty Reports, Negative Income Taxation, and the Constitution: An Analysis and Compromise Proposal", mimeo, University of Western Ontario, 1972). The near-universality of a tax policy mode of enactment clearly makes the mode of enrollment for the experiment a serious matter in design of the saturation experiment.

enrollment and a slightly modified version of targeted enrollment⁶ may be run simultaneously.⁷ This strategy involves a period of open enrollment which runs concurrently with a period of targeted enrollment. For best appraisal of results in the experiment as a whole, for control over history, and for control over self-selection, it is preferable that the second alternative be adopted and that this be done by building mode of enrollment into the experiment as an explicit experimental treatment.⁸ This sub-experiment would be a major administrative experiment of the project. This is important inasmuch as investigating administrative feasibility is an implicit goal of the project as a whole.

While at first blush it might be assumed that such an experiment could not be conducted unobtrusively in the saturation site, this assumption would be erroneous. To illustrate, one need only consider the more active enrollment procedure inherent in the purely targeted model. In that targeted model, enrollment takes a fixed time period determined by the researchers. For illustrative purposes, let us assume that enrollment is phased in over a 4-month period. A purely targeted enrollment operation would not be satisfactory because some families in need would not be enrolled until the fourth

⁶Certain questions regarding the impact of history and program-related expectations (in the sequential model) and contamination effects in the concurrent model of the experiment are problematic. In the first case controls over history and self-selection are absent so that estimation of these effects is not possible. In the second case partial control over contamination may be attained through selective interviewing of enrollees regarding perception of the program, how they learned about it, etc. These problems are of a similar magnitude, and essentially unavoidable in a single community experiment.

⁷As noted elsewhere in the proposal (Section III.C.6) the targeted enrollment treatment encompasses a voluntary enrollment option, since totally targeted enrollment is viewed as operationally unacceptable.

⁸Such an experiment does not, however, require any modification of the overall experimental design.

month. Therefore, we would, of necessity, allow for recipient-initiated enrollment in any case, so that persons in need could, in principle, come into the program voluntarily any time (within limits imposed by the capacity of the field office to process applicants) after Day-1 of operations. Mode of enrollment can be treated as an experimental variable unobtrusively because of three conditions: (a) a long time period is required for either mode of initial enrollment: members of the community can only be enrolled a few at a time; (b) members of the community can only have imperfect information as to why they are enrolled early in the program as opposed to later and vice versa; (c) allowing open enrollment concurrent with targeted enrollment means that targeted enrollment per se causes no hardship for any family -- a family which has not been contacted can, if it decides, initiate contact with the field office itself.

The enrollment experiment has the following basic characteristics:

- (a) The eligible population is randomly assigned to Treatment E_0 (open enrollment) and E_T (targeted enrollment).
- (b) During the initial period of the enrollment experiment, group E_0 receives no direct contact from the field office. Group E_T is systematically contacted during this period,⁹ although the total number of families enrolled in this manner will be E_T minus the number in E_T voluntarily enrolling and the number refusing E_T . At the conclusion of the administrative experiment the proportion of E_0 which has enrolled will be only that proportion which desired to do so on their own with no more stimulation than that provided by general public relations cum media contact. The proportion of E_T enrolled at this time is E_T minus those not desiring to enroll in the program when contacted. It is important to note that the fact that members of E_T may have enrolled either voluntarily

⁹As noted in section II.C.6, operational limitations will probably require the targeted enrollment to begin after an initial period of open enrollment of 4 to 6 weeks. This delay is viewed as necessary to prevent overload in the saturation site administrative office which might result if

or when contacted by an enrollment representative does not confound the basic integrity of the treatment as long as a reasonable portion of those people in E_T (say 40% or more) are enrolled under the targeted mode.

- (c) A residue remains at the conclusion of the administrative experiment. This residual group is that sub-set of E_0 which has not volunteered or opted into the program. This residual group can either be left dangling or it can be enrolled in a "mop-up" targeted enrollment phase.¹⁰

3. Discussion

Questions regarding power of the experiment hinge in part on the strength and purity of the treatments -- and in part on capacity to answer relevant research questions regarding distribution of utilization, level of utilization/underutilization, and direct behavioural effects.

First, the treatments represent clearly distinguishable alternatives for policy makers designing a universal GAI. Both models are of interest for obvious reasons. Open enrollment implies voluntary utilization, a greater degree of underutilization, and greater administrative economy. Targeted enrollment implies greater redistributive efficiency. It seems likely that a completely targeted enrollment system (one not allowing for any voluntary enrollment) is a non-viable policy alternative. The two treatments -- completely open enrollment, and targeted enrollment allowing for voluntary selection into the program -- therefore encompass the major alternative enrollment plans in a manner quite isomorphic with the possibilities likely to be allowed by the real world.

The major questions regarding the effects of open enrollment deal respectively with patterns of utilization and levels of utilization. In the first case the following questions concern us: Who are the people who come forward naturally, on their own initiative to be enrolled? Who are the people who do not come forward naturally? To

¹⁰Since voluntary enrollment across E_0 and centrally initiated recruitment for E_T should account for the bulk of the eligible population, it should not be absolutely necessary to push that residual group into a phase of targeted enrollment. From an experimental point of view, in fact, it is clearly advantageous to leave E_0 alone.

what extent can relevant groups not stimulated to enroll by normal public education activities be reached by more specialized modes of communication? What are the income-redistribution implications of a completely voluntary enrollment system? These questions can all be answered in the context of the experiment defined above. In particular, the characteristics of enrollers versus non-enrollers can be determined on the basis of correlational analysis of enrollment response (treated as a dummy variable) and data previously collected from the proportion of the population assigned to E_0 . Although explicit causal inference may be unwarranted, such analysis should provide useful insights into the factors motivating of facilitating voluntary enrollment. Further, it should be clear that the redistributational effectiveness of the open enrollment model can be assessed in a number of ways from data collected for E_0 . Redistributational effects of E_0 may be assessed in an absolute sense, for example, in terms of proportion of all very needy family units enrolled or the ratio dollars transferred to those most in need / dollars transferred to those least in need. Alternatively, comparisons may be made with effects of E_T .

The key questions regarding utilization/underutilization are: "How great will utilization be with open enrollment?" "How great will utilization be under targeted enrollment?" "How significant will the differences in levels of underutilization between the two enrollment plans be?" "Will patterns of underutilization be variable in such a way that equivalent underutilization in each plan could result in a different redistributational impact?" Questions regarding level of utilization/underutilization are generally simpler than those regarding distribution of utilization, but methodological problems confound appraisal of the results to a greater extent. Mainly this is because questions regarding utilization/underutilization are pinned very readily to exact figures rather than to a broadly based set of relations such as characterize the proposed research regarding distribution of utilization. Useful answers can nonetheless be arrived at, so long as we assume that the attempt to stimulate enrollment under the open system

involves a public education campaign roughly correspondent in intensity with that likely to be mounted in the case of a national GAI program. Assuming enrollment levels stabilize for E_0 during the course of the overall experiment (a reasonable assumption), it should be possible to predict the point at which utilization would level off, i.e. we would be predicting the proportion of the population likely to be enrolled in a universal program operating with open enrollment. Similar predications could be made for E_T assuming (again it seems reasonable) that some portion of E_T would not wish to be enrolled in the program.

Finally, behavioural responses may be affected by mode of enrollment. Variations in such responses may be detected by interrelating data regarding mode of enrollment with later data collected vis a vis labour supply, family behaviour, self development, etc. It might, for example, be hypothesized: that open enrollment results in greater long-run acceptability of an income maintenance program; or that targeted enrollment results in a lessening of stigma attached to the program. Such effects as these, and potential effects on labour supply, may turn out to be of considerable importance to policy makers.

Regardless of particular outcomes, it seems certain that an experiment with enrollment procedures expands considerably the administrative and policy relevance of the basic GAI experiment.

Two problems arise here. First, the GAI recipients with incomes between the exemption level (E) and break-even (B) face very high marginal tax rates ($t+k$ would yield rates of over 75% in Manitoba with $t = .50$ and over 80% with $t = .55$). The second problem revolves around the experimental need to control the effective tax rate.

(b) Tax integration where income tax is rebated to GAI recipients

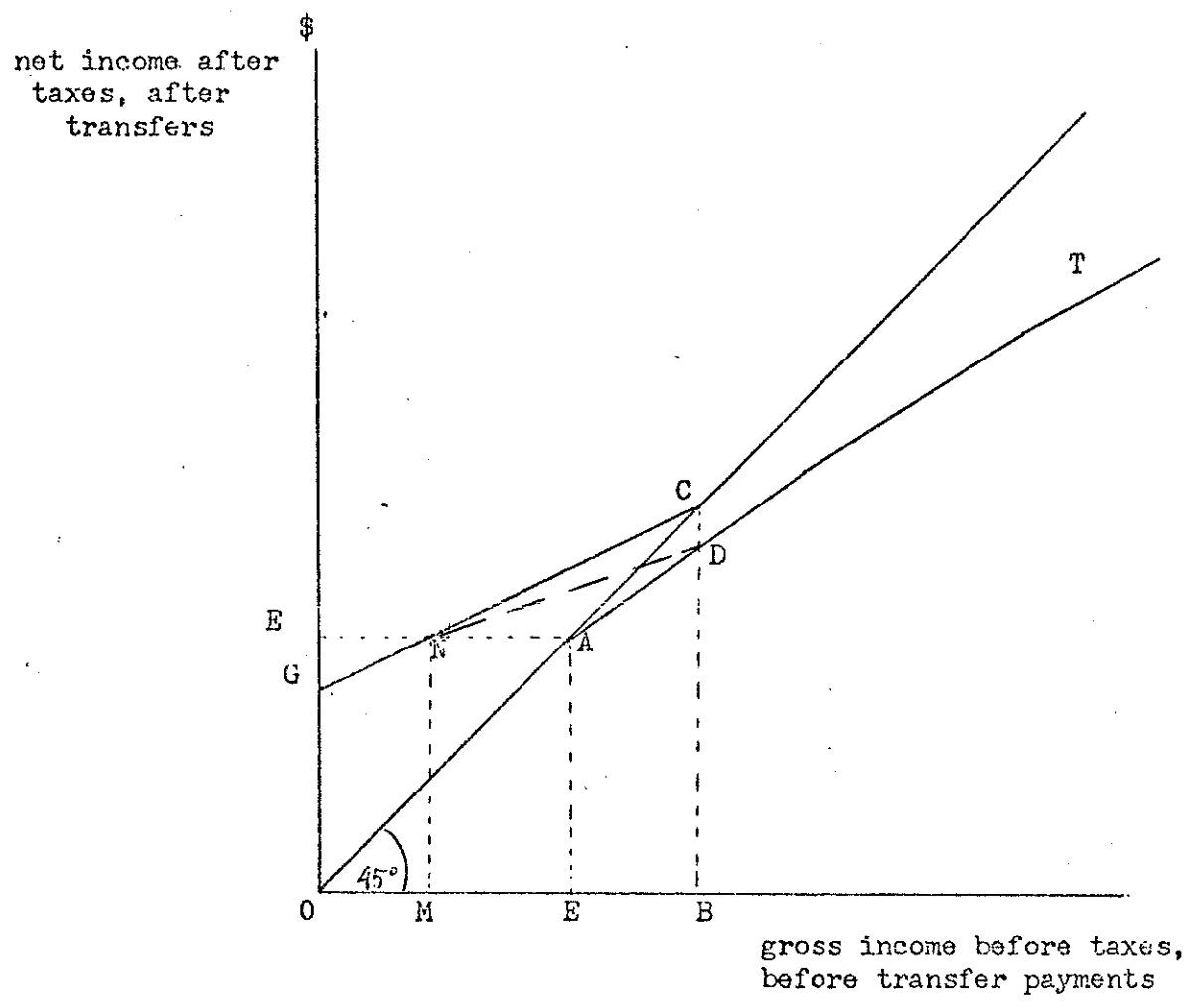
The high marginal tax rates incurred when the two programs are simply added together can be avoided by rebating income tax payments to GAI recipients. If no other adjustments are made, the net income-gross income relationship is represented by GCDT. This case embodies a "notch" (CD) at the break-even level of income (B) such that a family has a significantly higher net income for a gross income of $B - 1$ than for a gross income of $B + 1$. The obvious inequity involved here could have a serious disincentive effect.

To avoid this "notch effect", the two programs could be phased together by applying an integration tax rate (t' which must be greater than k^2) to all income above B until a point of tax equivalence (Y_j) was reached. Y_j is the level of income at which the tax liability under the positive income tax system equals the tax liability incurred using t' applied to income in excess of B. This approach is depicted by net income - gross income schedule GCFT in Figure C₁. Note that this approach involves a partial income tax rebate to families above break-even with the rebate declining from 100% at an income of B to zero at an income of Y_j . The marginal tax rates generated by this integration scheme are:

	<u>income</u>	<u>line segment</u>	<u>marginal tax rate</u>
(i)	0 to B	GC	t
(ii)	B to Y_j	CF	t' (which may be=t)
(iii)	over Y_j	FT	k

² The GAI tax rate t could simply be extended above B.

Figure C.2.
Integration of GAI and Positive Income Tax
with GAI Taxable



effective tax rate = (1-slope)

- G = guarantee level
- B = break-even level of income
- E = income tax exemptions

