Mincome Manitoba

Manitoba Basic Annual Income Experiment

The Objectives and Design of the Manitoba Basic Annual Income Experiment

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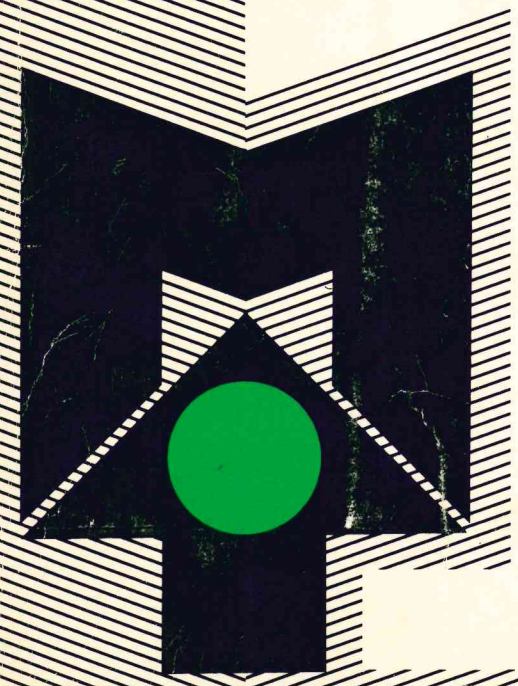

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FOREWORD

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

ACKNOWLEDGEMENT

The research on the overall design and specification of the experimental program parameters for the Manitoba Basic Annual Income Experiment represents the distilled efforts of many participants and advisors. A most prominent and substantial intellectual debt is owed to the many designers of the income maintenance experiments in the United States whose work often served as the starting point and prototype for the Canadian experience. It will be quite evident that the Manitoba Basic Annual Income Experiment has borrowed much from the pioneering efforts of predecessor experiments. The authors also wish to acknowledge the direct contribution of many others to this effort, principally Ted Harvey, Ron Hikel, David Horner, Réal Lavergne, Chuck Metcalf and Steve Miller. As well, many suggestions and contributions were made by individual members of the Experimental Research Unit, Department of Health and Welfare, Ottawa, under the direction of Brian J. Powell, and research staff members of Mincome Manitoba, directed successively by Michael Laub (1973-75) and Derek Hum (1975-79).

The opinions expressed herein are those of the authors and should not be construed as representing the opinions or policies of the Province of Manitoba, Canada, or any agency of either government. This report was drafted by Derek Hum, drawing freely from additional material and information provided by Michael Laub and Brian Powell. Responsibilities for any misleading interpretations and errors of fact lie with Derek Hum. Throughout the writing of this paper, encouragement and helpful comments were given by Professor Mordecai Kurz, Stanford University, California, and Dr. D. Lee Bawden, Program Director, Human Resources and Income Security, the Urban Institute, Washington.

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THE OBJECTIVES AND DESIGN OF THE MANITOBA BASIC ANNUAL INCOME EXPERIMENT

I. Introduction

The Manitoba Basic Annual Income Experiment is a research project jointly funded by Canada and Manitoba. It is the first large scale social experiment ever undertaken in Canada and is designed to evaluate the economic and social consequences of an alternative social welfare system based on the concept of a negative income tax (NIT). More specifically, the focus of the research design is on the study of labour supply responses of households and individuals to a guaranteed annual income.

There is increasing concern in Canada about the state of the overall social security system -- in terms of policy objectives and delivery systems. Of special concern has been a negative income tax proposal, in particular the behavioural incentives associated with such a structure. The work supply effects of income maintenance has provided an important focal point for much research effort and debate. However many investigations remain constrained by inadequate data as well as problems associated with establishing and measuring causal relations by means of non-experimental data.

An experimental approach has distinct advantages over conventional research techniques, especially when passive observations of non-experimental events are either judged inadequate or cannot take place. Social experiments involving explicit intervention can be used to generate behavioural response data for the analysis and assessment of specific policy alternatives without prejudging which particular configuration of program features is optimal. By affecting the range of variation of key program

parameters, causal inferences can be obtained directly, efficiently and more convincingly. Social experiments, however, are not without their limitations and special difficulties.

The design of the Manitoba Basic Annual Income Experiment involves selecting participants randomly from a number of sites and assigning them either to one of several negative income tax treatment programs or, alternatively, a control group. The participants assigned to the several treatment programs are then given monthly income-conditioned payments for a period of three years. The control group is not entitled to a guaranteed annual income but will provide valuable information for comparison purposes. Both treatment and control participants are interviewed three times a year to obtain detailed information on their work response as well as other important data.

The notion of experimental design is very broad. Viewed most generally, the design of an experiment would encompass such components as problem definition; development, planning and execution of an intervention; selection and assignment of experimental units to treatments; measurement of responses; and analysis of results and evaluation. The scope of the present paper is somewhat more limited. It is confined for the most part to defining the treatment parameters -- in particular, describing the choice of levels and factors to be experimentally varied. The research objectives of the design as well as a brief outline of the theory relating the responses to treatments are also indicated. Additionally, other important features of the design not experimentally varied but governing the conduct of participants are discussed and

explained. Finally, certain features relevant to the present experimental design are noted.[1]

II. Objectives of the Experimental Research Design

The Manitoba Basic Annual Income Experiment is concerned with the experimental evaluation of the economic and social consequences of the establishment of a NIT program. Such a program, if introduced on a permanent and universal basis, would have a wide range of effects on various aspects of society. Thus, any experiment concerned with the effects of a NIT program must be prepared for a broad range of outcomes having direct and indirect economic and social cost implications. At the same time, however, the research objectives must be sharply defined and priorities established in order to ensure that available research resources are not dissipated on peripheral rather than central issues.

The primary research focus of the experiment is on the labour supply response of families and individuals to various guaranteed annual income programs. Labour supply is considered to include, for example, such topics as the labour force participation decision of primary and secondary earners; the number of hours worked; the job search process; including time and resources devoted to job search; job satisfaction

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[1] The selection and assignment of experimental units to treatments is set forth in detail in D. Hum, M. Laub, C. Metcalf and D. Sabourin, "The Sample Design and Assignment Model of the Manitoba Basic Annual Income Experiment", Technical Report No. 2, Mincome Manitoba. A complete description of the payments system is the subject of a separate report, "The Design of the Payments System of Mincome Manitoba" (in preparation).

and attitudes towards work; and issues pertaining to investment in human capital.

Labour supply response is the primary concern because it has two types of cost implications if any permanent NIT program were established. First, changes in labour supply will result in changes in family income and this in turn will affect the cost of the transfer payments made under the NIT program. Systematic changes in the labour supply of a large segment of the population, if they occur, will significantly affect the total labour supply available and could result in a decline in total national income. Also, substantial changes in the supply of particular types of labour could cause severe adjustment problems for industries, sectors, or regions that are dependent upon that type of labour. The regional problem is of particular concern to Canadians.

In addition to the labour supply questions there are a number of secondary research objectives relating to the behavioural response of families and individuals to a NIT program. Some of these include: (a) behavioural responses of families, especially family formation and disintegration and nuclear family interactions; (b) behavioural responses of youths, to ascertain whether or not their response is different from the general population in order to detect possible long-term effects of a NIT program; (c) community involvement and participation, including co-operative behaviour and community-related values and attitudes; and (d) geographic mobility, whether or not a guaranteed income will affect the decision to change residential location.

There are also a number of limited objectives in the area of

program administration. These include assessments of: (a) the accuracy of the information supplied to the program by the participants; (b) the participants' comprehension of the administrative structures; and (c) the cost, responsiveness and equity of the experimental delivery system.

Any proposed delivery mechanism designed to effect income transfers can be evaluated with respect to its feasibility on the basis of its cost, responsiveness and equity. Central to any discussion of the administrative feasibility of a NIT program is its delivery cost. Although one cannot extrapolate cost patterns based upon an experimental experience to universal programs, individual components may offer guidance on such issues as the extent of misreporting detected, the amount of recipient contacts, etc. Also essential to any discussion of the delivery system are the issues of responsiveness and equity. Indications of the frequency with which participants enter or leave the program due to income changes, the magnitude and timing of payments to be made, and the stability or otherwise of the recipient population can address the question of the program's administrative responsiveness to changes in current income. Answers to such questions will bear on problems inherent in the choice of accounting systems and to the residual need for an emergency welfare arrangement.

A program which attempts to be responsive to current income needs must base payments upon the very recent income experience of participants. On the other hand, the accounting period must be reasonably longer; for example, the period over which a guaranteed annual income

entitlement is set is one year. Accordingly, the actual payments over the period must be reconciled with the entitlement based upon total income received within the accounting period. This reconciliation process addresses the issue of horizontal equity among participants since it ensures that families with the same total annual income receive the identical amount of payments without regard to the time pattern of receipts. Additionally, reconciliation procedures can recover overpayments, serve as audit procedures, etc., to ensure that equity is effected. For these and other reasons, the many administrative issues constitute important secondary objectives of the experiment as well as being of policy interest in their own right.

III. Description of the Negative Income Tax Payments Program[1]

The Manitoba Basic Annual Income Experiment incorporates a general guaranteed annual income payments program based upon the negative income tax approach. Among the many considerations influencing the design of the delivery mechanism is the issue of program realism; that is, to what degree does the experimental payments system approximate what would realistically obtain should a permanent national program be instituted? At the same time, certain features of program design are dictated for purely experimental reasons. Accordingly, in some instances the payments system parallels the manner in which a permanent national program might be operated; in other instances procedures are adopted for primarily research considerations.

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[1] Full details of the Design and Administrative Procedures of the Payments System are available in separate documents.

A general negative income tax program may be characterized as a universal, objective, income-conditioned transfer mechanism aimed at minimizing the work disincentives of the able-bodied poor. The program is universal in that it is not limited to specific recipient groups; it is objective since the benefit entitlement is not susceptive to discretionary adjustment; it is income-conditioned since the major eligibility criterion is the income of the recipient, and finally, it seeks to encourage labour force participation by reducing the benefit entitlement by less than the full amount of any earned wages, hence the individual is always left with a higher net income from working than not working. These salient features are incorporated in the design of the payments system.

The negative income tax (NIT) payments system of the Manitoba Basic Annual Income Experiment may be described in terms of two basic parameters; an annual basic support level and an offset tax rate. The annual support level (G) depends upon both family size and composition and represents the payment (guaranteed annual income) the family would receive if it had no other source of income or wealth. As the family's income and/or net worth increases, the payment for which they are eligible declines. The rate at which increments in earned income are offset against payments is determined by a constant offset tax rate (t). The rate at which increments in net worth or wealth are offset against payments is referred to as the net worth tax rate (r). The simplest form of this program for a typical family unit may be algebraically represented by:

(3.1) $P = G - t \cdot Y - r \cdot W$

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

The relationship of income and benefits of the NIT program for a representative family are illustrated in Figure III.1. In the upper panel of Figure III.1 the vertical and horizontal axes represent amounts of income including and excluding, respectively, the NIT payments. The intercept G on the vertical axis represents the amount of payments received by a family with no income and zero net worth. The amount of the NIT payment (P) at any level of income measured on the horizontal axis is indicated by the vertical distance between GA and the 45° line.[1] Under a NIT system with a constant offset tax rate, families will continue to receive positive payments up to the point at which their income exclusive of the NIT is equal to the breakeven level (B). The breakeven level, the support level, and the offset tax rate are algebraically related: $B = G/t$, so that specifying any two of these parameters determines the value of the third.[2]

[See Figure III.1]

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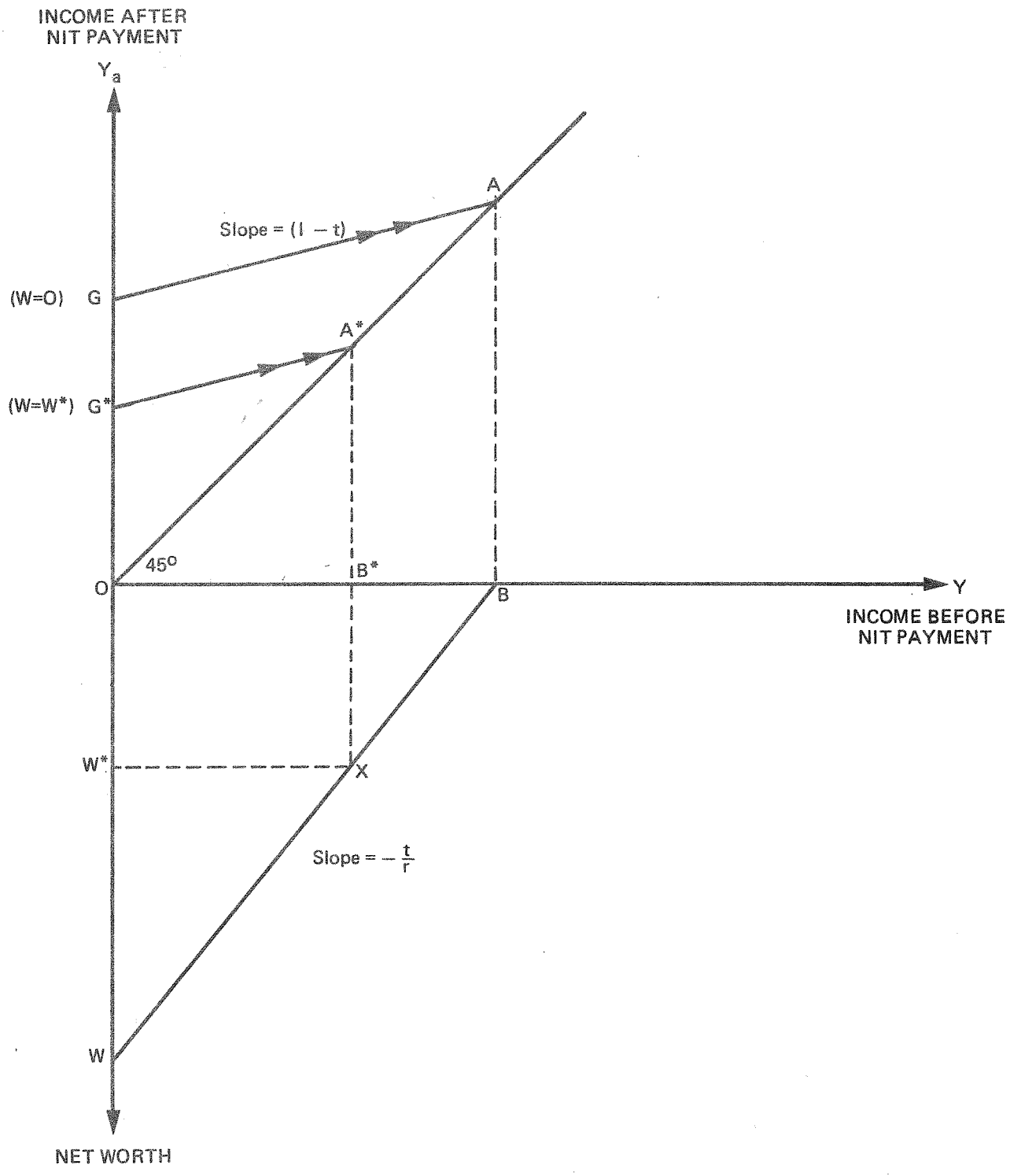
[1] This is for the case where either $r = 0$ or $W = 0$. As the level of net worth rises, the line segment GA will shift downwards retaining the same slope. Hence G*A* represents the post-NIT income locus under the same program for a family having a higher level of net worth ($W = W^*$).

[2] Again, this assumes that either r or net worth equals zero. When net worth is a factor, then $B = (G - r \cdot W)/t$. It should be noted that we confine our attention to the case of a fixed offset tax rate. A variable tax rate is also possible in which case the breakeven level is that income at which the support is exhausted.

FIGURE III.1



PAYMENTS AND ELIGIBILITY UNDER A SIMPLE NIT SCHEME
WITH NET WORTH ASSESSMENT

The lower panel of Figure III.1 depicts the combinations of net worth and income levels that set the boundaries for eligibility under the NIT program. Net worth is measured on the vertical axis. Any family having an income level equal to or greater than B, or a combination of net worth and income equal to or greater than amounts represented by WB, is ineligible for NIT payments. Thus, for a family having net worth of W*, their breakeven level of income would be B*. The amount of the NIT payment they would receive at any level of income below B* is represented by the vertical distance between G*A* and the 45° line in the upper panel. The slope of WB indicates the rate at which income can be substituted for net worth while holding payments constant (in this case at P = 0).[1]

A guaranteed annual income program implemented through a negative income tax scheme cannot be viewed in isolation of the positive income tax structure.

The problem of integrating the negative income tax (NIT) and positive income tax (PIT) programs arises whenever the breakeven level of income in the NIT exceeds the level of exemptions built into the positive income tax system. This situation is depicted in Figure III.2. The horizontal axis measures gross income before taxes or NIT payments. The vertical axis represents income after positive tax liabilities and

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[1] Totally differentiating the payments function: $P = G - t \cdot Y - r \cdot W$

we have $dP = \frac{\partial P}{\partial G} \cdot dG - \frac{\partial P}{\partial Y} \cdot dY - \frac{\partial P}{\partial W} \cdot dW$.

Along WB both $dP = 0$ and $dG = 0$, and since $\frac{\partial P}{\partial Y} = t$ and $\frac{\partial P}{\partial W} = r$, the slope of WB $= -\frac{t}{r}$.

negative income tax payments. The point E represents the level of exemptions granted by the PIT system to a representative family; the positive income tax structure has a constant marginal income tax rate, p.[1] As before, G is the annual support level, B is the breakeven level of income, and t is the offset tax rate. The after-tax locus of the PIT structure is OCFDT, and the income tax payable at any given level of income is represented by the vertical distance between the 45° line and the after-tax locus at that point. If no special integration mechanisms were implemented, the two systems would simply be added together with the results that families with incomes between E and B simultaneously receive NIT payments and pay positive income taxes, assuming that E and B do not coincide.

[See Figure III.2]

The method of integration of the PIT and NIT systems adopted by the Manitoba Basic Annual Income Experiment involves rebating all other tax payments in order to maintain experimental control over the tax rate faced by participants in the NIT program.[2]

To avoid the "notch effect"[3] that would result when the family's income rose above B (and hence suddenly faced positive income taxes again), the tax rebate is gradually phased out at levels of income in excess of B.

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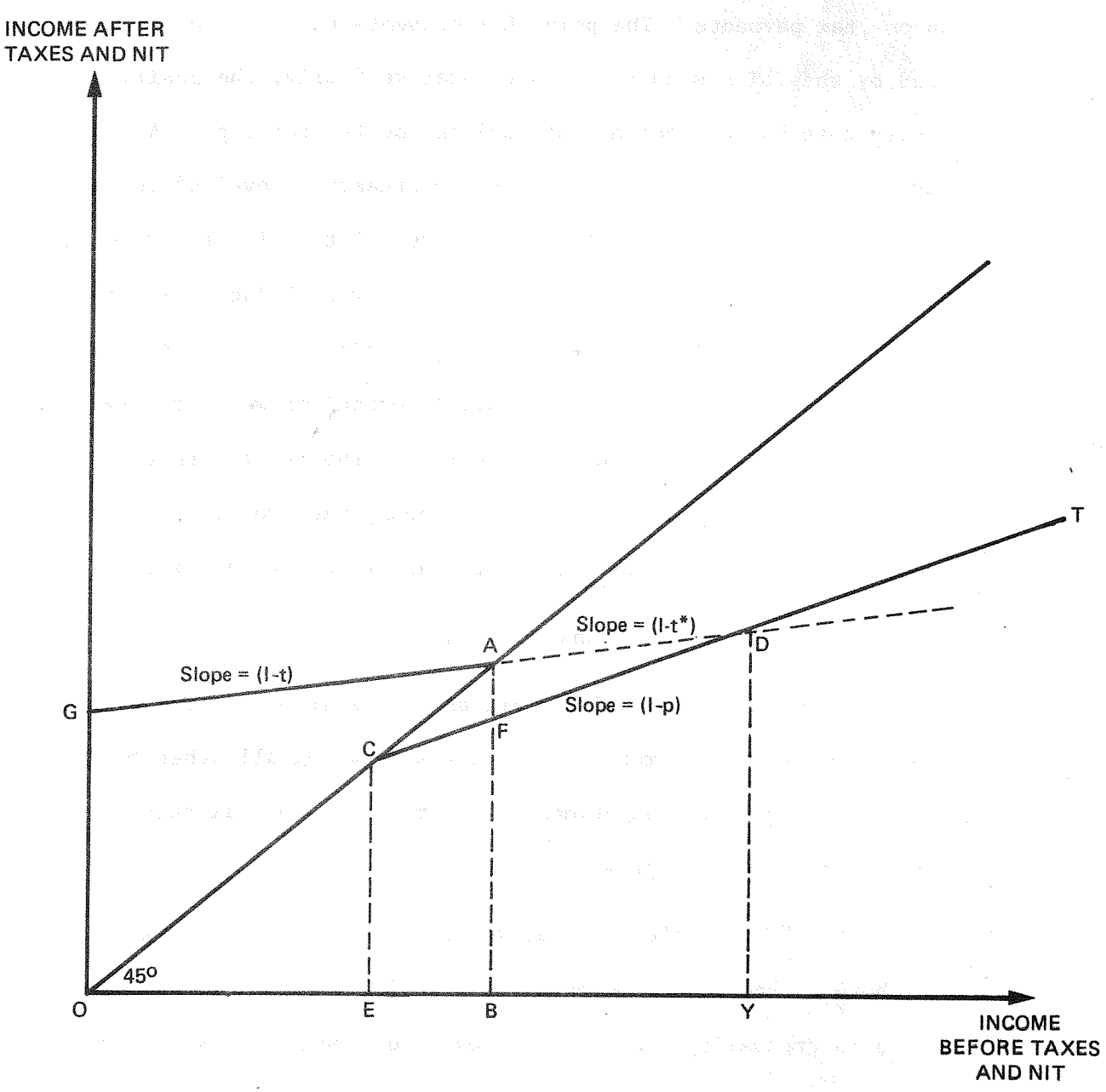
[1] For simplicity, the progressive nature of the positive income tax structure is ignored since the results of the analysis are not affected.

[2] See Section V.7 on "Positive Tax Rebates" for further discussion.

[3] If the rebate is given only to families receiving NIT payments, the post-NIT, post-tax income locus would be GAFDT. In such a case, as the family's income rose past B they would lose not only their NIT payments but also their income tax rebates, resulting in a drop in their net income of AF at B. This sudden drop is referred to as the "notch effect".

FIGURE III.2



INTEGRATION OF NIT AND POSITIVE INCOME TAX SYSTEM

For any level of income between B and Y the rebate is represented by the vertical distance between AD and FD. Thus, the amount of the rebate falls from 100% at B (=AF) to zero at Y (the point of tax equivalence). The new post-NIT, post-tax income locus is GADT. The size of the income range over which partial rebates are paid is determined by the difference between the positive tax rate (p) and the NIT integration rate (t*) applied to income above B. As t* rises relative to p, the distance between Y and B decreases.

IV. The Effect of NIT on Work Response: A Brief Statement of the Theory

The research focus of the experimental design is heavily influenced by theoretical considerations relating the effect of income maintenance to the labour-leisure choice. While models of the labour supply decision process can differ in many details, a definitive survey of the literature is beyond the scope of the present paper. Nonetheless, a representative model of work response to income maintenance may be illustrated as a standard application of static consumer choice theory to the problem of labour supply.

The rational individual is assumed to maximize a well-behaved utility function characterizing his preferences subject to a budget constraint. The basic resource of the individual is the total amount of available time, T, which may be either allocated to work for pay or non-market activities. Time allocated to work, H, provides earnings which may be used to purchase a "composite commodity", X, having price, p.

Aggregated non-market uses of time is called leisure, L, with price, w, the opportunity cost of leisure measured in wage units. Assuming the consumer-worker has unearned income of Y, the budget constraint is

$$pX = w(T - L) + Y = wH + Y \tag{4.1}$$

where $H \equiv T - L$ is hours worked. Faced with an exogeneously determined set of values for p, w, and Y, the individual chooses values of L and X so as to maximize his preference function: $U = U(X,L)$. Formally, the problem may be written:

$$\begin{aligned} &\text{Maximize} \quad U = U(X,L) \\ &\text{Subject to } pX - w(T - L) - Y = 0 . \end{aligned}$$

Introducing a Lagrangean multiplier, λ, we may maximize

$$U(X,L) + \lambda[pX - w(T - L) - Y]. \tag{4.2}$$

The necessary conditions are:

$$\begin{aligned} &\frac{\partial U}{\partial X} - \lambda p = 0 \\ &\frac{\partial U}{\partial L} - \lambda w = 0 \\ &pX - w(T - L) - Y = 0 . \end{aligned} \tag{4.3}$$

The first-order conditions for work-leisure equilibrium may be viewed as a set of three equations in the three unknowns, L, X, and λ, where λ is interpreted as the marginal utility of income. For given values of p, w, and Y, the unknowns may be solved as functions of p, w, and Y. In particular, we have

(4.4) $L = L(w,p,Y)$

as the demand function for leisure. Since $T - L \equiv H$, the equations which define the demand function for leisure may also be viewed as determining the supply function for labour

(4.5) $H = H(w,p,Y)$.

Consider now the effect of income maintenance on the supply of work effort. Availability of the NIT program changes the effective budget constraint to

(4.6) $pX = G + (1 - t)(wH + Y)$

where, as before, G is the support level or basic guarantee amount for period T, and t is the offset tax rate of the program. The above budget constraint applies only to families with $wH + Y < G/t = B$; that is, to families with income levels below the breakeven point. Because $(wH + Y)$ and $G + (1 - t)(wH + Y)$ are respectively the amounts of income available before and after the NIT subsidy, it is easy to see that the NIT payment is $G - t(wH + Y)$. Clearly, (4.6) is formally equivalent to (4.1) except that Y is replaced by $G + (1 - t)Y$ and w is replaced by $w(1 - t)$. Accordingly, the change in work effort consequent upon introduction of a NIT program is[1]

(4.7) $\Delta H = H[w(1 - t), p, G + (1 - t)Y] - H(w,p,Y)$.

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[1] The notation ΔH is used to denote the difference in equilibrium work effort after and before the introduction of the NIT program. For simplicity, the tax rate on earnings before the NIT is assumed to be zero. Alternatively t may be interpreted as the change in tax rate resulting from the introduction of the NIT program.

The assumptions of the standard theory of consumer choice applied to the problem of labour supply also allows the decomposition of the effect of a change in the wage rate on hours of work, $\partial H/\partial w$, into two components; namely, a substitution effect, S, and an income effect $(\partial H/\partial Y)\cdot H$, such that

$$(4.8) \qquad \partial H/\partial w = S + H\cdot(\partial H/\partial Y) \quad .$$

Differentiating totally (4.5) and substituting (4.8) we may write

$$(4.9) \qquad dH = S\cdot dw + \partial H/\partial Y \; [Hdw + dY]$$

where we assume that $dp = 0$. Consumer theory derives the result, $S > 0$. We also assume that leisure is a normal good, $\partial H/\partial Y < 0$; and that p, S, and $\partial H/\partial Y$ are constant over the range of variation in Y and w considered. Under these conditions, the impact of the NIT program is given by: $dw = -tw$ and $dY = G - tY$. Consequently, we have

$$(4.10) \qquad dH = -twS + (\partial H/\partial Y)[G - t(wH + Y)] \quad .$$

The above result is the theoretical effect of income maintenance on work response. With leisure not being an inferior good, a negative income tax is seen to reduce the work effort of those individuals having incomes less than the breakeven level not previously participating in such a program.[1]

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[1] The wage rate before and after introduction of the NIT are w and $(1 - t)w$ respectively. Hence the impact of the NIT program on the wage rate is to change the wage rate (dw) by $(1 - t)w$ minus w, hence $dw = -tw$. A similar argument holds for $dY = G - tY$. In (4.10) $w > 0$, $S > 0$, hence the first term, $-twS$, is negative. Since $\partial H/\partial Y$ is negative and $G - t(wH + Y)$ is positive for families below breakeven, the second term of (4.10) is negative and hence the entire expression (4.10) is negative.

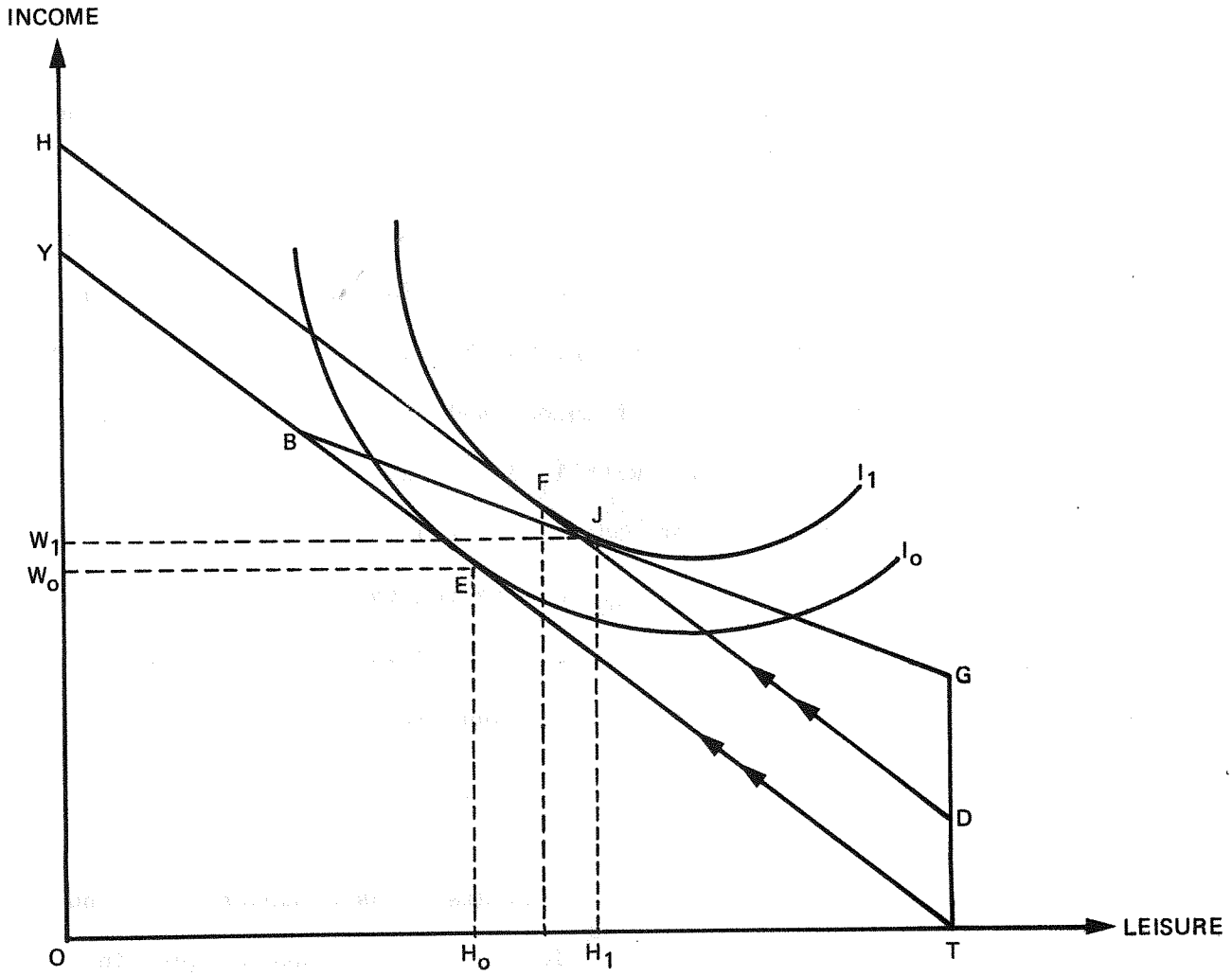
The significant and crucial problem for public policy concerns the magnitude of the work response for alternative values of the support level and offset tax rate since combinations of G and t determine the overall program coverage, as well as the amount of work disincentive.

A diagrammatic presentation of the effect of income maintenance on the supply of work effort is presented in Figure IV.1. The vertical axis measures income per unit time period T, the horizontal axis measures hours of leisure up to the maximum time available, T. The difference between T and the number of hours of leisure, L, is the number of hours worked, H. Equally satisfactory combinations of income and leisure are represented by indifference curves I_0 and I_1, with I_1 depicting a higher level of satisfaction than I_0. The budget constraint is depicted by YT, the slope of which represents the constant wage rate facing the individual. In the absence of income maintenance the consumer-worker is in equilibrium at E on indifference curve I_0, working H_0 hours and earning W_0.

[See Figure IV.1]

The negative income tax program provides a basic support of G and an offset tax rate represented by the slope of BG. The new budget line is now GBY. The consumer-worker equilibrium is now at J which is on a higher indifference curve I_1, with fewer hours worked, H_1, and a higher level of total income, W_1, composed of both wage earnings and income maintenance payments. Consequently, the effect of income maintenance is seen to reduce hours of work and earned income but to increase satisfaction and total income.

FIGURE IV.1



WORK RESPONSE UNDER A CONSTANT NEGATIVE INCOME TAX PROGRAM

Decomposition of the reduction in hours worked may be illustrated by drawing HD parallel to YT. The total reduction in hours worked is H_0H_1. The horizontal distance EF represents the income effect since the wage rate is identical at both points. The horizontal distance FJ is the substitution effect since the level of satisfaction is the same at both points. Finally, it should be noted that the income effect refers to the combined effects of the support level and the offset tax rate on the level of satisfaction, and not that of the guarantee alone.

The above illustrates how alternative values of the support level and offset tax rate can be significant in determining the level of payments a family will receive as well as influencing the extent of the work response. These two parameters -- the support level and the offset rate -- are crucial for policy purposes since combinations of G and t selected will largely determine labour supply effort and overall program costs. Because of their theoretical importance, the guarantee level and the offset tax rate are the only program parameters that have been selected as experimental variables. Other basic parameters are maintained constant over all financial treatments. Non-experimental parameters are chosen so as to ensure they lie within the range of policy relevance both individually and in combination.

V. Specification of the NIT Program Parameters

V.1 Introduction

The selection of the NIT program parameters is dictated by the primary objective of the experiment; namely, to be able to predict response (especially labour supply) to a range of NIT programs whose characteristics differ in terms of support levels and offset tax rates. Thus the selection of program parameters is not made solely on the basis of their direct policy relevance, but also on the basis of their ability to contribute to our capacity to predict response to the relevant range of programs.

V.2 Support Levels

Support levels specify the level of payment provided for a family with no other source of income or wealth. The Manitoba Basic Annual Income Experiment employed three different support levels to facilitate the detection of possible non-linearities in response. The range covered by these three levels had to be sufficiently large to permit identification of the income effect. In addition, the upper support level was limited by budget constraints, while the lower levels could not be too much below that of existing transfer programs because participation in the experiment is voluntary, and the program offered by the experiment must be a relatively attractive alternative. Accordingly, the following considerations guided the choice of support levels:

a. the range of support levels should cover policy relevant levels;

b. the range of support levels should be broad enough to permit separate measurement of the income effect;

c. the support levels should take into account cost considerations as well as existing transfer and tax programs; in particular, the lowest support level should yield an acceptable degree of domination over the existing welfare programs.

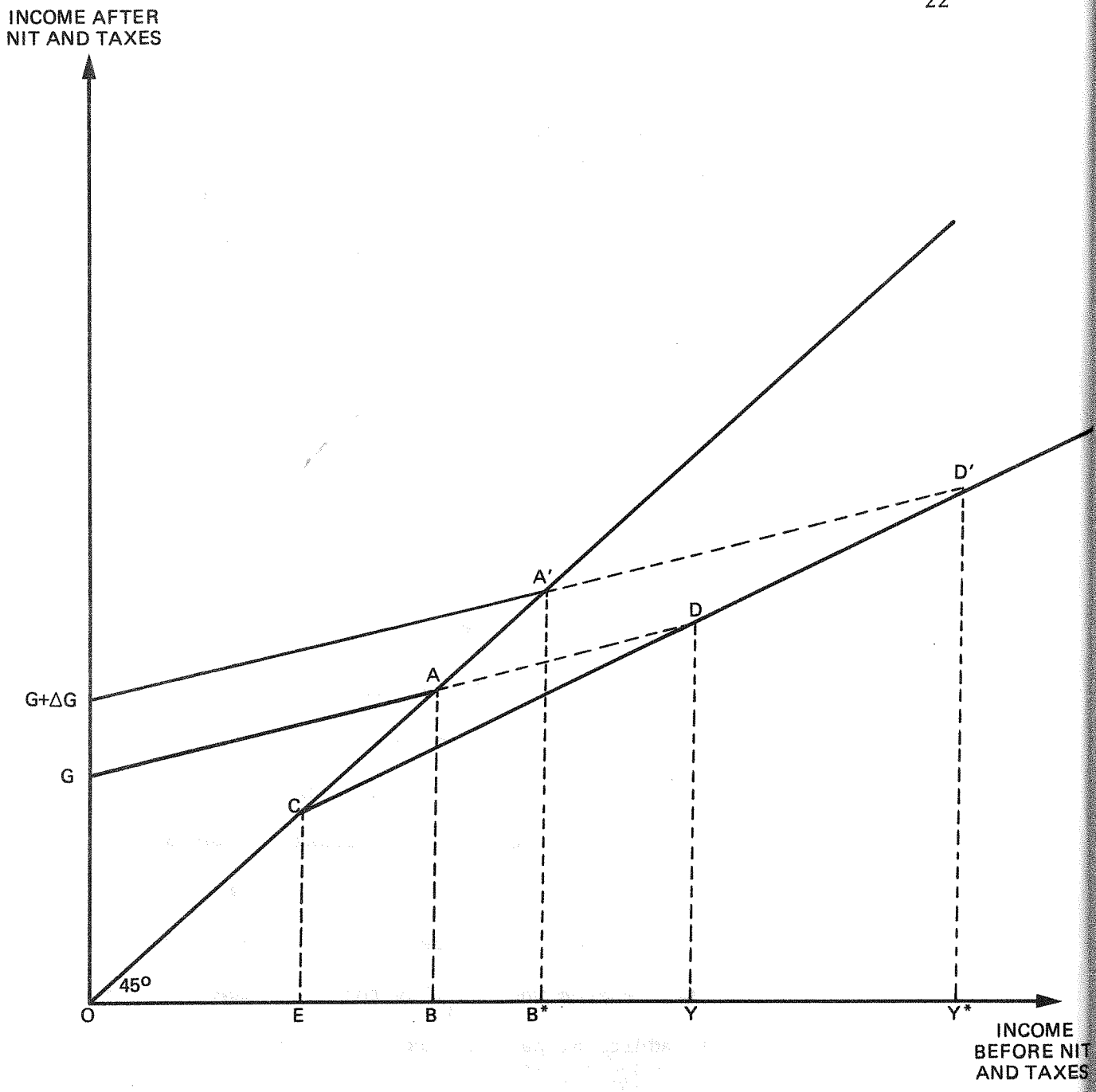
Any increase in support levels will result in more than proportionate increases in budgetary costs. This is because payments to existing participants increase more than proportionately, the proportion of the population eligible for benefits increases, and the induced labour supply response is likely to be greater.

The importance of the latter considerations is illustrated below. In Figure V.1 the increase in guarantees (ΔG) increases benefits to the former recipients (incomes $\leq$ B) by the amount of the vertical shift from GA to G + ΔGA'. However, the increase in guarantees has also extended eligibility for NIT payments to families with incomes up to B* (increased from B). The families between B and B* now receive NIT payments plus 100% tax rebates whereas they formerly received only partial tax rebates. In addition, partial tax rebates now continue until incomes reach Y*.

[See Figure V.1]

The increase in the point of tax equivalence (Y to Y*) deserves comment. The rate of convergence between the income tax schedule CD' and the NIT integration schedules (AD and A'D') depends on the magnitude

FIGURE V.1



EFFECT OF AN INCREASE IN GUARANTEE LEVEL

of the difference between the integration tax rate (t*) and the marginal income tax rate (p).[1] Since the positive income tax rate structure in Canada is progressive, the distance between B and Y increases substantially as B increases if t* is held constant. As a result, taxes could be rebated to a very significant proportion of the total population, with rebates for large families reaching into income levels that may be politically unacceptable.

Having the above factors in mind, the three support levels established by the experiment when payments commenced for 1975 were $3800, $4800, and $5800 for a family of size four, composed of two adults and two children.

The support levels are adjusted periodically to maintain approximately constant real value over the three-year duration of the experiment.

V.3 The Offset Tax Rates

Three experimental offset tax rates were also employed to allow detection of possible non-linearities in response. Many of the considerations influencing the choice of support levels were also relevant for establishing the offset tax rates. These include:

a. the range of the offset tax rates should extend over policy-relevant levels if possible;

b. the range of the tax rates should be broad enough to permit separate measurement of the tax rate effect;

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[1] We assume a constant marginal income tax rate for diagrammatic simplicity.

c. no tax rate should be so high as to remove all monetary incentive to work; specifically tax rates of 100% or greater were ruled out; and

d. any reduction in the tax rate results in an increase in program costs as both payments to eligible families and the proportion of the population who become eligible increase.

The effect of a decrease in the offset tax rate is illustrated in Figure V.2. The original tax rate is given by the slope of GAD. A decrease in the tax rate is represented by the slope of GA'D'. It is readily seen that the effect of the decrease in the tax rate is to extend the breakeven and tax equivalence levels from B and Y to B* and Y* respectively.

[See Figure V.2]

It should be noted that while different combinations of support levels and offset tax rates can yield the same budgetary costs, the distributional implications can be quite different. For example, a decrease in both the tax rate and guarantee may hold total costs constant but would redistribute the benefits towards eligible families with higher incomes and away from those with lower incomes.

The three offset tax rates established by the experiment were 35%, 50%, and 75%. The treatment combination for the saturation site[1] was set at the lowest support level with a 50% offset tax rate.

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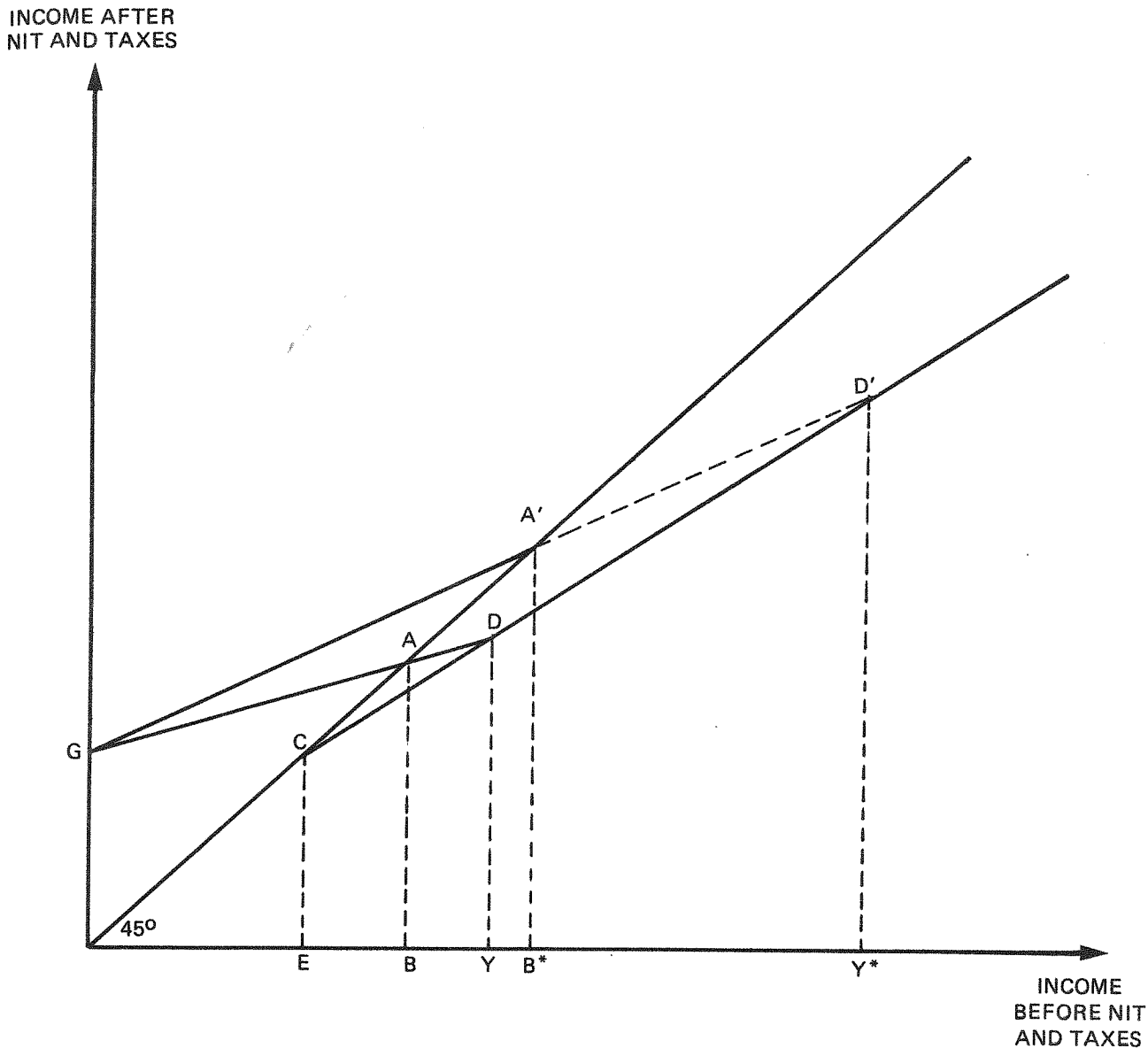
[1] A discussion of the Dauphin saturation site is given in the following chapter.

FIGURE V.2



EFFECT OF A DECREASE IN THE
OFFSET TAX RATE

V.4 The Experimental Treatments

The three guarantee levels and three offset tax rates together yield nine combinations. Inclusion of a control treatment group -- families which are simply interviewed but do not receive income-conditioned payments -- brings the total to ten possible experimental cells. Inclusion of control units is necessary to determine whether observed treatment effects can be attributed to chance or to non-treatment factors. Responses of control units provide a baseline from which to measure treatment effects.

As indicated in the matrix below, not all combinations are used. The combination of the highest guarantee level with the lowest tax rate is considered too far outside the bounds of policy relevance, while combining the lowest guarantee level with the highest tax rate would not sufficiently dominate competing cash transfer programs.[1] Accordingly, a total of eight experimental plans are employed -- seven treatment plans to which families can be assigned and a control plan in which families receive only a flat rate filing payment.

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[1] Initially, the combination of the lowest support level with the highest tax rate was employed as an experimental treatment (Plan 6). Because of the low payment levels received by participants in this plan, participants in this plan were redistributed to plans with the middle and high support levels but with the high tax rate. For further details consult Hum et al., "The Sample Design and Assignment Model of the Manitoba Basic Annual Income Maintenance Experiment".

Table V.1

Experimental Plans: Treatment and Control

		Tax Rate (t)		
		.35	.50	.75
Guarantee (G)	3800	Plan 1	Plan 3	
	4600	Plan 2	Plan 4	Plan 7
	5400		Plan 5	Plan 8

Plan 9 = Controls (IRF & PC)

As stated previously, the tax rate and guarantee level are expected to have an impact on enrolled families. Treatment families will undergo a change in both their level of real income and their effective after tax marginal wage rate.

At the same time, enrolled families are required to file income report forms (IRF's) as well as respond to regular in-depth interviews.[1] Although providing monthly income reports and being interviewed could affect the response behaviour of participants, a more serious issue is that the mere fact of regularly filing income reports may change what is captured on the interview. This may occur, for example, if intentional misreporting is done to obtain payments and participants answer interview questions in a manner which ensures "consistency" of their set of responses. Behaviour is to be measured by information from interviews, hence if only treatment families filed IRF's, one could not disentangle

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[1] Both treatment and control families were interviewed in depth before enrollment to obtain two measures of pre-experimental behaviour. After enrollment, families are interviewed every four months.

the treatment from the reporting effect. Accordingly, one-half of all control families (IRF controls) were required to submit IRF's regularly, even though the forms were not needed for the program itself since controls receive no income-conditioned payments. The other half of the controls (PC controls) are required to simply submit changes in family composition and address on a post card.

V.5 Family Size Index (f.s.i.)

Three basic support levels were established for a family of four, consisting of two adults and two children. In order to specify the support levels applicable to families of different sizes and composition a table of values specifying the ratio of these support levels to the basic support level was developed. These values are referred to as the family size index (f.s.i.).

In establishing the family size index the intent was to insure that the guarantee levels provide similar minimum standards of living to all family sizes and hence offer a "neutral" support structure that does not systematically discriminate against any family size or composition. However, no purely objective criteria can be derived from this goal of neutrality that can be used to evaluate alternative f.s.i. proposals. Subjective decisions regarding the structure of guarantees reflect the following considerations:

a. There are undoubtedly some economies of scale in family consumption; therefore the increment in benefits for each additional family member should decline as the size of the family

increases. Unfortunately, the extent of such economies is itself a complex problem since it depends on the patterns of consumption for the family; for example, scale arguments are especially strong with respect to consumption of durables such as houses, major appliances, cars. On the other hand little or no economies of scale may exist with respect to an item such as clothing. Furthermore, consumption patterns are, in turn, conditioned by income, tastes, stage of the life cycle, age/sex composition of the family, etc.

b. As per capita differentials in benefits among families of different sizes are increased, the incentive is increased for a family to change its structure by splitting into smaller units. Simultaneously we provide an incentive to fraudulently simulate or report such changes. The strength of this incentive to change family structure (to split) depends on the extent to which economies of scale in consumption actually exist relative to the implied economies of scale incorporated into the schedule of the guarantees.

Current welfare programs also vary the amount of their assistance with family size and composition. The variation used by the welfare program, however, may not be consistent with the goal of neutrality and tying the NIT f.s.i. to welfare would simply perpetuate any existing irrationalities in the welfare system. One result of the difference in the way each system responds to differences in family size and composition is that the amount and direction of the difference in benefits that a family would receive from each system depended upon the size and

composition of the family as well as the support level of the plan on which they would be enrolled in Mincome.

The set of family size indexes used by the experiment is given in Table V.2. For information purposes, these values are compared with other measures of low income levels often discussed in Canada.

[See Table V.2 and Table V.3]

V.6 Net Worth Tax Rate and Exemptions

A family's economic strength at any point in time cannot be measured by its money income flow alone. Net worth or wealth is also an important factor affecting the family's ability both to generate income-in-kind from its wealth and to directly consume its wealth. Therefore, within a purely static framework, considerations of horizontal equity require NIT payments to be adjusted to reflect both income flows and the stock of wealth. However, within a dynamic context, the situation is far less clear. Any wealth-conditioned reduction in NIT payments implies a tax on wealth, and could affect savings/consumption decisions not only of current recipients but also of those families who believe they may become NIT recipients in the future. The size of any savings disincentive effect would depend on the level of net worth taxation, the probability of the family becoming a NIT recipient, the expected duration and timing of their recipient status, i.e., when and how frequently they may be subject to the net worth tax. There is no firm basis for predicting the magnitude of this disincentive effect. The duration of the experiment relative

Table V.2

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

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

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

A Head is a person who satisfies the following conditions:

(i) He or she is the major earner or the spouse of the major earner in the Reporting Unit.

(ii) And he or she is an Adult Member of the Reporting Unit.

Table V.3

Mincome Support Levels Relative to Canadian Indicators of Low Income

(ALL MEASURES IN 1972 DOLLARS)

Family Size	1972 Median Cdn. Incomes[1]	Mincome Support Levels		Stat. Canada Low Income Line[2]	Senate Committee Poverty Line[3]	OAS-GIS[4]
		Low	High			
1	$ 3,331	$1,255	$1,915	$2,110	$2,378	$1,994
2	8,222	2,345	3,579	3,516	3,967	3,804
3	10,263	2,906	4,435	4,219	4,767	-
4	11,234	3,301	5,040	4,922	5,556	-
5+(=6)	11,806	3,963	6,048	5,626	7,145	-

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1 See Income Distribution by Size in Canada: Preliminary Estimates, 1972, Stat. Canada cat. no. 13-206, p.9.

2 These limits were established by J. R. Podoluk (see Incomes of Canadians, Queen's Printer, 1968, p.185) and have been adopted as unofficial poverty lines by the Economic Council of Canada and are used as low-income cut-offs by Statistics Canada in its Consumer Finance Survey Reports. These limits actually represent estimates of the 1961 poverty lines adjusted by the rise in the Consumer Price Index.

3 Based on the 1969 poverty line developed by the Special Senate Committee on Poverty, adjusted by the changes in the Consumer Price Index. See the Appendix to Poverty in Canada, Queen's Printer, 1971.

4 As of January 1974, the OAS-GIS program guarantees incomes of $2208 per annum for single pensioners and $4212 per annum for couples (both eligible).

to the probable time frame within which most savings decisions are made militated against experimenting on this important question. Also the budget would not allow the experimental duration to be greatly extended. Therefore, the net worth parameters of the NIT program are not experimentally varied, but were chosen and fixed on the basis of a priori judgments of their policy relevance.

Existing Canadian tax/transfer programs are often inconsistent in their treatment of wealth and hence do not provide complete guidance on this question of policy relevance. Certain welfare programs implicitly impose marginal tax rates of 100% above an exempted level on some forms of wealth, but ignore other forms of wealth completely (such as owner-occupied dwellings). Most non-welfare transfer programs are income-conditioned only and ignore both the existence of assets and the income-in-kind that they may generate.

After some examination of the role to be played by a NIT, together with possible behavioural distortions and budgetary implications for any provincial or national NIT program, the experiment adopted the following treatment of personal net worth. For those NIT recipients with relatively limited levels of net worth, $10,000 or less, the wealth in excess of a basic exemption ($3,000) is taxed at 4% per annum. Given an offset tax rate of 50% on income, this is equivalent to imputing a rate of return of 8% on wealth and taxing this imputed income from wealth at 50%. The implicit assumption is that such NIT recipients are neither expected to consume their wealth nor to save or add to their wealth while on the NIT program. The exemption is a basic $3,000 per family to avoid the administrative costs of dealing with small amounts of wealth. Net worth

in excess of $10,000 but less than $30,000 in excess of the basic exemption is taxed at a rate of 8% per annum.

Net worth above $30,000 is taxed at 16%. These rates imply that relatively wealthy NIT recipients should consume their net worth in excess of $10,000 to help maintain themselves during periods of low income.[1]

V.7 Positive Tax Rebate

The only parameters varied in the experiment are the support level and the offset tax rate. To ensure that participants in the experiment are in fact facing the support levels and tax rates of their treatment, all other government tax and cash transfer programs are taken into account and neutralized.

The problem of attempting to estimate the response of individuals who are subject to more than one tax rate as a result of simultaneous participation in several income-conditioned programs would be complex. Not only would combinations of tax rates result in non-linear aggregate tax functions which increase the difficulties of empirically estimating responses, but also it would be quite probable that individuals would misperceive such a complicated tax schedule. In addition, any changes in non-experimental programs during the course of the experiment would further complicate the estimation of response.

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[1] Farmers who potentially derive more than half of their non-transfer income from farming were allowed an additional net worth exemption of $20,000 so that the net worth tax would not impose a requirement that they consume part of their business assets. Other self-employed participants were not allowed a similar net worth exemption.

The combination of two or more tax rates could also result in net marginal tax rates that exceed the maximum rate employed by the experiment or, alternatively, 100%. Since the research objective of the experiment is concerned with estimating the effect of different tax rates on labour supply response, the resulting effective tax rates may be far from optimal from the point of view of estimation. A very complex tax rate structure for the experiment would have to be designed in order to reflect the changing marginal tax rates of the PIT system if overall effective marginal tax rates were to be controlled at all income levels. Further, any attempt to calculate the actual tax rate faced by an experimental participant would be complicated by the complexity of the positive tax system, particularly the very significant differences in income and filer definitions between the positive and negative tax systems. One very important difference is that the positive tax system is based upon individuals while the negative tax system is based upon family units. In sum, a basic experimental design issue is that of maintaining control over the marginal tax rate.

One method of achieving this control for treatment units is to rebate all other tax payments in order to maintain experimental control over the tax rate. Any technique other than rebating leaves us with the problem of calculating the effective tax rate from a complex structure of tax rates. At the same time, a rebate approach simulates a full integration of the two systems with common income definitions based on the family unit and a proportional marginal tax rate.

Rebating, however, involves a disadvantage concerning the risk of change in the positive tax system. Should the positive tax system be

modified over the course of the experiment, we would have isolated the treatment families from an exogeneous shock that affects the control families, hence reducing the latter's effectiveness as controls. However, given our evaluation of policy relevance, and our concern to maintain linear tax functions where possible, the experiment decided that income tax payments will be fully rebated to all experimental families enrolled in NIT treatment cells, up to the breakeven level (B). Positive tax payments are not rebated to control families.

In considering how to rebate positive income tax the alternative of rebating an estimated tax liability each month was considered but rejected. A possible advantage of such estimates is that they might correspond more closely to the final amount payable than will the amounts withheld each month thus reducing the amount of any over or under payment. The estimate, however, will not completely eliminate over or under payments because individuals use certain measures at year end to reduce their tax payable and because the estimates require assumptions concerning who will be tax filers and who will be claimed as dependents. Also, the calculation of an estimate is not a simple procedure.

The final procedures adopted involved a rebate of the actual amount of income tax (and CPP contributions and UIC premiums) withheld from earned income each month being included in the NIT payment for that month. For those who pay taxes in quarterly installments and therefore control the amount they pay, no tax rebates are made until a year-end reconciliation. Monthly rebating of positive income tax will result in overpayments of NIT when an individual has too much tax withheld and result in underpayments when the reverse tax situation holds. The

monthly rebating, however, does provide a more immediate and more continuous method of rebating positive income tax than a system of rebating the entire amount after the end of the year when positive income tax returns are filed.

V.8 Tax Integration, Rebates and Tax Equivalence Points

Rebating of positive income tax could cease whenever the family's income rose above breakeven. At this point the family would receive no income-conditioned transfers. This would imply that the family suddenly faces the positive income tax again with a resulting drop in their net income. In order to avoid this "notch effect" the tax rebate is gradually phased out at levels of income in excess of breakeven.

The amount of the rebate is 100% of the positive income tax when the family's income equals the breakeven level, and this amount gradually falls to zero over an income range whose size depends upon the difference between the positive tax rate and the NIT integration tax rate applied to income above breakeven. Over this range partial rebates of the positive income tax are paid. The income level at which the rebate paid becomes zero is called the tax equivalence point. (See Figure III.2 and discussion.)

With reference to Figure III.2 the offset tax rate t is illustrated as being extended until the point of tax equivalence is reached at D. All families with pre-tax incomes between B and Y are eligible for tax rebates. However, this situation has resulted in high values for Y, the tax equivalent point. The resulting tax rebate costs to the experiment would be substantial. In order to reduce the level of the tax equivalent

point, the experiment adopted the following procedures. The offset rate is extended to 20% above breakeven level, B, to avoid distorting the decisions of families just below breakeven. Then, as family income exceeds 1.2 times the breakeven level, the tax integration rate is increased so that in combination with the offset rate, the effective tax rate becomes 100%. This mechanism reduces the proportion of taxes rebated and lowers the value of Y, the tax equivalent point.[1]

The implications of the tax integration problem and its extension to the net worth dimension are illustrated in Figure V.3 below. Net worth for a representative family is measured on the vertical axis and income before taxes and NIT payments is measured on the horizontal axis. The locus WRQY now defines the domain of eligibility, taking into account positive tax rebates and the tax integration mechanism. For families having income between E and B, RB delineates the upper limit on the level of net worth that a family can possess and still be eligible for both NIT payments and a 100% positive income tax rebate. As a family moves towards the boundary RB, their NIT payments decline until the payments fall to zero at RB. However, the family continues to receive a 100% tax rebate right up to RB. If they were to lose the entire rebate as soon as they reached a combination of income and net worth represented by RB, there would be another "notch" effect. Accordingly, the rebates must be phased out gradually to avoid the inequities created by a notch effect.

[See Figure V.3]

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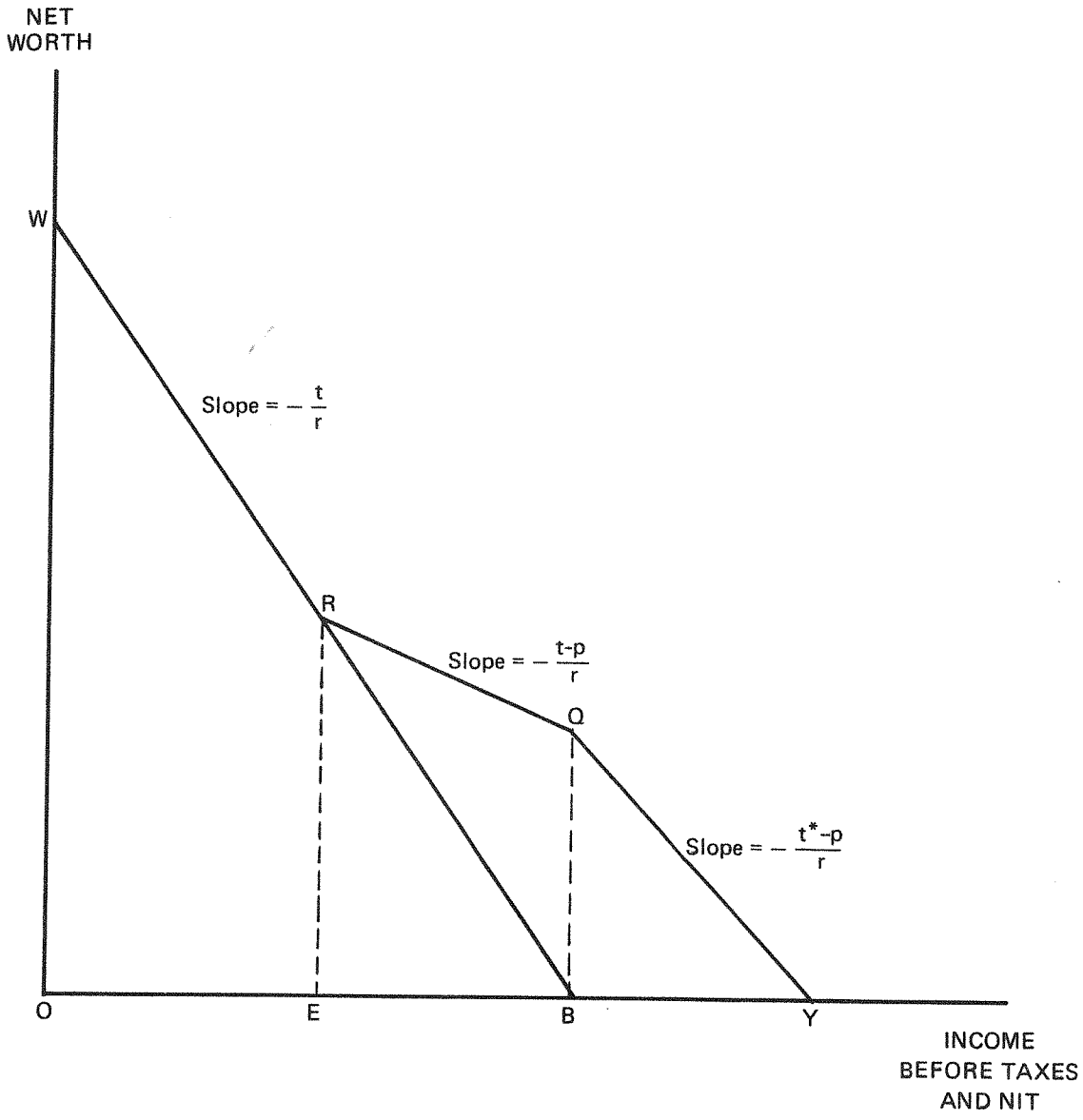
[1] Diagramatically with respect to Figure III.2 this introduces a kink at 1.20 B and decreases the slope of the line segment to the right of the kink so as to intercept CT at a lower level of Y.

FIGURE V.3



NET WORTH AND PAYMENTS ELIGIBILITY AS MODIFIED BY INTEGRATION WITH POSITIVE INCOME TAX

The regions indicated by Figure V.3 may be interpreted in the following way: By rebating positive income taxes, the domain of eligibility for non-zero payments has been extended and this is depicted by the locus WRQY. In addition,

a. all families with income/net worth combinations falling within the area OWB are eligible for both NIT payments and full income tax rebates;

b. all families with income/net worth combinations falling either within the area RQB or BQY receive no NIT payments but are eligible for partial income tax rebates;

c. all other families are not eligible for either NIT payments or income tax rebates.

The slope of WRQY indicates the rate at which income can be substituted for net worth while holding the sum of NIT payments plus income tax rebates constant (in this case at zero).[1]

Figure V.4 illustrates another aspect of the extension of eligibility. Not only is the breakeven level of income raised from B to B*,

[See Figure V.4]

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[1] For levels of income between E and B, the NIT payments function is:

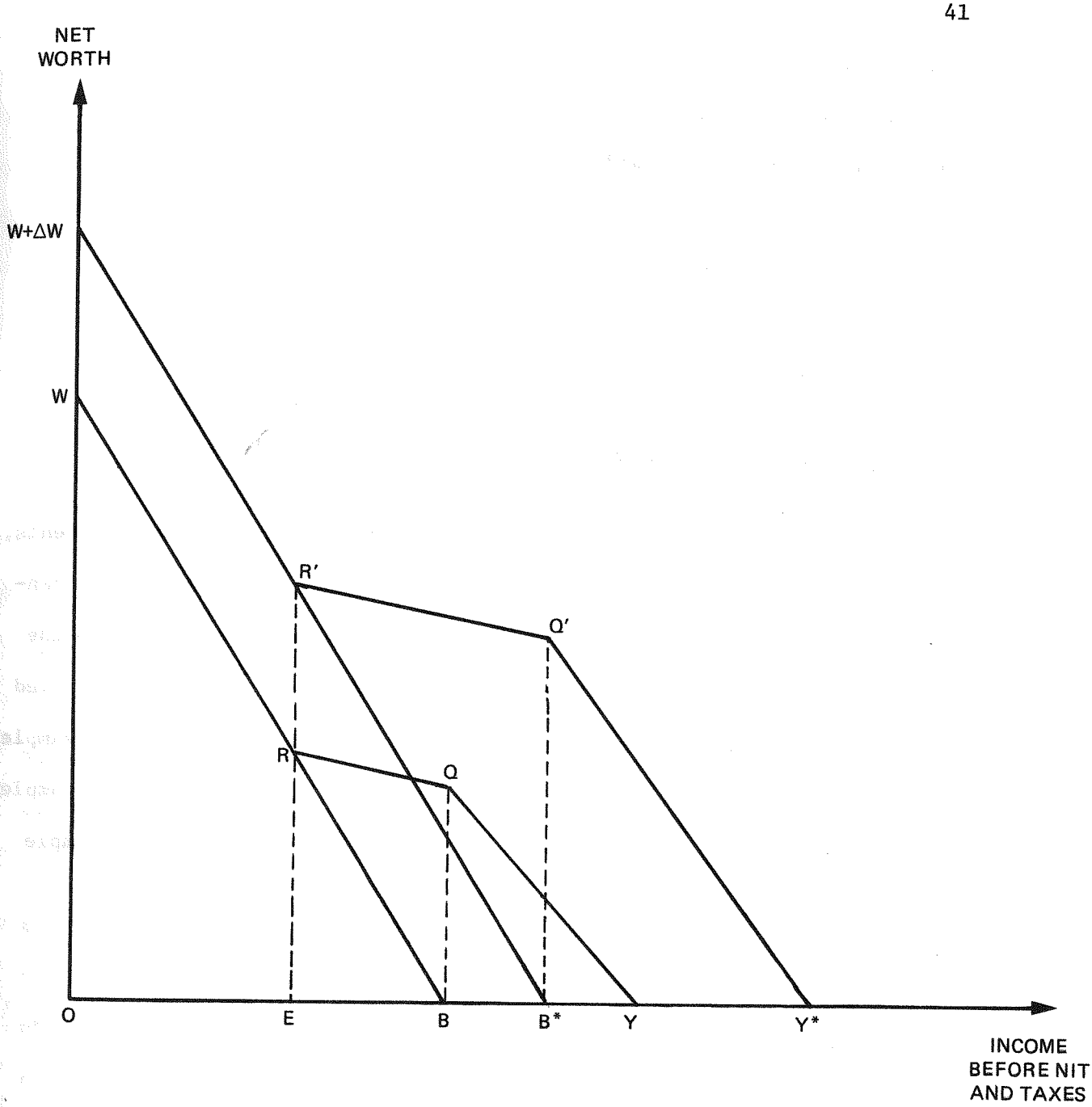
$$P = G - t \cdot Y - r \cdot W + p(Y - E)$$
$$= G - (t - p)Y - r \cdot W - p \cdot E \ .$$

Differentiating totally we have:

$$dP = \frac{\partial P}{\partial G} \cdot dG - \frac{\partial P}{\partial Y} \cdot dY - \frac{\partial P}{\partial W} \cdot dW - \frac{\partial P}{\partial E} \cdot dE \ .$$

(Continued)

FIGURE V.4



NET WORTH AND PAYMENTS ELIGIBILITY EFFECTS OF AN INCREASE IN GUARANTEE LEVELS

and the point of tax equivalence raised from Y to Y*, the entire WB schedule is shifted upward by $\Delta W = \frac{\Delta G}{r}$. This upward shift increases the number of eligible families with incomes below B by increasing the allowable level of net worth. Similarly, eligibility for the tax rebate is extended through the shift in RQY to R'Q'Y*.

VI. Experimental Sites, Sample Design and Allocation

VI.1 The Three Experimental Sites and Structure of the Sample: Overview

The structure of the sample comprises the following three segments, each associated with a distinct experimental site: (i) a stratified random sample drawn from the City of Winnipeg; (ii) sample points from the town and rural municipality of Dauphin, Manitoba; and (iii) a stratified random sample drawn from several smaller Manitoba communities. The sample points from Winnipeg are referred to as the urban dispersed module, sample points from Dauphin are referred to as the saturation module, and sample

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[1] Continued

Along RQ, $dp = dG = dE = 0$, while $\frac{\partial P}{\partial Y} = (t - p)$ and $\frac{\partial P}{\partial W} = -r$.

Therefore the slope of line segment RQ is:

$$\frac{dW}{dY} = -\frac{t - p}{r} .$$

For levels of income between B and Y, the NIT payments function covers rebates only:

$$R = p(Y - E) - t^*(Y - B) - r \cdot W$$
$$= (p - t^*)Y - pE + t^*B - r \cdot W .$$

Totally differentiating, setting $dR = dE = dB = 0$, and rearranging terms yields the slope of line segment QY:

$$\frac{dW}{dY} = -\frac{t^* - p}{r} .$$

points drawn from the several other smaller communities are collectively referred to as the rural dispersed module.

The decision to have three distinct experimental sites evolved as a result of many considerations and constraints. A balance was sought in the design between methodological rigour and policy relevance. The dispersed sample segment was chosen in order to effect a "controlled experiment", modelled after those common to the laboratory sciences. Similar to income maintenance experiments conducted in the United States, the urban dispersed module of the Manitoba experiment was designed to measure behavioural responses to differently structured NIT programs of selected subjects randomly assigned to treatment and control groups. Limited to choosing a site from Manitoba with a sufficiently large population to permit dispersed sampling techniques, the only possible site was Metropolitan Winnipeg, an area comprising approximately 600,000 persons and representing over 55% of the province's population.[1]

A unique design feature of the Manitoba Basic Annual Income Experiment is its incorporation of a saturation module in conjunction with the urban dispersed sample in Winnipeg. To this end, the entire community of Dauphin and surrounding area[2] was declared eligible to participate in the experiment and receive payments, and the behavioural responses of

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[1] The second largest community in Manitoba after Winnipeg is Brandon, which has a 1971 population of 31,000. Given the required sample size, it is highly improbable that the non-contamination objective of dispersed sampling could have been achieved in any other Manitoban site but Winnipeg.

[2] According to the 1971 Census, the Town of Dauphin has a population of 8,891 and the Rural Municipality of Dauphin has a population of 3,166.

those individuals and families receiving benefits was systematically recorded in identical manner to participants in the dispersed sample and control groups chosen from outside the saturation site. In addition, information was gathered from residents who refused to participate but who were otherwise eligible to receive benefits with a view towards ascertaining determinants of non-response to social programs.

One drawback of the standard dispersed sampling technique now common to income maintenance experiments is the artificial environment created by isolating treatment and control families from one another. A saturation site best represents the social milieu likely to be found under a universal GAI, thereby providing a more externally valid setting for the testing of the effects of a GAI. At the same time, the saturation site experiment allows policy makers to gain valuable knowledge about the operational feasibility and probable administrative costs of a guaranteed income.

The dispersed sample -- with its elaborate variation of treatments, control groups, and random selection and assignments -- offers a sound design in which to test the impact of various specific program parameters on individual and family behaviour. The saturation site is more in the nature of an elegant field experiment. Although methodologically distinct from the more pure experiment associated with the Winnipeg site, it will provide important information about issues probably unobtainable from the more artificial setting of the dispersed module. Together, the two experimental components -- dispersed and saturation samples -- are viewed as providing an overall experimental design which is optimally satisfactory both from the perspective of experimental methodology and policy requirements.

Finally, sample families were also selected from several smaller rural sites in order to complement the Winnipeg and Dauphin sites. In the following sections, we outline in greater detail the procedures for sample selection and assignment to treatments, the stratification features of the sample, the supplementary sample, and the rationale and treatment for the saturation and rural dispersed sites.

VI.2 The Formal Assignment Model for the Dispersed Sites[1]

The manner in which a sample is chosen and allocated to various treatments is a significant component of any experimental design. The sample design and assignment process of the Manitoba Basic Annual Income Experiment incorporated specific assumptions and constraints and utilized an adaptation of the Watts-Conlisk formal model to generate an optimum design for estimating response surfaces.

The Watts-Conlisk assignment model is a formal technique for optimally allocating observations or sample points among various experimental treatments in order to maximize the value of the information generated by the experiment. The basis of the assignment model is a rigorous benefit-cost analysis of the alternative sample allocations that are feasible within a given budget constraint. In this application benefits are measured

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[1] This section provides a summary only of the sample design and allocation model of the experiment. It is included to render the present document self-contained and to present the sample structure from an overall design viewpoint rather than from a detailed statistical or sampling context. For complete details on the sample design and assignment model see D. Hum, M. Laub, C. Metcalf and D. Sabourin, The Sample Design and Assignment Model of the Manitoba Basic Annual Income Experiment, Technical Report No. 2, Mincome Manitoba.

in terms of reductions in the variances of certain predicted values. Costs are measured in monetary terms reflecting the financial budget constraint.

More formally, the design model may be stated:

$$\text{Minimize} \quad \phi(n_1, \ldots, n_m) = \text{tr}[W \, \text{Var}(Pb)]$$

subject to

$$\text{(6.1)} \qquad \sum_i c_i n_i \leq C \quad ; \quad L(n_1, \ldots, n_m) = 0 \quad ,$$

$$n_i \geq 0 \qquad (1 = 1, \ldots, m)$$

where m is the number of design points, c_i is the cost of one observation at the i^{th} design point, n_i is the number of observations at the i^{th} design point, and C is the total available budget. $L(n_1, \ldots, n_m) = 0$ represents additional external or arbitrary linear constraints imposed on the choice set. A criterion to rank alternative feasible designs is given by the objective function ϕ. An estimation viewpoint for the experiment is adopted in specifying ϕ in terms of estimation error Var(Pb). In particular, a response function and regression model is specified and the optimal sample allocation is that which minimizes the weighted sum of the variances of the elements of Pb. Pb is a linear combination of the estimated vector of regression coefficients b, tr(·) is the trace operator and W is a positive definite diagonal weight matrix whose diagonal elements measure the relative importance (policy relevance) to the experimenter of the elements of Pb.

The above is a well-behaved programming problem involving minimization of a convex objective function over a set of linear constraints. Accordingly, the assignment model indicates for a given budget the optimal allocation of sample points which will yield the least prediction error for an estimated regression equation.

VI.3 The Stratification of the Sample

The selection of the sample employed information gathered from pre-experimental interviews. After ascertaining eligibility conditions, the pre-experimental information was employed for stratification purposes. The experimental sample was stratified by family structure and normal income.

Four family structure types were employed: (a) double-headed family, multiple earners; (b) double-headed family, single earner; (c) single-headed family; and (d) single individual. While undoubtedly other dimensions of family structure could also affect response to a guaranteed income program (such as education level of head, age of children, etc.), it was felt that these broad categories would be most relevant for the labour supply analysis. This decision reflects the hypotheses that unattached individuals will react differently from individuals with family responsibilities, that double-headed families will react differently from single-headed families, and that a significantly different labour supply response will be observed among secondary workers.

Families were also stratified in terms of a discrete definition of adjusted normal income -- that is, an income measure from which estimated transitory income components were removed and adjusted to a family

size of four, comprising two heads and two children under 15 years of age. The normal income measure is based upon pre-experimental information on earnings and work history and represents an estimate of the annual income the family would normally receive.

VI.4 The Supplementary Portion of the Sample in the Winnipeg Site

Shortly after payments were begun to experimental participants, preliminary indications revealed that an unacceptably small number of families were receiving non-minimum payments. This meant that, even if the amounts low-income families received from the experiment were sufficiently varied, there might not be a sufficient number of these families at the end of the experiment to allow an estimate of their labour supply response with an acceptable degree of precision. The attrition rate was also proving to be higher than anticipated, which, if continued, would lead to smaller sample sizes than desired for research purposes. Finally, additional information became available which led to a desire for certain design modifications and sample changes. More specifically, it was discovered that certain groups of households of policy relevance had been systematically excluded from selection and consequently a correction was warranted. Additionally, sample size and other considerations led to a design decision to eliminate an ineffective treatment plan; namely, Plan 6, the combination of the lowest support level and highest tax rate. This decision was based upon two factors. Firstly, the financial benefits of Plan 6 were slight when compared to the alternative available to the participant of attriting and accepting existing welfare payments. Secondly, given the size of the treatment sample and our estimate of the potentially

available population it appeared unlikely that an augmented sample could be drawn at acceptable cost to yield the desired final sample size. From an experimental design standpoint the salient feature to note is that the elimination of Plan 6 does not preclude the possibility of estimating non-linearities associated with tax rates and guarantee levels. The design space of the experiment retains its focus on a central design point even with the elimination of Plan 6 since each of the treatment parameters (tax rate or guarantee level) can be varied given the single, central value of the other.[1]

As a result of these and other considerations, portions of the sample were augmented. This supplementary sample was restricted to the Winnipeg site and confined to those household types and income strata for which current sample sizes were thought to be inadequate. The supplementary sample receives payments for three years, a duration identical to that of the original sample, but commencing one calendar year after the first payments to the initial sample. Payments to the original sample end in December 1977, while those to the supplementary sample end in December 1978. The same stratification variables were used in selecting the supplementary sample, with the added objective of assigning the appropriate number of additional units required to each experimental treatment.

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[1] For details concerning the supplementary sample, in particular how experimental participants were assigned to plans, see Hum et al., The Sample Design and Assignment Model of the Manitoba Basic Annual Income Experiment, Technical Report No. 2, Mincome Manitoba.

VI.5 Rationale for the Saturation Site Sample

Previous income maintenance experiments conducted in the United States have all utilized randomly drawn dispersed samples in their design. The methodological advantage of a dispersed sample lies in isolating treatment families within some reasonably homogeneous geographical domain from each other, thus making it possible to vary the NIT treatment parameters within the dispersed site. Provided that such a sample is very small relative to the population from which it is drawn, and is well dispersed among that population, the individual behavioural responses to the treatment programs will not be confounded by externalities nor macro-effects. However, this very isolation places the treatment families in a highly artificial environment. Unlike the situation under a national NIT whereby all eligible families will benefit from receiving cash transfer payments from a social welfare program, friends and relatives of treatment families are not eligible for and may not even be aware of the experimental NIT program in which the treatment families are enrolled. The artificiality of this situation is reinforced whenever a change in behaviour induced by the NIT involves any sort of social interaction. A change in work effort or leisure activity often involves individuals outside the immediate family. Since individual responses are often conditioned by the perceived expectations, norms and activities of others, the dispersed sample may yield unreliable results. For example, the labour supply response may be understated in the dispersed sample because all of their friends are still required to work, reducing their availability for joint leisure activities and possibly increasing their hostility to those who work less because of the guaranteed income. In

short, the fact that other members of the community are not participants may exert an influence on the response of dispersed NIT recipients.

A second, related aspect of the saturation site sample concerns the administrative realism of the delivery system. In addition to behavioural responses which are conditioned by a less artificial environment, the saturation site also allows for a more natural implementation of a GAI -- primarily because the GAI program is integrated within the wider social network of the community. Because exposure to the program is a shared community experience, administrative responses, recipient participation, and program costs should more clearly resemble those to be obtained under a universal, permanent program. In short, answers to questions about administrative feasibility, operational procedures, etc., concerning a universal-type GAI are facilitated by the less artificial environment of a saturated site experiment.

The dispersed sample is targeted towards individuals and families with lower incomes, although virtually the entire income spectrum is represented in the sample. The saturation site sample, with its feature of universal eligibility, is uniquely suited to investigate the circumstances of non-participation by eligible households. Again, the possible interaction between recipients and non-recipients afforded in the saturation site should result in a more representative response to a guaranteed income treatment.

Any implementation of a NIT program on a national scale with universal eligibility will result in a situation significantly different from that created by an experiment utilizing a dispersed sample. Despite

their advantages, dispersed samples also have inherent shortcomings. A saturation site sample serves to strengthen, in a consequential manner, the results of a dispersed sample while extending the capability of the experiment to address certain administrative issues as well. In recognition of this possibility, the Manitoba Basic Annual Income Experiment incorporated a saturation site sample, hoping to improve the extent to which results of dispersed sample experiments can be generalized and to aid prediction of the impact of a national program.

VI.6 Treatment Selection in the Saturation Site

With universal eligibility to receive payments, only a single financial treatment option is advisable since any horizontal inequities introduced into the saturated community would quickly become obvious to all participants. Multiple sets of NIT parameters might well lead to a readjustment of all benefits to the most generous treatment.

The choice of the specific NIT treatment to use in the saturation site was conditioned by several factors. Firstly, considerations of cost were important. In an experiment with a limited total budget, universal eligibility to participate in a chosen site implies that costs increase directly with the generosity of the NIT treatment. Secondly, policy preference for a particular NIT treatment plan is relevant. Unlike the multiple-treatment dispersed-sample component of the experimental design, emphasis need not be placed upon the objective of efficiently estimating a response surface over a design space with varying support

levels and tax rates.[1] Thirdly, the single treatment option for the saturation site must provide a sufficiently large payment to be considered a competitive replacement for current welfare programs in the community. Finally, it is desirable to minimize problems in making comparisons across the saturated and dispersed components of the experimental design. For this reason many of the survey, payments and administrative procedures are made the same in all sites. The desirability of connecting the dispersed and saturation components of the experiment assumes that behavioural responses in the saturation sample will be altered by social interaction, externalities, and administrative operations of a "universal type" GAI program in a more "life-like" setting. Accordingly, to facilitate this investigation, the tax rate and support levels in Dauphin, the saturation site, are identical to one of the financial treatment plans of the dispersed experiment implemented in Winnipeg. Direct comparisons can then be made between treatment subjects in the saturation site and subjects assigned to the corresponding treatment plan of the Winnipeg dispersed sample.

The single NIT treatment plan made openly available to all households in Dauphin was set at the low guarantee level ($3800 for a family of four, 1975), and the middle tax rate (50%, Plan 3).

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[1] For discussion and further details concerning the response surface and the multiple-treatment dispersed sample see D. Hum et al., The Sample Design and Assignment Model of the Manitoba Basic Annual Income Experiment, Technical Report No. 2, Mincome Manitoba.

VI.7 The Rural Dispersed Sites

The Manitoba Basic Annual Income Experiment also included treatment and control families in several rural sites in addition to Winnipeg and Dauphin. This was done in an attempt to provide an analytic bridge between the Winnipeg dispersed sample with its various treatment plans and the Dauphin saturated sample with its single treatment option. The treatment families in the several rural Manitoba communities are assigned to a financial program identical to that of the saturation site.

The objectives of dispersing treatment and control families throughout a number of rural Manitoba communities are twofold. Firstly, the sample points in the dispersed Winnipeg site are drawn from an urban population. Because of the existence of treatment plan variation in Winnipeg, the most direct experimental evidence will be obtained from this portion of the experimental sample. The rural dispersed sample can augment the Winnipeg dispersed design by spreading the direct testing of experimental effects over a wider set of conditioning environments, thereby increasing our ability to generalize from the dispersed results. Admittedly, this advantage will be limited by the single financial treatment implemented in the rural dispersed sites. Secondly, the use of a single saturation site presents problems in experimental control.[1] Exogeneous disturbances peculiar to the saturation site during the course of the experiment could seriously interfere with the eventual analysis. Cost and other considerations precluded multiple saturation sites. The saturation site experiment is oriented towards the behavioural responses

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[1] See the section on "Saturation Control" for a further discussion.

of individuals rather than communities. The experiment therefore very carefully collected pre-experimental micro-data, thereby allowing the saturation site sample to be used as its own control, much as in conventional empirical economic analysis or quasi-experimental research designs.[1] Within this context the role of the rural dispersed sample is again to augment the Dauphin saturation sample. The rural dispersed treatment families are on the same financial program as that available in Dauphin. By spanning a sufficient range of background characteristics such that Dauphin cannot be considered an outlier, the rural dispersed segment of the sample should provide a sound controlled data base with which to employ conventional analytic methods.

The selected rural sites usually included a town and the surrounding rural municipality.

[See Table VI.1]

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[1] For a definition and discussion of quasi-experimental research designs see Campbell, D.T. and J.C. Stanley, Experimental and Quasi-Experimental Design for Research, Rand McNally, Chicago, 1963.

Table VI.1

Rural Dispersed Sites: Selected Characteristics

	1971 Population (a)	% Growth of Population 1966-1971 (a)	1971 Ethnic Composition as Percentages (b)			1971 % Population Age 55+ (c)	1971 % Population Some High School + (d)
			British Isles	Largest Other	Next Largest		
Morris	1399	+ 4.48	24.04	G-43.55	N-11.15	20.71	40.35
Morris RM	3279	- 11.76	12.37	G-43.21	N-28.24	16.01	35.47
Carman	2030	+ 5.62	69.46	G-10.59	N- 7.14	34.24	47.54
Dufferin	2598	- 6.51	44.13	G-26.14	N-14.58	18.46	37.57
Stonewall	1583	+ 0.38	65.03	G- 9.51	S- 7.06	22.08	50.15
Rockwood	4073	+ 6.71	54.33	U-13.75	G- 8.19	17.47	46.70
Morden	3266	+ 5.46	29.47	G-41.83	N-18.32	29.35	40.76
Stanley	4345	- 9.52	5.31	G-66.32	N-23.53	13.58	27.19
Swan River	3522	+ 1.50	52.76	G-13.30	U-12.31	24.96	43.47
Swan River RM	4107	- 10.58	53.21	U-21.42	G- 8.59	22.75	37.09
Minnedosa	2621	+ 13.71	68.46	S- 8.72	U- 7.98	28.24	49.54
Minto	784	- 15.06	75.97	S- 7.14	U- 7.14	26.75	49.68
Odanah	702	- 10.00	68.84	S-15.22	G- 4.35	23.40	49.29
Neepawa	3215	- 0.43	70.15	P- 8.00	U- 7.85	37.17	45.37
Langford	749	- 14.60	85.31	S- 6.99	G- 2.10	18.67	52.11
Rosedale	2347	- 11.20	44.40	P-19.24	U-13.74	22.81	30.87
Portage la Prairie	12950	- 0.48	60.00	U- 9.36	F- 7.01	20.27	44.80
Portage la Prairie RM	7514	+ 0.44	57.03	G-17.19	F- 8.99	14.97	40.87

(Continued)

Table VI.1

Rural Dispersed Sites: Selected Characteristics

	1972 Average Income (e)	% Growth of Income 1967-1972 (e)	1971 Labor Force (f)		1971 % of Population Employed by (g)			1971 % of Farm Sales Under $2500 (h)
			Male %	Female %	Industry	Gov'ment	Industry + Government + Others Major	
Morris	4440	+ 27.9	66.06	33.94	10.44	2.50	17.44	--
Morris RM	4048	+ 34.0	73.06	26.94	--	--	--	16.1
Carman	4570	+ 27.2	64.63	35.37	0.79	1.72	3.49	--
Dufferin	3907	+ 21.7	73.97	26.03	--	--	--	19.6
Stonewall	5275	+ 36.4	60.48	39.52	3.47	5.37	10.80	--
Rockwood	4789	+ 35.6	68.45	31.55	--	--	--	35.3
Morden	4665	+ 34.3	59.71	40.29	6.77	2.91	12.62	--
Stanley	4343	+ 35.9	74.17	25.83	--	--	--	21.5
Swan River	4234	+ 20.1	59.39	40.61	1.93	2.56	9.63	--
Swan River RM	2950	+ 13.5	76.18	23.82	--	--	--	22.7
Minnedosa	4572	+ 18.1	65.38	34.62	11.45	2.67	16.18	--
Minto	3089	+ 14.7	72.46	27.54	--	--	--	27.1
Odanah	4035	+ 77.2	72.13	27.87	--	--	--	17.9
Neepawa	4326	+ 15.7	61.34	38.66	1.87	3.89	9.77	--
Langford	N/A	N/A	70.13	29.87	--	--	--	21.3
Rosedale	2963	+ 11.6	67.58	32.42	--	--	--	29.5
Portage la Prairie	5125	+ 41.2	59.63	40.37	3.68	6.99	16.33	--
Portage la Prairie RM	4037	+ 26.1	70.89	29.11	--	--	--	24.8

(Continued)

Table VI.1

Rural Dispersed Sites: Selected Characteristics

Source

(a) Regional Analysis Program Southern Manitoba. Manitoba Department of Industry and Commerce. Carvalho/Page Group. Winnipeg 1975. Update Volume One. pp. 5-33.

(b) 1971 Census of Canada. Population-Specified Ethnic Groups. Statistics Canada. Catalogue 92-774 (SP-4) May 1974. pp. 2-87 to 2-94.

(c) Carvalho/Page Group. Update Volume One. pp. 134-156.

(d) Carvalho/Page Group. Update Volume One. pp. 91-127.

(e) Carvalho/Page Group. Update Volume Two. pp. 290-300.

(f) Carvalho/Page Group. Update Volume Two. pp. 410-431.

(g) Community Report prepared by the Regional Development Branch of the Department of Industry and Commerce. 1971.
and
Carvalho/Page Group. Update Volume Two. pp. 332-343.

(h) Carvalho/Page Group. Update Volume Two. pp. 376-385.

Ethnic Codes

G = German		S = Scandinavian	
N = Netherlands		F = French	
U = Ukrainian		P = Polish	

Definitions

Labor force = experienced labor force employed + experienced labor force unemployed + new entrants seeking work.

VII. Other Design Issues

VII.1 Limited Duration of the Experiment

One of the many issues of the experimental design concerns the ability to estimate long run responses from a temporary experiment. By adopting a three-year experimental period, it is implicitly assumed that the experimental families will make the same adjustment to the limited duration experimental NIT program as they would to a permanent program. However, to the extent that the experimental families regard the program as transitory, they will not make the full long run adjustment. In such circumstances, biases can result. A NIT scheme will give rise to an income effect and a substitution effect, both of which will decrease labour supply. A temporary NIT program will reveal a smaller income effect and an increased substitution effect relative to a permanent program.[1] The ideal experimental solution for coping with the problem would be to institute a long term (lifetime?) program and compare the response of the long term experimental families with the three-year experimental families. This was not done and certain remarks are in order.

Firstly, the Seattle-Denver Income Maintenance Experiment (SIME-DIME) has a variable-duration component in its design and use can be made of this information when it becomes available. Secondly, another possibility involves a test of the extent to which the experimental families regard NIT payments as transitory income. Saving patterns can

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[1] For details see Charles E. Metcalf, "Predicting the Effects of Permanent Programs from a Limited Duration Experiment", Journal of Human Resources, IX, No. 4, 1974: 530-555.

be investigated within the context of the permanent income hypothesis which implies that transitory income is saved. Still, there are a number of limitations associated with such an approach, not the least of which is the requirement to relate the adjustment of savings patterns to the adjustment in labour supply and other response variables. Thus, although the test of the perception of the NIT program as a transitory income component can be a useful indicator of the presence of transitory effects, we will not know if these transitory effects are greater, smaller, or the same in any behavioural dimension other than consumption-savings patterns.

Budgetary considerations eventually ruled out the possibility of varying the length of the experiment and resulted in the decision to adopt a single, fixed three-year duration. The three-year experimental horizon is shared by income maintenance experiments in the United States and will facilitate comparison of Manitoba's results with those obtained elsewhere. For many low-income wage workers, three years is probably a relatively long time in terms of work decisions. As well, the majority of welfare recipients (77.2%) remain on welfare less than three years.[1] While there is evidence and expectation that most of the response will be fully exhibited in three years, we cannot test this directly with the present design.

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[1] A file survey of Winnipeg done during the winter of 1972-1973 to measure how long cases remain on welfare found 17.0% were on welfare for less than 3 months, 18.7% were on welfare for 4-12 months, 26.1% for 13-24 months, 15.4% for 25-36 months, 5.0% for 37-48 months, and 17.8% over 48 months.

VII.2 Saturation Control

A major difficulty in the design of the saturation site component of the experiment concerns the lack of an easily contrived set of control observations. It is important to remember that the primary focus of the saturation component is on the behavioural responses of individuals and families, and not on such effects as might be encompassed by changes in institutions or the economy. A study of community responses to a guaranteed annual income program is not wholly feasible. The current state of knowledge concerning institutional effects, community responses and welfare structures is not great nor perfectly understood. It is uncertain whether a rigorously designed study of macro-effects could be carried out except in a long-life experiment. Additionally, any experimental study designed as a major empirical effort to investigate institutional effects would require a large sample of saturated communities to give statistical confidence to changes detected unless such changes were indeed sizeable.[1]

As well, the saturation experiment involves selection of a single community and its surrounding rural area and the introduction of a single universal GAI for members of that community. Although "controls" may be defined for the saturation experiment, the saturation component of the design is, in some ways, methodologically distinct from the more pure experiment of the dispersed sample. It is, as previously noted, an elegant field experiment -- one which can provide important input

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[1] Myron J. Lefcowitz, "Introduction: Community Effects" in Larry Orr, R. Hollister and M. Lefcowitz (editors) Income Maintenance Interdisciplinary Approaches to Research, Chicago: Markham Publishing Company, 1971.

into the decision-making process regarding GAI socio-behavioural effects and GAI administrative feasibility which probably could not be derived from the more artificial environment of the dispersed experiment.

Unlike the controlled experiment of the Winnipeg dispersed module, no classical control group is possible in the case of the saturation component. In addition, the saturation sample size lacks any variation in the treatment variables. Because of these characteristics, the ultimate analysis must utilize other control observations and modelling methods. The analysis will clearly resemble traditional econometric work, in which a model is constructed to evaluate the influence of some variable of interest which has had a variation over time. The burden of control falls partially on the quality of the model building, and upon using Dauphin as its "own control".

Here change in hours of work from baseline (pre-experimental level) to some periodic (experimental time point) would be regressed on micro characteristics. While macro conditions would have also changed in the interim all sample points would share the same changes. Macro variables would not enter the model -- a weakness shared by the Winnipeg analysis as well.

The Manitoba experiment has augmented the above with the use of the rural dispersed control sites. The problem here, however, is firstly that the data will contain saturation-dispersion as well as treatment-control influences. The saturation-dispersion influence, while interesting, is a contaminant in the control of the "realistic" saturation environment. The Winnipeg-Rural Dispersed-Dauphin design likely

does not contain enough information to isolate the pure rural-urban, saturation-dispersed, and treatment effects and their interactions, so the "contamination" of the control could well be a serious difficulty.

VII.3 The Treatment of Welfare by the Experiment

VII.3.i Types of Welfare Units and Rules Relating to Welfare Units

The experiment distinguishes three different types of families having some connection with welfare; namely, (a) welfare controls, (b) welfare treatment units, and (c) welfare converts. Welfare controls are families selected as control units and receiving either provincial or municipal welfare. These units are treated the same as the other control units. Welfare treatment units are families receiving welfare when selected for enrollment as a treatment unit but chose to receive the Mincome payments instead. Welfare converts are units selected for enrollment as treatment units but chose to continue receiving regular maintenance from welfare rather than receive payments from Mincome.

Families eligible for any of the various welfare programs must choose between Mincome and welfare and are not allowed to be on both programs. Welfare consists of three main types of benefits; regular maintenance, benefits to meet special or other specific needs, plus a variety of services.

Certain rules were adopted by the experiment to deal with the regular maintenance portion of welfare. To this end welfare treatment

families were allowed to continue receiving certain welfare benefits other than regular maintenance. Specifically, these benefits include medical needs, special needs, and emergency assistance payments; however these amounts received are taxed as income by Mincome. For special needs and emergency assistance received, Mincome made case by case decisions whether the amounts should be exempt or counted as income and taxed at the 100% rate.

For programs that are not basically welfare; for example, demogrants such as family allowance, Mincome participants are allowed to receive benefits from these programs and remain on Mincome. Benefits from these programs are taxed at the 100% rate by Mincome so that total benefits are kept to the upper limit of the most generous program from which they receive benefits.

Mincome allowed welfare convert units to enroll on a control-like status, requiring them to file the monthly IRF and receive the ten dollars filing fee. These units did not, however, receive income conditioned payments from Mincome. Like all other enrolled units welfare converts complete the periodic interviews. They retained the right to stop receiving welfare payments and to receive Mincome income conditioned payments without penalty whenever they wish. Similarly, any welfare treatment unit has the right to become a welfare convert unit should they decide to do so.

VII.3.ii The Extent of Welfare Domination

Welfare domination is the term given to certain problems caused by the continued existence of the established income security and income supplementation programs while the NIT experiment is being carried out. Families selected for the NIT experiment continue to remain eligible for these programs but must choose between Mincome and welfare.

In dealing with programs designed to provide full income support, Mincome cannot permit families to participate in both Mincome and welfare programs as the amount they would then receive if they had no income would be approximately twice as much as a similar family not eligible for both programs. Such "stacking" of programs would make the actual support received twice the amount considered to be appropriate for such a family.

A family eligible for programs of income assistance designed to provide full income support will generally choose that program which "dominates" in the sense of providing more income than any alternative program.

If selected families receive more on Mincome than they would on welfare they will usually choose Mincome. When welfare provides more support than Mincome, the family will usually choose to remain on welfare. The eligibility of these families for welfare cannot be removed and families cannot be compelled to accept the NIT program instead of the alternative programs.

Where Mincome dominates all other welfare programs at all relevant income levels -- that is, up to the income level where both

programs stop paying benefits to the family -- there is no problem. When alternative welfare programs dominate Mincome at all relevant income levels, Mincome will lose sample points. When each of the programs dominates the other for part of the relevent income range the situation is mixed and complicated. Families now face a complex structure of alternative support levels and tax rates. Their decisions will depend upon this complex combined structure; not just upon the structure of the program in which they decide to eventually enroll. In order to analyze these cases, one must model the combined tax rate and support level structure. An estimate of the extent of domination of welfare for the three support levels adopted by the experiment is given in the tables below.

The domination problem was found to be almost totally concentrated in the disabled or single-headed family categories due to the eligibility conditions embodied in the provincial welfare system. Any disability serious enough to preclude participation in the labour force was considered grounds for exclusion from the experiment in the dispersed sites. Consequently, only single-headed families present a serious problem.

Tables VII.1 and VII.2 indicate the extent of welfare domination for the two sets of support levels in effect for 1975, the first year of experimental payments. The welfare rates are estimated as at January 1975 and incorporate the structure up to November 1974. The monthly support levels for Mincome in 1975 are set forth in Table VII.3.

[See Tables VII.1, VII.2, VII.3]

Table VII.1

Welfare Domination

(Estimated for January 1975)

Support Levels for January to June 1975

% of Welfare Cases with Budgets of the Indicated Amount or Less		Budget by Size of Family ($ per month) (to the nearest $)							
(%)		1	2	3	4	5	6	7	8+
5		$ 91	$116	$213	$298	$370	$439	$460	$512
10		96	139	246	318	387	459	493	557
15		100	165	256	326	398	474	518	570
20		105	182	265	334	408	479	528	580
25		109	194	273	341	417	492	537	591
30	low support	113	204	278	348	425	498	545	602
35	level	118	209	284	353	431	505	551	611
40		122	215	289	359	437	512	557	618
45		127	220	294	365	442	518	563	625
50		131	226	300	372	448	524	570	634
55		135	231	306	378	453	529	576	642
60		141	235	309	385	460	536	583	650
65		145	241	314	392	466	541	591	662
70	middle support	151	246	320	398	474	548	600	673
75	level	155	253	327	405	483	554	607	696
80		164	262	333	414	492	563	616	717
85		172	270	342	420	503	574	623	735
90	high support	182	282	352	430	515	587	633	765
95	level	197	298	368	444	541	602	650	803
100		354	378	513	559	648	693	694	959

Notes:

1. Family allowance benefits of $20 per child per month are incorporated assuming the number of children for a family of size n is 0 for $n \leq 2$; $n - 2$ for $3 \leq n \leq 6$; and 4 for $n > 6$.

2. No welfare rate changes are included after the November 1974 revisions.

3. Homeowners, institutionalized cases, foster children, and special diet cases are excluded.

Table VII.2

Welfare Domination

(Estimated for January 1975)

Support Levels for July to December 1975

% of Welfare Cases with Budgets of the Indicated Amount or Less (%)		Budget by Size of Family ($ per month) (to the nearest $)							
		1	2	3	4	5	6	7	8+
5		$ 91	$116	$213	$298	$370	$439	$460	$512
10		96	139	246	318	387	459	493	557
15		100	165	256	326	398	474	518	570
20		105	182	265	334	408	479	528	580
25		109	194	273	341	417	492	537	591
30		113	204	278	348	425	498	545	602
35	low	118	209	284	353	431	505	551	611
40	support	122	215	289	359	437	512	557	618
45	level	127	220	294	365	442	518	563	625
50		131	226	300	372	448	524	570	634
55		135	231	306	378	453	529	576	642
60		141	235	309	385	460	536	583	650
65		145	241	314	392	466	541	591	662
70	middle	151	246	320	398	474	548	600	673
75	support	155	253	327	405	483	554	607	696
80	level	164	262	333	414	492	563	616	717
85	high	172	270	342	420	503	574	623	735
90	support	182	282	352	430	515	587	633	765
95	level	197	298	366	444	541	602	650	803
100		354	378	513	559	648	693	694	959

Notes:

1. Family allowance benefits of $20 per child per month are incorporated assuming the number of children for a family of size n is 0 for $n \leq 2$; $n - 2$ for $3 \leq n \leq 6$; and 4 for $n > 6$.

2. No welfare rate changes are included after the November 1974 revisions.

3. Homeowners, institutionalized cases, foster children, and special diet cases are excluded.

Table VII.3

Monthly Support Levels

($ amounts)

January to June 1975

Family Size	Low	Medium	High
1	120.33	152.00	183.67
2	224.83	284.00	343.17
3	278.67	352.00	425.33
4	316.67	400.00	483.33
5	348.33	440.00	531.67
6	380.00	480.00	580.00
7	411.67	520.00	628.33
8	443.33	560.00	676.67

July to December 1975

Family Size	Low	Medium	High
1	139.33	171.00	202.67
2	260.33	319.50	378.68
3	322.67	396.00	469.33
4	366.67	450.00	533.33
5	403.33	495.00	586.67
6	440.00	540.00	640.00
7	476.67	585.00	693.33
8	513.33	630.00	746.67

Note: Families are assumed to have two adults, except size one, and all children are under 18 years of age.