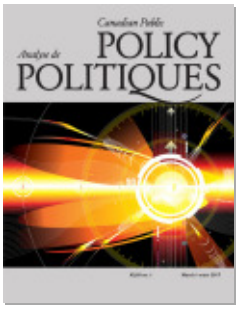
The Manitoba Basic Annual Income Experiment: Lessons Learned 40 Years Later



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COMMENTARY

The Manitoba Basic Annual Income Experiment: Lessons Learned 40 Years Later

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Le lancement récent, en Ontario, du Projet pilote portant sur le revenu de base, et le revenu de base mis en place en Finlande pour les chômeurs sont deux exemples qui reflètent l'intérêt croissant pour diverses formes de revenu annuel garanti (RAG). Il s'agit d'une idée qui circule depuis des décennies, et qui est maintenant ravivée face aux questions que posent les inégalités croissantes et les bouleversements du marché du travail. Or, le Mincom (Manitoba Basic Annual Income Experiment, ou Projet pilote de revenu annuel de base du Manitoba), qui a été réalisé il y a une quarantaine d'années et a constitué une expérience sociale audacieuse, avait justement été conçu pour évaluer les répercussions, en matière d'offre de travail, d'un impôt sur le revenu négatif, qui est une forme de RAG. Dans cet article, les auteurs examinent cette expérience pour clarifier ce que le Mincome nous a permis (ou non) d'apprendre sur les réactions des individus et des ménages qui ont profité de ce revenu garanti. Les auteurs montrent ainsi comment cette expérience peut nous aider à répondre à des questions liées aux projets actuels de revenu garanti ou de base, et expliquent comment les chercheurs peuvent aujourd'hui avoir accès aux données que l'expérience a permis de recueillir.

Mots clés : expérience sociale, Mincome, impôt sur le revenu négatif, revenu annuel garanti, revenu de base, répercussions en matière d'offre de travail

The recent announcements of the Ontario Basic Income Pilot and Finland's cash grants to jobless persons reflect the growing interest in some form of guaranteed annual income (GAI). This idea has circulated for decades and has now been revived, no doubt prompted by concerns of increased inequality and employment disruptions. The Manitoba Basic Annual Income Experiment (Mincome), conducted some 40 years ago, was an ambitious social experiment designed to assess a range of behavioural responses to a negative income tax, a specific form of GAI. This article reviews that experiment, clarifying what exactly Mincome did and did not learn about how individuals and households reacted to the income guarantees. This article reviews the potential for Mincome to answer questions about modern-day income experiments and describes how researchers may access these valuable data.

Keywords: social experiment, Mincome, negative income tax, guaranteed annual income, basic income, labour supply response

> Over the years, but particularly in the 1960s and 1970s, considerable effort has been spent in the examination of this system [of personal income and transfers] and its component programs, with a view to identifying potential improvements in the areas of fairness, effectiveness, efficiency, and simplicity. These interests led to one of the most significant social experiments in Canadian history, namely the Manitoba Basic Annual Income Experiment (Mincome).
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> —Judith Maxwell, chairman of the Economic Council of Canada, in the Foreword to Hum and Simpson (1991, ix). Reproduced with permission from the Minister of Public Works and Government Services Canada, 3 February 2017.

Introduction

Although made in the previous century, Maxwell's comments about redistributive taxation policy continue to resonate today. Emerging concerns about poverty in the United States and Canada in the 1960s led to a wave of social experimentation in the 1970s to evaluate proposals for a negative income tax program to alleviate or eliminate the problem, including the Manitoba Basic Annual

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Income Experiment (Mincome) between 1975 and 1978. These income maintenance experiments provided important evidence of the feasibility, impact, and effectiveness of programs of this nature, providing important lessons about income maintenance policy and attendant issues. Yet these significant policy events did not immediately translate into effective policies to address poverty, which remains an important unresolved social issue.

Although emerging international and Canadian research (Atkinson and Morelli 2014; Fortin et al. 2012) has concentrated on rising income inequality and distributional issues more generally, the related issues around poverty and income support remain an important component of the policy debate. This debate now includes the 2009 recommendation from Quebec's Comité consultatif de lutte contre la pauvreté et l'exclusion sociale for a "revenu minimum garanti" pegged at 80 percent of Statistics Canada's Market Basket Measure (Clavet, Duclos, and Lacroix 2013) and the publication of a proposal for the Ontario Basic Income Pilot (Segal, 2016). The Government of Canada has communicated mixed signals that some form of guaranteed minimum income could be included in discussions with the provinces to tackle poverty and improve Canada's social safety net (Curry, 2016). Mincome provides enduring lessons about the feasibility of such a program, the scope for testing, and what can be learned from such social experiments.

Our article is structured as follows. The next section, "Manitoba Basic Annual Income Experiment," briefly summarizes the Mincome experiment chronologically from conceptualization to completion, considering both its social context and the technical details of its execution while providing links to more extensive discussion elsewhere. The third section, "Demise and Resurrection of Mincome," then reviews what happened after the experiment was abruptly terminated in 1978, including the temporary mothballing of the evidence, the subsequent development and release of the digitized data, and what can be learned about the conduct of large social experiments from that sequence of events. The fourth section, "Mincome Research," summarizes the findings from the Mincome research, including both the early research on work disincentives and marital stability and the subsequent research on the impact of Mincome on broader social outcomes. The "Using the Mincome Database" section discusses the research questions that Mincome can still support and some issues in statistical modelling that researchers need to understand, and it describes how to access these data. The final section, "Old and New Lessons from the Mincome Experiment," presents some key lessons that current social experiments such as the Ontario Basic Income Pilot should consider. Appendix A provides details on the available documentation and data.

Manitoba Basic Annual Income Experiment

Prompted by the analysis of poverty from the Economic Council of Canada (1968), the Special Senate Committee on Poverty (1971) recommended implementation of a federally financed and administered Guaranteed Annual Income (GAI) program to address poverty. The Manitoba government led by Ed Schreyer was receptive to the idea of an experimental project to test such a program and submitted a proposal for funding to the Department of National Health and Welfare in March 1973. The minority federal Liberal government led by Pierre Trudeau, which had called for a review of Canada's social security system in the 1973 Throne Speech, was also receptive to the idea of a trial guaranteed income plan. The two governments entered discussions and reached formal agreement on the budget for a Basic Annual Income Experiment Project in June 1974.[1]

Mincome had two primary goals, the first explicit and the second of which evolved as the experiment progressed. The first goal was to "evaluate the economic and social consequences of an alternative social welfare system based on the concept of a negative income tax" and specifically to "examine the labour supply responses of households and individuals to a guaranteed annual income" (Hum, Laub, and Powell 1979, 1). Over time a second goal, not explicitly stated in the design documents, evolved, which was to understand the administrative and logistical challenges involved in implementing such a system across the population.

Although Manitoba appears to have initially conceived the idea as a modest demonstration or pilot project, Canada and Manitoba eventually agreed that Mincome would be a randomized controlled trial.[2] The precise design was left to researchers engaged by a separate legal entity created to manage the logistics of administering the payments, collect a wide range of socio-economic data from participants and non-participants, and analyze the results. Although sponsorship was jointly federal and provincial, it appears that the federal government focused primarily on the scientific aspects of the experiment, and the province supervised the operational aspects (payments and data collection).

The early design ideas behind the Mincome experiment appear in Hikel, Laub, and Powell (1974) and Hum, Laub, and Powell (1979). Very generally, qualifying low-income households in Winnipeg and rural areas were allocated at random to receive either a treatment of one of several guaranteed income plans or no treatment, to form a control or comparison group. The treatments consisted of an income guarantee of maximum support for families with no other source of income and a tax rate that determined how much of the guarantee would be removed per dollar of family income and net worth. The net benefit also depended on family structure and size.

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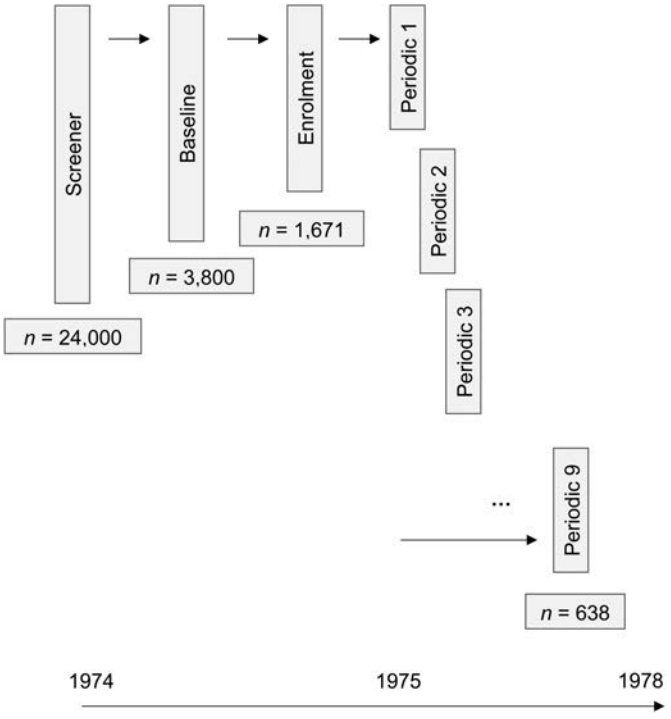

Figure 1: Panel Data Structure of the Manitoba Basic Annual Income Experiment
Source: Mason (2016)

The basic design reflected the approach used by the other negative income tax experiments underway in the United States at the time.[3] It presented the scientific argument for random allocation combined with a control group to allow uncontaminated (econometrically consistent) estimation of behavioural response to the guaranteed income treatments. Labour economists considered the experimental design crucial to providing credible estimates of the impact of the treatments on labour supply response and the important question of work disincentives that dominated the debate over the GAI in the 1970s.

Mincome created a panel data experiment, with waves of interviews occurring over a four-year period. Figure 1 presents the panel data structure of this experiment. The initial screening interviews narrowed eligibility to the baseline survey that collected information intended to populate the experimental design. The enrolment interview was the formal start to the experiment, and the subsequent follow-up interviews, termed *periodic surveys,* monitored labour market interactions and other socioeconomic variables for the households (heads as well as any member aged older than 15 years). Periodic interviews occurred approximately every three months, and all participants also completed monthly income reporting forms (IRFs). Mincome engaged in a very elaborate and comprehensive data collection process that necessitated extensive logistical and quality controls.

The basic design matrix for the main Winnipeg sample appears in Table 1. Note that Plan X was deemed too expensive and never implemented, and because Plan 6 resulted in low uptake at the first payment because of the high tax back and low basic income, participants were rolled into Plan 7 after Periodic Survey 1.[4,5]

The treatment groups in Mincome began with an array of three guarantee levels and three tax rates. The guarantee levels were $3,800, $4,600, and $5,400 for a family of two adults and two children aged younger than 15 years, adjusted according to family size for smaller and larger families.[6] Plan 9 included the control group that received no payments except an honourarium for completing the periodic surveys.

The Mincome agreement also included a unique provision for a pilot project in Dauphin, where everyone would be eligible for a single specific treatment (Plan 3). This saturation site did not involve random allocation of treatments or a control group but was advertised as open to all who qualified. The intent was to provide additional evidence of "administrative and community issues resulting from a less artificial environment" than the "dispersed samples" (Hum and Simpson 1991, 45) of a small fraction of Winnipeg and rural Manitoba communities.[7] Thus, the Dauphin saturation site was initially seen as a way to test uptake if a GAI were universally implemented. Other social scientists viewed the Dauphin site as an opportunity to study community and family dynamics when everyone was eligible and awareness of

Table 1: Mincome Guarantee Levels and Tax Rates by Plan Type (Just Before Periodic Survey 1): Main Winnipeg Sample

	Tax Rate on Total Income		
Guarantee at enrolment, $	35%	50%	75%
3,800	Plan 1 ($n = 55$)	Plan 3 ($n = 61$)	Plan 6 ($n = 49$)
4,600	Plan 2 ($n = 67$)	Plan 4 ($n = 70$)	Plan 7 ($n = 29$)
5,400	Plan X	Plan 5 ($n = 56$)	Plan 8 ($n = 45$)
		Plan 9 (Control; $n = 94$)	

Note: Mincome = Manitoba Basic Annual Income Experiment.
Source: Hum, Laub, and Powell (1979), Laub, Metcalf, and Sabourin (1979), and authors' calculations from Mincome payments data (Appendix B).

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who was receiving the support was high. In contrast, participants in the Winnipeg and rural dispersed sites were essentially anonymous.

The guarantees were adjusted for inflation, which was substantial during the period of the experiment.[8] The tax rates at which the income guarantee would decline with family income were 35 percent, 50 percent, and 75 percent, resulting in nine prospective plans. In the design process, the most generous plan (Plan X; $5,400, 35 percent) was dropped because cost considerations limited its policy relevance (Hikel et al. 1974, 27). As mentioned, the least generous Plan 6 ($3,800, 75 percent) had insufficient take-up and was folded into a more attractive Plan 7 ($4,600, 75 percent) after the first year of the experiment (Hum, Laub, Metcalf, and Sabourin 1979, 46), leaving seven treatment plans plus a control group to be assigned across the urban dispersed (Winnipeg) site.[9] The smaller rural dispersed sample was restricted to a control group and a single treatment, Plan 3 ($3,800, 50 percent), aligned with the Dauphin saturation site treatment. The rural dispersed control group could therefore provide a useful comparison group for the Dauphin treatment sample under certain conditions.[10] We discuss this further in the fifth section of the article.

The first stage of the experiment involved the identification (screening) of appropriate low-income families and their allocation to a treatment or a control group. This was a relatively simple exercise in Dauphin, where every family resident in that town as of 1 July 1974 was eligible for a single plan, subject to age and normal income ceilings of age 64 years and $9,000 for a family of four, and 586 families were eventually enrolled according to payments data (Crest et al., 1979, 7).[11] For the dispersed Winnipeg and rural samples, a screening survey followed by a baseline survey was conducted to collect data on earnings, work history, and income for families and unattached individuals. Information on earnings and work history from the baseline survey were used to estimate normal or permanent income to identify admissible low-income families and to determine initial payments (Hum, Laub, and Powell 1979, 47–48). Admissible families excluded those with an average annual income, adjusted for family size, that exceeded $13,000 for a family of four (equivalent to $64,600 in 2016) plus families with a head older than age 57 years, a language barrier, or disabilities; families in the armed forces or a religious order; or families in rooming house arrangements (Sabourin 1979, 5).

Because allocation of families to treatment plans or the control group had implications for the variation in outcomes across plan features and for program costs that depend on family income as well as treatment, the experiment used an assignment model developed by Conlisk and Watts (1979) for the New Jersey negative income tax experiment. The model, which introduces some data complexity, as we discuss in the fifth section, determined the optimal allocation of admissible families to the experimental treatment and control groups to achieve accurate estimation of a response (such as labour supply) given the design features (such as the plan features and the family's normal income), a budget constraint, and other sampling and policy constraints. A total of 1,255 families were allocated to the experiment at the urban and rural dispersed sites, 704 to the treatment plans and 370 to the control group in Winnipeg, 103 to the treatment plan and 78 to the control group in the rural sample (Hum, Laub, Metcalf, and Sabourin 1979, 63–64).

An important feature of all the income maintenance experiments was the reliance on self-reported data. Surveys, administered in person, were the prime data collection methods, because the experiments predated the availability of administrative files. This is in sharp contrast to the current potential to access administrative information such as tax and health records to support policy experimentation today.

Families enrolled in the experiment[12] were interviewed three times a year for three years, for a total of nine periodic surveys in addition to the baseline survey, as depicted in Figure 1. The periodic surveys collected a wide variety of information. Although labour supply was uppermost in the minds of policy-makers and a top priority for Mincome, the surveys also collected extensive information on related labour market topics—earnings, job search, human capital investment, unearned income, disability and unemployment benefits, and leisure time use—as well as assets and liabilities, family composition, geographic mobility, housing arrangements, and socio-psychological factors, including attitudes toward work, perceived standard of living and quality of life, and housing satisfaction.[13]

During the first year of the experiment, as the extent of attrition and non-participation became apparent, it was determined that a supplementary sample would be necessary to provide sufficient observations in certain treatment cells, primarily the lower income cells for double-headed families in Winnipeg. The supplementary sample enrolled an additional 196 families across the treatment cells and 97 families in the control group (Hum, Laub, Metcalf, and Sabourin 1979, 65), raising total enrolment in the Winnipeg dispersed sample to 900 treatment and 467 control families, or 1,367 families in total. The Supplementary Sample lagged the original sample by one year because the baseline survey for the Supplementary Sample was completed at the end of 1975 and the first periodic survey for the Supplementary Sample roughly coincided with the fourth periodic survey of the original sample shown in Figure 1.[14]

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Table 2: Selected Participant Attributes at the Baseline Survey

Attribute	Mean
Average Age Male Head, y	36.5
Average Age Female Head, y	34.5
Households Owning Their Home, *n*	801
Average Home Value, $	8,000
Average Rent Paid (Monthly), $	92
Number Single Headed Households	396
Average Family Income, $	7,520
Median Family Income, $	6,270

Source: Authors' calculations based on Mincome baseline summary file (see Appendix B).

Table 2 presents summary baseline statistics for the experimental participants (those in both treatment groups and the control group) for the three sites (Dauphin, Winnipeg, and rural sites). Remember these are 1974 dollars.

The payments mechanism was a critical feature of the experiment administered through a distinct payment agency, Mincome Manitoba, with offices in Winnipeg for the two dispersed samples and in Dauphin for the saturation sample (Crest et al. 1979, 10–15). Beyond being an experiment, Mincome was an incorporated entity. Those who agreed to participate went onto the Mincome "payroll," receiving a monthly income corresponding to the guarantee plus any additional income from pensions, work, and so forth. Payments from Mincome Manitoba were provided monthly on the basis of completion of an IRF.[15] Those who were receiving social assistance and other income support benefits before the experiment were assured that their monthly income would not drop and that their eligibility for these benefits would remain if they subsequently decided to withdraw from the experiment.

On the basis of family composition (number of heads and number and age of dependents), income (Y) and wealth (W) recorded on the IRF, and payments P to those in treatment cells identified by an experimentally assigned guarantee G and tax rate t were calculated according to the formula[16]

$$P = G - tY - rW. \quad (1)$$

Although intended to be a simple benefits calculation based on reported income, consistent with the classic negative income tax design, adjustments to Equation (1) were necessary. First, any federal or provincial income support payments received—such as Unemployment Insurance, Canada Pension Plan or Old Age Security benefits, and War Veterans Allowances—were deducted from the monthly payment. Second, there was a fixed payment adjustment at rate r for family net worth across all treatments, such that net worth below $3,000 was not taxed, but net worth between $3,000 and $10,000 was taxed at 4 percent, net worth between $10,000 and $30,000 was taxed at 8 percent, and net worth in excess of $30,000 was taxed at 16 percent.[17] Third, there was an annual reconciliation for overpayments and underpayments, including regular federal and provincial taxes owing, based on the calendar year (Hum, Crest, and Komus 1979, 26–28).[18] Mincome Manitoba prepared the tax returns of all treated families in the experiment.

A family in a particular treatment cell, assigned a specific guarantee on the basis of family composition and a specific tax rate, would receive payments as long as its eligible income fell below the break-even level B given by the ratio of the guarantee to the tax rate, G/t. Thus, a family with fluctuating income might have a Mincome payment one month but not the next when income exceeded B. This potential for a fluctuating level of support distinguishes negative income tax forms of the GAI from other approaches that make fixed payments.[19] Other changes in payments could occur because of changes in family composition, which would alter the reporting unit and the level of treatment. For example, a double-headed family that split into a lone parent family and an unattached individual would retain its treatment cell, but an adjustment to the guarantee level would be made to reflect the change in family composition of the two new families. Family unions, such as the case of an unattached individual or lone parent joining with another lone parent in a specific treatment plan, would be able to form a new unit with the specified treatment plan after a suitable waiting period (Hum, Crest, and Komus 1979, 12). Moreover, some families did not complete the experiment, and their payments were stopped.[20]

That, in the proverbial nutshell, was the Mincome experiment from conceptualization to completion. Those seeking further details should consult the Mincome experimental papers listed in Appendix A.

Demise and Resurrection of Mincome

The original proposed budget was $17 million, with the federal government responsible for three-quarters of it. Canada and Manitoba were jointly responsible for all issues related to research, and Manitoba assumed the remainder of the cost and sole responsibility for operational matters (data collection and administration of payments). Because Canada had no experience with social experimentation and only a distant view of the unfolding US trials, it would hardly be surprising if the budgeting exercise rested on shaky ground. What is more surprising today, when provisions for cost overruns are routine even for commonly occurring public projects, is that the $17 million became a hard-and-fast number as the experiment proceeded and the full cost

of completing all its features in an inflationary environment became apparent. As a consequence, Mincome was scaled down to preserve its highest priority functions, as we discuss later.

In 1978, the federal government expressed concerns about the cost of the project. Other factors may have weighed in its decision to withdraw from the experiment because the research began to incorporate a range of social and psychological substudies that diverged from its original purpose. Manitoba had also become less keen to maintain the experiment, and with the loss of its major financial partner, it agreed to terminate data collection and analysis. The decision to terminate resulted in a layoff of most of the staff; at its peak, Mincome employed about 200 full- and part-time employees, most engaged in payments administration, interviewing, and questionnaire processing. This termination was very stressful for all Mincome staff, who were quite passionate about the experiment and the GAI as a policy.

In the final year, the remaining staff, primarily research and computer programmers, completed the technical documentation and file digitization to support future analysis. It is important to note that during the operational phase from 1974 to 1978, most Mincome staff became engaged in the logistics of income maintenance, tax reconciliation, and managing a complex survey research process to gather detailed information on household behaviour and attitudes. Very little analysis was completed on the central labour supply hypotheses that formed the original purpose of the study. By 1979, Mincome staff had completed technical documentation and had coded the selected survey information electronically.[21] The technical documents, which are essential reading for anyone wanting to use the longitudinal data, are listed in Appendix A. The hard-copy records, consisting of completed surveys and voluminous payment and taxation records as well as computer tapes, were transferred to the federal government. The state of transferred hard-copy records remains chaotic to this day.

The fact that no analysis emerged after a multimillion-dollar expenditure was clearly an embarrassment to the government. In 1980, discussions started between the federal government and the University of Manitoba to develop a process to retrieve the Mincome data and make it accessible to the research community. These discussions culminated in the creation of ISER at the University of Manitoba in 1981. The federal government provided five years of funding to ISER, in part to restore the Mincome data and promote its use to the research community.[22] Work started immediately on the task of converting the tape format (PDP-11) used by Mincome to a more conventional IBM 360 format. It immediately became apparent that although the technical documentation (see Appendix A) had been completed to a very high standard, the computer files were undeveloped and not readable on mainframe equipment. Code books were rudimentary and aligned poorly with the electronic fields. A team from the Department of Computer Science at the University of Manitoba provided the key support that eventually cracked the data codes and reconstructed the Mincome survey data into an electronic format usable by researchers.

By 1983, all the relevant data had been reconfigured with detailed code books to support analysis. ISER undertook a communications process through 1984 to outline and promote the research possibilities to the research community. Between 1985 and 1991, a variety of research studies were completed, but this was followed by a quiet period, with occasional studies throughout the years.

By the mid-1980s, Mincome data development ceased. First, the ISER evolved into a for-profit research company, University of Manitoba Research, wholly owned by the university. This new entity had no responsibility for the Mincome digital files, technical documentation, and working papers, which were transferred to the University of Manitoba Library Archives. Second, interest in a GAI waned. With the cessation of the US income maintenance experiments and the completion of key studies from Mincome, the policy community concluded that implementation of this form of income support was unlikely in the foreseeable future. Third, the 1980s and 1990s witnessed relatively high growth in real incomes in North America (Brzozowski et al. 2010), and the issue of income inequality receded as a policy of interest.[23]

By 1990, the Mincome project had effectively ceased. At some point, the libraries at the University of British Columbia and University of Toronto secured the Mincome data and code books. Although the presence of the data and documentation at these sites may have served to remind the research community of the Mincome experiment and the potential value in its data, little research has resulted. In Spring 2016, the University of Manitoba Libraries "repatriated" the Mincome data and supported their installation in a Dataverse system.[24] All the relevant documentation and digitized data are now available in one location, accessible to the research community worldwide.[25]

Mincome has come through two cycles of rebirth since the experiment was terminated in 1979. First, the data were rehabilitated by ISER, supporting the realization of some key research on labour supply before slipping into obscurity in the early 1990s. In this version, the data remained on large data tapes and required mainframe computing to support the processing, but they were not available to the research community after 1990. Second, in the past five years, the data have been translated into Excel format, which substantially improves access. The installation of Mincome in the Dataverse

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system at the University of Manitoba means that researchers anywhere have access to the data and all relevant documentation, and they can process the information conveniently. The fifth section of this article summarizes the access point for Mincome in its current incarnation.

Mincome Research

Lack of resources for research in the Mincome budget did not shelve analysis of the experimental data. Research flowed steadily, if more slowly and with more limited exposure, after the development of the Mincome database at the University of Manitoba's ISER. In this section, we outline the published research to date in three parts that correspond roughly to the chronological order of the research undertaken. First, we review research using the Mincome baseline surveys, which constitute the earliest and most straightforward data from the experiment. We then examine the research using the panel data from the Winnipeg dispersed sample, which concentrated on addressing the labour supply response issues that were an important priority in the design of Mincome. Finally, we consider the more recent research related to the Dauphin saturation site and the rural dispersed sample.

As a pre-experimental cross-sectional survey, the baseline survey provided the earliest and most easily understood and analyzed data from Mincome. Although the data do not pertain to the experiment per se, they are part of the Mincome database and provided a rich set of details on labour supply and work histories, as well as a variety of other economic and sociodemographic topics, at a time when such data were in short supply.[26] Mason (1984) outlines the income and wealth data available from the Mincome baseline, the distribution of net worth across family types (double headed, single headed, and single individuals), and the relationship between income and wealth for these family types. He finds an inverted U–shaped relationship with age that is consistent with the life cycle model of household wealth accumulation for double-headed families in both the urban and the rural samples but not for single-headed (lone parent) households. The results are stronger when home ownership is included in the calculation of net worth, because housing is the dominant form of wealth accumulation for the lower income sample of families in Mincome.

Ehnes and Simpson (1983) use the labour supply and work history data in the Mincome baseline survey to analyze the labour supply behaviour of men in double-headed families. They estimate a standard family labour supply model of hours worked that depends on the wage of the male and female heads, unearned income, age, education, number of dependent children, and race. Prescott, Swidinsky, and Wilton (1986) also use the rich Mincome baseline data to study the labour supply behaviour of women in lower income households. They estimate a simultaneous model of wage offers and labour supply that allows for non-random selection of workers and non-workers. Wage offers depend on education, age, and location (Winnipeg, Dauphin, or rural), whereas average weekly hours worked depend on own wage, husband's wage (if household is double headed), other income, wealth, children (three age categories), and location. In addition, because the Mincome baseline captures child care costs, Prescott et al. subtract these costs from the gross female wage and find that this net wage variable improves model performance. Consistent with theoretical expectations, the female wage is positively correlated with hours worked, and other income, presence of preschool children, and husband's wage (for double-headed households) are negatively correlated.

At the heart of the justification and design of Mincome and the US experiments was the goal of estimating the labour supply response, or work-disincentive effects, associated with a negative income tax program involving an income guarantee and a benefit tax rate. It was therefore crucial to analyze the panel data for the dispersed samples, which provide rich information on labour supply response in the context of a series of treatments with differing guarantees and linear tax rates as well as a control group. Using standard academic resources and grant support, including the Social Sciences and Humanities Research Council and the Economic Council of Canada, Hum and Simpson (1991, 1993) provide an analysis of labour supply response in the Winnipeg dispersed sample. We consider this research in two stages, first considering the analysis of the direct impact of the experimental treatments and then discussing the analysis of labour supply elasticity estimates provided by the Winnipeg data.

The virtue of random allocation of participants to treatment and control cells is that it is possible in principle to obtain direct estimates of the impact of the experimental treatment in comparison with those receiving existing public income support in the control group. The basic model explains the change in labour supply (hours worked), Δh, between some experimental period and the pre-experimental baseline as a function of the set of experimental treatments T, appropriate control variables Δx that are not fixed through time, and random error $\Delta\xi$ in an analysis of variance framework (Hum and Simpson 1993, S276–77):

$$\Delta h = T\beta_T + \Delta x\beta + \Delta\xi \tag{2}$$

The experimental treatments ensure variation in guarantee levels and tax rates over time for the same individual as well as across individuals, depending on

their experimental assignment, to produce more efficient estimates of the experimental effects β_T. The control variables might include time effects and variables to capture potential bias arising from the Conlisk-Watts assignment model (Keeley and Robins, 1978) or participation and attrition (Robins and West, 1982).[27] Using a variety of specifications, Hum and Simpson (1993) find the overall effects of the Mincome experiment to be modest in the Winnipeg sample—1 percent for men, 3 percent for wives, and 5 percent for unmarried women—and statistically insignificant when time effects are controlled. These effects are smaller than the weighted average of effects from the US experiments—6 percent for husbands, 19 percent for wives, and 13 percent for single mothers (Burtless 1986; Robins 1985)—although only the effects for the Seattle and Denver experiments are statistically significant, and many of the estimates from the New Jersey and Gary experiments cited in these reviews are comparably small.[28] Comparison of the experimental results along these lines is complicated because treatments differ between the US and Canadian experiments. These summary comparisons can only convey some sense of the impact of a typical or average experimental plan on hours worked rather than the effect of any specific guaranteed income treatment.

Moreover, these direct or non-structural analyses of experimental impact cannot be compared with the non-experimental evidence on work disincentives, which is based on analysis of structural labour supply models. Keeley (1981) argues that general social policy analysis, and assessment of the impact of any proposed income maintenance policy on labour supply, will rely on structural labour supply models that can be estimated with either experimental or non-experimental data. In particular, the experimental evidence can be used to estimate the impact on labour supply of variation in unearned income and after-tax wages arising from the treatment guarantee and tax rate. That is, for a change in after-tax wages of Δw and a change in unearned income of Δy, the standard labour supply model implies that one can represent the experimental treatment variables in Equation (2) by variables that reflect the change in wages Δw and the change in income $h\Delta w + \Delta y$, where the latter term consists of the change in unearned income combined with the change in earned income that arises from the wage change measured at pre-experimental hours worked h (Hum and Simpson 1993, S277):[29]

$$\Delta h = \Delta w \eta_s + (h\Delta w + \Delta y)\eta_y + \Delta x\beta + \Delta\xi. \quad (3)$$

Although this formulation can be used for any panel data that capture hours worked, wages, and other family income, the experimental evidence has particular advantages in generating substantial exogenous changes in after-tax wages and other income as well as imposing linear tax regimes for those treated that simplify and potentially improve estimates of the crucial behavioural parameters, the own compensated wage effect η_s and the income effect η_y.

Hum and Simpson (1993) estimate variants of Equation (3) that include changes in the number of preschool children and time effects.[30] As might be expected from the non-structural estimates, the estimated labour supply elasticities are small and often insignificant. These results are not dissimilar to those obtained for the New Jersey and Gary experiments, although they show that US elasticity estimates were generally larger and more often consistent with theoretical expectations. The experimental evidence, along with prominent critiques by Mroz (1987), Jakubson (1988), and MaCurdy, Green, and Paarsch (1990), have led to both a sharpening and a lowering of labour supply estimates that has endured to this day.[31]

Although concerns about the work disincentives of a guaranteed income program were paramount, there were also fears that the program might provide adverse incentives to break up families. These fears were fuelled by Groeneveld, Tuma, and Hannan (1980), who found that those receiving the guaranteed income treatments in the Seattle and Denver experiments experienced a significantly higher marital dissolution rate relative to the control group. Hum and Choudhry (1992) and Choudhry and Hum (1995) argue that there are counteracting effects of a guaranteed income plan and that the issue is ultimately an empirical one. They apply path and survival analysis to the Mincome data and find contrary evidence that the treatment group experienced slightly greater marital stability than the controls, results that align better with the reanalysis of marital dissolution in the Seattle and Denver experiments by Cain and Wissoker (1990).

Bennett (1986) analyzes how well participants understood the Mincome program, based on a small sample of respondents to specific questions about knowledge of the impact of the guarantee, tax rates, and break-even income applicable to their circumstances. He finds that most participants who completed the experiment were generally aware of the impact of the Mincome parameters and how they affected their economic situation.

The saturation site in Dauphin remains a unique feature of Mincome. It was introduced after the main project was underway and was intended to assess administrative issues and community effects in a fashion not possible from the dispersed experimental sample design in Winnipeg and rural areas.[32] Although researchers ignored these data for the analysis of labour market impacts, the Dauphin experience has figured more prominently in recent research and media reports, largely through the innovative work of Evelyn Forget (2011, 2013). Her work centres on the impact of a GAI on health behaviour and outcomes. Because Mincome collected no health-related data, even self-reported health status, she relies on a quasi-experimental approach based

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on administrative health records to estimate the impact of the Mincome program in Dauphin. The underlying assumption is that the Dauphin saturation site and all its inhabitants can be used as a treatment area and contrasted with counterfactuals drawn from the remainder of the population. Using administrative health records spanning the Mincome payments period, Forget compared health behaviour and outcome variables of individuals residing in Dauphin and selected comparison areas. Propensity score matching refines the data points within the treatment group and the comparison group to improve the orthogonality between treatment and controls. Forget then estimates difference-in-differences regression models of the effect of residing in Dauphin, as opposed to residing in the comparison areas, for hospitalization rates and physician claims overall and for the subcategories of accidents and injuries and mental health diagnoses. This quasi-experimental approach estimates that Mincome accounted for an 8.5 percent reduction in hospitalization rates, concentrated in accidents and injuries and mental health diagnoses, and a reduction in physician claims for mental health disorders.

Forget (2011) also followed up on evidence from Hum and Simpson (1991) of larger work disincentives for tertiary workers by looking at high school completion rates to see whether young men were leaving the workforce primarily to invest in human capital. She also considers possible impacts on fertility, birth weight, and family dissolution but finds no evidence of a Mincome effect. Forget's research has served both to highlight the Mincome experiment and to raise the possibility that a GAI could result in important health outcomes.[33]

Calnitsky (2016) and Calnitsky and Latner (2016) have recently used the data gathered from Dauphin and the rural dispersed sample to investigate community attitudes and community effects on labour supply. Calnitsky (2016) concludes that Mincome was viewed differently, and decidedly more favourably, than welfare because it did not single out or stigmatize participation, a result that he suggests should strengthen social acceptance of a guaranteed income program. Calnitsky and Latner (2016) use the Mincome baseline and payments data to analyze the impact of the Dauphin treatment on labour force participation compared with the dispersed rural sample. Their basic difference-in-differences analysis estimates the total Dauphin treatment effect as an 11.3 percent decline in participation in Dauphin relative to a rural control group. The Dauphin social interaction effect is estimated as the difference between the Dauphin treatment effect and the rural treatment effect, which is 3.2 percent, or about 30 percent of the total Dauphin treatment effect.

Using the Mincome Database

University of Manitoba Libraries now maintains a Dataverse system for all Mincome-related materials including the technical manuals prepared by Mincome staff in 1979, working papers prepared by ISER between 1982 and 1986, the code books for the six Mincome data files that are available for analysis, and the Mincome data in Excel format. Appendix B presents an overview of the Mincome code books and data files held at the University of Manitoba, along with specific links to the websites. The most important document to consult before proceeding with any analysis is the *Mincome User Manual.* This document offers important information on the experiment and summarizes the 13 technical manuals.

By far the easiest data set to use is the baseline set.[34] These data are from a cross-sectional survey that clearly separates the three sites, with interviewing occurring over a relatively contiguous time frame of six months. The remaining data sets are all longitudinal files, featuring 11 recurring sets of variables corresponding to the 11 survey periods. The data are all in Excel files and, aside from the baseline data, present all of the periodic surveys. A unique family number serves as the key to concatenating the data sets.

Those seeking to use the data will need to consider four issues:

1. Variable construction is complex and pertains to institutional arrangements in 1974. For example, both the provincial and the municipal governments delivered social assistance at that time. This arrangement has since been amalgamated into a single provincial delivery system. Users need to be able to step back and interpret variables in the context of the times.
2. Second, the longitudinal files feature recurring sets of variables corresponding to each survey. It is important to note that the time between surveys may vary for any particular respondent, and the surveys overlapped. In other words, two respondents could be interviewed simultaneously for two successive surveys. Mincome staff endeavoured to maintain separation and fixed time sequencing of interviewing, but the vagaries of field operations interfered with these plans to some extent.
3. Although we have only mentioned this in passing, researchers will need to integrate the supplementary and main samples. Modecai Kurz, who provided an assessment of the Mincome sample (Kurz, 1979), considers the inclusion of the supplementary sample as essential to supporting efficient estimation based on the Mincome data.
4. Perhaps the most challenging aspect of the analysis of the Mincome panel data is potential sample

selection bias. In many ways, the data issues in Mincome are similar to those of the US experiments of the time and other non-experimental panel data that have been collected. Yet, although the rates of non-completions, refusals, and attrition between Mincome and the US experiments are similar (Kurz, 1979), less attention has been paid to sample selection bias arising from attrition in the Mincome research than in some of the US experiments, and new analytical techniques have emerged. We now review this specific challenge in a little more detail.

In general, sample selection bias in longitudinal experiments or surveys can arise before or during the experiment. Before data are collected, researchers must carefully select a sample of participants, and some of these participants may refuse, leading to possible bias from noncoverage at the sample selection stage. Sample selection bias due to noncoverage may be the fault of the experimental designers, or it may arise from the behaviour of participants if they refuse to be included in the study.

Sample selection bias at the outset, due to noncoverage, can arise if those selected and initially enrolled in the experiment are not a fair representation of the population. Avoidance of this issue requires a carefully designed experiment. The Mincome experiment has been lauded for its careful and meticulous experimental design (Hum, Laub, and Powell 1979; Kurz 1979), and selection bias at the experimental design level was thought to be unlikely. Mincome's baseline interview was relatively more complicated and provocative than that of other income maintenance experiments, however, and its refusal rate of 16.1 percent was high compared with the 6.9 percent refusal rate for the New Jersey experiment (Kurz, 1979). Future income experiments should understand the trade-off between an additional interview question and a potential sample selection bias.

During an experiment, sample selection bias may also arise because of attrition; participants exit experiments for a variety of reasons, including moves, deaths, and refusals. In longitudinal surveys in general, it is the norm rather than the exception that participants will drop out at various stages. As such, the statistical methods for dealing with the loss of observations in panel data are now well developed, unlike at the time of the Mincome experiment. We therefore consider the sources of sample selection bias and the likelihood of its presence in the Mincome experiment as well as potential tests and methodological options to address the problem.

The exit of participants, at any stage of the experiment, can be considered a problem of missing data. A critical consideration in dealing with missing data is whether the reason for missingness is random or non-random. The data may be missing completely at random (Little and Rubin 1987), in which case the problem is "ignorable" (Griliches 1986). In this situation, a simple approach is complete case analysis, which throws away incomplete observations. Although this was a common way of proceeding at Mincome's inception, it induces a loss of information that translates to a decrease in precision and efficiency. Contemporary methods seek to recover the loss of information by using partial observations. In panel data, this situation has been dubbed the "unbalanced panel," as in Baltagi (2013).

It is unlikely, however, that the missing data in Mincome are missing at random, and many studies involving similar data would agree, as we discuss later. If the data are not missing at random, then attention must be given to the mechanism that causes the "missingness," otherwise estimation may be biased (and inconsistent). First, we consider the case of selective attrition, which is more easily treated because there is more information on the characteristics of the "attriters" than on those who exit the experiment at the screening or baseline survey.

As with the other income maintenance experiments, there was considerable attrition in Mincome (Sabourin, 1979). The first year of Mincome saw 17.6 percent attrition resulting from refusals and 3.3 percent resulting from moves in Winnipeg. In the second year, 8.1 percent attrition resulted from refusals and 4.1 percent resulted from moves (Kurz, 1979). In total, 224 respondents of 591 left the experiment over the 11 surveys, with a similar pattern across the other sites. Although these attrition rates are higher than those of the other income maintenance experiments of the time, they are comparable to the attrition rates in many other familiar surveys, such as the Panel Study of Income Dynamics, the Current Population Survey in the United States, and the British Household Panel Survey.[35]

Recall that if attrition is random, then the missing observations are ignorable. One test for ignorability is a test of a difference in characteristics between attriters and non-attriters. Kurz (1979) attributes attrition rates resulting from the payment package, the enrolment process, mobility, and the organization of Mincome and argues that there is no clear "bias-creating pattern of attrition and refusals" (23), but the literature on attrition bias was not well developed at that time. A preliminary logistic regression of attrition on socio-economic characteristics using the baseline survey suggests that attrition is indeed non-random and is potentially non-ignorable. For example, the probability of attrition appears to be higher the more adults (excluding heads) there are in the family and lower the more children there are aged younger than six years. Not surprisingly, attrition probability also appears to be lower the more generous the benefit plan is.

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There has been considerable research on dealing with sample selection for panel data, beginning with Hausman and Wise (1979).[36] The Hausman and Wise paper was motivated by the Gary Income Maintenance Experiment and is pertinent for modelling the Mincome data, providing a basis for both testing and estimation procedures. The literature stemming from Hausman and Wise now provides several options for testing and modelling attrition bias. For example, Verbeek and Nijman (1992) and Wooldridge (1995) provide methods for the detection and correction of attrition bias. Nicoletti (2006) argues that testing for attrition bias is valid only when additional information is available on the reason for missingness, which is the case for the Mincome data because of the information contained in the baseline survey. We refer anyone interested in a more extensive treatment of this topic to Baltagi (2013).

The supplementary sample of 293 households presents an additional opportunity to test and correct sample selection bias resulting from attrition (Hirano et al., 2001). In anticipation of attrition, contemporary longitudinal studies use refreshment samples, and the data become a rotating panel. Kurz (1979) formally tests for demographic differences between the supplementary and original samples and concludes that the two samples come from the same population. Hence, methods for rotating panels (Baltagi, 2013) incorporating the supplementary sample should be applicable to the Mincome panel data.

We now turn to the other possible sources of sample selection bias: non-completions and refusals of the screening interview and non-completions, moves, and refusals of the baseline interview. Only 71.0 percent of screening and 54.5 percent of baseline interviews were completed. This is a potentially serious problem because the methods for correcting sample selection bias rely on at least some information about the missing participants. Furthermore, the validity of tests for attrition bias will rely on the information contained in the baseline interview. Kurz (1979) argues that the reasons for incomplete or absent screening and baseline interviews and the reasons for attrition are similar, in which case information on the attriters can be used to correct sample selection bias resulting from not only attrition but also incomplete and absent screening and baseline interviews. This provides another possible avenue for future research using Mincome.

Although the primary concern is correcting for participants who are absent from the sample (unit non-response), a secondary concern is dealing with incomplete interviews (item non-response). Discarding incomplete interviews would at the least be inefficient but could result in biased and inconsistent estimation for the same reasons associated with non-random attrition. For the incomplete interviews to be usable, either the missing values need to be imputed or the complete observations need to be weighted. Fortunately, the literature on item non-response has flourished since the time of Mincome, and many methods are available for handling it. Two popular broad approaches are maximum likelihood and multiple imputation. We refer anyone interested in a definitive overview of this issue and its treatment to Allison (2001).

Finally, it is important to note that the presence of sample selection bias depends critically on the estimation goal. If the reasons for attrition are independent from, or weakly related to, the outcome of interest, bias will be non-existent or minimal. Consider the specific goal of estimating the labour supply response to different guarantee–tax combinations. The impetus for the Hausman and Wise (1979) investigation of attrition bias was the idea that participants who benefit more from the experiment are less likely to exit, thereby potentially overstating the treatment effect. They found evidence of selective attrition but found that it had little effect on labour supply response in the Gary experiment. Similarly, Robins and West (1986) find little effect of attrition bias in the Seattle and Denver experiments.

In other contexts, attrition bias is minimal as well. Lillard and Panis (1998) find selectivity in attrition behaviour in the Panel Study of Income Dynamics, but the resulting biases are minimal. A similar juxtaposition between attrition selectivity and minimal attrition bias has been found in a variety of panel data (Fitzgerald, Gottschalk, and Moffitt 1998; van den Berg and Lindeboom, 1998; Zabel, 1998), and there is very little research to suggest otherwise. Thus, there is certainly a precedent for ignoring sample selection bias and treating the Mincome data as an unbalanced panel, but we encourage a thorough treatment of the data using modern empirical methods that would entertain the possibility of sample selection bias.

Although there are well-developed statistical methods for dealing with sample selection bias, these methods can only go so far. Future income experiments should recognize that the more obtrusive the data collection, the more likely it is that participants will refuse the experiment at various stages and that these refusals will be non-random. The risk of each interview or survey question should be carefully weighed, although this risk may be mitigated through the use of administrative data, as mentioned in the following section. A blanket of questions may increase the breadth of research possibilities, but it may also invalidate all of them as a result of sample selection bias.

Old and New Lessons from the Mincome Experiment

Mincome offers researchers important lessons about large-scale social experiments. We separate these lessons into conceptual, operational, and strategic categories.

Conceptual Lessons from Mincome

Many studies have examined the challenges of social experimentation in the wake of the income maintenance experiments that included Mincome.[37] A core issue is that participants know they are part of an experiment and will confront their pre-experimental environment at its conclusion. Mincome participants were aware that program benefits would only last three years, which limits what can be learned about the long-term labour supply responses associated with a permanent GAI.[38] Thus, measured labour supply response may be either understated or overstated. Individuals who elect to withdraw from the labour market, typically those close to retirement or who require major skills upgrading or who have become disabled, may decide that the GAI is an attractive option. Alternatively, as long as the guarantee is not overly generous, recipients may elect to upgrade skills and try to make the leap to well-paying employment. Short-term income maintenance experiments or pilots offer limited insight into these opposing reactions.[39] In addition, the net outcome of individual labour supply responses may have effects on the wage structure of the labour market. For example, if low-skilled workers can leave part-time and informal work, wages for these occupations may rise, and this development would likely have a ripple effect on higher skilled occupations. The dispersed sampling design of the Mincome and US experiments offers no insight into these general equilibrium effects. Similarly, there is no information in Mincome or the other income maintenance experiments that would allow researchers to assess longer term outcomes, especially important issues around income and health outcomes.

As with the other income maintenance experiments of the 1970s, Mincome relied solely on survey data to screen, enrol, and monitor participants. This has the important benefit of allowing researchers to define variables precisely to collect the information needed to test the essential elements of a policy, but reliance on survey data introduces a range of sampling and response issues that need to be addressed, as we discussed in the previous section. Furthermore, surveys require strict attention to informed consent and confidentiality, which can provoke non-random refusals at the outset.

Fortunately, modern data collection can address some of these problems. In particular, administrative data play an increasingly important role. Future social experiments and pilots to assess social policy could anchor the data collection in an administrative data set such as health, social assistance, and taxation records, augmented by surveys to collect information not otherwise available. By creating a consolidated administrative data set to support the screening of participants into treatments and controls, one can reduce response errors at the start. Social experiments that use anonymized administrative files need not engage in informed consent procedures. Administrative data sets that can support the unobtrusive downstream monitoring of the participant should reduce attrition, which was sufficient in Mincome to require a supplementary sample to sustain the experimental design.[40]

In sum, the conceptual lessons lie in the inherent short-term nature of a social experiment that seeks to address long-term responses and the biases that occur as a result of respondent selection and continued monitoring. Increased reliance on administrative data may mitigate panel losses in future experiments, but it is inevitable that the limited sample and length of a social experiment will provide challenges in interpreting the evidence.

Operational Lessons for Social Experimentation using Panel Data

Mincome employed about 200 full- and part-time staff to administer an experiment involving approximately 1,700 households. It was an incorporated entity that had delegated authority from federal and provincial tax departments to collect and remit income taxes. The staff were involved in a range of tasks including monthly payment delivery, tax remittance, interviews (quarterly behavioural surveys and monthly earned income tracking), human resources (for the experiment itself), and research. The experiment cost almost $50,000 per participant in 2016 dollars.

Mincome planners were caught somewhat off guard by the ever-increasing minutiae of screening, enrolment, creation of a parallel payments and taxation system, and the complexity of managing periodic surveys. These unforeseen costs bumped up against an inflexible budget. A central lesson from Mincome for future experiments and pilots is that these are formidably complex and costly projects. The technical documentation makes it clear that Mincome researchers were unwilling to compromise their commitment to a very high technical and scientific standard but that the research output suffered. In resource-constrained contexts, researchers who are unaware of the strict demands of panel data experiments may be tempted to manage budgets by taking short cuts and other compromises that reduce data validity and reliability, sometimes in unknown and undocumented

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ways. An enduring strength of Mincome is the transparency of its methods and the detailed documentation that supports research, even 40 years after data collection.

Such social experiments result in large data sets. These data sets require focused management to ensure the integrity and quality of the information, both for current analysis and to preserve the records for future study.

Strategic Lessons

Mincome concluded in 1978 without substantive research and no remaining budget for research. The technical documents prepared in 1979 contained hints of results, but substantive research results only developed slowly as limited resources and academic interest dictated. In retrospect, two lessons emerge. First, the budget encompassed a high proportion of direct income replacement payments. Therefore, the net cost of the experiment itself was considerably lower than commonly cited. If the annual reports had used net cost as the bottom line of the experiment, it may have mitigated the politicians' fiscal concern. Second, and more important, issuing research results earlier during the experiment may also have reduced the impression of a cash burn with nothing to show for it. Only recently has the issue of knowledge translation emerged as a priority for all forms of scientific research. Clear communication of results is a specific skill area, possessed only by select researchers. Future social experiments must pay attention to knowledge transfer.

It is likely that the Mincome researchers realized the political need to offer interim results, but several factors may have impeded such release. First, the unfolding complexity of the operations may have diverted energy away from research and the release of results. For example, the debate over whether to create a supplementary sample was intense and persisted for several months, which undoubtedly sapped research resources. Second, the Mincome researchers were committed to doing things better than the prior income maintenance studies in the United States. A feeling existed within the organization that Mincome should not only build on the previous research but also design and execute the experiment to a demonstrably higher standard. This focus on technical excellence may have encouraged the researchers not to release preliminary results that might require amendment later. Third, researchers of the day were concerned about possible behavioural modification if participants in the experiment were made more aware of their being observed, the so-called Hawthorne or observer effect that might arise from the public release of research before completion of the experiment. Finally, all the income maintenance experiments showed that a certain number of participants reduced their labour supply in the short run. Leading with this result may have overshadowed other outcomes and also overstated the longer term labour supply adjustment. Preliminary results can be misinterpreted, and revisions in light of better information can place researchers in an untenable position, especially among journalists keen to issue a quick story. In light of these risks, most researchers will understandably choose a conservative approach to releasing results.

At the same time, politicians and the public justifiably require transparency for large expenditures. Even if the net cost per participant were 50 percent of the full cost ($25,000 in 2016 dollars), a new experiment or pilot launched today and running for three years with 1,000 participants (the bare minimum) could still cost $25 million. If Mincome teaches us anything, therefore, it is that a communications strategy of releasing preliminary results is essential to maintaining political and public support for large projects. Mitigating the risks of funding cuts requires that expected outcomes be communicated early and placed in context. In the current era, this would mean using social media and cultivating journalists known to be able to present complex stories. It also would mean breaking down the outcomes into a sequence of initial changes that can be expected to support longer term outcomes. Such theories of change support the framing of initial results in the context of longer term implications.

Concluding Thoughts

The 1960s and 1970s were heady days when solutions to poverty were believed to be achievable, and social experimentation around a negative income tax or guaranteed income program was part of the way forward. Although poverty has endured and the limitations of social experimentation are more apparent, Mincome remains one of Canada's premiere social research studies. The technical manuals attest to the scientific rigour of the research and data, notwithstanding the challenges of assembling and interpreting experimental data that we enumerate in the preceding two sections. The fact that Ontario is proceeding with a pilot to test a basic annual income in 2016 shows that Mincome remains pertinent to today's policy context.

Although Mincome reflects the public policy and socio-economic context of four decades ago, the quality of the information is high, and the supporting documentation is strong. Basic labour supply assessment has been completed, but remaining issues that could be explored include effects on labour supply for specific age groups and the differential response of rural and urban residents. The effects on wealth remain another important area, because measures of inequality focus on income and do not analyze changes in wealth. Only a limited amount of research has been done to examine family stability over time or the changing attitudes of

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participants. Mincome offers information on male and female heads, which supports a thorough analysis of joint decision making within the household. This is an important advantage because modern privacy legislation often limits researchers' ability to construct households from administrative data extracts.

At the same time, it must be acknowledged that Mincome information dates from 40 years ago, and both labour markets and the broader socio-economic demographics have changed. Researchers using these data must be aware of the limits to extending interpretation across time.

With the demise of major longitudinal data sets such as the Survey of Labour and Income Dynamics, and with replacement data slow in coming, Mincome offers the research community a unique and high-quality data resource with which to address still pressing public policy issues. These data and documentation are available through the University of Manitoba Libraries Dataverse (http://dataverse.lib.umanitoba.ca/dataverse/mincome), and we invite the research community to explore this unique data set.

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Notes

1 For further details on the agreement and the social developments underlying it, see Hum and Simpson (1991, 43–44).

2 The early history of the design and development of Mincome remains buried in the administrative documentation retained by Library and Archives Canada. These documents have never been subject to systematic review.

3 These were the Seattle Income Maintenance Experiment, Denver Income Maintenance Experiment, and the New Jersey Income Maintenance Experiment.

4 Figures represent the assigned cell populations at the first payment. Attrition in Plan 6 was severe in Period 1, which prompted the combination of Plans 6 and 7 for Periodic Survey 2 and subsequent interviews.

5 There is a second dimension of taxation on wealth, but this was used to adjust payments and was not integrated into the experimental design.

6 See Technical Report No.1 (Hum, Laub, and Powell 1979, 27). These guarantees correspond to about $18,900, $22,800, and $26,800 at 2016 prices and constitute about 50 percent, 60 percent, and 70 percent of the Statistics Canada (1975, 19) Low Income Cutoff of $7,655 in 1974 for a family of four in a community of 30,000–100,000. The guarantee was adjusted according to family size from 38 percent of the guarantee for a family of four for the smallest families (single adults) to 247 percent of this value for the largest families (Hum, Laub and Powell 1979, 31). The guarantees were also increased by $600 for a family of four in July 1975 to improve program participation (Crest et al. 1979, 26).

7 For additional discussion of the considerations that led to the Dauphin site, readers are referred to Technical Reports No. 2 (Hum, Laub, Metcalf, and Sabourin 1979) and No. 8 (Sabourin et al. 1979). See Appendix A for a list of technical documentation and working papers pertaining to Mincome.

8 Annual inflation rates in Canada ranged from 7.2 percent to 10.7 percent between 1975 and 1978 (Canadian Inflation 2014).

9 The assignment across plans is summarized in Sabourin (1979, 77).

10 See Technical Reports No. 2 (Hum, Laub, Metcalf, and Sabourin 1979, 10–11, 43–44) and No. 6 (Sabourin 1979, 32–48) for additional discussion of the rural dispersed sample assignment and Technical Reports No. 6 (Sabourin 1979, 49–61) and No. 8 (Sabourin et al. 1979) for additional discussion of the saturation site sample in Dauphin.

11 Technical Report No. 8 (Sabourin et al. 1979) reports initial enrolment of only 322 and explores the reasons for low participation throughout the experiment at the Dauphin saturation site. Kurz (1979, 27–37) also discusses participation issues in Dauphin and estimates that at least 1,400 families were eligible for Mincome payments. He notes that the low participation rate occurred even though families did not need to be selected by Mincome because Dauphin was a saturation site, and any resident could self-enrol at any time.

12 Enrolment procedures are discussed in Technical Report No. 4 (Crest et al. 1979, 30–38).

13 Summary tables of questionnaire content and periodicity are provided in Rasmussen, Anderson, and Wright (1983, 18–19). A more detailed account of data development and availability is provided in the next section.

14 Because the periodic survey in the original sample was not stabilized until its third rendition, there are some differences in coverage between the first two periodic surveys in the original sample and those in the supplementary sample (Rasmussen, Anderson, and Wright 1983). Further discussion of this issue and the supplementary sample are contained in Technical Reports No. 2 (Hum, Laub, Metcalf, and Sabourin 1979, 46–50) and No. 6 (Sabourin 1979, 62–74). An evaluation of the supplementary sample design is provided by Kurz (1979, 39–41).

15 The IRF and its accompanying schedules was simplified in July 1975 to improve program participation (Crest et al. 1979, 25–26). To see whether income reporting affected behaviour, the control group was roughly divided into half who completed the IRF and half who did not (Hum, Laub, Metcalf and Sabourin 1979, 10). Families in the control group were provided with a small payment to complete the interviews and monthly IRFs.

16 See Technical Report No. 1 (Hum, Laub and Powell 1979, 30–34) or No. 3 (Hum, Crest and Komus 1979, 24–26) for details on the calculation of *G*, *Y*, and *W*.

17 Farmers in the rural dispersed sample were permitted an additional exemption of $20,000 (Crest et al. 1979, 19). The Mincome experiment was unique in its use of a wealth tax as well as the adoption of a saturation site, the inclusion of single individuals in its dispersed sample, and some experimentation with administrative parameters (Hikel et al. 1974, 49). The Mincome analysis of the appropriate treatment of farm income and wealth would provide useful

background for the design of a modern guaranteed income in which self-employment and non-wage income would be more prominent.

18 Those eligible for payments in any month would not pay regular taxes. For operational details of the annual reconciliation process, see Technical Report No. 4 (Crest et al. 1979, 62–69). See also Technical Report No. 9 (Basilevsky, Hum, and Sabourin 1979) for further details on income reporting and deductions.

19 Hum (1981) provides an extensive discussion of this problem and other design issues associated with negative income tax experiments in general and Mincome in particular.

20 Evidence on the extent of new family unit formation and inactive units is provided in the payments data in Crest et al. (1979, 8).

21 In 1979, Mincome digitized only a portion of the collected survey data, primarily relating to labour markets, changes in income and net worth, and selected attitudinal and household composition variables. The choice of what to digitize reflects the Mincome researchers' belief about what information was essential to test work-incentive behaviours and changes to the economic foundation of the household. Considerable information resides in the hard copies of survey and IRF forms stored in the federal archives that remain undigitized. The work of the Institute for Social and Economic Research (ISER) focussed on transforming the existing digital files (set up in PDP-11 formats) to run on the mainframes of the day, creating codebooks, and promoting the potential of the data to other researchers.

22 The University of Manitoba insisted that a portion of the funding be devoted to a range of other academic pursuits, beyond just restoring the Mincome data.

23 The Macdonald Royal Commission recommended a GAI in its 1985 recommendations (Canada, 1985).

24 Dataverse is a Harvard University product that supports the archiving and dissemination of digital objects. The University of Manitoba Libraries is a licensee.

25 Library and Archives Canada still maintains the administrative files and completed survey forms, essentially in the same form as in which they were originally received from Mincome in 1979.

26 New cross-sectional microdata on labour supply and work histories from the 1981 long-form Census and from the Survey of Work History, the forerunner to the Labour Market Activity Survey and the Survey of Labour and Income Dynamics, were also emerging from Statistics Canada at this time.

27 Issues arise because the assignment model and participation both depend on family income and prospective payments, which in turn depend on labour supply and introduce the potential for bias. Many other socio-economic and demographic covariates might be included, but insofar as they are either uncorrelated with the treatment variables by experimental assignment or fixed in time, they would be expected to have no effect on the estimates of the treatment effects.

28 See Table 2 in Hum and Simpson (1993, S279) for a summary of the non-structural labour supply response estimates from the US experiments and Mincome. The weighted average US effect is dominated by the final experiments in Seattle and Denver, which had the largest samples, the most complex designs, and the largest reported experimental effects (Hum and Simpson 1991, 63).

29 Virtual income $h\Delta w + \Delta y$ reflects the income guarantee consistent with a linear tax schedule and pre-experimental labour supply.

30 A complete discussion of the structural labour supply models analyzed using the Winnipeg sample is provided in Hum and Simpson (1991, 69–84).

31 See, for example, the review of recent empirical labour supply research for the US Congressional Budget Office (McClelland and Mok 2012). The estimated elasticities are small and within a narrow range for both men and women compared with the research reviewed in Hum and Simpson (1991, 5–39).

32 No deep scientific justification supported the selection of Dauphin as a saturation sites. Rather, based on undocumented anecdotal accounts, it seems that the provincial government of the day simply wanted a farm-oriented community to participate in Mincome.

33 The media have been captivated by the notion of Dauphin as a saturation site. Many stories have circulated that government spent a wad of cash and then abruptly terminated the project, leaving all the information to fester in dusty boxes. Nothing in Professor Forget's papers or presentations supports such misleading views.

34 The screening and enrolment surveys were not digitized by Mincome before data archiving.

35 See Lee (2003) for a survey of attrition rates in panel surveys.

36 See Greene (2012) for an overview of the development of this literature.

37 A standard reference is Campbell and Russo (1999).

38 The Seattle and Denver income maintenance experiments included a component that was intended to run for 20 years to explore this issue, but that sample was terminated much earlier with a settlement for the affected families. For those families who expected the treatment benefits to last 20 years, Robins (1984) finds that labour supply response is smaller than in 3- and 5-year samples, but the small size of the 20-year sample hampers formal testing.

39 Metcalf (1974) analyzes the response bias from a limited-duration experiment in the context of a model of labour supply over the life cycle.

40 Any future social experiment would likely need to merge administrative and survey data, which would require appropriate ethical procedures.

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Appendix A: Mincome Experimental Papers

These papers consist of the technical reports of the Manitoba Basic Annual Income Experiment and are available online at the University of Manitoba Libraries Dataverse at http://dataverse.lib.umanitoba.ca/dataverse/TechDocs. Full references are provided in the reference list for this article.

Technical Report No. 1, *The Objectives and Design of the Manitoba Basic Annual Income Experiment,* by Derek Hum, Michael E. Laub, and Brian J. Powell. 1979. 69 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/XAGGJT.

Technical Report No. 2, *The Sample Design and Assignment Model,* by Derek Hum, Michael Laub, Charles Metcalf, and Don Sabourin. 1979. 65 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/VPZILS.

Technical Report No. 3, *The Design of the Payments System of Mincome Manitoba,* by Derek Hum, David Crest, and David Komus. 1979. 35 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/BWRHBQ.

Technical Report No. 4, *The Administration of the Payments System of Mincome Manitoba,* by David Crest, Carol Billett, Derek Hum, David Komus, and Andrew Quarry. 1979. 69 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/UYWPHK.

Technical Report No. 5, *An Evaluation of the Experimental Sample of Mincome Manitoba,* by Mordecai Kurz. 1979. 68 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/Q1VF86.

Technical Report No. 6, *Sample Development over Time, Participation and Attrition,* by Donald Sabourin. 1979. 81 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/AXLATZ.

Technical Report No. 7, *An Analysis of Non-Response to the Manitoba Basic Annual Income Experiment,* by Donald Sabourin and Derek Hum. 1979. 58 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/KJCFS0.

Technical Report No. 8, *Program Participation in the Saturation Site of the Manitoba Basic Annual Income Experiment,* by Donald Sabourin, Derek Hum, William Harrar, and Alexander Basilevsky. 1979. 60 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/IHA3UI.

Technical Report No. 9, *Income Reporting Behaviour in a Negative Income Tax Program: A Comparison of Retrospective and Prospective Reporting Methods in Mincome Manitoba,* by Alexander Basilevsky, Derek Hum, and Donald Sabourin. 1979. 45 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/JE9UGG.

Technical Report No. 10, *The Accuracy of Income Reporting in Mincome Manitoba,* by Alexander Basilevsky and R. Sproule. 1979. 35 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/9XJUGP.

Technical Report No. 11, *Issues in the Administration of Mincome Manitoba: Three Preliminary Assessments,* by Carol Billet, David Komus, Derek Hum, Alexander Basilevsky, and Robert Sproule. 1979. 34 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/YCCBMI.

Technical Report No. 12, *Mincome Field Operations and Interviewing Techniques,* by Edgar Rasmussen, Andy B. Anderson, and James D. Wright. 1983. 44 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/YDGFHH.

Technical Report 13, *Quality Control and Mincome Data,* by Edgar Rasmussen, Andy B. Anderson, James D. Wright, and Eric Sang. 1983. 34 p. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/EU7QKS.

Mincome User Manual Version 1, by Institute for Social and Economic Research. 1983. http://dataverse.lib.umanitoba.ca/dataset.xhtml?persistentId=doi:10.5203/FK2/XLOXQF.

Appendix B: Mincome File Codebooks and Data

The code books are detailed data dictionaries for the Mincome files. They are available at the University of Manitoba Libraries Dataverse at http://dataverse.lib.umanitoba.ca/dataverse/Codebooks.

The data are in Excel format with two header rows—(a) variable numbering corresponding to the numbering in the code books and (b) a variable name. Users may change the name but should retain the original file with numbering to ensure data checking. The data are available at http://dataverse.lib.umanitoba.ca/dataverse/Mincome_Data.

MINC1—Mincome Baseline Summary File

MINC1 contains 91 variables constructed from data in the first major survey undertaken by the Mincome guaranteed annual income experiment—the baseline survey. The file contains three types of variables: those pertaining to the household (size, composition, etc.), those pertaining to the male head, and those pertaining to the female head. For double-headed households, information on both heads exists.

MINC2—Mincome Payments Summary File

MINC2 contains 495 variables constructed from the payments database of the Mincome guaranteed annual income experiment. The file consists of a header record of 14 variables for each household, followed by 37 months of records covering December 1974–December 1977 inclusive, each month repeating the same 13 variables.

MINC3—Mincome Baseline-Payments Data

MINC3 concatenates the baseline (MINC1) and payments (MINC2) data. This file contains 750 observations and 596 variables (91 from the payments data, 494 from the baseline survey, and 11 additional header records). A family (which could be a single- or double-headed household, with or without children) was included in MINC3 only if baseline information existed and if the family had received 24 months of payments.

MINC4—Longitudinal Labour Data File

MINC4 contains 338 variables constructed from the 11 major surveys (conducted approximately every three months) used to track labour market and other behavioural data. The data consist of 26 household variables, 154 variables tracking 14 variables for male heads for 11 surveys, 154 variables tracking the 14 variables for female heads for 11 surveys, and four records of header information. Double-headed households have information on both heads.

MINC5—Income and Net Worth

MINC5 contains 445 variables constructed from the data in the 11 major surveys undertaken between December 1974 and December 1977. The surveys tracked changes in financial assets, real property, and income for each of the 11 surveys, conducted approximately every 3 months.

MINC6—Family Composition and Attitudes

MINC6 has data only from the Winnipeg site and includes 1,408 variables constructed from the 11 surveys. There are 287 variables on family composition, 411 variables on male and female head attitudes, and 327 variables on double-headed households. There are 11 recurring cycles of about 35 attitudinal variables for male and female heads. Mincome collected attitudinal data on such issues as locus of control, housing satisfaction, weekend activity, awareness and understanding of the MINCOME experiment, attitudes toward work, and decision making in the household.

Caution: These codebooks date from 1984 and contain instructions on accessing the data using IBM Job Control Language at the University of Manitoba at that time. The current version of the data is in Excel format and may be read into most statistical software currently in use. The Excel data sets have two header rows, numbering the field and listing the variable name as identified in the applicable codebook. The Excel spreadsheets are all under 5 megabytes in size and therefore well within the limits of desktop and laptop computers.

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