Evaluating the basic income using an experiment

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Abstract

The basic income experiments of the seventies such as the Manitoba Basic Annual Income Experiment and more recently, the Ontario Basic Income Pilot (OBIP), offer important lessons for the design and conduct of large-scale policy experiments. Such policy analyses promise to test the validity of the expected consequences in advance of committing the full resources for interventions such as the basic income. However, these ambitious studies have failed for three main reasons. First, elaborate research projects require a level of sustained logistical support and patience that few governments seem willing or able to offer. Second, proponents for a basic income appear to argue that it is general antidote for many of modern society's ills. This has created complex theories of change linking a single intervention to a myriad of outcomes that that demand increasingly complicated analyses. Finally, social policy pilots continue to rely on survey data that introduces a multitude errors that undermine impact measurements. This paper examines these failures and then asks whether any experimental design might test the main behavioural hypotheses associated with a basic income. The conclusion is that large scale experiments are unlikely to shed light on the important questions and that if the political will exists to implement a basic income, careful monitoring of key outcomes in a quasi-experimental context over an extended period is the best option to evaluate this policy.

Introduction

The universal basic income (UBI) or guaranteed annual income has assumed iconic status as a core policy prescription with far reaching outcomes and capacity to transform society. It also has been subject to the most ambitious attempts to evaluate economic policy *ex-ante* using experiments

For decades experiments have fascinated social scientists, apparently attracted by the seemingly increased legitimacy afforded by studying policy using randomized control trials (RCTs). Since the mid-fifties educational researchers sought to improve the teaching and learning using experiments(Campbell & Stanley, 1966). The late sixties and seventies saw a spate of social and economic experiments to advance basic knowledge and to test policy. Two notable examples in health are the Rand Health Insurance study(Newhouse et al., 1982) and more recently the Oregon Health Insurance study(Finkelstein et al., 2012). The negative income became the focus of several large experiments in the United States, and of course Canada(Hum & Simpson, 1991; P. K. Robins & West, 1980). In the last several years, many countries have either started a basic income or a study to examine its impacts and feasibility. Mincome was the last in a series of "classic" negative income experiments (NIT) designed primarily to evaluate the impact of a basic income on work incentives (Hum & Simpson, 1993; P. Robins, 1985; Rothstein & von Wachter, 2017) The Manitoba Basic Annual Income Experiment (Mincome) that ran between 1974 and 1978 and the Ontario Basic Income Pilot (OBIP) that started in 2017 and ended in 2018, serve as two excellent sources of lessons learned for large-scale policy experiments.

In addition to structure of clinical trial with treatment and control groups, sponsors expected these to extend over at least three years involving monthly payments and periodic data collection. Besides assessing behavioural outcomes, the experiments also attempted estimate the long-run costs of a basic income as and to identify administrative challenges.

The paper starts with a brief review of the UBI concept and associated theories of change that link the income supplementation with intended/expected outcomes. Then I review the research designs common to negative income tax experiments, using Mincome and the OBIP as templates, followed logistical issues in implementing negative income taxes experiments as sampling, experimental scope, data collection, and sample selection biases. Key challenges in the methodologies and implementation limited the insights obtainable from what were very costly studies.

The paper concludes with a proposed design for a basic income experiment, that draws from the social experimental literature, while attempting to avoid the traps of the income maintenance experiments of the seventies and the most recent Ontario study. However, it is unlikely that any government will be able to fulfill the conditions for an experiment to evaluate the costs and benefits of a basic income *ex-ante*.

The universal basic income: two Canadian research studies.

Both Mincome and OBIP adopted a negative income tax approach to the universal basic income. Before examining these two studies in more detail, it is useful to present the basics of a basic income.

Basic form of a UBI

When Milton Friedman proposed the idea of a basic income, the intent was primarily to alleviate poverty (Friedman, 1962) with the secondary benefit of reducing the scope of government. In the last decade, the basic income has morphed into the swiss army knife of social policy, capable of mitigating many social ills.

The two forms of a basic income comprise a demogrant and a negative income tax (NIT). A demogrant makes regular payments conditional on some form of demographic eligibility (age, residency, or citizenship), but it is not conditioned by income, wealth, or work participation. A typical format for a demogrant involves tax free payments received by all eligible households, with additional income (regardless of source) above a guaranteed level taxed as usual or even higher than current rates. (Pasma & Mulvale, 2009). The demogrant has never really gained policy traction in North America, possibly because many lower income households unused to paying or even filing income tax returns could face nasty tax surprises with consequent political fallout. It also seems strange to offer income to well-off households.

A basic income modelled as negative income tax attempts to align payments to household income is by far the most common form of basic income. Figure 1 shows a threshold value G or support level, with the supplement rising at a slower rate (1-t) than income. Eventually the supplement payment falls to 0 at the breakeven level B. In this formulation $B = G/t$, so specifying two of the three parameters of a NIT identifies the third.

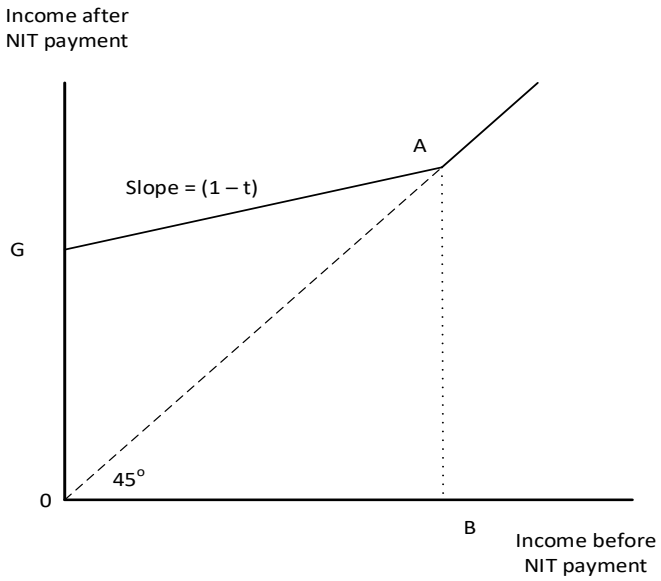

Figure 1: Negative Income Tax

The Manitoba Basic Income Experiment (Mincome)

Using the neo-classical theory of work-leisure tradeoffs, Mincome defined the support level G and the offset tax rate t, as "crucial for policy purposes since combinations of G and t selected will largely determine labour supply effort and overall program costs." (Hum et al. 1979a, p 19). For Mincome these two program parameters are the only policy relevant experimental variables that determine labour supply effects, program take-up and eventual costs.

The payment P also reflected differences in wealth, primarily in the form of equity in a home, cars and savings as well as different family sizes. (Hum, Laub, & Powell, 1979a). The payment received by a typical Mincome family is

$$P = G - t \bullet Y - r \bullet W \qquad \ldots 1$$

where G = the support level

t = the offset tax rate

Y = family income

r = tax rate on net worth

W = net worth

Using wealth to condition payments is important since most of the current discussion about the basic income focuses solely on income conditioned payments. It is common for some Canadians, even low-income households, to have wealth in the form of real estate and pensions. Paying a basic income to a low-income household with even modest net worth in the form of a house, remains a tricky policy issue.

Mincome identified three levels of support and three offset tax rates creating a nine-cell design space, with an additional control group as shown in Table 1, which shows the support payments for a household with two adults and two children under 18. The Canada Child Tax benefit did not exist in 1974, and Mincome included adjustments for family size to adjust the guarantee levels and therefore the payments reflect a similar standard of support based on family size and composition(Hum, Laub, & Powell, 1979a). The family size index reflected the economies of scale in adding persons to the household while minimizing incentives to change the family structure to qualify for higher benefits. Varying the benefits by family size and composition also reflected the practice of social assistance programs in Manitoba during the seventies.

Table 1: Design matrix for Mincome (two adults and two children

Guarantee at enrolment, $ (G)	**Tax Rate (t) on Total Income**		
	35%	**50%**	**75%**
3,800	Plan 1	Plan 3	Plan 6
4,600	Plan 2	Plan 4	Plan 7
5,400	Plan X	Plan 5	Plan 8
		Plan 9	

Source: (Hum, Laub, Metcalfe, & Sabourin, 1979)

With reference to Table 1, Plan X never became part of the experiment as it proved too expensive, and Plans 6 and 7 merged due to sample insufficiency. Plan 9 formed the comparison/control group.

As a reference, Table 2 shows the monthly support under Plan 4 in June 1975. Note that Mincome prepared the taxes for all participants, who received support on a monthly after tax basis and indexed to inflation which was around 9%. The variation for the same family size reflects the attempt by Mincome to accommodate potential recipients who could qualify for equal of higher financial assistance from income assistance by virtue of disability and other mitigating factors. The

values shown reflect an average payment. Mincome support levels were close to existing income assistance rates, unlike OBIP which were markedly higher. Enrollees into Mincome had to choose between the basic income and income assistance; they could not be on both programs. In the seventies, because of the so-called welfare wall, even earning a small amount could trigger tax obligations that could push one's net income below that which income assistance would pay. At the same time, income assistance rules allowed for additional payments based on disability, which creates the potential for welfare domination, where the Mincome experiment could lose participants to income assistance. Accordingly, both the payment structure and eligibility rules under Mincome was complex and reflected a range of individual circumstances of the household, necessitating a sophisticated administrative structure (Hum, Crest, & Komus, 1979).

Table 2: Mincome support under Plan 4 (June 1975) ($ monthly)

Family Size	
1	152
2	284
3	352
4	400
5	440
6	480
7	520
8	560

All households except size 1, were assumed to have two adults and all children were under 18. For example, a family of size 5 had two adults and three children under 18.

Mincome also featured three experimental sites – Winnipeg which was the main sample, a rural dispersed sample, and a saturation site located in Dauphin Manitoba. Mincome researchers always viewed the Winnipeg site as the main action since it featured the full design matrix and a control group. The rural dispersed sample featured treatment and control households in several towns (Hum, Laub, & Powell, 1979b) in an attempt to discern whether differences in labour market response existed between urban and rural households. The Dauphin saturation site holds considerable appeal since it held out the possibility for analyzing a NIT in a setting of universal eligibility. This might allow the researchers to examine the implications of a NIT in a setting where it became general policy. The paper returns to these points later.

Ontario Basic Income Pilot (OBIP)

By 2016, the genesis of the basic income experiment in Ontario, the anticipated outcomes of a basic income had evolved beyond the direct alleviation of poverty. A very diverse academic and popular literature has developed with proponents believing it supports a range of health, social and financial outcomes (Forget, 2011, 2018), enhances distributive justice (Zelleke, 2005), and manages the adverse employment effects of automation (McGaughey & Research, 2018).

The foundation paper for the OBIP (Segal, 2016) proposes a classic randomized control trial (RCT), but offered few details. Ontario eventually decided on formal implementation in 2017, and as Table 3 and Figure 1 show, a single person without disability and no income would receive almost $17,000 (G) and declining payment until his/her earnings reach a breakeven (B) of $32,000. OBIP is tax free, but recipients with additional income pay taxes on that income as would any other household.

Table 3: **Ontario Basic Income Pilot – Breakeven levels**

	Single	**Couple**
Maximum Basic Income Amount	$16,989	$24,027
Maximum Basic Income Amount plus Disability Supplement for one person with a disability	$22,989	$30,027

Source: (Government of Ontario, 2017)

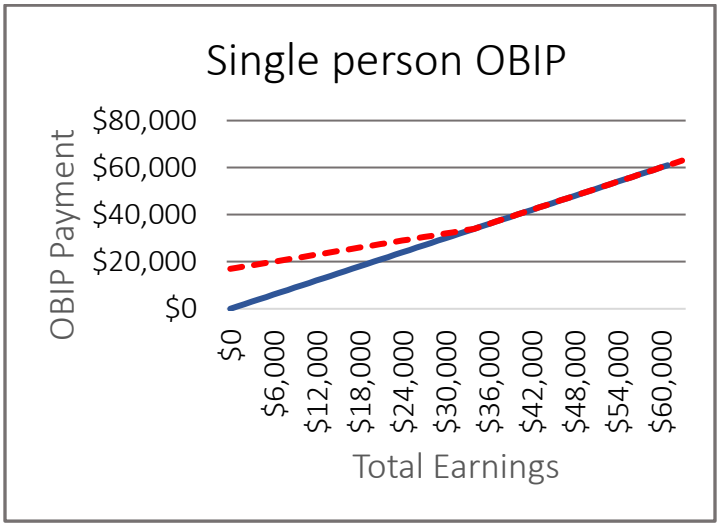

Figure 2: OBIP Payments
Source: Author's Calculations based on (Government of Ontario, 2017)

Under OBIP eligible households with children would also receive the Canada Child Benefit Program, which offered a significant top up as shown in **Table 4**. These examples ignore other possible income (financial and in kind) such as the GST rebate, rental assistance programs, and the like. All recipients must file an annual tax return. Aside from the obvious simplicity of the experiment (only one level of G and not tax back), the availability of the Canada Child Tax Benefit also allowed OBIP to avoid the complex system used by Mincome to adjust payment for different family sizes.

Table 4 Examples of support levels under OBIP
• A single individual, without a disability, earning $28,000, will receive a BI payment of about $2989 to make total income to $30,989. • A couple, with one person disabled, and with part-time employment between the two of them generating $16,009, will receive $22,027 in BI with will leave them with a total income of $38,027. • A single parent with two children under 6 and no earned income, will receive a BI of $16,989 plus the CCB of $12,800 to reach a total income of $29,789 tax free. • In the case of the single individual earning $28,000, the combined federal and provincial tax (on earnings) will be about $3,500 reducing their after-tax income (with the BI on top) to about $27,500.
Source: Author's Calculations

Originally designed as a classic randomized control trial (RCT), OBIP had two Phases. Phase 1 comprised a Baseline Study and formed a feasibility and learning exercise in the first year at two sites (Hamilton and Thunder Bay) in Phase 1, while Phase 2, the full pilot implementation started in the second year by adding a so-called "saturation site" in the town of Lindsay.

As an RCT, OBIP featured the design matrix shown in **Table 5**. An early issue for the pilot was that low-income households on social assistance – Ontario Works (OW) and the Ontario Disability Support Program (ODSP) – received free supplementary health insurance, not available to other Ontarians. Applicants to OBIP transitioning from OW/ODSP received these health benefits, while low income applicants in general did not. In principle, this design supported a test of how these extra benefits might affect program participation and outcome.

Table 5: Design matrix for OBIP	
Intervention	**Comparison**

Single (WO HB)	Couple (WO HB)	Single (WO HB)	Couple (WO HB)
Single (W HB)	Couple (W HB)	Single (W HB)	Couple (W HB)
Single D	Couple D	Single D	Couple D
WO HB – no health benefits on top of OBIP; W HB – health benefits on top of OBIP; D – disabled			

Aside from premature terminations due to political decisions , these two major exercises in economic policy analysis present important lessons in design and execution, offering insight into two experiments' eventual failures.

Implications of the design features of Mincome and OBIP

Both Mincome and OBIP attempted to create rigorous social experiments, the point of which is to replicate the randomized control trial common to science and seen as the "gold standard" for causal analysis. Setting aside for a moment whether an RCT can ever serve as a gold standard for social and economic policy design, an experimental approach offers the promise to isolate the influence of the intervention on expected outcomes by controlling many if not all confounding influences. Depending on the success in creating the experimental design, comparing the outcomes for those receiving the intervention to those not, is a simple matter of comparing means and variances. The more complete the experimental control, the simpler the mechanics of inferring net impacts of interventions. The theory of change, sample selection/allocation, and data collection are three themes that frame Mincome and OBIP and offer infight into the fundamental factors in their early termination.

Theory of Change

The program evaluation literature describes the theory of change

In the mid-seventies, the theory of

A common argument in favor of a BI is that increased income and increased income security will yield important health benefits. The work of Forget ((2011) is noteworthy for having attempted to test this using a quasi-experimental approach. Flowing from this idea is the notion that a UBI will affect all aspects of health outcomes specifically reduced use of mental health services, increased visits to primary care, and increased active management of chronic conditions. Another potential avenue for increased income to support health is the increase in food security. Willows et al (2009) do not connect food security to a basic income, but the connection seems plausible. However, Thoits and Hannan (1979b) use the Seattle-Denver experimental results to show that some increased distress occurred among recipients, a finding that is consistent with the notion that income changes, up or down, represent life events that tend to confront recipients opportunities to change aspects of their lives.

Associated with the notion that a basic income addresses inequality is the notion that technical change will displace and increasing share of lower skilled labour. Many have observed that the last decade has seen a "skills" divide. Technological advance displaces those with lower skills from the workplace, and a basic income both "recycles" income from rich to poor as well as offers the essential economic support. Whether this is actually occurring is a matter of debate (Autor(2014)), but one prediction of proponents is that the UBI will allow individuals with low skills to regroup and invest in education. Finally, others extend this to the idea that increased economic security will allow recipients to engage in more creative pursuits with the rejuvenation of arts and culture.

A common thread in the support for a basic income is the notion of universal justice. Pasma and Mulvale (2009) see the basic income as fundamental to "economic democracy that provides economic support and to all citizens." Further they argue a range of social benefits to those to covered by social assistance programs, increases social cohesion and protection for vulnerable persons. Zelleke (2005) evaluates a UBI using the principles of distributive justice and concludes the unconditional nature of a basic income aligns well with the concept of a property owning democracy articulated by Meade (1964). Huws (2017) argues from a feminist perspective that a universal income would compensate household members whose work is not recognized by the market economy, thereby becomes a policy supporting women's liberation.

Finally, many authors mention the idea that a basic income reduces the stigma associated with social assistance. To be sure with the era of direct deposit, the spectre of forming a queue in public view to receive a cheque no longer exists. Stigma can remain perceived by recipients of social assistance,

if they view that somehow the payments are not normal or "deserved." A very common reason for refusing to participate in Mincome was the perception that these payments were a form of handout (Kurz 1978). However, recent qualitative analysis of Mincome data by Calnitsky (2016) appears to suggest that those receiving payments under a NIT may well view such support less moralistically and less tainted by social stigma.

One emerging issue may be termed the "political economy" of a basic income. Identified by those working from a left perspective, this view attempts to identify the impact of a basic income on institutions such as unions and worker cohesion. As D'Ippoliti (2018) argues a basic income has potential negative impacts on these more abstract concepts. If we assume that a basic income is enough for any competent person to manage their affairs, then the market economy will take precedence over public provision of services. Subsidized housing is an example, where recipients of a BI would be expected to fend for themselves which in turn may create pressure on scarce affordable housing. Second, a potential exists for devaluing the public sector as payments become solely conditioned by income. For some, such as Milton Friedman ( ), the removal of welfare state bureaucracy represents a benefit. For others, however, a purely financial support system cannot compensate for the non-financial services that often attend the provision of social services. Assuming a purely cash transfer represents the sole support needed by low-income families may over simply the needs of families. Third a UBI may reduce union power even further. Fourth, it is possible that a UBI would remove low skilled persons even more from the labour market and create a permanent underclass who could never advance their economic situation. Fifth a UBI may attract increasing international migration, further alienating low income working populations.

Forty years later, OBIP adopted a broader causal framework using a more general experimental design. Both used repeated data collection with self-report data (participant surveys) as the main source of information, although OBIP was intending to access information directly from the Ontario Ministry of Health after the initial baseline survey.

This brief overview of the current thinking about the impacts of a UBI, needs joining with the traditional theory of change for a NIT to create a research program for an updated policy evaluation. Figure 2 presents a high-level logic flow for the basic income.

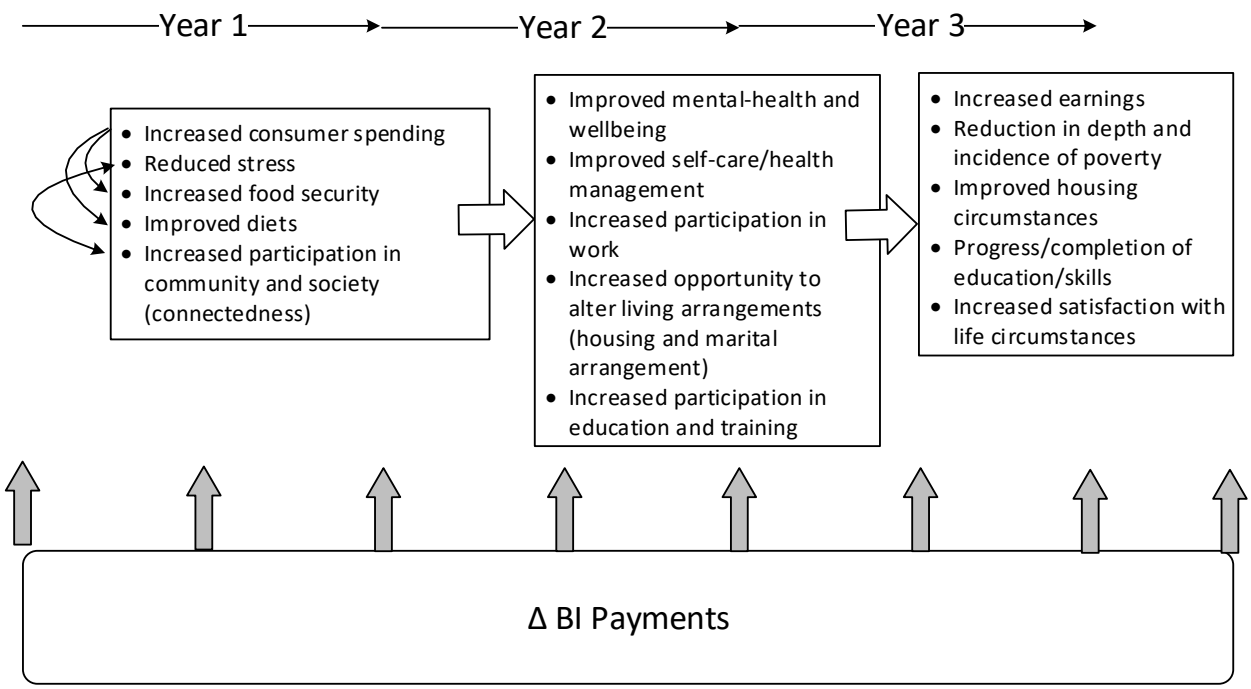

Figure 2 High Level logic model for Ontario Basic Income Pilot

(Explain diagram more)

Respondent selection and assignment

The NIT experiments of the seventies, including in Mincome, were exercises in economic policy. The funders and researchers of the day, viewed a negative income tax as taxation and income redistribution policy, with some potential for other social and psychological outcomes. Accordingly, Mincome applied a formal theoretical structure rooted in neo-classical economics with a sophisticated sample design to answer specific questions about labour supply elasticities under a NIT.

Commensurate with an experimental structure tightly bound to a theoretical model of about labour supply, Mincome adopted the then state-of-the-art sample assignment model (Conlisk & Watts, 1979). Because the observations in an income maintenance experiment were costly (primarily due to the income support payments to recipients) and since these costs varied with the family structure as

well as the cell to which the household was allocated, assigning equal numbers to each design cell also became statistically inefficient, especially if one might expect some cells to have little variation. For example, the cheapest cell was, of course, the control group, where participants incurred only data collection costs. In contrast, a cell w high cost cell that had a high support level and low offset such as Plan X in Table 1 becomes very expensive given the monthly support payments over the life of the experiment. If one could "predict" how participants might react to variations in G and t, then those cell responses with the highest variance could be assigned more observations and low variance cells fewer respondents. Early versions of this sampling method could produce many cells with no observations, a situation that created unease among many researchers. A modification suggested by James Tobin to ensure a minimum number of respondents in all cells, became the standard for all NIT experiments. In brief, this model uses information from the census and pre-enrolment interviews to allocate respondents to cells in the design matrix (Table 1) to "maximize the value of the information generated by the experiment." ((Hum, Laub, Metcalfe, et al., 1979). A complex technique literature emerged on this subject with Rossi and Lyall (1976) presenting a very useful synopsis.

This is not the place to debate the merits of this sample assignment in detail. The core issue is that such a sample uses expected outcomes to allocate sample points prior to the experiments even occurring. This approach creates endogeneity and an unbalanced design, with several implications. First, to correct for the endogeneity sample estimates of the labour supply elasticity with respect to changes in G and t need to incorporate the parameters of assignment model within any estimating structure(J. Hausman & Wise, 1983) . The standard ANOVA approaches used in experiments are not usable. Hum and Simpson (1991) discuss and illustrate the issue and its resolution. Second, researchers wishing to explore other outcomes, such as marital stability, also need to develop strategies for correcting such endogeneity and working with unbalanced designs. Keeley (1987a) illustrates this issues with respect to the impact of a NIT on marital dissolution. Third, the entire corpus of NIT experimental practice and evidence was at a very high technical level, with the result that results emerged slowly and only through a technical complex literature. This impeded clear and rapid communication of findings to the public and politicians.

Complicating this last point was the apparent "reversal" of findings upon reanalysis of certain outcomes. Whether a NIT accelerates marital dissolution is a case in point with several authors affirming the intuitive notion that a NIT increases marital dissolution because two singles obtain

more support than a couple and also allows partners to abandon a bad marriage (Groeneveld, Tuma, & Hannan, 1980b; Keeley, 1987b). However a counter opinion turned this conclusion on its head and a vigorous academic debate ensued (Cain & Wissoker, 1990a, 1990b; Hannan & Tuma, 1990) . The possibility that a NIT might change incentives to marriage would be of great interest to politicians and the public, but the complexity of the debate obscured the clarity needed for a common understanding of outcomes. Academically rigorous public policy research is essential, but to move political acceptance requires outcomes be understandable.

In contrast OBIP, reflecting the theory of change, OBIPm devellped a much simpler design. was almost simplistic in its approach to sample development. This reflected

Data collection

Attrition is a technical issue facing all longitudinal research especially those using surveys to collect outcomes.

Mincome, like the other NIT experiments, used in-person questionnaires to collect information on participants, treatment and controls. This included all socio-demographic data as well key outcomes (hours worked, earnings, etc.) and other expected impacts such as education, marital satisfaction, etc. The codebooks for Mincome Baseline Survey, Longitudinal Labour File, and Family Composition and Attitudes (Institute for Social and Economic Research, 1983a, 1983c, 1983b)offer a sense of the range of variables collected. At the time, other data collection methods (online, telephone, and mail) were either not developed or were deemed unsuitable for the extensive and complex information being sought.

The large-scale social policy experiments of the seventies and early eighties ran their course giving way to small field trials

A key feature of theories of change is their implied temporal element, unlike the comparative statics that characterizes most of economic policy. Policies are neither instantaneous nor final and outcomes occur over time. The value of logic models is their ability to make causal connection explicit.

Tmerination of Mincome abd OBIP

Mincome ran from 1974 to 1978, and after a hiatus, the data were catalogued and archived to support further research. Additional details on Mincome and its aftermath appear in (Simpson, Mason, & Godwin, 2017). As federal-provincial funded experiment, in later 1977 the federal government determined that the costs of the experiment had become excessive and announced it would not continue. The provincial government concurred as it could not afford to maintain the study independently.

The formal initiation of OBIP occurred in July 2017, with enrolment and first payment distribution. Phase 1 concluded in November 2017, and Phase 2 started in early 2018. It never collected follow-up information and so net impacts of the payments will never be known The newly elected Progressive Conservative provincial government terminated the pilot in May 2018, ostensibly because of cost, but during the election campaign its aversion to the concept of a basic income was no secret.

Theories of change also feature multidimensional and contextual causal relations, which become simultaneously a major element in creating a realistic causal structure, but also set the potential for crippling complexity that subverts tractable analysis. Balancing realism with tractability is an important goal for social experiments of "big" policy such as testing a basic income before the fact.

The recent literature on the UBI has mushroomed, especially with respect to speculating on the benefits of this form of support. Now, much more than just reducing poverty, many see the UBI as having the potential to influence a wider range of social ills as well as creating a foundation for a better society.

To extend the idea of political economic effects, the focus of basic income research is overwhelmingly on households that receive support. Indeed, much of the analysis resembles a neo-classical "two-factor" model … the low-income recipients and the high-income payers. In other cases, the dichotomy comprises the recipients and "government", aka tax-payers. In fact, it important to insert at least one additional income tranche, namely those whose income is just above the breakeven level. For example, in the recently cancelled Ontario Basic Income Pilot, the threshold for a single person was just above $16,000 annually. Any earnings above that resulted in an offset of 50% reduction in the basic income payment. Therefore, someone earning $14,000 (either in wages or other income), would receive an OBIP payment of almost $10,000, for a total income of $24,000.

Now here is the issue. Statistics Canada reports that the median income for Ontario in 2016 was $32,000. A check of popular job search sites such as Workopolis reveals that incomes for occupations such as data entry clerk typically start at around $30,000. With the advent of the so-called gig economy increasing numbers of younger Canadians can expect to cobble together several part-time jobs to make a living. Even though this strategy might result in incomes that would push someone beyond the reach of a basic income, it is conceivable that the availability of a basic income could induce those in the income tranche above the reach of the program to adjust their work effort. The elasticities measured in the seventies may have been unduly low because the focus was on those who received the benefit and because labour markets may have been more institutionally rigid.

This is similar, but not identical to what Kurz (1978) terms the "truncation" problem common to all social experiments that evaluate means tested programs. Most policy analysts failed to consider impacts of a means tested program on the income tranche just above the breakeven level. Further the data may not include sufficient numbers of those whose income lies between the payment (G) and the breakeven level (B). These households may self-select themselves out of the experiment under the belief they are not eligible. Hausman and Wise (1977) present techniques for estimating parameters I the face of such distortion.

Consideration of wealth remains an important omission from the entire inequality and basic income literature. The focus has been entirely on income. Yet examples of wealthy households with low income are increasingly common…these are seniors. Mincome did condition payments on wealth, but OBIP did not, with the result that it is entirely possible that unemployed singles could remain in their parents, rent free. Social assistance programs typically limit the wealth recipients may retain,

meaning that for most must deplete their assets, with the exception of primary residence. Most modern proposals for a basic income do not consider how wealth would condition benefits.

Finally, administrative issues have received limited attention from the NIT experiments. For most commentators, the issue of sending money to low income households appears simple, but complexities abound. A basic problem is "cadasterability" were a "cadaster" is a comprehensive list of land holdings (Wispelaere and Stirton (2007)). In the context of a basic income, it refers to the list of all eligible households. In principle, the income tax files could form a cadaster, but for two limitations. Obviously, income tax records track wealth poorly. More important is that many low-income individuals do not file tax returns. Social assistance payments are untaxed, and recipients would typically not need to file a return unless they received earnings where the employer deducted tax at source and filed statements with the income tax authority. Low- income households often work in the so-called underground economy and have incomes too low to trigger attention. They do not leave an administrative footprint.

Limited cadasterability has forced all NIT experiments to rely on a series of screening surveys to qualify participants. This raised the costs of the experiments and if maintained for the actual program, would blunt the potential of a basic income to trim bureaucracy. Reliance on surveys to monitor program outcomes remains a core defect of all NIT experiments.

1.1. The main themes and questions for a BI experiment

In most BI models, while households receive the benefits, it is important to track some responses at the individual adult member. This section poses some central issues for evaluating a basic income, grouped in three main themes with specific sub-questions. The final questions that will posed will reflect pragmatic concerns about data availability, the sustainability of data collection, and ethical/privacy constraints governing the use of administrative data and limits to primary data collection.

- *Theme 1: Changes in economic well-being and work effort*

Questions about how a basic income reduces poverty remain fundamental. Key sub-questions include

a. Do recipient households experience changed income at the individual and the household level?
b. Have reductions in earnings occurred for individual household members?
c. Has work effort changed for individual and household members (measured in hours of work)? Have these changes remained stable?
d. Have members of recipient invested in vocationally related education or training?
e. What non-work activities do members of recipient household engage in and how does this vary by major demographic?
f. Do recipients change housing consumption broadly defined? By broadly defined, is meant changes in housing condition, crowding, and location.
g. How to non-recipients in the immediately high tranche that the breakeven adjust their work effort? Is there any evidence that some reduce work effort to qualify for a BI?

- *Theme 2: Health and social outcomes*

The potential for a basic income to have positive impacts on health remain a central idea. Testing this idea within the context of a universal health care system such as exists in Canada,presents important measurement issues. Canadians typically do not face barriers to accessing primary care, except possibly in remote locations. More likely is that the "working poor" do not pay for access to supplementary health services. Those on social assistance usually obtain supplementary health services as part of the non-financial benefits package associated with this program.

Therefore, the causal link between an increased and more stable income as health outcomes is likely indirect, working through food security and changed stress associated with poverty. It is unclear whether increased resources will lead to reduced use of mental health services, reduced hospitalizations, and changed morbidity/mortality.

Based on the discussion in the previous section, another claim is that increased income supports increased engagement with society. Children especially may benefit when a household gains access to the internet and can afford a computer. Again, the causal relationships may be complex and indirectly lead to other outcomes, such as improved academic performance.

Key questions for the basic income – health link may include:

a. Do household members change (reduce/increase) use of mental health services?
b. Does the hospitalization rate change for members of recipient households?
c. How does the consumption of pharmaceutical change in terms of quality and type? What does any changed pattern indicate about changes in health status?
d. How do primary care change for household members (visits, immunizations, screening, etc…)
e. How does the BI affect family stability (divorce/marriage, adult children at home)?

It is important to stress that it is difficult to predict direction of some measures of health status or whether they imply an improvement in health status. Increased use of primary care is probably an indicator of future improvements, but more visits for mental health counselling may indicate the household members are taking a more proactive approach to care, or may be experiencing increased stress and reflect worsing mental health. A clear issue for any evaluation of the basic income is to undertake follow-up analysis to understand the changes in measured outcomes.

- *Theme 3: Administration of a BI*

The BI requires a means test on income. This requires identifying eligible responses, either by going to administrative data and/or enrolling using a constructed sample frame. The next section discussed the theoretical and practical issues of creating assembling the data for analyzing BI outcomes.

Some important administrative issues that need to evaluated include the following:

a. Will social assistance recipients (SAR) accept a transfer to a basic income? Will they remain within the BI experiment? What factors explain why SAR return to welfare?
b. Will the "working poor" accept an invitation to participate in a BI experiment?
c. What is the projected cost of a BI based on take-up and work effort adjustment?

These themes and sub-questions represent the core of avenues of inquiry for a BI pilot. Many have identified other social and cultural impacts, but no practical way exists to measure outcomes such increase artistic outputs. Changes in social cohesion and increase community spirit are similarly vague notions that as both hard to measure and would likely fail to motivate acceptance of a BI.

Answering these questions requires varying forms of data. The NIT experiments thus far, including the most recent OBIP, relied exclusively on survey data. This created serious biases due to non-response, item unreliability (respondent/interviewer variation in question interpretation), self-selection into the study, and self-selection out of the study (attrition). These data distortions undermine consistent estimation of impacts.

Given the evolution of large administrative datasets, an important goal for any future will be to align the themes/questions to the appropriate data sources. **Table 6** presents a comparison of administrative data (tax files and health records), sample surveys, and qualitative interviews in supporting different question constructs.

Table 6: Comparison of data form and support for question construct			
	Description	**Advantages**	**Disadvantages**
Administrative data	Information routinely collected due to respondent participation in service consumption activity (taxation records, health services,	• Fixed variable definition among respondents • Temporal alignment usually assured • High coverage rates across subject population • Low collection costs • Option to use entire population (if experimental costs permit) • Temporal alignment high	• Variable definition mat not align with theoretical constructs • Privacy barriers may impede use • Information sources may not share a common key (which creates barriers to merging datasets merged.)
Sample survey (fixed response categories)	Set questions with limited response options posed to a large sample (n>100)	• Samples usually randomly selected supporting statistical inference to population. • Question construct can closely align with theoretical concept. • Low cost per unit for collection and analysis.	• Interviewer/respondent misunderstanding of question intent. • Response categories and questions construct fail to capture theoretical concept • Non-response and selection biases. • The field of knowledge defined by researcher. • Temporal alignment careful logistical management.
Qualitative interview (open responses)	Open questions (respondent is free to respond in own words) posed to a small sample (n<50)	• Samples selected to include only respondents with relevant information • Respondents can expand the field of knowledge	• High cost per unit for collection and analysis • Analyst interpretation reduces reliability and validity • Temporal alignment low

Two elements of Table 6 may require explanation.

- A large sample survey requires fixed questions with stable response categories. The researcher defines the scope of responses and respondents must align their answers fit one (and usually only one) response category. The respondent cannot modify the answer except by adding a verbal comment or inserting a written comment if the researcher has allowed space in the margin or as an "Other(specify)" option. The cold reality is that most of the time researchers ignore such parenthetical comments.

  A qualitative interview, either singly or severally (focus group) poses general questions where respondents offer comments that researchers transcribe and then code into categories. Here the nature of responses shapes the categories, and in this sense the respondent shapes the information or field of knowledge, mediated of course by the researcher.

- The concept of temporal alignment receives little attention, largely because most longitudinal research uses administrative files. The NIT experiments have used survey data, where re-interviewing respondents at fixed intervals becomes almost impossible. Where external influences, such as a sharp change in unemployment or prices do not occur, temporal variability in interviewing may not pose much problem. Figure 3 shows this issue, while.

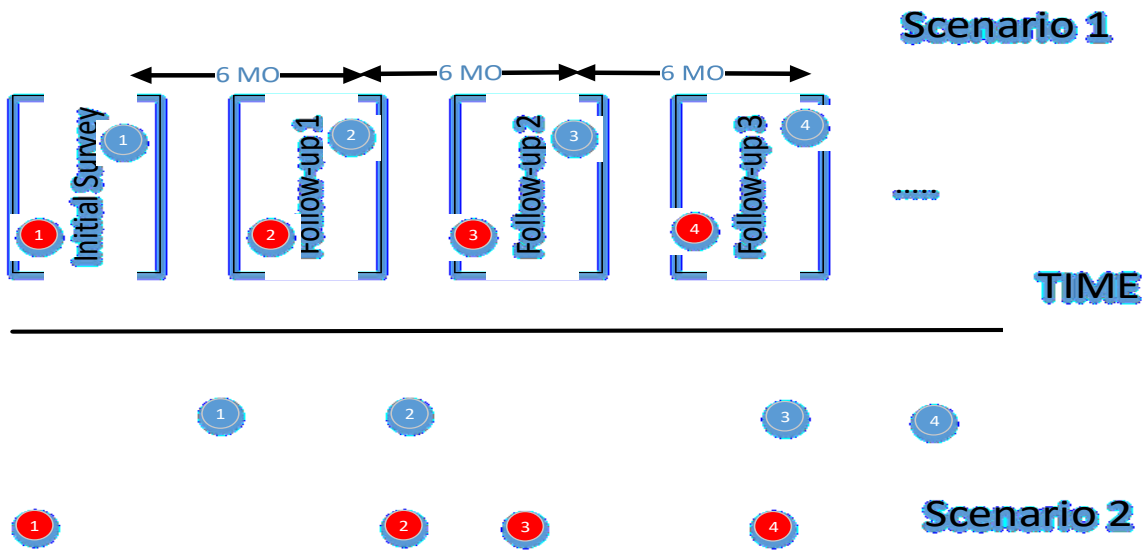

Figure 3 Temporal Alignment for panel data

Figure 4 illustrates the typical "spread" of interview timings of the Mincome periodic surveys. Note how different survey waves overlap. In the case of Mincome, where both adult household heads (if present) and children participated in an interview, revisits to secure all the interviews increased the costs considerably. Note the interviews conducted by Mincome, featured in-person, in-home interviews that typically required three hours and collected information on several hundred items. The modal interview numbers noted in Figure 3 illustrate the intensity of field effort required to collect primary data for a complicated panel study. In a modern context, where telephone or on-line techniques may be the method of data collection it may be possible to "tighten" the data collection, but this still requires substantial logistical control and incentives for respondents to complete surveys at the prescribed times.

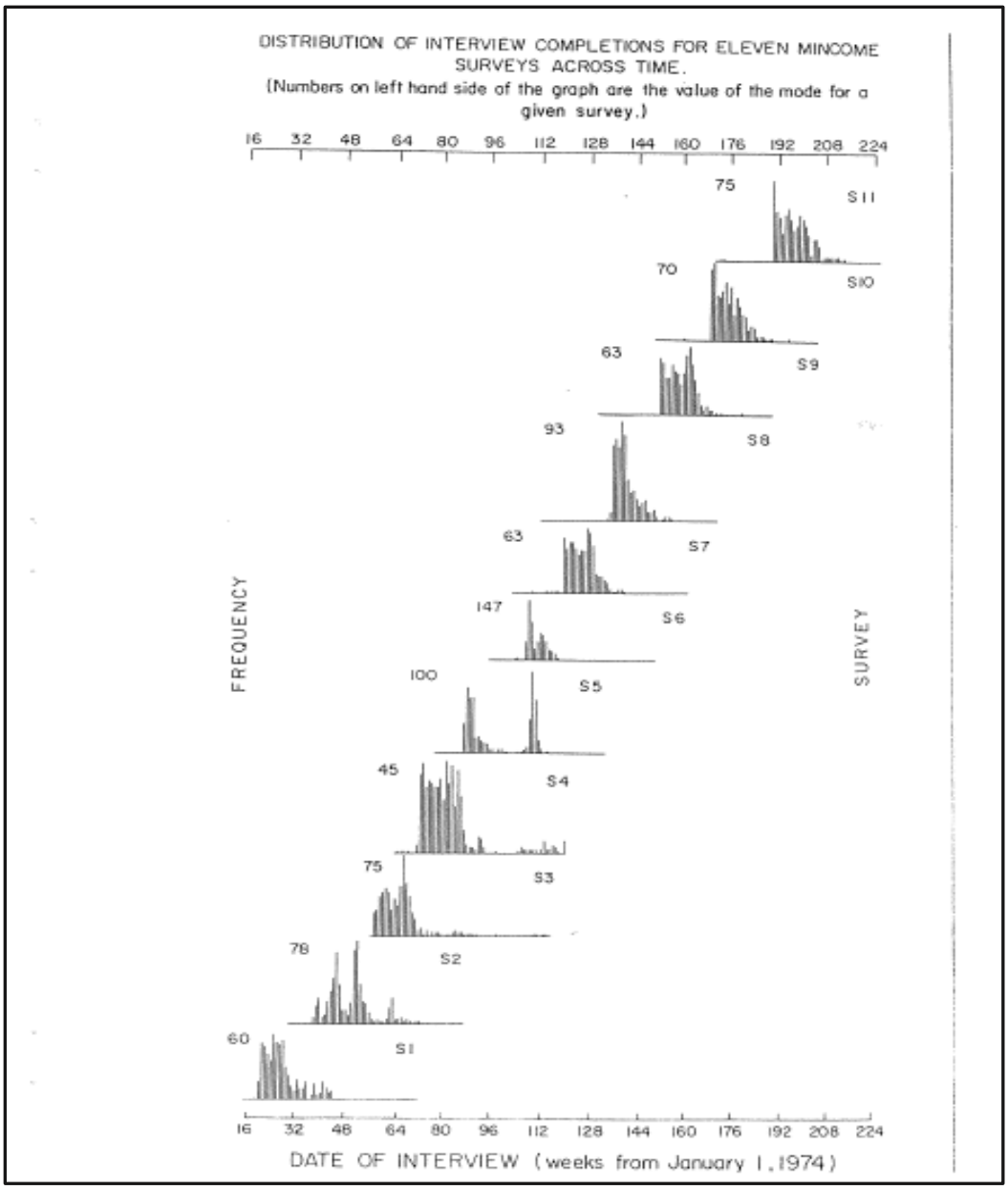

Figure 4 Distribution of Interview Completions for Mincome Survey

Table 7 aligns the evaluation themes/questions with the data source.

Table 7: Evaluation theme/question aligned to data source				
Validity/reliability/cost rating •••high •• moderate • low - not useful	**Administrative data**		**Sample Survey**	**Interview/Focus Group**
	Tax files	**Health Records**		
Theme 1: Changes in economic well-being and work effort				
a. Do recipient households experience changed income at the individual and the household level?	•••	-	••	-
b. Have reductions in earnings occurred for individual household members?	•••	-	••	-
c. Has work effort changed for individual and household members (measured in hours of work)? Have these changes remained stable?	•••	-	•	-
d. Have members of recipient invested in vocationally related education or training?	••	-	••	-
e. What non-work activities do members of recipient household engage in and how does this vary by major demographic?	••	••	•••	••
f. Do recipients change housing consumption broadly defined? By broadly defined, is meant changes in housing condition, crowding, and location.	••	••	••	-

Table 7: Evaluation theme/question aligned to data source				
Validity/reliability/cost rating •••high •• moderate • low - not useful	**Administrative data**		**Sample Survey**	**Interview/Focus Group**
	Tax files	**Health Records**		
g. How to non-recipients in the immediately high tranche that the breakeven adjust their work effort? Is there any evidence that some reduce work effort to qualify for a BI?	•••	-	•	••
Theme 2: Health and social outcomes				
h. Do household members change (reduce/increase) use of mental health services?	-	•••	••	
i. Does the hospitalization rate change for members of recipient households?	-	•••	•	•
j. How does the consumption of pharmaceutical change in terms of quality and type? What does any changed pattern indicate about changes in health status?	-	•••	-	-
k. How do primary care change for household members (visits, immunizations, screening,…)?	-	•••	••	-

Table 7: Evaluation theme/question aligned to data source				
Validity/reliability/cost rating **•••high** **•• moderate** **• low** **- not useful**	**Administrative data**		**Sample Survey**	**Interview/Focus Group**
	Tax files	**Health Records**		
l. How does the BI affect family stability (divorce/marriage, adult children at home)?	•••	•••	••	-
Theme 3: Administration of a BI				
a. Will social assistance recipients (SAR) accept a transfer to a basic income? Will they remain within the BI experiment? What factors explain why SAR return to welfare?	•••	-	••	•••
b. Will the "working poor" accept an invitation to participate in a BI experiment? Why do they decline to participate?	•••	-	••	•••
c. What is the projected cost of a BI based on take-up and work effort adjustment?	•••	-	••	-

This discussion reinforces the idea that administrative data, specifically tax files and health records should form the data foundation for any future BI pilot/experiment. In Canada, with its universal health records, provinces have access to health records on the population. Data on health provider billings, pharmaceutical usage, primary care screening, and other health services are becoming

increasingly complete and timely, especially as electronic medical records are finally permeating the system. Supplementary and complementary health services would require sample surveys.

The most serious defect of administrative data is that research must adapt concepts to the variables as defined, usually for administrative purposes and not with research needs in mind. Survey questionnaires can include latent and abstract measures such as food security as well as attitudinal and psychological states.

Income tax records can record earnings, other income sources, medical deductions, work deductions and education/training expenditures that also generate deductions. The tax records become the primary data source for predicting uptake in the BI and cost projections.

In this view, sample surveys that have formed the data "backbone" for prior NIT experiment become supplementary. Qualitative research, remains important to answer the "why" questions. The next section presents a methodological design for a NIT experiment.

Many see Mincome as a standard RCT, but allocation to the treatment and control groups did not feature the usual randomization process. Rather an "assignment" process placed participants in a treatment or control group based on their expected "informativeness". As explained by Hum et al. (1974) the allocation results from a solution to a linear programming algorithm designed to maximize informativeness subject to a cost constraint. Most of the later income maintenance experiments used this approach, and while it may have managed costs, has been subject to criticism (Lyall (1975), Bernstein (1975)). The assignment model results in a non-orthogonal design that impedes straightforward multivariate analysis. Most important is that the information function which the assignment model seeks to maximize, must include all relevant factors that determine work behaviours, but how can one know these before the fact?

Development of the sample, proceeded using a series of preliminary surveys, before the formal enrollment and assignment of participants to the treatment and control group. After enrollment, a series of periodic surveys tracked respondents over 224 weeks

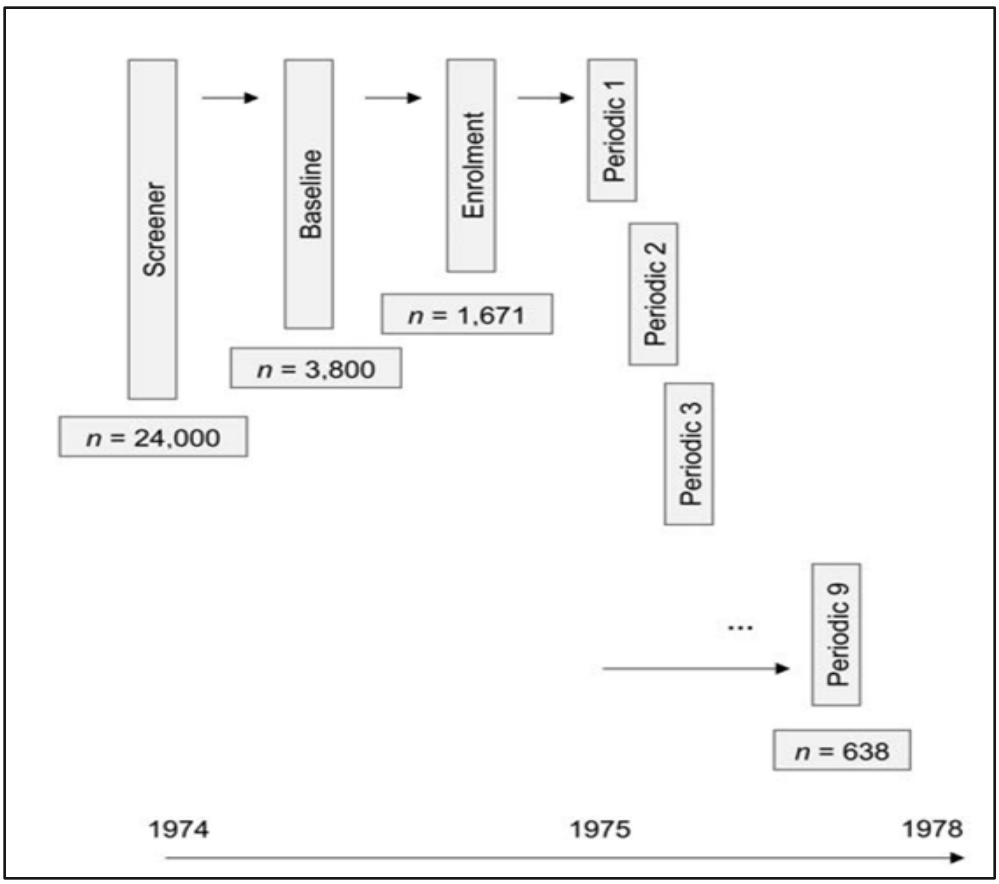

Figure 5 Survey structure for Mincome

Mincome samples shrink by about 30% during the panel period. Participants left the experiment for many reasons, but often because their earnings rendered them ineligible for benefits. While they could receive modest compensation for completing surveys, some elected not to once their payments from Mincome dwindled. Kurz (1978) expressed concern about the level of attrition and initial refusals, especially for the saturation site, Dauphin, but finally did conclude that the Mincome data were usable.

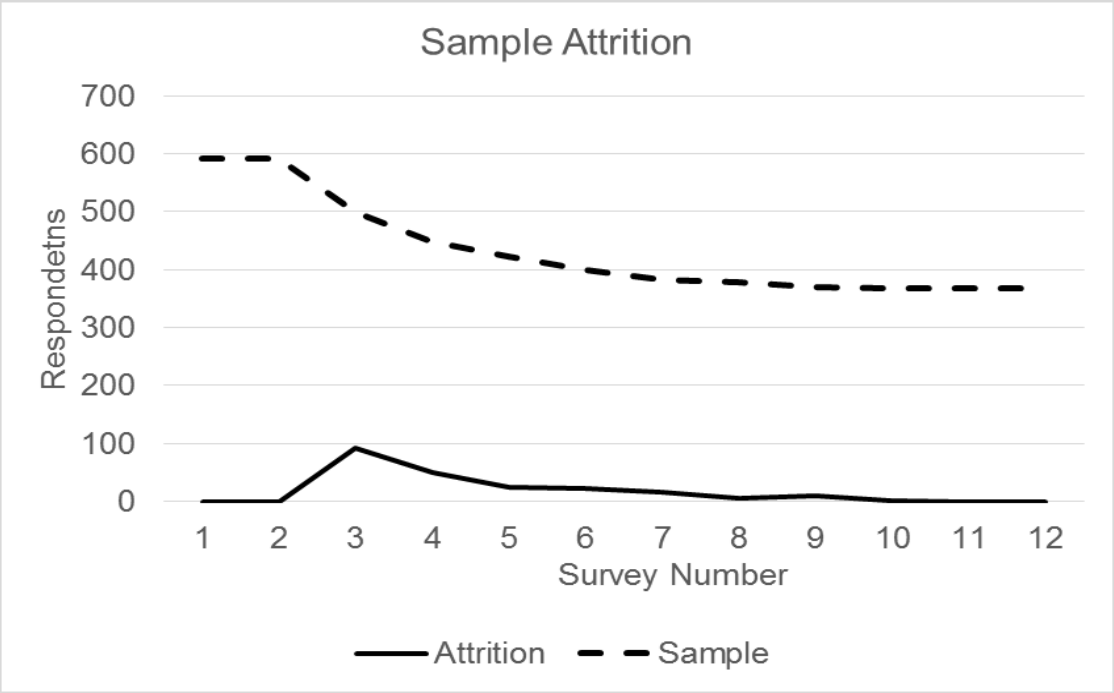

Figure 6: Typical Attrition in Mincome

A serious issue arose for Mincome at periodic 2. It appears that in the original sample development (Screener survey) a frame error resulted in under representation of a specific low-income cohort (Sabourin 1974). Further more, many respondents received unexpectedly high transfers (not social assistance) that rendered them eligible to anything but minimal payments. This blunted the experiment and raised serious concerns about the validity of the experiment. Accordingly starting with Periodic 3 a supplementary sample was initiated which explains why the surveys persisted beyond the originally planned nine periodic surveys. The relationship between the main and supplementary samples remains unclear.

The assignment allocation and attrition create non-random disturbances. In varying degrees, similar issues occurred for the other income maintenance experiments. Researchers have typically ignored these issues and proceeded as if the resulting data were orthogonal. Since the estimation of corrective weighs seems difficult, this approach is understandable.

A unique and important feature of Mincome was the administrative process. The sponsors (Canada and Manitoba) created a separate non-governmental non-profit entity to administer all aspects of the program including surveys, enrollment and payments. Mincome Inc. also became the face of government and prepared tax returns on behalf of the participants.

1.2. Ontario basic income pilot design features

OBIP emerged some 40 years after Mincome.

Many rued this termination but the pilot encountered some significant problems that may have limited its capacity to test key hypotheses. These limitations included:

- Distorted sample frame development that failed to enumerate the eligible population

One might imagine, that is easy to select a sample of low-income households. After all, there are income tax and social assistance records. Three things undermine the use of these records in social experiments.

First, Canada Revenue Agency (CRA) has always been very careful about sharing tax information. For example, only in 2016 could Statistics Canada join income tax records to census information. Provinces that wish to use federal income tax data for program administration need to negotiate a data sharing agreement with CRA. This was not done prior to OBIP.

Second, while Ontario has access to records since it levies a personal income tax, conducting such an experiment was not within the mandate of the Ministry of Finance. A negative income tax is a tax program and needs to be delivered by a tax authority, namely the Ministry of Finance. When the Ministry of Finance could not accept responsibility for OBIP, the Ministry of Community and Social Services became the home for the pilot. In principle, this should have opened access to social assistance records but privacy barriers forestalled access to these data as well.

Third and most importantly, many individuals who would qualify for a basic income "fly under the administrative radar". Those on social assistance and many low-income households do not file tax returns. So, sampling from income tax and social assistance records misses many potentially eligible participants.

- Flawed randomization process due to a convoluted and complex enrolment process.

OBIP to use an open enrollment process in the two main sites – Thunder Bay and Hamilton. The result was that enrollment relied on a letter sent to a general sample within the two test sites. This failed dramatically. First, after the privacy lawyers finished with the introductory materials, the invitation became long, legal, and impenetrable. It took more than a month to trim the introductory materials further delaying enrolment. Second, it always surprises planners, how many low-income individuals fail to enrol in programs that would increase their financial well-being. Everyone involved was astonished at the low take-up of the pilot. Third, it required extensive support and re-contacting to secure tax and banking (for direct deposit) information from applicants to finalize their eligibility

The Pilot started mailing invitations in June of 2017 and by September, after mailing 37,000 invitations it had managed to enroll barely 150 participants, well short of the original target of 2000. This prompted a revised enrollment process that involved direct solicitation through community organizations, which after great effort did manage to raise enrolment. However, we are now a long way from a random allocation of participants into treatment and control groups.

- Collapse of the control group

OBIP sent conformations to participants that suggested the recipient would be receiving cheques. In fact, the province then informed a subset that they would not receive benefits but would receive $50 for completing periodic interviews. Little wonder than many declined this offer, with the result that the control group never formed.

- Costly data collection processes

Even though most respondent had access to a smartphone, most respondents preferred to complete a printed questionnaire they needed to mail. Anyone familiar with survey methodology understands that such data collection requires persistent and costly follow-up to the point of harassment. A printed questionnaire with conventional mail back requires the most follow-up, as many as 10 attempts before the respondent is abandoned. Non-response is usually high in these situations.

- Multiple hypotheses require highly reliable and valid data and a strictly managed RCT.

The last question is probably the most important. The large-scale NIT experiments used sample designs that focused on core hypotheses about the labour market response of recipients. The researchers optimized the designs to detect the relatively small effects expected. Other hypotheses have been tested using these designs, but ideally each causal theme, such as the impact of a NIT on health outcomes requires a sample (treatment and controls) developed specifically for that set of hypotheses. OBIP was attempting the test a range of hypotheses, where the effects were all likely to be relatively small. This requires high quality data, and with an RCT that is strictly maintained over time. OBIP was unlikely to achieve this and researchers would have had to resort to complex statistical procedures to extract causal relationships. This would have likely created more debate rather than answering key questions. Going back to how the results of modest labour market impacts derailed the major income maintenance experiments of the seventies, it is crucial that any future experiments of major social policy produce unambiguous results. Contested outcomes, where researches engage in dueling op eds is not the way to move the basic income forward.

It is important to underscore that these criticisms of OBIP rest on Phase 1 results. It appears that Phase 2 was more successful in recruitment, but without any reporting of interim results from this phase, one can only speculate on the quality of the data and the integrity of the sample design. Specifically, would there have been an orthogonal control group? Would respondents have participated in follow-up surveys? Would respondents have agreed to link their survey responses to health records? Would respondents have allowed their tax information to the survey and health data? Would the cells of the design matrix (**Table 5**) retain sample over several survey cycles?

1.3. Methodological proposal for a basic income pilot

It is now possible to sketch the main parameters for a new basic income pilot, anchored around some core ideas

1. *Data development should rely on administrative information*

The most important principle is that administrative data must form the core of hypothesis testing. Specifically, tax and health records will support key hypotheses testing. In Canada, this means that

the federal and provincial governments must co-sponsor a pilot, unlike OBIP where the provincial government remained the sole sponsor.

Such cooperation is essential to ensuring that tax records and social assistance case files can form the basis for random selection. Data sharing agreements and privacy issues require careful scrutiny to smooth legal impediments to using confidential information to anchor a pilot.

2. *Designating geographic sites should be avoided*

Mincome and OBIP designated specific sites. For both OBIP and Mincome, part of the rationale was to minimized data collection costs. Other reasons for selecting regional cities for OBIP was to avoid the overheated Toronto housing market, although it is unclear that those who would participated in a basic income are affected by runaway price inflation in Rosedale or Richmond. A specific geographic site also controls for local economic changes. More likely is that politicians like the idea of defining geography where funding will flow to create ribbon cutting opportunities. Political considerations were at play when the provincial selected Dauphin as the so-called saturation site.

The notion of a saturation site to test basic income impacts is especially suspect. Much of the rationale centers on rather obscure notions of community effects and the potential for such "isolated" communities to serve as a utopian petri dish. Finding such communities is quite difficult, since in a digital age, the concept of isolation, where migration is low, this tends to occur only in remote settlements with constrained economic opportunity. The generalizability of any results emerging from such settings would be very low.

Prior to OBIP designating Lindsay as a saturation site, Mincome was unique in the NIT experiments in creating Dauphin as a test site. It is remarkable that the Mincome is often referred to as the "Dauphin experiment", when it was in many ways a complete failure. First the take-up rate was very low. At most perhaps as many as 50% of the eligible population was enrolled to receive benefits. Second, many recipients received other forms of income as the experiment progressed and the share of Mincome payments in the Dauphin "GDP" dwindled quite naturally. Third, and relate to the second point, the mid-seventies marked the first of a series of block buster sales of Canadian grain to Russia. As a regional agricultural centre, Dauphin residents and business could have benefitted

differentially compared to the rest of Manitoba. In that context, it is entirely possible that observed change in employment, social. and health outcomes may have had little to do with Mincome payments and more related to standard economic developments.

If administrative data become the core of any future pilot, it makes sense to "marble" participants throughout the population.

3. *Conducting a "quiet" experiment*

The last two points lead to the creation of a "quiet" experiment. Media just loved to locate participants in these social experiments and feature case studies. This undermines measurement as existing participants become self-conscious and researchers, especially interviewers, communicate intent unwittingly. Large social experiments, like all experiments must be quiet and anonymous, if we are to extract valid parameters.

4. *Multiple sub-experiments*

In line with the use of administrative data, which lowers costs, rather than a single experiment, why not conduct several experiments. Once can use tax records to assess labour market interactions and health records to assess health outcomes. This will mean trimming the range of hypotheses being tested, but it is far superior to conduct successful analysis on a few well-defined questions than to try to show-horn in every possible interaction within a single sample.

5. *Surveys have a place*

Some important questions will not be supported by administrative data alone. Placing the bulk of the data burden on administrative will allow surveys to become targeted and much shorter. This increases the likelihood of successful on-line survey management, thereby discipling costs. The Mincome survey of 1 – 2 hours and hundreds of items and even the OBIP survey that typically to required 30 minutes on the phone are just too long to maintain reliable and valid information over a series of panels.

6. *Administrative structure*

Mincome created a separate corporate entity to manage all aspects of the experiment. This became the face of government for recipients and aside from disbursing payments and managing the surveys, it prepared tax returns. This served to increase the accuracy of information collected in the

periodic surveys. It also allowed government to fund the entity using annual allocations rather than incrementally extending budgets.

The danged is that the separate entity can become disconnected from the political realities of a major policy study. A critical error of Mincome Inc. was to focus on administrative and scientific issues, and not maintain and information flow of interesting results. Funders became impatient that the increasing expenditure had not resulted in any release of results.

2. Summary and conclusions

The basic income endures as major policy proposal, yet empirical social science has been unable to shed light on the potential outcome. The NIT experiments have been marked by a series of missteps. However, with the increased use of "big data", it is possible to create a valid and lower cost evaluation process that could produce important insights to support a revolutionary anti-poverty policy.

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