Basic Income Experimentation Yesterday and Today:
Challenges, Achievements, and Lessons

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Abstract

Policy discussion around a basic income is often based on a limited understanding of a vast literature on income maintenance issues that has been compiled over the past half century. In particular, significant social experimentation has tried to understand how a basic income program might be effectively designed and delivered and what its impact might be on the twin objectives of equity and efficiency. Those who wish to promote the concept of a basic income would be well advised to understand what research has been done on income maintenance and how it relates to modern circumstances. And those contemplating basic income pilot projects can learn a great deal from the challenges and accomplishments of fairly ambitious social experimentation conducted in the past. This paper provides an overview of the experiments to assess what we have learned from them, what questions remain unanswered, and what new issues arise in a modern, technologically advanced economy and labour market.

Introduction: Why Basic Income?

> . . . a foolproof experiment requires only an unlimited budget, lots of time, and prior knowledge of almost everything that might be learned from the experiment. More realistically, future experimenters will have to deal with the complications of resource limitations, great yawning gaps in our knowledge, and political constraints inherent in the nature and timing of the impulse to do an experiment . . . (Watts & Bawden, 1978, p. 67)

The development of a basic income (BI) is a very active Canadian policy issue to address the social consequences of enduring poverty and the spectre of incipient technological displacement of workers and jobs. Concrete initiatives at the provincial level include the explorations into a *revenu minimum garanti* (minimum guaranteed income) in Quebec (Gouvernement du Québec, 2018), Ontario's recently cancelled basic income pilot project (Ontario Ministry of Finance, 2016; Segal, 2016), and now the plan for a focused look at a basic income in British Columbia (Government of British Columbia, 2018) that accompanies ambitious and legislated poverty reduction goals (Government of British Columbia, 2019).[1] These initiatives have a degree of support at the federal level, where the governing Liberals have introduced a formal poverty reduction strategy (Employment and Social Development Canada, 2018) and adopted a party platform proposal for a "minimum guaranteed income" (Liberal Party of Canada, 2016).

Policy discussion around a basic income is often based on a limited understanding of a vast literature on income maintenance issues that has been compiled over the past half century. In particular, significant social experimentation has tried to understand how a basic income program might be effectively designed and delivered and what its impact might be on the twin objectives of equity and efficiency. Those who wish to promote the concept of a basic income would be well advised to understand what research has been done on income maintenance and how it relates to modern circumstances. And those contemplating basic income pilot projects can learn a great deal from the challenges and accomplishments of fairly ambitious social experimentation conducted in the past. This paper provides an overview of the experiments to assess what we have learned from them, what questions remain unanswered, and what new issues arise in a modern, technologically advanced economy and labour market.

The remainder of the paper begins with a review of the North American negative income tax (NIT) experiments conducted from 1968 to 1983. These experiments generally concentrated on a relatively narrow set of issues around the efficiency costs of income redistribution, but they also provided a wealth of detail on the prospective design and delivery of a negative income tax in the 1970s. The experiments also raised a variety of issues concerning the evaluation of a social experiment and provided some answers to fairly specific questions such as the impact of a negative income tax on work disincentives. The paper then examines one unique feature of

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[1]There is also discussion of a possible basic income pilot for Prince Edward Island, which is at the exploration stage with that province's Committee on Poverty: https://www.assembly.pe.ca/committees/current-committees/special-committee-on-poverty-in-pei

the experiments, the Dauphin saturation site in the Manitoba Basic Annual Income Experiment, which can be interpreted as a bridge between the randomized controlled trials conducted on a dispersed sample of families and some of the later basic income pilots and research. I also consider technological, economic, and labour market developments since the period of negative income tax experimentation that might have implications for the lessons learned from the NIT experiments as well as raising new issues.

The paper then examines recent basic income research, including both a series of basic income pilot projects in Ontario and elsewhere and microsimulation analysis as it relates to the issues around a basic income. The section assesses the modern basic income projects in the context of the earlier negative income tax experiments to see what has been added to our understanding of the issues around the design, delivery, and evaluation of a basic income. As new issues have arisen around the larger questions of the social benefits and costs of a basic income, I consider what has been learned about these issues from the recent BI pilot projects and the extent to which these larger questions can be addressed through further experimentation. For many issues, an alternative to further experimentation is microsimulation, which can address issues of design and financing using modern information technology. The final section of the paper summarizes what has been learned from the negative income tax experiments and the basic income pilots about the effective delivery of a modern income support program. It also considers whether further experimentation can effectively supplement what we know or whether alternative methods, such as microsimulation, provide the best vehicle to assess basic income policy in the future.

The North American Negative Income Tax Experiments

Some Terminology: Basic Income, Negative Income Tax, Randomized Controlled Trial, Experiment

"Basic income" has become the popular term for universal unconditional income support initiatives, particularly since the Basic Income Earth Network (BIEN) was founded in Europe in 1986 (Caputo, 2008). The BIEN concept of a basic income involves a payment to all individuals regardless of income. The term has been loosely used to describe other income maintenance programs, including earlier negative income tax proposals which are based on the family rather than the individual and which are income tested to restrict benefits to lower-income families. The concept of a negative income tax is usually attributed to American economist Milton Friedman (1962), although precursors can also be cited. Friedman's popularization of the idea coincided with U.S. President Lyndon B. Johnson's much publicized War on Poverty (U.S. Congress, 1965), led to an important series of innovative social experiments to test specific negative income tax plans, and has endured as the preferred North American income support option. The term "basic income" often subsumes negative income tax proposals, at least in Canada and the United States.

Arguments for a negative income tax encountered resistance from those who feared significant efficiency costs in the form of work disincentives. Unconditional income transfers would be expected to have adverse effects on labour supply and output capacity, but how large might these effects be? At some point, the efficiency costs would outweigh the equity gains.

How could precise and credible answers to this specific question of the work disincentive effects of a negative income tax be provided? The answer was to conduct randomized controlled trials (RCTs). Although such experiments had been used widely in medicine, biology, and other areas since the 18th century, they were new to economics at least insofar as human subjects were concerned (Levitt & List, 2009).[2]

RCTs involve the random allocation of subjects to either a negative income tax treatment or a control group to provide unbiased estimates of the impact of the treatment, where the precision of the estimates could be predetermined by the experimental sample size. RCTs are considered the gold standard for empirical evidence in medicine, and their application to address the question of the size of the labour supply response to a negative income tax plan seemed appropriate and straightforward. Burtless (1995) provides a thoughtful and favourable review of the arguments for and against RCTs as a guide to economic policy decisions. He also notes that the term "experiment" is often used more broadly in the social sciences to consider other types of pilot programs or demonstration projects or simply innovative policies or practices that do not conform to a RCT. We return to this question in the discussion of modern basic income pilot projects later in the paper.

The Negative Income Tax Experiments: Conception and Design

The Office of Economic Opportunity (OEO) was created in 1964 to steer President Johnson's War on Poverty and soon became a strong advocate for a negative income tax program (Basilevsky & Hum, 1984, p. 6). The OEO also refined the arguments to the economic issue of work disincentives and supported the idea of an experiment designed to focus on this question, especially as it pertains to able-bodied adults in non-welfare low-income families.[3] When other funding sources could not be found, the OEO approved a grant from its own funds in 1967 to the Institute for Research on Poverty at the University of Wisconsin in conjunction with Mathematica Inc. for the first "work incentives experiment" (Levine, 1975). It is important to understand that the "experiment" was conceived as a RCT that randomly allocated a scattered or dispersed sample of families to treatment and control groups and not simply as a demonstration project without a randomly allocated control group, although this latter design had some support even as the experiment proceeded (Basilevsky & Hum, 1984, p. 7).

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[2] Levitt and List discuss the origins of modern field experimentation, including the important work of Ronald Fisher and Jerzy Neyman in developing valid randomization techniques for agricultural testing. This work involved agricultural plots rather than human subjects, however. Levitt and List attribute the idea for a field experiment to test a negative income tax to Heather Ross, who developed her PhD dissertation at the Massachusetts Institute of Technology around a proposal that formed the framework for the New Jersey experiment.

[3] This is most clear in Christopherson (1983) where he reports that the Seattle-Denver focus was the question: "How would non-welfare, low-income families respond to a NIT?" A clear statement of objectives is obviously a critical feature in designing an effective experiment.

Although there were advocates in the academic community for a nationally dispersed sample, logistical and cost considerations favoured a more localized experiment with significant state co-operation (Skidmore, 1975, p. 30). Enthusiasm in the New Jersey state government and several of its agencies for an experiment was reassuring, given the uncharted waters associated with a RCT to address social problems. Moreover, unlike many other states, New Jersey did not generously supplement the federal government's Aid to Families with Dependent Children (AFDC) payments, which would compromise the choice of treatment options and discourage the target group, males with dependents in urban industrial areas, from participating in the experiment. New Jersey thus became the "test bore" for experimentation on the negative income tax (Skidmore, 1975, p. 31).

Although New Jersey became the first income maintenance experiment out of the chute, it was quickly followed by other, often larger and more ambitious projects. The OEO quickly followed the New Jersey experiment with further funding through the Institute for Research on Poverty for a Rural Income Maintenance Experiment in Iowa and North Carolina. The Department of Health and Welfare then funded two additional experiments in Gary and Seattle-Denver.[4] Christopherson (1983, p. 8) lists three reasons for the follow-up experiments:

- assessment of the reliability of the results through replication
- assurance that the results were not site specific
- expansion of the experimental scope beyond basic NIT treatments

As Figure 1 illustrates, all four experiments were in the field by 1971. On reflection, one consequence of this experimental fervour was that there was little opportunity for the successive U.S. experiments to learn from their predecessors (Hausman, 1986). Each experiment was left with little guidance in social experimentation and likely strong incentives to mimic the New Jersey design in most respects. Although the Manitoba Basic Annual Income Experiment (Mincome) lagged behind its U.S. counterparts by a few years, its main component—the dispersed sample design in Winnipeg—largely followed the New Jersey lead as well, perhaps because critical assessment of that experiment and the others in the U.S. was not yet available. As a result, New Jersey will be treated as the basic design to discuss in more detail, leaving the focus of discussion for the other experiments to their adjustments and enhancements to the New Jersey design.[5] This includes the unique Dauphin saturation site beyond Mincome's Winnipeg sample, which will be discussed in a separate section below.

[4] Really three experiments, as the Seattle and Denver sites were far apart and commenced at different times.

[5] A similar approach is taken by Ferber and Hirsch (1982, chapters 4 and 5, 48–97).

Figure 1

Timelines for the Five Income Maintenance Experiments

1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978
	——→New Jersey (& Pennsylvania)——→									
		→Rural (Iowa & N. Carolina)→								
		→Seattle (1970)-Denver (1972)————————————————————→								
			→Gary——————————————→							
						→Manitoba——————————————————→				

Sources: Pechman and Timpane (1975) for New Jersey; Palmer and Pechman (1978) for Rural; SRI International (1983) for Seattle-Denver and Gary; Simpson, Mason, and Godwin (2017) for Manitoba.
Notes: Timelines reflect the period of field activity; the experimental design would have occurred earlier.

Each of the income maintenance experiments was designed to allocate families randomly to either one of a series of treatment groups or to a control group that continued with existing income support programs but was monitored in a similar fashion to those treated. The treatment groups were defined by an income guarantee (G) paid to a family with no eligible income and a negative income tax rate (t) that reduced payments (P) as eligible or taxable income (Y) rose. For New Jersey and most of the experiments that followed it, the tax rates were flat or linear rates, resulting in a simple benefits formula:

$$P = G - tY \qquad [1]$$

The guarantee was defined as a percentage of the poverty line, which in turn varied by family size and composition.[6] Payments were conditioned on income, or means tested, and would cease at the break-even level of income (*B*) that defined the spectrum of eligible beneficiaries:

$$B = {}^{G}\!/_{t} \qquad [2]$$

The top row of Table 1 shows the treatment design for New Jersey and each of the experiments. Each experiment involved multiple treatments, or combinations of G and t, ranging from four treatments in Gary to 11 treatments in Seattle-Denver. New Jersey had 10 treatments associated with tax or benefit reduction rates varying from 30% to 70%, and guarantee levels, or levels of maximum income support, varying from 50% to 125% of the poverty line. The treatment effects vary around a tax rate of 50%, a figure that has been regularly used in discussion of a prototypical negative income tax plan (e.g., Wikipedia, 2019).[7] For those

[6] The poverty line did not necessarily correspond to any simple formula and was generally lower across family size than the Social Security Administration index by about 5–15% (Kershaw and Fair, 1976, 8).
[7] Friedman's (1962) initial proposal adopted a negative tax rate of 50% and it seems to have endured in both academic and policy circles.

receiving an experimental treatment, any positive tax owing was offset so that the experimental tax rate (t) was operative. The guarantee levels are less easy to characterize but varied around 75% of the poverty line for New Jersey and the Rural experiment, with higher levels for Seattle-Denver and Gary and a lower range for Manitoba. It is important to note that the multiple treatments in each income maintenance experiment served a specific purpose: to understand how labour supply response would vary depending on the design (G, t) parameters that would redefine the budget line faced by treated participants. As we discuss further below, the question of labour supply response dominated design considerations for New Jersey and this preoccupation carried forward into all the other experiments. For other questions, had they been considered of similar importance, multiple treatments may not have been necessary.

The preoccupation with labour supply response to a negative income tax was likely also a determining factor in the decision to select a dispersed or scattered sample over specific local sites for each experiment.[8] The alternatives—saturation sites where everyone received a single treatment or a national scattered sample—were rejected as problematic. A saturation site invited criticism about the development of a suitable control group at another, perhaps distinctly different, set of locations and a national sample involved much greater administrative complexity and population heterogeneity that would reduce the precision of labour supply response estimates (Watts & Bawden, 1978, pp. 57–59). The criteria for the choice of specific local sites appears enigmatic, however, as this characterization of the New Jersey experience indicates:

> Early in the experiment, it was decided to enroll sites sequentially . . . to test the effects of differences in industrial composition and tightness of the labour market . . . Trenton was chosen as the first site because of its proximity to Princeton and Mathematica, because it was the seat of the New Jersey state government, and because of the cooperative attitude of the United Progress Incorporated of Trenton . . . [but] ethnic balance became an obvious problem. The Paterson-Passaic sample was as bad . . . We hoped that Jersey City would be white enough to redress the balance, but the sample . . . [was only] 13 percent white . . . The largest industrial areas close to existing experimental sites were Scranton-Wilkes-Barre-Hazelton and Allentown-Easton-Bethlehem . . . Scranton was presumed to have the largest low-income population and, after preliminary sampling, it was selected . . . The experiment now had an ethnically balanced sample—roughly one-third fell into each of the major ethnic groups. (Skidmore, 1975, pp. 52–53)

Because New Jersey had concentrated on smaller urban industrial areas in the Northeast, the subsequent experiments chose a series of rural sites in the U.S. South (North Carolina) and Midwest (Iowa), a Midwestern urban site with a large black population (Gary), and two larger urban areas in the West (Seattle and Denver), although the rationale for these choices from the abundant geographical options available is unclear. Christopherson (1983, p.

[8] The exception is Manitoba, where the design included the Dauphin saturation site that will receive separate treatment in section 3.

8) reports that concerns about the depressed Seattle labour market when the experiment was launched in 1970 accounted for the addition of Denver to that experiment in 1972.

Each experiment did provide at least one new experimental wrinkle. The Rural experiment concentrated on administrative matters, including treatments that varied the accounting period between one and three months and introduced a new payments system. Seattle-Denver, the largest and most ambitious of the experiments, varied the duration of the experiment and introduced declining (non-linear) tax treatments, counseling, and subsidies for direct training costs. Gary randomly assigned social services, counselling, daycare subsidies, and work requirements to participants. Manitoba introduced a tax on net worth in the determination of benefit payments as well as the Dauphin saturation site. Each of these experimental innovations was a “one-off” not repeated elsewhere.

As Table 1 suggests, the attractive simplicity of the negative income tax experimental design in terms of a series of (G, t) and other treatments administered to a dispersed sample of low-income families at specified localities belied a number of other essential practical issues.

The duration of the New Jersey experiment was fixed at three years, although this decision appears to have been more by accident than careful design, as a request to OEO during the experiment to extend the treatment for a subsample of participants was declined, a decision that Skidmore (1975, p. 33) characterizes as a “mistake.” The subsequent experiments retained the three-year time horizon despite concerns about the “time horizon effect” on labour supply and other responses (Ferber & Hirsch, 1982, p. 36), except the Seattle-Denver experiment which designed treatments lasting three, five, and 20 years to test the impact of experimental duration.

Eligibility rules for inclusion in the experiment involved both demographic and income considerations. New Jersey’s focus was households headed by a male between 18 and 58 years of age with a spouse and dependents. Moreover, the New Jersey and Pennsylvania sites were also cities with a population over 50,000, where only about one-third of Americans in poverty were living (Bawden & Harrar, 1978, p. 27). The subsequent U.S. experiments generally expanded the demographic scope to sample single parents (especially female heads), single individuals, and older families as well as people in rural areas, where another third of Americans in poverty resided. These experimental initiatives, as well as the geographical diversification, were designed to capture the national population more closely. The Manitoba experiment included a dispersed sample in its large urban area (Winnipeg), a saturation site in a smaller community (Dauphin), and a rural dispersed sample.

The other eligibility criterion was clearly income, as higher-income families would be above the break-even level for negative income tax payments and generally were not of interest. Moreover, the OEO resisted the idea of higher-income families receiving benefit payments, however small (Skidmore, 1975, p. 42). New Jersey therefore established an income cutoff of 150% of the poverty line for two-parent families headed by a male, and the Rural experiment followed suit. As a result, families with working wives typically exceeded the cutoff and were underrepresented in the New Jersey sample. Moreover, Hausman and Wise (1975, 1976) identified income truncation as a potential source of bias in the analysis of labour supply response. These concerns led to much higher cutoffs of 240% of the poverty line for Gary and about 275% for Seattle-Denver,[9] although Manitoba conformed more closely to the income cutoffs established by New Jersey.

It was recognized from the beginning that the appropriate measure of income to determine eligibility should conform to some permanent or long-term expected income standard, given the potential volatility of high-frequency measures of family income. That is, a short-term measure of income would tend to include higher-income families with a temporarily low income and exclude lower-income families with a temporarily high income. New Jersey based eligibility on "normal income," which amounted to the income for the previous year reported in the pre-experimental or baseline interview of prospective participants and an adjustment based on an "inspection" of family characteristics, principally the education and training of wives who were not working (Skidmore, 1975, p. 41). A longer and more systematic assessment of low income would have required either a longer series of pre-enrollment interviews or the incorporation of outside data on the determinants of family income.[10]

Once a family was determined to be eligible for participation in the experiment, it had to be allocated to a specific treatment or to the control group. Standard RCT protocol would involve simply randomly assigning families, usually in equal numbers, to the treatment and control groups so that household characteristics and treatments are orthogonal (uncorrelated). This experimental design ignored the important budgetary consideration that the cost of maintaining a sample point in a negative income tax experiment would vary according to the treatment level and the income and other characteristics of the family. For example, very generous plans with high G and low t would have high retention rates but would be quite expensive to maintain, whereas meagre plans with low G and high t would be inexpensive but would have more difficulty retaining participants.

Conlisk and Watts (1979) developed an assignment model for New Jersey that maximized the efficiency of the sample design given the design points (G, t), the resource constraints, and the response function of interest. For an eligible family with specified characteristics, the model provided an allocation that minimized the variance of the labour supply response function[11] subject to the estimated cost of an observation at each possible design point and the total available budget. They note that "economists may see the design problem as analogous to a utility maximization problem from consumer choice theory" where the

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[9] The poverty line in the U.S. in 1971 for a family of four was $3,968. Source: U.S. Census Bureau Historical Poverty Tables: People and Families—1959 to 2017, Table 1 at https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-people.html

[10] Kelly and Singer (1971) use an exponential moving average forecast of Aid to Families with Dependent Children (AFDC) records from 1960 to 1970 to predict and compare permanent incomes, wages, and hours of AFDC recipients, who were the prime target of the Gary Income Maintenance Experiment.

[11] The specific response function determines the change in earnings as a function of the treatment (G, t) and a measure of the wage rate relative to its poverty level equivalent. These explanatory variables will cause the cost of each treatment to differ for a given family and its characteristics (such as the proximity of family earnings to the break-even level of income) and will cause the cost of treatments to differ across families with different characteristics.

response function corresponds to an inverse measure of utility and the estimated treatment costs correspond to goods prices. The assignment dictated by the Conlisk-Watts model was generally modified in New Jersey and the other income maintenance experiments to ensure adequate sample sizes by treatment group and by family composition and ethnicity.

In contrast to an orthogonal design, the Conlisk-Watts model efficiently estimates the response function by assigning inordinate numbers of families to the control group and to inexpensive treatment cells.[12] As a consequence, obtaining reliable estimates of the treatment effects, the prime focus of the RCTs, is less straightforward with Conlisk-Watts than with the difference-in-differences methods that are typically suitable. Conlisk-Watts is also more likely to find insignificant treatment effects (Ferber and Hirsch, 1982, p. 32). Keeley and Robins (1980) argue that unbiased estimate-of-treatment effects must include all variables (such as family income) that affect the probability of assignment to treatment cells under the Conlisk-Watts scheme, and assignment variables should be interacted with treatment variables to obtain unbiased estimates of differential response or to extrapolate the experimental results to the U.S. population. To avoid these problems, they recommend that future experimental designs avoid allowing the cost of an experimental treatment to vary with income, as in the Conlisk-Watts model, even in circumstances where the response function of interest is well specified but not fully understood. Perhaps because of the tight timeline, however, the Conlisk-Watts algorithm was the basis for assignment in all the income maintenance experiments.

When designing the experiments, it could not be assumed that family income and composition would remain stable throughout. Indeed, an important consideration in the design of any negative income tax or modern basic income plan is the mechanism to handle these fluctuations. In New Jersey, the eligible family unit contained an able-bodied adult male between 18 and 58 years of age and a dependent by blood or adoption (Skidmore, 1975, pp. 34–35).[13] The introduction of a negative income tax could provide incentives for individuals to leave or join a family unit enrolled in the experiment, which would confound labour supply analysis as well as present its own problems. It is important to note that there are potentially two distinct effects on family composition. First, because the experimental treatments represent a dispersed sample of families, there are incentives to all those in the community who did not receive the treatment—either because they were not enrolled or they were enrolled in the control group—to join a treated family. Second, however, there are also the effects that a particular negative income tax plan would have on the size and composition of a treated family, and these effects would be entangled with the effects arising from the dispersed sample in the experiment.

Recognizing these issues led to careful consideration of eligibility for those joining or leaving families in New Jersey. To eliminate incentives for family friends and relatives to move

[12] Specifically, low-income families that lie far below the break-even level of a plan (design point) are likely to be allocated less generous (low G, high t) plans whereas higher-income families near the break-even level are likely to be allocated more generous (high G, low t) plans.
[13] The age 58 cut-off was chosen so that adult males would not become eligible for retirement during the experiment. About 80 male-headed single-parent families were enrolled in the experiment but generally excluded from the final labour supply analysis of experimental outcomes.

in to take advantage of experimental income support, only newborn children or other children who resided in the family continuously for six months were counted as new family members. A spouse leaving a family was allocated their share of the treatment, with the children's share going to the custodial spouse. Children who turned 18 and left the family took their attributable payment with them but did not become a new family for the purposes of treatment.[14]

Subsequent experiments altered these arrangements for family additions (joins) and subtractions (splits). The Rural experiment allowed spouses who split to retain their share of the benefits but also allowed them to form a new family under the allocated treatment after one year. Seattle-Denver went further, allowing men who joined a family in the experiment to receive its treatment. These innovations allowed some assessment of the impact of different design rules, albeit in the context of a dispersed experimental sample.

If fluctuations in family composition posed challenges for New Jersey and the other experiments, the question of the appropriate accounting method for the delivery of payments was an even greater challenge. The accounting method involves three distinct processes:

- the frequency of payments
- the determination of income for the basis of payments, and
- the basis for reconciliation of payments.

Consider the payments schedule defined by equation [1]. Payments are determined by the allocated treatment parameters (*G, t*) and income *Y*. If income is steady, then regular payments are easily determined. However, one important aspect of many families with low incomes is significant income fluctuation caused by moving between jobs and working inconsistent hours or having difficulty being paid effective hourly wages on the job. It is precisely the instances of income loss when support may be most needed; for example, frequent payments based on recent income experience. For this reason, the designers of the New Jersey experiment opted for monthly payments to conform to existing Social Security arrangements. They also chose an annual reconciliation of overpayments and underpayments to conform with the rules for income tax administration. Perhaps because the extent of actual income variability was unanticipated (Skidmore, 1975, p. 38), they did not appear to realize that these accounting arrangements, along with the method of determining income for the monthly payment, would also be critical and complex aspects of the experimental design. They eventually decided that income would be based on a three-month moving average plus any income above the break-even level from the previous 12 months, to avoid the problem of large overpayments.[15]

Kershaw (1978, pp. 47–49) argues that the accounting and payment arrangements are a critical issue for the development of a negative income tax plan, perhaps even more critical than the design parameters that were actually tested. Even the focus of the experimental design, the determination of labour supply response, depends on the method by which income is

[14] There was concern about a "dowry effect" if splitting family members were to receive the experimental treatment in their new families and therefore become more attractive marriage partners.
[15] These arrangements were developed during the first year of the experiment and will be discussed in the next section.

determined. Consider a family with two months of steady income whose income falls drastically in the third period. The income calculation, and hence payments and labour supply response, would differ quite significantly depending on whether only the previous month's low income was used, or the average of the three previous months plus allowance for income over the previous 12 months. Under the first arrangement, the family would receive a high level of support at a tax rate determined by the experimental treatment; under the second arrangement, the family might still find itself above the break-even level, receive no payment, and face no experimental taxation.[16] Once these issues surfaced, the designers at the University of Wisconsin wanted to use the accounting period as a treatment variable but the administrators of the experiment at Mathematica were unwilling to disturb families already under the experiment in this way. The Rural experiment did, however, introduce one- and three-month accounting periods as a treatment variable, albeit in the context of a rural economy with a much wider incidence of self-employment.

The simplicity of the negative income tax expressed by equation [1] obscures a large number of issues associated with the design of a negative income tax experiment or a modern basic income plan. Although making no claim that our list is comprehensive, we have considered what appear to be the most prominent of these design issues in the literature: site selection, experimental duration, family and income eligibility conditions, the treatment assignment mechanism, handling of family composition changes, and income accounting methods. Once these design issues were settled, at least at the outset of each experiment, the experimental payments had to be delivered and the information for research and accounting purposes had to be collected. Delivery issues are now considered.

Delivery of the Negative Income Tax Experiments

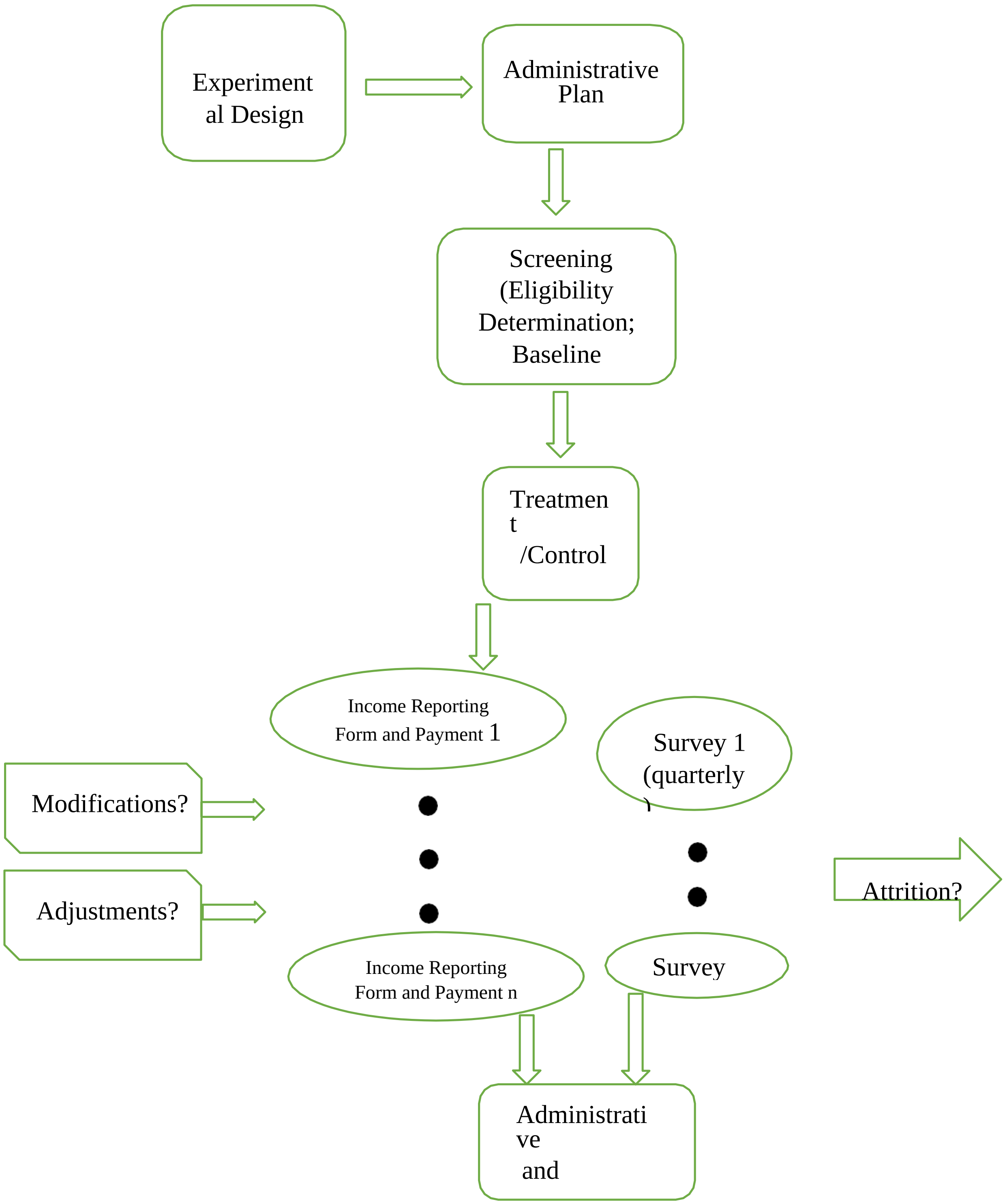
Beyond their focus on labour supply response to a NIT, the experiments provided opportunities to deliver specific experimental designs to a dispersed sample of eligible low-income families in a variety of locales. Those administrative lessons could also inform the implementation of a NIT plan that might be administered to the population. Thus, while experimental design and behavioural response were the research focus, the credibility of the experiments also depended on competent delivery of appropriate payments to families and careful collection of relevant family information as the experiments proceeded.

Figure 2 provides a simple flow chart of the main activities associated with the delivery of each of the income maintenance experiments. The schemata indicates the critical set of administrative tasks: screening to determine eligibility and capture experimental participants, allocation of participants to treatment and control groups according to the experimental design, administration of regular (typically monthly) payments to treatment groups on the basis of reported income, collection of survey information on the family and its behaviour during the experiment, and preparation of administrative reports and data for analysis by researchers. In

[16] This situation was more common than might have been expected because of the Conlisk-Watts assignment, which oversampled families near the breakeven income level.

Figure 2

Steps in the Delivery of the Income Maintenance Experiments



Note: For an alternative sketch of administrative procedures in a NIT experiment, see Ferber and Hirsch (1982, p. 44).

addition, Figure 2 indicates that attrition and other challenges occurred during the experiment, much of it unanticipated, which led to modifications to the experimental design and adjustments to the administrative procedures. Many of these modifications and adjustments reflect lessons learned that might be valuable in administering a modern basic income experiment or plan.

We should again note that the lessons learned were limited by the tight timeframe over which the experiments were conceived, designed, and initiated. As Figure 1 demonstrated, no more than two years elapsed between the commencement dates for New Jersey and Gary, the first and final U.S. experiments, and only another three years before the start of the Manitoba experiment. The first reports of the New Jersey experiment (Kershaw et al., 1974; Pechman & Timpane, 1975) only began to emerge when Mincome Manitoba was already in the field, which meant limited opportunity to learn from previous experience. Hence, it may be more useful to think of the five income-maintenance experiments as a cohesive set at a variety of sites rather than as a distinct and evolving series of experiments.

As we indicated in the previous section, the apparent simplicity of a negative income tax plan belies a variety of additional design considerations around site selection, experimental duration, family and income eligibility conditions, the treatment assignment mechanism, treatment of family composition changes, and income accounting methods. Similarly, the flow of experimental operations depicted in Figure 2 conceals a fairly complex information collection and payment delivery operation. While a comprehensive overview of these operations is beyond the scope and intent of this paper, we try to indicate the major challenges faced by those who implemented and administered the experimental designs. Table 2 summarizes the issues we discuss. Because of the tight timeline of the experiments, New Jersey was the prototype for both the design and delivery of the experiments that followed. Once again, we will discuss the New Jersey delivery mechanism in more detail and refer to the other experiments only when additional delivery issues emerged.

Although Mathematica Inc. in Princeton submitted the proposal the OEO accepted for the New Jersey experiment, the OEO provided the project grant to the Institute for Research on Poverty at the University of Wisconsin with the understanding that the Institute would subcontract with Mathematica to share the work (Skidmore, 1975, p. 32). The Institute took primary responsibility for planning and research while Mathematica dealt with the field operations. Moreover, it was decided that a separate organization, incorporated as the Council for Grants to Families, would handle the transfer payments system, recognizing that this unit would have to negotiate with local government social assistance agencies and deal with any legal or financial problems arising from the delivery of benefits (Ferber & Hirsch, 1982, p. 45).

Mathematica also set up a subsidiary, Urban Opinion Surveys, to collect information during the experiment. This became the model for the other U.S. experiments: distinct organizations to deal with the planning and research functions apart from field operations, or at least apart from the direct delivery of payments, a division of responsibilities often achieved through extensive subcontracting arranged by the principal agency. In practice, however, a successful experiment would require extensive co-operation between all units involved, particularly as problems arose.

At the screening stage, then, a short questionnaire had to be administered in accordance with the design of the experiment to determine the eligibility of families based on age, family composition, and an assessment of family income. Eligible families then completed a longer enrollment interview to collect baseline data before they were invited to participate in the experiment and allocated to a treatment or control group (Ferber & Hirsch 1982, p. 56). New Jersey used the 1960 Census to predict the number of households that would need to be contacted and assessed for eligibility, but this method proved disappointing from the outset. Far fewer eligible families were identified in Trenton, the initial site, and families with working wives as well as non-black families were underrepresented. A search for sufficient white families eventually led to the addition of Scranton, Pennsylvania, as a site, and this experience of expanded sampling to identify enough eligible families for each strata specified in the modified Conlisk-Watts allocation design was generally repeated across the U.S. experiments (Ferber & Hirsch, 1982, pp. 86–88).[17]

The Conlisk-Watts assignment mechanism allocated families to treatment and control groups to provide efficient (minimum variance) estimates of labour supply response for the available budget. Although this assignment had a compelling logic in the strict interpretation of New Jersey as an experiment to determine the work disincentive effects of a negative income tax on a predetermined budget, it heavily populated the extreme treatment combinations, especially the low cost observations associated with high tax rate treatments and the control group.[18] As the Conlisk-Watts mechanism was implemented and this feature became clear, a significant new complication occurred. On January 1, 1969, just as the New Jersey experiment was about to go into the field, the state of New Jersey introduced a generous extension to family welfare assistance. This extension, Aid to Families with Dependent Children for Unemployed Parents (AFDC-UP), dominated many proposed experimental treatments, particularly the [50,50] and [75,70] plans.[19] This led to the almost immediate addition of a more generous [125,50] plan to the design and a decision to engage prominent economist James Tobin to assess the assignment issues. Tobin recommended modifications to the Conlisk-Watts mechanism that had the effect of moving it closer to a classical analysis-of-variance design in which all treatments, even those implying more expensive observations, would be more equally represented. These modifications, and others to ensure appropriate sample sizes for designated family types, became a feature of the assignment process in the other experiments.

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[17] In Seattle-Denver, 93,851 households were contacted but only 53% completed the screening interview. Of those families who completed the screening interview, 19% completed the pre-enrollment interview, 12% were assigned to a treatment or control group, and 10% (4,800) enrolled in the experiment (Ferber and Hirsch, 1982, 88, Table 5.4).

[18] For a given guarantee and family income, a high tax rate treatment implied a low break-even income level and smaller payments (lower cost). Controls, of course, receive no NIT payment.

[19] That is, the plans with a guarantee of 50% of the poverty line and a 50% tax rate and 75% of the poverty line and a 70% tax rate, respectively. These were two of the seven proposed plans in the vicinity of the "sweet spot" of policy interest around a [75,50] plan. Moreover, the AFDC-UP benefits dominated other plans for certain income ranges (Skidmore, 1975, 44–45).

Once assigned to a treatment or control group, a family had to be regularly monitored in two ways, as shown in Figure 2. First, the family had to complete an income-reporting form (IRF) each month to determine its benefit payment for the next month. Families received a flat payment to complete the forms, but poor early completion rates led Tobin to recommend a fourfold increase in these payments for treatment families and a 60% increase for control families (Skidmore, 1975, p. 49). Second, families had to complete more comprehensive periodic surveys approximately once every four months to capture information about labour market activity and earnings—the focus of the experiment—but also other family matters of interest, including family composition and relationships, youth education, job characteristics and turnover, health, consumption patterns, and attitudes (Rossi, 1975). The periodic survey used questions from the Current Population Survey to take advantage of a tested methodology and to be compatible with other nationwide data sources, but the survey questionnaire was revised within a few months of field experience to capture information across the entire payment period rather than the previous week (Skidmore, 1975, p. 55).

"On the fly" modifications and adjustments in New Jersey were abundant and necessary: additional sites and sampling, new treatment plans, revisions to the assignment mechanism, more generous fee schedules for completing forms, and a redesign of the survey questionnaire all occurred once the experiment was already in the field. This was hardly atypical of the experience of the other experiments, which may have learned a few lessons from New Jersey but had little time to learn others. In part, the changes arose because of what were perceived to be alarming levels of non-participation and attrition among those in the program. Tobin's modifications to the assignment mechanism were in part designed to limit attrition in the New Jersey experiment to 20%, recognizing both the direct cost of these lost observations and the potential bias they introduced (Hausman & Wise, 1979).[20] Although attrition is a feature of any panel survey, the New Jersey attrition problem was exacerbated by both its design and unanticipated events, especially the introduction of AFDC-UP. This experience would be repeated in Manitoba, where runaway inflation led to severe attrition, adjustments to guarantee levels, and supplementary sampling during the Mincome experiment (Simpson et al., 2017). Although attrition can be limited by design and effective field operation, a social experiment cannot be protected from unpredictable economic and public policy developments that frustrate the intentions of experimental control.

One consequence of these modifications and adjustments was that the costs to deliver the New Jersey experiment were higher than anticipated. These cost overruns arose from multiple sources, including greater interviewing challenges, a system to audit and verify IRFs, and procedures to deal with fraud and other legal issues. Still, administrative costs amounted to only about 7% of payments (Ferber & Hirsch, 1982, pp. 57–58). In addition, the lessons to be

[20] Cain (1986) notes, for example, that a decline in earnings for a treated family provides an incentive to remain in the experiment and collect benefits but has no such effect on control families. Hence, one would expect higher attrition among control families experiencing earnings loss, as was observed, introducing potential biases into the treatment-control comparison of labour supply response.

learned from the administration of the experiments for basic income experimentation and policy today are dramatically confounded by technological change.

In addition to administrative costs, there were significant costs for data management, an integral component of any experiment. The complexity of assembling and merging the periodic surveys and the IRFs was underestimated in New Jersey and the other U.S. experiments, although we now have much greater experience developing panel data using advanced information technology (Ferber & Hirsch 1982, pp. 46–47). It is again difficult to know how much the experiences of the 1970s are relevant to the modern world, except as a warning not to discount such costs too heavily. Once the experiment is completed and the data are assembled, the research and analysis function takes over. What have the experiments revealed? We now turn to the outcomes.

Outcomes From the Negative Income Tax Experiments

The main objective of any social experiment should be to answer a specific set of questions; otherwise, its design, data collection and analysis, and assessment are compromised from the outset. In the case of the negative income tax experiments, the primary question was the impact of a negative income tax plan on work disincentives or labour supply, although supplemental questions were examined along the way. In this section, we provide a brief summary of the results for the labour supply and other outcomes as they are documented in the literature. Our coverage is necessarily a brief overview of a vast literature that has been carefully summarized elsewhere. The principal objective of this section is to identify and draw on these sources to provide a sense of the scope of the research. Our intention is not to minimize the importance of the research and the estimates of experimental outcomes. Indeed, as the experiments did with respect to the labour supply question, a proper basic income experiment should start from the question of its main objectives: What do we want to learn? This point of departure requires some understanding of what has been learned, the gaps in knowledge that remain, and how (or if) the gaps can be filled by further experimentation.

As labour supply response was the focus of the experiments and of economists concerned about the impact of a negative income tax plan, much has been written on what we learned from the experiments about this issue. Before summarizing the results, this section first looks at the limitations of the experiments that feature most prominently in discussions and interpretations of the analysis. We then provide a summary of the analysis of labour supply response across the experiments. Finally, we turn to what we consider the most useful and enduring results, the structural labour supply estimates based on the experimental evidence. As in previous sections, we begin with the limitations identified in the seminal New Jersey experiment and its post-mortem and discuss the other experiments as they contribute to the issue or add new issues to the discussion.

Aaron (1975, pp. 89–90) notes that, while New Jersey provided important advances in knowledge about the administration and implications of alternative negative income tax plans, there was also consensus that interpreting the results required caution for at least three reasons: the brevity of the experiment, site idiosyncrasies, and limited sampling across sites and family types. As we discussed earlier, the decision to limit the New Jersey experiment to three years lacked careful scrutiny. Skidmore (1975, pp. 32–33) notes that discussions about whether three years was long enough to assess a permanent labour supply response generated sufficient concern that the planners sought additional funds to extend the experiment for some participants. This request to OEO was declined, and only the Seattle-Denver experiment tested treatments longer than three years. About 30% of the Seattle-Denver participants were enrolled for five years, and studies found larger labour supply response for the five-year participants, although the result is not definitive (Robins, 1984).

Toward the projected end of the Seattle-Denver experiment, a small sample of 169 families was allocated to a 20-year plan to more closely represent a permanent NIT program, but the plan was terminated early. Robins finds that the labour supply response of those enrolled in the 20-year plan is smaller after three years than those enrolled in the three- and five-year plans. This finding is in contrast to earlier results that compared the responses for the three- and five-year participants, although the small and dwindling sample size of the 20-year plan and other analytical complexities do not allow for formal testing. Thus, the best that can be said is that there is no strong evidence of an experimental duration bias in either direction and far greater resources would have to have been allocated to address the issue adequately.

The decision to adopt a scattered sampling design across multiple sites was more considered than the duration of the New Jersey experiment, but there were also proponents of both saturation sites and a dispersed national sample. Watts and Bawden (1978, p. 58) conceded that "a saturation experiment enables a wider range of questions to be addressed and provides a more holistic evaluation of a prototype public policy [but] . . . the problem of finding control samples for entire labor markets would be severe" and would compromise the estimates of labour supply response.[21] Similarly, "a 'national sample' approach was explicitly considered, forcefully argued, and rejected" as too administratively complex and expensive for the budget available and the prime motivation of precise labour supply response estimation. It was quickly determined, however, that prospective sites in New Jersey were more idiosyncratic than anticipated: Trenton, Patterson-Passaic, and Jersey City all produced fewer white families living in poverty than predicted from the 1960 Census, leading to "a makeshift solution . . . to add one experimental site in Scranton, Pennsylvania, a virtually all white area [which] . . . produced a severe confounding of site, ethnicity, and timing, which reduced the ability to identify sources of differential response behaviour" (Watts & Bawden, 1978, p. 64).

Although New Jersey restricted the sample to males aged 18 to 58 who headed families with dependents, the need to ensure sufficient representation of black, white, and Hispanic (Puerto Rican) families sliced sample sizes by site and family type correspondingly. Similarly, efforts to represent the wider national population led the Rural experiment to divide its sample into sites in North Carolina, to represent the black Southern rural population living in poverty, and Iowa, to represent the white Midwest rural population living in poverty. Despite its modest

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[21] We revisit this question in our discussion of the Dauphin saturation site in the Manitoba Basic Annual Income Experiment and recent basic income pilot program initiatives.

budget, the Rural experiment also added female-headed families, older male heads, and singles, again slicing the sample pie. Seattle-Denver, although the largest experiment by far, also introduced by far the most stratification, as childless couples were added to the mix along with three experimental durations, five declining tax treatments, and training subsidies. Since Seattle's unemployment rate jumped from 3% in 1969 to 10.5% by 1971 (Basilevsky & Hum, 1984, p. 19), there was a strong argument for a "more normal" Denver site and for analysis that did not mix the results from the two urban areas.

This search for "national representativeness" in terms of family types and race within a small number of local sites contributed to small samples that defied the precise estimation that motivated experimentation in the first place, although insufficient sample size was perhaps inevitable given the budgets available and the ambitions of proponents. Christopherson (1983, p. 26) argues that the elaborate design of Seattle-Denver, involving 120 treatment cells, would have required a sample and corresponding budget six times as large as the actual budget to obtain statistically significant results. And this is the largest experiment of the lot! Despite quite clear motivation at the outset, the experimental designs were generally too ambitious for their intent, leading to economist Robert Solow's comment that the "surest generalization that emerges from all such experiments is that the implications for mass human behavior are weak . . . statistically significant response-coefficients are hard to come by" (Solow, 1986, p. 219). Solow argues that there is a lot to be learned from the experiments, but part of the lesson is just how difficult it is to pin down precise estimates of behavioural response in all the noise of a busy social setting, much of which defies control. It is important to note, however, that the alternative to date has been the false precision obtained from much larger Census or survey samples that are prone to bias; this is exactly the problem that the experiments were developed to address and the results remain an important resource even with these limitations.[22]

In short, the experiments face important but hardly debilitating criticism with respect to brevity, site peculiarities, and sample size and they remain an important resource for information on the impact of a negative income tax. We now turn to the experimental analysis and results, concentrating on labour supply response before turning to other results that were of secondary interest from the outset. The discussion and presentation of the labour supply results follow Hum and Simpson (1993), who argued that "the experiments represent a series of closely related trials with many common design features that can inform us about behavioral response to a guaranteed annual income plan in the North American population" (p. S274). Their analysis captures the summaries of the U.S. results from Keeley (1981), Robins (1985), and Burtless (1986) as well as the Manitoba experiment.

[22] That is, as Solow notes, that noisiness is a characteristic of any microdata that might shed light on labour supply response. The point of the experiments was to use random allocation to eliminate the biases in the estimation of response that are associated with omitted relevant variables, not the uncontrollable noise. Precise response estimates require both random allocation and sufficient sampling to overcome the noise, and the experiments may have fallen short on the latter objective.

Labour Supply Outcomes

The motivation for the experiments to study labour supply response to a negative income tax derives from a static labour supply model that explains individual hours worked (*h*) in terms of the after-tax wage rate (*w*), income (*Y*), and perhaps other factors (*z*) to capture personal circumstances and characteristics

$$h = f[w, Y; z] \qquad [3]$$

Ignoring variation in *z* for now, the model explains the percentage change in hours worked as

$$\frac{d_h}{h} = \left\{\frac{\partial_f}{\partial_w}\frac{w}{h}\right\}\frac{d_w}{w} + \left\{\frac{\partial_f}{\partial_Y}\frac{Y}{h}\right\}\left[\frac{d_w}{w} + \frac{d_Y}{Y}\right] = \eta_s\frac{d_w}{w} + \eta_Y\left[\frac{hd_w + d_Y}{Y}\right] \qquad [4]$$

where η_s is the substitution or compensated wage elasticity, assumed to be a positive pure price effect, and η_Y is the income elasticity, assumed to be negative.[23] The prototypical NIT design that raises income (*Y*) but introduces a negative income tax or benefit reduction rate that reduces the after-tax wage (*w*) would therefore be expected to lead to an unambiguous reduction in hours worked under these assumptions. The experiments, including the Conlisk-Watts assignment model based on a response function of the form of equation [3], were designed to answer this question of work disincentives as efficiently as possible. Lack of distinctive variation in *w* and *Y* and the intervention of other factors (*z*) compromised non-experimental evidence, but experimental variation in *w* and *Y* and random allocation of participants to treatment and control groups would yield unbiased results.

The most direct analysis, which is commonly used to evaluate experiments, is the non-structural or experimental effect given by regressing the response of interest, hours worked (h_{it}) for agent *i* in period *t*, on the treatment plans

$$h_{it} = x_{it}\,\beta_x + \epsilon_{it} \qquad [5]$$

where x_{it} consists of dummy variables to represent the treatment plan assigned to individual *i* at time *t*, ϵ_{it} is the error term, and β_x estimates treatment effects or the difference in hours worked between a specified treatment and the control group. Although all panel data collected by the experiment might be used, this equation would measure the average effect of a treatment during the experiment, so *t* might be confined to observations at the beginning and end of the experiment to provide a measure of the full experimental effect. Fixed effects regression in an ANOVA or ANCOVA framework can be used to sweep out unobserved fixed effects embedded in ϵ_{it}, and control variables can be added to capture time effects, non-random

[23] That is, a higher wage constitutes a higher price for leisure, which leads to a substitution away from leisure toward work, and a higher income leads to higher consumption of all normal goods, including leisure at the expense of work.

(Conlisk-Watts) assignment, break-even status, and program participation and attrition (Hum & Simpson, 1991, pp. 53–58).

Alternatively, structural models can be estimated by replacing x_{it} in [5] with direct measures of wages and income and estimating a variant of [4]:

$$\Delta h = \beta_0 + \beta_1 \Delta w + \beta_2 (h \Delta w + \Delta y) + \epsilon \qquad [6]$$

(Hum & Simpson, 1993, p. S281). Equation [6] explains the change in hours worked (Δh) between the experimental and pre-experimental periods in a form that allows for direct computation of the substitution and income elasticities, since $\beta 1$ and $\beta 2$ measure the substitution and income effects directly. Control variables can again be added to capture time effects, assignment, break-even status, program participation, and attrition. In particular, Hum and Simpson (1993) add a variable to capture the number of preschool children in the family.

Non-structural and structural estimates of labour supply response in terms of annual hours worked are presented in Table 3 for the four negative income tax experiments in the U.S.—as summarized in Keeley (1981), Robins (1985), and Burtless (1986)—as well as the comparable Winnipeg dispersed sample for the Manitoba experiment. The nonstructural or experimental results are modest for male heads and roughly three times as large for female heads. A simple average of the results of the four U.S. experiments from Robins (1985) and Burtless (1986) is a 6% reduction in annual hours worked for male heads compared to a 19% reduction for female heads and a 15% reduction for single female heads, while the results for Manitoba are a 1% reduction for male heads compared to a 3% reduction for female heads and a 7% reduction for single female heads. While few statistically significant results are reported, the evidence seems overwhelming that the group of negative income tax plans tested provided some work disincentive, an important component of the policy discussion and the experimental rationale at the time that the experiments were conceived.

As the experiments proceeded, economists provided a strong case for calculating structural estimates of labour supply response. These structural estimates, in the form of substitution and income effects or elasticities, would be independent of the experimental treatment and applicable beyond the experiments themselves (Keeley, 1981). These results, also provided in Table 3, are somewhat mixed. Some substitution elasticities are negative and some income elasticities are positive, contrary to theoretical expectations, although the preponderance of the evidence is consistent with the underlying static labour supply theory associated with equation [4].[24] The most important aspect of these structural estimates, however, is that they are uniformly small, indicative of inelastic labour supply response. The

[24] Robins (1985) found that one in five substitution and income effects from U.S. experiments contradicted the theory, and the signs for four of the six effects from the Winnipeg, Manitoba, sample are perverse as well. My interpretation of these results is that they reflect sampling error around a small (positive) substitution effect and an even smaller (negative) income effect.

substitution elasticities never exceed 0.1 for men and 0.4 for women, and the income elasticities never exceed 0.2 for men and 0.3 for women.

Although the small substitution and income elasticity estimates for men were consistent with the non-experimental evidence of the day, the similarly small estimates for women, particularly married women, were not. Hum and Simpson (1991) find substitution and income elasticities as large as 3 for the first generation of labour supply analysis and even higher substitution elasticities in a series of second-generation studies.[25] Subsequent research provided corroborating evidence of the experimental results. In particular, Mroz (1987) subjects the labour supply models in the literature to an extensive set of tests for specification bias using the popular Panel Study of Income Dynamics and finds only low elasticity estimates for those models where his tests cannot be rejected. MaCurdy et al. (1990) also find evidence of bias toward large substitution effects in models that attempt to address non-linear budget constraints. The experiments thus provided a stimulus for reconciling the coexisting large and small labour supply elasticities in the literature and rejecting a series of approaches to labour supply estimation with non-experimental data that had produced large labour supply estimates.

Good evidence of the progress that has been made in reducing the uncertainty around structural labour supply estimates can be found in the recent review by the Congressional Budget Office (McClelland & Mok, 2012). Their review of recent research indicates a likely range for the income elasticity of 0 to -0.1 for men and women and a likely range for the substitution elasticity of 0.1 to 0.3 for men and single women and 0.2 to 0.4 for married women.[26] Their consensus estimates are consistent with the earlier findings for the uncompensated wage elasticity by Evers et al. (2005) from a meta-analysis of 239 empirical estimates from 32 studies between 1978 and 2005.[27] Given the wide range of estimates for these elasticities in the literature prior to the experiments, the current consensus around a narrow range of relatively small labour supply elasticities represents a significant achievement for which the experiments and their impetus to research on labour supply can take ample credit.[28]

Some of the remaining uncertainty around labour supply response likely resides in our lack of understanding of family labour supply dynamics and observable but uncontrollable factors represented by z in equation [3]. Although the NIT payments were delivered on the basis of family income, the results for labour supply response are largely confined to the individual model represented by equation [3] even in families with two working adults.[29] An exception is

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[25] See Hum and Simpson (1991, Tables 2-2, 3-2 and 3-3). The distinction between first- and second-generation labour supply analysis is derived from Killingsworth (1983).

[26] Modern literature distinguishes between the internal and external margins of labour supply, and these estimates refer to the internal margin. McClelland and Mok also find a likely range for the participation elasticity (external margin) of 0.0 to 0.1 for men and single women and 0 to 0.3 for married women.

[27] Evers et al. find a mean uncompensated wage elasticity (sum of the substitution and income elasticities) of 0.4 for women and 0.1 for men, which is consistent with McClelland and Mok.

[28] The value of that research in microsimulation analysis of alternative income support programs will be discussed later in the paper.

[29] The New Jersey experiment analyzed the experimental effects on family earnings and hours (Basilevsky &

Hum & Simpson (1991, 1993), who find that changes in the number of preschool children in the family significantly reduces the labour supply of women and increases the labour supply of men in these families, which indicates important cross-substitution effects associated with child rearing. Such cross-substitution is also consistent with their findings of compensated cross-wage effects for married women; that is, that the labour supply of women is affected by not only their own wage and family income but also the wage of their male partners. These tentative results invite considerably more careful empirical study, but little progress appears to have been made. And as the labour force participation of married women has grown along with more formal childcare arrangements, the responses from the NIT experiments of the 1970s may have become less relevant.

Outcomes From the Experiments Other Than Labour Supply

While labour supply response clearly remained the focus, there were additional experimental features in the trials that followed New Jersey: variable accounting periods in the Rural experiment, counselling and training subsidies in Seattle-Denver, social services/counselling/daycare subsidies and work requirements in Gary, and the saturation site in Manitoba that is discussed in a separate section to follow. Moreover, the baseline and triannual surveys also collected a substantial amount of socioeconomic and demographic information, some of which has been analyzed on a more modest scale than the evaluation of labour supply response. As basic income research has proceeded, however, interest in the impact of a basic income on behaviour beyond labour supply has grown, and the NIT experiments represent a rich resource to understand behavioural response on a wider scale.

Table 4 attempts to provide an indication of this scale and a summary of the results. For an economist, these effects are generally secondary because they do not directly affect the cost of delivering a NIT or otherwise indicate the extent of the efficiency loss that might result from modifying the tax-transfer system. That efficiency loss is associated with the substitution elasticity, which the experiments and other subsequent research have found to be small but not negligible. It is larger than the income elasticity by a factor of 3 or 4, likely the most significant experimental result and an important consideration in the design of a basic income plan and the simulation of its impact.[30] Since the pre-eminent labour supply results are based on limited sampling that often yielded statistically insignificant results, it is not surprising that the results beyond labour supply are even more fragile. Even so, it is still useful to consider what evidence the experiments provided of NIT effects beyond labour supply, possibly as a basis for future analysis of basic income on a broader scale.

We summarize this evidence in six categories, as they are organized vertically in Table 4. First, some effects arise from the aforementioned additional experimental features of the trials that followed New Jersey and may affect the effective delivery of a NIT and related employment

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Hum, 1984, 108–123) and found a significant negative response to treatment for white and Hispanic families but not black families. A family labour supply model was not estimated.

[30] Recent simulation studies of a Canadian basic income that incorporate labour supply response include Boadway, Cuff, and Koebel (2016) and Stevens and Simpson (2017).

and income support services. Second, there may be some additional labour market effects associated with a NIT payment, such as changes in job search patterns and the quality of employment.[31] Third, there are effects associated with the change in labour-leisure time, as the drop in labour supply for some treated individuals raised the question of how they used their additional time. Fourth, the NIT plan could affect family composition, including marital stability and fertility decisions. Fifth, there is the consumption question of how individuals spent their additional income support beyond taking increased leisure. And finally, there are the effects of an NIT plan on health and well-being. We take these issues in turn.

Seattle-Denver and Gary combined NIT plans with various subsidy treatments to test their impact. The counselling and training subsidies increased education for both men and women in Seattle-Denver. Subsidies also increased the use of daycare in Gary when the subsidy rate was substantial (80% or 100% of direct cost) and did not impose a work requirement. In contrast, subsidies did not encourage the use of social services, as their use declined among treated families in Gary. Although these results are interesting from a policy perspective, they are generally neither surprising (subsidies promote consumption) nor are their effects quantified in a cost-benefit framework in the final reports.

There is reason to suspect that a NIT plan that improves the adequacy and stability of income support might affect labour supply beyond simply hours worked, as workers might take advantage of these circumstances to improve their labour market position. New Jersey found that treated older workers had lower job turnover and slightly shorter unemployment spells than their control counterparts, especially in more generous plans, which Rossi (1975, 175) interpreted as a wage-subsidy effect.[32] At the same time, younger workers exhibited higher mobility toward better jobs in families with more generous treatments, suggesting a role for NIT payments in improving the job-worker matching process for recent labour market entrants. Although the Rural and Seattle-Denver experiments found increased geographical mobility, the mobility might have been related to changes in housing rather than employment.

There also appears to be evidence that the treated families experienced improved educational results. New Jersey found that older male children in the families receiving treatment were slightly more likely to stay in school. Children in the primary grades in treated families in the Rural experiment in North Carolina improved their school performance along various measures. And Gary also found a significant experimental effect for male teenagers continuing in high school. The impact of improved human capital accumulation within families receiving the NIT would be both long term and difficult to quantify, but these spinoff effects could represent an important benefit of a basic income program and require further study.

Some concerns about family composition effects proved unfounded. Groeneveld et al. (1980) found that treated families in Seattle-Denver experienced significantly higher marital

[31] These are individual effects, not community labour market effects, which we discuss further below. A basic income may also have aggregate labour market effects, such as changes in market wage rates, but these issues are beyond the scope of the income maintenance experiments.
[32] A wage-subsidy effect would arise if the assigned NIT plan raised an individual's after-tax wage, which was not tested directly.

dissolution relative to the control group. A careful re-analysis involving all experimental time periods, allowance for attrition, and treatment plans with a training component refuted this result, however, finding no NIT treatment effect (Cain & Wissoker, 1990). Further exploration of this issue in Manitoba actually found slightly greater marital stability among the treated group (Hum & Choudhry, 1992; Choudhry & Hum, 1995).

The pre-enrollment and periodic survey collected information on consumption, giving voice to a simmering debate about whether some consumption patterns meet with greater approval than others and whether improvements in income support should be conditional on "good spending" rather than "bad spending" (Michael, 1978). Given that society imposes surtaxes on "bads" such as alcohol and tobacco and waives taxes on "goods" such as children's clothing, and given that assistance in kind such as food stamps or housing subsidies have been an important long-standing aspect of North American social assistance programs,[33] this issue cannot be easily dismissed from a policy perspective. The evidence from the NIT experiments is therefore reassuring. As Table 4 indicates, those provided NIT treatments improved their housing and spent more on durables, auto repairs, medicine, and clothing—all expenditures that generally fall under the category of necessities. There was also some evidence of reductions in farm debt in the Rural experiment and medical debt in Gary, and no mention of increased spending on alcohol, tobacco, other "sinful" consumption, or even what might be considered frivolous luxuries.

Although it might be expected that those receiving NIT treatment would be healthier and happier, the evidence is scant.[34] Indeed, New Jersey found no evidence of improvement in health or in social psychological measures intended to capture worry, self-esteem, social networks, and the like, although Rossi (1975, pp. 177–180) is critical of the methodology. There was some evidence of improved well-being in the Rural experiment, including a small improvement in nutrition in the North Carolina component.

Dauphin and Mincome: Forerunner of the Modern Basic Income Pilot

Each of the experiments that followed New Jersey contained unique experimental features. In the case of Manitoba, the dispersed Winnipeg sample followed the basic design of the other experiments and contributed an analysis of labour supply response that is consistent with the body of research from the other experiments (Hum & Simpson, 1993), as discussed earlier and summarized in Table 3. Its unique feature was the inclusion of a saturation site in Dauphin, where every resident of the town as of July 1, 1974, was eligible for the "50-50" plan, a guarantee of $3,800 for a family of four that corresponded to roughly 50% of the poverty line measured by Statistics Canada's low income cut-off (LICO) and a tax rate of 50%. In addition, a

[33] The U.S. Food Stamp Act dates back to 1964, while public housing and shelter subsidies are prominent in Canadian and U.S. welfare programs.

[34] The standard utilitarian model of economics leads to the conclusion that voluntary participants in an experiment would be better off, and the experiments were designed to be more attractive than existing social programs (transfer more income) to avoid excessive non-participation and attrition.

small rural dispersed sample was collected in the same fashion as the dispersed Winnipeg sample, except that only the 50-50 plan was allocated to those treated, 103 of the 181 families enrolled. The rural treatment was intentionally aligned with Dauphin so that its controls could serve as a comparison group for the saturation site.

The Manitoba experiment was the product of federal-provincial negotiations that followed the U.S. lead and the Canadian government's Special Senate Committee on Poverty (1971) recommendation for a guaranteed annual income (GAI), a negative income tax in all but name, to alleviate poverty. As in the New Jersey design, the merits of a randomized controlled trial to evaluate labour supply response prevailed over a demonstration or pilot project, but the Dauphin saturation site was added to address "administrative and community issues resulting from a less artificial environment" than a dispersed sample (Hum & Simpson, 1991, p. 45). In particular, the original design document refers to the value of a saturation site to assess possible influences of community members outside the family on labour supply and other time-use decisions and the opportunity to observe administrative responses and program participation and cost in "a more natural implementation of a GAI" than a dispersed sample of isolated participants (Hum et al., 1979, pp. 50–52). The rationale for selecting Dauphin as the saturation site remains unclear, reminiscent of the site selection process in New Jersey.

Field operations and benefit payments were conducted from a separate Dauphin office of Mincome Manitoba, the agency incorporated for the administration of the Manitoba experiment. While eligibility and treatment were much simpler than for the multi-treatment dispersed sample designs elsewhere, the baseline and periodic surveys were administered in much the same manner. Monthly payments were based on the completion of an IRF according to the standard calculation represented by equation [1] but also adjusted for a tax on net worth on a sliding scale: 0% up to $3,000, 4% between $3,000 and $10,000, 8% between $10,000 and $30,000, and 16% in excess of $30,000.[35] Treated families received assurance of higher payments than social assistance and other income support benefits could provide as well as a promise that they would retain eligibility for those benefits if they left the experiment. Mincome Manitoba prepared the tax returns of all treated families in the experiment, determined any regular federal and provincial taxes owing, and reconciled overpayments and underpayments on an annual basis by calendar year (Hum et al., 1979, pp. 26–28).

As in the U.S. experiments, Manitoba experienced significant non-participation and attrition, perhaps aggravated by rapid price inflation and depreciation of the real value of the NIT benefits. This led to "on the fly" adjustments after the first year that were typical of the other experiments but perhaps more extensive. The generosity of guarantee levels was increased by $600, an average increase of about 11% that just matched inflation, to restore the real value of the income support. The least generous 50-75 plan (a guarantee of 50% of the poverty line and a tax rate of 75%) that had little take-up in Winnipeg was folded into the 60-75 plan. And a supplementary Winnipeg sample of 196 treated families and 97 control families was enrolled to

[35] The same sliding net-worth tax scale was applied to the Winnipeg and rural dispersed samples, except that an additional exemption of $20,000 was applied to farmers in the rural sample.

provide sufficient observations in cells for lower-income two-parent families. In Dauphin, however, the modest 50-50 plan remained and likely accounted for the disappointing take-up of the NIT plan.

Sabourin et al. (1979) found disappointing enrollment of only 322, or 7% of all Dauphin families, by the end of the first year (1974), and participation never rose above 9%, well short of expectations of as much as 50% participation in a program for which any family was potentially eligible. Their concern translated into a survey of 700 Dauphin households in 1976 that yielded 302 completed interviews.[36] From those interviews, about one-third (94) were found to be eligible for benefits but only about one-third of those eligible (32) participated in the program. They found that most families (80%–90%) were aware of the program and what it offered, but most of these families either considered themselves ineligible on income grounds or were hostile to income support programs in general. Their study estimated that effective publicity and personal outreach could increase participation of those eligible from about one-third to 50%–60%, but that the attitudes of the remaining eligible families represented a barrier to greater participation.

In any case, analysis of the Manitoba experiment and its saturation site in Dauphin languished (Simpson et al., 2017). Federal funding to the University of Manitoba in 1980 established the Institute for Social and Economic Research (ISER) which rehabilitated, digitized, and documented the relevant survey data. The Winnipeg sample and especially the crucial labour supply response question was analyzed by Canadian academics, but the evidence from Dauphin was ignored until Forget (2011, 2013, 2018) conducted quasi-experimental and qualitative analyses of the social outcomes. Although Mincome did not collect health data, Forget matched administrative health records available from the Manitoba Centre for Health Policy at the University of Manitoba for residents of Dauphin with health records of other residents of rural Manitoba to compare hospitalization rates and physician claims. Her analysis estimated a treatment effect for Dauphin, the “town with no poverty” during Mincome, of an 8.5% reduction in hospitalization rates, concentrated in the categories of accidents and injuries and mental health diagnoses. She also found a reduction in physician claims for mental health disorders but no experimental effect on fertility, birth weight, and family dissolution.

Extending her analysis to administrative records from the Manitoba Department of Education allowed Forget to explore high school completion rates, which had been studied in the U.S. experiments with dispersed samples. She found an increased rate of continuation into grade 12 in Dauphin compared to the rest of Manitoba.[37] Forget’s (2018) book starts from

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[36] There were 146 ineligible households (primarily due to age), 173 refusals, and 79 other incomplete contacts or interviews. Sabourin et al. determined that at least 246 completed interviews would be needed to estimate eligibility and participation rates within a 95% confidence interval, not accounting for possible bias from incomplete interviews.

[37] Forget (2011) refers to the additional indirect or community effects which may have revealed the impact of the NIT plan on school attendance in Dauphin, but the direct effects of NIT treatment on school attendance were also captured in New Jersey and Gary, as discussed earlier.

Mincome, the U.S. experiments, and the Dauphin experience to argue for a basic income as the basis for a "healthier, happier, more secure life for all."

The rural dispersed sample received the same single treatment as the Dauphin saturation site along with a control group to facilitate analysis of experimental effects, but lack of resources for research confined experimental analysis to the Winnipeg dispersed sample until recently. Calnitsky and Latner (2016) provide an initial analysis of the impact of the Dauphin treatment on labour force participation using the Mincome data. Their basic difference-in-differences analysis estimates an 11.3% decline in labour force participation in Dauphin relative to the rural control group. They also estimate a 3.2% decline in labour force participation in Dauphin relative to the rural sample that received the same NIT treatment, a social interaction or community effect that would not be observed in a dispersed sample alone. Because this community effect constitutes almost 30% of the total experimental treatment effect in their exploratory analysis of labour force participation, more research of this nature is warranted. Sabourin found a significant degree of hostility to income assistance programs in general—and Mincome in particular—in his analysis of the reasons underlying the disappointing participation in Dauphin. Calnitsky (2016) argues that the qualitative evidence from Dauphin indicates that the Mincome NIT program was viewed decidedly more favourably than standard federal income assistance. In particular, he concludes that the NIT benefits were perceived to be inclusive rather than stigmatizing, potentially contributing to stronger community acceptance and participation in a guaranteed income program.[38]

The research on the Dauphin saturation site, a pilot or demonstration project that accompanied a RCT experiment involving a dispersed sample, expanded the framework in which to view a basic income and social and economic impacts. It may be seen as a forerunner of some of the modern basic income pilot projects that have recently proliferated after a long experimental pause. We now consider some of the important developments since the period of NIT experimentation that may bear upon the design of basic income policy and experimentation today, and then turn to a critical examination of modern pilot programs from the perspective of their experimental forebears.

The Interim: Developments Between the NIT Experiments and the Modern BI Pilots

The long interval between the end of the NIT experiments in the late 1970s and the beginning of the BI pilot projects in recent years is marked by significant changes in technology, the economy, and society that may have implications for the application of past experimental lessons. In this section we consider these developments in four categories: poverty, inequality, advancing technology, and changing labour market skill requirements.

Poverty Endures

[38] These disparate findings may be explained by sample selection, since Sabourin's results are based on a sample of Dauphin residents whereas Calnitsky's results are based on a sample of the experiment's participants, who might be expected to be more favourably disposed to a NIT plan.

The wave of NIT experimentation was motivated in large part by an emerging consensus in the U.S. and Canada that poverty was an important economic and social problem. The NIT experiments were not followed by any similar form of income support program, however, leaving poverty alleviation to alternative policy measures, including income assistance and employment programs and earnings subsidies.[39] In the intervening years, it is difficult to find much evidence that these poverty measures have had a strong effect on the Canadian or U.S. poverty rate, whether defined according to an official or unofficial poverty standard.

The U.S. introduced an official poverty standard in conjunction with President Johnson's War on Poverty, which also kicked off NIT experimentation. Based on this official standard, the U.S. poverty rate fell quite sharply throughout the 1960s and stood at 12% in 1969. The rate has moved within only a small range from 11% to 15% since then, standing at 13% in 2015 (UC Davis Center for Poverty Research, 2018). The absence of progress in reducing poverty appears to leave the U.S. with one of the highest levels of poverty in the OECD (OECD, 2019).[40]

Figure 3 presents the poverty rate in Canada and British Columbia from 1976 to 2017 using Statistics Canada's low income cut-off after taxes (LICO-AT), which has been the unofficial poverty standard in Canada since the 1960s. The poverty rate for Canada remained in a similar range to the U.S. of 10% to 15% until the late 1990s when it began to fall steadily, reaching 8% in 2017. The poverty rate in British Columbia has also fallen in the 21st century and is also about 8% according to the LICO-AT standard in 2017. The federal government has implemented Canada's first official poverty measure, the Market Basket Measure, which indicates that the incidence of poverty is 9.5% for Canada and 10.3% for B.C., or about one in every 10 families. Although modest progress appears to have been made in reducing the poverty rate in Canada,[41] if not in the U.S., poverty endures. The argument for income support that resonated in the 1960s remains.

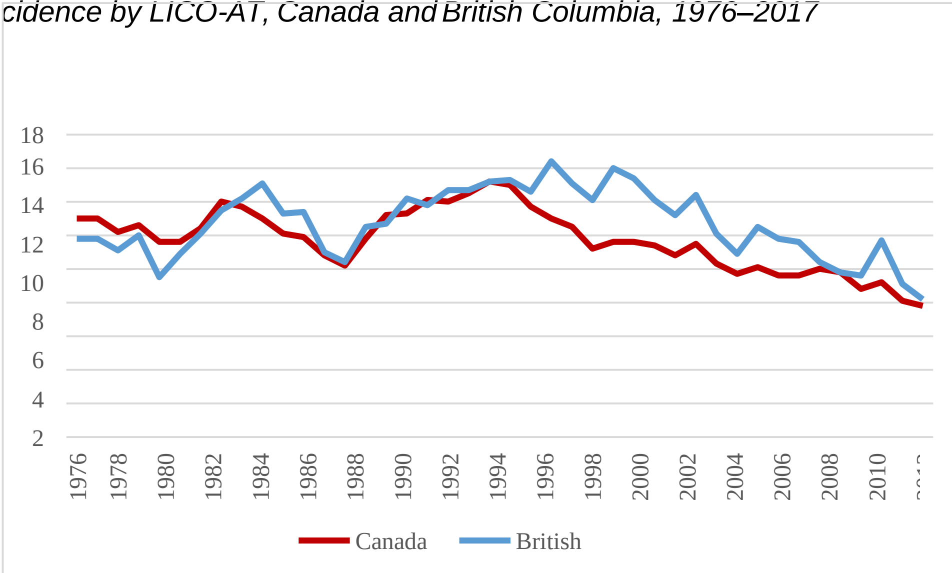
[39] The Earned Income Tax Credit (EITC) has been a primary innovation in anti-poverty policy in the U.S. but its Canadian counterpart, now the Canada Workers Benefit, has been far more modest. For a recent analysis of the impact of the U.S. EITC on poverty, see Hoynes and Patel (2018).

[40] For its international comparisons, the OECD uses the Low Income Measure, a relative income measure that is quite different from the consumption measures used in the U.S. and Canada. Canada's poverty rate is lower than the U.S. by this measure but remains higher than most OECD nations.

[41] The decline in Canada's poverty rate coincides with the introduction of the National Child Benefit in 1998, and the current Canada Child Benefit has been cited as a significant factor in meeting Canada's latest poverty target ahead of schedule: https://www.canada.ca/en/employment-social-development/news/2019/02/canadas-first-poverty-reduction- target-met-three-years-ahead-of-schedule.html. It can be argued that the Canada Child Benefit and its forerunners constitute a NIT-style basic income for families with children.

Figure 3

Poverty Incidence by LICO-AT, Canada and British Columbia, 1976–2017



Source: CANSIM series v96515829 for Canada and v96520725 for British Columbia.

Inequality Worsens

A long-standing concern with market capitalism has been the link or trade-off, if any, between economic growth and inequality. Lindert and Williamson (1985) review the historical debate and evidence, sometimes encapsulated in the Kuznets curve, and conclude that the link is tenuous at best based on the British and American experiences. Nonetheless, Heisz (2016) documents the evidence of rising income inequality in Canada and other OECD countries in recent decades. In the Canadian case, a trend to greater market-income inequality began in the 1980s but was masked by government tax-transfer policies until about 1995. Since then, after-tax inequality has drifted upward as incomes at the top have grown sharply and incomes at the bottom have stagnated.

One important aspect of the trend to greater market-income inequality has been weak wage performance for less-educated workers in Canada and elsewhere (Beach, 2014; Heisz, 2016). Autor (2019) has recently documented the U.S. case of falling real and relative wages of less-educated workers since 1980. He associates this pattern with occupational polarization, as the middle-skill jobs of non-college workers have disappeared and been replaced by low-skill jobs. Autor shows that the loss of middle-skill jobs among non-college workers has been concentrated in urban areas where wages are higher, an issue we discuss further below.

These labour market trends merit consideration in the context of income maintenance and basic income policy and research going forward. The cost of any income support plan will depend on the extent to which workers, especially workers without post-secondary education, are left behind.

Decisions about experimental site selection need to understand the changing labour markets in urban and rural areas, especially as these markets produce very different outcomes for less-educated workers. And the incentives to participate in the labour market, often a feature of past experimentation, may be less important than incentives to accumulate human capital, an issue which has received only passing attention in basic income experimental design.

Technology Advances, But Does It Mean a Jobless Future?

Technological progress is all around us but notoriously difficult to measure. One measure often cited is patents, which have been accelerating at a remarkable pace in the U.S. (Atlantic Re:think, 2015), consistent with concerns about the rising pace of technological change and worker displacement that often accompany calls for a basic income to support widespread job loss (Santens, 2017; Harari, 2018). The problem with this argument is that little evidence so far suggests that this jobless future is imminent. On the contrary, Figure 4 plots Canada's long-term employment rate since 1946. It provides a picture of steady growth in employment opportunities as technology has advanced, as job displacement is fully offset by job creation. The total employment rate has risen steadily from roughly 55% in 1946 to 61% in 2015 and 62.1% in 2019, as growth in female employment has outstripped more modest declines in male employment.[42] Similarly, the employment rate stands at 62.9% for British Columbia in 2019.[43] My sense of the serious literature and the evidence to date is that we should still be thinking about a basic income as a component of income maintenance consistent with earlier viewpoints, rather than as an expanding program to support a jobless future. The former suggests a more limited and less expensive role for basic income policy that reflects a continuity with earlier motivation and experimentation.

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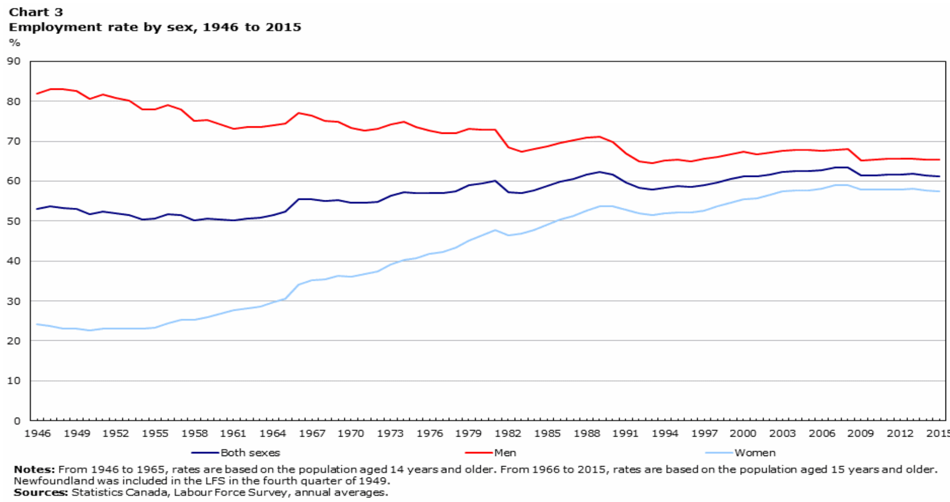
[42] This change has occurred for the population aged 15 and over, despite rising school attendance rates and earlier retirement for men.

[43] These figures for June 2019 are from Statistics Canada Table 14-10-0287-03 (formerly CANSIM table 282-0087) at https://www150.statcan.gc.ca/t1/tbl1/en/tv.action?pid=1410028703

Figure 4

Employment Rate for Canada, 1946–2015



Source: https://www150.statcan.gc.ca/n1/pub/75-005-m/75-005-m2016001-eng.htm

Skill Requirements are Changing as Never Before?

Do Autor's (2019) findings of occupational polarization and a loss of middle-skill jobs for less-educated workers hold for Canada and British Columbia since the era of the income-maintenance experiments? In this section we use the Census long form Public Use Master Files (PUMFs) from 1971 to 2016 to investigate changing occupational patterns and skill requirements using Autor's methodology that clusters occupations into three skill categories: low-skill manual and service occupations; middle-skill production, office, and sales occupations; and high-skill professional, technical, and managerial occupations.

The results for Canada, shown in Figure 5, tell a somewhat different story than Autor for the U.S., as low-skill occupations have steadily declined from 38% to 25% of all employment and been replaced by high-skill occupations, which have increased from 20% to 34% of all jobs.[44] The collapse of middle-skill jobs is not observed in the Canadian economy. The results for B.C., shown in Figure 5, similarly reflect a direct transfer of low-skill employment to high-skill employment without a net loss of middle-skill jobs.

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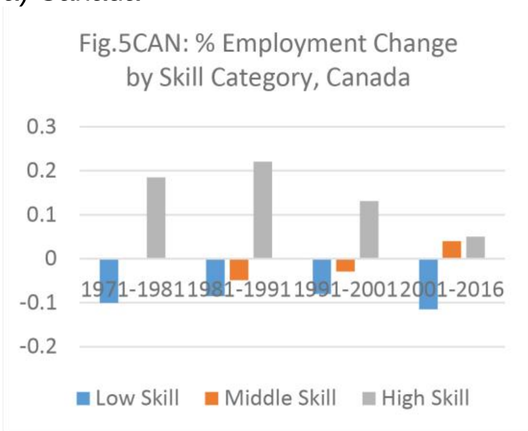
[44] The source for all subsequent results and graphs in this section is the Canadian Census PUMFs, 1981–2016. I acknowledge the valuable research assistance of Austin McWhirter for the calculations in this section. A full copy of the paper is available on my website: http://home.cc.umanitoba.ca/~simpson/Changing%20work%20and%20BI%20draft.pdf

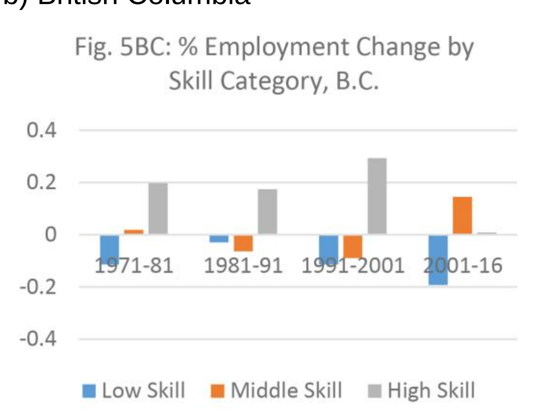
Figure 5

Percentage of Employment Change by Skill Category, Canada and British Columbia

a) Canada



b) British Columbia



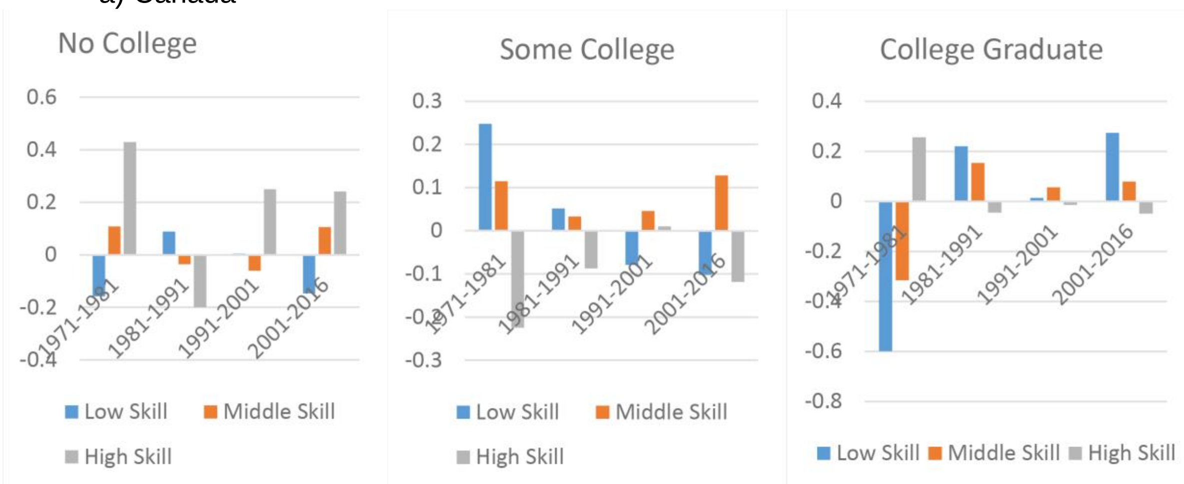
Although the reduction in low- and middle-skill jobs appears to be a positive development, those who remain in these jobs seem to face more precarious employment, as these jobs are increasingly part-time in Canada and B.C. Part-time employment in Canada rose from 18% of all low-skill employment in 1971 to 29% in 2016 and from 17% of all middle-skill jobs to 27%. The comparable figures in B.C. are a rise from 22% to 32% for low-skill jobs and 21% to 27% for middle-skill jobs. For high-skill workers, in contrast, part-time employment has not grown, staying at about 15% of all jobs for Canada and 17% for B.C. throughout the period. This increase in part-time employment for low- and middle-skill jobs is consistent with evidence of weaker wage performance for workers at the lower end of the earnings and education spectrum.

Autor considered only a college/non-college dichotomy of workers, but fully one-third of Canadian workers fall into an intermediate category. They have a trades certificate or a university certificate or diploma, or they have attended a post-secondary institution without completing a degree or diploma program. We therefore use a three-way classification of

Figure 6

Percentage Change in Employment by Skill Category and Educational Attainment, Canada and British Columbia, 1971–2016

a) Canada



b) British Columbia

education: a “no college” category for people with a high school diploma or less, a “some college” category for those who have received some post-secondary education without receiving a university degree, and a “college” category like Autor’s, which includes workers with at least one university degree. We again find no significant decline in middle-skill jobs for all education groups in Canada or B.C., as illustrated in Figure 6. The proportion of middle-skill jobs for no college workers actually rose from 41% to 45% between 1971 and 2016, fell only modestly from 23% to 21% for college workers, and rose sharply from 38% to 52% for workers with some college. Moreover, the decline in low-skill jobs is more marked and steady for no college workers than college workers, and college and no college workers have both experienced high-skill job growth. Simply put, Autor's dichotomy does not show up in the Canadian data, and the B.C. results are similar.

In summary, Autor's (2019) story of declining middle-class jobs and occupational polarization in the U.S. cannot be found in the Canadian Census microdata files using comparable occupational categorizations. His story of those middle-class jobs becoming low-skill jobs for workers without college and high-skill jobs for workers with college also does not hold up. Both no college and college workers have realized high-skill job growth. And workers with some college, who have lost high-skill employment, have lost those high-skill jobs to jobs in the middle of the skill spectrum, precisely where Autor finds jobs disappearing in the U.S. The absence of occupational polarization and its impacts on non-college workers may have moderated rising earnings inequality in Canada relative to the U.S.

These more optimistic findings of lower poverty, smaller growth in inequality, and retention of middle- and high-skill job opportunities for Canadian workers at all education levels do not mean that the case for a basic income has somehow disappeared, as significant poverty remains regardless of the state of the labour market. Rather, the findings of significant changes in the skill requirements of employment point to the importance of Canada's future education and training programs as well as the continuing need for income support. In that respect, it may be important to better understand the linkages, and potential opportunities, between income support and human capital accumulation in future research on basic income.

Basic Income Research Today: Pilot Projects and Microsimulation Studies

Interest in a basic income to address poverty and related social issues has spread across the world since the 1960s, especially in advanced economies where social safety nets are in place but are viewed as inadequate for at least some sizeable segments of the population. Canadian evidence that existing income support systems tend to favour seniors and families with children—leaving singles and childless couples under the age of 65 behind (Stevens & Simpson, 2017)—is echoed in studies of other advanced economies such as the U.S. (Hoynes & Rothstein, 2019). These circumstances should not be surprising, as limited resources for income support are directed to protect those who are most vulnerable, especially children, seniors, and persons with disabilities.

The Pilot Projects in Advanced Economies

Interest in a basic income has recently been translated into selective policy action in the form of pilot projects. This section examines the nascent basic income pilot project activity from the perspective of the income maintenance experiments of the 1970s. We restrict our study to basic income pilot projects in advanced economies where the context translates to Canada's modern, technologically advanced economy with a well-developed, if inadequate, social safety net to address poverty and related social issues. Although the Basic Income Earth Network (BIEN) was founded in 1986 to promote universal basic income programs, no country has adopted a program of this nature and experimentation using pilot projects is very recent. Figure

7 extends the timeline for experimentation from Figure 1. It depicts a very tight timeline for the development of the basic income pilots, reminiscent of the tight timeline associated with income maintenance experimentation. This speed very likely means that one basic income pilot has little opportunity to inform another, as the design phase of the pilot projects overlap. There is, however, ample opportunity for the pilot projects to have learned from the earlier income maintenance experiments.

Figure 7

Timelines for the Basic Income Pilots

1986		**2015**	**2016**	**2017**	**2018**	**2019**	**2020**
BIEN				–→Ontario————————→			
				→Finland→			
				–→Netherlands————→			
				–→Barcelona——————→			
						–→Oakland?	
						–→Stockton?	
						–→Scotland?	

Sources: Carnegie UK Trust (2018); Widerquist (2018a); The Young Foundation (2019).
Notes: Timelines reflect the period of field activity; the design would have occurred earlier. ? denotes that the pilot is only in the planning stage so far, based on Internet search.

Table 5 summarizes the basic income pilots in a manner similar to the earlier tables for the income maintenance experiments. It examines the six basic income pilots that have been conducted or are under development in roughly chronological order—Finland, the Netherlands, Ontario, Barcelona, Oakland, and Stockton[45]—and in terms of the same design, delivery, and outcome elements as before, but adds the stipulated objectives of the BI pilots from the literature available. Whereas New Jersey set the standard for conduct of the income maintenance experiments in a mutual effort to test labour supply response to NIT plans, common lines of motivation and conduct of the pilots are not evident. The details of each pilot project are discussed separately below.

The more scattered objectives of the basic income pilots stand in contrast to the focus of the income maintenance experiments on labour supply response, as the objectives listed in Table 5 suggest. Labour supply response is typically mentioned in some form, although often in a wider and less precise context, such as career choices and work behaviour (Ontario), the changing nature of work (Finland), or time use (Oakland). Other issues, many of which were studied in the income maintenance experiments but were clearly of secondary importance in their design and execution, appear to be given comparable billing to labour supply in the basic income pilot literature: social participation, education, health, housing, and well-being, to name a

[45] The Scotland BI pilot has recently produced an interim feasibility report (Citizen's Basic Income Feasibility Study Steering Group, 2019), but the report only sets out BI pilot options and invites feedback from the Scottish government and other stakeholders before a final feasibility report is released. Thus, there is not yet a specific BI pilot design to discuss.

few. How these issues factored into the planning, execution, and evaluation of the pilot projects will be a mystery until substantive reporting is available. To date, the literature does not reveal a continuity of goals and approaches that would constitute a starting point for the experimental design process and effective measurement of outcomes. Indeed, the sources of information about the pilots to this point consist of relatively brief online posts, a decidedly modern but also decidedly inadequate communication of what can be complex issues.

The Finnish Basic Income Experiment was the first BI pilot out of the blocks on January 1, 2017, but the design does not conform to a universal unconditional benefit in the spirit of a BI. Sponsored by the Finnish government and administered by the Social Insurance Institution of Finland (Kela), the pilot project was based on a randomly chosen sample of unemployed Finns aged 25 to 58 who were receiving a labour market allowance or basic employment assistance from Kela as of November 2016. The treatment was €560 (CAD780) per month for 24 months, regardless of income, family circumstances, employment subsequent to enrollment, or any other considerations. The experiment was intentionally straightforward in its design—to test a simplified benefit system that would reduce bureaucracy—but it was hardly universal or unconditional and it was not the model followed elsewhere.

Dissatisfaction with existing employment policies motivated the Netherlands to pass the Dutch Participation Act in 2015. The Act allows municipalities to devise RCT experiments to allow people receiving income assistance to retain more of their benefits as their income rises, and to be exempted from existing legal obligations to seek work or participate in training while receiving that assistance. The municipalities of Groningen, Wageningen, Tilburg, Deventer, and Ten Boer received permission from the Dutch Ministry of Social Affairs and Employment to proceed with proposed social assistance experiments in July 2017 (McFarland, 2017). The experiments, launched in late 2017 and designed to last for two years, randomly select social assistance beneficiaries[46] who are assigned either to a control group or to one of at least three treatment groups. The treatments test interventions to remove employment reintegration requirements, provide a more intensive form of reintegration service, or permit participants to earn additional income on top of their income assistance benefits. The latter treatment, most in line with the income maintenance experiments and the concept of a basic income, reduced the negative income tax (benefit reduction) rate from 75% to 50% on earnings up to a maximum of €199 (CAD300) per month.[47] An extension for the Groningen and Ten Boer experiments, which are coordinated, includes a treatment that allows participants to choose any one of the three treatment groups (removal of employment reintegration requirements, provision of a more intensive form of reintegration service, or reduced taxation of employment income). McFarland argues that the Dutch experiments are not true BI experiments or pilots for three reasons: (a) benefits remain means-tested and household-based which, however, is consistent with the NIT experiments and conception of a BI in North America, (b) participation is limited to people

[46] Unlike the Finnish experiment, participation is voluntary.
[47] In the Dutch welfare system, retention of 25% of earnings is capped at six months, whereas for those treated in the pilot the 50% retention rate on earnings would last for the two years of the experiment.

currently receiving income assistance, and (c) participants must adhere to existing Dutch requirements for people receiving income assistance to seek work or be removed from the experiment. That is, (a) is acceptable in the North American context, but (b) and (c) violate the BI concepts of universal and unconditional benefits, respectively.

The Government of Ontario committed in its 2016 budget to "create a Basic Income Pilot Project to test a growing view at home and abroad that basic income could provide a new approach to reducing poverty in a sustainable way" (Segal, 2019). What emerged from the planning and design phase was a project that conformed in essence to a NIT experiment along the lines of the Manitoba experiment in the 1970s. Three sites were chosen: a large urban centre (Hamilton), a smaller urban centre in the northwest (Thunder Bay) and surrounding municipalities, and a town in the southeast (Lindsay). Eligible households were 18 to 64 years of age and low income, under $34,000 for single individuals and $48,000 for couples. A single treatment, involving a guarantee of $16,989 for a single person and $24,027 for a couple plus a tax rate of 50% on any earned income, was randomly allocated to over 4,000 households. A randomly selected control group of 2,000 households was also enrolled in Hamilton and Thunder Bay, but Lindsay was designated as a "quasi-saturation" site along the lines of Dauphin.[48] The guarantees were intended to represent 75% of the Low Income Measure and thereby fall into the [75,50] sweet spot identified in the income maintenance experiments.[49]

The Ontario BI Pilot largely reflects the NIT idea of a means-tested, universal, unconditional benefit tested in the U.S. and Manitoba. It similarly excludes seniors, but persons 65 years of age and older already have a guaranteed income scheme in place in Canada, consisting of Old Age Security benefits coupled with the Guaranteed Income Supplement, and their poverty rates are low (Stevens & Simpson, 2017, Tables 1 and 2). Although the Segal (2019, p. 6) proposal cites Manitoba and other experiments, it argues for a fresh take to "fully inform the interactions between a Basic Income and some features of the Canada/Ontario environment, such a universal healthcare, employment insurance, public education, the existing tax and transfers system, etc. . . As well, the [Manitoba] experiment dates from a time when the realities faced by the workforce were quite different." The single treatment applied in the Ontario BI pilot coupled with its substantial sample size could have yielded quite useful and precise experimental impact estimates of labour supply response if the pilot had been allowed to run to its conclusion. Much like the other BI pilots, however, the Ontario experiment did not confine its primary focus to labour supply response but expanded it to health outcomes, life choices, education outcomes, community-level impacts, direct administrative costs or savings, food

[48] The Lindsay design choice seems questionable, since it was neither a full saturation site nor a dispersed site with a control group. It might best be described as a dispersed site without a control group, which seems to be an odd experimental design without specific analytical advantages.

[49] That is a guarantee amounting to 75% of the poverty line and a tax-back rate of 50%, as discussed earlier. Since the LICO standard used in discussing the guarantee levels in the Manitoba experiment are calculated by family and community size, there is no direct comparison, but figures published by the Canadian Immigration Consultant at https://fullskillsexamprep.com/blog/2017-lico/ list the LICO for 2017 as $24,600 for a single person and $30,625 for a couple (no children). The Ontario BI Pilot guarantees would then constitute 70% and 78%, respectively, of this standard.

security, perceptions of citizenship and inclusion, and mobility and housing arrangements as well as an expanded concept of work-related behavioural response to include job search, sideline and self-employment, and job tenure.[50] What is missing from the proposal and subsequent literature is any sense of the strategy to answer credibly this expanded list of questions.

Although the income maintenance experiments have been criticized for their disciplined focus on labour supply response, the alternative of an unfocused wish list of research questions without a careful plan of analysis appears to have been clearly on display in Ontario and the other BI pilots it followed. Had the experiment been completed, including its final reports and other subsequent literature, it could have helped us to understand whether these diverse goals were accomplished or not. However, the June 2018 election in Ontario toppled the Liberal government, and with it the BI Pilot Project. Payments ceased in April 2019 without funding or plans for a final report.

The city of Barcelona, supported by a grant from Urban Innovative Actions,[51] developed a plan for a basic income experiment, known as B-MINCOME in deference to the Manitoba experiment. Barcelona City Council developed a partnership with five research organizations and institutions to design and conduct the experiment: The Young Foundation, the Institute for Government and Public Policy at the Autonomous University of Barcelona, the Polytechnic University of Catalonia, the Catalan Institution for Evaluation of Public Policies, and NOVACT—International Institute for Nonviolent Action. The experiment was designed for households in the area of the city with the lowest income (Besòs) (McFarland, 2017). To be eligible, a household had to have at least one member aged 25 to 60 and be receiving benefits from the city's Municipal Social Services. Designed along the lines of a RCT, 1,000 households were randomly assigned to one of 10 treatment groups and another 383 households were designed as a control group[52] in October of 2017. The treatment groups received cash benefits but differed according to whether the benefits were means-tested and/or accompanied by additional programs.

Income support levels were as large as €1,676 (CAD2,334) a month but payments were based on household size and income in the tradition of a NIT so that a household of four without assets and an income of €900 would receive about €400 (CAD557) per month. A sample of 450 households received a benefit with the only condition that they continue to reside in the Besòs area until the experiment concluded in September 2019, but the benefit was means-tested on the basis of additional household earnings for one of the treatment groups but not the other. The remaining 550 treated households were divided into no less than eight treatment groups that received social programming in addition to cash payments. The social programming included an occupation and education program, a social and co-operative economy program, a guaranteed

[50] Sideline employment refers to taking an additional job or holding many jobs.
[51] UIA is an initiative of the European Commission to provide support for projects that investigate innovative and creative solutions to urban problems.
[52] The design provided for an additional pool to ensure a control group of 383, even with attrition.

housing program, and a community participation program. Although all these treatments would provide useful information to address poverty and unemployment, the very small sample sizes (much smaller than those realized in the income maintenance experiments) place a severe restriction on the reliability of any estimated experimental effects that might be obtained. Indeed, the discussion to date appears to signal a qualitative rather than quantitative emphasis (Colini, 2018; Young Foundation, 2019).

Y Combinator Research, the non-profit arm of Y Combinator that provides seed money to startup enterprises in the U.S., has set a goal of raising $100 million to develop a basic income pilot in the U.S. The original concept conforms more closely to the European notion of a basic income that is a universal, unconditional benefit without a means test, though it would be limited to people aged 21 to 40 and households whose income in the year before the experiment did not exceed the median household income for the community, a blunt form of means testing.[53] The original design called for a payment of $1,000 per month for three to five years to a minimum of 1,000 eligible households in Oakland, California, although the planners have now decided to delay their experiment, move it to communities in two other states to be determined, and partner with the University of Michigan to oversee the process (Widerquist, 2018a; Kotecki, 2018).[54] The loosely defined objectives call for data to be gathered on how beneficiaries use their time and money with a focus on the impact of the treatment benefits on social and physiological well-being, using both subjective and objective measures. There is no mention of a control group.

A similar, smaller-scale basic income pilot project supported by private-sector funding from Silicon Valley is underway in Stockton, California. The Stockton Economic Empowerment Demonstration (SEED) is funded by the Economic Security Project, a non-profit that sponsors other guaranteed income experiments.[55] The design involves residents of neighbourhoods with median income below the city average who were sent postcards in 2018, inviting them to participate in the project. Of those who responded, 125 participants were randomly chosen to receive an unconditional payment of $500 a month for 18 months, beginning in February 2019 for 18 months. The design also appears to include the random selection of a small control group from respondents, but details are scarce,[56] as is discussion of any clear research objective

[53] Blunt because a household one dollar over the median community income would receive no benefit whereas a household one dollar below the median income would receive the full benefit, introducing a sharp discontinuity or "notch" in the budget line of households in the vicinity of the median community income.
[54] The YCR overview document from September 2017 is still the only information available on their website at https://static1.squarespace.com/static/599c23b2e6f2e1aeb8d35ec6/t/5c53606b971a1879b1ad176c/1548968052 512/YCR-Basic-Income-Proposal-2018.pdf
[55] At present, the only planned project beyond Stockton involves payments of $1,000 per month for a year plus provision of support programs to 15 black mothers living in poverty in Jackson, Mississippi: http://springboardto.org/index.php/blog/story/introducing-the-magnolia-mothers-trust
[56] Details on this control group—its size, composition, refusal rate, etc.—are not available from the SEED/Economic Security Project website at https://www.stocktondemonstration.org/ If there was no payment to the control group to participate, as there was in the NIT experiments, then greater recruitment and retention problems are likely than were experienced in the larger NIT control groups, compromising standard treatment-control experimental analysis.

(Widerquist, 2018a). As it stands, the design tests the neighbourhood on the basis of income but not the recipients themselves, conforming more closely to a geographically confined demogrant than any form of NIT, but the postcard-response recruitment mechanism strays far from random allocation and, along with the small sample size, limits the analytical scope of the research.

Outcomes of Pilot Projects So Far

Basic income pilot projects are a very new phenomenon, and only the project in Finland has been completed. The Finnish Ministry of Social Affairs and Health issued a preliminary report in February (Kangas et al., 2019), which compared the treatment and control groups in terms of employment and well-being. The report finds no statistically significant difference in employment between the treatment and control groups during the first year of the experiment, a result that has been interpreted in some quarters as a failure of the plan (Lewis, 2019). There is no reason to expect a positive employment effect from an unconditional transfer, however, and those involved in experimental design and promotion should avoid such unrealistic claims.[57] [58] Their survey of participants does find positive effects of the pilot project on the well-being of basic income recipients relative to the control group in terms of health, stress, ability to concentrate, confidence in their own future, and trust in other people. It also finds a greater confidence in employment prospects, likelihood of accepting a job offer, and ease of starting a business, which suggests that the benefit payments may have a positive effect on employment in the longer term.

The Barcelona project issued an interim report in July 2019 (B-MINCOME, 2019) on its first year. In contrast to the detailed periodic surveying used in the NIT experiments, the analysis of the Barcelona pilot consists of a simple comparison of the treatment and control groups based on an initial survey in September 2017 and a follow-up survey in November 2018. Although characterized as small, the attrition or non-response rate was 15% for the treatment group and 40% for the control group, a disparity that should raise some concerns about the estimated impact effects.[59] The results—an improvement in general and economic well-being, a reduction in the severe material privation index and the incidence of mental illness, a reduction in “worrying about not having enough food” and the need to get money through means other

[57] The income effect acting alone implies a negative labour supply response, including a negative employment effect, at least in the short term of the Finnish experiment. The critical issue here is that the Finnish benefit, consistent with a basic income or NIT, is unconditional. Benefits conditional on employment, such as the Canadian Self-Sufficiency Project, can produce positive employment effects (Morris and Michalopoulos, 2000) but are inconsistent with the idea of a basic income. The Finnish experiment could have been designed to test the impact of conditionality on labour supply response by random allocation of basic income recipients to one of two treatment groups, one with benefits conditional on finding employment and the other with unconditional benefits.

[58] For discussion of the issues around communicating research findings for basic income experiments, see Widerquist (2005, 2018b).

[59] Non-response (or attrition) left 867 in the treatment group but only 231 in the control group, which would compromise the reliability of treatment-control comparisons in many instances.

than employment, improved quality of sleep, an increase in happiness and general satisfaction with life, and an increase in engagement with and participation in neighbourhood and community life—are described as significant, but no statistical tests are reported. Given the sample size, such tests are unlikely to corroborate significance in most cases. No significant results have been observed regarding work placement or in other dimensions related to employment, which should likely be interpreted as a positive result (the absence of a work disincentive effect).

The Barcelona results, and perhaps the Finnish results as well, await further study, and the other projects await completion (Netherlands and Stockton) or commencement (Oakland, now relocating, and Scotland). The Ontario basic income pilot was cancelled at its halfway point in July 2018 and no reports are expected. One concern at this point is that there is no detailed information on the design and delivery of the experiments. Issues such as the allocation process, income definitions when means testing, and the screening and interview processes that were carefully delineated in the income maintenance literature and final reports should receive comparable attention in the basic income pilots. Indeed, it is not even clear whether and to what extent funding has been allocated to document these tasks and investigate any issues that arise. Moreover, the Finnish project design restricts eligibility to people receiving employment benefits, and a universal and unconditional basic income would be far more inclusive. Kela has indicated that an experiment across a larger selection of low-income individuals was not an option with the budget available. Although the Finnish experiment has gained considerable attention, it does not provide a model for testing a basic income and its modest and restricted basis has not been followed in subsequent pilot projects. Those projects are all closer in design to the concept of a basic income or NIT.

Microsimulation Research on Basic Income

Although microsimulation research in economics can be traced to the era of NIT experimentation and before, it has only made significant advances more recently in concert with the rise in information technology, especially access to computing power and large administrative and survey microdata sets (Lambert et al., 1994; Bourguignon & Spadaro, 2006; Li & O'Donoghue, 2013).

Simulation models provide a potentially powerful tool to generate synthetic micro-unit-based data to answer many "what-if" questions that otherwise could not be answered (Li and O'Donoghue, 2013). Microsimulation models are quite versatile. They are used to estimate the impact of climate change (Hynes et al., 2009; Buddelmeyer et al., 2009) and to model disease spread and demographic simulations as well as spatial simulation (Cassells et al., 2012). A particularly important application has been assessing the distributional consequences of tax or benefit changes among heterogenous family groups and calculating the cost to government of proposed or hypothetical policy reforms (Creedy & Duncan, 2002). As such, these models are a potentially important tool in analyzing the cost and distributional outcomes from a basic income plan and could be an alternative to basic income pilot projects.

Many of these models are “non-behavioural” or “arithmetic” since they ignore or treat as exogenous behavioural responses to changes in the tax-transfer system (Bourguignon & Spadaro, 2006; Creedy & Duncan 2002). Li and O’Donoghue (2013) characterize these models as “static,” meaning they are used to evaluate the immediate distributional impact on economic agents of possible policy changes without reference to any behavioural adjustment or time dimension. Static models do not require that behavioural response parameters be imported or estimated, so they are easy to use, quick to run, and readily available to a wide range of users with different backgrounds while retaining the heterogeneity features of the survey data to which they are applied (Kalb, 2010). Popular examples of these non-behavioural/arithmetic/static models include TAXBEN at the Institute for Fiscal Studies (IFS), the EUROMOD model for the European Union (EU) at the Institute for Social and Economic Research (ISER), STINMOD at the National Centre for Social and Economic Modelling (NATSEM) in Australia, and the SPSD/M tax-benefit simulation model developed by Statistics Canada. A few simulation models also allow for the inclusion of detailed behavioural responses of individuals and households in response to changes in their budget constraints. These microsimulation models are referred to as “behavioural” models (Bourguignon & Spadaro, 2006; Creedy & Duncan 2002) or “dynamic” models (see Li & O’Donoghue, 2013 and 2014b). In addition, many static models, including SPSD/M, can now be programmed to handle some behavioural features, such as labour supply responses and participation decisions, making them more of a “hybrid” that bridges dynamic and static models.

The main advantage of microsimulation models is that they can be developed using a variety of data sets to capture population complexity or heterogeneity, thereby offering a wider scope of analysis than conventional econometric and other modelling (Bourguignon & Spadaro, 2006; Li et al., 2014a).[60] The models are particularly suitable for analyzing systems where decision-making occurs at the level of individual agents and where interactions within the system are so complex that it is impossible to find a solution by analytical means. Microsimulation models thus allow for practical production of results at the level of individuals, households, and different geographical regions for many socioeconomic policies—including such complex and controversial issues as basic income policies—with relative ease.

A major limitation of these models, however, is that the art of microsimulation, especially static microsimulation, includes only rules set by the researcher/modeller, which then determine the outcome of economic policies and thus simulate the first-order effect of policy changes (Klevmarken, 1997). Adjustment effects that follow due to changes in the behavioural response of individuals are ignored in static models and remain a significant challenge to dynamic models, since the complete modelling of behavioural response is a daunting task. A second

[60] These data sets can be the full set of household data collected from tax and transfer benefits data sources or synthetic/hypothetical data constructed for typical families in the population using a number of simplifying assumptions, as is the case for SPSD/M. The synthetic data approach limits population and analytical complexity and can be effective provided that the heterogeneity of the population is adequately captured for the purpose of the microsimulation. This characteristic is often difficult to assess without using the full set of data from which the synthetic data was constructed.

limitation of microsimulation models is that appropriate microdata, while improving with time and technology, remain limited, lacking in particular sufficiently long panel data to capture life cycle impacts.[61]

To date, the most important issue in addressing behavioural response in tax-benefit microsimulation models has involved the application of labour supply elasticities and, in particular, the application of elasticities either found in the literature or estimated within the model from the available data (Aaberge & Colombino, 2018). Critics of this approach have argued that the elasticity estimates are influenced by the choice of data and the modelling framework, although Lundberg (2017) argues that the convergence of behaviour of women and men is simplifying the estimation process and narrowing the range of elasticity estimates. She also argues that the use of exogenous elasticities from the literature allows for the connection of experimental and non-experimental methodologies and results and is more transparent, enabling robustness checks. The experience of the experimental evidence from the income maintenance experiments seems to corroborate this contention. It spurred a flurry of activity that led to a better understanding of the reasons for differences in the non-experimental and experimental results and rejected the econometric methods that arrived at much larger substitution elasticity estimates, particularly for women. Even so, other critics have argued that focusing exclusively on labour supply response makes the simulation models effectively supply-side partial equilibrium models (Kalb, 2010), ignoring subsequent second-order interactions on both the demand and supply sides of the market for labour.

As Table 6 indicates, the tax-benefit microsimulation models developed over the years have mostly been static models, although dynamic simulation models are now more common. Most simulation models have been created by government agencies in developed countries, although a few models have been produced for Brazil, Russia, and some African nations. For Canada, the standard is now SPSD/M (Statistics Canada, 2013), a non-confidential, statistically representative model of the Canadian population that can be used to assess the cost implications and distributional impacts of tax and benefit changes at both the federal and provincial levels. It is a static model that relies on a specifically designed and explicitly tailored synthetic database that is statistically representative of individuals in their family context, including information on individual tax payments and cash transfers received from the government (Décarie et al., 2012).

Microsimulation models have already contributed to the research and policy debate around strategies for income redistribution and poverty reduction. Honkanen (2014) uses the SISU simulation model and 2010 Finnish tax and transfer data on 9,300 households and 23,000 individuals to evaluate and compare the distributional consequences of (a) basic unconditional income support for everyone and (b) a negative income tax. For (a), he defines two levels of a basic income (BI) support: a normal BI for all adults above 18 years and a slightly higher BI for pensioners. He also uses different rates for the BI and NIT experiments but concentrates on the

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[61] An exception may be the administrative and census data files available in some Scandinavian countries, which could capture the life cycle behaviour of some birth cohorts.

case where the basic income is set at €600 and the basic pension at €850 per month, and the NIT is set to equivalent cost at a tax rate of 46%. Both the BI and NIT plans redistribute income, but the NIT plan reduces inequality (measured by the Gini coefficient) and poverty (measured as the percentage below 60%, 50%, or 40% of median income) to a greater extent. Both the BI and NIT reforms result in losses to the self-employed, with the BI also resulting in losses to pensioners and the NIT resulting in losses to employees with higher incomes.

Garfinkel et al. (2006) use the employment, income, and demographic data on 63,756 households obtained from the March 1994 Current Population Survey to simulate the distributional impact of four basic income guarantee (BIG) plans designed to place a high percentage of families above the poverty threshold in the U.S. The basic plan pays a taxable benefit of $2,175 per year for children up to the age of 18, $4,000 to adults between the ages of 18 and 65, and $8,000 to those over age 65, similar to the existing social security payment. The other three plans provide benefits a little below or above the basic plan to achieve roughly the same cost. The plans, with an estimated cost of about $1 trillion to provide coverage for 190 million adults and 70 million children, are financed by eliminating 15 existing federal cash-transfer programs and an additional tax equivalent to about 5% of all gross income. All four plans reduced the aggregate poverty rate and poverty gap, with the Adult Plus plan (higher benefits to adults aged 18 to 65) achieving the best result by reducing the poverty rate from 10% to below 6% and the poverty gap by about 80% once labour supply responses were considered. The income redistribution resulted in the lowest quintile receiving 5% of income, up from the pre-transfer level of 1%, and the highest quintile received 43%, down from the pre-transfer level of 50%.

Using the EUROMOD tax-benefit simulation model, Browne and Immervoll (2017) studied the mechanics of replacing existing benefit systems in Finland, France, Italy, and the U.K. with a basic income paid to all individuals at or below working age. In Finland and France, they found that replacing existing cash transfers and tax-free allowances for all groups but seniors with a basic income set at the same level as the guaranteed minimum income would be budget-neutral. They also found the budget-neutral basic income would be lower than the current level of guaranteed minimum income benefits in the U.K. and higher than the guaranteed minimum income introduced in Italy as of 2016. Gains are concentrated among the middle-income levels while losses are recorded among older workers. They find that the work incentives are stronger with a BI than existing benefit plans in these countries, especially for the lower-income households most dependent on the benefits.

Research in the area of basic income policy using microsimulation models is developing at a fast pace. Other papers in this area for advanced economies include Ericson et al. (2009) for Sweden using SWEtaxben; Immervoll et al. (2007) for 15 EU countries using EUROMOD; Sommer (2016) for Germany using IZA□MOD; and Monti and Pellizzari (2010) for Italy using EU-SILC data for 2016. The SPSD/M database and model for Canada has been used by the Parliamentary Budget Officer to estimate the cost of a national guaranteed income (NIT) program equivalent to Ontario's basic income pilot then in the field (PBO, 2018). The PBO estimated the annual gross cost of the Ontario BI plan extended across Canada would amount to $44 billion in 2018/19, net of the cost of existing benefits and transfers. In addition, three academic papers have studied alternative basic income plans:

- Stevens and Simpson (2017) consider the impact of converting most existing non-refundable tax credits to refundable tax credits (a NIT plan).
- Boadway et al. (2016) consider a NIT plan that would replace most existing transfers.
- Koebel and Pohler (2019) simulate the distributional impacts of a hybrid guaranteed basic income consisting of an expanded Working Income Tax Benefit and a guaranteed basic income set at the current social assistance level for a single individual in each province.

This research has set off a lively debate over the important issues of financing and behavioural response to a basic income plan in Canada (Kesselman, 2018; Stevens & Simpson, 2018; Boadway et al., 2018) at modest cost compared to any sort of basic income pilot. Although basic income pilot projects may be more suitable for some questions, such as administrative issues associated with the delivery of a basic income plan, simulation may be more useful for other issues, such as the design of a basic income plan to achieve specific financial or redistributive objectives.

Conclusions and Lessons for Future Basic Income Experimentation

The income maintenance experiments of the late 1960s and 1970s provide an important backdrop for the design, administration, evaluation, and documentation of the basic income pilot projects of today. The experiments were focused, some would say excessively focused, on the issue of labour supply response to a negative income tax. This focus, however, led to carefully directed planning and execution of randomized control trials to provide credible estimates of the impact of various NIT plans on annual hours worked and other measures of work disincentives. It also contributed ultimately to the relatively precise estimates of labour supply response—in the form of substitution, income, and participation elasticities—that can assist the assessment and simulation of the efficiency costs of any current NIT or basic income proposal.

The income maintenance experiments were ambitious by the standard of most of the basic income pilots proposed thus far. They were also complex, involving significant design and administrative hurdles that ranged from sample allocation to income determination to data capture and analysis. These difficulties have been carefully documented and assessed in technical reports and academic literature. This documented research provides a valuable legacy that continues to be built, as research continues on the Dauphin saturation site of the Manitoba experiment, arguably a progenitor of the current wave of basic income pilot projects. As yet, there is very little corresponding documentation on the basic income pilots, even where the design and delivery issues have been settled. Even if a basic income pilot project is devised strictly as a demonstration project rather than a true experiment (randomized controlled trial), careful documentation is important to understand what has been accomplished, what mistakes have been made, and how better pilot projects or a full basic income plan can be accomplished.

Returning to the comment by Watts and Bawden (1978, p. 67) that opened the paper, social experimentation is challenging amid the inevitable scarcity of funding, time, and prior knowledge that accompanies it. The income maintenance experiments provide ample lessons of that dictum, and these are carried over to the basic income pilots. For example, the experiments provided samples of 800 to 4,800 participants that were still too small to provide precise estimates of labour supply response given the number of distinct treatments in their design. The even smaller sample sizes in the basic income pilots will likely encounter even more acute difficulties in assessing outcomes. One lesson of the experiments is that we should resist the tendency to associate greater funding with opportunities to address more policy questions if the objective is more precise and reliable estimates of the outcomes to use in developing basic income policy. In that sense, the trend in the basic income pilots to expand the primary experimental objectives beyond narrowly defined labour supply response, while understandable, is a concern. Of course, it may be that the pilots will abandon the essence of the experiment (an RCT) and careful measurement of behavioural response in favour of demonstration projects, but only the basic income pilots in California appear to have taken that approach thus far. And the administrative and community lessons from strictly demonstration projects might be fairly quickly exhausted.

Lessons From the NIT Experiments to the BI Pilots

1. Scarcity rules even in an experimental setting (see also (4)).
2. Expect the unexpected, including design complications and policy developments that might compromise the experiment.
3. Expect participation and attrition problems (as long as participation is voluntary).
4. Focus is valuable: avoid asking too much and learning very little.
5. The design of the NIT experiments to focus on labour supply could be straightforwardly and valuably extended to time use, including the nature of leisure and non-market work, and consumption patterns.
6. Accelerating technological change and job destruction place greater emphasis on worker adaptation, and basic income experimentation should be concerned about worker skill development (as part of time use) with or without education and training subsidies.

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Table 1
Design Issues for the North American Income Maintenance Experiments

	New Jersey	Rural	Seattle-Denver	Gary	Manitoba[1]
Treatments [G,t]	G = 100%, 75%, 50% of poverty line; t = 70%, 50%, 30%; [50,70], [100,30] plans excluded; [125,50] added in 1969	G = 100%, 75%, 50% of poverty line; t = 70%, 50%, 30%; [50,30], [50,70], [100,30], [100,70] plans excluded; G = 125%, t = .7, .5, .3 plans added for Iowa	G = 140%, 120%, 95% of poverty line; t = 80%, 70%, 50%	G = 100%, 75% of poverty; t = 60%, 40%	G = 70%, 60%, 50% of poverty; t = 75%, 50%, 35%; [70,35] excluded, [50,75] combined with [60,75]
Extensions	Base case; experimental duration of 3 years	Farm operators and rural poor; 3-month and 1-month accounting periods; new payments system	Variable duration (3, 5, 20 years); 5 declining tax treatments (.025 per $1,000) for 70%, 80% rates; counseling; training subsidies (50%, 100% of direct cost)	Social services, counselling, daycare subsidies (35%, 60%, 80%, 100% of cost), work requirement for subsidies	Dauphin saturation site[1]; small rural dispersed sample with G = 50%, t = 50% (aligned with Dauphin saturation site); net worth tax on payments
Sites	Urban industrial sites: initially Trenton, then Patterson-Passaic, Jersey City, eventually Scranton, PA	Duplin County, North Carolina; Pocahontas and Calhoun Counties, Iowa	Seattle (1970) followed by Denver (1972)	Black low-income neighbourhood of Gary, Indiana	Winnipeg and rural Manitoba, Dauphin[1]
Filing unit (family eligibility conditions)	Able-bodied male head aged 18–58 with dependents	Female and older heads as well as male heads 18+; singles 21+	Married couples (children or not); families with dependents; able-bodied family head aged 18–58	Black families, head aged 18–58	Able-bodied family heads and singles aged 18–58

Splits/joins (family composition changes)	Splits kept benefits share; joins denied family treatment unless new birth	Splits kept benefits share for one year, full share as new household after one year; family treatment for births	Males joining eligible families entitled to treatment		Splits treated as separate families; joins allowed family treatment after a waiting period
Income definition	Modified IRS definition	Monthly income + accrued surplus (past income over break-even level)			
Accounting period	Monthly accounting; income averaged over 3 months but modified for past income in excess of break-even level over 12 months	1 month and 3 months, randomly assigned; payments biweekly	Monthly accounting	Monthly accounting; income averaged over 6 months to determine payments	
Overlapping taxes/benefits	Positive taxes fully offset annually	Positive taxes fully offset	Positive taxes fully offset; NIT payments tax exempt	Positive taxes fully offset monthly with annual reconciliation	
Sample selection	Normal (expected or permanent) income <150% of poverty line	Normal income <150% of poverty line	Income <$9,000 (single-headed) or $11,000 (double-headed family) ≈275% of poverty line	Income <240% of poverty line	Normal income <$13,000 (family of four) ≈170% of poverty line
Assignment model	Modified Conlisk-Watts	Modified Conlisk-Watts	Modified Conlisk-Watts	Modified Conlisk-Watts	Modified Conlisk-Watts
Sample size	725 treated, 632 controls	809 (587 male, 108 female, 114 seniors)	4,800 families (Denver 2,758; Seattle 2,042; 2,031 single-headed families; 2,053 NIT controls)	1,799 families (729 husband present, 1,070 no husband; 1,028 treated, 771 controls)	1,300 families and single individuals
Sample characteristics	Hispanic, black, and white	NC 56% black; Iowa 100% white	White, black, and Chicano	100% black, 60% female heads	
Total budget (including design,	USD7.8M ($5.428M operations and research;				CDN$17M

administration, and research)	$2.375M transfer payments)[2]				

Sources: Hum and Simpson (1993, Table 1); Pechman and Timpane (1975); Kershaw and Fair (1976); Palmer and Pechman (1978, 7); Ferber and Hirsch (1978); Christopherson (1983); Basilevsky and Hum (1984); Simpson, Mason, and Godwin (2017).

Notes: [1]The Dauphin saturation site of the Manitoba Basic Annual Income Experiment will be discussed separately.

[2] Broken down by fiscal year from 1968 to 1974 in Kershaw and Fair (1976, 18).

Blank indicates no information found.

Table 2
Delivery of the North American Income Maintenance Experiments

	New Jersey	Rural	Seattle-Denver	Gary	Manitoba[1]
Principal agency	Institute for Research on Poverty–UWisconsin and Mathematica Inc.	Institute for Research on Poverty	Stanford Research Institute	Mathematica Policy Research	Mincome Manitoba
Screening	Screening interview; pre-enrollment (baseline) interview of eligible families	Screening interview; pre-enrollment interview of eligible families	Screening interview; pre-enrollment interview of eligible families	Screening interview; pre-enrollment interview of eligible families	Screening interview; pre-enrollment interview of eligible families
Treatment allocation (enrollment)	Conlisk-Watts modified assignment	Conlisk-Watts, but stratified across 3 head types, 3 income levels, 2 locations	Conlisk-Watts, but stratified by location, race, family composition, experimental duration (20 strata)	Conlisk-Watts modified assignment	Conlisk-Watts modified assignment
Supplemental sampling	Yes: [G = 125%, t = 50%] added; Scranton, PA added to boost white sample	No	No, but Denver site added later	No	Yes: Supplementary sample drawn after one year, same plans
Attrition	20%; 25% for controls, 16% for treatments	10.3% (83/809)		35.2% of black males	20.9% in first year, 12.2% in second year

Experimental modifications	Monthly accounting; income averaged over 3 months but modified for past income in excess of break-even level over 12 months	1 month and 3 months randomly assigned; payments biweekly	Monthly accounting		Allowance for wealth in payments formula; saturation site in Dauphin[1]

Sources: Hum and Simpson (1993, Table 1); Pechman and Timpane (1975); Basilevsky and Hum (1984); Palmer and Pechman (1978, 7); Ferber and Hirsch (1978); Christopherson (1983); Simpson, Mason, and Godwin (2017).
Notes: [1]The Dauphin saturation site of the Manitoba Basic Annual Income Experiment will be discussed separately.
Blank indicates no information found.

Table 3

Labour Supply Response (Change in Annual Hours Worked) in the North American Income Maintenance Experiments

	Male heads[1]			**Female heads**[1]			**Single female heads**		
	Non-structural	Structural		Non-structural	Structural		Non-structural	Structural	
	Change in annual hours worked	Substitution elasticity	Income elasticity	Change in annual hours worked	Substitution elasticity	Income elasticity	Change in annual hours worked	Substitution elasticity	Income elasticity
New Jersey: Keeley (1981) Robins (1985) Burtless (1986)	 −7% −2% −1%	.09	−.02	 −33% −25% −25%	−.08	−.28	 n/a n/a n/a	n/a	n/a
Rural: Keeley (1981) Robins (1985) Burtless (1986)	 −9% −3% −3%	.09	.00	 −29%* −28% −28%	.28	.01	 n/a n/a n/a	n/a	n/a
Seattle/Denver: Keeley (1981) Robins (1985) Burtless (1986)	 −8%* −7%* −8%	.09	−.14	 −21%* −21%* −17%	.14	−.12	 −15%* −16%* −9%	.12	−.15
Gary: Keeley (1981) Robins (1985) Burtless (1986)	 −5% −2% −7%	.06	−.08	 −3% −20% +5%	.37	.26	 −28% −10% −30%	.14	−.20
All U.S. experiments: Robins (1985) Burtless (1986)	 −5% −7%	.08	−.10	 −21% −17%	.17	−.06	 −13% −17%	.13	−.16
Manitoba[2]: Hum & Simpson (1991, 1993)	−1%	−.07	−.03	−3%	−.08	.07	−7%	−.17	−.01

Notes: Primary source is Hum and Simpson (1993, Tables 2 and 3, pp. S279 and S282).
* Denotes statistical significance at the 5% level where reported; Burtless (1986) does not report statistical significance.

[1] Represents male and female heads of families with two adults and dependents, or “husbands” and “wives,” except Manitoba where male head includes single men (21% of male sample).
[2] Winnipeg only; the Dauphin saturation site is discussed separately.

Table 4

Outcomes Other Than Labour Supply from the North American Income Maintenance Experiments (Experimental Effects: Treated Versus Controls)

	New Jersey	**Rural**	**Seattle-Denver**	**Gary**	**Manitoba (Winnipeg)**[1]
Experimental extensions	Base case	3-month and 1-month accounting periods but no evidence of a differential effect	Counselling, training subsidies, increased education for men and women	Less use of social services; greater use of daycare at higher subsidy rates and without work requirement	Dauphin saturation site[1]
Employment, mobility	Less job turnover for older workers, especially in more generous plans; more mobility for younger workers to better jobs	Greater mobility	Greater mobility		
Education, other time use	Young men more likely to stay in high school	School performance improved in primary grades in NC; increased political participation, especially married women		Male teenagers more likely to continue high school	
Family composition	No fertility or marital dissolution effects	No fertility or marital dissolution effects	Some initial evidence of higher marital dissolution, but later revised to no effect		Slightly greater marital stability
Consumption	Larger investments in housing and durables; less reliance on public housing	Increased clothing, durables, and car expenditures; reduced short-term farm debt	Lower reliance on subsidized housing, especially female heads	Increased clothing, medicine, auto repairs, home ownership; reduced medical debt	

Health status, other well-being	No health or social psychological effects	Small improvement in nutrition in NC; well-being increases with plan generosity (guarantee level)			
Other					

Sources: Hum and Simpson (1993, Table 1); Pechman and Timpane (1975); Basilevsky and Hum (1984); Palmer and Pechman (1978); Ferber and Hirsch (1978); Christopherson (1983); Simpson, Mason, and Godwin (2017).
Notes: [1]The Dauphin saturation site of the Manitoba Basic Annual Income Experiment will be discussed separately.
Blank indicates no information found.

Table 5
Basic Income Pilot Projects in Advanced Economies

	Finland	Netherlands	Ontario	Oakland, CA	Stockton, CA	Barcelona
Objectives	Address changing nature of work; impact of improved work incentives; reduce bureaucracy; simplify benefits	To study the effects on labour market and social participation, health and well-being of people with income assistance	Replace federal income assistance and reduce poverty; assess impact on health, education, life and career choices, work behaviour, food security, mobility/housing, and perceptions of inclusion; evaluate community impacts, direct administrative costs	Study how participants use their time and money; impact of UBI on social and physiological well-being	No clear objectives	Poverty reduction and social exclusion; labour market participation, food security, housing security, energy access, economic situation, education participation/attainment, community networks and participation, health, happiness, well-being
Design	2,000 individuals aged 25–58 paid labour market allowance or basic UI in Nov 2016 chosen at random; benefit of €560 tax-free per month for two years 2017/18	Welfare recipients are enrolled in municipal experiments to reduce tax rates on benefits and work/training requirements	Age eligibility 18–65, income under $34,000 (1 person) or $48,000 (2 or more persons); Single treatment of $16,989 for a single person and $24,027 for a couple and a tax rate of 50%; 4,000 treatments, 2,000 controls; Lindsay “quasi-saturation site”	Age eligibility 21–40 years below median community household income; a minimum of 1,000 participants receiving $1,000 per month for 3 to 5 years	About 100 families would receive $500 per month for 18 months	Besòs households randomly allocated to controls (383, with replacement) and one of 10 treatment groups (1,000); treatments involve cash benefit with or without mean testing and other programs
Delivery	Administered by Kela, the Social Insurance		Administered by the Ontario Ministry	University of Michigan will	City of Stockton Economic Security Project	Barcelona City Council, Young Foundation,

	Institution of Finland; participation mandatory		of Community and Social Services	oversee the process		Universities of Barcelona and Catalonia, Catalan Institution for Evaluation of Public Policies, International Institute for Non-Violent Action are partners in design and delivery
Outcomes	Pilot completed in December 2018 and not renewed; improved financial security and well-being in the first year; no employment loss in the first year	Nothing reported yet	Pilot cancelled in July 2018; final payments in April 2019	Now planning to move the pilot to two other states	Still in the planning stage as of April 2019	Pilot project runs from October 2017 until September 2019
Funding	Government of Finland; €20M	Municipalities under the Dutch Participation Act	Government of Ontario; $150M over 3 years but discontinued halfway through	Y Combinator Research; $100M	City of Stockton Economic Security Project + donors; $3M so far	Barcelona City Council, Urban Innovative Actions

Sources: Carnegie UK Trust (2018); Widerquist (2018a); Young Foundation (2019); Segal (2019); Kela (2019)

Table 6

Current Tax/Benefit Microsimulation Models

Name	Type	Country	Uses	Inception	Current version
BRALAMMO	Dynamic	Brazil	Models Brazilian labour market for pension welfare analysis		BRALAMMO
EUROMOD	Hybrid	EU	Simulates individual and household tax liabilities and benefit entitlements	1996	I1.0+
STINMOD	Hybrid	Australia	Simulates government programs and tax and benefit changes	1994	STINMOD+
FASIT	Static	Sweden	Used by the Swedish Ministry of Finance for public policy simulation		FASIT
SWEtaxben	Hybrid	Sweden	Simulates impact of tax and benefit changes on households and central government budget	2009 (Ericson et al)	SWEtaxben
SLIMM	Dynamic	Sweden	Simulates impact of tax reforms on government revenue and individual tax liabilities including behavioural response	2017 (Lundberg)	SLIMM
TRIM	Static	U.S.	Simulates federal and state programs at the individual, family, and household level including eligibility, benefits, participation, and tax liabilities	1970s (Zedlewski & Giannarelli, 2015)	TRIM3
SPSD/M	Hybrid	Canada	Assesses cost implications and distributional impacts of tax and benefit changes at federal and provincial levels	1998	Version 28.0 (12/2020)
CTaCS	Static	Canada	Simulates Canadian tax and transfer systems, including refundable tax credit entitlements, tax burdens at different income levels, family types	1998 (Kevin Milligan	CTaCS 2016-2.01

IZA□MOD	Hybrid	Germany	Simulates distributional consequences, labour supply effects, employment effects, welfare effects and fiscal policy effects of tax/benefit policy reforms	First described in Peichl et al. (2010)	IZA□MOD
Sfb3	Separate static and dynamic models	Germany	Distributional analysis of personal incomes in 3 separate models: static model for tax/benefit simulation, dynamic cross-sectional model for population analysis, dynamic longitudinal model	First described in Galler (1980); also Galler and Wagner (1986), Wagner (1983)	Sfb3
DESTINIE	Dynamic	France	Evaluates social reforms (e.g., public pensions, retirement, intergenerational transfers)	Developed by INSEE in mid-1990s	DESTINIE II
HARDING	Dynamic	Australia	Analyzes lifetime taxes and transfers, Higher Education Contribution Scheme, lifetime redistributive impact of government health outlays	1990	HARDING
IFS Model	Dynamic	U.K.	Studies pensioner poverty under alternative tax and benefit policies		IFS Model
TAXBEN	Static	U.K.	Analyzes tax and benefit changes, including personal income taxes, expenditure taxes, benefit entitlements, tax credits	Early 1980s	TAXBEN
LIFEMOD	Dynamic	U.K.	Simulates the lifetime redistributive impact of a welfare state	1990	LIFEMOD
MICROHUS	Dynamic	Sweden	Models dynamic effects of changes to the tax-benefit system on the income distribution and economic-demographic effects of immigration	1992	MICROHUS
MICSIM	Dynamic	Germany	Powerful and user-friendly general microsimulation model to support applied research and teaching and to simulate German tax & pension reforms	First described in Merz and Buxmann (1996)	MICSIM 1.0.15

MOSART	Dynamic	Norway	Models the future cost of pensions, undertakes micro-level projections of population, education, labour supply and public pensions, incorporates overlapping generations, models within a dynamic microsimulation framework	1996 (Construction of MOSART 1 started in 1998)	MOSART 3
NEDYMAS	Dynamic	Netherlands	Models pension reform and the redistributive impact of social security schemes in a lifetime framework	1994	NEDYMAS
SAGE	Dynamic	U.K.	Dynamic demographic/tax model for the U.K.	1999–2005	SAGE
UKMOD		U.K.	An extension of the U.K. component of EUROMOD to handle in more detail tax and benefit policy changes in the U.K.	September 30, 2019	UKMOD
RUSMOD	Static	Russia	Simulates reforms to social benefits and personal income taxation, including poverty and inequality effects		
SWITCH	Static	Ireland	Simulates changes in both tax and welfare policies on individuals and households		SWITCH
TAXIPP	Static	France	Simulates distributional impacts of changes in French tax-benefit system on households	1997	TAXIPP 1.0 (TAXIPP 2.0 coming)
SIMPL	Static	Poland	Simulates direct taxes, social security contributions, and public benefits for 2003 and 2005	First used in Bargain et al. (2007)	SIMPL
LABORsim	Dynamic	Italy	Integrates demographic projections to model retirement rules and behaviour, migration, education, and labour market participation	First described in Leombruni and Richiardi (2006)	LABORsim
SISU	Static	Finland	Used by Central Statistical Office of Finland to model personal taxation and social	2011	SISU

			security to evaluate the cost and distributional consequences of policy changes		
PENSIPP	Dynamic	France	Models French population to 2060, including births, marital status, employment, earnings, and pensions		PENSIPP
GHAMOD	Static	Ghana	Simulates tax-benefit policy changes, including cost and budgetary implications of universal child or pensions benefits	Launched in May 2017	GHAMOD V1.5
ECUAMOD	Static	Ecuador	Simulate tax-benefit policies (e.g., the Human Development Transfer [Bono de Desarrollo Humano], universal child benefit, or pension payments)	2016	ECUAMOD V1.4
MicroZAMOD	Static	Zambia	Simulates impact of social cash transfer schemes, universal child benefits, universal pension payments, and youth unemployment benefits	November 2017	MicroZAMOD V2.3
MOZMOD	Static	Mozambique	Simulates costs of benefit policy changes, including universal child benefits, universal pension payments, youth unemployment benefits	June 2017	MOZMOD V2.3
TAZMOD	Static	Tanzania	Simulates costs of benefit policy changes, including universal child benefits, universal pension payments, youth unemployment benefits	September 2017	TAZMOD V1.8
MODGEN	Static/ Dynamic	Canada	Computer program designed to create both static and dynamic models for tax/benefit or general policy simulation		

Notes: Blank indicates no information found.