

REPORT



Lessons from Ontario's Basic Income Pilot

Michael Mendelson, Fellow, Maytree

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About the author

Michael Mendelson is a Fellow at Maytree. He was previously Senior Scholar at the Caledon Institute of Social Policy. He has also been a Deputy Minister in several portfolios in Ontario and Manitoba.

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77 Bloor Street West, Suite 1600
Toronto, ON M5S 1M2
CANADA

+1-416-944-2627

info@maytree.com

www.maytee.com

@maytree_canada

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Introduction

In 2016 the Canadian province of Ontario announced that it would sponsor:

[an]...evaluation of a Basic Income pilot. The pilot project will test a growing view at home and abroad that a basic income could build on the success of minimum wage policies and increases in child benefits by providing more consistent and predictable support in the context of today's dynamic labour market. The pilot would also test whether a basic income would provide a more efficient way of delivering income support, strengthen the attachment to the labour force, and achieve savings in other areas, such as health care and housing supports.
(Ontario 2016 p.168)

The Ontario Basic Income Pilot (OBIP) was duly set up, making its first benefit payments in late 2017. By the spring of 2018, the OBIP was completing its enrollment phase. Having barely begun, the experiment came to a crashing halt. In June 2018, Ontario elected a new Conservative government. Soon after its election, the new government cancelled the OBIP (benefits continued to be paid until March 2019 but research was ended immediately).

Ontario's Basic Income experiment is now over. But all is not lost. There are still lessons to be learned from Ontario's experience about how to – and how not to – set up future Basic Income trials.

Many advocates of Basic Income are passionate in their support. Criticism of anything to do with Basic Income, be it a pilot, an experiment or whatever else, has been interpreted as opposition to the very idea of a Basic Income. When the previous Ontario government announced its plan for the OBIP, it was seen by many advocates not as a pilot or an experiment, but as the first stage in the implementation of a Basic Income for all of Ontario. Questioning the OBIP's design was viewed as opposition to a possible Ontario-wide Basic Income program. Discussion of the experiment as an *experiment* – was it well-designed to provide good evidence on critical questions? – was not welcomed.

Now the cancellation of the experiment has doubled down on this challenge to analysis. Critical analysis of the OBIP may be seen as supporting the project's cancellation. This has left little room for disinterested analysis of what was right and what was wrong with the design of the OBIP.

Yet, Ontario’s OBIP attracted attention not just in Canada, but internationally. Other pilots or experiments are under discussion in many jurisdictions – two of which may be Canadian experiments in the provinces of British Columbia (B.C. Poverty Reduction n.d.) and Prince Edward Island (Legislative Assembly of Prince Edward Island n.d.). Any jurisdiction setting up a new Basic Income experiment or pilot, whether in Canada, the United States, Europe or elsewhere, can benefit from understanding key potential improvements and limitations in the design of Ontario’s Basic Income experiment – as an *experiment*, not as a symbol. That is the purpose of this paper.

In this paper, we focus on three aspects of the OBIP in which the experimental design fell short: lack of a “saturation” site, problems of enrollment, and use of the income tax system to test recipients’ income. These three features are highlighted here because, had they been implemented differently, they could have created an innovative and important trial of a Basic Income, instead of undermining it. First, however, we need to clarify what we mean by “Basic Income” and describe the design of the OBIP.

What do we mean by Basic Income?

The Basic Income Earth Network¹ (n.d.) defines a “Basic Income” as a program having all of these five characteristics:

- **Periodic:** it is paid at regular intervals (for example every month), not as a one-off grant.
- **Cash payment:** it is paid in an appropriate medium of exchange, allowing those who receive it to decide what they spend it on.
- **Individual:** it is paid on an individual basis—and not, for instance, to households.
- **Universal:** it is paid to all, without means [*ed.* or income] test.
- **Unconditional:** it is paid without a requirement to work or to demonstrate willingness-to-work.

But despite the Network’s efforts to establish its definition as the one and only type of program that may legitimately be called a Basic Income, the label “Basic Income” is now used for a much wider range of programs that may not share

all these five characteristics. Today, “Basic Income” is instead used as a generic label for programs that may not be universal but instead include an income or means test and also may be paid to a household as a group rather than to each individual in the household (in this paper rather than “family” we use the more neutral term “household” to include multiple adults and children living together as well as individuals living alone and people without actual homes).

In short, the term “Basic Income” is now being used for any program that provides an unconditional (or almost unconditional) periodic minimum income guarantee of a cash (i.e., not a voucher or an in-kind subsidy) payment to everyone in a defined group (i.e., programs that have characteristics one, two, and five as listed by the Basic Income Earth Network). Even the Alaska Permanent Dividend Fund, which has made an annual payment of about \$1,000 to \$2,000 per resident over the last few decades, has been described as a kind of Basic Income.²

All this labelling and relabelling has caused much confusion – not least with respect to the subject of this paper for, as we shall see, the OBIP is like that quip about the “Holy Roman Empire;” neither holy, nor Roman, nor an empire.

While the OBIP was indeed in the Canadian province of Ontario, the Basic Income Pilot is actually a Negative Income Tax (NIT), not a Basic Income at least according to the Network’s definition. Like the Network’s Basic Income, a Negative Income Tax would make unconditional and periodic cash payments, but it would pay households, not each individual in a household, and the household would be income tested; that is, the amount to be paid would be determined by the household’s income.³ Furthermore, if a pilot is primarily a test of the *administration* of a new program, and an experiment is primarily a test of the *outcomes* of a new program, the OBIP is not a pilot; it is a social experiment. Nor is it the first such experiment.

Past Basic Income experiments

The modern post World War II interest in some version of a Basic Income has waxed and waned over the decades (here and subsequently in this paper I am using “Basic Income” in the widest generic sense). The late 60s and early 70s were a period of ferment and societal change when many new, far-reaching initiatives were under serious consideration. In Canada, for example, a Federal-

Provincial Social Security review recommended not only a national Basic Income type of program, albeit conditional for working-age adults, but a substantial improvement in support for social services (Johnson 1975). Four Basic Income experiments were undertaken in the United States and one in Canada.

All of the North American Basic Income experiments had a “randomized control trial” component – called an RCT. In an RCT, subjects are randomly assigned either to a “treatment” group (sometimes multiple treatment groups if a number of alternatives are being simultaneously tested) which gets the intervention being tested (in this case an income guarantee) or to a comparison “control” group. These experiments were set up mainly to test the effects of a Basic Income on labour market behaviour – or, more precisely, on labour market supply. What did the experiments find?

The overall behavioural change in the RCT experiments can be summarized in a few words: nothing definitive and mostly small. There were decreases in hours of employment among women and teenagers, but little or no decrease in hours of employment among men. Participation by women in employment and by young adults in postsecondary education has increased dramatically since the 1970s so it is an open question whether these results have any application in today’s labour market. Incidentally to the focus on labour markets, in the American experiments there was initially found to be a statistically significant increase in marriage dissolution which was the result that had the most impact on the policy debate, but more recent analysis of the data has called this finding into question. (For a description of these experiments see Widerquist 2005, Forget 2011, and Simpson et al 2017.)

By the mid-1970s, when the experiments began to yield data, the economic and social context had transformed. Austerity and a counter-reaction had set in. In Canada, the Social Security Review fizzled out, social service funding was not improved, and the Canadian experiment’s research program was cancelled.⁴ Nevertheless, the experiments are still being studied and remain a source of evidence for the effects of a Basic Income. But the Canadian experiment is especially of current interest because it had a unique feature.

The 1970s Canadian experiment – the Manitoba Basic Annual Income Experiment known as Mincome – had a “saturation site” in addition to a randomized group of recipients. In a saturation site *everyone* in a location is

eligible for the program, just as if it were a regular program and not a trial. In contrast, in a randomized trial recipients are scattered across locations. In an RCT of a Basic Income, rarely, if ever, would a participant in the treatment group know anyone else eligible for a guaranteed income. In contrast, in a saturation site every neighbour, co-worker, and friend is eligible. Employers know that all their employees are eligible. Perhaps most important, in a saturation site every resident has a guaranteed income floor, even those whose income is not currently low enough to qualify for a payment from the program.

A saturation site therefore allows us to look at the effects of a Basic Income on everyone in a community – rich, poor, and in-between – as well as the community as a whole. A saturation site more realistically simulates the effects of a fully implemented Basic Income program in a jurisdiction. Within the constraints of a time-limited experiment (constraints discussed further below), it is possible to look at effects on labour demand as well as supply. A kind of pseudo-control can be created in sites that are economically and socially similar to the saturation site.

The saturation site in Manitoba was the town of Dauphin in which the entire population of about 10,000 was eligible for Mincome. That does not mean everyone in Dauphin got a payment from Mincome. If a Dauphin household's income was too high, that household would not get any Mincome money; but if the household's income fell below the maximum cut-off income level at a future date, the household would then be entitled to a payment from Mincome. While not everyone in Dauphin got a Mincome payment, everyone in Dauphin was *eligible* for a payment. Every household in Dauphin, high- or low-income, had the security of an income floor for the three years of the Mincome experiment.

For a limited time, Mincome functioned in Dauphin as a Basic Income would operate if it were implemented in the whole province (albeit as a temporary program and without the taxes needed to pay for the program). In Dauphin, it was therefore possible to assess effects of a Basic Income on a community, as opposed only to isolated households as in the RCT experiments.

In a seminal 2011 paper, economist Evelyn Forget undertook a new analysis of Mincome's impact in Dauphin using administrative data from Manitoba's health and education system. Her findings pointed to important effects on the population as a whole.

This essay uses a quasi-experimental design and routinely collected health administration data to revisit outcomes for the saturation site. We found a significant reduction in hospitalization, especially for admissions related to mental health and to accidents and injuries, relative to the matched comparison group. Physician contacts for mental health diagnoses fell relative to the comparison group. A greater proportion of high school students continued on to grade 12. We found no increase in fertility, no increase in family dissolution rates and no improvement in birth outcomes. Our results document the value of health administration data for historical analysis, and demonstrate that a relatively modest GAI can improve population health suggesting the possibility of health system savings. (Forget 2011 abstract)

Forget's research sparked renewed Canadian interest in Basic Income and the possibility that prior RCTs and their primary focus on individual labour market behaviour had missed important (and positive) implications for communities as a whole. Forget's research and especially her finding of a reduction in the use of health care services in Dauphin during the Mincome years was an important factor in influencing Ontario to initiate the OBIP.

Not just in Ontario or Canada, but worldwide, there is once again, as in the 1970s, growing interest in Basic Income. This new interest reflects some of the findings in recent research as noted⁵ but it also stems from dissatisfaction with current programs and a hunger for alternatives. Why does poverty persist in wealthy countries and why is inequality seemingly always increasing? Could a Basic Income, sufficient for a modest living standard, become a right for every resident? Would a Basic Income allow us to reduce bureaucracy and stigma associated with today's welfare system? Would a Basic Income reduce both labour force participation and wages? Or would it instead do exactly the opposite: increase labour force participation and wages? (And some argue that labour force participation does not matter at all since a time may be coming, they contend, when employment for wages will be massively displaced by automation so a generous Basic Income becomes a necessity to offset mass poverty.)

For many, a pilot or an experiment is seen as a way to help provide evidence-based answers to these and other questions about Basic Income. This brings us to the OBIP.

Designing the OBIP

In June of 2016, the OBIP was formally initiated by commissioning a discussion paper by Hugh Segal on designing the Basic Income project. Although Segal was not an expert on social experiments, he did have many decades of experience in policy and was able to access expert advice. He also had the advantage of being a well-known Conservative, giving a nonpartisan flavour to the then Liberal government's initiative. Segal's discussion paper "Finding a Better Way: A Basic Income Pilot Project for Ontario" (Segal 2016) provided a thoughtful guide which was the basis for much – but not all – of the OBIP.

The following outlines the main features of the OBIP design, with the Segal report as a starting point.

The amount of the Basic Income guarantee

Segal's report recommended a Basic Income guarantee of 75 percent of the Low Income Measure (LIM). The LIM is a common measure of a poverty level income, often used to compare levels of poverty internationally. The LIM is 50 percent of median income, so the guarantee was effectively 37.5 percent of median income – \$16,989 per year for a single person and \$24,027 per year for a couple. In addition, Segal recommended that the guarantee for persons with disabilities include \$6,000 a year over and above the basic benefit.

The treatment group in the RCT and the population in the saturation site would get OBIP instead of Ontario social assistance, if they chose to do so. Presumably everyone would make this choice because these recommended OBIP rates were substantially more than the rates for social assistance, particularly for adults with no children.

Of course, many of the households in the OBIP would include children. The rate structure for the OBIP did not have to include an amount for children because the adjustment for family size beyond two adults (i.e., for children) would be paid by the existing Canada-wide Canada Child Benefit and the provincial Ontario Child Benefit. The Canada Child Benefit in 2018 was up to \$6,496 per year for each child under the age of six and \$5,481 per year for each child aged 6 to 17 with the full amount paid to all families with less than \$30,450 taxable family income and a graduated reduction rate above that

income.⁶ In addition, the Ontario Child Benefit paid up to about \$1,400 for each child in low-income households.

This rate structure for the OBIP was accepted by the government as recommended by Segal.

The income test

The Segal report recommended that taxable family income as reported on income tax would be used to determine the amount of the Basic Income payment. Using household taxable income as reported through the income tax system to calculate the amount of OBIP payment meant that the amount paid would depend upon last year's income, not this year's income.

Canadian income tax is filed once a year up to four months after the end of the tax year. A household reports its 2017 annual taxable income in April 2018; its 2018 annual taxable income in April 2019; and so on. The Canada Revenue Agency only finalizes tax data in June – six months after the tax year being reported. Using annual retrospective taxable income to determine the OBIP benefit implies that the amount of OBIP benefits paid to a household could be seriously out of date with the household's current income. It also implies that the benefits paid would ordinarily be fixed for a whole year and thus be unresponsive to change in income during the year.

The Segal report recognized, but only in a footnote, that the retrospective and inflexible nature of the income test might prove problematic. However, the report did not recommend how to deal with this issue beyond suggesting in the footnote that “mechanisms should be set to allow for changes in family income and composition to be reflected in the payments between fiscal years” (Segal 2016 footnote 38). This is not so simple in practice.

The use of annual taxable income as the basis for setting the amount of the OBIP was accepted by the government, but without specifying how in-year adjustments were to be accommodated. As we discuss below, the use of the tax system to income test OBIP benefits raised significant challenges, but opened up new opportunities as well.

The “reduction rate”

In any NIT, the amount of the benefit paid is reduced by a percent of reportable income. For example, with a reduction rate of 50 percent, the amount of the Basic Income is reduced by 50 cents for each dollar. So with an OBIP benefit of \$16,989 for an individual and a reduction rate of 50 percent, a recipient would have to have \$33,978 income before the benefit was eliminated entirely. With a 25 percent reduction rate, the same recipient would need \$67,956 income before the benefit is eliminated entirely.⁷

The lower the reduction rate the greater the number of people with higher incomes who become eligible for a larger benefit and therefore the higher the cost of the program. On the other hand, economic theory predicts that a higher reduction rate reduces incentives for employment by decreasing “after-tax” hourly earnings – a 50 percent reduction rate means that \$20 an hour becomes effectively \$10 an hour in the income range where the reduction rate applies. So there is a trade-off: cost versus work incentives. The higher the reduction rate, the lower the cost: the lower the reduction rate, the greater the work incentive.

Segal suggested that the OBIP include multiple reduction rates to test which had a more significant impact on the labour market behaviour of recipients. This would have duplicated the design in several of the experiments four decades earlier. Having multiple NIT structures would have required a larger sample size to differentiate the (presumably) small differences in the effects of each structure: the larger the sample size the higher the cost of the OBIP and the more complicated the whole experiment. Instead, the Ontario government opted to test only one NIT reduction rate of 50 percent. However, other government income security payments usually considered a substitute for wage-income, such as payments from Employment Insurance (Canada’s ironically named contributory unemployment insurance program) or the Canada Pension Plan, would be deducted dollar for dollar, rather than at 50 percent.

The actual reduction rate on income can also be affected by interaction with the tax system.⁸ For example, if an OBIP recipient is also paying 15 percent federal tax and 5 percent Ontario provincial income tax on earnings over the minimum (over \$12,000 in 2018 for an individual with no other deductions), then the combined reduction rate on each additional dollar of earned income is not 50 percent, it is 70 percent – an extremely high tax rate. Consequently, the

Segal report recommended that the tax system be adjusted for recipients so that no income tax would be paid for those receiving OBIP benefits.

In practice, an adjustment was not made to take account of income tax simultaneously payable by OBIP recipients. Such an adjustment would in any case likely not be realistic were a Basic Income with OBIP rates actually implemented province-wide. OBIP benefits continued until earned income was \$33,978 for individuals and \$48,054 for couples; with an additional \$12,000 earned income for persons with disabilities.

Ensuring that no one receiving OBIP payments paid income tax would require exempting a substantial portion of the population from paying any income tax with a commensurate loss of tax revenue. As well, the amount of income tax currently due could not just begin when income went \$1 above the phase-out level for receipt of OBIP benefits (e.g., \$33,979 for an individual), since doing so would create a “notch” in the tax/transfer system wherein after-tax income fell due to before-tax income increasing. Instead, taxes would have to be only a percent of income *above* the phase-out level for OBIP, resulting in much more revenue loss, potentially all the way up the income scale. This issue is discussed further below.

OBIP test population

As noted, Segal recommended testing multiple NIT reduction rates. He also recommended RCTs in both a larger city and smaller towns in Ontario. In addition to the RCTs, Segal recommended three saturation sites to reflect different geographic and demographic realities in Ontario. This would have been an expansive and expensive experiment. The government chose instead a much more limited sample with many fewer sites.

The Ontario government selected three communities to participate in the OBIP: Hamilton (population of about 700,000), Thunder Bay (population of about 120,000), and Lindsay (population of about 20,000). Some surrounding areas were included in both Hamilton and Thunder Bay. The target was 1,000 households enrolled in the RCT component of the OBIP in each of Hamilton and Thunder Bay, and 2,000 in Lindsay. Lindsay was to be the saturation site; however, in all three sites, including Lindsay, enrollment was limited to those

who *at the time of enrollment* were entitled to at least some cash benefit from the OBIP.

The Segal report recommended that the OBIP be limited to persons who during all three years of the experiment would be between the ages of 18 to 64; i.e., maximum age 61 at enrollment. Why not include seniors? In Canada, every resident individual or couple over 64 already gets a Basic Income, made up of a quasi-universal benefit (called Old Age Security or OAS), that is taxed back from those with higher income, plus an income-tested benefit (called Guaranteed Income Supplement or GIS) which is taxed back at 50 percent above \$3,500 of taxable earned income.⁹

Three critical issues in the design of the OBIP

The missing saturation site

To have been a saturation site as was Dauphin, Manitoba, *every* resident of Lindsay regardless of income would have had to be eligible for the OBIP throughout the planned three years of the experiment. Instead, only those households were eligible which at the time of enrollment had income low enough to be entitled to a payment. This is not a saturation site. Saturation would have meant, for example, that a young adult could decide to quit work and go back to school to get a diploma or a degree, taking advantage of the OBIP. Or a Lindsay resident who had decided not to enroll in the first year could change her mind and sign up in year two. Or, perhaps, the guarantee of a minimum income could create a greater sense of security and reduce economic anxiety throughout the whole community, as even those who were not currently entitled to an OBIP payment would know that they had a real safety net “just in case.”

Is having a true saturation site important for a Basic Income experiment?

Previous experiments focused primarily on labour market behaviour, but as noted, Forget found important community-wide effects in Dauphin outside of the labour market, including a decrease in health care service use. In more recent research, David Calnitsky and Pilar Gonalons-Pons used administrative data and found a reduction in property crime in Dauphin during the Mincome experiment (Calnitsky and Gonalons-Pons forthcoming). Further community

effects aside from those on the labour market might also be found in a true saturation site.

But what about labour market effects? Could a saturation site obtain evidence about labour market effects different from those of an RCT? Aside from the self-evident missing test of whether people who are not currently poor will change their labour market behaviour due to the availability of an income guarantee, Calnitsky summarizes five additional “bits of social reality that make randomized experiments less useful than usually acknowledged...” even when we are looking specifically at labour market behaviour:

Role modeling. If people see others reducing work hours (or, say, returning to school) they might follow suit. And vice versa: If no one around you is part of the experiment and no one is adjusting their work schedule, you might not want to be the only one cutting back work hours.

New social norms. If a new, universalistic policy fosters a more pragmatic and less moralistic attitude toward program participation, the social stigma attached to government assistance and work reduction might diminish. Although a short experimental period may not allow new norms to develop, the sudden introduction of a radically different system of social provisioning might interrupt common sense around “appropriate” labor market behavior.

Business response. If people reduce work and labor markets tighten, firms might try to pull people back into the labor market with higher wages. Alternately, people might work less at higher wage rates. In either case, the randomization model assumes, implausibly, that local business would overlook the fact that their whole workforce has obtained an exit option from the labor market.

Collective action. If basic income increases the likelihood that employees pursue their material interests collectively, one of the objectives sought after might include reductions in work time.

Work for the underemployed. If some people reduce work hours, new work opportunities may open up for involuntarily unemployed or discouraged workers. (Calnitsky 2019 p25)

Calnitsky and Latner tested the hypothesis that there are “community” effects not found in isolated RCT households’ labour market behaviour. Calnitsky and Latner compared the reduction in labour market participation in Dauphin to that in a randomized group of rural Mincome recipients. This rural dispersed sample was created specifically to allow for comparisons to Dauphin’s largely rural economy and had not previously been studied. Calnitsky and Latner found that “nearly 30 percent of that 11.3 percentage point fall in labor market participation – or, about 3.1 percentage points — can be attributed to ‘social interaction’ or ‘community context’ effects” (Calnitsky and Latner 2017 p375).

Calnitsky went a step further and retrieved Mincome surveys of Dauphin employers from the archives that had not previously been digitized. So far as this author is aware this is the only attempt to look at the “demand side” of actual labour market behaviour in response to a Basic Income experiment, as opposed to simulations, and is only possible in a saturation site. Calnitsky found that “wage rates offered on advertised job vacancies and actual wages on new hires grew in Dauphin. In contrast, control firms report no wage growth on advertised job vacancies and slower wage growth on new hires” (Calnitsky 2018 p2).

Calnitsky, Forget, and others’ research of the effects of Mincome in Dauphin, which was the one and only Basic Income saturation site, provides strong evidence that there are potential effects of a Basic Income implemented across a whole jurisdiction which are simply not evident in an RCT. Some of these effects may be positive – such as on health services and crime – while others may be seen as negative – such as reduced labour force participation; whether positive or negative, the implication is that critically important consequences of a Basic Income are simply missed without a saturation site.

In the OBIP, the town of Lindsay has some characteristics of a saturation site in that at the time of enrollment everyone with at least one year residence in the town could apply to the OBIP, but anyone failing to be eligible for a payment at the time of enrollment or simply failing to enroll was cut off from then on. Consequently, Lindsay was not a saturation site.

The government did not provide an explanation as to why it failed to make Lindsay a true saturation site – indeed, it continued to refer to Lindsay as the saturation site. Whatever the rationale, the effect of this decision was to reduce

the possibility of finding any community-wide effects, and to increase the chance of “false negatives” (no evidence of community-wide effects that would be seen in a true saturation site). Yet with the inspiration of the Forget findings, possible community-wide effects were one of the fundamental reasons for establishing the OBIP in the first place.

Enrollment and randomization

You might think it is easy to give away free money, but it is not. The government’s initial plan was to mail out invitations to apply for the OBIP, using census data to identify areas where incomes were likely low enough so that many households would be eligible for a payment. The applicant would fill out a form, which included permission for the government to access its household tax, banking, and other information, otherwise protected under privacy laws. The household would be enrolled in the OBIP only if it met all the criteria, including low income, making it qualified for an OBIP payment. In practice, assessing eligibility usually required a lot of back and forth between the government and the applicant before a final determination could be made.

According to Greg Mason, an academic who was engaged to assist in the early design of the project: “Ontario started mailing invitations in June 2017 and by September, after mailing 37,000 invitations it had managed to enrol barely 150 participants, well short of the original target of 2,000” (Mason 2018). There are likely many contributing factors to this failure; the prevalence of “scam” offers that are difficult to authenticate; a profusion of legalistic language insisted upon by lawyers; complexity and difficulty in filling out the form; and the requirement to allow access to tax information. Under-enrollment should have been anticipated because a phenomenon across many wealthy western countries is that the so-called working poor are reluctant to register for programs that require a declaration of poverty or need: a take-up rate of 30 percent is not unusual, but a rate of under a half of one percent is a singular achievement.

Due to the continuing difficulty of recruiting participants, the OBIP revised its enrollment process. Local organizations were engaged to set up group meetings of households likely to be eligible for OBIP benefits. The OBIP was explained at the meetings, and questions were answered; those wishing to enroll could complete an application on the spot with assistance from staff. There are no official data on the number and location of households that were recruited by

the time of the experiment's cancellation, but even if the overall target numbers were fully reached, the recruitment method meant that the OBIP was no longer a randomized study.

People who come to a meeting at the behest of a local service agency are not a random sample. A sample could be “normalized” (stratified) through oversampling and then selecting the subgroups that match key demographics, income, and other critical characteristics of the population, or at least the target population. However, it is not known in advance exactly what the “critical” characteristics would be in respect of an OBIP: characteristics matching the population on social assistance, the low-income population more generally, or the whole population of the eligible age group? Technically, the variables used to stratify the sample cannot be any of the variables being tested by the experiment to explain outcomes, nor can they be highly correlated with these variables. This would rule out, for example, health or employment as variables in stratifying the sample.

Although not as problematic as in Lindsay, under-enrollment was also a challenge in Dauphin in the 1970s despite promotional advertising and staff having knocked on every door in the town (Simpson, Mason et al 2017). It is estimated that only about 34 percent of eligible households participated in Mincome in Dauphin (Hum 1981). Future experiments need to think through alternative and more pro-active routes to recruitment, as is suggested below in this paper.

How income was tested in the OBIP

As noted above, while in many respects the OBIP was a repeat of the 1970s guaranteed income experiments, there was one aspect of the OBIP that was entirely unique: in the OBIP, the income tax system was used as an annual “income test” to calculate the amount of benefits a recipient would get. In all the other experiments, including Mincome, a separate monthly income test specially designed just for this purpose was administered to determine the monthly benefit to be paid. Anecdotally,¹⁰ the monthly Mincome income test (and annual reconciliation) proved a substantial burden for administrators.¹¹

Most income security systems providing for basic needs, such as food and shelter, are designed to be “responsive” to changes in income. (See Whiteford,

Mendelson, and Millar 2003 for a discussion of responsiveness.) Households may experience substantial declines in income after a “good” year; for example, due to unemployment or illness. The received wisdom has been that low-income households need highly responsive programs because they cannot “average” their income over a number of months or years; they need to pay the rent and buy groceries this month. Low-income families usually can save little or nothing for emergencies and extraordinary expenses – in fact, most carry substantial debt.¹² However, it is possible that the received wisdom may be incorrect or at least overstated: there are some large Canadian programs in place today that rely on retrospective annual income tax data and appear to be well-accepted by Canadians.

The Canada Child Benefit bases its payments on the income tax system and timeliness has not turned out to be a visible problem in practice. The responsiveness issue has not been the subject of regular complaints to Members of Parliament or reports in the media about hard-done families. However, households in Canada continue to get maximum child benefits even with substantial incomes: the “turning point” where child benefits begin to be gradually reduced is over \$30,000. For many low-income households, child benefits will remain unchanged since the household will in any case likely remain under or close to the income level where child benefits are at the maximum despite fluctuations in household income. In most provinces, social assistance also provides a fall-back for those in extremely serious financial trouble.¹³

The OAS/GIS is another program which determines its benefits according to the previous year’s taxable income. The OAS/GIS may also be a special case because most pensioners’ income likely changes little from year to year. Moreover, the OAS is paid universally up to relatively high incomes, so it provides a cushion regardless of changes in annual income. In fact, the OAS and GIS together represent a kind of hybrid combined Universal Basic Income and Negative Income Tax for every Canadian citizen or legal resident 65 years of age or older who has resided in Canada for 40 years since the age of 18 (and prorated for residence between 10 and 40 years).

This author also notes that in a focus group for low-income households, organized by the Caledon Institute of Social Policy, recipients themselves expressed a unanimous and strong preference for child benefits based on income tax. They preferred “main stream” income testing and a predictable

benefit even at the expense of unresponsiveness. Although admittedly anecdotal and a tiny sample, the possibility that recipients may highly value predictability, and with it the implication of less bureaucracy and less stigma, should be taken seriously enough at least to consider whether a less responsive but more predictable Basic Income design is workable.

Thus, given Canada's functioning programs using tax data¹⁴ and possible recipient preferences, the OBIP would have been a valuable test whether annual tax data could indeed be used as the basis for income testing in a functional Basic Income program. While a full-fledged province-wide Basic Income would doubtless require much coordination with the federal Canada Revenue Agency, for the limited purposes of the OBIP the province could simply use tax data to which it already has access without involving the federal government. Whether the OBIP's use of income tax for income testing proved viable would have depended upon several factors including the number of recipient households experiencing fluctuations in income,¹⁵ the extent of these fluctuations, whether this left households in serious deprivation (or, perhaps, in serious "surplus" which would not have been a problem for recipients unless they then had to pay back overpayments), the "mechanism" needed as noted in the Segal report to respond to these fluctuations, and the added cost, if any, to both governments and recipients of this mechanism.

The use of income tax is rarely discussed in any depth by academics writing on the topic of Basic Income with a few notable exceptions (see Hum 1981 for a comprehensive discussion of taxation and Mincome; more recently see Tedds 2017), but it is not a minor technicality. On the one hand, if it is possible to use retrospective tax data, the administration of a Basic Income could be greatly simplified, administrative expenses substantially reduced, and, from a recipient perspective, potential stigma associated with the program would be greatly reduced or eliminated.¹⁶ On the other hand, using retrospective tax data implies that benefit payments in any given month may be wildly out of sync with household needs in that month.

In addition to the issue of responsiveness, use of the tax system also raises a number of other issues as set out by Hum and Tedds. These include the definition of income (for example, initial distributions from a Real Estate Investment Trust are not taxable income), residency, the absence of information on wealth, and other issues (Tedds 2017). Use of the tax system is not a black

and white issue: it is precisely the kind of question about which empirical evidence would be especially valuable. Finding out whether it would be possible to use the income tax system to “test” income for a Basic Income could have been an important result from the OBIP.

Regrettably, it is not clear whether the OBIP had established a standardized response to income fluctuations. Enrollees were given the option of reporting an in-year income decline, but were apparently not obligated to report in-year income increases. Precisely how much compensation recipients with an in-year decline in income were to be provided is unclear.¹⁷

A province-wide Basic Income that provided extra funds in response to an in-year income decline, but no adjustments for an in-year income increase and no reconciliation, would increase costs significantly. Asymmetrical treatment of income increases and decreases would also raise issues of fairness in the treatment of households with similar incomes but differing timing in the flow of income. Programs that have combined in-year adjustments and annual reconciliation in the UK and Australia have run into substantial problems, mainly because they involve overpayments that must either be repaid or ignored. (See Millar and Whiteford 2019 for a recent review of responsiveness in regard to programs in the UK and Australia.)

While the Segal report did not recommend a plan for issues arising from use of the income tax system, it did recommend that “administrative costs or savings” should be one of the focuses of the OBIP. At the time the experiment was cancelled, the OBIP did not yet have a plan to study administrative issues. Although administrative cost saving has been cited for at least the last half century as one of the hoped-for benefits of a Basic Income, with some proponents going so far as to claim that administrative cost savings will offset a major portion of the added cost of the Basic Income, administrative cost was not part of the research program in the earlier experiments (there are two minor exceptions – see Mendelson 1978). If administrative issues, including costs, are to be recognized as a serious issue, these need to be part of the research design of the experiment and not an afterthought.

A missed opportunity was also available given the use of the tax system: Lindsay, Ontario could have become a true population-wide saturation site by administering the OBIP in that town as an automatic tax-transfer program.

Everyone in town could have been enrolled with an opt-out rather than an opt-in provision using tax data so that take-up would have approached 100 percent. This would have added costs but, as there were only about 2,000 households below half of median income in the eligible age range in Lindsay (according to the 2016 census), the added cost would likely have been modest. The result would have been a ground-breaking and truly unique experiment to understand potential community-wide effects of a Basic Income.

Inherent limitations of a Basic Income experiment

On the one hand, the three issues highlighted here – saturation site, enrollment, and use of the tax system – compromised the integrity of the experiment; on the other hand, taken together these issues represented an opportunity to obtain evidence based on a more realistic simulation of a fully implemented Basic Income. With a real saturation site in which the whole population is automatically enrolled, and a well thought-out protocol for dealing with in-year income variation, the OBIP could have been the world’s first population-based Basic Income experiment in a whole community.

In addition to these three issues, other questions about the design of the OBIP could continue to be catalogued. One example: households were allowed to decide themselves who got the cheque in a two-adult household, raising questions related to gender. Another issue: the retention of earned income was actually more generous in Ontario’s social assistance program than the OBIP rate structure, since the first \$200 a month of earned income was exempted in the social assistance program but not in the OBIP. What did this imply for the employment incentive effects of the OBIP compared to the control group?

None of the criticisms in this paper imply that the OBIP was of no value. Notwithstanding the lack of a true saturation site, the OBIP would still have told us about the effect of a temporary income guarantee on individuals in the current economy. The OBIP also had a relatively high guarantee level. No social assistance system is overly generous, but in Ontario (and most Canadian provinces) adults who have no children and are not in the “disability” stream have especially inadequate levels of social assistance. A recipient may be able to stay alive, but for many only with the assistance of food banks and other

services. The OBIP would have told us about the consequences of a much more adequate income for these recipients.

Ironically, the OBIP's higher rate structure also presented a research problem: the main characteristic that separates a Basic Income from social assistance is not higher rates but non-conditionality. In all of the media interviews with recipients, the feedback was overwhelmingly positive – but the praise was always about the effects of higher rates and never about the OBIP being unconditional. How would the OBIP data have distinguished between the effects of higher rates versus the effects of the OBIP benefits being unconditional? On the other hand, given that participation in an experiment is voluntary and achieving enrolment targets is difficult, how can people be persuaded to participate unless benefits are appreciably higher than they would otherwise have enjoyed? (Wilderquist 2006 also discusses this issue.)

If non-conditionality were the critical variable to be tested, an alternative relatively inexpensive “Basic Income” experiment would be simply to make existing social assistance unconditional for a saturation site. The recent Finnish experiment tested non-conditionality of an existing unemployment benefit, but only for those already eligible for unemployment benefits and without a saturation site (Kangas et al 2019). On a small scale this is also what the city of Utrecht is doing in one stream of its current experiment, albeit again without a saturation site (City of Utrecht n.d.).

Yet, even if every problem were solved and the best possible experimental design was implemented, including a saturation site, the result would still not provide conclusive evidence about what would happen should a full-fledged Basic Income be implemented for a whole population as a permanent program. A Basic Income experiment will always have inherent limitations. A few examples of these limitations:

- Humans are not molecules. The behavioural response to a Basic Income will depend on the cultural and economic conditions at the time. Are people who are not working disparaged or admired? Is education highly valued or seen as at best instrumental? There was very little labour market response among adult males in the experiments in the 1970s: perhaps social attitudes have now changed and the results today would be different. And maybe the results would again be different another few

decades from now – or in a different country. Consequently, the results of an experiment cannot necessarily be extrapolated through social and economic contexts, and also cannot be extrapolated through time.

- An experiment is necessarily time-limited. This presents two distinct ways in which an experiment may fail to replicate a permanent Basic Income program. First, the experiment is temporary so people will make plans only for the short run. For example, people make life choices (e.g., quit a job and start a business; go back to school) based on the continuing availability of a guaranteed income but perhaps not if the guaranteed income will only last a few years. Generally, adjustments in the labour market or the economy in general as a consequence of a Basic Income would likely be different for a permanent program rather than a temporary experiment. Second, many effects of a permanent program might take many years or perhaps many decades to become apparent. Unemployment insurance in Canada is an example: in much of Atlantic Canada the number of hours of partial year employment is precisely the number of hours needed to qualify for unemployment insurance benefits, but this effect would not have been apparent in the first decade or so of the program.
- One limiting aspect of a Basic Income experiment is that a permanent full-fledged Basic Income program will require transformation of the tax system. As noted above, the Segal report recommended adjusting the tax system for recipients so that no income tax would be paid for those receiving OBIP benefits, but this was not done and possibly could not have been done.¹⁸ This is not a simple “fix.” Preventing simultaneous receipt of benefits and income tax liability would require exempting a substantial number of Canadians from paying any income tax at all and reducing income tax for many more (as discussed above). Presumably, if there were a full-fledged provincial or national program not only would the tax system have to be modified to “fit” with the new program; overall taxes would also have to be increased to pay for the new Basic Income program and to compensate for the tax no longer paid by those with increased exemptions. This problem is additive: the higher the Basic Income guarantee, the greater the increased tax required due both to higher levels of revenue lost and higher costs of the program itself.

There are behavioural consequences resulting from a tax increase as well as those resulting from the Basic Income. An experiment looks only at the response to the benefits paid out, but not at the other side of the equation. Even a pilot meant only to assess the administrative feasibility of a Basic Income could not test the feasibility of simultaneous changes in the tax system. This core “untestability” would be especially problematic for a truly universal Basic Income such as that advocated by the Basic Income Earth Network, which would demand profound changes to the tax system.

All the experiments undertaken so far have been oriented to the behavioural consequences of a Basic Income. Yet the limitations described above suggest that a time-limited experiment is at best indicative of possible behavioural responses and not, as sometimes assumed when the RCT is held up as the “gold standard” for evidence, definitive. Perhaps more stress should be placed in future experiments on the administrative issues, such as how best to enroll everyone eligible in a Basic Income, including hard-to-reach groups such as the homeless – in short, a little more of a pilot project (as the name of the OBIP suggested), in which reasonably definitive tests can be made of how to run a Basic Income.

Conclusion

Given world-wide interest, almost any project with the label “Basic Income” can gather a wave of publicity far beyond the project’s potential significance. For example, despite the modesty of Finland’s brief experiment with unconditional unemployment benefits, the phrase “Finnish Basic Income experiment” has 9,210,000 results from a Google search. Nor is the media too finicky about calling almost anything “Basic Income.” Consequently, governments may be tempted to get easy publicity by attaching the name “Basic Income” to any project bearing any relationship, no matter how tenuous, to a Basic Income.¹⁹

One danger is that governments may use this route as an inexpensive way to appear progressive and innovative while actually deferring action on substantive improvements to real programs today. Advocates should be wary of contributing

to this ruse by attempting to create a “bandwagon” effect through listing dozens of questionable and minor Basic Income projects across the globe.²⁰

Whatever one’s criticism of the OBIP, it certainly did not fall into the cheap and easy category – it was expensive and a real Basic Income experiment – or more precisely, a Negative Income Tax experiment. Nevertheless, it seems the OBIP fell prey to political expediency. Critical decisions were made without putting highest priority on the research goals of the experiment. Of course, decisions by governments are necessarily political. But once a government has made a decision to initiate a Basic Income experiment, set out the goals of the experiment, and provided the funding, the project needs to be insulated as much as possible from short-term political considerations.

The need for insulation ties into another political reality: most meaningful experiments will require longer than a single term of government. Not coincidentally, Mincome and the OBIP both were cancelled mid-stream by a new government coming into office. The lesson is that projects have to be run by a third party whose priority is the experiment, and the third party needs to be funded to complete the project from start to finish.

Substantively, if an experiment is to contribute meaningfully to evidence about the impact of Basic Income, the experiment needs to be designed to simulate as closely as possible the operation of a full-fledged Basic Income as it would be implemented for a whole jurisdiction. One of the implications of simulating a full-fledged Basic Income is that the experiment must include a true saturation site. Without a saturation site we can never obtain information about the effects of an actual program, let alone community-wide impacts. This is perhaps the most important missing piece from the OBIP, and all but one (Mincome) of the experiments carried out to date. If the next jurisdiction to implement a Basic Income experiment wants to do more than repeat bits and pieces of what has been done to date, it must include a saturation site.

Two different types of Basic Income experiments have been suggested in this paper. One would be an ambitious and path-breaking tax-based Basic Income with automatic enrollment of a whole community on an opt-out basis. The other would be a modest experiment simply making existing social assistance unconditional in a “test” community. These are at the opposite ends of the spectrum in terms of expense and innovation, but they have one aspect in

common: to make a new meaningful contribution, both types of experiment demand a saturation site.

Finally, an administratively oriented pilot is needed as much as a behaviourally oriented experiment. There is nothing stopping an experiment from simultaneously being a well thought-out administrative pilot, except the attention of the planners setting up the experiment.

In sum, given the experience of the OBIP, governments considering another Basic Income experiment should consider a five step process:

1. Invest the time and effort needed up-front to formulate the questions you want to answer.
2. Consult widely and thoroughly with experts on how to design an experiment that will provide good evidence towards answering these questions.
3. Design the experiment and field test it including feedback from participants and independent experts.
4. Based on a tested design, establish a realistic budget and timeline for the experiment from beginning to end, including analysis of the results.
5. Award an independent external agency the job of running the experiment and endow it with the funds needed to carry out the experiment from start to finish.

Endnotes

- 1 The Basic Income Earth Network was founded in 1986 as the Basic Income European Network.
- 2 “Alaska has had a version of UBI since 1976, thanks to a policy brought in by the Republican governor Jay Hammond. It is called the Alaska Permanent Fund...” (Lanchester 2019 p.6). The new Italian program which pays a conditional income tested unemployment benefit has also sometimes been described as a “Basic Income” and even a “Universal Basic Income.”
- 3 More precisely, in a Negative Income Tax or other income-tested Basic Income designs, the amount paid “up-front” is taxed prospectively. In a Universal Basic Income (UBI) as advocated by the Basic Income Earth Network, the amount paid “up-front” would be taxed retrospectively. Ignoring issues of household versus individual income, the net incremental incidence can be the same in both designs, depending upon the income tests and incremental taxes needed to pay for the programs. In short, the difference between an income-tested and universal program is often exaggerated. The more substantive difference between the Earth Network’s Basic Income and a standard Negative Income Tax may be that the latter is usually applied to a household rather than to an individual recipient; although even this difference is not necessary since an NIT could also be applied to individuals. The implication of this near equivalence is that in assessing preference for a UBI versus an NIT the issues that matter are political feasibility and administrative efficiency.
- 4 More precisely, the Mincome project was not cancelled: it was just allowed to run out of money. See Simpson et al 2017 for the history of Mincome.
- 5 Recent research includes Forget’s and Calintsky’s in Canada but a good deal of both quasi-experimental and non-experimental research is underway in Europe. See, for example, Torry 2019 for an interesting discussion of developing micro-simulation models of Basic Income.
- 6 The Canada Child Benefit (CCB) is calculated as follows: \$6,639 per year for each eligible child under the age of six; \$5,602 per year for each eligible child aged 6 to 17; for families with one eligible child the CCB reduction rate is 7% of the amount of “Adjusted Family Net Income” between \$31,120 and \$67,426 plus 3.2% over \$67,426; for families with two

eligible children the reduction is 13.5% between \$31,120 and \$67,426 plus 5.7% over \$67,426; for families with three eligible children the reduction is 19% between \$31,120 and \$67,426 plus 8% over \$67,426; for families with four or more eligible children the reduction is 23% between \$31,120 and \$67,426 plus 9.5% over \$67,426.

- 7 The formula is: the inverse of the reduction rate times the guarantee rate equals the amount of income where the payment is diminished to zero – often called the “break-even” point.
- 8 The reduction rate on child benefits would also be additive in the applicable income ranges.
- 9 In 2019 the combined OAS/GIS guaranteed a minimum income of about \$18,000 a year for an individual and \$27,400 for a couple. Oddly, this functioning Canadian guaranteed income, in operation for half a century, is rarely mentioned in either national or international discussions of Basic Income.
- 10 As a graduate student, this author was a summer student at the Mincome experiment.
- 11 In Mincome the payment period was monthly based on a monthly income test, but the total annual benefit amount depended on annual income, with the income tax filing used for annual income assessment with various adjustments. This meant that benefits had to be reconciled annually so that the sum of the total paid each month was equal to the amount that should have been paid based on annual income. See note 15.
- 12 “Average net saving for all Canadian households was \$852 in 2018, while the highest income quintile (the top 20% income earners in Canada) saved \$41,393 per household. Conversely, households in the lowest income quintile had net dissaving of \$27,935, as on average they consumed more than their annual income and either had to incur debt or draw down previous savings to finance their consumption” (*The Daily*, Statistics Canada, 27 March 2018).
- 13 There is no guarantee that social assistance would be retained if a Basic Income were implemented. In Ontario, the social assistance fallback for some child benefits was planned to be eliminated in 2019 in circumstances

analogous to the introduction of a Basic Income (see Kapoor 2019), but the government reversed its decision after a public outcry.

- 14 There are also a number of smaller programs in Canada with income related benefits both at the federal and the provincial level using annual income as reported in the income tax system; e.g., the Working Income Tax Benefit and the Ontario Trillium Benefit.
- 15 In the Mincome experiment it appears that income fluctuations sufficient to have an impact on eligibility and overpayments were experienced by over a third of recipients, who had low income part of the year and higher income in the remainder of the year. “This problem of divergences (of entitlement based on annual income versus monthly income) was especially evident in cases of families that start the year with income low enough to qualify for payments, but then enjoy increases in income sufficient to take them above the breakeven point. Over one-third of 1976 participants followed this pattern and arrived at year end with total payments in excess of what their total income would warrant” (Crest et al 1979 p63). Hum reports that “In the first eight months of 1975, for example, incomes of families enrolled in the NIT varied, on the average, 30 per cent from one month to the next” (Hum 1981). This was over 40 years ago and income patterns may or may not have changed substantially since then.
- 16 “The tax system and social benefits delivered through it lack the stigma associated with social welfare. There is a firmly etched notion that receiving support through social welfare is shameful and defaming, yet qualifying for and receiving support through tax benefits is almost praiseworthy — often sold by politicians as a means of getting money back into the hands of ‘hard-working Canadian families’” (Tedds 2017 p6).
- 17 As a member of the research advisory committee to the OBIP, this author asked to see the directives for supplemental benefits in response to an in-year decline in income and was told orally that this was being treated on a “case-by-case” basis.
- 18 Income tax rebates were paid to all participants in the Manitoba Mincome experiment to neutralize the additive effect of income tax on top of the Mincome reduction rate. The tax rebate was paid not only to those receiving an actual Mincome benefit but also a tapering off for those whose income was above the phase-out income level for Mincome benefits. This

added a whole extra layer of complexity to the calculation of payment and consequent adjustments. These are described in detail in Hum 1981. It is not clear whether such a rebate mechanism would be possible in today's tax environment, even in the context of a limited pilot or experiment.

- 19 The latest “big publicity/small project” example is in Stockton, California where a wealthy philanthropist has donated \$1.1 million to the city, which is giving \$500 a month unconditionally to 125 low-income beneficiaries. All the beneficiaries are doubtless happy with the program.
- 20 For an example of such a listing: “Fortunately, there is a large body of empirical evidence about the effects of UBI, thanks to a range of pilots and experimental schemes, from an extraordinary range of places: Manitoba, Iran, Finland, Stockton, Kenya, the Cherokee nation, Alaska, Brazil, Mexico, Liberia, Honduras, Indonesia, even the City of London” (Lanchester 2019 p.6).

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