Does a Universal Basic Income Reduce Labour Supply for All Groups? Evidence from Canada's Negative Income Tax Experiment

Chris Riddell
Department of Economics, University of Waterloo

Craig Riddell
Vancouver School of Economics, University of British Columbia

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Author Note

The authors can be contacted at c2riddell@uwaterloo and Craig.Riddell@ubc.ca.

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Abstract

We investigate the labour supply effects of the Canadian negative income tax (NIT) experiment known as MINCOME. The North American NIT experiments have received considerable attention in recent years given the renewed interest in a universal basic income. However, while many papers exist on the U.S. NITs, the Canadian NIT experimental literature consists of a single study. We are unable to replaicate the results of that study and question the validity of its conclusions. Our reassessment yields very different results compared to what is currently believed about MINCOME's labour supply impacts. We find large and statistically significant adverse effects on labour supply for women from two-headed households, but no compelling evidence of a movement out of the labour force. Our point estimates on hours worked for this group are similar to those found in the Seattle-Denver NIT study that offered more generous treatment plans, and the reduction is higher relative to baseline annual hours than U.S. NIT evidence. Conversely, for single parents (90% single mothers) we find large positive treatment effects on hours worked and on the likelihood of working. The single-parent results, which are robust to two different data sources and differences in sample construction, differ from previous NIT evidence in both countries, although are consistent with standard labour supply theory. Finally, we find no significant impact on either the intensive or extensive margin for men in two-headed families. Overall, our results for the Canadian experiment suggest that the guaranteed income benefit reduced hours for women from two-headed households and increased both movement into the labour force as well as hours worked for single parents—impacts that indicate a guaranteed income with NIT features can have offsetting positive and negative work incentive effects.

Introduction

A universal basic income (UBI) or basic income (BI) program is receiving considerable attention in many countries.[1] Many books and articles have recently been written by BI advocates in Canada (e.g., Forget, 2018; Segal, 2019), the U.S. (e.g., Lowrey, 2018; Murray, 2016; Yang, 2018), and Europe (e.g., Haagh, 2019; Van Parijs & Vanderborght, 2017). Numerous demonstration projects and experiments are underway or planned. For example, Finland recently conducted a pilot where 2,000 randomly selected individuals from the unemployment rolls were paid a regular monthly income of $635 (U.S. dollars at the time) for 2 years, which was not reduced if they found work. Randomized trials have also been conducted in Barcelona and in municipalities in the Netherlands, and have been initiated in North America.[2]

A central issue that societies face when considering introducing BI is its probable effect on work activity. How many of those currently in the workforce would choose to withdraw from paid employment and instead rely on the BI to meet living expenses? Among workers who choose to remain employed, what proportion would work fewer hours and/or move to less remunerative but different jobs—such as work that is less stressful, more rewarding, or a better fit with family responsibilities? Not all BI advocates regard reductions in paid work as a negative outcome—some view reductions in low-skill labour supply as needed because they would offset large predicted reductions in labour demand arising from technological change, especially artificial intelligence and robotics. Others view freeing some individuals from the need to work, or to engage in jobs with negative attributes, as a benefit of a BI policy. Nonetheless, a key question is whether there would be sufficient taxpayer support for a policy that transfers substantial amounts of money to those who choose not to work or to reduce their work activity.

The importance of understanding the likely labour supply impacts of various BI proposals has in turn resulted in greater attention to the lessons from past experience with financial transfers to the low-income population. This is especially the case for the guaranteed annual income or negative income tax (NIT) experiments carried out as part of the "War on Poverty" in the 1960s and 1970s. During that period, the U.S. and Canada conducted a landmark series of negative income tax experiments, including four U.S. experiments—(a) New Jersey; (b) Rural Income Maintenance Experiment (RIME) carried out in rural counties in Iowa and North Carolina; (c) Gary, Indiana; and (d) Seattle-Denver (or SIME-DIME)—and one Canadian experiment in the province of Manitoba (MINCOME). These experiments involve a set of policy parameters close to those in many BI proposals, including those defined by Hoynes and Rothstein (2019) and the Stanford Basic Income Lab.[3] As such, these experiments form the

[1] Some writers distinguish between basic income (not necessarily available to everyone) and universal basic income (universally available). For simplicity we refer to both types of proposals as BI.
[2] Stanford Basic Income Lab lists 11 BI experiments underway in North America, including one (in Ontario, Canada) that was subsequently cancelled: https://basicincome.stanford.edu/research/basic-income-experiments/.
[3] One potential difference in the cash benefit received in the NIT experiments relative to some basic income policies proposed today is the extent to which the payment is clawed back with earned income. Not all BI/UBI proposals include a clawback feature.

basis of much of our current understanding of what the effects of a basic income approach would be on individual and household outcomes.

Figure 1—taken from Moffitt (2003)—illustrates the direction of predicted labour supply responses to introducing a NIT using the basic income–leisure choice model, the workhorse of static labour supply theory. In the absence of a NIT and welfare, the budget constraint is the line AF, with slope equal to the hourly wage rate. Adding social assistance (welfare) that provides benefits AC and imposes a tax-back rate of 100% (i.e., reduces welfare benefits dollar-for-dollar on any market earnings) yields the budget constraint ACDF. Finally, introducing a NIT with guarantee-level AC and a tax-back rate less than 100% yields the dashed budget constraint ACD'F with the dashed NIT component CD'. For low-income workers not receiving welfare—those in the segment DD' on the budget constraint—static labour supply theory unambiguously predicts a reduction in labour supply because both the income and substitution effects imply reduced hours of work. Individuals in this group—the "working poor"—experience an increase in income and work fewer hours. Some of those in segment D'F—i.e., those working substantial hours such as multiple-job holders—may also reduce work activity and accept less income, albeit a smaller income reduction than would have occurred without the NIT. However, for welfare recipients—those at point C—labour supply is predicted to increase because the NIT has stronger work incentives than traditional welfare.[4]

A key focus of the NIT literature has been on providing credible estimates of the magnitudes of potential adverse effects on labour supply—i.e., the size of movements such as those depicted by arrows 2 and 3 in Figure 1. Indeed, other than impacts on marital status, which also received considerable attention, labour supply has been one of the few outcomes extensively studied.[5] Recent reviews of these relatively dated North American NIT experiments have now become commonplace in light of the basic income interest.[6] Many policy pieces and media articles from other countries that have either conducted or proposed basic income—including Finland, Netherlands, Spain, South Africa, and Kenya—have noted the high-level results from the North American NIT experiments.

Evidence on labour supply behaviour is, of course, available from many other sources—see, e.g., the extensive survey by Blundell and MaCurdy (1999) and the more recent review by Hoynes and Rothstein (2019). The strong renewal of interest in the results of the NIT experiments may reflect two key factors. One is the use of random assignment to treatment and control groups in a controlled setting—the "gold standard" for credible evidence. In addition, the NIT experiments specifically focus on work behaviour of the low-income population. Many other labour supply studies—e.g., on lottery winners (Cesarini et al., 2017) or New York City taxi

[4] Improved work incentives were central to early advocates of a NIT such as Milton Friedman (1962), who proposed replacing traditional welfare and other income support programs with a NIT.

[5] Other outcomes examined or proposed in the current wave of basic income pilots include happiness and well-being.

[6] Examples include Hoynes and Rothstein (2019), Widerquist (2005), Van Parijs and Vanderborght (2017), and Stanford Basic Income Lab (2018).

drivers (Ashenfelter et al., 2010)—examine demographic groups that are less likely to be affected by a BI policy.

In this study we focus on labour supply, although we also present results on other outcomes. As summarized in surveys such as Robins (1985) and Burtless (1986), the four U.S. NIT experiments generally found that the treatment group experienced a small but non-trivial reduction in labour supply with typically small negative impacts for men in two-parent families and larger effects for women in two-headed households than their male counterparts.[7] Single female household heads—for whom results are available only for the Gary and Seattle-Denver experiments[8]—were estimated to reduce hours worked by similar amounts (modestly lower in percentage terms) than women in two-parent families. For example, across the four experiments, average estimates of the reduction in annual hours worked range from 89 hours, a decline of 5%, to 119 hours or 7% for married men; 93 hours (17%) to 117 hours (21%) for married women; and 123 hours (13%) to 133 hours (17%) for single female household heads (Robins, 1985; Burtless, 1986).[9] Estimates of the decline in the probability of employment were 3 percentage points (a decline of 3.5%) for men in two-parent households, 6 percentage points (22.5%) for women in two-headed households, and 7 percentage points (16%) for single parents (Robins, 1985). These estimates are averages of the experimental treatment-control differences and do not include the large number of structural model estimates.

Underlying these averages across the four U.S. experiments is noteworthy heterogeneity in both the magnitudes of the estimated labour supply impacts and the precision with which they are estimated. Seattle-Denver stands out from the other experiments on both dimensions, with generally larger negative effects on labour supply—generally attributed to SIME-DIME's more generous (higher *G*, lower *t*) treatment plans—that are also typically highly statistically significant, reflecting much larger sample sizes. For men in two-parent families, the estimated impact on hours of work is −133 hours or 5% (Robins, 1985) and −113 hours or 7% (Burtless, 1986) and statistically significant at the 1% level, whereas for the other three experiments, the estimated effect is lower (−1% to −4%) and statistically insignificant. The estimated employment rate impact is −4 percentage points in SIME-DIME (significant at 1%) versus 0 to −1 percentage points and insignificant in the other studies. Similar differences in estimated magnitudes and statistical significance between SIME-DIME and the other three studies are evident for women in two-headed households and single parents.

The range of estimates reflects in part the different generosity levels of the programs, with all experiments offering differing sets of values of the guarantee level *G* and the implicit tax rate *t*, and Seattle-Denver—the U.S. experiment with by far the largest sample size—offering

[7] In discussing labour supply impacts, it is important to distinguish between absolute effects (e.g., on annual hours) and percentage changes (relative to pre-treatment annual hours). Estimated impacts on annual hours are similar for both men and women in two-parent families but much smaller in percentage terms for men who typically worked substantially more hours.

[8] The New Jersey NIT experiment enrolled only two-headed families. The number of single parents in the rural experiment (RIME) was too small for statistical analysis.

[9] Both Burtless (1986) and Robins (1985) report larger reductions in annual hours for single parents than for married women but the percentage changes are similar because single parents worked more hours per year.

more generous plans than the other U.S. NIT experiments. In addition, the range of estimates reflects other factors, such as the substantial differences in the target demographic groups selected for each study. For example, New Jersey enrolled Black, White, and Hispanic two-headed families that were resident in declining urban areas, while the RIME sample consisted principally of two-headed households but also included a few single-parent families. The Gary experiment consisted only of Black families, with 60% of the sample being single parents, while SIME-DIME included large numbers of White and Hispanic families as well as Black. Both the Gary and SIME-DIME samples included sufficiently large numbers of single-parent families to produce estimated impacts.

All four U.S. experiments had a substantial budget for research and analysis and produced detailed final reports. In addition, the data were generally accessible to other researchers and many subsequent studies were carried out. Widerquist (2005) cited more than 200 scholarly studies published in books and academic journals and noted that "there are at least 200 more unpublished memorandums, reports, discussion papers and other unpublished works on the experiments as well" (Widerquist, 2005, p. 79).

In contrast, MINCOME, which was funded by the Canadian federal and provincial governments, had a budget that was fixed in nominal dollars and was not adjusted during a period of high inflation.[10] In order to maintain high standards, those responsible for the experiment chose not to "cut corners" on operations and data collection. As a consequence, when the budget was exhausted, the project was shut down without any funding for research and analysis. No final report was produced. After the 1974 to 1978 operational phase ended, MINCOME staff completed technical documentation and coded some survey information electronically. The hard-copy records (completed surveys and payments and taxation records) and computer tapes were turned over to the federal government. In 1981 the federal government provided limited funding to create the Institute for Social and Economic Research (ISER) at the University of Manitoba with the purpose of restoring the MINCOME data and promoting its use to the research and policy analysis community. Important parts of the data, however, remain available only in hard-copy form at the Library and Archives Canada.

By 1983 the data that had been digitized, together with detailed codebooks, was available to researchers. Some research and analysis were subsequently carried out, but this was limited by the fact that interest in the guaranteed income policy had waned. As a consequence, MINCOME remains substantially under-researched relative to the four U.S. NIT experiments. Indeed, only one published study of labour supply effects using the MINCOME survey data—that of Hum and Simpson (1991)—has been carried out.[11] We have been unable to replicate the results (or even the reported sample sizes) of this study. Neither the data used by the authors nor their computer code is available for analysis. As we discuss in the appendix,

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[10] For a more detailed account of the MINCOME experience, see Simpson et al. (2017).

[11] Results from this study were also reported in Hum and Simpson (1993) that surveyed evidence from the U.S. and Canadian income maintenance experiments. Calnitsky and Latner (2017) carried out a non-experimental analysis of the extensive labour supply margin using administrative data from the Dauphin site, which (as discussed below) was not randomly assigned.

there are several reasons to be skeptical about Hum and Simpson's reported findings. Furthermore, as discussed subsequently, Mincome's sample was representative of the low income population of Manitoba – in contrast to the U.S. NITs that targeted specific demographic groups – including single men and women. In their analysis, Hum and Simpson (1991) pooled single men and married men and single females with single mothers, in both cases two groups with different labour supply behavior. Thus only their results for women and two-headed households correspond to demographic groups in other NIT experiments, to which their results are often compared.

In this paper, we revisit the MINCOME experiment using the available survey and previously untapped administrative data. Our assessment reveals substantially different findings than what is currently understood internationally on the basis of the Hum and Simpson (1991, 1993) studies. Indeed, the only demographic group for which we find similar results to the original Hum and Simpson (1991) study is men from two-headed households, where our results show essentially no statistically significant treatment effects on either the intensive or extensive dimension of labour supply. For other groups our results are as follows. First, we find large positive effects along both intensive and extensive margins for single parents (90% single mothers). To our knowledge, these are the first positive labour supply effects that have been estimated in the NIT literature.[12] Second, we find large negative effects on hours worked for women in two-headed households—larger effects relative to baseline hours worked than the U.S. experiments and much larger than the Hum and Simpson (1991) study, which estimated small negative but statistically insignificant impacts for this group. Finally, we examine single men and women—a quantitatively large fraction of the MINCOME sample—but conclude that this group appears to not have been randomly assigned.

Background on MINCOME[13]

MINCOME was a joint federal-provincial initiative carried out in Manitoba in 1974–1977. There were three experimental sites: Winnipeg, rural dispersed sites, and the town of Dauphin. The research design for Winnipeg and the rural sites was random assignment to treatment (*T*) and control (*C*) groups. A unique feature of MINCOME relative to the four U.S. NIT experiments was the inclusion of a "saturation site"—Dauphin, Manitoba—in which all resident families could enrol in the guaranteed annual income study if they met the eligibility criteria (i.e., there was no control group in Dauphin).

As was common in the U.S. NIT experiments, numerous treatments that combined different guarantee levels (*G*) and implicit tax rates (*t*) were offered in an attempt to facilitate estimates of the responsiveness of families to NIT plans with different incentives. There were

[12] In addition to experimental and structural estimation, the SIME-DIME final report carried out simulations designed to predict the labour supply impacts of introducing a NIT program nationwide. Some simulations predicted positive impacts on work activity for single parents (SRI International, 1983, Table 3.31).

[13] This section provides a brief overview of MINCOME. More detail is available in the various technical reports and studies referred to in Simpson et al. (2017).

three guarantee levels ($3,800, $4,600, and $5,400 for a family with two adults and two children younger than 15 years—equivalent to $18,900, $22,800, and $26,800 in 2016 dollars) and three implicit tax rates (35%, 50%, and 75%). These combined to yield nine potential NIT plans, of which the most expensive plan with the lowest tax rate and highest guarantee was dropped. Among the remaining eight plans, the one with the lowest guarantee ($3,800) and highest tax rate (75%) experienced very low take-up, so participants allocated to that plan were combined with those allocated to the $4,600, 75% plan. After these adjustments, there were seven distinct treatment plans plus the control group. While all seven plans were assigned to participants in Winnipeg, the rural and Dauphin sites employed a single treatment plan—G = $3,800 and t = 50%.

Several distinct family types were enrolled: two-headed families, single parents, and singles. Enrolment of single men and women—at about 20%, a substantial fraction of the MINCOME sample—is a departure from the U.S. NIT experiments. For some purposes, two-headed families were further separated into those with two earners and those with a single earner.

An important MINCOME feature—one also used in the four U.S. experiments—was the Conlisk-Watts assignment model for allocating families to treatment plans. This model was designed to optimize the allocation of families with different pre-treatment income levels to the seven treatment plans, taking account of the overall budget for the experiment. Pure random assignment of families to alternative treatment plans would result in some low-income families being offered very generous (high G, low t) treatment plans—resulting in very expensive observations. Essentially, this assignment model reduced the likelihood that low-income families (and raised the likelihood that families with high pre-treatment income) were enrolled in generous treatment plans relative to what would occur under pure random assignment. Specifically, families were divided into several "normal income" groups and assigned to different treatment plans based on their family type and normal income. Normal (or permanent) income was estimated from pre-treatment surveys discussed below.

An important consequence of the Conlisk-Watts assignment model is that for the sample as a whole, there is non-random assignment to treatment and control groups. Rather, random assignment took place within combinations of family type and normal income. In order to obtain unbiased estimates of treatment effects, it is therefore necessary to control for family type and normal income (Keeley, 1981; Keeley & Robins, 1980). In our analysis, we therefore report results separately by family type and control for normal income categories associated with each family type. Another consequence of the assignment model is that within family types, the number of observations allocated to each of the seven treatment plans differs, depending on which treatment plans are more informative for estimating responses to the NIT treatment. Similarly, unlike many experiments, families are not allocated evenly to treatment and control groups—the control group is typically much smaller than the treatment group.

Two additional implications of the research design and assignment model are worth noting. First, sample sizes for each individual "experiment" are very small. The combination of seven different treatment plans for a sample that is stratified by site (Winnipeg, rural), family type (two-headed families, single parents, singles), and normal income (four or five levels) results in a large number of individual randomized trials. With the available sample size, some form of aggregation is required to obtain meaningful estimates.[14] Second, because families are assigned to treatment plans based on their pre-treatment normal income, non-random attrition is likely to occur because attrition is more common from less generous combinations of G and t. As noted below, there was substantial attrition in MINCOME, as in the U.S. NIT studies (see, e.g., Ashenfelter & Plant, 1990).

Several pre-treatment surveys were carried out, followed after random assignment by nine periodic surveys. The initial "screener" survey (not publicly available) in late 1973 (number of completed interviews n = 21,700) collected a limited amount of income and family composition information designed to establish an eligible population.[15] Of these, 6,370 (about 30%) were deemed potentially eligible and approached for the baseline survey. Baseline data on 3,800 families (60% of those contacted) were obtained in the latter part of 1974. This detailed pre-treatment information was used to select 2,400 eligible households who were contacted to enrol in the experiment. The baseline survey was followed by an enrolment survey generally carried out at the time of random assignment. Approximately 80% (1,865 families) completed the enrolment survey. Subsequently, 1,255 were randomly assigned—1,074 in the Winnipeg site[16] (704 treatments and 370 controls) and 181 in the rural dispersed sites (103 treatments and 78 controls).[17] Following random assignment, nine periodic surveys were carried out every 4 months on average over the period 1974–77. We note that the specific chronology (i.e., calendar date) of these surveys varied substantially.[18]

The post–random assignment periodic surveys are publicly available only for Winnipeg and restrict the sample to intact households—that is, households with no change in marital status during the experiment. A wide variety of additional raw data (i.e., original hard copy survey reports) are available at Library and Archives Canada.

In addition to information from the baseline, enrolment, and post–random assignment periodic surveys, administrative data from the payments system are also available. A separate agency, MINCOME Manitoba, was established to operate the payments system. Treatment group participants were required to submit monthly "income reporting forms" (IRFs) and received monthly payments (depending on their earnings, guarantee level, and tax rate) from MINCOME Manitoba.[19] Staff from the payments group were available in person to assist

[14] Most studies estimate an average response across all treatment plans, separately by family type.

[15] Reported sample sizes are approximate and not always consistent in Mincome documentation. Unless otherwise noted, data on sample sizes in the text are from Mason (2016).

[16] Administrative data on enrollees are also available and discussed subsequently. Based on the administrative data in the payments file, the number of Winnipeg households ultimately enrolled was actually 907. The number of observations for the rurally dispersed sites in published sources also differs from the number of observations based on the payments file. It appears that 274 rural units were enrolled.

[17] In Dauphin, 586 families were stated to be enrolled in the saturation site study.

[18] Among other things, some units did two "baseline" surveys (i.e., had the baseline survey split into two sessions that were potentially months apart).

[19] We note, however, that some treatment group individuals or households who responded to the longitudinal labour surveys never filed an IRF. This is peculiar as (a) you were not supposed to be eligible to even participate in the

participants completing this form. MINCOME Manitoba also filed annual income tax returns for participants and, after reconciliation, handled adjustments for under- or overpayments. There were two types of control group participants: those who also agreed to fill out these IRF forms (known as IRF controls) as well as the surveys discussed above, and a second subgroup (known as PC controls) who were asked only to complete the surveys.

Because the digitized information from the periodic surveys is limited to intact households, a selected (and potentially non-random) subset of observations, the administrative payments data—which in principle constitutes the complete experimental sample—is a potentially important source of information to complement that from the surveys.[20] To our knowledge, other than the recent work by Calnitsky and Latner (2017), which focused on Dauphin, a small subset of the overall data, and used a difference-in-differences design to deal with non-random assignment in Dauphin, the data from the payments system has not been analyzed. The only way currently to examine the rural and Dauphin sites and households excluded from the publicly available surveys (non-intact families) is through the payments file.

The administrative and survey-based samples differ in two additional respects. As noted, some treatment group families responded to the periodic surveys but do not appear in the administrative data because they never filed an IRF. In addition, "IRF controls" were excluded from the surveys.

Because of the extent of non-participation and attrition during the first year of operation—especially in the lower-income cells for two-headed families in Winnipeg—a decision was made to add a supplementary sample. This additional sample of 196 treatment families and 97 controls lagged the original sample by about 1 year. Given the number of different treatment plans and the relatively small sample sizes, this supplementary sample provides potentially valuable data. However, to our knowledge, these data have not yet been analyzed—nor are they available in digital form.

Two design features distinguish MINCOME from the U.S. experiments. First—a feature that has been widely discussed—is the inclusion of a "saturation site," the town of Dauphin, Manitoba—in which all low-income families were eligible for the NIT earnings supplement. We do not analyze Dauphin in this paper, in part because of the absence of random assignment and also because the Dauphin survey data have not yet been digitized and made available to researchers. The second design feature—one that does not appear to be well known—is that the MINCOME sample is broadly representative of the low-income population of the province. One consequence is that MINCOME data provide the opportunity to examine impacts on demographic groups that were not included in the U.S. NIT samples—such as single adults (who constitute a significant fraction of the low-income population). An additional consequence is that the MINCOME results may have greater external validity than the U.S. experiments due

experiment (aside from take-up) without filing the IRF, and (b) you could not receive (even potentially) payment without filing an IRF. There does appear to be a legitimate question as to whether individuals assigned to the treatment group but who never filed an IRF realized they were in the treatment group.

[20] There was also substantial attrition from the periodic surveys, particularly for two-headed households (very little attrition for single parents). The payments file is thus also potentially very informative about the effects of attrition.

to the latter's target populations being more narrowly focused. Unfortunately, as we discuss later, we have thus far not been able to analyze single adults in this study because they do not appear to have been randomly assigned.[21]

We are, however, able to analyze labour supply responses of single parents, many of whom were on social assistance prior to random assignment. As noted previously, a key part of the original motivation for a NIT was to replace traditional welfare and related income support programs with an approach with stronger work incentives (e.g., Moffitt, 2003).

Data and Summary Statistics

We employ the various sources of information currently digitized for MINCOME: the baseline and enrolment surveys conducted prior to random assignment, the post–random assignment periodic surveys, and the monthly administrative data collected separately as part of the payments system. The baseline survey (referred to as MINC1) collected information on a limited set of individual and household characteristics, including labour market information, income, and receipt of government transfers for the 1973 and 1974 years. Most of this information is annual, such as weeks worked, income sources, and receipt of government transfers during those years. Only limited information from the enrolment surveys (that were conducted around the time of random assignment) has been digitized; we do not currently use these surveys other than for documenting hours worked prior to random assignment. The post–random assignment "longitudinal labour surveys" (known as MINC4) were collected approximately every 4 months for 3 years, resulting in at most nine post–random assignment observations for each participating family. We note that there was in fact considerable variation in the calendar date when both random assignment and these various surveys occurred. It is also the case that not all post–random assignment surveys were done 4 months apart. We do not currently account for these calendar time differences.

Our analysis is based on families that appear in both the baseline survey (MINC1) and the periodic post-randomization surveys (MINC4). This results in a sample size of 910 consisting of 506 two-headed households, 158 single parents, and 246 singles. Table 1 reports baseline summary statistics separately by family type for single parents and two-parent families.[22] For both family types, about 65% are assigned to the treatment group. As expected, home ownership is much higher among two-parent families. Weeks worked in the 2 years prior to the beginning of the experiment are similar for both groups, although a bit lower for single parents—in the 62%–64% range versus 64%–69% for two-parent families. Receipt of government benefits, however, is much higher among single parents—approximately 50% report receiving benefits versus one quarter to one third for two-parent families. Although not shown in the table, for single parents a strong majority of this is welfare (as opposed to

[21] It is possible that they were randomly assigned but, if so, the existing documentation does not provide sufficient information on how the Conlisk-Watts assignment model allocated single parents to treatment and control groups.
[22] As discussed later, our balancing tests reject random assignment for single men and women, so we do not report results for this group.

unemployment insurance payments). Interestingly, the majority of families fall in the top two normal income categories in both groups—over 80% of single parents and about two thirds of two-parent families are in the top two categories.[23]

In the surveys, some characteristics were collected at the household level and thus for two-headed households have the same value for men and women. Other characteristics, such as education level, were collected in individual-based questionnaires. Table 2 reports individual-level summary statistics for single parents and separately for men and women in two-headed households. Educational attainment, especially high school completion, is much lower among single parents. Note the large differences in annual hours worked prior to random assignment across the three groups, with men in two-parent families working substantially more hours than single parents and women in two-parent families working substantially fewer hours than single parents, differences that continue to be evident in the post-baseline surveys. Similar large differences across these three groups in work activity is also evident in weeks worked in 1974. Inspection of the sample sizes in the post-randomization surveys indicates that the attrition rate was around 30%, with most dropping out occurring early in the surveys. We explore attrition bias below.

Due to the declining real value of funding for the MINCOME experiment, an important sample selection restriction was made for the data based on the longitudinal labour surveys. In particular, these data were digitized for the Winnipeg sample only, and (perhaps more importantly) only for intact households—that is, households with no change in marital status over the three-year experiment.[24]All individuals who originally indicated they would participate responded to the baseline survey, and thus we can do a simple test of sample selection bias by examining characteristics of the individuals from Winnipeg who responded to the baseline survey but never appear in the longitudinal labour surveys (this is most likely the non-intact households).[25] As we show in Table 3, there were few such individuals from the single-parent group and their characteristics appear quite similar to those included in the surveys. In contrast, about one quarter of two-headed households appear to be excluded by this restriction. Comparing Tables 1 and 3 indicates that these excluded families differ in some dimensions from

[23] Normal income cells varied in number and corresponding nominal amounts across household type so are not comparable across family types.

[24] Mincome documentation states that 1,074 families were enrolled in Winnipeg. Our smaller sample size (910) presumably results from omission of non-intact families. Thus, there is no differential dropping out for males vs. females from the two-head sample.

[25] The administrative data from the payments system has no such restriction on intact households, and so we could also use these data to examine the potential implications of this sample selection in the longitudinal labour surveys. The payments file should, in theory, consist of the entire treatment group, both all sites (i.e., Winnipeg, rural dispersed sites, and Dauphin saturation site) and all families (i.e., not just intact households), as well as a subset of the control group. While the payments data allows for useful robustness checks, it does have the disadvantage that only wages are observed (not hours worked); moreover, it is household labour earnings and thus for two-headed households there is no individual measure of work.

included families—they are much less likely to be homeowners and less likely to have received government benefits in 1973 and 1974.

Tables 4 and 5 report the results of balancing tests for all household types in MINCOME. These regressions include controls for normal income cells and demonstrate that random assignment held for two household types: single parents and two-head households. For these groups the baseline characteristics of treatments and controls do not statistically differ from each other. However, there were a variety of differences in the (baseline) characteristics between treatments and controls for singles without (dependent) children, including very large differences in work behaviour in 1973 (see Table 5). We believe it is likely that the reason for random assignment failing for singles without dependent children is due to the different "normal income" approach to randomization for this group. In particular, it is possible that singles were randomly assigned conditional on normal income categories, but we are unable to determine from the MINCOME documentation what those categories were.

Table 6 provides evidence relating to possible attrition bias. We present tests of both differences in attrition rates across baseline characteristics, including random assignment as well as whether attritors (controlling for covariates) have different pre–random assignment labour supply. Similar to above, we find no evidence of attrition bias for single parents. Dropout rates are the same across all groups; of course, sample sizes are small, but still, differences in means are minor. Attritors have fewer hours worked pre–random assignment and the coefficient in absolute value is large, but variance is high, and the coefficient is not statistically significant. There is some evidence of attrition bias for two-head households, although it is unclear how strong this evidence is; of particular concern perhaps is that households with positive weeks worked in 1973 were more likely to drop out of the longitudinal surveys. On the other hand, there was no relationship between work in 1974—the year before random assignment (which generally took place at the end of 1974 or early 1975)—and the likelihood of dropping out of the longitudinal surveys.

Empirical Results on Labour Supply

This section reports experimental estimates (mean treatment-control differences) by family type using the nine post–random assignment surveys. Because not all participants responded to each survey, we also report estimates for the balanced panel of respondents to all surveys. When possible, we also provide evidence from the administrative data. All regressions control for normal income categories (in order to obtain unbiased estimates) and survey fixed effects. Because of sample size limitations, a single treatment dummy is used, as is common in earlier NIT studies. The estimates can thus be interpreted as the average impact across the seven treatment plans (combinations of G and t).

Tables 7 and 8 present estimated impacts on labour supply—both the extensive margin (employment) and intensive margin (hours worked)—for single parents and two-head households with Table 7 consisting of the full sample, and Table 8 the balanced sample. We find no statistically significant effects on either employment or hours of work for men from two-headed households, with coefficient estimates that are small and negative in both cases. This is the same result as found previously by Hum and Simpson (1991) and consistent with the New Jersey, RIME, and Gary U.S. experiments that found a negative coefficient for men that was typically small in size and not statistically different from zero. The outlier in the case of men in two-headed families is SIME-DIME, which found moderately large and statistically significant reductions in hours worked, a result that is generally attributed to SIME-DIME offering more generous plans than MINCOME and the other U.S. NIT studies.

For women from two-headed households, we find a statistically significant negative labour response for hours worked, but only weak evidence of a decline in the probability of working. In contrast, Hum and Simpson (1991) found no statistically significant effect on hours for this group. Moreover, our estimated reduction in hours for these women is large in magnitude, in fact larger in percentage terms than the U.S. evidence. The treatment effect for women is a 38.6-hour reduction based on a 4-month window, or 116 hours annually. The magnitude of this impact is similar in size to the average estimated effect in the four U.S. NIT experiments—93 hours (Burtless, 1986) or 117 hours (Robins, 1985).[26] Relative to mean hours worked during the calendar year 1974, the decrease in labour supply represents about a 40% reduction in hours worked, much greater than the roughly 20% average effect in the 4 U.S. NITs (and in SIME-DIME). The large percentage decline reflects the fact that about 40% of these women worked 0 hours in 1974, resulting in low average annual hours for this group (about 280 hours—see Table 2).

For single parents, however, the treatment effect is positive along both dimensions. Specifically, we find that single parents randomly assigned to the treatment group were about 14 percentage points more likely to work relative to the control group, a large effect in magnitude, and hours worked were about 72 hours higher based on the 4-month window measured in the surveys, or roughly 210 hours annually. Relative to mean baseline hours worked, the treatment effects on hours translates into about a 34% increase. The estimated impacts on annual hours and employment are even greater in the balanced sample.

Turning to the results using the monthly administrative data on wages from the payments file (see Table 9), we see that the findings outlined above appear robust. The payments file not only consists of administrative data as opposed to survey-based data, but also differs from the survey-based sample in several respects. First, non-intact households are included in the administrative records. Second, some households that never filed an IRF are included in the longitudinal labour survey results discussed above but would be excluded from the administrative file. Third, the control group is different as additional control group households were included in the payments file ("IRF controls") who were excluded from the surveys. Random assignment for the two household types was conducted correctly for both, but we do have different control households. Finally, attrition rates were much lower in the payments data than the 30%+ in the surveys, although unlike the surveys, there were

[26] It is also similar to SIME-DIME, where annual hours were estimated to fall by 101 (Burtless, 1986) to 141 (Robins, 1985).

discontinuous spells—i.e., individuals not filing an IRF in a given month and thus no wage observations, but then subsequently filing (as is common in administrative records). Overall, our evidence is that treatment effects for the probability of employment are very similar to the survey-based evidence. The estimated impact on the employment rate for single parents is almost identical to that in Table 7, though less precisely estimated, and the impact for two-headed families lies between the estimated effects for men and women in these families in Table 7.

Social Assistance in Manitoba during MINCOME

To provide some context for the labour supply results—especially those related to single mothers with dependent children—this section outlines salient features of social assistance in Manitoba during the 1970s. Historically, social assistance in Canada was provided at the local level, relying mainly on private and church charity. Provinces first entered this field toward the end of World War I with mothers' allowances programs for needy mothers with dependent children whose fathers were killed during the war. These mothers' allowances programs in Canada and the U.S. were the forerunners of government welfare/social assistance programs in North America.

During the 1970s, social assistance in Manitoba remained an area of shared responsibility, with municipalities providing support to "employables" whose social assistance spells were expected to be brief and the provincial government supporting longer-term cases—those unable to or not expected to work. This hybrid model also operated in several other provinces. Employables included the employable unemployed, short-term handicapped (< 90 days), transients, and mothers with dependent children deserted by the husband within 90 days or in prison with a sentence less than 90 days. Eligibility and benefit rates varied substantially across municipalities (Barber, 1972). Longer-term cases that came under the provincial social assistance legislation included single mothers with dependent children (after a waiting period), the elderly, the disabled, and the blind.[27]

When mothers' allowances were first introduced during World War I, single mothers were not expected to work—indeed, were discouraged from working outside the home, although they could engage in home-based work such as taking in boarders, knitting, and sewing. The philosophy at that time appeared to be based on a contract—the state provided some income support and in exchange the mother's job was to raise responsible future citizens (Fields, 2002). However, by the 1960s and 1970s, attitudes had changed substantially, in part reflecting rising female participation in the labour force. The composition of sole-support mothers had also shifted—with separated/divorced and never married representing the largest groups and widowed and abandoned being a much smaller fraction of the total. In Manitoba, the rising number of mothers' allowances cases was also a growing policy concern—in its annual report

[27] Eligibility under the provincial Social Allowances Act was more restrictive prior to the 1970s. For example, an unmarried mother with one child was not eligible for assistance until 1970 and mothers caring for two or more children in their home became eligible in 1967 (Bedard, 1994).

for 1972, the Department of Health and Social Development noted that mothers' allowances cases grew from under 23% of the provincial social assistance caseload in 1970 to 28% in 1971.

In his report to the provincial government on welfare policy in Manitoba, Barber (1972) also noted that single mothers constituted a significant part of overall social assistance costs and one that was growing as a proportion of total expenditures. He regarded sole-support mothers as the most employable group receiving provincial income support, followed by those with a disability. However, he noted that the existing system provided almost no incentive to earn additional income because—apart from some small exceptions, such as a $20 per month earnings exemption—recipients faced a 100% tax-back rate on market earnings.[28] He also characterized existing day-care services as "completely inadequate"—strictly commercially operated and having too few spaces available to allow a significant number of single mothers to enter the workforce. Although not noted in the Barber report, provincial social assistance also provided health services—dental, drugs, and optical—for recipients and their dependants. These non-cash benefits would cease upon leaving social assistance to enter the workforce, potentially resulting in a tax rate exceeding 100%.[29]

As part of his report, Barber carried out a case review (n = 40) and small survey (n = 45) of single parents on social assistance. Most had very low education and few specialized skills, but over 80% had worked in the past (average years of employment = 7), though not recently (average years since last employed = 9). The survey also provided some limited evidence on willingness to work. Over two thirds rated their chances of becoming independent within 5 years as fair (34%) or good (34%), although very few rated their chances of leaving welfare within 1 year as better than poor. Barber interpreted this evidence as indicating that a significant number (but far from a majority) of single mothers with dependent children would work part-time or full-time if suitable incentives were provided.

Shortly after Barber's December 1972 report, the provincial government began introducing incentives for social assistance recipients to work. In September 1973, a new Work Incentive program was implemented and given more attention in subsequent years (Manitoba Department of Health and Social Development, 1973–1978). Unfortunately, details of this program were not provided in the ministry's annual reports—other than stating that the program "ensures some financial gain to recipients from seeking and obtaining work."[30] The 1974 annual report noted that social assistance caseloads had fallen to their lowest level since 1969, and

[28] There was also a $7.50 per month allowance for clothing and "coffee money." Although details were not provided in his report, some allowance for transportation costs and child-care expenses was mentioned. At that time, the average monthly allowance for single mothers was $207, so the earnings exemptions constituted about 10% of monthly income.

[29] According to statistics in the annual reports of the Department of Health and Social Development, these health benefits cost 7% to 8% of social assistance benefits over the 1972 to 1978 period.

[30] Data provided in the department's annual reports from 1975 to 1978 indicate that the Work Incentive program was short-term in nature and thus not a continuing incentive such as an earnings disregard. Each year the new case intake and case outflow exceed, often substantially, the stock of cases at the beginning and end of the year. This suggests a program such as job search assistance or mentoring of job search and job retention.

credited the Work Incentive program with contributing to this decline. Mothers' allowances and long-term disability cases represented the two largest groups in the Work Incentive program caseload. Additional work-related programs were introduced in 1975—the Employment Services program designed to help those having difficulty finding and retaining work and Work Activity Projects for those with an unusually high degree of difficulty finding and retaining employment. The annual reports suggest that the Work Incentive, Employment Services, and Work Activity Projects were available to those receiving and not receiving social assistance. The Work Incentive and related work-related programs were cost-shared with the federal government under the Canada Assistance Plan.

Perhaps the most significant policy development affecting single mothers during the MINCOME experiment was the introduction of the child-care program in September 1974, a program that was enhanced substantially in subsequent years. Under this program—also cost-shared with the federal government under the Canada Assistance Plan—the provincial government provided advisory services to both group day-care and family day-care providers. Maximum fee levels for full-day and half-day care were established for day-care providers to be eligible under the program. Subsidies to parents were based on an income test and "social need," with single parents rated the highest priority and two-parent families with both parents working the next highest priority. In addition to working, attending an educational program constituted another basis for "need."

Child-care subsidies under this program were highly progressive. At family income levels approximating those paid to social assistance recipients, child care was fully subsidized. As family income rose, subsidies were reduced in a manner similar to a NIT with a 50% tax-back rate. The break-even point was reached when family income was approximately equal to the average income in Manitoba for that family type. In November 1975, subsidies to families using day care were made more progressive by changing tax-back rates to 0%, 0.25%, and 0.75%.

The annual reports of the Department of Health and Social Development document the dramatic growth in availability and use of day care under the child-care program:

Child-Care Program Statistics

	Nov 74	Nov 75	Nov 76	Nov 77	Nov 78
Group day care:					
Participating centres	11	55	165	172	172
Licensed spaces	374	2,637	4,598	4,795	4,814
Family day care:					
Participating care homes	3	73	206	201	179
Licensed spaces	14	136	594	609	575

In interpreting this strong growth, it is important to keep in mind that subsidized child care was available to all low-income families, not just those in the MINCOME experiment, and within MINCOME was available to families in both the treatment and control groups. Thus, a reasonable interpretation of our labour supply impacts for single mothers with dependent children is that a NIT program can encourage single mothers to leave welfare and enter the workforce in an environment in which the availability and cost of child care is not a serious obstacle. Because subsidized child care was available to single parents in both the treatment and control groups, our estimated treatment effects remain unbiased experimental estimates of the impact of the NIT on labour supply. Although the internal validity of our estimates is not compromised by the introduction and expansion of the child-care program, the external validity may be affected. In other words, our experimental estimates may not generalize to an environment in which subsidized child care is not widely available.

Similarly, because the Work Incentive program appears to have been available to both the treatment and control groups, its introduction during MINCOME should not affect the validity of our experimental estimates.

Additional Impacts on Families

Although a key focus of MINCOME—and the four U.S. NIT experiments—was on work activity impacts, the post-randomization surveys included a few questions on other outcomes that are potentially related to labour supply effects. In Table 10 we report results for men and women in two-headed families on family time allocation and marital satisfaction using responses to questions asked about 20 months after the experiment began. There is no clear evidence that being offered the NIT earnings supplement resulted in spouses being more likely to agree on time spent together, but wives in the treatment group were 8.7 percentage points more likely than controls to report that there was agreement on who did the housework and 11 percentage points more likely to report satisfaction with the husband being helpful. Each of these estimated impacts represent an improvement of almost 20% relative to control group wives. Both partners, especially husbands, reported large increases in marital satisfaction—9.5 and 15 percentage points respectively. For husbands, this represents an almost 30% improvement relative to control group husbands, and for wives (who report high levels of marital satisfaction in the absence of the intervention), a 12% increase.

Questions on life satisfaction were included in the seventh and ninth surveys approximately 20 and 36 months after random assignment. Results for single parents (sample size 227) show no evidence of greater happiness among those in the treatment group (Table 11). However, both wives and husbands in two-parent households reported increases in happiness—of 5 and 8.5 percentage points respectively, improvements relative to controls of 6% and 10% respectively. The responses to the questions on marital satisfaction and overall levels of happiness—together with the more pronounced impacts on men than women in two-headed households—are clearly consistent and could be related to the impacts on time spent working.

Conclusions

We investigate the labour supply effects of the Canadian NIT experiment known as MINCOME. The North American NIT experiments—conducted in the late 1960s and 1970s—have received considerable attention in recent years given the renewed interest in a universal basic income. However, while the U.S. experiments have received huge attention from scholars, including more than a hundred academic papers, the Canadian (randomized) experiment literature consists of a single study. We find a variety of problems with this previous analysis and question the validity of its conclusions. Other than for men from two-headed households, our reassessment of MINCOME's impacts on labour supply reveals very different results. First, we find very sizeable adverse effects on labour supply for women from two-headed households but no compelling story of a movement out of the labour force. Our point estimates of the reduction in hours worked are similar to those estimated in the U.S. NIT experiments, particularly the Seattle-Denver experiment, and—like SIME-DIME but in contrast to the other three U.S. experiments—are highly statistically significant. Relative to baseline hours worked by this group, however, our estimates are much larger than—approximately double—those found in the U.S. studies, including SIME-DIME.

Conversely, we find similarly large in size, but positive, treatment effects on hours worked and on the likelihood of working for single parents (90% single mothers). The latter result, which is robust to two very different data sources and differences in sample construction, is the opposite of the previous U.S. evidence. However, what has not been documented in the previous literature, including surveys and policy and synthesis papers, is that the single-parent Canadian sample had very high welfare use (over 70% of the sample received welfare payments in at least 1 of 2 years pre–random assignment). In the U.S., only the Gary and Seattle-Denver experiments enrolled sufficient numbers of single parents to permit statistical analysis. Single parents made up more than half of the Gary sample and estimated labour supply impacts were negative, small, and statistically insignificant. In Seattle-Denver, large negative and statistically significant impacts on hours worked and the employment rate were estimated for single parents. The striking differences in impact estimates for single parents between MINCOME and the Gary and Seattle-Denver NIT experiments clearly warrant further analysis.

There is also evidence of an impact of the NIT on marital satisfaction and overall levels of happiness among both partners in two-headed households, with these positive benefits being greater for men. Among single parents there is no similar impact on happiness.

Overall, our results suggest that for the Canadian experiment, the randomized guaranteed income benefit reduced hours for women from two-headed households (who were on average not working full-time prior to random assignment) and increased both movement into the labour force as well as hours worked for single parents. These impacts are consistent with predictions of standard labour supply theory and could be expected to have offsetting effects on both work incentives and the cost of introducing a basic income.

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Table 1

Household Summary Statistics: Baseline Survey Variables

	Single parents	**Two heads**
Treatment group	.652 (.038) [158]	.642 (.021) [506]
Homeowner	.241 (.034) [158]	.445 (.022) [506]
Worked positive weeks 1973	.636 (.038) [157]	.688 (.021) [501]
Worked positive weeks 1974	.624 (.039) [157]	.639 (.021) [505]
Received govt benefits 1973	.513 (.040) [158]	.336 (.021) [506]
Received govt benefits 1974	.489 (.040) [158]	.223 (.019) [506]
Normal income category 1	.013 (.009) [152]	.057 (.011) [489]
Normal income category 2	.145 (.029) [152]	.088 (.013) [489]
Normal income category 3	.388 (.040) [152]	.196 (.018) [489]
Normal income category 4	.453 (.040) [152]	.305 (.021) [489]
Normal income category 5	—	.354 (.021) [489]
Attritor	.322 (.037) [158]	.320 (.021) [506]

Note. Standard errors are in parentheses, sample sizes are in square brackets. Author calculations from the baseline survey (MINC1) for individuals appearing in both the baseline survey (MINC1) and the longitudinal survey (MINC4). Normal income cells for single parents are not equivalent to those for the two-head sample.

Table 2

Individual Summary Statistics: Baseline and Longitudinal Survey Variables

	Single parents	Two heads: Men	Two heads: Women
High school graduate	.252 (.035) [151]	.338 (.021) [479]	.343 (.021) [487]
Years of schooling	9.66 (.203) [151]	9.99 (.142) [477]	10.19 (.132) [486]
Hours worked in 1974	622.32 (47.19) [142]	951.50 (27.95) [503]	278.65 (19.44) [505]
Worked positive weeks 1974	.623 (.039) [151]	.824 (.017) [506]	.426 (.022) [502]
Hours worked post-baseline survey 1	339.48 (27.76) [130]	476.35 (17.26) [438]	178.96 (12.69) [439]
Hours worked post-baseline survey 2	298.02 (24.49) [125]	431.61 (12.71) [398]	133.96 (11.07) [399]
Hours worked post-baseline survey 3	297.82 (28.07) [118]	512.66 (17.42) [382]	161.91 (13.63) [382]
Hours worked post-baseline survey 4	286.15 (28.40) [114]	431.13 (16.02) [367]	136.81 (12.46) [367]
Hours worked post-baseline survey 5	251.14 (24.67) [113]	434.66 (13.44) [347]	128.06 (10.79) [347]
Hours worked post-baseline survey 6	250.06 (26.09) [112]	434.78 (14.06) [346]	126.45 (11.53) [346]
Hours worked post-baseline survey 7	311.22 (31.66) [109]	547.06 (17.49) [346]	173.86 (14.97) [346]
Hours worked post-baseline survey 8	277.32 (27.64) [108]	466.35 (16.75) [344]	164.83 (13.63) [344]
Hours worked post-baseline survey 9	313.98 (34.22) [107]	612.03 (19.93) [344]	200.75 (16.99) [344]

Note. Standard errors are in parentheses, sample sizes are in square brackets. Author calculations from the longitudinal survey (MINC4) for individuals appearing in both the baseline survey (MINC1) and the longitudinal survey (MINC4). Note that the specific calendar date and interval of time between post-baseline surveys varied across individuals and also within individual (i.e., for the latter, surveys for a given individual were not always 4 months apart).

Table 3

Summary Statistics for Baseline Characteristics: Excluded Households

	Single parents	**Two heads**
Treatment group	.695 (.098)	.598 (.036)
Homeowner	.304 (.098)	.250 (.032)
Worked positive weeks 1973	.681 (.101)	.793 (.030)
Worked positive weeks 1974	.565 (.106)	.679 (.035)
Received govt benefits 1973	.696 (.098)	.500 (.037)
Received govt benefits 1974	.565 (.106)	.283 (.033)
N	23	184

Note. Standard errors are in parentheses. “Excluded households” are households omitted from the digitization of the longitudinal labour surveys (i.e., MINC4).

Table 4

Balancing Tests: Baseline Characteristics

	Single parents						
	Homeowner	High school graduate	Years schooling	Worked 1973	Worked 1974	Govt benefits 1973	Govt benefits 1974
Treatment group	.048 (.080)	−.046 (.085)	−.389 (.457)	.035 (.072)	.044 (.071)	−.001 (.086)	−.060 (.085)
N	140	134	134	140	140	140	140
	Two heads: Men						
Treatment group	−.028 (.046)	.023 (.045)	.164 (.291)	.054 (.043)	.037 (.044)	.007 (.043)	−.061 (.047)
N	489	474	473	481	486	489	489
	Two heads: Women						
Treatment group	−.028 (.046)	−.006 (.047)	.166 (.285)	.054 (.043)	.037 (.044)	.007 (.043)	−.061 (.047)
N	489	473	474	481	486	489	489

Note. Standard errors are in parentheses. All regressions include controls for normal income cells. Author calculations from the baseline survey (MINC1) for individuals appearing in both the baseline survey (MINC1) and the longitudinal survey (MINC4). Note that the results for Two Heads Men and Women are identical for household characteristics.

Table 5

Balancing Tests for Singles with No Dependants

	Home-owner	**High school graduate**	**Years schooling**	**Worked 1973**	**Worked 1974**	**Gov't benefits 1973**	**Gov't benefits 1974**
Treatment group	−.049* (.026)	−.058 (.066)	−.601 (.465)	.185*** (.045)	.077 (.058)	−.010 (.060)	−.099* (.053)
N	246	225	225	246	246	246	246

Note. Standard errors are in parentheses. * $p < .10$. ** $p < .05$. *** $p < .01$.

Table 6

Tests for Attrition Bias

	Single parents		Two heads		
	Attritor	Annual hours worked pre–random assignment	Attritor	Annual hours worked pre–random assignment (men | women)	
Attritor	—	−97.62 (84.91)	—	33.42 (57.61)	8.52 (42.41)
Treatment group	−.074 (.082)	5.00 (38.55)	−.037 (.042)	31.28 (55.76)	−55.32 (41.04)
High school graduate	.138 (.091)	15.73 (44.06)	−.050 (.044)	−52.39 (.57.51)	151.40*** (42.09)
Homeowner	.075 (.091)	−37.08 (44.95)	−.075* (.043)	54.15 (55.46)	−46.09 (40.25)
Worked positive weeks 1973	.125 (.130)	—	.183*** (.063)	—	
Worked positive weeks 1974	.011 (.129)	—	−.013 (.060)	—	
Received govt benefits 1973	.017 (.101)	−3.66 (45.78)	−.026 (.050)	−16.70 (66.60)	73.16 (46.85)
Received govt benefits 1974	.045 (.105)	5.95 (47.30)	.081 (.057)	−457.4*** (75.35)	−110.56** (54.63)

Note. Standard errors are in parentheses. Attritor = 1 for households that dropped out of the longitudinal labour surveys (MINC4) between the first pre–random assignment labour survey (periodic survey 1) and a post–random assignment survey (periodic surveys 3 to 11); attritor = 0 for households that completed all surveys. Annual hours worked are from the first (pre–random assignment periodic) survey. * $p < .10$. ** $p < .05$. *** $p < .01$.

Table 7

Treatment Effects: Full Sample, Longitudinal Survey

	Single parents		**Two heads: Men**		**Two heads: Women**	
	Employed	Hours	Employed	Hours	Employed	Hours
Treatment group	.139** (.068)	72.43* (39.53)	−.030 (.028)	−21.90 (21.32)	−.047 (.041)	−38.58* (20.52)
Normal income fixed effects	Yes		Yes		Yes	
8 survey (time) fixed effects	Yes		Yes		Yes	
Sample size	1,018		3,209		3,209	
Number of individuals	127		426		426	
R^2	.32	.34	.25	.20	.07	.08

Note. Standard errors are in parentheses. Employed = 1 if positive hours, = 0 if no hours worked. Estimation is by random effects GLS. Refer to Table 1 for normal income categories. * $p < .10$. ** $p < .05$. *** $p < .01$.

Table 8

Treatment Effects: Balanced Sample, Longitudinal Survey

	Single parents		**Two heads: Men**		**Two heads: Women**	
	Employed	Hours	Employed	Hours	Employed	Hours
Treatment group	.232*** (.088)	110.37** (48.76)	−.016 (.029)	−13.02 (21.91)	−.072* (.041)	−56.73*** (22.38)
Normal income fixed effects	Yes		Yes		Yes	
8 survey (time) fixed effects	Yes		Yes		Yes	
Sample size	839		2,997		2,997	
Number of individuals	103		333		333	
R^2	.36	.40	.26	.22	.09	.10

Note. Standard errors are in parentheses. Employed = 1 if positive hours, = 0 if no hours worked. Estimation is by random effects GLS. Refer to Table 1 for normal income categories. * $p < .10$. ** $p < .05$. *** $p < .01$.

Table 9

Treatment Effects for Probability of Employment: Full Sample, Payments Administrative File

	Single parents	**Two heads**
Treatment group	.137* (.072)	−.041 (.031)
Normal income fixed effects	Yes	Yes
34 monthly fixed effects	Yes	Yes
Sample size	3,484	10,237
Number of individuals	126	363
R^2	.27	.13

Note. Standard errors are in parentheses. Employed = 1 if positive wages, = 0 if no wages earned. Estimation is by random effects GLS. Refer to Table 1 for normal income categories. * $p < .10$. ** $p < .05$. *** $p < .01$.

Table 10

Treatment Effects for Probability of Marital Satisfaction/Time Allocation

	Wives				Husbands	
	Agree on time spent together	Agree on who does housework	Satisfied husband helpful	Happy with marriage	Agree on time spent together	Happy with marriage
Treatment group	.047 (.061)	.087* (.057)	.110** (.056)	.095** (.041)	.035 (.042)	.150*** (.058)
Normal income fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. variable control group	.495	.467	.625	.809	.529	.519
Sample size	294	295	295	295	296	296
R^2	.04	.02	.06	.07	.03	.08

Note. Standard errors are in parentheses and adjusted for clustering at the level of the individual. Agree on time together and agree on housework = 1 if the individual responded that they strongly agree or agree that satisfied (= 0 if disagree or strongly disagree). Estimation is by OLS. Refer to Table 1 for normal income categories. All questions used in Table 10 were asked only once at periodic follow-up survey 7 (on average, 1 year, 8 months post–random assignment). * $p < .10$. ** $p < .05$. *** $p < .01$.

Table 11

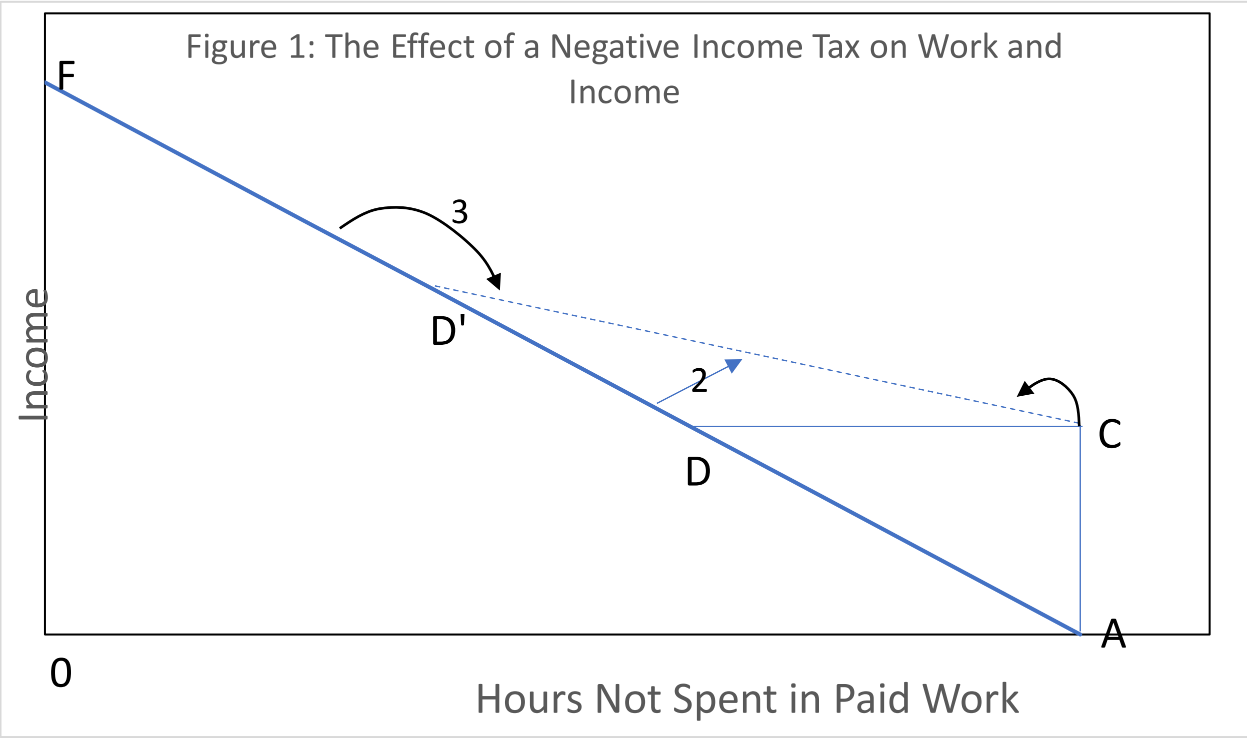
Treatment Effects for Probability of Being Happy

	Single parents	Husbands	Wives
Treatment group	−.060 (.047)	.085*** (.031)	.050* (.028)
Normal income fixed effects	Yes	Yes	Yes
Survey 7 dummy	Yes	Yes	Yes
Sample size	227	668	663
Number of individuals	121	336	334
Mean dep. variable control group	.821	.859	.888
R^2	.08	.05	.05

Note. Standard errors are in parentheses and adjusted for clustering at the level of the individual. The dependent variable = 1 if the individual reported that they were very happy or happy, = 0 for not too happy or not happy at all. The happiness question was asked only at the periodic follow-up survey 7 (roughly 1 year, 8 months post–random assignment) and survey 11 (the final periodic, roughly 3 years post–random assignment). Estimation is by OLS. * $p < .10$. ** $p < .05$. *** $p < .01$.

Figure 1

The Effect of a Negative Income Tax on Work and Income



Appendix

Our empirical results differ in important ways from those reported in Hum and Simpson (1991, 1993), hereafter HS. This appendix summarizes factors that may account for these differences.

1. We are unable, despite considerable effort, to come close to replicating the Hum and Simpson (1991) results. Based on correspondence with the authors, it appears that the data and computer code used in their study, as well as related documentation, no longer exists. The published study does not provide sufficient detail about how the data were handled to be helpful. Standards for providing detailed information of this nature have changed substantially since that time, and the requirement that is now common (at least for many academic journals) to make one's computer code and data available for replication purposes rarely existed at that time.

2. We do not understand how their sample was constructed. HS used the periodic surveys that we also use. However, their sample sizes are dramatically different. According to Mincome documentation (Mason, 2016), there were 1,074 intact households enrolled and randomly assigned at the Winnipeg site, consisting of 704 treatments and 370 controls. However, HS have a sample size of 1,187 intact families, 575 treatments (about 20% fewer treatments than were enrolled) and 612 controls (i.e., 65% more controls than were enrolled).

3. As discussed in the text, Mincome used the Conlisk-Watts assignment model, one consequence of which is unbalanced allocation to treatment and control families. In the U.S. NITs, which also used the Conlisk-Watts assignment model, the number of treatment families exceeds the number of control families. For example, in SIME-DIME there were 2747 treatments and 2053 controls and in Gary there were 1026 treatment families and 774 control families. Similarly, all reported sample sizes for Mincome have more treatment families than control families (Simpson et al 2017): Winnipeg site (704 treatments and 370 controls), rural sites (103 treatments and 78 controls), Winnipeg supplemental sample (196 treatments and 97 controls). According to Mincome Technical Report No. 2 (Hum et. al, 1979) the final allocation of the assignment model called for 60.1% of families to be allocated to the treatment group and 39.9% to the control group in the Winnipeg site. Thus it is odd that the sample used by HS contained more control families than treatment families, which suggests that there is something wrong with their sample.

4. HS reported results for three family types: (i) men (also called "husbands"); (ii) women in double-headed families ("wives"); and (iii) female single heads ("single mothers"). The "female single head" sample appears to consist of single, female parents and single women without dependants. However, the labour supply behaviour of single mothers

and single women without children differs dramatically, so pooling these two groups is questionable. Our “single parents” group is restricted to single parents, about 90% of whom are single mothers. HS also pooled single men with males in two-headed families, two groups whose labour supply behavior likely differs. No tests for the appropriateness of pooling single women with and without children and single and married men are reported.

5. No balancing tests are reported by HS. This we do not know whether random assignment held in their data.