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Basic Income in a Small Town: Understanding the Elusive Effects on Work

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ABSTRACT

This paper examines the impact of a guaranteed annual income experiment from the 1970s called the Manitoba Basic Annual Income Experiment (Mincome). We examine Mincome's "saturation" site located in Dauphin, Manitoba, where all town residents were eligible for payments. Would people work less if their basic needs were guaranteed outside the market? Never before or since the Dauphin experiment has a rich country tested a guaranteed annual income at the level of an entire town. A community-level experiment accounts for the fact that people make decisions in a social context, not in isolation. Using hitherto unanalyzed data we find an 11.3 percentage point reduction in labor market participation, and nearly 30 percent of that fall can be attributed to "community context" effects. Additionally, we show that withdrawals were driven disproportionately by young and single-headed households. Participants who provide qualitative explanations for work withdrawals typically cite care work, disability and illness, uneven employment opportunities, or educational investment.

KEYWORDS: Guaranteed Annual Income; Basic Income; Work; Economic Sociology; Poverty; Social Policy.

The problem of poverty persists in the world's wealthiest societies. While its persistence is often attributed to insufficient human capital or weak labor markets, a simpler view was expressed by Paul Samuelson: "The curse of the poor is literally their poverty. Give them more money" (Coyle and

Wildavsky 1986:169). The spirit of Samuelson's comment is captured by an ambitious and much discussed policy proposal called basic income (BI).

The goal of the proposal is to guarantee a basic standard of living to all in the form of cash payments without compulsory work requirements. Basic income, alternately termed guaranteed annual income (GAI), or negative income tax (NIT),[1] partially decouples standards of living from labor market earnings. According to a leading proponent, Philippe Van Parijs, the objective of basic income is to transform the deprivations linked to non-employment and poorly remunerated employment into "real freedom" (1995). Real freedom requires that individuals have not just the abstract liberal right to freedom, but the financial resources to make freedom a lived reality. Furthermore, by securing individuals' "power to say no" (Widerquist 2013), basic income reduces the vulnerability of poor and working people to exploitative relations in labor markets.[2] While there is a vibrant theoretical literature on the desirability of basic income (see debates in Van Parijs 1992 and Wright 2006), the question of its viability once implemented has not been extensively explored. In particular, the effects of basic income on work are not well understood. In our view, the best way to understand the transformation of a system as complex as the work–income relationship is to observe it directly.

Would people work less if their basic needs were guaranteed outside the market? What would happen if a community could achieve genuine economic security in the form of universalistic cash payments? For some (Van Parijs 1995; Widerquist 2013; Wright 2006) the central rationale for basic income—the "power to say no"—is for others, the central concern. That power, critics worry, would facilitate lower labor market participation (Anderson 1978; Kenworthy 2014; Moffitt 1981). Reducing work incentives and weakening economic activity would ultimately undermine the funding of a basic income.[3] These expectations are based typically on economic theory (i.e. Conlisk 1968; Pigou 1948) or individual behavior in micro-experiments (Burtless 1986; Greenberg and Robins 1986; Keeley 1981; Robins 1985; Widerquist 2005) that delink participants from community-wide consequences and social contexts.

We analyze the effect of a universally available basic income on labor market participation using never-before-analyzed data from a basic income experiment that took place in a rural town called Dauphin in the Canadian province of Manitoba. The Manitoba Basic Annual Income Experiment, or Mincome, operated in Dauphin for three years between 1975 and 1977. The exceptional feature of the experiment is its universal availability or "saturation" treatment in Dauphin, though Mincome also included randomized control trial portions with treatment and control participants dispersed across Winnipeg and various Manitoba towns. Both rural dispersed treatments and controls provide useful comparison points for Dauphin. It was the saturation site, however, that was unprecedented. Never before or since the Dauphin experiment has a rich country tested a guaranteed annual income at the level of an entire town. Never before or since has a rich country conducted an experiment in a setting intended to mirror the community-level experience and future administration of a program where the abolition of one family's poverty coincides with its abolition as such.

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1   While GAI was the term of art used in the 1970s, BI is currently in fashion; we use them interchangeably depending on the context. However, key administrative and symbolic differences should not be ignored: while both ensure a minimum income to all, BI is paid to everyone and then partially collected back through taxation, where the GAI/NIT is paid to anyone whose income falls below some threshold. Put differently, the NIT variant functions *ex post*, after incomes are assessed, where basic income operates *ex ante*, without prior assessment of incomes. Still, many scholars (Van Parijs 2006; Widerquist 2005) view the NIT variant as nearly equivalent to the BI model for two reasons: 1) the new incentives individuals face are effectively identical in both systems (Harvey 2006); and 2) both systems can achieve the identical post-tax-and-transfer income distribution (Groot 2004). It is also worth noting that although Mincome used the NIT mechanism, it self-identified as a "basic income" experiment. This is consistent with the view that the NIT is one variant of the umbrella category called basic income.

2   Widerquist's (2013) expression "freedom as the power to say no" requires freedom from certain kinds of "force." He appeals to G.A. Cohen's definition of force: "When a person is forced to do something, he has no *reasonable* or *acceptable* alternative. He need not have no alternative at all" (Cohen 1988:245).

3   The other objection is normative: It is simply unfair to allow the able-bodied to exploit the labor effort of others (Elster 1986).

No published research has analyzed the experiment's own qualitative or quantitative survey data in order to understand the experience in Dauphin.[4] Using a difference-in-difference model, our analysis shows that a moderately sized basic income generated an 11.3 percentage point reduction in labor market participation. For some perspective, this gap is not unlike the current labor market participation gap between countries like the U.S. or Canada on the one hand, and a country like Belgium on the other.[5] We estimate 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. The portion of the effect linked to the modified social dynamics or norms that appear in a community with a universally available basic income is the social interaction effect. Thus, ignoring the community-level context underestimates work reductions. Adjudicating between individual and community effects is a key contribution of this study.

We also examine the households behind the labor market withdrawals. To this end, additional difference-in-difference analyses on various subgroups reveal the distribution of experimental effects. We show that both young and single-headed households drive withdrawals disproportionately. Drawing on archived qualitative survey data we highlight some of the heterogeneous reasons behind work reductions that participants provide. Unlike the theoretical expectation that participants were simple leisure maximizers, participants that left the labor force typically cite limited employment opportunities, engagements in care work, disability, old age and illness related leaves, or educational investment.

In the second section below we introduce the history of this understudied social experiment and explain how the guaranteed annual income operated. In the third section we highlight the merits of the central and distinguishing feature of the experiment; namely, that it plays out at the community level and was available to all town residents rather than the more conventional micro level where isolated and randomized treatment subjects are matched to controls. Although there are trade-offs, we suggest that the main value added of the former is the ability to incorporate critical social interaction effects. In section four, we outline the various micro- and macro-level mechanisms through which basic income can affect patterns of work. Section five describes the data and methods used to examine the experimental impact of Mincome on work in Dauphin. Sections six and seven present quantitative and qualitative findings. We close with a discussion of the policy implications of the Dauphin experiment and the difficulty of attributing experimental treatment effects to one or more individual or social mechanisms.

THE HISTORY AND MECHANICS OF MINCOME

Mincome was launched in 1974 in the political context of Pierre Trudeau's federal Liberal government and Manitoba's first New Democratic Party government under Ed Schreyer. The experiment was introduced in the wake of the 1966 Canada Assistance Plan, a cornerstone of Trudeau's "Just Society," Canada's answer to Lyndon Johnson's "Great Society" (Van Loon, 1979). The Canada Assistance Plan consolidated provincial and federal social assistance commitments and is widely considered a major advance for anti-poverty policy within the confines of the liberal assistance tradition (Haddow 1993; Hum 1983). The framework was built on the typical dichotomy between those deemed unfit to work, and therefore meriting assistance, and those deemed employable and thus expected to meet their needs in the labor market. In the early 1970s, influential reports from the Department of National Health and Welfare (Canada 1970) and the Senate (Canada 1971) made appeals for a national GAI program to address the enduring problems of poverty, particularly among the working poor. In the Manitoba context, perhaps as noteworthy was the influence of economist

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4 One recent exception, Calnitsky (2016), is a related paper analyzing Mincome and social stigma.

5 Between 2000 and 2007, before the economic crisis, the difference in labor force participation between the U.S. and Belgium was 10.4 percentage points on average. The equivalent figure for Canada and Belgium is 11.9 percentage points (OECD, stats.oecd.org).

Clarence Barber, whose 1972 report—commissioned by the province to examine "all major aspects of welfare policy" including "possible alternatives"—advocated for a GAI (1972:5). The Manitoba government had already publicized a proposal for a "demonstration study . . . in a specific geographic location of the province . . . to demonstrate its validity and develop the most effective ways of operating GAI within our larger social policy context" (Manitoba, June 11, 1971). By February 1974, final approval for Mincome was announced. It was "expected to make an important contribution to the review of Canada's social security system" (Manitoba, February 22, 1974).

Mincome's design was inspired by four similar American GAI experiments begun in the 1960s (for summaries, see Lampman 1976; Levine 1975; Levine et al. 2005; Munnell 1986; Rossi and Lyall 1976; and Widerquist 2005). Like the American experiments, the chief aim of Mincome was to track the labor market effects of an income security program administered in the form of a negative income tax (Hum, Laub, and Powell 1979). Also like the American experiments, Mincome included "dispersed modules" in Winnipeg and rural Manitoba where randomly selected treatment participants would be evaluated against controls. However, due to an interest in gaining administrative experience with the GAI, not least among politicians (Manitoba, February 22, 1974; Hikel and Harvey 1973; Schreyer 1971), the central design difference was the inclusion of a saturation site in Dauphin, Manitoba.

In Dauphin, administrative and community issues could play out in a less artificial setting, shedding light on what a national GAI might look like. Researchers felt that the reduced chance for joint leisure activities or the fear of hostility to work time reductions might bias behavior in the dispersed sites (Atkinson, Cutt, and Stevenson 1973; Hum et al. 1979). Dauphin, whose population was just under 9,000 at the time, was thought to reflect provincial averages for various factors including rate of population growth, average income, rural/urban division, and family size (Manitoba, September 6, 1974; Hikel and Harvey 1973; Hikel, Powell, and Laub 1974). The saturation site was intended to simulate a delivery and administration system where costs, community participation and experience would resemble, at the community level, a Canada-wide program.

Dauphin participants were offered guaranteed incomes equivalent to $19,500 for a four-person family, which was about 38 percent of median family income (a measure that excludes relatively low income "non-family persons") or 49 percent of median household income in 1976.[6] At a negative tax rate of 50 percent, the NIT worked as follows: if you did not work at all your guaranteed income payment would be $19,500; if you went into the labor market and earned, say $6,000, your payment would be $16,500 (19,500 – 6,000 x 0.5) leaving your final income at $22,500 (16,500 + 6,000).[7] As shown in table 1, when market incomes rise, Mincome payments gradually phase out so that people could always increase their incomes by working (see Hum et al. 1979).

At least 706 Dauphin households—or about 2,128 individuals—received benefits at some point throughout the program.[8] In addition to households exiting before the final survey, this number includes "walk-ins" who registered late in the program (Dauphinites who did not join initially were able to join at any point). Before the start of the program Mincome staff knocked on the door of every home in Dauphin to introduce the experiment with an initial interview. After the interview, prospective participants would mail an application form and income statement to the Mincome office. The entire procedure could be completed through the mail. Although a guaranteed income floor was universally available, roughly 18 to 20 percent of the population participated for at least some amount of

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6   All dollar figures are quoted in 2014 Canadian dollars.

7   Note that this scheme is distributionally equivalent to households keeping the full Mincome payment and facing a steeper progressive income tax on total income.

8   Of these numbers we have data on 600 Dauphin households, or precisely 1,757 individuals. As we note below, digital data excludes farm households, thus the additional 106 households refer only to those farm families that initially enrolled in Mincome (Sabourin 1979). There is no information on walk-in farm families in Dauphin, though some families almost certainly joined late. For that reason, 2,128 individuals should be considered a lower bound. If the late-joining farm families number in the same proportions to non-farm families—just under half—there may as many as 94 uncounted farm families (roughly 329 individuals), leaving an upper bound estimate of 2,457 individuals.

Table 1. Illustration of Impact of Mincome for a Family of Four

Market Incomes	Mincome Payments	Post-Mincome Incomes	Percent of Positive Taxes Rebated
0	19,500	19,500	100
6,000	16,500	22,500	100
12,000	13,500	25,500	100
18,000	10,500	28,500	100
24,000	7,500	31,500	100
30,000	4,500	34,500	100
36,000	1,500	37,500	100
39,000	0	39,000	100
41,200	0	41,200	50
43,400	0	43,400	0

Notes: The "breakeven" point of $39,000 is the market income level where direct payments end. Up to this point all positive taxes are rebated; to avoid a "notch effect" where households suddenly face positive tax liabilities, rebates are gradually phased out until $43,400 at which point all Mincome benefits are exhausted.
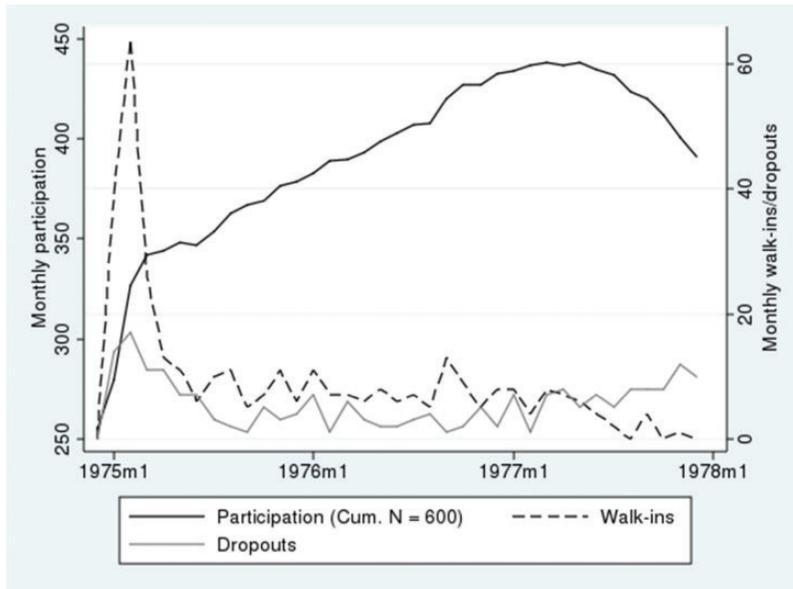
Figure 1. Total Mincome Households, "Walk-ins," and "Drop-outs" Each Month in Dauphin

time. As Figure 1 shows, household participation in the Mincome experiment steadily increased each month until the participation decline near the program's end.

Detailed econometric results from the experiment might have emerged more promptly had the project not faced major setbacks from the outset (Hum 1985). The original $79 million budget proved, over the course of the Mincome program, to be insufficient. Unlike the guaranteed incomes themselves, the budget was not indexed to inflation. The experiment in Dauphin was completed, though different research elements were jettisoned along the way, such as the embedded sociologists (see Rhyne 1979). Data were to be archived but not analyzed. The project concluded all activities in 1979, publishing no official report and leaving vast amounts of collected data unanalyzed. Reflecting on the unfortunate demise of the experiment, original Mincome research director Derek Hum (1985) recalled that "[t]he data, it was hoped, would at least be preserved for analysis at a later date" (p. 42).

At the conclusion of the Mincome program, a large amount of longitudinal survey data was collected into several datasets (Mason 1985). These datasets were used for the handful of academic papers published on Mincome in the 1980s and 1990s (i.e. Hum and Choudhry 1992; Hum and Simpson 1991; Hum and Simpson 1993; Prescott et al. 1986). Due to limited resources it was decided that most survey data would be digitized for the Winnipeg site rather than the Dauphin and Manitoba sites; as a result, previous analyses have been restricted to Winnipeg. However the "baseline" (or pre-Mincome) survey as well as administrative data from the "Payments Department"—the department responsible for calculating and distributing monthly payments to households—was digitized for all sites. These two sources of data serve as the basis for the analyses presented here, as described in the Data and Methods section.

The papers using Winnipeg data produced conclusions not unlike those in the American experiments. There was a modest work hour reduction in the Winnipeg "urban dispersed" site—about 1 percent for males, 3 percent for married females, and 4 percent for unmarried females (Hum and Simpson 1991). Though no published research has examined the survey records on Dauphin, recently Evelyn Forget (2011, 2013; Forget, Peden, and Strobel, 2013) has renewed public interest in the experiment. Using aggregate data from the Department of Education she showed that during the Mincome years, Dauphin students were more likely than their rural or urban counterparts to enroll in high school. Additionally, using Manitoba Health data, she showed that relative to controls Dauphinites saw a reduction in hospitalization rates during the Mincome years. As Forget's research demonstrates, an experiment of this sort elicits a variety of interesting macro-social questions. In the section below we motivate the value of experimentation at the community level.

FINDING THE APPROPRIATE LEVEL OF ANALYSIS

The main source of existing empirical evidence on the effects of basic income on work comes from the randomized control trial GAI experiments. These produced valuable datasets used chiefly to answer one key question: is there a difference between the labor supply of people that receive a guaranteed annual income and those that do not? The answer was a modest yes. Across the various experiments, men receiving the income supplement had worked between 0.5 and 9 percent fewer hours than men not receiving the supplement. Married women and single mothers typically saw somewhat larger labor supply effects (see Burtless 1986; Greenberg and Robins 1986; Hum and Simpson 1991; Keeley 1981; Robins 1985; Widerquist 2005). In keeping with this central question, this paper compares labor market participation among households with and without guaranteed annual incomes. However, rather than exclusively studying the effects on dispersed and isolated program participants this paper focuses on participants embedded in a community where everyone had the option to participate.

There are good and often-neglected reasons why community-level experiments may be preferable to randomized ones. While it is true that the randomized GAI experiments signaled a high-water mark in the art of studying poverty (Haveman 1997; Lang 2007), there were fundamental problems with these studies that were the direct consequence of their scientific precision. Randomized control trials are often thought of as the gold standard for empirical evidence as they exceed all rival approaches in eliminating selection bias and confounding variable problems (Banerjee and Duflo 2012; Shadish, Cook, and Campbell 2001). Nonetheless, if we are interested in the ways basic income affects work it is important to recognize that a revolutionized social policy will play out among people living in communities. Of course, the randomized control trial GAI studies also played out in communities. They were, however, communities without access to the GAI, and therefore not the *relevant* communities. The relevant community to study individual decision-making is one where individual decisions are made amidst neighbors facing the same decisions (see Blalock 1984; Durlauf 2001; Garfinkel, Manski, and Michalopoulos 1992; Hedström 2005; Przeworski 1974).
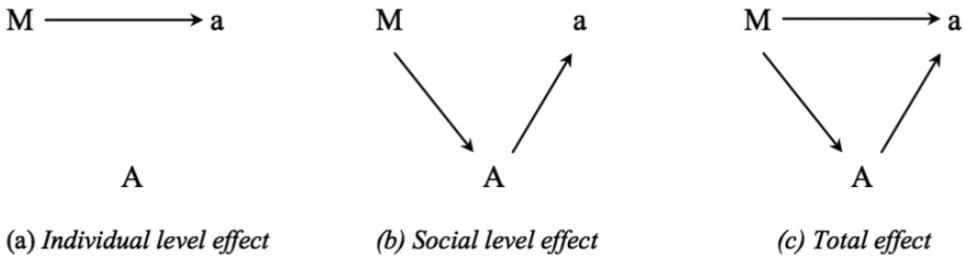
Figure 2. The Effect of Mincome, M, on Individual Treatment Subject a, Given the Presence and Absence of Neighbor/Community, A

    This is the set-up of a macro-experiment. For example, if we wish to understand the real-world effects of a new policy designed to incentivize high school completion, we should examine students' decisions in the context of peers considering the same options, not in the context of peers without the new policy (Akerloff and Kranton 2002; Coleman 1961; Solon, Page, and Duncan 2000). A student may be affected both by the new policy itself and by her friends' newly altered decisions to complete their schooling. The simple sociological idea is that friends, neighbors, and norms (Jencks and Mayer 1990; Sampson, Morenoff, and Gannon-Rowley 2002; Wilson 1987) all impinge upon people's inner psychology. For this reason, many randomized experiments may not be easily scaled-up (see Reddy 2012). In short, the appeal of macro-experimentation is rooted in a deeply sociological instinct that micro-level incentive adjustments are not independent from macro-level contexts.

    Macro-social feedback is as significant to the question of work as it is in the example of education. Yet, the implicit assumption of micro-experiments with dispersed participants is that context is uninteresting subject matter.[9] The possibility, for example, that an emerging social milieu makes work reduction more socially acceptable is ruled out. In fact, as spelled out in section four, the significance of community-level experimentation lies in the diversity of social interactions that develop around a revolutionized policy regime.

    As shown in Figure 2, the total effect of a program can be understood as the combination of individual and social effects. In our case, we estimate both the micro-level effect of the policy in dispersed and isolated Manitoba cases as well as the total macro and micro effect in the saturation context in Dauphin. By subtracting the micro effect from the total effect, we derive the social interaction effect or "community residual." Below, we outline the particular mechanisms of social interaction, along with the particular micro mechanisms, which operate in the case of labor market participation.

RETHINKING BASIC INCOME AND WORK

The Dauphin experiment sheds light on a number of interesting macro issues that cannot be discovered in the context of randomized controlled trials.[10] For this reason, thinking about basic income's effects on work requires distinctions between the individual-level effects that operate at the scale of changes in the micro-level incentive structure, and social-interaction effects involving a dynamic interdependence between individuals and the communities in which they are embedded. The former are analyzable in micro- and macro-experiments, the latter appear only in macro-experiments. Macro-experiments have the advantage that they do not require the unlikely pretense that an individual's behavior will not affect the behavior of others.

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9   This is not quite Hamlet without the prince. It is perhaps closer to Hamlet without the rest of Denmark.

10 One issue ignored in the GAI experiments, which we spend little time on here, was pointed out early on by Hyman Minsky (1969). The design of the RCT experiments—comprised only of participants with low incomes—made it impossible to incorporate effects on potential work withdrawals from the *non-poor* working population, a factor that could be relevant in a GAI implemented in the real world. In Dauphin, by contrast, the non-poor working population also had the option to join Mincome and reduce work.

Table 2 assembles a variety of plausible effects of basic income on waged work, sorted into individual-level and social-interaction categories. Typical GAI experiments exclude social effects by design, and attribute all individual-level effects to a single mechanism by assumption (i.e. Haveman 1997). This section presents a map of sometimes mutually reinforcing, sometimes crosscutting mechanisms at social and individual levels. While our data allow us to distinguish the social from the individual levels, it is inherently difficult to distinguish among the specific individual and social mechanisms. However, if we hope to understand the real-world effects of a policy like basic income, it is necessary to outline the relevant candidates. The case of basic income in a small town provides an opportunity to expand the theoretical toolkit and outline the constellation of mechanisms at work.

Individual Mechanisms

The central individual-level hypothesis dominating the debate on basic income argues that in the absence of the dull compulsion of economic relations, individuals may reduce work in formal labor markets.[11] This general hypothesis—the only mechanism considered by economists studying the work effects of the NIT—was analyzed within the highly stylized theoretical context of static consumer choice theory applied to rational individuals optimizing the trade-off between labor and leisure (Burtless and Greenberg 1982; Kesselman and Garfinkel 1978; Metcalf 1973). The precise identification of the extent to which work incentives weaken was the central policy question of the NIT studies, and this specification provided a mathematically tractable problem (Rossi and Lyall 1976). Tractability, however, does not justify attributing the entire treatment effect to this lone mechanism. While previous NIT studies *implied* that treatment effects were fully explained by this mechanism, in fact they reported effects (and correctly attributed those effects to the experiment) but did not explore the possibility that other mechanisms were operating (see also Cartwright 2011; Cartwright and Hardie 2012; and Deaton 2010, on the inability of randomized control trials to isolate underlying mechanisms. Related issues are considered in Leamer 2010; Reddy 2012; and Rodrik 2008).

We retain this general mechanism but interpret it more broadly. We see the mechanism as an individual's basic "exit option" from formal labor markets, but do not assume that it implies more leisure. For example, one family explained that they joined Mincome because "We had no other choice as my husband is disabled and with my health and age, I am not able to work full time." As we will see in our discussion, the abstract binary of formal labor market participation and leisure does not summarize the menu of relevant options for most people.

Likewise, we should expect that a basic exit option would play out differently for differently situated people. In particular, a wide range of groups facing high barriers to labor market participation, meager returns to work, or now-feasible alternatives, may be inclined to reduce work in the market. This group includes single-parent households, who must balance parenting and provisioning responsibilities and often face multiple barriers to work (Blank 2007; Danziger and Seefeldt 2003; Edin and Lein 1997). Secondary earners, often female, in dual-headed households might also be inclined to reduce work, perhaps to engage in unpaid care work (Connelly 1992; Gornick, Meyers, and Ross 1998; Kalleberg and Rosenfeld 1990; Killingsworth and Heckman 1986; Pateman 2004). Likewise, older workers or workers with health problems might be disproportionately inclined to reduce work (Bazzoli 1985; Hanoch and Honig 1983; Myles 2002; Schur 2003). Younger people might reasonably choose education and training over the labor market (Forget 2011; Maynard and Murnane 1979; McDonald and Stephenson 1979; Rea Jr. 1977; Venti 1984; Weiss, Hall, and Dong 1980). Finally, as we note below, workers with limited labour market opportunities or unpleasant jobs might decide to reduce paid work (Holzer 1996; Hotz, Mullin, and Scholz 2001; Kalleberg, Reskin, and Hudson 2000; Piketty 1999). Differently situated people are, naturally, affected by different immediate circumstances. As a result, a basic exit option will be channeled through different sub-mechanisms.

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11   As Irwin Deutcher (1979) put it, the "basic idea of this massive experiment is to determine how much able-bodied people will goof-off if their income is maintained at a poverty level" (p. 237).

Table 2. Theories of Labor Market Effects of Basic Income

		Factors That Should...	
		Increase Work	*Decrease Work*
Individual effects	Basic exit option		X
observable in	Removal of means-tested poverty traps	X	
all nit studies	Karelis effects	X	
Social-interaction	Underemployed find work more easily as 'overemployed' reduce work hours	X	X
effects observable			
only in macroexperiments	Rising wage offers (through multiple mechanisms)	X	X
	Community context and stigma effects		X

An entirely different kind of mechanism, still operating at the individual level, comes from philosopher Charles Karelis (2009; for analogous conclusions with different behavioral premises, see Mullainathan and Shafir 2013; and Shah, Mullainathan, and Shafir 2012). Karelis argues that contrary to much microeconomic theory, the law of diminishing marginal utility does not apply under conditions when (context-dependent) basic needs are unmet. This is a rejoinder to the neoclassical logic, which suggests that since the first dollar is the most valuable dollar, the poor are irrational for working less than the non-poor. Instead, Karelis' premise is that the poor are as rational and responsive to economic incentives as anyone else. He compares poverty to having dozens of bee stings. While having only one bee sting may induce individuals to work, say, an hour, to seek relief for that lone sting, working that same hour for relief from that same marginal sting is irrational amidst dozens of other stings. Thus, when basic needs are unmet, the rational poor will see "poverty reducing behavior"—in this case, an hour of work—as a suboptimal choice since it does little to diminish the array of challenges posed by living in poverty. Against the neoclassical vision, Karelis suggests that the context of poverty itself generates a low marginal utility of work. On this view, by reducing economic deprivation a basic income shifts people away from a position that disincentivizes work, and toward a position where work becomes easier. One Mincome participant expressed a similar rationale: "It is a good program because you can depend on a regular payment and work a good budget around it. The Mincome program helps people to get out of a rut, it guarantees a wage and if a person gets a low income job, the Mincome program seems to realize to take a job costs more money and expenses (gas, lunch etc.)."

Another argument made by many basic income advocates stresses that the scheme reduces work disincentives: no one is made worse off by taking a job. As Brian Steensland (2007) has argued, the guaranteed income was designed with the explicit purpose of balancing economic security with economic self-sufficiency. In fact, some proponents' central argument in favor of BI over traditional welfare programs has to do with the absence of "poverty traps"—positions where not working at all is preferable to working for a small amount of income (Atkinson 1995, 2015; Vanderborght and Van Parijs 2005). Welfare programs often reduce benefits dollar-for-dollar of earned income or maintain high marginal tax rates thereby making it difficult for welfare recipients to work (Brewer, Saez, and Shephard 2010; Caniglia 1996; Moffitt 1992, 2002). This dynamic may have been particularly strong in North American welfare programs in the 1960s and 1970s (Danziger, Haveman, and Plotnik 1981; Hum and Simpson 1991). Substituting a guaranteed income for welfare typically reduces the marginal tax rates faced by the poor, thereby reducing the penalty to labor market participation. "I think Mincome would replace many other welfare agencies and probably be more equitable. Many people on minimum wage would get the assistance they needed without quitting their employment to get

welfare assistance," wrote one Mincome participant. It is possible that by removing the work disincentives associated with poverty traps—particularly if the traps are deep—BI could actually increase the supply of labor from some households.

In sum, our three types of mechanisms describe the channels through which basic income affects work at the individual level. The first mechanism, the basic exit option, is the most widely applicable and likely therefore to be most salient. However, as with the social mechanisms described below, there is good reason to regard actual empirical results as combinations of the variety of forces.

Social Mechanisms

We identify four kinds of social mechanisms that should be considered when analyzing the realistic context of a macro-experiment: diminished stigmatization, labor demand effects, reductions of overemployment, and changes in power relations. The first reason why a macro-level experiment may operate differently is grounded in the idea of a community context effect: where behavior is mutually dependent, a newly introduced program that affects individual behavior can then affect others in the community, which can in turn feedback to reinforce the original behavior (Garfinkel et al. 1992; Granovetter 1978; Granovetter and Soong 1988; Schelling 1971; Wheeler 1966). Accordingly, norms around socially acceptable levels of work may adjust and intensify any work reductions rooted in individual effects (Lindbeck 1995). If the stigma of leaving work is diminished when a whole community simultaneously confronts this option (see Calnitsky 2016; Rainwater 1986), the labor supply may reduce to a greater extent than is expected in a setting where choices are made in the isolated background context of traditional choices and values. One young married man stated he joined Mincome with his family because "everybody else was." Put simply, individuals might be more inclined to reduce work hours when their friends and neighbors have already done so.[12]

It is worth noting that some analysts of other GAI experiments were aware that potentially important community context effects around work would be ignored in the micro-experimental setting (Harris 1985; Kurz and Spiegelman 1973). If these effects are as powerful as many social scientists imply (Aronson 2003; Elster 1989; Hedström 2005; Sampson et al. 2002; Sharkey 2013; Wilson 1987), then they indicate serious misestimations of behavioral effects in previous micro-level randomized analyses.

A second interactive aspect of markets was also ignored by the micro-experiments: the design of other NIT studies could not account for the most rudimentary market interactions between supply and demand (see Harris 1985; Kurz and Spiegelman 1973; Widerquist 2005).[13] The preoccupation with estimating individual labor supply adjustments ignored the fact that basic economic models determine labor supply interactively with labor demand. To be clear, it is not that labor demand was absent from other NIT studies. It was, however, not the *relevant* labor demand. The problem is that the labor demand interacting with labor supply was generated by firms operating under "normal" labor market conditions (as program participants were an imperceptibly small portion of the population). The implicit assumption was that firm behavior is unaltered in a world of basic income. Labor demand was not determined by employers bargaining with workers with access to guaranteed incomes, ones that might make adjustments in response to the adjustments of workers. It is for this reason that

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12   We should clarify that we identify no *independent* reasons for norm-related and community feedback effects to increase work withdrawal; however, since (1) this mechanism is an intensification of individual-level work mechanisms, and (2) in our judgment those mechanisms, on balance, give rise to withdrawals, we locate the community context mechanism on the right-hand column of Table 2.

13   It should be noted, however, that the Dauphin context (see Ryhne 1979) was not a typical, thick urban labor market; for example, it included a non-trivial component of agricultural employment and secondary earners in farm households might have worked partly in formal labor markets and partly informally on the family farm, shifting between them at different times (Kerachsky 1976; Palmer and Pechman 1978). Secondary workers receiving basic incomes may be under less pressure to work in formal labor markets, though they may be able to increase their work on the farm without affecting Mincome payments. For these reasons, Mincome researchers excluded all participating farm households from the data panel.

randomized control trials could not account for a realistic market interaction based on both sides of an elementary labor market model. Dauphin employers were in fact aware that they might be affected, and some expressed concerns about the program. On one occasion the Dauphin Chamber of Commerce invited Mincome's operations director to a general membership meeting to "clarify questions or air views . . . and discuss criticisms pertaining to the program" (Dauphin Herald, 19 February 1975).

The interaction with firms presents itself in a variety of ways. If tighter labor markets compel employers to raise wages, workers may experience a renewed incentive to work. Other NIT experiments only gave estimates of labor supply reductions, as if these were equivalent to potential real world market outcomes (see Widerquist 2005). In fact, if people cut their supply of labor, demand may kick in to pull wages up accounting for the lost supply. Economists call this the "substitution effect": as wages rise and every hour of work pays more it becomes more attractive to work more. If correct, this line of reasoning suggests that figures from previous NIT experiments might best be understood as the upper limits of work reduction rather than actual estimates. However, a counteracting theoretical scenario is captured by the "income effect": as wages rise, the same level of welfare can be achieved with *less* work. Though these mechanisms may neutralize each other, it is possible that at the bottom of the wage scale the substitution effect prevails (Hotz et al. 2001; Moffitt 2002). Exactly how this interaction plays out is an empirical matter overlooked in micro-level studies.

A third interactive effect is that "overemployed" workers—especially in a period of labor surplus—may reduce labor hours, making it easier for underemployed and unemployed workers to find work (Purdy 1988, 2008; see also Bosch and Lehndorff 2001; and Bruegel, Figart, and Mutari 1998). Thus, even if the labor supply decreases in terms of average work hours per worker, it is possible that the number of people employed stays constant or even grows.[14] This mechanism predicts not an increase or decrease, but a redistribution of available work.

One final issue that might stand out more clearly to economic sociologists is the changing power relations between groups. A meaningful "exit option" would not only strengthen individual workers' bargaining power, but it might also enhance the possibility of workers' collective ability to advance their interests (Lucarelli and Fumagalli 2008; Purdy 1988; Wright 2004, 2006). In the Dauphin context if this mechanism played out, it was most likely limited to simple cooperation with a co-worker in jointly demanding wage increases in a small workplace. However, basic income can be seen as an inexhaustible strike fund, and at the national level, there is no reason to rule out the more advanced forms of cooperation—perhaps including demands for work reductions—which it might facilitate.

No experiment has been able to disaggregate the relative significance of these various mechanisms, but we believe it is important to appreciate the array of forces underlying the treatment effect. In so doing, we avoid the pitfall of assuming that actual outcomes reflect the architecture of static consumer choice theory. Presenting this variety of mechanisms also introduces a number of complex contingencies that must be taken into account when thinking about generalizability. Some elements may have offsetting effects; others may be specific to particular circumstances and subgroups. We return to these issues below.

DATA AND METHODS

This section introduces hitherto unexamined data for this analysis: panel data on Mincome participants in Dauphin, as well as controls and treatment subjects dispersed across Manitoba. We then present the methods employed to identify comparative trends in labor market participation and analyze the effect of Mincome on work.

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14   Imagine a three-person economy with two people working 40 hours per week and one person not working at all. After an implemented BI, the two workers may reduce their weekly work hours to 30, potentially making a 20-hour job available for our non-worker. The average work hours per worker falls from 40 to 26.6, but the number of workers grows from two to three.

The primary information we use comes from two data sources containing variables on households across all Mincome sites. The first source is from the baseline survey conducted prior to the beginning of Mincome, which among other variables (family size, composition, age, etc.) includes household wage data in 1973 and 1974, the two years preceding the program. The second data source is from Mincome's "Payments Department," which provides the total monthly wages of household members and other variables for each of the 37 program months.

The two data sources are merged together and track a consistent set of households in Dauphin, the Manitoba treatment, and the Manitoba control. This excludes families whose participation is intermittent or for whom there is no baseline information. In addition to households that participate consistently across the study and the baseline, we include households that walk-in at or before the first six months of the program who (1) participated in the baseline interview, and (2) participated consistently until the final month.[15] The data are "balanced" in the sense that all families are in both data sets and participate in Mincome through the end of the study period. Merging baseline data with a fully balanced panel reduces our N to 147 households in Dauphin, 100 households in the Manitoba control, and 45 households in the dispersed Manitoba treatment group.[16]

The merged panel generates a before-after comparison for treatment and control groups. In relying on pre-experimental data in addition to Manitoba comparisons, Dauphin participants serve as their own control. This follows in the tradition of many quasi-experimental research designs (Shadish et al. 2001).

To understand the effects Mincome had on work, we examine labor market participation in Dauphin, the Manitoba treatment, and the Manitoba control. Labor market participation is defined by categorizing wages into a dichotomous variable by assigning ones or zeroes to each household-month, indicating participation or non-participation in the labor market across the baseline and study periods. The participation rate refers to labor market participants divided by the total study population in a given group. Following Card and Krueger (1994), we analyze labor market participation using a difference-in-difference set-up, where we present average baseline and study period participation for Dauphin, the Manitoba treatment, and the Manitoba controls, as well as the difference within groups over time (the first difference), the difference within time periods between groups (the second difference), and the baseline–study period difference between groups, or the difference-in-difference. This last measure, the difference-in-difference applied to Dauphin and the Manitoba control, expresses the treatment effect in Dauphin. Applying the same procedure to Dauphin and the Manitoba dispersed treatment captures the social interaction effect in Dauphin.[17] The social interaction effect should be understood as a proportion of the full experimental treatment effect, expressed by the first difference-in-difference. The difference-in-difference structure is able to account for differences in participation *levels* between groups at the cost of the assumption that participation *trends* between groups would not differ in the absence of the deviation induced by the treatment.

Finally, we perform additional difference-in-difference analyses using a variety of subgroups, disaggregated by age, family type, education, parental status, and unemployment or welfare history status. Although the number of observations decline in subgroup analyses, their dynamics are informative with respect to the resilience of treatment effects at different levels of disaggregation.

LABOR MARKET FINDINGS

Before examining the panel data, we take a coarse-grained look at labor force participation to help motivate our detailed analysis. Figure 3 presents aggregated Census data for 1971 (before the

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15   We therefore exclude the first six months of data, as they comprise an inconsistent set of participants. This increases panel participants substantially and reduces the number of months in our panel, but does not affect results.

16   The unbalanced payments data includes families who at any point participated in Mincome, even for a single month. This includes 600 households in Dauphin, 262 in the Manitoba control, and 188 in Manitoba treatment.

17   It is important to note that the Manitoba dispersed treatment group received the same guarantee and tax rate as the Dauphin group, as shown in Table 1.
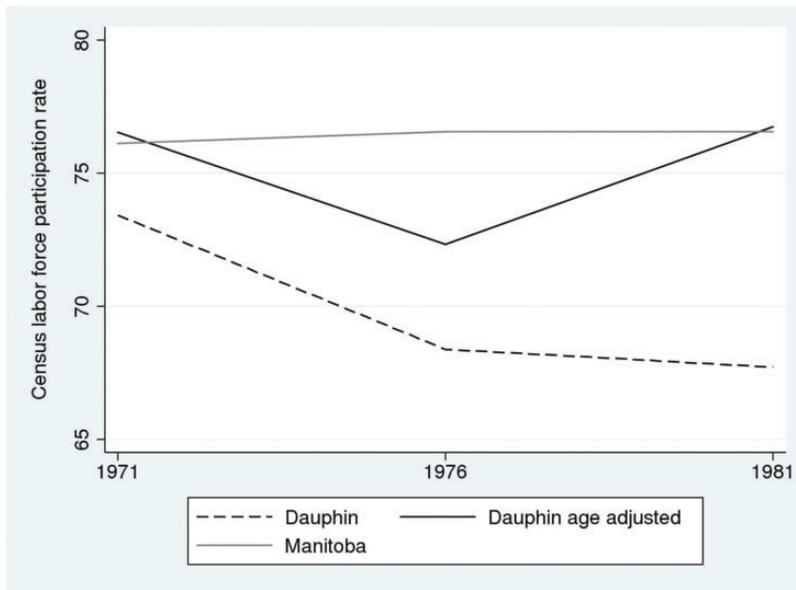
Figure 3. Male Labor Force Participation in Manitoba, Dauphin, and Dauphin Adjusted for Age

experiment), 1976 (the middle year of the experiment), and 1981 (after the experiment).[18] We tabulate the labor force participation rates in Dauphin and Manitoba (minus Winnipeg and Dauphin).[19] The chart also presents Dauphin's "age-adjusted" labor force participation to control for differences in the age distributions—Dauphin has more elderly people than Manitoba, and elderly people are less likely to work—across the two geographic constructs.[20] This compares our geographies *as if* both Dauphin and Manitoba groups had the same age distribution (see Treiman 2009).

The unadjusted male participation rate in Dauphin differs from the Manitoba rate by less than 3 percentage points in 1971. The gap is about three times larger by 1976 and remains high in 1981.[21] After adjusting for age, the gaps in 1971 and 1981 largely dissolve, but remain in 1976, perhaps reflecting the effect of Mincome. It must be acknowledged that this data presents the question of labor market participation at a high level of aggregation for a limited number of sample periods. It is, however, highly suggestive of an experimental treatment effect, which we turn to now.

Figure 4 uses the panel data discussed above to display trends in average labor market participation in Dauphin, the Manitoba dispersed treatment group, and the Manitoba control, between the baseline and study period. This tracks the guaranteed income in a community against isolated guaranteed income recipients as well as an untreated control group. We present six-month averages to reduce noise. The study period trend reveals a decline in Dauphin participation next to a smaller decline in the Manitoba control group, with the Manitoba dispersed treatment falling in the middle.

Table 3 summarizes this data into two-year baseline and three-year study period averages. A static comparison of these averages provides before-after labor market participation differences for each group. These internal differences, shown in the three cells in the bottom left, are 14.7 percentage

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18   Census data are constructed from a 33 percent sample database from the long form census questionnaire.

19   The participation rate refers to the employed and unemployed (individuals had looked for work in the past four weeks) divided by the population 15 years of age or older. Note that the employed group includes a subcategory called "Employed – Unpaid Worker," an attempt by Census Canada to account for informal aspects of rural labor markets. This subcategory, however, excludes unpaid work in the home.

20   Indeed, on average, Dauphin was aging more quickly than Manitoba. Adjusting for education as well as age generates similar findings.

21   Female labor market participation is less clear-cut; while Dauphin's unadjusted participation rate is slightly lower than Manitoba's during the experiment, this disappears after adjusting for age.
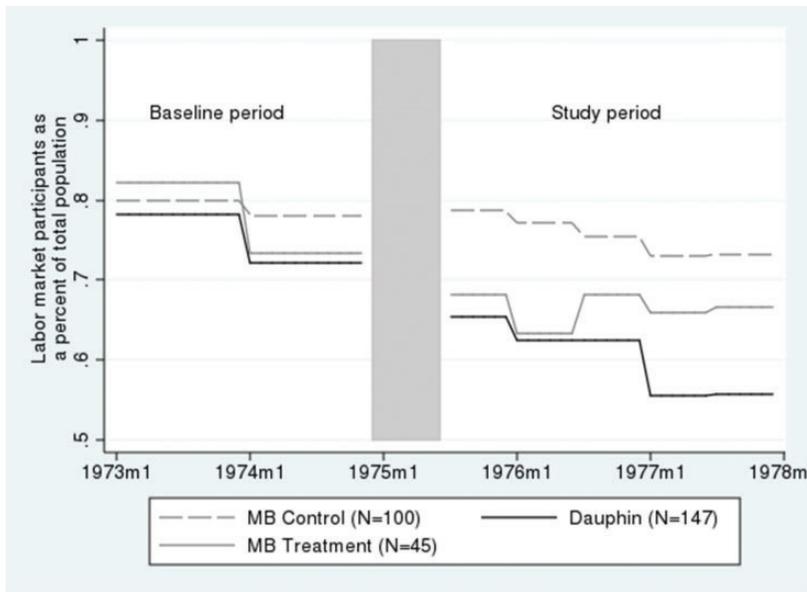
Figure 4. Labor Market Participation in Dauphin, Manitoba Dispersed Treatment, and Control Groups, during Baseline and Study Periods

Table 3. Average Labor Market Participation at the Baseline, Study Period, Differences Within Time Periods, Differences Within Groups, and Difference-In-Differences

	Manitoba Control b/(se)	Dauphin Saturation Treatment b/(se)	Manitoba Dispersed Treatment b/(se)	Difference 1: Experimental Effect (Dauphin – MB Control) b/(se)	Difference 2: Community Effect (Dauphin – MB Treatment) b/(se)
LMP baseline	79.0%	75.3%	78.0%	−3.7%***	−2.7%†
(2 year average)	(0.008)	(0.007)	(0.013)	(0.011)	(0.015)
LMP study period	75.7%	60.6%	66.4%	−15.1%***	−5.8%***
(3 year average)	(0.008)	(0.007)	(0.013)	(0.011)	(0.015)
Change in LMP	−3.4%**	−14.7%***	−11.6%***	−11.3%***	−3.1%†
	(0.011)	(0.01)	(0.018)	(0.015)	(0.021)

†($p < 0.15$), *($p < 0.05$), **($p < 0.01$), ***($p < 0.001$).

points in Dauphin, 11.6 in the Manitoba treatment, and 3.4 in the Manitoba control. Table 3 also shows differences between groups within each period in four cells in the top right.

Difference-in-difference results displayed in the two cells in the bottom right show the average treatment effect of Mincome in Dauphin and the portion attributable to social interaction effects. Difference-in-difference 1 captures Dauphin's internal change minus the change internal to the Manitoba control. It identifies an average treatment effect of 11.3 percentage points. Difference-in-difference 2 captures Dauphin's internal change minus the change internal to the Manitoba dispersed treatment. It identifies an average social interaction effect of 3.1 percentage points. Taken together, this means that relative to controls, just over 70 percent of the 11.3 percentage point reduction in labor market participation can be attributed to individual-level mechanisms and the remainder, nearly 30 percent, can be attributed to community effects.

To supplement this analysis we examine how the changes in participation are distributed across family types, running the above difference-in-difference analysis with a variety of disaggregated

subgroups. Although the number of households in each subgroup is low, it is useful to observe the distribution of withdrawals. First, Table 4 presents baseline descriptive statistics on the three groups in our data; we use these descriptives to construct the subgroup difference-in-difference analyses. Table 4 shows considerable differences in education and age across groups, and smaller differences in marital and parental status. As noted above, the difference-in-difference structure nets out any fixed characteristics of households, allowing each household to function as a control for themselves. Nonetheless, it is useful to isolate the treatment effects within the subgroups described in Table 4. Thus, Figure 5 graphs overall trajectories in order to get a general picture of subgroup trends. Subgroups are displayed as baseline and study period averages for ease of presentation, and treatment effects are shown in parentheses in Figure 5. Among the most consequential, Mincome's average treatment effect (the difference between changes in Dauphin and changes in the Manitoba control) for singles is a 16.2 percentage point fall in household participation in the labor market. Among young people there is a similarly large treatment effect, at 18.6 percentage points. Dual-headed households appear less sensitive to Mincome. For this group, the equivalent treatment effect is 7.4 percentage points. Thus, the overall experimental effect on labor market participation is disproportionately driven by changes in young and single-headed households.

Beyond the disaggregations in the overall experimental effect, disaggregations in the community effect are also instructive. However, the latter make use of Manitoba treatment subgroups, and consequently each is comprised of a small number of households (total N = 45). These effects are typically not significant and results are consequently somewhat more impressionistic. Though not shown, community effects play an important role among the dual-headed households and young households but not among the single-headed households. In accord with our analysis, this suggests that individual mechanisms (taking up exit options; being able to take care of children; being able to go to school; being able to retire early; taking disability-related leaves) dominate among singles. While community effects are absent among the singles, they play a larger role in the labor market decisions of young and dual-headed households. It is also worth noting that community effects are absent or small among those with no high school degree or with unemployment or welfare experience, and large among those households with high school degrees. We return to these decompositions and provide interpretation in the next section.

Overall, it is clear that a moderate guaranteed annual income does not induce a collapse in the formal labor market in our community context. This conclusion is roughly consistent with findings from the micro-level GAI studies (Burtless 1986; Greenberg and Robins 1986; Hum and Simpson 1993;

Table 4. Baseline Characteristics in Dauphin, Manitoba Treatment, and Manitoba Control

Sample Characteristics	MB Control (%) (N = 100)	Dauphin (%) (N = 148)	MB Treatment (%) (N = 45)
Cohabitation status			
Married	56	67	53
Single	44	33	47
Parent	71	67	82
Age			
Under 30	44	18	27
30-50	43	44	44
Over 50	13	39	29
Education			
High school graduate	45	19	20
Unemployed or welfare in 1973 or 1974	36	48	38
LMP in 1973 or 1974	83	82	84
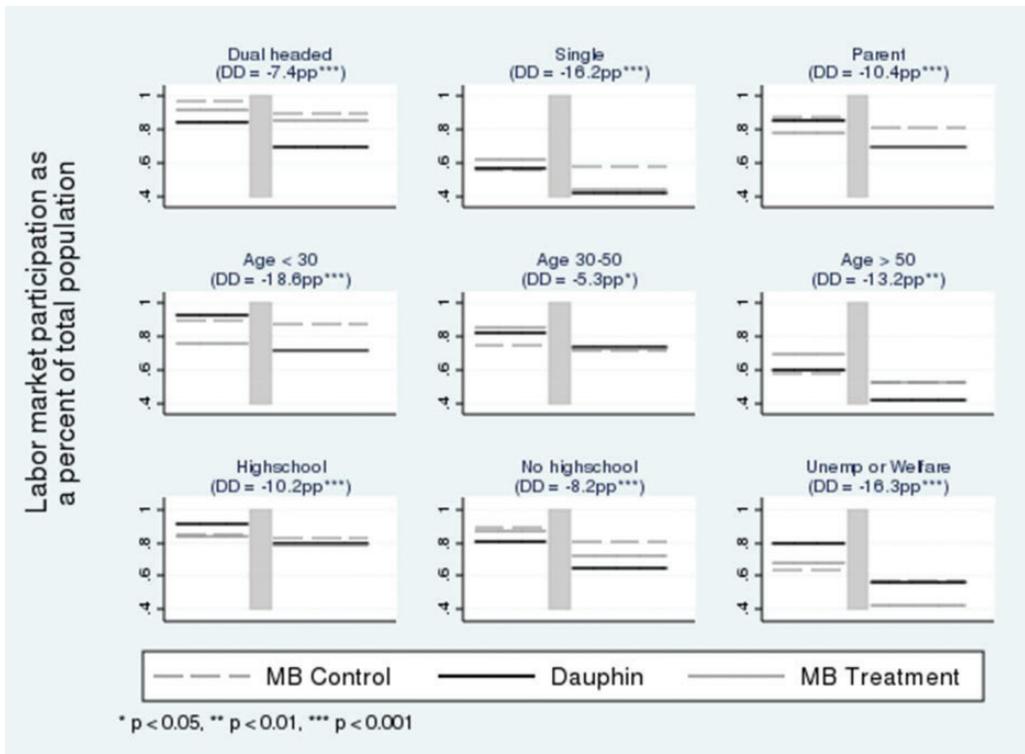
Figure 5. Labor Market Participation Disaggregated by Subgroup

Keeley 1981; Robins 1985; Widerquist 2005), though we find that social interaction effects intensify reductions in labor market participation. The next section draws on qualitative data to parse through the individual and social processes underlying our labor market findings.

QUALITATIVE ACCOUNTS OF MICRO AND MACRO PROCESSES

As the term denotes, the treatment effect should be understood as an outcome produced by the experiment. However, the precise attribution to particular mechanisms is not easily discernible even if the effects are fully attributable to the intervention. In section three we argued that a variety of mechanisms link basic income to different work effects; that this variety tends to be underexplored; and, that it is important to be cautious in emphasizing one or another channel. In particular we pointed to key macro-level factors that cannot be ignored if we wish to understand the social reality of basic income. In this section, we use qualitative survey data to make two brief points: (1) we reemphasize the issue of norms and social interaction in order to account for the amplifying effect of the community environment; and (2) we take another look at the individual-level mechanisms behind some of the work withdrawals.

As Elster (1989) argues, for norms to be *social*, they must be sustained by the approval and disapproval of peers. They are sustained further by the internalization of negative or positive feelings experienced by persons who violate or conform to them (see Therborn 1999). On this view it might be implausible to expect new norms to form and sustain in our short experimental period. On the other hand, the introduction of a radically different system of social provision that significantly altered the work-income relationship might have interrupted traditional common sense around appropriate labor market behavior. Moreover, some people might have "role modeled" various responses to Mincome. That is, the behavioral response of one person—say, to go to school or take time off to look for a better paying job—may have influenced the behavior of another.

Table 5. Reasons Provided for Joining Mincome

"Indicate the main reason why you decided to go on the Mincome program"	N	Percent
No direct indication of work reduction:		
Money/help/assistance	143	44.4
Other ("curiosity"; "was asked", "wanted to help program"; "don't know")	124	38.5
Participation related to social influence	11	3.4
Subtotal	**267**	**82.9**
Indication of potential work reduction in the future:		
If unable to work/in case of illness/security	20	6.2
Subtotal	**20**	**6.2**
Indication of potential work reduction during the experiment:		
Couldn't find work	12	3.7
Could not work/disabled/Ill/Elderly	8	2.5
Help to go to school	7	2.2
Help to care for family	8	2.5
Subtotal	**35**	**10.9**
Total comments	**322**	**100.0**

Notes: This data, digitized and coded by the authors and available in the Mincome accession, is from a one-off self-administered survey on "Community Experience" completed by 407 adults in Dauphin in August, 1976 (Survey completions account for 65% of adults enrolled in Mincome that month; incompletes were due to refusal or illiteracy). Of the 407 surveys, 79% provide at least some qualitative commentary on open ended questions, while Likert scale and yes/no questions (mentioned in text) were on average 97% complete.

A one-off survey on "Community Experience," completed at the midpoint of the experiment provides useful qualitative information (see details in Table 5, and further analysis in Calnitsky 2016) on some of the micro reasoning behind work withdrawals, as well as the macro reasons behind intensified community effects. Although survey questions did not directly inquire into our mechanisms of interest—they did not ask explicitly about work withdrawals or social influences—an open-ended prompt did lead some respondents to mention these issues nonetheless. It must be noted that when asked about their reasons for joining Mincome, the vast majority of respondents do not mention work reduction or social influences. Even if these issues were queried directly, this is not particularly surprising because (1) most people's work patterns were unaffected, and (2) other analyses have indicated that people rarely attribute their own decisions to social influences, even when it is shown to have strongly impacted their behavior (Nolan et al. 2008; see also Cialdini and Goldstein 2004). Despite this, just under 11 percent of respondents do point to individual-level reasoning, which describes the channels through which Mincome may have led to their work withdrawals.[22] Another 6 percent of respondents see Mincome as an insurance system, and imply that the program could at some point allow for withdrawals, or absorb them, when people face uncertainties with respect to health and work. Respondents in this group do not signal work withdrawals during the program; rather, they point to mechanisms through which future work withdrawals could occur: "To back up my financial state in case of sickness."

Comments such as these focus on an individual response to the details of the program, rather than any interaction with the community. However, other participant responses evince a kind of social reasoning. One participant's comments may reflect Mincome's social acceptability in the community setting; he and his partner, both in their mid-50s, joined "just to be in it like the others." A small but interesting group of participants—3.4 percent, as Table 5 shows—admit to joining because "friends told us about it," or a "mother-in-law . . . talked me into it," or "because my parents were on

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22 We take a broad rather than narrow interpretation, viewing, for example, comments about spending more time with children or going to school as an indication of some work withdrawal.

it." These examples—like the young married man above who joined because "everybody else was"—suggest that the behavior of others impacted at least some participants. Mincome participation also appears to have carried little stigma: when asked whether any problems with people or businesses in Dauphin happened because they were on Mincome, 98 percent of participants said "never." Likewise, 93 percent of participants said they "never" felt "embarrassed or uncomfortable . . . with people who are not on Mincome."[23] The individual-level option to exit the labor force allowed some households to reduce work; the social acceptability of these actions might have reinforced the tendency.

How should individual withdrawal decisions be understood? Qualitative reports shed some light on the exit option made possible by Mincome. Though most accounts (89 percent) make no mention of labor market withdrawal, the group that does signal possible withdrawals splits into a heterogeneous set of motives for exit (see Table 5). One 58-year-old single woman joined Mincome, noting: "From this stage on I believe I can't work much longer if any. Also I'm being laid off as my employer too is going out of business." A 31-year-old married woman joined to "Spend a year at home with my children." She added that "I still have two years left at university." A 21-year-old married woman stated that she joined because "I was pregnant and couldn't find a job." An older woman wrote: "We had no other choice as my husband is disabled and with my health and age, I am not able to work full time. . . . If it wasn't for Mincome, I don't know how we would survive as there would be no income whatsoever." One 58-year-old single man emphasized irregular employment opportunities: 'It helped me very much during winter months when work was not too plentiful." A 33-year-old married man noted that "we have the chance to improve our educational level in order to improve our income." Another young couple joined after an accident: "[John] had broken his leg and we needed help."

Although qualitative evidence provides insight into the reasoning behind individual withdrawals and suggests the influence of social context, it is not sufficiently fine-grained to distinguish individual and social effects in our decomposed subgroups. Above, we noted that individual mechanisms drive the declines among single-headed families, while both individual and social mechanisms drive declines among young and dual-headed families. One way to interpret this is to suggest that singles were more likely to be in difficult situations. Differently put, they were more directly motivated by the demands of their circumstances to take the opportunity to exit from the labor market (even without the influence of others), where dual-headed households were more "mainstream," and perhaps more influenced by community norms around the appropriate responses to Mincome. Likewise, it is reasonable to expect young people, considering decisions about education for example, to be disproportionately sensitive to peer influences. Further, we extend our interpretation of the circumstances of singles to those with no high school degree or with unemployment or welfare experience, where labor market exits happen with or without the community context. By contrast, like dual-headed families, those with high school degrees see large community effects.[24]

Overall, two points stand out clearly in the qualitative evidence above. The first is the diversity in the accounts. Mincome's flexibility in responding to diverse social needs, working-class needs, needs of the poor, students, parents, people at different life stages, and people facing various kinds of uncertainty, bears on its appeal to a broad segment of the population. The basic exit option, discussed in section 4.1, was valuable to a range of people with high barriers to labor market participation, meager returns to work, or now-feasible alternatives. The second is the absence of a rationale that resembles the conventional interpretation of the labor-leisure model. Historically, the labor-leisure model would likely define spending "a year at home with my children" as opting for "leisure" (i.e. Metcalf 1973), even though this is far from the intuition it conveys. Though at the highest level of abstraction this model may be applicable to a diversity of cases, in section three we framed the intuition broadly, in

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23   In both questions, the remainder is split roughly equally between "hardly ever," "occasionally," "often," and "always."

24   This dichotomous interpretation is less relevant to parents and households between 30 and 50 years, both of whom include more varied populations.

terms of an individual-level "exit option." The exits that appear in qualitative comments do not follow the conventional intuition. It is true that people might be reluctant to share information that paints them in a negative light; however, no qualitative account provides any evidence, even if stretched, which could be construed as documentation of the "Malibu surfer" (Van Parijs 1995). Instead, the diverse individual reasons to exit the labor force—some of which may have become increasingly socially acceptable due to Mincome's social milieu—may be more closely connected to care work, early retirement, leave related disability or illness, or educational investments.

DISCUSSION

This paper places emphasis on the multiplicity of mechanisms that must be accounted for in any analysis of the effects of a transformative program like basic income. However, recognizing the variety of mechanisms identified here add a degree of contingency to the results. For example, "Karelis effects" imply that basic income could increase the supply of labor for certain people. This mechanism is more likely to operate in a world where unemployment is increasingly explained by poorly remunerated jobs. On this score, trends over recent decades are contradictory: more men, but far fewer women, earn poverty-level wages in the U.S. and Canada (Jackson 2010; Mishel et al. 2012). Other mechanisms are less ambiguous. In the 1970s, the poverty traps associated with the means-tested welfare system were deeper than they are today. Labor market participation increases attributable to eliminating 1970s-style poverty traps—when many benefits were reduced by one dollar for each additional dollar of earnings—would likely be *weaker* in the context of the current assortment of social policies in the U.S. and Canada (for overviews, see Béland and Daigneault 2015; Lang 2007; Lightman 2003; Simpson 2015). On the other hand, at present, significant numbers of discouraged and unemployed workers suggest that many individuals would happily enter labor markets if opportunities emerge as others reduce hours. Further, there may be fewer women today inclined to exit labor markets to care for children at home, even if there may be more people overall motivated to care for elderly parents.

Another inevitable problem for generalizability concerns the gap between a small rural town and contemporary labor markets. In spite of that, the precarity of Dauphin's seasonal labor market bears a certain resemblance to the contemporary world of work (see Kalleberg 2009; Standing 2011).

Concerns such as these ought to be at the fore of any debate over the generalizability of particular results. As noted above, the typical strategy in economic analyses of NIT studies and other experiments tends to go as follows: (1) document an experimental effect (if one exists); (2) attribute the effect to the experimental treatment; and (3) attribute the experimental treatment to one individual-level mechanism, usually formalized in a labor-leisure model. The array of crosscutting mechanisms is excluded by design and by assumption. By contrast, we use a more multi-dimensional conception of social action and interaction, which involves a comprehensive set of micro and macro mechanisms that make attribution of relative weights difficult. Though the narrow approach does have the virtue of mathematical tractability, the broader sociological issues do not go away by simply excluding them from discussion. While it is difficult to specify the weights of particular mechanisms, the contribution of this paper has been to identify the macro-level component of the problem, and clearly differentiate it from the micro component.

This paper finds that a modestly sized basic income generated a moderate 11.3 percentage point reduction in labor market participation among program participants, 30 percent of which can be attributed to social interaction effects. This is far from the social disintegration scenario predicted by some critics of the proposal. First, it should be recalled that these are responses by program participants. People who participated in the program at some point constituted 18–20 percent of the town and lived among a majority of families that did not participate. Second, the finding that young and single-headed households rather than dual-headed households drove work withdrawals bears on the moral and pragmatic interpretation of this result. After all, youth and singles were more likely to be:

(1) considering education or training; (2) single parents engaged in care-work activities; or (3) elderly or in poor health.

However, there is a deep ambivalence in much of the literature advocating basic income. On the one hand, a key argument given in favor of universal income maintenance programs is connected to the objective of freeing people from the drudgery of unpleasant work. Nevertheless, the same advocates are often eager to find evidence showing that income guarantees do not reduce participation in the formal labor market. From our perspective, a desirable anti-poverty policy ought to be organized around the objective of reducing toil. Three decades ago, Adam Przeworski (1986) framed the matter bluntly: "Certainly making people toil unnecessarily, just so they can be paid something without others complaining and so they will not hang around with nothing to do, is to substitute one deprivation for another" (p. 696).

Still, it is reasonable for basic income advocates to be anxious about a scenario where work reductions diminish the underlying revenue source for the program. However, moderate declines in labor market participation should not trigger this anxiety. This is true not only because a moderate fall in participation rates will not undermine the revenue base supporting redistributive efforts. It is also true because a host of important activities from care work and education to community engagement and artistic endeavors may be unleashed when people's basic needs are secured outside the market. These alternate activities may be highly socially productive and may improve our collective wellbeing, even though, as sources of wealth, they are not tracked well by conventional income statistics.[25] This flip side of the Mincome experience is a key question that must be incorporated in any full accounting of its costs and benefits. The potential for investigation into the full social costs and benefits is the real value added of community-level experimentation. While no experiment can answer once and for all the deep questions posed by the broad reshaping of the work-income relationship, the Dauphin experiment could prove to be an important piece in the puzzle.

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