

**When Poverty Disappears:  
Investigating Manitoba's Basic Annual Income Experiment**

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## Extended Dissertation Abstract

This dissertation examines the impact of an understudied social experiment from the late 1970s called the Manitoba Basic Annual Income Experiment, or Mincome. Mincome tested a three-year guaranteed annual income (GAI) program where participants had the option to access payments equivalent to about \$19,500 for a family of four. While Mincome took place in three sites, this study focuses on one, the so-called “saturation” site located in the town of Dauphin, Manitoba, where eligibility was open to all town residents. The other two components were set in rural Manitoba and Winnipeg (the rural and urban “dispersed” sites). In contrast to the saturation approach, they were modeled on prior U.S. experiments and tested groups of randomly assigned treatment subjects against controls. These provide key comparisons for Dauphin. However, it was the saturation treatment that was unprecedented: never before or since has a rich country tested a GAI at the level of an entire town.

What would happen to a town if poverty were eliminated? What would happen if a community could achieve genuine economic security in the form of universalistic cash payments? This dissertation is divided into four standalone papers, assessing the following sets of empirical consequences: 1) labor market participation; 2) social stigma and community experiences; 3) domestic assault and gender dynamics; and 4) the economic and political response from businesses.

Mincome produced an immense amount of raw data, much of which was left unanalyzed in Archives Canada facilities. The experiment was underfunded, but rather than reducing payments to households, it was the analysis side of the project that was downsized. Mincome did not deliver a final report, and while some of the Winnipeg data was digitized, the survey data on the Dauphin experience was left, in large part, in hard copy form. The last three papers herein



correspond to three surveys, which I have digitized: a qualitative survey on community experiences; a survey of married women; and a survey of local businesses. The first paper makes use of panel data on labor market participation that was digitized by original researchers. Finally, I supplement these sources with additional, already digitized baseline and panel data, administrative data on crime and violence, aggregate census data, and historical archival data including letters, speeches, reports, and news articles, from a variety of archives.

The first paper inquires into the “social context” of work during the Mincome experiment, and asks the following: Would people work less if their basic needs were guaranteed outside the market? A community-level experiment accounts for the fact that people make decisions in a social context, not in isolation. Using hitherto unanalyzed data I 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, I show that work 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.

The second paper examines the impact of Mincome on social stigma. Drawing on archived qualitative participant accounts I show that the design and framing of Mincome led participants to view payments through a pragmatic lens, rather than the moralistic lens through which welfare is viewed. I find that, consistent with prior theory, Mincome participation did not produce social stigma. More broadly, this paper bears on the feasibility of alternative forms of socioeconomic organization through a consideration of the moral aspects of economic policy. The social meaning of Mincome was sufficiently powerful that even participants with particularly negative attitudes toward government assistance felt able to collect Mincome

payments without a sense of contradiction. By obscuring the distinctions between the “deserving” and “undeserving” poor, universalistic income maintenance programs may weaken social stigmatization and strengthen program sustainability.

The third paper examines the impact of Mincome on domestic violence and gender relations. The paper is motivated by an empirical finding: Using aggregate town-level crime data, I find that during the Mincome years, Dauphin saw a large fall in violent crime, most of which are assaults taking place within the family. To understand the causal pathways behind this fall in violence, I analyze an archived survey of married women, which was conducted at a baseline and during the study period, and was completed by both treatment and control groups. The survey facilitates investigation into three potential mechanisms. The first explores the link between Mincome and reductions in financial stress, disagreement, and conflict between partners; the second explores the link between Mincome and the ability of women to exit bad or potentially abusive marriages (thereby reducing exposure to violence); and the third explores the possibility that Mincome reduced violence by reshaping underlying power relations between partners. I find the most support for the first mechanism. The guaranteed income may have reduced financial stress and disagreement, thereby reducing the likelihood of domestic assault.

The fourth and final paper examines the impact of Mincome on local business in Dauphin and control towns. How do firms react when the whole labor force has access to a comprehensive guaranteed income? In light of the history of guaranteed income proposals, prevailing wisdom suggests that the business response is mixed: certain blocks of capital are staunchly opposed, others supportive. Compounding this ambiguous history are two competing theoretical perspectives. One suggests that the guaranteed income harms business interests by providing alternatives to workers, tightening labor markets, and pulling wages up. The other

view argues that the guaranteed income is an employer subsidy, facilitating low wages and a “low-road” industrial strategy. Using an archived raw survey of local firms—essentially a census of all businesses in Dauphin as well as in seven control towns at a baseline and during the study period—I analyze the effect of the guaranteed income on hiring, applications, work hours and, in particular, wages. I find a large increase in wages offered by firms in Dauphin, and little change in the control towns. By providing an exit option from work, the guaranteed income may improve the power of workers vis-à-vis employers, enabling them to negotiate higher wages. In order to contextualize these conclusions, this paper draws on a range of historical archival materials and reconsiders the history of business reactions to guaranteed income proposals in North America, providing an interpretation consistent with the above findings. The proposal details matter. Guaranteed income schemes that facilitate exploitation—those that provide minimal benefits, implement “two tiers” of support, or retain work requirements—may inspire business support. Proposals like Mincome, which curb exploitation and provide generous, universalistic payments with no work requirements, tend to fuel opposition from business.

## Acknowledgements

Falling in love with one's subject matter is a garden-variety academic danger—prolonged exposure can have that effect. But I did not intend to write a paean to basic income. My default mode is ruthless criticism, not reckless boosterism. From my perspective, cash incomes are purely instrumental, serving a broader normative commitment about which I am reckless: to human flourishing, real freedom, and a good society. If basic income does not serve those ends I am not interested in defending the idea.

I do, however, think that despite its drawbacks and colossal uncertainties, it is a good idea. This dissertation is an attempt, however partial, to fill in the giant lacuna of knowledge about its real-world application. There are many people to thank, all of whom, however, must be absolved of blame for any errors herein.

My advisor, Erik Wright, has been a wonderful guide through graduate school. He has been a fantastic critic, reader, and identifier of sloppy argumentation. When I met Erik I was an orthodox Marxist. On the other end of graduate school, I am still a Marxist, but a more ecumenical one (and hopefully an analytical one, though that is perhaps up to the reader). To a considerable extent, Erik is responsible. The logic crushes all resistance; I could only retain my dialectical twirling so long. Erik's influence shows up only in a few places in this dissertation, but he has infected much of my thinking about the world. I may forever have a tiny, cartoon ghost of EOW hovering over my shoulder when I'm constructing an argument.

Much of this dissertation strays from the tone and approach of Marxist academic work. However, my underlying interest in basic income is rooted in a deeply Marxist instinct. BI provides an alternative to the dull compulsion of economic relations; as an alternative it decommodifies labor-power and makes working for a capitalist a genuine choice, not an

economic necessity. Rejiggering a fundamental asymmetry in power between workers and bosses and eroding the background condition of market dependence are the core features of basic income that animated my original interest, and they continue to motivate this work. For example, I support but am less enthusiastic about policy proposals that look similar to basic income, but do not affect the distribution of power and the availability of alternatives to the market. I endorse the Earned Income Tax Credit, and similar policies, because they improve people's lives; however, they do not strengthen working people when facing off against their bosses, nor do they facilitate exit from relations of exploitation. They are ameliorative reforms, and therefore desirable, but they are not emancipatory reforms, to use Erik's language.

Marx maintained that philosophers have hitherto attempted to understand the world; the point, however, is to change it. Alvin Gouldner cautioned that the deep desire to change the world can corrupt our ability to correctly understand it. With this in mind, and with a few (unstated) caveats, I believe that there is a case to be made for an analytical picture of basic income as a social policy that paves a path toward broader social transformations. In particular, BI can help set in motion a dynamic process that empowers people to struggle to build a better society. It achieves this in two ways: the power of exit and the institutionalization of solidarity. The former allows poor and working people a better position to bargain from, instigating broader and more far-reaching gains; the latter, by reducing stigma and social exclusion, improves the odds that they do so collectively rather than individually.

Much about this dissertation was accidental. Entering graduate school I did not know what basic income was (I first learned about the idea in a seminar with Erik and Harry Brighthouse). I certainly knew nothing about the history of guaranteed annual income experiments conducted in the 1960s and 1970s. Having already selected my dissertation topic I was somewhat astonished

to learn that my office just so happened to be located next door to the Institute for Research on Poverty (IRP), an organization set up by the U.S. Office of Economic Opportunity to analyze the American guaranteed income experiments. I looked up from a book one day—Alice O'Connor's 2001 *Poverty Knowledge*, which traces the origins of the IRP and the GAI experiments—with the full realization that the U.S. experiments were hatched, supervised, and analyzed, not only from Madison, but literally from down the hall.

Equally coincidental was my data access. When I spoke to Evelyn Forget, who encouraged me to look at the Mincome files, I was very fortunate to discover that Archives Canada had just two years prior organized the files and written up a finding guide to make some sense of 1,800 boxes of raw data that had for decades been sitting like a pile of unfolded laundry. Indeed, without Evelyn this dissertation would not exist. She encouraged me to dive into the archive in the first place. And she offered helpful advice and encouragement along the way, cautioning against my instinct to over-interpret and pressing me to remain sensitive to context.

Tim Smeeding, I know, disagrees with me on basic income, but his wealth of knowledge on poverty has been tremendously valuable. He has been consistently generous with his time and energy.

Bob Freeland is my favorite human. He knows everything, he's broad in his thinking, his criticisms are always deadly accurate, and his unflinchingly cynical disposition matches my own, which is why we get along.

Christine Schwartz came to the committee late but has been very helpful, especially in talking through methodological issues of which I had only a weak grasp. The parts of this dissertation that are empirically persuasive most likely benefited from Christine's guidance.

A long list of professors, colleagues, and comrades commented on early drafts, steered me away from idiotic arguments, or gave useful advice in one form or another: Markus Gangl, Sarah Halpern-Meekin, Mike Massoglia, Bob Haveman, Pam Oliver, Tatiana Alfonso, Pilar Goñalons-Pons, Aliza Luft, Matias Cociña, Molly Noble, Michael Billeaux, Asher Spencer Dupuy, Barry Eidlin, João Pechanski, Jennifer Jarman, Toni Pickard, Roberta Hamilton, Nicole Cohen, John Myles, Kenneth Nelson, Gregg Olsen, Duncan Foley, Edo Navot, Ayca Zayim, Adam Slez, Jeffrey Malecki, Francesco Bova, and Muhammad Azim. Working with Jonathan Latner on the first paper was like enrolling in a master class in code writing; I've been truly fortunate to learn from him. I would be remiss not to mention a few others, not already noted above, who have made my experience in Madison intellectually and personally rewarding: Taylan Acar, Matt Nichter, Matias Scaglione, Patrick Barrett, Rose Worden, Naama Nagar, Emanuel Ubert, Elizabeth Wrigley-Field, Lucy Traverse, Ben Stein, Trevor Young-Hyman, Daniel Blocq, Guillaume Neault, Erin Madden, Alex Hanna, Loren Peabody, and Janaina Saad.

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I have a zillion thank-yous to my family: To my parents, Linda and Ed Calnitsky, for their freakishly inexhaustible, embarrassing, and unconditional love and support, and to my sisters,

Naomi and Shauna, for their humor, conversation, and friendship. Both Naomi and Shauna have been valuable intellectual interlocutors throughout—Naomi, the human encyclopedia, providing a bottomless fount of historical knowledge, and Shauna, the human counterfactual machine, providing a springboard for countless attempts at analytical arguments. Finally, I want to thank my partner, Maddie Ritts, as well as her extremely kind, fun, and openhearted family, Morton, Max, and Zoë. Maddie has talked through virtually every one of these ideas with me, counseled me through every setback, and as a social worker and anti-poverty activist she has taught me an immense amount about my own subject matter, poverty and social assistance in Canada. Life would be grim without her.

I am sure there are errors in the dissertation, and hopefully the future will provide room for criticism, revision, and improvement. If not, the Memorial Library basement will provide a suitable venue for the gnawing criticism of the mice.



# **1. Basic Income in a Small Town: Understanding the Elusive Effects on Work**

(With Jonathan Latner)

## **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.

## 1. Introduction

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),<sup>1</sup> 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.<sup>2</sup> While there is a vibrant theoretical literature on the desirability of basic income (see debates in Wright 2006 and Van Parijs 1992), 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.

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<sup>1</sup> While GAI was the term of art used in the 1970s, BI is currently in fashion; we use them

<sup>2</sup> 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).

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; Moffitt 1981; Kenworthy 2014). Reducing work incentives and weakening economic activity would ultimately undermine the funding of a basic income.<sup>3</sup> 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

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

experience and future administration of a program where the abolition of one family's poverty coincides with its abolition as such.

No published research has analyzed the experiment's own qualitative or quantitative survey data in order to understand the experience in Dauphin. 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.<sup>4</sup> 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

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<sup>4</sup> 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](http://stats.oecd.org)).

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 outlines 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.

## **2. 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 (Hum 1983; Haddow 1993). 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 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 et al. 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 et al. 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). 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—or about 38 percent of Dauphin’s median family income in 1976.<sup>5</sup> 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 ( $19,500 - 6,000 \times 0.5$ ) leaving your final income at \$22,500 ( $16,500 + 6,000$ ).<sup>6</sup> 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).

<Table 1 about here>

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<sup>5</sup> All dollar figures are quoted in 2014 Canadian dollars.

<sup>6</sup> Note that this scheme is distributionally equivalent to households keeping the full Mincome payment and facing a steeper progressive income tax on total income.

At least 706 Dauphin households—or about 2,128 individuals—received benefits at some point throughout the program.<sup>7</sup> 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 time. As Figure 1 shows, household participation in the Mincome experiment steadily increased each month until the participation decline near the program’s end.

<Figure 1 about here>

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

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<sup>7</sup> 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.



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 recalled that “[t]he data, it was hoped, would at least be preserved for analysis at a later date” (1985: 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 1993; Simpson and Hum 1991; 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 (Simpson and Hum 1991). Though no published research has examined the survey records on Dauphin, recently Evelyn Forget (2011; 2013a; 2013b) 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.

### **3. 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 et al. 2002). 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; Hedström 2005; Garfinkel et al. 1992; Przeworski 1974).

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 et al 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 et al 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. 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 variety of 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.

<Figure 2 about here>

#### **4. 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.<sup>8</sup> 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

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<sup>8</sup> 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.

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.

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.

<Table 2 about here>

#### *4.1 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.<sup>9</sup> 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; Rodrik 2008; and Reddy 2012).

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 1996). Secondary earners, often female, in dual-headed households might also be inclined to reduce work, perhaps to engage in unpaid care work

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<sup>9</sup> As Irwin Deutscher put it, the "basic idea of this massive experiment is to determine how much able-

(Connelly 1992; Gornick et al 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 (Hanoch and Honig 1983; Bazzoli 1985; Myles 2002; Schur 2003). Younger people might reasonably choose education and training over the labor market (Maynard and Murnane 1979; McDonald and Stephenson 1979; Weiss et al. 1980; Rea Jr. 1977; Venti, 1984; Forget 2011). 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 et al. 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.

An entirely different kind of mechanism, still operating at the individual level, comes from philosopher Charles Karelis (2007; for analogous conclusions with different behavioral premises, see Mullainathan and Shafir 2013; and Shah et al. 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

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bodied people will goof-off if their income is maintained at a poverty level" (1979: 237).

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 this 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 Sheppard 2010; Caniglia 1996; Moffit 1992; Moffitt 2002). This dynamic may have been particularly strong in North American welfare programs in the 1960s and 1970s (Danzinger, 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.

## *4.2 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 feed back 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 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.<sup>10</sup>

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 Widerquist 2005; Harris 1985; Kurz and Spiegelman 1973).<sup>11</sup> 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

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<sup>10</sup> 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.

<sup>11</sup> 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.

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 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, Mullin and Scholz 2001; Moffit 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 Bruegel et al. 1998; and Bosch and Lehndorff 2001). 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.<sup>12</sup> 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 (Wright 2004, 2006; Purdy 1988; Lucarelli and Fumagalli 2008). 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.

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<sup>12</sup> 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.

## 5. 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.

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.<sup>13</sup> 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.<sup>14</sup> 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.<sup>15</sup> 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

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<sup>13</sup> 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.

<sup>14</sup> 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.

<sup>15</sup> We use the standard difference-in-difference regression form:  $Y_{it} = \beta_0 + \beta_1 (Treatment_i) + \beta_2 (Study Period_{it}) + \beta_3 (Treatment_i \times Study Period_{it}) + \varepsilon$ . Considering households  $i$  at survey time  $t$ ,  $Treatment_i$  indicates Mincome status (1) or, in the first case, control status (0);  $Study Period_{it}$  identifies information from either the study period (1) or the baseline (0);  $Y_{it}$  is labor market participation; and  $\varepsilon$  is the error term. The interaction term,  $Treatment_i \times Study Period_{it}$ , is the focal variable, where  $\beta_3$  captures the difference-in-difference, or treatment effect.

captures the social interaction effect in Dauphin.<sup>16</sup> 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.

## 6. 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 experiment), 1976 (the middle year of the experiment), and 1981 (after the experiment).<sup>17</sup> We tabulate the labor force participation rates in Dauphin and Manitoba (minus Winnipeg and Dauphin).<sup>18</sup> 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

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<sup>16</sup> 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.

<sup>17</sup> Census data is constructed from a 33 percent sample database from the long form census questionnaire.

<sup>18</sup> 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.

elderly people are less likely to work—across the two geographic constructs.<sup>19</sup> This compares our geographies *as if* both Dauphin and Manitoba groups had the same age distribution (see Treiman 2009).

<Figure 3 about here>

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.<sup>20</sup> 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.

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<sup>19</sup> Indeed, on average, Dauphin was aging more quickly than Manitoba. Adjusting for education as well as age generates similar findings.

<sup>20</sup> Female labor market participation is less clear-cut; however, Dauphin's unadjusted participation rate is slightly lower than Manitoba's during the experiment, though this disappears after adjusting for age.



<Figure 4 about here>

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 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.

<Table 3 about here>

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.

<Figure 5 about here>

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; 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.

## **7. 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.

A one-off survey on “Community Experience,” completed at the midpoint of the experiment provides useful qualitative information (see details in Table 5) 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.<sup>21</sup> 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 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

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<sup>21</sup> We take a broad rather than narrow interpretation, viewing, for example, comments about spending

percent of participants said “never.” Likewise, 93 percent of participants said they “never” felt “embarrassed or uncomfortable ... with people who are not on Mincome.”<sup>22</sup> 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

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more time with children or going to school as implying an element of work withdrawal.

<sup>22</sup> In both questions, the remainder is split roughly equally between “hardly ever,” “occasionally,” “often,” and “always.”

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.<sup>23</sup>

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

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<sup>23</sup> This dichotomous interpretation is less relevant to parents and households between 30 and 50 years,

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 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.

## 8. 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 (Mishel et al. 2012; Jackson 2009). 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

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both of whom include more varied populations.



be *weaker* in the context of the current assortment of social policies in the U.S. and Canada (for overviews, see Lang 2007; Lightman 2003). 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 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” (1986, 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.<sup>24</sup> 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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<sup>24</sup> One account from the Bureau of Economic Analysis calculates that U.S. GDP in 2010 would be 26 percent higher after accounting for unpaid household production (Bridgman et al. 2012).

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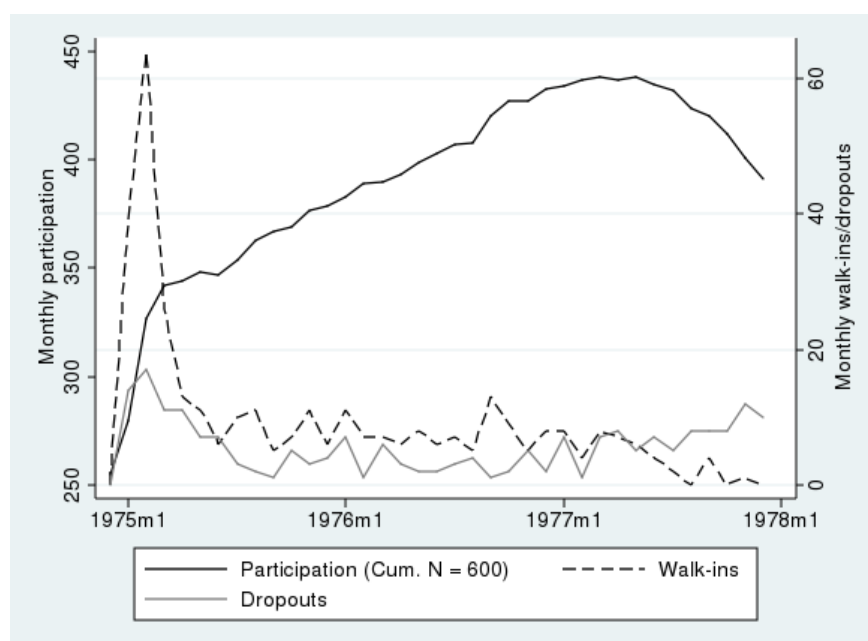
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## FIGURES AND TABLES

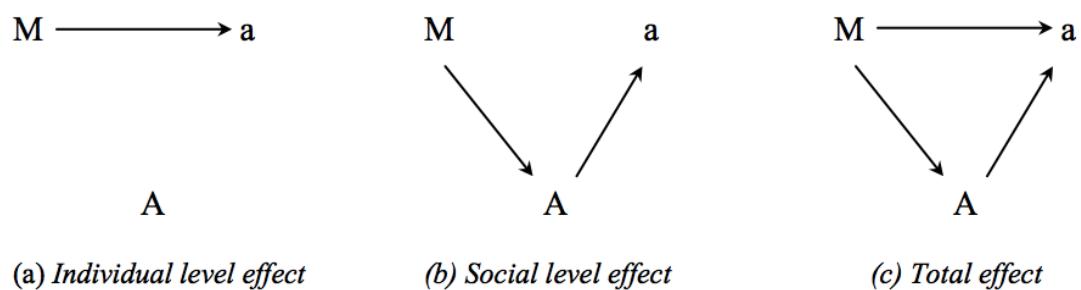
MARKET INCOMES	MINCOME PAYMENTS	POST-MINCOME INCOMES	PERCENT OF POSITIVE TAXES REBATED
0	19,500	19,500	100
6,000	16,500	22,500	100
12,000	13,500	25,500	100
18,000	10,500	28,500	100
24,000	7,500	31,500	100
30,000	4,500	34,500	100
36,000	1,500	37,500	100
39,000	0	39,000	100
41,200	0	41,200	50
43,400	0	43,400	0

NOTE: 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.

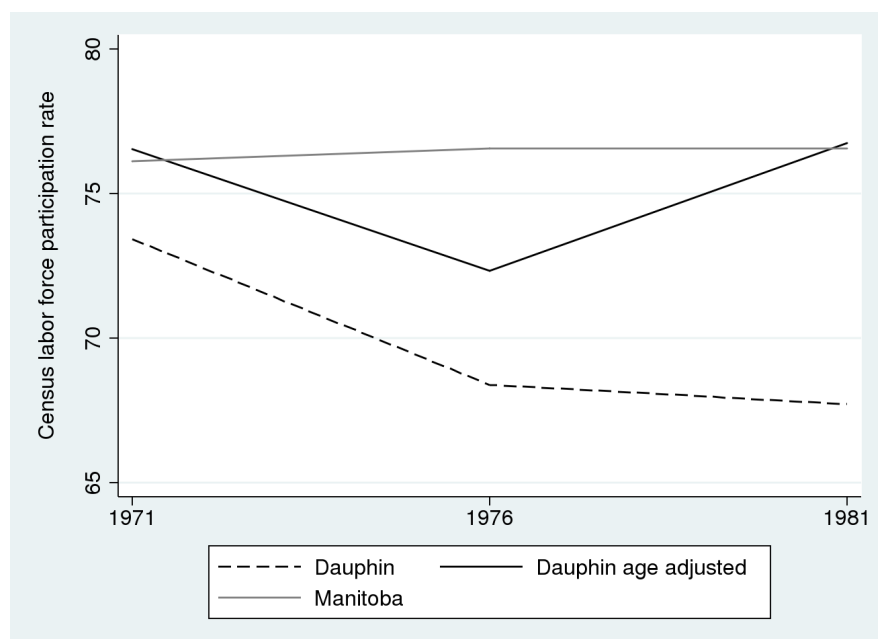
**Table 1.** Illustration of impact of Mincome for a family of four



**Figure 1.** Total Mincome households, “walk-ins,” and “drop-outs” each month in Dauphin



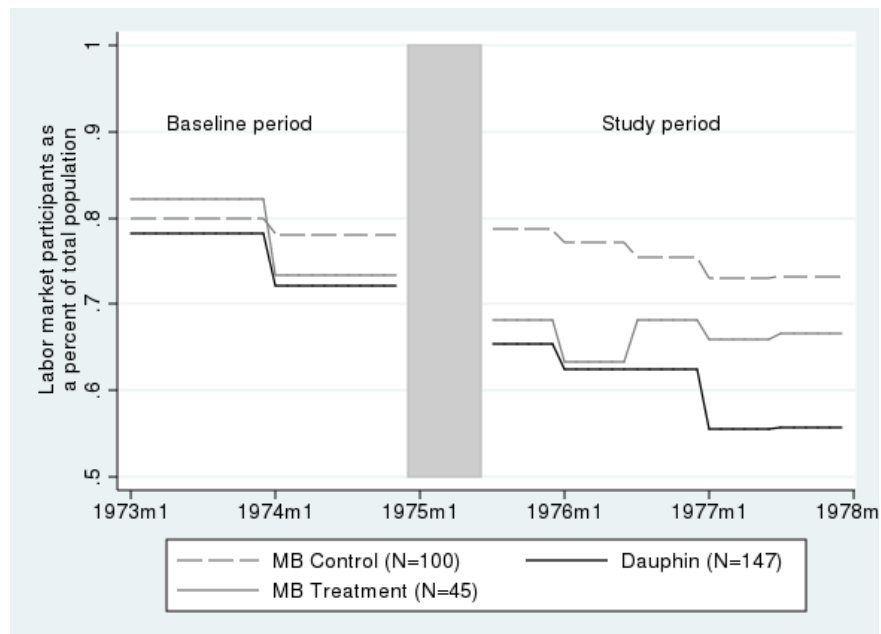
**Figure 2.** The effect of Mincome, M, on individual treatment subject a, given the presence and absence of neighbor/community, A



**Figure 3:** Male labor force participation in Manitoba, Dauphin, and Dauphin adjusted for age

		FACTORS THAT SHOULD...	
		INCREASE WORK	DECREASE WORK
<b>INDIVIDUAL EFFECTS OBSERVABLE IN ALL NIT STUDIES</b>	BASIC EXIT OPTION:		X
	REMOVAL OF MEANS-TESTED POVERTY TRAPS:	X	
	KARELIS EFFECTS:	X	
<b>SOCIAL-INTERACTION EFFECTS OBSERVABLE ONLY IN MACROEXPERIMENTS</b>	UNDEREMPLOYED FIND WORK MORE EASILY AS	X	X
	'OVEREMPLOYED' REDUCE WORK HOURS:		
	RISING WAGES AT THE BOTTOM	X	X
	(THROUGH MULTIPLE MECHANISMS):		
	COMMUNITY CONTEXT AND STIGMA EFFECTS:		X

**Table 2.** Theories of labor market effects of basic income



**Figure 4.** Labor market participation in Dauphin, Manitoba dispersed treatment, and control groups, during baseline and study periods

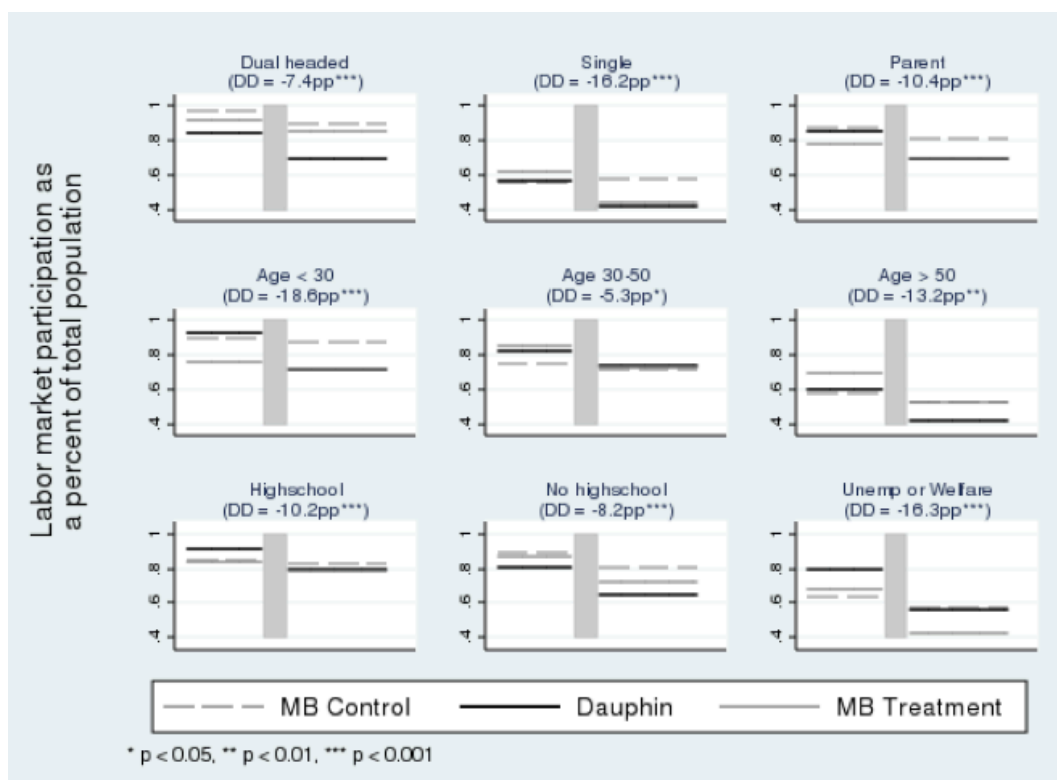
	MANITOBA CONTROL	DAUPHIN SATURATION TREATMENT	MANITOBA DISPERSED TREATMENT	DIFFERENCE 1: EXPERIMENTAL EFFECT (DAUPHIN – MB CONTROL)	DIFFERENCE 2: COMMUNITY EFFECT (DAUPHIN – MB TREATMENT)
	b/(se)	b/(se)	b/(se)	b/(se)	b/(se)
<b>LMP baseline (2 year average)</b>	79.0% (0.008)	75.3% (0.007)	78.0% (0.013)	-3.7%*** (0.011)	-2.7% <sup>†</sup> (0.015)
<b>LMP study period (3 year average)</b>	75.7% (0.008)	60.6% (0.007)	66.4% (0.013)	-15.1%*** (0.011)	-5.8%*** (0.015)
<b>Change in LMP</b>	-3.4%** (0.011)	-14.7%*** (0.01)	-11.6%*** (0.018)	-11.3%*** (0.015)	-3.1% <sup>†</sup> (0.021)
<sup>†</sup> (p<0.15), * (p<0.05), ** (p<0.01), *** (p<0.001)					

**Table 3.** Average labor market participation at the baseline, study period, differences within time periods, differences within groups, and difference-in-differences

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%

**Table 4.** Baseline characteristics in Dauphin, Manitoba treatment, and Manitoba control





**Figure 5.** Labor market participation disaggregated by subgroup

"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%
<b>Subtotal</b>	<b>267</b>	<b>82.9%</b>
Indication of potential work reduction in the future:		
If unable to work/in case of illness/security .....	20	6.2%
<b>Subtotal</b>	<b>20</b>	<b>6.2%</b>
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%
<b>Subtotal</b>	<b>35</b>	<b>10.9%</b>
<b>Total comments</b>	<b>322</b>	<b>100.0%</b>

Note: 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.

**Table 5.** Reasons provided for joining Mincome

## **2. “More Normal than Welfare”: The Mincome Experiment, Stigma, and Community Experience**

### **Abstract**

This paper examines the impact of a social experiment from the 1970s called the Manitoba Basic Annual Income Experiment (Mincome). I examine Mincome’s “saturation” site located in Dauphin, Manitoba, where all town residents were eligible for guaranteed annual income payments for three years. Drawing on archived qualitative participant accounts I show that the design and framing of Mincome led participants to view payments through a pragmatic lens, rather than the moralistic lens through which welfare is viewed. Consistent with prior theory, this paper finds that Mincome participation did not produce social stigma. More broadly, this paper bears on the feasibility of alternative forms of socioeconomic organization through a consideration of the moral aspects of economic policy. The social meaning of Mincome was sufficiently powerful that even participants with particularly negative attitudes toward government assistance felt able to collect Mincome payments without a sense of contradiction. By obscuring the distinctions between the “deserving” and “undeserving” poor, universalistic income maintenance programs may weaken social stigmatization and strengthen program sustainability.

## 1. Introduction

In the 1960s and 1970s both the American and Canadian governments launched among the most innovative and large-scale social experiments ever attempted. Five separate guaranteed annual income (GAI) experiments were implemented to test the mechanics of a revolutionized social policy that ensured a basic standard of living to all. The field studies involved huge expenditures of money, time, and human energy. This expense—particularly in the Canadian experiment—must be considered that much greater next to the somewhat limited contribution to knowledge they produced. Much was learned about the labor supply response (for summaries of the experiments, and the typically modest reductions in work effort, see Keeley 1981; Burtless 1986; Hum and Simpson 1993; Levine et al. 2005; and Widerquist 2005) and to a lesser extent about marital dissolution (Cain 1986; Cain and Wissoker 1990; Hannan and Tuma 1990). More recently Evelyn Forget has examined the health effects of the GAI in the Canadian context (2010; 2011; 2013). However, the original research agenda was cast in fairly narrow terms (Rossi and Lyall 1976; Haveman 1997).<sup>25</sup> Questions concerning social inclusion, social solidarity, and the well-being of communities were sidelined by researchers, as were considerations of the extent to which “welfare’s” social stigma was reproduced under a universalistic social policy.<sup>26</sup> In other words, there was little interest in what John Rawls called “the social bases of self-respect” (2009: 54). It is natural that a program distributing large amounts of money to diverse groups of people without work requirements would forefront the effects on work. However, achieving a decent standard of living might affect people in

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<sup>25</sup> Social consequences were often condensed into chapters covering “Noneconomic outcomes” (Hannan 1978), “Non-labor-supply-responses” (Hanushek, 1987), or “Non-labor Supply Experimental Responses” (Rossi and Lyall 1976). However, final reports for the U.S. experiments did include chapters reporting some small social psychological effects (Middleton and Allen 1977; Ladinsky and Wells 1977).

<sup>26</sup> I use the terms “welfare” and social assistance interchangeably.

significant and subtle ways that are poorly summarized by their propensity to reduce work hours by a few percentage points.

The labour supply results are important but tell us little about the actual people populating these studies, how they understood the program, how it impacted their experience of community life, and whether participation came with social-psychological costs. Most of the GAI studies did not collect much information on the motivations and experiences of the human subjects involved. As Lee Rainwater (1986) pointed out in the wake of the American experiments, there is a major “black box” element to these studies. We often know what goes in and what comes out, but it is never clear what was actually going on in between. We do not know how the guaranteed income was perceived in the context of community life, what motivated participants to join, and how community members and participants interpreted it relative to traditional means-tested social assistance. We do not know whether it provided benefits without stigma, as was often hoped and hypothesized (Adams 1971; Canada 1971a; Moynihan 1973; Tobin 1968; Offe 1992). Perhaps a reflection of the methodological preferences and prejudices of the time, the most rudimentary forms of qualitative description were largely absent from the GAI data. A descriptive account of experiences of individuals can put flesh on the bones of statistical findings.

Moreover, the consideration of community experiences and the moral interpretation of social policy bear on the question of political feasibility, which is sometimes absent from the overall feasibility analyses of social policy alternatives (e.g. Munnell 1986). In the name of economic feasibility, social policies may impose stigma costs on recipients. However, these policies may also be self-defeating if their unpopularity undermines their political feasibility. “Welfare” in North America is the paradigmatic case of a stigmatizing program whose overall

social and moral reception may have undermined its long run sustainability. Both in Canada and the U.S. welfare programs had shrunk and acquired more stringent conditions by the late 1990s (Danziger 2010; Peck 2001). Social policies that spotlight the moral quality of the poor, ones that hinge on the worthiness or unworthiness of recipients, may be less likely to be endorsed by the public. There is evidence that people's perception of the moral virtue of the poor (rather than class position alone) is a good predictor of their support for generous forms of redistribution (Williamson 1974; Moffitt, Ribar, and Wilhelm 1998; Fong 2001). Policies that take the question of the motivations and morality of the poor off the table may be more robust. Human beings, after all, are moral creatures.<sup>27</sup> As such, understanding the political feasibility of policies like the guaranteed annual income involves a consideration of the moral aspects of economic policy.

It is often argued that universalistic social policy produces solidarity and resilience, where income-tested or targeted social programs produce stigma and fragility (Korpi and Palme 1998; Brady and Bostic 2015). However, concrete social policies, including the guaranteed annual income, do not always fit well into this dichotomy. I reframe the hypothesis using qualitative survey data from the Manitoba Basic Annual Income Experiment (Mincome), a three-year (1975–1977)<sup>28</sup> experimental guaranteed annual income, which included a “saturation” site, the town of Dauphin, Manitoba, where all town residents had the option to collect payments.<sup>29</sup> Mincome was technically an income-tested program—to collect payments one's income must fall below a certain threshold—though it is best described by Theda Skocpol's (1991)

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<sup>27</sup> For summaries of the observational and experimental evidence on the complex relationship between material interests and moral sentiments, see papers in Gintis et al. (2005).

<sup>28</sup> There is some ambiguity around these dates; while payments were made between December 1974 and December 1977, Mincome staff began interviewing families before this period and remained in place for some time after.

<sup>29</sup> Though no analysis of the Dauphin portion of Mincome was completed in the wake of the experiment, recently Evelyn Forget (2011) has renewed public interest in the Dauphin sample. Using Manitoba Health data, Forget showed that relative to controls Dauphinites saw a reduction in hospitalization rates during the Mincome years.

expression, “targeting within universalism,” because it had strongly universalistic features: if *anyone’s* income fell below the threshold for whatever reason, they were eligible for payments.

Instead of emphasizing universalism per se, this paper argues that the moral reception of social programs pivot on (1) *the degree to which groups are treated differently or similarly*, (2) *the degree to which payments are automatic or open to discretion*, and (3) *the program’s semi-independent moral framing*.

First, Mincome’s design meant that typically separated groups were treated under a unified scheme, thereby facilitating a universalistic ethos and a broad appeal. Particularly salient was the absence of special rules for special categories of people—especially regarding work—that ultimately exclude some from the mainstream activities of life. By blurring lines of demarcation among low-wage workers, unemployed workers, and social assistance recipients, the guaranteed income was less likely to be interpreted as a program for “other” people. The program’s broad applicability provided a kind of ideological cover to participants, allowing them to sidestep typical constructions of social assistance receipt. This breadth facilitated a range of explanations to choose from when people explained their participation. Second, Mincome improved people’s incomes somewhat automatically, without subjecting them to invasive and degrading caseworker discretion. Finally, Mincome’s ideological framing by official sources impacted its social reception: the program tended to be framed as a contribution to science and as a program beneficial to “all Canadians.” This weakened the archetypal portrayal of social assistance as a form of “dependence” that encourages moral deficiency, low motivation, or cheating (Canada 1971b; Rainwater 1982; Fraser and Gordon 1994; Yoo 2008).

Together, these features reduced the social-psychological costs of social assistance. The program was, according to one participant, “more normal than welfare.” In stark contrast to

comments from welfare participants, Mincome's framing and design made it easy for participants to cite a variety of casual, pragmatic, or seemingly incidental reasons for participation—often failing to mention any actual or potential material benefits at all. Explanations frequently refer to “curiosity” or a desire “to help with research.”

I argue that the basic material benefits and design of the scheme facilitate and interact with ideological (or non-material) factors to explain why Mincome participants enrolled, and likewise, why they felt it was superior to welfare, which the majority resolutely refused to consider joining. Finally, I show that the social meaning of Mincome was powerful enough that even participants who themselves had particularly negative attitudes toward social assistance—people who opposed welfare on moral grounds, who saw welfare recipients in a negative light, and who believed strongly in the principle of earning one's own living—felt able to collect Mincome payments without a sense of contradiction. A man who wrote that “Welfare to me was accepting something for nothing” joined Mincome because it “Would be a benefit to me at some time.” Although it was a government assistance program, which targeted income disproportionately to the poor, survey respondents typically viewed payments in a pragmatic rather than a moralistic light. This paper examines the material and ideological factors behind people's participation in Mincome in order to locate the sources of this distinctive social meaning.

## **2. A Brief History of an Unusual Experiment**

The early GAI debates in Canada were shaped by influential reports from the Economic Council of Canada (1968) and the Department of National Health and Welfare (1970), which addressed, perhaps for the first time, the multidimensional problem of poverty. They made appeals to

evaluate the merits of a national GAI program. The Economic Council report cautioned that an exclusive focus on groups in the deepest poverty would fail to deal adequately with the problem; this would “neglect unduly the very considerable group whose poverty problems are associated not with an absence of earnings, but with an insufficiency of earnings” (1968: 113). The discussion came to centre on the income security of the working poor and the objective of extending welfare to new groups (Smith 1965; Leman 1980).

Between 1968 and 1973 the guaranteed income was ubiquitous in Canadian policy debates (Haddow 1993). Senator David Croll conducted an influential inquiry into poverty in Canada in 1971, concluding with a call for a comprehensive guaranteed income to supplant means-tested social assistance. Several staff members defected in protest from the committee, believing the chair to be insufficiently radical (see McCormack 1972). They produced their own *Real Poverty Report* (Adams et al. 1971) later that year. The *Croll Report* defined poverty as income deficiency, while the “renegade report” defined poverty as deprivations in power and status, but both ultimately recommended a sweeping transformation of income maintenance policy pivoting on a guaranteed income for all. A sufficiently generous GAI could solve deficits in income as well as deficits in status.

It was in this context that Manitoba’s New Democratic Party Premier Edward Schreyer announced that, “the Government of Manitoba is explicitly dedicated to try to provide greater equality in conditions of life to the individual citizens of our province” (1971: 1–2). Schreyer linked the goal of extending welfare to the old problem of dividing up the poor into separate social categories: “If we can get around the legal—and psychological—barriers which so rigidly separates the employed from those on welfare... then surely we have made a solid step forward” (Ibid.: 4). The historic gulf between the deserving and undeserving poor is what motivated



Schreyer's comment that "the time has come to give out welfare at the unemployment office" (1971: 1).

In an interview that summer, Schreyer declared, "we feel that a GAI is necessary and inevitable" (Green, Mardon, and Werier 1971: 16). The Mincome experiment, publicly announced in February 1974 (Manitoba 1974), was a means to locate any "difficulties in small scale" (Green et al. 1971: 16). Due to considerable interest in understanding the administrative aspects of a GAI (Atkinson, Cutt, and Stevenson 1973; Hikel et al. 1974) as well as some interest early on in the "impact on the community," an important aspect of the project would be conducted in a "contiguous area" (rather than an exclusively randomized control trial format), eventually Dauphin (Schreyer 1971: 8).

Dauphinites were offered guaranteed incomes equivalent to \$19,500 for a four-person household (the guarantee varied by household size).<sup>30</sup> People earning no labour market income, for whatever reason, could access the full guarantee, 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. At a negative income tax rate of 50 percent, people could always increase their incomes by working. Every dollar of labour market earnings reduced the guarantee by 50 cents; this meant that payments phased out once earnings reached \$39,000. Positive tax liabilities were rebated too; the rebate faded to zero once market earnings reached around \$43,400.

According to the 1971 census, real median household income for Dauphin and its rural municipality was only \$24,758 and median family income was \$39,166. By the middle of the experiment in 1976, I estimate that real median household and family incomes were \$39,382 and

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<sup>30</sup> All figures are reported in 2014 constant dollars.

\$51,055, respectively. Though the program itself affects 1976 data, these figures illustrate the accessibility of benefits to diverse social segments.

In a town with a population of 8,885, along with a 3,165 person rural municipality, at least 18 percent—2,128 individuals or 706 households—received benefits at some point throughout the program (This is a lower bound because available data excludes late-joining farm families; an estimate of this group increases the participant count to 2,457, or 20 percent of the population). 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. Income was reported by mail each month, and on that basis, checks were sent to homes (see Hum et al. 1979 and Sabourin et al. 1979). Welfare, by contrast, was characterized by highly visible and special treatment. It involved frequent contact with staff who held considerable discretionary power, conducted searching investigations of recipients' resources, and sometimes made unexpected home visits. Welfare recipients normally collected payments in person. They were often referred through doctors and counsellors, used vouchers, and had services paid for by the welfare office (Canada 1971b; Barber 1972; Ryant 1983). In all, Mincome was less visible, more automatic, involved less individual discretion, and could benefit diverse social groups. As I discuss in the next section, these features helped shape Mincome's social meaning.

### **3. Rethinking Universalism and Selectivity**

There is a large literature, which argues that universal programs will be internalized as natural rights of citizenship, and programs targeted to small groups of people may be less robust over

time (Esping-Andersen 1990; Korpi and Palme 1998; Moene and Wallerstein 2001; Larsen 2008). Where broad-based programs weaken divisions between the deserving and undeserving poor, highly targeted programs are said to be actively stigmatizing. In the former case, an expanded sense of community and social inclusion are at the explanatory core of the apparent association between the universality of a program and its resilience. In the latter case, targeting and income-testing requires the poor to stand up and self-identify as poor. This spotlights a stigmatized population and leaves programs vulnerable to funding cuts (Titmuss 1968).

However, the path from targeting to stigma to program unsustainability is not straightforward. Kenworthy (2011) studied total government transfer incomes in ten countries and found universal programs to be no more robust than targeted ones. Brady and Bostic (2015) found that while transfer share is positively associated with universalism, it is not negatively associated with targeting. Marx, Salanauskaite, and Verbist (2013) found that targeting *is* associated with higher levels of spending. Stigmatizing programs may be vulnerable; however, targeted programs need not always attach stigma to participants. The thesis needs to be refined: it is not targeting as such that generates stigma and potentially weakens programs. Various examples clarify the point: The income-tested Earned Income Tax Credit in the U.S. does not appear to be stigmatizing (Sykes et al. 2015) and has steadily grown in recent years, even inspiring similar programs in Canada (the Working Income Tax Credit) and elsewhere.

The purported theoretical link between the targeting of a program and its fragility operates through the negative subjective experience of participants and the moralistic evaluation of non-participant neighbours. Though the GAI is technically income-tested, the typical evaluation of income-tested programs does not map onto it easily. I identify three ways an

income-tested or targeted program can evade these dynamics. What is at issue in a stigmatizing program is not so much whether it is income-tested *per se* but,

1. The degree to which typically divided groups are treated in a uniform manner;
2. The degree to which payments are automatic rather than determined case-by-case; and,
3. The degree to which a program is framed as morally acceptable.

The first item suggests that uniform treatment reduces the chance and severity of exclusionary practices. Programs directed to specific groups of people may emphasize group differences. The process of categorization may highlight some moral deviation, in turn giving rise to special conditions. For example, from the standpoint of social policy, do you *happen* not to work or are you tagged with a special category identifying you as a “non-worker”? GAI proposals constituted sweeping transformations of social policy primarily because they dissolved the boundaries between the deserving and undeserving poor; they weakened insider–outsider distinctions (see Katz 2013). To this it could be added that a program will be less stigmatizing the more participation there is from privileged groups. Patrick Moynihan, one of the main planners of Nixon’s Family Assistance Plan (see Moynihan 1973; Wiederspan, Rhodes, and Shaefer 2015)—a GAI proposal nearly approved in the U.S. Congress in 1970—believed that the deserving status of the working poor would purify the program of its negative association with welfare policy (Steensland 2007).

The second consideration emphasizes the automaticity of program delivery rather than the binary consideration of whether or not a program is income-tested. Income-testing is often falsely conflated with demeaning experiences with bureaucrats and caseworkers. Much of the literature on social assistance interprets the income test as characterized by demeaning

application processes, punitive sanctions, and intrusive caseworker discretion that reinforces damaging stereotypes of welfare recipients (Sandfort, Kalil, and Gottschalk 1999; Stuber and Schlesinger 2006; Handler and Hasenfeld 2007; Watkins-Hayes 2009). However, income-testing is not inherently linked to these experiences. People undergo a uniform process of income-testing when filing their taxes annually. Likewise, the manifold targeted benefits delivered automatically to different groups through the tax system are self-evidently non-stigmatizing. The less of an ordeal is the procedure, the more automatic and less discretionary, the less likely it is to be accompanied by stigma.

The first two items are displayed as two dimensions in Figure 1. In the upper left, typical North American welfare systems are distinguished by high levels of caseworker discretion and by the special treatment received by recipients. Various tax expenditures in the bottom left, including the Earned Income Tax Credit, are marked by a degree of automaticity alongside differential treatment. Judges' powers, in the top right, exemplify the permutation of case-by-case discretion with an abstract uniform set of rules applicable to all. Although I characterize the GAI as a social policy that treats social groups in a uniform fashion, there remain non-recipient groups outside the umbrella of the GAI. Unlike, say, a universal basic income—to the right of the GAI in Figure 1—the fullest sense of uniformity in treatment is not a feature of the typical GAI design. A universal basic income, which operates without prior assessment of incomes, is also more automatic. It lacks even an initial sign-up procedure and therefore also falls below the GAI on Figure 1. Though the two programs can lead to identical post-transfer income distributions (Groot 2004) and individual incentives (Harvey 2006), they retain symbolic differences. It is worth noting that this measure of ambiguity in the GAI on the dimensions above makes the question of framing more important and less deterministic than might obtain in, say, a

universal basic income. This leads us to a third point about the way targeted programs might evade stigma.

[Figure 1 about here]

Social programs can be framed in more or less morally acceptable terms (Tversky and Kahneman 1981; Kahneman and Tversky 2000). Similar benefits may, for example, be portrayed as earned or as charity (Skocpol 1991). These frames will shape interpretation in the community (Nelson, Oxley, and Clawson 1997; Gamson and Modigliani 1989; Chong and Druckman 2007; Slothuus 2007; Steensland 2008; Wiederspan, Rhodes, and Shaefer 2015). There is no doubt that the ideological reception of a policy will be, in part, a reflection of its underlying design and material impact. However, another part is somewhat unmoored and open to more positive or more negative portrayals. Because the guaranteed income has certain aspects that are open to both portrayals—unlike programs with the strongest forms of discretion and group differentiation on the one hand, or the strongest forms of automaticity and uniform treatment on the other—its framing may be particularly consequential.

The Dauphin experiment, to some extent, satisfied the conditions above where welfare did not. Mincome guaranteed incomes to a mixed group of recipients in a fairly automatic fashion. It fostered an idiosyncratic, largely positive framing. A relevant hypothesis expects the program to generate subjective interpretations and community experiences quite different from the stigma of welfare participation. Before approaching these issues the next section introduces the data and methods.

#### **4. Survey Data and Methods**

A nine-page “community experience” survey was issued to every participating adult head in their homes once at the midpoint of the experiment in August 1976. It included open-ended questions, as well as yes/no and Likert-style questions concerning people’s day-to-day experience with the community and with Mincome itself. The survey was self-administered by participants, although interviewers were directed to explain instructions or define words when necessary. Interviewers explained that the survey was strictly confidential, that it would be unconnected to participants’ names, and had no bearing on Mincome payments. The survey was also optional, although interviewers encouraged completion; they introduced the survey with the following statement:

This questionnaire will help us to gather information about the way in which income assistance programs affect other areas of a person’s life, such as his daily activities and experiences with others in the community. We are depending on your assistance in filling out this questionnaire. This additional information will make an important contribution to our study of how well different types of assistance programs work.

The survey was completed by 407 Dauphinite household heads, roughly 65 percent of the adults enrolled at the time. Some files are incomplete due to illiteracy or refusal. Of the completed surveys, 79 percent provide at least some qualitative commentary on open-ended questions, while Likert scale and yes/no questions were on average 97 percent complete.

For purposes of comparison, I examine another 40 surveys completed by Manitoba welfare recipients and 98 surveys completed by non-recipient “controls” from various small Manitoba towns. Non-participants are a stratified random sample, rather than a fully random sample. This means that they consist of families whose income falls into a range where, in Dauphin, they could have been eligible for Mincome. This allows for a partial control of class differences. The two comparison surveys are virtually identical to the Mincome survey, substituting the word “welfare” for “Mincome” or skipping questions altogether where inappropriate.

The survey queries participants on time-use; difficulties with various community members; experiences with Mincome's bureaucracy; embarrassment related to being on Mincome; and comparisons of Mincome and welfare. Data are held by the Library and Archives Canada.<sup>31</sup> The surveys were photographed and transcribed into digital format. Rating scale, multiple choice, and yes/no questions (shown in Figures 2–9) contained a pre-existing coding scheme, and percentage frequencies are presented on the basis of original answer categories (i.e. Yes, No, Don't Know, Only if necessary). In some cases, I report only one central answer category in order to coherently present multiple question items in a single figure (e.g. respondents saying "Yes" or "None"). In other figures I combine answer categories (e.g. "occasionally", "often", and "always") when the grouping scheme captures most of the category-by-category variation. In cases of lists of similar items, I group persons who report that they have experienced at least one item in the list (e.g. a list of community positions) or answer yes to at least one in a set of similar questions (e.g. credit-related questions). The survey's open-ended questions (shown in Tables 1, 2, and 4) were inductively coded into answer categories in two steps, following Corbin and Strauss (2007) and Charmaz (2006). Line-by-line coding linked provisional categories to fit the data. Next, "axial" or "focused" coding involved a category-by-category examination. Here categories were identified relative to each other, and certain codes were identified as either core or provisional, where the latter were subsumed into the former.<sup>32</sup>

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<sup>31</sup> At the conclusion of the Mincome program, some of the longitudinal survey data was collected into several datasets, used for the handful of academic papers published on Mincome in the 1980s and 1990s (Hum and Choudhry 1992; Hum and Simpson 1993; Simpson and Hum 1991; Prescott et al. 1986). However, 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. In the wake of the experiment, the remaining raw data was left somewhat unorganized and without a finding guide, until being organized recently by Archives Canada.

<sup>32</sup> Since people sometimes make multiple comments, some answers are given more than one code.



## 5. Why Did People Participate in Mincome?

Understanding people's motives to join Mincome sheds light on its social meaning. As shown in Table 1, almost half of the respondents said they joined for the "money." Quite simply, they needed help. These answers varied from "I was financially desperate" to "it gives us the chance to pay a few bills."

[Table 1 about here]

Under the broad rationale of "money," Table 1 demonstrates a good amount of heterogeneity in people's circumstances. Some families saw Mincome money as risk reduction, some viewed it as supplementing insufficient incomes, and others saw it as their only access to a standard of living. However, as an answer to why a family would join Mincome, "money" obscures as much as it reveals. Welfare also provided money, but was universally disliked. Thus, money is a proximate cause and self-evident answer, but alone it does not expose why money is needed or the conditions under which it can be accepted. Beyond this undeniable material rationale for joining a GAI program, there were a wide variety of other material factors offered, including "insurance," disability ("[John] had broken leg and we needed help"), unemployment ("Shortage of jobs"), help in accessing education ("we have a chance to improve our educational level in order to improve our income"), and providing care for families ("To look after children while in school").

Putting "money" to the side, the modal "material" response refers to insurance or security, and typically refers to possible health problems—"To back up my financial state in case of sickness"—or possible income loss—"When I'm not working or let off it's nice to know you can get help from Mincome for when you need it." One participant joined Mincome because she

was “uncertain of [her] husband’s earning abilities for [the] winter months as seasons sometimes affects his earnings.” A 50-year-old single woman joined Mincome “For security in the event I lost my job for any reason.” She wrote: “As long as I can work for a decent wage I will do so. However, it is nice to know that Mincome is available to me if I ever need it ... I consider Mincome as an experiment which I am taking part in, even though I don't know if I will ever need it. I may want it someday.”

The above quote emphasizes material circumstances, but provides an additional key to the “ideological” aspects of Mincome’s reception. As Table 2 shows, the modal ideological response related to “helping” the experiment. In fact, this response was more common than the “security” rationale. One participant joined “To contribute to the success of the program.” Another joined “in order to aid in an adequate cross-reference of the community.” A third cited “Statistics regarding guaranteed income, might help in studies.”

[Table 2 about here]

It is necessary here to describe the portrayal of the guaranteed income by Mincome and provincial officials. In the months before the start of the program, potential participants received a short letter from Canada’s Minister of Health and Welfare inviting families to join the experiment. The letter refers to Mincome as “an experiment” designed “to assist in our efforts to improve Canada’s social security system.” It closes by stating: “I consider that your participation will contribute substantially to its success.” The language used in press releases between 1971 and 1974, and additional letters from the Manitoba government were virtually identical. One letter explained that the purpose of the project is to “collect information” on a “representative cross-section” of Manitobans. Finally, before joining, participants read the same seven-page

booklet introducing the Mincome experiment. On the first page, it asks, “Why is a test necessary?” The pamphlet explains:

A Basic Annual Income would be an efficient way of making sure that all Canadians have a reasonable and secure income, including those who are working. But both governments felt that more advance information was needed about what would happen if such a program came into being. To test this, a small-scale study has been set up.

This framing by Mincome staff and both levels of government is clearly reflected in the majority of contemporaneous articles and editorials in the town’s main newspaper, *The Dauphin Herald*. The earliest reporting on the experiment began with four major articles in 1973. The themes considered were often technical, mirroring the language of Mincome staff and government press releases. The articles discuss the scientific nature of the experiment, the economic survey of the area, the computer analysis to be used, the payments procedure, and other details of the project’s operation. In 1974, the year running up to the experiment, there were 16 articles and editorials about Mincome, typically emphasizing the scientific and experimental nature of the project. Some highlight specific scientific developments that Mincome will employ (“New Computer Techniques Aid Mincome,” 1974). Other articles discuss aspects of the research design (“Mincome moves to its Second Phase,” 1974) or how Dauphin was selected (“Dauphin Considered Best for Mincome Experiment,” 1974). By the end of the program, the paper’s reporting had shifted emphasis to the minutiae of daily operations, though it maintained its initial framing (“Mincome, Firstly, Just an Experiment,” 1978). The only piece of explicitly negative reporting published came out in 1978, after operations concluded (“Mincome No Cure All—Ritchie,” 1978).

Mincome planners framed the project as a “test” of a program intended to help “all Canadians.” Local coverage reinforced this portrayal. Participants commonly accepted this

frame, interpreting participation as aid in research rather than accepting public assistance. One participant had the following to say: “Have always been a firm believer that surveys and statistical data are a necessary program of our daily lives.”

Although some of the “ideological” responses directly reflect the framing of the project others appear at first glance to be so diverse as to be indecipherable. Some participants were simply “curious” about the program, others joined because they were “asked.” One man joined for “No special reason. Wanted to see what it was about.” Another wrote, “It sounded like an interesting experiment. ... The sociological booklets were fascinating.” What the majority has in common—and what is absent from responses of welfare recipients—is their banality. The portrayal of Mincome—its framing as a “test ... to improve the income security of Canadians”—provided a kind of ideological cover for would be participants. Mincome’s relatively unstigmatizing social meaning allowed recipients to evade the typical representations of public assistance receipt. In fact, many happily neglected any mention of actual or potential material benefits.

Mincome’s portrayal facilitated a variety of casual and even evasive seeming answers, never found among welfare recipients. When asked why he joined Mincome, one participant claimed he “never really thought about it before.” This I argue is a consequence of the program’s social meaning. Only if a program is generally perceived as non-stigmatizing will people join out of such prosaic motives. Meanwhile, it is desperation, not curiosity, that typically motivates participation in welfare.

Mincome allowed for a variety of personal justifications for joining. Indeed the lack of participation rules made it easier to find socially legitimate reasons to join. Its flexibility in responding to diverse social needs, working class needs, the needs of the poor, people at different

life stages, and people facing various kinds of uncertainty, bears on its appeal. That appeal, however, is also a product of its portrayal. Its framing as an experimental, universalistic program may have made participation easier. It provided leeway to contrive morally acceptable reasons for participation. A program's framing is critical even if a universalistic and non-stigmatizing portrayal will consolidate more easily on a program whose technical apparatus is somewhat flexible and inclusive. The interactions between a program's framing and its basic design features are subtle; although the Manitoba government attempted to frame the welfare system in a positive light in the 1970s (Barber 1972; Manitoba 1972), the underlying design may have made it less amenable to positive frames. I pick up the issue of design in the next section, and compare the program features of Mincome to those of welfare, in order to understand how they affect the moral reception of the two programs.

## **6. The Stigma of Welfare, the Normalcy of Mincome**

A combination of elements allowed residents, even those with largely negative attitudes toward social assistance, to comfortably participate in Mincome. As shown in Figure 2, only a small minority (6 percent) of Mincome participants reported a willingness to join welfare (if it would improve their incomes). The vast majority either refused or would only join if necessary. By contrast, welfare participants were overwhelmingly willing (70 percent) to join Mincome. Non-participating rural Manitobans were also far more likely to consider joining Mincome (42 percent) than welfare (5 percent).

[Figure 2 about here]

Participant accounts reinforce the notion that these programs were perceived quite differently. One 34-year-old married man joined Mincome because he “needed extra money.” Yet, he eschewed welfare, saying, “I’ll suffer instead.” What explains this widely divergent reception? What made Mincome more socially acceptable than welfare? This section describes survey data in percentage frequency graphs and then participant accounts to establish and explain perceived differences between Mincome and welfare, and locate the sources of social stigma.

However, before examining these survey data, it is important to look at differences among our three comparison groups—Mincome participants, welfare participants, and non-participant controls—in order to appropriately interpret the data. Table 3 provides descriptive statistics on familial status, presence of young children, age, and education across groups at a “baseline” interview before the experiment. Comparing the welfare and Mincome groups, the former has a much higher portion of singles, and of those singles, the welfare group has a higher portion of single parents. The welfare group is also older on average, somewhat more likely to have children under six, and much less likely to have a high school graduate in the household. Table 3 shows differences between Mincome and Manitoba community non-participant groups as well: non-participants include more married couples, fewer single-parents, more families with young children, and more families with a high school graduate head. My discussion of the figures in the next section attempts to take account of these demographic differences, and provide interpretation in light of them. For example, to improve comparability to the welfare group, where possible and most relevant I include additional comparisons to Mincome participants with experience in the welfare system in the two years prior to the experiment. Moreover, in comparing the Mincome participants and community non-participants I note that similarities in the figures below are particularly suggestive in light of the baseline demographic

differences; similarities in outcomes between a more privileged and a less privileged group only strengthen the suggestion of a community effect.

[Table 3 about here]

### *6.1. Community Experiences and Social Stigma*

Part of the argument in favour of broadly inclusive programs suggests that they reduce the barriers to community building and, at minimum, avoid exacerbating the potential social isolation of participants. Below I describe evidence on social stigma and community experience bearing on these hypotheses.

The community survey inquires into time-use, in order to discern the extent to which people's spare time is spent alone or in social contexts. Mincome participants and non-participating community members were less likely than welfare participants to report spending no spare time with friends, neighbours, relatives, or workmates (Figure 3). Both groups were also less likely to spend time at home (Figure 4a) and more likely to spend time at other people's homes (Figure 4b) than welfare participants. More interesting than the divergence with welfare recipients is that Mincome participants tend to have time-use patterns not unlike Manitoban non-participating community members, a group with higher average socioeconomic status. This suggests that one could participate in Mincome without forfeiting certain community experiences.

[Figures 3 and 4 about here]

Mincome may have escaped the strain and tension in the community commonly accompanying welfare receipt. Mincome participants (98 percent) were more likely than welfare participants (72 percent) to “never” attribute any community difficulties to program participation

(Figure 5a). Mincome participants (92 percent) were also more likely than welfare participants (65 percent) to report “never” feeling embarrassed or uncomfortable when they were with people not on the same program (Figure 5b). Note that Figures 5a and 5b (and onward through Figure 9) include additional comparisons with Mincome participants who have prior welfare experience. This group provides a useful contrast, as they are more likely to have social and class positions in common with welfare participants.

[Figure 5 about here]

Another indicator of community participation concerns people’s interactions with banks and various community members. As shown in Figure 6a, welfare participants (18 percent) were most likely to experience one or more credit-related difficulty, community non-participants (12 percent) less so, and interestingly, Mincome participants (8 percent) were the least likely to experience these problems. Mincome may have led to interactions with creditors that were more positive than the norm. Dauphin banks may have seen Mincome as a source of economic stability for participants; an increased confidence of repayment on the part of banks might have increased the availability of credit. Roughly similar patterns are found regarding community difficulties participants attributed to their income (Figure 6b).

With respect to landlord-related difficulties (Figure 7a) and difficulties with other community authorities (Figure 7b), Mincome participants tend to report community experiences that are much the same as those of non-participant controls. Similarly, Mincome participants and community non-participants were equally likely, and more likely than welfare participants, to have held positions in at least one community group (Figure 8). Finally, welfare participants are



twice as likely as Mincome participants to admit that they have attempted to hide participation from workmates, friends, or stores (Figure 9).

[Figures 6 to 9 about here]

Figure 6 onward includes additional comparisons with Mincome participants who have prior welfare experience. In some cases, in particular some data points in Figure 9, this comparison weakens the overall argument. However, more often than not the comparison with this subgroup strengthens the argument due to subgroup results that are similar to results in the full Mincome sample. In all cases, it is hard to argue that Mincome led to difficulties in the community in the way that welfare did. In large part it appears that Mincome participation was compatible with community experiences not unlike those experienced by non-participating Manitobans. This is particularly striking given the more “mainstream” demographic characteristics of the community group. Absent the experimental treatment, theory would predict this group to be less socially isolated and less stigmatized. Below, we develop these findings with qualitative accounts.

## *6.2. Moralism on Welfare, Pragmatism on Mincome*

Mincome participants who would not accept traditional social assistance sometimes explicitly and more often implicitly argued that the latter was stigmatizing. This did not mean that people did not need assistance. However, accepting aid only became possible when stigma was reduced: one man declined welfare, simply citing “status,” but joined Mincome for “extra income.” A woman refused to join welfare because “It would make me feel bad and think people may be

laughing at me.” She joined Mincome “To help along with the expenses.” This subsection describes the consistently divergent personal assessments shown in Table 4.

[Table 4 about here]

In the qualitative accounts of welfare participants, consciousness of social stigma and its psychological cost was overwhelming. Various studies have shown that welfare participants often share society’s negative attitudes toward them (Rainwater 1982; Bullock 1999; Canada 1971b). This is consistent with the 43 welfare participants in my sample. One welfare participant wrote, “I dislike welfare, it is degrading. ... Surely we are entitled to live in dignity”. Another wrote: “You never know what your proper place in [the] community is, as some people think you seem to be inferior to them.”

Among Mincome participants’ accounts, it was common for individuals who took strong moralistic positions against welfare to view Mincome in pragmatic terms. One man opposed welfare, stating: “Welfare should be used only for those who require it not abused by those who really don't need it.” Regarding Mincome, however, he wrote that, “Extra income really helps when one gets it today.”

It was not uncommon to view traditional social assistance as a program exclusively for people who were ill, disabled, “lazy,” or in some sense marginal: “Welfare is only for needy or bums.” The welfare system aggravated distinctions between people falling into different social categories. One man wrote, “I feel that [welfare] is more for disabled or people which are too lazy to work. It doesn't include us, we’re both able and willing to work but can’t get a job due to the low employment rate.” They joined Mincome simply because they were “Short of money.” Where it was easy to distance oneself from welfare recipients, Mincome was not tarnished as a

program for specific kinds of people. No equivalent linkages are made between the program and particular, undeserving groups. Rather, Mincome was practical support. As a practical program, participation was less likely to signal a person's moral worth.

Consistent with the discussion above, one could simultaneously view welfare as stigmatizing, and view Mincome as an experiment, as an aid to working people, and innocuous, more generally. "Other people abuse families on welfare and talk about them," noted a woman who joined Mincome "for the government experiment." A man who would not join welfare because "it makes a bad image on the family" joined Mincome "to take part in the Mincome experiment."

Participants want to retain their dignity and are open to a form of social assistance program that has been reframed. When families are asked to help, when their information is seen as valuable, it becomes easier to join. A man who avoided welfare citing "pride," joined Mincome because he was "asked to." Another refused welfare stating, "I wouldn't want to destroy my dignity and pride." He joined Mincome because "Mincome picked our name and asked us to be on the program."

Not all participants viewed Mincome as destigmatizing. At least four participants expressed explicit concern that Dauphinites might take "advantage" of Mincome. However, even these comments are contradictory, typically blending criticism with positive assessments. On the other side, some participants explicitly state that social stigma was reduced under Mincome: "It trusts the Canadian people and leaves a man or woman, their pride." One woman concluded, "Mincome seems more normal than welfare."

Though welfare was a normal part of everyday life, Mincome might have treated participants as "normal" people. Just after the midpoint of the program the director of Mincome

told *The Dauphin Herald* that Mincome “appears to have become a natural part of the community” (“Mincome Payments Made as Usual,” 1976). One woman reported that she had “always been put at ease” with interactions with Mincome staff. Some comments indicate that Mincome had a mainstream character. One man suggested that Mincome enabled his family to live at standards acceptable in the community. He joined, “To give my family a regular living standard, more in line with the people around us.” Another man may have observed the “normal” qualities of the program. He asked: “Will everyone in Manitoba soon be on Mincome?”

### *6.3. The Terms of Welfare, the Flexibility of Mincome*

This final subsection argues that the sources of Mincome’s normalcy described above are linked to design features that allowed recipients to participate in the normal activities of daily life, especially work life. Recipients were not separated out to be assigned special sets of rules. In particular, Mincome maintained incomes without revoking the autonomy and independence enjoyed by better off residents.

Survey participants give a variety of reasons for refusing welfare, but the biggest portion (see Table 4) emphasize their desire to work and support themselves. The desire to avoid welfare is not surprising, but people who refused welfare because they prefer to work—or more poignantly, because they prefer to earn their own incomes—often joined Mincome precisely to obtain additional income. “Welfare to me was accepting something for nothing,” said a man who joined Mincome because it “would be a benefit to me at some time.” One participant who avoided welfare, stating, “I am able to support myself,” joined Mincome because “I might get some assistance.” A 22-year-old single man who refused welfare because “I’m healthy and can be self-sufficient I feel,” joined Mincome because “It provided one with enough income to live

sufficiently.” Working allowed him to feel “self-sufficient,” but Mincome allowed him to “live sufficiently.” Another welfare refuser said he “felt better earning his own income,” yet he noted that Mincome’s “added income” was “perhaps [the] best feature!” One man refused welfare on moral grounds, saying “I believe if a person is capable of working he should work instead of accepting charity”; he joined Mincome with his family for pragmatic reasons: “To receive enough money to meet our needs.”

It was not uncommon for people who wish to earn a living on their own to simultaneously collect Mincome payments comfortably. Participants appreciate the feeling of independence that comes from “earning” a living, but often cannot earn sufficient employment income. Work provided a sense of autonomy, while Mincome helped people actually achieve a decent standard of living. It could be integrated into an already existing moral code of self-sufficiency and meritocracy. When income maintenance policies required recipients to violate mainstream values around work and autonomy, they were morally unacceptable; when they sidestepped confrontation with a mainstream work ethic, they were morally neutral.

The absence of regulation around the work lives of individuals stands out as a key part of “feeling independent.” Mincome’s smooth integration into work life and the sense of autonomy it facilitated were key features separating it from the social meaning of welfare. In one case, inclusion into the normal habits of daily work life was possible; in the other, exclusion from mainstream activities and special treatment was the rule. Mincome allowed participants to retain a mainstream ideology of meritocracy. They were not forced to question their place within broad community norms.

Beyond the regulation of work, there are myriad complaints about the stipulations and conditions of welfare, which single out recipients. Many of these conditions involve invasive and

degrading procedures that combine to nurture a pervasive sense of indignity. As Reich once noted, welfare administrators exercise their discretionary power “to impose standards of morality [on welfare recipients that are] not imposed on the rest of the community” (1963: 1359). More recent analyses of welfare administration reveal the endurance of these dynamics over time (Little and Morrison 1999; Chunn and Gavigan 2004; Herd et al. 2005)

Likewise, Mincome participants often object to the basic fact of caseworker discretion. One former welfare recipient compared Mincome’s automatic delivery and flexibility to welfare’s conditionality:

It helps the below the average earning without having to go through doctors etc. You can live where you want to. You can spend your money when you need to and save for the next month when more will be needed. ... People feel more secure knowing that if they need help it is there.

Another participant refused welfare, reporting, “Welfare is more uncertain. Workers are rude, incompetent.” She joined Mincome simply “For the money.” She continued: “Mincome people seem very considerate. ... I like Mincome in that one is left alone, never harassed or made to feel like you had to crawl to receive an almighty dollar. I don’t like the idea that it is intended to end shortly, with nothing to replace it but that same lousy welfare.”

The contrasting terms of welfare are stark. One welfare participant wrote:

The thing I don’t like is practically having to beg for ... extra money for expenses and the idea that if they don’t like your attitude you can be cut off. This can be a very strong weapon in some of the workers hands and creates bad feelings between people as it seems to make some of them adopt a very snotty attitude towards people unfortunate enough to be on welfare.

The assessment of welfare caseworkers as “rude” and Mincome staff as “friendly,” “nice,” and “super people” is a product of design details as much as anything else. Welfare workers were tasked with home visits where various criteria, much open to personal discretion,

are used to determine continued eligibility. By contrast, Mincome was relatively hands-off; the most sustained interaction with Mincome staff came during interviews, which were unrelated to actual payments. As emphasized in Figure 1 above, the absence of design features that treat one group of people in a separate fashion, and the substitution of automatic procedures for case-by-case determination produces social policy less conducive to the emergence of stigma.

## **7. Conclusion: Benefits Without Barriers**

Though Mincome produced fewer solid conclusions than it should have, the old questions of stigma and social inclusion are germane to a full consideration of the lessons to be learned from the experiment. Mincome bore a familial resemblance to welfare. It disproportionately directed benefits to the poor, and retained distinctions between Mincome recipients and non-recipients. However, participants and other Manitobans saw it as distinctly different from welfare, as a program providing assistance without also imposing the costs of stigma.

The importance of the moral aspects of social policy should not be underestimated. The most successful anti-poverty effort over the past century in North America, the growth in social insurance for the elderly (Myles 2000; Campbell 2003), was effective in part because the elderly are seen as morally deserving (Pettersen 1995; Van Oorschot 2000; 2006). Eliminating social stigma is important for its own sake; Rawls argued that “self-respect and a sure confidence in the sense of one’s own worth is perhaps the most important primary good” (1971: 348). But it is also important for instrumental reasons: if antipoverty tools are to be socially reproducible, if they are to provide a base from which to mobilize for broader reforms, they must consider the moral reasoning they foster.

Mincome did not single out groups to be treated in a manner that accentuated their separation from others. Participants avoided the special treatment of having their work and personal lives monitored and regulated. It did not force participants to transgress mainstream norms around work and meritocracy. Participants were treated, in sum, like “all Canadians,” and the program’s portrayal reinforced this image. As a consequence, the community’s reception was pragmatic, not moralistic. For these interacting reasons, Mincome appeared “normal” in the eyes of participants. The bright line dividing the deserving and undeserving poor turned fuzzy. The seeds of Rawls’ “social bases of self-respect” were planted.<sup>33</sup>

If part of Mincome’s positive reception can be attributed to its framing, a contemporary variant might be equally compatible with a variety of positive portrayals. In an age of precarity (Kalleberg 2009; Standing 2011), a modern guaranteed income may make common cause with a range of groups if portrayed as “insecurity insurance” or “low-income insurance” (Hacker, Rehm and Schlesinger 2013; Paskov and Koster 2015). Pitched in these terms, the guaranteed income may be understood as a collective resource, one that benefits even those people not drawing net benefits at any given moment (Sjöberg 2010). These considerations take it for granted that political feasibility is as significant as economic feasibility in the evaluation of income maintenance policy. If desirable social arrangements are to be robust, the political impact of their

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<sup>33</sup> Although these seeds may have been planted, it is possible that the complete eradication of social stigma—in particular, the stigma linked to “able-bodied” people outside the labour market—is incompatible with robust, sustainable capitalist labour markets. The conjecture here is that without stigmatizing those outside the labour market, without making the alternative to work painful, capitalists lose their negotiating power over those currently at work, and this in turn makes the game of capitalism far less sustainable or impossible over time. If this asymmetry in negotiating power is indeed truly necessary to the social reproduction of capitalism, then one could speculate that in a capitalist world with a generous guaranteed income, the power of “the sack” (Kalecki 1943) comes not from the deprivations of unemployment, but from the lingering pain of social stigma. Differently put: if one accepts a functionalist explanation of unemployment under capitalism (see debates in Elster 1982; Cohen 1982; Van Parijs 1982; and Berger and Offe 1982), then “unemployment as social stigma” may achieve the same functionality once achieved by “unemployment as poverty”, now off the table.



design details and framing ought to be at the fore of discussions of the social reproduction of social policy.

Welfare systems in the United States and Canada have changed in important ways since the 1970s. Apart from the shift to “workfare” (Bashevkin 2002; Peck 2001; Danziger 2010), and increasing barriers to eligibility (Kneebone and White 2009), some of the most extreme intrusions into people’s lives have been relaxed (Boychuk, 1998; Caputo 2011; Gustafson 2012). Yet, the distinction between the deserving and the undeserving poor persists. The moral regulation of the poor is an enduring feature of social assistance (Chunn and Gavigan 2004; Little and Marks 2006; Gazso 2012). One qualitative study of female social assistance recipients’ experiences with the welfare system in Canada found that recipients “said they were belittled, abused and treated as file numbers, and ‘non-persons’” (Reid 2009). Recent evaluations (Neysmith et al 2005; Wallace et al 2006) conclude that the social assistance system continues to be marked by deep social stigmatization. In some cases there is evidence that the “micro-regulation” of job search intentions and personal behaviour has expanded in the wake of welfare reform in the 1990s (Herd et al 2005). Social policies that destigmatize, ones that blur the boundaries between the deserving and undeserving poor, remain as relevant as ever.

Amartya Sen (1999) often refers to Adam Smith’s conception of deprivation as the inability to appear in public without shame. By reducing social stigma, the guaranteed income helps achieve this object. The question of whether a guaranteed income can actually enhance social solidarity among poor and working people is harder to argue persuasively. It achieves this end, in part, insofar as it provides benefits without erecting barriers to social inclusion. By obscuring the distinctions among low-wage workers, unemployed workers, and social assistance recipients, universalistic income maintenance programs may reduce the barriers to

communication between otherwise separated people. This does not quite equal the active nurturing of social solidarity, but avoiding its obstruction is a meritorious goal nonetheless.

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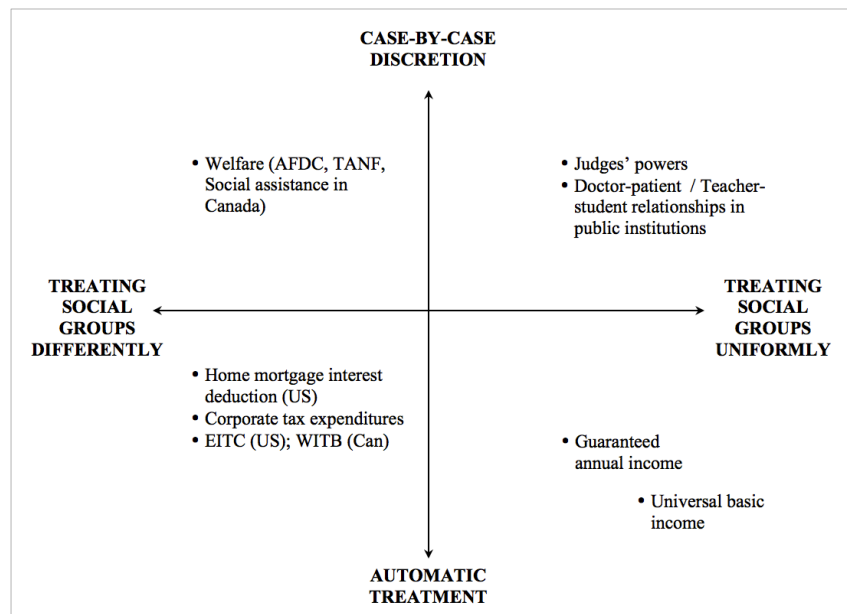
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## FIGURES AND TABLES



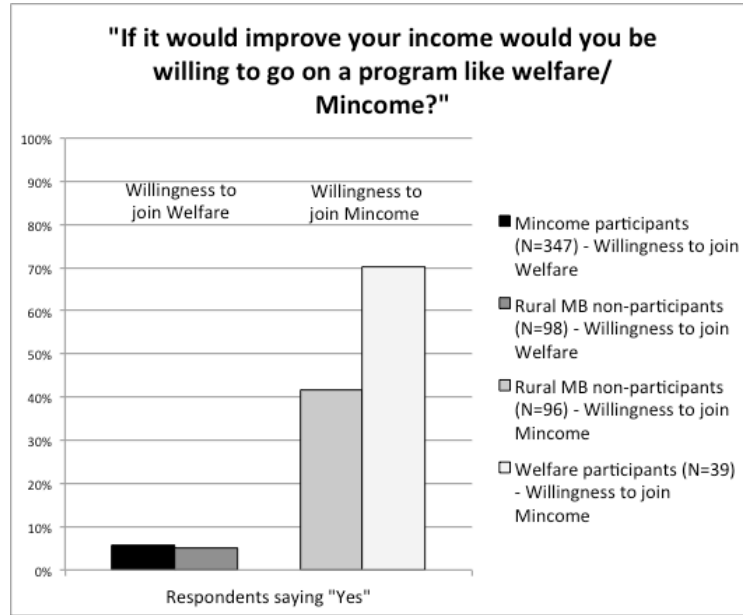
**Figure 1:** The GAI on a map of social policy attributes

Sample responses to question 1 (Indicate the main reason why you decided to go on the Mincome program) by "materialist" theme			
Coding category	N	%	Samples
Money/ assistance	143	44%	<p>"For the money"</p> <p>"Need more income with the cost of living now"</p> <p>"I was not making enough wages"</p> <p>"Business wasn't going good"</p> <p>"Need more income with the cost of living now"</p> <p>"I needed more help to support the boys"</p> <p>"I was financially desperate"</p> <p>"Didn't want to live off my parents"</p> <p>"No other income and found it very successful and a very great deal of help to my family"</p> <p>"Thought that the little bit would help a lot"</p> <p>"Because it makes up for what you don't earn"</p> <p>"To make it easier for me to support my family"</p> <p>"To give my family a regular living standard, more in line with the people around us."</p>
Security/ if unable to work/ in case of illness	20	6%	<p>"We don't receive any payments. I am self-employed and if I ever did become ill, Mincome would probably be paid to my family and I"</p> <p>"For security in the event I lost my job for any reason ... I have no wish to live without working for my pay, and as long as I can work for a decent wage I will do so. However it is nice to know that Mincome is available to me if I ever need it... I consider Mincome as an experiment which I am taking part in, even though I don't know if I will ever need it. I may want it someday."</p> <p>"uncertain of husband's earning abilities for [the] winter months as seasons sometimes affects his earnings. ... If one loses a job (or illness) I feel Mincome gives families a little more security and helps remove some extra fears."</p> <p>"Because if I ever got laid off I could live"</p> <p>"If for some reason I was unable to work for a short while, I would have a small income, until I was able to work again"</p> <p>"When I'm not working or let off its nice to know you can get help from Mincome for when you need it."</p> <p>"To back up my financial state in case of sickness"</p> <p>"security reasons ... people feel more secure knowing that if they need help it is there."</p> <p>"It gives me a good security feeling in case I can't work. ... All I can say [is] it is a very good program. It certainly helped me a great deal in fact an awful lot when I lost my husband for which I am very grateful and I thank you."</p> <p>"It helped me very much during winter months when work was not too plentiful. ... I don't have anything against Mincome. I think it's a very good program. The time when I wasn't working during winter. It has helped me very much."</p> <p>"It would be a guaranteed income if anything happened to my husband and he was unable to work."</p> <p>"I was on Mincome for three months when it started ... I think Mincome is good to people who are in need of it, as long as people do not take advantage of it. When I was on Mincome two years ago I was on a low wage bracket and I needed it ... When I got a better income I did not file anymore even though sometimes we could of used it."</p>
Could not find work	12	4%	<p>"Shortage of jobs and my husband was on the program"</p> <p>"No permanent job when Mincome was introduced"</p> <p>"Lack of jobs"</p> <p>"No work at the time, no income".</p> <p>"I was pregnant and couldn't get a job"</p>
Could not work/ disabled/ Ill/ Elderly	12	4%	<p>"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."</p> <p>"Dennis had broken leg and we needed help"</p> <p>"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."</p> <p>"We had no other choice as my husband is disabled and with my health and age, I am not able to work full time."</p> <p>"I felt it would help our situation: and invalid husband with no income."</p>
To help care for family	8	2%	<p>"My children were young and I felt I was needed at home"</p> <p>"I wasn't eligible for welfare and had to support my son somehow"</p> <p>"I have a child to take care of and didn't want to go on welfare ... I don't really believe in welfare"</p> <p>"Spend a year at home with my children".</p> <p>"To look after children while in school"</p> <p>Mincome did not provide "enough money to look after myself and 2 children. I still have 2 years left at University and it's rather a hard row to be when you're as poor as I am at this point."</p>
Help to go to school	7	2%	<p>"We have a chance to improve our educational level in order to improve our income."</p> <p>"Mincome has helped a lot to provide for my family and since my husband is a student it was a comfort to have the monthly cheque to look forward to"</p> <p>"husband was going to school".</p>
Better than welfare	3	1%	<p>"Because [it offered] more independence with money than welfare"</p> <p>"Welfare was unreasonable with me".</p>
TOTAL	322	(Table 1 & Table 2)	

**Table 1: "Material" reasons to join Mincome**

Sample responses to question 1 (Indicate the main reason why you decided to go on the Mincome program) by "ideological" theme			
Coding category	N	%	Samples
To help with research/project	39	12%	"To contribute to the success of the program" "To help the program along" "help in research" "To help the government get information" "For the benefit of the government study program" "In order to aid in an adequate cross-reference of the community" "Statistics regarding guaranteed income, might help in studies" "I feel they need all the help they can get if the programme is to succeed" "Have always been a firm believer that surveys and statistical data are a necessary program of our daily lives." "If and when the total statistics are formulated I would appreciate a copy so that I may continue a study on the relationship of families on Mincome and school performance."
We were asked	25	8%	"asked to" "I was asked to and volunteered to go on." "Was asked to participate" "Was approached by an interviewer" "I was asked to and volunteered to go on." "We were asked and we accepted" "Mincome picked our name and asked us to be on the program" "No particular reason. Was just asked and continued."
Curious/ wanted to see what it was about	15	5%	"No special reason. Wanted to see what it was about" "Thought it would be an interesting experience" "See what it was all about" "It sounded like an interesting experiment...The sociological booklets were fascinating—we thoroughly enjoyed them" "Tried to find out how it works out" "Curiosity"
Thought it was a good program/ helpful for people	14	4%	"Because I think it's good for the country - help the economy" "Because I think it is good for Canada" "Decided to as I felt it was for the cause" "To contribute to the success of the program" "Because of the Dauphin involvement and need of an balanced program" "[It is] an approach for some form of betterment"
Because family/ friends/ others were on it	11	3%	"Everybody else was" "Just to be in it like others" Husband: "My wife and mother-in-law both talked me into it." Wife: My mother is on it and has helped her considerably and we also needed help "Friends told us about it"
Other	17	5%	"Don't know why" "No reason" "I don't remember." "Never really thought about it before"
TOTAL	322	(Table 1 & Table 2)	

**Table 2: “Ideological” reasons to join Mincome**



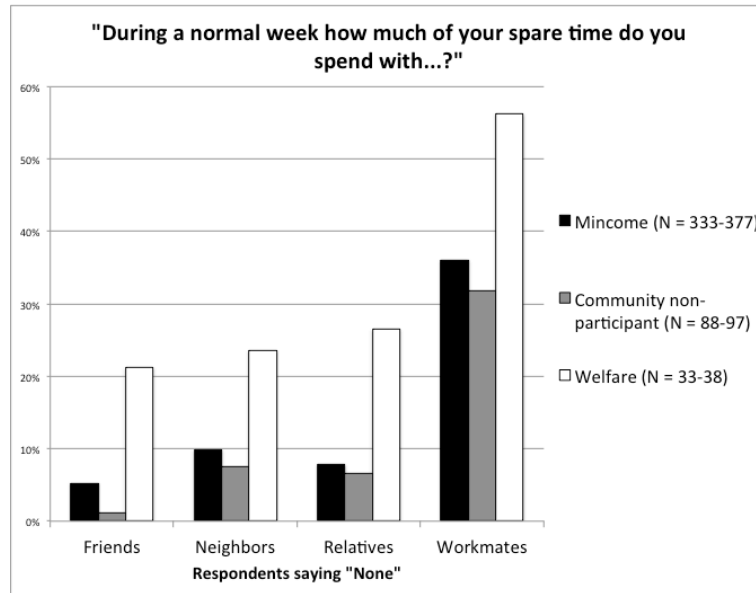
**Figure 2:** Willingness of Mincome participants, welfare participants, and non-participants to join welfare or Mincome

Baseline characteristics	MB community non-participants (N = 56 households)		Dauphin (N = 265 households)		MB Welfare (N = 36 households)	
	N	%	N	%	N	%
Familial status						
Married	43	76.8	156	58.9	8	22.2
Single	7	12.5	89	33.6	27	75.0
Single-parents	3	5.4	42	15.9	19	52.8
Missing	6	10.7	20	7.6	1	2.8
Young children						
No children under 6	28	50.0	133	50.2	20	55.6
Any children under 6	22	39.3	56	21.1	11	30.6
Missing	6	10.7	76	28.7	5	13.9
Age of individual heads						
Mean age of male head	39.05	(N=44)	41.23	(N=181)	47.10	(N=10)
Mean age of female head	35.65	(N=49)	38.00	(N=223)	43.00	(N=33)
Education						
No high school grad in household	31	55.4	130	49.1	21	58.3
High school grad (at least one head)	18	32.1	48	18.1	1	2.8
Missing or NA (i.e., under 21)	7	12.5	87	32.8	14	38.9

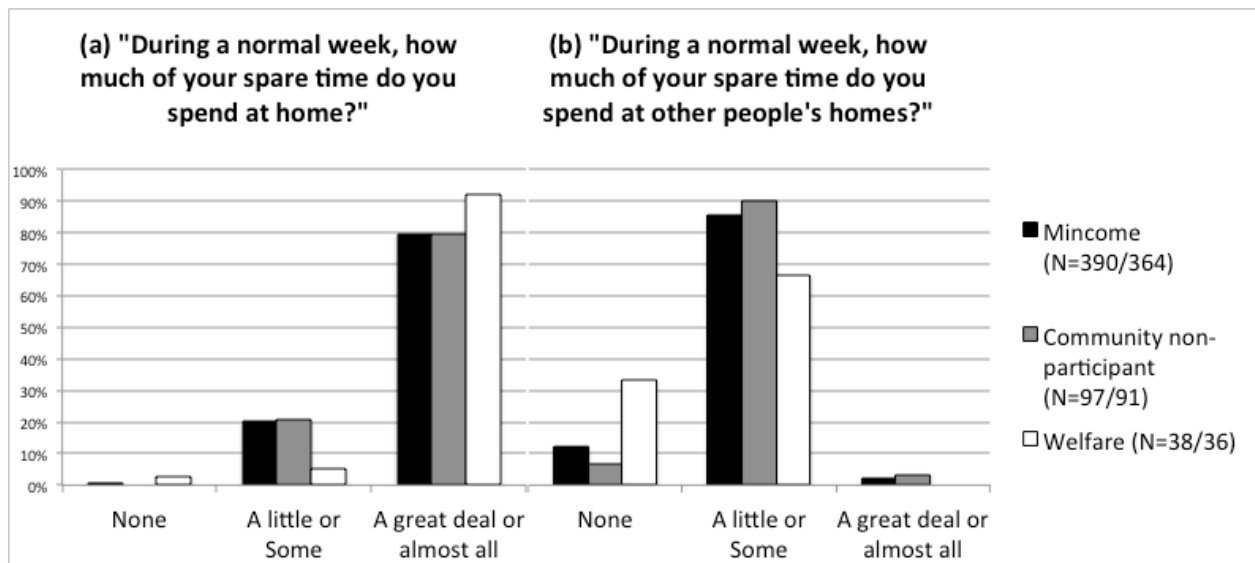
NOTE 1: To generate this table, the community survey discussed in this paper was merged with family ID numbers in "baseline" data on participant families, available from Library and Archives Canada. Missing data exist due to incomplete baseline information and "walk-ins" (in Dauphin) during the first experimental months. In order to fill in some missing data, I supplement the "baseline" data with another dataset containing information on actual payments made. This fills in data on age and familial status when missing in the baseline data but available in the payments data. Supplementary payments data is unavailable for education and children under 6, and as such those items contain more missing data.

NOTE 2: The observational unit in this table is the household rather than the individual.

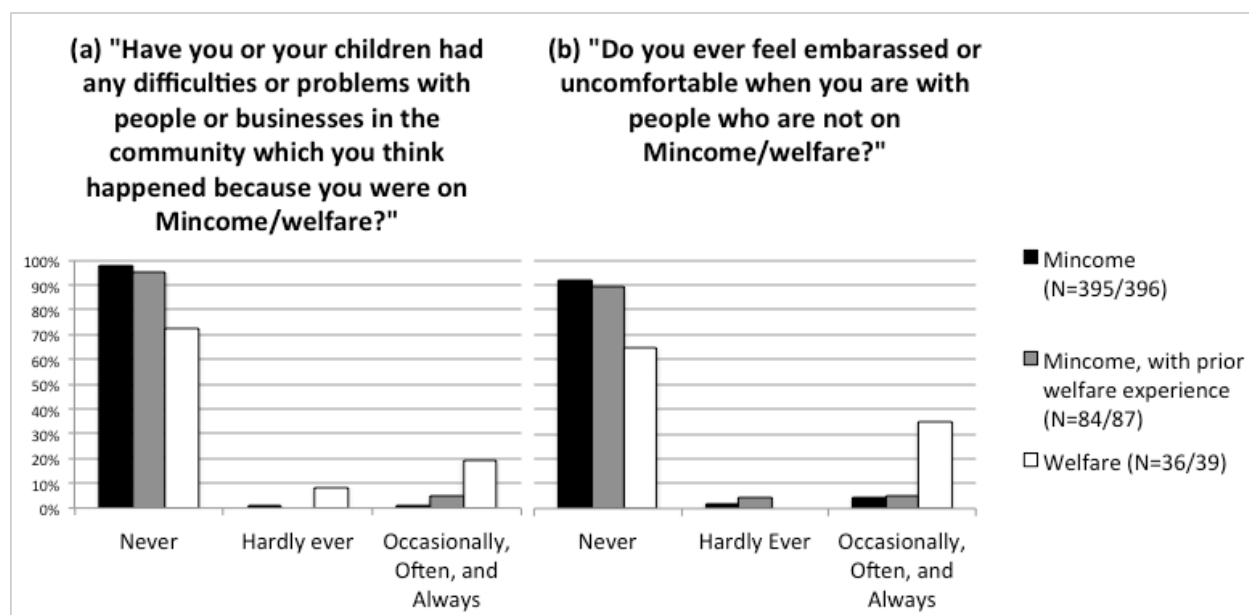
**Table 3:** Descriptive statistics at a baseline interview



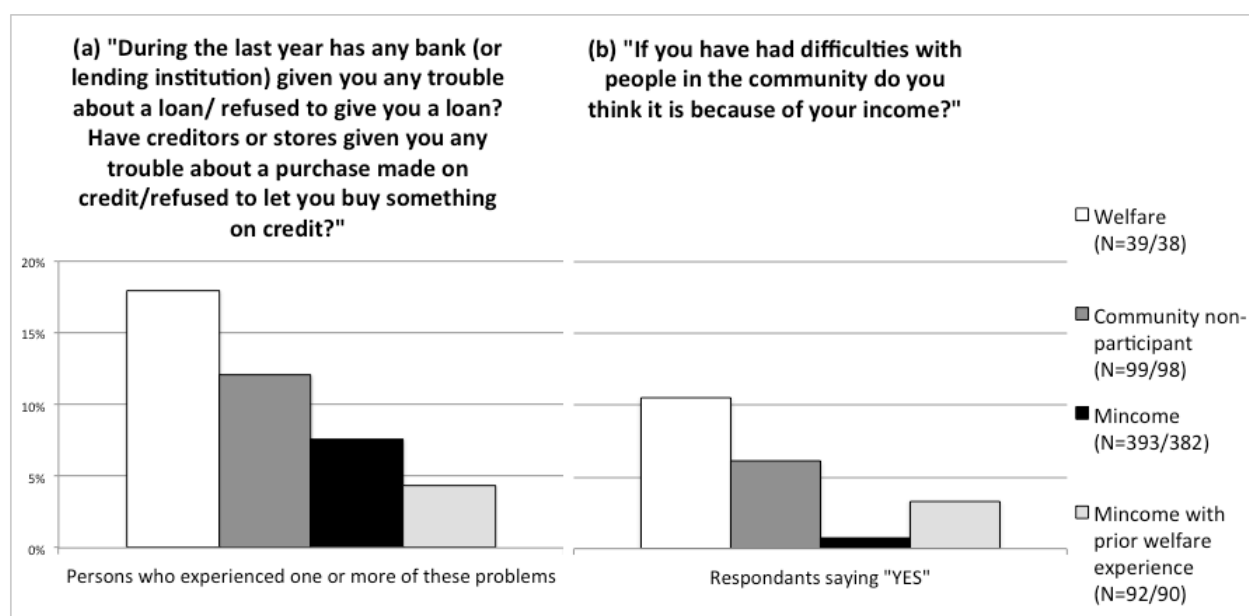
**Figure 3:** Spare time spent with various people



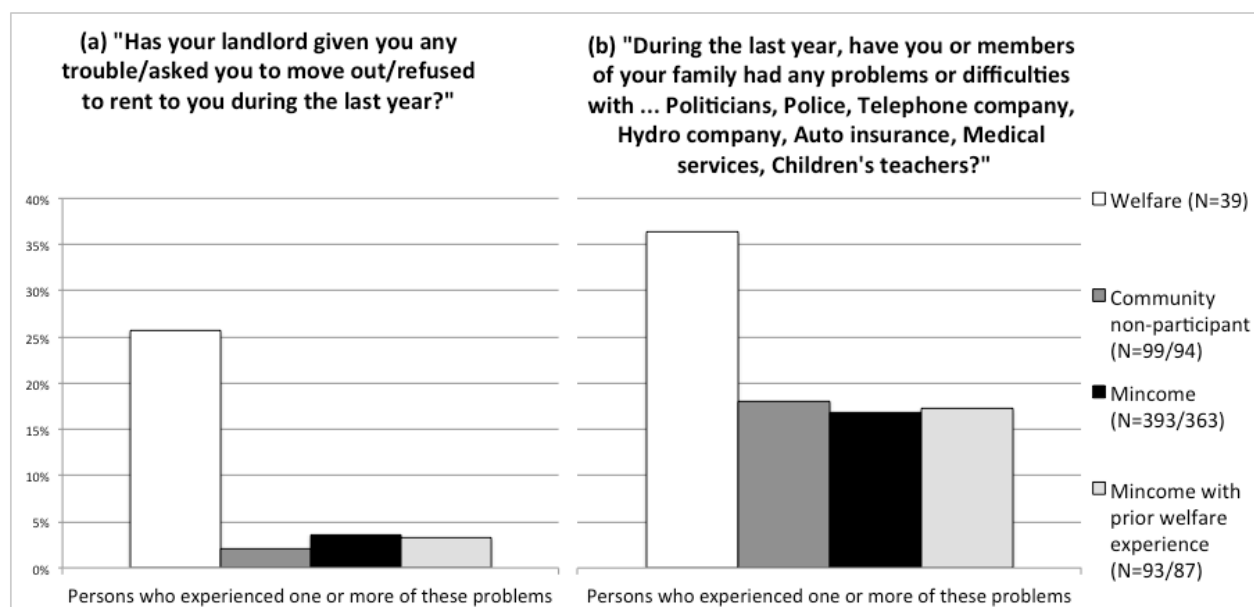
**Figure 4:** Spare time spent at home and at other people's homes



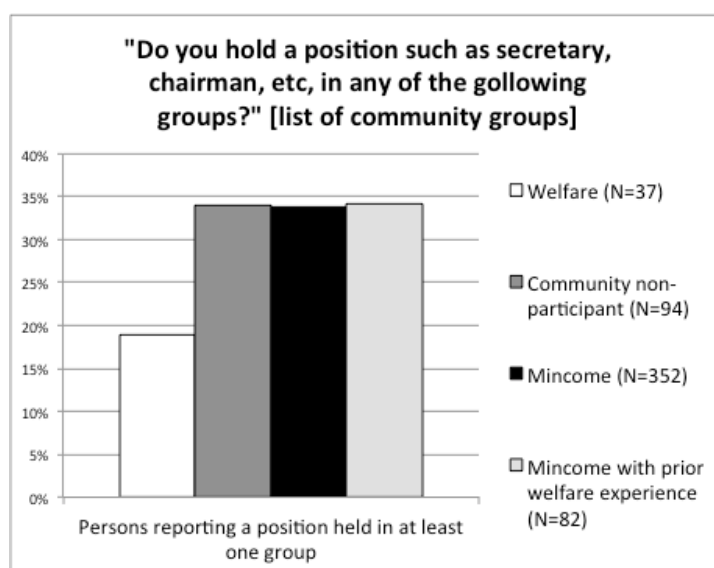
**Figure 5:** Difficulties and embarrassment in the community due to Mincome/welfare



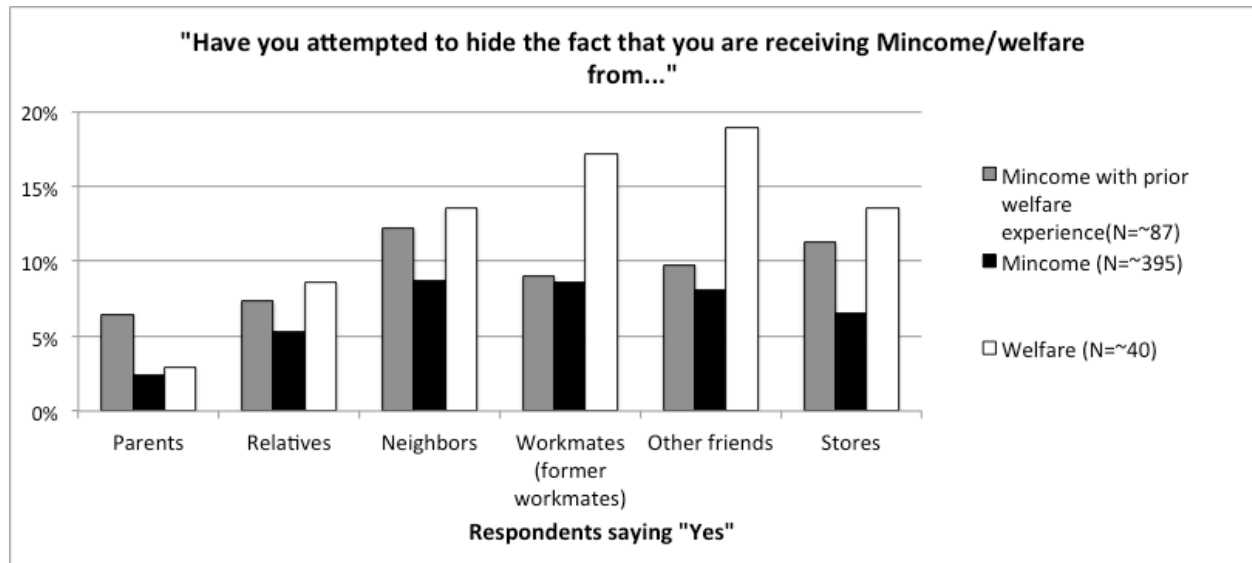
**Figure 6:** Difficulties with banks and difficulties related to income



**Figure 7: Difficulties with landlords and other community authorities**



**Figure 8: Positions in community groups**



**Figure 9:** Hiding program participation



Mincome participants who would NOT go on welfare (if it would improve their income), asked: "Why wouldn't you?"				Corresponding Subsample Answers (if available):	
CODING CATEGORY	N	%	SAMPLE ANSWERS	...	"Why Join Mincome?"
Would rather work/support myself	43	37%	"Because I'd go crazy doing nothing at home - I feel more useful working"	...	"To back up my financial state in case of sickness etc."
			"I can make my own living"	—	—
			"I would rather work if I could"	—	—
			"I'd rather work first"	...	"We were asked and we accepted"
			"I wouldn't go on welfare unless I was extremely desperate. I would like to think of myself as being capable of supporting myself ... I feel I could find work enough to keep me off welfare." She joined Mincome because "I felt it would help our situation, and invalid husband with no income."	...	"I felt it would help our situation, and invalid husband with no income."
			"Welfare to me was accepting something for nothing,"	...	"Would be a benefit to me at some time"
			"I am able to support myself"	...	"I might get some assistance"
			"I'm healthy and can be self-sufficient I feel"	...	"It provided one with enough income to live sufficiently."
			"felt better earning his own income"	...	"Statistics re: guaranteed income might help in studies ... added income perhaps best feature!"
			"I believe if a person is capable of working he should work instead of accepting charity"	...	"To receive enough money to meet our needs"
Don't want to/ need to	19	16%	"I am capable of earning a living and feel no reason to be on welfare"	...	"low income I receive"
			"Prefer to work for every dollar of income on my own,"	...	"to help in the survey"
			"I'm capable of working and I believe every able-bodied man should work"	...	"To help pay expenses that my salary did not quite cover... Being on Mincome helps to stretch the pay cheque a little farther each month. Which I for one appreciate."
			"Don't need any"	...	"No reason"
			"There is no need to"	...	"Test program"
			"Never would"	...	"Short of money"
			"No need of it."	...	"Thought it was a good program"
			"Don't need welfare"	...	"As help when a student and for survey"
			"I wouldn't"	...	"Need more money to make ends meet"
			"Because there is other ways to make a living. I went along with Mincome, since I feel it's my duty to Canada to go along with new social programs."	...	"Because I think it is good for Canada."
			"For myself, I don't think I would go on welfare."	...	"It helped me very much during winter months when work was not too plentiful."
The terms/ conditions of welfare not acceptable	14	12%	"Welfare would take everything"	...	—
			"Because we would have to forfeit all we own"	...	"Because we mink ranch and the money made from mink ranch would feed the mink but not the family."
			"It is too long of a story. I know what it is to be on welfare and I would never want to be on it again."	...	"security reasons" ... "It helps the below the average earning without having to go through doctors etc. You can live where you want to. You can spend your money when you need to and save for the next month when more will be needed. There are many more things I think Mincome is just the thing for the below the average earnings. People feel more secure knowing that if they need help it is there. I would need pages to be able to put down why I think Mincome was a wonderful way of helping people out."
			"Welfare is more uncertain. Workers are rude, incompetent, etc."	...	"For the money"..."Mincome people seem very considerate ... I like Mincome in that one is left alone, never harassed or made to feel like you had to crawl to receive an almighty dollar. I don't like the idea that it is intended to end shortly, with nothing to replace it but that same lousy welfare"
			"Can't sit still."	...	"For help."
			"Because they are too noisy and you always feel like they're watching you around the corner".	...	"No other income and found it very successful and a very great deal of help to my family"
			"Because we were on welfare when I was at home and I don't like the way people treat you."	...	"I don't know"
			"status"	...	"Extra income"
			"pride"	...	"Asked to"
			"I wouldn't want to destroy my dignity and pride."	...	"Mincome picked our name and asked us to be on the program"
The stigma of welfare not acceptable	9	8%	"It would make me feel bad and think people may be laughing at me"	...	"To help along with the expenses".
			"Other people abuse families on welfare and talk about them"	...	"For government experiment"
			"It makes a bad image on the family"	...	"I wanted to take part in the Mincome experiment."
			"Because I have my pride. As long as I am able to work I will own my own way"	...	"Because I was approached and thought it would help the average person with my information."
			"Because we were on welfare when I was at home and I don't like the way people treat you"	...	"I don't know"
			"Because Mincome seems more normal than welfare."	...	"We had no other choice as my husband is disabled and with my health and age, I am not able to work full time."
			"I would like to keep my self-respect which is lost when on welfare."	...	"Because of cost of living and to give my family a regular living standard, more in line with the people around us."
			"Welfare is only for needy or bums"	...	"Extra income"
			"Welfare is for people not able to work"	...	"Why not?"
			"I feel that [welfare] is more for disabled or people which are too lazy to work. It doesn't include us, we're both able and willing to work but can't get a job due to the low employment rate."	...	"Short of money to be able to survive"
Welfare is for other people	9	8%	"Welfare is for people who can't work. I'm able to work—even if it's not exactly the type of work I may prefer."	...	"research assistance"
			"I feel that is more for disabled or people which are too lazy to work. It doesn't include us, we're both able and willing to work but can't get a job due to the low employment rate."	...	"Short of money to be able to survive"
			"Welfare is for people who can't work."	...	"It sounded like it was a little fairer than unemployment insurance."
			"I believe in self help and don't see why people should receive money by not trying to earn it on their own"	...	"To help assess a proper rate of income to help people who require same"
			"I believe in working for your money"	...	"I was asked to."
			"Because I will just cheat the government and the people who are paying their taxes to support the welfare"	...	"It helps us a little bit in our family expenses"
			"Welfare should be used only for those who require it not abused by those who really don't need it."	...	"Extra income really helps when one gets it today"
			"I don't believe in it. Unless a person really has to."	...	"We were interviewed when they started with it."
			"Don't believe in it. Would get a better job if I had to."	...	"Help the program. Needed money."
			"I feel there are far too many able bodied people on welfare now who should or could be working."	...	"It sounded like good insurance."
Mincome is better	4	3%	"I don't really believe in welfare"	...	"I have a child to take care of and didn't want to go on welfare"
			"Mincome is better"	...	"Because I need the money"
			"Like the mincome system better."	...	"Didn't want to live off my parents."
			"Somebody has to pay for the welfare in taxes. If only the needy received welfare it would be alright, but there are a lot of freeloader..."	...	"test"
			"I'll suffer instead"	...	"needed extra money"
			"why should I"	...	"no reason"
			"I just wouldn't"	...	"I thought it was a good program"
				...	
				...	
				...	
TOTAL	116				

**Table 4: Why Mincome Participants would not join welfare; why they joined Mincome**

### **3. Domestic Violence and Gender Relations in Manitoba's Basic Annual Income Experiment**

#### **Abstract**

This paper examines domestic violence and gender relations in the context of an understudied social experiment from the late 1970s called the Manitoba Basic Annual Income Experiment, or Mincome. While Mincome took place in three sites, this research focuses on the so-called “saturation” site located in the town of Dauphin, Manitoba, where all town residents were eligible for unconditional payments. The paper is motivated by an empirical finding: Using aggregate town-level crime data, I find that during the Mincome years, Dauphin saw a large fall in violent crime, most of which are assaults taking place within the family. To understand the causal pathways behind this fall in violence, I analyze an archived survey of married women, which was conducted at a baseline and during the study period, and was completed by both treatment and control groups. The survey facilitates investigation into three potential mechanisms. The first explores the link between Mincome and reductions in financial stress, disagreement, and conflict between partners; the second explores the link between Mincome and the ability of women to exit bad or potentially abusive marriages (thereby reducing exposure to violence); and the third explores the possibility that Mincome reduced violence by reshaping underlying power relations between partners. I find most support for the first mechanism. The guaranteed income may have reduced financial stress and disagreement, thereby reducing the likelihood of domestic assault.

## 1. Introduction

What is the impact of economic resources on the family? Would unconditional cash payments weaken patterns of domestic abuse? Could it do so by ameliorating financial stress or by improving women's power and independence from husbands? The guaranteed income experiments of the 1970s (see Munnell 1986; Levine et al. 2005; and Widerquist 2005) provided an opportunity to explore these questions. Although attention was concentrated on labor market consequences, the experimental guaranteed annual income (GAI) literature also examined aspects of family dynamics (Tuma and Hannan 1990; Cain and Wissoker 1990; Cain 1986). However, with respect to familial relations, the specific question, as framed in the debates, was whether the guaranteed income would lead women to leave their husbands. Would the guaranteed income, in the lingo of the time, "undermine the family"? (Munnell 1986) In the U.S. experiments, some researchers purported to find evidence of marital dissolution (Groeneveld et al. 1983; Tuma and Hannan 1990), while others, using the same data, disputed these findings (Cain 1986; Cain and Wissoker 1990). Though the debate is still somewhat unresolved the first set of findings received a louder public hearing, and arguably (see Steiner 1981; Greenberg et al. 2003) was an important reason why movement on the GAI stalled.<sup>34</sup> In the late 1970s public debate on the question of the "strength of the family" aroused a highly emotional response (see Coyle and Wildavsky 1986), which led to energetic recantations by high-profile supporters. Daniel Patrick Moynihan, a liberal senator and early proponent of the guaranteed income, withdrew his support in light of the apparent evidence on marital dissolution. In the pages of *National Review* he wrote to William F. Buckley: "But we were wrong about a guaranteed income! Seemingly it is calamitous. It increases family dissolution by some 70 percent,

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<sup>34</sup> In interview economist Henry Aaron argued that the Seattle-Denver marital stability findings "destroyed what little chance there may have been" in passing GAI legislation (Greenberg et al. 2003: 144).

decreases work, etc. Such is now the state of the science, and it seems to me we are honor bound to abide by it for the moment” (Moynihan 1978). Conservatives too seized on the findings. George Gilder testified to Congress in 1980, declaring that the GAI would mean “millions” of “marriages would be in jeopardy” (1980: 378).

Both the academic literature and the subsequent popular debate framed the issues in an exceedingly narrow fashion. “Calamitous” outcomes meant that some marriages might break up. It was seldom acknowledged that if some marriages dissolved, perhaps they were bad or abusive marriages, formed and sustained in the context of limited alternatives. Likewise, if some marriages were stabilized—as others found—then perhaps it was because the guaranteed income ameliorated underlying stressors. This paper returns to the GAI experiments, focusing on an understudied experiment called the Manitoba Basic Annual Income Experiment, or Mincome, and expands the discussion of the relationship between the GAI and familial relations by turning attention to an adjacent but broader set of issues. I use town-level crime statistics, merged with census control variables, to demonstrate a significant negative relationship between the guaranteed income and domestic assault. I then use individual-level survey data to investigate the chain of events linking these variables. In particular, I investigate the shifting dynamics interior to the family, locating three potential mediator variables: (1) financial stress and disagreement; (2) women’s exits from relationships; and (3) the changing balance of power within relationships. I explore these channels theoretically and then examine survey data to discover whether these underlying mediators were in fact present.

I draw an immediate link to the early literature on marital dissolution for two reasons. First, that literature touched upon some of the mechanisms that help to explain the link to

domestic assault, albeit unintentionally.<sup>35</sup> Just as a guaranteed income might stabilize marriages by reducing financial stress, it might reduce domestic assault by weakening the link between financial stress and the risk of a violent incident. By the same token, while the guaranteed income might destabilize marriages by giving women an exit option, it might also reduce domestic assault by ending bad or potentially violent relationships. Second, the early debates serve as a touchstone from which to broaden the scope of normative concern. While the original discussion on marriage break-up was framed as a normative debate about the wellbeing of the family, the question of intimate partner violence, along with the mediators discussed below—financial stress, women’s independence, and power—are arguably of greater consequence for familial wellbeing than the second-order question of marital stability or dissolution. Insofar as the objects of our interest are the “calamitous” effects on *people* inside families, rather than the abstract strength of marriage as an end in itself, the main concern ought to be the wellbeing of those people rather than the wellbeing of those marriages.

This paper examines the effects of a guaranteed income on domestic violence and familial dynamics using data from the Mincome experiment, a three-year (1975–1977) guaranteed annual income (or more broadly, “basic income”) test conducted by the Canadian and Manitoba Governments. Participants were able to access a GAI equivalent to about \$19,500 CDN for a family of four. While Mincome took place in three sites, this study focuses on the so-called “saturation” site located in the town of Dauphin, Manitoba, where all town residents were eligible for Mincome payments. The other two components of the experiment, set in rural Manitoba and Winnipeg, were modeled on prior U.S. income maintenance experiments and tested randomly assigned treatment subjects against controls. The latter control group provides a key comparison point for this study.

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<sup>35</sup> This paper uses the terms “mechanism,” “mediator,” and “pathway” interchangeably.

While there is a good amount of evidence linking domestic abuse to economic hardship and financial stress (Gelles 1997; Benson et al. 2003; Hardie and Lucas 2010; Golden et al. 2013), these individual-level analyses typically cannot rule out the possibility that some third unmeasured variable caused both economic hardship and domestic abuse. The virtue of an experiment is the ability to link outcomes with a fundamentally exogenous cause, in this case the availability of Mincome payments.

On the other hand, even if experiments provide some of the strongest tools available to social scientists concerned with identifying outcomes, or treatment effects (Banarjee and Duflo 2013; Angrist and Pischke 2008), they are decidedly weaker in their ability to explain the mechanisms underlying those outcomes (Cartwright and Hardie 2012; Deaton 2010). One can discover a treatment effect induced by an experiment without having a good sense of the pathways tracing the experimental treatment to the outcome. The latter process can be as speculative as the former is rigorous. This paper first examines the link between the availability of Mincome and diminished rates of town-level violent assault, the central outcome of interest. I then attempt to locate the mechanisms underlying these changes by examining a detailed and, in the context of the GAI experiments, atypical survey of wives and husbands.<sup>36</sup> Rather than speculating on the possible theoretical channels through which this outcome variable, domestic violence, might have been impacted, this survey provides an opportunity to discover the presence or absence of the variety of potential mediators. In other words, the second part of this paper treats potential mediator variables as outcomes, which may or may not be detected. This helps to tease out the specific pathways that link Mincome to changes in domestic assault.

The second section introduces the Mincome experiment. Section 3 introduces three hypotheses in the context of the literature linking economic variables to domestic assault. The

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<sup>36</sup> My analysis, however, excludes husbands and uses data from wives only.

first mechanism suggests that Mincome reduced financial stress in families, which reduced the risk of a violent incident. The second mechanism suggests that Mincome led to marital splits—as women could move out and collect payments as single-person households—which reduced violence by ending bad relationships. The third mechanism suggests that actual separation is not the primary factor. Rather, the availability of an independent source of income shifted the domestic balance of power by increasing the credibility of the *threat* of exit. Thus, it was the changing positional power of women, women's expanded structural capacity, which reduced the risk of violence. This final mechanism is of special normative interest to feminists whose concern with the family is broader than the narrow preoccupation with marital stability.<sup>37</sup>

Sections 4 and 5 discuss data and methods. It is worth noting at the outset that Mincome was underfunded; but rather than reducing incomes to households, it was the analysis side of the project that was completely cut. The project produced no final report, and the survey data collected on Dauphin has never been analyzed. Subsequent to the end of the Mincome experiment, a small number of journal articles were produced from the digitized Winnipeg data (Hum and Simpson 1993; Simpson and Hum 1991; Prescott et al. 1986; Choudhry and Hum 1992); however, no published research has examined the survey records available on the Dauphin portion of the experiment. I focus on two primary data sources: (1) aggregate crime statistics, and (2) hitherto unexamined data from an archived survey of wives and husbands, which I digitized. In the context of other experimental GAI surveys, the archived survey is rare

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<sup>37</sup> The guaranteed income has clear appeal to many on the left because it promises to weaken relations of domination in one crucial arena of people's daily lives, the workplace. It can be viewed just the same as an opportunity to weaken relations of domination in an equally crucial arena of people's lives, the family. Poverty policy and poverty research is rarely concerned with reshaping power dynamics, the background conditions under which negotiations around work and familial roles take place (see Katz 2013 for an overview). Although the experimental GAI literature largely ignored these questions, there is good reason to believe the policy might have impacted power-relations in the family, which in turn may affect a range of outcomes, including domestic violence.

in its substantive focus, inquiring into disagreement and conflict, discussion of separation, and domestic power-relations. It was administered once at the baseline and once during the study period in the Dauphin group and in the Manitoba control group. In order to evaluate evidence on the second, and most controversial mechanism—that is, exits from marriage—I use additional data sources, triangulating the above survey data with census data and original panel data assembled by Mincome researchers. The panel data includes information on the attrition and split status of treatment and control couples during every survey period throughout the experiment (whereas the survey discussed above asks only about “divorce talk”).

Section 6.1, the first results section, uses a fixed effects model to examine Mincome and violence at the aggregated town-level. Section 6.2, the second results section, examines in turn the three proposed mechanisms primarily using the micro-level survey of married women. The mechanism with the most support suggests that Mincome reduced violence through the reduction of financial disagreement, as well as alcohol-related disagreement. The mechanism suggesting that Mincome led to increases in actual separations or discussion of separation is not supported. However, I do find some support for an ancillary issue, namely, that Mincome destabilized “bad” marriages, but stabilized “good” marriages. Couples in “bad” marriages at the baseline—i.e., high and very high disagreement marriages—saw increases in “divorce talk”, whereas couples in “good” marriages at the baseline—i.e., low disagreement marriages—saw declines in divorce talk. Because this growth in discussion of divorce for a subset of the full sample is not matched by significant differences in actual splits, I see no support for the second mechanism. Instead I consider the possibility that changes in the discussion of divorce reflects on the third mechanism, concerning changes in bargaining power and decision-making. However, direct tests of changes



in bargaining power see small, insignificant effects, and thus do not provide support for this final hypothesis.

Overall, I argue that the first mechanism, that Mincome reduced violence by reducing financial stress among families, is the most promising of the three. Recent research (Schneider et al. 2016) has shown that domestic abuse is linked not only with economic hardship, but also with economic uncertainty and anxiety. The guaranteed income reduces economic hardship, but it is designed in particular to respond to economic uncertainty, and as such there is good reason to view it as an important policy tool to block off a key pathway to domestic violence. I conclude with some remarks about the three proposed mechanisms, as well as the lessons and limitations of this study.

## **2. Mincome**

Mincome was devised in response to a parade of influential reports that publicized the extent and depth of poverty in Canada in the late 1960s and early 1970s. The Economic Council of Canada (1968) and the Department of National Health and Welfare (1970) presented the guaranteed annual income as an intriguing idea meriting serious consideration. The “Croll” Report (1971) and The Real Poverty Report (1971) posed the guaranteed income as the central policy solution of the era, an idea “whose time has come” (Canada 1971: 175). Inspired directly by four similar experiments in the U.S., it was hoped that Mincome would demonstrate the feasibility of the guaranteed income to the Canadian public.

As with the U.S. studies, the primary axis of the demonstration concerned potential effects on the labor supply. Questions regarding the well-being of families were not at the forefront of discussion in Canada. However, the early academic documents and reports

influencing the design and execution of Mincome showed demonstrable learning from the U.S. experiments (Hikel and Harvey 1973; Atkinson et al. 1973). In particular, they insisted on a more expansive vision of the role of poverty in social life.<sup>38</sup> Likewise, relative to their American counterparts, the secondary question of familial well-being was framed in broader terms. Potential effects on the stability of marriage never rose to the level of political debate as they did in the U.S. (Cain 1986) and early planning documents do not evince a singular preoccupation with the degree to which marriages were put in “jeopardy”:

We anticipate that policy makers will require a firm fix on the question as to whether or not effects of the GAI on families will be favourable, benign or harmful before a universal program could be considered. Therefore, this question will be approached from a number of perspectives. Here, key questions of the following sort concern us: ‘What are the effects of GAI upon family stability and family splitting?’ ‘How does GAI affect levels of family integration-disintegration?’ ‘How does GAI affect harmony within families’ ‘How does GAI affect family decision-making patterns regarding expenditures, work effort and other matters?’ (Hikel and Harvey 1973: 20).

A somewhat more holistic understanding of poverty, and of familial well-being, led to a broader research design, one including a “saturation” site. It also led to a more wide-ranging use of research tools, including ethnographers and an array of survey instruments. Indeed, there was enough interest in a variety of issues related to the family to motivate additional variables and even additional surveys beyond those directly measuring separation.<sup>39</sup> One unique survey,

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<sup>38</sup> Indeed, Marx and Weber were occasionally consulted. One early report includes the following: “[W]e now stress the importance of seeing poverty in broader focus. Four basic dimensions of insufficiency and deprivation can be usefully distinguished—wealth, status, power, and self-fulfillment. Marx has forced us to see these phenomena as interrelated, and, in the final analysis, perhaps determined by wealth, but Weber has properly demanded their analytic independence, and the evaluation of their empirical interrelationships. At any rate, this monograph takes the position that the choice of policy to alleviate poverty must be based on evidence that some programme has an optimal effect on increasing the standard of living of the poor in each of these four dimensions” (Atkinson et al. 1973: 6).

<sup>39</sup> One research plan, never carried out, went as follows: “The second methodological approach to the study of the family involves an in depth study of about 25 families. Admitting that the present state of sociological theory does not permit the formulation of well grounded hypotheses about the direct effect of a GAI on the internal dynamics of the family and recognizing that the questionnaire approach may not be sensitive enough to pick up the impact on family stability, part of our research will use this more

discussed in detail in the next section, was designed specifically to measure a broad range of outcomes with respect to the family—from power, decision-making, and the domestic division of labor, to various kinds of disagreement and harmony in the home. While this survey did not ask directly about domestic violence, it did ask about conflict between partners.

Before moving on, it is important to make a few points about the size, distribution and details of Mincome payments.<sup>40</sup> Dauphinites were offered guaranteed incomes equivalent to \$19,500 for a four-person household.<sup>41</sup> Families with no labor market income, for whatever reason, could access the full guarantee, 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. At a negative income tax rate of 50 percent, people could always increase their incomes by working. Every dollar of labor market earnings reduced the guarantee by 50 cents; this meant that payments were phased out entirely once earnings reached \$39,000.<sup>42</sup> To put these payment figures in perspective, real median household income for Dauphin and its rural municipality was only \$24,758, and median family income was \$39,166, according to the 1971 census. By the middle of the experiment in 1976, I estimate that real median household and family incomes were \$39,382 and \$51,055, respectively.

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qualitative anthropological approach. Three trained participant observers will observe the families by living with them for 2 or 3 days at a time, several times during the experiment. The focus of this study is that set of relationships between members of the family, between family and kin, and family and the community. Here again considerable insight will be gained into the distribution of wealth, decision-making, and time budgeting within the family” (Hikel, Laub and Powell 1974: 42).

<sup>40</sup> In a town with a population of 8,885, along with a 3,165 person rural municipality, at least 18 percent—2,128 individuals, or 706 households—received benefits at some point throughout the program. (This is a lower bound because available data excludes late-joining farm families; an estimate of this group increases the participant count to 2,457, or 20 percent of the population.)

<sup>41</sup> This figure is adjusted from the 1976 payment guarantee and presented, like all figures, in 2014 dollars.

<sup>42</sup> Positive tax liabilities were rebated too; the rebate faded to zero once market earnings reached around \$43,400.

Guarantee levels varied by family size and composition. They were, however, designed to be “neutral” with respect to marital separation (Hikel and Harvey 1973; Hum, Laub, and Powell 1979) while accounting for variation of real needs across family size. Mincome attempted to design a payment structure that generated neither penalty nor benefit to splitting, and one that did not systematically favor any one household size. This meant taking into account average economies of scale in the home. In particular, since per-person housing costs decline with additional household members, so should per-person household guarantee levels.<sup>43</sup> In practice this meant that a single-person and a couple without children, respectively, would receive 38 and 71 percent of the four-person standard.<sup>44</sup> The expressed aim was to eliminate any strictly economic reasons to dissolve a marriage.

Despite this, the payments design should not be characterized as neutral. What was eliminated—if, that is, the design was successful—was the economic benefit relating to one or another family size. This did not remove all economic reasons for separation, particularly, those rooted in motives related to economic independence. Even if the payment scheme did not advantage any one family size, and even if it correctly calculated average economies of scale, Mincome made real the possibility of exiting unpleasant or abusive relationships, thereby making possible the option of economic independence where it was otherwise off the table. By restructuring the set of available alternatives outside of marriage, Mincome reshaped the power dynamics interior to it.

Apart from research on the effect of the guaranteed income on marital dissolution, effects on the family more broadly were sidelined in the experimental literature. In the next section we

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<sup>43</sup> Of course, a purely neutral payment structure is unattainable because an estimate of average economies of scale will likely differ from those in any particular household.

<sup>44</sup> This meant that a couple living separately would together receive about 107 percent of what they would receive living together.

discuss the various mechanisms that mediate the relationship between the guaranteed income and domestic strife.

### 3. Violence and its Underlying Mechanisms

An important hypothesis advanced by advocates of basic income is that income guarantees may be inversely correlated with a range of crimes (Offe 1992; Groeneveld 1979).<sup>45</sup> This thinking supposes that crime declines with material deprivation because as marginalization disappears so should its associated social pathologies.<sup>46</sup>

Using aggregate town-level crime data, Figure 1 shows a roughly 23 percent decline in total crime in Dauphin, comparing Mincome years to non-Mincome years (the first payments began in December 1974, and baseline interviews were conducted earlier in the year).

Undercutting poverty, in this case, seems to substantially undercut crime. While these findings

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<sup>45</sup> The counter-hypothesis would suggest that insofar as crime has roots in “idleness” (e.g. Jacob and Lefgren 2003), which perhaps is fostered by basic income, we should expect crime to grow. “To be idle,” Samuel Johnson wrote, “is to be vicious” (1968: 145). In a similar vein, Jeremy Bentham saw the workhouse as a “mill to grind rogues honest and idle men industrious” (cited in Polanyi 2001: 126). However, the link between idleness and crime is not the sole province of eighteenth-century political pamphlets. For example, sociologists Sara McLanahan and Gary Sandefur argue that “idleness and inactivity are a sign of problems to come. Young adults who are not attached to the labor force or who work only intermittently may not develop the skills necessary for achieving economic security and social success later on. Being idle is also often associated with crime and drug or alcohol abuse” (1994: 21). On purported links between idleness and crime, see also Anderson et al. (2000), Snyder and Sickmund (1999), and Allan and Steffensmeier (1989).

<sup>46</sup> The only attempt to study the effects of the GAI on crime using experimental data was a preliminary analysis of the Seattle and Denver experiment by Groeneveld et al. (1979), revealing little or no effect. Perhaps the best evidence comes from a study that can be interpreted as mimicking a basic income experiment. Akee et al. (2010) tracked children in households where incomes are increased exogenously through a governmental transfer program. Midway through a study of mental health in North Carolina, which was collecting data on American Indian and non-Indian children, a casino opened on the Eastern Cherokee reservation, and began to distribute, unconditionally, a per-capita portion of casino profits to tribal members. Researchers found that “treatment” children had a statistically significant, 22 percent lower risk of committing minor offenses. Another potential source of evidence comes from a ten-month basic income project that was conducted by Lutheran missionaries in 2008, in Otjivero-Omitara, Namibia, where residents under 60 received a modest monthly grant, and one report finds a fall in overall crime (Haarmann et al. 2009).

are interesting, they are not particularly surprising from the perspective of a straightforward rational choice theory of crime à la Gary Becker (1968). More dramatically, Figure 2—which I analyze in section 6.1—shows a 44 percent decline in violent crimes comparing average Mincome years in Dauphin to non-Mincome years. Although much criminology accepts a direct and unmediated relationship between economic deprivation and property crime, the link between economic variables and violence is more elusive, usually operating through various mediating or conditional variables (Arvanites and Defina 2006; Rosenfeld and Messner 2013; Shihadeh and Ousey 1998; Kelly 2000).

<Figure 1 about here>

<Figure 2 about here>

The aggregate pattern in Figure 2 is highly suggestive, even if the underlying mechanisms remain unclear. Disaggregating this data shows that assaults accounted for 89 to 96 percent of the crimes of violence in Dauphin. Further, Statistics Canada notes that most of these offences are domestic in nature, frequently the result of family disputes (1977). However, even if violent crime reductions in Dauphin are largely attributable to changing social dynamics within familial and intimate partner relationships, there may be a range of mechanisms at work. Rather than providing theoretical speculation on the plausibility of various candidates, we describe three potential mechanisms below and test their validity in section 6.2.

Research on intimate partner violence has found a consistent pattern linking economic hardship to higher levels of domestic violence (Gelles 1997; Golden Perreira and Durrance

2013). In Canada, a statistical profile on family violence (Bunge and Levett 1998) isolated low income as a key correlate of spousal assault in observational data. Couples facing greater economic insecurity experience more stress, which may give rise to situational violence (Cunradi, Caetano, and Schafer 2002). Female respondents to Statistics Canada's one-off Violence Against Women Survey frequently cited stress over finances when asked open-ended questions about how violent incidents usually begin (Bunge and Levett 1998). In the literature, this is sometimes referred to as the "income" effect (Hannan et al. 1978), where an exogenous increase in incomes may reduce financial stress and disagreement and function to improve relationship dynamics.

Therefore, my first mechanism is as follows:

**(1) Risk of a violent incident is heightened by financial stress and financial conflict in the family; insofar as Mincome reduces financial stress, domestic assault will fall.**

A different mechanism pivots on the fact that the guaranteed income meant that women in unpleasant or potentially abusive relationships had the economic opportunity to move out to form new single-person households. The literature connected to the U.S. income maintenance experiments highlighted the "independence" effect of an exogenous income source (Hannan et al. 1978; see also Oppenheimer 1988). Recently, Cancian and Meyer (2014) found evidence to support the "independence" effect: additional income in the form of child support increased disadvantaged women's economic independence and lowered cohabitation rates.

As noted above, results from the experimental guaranteed income literature were fraught with controversy, and in the end, difficult to evaluate. Hannan and Tuma (1990) showed that the percentage of marriages experiencing dissolution was substantially higher for those with the guaranteed income than for the controls, 36 percent higher for blacks, and 40 percent higher for

whites. By contrast, Cain and Wissoker (1990), using the same data, found that the guaranteed income had virtually no effect on dissolution at all. Mincome data from the dispersed Winnipeg sample were analyzed by Hum and Choudhry (1992), who reported somewhat ambiguous results, lending no easy interpretation. The Winnipeg sample tested a range of payment levels: The most and least generous GAI schemes led to marital stability while the scheme in the middle led to instability. None of these coefficients were statistically significant. An updated paper on the same Winnipeg data (Choudhry and Hum 1995) again showed no statistically significant differences between treatment and control groups, suggesting no “independence” effect.

From the perspective of this paper, this mechanism pivots on the fact that (1) the guaranteed income meant that some women might exit from potentially bad relationships, and (2) reduced exposure to potentially violent partners may lessen the risk of a violent incident. For example, Reckdenwald and Parker (2012) found that the increase in the percentage of unmarried households may have reduced violence by reducing exposure to potentially violent partners.<sup>47</sup> On the other side of the coin, various studies (Kalmuss 1984; Golden, Perreira, and Durrance 2013) find that economic dependency on a romantic partner increased women’s exposure risk to intimate partner violence. Moreover, there is a good deal of research indicating that positive changes in women’s socio-economic status may empower them to leave abusive relationships at earlier stages (H. Johnson 1996, 2006; Bunge 2002; Dawson 2001; Dugan et al. 1999; Rosenfeld 1997). Absent an exit mechanism such as the guaranteed income, some women may be bound to potentially abusive relationships of economic dependence.

Lee Lakeman, a representative for the Canadian Association of Sexual Assault Centres, pointed out that in 1973 there were no rape crisis centers, transition houses, or women's centers

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<sup>47</sup> While fewer cohabiting adults may have reduced the exposure to violent partners, a counter-hypothesis suggests that the process of union dissolution itself may lead to violent incidents (See Jordan, Campbell, and Follingstad 2010; and M. Johnson 2006).



anywhere in Canada: “when they came into existence in major urban centers over the next five years they filled up in the first few months and stayed full” (cited in Cho, 2012). She noted that “as soon as there was an escape possibility, women took it, and they took it in large numbers” (ibid.). In a study of 48 large cities between 1976 and 1996, Dugan, Nagin, and Rosenfeld (2003) found that the growth in shelters, legal services and other resources made it easier for women to reduce exposure to abusive partners. At the time of the Mincome experiment these safety valves were far less available.<sup>48</sup> Access to a guaranteed income might have operated as the functional equivalent of a transition house.<sup>49</sup>

For the reasons given above, a second mechanism argues:

**(2) Since women were able to exit marriages of economic convenience by collecting payments as single-person households, Mincome reduced violence by facilitating direct exit from relationships of dependence, thereby reducing exposure to abusive partners.**

A variant on the hypothesis above suggests that Mincome may interact differently with particular household types. The program may destabilize “bad” marriages, but stabilize “good” marriages. That is, perhaps Mincome facilitates the possibility of separation in marriages where divorce or divorce threat is appealing because the partnership is an unhappy one. By contrast, happier partnerships may use Mincome payments as a source of stability, ameliorating potential conflict rooted in economic hardship. Therefore, rather than looking exclusively for an overall effect, we examine an additional, more narrow version of mechanism two:

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<sup>48</sup> In Canada, more than nine-tenths of the 2000 Transition Home Survey respondents reported that they were “unable to find affordable housing.” In 1998, according to data from Statistics Canada’s 1992–93 Transition Home Survey, there were 550 shelters and facilities for abused women across the country. Between 1975 and 1979, this number was 57, and only 18 such facilities existed prior to 1975.

<sup>49</sup> The Transition Home Survey gives a picture of female residents’ reasons for admission to shelters. In 2000, about 81 percent of women reported reasons connected to “abusive situations” (Statistics Canada, CANSIM Table 256-0004). These facilities are in demand in Manitoba; over the past fifteen years, annual admissions reported by shelters have hovered between 4,400 and 8,600 (Statistics Canada, CANSIM Table 256-0013).

**(2.i) Mincome facilitates the divorce threat in high-disagreement marriages, while stabilizing low-disagreement marriages.**

An adjacent but different mechanism considers the issues related to domestic violence in terms of “prevention” rather than “escape.” Evidence from the Violence Against Women Survey indicates that in a third of all reported violent incidents women were able to halt their husbands’ further use of violence after one initial episode by leaving or by *threatening* to leave (Johnson, 1996). Likewise, Farmer and Tiefenthaler (2003) argue that when women suffering abuse can support themselves they are more likely to exit. However, the authors also argue that when those women decide to stay, they are more likely to improve their power within their relationships. Both factors reduce the incidence of domestic violence. Rather than simply making exits more likely, Mincome may have affected the balance of power within relationships by making the threat of exit credible. It may also mean that relationships prone to large inequalities in power were less likely to form and solidify.

Like the second mechanism above, the factor at the core of this hypothesis is power. In this case, however, power operates differently. Rather than measuring power through its direct deployment, it is conceived as a structural capacity. The power to exit from a marriage may be present even if it is not used. An analogy here is union power: the power of unions can be measured both by the presence and by the absence of strikes (Perrone 1983). A goal is sometimes better realized through the threat, not the direct exercise, of power.

Feminist theorists have long argued that domestic violence results fundamentally from gender-power inequities (Heise 1998; Dobash and Dobash 1992). Indeed, there is evidence directly linking power and domestic violence. Male control over their partners operates as a key

feature in two of Johnson's (2006) four empirically derived categories of intimate partner violence (see also Johnson and Ferraro 2008; Johnson 1995). Likewise, Levinson (1989) found that one of the strongest predictors of intimate partner violence was male economic and decision-making power in the family. Aizer (2010) found that increasing women's bargaining power and improving outside options (manifested as declines in the gender wage gap) lowers levels of domestic violence.<sup>50</sup> Pollak (2005) extends this perspective to define women's alternatives in terms of overall labor market conditions, implying that violence falls not on the basis of women's actual incomes but their *opportunities* to earn independent income. In the 1970s, when the alternatives to family life were limited for many women, these issues may have been more pronounced. The 1975 U.S. National Family Violence Survey shows that domestic violence is almost four times more likely among couples with a clearly dominant husband as compared to couples where women had roughly equal decision-making power (Straus et al. 1980, cited in Heise 1998).

For these reasons, the third mechanism shifts attention from the dissolution of marriage to changes in the power relations interior to them, from actual exit to the threat of exit:

**(3) Since women were able to exit marriages of economic convenience by collecting payments as single-person households, Mincome reduced violence by making credible the *threat* of exit, thereby increasing the bargaining power of wives vis-à-vis husbands.**

#### 4. Data

##### *Town-level violence data*

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<sup>50</sup> By contrast, theories of "male backlash" predict that women's financial independence or gender equality tends to threaten norms of male dominance, thereby *increasing* domestic violence (Macmillan and Gartner 1999). See also Hoffman and Duncan (1995).

The analysis in this paper draws on a variety of data sources, and to keep track of them, Table 1 summarizes each. The central data source is one hitherto-unexamined raw survey of married women that I have digitized. However, I begin my analysis by examining aggregated total crime and violent crime data from Statistics Canada's Uniform Crime Reporting survey.<sup>51</sup> Here, I use data for all (N=16) Manitoba and Saskatchewan towns with mid-1970s populations between 5,000 and 50,000. I examine data on crimes of violence as well as overall crime for nine years, from the point of availability in 1972 to various municipal boundary changes after 1980.

To obtain a variety of control variables, the data are then merged with town-level census data constructed by Statistics Canada. These town-level variables include common correlates of violence, available from the census. Some correlates proxy for socioeconomic status (Johnson and Dawson 2011; Bunge and Levitt 1998), such as the rate of labor-force participation, average family income, and percentage of high-school graduates. Other correlates, such as relationship status (Johnson and Hotton 2003; Jewkes 2002), may predict intimate partner violence for a variety of reasons, including the degree of social isolation of women (Michalski 2004). Here I include variables for the percentage of single-female households and the percentage of females that are divorced. Age is a common correlate of offending and victimization (Johnson 2011), and as such, I include the percentage of the population between 20 and 24. I also include the ratio of female-to-male average incomes as a proxy for the degree of symmetry or asymmetry in the domestic power structure, which is sometimes seen as a risk factor in violent relationships (Heise 1998). These variables use data from three censuses (1971, 1976, 1981), and I linearly interpolate data for intervening years. Otherwise no data is missing. Finally, Mincome, the focal

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<sup>51</sup> These data are police statistics based on a nation-wide system of uniform crime reporting using standard definitions for similar activities.

independent variable is a binary variable set at the town-year level (1=Mincome payments available; 0=No Mincome).

*Mechanism 1: Financial Conflict and Disagreement*

After looking at town-level violence data I move to the micro-level survey data, in order to analyze our array of underlying mechanisms. This survey, held in raw hard-copy form at an Archives Canada facility, inquires into domestic social relations and was separately completed by husbands and wives. I analyze wives' answers only. Survey participants include treatment subjects in Dauphin and control subjects in other Manitoba towns (Total N=~328). The survey was administered twice, once at the baseline and again at the mid-point of the experiment.<sup>52</sup> Only married couples completed the survey. Likewise, the data were archived only when both husband and wife successfully completed the survey. If, say, only the wife completed the survey, it was not archived. Finally, it is important to note that this survey was not mandatory. Completed family surveys account for roughly 77 percent of married program participants at the time of administration.

Questions focus on various subjects, including the domestic division of labor; control over money; power and decision-making in the home; the frequency of (dis)agreement over a variety of financial and non-financial issues, including money, purchases, work, and alcohol-use; the extent to which couples relate to one another in a harmonious, stress-free, and mutually supportive fashion; the extent to which couples are happy with their relationship; and the extent to which couples have talked about separation or divorce.

The survey questions herein often use Likert scale data. For example, participants are asked, *"To what extent have you and your husband disagreed about each of these financial areas*

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<sup>52</sup> Couples with survey data limited to the baseline or study period only are dropped.

*of family life in the last several months ... Whether the money you have is enough to meet your family's needs* (Always agree = 1 ... Always disagree = 5).” Another five similarly phrased questions are asked about other kinds of financial disagreement, including how “*your husband spends money*,” “*how you personally spend money*,” “*your working outside the home*,” and “*whether to save or spend*,” and “*the purchase of alcohol*.” These survey questions bear on the first hypothesis and were combined into an index variable (an average of the six items) for financial stress and disagreement (the higher the index, the more disagreement).<sup>53</sup> Cronbach’s alpha, a coefficient measuring scale reliability, is above 0.8, indicating that the items are sufficiently reliable to use in a single scale. This index provides the main dependent variable for mechanism one, hypothesizing that declining violence is explained by declining financial stress and disagreement in couples.<sup>54</sup>

As I discuss below, the focal independent variable is a “difference-in-difference” dummy variable, generated by multiplying a Mincome status dummy variable (treatment = 1; control = 0) by a study-period dummy variable (study period = 1; baseline = 0). To obtain control variables, the survey data on women’s reports of marital dynamics are merged with already existing pre-study “baseline” data, which contain information for a variety of demographic and socioeconomic characteristics for each household at the start of the experiment, including age, education, parental status, family size, and labor-force participation. These controls are used as

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<sup>53</sup> In addition to this index I also construct an alternative version of the dependent variable, a binary variable where any report of financial disagreement on any of the six items sets the variable to 1, and is 0 if only agreement is reported. Results are reported in Figure 4.

<sup>54</sup> In this section I construct an additional scale asking not about financial disagreement, but disagreement in general. These questions are structured in the same way, but ask about disagreement related to “how much leisure time you spend together,” “the amount of affection you show for each other,” “how you get along with relatives,” “helping with work around the house,” “you or your husband’s personal habits, e.g. dress, cleanliness, drinking, smoking, etc.,” “religious beliefs,” “you or your husband’s choice of friends.” Again, Cronbach’s alpha is above .8. Following the footnote above, I construct an alternative version of this general disagreement scale as well, where any disagreement on any item sets the variable to 1.

independent variables for our first mechanism above, and again for the similar tests described below for mechanisms two and three. Listwise deletion for missing data on the set of independent and dependent variables results in dropping 24-30 percent of observations from the full dataset.<sup>55</sup>

### *Mechanism 2: Divorce-talk and Separation*

As I have argued, the second mechanism, which suggests that Mincome reduced violence by facilitating direct exit from relationships of dependence, is the most complex, and as such will benefit from using various approaches. In examining this mechanism, I use three data sources and methods, and I begin with a new question item from the survey of married women discussed above. The survey does not include information on actual splits, but rather inquires into *discussions* of divorce and separation. The dependent variable is a single survey item asked to married women: “*In the last several months have you and your husband ever talked about separating or getting a divorce* (Yes, often=1; Yes, occasionally=2; Yes, once or twice=3; No, not at all=4).” As above, I have merged this information with (pre-treatment) baseline data to obtain control variables, and use the Mincome dummy and study-period dummy to create the focal independent variable, the difference-in-difference dummy.

Second, I use aggregated town-level census data, constructed from a 33 percent sample database from the long-form census questionnaire, to examine changes in family status. Here, I

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<sup>55</sup> Using a multiple-imputation approach to missing data produces results quite close to those produced by the listwise deletion strategy. However, because virtually all of the missing data occur in the dependent variables, and because the imputation model is identical to the regression model, the multiple-imputation approach does not improve on the listwise approach. Also note that the sample sizes for the regressions below vary because of missing information in the dependent variables. I use the maximum number of observations for each regression rather than restricting the whole analysis to the smaller subset with missing data on any of dependent variables.

look at the portion of single-female and female lone-parent households before, during, and after the Mincome period.

Finally, I use a third data source, Mincome's already-existing panel data, which includes some limited information on families throughout the experiment.<sup>56</sup> I first use this data to present basic descriptive statistics on overall splits in the treatment and control groups, and then present relationship status (i.e., intact couple or split) and attrition starting at every interview wave during the course of the experiment, that is, at the 1974 baseline and every four months between 1975 and 1977. While the survey of married women described above applies only to a consistent (or "balanced") set of in-tact couples completing baseline and study period surveys, this relationship data makes it possible to discern the fate of all couples at each of the nine periodic survey waves of the experiment, whether they remain in the study in-tact, split, or drop out. This larger group includes all couples in the Dauphin treatment group and the Manitoba control group, rather than the smaller sample of intact couples who completed surveys at both baseline and study periods. Moreover, this data determines whether and when splits occur, as well as whether and when censoring occurs. This facilitates the research method, described below, which attempts to capture the time structure of splits in order to discover experimental effects on actual exits from relationships.

### *Mechanism 3: Bargaining Power*

The third and final mechanism hypothesizes that it is not changes in relationship status but rather changes in power among husbands and wives that affect the risk of violence. Here I return to data pulled from our survey of married women discussed above, and focus on a cluster of

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<sup>56</sup> Although original researchers assembled this periodic panel data into a dataset titled "Baseline," it does in fact include information beyond the baseline, namely each family's relationship status, participation, and attrition throughout the experiment.



question items inquiring into decision-making power in the family. The first two questions ask: *“In every family couples have different ways of deciding things ... people have ideas about how decisions should be made and who should make them. How do you feel each of the following decisions should be made by a family ... Who should make the final decision about what job you should take? Who should make the final decision about what job your husband should take?* (Husband should always decide; Husband and wife should decide together; Wife should always decide).”<sup>57</sup> Another question asks: *“When there’s a really important decision on which you and your husband are likely to disagree, who usually wins out? (I usually win; My husband usually wins; Neither of us win, we drop the subject; Sometimes I do, sometimes my husband does; Decisions are mutual).”* The final question asks the following, *“Now think about one thing that causes the most disagreement between you and your husband. How would you say this disagreement usually gets dealt with? (I end up having things my way; My husband ends up having things his way; Neither of us gives in; we just eventually drop the subject; Sometimes she gives in, and sometimes I do; We each give in a little to the other).”* These four question items are treated as separate dependent variables. As with the previously discussed questions from the survey of married women, I use pre-treatment socioeconomic and demographic data for control variables, and again use a “difference-in-difference” dummy as the focal independent variable.

<Table 1 about here>

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<sup>57</sup> I decided that the questions referring to decision-making about jobs to be the most substantively important. However, the survey included questions about decision-making on other issues. As such, rather than analyzing the above questions alone I constructed an alternative measure, a binary dependent variable where any report of the wife stating that the “wife should always decide” on any of the nine items sets the variable to 1, and is 0 if otherwise. The remaining seven question items used to construct the variable are phrased identically to those above, but refer to decisions about car purchases, expensive item purchases, housing decisions, decisions about food purchases, decisions about doctors, decisions about “going out”, and decisions about insurance purchases (I also constructed versions of this measure excluding the gender-typed items, food and “going out”).

## 5. Analytical Strategy

### *Town-level violence outcomes*

In the first results subsection immediately below I begin by considering aggregate crime and aggregate violence data using fixed effects models to control for unobserved heterogeneity across towns and years. Town-level fixed effects account for potential spuriousness emerging from any unique fixed characteristics of any given town (for example, baseline age distributions, baseline poverty levels, stable cultural differences, or idiosyncrasies in local policing). Year-level fixed effects account for sources of spuriousness rooted in any province-wide trends that are shared across all towns (for example, provincial crime trends, economic trends, agricultural price changes, or federal/provincial social policy changes). In the models predicting town-level crime and town-level violence I present results first without controls, then add in census-based controls, and finally include controls as well as town and year fixed effects.

Considering town  $t$  at year  $y$ , the full regression equation is as follows:

$$Y_{ty} = \beta_0 + \beta_1 (Mincome_{ty}) + \beta_2 (Controls_{ty}) + \beta_3 (Town_t) + \beta_4 (Year_y) + \varepsilon_{ty}$$

$Y_{ty}$  refers to one of the two outcome variables, the rate of violent crime or total crime for a given town-year;  $Mincome_{ty}$  is the town-year presence or absence of Mincome;  $Controls_{ty}$  refers to a vector of town-year census control variables;  $Town_t$  specifies the town fixed effects;  $Year_y$  specifies the year fixed effects; and  $\varepsilon_{ty}$  is the idiosyncratic error.

### *Micro-level Married Women's Survey: Three mechanisms*

If the first results subsection—6.1—is a search for outcomes, the second results subsection—6.2—is a search for mediators. I interrogate the proposed three mechanisms and treat these mediators as regression outcomes themselves. The core of the second subsection is an analysis of survey data on married women in a “difference-in-difference” model, a now-standard method of program evaluation (Card and Krueger 1994; Gangl 2010). This model isolates the average effect of the experimental treatment by taking into account any already existing differences between treatment and control groups. The main assumption of this method is that in the absence of the experiment, the baseline to study period *trends* (not the absolute levels) would be identical for any treatment and control group variable. The method allows absolute levels in a variable to differ between groups, so long as we accept the comparatively less heroic assumption that groups would move in parallel absent any deviation induced by the experiment. The comparison is between before-after changes internal to the control and before-after changes internal to the treatment group; the difference between these two internal changes is the difference-in-difference, or the treatment effect. This analysis is used in sections 6.2.1, 6.2.2, and 6.2.3, and depending on the dependent variable in question I use either OLS, logit, or multinomial logit regression techniques. Considering couple  $i$  (1, 2, 3...) at survey time  $t$ , the regression equation is as follows:

$$Y_{it} = \beta_0 + \beta_1 (Treatment_i) + \beta_2 (Study Period_{it}) + \beta_3 (Treatment_i \times Study Period_{it}) + \beta_4 (Controls_{it}) + \varepsilon$$

$Y_{it}$  is one of the three outcome types discussed above (financial disagreement, divorce-talk, or decision-making power) for each married woman;  $Treatment_i$  is a dummy variable identifying

whether a given couple received the Mincome treatment (1) or was in the control group (0);  $Study\ Period_{it}$  is a dummy variable that identifies whether the question was answered during the study period (1) or the baseline (0);  $Controls_{it}$  refers to a vector of baseline control variables for each couple; and  $\varepsilon$  is the idiosyncratic error. The focal variable, the interaction of  $Treatment_i$  and  $Study\ Period_{it}$  is the difference-in-difference variable, which isolates Mincome treatment couples during the study period. Thus,  $\beta_3$ , the difference-in-difference estimator, captures the treatment effect.

#### *Micro-level panel data: Mechanism Two*

Finally, the analysis of the second mechanism includes, in addition to the survey of married women, a different data source and unique methods. It is worth making special note of the thorny methodological problems involved in analysis of marital split frequency with household-level panel data. The examination of treatment and control differences in marital splits are fraught with a few sources of potential bias, which are difficult to fully account for in research. First is the problem of differential attrition. While attrition from panel data is always a concern, worse are circumstances where attrition is distributed unevenly between groups, where treatment and control groups attrite in distinctive patterns. In the case of guaranteed income experiments, the control group, receiving no income boost, has a stronger incentive to drop out of the experiment than does the treatment group. If the control group experiences a greater risk of “right censoring,” the data might exclude some of their unrecorded (i.e., post-drop out) splits, thereby

overstating the treatment effect.<sup>58</sup> Missing data may not be missing at random, and as such they cannot be corrected easily in most statistical procedures (Blossfeld, Golsch, and Rohwer 2007).

Fortunately, in the Dauphin Mincome data *differential* attrition appears to be somewhat minimal. The Dauphin treatment group had 223 couples, of which 69 (27.6 percent) dropped out and 23 split (8.3 percent) at some point during the experiment. In the Manitoba control group there were 253 couples, of which 74 dropped out (26.8 percent) and 27 split (10.8 percent) (See Table 6).

A second set of issues, termed duration biases, may overstate the splits in the treatment group: couples considering a split may realize that this three-year period is their only chance. If so, splits during the three years of the experiment will be greater than the number of splits during three years of a permanent program. There are additional issues relating to variation over time that are more complex still. For example, near the start of the three-year experiment the total value of the windfall is greater than it is in later months. This may make the risk of separation greater in the early months. Moreover, the guaranteed income, as a sudden windfall, may lead “bad” marriages to dissolve early on, an effect that could dissipate over time. Finally, if bad marriages do dissolve early, then the remaining stock of married couples are disproportionately from good marriages and at a lower risk of dissolution. These reasons point to the value of looking at the time structure of separations.

Thus, in understanding marital dissolution, more relevant than the *incidence* of marital splits—a calculation of the number of splits per household in the experiment—is the *rate* of marital splits—a calculation of the number of splits per household-period in the experiment (Cain 1986). The methods discussed below examine splits per household-period while taking

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<sup>58</sup> That is, there is a reasonable risk that instead of reporting a split, controls might simply drop out of the experiment.

stock of differences in the lengths of time each household is actually in the experiment and therefore exposed to the risk of dissolution.<sup>59</sup> Examining the rate of splits at different points in the experiment, and the use of statistical tests that place greater weight on early splits help to account for duration biases.

We can get a clearer picture of the issues, and hopefully a reality check, by using a variety of methodological tools and data sources. Thus, the subsection on the second mechanism—6.2.2—examines: 1) simple descriptive statistics of family status changes in aggregated census data; 2) difference-in-difference regression analyses of survey data on “divorce talk” among intact couples (as discussed above); 3) simple descriptive statistics of actual separation in the Mincome panel data; and finally, 4) non-parametric Kaplan-Meier estimates of separation risk in the Mincome panel data.

The latter, the Kaplan-Meier (or product limit) survival function, estimates the chances of separation in treatment and control groups, and has been used in light of the problems outlined above. This method provides a useful way to account for different durations of “exposure” to the risk to marital dissolution across families, that is, the different amounts of time that families are in the study. The method uses a “risk-set” calculated at every period, which accounts for censored observations (i.e., couples who have attrited in the previous periods). The survivor function provides estimates of the likelihood a couple will split (rather than survive intact) after a given survey, conditional on having not separated up to the prior survey.

Analysis time refers to periodic surveys over the course of the experiment. Unlike the pre-post survey of married women, this panel data includes nine periodic surveys (and one 1974

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<sup>59</sup> Despite their overall sophistication, attrition adjustments in other NIT studies are not overly satisfying. Cain et al. (1990) simply assume that couples in the control that dropped out of the experiment were 25 percent more likely to split than those who remained. They speculate that the bias among treatment couples goes the other way, assigning the treatment group dropouts a split rate that is 50 percent lower than the non-dropout treatment couples (see also Hill 1997).

baseline survey) over the three years, with about four months between each survey.  $S(t_{(j)})$ , is the probability of surviving beyond the periodic survey  $t_{(j)}$ , and is equivalent to 1 minus the probability of “failing” (i.e., splitting) at each time value,  $t_{(j)}$ . If  $T$  is time to failure,  $S(t_{(1)})$  is the probability of surviving past the first value  $t_{(1)}$ , and equals 1 minus the probability of failing at  $t_{(1)}$ , the Kaplan-Meier estimate is defined as follows:

$$S'(t_{(1)}) = 1 - P'(T = t_{(1)}) = 1 - \frac{d_1}{n_1}$$

Here,  $d_1$  is the number of couples who split at  $t_{(1)}$ , and  $n_{(1)}$  is the number of couples at risk just before  $t_{(1)}$ .

## 6. Results

### 6.1 Town-level analysis

Table 2 presents descriptive statistics for the town-level dependent and independent variables in Dauphin and sixteen Manitoba and Saskatchewan towns with populations between 5000 and 50,000. These summary statistics represent an average across the 1970s, rather than a cross-sectional snapshot from any single point in time. Consequently, descriptive statistics for Dauphin represent study period and experimental years together, which may be reflected in the high standard deviations in total crime and violent crime rates. The table also reveals some town-level differences in basic demographic and socioeconomic variables between Dauphin and the aggregation of the non-Dauphin towns. These controls are included in the regression analyses below.

<Table 2 about here>

Table 3 presents the results of the fixed effects models on town-level violent crime and total crime rates. Both models include year and town fixed effects. Models 1 and 4 present regressions results excluding census control variables, while Models 2 and 5 include them. Models 3 and 6 include census controls and fixed effects. All models indicate that Mincome, a variable occurring at specific town-year periods, has a negative effect on the violent-crime and total-crime rate at the town level. Model 3 shows that after controlling for socioeconomic and demographic variables, the relationship between Mincome and violent crime remains statistically significant. While the effect of Mincome on total crime is statistically significant in Model 4, as census controls and fixed effects are added in Model 5 and Model 6, statistical significance begins to fade. This may not be a reason to disregard the crime effects, but it does point to the sensitivity of outcomes to modeling choice. For this reason, and because of the focus of the paper, I turn attention to changes in violent assault.

<Table 3 about here>

The main question then is this: What is the reason for this fall in violence? Our next section attempts to locate the mechanisms behind the negative relationship between Mincome and violence.

## 6.2. *Mechanisms*

### 6.2.1 *Financial stress and disagreement*



**Mechanism 1:** Risk of a violent incident is heightened by financial stress in the family; insofar as Mincome reduced financial stress, various kinds of disagreement and domestic assault fell

Table 4 shows that according to married women, Mincome did reduce financial disagreement. The “treatment effect,” shown by the difference-in-difference coefficient—which, again, compares before-after changes in control individuals to before-after changes in treatment individuals—in Model 1 is a statistically significant change of -0.24 Likert points. On a 5-point Likert scale, with a maximum movement of 4 possible points, this fall in disagreement translates to 6.1 percent of all possible change. It is worth noting here that the constant, at 2.3, implies that there is already a good amount of agreement (1 = Always Agree), and therefore there is not much room for improvement in agreement.<sup>60</sup> On a scale of non-financial disagreement (Model 2), however, while the coefficient is negative, it is not statistically significant.

<Table 4 about here>

There is more detail when we pull our scales apart in Models 3 and 4. While the non-financial items are non-significant overall, one item in the index stands out. As shown in Model 3, wives were asked about disagreement related to their husband’s habits, including drinking, and on that issue there was a treatment effect of -0.39 points—9.9 percent of all possible change. That is, Dauphin wives, compared with control subjects, saw a large and statistically significant fall in alcohol-related disagreement. We can also pull apart our findings on financial disagreement from Model 1. While individual items on this scale are all quite similar, the strongest negative effect comes from the question asking about disagreement on whether to

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<sup>60</sup> For example, a seven-point Likert scale might have left more room for increased agreement.

spend or save money (Model 4). Here, I find a significant change of -0.28 points, or a fall of about 7 percent of possible change.

Finally, as noted in a footnote above, I created alternative versions of the financial and non-financial disagreement scales. To construct these two alternative measures any financial or non-financial disagreement item, respectively, sets the variables to 1, and is 0 otherwise. Logit models 5 and 6 both show odds ratios indicating significant reductions in financial and non-financial disagreement.

In sum, these findings lend support to the mechanism suggesting that violence falls through a reduction in financial stress and financial disagreement. In order to get a visual sense of the main results above, Figure 3 uses regression estimates from Model 1 to graph before-after trends in the financial disagreement index for the Dauphin treatment and control groups.<sup>61</sup>

<Figure 3 about here>

### 6.2.2 *Divorce-talk and separation*

**Mechanism 2:** Since women were able to exit marriages of economic convenience by collecting payments as single-person households, Mincome reduced violence by facilitating direct exit from relationships of dependence.

Because the history behind this mechanism is so contested (Tuma and Hannan 1990; Cain and Wissoker 1990; Cain 1986), I examine this issue using a diverse mix of data sources and methods. I begin with descriptive trends on single-female and female lone-parent households using census data. The American GAI experiments assigned treatments to families dispersed

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<sup>61</sup> To understand this graph it is worth clarifying the interpretation of each coefficient in the difference-in-difference model. The baseline control level is captured by  $\beta_0$ ; the study-period control level is captured by  $\beta_0$  plus  $\beta_2$ , the coefficient on the “study period” variable; the baseline treatment level is captured by  $\beta_0$  plus  $\beta_1$ , the coefficient on the “treatment” variable; and the study-period treatment level is captured by the sum of  $\beta_0$ ,  $\beta_1$ ,  $\beta_2$ , and  $\beta_{12}$ , the coefficient on the product of the “treatment” and “study period” variables.

across towns and counties, making it impossible to use census-based reality checks of this kind. As a snapshot once every five years, census data has its limits; but it can provide a sense of the larger picture before asking whether the micro-level data conforms to it. At minimum it will pick up dramatic changes in familial organization—on the order of the very large increases in separation, as estimated by Tuma and Hannan (1990)—if they indeed occurred. Next, this section returns to the survey discussed above to examine *discussion* of separation among intact couples. Of those couples completing the survey of social relations in the family before and during the experiment, what is the treatment effect on discussion of divorce or separation? Was there an experimental effect on “divorce talk”? An ancillary question looks for variation within this group: Did Mincome affect “high-conflict” and “low-conflict” marriages differently? Finally, I turn to micro-data on married couples to examine the record of actual splits in treatment and control couples. Here, I begin with the stock of all married couples and look at “survival rates” in treatment and control couples across the duration of the experiment; I use Kaplan-Meier estimates to examine the chances of separation in both treatment and control groups over time.

Figure 4 shows census data on family status changes among females aged 20 to 54. The left pane shows single females (i.e., female one-person households) as a portion of total household persons in census snapshots before, during, and after the experiment. Dauphin shows a slightly faster increase in the portion of females that are single relative to the other geographies between 1971 and 1976. The right pane shows female lone parents as a percentage of census family persons, and here there is no clear effect. The story from these aggregate data is a non-story: while there may be some growth in the portion of females who are unattached during the

Mincome period, overall, from these snapshots, one cannot discern any noteworthy changes in family status.

<Figure 4 about here>

Next, I return to the survey of married women to examine changes in “talk” of separation and divorce. Women in the treatment and control groups were asked whether they had talked with their husbands about divorce or separation in the past several months. Model 1 of Table 5 shows a very small, and statistically insignificant, increase in talk of divorce across the entire sample. There appears to be no general effect of Mincome on couples’ propensity to discuss separation, according to wives.

However, as suggested by our ancillary hypothesis (2a), there may be additional variation within the Mincome group. Is it the case that Mincome couples in high-conflict marriages at the baseline were destabilized, but Mincomers entering the experiment in low-conflict marriages were stabilized? Models 2 to 4 in Table 5 examine whether divorce-talk in Dauphin interacted with women’s pre-treatment assessment of marital disagreement. Disagreement is defined here as an index of financial disagreement, as above. I use cut-off points from the index to restrict the sample to couples entering the experiment with “very high-disagreement,” “high-disagreement,” and “low-disagreement” relationships.<sup>62</sup>

While there is no effect in the full sample, Model 2 shows that “very high-disagreement” couples see a large and significant increase in divorce talk. They move 0.71 of a Likert point,

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<sup>62</sup> The index (1=Always disagree; 2=Almost always disagree; 3=Occasionally disagree; 4=Almost always agree; 5=Always agree), defines “very high disagreement” couples as those with an average score below 3, “high disagreement” couples as those with score below 4, and “low disagreement” couples as those equal to or above 4.

about 24 percent of all possible change of three points on a four-point scale. Consistent with this, Model 3 finds that “high-disagreement” couples, a much larger group, also see significant increases in talk of divorce. This group experienced an increase of 0.24 of a Likert point, a change of about 8 percent of all possible change. Finally, Model 4 shows that “low-disagreement” couples see *declines* in divorce talk. Interestingly, the sign actually moves from positive to negative. The effect size, at -0.12 of a Likert point or about 4 percent of all possible change, is small but statistically significant. This outcome is not trivial, particularly in light of “ceiling effects”: with a constant close to 1, there is little room for improvement. That is, many women at the baseline enter the experiment *already* reporting, “No, not at all”; they never talk about separation.

There is some support for a “threshold” effect: Mincome destabilizes “bad” marriages, but when the program interacts with “good” marriages, it provides stability. In order to see these effects more clearly, Figure 5 graphs the changes in discussion of divorce for treatment and control groups using regression estimates from Table 5. While there is little effect in the full sample from Model 1, “low-disagreement” couples in Dauphin’s Mincome group see falls in divorce talk, “high-disagreement” couples see increases in divorce talk, and “very high-disagreement” couples see even faster increases in divorce talk.

<Table 5 about here>

<Figure 5 about here>

I now turn from talk of separation to actual separation. Before examining Kaplan-Meier survival estimates, Table 6 shows basic descriptive statistics in the Dauphin treatment and Manitoba control groups. As the descriptive table suggests, incidence of splits in the Dauphin treatment group were slightly greater than in the control group. However, using the total stock of participating couples in the denominator is not the best way to understand the changing risk of a separation. Instead, the product-limit method calculates a risk set for every time period in the experiment, plotting the risk of a split while taking into account the censoring and splits that have already occurred.

<Table 6 about here>

Using the panel data summarized in Table 6, Figure 6 presents product-limit survival estimates for experimental and control couples. “Survival” simply refers to couples that had not separated by the end of a given survey period. At the start of the experiment, by definition, survival is equal to one for treatment and control groups. As the experiment moves through its three years of periodic surveys, the chance of split naturally increases (or, the survival function falls). Survival rates for treatment and Mincome groups are very close at the start of the experiment, but once the program becomes more familiar by the third and fourth survey wave, a divergence emerges. The Mincome group has a higher chance of splitting early on in the experiment, when the sum total of the Mincome windfall is highest. In other words, one can discern an “independence” effect for women in the treatment group. This effect then weakens in second half of the experiment as the end date comes into view.

While these effects are discernable from survival estimates, a barrage of tests (Log-rank, Wilcoxon, Peto-Peto, and Tarone-Ware) of the null hypothesis shows no significant difference between groups.<sup>63</sup> Because the Wilcoxon test gives most weight to variation early on it is natural that its p-value is lowest, though even this result is far from significant. There may be a small “independence” effect induced by Mincome; however, one cannot exclude the possibility that this is chance variation, and thus we gain no confidence in the plausibility of the second mechanism.

<Figure 6 about here>

How to synthesize findings from the diverse data sources in this section? Overall there is little solid indication that Mincome led to actual splits, and this must, in sum, rule out mechanism two. The absolute number of splits, and the relative percentage differences in splits between treatment and control groups are both quite small, suggesting that this is an unlikely channel to explain changes in domestic violence.

How then to interpret the rise in divorce talk among some couples? While these changes occur only in a portion of the full sample, the experience of increasing divorce talk is much more widespread than actual splits. Rather than interpreting this as evidence bearing on hypothesis two, it may be most relevant to hypothesis three. While it may be rare for married women to use Mincome payments to facilitate actual separation, it may affect power dynamics in relationships

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<sup>63</sup> The log-rank test weights each time period equally and is best when the hazard functions are proportional between groups; the Wilcoxon test is more sensitive to variation early on; the Tarone-Ware, similar to the Wilcoxon test, also gives more weight to early failure times, though not as much as the Wilcoxon test; finally, the Peto-Peto test uses a weight function similar to the Kaplan-Meier estimator, and is not susceptible to differences in censoring patterns between groups.

more broadly. Increasing talk of separation may be an indirect indicator of shifting power relations. Mincome may lead to shifts in power that show up as an increasingly available *threat* of exit. The next section takes this interpretation, and tests it with more direct measures of changing domestic power relations.

### 6.2.3 Decision-making Power

**Mechanism 3:** Since women were able to exit marriages of economic convenience by collecting payments as single-person households, Mincome reduced violence by making credible the *threat* of exit through an increase in the bargaining power of wives vis-à-vis husbands.

This section asks about broad changes in power relations between men and women, a potential candidate to explain changes in intimate partner violence. Even if the exit option Mincome afforded to women did not directly lead to splits, perhaps it changed the bargaining power in the family, which itself led to a decline in violence. As noted above, in the absence of actual separation, it is reasonable to interpret changes in “discussion” of separation and divorce as an increasing viable threat of exit. To support this interpretation, I look at more direct measures of domestic power. Here, I examine potential changes in decision-making power over important aspects of domestic life, asking who tends to make key decisions, who wins out, and who gives in when there is disagreement. To what extent do we find direct evidence for changing power relations?

Table 7 displays the experimental effects of three survey questions on power and decision-making. The first and second questions ask about who should make final decisions about the wife and the husband’s jobs.<sup>64</sup> The third question asks about who wins when there is a

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<sup>64</sup> In a footnote above I noted that I also created an alternative decision-making measure combining all items related to decision-making in order to create a binary variable indicating any report of women



“really important decision” on which partners disagree. The table shows multinomial logit models for each question, presenting coefficients as odds ratios. Models control for the same independent variables as regressions above (to preserve space they are not shown). The first two models estimate the odds that women report an increased likelihood that they should make the final decision (columns 2 and 4) or that the decision should be mutually decided upon (columns 1 and 3) relative to the base outcome that the husband should make the final decision. The third model, in column 5, estimates the odds that women report an increase in the likelihood that they usually win out in decisions where there is disagreement, relative to the base outcome that the husband usually wins out. Column 6 makes the comparison between mutually decided upon outcomes and the husband usually winning, while column 7 compares a growth in all other reported decision-making possibilities relative to the husband winning out.

<Table 7 about here>

In columns 2 and 5 the odds ratio is above 1, indicating that there is improvement in women’s decision-making power (relative to the husband) over her job and over important decisions by a factor of 1.38 and 1.23, respectively. Likewise, column 6 shows that relative to a baseline outcome of the husband winning, there is an increase in “mutual” decision making over important decisions by a factor of 1.87. However, column 4 shows *declines* in her decision-making power over the husband’s job, by a factor of 0.6. None of these outcomes, however, are

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making final decisions. Using a logit model, I find a very small *reduction* in answering, “the wife should always decide”, however the effect was non-significant. This outcome was the same when the female gender-typed items were excluded from the variable. Results available upon request.

statistically significant. Moreover, when we look at smaller samples (not shown), such as the high disagreement group, these coefficients change little.

A fourth question asks about who tends to give in and who gets their way on an issue that causes the most disagreement between partners. Table 8 shows multinomial logit results, where each column shows odds ratios relative to the baseline category, “my husband ends up getting his way.” The first and fourth columns may be the most important indicators of women’s power improving. While the coefficient in the first column is significant, the effect size, in scientific notation, is close to zero. In this case the absolute numbers tell the real story: it was extremely rare for women either in the treatment or control, before or during the study, to select this category (from baseline to study period, in the treatment group, three and then zero women selected this answer; in the control, six and then six women did so). The fourth column, an indicator of changes in mutual decision-making, increases by a factor of 1.85. To some extent, the other columns may be understood as an improved gender balance of power, relative to the husband “having things his way” category. While there is growth in these answer categories, none of these changes are statistically significant.

<Table 8 about here>

To clarify the findings in Tables 7 and 8, Figure 7 graphs selected results in a forest plot. In most cases, odds ratios show that Mincome effects generated little change relative to the baseline answer of husband dominance. Odds ratios tend to be close to a factor of 1—with confidence intervals always overlapping 1—indicating no change.

<Figure 7 about here>

Overall, there is no clear message that women's bargaining power and decision-making improved; as such, it is an unlikely candidate as an explanation for changes in domestic assault. While discussion of separation does increase for some groups, there is little evidence of other, more direct indicators of changing power relations. While Mincome destabilizes "bad" marriages, this may not play a role in the narrative proposing that changes in power dynamics in the home reduced domestic violence. With ambiguous evidence for changing domestic power relations, there is little to suggest that mechanism three was in operation.

## 7. Discussion

This paper returns to the Mincome experiment and finds a negative relationship between an exogenous income boost in economic resources and family violence. To gain some purchase on the social dynamics in the family underlying this relationship I examine three potential mediators: financial stress and disagreement, women's exits from relationships, and the changing balance of power within relationships. I find support for the reduction of financial stress mechanism, but find either ambiguous or little support for the latter two mechanisms.

Additionally, it appears that Mincome may have impacted different families differently, strengthening familial bonds in some, but undermining other, already unhappy relationships.<sup>65</sup> This ancillary finding is a kind of threshold effect: Mincome destabilizes marriages until it stabilizes them. The idea that Mincome destabilized "bad" relationships and stabilized "good" ones is highly suggestive, even if it does not tell an entirely consistent story. While the second half of the finding—stabilizing good marriages—dovetails with reductions in financial stress and

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<sup>65</sup> The latter effect can be seen in a positive light; in Marxist jargon it would be deemed an acceleration of familial contradictions.

conflict, the first half—destabilizing bad marriages—is not reflected in our deeper examination of women’s exits from marriages or changing power relations in the family.

While I do not find changes in power relations between husbands and wives, it is possible that this non-finding comes down to a basic design feature, one that distinguishes a Mincome-style guaranteed annual income from the universal basic income. Where the latter is distributed to all individuals, the former is allocated at the level of the household. It is certainly conceivable for the guaranteed annual income to facilitate someone’s exit from a household: forming a single-person household and collecting payments individually was an available option for women under Mincome. Some took that option, and others surely considered it. However, the universal basic income is automatically directed to *individuals*, not households. This means that it not only provides an exit option, it also provides resources—and therefore power—immediately to people, typically women, who are in the midst of relationships characterized by unequal power dynamics. Allocation to individuals rather than families diminishes the risk that the more powerful party takes control of payments. It stands to reason that relative to the guaranteed income, a universal basic income may be even more likely to generate reductions in domestic violence, as it is better positioned to tap into both the third and the first mechanisms above. It may reduce financial conflict *and* offset familial power inequalities.

Can findings linked to domestic assault and relationship dynamics from the 1970s have any bearing on the contemporary world? Is it not the case that transformations in the structure of women’s work opportunities have already changed the shape of domination in families? Has the historic decline in violent assault (Bunge 2002; Sinha 2013) rendered these effects less meaningful?

These questions cannot be dismissed, but they overstate the case. It is true that relationships have changed over time. For example, while husbands continue to be the modal perpetrators of domestic assault, their assault numbers have fallen since the 1990s. However, over the same period the number of boyfriends reported to police for intimate-partner violence has risen (H. Johnson 2006). Indeed, terms such as “intimate-partner violence,” “common-couple violence” and a variety of other classifications (M. Johnson 1995; M. Johnson 2006) have been introduced to the literature precisely because patterns of violence persist in the contemporary ecology of relationship types.

Moreover, while gender-based violence has fallen since the 1970s, it is far from eliminated. According to Canada’s General Social Survey (GSS) in 2009 the rate of self-reported violent incidents committed against women nation-wide in the previous 12 months was 11.2 percent (Sinha 2013). However, questions about *lifetime* prevalence of gender-based violence are not included in the GSS, and naturally these numbers are much higher. Lifetime prevalence questions were included in Statistics Canada’s one-off Violence Against Women Survey (1993), the first and only large-scale national survey designed to respond to a number of inadequacies in prior surveys. In this survey, about half of all women—51 percent—reported at least one episode of physical or sexual violence since age 16 (H. Johnson 1996; H. Johnson 2011). Even if these numbers have fallen since the 1990s, the Violence Against Women Survey demonstrated that male violence against women was a common experience, and there is little doubt that lifetime prevalence of gender-based violence remains widespread.

Next, it is worth taking stock of the historic decline in rates of domestic assault. Why did these changes occur in the first place? One of the key factors has been the expansion of viable alternatives to domesticity (Kalmuss and Straus 1985; Pollak 2005). The expansion of labor

market opportunities for women had emancipatory effects precisely because it provided an exit option from traditional patterns of economic dependence on male breadwinners. Basic income does just this, but arguably in a far more direct manner. It is more effective than job growth because it is not contingent on the vicissitudes of the market. And in light of the uncertainties with respect to future job growth, the old strategy of substituting economic dependence on husbands with economic dependence on bosses may be increasingly ineffective. Finally, instead of providing only partial exit through poorly remunerated or part-time jobs, basic income has the potential to provide a direct alternative to domination in family life. Although this mechanism does not appear unambiguously in this study, I do find that some unhappy marriages were destabilized, and believe that the mechanism deserves further and broader exploration.<sup>66</sup>

Finally, in a period marked by new sources of economic insecurity (Kalleberg 2009; Standing 2011), there is good reason to think that financial stress and conflict will continue to be a pervasive social reality. Reducing the sense of insecurity over finances through an automatic and regular stream of cash income will very likely serve to stabilize people's everyday lives and temper a key site of conflict in relationships. In this way, a meaningful source of financial stability in a period marked by real uncertainties may prove to be more relevant than ever.

The literature around income maintenance programs tends to focus narrowly on the extent to which income guarantees reduce people's participation in the formal labor market (see Levine et al. 2005; and Widerquist 2005). The virtue of looking at the guaranteed income through a sociological lens may be the inclination to forefront an array of *social* consequences generated by programs designed to eliminate poverty. While this kind of analysis should be of interest to those preoccupied with understanding the diverse effects of income maintenance

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<sup>66</sup> For example, another avenue of related research—which I could not study with this data—might explore how over a long period, basic income could facilitate marriage delay rather than dissolution. There is evidence linking marriage delays and reduced violence in relationships (i.e. Dugan et al. 1999).

policies, it should be of equal interest to feminist scholars concerned with understanding domination in the family. If sociology can contribute to transformative social policy by facilitating fuller, more socially tuned analyses of costs and benefits, experiments such as Mincome may in turn benefit sociology by clarifying the mechanisms through which socioeconomic variables affect gender dynamics and domestic violence.

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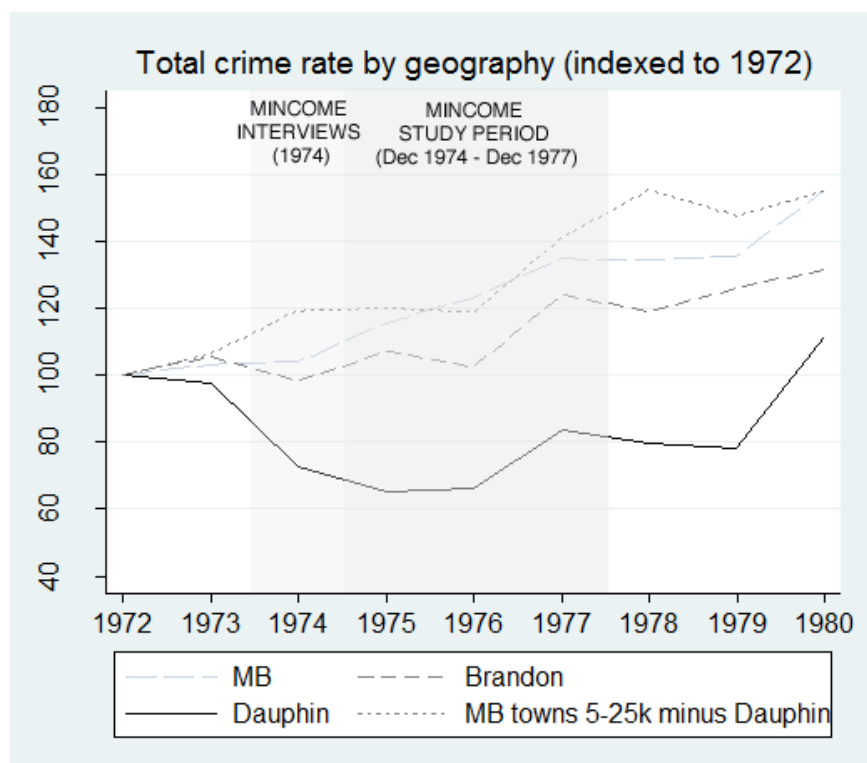
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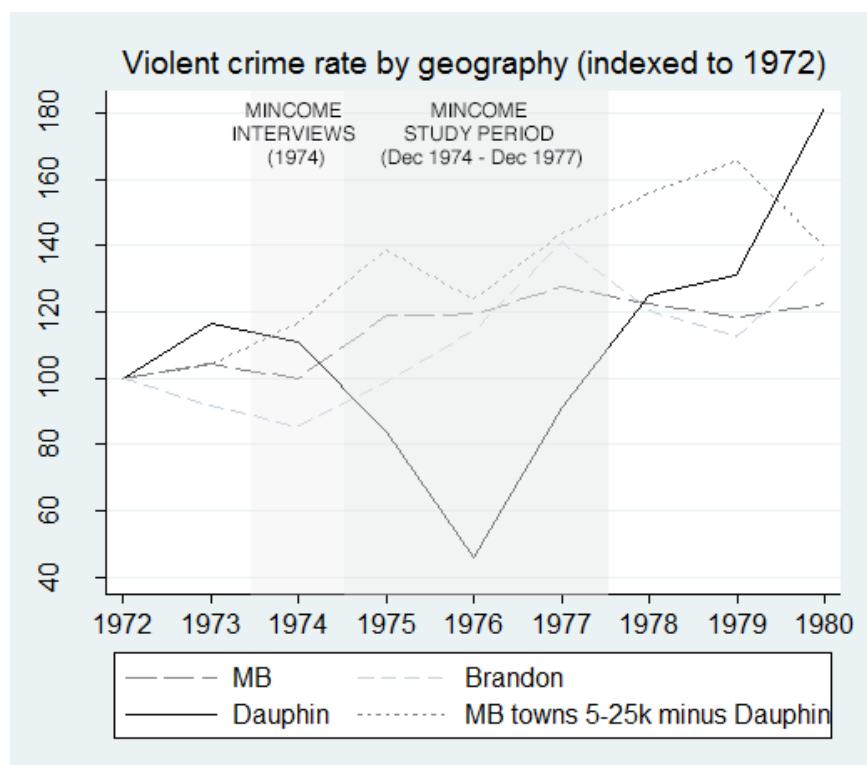
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## Figures and Tables



**Fig. 1.** Total crime in Dauphin, Manitoba, and similarly sized Manitoba towns



**Fig. 2.** Violent crime in Dauphin, Manitoba, and similarly sized Manitoba towns

Focal data sources (occurrence)...	Merged with data from...	Used in...	Analytical strategy...
Town-level crime and violence statistics (annual)	Town-level census data from 1971, 1976 & 1981 (to obtain control variables)	Section 6.1 (Focal outcome: Violent assault)	Fixed-effects OLS regression
Micro-level survey of married women in Dauphin and Manitoba non-recipient groups (1974 baseline and 1976 study period surveys)	Micro-level pre-study survey (to obtain individual/household demographic and socioeconomic control variables at 1974 baseline)	Sections 6.2.1 (Mechanism 1: Financial stress); Section 6.2.2 (Mechanism 2: Divorce-talk & separation); and Section 6.2.3 (Mechanism 3: Bargaining power)	Difference-in-difference OLS and multinomial logit regression
Micro-level panel survey of attrition/split status in all participating households in Dauphin and Manitoba non-recipient groups (1974 baseline and nine study-period surveys—every four months between 1975 and 1977)	N/A	Sections 6.2.2 (Mechanisms 2: Divorce-talk & separation)	Descriptive statistics and Kaplan-Meier survival estimates
Town-level census data on family status (1971; 1976; 1981)	N/A	Sections 6.2.2 (Mechanisms 2: Divorce-talk & separation)	Descriptive statistics

**Table 1.** Summary of data and methods

	Dauphin		Non-Dauphin towns	
	Mean	SD	Mean	SD
<i>Dependent Variables</i>				
Violent crime rate (per 100000 pop)	744.5	440.5	559.8	190.4
Total crime rate (per 100000 pop)	9408.0	3385.6	7668.7	1468.2
<i>Independent Variables</i>				
Family avg. income	23563.7	2437.3	20954.7	1132.8
Female employment rate	43.47	3.71	38.82	2.78
Female/male avg. income ratio	44.01	4.36	46.26	3.06
Percent single female	4.80	1.54	4.93	1.71
Percent divorced	2.56	0.95	1.73	0.30
Percent 20 to 24	9.66	1.37	6.86	0.34
Percent with any post-secondary education	30.45	4.24	24.31	2.93

**Table 2.** Descriptive Statistics, averaged across study period and experimental years



	Predicting Violent Crime			Predicting Total Crime		
	Model 1 (no FE, no controls)	Model 2 (FE, no controls)	Model 3 (controls & FE)	Model 4 (no FE, no controls)	Model 5 (FE, no controls)	Model 6 (controls & FE)
<i>Focal variable</i>						
Mincome	-364.1*** (70.42)	-293.3*** (69.75)	-326.6*** (83.55)	-2,799*** (538.0)	-1,323* (792.3)	-863.9 (803.1)
<i>Independent variables</i>						
Family avg. income			-0.105** (0.0438)			-0.901** (0.370)
Fem employment rate			-25.06 (15.94)			154.6 (123.6)
Female/male avg. income ratio			32.46 (35.13)			981.0*** (259.5)
Percent single female			4.480 (58.39)			-1,320** (524.2)
Percent divorced			-4.649 (108.4)			1,748* (930.9)
Percent 20 to 24			-78.23*** (27.03)			352.1 (235.9)
Percent with any post-sec ed.			-43.77* (25.07)			-744.8*** (183.3)
Constant	740.3*** (37.71)	439.7*** (62.67)	4,454** (2,142)	9,354*** (290.7)	10,029*** (559.8)	529.7 (18,723)
Observations	135	135	135	135	135	135
R-squared	0.016	0.819	0.869	0.016	0.822	0.872
Town FE	NO	YES	YES	NO	YES	YES
Year FE	NO	YES	YES	NO	YES	YES

Robust standard errors in parentheses

\*\*\* p&lt;0.01, \*\* p&lt;0.05, \* p&lt;0.1

**Table 3:** Fixed-effects models predicting town-level violent crime and total crime

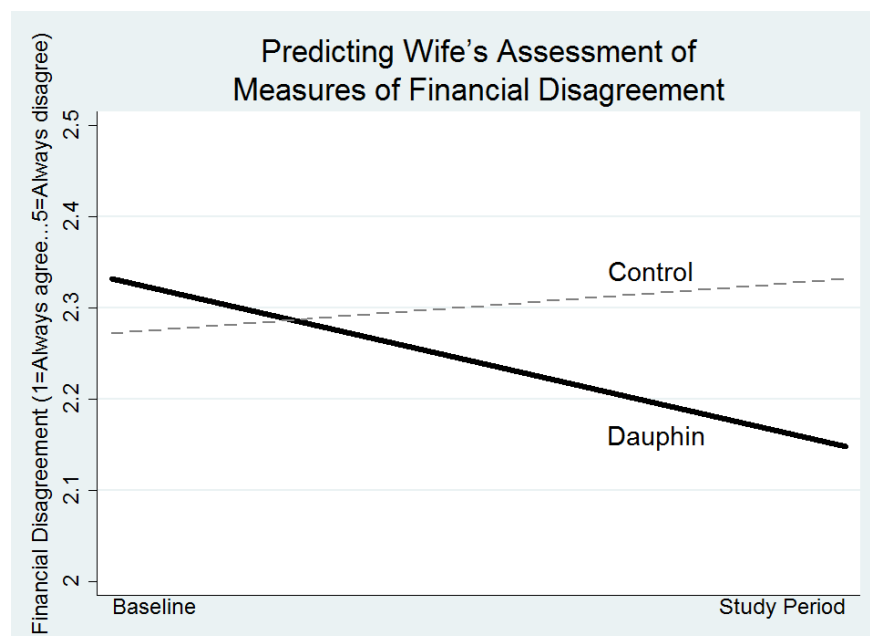
	(1) OLS: Financial disagreement scale	(2) OLS: Non-Financial disagreement scale	(3) OLS: Non-financial disagreement item: Alcohol-related Disagreement	(4) OLS: Financial disagreement item: Whether to save or spend	(5) Logit 1: Any Financial Disagreement dummy (Odds ratios)	(6) Logit 2: Any Non- Financial Disagreement dummy (Odds ratios)
<b>Difference-in-difference</b>	<b>-0.243**</b> (0.118)	<b>-0.142</b> (0.101)	<b>-0.391***</b> (0.141)	<b>-0.281*</b> (0.147)	<b>0.522*</b> (0.192)	<b>0.499*</b> (0.195)
<i>Controls</i>						
Female - LFP	-0.132 (0.0949)	0.0537 (0.0898)	0.0218 (0.128)	-0.0472 (0.124)	1.089 (0.424)	1.819 (0.696)
Male - LFP	0.0200 (0.121)	-0.132 (0.0942)	-0.0927 (0.154)	-0.216 (0.152)	1.555 (0.593)	0.650 (0.245)
Male - weeks employed	-0.00216 (0.00188)	0.00124 (0.00149)	-0.00215 (0.00239)	0.000587 (0.00230)	0.991 (0.00631)	1.006 (0.00604)
Female - weeks employed	0.00177 (0.00203)	-0.000988 (0.00200)	0.000760 (0.00269)	-0.000636 (0.00256)	1.003 (0.00799)	0.986* (0.00779)
Home ownership dummy	-0.154** (0.0639)	-0.144** (0.0561)	0.0240 (0.0761)	-0.0728 (0.0778)	0.525*** (0.105)	0.621** (0.131)
Number of kids under 6	0.00237 (0.0417)	0.00189 (0.0362)	-0.0487 (0.0467)	-0.119** (0.0495)	0.805* (0.101)	0.848 (0.124)
Family size	0.0256 (0.0197)	-0.00120 (0.0168)	0.00170 (0.0258)	0.0508** (0.0249)	1.090 (0.0663)	1.047 (0.0631)
Female - Age	-0.00494 (0.00329)	-0.00560* (0.00291)	-0.00756* (0.00407)	-0.00776* (0.00404)	0.970*** (0.0101)	0.960*** (0.0104)
Female - high school grad	0.0571 (0.0559)	0.0451 (0.0472)	0.00486 (0.0677)	-0.0684 (0.0667)	0.426*** (0.0844)	0.886 (0.189)
Constant	2.272*** (0.187)	2.492*** (0.161)	2.791*** (0.228)	2.485*** (0.214)	10.35*** (6.132)	19.08*** (12.18)
Observations	640	656	642	630	640	656

Note: Models control for treatment dummy and study period dummy, not shown.

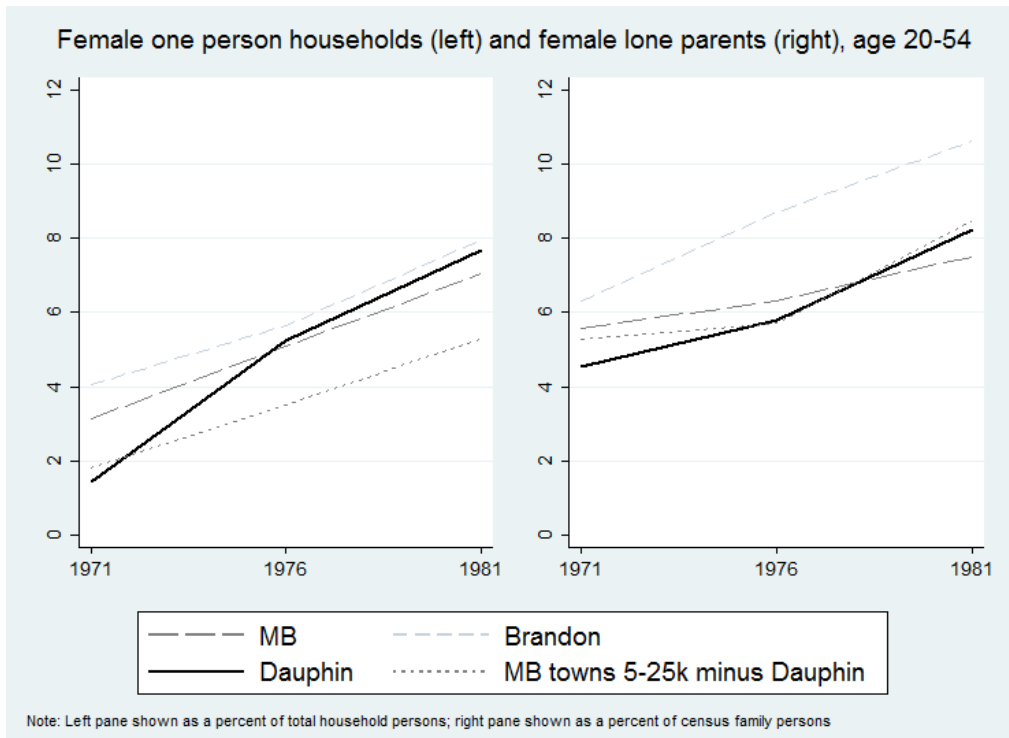
Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 4:** Predicting wives' assessment of measures of disagreement



**Fig. 3:** Wives' assessment of financial disagreement before and during Mincome



**Fig. 4:** Census snapshots of family status changes

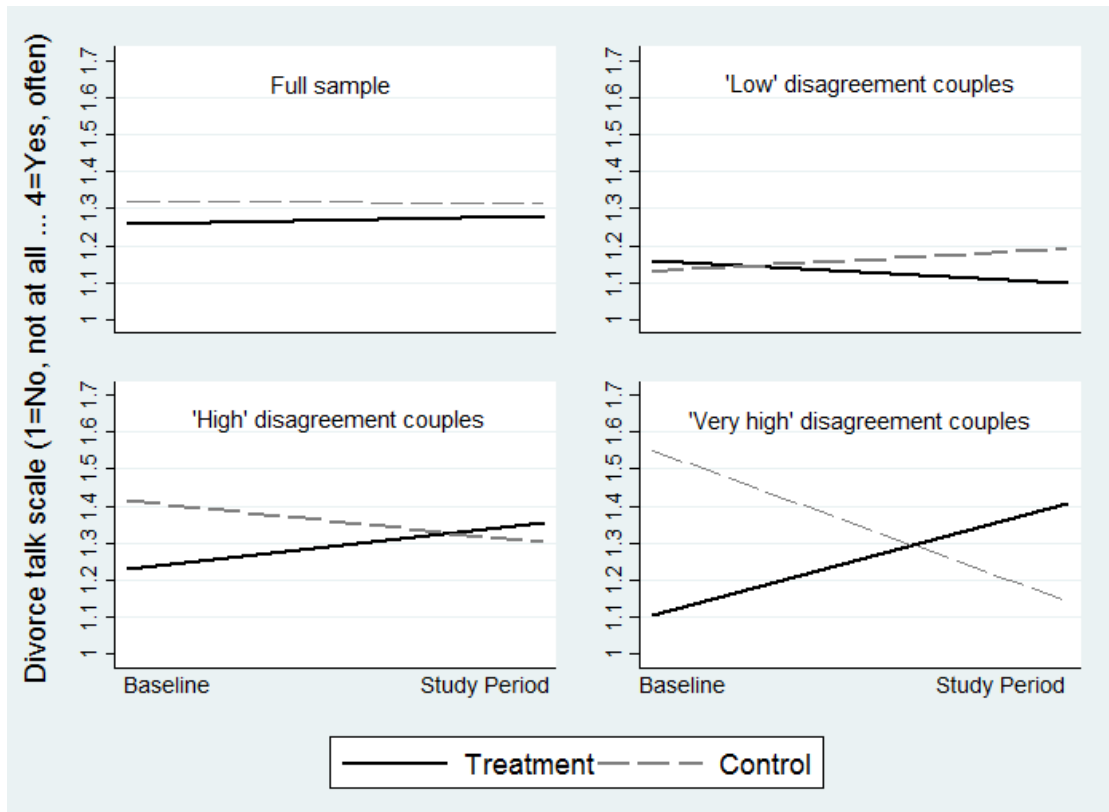
	OLS Model 1: Divorce talk	OLS Model 2: Divorce talk - Very high disagreement marriages	OLS Model 3: Divorce talk - High disagreement marriages	OLS Model 4: Divorce talk - Low disagreement marriages
<b>Difference-in-difference</b>	<b>0.0317</b> (0.0647)	<b>0.712**</b> (0.277)	<b>0.237**</b> (0.110)	<b>-0.122**</b> (0.0589)
<i>Controls</i>				
Female - LFP	-0.0457 (0.0665)	-0.179 (0.266)	-0.112 (0.0792)	0.00838 (0.0365)
Male - LFP	-0.00661 (0.0788)	0.0676 (0.243)	-0.129 (0.119)	0.166 (0.119)
Male - weeks employed	-0.000544 (0.00126)	0.00247 (0.00466)	0.00125 (0.00178)	-0.00320 (0.00196)
Female - weeks employed	0.00141 (0.00142)	0.00227 (0.00564)	0.00303 (0.00184)	0.000323 (0.000947)
Home ownership dummy	-0.0759** (0.0375)	-0.0179 (0.147)	-0.0756 (0.0554)	-0.0633 (0.0407)
Number of kids under 6	-0.0319 (0.0198)	-0.147 (0.100)	-0.0369 (0.0366)	-0.0266* (0.0158)
Family size	0.00243 (0.0110)	-0.00568 (0.0547)	0.00361 (0.0165)	0.00529 (0.0128)
Female - Age	-0.00391*** (0.00147)	-0.00718 (0.00740)	-0.00396* (0.00208)	-0.00191 (0.00194)
Female - high school grad	0.0320 (0.0278)	0.238* (0.129)	0.0700* (0.0413)	-0.00947 (0.0365)
Constant	1.321*** (0.103)	1.551*** (0.442)	1.414*** (0.172)	1.131*** (0.0998)
Observations	644	76	322	294

Note: Models control for treatment dummy and study period dummy, not shown.

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

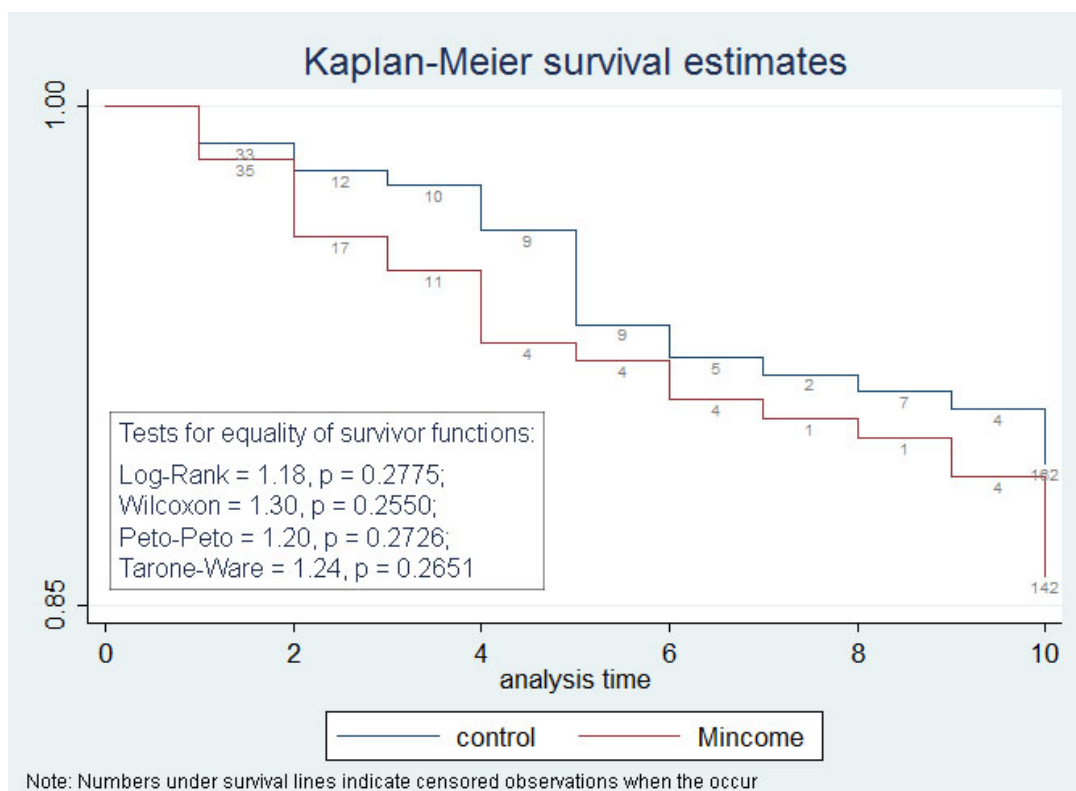
**Table 5:** Predicting wives' assessments of recent talk about divorce or separation



**Fig. 5:** “Divorce talk” estimates from difference-in-difference regressions for Mincome treatment and control groups, by level of baseline disagreement

	Family Split Status		
	Intact	Split	Total
Control	253 91.67%	23 8.33%	276 100%
Treatment	223 89.20%	27 10.80%	250 100%
Total	476 90.49%	50 9.51%	526 100%

**Table 6.** Descriptive statistics on incidence of couple splits during the experiment



**Fig. 6:** Mincome and control survival estimates

	Base category = husband decides				Base category = husband usually wins		
	Mlogit 1: "Who should make the final decision about what job you take?" (i.e., wife)		Mlogit 2: "Who should make the final decision about what job your husband takes?"		Mlogit 3: "When there's a really important decision on which you and your husband are likely to disagree, who usually wins out?"		
	(1) Husband & Wife should decide together	(2) Wife should decide	(3) Husband & Wife should decide together	(4) Wife should decide	(5) Wife usually wins	(6) Decisions are mutual	(7) Other outcomes
Difference-in-difference	1.385 (0.998)	1.381 (0.997)	1.090 (0.450)	0.596 (0.687)	1.123 (1.101)	1.870 (1.178)	1.205 (0.649)
Constant	66.30*** (94.33)	117.2*** (167.8)	0.184** (0.123)	0.000183*** (0.000525)	0.0751* (0.105)	0.266 (0.272)	0.930 (0.714)
Observations	640	640	650	650	646	646	646

Note 1: All models control for treatment dummy and study period dummy, and the following variables: Female LFP, male LFP; Weeks employed, male; Weeks employed, female; Home ownership dummy; Number of kids under 6; Family size; Age, female; High school grad, female (not shown).

Note 2: Mlogit 3 combines the following categories into "Other outcomes": "Neither of us wins; we drop the subject" and "Sometimes I do, sometimes my husband does".

Robust se in parentheses

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

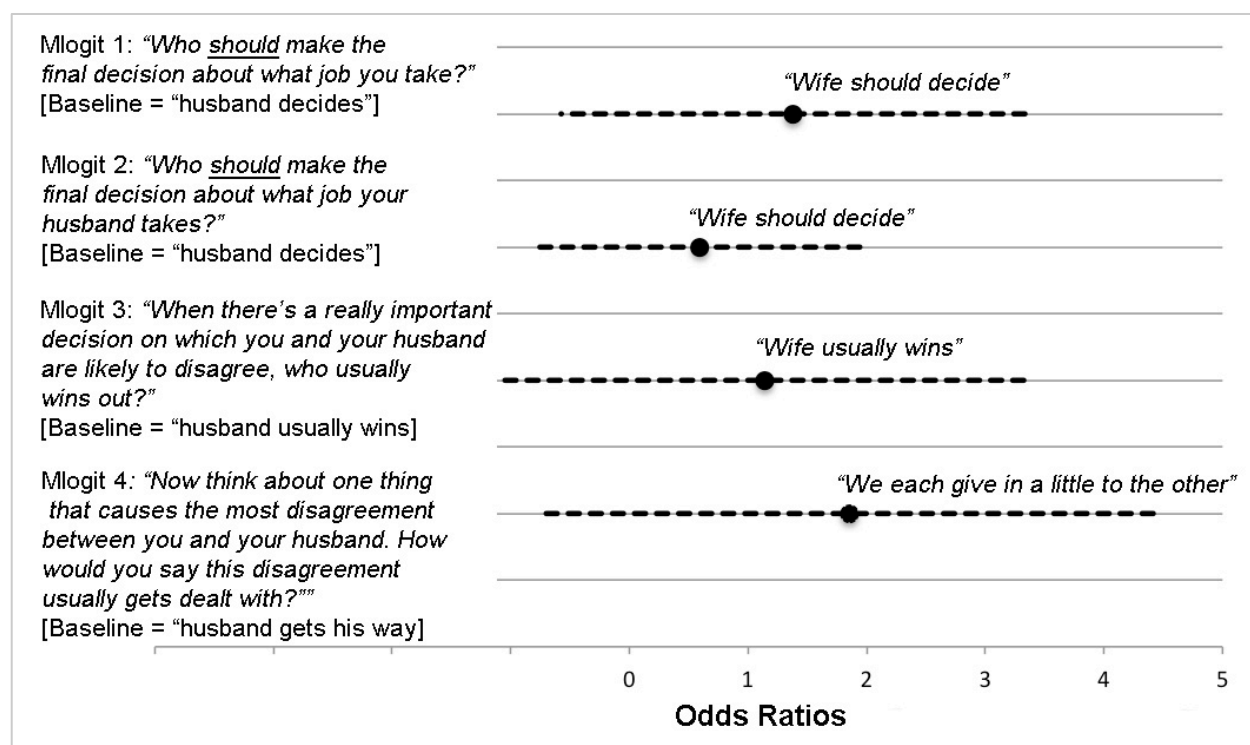
**Table 7:** Predicting wives' assessments of decision-making across three specific question items, multinomial logistic odds ratios

Baseline = My husband ends up having things his way				
Mlogit Model: "Now think about one thing that causes the most disagreement between you and your husband. How would you say this disagreement usually gets dealt with?"				
	(2) I (ie. wife) end up having my way	(3) Neither of us gives in; we just eventually drop the subject	(4) Sometimes he gives in and sometimes I give in	(5) We each give in a little to the other
Difference-in-difference	1.38e-06*** (1.42e-06)	2.842 (2.405)	1.405 (0.988)	1.853 (1.309)
Constant	0.0445 (0.145)	2.243 (2.998)	7.074* (8.284)	4.373 (5.233)
Observations	612	612	612	612

Note: All models control for treatment dummy and study period dummy, and the following variables: Female LFP, male LFP; Weeks employed, male; Weeks employed, female; Home ownership dummy; Number of kids under 6; Family size; Age, female; High school grad, female (not shown).  
Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 8:** Predicting wives' assessment of who "gives in," multinomial logistic odds ratios



**Fig. 7:** Wives' assessments of decision-making power, relative to husbands' power

## 4. The Impact of the Guaranteed Annual Income on Business

### Abstract

How do firms react when the whole labor force has access to a comprehensive guaranteed income without work requirements? In light of the history of guaranteed income proposals, prevailing wisdom suggests that the business response is mixed: certain blocks of capital are staunchly opposed, others supportive. Compounding this ambiguous history are two competing theoretical perspectives. One suggests that the guaranteed income harms business interests by providing alternatives to workers, tightening labor markets, and pulling wages up. The other view argues that the guaranteed income is an employer subsidy, facilitating low-wages and a “low-road” industrial strategy. To gain some traction on the problem, this paper examines the impact of an understudied social experiment from the late 1970s called the Manitoba Basic Annual Income Experiment, or Mincome. This research focuses on Mincome’s “saturation” site, the town of Dauphin, Manitoba, where all town residents were eligible for unconditional payments. Using an archived raw survey of local firms—essentially a census of all businesses in Dauphin as well as in seven control towns at a baseline and during the study period—I analyze the effect of the guaranteed income on hiring, applications, work hours, and in particular, wages. I find a large increase in wages offered by firms in Dauphin, and little change in the control towns. By providing an exit option from work, the guaranteed income may improve the power of workers vis-à-vis employers, enabling them to negotiate higher wages. In order to contextualize these conclusions, this paper draws on a range of archival sources and reconsiders the history of business reactions to guaranteed income proposals in North America, providing an interpretation consistent with the above findings. The proposal details matter. Guaranteed income schemes that facilitate exploitation—those that provide minimal benefits, implement “two tiers” of support, or retain work requirements—may inspire business support. Proposals like Mincome, which curb exploitation and provide generous, universalistic payments with no work requirements, tend to fuel opposition from business.



## 1. Introduction

Much has been written about workers' behavioral responses to the guaranteed annual income (GAI) (Burtless 1986; Hum and Simpson 1993; Keeley 1981; Widerquist 2005). The employer response to this transformative policy has, by contrast, received very little systematic investigation. In light of the history of guaranteed income proposals, prevailing wisdom suggests that the business response is mixed: certain blocks of capital are staunchly opposed, others supportive (Moynihan 1973; Block 2013; Steensland 2007; Haddow 1993). This paper examines a question at the core of an emancipatory political economy: How would a social policy that guaranteed an unconditional minimum standard of living to all citizens affect the interests of business? In order to gain some purchase on the macroeconomic and macropolitical ramifications of this much-discussed social policy, I analyze the reaction of business to an experimental GAI program.

I examine a three-year social experiment, called “Mincome,” which took place in a small Manitoba town in the 1970s. Mincome provided all residents of the town of Dauphin with the option to access substantial cash payments with no work requirements. Using hitherto unexamined raw data from surveys completed by local firms—essentially a census of businesses in Dauphin, as well as in seven control towns, at a baseline and during the study—I analyze the effect of the guaranteed income on wage rates, applications, hiring, and work hours.

Firm-level surveys provide a direct measure of the effect of a substantial, unconditional guaranteed income on the circumstances and interests of local business. Did workers use their threat of exit from the labor market to bargain for better wages? Did employers reduce wages knowing government would top them up? Did firms find it harder to recruit at the bottom of the labor market? Compelling answers to these questions bear directly on the normative and

pragmatic debates over these ambitious policies. In the Dauphin case study, Mincome led businesses to raise wages: wage rates offered on advertised job vacancies and actual wages on new hires grew in Dauphin, relative to controls. Moreover, relative to controls, applications to local Dauphin firms as well as new hires fell during the experimental period. In short, a guaranteed income that curbed exploitation had adverse effects on the interests of business.

As a complement to the analysis of these business surveys, this paper provides an interpretation of the ambiguous historical evidence bearing on how business organizations have reacted to North American guaranteed income proposals over the years. I use archival sources, including letters, speeches, reports, and other historical materials, in order to decode some of this history and provide a consistent interpretation of the seemingly uneven and equivocal responses from Canadian and U.S. business groups. In both Canada and the United States, GAI proposals were floated at different times, and businesses had ample opportunity to form positions on them.<sup>67</sup> Daniel Patrick Moynihan's history of the Family Assistance Plan (FAP)—a GAI proposal which was nearly approved in the U.S. Congress in 1970—stresses that while most groups were divided on the program, “nowhere did the division attain the degree of clarity, precision, and completeness that occurred all but instantly among representatives of business” (1973: 285). In Canada, guaranteed income schemes from the Family Income Security Plan (1970) and Quebec's Castonguay-Nepveu proposal (1970) to the Macdonald Commission's Universal Income Security Program (1985) garnered equally schizophrenic responses from business, ranging from supportive to apoplectic (Leman 1980; Haddow 1993).

Dovetailing with this ambiguous history are two competing theoretical perspectives. One position, asserted first in the 1970s and again more recently, characterizes the guaranteed income

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<sup>67</sup> These policies took the form of a negative income tax (NIT), which I take to be synonymous to the GAI. The crucial feature of the NIT is the phase out: a guaranteed level is set, and as a recipient's market income rises, guaranteed payments are phased out at some “tax back” rate.

as a subsidy to employers, a policy that facilitates the efforts of large, labor-intensive companies to maintain poverty-level wages (Levi 1970; Howell 1997; Young and Mulvale 2009). The second view argues that an exit option will reduce competition at the bottom of the labor market, improving the bargaining power of labor and pulling wages up (Block and Manza 1997; Widerquist 2005; Wiederspan, Rhodes, and Shaefer 2015). The main problem with these debates is that the object under scrutiny is ever-changing. I show that the complexity and diversity of policies in the U.S. and Canada designated as “guaranteed income” thus clouds analyses of the business response to these proposals.

The guaranteed annual income is a Janus-faced policy proposal. Some of the versions debated over the past four-plus decades have been available only to a select group of people, others had built-in work requirements, and others still provided the means to sustain a basic standard of living universally and unconditionally. When these policies were proposed in the 1970s and 1980s, these crucial policy details were often opaque. Section two parses through the historical evidence and argues that much of the disagreement on the guaranteed annual income comes down to disagreements about how these programs should and actually do work. In sections three and four I clarify the theoretical framing and argue that the concept of exploitation provides a key to deciphering a range of guaranteed income schemes with superficial similarities. When businesses take favorable positions toward the guaranteed income, typically this is because the scheme in question facilitates—or at minimum, *allows*—rather than obstructs continued exploitation. In contrast, guaranteed income schemes that improve workers’ negotiating position, those that reduce people’s market dependence and provide the tools to curb exploitation, should best be understood as inimical to business power.

I apply this “exploitation-based” interpretation to the history of the business reaction to guaranteed income proposals. In the same way, I show that Mincome eroded business dominance in the local labor market by reducing workers’ market dependence. Sections five and six provide background on the Mincome experiment and the survey of firms. In section seven I present descriptive statistics from survey data on wages and other labor market indicators. I conclude that a guaranteed income scheme that made substantial payments, available to all without work requirements, reduced the power of employers over workers and dampened exploitation in the labor market.

The broad objective of this paper is to elucidate the interests of a key political actor—business organizations—in order to clarify the contours of support and opposition to broad and universal income maintenance policies. To the extent that business has historically exhibited support for guaranteed income policies, the actual policies tended to be “two-tiered” rather than unitary, selective rather than universal, and miserly rather than generous. Schemes that have gone under the moniker of “guaranteed income” have been quite diverse. However, generous, unconditional guaranteed income policies that reduce workers’ market dependence will garner little support from business organizations. For policy-makers and activists eager to find support for Mincome-like policies, it is valuable to have a clear understanding where natural allies and opponents stand in relation to the crucial policy details, not in relation to the generic policy umbrella.

## **2. History of the Business Response to the Guaranteed Income**

How have business organizations reacted to guaranteed income proposals? This section provides an interpretation of business positions on a range of American and Canadian guaranteed income

proposals in order to assemble the puzzle pieces of this disputed history. I show that, in a number of high-profile proposals, behind the apparent dissensus among business groups lies a consensus stance against universalistic and unconditional guaranteed income schemes.

### *2.1. American Business Organizations and the FAP Experience*

Nixon's Family Assistance Plan (FAP) did not lend itself to the easy formation of political opinion, as it took on multiple forms over its short lifespan. The first iteration of the policy proposed a negative income tax (NIT) that entailed no coercive work requirement, although after debate and controversy, and the publicity of the policy details, later variants embraced a workfare component (Steensland 2007; Bowler 1974; Burke and Burke 1974; Davies 1996). Daniel Patrick Moynihan's (1973) history of the proposal emphasizes that no group was more divided on the plan than business: "The range of business opinion was as wide as the subject permitted: from complete endorsement of a guaranteed income to complete rejection" (1973: 287). In this story, the U.S. Chamber of Commerce was opposed, even hysterical, but the Committee for Economic Development (CED) and the National Association of Manufacturers (NAM) were supportive. Behind the outward semblance of disagreement between the three business organizations was a misunderstanding of what the FAP actually entailed. What unified the three was an unambiguous and principled commitment to an income maintenance policy with work requirements.

There is truth, however, to the fact that these groups had distinct economic interests. The national Chamber was dominated by small and mid-sized labor-intensive firms whose profits depended heavily on local labor conditions (Quadagno 1990). As Quadagno writes, "The key issue in Chamber opposition was the threat the FAP represented to its members' labor supply."

Quadagno also highlights the role of gender, namely, the desire to preserve “a supply of low-wage female labor” (1990: 20). It was natural that the Chamber would unambiguously oppose the FAP, especially once they discovered the lack of a work requirement (Moynihan 1973). Likewise, Myles and Pierson (1997) argue that the FAP presented a clear threat to Southern business interests whose competitive advantage was a cheap and disorganized supply of low-wage workers. Some reporting at the time drew attention to the possibility that the FAP could undermine employer domination. One Georgia representative voiced this sentiment: “There's not going to be anybody left to roll these wheelbarrows and press these shirts” (Armstrong 1970: 67). A field coordinator for the Federation of Southern Cooperatives in Atlanta made the same prediction: “I know a lot of white people who will get told to go to hell” (Ibid.).<sup>68</sup>

In contrast to the Chamber, the CED was composed of an elite group of internationally oriented, capital-intensive firms. This meant that they represented firms whose labor costs were a smaller portion of their total costs and who were generally less sensitive to local labor conditions. Moreover, executives of oil companies, the quintessential capital-intensive industry, headed the CED committee analyzing the guaranteed income. Before being supplanted by the Business Roundtable in the mid-1970s, the CED was, according to G. William Domhoff (2013), the most prominent voice of corporate interests.<sup>69</sup> While it is true that the CED represented capital-intensive firms that were less hostile to the FAP, the dissensus among business organizations Moynihan points to is overstated.

On closer inspection it is not that these business groups diverged greatly in their positions on the GAI. The relevant difference is their divergence on what they thought the FAP would

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<sup>68</sup> Richard Armstrong was one of the few journalists at the time who clearly outlined the effect of the FAP on social power relations: “The new program, if passed, would have an explosive effect on black (and poor-white) incomes and give a powerful boost to black political movements all over the lower South” (1970: 66).

<sup>69</sup> Domhoff identifies the CED as “corporate moderates” (2013: 174).

actually entail. The Chamber opposed the FAP because it contained no work requirement; the CED on the other hand supported it, citing their endorsement of the work requirement.

The Chamber was alarmed that the GAI would allow the able-bodied workforce to escape work: “the businessman ... may soon come to realize ... that self-reliance has been reduced from an ideal to an option” (cited in Moynihan 1973: 286).<sup>70</sup> Meanwhile, Moynihan argues that the CED’s position was strongly in favor of the FAP, and “diametrically opposed” to the Chamber’s position. As a representation of the enlightened business view, Moynihan quotes a statement by CED chair Joseph Wilson, who wrote that “neither training nor work should be made a condition for continuance of public assistance to women heads of households.” It is true that the CED took strong liberal positions—especially in their call for adequate daycare services—and in congressional hearings they refer to the plan as an “enlightened investment” (Congress 1970: 1457). However, later in the same statement, Wilson clearly articulates the importance of the work requirement: “As a matter of principle, we believe that those who are able to work should work” (Ibid.).<sup>71</sup> Moreover, a 1970 CED policy report laid out their basic position at the time: “A major flaw in the welfare structure is that until recently it has afforded no inducements to work. ... A system of incentives to work would be an essential component” of any alternative (1970: 11-12). Even though there was no technical work requirement in the FAP, what the CED seemed to find appealing was its centrality:

A basic element of the Administration’s program is its emphasis on work. All applicants for benefits who are not working, except mothers with children under six, must register with

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<sup>70</sup> In its first permutation, the FAP was indeed radical. A journalist in *The New Republic* described Nixon’s plan as follows: “All it would do is put an income floor under everybody, and the roar of protest you don’t hear coming out of Washington today is from quivering conservatives gaping with dropped jaws at their peerless leader. Yes, if Truman had launched it, or FDR, or Kennedy or Johnson, the sonic boom of rage would have blown the petals off the cherry trees” (1970: 4).

<sup>71</sup> Wilson also argued that “I would think that we would do better to subsidize some jobs rather than have them just rely entirely on welfare” (Congress 1970; 1467).

state employment services for training or “suitable” employment, or lose their portion of the family benefit. (1970: 13).

The CED sought an alternative to the existing welfare system, and there is no doubt that their positions were more progressive than those of the Chamber, but they did not favor an unconditional and universalistic program.<sup>72</sup>

The reaction from the Chamber of Commerce had its own superficial dissensus. Had he noted that internal divisions existed within the Chamber itself, Moynihan might have strengthened his case for disagreement within the business community. When the Chamber conducted an internal poll, it found that 86.5 percent of respondents supported the FAP. However, according to Chamber general manager Milton Davis’s statement to Congress, at the time of the poll “the administration’s program was not recognized as something that would guarantee an income for many families with fully employed fathers” (Congress 1970: 11897). He noted that “47% said that what they wanted most was ‘to require welfare recipients who can do so, to take work or take training.’ Another 31% said they wanted most ‘to make taxpayers out of many welfare recipients’”. Davis concludes that it was clear that “majority opinion in the poll was for substituting workfare for welfare where possible” (Ibid.).

Moynihan also contends that support for the Family Assistance Plan was strong among the National Association of Manufacturers, another group dominated in large part by capital-intensive firms (Burch, 1973). But their support, too, was based on ambiguity about what it was they were supporting. In congressional hearings on the Family Assistance act of 1970, NAM

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<sup>72</sup> In their congressional hearing another CED representative, C.W. Cook of General Foods, opined: “it was not easy for me to come around to what the popular press calls a guaranteed annual income. It happens to be contrary to the fundamental beliefs of the business system in which I have spent 39 years... and my ideals with regard to each individual’s responsibilities as a citizen.” His decided later to support the FAP, as long as “it is a plan based firmly on providing incentive to work and not merely continuing relief” (Congress 1970: 1463-4). In no uncertain terms, he wrote: “We support the incorporation of a requirement for training or work for the able-to-work an integral element of any income maintenance system.” (Congress 1970: 1461).



representative Archer Bolton argued that the group would favor the FAP “if the basic allowance is a realistic minimum, and if the earnings disregard provides a true incentive to work and advancement and if the work requirement is strong” (Congress 1970: 1928). They also wanted to ensure no plan could operate as leverage during a strike—the guaranteed income has been sometimes referred to as an inexhaustible strike fund (Wright 2004). They argued to Congress: “We suggest that anyone directly involved in a labor dispute should be ineligible for benefits under the family assistance plan” (Ibid.). The critical factor in any program, they concluded, is that it “strengthens rather than weakens the connection between work and income” (Congress 1970: 1928). When an evidently confused senator asked whether NAM was endorsing the bill, Bolton proclaimed, “Yes, sir. ... With several suggested changes.” (Congress 1970: 1929). As the comments above attest, these changes were foundational, not cosmetic.

Other business leaders had given favorable positions on the negative income tax in the years running up to the FAP. According to interviews with corporate heads in the late 1960s (Rosen, 1968; Auspitz, 1967), one key source of their support was the policy’s conservative pedigree, its association with Milton Friedman, which was well publicized in a 1966 Chamber of Commerce symposium on the GAI.<sup>73</sup> However, the most common argument consistently at the core of business support was that relative to the existing welfare system, which discouraged employment, a negative income tax would actually provide employment incentives.

In short, although American business organizations may have been split on the FAP and the NIT, they were not split on the importance of a public policy that made work non-optional. Rather, like many parties, they were split on what the FAP actually was.

## 2.2. *Views of Canadian Business from FISP to UISP*

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<sup>73</sup> Archives of Manitoba, Winnipeg, MB; Winnipeg Chamber of Commerce fonds.

Divergent positions on a cluster of guaranteed income proposals throughout the 1970s and 1980s took on a similar pattern in Canadian business circles. In Canada, it was actually a businessman and chartered accountant named D. B. Smith (1965) who first laid out a fully elaborated scheme for a guaranteed annual income. Six years later, Smith had persuaded Manitoba's New Democratic Party Premier Ed Schreyer—who already had the Mincome proposal in the works—to pitch the idea to the Canadian Institute of Chartered Accountants. Schreyer made a case for the guaranteed income that could appeal to the business community:

In other words, we want to enhance the quality of life in Manitoba. And when the quality of life is enhanced it reaps many dividends, some of which accrue directly to the business community. People are better educated, better skilled, and happier producers able to raise the level of productivity. Indeed, the quality of life, and quality of living, are among the factors that business developers look for in their own investment and expansion programs (1971: 2).<sup>74</sup>

Two years prior, federal government representatives had tested out the popularity of the guaranteed income concept with the Canadian Chamber of Commerce, prompting one Chamber representative to respond: “My, how times have changed; you would never have heard this kind of talk even a few years ago” (Brudner 1970: 33). The receptive atmosphere in policy circles had penetrated even the business community.

Relatively positive assessments from business were not uncommon, especially in the early 1970s. However, the guaranteed income in virtually all Canadian proposals in the 1970s and 1980s contained no threat to their economic interests. No major guaranteed income proposal in Canada—unlike early iterations of Nixon's Family Assistance Plan—would free workers from work. The 1970 Family Income Security Plan (FISP), comparable in some ways to Nixon's FAP, would provide an annual payment of less than a quarter of that offered in the FAP, ensuring no

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<sup>74</sup> University of Manitoba Archives, Ed Schreyer fonds, 19-6. 1971: 2.

loosening of workers' market dependence (Leman, 1980; Shifrin, 1977).<sup>75</sup> The massive seven-volume Castonguay-Nepveau proposal of 1971 was explicitly designed as a "two-tiered" system, with work requirements for the able-bodied. The 1973 "Orange Paper", a major welfare-reform proposal (Lalonde, 1973a), similarly proposed a "GAI by category" (Gordon, 1973), offering one plan for those who could not work and a supplement available to those who could. At the tail end of a burst of interest in the guaranteed income, the controversial Macdonald Commission of 1985 proposed a Universal Income Security Program (UISP), another two-tiered guaranteed income scheme, and perhaps the ideal typical case for regarding the policy as a neoliberal wolf in sheep's clothing. The proposal called for the abolition of virtually every existing government income security measure (Hum 1986; Kesselman 1986; Ross 1986; Ruimy 1985; Shifrin 1985).<sup>76</sup> Had it succeeded, the UISP might have morphed into a broader, more generous and truly universal policy, but as recommended in 1985, the benefit level was low, the conditions were many, and it was energetically opposed by labor (Haddow 1994).

A.W. Johnson, former Deputy Minister of Health and Welfare Canada, summed up the income maintenance debates in the 1970s, claiming that "the payment of universal allowances large enough to meet the full costs of ... child raising ... was never really considered" (1975: 466). Likewise, "the single guaranteed income system as a replacement for all existing programmes, was also rejected by Ministers, without extensive debate or discussion" (Ibid.). Johnson's remarks can be extended to the mid-1980s. The guaranteed income schemes on the

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<sup>75</sup> It would pay somewhat more to poor families by paying somewhat less in universal family allowances, but meager payments guaranteed that the labor supply would be unaffected.

<sup>76</sup> As Kesselman summarizes: "The Commission pursues a categorical strategy to keep the income guarantee for employable individuals low relative to the level guaranteed to those unable to work" (1986: 105). A low guarantee is a work requirement; however, in addition, Kesselman notes "it suggested a possible role for tests to require that employable UISP recipients be working, searching for work, or participating in job creation projects" (1986: 107).

table in Canada were small, selective, and two-tiered. Tentative support from business organizations should be understood in this context.

By the early-to-mid-1970s, federal policy-makers had become aware of the attractions of the two-tiered GAI, with one guarantee level and set of conditions for those expected to work and another for those unable to work (Van Loon 1979). A unitary system was considered the “major flaw in Nixon’s then recently deceased Family Assistance Program”; planners had read and were “strongly influenced by Daniel Moynihan’s recently published *The Politics of a Guaranteed Annual Income*” (Van Loon 1979: 478; 483). Even Saskatchewan’s short-lived Family Income Plan adopted a two-tier system (Riches 1971). In spite of the greater administration costs, separate treatment for the “able-bodied” poor had political appeal.

Some of these schemes mustered a degree of business support, others garnered opposition (Leman 1980; Haddow 1993), but none of the actual proposals—excepting the Mincome program—provided exit options from the labor market to the whole workforce. Despite some flirtation with guaranteed income type policies, Canadian business remained solidly opposed to comprehensive policies that diminished the market dependence of workers. In 1970, the Chamber articulated their position on the guaranteed income in a detailed policy submission to the Senate. They distinguished between two versions of the policy: one, which they opposed, was associated with Robert Theobald and entailed “universal payments” to everyone below the poverty line; the other was a vaguely articulated supplement, but was associated with Milton Friedman. They were circumspect but took a moderately favorable position on the latter, as long as it substituted for all existing social security and welfare programs and promised a “sensible, selective approach to social security” (1970: 18; 29).

Rodney Haddow (1993) tracked the positions of the Canadian Chamber and the Canadian Manufacturers' Association (CMA) through the late 1960s and early 1970s, noting their consistent commitment to sound actuarial principles and selective benefits over universal ones. To the extent that groups like the Chamber and the CMA supported plans such as the FISP, what they supported was the principle of selectivity as a means to increase support for the poor (see also Blake 2009: 226). CMA president A.G.W. Sinclair wrote to Health and Welfare Canada Minister John Munro to declare support for the FISP, but clarified: "[T]he Association favours programs which are selective and ... retain the incentive for an individual to work and to accept responsibility for his own welfare" (McInnes, 1978: 332).

Business support for the FISP is evidence not that a comprehensive, bona fide guaranteed income turns out to be somewhat compatible with employer interests, but rather, that the guaranteed income proposals on offer at the time boasted minimal benefits and in-built work requirements. The proposals of the early 1970s had some merit for business organizations because they were built on the premise of a vulnerable workforce. When it came to a "guaranteed annual income for everybody," it was clear to the Chamber by 1974 that the proposal, if broad and comprehensive, could breed the "repudiation of the currently accepted work ethic" (Haddow 1993:173). Over the course of the 1970s business organizations turned sour on the guaranteed income. In response to Chamber worries, the Federal government promised that no new universal income maintenance programs would be considered (Lalonde 1973).

When the Macdonald Commission put the guaranteed income back on the agenda in the mid-1980s, the Business Council on National Issues (BCNI) had become perhaps the most prominent and sophisticated pro-business lobby in Canada. The BCNI was formed in 1976, shortly after the

founding of the U.S. Business Roundtable; they were similarly dominated by large and internationally oriented firms and therefore, among business groups, the most likely candidates to support a robust income security plan. The positions of the BCNI are interesting because the organization distanced themselves from the “carping and special pleading” that characterized other business groups; they instead identified with a more enlightened and far-sighted approach (Lagille 1987: 42; see also Dobbin 2003).<sup>77</sup> If there would be support for a guaranteed income it would come from the BCNI. However, in a 1986 discussion paper they took a cautious position on the already anemic UISP. The BCNI felt the UISP was a “simplistic solution to a complex problem” (BCNI 1986: 32). Although they were “not opposed in principle to the model” behind the UISP, this version suffered from being overly “comprehensive”; they cautioned policy-makers not to “lose sight of the overall goals of achieving greater selectivity and of providing incentives to encourage positive labour market behaviour” (BCNI 1986: 32; 63).

The historical record in the U.S. and Canada demonstrates that the range of business opinion on the guaranteed income was in fact fairly narrow. If there was “clarity, precision, and completeness” within business on these issues, it existed only on the subject of a comprehensive and unconditional guaranteed income, where it formed a solid block of opposition.

### **3. The Exploitation Pivot**

A variety of income-support schemes—including the proposals above, but also including the Earned Income Tax Credit, The Alaska Permanent Dividend Fund, the universal basic income, the NIT at different support levels, claw-back rates, and conditions—are frequently lumped

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<sup>77</sup> Also important in this debate was the Canadian Manufacturers’ Association, whose “detailed” recommendations during the 1983 public hearings of the Macdonald Commission recommended a selective guaranteed income to replace all existing income transfers (Haddow 1994; Iacobacci and Seccareccia 1989).

together in popular and academic discussion. Behind the superficial agreement on guaranteed income policies lies deep disagreement on what they should do and how they should operate.

The central difference among programs that are often grouped together, I argue, is their orientation to the concept of exploitation. Do they diminish worker exploitation or do they facilitate it? It is worth briefly describing the term because it has never gained common currency in social science circles. The concept of exploitation has a long history, but the classical explanation defines it as a relationship where workers produce more economic value through their laboring effort than they receive in wages (Marx 1990; Samuelson 1971; Elster 1985; Roemer 1985). In the Marxian tradition, the difference between these quantities, or, the portion appropriated from workers, is considered profit or surplus value. The sociology behind the relation of exploitation is perhaps more germane, if less easily quantifiable. The concept identifies a mutually antagonistic relationship of dependence, where the welfare of the owners of scarce, productive resources depends on the deprivations of non-owners (Wright 1995; 1997). Employers, from this perspective, depend on the existence of a laboring class that remains perpetually dependent on owners to maintain their standard of living. Without the background condition of deprivation, the extraction of labor effort from employees would be uncertain, and the dominance of employers in the workplace would be diminished.

Thus, exploitation is reduced *quantitatively* when workers are empowered to negotiate their wages upwards and force down the portion appropriated by employers as profits. From a sociological perspective, exploitative relations are reduced *qualitatively* when poor and working people are increasingly able to enjoy opportunities to maintain their standard of living outside the market. That is, when their labor is increasingly decommodified, when they are less dependent on the market for their survival (Esping-Anderson 1990). Through a qualitative lens, fewer

applications, new hires, and work hours indicate that people became less ensnared in exploitative relations.

The virtue of the exploitation concept is that it brings the relevant actors—employers and workers—into relation with one another. By contrast, the near-ubiquitous language in guaranteed income analyses centers on “work incentives,” as if this individual behavioral attribute, drained of its social significance, is a good in itself, and unrelated to the relationships between people in the labor market. It is not simply that “work incentives” have an abstract and morally desirable quality. Fundamentally, it is in the material interest of business to have a workforce with limited alternatives. Domination at work depends on the lack of alternatives outside of work: the fewer viable outside options there are, after all, the more pliable will be the workforce and the more power will reside with employers. The main source of employer power is the ability to sack an employee (Kalecki 1943). Decommodification, however, reduces the sting of the sack and thereby an employer’s ability to impose his will on employees.

In sum, an exploitation-based analysis highlights the ways in which business maintains a positive interest in a supply of workers with a limited or undesirable set of outside choices. Where the concept of work incentives obscures the relational aspect of labor markets, the concept of exploitation brings it into relief. From this perspective, the wellbeing of employers depends on circumstances where workers are dependent on the market for their survival.

Thus, we can ask of any concrete policy proposal, does the scheme reduce the market dependence of workers or does it rely on market dependence? Does it decommodify labor by subordinating market imperatives to non-market forms of social reproduction? Or, is the policy contingent on workers selling their labor to employers?



The Alaska Permanent Dividend Fund (Widerquist and Howard 2012), for example, does not undermine continued exploitation because the amount is so minimal it does little to facilitate escape from the labor market. Similarly, Canada's Guaranteed Income Supplement (Johnson 1987) is available only to the elderly who have already left the workforce. The Earned Income Tax Credit (Sykes et al. 2015), although designed with NIT-type mechanisms, is conditional on work. And two-tiered systems like those discussed above provide generous support for the "deserving" poor, while ensuring that the "undeserving" poor remain market-dependent (Katz 2013). Rather than reducing the dependence of workers on employers, these schemes entrench it. Any negative income tax entailing work requirements or offering a guarantee level that is sufficiently low falls into this category, and would not weaken the power of business over labor.

What to make, however, of the more unitary and less conditional versions? The Mincome experiment is interesting because its design was stronger and more comprehensive than other proposals. It was a unitary scheme that provided a decent standard of living with no work requirements. For many people, it offered autonomy and made work a choice; it provided a decommodified alternative to the market. It is the best case, then, for gaging the overall business response to such guaranteed income plans. Did Mincome reduce the vulnerability of poor and working people to exploitative employment relationships, or did it facilitate exploitation? Before describing the experiment and the business survey, the next section clarifies the two main theoretical positions on the guaranteed income and business.

#### **4. Theorizing the Role of Business in a Guaranteed Income Regime**

Would a comprehensive and unconditional guaranteed income reinforce or weaken business dominance? Would it lead firms down the low-road developmental path? Or would it tighten labor markets, forcing wages up and expanding workers' bargaining power?

The first view argues that employers will reduce wages in response to a GAI program, since workers are now receiving a top-up from government. For example, Margaret Young, from the Canadian Centre for Policy Alternatives, has argued that "a guaranteed income will amount to no more than a simple employer subsidization and an incentive for employers to reduce workers' wages" (2009: 22). Throughout its history, critics have compared the guaranteed income to the Speenhamland system—the eighteenth-century British poor relief scheme—arguing that both amount to an employer subsidy that facilitates employers' efforts to pay below-subsistence wages (Droppelt 1975; Levi 1970; Lekachman 1969; Burke 1970; Howell 1997; Young 2009). Block and Somers (2005) track the history of the analogy and point to news articles explicitly comparing the two policies. The putative lesson learned from the Speenhamland episode is that programs like the guaranteed income have perverse consequences; when enacted, they have "allowed employers to hire workers at below-subsistence wages, and allowed landlords to raise rents" (Chwialkowska, cited in Block and Somers 2005: 33).

Pierson and Myles (1997) make a similar arguments. They point out that in the 1970s Canadian labour worried that the GAI could both subsidize low wage employers and substitute for a full employment policy (456).<sup>78</sup> David Howell echoes Pierson and Myles's conclusions,

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<sup>78</sup> Before the FISP and Lalonde proposals, however, Canadian labour strongly advocated for the GAI. In 1972, CLC Vice President argued: "I point to the experience of the trade union movement in its fight for a bigger slice of the economic pie over the course of the last century... what has taken place is ... a higher standard of living for all. The effects of a GAI, I would urge, would be similar." In 1970, the CLC, together with the Ontario Federation of Labour, produced posters and leaflets advocating for the GAI and bought 60-second radio spots on fourteen radio stations to pitch the GAI across Ontario. One ad went like this: "A guaranteed annual income for all Canadians... That's what the Ontario Federation of Labour says is needed today. ... We cannot long afford to have over twenty percent of our people living in poverty

arguing that “the NIT is really a grown-up version of the EITC ... a broad-based wage subsidy” (1997: 535). “With a wage subsidy,” Howell argues, “the minimum socially acceptable wage can be expected to drop, just as it did in the Speenhamland episode” (1997: 535). The guaranteed income will “facilitate even further the low-wage path ... inadvertently revisiting Speenhamland.” In this depiction, the guaranteed income amounts to guaranteed exploitation. Howell concludes: “Rather than encourage low-wage flexibility, why not support ‘voice’ over ‘exit’?” (1997: 536; 538).

The employer subsidy question was not a concern restricted to skeptics. For Manitoba NDP Premier Ed Schreyer, shedding light on this issue was a key justification of the Dauphin experiment. In a 1971 speech, he asked:

In a sense is there a danger we will be just subsidizing local merchants? Would employers hold back on wage increases which otherwise might have been granted, because the wages, in effect, would be supplemented? Such a result would defeat the ends we are trying to achieve (1971: 8).<sup>79</sup>

At the national level, in a 1973 speech to the CMA, Health and Welfare Minister Marc Lalonde voiced the same concern: a separate category of income supplements for the working poor meant that “we potentially face a severe attack from those who will claim that it is simply a subsidy program for low-wage employers – a means of permitting them to keep their payroll costs down, to continue to pay substandard salaries to workers or even to reduce salaries” (1973b: 11).

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while the rest of us enjoy all the good things of life. That is why we would urge you to support our campaign for a guaranteed annual income for all Canadians” (LAC, Ottawa, GAI Microfilm reels, R5699-67-3-E. Microfilm reels H-698 (1970-1972) and H-725 (1969, 1971).

<sup>79</sup> University of Manitoba Archives, Ed Schreyer fonds, 19-6. 1971: 8. Likewise, in a speech at the Canadian Council of Social Welfare, Minister of National Health and Welfare John Munro asked whether “any advances in real income incurred under a Guaranteed Income might be completely negated by unscrupulous profiteering by landlords and merchants, on the people who would benefit from such an income floor (3-4).” LAC, Ottawa, ON. GAI Microfilm reels.

The second view suggests that a comprehensive guaranteed income would give workers more negotiating power, thereby *reducing* exploitative relations in labor markets. A genuine exit shifts the balance of power in the workplace. Contra Howell, the possibility of “exit” is precisely what facilitates “voice”. There are a few moving parts to this mechanism, some or all of which may be in operation. First, an exit option gives workers a better position to bargain up wages with their current employers. Second, some workers might exit the workforce or reduce work hours, and tighter labor markets could then compel employers to bring wages up for their current workers. Finally, new job applicants, hesitant to accept the lowest wages available, may be in a position to demand more attractive offers. Fred Block and Jeff Manza make an argument in favor of a progressive negative income tax along these lines: “If a negative income tax placed a floor under income” it would provide “the economic security to reduce their hours of work. With such reductions, it would be possible to tighten the labor market by reducing the aggregate hours of work supplied by those already in the labor force” (1997: 483). In sum, competition at the bottom end of the labor market would be blunted.

The hypothesis taken in this paper is that this view is correct. The view suggesting that the guaranteed income functions along the lines of the Earned Income Tax Credit is a category error. Conditionality on work is the fundamental difference. The EITC provides no support for households with zero or insignificant market incomes, it provides no alternatives to work, and it has no market-tightening effect. In fact, the EITC may increase competition at the bottom of the labor market and even drive wages down (Rothstein 2010). In short, unlike Mincome, which enables labor-market exit, the EITC provides no opportunity to curb exploitation, in either qualitative or quantitative senses.

Thus, in the Mincome case, both wages offered and actually provided should rise, not fall. Moreover, because of the exit option available to workers, applications to work should decline during the experimental period in Dauphin. Indeed, this is part of the explanation of higher wages and, if true, would lend support to the latter hypothesis. Additionally, fewer applications should mean fewer new hires. Finally, not only should the new hires work for higher wages, they should also demand to work fewer hours. Before turning to the firm survey, I expand on some of the history and specificity of Mincome.

## **5. The Mincome Experiment**

Mincome was devised in response to a number of influential reports that publicized the extent and depth of poverty in Canada in the late 1960s and early 1970s. The Economic Council of Canada (1968) and the Department of National Health and Welfare (1970) presented the guaranteed annual income as an intriguing idea meriting serious consideration. The “Croll” Report (1971) and The Real Poverty Report (1971) posed the guaranteed income as the central policy solution of the era, an idea “whose time has come” (Canada 1971: 175). Inspired directly by four similar experiments in the U.S., it was hoped that Mincome would demonstrate the feasibility of the guaranteed income to the Canadian public.

As with the U.S. studies, the primary axis of the demonstration concerned the potential effects on the labor supply. However, the early academic documents and reports influencing the design and execution of Mincome show demonstrable learning from the more narrowly focused randomized controlled experiments in the U.S. (Hikel and Harvey 1973; Atkinson et al. 1973), which restricted analysis to small groups of recipients dispersed across geographies. It became clear that isolated individual responses to a randomized guaranteed income experiment could be

confounded by macro-effects (Hikel, Laub and Powell 1974; Atkinson et al. 1973). In particular, the American GAI experiments studied individual labor supply effects as if there were no interactions with business.<sup>80</sup> The implicit, and untenable, assumption was that firm behavior is unaltered in a world of basic income. By contrast, the early Mincome planning reports made some recognition that labor supply should not be studied apart from its interaction with labor demand (Ibid.). This meant that a more ambitious research design was needed, one including a “saturation” site with open enrollment—Dauphin—rather than the conventional randomized control trials.

A more ambitious design naturally led to a more wide-ranging use of research tools and survey instruments. Because Dauphin’s whole labor force had the option to collect Mincome payments, it was natural to study the effect on local firms directly. One unique survey, discussed in the next section, was designed specifically to measure the broad labor market effects from the perspective of local business.

Before moving to this survey, it is important to make a few points about the size, distribution and details of the Mincome payments.<sup>81</sup> Dauphinites were offered guaranteed incomes equivalent to \$19,500 for a four-person household.<sup>82</sup> Families with no labor market income, for whatever reason, could access the full guarantee, 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. At a negative income tax rate of 50 percent, people could always increase their incomes by working. Every dollar of labor market earnings reduced the guarantee

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<sup>80</sup> This is not quite Hamlet without the prince. It is perhaps closer to Hamlet without the rest of Denmark.

<sup>81</sup> In a town with a population of 8,885, along with a 3,165 person rural municipality, at least 18 percent—2,128 individuals, or 706 households—received benefits at some point throughout the program (This is a lower bound because available data excludes late-joining farm families; an estimate of this group increases the participant count to 2,457, or 20 percent of the population).

<sup>82</sup> All figures are in constant 2014 dollars.

by 50 cents; this meant that payments phased out once earnings reached \$39,000. To put these payment figures in perspective, real median household income for Dauphin and its rural municipality was only \$24,758, and median family income was \$39,166, according to the 1971 census.

For many people, Mincome payments made exits from work possible. Moreover, the experiment's design made it possible to incorporate effects on labor withdrawals from the *non-poor* working population, another factor ignored in the negative income tax studies where all randomized participants had low incomes (see Minsky 1969, who made this rarely acknowledged point about the American GAI experiments). An experiment that makes work optional necessitates an analysis of the interaction with employers. The unique design feature of the Dauphin study makes it the only guaranteed income experiment where the role of business—a crucially important economic and political actor in any policy affecting the labor market—could be explored. The next section describes the survey of businesses.

## 6. Survey of Firms

This paper analyzes a hitherto unexamined raw survey of local business in Dauphin (N=292) and seven control towns (N=1155) before and during the experiment. Completed hard copies were found in eight boxes in the Library and Archives Canada's Mincome accession. Each individual survey was first scanned and then transcribed into a dataset. The survey differs from all other surveys conducted by original Mincome researchers because it was administered to non-participant firms rather than the panel of experiment participants. The original purpose of the survey was to estimate labor demand conditions in Dauphin and in rural control sites.

The survey was sent to all businesses in the towns of Dauphin, Carmen, Morris, Portage La Prairie, Stonewall, Minnodosia/Neepawa, Morden/Winkler, and Swan River. The first “baseline” survey was sent to businesses in November 1974, before the experiment began. The second survey was sent in late August 1975, once the program had been up and running for about nine months. It arrived in the mail with a short letter explaining the experiment and requesting the cooperation of each business. The letter emphasizes the importance of understanding job market effects:

I am writing to you on behalf of Mincome Manitoba ... We are studying the effects of government income assistance on people's work habits. For this, we need accurate information on the current job market in Manitoba ... The question is a critical one. Perhaps your own business has been affected by the operation of government assistance programmes.<sup>83</sup>

The survey was discontinued after the second wave in the midst of an overall downscaling of the analysis side of the experiment, which also saw the abandonment of ethnographic analysis, a survey of farm families, and a survey of class relations (Rasmusen et al. 1979).<sup>84</sup> Along with the rest of the survey data collected on the Dauphin experiment, this firm-level questionnaire was archived but never analyzed.

Roughly 19.5 percent of all surveys were mailed back, valid, and non-blank. Despite a low response rate, 19.5 percent of all local businesses is a non-trivial amount of information. Is there reason to think there are systematic biases to non-response? While there may be biases in term of which firms completed and returned the survey, there is little reason to think that these biases are unique to Dauphin. One possible source of bias along these lines may be that Dauphin firms, hearing about the experiment, would be more likely to express their views as the experiment

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<sup>83</sup> Archives of Manitoba, Winnipeg, MB, Winnipeg Chamber of Commerce fonds. M-91-3-6-22.

<sup>84</sup> The survey used herein is available at LAC, Winnipeg, MB., Mincome files. Boxes 69393-69402. Discontinued and incomplete surveys are available at LAC-Winnipeg, Boxes 69746-69747; 69412-69486.



went along. Perhaps firms with negative experiences were more likely to submit surveys during the experimental period. However, firms' response rates do not differ between the baseline and study period. Similarly, perhaps employers in Dauphin—both at the baseline and during the study—were more inclined than employers elsewhere to submit surveys. Again, the response rate between Dauphin (21.8 percent) and control towns (19.0 percent) varies only slightly. Moreover, the consistency of some of the results themselves—see figures below where results, apart from the Dauphin study period results, are quite close—imply that the data may be a good representation of the full population of local business.

The survey inquires into whether or not new hires were made in the past four months, and what starting wages and hours were agreed upon.<sup>85</sup> It also asks whether firms actively sought new workers at some point during the previous four weeks, and about the wage rates and hours on those vacancies. Finally, it asks whether employers had received applications for job openings in the past four months. It is important to note that while the total observation count is 1,447 (292 + 1,155), individual questions are often missing because, for example, many firms made no new hires or had no vacancies and therefore skipped questions inquiring into corresponding wage rates and hours (see below). The N-counts listed in the graphs below refer to the number of firms answering particular questions rather than the total number of valid firm surveys.

This paper uses simple descriptive statistics to understand the business reaction to the guaranteed income. The approach is one of a descriptive difference-in-difference analysis (see Card and Krueger 1994), where the before-after change internal to the treatment group is compared with the before-after change internal to the control.

## **7. Wages, Applications, Hiring, and Work Hours**

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<sup>85</sup> The question phrasing is provided in the next section, with results.

The central findings from this survey are basic descriptive statistics for median hourly wages on new hires and advertised job vacancies. Figure 1 presents the median hourly wages on all advertised job openings according to both Dauphin and non-Dauphin firms. The graph compares a baseline survey from the fall of 1974, before the experimental period, to a study period survey conducted in the first year of the experimental program. While there are no changes between the baseline and study-period survey in the control towns, median hourly wage offers increased by more than 22 percent in Dauphin. In other words, the non-parametric “treatment” effect of the experiment—identified by comparing the difference between the internal change in Dauphin and the internal change in non-Dauphin towns—is a 22 percent increase in wage offers. These numbers do not control for year-to-year inflation. While this does not affect the size of the treatment effect, it is interesting to note that in non-Dauphin towns, inflation-controlled wage offers actually fell during this period, while Dauphin wage offers increased at a slightly slower pace than shown.

<Figure 1>

<Figure 2>

Figure 2 is similar to Figure 1, but instead of reporting median hourly wages on job openings, it reports median hourly wages on all new hires in Dauphin and control firms. Where Dauphin median wage rates increased by more than 18 percent, non-Dauphin towns saw a 12 percent increase. The treatment effect here is more than 6 percentage points. Again, it is worth noting that in the control towns inflation eroded wage gains down to zero, where real wage gains remained in Dauphin.

How to understand these two pieces of evidence? The wages of new hires may appear to be more relevant than wage offers—they represent, after all, the wages that were actually negotiated—but there is reason to think that wage offers are equally interesting. The reason Figure 1—presenting data on the four weeks prior to the survey—may give us a better picture of the experiment is because it represents the most recent circumstances of firms. Figure 2, on the other hand, includes the hires over the prior four months prior to the survey. While Mincome was solidly in place for nine months at the time of the second survey, workers and firms may have not fully adjusted to the new program early on. Indeed, the burst of new entrants joining the program only settles down after the first few months of the program. It is thus important to examine the state of affairs once the new program had time to settle. Figure 1 reports on the most up-to-date business conditions facing employers in the local labor market.

One factor behind the rise in wages, as suggested above, may be changes in applications and new hires by Dauphin businesses. Figure 3 presents survey data inquiring into applications received by businesses in the four months prior to the survey. At the baseline, about 54 percent of Dauphin employers reported that “yes,” they had received applications for job openings. That number falls to 42 percent during the study period. In the controls towns, employer answers reveal virtually no change—at both baseline and study period the number of employers reporting having received applications was about 55 percent. This information helps to paint a picture of local labor market changes. There is good reason to view these adjustments as evidence of a general tightening of the labor market in Dauphin.

<Figure 3>

Fewer applications shown in Figure 3 may then translate into fewer new hires. Figure 4 presents reported new hires in the four months prior to the survey. At the baseline, about 56 percent of Dauphin employers reported that they had hired no new workers during this time. That number grew to 73 percent during the study period. In control towns, employer answers, again, reveal little change. At the baseline about 63 percent of employers reported no new hires. That number was 61 percent during the study period.

Rather than examining whether or not firms had new hires, this data can also be expressed as the mean number of new hires per firm (not shown). At the baseline in Dauphin, firms brought on an average of 1.94 new employees in the previous four months. During the study that number fell to 1.10. By contrast at the baseline non-Dauphin firms hired 1.41 new workers, and that number remained roughly the same, at 1.39, during the study period.

<Figure 4>

One final piece of evidence worth observing is work hours. Here, the top panel of Figure 5 presents the hours per week for new hires. The change is small, but the direction is consistent with the hypotheses above. In all cases, work hours hover just below 40 per week. However, while Dauphin businesses reduced mean hours worked by 2.98 percent from baseline to study period, in the control group, hours worked for new hires increased by 4.53 percent. The bottom panel of Figure 5 shows the hours per week for advertised vacancies. In this case, both Dauphin and the control towns see less than a one percent increase in work hours. By the first measure, there is a small treatment effect, suggesting a fall in work hours; by the second measure, there is no observable effect.<sup>86</sup>

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<sup>86</sup> There is an important caveat to be made about the above graphs. Firm-level data herein includes large unionized firms. This is relevant for two reasons. First, wage and price controls typically held annual

<Figure 5>

## 8. The Business Reaction to Mincome

Together these findings lend support to the second hypothesis stated above suggesting that business power diminished under Mincome. As the labor market tightened, business demand pulled wages up. Indeed, this may be why some Dauphin employers aired concerns about the program. The business survey offered few opportunities for qualitative commentary. However, one Dauphin business owner expressed overt hostility to the program. He used the limited survey space available to criticize the effect of Mincome on wages and articulate his material interest in a vulnerable workforce:

If the government wants to do something about the basic annual income in Manitoba the best thing they can do is get out of the picture and let supply and demand rule and govern what the wages and hours should be. At this rate if one wanted to eat they would have to work. ... [The program is] just spoiling people rotten and upsetting the workforce something unreal. The hours people have to work, the wages they get, and the output they give (which isn't much) just make it impossible for the average employer to even stand a chance at hiring help.<sup>87</sup>

It is unclear how common this perspective was as firms were given little space for detailed commentary. One other business owner made a short but substantive comment. As an employer of a 40-person workforce, he complained that applicants were “unacceptable” because they were “not willing to train at reduced salary.”

Dauphin employers were aware that the program could adversely affect their interests, and some expressed concerns about the program. On at least one occasion, when the program had been in place for nearly three months, the Dauphin Chamber of Commerce invited Mincome's

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nominal wage increases at roughly 10 percent. Second, larger, better paying, and unionized workplaces may be more likely to have a commuting work force: non-Dauphin residents, for example, may come to Dauphin to work. Smaller employers, ones who are unaffected by wage and price controls, will be the most likely to be impacted by changes in the local labor market. For that reason, it would be useful to exclude large unionized workplaces in future analyses.

<sup>87</sup> LAC, Winnipeg, MB. Mincome files. Boxes 69746-69747.

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).

Dauphin’s Chamber of Commerce was not the only business organization to show an interest in the experiment. The Atlantic Chamber of Commerce, a group representing businesses in Canada’s Maritime provinces, wrote to the National Health and Welfare Minister to inquire into the Dauphin experiment when it first came on their radar.<sup>88</sup> The Canadian Chamber also wrote to Health and Welfare to request information on Mincome, “being very interested in developments of this kind.”<sup>89</sup> Likewise, when Mincome was still in its infancy, the Winnipeg Chamber of Commerce and the Manitoba-Saskatchewan division of the Canadian Manufacturers’ Association struck up a mail correspondence about the experiment, planning a joint seminar on the subject and agreeing to investigate the “function and aim of the project.”<sup>90</sup> Had Mincome not died a quiet death, had it published results from firm-level surveys, the business organizations of the day would have surely taken note. Indeed, they would have, in all likelihood, resisted the implementation of a Mincome-style guaranteed income.

## 9. Conclusion

It is often hypothesized that a guaranteed annual income will facilitate a “low road” economy as government top-ups free employers to reduce wages. To the contrary, this paper lends support to the hypothesis that a comprehensive guaranteed income would increase, not decrease, workers’ bargaining power and wages in the labor market. It is best to see the guaranteed annual income as an instrument of worker power, not business power. Moreover, I show that in the Dauphin

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<sup>88</sup> LAC, Ottawa, ON. Mincome Files. Box 3.

<sup>89</sup> LAC, Ottawa, ON. Mincome Files. Box 8.

<sup>90</sup> Archives of Manitoba, Winnipeg, MB., Winnipeg Chamber of Commerce fonds. M-91-3-6-22.

case, a moderately sized, but unitary and universally available, guaranteed annual income pulled wage rates up by a considerable amount.

The historical evidence suggests that this conclusion is contingent on a few crucial policy details. Indeed, the details bearing on negotiation and power in the labor market determine whether the guaranteed income will facilitate or obstruct exploitation in the labor market. When payment is conditional on work, when it is minimal or narrowly targeted, the GAI increases workers' market dependence and facilitates exploitation, but when it is generous, broadly available and delinked from work it obstructs exploitation.

Pierson and Myles argue that the adoption of NIT-like designs in social policy was a "child of retrenchment" (1997: 458). As a matter of history this assertion contains some truth. However, it need not be true in the future. Critical analysis of the guaranteed income sometimes takes the form of a genealogical argument: owing to its conservative roots, the policy is inherently regressive. The politics of social policies such as Mincome should instead be analyzed on the basis of who is helped and who is harmed, and whether exploitation is reined in or entrenched. The guaranteed income comes in all shapes and sizes, and the ramifications of the policy will be highly contingent on the details proposed. Pierson and Myles themselves argue that the reason Southern business was opposed to the earliest version of the FAP was because it raised the reservation wage of labor. On these grounds alone, it is unpersuasive to bundle together, as do Pierson and Myles, very diverse forms of "NIT-type spending" (1997: 458).

The guaranteed income is often depicted as a policy that has the unusual attribute of exciting the interest and support of many different parties.<sup>91</sup> In 1970, one Canadian journalist put it this way: "For the first time, the archetypal conservative waging the never-ending fight against sloth

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<sup>91</sup> For example, a survey conducted by Lee Rainwater on public attitudes toward the FAP seemed to show no difference between conservative and liberal responses (1974: 214).

and his liberal counterpart squeezing the social conscience dry, stand blinking at each other on the same platform” (Brudner 1970: 34). In contrast, on the basis of arguments advanced by three guaranteed income advocates at the 1966 Chamber of Commerce Symposium on the implications of the GAI, one can glean at least three interpretations of the policy.<sup>92</sup> Robert Theobald described a radical policy, James Tobin proposed a liberal one, and Milton Friedman’s policy, with a low guarantee level and a high tax back rate, was distinctly libertarian (1966). Wide-ranging support for the guaranteed income should be expected only when the proposal remains at the level of newspaper headlines.

From the above history of the guaranteed income, it is plain that business—including its more enlightened wings in the U.S. and Canada—would have opposed a nation-wide scheme along the lines of Mincome. This is evident from careful observation of their positions on guaranteed income proposals from the 1970s and 1980s. It is equally clear in the trends revealed by Mincome’s own business survey. Employers will rarely assent to public policies that weaken their dominance over their workforce. Although business has taken different positions on the guaranteed income, this is reducible to the fact that the guaranteed income has manifested in different forms, some of which facilitate exploitation instead of obstructing it.

The guaranteed income garners agreement on the left and the right only in the most superficial sense. Whenever there is this much agreement about an idea one can be sure that there is disagreement about what it means. A social policy that dramatically expands the autonomy and power of working people in capitalist labor markets is an unlikely candidate for business endorsement.

This paper aims to shed light on the likely sources of support for the guaranteed income. In particular, the objective is to elucidate the interests of a key political actor—business

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<sup>92</sup> Archives of Manitoba, Winnipeg, MB; Winnipeg Chamber of Commerce fonds.



organizations—in order to clarify the battle lines in the struggle toward comprehensive and universalistic income-maintenance policies. Exploring the history of GAI proposals, as well as the business experience under experimental conditions, aids in understanding the forces that will help or hamper efforts to build a path to such policies.

A final lesson can be drawn from the history of policy evolution through the political process. As Brian Steensland's (2007) analysis of the Family Assistance Plan demonstrates, after multiple proposal iterations, a policy's metamorphosis can be quite dramatic. Steensland shows that one of the lasting policy legacies of the FAP turned out to be the work-conditional EITC and the categorical Supplemental Security Income. There is no doubt that the success of these programs was historic. They are not, however, the kind of emancipatory and empowering social policy that many supporters of the guaranteed income envision. A relevant concern is that superficial business support for a policy may hinge on the elimination of features that are crucial for many proponents—universalism, unconditionality, generosity. Advocates of these features may discover that it is preferable to rally the sources of support for the policy features themselves, rather than support for a generic policy proposal suffering from overly malleable and mutable definitions.

**Appendix: Archival sources**

Archives of Manitoba, Winnipeg, MB; Winnipeg Chamber of Commerce fonds, Guaranteed Annual Income (Mincome), 1973-1974, M-91-3-6 file 22.

Library and Archives Canada (LAC), Ottawa, ON; RG-29 Accession 1993-94/008 Mincome Files Manitoba 1971-1987, Box 1-11.

Library and Archives Canada (LAC), Ottawa, ON; Guaranteed Annual Income. General files (R5699-67-3-E). Microfilm reels H-698 (1970-1972) and H-725 (1969, 1971).

Library and Archives Canada (LAC), Winnipeg, MB; Department of Health fonds including the former Department of National Health and Welfare fonds, RG-29; and Policy, Planning and Information Branch sous-fonds, branch accession number 2004-01167-X, Operational Files of Manitoba Basic Annual Income Project (Mincome).

Library of Parliament. Ottawa, ON. See: [www.parl.gc.ca/library](http://www.parl.gc.ca/library)

University of Manitoba Archives, Winnipeg, MB; Ed Schreyer fonds, 1950-1988, MSS-145, PC 153, TC-99.

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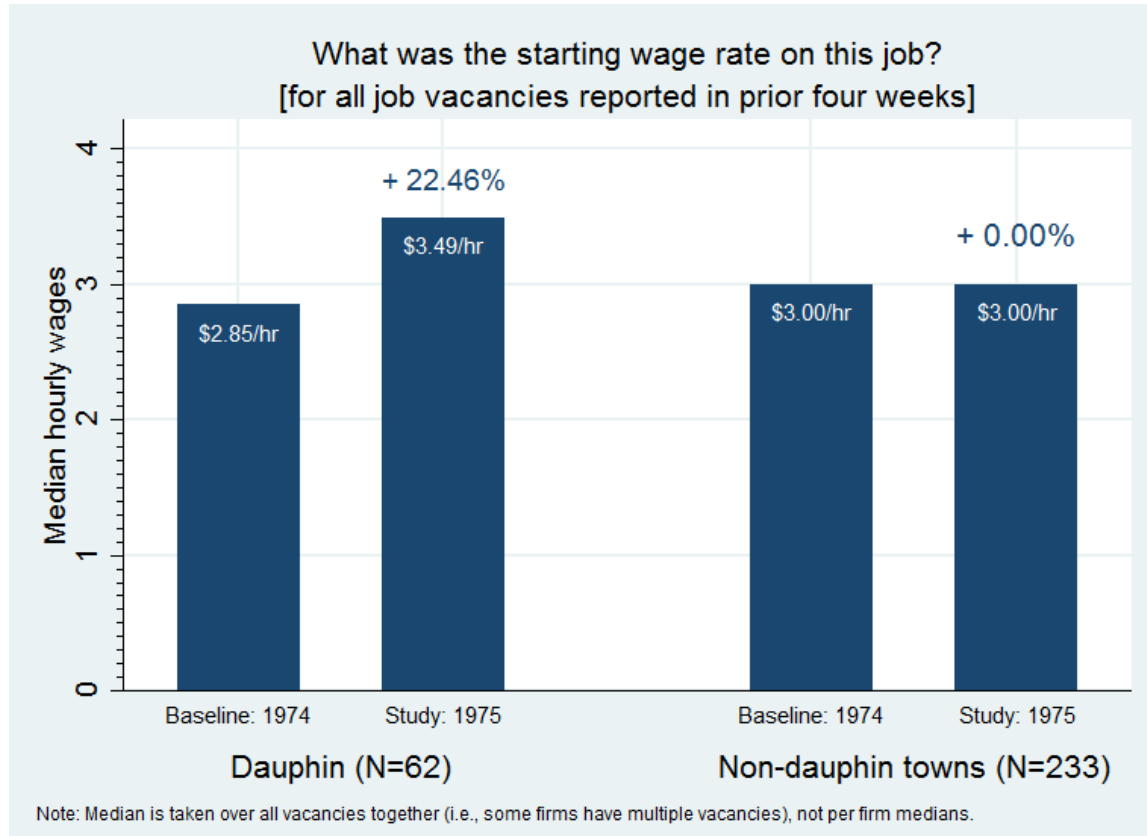
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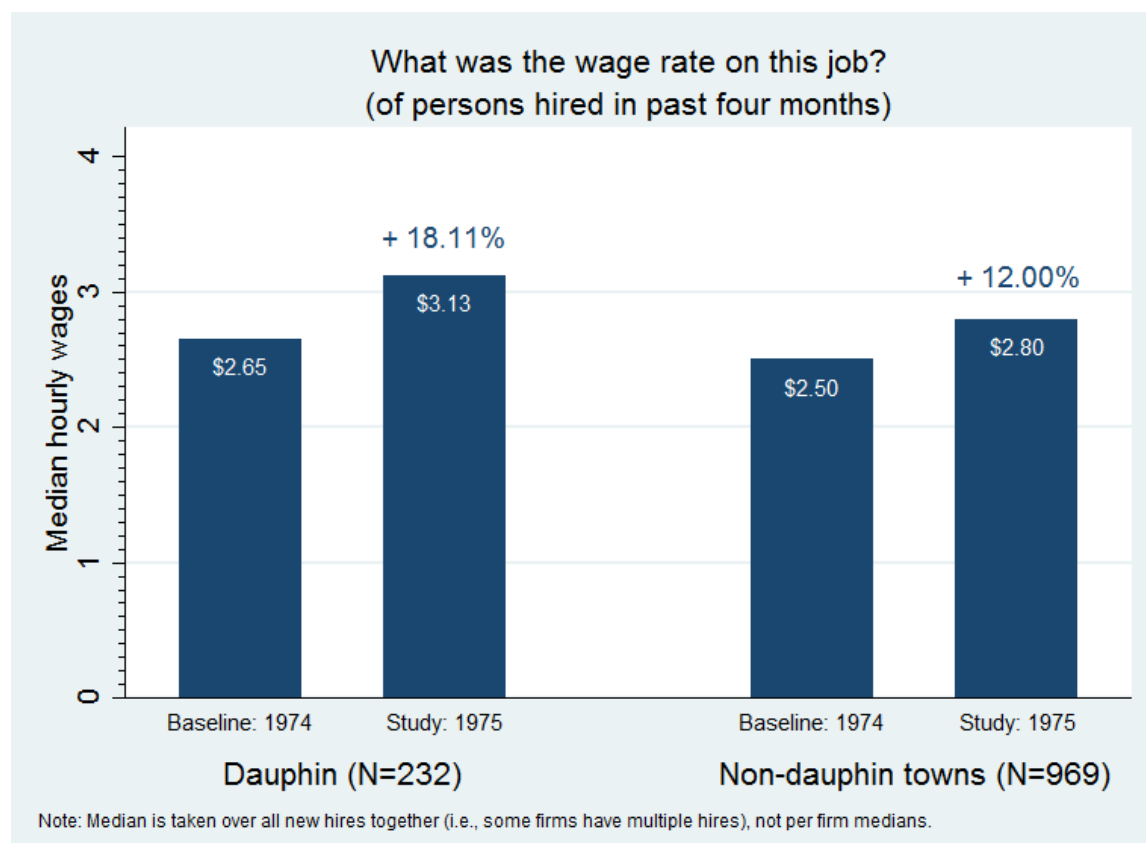
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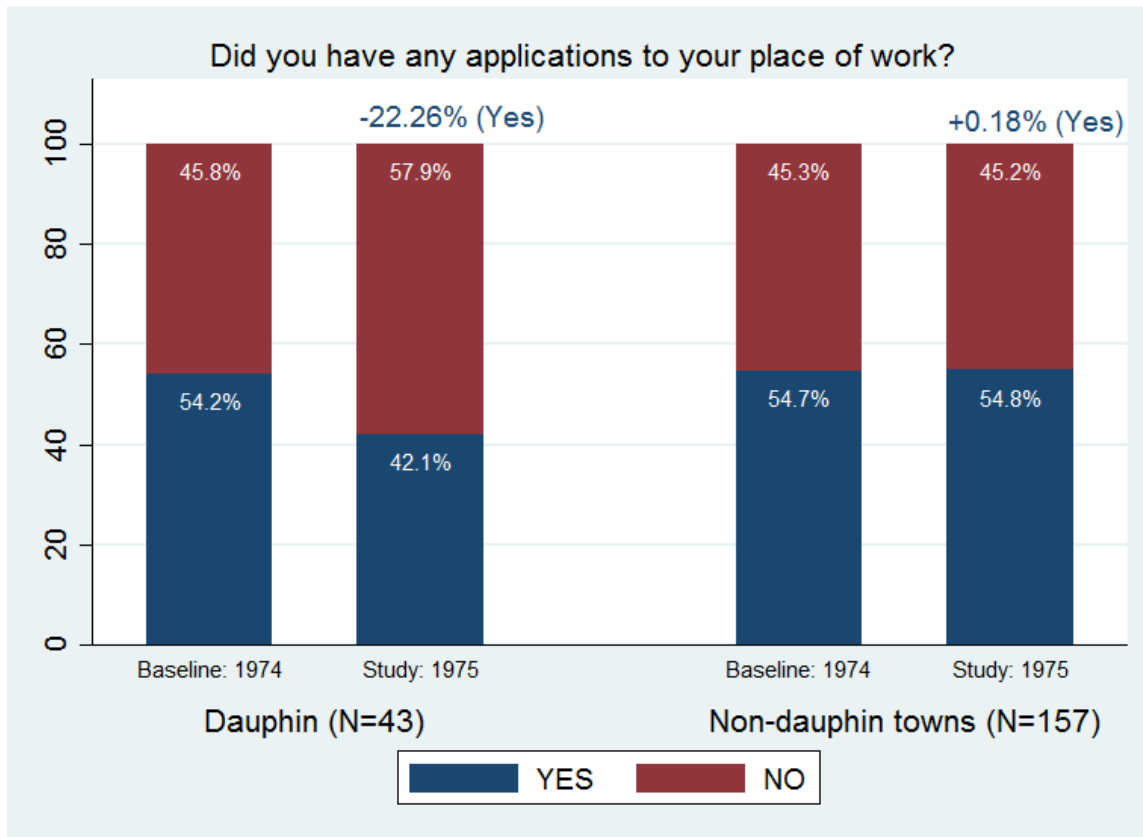
**Figures**

**Figure 1.** Median hourly wages on all job openings in Dauphin and control firms

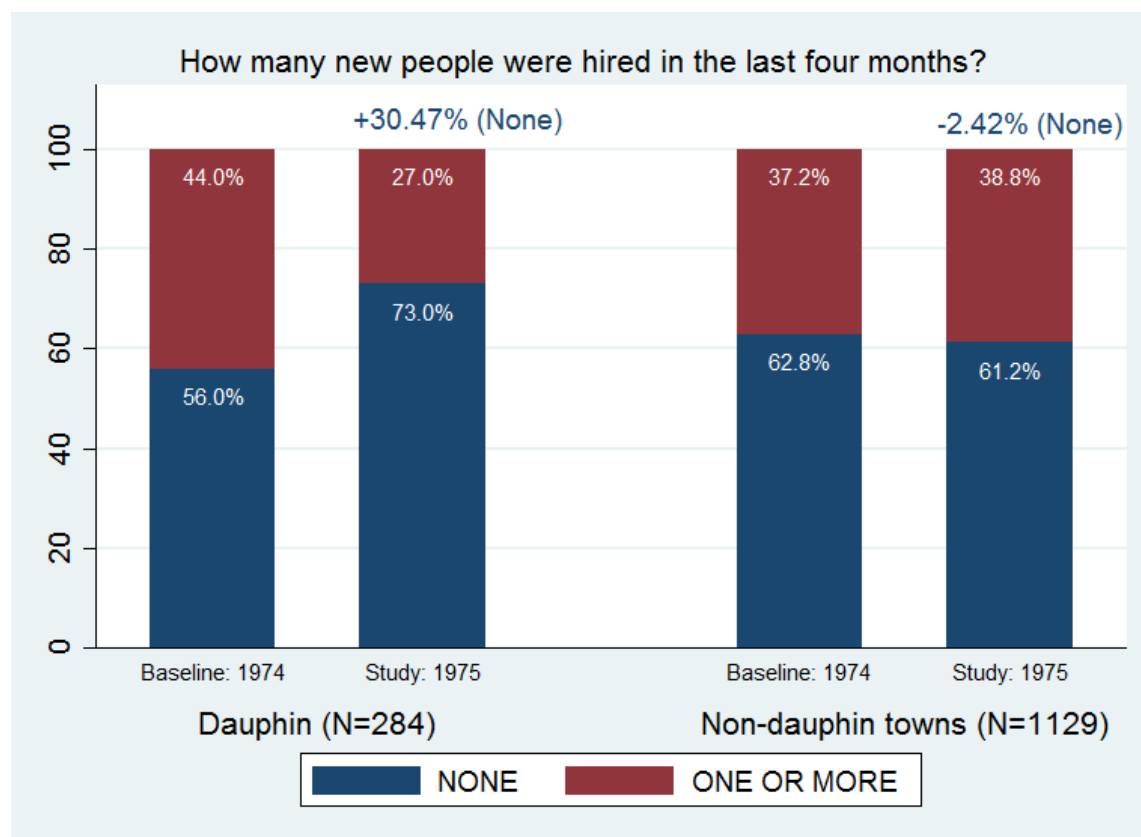




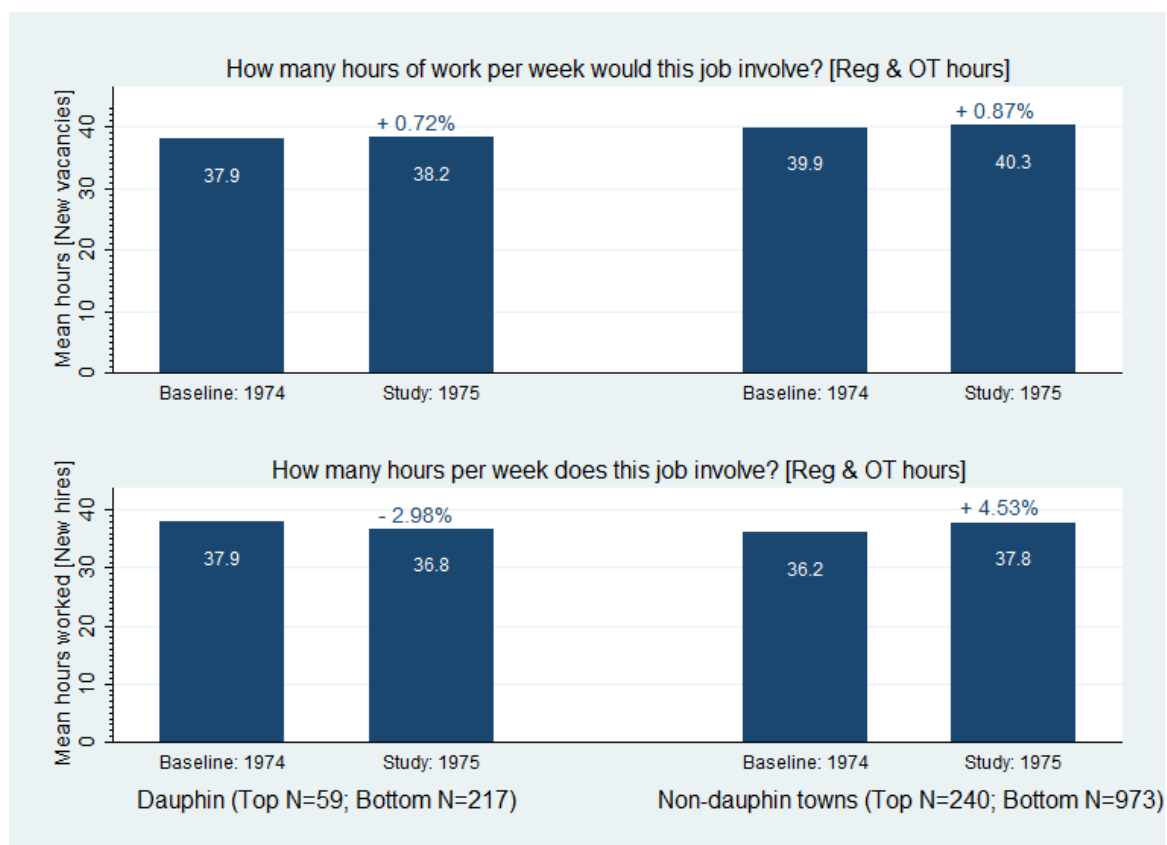
**Figure 2.** Median hourly wages on all new hires in Dauphin and control firms



**Figure 3.** Percent of businesses received job applications in Dauphin and control firms



**Figure 4.** Percent of businesses making new hires in Dauphin and control firms



**Figure 5.** Weekly hours for new hires and vacancies in Dauphin and control firms