

Article

The employer response to the guaranteed annual income

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Abstract

How do firms react when the whole labor force has access to a guaranteed income? One view argues that the guaranteed income is an employer subsidy, facilitating low wages and a ‘low-road’ industrial strategy. The second view suggests that in providing an alternative to work, the guaranteed income tightens labor markets and pulls wages up. This article 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 residents were eligible for unconditional payments. Using an archived survey of local firms that inquires into wage rates, applications, hiring, and work hours, I find support for the second view. I close by examining the mechanisms behind the employer subsidy argument and considering the conditions under which a variety of income-support policies might increase or decrease wages, and more broadly, foster compromise or conflict in the labor market.

Key words: firms, poverty, social policy, low-wage employment, wages

JEL classification: I38, J53, P12

“Necessity never made a good bargain”

—Benjamin Franklin, cited in [Isaacson](#), p. 99

1. Introduction

Much has been written about workers’ behavioral responses to the guaranteed annual income (GAI, or synonymously, ‘basic income’) ([Keeley, 1981](#); [Burtless, 1986](#); [Hum and Simpson, 1993](#); [Widerquist, 2005](#); [Calnitsky and Latner, 2017](#)).¹ The employer response to

1 There are different terms in use for these policies. When referring to the Mincome experiment and the proposals from the 1970s and 1980s, I use the term ‘GAI’, which was commonly used at the time

this transformative policy has, in 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; Haddow, 1993; Steensland, 2007; Block, 2013). This article 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 was conducted 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.

There are two competing theoretical perspectives that seek to explain the impact of the guaranteed income on employers. One position, asserted first in the 1970s and again more recently, characterizes the guaranteed income 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 the option of exiting the labor force 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 et al., 2015).

Firm-level surveys from the Mincome experiment 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 examined here, the guaranteed income led businesses to raise wages from their baseline level: this article finds that wage rates offered on advertised job vacancies and actual wages on new hires grew in Dauphin. In contrast, control firms report no wage growth on advertised job vacancies and slower wage growth on new hires. Moreover, I find that relative to controls, applications to local Dauphin firms and new hiring declined during the experimental period. In short, a guaranteed income that curbed exploitation had adverse effects on the interests of business.

An important problem with the debates over the GAI is that the object under scrutiny is ever-changing. 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

and typically refers to a negative income tax (NIT), where payments phase out as market income rises. Later in the paper, especially in the discussion section, I use the more general umbrella term ‘basic income’, which refers either to a GAI/NIT or to a universal basic income (UBI). There are differences between UBI and GAI/NIT, the most notable of which is the *ex-post/ex-ante* distinction: payments can be made either before or after incomes are assessed. However, from the perspective taken in the article, I believe that when payments are sufficiently high, the two are effectively synonymous, as they both allow for exit from the labor market.

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 North America in the 1970s and 1980s, crucial policy details were often opaque, and the heterogeneous business responses to these proposals ought to be understood in that light. As an ancillary objective, this article attempts to clarify the mechanisms through which the specific design details of differing income support policies will impact on business.

In Sections 2 and 3, I outline competing theoretical perspectives on the impact of the guaranteed income on employers and argue that the concept of exploitation provides a key to deciphering a range of guaranteed income schemes with superficial similarities. When businesses take a favorable position 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' bargaining 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 analyze the impact of the guaranteed income on business and argue that Mincome eroded business dominance in the local labor market by reducing workers' market dependence. Sections 4 and 5 provide background on the Mincome experiment and the survey of firms. In Section 6, I present regression results showing the impact of Mincome on wages and other labor market indicators, and in Section 7, I provide some qualitative evidence on the reaction of businesses to Mincome. Finally, in Section 8, I discuss the conditions under which the 'employer subsidy' hypothesis might in fact hold. More broadly, I consider the policy details that could allow income-support policies to operate as a 'beneficial constraint' (Streeck, 1997) that fosters a positive-sum economy. I conclude, however, that comprehensive and generous basic income policies that facilitate labor market exit are best characterized as a zero-sum game. This conclusion is roughly in line with the theoretical tradition associated with the power-resources approach (Korpi, 1983, 2006), which I interpret as a macro-social reflection of the micro-level focus on exploitation. Thus, income support schemes that make substantial payments, available to all without work requirements, will likely reduce the power of employers over workers and dampen exploitation in the labor market.

In presenting and providing an interpretation of firm-level survey data, this article aims to elucidate the interests of a key political actor in the labor market—business organizations—in order to clarify the contours of support and opposition to broad and universal income-maintenance policies. While business groups might show some support for income-support policies that are 'two-tiered', selective or entail work requirements, it is unlikely that unconditional, generous basic income policies that reduce workers' market dependence will garner 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. Varieties of income support and the exploitation pivot

A heterogeneous group of income-support schemes—including the Earned Income Tax Credit (EITC), the Alaska Permanent Dividend Fund, Canada's Guaranteed Income Supplement, the universal basic income (UBI) and the negative income tax (NIT) at different

support levels and claw-back rates—are frequently lumped together in popular and academic discussion. But behind the superficial agreement on guaranteed income policies lies deep disagreement on what they should do and how they should operate.

Examples of diverse schemes—ones with crucial differences—abound in the history of basic income in the 1970s and 1980s in North America. In the United States, the Family Assistance Plan (FAP)—a GAI proposal that was nearly approved in the US Congress in 1970—itself took on multiple forms over its short lifespan. The first iteration of the policy proposed an NIT that entailed no coercive work requirement, although after debate and controversy, and publicity of the policy details, later variants embraced a workfare component (Moynihan, 1973; Burke and Burke, 1974; Davies, 1996; Steensland, 2007). This section and the discussion below contain references to a number of different income maintenance policies—both generic and concrete proposals and programs—and to keep track of them, Table 1 includes basic details on the main policies discussed.

In Canada, unlike early versions of Nixon's FAP, no major guaranteed income proposal was designed to free workers from work. The 1970 Family Income Security Plan (FISP), comparable in some ways to the FAP, provided an annual payment of less than a quarter of that offered in Nixon's proposal, ensuring no loosening of workers' market dependence (Shifrin, 1977; Leman, 1980). The seven-volume Castonguay–Nepveau proposal of 1970 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), a two-tiered guaranteed income scheme that was proposed alongside the abolition of virtually every existing government income-security measure (Shifrin, 1985; Hum, 1986; Kesselman, 1986). However, compared with the Canadian proposals above, the Mincome scheme was sweeping in its ambition and scope; its design made an exit option from the labor market available to the whole workforce.

In the history of the guaranteed income, the range of actual policies floated in its name have been too diverse to permit the easy formation of a general and meaningful normative opinion on the subject. As such, these policies can be usefully divided into two basic subcategories: those that provide cash alternatives to the labor market and thereby allow for withdrawals from work, and those that provide cash supplements but in one way or another make work withdrawal difficult or impossible. Put differently, 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. Although the term carries with it an inescapably normative valence, it is intended here to be an analytical rather than a normative concept. The classical usage defines exploitation as a relationship in which workers produce more economic value through their laboring effort than they receive in wages (Samuelson, 1971; Elster, 1985; Roemer, 1985, 1988; Marx, 1990). In the Marxian tradition, the difference between these quantities, or the portion appropriated from workers, is considered profit or surplus value. However, the sociology behind the relation of exploitation is perhaps more germane to the context of this article. 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

Table 1. Varieties of income support and their attributes

	Income maintenance policy	Dates	Conditional on work?	Income-tested?	Max benefit at/near 'poverty line'?	Enables work withdrawal?
Generic policies	Universal basic income		No	No	Yes	Yes
	Negative income tax		No	Yes	Yes	Yes
Concrete policies	Earned Income Tax Credit (US; implemented policy)	1975-present	Yes	Yes	No	No
	Family Assistance Plan (US; proposal)	proposed 1969	Early v.: No/Later v.: Yes	Yes	Yes	Early v.: Yes/Later v.: No
	Alaska Permanent Fund (US; implemented policy)	1975-present	No	No	No	No
	Mincome (CAN; experiment)	1974-1977/8	No	Yes	Yes	Yes
	Family Income Security Plan (CAN; proposal)	proposed 1972	No	Yes	No	No
	Universal Income Security Program (CAN; proposal)	proposed 1984	Yes	Yes	No	No

Notes: An "income-tested" program is one that phases out as market income rises; this can be contrasted to a "demogrant" (i.e. UBI, the Alaska fund) which is available to all and fully disconnected from market income. Work conditionality refers to whether or not program benefits are available only to those in the labor market. Finally, Mincome payments in Dauphin ended in December 1977, but in Winnipeg a supplementary sample continued through December 1978.

perspective, depend on the existence of a laboring class, who in turn remain 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. As discussed below, I interpret changing wage levels in my survey data from this viewpoint. 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 survival (Esping-Andersen, 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. In contrast, the near-ubiquitous language in guaranteed income analyses centers on ‘work incentives’ (i.e. Pechman and Timpane, 1975), as if this individual behavioral attribute, drained of its social significance, were a good in itself, and unrelated to the relationships among 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 meager 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). The decommodification of labor, however, reduces the sting of the sack and thereby an employer’s ability to impose his or her will on employees.

In sum, an exploitation-based analysis highlights the micro-level bargaining context and sheds light on the ways in which business maintains an active 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. Moreover, exploitation entails a particular kind of relationship. It is not only that the welfare of the exploiters depends on the deprivations of the exploited—exploitation involves more than a class of people who are dispossessed of productive assets, for example. Exploitation requires that the exploiters themselves *need* the labor of the exploited. Crucially, this is a relationship of ‘inverse interdependent welfare’ (Wright, 1997, p. 10), where the exploiter’s welfare depends causally on the appropriation of the fruits of the labor effort of the exploited. From this perspective, the wellbeing of employers depends on circumstances where workers are dependent on the labor 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 that it does little to facilitate escape from the labor market. In a starker case, Canada’s Guaranteed Income

Supplement (Johnson, 1987) is categorical and available only to the elderly who have already left the workforce. The Earned Income Tax Credit in the US (Sykes et al., 2015), although designed with NIT-type mechanisms, is conditional on work. And two-tiered systems like the Universal Income Security Program noted above provide income 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 often entrench it. Any negative income tax entailing work requirements 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 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 a degree of autonomy, and in providing a decommodified alternative to the market it made work a genuine choice. It is the best case, then, for gauging 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.

3. 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. From this perspective, ‘a guaranteed income will amount to no more than a simple employer subsidization and an incentive for employers to reduce workers’ wages’ (Young and Mulvale, 2009, p. 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 (Lekachman, 1969; Levi, 1970; Droppelt, 1975; Howell, 1997; Young and Mulvale, 2009). The putative lesson 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, as cited in Block and Somers, 2003, p. 284).

Paul Pierson and John Myles (1997) make a similar argument. They point out that in the 1970s Canadian labor worried that the GAI could both subsidize low-wage employers and substitute for a full employment policy. David Howell echoes Pierson and Myles’s conclusions, arguing that ‘the NIT is really a grown-up version of the EITC, ... a broad-based wage subsidy’ (1997, p. 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, p. 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, pp. 536, 538).

The employer subsidy question was not a concern restricted to skeptics. For Manitoba New Democratic Party 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, p. 8)²

At the national level, in a 1973 speech to the Canadian Manufacturers Association, 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, p. 11).

The second view suggests that a comprehensive guaranteed income would give workers more negotiating power in the bilateral bargaining context, thereby *reducing* exploitative relations in labor markets. A genuine threat of 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 workers with ‘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, p. 483). In sum, competition at the bottom end of the labor market would be blunted.

The hypothesis taken in this article is that this second view is correct—in other words, I believe there is good reason to expect labor markets to tighten and wages to rise. The first perspective, suggesting that a sufficiently generous and universal guaranteed income functions along the lines of the work-conditional EITC, rests on a category error. The EITC provides no support for households with zero or insignificant market incomes, it provides no alternatives to work, and it may even increase competition at the bottom of the labor market, driving wages down (see Rothstein, 2010, and further discussion below). 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. Additionally, fewer applications should mean fewer new hires. Finally, not only

2 UMA, 19-6, p. 8. Likewise, in a 1970 speech, Health and Welfare Minister 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’ (pp. 3–4) (LAC-GAI).

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.

4. 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 (Adams, 1971) posed the guaranteed income as the central policy solution of the era, an idea ‘whose time has come’ (Canada, 1971, p. 175). Inspired directly by four similar experiments in the USA, it was hoped that Mincome would demonstrate the feasibility of the guaranteed income to the Canadian public.

As with the US 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 US (Hikel and Harvey, 1973; Atkinson et al., 1973), which restricted analysis to small groups of recipients dispersed across geographies.³ It became clear that isolated individual responses to a randomized guaranteed income experiment could be confounded by macro-effects (Atkinson et al., 1973; Hikel et al., 1974). In particular, the American GAI experiments studied individual labor supply effects as if there were no interactions with business. The implicit, and untenable, assumption was that firm behavior is unaltered in a world of basic income. In 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 enrolment—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.⁴ Dauphinites were offered guaranteed incomes equivalent to \$19 500 for a four-person household.⁵ Families with no labor market income, for whatever reason, could access the full guarantee, which was about 49% of median household income in 1976. At a negative income tax rate of 50%, every dollar of labor market earnings reduced the guarantee by 50 cents; this meant that payments phased out once earnings reached \$39 000, as shown in Figure 1. To put these payment figures in perspective, the real median household income for Dauphin and its rural municipality was only \$24 758, according to the 1971 census.

3 For a recent analysis of Mincome’s effects on health and education, see Forget (2011). For an analysis of the effects on social stigma, see Calnitsky (2016).

4 In a town with a population of 8885, along with a 3165-person rural municipality, roughly 20%—2457 individuals—received benefits at some point throughout the program.

5 These figures are in constant 2014 dollars.

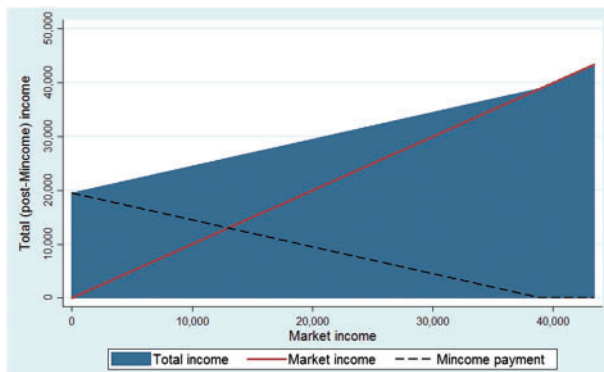


Figure 1. Illustration of impact of Mincome for a family of four.

Note: When market income is 0, Mincome payment and post-Mincome total income equal \$19 500. As market income rises by \$100, Mincome payment falls by \$50 and total income rises by \$50 until the ‘break-even’ point where market income reaches \$39 000 and direct payment ends. Up to this point all positive taxes are rebated; to avoid a ‘notch effect’ where households sudden face positive tax liabilities, rebates are gradually phased out until \$43 400 at which point all Mincome benefits are exhausted.

For many families, poor and non-poor, Mincome payments made an exit from work possible. Indeed, as I note in Section 8, some recipient families took that option (Calnitsky and Latner, 2017). An experiment that makes work optional necessitates an analysis of the program’s 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—can be explored. The next section describes the survey of businesses.

5. Survey data and methods

This article analyzes a hitherto unexamined raw survey of local businesses 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 others conducted by original Mincome researchers because it was administered to non-participant firms rather than the experiment participants. Moreover, it is a repeated cross-sectional study, administered twice, rather than a panel survey of participants.⁶ The original purpose of the survey was to estimate labor demand conditions in Dauphin and in rural control sites.

6 In experiments using ‘saturation sites’, rather than random samples of participants, there is good reason to favor repeated cross-sectional studies over panel studies. This is because saturation sites allow participants to select into the sample—in the Mincome case, specific families decide to join the study—and any panel study results will therefore apply to participants rather than the population. For an analysis of the full experimental effect, a repeated cross-sectional study would appropriately include the impact on participants and non-participants alike. For a discussion of these issues, see Harris (1985).

The survey was sent to all businesses in the Manitoba towns of Dauphin, Carmen, Morris, Portage La Prairie, Stonewall, Minnodosa/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.⁷

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 work and community involvement (Rasmusen et al., 1979). 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% of all surveys were mailed back, valid and non-blank. Despite a low response rate, 19.5% of all local businesses represents a non-trivial amount of information. Is there reason to think there are systematic biases to non-response? While there may be biases in terms 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 were more likely to share survey data in the hopes that their experiences of the experiment were recorded. 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%) and control towns (19.0%) varies only slightly. Moreover, the consistency of some of the results themselves—see the table 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. 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 1447 (292 + 1155), 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 table below therefore differ on the basis of (1) questions applicable only to certain employers based on responses to prior questions (i.e. the 'skip logic'), and (2) missing item-level data.⁸

7 Available at AM, M-91-3-6-22.

8 Rather than restricting analysis to a smaller subset of observations, I use the maximum number of observations for each question. For example, in cases where 'hours' data are missing but 'wage' data are available, I do not delete wage data.

To better understand the survey it is worth laying out the skip logic of the various survey sections. For example, only some employers had vacancies, and only those that did answered vacancy-related questions (and therefore are ultimately included in the observation count for that question). All survey respondents were asked whether they had job vacancies during the reference week of the survey. If they answered in the affirmative they were led to a number of questions about those vacancies, including the hours per week that the job would involve and the starting wage. If employers stated that they did not have vacancies, they skipped the section entirely, and are not included in the population. With respect to the question on applications, the skip logic goes as follows: all employers were asked whether they had job openings during the prior four months. If they answered ‘yes’, they were asked about applications on those jobs. Excluding those who answered ‘no’ on the first question—businesses that had no openings—considerably reduces that respondent population. Finally, all employers were asked about new hires in the prior four months, providing a number of new hires or reporting ‘none’. If none, the respondent skipped the rest of the section; those firms that made new hires were then asked about wage rates and hours on those hires.

This article uses difference-in-difference analysis (see [Card and Krueger, 1994](#)) to understand the business reaction to the guaranteed income, where the before–after change internal to the treatment group is compared with the before–after change internal to the control. Here, the difference within groups over time is the first difference, the difference within time periods between groups is the second, and the baseline–study period difference between groups is the difference-in-difference. 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 standard difference-in-difference regression is as follows:

$$Y_{it} = \beta_0 + \beta_1 (\text{Treatment}_t) + \beta_2 (\text{Study Period}_{it}) + \beta_3 (\text{Treatment}_t \times \text{Study Period}_{it}) + \varepsilon.$$

Considering firm i at survey time t , Treatment_t is a dummy variable indicating Dauphin (1) or control status (0); Study Period_{it} indicates whether the question was asked during the study period (1) or at the baseline (0); Y_{it} is one of the separate outcome variables; and ε is the error term. The interaction term, $\text{Treatment}_t \times \text{Study Period}_{it}$, is the focal variable, where β_3 captures the difference-in-difference, or treatment effect. In cases of mean and proportion comparison, I use OLS regression and in cases of median comparison I use quantile regression.

A final cautionary remark should be added about the methodological difficulties inherent in analyzing business responses to policy initiatives. In particular, there are three general issues worth keeping in mind when assessing the results. The first is retrospective sense-making ([Weick 1995](#)), which can be understood as recreating a story about a relevant outcome based on hindsight. To an extent, questions that inquire especially into events over a longer period, despite their advantages noted above, may be subject to this bias. The second is confirmatory or social desirability bias—telling the researcher what they want to hear—which may be present in business and other surveys ([Roxas and Lindsay 2012](#)). In the case of Mincome, a highly publicized policy initiative ([Calnitsky 2016](#)) closely linked with the New Democratic Party government, a respondent’s political allegiances may have further

Table 2. Regression output for (a) hourly wages on job openings and (b) new hires, proportion of businesses (c) receiving job applications and (d) making new hires, and work hours for (e) vacancies and (f) new hires

	N total	N baseline	N study	1974 Baseline median hourly wages	1975 Study period median hourly wages	Difference-in- difference β 3/(SE)	90% CI
(a) What was the starting wage rate on this job? [for all job vacancies reported in prior week]							
Dauphin	62	32	30	2.94	3.61	0.66*	0.01 to 1.31
Controls	233	113	120	3.00	3.00	(0.39)	
Row N	295	145	150				
(b) What was the wage rate on this job? [of persons hired in the past four months]							
Dauphin	232	105	127	2.65	3.13	0.17	-0.10 to 0.44
Controls	969	386	583	2.50	2.80	(0.16)	
Row N	1201	491	710				
(c) Did you have any applications to your place of work? [Proportion answering 'Yes']							
Dauphin	43	24	19	0.5417	0.4211	-0.1216	-0.4104 to 0.1672
Controls	157	95	62	0.5474	0.5484	(0.1747)	
Row N	200	119	81				
(d) How many new people were hired in the last four months? [Proportion answering 'None']							
Dauphin	284	84	200	0.5595	0.7300	0.1857**	0.0718 to 0.2995
Controls	1129	435	694	0.6276	0.6124	(0.0692)	
Row N	1413	519	894				
(e) How many hours of work per week would this job involve? [Regular and OT hours]							
Dauphin	59	35	24	37.94	38.22	-0.07	-4.52 to 4.37
Controls	240	112	128	39.91	40.26	(2.69)	
Row N	299	147	152				
(f) How many hours per week does this job involve? [Regular and OT hours]							
Dauphin	218	105	113	37.90	36.77	-2.76*	-5.43 to -0.10
Controls	973	393	580	36.19	37.83	(1.62)	
Row N	1191	498	693				

Notes: * ($P < 0.1$), ** ($P < 0.01$). For questions a, b, e and f, all new hires or vacancies are taken together (i.e. some firms have multiple hires/vacancies), not averaged first for each firm. The top two panes report coefficients from quantile regressions (used often for comparing skewed variables such as wage data). The remaining four panes use OLS regression.

impacted their responses. The third includes strategic processes such as deliberate misreporting (Willis 1996)—similar issues were discussed above in terms of non-response. To be explicit, businesses may take action to directly influence the policy's future. Here, employers see themselves as potentially impacted by the policy and report outcomes in a manner designed to generate what they interpret to be unfavorable results.

Although the third issue stands out most strongly, it is hard to rule out any of them. Administrative data would constitute major improvements on all of these validity-related concerns. The fact, however, of before–after comparisons means that insofar as these biases play out during the study period, they operate in much the same way at the baseline and thus may cancel out; in instances where they do not, perhaps in the case of the third concern,

for the bias to operate strongly the respondent needs not only a motive but also some understanding of experimental methods (to rule out a motivated response from the control group) and the timing of the surveys (to rule out a motivated response at the baseline), which in combination may be somewhat rare.

6. Wages, applications, hiring and work hours

Table 2 presents the data from the baseline survey conducted in the fall of 1974, before the experimental period, as well as the study period survey conducted in the first year of the experimental program. In addition to levels from the baseline and study periods I present difference-in-difference results (β_3) for each question item. All items in Table 2 are produced from regression coefficients. Coefficients in the difference-in-difference model can be read as follows: the baseline control level is captured by β_0 ; the study-period control level is captured by β_0 plus β_2 , the coefficient on the study-period variable; the baseline treatment level is captured by β_0 plus β_1 , the coefficient on the treatment variable; and the study-period treatment level is captured by the sum of β_0 , β_1 , β_2 and β_3 , the difference-in-difference coefficient.

The first two panes of Table 2 report baseline and study-period medians as well as difference-in-difference coefficients for hourly wages on new hires and job vacancies. Pane 2a presents the median hourly wages on all advertised job openings in both Dauphin and control firms. There are no changes between baseline and study period in the control firms, and a large increase between baseline and study-period surveys in Dauphin firms, with the treatment effect of the experiment, or difference-in-difference, at 66 cents—relative to the baseline control, this effect corresponds to an increase of 21.5%. These numbers do not control for year-to-year inflation—though this does not impact the size of the treatment effect, it is interesting to note that in the control towns, inflation-controlled wage offers actually declined during this period, while Dauphin wage offers increased at a slightly slower pace than shown.

Pane 2b is similar to 2a, but instead of reporting regression output for hourly wages on job openings, it reports hourly wages on all new hires in both Dauphin and control firms. In this case, Dauphin median wage rates increased by more than the increase in control towns, with a difference-in-difference effect of 17 cents—here, the treatment effect of the experiment corresponds to an increase of 6.8% relative to the baseline control. Again, it is worth noting that in the control towns inflation eroded wage gains down to zero, whereas 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—after all, they represent wages that were actually negotiated—but there is reason to think that wage offers are equally interesting. The reason 2a—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. The question from 2b, on the other hand, inquires into hires over the 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 not have fully adjusted to the new program early on. Indeed, the burst of new entrants only subsided 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. Pane 2a 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. Pane 2c presents survey data inquiring into

applications received by businesses in the four months prior to the survey. At the baseline, about 54% of Dauphin employers reported that 'yes', they had received applications for job openings. That number falls to 42% during the study period. In the control towns, employer answers see virtually no change—at both baseline and study period the number of employers reporting having received applications was about 55%. The treatment effect of the experiment, or difference-in-difference, represents a decline of 12.2 percentage points. This information helps to paint a picture of a tightening labor market in Dauphin.

Fewer applications shown in pane 2c may then translate into fewer new hires. Pane 2d presents reported new hires in the four months prior to the survey. At the baseline, about 56% of Dauphin employers reported that they had hired no new workers during this time. That number grew to 73% during the study period. In control towns, employer answers, again, reflect little change. At the baseline about 63% of employers reported no new hires; that number fell by about one and a half percentage points, to 61%. Together, the difference-in-difference or treatment effect shows an increase of 18.6 percentage points.

One final piece of evidence to examine is work hours. Panes 2e and 2f present the hours per week for new hires and vacancies, respectively. The changes are consistent with the hypotheses above. In all cases, work hours hover just below 40 per week. In pane 2e, both Dauphin and control towns saw an increase in work hours of less than 1%—the difference-in-difference here is a decline of 0.07 hours. In 2f, Dauphin businesses reduced mean hours worked by 1.13 hours from baseline to study period, whereas in the control group hours worked for new hires increased by 1.64 hours. All together, the difference-in-difference shows a decline of 2.76 hours. By the second measure, the treatment effect suggests a relative fall in work hours; by the first, the negative effect is barely observable.

It is worth making note of the statistical significance of the results discussed above. [Table 2](#) presents the difference-in-difference coefficients alongside 90% confidence intervals. The difference-in-difference coefficients are statistically significant in 2a, 2d and 2f; where 2d is significant at the 0.01 level, the other two are narrowly significant at the 0.1 level. In a good number of cases in [Table 2](#), the possibility of significance is limited given the small samples; indeed, cases demonstrating statistical significance have effect-sizes or sample sizes that are comparatively large.

7. 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, employers were compelled to offer higher wages. Indeed, this may be why some Dauphin employers aired concerns about the program. The business survey offered few opportunities for qualitative commentary, though 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.

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 publically expressed concerns about the program. On at least 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). 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.⁹ The Canadian Chamber also wrote to Health and Welfare to request information on Mincome, ‘being very interested in developments of this kind’.¹⁰ 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’.¹¹ Had Mincome not died a quiet death, had it published results from the 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.

8. Discussion

The data herein suggest that a generous guaranteed income forced wages up and reduced applications to Dauphin firms. It is reasonable to conclude from the evidence above that Mincome was decidedly not in the material interests of local employers. However, this evidence could coincide with employer interests if there was additional evidence that the local economy grew faster than wages. More broadly, can we imagine a somewhat differently designed income supplement that might clearly benefit business? Is there a basic income variant that operates as a kind of ‘beneficial constraint’ (Streeck, 1997, 2004; see also Tsakalotos, 2004; Wright, 2000, 2004b) on employers, one that might be costly but functions as a source of stability and a problem-solver at the macro-institutional or micro-firm level? I consider these matters below, and begin with the positions employer groups actually took on the GAI schemes proposed during the 1970s and 1980s.

In the history of guaranteed income proposals, business groups have taken a diverse set of positions that reflects a diversity of policy designs. Daniel Patrick Moynihan’s (1973) history of the Family Assistance Plan (FAP) 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, p. 287). The US Chamber of Commerce was hysterical in their opposition to the FAP, but the Committee for Economic Development (CED) and the National Association of

9 LAC-0, Box 3.

10 LAC-0, Box 8.

11 AM, M-91-3-6-22.

Manufacturers (NAM) were supportive of a version of the program as long as it contained work requirements and set the allowance at a 'realistic minimum' (Congress, 1970, p. 1928). Behind the outward semblance of disagreement among business organizations was a misunderstanding of what the FAP actually entailed. The Chamber opposed the FAP because it contained no work requirement; on the other hand, the CED and the NAM supported it, citing their endorsement of its work requirement (ibid.; CED, 1970). What unified the three was an unambiguous and principled opposition to a variant of the guaranteed income that made work optional.

Likewise, throughout the 1970s and 1980s, business lobbies in Canada were sometimes amenable to a version of the guaranteed income, even if they remained solidly opposed to comprehensive policies that diminished the market dependence of workers. For example, the Canadian Chamber of Commerce, emboldened by [Milton Friedman's endorsement \(1962\)](#), took a moderately favorable position on one version of the guaranteed income, as long as the supplement maintained a 'sensible, selective approach to social security' (1970, p. 18).

While no major business group has ever supported an unconditional and generous basic income, history reveals their receptiveness to certain variants of the scheme. Although programs of this sort may entail substantial tax increases, they might also solve problems for business by (1) easing a burdensome wage bill, or (2) producing stable consumption growth. I explore each of these sources of potential employer benefit in turn.

In considering the first problem-solving channel, it is worth returning to the employer subsidy perspective to examine the mechanisms through which the wage bill might be reduced. Various income support policies are often described as 'employer subsidies', but the underlying causal mechanisms that actually allow employers to lower their operating costs are seldom made explicit. I believe there are two interpretations of the theory, each resting on an implicit model of the labor market, one of which has its roots in classical political economy and the other in neoclassical economics. Stating the two theories explicitly will help clarify how they might function under different income-support designs and whether they are in fact persuasive.

The first theory of income-support-as-employer-subsidy entails a functionalist view of the labor market where workers as a matter of course receive some predetermined level of income at an amount that ensures their social reproduction. Lurking in the background of this model of the labor market is the idea of the subsistence wage (see [Roemer, 1981](#), ch. 7; [Bharadwaj, 2008](#)). The theory makes an implicit assumption that *any* government supplement that makes workers better able to survive will allow employers to reduce their contribution to that subsistence. For example, in David Howell's description, the Family Assistance Plan 'recreates Speenhamland', whereby employers could 'lower the wage below the subsistence level' (1997, p. 534; see equivalent comments in [Young and Mulvale, 2009](#), p. 23).

There are three points to consider. First, if this theory is true it applies equally to an unconditional basic income, the EITC, public healthcare, or any state-funded benefit or service that contributes in some way to the bundle goods that make up subsistence. Second, the past two hundred years of wage dynamics make the subsistence wage idea both unpersuasive and difficult to test.¹² Third, the theory is sorely lacking in

12 There are actually multiple versions of the subsistence wage theory. In one, workers are paid their physiological subsistence and no more. Whether the wage burden is incurred by the state, landowners, or capitalists, and no matter how much the subsistence bundle costs, the workers in the end will be paid exactly their subsistence: 'he will get dry bread in one case and dry bread in the

micro-foundations.¹³ The micro-foundations emphasized in this article are explicit: a generous and work-unconditional basic income provides a direct exit option to workers, thereby increasing their bargaining power when negotiating with bosses. In the case of the argument here, even if firms would like to impose lower wages once they notice that some portion their workers' subsistence has been covered by an EITC-type policy, there is no clear mechanism that would give them the power to realize their wishes.

However, income-maintenance policies may still operate as 'employer subsidies'. The second income-support-as-employer-subsidy theory has clearly identifiable micro-foundations. In this view, a program like the EITC lowers wages through a simple neoclassical mechanism: as a work-conditional program, it increases the labor supply (Rothstein 2010; Nichols and Rothstein 2015). Lower wages are a by-product of greater competition, and they obtain whether the firms are aware of the policy or not. In this case, the term 'employer subsidy' is a shorthand for an unintended consequence that lowers wages.

If the first theory is false and the second is true, policies along the lines of the EITC that provide financial support to the working poor while simultaneously increasing the labor supply will benefit firms through reduced wages, but generous and comprehensive basic income schemes that allow for labor market exit should reduce the labor supply and pull wages up. Although it operates 'behind the backs' of employers and recipients, with respect to Mincome this theory makes predictions that are identical to those derived from the bilateral bargaining framework discussed above. Extending the logic of the theory, any income support scheme that neither increases nor decreases the labor supply—such as the Alaska dividend, perhaps—will have no obvious impact on wages.¹⁴

An adjacent but separate aspect of basic income as a potential problem-solver for employers concerns the policy's role as a 'beneficial constraint' that provides a consumer base with greater purchasing power. It is possible that income supplements—of any variety, generous or miserly, conditional or unconditional—will improve the purchasing power of poor and working people. In particular, businesses may benefit from a downward distribution of income towards people with a higher-than-average marginal propensity to consume.¹⁵ This mechanism, if true, construes basic income as a policy tool that helps foster the conditions for a stable and non-zero-sum economy.

It also provides an alternative interpretation of the wage data above. If aggregate demand increased during the Mincome years, perhaps overall town-level growth and employment opportunities increased and labor markets tightened, even without any change in the supply of labor. While it is impossible to rule out this interpretation, it is worth making two points.

First, separate data show that labor-force participation did in fact decline in Dauphin; the treatment effect of the experiment was an 11 percentage point fall in labor-force

other' (Alexander Baring cited in Heilbroner, 2011, p. 81). In Marx's version, there is an 'historical and moral element' (1990, p. 275) that enters into the subsistence wage. While this may be true, as a theory it becomes much harder to evaluate.

- 13 There is, however, a population-level mechanism underlying one subsistence wage theory: wages hover around subsistence because when they move too high (or low), population growth (or decline) and more (or less) competition bring wages back in line with subsistence. However interesting, this mechanism does not operate in wealthy economies.
- 14 If, however, the Alaska Fund increases the reservation wage, both the neoclassical view and the bargaining view would predict wage increases.
- 15 For a defense of 'underconsumptionism', see Sherman (2014); for a critique, see Kliman (2012).

participation (Calnitsky and Latner, 2017). This is consistent with wage increases resulting from changes in bargaining power, as well as the diminished labor market competition described above. Moreover, it is roughly consistent with other negative income tax experiments showing, to various degrees, a fall in labor supply (Keeley, 1981; Burtless, 1986; Hum and Simpson, 1993; Widerquist, 2005).¹⁶ Second, it is worth noting two cases operating in the Dauphin experiment where purchasing power did not increase. A non-trivial portion of Mincome recipients were either former welfare recipients or non-poor workers who reduced their labor hours. In the first case, purchasing power would have increased by very little, if at all; in the second case, purchasing power might have actually fallen as people reduced their market earnings. That is, in a good number of cases the *source* rather than the *quantity* of purchasing power changed. Nonetheless, aggregate demand in Dauphin may have increased overall, and it is reasonable to suspect that this mechanism may account for some portion of the observed changes.

A final point can be made about the prospects of basic income operating as a beneficial constraint. The most straightforward interpretation of the evidence above is that on balance a generous basic income ought to be understood as a zero-sum rather than a positive-sum game. Mincome was unlike public health insurance, social security, or unemployment insurance schemes that under some circumstances helped to ease burdensome employer expenses or relieve the costs of employer-based welfare schemes (Gordon, 1991; Woolhandler et al., 2003). A generous basic income, one that provides a genuine exit from the formal labor market, means that people will sometimes exercise that option, and firms depending on a steady supply of low-wage labor will have to find new strategies to survive. One happy possibility is that firms, hoping to attract and retain workers in a basic income world, would eventually move toward strategies that entail far deeper forms of cooperation with workers concerning the regulation of work and production (Wright, 2004a). One can speculate further that this symbiotic configuration is more plausible in economies that already have either the institutional capacity for labor market coordination (Thelen and Kume, 2006; Thelen, 2012) or worker organizations that are powerful enough vis-à-vis employers to enforce cooperation and punish defection (Bowman, 1985a, 1985b, 1989).

Nonetheless, in accord with a range of perspectives within the power-resource tradition (Korpi 1983, 2006; see also Emmenegger and Marx's 2011 'conflict-oriented' description of job security regulations in Germany), this analysis suggests that employers will oppose generous basic income schemes even if they manage to develop survival strategies at some less-than-optimal equilibrium. Whether or not there is an ultimate convergence on a positive-sum basic income game (see Carling, 1991, ch. 17), the short-to-medium-term impact of a large fall in labor market participation induced by a generous basic income is unlikely to improve the smooth functioning of capitalist economies, even if it does improve the justice of their income distributions.

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 article lends

¹⁶ See also Greenberg (1983), who uses experimental behavioral response data in microsimulation models with national data and finds little potential for demand to offset labor supply reductions.

support to the hypothesis that a comprehensive guaranteed income would increase, not decrease, workers' bargaining power and wages in the labor market. I show that in the Dauphin case, a moderately sized, but unitary and universally available, guaranteed income pulled wage rates up by a considerable amount. It is best to see the policy as an instrument of worker power, not business power.

There is reason to believe, however, 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 basic income will facilitate or obstruct exploitation. When payment is conditional on work or narrowly targeted, basic income will increase workers' market dependence and facilitate exploitation, but when it is generous, broadly available, and crucially, delinked from work, it will obstruct exploitation. Likewise, a common worry is that basic income schemes will be designed primarily as a substitute for existing welfare state functions, rather than a complement to them. If, in that case, a basic income replaces in-kind programs with cash but does not effectively deliver greater real freedom to poor and working people, there is no reason to forecast the wage effects found in this article.

From the trends revealed by Mincome's business survey it is reasonable to conclude that business would oppose a nation-wide scheme along the lines of Mincome. There are, of course, reasons to be cautious in generalizing from the Mincome case to the national economy. One limitation of this experiment is the funding scheme, which implicitly assumes an external funding mechanism that will be internalized in any national implementation. In a future basic income world, the employer response as well as aggregate demand effects will to some extent depend on where in the income distribution the increasing tax burden falls, and the variety of possible funding strategies entails a good deal of contingency. Another issue to note is that it is possible that Mincome's observed wage effects in Dauphin may differ from what we might have observed in Dauphin under the background condition of a fully implemented nation-wide version: while wages are impacted by changing local conditions, they will also be impacted by changing national conditions, and the town-level design of the Dauphin experiment excludes potential disinvestment from firms in non-local but related industries. Finally, there is little doubt that it is inherently difficult to extrapolate from a semi-rural town in the 1970s to contemporary urban labor markets. For example, the portion of low-wage workers impacted in the Mincome case may differ from the portion impacted today. Though this may be the case, it is worth noting that the precarious structure of the contemporary labor market finds some parallels in the precarity of Dauphin's seasonal labor market. While these concerns make it complicated to extrapolate Dauphin findings to the national-scale today, especially in terms of the precise effect sizes reported, they do not provide reason enough to question the basic directionality of the reported effects.

The underlying fact of business opposition is likely therefore to generalize: employers will rarely assent to public policies that weaken their dominance over their workforce. Although business has historically taken different positions on the guaranteed income, this may be reducible to the fact that the policy has manifested itself in different forms, some of which facilitate exploitation instead of obstructing it. Likewise, 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 article aims to shed light on the likely sources of support for the guaranteed income. In particular, elucidating the potential consequences facing a key political actor—business organizations—may help to clarify the battle lines in the struggle toward comprehensive and universalistic income-maintenance policies. Exploring business experiences 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 basic income supporters 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—universality, unconditionality, generosity. Advocates of these features may discover that it is preferable to rally support for the policy features themselves, rather than for a generic policy proposal suffering from overly malleable and mutable definitions.

Acknowledgements

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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. Abbreviated as AM.

Library and Archives Canada, Ottawa, ON; RG-29 Accession 1993–1994/008 Mincome Files Manitoba 1971–1987, Box 1-11. Abbreviated as LAC-O.

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

Library and Archives Canada, Winnipeg, MB; Department of Health 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). Abbreviated as LAC-W.

Library of Parliament. Ottawa, ON. See: www.parl.gc.ca/library. Abbreviated as LAP.

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

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