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## Exit, Voice, and Loyalty in the Family


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## Exit, Voice, and Loyalty in the Family

### Abstract

This paper investigates how a basic income could transform families and gender power relations within them. We draw on Hirschman's exit, voice, and loyalty framework to argue that a basic income can offer a structural foundation for a radical shift towards more equitable family relations. This is because a basic income can support couples through economic uncertainty and reduce women's structural vulnerability to economic dependency within marriages that strips them of exit and voice. We build our case on novel data from an understudied social experiment from the late 1970s called the Manitoba Basic Income Experiment, or Mincome. Using difference-in-difference regression with individual fixed-effects, we analyze three types of family outcomes: separation, bargaining power, and marital conflict. We find that during Mincome unhappy couples became more likely to consider separation, but that separation overall did not increase. We also find that Mincome reduced marital conflict associated with financial stressors and that some measures of wives' bargaining power increased. Taken together, our results speak in favor of the view that a basic income has the potential to foster more equitable family lives.

### Keywords

basic income, family, gender, bargaining power, divorce

### Disciplines

Demography, Population, and Ecology | Family, Life Course, and Society | Gender and Sexuality | Inequality and Stratification | Social and Behavioral Sciences | Sociology | Work, Economy and Organizations

### Comments

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## **EXIT, VOICE, AND LOYALTY IN THE FAMILY**

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## **EXIT, VOICE, AND LOYALTY IN THE FAMILY: FINDINGS FROM A BASIC INCOME EXPERIMENT**

This paper investigates how a basic income could transform families and gender power relations within them. We draw on Hirschman’s exit, voice, and loyalty framework to argue that a basic income can offer a structural foundation for a radical shift towards more equitable family relations. This is because a basic income can support couples through economic uncertainty and reduce women’s structural vulnerability to economic dependency within marriages that strips them of *exit* and *voice*. We build our case on novel data from an understudied social experiment from the late 1970s called the Manitoba Basic Income Experiment, or Mincome. Using difference-in-difference regression with individual fixed-effects, we analyze three types of family outcomes: separation, bargaining power, and marital conflict. We find that during Mincome unhappy couples became more likely to *consider* separation, but that separation overall did not increase. We also find that Mincome reduced marital conflict associated with financial stressors and that some measures of wives’ bargaining power increased. Taken together, our results speak in favor of the view that a basic income has the potential to foster more equitable family lives.

### **INTRODUCTION**

How would a basic income impact families and the gendered power dynamics within them?

Would more couples split up were they not so economically dependent on one another? Would gender power asymmetries within couples be redressed if both partners had the means to leave the relationship? Would conflict inside relationships diminish if economic security improved?

The basic income—or, guaranteed annual income (GAI)<sup>1</sup>—experiments of the 1970s (see Munnell 1986; Lewis et al. 2005) provide an opportunity to explore these questions. These experiments offered an income allowance to families that was unconditional on work and guaranteed a decent standard of living. In doing so, the guaranteed income experiments shifted the economic conditions within which individuals made decisions about their family lives. The question of how basic income impacts family life is a timely one; basic income is increasingly debated in public venues but there is little research available to policy-makers. We return to these

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<sup>1</sup> Guaranteed Annual Income (GAI) and Universal Basic Income (UBI) are terms often used to denote a policy design difference: a GAI usually refers to a negative income tax, as was the case with Mincome, and a UBI usually refers to a demogrant. Basic income is the umbrella term that encompasses both types of policies. In the 1970s and 1980s “guaranteed income” was most often used, and today, “basic income” is more common. We use GAI to signal the distinctive design applied in the Mincome case and use “basic income” as the broader category.

extraordinarily ambitious but understudied experiments and pose questions about family dynamics and the gender power inequalities within them in a fresh light. Despite changes in family and economic structures since the 1970s, we argue that lessons from these experiments remain important for contemporary cases, in particular as economic uncertainty continues to play a major role in family life, and income inequality within couples remains large, especially after childbirth (Musick et al. 2020). In this article, we offer a novel framework and hitherto-unused empirical evidence to conceptualize the various pathways through which a GAI could shape the future of family life.

The academic literature on the GAI experiments of the 1970s was primarily focused on labor market consequences (i.e., will the GAI reduce the labor supply?) and only secondarily concerned with family dynamics; but it was the latter that triggered intense public debate and sealed the fate of the guaranteed income in North America. The key question that animated the American debate was whether the policy 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. 1980; Hannan et al. 1977; Tuma and Hannan 1990), while others, using the same data, disputed these findings (Cain 1986; Cain and Wissoker 1990a, 1990b). Though the results were inconclusive, the first set of findings received a louder public hearing and was arguably (see Steiner 1981; Greenberg et al. 2003) an important reason why the movement to implement a guaranteed income policy in the United States stalled. 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), leading 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, 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.” Both the academic literature and the subsequent popular debate framed the GAI in an exceedingly narrow fashion, focusing on the “calamitous” outcome that some marriages might break up.

This article shifts the debate away from the narrow focus on how the GAI could “undermine the family,” and instead explores the possibility that the GAI might offer a structural foundation for a shift towards more equitable family relations. The old debate 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 some studies found—then perhaps it was because the guaranteed income ameliorated underlying stressors. The framing of divorce as calamity prevented scholars from examining how the GAI could reshape family relations more broadly - how it could affect not only individuals’ decisions to stay in marriages - but also how they feel and relate to one another inside them. Our article draws on feminist arguments that see basic income as a way to address gender inequalities. But the desirability of a UBI remains contentious among feminists; some support it (Baker 2008 McKay 2005; Parker 1993; Sherwin and Piven 2019; Weeks 2011; Zelleke 2008; 2011) but others are critical (Bergmann 2004; Gheaus 2008; O’Reilly 2008; Robeyns 2001).<sup>2</sup> Our study tackles questions about how a basic income can shape conflict in family life, women’s bargaining power within the family, and their ability to opt out of

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<sup>2</sup> Feminists remain divided on the desirability of UBI-type policies and their effectiveness at promoting gender equality and justice. On one side of the debate are scholars who argue that a UBI could reduce societal biases that devalue care work (McKay 2001, 2005; Parker 1993), facilitate the de-gendering of care work and sharing of paid and unpaid work (Elgarte 2008, Zelleke 2008), and provide a means to economic independence beyond wage work (Zelleke 2011; Weeks 2011). On the other side of the debate are scholars who argue that a UBI would exacerbate the gender division of labor and reverse progress on gender equality in the labor market and in the public sphere more broadly (Robeyns, 2001; Gheaus 2008). These scholars also argue that a UBI is less effective than, and potentially incompatible with, other policies based on universal services (O’Reilly 2008; Bergmann 2004). Our paper does not aim to weigh in directly on this debate, which remains mostly speculative. We instead attempt to answer calls for more empirically based research (Robeyns 2008) to inform the political discussion.

traditional family structures, but it does not address gender inequality concerning paid and unpaid work.

Our framework borrows Hirschman's (1970) concepts of *exit*, *voice*, and *loyalty*—designed originally to analyze the firm—to work out how the GAI can shift the equilibrium of family relations.<sup>3</sup> Existing literature largely focuses on three key mechanisms when examining the relationship between income and family relations: the independence, bargaining and income effects. The independence effect says that income allows individuals to end relationships; the bargaining effect posits that income gives individuals more bargaining power within a relationship; and the income effect asserts that income reduces financial stressors. The standard language from economics tends to see each as a separate effect and research often focuses on whether one effect dominates the others (i.e., Bitler et al. 2004; Cain and Wissoker 1990a; Groeneveld et al. 1980). In contrast, Hirschman's framework offers a template to think about the independence, bargaining, and income effects as part of an interrelated system that together shape the power dynamics and constraints that characterize different equilibria. The first two concepts—*exit* and *voice*—refer to options available to individuals who are unsatisfied with their relationship status quo; when people are unhappy in a relationship, they can either *exit* it or *voice* concerns about it. These two concepts are analogous to the *independence* and *bargaining* effects. The third concept—*loyalty*—refers to remaining in a relationship despite dissatisfactions. We use *loyalty* to broadly conceptualize the characteristics and conditions of those who remain in relationships, and we use *income* effects to incorporate the prediction that economic means improve marital relationships. In Hirschman's framework each of these components shapes each other and the whole system: the possibility of *exit* is necessary for *voice* to have volume (i.e., serious complaints about a relationship will be harder to make if one is unable to end it), *loyalty* tends to encourage *voice* rather than *exit* (i.e., someone in a relationship will tend to talk through issues before separating and this might improve the relationship when successful). Thus, both

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<sup>3</sup> Our adaptation of Hirschman's concepts to family relations follows in part England and Kilbourne (1990) who used this framework to offer a theory about why husbands' higher earnings turned into higher bargaining power.

*exit* and *voice* are necessary to lower the costs of *loyalty*, or the level of dissatisfaction among those who remain in relationships. A change in structural constraints on *exit* and/or *voice* result in changes in the overall outputs of the system. When this theory is applied to the family, the structural changes in family economics can shift the constraints on romantic relationships. For instance, in a context where divorce is illegal or infeasible posing a structural constraint on *exit*, one's *voice* to protest within a relationship is weaker. This could result in an equilibrium of unhappy marriages. When divorce is legal and genuinely feasible, partners will tend to *voice* their concerns to improve a relationship, and may *exit* when improvement is lacking. Here, *exit* and *voice* can facilitate a happier equilibrium.

Following Hirschman, we propose that the GAI's promise of economic security and independence can shift the structural economic constraints on *exit* and *voice*, thereby offering a foundation for happier and more equitable relationships. The fear of poverty is a major reason to not *exit* a relationship, particularly for women who have access to fewer economic resources than men. Women's structural vulnerability to economic dependence can strip them of *exit* and *voice*, reinforcing male dominance within families. Poverty and economic uncertainty are also important sources of conflict, and continued conflict within a partnership can degrade the conditions that facilitate *loyalty*. A GAI can extend the availability of *exit* from low quality relationships, promote greater *voice* by diminishing women's structural vulnerability to economic dependence, and support couples through bouts of economic uncertainty, ameliorating sources of conflict. Put differently, Mincome could provide a better foundation for loyalty, one based on relationship satisfaction and happiness rather than economic dependency or need.

In this article, we study three types of relationship outcomes to empirically analyze this hypothesis. We study couple separations to determine how the GAI shapes *exits*, bargaining measures to determine how the GAI shapes *voice*, and conflict measures to determine how the GAI shapes marital satisfaction and quality. If the GAI offers a foundation for more equitable familial relations, we should find that independence effects—*exits*—are concentrated among unhappy partnerships. In parallel, we should find evidence for bargaining effects—that women's



*voice* increases. Moreover, we should find income effects—that conflict decreases as economic worries wane – and we should find declines in other marital conflicts too, either as a result of *voice* and/or income effects. Altogether, these predictions indicate that a basic income should lower the costs of *loyalty*, that is, improve relationship quality among those who remain married.

We make our case using previously unused data from an understudied experiment called the Manitoba Basic Annual Income Experiment, or Mincome. The Mincome experiment was a three-year (1975-1977) guaranteed annual income study conducted by the Canadian and Manitoba Governments in which participants were able to access a GAI equivalent of about \$19,500 CDN (2014 dollars are used throughout) for a family of four. Mincome was designed as a negative income tax and payments were delivered to households rather than individuals, but all adults had an exit option in that they could separate and collect payments independently as single-person households. Collected data includes a household panel register that tracks couple separations during the experiment and a unique and newly digitized two-wave survey of married women that asked about shifting dynamics within the family. This is the first use of the married women’s survey data which in large part has remained undigitized until now. The survey was conducted by Mincome staff and occurs at a baseline and during the study period for both treatment and control groups. Crucially for our study, this survey includes direct measures of bargaining power and marital conflict. Our analyses use data from two sites using different experimental designs: (1) the Dauphin site, which used a “saturation” design where all residents were eligible for Mincome payments, and (2) the Winnipeg site, which used a randomized control trial design following prior U.S. income maintenance experiments.

We find that Mincome did not lead to a disproportionate increase in divorce, but Mincome increased “divorce talk” among unhappy couples and lowered conflicts related to financial concerns. Additionally, we find that Mincome increased some expressions of wives’ bargaining power but had no effect on others. Taken together, our results provide some evidence consistent with the claim that a guaranteed income can offer a foundation for more equitable family relationships.

## **ECONOMIC RESOURCES AND GENDERED FAMILY RELATIONS**

Economic resources play a central role in relationship dynamics. Money shapes who gets in and out of relationships as well as the quality of those relationships. An abused partner might feel unable to divorce due to insufficient means to set-up an independent home. A stay-at-home wife might be unhappy with her partner's expenses on gambling and alcohol but feel powerless to change it. A third couple might have been happy and committed at first, but become unhappy after a bout of unemployment and poverty. These situations respectively correspond to the independence effect, the bargaining effect, and the income effect, representing the three approaches commonly used to understand how economic resources shape family life.

The independence effect emerged from Becker's theory of marriage, which argues that the gains to marriage are maximized when couples specialize and are mutually dependent, as in the breadwinner-housewife model (Becker 1974). Deviations from specialization and wives' economic dependency reduce the gains to marriage relative to other options, like divorce or staying single. There is a vast literature on the economic independence effect related to increases in women's employment, with overall mixed results (Sayer et al. 2011; Killewald 2016; for a review of previous literature, see Sayer and Bianchi 2000). The GAI's effect is different from women's employment because it increases women's incomes without necessarily altering the pattern of specialization: a housewife might continue to specialize in home production after receiving a GAI but she is no longer entirely economically dependent on her husband. One of the concerns raised by feminist critics of basic income policies is in fact that they may reinforce the gendered division of labor by encouraging women to stay home (Gheaus 2008; Robeyns 2001).

The independence effect expects a general increase in divorce as a result of the GAI: it posits that economic independence makes marriage relatively less appealing and divorce less costly (Bumpass 1990; Popenoe 1993). This expectation of *exit* from marriages fueled much of the controversy around GAI experiments mentioned in the introduction, although empirical research offered mixed results. Hannan et al. (1977) found evidence for the economic

independence effect among families who participated in the Seattle and Denver Income-Maintenance Experiments, finding that recipient couples were more likely to split up. Similarly, Hannan and Tuma (1990) showed that divorce was substantially higher for families with the guaranteed income than for the controls: 36 percent higher for blacks, and 40 percent higher for whites. By contrast, Cain and Wissoker (1990a, 1990b) used the same data and found that the guaranteed income had virtually no effect on dissolution at all.<sup>4</sup> Hum and Choudhry (1992) used Mincome data from the dispersed Winnipeg sample and reported no significant results, although the directionality of their coefficient shows stabilizing effects at high and low payment levels and destabilizing effects at mid-level payments, lending no easy interpretation. Choudhry and Hum (1995) updated the analysis with the same data and showed no statistically significant differences between treatment and control groups, suggesting no “independence” effect.

A more compelling interpretation of the independence effect takes the quality of a relationship into account. In this formulation, economic independence does not lower interest in marriage across the board, but provides a way out of “bad” marriages that individuals, mostly women, might enter into or stay in due to economic need (Cherlin 2000; Ruggles 1997; Schoen et al. 2002; Sayer et al. 2011). A guaranteed income means that women in unpleasant or potentially abusive relationships have the economic opportunity to move out and form new single-person households without fear of poverty if the guaranteed income is sufficiently generous as was the case with Mincome. This suggests that divorce might increase as a result of GAI, but for a good reason: because bad partnerships end. This can include cases of domestic abuse and violence, consistent with research indicating that positive changes in women’s socio-

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<sup>4</sup> The results from the experimental guaranteed income literature were fraught with controversy, particularly because studies used the same data and came to different conclusions. Discrepancies between these studies were most likely due to modeling decisions related to time-coverage, time-varying effects, and how to define treatment groups. For instance, Hannan and Tuma’s (1990) analyses did not include the last two experimental years of a five-year experiment, did not allow for time-varying effects, included inconsistent results (such as, more generous benefits leading to smaller effects than less generous benefits), and inappropriately defined the GAI treatment group as families receiving “training” as well as the GAI rather than the pure GAI group. Our own reading of these debates agrees with the skeptical side, we do not see sufficiently robust evidence to conclude that those basic income experiments increased marital dissolution. A discussion paper by Cain (1986) persuasively shows that findings about high levels of marital dissolution are very sensitive to modeling decisions.

economic status may empower them to leave abusive relationships at earlier stages (Johnson 2006; Bunge 2002; Dawson 2001; Dugan et al. 1999; Rosenfeld 1997).

This interpretation of the independence effect equips us with two hypotheses concerning *exit*:

H1a: Mincome couples will be at higher risk of separation than control couples because reduced economic dependence lowers the financial motivations for marriage.

H1b: High-conflict Mincome couples will be at higher risk of separation than high-conflict control couples because those caught in bad relationships will use Mincome to *exit*.

The bargaining effect also notes that independent economic resources provide an exit option to marriages, but asserts that economic resources can also be leveraged to change relationships and not just to exit them (Lundberg and Pollak 1994; 1996). The bargaining idea is that access to an economic alternative can increase one's power to voice grievances, negotiate, and ultimately improve marital satisfaction. For housewives, independent access to sufficient economic resources means that one can convincingly threaten divorce as a bargaining device to demand changes inside a relationship (Lundberg and Pollak 1996). Thus, rather than simply making exits more likely, a GAI may affect the balance of power within relationships by making the threat of exit credible. The mechanism at the core of this hypothesis is power: the power to exit from a marriage may be present even if it is not exercised. The possibility of *exit* facilitates *voice*. Accordingly, we should expect that Mincome increases women's bargaining power within relationships, which could manifest as an increased use of divorce threats or women pressing for greater decision-making power within the partnership. While Mincome payments were delivered to households rather than individuals, its effects on shifting bargaining dynamics would operate through the enhanced capacity to make a credible threat to leave (and collect Mincome payments independently as a single-person household), rather than an increase in women's direct control of income. This is a question we return to in the discussion. Empirical studies examining the impact of economic resources on direct bargaining outcomes are rare, but literature on cash transfers to

women in Latin America suggests that women's decision-making power increases as their economic dependency decreases (i.e., Adato et al. 2000; De Brauw et al. 2014; Rubalcava et al. 2009; for a review see Lomelí 2008).

The bargaining effect expects Mincome to shift power relations within couples because it gives all women the exit option that facilitates voice. More specifically, we should observe that:

H2a: Mincome women will be more likely to bargain with their partners relative to controls

H2b: Mincome women will increase their decision-making power relative to controls

Lastly, the income effect sees increased resources as generally positive for familial relations. This is based on the theory that economic resources reduce financial instability, a perennial source of family conflict (Komarovsky 1971; Liker and Elder 1983). There is solid evidence showing that economic hardship and financial stress increase the risk of severe conflicts and domestic violence (Gelles 1997; Benson et al. 2003; Hardie and Lucas 2010; Golden et al. 2013; Schneider et al. 2016). Studies on divorce and separation have also repeatedly shown that couples are destabilized by lack of income (i.e. Brines and Joyner, 1999; Dechter, 1992; Kalmijn et al. 2007). In the GAI literature on divorce there is some support for an income effect, too. Hannan et al. (1978), for instance, found that that couples receiving the GAI, particularly those who received the more generous income support supplements, were less likely to split up.

The income effect expects Mincome to affect conflicts within families, insofar as economic insecurity is a major source of disagreement and can spill over to generate other conflicts and undermine the conditions of loyalty. In conjunction with the *independence* effect as high-conflict couples divorce, and the *bargaining* effect which provides new ways to resolve conflicts via *voice*, we hypothesize that:

H3a: Mincome will reduce marital conflict related to finances relative to controls.

H3b: Mincome will reduce marital conflict on other issues relative to controls.

Altogether, conceptualizing the hypotheses on the independence, bargaining, and income effects as interrelated parts of a system indicates that the GAI can have notable effects on family relations by shifting structural constraints on *exit* and *voice*, as well as the costs associated with *loyalty*. With respect to likelihood of divorce, Mincome had simultaneously positive and negative effects. Our view suggests that the GAI will destabilize “bad” marriages, but stabilize “good” marriages. That is, Mincome should facilitate the threat of separation in marriages where 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. The average effect on *exit*, therefore, will likely reflect these two patterns. Additionally, examining heterogeneity patterns by couples’ marital satisfaction before Mincome can shed more light on this point. Analyses of bargaining power and couple conflict measures will offer insight on how Mincome shapes relationship dynamics among couples who remain together. We expect Mincome to increase women’s bargaining power within relationships and to reduce conflicts. Overall, and contrary to suggestions that GAI might “undermine the family,” we see potential for the GAI to improve family relations by providing a way out of bad and abusive relationships, supporting families through economic uncertainty and hardship, and facilitating more egalitarian power relations.

## **MINCOME**

Mincome was devised in response to a cluster 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 and subsequent reports posed the GAI 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.

The Mincome GAI experiment included two main sites: (1) a saturation site in Dauphin where all residents were eligible to receive the benefit; and (2) a randomized site in Winnipeg. The experiment was designed to last three years and the public was informed about the timeline; participants knew transfers would end three years after the experiment was launched. In the Dauphin saturation site participants were offered guaranteed incomes equivalent to \$19,500 for a four-person household in 2014 Canadian dollars. Families with no labor market income could access the full guarantee, which was about 49 percent of the Dauphin's median household income in 1976. Benefits were also available to people with incomes above the guarantee baseline. 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, which meant that payments were phased out entirely once earnings reached \$39,000. Positive tax liabilities were rebated too; the rebate faded to zero once market earnings reached around \$43,400. For context, according to the 1971 census, real median household income for Dauphin (together with its rural municipality) was only \$24,758 and by the middle of the experiment in 1976, real median household income was \$39,382. These figures illustrate the accessibility of benefits to a fairly broad group of residents. In Dauphin about 20 percent of the total population received Mincome benefits at some point throughout the program, having a notable impact on households' incomes. In the Winnipeg site, assignment to the control and treatment groups was randomized and Mincome benefits operated similarly, but participants were divided into groups and assigned different treatment plans consisting of one of three guarantee levels (\$19,500, \$23,800, or \$28,215) and one of three tax back rates (.35, .5, or .75).

Guarantee levels varied by family size and composition in both experimental sites. 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 average economies of scale in the home into account. In particular, since per-person housing

costs decline with additional household members, so should per-person household guarantee levels. In practice this meant that a single person would receive 38 percent of the four-person standard, and a couple without children would receive 71 percent. For those without children, a couple living separately would receive, when combined, 107 percent of what they would receive living together. The expressed aim was to eliminate any strictly economic reasons to dissolve a marriage.

Unlike previous experiments in the US, Mincome applied a broad research design based on a more holistic understanding of poverty and familial well-being. Additional variables related to issues of the family were included as well as surveys beyond those directly measuring separation. One unique survey, 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. Unfortunately, Mincome was underfunded and instead of reducing incomes to households, the analysis part of Mincome was completely cut. No final report was produced, and most of the survey data collected on Dauphin has never been analyzed. After the conclusion of the Mincome experiment, a small number of journal articles were produced from the digitized Winnipeg data (Hum and Simpson 1991; 1993; Prescott et al. 1986; Hum and Choudhry 1992). Until recently however, no published research has examined the original survey records (Calnitsky 2016; 2018; Calnitsky and Latner 2017) or administrative health data (Forget 2011) from the Dauphin portion of the experiment.

The Mincome experiment took place in a context of slowly expanding women's rights in the family, large gender economic inequalities, and women's near exclusive responsibility for unpaid care work. Divorce had been legal in the Prairie Provinces (which includes the province of Manitoba where Dauphin and Winnipeg are located) since 1870. By 1971 divorced adults represented 2 percent of the married population in Manitoba and 1.2 percent in Dauphin (Census data, authors' analyses). Gender economic inequality was high, with only about 44 percent of adult women in the labor force and women's average income at about 47 percent of men's



(Census data for Manitoba, authors' analyses). A good share of women's employment was likely part-time, as part-time employment represented about a third of women's total employment nationwide (Ferraio 2010). Social policy was rapidly evolving at the time, with the introduction of major policies like the Canada Pension Plan (Guest 2003) or the Family Allowance Act (Blake 2009). While these policies offered support for families with caregiving responsibilities, they did not offer substitutes to family caregiving. In absence of social infrastructure for childcare or eldercare, the responsibility of caregiving was largely managed by women inside families. This context is useful in interpreting the analyses we present below and to grasp how and why patterns might differ if Mincome were implemented today; we will expand on those reflections in the discussion section.

## **ANALYTIC STRATEGY**

### *Data*

Mincome staff collected data on both control and treated households before and during the experiment. In our analyses we employed two data sources: (1) panel data on relationship status, and (2) a two-wave survey of married couples. The latter was recently recovered from the archives and is newly digitized; this is the first analysis using this survey data on all couples' relationships during the experiment. The panel data on household status contains basic socio-demographic information on all control and treatment households during Mincome from 1974 to 1977, and tracks changes in relationship status and family composition. It includes 10 waves of data, beginning with a baseline survey in 1974, before the experiment, and was updated every four months during Mincome, between 1975 and 1977.

The two-wave survey of married couples was designed to collect information on domestic social relations and power dynamics in the family. It includes only two waves of data, one before and one during Mincome, in 1974 and 1976 respectively. Unlike the panel data on household status, the couple survey was not mandatory but many completed it; to limit refusals a payment equivalent to 25 current dollars per survey was offered to all respondents. Completed

surveys account for about 70 percent of married program participants at the time of administration. Questionnaires were separately completed by husbands and wives, and we use wives' answers only. The questions focus on various subjects, including the domestic division of labor; control over money; power and decision-making in the home; the frequency of disagreement 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. Analyses reported below use the balanced sample of couples who completed the survey before and during Mincome, which excludes attritors and those who split up during survey. The results are robust to use of the unbalanced sample.

We use panel data to analyze *independence effects*—the use of exit—and the two-wave couples survey data to analyze *bargaining effects*—the use of voice— and *income effects*, measured by the frequency of conflicts. Taken together, these patterns determine the conditions and costs of *loyalty* between partners. Table 1 shows descriptive statistics for all dependent variables and the respondents' socio-demographic background (for more details on sample size, attrition, and missing values in key variables see Tables S1-S2 in the Online Appendix). We report data for the total sample and for the two comparisons of interest, identified below as either Dauphin or Winnipeg treatment effects. The Dauphin treatment effect contrasts couples in Dauphin (the treatment saturation site) to Manitoba control couples; the Winnipeg treatment effect contrasts randomized treatment and control groups in Winnipeg. It is important to keep in mind that the identification of the Dauphin treatment effect is more vulnerable to unobserved heterogeneity effects than the Winnipeg treatment effect, because in Dauphin treatment was not randomized. In supplementary analyses, we tested the sensitivity of the Dauphin results by further adjusting the treatment and control sample using entropy balancing methods (Hainmueller 2012; Hainmueller and Xu, 2013) and the results are not substantively different (see Online Appendix Figures S1-S4). We further discuss the limitations of these analyses in the

discussion section. Descriptive statistics show that treatment and control samples are reasonably similar; the Winnipeg randomized design balances the treatment and control sample quite well, whereas the Dauphin saturation design means that treatment and control sample are not as similar, particularly with respect to wives' age and husbands' unemployment. Below we describe our measures to examine each of these treatment effects and analyze separation, bargaining power, and conflict.

<Table 1>

### *Measures*

We measure the independence effect in two ways: couple splits and "divorce talk". The first dependent variable, actual couple splits, comes from the household status panel data, which tracks changes in relationship status in both treatment and control groups. We are able to discern the fate of all couples at the 1974 baseline and every four months between 1975 and 1977. A woman is defined as split when she no longer lives with her husband.

The second dependent variable for separation captures risk via a measure of discussions of divorce, taken from a question in the couple survey that asks how often the couple talks about divorce. We use a scale ranging from 1 ("never") to 4 ("often"). See Table 2 for survey question wording. Because most separations will not happen overnight and most couples will talk about divorce before splitting up, this measure of separation risk is a useful complement to the measure of actual separations. This measure could alternatively be read as an expression of wives' divorce threats and bargaining power within the couple. We will consider this interpretation too, but, because the question as worded does not identify who initiates these discussions, we use it primarily as a measure of divorce risk and analyze wives' bargaining power with other, more direct, measures.

We measure the bargaining power effect using two measures: (a) reports of wives' temporary break-ups, and (b) wives' decision-making power.

- (a) Wives reports about initiating temporary break-ups from the household due to marital conflict; 1 = any temporary break-up, 0 = otherwise. See Table 2.
- (b) Wives' power in decision-making is gauged by a series of questions posed in the couple survey about who should have authority over making decisions, who usually wins arguments in the couple, and how disagreements are typically resolved. We code responses as dummy variables, where 0 means the husband gets his way, and 1 means that either the wife has power or a compromise is found. See Table 2 for more details.

Lastly, we test for income effects using measures that capture couples' disagreements over finances and other related issues. We employ six items that explicitly deal with financial questions, including family expenditures and women's work outside of the home (see Table 2 for full list). Respondents answered using a Likert scale ranging from 1 ("always agree") to 5 ("always disagree"). We combined these items into an index using the average of the six items: the higher the index, the more financial disagreement. Cronbach's alpha, a coefficient measuring scale reliability, is above 0.7, indicating that the items are sufficiently reliable to use in a single scale.

We also constructed an additional scale to gauge disagreement in general. These questions from the survey are structured in the same way, asking about disagreement in areas including leisure and housework (see Table 2). We combine these using the steps described above. Again, Cronbach's alpha is above 0.7.

<Table 2>

### *Methods*

Our first analysis of the separation risk in the Mincome panel data uses a Kaplan-Meier survival function to estimate the chances of separation in both treatment and control groups. This method provides an effective test of different durations of "exposure" to the risk of marital dissolution across families—that is, the different amounts of time that families are in the study—and uses a "risk-set" calculated at every period to account for censored observations. The survival function

provides estimates of the likelihood a couple will split after each given survey, conditional on having not separated up to the prior survey.

The remainder of our analyses use difference-in-difference (DiD) models with individual-level fixed-effects to test whether Mincome has an effect on the outcomes of interest (Angrist and Pischke 2009; Lechner et al. 2016). DiD estimates compare the change in the outcome variable before and after treatment in the treatment vs control groups. We compare the 1974-to-1976 change in family outcomes of interest in the treatment group to the change in the control group over the same period. DiD estimates with balanced samples and two-survey waves are equivalent to first-differences models (Allison 2009). By adding individual-level fixed effects we leverage only within-unit variation, thus eliminating biases driven by stable and unobserved heterogeneity between units (e.g., fixed personality differences). DiD estimates are unbiased if the parallel trends assumption holds, that is, if change in the outcome variable would have been the same in the treatment and control group if treatment was absent. When data includes several pre-treatment observations it is possible to evaluate whether pre-treatment trends between the treatment and control group differ, but we are unable to do this because both of our datasets only include one pre-treatment observation. The potential bias arising from violating the parallel trends assumption is relatively greater in the Dauphin experimental site, because Winnipeg's randomization should, in principle, eliminate pre-treatment differences. We further discuss the implications of this assumption below and in the Online Appendix. The standard DiD equation can be formalized as follows:

$$Y_{it} = \beta_0 + \beta_1 M_{it} + \beta_2 S_{it} + \beta_3 S_{it}M_{it} + \beta_4 Z_{it} + \varepsilon_{it}$$

$Y_{it}$  is the outcome variable at time  $t$ ,  $M$  is a treatment dummy variable that takes the value 1 if the respondent receives Mincome,  $S$  identifies the study period, and  $Z$  captures the time-varying covariates.  $\beta_1$  identifies pre-treatment differences between the treatment and control groups,  $\beta_2$  captures changes in the outcome variable for the control group, and  $\beta_3$  is the key estimate of interest capturing differences in the change of the outcome variable between the

treatment and control group. We estimate this equation with individual fixed effects, which means that we do not estimate  $\beta_1$ . Applied to the analysis on divorce talk, for instance,  $Y_{it}$  is the frequency of divorce talk and  $\beta_3$  tests whether couples became more likely to talk about divorce when they received Mincome compared to couples in the control group. More specifically, it tests whether the change in couples' divorce talk during Mincome was different from that of other similar couples who did not receive Mincome.

We analyze two experimental sites: (1) the Dauphin saturated site, where Dauphin residents receiving Mincome are the treatment group and residents across Manitoba who do not receive Mincome are the control group; and (2) the Winnipeg randomized site, where Winnipeg residents receiving Mincome are the treatment, and Winnipeg non-recipients are the control. We use the shorthand Dauphin treatment and Winnipeg treatment to refer to these two comparisons, respectively. Analyzing both experimental sites allows us to benchmark the estimates against one another and to evaluate different potential sources of variation between the two sites, including differences related to experimental design type (saturation vs randomization) as well as social context (rural vs urban). As noted above, the non-randomized design of the Dauphin experiment means that the parallel trends assumption might be violated in this case, meaning that participants in Dauphin might be selected on some unobserved characteristic resulting in systematically different trends from the control group. Our grounds for causal inference are relatively stronger in the Winnipeg treatment than in the Dauphin treatment; by providing results for both treatments we can evaluate the potential bias.

## **RESULTS**

### *Exit*

Did Mincome increase divorce, as pundits across the political spectrum professed in the 1970s? We examine this issue using micro-data on married couples, noting the number of actual splits in both treatment and control couples. We begin with the stock of all married couples and look at “survival rates” across the duration of the experiment.

Figure 1 presents Kaplan-Meier survival estimates for Dauphin and Winnipeg contrasts, including treatment and control groups. In the Dauphin case, survival rates for treatment and control 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, suggesting some potential for an “independence” effect for women in the treatment group. This divergence narrows in the second half of the experiment as the end date comes into view. By contrast, in Winnipeg there is no discernable divergence at all.

Overall there is little indication that Mincome led to actual splits, consistent with Hum and Choudhry (1992) and Choudhry and Hum (1995). The absolute number of splits and the relative percentage differences in splits between treatment and control groups are both quite small. While treatment effects in Dauphin appear 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. 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. The divergence in split rates are so small that one cannot exclude the possibility that this is chance variation, and thus we gain no confidence that Mincome increased marital separation.

<Fig. 1>

When examining the independence effect, we fail to find evidence for H1a, that Mincome couples will be at higher risk of separation than control couples because reduced economic dependence lowers the gains to marriage. This null-effect could be due to an independence effect too small to detect. Or, if it is true that Mincome increased divorce among unhappy couples but reduced it among happy couples, these two effects may cancel each other out. It is also possible that design features of Mincome, in particular the fact that benefits were delivered to the household and were accessible to individuals who might want to separate only after they separated, makes divorce threats and acting on it less powerful than they otherwise would. We next look at divorce talk, as a measure of separation risk.

Women in the treatment and control groups were asked whether the couple had talked about divorce or separation in the past several months. Figure 2 shows a substantial increase in divorce talk across both sites. Dauphin treatment results are in black, supplemented by Winnipeg treatment results in grey (see Table A2 for full results). In Dauphin the coefficient is 0.11 and statistically significant; in Winnipeg the coefficient is still positive but somewhat weaker and not significant. Because the result is only statistically significant in the Dauphin sample, we cannot conclude with certainty that there is a substantial increase in this measure of separation risk in the overall sample.

However, we find that these discussions were concentrated among couples with low marital satisfaction. We used women's pre-treatment assessment of marital disagreement and interacted "divorce talk" with a dichotomous variable identifying "high-disagreement" and "low-disagreement" relationships. Figure 2 shows that the positive effect in the full sample is in fact due to a large and significant increase in divorce talk among "high-disagreement" couples in both samples. They move 0.58 of a Likert point in Dauphin, about 18 percent of all possible change of three points on a four-point scale. In Winnipeg too, we find a significant coefficient of 0.53.

<Fig. 2>

Moreover, Figure 2 shows that "low-disagreement" couples might see a tiny effect or *decline* in divorce talk, although those coefficients are not statistically significant. In the Winnipeg sample the sign actually moves from positive to negative, though effects are non-significant. It is worth noting that in light of "ceiling effects" there is little room for improvement. That is, with a constant close to 1, many women at the baseline enter the experiment *already* reporting "No, not at all"; they never talk about separation.

This analysis of divorce talk provides some support for H1b, that high-conflict Mincome couples will be at higher risk of separation than high-conflict control couples because those caught in bad relationships will use Mincome to *exit*. It also suggests that Mincome does not seem to correlate with changes in divorce talk among low-conflict couples. The measure of



divorce talk does not explicitly identify the wife as the initiator of divorce talk but could also be interpreted as an expression of wives' divorce threats, in which case it would indicate that Mincome increases wives' *voice* within high-conflict partnerships.

### *Voice*

This section examines the potential for broader changes in power relations within marriages. Even if the *exit* option Mincome afforded to women did not directly lead to splits, perhaps it changed the power relations within the family affording women more bargaining power or decision-making power. We use data from a survey question about instances in which the wife left the home for a night as well as questions regarding wives' decision-making power on a range of issues to examine H2a and H2b—that Mincome women will be more likely to bargain with their partners relative to controls and Mincome women will increase their decision-making power relative to controls.

Married women were asked whether they left home, even if only for a night. If Mincome increased wives' capacity to bargain, we should observe an increase in wives' temporary break-ups among Mincome couples. Results in Figure 3 provide some support for this idea in the Dauphin sample. Wives' in Dauphin became more likely to temporarily break-up with their partners than their control peers. While the Dauphin effect is significant, in Winnipeg the effect is non-significant. We interpret this result as weak support for hypothesis H2a.

We now turn to more general measures of domestic power. Here, we examine potential changes in decision-making power over important aspects of domestic life, asking who tends to make key decisions, who wins out in decision-making, and who gives in when there is disagreement. To what extent do we find direct evidence for changing power relations?

Figure 3 displays the experimental effects of three survey questions on power and decision-making. Positive coefficients indicate that the wife's power increases. The first question asks who should make final decisions about the wife's job. The model estimates the changing likelihood that either she should make the final decision or that the decision should be mutually

decided upon, relative to the base outcome that the husband should make the final decision. Results show small and non-significant declines in the husband deciding in both Dauphin and Winnipeg groups.

The second question asks about who wins when there is a “really important decision” on which partners disagree. Our model estimates the changing likelihood that women usually win out, that decisions are mutual, that neither wins out, or that both sometimes win, relative to the base outcome that the husband usually wins out. Results here are extremely small and non-significant in both groups.

<Fig. 3>

A third question asks about who tends to give in and who gets their way on an issue that causes the most disagreement between partners. Our model compares the baseline category “my husband ends up getting his way” to all other outcomes, and the coefficients show positive increases toward other outcomes but the effect sizes are very small and non-significant. Overall, the evidence that wives’ bargaining power and decision-making improved is very weak. We find some evidence consistent with increases in exercising strong bargaining via temporary break-ups (as measured by divorce talk and reports of the wife leaving for a night or so) in Dauphin, but not in Winnipeg. There is no evidence for other, more general indicators of changing power relations in Dauphin nor Winnipeg. One interpretation of the difference between the Dauphin and Winnipeg result on temporary breakups is that the Dauphin sample is more negatively selected on marital quality. Another possibility is that Dauphin residents’ responses are shaped by community-level effects as well as individual-level effects, that is, it is possible that their outcomes also reflect the fact that their neighbors are receiving Mincome too or at least are eligible to receive it. The available data does not allow us to further disentangle between these two possibilities. In sum, the results suggest weak support for H2a and no support for H2b. While Mincome destabilizes “bad” marriages, Mincome may or may not play a role in changing power dynamics in the home more broadly.

### *Loyalty*

Lastly, we turn to the idea that Mincome might improve relationships by reducing financial conflict and disagreement through the “income effect.” We also consider the possibility that Mincome might reduce marital conflicts more generally due to spill-over effects, *exits* of low-quality couples, and *voice* empowerment. Drawing on Hirschman’s framework, our expectation is that Mincome might reduce the costs of *loyalty*, or the level of dissatisfaction among those who remain in relationships. Evidence for the income effect would also support the idea that the null result in the separation analysis is likely due to two effects cancelling each other out: Mincome increasing the risk of divorce among poor-quality couples but reducing it among other couples by reducing conflict due to financial stress.

We find evidence in survey responses from married women to support H3a, that Mincome will reduce marital conflict related to finances relative to controls, but find weaker statistically significant trends to support H3b, that Mincome will reduce marital conflict on other issues relative to controls. Figure 4 reports the key coefficient of interest from fixed-effects regressions on measures of financial stress and disagreement (see Table A2 for full results); in the top panes, the dependent variables are financial and non-financial disagreement scales, and in the bottom panes the dependent variables are specific items from these respective scales. The results for the financial disagreement index show a statistically significant change of -0.15 Likert points in Dauphin. On a 5-point Likert scale, with a maximum movement of four possible points, this fall in disagreement translates to 3.7 percent of all possible change. It is worth noting here that the constant, at 2.15, implies that there is already a good amount of agreement (1 = Always Agree), and therefore little room for improvement in agreement. In the Winnipeg case we find a similar significant coefficient (-0.17). On the scale of non-financial disagreement, however, while both Dauphin and Winnipeg coefficients are negative and of similar magnitude, they narrowly fall short of statistical significance.

<Fig. 4>

When examining the financial disagreement scale in more detail, we find that coefficients for individual items are generally negative and similar in size but not always statistically significant. Regarding the question on financial disagreement related to whether respondents have enough money to meet family needs, Dauphin shows a negative non-statistically coefficient of -0.1 and Winnipeg shows a very large and statistically significant negative coefficient of -0.33 as shown in the middle-left pane. On the item concerning disagreement on whether to spend or save money, shown in the bottom-left pane; we find a non-significant change of -0.10 points in Dauphin, or a fall of about 4.5 percent of all possible change. Winnipeg shows an even larger and statistically significant coefficient at -0.33. In general these findings support the hypothesis that Mincome reduces financial stress and financial disagreement.

We can also pull apart our findings on non-financial disagreement. While the non-financial disagreement index is not significant overall, two items in the index stand out. As shown in the middle-right pane, wives were asked about disagreement related to their husband's habits, including drinking, and on that issue we find a statistically-significant coefficient of -0.25 points in Dauphin, or 6.3 percent of all possible change. That is, Dauphin wives, compared with control subjects, saw a large and statistically significant fall in alcohol-related disagreement. The equivalent figure in Winnipeg also shows a moderately negative coefficient at -0.1 that is not statistically significant. The question on disagreement about husband's choice of friends shows large negative coefficients in Dauphin (-0.17) and Winnipeg (-0.29), but is only significant in the latter. Based on these results, it is plausible to conclude that reductions in financial stressors translate into reductions in various types of disagreement providing some support for H3b.

## **DISCUSSION**

This paper returns to the Mincome experiment of the 1970s and re-examines the hotly debated issue of the guaranteed income and family relations. It shifts the debate—away from a blinkered focus on the calamity of divorce and the “undermining of the family”—to consider the possibility that income maintenance might offer a foundation for more equitable and happier

familial relationships. We broaden the scope of previous research and analyze three dimensions of family life: separation, bargaining power, and marital conflict. By drawing Hirschman into the family context, we offer an encompassing framework that conceptualizes the independence, bargaining, and income effects as part of an interrelated system that together shape the power dynamics and constraints that characterize different (marital) relationships equilibria. Overall, our results speak in favor of the view that the guaranteed income shifts the economic foundations of family relations in ways that could encourage more equitable family lives.

We find evidence consistent with the hypothesis that Mincome impacted different families differently, strengthening familial bonds in some, but undermining them in already unhappy relationships. This ancillary finding is a kind of threshold effect: we show that Mincome destabilizes high-disagreement but not low-disagreement marriages. The idea that Mincome destabilizes “bad” relationships and has little effect on “good” ones is highly suggestive, and consistent with the conditional version of the independence effect and the income effect. For example, the second half of the finding—having little effect on good marriages—dovetails with our finding of reductions in financial stress and conflict. The first half—destabilizing bad marriages—is reflected in separations and divorce talk, but is too small to impact the overall divorce risk. Our results are consistent with previous studies finding null or very small and contextual divorce effects associated with other guaranteed income experiments (Cain and Wissoker 1990a, 1990b; Hum and Choudhry 1992; Choudry and Hum 1995), and thus adds to the skepticism about the validity of other studies that report widespread and large increases in divorce (Hannan et al. 1977; Hannan and Tuma 1990).

Results yield limited support for the idea that the GAI increases wives’ *voice*. Results from the Dauphin sample suggest that wives may gain bargaining power as measured by initiations of temporary break-ups and also measured by divorce talk – assuming women initiate most of these conversations as research suggests (Sayer et al. 2011) – but the results from the Winnipeg sample are not statistically significant. Results on other decision-making power measures are not statistically significant in either sample. This null result could be due to couples

experiencing increases in bargaining power being more likely to split up and leave the sample. While this is a possibility that we cannot entirely rule out, supplementary analyses comparing pre-treatment differences between couples who remain together, split, or attrite do not support this interpretation (see Online Appendix discussion and Tables S5-S6). The null-results on bargaining measures might instead be due to the fact that, while the guaranteed income makes *exit* credible, it does not eliminate unequal gendered power dynamics within families. It is also 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; the latter is distributed to all individuals and 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 individuals 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 (or a guaranteed income distributed to individuals rather than families) may be even more likely to generate changes in gender power relations in family life. This interpretation is conditional on all else remaining equal, in particular the division of labor. If basic income encourages other changes in family life that exacerbate women's specialization on unpaid housework and care work, the net overall change on women's bargaining power is unclear. A basic income might succeed at providing a more universal access to *exit* (meaning less dependence on markets and less incompatibility between market and care demands), but it could simultaneously discourage women's advances in the labor market (Robeyns 2001; Gheaus 2008). The design of the basic income benefit and characteristics about the context (availability of public services for childcare and eldercare, women's economic position, and gender norms)

might importantly moderate which effect dominates. If this moderating effect is present, it ought to be noted that it was likely far stronger in the context of the 1970s when gender wage gaps were higher than today; correspondingly, the probability that basic income stimulates a return of female domesticity seems lower today.

Next, it is worth considering the differences between the Dauphin and Winnipeg outcomes. There are four potential reasons why slight differences in effect sizes are present. First, selection effects mean that Dauphin participants—as in a real-world guaranteed income—chose to join, where Winnipeg participants had randomized participation. The specific selection effect in this case is not obvious. It is possible that low-quality marriages would be more likely to join the experiment in Dauphin relative to Winnipeg and that this biases our Dauphin results, but because the experiment delivered payments to households as opposed to individuals this possibility is less straightforward. Furthermore, the signs and statistical significance test of treatment coefficients are consistent in the majority of analyses, and the signs of treatment coefficients are consistent in nearly all analyses, providing little indication of strong systematic differences between the two experimental sites due to selection. Second, it is possible that these differences capture real-world differences in how Mincome shapes family relationships in urban vs rural contexts; Dauphin is a more rural community and the familial dynamics might be different. Third, community effects are present in Dauphin, but not Winnipeg. Because Dauphin participants are surrounded by neighbors who might also be participants, there is the potential that people's decisions might be impacted not only by the program directly, but also by the actions of other participants (see Calnitsky and Latner 2017). Fourth, the payment amount and tax-back rates in Winnipeg included packages that were more generous than in Dauphin. Despite these differences, the results are almost always consistent, suggesting that differences in the experimental circumstances did not impact the overall narrative.

The lessons gleaned from the guaranteed income experiments of the 1970s are relevant to the basic income debate today, particularly given the very limited amount of empirical research on family and gender outcomes (Cantillon and McLean 2016; Robeyns 2008). However, is it

worth asking: haven't transformations in the structure of women's work opportunities *already* changed the shape of domination and economic dependency in families? It is true that relationships have changed over time and women's employment has increased. In fact, one of the key factors behind declines in domestic abuse was the expansion of viable alternatives to domesticity (Kalmuss 1984; 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 (England and Kilbourne 1990). 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 (Weeks 2011) and it is compatible with caregiving demands (Zelleke 2011), which continue to be a primary reason why women become economically dependent on male partners (Goldin and Mitchell 2017; Musick et al. 2020). Thus, although changes in social context might have shifted who and how people would use a basic income to consider *exit*, increase their *voice*, or help quality *loyalty*, we believe the fundamental logic of the relationships identified in this study ought to shape our priors.

A continued debate on the guaranteed or basic income is also critical in light of increasing precarity and informality in employment. With widespread uncertainty about the future of job growth, women in particular will likely continue to bear the costs of structural incompatibilities between full-time employment and caregiving. And in a period marked by new sources of economic insecurity (Kalleberg 2009; 2018), 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 (Cherlin 2014). In this way, a meaningful source of financial stability in an era characterized by real uncertainties may prove to be more relevant than ever.

This article argues that a guaranteed or basic income has the potential to offer an economic foundation for more equitable family relations that could have further reaching implications as well. For instance, women's economic power might improve as family dynamics



currently play an important role in structuring women's disadvantage in the labor market. Women's exposure to violence, in particular domestic violence, could also decline as many instances of violence are facilitated by economic dependency. A related paper (Calnitsky and Gonalons-Pons 2018) finds a strong negative association between Mincome and violent crime, which very likely stems from declines in domestic violence. These results are consistent with feminists who view basic income as a useful tool to obstruct some sources of structural gender inequality (Weeks 2011; Zelleke 201; McKay 2005). But our data does not speak to one of the thorny points of debate—whether a basic income would reinforce the gender division of labor and norms about women's caregiving obligations (Gheaus 2008; Robeyns 2001). As noted above, our optimistic interpretation of these results relies on the assumption that all else remains equal, or *ceteris paribus*; in particular, that a basic income does not simultaneously incentivize regressive changes in the division of labor which remain unmeasured in this study. We also do not address important questions about the relative effectiveness of basic income compared to alternatives, such as universal services (Bergman 2004; 2008; O'Reilly 2008). Our study does not provide definitive answers, nor does it intend to defend one side against the other; we believe that there are good arguments on both the optimistic and cautious sides of the debate. Our article contributes to the growing body of empirically based research on basic income and seeks to help inform political discussion. Future feminist research ought to analyze the full spectrum of questions related to housework, caregiving, and employment across contexts to evaluate how basic income policies can shape gender equality and justice more broadly.

Families are locations of love, but also of domination, abuse, and violence, and the economic structure on which they rest will foster one or another type of relationship. Our goal is to offer a framework that broadens sociological thinking around transformative social policy in family life and provides empirical evidence for its application. Early debate about the guaranteed income and the family was myopic in its fixation on divorce, framing separation as uniformly bad, and indeed, seemingly the only harm a family might suffer. But, as we know, unhappy families can be unhappy in a myriad of ways. Blind loyalty and barred exits formed the bedrock

of an older, unhappy equilibrium. A new model of partnership can reconfigure family dynamics into a happy one. If loyalty in family life has value, it is only when exit and voice are also available. Basic income may offer some of the tools to assemble a happy equilibrium. Achieving it will require more than only finances, but a solid base of economic security is surely a necessary piece.

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## FIGURES AND TABLES

**Table 1.** Descriptive Statistics of Treatment and Control Samples

|  | Total sample |              | Dauphin site |           | Winnipeg site |           |
|--|--------------|--------------|--------------|-----------|---------------|-----------|
|  | Range        | Sample means | Control (2)  | Treatment | Control       | Treatment |
| <b>Household panel</b>                             |              |              |              |           |               |           |
| Separation   | 0 - 1        | 0.09         | 0.08         | 0.11      | 0.10          | 0.09      |
| Wives' age   | 15-64        | 32.34        | 32.11        | 37.73     | 31.00         | 29.83     |
| Husbands' age                                      | 18-72        | 35.85        | 35.18        | 42.23     | 33.73         | 33.25     |
| Has young kid <6                                   | 0 - 1        | 0.60         | 0.62         | 0.40      | 0.67          | 0.68      |
| Wives' high school                                 | 0 - 1        | 0.32         | 0.33         | 0.24      | 0.35          | 0.36      |
| Wives' employment                                  | 0 - 1        | 0.33         | 0.38         | 0.22      | 0.38          | 0.34      |
| Husbands' employment                               | 0 - 1        | 0.89         | 0.93         | 0.80      | 0.94          | 0.91      |
| N (married couples)                                |              | 964          | 276          | 250       | 218           | 385       |
| <b>Two-wave couples survey</b>                     |              |              |              |           |               |           |
| Divorce talk                                       | 1 - 4        | 1.15         | 1.17         | 1.08      | 1.17          | 1.17      |
| Wife left for one night                            | 0 - 1        | 0.08         | 0.08         | 0.04      | 0.07          | 0.10      |
| Who should decide about her job?                   | 0 - 1        | 0.31         | 0.36         | 0.21      | 0.35          | 0.32      |
| When there is an important decision, who wins out? | 0 - 1        | 0.87         | 0.84         | 0.90      | 0.86          | 0.87      |
| When there are disagreements, who gets their way?  | 0 - 1        | 0.92         | 0.93         | 0.90      | 0.92          | 0.92      |
| Financial disagreement index                       | 1 - 5        | 2.13         | 2.18         | 2.11      | 2.20          | 2.14      |
| Nonfinancial disagreement index                    | 1 - 5        | 2.23         | 2.28         | 2.17      | 2.30          | 2.25      |
| N (married couples)                                |              | 443 (1)      | 144          | 100       | 107           | 199       |

Notes: (1) 443 represents 68% of eligible married couples at the time of the second couple survey; 641 couples (2) Dauphin's control group includes Manitoba and Winnipeg controls.

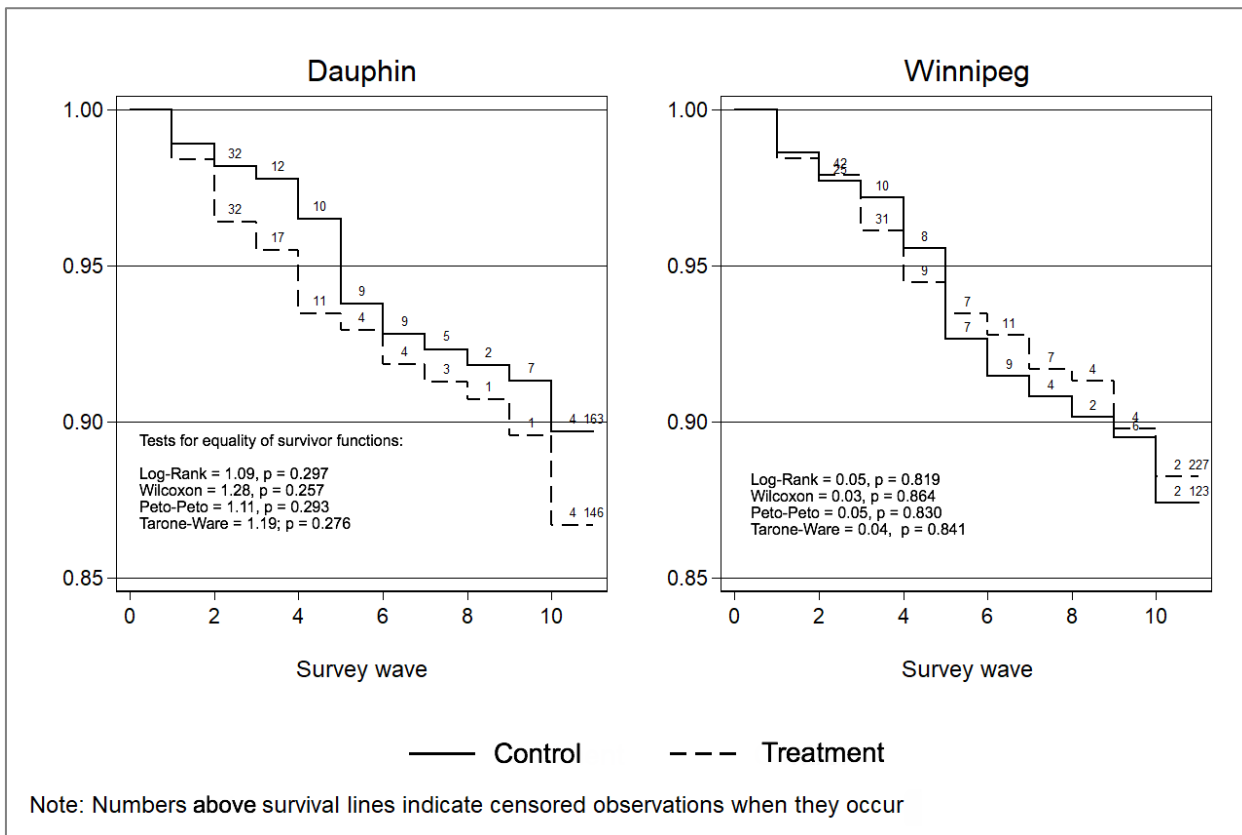
Sources: Mincome Couple survey and Mincome Household panel

**Table 2.** Survey Questions, Answers, and Coding Scheme

| Variables                  | Survey questions  | Answers  | Coding scheme   |
|----------------------------|---|--|---|
| Divorce talk frequency     | In the last several months have you and your husband ever talked about separating or getting a divorce  | Yes, often; Yes, occasionally; Yes, once or twice; No, not at all  | 1 = never, 4 = often  |
| Wives' temporary break-ups | In the last year, have you ever been so upset or discouraged that you left home and went to stay somewhere else for a little while...even if only for a day or so   | Yes/No   | 0 = no; 1 = yes   |
|                            | In every family couples have different ways of deciding things...people have ideas about how decisions <u>should</u> be made and who should make them. How do you feel each of the following decisions <u>should</u> be made by a family... Who should make the final decision about what job you should take?  | Husband should always decide; Husband and wife should decide together; Wife should always decide   | 0 = husband should always decide; 1 = all other answers   |
| Bargaining power           | 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  | 0 = my husband usually wins; 1 = all other answers  |
|                            | Now think about one thing that causes the most disagreement between you and your husband. How would you say this disagreement <u>usually</u> 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 he gives in, and sometimes I do; We each give in a little to the other | 0 = my husband ends up having things his way; 1 = all other answers   |
| Financial disagreement     | To what extent have you and your husband disagreed about each of these financial areas of family life in the last several months?<br>(1) whether the money you have is enough to meet your family's needs<br>(2) how your husband spends money<br>(3) how you personally spend money<br>(4) your working outside the home<br>(5) whether to save or spend<br>(6) the purchase of alcohol  | 5-point likert scale: 1 = always agree, 5 = always disagree  | Index variable using the average of the six items, 1 = always agreeing on all items to 5 = always disagreeing on all items. |
| Non-financial disagreement | To what extent have you and your husband disagreed about each of these financial areas of family life in the last several months?<br>(1) how much leisure time you spend together<br>(2) the amount of affection you show for each other<br>(3) how you get along with relatives<br>(4) helping with work around the house<br>(5) you or your husband's personal habits, e.g. dress, cleanliness, drinking, smoking, etc.<br>(6) religious beliefs<br>(7) you or your husband's choice of friends | 5-point likert scale: 1 = always agree, 5 = always disagree  | Index variable using the average of the six items, 1 = always agreeing on all items to 5 = always disagreeing on all items. |

Notes: In sensitivity analyses we also constructed an alternative version using a binary variable where any report of disagreement on any of the six items set the variable to 1 and 0 if only agreement is reported. Results are robust to this alternative specification, available upon request

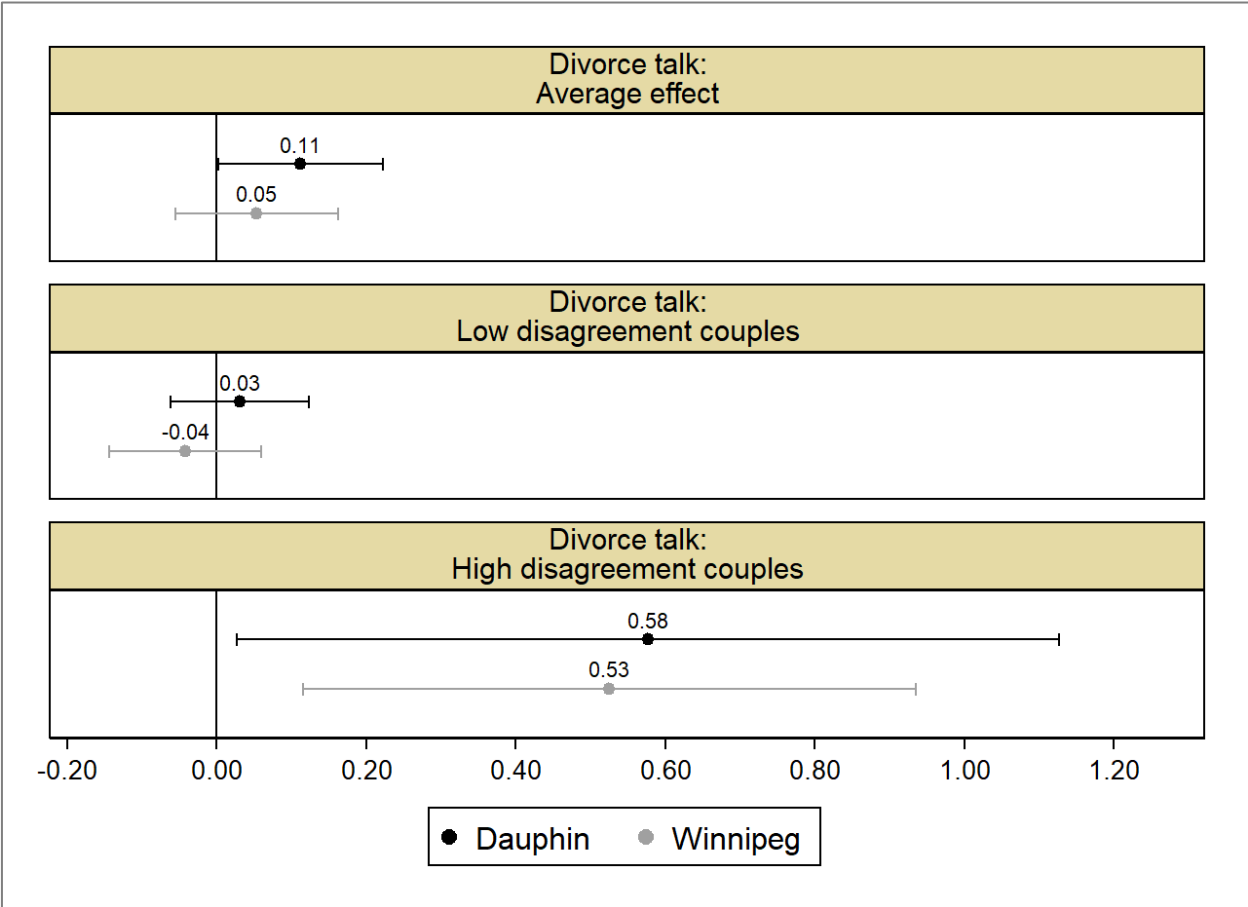
**Figure 1.** Kaplan-Meier Survival Estimates For Separation



Source: Mincome Household panel

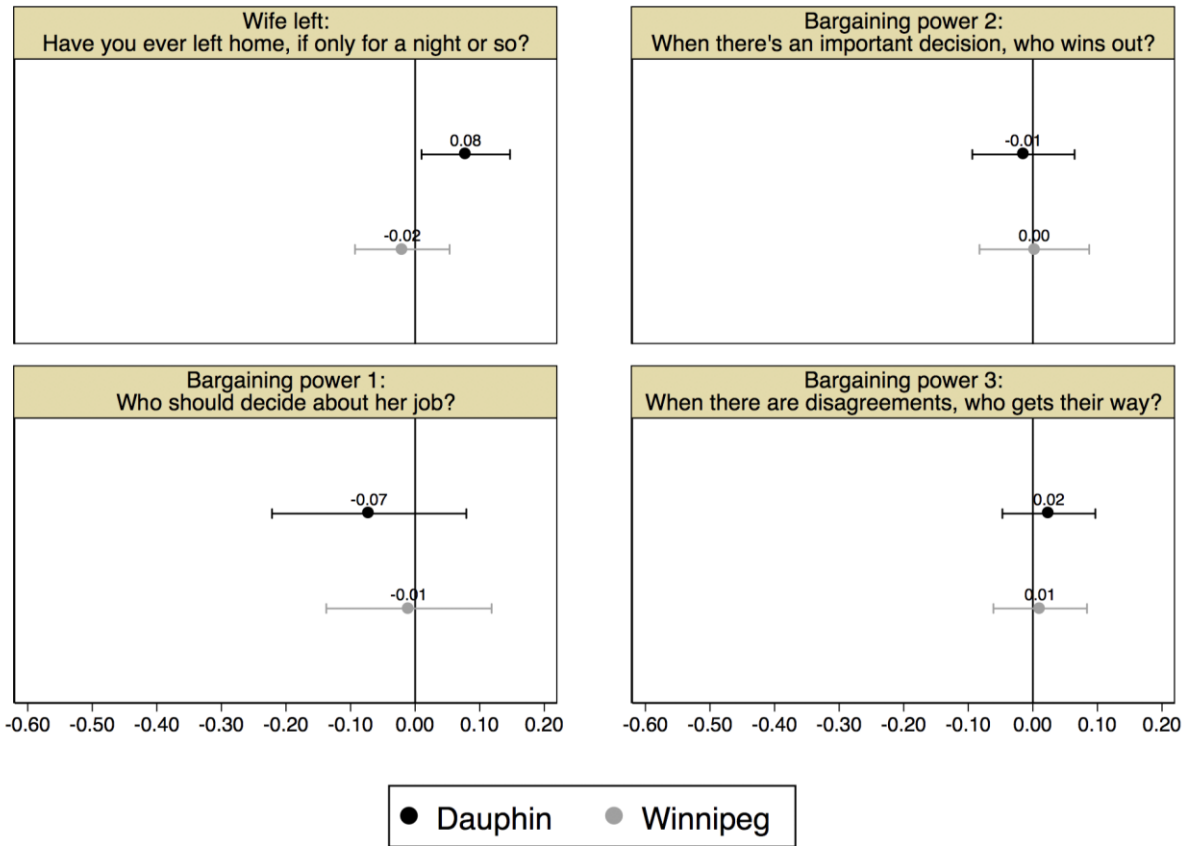


**Figure 2.** Examining Exit. Coefficient Plot for Mincome Effect on Divorce Talk, by Level of Disagreement



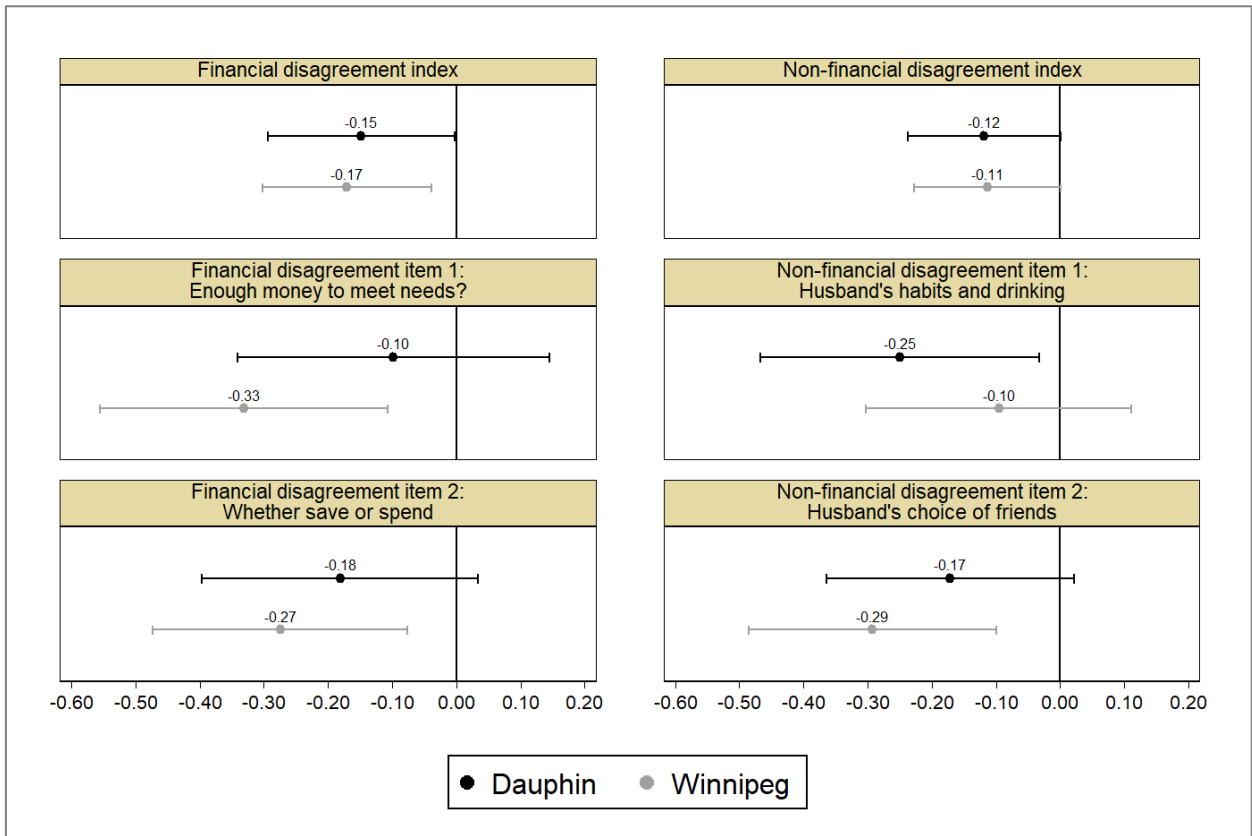
Source: Mincome Couples' Survey

**Figure 3.** Examining Voice. Coefficient Plot for Mincome Effect on Bargaining and Decision-Making Measures



Source: Mincome Couples' Survey

**Figure 4.** Examining the Costs of Loyalty. Coefficient Plot for Mincome Effect on Financial and Non-Financial Disagreement



Source: Mincome Couples' Survey

## APPENDIX

**Table A1.** Fixed-Effects Models Predicting Bargaining and Decision-Making Measures

|                                   | Indicators of divorce threat |          |                           |          |          |          | Wives' decision-making power (1)     |          |                               |          |   |          |
|-----------------------------------|------------------------------|----------|---------------------------|----------|----------|----------|--------------------------------------|----------|-------------------------------|----------|---|----------|
|                                   | Wife left for a night        |          | Frequency of divorce talk |          |          |          | Who wins when there's a disagreement |          | How disagreements usually end |          | Who should make the final decision about your job |          |
|                                   | D                            | W        | D                         | W        | D        | W        | D                                    | W        | D                             | W        | D   | W        |
| Treatment effect ( $\beta_3$ )    | 0.0784*                      | -0.0199  | 0.112*                    | 0.0536   | 0.0311   | -0.0416  | -0.0146                              | 0.00232  | 0.0248                        | 0.0114   | -0.0712   | 0.00958  |
|                                   | (0.0415)                     | (0.0444) | (0.0666)                  | (0.0659) | (0.0705) | (0.0711) | (0.0479)                             | (0.0515) | (0.0436)                      | -0.0439  | (0.0911)  | (0.0776) |
| Treatment effect#highdisagreement |                              |          |                           |          | 0.546*** | 0.567*** |                                      |          |                               |          |   |          |
|                                   |                              |          |                           |          | (0.193)  | (0.174)  |                                      |          |                               |          |   |          |
| Constant                          | 0.0736***                    | 0.117*** | 1.122***                  | 1.161*** | 1.124*** | 1.160*** | 0.870***                             | 0.854*** | 0.926***                      | 0.945*** | 2.466***  | 2.569*** |
|                                   | (0.0145)                     | (0.0150) | (0.0232)                  | (0.0223) | (0.0228) | (0.0220) | (0.0166)                             | (0.0175) | (0.0153)                      | (0.0147) | (0.0317)  | (0.0265) |
| Observations                      | 467                          | 582      | 475                       | 588      | 475      | 588      | 476                                  | 590      | 455                           | 565      | 471   | 589      |
| R-squared                         | 0.018                        | 0.024    | 0.013                     | 0.005    | 0.057    | 0.042    | 0.001                                | 0.003    | 0.004                         | 0.013    | 0.007   | 0.005    |
| N                                 | 244                          | 305      | 244                       | 305      | 244      | 305      | 243                                  | 303      | 242                           | 301      | 243   | 305      |

Standard errors in parentheses

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

Notes: (1) Baseline category refers to the husband deciding or getting his way.



**Table 2.** Fixed-Effects Models Predicting Financial and Non-Financial Disagreement

|                                | Financial disagreement |          |                                     |          |                                  |          | Nonfinancial disagreement |          |                                       |          |                                     |          |
|--------------------------------|------------------------|----------|-------------------------------------|----------|----------------------------------|----------|---------------------------|----------|---------------------------------------|----------|-------------------------------------|----------|
|                                | Index                  |          | Item 1: Enough money to meet needs? |          | Item 2: Whether to save or spend |          | Index                     |          | Item 1: Husband's habits and drinking |          | Item 2: Husband's choice of friends |          |
|                                | D                      | W        | D                                   | W        | D                                | W        | D                         | W        | D                                     | W        | D                                   | W        |
| Treatment effect ( $\beta_3$ ) | -0.148*                | -0.171** | -0.0980                             | -0.331** | -0.181                           | -0.275** | -0.118                    | -0.114   | -0.250*                               | -0.0957  | -0.172                              | -0.293** |
|                                | (0.0883)               | (0.0799) | (0.147)                             | (0.136)  | (0.130)                          | (0.120)  | (0.0723)                  | (0.0694) | (0.132)                               | (0.125)  | (0.117)                             | (0.117)  |
| Constant                       | 2.151***               | 2.170*** | 2.333***                            | 2.384*** | 1.957***                         | 1.989*** | 2.207***                  | 2.281*** | 2.386***                              | 2.424*** | 2.039***                            | 2.223*** |
|                                | (0.0309)               | (0.0272) | (0.0510)                            | (0.0462) | (0.0451)                         | (0.0407) | (0.0251)                  | (0.0236) | (0.0453)                              | (0.0424) | (0.0405)                            | (0.0396) |
| Observations                   | 477                    | 589      | 472                                 | 581      | 471                              | 584      | 482                       | 594      | 474                                   | 591      | 477                                 | 590      |
| R-squared                      | 0.016                  | 0.017    | 0.006                               | 0.031    | 0.014                            | 0.031    | 0.014                     | 0.010    | 0.016                                 | 0.004    | 0.014                               | 0.028    |
| N                              | 243                    | 305      | 243                                 | 304      | 243                              | 305      | 244                       | 305      | 242                                   | 305      | 242                                 | 304      |

Standard errors in parentheses

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

