

The Impact of an Experimental Guaranteed Income on Crime and Violence

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ABSTRACT

Would unconditional cash payments reduce crime and violence? This paper examines data on crime and violence in the context of an understudied social experiment from the late 1970s called the Manitoba Basic Annual Income Experiment, or Mincome. We combine town-level crime statistics for all medium-sized Canadian Prairie towns with town-level socio-demographic data from the census to study how an experimental guaranteed income affected both violent crime and total crime. We find a significant negative relationship between Mincome and both outcomes. We also decompose total crime and analyze its main components, property crime and “other” crime, and find a significant negative relationship between Mincome and property crime. While the impact on property crime is theoretically straightforward, we close by speculating on the mechanisms that might link the availability of guaranteed annual income payments to a decline in violence, focusing on the mechanisms that shape patterns of inter-partner violence.

KEYWORDS: guaranteed annual income; basic income; crime; violence.

*The upper five have twenty
baubles and bangles a plenty
The bottom twenty have five—
just enough to stay alive;
When the pie's in such a plight,
better lock your door at night.*

Quoted in Ronald Holmberg, 1971

What is the impact of economic resources on crime and violence? Would unconditional cash payments reduce property crime? Could it reduce violent crime? Although the guaranteed income experiments (GAI) of the 1970s provided an opportunity to explore these questions (see Levine et al. 2005; Munnell 1986; and Widerquist 2005), attention at the time was concentrated primarily on labor market consequences (e.g., Burtless 1986; Hum and Simpson 1993) and secondarily on “marital dissolution” (e.g., Cain 1986; Cain and Wissoker 1990a, 1990b; Hannan and Tuma 1990). By contrast, the experimental guaranteed annual income literature produced only one preliminary

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analysis on the subject of crime and violence (Groeneveld, Short, and Thoits 1979). The dearth of analysis is surprising because insufficient economic resources are a central driver of criminality (Rosenfeld and Fornango 2007; Rosenfeld and Messner 2013). And since the GAI effectively eliminates poverty, it seems plausible to expect it to reduce crime, particularly property crime. But the impact of a guaranteed income might extend beyond property crime to forms of violent crime. For instance, insofar as risk of a violent incident is heightened by financial stress and financial conflict in the family, and insofar as the guaranteed income reduces financial stress, we should expect inter-partner violence to fall. The guaranteed income might reduce other kinds of violent crime, too, if they are correlated with property crimes, as is often the case (Messner and Rosenfeld 2012; Rosenfeld 2009).

While these mechanisms seem plausible—and the subject matter is highly relevant to a full and “social” cost-benefit analysis of the guaranteed income—both the academic literature that emerged out of these experiments and the subsequent popular debate framed the issues in an exceedingly narrow fashion. Despite changes in familial and economic life since the 1970s, lessons from these unique multi-million dollar experiments remain important for the contemporary debate, especially as economic insecurity (Kalleberg 2018) continues to play a major role in family dynamics, and the social fallout of high-inequality regimes remains largely unaddressed (Atkinson 2015; Grusky and MacLean 2016).

This paper returns to the GAI experiments, focusing on an understudied experiment called the Manitoba Basic Annual Income Experiment (1975–1977), or Mincome, and expands the discussion of the GAI to a set of issues that are broader than those usually considered. Mincome participants were able to access a GAI equivalent to about \$19,500 CAD (2014 dollars) for a family of four. Unlike a universal basic income, which phases out in its *net* impact (here, equal payments are issued to each citizen, but tax liabilities rise with market incomes and steadily exceed payments), guaranteed income payments phase out directly as market incomes rise. Nonetheless, the two policies are quite similar in their economic effects. Both can achieve the identical post-tax-and-transfer income distribution, and work-unconditionality means that both make exits from work and marriage more feasible than they would otherwise be.

While Mincome took place in three sites, this study focuses on the so-called “saturation” site located in the town of Dauphin, Manitoba, where all town residents were eligible for Mincome payments. Unlike more common experimental designs that randomize individuals or families who receive benefits, this distinctive town-level experimental design allows us to ask macro-social questions, including how treatment effects spill over and affect community-level processes (Calnitsky 2019; Calnitsky and Latner 2017). Mincome was unique with respect to this design feature. All other GAI experiments, including the ones now underway—for example, in Stockton, California (Martin-West et al. 2019), Finland (Kangas 2016) and the Netherlands (McFarland 2017)—were set up as randomized controlled trials, which make macro-social questions unaskable by design, as recipients represent a tiny percentage of the town or city population. While these new experiments can, in principle, inquire into crime and intimate partner violence, they are simply unable to examine how a guaranteed income might impact those phenomena by way of shaping social interactions and society more broadly. These questions are highlighted in the literature on peer effects and crime (Carrington 2002; Glaeser, Sacerdote, and Scheinkman 1996; Reiss 1980; Sah 1991). Insofar as people are impacted by social life, this is a serious shortcoming of the new experiments.

There are good reasons to seek out social experiments to study the relationship between economic resources and crime. The virtue of an experiment is the ability to link outcomes with a fundamentally exogenous cause, in this case the availability of Mincome payments. The Mincome experiment is essentially a kind of external shock to people’s incomes, and as such we have a strong case for making a causal argument about its impact on a range of variables. With respect to inter-partner violence, for instance, there is solid evidence linking it to economic hardship and financial stress (Benson et al. 2003; Gelles 1997; Golden, Pereira, and Durrance 2013; Halliday Hardie and Lucas 2010); but these individual-level analyses typically cannot rule out the possibility that some third unmeasured variable

caused both economic hardship and inter-partner abuse. By bringing experimental evidence to debates about the relationship between income and different forms of crime, our analysis improves the robustness of the causal claims in this literature.

We use town-level crime statistics on Dauphin and all similarly-sized Prairie towns, merged with sociodemographic controls obtained from census data. We analyze this time-series data using a difference-in-difference regression that includes town and year fixed effects. These analyses test whether changes in crime rates in Dauphin, the treatment site, deviate from those of other Prairie towns during the Mincome experiment. Our results show a robust and significant negative relationship between the guaranteed income and both violent crime rates and total crime rates. We also decompose total crime and analyze its main components, property crime and “other” crime, and find a significant negative relationship between Mincome and property crime. While commentators have often speculated on these relationships, this is the first paper to use data from a rich country to provide evidence for them.¹

MINCOME

Before examining the data, it is important to provide some background context to the Mincome experiment. Mincome was concocted in response to a cluster of reports that publicized the extent and depth of poverty in Canada in the late 1960s and early 1970s. The Economic Council of Canada (Canada 1968) and the Department of National Health and Welfare (Canada 1970) presented the guaranteed annual income as an intriguing idea meriting serious consideration. These initial volleys in Canada’s war on poverty were followed by the 1971 “Croll Report” (a document on par with the British *Beverage Report*), which posed the guaranteed income as the central policy solution of the era, an idea “whose time has come” (Canada 1971:175). A group of its writers defected from the Croll team and published their own “renegade report,” *The Real Poverty Report* (Adams et al. 1971), which was meant to denounce the bourgeois conception of poverty espoused in the Croll Report. Nonetheless, exactly like the Croll Report, it went on to advance the guaranteed income as the natural solution to their more expansive conception of poverty (see McCormack 1972). The bourgeois researchers of the era and their radical critics were both converging on the same systemic solutions to poverty. They were inspired directly by four similar experiments in the United States, and it was hoped that Mincome would demonstrate the feasibility of the guaranteed income to the Canadian public.

The project was approved and the Mincome experiment was rolled out. Dauphinites were offered guaranteed incomes equivalent to \$19,500 for a four-person household.² Families that for whatever reason had no labor market income could access the full guarantee, which was about 38 percent of median family income (a measure that excludes relatively low-income “non-family persons”), or 49 percent of median household income in 1976. At a negative income tax rate of 50 percent, people could always increase their incomes by working: every dollar of labor market earnings reduced the guarantee by 50 cents, which meant that payments were phased out entirely once earnings reached \$39,000.³ To put these payment figures in perspective: real median household income for Dauphin and its rural municipality was only \$24,758, and median family income was \$39,166, according to the 1971 census. By the middle of the experiment in 1976, we estimate that real median household and family incomes were \$39,382 and \$51,055, respectively.⁴ Guarantee levels varied by family size and

1 For a recent working paper on income changes and intimate partner violence in Kenya, see Haushofer et al. (2019). Other recently published studies used the Alaska Permanent Dividend Fund—which annually distributes a small but universal basic income of one to two thousand dollars to (almost) every citizen—to study the impact on crime. See Watson, Guettabi, and Reimer (2019) and Dorsett (2019).

2 This figure is adjusted from the 1976 payment guarantee and presented, as are all the figures, in 2014 CAD dollars.

3 Positive tax liabilities were rebated too; the rebate faded to zero once market earnings reached around \$43,400.

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

composition. By accounting for economies of scale in the home, the payment structure was designed to avoid advantaging one or another family size (Hikel and Harvey 1973; Hum, Laub, and Powell 1979). This scheme, regardless of the precise accuracy in accounting for economies of scale, made real the possibility of exiting bad or abusive relationships.

The project, however, was underfunded, but rather than reducing incomes to households, the analysis side of Mincome was completely cut. No final report was produced, and most of the survey data collected on Dauphin were left unanalyzed. Subsequent to the end of the Mincome experiment, a small number of journal articles were produced from the digitized Winnipeg data (Choudhry and Hum 1995; Hum and Choudhry 1992; Hum and Simpson 1991, 1993; Prescott, Swidinsky, and Wilton 1986); however, until recently, no published research has examined the original survey records (Calnitsky 2016, 2018a; Calnitsky and Latner 2017) or administrative data (Forget 2011) on the Dauphin portion of the experiment.

As with the U.S. studies, the primary axis of the demonstration concerned the potential effects on labor supply; broader social questions about the impact of giving people money were deemed secondary or not asked at all. However, the early academic documents and reports influencing the design and execution of Mincome showed demonstrable learning from the U.S. experiments (Atkinson, Cutt, and Stevenson 1973; Hikel and Harvey 1973). In particular, they insisted on a more expansive vision of the role played by poverty in social life.⁵ Likewise, relative to their American counterparts, the question of people's wellbeing was framed in broader terms. Indeed, one early Canadian guaranteed annual income planning document ventured some hypotheses related to crime:

Furthermore, insofar as the guaranteed annual income releases family members—particularly mothers—from work, we expect that greater parental control and attention in family relations will reduce the incidence of juvenile delinquency, and increase, as already mentioned, levels of educational achievement for children of recipient families. We hypothesize, therefore: Records of children of recipient families involved in misdemeanors and criminal activity will decrease in the period after as compared to the period before the introduction of a guaranteed annual income programme in the community. (Atkinson et al. 1973:236)

The authors hypothesize further that:

Inasmuch as the guaranteed annual income increases the ability of the poor to participate in community organizations and to enjoy a standard of living closer to that of the community average, we expect that the incidence of crime and mental health problems will decrease in the poorer sections of the community. We hypothesize, therefore: Records of crime and mental health problems for recipient families will decrease in the period after as compared to the period before the introduction of guaranteed annual income programme in the community. (Atkinson et al. 1973:236)

A somewhat more holistic understanding of poverty, and of familial wellbeing, led to a broader research design, one including a “saturation” site, Dauphin. It also led to a more wide-ranging use of researchers and their tools, including ethnographers and an array of survey instruments. For our purposes, a saturation site is relevant because it makes possible a macro-level analysis of changes in

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

aggregate crime and violence. Nonetheless, while there was an interest at the outset in broader social questions, there are no further references to violence or crime in the Mincome documentation. Much like the rest of GAI experimental literature that we review next, these issues were left largely unexplored. And, apart from research focused directly on marital dissolution, so too were questions about the impact of changing family dynamics on inter-partner abuse.

CRIME, VIOLENCE, AND CASH

An important hypothesis suggested by advocates of basic income is that income guarantees may be inversely correlated with a range of crimes (Offe 1992). This thinking supposes that crime declines with material deprivation because as marginalization disappears, so should its associated social pathologies. However, there is a counter-hypothesis suggesting that insofar as crime has roots in “idleness” (i.e., Jacob and Lefgren 2003), which perhaps is fostered by basic income, we should expect crime to grow. The reasoning is at least as old as the Book of Proverbs: “idle hands are the devil’s workshop.” This counter-hypothesis should not be dismissed out of hand; the link between idleness and crime is not the sole province of editorial pages and eighteenth-century political pamphlets.⁶ For example, sociologists Sara MacLanahan and Gary Sandefur argued that “idleness and inactivity are a sign of problems to come. Young adults who are not attached to the labor force or who work only intermittently may not develop the skills necessary for achieving economic security and social success later on. Being idle is also often associated with crime and drug or alcohol abuse” (1994:21).⁷ While it may be the case that limited economic opportunities explain both idleness and crime, it is at least possible that, following MacLanahan and Sandefur, basic income could bring about an increase in criminal behavior.

Despite this, from a rational choice perspective (Becker 1968) the former theory may be the most straightforward and plausible: *a causal link between poverty and property crime exists because reducing poverty reduces the benefit-to-cost ratio of committing crimes*. The mechanism, stated more plainly, says that a society that gives people fewer reasons for crime will see fewer committed. Several studies suggest this to be the case (Chiu and Madden 1998; Hannon 2002; Kelly 2000; Messner and Rosenfeld 2006; Pratt and Cullen 2005; Savolainen 2000), but evidence using experimental designs, such as the GAI, is limited.

Groeneveld et al. (1979) attempted a test using preliminary data from the Seattle and Denver GAI experiments. While the examination was not followed up on, the early analysis revealed little impact. Perhaps the closest evidence comes from a study that can be interpreted as mimicking a basic income experiment.⁸ Akee et al. (2010) track children in households where incomes are increased exogenously through a governmental transfer program. Midway through a study of mental health in North Carolina, which was collecting data on American Indian and non-Indian children, a casino opened on the Eastern Cherokee reservation and began to distribute, unconditionally, a per capita portion of casino profits to tribal members. Researchers found that treatment children had a statistically significant, 22 percent lower risk of committing minor offenses.⁹

Although much criminology literature accepts a direct and unmediated relationship between economic deprivation and property crime, the link between economic variables and violent crime is

6 “To be idle,” Samuel Johnson wrote, “is to be vicious” (1968:145). In a similar vein, Jeremy Bentham saw the workhouse as a “mill to grind rogues honest and idle men industrious” (cited in Polanyi 2001:126).

7 There are other social science papers that purport to demonstrate, or are interpreted as demonstrating a causal link between idleness and crime; see, for example Jacob and Lefgren (2003), Anderson et al. (2000), Snyder and Sickmund (1999), and Allan and Steffensmeier (1989).

8 Another potential example comes from a ten-month BI pilot project that was conducted by a Lutheran bishop and two Lutheran missionaries in 2008 in Otjivero-Omitara, Namibia, where residents under 60 years of age received a modest monthly grant. One report finds a 42 percent drop in the overall crime rate, while “stock theft” and “other theft” fell by 43 and 20 percent, respectively (Haarmann 2009).

9 Also worth mentioning here is the Moving to Opportunity randomized experiment: a move to a low-poverty neighborhood reduced criminal behavior by teens (Ludwig, Duncan, and Hirschfield 2001).

more elusive, usually operating through various mediating or conditional variables (Arvanites and Defina 2006; Rosenfeld and Messner 2013). Some articles show direct associations between economic deprivation and violence (Shihadeh and Ousey 1998), as well as inequality and violence (Kelly 2000); both bodies of literature propose underlying mechanisms that could be in play during the Mincome experiment.

Studies on the relationship between inequality and violent crime propose mechanisms that involve relative deprivation and social cohesion. Kelly's (2000) study of rising inequality and violent crime is furnished with a causal story that appeals to classic "strain" theories originally associated with Robert Merton (1938), and, more recently, with Robert Agnew (2005). For Agnew and Merton, low-status individuals are frustrated when confronted by the relative privilege of those around them and are at higher risk of committing crimes (for further empirical support, see Blau and Blau 1982; and Rebellon et al. 2009). Other criminologists have attempted to explain the positive correlation between income inequality and violent crime through what they call "institutional-anomie theory" (Rosenfeld and Messner 2013). The theoretical claim here is that in more market-dominated societies the norms around means and ends become upended; the moral status of the means selected to achieve different ends become increasingly irrelevant as marketization advances.¹⁰ These authors find empirical support from studies showing significant relationships between rates of violent crime and "decommodification" measures based on social welfare policy (Rosenfeld and Messner 2013; Savolainen 2000). Although the mechanisms may be different, this school of thought also invokes a link between institutions that soften the impact of market inequalities and society-wide levels of uncertainty. They point to the spike in violence in post-Soviet Russia, arguing that it can be explained by the unease and uncertainty facing citizens as the social safety net abruptly unraveled (Pridemore, Chamlin, and Cochran 2007). Despite a proliferation of approaches, in recent years there has emerged a "consensus of doubt" (Bushaway 2010; Chiricos 1987; Rosenfeld 2010) surrounding the precise mechanisms linking the economy and violence.

One underexplored but seemingly plausible approach to thinking about the economy-violence dynamic may be to examine the relationship between the guaranteed income and inter-partner violence. This kind of violence accounts for a sizeable share of violent crime. At the time of Mincome, Statistics Canada published a report stating that assaults made up a large portion of violent crime and that most of these offences were domestic in nature, frequently the result of family disputes (1977).¹¹ The data we analyze below supports this claim. For the years that we can disaggregate violent crime (only 1972–1973), we see that assaults accounted for 89 to 96 percent of the crimes of violence in Dauphin. Even if only half of these assaults were cases of intimate partner violence, any changes in the dynamics of this type of violence would likely leave a notable imprint on overall violent crime rates. These patterns strongly suggest that, even though intimate partner violence is systematically and massively underreported (Johnson and Dawson 2011), it is very likely to be an underappreciated driver of violent crime rates—and also a plausible candidate for the kind of violence that might be impacted by Mincome.

Intimate partner violence is a complex, multi-causal phenomenon, which we do not attempt to comprehensively address in this paper (for overviews, see Johnson 1996; Johnson and Dawson 2011). It is one piece of the broader phenomena of gendered violence and violence against women, which comprise forms of violence rooted in heterosexist gender beliefs or dispositions and are motivated by the desire to maintain women's subordination to men's authority (Manne 2017; Walby 1990). Intimate partner violence is widespread but extensively underreported, in large part because

10 Although this kind claim is argued by philosophers like Michael Sandel (2012), it is not clear that the general conviction is supported by ethnographic and experimental evidence (Henrich et al. 2004).

11 Statistics Canada's 1977 statement reads: "There were 101,861 assaults reported during 1977. . . . This offence continues to represent the largest number of crimes reported in the violent crime group. . . . Many of these offences are domestic in nature, frequently the result of family disputes which can be cleared by police other than by charge when the victim/complainant refuses to prosecute" (1977).

the cultural and legal environment systematically undermines survivors' credibility (Johnson and Dawson 2011). While intimate partner violence has numerous moving parts, research has shown that patterns of incidence are often aggravated by poverty and deprivation, conditions which Mincome sought to eliminate.

Research on intimate partner violence has found a consistent pattern linking economic hardship to higher levels of inter-partner violence (Gelles 1997; Golden et al. 2013). In Canada, a statistical profile on family violence (Bunge and Levett 1998) isolated low income as a key correlate of spousal assault in observational data. There are several mechanisms through which lower economic resources generate higher incidences of intimate partner violence. We identify three. The first is financial stress: couples facing greater economic insecurity experience more stress, which may give rise to situational violence (Cunradi, Caetano, and Schafer 2002). Female respondents to Statistics Canada's one-off Violence Against Women Survey frequently cited stress over finances when asked open-ended questions about how violent incidents usually begin (Bunge and Levett 1998). In the literature, this is sometimes referred to as the "income" effect (Hannan, Tuma, and Groeneveld 1978), where an exogenous increase in incomes reduces financial stress and serves to improve relationship dynamics. Thus, the first mechanism suggests that (1) *inasmuch as the risk of a violent incident is heightened by financial stress and financial conflict in the family, Mincome, as a policy that reduces financial stress, may reduce inter-partner assault.*

Additional mechanisms connecting economic resources with intimate partner violence emphasize how economic dependency increases vulnerability. The lack of independent economic resources is one important reason why women stay in unhealthy or violent relationships (on this literature, see Cancian and Meyer 2014; Hannan et al. 1978; Oppenheimer 1988). Economic dependency also lowers bargaining power within partnerships, and this increases vulnerability to violence by making threats to leave less credible (Johnson 1996). Thus, Mincome payments could provide women a way out of abusive relationships through separation or divorce and could also change the power balance interior to the relationship, thereby fostering better and healthier relationships. The latter point may be important insofar as inter-partner violence is rooted in gender-based power inequalities. The two potential mechanisms here, therefore, are the following: (2) *Mincome reduces exposure to violence by increasing women's ability to exit from marriages,* and (3) *Mincome reduces violence by shifting domestic bargaining power and increasing women's power to threaten to exit.*

To summarize, the literature reviewed above leads to two general hypotheses:

(H1): *Mincome ought to be associated with a decline in total crime. This reduction is expected to be partly driven by a decline in the benefit-to-cost ratio of engaging in property crime, but also by a reduction in other forms of crime that are often associated with property crime and with poverty more generally.*

(H2): *Mincome ought to be associated with a decline in violent crime. We expect violent crime to decline as a result of declines in both violent crimes linked to property crime and also declines in assault, which are largely comprised of cases of intimate partner violence.*

ANALYSIS PLAN, DATA, AND METHODS

We draw on the potential outcomes framework (Morgan and Winship 2015) and define our outcome of interest as the difference between crime rates in Dauphin (the treatment site) and the unobserved counterfactual—or crime rates that would have been observed in Dauphin if Mincome had not been implemented. Following other studies on macro-policy interventions, including the Alaska Permanent Fund's cash dividend (Jones and Marinescu 2018), we approximate this unobserved counterfactual using data from similar settings and compare change-over-time trends. The key substantive question is whether Mincome made Dauphin's crime trends deviate from its neighbors'

trends, under the assumption that Dauphin's crime rates would have followed those of other towns had the experiment not occurred. Our identification method, detailed further below, uses a multiyear difference-in-difference estimator that relies on within-town over-time variation in crime rates and is adjusted for other town-level time-varying demographic and socioeconomic characteristics.

We obtained town-level crime data from Statistics Canada's Uniform Crime Reporting surveys, which contain information on different types of crime on a yearly basis for all Canadian towns and municipalities.¹² We analyze data for all (N=15) Manitoba and Saskatchewan towns with mid-1970s populations between 5,000 and 50,000, using data on crimes of violence as well as overall crime, from the point of availability in 1972 to various municipal boundary changes after 1980. All our analyses rely on these reported crime statistics, which do not cover the full universe of crime. Some underreporting is common in all forms of crime but, as noted above, underreporting is highly prevalent in the realm of intimate partner violence. Because our analysis relies on change-scores, underreporting would bias our estimates only if it were affected by the treatment or if trends in underreporting did not linearly map onto crime prevalence trends. In the case of certain forms of gender-based violence for which data are available, previous comparisons of survey and administrative crime show that the two sources tend to follow the same trends, even though rates are much higher in survey data than in administrative data (Kruttschnitt, Kalsbeek, and House 2014), thus minimizing the second potential source of bias. With respect to the first source of bias, we have no reason to expect Mincome to affect crime underreporting. Thus, although our analyses can speak only of reported crime rates, these data are regularly used, and we believe these results can reasonably extrapolate to crime trends more generally.

Our analyses include four main dependent variables: overall crime rate, violent crime rate, property crime rate, and other crime rate. Ideally, we would like to have further disaggregated data, in particular for violent crime, but such data are, unfortunately, not available. As noted above, however, assaults constitute the vast majority of violent crimes; the remainder includes murder, attempted murder, rape, and robbery. We disaggregate overall crime into its two largest parts, property crime and "other" crime; together they usually make up around seven-eighths of the total (with violent crime comprising most of the rest).¹³ We then separately analyze these two subcategories in order to see the stronger driver of changes in total crime. Property crimes include breaking and entering, theft, and auto theft; "other" crimes include a range of items that sometimes fall in between violent crime and property crime but are not neatly captured by either: arson, bail violation, counterfeit currency, disturbing the peace, escaping custody, indecent acts, kidnapping, prostitution, trespassing, and mischief.

We combine the data with census information on sociodemographic characteristics for each Prairie town. The purpose of these variables is to adjust for shifts in sociodemographic composition that could affect the outcomes of interest. For instance, if all towns except for Dauphin were shedding young people and this led to increases in crime rates, we would be at risk of overestimating or even misattributing Dauphin's hypothetically lower crime rates to Mincome. Census data are available only for 1971, 1976, and 1981, and we linearly interpolate data for the intervening years; otherwise no data are missing.¹⁴

12 These data are comprised of police statistics based on a nationwide system of uniform crime reporting using standard definitions for similar activities.

13 The balance is almost entirely made up of crimes of violence, but there are usually a handful of violations of federal statutes as well.

14 There is a concern that our results could be sensitive to interpolation. Ideally, we would have sociodemographic information for all years, as we do for crime data, but this is not available. We assess this concern in a couple of ways. First, we checked trends in sociodemographic characteristics and observed no large or potentially concerning patterns. Second, we tested the robustness of our results to analyses that do not use interpolation and use only years for which we have census data. The main results are robust to this specification (available upon request), as would be expected. We note, however, that those analyses are less valid than these presented here because they lump together changes that happen before and after the experiment, making them less accurate.

The first set of census controls proxy for population-level changes in socioeconomic status, which are common correlates of crime and violence (Bunge and Levett 1998; Johnson and Dawson 2011). We use the rate of labor-force participation, average family income, and percentage of high-school graduates. Age is also a common correlate of offending and victimization (Johnson and Dawson 2011), and as such, we include the percentage of the population between 20 and 24. Given our interest in the inter-partner violence hypothesis, we include a second set of correlates that measure women's relationship status (Jewkes 2002; Johnson and Hotton 2003). These variables may predict intimate partner violence for a variety of reasons: they may capture, for example, the degree of women's social isolation or social network support (Michalski 2004), or their exposure to intimate partner violence. Here we include the percentage of single-female households and the percentage of divorced females. We also include the ratio of female-to-male average incomes as a proxy for the degree of asymmetry in the domestic power structure, which is sometimes seen as a risk factor for violence in relationships (Heise 1998).

The estimation method we employ is based on town-level fixed-effects regression models to test whether Mincome has an effect on the outcomes of interest. Fixed-effects regression models are equivalent to difference-in-difference estimators when studies contain two survey waves, and they produce a treatment effect average across waves when studies contain more than two survey waves, as in our case (Allison 2009). Fixed-effects regressions leverage only within-unit variation, thus eliminating biases driven by stable and unobserved heterogeneity between units (e.g., idiosyncrasies in local policing, fixed cultural differences, or baseline poverty levels). Additionally, we include time-period fixed-effects to control for time-varying heterogeneity that is uniform across units (e.g., changes in agricultural prices, provincial crime trends, economic trends, or provincial policy changes). This provides an unbiased estimate of Mincome's causal effect under the assumption that there are no relevant sources of unobserved time-varying heterogeneity, also known as the parallel trends assumption. The standard equation we use is 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 Mincome, S identifies the study period, and Z captures the time-varying covariates. Mincome is a dummy variable that identifies treatment for town-years, equaling 1 when the town received Mincome payments in a given year. Dauphin during the Mincome years is the treatment, and all other town-years across 15 other Prairie towns form the control. Y_{it} refers to town-level crime in each year, and β_3 tests whether each town's crime rate of change varies when Mincome is present. More specifically, it tests whether Dauphin's crime rate of change during Mincome is different from that of other similar towns. Applying the fixed-effects transformation means that all variables are demeaned: each value corresponds to the deviation from the town-specific mean. We also test the robustness of our results using placebo tests and synthetic control methods, which we discuss below.

Table 1 shows descriptive statistics for crime rates and other sociodemographic characteristics. On average, Dauphin's violent crime rate was about 600 per 100,000 people between 1972 and 1980, a rate lower than those of other similar Prairie towns that averaged about 800 violent crimes per 100,000 people. Total crime rates are, naturally, much higher than rates of violence, averaging about 8,000 per 100,000 for Dauphin, and 10,000 per 100,000 for other Prairie towns. Property crime rates amount to nearly half of the total crime rates. Other variables show that Dauphin had fewer women in the labor force and fewer divorces, while it resembled comparable towns in all other variables. Note that any changes in these variables are included among the controls, and any stable differences across sites do not impact our results because fixed-effects estimators rely exclusively on within-town variation.

Table 1. Descriptive Statistics, Averaged across Study Period and Experimental Years

	<i>Dauphin</i>		<i>Non-Dauphin towns</i>	
	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>
<i>Dependent Variables</i>				
Violent Crime (per 100000 pop)	600.13	228.65	822.05	533.83
All crime	8149.98	1729.74	10207.58	3860.33
Property crime	4260.77	892.18	5652.33	2059.41
Other crime	2901.38	571.12	3187.76	1682.44
<i>Independent Variables</i>				
Family income	20954.62	1132.90	23563.72	2437.33
Female employment rate	38.82	2.78	43.79	4.01
Female/male avg. Income ratio	0.46	0.03	0.44	0.04
Percent single female	4.93	1.72	4.80	1.54
Percent divorced	1.73	0.30	2.56	0.95
Percent 20 to 24	6.86	0.34	9.66	1.37
Percent with any post-secondary education	24.31	2.91	30.45	4.24

Figure 1 shows crime rate trends in Dauphin, towns with 5,000–25,000 people, Brandon (the only town with a population between 25,000 and 50,000), and Manitoba. We include vertical lines to indicate key dates in Mincome implementation. Baseline and screener interviews started at the beginning of 1974 (for which people were given a nominal payment) and had the effect of advertising the program, which formally began right before Christmas. Mincome's first payments were issued December 1, 1974, and continued in full effect for all of 1975, 1976, and 1977. We plot annual change in crime rates indexed at 1972. Starting from the top-left going clockwise, panels show overall crime, violent crime, property crime, and other crime. Figure 1 offers descriptive support for the hypothesis that Mincome reduced crime levels. Before Mincome, Dauphin's crime rate trends were similar to those of other towns, and they began deviating only when Mincome was implemented. This pattern is most noticeable in violent crime rates, but it is also clear in all other crime rates.

RESULTS

We begin our analysis by examining the association between Mincome and rates of violence. Figure 2 presents the results of the fixed-effects models on town-level violent crime rates by plotting the main coefficient of interest (β_3), which tests whether changes in the outcome variable differed between treatment and control groups. The top panel of Figure 2 shows the coefficient from a fixed-effects regression with controls, and we compare this estimate to those obtained from a fixed-effects regression without controls, below. Table 2 presents the full results.

The results show that Mincome is associated with a decline in violent crime. This is consistent with our hypotheses regarding inter-partner violence, as well as with the hypothesis that a decline in property crime, which we test next, may lead to a reduction in its associated violent crime (Messner and Rosenfeld 2012; Rosenfeld 2009). More specifically, Mincome is associated with a change in violent crimes that amounts to 350 fewer violent crimes per 100,000 people compared to other towns. The magnitude of this effect is quite large, considering that Dauphin's violent crime levels averaged about 600 per 100,000 people. The lower panels show that estimates from fixed-effects without control variables slightly overestimate the effect of Mincome, a result consistent with the fact that Dauphin had a slightly lower crime rate at the beginning of the series.

As expected then, we see a significant effect on reported violent crime—but what about total crime? Like Figure 2, Figure 3 presents the results of fixed-effects models by plotting the main

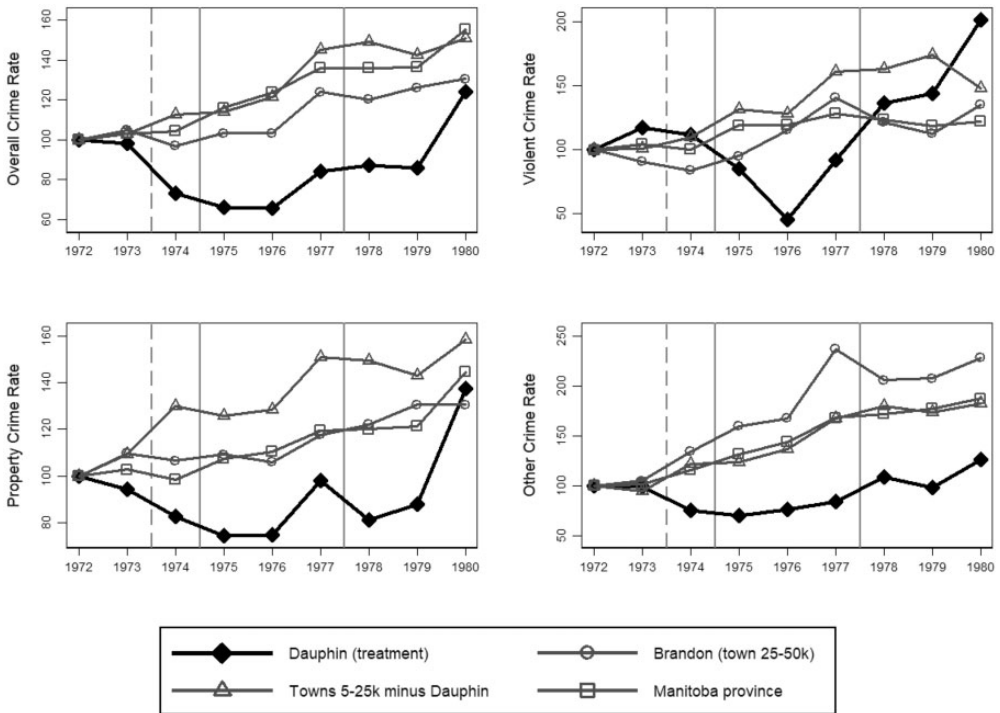


Figure 1. Descriptive Trends in Rates of Violent Crime, Total Crime, Property Crime, and “Other” Crime

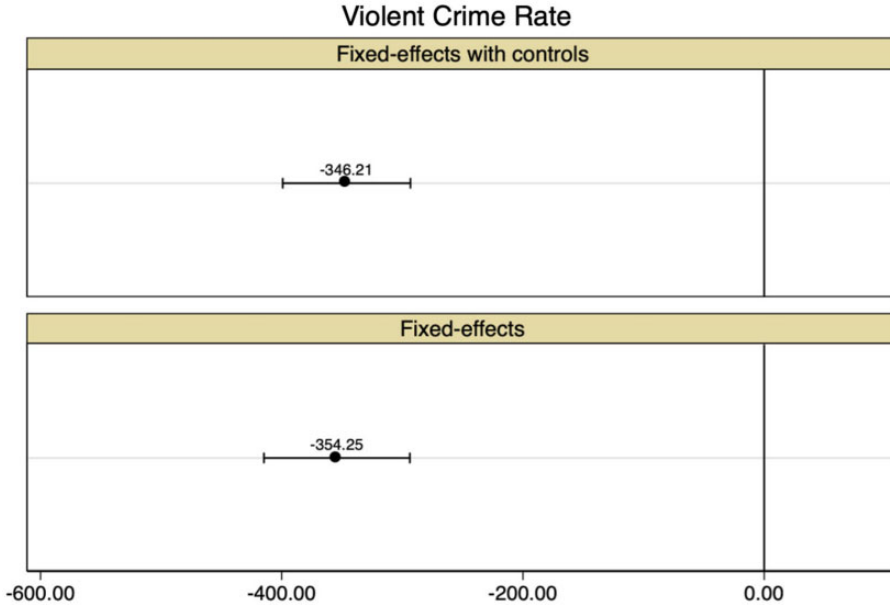


Figure 2. Coefficient Plot of Mincome Effect on Town-Level Violent Crime

coefficient of interest (β_3). Again, the full results are shown in Table 2. Here, relative to similar town-years, Mincome is associated with a change that amounts to 1,400 fewer total crimes per 100,000 people. The magnitude of this effect is large, although smaller compared to the effect on violent crime; considering that Dauphin’s overall crime rate averaged about 8,000 per 100,000 people, the

Table 2. Regression Models Predicting Town-Level Violent Crime and Total Crime (two-tailed tests)

	Predicting Property Crime		Predicting Other Crime	
	Model 2 (FE, no controls)	Model 3 (FE & controls)	Model 5 (FE, no controls)	Model 6 (FE & controls)
<i>Focal variable</i>				
Mincome	-354.3*** (34.34)	-346.2*** (30.07)	-1,869*** (337.8)	-1,438*** (431.6)
<i>Independent variables</i>				
Family avg. income		-0.139** (0.0628)		-1.011* (0.485)
Fem emploment rate		23.38 (17.23)		263.5 (165.2)
Female/male avg. income ratio		2,726 (4,378)		97,427*** (28,180)
Percent single female		7.890 (54.35)		-1,121** (384.6)
Percent divorced		-40.20 (163.1)		1,853 (1,054)
Percent 20 to 24		-64.85 (42.96)		463.3 (383.8)
Percent with any post-sec ed.		-76.25* (37.34)		-976.8*** (233.8)
Constant	689.2*** (66.95)	4,313 (3,031)	8,863*** (613.8)	2,030 (29,706)
R-squared	0.169	0.435	0.394	0.617
N town-years	135	135	135	135
N towns	15	15	15	15

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

change is considerable. Because violent crime comprises just a small portion of the overall crime rate, this result suggests that Mincome also contributed to reduce other forms of crime, albeit perhaps not with as large of an effect size as it did for violent crime.

We hypothesized that Mincome's effects on total crime might be largely driven by its impact on property crime. To examine this, we isolate two key components of total crime, property crime and "other" crime, presented in Figures 4 and 5 (and Table 3). Figure 4 shows results for property crime and confirms that Mincome is associated with a substantial decline in the prevalence of this type of crime. This result conforms to a fairly straightforward rational choice theory of crime à la Gary Becker (1968). Figure 5 shows results for "other crime." We find that Mincome is also associated with a decline in this category of crime, but the effect size is much smaller than that for either property or violent crime, and the estimate does not reach standard levels of statistical significance. Overall, the results show that Mincome turns out to reduce property crime. We commit fewer crimes when we have fewer reasons to do so.

As we note above, much contemporary criminology accepts a direct relationship between material deprivation and property crime, and this helps to explain our results in Figures 3 and 4. However, the link between economic variables and violence shown in Figure 2 is harder to untangle theoretically. And yet the effect on violence is even stronger than the property crime effect. Our discussion section speculates further on the mechanisms behind the negative relationship between Mincome and violence.

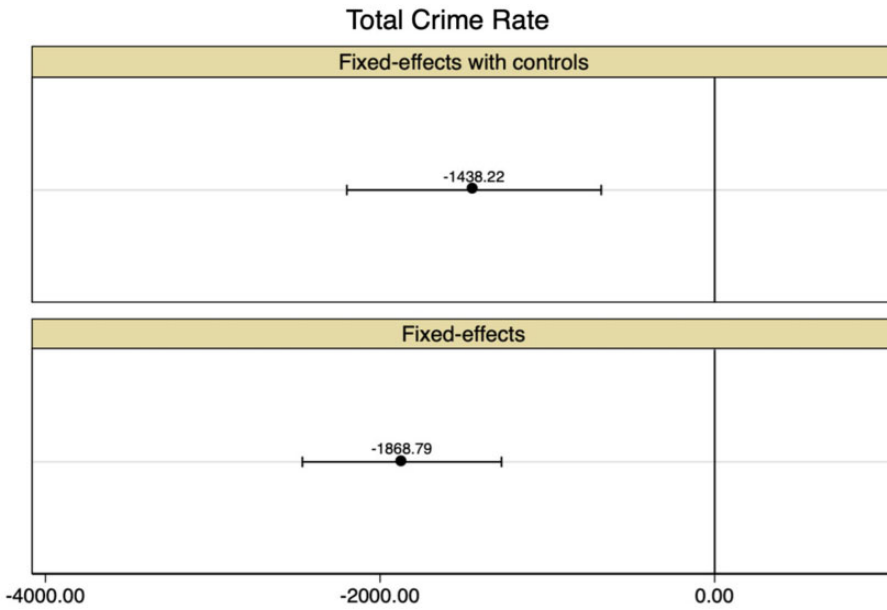


Figure 3. Coefficient Plot of Mincome Effect on Town-Level Total Crime

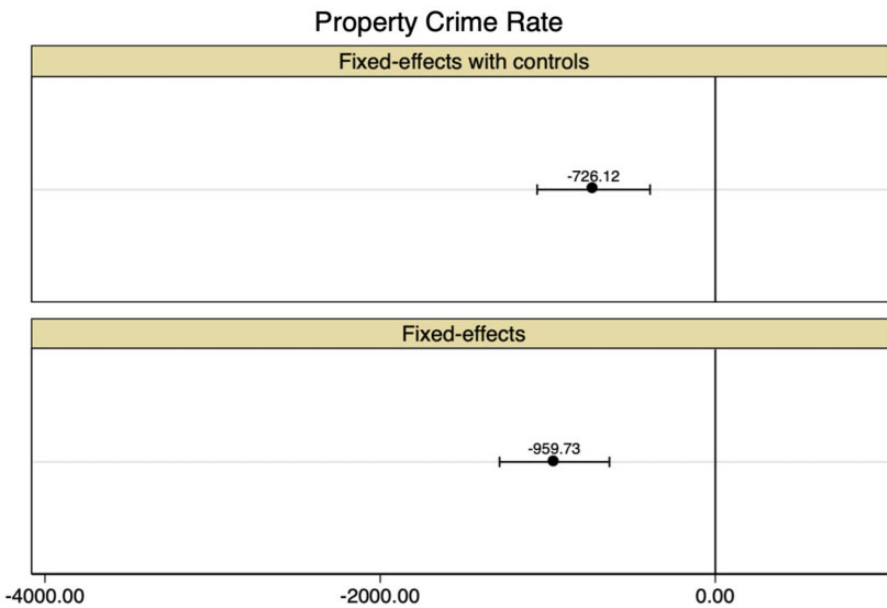


Figure 4. Coefficient Plot of Mincome Effect on Town-Level Property Crime

Robustness Tests

Our analyses have limitations, and we conduct two sets of robustness checks to confirm the sensitivity of our results. One concern is that our results are driven by an unobservable time-varying trait that is unique or particular to Dauphin and unrelated to Mincome. We conduct a placebo test to address this, which consists of switching the treatment years to 1978–1980 (instead of Mincome treatment 1975–1977). Even if Mincome had lingering effects after its implementation, finding placebo

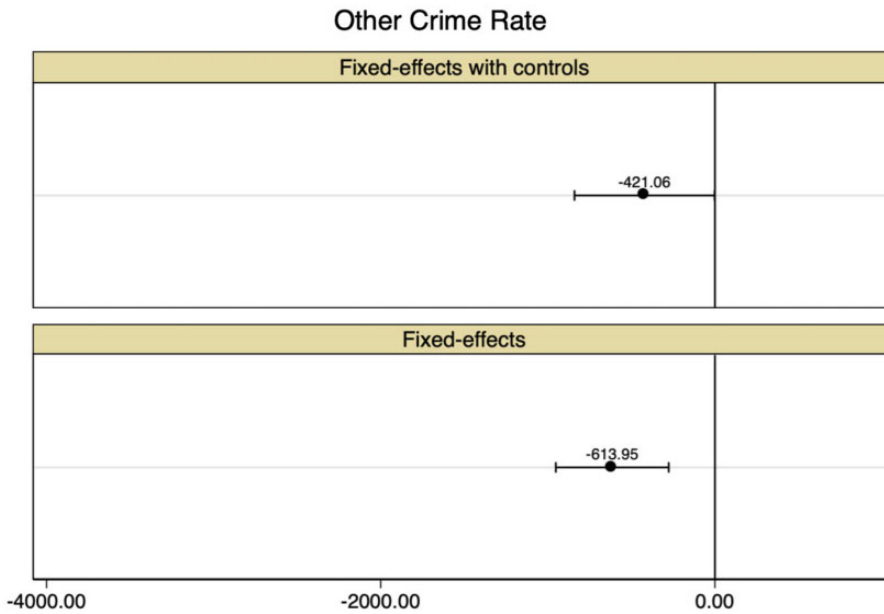


Figure 5. Coefficient Plot of Mincome Effect on Town-Level “Other” Crime

results that resemble the estimates presented above would raise concerns and potentially challenge our conclusions. Results (available upon request) show no evidence for an effect of this Mincome placebo on any of our outcome variables.

A second concern is that our set of control towns offers an inadequate comparison group to proxy a Dauphin counterfactual. This could be true if Dauphin was a unique town within this set, or a town that was really only comparable to a few of the towns included in the analysis. We address this concern using a synthetic control method (Abadie, Diamond, and Hainmueller 2010; 2015), which has been developed for small-N quasi-experimental research designs like ours. This method constructs a synthetic control for the treated unit (Dauphin) as a weighted average of towns in the donor pool (control towns). The logic of this method is very similar to conventional matching estimators (see Abadie, Diamond, and Hainmueller 2010; 2015 for more details). Results from these analyses confirm the estimates obtained using fixed-effects regressions for all outcome variables. This full set of results is available upon request.

DISCUSSION

This paper finds a negative association between exogenous Mincome payments and the rates of total crime and property crime. The most plausible causal explanation reasons that additional economic resources diminish the appeal of property crime. We also find a strong negative association between Mincome and violent crime, which does not allow for as intuitive an explanation. What kind of violence might this be? As noted above, most of the violent crime consists of assaults—and not, say, homicide and rape. As also noted above, Statistics Canada at the time pointed out that most of these offenses are domestic in nature. Intimate partner violence may then be the most plausible candidate as the main kind of violence we see in decline. Here we revisit the two mechanisms that are most promising on theoretical grounds.

First, it may be the case that financial stress declines with Mincome, and reducing financial stress reduces the likelihood of a violent incident. Second, it may be that Mincome changes the balance of power within relationships: the option of starting a new single-person household did not so much increase the actual incidence of exit, but rather the *threat* of exit. Here, improvements in bargaining

Table 3. Regression Models Predicting Town-Level Property Crime and “Other” Crime (two-tailed tests)

	Predicting Property Crime		Predicting Other Crime	
	Model 2 (FE, no controls)	Model 3 (FE & controls)	Model 5 (FE, no controls)	Model 6 (FE & controls)
<i>Focal variable</i>				
Mincome	−959.7*** (186.0)	−726.1*** (191.1)	−614.0*** (191.7)	−421.1* (237.9)
<i>Independent variables</i>				
Family avg. income		−0.199 (0.284)		−0.413 (0.284)
Fem employment rate		156.7* (87.06)		116.8 (85.04)
Female/male avg. income ratio		47,753* (22,678)		53,809*** (16,156)
Percent single female		−592.1** (242.3)		−492.4** (221.3)
Percent divorced		1,594* (875.0)		951.3* (497.8)
Percent 20 to 24		306.7 (221.3)		347.1 (201.2)
Percent with any post-sec ed.		−570.7*** (143.4)		−340.5** (133.9)
Constant	4,508*** (323.2)	−5,604 (16,605)	2,407*** (265.7)	−9,921 (16,746)
R-squared	0.375	0.557	0.424	0.580
N town-years	135	135	135	135
N towns	15	15	15	15

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

power and the empowerment of women in relationships could reduce the chance of inter-partner assault. Indeed, in a separate paper we find some survey evidence from Mincome providing support for both of these mechanisms. Relative to controls, married women see a reduction in financial stress and disagreement with spouses, and likewise a range of survey items demonstrate Mincome’s positive treatment effect with respect to women’s bargaining power (Gonalons-Pons and Calnitsky, forthcoming).

It is worth noting that on this front there is an important design feature that distinguishes a Mincome-style guaranteed annual income from the universal basic income, more often discussed in the media today. Where the latter is distributed to all *individuals*, the former is allocated at the level of the household. It is certainly conceivable for the guaranteed annual income to facilitate someone’s exit from a household: separating from a partner and collecting payments individually was an available option for women under Mincome. Some took that option, and others surely considered it, giving the impact on bargaining power some *prima facie* plausibility. However, because the universal basic income is automatically directed to individuals, not households, it provides not only an exit option, but also resources—and, therefore, power—directly to people, typically women, inside relationships characterized by unequal power dynamics. Allocation to individuals rather than families diminishes the risk that the more powerful party takes control of payments. It stands to reason that

relative to the guaranteed income, a universal basic income may be even more likely to generate reductions in inter-partner violence, as it is better positioned to offset familial power inequalities.

Before moving to the conclusion, we need to briefly discuss the contemporary relevance of these findings. Can we generalize from the 1970s? Can findings about effects on property crime and violent crime from the 1970s have any bearing on the contemporary world? With respect to property crime, there is little reason to suspect that the underlying mechanism has changed; poverty we still have with us, and as such there is no reason to believe that the economic motivations for property crime have dissolved. What about violent crime? Is there reason to believe that even if inter-partner violence was the operative mechanism in the *Mincome* years, it is likely to be less relevant as a mechanism today? For example, is it not the case that transformations in the structure of women's work opportunities have *already* changed the shape of domination in families? Perhaps the low-hanging fruit has been grasped, and the decline in violent assault (Bunge 2002; Sinha 2013) has rendered any potential effects less meaningful.

These questions cannot be dismissed, but it is important not to overstate the case. It is true that relationships have changed over time. For example, while husbands continue to be the modal perpetrators of inter-partner assault, their assault numbers have fallen since the 1990s. However, over the same period, the number of boyfriends reported to police for intimate-partner violence has risen (Johnson 2006). On the whole, some forms of gender-based violence have fallen since the 1970s, but many persist. According to Canada's General Social Survey (GSS) in 2009, the rate of self-reported violent incidents committed against women nationwide in the previous 12 months was 11.2 percent (Sinha 2013). However, questions about *lifetime* prevalence of gender-based violence are not included in the GSS, and naturally these numbers are much higher. Lifetime prevalence questions were included in Statistics Canada's one-off Violence Against Women Survey (1993), the first and only large-scale national survey designed to address a number of inadequacies in prior surveys. In this survey, about half of all women—51 percent—reported at least one episode of physical or sexual violence since age 16 (Johnson 1996; Johnson and Dawson 2011). Even if these numbers have fallen since the 1990s, the Violence Against Women Survey demonstrated that male violence against women was a common experience, and there is little doubt that lifetime prevalence of gender-based violence remains widespread.

Furthermore, it is worth taking stock of the context behind the decline in rates of inter-partner assault. Why did these changes occur in the first place? One of the key factors has been the expansion of viable alternatives to domesticity (Kalmuss and Straus 1982; Pollak 2005). The expansion of labor market opportunities for women had emancipatory effects precisely because it provided an exit option from traditional patterns of economic dependence on male breadwinners. Basic income does just this, but, arguably, in a far more direct manner. It might be even more effective than job growth, because it is not contingent on the vicissitudes of the market. And in light of the uncertainties with respect to future job growth, the old strategy of substituting economic dependence on husbands with economic dependence on bosses may be increasingly ineffective. Instead of providing only partial exit through poorly remunerated or part-time jobs, basic income has the potential to provide a direct alternative to domination in family life. Although this mechanism cannot be tested here, it deserves further and broader exploration in basic income experimentation going forward.¹⁵

Finally, in a period marked by new sources of economic insecurity (Hacker et al. 2014; Kalleberg 2009; Kalleberg 2018), there is good reason to think that financial stress and conflict related to financial resources continue to be a pervasive social reality. Recent research (Schneider, Harknett, and MacLanahan 2016) has shown that inter-partner violence is linked not only to economic hardship, but also with economic uncertainty and anxiety. The guaranteed income reduces economic hardship,

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

but it is designed to respond to economic uncertainty in particular, and as such there is good reason to view it as an important policy tool to block off a key pathway to violent incidents. An automatic and regular stream of cash income will very likely serve to stabilize people's everyday lives and temper conflict in relationships. In an economic climate characterized by sharply rising economic insecurity, guarantees of a baseline level of financial stability may prove to be more relevant than ever.

CONCLUSION

Will this new round of basic income experimentation shed light on these questions about crime and material welfare? There is some reason for skepticism. In the main, this is because labor market impacts continue to be the most important object of study. Indeed, as far as we know, there is only one example, the Kenya case (Haushofer et al. 2019), in which an experiment has included survey data aimed at collecting evidence on intimate partner violence. Another experiment proposes to study crime, perhaps with administrative data (Rhodes 2017). But if these issues remain secondary, problems with data collection and experimental design may emerge. To understand crime and assault, it will be useful for future experiments to collect both survey data and various kinds of administrative data, and evaluate the shortcomings of each (Jencks 1991; Johnson 1996, 2006; Johnson and Dawson 2011).

For reasons that are well known, survey respondents may not report violence, especially for incidents months in the past or when it is an everyday occurrence (see also a summary of shortcomings of traditional victimization surveys in Johnson and Sacco 1995). And surveys may under-sample people who experience the highest levels of violence—the homeless population, for example. On the other hand, police statistics underestimate violence far more than survey data (Johnson 2006). In general, the advantage of police-reported data is its annual nature, comprehensive geographic coverage, and basis in physical evidence. The central shortcoming is that it includes only what is reported to police, and this is a major issue, in particular for intimate partner violence and sexual violence (Lievore 2005 provides a summary of the barriers impacting reporting decisions). It is worth noting, however, that at least with respect to a number of crime categories, survey data and administrative data often follow similar trends (Kruttschnitt et al. 2014). And more important for our own paper, even if there are limits to police-reported data, these are issues that will be consistently present, before, during, and after our experimental period; there is, thus, little reason to think that the trends we identify will be impacted by those limits. As such, even if these data sources do not produce valid evidence, they are likely to be reliable; future research might need to be content to pursue changing trends rather than accurate levels.

A related and under-recognized reason for skepticism highlighted by our study is that analyses such as ours will be impossible in those new experiments testing randomized and scattered recipients rather than whole towns. Randomization rules out macro-social data collection, and insofar as peer effects and social networks are important, data from randomized trials may underestimate real-world effects. Although our data do not allow us to identify which particular social interaction mechanisms are in play, spillovers will at least be picked up where randomized studies assume them out of existence. Moreover, in small experiments, survey data may be required because effects will be too small to be picked up by traditional police statistics. This is not to suggest that administrative data will be off the table, but they will have to be collected in creative ways. These are further reasons why experiments ought to consider Dauphin-style “saturation” approaches to testing basic income (see also Calnitsky 2019 and Calnitsky and Latner 2017).

The literature around income maintenance programs tends to focus narrowly on the extent to which income guarantees reduce people's participation in the formal labor market (see Levine et al. 2005 and Widerquist 2005). The virtue of looking at the guaranteed income through a sociological lens is that it brings to the forefront an array of *social* consequences generated by programs designed to eliminate poverty (for overviews, see Calnitsky 2017, 2018b and Calnitsky, Latner, and Forget

2019). While this kind of analysis should be of interest to those preoccupied with understanding the diverse effects of income-maintenance policies, it should be of equal interest to scholars concerned broadly with crime, violence, and perhaps domination in family life. If sociology can contribute to transformative social policy by facilitating fuller, more socially tuned analyses of costs and benefits, future basic income experiments, depending on their design, may, in turn, benefit sociology by clarifying the mechanisms through which socioeconomic variables affect crime and violence.

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