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THE CAUSES OF HOUSEHOLD BANKRUPTCY:
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The Causes of Household Bankruptcy:
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Abstract

We examine how household balance sheets and income statements interact to affect bankruptcy decisions following an exogenous income shock. For identification, we exploit government payments in one but not any other Canadian province that varied exogenously based on family size. Receiving a larger income shock from the payment (relative to household income) reduces the count of bankruptcies, with fewer remaining filers having higher net balance sheet benefits of bankruptcy (unsecured debt discharged minus liquidated assets forgone). Receiving an income shock thus causes households that would receive lower net balance sheet benefits under bankruptcy law to select out of bankruptcy.

Keywords: household bankruptcy, income shocks, balance sheet

JEL Codes: H31, D41

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1. Introduction

While some explanations of household default and bankruptcy relate to the structure of household balance sheets, other explanations focus on the structure of household income statements. Balance sheet explanations (Fay, Hurst, and White, 2002) suggest that households may default or file for bankruptcy because of the benefits these decisions can bring to their balance sheets (i.e., reductions in liabilities relative to assets). Income statement explanations, on the other hand, suggest that households could select into default or bankruptcy because of liquidity constraints relating to the structure of their income statements — in other words, not having cash (liquidity) available to make debt payments on time (Elul et al., 2010) or because of exogenous income shocks (Gross and Notowidigdo, 2011; Hankins, Hoekstra, and Skiba, 2011). Every household, of course, has both balance sheet characteristics and income statement characteristics; thus, it is possible that the balance sheet explanations and the income statement explanations may not be mutually exclusive but could interact with each other.

We contribute to the literature by providing evidence that *both* income statement and balance sheet characteristics of households interact to determine bankruptcy choices following an exogenous income shock. Our work builds on that of Elul et al. (2010), who conclude that, but do not provide causal evidence for, both income statement (liquidity) and balance sheet characteristics of consumers are important for understanding reasons behind mortgage defaults.

We test for the effects of balance sheets and income statements on bankruptcy using an exogenous and politically motivated, one-time-only fiscal cash transfer paid to every resident of Alberta in January 2006. This payment provides us with two measures of treatment. The first is that we can distinguish exactly which Canadians received the unexpected cash payment (i.e., Alberta residents in 2006). The second measure of treatment is based on the unique feature of this payment that provided every resident of Alberta, including all children and all adults, with the same C$400 payment. Critically, our data, which are provided by the Canadian bankruptcy regulator, include the number of residents living in the household of each bankruptcy filer; thus, we can accurately observe the variation in the dollar magnitude of the payment received by each household by multiplying the number of household residents by C$400. Our data also include the full balance sheet and the full income statement of bankruptcy filers, which enables us to test our hypothesis that balance sheet and income statement characteristics interact to determine which households select into bankruptcy.

We have two main empirical findings concerning the impact of the exogenous income payment on consumer bankruptcy. First, we find that the exogenous income payment causes 7 percent of households to select out of bankruptcy filing, as measured by the total count of bankruptcy filers (without regard to balance sheet characteristics). This finding is consistent with the standard income shock hypothesis, which states that a positive income shock should reduce bankruptcy filings. Second, we find households that filed for bankruptcy despite the exogenous income payment received on average C$1,200 more in balance sheet benefits from bankruptcy than the control group. Balance sheet benefits (BSBs) of bankruptcy are the net financial benefits from bankruptcy received by a filer based on bankruptcy law (broadly speaking, unsecured liabilities forgone minus secured assets liquidated to pay creditors). These two findings together are consistent with an argument that households that have low BSBs under bankruptcy law use the exogenous income payment to avoid bankruptcy, but households that would benefit more from bankruptcy under bankruptcy law proceed to file for bankruptcy despite receiving the exogenous income payment. Our main conclusion, therefore, is that both balance sheet and income statement characteristics matter for household bankruptcy choices following exogenous income shocks.

Our paper contributes to recent empirical research testing the negative income shock hypothesis of bankruptcy by exploiting plausibly exogenous shocks received by some but not other individuals (i.e., treatment and control groups) and examining the resulting effect on total counts of bankruptcy filings. Gross and Notowidigdo (2011) examine exogenous increases in U.S. state-level Medicaid coverage and find evidence to support the standard income shock hypothesis: A positive shock reduces bankruptcies. Hankins, Hoekstra, and Skiba (2011) examine the random differences of small and large lottery winnings on bankruptcy and conclude that winning the lottery does not reduce but rather only postpones bankruptcy, which is inconsistent with the standard income shock hypothesis. Gross, Notowidigdo, and Wang (2014) examine the randomized timing of U.S. tax rebate payments on bankruptcy and find that receipt of these payments actually causes increased bankruptcies, which is the opposite of what would be predicted by the standard negative income shock hypothesis.

This study also relates to papers that emphasize the importance of balance sheet and income statements in households' responses to exogenous shocks (e.g., Mishkin, 1978; Olney, 1999; Koo, 2003; Agarwal, Liu, and Souleles, 2007; Mian and Sufi, 2011, 2012; Mian, Rao, and

Sufi, 2013; Scholnick, 2013; Kaplan and Violante, 2014a, 2014b; Jappelli and Pistaferri, 2014; Baker, 2015). We also contribute to the literature on the effects of fiscal stimulus payments on various economic outcomes (e.g., Johnson, Parker, and Souleles, 2006; Bertrand and Morse, 2009; Evans and Moore, 2011; Agarwal and Qian, 2014). In addition, this research augments a growing literature on the causes and consequences of personal bankruptcy (e.g., Fay, Hurst, and White, 2002; Gross and Souleles, 2002; Han and Li, 2011; Dobbie and Song, 2015).

2. Literature and Conceptual Framework

The existing household bankruptcy literature has emphasized two separate determinants of household bankruptcy decisions based on: (1) household balance sheet characteristics and (2) household income statement characteristics. In this section, we present a simple conceptual framework of the bankruptcy decision when *both* balance sheet and income statement characteristics of the household interact. This follows a variety of authors (e.g., Elul et al., 2010; Gross, Notowidigdo, and Wang, 2014; Wang and White, 2000) who also argue that households consider both balance sheet and income statement characteristics when deciding to default or file for bankruptcy.

The role of household balance sheets in bankruptcy decisions flows directly from bankruptcy law, in which the characteristics of balance sheets define the various costs of bankruptcy (e.g., secured assets, such as houses, which are liquidated and paid to creditors) and benefits of bankruptcy (e.g., unsecured liabilities, such as credit card debt, which are forgone). Following Fay, Hurst, and White (2002), extensive literature has examined the role of balance sheet characteristics as a determinant of bankruptcy choices. We describe how we measure these various elements of bankruptcy law relating to household balance sheets in detail in this section.

A separate literature has examined income statement characteristics, and in particular exogenous income shocks, as a determinant of bankruptcy filings. A variety of different exogenous income shocks were examined in this literature. For example, Hankins, Hoekstra, and Skiba (2011) explore the random differences of small and large lottery winnings on bankruptcy, and Gross, Notowidigdo, and Wang (2014) study the randomized timing of U.S. tax rebate payments on bankruptcy. The argument in studies examining income shocks is that the income shocks impact short-term liquidity of the household, thus affecting bankruptcy choices. The standard version of the income shock hypothesis states that an exogenous income shock allows

the households to meet current expenses (e.g., current debt payments) and thus lower the possibility of the household filing for bankruptcy.[1]

Table 1. Conceptual Framework of Bankruptcy Decisions

Panel A: Without Income Shock		
	Low BSBs	High BSBs
Low liquidity	Do not want to file; may be forced to file	Want to file
High liquidity	Do not want to file; can avoid filing	Want to file
Panel B: With Income Shock		
	Low BSBs	High BSBs
Low liquidity	Do not want to file; can avoid filing	Want to file
High liquidity	Do not want to file; can avoid filing	Want to file

In our framework, summarized in Table 1, we assume that households make bankruptcy decisions on the basis of both their balance sheet characteristics, which we designate as "net BSBs," and their income statement characteristics, specifically income net of expenses, which we designate as "liquidity." For simplicity, we categorize consumers into four groups based on the interaction of low or high BSBs and of low or high liquidity. In Panel A of Table 1, we describe the choices of households without the exogenous income shock. In Panel B, we describe the choices with the exogenous income shock.

We assume that high-BSB debtors (right column in both panels) always choose to file for bankruptcy, and low-BSB debtors (left column in both panels) want to avoid bankruptcy if possible. This assumption is based on the argument in the literature (Fay, Hurst, and White, 2002) that there is a certain threshold of BSBs from bankruptcy, below which no household chooses to file. This threshold may come from costs of bankruptcy not related to balance sheet costs and benefits (e.g., bankruptcy stigma costs (Fay, Hurst, and White, 2002) or potential future costs of bankruptcy from lower access to credit or higher cost of credit after filing). Because of these various non-balance-sheet costs, a certain household specific level of net BSBs from bankruptcy are necessary to make it worthwhile to file. We assume that low-BSB debtors

[1] Gross, Notowidigdo, and Wang (2014) provide evidence that an exogenous positive income shock leads to *more* bankruptcies, which they ascribe to filers using that income shock to pay filing fees. As we describe later, bankruptcy filing fees work differently in Canada than they do in the U.S.

(left column) have benefits below the threshold and thus would like to avoid filing and that high-BSB debtors (right column) are above the benefit threshold and thus would like to file.

The income statement or liquidity variable, across the rows, is based on the argument in the literature that some households may not have enough current income to pay their current expenses (e.g., current debt payments) on time (Elul et al., 2010) and may be forced into bankruptcy even if they have low BSBs from filing. For simplicity, across the rows of Table 1, we assume that low-liquidity households do not have sufficient income to cover current expenses (e.g., debt payments) and that high liquidity households can cover their current expenses.

The exogenous shock in our framework is an income shock affecting liquidity but not balance sheet characteristics of households. (We describe in the next section on institutional background how the Canadian bankruptcy regulator treated these particular fiscal payments, which is consistent with this assumption). We assume that the income shock will reduce liquidity constraints for low-liquidity households by increasing their current income. The income shock will not, however, reduce the liquidity constraints of high liquidity households because the liquidity constraint is not binding for these households and adding even more liquidity is unlikely to have an effect.

Panel A of Table 1 summarizes the decisions of debtors without the income shock. It implies all households with high BSB (right column) file for bankruptcy. Households with high liquidity and low BSB (bottom left cell) would like to avoid filing because their benefits are low. They can avoid filing because they have high enough liquidity to pay off current expenses. The key cell in our analysis is households with low liquidity and low BSB (top left cell). They would like to avoid filing because their balance sheet benefits are low. However, they may be forced to file because of low liquidity; in other words, they do not have enough current income to cover current expenses (e.g., debt repayments).

Panel B of Table 1 shows the situation with the exogenous income shock, which increases the income (i.e., the liquidity) of all households. The exogenous income payment will reduce liquidity constraints for the previously low-liquidity households (top row in each panel) but will not affect liquidity constraints for the previously high-liquidity households (bottom row in each panel) that already have adequate liquidity. As Panel B of Table 1 shows, the only households that change their bankruptcy decision with the exogenous income payment are low-liquidity and low-BSB debtors (top left cell of each panel). It is never beneficial for them to file

because of their low BSBs. However, with the exogenous income shock, some of them can indeed avoid filing because they can cover their current expenses (debt payments) using the exogenous income payment.

This simple framework leads to the following testable hypotheses on the effect of the exogenous income shock (i.e., comparing Panels A and B):

Hypothesis 1. The exogenous income payment will reduce the *count* of bankruptcies because some low-BSB and low-liquidity households (top left cell in each panel) will be able to use the income shock to select out of bankruptcy.

Hypothesis 2. Since some low-BSB households will select out of the pool of bankruptcy filers after the exogenous income payment (top left cell in each panel), on average, the net BSBs of bankruptcy will increase among the few filers remaining with the income shock. In other words, the average BSBs of filers in Panel B (the two cells in the right column) will be larger than the average BSBs of filers in Panel A (the two cells in the right column plus the top left cell).

3. Institutional Background and Data Sources

3.1. Personal Bankruptcy in Canada

There are both similarities and differences between the personal bankruptcy systems in Canada and the United States. Bankruptcy in Canada is federally regulated by a single regulator, the Office of the Superintendent of Bankruptcy (OSB), to which every bankruptcy filing must be made. This is very different from bankruptcy in the U.S., where there are 94 separate bankruptcy court districts to which bankruptcy filings are made. The single Canadian bankruptcy regulator is an important reason we were able to access the large, Canada-wide database used in this paper.

There are two types of personal insolvency in Canada: *bankruptcy*, in which the filer writes off unsecured debt in exchange for liquidating secured assets that are used to repay debts to creditors, and *proposal*, which is a negotiated agreement with creditors to reduce or delay debt repayments without any liquidation of assets; these mechanisms are broadly similar to Chapter 7 and Chapter 13 bankruptcies in the U.S., respectively. This paper focuses only on Canadian consumer bankruptcy.

Every bankruptcy filing in Canada has to be made to the OSB by a bankruptcy trustee. The trustee is typically a professional accountant licensed by the OSB to act in bankruptcy

filings. The trustee, who is considered an officer of the court, is designed to be impartial between creditors and debtors. The values of all balance sheet and income statement data used in this paper are determined by the trustee rather than by the individual bankruptcy filer, based on legal standards established by the OSB.

Ramsay (1999) shows that approximately 98 percent of all personal bankruptcies in Canada are filed under "summary administration," which is a highly automated process used for relatively simple and routinized files. Furthermore, Ramsay shows that in only 5 percent of individual bankruptcy cases do creditors object to the bankruptcy. In other words, for the overwhelming majority of individual bankruptcy filers in Canada, the individual debtor is not required to appear in court to face creditors.

An important institutional distinction between the bankruptcy processes in the U.S. and Canada may explain the difference between our Canadian results (that exogenous payments reduce bankruptcy) and the U.S. findings of Gross, Notowidigdo, and Wang (2014) (that exogenous payments increase bankruptcy). Gross, Notowidigdo, and Wang argue that their finding is due to liquidity-constrained individuals using the exogenous payment to pay bankruptcy filing fees and filing for bankruptcy. The key institutional detail that distinguishes the Canadian situation is that, in Canada, bankruptcy filing fees can be paid over a nine-month period after the filing date. In other words, the filing fees constraint is less binding for bankruptcy filers in Canada.

3.2. The Exogenous Shock: The Alberta 2006 "Ralph Bucks" Cash Payment

We use as an exogenous shock the "Ralph bucks" payments made by the government of Alberta to every resident of Alberta, but not to other Canadians, in January 2006. The magnitude of the Alberta cash payment was C$400 for every resident of Alberta (including all adults and all children). The only Alberta residents not eligible for the payment were prison inmates. The magnitude of the fiscal cash transfers in this study is very similar to the magnitude of the exogenous fiscal cash transfers (U.S. tax rebates) examined by Gross, Notowidigdo, and Wang (2014) in their bankruptcy study, which typically fell between US$300 and US$1,200 per household.

There was much discussion in the Albertan popular press at the time that the motivation for this one-off payment was a "vote grab" designed to increase the political popularity of the

then-Premier of Alberta Ralph Klein. The politically motivated nature of these cash transfers is indicated by the fact that this kind of payment never occurred before (or since) in the recent history of Alberta. Because of the perceived political motivation for these one-off payments, Albertans almost universally referred to them as "Ralph bucks." We follow a variety of authors (e.g., Levitt, 1997) who argue that politically motivated actions of politicians are a good source of exogenous variation.

Based on data made available to us by the government of Alberta, 92.2 percent of these payment checks were delivered in January 2006; thus, in our tests that follow, we use January 2006 as our "event month." The transfer was exempt from taxes, and it did not alter eligibility for other government programs.

An important institutional detail focuses on how the Canadian bankruptcy regulator, the OSB, dealt with this payment. The OSB stated very explicitly that the Alberta 2006 cash payments were exempt from seizure in bankruptcy. Specifically, the OSB ruled that "the rebate amounts are exempt from execution or seizure, and cannot be assigned …. The rebates are considered property of the bankrupt that is *not* divisible amongst the creditors" (italics added).[2] Therefore, the fiscal transfer did not affect either assets or liabilities of bankruptcy filers, and it was not considered part of the income that could be distributed to creditors.

3.3. Geographic Structure

We run two separate tests in this paper: The first examining bankruptcy counts in small geographic areas and the second examining individual bankruptcy filer–level data. The geographic area that forms the unit of analysis of our count tests is a Canadian Census geographic unit known as a dissemination area (DA). These DAs are very small, containing approximately 200 households on average, with a geographic area of 0.2 square kilometers on average. We conduct our count-level analysis at the DA level because DAs are the smallest geographic units in Canada for which neighborhood data are available from the Canadian Census. Thus, by using DAs as our geographic unit of analysis, we can use a large variety of DA-level observables for propensity score matching and so on.

[2] www.ic.gc.ca/eic/site/bsf-osb.nsf/eng/br01567.html

3.4. Data Sources

All our bankruptcy data are provided by the OSB, and DA-level control variables from the census are provided by Statistics Canada. Table 2 provides summary statistics for count-level data aggregated at the DA level, and Table 5 provides summary statistics for individual bankruptcy filer-level data. As can be seen in Table 5, our filer-level balance sheet data are very rich because we can observe the full balance sheets of all electronic filers.

4. Identification Strategy

Our identification strategy exploits two separate measures of treatment implicit in the Ralph bucks payments of January 2006. The first is that the payments were only made to residents of the province of Alberta and not to other Canadians in all other provinces. Thus, our first measure of treatment is based on the comparison of Albertan bankruptcy filers who received the payment with other Canadians who did not receive the payment.

Our second measure of treatment relies on the institutional fact that every Albertan resident, including all adults and all children, received the identical amount of C$400. Critically, our bankruptcy data allow us to observe the total number of residents (including all adults and children) in the household of each bankruptcy filer. We can thus accurately measure variations in the magnitude of the payment across individual households based on household size. Our assumption is that the number of individuals in the household is exogenous with respect to the payment. We argue that it would be unlikely that households would adjust the number of residents living in the household simply to take advantage of the one-off Ralph bucks payments.

As in many other papers examining the effect of exogenous income shocks on bankruptcy filers (e.g., Gross and Notowidigdo, 2011; Hankins, Hoekstra, and Skiba, 2011; Gross, Notowidigdo, and Wang, 2014), we are only able to observe bankruptcy filers and not households that do not file for bankruptcy. Similar to the previous studies of consumer bankruptcy, we overcome this problem by constructing counts of bankruptcy filings per geographic unit per time — in our case, per census DA per quarter. DAs are the smallest geographic area at which neighborhood census data are available (approximately 200 households). Thus, in our empirical specifications, our treated group consists of DAs in Alberta, residents of which received the payment in 2006, and our control group includes DAs that did not receive the payment. The variation in the magnitude of the payment received (based on

household size) can be considered as exogenous variation in the intensity of treatment. Our empirical models shown here follow the structure of difference-in-differences models (before or after payment times receipt or nonreceipt of payment), with variation in the intensity of treatment (size of payment for treated households that receive the payment).

While our treated group consists of DAs in Alberta that received the Ralph bucks payment, we use a variety of approaches to construct our control groups. Our first approach is simply to include all DAs in the rest of Canada in our control group. However, one possible concern with this approach is that our treatment group (Alberta) may have systematically different characteristics from our control group (the rest of Canada). To overcome this possible dissimilarity in the characteristics of the treatment and control groups, we follow a standard approach in the literature (e.g., Agarwal and Qian, 2014) and use propensity score matching developed by Heckman, Ichimura, and Todd (1997, 1998).

The top panel of Table 2 shows the summary statistics for various observable census variables for DAs in Alberta and outside of Alberta before the propensity score matching. The last column of this table reveals that, before the propensity score matching, DAs in the treatment and control groups are indeed statistically different across a number of dimensions, such as median personal income, unemployment rate, lagged neighborhood bankruptcy rate, education, family separations, and age distribution.

We thus generate a propensity score–matched control group based on matching each DA region in the treatment group (Alberta) with a DA region in the control group (rest of Canada) using the large number of census characteristics described in Table 2. We follow a standard procedure for propensity score matching in that we examine observable variables before the treatment (i.e., available in Q1:2005). In particular, we implement one-to-one nearest neighbor matching without replacement and without caliper. We use a logistic regression to estimate the propensity score. As matching variables, we use median DA personal income, unemployment rate, region's urbanization index, past neighborhood bankruptcies, numerical literacy, proportions of divorced and separated, age and gender distributions, proportion of homeowners, and DA average levels of educational attainment. Table A1 in the Appendix provides full results of the logistic regression specification, which we use in the propensity score matching.

The bottom panel of Table 2 shows the summary statistics for DAs in the treatment and control groups after the propensity score matching. The last column of this table shows that

differences in observable DA characteristics between treatment- and propensity score–matched control groups are not statistically different from zero for all observable variables at the 5 percent level of significance.

Figure 1 plots the average quarterly count of bankruptcies per DA in the treatment group (Alberta) and the control group (rest of Canada). In Figure 1, quarter t = 0 is the announcement date of the Ralph bucks payments, and quarter t = 1 is the payment date. This figure seems to visibly show the existence of parallel trends for the treatment and control groups in the period before the announcement date (up to seven quarters before the announcement). It also shows how the number of bankruptcies in the treatment group decreases substantially relative to the control group after the announcement and payment dates. This visual evidence is thus consistent with our hypothesis that receipt of the payment reduces the count of bankruptcy filings, for which we provide more formal evidence in the next section.

5. Hypothesis 1: Bankruptcy Counts Tests

This section empirically tests Hypothesis 1 developed earlier, which predicts that an exogenous income payment should reduce the *count* of bankruptcy filings. The standard version of the income shock hypothesis for bankruptcy states that a positive shock (e.g., unexpectedly receiving a cash payment) should lead to a reduction in bankruptcy filings because individuals would use that payment to avoid bankruptcy. Recent empirical research has attempted to test this hypothesis by exploiting plausibly exogenous shocks received by some but not other households and examining the resulting effect on total counts of bankruptcy filings (e.g., Gross and Notowidigdo, 2011; Hankins, Hoekstra, and Skiba, 2011; Gross, Notowidigdo, and Wang, 2014).

Our first tests, reported in this section, follow much of this existing literature in that they examine whether an exogenous shock impacts the simple count of the number of bankruptcy filers within regions (without regard to balance sheet characteristics of households). The next section of this paper presents our main results, which test whether the balance sheet characteristics of individual households affect the bankruptcy responses to the exogenous shock. The main reason for first documenting the bankruptcy count results follows from our ability to observe all bankruptcy filers but our inability to observe households that do not file for bankruptcy. Once we have shown that an exogenous payment shock *reduces* the count of

bankruptcy filers (in this section), we can then describe the individual balance sheet characteristics of those few individuals who continue to file in spite of receiving the payment shock (in the next section). Our main hypothesis in this section implies that there is a negative coefficient on the difference-in-differences term (i.e., receipt of the treatment reduces the count of bankruptcy filers after the treatment).

Here, we first examine a simple difference-in-differences specification (where we aggregate quarters into either pre- or post-event periods) and then use a distributed lag specification (in which we separately examine individual quarters to test for pretreatment, announcement, and disbursement effects).

5.1. Difference-in-Differences Specification

Our main difference-in-differences specification is as follows:

$$(1) \qquad Y_{dt} = \beta_{pre} \times RI_d \times 1_{pre} + \beta_{ann} \times RI_d \times 1_{ann} + \beta_{post} \times RI_d \times 1_{post} + \alpha_d + \delta_t + \varepsilon_{dt},$$

where *d* denotes DA and *t* denotes time in quarters. *Y* is DA bankruptcy count per 1,000 persons. The effect of the income shock is captured by β_{post}, which is the coefficient on the interaction of 1_{post}, the binary variable for the post-treatment period (Q4:2005 – Q4:2006), with RI_d, the relative income shock (the dollar amount of the transfer received by a family divided by the average family income in each DA in Alberta). We describe how we measure RI_d in detail next. RI_d is equal to zero outside of Alberta because there was no transfer payment outside this province. We include the interaction of a binary variable equal to 1 in Q3:2005, 1_{pre}, with the relative income shock, *RI*, to test if the trends in the treatment and control groups are similar before the treatment (Ralph bucks payment).[3] We also include the interaction of *RI* with an announcement binary variable 1_{ann}, which is equal to 1 in Q4:2005, to measure the announcement effect of the transfer. All these coefficients will show changes in bankruptcies relative to the omitted period (Q1:2005 – Q2:2005). To account for possible heteroscedasticity,

[3] We also estimated specifications without the pretrend indicator and found very little effect of this on the main variables of interest.

we cluster standard errors at the DA level (the most disaggregated level in our study). We include calendar quarter fixed effects, δ_t, to capture seasonal variation in bankruptcy filings. We also include DA fixed effect, α_d, in the regression to account for unobservable but time-invariant local DA variables.[4]

Our treatment group is defined as DAs that received the payment (Alberta), and the control group is those that did not receive the payment (all other Canadian DAs). We report results in which our control group is defined as either all non-Albertan DAs or alternatively non-Albertan DAs matched using the propensity score matching technique described previously.

The intensity of treatment, relative income shock RI_d, reflects the amount of payment received across those treated households divided by a measure of liquidity. The reason why we focus on relative, rather than absolute, income shocks (i.e., dividing the amount of the shock by a measure of liquidity such as income) flows directly from our conceptual framework in Table 1. Our argument in Table 1 was that in the bankruptcy context, an exogenous income shock should be more salient to households with low liquidity because those households can use the exogenous income shock to pay current expenses outstanding (e.g., debt) and thus avoid bankruptcy. On the other hand, in our context, the exogenous income shock should be less salient for households with high liquidity before the shock because they already have adequate liquidity to pay off current expenses. Our measure of the income shock divided by a measure of liquidity, RI_d, thus captures the relative effect of the shock across households with varying amounts of liquidity prior to the shock. Similar arguments in the bankruptcy context concerning the importance of evaluating the impact of the shock relative to household liquidity are made by Gross, Notowidigdo, and Wang (2014).

In our main *individual-level* empirical specifications, described in full in the next section, we are able to very accurately calculate *RI* across individual households. This is because we can observe both the number of residents in the household of each bankruptcy filer (thus allowing us to calculate the dollar magnitude of the income shock) and individual income statements (thus allowing us to measure the size of the exogenous income shock *relative* to the net income of

[4] Province fixed effects are dropped from this regression because they are perfectly collinear with DA fixed effects.

individual households). In this section, however, because our unit of observation is *counts of bankruptcy filers per DA*, our variation is across DAs rather than households. Thus, we cannot, by definition, examine variation at the level of individual household but rather have to use aggregate DA-level measures of average household size and average household income in the DA in our calculation of RI_d. The issue of losing the ability to observe the variation in treatment across individual households when aggregating households into geographic areas is common in this literature (e.g., Mian, Rao, and Sufi, 2013, who use zip code-level aggregation of households). Note that because we include DA-level fixed effects, we are not able to include the RI_d variable as a separate term because it is calculated based on DA-level data for household size and income. However, we include this variable in the specification without DA fixed effects (column (3) in Table 3).[5]

Given the structure of our data (i.e., bankruptcy counts per region) and identification strategy (i.e., based on the one-off Ralph bucks payment), it is important to note that we cannot distinguish whether receipt of the Ralph bucks income shock allowed a household to permanently avoid filing or whether the income shock merely caused the household to delay filing. This caveat comes from our inability to observe the same household over time and especially in the periods when the household does not file for bankruptcy. Gross, Notowidigdo, and Wang (2014) have similar data constraints and are unable to distinguish whether the effect of the income shock on bankruptcy filings is permanent or temporary.

Our main count data results are summarized in Table 3. Panel A of this table shows the estimated results of the simple difference-in-differences model with intensity of treatment presented in equation (1). Column (1) shows results for the whole sample. Columns (2) and (3) summarize results for the propensity score matched sample. While columns (1) and (2) include DA fixed effects, specification in column (3) drops these fixed effects and includes all DA-level control variables described in Table 2.

Results in Panel A of Table 3 show that there are no statistically significant pretreatment differences between the treatment group (Alberta) and the control group (the rest of Canada). Similar to Figure 1, this finding provides suggestive evidence to support the difference-in-

[5] In our main specification, we examine relative magnitudes by dividing the payment received by the DA average family income. In robustness checks (not reported), we find very similar results when examining absolute payments.

differences assumption that, without the Ralph bucks payment, bankruptcies in Alberta and the rest of Canada would evolve in a similar fashion (the parallel trends assumption). This panel also shows that there is a highly statistically significant negative effect of the exogenous income shock on bankruptcy counts per 1,000 population. The coefficient on the interaction of the post-treatment binary variable (equal to 1 for Q4:2005–Q4:2006) and the relative income shock (RI_d) shows that providing one unit of liquidity (relative income) reduces DA bankruptcies by -0.83 (using our preferred specification in column (2)). We illustrate the economic magnitude of this coefficient using the following example. In our sample, the average DA monthly family income is C$6,735. Thus, a Ralph bucks payment of C$400 is equal to a 5.9 percent liquidity injection (relative to the average family income). To estimate the effect of this additional liquidity on bankruptcy counts, we multiply 5.9 percent by -0.83 (the estimated effect of Ralph bucks payment on bankruptcy), which is equal to -0.049. Using this result, we can infer that C$400 would reduce DA bankruptcies per 1,000 persons by 0.049. This reduction in bankruptcies is a 7 percent decrease relative to 0.691 bankruptcies on average per 1,000 population per quarter in the sample.

5.2. Distributed Lag Specification

Because the Ralph bucks payment was announced in September 2005 (end of Q3:2005) and paid in January 2006 (Q1:2006), in our distributed lag specification, we define the pretreatment period as the first three quarters of 2005, the announcement period as the last quarter of 2005, and the disbursement periods as all quarters of 2006.

To measure the effect of the transfer over time, we follow the earlier literature (e.g., Agarwal, Liu, and Souleles, 2007; Agarwal and Qian, 2014) in defining a distributed lag, events study econometric model:

$$(2) \qquad Y_{dt} = \sum_{s=-1}^{4} \beta_s \times RI_d \times 1_{quarter\_s} + \alpha_d + \delta_t + \varepsilon_{dt},$$

where $1_{quarter\_s}$ is equal to 1 in the quarter s. The event time s is measured relative to the quarter of the transfer announcement (Q4:2005). Hence, time -1 is Q3:2005, time 0 is the quarter of the announcement (Q4:2005), time 1 is one quarter after announcement (Q1:2006), and so on. This

specification allows us to estimate the difference between the treatment and control groups before the treatment as β_{-1}. β_0 would capture the announcement effect and β_1 to β_4 the effect of disbursement over time (up to four quarters after disbursement). All these effects show changes in bankruptcy counts relative to the benchmark period, Q1:2005 – Q2:2005.

Panel B of Table 3 summarizes our estimation results for the distributed lag, events study model presented in equation (2). Similar to specifications in Panel A, we include the pretreatment indicator (equal to 1 in Q3:2005) interacted with the intensity of treatment variable to measure any pretreatment differences in the treatment and control groups.[6]

Our main finding in Panel B of Table 3 is that there is a significant negative effect of the transfer disbursement on DA bankruptcy counts per 1,000 people. In our preferred specification in column (2), the effect is negative and persistent in all quarters of 2006. It is also similar in magnitude to the effect estimated in Panel A. The finding that the effect of the Ralph bucks payment is present in all quarters of 2006 is consistent with the previous literature on bankruptcy filings, which finds that there are long and individually specific lags between income shocks and bankruptcy filings. For example, Hankins, Hoekstra, and Skiba (2011) consider up to five years after income shocks (lottery winnings) to capture the full effect of income shocks on bankruptcy filings. Another reason for long and variable lags between income shocks and bankruptcy filings is that a bankruptcy filing is a complicated and document-intensive process that usually takes several months to complete. The length of the process can depend on the individual filer's circumstances and debt obligations, document production and verification, and bankruptcy trustee workload.

In addition to our main finding, Panel B of Table 3 also shows that there is a negative effect of the transfer announcement on DA bankruptcy counts per 1,000 residents. However, in most specifications, the magnitude of this effect is approximately half the effect of disbursement. The finding that the announcement effect of the income transfer on bankruptcy counts is smaller than the disbursement effect is intuitive because marginal debtors close to filing for bankruptcy and who are most likely to benefit from the transfer are usually cut off from credit markets, have maxed-out credit cards, and have very few opportunities to borrow. Therefore, unlike other individuals, these credit-constrained debtors may have difficulties borrowing against future

[6] The exclusion of this indicator from the specification has no material effect on the main distributed lag terms.

income payments. This finding is roughly consistent with the previous literature (e.g., Agarwal and Qian, 2014) showing that credit-constrained individuals do not react to income payment announcements.

5.3. Electronic and Paper Bankruptcy Filings

The OSB provided us with two separate databases: the "count" database, used in this section, which includes a full count of every personal bankruptcy in Canada by year and by six-digit postal code and DA, and the "individual" database, used in the next section, which includes full balance sheet and income statement data from each individual electronic (but not paper) filing. Because our individual-level balance sheet data, used here, are limited to only electronic rather than paper filers, we need to provide evidence that there is no sample selection bias in our individual-level data.

While our individual-level balance sheet data are limited to only electronic filers, our count data used in this section allow us to observe the count of both electronic filers and paper filers in each DA. Thus, one simple test of possible selection bias from limiting our individual balance sheet data to only electronic filers is to replicate the count results reported in Table 3, where we used total DA counts of all filers, with new results using data that only reflect DA-level counts of electronic filers. We report these results in Table 4. Our main conclusion from the results in Table 4 is that they are qualitatively very similar to the results in Table 3 in terms of the exogenous shock significantly reducing the number of bankruptcy filers in the treatment group relative to the control group. The smaller coefficients in this table relative to the coefficients in Table 3 are due to the fact that electronic filings here constitute a portion of all filings in Table 3.[7] By construction, the number of electronic filings in any DA is smaller than or equal to the number of all filings (which include paper filings). Overall, these results for electronic bankruptcy counts, and their similarity to the results for all bankruptcy counts are one possible indication that we do not face issues of sample selection bias when examining our individual-level results in the next section.

In addition to these quantitative results in Table 4, there are a number of institutional reasons why limiting individual-level balance sheet data to electronic filings is not likely to bias

[7] The percentages of all filings made electronically were as follows: 62.2 percent in 2005 and 77.4 percent in 2006.

our results. In Canada, the choice of whether to file electronically or via paper to the bankruptcy regulator (the OSB) is completely the decision of the bankruptcy trustee rather than the individual filer. As previously described, a trustee is typically a professional accountant licensed by the OSB to be a trustee and who, as an officer of the court, is designed to be impartial between debtors and creditors. We thus argue that the trustee's choice of an electronic- or paper-filing mechanism should not have any relationship at all to the financial situation of the individual bankruptcy filer. Indeed, it is probable that filers are unaware of whether their trustee uses an electronic- or paper-filing system.

6. Hypothesis 2: Bankruptcy Balance Sheet Tests

This section provides the main results of this paper, which describe how individual households with heterogeneous balance sheets react to the exogenous income shock. While the previous section examined the impact of the exogenous shock on bankruptcy counts per 1,000 population, this section examines the impact of the exogenous shock on the household-level balance sheet characteristics of bankruptcy filers. The aim of this section is to evaluate the balance sheet characteristics of those few households that continued to file for bankruptcy (as documented previously) despite receiving the exogenous payment shock.

In this section, we test Hypothesis 2 developed in Section 2. This hypothesis states that receipt of the exogenous income shock should reduce filings from individuals whose BSBs of bankruptcy are small (whom we label marginal potential filers). As we explain in Section 2, without the income shock, low-BSB filers may be forced to file despite having low benefits if they are liquidity constrained. However, the income transfer may allow some of these low BSB filers to avoid filing because it can relieve their liquidity constraints. On the other hand, we hypothesize that filers with high BSBs from bankruptcy will not change their intention to file for bankruptcy despite receiving the exogenous income shock.

We are able to test this hypothesis in a difference-in-differences framework by comparing bankruptcy filers before and after the exogenous shock. As is common in this literature, while we can observe all bankruptcy filers, we cannot observe households that select not to file. Thus, in this section, our treated group is households that proceed to file despite having received the exogenous shock. As we describe in Section 2, our prediction is that the average BSB of those few households that proceed to file after receiving the income shock should be higher than the

control group of households that did not receive the income shock. Thus, our main empirical prediction is that there should be a positive coefficient on the exogenous payment term of the treated group, in a regression on the BSB of each filer, because those few households that continue to file despite receiving the positive income shock should have a higher level of BSBs compared with the control group.

6.1. Calculating Individual Balance Sheet Benefits of Bankruptcy

Bankruptcy law defines very exactly how different elements of the filers' balance sheets are dealt with in bankruptcy. In this section, we describe how we use our detailed individual-level balance sheet data to accurately calculate the exact net BSBs of bankruptcy under bankruptcy law for each filer in our database. This variable is our dependent variable in the regressions that follow.

Our bankruptcy balance sheet data are taken from OSB Form 79, which lists all assets and all liabilities of the bankruptcy filer. In particular, the data allow us to observe different classes of assets and liabilities (e.g., all secured and unsecured liabilities of different types). Furthermore, these data also include the current estimated market value of real estate and other assets as determined by the bankruptcy trustee.

These data allow us to use the formula of Fay, Hurst, and White (2002) to calculate the net BSBs of bankruptcy for each bankruptcy filer:

(3) $$BSB_i = \max[D_i - \max[W_i - E_i, 0], 0],$$

where D_i is unsecured liabilities of filers eliminated in bankruptcy, W_i is net wealth of bankruptcy filers, and E_i represents bankruptcy exemptions available to filers in a particular year and province. The formula states that the benefits of bankruptcy accrue from the unsecured debt discharged (D). The costs of bankruptcy are the liquidated net wealth (W) that must be paid to secured creditors net of the provincial exemption level (E). If, for example, net wealth (W) is less than the provincial exemption level (E), then no wealth is liquidated.

Our measure of unsecured debt D is taken directly from the data on OSB Form 79, which lists the amount of all unsecured debt (e.g., credit card) outstanding at the time of bankruptcy. Our measure of net wealth (W) is also taken directly from Form 79, which lists the bankruptcy trustee's current market valuation of all assets (e.g., the bankruptcy trustee's valuation of real estate assets, vehicle assets, securities, and the cash surrender value of insurance) and the value

of all secured debt outstanding (e.g., mortgage and car loans). Net wealth is the positive equity (current value of assets minus secured debt) the individual has in those assets, which will be liquidated and transferred to creditors under bankruptcy. All provincial bankruptcy exemptions (E) allowed in different provinces of Canada during our study period are included in our calculations of BSBs. These exemptions are typically related to particular assets such as principal residence, car, furniture, and pension accounts.

As previously described, the OSB ruled that the Ralph bucks cash transfer should be considered exempt from distribution to creditors. Thus, the cash from the exogenous payment should not affect BSBs. That is, in terms of the BSB equation just given, both W and E will increase by the size of the payment, and thus the net effect of the payment on BSBs, (W-E), will be zero. OSB Form 79 lists cash on hand as either exempt (from creditors) or nonexempt; thus, we only include nonexempt cash as an asset in our calculation of BSBs.

In Figure 2, we plot the cumulative distributions of BSBs in Alberta before (Q1:2005) and after (Q1:2006) the transfer disbursement. This figure shows that the treated cumulative distribution after treatment is below the treated cumulative distribution before treatment. In the next section, we provide more rigorous estimates of the magnitude by which the exogenous income payment affected the average level of BSBs in the treatment and control groups.

6.2. Difference-in-Differences Specification

Our empirical methodology in this section follows closely the empirical specification in the bankruptcy count section. Similar to equation (1), we consider pretreatment (Q3:2005), announcement (Q4:2005), and post-treatment periods (Q1:2006–Q4:2006) and estimate the following equation:

$$Y_i = \beta_{pre} \times RI_i \times 1_{pre} + \beta_{ann} \times RI_i \times 1_{ann} + \beta_{post} \times RI_i \times 1_{post} + \gamma RI_i + \theta X_i + \alpha_d + \delta_t + \varepsilon_i , \quad (4)$$

In this and the following equations, index *i* denotes the individual filer. These tests rely on individual-level data, not DA-level data, and use a cross-section of filers, not a panel dataset. These data are cross-sectional because all filers in the two-year study period file for bankruptcy only once. However, because these debtors file in different quarters, we can examine how the characteristics of filers change over time before and after the transfer announcement and payment. The dependent variable Y in these specifications is the BSBs of bankruptcy, which is calculated as described in equation (3).

Similar to Section 5.1, the *RI* variable in equation (4) captures the size of the income shock relative to the liquidity of the household. This design is based on the argument, developed in Table 1, that in our bankruptcy context, the income shock is more important for low-liquidity households because it allows them to pay current debts, thus avoiding bankruptcy. Similarly, the income shock will have less of an impact on high-liquidity households in our bankruptcy context that already have adequate liquidity to pay current debts.

An important advantage of the individual-level data used in this section, relative to the DA-level count data in the previous section, is that they allow us to observe very detailed data on both the size of the income shock received across households and the detailed measures of household liquidity taken from the income statements of bankruptcy filings. Our bankruptcy data allow us to observe exactly the household size for every bankruptcy filer. Thus, we can calculate the size of Ralph bucks shock as C$400 × Household size. The OSB provides data on all income and expenses of every bankrupt household on OSB Form 65. We thus use the OSB measure of discretionary family income as the denominator of RI. The discretionary family income variable is defined as total monthly income minus nondiscretionary monthly expenses (e.g., spousal and child support payments, court fines, medical condition expenses). The RI term is thus defined as C$400 × Household size/Discretionary family income across all bankruptcy filing households in Alberta. It is equal to zero for filers outside of Alberta who did not receive the payment.[8]

Our individual data results are summarized in Table 6. Similar to Table 3, we report a simple difference-in-differences specification in Panel A and a distributed lag, events study model in Panel B. We present results for the whole sample (column (1)) and the propensity score–matched sample (columns 2 and 3). While columns (1) and (2) include DA fixed effects, column (3) includes only DA control variables described in Table 5. All specifications include individual control variables listed in Table 5. We also cluster standard errors at the DA level and include DA (or province) and year-months fixed effects in all regressions.

[8] These income and expenses data are recorded at the specific time of the bankruptcy filing and reflect monthly income and expenses in the period immediately before the bankruptcy filing. Our data include all monthly income from various sources (e.g., employment, pension, spousal support, social assistance) and all monthly expenses of various kinds (e.g., childcare, health expenses, rent, taxes, food, transport). All of these amounts are determined by the bankruptcy trustee rather than by the bankruptcy filer. Importantly, for bankruptcy decisions, the expense data do not include payments on debts that are discharged in bankruptcy.

Our major finding in Table 6 is that the BSBs of bankruptcy filers who proceed to file despite receipt of the liquidity shock are higher than the BSBs of filers in the control group of filers. Panel A of Table 6 indicates that, on average, the BSBs in the treatment group are C$1,247 higher than BSBs in the control group (using our preferred specification in column (2)). This effect is economically and statistically significant across different specifications and samples. The coefficient implies that providing liquidity equal to one month of discretionary family income (our definition of relative income) to borrowers close to filing for bankruptcy persuades some of them with low BSBs to avoid filing, thus increasing the average BSBs of the remaining filers by C$1,247.

6.3. Distributed Lag Specification

Using the same individual-level data as just given, we can estimate the distributed lag version of the above difference-in-differences model to examine quarter-by-quarter effects. The individual-level specification equivalent to equation (2) is as follows:

$$Y_i = \sum_{s=-1}^{4} \beta_s \times RI_i \times 1_{quarter\_s} + \gamma RI_i + \theta X_i + \alpha_d + \delta_t + \varepsilon_{it}. \quad (5)$$

Panel B of Table 6 reports results from the distributed lag model specifications as in equation (5). The specifications in this panel allow us to disentangle the total effect of the payment into the announcement and disbursement effects. Results in Panel B indicate that there was no statistically significant announcement effect of the Ralph bucks payment. This finding is consistent with our earlier finding that the announcement effect in bankruptcy counts was relatively small.

The distributed lag model coefficients for Q1:2006 – Q4:2006 in Table 6 indicate that the largest and statistically significant effects of the transfer are concentrated in the second and fourth quarters after Ralph bucks disbursement. This finding corresponds well with our earlier results on bankruptcy counts indicating that bankruptcies declined in response to the payment throughout 2006. The economic magnitudes implied by the coefficients of the distributed lag model are similar to the effects in the difference-in-differences model and are between a $C1,200 to C$1,600 increase in BSB, on average.

7. Conclusion

There is little agreement in the recent literature on the causes of consumer defaults. Various papers emphasize how balance sheet or income statements may affect this consumer decision. We follow the idea of Elul et al. (2010) and empirically test if both of these channels can interact to cause household bankruptcy. Our paper contributes to the literature by providing causal evidence on this question using a plausibly exogenous income shock and rich data on balance sheets and income statements of bankruptcy filers in Canada.

Our methodology exploits an exogenous, politically driven government cash payment provided in one but not any other Canadian provinces. Because the payment was made to each individual across households, we can observe exogenous variation in the magnitude of the payment received across households. We use a unique database that allows us to observe the complete balance sheet of every Canadian bankruptcy e-filer, thus allowing us to calculate the net BSBs received by each bankruptcy filer, as defined by bankruptcy law. Our main empirical conclusion is that the exogenous income shocks cause potential bankruptcy filers with lower BSBs from bankruptcy to select out of bankruptcy following the exogenous income shock.

Our main contribution, therefore, is to provide causal empirical evidence that both balance sheets and income statements matter in the household decision to file for bankruptcy following exogenous income shocks. We show that the positive income shock from the Ralph bucks payment can work through the income statements of households to reduce liquidity constraints and motivate them to select out of bankruptcy. In addition, we observe that those selecting out of bankruptcy have lower BSBs of filing. These two findings together imply that both income statements and balance sheets are important in the bankruptcy decision.

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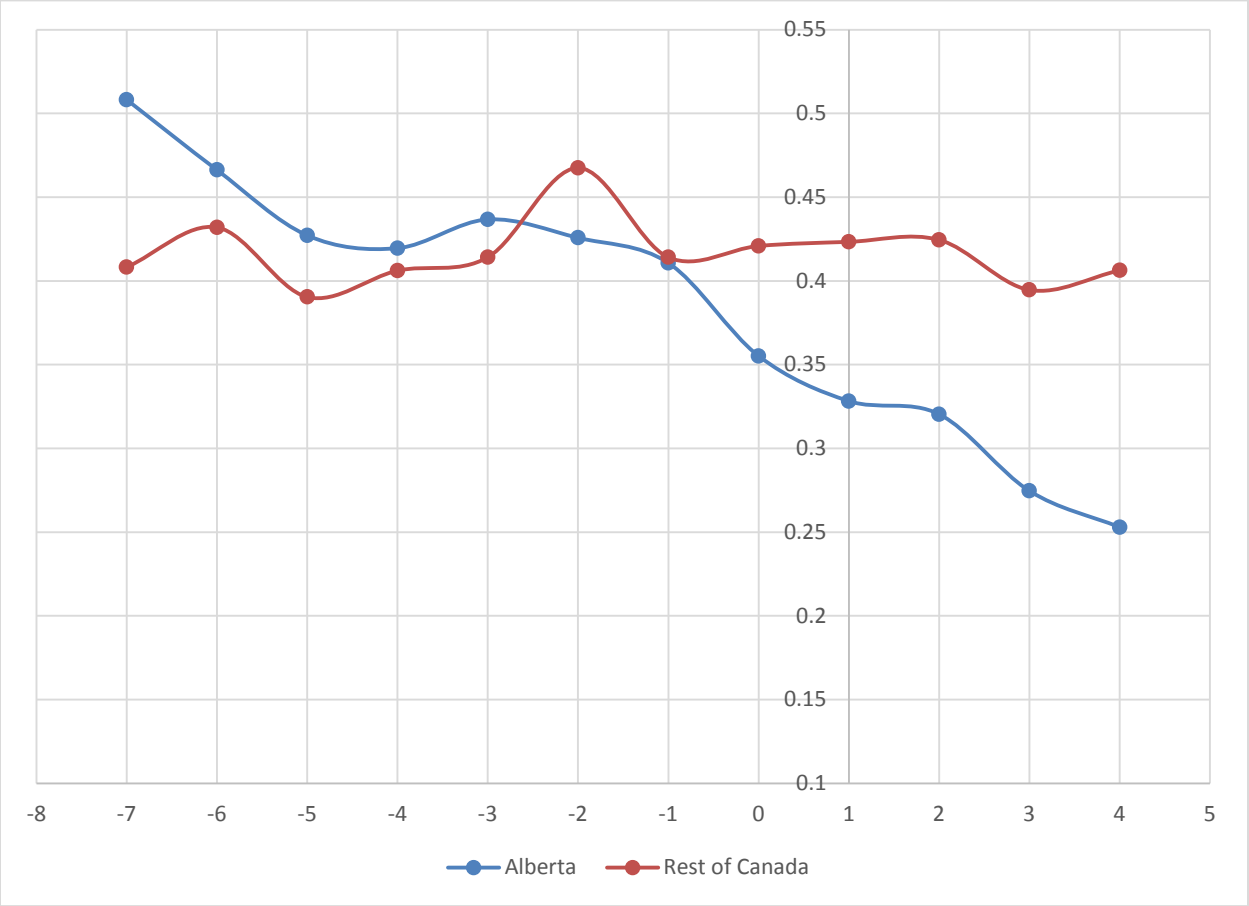
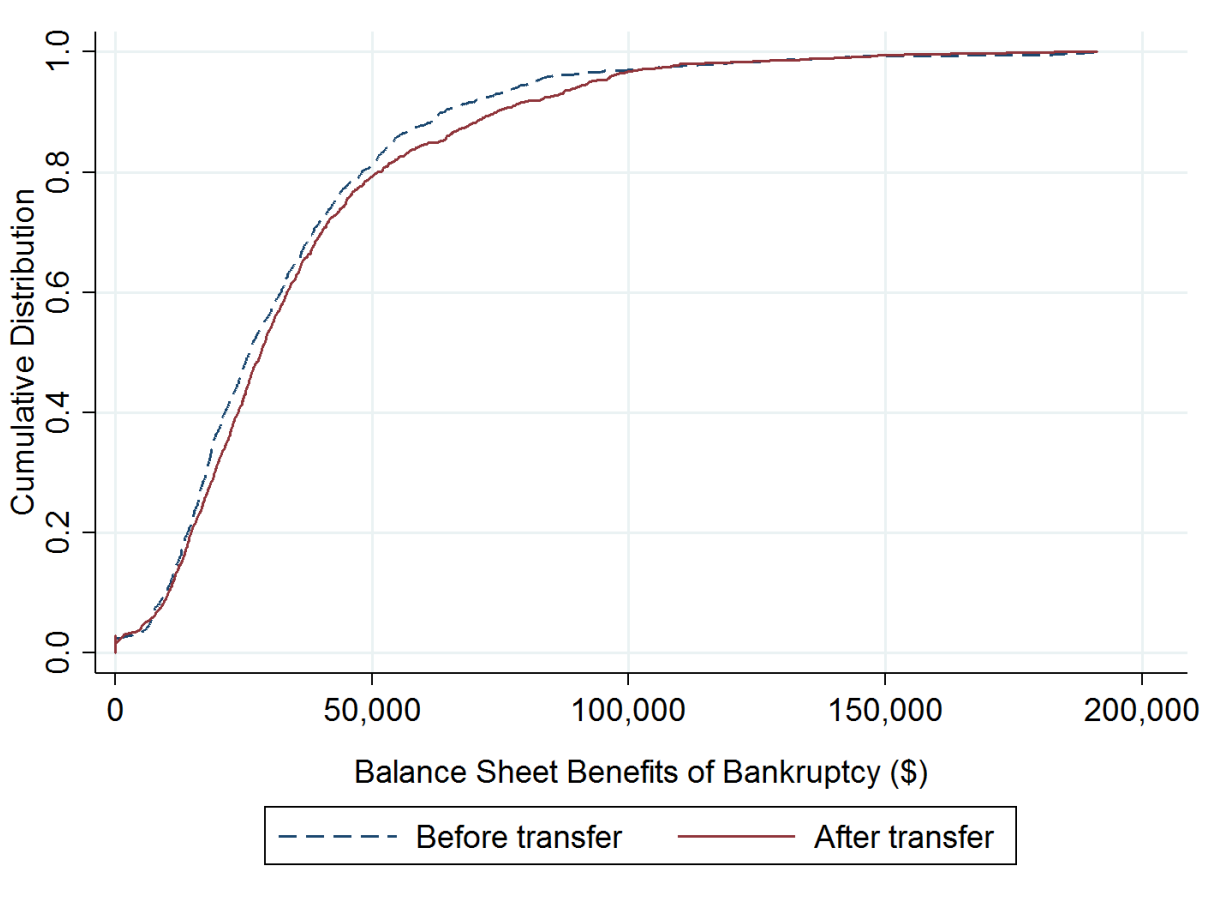
Figure 1. Average Quarterly Bankruptcies per DA in Alberta and the Rest of Canada (Time 0 — Announcement, Time 1 — Disbursement)



Notes: This figure shows the average number of bankruptcy filings per DA in the treatment group (Alberta) and the control group (DAs from the rest of Canada) before and after the transfer announcement (time 0) and payment (time 1). The data consist of quarterly bankruptcy counts in 2004 to 2006. Time -7 corresponds to Q1:2004 and time 4 to Q4:2006. This figure suggests parallel trends in bankruptcy filings in the treatment and control groups before the transfer. It also suggests that bankruptcies in the treatment group declined after the transfer payment.

Figure 2. Impact of the Exogenous Income Shock on Balance Sheet Benefits



Notes: Benefits are constrained to be below $200,000 to avoid very long right tails. For this figure, we compare filers in Alberta Q1:2005 (before treatment) and Alberta Q1:2006 (after treatment). These two distributions are different from each other according to a Kolmogorov-Smirnov test. This test rejects the null hypothesis that the distributions are equal with a p-value of 0.043.

Table 2. Summary Statistics Before and After Propensity Score Matching

	Not Matched Treatment Group		**Not Matched Control Group**		
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Difference**
Q1:2005 bankruptcy count	0.715	1.377	0.702	1.467	0.01
Lagged neighborhood bankruptcy	8.56	8.59	7.33	8.84	1.23***
Annual median income (DA)	29,489	8,862	27,001	8,900	2,488***
Region's urbanization (1–8)	1.880	1.600	1.912	1.604	-0.03
Unemployment rate	4.25	4.02	6.93	6.14	-2.67***
Numerical literacy	276.29	11.84	267.60	13.99	8.69***
Proportion of DA population:					
Divorced (DA)	0.082	0.034	0.076	0.036	0.006***
Separated (DA)	0.029	0.016	0.032	0.019	-0.003***
Homeowners (DA)	0.737	0.251	0.738	0.254	-0.001
Age 40–64 years (DA)	0.341	0.069	0.361	0.062	-0.020***
Age older than 65 years (DA)	0.115	0.093	0.142	0.093	-0.027***
College (DA)	0.199	0.069	0.187	0.074	0.012***
University (DA)	0.241	0.160	0.247	0.164	-0.006**
Males (DA)	0.499	0.033	0.488	0.032	0.012***
Observations	4,195		30,579		
	Matched Treatment Group		Matched Control Group		
	Mean	Std. Dev.	Mean	Std. Dev.	Difference
Q1:2005 bankruptcy count	0.715	1.377	0.771	1.653	-0.056*
Lagged neighborhood bankruptcy	8.56	8.59	8.51	12.09	0.048
Annual median income (DA)	29,489	8,862	29,308	8,662	180.7
Region's urbanization (1–8)	1.880	1.600	1.83	1.57	0.047
Unemployment rate	4.25	4.02	4.18	3.65	0.079
Numerical literacy	276.29	11.84	276.46	13.35	-0.174
Proportion of DA population:					
Divorced (DA)	0.082	0.034	0.082	0.035	-0.000
Separated (DA)	0.029	0.016	0.029	0.016	0.000
Homeowners (DA)	0.737	0.251	0.740	0.253	-0.003
Age 40–64 years (DA)	0.341	0.069	0.343	0.064	-0.003*
Age older than 65 years (DA)	0.115	0.093	0.116	0.080	-0.001
College (DA)	0.199	0.069	0.198	0.076	0.001
University (DA)	0.241	0.160	0.241	0.161	0.000
Males (DA)	0.499	0.033	0.499	0.032	0.000
Observations	4,195		4,195		

Table 3. The Effect of the Exogenous Payment on Bankruptcy Counts

	(1) Whole Sample	(2) PSM Matched	(3) PSM Matched
Panel A. Difference-in-Differences Specification			
Pretreatment × Relative income	-0.007	0.115	0.115
	(0.155)	(0.201)	(0.188)
Announcement × Relative income	-0.522***	-0.395**	-0.395**
	(0.151)	(0.194)	(0.182)
Post-treatment × Relative income	-0.971***	-0.825***	-0.822***
	(0.110)	(0.143)	(0.133)
Observations	281,302	66,502	66,304
R squared	0.382	0.422	0.214
Panel B. Distributed Lag Model Specification			
Q3:2005 × Relative income	-0.007	0.115	0.115
	(0.155)	(0.201)	(0.188)
Q4:2005 × Relative income	-0.522***	-0.395**	-0.395**
	(0.151)	(0.194)	(0.182)
Q1:2006 × Relative income	-0.786***	-0.698***	-0.696***
	(0.149)	(0.196)	(0.183)
Q2:2006 × Relative income	-0.880***	-0.781***	-0.779***
	(0.148)	(0.193)	(0.181)
Q3:2006 × Relative income	-0.946***	-0.687***	-0.685***
	(0.141)	(0.187)	(0.174)
Q4:2006 × Relative income	-1.272***	-1.132***	-1.130***
	(0.142)	(0.187)	(0.175)
Observations	281,302	66,502	66,304
R squared	0.382	0.423	0.214
DA fixed effects	Y	Y	N
Controls	N	N	Y

Notes: This table shows the response of DA bankruptcy counts to the Ralph bucks payment. A negative coefficient implies a reduction in bankruptcies following the transfer payment. Panel A summarizes the results for the specification in equation (1), and Panel B presents results for equation (2). The data consist of DA-level quarterly bankruptcy counts per 1,000 persons in 2005 and 2006. Pretreatment and Announcement are indicator variables equal to 1 in Q3:2005 and Q4:2005, respectively. Post-treatment is an indicator variable equal to 1 in Q4:2005 to Q4:2006. Relative income is equal to $400 × Average household size/Monthly family income in Alberta and is 0 otherwise. All specifications include calendar quarter fixed effects. Table 2 describes control variables that are included in column (3). Propensity score matched sample is constructed as described in the text. All specifications are estimated using ordinary least squares. Standard errors are clustered at the DA level and reported in parentheses. *** indicates significance at 1%, ** indicates significance at 5%, and * indicates significance at 10%.

Table 4. The Effect of the Exogenous Payment on Electronic Bankruptcy Counts

	(1) Whole Sample	(2) PSM Matched	(3) PSM Matched
Panel A. Difference-in-Differences Specification			
Pretreatment × Relative income	0.177	0.243	0.243
	(0.126)	(0.165)	(0.154)
Announcement × Relative income	-0.083	0.015	0.015
	(0.125)	(0.162)	(0.151)
Post-treatment × Relative income	-0.441***	-0.411***	-0.408***
	(0.092)	(0.123)	(0.115)
Observations	281,302	66,502	66,304
R squared	0.357	0.367	0.164
Panel B. Distributed Lag Model Specification			
Q3:2005 × Relative income	0.177	0.243	0.243
	(0.126)	(0.165)	(0.154)
Q4:2005 × Relative income	-0.083	0.015	0.015
	(0.125)	(0.162)	(0.151)
Q1:2006 × Relative income	-0.355***	-0.401**	-0.398**
	(0.130)	(0.173)	(0.162)
Q2:2006 × Relative income	-0.449***	-0.425**	-0.422***
	(0.129)	(0.171)	(0.160)
Q3:2006 × Relative income	-0.315**	-0.167	-0.164
	(0.127)	(0.170)	(0.159)
Q4:2006 × Relative income	-0.644***	-0.652***	-0.649***
	(0.121)	(0.165)	(0.154)
Observations	281,302	66,304	66,304
R squared	0.357	0.366	0.164
DA fixed effects	Y	Y	N
Controls	N	N	Y

Notes: This table shows the response of DA electronic bankruptcy counts to the Ralph bucks payment. A negative coefficient implies a reduction in bankruptcies following the transfer payment. Panel A summarizes the results for the specification in equation (1), and Panel B presents results for equation (2). The data consist of DA-level quarterly electronic bankruptcy counts per 1,000 persons in 2005 and 2006. Pretreatment and Announcement are indicator variables equal to 1 in Q3:2005 and Q4:2005, respectively. Post-treatment is an indicator variable equal to 1 in Q4:2005 – Q4:2006. Relative income is equal to $400 × Average household size/Monthly family income in Alberta and is 0 otherwise. All specifications include year-quarter fixed effects. Table 2 describes control variables that are included in column (3). Propensity score matched sample is constructed as described in the text. All specifications are estimated using ordinary least squares. Standard errors are clustered at the DA level and reported in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 5. Summary Statistics of OSB Individual Filer Data and Merged Neighborhood Data

	Matched Treatment Group		Matched Control Group	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
OSB Individual Filer Data				
Financial benefits ($)	37,928	40,540	43,233	54,894
Age (years)	41.7	13.2	42.0	12.8
Age squared	1909.8	1225.8	1933.1	1202.0
Self-employment (dummy)	0.057	0.232	0.064	0.245
Divorce (dummy)	0.157	0.364	0.135	0.341
Prior defaults (dummy)	0.158	0.365	0.136	0.343
Reasons for Financial Distress				
Overuse of credit	0.567	0.495	0.643	0.479
Insufficient income	0.345	0.475	0.334	0.472
Health concerns	0.257	0.437	0.216	0.411
Unemployment	0.257	0.437	0.276	0.447
Marital breakdown	0.222	0.415	0.192	0.394
Business failure	0.105	0.307	0.110	0.313
Supporting relatives	0.080	0.272	0.058	0.234
Tax liabilities	0.053	0.224	0.067	0.250
Moving or relocation	0.043	0.202	0.025	0.155
Substance abuse	0.035	0.184	0.026	0.159
Gambling	0.041	0.199	0.025	0.157
Accidents or emergencies	0.034	0.180	0.028	0.164
Legal action	0.023	0.149	0.017	0.129
Loans to friends	0.020	0.139	0.014	0.116
Garnishee	0.017	0.129	0.016	0.124
Bad or poor investments	0.013	0.114	0.013	0.114
Student loans	0.005	0.071	0.007	0.085
Neighborhood-Level Data (Based on Filer's DA)				
Lagged neighborhood bankruptcy	5.1	11.2	6.1	13.4
Annual median income (DA) ($)	27,866.5	6,788.4	26,695.9	7,788.0
Unemployment rate (percent)	4.6	4.2	6.9	6.6
Numerical literacy (score from 100 to 500)	274.5	11.2	274.1	14.9
Region's urbanization (1–8)	1.831	1.554	1.906	1.533
Males (DA)	0.499	0.033	0.493	0.035
College (DA) (proportion of DA population)	0.196	0.064	0.197	0.077
University (proportion of DA population)	0.212	0.137	0.208	0.150
Observations	8,081		9,418	

Table 6. The Transfer Effect on Balance Sheet Benefits of Bankruptcy Filers

	(1) Whole Sample	(2) PSM Matched	(3) PSM Matched
Panel A. Difference-in-Differences Specification			
Pretreatment × Relative income	1,901	967	1,658
	(2,437)	(2,521)	(1,556)
Announcement × Relative income	3,995	6,256	6,224*
	(5,691)	(5,389)	(3,491)
Post-treatment × Relative income	1,120**	1,247***	1,213***
	(478)	(477)	(303)
Observations	77,498	17,096	17,096
R squared	0.48	0.458	0.123
Panel B. Distributed Lag Model Specification			
Q3:2005 × Relative income	1,800	919	1,642
	(2,432)	(2,522)	(1,558)
Q4:2005 × Relative income	3,902	6,199	6,213*
	(5,736)	(5,418)	(3,495)
Q1:2006 × Relative income	567	1,322	851
	(1,010)	(1,005)	(909)
Q2:2006 × Relative income	1,397**	1,175**	1,379***
	(576)	(575)	(512)
Q3:2006 × Relative income	-1,048	-699	1,023
	(2,808)	(3,117)	(2,550)
Q4:2006 × Relative income	1,328**	1,559***	1,192***
	(667)	(574)	(241)
Observations	77,498	17,096	17,096
R squared	0.48	0.458	0.123
DA fixed effects	Y	Y	N
DA Controls	N	N	Y

Notes: This table shows the response of balance sheet benefits of bankruptcy filers to the Ralph bucks payment. A positive coefficient implies an increase in benefits and may show that filers with smaller benefits are not filing for bankruptcy after the transfer. Panel A summarizes the results for the specification in equation (4), and Panel B presents results for equation (5). The data consist of individual bankruptcy files in 2005 and 2006. Pretreatment and Announcement are indicator variables equal to 1 in Q3:2005 and Q4:2005, respectively. Post-treatment is an indicator variable equal to 1 in Q4:2005 to Q4:2006. Relative income is equal to \$400 × Household size/Discretionary family income in Alberta and is 0 otherwise. All specifications include calendar month fixed effects and individual control variables described in Table 5. Propensity score matched sample is constructed as described in the text. Ordinary least squares used in all specifications. Standard errors are clustered at the DA level and reported in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

The Causes of Household Bankruptcy:
The Interaction of Income Shocks and Balance Sheets

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Appendix (for Online Publication)

A1. Individual Filers' Control Variables

Our individual-level OSB filing data provide us with a variety of demographic variables, including individual-level data on each filer's age, marital status (specifically divorce), household size, self-employment status, and prior insolvency. A unique element of our filer-level OSB data is our ability to capture the reasons given by filers for their financial distress. OSB Form 79 includes responses to the following open-ended question: "Give reasons for your financial distress." Our data include the full textual responses to this question from every filer in our database, and textual analysis software was used to code these responses into 17 separate categories (listed in Table 5). Each category is represented by a dummy variable, with multiple responses allowed per filer.

Our count data allow us to capture counts of *past* bankruptcies in the individual's postal code. The bankruptcy literature (e.g., Gross and Souleles, 2002; Fay, Hurst, and White, 2002; Livshits, MacGee, and Tertilt, 2010; White, 2011; Scholnick, 2014) has argued that past bankruptcies can influence contemporaneous bankruptcies because of reductions in bankruptcy stigma and/or information spillovers that these past bankruptcies generate. To control for this, we measure bankruptcy counts for each DA in the five-year period 2000 to 2004. This period falls before the data used in our main specification, 2005 to 2008.

A large literature links issues such as bankruptcy with levels of financial literacy and numeracy (e.g., Lusardi, 2012, among many others). We use a measure of numerical literacy, available at the DA level, as a control variable. Our numerical literacy data were developed by Murray (2011) and are computed using the 2003 International Adult Literacy and Skills Survey (IALSS) and the 2006 Canadian Census. IALSS evaluated numerical skills for a very large sample of the Canadian population. The average level of numerical literacy for each DA was estimated based on the demographic characteristics of that DA. This measure is included among our individual control variables described in Table 5.

Table A1. Propensity Score Matching Regression Results

	Coefficient	SE
Lagged neighborhood bankruptcy	0.018***	(0.002)
Annual median income (DA)	0.000***	(0.000)
Region's urbanization dummies:		
1	0.576*	(0.300)
2	0.864***	(0.306)
3	0.035	(0.305)
4	-0.516	(0.327)
5	0.560*	(0.307)
6	0.992***	(0.305)
Unemployment rate	-0.090***	(0.005)
Numerical literacy	0.068***	(0.002)
Proportion of DA population:		
Divorced (DA)	8.677***	(0.657)
Separated (DA)	-18.043***	(1.310)
Homeowners (DA)	0.271**	(0.105)
Age 40–64 years (DA)	-7.926***	(0.330)
Age older than 65 years (DA)	0.988***	(0.268)
College (DA)	-1.843***	(0.281)
University (DA)	-4.042***	(0.183)
Males (DA)	13.867***	(0.677)
Constant	-24.322***	(0.756)
Observations	34,728	
Pseudo R-squared	0.173	

Notes: This table shows the propensity score matching logistic regression's estimates. The dependent variable is equal to 1 for Alberta's DAs (treated) and equal zero for DAs in the rest of Canada (control). The data consist of all DAs in Canada in Q1:2005 (before the transfer was announced). Standard errors are in parentheses. *** indicates significance at 1%, ** indicates significance at 5%, and * indicates significance at 10%.