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Banks' Funding Stress, Lending Supply and Consumption Expenditure

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Abstract

We employ a unique identification strategy linking survey data on household consumption expenditure to bank-level data to estimate the effects of bank funding stress on consumer credit and consumption expenditures. We show that households whose banks were more exposed to funding shocks report lower levels of non-mortgage liabilities. This, however, only translates into lower levels of consumption for low income households. Hence, adverse credit supply shocks are associated with significant heterogeneous effects.

Keywords: credit supply, banking, financial crisis, consumption expenditure, liquid assets, consumption smoothing

JEL classification: E21, E44, G01, G21

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This paper studies the effects of bank funding stress on household consumption. If banks' inability to access funding adversely affects household consumption due to, for instance, a shift in credit supply, this may have first-order macroeconomic consequences that would exacerbate the real effects of disruptions in the banking sector.

We examine the question using Canadian data for the period 2005-2009. Aggregate Canadian data suggest that there was a noticeable dip in household credit growth, in consumption and, even more substantially, in durable consumption in 2008/2009 relative to 2007, with a subsequent (weak) recovery in 2010 (Figure 1). This is despite the fact that Canadian banks were by all accounts only relatively mildly affected by the U.S. financial crisis (for an example, see Ratnovski and Huang (2009)). Attempting to distinguish how much of this dip is due to households reducing their *demand* for consumer credit and consumption in the face of the financial crisis versus banks reducing *consumer loan supply*, resulting in a supply driven contraction of consumption, is the challenging identification problem we face in this paper. The data we use offer three distinct advantages in meeting this challenge. First, they provide detailed information on a large set of Canadian households, not only for assets and liabilities, but also for consumption expenditures. Second, the data establish a clean link between households and their main bank, which in turn can be linked to the bank's balance sheet. Third, we have access to confidential bank-level data on exposures to the U.S. interbank market, which we assume is exogenous to household behavior. We use this information to distinguish banks with high exposure to the United States (referred to as "exposed banks") from those with low or no exposure (referred to as "unexposed banks"). We document that, compared to unexposed banks, exposed banks reduced their loan supply significantly more in 2008/2009, exposing their customers to an adverse credit shock.

[INSERT FIGURE 1 HERE]

Using this identification setup, we document a statistically and economically significant reduction in the non-mortgage credit supply of exposed banks to households. This reduction in credit resulted in a significant contraction in consumption expenditure of low income households. High income households maintained their consumption levels even when faced with an adverse credit supply shock. When we aggregate the estimated consumption expenditure effects the adverse supply shock explains more than half of the aggregate dip in consumption expenditure in Canada in Figure 1. In addition, the results point towards significant heterogeneity in the impact of bank funding stress across the income distribution. We then examine a number of robustness checks and alternative explanations for our findings. For example, we delineate the effects from a household balance sheet channel in the spirit of Mian et al. (2013) and Keys et al. (2014), in which households faced with a decline in home values and a corresponding decline in their net worth reduced their consumption expenditure.

The paper links the literature on credit frictions and consumption with that on the real effects of finance. Most consistent with our findings is the mechanism in Bernanke and Lown (1991), who suggest that adverse liquidity shocks may cause capacity constraints in bank lending due to an imperfectly elastic market for bank liabilities during a crisis. Banks faced with such a shock pass it on to their customers by shifting their credit supply curve inward. In the presence of such frictions, adverse shocks to bank funding may result in reductions in lending supply, which in turn may ultimately affect household expenditures. Empirically, the paper closest to ours is Jensen and Johannesen (2017), who document an overall effect on consumption expenditure of Danish households in the last crisis.

Several authors have investigated related questions using variation from quasi-natural experiments, much like this paper, but outside of a financial crisis environment. For example, Agarwal et al. (2007) study tax refunds and show that consumers first pay down debt and then increase spending. Gross and Souleles (2002) investigate an exogenous change in the credit limit for credit cards and find that households tend to spend more in response to this change. Leth-Petersen (2010) shows that credit-constrained households increased consumption in response to a credit market reform in Denmark that gave households access to housing equity as collateral for consumption loans. Abdallah and Lastrapes (2012) use a constitutional amendment in Texas that relaxed restrictions on home equity lending to identify the effect of credit constraints on consumption expenditure. They find significant positive effects on consumption, suggesting the presence of credit constraints repressing consumption before the amendment. Agarwal et al. (2015) study the impact of a credit tightening policy on consumer spending in Turkey between January 2010 and August 2013. They find a significant reduction in the spending of highly indebted consumers, which persisted for many months after the policy's implementation.

Our findings complement the literature on the impact of income shocks on consumer expenditure. This literature (for recent surveys see Jappelli and Pistaferri (2010) and Meghir and Pistaferri (2011)) can be interpreted to suggest that permanent and temporary changes in credit supply to households would have different effects on consumption expenditure. In particular, as long as households expect credit conditions to improve in the foreseeable future, they may offset a decline in credit supply through drawing down liquid assets in order to maintain consumption. This is consistent with our results.[1] It should also be noted that the consumption smoothing effect relies on these households simultaneously carrying debt and holding liquid assets. Although puzzling upon first glance, this behavior has been frequently observed in the literature (for example, by Gross and Souleles (2002)). As an explanation, Telyukova and Wright (2008) argue that households simultaneously carry debt and hold liquid assets since some unexpected expenses cannot be paid for by credit. Therefore, holding liquid assets, even at the expense of carrying some debt, can loosely be considered a type of precautionary savings. In any event, we do not examine this question empirically in this paper, but emphasize that the patterns observed in our data correspond closely with data in other household surveys in this regard.

[1] Agarwal and Qian (2014) examine an unanticipated temporary income shock and show that the consumption of households with low liquid assets respond more strongly compared to those with high liquid assets. Di Maggio et al. (2014) examine the effects of expansionary monetary policy on household consumption, which they translate into an anticipated reduction in monthly interest payments on credit. They also show significant heterogeneity in the marginal elasticity to consume out of the lower interest expenditure.

Our results also add to recent findings on the effect of adverse lending supply shocks on *firms*. While the supply of bank credit to firms declined during the recent crisis (Cohen-Cole et al. (2008)), this decline was less pronounced for banks with a larger reliance on retail deposits (Ivashina and Scharfstein (2010)). Furthermore, banks that incurred larger subprime losses charged their corporate borrowers higher loan rates (Santos (2011)). Temporary contractions of lending supply, very much similar to those observed in this paper, also tend to affect investment spending and employment by firms. For example, Chodorow-Reich (2014) shows that firms borrowing from fragile banks were less likely to obtain a loan during the crisis and they also had greater reductions in employment. Puri et al. (2011) show that banks with a larger exposure to the recent financial crisis reduced credit to firms by a larger amount.[2] Finally, Paravisini et al. (2015) use Peruvian banks' reliance on foreign funding to identify credit supply shocks during the recent financial crisis, in a manner very similar to ours. They conclude that such capital flow-driven credit shortages had a negative impact on the supply of exports. Since these studies do not find any evidence of a mitigating "dissaving" behavior of firms, our paper shows that credit supply shocks and funding stress affect firms and households quite differently.

1 Data

1.1 Data Sources

In order to go beyond mere correlations between variables and to establish a causal link, it is necessary to relate exogenous variation in banks' lending to household consumption. Hence, one needs data that (i) capture adverse shocks to bank balance sheets that affect loan supply, but are exogenous from the perspective of the customers of the bank, (ii) identify variation in these shocks across banks, (iii) provide detailed information on household characteristics, banking habits, and consumption patterns, and (iv) link household information to bank

[2] Earlier contributions to the literature on the effect of lending supply shocks include Peek and Rosengren (2000) and Peek et al. (2003).

information. Our data meet all of these requirements.

Aggregate data from Statistics Canada (Figure 1) suggest that there was a significant decline in consumption in 2008/2009 relative to 2007, especially for durable goods, with a subsequent recovery in 2009 and 2010. Furthermore, there is a notable decline in the growth rate of household credit. After peaking at about 3% in 2007, the growth rate fell sharply to about 1.5% by late-2008 and remained at about 1.5-2% for the rest of the period. We access two data sets that link quarterly detailed bank balance-sheet information of Canadian banks to Canadian household survey data on consumption. In particular, our first data set contains detailed information on the geographic source of wholesale funding of banks, including the extent to which they rely on interbank deposits from the United States. We interpret such U.S.-based interbank deposits as money market funding. For Canadian banks, our data come from regulatory returns filed by all federally chartered banks, including a quarterly return that shows the geographical origin of certain assets and liabilities. We use this confidential return to extract information on interbank deposits from the United States. For credit unions, the relevant data come from annual reports or provincial regulators (since all Canadian credit unions were provincially-regulated during our sample period).

Our second data set is a household survey that contains detailed information on durable and non-durable consumption, households' assets and liabilities, as well as information about the identity of the household's main bank. The data come from the *Canadian Financial Monitor* (CFM) survey, which has been conducted annually since 1999 by Ipsos Reid Canada.[3] The CFM targets an annual sample of 12,000 households stratified such that it is representative of the Canadian population with respect to household size, age, income, size of the city they live in, homeownership status (own/rent), employment status, and the province of residence. We can construct a panel since some past participants are reinterviewed. The CFM also covers detailed demographic information, such as household composition, age, household income, and employment status. Finally, the survey contains information on the

[3]The data set has been used in previous research, for example by Foerster et al. (2017).

assets and liabilities of households, including non-mortgage credit and liquid assets.

The CFM, like most surveys of household assets and liabilities, oversamples wealthy households. Limiting the sample to households in the panel, rather than the overall CFM sample, further enhances this effect (see Online Appendix Section A.2). While this allows a closer match of the survey data with aggregate assets and liabilities in the economy, it also results in the under-representation of low income, low asset households. To address this issue, we adjust our consumption-related findings by using population weights for different income groups when we calculate a macro effect of credit supply on consumption.

Linked together, these data sources (U.S. exposure by Canadian banks and the CFM) enable us to investigate how adverse funding shocks to banks affect lending to households, and how these changes in lending supply in turn translate into changes in consumption.

1.2 Bank Exposure Sample Construction

CFM respondents choose their main financial institution(s) from a list that includes banks, trust companies (similar to savings and loans in the United States) and credit unions. The inclusion of credit unions is important, because although the Canadian banking sector is dominated by six large banks (known as the "Big Six") that have around 90% of all banking assets, credit unions provide some competition when it comes to retail banking activities. In our final panel sample around 72% of respondents report a Big Six bank as one of their main financial institutions, while 16% bank with institutions that are categorized as "credit unions." Most of the remaining households bank with low- or no-fee banks that primarily operate online. In total, there are 29 financial institutions in our sample.[4]

We use the share of interbank deposits from the United States in total deposits in the fourth quarter of 2006 as a proxy for a bank's exposure to the U.S. prior to the start of the

[4]The "Big Six" banks are the Bank of Montreal, Bank of Nova Scotia, Canadian Imperial Bank of Commerce, National Bank of Canada, Royal Bank of Canada, and TD-Canada Trust. The main institutions in our "credit union" category (which also covers "cooperative credit institutions") are Alberta Treasury Branches, "any community or occupational credit union," Desjardins and Vancity. The main low- or no-fee online banks in our sample are ING Canada and PC Financial. For brevity's sake, we will refer to all financial institutions in our sample as "banks."

crisis (*Exposure*). Concentrating on interbank deposits from the U.S. allows us to identify whether tightness in U.S. funding markets were transmitted to the Canadian household sector by limiting Canadian banks' ability to supply credit under stressful funding conditions. We use a narrow definition of "exposure" by concentrating on funding from the U.S., given that the shock originated there. Nevertheless, our exposure measure is designed to capture a broader tendency by the average exposed bank to rely on unstable foreign wholesale funding as a whole. In other words, we assert that banks that had high levels of U.S. interbank deposits in their liabilities portfolio were also relying on other sources of funding from global markets. Although the funds raised in the U.S. interbank market may not have been used for making loans in Canada, the loss of U.S. interbank deposits during the crisis would likely signal a loss of other types of foreign funding, which would then likely affect the bank's business decisions back home. This approach is similar in spirit to the one used by Paravisini et al. (2015), who measure exposure to the financial crisis by identifying Peruvian banks that relied more on foreign funding to extend export-related credit and to Iyer et al. (2014), who capture exposure of Portuguese banks to the interbank market by considering the total share of borrowing coming from abroad.

We separate the banks into "exposed" and "unexposed" categories based on our observation that *Exposure* features a natural break around 3% in 2006q4. The share of interbank deposits from the United States ranges from zero to slightly below 2% for one group of banks and from just over 3% to over 11% for a second group. We tested for breaks and this is the only natural break in the data.[5] Accordingly, a bank is classified as exposed if more than 3% of its total deposits were interbank deposits from the United States. For data confidentiality reasons, we are unable to provide more details on *Exposure* or on the identities of the "exposed" vs. "unexposed" banks. However, we can report that only three of the Big Six banks and at least one of the largest credit unions are in the exposed category, and that exposed

[5]We used two different k-means cluster analysis algorithms applied to a single-dimension (Hartigan (1975)). Both approaches yielded exactly that same categorization of banks as our initial grouping of the banks based on a breakpoint at 3%.

banks account for approximately 50% of all pre-crisis banking sector assets (as of 2006q4).[6] In the robustness section, we discuss other definitions of bank exposure such as the ratio of foreign currency-denominated liabilities to total liabilities. These other exposure measures are highly correlated with the measure of interbank deposits from the U.S., as they tend to result in identical classifications of banks as exposed vs. unexposed. Figure 2 shows that the use of U.S. interbank deposits by exposed Canadian banks sharply declines after 2008q1, likely capturing the unavailability of such funds once the crisis started. Taken together with the high correlation between the use of U.S. interbank deposits and use of foreign funding in general, the patterns in Figure 2 strongly suggest that the crisis substantially disrupted exposed banks' overall funding models.[7]

[INSERT FIGURE 2 HERE]

We then turn our attention to establishing the degree of funding stress experienced by the exposed banks. For this task, we use quarterly balance sheet and income statement data to construct a simple proxy for each bank's *incremental cost of funding*, defined as "Change in Total Interest Expense/Change in Interest Bearing Liabilities". This measure captures the additional interest expense incurred for an additional $1 of liabilities obtained during a given quarter.[8] Figure 3 shows the difference between average incremental funding costs of exposed vs. unexposed banks during our sample period. While the two groups of banks exhibited broadly similar incremental costs of funding during the pre-crisis period, the difference increased rapidly during the crisis. Although their cost disadvantage declined in

[6] If all (or most) of the Big Six banks were in the same category, this might raise the valid concern that our separation of banks simply captures a fundamental difference in the business strategies of these very large banks versus their smaller (mainly credit union) competitors. The fact that the Big Six banks are evenly distributed across the two categories alleviates this concern.

[7] For example, the ratio of foreign currency-denominated liabilities to total liabilities (an alternate exposure measures considered in our robustness section) behaves in a manner quite similar to Figure 2. In 2006q4, approximately 26% of exposed banks' liabilities were in foreign currency (12.5% for unexposed banks). At the start of 2008, this ratio was 21.1% (13% for unexposed) and by the end the crisis (2009q4), only 11.2% of exposed bank liabilities were denominated in foreign currency (10% for unexposed).

[8] "Change in Interest Bearing Liabilities" is the quarterly change in all deposits (wholesale or retail), bankers' acceptances, subordinated debt and repos.

2009, exposed banks still had a higher incremental cost of funding at the end of our sample period. Hence, there was a sustained disruption in exposed banks' funding during the crisis.

[INSERT FIGURE 3 HERE]

Next, Figure 4 considers the potential impact of this funding stress on Canadian households by comparing the lending behavior of exposed and unexposed banks. We define lending as the annualized quarterly growth rate in CPI-adjusted consumer loans made within Canada.[9] The figure shows a difference in credit extension between the two groups for most of the crisis period. The growth of consumer lending slowed among exposed banks during the crisis and eventually turned negative, while remaining relatively constant for unexposed institutions. The patterns in Figure 4 support our approach to categorizing Canadian institutions.[10]

[INSERT FIGURE 4 HERE]

Once banks are identified as either exposed or unexposed, we classify each household based on that identification. For instance, if the household reports only one "main" bank, then it obtains that bank's classification. If the household reports more than one "main" bank (approximately 64% of the households in our sample), then it is classified as exposed only if all banks are exposed. We pursue this conservative approach because as long as the household has a relationship with at least one unexposed bank, it can satisfy its consump-

[9] The figure excludes personal lines of credit from consumer loans, since during our sample period the reporting of home equity lines of credit (HELOCs) across Canadian financial institutions was not uniform and some institutions reported HELOCs as mortgages. Therefore, by excluding mortgages as reported on balance sheets, we may also be excluding the HELOCs of some institutions but not others. Excluding all lines of credit (and hence the HELOCs not reported as mortgages) from the figure avoids this inconsistency.

[10] Plotting the growth rate of commercial lending in Canada (not shown) yields a similar pattern: lending by exposed banks slows down in late-2007/early-2008, while there is little change in the lending behavior of unexposed banks. The commercial lending of both groups converge again later on during the crisis period. Finally, a comparison of total assets (domestic and global) also confirm these patterns: total assets of exposed banks grow more slowly during 2008 and shrink in 2009. Although total asset growth also slows down for unexposed banks, it remains higher than exposed banks and never turns negative.

tion needs by obtaining loans through the unexposed bank(s). As a result, our classification of household exposure implicitly acknowledges that households can switch among the main banks they already had during the pre-crisis period. However, we also assert that in the event that all of the main banks of a household were exposed, this household was either unwilling or unable to switch to a new (and unexposed) main bank during the crisis period. Previous studies of Canadian retail banking industry, such as Allen et al. (2018), have found that the combination of high search costs and brand loyalty lead to low incidence of financial institution switching among Canadian households. The importance of main banks in the provision of non-mortgage credit is also evident in our data; throughout our sample period, approximately 83% of all non-mortgage credit products in CFM were originated by an institution identified as a main bank by the borrower.

1.3 Panel CFM Sample Construction

The CFM is a repeated cross-sectional survey, although it is relatively common for the same household to appear in two or more (usually consecutive) years. We take advantage of this feature to construct an unbalanced panel of households. We start by determining the "crisis" and "pre-crisis" periods. We consider January 2008 to December 2009 as the crisis period and define the pre-crisis period as January 2005 to December 2006. We drop 2007 since it is not clear whether it would belong in the pre-crisis or crisis periods.[11]

Next we look for households that completed the CFM survey at least once during 2008-2009 and at least once during 2005-2006. This ensures that we have at least two observations per household, one in each period. However, since there are also households in CFM that have completed the survey more than twice during our sample period, we end-up with an unbalanced panel that includes two to three observations per household. As in Leth-Petersen (2010), we also remove all households where the youngest head is older than 65 during the crisis, to avoid interference from retirement decisions. After households with missing data are

[11] For example, there was a liquidity crisis in the Canadian asset-backed commercial paper market in the summer of 2007, which implies that some financial instability may have started as early as mid-2007.

eliminated, the final sample includes 3,804 households, of which 1,246 are with an exposed bank and 2,558 are with an unexposed bank.

1.4 Consumption, Credit and Liquid Asset Variables

Starting in 2008, the CFM includes a section titled "Household Expenditure," in which respondents state how much they approximately spent on sixteen items during the past month and on an additional five items during the past year (see Table 1). Survey respondents answer each spending question by choosing the "bin" that their answer falls into ($0 to $24, $25 to $49, etc.). We consider the midpoint of the bin specified by the respondent to be the actual spending amount.[12]

[INSERT TABLE 1 HERE]

Using the answers to the expenditure questions, the "total consumption" of each household is calculated in a manner similar to Browning and Leth-Petersen (2003). We first convert the monthly spending questions to annual spending by multiplying last month's spending by 12. These amounts are then combined with the annual spending questions to create the overall annual total spending.[13] This variable is adjusted for the month of the year in which the survey was completed, by regressing the annualized spending amounts on twelve month dummies and extracting the residuals. Households that have zero or negative annual total consumption are subsequently eliminated from the sample. Finally, we adjust total consumption by the overall Canadian CPI (to account for the two different years that the data are taken from) and winsorize the data at 1% and 99%, to ensure that the households who consistently choose the top or the bottom bins are not driving our results.

[12]The "top-coded" bin is "$20,000 and over," which we interpret as $20,000 of spending. This top bin is chosen on only very few occasions (auto purchases, home improvements and vacations), and changing the top code to a higher amount does not affect our results.

[13]It is possible that some of the monthly spending questions capture expenditures that are infrequently incurred (such as property/municipal taxes, automobile maintenance, clothing/footwear and insurance premiums) and hence should not be multiplied by 12. Our findings are unchanged whether or not we perform this adjustment.

Before proceeding with this survey-based consumption data, we first examine its coverage and overall quality. We do this by aggregating the CFM expenditure data using survey weights and comparing the outcome to aggregate household expenditure data from Canadian national accounts for 2008-2012 (after limiting both data sources to common spending items). We find that the ratio of aggregated CFM data to the aggregate national accounts data is stable in the 0.62-0.65 range. Since this is comparable to many surveys that are custom designed to measure expenditure (Barrett et al. (2015)), we conclude that excessive noise in our survey data does not seem to be a major concern. The Online Appendix provides the details of this exercise.

To separate any effects of bank lending on subcategories of consumption during the crisis period, we also construct a "durables spending" variable that consists of

$$\begin{aligned} \textit{Durables} &= \textit{Clothing/Footwear} + \textit{New or Used Car, Truck, etc.} + \textit{Home Furnishings} \\ &+ \textit{Home Appliances and Electronics}. \end{aligned}$$

Total non-mortgage liabilities and liquid asset holdings for both the pre-crisis and crisis periods are defined as:

$$\begin{aligned} \textit{Non-mortgage Liabilities} &= \textit{Credit Card Balances} + \textit{Personal Loan Balances} \\ &+ \textit{Personal Line of Credit Balances} + \textit{Lease Balances}, \\ \textit{Liquid Assets} &= \textit{Checking Account Balances} + \textit{Savings Account Balances} \\ &+ \textit{Cashable Guaranteed Investment Certificate Balances}. \end{aligned}$$

where guaranteed investment certificates (GICs) are financial products that offer a fixed return over a predetermined time period, similar to a U.S. certificate of deposit. Given that early GIC withdrawals are either heavily penalized or outright banned, we limit our definition to GICs that are convertible to cash on short notice. We leave other investment products, such as mutual funds, stocks or bonds, out of our liquid asset definition, for three reasons. First, relatively few survey respondents hold these products. Second, most of these investments are part of retirement or educational savings accounts, making them difficult to liquidate. Third, the large price fluctuations during the crisis period make it difficult to determine whether changes in the holdings of such instruments by a household are due to changes in price or quantity. Both the non-mortgage liability and liquid asset variables are winsorized at 1% and 99%, consistent with the consumption variables.

Finally, given that the section on consumption expenditures was added to the CFM in 2008, we use an identity commonly used in the literature, such as Leth-Petersen (2010) and Jensen and Johannesen (2017), to calculate pre-crisis consumption:

$$\begin{aligned}(Consumption_{Cri} - Consumption_{Pre}) &= (Income_{Cri} - Income_{Pre}) \\ &- (\Delta Wealth_{Cri} - \Delta Wealth_{Pre}) \qquad (1)\end{aligned}$$

where $\Delta Wealth$ represents the change in financial wealth between one period and the period that precedes it ("savings" or "dissavings"). Since we observe all of the variables in this identity other than $Consumption_{Pre}$, we can directly calculate pre-crisis period consumption for all of the households in the panel CFM sample.

Two issues arise while calculating $Consumption_{Pre}$. First, we need to identify households from our panel sample that have also completed the survey during a "pre pre-crisis" period, so that $\Delta Wealth_{Pre}$ can be observed. However, given the repeated cross-sectional nature of the CFM survey, this leads to a substantial loss in sample size (we cannot calculate $\Delta Wealth_{Pre}$ for almost 35% of our sample). Second, we need to decide which financial assets will be included in the definition of *Wealth*, since for stocks, bonds and mutual funds, we are unable to determine whether changes in values are driven by price fluctuations or by households buying/selling these assets. Meanwhile, given our interest in identifying pre-crisis spending on housing, vehicles, etc. (since they are included in the actual $Consumption_{Cri}$ data), we purposefully exclude home, vehicle, etc. values from our definition of *Wealth*.

For our baseline analysis, we address these issues in the following manner. In order to maximize sample size, we assume that no change in wealth occurred during the pre-crisis period, so $\Delta Wealth_{Pre} = 0$. Furthermore, we only include liquid assets in our definition of *Wealth* and leave out stocks, bonds and mutual funds. However, in the robustness section, we show that our results are entirely unchanged if we use a smaller sample where $\Delta Wealth_{Pre} \neq 0$ and/or if other financial assets are also included in the calculation of $Consumption_{Pre}$, with appropriate adjustments for fluctuations in stock and mutual fund prices.[14] The baseline version of the identity that we use is thus given by:

$$
\begin{aligned}
(Consumption_{Cri} - Consumption_{Pre}) &= (Income_{Cri} - Income_{Pre}) + (Mortgage\ Debt_{Cri} \\
&- Mortgage\ Debt_{Pre}) + (Non\text{-}mortgage\ Liabilities_{Cri} \\
&- Non\text{-}mortgage\ Liabilities_{Pre}) \\
&+ (Liquid\ Assets_{Pre} - Liquid\ Assets_{Cri})
\end{aligned}
$$

where *Non-mortgage Liabilities* and *Liquid Assets* are calculated as discussed above. *Mortgage Debt* is the combined balance of all mortgage loans of the households (excluding HELOCs, which are included in *Non-mortgage Liabilities*). *Income* is defined as combined pre-tax family income, a yearly question answered by all CFM respondents.[15]

This calculation is subject to measurement error, due to savings and borrowing instruments that are excluded from the survey (such as keeping cash under the mattress or personal loans from friends/family). As such, it is possible for pre-crisis consumption to be negative. For some further discussion of this common problem when using wealth data to impute

[14] Furthermore, in the Online Appendix, we check the precision of our methods by calculating 2008 consumption (using 2007, 2008 and 2009 CFM) and comparing it to the actual consumption data reported in the 2008 survey.

[15] Members of a household are only asked about their pre-tax income. Although after-tax income might be more appropriate for our identity, the fact that we are taking the difference in gross income is likely to cancel out most of the taxes as long as there are no major changes in income (that cause changes in tax brackets). Few households exhibit such major changes between the crisis and pre-crisis periods.

consumption see also Leth-Petersen (2010) and Browning and Leth-Petersen (2003). Accordingly, we set the minimum value of pre-crisis consumption to equal the minimum value of actual crisis consumption.[16] However, we cannot calculate *Durables* for the pre-crisis period, so we are unable to perform difference-in-differences analysis for this variable.

Table 2 displays summary statistics for our consumption, liability and liquid asset variables (we discuss additional parallel trends tests in the robustness section). There are some differences in these variables both *across* (exposed vs. unexposed) and *within* (pre-crisis vs. crisis) categories, such as a higher mean level of consumption for unexposed households, along with a decrease in consumption between the pre-crisis and crisis periods. Furthermore, the average level of non-mortgage liabilities decreases for both groups of households during the crisis. Liquid assets, on the other hand, appear to increase during the crisis period for both groups, although more slowly for exposed households. It is possible that at least some of this increase in liquid assets is related to precautionary savings, as evidenced by the drop in mean (but not median) consumption. While these patterns provide preliminary evidence of a negative credit supply shock and its impact, the selection issues involved in the assignment of households to exposed vs. unexposed banks require us to consider a deeper empirical approach to investigate any causal effects.

[INSERT TABLE 2 HERE]

2 Empirical Methodology

2.1 Difference-in-Differences

Our goal is to examine the effects of negative credit supply shifts (driven by adverse shocks to bank liquidity) on household consumption expenditures. Bernanke and Lown (1991) suggest that adverse liquidity shocks to banks may cause capacity constraints in bank lending due

[16] This allows us to maintain our sample size even after log-transforming the consumption data. We obtain similar consumption results if we do not preform this adjustment.

to an imperfectly elastic market for banks' liabilities. During the crisis, the U.S. interbank market collapsed (see Heider et al. (2015)) and as a result, Canadian banks with exposure to the U.S. interbank market faced a shock. These banks may have then passed on the shock on to their customers by shifting their credit supply curve inward. Following Johnson et al. (2006) and Leth-Petersen (2010) the starting point for the econometric analysis is therefore a difference-in-differences (DiD) model of the form

$$Q_{it} = \beta_0 + \beta_1 \cdot Cri_t \cdot Exposed_i + \beta_2 \cdot Cri_t + B \cdot X_t' + \delta_i + \epsilon_{it}, \tag{2}$$

where Q_{it} represents some financial measure of household i at time t, $Exposed_i$ represents a dummy indicating that the bank of household i had high exposure to the U.S. interbank market in 2006 as defined in section 1.2, and Cri_t indicates the crisis period. X represents a set of time-varying controls (income in logs, house value in logs and home equity divided by house value), and δ_i represents household fixed effects. β_1 measures the effect on Q for households that bank with an exposed institution during the crisis. The results in Table 3 show significant DiD effects for non-mortgage liabilities and liquid assets, but no significant effect on consumption expenditures. However, estimating equation (2) using ordinary least squares (OLS) may expose us to the sensitivity of OLS to differences in the covariate distribution between households affiliated with high- vs. low-exposure banks. Hence, in our primary specifications below, we use a matching estimator to alleviate these concerns.

[INSERT TABLE 3 HERE]

2.2 Matching and the Choice of Covariates

Our identification strategy relies on obtaining a sample of households that are characterized by an identical demand for credit and differ only in terms of their bank affiliation.[17] Clearly,

[17] The identification does not depend on the assumption that exposed and unexposed banks are identical. On the contrary, we rely on the idea that these banks ex post differ in their credit supply due to their ex ante decision to expose themselves more to the U.S. interbank market.

households may not be randomly assigned to banks. It is possible that banks with high exposure to the crisis had different customers compared to banks with low exposure. For example, banks with more U.S. interbank exposure may attract customers who also have more exposure to the United States and, hence, respond more strongly to the financial crisis originating there. This implies that estimating the unconditional elasticity of consumption to lending supply shocks may be biased.[18]

Hence, we use propensity score matching to obtain estimates of β_1. A matching estimator balances the covariates between the households affiliated with low-exposure banks with those households affiliated with high-exposure banks without imposing functional form assumptions. Consider

$$\begin{aligned} E[(Q^1_{i,Cri} - Q^1_{i,Pre}) - (Q^0_{i,Cri} - Q^0_{i,Pre}) | Exposed = 1, X_{i,Pre}] = \\ E[(Q^1_{i,Cri} - Q^1_{i,Pre}) | Exposed = 1, X_{i,Pre}] - E[(Q^0_{i,Cri} - Q^0_{i,Pre}) | Exposed = 1, X_{i,Pre}], \quad (3) \end{aligned}$$

where $E[\cdot]$ is the expectation operator and $(Q^1_{i,Cri} - Q^1_{i,Pre})$ is the change in expenditure (or consumer credit, liquid assets, etc.) of exposed household i between the crisis and pre-crisis periods. Equation (3) measures the difference in expenditure between exposed and unexposed households during the crisis period relative to the non-crisis period. This corresponds to β_1 in equation (2) and is known as the "average treatment effect on the treated" (ATT).

There is no sample counterpart for the second term on the right-hand side of equation (3). It is a counterfactual; i.e., the change in consumption of an exposed household had it been affiliated with an unexposed bank. We can, however, still recover the causal effect β_1 if the assignment of a household to a bank is random conditioning on $X_{i,Pre}$. We follow the propensity score matching procedure suggested by Abadie and Imbens (2006, 2016) to

[18] In line with Leth-Petersen (2010), we would expect the bias to go against finding significant differences across households. If wealthier households, which we would expect to react less to a reduction in lending supply, are disproportionately associated with banks that have a higher exposure to the crisis, this would reduce the observed difference in the change of expenditures between this group and the group of low-wealth households that bank disproportionately with banks with little crisis exposure.

estimate the counterfactual. While the choice of covariates is crucial for this procedure, there is no formal approach for doing so. The goal is to compare consumption patterns of households that have identical consumption- and borrowing-related characteristics, and that differ only in their choice of a banking institution (i.e., an exposed vs. an unexposed bank). Therefore, we include standard household characteristics such as age, family size and marital status, but also financial characteristics such as home ownership or income.

3 Results

3.1 Propensity Score Estimation and Match Quality Assessment

We estimate a probit model and obtain the probability of banking with an exposed financial institution (i.e., the propensity score) as a function of home equity, home value, gross income, age of the head of household, marital status, unemployment status, house size, labor supply, self-employment, level of education, number of children, a dummy equal to one if the household lives in a major metropolitan area, a dummy variable indicating whether the household rents, a dummy variable indicating whether the household's main language is French, and an indicator variable controlling for the household's level of risk aversion.[19] The model also includes squared continuous variables to allow for a non-linear relation with the dependent variable. All variables are measured at the pre-crisis period, except risk aversion.

The estimation results in Table 4 show few significant differences between exposed and unexposed households, even in the raw data. Among those that are significant, we find that the probability of banking with an exposed institution is positively correlated with the share of home equity. However, households that report higher gross income are less likely

[19] The risk aversion variable is calculated using a segment added to the CFM in 2007. In this "attitudinal section," respondents are asked about their agreement/disagreement on a variety of statements regarding risk tolerance. We place equal weight on two such questions ("I don't like to invest in the stock market because it is too risky" and "I am willing to take substantial risks to earn substantial returns") to calculate a risk aversion index. Since the attitudinal questions are available only from 2007 onwards, we use the 2007 values for our panel households that have also completed the 2007 survey (approximately 65%). For the rest of the households, we use the 2008 risk aversion data and implicitly assume that the onset of the crisis did not drastically change attitudes toward risk.

to be associated with an exposed bank. The chance of having an exposed bank is lower for households whose female head participates in the labor force. Residents of big cities are less likely, and households whose main language is French are more likely, to bank with an exposed institution. Finally, households that report higher levels of risk aversion are more likely to bank with an exposed institution.

[INSERT TABLE 4 HERE]

After obtaining the propensity scores from the probit model, we perform our matching procedure. For each household in the exposed group, we find four unexposed households with the closest propensity score,[20] calculate the average level of the log of the measure of interest (liquid assets, non-mortgage liabilities, consumption), and compare it to the respective measure by the exposed household. Matching is done with replacement, so the same unexposed household can be matched with different exposed households. Reusing observations minimizes the risk of unexposed households not looking like their exposed matches, but potentially at the expense of a loss of precision. We also calculate and report robust standard errors that account for possible estimation error in the propensity score (which can lead to inaccurate confidence intervals), as described in Abadie and Imbens (2016).

Since the purpose of the matching procedure is to balance the covariates across the two groups, we report two-sample t-statistics for all explanatory variables in Table 5. A failure to reject the test indicates that, on average, there is no difference between households that bank with an exposed vs. an unexposed financial institution. The reported t-tests show no evidence of differences in the characteristics of the two groups. Finally, the validity of the matching estimator depends on the presence of common support for the propensity scores of exposed vs. unexposed households. As shown in Figure 5, there is ample common support

[20] According to Imbens and Wooldridge (2009), "little is known about the optimal number of matches, or about data-dependent ways of choosing it." Nevertheless, using more than one match for each treated observation seems to improve the Abadie and Imbens (2006, 2016) procedure. We choose four matches, given our sample size and the number of households in our control sample.

between exposed and unexposed households, alleviating these concerns.[21]

[INSERT TABLE 5 HERE]

[INSERT FIGURE 5 HERE]

3.2 Main Results

This section reports our main results of estimating the difference-in-differences average treatment effect on the treated (DiD ATT) of banking with an exposed institution for the variables of interest (consumption expenditure, non-mortgage liabilities and liquid assets). These are based on the pre-crisis to crisis period change for those variables between exposed and unexposed households. We start with calculating DiD ATT in the overall sample (we also calculate ATTs for differences in our variables of interest separately for both the pre-crisis and crisis periods). However, as we will see below the average effects mask considerable heterogeneity across the income distribution.

Table 6 shows significantly negative DiD ATT on liquid assets and non-mortgage liabilities for the exposed households (i.e., negative and significant ATTs for the "difference in differences" terms), along with an insignificant effect on consumption. This is consistent with the OLS results shown in the previous section. For non-mortgage liabilities we obtain a DiD ATT of -0.338 (statistically significant at the 5% level), for liquid assets a DiD ATT of -0.463 (statistically significant at the 1% level). For consumption we obtain a statistically and economically insignificant coefficient (-0.007).

[INSERT TABLE 6 HERE]

We conclude that households banking with exposed institutions report significantly lower non-mortgage liabilities compared to households with unexposed institutions that otherwise

[21] In the robustness section we perform further analysis that measures the sensitivity of our method to hidden bias ("Rosenbaum bounds").

exhibit identical observables. However, on average, there is only insignificant evidence of consumption patterns between the two groups being different in the crisis period. Thus, households are able to compensate for the deterioration in their access to credit by drawing down liquid assets and largely maintain a smooth consumption stream. We confirm this conclusion by looking at the magnitudes suggested by our empirical findings. As seen in Table 6, the difference ATT for non-mortgage liabilities during the crisis period is -0.462. This ATT implies that the average difference between the crisis period non-mortgage liabilities of an exposed and a matched household equals 37% of the matched household's liabilities. Since the average level of non-mortgage liabilities held by matched households during the crisis is 17,688 CAD, the average difference in crisis period non-mortgage liabilities is -6,544 CAD. Meanwhile, the difference ATT for non-mortgage liabilities during the pre-crisis period is 0.001, implying that the average exposed household's non-mortgage liabilities is 0.1% higher than those of a matched household. The implied pre-crisis period difference is 17 CAD (the average non-mortgage liabilities of a matched household during the pre-crisis period is 17,449 CAD). Subtracting the average pre-crisis difference from the average crisis period difference yields the average difference-in-differences in dollar terms, which is -6,562 CAD. A similar calculation for liquid assets yields an average dollar difference-in-differences of -5,419 CAD (the average liquid asset holdings of matched households is 19,789 in the crisis period and 16,917 in the pre-crisis period). Hence, the CAD reduction in borrowing by exposed households is mostly offset by a corresponding drawdown of liquid assets, resulting in a negligible impact on consumption.

Our next step is to examine the ATT on consumption in more detail. Despite the absence of an average consumption effect in our analysis so far, it is possible that banking with an exposed financial institution can lead to lower consumption expenditures for some households. Therefore, we examine the effects on consumption across the income distribution, based on the pre-crisis incomes of exposed households. If the difference-in-differences ATT on total consumption is more negative among lower income households, this would be evidence in support of income playing a big role in the ability of households to withstand the negative credit supply shock.

In order to look at treatment effects across the pre-crisis income distribution, we modify our empirical approach in order to match on *both* propensity score *and* pre-crisis income. We define lower income households as those with incomes below the sample median of 60,000 CAD, while households with pre-crisis incomes above 60,000 CAD are considered to be higher income households. We then calculate and report separate difference-in-differences ATTs for the matches involving these two different income groups.[22]

The results in Table 7 show that the DiD ATTs on total consumption vary by exposed households' pre-crisis income. Households below 60,000 CAD in pre-crisis income, which are most vulnerable to an adverse credit supply shock, exhibit a statistically significant decline in their total consumption during the crisis. The negative and significant treatment effect on crisis period consumption (i.e. the difference) confirms this observation and ensures that the DiD ATT is not being driven by noise in our pre-crisis imputation. In contrast, high income exposed households with pre-crisis incomes above 60,000 CAD exhibit neither a significant DiD ATT on total consumption, nor a significant ATT on crisis period consumption.

[INSERT TABLE 7 HERE]

This heterogeneity in DiD ATTs on total consumption (and the difference ATTs on crisis-period consumption) can be due to two factors. It is possible that Canadian banks primarily decreased lending to riskier households and this heterogenous lending shock led to a relatively greater credit supply shock to lower income households (a "flight to quality"). The other possibility is that all households received a relatively homogeneous shock, but lower income

[22] It is not possible to calculate Abadie and Imbens (2006, 2016) standard errors when matching on propensity score *and* another variable. Instead, we report heteroscedasticity-robust standard errors. Abadie and Imbens (2016) show that their adjustment to the standard error of an average treatment effect on the treated (ATT) can be either positive or negative, depending on the underlying data generation process. Therefore, we cannot be certain whether the heteroscedasticity-robust standard errors are bigger or smaller than the corrected standard errors.

households were less able to smooth this shock, resulting in heterogeneous consumption responses. In order to investigate these two possibilities, Table 7 also shows DiD ATTs on non-mortgage liabilities and liquid assets for different income groups, when households are matched on both propensity score and pre-crisis income category. The group-specific treatment effects suggest that both factors might have played a role in the heterogeneous consumption responses. Although the lower income households exhibit the larger credit supply shock, exposed households with higher incomes also experienced a negative credit supply shock and also drew down their liquid assets in response. Although the significance of the credit supply shock for the higher income groups is weaker (and the size of the shock is smaller), there is considerably less heterogeneity in credit supply, compared to the consumption response.

3.3 Economic Magnitudes

In this section, we report the micro- and macroeconomic effects of our findings. We start by using the heterogeneity across two income groups in an attempt to reconcile the decrease in the aggregate consumption data during the crisis with our estimates. In order to do this, we must correct for the selection bias in the CFM survey, which, like almost all data sets focused on household assets and liabilities, over-samples wealthy households. Hence, simply using the consumption-related ATTs from Table 6 will not yield the correct aggregate effect on consumption for the Canadian population. Accordingly, Table 8 compares the representation of the two income groups in our CFM sample vs. the Canadian population, using data on the distribution of income across all Canadian households (provided by Statistics Canada). The table shows that lower income households (income below 60,000 CAD) are substantially under-sampled in our data set. Hence, we calculate population weights for each category, using the share of households in different income brackets from Statistics Canada.

Using these weights and the share of exposed households in each income group (from CFM), we can approximate the total number of exposed Canadian households in each income category by multiplying the share of exposed households in each category (from CFM) with the total number of households (from Statistics Canada). For example, we establish that there were 2,583,220 exposed Canadian households in the "Less than 60,000 CAD" income group. This approach allows us to calculate a weighted effect for the entire economy and correct for the over-sampling of higher income households in our data.

While calculating this weighted macroeconomic effect, we recognize the fact that the ATT on total crisis period consumption from Table 7 is only significant for the households with pre-crisis incomes below 60,000 CAD. Therefore, we assume that there was no difference in the crisis-period consumption for the higher income group. For the "Less than 60,000 CAD" group, we convert the difference ATT on total crisis-period consumption into a level difference between exposed and matched households. This ATT is -0.068, which translates into a 6.6% difference between the levels of exposed and matched households' consumption. Using the average level of crisis-period consumption by the matched households in this income category (approximately 27,560 CAD), we calculate an average difference of -1,819 CAD between the consumption of exposed and matched households. Multiplying the average consumption difference with the total number of exposed households yields a 4.69 billion CAD aggregate "loss" in consumption for this income category. Given that there is no consumption effect for the other income category, 4.69 billion CAD is also the economy-wide credit supply effect on consumption[23], which is more than half of the 7.5 billion CAD drop in aggregate consumption (beginning of the crisis to the trough) reported in Figure 1.[24]

[INSERT TABLE 8 HERE]

[23]Whereas, the un-weighted treatment effect reported in Table 6 of -0.009 yields an aggregate effect of about 1 billion CAD.

[24]A decline in consumption due to a negative income shock can explain part of the remaining decline in aggregate consumption. According to Statistics Canada, the median household income fell by approximately $450 during the crisis. This decrease in income and the sensitivity of non-durables consumption in Canada to income changes estimated by Ostergaard et al. (2002), can help explain another $1 billion decline in total consumption. However, we cannot approximate the full impact of this income shock on total consumption without an estimate of the sensitivity of durables consumption to income. In addition, it is also possible that some Canadian households reduced consumption expenditure because they were unsure about how the crisis in the United States and elsewhere would affect their future economic well-being.

4 Robustness

4.1 Crisis-Period Employment and House Value Outcomes

To ensure that our results are indeed driven by a negative credit supply shock due to banking sector distress, we need to rule out two other likely explanations. The first alternative explanation is that our results are due to differences in employment dynamics between exposed and unexposed households. Specifically, if exposed households are more likely to become unemployed during the crisis (perhaps due to the geographical heterogeneity between the them and unexposed households, which will be discussed below, or due to occupational differences, which we cannot observe), this could explain the lower levels of non-mortgage liabilities among exposed households.

The second alternative scenario is based on the "housing net worth channel" of Mian and Sufi (2014) and Mian et al. (2013). If the exposed households in our sample experienced a larger decline in house values compared to unexposed households, this may have suppressed their demand for goods and services, and therefore their demand for credit. This would contradict our argument of a supply-driven reduction in non-mortgage credit.

In order to rule out these possibilities, we calculate treatment effects on pre-crisis and crisis levels of unemployment, income and house values, along with the difference-in-differences in these variables. For house values, we would like to eliminate any impact on the treatment effect from households that might purchase or sell a house between the pre-crisis and crisis periods, so we restrict our sample to households that owned a home in both periods (including renters in the analysis yields similar results as well).

[INSERT TABLE 9 HERE]

As seen in Table 9, there are no differences in unemployment patterns and income between exposed and matched households. The "housing net worth channel" also plays a limited role in our findings, given the absence of any significant differences in the house values of exposed vs. matched households throughout our sample period. Overall, we fail to find evidence in favor of either the employment dynamics scenario or the housing net worth channel.[25]

In addition, we fail to find significant differences in income dynamics when households are matched on propensity score and income. The difference-in-differences in income between exposed vs. matched households are statistically and economically insignificant for both the low income and high income households (full set of results available upon request). Therefore, our findings on the heterogeneity in consumption responses across the income distribution are not being driven by lower income exposed households being more likely to receive a negative income shock during the crisis.

Our findings of no differences between ex post outcomes in house values, income and employment also may alleviate concerns that ex ante differences in expectations about future consumption between exposed and unexposed households underlie our results. As discussed e.g. in Kennan (1979), under rational expectations, the actual outcome is an unbiased predictor of the expectation. Table 9 shows that we do not observe any significant differences in the actual outcomes related to unemployment status and house value dynamics of exposed vs. unexposed households during the crisis. Hence, even though our data does not provide us with a measure of household expectations, based on this line of argument, it is unlikely that differences in expectations between exposed and unexposed drive the consumer debt and consumption results.

[25] In order to ensure that the house price dynamics of the households in our sample are representative of the Canadian housing market, we performed a comparison of the reported home value changes in our CFM sample to the MLS® Home Price Index (HPI) data for eight major metropolitan areas (from the Canadian Real Estate Association). We matched the households in our probit sample to the eight metropolitan areas and calculated the difference between the reported (and annualized) home value growth rate and the actual home price index growth rate in the household's city. For CFM households living outside these metropolitan areas, we used the composite Canadian home price index to calculate this difference. The mean difference between the reported and actual house price growth rate was 0.04% for exposed households and 0.8% for unexposed households, implying that house values within our CFM sample were moving in a manner very similar to the broader housing market.

4.2 Alternative Exposure Measures

Here we discuss the robustness of our results to different definitions of bank exposure. One possible concern is that banks may use interbank deposits as a source of liquidity and not financing. Therefore, we consider the deposits of Canadian banks in U.S. banks (i.e. the "asset side" of the balance sheet) to make sure that total financial resources of exposed banks actually declined during the crisis. Accordingly, we create a "net U.S. interbank exposure" measure defined as "(Total Interbank Deposits from the U.S. - Total Interbank Deposits in the U.S.)/Total Deposits", which captures the net inflow of deposits from U.S. banks.

As discussed above, the share of interbank deposits in total deposits of Canadian banks is modest (with a natural break at 3% and a maximum of 11%). In order to confirm that our exposure measure is correlated with broader tendency by a bank to relay on unstable foreign and/or foreign currency (FX) wholesale funding, we construct a broad "foreign currency liabilities exposure" measure, which captures the share of total assets funded by FX-denominated liabilities.[26]

We also consider Canadian banks' exposure to the crisis through their asset-side interactions with foreign financial institutions. Our "foreign currency interbank assets exposure" captures the share of FX-denominated and financial institution-related assets in the overall asset portfolio.[27] These foreign currency liability and interbank asset measures form a more substantial part of Canadian banks' portfolios; "foreign currency liabilities exposure" ranges between 0% and 27%, while "foreign currency interbank assets exposure" has a range of 0% to 25% at the end of 2006. Finally, we calculate a "net foreign currency exposure" by subtracting the "foreign currency interbank assets exposure" from the "foreign currency liabilities exposure."

Re-grouping the banks in our sample using natural breaks in these new exposure measures

[26]Specifically, we include "deposits from depository institutions", "bankers acceptances", "repurchase agreements", "obligations related to borrowed securities", "derivative related amounts" and "subordinated debt" in this measure.

[27]This measure includes "call and other short loans to investment dealers and brokers, secured", "loans to regulated financial institutions", "reverse repurchase agreements" and "derivatives related amounts".

yield "exposed" vs. "unexposed" banks that are almost identical to our original classification.[28] Only five banks (out of 29) are reclassified under some of the alternative exposure measures. We re-estimate our baseline specification after reclassifying these five banks and find that the results are robust. For all specifications we obtain negative and significant DiD ATTs for non-mortgage liabilities and liquid assets, but an insignificant DiD ATT for total consumption, confirming our main conclusions.

4.3 Switching and Geographical Issues

This subsection addresses two additional concerns regarding our identification. First, we explore the potential effect of households switching between exposed and unexposed banks, and second, we address concerns that our results may be driven by regional differences in macroeconomic performance across Canada. We classify the household's bank(s) before the crisis and assume that the household maintains the same set of main banks throughout the sample period. Although this assumption is based on evidence pertaining to brand loyalty and high switching costs among Canadian households, some households could still switch banks and establish a new main banking relationship with an unexposed bank during the crisis period. Therefore, even if credit is unavailable at the incumbent main bank, the household can obtain such credit from a competitor that may be unexposed.[29] If households formed such new main bank relationships during the crisis, this could result in a downward bias in the ATTs. We assess the magnitude of this problem using two approaches: one based on actual searching and switching behavior (i.e., *ex post*) and another based on a tendency to search and switch (i.e., *ex ante*).

In the "*ex post*" approach, we calculate the share of households classified as exposed in the pre-crisis period that obtained a new main bank relationship with an unexposed

[28] For example, the natural break for both "foreign currency liabilities exposure" and the "foreign currency interbank assets exposure" is at 16%. A more detailed set of results are available upon request.

[29] Recall that we classified households that have a relationship both with an exposed and an unexposed bank as "unexposed". This already allows the household to obtain credit from an already existing unexposed main bank. In order to examine the sensitivity of the results to this decision, we also re-estimated the model without these households and found that the results are robust.

institution in the crisis period. We find that only 4% did so. This low number is consistent with the previous evidence in Allen et al. (2018). Our results are unaffected when we drop these searching and switching households from our sample. Our "*ex ante*" approach utilizes a set of specific survey questions that qualitatively measure households' propensity to switch institutions. We construct an "intention to switch" index (ranging from 0 to 1) using six attitudinal questions.[30] We find very little cross-sectional variation in this index, since most respondents are at the mean of 0.48, which indicates that they are neither likely nor unlikely to switch banks. These findings suggest that households do not shop around for non-mortgage credit, perhaps due to lack of information about alternatives (i.e. high search costs). They appear to expect that the likelihood of being turned down is at least as high at a competitor bank as it is with their current bank.

A further concern is that our matching procedure could produce matches of households in different parts of Canada. This could create a bias in the treatment effects. For example, Alberta had robust growth prior to the financial crisis due to high natural resource prices. However, it was hit hard when natural resource prices dropped during the crisis. If our empirical procedure yields matches where the exposed households are more likely to live in areas experiencing a more severe economic shock but the matched households are not, differences in the crisis-period employment patterns could explain our findings. The fact that we do not find any significant ATT effects on the crisis-period unemployment of households (as discussed above), largely alleviates this concern. However, it is still possible that a local negative economic shock increases predictable default risk for *all* borrowers in that area, causing spatial variation in credit market conditions for some types of loans (Hurst et al. (2016)). In this case, it is possible that an exposed but employed household in an area that experiences a negative economic shock will receive less credit compared to an unexposed and employed household living elsewhere, but this difference will be driven by regional factors and

[30]The questions include "there are big differences between financial institutions," "I prefer to deal with people when I bank, rather than using an automated machine or the Internet," "I always actively look for new offers and check that I am getting the best deal from my financial institution." Each question was to be answered on a score from 1 to 10 by the respondent.

not by the exposure of different banks. Finally, if exogenous regional variation in economic conditions (such as Alberta's natural resource-driven economy) is correlated with a higher exposure of banks in certain regions to the United States, this could result in the confounding of the supply effect with demand factors.[31]

We address these concerns by matching on propensity score and region, and re-estimating the treatment effects. As before we report robust standard errors. The results in Table 10 show that our findings are robust to inclusion of region as a matching variable. We therefore conclude that regional differences, including spatial variation in credit market conditions as discussed in Hurst et al. (2016), are unlikely to apply in our case.

[INSERT TABLE 10 HERE]

4.4 Pre-Existing Trends Analysis

In this section, we rule out the presence of pre-existing trends in the most important drivers of consumption: non-mortgage liabilities, income and unemployment. In addition, by using total credit card charges as a proxy, we also show that there were no differences in the spending patterns of exposed vs. unexposed households in the years before the crisis.[32]

Given that CFM is a repeated cross-section, we cannot completely extend our panel sample to years prior to the 2005-2006 period. However, some of the households in our sample have also completed the survey in years prior to the sample period. Accordingly, we create two additional pre-crisis time periods (2003-2004 and 2001-2002) and identify the households from our sample who have also completed the survey in either of these periods. We find that 2,516 households from our original sample (out of 3,804) also appear in the 2003-

[31] Similarly, it is possible that exposed households are more likely to work for U.S. firms or in parts of Canada that are otherwise more integrated with the United States.

[32] The CFM survey includes the question "What is the total amount you charged to the card last month?" for each individual credit card belonging to a household. We take the sum of all reported monthly credit card charges by a household (which can be zero, either because the household doesn't have any credit cards or due to a lack of credit card use) and annualize it in a manner similar to our total consumption variable (discussed in Section 1.4). A number of studies in the literature, such as Mian et al. (2013), have also used credit card spending as a proxy for consumption expenditure.

2004 surveys but only 2,331 report all the required information. This includes 843 exposed households. Similarly, we observe that 1,390 households (515 exposed and 875 unexposed) complete the survey in 2001-2002.

Using these sub-samples, we slightly modify our empirical approach[33] and perform our matching procedure separately for 2001-2002 and 2003-2004. This should reveal any statistically significant differences in non-mortgage credit, income, unemployment and credit card charges between exposed and unexposed households, which would, in turn, reveal any existing pre-trends. For this exercise, we categorize households as exposed vs. unexposed using the bank exposure classification for 2006q4. Table 11 shows that the ATTs on all variables are small and statistically insignificant.

We also examine the possibility of selection bias for households that complete the survey in 2008-2009, 2005-2006 *and* 2001-2002 or 2003-2004. To do so, we take *all* of the CFM data for these two earlier time periods. We then perform our matching procedure, irrespective of whether these households are included in our main sample or not. This approach yields results that are very similar to those shown in Table 11 (not shown but available upon request). Overall, our main findings regarding non-mortgage credit and consumption spending during the crisis do not reflect pre-existing trends in key variables.

[INSERT TABLE 11 HERE]

4.5 Rosenbaum Bounds

Propensity score matching estimators may not be consistent if the assignment to treatment is endogenous (Rosenbaum (2002)). Unobserved variables that affect the assignment to exposed versus unexposed banks may also be related to the outcome variables; i.e., consumption, liabilities or liquid assets. Further, the matching is based on the conditional independence assumption, which states that all variables should be simultaneously observed both influ-

[33]We exclude "risk aversion" from our probit estimation given the unavailability of this variable in the early years of CFM.

encing the participation decision (propensity to be with an exposed bank, the treatment) and outcome variables (non-mortgage liabilities, consumption, liquid assets). To estimate whether "selection on unobservables" may bias our qualitative and quantitative inferences about the effects, we conducted the sensitivity analysis as outlined in Rosenbaum (2002). Rosenbaum bounds assess how strongly an unmeasured variable would have to impact the selection process to invalidate the matching analysis. This does not test the unconfoundedness assumption directly, but rather provides evidence on the degree to which the results hinge on this untestable assumption.

The Rosenbaum bound can be calculated using the probability for a household to bank with an exposed bank (i.e., receive the "treatment"):

$$P_i = P(Exposed = 1|X_{i,Pre}, u_i) = F(\beta \cdot X_{i,Pre} + \gamma \cdot u_i),$$

where $X_{i,Pre}$ are observed characteristics, u_i is the unobserved variable and F is a cumulative density function. γ measures the impact of the unobserved variable u_i on the decision to bank with an exposed bank. Next, consider a matched pair of households that have the exact same characteristics ($X_{i,Pre} = X_{j,Pre}$). If there is no hidden bias through u_i, then $\gamma = 0$ and the log-odds ratio $P_i/P_j = 1$. However, if there is hidden bias, then $P_i/P_j \neq 1$, and the Rosenbaum bounds calculate the upper bound of the bias that can be tolerated without changing the statistical significance of the treatment effect.[34]

Our estimates of Rosenbaum bounds are around 1.15 for all of our outcome variables. This implies that any hidden bias that exists must cause the log-odds ratio of being assigned to an exposed bank to differ between exposed and unexposed households by a factor of about 1.15. The magnitude of hidden bias that might call our findings into question can be illustrated using the methodology outlined in Barath et al. (2011). If a logistic regression is utilized, then the ratio of propensities will change by a factor of $1.15 = exp(\beta_k \cdot s_k \cdot n)$,

[34] For a more detailed technical discussion of Rosenbaum bounds, please see Barath et al. (2011) and Fung et al. (2014), who also provide applications of Rosenbaum bounds in a context similar to ours.

where β_k is the logit coefficient for covariate k, s_k is covariate k's standard deviation and n is the number of standard deviations that covariate k has to change by in order for the ratio of propensities to increase to 1.15. Therefore, we can solve for n for each of the continuous covariates in our model and determine how large an average change in these covariates is required in order to mimic the effect of a hidden bias. For most of our covariates, we observe that a large change would be required (a +88% change in *House Value* and a +32.5% change in *Home Equity/House Value*). The changes required for *Gross Income* and *Age* are smaller at -7.8% and -8.4%, respectively. Nevertheless, these required changes are also non-trivial and are unlikely to be plausible. Therefore, we conclude that it is unlikely that such powerful unobserved covariates exist as to render our estimates invalid.

4.6 Calculating Pre-Crisis Consumption

As discussed above, our calculation of pre-crisis consumption using equation (1) involved the assumption of no change in wealth during the pre-crisis period ($\Delta Wealth_{Pre} = 0$). To check the robustness of our findings to this specification, we calculate $\Delta Wealth_{Pre}$ for the 2,516 (out of 3,804) households that completed the survey during the earlier 2003-2004 period. This way, we can repeat our analysis with a pre-crisis consumption variable that includes wealth changes during the pre-crisis period, albeit with a reduced sample.

In addition, we consider the possibility that our decision to exclude stocks, bonds and mutual funds from our definition of *Wealth* is causing pre-crisis consumption and hence the change in consumption to be misspecified (since we use actual consumption data in the crisis-period, our crisis period difference ATTs for consumption will not be affected by any such misspecification). However, as discussed above, including the change in the amount of stock and mutual fund holdings in $\Delta Wealth$ raises the issue of whether the changes are being caused by price movements or the household buying or selling such assets.

We address this issue of "price vs. quantity changes" in stock portfolios by calculating the percent change in the general stock index (the "TSX 60") between time $t-1$ and t

for each household. Since the month in which a household completes the CFM varies, this stock market return varies across different households in our sample. We then multiply this stock market return with the household's stock holdings at time $t-1$ to find the household's capital gains and subtract them from $\Delta\, Wealth$.

Regarding mutual fund holdings, we use information on the different types of funds in the household's portfolio. CFM respondents categorize each mutual fund in their portfolios using a list of "fund types", such as "Canadian-Balanced", "US-Equity", "International/Global-Money Market", etc. Using quarterly fund return and fund size data from Morningstar, we calculate a fund size-weighted average market return for each CFM fund category. We combine these quarterly market returns with the household's total holdings of each fund type at time $t-1$ to calculate the change in the value of the fund portfolio between $t-1$ and t, which is then subtracted from $\Delta\, Wealth$.

Combining these two variations, we consider DiD ATT for total consumption based on three modified versions for our pre-crisis consumption variable and present the results in Table 12. Overall, our findings remain entirely unchanged for both the baseline analysis and the analysis when pre-crisis incomes of households are considered.

[INSERT TABLE 12 HERE]

5 Conclusion

In this paper we seek to empirically establish a link between bank funding stress, credit supply to households and consumption expenditures. If funding stress in banking adversely affects household consumption due to a leftward shift in the credit supply function, this may have first-order macroeconomic consequences. We find evidence in favor of a lending supply effect with significantly adverse consequences for the consumption expenditure of low income households. There is no corresponding effect on consumption for high income households, however, as these households can smooth their consumption by drawing down liquid assets even when faced with an adverse credit shock. After using population weights that account for the distribution of income in Canada, supply effects appear to explain more than half of the observed aggregate decline in consumption.

The results have important policy implications. For example, they suggest that some households will be able to maintain their consumption levels by drawing down liquid assets even in the face of adverse credit supply shocks. At the same time, by doing this, these households may exacerbate the funding problems of banks. The results also suggest that low income households are more severely affected by a negative shift in credit supply; as such, financial crises appear to have significant heterogeneous effects. Finally, the significant decline in aggregate consumption expenditures in Canada during the financial crisis was still in part due to consumption demand, rather than credit supply effects. This is striking, given that the Canadian economy did not experience the bursting of a housing bubble and was, by most accounts, not strongly affected in terms of fundamentals.

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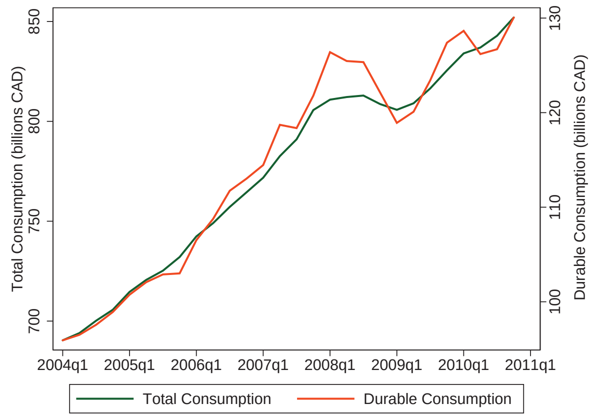
Figure 1: Total consumption expenditures, durable consumption expenditures and the growth rate of household credit in Canada for 2004q1-2010q4.

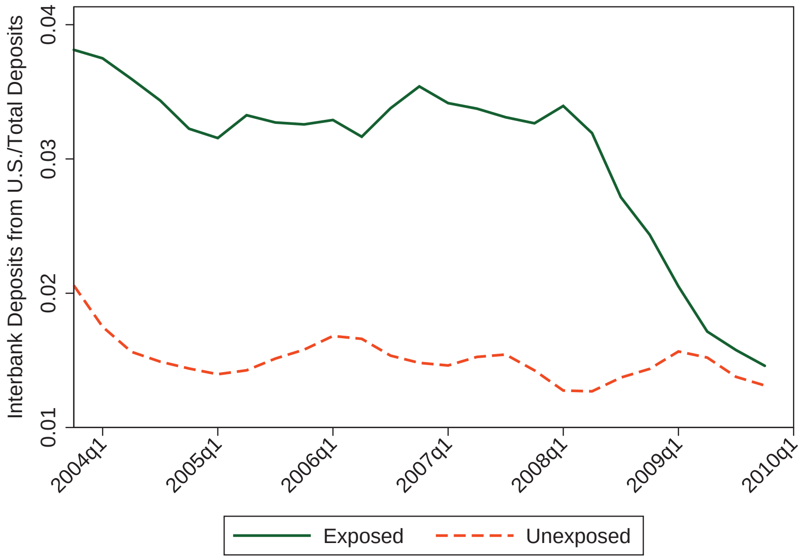


(a) Consumption Expenditures

(b) Growth Rate Household Credit

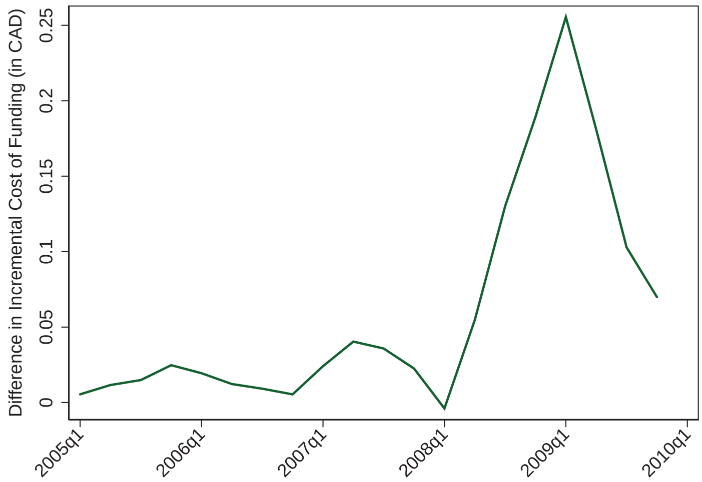
Note: Data on consumption expenditure is from Statistics Canada's *National Income and Expenditure Accounts* and household credit data comes from Bank of Canada's *Banking and Financial Statistics.*

Figure 2: Average U.S. exposure of Canadian banks, where exposure is measured as the share of interbank deposits from the United States to total deposits (credit unions excluded) for 2004q1 to 2009q4, centered five-quarter moving average.



Note: Exposure is calculated using (confidential) regulatory returns for all federally regulated deposit-taking institutions (banks, trust companies and loan companies). Credit unions are excluded from this figure, since for some of them, exposure is available only for 2006q4, being provided by provincial regulators at a confidential and qualitative basis (i.e. "below cutoff" vs. "above cutoff"), as opposed to an actual figure. For the credit unions where actual exposure data was provided, the data are at an annual (and not quarterly) basis. The categorization of banks into "exposed" vs. "unexposed" is as discussed in the text. The centered five-quarter moving average is calculated as $(Expo_{t-2} + 2 \cdot Expo_{t-1} + 2 \cdot Expo_t + 2 \cdot Expo_{t+1} + Expo_{t+2})/8$.

Figure 3: Difference between *exposed* and *unexposed* Canadian banks' average incremental costs of funding for 2005q1-2009q4



Note: Difference in the centered five-quarter moving average of incremental cost of funding for the two groups (exposed - unexposed). Incremental cost of funding is calculated as "Change in Total Interest Expense/Change in Interest Bearing Liabilities" using quarterly income statements and balance sheets of all deposit-taking institutions (including those credit unions with quarterly financial statements). Group averages are weighted using total asset size.

Figure 4: Annualized quarterly growth rate of consumer lending (excluding mortgages and personal lines of credit) for *exposed* vs. *unexposed* Canadian banks for 2006q4-2009q4.

Note: Only consumer lending within Canada is included. The data are obtained from the (confidential) regulatory return on "regional distribution of select assets and liabilities" filed by federally regulated deposit-taking institutions (banks, trust companies and loan companies), which allows for the separation of lending within and outside of Canada. Credit unions are excluded from the figure, since they do not complete a similar regulatory return. Although the largest credit unions report similar information in annual reports, the frequency of this data are annual, making it incompatible with the bank data.

Figure 5: Kernel densities of propensity scores for exposed and unexposed households.

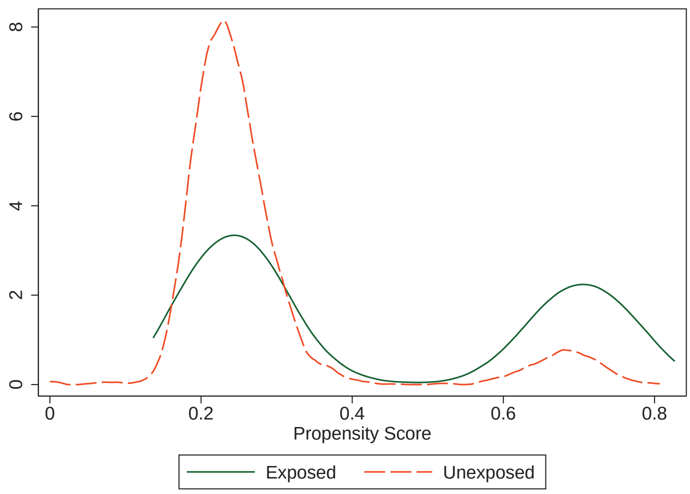


Table 1: Expenditure Questions in the *Canadian Financial Monitor*.

Variable	Time Frame	Total Spending	Durables Spending
Hydro bills (heat, water, etc.)	Last month	Yes	No
Other utilities (cable, phone, etc.)	Last month	Yes	No
Insurance Premiums	Last month	Yes	No
Rent or condo fees	Last month	Yes	No
Property/municipal taxes	Last month	Yes	No
Domestic and child care services/school	Last month	Yes	No
Groceries, including beverages	Last month	Yes	No
Food and beverages at/from restaurants /clubs/bars	Last month	Yes	No
Snacks and beverages from convenience stores	Last month	Yes	No
Recreation (movies, concerts, fitness club, etc.)	Last month	Yes	No
Health services (drugs, hospital care, vision care, chiropractor, etc.)	Last month	Yes	No
Automobile maintenance/gas	Last month	Yes	No
Public and other transportation	Last month	Yes	No
Clothing/footwear	Last month	Yes	Yes
Gifts or donations	Last month	Yes	No
Health and beauty aids/personal grooming	Last month	Yes	No
A new or used automobile/RV/motorcycle/truck	Last year	Yes	Yes
Home appliances and electronics (small or large)	Last year	Yes	Yes
Home furnishings	Last year	Yes	Yes
Vacation/trip	Last year	Yes	No
Home improvement/renovation	Last year	Yes	No

Table 2: Summary Statistics for Consumption, Credit and Liquid Asset Variables.

	Exposed Households (N = 1,246)			Unexposed Households (N = 2,558)		
Variable	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Pre-Crisis Period (2005-2006)						
Total Consumption	40723	26693	44698	45685	29794	50949
Non-Mortgage Liabilities	15998	4550	27115	18829	6250	31575
Liquid Asset Holdings	11956	4300	21315	16412	6250	27768
Crisis Period (2008-2009)						
Total Consumption	33727	29464	21354	35008	31955	20289
Durables Spending	8322	3923	10299	8649	4511	10138
Non-Mortgage Liabilities	15393	3375	26251	18107	4500	29681
Liquid Asset Holdings	14017	5350	23682	19668	7750	28973

Table 3: OLS estimation of the effect of the crisis on exposed households.

	ln(Non-Mortgage Liabilities)		*ln(Liquid Assets)*		*ln(Total Consumption)*	
Variable	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Cri · Exposed	**-0.179**	**0.104**	**-0.333**	**0.109**	-0.006	0.034
Cri	**-0.313**	**0.086**	**0.241**	**0.032**	**0.251**	**0.027**
N	7,663		7,663		7,663	
R^2	0.709		0.615		0.708	

Note: All regressions include household fixed effects and time-varying household controls. The dataset only includes households that completed the survey at least once in 2005-2006 and at least once in 2008-2009. The number of observations per household varies depending on which of the four years (2005, 2006, 2008 and 2009) the household completed the CFM survey. Standard errors are clustered at the bank level.

Table 4: Probit Estimates for banking with an exposed institution.

Variable	Coef.	Std. Err.
Home equity / house value	**0.411**	**0.249**
(Home equity / house value)^2	**-0.332**	**0.199**
ln(House value)	0.041	0.051
ln(House value)^2	**-0.006**	**0.004**
ln(Gross income)	**-1.136**	**0.619**
ln(Gross income)^2	**0.052**	**0.029**
Age	-0.019	0.014
Age^2	0.000	0.000
Single	0.022	0.06
Unemployed	-0.005	0.128
House size	0.082	0.134
(House size)^2	-0.008	0.017
Labor supply, male	-0.011	0.056
Labor supply, female	**-0.166**	**0.051**
Self-employed, male	0.142	0.093
Self-employed, female	-0.062	0.088
College some	0.083	0.075
College degree	0.04	0.061
1-2 children	0.032	0.059
3-4 children	0.022	0.17
5 children	-0.207	0.319
Big city	**-0.093**	**0.050**
Renter	-0.039	0.295
French	**1.205**	**0.057**
Risk aversion	**0.229**	**0.116**
Constant	5.940*	3.33

Note: Dependent variable = 1 if the household's bank is categorized as exposed. 32.75% of the households are classified as banking with an exposed bank. Number of households = 3,804.

Table 5: Balance of household characteristics *(two-sample t-test)*.

	Exposed	Unexposed	p-value
Home equity / house value	0.642	0.644	0.847
ln(House value)	9.813	9.821	0.956
ln(Gross income)	10.93	10.954	0.259
Age	49.489	49.642	0.626
Single	0.334	0.355	0.158
Unemployed	0.036	0.029	0.274
House size	4.479	4.408	0.366
Labor supply, male	0.562	0.555	0.646
Labor supply, female	0.603	0.589	0.281
Self-employed, male	0.073	0.087	0.103
Self-employed, female	0.065	0.076	0.169
College some	0.157	0.164	0.583
College degree	0.618	0.643	0.107
1-2 children	0.244	0.225	0.164
3-4 children	0.017	0.019	0.552
5 children	0.004	0.002	0.19
Big city	0.371	0.379	0.555
Renter	0.209	0.21	0.911
French	0.409	0.409	0.655
Risk aversion	0.464	0.463	0.895
Observations	1246	1246	

Table 6: Baseline estimation of the average effect of the crisis on exposed households.

	N	Mean Diff.	Std. Err.	p-value
ln(Liquid Assets)				
Pre-crisis (2005-06)	1246	-0.091	0.089	0.31
Crisis (2008-09)	1246	**-0.428**	**0.113**	**0.00**
Difference-in-differences	1246	**-0.338**	**0.133**	**0.01**
ln(Non-Mortgage Liabilities)				
Pre-crisis (2005-06)	1246	0.001	0.209	0.99
Crisis (2008-09)	1246	**-0.462**	**0.202**	**0.02**
Difference-in-differences	1246	**-0.463**	**0.194**	**0.02**
ln(Total Consumption and Components)				
Total Consumption, Pre-crisis (2005-06)	1246	-0.002	0.059	0.98
Total Consumption, Crisis (2008-09)	1246	-0.009	0.034	0.79
Total Consumption, Difference-in-differences	1246	-0.007	0.053	0.89
Durables, Crisis (2008-09)	1246	0.109	0.133	0.41

Note: Mean difference between exposed and unexposed households. Standard errors are calculated based on the procedure described in Abadie and Imbens (2016). N refers to the number of exposed households. Each exposed household is matched to four unexposed households. Unexposed households may be matched to several exposed households.

Table 7: Estimation of the Average Treatment Effect on the Treated (ATT) for different pre-crisis income groups.

	N	Mean Diff.	Std. Error	p-value
ln(Liquid Assets), Difference-in-Differences				
< \$60K	610	**-0.386**	**0.127**	**0.002**
≥ \$60K	636	**-0.271**	**0.212**	**0.026**
ln(Non-Mortgage Liabilities), Difference-in-Differences				
< \$60K	610	**-0.408**	**0.195**	**0.037**
≥ \$60K	636	-0.287	0.204	0.159
ln(Consumption), Difference-in-Differences				
< \$60K	610	**-0.108**	**0.051**	**0.036**
≥ \$60K	636	-0.047	0.061	0.448
ln(Consumption), Crisis				
< \$60K	610	**-0.068**	**0.029**	**0.022**
≥ \$60K	636	0.015	0.025	0.550

Note: Mean difference between exposed and unexposed households. Heteroscedasticity-robust standard errors are reported. N refers to the number of exposed households. Each exposed household is matched to four unexposed households within the same income category. Unexposed households may be matched to several exposed households.

Table 8: Income distribution across the Canadian population vs. the CFM survey and calculation of the population-weight adjusted effect on crisis period consumption.

Income Category	Share in Population	Share in CFM	No. of Exposed HHs	Consumption Effect (CAD)	Aggregate Consumption Effect (bil. CAD)
< \$60K	0.581	0.444	2,583,220	-1,819	-4.69
$\geq$ \$60K	0.419	0.556	1,543,944	0	0
Total			4,127,164		-4.69

Note: Share in Population is the percentage of Canadian families with 2005/2006 income in each income category. *Share in CFM* is the percentage of households in our panel CFM sample with pre-crisis incomes in each category. *No. of Exposed HHs* is an estimate, calculated by multiplying the share of exposed households in each income category from CFM with the total number of households in that category (not shown for brevity). *Consumption Effect* is the average difference in the crisis period consumption of an exposed vs. matched household in each income category, using the relevant ATT on crisis period consumption from Table 7 (if statistically significant). Such an ATT is then converted into an average difference in the level of consumption during the crisis $(1 - e^{ATT})$ and multiplied by the average crisis period consumption of a matched household in that income group (please see Section 3.3 for details). Finally *Aggregate Consumption Effect* is calculated as (*Consumption Effect*) $\cdot$ (*No. of Exposed HHs*). Statistics related to the entire Canadian population are calculated using data from CANSIM, table 111-0012 (Statistics Canada). Elderly households are excluded from all figures and calculations.

Table 9: Estimating the effect of the crisis on unemployment, income and house values.

	N	Mean Diff.	Std. Err.	p-value
Unemployed				
Pre-crisis	1246	0.007	0.006	0.261
Crisis	1246	0.006	0.009	0.402
Difference-in-Differences	1246	-0.001	0.007	0.786
ln(Gross Income)				
Pre-crisis	1246	-0.025	0.027	0.348
Crisis	1246	-0.046	0.029	0.116
Difference-in-Differences	1246	-0.021	0.018	0.256
ln(House Value)				
Pre-crisis	756	-0.035	0.029	0.230
Crisis	756	-0.066	0.041	0.107
Difference-in-Differences	756	-0.031	0.031	0.313

Note: Mean difference between exposed and unexposed households. Standard errors are calculated based on the procedure described in Abadie and Imbens (2016). N refers to the number of exposed households. Each exposed household is matched to four unexposed households. Unexposed households may be matched to several exposed households.

Table 10: Baseline estimation of the average effect of the crisis on exposed households when households are matched on propensity score *and* region.

	N	Mean Diff.	Std. Err.	p-value
ln(Liquid Assets), Difference-in-differences	1246	**-0.408**	**0.090**	**0.000**
ln(Non-Mortgage Liabilities), Difference-in-differences	1246	**-0.410**	**0.140**	**0.004**
Total Consumption, Difference-in-differences	1246	-0.023	0.040	0.560

Note: Mean difference between exposed and unexposed households. Heteroscedasticity-robust standard errors are reported. N refers to the number of exposed households. Each exposed household is matched to four unexposed households from the same region. The regions are Eastern Canada (Quebec, New Brunswick, Newfoundland and Labrador, Nova Scotia and Prince Edward Island), Ontario, the Prairie provinces (Manitoba and Saskatchewan), and Western Canada (Alberta and British Columbia). Unexposed households may be matched to several exposed households.

Table 11: Analysis of pre-existing trends in non-mortgage credit, income and unemployment through the estimation of a crisis effect for years prior to the pre-crisis period.

	Mean Diff.	Std. Err.	p-value	N (exposed)	N (probit)
ln(Non-Mortgage Liabilities)					
Period: 2001-2002	-0.181	0.256	0.478	515	1,390
Period: 2003-2004	-0.207	0.224	0.356	843	2,331
ln(Gross Income)					
Period: 2001-2002	-0.036	0.043	0.399	515	1,390
Period: 2003-2004	-0.034	0.027	0.208	843	2,331
Unemployed					
Period: 2001-2002	-0.008	0.014	0.581	515	1,390
Period: 2003-2004	0.001	0.005	0.904	843	2,331
ln(Credit Card Charges)					
Period: 2001-2002	-0.051	0.256	0.841	515	1,390
Period: 2003-2004	-0.083	0.198	0.673	843	2,331

Note: Mean difference between exposed and unexposed households. Standard errors are calculated based on the procedure described in Abadie and Imbens (2016). Each exposed household is matched to four unexposed households. Unexposed households may be matched to several exposed households.

Table 12: Difference-in-differences ATT estimates for total consumption for the entire sample (baseline analysis) and for different pre-crisis income groups, with pre-crisis consumption data calculated using alternate methods. The specifications using "all financial assets" include adjustments for capital gains in stock and mutual fund portfolios.

	N	Mean Diff.	Std. Err.	p-value
$\Delta\, Wealth_{Pre} \neq 0$, *no stocks, bonds, mutual funds*				
Baseline Sample	826	-0.065	0.093	0.48
Income < \$60K	407	**-0.222**	**0.073**	**0.002**
Income ≥ \$60K	419	-0.085	0.087	0.33
$\Delta\, Wealth_{Pre} \neq 0$, *all financial assets*				
Baseline Sample	826	0.059	0.087	0.49
Income < \$60K	407	**-0.146**	**0.086**	**0.088**
Income ≥ \$60K	419	0.137	0.096	0.150
$\Delta\, Wealth_{Pre} = 0$, *all financial assets*				
Baseline Sample	1246	-0.023	0.067	0.734
Income < \$60K	610	**-0.134**	**0.058**	**0.022**
Income ≥ \$60K	636	0.002	0.068	0.977

Note: Only consumption-related findings are presented given that the change in the calculation of pre-crisis consumption does not affect the analysis of liquid assets and non-mortgage liabilities. Baseline sample is matched on propensity score only, while for the pre-crisis income groups, households are matched on propensity score and income group. Mean difference between exposed and unexposed households. Abadie and Imbens (2016) standard errors are reported for the baseline sample and heteroscedasticity-robust standard errors are reported for pre-crisis income groups. N refers to the number of exposed households. Each exposed household is matched to four unexposed households. Unexposed households may be matched to several exposed households.

A For Online Publication: Appendix

A.1 Validation of CFM Consumption Data

One concern in using survey-based consumption expenditure data is data quality. Survey-based data on household expenditures can be noisy due to underreporting, along with high levels of non-participation and/or non-response rates. In our case, this raises the possibility that the lack of an ATT on consumption expenditures is not necessarily driven by consumption smoothing behavior, but is the result of noisy expenditure data. While the lack of a significant ATT for *imputed* consumption data (in section A.3) somewhat alleviates this concern, we also address this issue by following the literature and comparing "aggregated" CFM expenditure data to aggregate household consumption expenditure data from Canadian national accounts (provided by Statistics Canada). In addition, we investigate data-quality issues for 2008, given that this was the consumption expenditure questions were added to the CFM survey. Specifically, we rule out the possibility of response rates within our sample being lower in the first instance of expenditure questions being a part of the CFM survey, which could have introduced noise into our expenditure data and affected our findings.

As discussed in Bee, Meyer, and Sullivan (2015) and Barrett et al. (2015), any comparison of survey data to national accounts is complicated by the fact that the two different data sources do not cover the same spending items. Therefore, it is important to eliminate items that are covered by one data source but not the other. We perform such an adjustment by eliminating a number of spending categories from the aggregate data that are clearly not covered in the CFM. The most important of these categories are "imputed rental fees on housing", "tobacco", "games of chance", "implicit loan charges", "implicit deposit charges", "mutual funds" and "other actual financial charges".*

*In addition, we eliminate smaller categories such as "stock and bond commissions", "pets and pet food", "veterinary and other services for pets", "newspapers and periodicals", etc. Overall, we follow a conservative approach in this process and do not take out categories where part of the spending item may be covered in CFM. For example, "musical instruments and major durables for indoor recreation" is not eliminated from the aggregate data, in case some of the items in this category are included in the "recreation (movies, concerts, fitness club, etc.)" item of CFM.

After creating aggregate household spending figures that are more comparable with the CFM, we "aggregate" the CFM spending data using the following procedure: for every year between 2008 and 2012, we take the entire CFM sample, calculate and seasonally-adjust annual total consumption expenditure in the manner described in section 1.4. We also identify all households whose yearly total consumption is either missing or too low (i.e., under 3,000 CAD). These account for between 3.4% to 7.5% of the CFM sample depending on the year (after adjusting for survey weights). We impute total consumption for such households by regressing total consumption on household characteristics that influence spending amounts, and obtaining the predicted values.[†] Then, we multiply each household's annual consumption spending by its survey weight and aggregate the weighted spending for each year.

Once the micro and macro expenditure data are restricted to common spending items and the missing CFM data is imputed, it is possible to calculate a "coverage rate". The coverage rate is defined as the ratio of "aggregated" CFM data to the aggregate spending data from the national accounts. CFM's coverage rate for the period 2008-2012 is:

	2008	2009	2010	2011	2012
Coverage Rate	0.62	0.65	0.66	0.62	0.60

The coverage rate for the CFM is quite comparable to the coverage rates of household expenditure surveys for the U.S., U.K. and Australia, which range from 0.60 to 0.75 (Barrett et al. (2015)). Interestingly, the only household expenditure survey in Barrett et al. (2015) with a coverage rate around 1.00 is the Canadian Survey of Household Spending (SHS), conducted by Statistics Canada.

Although the CFM does not have a coverage ratio as high as the SHS, this is not a very surprising result. For almost all of our sample period (except 2010), the SHS involves a

[†]Specifically, we regress log(total consumption) on log(income), log(total wealth), education level, marriage status, age group (below 25, between 25 and 34, between 35 and 44, etc.) and province dummies. Each regression has an R-squared between 0.232 and 0.293 (results available upon request). Running a single regression covering 2008-2012 and with individual year dummies yields very similar results.

lengthy (around one hour and forty minutes) recall interview, where respondents are asked about their spending in the previous year. As discussed by Brzozowski and Crossley (2011), such a long recall period (asking respondents about their spending over the last *year* as opposed to the last *month*) may exacerbate recall problems, but Statistics Canada expends considerable effort to enhance data quality.[‡] Furthermore, an annual recall period might be preferable to the shorter period used for most expenditure questions in CFM (previous month), since short recall questions can miss infrequent purchases.

There is also evidence in the literature that expenditure surveys that do not involve an interviewer, such as diaries, have lower coverage ratios. For the 2010 U.S. Consumer Expenditure Survey, Bee, Meyer, and Sullivan (2015) find a coverage rate of 0.74 for the interview component, but a coverage rate of 0.57 for the diary component. Although CFM does not have a diary format, it is still completed by the respondent without the presence of an interviewer and it may suffer from similar quality of reporting problems as the ones discussed in Bee, Meyer, and Sullivan (2015). For example, the head of household that completes the CFM survey may not be fully aware of spending by other household members, and without the prompting of an interviewer, may be unlikely to invest the time and effort to find out. Factors such as these may play a role in CFM having a somewhat lower coverage rate compared to the SHS. Nevertheless, the relatively high and rather stable coverage rate of CFM, which compares quite well to a number of other well-regarded expenditure surveys, suggests that excessive noise in the CFM spending data is not a major concern.

As a final exercise, we check for the prevalence of non-response rates in the early years of the CFM's consumption questions. Given that these questions were first introduced in 2008, we want to rule out the possibility of higher than normal non-response rates and the

[‡]For example, respondents are encouraged to consult bills, receipts, credit card statements and income tax records to aid their recollection. Furthermore, a "balance edit" approach reconciles annual income, annual expenditure and asset changes. Households that appear to be significantly out of balance during this balance edit are probed further to identify possible reporting errors. If a household recalls having an expenditure but cannot specify an amount, the amount is imputed using a "nearest neighbor" method. Finally, there is extensive follow-up for households that refuse to complete the survey, via additional phone calls, letters and visits (see Brzozowski and Crossley (2011) and Barrett et al. (2015) for more details).

noise it might cause. We find that among our sample of CFM respondents, the response rate is already quite high in 2008 (at 90% or better for each individual question), with only a negligible improvement in 2009. Finally, response rates within our sample compare quite favorably to those within the CFM as a whole, even beyond 2009. Therefore, we conclude that noise coming from non-responses is unlikely to be an issue, even in the very first year that the expenditure questions are introduced.

A.2 Comparison of CFM Panelist Characteristics

Given that our panel dataset is built using the subset of households that choose to complete the CFM in multiple years, it is possible that these households have different characteristics compared to households that complete the survey once during the sample period. In order to investigate this possibility, we compared non-mortgage credit, liquid asset, banking relationship, income and other socioeconomic characteristics of our panelist households to the rest of CFM in 2006. However, given that our panel sample excludes elderly households, we also excluded elderly households from the "rest of CFM". The results of the comparison are given in Table A1 below. Overall, panelist households appear to be representative of the overall CFM sample when it comes to banking relationships and non-mortgage credit. However, panelist households appear to have higher incomes and more savings. CFM households in our panel sample are also more likely to be home owners, be older and be better educated.

[INSERT TABLE A1 HERE]

The differences in the characteristics of panelist households vs. the rest of CFM may not be very surprising; older households with higher incomes may have more spare time to participate in surveys. It should also be noted that the differences in panelist households should also bias us *against* finding a negative credit supply shock, since (as discussed above), banks may be reluctant to cut lending to higher income households who are also more likely to have better collateral (more savings and a home). Also, recall that for our aggregate effects analysis, we ensure the representativeness of the sample for the Canadian population.

A.3 Validation of Pre-Crisis Consumption Calculation Methodology

Since our analysis of difference in pre-crisis consumption and difference-in-differences in consumption use the calculated pre-crisis consumption data, we examine the precision of our calculation method via a simple exercise. Given that consumption questions in CFM start in 2008, it is possible to use the 2008 and 2009 surveys to calculate consumption for 2008 using variants of equation (1). We can then compare this "calculated consumption" to the "actual consumption" reported by survey participants.

For this exercise, we create a panel dataset in a manner similar to our empirical analysis. We identify households that completed the survey in both 2008 and 2009 (4,752 households). We then eliminate households where the youngest head of household is older than 65 in 2008, given the differences in the saving and spending patterns of retired persons. We also eliminate households with missing liabilities, savings or spending data. This yields a final sample of 2,810 households. After defining total consumption, non-mortgage liabilities, liquid assets and mortgage debt in the same manner as our main dataset, we can calculate 2008 consumption in the same manner as in Section 1.4 above:

$$\begin{aligned}
(\textit{Consumption}_{09} - \textit{Consumption}_{08}) &= (\textit{Income}_{09} - \textit{Income}_{08}) + (\textit{Mortgage Debt}_{09} \\
&- \textit{Mortgage Debt}_{08}) + (\textit{Non-mortgage Liabilities}_{09} \\
&- \textit{Non-mortgage Liabilities}_{08}) \\
&+ (\textit{Liquid Assets}_{08} - \textit{Liquid Assets}_{09})
\end{aligned}$$

In addition, we calculate 2008 consumption for the other alternate consumption calculation specifications discussed in our robustness section (Section 4.6). The calculation of 2008 with the assumption of no change in wealth during 2008 (i.e. $\Delta Wealth_{2008} = 0$) but with stocks, bonds and mutual funds included in *Wealth* is a straight-forward exercise. However, in order to calculate 2008 consumption while accounting for wealth changes during 2008, we further limit our sample to the 411 households that have completed the survey in 2007, 2008 and 2009. For these households, we re-calculate 2008 consumption where $\Delta Wealth_{2008} \neq 0$, with and without including stocks, bonds and mutual funds in *Wealth*. For the calculations that include stocks, bonds and mutual funds in the definition of wealth, we also account for market-driven changes in the values of stock and mutual fund portfolios in a manner similar to Section 4.6. Therefore, we end up with four different calculated 2008 consumption measures.

Table A2 compares “actual/reported 2008 consumption” to “calculated 2008 consumption.” Focusing on the calculation we use in our main analysis, the two distributions (columns 1 and 2) seem to match each other quite well, especially in the bottom three quartiles. This observation is confirmed by Figure A1, which is a ”quantile-quantile plot” of the distributions of actual and calculated expenditure for 2008. This plot shows that the distribution of calculated expenditure is very close to that of actual expenditure, except for households with very high levels of consumption (above 200,000 CAD). Similar patterns hold for the other three consumption variations.

[INSERT TABLE A2 HERE]

[INSERT FIGURE A1 HERE]

Following Browning and Leth-Petersen (2003) we also calculate the actual vs. calculated consumption for each individual households and report their distributional statistics in column 3 of Table A3. The figures in the table are somewhat difficult to interpret. Hence, we use the imputation of Browning and Leth-Petersen (2003) as a reference point in the literature (this imputation method was subsequently used in Leth-Petersen (2010) to estimate the consumption response of introducing home equity lines of credit in Denmark). Using the similar distributional measures provided in Table 2[§] of Browning and Leth-Petersen (2003), we conclude that the quality of our imputation is comparable to that of Browning and Leth-Petersen (2003). Please note that there are two important differences between the respective imputation approaches. Browning and Leth-Petersen (2003) have access to after tax income, while we do not. On the other hand, we observe actual consumption expenditure during the crisis period and hence only have to impute pre-crisis consumption using changes in the determinants of consumption (income, liquid assets etc.). However, it is clear that the consumption measures that assume $\Delta \mathit{Wealth}_{2008} = 0$ appear the fit the actual data better.

Internet Appendix References

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Bee, A., B. D. Meyer, and J. X. Sullivan (2015): "The Validity of Consumption Data: Are the Consumer Expenditure Interview and Diary Surveys Informative?," in *Improving the Measurement of Consumer Expenditures*, ed. by C. Carroll, T. Crossley, and J. Sabelhaus. University of Chicago Press, Chicago, IL.

Browning, M., and S. Leth-Petersen (2003): "Imputing Consumption from Income and Wealth Information," *Economic Journal*, 113, F282–F301.

Brzozowski, M., and T. F. Crossley (2011): "Measuring the Well-Being of the Poor with Income or Consumption: A Canadian Perspective," *Canadian Journal of Economics*, 44(1), 88–106.

[§] Browning and Leth-Petersen (2003), page F290. We chose to compare our results to Table 2 ("house owners and people in cooperative housing") as opposed to Table 1 ("renters") since most of our CFM respondents are home-owners.

Leth-Petersen, S. (2010): "Intertemporal Consumption and Credit Constraints: Does Total Expenditure Respond to an Exogenous Shock to Credit?," *American Economic Review*, 100(3), 1080–1103.

Figure A1: Quantile-Quantile plot of actual vs. calculated consumption expenditure (as described in Section 1.4) of 2008 CFM survey respondents

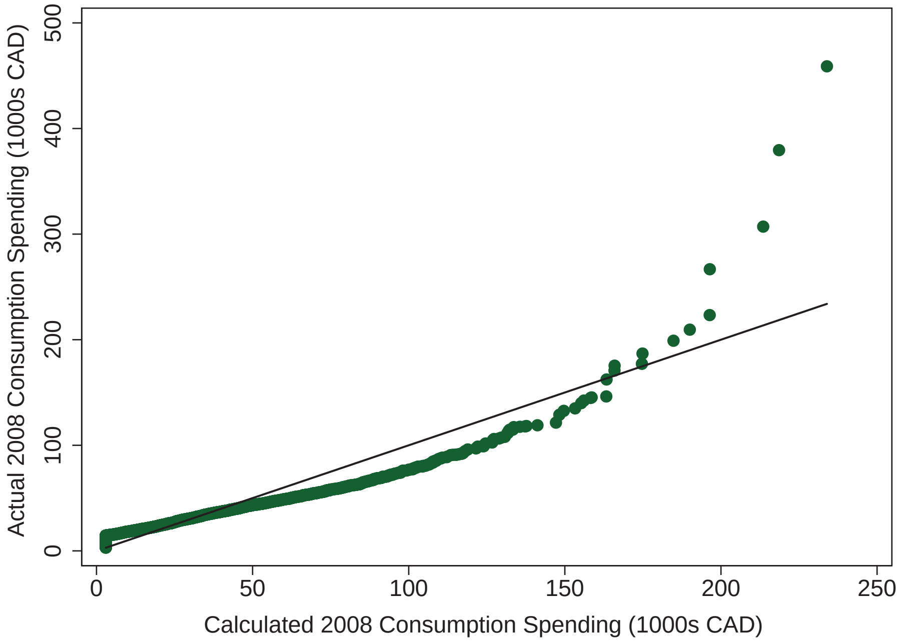

Table A1: Mean comparison test of financial and socioeconomic characteristics for panel CFM households vs. the rest-of-CFM *(two-sample t-test)*

	Panelist	Rest-of-CFM	p-value
No. of Banks	1.410	1.393	0.176
ln(Non-mortgage liabilities)	6.227	6.271	0.692
ln(Liquid assets)	8.344	7.593	0.000
ln(Gross income)	10.973	10.795	0.000
Non-mortgage liabilities/Gross income	0.302	0.314	0.461
Age	49.383	45.011	0.000
Renter	0.178	0.276	0.000
Single	0.323	0.326	0.780
College some	0.162	0.191	0.002
College degree	0.631	0.562	0.000
Observations	2,389	6,340	

Note: The comparison is for the year 2006 (a comparison for 2005 also yields similar results). Elderly households (youngest head of household is older than 65) are excluded from both samples.

Table A2: Comparison of distributional measures for actual vs. calculated consumption expenditure in 2008, calculated according to the different specifications considered in the analysis (using 2007, 2008 and 2009 CFM surveys). The specifications using "all financial assets" include adjustments for capital gains in the stock and mutual fund portfolios.

	Actual	Calculated	Actual - Calculated
$\Delta Wealth_{Pre} = 0$, *no stocks, bonds, mutual funds. N: 2,810*			
Minimum	3,067	3,000	-208,630
25th Percentile	18,609	10,707	-17,650
Median	29,320	27,484	359
Mean	33,844	34,902	-1058
75th Percentile	43,605	50,279	16,156
Maximum	458,895	233,949	422,606
$\Delta Wealth_{Pre} \neq 0$, *no stocks, bonds, mutual funds. N: 411*			
Minimum	3,048	3,000	-162,624
25th Percentile	12,273	3,000	-28,108
Median	20,361	21,289	-7,103
Mean	28,174	35,277	569
75th Percentile	56,294	47,972	13,364
Maximum	423,465	207,909	420,465
$\Delta Wealth_{Pre} \neq 0$, *all financial assets. N: 411*			
Minimum	3,048	3,000	-385,576
25th Percentile	12,273	3,000	-44,702
Median	20,361	17,250	1,916
Mean	28,174	49,409	-21,235
75th Percentile	35,977	74,093	17,148
Maximum	423,465	434,793	420,465
$\Delta Wealth_{Pre} = 0$, *all financial assets. N: 2,810*			
Minimum	3,067	3,000	-212,602
25th Percentile	18,609	3,000	-20,488
Median	29,320	24,922	1,313
Mean	33,844	38,523	-4,683
75th Percentile	43,605	53,243	18,846
Maximum	458,895	297,248	390,893

Note: For consistency with the definition of actual consumption in Section 1.4, all values of calculated consumption below 3,000 CAD has been replaced with 3,000 CAD.

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