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SAFE

Working Paper Series

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Banks' financial distress, lending supply and consumption expenditure

SAFE Working Paper Series No. 39

Non-Technical Summary

This paper studies the effects of bank financial distress on household consumption. If financial distress in banking adversely affects household consumption, due to for instance exacerbating household credit constraints, this may have first-order macroeconomic consequences and would exacerbate the real effects of banking distress. To our knowledge, this is the first paper that attempts to identify the effect of bank distress on consumption.

We examine this question using Canadian data. Aggregate data for Canada suggest that there was noticeable dip in credit to households, in consumption and even more substantial in durable consumption in 2008/2009 relative to 2007 with a subsequent (weak) recovery in 2010. This is despite the fact that Canadian banks were only affected by the US financial crisis in as much as they depended on short-term finance in the US money market. In this paper, we attempt to distinguish how much of this dip is due to households reducing demand for consumption in the face of the financial crisis, versus banks reducing loan supply, forcing households to reduce their consumption.

In this paper we use propensity score matching techniques to disentangle supply and demand effects. We document a statistically and economically significant reduction in non-mortgage credit supply of distressed banks to households - on the order of 8.1 billion Canadian Dollars (a 2.2 per cent decline). However, we also show that a temporary short run contraction in credit supply to households has only a negligible effect on consumption. Most households that are faced with an inability to borrow do not reduce consumption expenditures, but rather draw down liquid assets to maintain a smooth consumption stream. We show a reduction in consumption only for households that do not have sufficient liquid assets to compensate for the decline in access to credit. Overall, the results are consistent with the permanent income hypothesis and consumption smoothing and suggest that short-run contractions in credit supply to households may only have mild effects on consumption expenditure.

The results have important policy implications. For example, they suggest that households will not reduce their consumption based on temporary credit supply shocks, as long as they can draw on liquid assets. This stands in stark contrast to recent results for firms, where even a short run contraction in credit supply affected firm investment and employment. At the same time, households by drawing down liquid assets, for example time deposits with banks, may have exacerbated the funding problems of these banks. Further, our results suggest that the significant decline in aggregate consumption expenditures during the crisis observed in Canada was largely unrelated to credit supply, but rather consumption demand. 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. The results presented in this paper suggest that there was a "pure" contagion effect at work: Canadian households reduced consumption expenditure, because they were unsure about how the crisis in the US and elsewhere would affect their future economic wellbeing ("CNN effect").

Banks' financial distress, lending supply and consumption expenditure*

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February 2014

Abstract

We employ a unique identification strategy linking survey data on household consumption expenditure to bank-level data to estimate the effects of bank financial distress 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, does not result in lower levels of consumption. Households compensate by drawing down liquid assets to smooth consumption in the face of a temporary adverse lending supply shock. The results contrast with recent evidence on the real effects of finance on firms' investment and employment decisions.

Keywords: Credit supply, banking, financial crisis, consumption expenditure, liquid assets, consumption smoothing
JEL classification: E21, E44, G21, G01

*We would like to thank Jason Allen, Christopher Carroll, Michael Ehrmann, Martin Goetz, Kim Huynh, John Krainer, Lorenz Kueng, Oren Levintal, Jim MacGee, Leonard Nakamura, Deyan Radev, Nicolas Serrano-Velarde, Jim Stock, Francesco Trebbi, and conference/seminar participants at the Joint Central Bank Conference at the Federal Reserve Bank of Cleveland, Frankfurt/Mannheim Macro Workshop, CAREFIN/Bocconi Conference on Financing the Recovery After the Crisis, 11th International Industrial Organization Conference, IBEFA 2012 Summer Meetings, Goethe University Frankfurt, the European Central Bank, the Federal Reserve Bank of San Francisco, the Bank of Israel and the Bank of Canada for valuable comments at various stages of this paper. The paper was written while Gropp was a Duisenberg research fellow at the European Central Bank and the hospitality of the ECB is gratefully acknowledged. The views expressed in this paper are our own and do not necessarily reflect the views of the Bank of Canada, the Eurosystem or the European Central Bank.

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This paper studies the effects of bank financial distress on household consumption. If financial distress in banking adversely affects household consumption, due to, for instance, exacerbating household credit constraints, this may have first-order macroeconomic consequences and would exacerbate the real effects of banking distress. To our knowledge, this is the first paper that attempts to identify the transmission from banks' financial distress to household consumption expenditure.

Using Canadian household data before and during the financial crisis, we document a statistically and economically significant reduction in the non-mortgage credit supply of distressed banks[1] to households - on the order of 38.2 billion Canadian dollars (a 13.6 per cent reduction). However, we also show that this temporary short-run contraction in credit supply to households has only a negligible effect on consumption. Most households that are faced with an inability to borrow do not reduce consumption expenditures, but rather draw down liquid assets to maintain a smooth consumption stream. We show a reduction in consumption only for households that do not have sufficient liquid assets to compensate for the decline in access to credit. Overall, the results are consistent with the permanent income hypothesis and consumption smoothing, and suggest that short-run contractions in credit supply to households may only have mild effects on consumption expenditure.

Aggregate Canadian data suggest that there was a noticeable dip in credit to households, 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 only affected by the U.S. financial crisis inasmuch as they depended on short-term finance in the U.S. money market. Attempting to distinguish how much of this dip is due to households reducing their demand for consumption in the face of the financial crisis[2] versus banks reducing the loan supply is the challenge in the identification strategy 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 the household and its 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. money market, which we assume is exogenous to household behaviour. We use this information to distinguish banks with high exposure to the

[1]As discussed in more detail below, we define "distress" only as the inability to obtain short-term funding from the United States, which might cause a reduction in the amount of credit supplied by banks. We do not consider more severe forms of distress such as insolvency or failure, which often require a bailout, given that there were no such instances in Canada during the recent financial crisis.

[2]The so-called "CNN effect": Canadians, even though they were not directly affected by the crisis, may have reduced or postponed demand for large consumption items simply in the face of reporting from the United States.

United States (referred to as "exposed banks") from those with low or no exposure (referred to as "unexposed banks").

The paper links the literature on consumption smoothing with that on the real effects of finance. Adverse selection models of credit (e.g., Stiglitz and Weiss (1981)) would suggest that it may be optimal to cut off some households from credit entirely, rather than charge them higher interest rates to compensate for higher risk. In the presence of such frictions, changes in lending supply may affect household expenditures. At the same time, the standard life cycle/permanent income model predicts that temporary changes in access to credit have no effect on expenditure patterns.

Several authors have investigated these questions using variation from quasi-natural experiments. For example, Agarwal, Liu and Souleles (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. Alessie, Hochguertel and Weber (2005) use the introduction of a usury law that limits interest rate charges on consumer loans and document a positive effect on the demand for credit. 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. Most recently, 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. Finally, Mian and Sufi (2010) show that households with high leverage as of 2006 exhibited a sharp relative decline in durable consumption starting in the third quarter of 2006 and continuing throughout the financial crisis of 2008/2009. However, they do not attempt to distinguish demand from credit supply effects.

Our findings also contribute to the literature on the impact of income shocks on consumer expenditure. Although we examine the effects of a reduction in the supply of credit, as opposed to lower income, both of these shocks tighten the current constraints faced by households and are likely to have similar effects on spending. Existing studies on the response of consumption to income changes, however, have mostly focused on *permanent* shocks. The literature (for recent surveys see Jappelli and Pistaferri (2010) and Meghir and Pistaferri (2011)) would suggest that permanent and temporary changes in credit supply to households would have quite different effects on consumption expenditure. In particular, as long as households expect credit conditions to improve in the future, they may offset a decline in credit supply through drawing down assets in order to maintain consumption.

It should also be noted that our findings are based on households simultaneously carrying debt and holding liquid assets. Specifically, for the negative credit supply to have an impact on household spending during the crisis period, one expects households to be using debt for spending (or investment) even during the pre-crisis period. On the other hand, most households use liquid assets to smooth consumption during the crisis, so the households in our sample are in fact keeping liquid assets and carrying debt at the same time. Although puzzling upon first glance, this behavior has been frequently observed in the literature (for example, by Gross and Souleles (2002)). Among the few proposed explanations, 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 be loosely considered as a type of precautionary savings. We do not take a stance on why the households in our sample might be displaying this behavior, since the precise mechanism is not directly relevant for our results.

Our results contrast with recent findings on the effect of adverse lending supply shocks on investment.[3] Temporary contractions of lending supply tend to affect investment spending and employment by firms. For example, Campello, Graham and Harvey (2010) show that credit-constrained firms planned deeper cuts in spending and employment. They also find that the inability to borrow externally caused many firms to circumvent attractive investment opportunities. Dell'Ariccia, Detragiache and Rajan (2008) find evidence that business sectors more dependent on external finance perform relatively worse during banking crises. Puri, Rocholl and Steffen (2011), using an empirical approach similar to ours, show that banks with a larger exposure to the recent financial crisis reduced credit to firms by a larger amount.[4]

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

[3]Cohen-Cole et al. (2008) show that credit supply declined during the crisis. However, it did not decline by as much for banks with a larger reliance on retail deposits (Ivashina and Scharfstein (2010); Gozzi and Goetz (2010)). Furthermore, banks that incurred larger subprime losses charged their corporate borrowers higher loan rates (Santos (2011)).

[4]Earlier contributions to the literature on the effect of lending supply shocks include Peek and Rosengren (1997, 2000) and Peek, Rosengren and Tootell (2003).

data that (i) capture exogenous adverse shocks to bank balance sheets that affect the loan supply, (ii) identify variation in these exogenous shocks across banks, (iii) provide detailed information on household characteristics, banking habits, and consumption patterns, and (iv) link household information to bank 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 the second half of 2008, while staying 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 regarding 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 the Tri-Agency Database System and contain the quarterly regulatory returns of all federally chartered banks, including a 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.[5]

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.[6] A sample of approximately 12,000 households is chosen out of a pool of about 60,000 units that indicate in advance their participation interest. Although the CFM is a repeated cross-sectional survey and is not designed as a panel, some households complete the survey more than once, usually in consecutive years, before dropping out of the respondent pool, which is frequently refreshed. We use such households to create a panel subsample. The CFM usually tends to oversample higher-income and older households, but our empirical methodology is designed to deal with this selection issue, as discussed below.[7]

[5]In Canada, all credit unions are regulated at the provincial level.

[6]The data set has been used in previous research, for example by Allen, Clark and Houde (2008) and Kartashova and Tomlin (2013).

[7]The 2008/2009 survey is divided into nine distinct sections that ask respondents detailed questions about their banking habits, account holdings and usages (checking, savings, credit cards), outstanding debts (mortgages, personal loans, lines of credit, leases, mortgage refinancing), insurance policies, expenditures on durable and non-durable goods, and investments (guaranteed investment certificates, bonds, stocks, and mutual funds). Finally, the survey identifies

The CFM also contains detailed demographic information, such as household composition, age, household income, occupation and employment status. These variables are used to further control for possible demand effects. Finally, the survey allows us to calculate household savings, which is an important variable that facilitates consumption smoothing in the face of a (short-term) unavailability of credit.

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

1.2 Bank Exposure Sample Construction

In the CFM survey, 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 the credit unions in this list 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 to these six banks when it comes to retail banking activities.[8] In our final panel sample, described in detail below, around 72% of respondents report having a Big Six bank as one of their main financial institutions. Around 16% bank with institutions that can be categorized as "credit unions." Most of the remaining households bank with low- or no-fee banks that primarily operate online.[9]

We use the share of interbank deposits from the United States in total deposits at 2006Q4 as a proxy for a bank's exposure to the United States prior to the start of the crisis ($Exposure$).[10] Concentrating on interbank deposits from the United States allows us to identify whether tightness in U.S. funding markets were transmitted to the Canadian household sector. As shown in Figure 2, Canadian banks' use of such interbank deposits steadily declined after 2008Q1, potentially capturing the unavailability of such funds once the crisis started. We separate the banks into "exposed" and

households' attitudes and profiles.

[8] For brevity's sake, we will refer to all financial institutions in our sample as "banks."

[9] 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 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.

[10] The share of interbank deposits from the United States is highly correlated with other measures of U.S. exposure, such as deposits of Canadian banks in the United States, or even a more general reliance on wholesale funding (see section 4 and the online appendix).

"unexposed" categories based on our observation that *Exposure* features a clear natural break around 3%. 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. 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. In the robustness section, we confirm the results by classifying banks according to the extent to which they relied on wholesale funding, motivated by the recent literature on the effect of the financial crisis on bank lending to firms (Ivashina and Scharfstein (2010)).[11]

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" institution, then it obtains that institution's classification. If the household reports more than one "main" institution, then it is classified as exposed only if all banks are exposed. This is a conservative approach, because as long as the household transacts with at least one institution that is unexposed, that household can satisfy its consumption needs by obtaining loans through the unexposed institution.

Figure 3 compares the lending behavior between exposed and unexposed banks. We define lending as the annual growth rate in CPI-adjusted consumer loans made within Canada.[12] In general, 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, while remaining relatively constant for unexposed institutions. The patterns in Figure 3 support our approach to categorizing Canadian institutions.

[11] 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.

[12] 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 (which will include the HELOCs not reported as mortgages) from the figure avoids this inconsistency.

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 a panel sample of households. We start by determining the "crisis" and "pre-crisis" periods. We assume that January 2008 to December 2009 is the crisis period and define the pre-crisis period as January 2005 to December 2006. We leave 2007 out of our analysis, since it is not clear whether it would belong in the pre-crisis or the crisis periods.[13]

Having determined the pre-crisis and crisis periods, we identify the households that repeat in 2005 or 2006 and the 2008 or 2009 CFM surveys. We treat households that show up in both the 2008 and 2009 surveys as two distinct observations in order to maximize the size of our panel data set (since we are primarily interested in the crisis level of consumption). For households that appear in 2005 and 2006, we keep only the 2006 survey response. As in Leth-Petersen (2010), we remove all households where the youngest head of the household (male or female) is older than 65, to avoid interference from retirement decisions.

The exposure of a household is determined by whether the household's stated main financial institution fell into the "exposed" or "unexposed" category in 2006Q4, as discussed above. After eliminating households with missing matching covariates, missing main institution data and zero/negative consumption (discussed below), our resulting sample consists of 3,804 households, of which 1,246 do their day-to-day banking with an exposed bank.

1.4 Consumption, Credit and Liquid Asset Variables

Starting in 2008, the CFM includes a section titled "Household Expenditure," in whick respondents state how much they approximately spent on sixteen items during the past month and on an additional five items during the past year. These expenditure questions and their respective time frames (last month vs. last year) are given in 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.[14]

Using the answers to the expenditure questions, the "total consumption" of each household is

[13] 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 in Canada as early as mid-2007.

[14] 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.

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. 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 the total consumption figures 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%, in order to ensure that the households who consistently choose the top or the bottom bins are not driving our results.

To separate any effects of bank lending on subcategories of consumption, we also construct "durables spending" and "luxury spending" variables. The different items included in each of these categories are

$$\begin{aligned} \textit{Durables} &= \textit{Clothing/Footwear} + \textit{New or Used Car, Truck, etc.} + \textit{Home Furnishings} \\ &+ \textit{Home Appliances and Electronics}, \\ \textit{Luxuries} &= \textit{Vacation/Trip} + \textit{Food/Beverages at Restaurants/Clubs/Bars} + \textit{Recreation}. \end{aligned}$$

Given that the section on spending was added to the CFM in 2008, we do not have pre-crisis spending data, and for consumption expenditure we are unable to perform a difference-in-differences analysis. We are, however, able to calculate total non-mortgage liabilities and liquid asset holdings for both the pre-crisis and crisis periods:

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

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 reported to be 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 quite 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.

Summary statistics for all of our consumption, liability and liquid asset variables are reported in Table 2. The table shows 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 and a decrease in the average non-mortgage liabilities of both groups of households during the crisis. Regardless, 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.

2 Empirical Methodology

2.1 Difference-in-Differences

There are at least two possible ways in which financial distress from banks is transmitted to households. First, banks may simply charge higher interest rates for equally qualified households. The literature shows that risk premia may increase in crisis periods (Santos (2011)). This would imply that the effect of bank financial distress on households depends on the elasticity of demand for loans, which may vary across households. Second, banks may engage in credit rationing (Stiglitz and Weiss (1981)), with some households becoming unable to obtain the desired amount of credit at any interest rate. This channel suggests that banks' financial distress increases the proportion of credit-constrained households but does not affect households with financial slack. In this paper, we will focus on quantities of credit, rather than prices, without also implying that higher interest rates may not be operable in addition to what we identify. Following Johnson, Parker and Souleles (2006) and Leth-Petersen (2010), the starting point for the econometric analysis is a difference-in-differences (DID) model of the form

$$Q_{ikt} = \beta_0 + \beta_1 \cdot Cri_t + \beta_2 \cdot Exposed_k + \beta_3 \cdot Cri_t \cdot Exposed_k + B \cdot X_t' + \delta_i + \gamma_k + \epsilon_{ikt}, \tag{1}$$

where Q_{ikt} represents some financial measure of household i at time t affiliated with bank k, $Exposed_k$ represents a dummy indicating that bank k had high exposure to the U.S. market in 2006 as defined in section 1.2, Cri_t indicates the crisis period, X represents a set of controls, and δ_i, γ_k represent household and bank fixed effects, respectively. β_3 measures the effect on Q for households that bank with an exposed institution during the crisis.

2.2 Matching and the Choice of Covariates

Our identification strategy relies on identifying a sample of households that are characterized by an identical demand for credit and differ only in whether they are affiliated with an exposed or unexposed bank.[15] Clearly, households may not be randomly assigned to banks. It is possible that banks with high exposure to the crisis had significantly 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.[16] At the same time, estimating equation (1) using ordinary least squares (OLS) may expose us to the sensitivity of OLS to differences in the covariate distribution between households affiliated with high-exposure banks and households affiliated with low-exposure banks.

Instead, we use propensity score matching to obtain estimates of β_3. 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

$$\begin{aligned} &E[(Q^1_{i,Cri} - Q^1_{i,Pre}) - (Q^0_{i,Cri} - Q^0_{i,Pre}) | Exposed = 1, X_{i,Pre}] = \\ &E[(Q^1_{i,Cri} - Q^1_{i,Pre}) | Exposed = 1, X_{i,Pre}] - E[(Q^0_{i,Cri} - Q^0_{i,Pre}) | Exposed = 1, X_{i,Pre}], \end{aligned} \tag{2}$$

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 pre-crisis and crisis periods. Equation

[15] The identification does not rely 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.

[16] 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 exposure to the crisis.

(2) measures the difference in consumption expenditures between exposed and unexposed households during the crisis period relative to the non-crisis period. This corresponds to β_3 in equation (1) 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 (2). It is a counterfactual; i.e., the change in consumption expenditure of households affiliated with an exposed bank had they been affiliated with an unexposed bank. We can, however, still recover the causal effect β_3 if the assignment of a household to a bank is random conditioning on $X_{i,Pre}$. We follow the matching procedure suggested by Abadie and Imbens (2006) to estimate the counterfactual. For each household in the exposed group, we obtain the closest four matches from the unexposed group,[17] 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 that the same unexposed household can be matched with different exposed households. Reusing observations minimizes the risk that the unexposed households do not look like their exposed matches, but potentially at the expense of a loss of precision.

Choosing covariates is crucial, but unfortunately there is no formal approach for doing so. The goal is to compare consumption patterns of households that have identical characteristics that may be related to consumption and borrowing, and that differ only in their choice of a banking institution (i.e., an exposed vs. an unexposed bank). Therefore, we use 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

[17] 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) procedure. We choose four matches, given our sample size and the number of households in our control sample.

indicating whether the household's main language is French, and an indicator variable controlling for the household's level of risk aversion.[18] In addition, the model 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. As discussed above, the sample includes 3,804 panel observations where each household is present at least once in both the pre-crisis (2005-2006) and crisis (2008-2009) periods, and we are able to identify the household's main bank and its exposure (along with having data on all of the probit variables for the pre-crisis period). About 32% of the households are classified as exposed.

The estimation results are reported in Table 3 and suggest that households with an exposed bank are quite similar to households with an unexposed bank even in the raw data. Only a few of the determinants are significant. For instance, 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 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.

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 4. 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 4, there is ample common support between exposed and unexposed households, alleviating these concerns.[19]

[18] The risk aversion variable is calculated using another new 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.

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

3.2 Main Results

This section reports our main results of estimating the average effect on the variables of interest (consumption expenditure, non-mortgage liabilities and liquid assets) of banking with an exposed institution. We start with effects in the overall sample and subsequently move to some subsamples of households that may suffer more from a reduction in credit supply. Ideally, we would compare consumption patterns between the two groups in the pre-crisis and crisis periods, but since the survey starts covering consumption in 2008, we observe this variable only in the crisis period. Hence we initially focus on the changes in liquid assets and non-mortgage liabilities, which are reported throughout the analysis period, and differences in the level of consumption expenditures during the crisis between exposed and unexposed households.[20] As long as our matching procedure is successful, differences in crisis consumption should be informative about the effect of being with an exposed bank. We report the results for imputed consumption using a DID framework in the robustness section.

The results in Table 5 indicate that, overall, there was a significantly negative effect on the level of liquid assets and non-mortgage liabilities for the exposed households (i.e., negative and significant ATTs), along with an insignificant effect on consumption during the crisis. First, we note that exposed households are indistinguishable from unexposed households in the 2005-2006 period (i.e., pre-crisis). The differences between the groups with respect to liquid assets and non-mortgage liabilities are statistically insignificant. However, during the 2008-2009 period (i.e., the crisis), exposed households report relatively lower log levels of non-mortgage liabilities (the ATT is -0.462, which translates to a 37% difference) and liquid assets (an ATT of -0.428, or a 34% difference). The DID is statistically significant at the 5% and 1% levels, respectively. Households banking with exposed institutions report significantly lower non-mortgage liabilities compared to households with unexposed institutions that otherwise exhibit identical observables. Second, there is no evidence that consumption patterns between the two groups are different in the crisis period. This is the central finding of our study: faced with banks' inability to lend, customers of affected institutions, rather than reduce consumption, draw down their liquid assets. This is consistent with consumption smoothing in the face of temporary shocks, as predicted by the literature (e.g., Jappelli and Pistaferri (2010)). It also suggests that Canadian households perceive their inability to obtain credit as temporary, rather than permanent. We explore some of the macroeconomic consequences

[20] The difference in the level of consumption between exposed and unexposed households can be represented as $(Q^1_{i,Cri} - Q^0_{i,Cri})$, which is easily obtained by rearranging the terms in equation (2).

of this finding below.[21]

Our next step addresses two important concerns that arise out of the link between exposure to the crisis and the level of non-mortgage liabilities. First, although the nature of our matching procedure makes a demand shock unlikely, we would like to provide additional evidence that what we are observing is a *supply shock* and not a demand-driven decrease in borrowing by exposed households. Second, in light of the extensive literature on credit constraints and consumption patterns, we further investigate whether the lower levels of borrowing are more pronounced among exposed households that were more likely to become credit constrained during the crisis. If the likelihood of becoming credit constrained during the crisis plays a role in the borrowing patterns, then accounting for this variable can improve our matching procedure and allow us to uncover any consumption effects that might exist among financially constrained and exposed households.

We address these concerns by looking for systematic differences across the two groups of households while controlling for their likelihood of becoming credit constrained. In order to identify households that are more likely to be financially constrained, we consider all households who are homeowners and have at least a 20 per cent equity stake in their house during the pre-crisis period. Within this subsample, we define financially constrained households as those without a home equity line of credit (HELOC). These are households that have equity in their homes, but they do not have the means to extract it.[22] This makes such households more constrained compared to households with both the equity and the means to extract it. During times of financial stress, banks might be reluctant to grant new HELOCs even to households with sufficient equity, but they will be much less likely to prevent customers from drawing down existing HELOCs.

We use a HELOC-based definition of financial constraint, since HELOCs have higher credit limits, more-flexible payment terms and lower borrowing rates than other kinds of revolving consumer credits (DBRS, 2012). Moreover, Hurst and Stafford (2004) argue that households use their housing equity as a "financial buffer," which is accessed via HELOCs or refinancing when needed. In our context, it is likely that at least some Canadian households attempted to extract home equity during the crisis period. Therefore, any differences in the ATTs for non-mortgage liabilities between constrained and unconstrained households will be additional proof of a supply effect. If the link between exposure and non-mortgage liabilities discussed above is driven by demand, then we should

[21] As a robustness check, we also estimate equation (1) by OLS using the same covariates as in the probit analysis (Table 3) with clustered standard errors at the bank level. We obtain results that are statistically and economically consistent with the ATTs reported in Table 5.

[22] Given that our constraint definition is based on having, and potentially extracting, home equity, we eliminate renters, households with less than 20% equity (the minimum required by regulators to qualify for a HELOC) and households that switch home ownership status between the pre-crisis and crisis periods.

expect to see negative ATTs on non-mortgage liabilities for both constrained and unconstrained households. However, if a negative treatment effect is observed for the constrained households only, then we can argue that all exposed households attempted to extract home equity during the crisis period, but only those with an existing HELOC were able to do so.[23] The implication is that exposed households without HELOCs were unable to obtain a HELOC, making a credit supply shock the likely explanation.

Once the households are classified, we follow the same procedure and match on propensity score and credit constraint. Estimates of the ATTs for constrained vs. unconstrained households are reported in Table 6. Considering financially constrained households first, as before, the two groups (exposed vs. unexposed) are indistinguishable in the pre-crisis period, since there are no significant differences in the levels of liquid assets and non-mortgage liabilities. However, during the crisis, the differences between the two groups become significant as constrained exposed households report lower log levels of liquid assets (the ATT implies a 26% difference) and non-mortgage liabilities (52%), with similar statistical significance for the DID estimators. As for consumption, we find no evidence of differences between the two groups during the crisis period. The results suggest that while constrained households are more affected by the treatment (being with an exposed bank) in terms of their non-mortgage liabilities, they are able to compensate for the inability to borrow by drawing down liquid assets. Consumption is unaffected even for these households.

Our findings regarding non-mortgage liabilities and liquid assets strongly point to the presence of a supply shock that affected financially constrained households. While exposed but unconstrained households were able to draw down their HELOCs, exposed and constrained households were unable to obtain the means to extract their home equity. Subsequently, constrained and exposed households used liquid assets to smooth their consumption, while unconstrained households' liquid assets remained relatively unchanged between the pre-crisis and crisis periods. We conjecture that the ability of constrained and exposed households to draw down their liquid assets also explains the absence of a consumption effect in the face of a negative credit supply shock.

Despite the absence of a consumption effect in our analysis so far, it is still possible that banking with an exposed financial institution can lead to lower consumption expenditures for households that are both illiquid and exposed. Given that exposure is associated with lower levels of non-

[23]We do not have a stance on whether households attempted to access home equity in order to smooth income shocks or to take advantage of stimulus programs such as "home renovation tax credit," low interest rates on new automobiles or other programs that were available in Canada during the crisis. In other words, we do not attempt to distinguish between the "financial motivation" and the "consumption-smoothing motivation" discussed in Hurst and Stafford (2004).

mortgage liabilities, households that have little or no liquid assets to compensate may end up lowering consumption. We investigate this possibility by concentrating on the distribution of the ATT on total consumption for our baseline analysis (which does not account for the financial constraint variable). Although this treatment effect is small and negative, its distribution (plotted in Figure 5) shows a left tail with large negative values. It is possible that the matches with such negative treatment effects on consumption spending involve households with low liquid asset holdings.

Our specific approach involves a comparison between the matches that are in the bottom quartile of the treatment effect on total consumption expenditure and the rest of the sample. Splitting the matches into these two groups enables us to calculate average treatment effects within each sample, and allows us to determine whether ATTs of other variables are also different for the matches with large and negative treatment effects on total consumption spending. In addition, we can also determine whether the pre-crisis liquid asset holdings of the exposed households involved in such matches are significantly lower than the rest of the exposed households.

The results of this analysis are reported in Table 7 and they broadly confirm that low levels of liquid asset holdings are associated with a negative treatment effect on consumption expenditure. The ATTs on total consumption (by design), durables and luxuries are significantly more negative for the bottom quartile of the total consumption treatment effect distribution, and the differences in the ATTs between the two groups are statistically significant. The ATTs on the change in non-mortgage liabilities and liquid assets, on the other hand, are the same across the two groups, suggesting that the exposed households in both groups experienced a similar (negative) credit supply shock. The difference in their consumption spending can be explained by the lower pre-crisis liquid asset holdings of the exposed households in the bottom quartile. This difference in liquid asset holdings (which is statistically significant) explains why the exposed households in the bottom quartile were unable to compensate for the credit supply shock by using their liquid assets.

3.3 Economic Magnitudes

In this section, we report the micro- and macroeconomic effects of our findings. The ATT in non-mortgage liabilities between exposed and matched households of -0.462 from Table 5 translates into a 37% difference between the levels of exposed and matched households' non-mortgage liabilities during the crisis. Since the average level of non-mortgage liabilities held by matched households during the crisis is 16,451 Canadian dollars (CAD), this implies an average difference of 6,086 CAD in non-mortgage liabilities between an exposed household and a matched household. Correspondingly, the ATT of -0.428 for liquid assets in Table 5 translates into a 34% difference in the levels of liquid assets between exposed and matched households. The average level of liquid assets held by matched households during the crisis is 17,503 CAD, implying an average difference of 5,951 CAD between an exposed household and a matched household. Hence, the CAD reduction in borrowing is almost completely offset by a corresponding drawdown of liquid assets, resulting in a zero consumption effect. This figure is also in line with the lack of an overall consumption effect, given that the average liquid asset holdings of exposed households before the crisis are 11,956 CAD.[24]

Next, in order to obtain some sense of the macroeconomic magnitudes, we return to the full 2008 and 2009 CFM samples (independent of whether the household also completed the survey in the pre-crisis period or not). As before, we exclude the households where the youngest head of household is older than 65. Out of the remaining 17,693 households, we identify 6,742 as transacting mainly with exposed banks. We then use the survey weights of these exposed households to create the population of affected households during the crisis period (i.e., 2008-2009). This is done quarterly, based on when the exposed household completed the survey. We sum the weights for each quarter and multiply this sum with the average difference in non-mortgage liabilities (6,086 CAD) to create the level of quarterly lost lending. Finally, we add this forgone lending to the actual outstanding level of credit in each quarter during the crisis, adjust for inflation using the consumer price index, come up with a counterfactual (i.e., the level of actual plus cumulatively-forgone credit), and plot the result in Figure 6. By construction, early in 2008 the forgone credit tends to be small, but it grows as we approach the latter stages of 2009. Throughout the two-year period, the cumulative loss in lending adds up to about 38.2 billion CAD (in 2002 CADs), or about 13.6% of total outstanding non-mortgage credit at the end of 2009.

A different approach to assess the macroeconomic impact of our results is to compare the ability of a median Canadian household to withstand a credit supply shock with that of a median U.S. household. In the above analysis, we show that in the pre-crisis period, the median exposed Canadian household reports 4,300 CAD of liquid assets. Using a similar approach and utilizing the 2007 Survey of Consumer Finances, we calculate the liquid asset holdings of a median U.S. household at 3,415 US dollars (USD). This suggests that when faced with a similar transitory shock (i.e., if the median

[24] We observe similar patterns if we use matched households' *median* holdings of non-mortgage debt and liquid assets during the crisis. The implied difference is 1,739 CAD for non-mortgage liabilities (37% of 4,700 CAD) and 2,380 CAD for liquid assets (34% of 7,000 CAD). Again, the implied differences in non-mortgage liabilities and liquid assets are quite comparable and, since the median liquid asset holdings among exposed households in the pre-crisis period are 4,300 CAD, a zero consumption effect is not very surprising.

credit supply drops by 1,739 USD), U.S. households will exhaust their liquid asset 20% sooner.

4 Robustness

In this section we briefly discuss the robustness checks, most of which are described in further detail in an online appendix. First, we test the reliability of the PSM estimators using the bounding approach proposed by Rosenbaum (2002). The approach determines how strongly an unmeasured variable must influence the selection process in order to undermine the implications of the matching analysis. Based on this sensitivity analysis we conclude that selection on unobservables is unlikely to drive our results. Furthermore, we also calculated the "average treatment effect" (ATE) instead of the "average treatment effect on the treated" (ATT) and found very similar results to those in Table 5.

Second, we confirm the lack of a consumption effect using a DID estimator for imputed consumption. Recall that we do not have consumption data from the pre-crisis period (2005-2006) since the CFM survey starts collecting this information in 2008. However, following Browning and Leth-Petersen (2003), we use income and changes in wealth to impute consumption for both the pre-crisis and crisis periods. Data limitations require us to exclude capital gains component of wealth and to estimate disposable income using federal and provincial tax rates. Given our measure of imputed consumption, we do not observe a significant treatment effect for the change in imputed consumption and the results confirm the evidence on consumption smoothing.

Third, we examine the sensitivity of the results to the measure of "exposure", using an alternative exposure measure that captures banks' reliance on wholesale funding. Ivashina and Scharfstein (2010) document that during the crisis banks heavily relying on wholesale funding reduced their lending more than banks that relied on retail deposits. We replicate the analysis by first categorizing Canadian banks according to their wholesale funding use. Once we confirm the balance of covariates between the two groups, we show that the results based on this alternative measure are similar to the ones discussed above: exposed households report lower levels of non-mortgage liabilities, but are able to smooth consumption due to the availability of liquid assets.

Fourth, we investigate whether the fact that we do not observe any treatment effects on consumption is primarily due to noisy measurement of household-level consumption expenditure. We address this possibility by "aggregating" the CFM expenditure data using survey weights and comparing the outcome to aggregate household expenditure figures from Canadian national accounts for

2008 to 2012 (the last available year). In a manner similar to Barrett, Levell and Milligan (2012), we calculate a "coverage rate", which is defined as the ratio of the "aggregated" CFM data to the aggregate data from national accounts, after limiting both data sources to common spending items. We find that CFM does not perfectly match the aggregate data, but it has a stable coverage ratio that is in the 0.62-0.65 range. Since this coverage rate is roughly comparable to many surveys that are custom-designed to measure expenditure (Barrett, Levell and Milligan (2012)), we conclude that excessive noise in our survey data does not seem to be a major concern.

Finally, we address two 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 in Canada. We measure the household's bank before the crisis and implicitly assume that the household stays with this bank throughout the sample period. However, households can switch banks and, if credit is unavailable at the incumbent bank, obtain the desired levels of credit from a competitor that may be unexposed. If households switched banks during the crisis, this could result in a downward bias in the ATTs. We assess the magnitude of this problem using two approaches: one that is based on actual switching behavior (i.e., *ex post*) and another that is based on a tendency to switch (i.e., *ex ante*).

In the "*ex post*" approach, we calculate the probability of households classified as exposed in the pre-crisis period switching to an unexposed institution in the crisis period. We find that only 4% did so. This low number is consistent with the previous evidence in Allen, Clark and Houde (2012). Our results are unaffected when we drop the 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.[25] 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 very likely nor very unlikely to switch banks.

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 this exogenous variation is correlated with

[25] 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.

a higher exposure of banks in this region to the United States, this could result in the confounding of the supply effect we attempt to estimate with demand factors.[26] We perform a number of calculations to address this concern. First, we estimate ATT effects on the unemployment of households. If matched households lived in regions that had better economic performance, we should be able to detect differences in unemployment rates among exposed and unexposed households during the crisis. We do not find such an effect. Second, we check for the proportion of exposed households that were matched to at least one household in the same region.[27] This proportion is 66%, suggesting that, in general, we do a decent job of matching households within regions. Finally, suppose we were unable to pick up all heterogeneity in this regard. This would lead us to overstate differences in non-mortgage liabilities and consumption. However, we do not find a difference in consumption between exposed and matched households. Instead, we find significant effects on liquid assets. If an important part of our results were driven by a demand effect, we should find significant differences in consumption expenditure and insignificant differences in crisis levels of liquid assets. This, however, is not what we find.

In summary, our results are neither highly susceptible to a hidden bias nor are they affected by the lack of consumption data from the pre-crisis period or any noise in our crisis period consumption data. The findings are also robust to an alternative exposure-classification measure, and are neither driven by households' switching behavior/attitude, nor are they affected by households' regional classification.

5 Conclusion

In this paper we seek to empirically establish a link between bank financial distress, credit supply to households and household consumption expenditures. If financial distress in banking adversely affects household consumption due to, for instance, exacerbating household credit constraints, this may have first-order macroeconomic consequences. We find evidence in favour of a lending supply effect: distressed banks reduce lending, especially to financially weaker households. There is no corresponding effect, however, on consumption expenditure. Households smooth their consumption by drawing down their liquid assets. Our results are consistent with the interpretation that households perceive an adverse lending supply shock as temporary.

[26] 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.

[27] These regions are Atlantic Canada (consisting of four provinces), Quebec, Ontario, the Prairie provinces (consisting of two provinces), Alberta and British Columbia.

The results have important policy implications. For example, they suggest that households will not reduce their consumption based on temporary shocks, as long as they can draw on liquid assets. This stands in stark contrast to recent results for firms (Campello, Graham and Harvey (2010); Puri, Rocholl and Steffen (2011)), where the same shock affected firm investment and employment decisions. At the same time, households, by drawing down liquid assets, may have exacerbated the funding problems of banks. Further, the significant decline in aggregate consumption expenditures in Canada during the financial crisis was largely unrelated to credit supply, but rather consumption demand. 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. The results reported in this paper suggest that there was a "pure" contagion effect at work: 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 (the "CNN effect").

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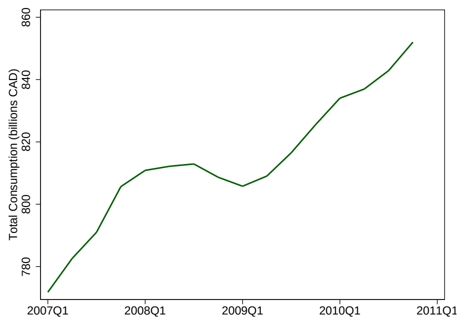
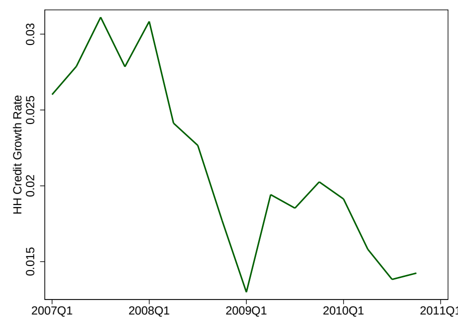
Figure 1: Total consumption expenditures, durable consumption expenditures and the growth rate of household credit in Canada (2007Q1-2010Q4)



(a) Total Consumption Expenditure



(b) Growth Rate Household Credit

(c) Durable Consumption Expenditure

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)

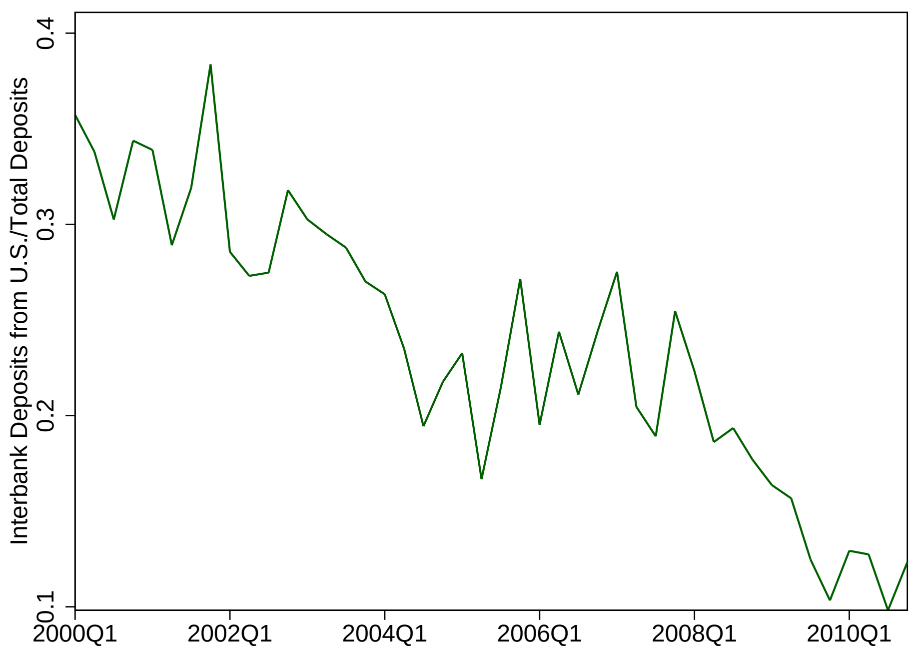


Figure 3: Annual growth rate of consumer lending (excluding mortgages and personal lines of credit) for *exposed* vs. *unexposed* Canadian banks (excluding credit unions)

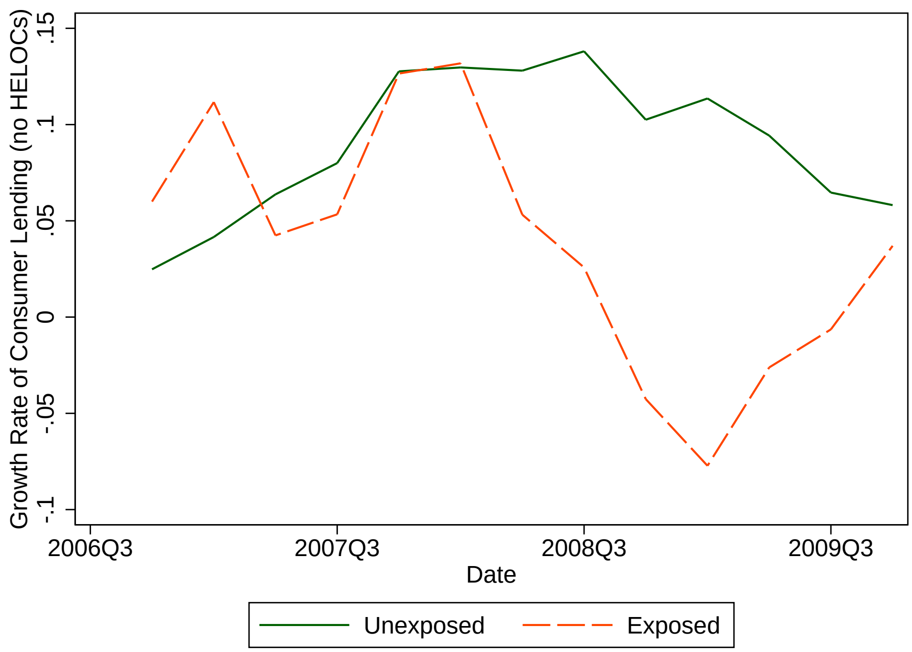


Figure 4: Kernel densities of propensity scores for exposed and unexposed households

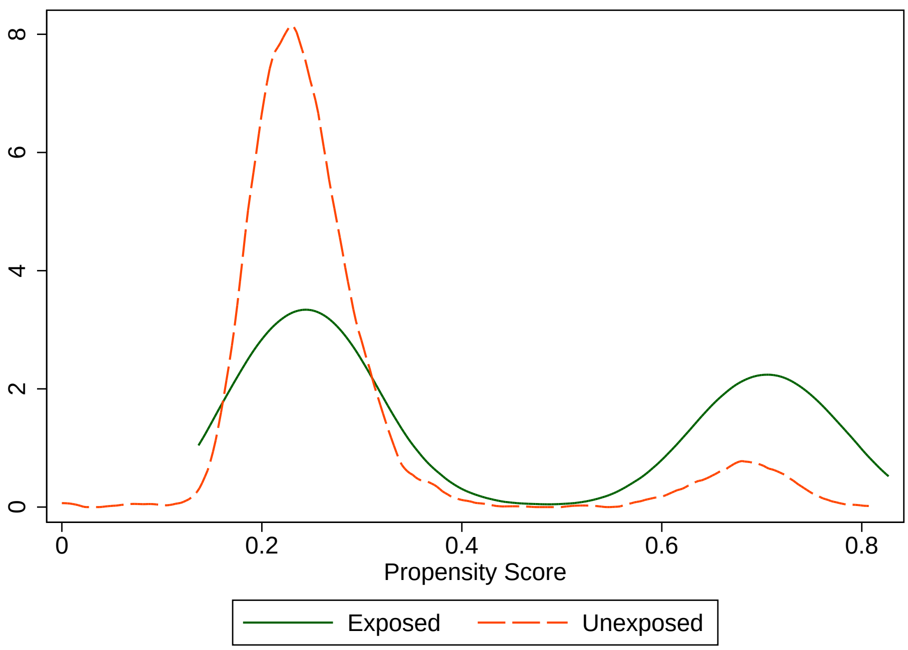


Figure 5: Kernel density of the treatment effect on total consumption

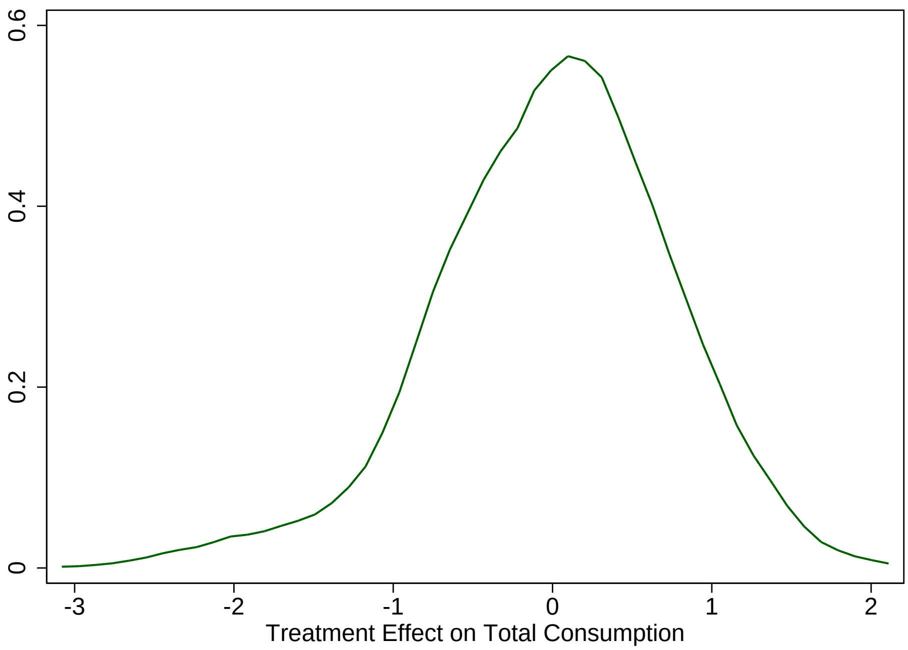


Figure 6: Expected vs. actual CPI adjusted levels (in billions of 2002 Canadian dollars) of non-mortgage liabilities

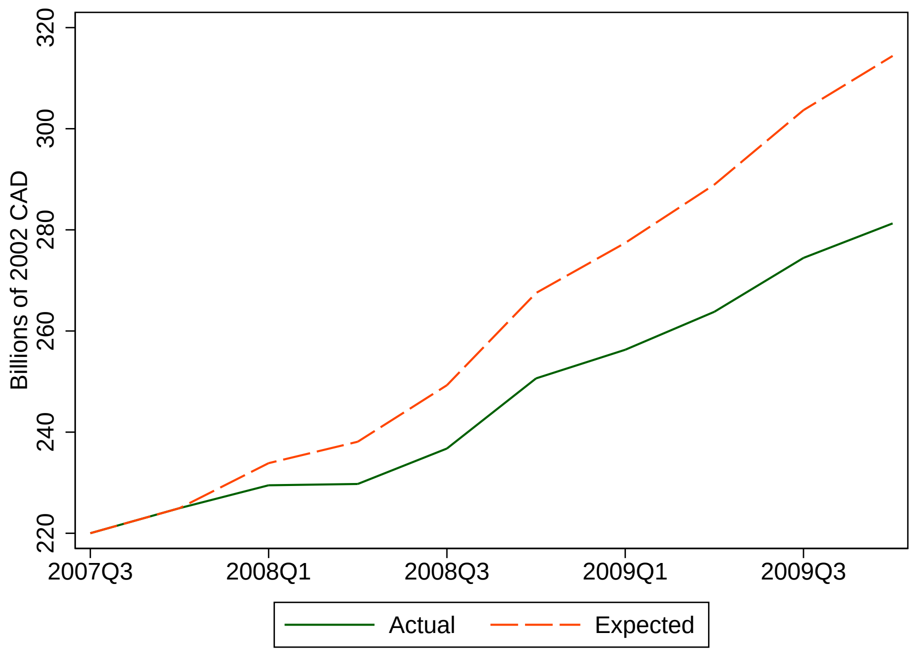


Table 1: Expenditure Questions in the *Canadian Financial Monitor*

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

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

	Exposed Households			Unexposed Households		
Variable	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Crisis Period (2008-2009)						
Total Consumption	33746	29472	21358	35010	31973	20285
Durables Spending	5313	2248	6995	5062	1971	7034
Luxury Spending	3954	2341	4554	4573	2941	4742
Non-Mortgage Liabilities	15380	3250	26244	18153	4500	29765
Liquid Asset Holdings	14006	5300	23676	19716	7750	24067
Pre-Crisis Period (2005-2006)						
Non-Mortgage Liabilities	15985	4550	27108	18885	6250	31697
Liquid Asset Holdings	11958	4300	21307	16442	6250	27803

Table 3: 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*	0.05
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 ranked above the sample's median based on the measure of exposure. 32.75% of the households are classified as exposed. Number of households = 3,804. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

Table 4: 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 5: Baseline estimation of the average effect of the crisis on exposed households

	N	Mean Diff.	p-value
ln(Liquid Assets)			
Pre-crisis (2005-06)	1246	-0.091	0.43
Crisis (2008-09)	1246	**-0.428**	**0.00**
Difference-in-differences	1246	**-0.338**	**0.01**
ln(Non-Mortgage Liabilities)			
Pre-crisis (2005-06)	1246	0.001	0.99
Crisis (2008-09)	1246	**-0.462**	**0.03**
Difference-in-differences	1246	**-0.463**	**0.03**
ln(Consumption During the Crisis (2008-09))			
Durables	1246	0.1425	0.29
Luxuries	1245	**-0.164**	**0.09**
Total consumption	1246	-0.009	0.77

Note: Mean difference between exposed and unexposed households. Standard errors are calculated based on the procedure described in Abadie and Imbens (2006, Theorem 7). 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 6: Estimation of the average effect of the crisis on exposed households controlling for credit constraints

	Constrained			Unconstrained		
	N	Mean diff	p-value	N	Mean diff	p-value
ln(Liquid Assets)						
Pre-crisis (2005-06)	794	-0.009	0.949	164	-0.313	0.124
Crisis (2008-09)	794	**-0.305**	**0.056**	164	**-0.487**	**0.04**
Difference-in-differences	794	**-0.296**	**0.08**	164	-0.173	0.492
ln(Non-Mortgage Liabilities)						
Pre-crisis (2005-06)	794	0.175	0.542	164	0.119	0.788
Crisis (2008-09)	794	**-0.734**	**0.01**	164	0.678	0.148
Difference-in-differences	794	**-0.909**	**0.001**	164	0.559	0.262
ln(Consumption During the Crisis (2008-09))						
Durables	794	0.066	0.695	164	-0.084	0.763
Luxuries	794	-0.066	0.586	163	-0.049	0.775
Total consumption	794	0.02	0.619	164	-0.014	0.824

Note: Mean difference between exposed and unexposed households. Constrained households are defined as having at least 20% home equity and no HELOC in the 2005-06 period (i.e., pre-crisis). Unconstrained households are defined as having at least 20% home equity and a HELOC in the pre-crisis period. N refers to the number of exposed households in each category. Standard errors are calculated based on the procedure described in Abadie and Imbens (2006, Theorem 7).

Table 7: Do large consumption effects imply low levels of liquid assets?

	Bottom Quartile of Total Consumption ATT			Top Three Quartiles of Total Consumption ATT			
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Mean Diff.
ATT on *ln(Total Consumption)* (Crisis)	312	-0.974	0.492	933	0.316	0.492	-1.291***
ATT on *ln(Durables)* (Crisis)	312	-1.662	3.272	933	0.747	2.923	-2.409***
ATT on *ln(Luxuries)* (Crisis)	312	-1.486	2.394	933	0.279	2.07	-1.765***
ATT on Change in *ln(Liquid Assets)*	312	-0.437	3.258	933	-0.294	3.122	-0.142
ATT on Change in *ln(Non-Mortgage Liab.)*	312	-0.514	5.413	933	-0.449	4.984	-0.064
ln(Liquid Assets) of Exposed HHs (Pre-crisis)	312	7.724	2.274	933	8.132	2.277	-0.409***
ln(Liquid Assets) of Exposed HHs (Crisis)	312	7.561	2.852	933	8.092	2.698	-0.531***
ln(Non-Mortgage Liab.) of Exposed HHs (Pre-crisis)	312	5.496	4.495	933	6.385	4.547	-0.889***
ln(Non-Mortgage Liab.) of Exposed HHs (Crisis)	312	5.172	4.638	933	5.969	4.679	-0.797***

Note: N refers to the number of exposed households in each category (bottom quartile vs. the top three quartiles). ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

A For Online Publication: Appendix for Robustness Checks

A.1 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 influencing the participation decision (propensity to be with an exposed bank, the treatment) and outcome variables (non-mortgage liabilities, consumption, liquid assets). In order to estimate the extent to which such "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 on 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.[28]

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

[28] For a more detailed technical discussion of Rosenbaum bounds, please see Barath et al. (2011) and Fung, Huynh and Sabetti (2012), who also provide applications of Rosenbaum bounds in a context similar to ours.

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 $1.15 = exp(\beta_k \cdot s_k \cdot n)$, 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.

A.2 Imputing Consumption Data

In our main empirical analysis, we are unable to calculate a true difference-in-differences term for spending (total, durables or luxury), due to the lack of consumption data in the CFM for the pre-crisis period. It is, however, possible to impute consumption using income and wealth data in a manner similar to Browning and Leth-Petersen (2003). Specifically, we use their "accounting imputation" method, which specifies consumption as

$$c_t = y_t - \Delta W_t + \sum_k (p_{kt} - p_{kt-1}) A_{kt-1}, \tag{A1}$$

where c_t is consumption, y_t is disposable income and ΔW_t is the change in wealth between $t-1$ and t. A_{kt-1} is the amount of asset k held by the household at time $t-1$ and the term in parentheses is the change in the price of asset k between $t-1$ and t (capturing capital gains).

Imputing consumption using equation (A1) requires the addition of another time period to our CFM panel sample. Accordingly, we further limit our sample to households that complete the CFM survey in 2003 or 2004, 2005 or 2006, and 2008 or 2009. This reduces our sample to 1,660 households (of which 532 are exposed).

We then make some adjustments to equation (A1) in order to make the imputation feasible. Unlike the data used by Browning and Leth-Petersen (2003), the CFM reports gross income. We use federal and provincial income tax rates to approximate disposable income for each household.

However, since we are unable to account for tax credits and capital gains taxes, this is likely to yield a noisy disposable income variable. Furthermore, given the unavailability of price data for the financial assets held by the CFM respondents (and similar to Browning and Leth-Petersen (2003)), we ignore the "capital gains" component of equation (A1).

Regarding household wealth (W_t), we consider two approaches:

$$
\begin{aligned}
\textit{Basic Wealth} &= \textit{Checking Account Balances} + \textit{Savings Account Balances} + \textit{GIC Balances}, \\
\textit{Complete Wealth} &= \textit{Checking Account Balances} + \textit{Savings Account Balances} + \textit{GIC Balances} \\
&+ \textit{House Value} + \textit{Auto Value} + \textit{Bond Holdings} + \textit{Stock Holdings} \\
&+ \textit{Mutual Fund Holdings}.
\end{aligned}
$$

Basic Wealth is included in our analysis given our concerns related to the fluctuations in the prices of stocks and bonds during our sample period (especially the crisis period). Using the two imputed consumption measures implied by these wealth measures ("basic" vs. "complete"), we estimate treatment effects on imputed consumption during the crisis, the pre-crisis period and the change in imputed consumption. Since imputed consumption exists for only a part of our sample, we perform a new matching procedure to ensure that all of the unexposed households that get matched to an exposed household have valid imputed consumption observations.[29]

The results of our baseline matching and our analysis using financially constrained households are reported in Tables A1 and A2. The lack of any significant treatment effects for pre-crisis or crisis consumption levels broadly confirms our conclusions of consumption smoothing by exposed households, despite lower credit supply during the crisis period.

A.3 Alternative Exposure Measure

Although our main empirical analysis used interbank deposits from the United States as a measure of exposure to the crisis, it is possible to construct another exposure measure based on the existing literature. As discussed by Ivashina and Scharfstein (2010), banks that were more dependent on wholesale funding prior to the crisis reduced their lending more during the crisis than did banks relying on retail deposits. Therefore, categorizing Canadian banks according to their wholesale

[29] Another approach would be to keep our original matches (based on our full sample) and calculate a treatment effect using only the matches for which imputed consumption data exist for the exposed household and at least one of the matched households. Following this approach does not change our findings.

funding can give us an alternative measure for *Exposure*. For this categorization, we define wholesale funding as follows:

$$WSF = \frac{\textit{Interbank Deposits} + \textit{Acceptances} + \textit{Repurchase Arrangements}}{\textit{Total Assets}}.$$

The banks (and credit unions) in our sample are then divided into "exposed" vs. "unexposed" categories in a manner similar to our main analysis above. We look for a "natural break" in *WSF*, which occurs in two places. The first break occurs around 1%, since some of the smaller banks and credit unions use little or no wholesale funding. However, given the distribution of the CFM respondents' main banks, categorizing all banks with $WSF \geq 1\%$ as "exposed" would result in almost all households in our sample being categorized as such. The second natural break occurs around 15%, with *WSF* ranging from 0% to approximately 13.5% for one group of banks and 17.5% to approximately 35% for another group of banks. We use this second natural break and categorize all banks with $WSF \geq 15\%$ as "exposed."[30] This categorization results in 1,658 exposed and 2,119 unexposed households.[31]

The balance of covariates between the exposed and matched sample (Table A3) indicates that there are no statistically significant differences between the exposed and matched households. Table A4 shows the results of our baseline analysis. The findings are quite similar to our main empirical analysis above, with one exception, namely that the average treatment effect for the change in non-mortgage liabilities is negative but insignificant. Finally, Table A5 displays the results of our analysis based on the distribution of the treatment effect on total consumption. The findings confirm our earlier conclusions, given that the exposed households with the most negative treatment effects on consumption also had lower levels of liquid assets prior to the crisis. Therefore, when faced with a credit shock (which again appears to be the same across all exposed households), these households were unable to maintain consumption.

[30] For data confidentiality reasons, we are unable to discuss the similarities and differences in the exposure categorization of the banks in our sample under our two different measures. This is due to the fact that, although most of the data used to calculate *WSF* are publicly available, the interbank deposits from the U.S. data used in our main empirical analysis are confidential. We can, however, report that the two categorizations are not identical, but there is considerable overlap between the two.

[31] The total number of households is lower for this specification, since we are unable to calculate *WSF* for some credit unions (unlike information on their interbank deposits from the United States, which was provided to us by their regulators).

A.4 Validating CFM Expenditure Data

One major 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; instead our consumption results (or the lack thereof) are a result of noisy expenditure data. While the lack of a significant ATT for *imputed* consumption data (in section A.2) 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).

As discussed in Bee, Meyer and Sullivan (2012) and Barrett, Levell and Milligan (2012), 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".[32]

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 1,000 CAD). These account for between 2.9% to 7.1% 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.[33] Then, we multiply each

[32] 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, just in case some of the items in this category are included in the "recreation (movies, concerts, fitness club, etc.)" item of CFM.

[33] Specifically, we regress the (log of) total annual consumption on (log of) income, (log of) total wealth, education level, marriage status, age group (below 25, between 25 and 34, between 35 and 44, etc.) and province dummies. Each of these imputation regressions has an R-squared between 0.228 and 0.295 (results available upon request). Running a single regression covering 2008-2012 and including individual year dummies yields very similar results.

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	0.65	0.62	0.59

The coverage rate for the CFM survey is quite comparable to the coverage rates of household expenditure surveys for the U.S., U.K. and Australia calculated in Barrett, Levell and Milligan (2012), which are all in the 0.60 to 0.75 range for our sample period. Interestingly, the only household expenditure survey in Barrett, Levell and Milligan (2012) 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 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.[34] 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 (2012) 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

[34]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 in order 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 refusing the complete the survey, via additional phone calls, letters and visits. Brzozowski and Crossley (2011) and Barrett, Levell and Milligan (2012) provide more details on the SHS methodology and its data quality.

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 (2012). For example, the head of household that completes the CFM survey may not be fully aware of the spending of 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.

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Table A1: Baseline estimation of the average effect of the crisis on the imputed consumption of exposed households

	N	Mean Diff.	p-value
ln(Basic Imputed Consumption)			
Pre-crisis (2005-06)	532	-0.243	0.443
Crisis (2008-09)	532	0.205	0.56
Difference-in-differences	532	0.447	0.305
ln(Complete Imputed Consumption)			
Pre-crisis (2005-06)	532	-0.767	0.314
Crisis (2008-09)	532	-0.722	0.309
Difference-in-differences	532	0.045	0.964

Note: Mean difference between exposed and unexposed households. Standard errors are calculated based on the procedure described in Abadie and Imbens (2006, Theorem 7). 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 A2: Estimation of the average effect of the crisis on the imputed consumption of exposed households controlling for credit constraints

		Constrained			Unconstrained	
	N	Mean diff	p-value	N	Mean diff	p-value
ln(Basic Imputed Consumption)						
Pre-crisis (2005-06)	373	-0.036	0.349	159	0.234	0.702
Crisis (2008-09)	373	0.465	0.934	159	0.398	0.384
Difference-in-differences	373	0.502	0.431	159	0.164	0.783
ln(Complete Imputed Consumption)						
Pre-crisis (2005-06)	373	-1.188	0.213	159	-0.952	0.872
Crisis (2008-09)	373	-0.856	0.358	159	-0.256	0.872
Difference-in-differences	373	0.332	0.799	159	0.696	0.701

Note: Mean difference between exposed and unexposed households. Constrained households are defined as having at least 20% home equity and no HELOC in the 2005-06 period (i.e., pre-crisis). Unconstrained households are defined as having at least 20% home equity and a HELOC in the pre-crisis period. N refers to the number of exposed households in each category. Standard errors are calculated based on the procedure described in Abadie and Imbens (2006, Theorem 7).

Table A3: Balance of household characteristics, using the wholesale funding-based *Exposure* measure *(two-sample t-test)*

	Exposed	Unexposed	p-value
Home equity / house value	0.663	0.657	0.623
ln(House value)	10.442	10.339	0.416
ln(Gross income)	10.986	10.992	0.788
Age	49.51	49.514	0.986
Single	0.324	0.340	0.260
Unemployed	0.030	0.028	0.787
House size	4.694	4.631	0.327
Labor supply, male	0.652	0.657	0.698
Labor supply, female	0.554	0.549	0.711
Self-employed, male	0.076	0.065	0.146
Self-employed, female	0.084	0.083	0.851
College some	0.167	0.164	0.762
College degree	0.653	0.664	0.384
1-2 children	0.246	0.238	0.461
3-4 children	0.014	0.010	0.246
5 children	0.002	0.001	0.327
Big city	0.390	0.399	0.502
Renter	0.166	0.175	0.376
French	0.470	0.468	0.659
Risk aversion	0.124	0.126	0.508
Observations	1658	1658	

Table A4: Baseline estimation of the average effect of the crisis on exposed households, using the wholesale funding-based *Exposure* measure

	N	Mean Diff.	p-value
ln(Liquid Assets)			
Pre-crisis (2005-06)	1658	-0.069	0.42
Crisis (2008-09)	1658	**-0.264**	**0.00**
Difference-in-differences	1658	**-0.195**	**0.05**
ln(Non-Mortgage Liabilities)			
Pre-crisis (2005-06)	1658	**0.348**	**0.04**
Crisis (2008-09)	1658	0.126	0.47
Difference-in-differences	1658	-0.221	0.21
ln(Consumption During the Crisis (2008-09))			
Durables	1658	0.089	0.41
Luxuries	1658	-0.047	0.55
Total consumption	1658	-0.011	0.66

Note: Mean difference between exposed and unexposed households. Standard errors are calculated based on the procedure described in Abadie and Imbens (2006, Theorem 7). 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 A5: Do large consumption effects imply low levels of liquid assets, when the wholesale funding-based *Exposure* measure is used?

	Bottom Quartile of Total Consumption ATT			Top Three Quartiles of Total Consumption ATT			
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	Mean Diff.
ATT on *ln(Total Consumption)* (Crisis)	415	-0.950	0.486	1242	0.305	0.466	-1.251***
ATT on *ln(Durables)* (Crisis)	415	-1.724	3.215	1242	0.695	2.942	-2.419***
ATT on *ln(Luxuries)* (Crisis)	415	-1.567	2.683	1242	0.461	1.947	-2.028***
ATT on Change in *ln(Liquid Assets)*	415	-0.325	3.085	1242	-0.145	2.969	-0.180
ATT on Change in *ln(Non-Mortgage Liab.)*	415	-0.481	5.017	1242	-0.133	5.234	-0.348
ln(Liquid Assets) of Exposed HHs (Pre-crisis)	415	7.906	2.080	1242	8.313	2.238	-0.407***
ln(Liquid Assets) of Exposed HHs (Crisis)	415	7.837	2.621	1242	8.466	2.349	-0.629***
ln(Non-Mortgage Liab.) of Exposed HHs (Pre-crisis)	415	6.359	4.311	1242	6.651	4.501	-0.292
ln(Non-Mortgage Liab.) of Exposed HHs (Crisis)	415	5.803	4.557	1242	6.286	4.704	-0.483*

Note: N refers to the number of exposed households in each category (bottom quartile vs. the top three quartiles). ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

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