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PhD Dissertation

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Credit Supply and The Real Economy

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Thais Lærkholm Jensen

Summary

An entrepreneur with a great new idea for starting a new business, or a young couple looking to buy a house will likely find themselves in a position where they will need to ask others for money to carry out their ambitions. In this situation, they turn to the financial markets to assist them in making these dreams come true, and in effect they are intrinsically dependent on the availability of credit. The recent financial crisis, with the recession that ensued, has accentuated the need for understanding how the financial markets and in particular the availability of loanable funds impacts the economy at large, including the entrepreneur and the couple buying a house. This dissertation is comprised of four self-contained chapters concerned with how leverage, banks and credit supply impacts and interacts with the real economy.

Chapter 1, *"Housing Collateral, Credit Constraints, and Entrepreneurship: Evidence from a Mortgage Reform"*, investigates how a Danish mortgage reform that exogenously increased access to credit impacted entrepreneurship. The reform setup allows us to separate the role of credit access from wealth effects that typically confounds the analysis of the collateral channel. We find evidence that increased credit access leads to more entrepreneurship but that the overall magnitudes are small. Moreover we show that new entrants were more likely to start businesses in sectors where they had no prior experience, and were also more likely to fail than those who did not benefit from the reform, suggesting that the marginal entrant is of lower quality.

Chapter 2, *"The Real Effects of Higher Capital Requirements to Banks: Evidence from Danish Firm-Level Data"*, using firm-level data, estimates that an increase in the minimum regulatory capital requirement of a firm's primary bank induces the firm to borrow 3 percent less. While I show that firms' borrowing are sensitive to capital requirements of their primary bank, I find, on average, no material effect on firms' assets growth as firms are able to substitute towards equity financing instead of reducing their balance sheets. Investigating the heterogeneous effects, however, I find that young firms with negative earnings are particularly sensitive to capital requirements of their primary bank and are led to reduce assets growth.

Chapter 3, *"Household Debt and Consumption During the Financial Crisis: Evidence from Danish Micro Data"*, examines whether high leverage of household prior to the financial crisis amplified the reduction in household spending over the course of the crisis. We find a strong negative correlation between pre-crisis leverage and the change in spending during the crisis. This reflects that highly-levered households spent a larger share of their income than their less-levered peers prior to the crisis, resulting in larger increases in debt in these years. Once we condition on the size of the pre-crisis change in debt, a high level of debt is no longer associated with a larger spending decline. Our results also suggest that the larger decline in spending among high-leverage households is the result of a spending normalization pattern that is also found in other

years, rather than a causal effect of high debt levels suppressing household spending during the crisis.

Chapter 4, *"The Consumption Effects of the 2007-2008 Banking Crisis: Evidence from Household-Level Data"*, is concerned with to extent to which the drop in household consumption following the 2007-2009 financial crisis can be attributed to a contraction of the credit supply by funding constrained banks. We study this question with a dataset that contains observations on all accounts in Danish banks as well as comprehensive information about account holders and banks. We show that banks exposed to the financial crisis reduced their credit supply significantly and that their customers reduced both borrowing and consumption relative to customers in non-exposed banks. Our results further suggest that heterogeneous costs of switching banks at the level of customers may explain why they did not fully compensate with credit from other sources when their banks tightened credit. Finally, we quantify the contribution of the credit supply channel to the spectacular drop in aggregate private consumption observed in Denmark between 2007 and 2009. Around one third of the consumption loss can plausibly be attributed directly to tightened bank credit.

Resumé (in Danish)

En spirende iværksætter med en ny idé til at starte virksomhed, eller et ungt par der ønsker at købe et hus, vil sandsynligvis befinde sig i en position, hvor de har brug for at bede andre om penge til at føre deres planer ud i livet. I denne situation vil det være nødvendigt for dem at benytte sig af de finansielle lånemarkeder for at få den nødvendige kapital til at gøre deres drømme til virkelighed, og i den sammenhæng er de afhængige af tilgængeligheden af kredit. Den seneste finanskrise, med recessionen der fulgte, har understreget behovet for at forstå, hvordan de finansielle markeder og især adgangen til kredit påvirker økonomien generelt, herunder iværksætteren og parret i færd med at købe et hus. Denne afhandling består af fire selvstændige kapitler omhandlende hvordan kreditadgang, banker og gældsætning påvirker og interagerer med realøkonomien.

Kapitel 1, *"Housing Collateral, Credit Constraints, and Entrepreneurship: Evidence from a Mortgage Reform"*, undersøger, hvordan en dansk realkreditreform, som eksogent øgede adgangen til kredit, påvirkede iværksætteri. Udformningen af reformen giver os mulighed for at adskille effekten af kreditadgang fra formueeffekter, der typisk besværliggør muligheden for separat at analysere effekten af øget adgang til kredit. Vi finder evidens for, at øget kreditadgang fører til mere iværksætteri, men at de overordnede effekter er beskedne. Desuden viser vi, at nye iværksættere var mere tilbøjelige til at starte virksomheder i sektorer, hvor de ikke havde nogen forudgående erfaring, og at de også var mere tilbøjelige til at mislykkes, relativt til dem der ikke startede på grund af reformen. Dette tyder på, at den marginale iværksætter er af lavere kvalitet.

Kapitel 2, *"The Real Effects of Higher Capital Requirements to Banks: Evidence from Danish Firm-Level Data"*, analyserer, på baggrund af virksomhedsdata, hvordan en stigning i det lovpligtige minimumskapitalkrav for en virksomheds primære bank reducerer virksomhedens låntagning med 3 procent. Mens jeg viser, at en virksomheds låntagning er følsomt overfor kapitalkravet stillet til deres primære bank, finder jeg i gennemsnit ingen væsentlig effekt på virksomhedens aktivvækst idet virksomhederne er i stand til at erstatte reduktionen i låneudbuddet med egenkapitalfinansiering. Imidlertid finder jeg, at unge virksomheder med negativ indtjening er særligt følsomme over for kapitalkrav til deres primære bank, og at disse virksomheder ultimativ finder deres vækstmuligheder begrænset heraf.

Kapitel 3, *"Household Debt and Consumption During the Financial Crisis: Evidence from Danish Micro Data"*, undersøger, om høj gældsætning blandt husholdningerne før finanskrisen forstærkede reduktionen i husholdningernes forbrug i løbet af krisen. Vi finder en stærk negativ korrelation mellem niveauet af gældsætning før krisen og det efterfølgende fald i forbruget under krisen. Det afspejler, at højt gældsatte husstande forbrugte en større andel af deres indkomst end deres mindre gældsatte modparter før

krisen. Når vi betinger på størrelsen af ændringen i gæld før krisen, er et højt gældsniveau imidlertid ikke længere forbundet med en større forbrugsreduktion. Da mønsteret om en kraftig reduktion i forbruget blandt højt gældsatte hustande genfindes i andre år, der ikke relaterer sig til finanskrisen, tyder det på, at det større fald i forbruget blandt højt gearede husstande er resultatet af en forbrugsnormalisering fremfor en kausal effekt af at høje gældsniveauer, begrænsede husholdningernes forbrug under krisen.

Kapitel 4, *"The Consumption Effects of the 2007-2008 Banking Crisis: Evidence from Household-Level Data"*, beskæftiger sig med, i hvilket omfang faldet i husholdningernes forbrug efter finanskrisen 2007-2009 kan tilskrives en reduktion i kreditudbuddet hos banker påvirket af den finansielle krise. Vi studerer dette spørgsmål med et datasæt, der indeholder information om alle konti i danske banker sammenkoblet med omfattende information om kontohavere og banker. Vi viser, at banker, der var særligt udsatte i forhold til den finansielle krise, reducerede deres kreditudbud betydeligt, og at dette resulterede i at deres kunder reducerede både låntagning og forbrug i forhold til kunder i ikke-eksponerede banker. Vores resultater tyder envidere på at heterogene omkostninger forbundet ved at skifte banke kan forklare, hvorfor kunderne ikke fuldt ud søgte kredit fra andre kilder, da deres banker reducerede kreditudbuddet. Endelig kvantificerer vi effekten af bankernes reducerede kreditudbud under krisen: Omkring en tredjedel af faldet i forbruget fra 2007-2009 kan skønnes at kunne henføres direkte til reduktionen i bankers låneudbud til private husholdninger.

Contents

Chapter 1

Housing Collateral, Credit Constraints and Entrepreneurship

This chapter, with only minor differences, is published as a working paper with the title "Housing Collateral, Credit Constraints and Entrepreneurship - Evidence from a Mortgage Reform", *NBER Working Paper No. 20483*, October 2014

Housing Collateral and Entrepreneurship: Evidence from a Mortgage Reform*

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Abstract

We study how a mortgage reform that exogenously increased access to credit had an impact on entrepreneurship, using individual-level micro data from Denmark. The reform allows us to disentangle the role of credit access from wealth effects that typically confound analyses of the collateral channel. We find that a $30,000 increase in credit availability led to a 12 basis point increase in entrepreneurship, equivalent to a 4% increase in the number of entrepreneurs. New entrants were more likely to start businesses in sectors where they had no prior experience, and were more likely to fail than those who did not benefit from the reform. Our results provide evidence that credit constraints do affect entrepreneurship, but that the overall magnitudes are small. Moreover, the marginal individuals selecting into entrepreneurship when constraints are relaxed may well be starting businesses that are of lower quality than the average existing businesses, leading to an increase in churning entry that does not translate into a sustained increase in the overall level of entrepreneurship.

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1 Introduction

One of the most robust findings in the entrepreneurship literature is the strong positive correlation between personal wealth and the propensity to engage in entrepreneurship (Evans & Jovanovic, 1989; Holtz-Eakin et al., 1994; Gentry & Hubbard, 2004). For example, Gentry & Hubbard (2004) find that entrepreneurs comprise just under 9% of households in the US, but hold about 40% of total net worth. Several other studies have documented that entrepreneurs are not just wealthier, but wealthier individuals are also more likely to become entrepreneurs (Hurst & Lusardi, 2004).

The most common explanation for this correlation is that credit constraints pose an important barrier to entry for less wealthy individuals (Stiglitz & Weiss, 1981; Evans & Jovanovic, 1989). Evans & Jovanovic (1989) argue that these constraints are likely to be binding for the most productive entrepreneurs, suggesting that the returns to relaxing constraints are large. However, others have questioned the degree to which financing constraints are salient for entrepreneurship, particularly in advanced economies where firms have adequate access to capital. This work has argued that a correlation between wealth and entry might exist due to unobserved differences in productivity, or preferences for entrepreneurship, that are correlated with wealth rather than due to the presence of credit constraints (Hamilton, 2000; Moskowitz & Vissing-Jørgensen, 2002; Hurst & Lusardi, 2004; Hurst & Pugsley, 2011).

A central approach to addressing this debate has been to examine how unexpected changes in personal wealth impact entrepreneurship. For example, Blanchflower & Oswald (1998), Holtz-Eakin et al. (1994), and Andersen & Nielsen (2012) have all examined the impact of bequests on entrepreneurship. More recently, several papers have examined how increases in the value of home equity due to house price appreciation might impact entrepreneurship via the collateral channel (e.g., Black et al. (1996); Schmalz et al. (2013); Harding & Rosenthal (2013); Fairlie & Krashinsky (2012); Adelino et al. (2013)). Although this work finds that an increase in personal wealth leads to entrepreneurship, it is unable to fully isolate the effect of credit constraints on entrepreneurship, because large increases in wealth, while alleviating credit constraints can also lead to wealth effects. For example, increases in wealth can change an individual's risk aversion (Evans & Jovanovic, 1989; Kihlstrom & Laffont, 1979) or preferences (Hurst & Lusardi, 2004) and hence change their propensity to engage in entrepreneurship independent of credit constraints. Distinguishing between these two underlying factors is important, because a wealth shock may drive entrepreneurship even if credit constraints are not important. Isolating the impact of credit constraints therefore requires an exogenous change in the financing environment for entrepreneurs that also does not impact their wealth.

In this paper, we exploit a unique mortgage reform in Denmark coupled with extremely rich micro data to overcome this inferential challenge. Prior to this reform, individuals could only use mortgage loans to finance the purchase of their home, and were precluded from using home equity as collateral for personal loans needed to finance consumption or investment. The reform, that was passed in 1992, allowed home owners, for the first time, to borrow against the home for purposes other than financing the home itself – thereby unlocking access to credit without changing their wealth. In addition, since the amount of housing collateral that was unlocked was a function of the mortgage they had outstanding at the time of the reform, the degree to which individuals were able to borrow against their home for other purposes was driven in large part by the timing of their house purchase relative to the reform.[1] Households therefore entered the reform with different equity-to-value ratios and were thus effectively treated differentially by the reform. Individuals therefore entered the post-reform period with a differential increase in credit access based on their outstanding mortgage in 1991. By exploiting this cross-sectional variation in the intensity of the reform's treatment across otherwise equivalent individuals, we are able to isolate the impact of an exogenous increase in access to credit through the collateral channel and examine how this impacted both entry rates and the survival of existing businesses. The micro data allow us control for important covariates, as well as to distinguish between net and gross flows of entrepreneurs, which turns out to be important in our context. We use this to study whether the overall number of entrepreneurs was affected by the credit availability as well as whether the reform generated an inflow of new entrepreneurs that had different performance characteristics relative to those entering before the reform.

We find that the reform unlocked a substantial amount of home equity that could be used as collateral for personal loans - about $30,000 on average and equivalent to more than a year's disposable income for the median treated individual in our sample. Furthermore, we find that many individuals with access to more home equity did in fact increase their personal debt substantially. On average, a $1 in-

[1] Although individuals could refinance their mortgage to lock in lower interest rates prior to the reform, refinanced loans had to be of the same maturity as the original loan and the principal could not be expanded. People could also prepay their loan, but it was not possible to then extract the equity through another mortgage loan. The only way to unlock equity from a home prior to the reform was to sell one's house, but in unreported analyses we find no evidence that those becoming entrepreneurs prior to the reform were more likely to have moved houses more frequently than non-entrepreneurs.

crease in collateral was associated with a \$0.19 increase in personal debt. Yet, we find that the relative increase in the number of active entrepreneurs was 12 basis points, equivalent to about a 4% increase in the number of active entrepreneurs before the reform. When looking at the characteristics of the businesses, we find that existing businesses that were more likely to survive were marginally weaker, and new entrants had much greater failure rates than the control group. Even those entrants that survived had lower sales, profits and employment relative to the control group, suggesting that businesses started by the marginal entrant who benefited from the reform were of lower quality than those started by equivalent individuals who did not get increased access to housing collateral. Since the reform allowed individuals to access external finance without mortgage banks having to screen the specific projects of potential entrepreneurs, our latter result suggests that individuals may have been starting lower quality projects because they had a preference for entrepreneurship (Hurst & Pugsley, 2011) but didn't face the discipline of external finance.

Our findings are relevant to the extensive literature looking at financing constraints and entrepreneurship. A number of models suggest that individuals are either precluded from entry or that firms enter small and then grow because of the fact that they face financing constraints (Cooley & Quadrini, 2001; Evans & Jovanovic, 1989; Gentry & Hubbard, 2004; Cabral & Mata, 2003; Holtz-Eakin et al., 1994; Cagetti & De Nardi, 2006; Rajan & Zingales, 1998; Buera et al., 2011). Others looking at entry into entrepreneurship have found less support for this view (e.g., Hamilton (2000); Hurst & Lusardi (2004)). We present analyses based on a research design that is able to cleanly identify the effect of credit without varying wealth at the same time, as well as separate out entry from survival. Our results provide evidence that credit constraints do affect entrepreneurship, but that the overall magnitudes are small. In part, this is due to the fact that new entrants that benefit from a reduced constraint may well be starting businesses that are of lower quality than the average existing businesses, leading to churning entry that does not contribute to a equivalent boost in the stock of entrepreneurs.

The rest of the paper is structure as follows: in Section 2, we discuss the literature examining credit constraints in entrepreneurship and elaborate on the mortgage reform we study. In Section 3, we outline the data used in the analyses. Section 4 discusses our results and the robustness tests we perform. Section 5 concludes.

2 Theoretical Considerations

Since new businesses typically require some amount of capital investment before they can generate returns, the expected value of a new venture is an increasing function of the capital invested in the startup, up to an optimal level. If individuals face credit constraints, then the amount they invest in the business will be less than the optimal level of capital, lowering expected income from entrepreneurship, and hence lowering the probability that the individual will become an entrepreneur.

Debt finance is the principal form of external finance for most businesses (Berger & Udell, 1998; Robb & Robinson, 2012) and banks will often use the personal wealth of the owner to assess creditworthiness of new ventures as they have no track record of the firm's performance on which to lend to the business, even if these are young incorporated firms Berkowitz & White (2004). One common approach to testing credit constraints is therefore to regress an indicator of entry into entrepreneurship on a measure of the individual's personal wealth and a range of controls. If individuals do not face financing constraints, then the amount of capital that they invest in an equivalent business should not be systematically associated with their personal wealth and hence differences in wealth should not be relevant in predicting entry into entrepreneurship. If the coefficient on individuals' personal wealth is positive, however, it suggests that individuals may be credit constrained (Evans & Jovanovic, 1989; Gentry & Hubbard, 2004).

However, a positive association between personal wealth and entrepreneurship does not necessarily imply the presence of financing constraints. It is possible that an individual's personal wealth is endogenous. For example, if individuals with low ability are less likely to generate savings and also less likely to become entrepreneurs, the observed correlation between personal wealth and entrepreneurship may reflect this unobserved attribute rather than the causal effect of financing constraints. Further, suppose that wealthier individuals are more productive as entrepreneurs than as wage employees, say because they have access to better entrepreneurial opportunities or networks, they may be more likely to systematically sort into entrepreneurship than those who are less wealthy. In order to control for such a spurious correlation, researchers have sought to find exogenous shocks to personal wealth and study their effect on selection into entrepreneurship. For example, Lindh & Ohlsson (1996) have shown that those who win lotteries are more likely to be entrepreneurs than those who do not. Andersen & Nielsen (2012), Holtz-Eakin et al. (1994) and Blanchflower & Oswald (1998) have used inheritances as a source of an unexpected shock to wealth that reduces potential financing constraints.

While these studies have shown the causal impact of a wealth increase on entrepreneurship, their data and empirical set up is such they are not able to isolate the mechanism behind the increase in entrepreneurship. For example, wealthy people may have lower absolute risk aversion, making them more likely to become entrepreneurs (Evans & Jovanovic, 1989; Kihlstrom & Laffont, 1979), or preferences, such as the desire to be one's own boss, might rise with wealth (Hurst & Lusardi, 2004). If these mechanisms are important, they would lead to a positive association between wealth and entrepreneurship even if the wealth increase was exogenous and they were not affected by credit constraints. The concern above also applies to studies that have shown that increases in house prices that are unrelated to economic activity have a causal impact on entry into entrepreneurship. While these papers are focused on showing that house price increases cause entrepreneurship, they are still unable to fully isolate the effect of credit constraints, because while house price increases can improve access to collateral, they also raise an individual's wealth. This concern might be particularly salient given that these papers are based on time periods with extremely large house price increases.

In this paper, we use a unique reform combined with micro data on individuals to overcome the inferential challenge outlined above. Four features of the setting are attractive from the perspective of our study: first, we exploit a mortgage reform that unlocked the ability to access credit backed by housing collateral but did not directly impact the level of individuals' wealth. We are thus able to isolate the impact of credit constraints from wealth effects that typically confound such studies. Second, the amount of housing collateral that was unlocked at the time of the reform was driven the timing of the house purchase relative to the reform. As we outline in greater detail below, the notion of using the house as collateral for the business when borrowing from a mortgage bank did not exist in Denmark, so the timing of the house purchase was not driven by an anticipation of unlocking collateral to finance the business. This allows us to exploit cross-sectional variation in the intensity of the reform's treatment, in order to generate stronger identification for our study than a simple pre-post analysis. Third, detailed micro data collected by the Danish government and made available to us allows us to directly observe the timing of home ownership, housing equity, entry decisions and a whole range of individual-level correlates. This allows us to directly trace out the effects of the reform instead of relying on aggregate data that may be confounded by omitted variables. Since we have individual-level panel data, we can also include individual fixed effects to account for any systematic unobserved individual factors that might confound our analysis. Finally, unlike many reforms where one has an exogenous change but where it is hard to estimate the size of the treatment, we have a relatively precise estimate of the size of the treatment in terms of the amount of housing equity that was unlocked for each individual. This allows us to estimate the magnitude of the response and hence also shed light on the degree to which individuals respond to a large exogenous increase in access to credit.

Given the importance of the institutional setting for our identification, we first outline the key aspects of the mortgage market and the 1992 reform, before moving to a description of the data.

2.1 The Danish mortgage market and the mortgage reform of 1992

Until 2007, mortgage debt in Denmark was provided exclusively through mortgage banks, which are financial intermediaries specialized in the provision of mortgage loans. When granting a mortgage loan for a home in Denmark, the mortgage bank issues bonds that directly match the repayment profile and maturity of the loan granted. The bonds are sold on the stock exchange to investors and the proceeds from the sale are paid out to the borrower. A basic principle underlying the design of the Danish mortgage market is the balance-principle whereby total repayments from the borrowers and total payments from mortgage banks to bond holders must be in balance. This principle ensures that the mortgage banks face no funding risk and it also prevents them from charging any risk premium. Once the bank has screened potential borrowers based on the valuation of their property and on their ability to service the loan, (i.e. on household income), all borrowers who are granted a loan at a given point in time face the same interest rate.

Mortgage bonds are perceived as low risk by investors because of the detailed regulation of the mortgage market. First, mortgage banks are subject to solvency ratio requirements monitored by the Financial Supervision Authority, and there is a legally defined threshold of limiting lending to 80% of the house value at loan origination. In addition, each plot of land in Denmark has a unique identification number, the title number, to which all relevant information about owners and collateralized debt is recorded in a public title number registration system. Mortgage loans have priority over any other loan and the system therefore secures optimum coverage for the mortgage bank in case of default and enforced sale. Creditors can enforce their rights and demand a sale if debtors cannot pay. The combination of the regulation around mortgage lending and protection afforded by the title registration system implies that the loans offered by mortgage banks are very safe for lenders and typically much cheaper than collateralized loans obtained through commercial banks.

The Danish credit market reform studied in this paper took effect on 21 May 1992. The reform was part of a general trend of liberalization of the financial sector in Denmark and in Europe, although the exact timing appears to be motivated by its potential stimulating impact to the economy during the recession of 1992. The reform was implemented with short notice and passed through parliament in three months. The short period of time from enactment to implementation is useful for our identification strategy as it suggests that it is unlikely that the timing of individuals' house purchases was systematically linked to a forecast of unlocking housing collateral for the business. The reform changed the rules governing mortgage loans in two critical ways that are relevant to our study. The most important here is that it introduced the possibility of using the proceeds from a mortgage loan for purposes other than financing real property, i.e. the reform introduced the possibility to use housing equity as collateral for loans established through mortgage banks where the proceeds could be used for, among other things, starting or growing a business. The May 1992 bill introduced a limit of 60% of the house value for loans for non-housing purposes. This limit was extended to 80% in December 1992. A second feature of the reform increased the maximum maturity of mortgage loans from 20 to 30 years. For people who were already mortgaged to the limit prior to the reform, and who therefore could not establish additional mortgage loans for non-housing consumption or investment, this option potentially provided the possibility of acquiring more liquidity by spreading out the payments over a longer period and hence reducing the monthly outlay towards paying down the loan. Both these features therefore impacted individuals' access to credit without changing the value of their wealth.

Commercial banks were not restricted in offering conventional bank loans using the house as collateral, either before or after 1992. However the granting of such bank loans was subject to a regular credit assessment based on project's projected cash flows and furthermore, the riskiness of the project was priced into the loan. In practice such loans were mainly used to cover the part of the house price that exceeded the legal limit for mortgage loans. Our discussions with practitioners suggests that bank loans using housing equity as collateral were rarely used for financing business-startups.[2] Even when granted, however, the discussion above helps to put in context that while mortgage loans had a fixed rate and were not assessed a risk premium, the interest rate on collateralized bank loans would be set by the bank and include a premium for the project's riskiness. In practice, therefore, even those who might have borrowed from commercial banks would have experienced a decline in the cost of finance due to the mortgage reform. Overall, therefore, the reform gave households access to credit at a significantly lower price than was possible before and allowed borrowers who could not previously obtain secured loans in commercial banks because they were deemed too risky to now get access to credit through mortgage banks.[3]

[2] We have conducted extensive interviews with practitioners, including a director of a major commercial bank who was responsible for collateralized loans in this period. He states that loans based on housing equity as collateral were practically never used for financing business start-ups as the bank typically considered projects needing loans of this type to be of too low quality or too risky.

[3] The minutes of the parliamentary committee preparing the mortgage reform, obtained through the physical archives of the Danish Parliament, state that the mortgage reform was

As noted in the Introduction and in footnote 1, the highly structured mortgage market in Denmark at the time was such that the equity unlocked by the reform was driven largely by the timing of the house purchase and the level of the down-payment. That is, while it was possible to refinance mortgage loans prior to the reform to lock in lower interest rates, refinanced loans had to be of the same maturity and the principal could not be expanded. Similarly, people could prepay their loan, but having done so, it was not possible to extract equity through a mortgage loan on the same house. These restrictions implied that mortgage-loan-to-value ratios across individuals in 1992 were determined primarily by the timing of the house purchase relative to the reform. We use this cross-sectional variation in the available equity at the time of the reform to identify the effect of getting access to credit by comparing the propensity to become a business owner across households who entered the reform period with high vs. low amounts of housing equity that could be used to collateralize loans for the business.

Table 1 details this cross sectional variation that we exploit in our analysis. For those in our sample in 1991, it shows the equity to value (ETV), or the percentage of house value that is available to collateralize for investments other than the home, broken down by an individual's age and when they bought their house. As can be seen from Table 1, the level of equity is much more stable across rows than within columns. That is, the primary driver of the amount of housing equity available to collateralize seems to be driven by the timing of the home's purchase. Those who bought their home after 1984 tend to have less than 20% of their housing equity available to draw on, while those who bought their houses earlier tend to have much greater housing equity available to borrow against. While age, which proxies for life cycle factors that would impact the timing of the home purchase, is clearly important, Table 1 documents that there was significant variation in available equity within age buckets, which in turn was strongly correlated with the year in which the house was bought. Although our discussion above helps document that the reform was unanticipated, the timing of the house purchase is clearly not random, and there may be a concern that those who buy homes early vs. late may be systematically different along some unobserved dimension that may matter for entrepreneurship. Our detailed demographic covariates, as well as panel data are extremely valuable to address this concern, as they allow us to control for numerous observables as well as include individual fixed effects.

expected to reduce the interest rate on secured loans from 15% to 11% for the average borrower, but the changes would be of different magnitudes across households depending on risk characteristics of the project and the borrowers ability to service the loan.

3 Data

We use a matched employer-employee panel dataset for this study that is a significant improvement over data used in most prior studies on financing constraints. The data is drawn from the Integrated Database for Labor Market Research in Denmark, which is maintained by the Danish Government and is referred to by its Danish acronym, IDA. IDA has a number of features that makes it very attractive for this study.

First, the data is collected from government registers on an annual basis, and has detailed micro data on the labor market status of individuals, including their primary occupation. An individual's primary occupation in IDA is characterized by their main occupation in the last week of November. This allows us to identify entrepreneurs in a much more precise manner than many prior studies. For example, we can distinguish the truly self-employed from those who are unemployed but may report themselves as self-employed in surveys. We can also distinguish the self-employed from those who employ others in their firm. Finally, since our definition of entrepreneurship is based on an individual's primary occupation code, we are also able to exclude part-time consultants and individuals who may set up a side business in order to shelter taxes.

Second, the database is both comprehensive and longitudinal: all legal residents of Denmark and every firm in Denmark is included in the database. This is particularly useful in studying entry into entrepreneurship where such transitions are a rare event. Our sample size of entrepreneurs is therefore considerably larger than most studies of entrepreneurship at the individual level of analysis. Our analyses are based on a 25% random extract from this database, which provides annual observations on each included individual for nine years from 1988-1996. It also allows us to control for many sources of unobserved heterogeneity at the individual level, including individual fixed effects. Given that the reform was first introduced in May of 1992 and data are recorded as of November, we include 1992 in our post-reform period and measure individual attributes as of 1991.

Third, the database links an individual's ID with a range of other demographic characteristics such as their age, gender, educational qualifications, marital status, number of children, as well as detailed information on income, assets and liabilities.[4] House value, cash holdings, mortgage debt, bank debt, and interest payments are reported automatically at the last day of the year by banks and

[4]Assets are further broken into six categories: housing assets, shares, deposited mortgage deeds, cash holdings, bonds, and other assets. Liabilities are broken into four different categories up to 1992: mortgage debt, bank debt, secured debt and other debt. Importantly, the size of the mortgage is known up to 1993. After this point definitions of the available variables are changed. A measure of liabilities that is consistent across the entire observation period can only be obtained for the total size of the liability stock.

other financial intermediaries to the tax authorities for all Danish tax payers and are therefore considered very reliable. While cash holdings and interest payments are recorded directly, the house value is the tax assessed value scaled by the ratio of the tax assessed value to market value as is recorded among traded houses in that municipality and year, and mortgage debt is recorded by the market value of the underlying bonds at the last day of the year. The remaining components, including the data on individual wealth, are self-reported, but subject to auditing by the tax authorities because of the presence of both a wealth tax and an income tax. The detailed data on liabilities, assets and capital income is particularly useful for a study looking at entrepreneurship where wealth is likely to be correlated with a host of factors that can impact selection into entrepreneurship (Hurst & Lusardi, 2004).

We match this individual-level data from IDA into two other registers: first, we match individuals to a register that tracks home ownership and the date that an individual last moved from an address. This register goes back to 1970, so although our panel starts in 1988, we are able to code the date of last move for a home owner in our database going back much further. As seen in Table 1, this match allows us to document that the timing of the house purchase is a strong driver of the amount of equity an individual had in their house in 1991. Second, we match entrepreneurs in the IDA data to a register recording the VAT balances of firms. While the match on this register is not perfect (we are able to match 60% of the individuals we classify as entrepreneurs in the IDA data), we use this as a way to examine more details on firm outcomes such as the level of sales or profit at entry and over the life of the firm.

3.1 Sample

Since we are exploiting a mortgage reform for our analysis, we focus on individuals who are home-owners in 1991 (the year before the reform). Among home owners, we focus on those who are between the age of 25 and 50 in 1991, to ensure that we do not capture individuals retiring into entrepreneurship. Therefore, the youngest person at the start of our sample (in 1988) is 22 and the oldest person at the end of our sample (in 1996) is 55. Finally, we focus on individuals who are not employed in the agricultural industry in 1991, because, like many western European nations, the agricultural sector in Denmark is subject to numerous subsidies and incentives that may interact with entrepreneurship. We have access to a nine year panel for a 25% random sample of these individuals (who were home owners, between the ages of 25 and 50 and not involved in the agricultural sector, all in 1991), yielding data on 303,431 individuals for the years 1988-1996. There is some attrition from our panel due to death (after 1991) and individuals who are living abroad and hence not in the tax system in a given year (both before and after 1991). However, as can be seen from Table 2, this attrition leads to less than a 1% fall relative to a balanced panel, yielding a total of 2,708,892 observations.

3.2 Definition of Entrepreneurship

We focus our analysis of entrepreneurship on individuals who are employers (that is, self employed with at least one employee) in a given year. We use this measure to focus on more serious businesses and make our results more comparable with studies that use firm-level datasets (e.g. such as the Longitudinal Business Database in the US, that are comprised of firms with at least one employee) as well as those that study employment growth in the context of entrepreneurship. Table 2 documents that about 3% of our sample are coded as entrepreneurs in a given year and that the annual probability that an individual enters entrepreneurship is 0.56%.[5]

3.3 Descriptive Statistics

Tables 3A, 3B and 3C provide descriptive statistics on the main dependent variable and the control variables, broken down by the buckets of housing equity available at the time of the reform. They highlight that individuals across these buckets look quite similar on many observable dimensions, including in their propensity to be entrepreneurs. Reflecting the variation shown in Table 1, Panel A shows that those with greater equity to house value (ETV) bought their home earlier than those with low ETV and that they tend to be slightly older. Reassuringly, the differences in age and other demographic characteristics do not seem to be large and we have verified that the trends in entry across these groups look similar before 1991. In addition, as outlined below, we include a full set of covariate-year fixed effects to address residual sources of unobserved heterogeneity.

4 Regression Results

We start by documenting that the reform impacted a large number of individuals and that it was substantial. Figure 1 plots the amount of equity that was unlocked for the individuals in our sample. The X-axis buckets individuals into 100 bins of equity to value (ETV) in 1991. We then plot the amount of equity that was unlocked for individuals in each of these buckets (measured on the left

[5]These probabilities are lower than those typically associated with self employment, because we exclude self employed individuals without any employees from our definition of entrepreneurship. As an example, of the 26 million firms in the US, 20 million are comprised of self employed individuals without any employees. Studies using the Longitudinal Business Database and other equivalent Census data in the US have figures that are comparable to ours. For example, Kerr, Kerr and Nanda (2014) use a definition that includes all initial employees of new firms in the US, and get entrepreneurship rates in the US of about 5%.

Y-axis) at the mean, 25th percentile, median and 75th percentile. These lines document two important facts. First, the amount of equity unlocked was substantial. The average and the median (which track each other closely) amount of equity unlocked by someone with an ETV of 0.6 was over 200,000 DKK (over $30,000). Some individuals with high levels of ETV had over 400,000 DKK unlocked by this reform. Second, the slope of the lines are constant, which documents that the dollar value of equity unlocked was a constant proportion of the ETV in 1991. In other words, the average house value across those in different ETV buckets was extremely well-balanced, suggesting that ETV in 1991 is a good measure for the total amount of credit that was unlocked across the buckets.

Table 3 documents that about 50% of the individuals in our sample benefited from the reform and for these people, Figure 2 calculates the amount of equity unlocked as a share of the individual's annual disposable income in 1991 and shows that even in these terms, the amount unlocked was substantial. Figure 2 shows that the median treated individual (i.e. where ($ETV > 0.2$) got access to credit amounting to at least a year's disposable income and some got access to a lot more. The median amount of housing equity unlocked was 147,000 DKK and the average equity unlocked was 200,000 DKK, $33,819 (using the end of 1991 exchange rate of 5.91).

4.1 Impact of Mortgage Reform

Having shown that a large number of individuals had a substantial amount of housing equity unlocked by the reform, we next turn to documenting that individuals did respond to this access to collateralized credit by increasing their personal debt. That is, we document that the channel through which the reform was meant to operate did in fact show substantial traction. We focus on interest payments on all outstanding debt rather than the debt level itself because the interest payment measure is less noisy. However, we have verified that our results hold by looking directly at debt as well.[6] In Column 1 of Table 4, we report the results from a differences in differences specification:

$$InterestPayments_{it} = \beta_0 + \beta_1 POST_t * I(ETV_{91} > 0.25)_i * + \beta_2 I(ETV_{91} > 0.25)_i + \gamma_i X_{i,t} + \epsilon_{it} \tag{1}$$

where $InterestPayments_{it}$ refers to the total interest payments on outstanding debt paid by individual i in year t. $X_{i,t}$ refer to fixed effects for the control variables outlined in Panel A of Table 3 interacted with year dummies. This

[6] The majority of debt is composed of mortgage debt which is recorded in our data by its market value which is influenced by market fluctuations in the interest rate. Only fixed rate mortgages are available in this period, and interest payments are therefore deterministically related to the coupon rate and thus free of influence from market fluctuations.

includes fixed effects for the individual's gender, educational background, marital status, children, as well as fixed effects for age cohorts (one for each year from 25-50) interacted with year fixed effects.[7] The dummy $I(ETV_{91} > 0.25)_i$ takes the value 1 for individuals whose ETV in 1991 was greater than 0.25. We focus on 0.25 as those below this level are unlikely to have benefited from the reform. The reform only allowed individuals to borrow up to 80% of the home value; even if individuals lowered their payments by extending a mortgage from 20-30 years, those below 0.25 in ETV would have not gained sufficient equity to extract any debt for non-housing purposes. Thus $I(ETV_{91} > 0.25)_i$ captures those who were treated by the reform. $POST_t$ takes a value of 1 for the years 1992-1996 and zero otherwise. Standard errors are clustered at the individual level. Our main coefficient of interest is β_1 which captures the relative impact of the treatment group in the post period compared to the pre-period within the cells specified by the covariate-year fixed effects.

As can be seen from column 1 of Table 4, those in higher ETV buckets, by construction, had smaller interest payments prior to the reform. However, those in the treated group increased their interest payments by approximately 3,200 DKK more than the control group from 1992-1996. Column 2 shows that this was not driven by the fact that those in high ETV buckets were in municipalities that experienced differential house price changes or happened to be working in certain industries. It is robust to the inclusion of municipality-year and industry-year fixed effects. Municipality-year fixed effects refer to a fixed effect for each of 297 municipalities interacted with year dummies. Industries are measured at the SIC 1 level and hence control for being in one of 9 industries associated with the individual's primary occupation in 1991. Finally in Column 3, we add individual fixed effects. Since our identification is driven off the timing of the house purchase relative to the reform – which, although unanticipated is not random – we need to account for the fact that those who bought their homes earlier (or did not move) may be systematically different to those who did not. Including individual fixed effects is particularly effective as it helps us document the impact of the reform within individual, by accounting for any fixed differences across individuals in our sample. The inclusion of individual fixed effects implies that our identification now comes from within-individual differences over time. The fact that the coefficients are so stable across columns 1-3 is reassuring, since it suggests that conditional on controlling appropriately for covariates, the amount of equity released by the reform was unrelated to fixed individual attributes.

Columns 4-6 of Table 4 break up the dummy variable $I(ETV_{91} > 0.25)_i$ into

[7] That is, we compare those who are, say 25 years in 1988 with other 25 year olds in 1988 over the entire sample period and not to those who are 24 in 1988, so are 25 in 1989.

three categories, so that we now compare how being in each of the top three quartiles of ETV distribution had an impact on credit extraction following the reform. The results again provide a clear pattern of increasing credit extraction for those with greater unlocked housing equity, with interest payments rising from about 2,000 DKK more in the post period for those whose ETV was between 0.25 and 0.5 to 4,600 DKK more for those in the highest ETV bucket. The magnitude of the increase in interest payments in column 3 corresponds to an increase in the debt level of about 37,031 DKK ($6,266) which is equivalent to homeowners borrowing an average of $0.19 for each dollar of housing collateral unlocked by the reform. Interestingly, this increase in borrowing is identical to the elasticity of borrowing reported by Mian and Sufi (2014), when studying house price gains and US household spending from 2002-2006. While on average, the increased borrowing is about a fifth of the increase in available collateral, a few people extract a lot of credit while many choose not to. This variance in credit extraction is masked in the OLS regressions, but in unreported quantile regressions we find that, as might be expected, the average results are being driven by a smaller number of individuals extracting a much greater percentage of the collateralized credit available for them to draw on.

The results from Table 4 document that the reform not only had the potential to unlock credit but it in fact did lead to a strong 'first stage' where those in the treatment group extracted more credit that those in the control group. In Appendix B and Appendix Figure B2, we report the coefficients from a dynamic specification, where the coefficient in column 3 of Table 4 is interacted with year dummies and is shown relative to 1992. It shows a pattern consistent with reform leading to the increase in credit for those in the treated group.

4.1.1 Change in Net Entrepreneurship

Having established that the reform unlocked a significant amount of housing collateral and that those in the treatment group responded to this by increasing their personal debt, we now turn to study the impact of the reform on entrepreneurship. If credit constraints were holding back potential entrepreneurs in our sample, we should see that those who received an exogenous increase in access to credit would be more likely to be entrepreneurs. To examine this, we report the results from the difference in differences reduced form specification:

$$ENTREPRENEUR_{it} = \beta_0 + \beta_1 POST_t * I(ETV_{91} > 0.25)_i + \beta_2 I(ETV_{91} > 0.25)_i + \gamma_i X_{i,t} + \epsilon_{it} \qquad (2)$$

where the empirical framework and the identification strategy is the same as that for the regressions in Table 4, but where we now have entrepreneurship as the outcome variable. Specifically, the dependent variable is an indicator that takes a value of 1 if the individual is coded as being an entrepreneur in year t. All regressions in Table 5 are run as linear probability (OLS) models rather than non-linear logit or probit regressions given the large number of fixed effects. Note that since we include entrepreneurs and non-entrepreneurs in our sample in each year, these estimations measure the impact of the reform on net entrepreneurship (being an entrepreneur), as opposed to remaining an entrepreneur or becoming an entrepreneur (which we examine in subsequent analyses).

Columns 1-3 of Table 5 report the results for the indicator $I(ETV_{91} > 0.25)_i$ and as with Table 4, build up from including only covariate-year fixed effects to including individual fixed effects. Note that since our dependent variable is a binary variable, the regressions with individual fixed effects are effectively identifying off switchers - that is, those who either enter or exit entrepreneurship. The fact that, as with Table 4, the coefficients on the interaction term $POST_t * I(ETV_{91} > 0.25)_i$ are extremely stable is reassuring as it suggests that the subset of individuals who switched into or out of entrepreneurship was representative of the larger cross-section of individuals studied in Columns (1) and (2).

The magnitudes of the coefficients in Table 5 are small. The coefficient on $POST_t * I(ETV_{91} > 0.25)_i$ in column 3 of Table 5 implies a 12 basis point increase in net entrepreneurship. Given the baseline probability of being an entrepreneur was 3% in the pre-period (as seen in Table 3), this implies about a 4% relative increase in the probability of being an entrepreneur for the treated group in the post period. This increase is small given the average increase in available home equity of approximately \$30,000, which is large in absolute terms and in relative terms to annual disposable income. Figure 3 plots the coefficients of the dynamic specifications, where the interaction shown in column 3 is instead broken into annual interactions, and shown relative to 1992. The dynamic specifications show a pattern consistent with the reform driving the increases in net entrepreneurship and also show that the small coefficient is in fact quite stable over the few years following the reform.

Columns 4-6 break the dummy variable $I(ETV_{91} > 0.25)_i$ into three equal categories and show that the increase is driven largely by those with an $ETV > 0.5$. While those with an $ETV > 0.25$ do exhibit a slight increase, it is not statistically significant. The magnitudes in Column 6, together with the baseline entry rates shown in Table 3 suggest that the reform increased the propensity to be an entrepreneur for those with substantial increases in equity by about 5.5%. Given that the average amount of equity unlocked in the highest ETV bracket was approximately \$55,000, our results highlight that while clearly impacting entrepreneurship, the effect of the relaxed constraints were small relative to the size of the treatment, even for those with large increases in available housing collateral.[8]

Our results showing an increase in the amount of entrepreneurship leads us to examine the channel through which this occurred. The reform could have impacted existing businesses that were more likely to survive and/or impact the entry of new businesses. We now turn to examine these two channels.

4.1.2 Survival of existing businesses

To look at the impact of the reform on surviving businesses, we focus on all individuals who were active entrepreneurs in 1988 and study the survival of these businesses until 1996. Table 2 documents that 9,183 individuals were active entrepreneurs in 1988. For these entrepreneurs, we run the same difference-in-difference specification outlined in equation (2) above, and where the dependent variable continues to take a value of 1 if they are alive in year t, but takes a value of 0 if they fail.

Looking across Columns 1-3 of Table 6, we can see that as with Table 4 and 5, the inclusion of industry-year, municipality-year and individual fixed effects do not impact the coefficient on $POST_t * I(ETV_{91} > 0.25)_i$. The coefficient in column 3 of Table 6 documents a statistically significant effect on survival for existing businesses. About 65% of the businesses in the control group are still alive in 1996, implying that the 3.2 percentage point increase in survival is equivalent to a 5% higher likelihood of survival relative to those with low ETV. Columns 4-6 show that these effects are even stronger for those that received the largest treatment, rising to about a 7% higher chance of survival relative to the control group for those in the highest quartile of ETV, but only 3% for those in the 0.25-0.5 bucket. The fact that the results are stronger for the the group that received the largest treatment is reassuring, since it supports the mechanism through which we expect the response to occur.

One would expect that firms in industries that are more reliant on external finance to benefit more from the ability to borrow against the home. In order to look in to this we allocate firms to industries that are more versus less dependent on external finance. We do this by by calculating, in a pre-period, the change in debt associated with starting a business in each of 111 different industries. Industries that are above the median according to this measure are classified as being more dependent on external finance. The details of the industry allocation are provided in Appendix A, where we also show a positive correspondence to a

[8] In unreported regressions and consistent with Hurst & Stafford (2004), Leth-Petersen (2010)and Mian & Sufi (2011) we find that the majority of credit that was extracted was used for large consumption items such as home improvement or buying a new car, rather than in investment into businesses.

similar measure constructed using the Survey of Small Business Finances in the US.

Table 7 expands Table 6 by splitting it into firms in industries that are capital intensive vs. not. Comparing column (1) and (2) suggests that the effect of the credit market reform was bigger for firms in capital intensive industries, but we note that the difference between the effect estimated in column (1) and (2) is not statistically different from zero. Expanding the number of ETV categories, as is done in columns (3) and (4) reveals that the effect is driven by the higher ETV groups, and that it is only the highest ETV group where one sees magnitudes that are statistically different from zero.

Although we see existing firms being more likely to survive when their owners receive a larger increase in available credit, this could also be driven by two possible mechanisms. On the one hand, it could imply that firms that were previously constrained were forced to shut down and could now benefit from the increased credit availability to support the operations of the firm. On the other hand, one might imagine that the increase in credit may have led firms that were badly run to continue operating because their founders had a preference for being self employed, but did not need to justify this decision to the bank. To tell these two mechanisms apart, we look in Table 8 at firm performance for the set of firms that were in existence at the time of reform. In particular, we focus on firm-level employment, sales and gross profit (sales less purchases). These outcomes are obtained from VAT accounts, which as we outlined above, only give us a 60% match with the firms in our sample.[9]

The first three columns of Table 8 report results for the balanced panel of firms. That is, when a firm exits the sample, we code their sales, profits and employment as zero. This has the advantage of ensuring the results are not driven by selection, but on the other hand, they confound performance and survival and hence provide an upper bound for the performance effects of the reform. The next three columns focus on the set of firms that survived until 1996, so they are not confounding performance and survival, but they are a selected sample since they were strong enough to survive across the entire period. The first three columns of Table 8 show that on average, existing firms increase profit added by about 40,000 DKK, sales by about 117,000 DKK and employment by the equivalent of 0.2 full time employees. The effects are estimated imprecisely and are only significant at the 10% level. This marginal increase in performance is, of course, conflated by the higher survival probabilities of the firms. In Columns 4-6 we restrict our

[9] In order to ensure that our performance results are not due to a sample selection bias, we have reproduced table 6 using only the subset of firms we were able to match in Table 8. These result were not different from those presented in Table 8, convincing us that our performance results were not driven by sample selection bias.

analysis to firms that survived the entire period and find that for these firms, the results are reversed. The marginal firm that survived over the entire period due to the reform seems to have been of lower quality (although imprecisely estimated). In sum, our results suggest that the reform increased survival, but that it did not lead to an increase in performance conditional on survival.

4.1.3 Entry into entrepreneurship

We next turn to examining entry into entrepreneurship. Table 9 reports the coefficients from the linear probability models with the same specifications, where the dependent variable now takes a value of 1 if the individual was not an entrepreneur in $t-1$ but became an entrepreneur in year t. As with Tables 4, 5 and 6, Table 9 shows the coefficients are extremely stable across columns. It shows that there was also a marked increase in entry following the reform. Given that the baseline probability of entry is 0.56% (as seen in Table 2), the coefficient in column 3 of Table 9 implies that the treated group experienced a 10% increase in entry following the reform. Columns 4-6 show that similar to the patterns in Table 6, the entry was largely driven by those in the highest ETV bucket, suggesting that the amount of equity that needs to be released for the collateral channel to play a role can be substantial.

Table 10 further breaks this entry into those starting businesses in industries that were classified as being more dependent on external finance vs. less dependent. Comparing columns 1 and 2 of Table 10 with column 3 of Table 9 shows that the majority of the increased entry came from those entering more capital intensive businesses. In fact, they show that the increase in less capital intensive industries was not statistically different from zero. On the other hand, entry into capital intensive industries was not only statistically significant, but larger than the entry into less capital intensive industries. This finding is also reinforced by looking at columns 3 and 4 of Table 10, where the greatest impact of the reform seems to be among those in the highest ETV bucket starting businesses that were more dependent on external finance.

The results associated with net entrepreneurship in Table 5 show smaller elasticities than would be expected seeing the results in Table 9. To investigate further, therefore, we examine the extent to which the entrants start businesses that survive a long period of time. We separate entrants into those who started businesses that last less than 3 years relative to those who found businesses that last at least 3 years. These results are reported in Table 11. Comparing these businesses reveals a striking pattern. The vast majority of the entrants are those that fail within 2 years of entry, which is why the overall number of entrepreneurs, reported in Table 5, shows a much smaller increase. Columns 3 and 4 show that the churning is associated with all the buckets of ETV, while those with the largest increase in available collateral also start some firms that last more than 3 years. Interestingly, looking at columns 5 and 6 shows that one potential reason that these business owners seem to fail is because those in the treatment group significantly increase the likelihood that they will start businesses in industries where they have no prior experience.

This result is interesting as it suggests that part of what the reform allowed individuals to do was experiment by starting businesses that the bank may not have given them credit for. This could be seen as either good or bad: on the one hand, if asymmetric information prevented banks from lending to high quality businesses, then the reform would facilitate the entry of better firms. For example, the banks might incorrectly ration credit to individuals who had no prior background in an industry, but who were potentially high quality entrepreneurs. Similarly, since banks are concerned with downside protection, it is possible that the access to housing collateral allowed individuals to start riskier firms, that may have been more likely to fail, but conditional on surviving, in fact did better. On the other hand, if banks were rationing credit to those who should not have started businesses because the projects were of low quality, this suggests that the credit market may have been working reasonably well prior to the reform.

In order to tease these two explanations apart, we turn to examine the performance of the businesses, similar to the estimations in Table 8. In Table 12, we study the three-year gross profits, sales and employment of entrants, for all firms that entered between 1988 and 1996. These outcomes are obtained from VAT accounts, which as we outlined above, only give us a 60% match with the firms in our sample. Since we have fewer observations in this table, we are unable to include a full set of controls interacted with year dummies but instead include individual controls observed in 1991 as well as year fixed effects in all regressions, and add municipality fixed effects for some specifications. The results show that profits, sales and employment were lower in the post period for firms started by owners who got access to home equity, and even when considering the subset of entrants who survived at least three years in Columns 3-4, we do not find any evidence that the performance metrics improved as a consequence of the reform. Columns 5-7 report the results from quantile regressions to show that the results in columns 1-4 are not driven by outliers and that they are equally present across the profit distribution. Overall, these results point to the fact that the reform seems to have lowered the discipline of external finance. While we cannot conclusively say whether these were negative NPV projects, it suggests that the possibility of tapping into home equity either allowed individuals to start lower quality projects, that would have had a hard time getting financed by the bank, (but could be funded by personal debt since the bank was no longer lending based on the attributes of the project). That is, the marginal project funded in the post period by those with access to home equity was of lower quality than the average quality of projects started prior to the reform. This is an interesting result that also helps to reconcile the fact that gross entry following the reform was larger than the net effect of the reform on entrepreneurship.

5 Discussion and Conclusions

We combine a unique mortgage reform in Denmark with micro data to study how an exogenous increase in access to credit through the unlocking of housing collateral for personal loans had an impact on entrepreneurship. Our context is particularly attractive since it allows us to distinguish the credit channel from wealth effects, as well as quantify the size of the increased access to credit, allowing us to precisely estimate the magnitude of credit constraints in our context. The reform had a sizeable impact on the ability to draw on debt backed by home equity. The average increase in home equity was $30,000, equivalent to over a year's worth of disposable income for the median treated individual in our sample. Yet we find that this led to only a 12 basis point increase in entrepreneurship on average, which translated into a 4% increase in net entrepreneurship relative to the baseline. Thus, although we find a positive and statistically significant effect of relaxing credit constraints on entrepreneurship, the magnitudes are small. Furthermore, we find that an important reason for the small magnitude was that the marginal business founded in the post period by those who benefited from the reform was of lower quality, leading to mostly churning entry, where the new entrants failed within two years of entry. This is similar to findings by Kerr & Nanda (2009) who find that while the US banking deregulations over the 1980s led to an increase in entrepreneurship, a disproportionate share of this increase was in churning entry, implying that the net effect of deregulation was less than that suggested by papers looking only at gross entry (Black & Strahan, 2002; Cetorelli & Strahan, 2006).

Our results therefore paint a more nuanced picture of the extent to which financing constraints are important in settings with well-developed credit markets, and the role that home equity can play in alleviating these. The fact that housing collateral shifts the bank's adjudication decision from a specific project to the creditworthiness of the borrower has the potential to be a dual edged sword: on the one hand, good projects that were precluded from entry due to asymmetric information may be able to be started or sustained. On the other hand, optimistic entrepreneurs or those with non-pecuniary benefits to own businesses may start lower quality businesses because they do not face the same discipline from the bank.

Our results also speak to the longstanding question of the importance of credit constraints for entrepreneurship. They highlight the importance of considering both entry and net entrepreneurship as outcome variables, since policies that aim to increase entry may not necessarily translate into equivalent increases in net entrepreneurship if the marginal entrants are of lower quality and are much more likely to fail.

Table 1: Average equity-to-value in 1991 for houseowners, based on age and year of last move

Table 1 shows the mean equity to value in 1991 by age of individual and the year of house purchase. Based on a 25% sample. Cells with fewer than 100 observations are excluded.

Year of last move	Age in 1991																									
	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50
1970 or before																	0.62	0.62	0.62	0.62	0.65	0.65	0.65	0.66	0.67	0.69
1971																	0.59	0.62	0.63	0.62	0.61	0.62	0.62	0.64	0.65	0.68
1972															0.58	0.63	0.59	0.59	0.60	0.61	0.59	0.61	0.60	0.63	0.62	0.64
1973														0.59	0.57	0.58	0.57	0.56	0.57	0.57	0.60	0.59	0.61	0.61	0.63	0.64
1974													0.56	0.54	0.52	0.56	0.55	0.56	0.56	0.55	0.57	0.59	0.58	0.58	0.61	0.62
1975												0.48	0.49	0.47	0.50	0.50	0.49	0.50	0.51	0.52	0.55	0.53	0.55	0.56	0.56	0.58
1976											0.43	0.45	0.41	0.43	0.45	0.44	0.46	0.46	0.49	0.50	0.48	0.50	0.51	0.51	0.54	0.54
1977										0.39	0.38	0.41	0.39	0.42	0.41	0.44	0.45	0.45	0.47	0.47	0.47	0.49	0.49	0.52	0.53	0.53
1978								0.36	0.38	0.34	0.36	0.37	0.40	0.37	0.39	0.38	0.40	0.41	0.42	0.44	0.43	0.44	0.45	0.47	0.48	0.50
1979							0.31	0.30	0.32	0.31	0.31	0.31	0.31	0.35	0.35	0.36	0.34	0.34	0.35	0.37	0.38	0.38	0.38	0.41	0.43	0.45
1980						0.34	0.34	0.31	0.28	0.30	0.29	0.29	0.28	0.32	0.30	0.31	0.32	0.33	0.33	0.36	0.34	0.36	0.35	0.39	0.41	0.38
1981					0.22	0.28	0.25	0.29	0.28	0.29	0.29	0.30	0.29	0.30	0.33	0.32	0.36	0.36	0.32	0.34	0.37	0.38	0.37	0.40	0.39	0.41
1982				0.22	0.26	0.26	0.23	0.25	0.25	0.26	0.29	0.27	0.28	0.26	0.28	0.29	0.29	0.29	0.31	0.31	0.30	0.31	0.31	0.33	0.34	0.35
1983			0.24	0.22	0.23	0.21	0.22	0.22	0.22	0.23	0.23	0.23	0.23	0.24	0.22	0.25	0.26	0.24	0.27	0.26	0.26	0.27	0.26	0.29	0.30	0.31
1984		0.21	0.16	0.17	0.18	0.15	0.16	0.16	0.14	0.17	0.16	0.15	0.18	0.18	0.19	0.17	0.18	0.17	0.19	0.18	0.18	0.20	0.21	0.22	0.21	0.22
1985	0.18	0.17	0.16	0.14	0.15	0.15	0.14	0.14	0.15	0.15	0.14	0.14	0.17	0.16	0.16	0.15	0.16	0.16	0.17	0.18	0.21	0.17	0.20	0.21	0.21	0.23
1986	0.14	0.13	0.13	0.13	0.12	0.12	0.12	0.12	0.11	0.12	0.13	0.13	0.13	0.13	0.14	0.13	0.14	0.14	0.15	0.16	0.16	0.17	0.19	0.18	0.20	0.21
1987	0.14	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.13	0.13	0.12	0.14	0.14	0.14	0.15	0.15	0.15	0.15	0.15	0.18	0.16	0.18	0.20	0.22	0.21	0.20
1988	0.12	0.13	0.11	0.12	0.12	0.12	0.13	0.14	0.12	0.13	0.14	0.13	0.15	0.15	0.14	0.15	0.15	0.17	0.17	0.17	0.19	0.20	0.18	0.18	0.22	0.23
1989	0.13	0.13	0.13	0.13	0.14	0.14	0.14	0.13	0.14	0.13	0.13	0.15	0.16	0.16	0.17	0.15	0.18	0.19	0.17	0.18	0.21	0.19	0.20	0.21	0.21	0.23
1990	0.15	0.14	0.15	0.15	0.14	0.16	0.15	0.15	0.16	0.17	0.16	0.17	0.16	0.16	0.16	0.19	0.19	0.19	0.19	0.20	0.22	0.21	0.22	0.26	0.24	0.26

Table 2: Stock of entrepreneurs and transition probability

Table 2 shows stock of entrepreneurs and the probability of transitioning in to entrepreneurship for those in our sample.

	Stock of entrepreneurs			Transition into entrepreneruship		
	Total sample	Employers	Employer share of total	Potential Entrants	New Entrants	Transition probability
1988	300,758	9,183	3.05%	291,850	1,639	0.56%
1989	301,453	9,380	3.11%	292,271	1,558	0.53%
1990	302,445	9,279	3.07%	293,064	1,585	0.54%
1991	303,431	8,949	2.95%	294,149	1,780	0.61%
1992	302,283	9,651	3.19%	293,355	2,397	0.82%
1993	301,129	9,590	3.18%	291,497	1,517	0.52%
1994	300,057	9,615	3.20%	290,496	1,521	0.52%
1995	299,109	9,655	3.23%	289,521	1,364	0.47%
1996	298,227	9,774	3.28%	288,600	1,302	0.45%
Total	2,708,892	85,076	3.14%	2,624,803	14,663	0.56%

Table 3: Summary statistics

Panel A presents summary statistics for the 303,431 individuals in our sample based on their equity to value ratio in 1991 being either in the range of [0%-25%], (25%-50%], (50%-75%] or (75%-100%]. Panel B shows summary statistics for subset of individuals that where active employers in 1991. Panel C shows summary statistics for the subset of individuals that were new employers in 1991. Housing assets refer to the tax assessed valuation of the individual's property scaled with the ratio of market prices to tax assessed house values for house that have been traded in that municipality and year. Non housing assets include the individual's other assets including stocks, bonds and deposits. All variables are measured in 1991 before the reform. Value-add, sales and employment are computed based on the firms where information is available based on a match to the VAT register.

Panel A: Sample population

	Means by ETV91			
	[0.00-0.25]	(0.25-0.50]	(0.50-0.75]	(0.75-1.00]
Active employer	0.028	0.031	0.030	0.032
Age	36.44	39.96	43.04	42.44
Female=1	0.49	0.51	0.54	0.57
Partner=1	0.87	0.89	0.92	0.86
Kids=1	0.66	0.66	0.61	0.53
Educ, Vocational,	0.47	0.47	0.49	0.46
Educ, BSc	0.15	0.14	0.14	0.13
Educ, MSc, PhD	0.06	0.04	0.04	0.04
Housing assets, tDKK	733	845	879	705
Non-Housing assets, tDKK	68	76	86	132
Year of last address move	1985	1981	1977	1978
Wage employment	0.85	0.83	0.84	0.78
Self-employment but not active employer	0.03	0.04	0.04	0.06
Observations	170,632	56,578	41,103	35,118

Panel B: Active firm owners

	Means by ETV91			
	[0.00-0.25]	(0.25-0.50]	(0.50-0.75]	(0.75-1.00]
Age	39.45	42.25	44.35	43.63
Female=1	0.24	0.21	0.20	0.21
Partner=1	0.88	0.90	0.92	0.88
Kids=1	0.70	0.66	0.63	0.58
Educ, Vocational,	0.54	0.58	0.59	0.59
Educ, BSc	0.05	0.06	0.06	0.05
Educ, MSc, PhD	0.15	0.10	0.12	0.09
Housing assets, tDKK	893	965	853	723
Non-Housing assets, tDKK	211	186	243	815
Year of last address move	1984	1980	1977	1978
Wage employment	0.11	0.08	0.07	0.05
Self-employment but not active employer	0.09	0.08	0.09	0.08
Fraction alive after 3 years	0.64	0.70	0.73	0.74
Value-add, tDKK	888	914	931	854
Sales, tDKK	2720	2742	2698	2751
Number of employees	4.55	4.30	4.35	4.28
Observations	4,826	1,760	1,253	1,110

Panel C: Entrants				
	Means by ETV91			
	[0.00-0.25]	(0.25-0.50]	(0.50-0.75]	(0.75-1.00]
Age	37.72	40.42	43.16	42.37
Female=1	0.30	0.31	0.25	0.36
Partner=1	0.86	0.88	0.91	0.89
Kids=1	0.68	0.66	0.63	0.55
Educ, Vocational,	0.51	0.53	0.58	0.56
Educ, BSc	0.09	0.08	0.07	0.05
Educ, MSc, PhD	0.11	0.10	0.06	0.08
Housing assets, tDKK	885	901	1,069	543
Non-Housing assets, tDKK	152	141	389	183
Year of last address move	1985	1981	1978	1979
Wage employment	0.48	0.43	0.39	0.33
Self-employment but not active employer	0.41	0.45	0.53	0.58
Fraction alive after 3 years	0.44	0.51	0.48	0.45
Value-add, tDKK	373	556	437	471
Sales, tDKK	1251	1610	1351	1612
Number of employees	2.28	2.49	2.37	2.70
Observations	1,075	328	214	156

Table 4: The effect of the reform on the level of personal debt

Table 4 reports estimates from OLS regressions, where the dependent variable is the individual's total interest payment in each year. The main RHS variables are the bucket of equity to value in 1991 and the buckets interacted with an indicator for the post mortgage reform period. All columns include year fixed effects interacted with fixed effects for birth-cohort, educational level, partner, gender and having children, each measured in 1991. Column (2-3) and (5-6) include municipality-year fixed effects and industry-year fixed effects. Column (3) and (6) further include individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses. *, **, *** indicate statistically different from zero at 5%, 1% and 0.1% level.

Dependent Variable: Total interest payments, DKK						
	2 ETV groups			4 ETV groups		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*I(ETV91>0.25)	3,284 ***	3,298 ***	3,222 ***			
	(244)	(226)	(225)			
I(ETV91>0.25)	-15,134 ***	-14,753 ***				
	(362)	(344)				
Post*ETV91(.25-.50]				2,103 ***	2,134 ***	2,078 ***
				(245)	(228)	(226)
Post*ETV91(.50-.75]				3,913 ***	3,919 ***	3,841 ***
				(300)	(278)	(278)
Post*ETV91(.75-1.0]				4,742 ***	4,740 ***	4,642 ***
				(376)	(372)	(369)
ETV91(.25-.50]				-10,493 ***	-10,147 ***	
				(348)	(328)	
ETV91(.50-.75]				-20,222 ***	-19,913 ***	
				(420)	(393)	
ETV91(.75-1.0]				-17,928 ***	-17,408 ***	
				(724)	(713)	
Covariates-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-year fixed effects	No	Yes	Yes	No	Yes	Yes
Industry-year fixed effects	No	Yes	Yes	No	Yes	Yes
Individual fixed effect	No	No	Yes	No	No	Yes
Observations	2,708,881	2,708,881	2,708,881	2,708,881	2,708,881	2,708,881

Table 5: Effect of the reform on net entrepreneurship

Table 5 reports estimates from OLS regressions, where the dependent variable is an indicator variable that takes the value of 1 if the individual is an entrepreneur in a given year. The main RHS variables are the bucket of equity to value in 1991 and the buckets interacted with an indicator for the post mortgage reform period. All columns include year fixed effects interacted with fixed effects for birth-cohort, educational level, partner, gender and having children, each measured in 1991. Column (2-3) and (5-6) include municipality-year fixed effects and industry-year fixed effects. Column (3) and (6) further include individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses. *, **, *** indicate statistically different from zero at 5%, 1% and 0.1% level.

Dependent Variable: Dummy for being an active employer						
	2 ETV groups			4 ETV groups		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*I(ETV91>0.25)	0.00120 *	0.00118 *	0.00119 *			
	(0.00047)	(0.00047)	(0.00047)			
I(ETV91>0.25)	-0.00317 ***	-0.00397 ***				
	(0.00062)	(0.00061)				
Post*ETV91(.25-.50]				0.00075	0.00072	0.00073
				(0.00058)	(0.00058)	(0.00058)
Post*ETV91(.50-.75]				0.00157 *	0.00169 *	0.00170 **
				(0.00066)	(0.00066)	(0.00065)
Post*ETV91(.75-1.0]				0.00160 *	0.00146 *	0.00143 *
				(0.00072)	(0.00072)	(0.00072)
ETV91(.25-.50]				-0.00219 ***	-0.00282 ***	
				(0.00077)	(0.00076)	
ETV91(.50-.75]				-0.00711 ***	-0.00760 ***	
				(0.00093)	(0.00091)	
ETV91(.75-1.0]				-0.00049	-0.00197 *	
				(0.00102)	(0.00100)	
Covariates-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-year fixed effects	No	Yes	Yes	No	Yes	Yes
Industry-year fixed effects	No	Yes	Yes	No	Yes	Yes
Individual fixed effect	No	No	Yes	No	No	Yes
Observations	2,708,892	2,708,892	2,708,892	2,708,892	2,708,892	2,708,892

Table 6: Effect of the reform on existing firms

Table 6 reports estimates from OLS regressions, where the dependent variable is an indicator variable if the individual is an entrepreneur in that year. The sample consists of individuals who were entrepreneurs in 1988 over the period 1988-1996. The main RHS variables are the bucket of equity to value in 1991 and the buckets interacted with an indicator for the post mortgage reform period. All columns include year fixed effects interacted with fixed effects for birth-cohort, educational level, partner, gender and having children, each measured in 1991. Column (2-3) and (5-6) include municipality-year fixed effects and industry-year fixed effects. Column (3) and (6) further include individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses. +, *, **, *** indicate statistically different from zero at 10%, 5%, 1% and 0.1% level.

Dependent Variable: Dummy for being an active employer						
	2 ETV groups			4 ETV groups		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*I(ETV91>0.25)	0.03810 ***	0.03386 ***	0.03280 ***			
	(0.00778)	(0.00790)	(0.00781)			
I(ETV91>0.25)	0.02849 ***	0.02655 ***				
	(0.00570)	(0.00578)				
Post*ETV91(.25-.50]				0.02414 *	0.01883 +	0.01966 *
				(0.00991)	(0.01006)	(0.00994)
Post*ETV91(.50-.75]				0.04218 ***	0.03816 ***	0.03589 **
				(0.01110)	(0.01120)	(0.01109)
Post*ETV91(.75-1.0]				0.05572 ***	0.05330 ***	0.05048 ***
				(0.01138)	(0.01163)	(0.01152)
ETV91(.25-.50]				0.02706 ***	0.02599 ***	
				(0.00719)	(0.00726)	
ETV91(.50-.75]				0.02876 ***	0.02571 **	
				(0.00810)	(0.00818)	
ETV91(.75-1.0]				0.03047 ***	0.02837 ***	
				(0.00850)	(0.00860)	
Covariates-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-year fixed effects	No	Yes	Yes	No	Yes	Yes
Industry-year fixed effects	No	Yes	Yes	No	Yes	Yes
Individual fixed effect	No	No	Yes	No	No	Yes
Observations	79,733	79,733	79,733	79,733	79,733	79,733

Table 7: Effect of the reform on existing firm survival in industries that are more vs. less dependent on external finance

Table 7 reports estimates from OLS regressions, where the dependent variable is an indicator variable if the individual is an entrepreneur in that year. The sample consists of individuals who were entrepreneurs in 1988 over the period 1988-1996. The main RHS variables are the bucket of equity to value in 1991 and the buckets interacted with the post mortgage reform period. All columns include year fixed effects interacted with fixed effects for birth-cohort, educational level, partner, gender and having children, each measured in 1991, municipality-year fixed effects, industry-year fixed effects and individual fixed effects. Columns 1 and 3 report estimations for individuals in industries that were more dependent on external finance and columns 2 and 4 report estimations for individuals in industries that are less dependent on external finance. Standard errors are clustered at the individual level and are reported in parentheses. *, **, *** indicate statistically different from zero at 5%, 1% and 0.1% level.

Dependent Variable: Dummy for being an active employer				
	Capital Intensity			
	High	Low	High	Low
	(1)	(2)	(3)	(4)
Post*I(ETV91>0.25)	0.03737 **	0.02399 *		
	(0.01136)	(0.01123)		
I(ETV91>0.25)				
Post*ETV91(.25-.50]			0.02772	0.00901
			(0.01450)	(0.01406)
Post*ETV91(.50-.75]			0.02468	0.04172 *
			(0.01581)	(0.01621)
Post*ETV91(.75-1.0]			0.06629 ***	0.03101
			(0.01660)	(0.01715)
ETV91(.25-.50]				
ETV91(.50-.75]				
ETV91(.75-1.0]				
Covariates-year fixed effects	Yes	Yes	Yes	Yes
Municipality-year fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effect	Yes	Yes	Yes	Yes
Observations	40,683	39,050	40,683	39,050

Table 8: Performance of Existing Firms

Table 8 reports estimates from OLS regressions, where the dependent variable is the measure of performance in that year. The sample consists of individuals who were entrepreneurs in 1988 over the period 1988-1996, for whom we could find a match in the VAT register. The main RHS variables are the bucket of equity to value in 1991 and the buckets interacted with the post mortgage reform period indicator. All columns include year fixed effects interacted with fixed effects for birth-cohort, educational level, partner, gender and having children, each measured in 1991, municipality-year fixed effects, industry-year fixed effects and individual fixed effects. Columns 1-3 report the results for all individuals so conflate performance with survival, while Columns 4-6 restrict the sample to those who survived till 1996. Standard errors are clustered at the individual level and are reported in parentheses. *, **, *** indicate statistically different from zero at 5%, 1% and 0.1% level.

	Dependent Variable: Outcomes					
	All entries			Conditional on survival until at least 1996		
	Gross Profit	Sales	Employment	Gross Profit	Sales	Employment
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: 2 ETV Groups					
Post*I(ETV91>0.25)	38.88 +	117.20 +	0.18 +	-12.58	-49.54	-0.06
	(21.80)	(64.76)	(0.10)	(26.51)	(75.51)	(0.13)
I(ETV91>0.25)						
	Panel B: 4 ETV Groups					
Post*ETV91(.25-.50]	32.32	131.02 +	0.18	-24.3	-57.42	-0.04
	(26.58)	(73.78)	(0.13)	31.5	(84.14)	(0.16)
Post*ETV91(.50-.75]	18.29	130.83	0.19	-53.7	-85.93	-0.21
	(31.62)	(89.36)	(0.14)	39.3	(99.49)	(0.17)
Post*ETV91(.75-1.0]	68.94 *	83.54	0.27 +	43.0	-3.64	0.07
	(29.18)	(92.26)	(0.14)	35.7	(107.39)	(0.18)
ETV91(.25-.50]						
ETV91(.50-.75]						
ETV91(.75-1.0]						
Covariates-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37,547	37,547	37,547	18,753	18,753	18,753

Table 9: Effect of the reform on Entry

Table 9 reports estimates from OLS regressions, where the dependent variable is an indicator variable that takes the value of 1 if the individual is an entrepreneur in a given year and was not an entrepreneur in the prior year. The main RHS variables are the bucket of equity to value in 1991 and the buckets interacted with an indicator for the post mortgage reform period. All columns include year fixed effects interacted with fixed effects for birth-cohort, educational level, partner, gender and having children, each measured in 1991. Column (2-3) and (5-6) include municipality-year fixed effects and industry-year fixed effects. Column (3) and (6) further include individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses. *, **, *** indicate statistically different from zero at 5%, 1% and 0.1% level.

	Dependent Variable: Dummy for entering as an active employer					
	2 ETV groups			4 ETV groups		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*I(ETV91>0.25)	0.00065 ***	0.00061 **	0.00060 **			
	(0.00019)	(0.00019)	(0.00019)			
I(ETV91>0.25)	-0.00142 ***	-0.00148 ***				
	(0.00015)	(0.00015)				
Post*ETV91(.25-.50]				0.00039	0.00034	0.00033
				(0.00023)	(0.00023)	(0.00023)
Post*ETV91(.50-.75]				0.00046	0.00049	0.00046
				(0.00026)	(0.00026)	(0.00026)
Post*ETV91(.75-1.0]				0.00134 ***	0.00126 ***	0.00126 ***
				(0.00030)	(0.00030)	(0.00029)
ETV91(.25-.50]				-0.00117 ***	-0.00120 ***	
				(0.00019)	(0.00018)	
ETV91(.50-.75]				-0.00201 ***	-0.00204 ***	
				(0.00021)	(0.00021)	
ETV91(.75-1.0]				-0.00119 ***	-0.00137 ***	
				(0.00023)	(0.00023)	
Covariates-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-year fixed effects	No	Yes	Yes	No	Yes	Yes
Industry-year fixed effects	No	Yes	Yes	No	Yes	Yes
Individual fixed effect	No	No	Yes	No	No	Yes
Observations	2,708,892	2,708,892	2,708,892	2,708,892	2,708,892	2,708,892

Table 10: Effect of the reform on entry into more vs. less capital intensive industries

Table 10 reports estimates from OLS regressions, where the dependent variable is an indicator variable that takes the value of 1 if the individual is an entrepreneur in a given year and was not an entrepreneur in the prior year. The main RHS variables are the bucket of equity to value in 1991 and the buckets interacted with an indicator for the post mortgage reform period. All columns include year fixed effects interacted with fixed effects for birth-cohort, educational level, partner, gender and having children, each measured in 1991, municipality-year fixed effects, industry-year fixed effects and individual fixed effects. Columns 1 and 3 report entry into capital intensive industries while columns 2 and 4 report entry into less capital intensive industries. Standard errors are clustered at the individual level and are reported in parentheses. *, **, *** indicate statistically different from zero at 5%, 1% and 0.1% level.

Dependent Variable: Dummy for entering as an active employer						
	Capital Intensity					
	High		Low	High		Low
	(1)		(2)	(3)		(4)
Post*I(ETV91>0.25)	0.00043	***	0.00017			
	(0.00013)		(0.00014)			
I(ETV91>0.25)						
Post*ETV91(.25-.50]				0.00027		0.00006
				(0.00016)		(0.00017)
Post*ETV91(.50-.75]				0.00029		0.00018
				(0.00018)		(0.00019)
Post*ETV91(.75-1.0]				0.00091	***	0.00036
				(0.00022)		(0.00020)
ETV91(.25-.50]						
ETV91(.50-.75]						
ETV91(.75-1.0]						
Covariates-year fixed effects	Yes		Yes	Yes		Yes
Municipality-year fixed effects	Yes		Yes	Yes		Yes
Industry-year fixed effects	Yes		Yes	Yes		Yes
Individual fixed effect	Yes		Yes	Yes		Yes
Observations	2,708,892		2,708,892	2,708,892		2,708,892

Table 11: Effect of the reform on selection into entrepreneurship

Table 11 reports estimates from a linear probability model where the dependent variable takes a value of 1 if the individual was an entrepreneur in a given year and not an entrepreneur in the prior year. The main RHS variable of interest is the bucket of equity to value in 1991 and the buckets interacted with an indicator for the post mortgage reform period. Columns (1-4) delimits the outcome variable to entries that survived at least 3 years after entry (≥ 3 years), or less than 3 years after entry (< 3 years). Columns (5-8) delimit the entry variable to entries that occurred in the the same industry as the individual was occupied in prior to entry (Exp) or entries occurring in another industry than the individual was previously occupied in (No Exp). All columns include year fixed effects interacted with fixed effects for birth-cohort, educational level, partner, gender and having children, all measured in 1991. All columns also include municipality-year fixed effects, industry-year fixed effects and individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses. *, **, *** indicate statistically different from zero at 5%, 1% and 0.1% level.

Dependent Variable: Dummy for entering as an active employer								
	Survival				Prior experience in entering industry			
	≥ 3 years	< 3 years	≥ 3 years	< 3 years	Exp	No Exp	Exp	No Exp
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*I(ETV91>0.25)	0.00017	0.00047 ***			-0.00001	0.00062 ***		
	(0.00014)	(0.00013)			(0.00014)	(0.00014)		
I(ETV91>0.25)								
Post*ETV91(.25-.50]			-0.00010	0.00046 **			0.00009	0.00024
			(0.00018)	(0.00016)			(0.00017)	(0.00016)
Post*ETV91(.50-.75]			0.00023	0.00028			-0.00016	0.00062 ***
			(0.00019)	(0.00018)			(0.00019)	(0.00018)
Post*ETV91(.75-1.0]			0.00059 **	0.00071 ***			-0.00004	0.00131 ***
			(0.00021)	(0.00021)			(0.00021)	(0.00021)
ETV91(.25-.50]								
ETV91(.50-.75]								
ETV91(.75-1.0]								
Covariates-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,708,892	2,708,892	2,708,892	2,708,892	2,708,892	2,708,892	2,708,892	2,708,892

Table 12: Performance of Entrants

Table 12 shows results from a repeated cross-section regression where the dependent variable is the gross profit (sales less purchases) [Panel A], sales [Panel B] and employment [Panel C] for the first three years for each entry-cohort. The key RHS variable is the bucket of equity to value >0.25 in 1991 and the bucket interacted with an indicator for the post mortgage reform period. In each year the dependent variable is censored at the 1st and 99th percentile. Column (1-7) include year fixed effects, and individual controls measured in 1991 (birth-cohort, gender, educational level, partner and kids dummy). Columns (2), (4) and (5-7) include municipality fixed effects. For OLS regressions standard errors are clustered at the individual level and are reported in parentheses. For Quantile regressions standard errors are bootstrapped with 100 replications. +, *, **, *** indicate statistically different from zero at 10%, 5%, 1% and 0.1% level.

Dependent Variable: Cumulative outcome in first three years after entry														
	OLS								Quantile regression					
	All entries				Conditional on survival				P25		P50		P75	
	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
Panel A: Value Add														
Post*I(ETV91>0.25)	-198	*	-187	*	-209		-217		-94		-205	**	-263	*
	(90)		(93)		(150)		(161)		(59)		(70)		(106)	
I(ETV91>0.25)	-6		-9		-6		3		59		88		39	
	(77)		(81)		(126)		(139)		(51)		(61)		(95)	
Panel B: Sales														
Post*I(ETV91>0.25)	-304		-228		-45		255		-130		-365	+	-587	+
	(331)		(337)		(594)		(620)		(139)		(188)		(308)	
I(ETV91>0.25)	-107		-193		-493		-663		115		264		198	
	(296)		(301)		(533)		(558)		(115)		(182)		(268)	
Panel C: Employment														
Post*I(ETV91>0.25)	-1.19	*	-1.07	*	-0.97		-0.50		0.00		-0.81	**	-1.30	+
	(0.51)		(0.52)		(0.89)		(0.96)		(0.12)		(0.31)		(0.67)	
I(ETV91>0.25)	0.01		-0.09		-0.75		-0.98		0.00		0.33		-0.05	
	(0.44)		(0.45)		(0.77)		(0.83)		(0.07)		(0.24)		(0.54)	
Individual controls	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Municipality fixed effects	No		Yes		No		Yes		Yes		Yes		Yes	
Observations	7,089		7,089		3,489		3,489		7,089		7,089		7,089	

Figure 1: Average value of housing equity unlocked by the reform

Figure 1 shows the mean, median, 75th percentile and 25th percentile value, in thousands of Danish Kroner, of housing equity that was unlocked by the reform, for individuals with different levels of equity-to-value in 1991, ranked from the 1st to the 99th percentile in ETV. The released equity is calculated as value of the house in 1991, multiplied by the difference between the equity-to-value in the house in 1991 and the 80% threshold that individuals were allowed to borrow up to.

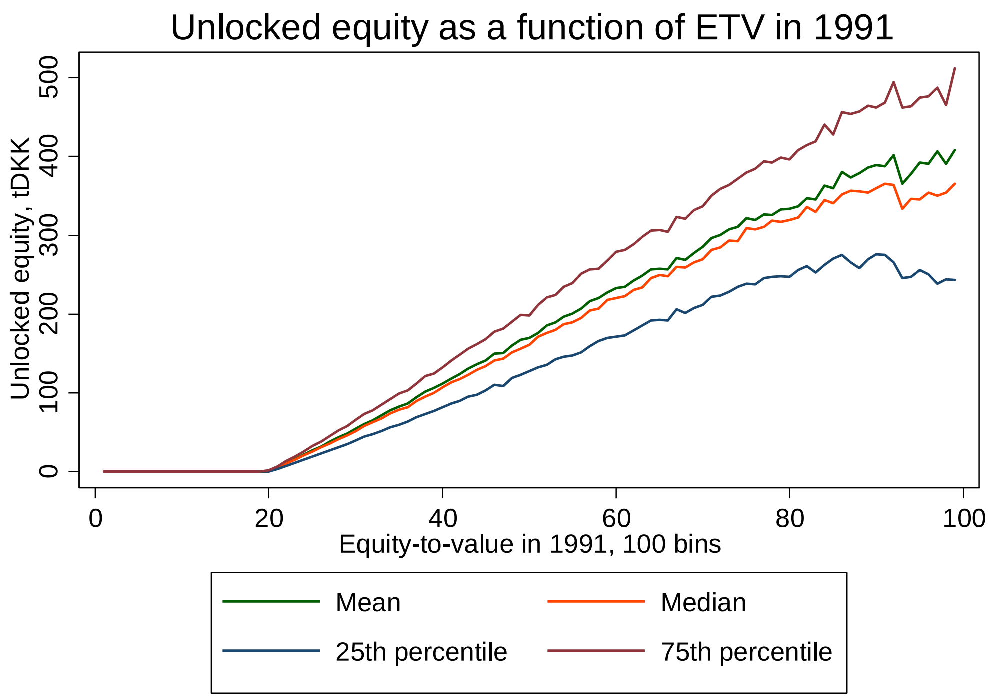


Figure 2: Unlocked equity in 1991 as a percentage of annual disposable income, conditional on equity-to-value being greater than 0.2

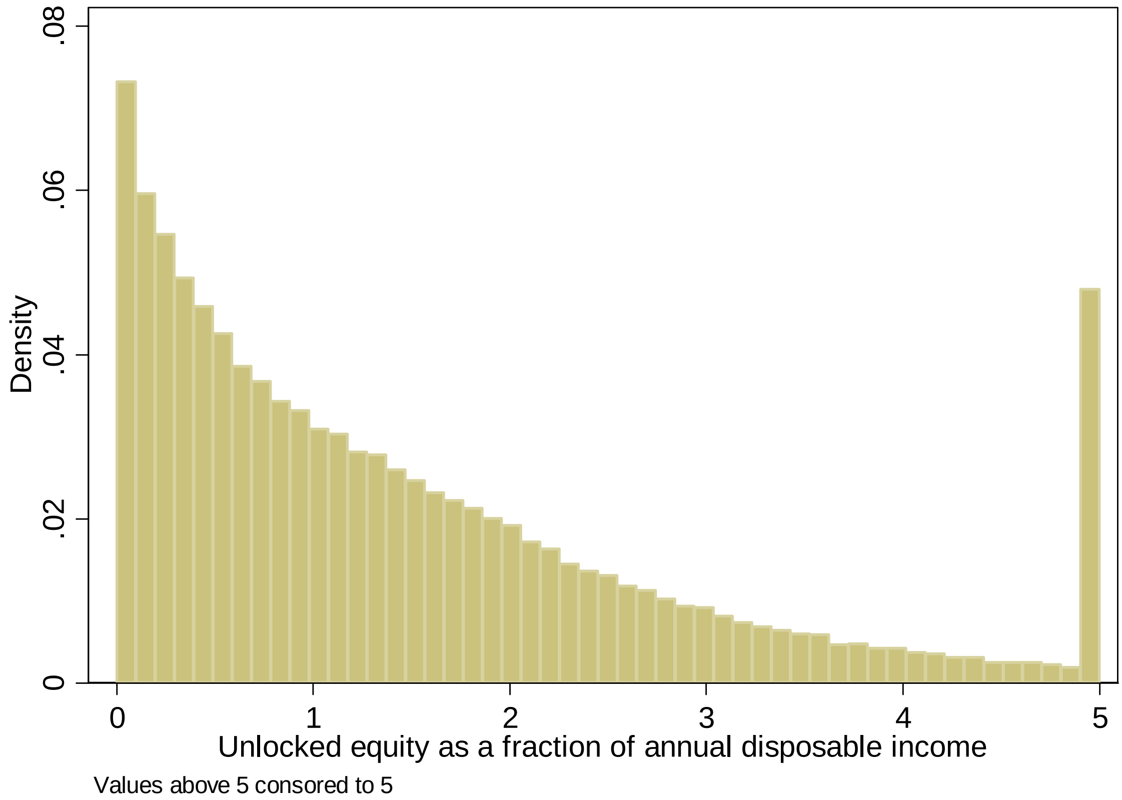


Figure 3: Effect of the reform on net entrepreneurship.

Figure 3 shows a dynamic version of model (3) in table 5, where an indicator of being an individual who was treated by the reform is interacted with year dummies and shown relative to 1992. The model includes the full set of covariate-year fixed effects as well as individual fixed effects and standard errors are clustered at the individual level.

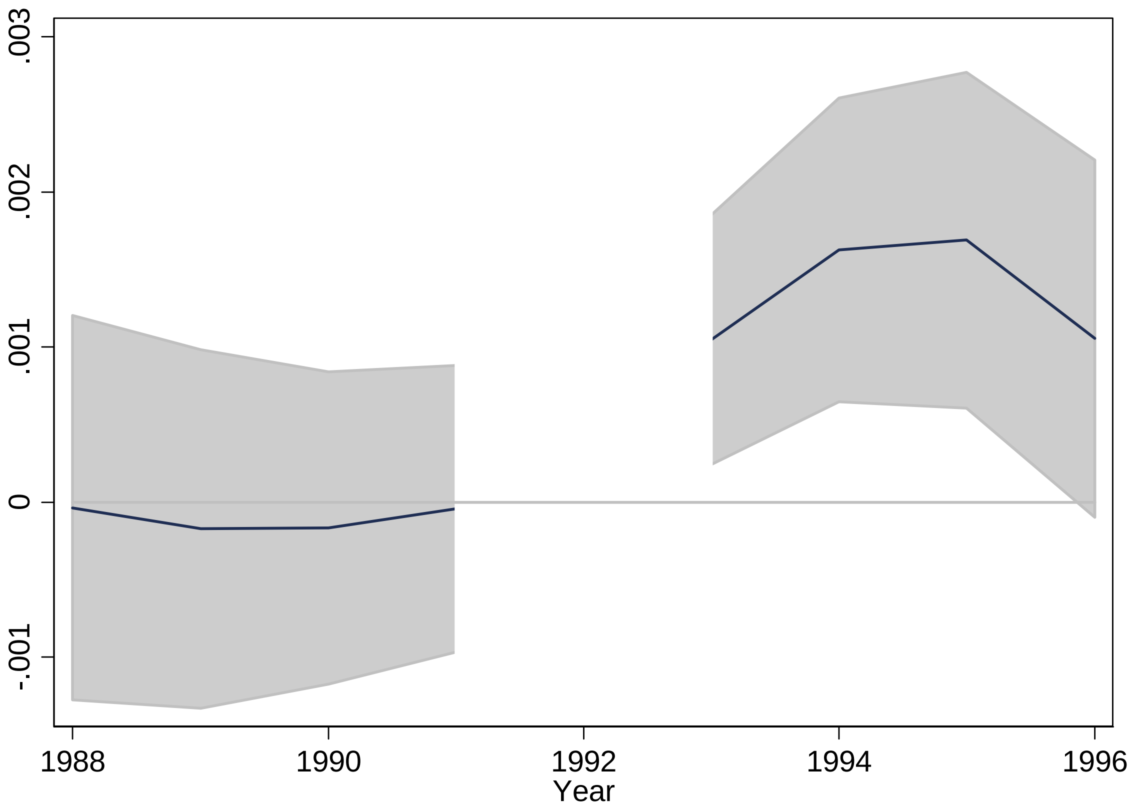
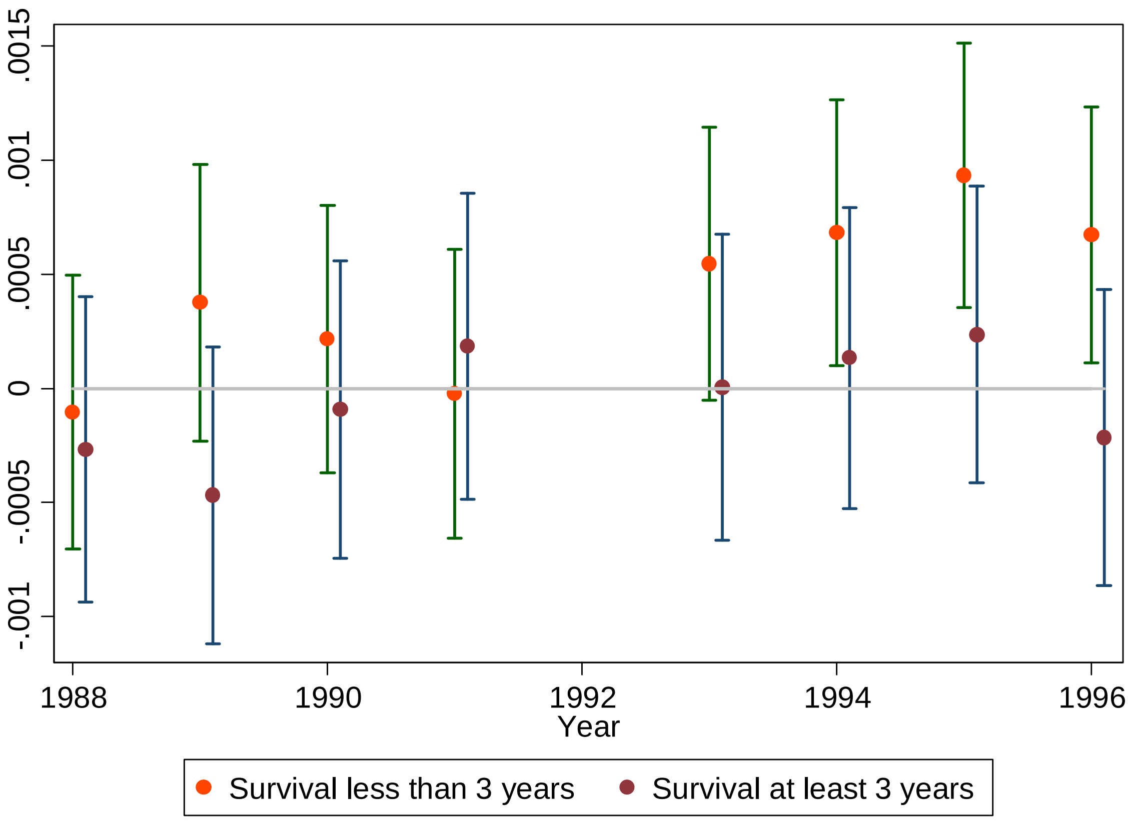


Figure 4: Effect of the reform on long-term and churning entry.

Figure 4 shows a dynamic version of models (1) and (2) in table 11, where an indicator of being an individual who was treated by the reform is interacted with year dummies and shown relative to 1992. As with Table 11, they show that churning entry increased substantially after the reform relative to the control group, while longer-term entry did not change on a relative basis. The model includes the full set of covariate-year fixed effects as well as individual fixed effects and standard errors are clustered at the individual level.



Appendix A: Capital Intensity measures

Our measure of capital intensity is constructed from the reliance of external finance of firm starts in the pre-reform period. With 111-industry classifications, we take all entries occurring in the period from 1988-1991 into a given industry and take the average change in total interest payment from time *t-1* to time *t* of the entrepreneur starting a firm in a given industry at time t. We next sort these industry averages from high to low and define high capital intensive industries as industries above the median. The median change is 28,000 DKK (approximately 4,700 USD). With a prevailing interest rate of roughly 10% in the period this corresponds to a debt increase of 280,000 DKK (approximately 47,000 USD) for an individual starting a median capital intensive firm.

As validation exercise of our capital intensity measure, table A1 reports the correlation coefficients with other measures of capital intensity, both weighted and un-weighted by the number of entries that occurs in a given industry. First, the measure is robust to measuring interest payments from t-1 to t+1 as opposed to t-1 to t relative to entry. Further, the change interest rate payments associated with entry in a given industry is positively correlated with the first year record sales for the same industry. Finally the measure is positively correlated with the mean and median amount of external financing need reported in the Survey of Small Business (SSB) based on US-data at the 2-digit SIC level.

Table A1: Correlation of capital intensity measures

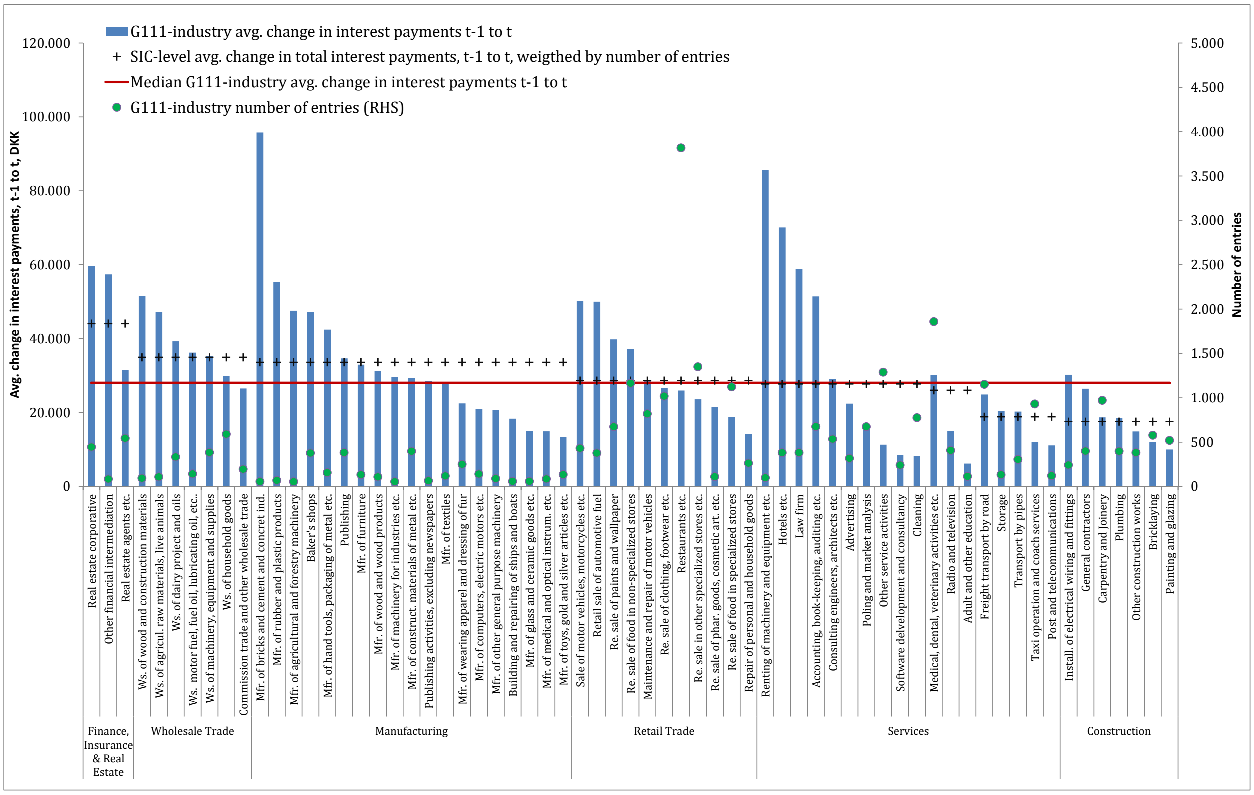
Table A1 reports correlation coefficients between average changes in total interest payments from t-1 to t for an entrepreneur entering a given industry in 1988-1991 with other measures of capital intensity. First year sales are computed based on the firms for which we observe VAT data during its first year of operation. SSB average and median are survey numbers taken Survey of Small Business. * Indicate significance at the 10% level.

	Measure: Avg, ΔInterest payments, *t-1* to *t*			
	Avg. ΔInterest payments, *t-1* to *t+1*	Avg. First year sales	SSB, average	SSB, median
Weighted by #entries in industry	0.91*	0.40*	0.27*	0.26*
Un-weighted	0.86*	0.38*	0.20*	0.16

Figure A1 below shows the distribution of increases in interest payments at business start-ups across selected G111 industries. We define capital-intensive industries as industries that have above median growth in interest payments at the point of the start-up as is indicated by the red line. The figure shows that there is considerable variation within broad industry classes, so that we observe entries that are capital intensive and not within almost all broad industry groups.

Figure A1: Capital intensity by G111-industries

Only industries with more than 50 entries in the period 1988-1991 are shown.



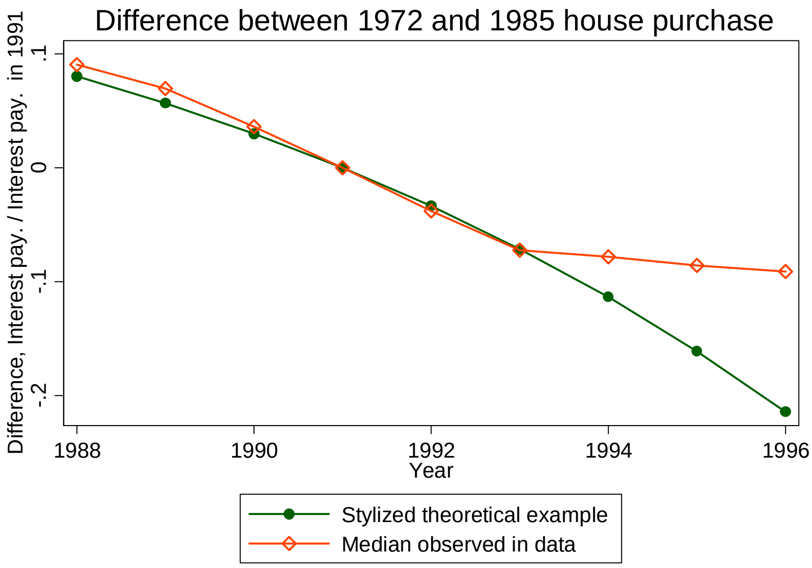
Appendix B: Interest payments and amortization

During the period of analysis, the typical mortgage taken out for the financing of house purchases is a 30 year mortgage bond with fixed yearly instalments. With fixed installments, over time, the proportion of the installment that goes to accruing interest payments will fall and, conversely, principal repayment will increase. Given that we study interest payments, this will, absent the 1992-mortage reform, introduce a particular time trend in the interest payments depending on how long the household has had the mortgage, where the rate at which the principal is re-paid increases with the time the mortgage has been held.

To illustrate this point, we compare a stylized theoretical example of two identical households that buys a house in 1972 and 1985 via, respectively, a 10% and 12% fixed rate mortgage (the prevailing interest rate at the time of purchase). In our data we locate their counterparts and compare median value of the interest payments relative to the 1991 level. Figure B1 below plots the difference between the relative amount of interest payments for the stylized example and the sample analog. We note that the post reform period is confounded by the ability to extract equity, and hence the divergence post the reform between the data and the stylized example should be attributed to the reform. This difference is consistent with the Figure B2 which shows a dynamic specification of model (3) in table 4 where an analogous pre-trend is shown. Absent the reform we would have expected the relative difference to have continued along the same trajectory as in the stylized theoretical example.

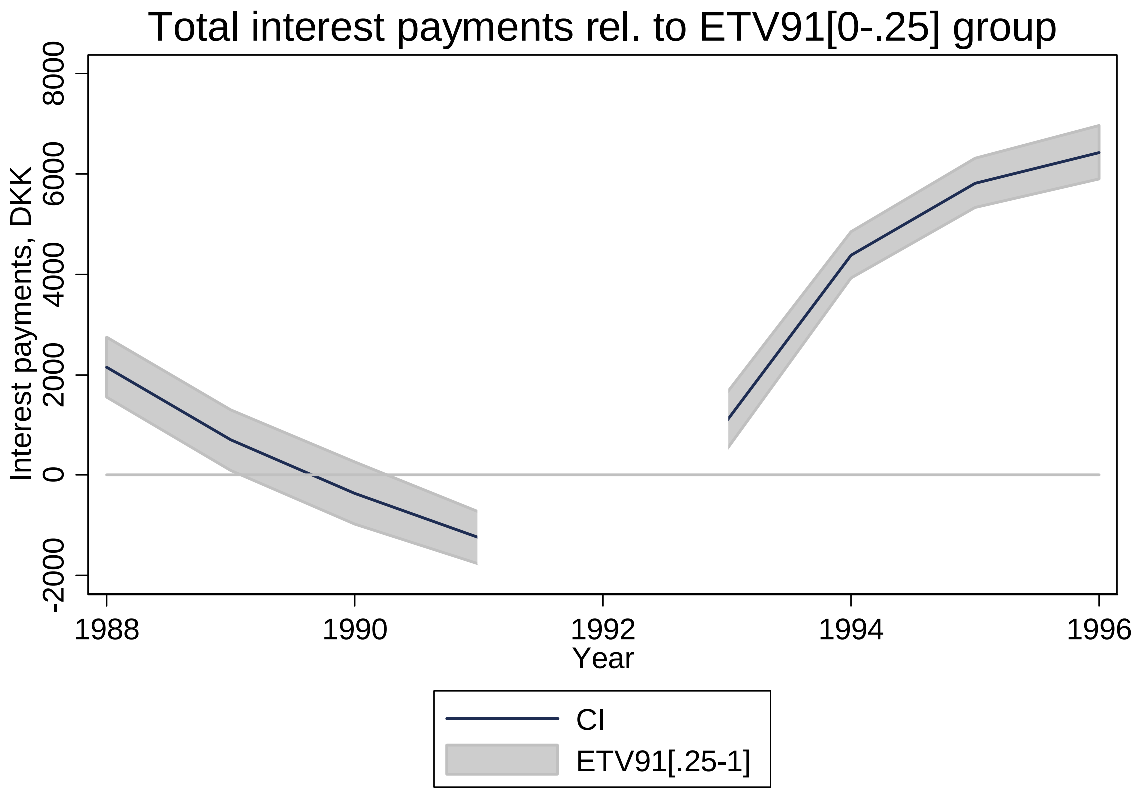
$$Difference_t^{1972-1985} = \frac{Interest_t^{HP=1972}}{Interest_{1991}^{HP=1972}} - \frac{Interest_t^{HP=1985}}{Interest_{1991}^{HP=1985}}$$

Figure B1: Difference in (median) interest payments relative to 1991, by year of purchase



Appendix Figure B2: Total interest payments by equity to value in 1991

Appendix Figure B2 shows a dynamic version of model (3) in table 4, where an indicator of being an individual who was treated by the reform is interacted with year dummies and shown relative to 1992. The model includes the full set of covariate-year fixed effects as well as individual fixed effects and standard errors are clustered at the individual level. The trend observed in the pre-period is due to the mechanical nature of payments for those with more vs. less mortgage outstanding (see appendix B above for details).



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Chapter 2

The Real Effects of Higher Capital Requirements to Banks

The Real Effects of Higher Capital Requirements: Evidence from Danish Firm-level Data

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Abstract

This paper considers how increases in individual banks' capital requirements affect borrowing and growth at the firm-level. Using a novel data set of regulatory injunctions to Danish banks' individual capital requirements, I find evidence that an increase to the minimum capital requirement of a firm's primary bank is associated with 3 percent less borrowing, relative to firms not facing increased capital requirements to their primary bank. While firm borrowing is sensitive to capital requirements of their primary bank, I find, on average, no material effect on firm's assets growth as firms are able to substitute towards equity financing instead of reducing their balance sheets. Investigating the heterogeneous effects, however, I find that young firms with negative earnings are particular sensitive to capital requirements of their primary bank and are led to reduce assets growth.

Keywords: Financial Regulation, Capital Requirements, Credit Supply

JEL-Codes: G21, G28, G32

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1 Introduction

The ramifications of an unstable financial system have been felt deeply during the recent financial crisis and have accentuated the role of capital requirements to banks to ensure continued economic and financial stability. While increased capital requirements to hold more equity is likely to increase resilience in the financial sector, there exists limited evidence on the adverse transmission mechanisms through which higher capital requirements will affect the real economy.

Existing work have already documented how adverse changes in the supply of loanable funds propagate to the firm sector. Slovin et al. (1993) showed that the distress of a large U.S. bank, which inhibited its ability to supply credit, was associated with material losses in the market value of firm-clients. Subsequently other studies have also documented that bank-specific shocks affect the real economy and cause firms to reduce total borrowing (Khwaja & Mian, 2008; Lemmon & Roberts, 2010), borrow at higher interest rates (Santos, 2010), shrink exports, (Amiti & Weinstein, 2011), slash investments (Paravisini, 2008; Klein et al., 2002) and reduce employment (Chodorow-Reich, 2014).

Whether higher capital requirements to banks constitute a shock to the banks' funding structure and their cost of supplying credit is, however, subject to continuing debate. From a theoretical perspective, proponents of increased capital requirements, with reference to the seminal article of Modigliani & Miller (1958), argue that higher capital requirements will make banks safer and hence reduce the cost of bank's equity financing leaving the supply of credit largely unaltered (Amiti & Weinstein, 2011; Kashyap et al., 2010). On the contrary, increased capital requirements can be rationalized to influence banks' choice to supply credit due to information asymmetries between lender and borrower (Stiglitz & Weiss, 1981; Thakor, 1996; Sharpe, 1990; Agur, 2013) and the costs of issuing equity (Myers & Majluf, 1984). In an equilibrium setting, additional equity in the capital structure of banks may also be conjectured to increase banks cost of funding due to liquidity considerations (Brunnermeier & Pedersen, 2009; DeAngelo & Stulz, 2013), tax-advantages of debt and implicit government guarantees.

The difficulties, however, in determining the causal impact on firms of higher capital requirements revolves around simultaneously distinguishing between the supply and demand for credit. On lending data aggregated at the bank-level, Peek & Rosengren (1997) have used the Japanese stock market crash as a possible source of exogenous variation in the supply of credit to show how U.S.-subsidiaries contracted lending in response to Japanese parent bank breaching the BASEL I 8% minimum capital requirement. Other studies using aggregate bank-level data have tended to also find that cross-sectional variation in banks' capitalization comove with the aggregate lending (Rime, 2001; Berrospide & Edge, 2010; Francis & Osborne, 2012; Osborne et al., 2012).

The drawback, however, of using aggregate lending data, is that the aggregation implicitly disregards differences in banks' loan portfolio and in particular the characteristics of the individual borrowing firms. An analysis carried out on aggregate lending data will be susceptible to the criticism that estimated results can be partially driven by unobserved differences in banks' composition of loans. Also, without directly observing the individual bank-firm relationship, it is not feasible to determine whether the firms are able to mitigate the effect of higher capital requirements by substituting towards other banks or other sources of financing,

To overcome the shortcomings associated with aggregated lending data, the identification strategy of this paper deploys a firm-level difference-in-difference methodology for estimating the effect of higher capital requirements controlling for differences in characteristics of bank-clients while also allowing for extensive margin responses of firms switching lender. The variation in capital requirements used in this study is obtained from bank examination carried out by Danish Financial Supervisory Authority's (FSA) during the period of 2010-2011, where a subset of the bank examinations were accompanied by an injunction for the individual bank to increase its minimum capital requirement.

Specifically, to separate a firm's credit demand from credit supply, I construct groups of firms whose banks were respectively affected and unaffected by injunctions to increases in capital requirements during 2010-2011. I use these capital requirement injunctions to study how changes in minimum capital requirement of banks affect debt-taking and assets growth during 2009-2011, at the firm-level, while controlling for the characteristics of the firm, its industry and its banks.

One concern regarding the use of injunctions is that these are not randomly assigned to banks, but varies systematically with the characteristics of the borrowing firms at the individual bank. In addition to introducing an extensive set of firm and bank controls, two additional robustness checks help alleviate this concern. First, if banks receiving injunctions correspond to the banks that faired worse during the financial crisis, we should expect that the change in debt from the onset of the crisis to 2009 to also be negative relative to the control group. Using the change in debt from 2007-2009 as a placebo outcome, I find no differences, suggesting that the estimated effect of higher capital requirements is not spuriously related to the banks' exposure to the financial crisis. Second, while greatly reducing the sample size, I compare firms only within the group of banks that receive an injunction, utilizing the differential timing of the injunctions to show that the results are robust to this within-bank type comparison.

In terms of results, I first demonstrate that firms' borrowing decrease by approximately 3 percent from 2009-2011 if its primary bank saw an injunction to increase its capital requirements relative to the control group. This however, does not lead to material impact of assets growth as the point estimate suggests that firms on average only reduce assets by 0.2 percent. I substantiate this finding by showing that firms substitute towards equity financing to offset the lower borrowing. While no economically meaningful effect is found on average, I find heterogeneous effects that vary by firm age and earnings, where young firms with negative earnings are the only group found to have significantly lower asset growth conditional on having a primary bank that receives an injunction to increase its capital requirement.

Withstanding the continuing debate of higher capital requirements to banks, the estimated results are important in the context of empirically informing policy makers on the magnitude of adverse transmission effects of heightening capital requirements when explicitly considering firm outcomes that allows for substitution and focuses the attention to real outcomes. To this end, higher capital requirements are generally found to reduce firm borrowing, but only for younger firms with negative financial results does this translate into lower assets growth.

The remainder of the paper is organized as follows. Section 2 considers the institutional details of relating capital requirements to credit supply. Section 3 describes the data and section 4 discusses the estimation strategy. Section 5 presents the results and section 6 contains several robustness tests. Finally, section 7 concludes.

2 Institutional Details of Linking Capital Requirements to Credit Supply in Denmark

Capital requirements to banks in Denmark are governed through the BASEL accords under the supervision of the Danish FSA. Under the current regulatory setup, a bank is responsible for calculating, according to guidelines set forward by of the FSA, its own minimum capital requirement. For a bank to retain its banking licenses it must maintain an actual capital level above the calculated minimum capital requirement.

A number of studies have considered how changes in capital ratios at the aggregate level predicts subsequent lending growth (Rime, 2001; Berrospide & Edge, 2010; Francis & Osborne, 2012; Osborne et al., 2012). However, as banks endogenously change their capital ratios to reflect the risk in their underlying portfolio, a positive correlation between higher capital ratios and lower subsequent loan growth could also reflect banks realizing that its clients are doing worse than previously anticipated. This would lead to both clients' demand for credit to go down, and also the likelihood of default to increase – where increased likelihood of default should prompt the banks to raise capital cushions leaving a spurious correlation between changes to capital ratios and lending growth.

To overcome the challenge that banks endogenously change their own capital requirements, the variation in capital requirements used for this study comes from bank-specific injunctions made by the Danish FSA to the individual minimum capital requirements of banks.

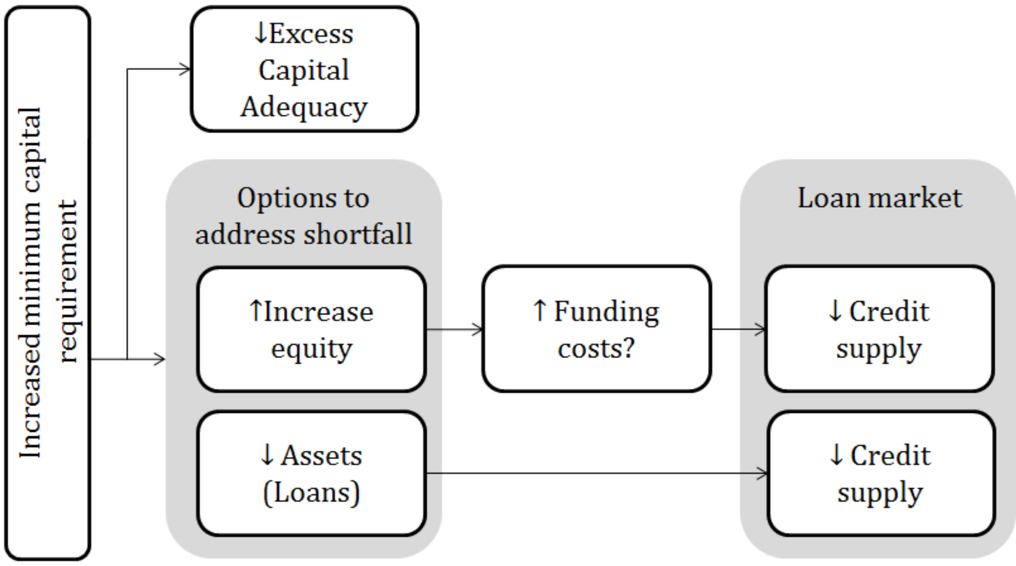
As part of its task in ensuring compliance with banking regulations, the FSA routinely carries out bank examinations of the individual banks, assessing whether the bank's minimum capital requirements are appropriate to cover the banks' risks. If the examinations reveal that the given bank have assessed its own minimum capital requirement to be too low, the FSA authorities have, under pillar 2 in the BASEL II framework[1], discretionary power to raise the individual bank minimum capital requirements through an injunction.

Faced with an injunction to increase minimum capital requirements, the bank has several options to address the heightened minimum capital requirement as outlined in Figure 1. First, if the bank has sufficient capital in excess of the minimum capital requirement, the bank may choose to do nothing at all. This would reduce the bank's difference between its actual capital and minimum capital requirement. To the extent that the bank actively seeks to address its shortfall in its capital adequacy, it may improve its capitalization by issuing equity, or retaining dividends. Alternatively, the bank may also reduce the amount of loans that needs to be capitalized.

[1]The BASEL II accord operates with three overarching pillars: (1) minimum capital requirements, (2) supervisory review and (3) market discipline. Banks are required to have processes for assessing their overall capital adequacy in relation to their risk profiles and a strategy for maintaining their capital levels (BIS, 2006, page 205). Under pillar 2, the supervisors are then in turn obligated to review and evaluate banks' internal capital adequacy assessments and strategies, as well as their ability to monitor and ensure their compliance with regulatory capital ratios. If deemed insufficient, the supervisors should take appropriate supervisory action (BIS, 2006, page 209). These actions include the option of issuing an injunction for the bank to change it methodology for calculating its minimum required capital level.

Figure 1: Transmission of increased capital requirements

Notes: Figure 1 outlines possible options available to a bank that has to address an increase in its minimum capital requirement



As a higher capitalization would reduce the overall risk of the bank, Amiti & Weinstein (2011) and Kashyap et al. (2010) argue that in efficient capital markets, the cost of funds will be largely unaltered and, as a corollary, leave banks' lending supply unaffected. However, if there are frictional costs associated with issuing equity (Myers & Majluf, 1984) or additional equity would increase the banks effective cost of funding due to tax-advantages of debt, implicit government guarantees or liquidity considerations (Brunnermeier & Pedersen, 2009; DeAngelo & Stulz, 2013), this would induce the bank to try transferring the increased cost of holding additional equity onto its clients. As an alternative to improving capitalization through additional equity, the bank may also choose to reduce the amount of loans that needs to be capitalized, which in turn would lead to a contraction in the bank's lending supply.

The bank's specific choice to address the increased minimum capital requirement depends on the relative cost and benefits of addressing the shortfall through each its available options, with the possibility of adjustment along all of the outlined margins. The empirical question that this paper seeks to answer is if, and to what extent, the injunction to increase minimum capital requirements induces the bank to cut its lending supply with possible real effects on the firm's outcomes.

3 Data

3.1 Firm-level data

The data of the individual firms are obtained from the private data provider Experian/KOB, which collects information contained in financial statements of Danish limited liability firms, obtained through public records. The data set contains information on 158,218 firms during the period from 2005-2011, with less than 30 percent of the firms being observed in all years due to firm entry and exit. Focusing on 2009, that data set holds information on 103,319 non-financial limited liability firms, both active and inactive. Purging the data set of observations that are either inactive, do not fulfill balance sheet checks or have negative equity leaves 82,540 firms. I further concentrate on firms that have non-missing industry information to enable the inclusion of industry fixed effects, leaving 72,220 observations. In the main specifications, I also require that I observe the firm from 2009-2011 to study the effect of firm outcomes during this period, further reducing the sample to 63,328 firms.[2]

To facilitate the analysis of investigating how bank capital requirements affect individual firms, I classify firms according to their primary banking relationship in 2010. The primary banking relationship is recorded in the financial statement or surveyed by Experian. The coverage ratio for this variable is 38% and hence I drop firms without a primary bank from the analysis.[3] I further restrict attention to the 96% of firms with non-foreign banks that has a least 10 firms reporting the bank as their primary bank. This final exclusion is due to foreign banks not being under the supervision of the Danish FSA and some very small banks being largely

[2]For the firms with information available on primary bank, I can investigate the sample selection criteria in relation to injunction status of the primary bank by comparing the ratio of number firms included in the sample relative to total number of firms in the raw sample. The fraction included is 0.77 for firms with banks receiving injunctions relative to 0.76 for firms with banks not receiving injunctions, with no significant difference between the two ratios.

[3]To verify that the missing bank information in the Experian database is uncorrelated with injunction status of the bank I compare the number of observations observed in the cleaned data set against the total lending of each bank. The rank-rank correlation is 0.89 between the number of bank-firm observations and bank lending, illustrating a high correspondence between the banks size and the number of associated firm clients it has in the data set. Appendix Figure A.1 further shows the relationship between the number of firms reporting the bank as its primary bank, its size and injunction status, showing no systematic relationship between coverage ratio and injunction status or size of the bank. Appendix Figure A.2 tabulates missing bank information on firm age, size and industry to show that it is predominately small and young firms that do not report bank information. The identification strategy, to be introduced in section 4, requires that the allocation to injunction-banks and control group banks is as good as random after conditioning on observable characteristics. Inference can then in turn only be drawn on the firms that are adequately represented in the sample. This inference limitation is equivalent to e.g. Chodorow-Reich (2014) not being able to draw inference, in the U.S., for small and medium sized enterprises when considering the supply shocks to the syndicated loan market only accessible to large corporations.

inactive in the firm lending segment. When comparing firms with a primary bank that received an injunction in 2010 to firms that had a primary bank in 2010 that did not receive an injunction during the period from 2010-2011, the main data set holds information on 21,254 firms, where 10,859 belong to the group of firms whose primary bank received an injunction during 2010.

In addition to the firm's primary banking relationship and industry, a number of characteristics of the firms are obtained from the financial statements. Unfortunately, under Danish accounting standards, not all firms are obligated to report sales or number of employees, and consequently this information is missing for the majority of the firms in the sample. As a result, I choose to focus on total debt and total asset as the primary outcome variables and include available controls for size, age, profitability, leverage, governance, industry, geographical location and probability of default. Table 1 outlines the measurement of these variables.

[**Table 1: Firm-level variables**]

3.2 Bank-level data

Prior to 2010, the bank examination reports issued by the Danish FSA have been confidential, but subsequent to the financial crisis, in an effort to increase transparency, the Danish parliament decided that result of these examinations should be made publicly available starting from 2010. The outcome of the bank examinations are obtained by manually reading through all bank examination reports published during the period from 2010-2011. I record the timing and classify a binary variable as to whether or not the examination led to a subsequent increase the minimum capital requirement. During the period 2010-2011, the Danish FSA carried out 40 bank examinations of the 123 banks under its supervision. As the analysis focuses on firms, the analyzed sample includes observations on 41 banks where the omitted banks are mainly small savings banks inactive in the firm lending segment.[4] The included banks received 21 examinations during 2010 where 11 of them resulted in an injunction for the bank to increase minimum capital requirements.

I choose to focus on the binary outcome of the injunction increase minimum capital requirements rather than the severity of the injunction, i.e. how much the minimum capital required was raised, for two reasons. First, for two out of the 11 injunctions studied, I cannot adequately determine the severity of the injunction but only infer that it led to an increase in the minimum capital requirement.

[4] The 41 banks represented in sample accounted for 88% of total bank lending in Denmark in 2009.

Secondly, for 91% of the injunction group firms, the injunction of the FSA was associated with a 0.6 percentage point increase in the minimum capital requirement, leaving limited variation in severity of the injunction.

I augment the outcome of the bank examinations with public information from the Danish FSA on bank size and key banks ratios to control for the type of bank that receives an injunction. The measures of bank controls are log of bank total assets, core capital ratio, interest rate risk, liquidity coverage ratio, fraction of large exposures, impairment ration and loan growth. These variables are further explained in table 2. While this list is not exhaustive, the choice of these variables are motivated by the Danish FSA's introduction in 2010 of the so-called 'Supervisory Diamond' outlining five benchmarks to indicate banking activities, which the FSA characterize to be associated with high risk banks.[5] The controls I include below are not identical to the elements of the supervisory diamond, as information on the variables included in the supervisory diamond is not publicly available for 2009. Most noticeably, I lack information on individual banks' exposure commercial property exposure and I instead include impairment ratio as a proxy for commercial property exposure, as this segment witnessed large write-offs in 2009, in the aftermath of the financial crisis.

[Table 2: Bank-level variables]

4 Empirical Strategy

4.1 Identification

In order to grow, a firm uses a combination of equity and debt to fund its investments. In a stylized setting, the investment decision can be characterized as a function of the firm's marginal cost of funds, (expected) demand for a firm's product and a set of control variables, where the firm invests up until marginal cost equals marginal profits. Letting $g_{i,b}$ denote the growth in the outcome variable of interest for firm i at bank b, the relationship can be written as

$$g_{i,b} = I(r_{i,b}, \mathbf{X}_i, \mathbf{U_i}, \epsilon_i) \tag{1}$$

where firm investment, I, is a function of the marginal cost of funds, $r_{i,b}$, an observable vector of covariates $\mathbf{X_i}$, unobserved characteristics $\mathbf{U_i}$and idiosyncratic

[5]The five elements of the supervisory diamond include (1) having excess liquidity coverage above 50 percent, (2) loans-to-deposits under 1 (3) sum of large exposures below 125 percent (4) lending growth under 20 percent year to year and (5) Commercial property exposure less than 20 percent. See Finanstilsynet (2015) for details.

shocks, ϵ_i. Heightened capital requirements are hypothesized to increase the firm's marginal cost of capital, as the bank receiving the injunction may increase interest rates or ration credit in order to reestablish its capital adequacy. To the extent this is the case, this would leave only more expensive sources of financing available to the firm and ultimately prompt the firm to scale back investment.

The identifying assumption is that, conditional on observable characteristics, unobserved characteristics of the firm are uncorrelated with the likelihood of the bank receiving an injunction to increase capital requirements. The Danish FSA states that *"on average a bank examination is carried out every 3-4 years with larger banks and higher risks banks being visited more often"*, implying that while the bank examinations leading to injunctions are exogenous to the individual firm they are, however, not randomly assigned to banks.

A potential concern with the non-randomness of bank examinations relates to the type of firms that have relationships with banks that receives injunctions being different along an unobserved dimension relative to firms that have banking relationships that did not receive an injunction. Using bank and firm-level controls, as opposed to using aggregate loan data, reduces the scope for this to be an invalidating concern. Reassuringly, the firms also appear balanced along observable characteristics. To further examine the validity of the assumption, I show that the results are robust to a within injunction group comparison, utilizing only the differential timing of injunctions to identify the effect of heightened capital requirements. This specification explicitly address the potential concern that banks receiving injunctions not being comparable to banks that did not receive an injunction, as it only allows for the timing of the injunctions to be different within the group of banks that receive injunctions.

A second concern is that banks may anticipate the visit from the financial authorities. This will likely to lead to an underestimation of the effect of increased capital requirements as a bank anticipating an injunction is likely to actively seek to adjust beforehand, in an attempt to avoid reputational consequences of receiving an injunction. With a preemptive action of the bank, part of the response to increased capital requirements will fall already in the pre-injunction period being used for comparison. Therefore, the full effect of the increased minimum capital requirements would be understated relative to the scenario where the bank had not anticipated the inspection.

Moreover, it is important to point out that the identified effect will relate to one bank receiving an injunction to increase its capital requirements as opposed to an effect of a policy maker who increase capital requirements for all banks simultaneously. While this study allows for the firm to adjust along the extensive margin by substituting towards unconstrained lenders (since I study total borrowing at the firm-level), the injunctions to individual banks still leaves the rest of the banking sector unaffected. This would leave the banks not receiving an injunction in a better position to compensate the reduction in credit by the (relatively few) banks receiving injunctions, relative to the scenario where all banks are simultaneously required to increase the minimum capital requirement. To this end, the results obtained in this paper can be thought of as a lower bound estimate of the total effect of changing capital requirements to the entire banking sector.

4.2 Estimation

In the estimation, I classify a firm whose primary bank received an injunction to increase their minimum capital requirement in 2010 as an exposed firm. In my baseline results, I compare the exposed firms with a control group of firms that had a primary bank that did not receive an injunction in 2010 or 2011. As a first step, Figure 2 shows the difference in average debt-to-assets ratios among these two groups of firms which are observed in the data in all years. Reassuringly, the two groups have similar trends in their debt-to-asset ratios prior to 2010 with the exposed firms starting to rely less on debt concurrently with their primary banks receiving injunctions in 2010.[6]

Introducing the requirement that firms should be observed in the entire period leads me to drop a large number of particularly smaller firms, which may potentially be the firms most susceptible to credit supply shocks (Petersen & Rajan, 1994; Berger & Udell, 1995; Cotugno et al., 2013). To avoid this selection, I instead chose to focus the main part of the analysis on changes in outcomes over the period 2009-2011, including also young firms that entered the sample during 2005-2009.[7]

[6] Appendix Figure A.3 shows a figure equivalent to figure 2, where the number of firms is allowed to vary over time, as firms enters and exits the sample.

[7] In the robustness section, I verify that outcomes are similar in pre-injunction period for the firms that have available information.

Figure 2: Average leverage ratios of firms by primary bank status

Notes: Figure 2 shows the average debt-to-asset ratio for firms in the analyzed sample based on the injunction status of their primary bank in 2010. The "Injunction"group are firm's whose primary bank received an injunction during 2010. The "No Injunction" group refers to firms whose primary bank did not receive an injunction in 2010 or 2011. Only firms that are observed in all years are included.

Specifically, using the cross-sectional variation, the main specification estimates the following relationship between changes in log total debt from 2009-2011 regressed on injunction status of primary bank along with firm and bank level controls:

$$\Delta Ln(Debt_i)^{09-11} = \delta \cdot INJUNCTION^{10}_{i,b} + \gamma \mathbf{B}^{09}_b + \beta \mathbf{X}^{09}_i + u_{ib} \qquad (2)$$

where subscripts refer to firm i, with a primary banking relationship in 2010 at bank b. The coefficient of interest is δ, which is the coefficient pertaining to the indicator variable taking the value of 1 if the firm's primary bank received an injunction to increase capital requirements in 2010. If firms' borrowing are unaffected by higher capital requirements of their primary bank, we would expect $\delta = 0$ conditional on firm and bank observables. Since the injunction occurs at the bank-level, changes in firm-outcomes across banks may be correlated. Therefore, all regressions cluster errors at the bank level.

The explanatory variables include both bank-controls, $\mathbf{B^{09}_b}$, along with firm-level controls included in the vector $\mathbf{X_i^{09}}$. All of these control variables are measured in 2009, prior to the injunction occurring. The bank controls are as discussed in section 3.1: log of bank total assets, core capital ratio, interest rate risk, liquidity coverage ratio, fraction of large exposures, impairment ration and loan growth in the control set. The firm-level controls include log of total assets, log of firm age, return on assets, debt to assets, indicator for legal form of the firm, indicator for critical comment from auditor, an estimated default probability along with region and industry fixed effects. The outcome variable and explanatory variables are winsorized at the 1st and 99th percentile to the limit the influence of outliers. In the preferred specification, I further include a full set of industry-by-firm-age fixed effects interacted with quartile debt-to-assets, to verify that the obtained results are not driven by unbalances in the distribution across industry, firm-age or initial reliance on debt.

5 Results

5.1 Descriptive Statistics

Table 1 below presents summary statistics for firms, measured ex-ante in 2009, based on the injunction status of their primary bank in 2010.

[**Table 3: Summary Statistics**]

They highlight that firms in the injunction and control group are generally of the same size and age and have similar leverage-structure, profitability and default risk. The only significant difference between the two groups is that injunction banks appear less exposed to the real estate sector while having relatively more firms in the construction sector, motivating the inclusion of industry-fixed effects in the regression setup.

5.2 Loan Outcomes

Table 4 presents the result of regressing $\Delta Ln(DEBT_i)^{09-11}$ on the injunction status of the primary bank. Model (1) introduces no controls other than the injunction status of the firm's primary bank, and finds a negative point estimate of -0.0127 interpreted as firms with a primary bank that receive an injunction reduce total borrowing by 1.3 percent relative to firms that have primary banks that do not receive injunctions, unconditional on any observerable characteristics. Adding bank level controls in model (2) the point estimate becomes more negative, to show that the effect amplifies when controlling for bank risk. However, sequentially adding firms-level controls in model (3)-(5) have no altering effect on the point estimate, suggesting that differences in the bank's loan portfolio are unlikely to be salient in explaining loan outcomes of banks facing higher capital requirements. In the preferred specification (5), I include both bank and firm controls along with regional fixed effects and I further include a full set of fixed effects for quartiles of the firms' leverage ratio interacted with industry by age fixed effects to account for the possibility of differential use of leverage through the firm life cycle in separate industries. The point estimate of model (5) is -0.0301, with a p-value of 0.05, which is interpreted as a 3 percent lower debt taking among firms with a primary bank facing an injunction to increase capital requirements.

To shed light on whether the transmission of higher capital requirements are driven through banks setting higher interest rates for borrowing firms, model (6) and (7) has financial expenditure to debt as the outcome variable, measuring an average cost of debt for the firm. While the results of these regressions are ultimately inconclusive, as firms facing the highest borrowing cost may cease to borrow, the insignificant point estimate suggests that the transmission of higher capital requirements are primarily driven through adjustments in quantities rather than prices of credit. This reconciles with the speed of the adjustment of the bank facing a capital shortfall. Reducing outstanding credit directly curbs the capital shortfall whereas increasing interest rates only over time while increase profitability and allow the bank to retain earnings to bolster capital reserves.

[**Table 4: Loan Outcomes**]

5.3 Real Outcomes

Including bank, firm and regional controls along with leverage quartile interacted with industry by age fixed effects; model (1) of Table 5 shows the effect on real outcomes measured by $\Delta Ln(ASSETS)^{09-11}$. While I found a significant 3 percent reduction in debt taking, the point estimate of the effect on assets is close to zero, with -0.2 percent lower asset growth of firms with a bank facing higher capital requirements. The point estimate is precisely estimated and I can reject, at the 95% confidence-level, that effect on assets exceeds 1.6 percent point, demonstrating that the 3 percent lower borrowing does not map fully into lower investment measured by total assets. The finding of no material impact on asset growth also implies that in order to sustain the same asset growth, while relying less on borrowing, the share of equity financing must increase. To verify this, model (2) regress the change in equity-to-asset ratio from 2009-2011 on injunction status of the primary bank to show that firms offset the lower debt borrowing, by using more equity financing instead.[8]

Another margin of adjustment of the firm facing a contraction in the supply of credit from its primary banking relationship may be to switch bank. For the firms that also report their primary bank in 2011, model (3) reports the propensity for the firm to switch its primary bank in 2011 relative to the 2010 bank. The positive estimates on injunction status of the primary bank in 2010, suggests that firms of banks facing injunctions are 1.5 percentage points more likely to switch lender, although insignificant. This point estimate on the switching propensity should be seen in relation to the 1.8 percent switching probability of firms in the control group, corresponding to an 83% increase in the relative propensity to switch bank.

Model (4) and (5) focus attention to the extensive margin result of firm exit. In model (4) I additionally include the 985 firms that exit in 2011, and I code an indicator variable as 1 if the firm exit and zero otherwise. The point estimate on injunction status of the bank is positive although insignificant. In model (5) the intensive and extensive margin response is modeled jointly by setting firms that disappears out of the sample, to have zero total debt in 2011, to allow for the possibility that these firms exit, due to the inability to obtain funding. Reflecting the result of the model (4), the point estimate increases to approximately 4.3 percent less borrowing relative to the 3 percent estimate found in Table 4, when focusing on the intensive margin only. With the larger variability in the outcome variable due to firm exit, the point estimate, however, also remains insignificant. While this is suggestive of extensive margin result, the remainder of the paper focuses its attention to results obtained on the intensive margin.

[**Table 5: Real outcomes**]

5.4 Heterogeneous effects

While no effects are found for total assets on average, the possibility that some firms are more severely affected by changes in capital requirements should not be dismissed. In particular, as firms are found, on average, to substitute toward equity financing, firms that have negative earnings are unable to substitute toward equity by retaining earnings and may therefore be more vulnerable to higher capital requirements. Moreover, in line Petersen & Rajan (1994), Berger & Udell (1995) and Cotugno et al. (2013), young firms have been shown to be more reliant on

[8] The average equity-to-asset ratio in the sample is 0.39, and with a point estimate of the change in equity-to-assets ratio of 0.0134, this would imply that for a firm with a average level of assets, the amount of equity increases by 0.0134/0.39=3.4 percent.

their banking relationship to overcome borrow-lender informational asymmetries, and with a credit supply shock stemming from their primary bank, these younger firms may also find it more difficult to obtain alternative financing.

To test whether this is the case, Table 6 separates the effect on borrowing by an indicator for negative earnings of the firm (model 1), age of the firm (model 2) and negative earnings and age (model 3). The results of model (1) show a significant 5 percent reduction in borrowing for firms with negative earnings, with only a 2 percent reduction for firms with positive earnings. The difference between the point estimate of firms with negative and positive earnings is significant at the 10 percent level. Model (2) further shows that firms with less than median age, corresponding to less than 10 years in the analyzed sample, see a significantly larger percentage point reduction in the use of debt consistent with a higher dependence the individual banking relationship. Testing the joint effect of earnings status of the firm and age, I find, that although all point estimates are negative, the effect of higher capital requirements on borrowing are primarily borne by young firms with negative earnings which reduce borrowing by 8 percent relative to the control group. Repeating this analysis with asset growth as the dependent variable, models (4)-(6) show that, while I find significant reductions in debt taking across groups, it is only for firms which are less than 10 years and also have negative earnings that it translates into a significant reduction in assets of 3 percent relative to the control group. While this is a tangible heterogeneous effect, this group of firms constitutes only 14 percent of the sample and 11 percent of total assets, reconciling the result of limited effect of higher capital requirements on asset growth, found on average.[9]

[Table 6: Heterogeneous effects]

6 Robustness

This section presents two robustness checks to further alleviate the concern of injunction status of the bank varies systematically with unobserved characteristics of the bank or firm.

[9] Appendix Figure A.3 shows that the heterogeneous results presented above are not driven by firms in one particular industry by further interacting injunction status of the primary bank with age-by-negative earnings of the firm and its industry.

6.1 2007-2009 as a placebo outcome

A number of studies have used banks' exposure to the financial crisis as a measure of credit supply shock (see e.g. Ivashina & Scharfstein, 2010; Chodorow-Reich, 2014; Jensen & Johannesen, 2015). To the extent that the FSA targets high risk banks that coincidentally also where more exposed to financial crisis due to e.g. poor liquidity management, a significant negative effect on injunction status of the bank may spuriously pick up the bank's exposure to the financial crisis if its credit supply remains impaired during 2009-2011. If this is the case we should expect that the credit supply of the banks facing injunction to already constrain lending from the onset of the financial crisis. Using the change in log debt from 2007-2009 as a placebo outcome, Table 7 shows no significant differences, suggesting that the estimated effect of injunctions in Table 3 is not related to the banks' exposure to the financial crisis, but rather coincides with the timing of the heightened capital requirements in 2010, also evident in Figure 2.

[**Table 7: Placebo outcome**]

6.2 Within-injunction bank comparison

A second concern relates to potential non-random matching of firms and banks, where firms that have a bank receiving an injunction are different along an unobserved dimension, and that these unobserved characteristics correlate with a contraction in the firms' demand for borrowing during 2009-2011. To address the possibility that firms that have the type of bank that are prone to receiving injunctions face unobserved demand chocks, I consider an alternative control group of firms that also received an injunction, but not until 2011. While greatly reducing the sample size, the idea is that banks receiving injunctions towards the end of the period should have had less opportunity to adjust, leaving their associated firms relatively unaffected when observing their outcomes in 2011. With this control group, I thereby utilize only the differential timing of the injunctions to show that the firms with the bank receiving an injunction in 2010 remain more severely affected relative to the group of firms with a primary bank that do not receive an injunction until 2011. The results of this estimation, presented in Table 8 model (1), show that the point estimates of the change in debt remain at 3 percent, although now not significant, arguably due to the smaller sample size. Furthermore, the heterogeneous split by age and earnings status in model (2)-(4) shows that the point estimate remains negative in all specifications and that firms with negative earnings remain more severely affected by heightened capital requirements while I cannot statistically differentiate the effect of higher capital requirements on young and old firms in this reduced sample.

[**Table 8: Within-injunction bank comparison**]

7 Conclusion

The paper considered how changes in banks' minimum capital requirements affect the lending behavior towards individual firms at the firm-level. The results show that an injunction to increase minimum capital requirements leads to approximately 3 percent less borrowing in the period 2009-2011 for firms associated with banks facing higher capital requirements. While I find firm borrowing to decline, this has no material effect on asset growth on average, as firms are generally able to mitigate the effects of increased capital requirements to their primary bank by substituting towards equity financing instead and switch banks. While no material effect is found on average, I find that effect of higher capital requirements does depend on the firm's cash-flow position and age, where the subset of firms that are less than 10 years and have negative earnings are unable to substitute to equity financing and ultimately curtail borrowing by 8 percent and asset by 3 percent relative to the comparison group.

Through two additional robustness tests, I further validate that the estimated effects are not driven by the banks' exposure to the financial crisis and are robust to only considering variation in the timing of the injunction to address the possibility of non-random matching of firms and banks.

From a policy maker's perspective the estimated results are important in the context of assessing the magnitude of adverse transmission effects of heightening capital requirements when explicitly considering firm outcomes that allows for substitution. While limited effects appear on average, a subset of young firms with negative earnings appear particularly vulnerable to heightened capital requirements and this should be taken into account when setting optimal capital requirements for banks.

Tables

Table 1: Firm-level variables

Notes: The table outlines the measurement of firm control variables included in the regressions. The default probability is obtained from the National Bank of Denmark and is a Probit estimated probability of default based on observable characteristics of the firm. All other variables are obtained the financial statements of firms obtained from Experian/KOB.

Size Log of total assets

Age Log firm age and age fixed effects in the preferred specification.

Profitability Return on assets

Leverage Debt to total assets

Governance Two indicator variables are included to account for firm governance: (1) an indicator variable taking the value 1 if the firm has received a critical comment from the auditor on its financial statements and (2) an indicator variable taking the value 1 if the the firm has the legal form of "A/S". In Denmark, firms can incorporate as two forms of limited liability companies. They can either choose to incorporate as an 'Anparts Selskab (APS) or 'Aktie Selskab' (A/S). A/S-firms face higher requirements for start-up capital in addition to being required to constitute an executive board and report financial statements in more detail.

Industry Fixed effects for the firm's primary industry based on Danish DB07 codes.

Location Regional effects are included for each of the 5 major regions in Denmark.

Default Banks rationally charge higher interest to firms with higher probability of not repaying the loan. The National Bank of Denmark estimates probit model of default probability for each firm observed in the sample and I include this estimated default probability to proxy for risk (See Nationalbanken (2013) for details).

Table 2: Bank-level variables

Notes: The table outlines the measurement of bank control variables included in the regressions. The data is based on public available data obtained from the Danish Financial Supervisory Authority.

Size Total log of assets according to the financial statement of the bank.

Core_capital_ratio Core capital (less statutory deductions) as a percentage of risk-weighted assets.

Interest_rate_risk The figure illustrates the percentage of the core capital that would be lost due to a 1 percent increase in the interest rate.

Liquidity_coverage_ratio Excess coverage after fulfillment of the statutory minimum liquidity requirements

Large_exposures The sum of large exposures as a percentage of core capital + supplementary capital. Large exposures are defined as the sum of assets and off-balance-sheet items that, after a reduction for secured exposures, exceeds 10 percent of the combined core capital + supplementary capital.

Impairment_ratio Loans and impairment losses as a percentage of deposits.

Loan_growth Growth in loans, year-on-year

Table 3: Summary Statistics

Table 3 presents summary statistics for the 21,254 firms in the main analyzed sample based on the injunction status of their primary bank in 2010. The "Injunction" group are firm's whose primary bank received an injunction during 2010. The "No Injunction" group refers to firms whose primary bank did not receive an injunction in 2010 or 2011. The sample includes firms who reports industry and bank relationship and remain in the sample in the period from 2009-2011. "Difference" denotes the difference in means between the two groups, and "P-value" denotes the significance level of the difference, with standard errors clustered at the bank level. All variables are measured in 2009 and winsorized at the 1st and 99th percentile.

	Injuncton	No Injunction	Difference	P-value
Ln(Assets)	8.89	9.01	-0.13	0.35
	(1.58)	(1.66)		
Ln(Age)	2.74	2.84	-0.10	0.07
	(0.91)	(0.92)		
Debt to Assets	0.60	0.59	0.01	0.16
	(0.24)	(0.25)		
Return on Assets	0.04	0.04	0.00	0.34
	(0.15)	(0.15)		
Credit Score	0.02	0.02	0.00	0.39
	(0.02)	(0.02)		
Critical Auditor	0.09	0.08	0.01	0.35
	(0.28)	(0.27)		
Legal form A/S	0.47	0.44	0.03	0.14
	(0.50)	(0.50)		
Retail	0.35	0.36	-0.01	0.12
	(0.48)	(0.48)		
Construction	0.16	0.13	0.03	0.05
	(0.37)	(0.34)		
Transportation	0.10	0.09	0.00	0.81
	(0.29)	(0.29)		
Real estate	0.22	0.23	-0.01	0.04
	(0.41)	(0.42)		
Manufacturing	0.18	0.18	-0.01	0.42
	(0.38)	(0.39)		
Observations	10,859	10,395		21,254

Table 4: Loan Outcomes

*Model (1)-(5) shows OLS regressions where the dependent variable is firm's change in log debt from 2009 to 2011 and model (6)-(7) has the change in average interest rate from 2009 to 2011 as the dependent variable. Average interest rate is calculated as the sum of the firm's financial expenditures in a given year, divided by the average of total debt outstanding beginning and end of year. The main RHS variable is the indicator variable taking the value 1 if the firm's primary bank received an injunction during 2010. The comparison group is firms whose primary bank did not receive an injunction in 2010 or 2011. All control variables are measured in 2009. Industry fixed effects include indictor for the firms main industry being in construction, retail, manufacturing, real estate or transportation while regional fixed effects include indicators for the five main regions in Denmark. Industry-age-leverage is fixed effects for quartile of firms debt-to-assets interacted with a full set of industry-by-firm age fixed effects. The outcome variable and explanatory variables are winsorized at the 1st and 99th percentile. The sample includes firms who reports industry and bank relationship and remain in the sample in the period from 2009-2011. Standard errors are clustered at the bank level with significance levels indicated by + $p < 0.10$, * $p<0.05$, ** $p < 0.01$."*

			Δ Ln(Debt)			Δ Interest	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Injunction Cap. Req.	-0.0127	-0.0286^{+}	-0.0310*	-0.0316*	-0.0301^{+}	-0.000761	0.00176
	(0.0105)	(0.0144)	(0.0147)	(0.0147)	(0.0152)	(0.00300)	(0.00195)
Bank; Ln(Total lending)		0.120	0.269	0.392	0.126		-0.236*
		(0.364)	(0.345)	(0.344)	(0.378)		(0.0890)
Bank; Core Capital Ratio		-0.0956	-0.109	-0.150	-0.0154		-0.0207
		(0.215)	(0.215)	(0.235)	(0.259)		(0.0386)
Bank; Interest rate risk		-0.621	-0.747	-0.944^{+}	-0.692		0.151
		(0.492)	(0.469)	(0.481)	(0.483)		(0.108)
Bank; Liquidity coverage ratio		0.00219	0.00633	0.00906	0.00600		-0.00703*
		(0.0138)	(0.0131)	(0.0141)	(0.0157)		(0.00303)
Bank; Large exposures		-0.0405*	-0.0395*	-0.0331^{+}	-0.0251		-0.00515
		(0.0189)	(0.0188)	(0.0194)	(0.0199)		(0.00395)
Bank; Impairment ratio		0.651	0.765	0.767	0.586		0.0203
		(0.844)	(0.823)	(0.853)	(0.825)		(0.279)
Bank; Loan growth y/y		0.0710	0.0784	0.0733	0.0390		-0.00294
		(0.0794)	(0.0753)	(0.0689)	(0.0690)		(0.0200)
Ln(assets)				0.000878	-0.00351		0.00356**
				(0.00214)	(0.00286)		(0.00106)
Ln(age)				-0.0885**	0.00971		-0.00654**
				(0.00639)	(0.0158)		(0.00227)
Debt to assets				-0.422**	-0.661**		-0.142**
				(0.0226)	(0.0752)		(0.0237)
Return on assets				0.0249	0.0235		0.174**
				(0.0204)	(0.0171)		(0.0119)
Credit Score				1.771**	1.391**		0.983**
				(0.191)	(0.193)		(0.123)
Critical Auditor				-0.0477*	-0.0398*		-0.0203**
				(0.0204)	(0.0192)		(0.00629)
Legal form (A/S)				-0.0867**	-0.0783**		-0.000637
				(0.00586)	(0.00790)		(0.00213)
Bank controls	No	Yes	Yes	Yes	Yes	No	Yes
Industry FE	No	No	Yes	Yes	.	No	.
Region FE	No	No	Yes	Yes	Yes	No	Yes
Firm controls	No	No	No	Yes	Yes	No	Yes
Industry-age-leverage-FE	No	No	No	No	Yes	No	Yes
Observations	21254	21254	21254	21254	21254	21254	21254

Table 5: Real Outcomes

*Model (1)-(4) shows OLS regressions where the dependent variable varies in each model, but all other control variables remain the same. Model (1)-(2) include firms that remain in the sample from 2009-2011 with model (1) having the change in the firm's total log assets from 2009 to 2011 as the dependent variable and model (2) has the change in the firm's equity-to-assets ratio from 2009-2011. Model (3) has the dependent variable as an indicator for switching primary bank between 2010 to 2011. Model (4)-(5) includes firms that exit the sample where model (4) has an indicator variable taking the value 1 if the firm exits the sample in 2011 while model (5) includes firms that exit the sample by setting their change in log debt to be the negative of log debt measured in 2009. The main RHS is the indicator variable taking the value 1 if the firm's primary bank received an injunction during 2010. The comparison group is firms whose primary bank did not receive an injunction in 2010 or 2011. All control variables are measured in 2009. Industry fixed effects include indictor for the firms main industry being in construction, retail, manufacturing, real estate or transportation while regional fixed effects include indicators for the five main regions in Denmark. Firm-level controls include log assets, log age, debt-to-assets, return-on-assets, the firm's estimated credit score, indicator for critical auditor, and a dummy for legal form of the firm. Industry-age-leverage is fixed effects for quartile of firms debt-toassets interacted with a full set of industry-by-firm age fixed effects. The outcome variable and explanatory variables are winsorized at the 1st and 99th percentile. Standard errors are clustered at the bank level with significance levels indicated by + $p < 0.10$, * $p<0.05$, ** $p < 0.01$.*

	Δ Ln(Assets)	Δ E/A	Pr(Switch)	Pr(Exit)	Δ Ln(Debt)
	(1)	(2)	(3)	(4)	(5)
Injunction Cap. Req.	-0.00233	0.0137*	0.0149	0.00167	-0.0427
	(0.00688)	(0.00507)	(0.0196)	(0.00392)	(0.0325)
Bank; Ln(Total lending)	-0.175	-0.0470	0.133	0.0960	-0.0918
	(0.201)	(0.148)	(0.287)	(0.140)	(0.943)
Bank; Core Capital Ratio	-0.155	-0.116	-0.409	0.0272	-0.302
	(0.138)	(0.0890)	(0.487)	(0.0839)	(0.672)
Bank; Interst rate risk	-0.401	0.0657	-2.229+	-0.0803	-0.208
	(0.240)	(0.185)	(1.182)	(0.183)	(1.186)
Bank; Liquidity coverage ratio	0.00792	0.000378	0.0217	0.00495	-0.0236
	(0.00669)	(0.00589)	(0.0271)	(0.00581)	(0.0409)
Bank; Large exposures	-0.00127	0.00431	0.106*	-0.00569	0.00660
	(0.0103)	(0.00674)	(0.0515)	(0.00802)	(0.0572)
Bank; Impairment ratio	-0.886+	-0.342	0.482	-0.117	2.137
	(0.474)	(0.313)	(1.011)	(0.331)	(2.134)
Bank; Loan growth y/y	-0.0284	-0.0251	0.0397	0.0351	-0.180
	(0.0501)	(0.0252)	(0.0558)	(0.0325)	(0.221)
Bank controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Industry-age-leverage-FE	21254	21254	20751	22239	22239

Table 6: Heterogeneous effects

*Model (1)-(3) shows OLS regressions where the dependent variable is firm's change in log debt from 2009 to 2011 as the dependent variable. The indicator variable, taking the value 1 if the firm's primary bank received an injunction during 2010, is interacted with whether the firm had a positive or negative earnings in 2009 in model (1), with whether the firm where above or below median age in model (2) and in model (3) by both age and earnings. The comparison group is firms whose primary bank did not receive an injunction in 2010 or 2011. Model (4)-(6) are analogous to the setup in model (1)-(3) except that the dependent variable is change in log assets from 2009 to 2011. All control variables are measured in 2009. Bank-level controls include log total lending, core capital ratio interest rate risk, liquidity coverage ratio, the fraction of lending to large exposures, impairment ratio and yearly loan growth. Industry fixed effects include indictor for the firms main industry being in construction, retail, manufacturing, real estate or transportation while regional fixed effects include indicators for the five main regions in Denmark. Firm-level controls include log assets, log age, debt-to-assets, return-on-assets, the firm's estimated credit score, indicator for critical auditor, and a dummy for legal form of the firm. Industry-age-leverage is fixed effects for quartile of firms debt-to-assets interacted with a full set of industry-by-firm age fixed effects. The outcome variable and explanatory variables are winsorized at the 1st and 99th percentile. The sample includes firms who reports industry and bank relationship and remain in the sample in the period from 2009-2011. Standard errors are clustered at the bank level with significance levels indicated by + p < 0.10, * p<0.05, ** p < 0.01.*

	Δ Ln(Debt)			Δ Ln(Assets)		
	(1)	(2)	(3)	(4)	(5)	(6)
Inj X neg. result	-0.0509*			-0.0156		
	(0.0189)			(0.0109)		
Inj X pos. result	-0.0202			0.00426		
	(0.0149)			(0.00609)		
Inj X young		-0.0421*			-0.00656	
		(0.0157)			(0.00863)	
Inj X mature		-0.0158			0.00275	
		(0.0174)			(0.00732)	
Inj. young - neg. result			-0.0847**			-0.0328*
			(0.0225)			(0.0144)
Inj. young - pos. result			-0.0233			0.00554
			(0.0146)			(0.00748)
Inj. mature - neg. result			-0.0174			0.00114
			(0.0236)			(0.0110)
Inj. mature - pos. result			-0.0150			0.00336
			(0.0170)			(0.00689)
Neg. Result	-0.0282**		-0.00786	-0.0522**		-0.0416**
	(0.00957)		(0.0163)	(0.00852)		(0.0132)
Mature X Neg. Result			-0.0385			-0.0207*
			(0.0264)			(0.00993)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-age-leverage-FE	21254	21254	21254	21254	21254	21254

Table 7: Placebo outcome: Change in log debt 2007-2009

*Model (1)-(5) shows OLS regressions where the dependent variable is firm's change in log debt from 2007 to 2009. The main RHS is the indicator variable taking the value 1 if the firm's primary bank received an injunction during 2010. The comparison group is firms whose primary bank did not receive an injunction in 2010 or 2011. All control variables are measured in 2009. Industry fixed effects include indictor for the firms main industry being in construction, retail, manufacturing, real estate or transportation while regional fixed effects include indicators for the five main regions in Denmark. Firm-level controls include log assets, log age, debt-to-assets, return-on-assets, the firm's estimated credit score, indicator for critical auditor, and a dummy for legal form of the firm. Industry-age-leverage is fixed effects for quartile of firms debt-to-assets interacted with a full set of industry-by-firm age fixed effects. The outcome variable and explanatory variables are winsorized at the 1st and 99th percentile. The sample includes firms who reports industry and bank relationship and remain in the sample in the period from 2009-2011. Standard errors are clustered at the bank level with significance levels indicated by + p < 0.10, * p<0.05, ** p < 0.01.*

			Δ Ln(Debt)		
	(1)	(2)	(3)	(4)	(5)
Injunction Cap. Req.	0.00293	-0.00649	-0.00655	0.0143	0.0153
	(0.00539)	(0.0120)	(0.0121)	(0.0102)	(0.0115)
Bank; Ln(Total lending)		-1.044**	-0.812*	-0.632*	-0.622*
		(0.376)	(0.335)	(0.307)	(0.306)
Bank; Core Capital Ratio		0.644*	0.591*	0.496**	0.365+
		(0.252)	(0.232)	(0.164)	(0.187)
Bank; Interst rate risk		0.637	0.532	0.706+	0.809*
		(0.461)	(0.410)	(0.383)	(0.401)
Bank; Liquidity coverage ratio		-0.0305+	-0.0198	-0.00843	-0.00825
		(0.0160)	(0.0141)	(0.0101)	(0.0107)
Bank; Large exposures		0.0457*	0.0400*	0.0392*	0.0305+
		(0.0198)	(0.0186)	(0.0162)	(0.0175)
Bank; Impairment ratio		-1.237	-0.904	-0.813	-0.823
		(1.048)	(0.919)	(0.787)	(0.894)
Bank; Loan growth y/y		-0.139+	-0.107	-0.153+	-0.173*
		(0.0788)	(0.0643)	(0.0775)	(0.0837)
Bank controls	No	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes	.
Region FE	No	No	Yes	Yes	Yes
Firm controls	No	No	No	Yes	Yes
Industry-age-leverage-FE	No	No	No	No	Yes
Observations	20734	20734	20734	20734	20734

Table 8: Within Injunction Banks Comparison

*Model (1)-(4) shows OLS regressions where the dependent variable is firm's change in log debt from 2009 to 2011 as the dependent variable. The indicator variable, taking the value 1 if the firm's primary bank received an injunction during 2010 is the main RHS variable in model (1). This indicator variable is interacted with whether the firm had a positive or negative result in 2009 in model (2), with whether the firm were above or below median age in model (3) and in model (4) by both age and financial result. The comparison group is firms whose primary bank received an injunction in 2011. All control variables are measured in 2009. Bank-level controls include log total lending, core capital ratio interest rate risk, liquidity coverage ratio, the fraction of lending to large exposures, impairment ratio and yearly loan growth. Industry fixed effects include indictor for the firms main industry being in construction, retail, manufacturing, real estate or transportation while regional fixed effects include indicators for the five main regions in Denmark. Firm-level controls include log assets, log age, debt-to-assets, return-on-assets, the firm's estimated credit score, indicator for critical auditor, and a dummy for legal form of the firm. Industry-age-leverage is fixed effects for quartile of firms debt-to-assets interacted with a full set of industry-by-firm age fixed effects. The outcome variable and explanatory variables are winsorized at the 1st and 99th percentile. The sample includes firms who reports industry and bank relationship and remain in the sample in the period from 2009-2011. Standard errors are clustered at the bank level with significance levels indicated by + $p < 0.10$, * $p<0.05$, ** $p < 0.01$.*

	Δ Ln(Debt)			
	(1)	(2)	(3)	(4)
Injunction Cap. Req.	-0.0325			
	(0.0243)			
Inj X neg. result		-0.0713*		
		(0.0255)		
Inj X pos. result		-0.0157		
		(0.0273)		
Inj X young			-0.0293	
			(0.0279)	
Inj X mature			-0.0365	
			(0.0246)	
Inj. young - neg. result				-0.0622*
				(0.0256)
Inj. young - pos. result				-0.0149
				(0.0308)
Inj. mature - neg. result				-0.0848*
				(0.0360)
Inj. mature - pos. result				-0.0152
				(0.0284)
Neg. Result		-0.0107		-0.0348
		(0.0257)		(0.0226)
Mature X Neg. Result				0.0484
				(0.0406)
Bank controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Industry-age-leverage-FE	12464	12464	12464	12464

Appendix

Figure A.1: Rank-rank correlation of bank size and observations

Notes: The x-axis ranks banks according to their number of observation in the Experian data while the y-axis ranks banks according to total lending. With perfect coverage from Experian, the correlation may however not be perfect as some banks specialize more in firms relative to households and total lending contains both. Banks that received an injunction during 2010 are marked with the "+"-symbol. The only bank that appears underrepresented in data based on total lending is Alm. Brand Bank A/S. All presented results are robust to excluding the 25 firms (0.11 percent of the sample) whose primary bank is Alm Brand Bank.

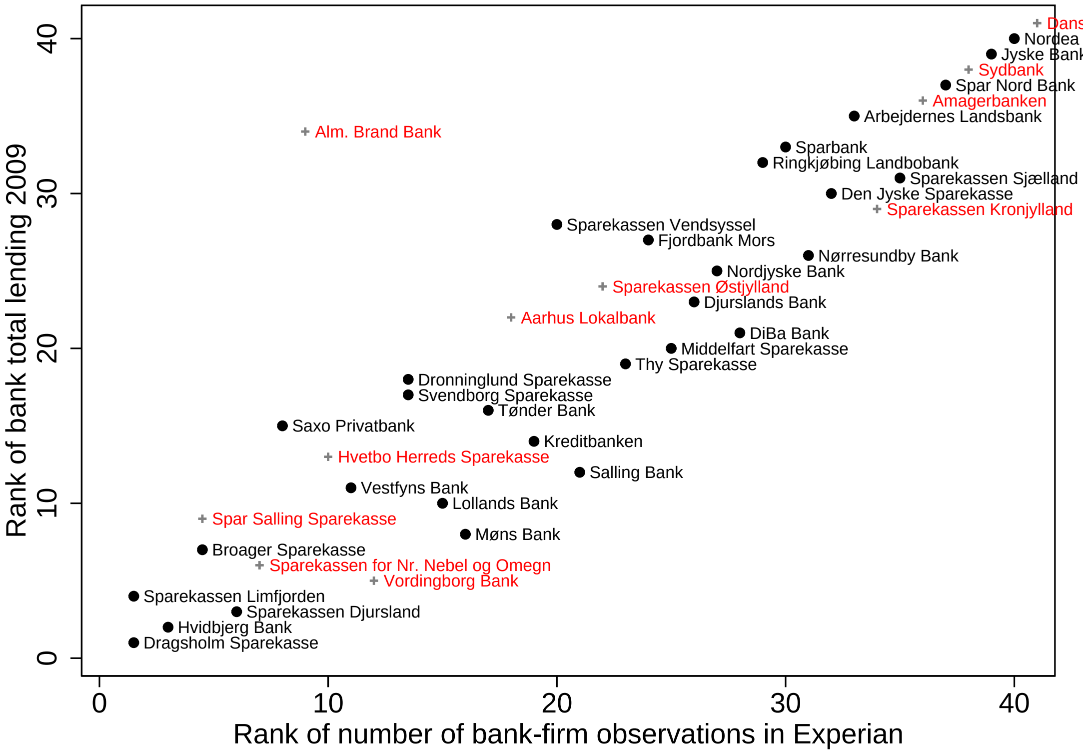
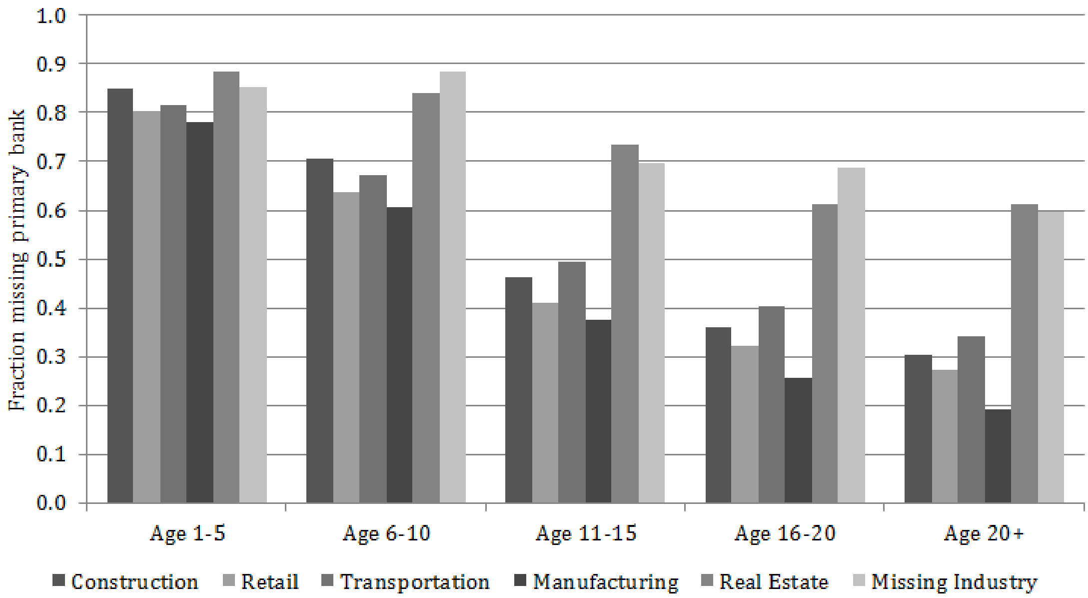


Figure A.2: Fraction missing bank information

Notes: Figure A.2 shows the fraction of firms missing information on primary bank tabulated by characteristics of the firm. Panel A shows the fraction of firms not reporting primary bank in 2010 by industry and firm age. Panel B shows the fraction of firms not reporting primary bank by industry and 5 equally sized bins of total assets, with bin 1 being the smallest and bin 5 the largest.

Panel A:



Panel B:

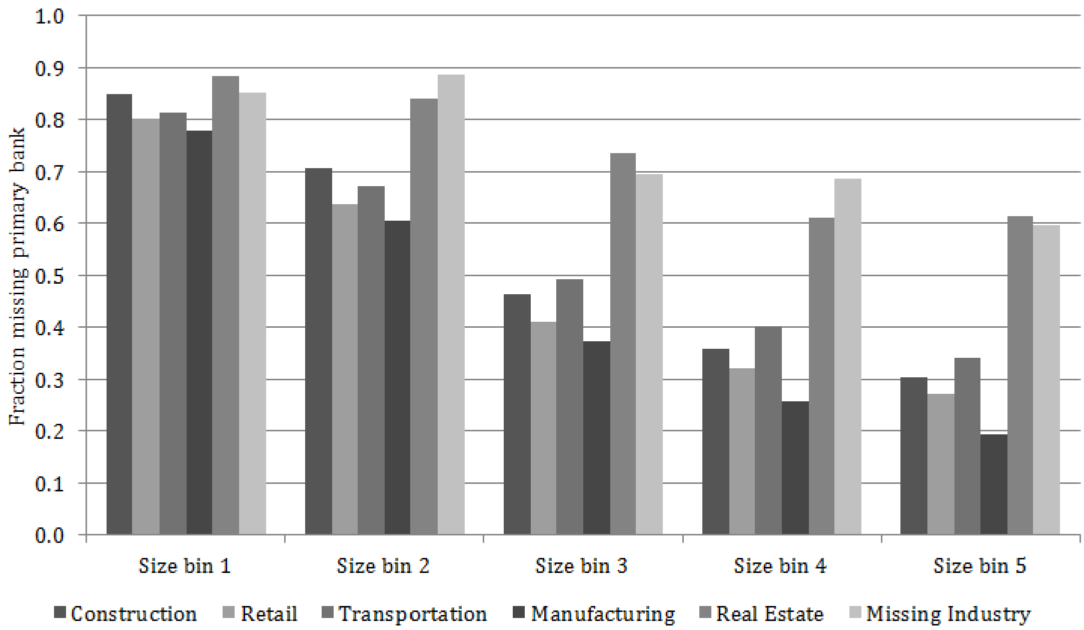


Figure A.3: Average leverage ratios of firms by primary bank status

Notes: Figure A.3 shows the average debt-to-asset ratio for firms in the analyzed sample based on the injunction status of their primary bank in 2010. The "Injunction"group are firms whose primary bank received an injunction during 2010. The "No Injunction" group refers to firms whose primary bank did not receive an injunction in 2010 or 2011. The number of firms in the years prior to 2009 is allowed to vary due to new firms entering the sample.

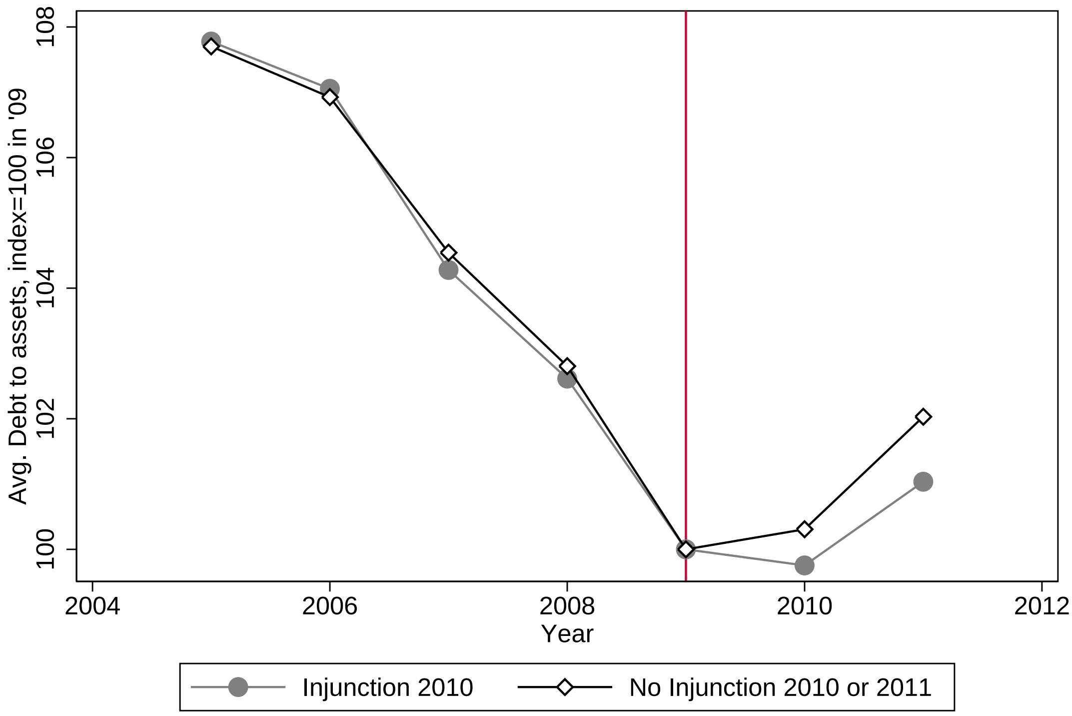
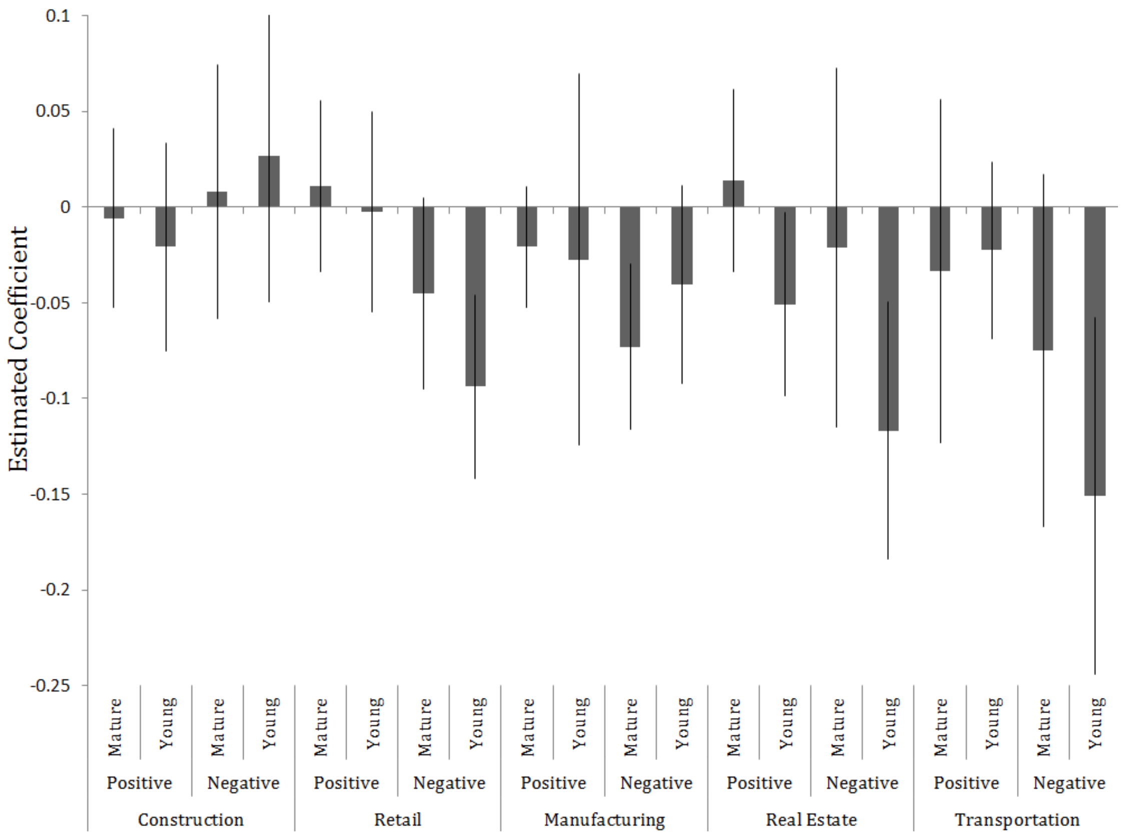


Figure A.4: Heterogeneous Effects by Industry, Earnings and Age

Notes: Figure A.4 shows the estimated coefficients obtained from a regression with the firm's change in log debt from 2009 to 2011 as the dependent variable. The indicator variable, taking the value 1 if the firm's primary bank received an injunction during 2010, is interacted with industry, indicator for above or below median age and indicator for negative of positive financial results in 2009 to show the heterogonous effects within each group. The control variables and sample selection are identical to the estimations carried out in table 6, with bank, industry, region, firm controls and industry-age-leverage fixed effects.



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Chapter 3

Household Debt and Spending During the Financial Crisis

This chapter is a revised version of *"Household Debt and Consumption during the Financial Crisis: Evidence from Danish Micro Data"*, Danmarks Nationalbank Working Paper Serires No. 89, March 2014, to be resubmitted at *European Economic Review*.

Household Debt and Spending During the Financial Crisis: Evidence from Danish Micro Data*

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June 2015

Abstract

We use data for nearly 500,000 Danish households to study the relationship between household leverage prior to the financial crisis of 2007-09 and the development in spending over the course of the crisis. We find a strong negative correlation between pre-crisis leverage and the change in spending during the crisis. This reflects that highly-levered households spent a larger share of their income than their less-levered peers prior to the crisis, resulting in larger increases in debt in these years. Once we condition on the size of the pre-crisis change in debt, a high level of debt is no longer associated with a larger spending decline. Our results suggest that the larger decline in spending among high-leverage households is the result of a spending normalization pattern that is also found in other years, rather than a causal effect of high debt levels suppressing household spending during the crisis.

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1 Introduction

High household debt is often put forward as a main factor when explaining the severity of the recession that hit countries all over the world in the aftermath of the financial crisis of 2007-09. For example, Paul Krugman wrote in a column for the New York Times in December 2010 that *"The root of our current troubles lies in the debt American families ran up during the Bush-era housing bubble."*[1] Understanding the role of debt and leverage in household spending decisions during times of financial crisis is important for guiding macro prudential policy. If high debt prompts a larger reduction in consumption when the economy is hit by financial unrest, policies aimed at curbing excessive household borrowing during economic upturns, or at providing relief to under-water borrowers once the damage is done, may be successful in reducing macroeconomic volatility. If not, such policies may hamper households' ability to smooth consumption and/or strain government budgets, with no significant benefits in return.

In this paper we use household-level micro data from Danish administrative registers to analyse the relationship between pre-crisis debt levels and the development in household spending over the course of the crisis. The balanced sample used in our analyses covers nearly 500,000 households in the years 2003-11.

As in many other countries, the financial crisis had severe consequences for the real economy in Denmark. As shown in Figure 1, aggregate household consumption grew rapidly until the 1st quarter of 2008. It then dropped by more than six percent within a single year, followed by an extremely slow recovery in the subsequent years. Also paralleling the experience from other countries, the crisis was preceded by a drastic increase in household debt. Figure 1 shows that the aggregate household debt-to–disposable-income ratio increased from 210 percent in early 2003 to 286 percent at the peak of the boom five years later. It even continued to rise during the darkest months of the financial crisis, reaching a level above 300 percent in late 2009, followed by a slow decline in the subsequent years. These developments naturally raise the question of what role the high level of household debt played in the severity of the crisis and the sluggishness of the subsequent recovery.

[Figure 1 here: Aggregate household debt-to-income ratio and aggregate consumption, 2003-11]

[1]Paul Krugman, "Block Those Metaphors", *The New York Times*, 12 December 2012

The idea that a high debt level in the household sector can lead to macroeconomic instability goes back to Irving Fisher's (1933) debt-deflation theory, according to which an excessively high debt level in a society can trigger a vicious cycle of deflation and falling economic activity. Related ideas have later been expressed by Minsky (1986), Bernanke & Gertler (1989), King (1994) and Eggertsson & Krugman (2012). On the empirical side, a number of studies have examined the role of debt in economic outcomes at the aggregate level. Analyzing country variation in leverage, Cecchetti et al. (2011) and Cecchetti & Kharroubi (2012) argue that leverage above a certain threshold depresses economic growth, while Dabla-Norris & Srivisal (2013) find that higher levels of debt amplify macroeconomic volatility.

At the micro level, several papers have documented a negative correlation between pre-crisis household indebtedness and spending growth during the crisis. Mian & Sufi (2010) study US county data and find that local areas with a larger run-up in household leverage prior to the crisis witnessed a more severe recession in the years 2007-09. Similarly, Mian et al. (2013) show that retail sales declined more in counties where households were highly leveraged prior to the crisis. Dynan (2012) makes use of the US Panel Study of Income Dynamics to show that households with high loan-to-value ratios in 2007 reduced spending more from 2007 to 2009 than households with lower loan-to-value ratios, while Bunn & Rostom (2014) document a similar pattern among U.K. households. Finally, Baker (2015) finds that spending was more sensitive to changes in income among U.S. households with a high level of debt than among those with less leverage during the recession of 2007-09.

But while the correlation between leverage and spending cuts during the crisis is well-established empirically, the mechanism behind it is not well understood. A common interpretation is that the correlation reflects a negative causal impact of household leverage on consumption. One potential mechanism behind such a causal effect is that households with high levels of debt prior to the crisis were suddenly facing binding borrowing constraints when the crisis broke out, forcing them to cut spending. A closely related explanation is that highly-levered households cut spending voluntarily due to precautionary motives. However, the correlation between leverage and subsequent spending cuts could also reflect that the high debt level among some households was simply the result of high spending in previous years, while the subsequent drop in spending reflected a return to normal levels. In this latter interpretation, the observed correlation does not reflect a causal effect from high debt on spending.

Our paper studies the link between leverage and spending among Danish households with the explicit purpose of understanding the mechanism behind this link during the crisis years. The paper contributes to the literature in five ways: First, we find the same negative correlation between leverage and spending growth in 2007-09 that other studies have found, but in a different institutional setting and in a richer data set. Compared to other households in our sample of Danish homeowners, households in the top 25 percent of the debt-to-income distribution in 2007 reduced spending by an extra 4 percentage points of their 2007-income from 2007 to 2009. This difference is robust to controlling for a wide range of observable household characteristics. Second, unlike previous studies we study differences across households in spending *levels* as well as in changes. Our results document that the larger spending decline among high-leverage households reflects that they came from a higher initial level before the crisis than low-leverage households, not that they spent less during and after the crisis. Third, and relatedly, we explicitly distinguish between changes in debt and levels of debt as the appropriate measure of pre-crisis leverage, a distinction that is not made clearly in the existing literature. We show that once we control for the *change* in debt in the year leading up to the crisis, the *level* of debt no longer has a separate role in explaining the larger decline in spending among high-leverage households. This result speaks against any interpretation of the data that emphasizes a causal effect of a high debt level on subsequent spending growth. Fourth, we find that the negative correlation between leverage and spending growth is neither confined to groups of households that are likely to become credit constrained, nor to the years surrounding the financial crisis. Similar results are found in all other years in our analysis sample, including in the pre-crisis period, suggesting that the correlation is not driven by factors that are unique to the financially turbulent years of 2007-09. Fifth and finally, we find that the observed differences between high- and low-leverage households are easily replicated when using car purchases as an alternative indicator of household spending. This suggests a prominent role for the timing of purchase of durable consumption goods in explaining the correlation between leverage and spending growth.

Overall, our results provide strong support for the hypothesis that the correlation between high pre-crisis leverage and weak subsequent spending growth is driven by a *spending normalization* pattern, in which households that have previously increased their debts to finance a temporarily high spending level subsequently return to a lower, "normal" level. In this interpretation, the large pre-crisis build-up of debt among some households was a consequence of high spending in these years. The subsequent decline in spending could reflect a downward adjustment of expectations about future income, or it could simply reflect that households that purchased a large durable good, such as a car, in one year is unlikely to make a similar-sized purchase in the next year. However, our results suggest that the decline in spending among high-leverage households was not caused by the high level of debt held by this group. In our view, this speaks against the widespread view that the macroeconomic recovery after the financial crisis has been suppressed by a "debt overhang" in the Danish household sector.

The paper continues as follows: Section 2 discusses theoretical arguments for why a high debt level may exacerbate the impact of negative shokcs on household spending and presents two alternative hypotheses that can explain why households with a high initial debt level reduced spending more during the financial crisis than households with little debt. In section 3 we describe the data used in the empirical analyses, while section 4 presents some descriptive statistics and basic correlations between the main variables of interest. In section 5 we present an empirical framework to analyse the link between leverage and spending. Section 6 focuses on the important issue of whether it is changes in debt or levels of debt that drives the correlation between leverage and spending growth in 2007-09. Section 7 analyses heterogeneity in the leverage-spending relationship across different groups of households. In section 8 we provide further analyses of what might drive the observed spending normalization pattern. Finally, section 9 provides concluding remarks and briefly discusses implications for policy.

2 The borrowing constraints hypothesis and the spending normalization hypothesis

This section presents two hypotheses that can potentially explain why high leverage prior to the financial crisis and spending growth during the crisis might be negatively correlated at the household level. The theoretical framework underlying both of these hypotheses is the well-known life-cycle model of consumption and saving (Brumberg & Modigliani 1954; Ando & Modigliani 1963; see Browning & Crossley 2001 for a modern review).

2.1 The borrowing constraints hypothesis

The first hypothesis centers around the importance of borrowing constraints that forced already-indebted households to cut back on consumption during the crisis. To understand this hypothesis, it is useful to think about how the financial crisis affected households' finance: Asset prices, including house prices, plummeted. Some families experienced a drop in current income; presumably, many more experienced a drop in expected future income, as well as increasing uncertainty about this future income. Finally, lending policies in financial institutions serving households were tightened. Alan et al. (2012) simulate the effects of such shocks in a life-cycle model and show that a recession involving these types of shocks leads to a sharp reduction in consumption. There is good reason to expect that the impact on consumption of such adverse shocks will be increasing in the level of initial leverage. First, when a recession occurs and income and asset prices drop, some households, and particularly those that already had a lot of debt, may find themselves unable to borrow, since there is typically a limit to the amount of debt that a household can have relative to either its income or its assets, or both. Without the possibility of further borrowing, these households may be forced to cut back on consumption. The same mechanism applies if credit standards are tightened via a lowering of the upper limit on the level of debt: Those who already have high debt at the onset are more likely to be constrained by such a tightening and must therefore cut back on consumption. Second, a constraint on borrowing can affect behavior even when it does not bind (Crossley & Low, 2014). An increase in uncertainty about future income may induce households to lower consumption through a precautionary saving motive, even in the absence of a shock to current income or a tightening of credit conditions (Deaton, 1991; Carroll, 1997). The reason is that households fear being constrained in the future in case of a negative income shock, so they self-insure against this by lowering consumption and accumulating wealth. Again, it is plausible that such an effect is stronger for households who have high initial debt, since these are closer to the upper limit and are thus more likely to become constrained.

In summary, borrowing constraints - whether bindng or not - play a central role in the arguments presented here for why high household debt may lead to lower consumption growth. We therefore refer to these arguments collectively as *the borrowing constraints hypothesis* in the remainder of this paper.

2.2 The spending normalization hypothesis

The second hypothesis, which we label *the spending normalization hypothesis*, reverses the order of causality: According to this hypothesis, some households decided (for reasons that we shall return to shortly) to temporarily boost spending to levels well above their disposable income in 2007. Financed by borrowing, the high spending level pushed up pre-crisis debt–to-income ratios for these households. But since the spending boost was only temporary, spending subsequently dropped more for these households than for others, thus generating a negative correlation between pre-crisis leverage and spending growth during the crisis. An important question is which economic factors could be behind a such temporary increase in spending. Michael Woodford offers a suggestion in the comments to Dynan (2012): Imagine that households in 2007 differed in their expectations for the future. The life-cycle model then predicts that those who had unusually optimistic expectations about future income growth would have spent a higher fraction of their income than less optimistic households. But the fact that these households were unusually optimistic before the crisis also meant that they lowered their expectations about future income more than others once the crisis hit, prompting a larger cut in spending. Another potential factor is the timing of purchase of large durable consumption goods, such as cars: Households that happened to buy a car in 2007 most likely saw a large upwards spike in spending in that year. Assuming, realistically, that most of them did not buy a car again within the next two years, the spike in spending was then followed by a large subsequent drop. If financed by borrowing, the car purchase would at the same time have implied a significant increase in debt in 2007 for these households. So, under these assumptions, even random differences in the timing of purchase of large durables can potentially explain the observed correlation between pre-crisis leverage and weak subsequent spending growth.

2.3 Policy implications

The two hypotheses presented above have very different implications for policy. According to the borrowing constraints hypothesis, in any of its various forms, high debt was a *causal* factor behind the disproportionally large drop in spending during the crisis among households with high pre-crisis leverage. If this is true, policies aimed at curbing high debt levels in the household sector - whether by preventing them from ever occurring or by providing relief once they are there - would have led to a less severe drop in consumption during the recession and, possibly, a faster recovery. On the other hand, the spending normalization hypothesis posits that the correlation between leverage and subsequent spending reduction does not reflect a causal relationship. According to this hypothesis, both the high debt level and the drop in spending among the high-leverage households were caused by unusually high spending prior to the crisis. In that case, debt relief policies aimed at these households would most likely only have had a modest impact on the depth and duration of the recession that followed the crisis, since it was not the high debt level itself that prompted them to cut spending.

3 Data

The data used in this paper comes from several administrative registers, covering all individuals residing in Denmark. The data is anonymized and made available to researchers by Statistics Denmark. Information on income, wealth and debt originates from the personal income register. The main source for this register is tax returns based on third-party reports. Information regarding e.g. age, area of residence and family relations stems from the population register. Using the information on family relations, we aggregate all individual data on income, wealth and debt to the household level. A household is here defined as either one or two adults living together, plus any number of children (see data appendix for details).

Our data covers the years 2002-11, but differencing of selected variables (see section 4.1 below) requires us to drop one year, so that our analysis period spans the years from 2003 to 2011. Starting from the full population of 2,775,500 separate households in 2007, we first restrict our analysis to the balanced sample of households that appear in the data in all years between 2003 and 2011 with an unchanged composition of adult members. This excludes newly established households, as well as households that break due to e.g. divorce or death of a spouse, leaving 1,578,678 households observed in nine consecutive years. Second, we exclude households in which at least one of the adults is self-employed at any point between 2003-11, since income and wealth are often measured imprecisely in this case. Households in which at least one member is not fully liable to taxation in Denmark are also excluded. Further, we exclude renters and restrict our sample to homeowner families in which the oldest person was between 25 and 99 years of age in 2007. Finally, for reasons explained in the next subsection, we exclude families that either bought or sold one or more homes during the period of analysis. After these restrictions, we are left with a sample of 492.194 households and 4,429,746 household-year observations.

3.1 Imputing household spending from income and wealth data

Register-based data on consumption or spending are unfortunately not available at the household level. Following Browning & Leth-Petersen (2003), Leth-Petersen

(2010), and Browning et al. (2013), we instead rely on a measure imputed from data on household disposable income, assets and liabilities. The approach behind this measure starts from the cash-flow identity that household i's disposable income (net of interest payments) in year t, Y_{it}^d, must equal the sum of spending, net purchases of assets and net debt repayments. Disposable income is directly observable from our data, while net purchases of assets and debt repayments are not. We approximate the latter two with the change in the value of household i's total assets from year $t-1$ to t minus the change in its total debt. This implies that spending in year t can be calculated as:

$$Spending_{it} \approx Y_{it}^d - \left(\sum_k \Delta a_{it}^k - \Delta d_{it} \right)$$

where Δa_{it}^k and Δd_{it} denote the changes in the values of household i's holdings of asset type k and debt, respectively, from year $t-1$ to t. Put more simply, spending in year t is measured as disposable income in year i minus the change in net nominal wealth from year $t-1$ to t.

The main issue with this approach is that the change in the value of a household's holding of a particular asset (or liability) does not necessarily reflect a change in the physical stock of that asset, i.e. net purchases. Changes in the asset's price, i.e. capital gains or losses, are also included, and it is generally not possible to separate the two sources of variation. This means that the imputed measure of spending may contain measurement error.

There are three important cases where we are in fact able to do something about the above-mentioned problem: First, for most homeowners, fluctuations in housing prices are undoubtedly the most important source of capital gains or losses. Fortunately, our data allows us to identify those households that are involved in a real estate trade in any given year. As mentioned in the previous subsection, we exclude these households from our sample in all that follows. For the remaining households in the sample, who do not change their physical stock of housing during the year, any change in the value of their housing wealth must be due to capital gains or losses.[2] We therefore exclude housing wealth when summing over the changes in the values of the households' assets.

Second, for one particular type of asset, pension savings, we do actually have accurate data for net purchases, in the form of yearly contributions to individual

[2] We here ignore changes in the physical stock of housing that result from home improvements or extensions. This implies that expenses for such projects are included in our measure of spending in the year in which they are paid.

pension accounts. In this case, there is no need for differencing the value of the stock, and we use the yearly contributions as a direct measure of this particular component of net asset purchases.[3]

Third, fluctuations in stock prices is another important source of capital gains or losses for stock-owning families. Our data does not allow us to separate the effect of changing stock prices from the effects of actual buying and selling. Instead, we use a crude adjustment based on the overall development in stock markets: For each family, we multiply the value of stock portfolio at the beginning of the year with the over-the-year growth rate of the C20 index, the top-tier index of the Copenhagen Stock Exchange. The result of this calculation can be seen as an approximation of the capital gain earned on the family's stock portfolio during the year, so we subtract it from the change in the value of the family's stock portfolio. Naturally, this crude adjustment completely ignores the large variation in price movements between different stocks, but it should take us a long way in removing any systematic differences in the imputed measure of spending between stock owners and non-owners.

It should be noted that the imputed measure is a measure of out-of-pocket expenses; it is not a measure of consumption. It includes spending on non-durable consumption goods, but also on durables bought in year t, such as cars. The consumption services provided by durables bought in previous years are not included, however. For homeowners, this implies that the consumption value provided by their homes is not included in our spending measure, since this does not involve any direct cash-flow. For renters, on the other hand, housing services do involve an out-of-pocket expense in the form of rent, so the imputed measure captures non-housing spending as well as housing expenses. For this reason, we focus strictly on homeowner families in our analyses.

3.2 The DTI ratio and other economic variables

Our preferred measure of leverage is the household's debt-to-income (DTI) ratio. The DTI ratio is measured as the family's total debt owed to Danish financial institutions, divided by its total pre-tax income. Total debt is measured at year-end. Property financing in Denmark mainly takes place via specialized mortgage banks. Debt owed to such banks is always secured against real property and must not exceed 80 percent of the value of the property at the time of origination. Total debt also

[3]Most pension saving accounts are employer-administered, which means that contributions into the accounts are paid directly by the employer. These contributions do not enter the disposable income of the family, so there is no need to subtract them in the imputation. Only contributions to privately administered accounts are subtracted.

includes debt owed to universal (i.e. non-specialized) Danish banks, which may or may not be secured. Other types of unsecured debt, such as credit card debt, and debt owed to the government are also included. Total pre-tax income includes labour market income, capital income as well as transfers from the government. Figure 2 shows the distribution of the DTI ratio in 2007 in our analysis sample. The mean DTI ratio in 2007 was 1.63 while the median was 1.4.[4] 11.4 percent of the households had exactly zero debt in 2007, while 18 percent had a DTI ratio below 0.15. At the other end of the distribution, 10.3 percent had debt worth at least three times their annual household income, while 4.2 percent had a DTI ratio above four.

[Figure 2 here: Histogram for DTI ratios, 2007]

Net wealth is calculated as total assets minus total debt, measured at year-end. Total assets include real property, financial assets, and bank deposits. Our measure of the value of a household's real property is based on the official property valuations made by the Danish tax authority, which are available in the personal income register. We multiply these valuations by a scaling factor that reflects the average ratio of actual sales prices to public valuations for the relevant combination of property type, geographical area of residence, and year. Such scaling factors are published by Statistics Denmark on a regular basis. Pensions savings, cash holdings, the value of the family's durable goods (such as cars, boats, household effects and art) and the value of private cooperative housing are not included in our measure of total assets, due to a lack of data, whereas any debt raised to acquire these assets is included in total liabilities. We distinguish between liquid and non-liquid assets. The former are defined as deposits in banks, the market value of bonds, mortgage deeds, stocks and investment certificates in the custody of a bank.

Finally, we use individual-level information on car ownership obtained from Statistics Denmark, which includes data on the number of cars each person owns as well as the year in which each car was first registered with the tax authorities. This information is available from 2004 and onwards and we use it to construct an indicator variable for whether the household bought a new car in a given year.

[4]Due to data confidentiality requirements we are not allowed to report information pertaining to any single observation in the dataset, so we report percentiles with only one decimal throughout the paper.

4 Some descriptive statistics and basic correlations

The main focus of this paper is to examine the empirical case for each of the two hypotheses presented in section 2. But the first step towards this end-goal is to present the basic empirical observation that the hypotheses are meant to explain, namely the negative correlation between pre-crisis leverage and subsequent spending growth. Figure 3 illustrates this correlation among the subsample of households in which the oldest member was 45 years old in 2007.[5] The figure displays the results of a simple non-parametric analysis of the relationship between the change in household spending from 2007 to 2009 - measured relative to household pre-tax income in 2007 - and the debt-to-income ratio in 2007, using a smoothed local polynomial regression. The figure illustrates that the relationship between pre-crisis leverage and subsequent spending growth is not necessarily linear, and perhaps not even monotonic. But the important thing that stands out in the figure is the negative correlation between leverage in 2007 and spending growth in 2007-09: On average, households with a high DTI ratio in 2007 reduced spending more between 2007 and 2009 than households with a low DTI ratio in 2007. This is the basic finding that we attempt to explain in this paper. For this reason, our empirical analyses below focus on the differences in spending patterns between households with high leverage in 2007 vs. everyone else. More precisely, we compare the spending development among the households in the top quartile of the DTI distribution in 2007, equivalent to those who had a DTI ratio of at least 2.2, with the development in spending among the remaining 75 percent of households in our analysis sample. For simplicity, we label these two groups the high-leverage households and the low-leverage households, respectively.

[**Figure 3 here: DTI ratio in 2007 and change in spending from 2007-09, households in which the oldest member is 45 years old**]

Table 1 shows descriptive statistics for the households in our sample, separating high-leverage households from low-leverage households. Starting with their characteristics in 2007, there were a number of differences between the two groups in this year. The high- leverage households were generally younger, had more children, and had lived at their current address for a shorter period of time than the low-leverage families. They also had lower total income in 2007, and their net worth was lower.

[5]We focus on a single cohort as a simple way of controlling for age, which correlates strongly with leverage. We have constructed identical plots for all other age cohorts and they all display a similar relationship between the DTI ratio in 2007 and the change in spending from 2007 to 2009.

Comparing the mean values of our measure of imputed spending to the corresponding means of disposable income, we also see that low-leverage households where on average net savers while high-leverage households spent well above their disposable income in 2007.

[**Table 1 here: Descriptive statistics, high-leverage households vs. low-leverage households**]

Both low- and high-leverage households saw house price increases of 17 percent over the period from 2005-2007, while the two groups experienced income growth of 7 percent and 4 percent, respectively, in these years. Turning to the development from 2007 to 2009, we see that the high-leverage households experienced income growth of almost 6 percent, against only 1.5 percent for the low-leverage households. Due to falling house prices, both groups experienced substantial declines in their housing wealth over the course of the financial crisis: For the low-leverage households, the mean price drop was 11.4 percent over the two-year period, for high-leverage households it was 13.0 percent.[6]

Summing up, the simple comparisons in this section show that households that were highly leveraged prior to the financial crisis reduced spending more than other homeowners during the crisis, despite a better development in income and a similar development in housing wealth. This is in line with existing studies that have found a negative impact of high leverage on spending in the U.S. and elsewhere. But as we have seen in this section, the high- leverage households also differed from other homeowners in a number of other dimensions that may have influenced spending responses during the crisis. In the next section, we present results from regressions in which we control for these observable differences between the two groups.

5 Baseline econometric analysis

This section provides a descriptive analysis of the relationship between household

[6]Recall that we exclude all families that were involved in a real estate trade in the period under consideration. For the remaining families, a change in the value of their housing stock must therefore reflect changing house prices and/or home improvements to the existing stock. In our main analysis we include municipality-year fixed effects that effectively absorb any differential developments in local house prices.

leverage in 2007 and the development in spending in the years 2003-11. This analysis serves three purposes: First, we examine whether the observed correlation between high pre-crisis leverage and weak spending growth persists when we condition on a battery of other observable household characteristics that also correlate with DTI ratios in 2007. Second, the analysis sheds light on whether (and how) high- and low leverage families differed in their spending behavior not only during and after, but also *before* the financial crisis. Third and finally, the analysis provides a baseline empirical approach from which we depart in the following sections when we examine the empirical performance of the borrowing constraints hypothesis vs. the spending normalization hypothesis.

5.1 Econometric specification

We examine the leverage-spending relationship by estimating variants of the following regression using OLS:

$$\begin{aligned} C_{i;t} \;&=\; \alpha + \beta_1 DTI^{high}_{i;2007} + \beta_2 \mathbf{year}_t + \beta_3 DTI^{high}_{i;2007} \cdot \mathbf{year}_t \\ &\quad + \gamma_1 \mathbf{X}_{i;2007} + \gamma_2 \mathbf{X}_{i;2007} \cdot \mathbf{year}_t + \varepsilon_{i;t} \end{aligned} \tag{1}$$

where the dependent variable is the level of spending for household i in year t, $t = 2003, .., 2011$. To ensure comparability across households with different income levels, spending is always measured relative to household i's pre-tax income in 2007.[7] The key explanatory variable is the debt-to-income ratio in 2007. This is represented by a dummy variable, $DTI^{high}_{i;2007}$, that takes the value 1 if household i belonged to the group of high-leverage households (i.e. the top 25 percent) in 2007.[8] We include this variable by itself, as well as interacted with a vector of year dummy variables, $\mathbf{year}_t$. This vector, which has 2007 as the omitted base category, is also included by itself.

The vector of control variables $\mathbf{X}_{i;2007}$ contains a range of household characteristics as of 2007, each included by itself as well as interacted with the year dummies. It includes household i's net wealth and stock of liquid assets, both measured relative to household income. Household income itself is also included. We also control for the age of the oldest household member, the number of children, the number of

[7]We choose to scale spending in all years relative to income in a fixed year (2007), rather than in different years. This is in order to avoid conflating differences in the development of spending with differences in the development of income over the analysis period across households with different DTI levels in 2007.

[8]We have also estimated an alternative version of the model in which spending depends linearly on the debt-to-income ratio in 2007. The results are qualitatively no different from the results we present here and in the interest of space we choose not to report them.

years since moving to the current address, education level, retiree status and the geographical area of residence. To allow for potential non-linearities, we exploit the large number of observations in our sample and choose a flexible functional form for all of our control variables: The income, net wealth and liquid assets variables are treated as categorical variables, and each is represented by a set of dummies that indicate which decile the household belongs to in the distribution of the variable in question. For the age variable, the number of children, and the duration of the current residency we include full sets of dummy variables that cover all of the discrete values that the variables take in the analysis sample. Education is represented by a simple dummy varable that takes the value 1 if any member of the household holds a higher education degree, and retiree status is similarly represented by a simple dummy. Finally, the area of residence is represented by dummy variables for each of the 98 municipalities in Denmark. The fact that the controls are included both by themselves and interacted with the year dummies means that we take into account any differences in spending levels across households with different observable characteristics, as well as heterogeneity in the time profile of spending across such groups. For example, including a full set of municipality dummies interacted with time dummies for each year means that we are allowing heterogeneity in local spending levels, as well as in local trends in spending. Intuitively, this implies that our estimates of the coefficients in β_1 and β_3 are based on comparisons between high- and low-leverage families that reside in the same geographical area and face the same local economic outlook. We estimate equation (1) by OLS using clustered standard errors at the household level.

The specification in (1) differs from that used in e.g. Dynan (2012), who regresses the *change* in spending from 2007 to 2009 on leverage in 2007.[9] We believe there are some notable advantages to the specification in (1). First, the specification in equation (1) informs us not only about differences in spending *growth* between high- and low leverage families, but also about the differences in spending *levels* between these two groups. This is expressed by the coefficient β_1, which captures the difference in spending levels between the two groups in the base year 2007. Second, it informs us about how this level difference varies from year to year, not just in the period after 2007 but also in the preceding period. This is captured by the coefficients in the vector β_3, which express the *differences-in-differences* in spending compared to 2007. Of particular interest is the β_3-coefficient associated with 2009. Let this be denoted

[9]Similarly, Baker (2015) regresses the change in spending on the interaction between the change in income and the level of debt, measured relative to either income or assets. Mian & Sufi (2010) regress changes in consumption on changes in debt-to-income ratios at the zip-code level.

by β_3^{09}. This coefficient measures the difference in the change in spending from 2007 to 2009 between high-leverage households and low-leverage households, conditional on other observable 2007-characteristics.[10]

5.2 Results

Table 2 reports estimates of β_3^{09} for different compositions of the vector of control variables $\mathbf{X}_{i;2007}$.[11] Column 1 reports the estimate when no controls are included: Parallelling the picture shown in Figure 3, we find a negative and strongly significant coefficient estimate. In columns 2 to 5 we sequentially add control variables: Municipality dummies are added first, then age dummies, other demographic characteristics, and financial variables, all included by themselves as well as interacted with a full set of year dummies. Adding these control variables does not alter the estimate of β_3^{09} in any material way. In other words, the negative correlation between pre-crisis leverage and spending growth during the crisis persists when we condition on a broad range of household characteristics. Measured relative to income in 2007, the estimate in column 5 says that high-leverage households cut spending by 4.5 percentage points more than low-leverage households with similar characteristics from 2007 to 2009.

[Table 2 here: Regression results for β_3^{09}, various controls]

The estimates for β_3^{09} reported in Table 2 parallel the results in Dynan (2012) for U.S. households. But as explained above, the specification in (1) allows us to obtain a much more detailed picture of the differences in the development of spending between high- and low-leverage households. Figure 4 provides a graphical illustration of the results from the regression in column (5) of Table 2, using the full set of estimated coefficients. The figure is constructed by plotting the average predicted values from this regression when $DTI_{i;2007}^{high} = 1$ (black) and when $DTI_{i;2007}^{high} = 0$ (dashed), using actual values for all other explanatory variables in each year.[12] The figure clearly

[10] The 2009-coefficient in β_3 is the direct equivalent to the coefficient on the leverage variable that would be obtained from a regression of the change in spending from 2007 to 2009 on $DTI_{i;2007}^{high}$ and $\mathbf{X}_{i;2007}$.

[11] We restrict our attention to the differences-in-differences estimate for 2009 here, but estimates for all beta coefficients can be found in appendix table A.1. Estimates for all coefficients in (1), including on the control variables, are available from the authors upon request.

[12] That is, the black curve plots the within-year sample averages of $\widehat{C}_{i;t}^{high} = \widehat{\alpha} + \widehat{\beta}_1 + \widehat{\beta}_2^t + \widehat{\beta}_3^t + \widehat{\gamma}_1 \mathbf{X}_{i;2007} + \widehat{\gamma}_2^t \mathbf{X}_{i;2007}$, where $\widehat{\beta}_2^t$, $\widehat{\beta}_3^t$, and $\widehat{\gamma}_2^t$ are estimates of the elements of β_2, β_3, and γ_2, respectively, that are associated with year t. Similarly, the dashed curve plots the within-year sample averages of $\widehat{C}_{i;t}^{low} = \widehat{\alpha} + \widehat{\beta}_2^t + \widehat{\gamma}_1 \mathbf{X}_{i;2007} + \widehat{\gamma}_2^t \mathbf{X}_{i;2007}$. Note that the 95 percent confidence intervals of all estimates

shows that the high-leverage households reduced spending more in the years after 2007 than low-leverage households with similar characteristics. But importantly, it also shows that the former group came from a much higher pre-crisis level of spending than the latter group. Measured relative to household income, this level difference amounted to 10 percentage points in 2007, with similar differences in the preceding years. The gap between the two groups then narrowed from 2008 onwards, until stabilizing at around 4 percentage points in 2010.

[Figure 4 here: Predicted levels of consumption, high leverage vs. low leverage]

The large initial difference in spending levels and the subsequent convergence between the two groups is exactly what the spending normalization hypothesis would predict. The borrowing constraints hypothesis makes no such predicition. Moreover, the results raise doubt about the widespread notion that aggregate consumption has been suppressed by strong deleveraging efforts by highly indebted households in the aftermath of the financial crisis, as asserted by Dynan (2012) for the U.S. case. If that were true in Denmark, we would expect to see lower post-crisis consumption levels among highly leveraged households than among other households, conditional on other family characteristics. Our results document that this has not been the case.

6 Levels of debt vs. changes in debt

Having established the main set of facts that we are trying to explain in this study, we now turn our attention to testing the empirical power of the two hypotheses presented in section 2. An important difference between the two hypotheses concerns the question of whether it is a high level of debt or a large increase in debt that is behind the larger spending reduction for the high-leverage households. A borrowing constraints hypothesis that assumes an upper limit to the stock of debt that a household can have predicts a negative impact on spending from a high *level* of debt, since a high initial level increases the likelihood of hitting the upper limit. In contrast,

are also shown in the figure but the large number of observations implies that these are extremely narrow.

the spending normalization hypothesis says that the large decline in spending is the result of temporarily high pre-crisis spending, which also results in a large pre-crisis *increase* in debt. In other words, we should expect to see a negative correlation between pre-crisis *changes* in debt and subsequent spending growth, whereas the level of debt should not matter in itself.

The existing literature is not clear about whether it is the increase in debt up to the crisis or the level of debt at the outbreak of the crisis that drives the correlation with post-crisis spending reductions. Mian & Sufi (2010) use a measure of leverage *growth* between 2002 and 2006 to explain changes in county-level economic outcomes from 2006 to 2009. Meanwhile, Dynan (2012) and Baker (2015) use *levels* of debt - relative to either income or assets - as their key explanatory variables. None of them examine whether it is actually changes or levels of debt that drive their results. However, as the discussion above suggests, this distinction is important for understanding the mechanism behind the leverage-spending relationship.

The discussion of levels vs. changes in debt is complicated by the fact that the two are closely correlated. This is documented for our sample in Table 3. The table reports descriptive statistics for the change in debt from 2006 to 2007, measured relative to income in 2007, broken down by the usual division of households into high- and low leverage groups in 2007. The average increase in debt from 2006 to 2007 was much higher among the high-leverage families than among low-leverage families. This is unsurprising, since the level of debt in any given year is of course equal to the sum of all previous changes. What is more interesting to note is that 48 percent of the high-leverage households increased debt by at least 1.9 percent of their 2007-income (the 75th percentile in the overall sample), against only 17 percent of the low-leverage families. And within these 48 percent, the average change in debt from 2006 to 2007 amounted to 49 percent of their income in 2007. This shows that for a considerable fraction of the high-leverage families, a substantial part of their total debt was incurred in the last year before our chosen cut-off date.

[Table 3 here: Change in debt, 2006-07. High leverage households vs. low leverage households]

Could the large *increases* in debt from 2006-07 among the high-leverage households be the actual driver behind the observed debt-spending relationship in 2007-09, rather than the level of debt in 2007? To examine this, we define a dummy variable $\Delta DTI^{high}_{i;2006-07}$ that takes the value 1 if the change in debt from 2006 to 2007 exceeds

1.9 percent of household income in 2007. We then include this as an extra explanatory variable alongside the debt-to-income ratio in 2007. For simplicity, we now focus directly on the change in spending from 2007-09, so we estimate a difference-equation of the following form

$$\Delta C_{i;2007-09} = \alpha + \delta_1 DTI^{high}_{i;2007} + \delta_2 \Delta DTI^{high}_{i;2006-07} + \lambda \mathbf{X}_{i;2007} + u_i \tag{2}$$

where the dependent variable is the change in spending from 2007 to 2009, measured relative to income in 2007. The results from this regression are shown in Table 4. In the first column we only include the level of debt in 2007, as measured by $DTI^{high}_{i;2007}$. Once again, we find a negative correlation between a high debt level in 2007 and spending growth from 2007 to 2009.[13] In the second column we replace the level variable $DTI^{high}_{i;2007}$ with the indicator of changes in debt, $\Delta DTI^{high}_{i;2006-07}$. We find a negative and strongly significant coefficient on this variable. This is as expected, given the strong correlation between $DTI^{high}_{i;2007}$ and $\Delta DTI^{high}_{i;2006-07}$. Finally, column three includes both variables simultaneously, with a striking result: We now find a *positive* coefficient on $DTI^{high}_{i;2007}$, while the coefficient on $\Delta DTI^{high}_{i;2006-07}$ is still significantly negative and almost identical to the one reported in column two. This shows that when we make comparisons within groups of households that experienced similar-sized *changes* in debt from 2006 to 2007, those with a high level of debt in 2007 *did not* reduce spending more from 2007 to 2009 than those with only little debt in 2007. But those that experienced a large increase in debt from 2006 to 2007 cut spending by more than those that did not experience a large increase, even if they had similar debt levels at the end of 2007.[14] In other words, the negative correlation between leverage in 2007 and spending growth in 2007-09 is entirely driven by the fact that a large share of the households with high DTI ratios in 2007 had *increased* their debt by large amounts during 2007. The level of debt itself does not help explain the larger spending decline for this group.

[Table 4 here: Regressions of change in spending, 2007-09. Levels of debt

[13] The specification in column one of Table 4 is a direct parallel to the levels specification in equation (1), but using data for 2007 and 2009 only. Notice the similarity between the coefficient on $DTI^{high}_{i;2007}$ in column one of Table 4 and the estimate of β_3^{09} in column five of Table 2.

[14] We have run the same regressions with continuous versions of both the DTI ratio and the change in debt, assuming that they both enter linearly on the RHS of (2). We have also run regressions where the change in debt is measured over the years 2003-07, rather than from 2006 to 2007. In both cases, the results are qualitatively the same as those reported in Table 4.

vs. changes in debt]

The results in Table 4 do not tell us *why* the size of the spending drop in 2007-09 is negatively related to the increase in debt from 2006 to 2007, but not to the level in 2007. But the point is that any credible explanation of the leverage-spending relationship should be consistent with these findings. As explained above, the spending normalization hypothesis lives up to this criterion. The borrowing constraints hypothesis does not.

7 Further tests of the borrowing constraints hypothesis: Heterogeneity in responses

The version of the borrowing constraints hypothesis that we presented in section 2 implicitly assumed that credit restrictions take the shape of a constraint on the size of the *stock* of debt that a household may have.[15] As demonstrated in the previous section, this version of the hypothesis is inconsistent with the data. But as emphasized by Alan et al. (2012), the tightening of credit supply during the crisis did not come in the form of calling of existing loans; rather, it took the form of limited access to new borrowing, i.e. a constraint on the *flow* of new debt. A borrowing constraints story in this form could actually be consistent with the results above: According to this version of the hypothesis, the tightening of access to new loans hit those who relied on continued borrowing, i.e. those who had a high debt-financed level of spending before the crisis, irrespective of their initial debt level.

To test the empirical validity of this version of the hypothesis, we examine whether and how the relationship between changes in debt from 2006 to 2007 and the change in spending between 2007 and 2009 varies across different subgroups of our sample. If tightened access to new credit were the main force behind the large spending reduction of high-leverage households, we would expect this relationship to be confined to households that were dependent on further credit to maintain their previous spending level. Such households are arguably more likely found among the young, those with a small stock of liquid assets, and households who experienced a large drop in income between 2007 and 2009. We test this using a variant of the specification in equation (2) in which we interact the change in debt, captured by $\Delta DTI^{high}_{i;2006-07}$, with (i) the

[15] This is also the most common way of modelling borrowing constraints in the macroeconomic literature. See for example Eggertsson & Krugman (2012)

ratio of liquid assets to income in 2007, (*ii*) the age of the household's oldest member in 2007, and (*iii*) the growth in household income from 2007 to 2009. Each of these variables is represented by a set of dummy variables indicating which decile in the distribution the household belongs to. This produces 10 coefficients on $\Delta DTI^{high}_{i;2006-07}$ in each case. These coefficients are illustrated in Figure 5: Panel A plots the coefficient on $\Delta DTI^{high}_{i;2006-07}$ across deciles of liquid-assets-to-income. There is a systematic upward trend as we move from the bottom to the top of the distribution, which is in fact what the "no-new-borrowing" version of the borrowing constraints hypothesis would predict. However, it is important to note that despite this upward trend the coefficient estimate is significantly negative across the entire distribution. In other words, although it is weaker than at the bottom of the liquid assets distribution, the correlation between leverage in 2007 and spending growth from 2007 to 2009 is negative even among those that had very large holdings of liquid assets in 2007. In our view, this fact is hard to reconcile with the hypothesis that the decline in spending among high-leverage families was forced upon them through a sudden stop to all new lending. A similar picture emerges in Panel B, where we study heterogeneity across different age groups. The figure shows that the negative correlation between debt increases and subsequent spending growth is in fact somewhat stronger among the youngest households (i.e. those in the bottom decile of the age distribution), as the above version of the borrowing constraints hypothesis would predict. But apart from that there is no systematic pattern across age groups, and the coefficient estimate is again negative throughout the entire age distribution. Finally, Panel C shows that the coefficient estimate is also negative at all levels of income growth between 2007 and 2009, with no sign of a systematic heterogeneity.

[**Figure 5 here: Heterogeneity across subgroups**]

Overall, the evidence supporting the no-new-borrowing version of the borrowing constraints hypothesis is weak. While we cannot completely reject the idea that limited access to new borrowing played some role in the decline in spending among high-leverage families, the evidence shows that it cannot be the full explanation.

8 What is the factor underlying spending normalization?

Our results so far favor the spending normalization hypothesis as the more likely explanation for the empirical relationship between leverage and spending growth during the financial crisis. In this section we take a closer look at this hypothesis, with the overall aim of answering the question raised previously: If spending nomalization is the main explanation for the unusually large spending cuts made by high-leverage households during the crisis, what can explain why these households spent and borrowed so much in the first place?

8.1 Results using other base years

A natural first step is to ask whether the negative correlation between leverage and subsequent spending growth is also present in other years. If the boom-bust pattern of spending among the high-leverage households was caused by any of the unique events that unfolded in the crisis years, then the correlation should be confined to this particular period of time - or at least be significantly stronger here than in other years. To examine whether this is the case we have estimated different variants of our baseline specification in equation (1), each time using a different base year in which the split between high-leverage and low-leverage households is made. The cut-off between these two groups is always defined as the 75th percentile in the distribution of the DTI ratio in the given base year.[16] The left-hand side of Figure 6 depicts the results for three different base years: 2004, 2007, and 2010. The 2007-version is identical to Figure 4 and is shown here only to ease comparisons. The figure shows that the patterns observed in Figure 4 is also present when using other base years: Households with high leverage in the base year spend a higher fraction of their income in this year than low-leverage households, but they reduce spending more in the following year, thus narrowing the gap between the two groups[17]. Section 6 demonstrated that in the case of 2007 as the base year, the key driver behind this pattern is the fact that a large share of the high-leverage families experienced a substantial increase in debt in the base year. The panels on the right-hand side of Figure 6 suggest that this is also the case when using other base years. To con-

[16] We have also done the same thing with a fixed cut-off, namely the 75th percentile in the DTI distribution in 2007, in all years. This does not change the results in any important ways.

[17] Figure 6 illustrates this point for only three selected years, but we get similar pictures for any other base year between 2003 and 2010.

struct these panels, we repeated the exercise explained above, only now using the change in debt in the base year as the indicator of high leverage, rather than the level of debt. In all years, spending rose sharply in the base year among those that saw a large increase in debt in this year, only to drop equally sharply in the following year.

[**Figure 6 here: Spending and leverage, various base years**]

The lesson from Figure 6 is that whatever the exact mechanism behind the leverage-spending correlation is, it is not confined to the crisis years. It is hard to reconcile this finding with the no-new-borrowing version of the borrowing constraints hypothesis, since there is no evidence to suggest that banks were unwilling to provide households with new loans in the years before the crisis. In contrast, the results in Figure 6 are fully consistent with the spending normalization hypothesis: Households that spend a lot in any given year often do so by borrowing, thus increasing their debt, but they also tend to spend less in the following year, thereby generating a negative correlation between leverage and subsequent spending growth.

Of course, the fact that the spending normalization pattern is also present in other years does not rule out that something else is also going on in the 2007-09 period, over and above the factors driving the spending normalization in other years. In that case, we would expect the negative correlation between leverage and subsequent spending growth to be stronger in this period than in other two-year periods. To test this formally, we estimate a model similar to that in equation (2), only now pooling data from all years in our analysis period and allowing the coefficient on the leverage variable to vary across years.[18] Table 5 reports results from this pooled regression. In the first column we restrict the coefficient on the leverage variable to be the same fo all base years. This yelds an estimate of -0.041, which is similar to what we found in previous sections using 2007 as the single base year. In column two we allow this coefficient to shift for base years after 2006, capturing the idea that the leverage-spending dynamics might have changed with the arrival of the financial crisis. We can firmly reject the hypothesis that the coefficient on the leverage variable is more negative after 2006;

[18]That is, we estimate the following model:
$\Delta C_{i;t,t+2} = \alpha + \phi_1 DTI^{high}_{i;t} + \phi_2 DTI^{high}_{i;t} \cdot \mathbf{year}_t + \phi_3 \mathbf{year}_t + \lambda \mathbf{X}_{i;t} + u_{it}$, where $t = 2003, ..., 2009$. This version uses the DTI ratio in year t as the measure of leverage, but we have also estimated the model using the change in debt from year $t-1$ to t. The results from this regression are reported in appendix table A.2. The basic point made in this section, namely that the coefficient on leverage is not significantly more negative in the crisis years than in other years, is not affected by this alternative choice of leverage measure.

if anything, it seems that the opposite is true. Finally, in column three we allow the coefficient on the leverage variable to vary freely across years. We find no systematic differences across base years in the relationship between leverage and subsequent spending growth. This further strengthens the impression that our results reflect a general spending normalization pattern that is not specific to the financial crisis years.

[Table 5 here: Pooled regression of changes in spending on debt-to-income ratio]

8.2 Results for car purchases

Having established that the spending normalization pattern is present in all years in our analysis period, we now focus on potential explanations for what might lie behind this pattern. That is, what is behind the temporary spikes in spending and borrowing that generate the negative correlation between leverage and subsequent spending growth? In the introduction we mentioned two candidates: Mean reversion in expectations about future income and/or house prices, and the timing of purchases of durable consumption goods. To this, we now add a third potential candidate: Measurement error. Note that due to the way our imputed measure of spending is constructed, any noise in our measure of debt will be transmitted directly on to our spending measure. If the noise term is serially uncorrelated, a large positive error in the measure of debt in the base year will, on average, show up in the form of (i) large increases in debt and spending in the base year, and (ii) a large subsequent reduction in spending in the following year when a new realization of the noise term is drawn. As shown in previous sections, this is exactly the pattern that we observe for the high-leverage families.

It is important to note that this particular problem from measurement error only arises because of the direct link between changes in (measured) debt and our imputed measure of spending.[19] To gauge the influence of measurement error, we therefore estimate a regression model using an alternative indicator of spending that is not

[19] Also note that measurement error in any of the other variables used in the imputation of spending cannot account for the observed relationship between leverage and spending growth. Measurement error in any of these variables, will lead to mean reversion in imputed spending, but there is no reason to expect that it would produce a negative correlation with the initial debt level.

influenced by measurement error in the debt variable: Car purchases. We once again estimate an equation like (1), only this time with an indicator for whether the household bought a new car as the dependent variable.[20] Figure 7 illustrates the results from this regression. Panel A shows the average predicted values when the DTI ratio in 2007 is used as the leverage measure on the right-hand side of the equation, while panel B shows the corresponding results when the change in debt from 2006 to 2007 is used. The results look remarkably similar to those for the imputed measure of spending. The share of households purchasing a new car is much higher among the high-leverage families than among the low-leverage families in 2007 and in the preceding years, conditional on other household characteristics, but the gap then closes completely in 2008. Looking at the panel B, we see that there is a large spike in car purchases in 2007 among the households with a large increase in debt from 2006 to 2007, just as it was the case for the imputed spending measure. This is yet another illustration of the spending normalization pattern that we have documented in this paper: Many of the households that bought a new car in 2007 financed the purchase by borrowing. This brought large increases in both spending and debt, leading to a high debt level at the end of 2007 and a large decline in spending in the subsequent year.

The analysis of car purchases shows that the correlation between pre-crisis leverage and subsequent spending reductions is driven by real spending decisions; it is *not* an artefact of our imputation method coupled with measurement error. At the same time, the analysis shows that purchases of durable consumption goods, of which cars is the most significant one, is an important element in explaining the spending normalization pattern we have uncovered.

Can the observed leverage-spending correlation then be entirely explained by differences in the timing of car purchases? To get an idea of the answer to this question, note in panel B that the share of households with large debt increases that purchased a new car in 2007 was much higher than for other households, but it was still only about 15 percent. For the remaining 85 percent of these households, something else must have been behind the large increase in debt and subsequent drop in spending.[21] A downwards adjustment in expectations about future income and/or house prices

[20] To be precise, the dependent variable is equal to one in year t if the household purchased a new car *and* increased the number of cars in their possession by at least one during year t.

[21] To support this point, we have also estimated equation (1) for those households that did not buy a new car at any point during our analysis period. The spending normalization pattern is also present among this group of households, suggesting that the timing of car purchases cannot fully account for the observed correlation between leverage and spending growth.

among households that had been highly optimistic in the pre-crisis years is a plausible explanation. But unfortunately, a lack of data on such expectations means that a further exploration of this theory is outside the scope of this paper.

9 Concluding remarks

This article uses administrative register data on almost 500,000 Danish households to examine whether and how the level of household debt before the recent financial crisis affected spending during the crisis. We find a strong and economically significant negative correlation between a household's debt-to-income ratio in 2007 and the change in its spending from 2007 to 2009. Similar results have been found in other countries.

Our results shed light on the possible mechanisms behind this correlation. Exploiting the panel structure of our data, we document that the high-leverage households spent a much larger fraction of their income than low-leverage households in the years leading up to crisis and that a substantial part of their debt was incurred between 2006 and 2007. We find evidence that it is in fact this larger *increase* in debt in the years before the crisis, not the higher level of debt, that drives the correlation between high leverage and large spending reductions.

These results are highly problematic for any hypothesis that predicts a negative impact on spending from a high level of debt, including the widely-held belief that the large spending reductions in 2007-09 were the result of levered households hitting an upper limit on the amount of debt they could hold. While we cannot refute the possibility that some highly leveraged households faced binding borrowing constraints during the crisis, the evidence presented in this paper shows that this cannot be the main explanation behind the observed relationship between leverage and spending in the crisis years. Rather, the build-up of debt among some households in the years preceding the crisis was the result of unusually high spending levels in these years, prompting a large subsequent reduction back to normal levels. We refer to this pattern as a *spending normalization* and we document that the same pattern can be found in other years not plagued by financial and macroeconomic turmoil.

The analysis augments our understanding of the roots and causes of the decline in aggregate consumption during the crisis. In our view, the analysis presented is therefore also relevant to policy makers concerned with safeguarding macroeconomic stability. Our results suggest that such policymakers should pay attention to to rapid *increases* in household borrowing, rather than just the level of debt in the household sector. Such rapid increases could serve as a warning indicator signifying a heightened risk of a relapse in consumption, especially if they are driven by unusually high household spending, and reining them in could help obtaining a more stable macroeconomic development. In contrast, our results do not provide empirical justification for debt relief policies that target under-water borrowers, since we find no evidence that it is a high level of debt in itself that prompts these borrowers to cut back on spending during times of financial unrest.

Figure 1: Households' agggregate debt-to-income ratio and consumption

The figure shows the development in the aggregate debt-to-income ratios of the Danish households (LHS) along with aggregate seasonally adjusted quarterly consumption in 2010 billion DKK (RHS). The data series are obtained from Statistics Denmark. The red vertical line marks 2007Q4.

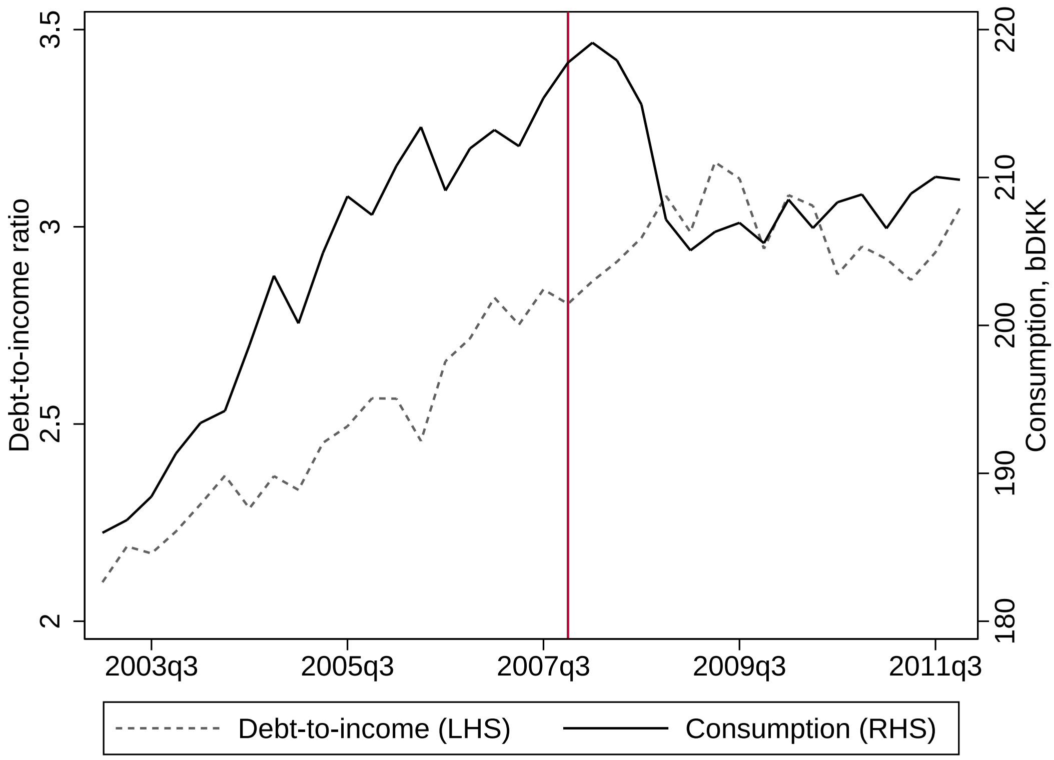


Figure 2: Histogram for debt-to-income ratios, 2007

The figure shows the distribution of debt-to-income ratio in 2007. The debt-to-income ratios are winsorized at the 1st and 99th percentiles.

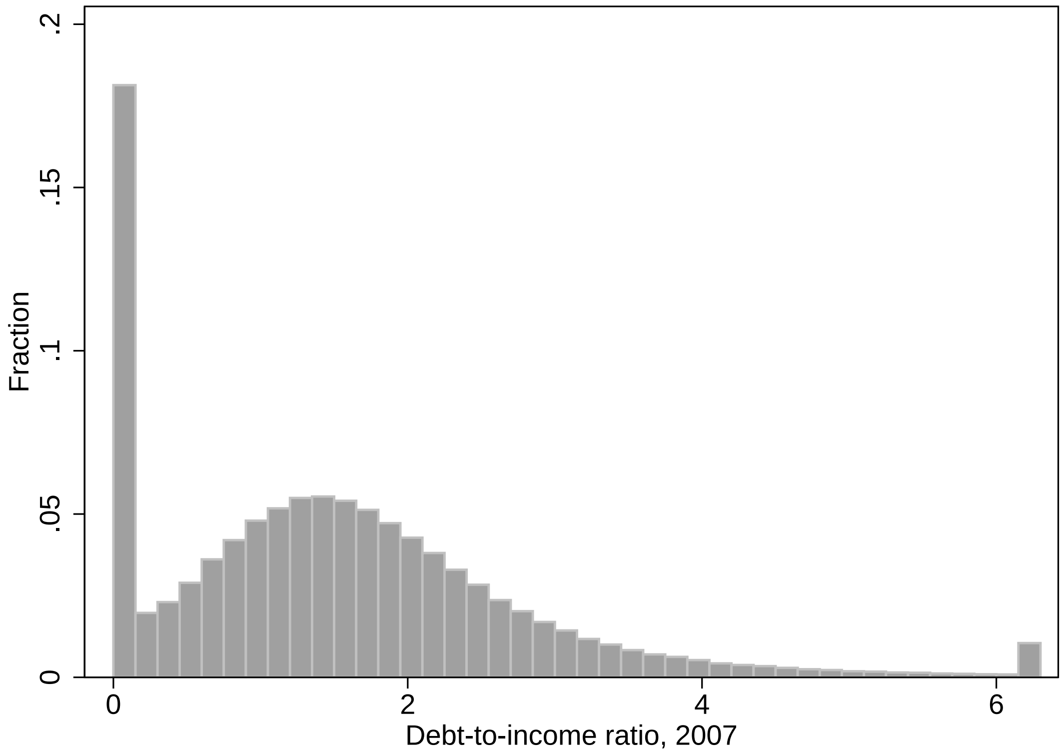


Figure 3: DTI ratio in 2007 and change in spending from 2007-2009

The figure shows results from a local polynomial regression of the change in imputed spending from 2007-2009 on the debt-to-income ratio measured in 2007 among households in which the oldest member was 45 years old in 2007. The change in spending is measured relative to household pre-tax income in 2007. Both variables are winsorized at the 1st and 99th percentiles. The solid line represents the point estimate while the shaded area represents the 95 percent confidence intervals.

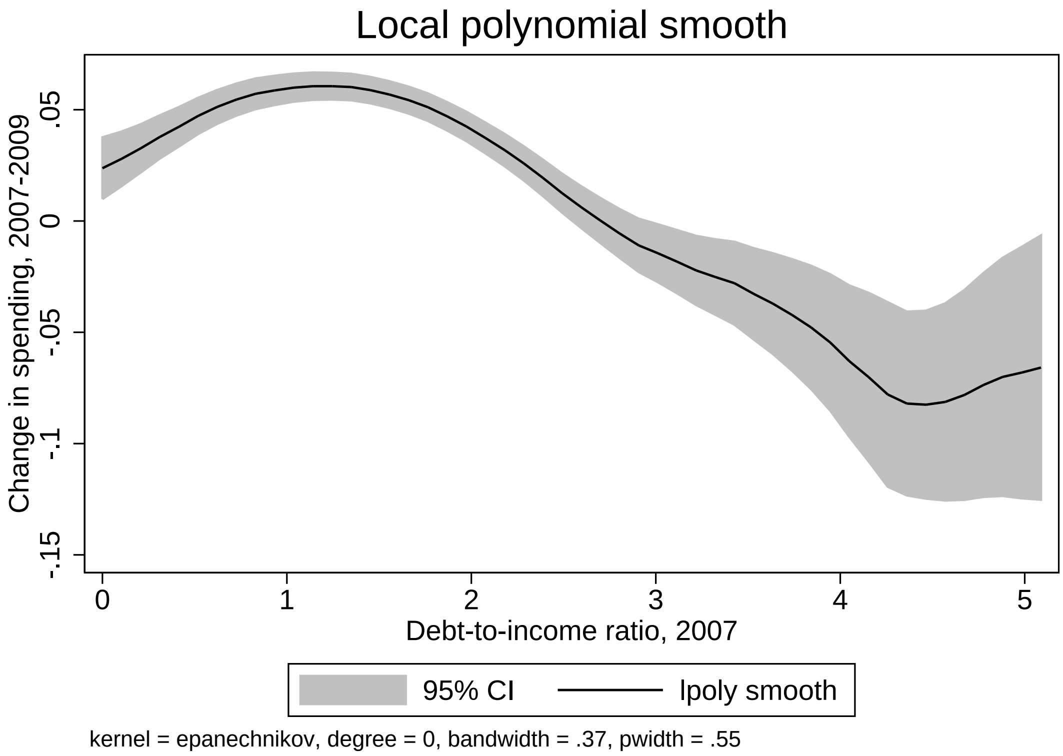


Figure 4: Predicted levels of spending, high vs. low leverage

The figure shows the average predicted values from the regression estimating equation (1) with the full set of controls when $DTI^{High}_{i,2007} = 1$ *(black) and* $DTI^{Low}_{i,2007} = 1$ *(dashed), using actual values for all other explanatory variables. 95 percent confidence intervals are marked by upper and lower bars around each point estimate.*

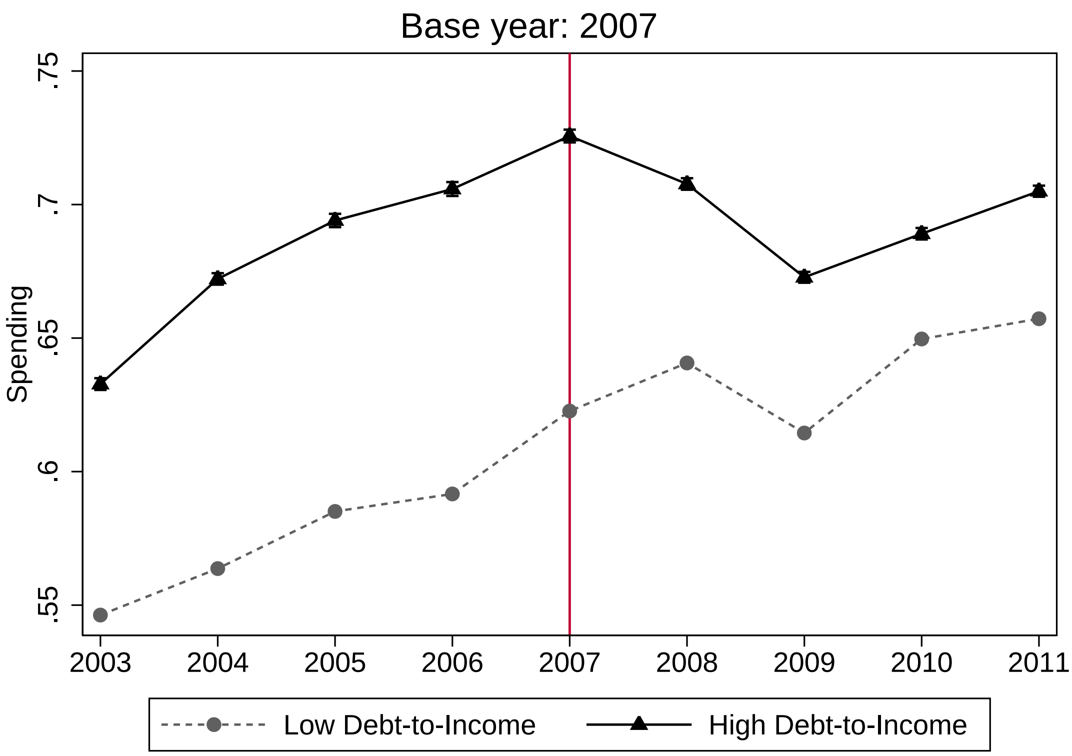
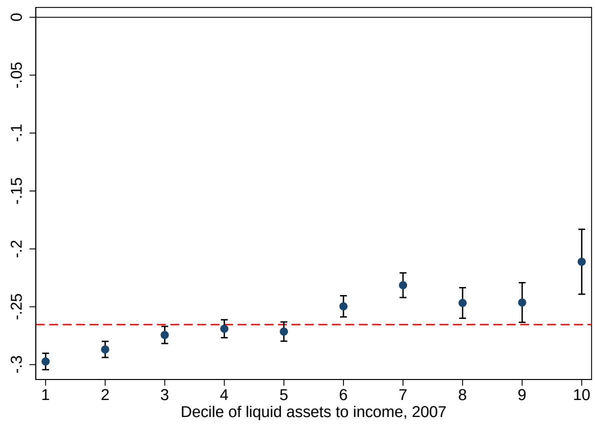


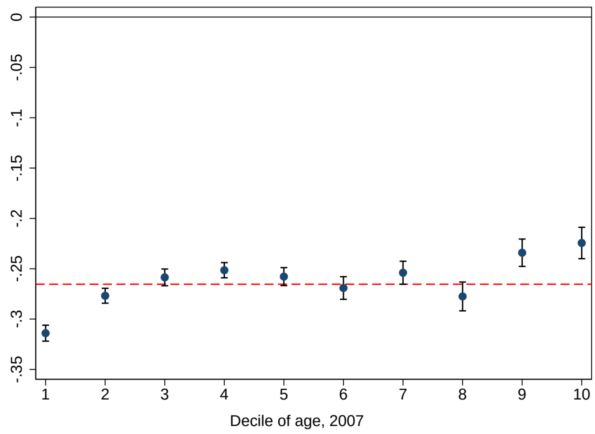
Figure 5: Heterogeneity across subgroups

The figure shows estimation results for an extended version of equation (2) in which the change in debt from 2006 to 2007 is interacted with selected covariates. Each panel shows the estimated coefficent on the change in debt at different levels of the interacted covariates. The interacted covariates are the decile of liquid assets-to-income (Panel A), the decile of the age of the oldest family member (Panel B), and the decile of income growth from 2007 to 2009 (Panel C). The red dashed line indicates the estimated coefficient on the change in debt with no interactions, i.e. the estimated average partial effect of $\Delta DTI^{High}_{i,2007}$ *obtained in Table 4, column 2.*

Panel A



Panel B



Panel C

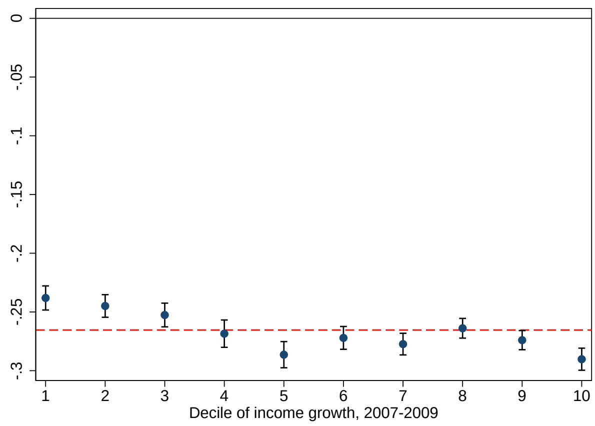


Figure 6: Spending and leverage, various base years

The figure presents the results from estimating equation (1) with various base years. The dependent variable is spending in a given year relative to income in the base year. Leverage is measured both in levels ($HighDTI_{base}$, left panels) and changes ($High\Delta DTI_{base}$, right panels). The figures show the average predicted values when leverage is low vs. high, using actual values for the full set of other explanatory variables.

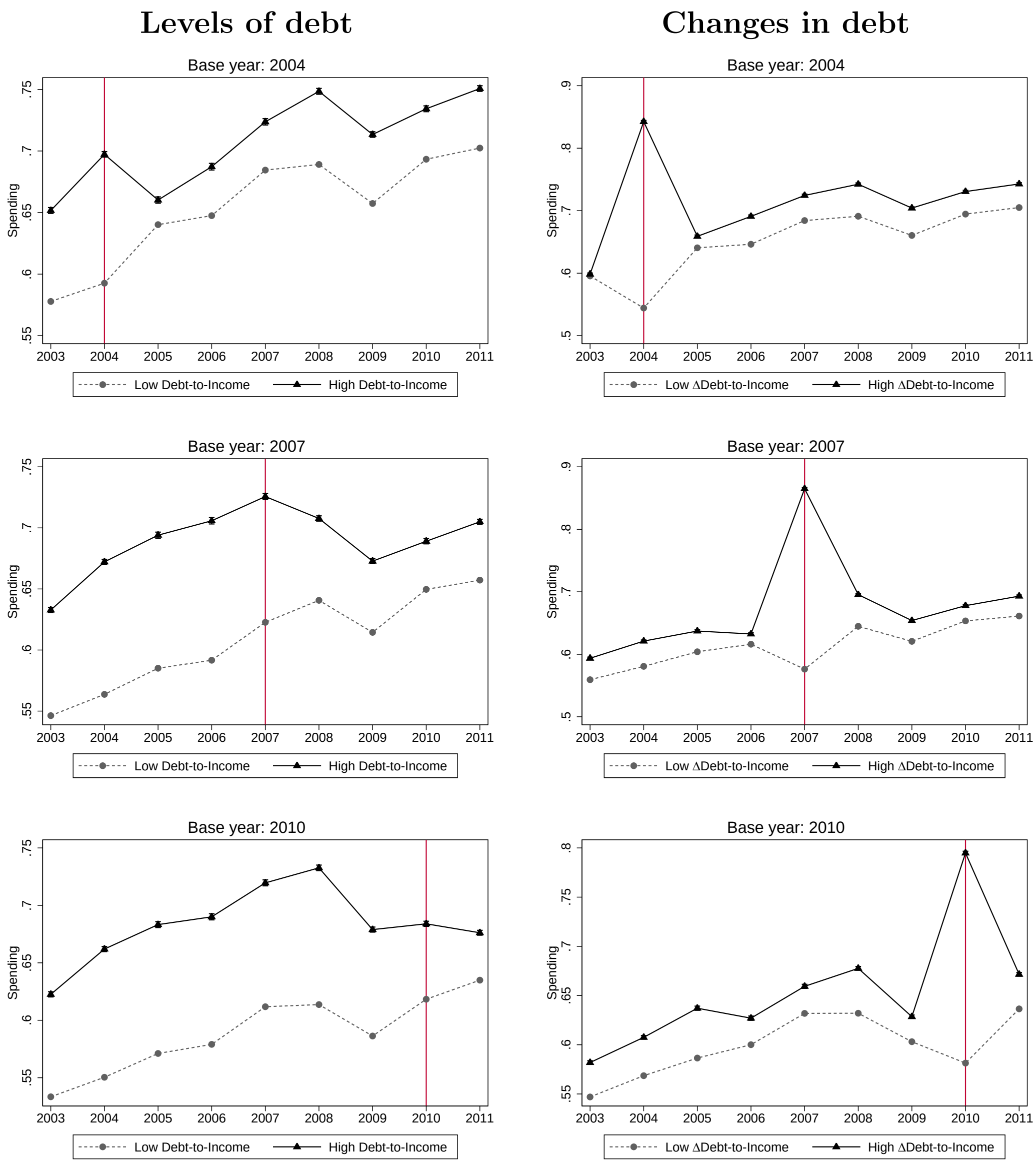


Figure 7: Car purchases and leverage

The figure presents the results from estimating equation (1) with various base years. The dependent variable an indicator for the household buying a new car in a given year. Leverage is measured both in levels ($HighDTI_{base}$, left panels) and changes ($High\Delta DTI_{base}$, right panels). The figures show the average predicted values when leverage is low vs. high, using actual values for the full set of other explanatory variables.

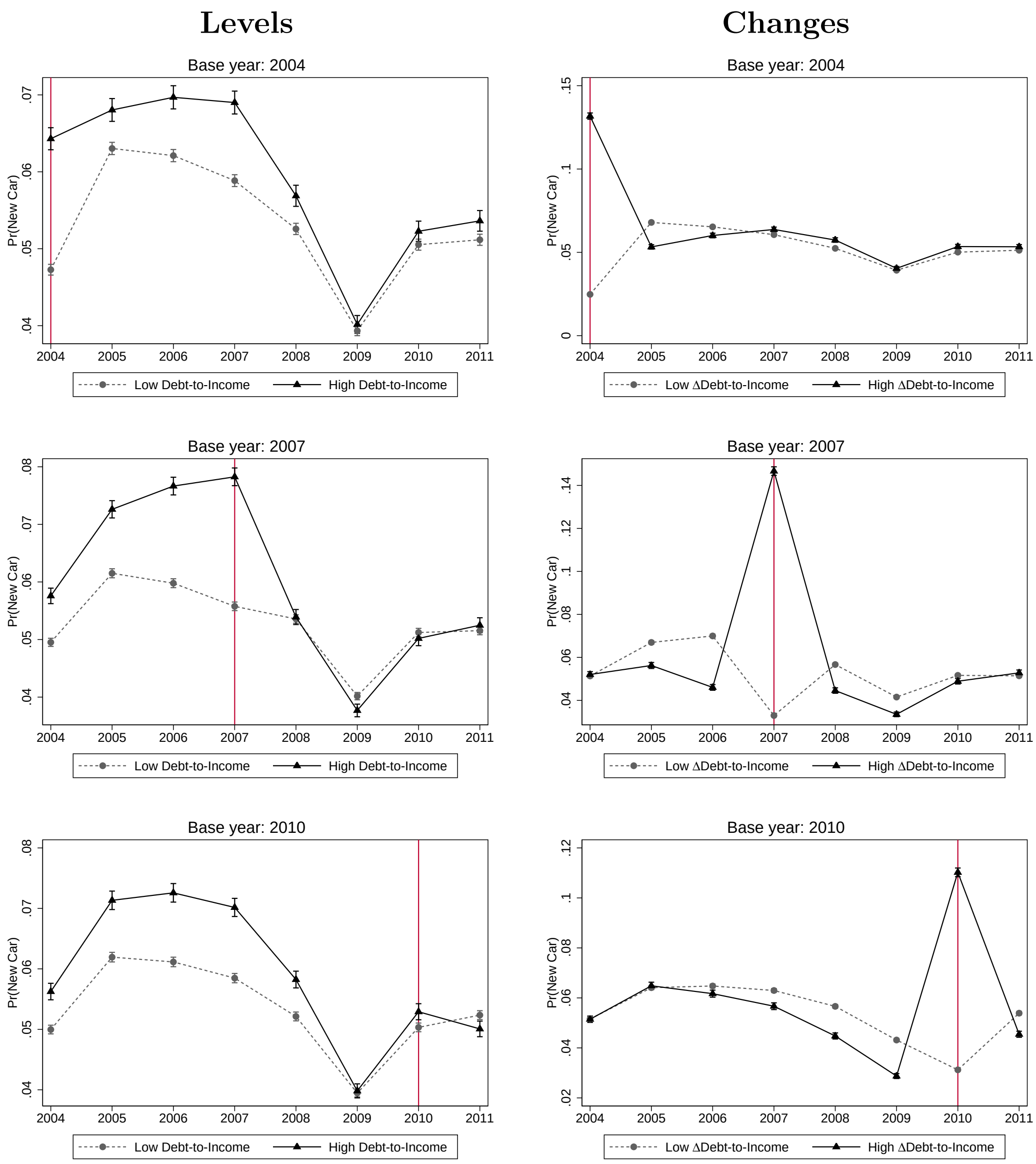


Table 1: Descriptive statistics, 2007

The table presents summary statistics for the 492,194 households in our analysis sample. Low DTI $_{2007}$ and High DTI $_{2007}$ refer to households with a debt-to-income ratio in 2007 ratio below and above the 75th percentile, respectively. The variables are winsorized at the 1st and 99th percentiles before calculating the means.

	Low DTI$_{2007}$	High DTI$_{2007}$
	Mean/ (Std.dev.)	Mean/ (Std.dev.)
Debt-to-income	0.96	3.17
	(0.70)	(1.04)
Disposable income, DKK	320,003	256,108
	(143,207)	(121,806)
Total income, DKK	546,283	484,731
	(284,727)	(253,126)
Total Liabilities, DKK	578,065	1,447,061
	(524,715)	(713,505)
Total Assets, DKK	2,351,834	2,630,432
	(1,452,939)	(1,484,565)
Housing value, DKK	1,950,469	2,364,601
	(1,143,707)	(1,247,126)
Net wealth, DKK	1,767,270	1,179,480
	(1,466,031)	(1,266,720)
Number of kids	0.59	0.69
	(0.94)	(1.01)
Age of eldest family member	58.17	55.87
	(12.92)	(13.46)
Years since moving to address	24.23	18.70
	(18.09)	(15.99)
Imputed spending, DKK	314,987	345,909
	(197,755)	(251,546)
Growth in houseprice, 2005-2007	0.173	0.166
	(0.16)	(0.17)
Growth in total income 2005-2007	0.070	0.037
	(0.17)	(0.18)
Growth in houseprice, 2007-2009	-0.114	-0.130
	(0.15)	(0.16)
Growth in total income 2007-2009	0.015	0.057
	(0.16)	(0.17)
	369,146	123,048

Table 2: Regression results for equation (1)

*The table presents results for estimation of equation (1). The dependent variable is imputed spending relative to pre-tax income in 2007, winsorized at the 1st and 99th percentiles within each year. We only report estimates for the coefficient on High DTI $_{2007}$ x year $_{2009}$, where High DTI $_{2007}$ is the indicator for the household having a debt-to-income ratio above the 75th percentile in 2007, and year $_{2009}$ is an indicator for the year 2009. This coefficient reflects the difference in the change in spending from 2007-2009 between high-leverage households and low-leverage households. All control variables are measured in 2007, and each is included by itself as well as interacted with a full set of year dummies. Standard errors are clustered at the household level and reported in parentheses. *, ** and *** denote significance at the 5, 1, and 0.1 percent level, respectively.*

	(1)	(2)	(3)	(4)	(5)
High DTI$_{2007}$ x year$_{2009}$	-0.0324***	-0.0266***	-0.0341***	-0.0361***	-0.0447***
	(0.0016)	(0.0016)	(0.0016)	(0.0017)	(0.0017)
Year dummies	Yes	Yes	Yes	Yes	Yes
Municipality x Year	No	Yes	Yes	Yes	Yes
Age of oldest member x Year	No	No	Yes	Yes	Yes
Number of children x Year	No	No	No	Yes	Yes
No. of years since moving in x Year	No	No	No	Yes	Yes
Higher education x Year	No	No	No	Yes	Yes
Retirees x Year	No	No	No	Yes	Yes
Decile of Income x Year	No	No	No	No	Yes
Decile of Net wealth to income x Year	No	No	No	No	Yes
Decile of Liquid assets to income x Year	No	No	No	No	Yes
Observations	4,428,888	4,428,888	4,428,888	4,428,888	4,428,888
R-squared	0.0312	0.0354	0.0787	0.0940	0.1303

Table 3: Changes in debt from 2006-2007

The table shows descriptive statistics for the change in debt from 2006 to 2007, measured relative to household income in 2007 (ΔDTI_{2007}). The statistics are reported separately for households with a debt-to-income ratio above the 75th percentile in 2007 (High DTI_{2007}) and those below (Low DTI_{2007}). High ΔDTI_{2007} denotes that ΔDTI_{2007} is above 0.019 (the 75th percentile).

	Low DTI_{2007}	High DTI_{2007}
Mean $\Delta DTI_{2006\text{-}2007}$	-0.003	0.179
Share with High $\Delta DTI_{2006\text{-}2007}$	0.17	0.48
Mean $\Delta DTI_{2006\text{-}2007}$ given High $\Delta DTI_{2006\text{-}2007}$	0.24	0.49

Table 4: Regression results for equation (2)

*The table presents results for estimation of equation (2). The dependent variable is the change in imputed spending from 2007-2009 relative to pre-tax income in 2007, winsorized at the 1st and 99th percentiles. High DTI $_{2007}$ is the indicator for the household having a debt-to-income ratio above the 75th percentile in 2007. High ΔDTI $_{2007\text{-}2006}$ is an indicator for the household having a change in debt to income ratio from 2006-2007 above the 75th percentile. High ΔDTI $_{2007\text{-}2003}$ is an indicator for the household having a change in debt to income ratio from 2003-2007 above the 75th percentile. All control variables are measured in 2007, and each is included by itself as well as interacted with a full set of year dummies. Standard errors are reported in parentheses. *, ** and *** denote significance at the 5, 1, and 0.1 percent level, respectively.*

	(1)	(2)	(3)	(5)	(6)	(7)
	Change from 2006-2007			Change from 2003-2007		
High DTI$_{2007}$	-0.0448***		0.0224***	-0.0448***		0.0211***
	(0.0017)		(0.0017)	(0.0017)		(0.0019)
High ΔDTI$_{2006\text{-}2007}$		-0.2601***	-0.2654***			
		(0.0016)	(0.0016)			
High ΔDTI$_{2003\text{-}2007}$					-0.1267***	-0.1363***
					(0.0016)	(0.0018)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Municipality x Year	Yes	Yes	Yes	Yes	Yes	Yes
Age of oldest member x Year	Yes	Yes	Yes	Yes	Yes	Yes
Number of children x Year	Yes	Yes	Yes	Yes	Yes	Yes
No. of years since moving in x Year	Yes	Yes	Yes	Yes	Yes	Yes
Higher education x Year	Yes	Yes	Yes	Yes	Yes	Yes
Retirees x Year	Yes	Yes	Yes	Yes	Yes	Yes
Decile of Income x Year	Yes	Yes	Yes	Yes	Yes	Yes
Decile of Net wealth to income x Year	Yes	Yes	Yes	Yes	Yes	Yes
Decile of Liquid assets to income x Year	Yes	Yes	Yes	Yes	Yes	Yes
Observations	492,194	492,194	492,194	492,194	492,194	492,194
R-squared	0.0488	0.0972	0.0976	0.0488	0.0592	0.0594

Table 5: Pooled regression of changes in spending regressed on DTI

*The table presents results from the regression pooling the years 2003-2009, where the dependent variable is the change in imputed spending from t to t+2, relative to pre-tax income in t, winsorized at the 1st and 99th percentiles. In each year the cut-off for the High DTI $_{\text{base}}$ is defined as the 75th percentile in the distribution of the DTI ratio in the given base year. All control variables are measured in the base year and each is included by itself as well as interacted with a full set of year dummies. Standard errors are clustered at the household level and reported in parentheses. *, ** and *** denote significance at the 5, 1, and 0.1 percent level, respectively.*

	(1)	(2)	(3)
High DTI$_{\textit{base year}}$	-0.0411***	-0.0444***	
	(0.0004)	(0.0005)	
High DTI$_{\textit{base year}}$ x *I[base year>2006]*		0.0077***	
		(0.0008)	
High DTI$_{\textit{base year=2003}}$			-0.0059***
			(0.0012)
High DTI$_{\textit{base year=2004}}$			-0.0409***
			(0.0012)
High DTI$_{\textit{base year=2005}}$			-0.0775***
			(0.0012)
High DTI$_{\textit{base year=2006}}$			-0.0510***
			(0.0012)
High DTI$_{\textit{base year=2007}}$			-0.0212***
			(0.0012)
High DTI$_{\textit{base year=2008}}$			-0.0538***
			(0.0011)
High DTI$_{\textit{base year=2009}}$			-0.0346***
			(0.0010)
Year dummies	Yes	Yes	Yes
Municipality x Year	Yes	Yes	Yes
Age of oldest member x Year	Yes	Yes	Yes
Number of children x Year	Yes	Yes	Yes
No. of years since moving in x Year	Yes	Yes	Yes
Higher education x Year	Yes	Yes	Yes
Retirees x Year	Yes	Yes	Yes
Decile of Income x Year	Yes	Yes	Yes
Decile of Net wealth to income x Year	Yes	Yes	Yes
Decile of Liquid assets to income x Year	Yes	Yes	Yes
Observations	3,444,619	3,444,619	3,444,619
R-squared	0.0267	0.0267	0.0276

Appendix Table A.1: Regression results for equation (1)

*The table presents results for estimation of equation (1). The dependent variable is imputed spending relative to pre-tax income in 2007, winsorized at the 1st and 99th percentiles. All control variables are measured in 2007, and each is included by itself as well as interacted with a full set of year dummies. Standard errors are clustered at the household level and reported in parentheses. *, ** and *** denote significance at the 5, 1, and 0.1 percent level, respectively.*

	(1)	(2)	(3)	(4)	(5)
High DTI_{2007} x $year_{2003}$	-0.0058***	-0.0032*	-0.0042**	-0.0130***	-0.0165***
	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0018)
High DTI_{2007} x $year_{2004}$	0.0154***	0.0183***	0.0186***	0.0143***	0.0055***
	(0.0016)	(0.0017)	(0.0017)	(0.0017)	(0.0018)
High DTI_{2007} x $year_{2005}$	0.0177***	0.0189***	0.0197***	0.0169***	0.0060***
	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0019)
High DTI_{2007} x $year_{2006}$	0.0273***	0.0278***	0.0272***	0.0246***	0.0112***
	(0.0018)	(0.0019)	(0.0019)	(0.0019)	(0.0020)
High DTI_{2007} x $year_{2008}$	-0.0425***	-0.0397***	-0.0449***	-0.0473***	-0.0360***
	(0.0016)	(0.0017)	(0.0017)	(0.0017)	(0.0018)
High DTI_{2007} x $year_{2009}$	-0.0324***	-0.0266***	-0.0341***	-0.0361***	-0.0447***
	(0.0016)	(0.0016)	(0.0016)	(0.0017)	(0.0017)
High DTI_{2007} x $year_{2010}$	-0.0678***	-0.0616***	-0.0674***	-0.0699***	-0.0636***
	(0.0016)	(0.0016)	(0.0017)	(0.0017)	(0.0018)
High DTI_{2007} x $year_{2011}$	-0.0579***	-0.0510***	-0.0580***	-0.0595***	-0.0552***
	(0.0016)	(0.0016)	(0.0016)	(0.0017)	(0.0017)
High DTI_{2007}	0.1242***	0.1258***	0.1299***	0.1294***	0.1030***
	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
$Year_{2003}$	-0.0790***	-0.0739***	0.3985***	0.0385	0.0081
	(0.0006)	(0.0041)	(0.0770)	(0.0642)	(0.0635)
$Year_{2004}$	-0.0614***	-0.0689***	-0.1759***	-0.2075***	-0.2080***
	(0.0006)	(0.0039)	(0.0436)	(0.0525)	(0.0514)
$Year_{2005}$	-0.0405***	-0.0455***	-0.1679***	-0.1920***	-0.1777***
	(0.0006)	(0.0043)	(0.0512)	(0.0606)	(0.0599)
$Year_{2006}$	-0.0351***	-0.0302***	-0.0467	0.0151	0.0508
	(0.0007)	(0.0044)	(0.0384)	(0.0509)	(0.0506)
$Year_{2008}$	0.0197***	0.0133***	0.0094	-0.0180	-0.1008*
	(0.0006)	(0.0041)	(0.0536)	(0.0618)	(0.0612)
$Year_{2009}$	-0.0113***	-0.0264***	-0.0513	-0.0809	-0.0925
	(0.0007)	(0.0041)	(0.0506)	(0.0585)	(0.0567)
$Year_{2010}$	0.0281***	0.0107***	-0.0801*	-0.1514***	-0.2153***
	(0.0006)	(0.0040)	(0.0481)	(0.0563)	(0.0557)
$Year_{2011}$	0.0352***	0.0129***	0.0298	-0.0609	-0.1137**
	(0.0006)	(0.0040)	(0.0517)	(0.0595)	(0.0578)
Year dummies	Yes	Yes	Yes	Yes	Yes
Municipality x Year	No	Yes	Yes	Yes	Yes
Age of oldest member x Year	No	No	Yes	Yes	Yes
Number of children x Year	No	No	No	Yes	Yes
No. of years since moving in x Year	No	No	No	Yes	Yes
Higher education x Year	No	No	No	Yes	Yes
Retirees x Year	No	No	No	Yes	Yes
Decile of Income x Year	No	No	No	No	Yes
Decile of Net wealth to income x Year	No	No	No	No	Yes
Decile of Liquid assets to income x Year	No	No	No	No	Yes
Observations	4,428,888	4,428,888	4,428,888	4,428,888	4,428,888
R-squared	0.0312	0.0354	0.0787	0.0940	0.1303

Appendix Table A.2: Pooled regression of change in spending regressed on ΔDTI

*The table presents results from the regression pooling the years 2003-2009, where the dependent variable is the change in imputed spending from t to t+2, relative to pre-tax income in t, winsorized at the 1st and 99th percentiles. In each year the cut-off for the High ΔDTI_{base} is defined as the 75th percentile in the distribution of the ΔDTI ratio from t-1 to t, the base year. All control variables are measured in the base year and each is included by itself as well as interacted with a full set of year dummies. Standard errors are clustered at the household level and reported in parentheses. *, ** and *** denote significance at the 5, 1, and 0.1 percent level, respectively.*

	(1)	(2)	(3)
High $\Delta DTI_{base\ year}$	-0.2009***	-0.2154***	
	(0.0004)	(0.0006)	
High $\Delta DTI_{base\ year} \cdot I[year>2006]$		0.0340***	
		(0.0008)	
High $\Delta DTI_{base\ year=2003}$			-0.1727***
			(0.0010)
High $\Delta DTI_{base\ year=2004}$			-0.2079***
			(0.0011)
High $\Delta DTI_{base\ year=2005}$			-0.2537***
			(0.0011)
High $\Delta DTI_{base\ year=2006}$			-0.2272***
			(0.0011)
High $\Delta DTI_{base\ year=2007}$			-0.1823***
			(0.0011)
High $\Delta DTI_{base\ year=2008}$			-0.1949***
			(0.0010)
High $\Delta DTI_{base\ year=2009}$			-0.1668***
			(0.0010)
Year dummies	Yes	Yes	Yes
Municipality x Year	Yes	Yes	Yes
Age of oldest member x Year	Yes	Yes	Yes
Number of children x Year	Yes	Yes	Yes
No. of years since moving in x Year	Yes	Yes	Yes
Higher education x Year	Yes	Yes	Yes
Retirees x Year	Yes	Yes	Yes
Decile of Income x Year	Yes	Yes	Yes
Decile of Net wealth to income x Year	Yes	Yes	Yes
Decile of Liquid assets to income x Year	Yes	Yes	Yes
Observations	3,444,619	3,444,619	3,444,619
R-squared	0.0917	0.0922	0.0933

Data appendix

Statistical definition of a family

The unit of analysis in this article is the family, as defined by Statistics Denmark. By this definition, a family consists of either one or two adults and any children living at home. Two adults are regarded as members of the same family if they are living together and meet at least one of the following criteria:

1. Are married to each other or have entered into a registered partnership
2. Have at least one common child registered in the Civil Registration System (the CPR)
3. Are of opposite sex and have an age difference of 15 years or less, are not closely related and live in a household with no other adults

Adults living at the same address who do not meet at least one of the above criteria are regarded as singles. Children living with their parents are regarded as members of their parents' family if they are under 25 years old, have never been married or entered into a registered partnership and do not themselves have children who are registered in the CPR. A family meeting these criteria can consist of only two generations. If three or more generations live at the same address, the two younger generations are considered one family, while the members of the eldest generation constitute a separate family.

Variables used for imputing non-housing consumption

Non-housing consumption is imputed as follows:

$$\begin{aligned} Consumption \;=\; & \; Disposable\ income \\ & -contributions\ to\ privately\ administered\ pension\ schemes \\ & -change\ in\ value\ of\ assets\ (excl.\ pensions\ and\ real\ property) \\ & +change\ in\ liabilities \end{aligned}$$

Disposable income is gross personal income (including wage- and capital income and all government transfers) plus one-off payments from capital pensions and publicly administered pension schemes, less all taxes, interest payments, alimony, and repaid social benefits. Note that the rental value of owner-occupies housing is not included in our measure of disposal income. Neither are contributions to employer-administered pension schemes. These are tax-deductable and, unlike contributions to privately administered pension schemes, they are paid directly by employers and do not enter the family's cash-flow. Hence, only contributions to privately administered schemes need to be subtracted in the imputation.

The change in the value of assets is calculated as the sum of changes in bank deposits, the market value of bonds and mortgage deeds, the (adjusted) market value of stocks, and the value of foreign assets (financial as well as real). In most cases, the value of foreign assets is self-reported. The change in the market value of stocks is adjusted for price changes in the following way:

$$\widetilde{\Delta v_t} = \Delta v_t - v_{t-1} \cdot p_t$$

where Δv_t is the actual change in the value of stocks over the year, v_{t-1} is the value of stocks at the beginning of the year, and Δp_t is the relative change in average stock prices over the year, as measured by the C20 index of the Copenhagen Stock Exchange. Thus, the adjustment term in the equation above is equal to the capital gain that the family would have received if i) they did not buy or sell stocks over the year, and ii) the price of their stock portfolio moved in parallel with the overall price development in the stock market over the year.

The change in liabilities is calculated as the sum of changes in debt owed to specialized mortgage banks, debt to universal (i.e. non-specialized) banks, debt raised through mortgage deeds held by non-bank lenders, and debt owed to foreign lenders. Debt owed to central and local governments, pension funds, and insurance companies is also included in total liabilities. Any other debt, e.g. debt owed to private individuals, is not included. Debt owed to specialized mortgage banks constitutes the lion's share of Danish households' total debt. Loans from these banks are financed through issuance of mortgage bonds with maturity up to 30 years, and the remaining debt on such loans is reported at the market value of the underlying bonds. This introduces an additional source of measurement error in our imputed measure of consumption, since changes in debt owed to mortgages banks may stem from fluctuations in bond prices (i.e. capital gains), as well as from payment of the principal (i.e. saving).

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Chapter 4

The Consumption Effects of the 2007-2008 Banking Crisis

The Consumption Effects of the 2007-2008 Financial Crisis: Evidence from Household-level data*

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Abstract

Did the financial crisis spread from distressed banks to households through a contraction of the credit supply? We study this question with a dataset that contains observations on all accounts in Danish banks as well as comprehensive information about account holders and banks. We show that banks exposed to the financial crisis reduced their credit supply significantly and that their customers reduced both borrowing and consumption relative to customers in non-exposed banks. Our results further suggest that heterogeneous costs of switching banks at the level of customers may explain why they did not fully compensate with credit from other sources when their banks tightened credit.

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1 Introduction

The global banking crisis in 2007-2008 was followed by the Great Recession where corporate investment, employment and household consumption fell sharply in virtually all developed countries. This pattern of a financial bust followed by a severe contraction of the real economy has played out numerous times over the last centuries (Reinhart & Rogoff, 2009).

A central question faced by economists trying to grasp the dynamics of the Great Recession is whether the crisis in the banking sector was transmitted to the real economy through a reduction in credit supply. The tightening of credit by banks in financial distress is one among several possible explanations why firms stopped investing and households reduced consumption in the aftermath of the financial crisis. Understanding the strength of this transmission mechanism is important for guiding policy responses to future financial crises. To the extent that tightened credit is responsible for the transmission to the real economy, it may be possible to contain a financial crisis by securing credit to the firms and households served by banks in distress.

A substantial body of evidence demonstrates that adverse shocks to banks can spill over into the corporate sector through the credit supply channel. Several papers show that the total borrowing of firms drops when their bank relation is in distress (Khwaja & Mian, 2008; Jiménez et al., 2014). This finding highlights that firms are unable to fully compensate with credit from other sources when their existing bank relation tightens its credit supply. In these circumstances, firms are induced to slash investment (Klein et al., 2002; Dwenger et al., 2015) and reduce employment (Chodorow-Reich, 2014; Bentolila et al., 2015)

The literature linking financial crises to household outcomes through the credit supply channel is much smaller and has produced somewhat mixed results. Two papers make a compelling case that banks with high exposure to the 2007-2008 financial crisis contracted the supply of credit to households in its aftermath (Ramcharan et al., 2015; Puri et al., 2011). While this finding suggests that the credit supply channel can explain part of the drop in demand for housing, automobiles and other consumption items after the financial crisis, it has the limitation that only bank-level and not household-level outcomes are analyzed. Hence, unlike the firm evidence discussed above, these studies cannot determine if households were able to compensate with credit from other sources and thus maintain their desired level of consumption. One recent paper addresses this issue by combining bank and household survey data from

Canada and concludes that there was no effect of the financial crisis on household consumption through the credit supply channel (Damar et al., 2014).

In this paper, we provide new evidence on how adverse shocks to banks affect the borrowing and consumption of households through the credit supply channel. We leverage a dataset from the Danish tax authorities, which contains information about the balance of all loan accounts in Danish financial institutions for the period 2003-2011, and add comprehensive information about account holders from administrative records as well as detailed balance sheet information about banks. We can thus track the borrowing of households in each bank and assess the extent to which they reduced total borrowing or compensated with borrowing from other sources when their bank suffered an adverse shock. We can also estimate the effects on consumption choices concerning real estate and automobiles as well as total spending imputed from income and wealth information (Browning & Leth-Petersen, 2003).

Our empirical strategy exploits that the financial crisis in 2007-2008 affected Danish banks differentially depending on the structure of their balance sheet. While the origin of the crisis was losses on US mortgage-backed securities, it spread within the banking sector through the markets for short-term funding (Brunnermeier, 2009; Shin, 2009; Gorton & Metrick, 2012). Danish banks generally had limited direct exposure to US mortgage-backed securities (Rangvid, 2013), however, those that relied heavily on wholesale funding experienced a severe liquidity shock when funding markets froze in 2008. Hence, the financial crisis plausibly induced a differential credit supply shock to Danish households because banks with a stable funding base and relatively liquid assets were able to continue lending as before, whereas banks with an unstable funding base and relatively illiquid assets were forced to reduce their lending.

Based on these considerations, we measure a bank's exposure to the financial crisis and thus the severity of the credit supply shock suffered by its customers with the ratio of loans to deposits in 2007 where the denominator reflects relatively illiquid assets and the denominator reflects relatively stable funding. We document that banks with a higher ratio of loans to deposits in 2007 reduced lending significantly over the period 2008-2011 relative to other banks, which is consistent with existing studies of lending dynamics during the financial crisis (Ivashina & Scharfstein, 2010; Cornett et al., 2011) and arguably driven by a tightening of the credit supply.

In the main analysis, we match each individual with their primary bank in 2007 and follow their credit and consumption outcomes over time. Loosely speaking, we are asking whether individuals who in 2007 were customers in banks that were exposed to the imminent financial crisis fared worse in subsequent years than others. In other words, we identify how the financial crisis affected households through the credit supply channel by comparing customers in banks with an above-median ratio of loans to deposits ("exposed banks") to customers in banks with a below-median ratio of loans to deposits ("non-exposed banks").

The main challenge for identification is that banks' exposure to the financial crisis may correlate with characteristics of their customers relevant to their credit demand. We address this issue in various ways. First, we show that the observable characteristics of customers in exposed and non-exposed banks are remarkably similar. This is an important indication that credit demand is likely to have been similar across customers in the two types of banks. Second, given our very large sample, we are able to control flexibly and non-parametrically for individual characteristics by including individual fixed effects as well as observable characteristics interacted with year dummies. This implies that our estimates are identified from a comparison of individuals whose banks have different exposure to the crisis, but live in the same municipality, work in the same industry, have the same age and educational level, and so on. Third, we show that pre-crisis trends in outcomes are parallel across individuals whose banks were exposed differently to the crisis. This strengthens the case that unobservable individual characteristics are uncorrelated with bank characteristics and excludes the possibility that less borrowing by customers in exposed banks after the crisis was caused by excessive borrowing before the crisis (Mian & Sufi, 2010). Finally, we exploit that a non-trivial number of individuals have loans in two or more banks to estimate models that include individual-time fixed effects. These models fully absorb credit demand shocks and cleanly identify the credit supply channel by comparing the credit outcome of the same individual in banks with different exposure to the financial crisis (Khwaja & Mian, 2008).

The first set of results provides strong evidence that the financial crisis reduced household borrowing through the credit supply channel. The total bank debt of customers in exposed banks decreased by around DKK 6,700 (around USD 960) relative to customers in non-exposed banks, which is equivalent to a 4.8% decrease at the sample mean. The corresponding decrease in debt at the pre-crisis primary bank was around DKK 15,800 (around USD 2,250). This suggests that customers in exposed banks increased debt in other banks than their pre-crisis primary bank by around DKK 9,100 implying that almost 60% of the decrease in credit supply by exposed banks was neutralized by their customers switching to non-exposed banks. This finding highlights the importance of studying customer-level rather than bank-level outcomes when assessing the effective transmission of bank shocks to their customers.

Turning to an alternative financial outcome, we show that the likelihood of liquidating a tax favored pension savings account increased significantly for customers in exposed banks relative to customers in non-exposed banks. While the average effect on liquidity is modest in economic terms, the fact that liquidating this type of account is associated with a significant penalty is highly suggestive that customers in exposed banks were more likely to have become credit constrained than customers in non-exposed banks.

Moreover, strengthening the case for a causal interpretation of the estimated effect on bank debt, we show that disposable income and unemployment followed the same trends for customers in exposed and non-exposed banks throughout the sample period. Hence, there are no signs that the results are driven by unobserved customer-level shocks to creditworthiness or liquidity correlating with bank-level exposure to the financial crisis.

The second set of results presents evidence that the decrease in borrowing was accompanied by a decrease in consumption. Most importantly, the annual spending of customers in exposed banks decreased by around DKK 3,500 (around USD 500) relative to customers in non-exposed banks when comparing the average pre-crisis and the post-crisis levels. The spending measure is imputed from detailed information on income and wealth and differs from standard definitions of consumption by including spending on real estate.

Using a host of other outcome variables directly related to automobile and real estate choices, we show that both of these consumption margins were affected. Specifically, customers in exposed banks bought smaller houses with a lower assessed value, acquired less expensive cars and became less likely to own two or more cars relative to customers in non-exposed banks.

The last set of results investigates why individuals did not fully compensate for the tightening of credit in exposed banks by switching to non-exposed banks. In the absence of market frictions, we should expect individuals who demand credit to obtain it as long as any bank is willing to supply it; in other words, the characteristics of an individual's current bank should not matter for later credit outcomes. Our finding that the pre-crisis bank relation had a strong effect on post-crisis borrowing thus raises the question which frictions were at play.

We hypothesize that the relevant friction is switching costs. If customers found it costly to switch to another bank, either in terms of search costs or mental costs of leaving an established bank relation, this could potentially explain the observed drop in borrowing by customers in exposed banks. We test this hypothesis by constructing three proxies for switching costs and separately estimating the decrease in credit for customers with high and low proxies for shifting costs. Specifically, we posit that individuals who had a different bank than their parents, individuals who had loans in two or more banks, and individuals who switched their primary bank during the pre-crisis period have relatively low switching costs. For all three proxies, and consistent with the notion of switching costs being salient, we find a smaller decrease in borrowing for customers with arguebly low switching costs than for those with high switching costs.

Finally, we ask how much of the drop in aggregate private consumption observed in Denmark after the financial crisis can plausibly be explained by the credit supply channel. We find that the direct effect of tightened bank credit on non-real estate spending, a reasonable measure of consumption, can account for roughly one third of the 4% drop in private consumption in national accounts from the peak in 2007 to the trough in 2009. The full effect may be even larger considering the likely multiplier effect of the initial drop in consumption and the indirect effect of weakened real estate spending on consumption through real estate prices.

The contribution of this paper is two-fold. First, we add to the literature on transmission of bank crisis to the real economy. Specifically, we provide the first compelling evidence that a reduced credit supply of distressed banks has the potential to reduce the total borrowing of the banks' customers as well as their consumption. We also shed light on the underlying mechanism by showing that the effect is considerably larger for households that plausibly have relatively high switching costs. Second, we add to our understanding of the sharp decrease in household consumption that followed the financial crisis. Other studies emphasize the role of excessive leverage (Mian & Sufi, 2010), falling house prices (Mian et al., 2013) and increased uncertainty (Alan et al., 2012), but our analysis points to a complementary channel through the contracted credit supply of distressed banks. This also relates to a broader literature on credit constrained consumers (e.g. Gross & Souleles 2002; Adams et al. 2009; Leth-Petersen 2010).

The paper proceeds in the following way. Section 2 provides background information on the financial crisis in Denmark. Section 3 describes the data sources and reports summary statistics. Section 4 discusses the empirical strategy. Sections 5 and 6 present the results concerning financial outcomes and consumption outcomes, respectively. Section 7 presents the results concerning heterogeneity in outcomes. Section 8 discusses the implications of the results for aggregate consumption. Section 9 concludes.

2 Background

2.1 The financial crisis 2007-2008 and its aftermath

In the years before the global financial crisis in 2007-2008, the Danish economy was growing at a rapid pace, the real estate market was booming and banks expanded their lending substantially. Since lending grew much faster than deposits, some banks relied increasingly on international credit markets to finance their expansion, often through loans at short maturities (Rangvid, 2013).

While Danish banks generally had very limited exposure to the U.S. mortgage-backed securities that triggered the financial crisis, some banks reached dangerously low levels of liquidity when global markets for wholesale funding froze (Rangvid, 2013; Shin, 2009). Between May and September 2008, the central bank therefore intervened several times to provide liquidity to the banking system and in October 2008, shortly after the collapse of Lehmann Brothers, the government was compelled to extend a two-year unlimited guarantee to all bank liabilities.

A distinctive feature of the Danish financial market is the specialized mortgage institutions: highly regulated financial institutions that only lend with collateral in Danish real estate (up to 80% of its market value) and are fully funded with bonds precisely matching lending in terms of maturity and interest rate. Because of the match between assets and liabilities, the mortgage institutions did not face the same challenges as banks during the financial crisis.

Figure 1 recounts the story of the Danish boom and bust over the period 2003-2013: rapidly increasing household credit and consumption until the peak of the financial crisis and then a sharp decline in both outcomes. The ultimate goal of the paper is to investigate how much of the decline in consumption can be explained with a decrease in banks' credit supply.

[Figure 1 around here]

2.2 The differential credit supply shock

The main premise of our analysis is that banks with fewer deposits on the liability side of their balance sheet and more loans on the asset side, tightened their credit supply more in response to the financial crisis.

Figure 2 provides evidence in support of this premise. While banks with a ratio of loans to deposits above and below the median (measured in 2007) exhibited very similar growth rates in lending during the period 2005-2007, there was a sharp divergence over the period 2008-2012: whereas banks with a low loan-deposit ratio continued to expand lending, banks with a high loan-deposit ratio reduced lending considerably in a sudden reversal of the trend in the previous years. Table 1 shows that the correlation between banks' pre-crisis loan-deposit ratio and subsequent growth in lending is statistically significant regardless of whether the regressions are unweighted or weighted with bank size and whether the loan-deposit ratio is used as a continuous variable or transformed into a dummy variable indicating a loan-deposit ratio above the sample median. These results are in line with existing studies of bank lending during the financial crisis (Ivashina & Scharfstein, 2010; Cornett et al., 2011).

[**Figure 2 around here**]

While the bank-level analysis is consistent with a differential credit supply shock that caused a decrease in the borrowing of customers in banks exposed to the crisis, it has at least two shortcomings. First, it is not clear whether customers in exposed banks were affected at all; based on the evidence presented above it cannot be excluded that the diverging lending outcomes of exposed and non-exposed banks were driven by customers switching from the former to the latter and thus neutralizing the effect of differential credit supply shocks. Second, we cannot be sure whether the bank-level lending patterns reflect differential supply or demand shocks; strictly speaking it could be customers' demand for credit that for one reason or the other correlated with banks' loan-deposit ratios rather than banks' own credit supply.

[**Table 1 around here**]

For these reasons, our main analysis studies outcomes at the individual level and at the even more granular account level. This allows us to study the full effect of credit supply shocks on bank customers while taking into account substitution toward other sources of credit and controlling for confounding changes in credit demand.

3 Data

3.1 Variables, sources and sample

The main data innovation of this paper is to establish a link between individuals and their bank relations from tax records. At the end of each year, financial institutions in Denmark report the balance of their customers' deposit and loan accounts to the tax authorities. The reports are compulsory and reliable since they are used for tax enforcement. We thus have a complete mapping of all loans and deposits with domestic financial institutions held by all individuals in Denmark.[1]

In principle, this type of information could be obtained in many other countries where banks are required to report financial information to the tax authorities. This has the potential to facilitate micro-econometric research on the interactions between the financial sector and the real economy, which is, however, currently scarce as it is constrained by the difficulty of linking individual customers to their banks.

To the raw administrative records of the Danish tax authorities, we add comprehensive information about the individual account holders from a number of other administrative registers. This includes demographic information such as age, gender, education, home municipality and identity of children and parents; labor market information such as wage income, industry and unemployment spells; income and wealth information such as capital income, social transfers, value of stock portfolios and pension accounts; auto register information such as the weight and production year of each registered automobile; real estate register information such as the size and value of each registered property. We also add detailed balance sheet information about the reporting banks obtained from the Danish Central Bank.

In the resulting dataset, we thus observe the following information for all individuals resident in Denmark for the period 2003-2011: the balance of each of their loan and deposit accounts; balance sheet information about the bank in which the account is held; and comprehensive background information about individual account holders from government registers.

Before conducting the analysis, we restrict the sample in several ways. First, we remove self-employed individuals since it is generally not possible to separate borrow-

[1]In practice, we obtain the link between individuals and banks in the following way. The first four digits of the bank account numbers that we observe in the tax records uniquely identify the branch of the bank where the accounts are held in a given year. We then hand-collect lists of branch id-numbers and the corresponding banks from publications by Nets, a payment solutions provider, for each of the years 2003-2011. This establishes the dynamic link between individual account numbers and bank identity.

ing for business and private purposes on the balance sheet of those operating a firm in their own name. Second, we restrict the sample to individuals who were between 20 and 50 years in 2007 to limit the scope for the analysis to be confounded by retirement decisions. Finally, we study a 25% random sample of the resulting population for computational tractability. This leaves us with around 476,000 individuals, more than 3.7 million individual-years and more than 5 million individual-account-years in the final dataset.

3.2 Imputed spending

One of our key outcome measures is spending, which we impute from income and wealth variables. The main idea is that spending in a given period, by definition, equals disposable income minus the increase in net wealth. Hence, to the extent that disposable income and wealth can be measured precisely, it is possible to infer the level of spending. Several papers show that while imputed measures of spending are noisy, they contain significant information about true spending, notably for individuals with simple balance sheets (Browning & Leth-Petersen, 2003; Kreiner et al., 2014).

The imputation method, however, has at least two shortcomings. First, we observe the value of real estate as assessed for tax purposes, but not the market value. To see the implications of this mismatch for the imputation procedure, consider an individual who purchases a house with a market value of DKK 3 million and an assessed value of DKK 2 million. If one were to include real estate in net wealth, the resulting spending measure would conceptually be a fairly good measure of consumption by excluding spending on real estate, however, the discrepancy between market value and assessed value would introduce an error. In the example, real assets would increase by DKK 2 million whereas net financial assets would decrease by DKK 3 million implying that the house purchase would reduce net wealth by DKK 1 million and thus increase imputed spending by the same amount. To address this issue, we do not include real estate in net wealth when we impute spending. In the example, the house purchase thus reduces net wealth by DKK 3 million and increases imputed spending by the same amount. This eliminates the error introduced by the mismeasurement of real estate, but implies that imputed spending can no longer be interpreted as consumption because it includes spending on real estate.

Second, one of the components of wealth is stock portfolios that are observed at current market prices. When the value of a stock portfolio increases due to increases in share prices, our measure of net wealth increases while income is unchanged, hence imputed spending decreases. Essentially, the error is due to the fact that unrealized capital gains are not observed as an income component. We address this issue in two ways. Most effectively, our main regressions of spending exclude the 23 percent of all individuals owning stocks. When our regressions include such individuals, we impute their unobserved capital gains by applying the price change of the market portfolio. With this procedure, stock price changes do not lead to a measurement error in imputed spending for individuals who hold the market portfolio, but will cause it to be overestimated (underestimated) for individuals whose stock portfolio underperforms (overperforms) relative to the market portfolio.

3.3 Summary statistics

Once the dataset is constructed, the first step of our empirical analysis is to define a unique primary bank for each individual in 2007. We use the following procedure: For individuals who only had one bank relation in 2007, this is their primary bank. For individuals who had multiple bank relations in 2007, but only had a loan in one of those banks, this is their primary bank. For individuals who had loans in multiple banks in 2007, the bank in which the loan balance was largest is their primary bank. For individuals who had no loans, but had deposits in multiple banks in 2007, the bank in which the deposit balance was largest is their primary bank. The procedure thus rests on the assumptions that loans provide a stronger bank relation than deposits and that bank relations are stronger the larger the account balance.

In the next step, we split the sample of individuals based on the loans to deposits ratio of their primary bank in 2007. We split the sample at the median individual so that the number of individuals with exposed and non-exposed banks is approximately the same.

Table 2 reports pre-crisis summary statistics on the main variables used in the analysis for customers in banks with high and low loans to deposits ratios separately. This information is useful by providing a sense of the demographic characteristics and financial situation of the individuals in our sample. The table shows that individuals were roughly equally distributed across the four education categories, around two thirds had a cohabitating partner and more than half had children. The average total income was around DKK 250,000 and the average disposable income after taxes and interest payments around DKK 200,000. By comparison, the average imputed spending was around DKK 220,000, which implies that the average individual in our sample reduced net wealth by around DKK 20,000 in 2007. Finally, the average level of debt was around DKK 500,000 of which bank debt accounted for less than one third due to the extensive use of specialized mortgage institutions to finance real estate purchases.

[Table 2 around here]

Moreover, the table allows us to assess whether pre-crisis customers in banks with high and low loan-deposit ratios were different with respect to their observed characteristics and, hence, whether it is a priori likely that the divergence in lending by the two types of banks documented in Figure 2 was driven by differential credit demand shocks. This does not seem to be the case. Table 2 shows that customers in banks with high and low loan-deposit ratios are strikingly similar. For none of the observed variables are the differences between the two means close to statistical significance.

4 Empirical strategy

The aim of the empirical analysis is to estimate the effect of banks' credit supply on the credit and consumption outcomes of households. Our main empirical strategy is to compare individuals, whose primary bank was exposed to the global liquidity shock associated with the financial crisis and therefore reduced its credit supply, to individuals whose primary bank was less exposed to the financial crisis. We implement this comparison with the following baseline model:

$$outcome_{it} = \alpha\Omega_i + \gamma\Omega_t \times exposed_i + \delta\Omega_t \times X_i + \epsilon_{it} \qquad (1)$$

where $outcome_{it}$ is a financial or consumption outcome of individual i at time t; Ω_i is a vector of individual fixed effects; Ω_t is a vector of dummy variables for each year in the sample (except 2007 which is the omitted category); $exposed_i$ is a dummy variable indicating if the primary bank of individual i had a loans to deposits ratio above the median; and X_i is a vector of characteristics of individual i in 2007.

The estimated vector β contains the main coefficients of interest. For each year it measures the average change in the outcome variable relative to 2007 for individuals who were customers in exposed banks in 2007 over and above the average change over the same period for individuals who were customers in non-exposed banks. The baseline model thus yields difference-in-difference estimates of how the financial crisis affected households through the credit supply channel for each of the years 2008-2011.

The key methodological challenge is that credit demand shocks could correlate with credit supply shocks, which would invalidate inference based on a simple comparison of customers in exposed and non-exposed banks. For instance, it may be that customers in exposed banks incidentally had educational backgrounds, lived in geographical regions or worked in industries that made them more affected by the crisis through other channels. Alternatively, they may have had different unobserved characteristics, such as risk attitudes or time preferences, which made them behave differently during the crisis. In either case, the credit demand shocks of individuals may have varied systematically with the exposure of their bank. We address this identification issue in various ways.

First, the difference-in-difference estimates are computed conditional on a comprehensive set of controls. For each control variable, we include the value in 2007 as well as its interactions with year dummies. With this procedure we effectively identify the effect from a comparison of individuals with the same observed characteristics in 2007, of which some were customers in exposed banks and others were customers in non-exposed banks. To the extent that credit demand evolved similarly for customers in exposed and non-exposed banks with the same observed characteristics, the baseline model therefore correctly identifies the credit supply channel.

The baseline model includes variables that capture the following characteristics: gender (dummy for being a woman), age (dummies for each 1-year age group), education (dummies for short, medium and long education with no education as the omitted category), home ownership (dummy for owning real estate), children (dummy for having children), civil status (dummy for cohabitation with partner), student (dummy for being a student), unemployment (dummy for unemployment spells during 2006-2007), bank debt (dummies for the deciles of the bank debt distribution in 2007), income (dummies for the deciles of the income distribution in 2007), income growth (dummies for the deciles of the distribution of income changes over the period 2003-2007), home municipality (dummy for each of 98 Danish municipalities), and industry (dummy for each of 9 occupation sectors).

Second, β allows us to assess directly whether customers in exposed and non-exposed banks followed similar trajectories in terms of borrowing and consumption over the period 2003-2007 conditional on observed characteristics. If trends in outcomes are parallel during a period where there was presumably no major differential shocks to credit supply, it is suggestive that the unobserved characteristics shaping credit demand are roughly balanced across customers in the two types of banks.

Finally, we estimate the following account-level model that absorbs all variation in credit demand with individual-year fixed effects:

$$outcome_{ibt} = \alpha\Omega_i \times \Omega_t + \beta\Omega_t \times exposed_b + \epsilon_{ibt} \quad (2)$$

where outcomebit is the outcome of individual i in bank b at time t and $exposed_b$ is a dummy variable indicating if bank b had a loans to deposits ratio above the median in 2007.

In the account-level model, β measures the change in borrowing by a given individual in exposed banks over and above the change in borrowing by the same individual in non-exposed banks. Given that the individual-year fixed effects capture the credit demand of each individual at each point in time, any differential changes in borrowing across different banks can be attributed to differential changes in the credit supply. This is essentially the within-estimator proposed by Khwaja & Mian (2008).

It should be noted that β is not identified in the latter model when there is only one observation per individual and year. The model can therefore only be applied to credit outcomes, in which case β is identified exclusively by individuals with loan accounts in more than one bank. While such individuals do not constitute a random subset of the total population and the results from this model do therefore not necessarily generalize to the full sample, the account-level model nevertheless remains a useful framework for testing the power of the credit-supply channel in a setting where potentially confounding credit demand factors are effectively eliminated.

Another potential identification problem derives from the fact that even banks with a low loan-deposit ratio may have been affected by the financial crisis and may consequently have tightened their credit supply, although presumably to a lesser extent than banks with a high loan-deposit ratio. To quantify the credit supply effect of the financial crisis, we effectively use customers in banks with low loans to deposits ratios as a counterfactual for customers in banks with high loan-deposit ratios, which is only accurate to the extent that the credit supply in the former banks was completely unaffected by the financial crisis. In a sense, our estimates therefore provide a lower bound on the true effect of the financial crisis through the credit supply channel.

Finally, we note that all point estimates are reported with standard errors clustered at the level of the primary bank in 2007. This conservative clustering strategy widens the standard errors considerably given that the baseline sample includes close to 3.8 million observations at the individual-year level, but only around 100 banks.

5 Results: Financial outcomes

We first use the baseline model with individual fixed effects and a full set of controls to study individuals' total debt. This outcome comprises bank debt as well as debt in specialized mortgage institutions. Figure 3 plots the estimated coefficients on the interaction terms between the year dummies and the dummy variable indicating that the individual's primary bank in 2007 was exposed to the financial crisis (i.e. the elements in the vector β). The full regression output is available in the Appendix.

[Figure 3 around here]

For 2004-2006, the point estimates are almost precisely zero suggesting that the average total debt of customers in exposed and non-exposed banks grew at almost exactly the same speed before the financial crisis. For 2008-2011, the point estimates are below zero suggesting that the total debt of customers in exposed banks decreased relative to the total debt of customers in non-exposed banks after the financial crisis. The individual point estimates for 2009 and 2010 are highly significant as indicated by the confidence bands whereas those for 2008 and 2011 are borderline significant. Since our observations are end-of-year, the gradually decreasing point estimates of around -3,000 for 2008, -10,000 for 2009 and -12,000 for 2010 imply that most of the divergence between customers in the two types of banks occurred in the course of 2009.

Next, we conduct the same exercise using bank debt as the outcome variable. Figure 4 shows a similar pattern as the previous figure. The relatively small point estimates for 2004-2006 suggest that customers in exposed and non-exposed banks followed very similar trajectories until 2007 whereas the negative and gradually decreasing point estimates for 2008-2011 suggest that customers in exposed banks experienced falling bank debt relative to customers in non-exposed banks from 2008. In absolute terms, the estimated effects are somewhat smaller for bank debt than for total debt with point estimates hovering around -6,000 in 2009-2011.

[Figure 4 around here]

The fact that we find a larger effect for total debt than for bank debt suggests that the reduction of credit supply by banks spilled over on non-bank debt; in other words, bank and non-bank debt appear to be complements rather than substitutes. This is not surprising in the Danish institutional context where many households rely on senior debt from mortgage institutions to finance house purchases up to the regulatory limit of 80% of the house value and junior debt from banks to finance the residual. It is plausible that when exposed banks tightened credit standards, some of their customers were induced to buy less expensive houses and, thus, borrow less from both banks and mortgage institutions. This is consistent with findings reported in the next section.

While Figures 3 and 4 showed the results from the full baseline model, we now estimate a more compact version of the baseline model where outcomes are averaged over the periods 2005-2007 ("pre-crisis") and 2009-2011 ("post-crisis"). Since we collapse the time dimension of the dataset to two periods, the vector of time dummies is replaced with a simple post-crisis dummy. The compact model is useful because it sums up the bank supply channel in a single coefficient, the interaction term between exposed and post, which enhances comparability between different specifications.

Equipped with this model, we study how the large set of controls shapes our results by moving sequentially from a specification with no controls, which is essentially a raw comparison of average levels, to the full specification will all controls. Column (1) in Table 3 shows that the average total debt was DKK 10,441 higher for customers in exposed banks than for customers in non-exposed banks in the pre-crisis years whereas it was DKK 1,709 lower in the post-crisis years, a relative decrease of DKK 12,150. To this most parsimonious specification, Columns (2) and (3) sequentially add covariates, municipality dummies and industry dummies, all interacted with year dummies. While the pre-crisis level difference in total debt between customers in exposed and non-exposed banks almost vanishes and the R-squared increases considerably, the key estimate, the decrease in total debt of customers in exposed banks relative to customers in non-exposed banks, is barely affected. Hence, observed characteristics are successful at explaining level-differences in total debt, but are almost orthogonal to the growth-differences between customers in the exposed and non-exposed banks. Column (4) finally adds individual fixed effects, which also leaves the key estimate virtually unchanged.

[**Table 1 around here**]

The next 4 columns present similar results for debt in all banks; and the final

4 columns for debt in the bank that served as primary bank in 2007. For both outcomes, the estimates are very stable across all specifications. The results suggest that starting from very similar initial levels of bank debt, customers in exposed banks decreased the overall bank debt by DKK 6,741 and the debt in their primary bank by DKK 15,800 relative to customers in non-exposed banks. A comparison of the two estimates is interesting by suggesting that customers in exposed banks increased debt in other banks than their primary bank by DKK 9,059.[2] This is informative about the extent to which households mitigated bank-level credit supply shocks by switching banks. Specifically, the point estimates suggest that customers in banks exposed to the financial crisis neutralized almost 60% of the effect of tightened credit in their primary bank by obtaining credit in other banks.

To address the concern that the baseline model does not account for differential changes in credit demand driven by differences in unobserved characteristics, we now turn to the account-level model described in the previous section. It should be noted that loan balances are considerably more difficult to model at the account-level than at the individual-level. For instance, when an existing loan is refinanced in a new bank, we observe a large increase in the account-level balance in the new bank and a corresponding decrease in the account-level balance in the old bank whereas the individual-level balance is unchanged. This variability in account-level balances tends to increase standard errors considerably. Our preferred outcome variable is therefore a dummy variable indicating a positive change in the balance of a pre-existing loan account. The transformation of the dependent variable to a dummy variable eliminates the problem of highly volatile account-level loan balances.

Figure 5 illustrates the results by plotting the estimated coefficients on the interaction terms between the year dummies and the dummy variable indicating that the bank was exposed to the financial crisis (i.e. the elements in the vector β). The full regression output is available in the Appendix.

[**Figure 5 around here**]

The coefficients are very close to zero for the years 2004-2006, hence there was no change in the likelihood of increasing the loan balance in exposed banks relative to the likelihood of increasing the loan balance in non-exposed banks for individuals

[2] Estimating the full baseline model with debt in other banks than the primary bank as the dependent variable yields a point estimate of DKK 8,730.

who held loan accounts in both types of banks during the pre-crisis period. For 2009, the coefficient is significantly negative and the point estimate suggests that individuals with multiple accounts were 15% less likely to increase the loan balance in exposed banks than in non-exposed banks compared to the pre-crisis period. For 2010 and 2011, the point estimates are also negative, but not statistically significant. The finding that most of the decrease in borrowing in exposed banks occurred in the course of 2009 is consistent with the results from the baseline model.

Since the individual-year fixed effects in the account-level model effectively absorb the credit demand of each individual at each point in time, it is difficult to explain these results in other ways than that exposed banks tightened their credit supply relative to non-exposed banks after the financial crisis. This further strengthens the credibility that the results from the baseline model are driven by differential credit supply shocks rather than credit demand shocks.

Having established that the differential credit supply shock induced by the financial crisis affected credit outcomes in the household sector, we now study whether there was an effect on financial assets. It is conceivable that individuals whose access to credit was constrained responded by running down financial assets and this mechanism may prevent a decrease in credit from causing a decrease in consumption (Damar, Gropp and Mordel, 2014).

The results are reported in Table 4. As shown in Columns (1)-(2), customers in exposed banks reduced the value of their bank deposits and stock portfolios relative to customers in non-exposed banks. However, the combined decrease in liquid assets of DKK 1,627 is modest relative to the corresponding decrease in debt of DKK 10,560 (from Table 3, Column 4) and not statistically significant.

[**Table 4 around here**]

By contrast, as shown in Column (3) and further shown in Figure 6, customers in exposed banks increased the withdrawals from tax favored pension savings accounts significantly relative to customers in non-exposed banks. While the average annual effect of DKK 37 is negligible, the finding is interesting because liquidation of this type of savings accounts is subject to 60 percent penalty taxation and therefore likely to be the last resort for individuals with no access to credit and no liquid assets.

[Figure 6 around here]

Finally, we consider two non-financial outcomes, disposable income and unemployment, which may influence banks' effective credit supply to households and could therefore possibly confound our results. If customers in exposed banks suffered decreases in their income and increases in their unemployment risk relative to customers in non-exposed banks with the same observed characteristics, the implied deterioration in creditworthiness could potentially explain why the former obtained less credit than the latter. As shown in Column (4) of Table 4, we find a small differential decrease in disposable income of DKK 250 for customers in exposed banks. The effect is far from statistical significance, however, and, as illustrated in Figure 6, there is no clear trend over the sample period that could explain the differential decrease in credit for customers in exposed banks.[3] As shown in Column (5), we also find a small and statistically insignificant differential decrease in unemployment for customers in exposed banks. If anything, this would tend to increase their creditworthiness and therefore cannot explain the observed differential decrease in borrowing.

6 Results: Consumption outcomes

We start the analysis of consumption outcomes by estimating the baseline model with individual fixed effects and a full set of controls using imputed spending as the dependent variable. The sample conservatively excludes individuals who own stocks since capital gains accruing differentially to customers in exposed and non-exposed banks could potentially confound our results.

As shown in Figure 7, the estimated coefficients on the key interaction between exposed and time are small for the years 2004-2006 suggesting that spending by customers in exposed and non-exposed banks evolved similarly before the financial crisis. In 2008-2009, however, there was a significant differential decrease in spending by customers in exposed banks. Specifically, from 2007 to 2008, their spending fell by around DKK 4,000 relative to customers in non-exposed banks, and from 2008 to 2009 by an additional DKK 4,500. Over the next years, the spending of customers in exposed banks slowly caught up with the spending of customers in non-exposed banks.

[3] We have also estimated the baseline model for credit outcomes while adding contemporaneous disposable income to the set of explanatory variables and this has virtually no impact on the results (not reported).

[**Figure 7 around here**]

It is important to note that since spending is a flow variable, it is natural that the differential decrease suffered by customers in exposed banks after the financial crisis was temporary. In other words, a temporary drop in current spending and a subsequent catch-up imply a permanent effect on cumulative spending, which is consistent with the permanent effect on total debt reported in the previous section. In terms of timing, the results for spending and debt also tell a coherent story: the relative drop in current spending between 2007 and 2009 coincided with the large relative drop in total debt and the catching up from 2010 occurred when the relative drop in debt slowed down.

Next, we estimate the compact version of the full baseline model for a number of consumption-related outcome variables and report the results in Table 5. Column (1) shows that the differential decrease in spending over the period 2009-2011 was DKK 3,522 in the baseline sample without stock-owners and highly statistically significant. Column (2) shows that this effect is similar when stock-owners are included in the sample.

[**Table 5 around here**]

In the following columns, we study automobile consumption. Column (3) shows that the number of cars owned by customers in exposed banks dropped by 0.00185 relative to customers in non-exposed banks. This suggests that 1 out of roughly 500 customers in exposed banks owned 1 car less than they would have owned, had they been customers in non-exposed banks. Column (4) shows that the relative drop in car ownership mostly concerns multiple car ownership. The propensity to own two cars or more dropped by 0.00146 suggesting that 1 out of roughly 700 customers in exposed banks owned at most one car whereby they would have owned at least two cars, had they been customers in non-exposed banks. Column (5) shows a relative decrease in the average weight of cars owned by customers in exposed banks of 2.857 kilo. The sample average is around 1,400 kilo suggesting a decrease in auto weight of around 0.2%.

These results point to a relatively modest effect of the financial crisis on automobile consumption through the credit supply channel. To illustrate, assuming realistically that a car in Denmark is worth on average DKK 100,000, the point estimate of

0.00185 suggests that the extensive margin of auto ownership can account for around DKK 185 of the relative decrease in spending suffered by customers in exposed banks. Further, given the elasticity of car value with respect to weight of around 2.8 found in existing studies of Danish car data (Munk-Nielsen, 2015), the estimated weight decrease of 0.2% explains an additional DKK 560 of the decrease in spending.

Turning to housing outcomes, Column (6) shows that the average public property valuation of homes owned by customers in exposed banks decreased by DKK 8,796 relative to customers in non-exposed banks. This effect is not, however, statistically significant, possibly because the sample includes a large number of individuals who rarely or never change their residence through the sample period.

We therefore proceed to estimate models where the sample is restricted to individual-years where a real estate purchase takes place. In this modified version of the baseline model, we effectively compare a different set of individuals in each year: customers in exposed banks who bought real estate during the relevant year and customers in non-exposed banks who bought real estate during the same year. Compared to the full baseline model, we drop individual fixed effects to avoid restricting the identifying variation to the limited number of individuals who buy several homes during the sample period, but retain all other controls.

Column (7) shows that the increase in the public property valuation triggered by a real estate transaction fell by DKK 33,048 for customers in exposed banks relative to customers in non-exposed banks. Similarly, as shown in Column (8), there was a differential decrease in the average new debt of DKK 24,425 and, as shown in Column (9), a differential decrease in the gain in home size of 0.396 square meters for customers in exposed banks purchasing real estate. These results are strongly suggestive of customers in exposed banks were induced to buy smaller and less valuable houses when their banks tightened credit in response to the financial crisis.

7 Results: Heterogeneity in outcomes

Our finding that customers in exposed banks obtained significantly less credit than customers in non-exposed banks after the financial crisis points to the existence of financial frictions that prevented at least some of the customers in exposed banks to fully compensate with credit from other sources. An argument often invoked in the context of lending to firms is that information asymmetries create valuable bank-customer relationships (Stiglitz & Weiss, 1981; Petersen & Rajan, 1994). This may cause relationships to be sticky even when banks temporarily limit their credit supply in response to adverse shocks.

The informational argument is somewhat less persuasive in the context of households that generally have less complex balance sheets and more predictable income streams than firms. While there is some evidence that relationships matter also in the context of consumer lending (Puri & Rocholl, 2008) and retail deposits (Iyer & Puri, 2012), the mechanism underlying relationships may very well be different.

We hypothesize that switching costs could play an important role in explaining why some individuals do not switch bank even when they demand credit that other banks, but not their own, are willing to supply. Switching costs could be related to the cost of search. It takes time to identify a new bank that is willing to supply the desired credit and, moreover, the outcome of the search is a priori uncertain. In fact, if people are not fully aware that banks differ with respect to their effective credit supply, they may erroneously infer from their own bank rejecting a loan application that other banks would reject it too. There may also be important mental barriers to bank switches. Some bank customers plausibly build personal relationships with individual loan officers, which create trust and confidence in their financial advice as well as a sense of moral obligation to remain at the bank despite its hardships.

To test this hypothesis, we construct three variables that capture the stickiness of bank relationships and therefore plausibly the magnitude of switching costs. Our preferred measure is an indicator of whether individuals have a different primary bank than their parents. Presumably, most people open their first account in the bank used by their parents when they are children or teenagers. Having a different bank than the parents is therefore a signal that the individual has switched banks at least once and, thus, is likely to have low switching costs. Alternative measures are indicators of whether individuals simultaneously had loan accounts in at least two banks or switched their primary bank at some point during the pre-crisis period 2003-2007.

We split the sample along each of those three dimensions and estimate the baseline model for the six subsamples separately. The results are reported in Table 6. As shown in Columns (1)-(2), the differential decrease in total debt is substantially smaller for individuals who had a different bank than their parent, DKK 6,959, than for those who had the same bank, DKK 16,984.[4] The other measures of switching costs yield qualitatively similar results. Individuals who had switched their primary

[4]The two point estimates are significantly different in a version of the baseline model that uses the full sample and conditions the differential decrease in bank debt on the dummy indicating same bank as parents.

bank or held loan accounts in multiple banks before the crisis were less adversely affected if they were customers in an exposed bank.

[**Table 6 around here**]

While the results are consistent with the hypothesis that heterogeneous switching costs matter for the extent to which individuals are affected by adverse shocks to banks, they are not conclusive. Importantly, we cannot exclude that other dimensions of heterogeneity correlating with our measures of switching costs are in fact driving the results.

8 Discussion

This section briefly discusses how much of the spectacular drop in aggregate private consumption illustrated in Figure 1 can plausibly be explained by the tightening of bank credit.

As shown in Figure 7, the differential decrease in spending by customers in exposed banks between 2007 and 2009 was around DKK 8,500. This number includes spending on real estate, which is not consumption in the sense used by national accounting where purchases of real estate are treated as investment. To obtain comparable estimates net of real estate spending, we repeat the estimation while excluding individual-years where a real estate transaction takes place (results not reported). The point estimates imply a differential decrease in the non-real estate spending of customers in exposed banks of around DKK 5,200 between 2007 and 2009, which corresponds to 2.4% of their total spending in 2007 (reported in Table 2).

By comparison, aggregate private consumption dropped by around 4% in real terms between 2007 and 2009. Since our estimate of 2.4% only applies to half of the population, those who were customers in exposed banks, our results suggest that the credit supply channel can explain a decrease in aggregate consumption of 1,2% or roughly one third of the total drop in aggregate private consumption from the pre-crisis peak in 2007 to the post-crisis trough in 2009.

There are several reasons to believe, however, that this simple computation underestimates the full impact of the credit supply channel. First, our identification rests on a comparison between customers in exposed and non-exposed banks and therefore effectively assumes that non-exposed banks did not change their credit supply after the financial crisis. If non-exposed banks did in fact tighten credit, albeit less than exposed banks, this would tend to bias our estimates toward zero. Second, the direct effect of credit supply on consumption plausibly created multiplier effects. Since these indirect effects are likely to be similar for customers in exposed and non-exposed banks, they are not captured by our estimates. Third, the decrease in real estate spending does not in itself reflect a decrease in consumption, but may have affected consumption indirectly through its effect on house prices. The estimated decrease in real estate spending by individuals purchasing real estate of DKK 33,048 amounts to around 15% of the average real estate spending conditional on purchase. To the extent that the reduced credit-supply weakened real estate demand and hereby contributed to the considerable drop in Danish real estate prices over the period 2007-2009, this is likely to also have affected consumption through the balance sheet channel (Mian et al., 2013).

9 Conclusion

This paper has studied whether the financial crisis spread from distressed banks to households through a contraction of the credit supply. We first argued that banks with a large reliance on non-deposit funding and many assets tied up in illiquid loans were especially exposed to the global credit crunch associated with the financial crisis in 2007-2008 and documented that banks with a high loan-deposit ratio in 2007 reduced their credit supply significantly in the following years relative to banks with a low loans to deposits ratio. We then showed that customers in exposed banks reduced their total borrowing as well as consumption after the financial crisis relative to customers in non-exposed banks. This finding suggests that the tightening of credit by banks exposed to the crisis had significant adverse effects on the households that were their customers.

Besides being the first to provide compelling evidence that shocks to banks can affect household-level consumption outcomes through the credit supply channel, the paper makes several contributions. First, we show that tightened bank credit is mitigated by some households with switches to other banks. This highlights the importance of studying customer-level rather than bank-level outcomes when assessing the effective transmission of bank shocks to their customers. Second, we provide suggestive evidence that switching costs may play an important role in explaining why some customers do not fully compensate with credit from other sources when their bank tightens credit. This is an alternative to the explanations based on asymmetric information often invoked in the context of bank-firm relationships. Finally, we quantify the contribution of the credit supply channel to the spectacular drop in aggregate private consumption observed in Denmark between 2007 and 2009. Around one third of the consumption loss can plausibly be attributed directly to tightened bank credit.

Figure 1: Total consumption and bank debt

The figure shows the development in total aggregated household consumption and aggregated total bank lending over the period from 2003Q1-2012Q4. Both time series are stated in billions of 2010-Danish Kroner. The data is obtained from National Accounts and MFI-statistics obtained from Statistics Denmark.

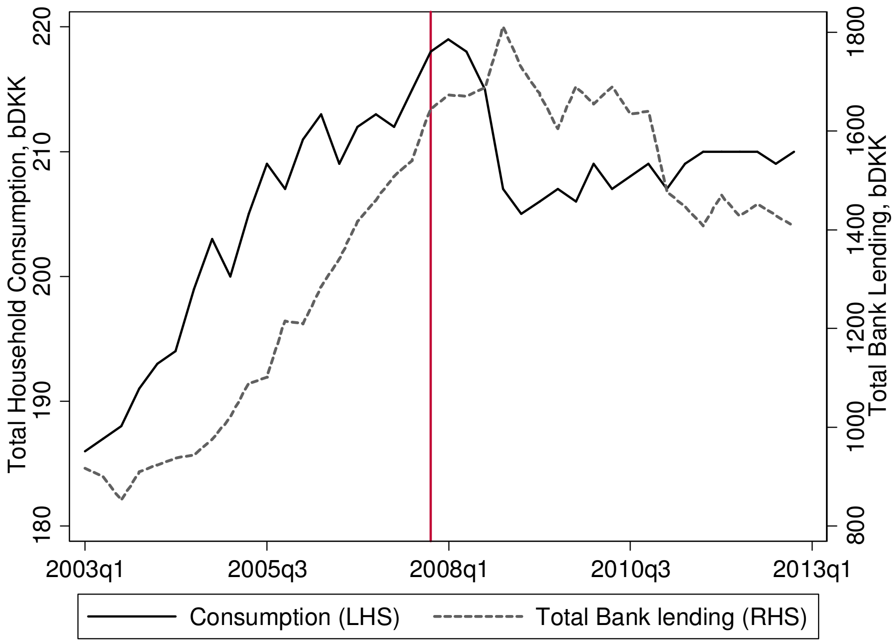


Figure 2: Lending growth by loans to deposits ratio

The figure shows the average growth in total lending relative to 2007 by banks respectively above and below the median level of loans to deposits ratio in 2007. The bank sample is obtained from the Danish Central Bank and consists of 89 banks with positive lending in all years during 2005-2012 and hence excludes failed banks. The relative lending growth is winsorized at the 5th and 95th percentile to limit the influence of mergers and acquisitions.

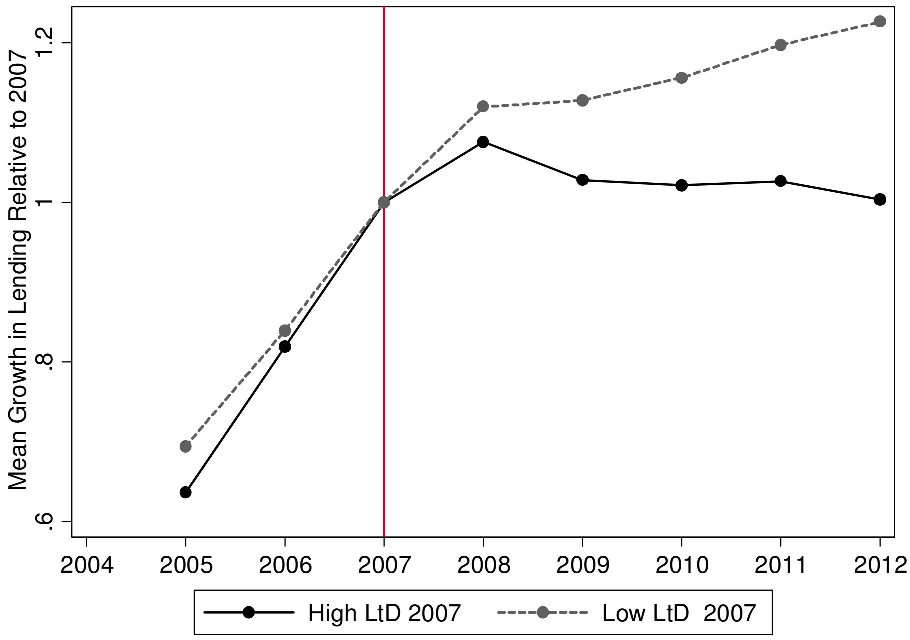


Figure 3: Total liabilities

Figure 3 shows a dynamic version of model (4) from table 3 where an indicator for having a primary bank with above median loans to deposits ratio in 2007 is interacted with year dummies relative to 2007. The dependent variable is total liabilities of the individual winsorized at the 1st and 99th percentile within each year. The model includes categorical controls, all measured in 2007, for age and educational level, indicator for gender, home ownership, partner, and positive unemployment spells during current and preceding year, deciles of bank debt, deciles of income and deciles of income growth 2003-2007, all interacted with year fixed effects. The model further includes 98 municipality dummies and 9 occupation industry dummies both interacted with year dummies along with individual fixed effects. Standard errors are clustered at the bank level and the confidence bands report the 95 percent confidence level.

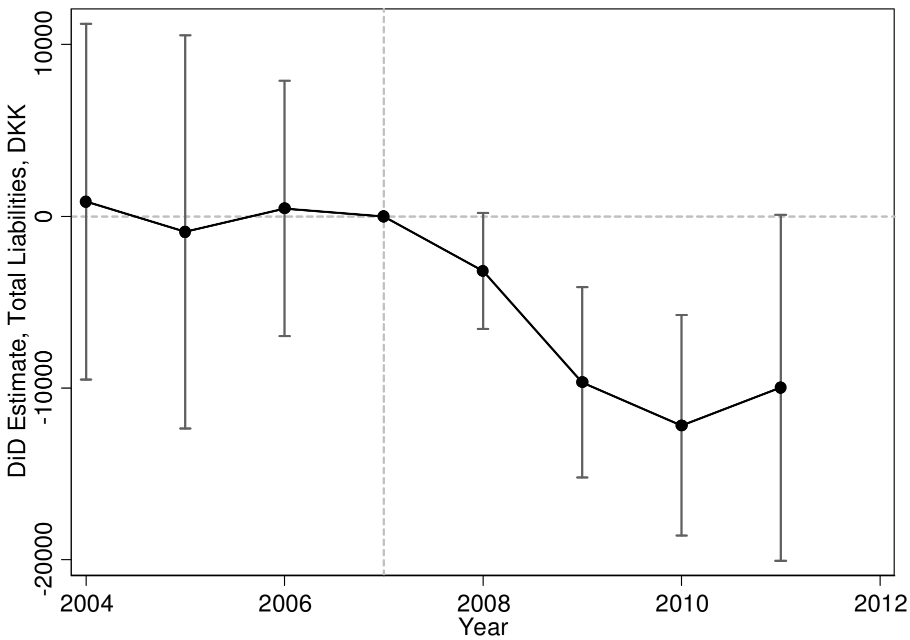


Figure 4: Total bank debt

Figure 4 shows a dynamic version of model (8) from table 3 where an indicator for having a primary bank with above median loans to deposits ratio in 2007 is interacted with year dummies relative to 2007. The dependent variable is total bank debt of the individual winsorized at the 1st and 99th percentile within each year. The model includes categorical controls, all measured in 2007, for age and educational level, indicator for gender, home ownership, partner, and positive unemployment spells during current and preceding year, deciles of bank debt, deciles of income and deciles of income growth 2003-2007, all interacted with year fixed effects. The model further includes 98 municipality dummies and 9 occupation industry dummies both interacted with year dummies along with individual fixed effects. Standard errors are clustered at the bank level and the confidence bands report the 95 percent confidence level.

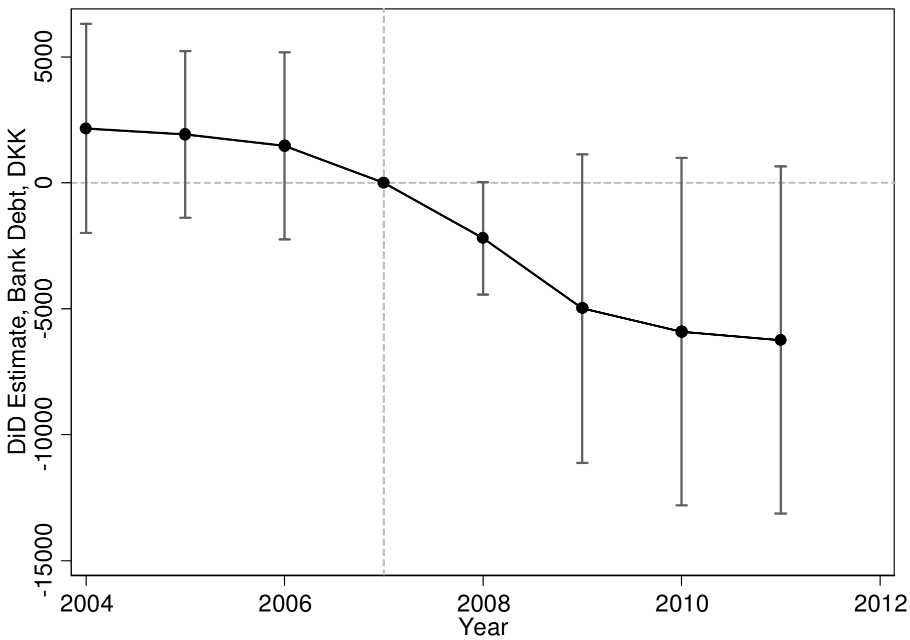


Figure 5: With-in estimator

Figure 5 shows a model where an indicator for a bank with above median loans to deposits ratio in 2007 is interacted with year dummies relative to 2007. The sample consists of account level data of the individual. The model includes individual year fixed effects that absorb time variation in the demand for credit at the individual level. The dependent variable is the probability of getting a new loan within the given bank. Standard errors are clustered at the bank level and the confidence bands report the 95 percent confidence level.

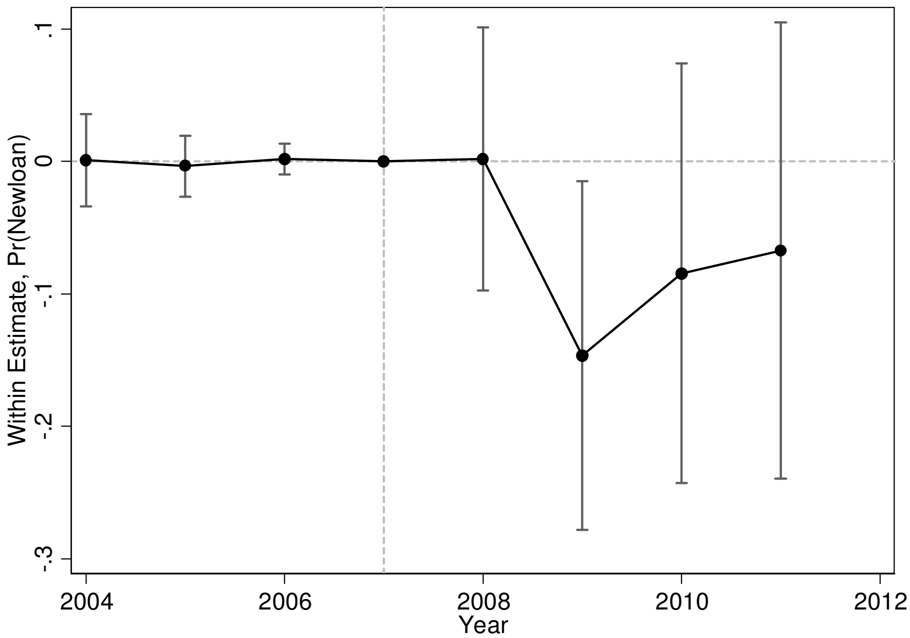
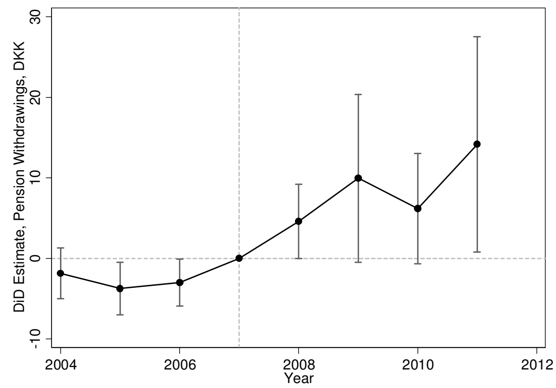


Figure 6: Other outcomes

Figure 6 shows a dynamic version of model (3) and (4) from table 4 where an indicator for having a primary bank with above median loans to deposits ratio in 2007 is interacted with year dummies relative to 2007. The dependent variable is disposable income measured in DKK (panel A) and pension withdrawings measured in DKK (panel B) of the individual winsorized at the 1st and 99th percentile within each year. The model includes categorical controls, all measured in 2007, for age and educational level, indicator for gender, home ownership, partner, and positive unemployment spells during current and preceding year, deciles of bank debt, deciles of income and deciles of income growth 2003-2007, all interacted with year fixed effects. The model further includes 98 municipality dummies and 9 occupation industry dummies both interacted with year dummies along with individual fixed effects. Standard errors are clustered at the bank level and the confidence bands report the 95 percent confidence level.

Panel (A)



Panel (B)

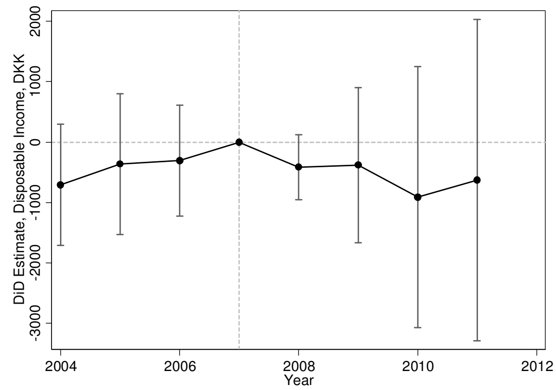


Figure 7: Spending

Figure 7 shows a dynamic version of model (1) from table 5 where an indicator for having a primary bank with above median loans to deposits ratio in 2007 is interacted with year dummies relative to 2007. The dependent variable is imputed spending of the individual winsorized at the 1st and 99th percentile within each year. The sample excludes individuals holding stocks. The model includes categorical controls, all measured in 2007, for age and educational level, indicator for gender, home ownership, partner, and positive unemployment spells during current and preceding year, deciles of bank debt, deciles of income and deciles of income growth 2003-2007, all interacted with year fixed effects. The model further includes 98 municipality dummies and 9 occupation industry dummies both interacted with year dummies along with individual fixed effects. Standard errors are clustered at the bank level and the confidence bands report the 95 percent confidence level.

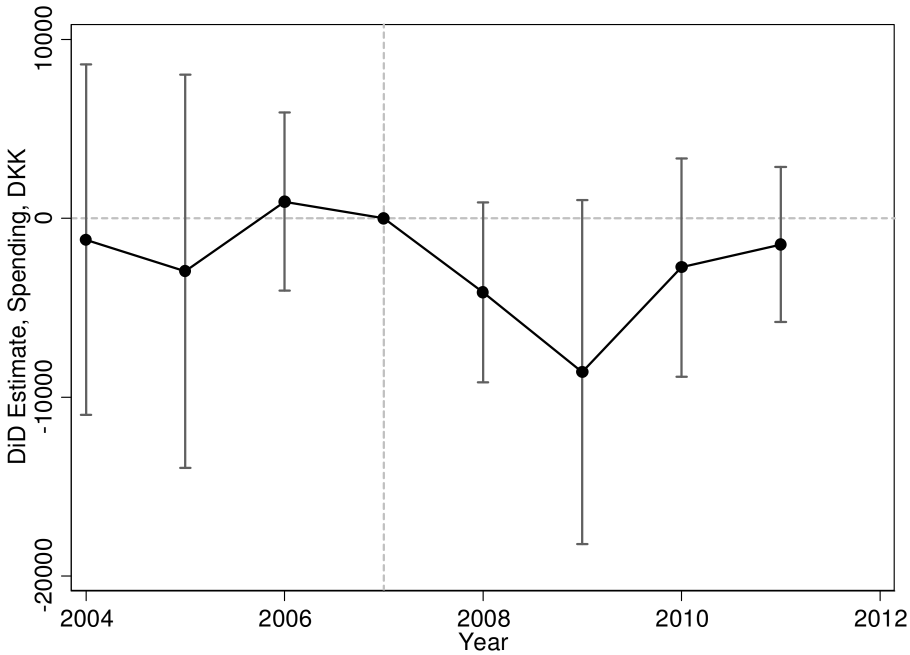


Table 1: Bank lending growth

*Table 1 reports estimates from OLS regressions, where the dependent variable is the growth in total bank lending from 2007-2011. The bank sample is obtained from the Danish Central Bank and consists of 89 banks with positive lending all years during 2005-2012. Lending growth and the loans to deposits ratio is winzorized at the 5th and 95th. Models (1) and (2) are unweighted regressions and models (3) and (4) weight each observation by the size of the bank, measured by total lending in 2007. Models (1) and (3) have the indicator variable for above median loans-to-deposits ratio whereas models (2) and (4) have a continuous measure of loans to deposits ratio as the right hand side variable. Standard errors are reported in parentheses. *, **, *** indicate statistically different from zero at 5%, 1%, and 0.1% level.*

	Unweighted		Weighted	
	High/Low	Continuous	High/Low	Continuous
	(1)	(2)	(3)	(4)
High Loans-to-Deposits, 2007	-0.1764***		-0.3484**	
	(0.0559)		(0.1384)	
Loans-to-Deposits, 2007		-0.1907***		-0.5215***
		(0.0684)		(0.0771)
Constant	0.1579***	0.2708***	0.2026	0.5546***
	(0.0393)	(0.0771)	(0.1369)	(0.1038)
Observations	89	89	89	89
R-squared	0.1029	0.0821	0.0679	0.3446

Table 2: Summary statistics

Table 2 presents summary statistics for the 469.742 individuals in our sample based on their primary bank in 2007. An individual that has a bank with above median loans to deposits ratio in 2007 is classified as exposed while below median individuals are classified as non-exposed. Standard errors are in parentheses. Difference denotes difference in mean, and P-value denotes the significance level of the difference, with standard errors clustered at the bank level. All variables are winsorized at the 1st and 99th percentile.

	Exposed: High Loans-to-Deposits Bank	Non-exposed: Low Loans-to-Deposits Bank		
	Mean		Difference	P-value
Age	35.41	35.70	-0.29	0.19
	(8.18)	(8.18)		
Education, Short	0.27	0.27	0.00	1.00
	(0.45)	(0.45)		
Education, Medium	0.37	0.36	0.01	0.62
	(0.48)	(0.48)		
Education, Long	0.24	0.24	0.00	0.92
	(0.43)	(0.43)		
Female	0.50	0.50	0.00	0.28
	(0.50)	(0.50)		
Partner	0.65	0.64	0.00	0.91
	(0.48)	(0.48)		
Student	0.03	0.03	0.00	0.49
	(0.18)	(0.18)		
Kids	0.57	0.57	-0.01	0.77
	(0.50)	(0.50)		
# of cars	0.48	0.46	0.02	0.50
	(0.50)	(0.50)		
Unemployment, Per Mille	28.7	31.6	-2.9	0.43
	(102)	(102)		
Disposable Income, kDKK	191	193	1.2	0.81
	(81)	(81)		
Total Income, kDKK	258	256	-1.3	0.92
	(164)	(164)		
Total Liabilities, kDKK	507	519	12.1	0.79
	(577)	(577)		
Total Bank Debt, kDKK	141	141	0.2	0.97
	(197)	(197)		
Total Deposits, kDKK	69	68	-1.3	0.68
	(158)	(158)		
Total Spending, kDKK	217	220	3.5	0.73
	(269)	(269)		
Observations	238,616	231,126		

Table 3: Lending outcomes

*Table 3 reports estimates from OLS regressions, where the dependent variable is total liabilities (1-4), total bank debt (5-8) and total bank debt in 2007 primary bank (9-12), all measured in DKK. The main RHS variable is the indicator variable for whether the individual's primary bank in 2007 had a high loans to deposits ratio (exposed) interacted with an indicator for the crisis period 2009-2011. The dataset is collapsed into a pre- and post- period containing average values for each individual during 2005-2007 (pre) and 2009-2011 (post), with 2008 omitted. Covariates-year fixed effects include categorical controls, all measured in 2007, for age and educational level, indicator for gender, home ownership, partner, and positive unemployment spells during current and preceding year, deciles of bank debt, deciles of income and deciles of income growth 2003-2007, all interacted with year fixed effects. The outcome variables are winsorized at the 1st and 99th percentile separately within the pre and post period. Columns (3-4) , (7-8) and (11-12) include 98 municipality fixed effects and 9 occupation industry fixed effects interacted with year dummies. Columns (4), (8) and (12) further include individual fixed effects. Standard errors are clustered at the bank level and are reported in parentheses. *, **, *** indicate statistically different from zero at 10%, 5% and 1% level.*

	Total Liabilities, DKK				Total Bank Debt, DKK				Total Bank Debt in 2007 Primary bank, DKK			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
High Loans-to-Deposits	10,441	10,149	2,085		450.4	-467.3	-464.0		-655.6	-1,134	-814.3	
	(38,094)	(10,256)	(8,031)		(5,190)	(1,294)	(1,264)		(4,483)	(3,422)	(3,633)	
Post X High	-12,150	-10,344*	-10,580*	-10,560*	-6,231	-5,861	-6,802**	-6,741**	-15,736	-15,612*	-15,898*	-15,800*
	(9,409)	(5,393)	(6,209)	(5,878)	(4,526)	(3,703)	(2,705)	(2,698)	(9,836)	(9,215)	(8,522)	(8,496)
Covariates-year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Municipality-year FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Industry-year FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Individual FE	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Observations	933,927	933,927	933,927	933,927	942,668	942,533	942,533	942,533	941,958	941,823	941,823	941,823
R-squared	0.017	0.500	0.518	0.184	0.010	0.532	0.534	0.064	0.002	0.433	0.435	0.020

Table 4: Other outcomes

*Table 4 reports estimates from OLS regressions, where the dependent variable is total deposits measured in DKK (1), the value of stocks measured in DKK (2), pension withdrawings measured in DKK (3), disposable income measured in DKK (4) and share of the year spent in unemployment measured on a scale from 0 to 1000 where 0 is full employment and 1000 is unemployed all year (5). The main RHS variable is the indicator variable for whether the individual's primary bank in 2007 had a high loans to deposits ratio (exposed) interacted with an indicator for the crisis period 2009-2011. The dataset is collapsed into a pre- and post- period containing average values for each individual during 2005-2007 (pre) and 2009-2011 (post), with 2008 omitted. The outcome variables are winsorized at the 1st and 99th percentile separately within the pre and post period. Covariates-year fixed effects include categorical controls, all measured in 2007, for age and educational level, indicator for gender, home ownership, partner, and positive unemployment spells during current and preceding year, deciles of bank debt, deciles of income and deciles of income growth 2003-2007, all interacted with year fixed effects. Municipality-year fixed effects include 98 dummies for municipality of residence interacted with year fixed effects and industry-year fixed effects include 9 categories of occupational industry interacted with year dummies. Standard errors are clustered at the bank level and are reported in parentheses. *, **, *** indicate statistically different from zero at 10%, 5% and 1% level.*

	Deposits, DKK	Value of stocks, DKK	Pension Withdrawings, DKK	Disposable income, DKK	Unemployment, Per Mille
	(1)	(2)	(3)	(4)	(5)
High Loans-to-Deposits					
Post X High Loan-to-Deposits	-1,292	-335.0	37.32**	-250.4	-0.616
	(1,889)	(1,023)	(15.75)	(705.4)	(0.602)
Covariates-year FE	Yes	Yes	Yes	Yes	Yes
Municipality-year FE	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Observations	942,533	942,533	942,533	933,927	924,966
R-squared	0.047	0.007	0.027	0.443	0.187

Table 5: Spending outcomes

*Table 5 reports estimates from OLS regressions, where the dependent variable is imputed spending (1-2), number of active cars (3), indicator for having two cars (4) and among individuals with cars, the average weight of the car(s) (5). In model (6) the dependent variable is public real estate value of any property owned. Conditional on having purchased a house, model 7-11 has the change in the public real estate valuation (7), change in total liabilities (8) and change in house size measured in square meters (9). The main RHS variable is the indicator variable for whether the individual's primary bank in 2007 had a high loans to deposits ratio (exposed) interacted with an indicator for the crisis period 2009-2011. The dataset is collapsed into a pre- and post- period containing average values for each individual during 2005-2007 (pre) and 2009-2011 (post), with 2008 omitted. The outcome variables are winsorized at the 1st and 99th percentile separately within the pre and post period. Covariates-year fixed effects include categorical controls, all measured in 2007, for age and educational level, indicator for gender, home ownership, partner, and positive unemployment spells during current and preceding year, deciles of bank debt, deciles of income and deciles of income growth 2003-2007, all interacted with year fixed effects. Municipality-year fixed effects include 98 dummies for municipality of residence interacted with year fixed effects and industry-year fixed effects include 9 categories of occupational industry interacted with year dummies. Standard errors are clustered at the bank level and are reported in parentheses. *, **, *** indicate statistically different*

	Spending, DKK		Car Outcomes			House Outcome			
							Conditional on House Purchase in the Year		
	Without stockowners	Including stockowners	# of Active Cars	Indicator for Two Cars	Avg. Weight of Cars	Public Real-Estate Value, DKK	ΔPublic Real-Estate Value, DKK	ΔTotal Liabilities, DKK	ΔHouse Size, Square Meters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High Loans-to-Deposits							10,606**	-2,179	0.146
							(4,092)	(5,833)	(0.113)
Post X High	-3,522***	-3,895***	-0.00185*	-0.00146**	-2.857*	-8,796	-33,048***	-24,425***	-0.396**
	(1,334)	(1,265)	(0.000950)	(0.000636)	(1.543)	(5,461)	(6,776)	(7,421)	(0.185)
Covariates-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Observations	720,644	933,927	942,533	942,533	433,539	933,927	262,560	262,560	257,550
R-squared	0.164	0.150	0.022	0.014	0.016	0.093	0.267	0.247	0.018

Table 6: Heterogeneous results

*Table 6 reports estimates from OLS regressions, where the dependent variable is total liabilities. Models (1) and (2) splits the sample by whether the individual had a primary bank that was identical to any parent. Models (3) and (4) split the sample by whether or not the individual changed its primary bank during 2004-2007. Model (5) and (6) split the sample why whether the individual had multiple banks with active loan accounts during 2004-2007. The main RHS variable is the indicator variable for whether the individual's primary bank in 2007 had a high loans to deposits ratio (exposed) interacted with an indicator for the crisis period 2009-2011. The dataset is collapsed into a pre- and post- period containing average values for each individual during 2005-2007 (pre) and 2009-2011 (post), with 2008 omitted. The outcome variable is winsorized at the 1st and 99th percentile separately within the pre and post period. Covariates-year fixed effects include categorical controls, all measured in 2007, for age and educational level, indicator for gender, home ownership, partner, and positive unemployment spells during current and preceding year, deciles of bank debt, deciles of income and deciles of income growth 2003-2007, all interacted with year dummies. Standard errors are clustered at the bank level and are reported in parentheses. *, **, *** indicate statistically different from zero at 10%, 5% and 1% level.*

	Different bank than parent, 2007		Change of primary bank 2004-2007		Multiple banks during 2004-2007	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
High Loans-to-Deposits						
Post X High	-6,959	-16,984***	-9,132	-11,810***	-7,665	-13,209***
	(6,625)	(4,052)	(6,295)	(3,622)	(6,511)	(3,735)
Covariates-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	608,023	325,904	327,391	606,536	388,597	545,330
R-squared	0.179	0.196	0.210	0.168	0.195	0.177

Appendix Table A.1: Dynamic figure coefficients

Table A.1 presents the Difference-in-Difference estimates and 95% confidence intervals reported in Figures 3-7

	Total liabilities		Bank Debt		Disposable income		Pension Withdraw.		Spending (no stocks)		With-in estimate	
Coefficient	Est.	CI	Est.	CI	Est.	CI	Est.	CI	Est.	CI	Est.	CI
High X 2004	846	[-9493:11186]	2,154	[-1997:6304]	-708	[-1712:295]	-2	[-5:1]	-1,195	[-10994:8605]	0.001	[-0.03:0.04]
High X 2005	-914	[-12372:10544]	1,921	[-1530:799]	-365	[-1530:799]	-4	[-7:0]	-2,959	[-13959:8042]	-0.003	[-0.03:0.02]
High X 2006	456	[-6975:7887]	1,462	[-1226:615]	-305	[-1226:615]	-3	[-6:0]	941	[-4046:5927]	0.002	[-0.01:0.01]
High X 2007	.	.	0	.	.	.	.	.	.	.	.	.
High X 2008	-3,189	[-6565:186]	-2,202	[-956:124]	-416	[-956:124]	5	[0:9]	-4,136	[-9164:893]	0.002	[-0.1:0.1]
High X 2009	-9,669	[-15212:-4126]	-4,986	[-1667:903]	-382	[-1667:903]	10	[0:20]	-8,601	[-18227:1024]	-0.147	[-0.28:-0.01]
High X 2010	-12,187	[-18611:-5764]	-5,905	[-3069:1252]	-908	[-3069:1252]	6	[-1:13]	-2,739	[-8846:3367]	-0.084	[-0.24:0.07]
High X 2011	-9,982	[-20053:89]	-6,235	[-3285:2027]	-629	[-3285:2027]	14	[1:28]	-1,451	[-5779:2877]	-0.067	[-0.24:0.1]
Observations	3,735,529		3,767,763		3,735,529		3,767,763		2,916,493		5,000,472	

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