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Real Effects of Relaxing Financial Constraints for Homeowners: Evidence from Danish Firms

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Abstract

We study how a reduction in financial constraints faced by homeowners affects consumption, and in turn employment creation and the skill composition of the workforce over the business cycle. We leverage a wide set of Danish administrative registries, and implement a difference-in-differences strategy based on the introduction of a new mortgage product in 2003 to estimate the causal effect of a relaxation of homeowners' borrowing constraints on consumer expenditure. The estimated exogenous shift in consumption, which varies across municipalities and over time, is a strong predictor of the growth in employment for local establishments. However, the positions created due to a credit-induced demand shock differ from pre-trends: they require lower skill levels and are filled by younger, less experienced and less educated workers.

These workers, particularly those filling low-skilled positions between 2004 and 2007, face earlier separations and a higher degree of unemployment ex-post.

Resume

Vi undersøger, hvordan en lempelse af finansielle begrænsninger for boligejere påvirker forbrug, jobskabelse og sammensætningen af arbejdsstyrkens kvalifikationer over en konjunkturcykel.

For at estimere årsagssammenhængen mellem en lempelse af boligejernes lånebegrænsninger og forbrugsudgifter benytter vi et omfattende sæt af danske administrative registrer data samt en "forskel i forskel"-strategi, der er baseret på indførelsen af et nyt realkreditprodukt i 2003. Det estimerede eksogene skift i forbrug, som varierer mellem kommuner og over tid, er en stærk indikator for beskæftigelsesvækst i lokale virksomheder. De nyoprettede stillinger, der skabes som følge af et kredit-baseret efterspørgselsstød, adskiller sig imidlertid fra den forhenværende trend: de kræver et lavere niveau af færdigheder og bliver besat af yngre, mindre erfarne arbejdstagere med et lavere uddannelsesniveau. Disse arbejdstagere, og herunder især dem der fik en ufaglærte stilling i perioden 2004-07, har en øget risiko for at blive afskediget tidligere og udviser en højere grad af arbejdsløshed ex-post.

Key words

Mortgages, consumption, employment.

JEL classification

E32, D22, J21

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18 June 2019

Abstract

We study how a reduction in financial constraints faced by homeowners affects consumption, employment creation and the skill composition of the workforce over the business cycle. We leverage a wide set of Danish administrative registries, and implement a difference-in-differences strategy based on the introduction of a new mortgage product in 2003 to estimate the causal effect of a relaxation of homeowners' liquidity constraints on consumer expenditure. The estimated exogenous shift in consumption, varying across municipalities and over time, is a strong predictor of the growth in employment for local establishments. However, the positions created due to a credit-induced demand shock differ from pre-trends: they require lower skill levels and are filled by younger, less experienced and less educated workers. These workers, particularly those filling low-skilled positions between 2004 and 2007, face earlier separations and a higher degree of unemployment ex-post.

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1 Introduction

The events of recent years have stressed the importance of credit as a trigger and amplifier of shocks affecting the real economy.¹ Among other channels, financial frictions operate through households' borrowing and spending capacity. The interaction between house prices, credit growth and consumption suggests that a change in credit conditions faced by homeowners, in particular, can generate substantial swings in aggregate demand.²

Firms react to changes in demand by adjusting investment, employment and prices (Mian and Sufi, 2014; Giroud and Mueller, 2017; Di Maggio and Kermani, 2017; Mian et al., 2017b; Stroebel and Vavra, 2019). However, the employment effects of credit booms tend to be of short-term nature, and the recessions following these episodes are prolonged and characterised by amplified job losses.³ The Great Recession, in particular, appears to have hit the weakest segments of the workforce relatively harder (Hoynes et al., 2012).⁴

Part of this phenomenon can be explained by a contraction in credit supply to firms which, in downturns, affects workers at the extremes of the skill distribution the most (Barbosa et al., 2017; Berton et al., 2018). Despite the wide ongoing debate about job polarisation and jobless recoveries, we have yet to understand how credit shocks that propagate through households' balance sheets affect the composition of the workforce over the business cycle. We aim at filling this gap in the existing literature, by studying how a reduction in liquidity constraints faced by homeowners affects consumer spending, and in turn job creation in local firms and the skill composition of the workforce over the business cycle. To do so, we exploit detailed administrative data covering the entire population of households and firms living and operating in Denmark during the 2000s. Our analysis unfolds in three steps.

We begin by estimating how homeowners' consumption reacts to the introduction of interest-only (IO) mortgages in 2003. The reform allowed current and prospective homeowners to avoid repayment of the principal on their mortgage for up to ten years. This effectively reduced housing expenditure for IO uptakers, freeing resources for non-housing consumption.

We follow a difference-in-differences strategy, relying on the intuition that homeowners with low levels of liquid assets ex-ante are more likely to make use of the opportunity

¹Chodorow-Reich (2013); Di Maggio and Kermani (2017); Bentolila et al. (2017); Karmakar et al. (2017).

²Leth-Petersen (2010); Mian et al. (2013); Mian and Sufi (2014); Kaplan et al. (2016); Aladangady (2017).

³Jordà et al. (2013, 2016); Mian et al. (2017b); Martin and Philippon (2017); Di Maggio and Kermani (2017); Giroud and Mueller (2018). Giroud and Mueller (2018) reject the hypothesis that these dynamics are simple mean-reverting fluctuations in employment since resulting job creation is temporary while employment losses are persistent.

⁴See also Jaimovich and Siu (2012); Berger (2018); Deming et al. (2018); Hershbein and Kahn (2018) for a discussion of job polarization and jobless recoveries.

of a mortgage product which guarantees higher levels of liquidity ex-post (Leth-Petersen, 2010). As in Larsen et al. (2018), we find that on average liquidity-constrained homeowners significantly shift their consumption after the reform compared to a similar group of non-liquidity-constrained households.

In the second step, we construct a measure of regional variation in reform intensity, exploiting the fact that liquidity-constrained homeowners were not uniformly distributed across Danish municipalities, ex-ante. Regions which in 2002 had a higher proportion of liquidity-constrained households, because they were populated by either young or older wealthy-hand-to-mouth families, were more likely to be subject to larger demand shocks following the introduction of IO loans. Following this logic, we develop a municipality-based, time-varying measure of reform intensity, constructed as the interaction between the average reform effect in any given year, estimated in the first step, and the number of constrained homeowners living in the municipality in 2002. This interaction term provides us with a measure of *regional* consumption shocks that can be directly attributed to the introduction of IO mortgages.

Finally, we develop a panel dataset linking all establishments operating in the country to their parent firms and location, to study the effect of local demand shocks on job creation in local firms. Through municipality identifiers we link the establishment-level data to our regional measure of credit-induced local demand shocks, for each year between 2004 and 2010. The high granularity of the data is crucial for the causal identification of the effects of expenditure on employment, since it allows us to partial out all firm-level time-varying shocks. These include changes in credit supply *to the firm*, which might be triggered by the IO reform and, potentially, drive job creation directly. Since credit lines are negotiated at the firm level, the inclusion of firm-by-year fixed effects allows us to isolate the role of local demand.

Firms respond to an increase in demand by creating jobs: Our preferred specification indicates that for each 10 percent increase in annual household consumption at the municipality level, employment for local establishments increases by 3 percent in any given year. The employment effect is significantly larger for non-tradable firms than for tradable ones, consistently with Di Maggio and Kermani (2017) and Mian et al. (2017b).

By matching employers with their employees we take an additional step compared to the existing literature, which up to now predominantly made use of US data, and study how credit-induced demand shocks affect job and worker characteristics of new matches. We find that, within the same firm, establishments that are subject to larger demand shocks create more jobs characterised as low-skilled occupations, filled by younger, less experienced and less educated workers. Also, new workers have different ex-post outcomes compared to employees hired into the same establishment but at different phases of the business cycle:

they are more likely to experience earlier separations and to spend more time in unemployment in the subsequent years. This applies in particular to low-skilled workers hired during the boom phase. A 10 percent increase in regional consumption leads to an increase in the unemployment share of low-skilled workers hired in the boom phase worth 0.7 years, across all years subsequent to the match. This result holds even when controlling for the contemporaneous unemployment rates of low-skilled workers who joined the same establishment, just in a different phase of the business cycle.

This paper is, to the best of our knowledge, the first to document how a credit-induced demand shock affects the composition of the workforce over the business cycle. Furthermore, our findings provide a new lens to understand why the Great Recession hit low-skilled workers harder. We show that negative credit supply shocks to firms are not the only channel through which financial frictions induce a differential business cycle vulnerability across occupations: credit-fuelled demand shocks in the boom phase sow the seed for excess volatility in unemployment of low-skilled workers ex-post.

These results bridge two strands of literature: one dealing with the relationship between credit frictions and real economic activity and the other with the volatility of employment across jobs and worker characteristics over the business cycle.

Mian and Sufi (2014) find that during the Great Recession U.S. areas which witnessed the largest drops in housing net worth also experienced the largest reductions in employment; focusing on firm-level heterogeneity, Giroud and Mueller (2017) look at the response of employment to consumer demand shocks in the Great Recession, finding that relatively more levered firms reduced employment to a larger extent in areas where the decline in house prices was relatively more severe. Another strand of the literature deals with changes in credit conditions to firms: Chodorow-Reich (2013) finds that a drop in credit availability to firms had a significant role in explaining job losses during the Great Recession, while Giroud and Mueller (2018) show that establishments operating in regions with larger firm-credit buildups experience stronger short-run employment growth which is followed by more severe job destruction in the medium run.

Most closely related to our work are Mian et al. (2017a) and Di Maggio and Kermani (2017) who focus on the employment effects of a change in credit provision to households (as opposed to firms) and, like us, focus not only on the bust phase of the cycle, but study also the dynamics in a boom. Our paper confirms that credit provision to households can work as a strong amplifier of the business cycle, not only in downturns but also during upswings. We contribute to this literature by studying the composition and characteristics of the jobs created due to credit-induced local business cycles and by providing new evidence

that suggest that certain job categories may be particularly vulnerable to these fluctuations.⁵

By uncovering substantial heterogeneity in workers' characteristics and job typologies created due to a shift in household credit, we also speak to the literature on sector and skill level heterogeneity over the business cycle. Particular sectors and types of jobs seem to be relatively more exposed to economic volatility. Cross-country comparisons, for example, suggest a positive correlation between credit expansion to firms and the growth of low-productivity sectors (Borio et al., 2016). Moreover, young and low-educated workers' employment fluctuations seem to be relatively more cyclical (Hoynes et al., 2012).⁶ Also, workers associated with firms that are affected by adverse credit supply shocks have longer subsequent unemployment spells (Hochfellner et al., 2015), are low-educated (Berton et al., 2018) or at the extremes of skill distributions (Barbosa et al., 2017). While these studies focus on the effect of a contraction in credit supply to firms, we show that the expansion in credit to households in a boom phase carries additional explanatory power towards these dynamics, by inflating low-skilled jobs which are, however, short-lived.

This paper proceeds as follows: section 2 describes the different administrative data sources and defines the variables of interest, while section 3 presents our empirical framework. Section 4 presents the results on how IO loans affect consumption and, in turn, employment. Section 5 introduces heterogeneity across the labour force, and studies the ex-post outcomes for these workers. Section 6 concludes.

2 Data

Our data stems from several administrative registries covering the entire population of households and firms living and operating in Denmark. These are collected and administered by Statistics Denmark (DST). This section describes the various sources and procedures we use in order to develop our variables of interest.

⁵Methodologically, our paper is also related to the vast macroeconomic literature that applies microdata and regional heterogeneity to infer aggregate dynamics. Despite the fact that geographic differences may wash out in the aggregate, Nakamura and Steinsson (2018) highlight how useful this approach can be for assessing the importance of a channel for aggregate dynamics. Beraja et al. (2016) find that for short time horizons and especially for the crisis years 2007-2010, inferring aggregate responses from regional employment effects of demand shocks leads to similar results as those provided by a model that takes both regional and aggregate data into account.

⁶See also Berger (2018); Deming et al. (2018); Hershbein and Kahn (2018); Jaimovich and Siu (2012) on jobless recoveries and job polarisation with a focus on changes in skill requirements within occupations.

2.1 Individuals, households and consumption imputation

Each person who is tax liable in the country can be identified in the individual registries through an anonymized version of CPR number, the Danish equivalent of social security numbers.⁷ This number can be used to match the population registries to their income, taxes, wealth and the balance sheet components across registries. Data on income and assets comes directly from the Central Customs and Tax Administration (SKAT), an affiliated agency of the Danish Ministry of Taxation (Skatteministeriet), and is available through the registry IND on an annual basis.⁸

Individuals are mapped to households which have a unique family identifier: a family is defined as one adult or two adults co-habiting plus dependent children. Adult children below the age of 25 are considered part of the household if residing at the same address as their parents. The family data provides information on the households' residence location, number of children and the age of the youngest and the oldest child. Furthermore, we observe the form of residence (owner/renter) for every year and the ownership start date for families owning a property. Therefore, we can observe whether households move, buy or sell property. The value of the households' residence (if owned) is derived from property tax records and is available since 2004.⁹

Household-level income and balance sheets can be used to impute annual household expenditure, defined as the sum of disposable income minus changes in net wealth, following the work of Browning and Leth-Petersen (2003), Browning et al. (2013) Leth-Petersen (2010), Andersen et al. (2016) and Hviid and Kuchler (2017). In particular, disposable income is the sum of wage income, capital income and social benefits. From this sum we deduct taxes (e.g. on labour and capital income, municipal taxes and property value tax on housing), alimony payments and tax-exempted interest expenses. The latter include interest paid to banks for mortgages and student loans.¹⁰ The change in net wealth consists of net income from pension schemes as well as the change in stock assets and deposits from the previous to the current year-end, respectively.¹¹ Changes in deposits include not only changes in cash

⁷Foreigners who live in a dwelling and stay in Denmark for more than 3 months have a CPR number (Bertheau et al., 2017).

⁸Labour incomes (and other information) are directly reported by employers, see <https://www.dst.dk/da/TilSalg/Forskningservice/Dokumentation/hoekvalitetsvariable/beskaeftigelsesoplysninger-fra-det-centrale-oplysningssedelregister>

⁹We impute data for 2002 property value using the average coefficient of residential property appreciation at the municipality level between 2002 and 2004 and applying it to the recorded house value in 2002.

¹⁰The definition of disposable income also includes the calculated rental value of own housing. However, we deduct this again when imputing household spending and it can therefore simply be disregarded.

¹¹Because data on private pension savings is unavailable before 1999, we also impute consumption excluding private pension contribution. The imputation of consumption with and without pension savings is substantially different for self-employed workers, which we therefore exclude from the sample. Some system-

holdings but also evolution in the market value of bonds and mortgage deeds from one year to the next. The latter aims at approximating payments on mortgage debt which are to be deducted from disposable income when imputing consumption. Payments on other forms of bank debt are similarly calculated as the yearly change in a household's outstanding debt.

A general caveat of the procedure is that changes in asset prices can falsely change imputed consumption even though asset revaluations might not affect actual spending. For instance, the data does not allow us to distinguish between changes in net wealth due to innovations in stock prices and realised capital gains or losses from trading activities: if a household invests into stocks this savings decision will correctly reduce spending in that year. However, if the price of an asset that a household holds (and does not sell) increases, this will erroneously reduce imputed consumption through an increase in the value of its savings.¹² To prevent an analogous bias arising from changes in house values, we exclude housing assets from the imputation just as Browning et al. (2013), Leth-Petersen (2010), Jensen and Johannesen (2017) and Hviid and Kuchler (2017).

Another bias which is particularly important in our context relates to mortgage debt. In Denmark each mortgage loan is funded through a bond issued by the lender. The mortgage bond matches the maturity and the type of loan contract regarding fixed- versus adjustable-rate (ARM or FRM) and borrowers can refinance their mortgages at any time by buying the underlying bond(s) at par and issuing a new one. This can be especially attractive in times of falling market rates for fixed-rate mortgage holders.

Since we only observe the market value of the underlying bond, changes in market rates may bias our consumption measure. If market rates fall, the bond value of FRM increases which erroneously raises imputed expenditure (since the market value of the households' liabilities increases, and in turn net wealth is reduced). Market rates experienced a continuous decrease in the years between 2001 and 2003, remained stable until the end of 2005 after which they gradually rose until the beginning of the fourth quarter of 2008.¹³ Refinancing fixed-rate contracts is mainly attractive in years of falling market rates (Browning and Leth-Petersen, 2003) and therefore unlikely to affect our consumption measure in these years. ARM are naturally unaffected by changes in market rates. Therefore, the size of the

atic differences also arise for people above the age of 50. However, since the estimations rely on within-family variation over time, to a large extent these differences in values between the original consumption measure and the one excluding private pensions will be taken into account.

¹²In robustness tests, Jensen and Johannesen (2017) ensure that adding changes in the general stock market index back into imputed consumption or excluding all stock owners from the sample does not make a substantial difference for imputed consumption over the period 2003–2011. We follow Leth-Petersen (2010) by not attempting to control for these changes as we have no information on the type and quantities of stocks held by a household.

¹³See e.g. http://www.nationalbanken.dk/en/marketinfo/official_interestrates/Pages/Default.aspx

bias also depends on the importance of FRM and their evolution over time. In 2003, the value of outstanding mortgages of this type stood at around DKK 580 billion out of total outstanding mortgages of almost DKK 800 billion. However, the nominal amount of FRM remained fairly stable over the following 13 years while the adjustable rate market (conventional mortgages and IO mortgages) has grown substantially (Larsen et al., 2018). The share of ARM in all mortgages rose from 30 percent in 2003 to 63 percent in 2015 (Larsen et al., 2018).

Moreover, most interest only (IO) mortgages, which after their introduction in 2003, quickly became the main mortgage typology (they accounted for 56 percent of all mortgages in 2012–2013) are adjustable-rate (Larsen et al., 2018) and therefore not affected by movements in market rates. This implies that the arising bias should become smaller over the years.¹⁴

Finally, changes in mortgage types and refinancing do not change imputed consumption as long as the underlying bond values are identical. Imagine a household switching from a conventional mortgage to an IO mortgage with identical underlying bond values. While under the old contract the household’s mortgage payments consisted of instalments towards the principal plus interest, under the new contract only interest is paid. Because we do not directly observe mortgage payments, if the household spends only a part of the resulting decrease in overall mortgage payment this will falsely *decrease* imputed consumption compared to the previous year – even though actual spending rose. This is because we only observe that liabilities (the bond value) are unchanged but deposits in the bank increased. Even if the household spent all the additional cash on hand, the measure of imputed consumption would be unchanged despite the fact that actual spending might have risen substantially. As a result, we lose all boosts in consumption that occur due to switchings happening without an increase in the underlying principal. In other words, we interpret our evidence as reflecting a lower bound in the consumption estimates.

We restrict our estimations to a balanced panel of households continually observed between 1996 and 2010. We furthermore restrict the sample to households in which the eldest person must be at least 24 and younger than 66 (in order to study the decisions independent from heavy life-cycle considerations); we exclude households who at any point in time change tenure status (switching from renting to owning, or vice versa) or buy or sell a housing property at any given point in time (Leth-Petersen, 2010). This is because we are interested in evaluating expenditure in goods other than housing: including households who buy, or

¹⁴From 2009 onwards, detailed data on repayment and revaluation of mortgages is available. Hviid and Kuchler (2017) show that when improving the consumption measure for the years 2009–2015 by only considering changes in repayment, analysed consumption patterns are robust to those simply using the change in market values of bonds.

switch from renting to owning would impede this, as the reform is likely to affect their housing consumption primarily.

2.2 Firms and establishments

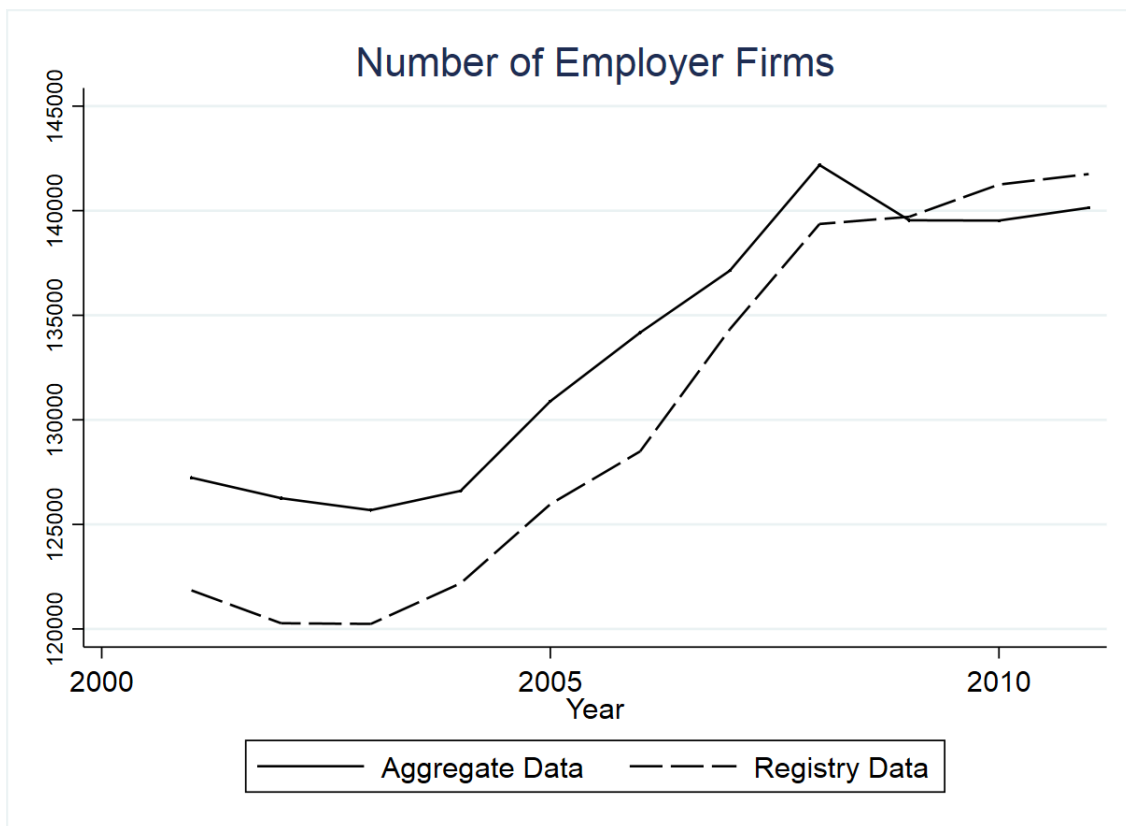
The Central Business Register (*CVR, Det Centrale Virksomhedsregister*), owned by the Danish Business Authority (*Erhvervsstyrelsen*), assigns each legal entity in Denmark a unique (anonymised) identifier (CVR Number). Sector and industry codes are available for each firm as well as detailed balance sheet information on an annual basis, all available from the firm-level dataset FIRM. The registry-based data matches reasonably well the aggregate figures on the number of firms provided by Statistics Denmark in any given year and its trend over time. We observe that number of employer firms (i.e. active firms with at least one employee, excluding self employed entrepreneurs) is lower than the aggregate figures by about 5,000 units out of 120,000, a difference that is roughly consistent across the years (Figure 1). This is explained by the fact that our micro-data excludes financial companies and firms active in the agricultural sector. The year 2009, however, shows a discontinuity, which can be attributed to the change in definition of non-employer firms for the purpose of compiling aggregate statistics in the year 2008.

We exclude firms without any employees at the end of the year (sole proprietors) and firms with imputed balance sheets. Other than these exclusions, we work with the entirety of employer firms operating in Denmark between 2004 and 2010.

Based on the data on firms' balance sheets (registries FIRE/FIRM), we define tradable firms by the sum of their export and import volumes divided by the number of employees at the firm level. To be consistent with Mian and Sufi (2014), a firm is defined as "tradable" if this ratio in a given year exceeds DKK 70,000 (roughly USD 10,000). We also observe other balance sheet characteristics, such as assets and investment at the firm level on an annual basis.

Finally, through the registry FIDA, firms can be linked to each of their establishments (i.e. plants/workplaces) and to the establishments' municipality of operation. Each establishment in that dataset is uniquely identified through the ARBNR (Arbejdsstedsnummer). If a firm is a single-establishment firm, the firm and establishment are by definition identical and included in our establishment level dataset along with all the above information. Each establishment can also be linked to the employees on its payroll in each given tax year. In particular, FIRA provides the number of workers (defined as full-time equivalents) at the establishment level for every year (i.e. at the end of November). For the years 2004 until 2010, we compute the log of employment at the establishment level as the head count of

Figure 1: Performance of registry data in matching the aggregate figures



full-time equivalents.

2.3 Jobs

To construct a dataset with information on job typologies and worker characteristics, we make use of the possibility to link the individual with the establishment data. Through FIDA, we merge uniquely identified individuals (CPR) and establishments (ARBNR) which in turn are linked to their characteristics via the individual- and the firm/establishment-level datasets, respectively. The availability of employer-employee relationships allows us to identify the characteristics of new matches created in each year. A new match is created when an individual is associated with an establishment and was not associated with such establishment in the previous year.

Using the individual-level worker data, we can compute establishment-level averages of several different characteristics of the existing workforce and the new hires, such as age, experience, education. We furthermore count the number of new matches classified as low,

intermediate or high-skilled positions (from the registry AKM).

Finally, for each individual match, we define two forward-looking variables. The first is the job tenure expressed in years, or the length of the employment relationship looking forward. Whenever a CPR is no longer associated with the establishment (ARBNR), we consider the employment relationship terminated. Due to the nature of the data we cannot distinguish between voluntary and involuntary separations; however, we proxy for involuntary separations by constructing the second forward-looking measure, the percentage of unemployment that each worker will experience as an average in the years subsequent to a match, which we compute through the registry IDAP. For both of these forward-looking variables (tenure at workplace and percentage of unemployment in the future) we take establishment-level averages for the 2002 workforce as well as for new hires in every year between 2004 and 2010. We do so for all three skill levels (high, intermediate, low), in order to control for ex-ante levels in workforce composition.

3 Methodology

This section describes our empirical approach. We begin by outlining how we estimate the effect of the reform on household expenditure and how we use geographical identifiers to link these local demand shocks to firms. Finally, we discuss the methodology for analyzing firm-level outcomes.

3.1 Mortgage reform

Denmark experienced a major mortgage reform in 2003. From the first of October, interest-only (IO) mortgages became available, allowing current and prospective homeowners to delay payment towards the principal of their mortgage by 10 years, with the option to extend the interest-only period upon satisfaction of the bank's own assessment of lending risk.¹⁵ IO mortgages quickly became a very common mortgage typology, particularly among young and old households (Larsen et al., 2018). The market value of outstanding IO mortgages accounted for 50 percent of total mortgage volume in 2007 and 56 percent in 2012–2013 (Larsen et al., 2018).

This reform can be seen as an unanticipated and exogenous shift in liquidity constraints faced by all existing homeowners.¹⁶ Simple economic intuition, however, suggests that poorer

¹⁵If after those ten years, the value-to-mortgage ratio is below 80 percent, there is the possibility for the borrower to refinance into a new IO mortgage which effectively prolongs the period of interest-only payments by another 10 years (Larsen et al., 2018).

¹⁶Consider that we exclude people who switch from renting to owning a house in all our estimations.

and more constrained households might be more likely to make use of the reform: while this entails a lower level of savings (i.e. capital repayment) and possibly higher interest rates, it simultaneously allows them to divert resources from housing expenditure (i.e. mortgage principal repayment) to other forms of expenditure.

From a theoretical perspective, financially or liquidity constrained households would, in the absence of these constraints, consume relatively more.¹⁷ Indeed, Larsen et al. (2018) document that young households with relatively high ex-ante leverage or old, wealthy-hand-to-mouth consumers, are more likely to take up an IO mortgage.

3.2 Effect of the reform on consumption

According to this simple theoretical framework, homeowners with low liquid assets should be more likely to take up IO mortgages and use this excess liquidity for consumption.¹⁸ We exploit this intuition in a difference-in-differences setting, comparing households with similar income, demographics and assets ex-ante who hold different levels of liquid savings before the reform, in order to study how their consumption paths develop ex-post. In doing so, we follow Leth-Petersen (2010) who adopted this for estimating the consumption effects of another mortgage reform taking place in Denmark in 1992, allowing for the first time home-equity-based borrowing.

Our outcome variable is the within-household change in imputed consumption. This is computed as the moving average for each given year after the reform, with respect to a similar time window prior to the reform, to alleviate the noise in the consumption measure which arises due to imputation. Moving averages are useful in removing excess noise in the imputed consumption data, as often year-on-year changes display significant outliers (Leth-Petersen, 2010). For each household-year cell, the consumption measure is defined as follows:

$$\Delta C_{h,2004} = C_{2004} - C_{2002}$$

$$\Delta C_{h,2005} = [C_{2004} + C_{2005} - (C_{2002} + C_{2001})]/2$$

$$\Delta C_{h,2006} = [C_{2004} + C_{2005} + C_{2006} - (C_{2002} + C_{2001} + C_{2000})]/3$$

$$\Delta C_{h,2007} = [C_{2004} + C_{2005} + C_{2006} + C_{2007} - (C_{2002} + C_{2001} + C_{2000} + C_{1999})]/4$$

and so on, for each year up to 2010.

Because we do not observe mortgage choice directly for this time frame, our difference-

¹⁷In such a framework, even temporary increases in current income may affect consumption positively in the short run. For early work on life-cycle models with liquidity constraints, see Dolde (1978); Alessie et al. (1997). (Dolde, 1978; Muellbauer, 1983). For later work on liquidity-constrained households that can borrow against home equity in order to smooth consumption, see Hurst and Stafford (2004).

¹⁸Pedersen (2017) specifically derives from a theoretical model that IO mortgages have a permanent positive effect on the indebtedness of households and a positive response in consumption appears due to a relaxation of their financial constraints.

in-differences framework is based on an alternative individual-level observable: following Leth-Petersen (2010), treated households are those defined as liquidity constrained. Our chosen cutoff defines as liquidity-constrained the households possessing liquid assets worth less than 1.5 months of disposable income before the reform, or at the end of 2002.¹⁹ The threshold of 1.5 months of income is broadly consistent with the existing literature (Leth-Petersen, 2010); furthermore, it splits our sample of homeowners exactly in half, enabling common support between treatment and control groups.²⁰ This threshold is also highly correlated with IO ownership, measured ex-post.²¹ Of course, our control group is likely to have been affected by the reform as well, but we rely on the intuition that treatment probability and intensity differ between our two groups.

Identification follows from the assumption that assignment to the respective group – or the share of liquid savings to income – is exogenous to the reform, and defines the probability of treatment.²²

Equation 1 defines the setup:

$$\Delta C_{h,t} = \alpha_{0,t} + \alpha_{1,t}D_{h,2002} + \alpha_{2,t}X_{h,2002} + \epsilon_{h,t} \quad (1)$$

We regress for each year $t = 2004 - 2010$ our outcome variable, $\Delta C_{h,t}$, on a constant and a set of household characteristics such as age, income, family size, house value, $X_{h,2002}$, measured in 2002.²³ The dummy $D_{h,2002}$ takes value one if a household was liquidity constrained in 2002 and $\alpha_{1,t}$ is (in each year t) our coefficient of interest. As discussed above, the estimated average expenditure effect of the reform is based exclusively on households who were homeowners in 2002 and remain homeowners throughout the sample period; who do not sell or buy housing; and who do not change housing tenure status. This estimation yields a time-varying coefficient on the average change in the consumption path of liquidity-constrained households vis-à-vis non-liquidity-constrained ones after the reform.

¹⁹Liquid assets are defined as the sum of stocks, bonds and bank deposits held at the end of the year

²⁰See Figure A1.

²¹We observe the typology of mortgage holdings only starting with 2009, through the registry REAL. The relationship between liquid savings to income in 2002 and IO mortgage holdings in 2009 is highly non-linear; up to two months of income the correlation is strongly positive and declines afterwards (see Figure A2).

²²The bill regarding IO mortgages was passed on June, 1st, 2003 and was first debated in spring of the same year. This short time horizon leads us to believe that the reform was unexpected up to the end of 2002.

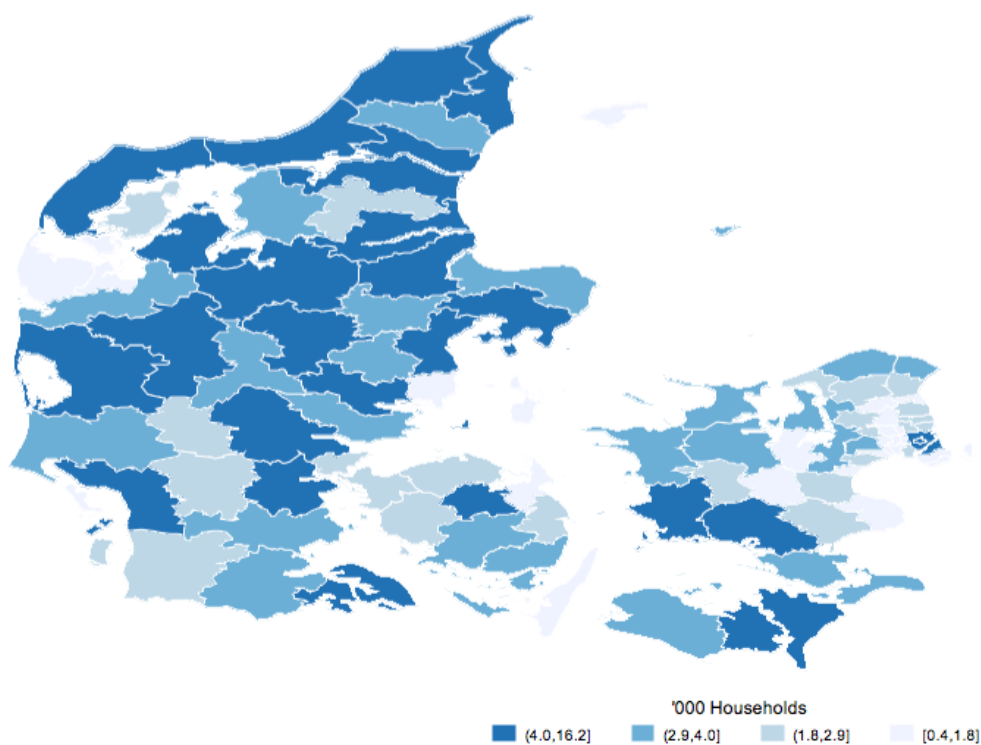
²³Since those variables measured ex-post might in turn be affected by the reform, we take ex-ante values, following the approach in Leth-Petersen (2010).

3.3 A regional measure of expenditure shocks

In order to link the decisions of households to the decisions of firms, we exploit regional variation in treatment intensity, as firms are more likely to react to the expenditure shocks that they observe directly, those occurring in the areas in which they operate. Figure 2 shows that while the areas surrounding Copenhagen and the general capital region feature high numbers of households that are liquidity constrained before the reform, in other regions these households are a minority in absolute terms.

The municipalities that ex-ante (in 2002) hosted a larger number of liquidity-constrained homeowners are likely to have experienced a more pronounced increase in household expenditure after the reform, and treatment intensity will therefore vary across the territory.²⁴

Figure 2: Number of liquidity-constrained homeowners in 2002, by municipality



Formally, our regional measure of credit-fuelled expenditure shocks is defined as follows:

$$C_{mt} = \alpha_{1,t} * N_{2002,m} \quad (2)$$

²⁴Currently there are 98 municipalities in Denmark, following a reform in 2007 that reduced their number from the 270 before the reform. Since some municipalities were merged and others were split, we assign households and firms to the new municipality codes based on the municipality code assigned to the individual building (BOPIKOM) after 2007.

where $\alpha_{1,t}$ are the coefficients estimated in Equation 1, expressing the average treatment effect on liquidity-constrained homeowners; these coefficients vary only over time, between 2004 and 2010; and $N_{2002,m}$ is the number of ex-ante liquidity-constrained homeowners in a given municipality in the year 2002 (prior to the introduction of the reform). The outcome reflects a shift-share measure for the aggregate increase in consumption that varies across municipalities and over time.

In doing so, we follow different strands of the related empirical literature, combining time-varying measures of treatment intensity based on regional characteristics as in Mian and Sufi (2014) and Aladangady (2017) with approaches that exploit the underlying composition of the population, as in Di Maggio and Kermani (2017).

To provide a numerical example, the number of constrained homeowners living in Copenhagen in 2002, for whom we have observations in every year afterwards and who never bought, sold or moved is 12,100. The estimated shift in consumption for the municipality of Copenhagen in 2004 will be the product of the aggregate $\alpha_{1,t}$ in 2004 (DKK 5,100) multiplied by 12,100, or roughly DKK 62 million (USD 10 million). In the same year, Roskilde experienced a demand shock worth only DKK 18 million (USD 3 million), since the number of liquidity constrained homeowners living in Roskilde in 2002 were roughly a third of those living in Copenhagen.

Clearly, these magnitudes are likely to underestimate the aggregate effects of the reform, for a variety of reasons. To begin with, we exclude from these calculations the potential effects of the reform on non-liquidity constrained homeowners, who are nevertheless likely to be treated and shift their expenditure in response to the introduction of IO loans. We also exclude people who switched into homeownership in any given year after the reform introduction. These consumers probably reduced their cash-flow housing expenditure with respect to renting a similar housing unit, and therefore might have increased non-housing consumption. Finally, we focus exclusively on the refinancing channel which pertains to existing homeowners, and completely disregard the general equilibrium effects that could arise through the effect of the IO reform on house prices, and the associated wealth and collateral effects.

Of course, a potential concern with this regional time-varying measure is that the distribution of constrained homeowners in 2002 across the territory is far from being random. This measure is in fact endogenous to many other confounding factors, such as housing appreciation and income growth, for example. The fact that our measure in Equation 2 is developed as an interaction term, however, allows us to control for time-invariant characteristics of the municipalities, including the direct effect of having a higher share of constrained

homeowners in 2002 on employment.²⁵

3.4 Establishment-level outcomes

To evaluate the effects of this exogenous shift in consumption on labour market outcomes, we exploit registry data on the universe of firms/establishments operating in Denmark between 2004 and 2010, partly in combination with the individual data on each of their employees. Exploiting the high data granularity, we define our baseline model as follows:

$$Y_{ifmt} = \alpha + \beta_1 \log(C_{mt}) + \beta_2 \log(X_{f,t-1}) + \beta_3 \log(HP_{m,t-1}) + \psi_m + \theta_f + \phi_t + \epsilon_{ifmt} \quad (3)$$

where Y_{ifmt} represents outcome variables such as employment or the characteristics of the workforce measured for establishment level i , located in municipality m and belonging to firm f , at the end of year t .²⁶

Our coefficient of interest is β_1 , which measures the effect of the credit-induced local demand shock. This measure is defined in Equation 2.

While the number of constrained homeowners in 2002 is not randomly distributed across the territory and can be related to omitted municipality-level characteristics driving expenditure and employment growth directly, the introduction of municipality and year fixed effects requires only the *interaction* between the reform effect and the count of constrained homeowners to be exogenous to local employment. Adding municipality fixed-effects controls for the time-invariant characteristics of these regions, including the direct effect of the number of constrained homeowners in 2002 on labour market outcomes; year fixed effects capture aggregate macroeconomic shocks (e.g. interest rates, inflation), including the overall average effect of the reform on employment. In other words, we follow the approach in (Aladangady, 2017) in computing a shift-share measure based on two variables that cannot be considered exogenous in themselves, but whose interaction is conditional on controlling for other time-varying factors. Of course, these municipalities might have been on different consumption and employment trends already before the reform. However, this does not seem to be the

²⁵Furthermore, within-municipality growth in consumption does not differ depending on the number of liquidity-constrained homeowners before 2003; consumption trends appear to diverge between high-treatment-intensity municipalities and low-treatment intensity ones only *after* the introduction of IO loans. See Figure A4 in Appendix A.

²⁶We begin by analysing the employment effects of the demand shock, where employment is calculated as the head count of employees on the establishment i 's payroll at the end of year t ; we follow with analysing the characteristics of the workers hired in these establishments in any given year, calculated as the establishment-level averages of age and experience (in years), as well as the head count of workers with a university degree and head count of jobs classified as low, intermediate and high skilled. Finally, we develop two forward-looking characteristics for all workers in a given establishment: the length of the employment relationship in years for both old and new hires; and the average share of unemployment in each given year after year t .

case: growth in aggregate regional expenditure is uncorrelated with the number of liquidity-constrained homeowners before 2003 and positively correlated only after 2003 (see Figure A4 in Appendix A). Similarly, employment trends in regions do not react differently depending on the number of liquidity-constrained homeowners before 2003; the switch occurs after the reform is introduced (Figure A5).²⁷

With the addition of firm fixed effects, β_1 measures the effect of the local demand shift exploiting within-firm variation over time, hence comparing outcomes for the same firm once it is subject to different degrees of local demand shocks, after partialling out macroeconomic fundamentals and time-invariant characteristics of the municipalities in which they operate.

To take into account that firms' balance sheets might matter for our outcomes directly, we control for a vector of firm-level balance sheet factors, $X_{f,t-1}$, such as total assets and investment. A more restrictive specification includes firm-by-year fixed effects, which instead of using variation within the firm exploits cross-sectional variation across establishments belonging to the same firm in any given year. This model has the downside of restricting our specifications to multi-establishment firms, but it allows us to control for all firm-time varying shocks, including changing credit supply and credit demand. This is of particular importance because credit lines available to the firm can be positively affected by the IO reform, and this may in turn affect our outcomes directly. Since credit lines are negotiated by the legal entity (firm), partialling out all firm-specific time-varying shocks captures the role of credit supply to the firm and makes it possible to isolate the effect of local demand.

A further challenge to this specification could arise from time-varying regional shocks which are simultaneously correlated with the differential growth in consumption of liquidity-constrained homeowners and with local employment. The most obvious candidate is house price growth, and the associated wealth and collateral effects which would induce financially constrained homeowners to consume more than similar homeowners with more liquid savings, independently from credit availability. However, such effects should be at least partially captured by the coefficient β_3 , which measures the direct effect of $\log(HP_{m,t-1})$, a municipality-level yearly house price index. Furthermore, following recent literature, we run some placebo tests using the degree of tradability/non-tradability of firms and sectors (Mian and Sufi, 2014; Di Maggio and Kermani, 2017; Mian et al., 2017b). If our coefficient β_1 is capturing yearly shocks unrelated to local demand, such as changes in productivity or policy, the employment effects of the reform will most likely affect all firms operating in a given municipality. On the other hand, if what we are capturing is really the effect of an exogenous shift in local demand, our effects should be dominant in the tradable firms

²⁷The identification of municipality for establishments is not possible before 2001; therefore we cannot compute pre-trends of our employment variable at the municipality level before this year.

and sectors, which are more exposed to local consumers' expenditure, and therefore to the introduction of the credit reform.

Finally, when examining job characteristics and ex-post outcomes of the new hires, we add controls for the ex-ante differences in an establishment's composition of the workforce, measured as the pre-reform values of the respective dependent variable, $Y_{ifm,2002}$. The inclusion of these controls in the analysis on job characteristics stems from the intuition that different establishments may employ different typologies of workers, since they fulfil different needs within the same firm (i.e manufacturing vs IT). In other words, while head-count employees measure a homogenous quantity, workforce characteristics are an intrinsically heterogeneous one. Without controlling for the ex-ante composition it would still be possible to say that establishment A is hiring more low-skilled employees with respect to establishment B, for example; but this could simply be due to the fact that the number of low-skilled employees required for the particular production structure of establishment A is higher than in establishment B in steady-state. Controlling for the pre-existing composition of the workforce at the establishment level captures these level differences and allows us to identify whether two establishments with a similar composition of the workforce ex-ante change their composition over time, and whether they do so differentially depending on the degree to which they are subject to a demand shock.

4 Consumption and employment

In this section, we estimate the effects of the reform on consumption and outline the first part of our main results: the effect of credit-induced local demand shocks on employment.

4.1 The effects of the reform on expenditure

Table 1 displays the average effect of the IO reform on yearly household expenditure. The coefficients depict the differential consumption paths of constrained versus non-constrained homeowners with respect to the pre-reform period, as defined in $\alpha_{1,t}$ from Equation 1. Columns 1 to 3 test whether we observe any discontinuity in the consumption paths of these two groups of homeowners before 2003.²⁸

Before 2002, liquidity-constrained homeowners systematically increased their expenditure levels by less than a comparable group of non-constrained homeowners. In 2002, the change is positive, albeit very close to zero and not statistically significant. However, after 2003,

²⁸We also run placebo tests, using as dependent variables a two-year moving average of consumption around pre-reform years that should have no effect on outcomes (1998, 1999, 2000). The results are presented in Table A4 and show that the shift in consumption occurs exclusively after 2003.

this pattern reverses: the change in annual consumption between 2002 and 2004 is DKK 5,100 (roughly USD 780) higher for the treatment group, constrained homeowners, than for the control group (column 4). This estimated effect of the 2003 reform rises progressively over the years, up to DKK 10,900 (roughly USD 1,650) in 2007 and then progressively fades out through the crisis and recession years.²⁹ In 2010 the estimated effect of the reform is more noisy and only amounts to DKK 1,300 (roughly USD 200) a year in additional expenditure.³⁰

Despite the absence of positive pre-treatment trends, the concern that the two groups of homeowners may be fundamentally different in some unobserved characteristics remains. Table 2 addresses this issue by estimating the same DiD on renters who cannot be affected by the reform, independently of their degree of liquidity constraints. This estimation provides a useful placebo test. Consistently with the results found among homeowners, before 2003 liquidity-constrained renters change their consumption negatively with respect to similar unconstrained renters (columns 1 and 2). However, this difference between the two groups remains negative in the post-reform period (columns 3 - 8). This suggests that common shocks affecting liquidity-constrained households after the reform are unlikely to be driving the main results presented in Table 1.

4.2 Employment effects

The introduction of IO mortgages at the end of 2003 had a significant impact on consumer demand. Liquidity-constrained homeowners increased their spending by up to DKK 10,900 per year at peak.³¹ In this section we use the interaction between the yearly coefficients in Table 1 and the number of liquidity-constrained homeowners living in a given municipality before the reform to study the employment effects of local demand shocks. Using Equation 3 as a reference, Table 3 presents the baseline employment estimations.

On average, a 10 percent change in household expenditure leads to a 3 percent within-

²⁹Given the nature of our consumption imputation, in order to significantly increase expenditure households should increase leverage, all else held equal. We check whether this is the case in Table A1 in Appendix A. Our treatment group is more indebted already ex-ante (columns 1–3); however, they lever up significantly more after 2003 (columns 4–10), a finding that is consistent with that of Larsen et al. (2018).

³⁰These changes in consumption could of course reflect differential income paths of liquidity constrained homeowners versus non-constrained ones occurring after the reform, particularly since this category of consumers tends to be either younger or closer to retirement. We address this concern (omitted income variable) in Table A2 in Appendix A. In most years the income of liquidity-constrained homeowners changes negatively as opposed to non-constrained ones. In 2004–2006, these income changes are positive, however, amounts do not cover the positive shifts in consumption in the respective years. Furthermore, the results are robust to controlling for the contemporaneous changes in income and other explanatory variables (Table A3).

³¹When summing up the yearly regional demand shocks, aggregate spending shifted out by up to DKK 3.5 bln a year, due to the mortgage reform, see Figure A3 in Appendix A.

Table 1: Effect of reform on homeowners' expenditure

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ΔC_{2000} Pre Reform	ΔC_{2001} Pre Reform	ΔC_{2002} Pre Reform	ΔC_{2004}	ΔC_{2005}	ΔC_{2006}	ΔC_{2007}	ΔC_{2008}	ΔC_{2009}	ΔC_{2010}
Constrained ₂₀₀₂	-10.3*** (1.27)	-8.5*** (1.17)	0.5 (1,07)	5.1*** (1.2)	4.5*** (0.95)	9.1*** (0.84)	10.9*** (0.78)	8.4*** (0.74)	5.9*** (0.69)	1.3** (0.65)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	99,443	99,471	99,472	99,491	99,487	99,455	99,421	99,387	99,347	99,283
R-squared	0.008	0.011	0.024	0.005	0.019	0.045	0.088	0.130	0.164	0.175

Source: Registry data, Danish population, 1996-2010. *Notes:* Results include only a balanced panel of households that were recorded as homeowners for tax purposes in the year 2002 and never moved or changed tenure status through the sample period. Coefficients to be interpreted in DKK thousands. The dependent variable is the within-household change in the moving average of imputed consumption for each given year from before and after the reform. Household level controls include the house value in 2002 (logs); annual household income (logs); age of the household head (defined as the person with the highest income); number of family members and number of children in 2002. Standard errors in parentheses are robust to heteroskedasticity and are clustered at the municipality level.*** p<0.01, ** p<0.05, * p<0.1.

Table 2: Effect of reform on renters' expenditure

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ΔC_{2000}	ΔC_{2001}	ΔC_{2002}	ΔC_{2004}	ΔC_{2005}	ΔC_{2006}	ΔC_{2007}	ΔC_{2008}	ΔC_{2009}	ΔC_{2010}
Constrained ₂₀₀₂	-3.1*** (0.38)	-0.58 (0.37)	4.9*** (0.38)	-3.1*** (0.62)	-4.3*** (0.44)	-2.1*** (0.38)	-0.3 (0.37)	-0.7* (0.36)	-1.4*** (0.34)	-1.9*** (0.35)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	98,554	98,783	98,799	98,849	98,840	98,798	98,759	98,714	98,647	98,376
R-squared	0.047	0.050	0.075	0.010	0.030	0.073	0.119	0.165	0.199	0.231

Source: Registry data, Danish population, 1996-2010. *Notes:* Results include only a balanced panel for of households that were recorded as renters for tax purposes in the year 2002 and never moved or changed tenure status through the sample period. Coefficients to be interpreted in DKK thousands. The dependent variable is the within-household change in the moving average of imputed consumption for each given year from before and after the reform. Household level controls include annual household income (logs); age of the household head (defined as the person with highest income); number of family members and number of children in 2002. Standard errors in parentheses are robust to heteroskedasticity and are clustered at the municipality level.*** p<0.01, ** p<0.05, * p<0.1.

firm increase in employment at the end of the year, after controlling for firm, time and municipality fixed effects (column 1, Table 3). The effect does not change once we control for lagged municipality-level house prices (column 2). In column 3 we add time-varying firm balance sheet variables, such as assets and investment: while these are positively related to employment creation over time, their influence barely alters the main effect (column 3). When including the entire set of time-varying controls, the effect of a 10 percent shift in household spending still amounts to 3 percent employment growth (column 4).

The effect appears to be stronger for non-tradable firms (+4.3 percent, column 5), than for tradable ones (+1.9 percent, column 6). Tradable firms are less affected by shifts in local demand, since they rely more on foreign markets both for inputs and outputs. We interpret this as evidence that time-varying common factors affecting all firms operating within municipalities are unlikely to be driving the result. This difference is confirmed when running the same estimation with an interaction term (column 7).

Despite the efforts in trying to partial out fundamental shocks through year fixed-effects, our baseline results could, in principle, reflect a shift in credit supply to firms, since the reform allowed also companies to save on their mortgage payments by taking out IO mortgages. In order to rule out this potential confounder, Table 4 introduces firm-specific year fixed-effects, effectively partialling out any firm-specific shock occurring at any given point in time. The remaining variation is purely cross-sectional, comparing establishments belonging to the same firm in any given year, while at the same time controlling for the time-invariant characteristics of the municipalities in which they operate. This specification allows us to control for credit conditions, which are negotiated at the firm level. The downside is that we restrict the sample to firms having multiple establishments at the same point in time, which are a minority in our sample Figure 3.

Despite this very restrictive specification, the baseline results are fundamentally unchanged. A 10 percent increase in consumption at the municipality level increases employment in local establishments by 2.9 percent (column 1, Table 4). The effect is significantly larger within the sample of non-tradable firms (columns 2 and 3 respectively). A specification with interaction terms once again confirms this difference: establishments belonging to firms classified as tradable change their employment levels by 0.6 percentage points less than a comparable set of establishments belonging to firms classified as non-tradables (column 4).

The differential developments between tradable and non-tradable firms help us building credibility to the hypothesis that our results reflect an exogenous shift in local demand, rather than other time varying municipality characteristics. Similarly, they help us staving off concerns that our specifications are biased by reverse causality, since if employment was driving contemporaneous shifts in local demand, there is no reason why tradable and non-

Table 3: Average effects of the reform on employment

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Employment All	Employment All	Employment All	Employment All	Employment Non-tradables	Employment Tradables	Employment All
Consumption	0.303*** (0.050)	0.304*** (0.055)	0.299*** (0.051)	0.309*** (0.057)	0.432*** (0.057)	0.193*** (0.063)	0.318*** (0.054)
Tradable							0.346** (0.148)
Cons. x Trad.							-0.028*** (0.010)
House Price		0.001 (0.015)		-0.009 (0.016)	-0.007 (0.019)	0.004 (0.027)	-0.006 (0.016)
Assets			0.063*** (0.003)	0.064*** (0.003)	0.056*** (0.003)	0.077*** (0.006)	0.064*** (0.003)
Investment			0.012*** (0.001)	0.012*** (0.001)	0.010*** (0.001)	0.013*** (0.002)	0.012*** (0.001)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	565,030	525,348	369,065	367,368	243,845	117,239	367,368
R-squared	0.768	0.779	0.750	0.751	0.780	0.679	0.751

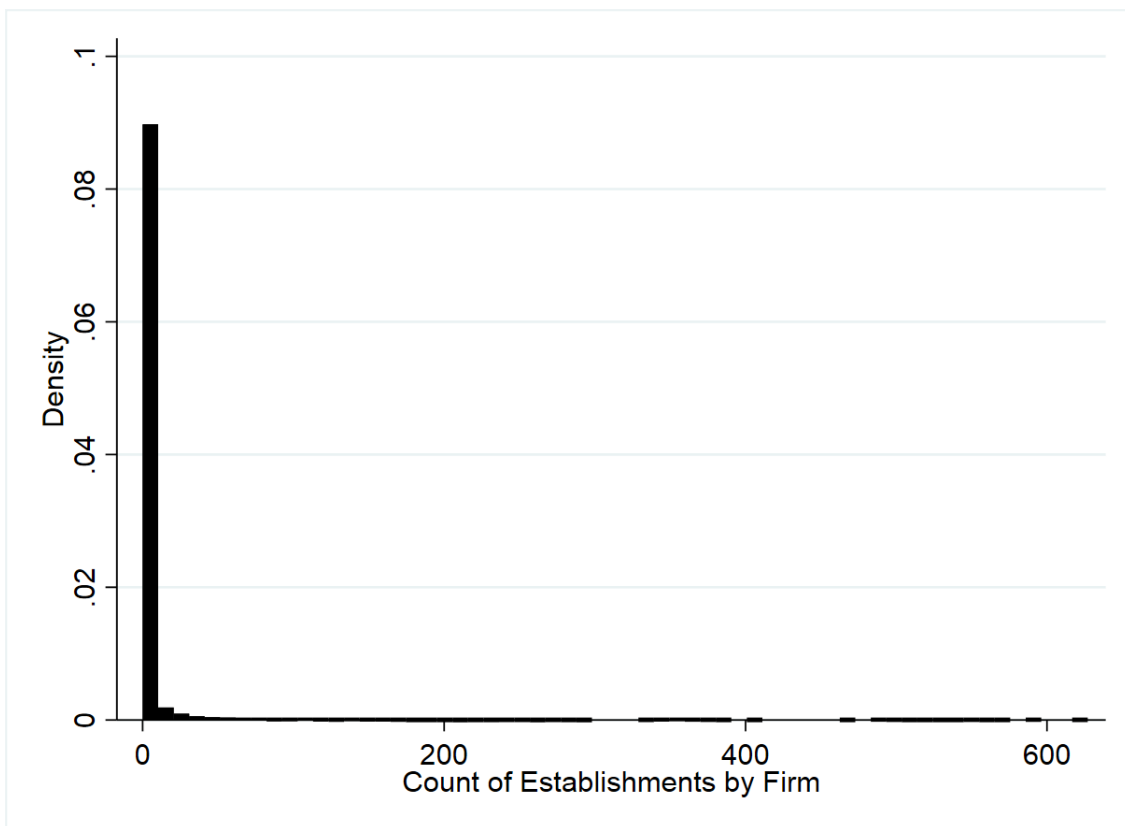
Source: Data from Statistics Denmark, registries FIRE, FIDA, FIRM, 2004-2010. *Notes:* The dependent variable is the number of employees measured at the level of establishments (workplaces) in any given year, defined as head-count and in a logarithmic scale. Consumption includes the values for municipality and time varying consumption shifts (previously defined as C_{mt}) in a logarithmic scale. Tradable is a dummy that takes value one if a firm is defined as tradable. Tradable firms are defined as those with import-export larger than DKK 70,000 per employee in any given year. House prices are measured at the municipality level and are an average per square meter based on home values estimated through tax records, in a logarithmic scale. Total assets, Investments are defined at the firm-level (CVR) and are end-year values based on the tax records, in a logarithmic scale. Standard errors are in parentheses and clustered at the municipality-level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Employment effects: firm by year fixed-effects

VARIABLES Sample	(1) Employment All	(2) Employment Non-Tradables	(3) Employment Tradables	(4) Employment All
Consumption	0.285*** (0.054)	0.372*** (0.109)	0.207*** (0.055)	0.312*** (0.048)
Consumption x Tradable				-0.056** (0.022)
House price	0.022 (0.036)	0.073 (0.088)	-0.015 (0.051)	0.020 (0.037)
Firm x Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Observations	132,152	66,165	65,987	132,152
R-squared	0.528	0.536	0.501	0.528

Source: Data from Statistics Denmark, registries FIRE, FIDA, FIRM, 2004-2010. *Notes:* The dependent variable is the number of employees measured at the level of establishments (workplaces) in any given year, defined as head-count and in a logarithmic scale. Consumption includes the values for municipality and time varying consumption shifts (previously defined as C_{mt}) in a logarithmic scale. Tradable is a dummy that takes value one if a firm is defined as tradable. Tradable firms are defined as those with import-export larger than DKK 70,000 per employee in any given year. House prices are measured at the municipality level and are an average per square meter based on home values estimated through tax records, in a logarithmic scale. Standard errors in parentheses are clustered at the municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 3: Distribution of number of establishments by firm



tradable firms should correlate differently with local demand shocks.

Our definition of tradability based on firm-level balance sheet characteristics follows the conventions used in the relevant literature (Mian and Sufi, 2014), but remains rather arbitrary. For this reason, we run some alternative specifications using a definition of tradability based on broad sectors of economic activity (NACE 1 digit). Tables 5 and 6 display the differential effects of the local demand channel on firms operating in different sectors.³²

While overall retail trade is unaffected by local demand shocks (column 1, Table 5), other sectors traditionally classified as non-tradable are. These include hospitality (column 2), with restaurants, bars and hotels experiencing an increase in employment worth 1.2 percent for each 10 percent increase in local consumption, as well as transportation (column 3) and services (column 4), with 0.8 and 7.4 percent, respectively. The sector more commonly considered as tradable, manufacturing, is unaffected (column 5). The results in Table 5 also help rule out the concern that the overall employment dynamic was driven exclusively by

³²These tables include both tradable and non-tradable firms, as defined according to their balance sheet characteristics.

the construction sector. While this sector experiences a 0.9 percent increase in employment for each 10 percent demand shock in any given year (column 6), this result is comparatively small with respect to other sectors.³³

The additional liquidity granted to constrained homeowners after the reform might not have been spent uniformly on all categories of goods. Table 6 splits retail trade across its different NACE 2-digit codes, to understand whether the overall null effect masks heterogeneity in the categories of goods sold by these companies.

Column 1 displays that cars and automotive retail companies respond significantly to a change in local demand, in contrast with the average for their sector. On the other hand, employment in the food and beverage retail stores responds negatively to the same demand shock, decreasing employment by 0.6 percent (column 2). Stores selling technology and household goods display no significant relationship with the measured change in consumption (columns 3 and 4) while clothes' shops (column 5) and bookstores, cinemas, theatres and concert venues (column 6) do. A common factor across affected industries is that their products have a high elasticity of demand to income.

Overall, these results suggest that the liquidity granted to consumers through the mortgage has helped inflating some sectors, but not others. This is consistent with recent evidence based on the U.S. case, which stresses how the household demand channel inflates the size of the non-tradable sector, sometimes at the expenses of the tradable one (Mian and Sufi, 2014; Mian et al., 2017b; Di Maggio and Kermani, 2017).³⁴

³³Furthermore, the baseline results are strengthened by excluding the real estate and construction sector entirely, as in Appendix Table B1. This holds also when measuring employment not in log-levels, but as a year-on-year percentage change in the count of employees (Appendix Table B2).

³⁴While we do not look at labour reallocation or productivity, our results are in line with previous work finding that in credit booms labour is reallocated to relatively low-productivity sectors (Borio et al., 2016).

Table 5: Employment effects across sectors

VARIABLES Sector	(1)	(2)	(3)	(4)	(5)	(6)
	Employment Retail	Employment Hospitality	Employment Transportation	Employment Services	Employment Manufacturing	Employment Construction
Consumption	0.003 (0.008)	0.123*** (0.025)	0.087*** (0.018)	0.739*** (0.145)	-0.019 (0.118)	0.089*** (0.010)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	92,293	21,613	22,114	54,093	28,856	79,680
R-squared	0.719	0.796	0.807	0.700	0.746	0.814

Source: Data from Statistics Denmark, registries FIRE, FIDA, FIRM, 2004-2010. *Notes:* The dependent variable is the number of employees measured at the level of establishments (workplaces) in any given year, defined as head-count, in a logarithmic scale and per indicated sector. Consumption includes the values for municipality and time varying consumption shifts (previously defined as C_{mt}) in a logarithmic scale. Tradable is a dummy that takes value one if a firm is defined as tradable. Tradable firms are defined as those with import-export larger than DKK 70,000 per employee in any given year. House prices are measured at the municipality level and are an average per square meter based on home values estimated through tax records, in a logarithmic scale. Standard errors in parentheses are clustered at the municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Employment effects: retail sector

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Sector	Employment Automotive	Employment Food	Employment Technology	Employment Household	Employment Clothes	Employment Recreation
Consumption (log)	0.050*** (0.014)	-0.063*** (0.013)	-0.028 (0.038)	0.023 (0.019)	0.140** (0.065)	0.305** (0.117)
Constant	0.087 (0.720)	2.527*** (0.644)	1.290 (1.421)	2.046** (0.861)	-1.481** (0.668)	-3.864*** (1.334)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,623	22,384	3,007	9,473	16,729	6,036
R-squared	0.811	0.668	0.661	0.767	0.591	0.697

Source: Data from Statistics Denmark, registries FIRE, FIDA, FIRM, 2004-2010. *Notes:* The dependent variable is the number of employees measured at the level of establishments (workplaces) in any given year, defined as head-count, in a logarithmic scale and per indicated sector. Consumption includes the values for municipality and time varying consumption shifts (previously defined as C_{mt}) in a logarithmic scale. Tradable is a dummy that takes value one if a firm is defined as tradable. Tradable firms are defined as those with import-export larger than DKK 70,000 per employee in any given year. House prices are measured at the municipality level and are an average per square meter based on home values estimated through tax records, in a logarithmic scale. Standard errors in parentheses are clustered at the municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.3 Aggregate implications of baseline results

To retrieve the macroeconomic implications of our micro-level elasticities, we implement a simple back-of-the-envelope approach. Beraja et al. (2016) show that for the U.S., these calculations perform well in determining the impact of demand shocks on employment losses.³⁵ Since our analysis relies on regional variation, we follow closely (Hagedorn et al., 2015) and estimate a regionally-weighted measure of the percentage change in aggregate employment that is caused by our measure of credit-induced shift in consumption:

$$\pi_{DK} = \beta_1 * \sum_{m=1}^N \left[\frac{E_{m,2002}}{E_{DK,2002}} * \sum_{t=2004}^{2010} \frac{C_{mt}}{Consumption_{m,2002}} \right] = 0.9\% \quad (4)$$

Where t defines each post-reform year (2004–2010). β_1 is the coefficient in Equation 3, or the effect of IO expenditure on employment, which following the results presented in Table 3 and Table 4, column 1, we set at 0.3. $C_{m,t}$ measures the regional consumption shift that is due to the IO reform in every year, as defined in Equation 2; this term is expressed as a percentage change with respect to aggregate consumption in the same municipality before the reform. E_{m2002} measures employment in municipality m in 2002, and E_{DK2002} is aggregate pre-reform employment in Denmark. This methodology allows us to weight each municipality-level change in consumption for the relative relevance of the same municipality in the Danish labour market.

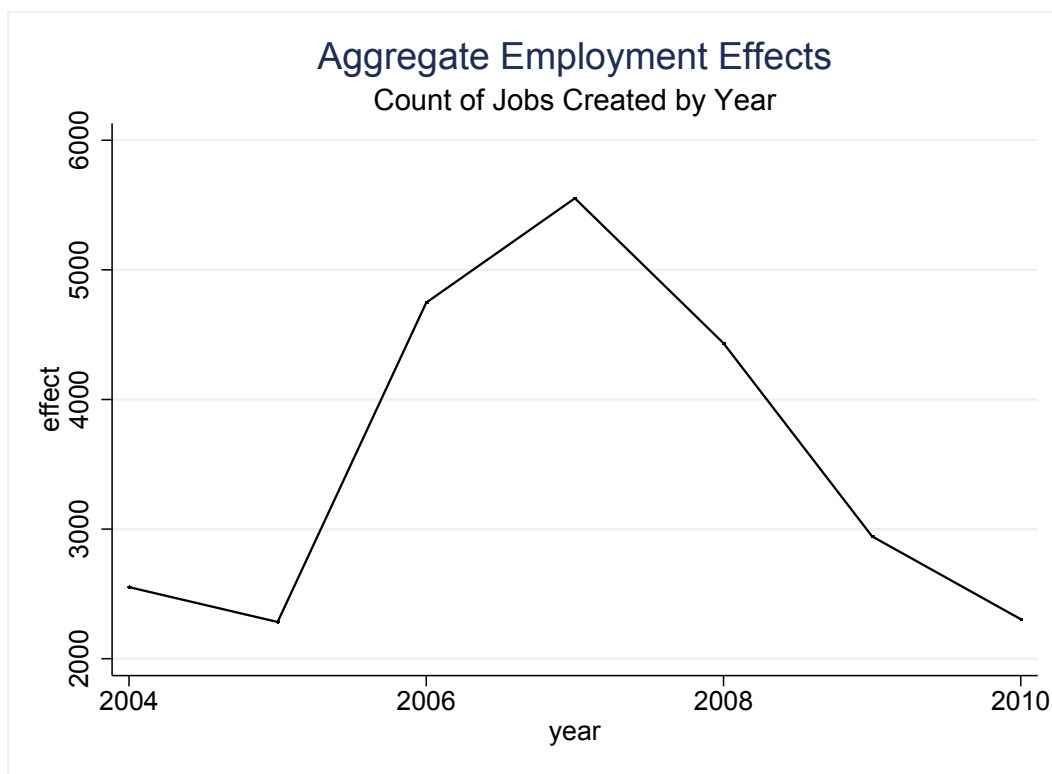
This estimation suggests that between 2004 and 2010 the IO reform shifted aggregate employment by close to one percentage point of pre-reform employment, or:

$$\delta_{DK}^E = \frac{\pi_{DK} * E_{DK,2002}}{1 + \pi_{DK}} = 23,771 \quad (5)$$

Or slightly less than 24,000 jobs were created due to the refinancing channel of IO loans, between 2004 and 2010. We interpret these estimates as very conservative, as they reflect only one channel through which the reform affected consumption: mortgage refinancing from old loans to new, IO, ones. This estimation excludes the consumption effects taking place through homebuyers, or the indirect effect of the reform on house prices. Estimating the same equation for each year suggests that the bulk of job creation occurred in 2006–2008 (see Figure 4). In each of these years the job creation that can be attributed to our channel accounts for about 5-12 percent of net employment change.

³⁵Beraja et al. (2016) find that the responses of aggregate employment to regional variation in household demand perform equally well when calculated with back-of-the-envelope or when simulated in a heterogeneous agents model. At the same time, using regional variation outperforms those estimations that rely on aggregate data

Figure 4: Back of the envelope: job creation by year



Source: Statistics Denmark. Authors' estimation of the yearly effect of of IO-induced consumption on employment creation.

5 Worker-level heterogeneity

This section presents results based on employer-employee matched datasets. We show how credit-induced household demand shocks affect workforce composition and how credit cycles amplify regional business cycles, for certain categories of workers.

5.1 Job characteristics

A credit-induced shift in demand creates jobs, particularly in non-tradable firms and sectors. The inflation of certain sectors might automatically imply a rise in demand for certain typologies of jobs. However, local demand shocks might affect the composition of the workforce even *within* given sectors and firms. For example, if firms anticipate the change in demand to be of a temporary nature, they might hire workers that require less investment in terms of training and recruiting (i.e. low skilled positions) as opposed to making long-term investment in hiring managers capable of restructuring their business, reflecting a permanent shift

in expected production necessities. Similarly, certain job categories might be more flexible than others. Waiters or salesmen are relatively easier to hire than specialised engineers, as their skills are more general and transferable across firms. These types of matches also have lower opportunity costs in firing and can be used to serve a firm in the short term.

In order to examine whether the composition of the workforce changes within firms, we compute the average characteristics of the workforce that joins the workplace for each establishment-year cell. These are age, educational attainment, experience and the count of positions filled within each skill level.³⁶ To test whether new workers have similar ex-post outcomes with respect to their colleagues, we exploit the panel structure of our dataset and compute two variables indicating ex-post developments: the average length of new working relationships in years (“tenure”) and the average percentage unemployment that each new hire faces in each year subsequent to the year in which s/he is hired. We remain consistent with the rest of our estimations by keeping the establishment as our unit of observation. The model is therefore identical to that presented in Equation 3, with the addition of a control for establishment-level values of the dependent variable before 2002. These variables have the purpose of controlling for time-invariant establishment-level characteristics that may affect the composition of that particular establishment’s workforce independently from the 2003 reform.³⁷

Table 7 shows how workforce characteristics vary within the firm, as it is exposed to different levels of local demand. While the ex-ante worker characteristics matter (i.e. establishments with a more experienced workforce in 2002 in general hire more experienced new workers), the worker composition is significantly altered by the size of the demand shift. Establishments subject to a larger shift in consumption on average hire relatively younger and less experienced workers, as well as a marginally lower number of university-educated individuals (columns 1, 2 and 3). Furthermore, they do not hire more high-skilled workers relative to their pre-trends (column 4), but they do employ relatively more intermediate and, especially, low-skilled people (columns 5 and 6). In terms of magnitudes, local establishments operating in areas which experience a 10 percent shift in household consumption decrease both age and experience of the workforce by around a month and deviate from their 2002 pre-trends in job characteristics by creating 0.02 and 0.07 percent more positions requiring intermediate and low skills, respectively.

These outcomes indicate that, irrespectively of the sectoral attachment, credit-induced

³⁶In doing so we follow the classification provided by Statistics Denmark, which divides job categories as requiring low, intermediate or high skills (register AKM, based on the variable SOCIO13). These correspond roughly to manual workers broadly defined; office workers, clerks, salesmen and technicians; and managers and higher professional categories.

³⁷This restricts the estimation to establishments that were already active in 2002.

upturns create particular types of jobs. Some of these jobs, such as low-skilled positions, are found to be generally more unstable over the business cycle (Jovanovic, 1979; Hoynes, 2000).³⁸

The jobs created due to credit-induced demand shocks have a shorter life, as the length of the employer-employee relationship for new matches depends negatively on consumption, even after controlling for both average tenure of existing workers in the same establishments and the timing of the match creation (column 7).³⁹

Since we are unable to distinguish between voluntary separations and layoffs, lower tenure could, in principle, stem from voluntary decisions of workers to climb the job ladder. In this case we should observe that new workers spend on average less time in unemployment in the years following the match. On the contrary, average future unemployment rates of new hires are increasing in the consumption shock experienced by the firm at the time of their hiring (column 8). Even after taking into account contemporaneous unemployment of workers hired in a different phase of the business cycle, workers' subsequent average yearly share in unemployment is higher by 0.5 percentage points for each 10 percent shift in credit-induced consumption.

This evidence provides support to the well-established notion that less educated and low-skilled hires experience higher turnover in general, and are particularly vulnerable to recessions (Jovanovic, 1979; Gautier et al., 2002; Hoynes, 2000; Hoynes et al., 2012). However, they also show that credit shocks have additional explanatory power for labour market dynamics over and above symmetric (short-run) effects over the cycle, concurring with Giroud and Mueller (2018). Our results unify these two frameworks. We show that the credit-induced demand shock creates a particular type of match, and that these workers fare differently not only due to the inherent nature of their positions, but also depending on the degree of demand shock their firms are subject to, and with respect to co-workers hired during "normal" times.

³⁸The employment regressions by sector and skill level indicate that the creation of intermediate and low-skilled jobs outlined in Table 7 is largely a result of an inflation of certain sectors that always require a higher degree of these types of employees (e.g. construction). However, the skill composition changes also within some sectors - see Table B3 and Table B4 in Appendix B.

³⁹The negative relationship between separations and tenure is a well-established result (Jovanovic, 1979). However, our results also indicate that the slope of this relationship changes depending on the relative degree of the demand shock the hiring firm is subject to at any given point in time. This suggests that local credit cycles have additional explanatory power towards job security, over and above the common inverse relationship of a match's tenure and life span.

Table 7: Characteristics of new matches and consecutive tenure and unemployment

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$Age_{i,t}$	$Exp_{i,t}$	$Uni_{i,t}$	$High_{i,t}$	$Interm_{i,t}$	$Low_{i,t}$	$Tenure_{i,t}$	$Unemp_{i,t}$
	Years							
	Head count							
	Years (ex-post)							
	% Years (ex-post)							
Consumption	-0.821*** (0.086)	-1.115*** (0.064)	-0.005*** (0.001)	0.071 (0.058)	0.216** (0.088)	0.693*** (0.228)	-0.026*** (0.003)	0.0466*** (0.0116)
$Age_{i,2002}$	0.298*** (0.014)							
$Exp_{i,2002}$		0.374*** (0.018)						
$Uni_{i,2002}$			0.291*** (0.032)					
$High_{i,2002}$				0.257*** (0.024)				
$Interm_{i,2002}$					0.280*** (0.046)			
$Low_{i,2002}$						0.312*** (0.062)		
$Tenure_{i,2002}$							0.131*** (0.008)	
$Unemp_{i,2002}$								0.820*** (0.014)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	205,772	205,710	202,887	146,582	146,582	146,582	276,441	154,444
R-squared	0.552	0.554	0.512	0.559	0.368	0.407	0.916	0.576

Source: Data from Statistics Denmark, registries AKM, FIDA, FIRM, IDAP, IDAN, UDDA, 2002-2011. *Notes:* The dependent variables in cols 1-3 measure yearly averages for each establishment of the following characteristics of new hires: age (years); experience (years); university degrees (head count). Cols 3-6 measure the number of new matches classified as requiring high, intermediate or low skills, respectively. Cols 7 & 8 measure yearly averages for each establishment of the following characteristics (forward-looking): length of tenure (years) in the current position that each year's new matches will have (col 7) and average yearly share in unemployment of each year's new matches (in percent). Consumption includes the values for municipality and time varying consumption shifts (previously defined as C_{mt}) in a logarithmic scale. Controls include the establishment-level averages of the same characteristics measured for the same establishment in the year 2002, respectively. House prices are measured at the municipality level and are an average per square meter based on home values estimated through tax records, in a logarithmic scale. Firm level controls include total assets and investments. Standard errors in parentheses are clustered at the municipality-level. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Unemployment volatility by skill level

Our findings suggest that the burden of credit-induced boom-bust cycles may be unequally distributed across the population, with low-skilled workers bearing the brunt of adjustment. Previous work indeed finds that young, less experienced and low-educated or low-skilled workers are especially affected when negative credit supply shocks hit firms (Hochfellner et al., 2015; Barbosa et al., 2017; Berton et al., 2018). Our setting allows us to assess whether this is also true when credit supply shocks affect households. In particular, we can examine whether our credit-induced demand shock affects ex-post unemployment of new matches *within* a given skill typology and hiring vintage.

To do so, we split the sample into skill categories and two hiring vintages: new matches created between 2004 and the end of 2007, and those established between 2008 and 2010. Controlling for unemployment of pre-existing matches at the same point in time allows us to draw conclusions on whether new matches fare differently from their peers hired at different phases of the business cycle; or, in other words, whether a credit-induced demand channel amplifies local business cycles for these types of jobs.

Between 2004 and 2010, workers in low skilled positions do not seem to experience significantly different ex-post unemployment across establishments, depending on the degree of the demand shock (column 1, Table 8). However, when splitting the sample between a boom and a bust phase, we find the effect of local demand to be asymmetric. Column 2 shows that new matches filling low-skilled positions during the boom years (2004- 2007) spend more time in unemployment, ex-post. This is not just an effect of the business cycle (i.e. everybody experiences higher unemployment rates), since we control for the contemporaneous unemployment rates of low-skilled workers employed in the same establishment, but who were hired before 2003. Low-skilled workers that were hired in response to a relatively stronger demand shock are more exposed to the bust phase of the cycle. The effect for low-skilled jobs is larger than the overall effect presented in Table 7; for every 10 percent increase in consumption, subsequent unemployment of low-skilled new hires made in the boom period is higher by 0.7 percentage points. Workers hired between 2007 and 2010, instead, have substantially lower ex-post unemployment (column 3). This could imply that in a low-growth phase, new matches require to be relatively more productive in order to exist in the first place (Berger, 2018), facing a lower layoff probability.

Table 8: Unemployment for low-skilled new workers by year of hiring

VARIABLES	(1) Unempl. Low skilled % per year All Years	(2) Unempl. Low skilled % per year Hired up to 2007	(3) Unempl. Low skilled % per year Hired after 2007
Consumption	-0.0243 (0.0406)	0.0674** (0.0327)	-0.3027*** (0.1107)
$Unemp_{Lowskilled_{i,2002}}$	0.986*** (0.019)	0.985*** (0.012)	0.918*** (0.034)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	120,643	72,033	32,830
R-squared	0.460	0.515	0.515

Source: Data from Statistics Denmark, registries AKM, FIDA, FIRM, IDAP, IDAN, UDDA, 2002-2011.

Notes: The dependent variables are measured as yearly averages for each establishment of the following characteristic of new hires: average yearly share in unemployment (in percent) of each year's new matches which are classified as low skill and created throughout (col 1), created up until 2007 (col 2) and created after 2007 (col 3), respectively. Controls include the establishment-level averages of the same characteristic measured for the same establishment in the year 2002. House prices are measured at the municipality level and are an average per square meter based on home values estimated through tax records. Firm-level controls include Total assets, Investments based on the tax records. Standard errors in parentheses are clustered at the municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Unemployment durations of workers hired for intermediate skilled jobs, however, never deviate from the trends of existing matches (Table 9).

Finally, when considering the entire time frame, ex-post unemployment among high-skilled new hires is actually decreasing in the demand shock (column 1, Table 10). Combined with the fact that ex-post unemployment of all new workers (irrespective of the skill level) seems to be higher in boom areas (Table 7), this suggests that high-skilled workers are less affected by local business cycles as a whole. However, when focusing on new matches created up until 2007 (column 2), high-skilled workers' unemployment increases in the local demand shock. A 10 percent increase in household spending is associated with a higher ex-post average yearly unemployment share by 1.5 percentage points for these matches. This outcome seems consistent with the notion that jobs at the extremes of the skill distribution are more affected by local business cycles, particularly by separations in downturns (Barbosa et al., 2017).

These findings provide an alternative lens to understand why low-skilled workers suffered

Table 9: Unemployment for intermediate-skilled new workers by year of hiring

VARIABLES	(1) Unempl. Int. skilled % per year All Years	(2) Unempl. Int. skilled % per year Hired up to 2007	(3) Unempl. Int. skilled % per year Hired after 2007
Consumption	0.0142 (0.0365)	-0.008 (0.0461)	0.0108 (0.1047)
$Unemp_{Int.skilled_i,2002}$	1.014*** (0.023)	1.003*** (0.016)	1.007*** (0.039)
Observations	44,061	25,500	12,637
R-squared	0.521	0.521	0.577
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Source: Data from Statistics Denmark, registries AKM, FIDA, FIRM, IDAP, IDAN, UDDA, 2002-2011.
Notes: The dependent variables are measured as yearly averages for each establishment of the following characteristic of new hires: average yearly share in unemployment (in percent) of each year's new matches which are classified as intermediate skill and created throughout (col 1), created up until 2007 (col 2) and created after 2007 (col 3), respectively. Controls include the establishment-level averages of the same characteristic measured for the same establishment in the year 2002. House prices are measured at the municipality level and are an average per square meter based on home values estimated through tax records. Firm-level controls include Total assets, Investments based on the tax records. Standard errors in parentheses are clustered at the municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Unemployment for high-skilled new workers by year of hiring

VARIABLES	(1) Unempl. High skilled % per year All years	(2) Unempl. High. skilled % per year Hired up to 2007	(3) Unempl. High skilled % per year Hired after 2007
Consumption(log)	-0.1047*** (0.290)	0.1510** (0.617)	-0.0342 (0.436)
$Unemp_{Highskilled_i,2002}$	1.107*** (0.040)	1.051*** (0.036)	1.120*** (0.067)
Observations	19,323	11,141	5,287
R-squared	0.581	0.579	0.633
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Source: Data from Statistics Denmark, registries AKM, FIDA, FIRM, IDAP, IDAN, UDDA, 2002-2011.
Notes: The dependent variables are measured as yearly averages for each establishment of the following characteristic of new hires: average yearly share in unemployment (in percent) of each year's new matches which are classified as high skill and created throughout (col 1), created up until 2007 (col 2) and created after 2007 (col 3), respectively. Controls include the establishment-level averages of the same characteristic measured for the same establishment in the year 2002. House prices are measured at the municipality level and are an average per square meter based on home values estimated through tax records. Firm-level controls include Total assets, Investments based on the tax records. Standard errors in parentheses are clustered at the municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

more in the Great Recession (Hoynes et al., 2012; Hochfellner et al., 2015; Barbosa et al., 2017; Berton et al., 2018). Not only credit-driven booms generate an inflation of jobs which are more volatile per se; they also *amplify* the general volatility in employment of workers filling a particular type of position. A possible channel through which these results may arise is matching quality: as unemployment was at historical lows between 2004 and 2007, the productivity of a given employer-employee match needn't be too high for such a match to exist, while the opposite is true in a low-growth phase (Berger, 2018).

6 Conclusions

We study how a reduction in liquidity constraints faced by homeowners affects job creation and destruction over the business cycle. The channel we propose works through consumer demand: consumers use mortgage credit to fuel their non-housing expenditure, which in turn triggers job creation in local establishments. However, this mortgage-fuelled expenditure boom also changes the composition of local labour markets. It increases the relevance of non-tradable sectors and creates many low-skilled positions, filled by younger, relatively less experienced and less educated workers. These workers experience a higher volatility of their employment relationships ex-post and higher unemployment, compared to similar workers hired at a different phase of the cycle.

These results suggest that an expansion in mortgage credit availability generates business cycles that differ from normal times. We also provide a lens to explain why low-skilled workers suffered more in the Great Recession, as this episode was preceded by a household credit boom. Our findings hint at potentially relevant distributional consequences of credit-driven fluctuations in real economic activity, since these fluctuations affect more vulnerable workers (younger, low-skilled) the most. In a bust phase, low-income workers may turn out to be contemporaneously more vulnerable to the loss of employment and to a contraction of credit availability.

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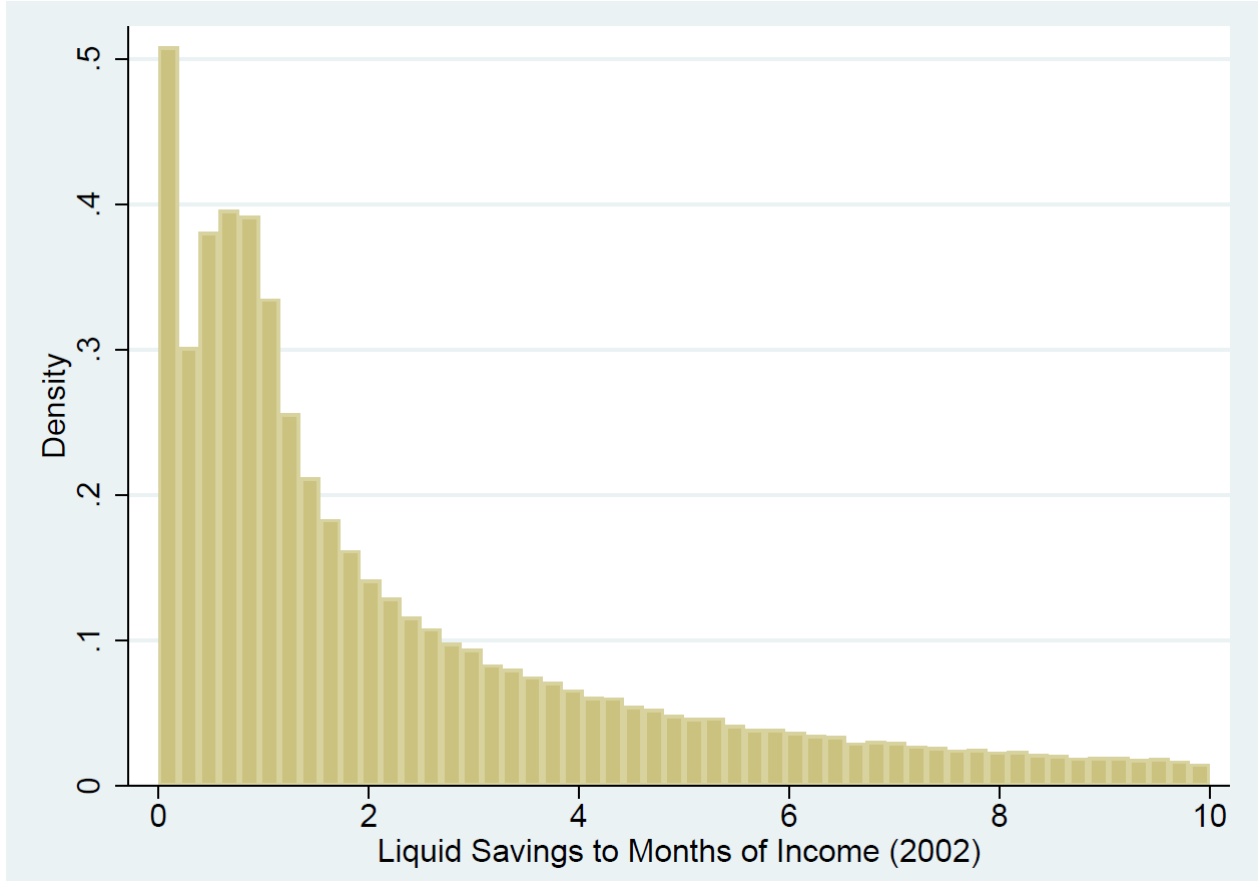
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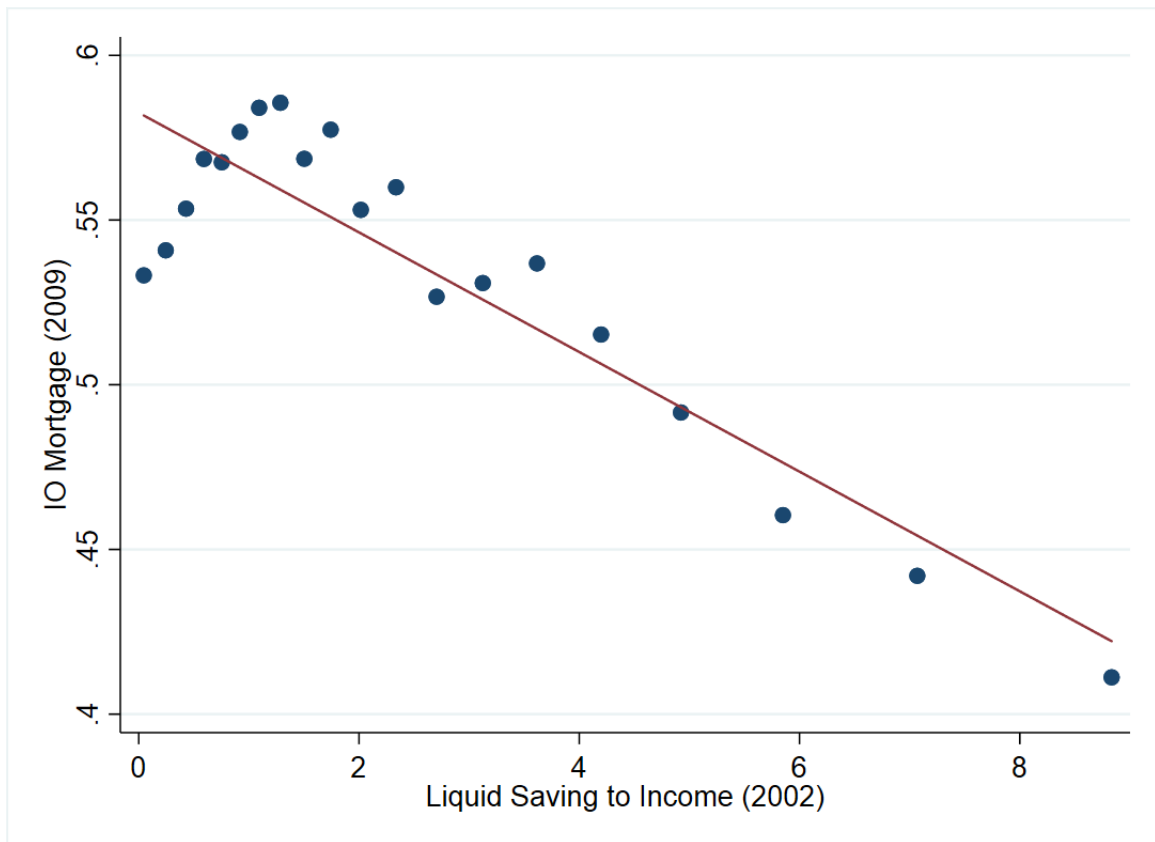
A Appendix on households

Figure A1: Liquid savings-to-income ratio in 2002, homeowners



Source: Statistics Denmark. *Notes:* Distribution of the liquid savings/months of income ratio in 2002. Liquid savings are defined as the sum of cash held in bank accounts, stocks and bonds.

Figure A2: Relationship between liquid savings-to-income ratio in 2002 and IO mortgage holdings in 2009



Source: Statistics Denmark. *Notes:* The regression displays the relationship between ownership of an IO mortgage in 2009 and liquid savings/months of income for the same household in 2002.

Table A1: Homeowners' mortgages

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ΔM_{2000}	ΔM_{2001}	ΔM_{2002}	ΔM_{2004}	ΔM_{2005}	ΔM_{2006}	ΔM_{2007}	ΔM_{2008}	ΔM_{2009}	ΔM_{2010}
Constrained ₂₀₀₂	50.6*** (1.44)	46.8*** (1.47)	32.2*** (1.48)	54.6*** (1.57)	62.5*** (1.70)	80.5*** (1.88)	98.4*** (2.07)	114.9*** (2.25)	131.8*** (2.44)	149.1*** (2.60)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	99,465	99,471	99,472	99,491	99,487	99,455	99,421	99,387	99,347	99,305
R-squared	0.114	0.093	0.089	0.036	0.072	0.106	0.130	0.147	0.163	0.179

Source: Registry data, Danish population, 1996-2010. *Notes:* Results include only a balanced panel of households that were recorded as homeowners for tax purposes in the year 2002 and never moved or changed tenure status through the sample period. Coefficients to be interpreted in DKK such as low-skilled positions. The dependent variable is the within-household change in the moving average of mortgages for each given year from before and after the reform. Household level controls include the house value in 2002 (logs); annual household income (logs); age of the household head (defined as the person with the highest income); number of family members and number of children in 2002. Standard errors in parentheses are robust to heteroskedasticity. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Homeowners' income

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ΔI_{2000}	ΔI_{2001}	ΔI_{2002}	ΔI_{2004}	ΔI_{2005}	ΔI_{2006}	ΔI_{2007}	ΔI_{2008}	ΔI_{2009}	ΔI_{2010}
Constrained ₂₀₀₂	-4.3*** (0.34)	-5.6*** (0.34)	-4.9*** (0.32)	1.9*** (0.41)	2.0*** (0.40)	0.76* (0.41)	-1.3*** (0.42)	-0.9*** (0.45)	-4.0*** (0.45)	-4.9*** (0.49)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	99,465	99,471	99,472	99,491	99,487	99,455	99,421	99,387	99,347	99,305
R-squared	0.137	0.139	0.169	0.060	0.112	0.182	0.294	0.336	0.407	0.441

Source: Registry data, Danish population, 1996-2010. *Notes:* Results include only a balanced panel of households that were recorded as homeowners for tax purposes in the year 2002 and never moved or changed tenure status through the sample period. Coefficients to be interpreted in DKK thousands. The dependent variable is the within-household change in the moving average of disposable income for each given year from before and after the reform. Household level controls include the house value in 2002 (logs); annual household income (logs); age of the household head (defined as the person with the highest income); number of family members and number of children in 2002. Standard errors in parentheses are robust to heteroskedasticity. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Homeowners' expenditure

VARIABLES	(1) ΔC_{2004}	(2) ΔC_{2005}	(3) ΔC_{2006}	(4) ΔC_{2007}	(5) ΔC_{2008}	(6) ΔC_{2009}	(7) ΔC_{2010}
Constrained ₂₀₀₂	5.1*** (1.18)	3.7*** (0.92)	8.5*** (0.80)	9.3*** (0.72)	7.4*** (0.66)	5.5*** (0.61)	4.4*** (0.57)
age (hh head)	0.23 (0.23)	0.43** (0.17)	0.71*** (0.15)	0.77*** (0.13)	0.79*** (0.12)	0.84*** (0.12)	0.93*** (0.11)
no. hh members	-4.3*** (1.42)	-6.1*** (1.12)	-9.3*** (0.89)	-5.7*** (0.78)	-4.9*** (0.76)	-4.4*** (0.84)	-4.2*** (0.91)
House Prices (sqm)	-2.6 (0.10)	-3.5** (0.78)	-10.9*** (0.68)	-1.1 (0.61)	4.3*** (0.58)	5.7*** (0.53)	3.5** (0.49)
no. children	16.0*** (0.20)	14.3*** (0.19)	14.4*** (0.13)	8.2*** (0.11)	6.9*** (0.11)	6.1*** (0.11)	5.8*** (0.11)
i44	0.52*** (0.05)						
i45		0.58*** (0.04)					
i46			0.58*** (0.02)				
i47				0.61*** (0.02)			
i48					0.64*** (0.02)		
i49						0.67*** (0.03)	
i410							0.65*** (0.03)
Constant	0.28 (0.52)	-0.22*** (0.36)	-0.29*** (0.28)	-22.38 (0.145)	-58.38*** (0.14)	-43.98*** (0.13)	-1.61 (0.14)
Observations	99,491	99,487	99,455	99,421	99,387	99,347	99,283
R-squared	0.044	0.109	0.160	0.197	0.277	0.332	0.385

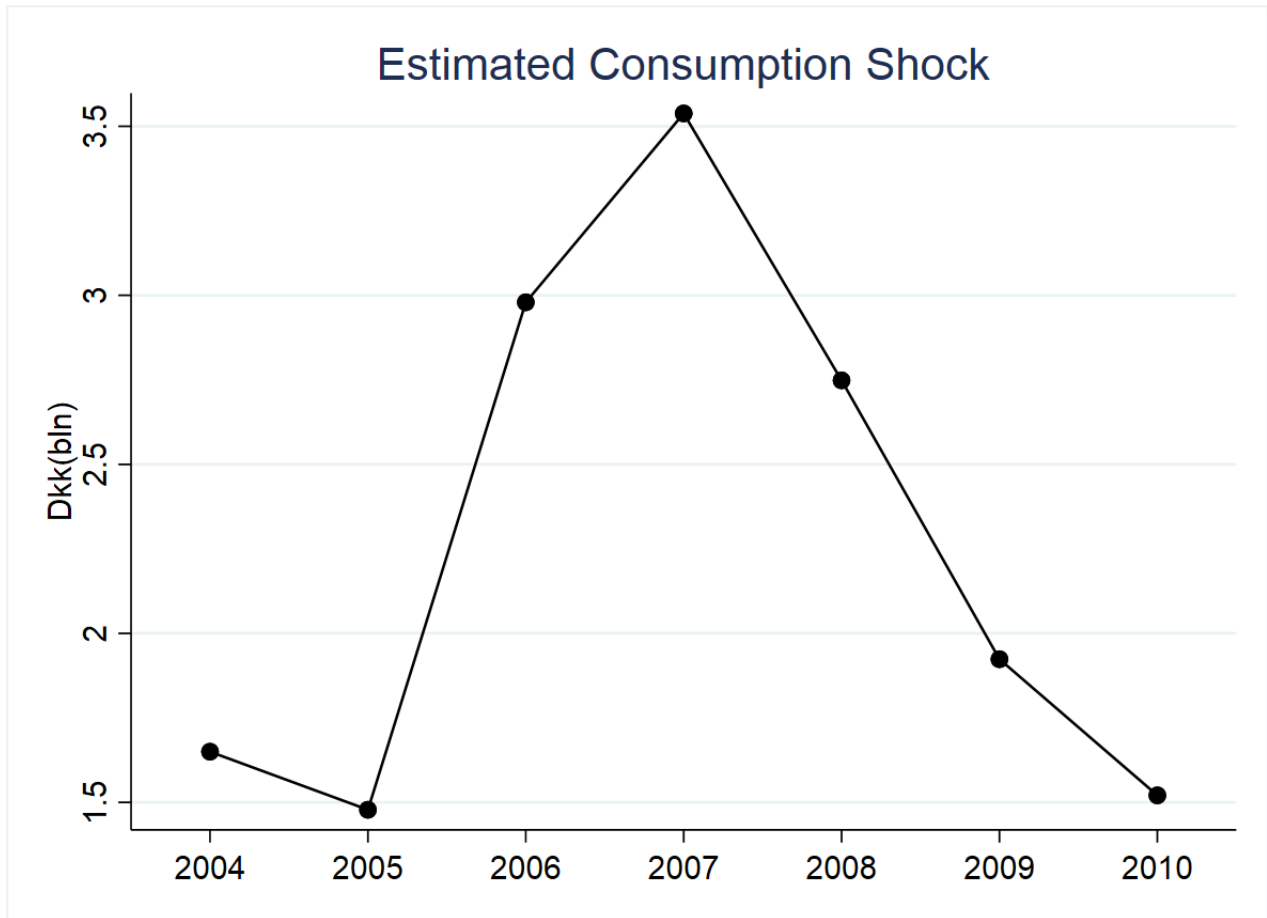
Source: Registry data, Danish population, 1996-2010. *Notes:* Results include only a balanced panel of households that were recorded as homeowners for tax purposes in the year 2002 and never moved or changed tenure status through the sample period. Coefficients to be interpreted in DKK thousands. The dependent variable is the within-household change in the moving average of imputed consumption for each given year from before and after the reform. Household level controls include contemporaneous house value (logs); respective contemporaneous moving averages of household income (logs); contemporaneous age of the household head (defined as the person with the highest income); contemporaneous number of family members and number of children. Standard errors in parentheses are robust to heteroskedasticity and are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Placebo test

VARIABLES	(1) $\Delta C_{h,2000}$ Constrained in 1998	(2) $\Delta C_{h,2001}$ Constrained in 1999	(3) $\Delta C_{h,2002}$ Constrained in 2000
Constrained ₁₉₉₈	-35.9*** (1.9)		
Constrained ₁₉₉₉		-32.6*** (1.1)	
Constrained ₂₀₀₀			-13.0*** (1.0)
Municipality FE	Yes	Yes	Yes
Family controls	Yes	Yes	Yes
R-squared	0.030	0.049	0.050
Observations	99,442	99,468	99,471

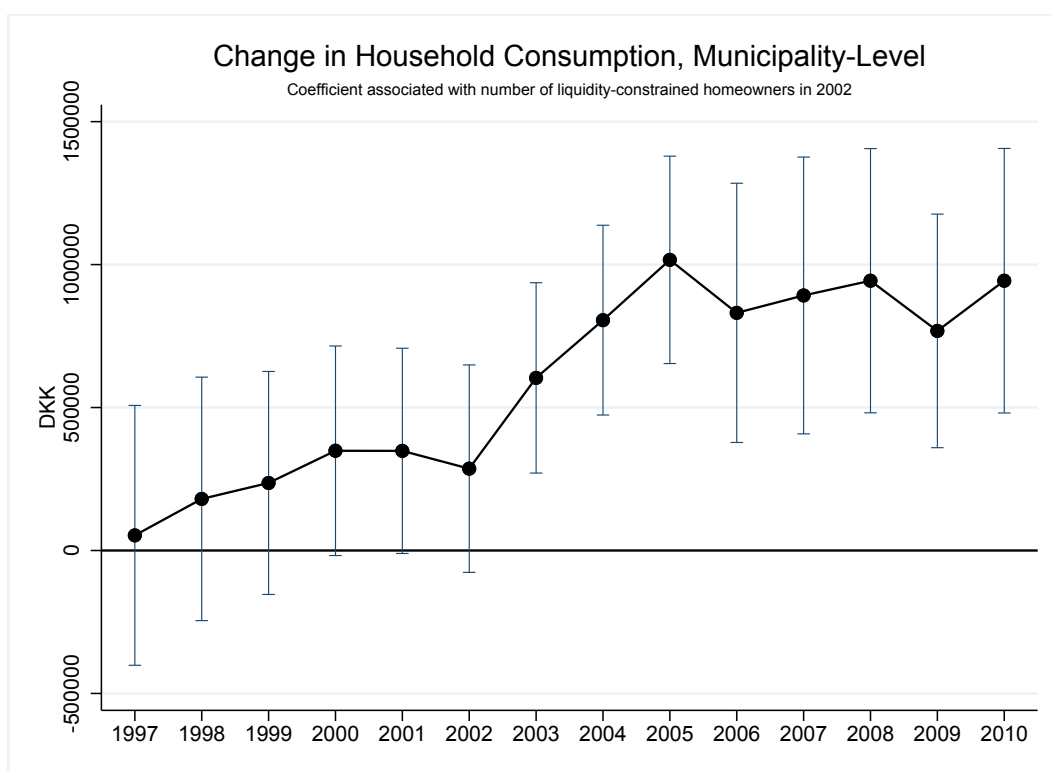
Source: Registry data, Danish population, 1996-2010. *Notes:* Results include only a balanced panel of households that were recorded as homeowners for tax purposes in the year 2002 and never moved or changed tenure status through the sample period. Coefficients to be interpreted in DKK thousands. The dependent variable is the within-household change in the 2-year-moving average of disposable income for each given year from before and after the placebo reform years 1999, 2000 and 2001, respectively. Assignment to treatment or control group is based on the liquid savings-to-income ratio in the respective placebo pre-reform year (i.e. 1998, 1999 and 2000). Household level controls include the house value in 2002 (logs); annual household income (logs); age of the household head (defined as the person with the highest income); number of family members and number of children in 2002. Standard errors in parentheses are robust to heteroskedasticity.*** p<0.01, ** p<0.05, * p<0.1.

Figure A3: Estimated aggregate consumption shift



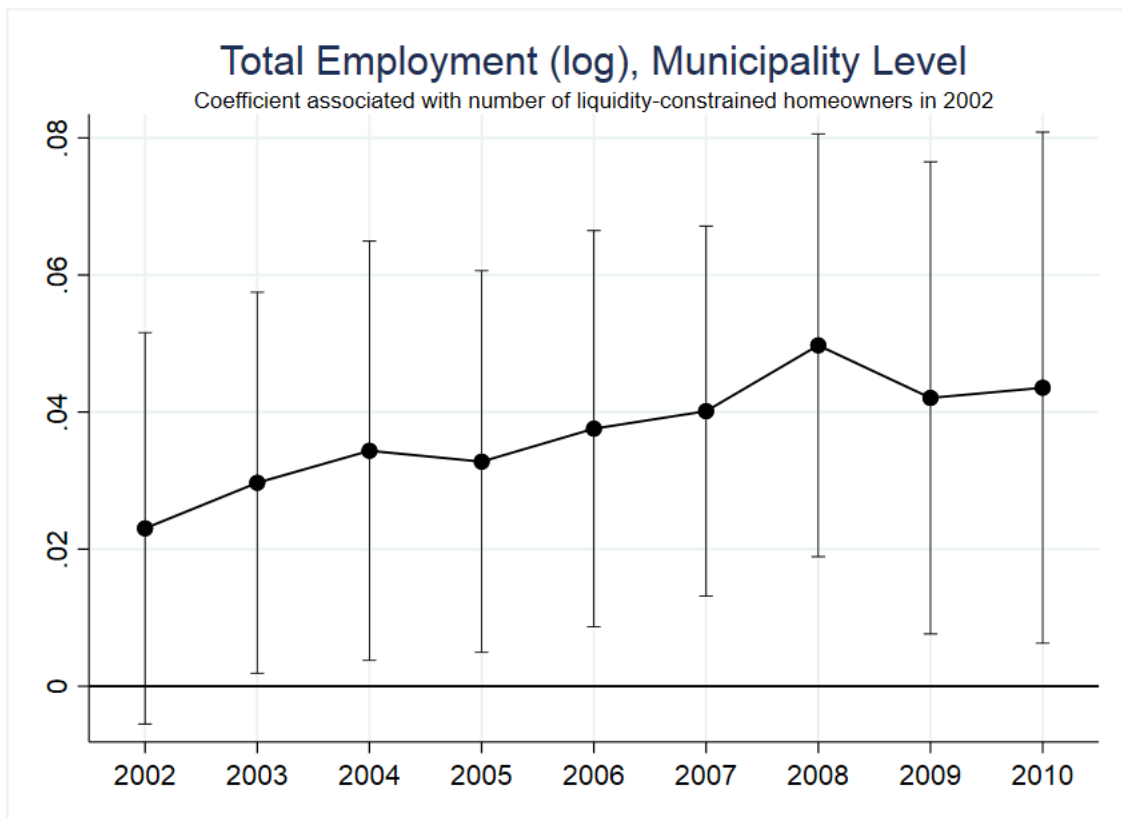
Source: Statistics Denmark. *Notes:* Total expenditure shocks due to the refinancing channel of IO loans for each year. This is computed as the sum of all the estimated consumption shifts for each municipality, obtained in Equation (2).

Figure A4: Regional difference-in differences: Expenditure



Source: Statistics Denmark. *Notes:* The dependent variable is the aggregate household consumption for each year/municipality cell. The graph depicts the coefficient associated with the interaction between year dummies and the number of liquidity-constrained homeowners living in the municipality in 2002. Municipality and year fixed-effects are included.

Figure A5: Regional difference-in differences: Employment



Source: Statistics Denmark. *Notes:* The dependent variable is the aggregate employment in each establishment measured by FIDA for each year/municipality cell. The graph depicts the coefficient associated with the interaction between year dummies and the number of liquidity-constrained homeowners living in the municipality in 2002. Municipality and year fixed-effects are included.

B Appendix on firms

Table B1: Average effects of the reform on employment excluding real estate and construction sector

VARIABLES	(1)	(2)	(3)	(4)
	Employment (log) All	Employment (log) Non-tradables	Employment (log) Tradables	Employment (log) All
Consumption(log)	0.267*** (0.048)	0.323*** (0.099)	0.200*** (0.052)	0.299*** (0.044)
Tradable x Consumption (log)				-0.063*** (0.021)
Firm x Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Observations	118,663	54,878	63,785	118,663
R-squared	0.528	0.537	0.504	0.528

Source: Data from Statistics Denmark, registries FIRE, FIDA, FIRM, 2004-2010. *Notes:* The dependent variable is the number of employees measured at the level of establishments (workplaces) in any given year, defined as head-count and in a logarithmic scale. Consumption includes the values for municipality and time varying consumption shifts (previously defined as C_{mt}) in a logarithmic scale. Tradable is a dummy that takes value one if a firm is defined as tradable. Tradable firms are defined as those with import-export larger than DKK 70,000 per employee in any given year. Real estate and construction sector (NACE2 codes 41,42,43,68,71,81) excluded. Standard errors in parentheses are clustered at the municipality-level, in a logarithmic scale. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Average effects of the reform on employment in changes

VARIABLES	(1)	(2)	(3)	(4)
	Change Employees (%) All	Change Employees (%) Non-tradables	Change Employees (%) Tradables	Change Employees (%) All
Consumption (log)	0.478*** (0.133)	0.757*** (0.242)	0.233** (0.104)	0.491*** (0.136) -0.027** (0.012)
Tradable x Consumption (log)				
Observations	127,109	63,228	63,881	127,109
R-squared	0.184	0.164	0.233	0.184
Firm x Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes

Source: Data from Statistics Denmark, registries FIRE, FIDA, FIRM, 2004-2010. *Notes:* The dependent variable is the number of employees measured at the level of establishments (workplaces) in any given year, defined as year-on-year percentage change in the head-count of employees. Consumption includes the values for municipality and time varying consumption shifts (previously defined as C_{mt}) in a logarithmic scale. Tradable is a dummy that takes value one if a firm is defined as tradable. Tradable firms are defined as those with import-export larger than DKK 70,000 per employee in any given year. Real estate and construction sector (NACE2 codes 41,42,43,68,71,81) excluded. Standard errors in parentheses are clustered at the municipality-level, in a logarithmic scale. *** p<0.01, ** p<0.05, * p<0.1.

Table B3: New matches in low-skilled jobs by sector

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Sector	Low Construction	Low Retail	Low Hospitality	Low Logistic	Low Services	Low Manuf.
Consumption	0.153 (0.321)	0.857*** (0.219)	1.468** (0.698)	0.456 (0.439)	2.613 (1.630)	0.060 (0.665)
$Low_{i,2002}$	0.386*** (0.092)	0.755*** (0.057)	0.414*** (0.107)	0.391*** (0.053)	0.892*** (0.104)	0.163*** (0.019)
Observations	28,743	38,451	6,119	7,721	17,541	15,348
R-squared	0.337	0.698	0.627	0.625	0.488	0.580
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Source: Data from Statistics Denmark, registries AKM, FIDA, FIRM, IDAP, IDAN, UDDA, 2002-2011.

Notes: The dependent variable measures the number of new matches classified as requiring low skills. Controls include the establishment-level averages of the same characteristics measured for the same establishment in the year 2002, house prices are measured at the municipality level, investment and firms' total assets (logs). Standard errors in parentheses are clustered at the municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B4: New matches in intermediate-skilled jobs by sector

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Sector	Medium Construction	Medium Retail	Medium Hospitality	Medium Logistic	Medium Services	Medium Manuf.
Consumption	-0.421 (0.389)	0.161** (0.068)	0.066 (0.220)	0.221 (0.173)	0.924 (0.754)	0.021 (0.187)
$Interm_{.i,2002}$	0.542 (0.345)	0.529*** (0.077)	0.231*** (0.057)	0.113*** (0.037)	0.376*** (0.102)	0.149*** (0.011)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,743	38,451	6,119	7,721	17,541	15,348
R-squared	0.226	0.371	0.376	0.456	0.298	0.526

Source: Data from Statistics Denmark, registries AKM, FIDA, FIRM, IDAP, IDAN, UDDA, 2002-2011.

Notes: The dependent variable measures the number of new matches classified as requiring intermediate skills. Controls include the establishment-level averages of the same characteristics measured for the same establishment in the year 2002, house prices are measured at the municipality level, investment and firms' total assets (logs). Standard errors in parentheses are clustered at the municipality-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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