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HOUSING COLLATERAL, CREDIT CONSTRAINTS AND ENTREPRENEURSHIP
- EVIDENCE FROM A MORTGAGE REFORM

Thais Lærkholm Jensen
Søren Leth-Petersen
Ramana Nanda

Housing Collateral, Credit Constraints and Entrepreneurship - Evidence from a Mortgage
Reform
Thais Lærkholm Jensen, Søren Leth-Petersen, and Ramana Nanda
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ABSTRACT

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

Thais Lærkholm Jensen
Department of Economics
University of Copenhagen
Oster Farimagsgade 5
Building 26
DK-1353 Copenhagen K
Denmark
tlj@econ.ku.dk

Søren Leth-Petersen
Department of Economics
University of Copenhagen
Oster Farimagsgade 5
Building 26
DK-1353 Copenhagen K
Denmark
Soren.Leth-Petersen@econ.ku.dk

Ramana Nanda
Harvard Business School
Rock Center 317
Soldiers Field
Boston, MA 02163
and NBER
rnanda@hbs.edu

I Introduction

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

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

A central approach to addressing this debate has been to examine how unexpected changes in personal wealth impact entrepreneurship. For example, Blanchflower and Oswald (1998), Holtz-Eakin, Joulfaian and Rosen (1994), and Andersen and Nielsen (2012) have all examined the impact of bequests on entrepreneurship. More recently, several papers have examined how increases in the value of home equity due to house price appreciation might impact entrepreneurship via the collateral channel (e.g., Black et al. (1996); Schmalz, Sraer and Thesmar (2014), Harding and Rosenthal (2013), Fairlie and Krashinsky (2012) and Adelino, Schoar, Severino (2012)). Although this work finds that an increase in personal wealth leads to entrepreneurship, it is unable to fully isolate the effect of credit constraints on entrepreneurship, because large increases in wealth, while alleviating credit constraints can also lead to wealth effects. For example, increases in wealth can change an individual's risk aversion (Evans and Jovanovic, 1989; Khilstrom and Laffont, 1979) or preferences (Hurst and Lusardi, 2004) and hence change their propensity to engage in entrepreneurship independent of credit constraints. Distinguishing between these two underlying factors is important,

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because a wealth shock may drive entrepreneurship even if credit constraints are not important. Isolating the impact of credit constraints therefore requires an exogenous change in the financing environment for entrepreneurs that also does not impact their wealth.

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

We find that the reform unlocked a substantial amount of home equity that could be used as collateral for personal loans - about \$30,000 on average and equivalent to more than a year's disposable income for the median treated individual in our sample. Furthermore, we find that many individuals with access to more home equity did in fact increase their personal debt substantially. On average, a \$1 increase in collateral was associated with a \$0.19 increase in personal debt. Yet, we find that the relative increase in the number of active entrepreneurs was 12 basis points, equivalent to about a 4% increase in the number of active entrepreneurs before the reform. When looking at the characteristics of the businesses, we find that existing businesses that were more likely to survive were marginally weaker, and new entrants had much greater failure rates than the control group. Even those entrants that survived had lower sales, profits and employment relative to the

control group, suggesting that businesses started by the marginal entrant who benefited from the reform were of lower quality than those started by equivalent individuals who did not get increased access to housing collateral. Since the reform allowed individuals to access external finance without mortgage banks having to screen the specific projects of potential entrepreneurs, our latter result suggests that individuals may have been starting lower quality projects because they didn't face the discipline of external finance.

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

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

II   Theoretical Considerations

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

Debt finance is the principal form of external finance for most businesses (Berger and Udell,

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1998; Robb and Robinson, 2013) and banks will often use the personal wealth of the owner to assess creditworthiness of new ventures as they have no track record of the firm's performance on which to lend to the business, even if these are young incorporated firms (Berkowitz and White, 2000). One common approach to testing credit constraints is therefore to regress an indicator of entry into entrepreneurship on a measure of the individual's personal wealth and a range of controls. If individuals do not face financing constraints, then the amount of capital that they invest in an equivalent business should not be systematically associated with their personal wealth and hence differences in wealth should not be relevant in predicting entry into entrepreneurship. If the coefficient on individuals' personal wealth is positive, however, it suggests that individuals may be credit constrained (Evans and Jovanovic,1989, Gentry and Hubbard, 2004).

However, a positive association between personal wealth and entrepreneurship does not necessarily imply the presence of financing constraints. It is possible that an individual's personal wealth is endogenous. For example, if individuals with low ability are less likely to generate savings and also less likely to become entrepreneurs, the observed correlation between personal wealth and entrepreneurship may reflect this unobserved attribute rather than the causal effect of financing constraints. Further, suppose that wealthier individuals are more productive as entrepreneurs than as wage employees, say because they have access to better entrepreneurial opportunities or networks, they may be more likely to systematically sort into entrepreneurship than those who are less wealthy. In order to control for such a spurious correlation, researchers have sought to find exogenous shocks to personal wealth and study their effect on selection into entrepreneurship. For example, Lindh and Ohlsson (1999) have shown that those who win lotteries are more likely to be entrepreneurs than those who do not. Andersen and Nielsen (2012), Holtz-Eakin, Joulfaian, and Rosen (1994) and Blanchflower and Oswald (1998) have used inheritances as a source of an unexpected shock to wealth that reduces potential financing constraints.

While these studies have shown the causal impact of a wealth increase on entrepreneurship, their data and empirical set up is such they are not able to isolate the mechanism behind the increase in entrepreneurship. For example, wealthy people may have lower absolute risk aversion, making them more likely to become entrepreneurs (Evans and Jovanovic,1989; Khilstrom and Laffont, 1979), or preferences, such as the desire to be one's own boss, might rise with wealth (Hurst and Lusardi, 2004). If these mechanisms are important, they would lead to a positive association between wealth and entrepreneurship even if the wealth increase was exogenous and they were not affected by credit constraints. The concern above also applies to studies that have shown that increases in house prices

that are unrelated to economic activity have a causal impact on entry into entrepreneurship. While these papers are focused on showing that house price increases cause entrepreneurship, they are still unable to fully isolate the effect of credit constraints, because while house price increases can improve access to collateral, they also raise an individual's wealth. This concern might be particularly salient given that these papers are based on time periods with extremely large house price increases.

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

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

II.A The Danish mortgage market and the mortgage reform of 1992

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

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

The Danish credit market reform studied in this paper took effect on 21 May 1992. The reform was part of a general trend of liberalization of the financial sector in Denmark and in Europe, although the exact timing appears to be motivated by its potential stimulating impact to the economy during the recession of 1992. The reform was implemented with short notice and passed through parliament in three months. The short period of time from enactment to implementation is useful for our identification strategy as it suggests that it is unlikely that the timing of individuals' house purchases was systematically linked to a forecast of unlocking housing collateral for the

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business. The reform changed the rules governing mortgage loans in two critical ways that are relevant to our study. The most important here is that it introduced the possibility of using the proceeds from a mortgage loan for purposes other than financing real property, i.e. the reform introduced the possibility to use housing equity as collateral for loans established through mortgage banks where the proceeds could be used for, among other things, starting or growing a business. The May 1992 bill introduced a limit of 60% of the house value for loans for non-housing purposes. This limit was extended to 80% in December 1992. A second feature of the reform increased the maximum maturity of mortgage loans from 20 to 30 years. For people who were already mortgaged to the limit prior to the reform, and who therefore could not establish additional mortgage loans for non-housing consumption or investment, this option potentially provided the possibility of acquiring more liquidity by spreading out the payments over a longer period and hence reducing the monthly outlay towards paying down the loan. Both these features therefore impacted individuals' access to credit without changing the value of their wealth.

Commercial banks were not restricted in offering conventional bank loans using the house as collateral, either before or after 1992. However the granting of such bank loans was subject to a regular credit assessment based on project's projected cash flows and furthermore, the riskiness of the project was priced into the loan. In practice such loans were mainly used to cover the part of the house price that exceeded the legal limit for mortgage loans. Our discussions with practitioners suggests that bank loans using housing equity as collateral were rarely used for financing business-startups.[1] Even when granted, however, the discussion above helps to put in context that while mortgage loans had a fixed rate and were not assessed a risk premium, the interest rate on collateralized bank loans would be set by the bank and include a premium for the project's riskiness. In practice, therefore, even those who might have borrowed from commercial banks would have experienced a decline in the cost of finance due to the mortgage reform. Overall, therefore, the reform gave households access to credit at a significantly lower price than was possible before and allowed borrowers who could not previously obtain secured loans in commercial banks because they were deemed too risky to now get access to credit through mortgage banks.[2]

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

[2] The minutes of the parliamentary committee preparing the mortgage reform, obtained through the physical archives of the Danish Parliament, state that the mortgage reform was expected to reduce the interest rate on secured loans from 15% to 11% for the average borrower, but the changes would be of different magnitudes across households depending on risk characteristics of the project and the borrowers ability to service the loan.

The highly structured mortgage market in Denmark implied that mortgage-loan-to-value ratios across individuals in 1992 were determined primarily by the timing of the house purchase and the size of the down payment. Households therefore entered the reform with different equity-to-value ratios and were thus effectively treated differentially by the reform. We use this to identify the effect of getting access to credit by comparing the propensity to become a business owner across households who entered the reform period with high vs. low amounts of housing equity that could be used to collateralize loans for the business.

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

III   Data

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

First, the data is collected from government registers on an annual basis, and has detailed micro

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

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

Third, the database links an individual's ID with a range of other demographic characteristics such as their age, gender, educational qualifications, marital status, number of children, as well as detailed information on income, assets and liabilities.[3] House value, cash holdings, mortgage debt, bank debt, and interest payments are reported automatically at the last day of the year by banks and other financial intermediaries to the tax authorities for all Danish tax payers and are therefore considered very reliable. While cash holdings and interest payments are recorded directly, the house value is the tax assessed value scaled by the ratio of the tax assessed value to market value as is recorded among traded houses in that municipality and year, and mortgage debt is recorded by the market value of the underlying bonds at the last day of the year. The remaining components, including the data on individual wealth, are self-reported, but subject to auditing by the tax authorities because of the presence of both a wealth tax and an income tax.

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

The detailed data on liabilities, assets and capital income is particularly useful for a study looking at entrepreneurship where wealth is likely to be correlated with a host of factors that can impact selection into entrepreneurship (Hurst and Lusardi, 2004).

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

III.A Sample

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

III.B   Definition of Entrepreneurship

We focus our analysis of entrepreneurship on individuals who are employers (that is, self employed with at least one employee) in a given year. We use this measure to focus on more serious businesses and make our results more comparable with studies that use firm-level datasets (e.g. such as the Longitudinal Business Database in the US, that are comprised of firms with at least one employee) as well as those that study employment growth in the context of entrepreneurship. Table 2 documents that about 3% of our sample are coded as entrepreneurs in a given year and that the annual probability that an individual enters entrepreneurship is 0.56%.[4]

III.C   Descriptive Statistics

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

IV   Regression Results

We start by documenting that the reform impacted a large number of individuals and that it was substantial. Figure 1 plots the amount of equity that was unlocked for the individuals in our sample. The X-axis buckets individuals into 100 bins of equity to value (ETV) in 1991. We then plot the amount of equity that was unlocked for individuals in each of these buckets (measured on the left Y-axis) at the mean, 25th percentile, median and 75th percentile. These lines document two important facts. First, the amount of equity unlocked was substantial. The average and the median (which track each other closely) amount of equity unlocked by someone with an ETV of 0.6

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

was over 200,000 DKK (over \$30,000). Some individuals with high levels of ETV had over 400,000 DKK unlocked by this reform. Second, the slope of the lines are constant, which documents that the dollar value of equity unlocked was a constant proportion of the ETV in 1991. In other words, the average house value across those in different ETV buckets was extremely well-balanced, suggesting that ETV in 1991 is a good measure for the total amount of credit that was unlocked across the buckets.

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

IV.A   Impact of Mortgage Reform

Having shown that a large number of individuals had a substantial amount of housing equity unlocked by the reform, we next turn to documenting that individuals did respond to this access to collateralized credit by increasing their personal debt. That is, we document that the channel through which the reform was meant to operate did in fact show substantial traction. We focus on interest payments on all outstanding debt rather than the debt level itself because the interest payment measure is less noisy. However, we have verified that our results hold by looking directly at debt as well.[5] In Column 1 of Table 4, we report the results from a differences in differences specification:

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

where $InterestPayments_{it}$ refers to the total interest payments on outstanding debt paid by individual i in year t. $X_{i,t}$ refer to fixed effects for the control variables outlined in Panel A of Table 3 interacted with year dummies. This includes fixed effects for the individual's gender, educational

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

background, marital status, children, as well as fixed effects for age cohorts (one for each year from 25-50) interacted with year fixed effects.[6]   The dummy $I(ETV_{91} > 0.25)_i$ takes the value 1 for individuals whose ETV in 1991 was greater than 0.25. We focus on 0.25 as those below this level are unlikely to have benefited from the reform. The reform only allowed individuals to borrow up to 80% of the home value; even if individuals lowered their payments by extending a mortgage from 20-30 years, those below 0.25 in ETV would have not gained sufficient equity to extract any debt for non-housing purposes. Thus $I(ETV_{91} > 0.25)_i$ captures those who were treated by the reform. $POST_t$ takes a value of 1 for the years 1992-1996 and zero otherwise. Standard errors are clustered at the individual level.   Our main coefficient of interest is β_1 which captures the relative impact of the treatment group in the post period compared to the pre-period within the cells specified by the covariate-year fixed effects.

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

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

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

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

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

IV.A.1 Change in Net Entrepreneurship

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

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

where the empirical framework and the identification strategy is the same as that for the re-

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

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

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

Columns 4-6 break the dummy variable $I(ETV_{91} > 0.25)_i$ into three equal categories and show that the increase is driven largely by those with an $ETV > 0.5$. While those with an $ETV > 0.25$ do exhibit a slight increase, it is not statistically significant. The magnitudes in Column 6, together with the baseline entry rates shown in Table 3 suggest that the reform increased the propensity to be an entrepreneur for those with substantial increases in equity by about 5.5%. Given that the average amount of equity unlocked in the highest ETV bracket was approximately $55,000, our

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results highlight that while clearly impacting entrepreneurship, the effect of the relaxed constraints were small relative to the size of the treatment, even for those with large increases in available housing collateral.[7]

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

IV.A.2   Survival of existing businesses

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

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

One would expect that firms in industries that are more reliant on external finance to benefit more from the ability to borrow against the home. In order to look in to this we allocate firms to industries that are more versus less dependent on external finance. We do this by by calculating, in a pre-period, the change in debt associated with starting a business in each of 111 different

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

industries. Industries that are above the median according to this measure are classified as being more dependent on external finance. The details of the industry allocation are provided in Appendix A, where we also show a positive correspondence to a similar measure constructed using the Survey of Small Business Finances in the US.

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

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

The first three columns of Table 8 report results for the balanced panel of firms. That is, when a firm exits the sample, we code their sales, profits and employment as zero. This has the advantage of ensuring the results are not driven by selection, but on the other hand, they confound performance and survival and hence provide an upper bound for the performance effects of the reform. The next three columns focus on the set of firms that survived until 1996, so they are not confounding performance and survival, but they are a selected sample since they were strong enough to survive across the entire period. The first three columns of Table 8 show that on average, existing firms increase profit added by about 40,000 DKK, sales by about 117,000 DKK and employment by the

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

equivalent of 0.2 full time employees. The effects are estimated imprecisely and are only significant at the 10% level. This marginal increase in performance is, of course, conflated by the higher survival probabilities of the firms. In Columns 4-6 we restrict our analysis to firms that survived the entire period and find that for these firms, the results are reversed. The marginal firm that survived over the entire period due to the reform seems to have been of lower quality (although imprecisely estimated). In sum, our results suggest that the reform increased survival, but that it did not lead to an increase in performance conditional on survival.

IV.A.3 Entry into entrepreneurship

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

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

The results associated with net entrepreneurship in Table 5 show smaller elasticities than would be expected seeing the results in Table 9. To investigate further, therefore, we examine the extent to which the entrants start businesses that survive a long period of time. We separate entrants into those who started businesses that last less than 3 years relative to those who found businesses

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that last at least 3 years. These results are reported in Table 11. Comparing these businesses reveals a striking pattern. The vast majority of the entrants are those that fail within 2 years of entry, which is why the overall number of entrepreneurs, reported in Table 5, shows a much smaller increase. Columns 3 and 4 show that the churning is associated with all the buckets of ETV, while those with the largest increase in available collateral also start some firms that last more than 3 years. Interestingly, looking at columns 5 and 6 shows that one potential reason that these business owners seem to fail is because those in the treatment group significantly increase the likelihood that they will start businesses in industries where they have no prior experience.

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

In order to tease these two explanations apart, we turn to examine the performance of the businesses, similar to the estimations in Table 8. In Table 12, we study the three-year gross profits, sales and employment of entrants, for all firms that entered between 1988 and 1996. These outcomes are obtained from VAT accounts, which as we outlined above, only give us a 60% match with the firms in our sample. Since we have fewer observations in this table, we are unable to include a full set of controls interacted with year dummies but instead include individual controls observed in 1991 as well as year fixed effects in all regressions, and add municipality fixed effects for some specifications. The results show that profits, sales and employment were lower in the post period for firms started by owners who got access to home equity, and even when considering the subset of entrants who survived at least three years in Columns 3-4, we do not find any evidence that the performance metrics improved as a consequence of the reform. Columns 5-7 report the results from quantile regressions to show that the results in columns 1-4 are not driven by outliers and that they are equally present across the profit distribution. Overall, these results point to the fact that the

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reform seems to have lowered the discipline of external finance. While we cannot conclusively say whether these were negative NPV projects, it suggests that the possibility of tapping into home equity either allowed individuals to start lower quality projects, that would have had a hard time getting financed by the bank, (but could be funded by personal debt since the bank was no longer lending based on the attributes of the project). That is, the marginal project funded in the post period by those with access to home equity was of lower quality than the average quality of projects started prior to the reform. This is an interesting result that also helps to reconcile the fact that gross entry following the reform was larger than the net effect of the reform on entrepreneurship.

V   Discussion and Conclusions

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

Our results therefore paint a more nuanced picture of the extent to which financing constraints are important in settings with well-developed credit markets, and the role that home equity can play in alleviating these. The fact that housing collateral shifts the bank's adjudication decision from a specific project to the creditworthiness of the borrower has the potential to be a dual edged sword:

on the one hand, good projects that were precluded from entry due to asymmetric information may be able to be started or sustained. On the other hand, optimistic entrepreneurs or those with non-pecuniary benefits to own businesses may start lower quality businesses because they do not face the same discipline from the bank.

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

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

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

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

Table 2: Stock of entrepreneurs and transition probability

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

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

Table 3: Summary statistics

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

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

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

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

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

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

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

Table 5: Effect of the reform on net entrepreneurship

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

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

Table 6: Effect of the reform on existing firms

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

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

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

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

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

Table 8: Performance of Existing Firms

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

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

Table 9: Effect of the reform on Entry

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

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

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

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

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

Table 11: Effect of the reform on selection into entrepreneurship

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

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

Table 12: Performance of Entrants

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

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

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

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

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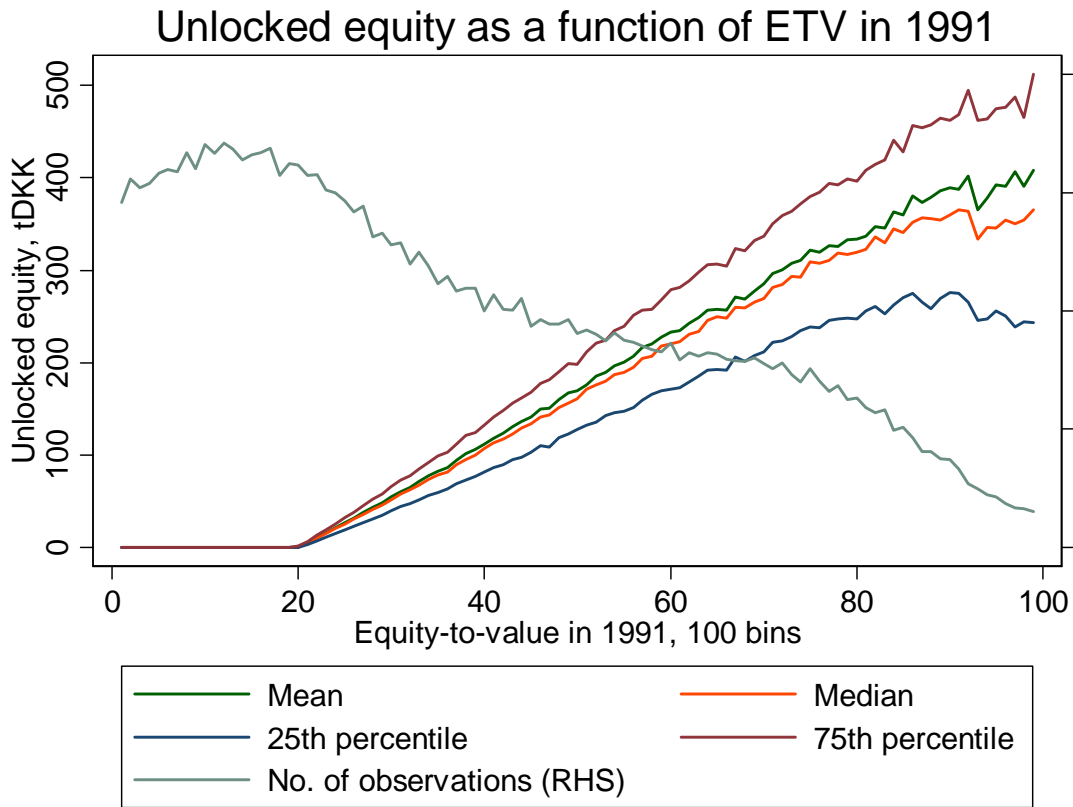
Figure 2: Unlocked equity in 1991 as a percentage of annual disposable income, conditional on equity-to-value being greater than 0.2

Values above 5 consored to 5
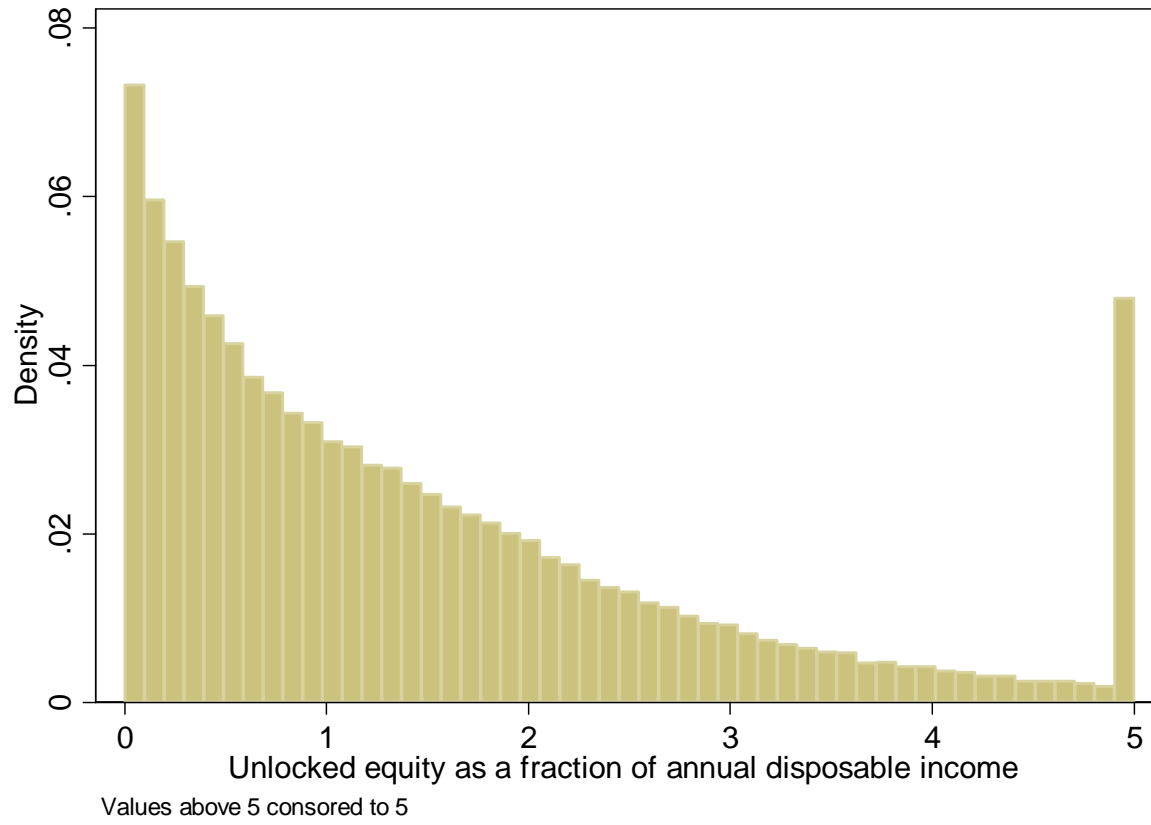
Figure 3: Effect of the reform on net entrepreneurship.

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

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

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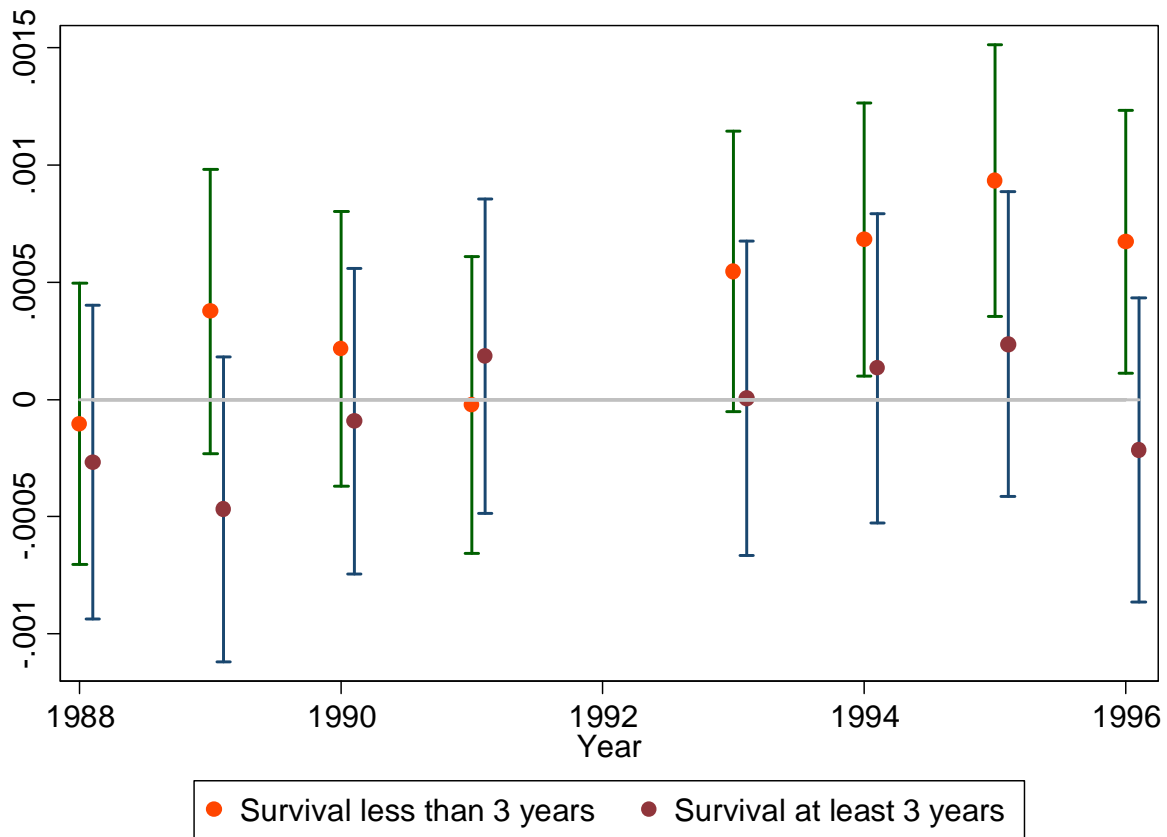
Appendix A: Capital Intensity measures

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

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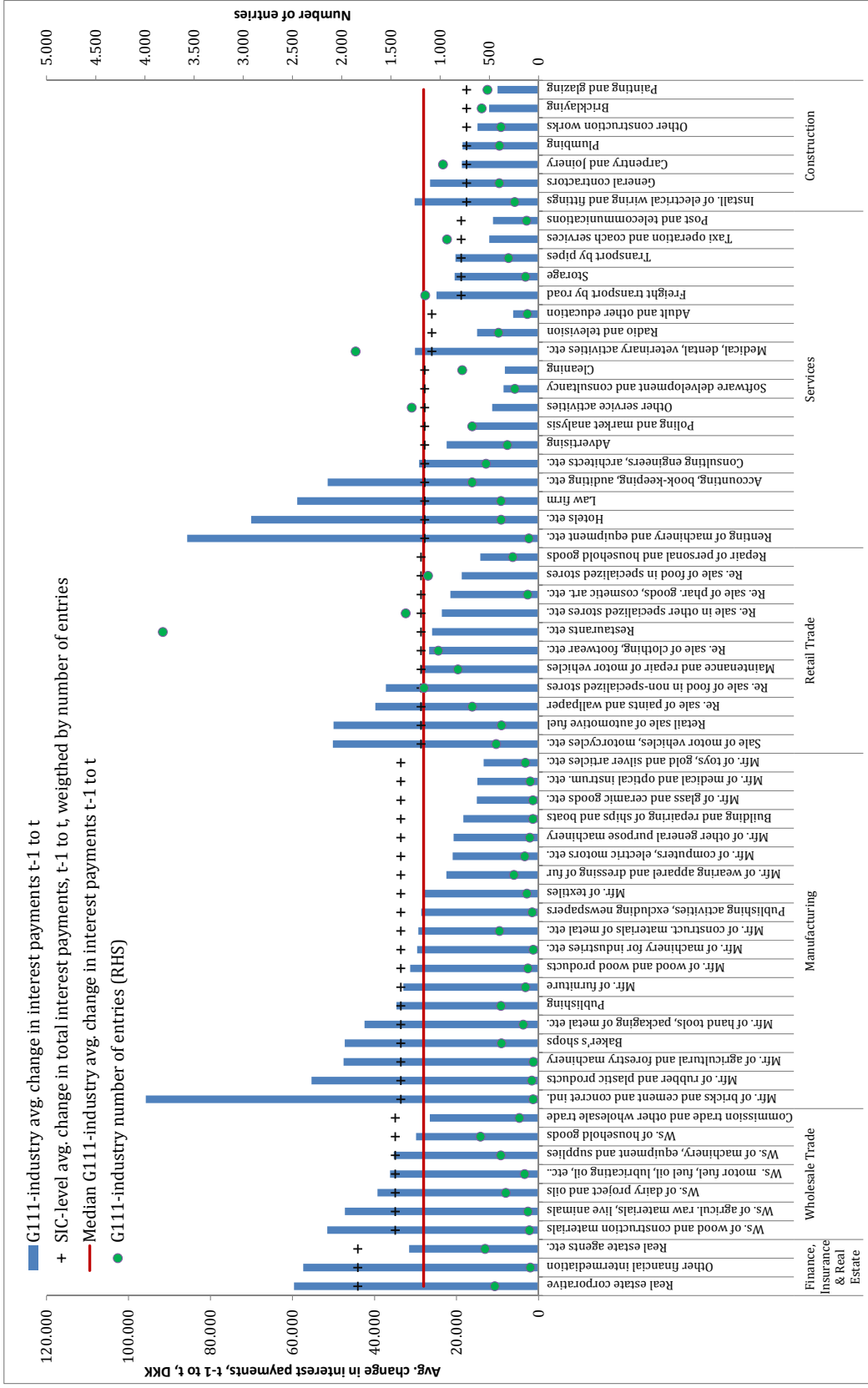
Table A1: Correlation of capital intensity measures

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

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

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

Figure A1: Capital intensity by G111-industries

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

Legend:
- ▬ G111-industry avg. change in interest payments t-1 to t
- + SIC-level avg. change in total interest payments, t-1 to t, weigthed by number of entries
- ▬ Median G111-industry avg. change in interest payments t-1 to t
- ● G111-industry number of entries (RHS)

Appendix B: Interest payments and amortization

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

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

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

Figure B1: Difference in (median) interest payments relative to 1991, by year of purchase
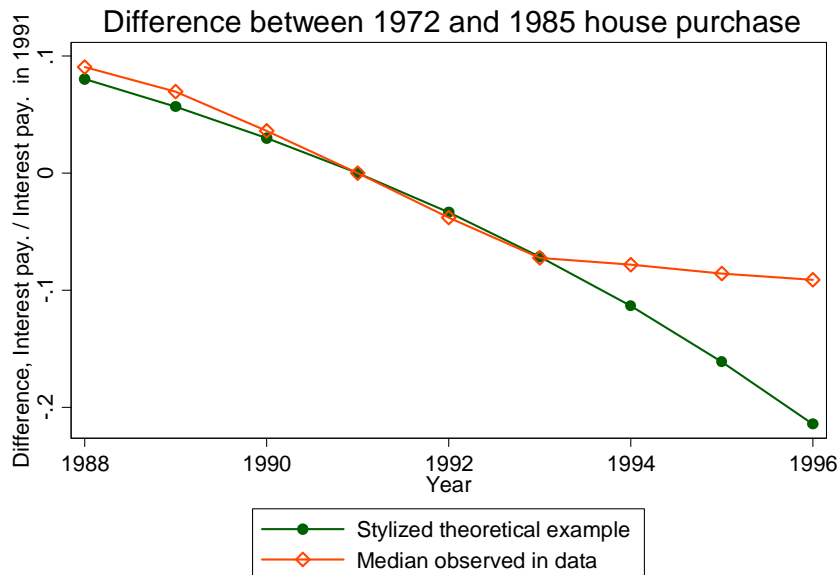
Appendix Figure B2 shows a dynamic version of model (3) in table 4, where an indicator of being an individual who was treated by the reform is interacted with year dummies and shown relative to 1992. The model includes the full set of covariate-year fixed effects as well as individual fixed effects and standard errors are clustered at the individual level. The trend observed in the pre-period is due to the mechanical nature of payments for those with more vs. less mortgage outstanding (see appendix B above for details).

Total interest payments rel. to ETV91[0-.25] group