Financing constraints, home equity and selection into entrepreneurship[*]

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Abstract

We exploit a mortgage reform that differentially unlocked home equity across the Danish population and study how this impacted selection into entrepreneurship. We find that increased entry was concentrated among entrepreneurs whose firms were founded in industries where they had no prior work experience. Nevertheless, we find that marginal entrants benefiting from the reform had higher pre-entry earnings and a significant share of these entrants started longer-lasting firms. Our results are most consistent with a view that housing collateral enabled higher ability individuals with less-well-established track records to overcome credit rationing and start new firms, rather than only leading to 'frivolous entry' by those without prior industry experience.

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1. Introduction

Startups play a disproportionate role in the economy in terms of aggregate job creation and productivity growth (Haltiwanger et al. 2013; Adelino et al. 2017). Reducing financing frictions for new firms therefore remains a key policy goal across the world (Bernanke 2010; Mills and McCarthy 2014).

Despite the importance of the issue and widespread policies across countries to promote entrepreneurship, little is known about how relaxing financing constraints impacts *who* selects into entrepreneurship, both in terms of the characteristics of the entrepreneurs who enter as well as the performance of the firms they found. An understanding of how the composition of potential entrepreneurs shifts when constraints are relaxed is also key to understanding the efficacy of broad-based policies aimed at facilitating entrepreneurial entry, particularly in light of growing evidence of heterogeneity in the motivations and nature of constraints facing different potential entrepreneurs (Hamilton 2000; Hvide and Møen 2010; Hurst and Pugsley 2011; Andersen and Nielsen 2012; Levine and Rubinstein 2017, 2018; Bellon et al. 2021).

The dearth of evidence on this question is driven in large part by the difficulty of connecting an exogenous change in financing constraints to comprehensive, population-wide data that can connect individuals and firms in a longitudinal manner. We overcome these challenges by combining individual- and firm-level panel data, drawn from administrative records in Denmark, together with a major mortgage reform in 1992 that unexpectedly and differentially unlocked home equity across the entire population (Leth-Petersen 2010).

Before the reform, mortgages in Denmark could only be used for buying a house. This changed in 1992 when home-owners could now take out a mortgage and use the proceeds for any purpose, including the financing of a business start-up. Since, at the time, only regular fixed-rate mortgage contracts with instalments were available, equity-to-value at the point of the reform was in part determined by the time since the house was purchased, creating cross-sectional variation in the access to home equity across individuals. We detail below how the reform was unanticipated, and how we use the resulting exogenous variation in access to home equity across individuals to study selection into entrepreneurship. A key element of our study is that in addition to measuring the magnitude of the response to relaxed constraints, we are also able to characterize the marginal entrants in terms of their ex ante background, as well as ex post survival of the startups they found.

We first trace the effect of the reform through home-equity based borrowing to an increase in entrepreneurship. Those benefiting from the reform increased personal debt by 18% relative to those who did not benefit from unlocked collateral. Differences-in-differences estimations document that on average, entry into entrepreneurship for the treated group increased by 14% following the reform. Consistent with prior work documenting the housing collateral channel in enabling entrepreneurship (Adelino et al. 2015; Corradin and Popov 2015; Schmalz et al. 2017), we find this average effect was driven by the sub-population of homeowners who had the largest increase in access to housing collateral.

When looking at the characteristics of entrants and the firms they found, we find two sets of key results. First, we find that the increase in entrepreneurship among the treated group was driven almost entirely by individuals *without* prior experience in the industry that they entered, suggesting that individuals who took advantage of the unlocked home equity were entrepreneurs with less well established track records, who could now overcome credit rationing by pledging tangible assets themselves. Since entrepreneur experience and skill is believed to be an important driver of success, the potential lack or relevant experience for the entrepreneurship we document could be seen as 'frivolous' or driven by those with low human capital (Andersen and Nielsen (2012). Alternatively, it could reflect credit rationing to entrepreneurs who were harder to screen on observable dimensions Stiglitz and Weiss (1981), but who were nevertheless building legitimate firms. Our second set of findings speak to this question of entrepreneur quality. We find that on average, individuals in the treatment group who became entrepreneurs had higher pre-entry income in the year prior to starting their firm than equivalent individuals who founded firms before the reform. Moreover, a substantial share of entry among the treated group was comprised of startups that survived at least three years – which is the median survival age of new firms. These findings are consistent with Evans and Jovanovic (1989), who predict that higher ability entrepreneurs are most likely to be constrained when collateral constraints bind. While we cannot rule out the possibility of 'frivolous' entry in some cases, our results appear most consistent with the view that access to home equity enabled higher ability individuals with less well established track records to overcome credit rationing and start new firms.

Our results relate to a number of strands of the literature on financing constraints in entrepreneurship. While the broader topic of credit constraints facing potential entrepreneurs has received substantial academic attention, a lack of population-level data together with exogenous shifts in credit constraints means that little is known about the characteristics of entrepreneurs who benefit most when constraints are relaxed. Our work highlights how credit rationing appears tied to key elements of a potential entrepreneur's background, which has important implications for who selects into entrepreneurship when constraints are relaxed. In fact, the lack of entry response to increased credit availability among those with prior industry experience, including those with substantial increases in housing collateral entering more capital intensive industries, suggests that credit rationing in the pre-period was related more to financiers' perceptions of an entrepreneur's relevant experience rather than attributes of the industry per se. Given that many entrepreneurs typically found businesses in areas where they have prior experience (e.g., Bhide (1999)), this finding is line with theoretical models, such as Stiglitz and Weiss (1981), where entrepreneurs face credit rationing in equilibrium when they are perceived as being too risky or their ability to be successful is harder to evaluate. An important paper along these lines is Andersen and Nielsen (2012), who find that the marginal entrepreneur who starts a business after receiving a windfall bequest is of lower quality. As with Andersen and Nielsen (2012), we also find an increase in entrants who quickly fail. However, our results on the pre-entry salary of entrants and the presence of longer-term entry suggests that not all the entry in our context was comprised of those with low ability. Indeed, in our context, on average, the marginal entrant had higher ability as proxied by their salary prior to founding their firm.

Our work also speaks to the growing literature that documents the role of housing collateral for enabling entrepreneurship (Adelino et al. 2015; Black et al. 1996; Corradin and Popov 2015; Schmalz et al. 2017; Bracke et al. 2018), since home equity allows banks to rely on pledgable collateral when their screening technology cannot fully overcome the challenges associated with asymmetric information on young firms' businesses (Chaney et al. 2012). While prior studies focus on the ability of home equity to alleviate financing frictions, our results shed light on the characteristics of founders or businesses that may be most likely to benefit from housing collateral: those that financiers find harder to evaluate, rather than just those starting businesses with a higher reliance on external finance. In particular, our finding that those with prior experience entering capital intensive industries did not appear, on average, to be facing constraints is related to recent work showing the high prevalence of lending to firms based on anticipated cash flow (Kermani and Ma 2020) rather than simply the tangibility of collateral. Our finding can also help rationalize why housing collateral is simultaneously shown to be extremely valuable to alleviate financing constraints for some entrepreneurs but also far from universally needed for startups raising external finance (Kerr et al. 2019).

2. The Danish mortgage market and the mortgage reform of 1992

The setting for our study is the Danish mortgage market reform of 1992. Several features make this an attractive setting for such an analysis. First, Denmark's mortgage market has been dominated by fixed-rate mortgage loans that can be prepaid without penalties at any time prior to maturity. In this sense the Danish mortgage market is similar to the US market, where long-term, fixed-rate loans are common, and refinancing is also possible (Campbell 2013). Due to this and the detailed data collected by the Danish authorities, the Danish mortgage market has been the setting for a number of influential analyses in recent years (e.g., Leth-Petersen (2010); Andersen et al. (2020)), highlighting the generalizeability of its context.

Second, the Danish reform provides a unique opportunity to examine the role of home equity in enabling entrepreneurship by understanding the characteristics of those who benefited most from the reform. Until 2007, mortgage debt in Denmark was provided exclusively through mortgage banks, which are financial intermediaries specialized in the provision of mortgage loans. The Danish credit market reform we examine in this paper took effect on May 21, 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.[1] The reform was implemented with short notice and passed through parliament in three months. The short period of time from enactment to implementation is useful for our identification strategy as it suggests that it is unlikely that the timing of individuals' house purchases was systematically linked to a forecast of unlocking housing collateral for the business.

The reform changed the rules governing mortgage loans in two critical ways that are relevant to our study. The most important here is that it introduced the possibility of

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[1]We discuss in Section 4.1 how our identification strategy addresses business cycle effects.

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.

The highly structured mortgage market in Denmark at the time was such that the equity unlocked by the reform was driven largely by the timing of the house purchase and the level of the down-payment. That is, while it was possible to refinance mortgage loans prior to the reform to lock in lower interest rates, refinanced loans had to be of the same maturity as the original loan and the principal could not be expanded. Similarly, people could prepay their loan, but having done so, it was not possible to extract equity through a mortgage loan on the same house. These restrictions implied that mortgage-loan-to-value ratios across individuals in 1991 were determined to a large extent by the timing of the house purchase relative to the reform. Individuals therefore entered the post-reform period with different loan-to-value ratios, implying a differential ability to use home equity to finance their businesses. We use this cross-sectional variation in the available equity at the time of the reform to identify the effect of getting access to credit by comparing the propensity to become a business owner across households who entered the reform period with high versus low amounts of housing equity that could be used to collateralize loans for the business. Section 4.1 provides a more detailed description of our identification strategy.

Commercial banks were not restricted in offering conventional bank loans, 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 as opposed to solely on the basis of the value of housing collateral, as was the case with the mortgage banks.[2]

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[2] The combination of the regulation around mortgage lending and protection afforded by the title registration system and the buffer to cover loan defaults implied that the loans offered by mortgage banks were very safe, justifying lending based solely on the value of collateral. Specifically, when granting a mortgage loan for a home in Denmark, the mortgage bank issued bonds that directly matched the repayment profile and maturity of the loan granted. The bonds were sold on the stock exchange to investors and the proceeds from the sale paid out to the borrower. Once the bank had screened potential borrowers based on the valuation of their property and on their ability to service the loan, all borrowers who were granted a loan at a given point in time faced the same interest rate. This was feasible because of the detailed regulation of the mortgage market. First, mortgage banks were subject to solvency ratio requirements monitored by the Financial Supervision Authority, and there was 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. Furthermore, mortgage banks accumulate a buffer through contributions from all borrowers, and they

Overall, therefore, the reform allowed home owners to raise debt backed by home equity – for any purpose based solely on the assessment of the collateral and not on how the capital would be used. In this way, the riskiness or potential of an individual's start-up did not play a role in their ability to take out a new loan. Studying the characteristics of 'treated individuals' who were most likely to start new firms in the post period therefore also helps us understand the types of individuals who benefited most from being able raise capital in this way, without regard for how it would be used.

3. Data

We use a matched employer-employee panel dataset 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 are collected from government registers on an annual basis, providing detailed 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 precise manner that does not rely on survey evidence. 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 as a means to reduce their tax burden.

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 of entrepreneurs is therefore considerably larger than most studies of entrepreneurship at the individual level. Our analyses are based on a sample of about 300,000 individuals over the 1988-1996 period, leading to 2.7 million observations. It also allows us to control for many sources of heterogeneity at the individual- industry- and region-level.

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

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use this buffer to cover loans defaults.

[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.

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 as the market value of the underlying bonds at the last day of the year. The remaining components, including the data on individual wealth, are self-reported, but subject to auditing by the tax authorities because of the presence of both a wealth tax and an income tax. The detailed data on liabilities, assets and capital income is particularly useful for a study looking at entrepreneurship where wealth is likely to be correlated with a host of factors that can impact selection into entrepreneurship (Hurst and Lusardi 2004).

3.1. Sample

Since we are exploiting a mortgage reform for our analysis, we focus on individuals who are homeowners in 1991 (the year before the reform). Among homeowners, 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 create a nine-year panel for a 25% random sample of these individuals (who were homeowners, 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, this attrition leads to less than a 1% fall relative to a balanced panel, yielding a total of 2,708,892 observations.

3.2. Definition and validation of entrepreneurship measure

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, which are comprised of firms with at least one employee), as well as those that study employment growth in the context of entrepreneurship. As shown in Fig. 1, these are also the entrepreneurs relying considerably on debt finance, who would be impacted by the reform. We define individuals as having entered entrepreneurship if they were not an entrepreneur in $t-1$ but became an entrepreneur in year t.

Fig. 1 compares the trajectory of interest payments on and personal debt for individuals in our sample who transitioned from employment to employers in 1990 (two years prior to the reform) with individuals who remained in employment over the 1990 period.[4] As seen in Fig. 1, those who transitioned to self-employment and to becoming employers had higher *levels* of interest payments but similar pre-trends. This is consistent with them

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[4]The 1990 cohort is useful because it gives us a two year "pre-trend" and allows us to look at debt accumulation up to a year after entry in the pre-reform period.

being wealthier and owning larger houses, as shown in prior research linking personal wealth to the propensity to become an entrepreneur (e.g., Hurst and Lusardi (2004)).

However, Fig. 1 also shows that the sharp increase in interest payments around the year of entry is seen principally among employers as opposed to those entering self-employment. The increase in interest payments in the year of entry is equivalent to a 230,000 DKK increase in debt around the entry year (equivalent to just under $40,000). Individuals becoming employers are therefore more likely to need external finance and hence face financing constraints. A second element of this analysis is that it highlights both the importance of debt financing for new firms, as noted in Robb and Robinson (2014), as well as how a number of individuals were able to raise debt to finance their businesses in the pre-reform period despite not having access to housing collateral.

3.3. Descriptive statistics

Table 1 documents that the annual probability that an individual enters entrepreneurship is 0.58%. These numbers are very consistent with those seen in US. It also provides descriptive statistics, of the covariates across the treatment and control groups used in our subsequent analysis. About 45% of the individuals in our sample were in the treated group. The table highlights that, on average, those in the treated group bought their house several years earlier (on average in 1979), whereas those in the control group bought their house in 1985. This difference in the timing of when the home was bought is the key source of variation we want to exploit. Unsurprisingly, the individuals with greater than 0.25 in ETV are different from those with an ETV below 0.25, along dimensions related to life cycle, wealth, and family choice. For example, individuals in the treated group are older, somewhat less likely to have children, and are wealthier, which intuitively relate to having greater cash available for a downpayment or having bought the house earlier in time. However, as we elaborate in more detail below, our estimation design aims to control for these differences (not only in levels, but also in terms of their differential impact across time) by interacting these covariates with a full set of year fixed effects.

4. Results

4.1. Identification strategy

As noted in Section 2, the mortgage reform we study took effect in 1992, and enabled individuals, for the first time, to borrow against their home for uses other than the property itself. Our identification strategy exploits cross-sectional variation in the intensity of the reform's treatment across individuals. The reform allowed individuals to borrow up to a maximum of 80% of the home value. Even if individuals lowered their payments by extending a mortgage from 20 to 30 years, those with more than 0.75 in loan-to-value (LTV) would have not gained sufficient equity to extract any debt for non-housing purposes. We therefore focus on individuals with less than 0.75 in LTV or those with at least 0.25 in equity-to-value (ETV) in 1991 as our treated group. In our core specifications, we therefore compare the differential response of individuals who had home equity unlocked by the reform to those who did not get any equity unlocked. 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. The core specification takes the form:

$$y_{it} = \beta_1 I \left(ETV_{91} > 0.25 \right)_i + \beta_2 POST_t \times I \left(ETV_{91} > 0.25 \right)_i + \gamma \mathbf{X}_i^{91} \times \phi_t + u_{it} \quad (1)$$

where $I \left(ETV_{91} > 0.25 \right)$ is an indicator that takes a value of 1 if the individual was treated by the reform, $POST_t$ is an indicator that takes a value of 1 for the 1992-1996 period, $\mathbf{X}$ is a matrix of individual-, municipality- and industry-level controls, ϕ_t refer to year fixed effects which, as shown in Eq. (1) are interacted with the sets of individual, municipality and industry covariates. Standard errors are clustered at the individual level. Our key coefficient of interest is β_2, which measures the response of individuals who got access to home equity following the reform relative to those who did not get access.

While $I \left(ETV_{91} > 0.25 \right)_i$ is an indicator in the base specification, we also estimate specifications where $I \left(ETV_{91} > 0.25 \right)_i$ is expanded to be a vector of dummy variables indicating different levels of equity to value in 1991. We do this to explore whether effects vary with the amount of credit that homeowners gain access to with the 1992 mortgage reform but without imposing arbitrary functional form assumptions. We also report results using a continuous measure of ETV in the Appendix to document the robustness of the results.[5]

As shown in Eq. (1), we account for the differential response of individuals at different points in the life cycle, wealth, working in different industries and living in different municipalities by including an interaction between these individual covariates and year fixed effects.[6] Specifically, we include in $\mathbf{X}_i^{91}$ indicators to the individual's gender, educational background, marital status, children, age (one for each year from 25-50), household wealth (fixed effect for the decile of household wealth), the municipality of residence and the industry that the person works in. We interact each of these characteristics with year dummies, ϕ_t, to control for different trends in debt accumulation and entrepreneurship across people with different observable characteristics. Given the structured mortgage system at the time, a significant driver of who got access was driven by when the home was purchased. Table A.1 in the Appendix documents ETV, or the percentage of house value that is available to collateralize for investments other than the home, in the year prior to the reform, broken down by an individual's age and when they bought their house. As can be seen from Table A.1, the level of equity is much more stable across rows than within columns. That is, a significant driver of the amount of housing equity available to collateralize seems to be the timing of the home purchase. Those who bought their home after 1984 tend to have less than 25% 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.

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[5]As we show below, the relationship between access to equity and entrepreneurship appears to be highly non-linear. Individuals with the largest amount of unlocked equity respond with substantially greater elasticities. Because of this, we prefer the non parametric specifications to estimate magnitudes. However, as seen in Appendix Tables A.3 and A.4, the results are robust to imposing a linear relationship between unlocked equity and entry into entrepreneurship.

[6]There are 98 municipalities in Denmark, which is a level of aggregation that is larger than a ZIP code but smaller than a county. To put this in perspective, Denmark's population is approximately the size of Massachusetts, which has fourteen counties and 536 ZIP codes.

While age, which proxies for life cycle factors that would impact the timing of the home purchase, is clearly important, Table A.1 documents that there is significant variation in available equity within age buckets, which in turn is strongly correlated with the year in which the house was bought. This plausibly exogenous variation in the timing of house purchase by some years relative to the reform is the source of our identification. In effect, we are examining the relative response of two identical individuals (in terms of their age, gender, educational background, wealth, marital status, and children) who work in the same industry and live in the same municipality, but one who bought the home some years before the other.[7]

Our identification is therefore predicated on the assumption that, controlling for covariate-times-year fixed effects, the timing of the house purchase is unrelated to the propensity to become an entrepreneur. Our discussion above helps document that the notion of using home equity did not exist before the reform and that it was passed quickly enough that it could not have directly impacted the decision to purchase a house to unlock collateral.[8]

4.2. Borrowing based on home equity

We start by documenting that the reform impacted a large number of individuals and that it was substantial. In Fig. 2, we plot 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 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 equity unlocked was 200,000 DKK ($33,819 using the end of 1991 exchange rate of 5.91). This amount was large both in relative terms (the median treated individual got access to at least a year's disposable income) and in terms of the starting capital of business. Some individuals with high levels of ETV had over 500,000 DKK (over $70,000) 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 both that ETV in 1991 is a good measure for the total amount of credit unlocked across the buckets and that ETV did not vary dramatically across wealth.

We next document that individuals in the treated group responded to the reform by substantially increasing the amount of personal debt outstanding. In Table 2, we present results from a version of Eq. (1), where the dependent variable is the level of household debt in each year, measured in constant 1991 DKK. Column 1 documents that after

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[7]In principle, those who got access to housing collateral may also have differential house price changes *within a given municipality* in the pre-period, as any across-municipality differences in house prices are addressed through municipality-by-year fixed effects. In practice (in unreported regressions), we find no difference in the trajectory of house prices within municipalities for those in the treatment group relative to the control group.

[8]We document parallel trends in the pre-period in Fig. 3, 5, 6 and 7. We therefore believe that Eq. (1) enables us to estimate the impact of a release of home equity on household borrowing and entrepreneurship.

controlling for covariate-by-year fixed effects, the estimated debt outstanding for Danish households in 1990 was 654,605 DKK (approximately $110,000). As expected, those with ETV> 0.25 are individuals who bought their homes earlier, implying that they had more of their mortgage paid down and had lower levels of debt. On average, individuals in the treated bucket held about half the debt of those in the control category (654,605 - 322,744 = 331,861 DKK in debt). The average increase in household debt for the treated group in the post period was 62,204 DKK. Column 2 shows that the inclusion of municipality-by-year fixed effects barely shifts the coefficients, indicating that the equity extraction was not different across municipalities of Denmark. Comparing the average increase in household debt in the post period for the treated group (62,046 DKK) with the average level of debt in the pre-period (651,304 - 315,122 = 336,182), suggests that the treatment group increased household debt by an average of 19% in the post period, consistent with prior work looking at the elasticity of household debt extraction with respect to changes in collateral value (e.g. Hurst and Stafford (2004); Mian and Sufi (2011)).

Columns 3 and 4 document how this average of 19% varies across the size of the treatment. For example, those with ETV between 0.25 and 0.5 increased debt by 7% (33,810/(654,437-193,746)) in the post period, while those with an ETV between 0.75 and 1 increased household debt by an average of 66% (106,275/(654,437-494,285)). Table A.3 shows that the large average increases in household debt were driven by about 10% to 15% of households extracting an exceptionally large amount of debt together with many households that did not increase debt substantially, or at all.

In Fig. 3 we show the dynamics of household debt around the reform, broken down by ETV bucket, by reporting results of dynamic specifications corresponding to column 4 of Table 2. This figure documents two important patterns that provide further confidence that the collateral channel was driving the results noted in Table 2. First, it documents a strong reversal in the level of household debt, corresponding exactly to the timing of the reform. Second, the cross-sectional variation in the patterns noted – where those with higher ETV show a stronger reversal – are also exactly what one would have predicted if the increase in debt was being driven by the increased access to housing collateral.

4.3. Entry into entrepreneurship

Having established that the reform unlocked a significant amount of housing collateral and that those in the treatment group responded by increasing their personal debt, we now turn to studying 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.

We look next at entry into entrepreneurship. Table 3 reports the coefficients from the differences-in-differences specification outlined in Eq. (1), where the dependent variable takes a value of 1 if the individual was not an entrepreneur in $t-1$ but became an entrepreneur in year t.

Table 3 shows that as with household debt, the treated group had a larger increase in entrepreneurship in the post period. Moreover, the coefficients are extremely stable when including a larger number of fixed effects. Columns 2 and 4 not only include municipality-by-year fixed effects but also industry-by-year fixed effects to control for differential increases in entry rates across industries that might confound the estimates. The stability of the coefficients suggests that neither geographic nor industry differences

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in the entry across treated and control groups is responsible for the results reported in columns 1 and 3 respectively.[9]

We conduct two further tests to bolster the view that our results are picking up the impact of the reform. First, in Fig. 4 we plot dynamic specifications corresponding to column 2 of Table 3, showing parallel trends prior to the reform and an increase in entry emerging after the reform. Table A.6 reports results from placebo regressions, where we run the same estimations as before, but on the sample of individuals with ETV<=0.25 so that none of these individuals should have benefited from the reform. We split this group into those with higher ETV (i.e., those with an ETV between 0.125 and 0.25) and examine whether there is a stronger effect for this group in the post period. As can be seen from Table A.6, the coefficient on the interaction is insignificant, suggesting that it is treatment rather than some other factor associated with higher ETV that is driving the result.

Calculating the magnitude of the effect in the same way as was calculated in Table 2, column 2 of Table 3 documents that the baseline probability of entry after controlling for covariate-by-year fixed effects is 0.59% and that those in the treated group had slightly lower entry rates in the pre-period. Accounting for this difference in baseline entry probabilities, column 2 shows that the increase in entry of 0.061 percentage points due to the unlocked home equity corresponded to a 14% increase in entrepreneurship for the treated group relative to the pre-period. Columns 3 and 4 show that this average effect was driven by the much larger impact on those in the 0.75-1 ETV bucket, in other words those who benefited the most from treatment. For this group of individuals who comprised about 10% of the total sample, the increase in entrepreneurship relative to the pre-period was 28%. Although the magnitude is smaller and not precisely estimated, the pattern associated with the other two treated buckets also document a increase in entry relative to the control group in the post-period.

In understanding the magnitude of the response, it is noteworthy that the 14% average appears driven to a large extent by those in the highest ETV bucket, where as noted above, we report a 28% increase in entry in the treated group relative to the pre-period. Unlocked equity was substantial in this group – equivalent to an increase in home equity arising from a 75% increase in house prices in a context such as the US. This non-linear relation between unlocked equity and entrepreneurship suggests that the benefit of housing collateral may be particularly valuable when it facilitates comparatively larger loans.

4.4. Characteristics of entrants

Thus far we have documented that the reform unlocked substantial equity for homeowners, that the elasticity of debt extraction was similar to prior studies and that the average increase in entry of 14% among the treated group relative to the control was driven by those who had the most intensive treatment. These results document that po-

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[9]Identification in our setting comes primarily from cross-sectional variation since most individuals only attempt entrepreneurship once. However, we have within variation and hence are mechanically able to estimate individual fixed effects regressions. As can be seen from Table A.5, the coefficients remain stable when including individual fixed effects, implying that there are no residual correlated individual effects that matter for our parameter of interest. Given this, we report the results exploiting cross-section variation but have verified that all results are robust to the inclusion of individual fixed effects.

tential entrepreneurs facing credit constraints could overcome them by pledging housing collateral to raise the capital needed to start their business.

We turn next to understanding the characteristics of entrants in order to understand the types of entrepreneurs that might benefit most from being able to pledge housing collateral. As noted above, a unique element of our setting is that we can examine the types of individuals for who we see the greatest relative response from the treated group, which enables us to characterize the marginal entrant that benefited most from the reform.

To characterize the entrepreneurs who benefit most from getting access to housing equity, we look at the entry responses based on the experience the entrepreneurs had in the industry where they found their firm. Specifically, we examine the industry into which the individual entered (at the granularity of 111 industries similar to that documented in Fig. 5[10]) and code entry based on whether the industry they worked in period $t-1$ is the same as that of the industry in which they founded their startup. These results are reported in Table 4.

Before interpreting the results in Table 4], note that so as to focus on the key results, we now report only the specifications with the full set of fixed effects (equivalent to columns 2 and 4 of Table 3. Note also that while the β_1 coefficients are included in all estimations we only report the β_2 coefficients from Eq.(1). so as to focus on the key coefficients of interest. Finally, the dependent variable in these estimations corresponds to entry in industries where the entrepreneur had prior experience vs. no prior experience. In other words, all regressions are run on the same sample rather than being split on any dimension.

This approach has two benefits. First, we are able to control for systematic trends related to an industry an individual starts a business in, without having to split the sample on any dimension. Second, due to the fact that entry the dimensions we use are mutually exclusive and collectively exhaustive, the sum of the coefficients should equal the overall coefficient associated to entry reported in Table 3. This implies that in addition to examining whether or not entry into these buckets is statistically significant from zero, one can examine the share of the overall entry response that comes from those starting businesses in where they had prior experience.

Looking at columns 1 and 3 of Table 4 shows that the entire response we measure in column 2 of Table 3 comes from those entering industries where they do not have any prior experience. Comparing columns 2 and 4 reinforces this finding, showing that this is equally true across all the treatment buckets. In fact, the effect is so strong that even those in the 0.5-0.75 ETV bucket show a statistically significant response, something that we did not see in Table 3. Our results document the apparent importance of having prior experience in an industry as a strong signal for financiers, which is consistent with studies that highlight the degree to which financiers focus on entrepreneurial backgrounds when making capital allocation decisions (Bernstein et al. 2017).

In Figures 6 and 7, we graph the results from dynamic estimations, where we report year-by-year effects relative to 1991. Fig. 6 reports the coefficient of dynamic estimations corresponding to columns 1 and 3 of Table 4, while Fig. 7 reports the coefficients on the

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[10]In the interest of space, Fig. 5 depicts the capital intensity for the main 1 digit SIC industries which is why the sub-categories of industries in the figure do not represent all 111 industries used in the analysis.

highest ETV bucket of dynamic estimations corresponding columns 2 and 4 of Table 4. As can be seen with both figures, there are parallel trends prior to the reform, a sharp and persistent increase in entry among those entering in industries where they did not have prior experience, and no differential increase among the treated group for those entering in industries where they had prior experience.

The fact that we see no measured response from treated individuals starting businesses with prior experience, even among those with substantial treatment, is interesting. It suggests that the individuals for whom access to home equity reduced constraints was primarily the subset of those starting firms in industries where it was harder for a financier to evaluate whether the entrepreneur was qualified to run the venture. One way to examine whether the channel is related to entrepreneurial characteristics vs. attributes of the industry, such as capital intensity, is to examine the differential response of individuals entering more vs. less capital intensive industries based on their prior experience. We measure capital intensity of an industry by calculating the change in debt associated with starting a business as an employer in each of 111 different industries in the pre-period, as shown in Figure 5. Industries that are above the median according to this measure are classified as being more dependent on external finance, and as seen in Figure 5, this variation exists both across broad industry classifications but also within industry classifications, so continues to be identified when including the broader industry-by-year fixed effects.[11]

Table 5 reports results from the to jointly study entry into more vs. less capital intensive industries based on whether the entrepreneur had prior experience in that industry or not. Looking across columns 1 through 4 shows that there is a negligible response for those with prior experience in their founding industry. Of particular note, as seen in columns 1 and 2 is that we see no measurable impact of relaxed constraints even among those entering capital intensive industries and with the highest level of treatment. The coefficients here are precisely estimated, so we can rule out that our null effect is due to a lack of power. On the other hand, we see substantial effects among those without prior experience. Here we see a statistically significant response for all the treated groups entering capital intensive industries, not just those with ETV>0.75 (column 7). Second, we now see statistically significant responses for two of the three buckets among those without experience entering less capital intensive industries (column 8). Entrepreneurs with less-well-established track records faced credit rationing even when raising financing for less capital intensive industries. In addition, since an absence of a measured response from a group when financing constraints are relaxed can be interpreted as that given group not facing borrowing constraints (Banerjee and Duflo 2014), our results point to the fact that founder experience rather than capital intensity per se seems more relevant in driving the credit rationing facing entrepreneurs.

4.5. Founder ability

Our results thus far highlight how the entry response to the mortgage reform was concentrated among those with a lack of experience in the industry where they founded

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[11]In unreported analyses, we also find a positive correspondence to a similar measure constructed using the Survey of Small Business Finances in the US.

their firm, pointing to the fact that these individuals were most likely to have been constrained prior to the reform. Given the perceived importance of prior background in entrepreneurial success, it is possible that this could have been frivolous entry driven by personal preferences, or comprised of low ability entrepreneurs founding 'marginal' businesses (Andersen and Nielsen 2012). On the other hand, theories such as Evans and Jovanovic (1989) predict that higher ability entrepreneurs are most likely to be constrained when collateral constraints bind.

To understand founder ability, we proceed in two steps. First, we examine ability using an ex ante measure, proxied by the income rank of the individual in the year prior to entry. On average higher ability individuals are likely to earn more, so systematic shifts in the average ability of individuals can be measured through shifts in the average income rank of individuals. Second, we examine the ex post outcome of the ventures they found, looking in particular at firm survival. Since half of new ventures fail within three years of entry, looking at the degree to which these startups survive at least three years can provide an indication of their quality.

In Table 6 we report results from estimations where the dependent variable is the interaction between an indicator that takes a value of 1 if the individual is classified as being an entrant in Table 4 and their percentile within the income distribution in the year prior to their entry. Higher income is coded as having a higher income percentile, so that a positive coefficient implies that marginal entrants benefiting from the reform had higher income in the year prior to their entry. As can be seen from columns 3 and 4 of Table 6, on average, treated entrants without experience had higher pre-entry income in the post-period, suggesting that the marginal entrant from the treatment group had a higher outside option compared to equivalent entrants prior to the reform. This finding is consistent with Evans and Jovanovic (1989), who predict that higher ability individuals are more likely to be constrained when collateral constraints bind so that a relaxation of a financing constraint should lead the marginal entrant to be higher ability than those who entered when constraints were binding.

While Table 6 examines founder quality using an ex ante measure of pre-entry earnings, we are also able to trace the survival of the firms that entrepreneurs found, to examine an ex post measure of firm success - namely survival. Prior work has documented that about half of all entrants in a given cohort fail within three years of entry (Kerr and Nanda 2009). Studying the degree to which the firms started by treated individuals survived for a longer period of time allows us to unpack the degree to which this was 'frivolous' or churning entry that quickly ended in failure, or whether the firms that were founded were those that survived a longer period of time. We document the results of this analysis in Table 7, where the dependent variable is an indicator that takes a value of one if the firm entered and survived at least three years. As can be seen from Table 7, a substantial share of the entrants following the reform had firms that did not quickly fail, which is consistent with the fact that these were high ability individuals starting legitimate firms.

Putting the results from Table 6 and Table 7 together, we find that marginal entrants benefiting from the reform had higher pre-entry earnings and included a substantial number of longer-lasting firms, suggesting that the reform did not just lead to 'frivolous entry'. Rather, housing collateral enabled high ability individuals with less-well-established track records to overcome credit rationing and start new firms.

5. Conclusion

In this paper, we combine a unique mortgage reform with population level matched employer-employee micro data to study how an exogenous increase in the ability to access home equity finance impacted selection into entrepreneurship. A critical element of our setting is the fact that prior to the 1992 reform, individuals in Denmark were precluded from borrowing against their home for uses other than financing the underlying property. The reform therefore enabled home equity loans for the first time, and hence allowed individuals who were previously credit constrained to borrow from mortgage banks based on the strength of their housing collateral.

We highlight how this allows us to not only measure the quantitative impact of relaxing financing constraints, but also to understand the characteristics of entrepreneurs who demonstrated the greatest response. In doing so, we can shed more light on the types of individuals who benefit the most from a relaxation in collateral constraints.

We find the reform lead to a 14% increase in entry on average, with substantially stronger increases among individuals who had more housing equity unlocked by the reform. When understanding the types of individuals who benefited the most, we find that the increased entry was concentrated among the set of individuals starting businesses in industries they where they did not have prior experience. In looking at the quality of firms being founded, we find that on average, treated individuals with higher pre-entry income became entrepreneurs after the reform and that a substantial share of this entry was longer lived. While we do find entry that was comprised of early failure and cannot rule out that some of this was 'frivolous' or more marginal entry, our results suggest that on average, it was higher ability individuals without well-established track records who were among the biggest beneficiaries of the reform. This is also similar to findings of the way in which banking deregulations in the US enabled entrepreneurship (Black and Strahan 2002; Cetorelli and Strahan 2006), where Kerr and Nanda (2009) find that deregulations led to both an increase in longer-term and churning entry.

Our results are relevant to the extensive literature on entrepreneurial entry, and the degree to which this is shaped by credit constraints. While substantial work has documented the presence of financing constraints and the degree to which housing collateral can alleviate them, less is understood about *who* benefits more when constraints are relaxed. Our results shed light on the characteristics of founders or businesses that may be most likely to benefit from housing collateral: those that financiers find harder to evaluate, rather than just those starting businesses with a higher reliance on external finance. In doing so, these results also provide strong empirical support for canonical models of credit constraints in entrepreneurship that predict credit rationing when screening is difficult (Stiglitz and Weiss 1981) and that the marginal entrant who benefits from relaxed constraints is likely to be of higher quality than those entering prior to a constraint being relaxed (e.g. Evans and Jovanovic (1989)).

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Figure 1: Use of debt financing by entrepreneurs, self-employed and those in paid employment.

This figure uses interest payments on personal debt to document the degree to which reliance on debt changes for individuals who transitioned from employment to being entrepreneurs, non-employer entrepreneurs (self employed) and those who remained in employment over the 1990 period.
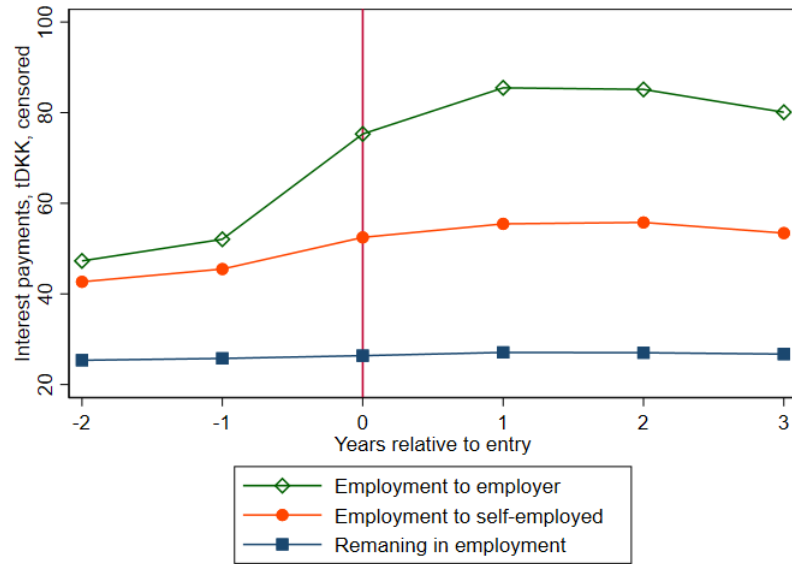
Figure 2: Unlocked equity as a function of ETV in 1991

This figure 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.
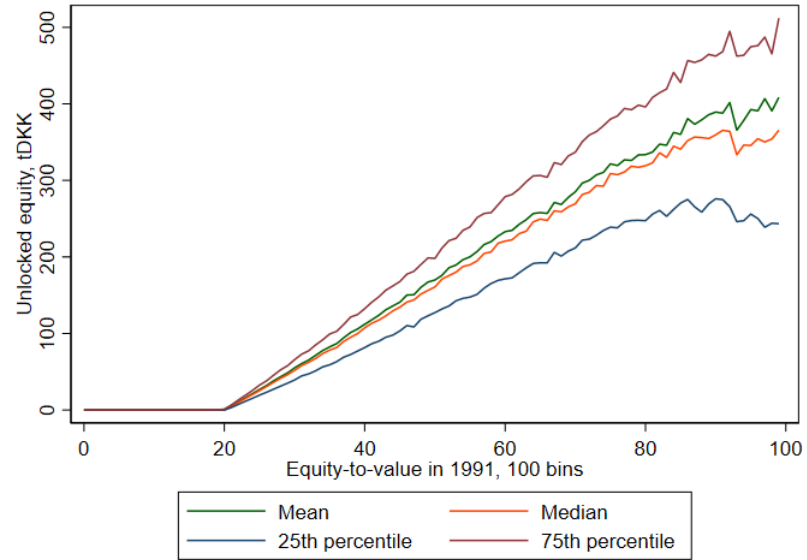
Figure 3: Relative change of household debt for treatment groups

This figure plots the point estimate and ninety-five percent confidence intervals from dynamic estimations corresponding Table 2, where we report year-by-year effects relative to 1991.
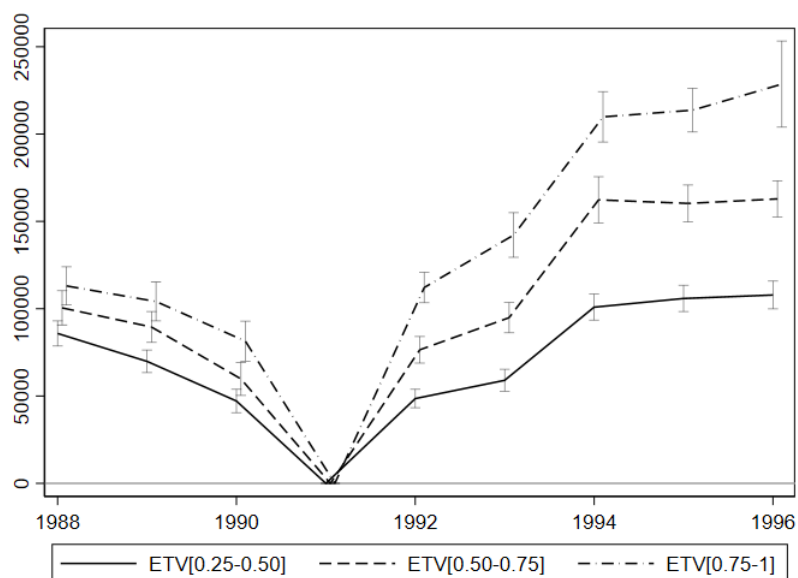
Figure 4: Relative increase in treated group's entry

This figure plots the point estimate and ninety-five percent confidence intervals from dynamic estimations corresponding to column 2 of Table 3, where we report year-by-year effects relative to 1991.
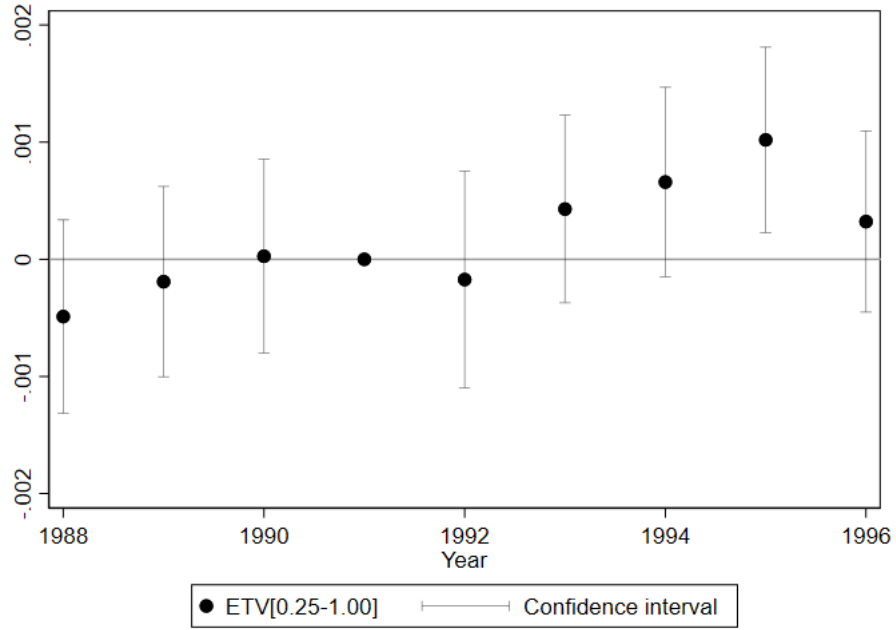
Figure 5: Capital Intensity by Industry

This Figure depicts the capital intensity for a subset of the 111 industries in our analysis that are part of the main 1 digit SIC industries. Industries that are above the median according to this measure are classified as being more dependent on external finance.
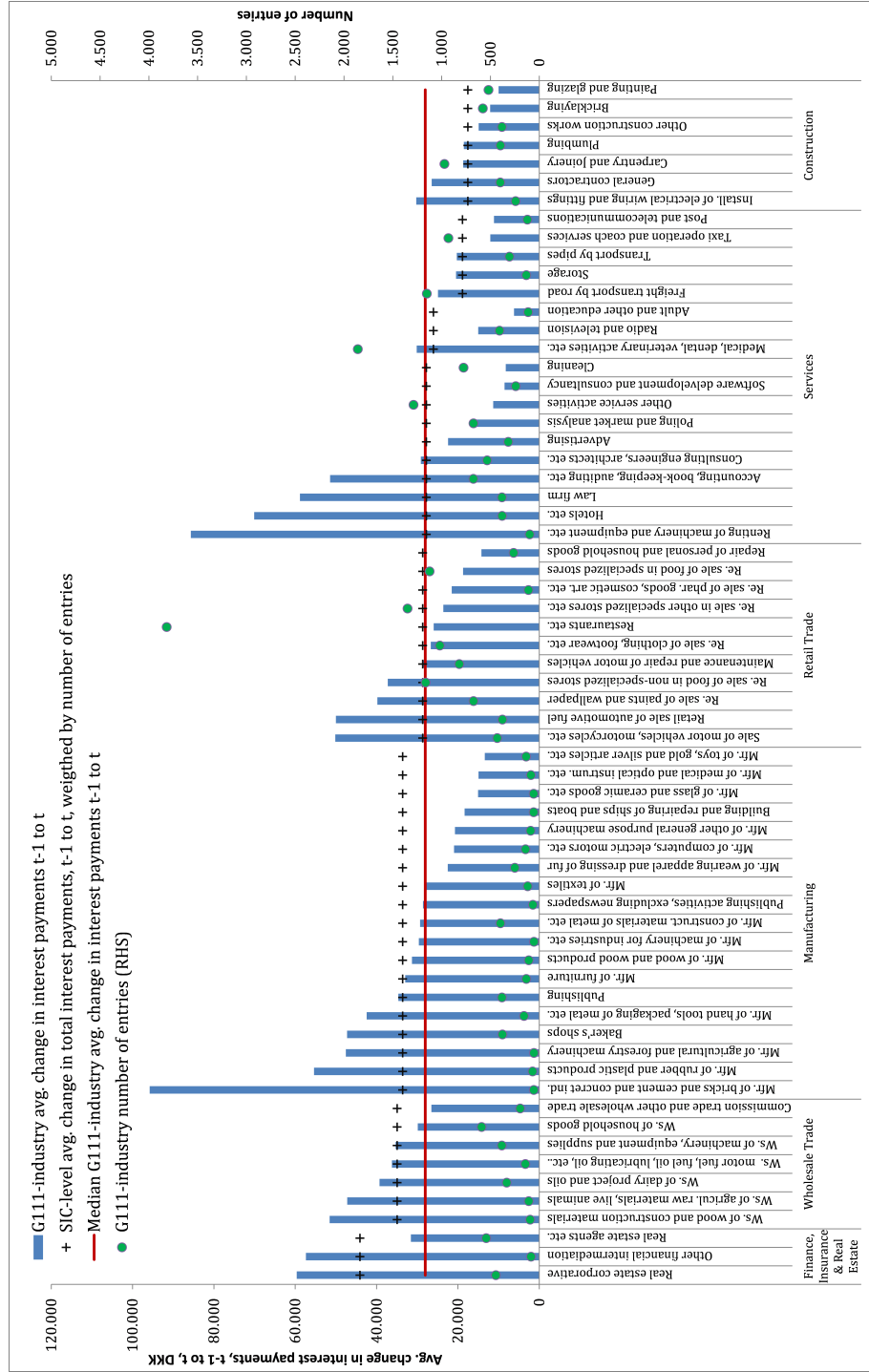
Figure 6: Relative increase in treated group's entry for those with and without prior experience in the startup's industry

This figure plots the point estimate and ninety-five percent confidence intervals from dynamic estimations corresponding to columns (1) and (3) of Table 4, where we report year-by-year effects relative to 1991.
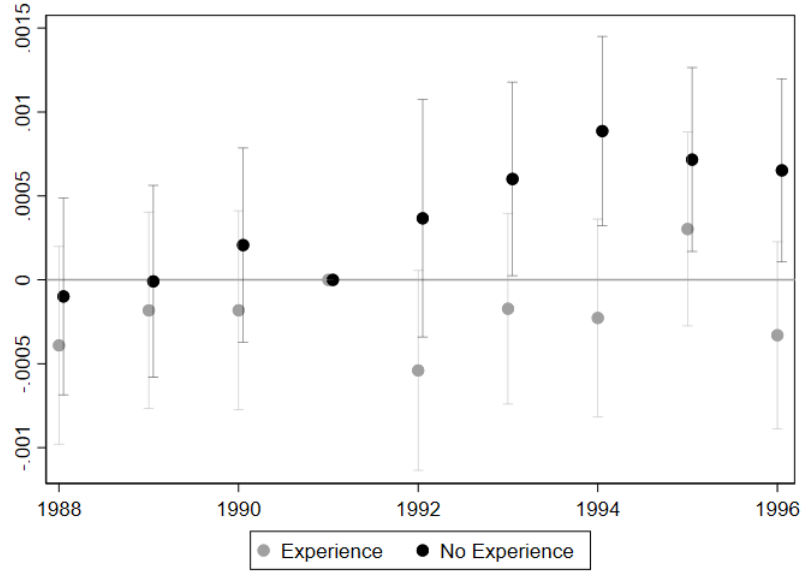
Figure 7: Relative increase in group receiving the highest level of treatment for those with and without prior experience in the startup's industry

This figure plots the point estimate and ninety-five percent confidence intervals from dynamic estimations corresponding to the highest ETV bucket of the estimations documented in columns (2) and (4) of Table 4, where we report year-by-year effects relative to 1991.
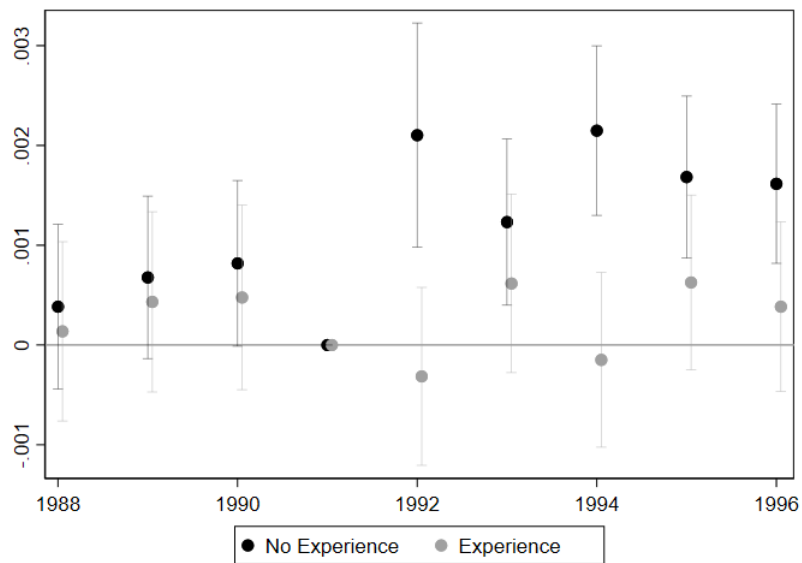
Table 1

Summary statistics

This table presents descriptive statistics for the 303,431 individuals in our sample based on whether they were the treatment or control group in 1991. The treatment group comprises individuals whose equity-to-value (ETV) in 1991 was between 0.25 and 1, and is further broken down into three equal buckets of ETV. The control group are those whose ETV in 1991 was less than 0.25. 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 houses that have been traded in that municipality and year. Liquid, non housing assets comprise the individual's other financial assets such as stocks, bonds and bank deposits. All variables are measured as of 1991, the year before the reform.

	Total	[0-0.25]	[0.25-0.5]	[0.5-0.75]	[0.75-1]
Average year of house purchase	1982	1985	1981	1977	1978
Age	38.7	36.4	40.0	43.0	42.4
Female=1	0.51	0.49	0.51	0.54	0.57
Partner=1	0.88	0.87	0.89	0.92	0.86
Kids=1	0.64	0.66	0.66	0.61	0.53
Educ, Vocational,	0.47	0.47	0.47	0.49	0.46
Educ, BSc	0.15	0.15	0.14	0.14	0.13
Educ, MSc, PhD	0.05	0.06	0.04	0.04	0.04
Housing assets, tDKK	770,576	733,381	844,544	879,324	704,639
Liquid (non-housing) assets, tDKK	138,667	112,417	116,119	162,561	274,567
Average debt in 1991, tDKK	572,068	727,594	500,796	359,039	180,553
Probability of transitioning to entrepreneurship in 1991	0.0058	0.0063	0.0058	0.0052	0.0044
Observations	303,431	170,632	56,578	41,103	35,118

Table 2

The impact of unlocked collateral on household debt (in DKK)

This table reports estimates from OLS regressions, where the dependent variable is level of debt measured in constant 1991 DKK. The main RHS variables are indicators corresponding to different buckets for an individual's home equity as a share of home value (ETV), measured before the reform in 1991 and these indicators interacted with an indicator for the post mortgage reform period (1992-1996). All columns include year fixed effects interacted with fixed effects for the individual's age (fixed effect for each year from 25-50), educational background (4 categories), gender, marital status, number of children and household wealth (fixed effect for the decile of household wealth). Columns (2) and (4) also include municipality-by-year 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 respectively.

	(1)	(2)	(3)	(4)
[ETV91 > 0.25] x POST	62,204***	62,046***		
	(2,224)	(2,175)		
[ETV91 > 0.25]	-322,744***	-315,122***		
	(2,208)	(2,145)		
ETV91 [0.25-0.50] x POST			33,954***	33,810***
			(2,403)	(2,399)
ETV91 [0.50-0.75] x POST			69,055***	69,035***
			(3,017)	(2,987)
ETV91 [0.75-1.00] x POST			106,523***	106,275***
			(4,831)	(4,774)
ETV91 [0.25-0.50]			-200,888***	-193,746***
			(2,378)	(2,321)
ETV91 [0.50-0.75]			-359,670***	-355,504***
			(3,176)	(3,092)
ETV91 [0.75-1.00]			-506,276***	-494,285***
			(4,077)	(4,048)
Constant	654,605***	651,304***	657,691***	654,437***
	(1,288)	(1,219)	(1,288)	(1,218)
Observations	2,708,892	2,708,892	2,708,892	2,708,892
Birth cohort X Year FE	YES	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES	YES
Municipality x Year FE	NO	YES	NO	YES

Table 3

The impact of unlocked collateral on entrepreneurship

This table reports estimates from OLS regressions, where the dependent variable is an indicator that takes a value 1 if the individual was not classified as an entrepreneur in *t-1* but is classified as an entrepreneur in year *t* . The main RHS variables are indicators corresponding to different buckets for an individual's home equity as a share of home value (ETV), measured before the reform in 1991 and these indicators interacted with an indicator for the post mortgage reform period (1992-1996). All columns include year fixed effects interacted with fixed effects for the individual's age (fixed effect for each year from 25-50), educational background (4 categories), gender, marital status, number of children and household wealth (fixed effect for the decile of household wealth). Columns (2) and (4) also include municipality-by-year fixed effects and industry-by-year 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 respectively.

	(1)	(2)	(3)	(4)
[ETV91 > 0.25] x POST	0.00065***	0.00061**		
	(0.00019)	(0.00019)		
[ETV91 > 0.25]	-0.00142***	-0.00148***		
	(0.00015)	(0.00015)		
ETV91 [0.25-0.50] x POST			0.00039	0.00034
			(0.00023)	(0.00023)
ETV91 [0.50-0.75] x POST			0.00046	0.00049
			(0.00026)	(0.00026)
ETV91 [0.75-1.00] x POST			0.00134***	0.00126***
			(0.00029)	(0.00030)
ETV91 [0.25-0.50]			-0.00117***	-0.00120***
			(0.00019)	(0.00018)
ETV91 [0.50-0.75]			-0.00201***	-0.00204***
			(0.00021)	(0.00021)
ETV91 [0.75-1.00]			-0.00119***	-0.00137***
			(0.00023)	(0.00023)
Constant	0.00586***	0.00590***	0.00587***	0.00591***
	(0.00007)	(0.00007)	(0.00007)	(0.00007)
Observations	2,708,892	2,708,892	2,708,892	2,708,892
Birth cohort X Year FE	YES	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES	YES
Industry x Year FE	NO	YES	NO	YES
Municipality x Year FE	NO	YES	NO	YES

Table 4

Entry into industries with prior experience versus no prior experience

This table reports estimates from OLS regressions, where the dependent variable is an indicator that takes a value of 1 if the individual was not classified as an entrepreneur in year t-1 but is classified as an entrepreneur in year t, entering an industry where they either had, or did not have prior experience. The main RHS variables are indicators corresponding to different buckets for an individual's home equity as a share of home value (ETV), measured before the reform in 1991 and these indicators interacted with an indicator for the post mortgage reform period (1992-1996). All columns include controls for ETV, year fixed effects interacted with fixed effects for the individual's age (fixed effect for each year from 25-50), educational background (4 categories), gender, marital status, number of children and household wealth (fixed effect for the decile of household wealth), as well as municipality-by-year fixed effects and industry-by-year 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 respectively.

	(1)	(2)	(3)	(4)
	Prior experience in entering industry		No prior experience in entering industry	
[ETV91 > 0.25] x POST	-0.00001		0.00062***	
	(0.00014)		(0.00014)	
ETV91 [0.25-0.50] x POST		0.00010		0.00024
		(0.00017)		(0.00016)
ETV91 [0.50-0.75] x POST		-0.00015		0.00064***
		(0.00019)		(0.00018)
ETV91 [0.75-1.00] x POST		-0.00003		0.00129***
		(0.00021)		(0.00021)
Constant	0.00299***	0.00299***	0.00291***	0.00291***
	(0.00005)	(0.00005)	(0.00005)	(0.00005)
Observations	2,708,892	2,708,892	2,708,892	2,708,892
Control for ETV Main effect	YES	YES	YES	YES
Birth cohort X Year FE	YES	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES
Municipality x Year FE	YES	YES	YES	YES

Table 5

Entry into industries by capital intensity and prior experience

This table reports estimates from OLS regressions, where the dependent variable is an indicator that takes a value of 1 if the individual was not classified as an entrepreneur in year t-1 but is classified as an entrepreneur in year t, entering an industry where they either had, or did not have prior experience as well as whether it was a more or less capital intensive industry. Please see Table 4 for details on controls and 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 respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Prior experience in entering industry				No prior experience in entering industry			
	More Capital Intensive Industries		Less Capital Intensive Industries		More Capital Intensive Industries		Less Capital Intensive Industries	
[ETV91 > 0.25] x POST	0.00001		-0.00001		0.00044***		0.00018	
	(0.00009)		(0.00010)		(0.00010)		(0.00010)	
ETV91 [0.25-0.50] x POST		0.00005		0.00005		0.00024*		0.00001
		(0.00011)		(0.00013)		(0.00011)		(0.00012)
ETV91 [0.50-0.75] x POST		-0.00003		-0.00011		0.00035**		0.00030*
		(0.00013)		(0.00014)		(0.00013)		(0.00013)
ETV91 [0.75-1.00] x POST		-0.00004		0.00001		0.00093***		0.00035**
		(0.00015)		(0.00015)		(0.00017)		(0.00013)
Constant	0.00120***	0.00120***	0.00178***	0.00179***	0.00140***	0.00139***	0.00152***	0.00152***
	(0.00003)	(0.00003)	(0.00004)	(0.00004)	(0.00003)	(0.00003)	(0.00004)	(0.00004)
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
Control for ETV Main effect	YES	YES	YES	YES	YES	YES	YES	YES
Birth cohort X Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipality x Year FE	YES	YES	YES	YES	YES	YES	YES	YES

Table 6

Pre-entry income rank of entrepreneurs

This table reports estimates from OLS regressions, where the dependent variable is an interaction between an indicator that takes a value of 1 if the individual is classified as being an entrant in Table 4 and their income rank in the year prior to entry. Higher income is coded as having a higher income rank, so that a positive coefficient implies that marginal entrants benefiting from the reform had higher income in the year prior to their entry. Please see Table 4 for details on controls and 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 respectively.

	(1)	(2)	(3)	(4)
	Prior experience in entering industry		No prior experience in entering industry	
[ETV91 > 0.25] x POST	-0.0044		0.0360***	
	(0.0092)		(0.0096)	
ETV91 [0.25-0.50] x POST		0.0020		0.0135
		(0.0114)		(0.0115)
ETV91 [0.50-0.75] x POST		-0.0146		0.0299*
		(0.0127)		(0.0129)
ETV91 [0.75-1.00] x POST		-0.0045		0.0846***
		(0.0143)		(0.0149)
Constant	0.1794***	0.1796***	0.1857***	0.1858***
	(0.0033)	(0.0033)	(0.0035)	(0.0035)
Observations	2,708,892	2,708,892	2,708,892	2,708,892
Control for ETV Main effect	YES	YES	YES	YES
Birth cohort X Year FE	YES	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES
Municipality x Year FE	YES	YES	YES	YES

Table 7
Entrants that survive at least 3 years from founding

This table reports estimates from OLS regressions, where the dependent variable is an indicator that takes a value of 1 if the individual classified as being an entrant in Table 4 remained an entrepreneur for at least 3 years following entry. Please see Table 4 for details on controls and 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 respectively.

	(1)	(2)	(3)	(4)
	Prior experience in entering industry		No prior experience in entering industry	
[ETV91 > 0.25] x POST	-0.00008		0.00028**	
	(0.00010)		(0.00010)	
ETV91 [0.25-0.50] x POST		-0.00011		0.00004
		(0.00013)		(0.00012)
ETV91 [0.50-0.75] x POST		-0.00013		0.00036***
		(0.00014)		(0.00013)
ETV91 [0.75-1.00] x POST		0.00002		0.00062***
		(0.00016)		(0.00015)
Constant	0.00157***	0.00157***	0.00146***	0.00146***
	(0.00003)	(0.00003)	(0.00003)	(0.00003)
Observations	2,708,892	2,708,892	2,708,892	2,708,892
Control for ETV Main effect	YES	YES	YES	YES
Birth cohort X Year FE	YES	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES
Municipality x Year FE	YES	YES	YES	YES

Table A.1

Average equity-to-value in 1991 for homeowners, based on age and year of last move

This table shows the mean equity to value in 1991, by age of individual and the year of house purchase.

Year of house purchase	\	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	0.56	0.49	0.50	0.51	0.52	0.55	0.53	0.55	0.56	0.56	0.58
1976											0.43	0.45	0.49	0.47	0.50	0.50	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	0.41	0.43	0.45	0.44	0.45	0.45	0.47	0.47	0.47	0.49	0.49	0.52	0.53	0.53
1978								0.36	0.38	0.34	0.36	0.37	0.40	0.42	0.41	0.44	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	0.35	0.37	0.39	0.38	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	0.32	0.35	0.35	0.36	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	0.32	0.30	0.31	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	0.30	0.33	0.32	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	0.24	0.26	0.28	0.29	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	0.24	0.22	0.25	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	0.18	0.19	0.17	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	0.16	0.16	0.15	0.14	0.14	0.15	0.16	0.16	0.17	0.19	0.18	0.20	0.21
1987	0.14	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.13	0.13	0.12	0.14	0.15	0.13	0.14	0.13	0.15	0.15	0.15	0.18	0.16	0.18	0.20	0.22	0.21	0.20
1988	0.12	0.13	0.11	0.12	0.12	0.12	0.13	0.14	0.12	0.13	0.14	0.13	0.15	0.15	0.15	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	0.14	0.15	0.18	0.19	0.17	0.18	0.21	0.19	0.20	0.21	0.21	0.23
1990	0.15	0.14	0.15	0.15	0.14	0.16	0.15	0.15	0.16	0.17	0.16	0.17	0.16	0.16	0.17	0.19	0.19	0.19	0.19	0.20	0.22	0.21	0.22	0.26	0.24	0.26

Table A.2

Share of households with large increases in debt

This table reports estimates from OLS regressions, where the dependent variable takes a value of 1 if the increase in debt as noted in Table 2 is greater than the threshold values corresponding to the columns. Please see Table 2 for details on controls and 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 respectively.

	(1)	(2)	(3)	(4)
	Increase in Debt is Greater than 150,000 DKK		Increase in Debt is Greater than 500,000 DKK	
[ETV91 > 0.25] x POST	0.06553***		0.01699***	
	(0.00114)		(0.00057)	
[ETV91 > 0.25]	0.00272***		0.00011	
	(0.00075)		(0.00035)	
ETV91 [0.25-0.50] x POST		0.03675***		0.01060***
		(0.00137)		(0.00060)
ETV91 [0.50-0.75] x POST		0.08035***		0.01795***
		(0.00167)		(0.00073)
ETV91 [0.75-1.00] x POST		0.10208***		0.02776***
		(0.00196)		(0.00115)
ETV91 [0.25-0.50]		0.00641***		-0.00362***
		(0.00090)		(0.00036)
ETV91 [0.50-0.75]		-0.00673***		-0.00606***
		(0.00107)		(0.00043)
ETV91 [0.75-1.00]		0.00666***		0.01410***
		(0.00129)		(0.00073)
Constant	0.09288***	0.09254***	0.01776***	0.01757***
	(0.00038)	(0.00038)	(0.00017)	(0.00018)
Observations	2,708,892	2,708,892	2,708,892	2,708,892
Birth cohort X Year FE	YES	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES	YES
Municipality x Year FE	YES	YES	YES	YES

Table A.3

Increase in household debt using a continuous measure of ETV

This table documents the robustness of results of reported in Table 2 to imposing a linear relationship between equity to value and changes in household debt. Please see Table 2 for details on controls and 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 respectively.

	(1)	(2)
ETV91 in 1991 x POST	127,636***	127,305***
	(4,548)	(4,456)
ETV in 1991	-626,560***	-613,375***
	(4,059)	(3,983)
Constant	689,636***	685,862***
	(1,404)	(1,333)
Observations	2,708,892	2,708,892
Birth cohort X Year FE	YES	YES
Individual covariates x Year FE	YES	YES
Municipality x Year FE	NO	YES

Table A.4

Selection into entrepreneurship using a continuous measure of ETV

This table documents the robustness of results of reported in Tables 3 and 4 to imposing a linear relationship between equity to value and changes in household debt. Please see Tables 3 and 4 for details on controls and 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 respectively.

	(1)	(2)	(3)
	All entrants	Prior experience in entering industry	No prior experience in entering industry
ETV91 in 1991 x POST	0.00131***	-0.00007	0.00138***
	(0.00032)	(0.00023)	(0.00023)
ETV in 1991	-0.00216***	-0.00062***	-0.00154***
	(0.00025)	(0.00018)	(0.00017)
Constant	0.00582***	0.00296***	0.00285***
	(0.00008)	(0.00005)	(0.00005)
Observations	2,708,892	2,708,892	2,708,892
Birth cohort X Year FE	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES
Industry x Year FE	YES	YES	YES
Municipality x Year FE	YES	YES	YES

Table A.5

Entry regressions with individual fixed effects

This table shows that the results noted in Tables 3 and 4 with the additional inclusion of 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 respectively.

| | Table 3 with individual fixed effects | | Table 4 with individual fixed effects | | | |
| | | | Prior experience in entering industry | | No prior experience in entering industry | |
	(1)	(2)	(3)	(4)	(5)	(6)
ETV91 in 1991 x POST	0.00060**		-0.00001		0.00062***	
	(0.00019)		(0.00014)		(0.00014)	
ETV91 [0.25-0.50] x POST		0.00033		0.00009		0.00024
		(0.00023)		(0.00017)		(0.00016)
ETV91 [0.50-0.75] x POST		0.00046		-0.00016		0.00062***
		(0.00026)		(0.00019)		(0.00018)
ETV91 [0.75-1.00] x POST		0.00126***		-0.00004		0.00131***
		(0.00029)		(0.00021)		(0.00021)
Observations	2,708,873	2,708,873	2,708,873	2,708,873	2,708,873	2,708,873
Birth cohort X Year FE	YES	YES	YES	YES	YES	YES
Individual covariates x Year FE	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES
Municipality x Year FE	YES	YES	YES	YES	YES	YES
Individual Fixed Effects	YES	YES	YES	YES	YES	YES

Table A.6

Placebo entry using constrained sample

This table reports estimates from OLS regressions, where the dependent variable is an indicator that takes a value 1 if the individual was not classified as an entrepreneur in t-1 but is classified as an entrepreneur in year t. The sample is restricted only to those who did not benefit from the reform (i.e., ETV<0.25) and is divided into those with ETV<0.125 (the placebo control group) and those with ETV between 0.125 and 0.25 (the placebo treatment group) . The main RHS variables include an indicator for an individual being in the placebo treatment group and this indicator interacted an indicator for the post mortgage reform period (1992-1996). All columns include year fixed effects interacted with fixed effects for the individual's age (fixed effect for each year from 25-50). Column (2) further includes year fixed effects interacted with fixed effects for educational background (4 categories), gender, marital status, number of children and household wealth (fixed effect for the decile of household wealth). Column (3) further includes municipality-by-year fixed effects and industry-by-year 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 respectively.

	(1)	(2)	(3)
[ETV91 0.125-0.25]	-0.00093***	-0.00092***	-0.00091***
	(0.00024)	(0.00024)	(0.00024)
[ETV91 0.125-0.25] x POST	0.00007	0.00008	0.00007
	(0.00030)	(0.00030)	(0.00030)
Constant	0.00610***	0.00610***	0.00610***
	(0.00008)	(0.00008)	(0.00008)
Observations	1,384,104	1,384,104	1,384,104
Birth cohort X Year FE	YES	YES	YES
Individual covariates x Year FE	NO	YES	YES
Industry x Year FE	NO	NO	YES
Municipality x Year FE	NO	NO	YES