

# Homeownership and Entrepreneurship

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## Abstract

We study the link between homeownership and entrepreneurship by exploiting the longitudinal dimension of the British Household Panel Survey (BHPS) and constructing a detailed monthly-spell dataset that tracks individuals' job history and tenure choice, coupled with other time-varying characteristics. Our fixed-effects estimates show that purchasing a house reduces the likelihood of starting a business by 20-25%. This result is driven by homeowners with mortgages and persists for several years after entering homeownership. The negative link can be rationalized by portfolio considerations: leveraged housing investments crowd out entrepreneurial investments. Alternative explanations based on credit constraints find little support in our data.

JEL Classifications: L26, D14, G11, R21

Keywords: Entrepreneurship; homeownership; panel estimation

## 1. Introduction

Over the past decades, the vast majority of developed countries have adopted tax policies that subsidize homeownership. This set of incentives includes tax relief for mortgage interest payments, low or no taxes on imputed rents, non-taxation of capital gains on principally owner-occupied dwellings, and subsidies to low-income families to reduce the financing cost of homeownership. These policies can be extremely costly. For example, the mortgage interest deduction in the United States represents the second largest US tax expenditure, estimated to be \$104.5 billion in foregone tax revenue for the fiscal year 2011. In the United Kingdom, the „Mortgage Interest Relief at Source“ (MIRAS) was abolished in 2000. Yet the UK still heavily subsidizes homeownership: a landlord’s rental income is typically taxed at a marginal rate of 40-50 percent, whereas the equivalent „imputed rental income“ of owner-occupiers is tax free.<sup>1</sup> While tax subsidies to homeowners are expensive, they may be justified on economic grounds if the social benefits associated with homeownership are large.<sup>2</sup>

In this paper, we highlight a previously undocumented *negative* externality of homeownership: we show that purchasing a home reduces the likelihood of starting an entrepreneurial activity by 20-25%. The effect is larger and more significantly estimated when focussing on entrepreneurs who employ dependent workers or on those who hold managerial and professional positions. This indicates that homeownership is negatively linked to „genuine entrepreneurship“ – and thereby to firm creation, innovation and ultimately economic growth<sup>3</sup> – and not to „self-employment out of necessity“ or as a „last resort option“ (Alba-Ramirez, 1994; and Martinez-Granado 2002).

We argue that this finding can be rationalized by the fact that homeowners typically have to overinvest in housing (Brueckner, 1997; Flavin and Yamashita, 2002) and therefore cannot adequately diversify their portfolio. As a consequence, individuals choose *not* to start-up their own business venture at the same time as becoming homeowners since this would imply taking on significant additional risk<sup>4</sup>. Stated differently; investments in homeownership crowd out entrepreneurial engagement.

In order to explore these issues, we exploit the longitudinal dimension of the British Household Panel Survey (BHPS) covering the period between 1991 and 2008. The structure of the BHPS allows us to construct a detailed monthly-spell dataset that tracks individuals’ job histories

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<sup>1</sup> Landlords can also deduct some expenses. Nevertheless, the non-taxation of imputed rental income represents a substantial subsidy for owner-occupiers in the UK. In fact very few countries tax imputed rental income of owner-occupiers, thus implicitly subsidizing homeownership.

<sup>2</sup> It is however not clear whether tax subsidies *per se* increase homeownership attainment. Hilber and Turner (2010) show that the US mortgage interest deduction has no overall positive effect on homeownership.

<sup>3</sup> See Acs *et al.* (2004) and Michelacci (2003) for a formal analysis of the role of entrepreneurs in endogenous growth models, and Acs and Audretsch (2004) for a review of the empirical literature.

<sup>4</sup> See Knight (1921), Kanbur (1979) and Kihlstrom and Laffont (1979) for a discussion of the link between entrepreneurial risk and uncertainty, risk aversion and entrepreneurship.

and tenure choices, coupled with information on time-varying background characteristics. We exploit this data to estimate regressions that include individual fixed-effects to control for individual time-fixed unobserved characteristics, and isolate the precise timing of individuals' transitions into homeownership and entrepreneurial jobs.

Our cross-sectional regressions that only control for individual observable attributes and geographical factors identify a positive link between homeownership and various measures of self-employment. However, once we use fixed-effects to control for time-invariant unobservables – such as innate entrepreneurial spirit, risk tolerance or persistent wealth – we find that becoming a homeowner significantly *reduces* the propensity of becoming an entrepreneur. Moreover, we show that this negative link is much stronger and more precisely estimated when focussing on homeowners with mortgages, and that it loses its significance once we include the mortgage loan-to-value (LTV) ratio as an explanatory variable in our regressions. This implies that leverage considerations may exacerbate portfolio distortions due to undiversified risk of investment in housing, and may sharpen the trade-off between becoming a homeowner and starting a business. Consistent with this interpretation, we also find that the negative link between homeownership and entrepreneurship remains strong and significant for 24 to 42 months after purchasing a house – when leverage is highest – and then wanes out. However, we find no evidence that the link between homeownership and entrepreneurship turns significant and positive as more time goes by.

In order to provide further evidence in support of our explanation based on portfolio considerations, we directly assess whether homeowners shy away from more risky entrepreneurial ventures. To test this proposition, we collect data on company profits at a detailed sectoral level, as well as information on capital spending per worker, and construct a series of proxies for the riskiness of entrepreneurial ventures based on profit variability and cost sunkness. Using this information, we show that the negative link between homeownership and entrepreneurship predominantly holds for individuals operating in risky sectors, but not for entrepreneurs working in industries with lower profit variability and smaller sunk costs. This lends strong support to our explanation based on overinvestment in housing and portfolio distortions.

Nevertheless, these results could also be consistent with a theory based on credit constraints, whereby leveraged homebuyers are prevented from taking on additional credit to start a business. The role of financing constraints in entrepreneurial start-ups has been widely investigated in the literature (e.g. Evans and Jovanovic, 1989; Holtz-Eakin et al., 1994; and Blanchflower and Oswald, 1998). However, for this explanation to hold true, we would need to be able to detect a positive relationship between house price increases and entry into entrepreneurship. This is because as home values increase, LTV ratios are pushed down and more equity is built into an individual's

position in relation to his/her own housing investment. This could in turn be used as collateral to borrow additional funds and relax credit constraints in relation to a business start-up.

In order to directly test this alternative explanation, we use information on local house prices at a detailed level of aggregation to study the dynamics of local housing values and measure capital gains accrued on owner-occupied housing. Our results clearly show that local house price variation and cumulative gains have no explanatory power in our analysis and cannot account for or reverse the negative link between homeownership and entrepreneurship. This finding is very similar to Hurst and Lusardi (2004), and casts doubt on the view that credit constraints play an important role in explaining our findings.

We believe our findings are of general interest and contribute to three major strands of the economics literature, namely: the external effects of homeownership on socio-economic and labour market outcomes; the effects of homeownership on portfolio choices; and the role of credit constraints in entrepreneurship.

In relation to the first topic, a large number of studies have documented positive externalities associated with homeownership, including higher investments in local social capital (DiPasquale and Glaeser, 1999; Hoff and Sen, 2005; and Hilber, 2010), better control of local governments (e.g., Fischel, 2001; Dehring *et al.*, 2008), higher attention towards environmental issues and children's education (Dietz and Haurin, 2003), as well as school quality investments (Hilber and Mayer, 2009). However, as emphasized by Oswald (1996, 1998, 1999), homeownership might generate negative externalities in relation to labour market outcomes. This is because homeowners are less mobile than renters due to significant transaction costs (Haurin and Gill, 2002), and thus less likely to relocate to find an alternative occupation if they lose their job. Oswald's seminal articles remain highly controversial and have initiated a large body of literature that studies the impact of housing tenure on both unemployment incidence and unemployment duration. In two recent articles, Munch *et al.* (2006) and Battu *et al.* (2008) use duration models applied to micro-level data for Denmark and the UK respectively, and find no evidence that homeowners are more likely to become unemployed or have longer unemployment spells. While this set of findings is reassuring, the recent financial crisis (2007-2009) and housing bust that hit a number of OECD countries (in particular, the US) has reignited the debate on the benefits and detriments of homeownership as well as the economic costs of excessive leverage and negative equity. Ferreira *et al.* (2010 and 2011) suggest that owners in negative-equity are significantly less mobile, and argue that this could have significant implications for the design of public policies. Our results highlight an important and previously neglected channel whereby housing policies could perversely affect employment outcomes.

Entrepreneurship is not only a labour market decision: starting an entrepreneurial venture could be conceptualized as an investment choice and analyzed in the context of portfolio decisions. From this perspective, housing plays a very prominent – although distorting – role. Henderson and Ioannides (1983) were the first to formulate the proposition that owner-occupiers overinvest in housing, while Brueckner (1997) showed that when the investment constraint induced by owner-occupied housing is binding, homeowners cannot adequately diversify their portfolio. Flavin and Yamashita (2002) examine a household’s portfolio problem when housing matters both as consumption and investment. They find that the optimal consumption level might exceed the optimal investment quantity. More recently, Cocco (2005) and Chetty and Szeidl (2010) show that homeownership – and in particular a large mortgage – significantly reduces a household’s exposure to risky assets such as stocks. Our results are fully consistent with the logic presented in this strand of literature: homebuyers engage in a relatively illiquid and large investment – with a hard-to-hedge risk – and this leaves less room for investment in risky entrepreneurial ventures.

Finally, our work contributes to the large empirical literature that has investigated the role played by credit constraints in the decision to become an entrepreneur (Evans and Jovanovic, 1989; Holtz-Eakin et al., 1994; Blanchflower and Oswald, 1998; Michelacci and Silva, 2007; and Fairlie and Krashinsky, 2011). In particular, two related studies explore the role of housing collateral (Black *et al.*, 1996) and capital market constraints (de Meza and Webb, 1999) for business formation. Black *et al.* (1996) point out that bank loans are often secured on an entrepreneur’s house, and go on to show using UK macro-data that a 10 percent increase in the value of unreleased net housing equity increases the number of new VAT registrations by about 5 percent. Although their findings suggest that aggregate wealth boosts the number of start-ups, they do not directly investigate the link between homeownership and entrepreneurship. In a related study, de Meza and Webb (1999) argue that the usual interpretation of this finding is that liquidity constraints play a major role in determining who sets up a business, and that capital-market failure holds back enterprise.<sup>5</sup> A remarkably dissenting view in this field is the work by Hurst and Lusardi (2004), who suggest that the relation between wealth and entrepreneurship is only significant at the very top of the wealth distribution. Moreover, they find that households living in areas which experience strong house price appreciation are *not* significantly more likely to start an

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<sup>5</sup> The view that homeownership helps entrepreneurship is popular among policymakers and the media. The US Department of Housing and Development stated that “through homeownership a family (...) invests in an asset that can (...) provide the capital needed to start a small business” (HUD, 1995). Similar claims have been put forward in the UK policy environment where it has been suggested that recent economic developments might hamper entrepreneurship since the requirement “to provide collateral may prove a problem for individuals (...) whose levels of asset ownership – e.g. a house – is low” (BIS, 2010). Finally, the media has amplified the resonance of this debate by arguing that politicians designing housing policies should bear in mind that “homeownership is a key factor in being able to finance (...) a small-business, expand an existing business, or keep a business alive” (USA Today, 2011).

entrepreneurial venture. This result, which we fully replicate in our paper using UK data, questions the relevance of credit constraints in determining entry into entrepreneurship.

To the best of our knowledge, our study is the first to document that homeownership crowds out entrepreneurship. The only two other papers that investigate the link between homeownership and entrepreneurship using micro-level data are Fairlie (2010) and Wang (2011). Fairlie (2010) presents cross-sectional evidence for the US suggesting that homeownership has small positive effects on business creation. Consistently, we find similar positive effects when exploring cross-sectional variations in our data. However, the effect of homeownership on entrepreneurship turns significantly negative once we exploit the longitudinal dimension of our data to control for time-invariant unobservable characteristics. Wang (2011) investigates the effects of a policy that allowed Chinese public-sector employees renting state-owned housing to buy their properties at subsidized prices. The author shows that the program increased transition into self-employment and argues that part of this effect can be explained by the relaxation of credit constraints. While Wang's results are in contrast with ours, the specific institutional context and workings of the policy can account for these differences. First, as noted by the author (footnote 9), China's financial sector and lending from banking institutions are far less developed than in countries such as the UK and the US, potentially exacerbating the importance of credit constraints. Second, most subsidized home-buyers paid less than 15 percent of the market value of their home, bought their property without a mortgage, and sold it at market prices soon after purchase. This suggests that they realized large and immediate pecuniary windfalls, which is uncommon when analyzing more regular routes into homeownership. Finally, the author suggests that her results can also be explained by the fact that the policy unbundled employment and tenure decisions. This is in marked contrast with our proposed mechanism where individuals face a trade-off between homeownership and entrepreneurial investments, with the former crowding out the latter.

We believe our results carry profound implications for the role of housing markets and policies in shaping labour market outcomes and economic performance. Our findings imply that homeownership subsidies cannot be justified on the grounds that they encourage entrepreneurship and thus stimulate economic activity and growth. More broadly, our evidence is particularly relevant given the ongoing discussions about the long-lasting effects of the recent financial crisis which originated in housing markets and was arguably exacerbated by policies aimed at promoting homeownership by making mortgaging more affordable.

The rest of the paper is structured as follows. In Section 2 we describe how we use the BHPS to construct a monthly-spell panel. Section 3 discusses our main findings on the link between homeownership and entrepreneurship. Section 4 explores different mechanisms and explanations for our key results. Finally, we provide concluding remarks in Section 5.

## 2. Data and descriptive statistics

### 2.1 A monthly panel dataset using the BHPS

The BHPS is a long panel dataset covering the period between 1991 and 2008, and providing detailed information on a household's tenure choice and characteristics, as well as on an individual's current occupation, job-history between interviews, personal characteristics, income and financial situation/perceptions. The first wave of the panel consists of approximately 5,500 households and more than 10,000 individuals living in the UK (booster samples were included in 1999 and 2001 to add more individuals from Scotland, Wales and Northern Ireland). One of the significant advantages of the BHPS is that it is very successful in following the same individuals over time, even when they move residence or go on to form new households (e.g. the children of the original BHPS-families or divorcees).

At the time of the interview (normally in September; in exceptional cases in subsequent months), respondents are asked to describe their current labour force status. If they are working, detailed information about their occupation is collected. Survey respondents are also asked whether their labour force status has changed since their last interview. If the answer is positive, a set of detailed questions is asked about all the labour force and occupational spells occurred between the interview taking place and September of the previous year.<sup>6</sup>

The way in which the BHPS is structured makes it possible that some inconsistencies arise in the description of the same labour force spell provided by the same person in two different waves. Many authors have discussed the complicated task of reconstructing detailed monthly spells from the BHPS (Upward, 1999; Halpin, 2000; Paull, 2002; Mare, 2006). To tackle this issue, we follow the principle that information recorded closest to the date of the beginning of the spell is the most accurate. A similar approach is used in Upward (1999) and Battu *et al.* (2008). Our method works sequentially by appending subsequent waves of data. It thus allows solving inconsistencies that arise within and between waves. In Appendix A, we provide a detailed description of our procedure.

In order to identify the effect of homeownership on entrepreneurship, we also need information about individuals' tenure choices with some specific attention to the timing of this event. A particular concern is reverse causality, whereby individuals may first become successful entrepreneurs and then purchase a property by tapping into their cumulated wealth. To deal with this issue, we first gather information about a respondent's present tenure status. The possible categories are: homeowner with mortgage, homeowner without mortgage, private tenant, and

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<sup>6</sup> In their first wave respondents are asked whether their labour forces status has changed since 1<sup>st</sup> of September of the previous year, and – if so – precise information about their job history is collected. In this way, the BHPS covers every month of the labour history of the respondents since one year before their first interview to present.

social tenant. There are other rare options, such as living in an accommodation paid by the employer, which we do not consider in our analysis. This exclusion does not affect our findings. Second, we use the date in which the respondents say they moved to their present address to identify the timing of changes in an individual's tenure status. If the respondent changed his or her tenure status from one wave to another *and* there is a moving date, we take this date as the transition date. Approximately 93% of the individuals have a moving date when making a transition into/out of homeownership. If the respondent changes his or her tenure status but there is no moving date, the transition date is imputed as the date of the current interview.<sup>7</sup>

Finally, other controls (such as education level, age, marital status and number of children; see next section for more details) are treated as constant between one wave and the other. Changes are assumed to take place at the date of the annual interview.

In terms of sampling, we begin with an initial set including all respondents who gave a full interview in Wave 1 or one of the following waves, and follow them until they exit the survey for the first time, even if they come back at a later stage. This restriction is imposed because we need to be able to construct a continuous account of an individual's labour force status for every month combined with precise information on his or her tenure status. It is not possible to reconstruct in-between labour market spells and tenure choices for people who skip an interview. In Wave 1 (1991) we have 9,892 individuals, whereas in Wave 18 (2008) we have 6,309 individuals, of which 3,642 are from the initial sample interviewed in Wave 1. Observations decrease gradually, reflecting aging and attrition in the original sample. On the other hand, children and spouses of original members join the dataset, partially counterbalancing the decreasing tendency.

In our core analysis, we focus on heads of households in their prime working age (between 20 and 55) and consider only their employment spells (either as workers or self-employed). By focusing on these individuals, we limit the importance of issues related to labor market participation – since in our data „head of household“ conventionally refers to the individual within the household who manages the financial aspects and is considered the main economic actor. Moreover, we restrict our attention to the choice between entrepreneurship and dependent employment. However, as we show in robustness checks, including unemployment and other labour market status spells in our analysis does not alter our results. Finally, we only focus on individuals living in England, because for this group we can match precise information about prevailing local economic and housing market conditions. We will exploit this when trying to disentangle the mechanisms that explain our findings. Our main results are identical if we include

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<sup>7</sup> It is possible to change tenure status without changing address. In the UK, for instance, the “right-to-buy” program allows social tenants to buy their house or flat from the local authority (van Ham *et al.*, 2010). Similarly, individuals could buy from their current private landlord. However, this does not seem widespread.

individuals living in Wales, Scotland and Northern Ireland. These findings are not tabulated for space reasons, but are available upon request. After implementing these restrictions, our sample includes approximately 360,000 observations and 5200 individuals.

The richness and detail of the dataset is a crucial and novel element of our analysis. Most panel-type studies of entrepreneurship (e.g. Hurst and Lusardi, 2004; Disney and Gathergood, 2009) rely on annual observations. This neglects employment and self-employment spells with duration below one year. More importantly, annual data do not allow pinning down the precise timing of individuals' transitions into homeownership and entrepreneurial jobs. Since we are interested in identifying the relation running from changes in housing tenure to transitions into entrepreneurial occupations, we need a detailed and consistent set of monthly information on individuals' job and tenure spells.

## 2.2 *Descriptive statistics*

Descriptive statistics for the BHPS monthly-spell dataset are presented in Table 1. Panel A focuses on the main variables of interest, namely individual's occupational choice and housing tenure status. Panel B describes a set of time-varying background characteristics.

Panel A presents descriptive statistics for three different proxies for entrepreneurial occupations. To begin with, we identify individuals who are self-employed and label this category as „entrepreneur: all“. However, previous research suggests that some workers might choose self-employment out of unemployment or lack of alternative opportunities. To address this issue, we create two further measures of self-employment, which are meant to narrow down our definition in ways that allow us to capture more properly defined entrepreneurial jobs. First, we consider only self-employed workers who employ other people, irrespective of their number. We label these as „entrepreneur: dependent“. Next, using the socio-economic classification of jobs provided by the BHPS (SOC2000 at the 1-digit level), we identify self-employed who are “managers and senior officials”, or work in “professional occupations”, or identify themselves as “associate professional and technical occupations”. We label this group „entrepreneurs: manager“. Whereas the first definition is meant to capture entrepreneurs who create jobs, the second definition aims at identifying entrepreneurs with higher levels of human capital.

Panel A reveals that the percentages of entrepreneurs are similar to those one would gather from the annual BHPS, irrespective of the definition we use. On average 14.4% of individuals are self-employed (14.2% in the annual data), while the shares of „entrepreneur: dependent“ and „entrepreneur: manager“ are smaller at 4.7% and 7%, respectively (4.5% and 6.9% in the annual data). Similarly, the fraction of homeowners in the monthly-spell data (81%) is close to the annual-data figure (78%). Note that around 71% of the observations involve homeownership with

a mortgage, whereas only 9.6% refer to owners with no mortgage. Finally, 8.7% of the observations are from individuals renting a private property. 8.5% represent public renters.

A comparison of the control variables in Panel B with the corresponding values in the yearly dataset also shows little evidence of any significant differences. In particular, individuals are approximately of the same age (39.4 vs. 39.1), the incidence of males is very closely matched (0.788 vs. 0.778), and both individual and household total incomes in the year prior to the survey are in the same range (£20,990 /£31,728 vs. £21,044/£31,701). This broad consistency is very reassuring since it suggests that our analysis of the link between homeownership and entrepreneurship using monthly-spell data is representative of the BHPS sample – and thus of the UK overall – and that our data construction did not affect the main properties of the data.

Furthermore, the percentage of self-employment that we report (14.4%) is consistent with Blanchflower and Shadforth (2007), who use several years of quarterly data from the (cross-sectional) Labour Force Survey. They document that self-employment in the UK has stayed between 12% and 15% in the 1991 to 2007 period. Similarly, our percentage of homeowners (81%) is close to the one reported by Battu *et al.* (2008) (79%), and our shares of private and public renters (at 8.7% and 8.4%) are comparable to theirs (at 7.7% and 9.1%).<sup>8</sup> However, the incidence of males in our study is substantially higher (79% vs. 44%), which is explained by the fact that we focus on heads of household only.

Before presenting our findings, we discuss the incidence of transitions into and out of homeownership and in and out of entrepreneurial job spells since this information is relevant for our fixed-effects identification. Summary statistics are presented in Appendix Table 1. Overall, around 18% of all individuals make at least one homeownership transition (for example „rent“ to „own“) and 5.4% at least two transitions (e.g. „rent“ to „own“ to „rent“ again). The corresponding numbers for the various measures of entrepreneurship vary between 6 and 16% (one transition), and 3 and 8% (more than one transition). The share of people transiting into and out of entrepreneurial spells with dependent workers or in managerial and professional positions is smaller than for the self-employed. This suggests that these two definitions capture the more stable jobs and thus represent truly entrepreneurial spells. Finally, the fraction of workers with at least one tenure transition *and* one entrepreneurial transition varies between 2% and 4.5%. These figures are higher – between 3% and 6.5% – when considering transitions in and out of homeownership with a mortgage.

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<sup>8</sup> These small difference are most likely explained by the fact that our analysis stretches up 2008 (whereas they stop at 2003), and that we include in our sample individuals whose first interview was not in Wave 1 (whereas they focus their analysis on the „original“ members of the survey).

Note that we also investigated the characteristics of individuals who transit into and out of homeownership and entrepreneurship. Relative to those who become homeowners without a mortgage, individuals who use a loan to purchase their property are younger (30.4 vs. 37.4 years of age), less likely to have children (62.3% vs. 67.7%), and less affluent (£13,547 vs. £16,105). Individuals who become entrepreneurs with dependent workers or are self-employed in managerial and professional occupations are slightly older (at 35.4 and 34.5, respectively) than individuals who become self-employed (33.3), and substantially better off in terms of prior income (£17,539 and £17,373 vs. £14,470). These patterns are not unexpected given the possibility that some self-employment spells represent last-resort choices. However, we do not detect any clear pattern in terms of age, family arrangements and income for people transiting out of homeownership and entrepreneurship. This suggests that these movements cannot be easily explained by demographic factors, and that other individual specific considerations might be taking place in ways that simultaneously affect tenure status and entrepreneurship. We will return to these issues in the next section.

### 3. The negative link between homeownership and entrepreneurship

#### 3.1 Main finding

Our first set of results is presented in Table 2. The three different panels (A, B, and C) refer to our three different definitions of entrepreneurs („entrepreneur: all“, „entrepreneur: dependent“, and „entrepreneur: manager“). We estimate the following linear-probability model:

$$Entrep_{ilt} = \alpha_i + \beta Home_{ilt} + X'_{it}\gamma + \varphi_l + \epsilon_{ilt} \quad (1)$$

where the dependent variable  $Entrep_{ilt}$  is one of the three binary outcomes proxying for entrepreneurial jobs, and the explanatory variable of interest is an individual's housing tenure status  $Home_{ilt}$ . The subscript  $ilt$  identifies individual  $i$  living in location  $l$  at time  $t$ .  $X'_{it}$  is the set of time-varying controls discussed above and described in Table 1, while  $\varphi_l$  represents location time-fixed effects, such as persistent geographical disparities in labour and housing markets and differences in local political and institutional factors. Finally,  $\alpha_i$  captures unobserved individual factors – such as ambition and risk tolerance – which could simultaneously determine occupational choice and tenure status. The error-term  $\epsilon_{ilt}$  is assumed to be uncorrelated with all the right hand side variables, although we allow for correlation in residual shocks across individuals within locations and cluster standard errors at the Local Authority (LA) level. LAs are local

constituencies empowered to exercise planning functions, and can be thought of as self-contained housing markets from a regulatory point of view. England consists of 354 LAs.<sup>9</sup>

Columns (1) and (2) of Table 2 present simple cross-sectional estimates (OLS) of Equation (1). In Column (1), we append year-of-interview and month-of-interview effects, as well as dummies for the sector of employment (using the SIC92 classification at 1-digit level), while in Column (2) we further include the controls detailed in Table 1, as well as LA dummies. The two specifications indicate a positive and significant association between homeownership and entrepreneurship. Although the estimated coefficients are attenuated when adding individual controls and LA effects, the implied effects remain sizable and highly significant: homeownership increases the probability of being an entrepreneur with dependent workers or in a managerial and professional occupation by approximately 30%. Excluding potentially problematic controls, such as sector of occupation or individual and household income in the year prior to the survey, does not affect our findings.

However, cross-sectional regressions cannot control for individuals' unobservables –  $\alpha_i$  in Equation (1). In order to partial out these unobserved factors, we estimate various fixed-effect models, which are presented in Columns (3) to (7) of Table 1. In stark contrast to the OLS regressions, we find that once we control for individual fixed effects homeownership becomes negatively associated with entrepreneurship. While this negative effect is not significant for all self-employed, it is significant for entrepreneurs with dependent workers and for entrepreneurs in a managerial/professional occupation.

To assess the robustness of our findings to time-varying individual and household characteristics and to local unobservable factors, in Column (4) we add to the fixed-effect models the control variables detailed in Table 1, as well as LA dummies. The set of controls includes both individual and household total income in the year prior to the survey (in logs). Conditional on individual fixed-effects, these variables capture changes in the financial situation of an individual and his/her household with respect to the previous year, and therefore act as good proxies for changes in an individual's wealth. This is a very important set of controls to include in our analysis given the evidence on the importance of wealth in the decision to become an entrepreneur (Evans and Jovanovic, 1989; Holtz-Eakin et al. 1994; Blanchflower and Oswald, 1998). Finally, in Column (5) we retain the set of controls included in Column (4), but we drop LA dummies since only 30% of the individuals change their place of residence over the period of our analysis (for immobile individuals local effects are absorbed by the individual fixed-effects). Results in both Columns (4) and (5) confirm the intuition gathered from Column (3): there is a significant

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<sup>9</sup> Note that we experimented with the inclusion of Travel-to-Work Area (TTWA) effects and with clustering at this level of aggregation and came to similar conclusions. TTWAs are 243 functional areas drawn by the Office for National Statistics to identify self-contained local labour markets. We also experimented with two-way clustering at the individual and LA level, which also did not affect the statistical significance of our findings.

negative association between homeownership and the probability of self-employment, and this effect is larger when focussing on more stringent definitions of entrepreneurship, namely self-employed workers with dependent workers and in managerial/professional occupations. The estimates represent sizable effects: given the mean probability of being an entrepreneur in these two categories, becoming a homeowner reduces the chances of starting-up a business by 20-25%.

The results presented so far suggest that transiting *into* homeownership significantly reduces an individual's chances of becoming an entrepreneur. However, as discussed above, a number of individuals transits *out of* homeownership and their background characteristics suggest that this mobility cannot be easily explained by family matters and demographic factors, such as aging. Hence, part of our results might be driven by individuals who sell their property in order to „cash in“ (extract equity from their home), gather enough liquidity to undo underlying credit constraints and become entrepreneurs. To directly address this concern, in Columns (6) and (7) of Table 2, we focus on individuals' spells that correspond to transitions into and out of homeownership, respectively. More precisely, in Column (6), we follow individuals who start off as renters and then become homeowners until eventually switching back to renting (plus individuals who start off as owners and stay as such throughout the period). In Column (7), by contrast, we track individuals who finish as renters after having been homeowners (plus individuals who start off as renters and do not change tenure throughout the period), and exclude any renting spell that took place before homeownership. Our findings suggest that the estimated negative impact of homeownership only comes from individuals who become homeowners. The estimated effect is larger and more precisely estimated than before. Conversely, the link between tenure status and entrepreneurship for individuals switching out of homeownership is estimated to be small, inconsistently signed and insignificant.

In conclusion, the evidence gathered so far clearly shows that individuals who become homeowners are significantly less likely to become entrepreneurs. The estimated effect is not only significant, but also sizable: in our baseline regressions reported in Column (5) of Table 2, becoming a homeowner reduces the probability of becoming an entrepreneur by approximately 25%. One concern is that our fixed-effects identification strategy partials out individual, family and location time-fixed unobservables, but cannot control for time-varying unobserved factors. Adding time-varying individual and household level controls mitigates this problem. In particular, we can control for income, number of children and marital status, which have been shown to be strong determinants of homeownership (Linneman and Wachter, 1998; Hilber, 2007). Moreover, other plausible time-varying unobserved factors – such as winning the lottery or receiving an inheritance – would bias our results towards finding a positive link between homeownership and entrepreneurship, since wealth is positively associated to both purchasing a home and becoming an

entrepreneur (Blanchflower and Oswald, 1998). Nevertheless, since these are serious concerns, we next subject our findings to a large number of robustness checks.

### 3.2 *Robustness checks*

In this section, we assess the robustness of our results along a number of dimensions related to individuals' labour market experiences, geographical location and sectoral considerations. Our findings are presented in Appendix Table 2 for entrepreneurs with dependent workers only. We focus on this definition since we believe this better captures genuinely entrepreneurial spells. Results for the other definitions are similar and available upon request.

To begin with, we check that our results are not driven by short spells of employment, i.e. self-employment and employment experiences lasting less than 12 months. The concern is that we might be misrepresenting some self-employment spells as entrepreneurial even though they simply capture stop-gap jobs.<sup>10</sup> Column (1) reveals that excluding short employment spells does not affect our estimates, supporting the intuition that homeownership discourages the transition into entrepreneurial occupations, and not into occasional self-employment. Similarly, as shown in Column (2), including in our data individuals' unemployment spells (in addition to employment and self-employment spells) does not change our findings.

Next, in Columns (3) to (6), we assess whether our results may be driven by residential location and mobility of workers upon becoming homeowners. One concern is that individuals who choose to purchase a house might leave urban areas and that this might affect their chances of becoming entrepreneurs. Previous evidence shows that more properties are rented as opposed to owner-occupied in riskier urban centres (Hilber, 2005), and that more entrepreneurs tend to cluster into denser cities because of agglomeration and localization economies (Glaeser and Kerr, 2009, and Glaeser, 2009). To address this concern, in Column (3) we exclude from our analysis individuals who make either urban-to-rural or rural-to-urban residential moves. This sub-set includes approximately 87% of the observations. Despite the reduction in sample size, we still find a sizable negative association between homeownership and entrepreneurship (-0.010), significant at the 10% level. We also checked that our findings are not more generally driven by individuals' mobility decisions by focusing on workers who live in the same region throughout the period of analysis (approximately 80% of the observations).<sup>11</sup> Even then, we still find a significant and sizable negative association between homeownership and entrepreneurship, estimated at -0.014

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<sup>10</sup> Note that this is not equivalent to using data from annual surveys, since we still exploit the precise timing of when an employment spell begins and we link this to the precise timing of a tenure status change.

<sup>11</sup> The 16 English regions we consider are: Inner London, Outer London, East Anglia, Rest of South East, South West, East Midlands, West Midlands Conurbation, Greater Manchester, Rest of West Midlands, Merseyside, Rest of North West, South Yorkshire, West Yorkshire, Rest of Yorkshire and Humberside, Tyne and Wear and Rest of North.

with a standard error of 0.007.<sup>12</sup> Finally, one related consideration is that our results may be driven by London since this is a place where many entrepreneurial activities tend to concentrate and more people tend to rent. In Column (4) we exclude individuals who live London (approximately 12% of all observations), and find that our results are virtually unchanged.

Next, in Columns (5) and (6) we investigate whether our results are different for urban and rural areas. Our point estimates suggest that homeownership is negatively associated with entrepreneurship across the board, although our results are statistically significant (at the 10% level) only for urban areas. However, our estimates for rural locations are larger in magnitude despite being statistically insignificant. The overall lack of significance is perhaps unsurprising as this breakdown leaves us with 79% of the observations in urban areas and only 21% in rural ones. All in all, however, we take these findings as suggestive that our key result applies across England, with little evidence of significant spatial heterogeneity.

Another concern is that our baseline omitted category is a heterogeneous group bundling together private and public renters. In their analysis of the effect of homeownership on unemployment, Battu *et al.* (2008) report their effects separating private from public renters because the binding mobility constraints faced by public renters might affect their chances of remaining unemployed. Mobility is not a particularly worrying issue in our analysis as entrepreneurs tend to be predominantly local and immobile (Michelacci and Silva, 2007). Nevertheless, we investigated whether our results change when we separately include public and private renters. The results (not tabulated) reveal that homeowners are always significantly less likely to become entrepreneurs than any other category, including public renters.

In the remaining three columns of Appendix Table 2, we conclude our robustness checks by analyzing whether our results only stem from a handful of sectors, or whether they are economy-wide. In Column (7) we use the SIC92 industrial classification at 1-digit level to exclude the following sectors: „agriculture; fishing and forestry“; „electricity, gas and water“; „public administration“; „private households with employees“; and „workers of international organizations/bodies“. In doing so, we follow the work of Glaeser (2009) and Faggio and Silva (2011) who use self-employment data to study the spatial distribution of entrepreneurial activities in the US and UK, respectively. When we do this, we still find that homeownership significantly reduces the chances of becoming an entrepreneur. Finally, in the last two columns of the table, we investigate the robustness of our results when we only consider services (Column 8) or manufacturing (Column 9). Our estimates reveal that our conclusions remain broadly valid when

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<sup>12</sup> We further tried restricting our analysis to the sample of individuals who do not change their LA of residence. The results confirm the picture presented so far, although the effects are smaller and less precisely estimated, most likely because of the significant loss of observations (more than 30%).

we focus on services. However, the point estimates are small and not significant when focusing on individuals working in manufacturing. This result may be due to the fact that only approximately 25% of the observations come from individuals working in manufacturing. Moreover, the share of entrepreneurs with dependent workers is significantly smaller for this sector, at 2.3%. The coefficient in Column (9) still implies a 12.5% negative effect of homeownership on entrepreneurship. Despite its lack of statistical significance, this still represents a significant economic effect.

### *3.3 Dissecting the fixed-effect result: timing and dynamics*

The fixed-effect regressions discussed above are silent on whether the link between homeownership and entrepreneurship represents an instantaneous and permanent effect, or conversely whether this link takes some time to build and then dissipates over time. Any proposed explanation for our main finding should be consistent with the answer to this question.

We present evidence on this issue in Table 3. To begin with, recall that for approximately 7% of the individuals we could not properly identify the date at which they made a transition into/out of homeownership (and thus we imputed it using the timing of their interview). In Column (1) we replicate our analysis excluding these individuals from the sample. The estimates we obtain are now larger and more precisely estimated than before, implying that homeownership reduces the probability of becoming an entrepreneur by up to 35%. This evidence reinforces our earlier claim that using a monthly-spell panel is crucial to investigate the link between tenure status and entrepreneurship since it allows pinning down the precise timing of the transitions into homeownership and entrepreneurial occupations. In the remaining analysis, we focus on people with non-imputed transition dates.

Next, in Column (2), we start our analysis of dynamic effects by including in the empirical model a count of the monthly duration since the individual became a homeowner. This variable displays a positive, but very small and insignificant coefficient. However, when we add to our specification both a linear and a quadratic term in the monthly duration (Column 3), we find that the linear term becomes positive and significant (0.016; s.e. 0.008), while the coefficient on the squared duration is negative and significant at the 10% level (-0.005; s.e. 0.003). This implies an inverted U-shaped relationship between the time since becoming a homeowner and the probability of becoming an entrepreneur. We present this graphically in the top Panel A of Figure 1. The results imply that – on impact – the effect of homeownership on entrepreneurship is as large as -0.018, but that as time goes by, this negative effect becomes less quantitatively meaningful. Note that it takes 4 years (48 months) for the effect to become statistically insignificant at the 5% level.

Moreover, the effect of homeownership never turns positive, even when considering fairly long time-horizons, e.g. after 10 years (120 months).

In the remaining two columns of the table, we investigate two important and related questions. First, we explore whether the negative impact of homeownership on entrepreneurship peaks when the person becomes a homeowner or whether the effect is delayed (Column 4). Next, we analyze whether the negative link happens upon transition into homeownership, or whether part of this effect is anticipated (Column 5). To do so, we append lags and leads in homeownership to the main regression specification. In Column (4), we include variables capturing whether the individual was a homeowner 3, 6, 9 and 12 months before the present date. In Column (5) we control for whether the individual will become a homeowner in 3, 6, 9 and 12 months from now.

Our results show that adding lags does not affect our main conclusion: we still find a negative and significant effect of homeownership on entrepreneurship, quantitatively not dissimilar from before. This suggests that the effect of tenure choice on business start-up decisions is not delayed. Conversely, when we add leads in homeownership, the coefficient on homeownership becomes smaller (at -0.009) and only significant at the 10% level. Controlling for leads in homeownership effectively tests for whether the effect of homeownership on entrepreneurship is anticipated. The evidence suggests that part of our findings might be attributed to would-be homebuyers, who change their preferences and risk attitudes before becoming homeowners, and shy away from entrepreneurial occupations before changing their tenure status. However, an F-test that the leads are jointly insignificant accepts the null with a p-value of 0.212.

Since this is an important issue that could assist our interpretation of the results, we further investigate the dynamics of the effects of homeownership using a complementary approach. Our results are presented graphically in Panel B of Figure 1. The graph plots coefficients and confidence intervals obtained by running 18 separate regressions (plus our benchmark result) where we consider the effect of current homeownership on lags and leads of entrepreneurship. On the positive axis of the graph, we check whether present homeownership has an effect on the probability of being an entrepreneur with dependent workers between 3 months and 36 months *after* becoming a homeowner. The negative side of the axis investigates whether current homeownership is related to entrepreneurship between 3 months and 36 months *before* actually purchasing a home. Each estimate is obtained from a different regression, and therefore these effects should be interpreted as cumulative.<sup>13</sup>

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<sup>13</sup> Note also that by looking at the effects of current homeownership on leads and lags of entrepreneurship we fix the controls at the time of transition into homeownership. We also experimented with an alternative approach analyzing the effect of leads and lags of homeownership on current entrepreneurship, which centres the controls at the time of the employment transition. This second method gave nearly identical results.

Although this approach is very flexible in analyzing anticipation and long-lasting effects of homeownership, one major drawback is that – by using leads and lags – it significantly reduces sample size. This loss of observations is particularly severe when moving further into the future or into the past. For example, when considering 18-months leads/lags, we are left with approximately 310,000 observations over 4300 individuals (out of the original 5193 workers), further dropping to around 270,000 for 3700 individuals when focusing on 36-months leads/lags.

Nevertheless, the main intuition from this analysis supports our previous findings. On the one hand, we find that the negative effect of homeownership on entrepreneurship is stronger on impact and then slowly fades away. The effect becomes insignificant 18 to 24 months after transition into homeownership, and then flattens out without ever becoming positive. On the other hand, we find that anticipation effects are already evident and significant 12 to 18 months before entry into homeownership. This time difference can be interpreted as the lag between the *decision* to buy a house and its *actual* purchase, and suggests that the purchase decision affects individuals' behavior in relation to entrepreneurship even if the house has not been actually bought. Note that this pattern is perfectly consistent with the view that homeownership crowds out entrepreneurship because of portfolio risk considerations.

#### **4. Exploring the mechanism: leverage and portfolio considerations**

##### *4.1 The role of housing leverage in crowding out entrepreneurship*

In this section, we investigate a number of mechanisms that could give rise to a negative link between homeownership and entrepreneurship. To begin with, we explore the role of mortgage finance and leverage by constructing a time-varying measure of the loan-to-value (LTV) ratio. Specifically, we use the initial amount of money borrowed and time-varying data on the outstanding amount of mortgage debt owed by the individuals, coupled with self-assessed house values, to construct a measure of the LTV ratio on the outstanding mortgage loan. Note that this is likely to be a noisy proxy for the actual LTV because the house value is self-assessed. To address this issue, we use an instrumental variable (IV) approach that exploits time-varying information on local loan-to-value ratios at the place of residence.<sup>14</sup>

Our results are reported in Table 4. In Column (1) we include our proxy for the LTV on the mortgage alongside an indicator for whether an individual owns the property. Our results illustrate that conditional on the LTV ratio, homeownership is no longer negatively and significantly

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<sup>14</sup> We experimented with a set of alternative proxies that gave similar results. For example, we used information on the „residual life“ of the mortgage (i.e. the number of years left to repay the mortgage) coupled with information on the initial LTV ratio (based on the purchase price) to construct a proxy for the outstanding amount of debt at any particular point in time. Alternatively, we used the initial house price paid by individuals in combination with changes in local house prices to compute a time-varying measure for the value of the home, and thereby an LTV ratio.

associated with entrepreneurship – its effect is estimated to be precisely zero. As for the LTV ratio, this enters our specification with a negative and sizable effect, significant at the 5% level. The point estimate implies that a one standard deviation increase in the LTV is associated with a reduction in the probability of being an entrepreneur with dependent workers by about 9%. Clearly this result warrants further investigation.

A complementary way to evaluate the importance of leverage is to measure whether homeownership with and without a mortgage has a differential effect. To shed light on this issue, in Column (2), we tabulate estimates of the link between homeownership and entrepreneurship separately for homeowners with and without a mortgage. The dummy variable that represents homeownership with a mortgage can be thought of as a coarse measure of leverage. We find little evidence that homeowners without a loan are less entrepreneurial than renters. The coefficient is negative, albeit completely statistically insignificant. In contrast, homeowners burdened by a mortgage are significantly less likely to be entrepreneurs. The difference between the effect of homeownership with/without a mortgage on entrepreneurship is significant with a p-value of 0.081. Note that the implied economic magnitude is also non-negligible: becoming a homeowner with a mortgage reduces the probability of becoming an entrepreneur by approximately 30%.

Next, in Column (3) we continue our investigation by running a similar regression, but including both the LTV on the outstanding mortgage and both types of homeownership, i.e. with and without mortgage. The effect of the LTV is still negative, but less precisely estimated (p-value: 0.130) than in Column (1). Similarly, the effect of outright homeownership and homeownership with a mortgage are both insignificant, and the negative link between leveraged homeownership and entrepreneurship is much attenuated. This overall lack of significance is perhaps not surprising since these variables are conceptually strongly related: owning a property with a mortgage implies having a positive LTV on the outstanding mortgage, whereas owning a property outright means having fully repaid the loan (so the LTV is set to zero). Therefore, in Column (4) we present a specification where we only include the proxy for the LTV on the outstanding mortgage alongside an indicator for outright homeownership, but we drop the variable indicating whether an individual owns the property with a mortgage. Once we do this, we find that the LTV on the mortgage has a negative and significant effect on the probability of becoming an entrepreneur. This effect is once again non-negligible in terms of its economic impact: a one standard deviation increase in the LTV corresponds to an 8.5% reduction in the probability of becoming an entrepreneur. Alternatively, going from the 25<sup>th</sup> percentile of the LTV distribution (0.283) to the 75<sup>th</sup> percentile (0.684) reduces the chances of an entrepreneurial spell by 12%.

As noted above, one concern with the LTV-proxy is that it may measure the actual LTV on the outstanding mortgage with noise. If this was the case our estimates would be downward biased.

In contrast, one might argue that the LTV at which an individual borrows as well as the LTV on the outstanding mortgage are potentially endogenous and driven by time-varying individual unobservables. This is because, in the UK context, individuals not only have some choice about the initial LTV ratio, but also some discretion about the LTV on the outstanding amount of mortgage at later stages because of refinancing decisions and flexible contractual arrangements (see Muelbauer, 2002). Although we are concerned with an individual's time-fixed unobserved attitudes such as risk-tolerance or financial sophistication since these are „absorbed“ in our fixed-effect strategy, one might worry that our estimated effects are upward biased by unobservable changes in an individual's preferences or financial circumstances.<sup>15</sup> To address this concern, we devise an instrumental variable strategy that exploits information on the LTV of newly originated mortgages in the LA of an individual's residence obtained from the Survey of Mortgage Lenders (SML).<sup>16</sup> Specifically, we construct an instrument which is set to zero before an individual becomes a homeowner and equal to the time-varying local LTV in the LA of an individual's residence thereafter. The aim of this variable is to predict the initial LTV at which an individual borrows and the subsequent LTV on the outstanding mortgage using prevailing local housing market conditions, thus helping us to by-pass concerns about the endogeneity of the LTV ratio driven by individuals' time-varying unobservables.

Our fixed-effects instrumental-variable (FE+IV) results are reported in Column (5), with first-stage statistics tabulated at the bottom of the table. The first stage statistics indicate that there is clearly a strong and positive link between an individual's LTV and local prevailing housing market conditions. More importantly, the second stage results are in line with the fixed-effect results presented in Columns (4) and show that the LTV ratio of the outstanding mortgage has a negative and significant effect on entrepreneurship, but outright homeownership does not have any impact. Note that the estimated impact of the LTV is slightly larger than the corresponding results in Column (4), where we did not use an IV approach, although the difference is not statistically significant. This suggests that measurement error – biasing our results towards zero – might be a more serious concern than endogeneity.

To further assess the validity of our results, we performed a number of additional robustness checks. To begin with, we run specifications controlling for the monthly duration in homeownership to avoid attributing some of the effects of variation in time to changes in the LTV. Although the correlation between tenure duration and LTV is negatively signed as expected, this is

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<sup>15</sup> For example, winning the lottery might push an individual to become an entrepreneur at the same time as he/she repays the mortgage or reduces its value. This would give rise to a negative link between LTV and entrepreneurship.

<sup>16</sup> The SML has a broad coverage of UK mortgage lenders in addition to building societies, and collects a wide range of mortgage-related information such as the amount of gross interest rates charged, whether the rate is fixed or variable, various repayment methods, purchase price and mortgage amount. We exploit this data to construct a measure for the prevailing LTV in a given year for each LA.

not particularly strong at -0.483. In any case, the inclusion of this additional control does not affect our results. Further, we checked whether our results might be explained by changes in housing values that reduce the size of the LTV and thereby positively affect entrepreneurship. To test for this possibility, we included in our specification information on house prices at the LA level. This inclusion does not affect our results. Finally, we ran specifications that focus on individuals who do not change the LA of residence during the period of analysis. Results using this subset of workers were similar to those tabulated in Table 4, although the effects were less precisely estimated. This is explained by the fact that more than 30% of the individuals are dropped from the analysis when imposing this restriction.

Overall our results clearly suggest that leveraged homeownership is significantly and negatively associated to entrepreneurship, and that this effect is economically meaningful. We believe this finding can be rationalized by overinvestment in housing and portfolio distortions (discussed in the Introduction): purchasing a house tends to concentrate an individual's wealth into one single asset, which implies that individuals cannot adequately diversify their investment risk, in particular as it is difficult to hedge housing-value risk in the absence of contracts that allow to „short it“. This effect is particularly significant for highly leveraged homeowners: As a consequence, individuals choose not to start-up their own business venture since this would imply taking on significant additional risk (Knight, 1921; Kanbur; and 1979, Kihlstrom and Laffont 1979). In the next section, we go on to provide more direct evidence to support this proposition.

#### 4.2 *Direct evidence on portfolio distortions: profit variability and sunk costs*

If our intuition is correct, the negative effect of homeownership on entrepreneurship should be more pronounced for sectors where entrepreneurial activities are more risky. In order to test this proposition, we collect information contained in the Structural Business Statistics prepared by Eurostat.<sup>17</sup> In particular, we assemble data on industry-level profits and investment (capital spending) per employee in the UK. Both variables are available at the NACE 2-digit sector level on an annual basis for the 1997 to 2007 period. This sectoral level aggregation can be mapped to the standard industry classification provided in the BHPS (SIC92), providing a sufficient level of detail by dividing the economy in 45 sectors.

Using this data, we calculate three measures that capture sector-specific riskiness. First, we compute the coefficient of variation of industry-level profits for the available period.<sup>18</sup> Our second proxy capturing sector-specific risk is the average investment (capital spending) per employee for

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<sup>17</sup> These can be accessed at [http://epp.eurostat.ec.europa.eu/portal/page/portal/european\\_business/data/database](http://epp.eurostat.ec.europa.eu/portal/page/portal/european_business/data/database), where more information on the data construction and availability is also provided.

<sup>18</sup> This is simply obtained by dividing the standard deviation of profits within-sector over-time by average profits within sector over time.

the available period. This variable measures the sunk component (irreversible) of a company's investments, representing a risk that entrepreneurs must bear when starting up a business. Finally, we combine the two measures into a comprehensive risk variable by multiplying profit volatility by average investment per employee.<sup>19</sup> Note that this variable will be at its highest when profits are highly volatile and sunk costs significant, while it will be at its lowest when either of the two components is close to zero.

Using these three measures, we divide our sample into two groups: individuals who work in risky sectors and those who do not. Subsequently, we run separate regressions on the two subsamples to investigate whether the negative effect of homeownership is more pronounced and significant in industries characterised by more risk.<sup>20</sup> Note also that to emphasize the role of leverage, we still distinguish between homeowners with a mortgage and outright homeowners.

Our results are displayed in Table 5. First, in Columns (1) and (2) we split our sample according to profit variability and using the median of the distribution of the coefficient of variation in the individual sample (at 0.1348). Columns (1) and (2) confirm that outright homeownership is not significantly related to entrepreneurship. In contrast, a comparison of the two columns for homeowners with mortgages indicates that leveraged homeownership significantly reduces transition into entrepreneurship, but only for individuals in risky sectors.

Next, in Columns (3) and (4) we split the sample using the median of the distribution of capital intensity (at 5.811; measured in thousands of Euros per employee). Again, we find that the negative effect of leveraged homeownership is more significant for individuals working in risky sectors as proxied by the „sunkness“ of their investments. However, the magnitude of the coefficient for homeowners with a mortgage working in non-risky sectors is the same as the one for workers in risky industries, suggesting that leveraged homeownership reduces business start-up rates across the board. Nevertheless, it should be noted that this may be because this variable on its own might capture the extent to which capital requirements are important in a given sector, and thus also proxy for the presence of credit constraints. If this was the case, the evidence in Columns (3) and (4) would suggest that financing issues cannot explain our results. We will return to this point in the next section.

Finally, in Columns (5) and (6) we split the sample using the median of our favorite proxy for risk obtained by interacting profit variability and sunkness of investments. Once again, our results

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<sup>19</sup> A similar proxy is devised by Picchizzolu (2010), who uses Eurostat data to investigate entrepreneurial risk and industrial concentration in the UK.

<sup>20</sup> An alternative way of performing the same test is to split our dependent variable into entrepreneurs in risky sectors and entrepreneurs in non-risky sectors. When we do this and run two regressions with the two different dependent variables (as opposed to two different samples), we still find that leveraged homeownership crowds out entry into risky entrepreneurship, but has a negligible effect on entry into non-risky entrepreneurship. Results are not shown in the interest of brevity but are available from the authors upon request.

show that outright homeownership is not significantly associated with entrepreneurship. In contrast, leveraged homeownership is negatively and significantly associated with the decision to become an entrepreneur for individuals working in risky sectors, whereas this is not the case for workers in other industries. This effect is not only significant but also economically sizable: becoming a homeowner with a mortgage reduces the chances of becoming an entrepreneur with dependent workers in a risky sector by approximately 35%.<sup>21</sup>

Finally, it is worth noting that the negative relationship between homeownership and entrepreneurship should not be dissimilar from the relationship between homeownership and any other risky investment. In other words, there should be a negative correlation between purchasing a house and investing in stocks or risky bonds. As discussed in the introductory section, a significant literature has examined the portfolio effects of homeownership and has come to exactly this conclusion. Recently, Chetty and Szeidl (2010) have investigated whether this negative relationship can be interpreted as causal. To obtain clear identification, the authors instrument home equity and mortgage exposure using both current and initial average house prices in an individual's state of residence. They further compare investment in stocks before and after housing purchases exploiting panel data techniques. Their results show that homeownership reduces stock investment, and that a \$10,000 dollar increase in mortgage debt (approximately one standard deviation change) reduces the share of investments in stocks in the liquid wealth by approximately 6% (holding total wealth constant).

Unfortunately, it is very difficult to replicate Chetty and Szeidl's (2010) results using the BHPS. This is because information on savings and asset allocation was only collected in three waves of the Survey (1995, 2000 and 2005). Moreover, only a small fraction of the sampled individuals were present in all three waves (up to ten years apart), and/or reported a positive amount of savings and invested assets. Nevertheless, we attempted to replicate their analysis by regressing the share of individual savings that are allocated to risky assets (stock shares, premium bonds and shares of investment trusts) on homeownership, while controlling for the usual set of individual, household, and location characteristics. Simple cross-sectional results reveal a negative and significant correlation between homeownership and risky asset investments. Similarly, when moving to a fixed-effects approach, we find a negative association between homeownership and risky investments, which is more pronounced for homeowners with mortgages. However, none of

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<sup>21</sup> Note that one further implication of the portfolio-distortion argument is that leveraged homeownership should have a symmetric effect on would-be and current entrepreneurs. In other words, entrepreneurs who become homeowners should be more likely to switch-out of entrepreneurship, while workers who purchase a house should be less likely to become entrepreneurs. We tested this implication in our data and found some support for it. The effect of homeownership with a mortgage for individuals currently working as employees is -0.0072 (s.e. 0.0057), while its effect for current entrepreneurs is -0.0081 (s.e. 0.0067). Although neither estimate is significant at conventional levels (p-values of 0.206 and 0.231, respectively), the pattern provides additional support for a portfolio-distortion interpretation of our results.

the fixed-effects estimates is significant at conventional levels, which we attribute to the lack of repeated observations (only approximately 700 out of 2600 individuals appear in more than one of the three waves). Finally, we replaced leveraged homeownership with the proxy for the LTV on the outstanding mortgage described above and instrumented the latter using local LTV values as discussed in Section 4.1. Following this approach, we find that a one standard deviation increase in the LTV on the mortgage reduces the share of the portfolio held in risky assets by 6-7%. Although this effect is not statistically significant at conventional levels (coefficient: -0.126; s.e.: 0.128), this finding is consistent with Chetty and Szedil (2010). All in all, we take this as further evidence that leveraged homeowners shy away from entrepreneurial jobs because of overinvestment in housing, which in turn implies that they cannot adequately diversify their investment risk.

#### 4.3 *Credit constraints as an alternative explanation? Some dispelling evidence*

The results in Table 5 support our intuition that portfolio risk considerations push homeowners to avoid entrepreneurship, in particular in the initial years of their tenure, when their investment is most leveraged. However, these results could also be consistent with an explanation based on credit constraints (Evans and Jovanovic, 1989; Holtz-Eakin et al., 1994; Blanchflower and Oswald, 1998): leveraged homeowners might find it hard to obtain additional finance to start-up their business because they are already burdened with a substantial loan on their house and this effect might be more pronounced for would-be entrepreneurs in risky, capital intensive sectors. In the remainder of this section, we present a host of results that convincingly show that credit constraints cannot rationalise our findings. These findings are reported in Table 6.

To begin with, notice that if an explanation based on credit constraints and housing was to hold true, then we should observe that initially-constrained homeowners subsequently become able to use the potential capital gains accumulated on their homes as collateral to finance their entrepreneurial activities (as suggested by Black *et al.*, 1996 and Wang, 2011). Stated differently, homeowners that live in areas with positive house price appreciation should see their credit constraints relaxed over time and enter entrepreneurship more easily. This could in turn explain the initial negative link between homeownership and business start-ups.

In order to investigate this hypothesis, we use annual house price data available at the LA level matched to our monthly BHPS dataset. LA-level average mix-adjusted house prices are computed combining data from the Survey of Mortgage Lenders and the Land Registry.<sup>22</sup> To start with, we add local house prices (in logs) as a control in our main regression (as specified by Equation 1). As shown in Column (1) of Table 6, this has no discernible effect on our main finding:

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<sup>22</sup> The average house price prevailing in a given local authority is adjusted for the composition of the housing stock. See Hilber and Vermeulen (2010) for details.

homeowners are significantly less likely to become entrepreneurs. More importantly, the dynamics of local house prices are not significantly related to the chances of becoming an entrepreneur.

Next, we calculate the cumulative percentage change in housing prices prevailing in the LA of an individual's residence between the time when he/she purchased the property and the current date. This gives us a neat measure of the capital gains (or losses) accrued to an individual through homeownership, which allows us to explicitly test whether the equity position built into someone's real estate investment can be used as collateral to borrow and relax credit constraints in setting up a business. As shown in Column (2) of the table, this does not seem to be the case: the effect of the cumulative house prices gains on the probability of becoming an entrepreneur is estimated to be small, insignificant and negatively signed. In contrast, the direct negative effect of homeownership on entrepreneurship is negative, significant and sizable at  $-0.014$  (s.e.  $0.006$ ).

Even when we separately consider homeowners with and without a mortgage and interact housing capital gains with leveraged and outright homeownership (Column 3), we find no evidence in favor of the credit constraints hypothesis: irrespective of whether an individual owns his/her property outright or with a mortgage, there is no link between cumulative house price gains and entrepreneurship. In contrast, the direct negative effect of leveraged homeownership remains large and strongly significant.

Next, in Column (5) we use more aggregated regional house price data obtained from Nationwide to calculate cumulative housing value gains for homeowners. This alternative variable should address concerns that noise in our disaggregated LA-level proxy may lead to an underestimate of the effect of equity building into individuals' homes. However, this change does not affect our main finding: homeowners with mortgages are significantly less likely to become entrepreneurs, while cumulative house price gains are not significantly related to the chances of setting-up a business.

To conclude this extensive battery of tests, we construct a proxy for the residual amount of cash accruing to an individual after mortgage payments. In order to obtain this measure, we consider mortgage payments in the month preceding the interview and subtract this quantity multiplied by twelve (assuming constant payments within the year) from the overall individual annual income. As shown in Column (5), controlling for this proxy does not change our headline finding. More importantly, the coefficient on residual cash flows is positive, but very small and not significant at conventional levels (coefficient:  $0.009$ ; s.e.:  $0.010$ ). We also investigated whether considering some self-reported measures of individuals' perceptions about their current financial situation and financial expectations for the year ahead could confound our results and provide some evidence in favor of the credit constraints proposition. More precisely, we included in our analysis answers to the following two questions: (i) "How well would you say you are managing

financially these days? Living comfortably; going alright; just getting by; finding it difficult; finding it very difficult”; and (ii) “Looking ahead, how do you think you will be financially a year from now? Better than now; worse than now; same as now”. Adding these controls to our specifications did not alter our key finding. More importantly, these proxies did not enter our regressions with significant and consistently signed coefficients.

In conclusion, this set of tests clearly speaks against credit constraints as being the mechanism behind the novel finding documented in our paper. More generally, the estimates discussed in this section cast doubt on the importance of credit constraints in business start-ups. While this result is at odds with a large literature on the effects of wealth, income windfalls and financing issues on the decision to become entrepreneur (see introductory section) it is fully consistent with the recent work by Hurst and Lusardi (2004). The authors use US micro-level data from the PSID to show that the relationship between wealth and entrepreneurship is only significant at the very top of the wealth distribution. More to the point, they show – exactly as we do – that households living in areas which experience strong house price appreciation are *not* significantly more likely to start an entrepreneurial venture.<sup>23</sup> All in all, this lends strong credibility and external validity to our results.

## **5. Concluding remarks and discussion**

In this paper, we have studied the previously unexplored link between homeownership and entrepreneurship. Our main interest in studying this relationship rests on the notion that flourishing entrepreneurial activities can be associated to the creation of new businesses and an acceleration of innovation, both of which are conducive to higher economic growth. Previous analysis of the labour market effects of homeownership has only focused on employment and unemployment opportunities, thus neglecting an important channel whereby housing policies might affect the country-wide economic performance.

In order to investigate these issues, we have used information from the BHPS to construct a monthly dataset that tracks an individual’s job history and tenure choice. We have then exploited this data to identify the link between homeownership and entrepreneurship while controlling for both time-fixed individual unobservables and time-varying individual observables. The use of panel techniques on monthly data to investigate the determinants of entrepreneurship is an improvement over the previous literature, and in our context this is crucial to isolate the precise timing of transitions into homeownership and entrepreneurial jobs.

Naïve cross-sectional analysis reveals a positive and significant correlation between homeownership and various measures of self-employment and entrepreneurship. However, our

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<sup>23</sup> More recently, Disney and Gathergood (2009) replicated Hurst and Lusardi’s (2004) results using BHPS data, with an analysis similar to the one presented here.

panel regression analysis reveals that, once we include individual fixed effects to partial out time-fixed unobserved individual characteristics, becoming a homeowner significantly *reduces* the probability of becoming an entrepreneur. Importantly, this effect is stronger when focusing on self-employed with dependent workers and self-employed in managerial and professional occupations. This suggests that our evidence predominantly captures a negative link between homeownership and genuine entrepreneurship, and not self-employment out of necessity.

Furthermore, we find that this effect is stronger for homeowners with a mortgage, but this cannot be satisfactorily explained by the presence of credit constraints. Conversely, we provide compelling evidence that our findings can be rationalized by overinvestment in housing and portfolio distortions. In a nutshell, purchasing a house concentrates an individual's wealth into one single asset and this makes it difficult for individuals to adequately diversify investment risk. This effect is particularly significant for highly leveraged homeowners. As a result, individuals choose not to start-up their own business venture since this would imply taking on additional risk.

We think these findings are novel and policy relevant. In particular, a large number of countries have set in place policies that favor homeownership, mostly by making it easier to finance home purchases with a loan. These policies include mortgage interest rate deductibility, non-taxation of owner-occupation related capital gains and imputed rents, or the creation of secondary mortgage markets and housing-finance giants such as Fannie Mae and Freddie Mac (see Frame and White, 2005). The evidence provided here – namely that access to homeownership using leverage significantly depresses entrepreneurial activities – carries profound implications for the role of housing policies in shaping economic performance.

How general are our results? The UK – and England, in particular, which was the focus of our analysis – is a large, open economy with developed financial and housing markets, as well as a dense entrepreneurial environment. The average homeownership rate in the UK prior to the recent financial crisis (2007-2009) was similar to the one prevailing in the US at around 68%, and higher than in other European countries. Similarly to the US, the UK also counts a number of internationally well-known entrepreneurs who established themselves in technology, media and retail (e.g. Lord Alan Sugar and Sir Richard Branson), as well as a thriving entrepreneurial clusters (e.g. the Silicon Roundabout in London and the Cambridge High Tech Cluster). We believe these features make the UK an interesting laboratory to investigate the relationship between homeownership and entrepreneurship, and provide support for the external validity of our results.

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**Table 1: Summary statistics – BHPS individual level monthly dataset**

Variable	Mean	Std. Dev.
<i>Panel A: Entrepreneurs + homeowners</i>		
Entrepreneurs: all	0.144	0.351
Entrepreneurs, excl. elementary/machinery	0.074	0.262
Entrepreneurs: dependent	0.047	0.212
Entrepreneurs: managers	0.070	0.256
Home owner	0.811	0.391
Home owner, with mortgage	0.714	0.451
Homeowners, outright (no mortgage)	0.096	0.295
Private renter	0.087	0.282
Public renter	0.084	0.277
<i>Panel B: Controls</i>		
Age	39.38	8.95
Male	0.788	0.409
Household total income (previous year)	31728.0	22046.3
Individual total income (previous year)	20990.0	16144.4
Children under 16 (yes=1, no=0)	0.457	0.498
Coupled (yes=1, no=0)	0.745	0.436
Education: Higher Degree	0.039	0.193
Education: First Degree	0.151	0.358
Education: Higher Non Degree/Teaching Qual.	0.080	0.272
Education: A Level (or equiv.)	0.228	0.419
Education: O Level (or equiv.)	0.265	0.441
Education: CSE (or equiv.)	0.071	0.256
Education: None of these	0.166	0.372

Note: The sample only includes heads of household aged between 20 and 55 living in England (excludes Scotland and Wales). Summary statistics of control variables refer to the sample where all controls are non-missing. Number of observations: 358070. Number of individuals: 5169. Panel is unbalanced. „Entrepreneur: all“ include all entrepreneurs (self-employed); „Entrepreneur: excluding elementary/machinery“ excludes entrepreneurs working in elementary occupations or operating plants, machinery or processes; „Entrepreneur: dependent“ includes entrepreneurs with dependent employees; „Entrepreneur: manager“ includes entrepreneur in managerial and professional jobs. Log household income descriptive statistics: mean=10.174; std.dev.=0.676. Log individual income descriptive statistics: mean=9.717 std.dev.=0.891. In the regression analysis age is controlled semi-parametrically by including the following dummies: age between 20 and 24; age between 25 and 29; age between 30 and 34; age between 35 and 39; age between 40 and 44; age between 45 and 49; age between 50 and 54; age 50 or above.

**Table 2: OLS and fixed effects regressions – Various definition of entrepreneurs**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	FE	FE	FE	FE	FE
			All Trans.	All Trans.	All Trans.	Trans. In	Tran. Out
<i>Panel A: Entrepreneur, all</i>							
Homeowner	0.039	0.032	-0.015	-0.013	-0.014	-0.026	0.000
	(0.009)**	(0.010)**	(0.008)	(0.008)	(0.008)	(0.012)*	(0.021)
<i>Panel B: Entrepreneur, dependent</i>							
Homeowner	0.036	0.014	-0.011	-0.012	-0.013	-0.022	-0.009
	(0.006)**	(0.006)*	(0.005)*	(0.006)*	(0.005)**	(0.009)*	(0.010)
<i>Panel C: Entrepreneur, manager</i>							
Homeowner	0.040	0.021	-0.018	-0.018	-0.017	-0.031	0.001
	(0.008)**	(0.008)**	(0.006)**	(0.007)**	(0.006)**	(0.010)**	(0.009)
Year&Month dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LPA dummies	No	Yes	No	Yes	No	No	No
Controls	No	Yes	No	Yes	Yes	Yes	Yes

Note: The sample only includes heads of household aged between 20 and 55. Number of observations: 366168. Number of individuals: 5193. Panel is unbalanced. Column (6) only includes people who are or become homeowners. Column (7) only includes people who are or become renters. Year dummies refer to the year when the BHPS interview was carried out. Month dummies refer to calendar months during which the employment spell took place. LPA dummies refer to the Local Planning Authority of residence (343 LPAs matched to English-resident BHPS individuals). LPA dummies excluded in fixed-effects models in Columns (5) and following since only 60% of the individuals change LPA of residence over the period of the sample. Standard errors clustered at the LPA level. \*\* p<0.01, \* p<0.05. Controls as listed in Table 4. Household and individual income included in logs. „Entrepreneur: all“ include all entrepreneurs (self-employed); „Entrepreneur: dependent“ includes entrepreneurs with dependent employees; „Entrepreneur: manager“ includes entrepreneur in managerial and professional jobs.

**Table 3: Entrepreneurs with dependent workers – Timing and dynamics**

	(1)	(2)	(3)	(4)	(5)
	FE	FE	FE	FE	FE
	No Input Trans. Date	Duration Linear	Duration Squared	Control For Lags	Control For Leads
Homeowner	-0.017 (0.006)**	-0.013 (0.006)*	-0.018 (0.006)**	-0.014 (0.006)*	-0.0085 (0.0050) <sup>+</sup>
HO Duration (× 100)		0.003 (0.005)	0.016 (0.008)*		
HO Duration Squared (× 1000)			-0.0005 (0.0003) <sup>+</sup>		
<i>P-value, significance of leads/lags</i>	--	--	--	0.6995	0.2120

Note: Regressions run on the monthly dataset. All regressions include year dummies; monthly Dummies; SIC92 1-digit sector dummies; and individual controls. See notes to Table 2 for more details. Standard errors clustered at the LA level. \*\* p<0.01, \* p<0.05. Column (1) excludes individuals with imputed transition date into homeownership (approx. 7.3%). Columns (2) and (3) controls for number of months since becoming homeowner (linear and squared terms). Descriptive statistics for duration in months: mean = 122.22; std. dev. = 83.12. Column (4) includes „lags“ that control for homeownership status in 3, 6, 9 and 12 months before current date. Column (5) includes „leads“ that control for homeownership status in 3, 6, 9 and 12 months from current date. Note that Columns (4) and (5) only consider individuals with no imputed transition date into/out of homeownership.

**Table 4: Entrepreneurs with dependent workers – Exploring our findings: the role of leverage**

	(1)	(2)	(3)	(4)	(5)
	FE	FE	FE	FE	FE + IV
Homeowner	-0.000 (0.007)				
Homeowner, mortgage		-0.014 (0.005)**	-0.002 (0.007)		
Homeowner, outright		-0.002 (0.009)	0.002 (0.009)	0.004 (0.007)	0.006 (0.007)
Loan-to-value (LTV) of mortgage	-0.015 (0.008)*		-0.013 (0.008)	-0.014 (0.006)*	-0.0172 (0.0091)*
<i>First-stage:</i>	--	--	--	--	0.829
<i>Coeff. (s.e.) on instrument</i>					(0.013)
<i>T-Stat on instrument</i>	--	--	--	--	63.93

Note: Regressions run on the monthly dataset. All regressions include year dummies; monthly dummies; SIC92 1-digit sector dummies; and individual controls. See notes to Table 2 for more details. Standard errors clustered at the LPA level. \*\* p<0.01, \* p<0.05. Loan-to-value (LTV) of outstanding mortgage calculated using outstanding amount of mortgage and individual's assessment of property value. LTV capped at 1.25; values above 1.25 recoded as missing. Descriptive statistics for LTV as follows. Mean=0.485; std.dev.=0.259. Instrumental variable regressions instrument individual's LTV with local LTV obtained using data from the Survey of Mortgage Lenders at the LPA level. The instrument is time-varying and set to zero for years in which individuals are renters. Descriptive statistics of local LTV as follows. Mean=0.751; std.dev.=0.059.

**Table 5: Entrepreneurs with dependent workers – Risk and cost sunkness**

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Coeff. of Variation</i>		<i>Cost Sunkness</i>		<i>Measure of Risk</i>	
	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median
Homeowner, mortgage	-0.024 (0.011)*	-0.004 (0.08)	-0.013 (0.006)*	-0.013 (0.012)	-0.015 (0.006)*	-0.002 (0.009)
Homeowner, outright	0.003 (0.020)	-0.009 (0.013)	0.001 (0.012)	-0.007 (0.022)	0.006 (0.015)	-0.003 (0.017)

Note: Regressions run on the monthly dataset. All regressions include year dummies; monthly dummies; SIC92 1-digit sector dummies; and individual controls. See notes to Table 2 for more details. Standard errors clustered at the LPA level. \*\* p<0.01, \* p<0.05. Columns (1) and (2) split sample above/below median of the coefficient of variation of profits in the sector of employment. Columns (3) and (4) split sample above/below median of the capital spending per occupied worker in the sector of employment. Columns (5) and (6) split sample above/below median of the „risk“ in the sector of employment. This is measured as the product of the sectoral coefficient of variation (profit variability) times the sectoral capital intensity as a measure of the sunk component of the company investments. Data obtained from Eurostat for the years 1997 to 2007 and averaged across available years. Data merged using NACE sector at the 2-digit level. Median values of coefficient of variation, capital intensity and risk as follows: 0.1348; 5.811 and 1.3134.

**Table 6: Entrepreneurs with dependent workers – House price dynamics and credit constraints**

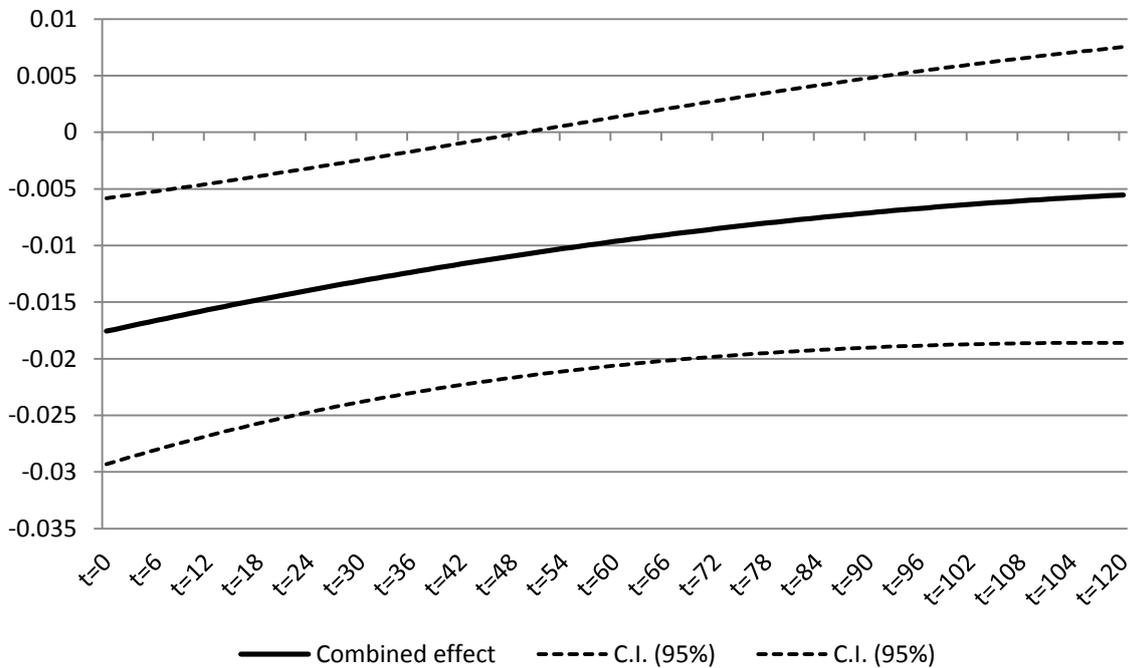
	(1)	(2)	(3)	(4)	(5)
	FE	FE	FE	FE	FE
Homeowner	-0.012 (0.006)*	-0.012 (0.006)*			
Homeowner, mortgage			-0.014 (0.006)**	-0.015 (0.005)**	-0.018 (0.006)**
Homeowner, outright			0.007 (0.011)	0.006 (0.010)	-0.003 (0.009)
Local HP (logs)	-0.002 (0.012)				
Cumulative HP gains		-0.002 (0.004)			
Cumulative HP gains × Home., mortgage			-0.002 (0.004)	0.003 (0.005)	
Cumulative HP gains × Home., outright			-0.008 (0.007)	-0.010 (0.008)	
Residual Cash flow (× 100)					0.009 (0.010)
House price measure	Local	Local	Local	Regional	--

Note: Regressions run on the monthly dataset. All regressions include year dummies; monthly dummies; SIC92 1-digit sector dummies; and individual controls. See notes to Table 2 for more details. Standard errors clustered at the LPA level. \*\* p<0.01, \* p<0.05. Column (1) includes (log of) local house prices (HP) alongside LPA dummies. Cumulative HP gain refers to the cumulative house price change from time of purchase up to that period for homeowners. Housing price series at the LPA level used in Columns (1) to (3) obtained from the Land Registry data. Housing price series at the regional level used in Column (4) obtained from the Nationwide data. Descriptive statistics of cumulative gain for homeowners as follows. LPA level: mean=0.479; s.d.=0.783. Regional level: mean=0.432; s.d.=0.695. Residual cash flow calculated as (individual annual income) – (12 × mortgage payment in previous month). Descriptive statistics for residual cash flow: mean = 29694.5; std. dev. = 20878.9.

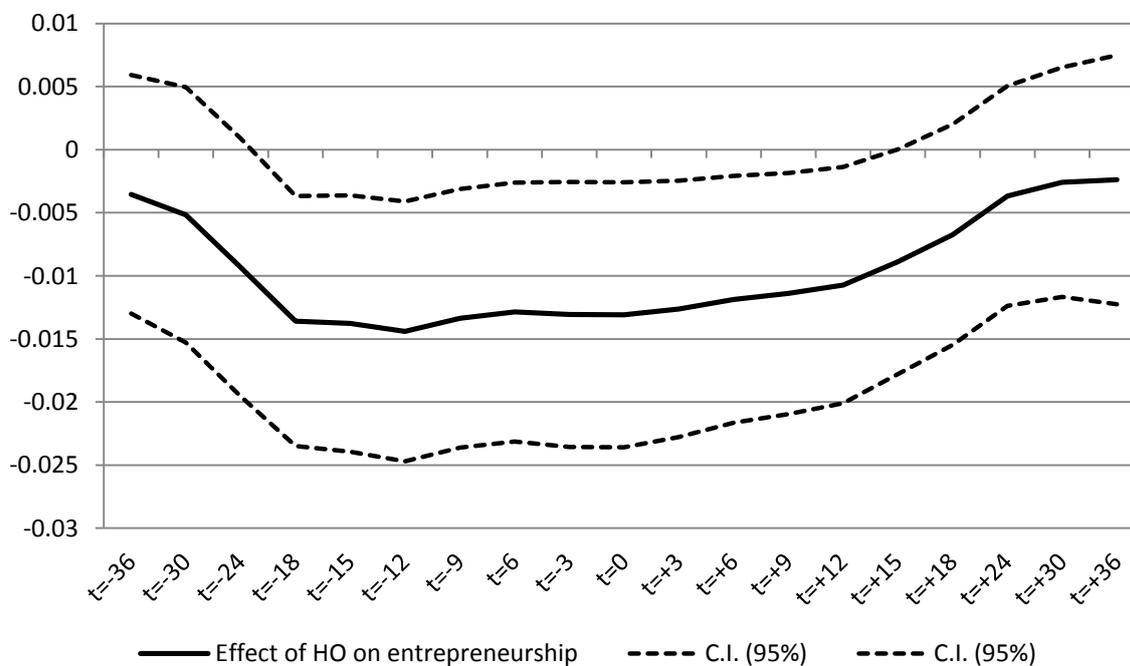
## Figures

**Figure 1: Dynamic effect of homeownership – Duration, leads and lags**

*Panel A: Combined effect of homeownership, duration and duration squared*



*Panel B: Effect of homeownership on entrepreneurship, leads and lags*



Note: Results used to obtain the graph in Panel A come from the specification presented in Table 3, Column (3). Results used to obtain the graph in Panel B come from 19 separate regressions of lag/lead of entrepreneurship on current homeownership status, conditional on the usual controls. See notes to Table 3 for more details. Dashed lines are confidence intervals at the 95% level obtained from standard errors clustered at the LA level.

## Appendix A - Construction of monthly job histories

In what follows, we provide a description of the way we have constructed monthly job spells and tried to solve inconsistencies in the BHPS. In general, we follow the principle that information recorded closest to the date of the beginning of the spell is the most accurate. A similar approach is used in Upward (1999) and Battu *et al.* (2008).

To begin with, consider that the BHPS contains a longitudinal file identifying every person that ever appeared in the survey, indicating in which waves he or she was interviewed. From this file we construct the list of individuals that belong to the initial sample, i.e. those with a full interview in Wave 1, as well as those who fill in a full interview for the first time in one of the subsequent waves.

Next, in every wave of the BHPS, interviewed individuals appear in a '*respondent file*', which contains information on the current labour force and occupational status – and if they have changed their labour market status between two waves – in a '*job history file*' that collects detailed information for every occupational spell (including unemployment and inactivity spells), such as job characteristics, starting date, ending date and sector of occupation. In order to construct labour market spells, we use the following iterative strategy for every wave of the BHPS, starting from Wave 1 (1991) or the first wave in which an individual first appears, and working towards to the most recent wave (namely Wave 18 in 2008):

- (i) We open the '*job history file*' and the '*respondent file*' and carry out some consistency checks in both of them separately (more details below);
- (ii) We append the '*respondent file*' on top of the '*job history file*' in order to check the consistency between the two – in particular regarding the starting date of the current job and the history of jobs reported in the history file. We name the resulting file '*wave w*' file, where *w* indicates the wave under consideration;
- (iii) We append the file '*wave w*' on top of the combined file from the previous wave, that is, '*wave w-1*' and check the consistency of the information provided in the two files.
- (iv) Once we have appended all waves, we compute the duration in months of every spell and we expand the dataset so that every observation corresponds now to one specific month. We call the resulting file the '*labour spell file*'.

In the original BHPS data, every labour market spell comes with a starting/ending date, and inconsistencies arise because of overlaps between these dates. In order to address inconsistencies, we take a double approach of looking for problematic cases both: (a) within-file, i.e. within the '*job history file*' and the '*respondent file*' separately; and (b) within-wave, i.e. within the

combined file obtained by appending the *'respondent'* and the *'job history'* files. The general idea is to resolve overlaps by preferring answers recorded closest to the date of the beginning of the spell. Note that our „within-file“ and „within-wave“ approach also solves situations that could arise because of between-wave overlaps. In detail, we proceed as follows:

Within-file checks: (a) Spells that display a starting date earlier than the interview of the previous year are recoded as starting on the day of the interview of the previous year. This is because, up to the date of the previous interview, we „trust“ information from the previous wave more than retrospective information<sup>24</sup>; (b) Spells starting after the current date of interview are considered as starting on the date of interview. Discrepancies of this type probably emerge as a coding error in the original data; (c) For the *'job history file'* only, we check that the sequence of spell starting dates is increasing. If this is not the case, we drop the spell(s) that cause the inconsistency.

Within-wave checks: (a) If a spell from the *'job history file'* has a missing starting date, the starting date is imputed as the mean of the starting dates of the two adjacent job history spells. Stated differently, we centre this job spell in the middle of the two adjacent ones. (b) If a spell from the *'respondent file'* has the starting date missing, two possibilities arise. If there is no *'job history file'* spell for the same individual, the starting date of this spell is imputed as the date of the previous interview. If instead there is a pre-dating spell in the *'job history file'*, the starting date of the current job is imputed as the date of current interview; (c) Next, we check that the sequence of starting dates in the combined *'respondent'/'job history'* file – i.e. the *'wave'* file – is increasing. If not, we drop the spell that causes the inconsistency; (d) Finally, we check that point (c) holds true when we iteratively append *'wave files'* from subsequent waves of the BHPS.

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<sup>24</sup> For the first wave in which an individual appears, we can't use the date of the previous interview as a reference point. Hence, we decide to consider only spells starting after 1<sup>st</sup> September of the year before the first interview.

## Appendix B - Additional Tables

**Appendix Table 1: Transitions into and out of homeownership and entrepreneurship**

	% of individuals making at least...	
	One transition	Two transitions
<i>Panel A: Homeownership</i>		
Overall	18.3	5.8
Transition in	13.3	0.9
Transition out	10.8	1.0
<i>Panel B: Homeownership with mortgage</i>		
Overall	25.5	7.7
Transition in	16.2	1.4
Transition out	17.1	1.7
<i>Panel C: Homeownership without mortgage</i>		
Overall	12.6	3.6
Transition in	9.7	1.0
Transition out	6.5	0.6
<i>Panel D: Entrepreneur, all</i>		
Overall	16.8	8.7
Transition in	13.3	2.7
Transition out	12.2	2.5
<i>Panel E: Entrepreneur, dependent</i>		
Overall	5.9	3.3
Transition in	4.8	1.1
Transition out	4.4	1.0
<i>Panel F: Entrepreneur, manager</i>		
Overall	8.8	5.1
Transition in	7.4	1.6
Transition out	6.6	1.4

Note: The sample only includes heads of household aged between 20 and 55 living in England (excludes Scotland and Wales). Number of individuals: 5193. Panel is unbalanced.

**Appendix Table 2: Entrepreneurs with dependent workers – Robustness and heterogeneity**

	(1)	(2)	(3)	(4)	(5)	(7)	(7)	(8)	(9)
	Excluding Short Spells	Including UN Spells	Immobile Workers	Excluding London	Urban Areas	Rural Areas	Excl. Selected Sectors	Services Only	Manuf. Only
Homeowner	-0.013 (0.005)**	-0.013 (0.005)**	-0.010 (0.006) <sup>+</sup>	-0.014 (0.006)*	-0.010 (0.006) <sup>+</sup>	-0.015 (0.013)	-0.012 (0.006)*	-0.013 (0.007)*	-0.003 (0.004)

Note: All regressions include year dummies; monthly Dummies; SIC92 1-digit sector dummies; and individual controls. See notes to Table 2 for more details. Standard errors clustered at the LPA level. \*\* p<0.01, \* p<0.05, + p<0.10. Column (1) excludes employment and entrepreneurial spells shorter than 12 months. Column (2) considers employment, entrepreneurship and unemployment spells. Column (3) only considers individuals always living either in urban areas or in rural areas (no urban-to-rural and rural-to-urban movers). Urban and rural areas determined on the basis of population density. See Faggio and Silva (2011) for more details. This sample includes approximately 87% of the observations. Column (4) excludes London; sample includes around 88% of the observations. Urban and rural sample includes approximately 79% and 21% of the observations respectively (Columns (5) and (6)). Regressions in Colum (7) exclude the following sectors: Agriculture; Fishing and Forestry; Mining; Electricity, Gas and Water; Public Administration; Household with Employees; and International Organizations. Sample includes approximately 88% of the observations. Samples including only workers in manufacturing and services include approximately 24% and 56% of the observations, respectively (Columns (8) and (9)).

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