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-effect 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.

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1 Introduction

Over the past decades, most developed countries have adopted tax policies aimed at promoting homeownership. Government-induced incentives include tax relief on 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. While tax subsidies to homeowners are expensive, they may be justified on economic grounds if the social benefits associated with homeownership are large.[1]

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 statistically more significant when focusing on entrepreneurs who employ dependent workers or hold managerial and professional positions. This indicates that homeownership is negatively linked to genuine entrepreneurship and not to 'self-employment out of necessity' or as a last-resort option (Alba-Ramirez, 1994; Martinez-Granado, 2002).

Our main 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. Stated differently, investments in homeownership crowd out investments in entrepreneurship.

In order to document these facts, our empirical analysis exploits 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 and tenure choices, coupled with information on time-varying background characteristics. We exploit this data to estimate regressions that include individual fixed effects and isolate the precise timing of individuals' transitions into homeownership and entrepreneurial jobs.

Our cross-sectional OLS regressions 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. This negative link is stronger when focusing on homeowners with mortgages, and 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 exacerbate portfolio distortions due to undiversified risk of investment in housing, and 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 positive and significant as time goes by.

In order to provide further evidence in support of our explanation based on portfolio considerations,

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[1]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.

we directly assess whether homeowners shy away from more risky entrepreneurial ventures. 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.

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. However, for this explanation to hold true, we should detect a positive relationship between house price increases and entry into entrepreneurship. This is because, as home values increase, LTV ratios are pushed down. We find, however, that local house price variations have no explanatory power in our analysis and cannot 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 results.

1.1 Related literature

Our findings contribute to three strands of the literature: the external effects of homeownership on socio-economic and labor 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; Hilber, 2010), better control of local governments (Fischel, 2001; Dehring and Ward, 2008), higher attention towards environmental issues and childrens 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 labor market outcomes. 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. However, Munch and Svarer (2006) and Battu and Phimister (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 drawbacks of homeownership as well as the economic costs of excessive leverage and negative equity. Ferreira et al. (2010, 2011) suggest that owners in negative-equity are significantly less mobile. They 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 labor market decision: it is also an investment choice to be analyzed in the context of portfolio decisions. From this perspective, housing plays a very prominent—although distorting—role. Henderson and Ioannides (1983) are the first to formulate the proposition that owner-occupiers overinvest in housing, while Brueckner (1997) shows 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 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 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 investigates the role played by credit constraints in the decision to become an entrepreneur (Evans and Jovanovic, 1989; Holtz-Eakin and Rosen, 1994; Blanchflower and Oswald, 1998; Taylor, 2001; Michelacci and Silva, 2007; Fairlie and Krashinsky, 2011). 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 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 liquidity constraints play a major role in determining who sets up a business, and that capital-market failure holds back enterprise.[2] A remarkable dissenting view is provided 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 entrepreneurial venture. This result, which we replicate 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 (2012). 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 the 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 (2012) 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 at first glance Wang (2012)'s results appear to be in contrast with ours, the specific institutional context and workings of the policy can account for these differences.[3]

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

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[2]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 have 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).

[3]First, as noted by Wang (2012), 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, Wang (2012) 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.

and entrepreneurship. Section 4 explores different mechanisms and explanations for our key results. Finally, we provide some 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 households' tenure choices and characteristics, as well as on individuals' 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 quite successful in following the same individuals over time, even when they move residence or 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 labor force status. If they are working, detailed information about their occupation is collected. Survey respondents are also asked whether their labor force status has changed since their last interview. If the answer is positive, a set of detailed questions is asked about all the occupational spells occurred between the interview taking place and September of the previous year.[4]

The way in which the BHPS is structured makes it possible that some inconsistencies arise in the description of the same labor force spell provided by the same person in two different waves. Several authors have discussed the complicated task of reconstructing detailed monthly spells from the BHPS (Paull, 2002; Maré, 2006). 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 and Phimister (2008). We provide a detailed description of our procedure in Appendix A.

In order to identify the effect of homeownership on entrepreneurship, we need information about individuals' tenure choices with special attention to the timing of events. We first gather information about respondents' present tenure status. The possible categories are: homeowner with mortgage, homeowner without mortgage, private tenant, and social tenant.[5] We then use the date in which respondents say they moved to their present address to identify the timing of changes in an individuals 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.[6] Other controls—such as education level, age, marital status and number of children—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

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[4]In their first wave respondents are asked whether their labor forces status has changed since 1st 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 labor history of the respondents since one year before their first interview to present.

[5]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.

[6]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.

in Wave 1 or one of the following waves. We 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 labor force status for every month combined with precise information on her tenure status. It is not possible to reconstruct in-between labor market spells and tenure choices for people who skip an interview. In Wave 1 (1991) we have 9,892 individuals. 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 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' 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 labor 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 detail when trying to disentangle the mechanisms that explain our findings. Our main results are virtually identical if we include 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 5,200 individuals.

The richness and detail of the dataset is a crucial and novel element of our analysis. Most panel-type studies of entrepreneurship (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 individuals 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:

Notes: The sample only includes heads of household aged between 20 and 55 living in England. Summary statistics of control variables refer to the sample where all controls are non-missing. Number of observations: 366,168. Number of individuals: 5,193. Panel is unbalanced. 'Entrepreneur: all' include all entrepreneurs (self-employed); '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 55 or above.

Variable	Mean	Std. Dev.
Panel A: Entrepreneurs + homeowners		
Entrepreneurs: all	0.144	0.351
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
Panel B: Controls		
Age	39.38	8.95
Male	0.788	0.409
Household total income (previous year)	31,728.0	22,046.3
Individual total income (previous year)	20,990.0	16,144.4
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

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 on average 14.4% of individuals are self-employed, while the shares of entrepreneur: dependent and entrepreneur: manager are smaller at 4.7% and 7%, respectively. The fraction of homeowners in the monthly-spell data is 81%. 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 and 8.4% represent public renters. Panel B shows that the average individual is 39.4 years old, males represent 79% of observations, and individual and household total incomes stand at £20,990 and £31,728 respectively.

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 and Phimister (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%).[7] 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

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[7]These small difference are most likely explained by the fact that our analysis stretches up to 2008 (whereas theirs stop at 2003), and our sample includes individuals whose first interview was not in Wave 1 (whereas they focus their analysis on the original members of the survey).

and in and out of entrepreneurial jobs since this information is relevant for our fixed-effect identification. Summary statistics are presented in Appendix Table 7. 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 transitioning 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.

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), 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 (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 below.

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:

$$Entrep_{ilt} = \alpha_i + \beta\ own_{ilt} + \phi_l + \omega_t + \varepsilon_{ilt} \tag{1}$$

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 own_{ilt}. The subscript ilt identifies individual i living in location l at time t. X_{ilt} is the set of time-varying controls discussed above and described in Table 1, while ϕ_l and ω_t represent location and time fixed effects. Location fixed effects (ϕ_l) include persistent geographical disparities in labor and housing markets and differences in local political and institutional factors, whereas the time fixed effects (ω_t) capture unobserved factors that are specific to the year and/or month of interview. Finally, α_i captures unobserved individual factors —such as ambition and risk tolerance—which could simultaneously determine occupational choice and tenure status. The error-term ε_{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

Notes: The sample only includes heads of household aged between 20 and 55. Number of observations: 366,168. 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-effect 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 1. 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.

	(1) OLS	(2) OLS	(3) FE All. Trans.	(4) FE All. Trans.	(5) FE All. Trans.	(6) FE Trans. In	(7) FE Tran. Out
Panel A: Entrepreneur, all							
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)
Panel B: Entrepreneur, dependent							
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)
Panel C: Entrepreneur, manager							
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)
Time dummies (Year and month)	✓	✓	✓	✓	✓	✓	✓
Sector dummies	✓	✓	✓	✓	✓	✓	✓
LPA dummies		✓		✓			
Controls		✓		✓	✓	✓	✓

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.[8]

Columns (1) and (2) of Table 2 present simple cross-sectional (OLS) estimates 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%.

However, cross-sectional regressions cannot control for individuals' unobservables—α_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

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[8]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 labor 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.

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 individuals wealth. This is an 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 and Rosen, 1994; Blanchflower and Oswald, 1998). Finally, in Column (5) we retain the set of controls included in Column (4), but 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 Columns (4) and (5) confirm the intuition gathered from Column (3): there is a significant negative association between homeownership and the probability of self-employment, and this effect is larger when focusing on more stringent definitions of entrepreneurship, namely self-employed workers with dependent workers and in managerial/professional occupations.[9] 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 homeownership reduces an individual's chances of becoming an entrepreneur. However, as discussed above, some individuals transit into homeownership and some others transit out of it. 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 explore the relevance of this channel, in Columns (6) and (7) of Table 2, we focus on individuals spells that correspond to transitions into and out of homeownership, respectively. In Column (6), we follow individuals who start off as renters and then become homeowners till 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.

One concern with our fixed-effect identification strategy is that it partials out individual, family, location and 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, 1989; 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, as these are serious concerns, we subject our findings to a large number of robustness checks.

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[9]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.

3.2 Robustness Checks

Our robustness checks are reported in Appendix Table 8 for entrepreneurs with dependent workers only. We focus on this definition as we believe it 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. Column (1) reveals that excluding short employment spells does not affect our estimates. 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 Column (3), we assess whether our results may be driven by the geographical mobility of workers upon becoming homeowners. One concern is that individuals who choose to purchase a house might leave urban areas and this might affect their chances of becoming entrepreneurs. Previous evidence shows that more properties are rented as opposed to owner-occupied in riskier urban centers (Hilber, 2005), and that more entrepreneurs tend to cluster into denser cities because of agglomeration and localization economies (Glaeser and Kerr, 2010; 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 subset 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. One related consideration is that our results may be driven by London, where many entrepreneurial activities tend to concentrate and more people tend to rent. In Colum (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. 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, 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 and Phimister (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 8, 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

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TABLE 3: ENTREPRENEURS WITH DEPENDENT WORKERS—TIMING AND DYNAMICS

Notes: 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.

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

activities in the US and UK, respectively. When doing 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 we focus on services. However, the point estimates are comparably 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%. Nevertheless, the coefficient in Column (9) still implies an economically meaningful 12.5% negative effect of homeownership on entrepreneurship.

3.3 Dissecting the Fixed-Effect Results: 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 whether this link takes some time to build and then dissipates over time.

We present evidence on this point in Table 3. Recall that for approximately 7% of the individuals we cannot 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%. In the remaining analysis in this section, we focus on people with non-imputed transition dates.

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 borderline significant (-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. 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 Table 3, 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 the effect is delayed (Column 4). Next, we analyze whether the negative link happens upon transition into homeownership, or 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 predict that they will soon purchase a house and shy away from entrepreneurial occupations before actually changing their tenure status. However, an F-test of the coefficients cannot reject the null hypothesis that the leads are jointly insignificant (p-value of 0.212).

The dynamics of the effect of homeownership on entrepreneurship are an important issue that could assist the interpretation of our results. Hence, we further investigate this point 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.[10]

Although this approach is very flexible in analyzing anticipation and long-lasting effects of homeownership, one 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 4,300 individuals (out of the original 5,193 workers), further dropping to around 270,000 for 3700 individuals when focusing on 36-months leads/lags.

Nevertheless, the main insights from this analysis support 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

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[10]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 centers the controls at the time of the employment transition. This second method gave nearly identical results.

FIGURE 1: DYNAMIC EFFECT OF HOMEOWNERSHIP—DURATION, LEADS AND LAGS

Notes: 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.
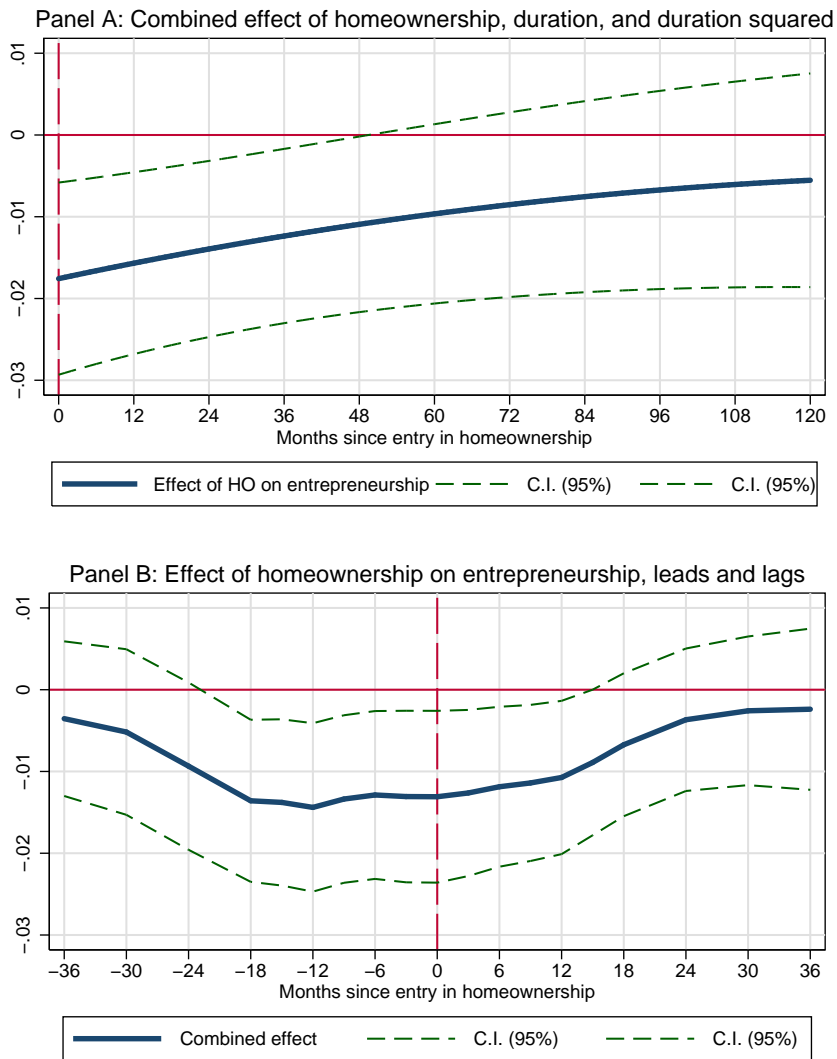

Panel A: Combined effect of homeownership, duration, and duration squared

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

Notes: 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 individuals 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 individuals 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.

	(1) FE	(2) FE	(3) FE	(4) FE	(5) 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)*
First-stage: Coeff. (s.e.) on instrument	–	–	–	–	0.829 (0.013)
T-Stat on instrument	–	–	–	–	63.93

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. This pattern is 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. 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.[11]

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

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[11]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.

its effect is estimated to be precisely zero. As for the LTV ratio, it 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%.

A complementary way to evaluate the importance of leverage is to measure whether homeownership with and without a mortgage has a differential effect. 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 likely to become entrepreneurs than renters. The coefficient is negative, albeit completely statistically insignificant. In contrast, individuals purchasing a house with a mortgage are significantly less likely to become entrepreneurs. The difference between the effects of homeownership with/without a mortgage on entrepreneurship is significant with a p-value of 0.081. The implied economic magnitude is also non-negligible: becoming a homeowner with a mortgage reduces the probability of becoming an entrepreneur by approximately 30%.

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: 0.13) 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 as 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 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 25th percentile of the LTV distribution (0.283) to the 75th percentile (0.684) reduces the chances of an entrepreneurial spell by 12%.

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. Furthermore, the LTV at which an individual borrows as well as the LTV on the outstanding mortgage may be 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 (Muelbauer, 2002). Our fixed-effect strategy controls for an individual's time-fixed unobserved attitudes such as risk-tolerance or financial sophistication. However, one might worry that our estimated effects are upward biased by unobservable changes in an individuals preferences or financial circumstances.

To address these concerns, we devise an instrumental variable strategy that exploits information on the LTV of newly originated mortgages in the LA of an individuals residence obtained from the Survey of Mortgage Lenders (SML).[12] Specifically, we construct an instrument which is set to zero before an

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[12]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,

individual becomes a homeowner and equal to the time-varying local LTV in the LA of an individuals 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-effect 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 a strong and positive link between an individuals LTV and local prevailing housing market conditions. 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. 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 perform 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 not particularly strong at -0.483. In any case, the inclusion of this additional control does not affect our results. Further, we check 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 include in our specification information on house prices at the LA level. This inclusion does not affect our results. Finally, we run specifications that focus on individuals who do not change the LA of residence during the period of analysis. Results using this subset of workers are similar to those tabulated in Table 4, although the effects are 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 show that leveraged homeownership is negatively associated to entrepreneurship and this effect is both statistically significant and economically meaningful. This finding can be rationalized by overinvestment in housing: purchasing a house tends to concentrate wealth into one single asset, which implies that individuals cannot adequately diversify their investment risk. As a consequence, individuals choose not to start-up their own business venture since this would imply taking on significant additional risk.[13] In the next subsection, we go on to provide more direct evidence supporting 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.[14] In particular, we assemble data on industry-level profits and investment (capital spending) per employee in the UK.

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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.

[13]Our results are consistent with Davidoff (2006) who shows that individuals whose labour income co-varies strongly with housing values purchase relatively inexpensive homes or rent.

[14]This can be accessed at 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.

Notes: 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.

	(1)	(2)	(3)	(4)	(5)	(6)
	Profit Variability		Cost Sunkness		Overall Risk	
	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median
Homeowner, mortgage	-0.024	-0.004	-0.013	-0.013	-0.015	-0.002
	(0.011)*	(0.08)	(0.006)*	(0.012)	(0.006)*	(0.009)
Homeowner, outright	0.003	-0.009	0.001	-0.007	0.006	-0.003
	(0.020)	(0.013)	(0.012)	(0.022)	(0.015)	(0.017)

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.[15] Our second proxy capturing sector-specific risk is the average investment (capital spending) per employee for the available period. This variable measures the sunk component (irreversible) of a companys 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.[16] This variable is at its highest when profits are highly volatile and sunk costs are significant.

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 sub-samples to investigate whether the negative effect of homeownership is more pronounced and significant in industries characterized by more risk.[17] To emphasize the role of leverage, we still distinguish between homeowners with a mortgage and outright homeowners.

Our results are displayed in Table 5. 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.

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

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[15]This is simply obtained by dividing the standard deviation of profits within-sector over-time by average profits within-sector over-time.

[16]A similar proxy is devised by (Picchizzolu, 2010), who uses Eurostat data to investigate entrepreneurial risk and industrial concentration in the UK.

[17]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.

leveraged homeownership is more significant for individuals working in risky sectors as proxied by the 'sunkness' of their investments.

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 indicate 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%.

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, they instrument home equity and mortgage exposure using both current and initial average house prices in an individuals 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 a 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 (2010)'s 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-effect approach, we find a negative association between homeownership and risky investments, which is more pronounced for homeowners with mortgages. However, none of the fixed-effect estimates is significant at conventional levels. We attribute this to the lack of repeated observations (only approximately 700 out 2600 individuals appear in more than one of the three waves). Finally, we replace leveraged homeownership with the proxy for the LTV on the outstanding mortgage described above and instrument the latter using local LTV values, as discussed in Section 4.1. 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%. This effect is not statistically significant at conventional levels (coefficient: -0.126; s.e.: 0.128). However, the sign and size of the coefficient are consistent with Chetty and Szeidl (2010)'s findings.

Notes: 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.

	(1) FE	(2) FE	(3) FE	(4) FE	(5) 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	–

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: 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 suggest that credit constraints cannot rationalize our findings. These estimates are reported in Table 6.

If an explanation based on credit constraints and housing is to hold true, 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, 2012). 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. 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 effect on our main finding: 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 individuals residence between the time when 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, allowing us to explicitly test whether the equity position built into someones 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.

In Column (4) 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, using more aggregated house price data 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. Moreover, 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 investigate 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 include 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 does not alter our key finding. Moreover, these proxies do not enter our regressions with significant and consistently signed coefficients.

This set of tests suggests that credit constraints are not the main mechanism behind the novel finding documented in our paper. More generally, the estimates discussed in this section cast some 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 an entrepreneur (see introductory section) it is 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.[18]

5 Conclusion

In this paper, we study the previously largely 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 analyses of the labor market effects of homeownership have focused on employment and unemployment opportunities, thus neglecting an important channel whereby housing policies might affect the country-wide economic performance.

To carry out our analysis, we use information from the BHPS to construct a monthly dataset that tracks an individual's job history and tenure choice. We exploits 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 suggests a positive and significant correlation between homeownership and various measures of self-employment and entrepreneurship. However, our 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. 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 captures a negative link between homeownership and genuine entrepreneurship rather than self-employment out of necessity.

Furthermore, we find that this effect is stronger for homeowners with a mortgage. 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 individuals 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 (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

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[18] Disney and Gathergood (2009) replicate Hurst and Lusardi (2004) results using BHPS data, with an analysis similar to the one presented here. Further, Taylor (1999) finds that housing equity does not affect the duration of self-employed ventures. This micro-evidence is in contrast with the findings documented by studies that use more aggregate data. For example, Black et al. (1996) exploit information on new company registrations to document a positive link between house prices and entry into entrepreneurship at the national and regional level. Similarly, Blanchflower and Shadforth (2007) show that regional self-employment rates are positively correlated with local house prices. These discrepancies could be explained by unobservables which are better controlled for using micro-econometric techniques.

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 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.

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Appendices

A Construction of Monthly Job Histories from the British Household Panel Survey

In what follows, we provide a description of the way we construct monthly job spells and 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 and Phimister (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 labor force and occupational status—and if they have changed their labor 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 labor 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):

1. We open the 'job history file' and the 'respondent file' and carry out some consistency checks in both of them separately (more details below);

2. 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;

3. 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.

4. 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 'labor spell file'.

In the original BHPS data, every labor 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 ; (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 center 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.

B   Additional Tables

TABLE 7: TRANSITIONS INTO AND OUT OF HOMEOWNERSHIP AND ENTREPRENEURSHIP

Notes: 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.

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

	(1) Excluding Short Spells	(2) Including UN Spells	(3) Immobile Workers	(4) Excluding London	(5) Urban Areas	(7) Rural Areas	(7) Excl. Selected Sectors	(8) Services Only	(9) Manuf. Only
Homeownership	-0.013 (0.005)**	-0.013 (0.005)**	-0.01 (0.006)+	-0.014 (0.006)*	-0.01 (0.006)+	-0.015 (0.013)	-0.012 (0.006)*	-0.013 (0.007)*	-0.003 (0.004)