Prices, Rents, and Homeownership: Three Essays on Housing Markets

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorization does not, to the best of my belief, infringe the rights of any third party.

London, July 25, 2012

Philippe Bracke

Statement of conjoint work

Chapter 2 of this thesis was jointly co-authored in equal shares with Christian Hilber and Olmo Silva.

London, July 25, 2012

Philippe Bracke

Contents

List of Tables

List of Figures

Abstract

This thesis includes three self-contained chapters whose common theme is the analysis of house price and rent movements, and how these movements influence the economic actions of individuals.

In Chapter 1, I analyse a micro dataset on housing sales and rentals in Central London. I show that the ratio between prices and rents differ across property types: bigger and better located properties have higher price-rent ratios. These differences in price-rent ratios can be explained through a hedging model where households avoid rent risk by increasing their demand for homeownership. Consistently with this hypothesis, I find that rental prices for bigger properties and properties in more expensive neighbourhoods are not growing significantly faster than for other properties, but are more volatile.

In Chapter 2, together with my two coauthors Christian Hilber and Olmo Silva, I study the relationship 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 histories and tenure choices, 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 relationship can be rationalised by portfolio considerations: leveraged housing investments crowd out entrepreneurial investments. Alternative explanations based on credit constraints find little support in our data.

In Chapter 3, I analyse the duration of house price upturns and downturns in the last 40 years for 19 OECD countries and provide two results. First, upturns display duration dependence: they are more likely to end as their duration increases. Second, downturns display lagged duration dependence: they are less likely to end if the previous upturn was particularly long. Both these facts are consistent with a boom-bust view of housing price dynamics, where booms represent departures from fundamentals that are increasingly difficult to sustain, and busts serve as readjustment periods.

To Valeria,

for her continuous love and support

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In summer 2010 I did an internship at the IMF in Washington, D.C. This experience allowed me to start a research project that ended up being the third chapter of this thesis.

More importantly, I had the chance to meet and interact with great people such as Prakash Loungani, Deniz Igan, Giovanni Favara, and my friend Yi Huang.

Shortly after my experience at the IMF I had the chance to meet James Wyatt of John D Wood & Co., who not only has the practical instinct of someone who has spent years in the "real" housing market, but also has the intellectual acumen and curiosity typical of successful researchers. He granted me access to the data of his company and provided all the assistance I needed to pursue my research. He deserves my gratitude: the first chapter of this thesis could not have been written without his help.

I will never forget that my pursuit of a PhD was made possible by the generous help of sponsors: the Warnford Group, Associazione Borsisti Marco Fanno, and Luca Arnaboldi. I am extremely grateful to all of them.

I want to thank my parents Luc and Elena and my sister Paola for the confidence with which they have always supported me in everything I do. Their presence is for me a source of strength.

Lastly, this thesis is dedicated to Valeria. Her love, patience, understanding, and sense of humour have illuminated each day of these years.

Preface

Housing has a pivotal role in economic life. Every household needs to consume housing services, either by renting or owning a property. The costs involved in both these options are substantial. For most tenants, rents represent the greatest monthly expenditure item. For most homeowners, real estate is the largest portfolio asset. The common theme that bounds together the chapters of this thesis is the following: movements in house prices and rents are an important source of economic risk for individuals.

In Chapter 1, I use a novel proprietary dataset from a Central London real estate agency to analyse housing sales and rentals at the unit level. I show that the ratio between prices and rents differ across property types: properties that are bigger and located in more expensive neighbourhoods have higher price-rent ratios. These same properties tend to be part of a thinner rental market and, consequently, have more volatile rents—this pattern is documented by looking at the historical growth and volatility of prices and rents for different property categories. While price-rent ratios and volatilities are concepts that come straight from the financial literature, the mechanism highlighted in Chapter 1 is specific to the housing market. Price-rent ratios differ across property categories not because expected rent growth rates or risk premia are significantly different—as predicted by a standard dividend discount model—but because individuals prefer homeownership for the types of properties were rent risk is perceived to be higher. In addition to making this point, Chapter 1 also discusses the use of various econometric methods to analyse datasets where both price and rent micro data are available.

In Chapter 2, together with my two coauthors Christian Hilber and Olmo Silva, I study whether being a homeowner influences the probability of becoming an entrepreneur. Homeownership can be seen as a risky investment because it usually involves a substantial amount of cash upfront plus a mortgage. Starting a business is obviously a high-risk activity on its own, and could conflict with the sense of security that homebuyers are looking for. Consistently with household portfolio models, we find that individuals who have bought a house are less likely to switch to an entrepreneurial job, especially in the years surrounding the purchase of their property.

In Chapter 3, I analyse macro data on national house prices. Using the econometric techniques of duration analysis, I show that long housing booms are more likely to end as they get longer. Moreover, the duration of housing busts depends on the length of the preceding booms. These findings imply a rejection of the hypothesis that aggregate house prices follow a random walk. The existence of predictable patterns in housing cycles is of great interest both for policy makers and portfolio managers.

All three chapters can be characterised as applied economics. Different econometric techniques are used to verify whether real world data conform to some economic theory. The scale of the analysis varies. Chapter 3 studies the most aggregate dataset: nation-level house prices over 40 years. Chapter 2 focuses only on one country, the United Kingdom, and 18 years (from 1991 to 2008). This reduced geographical and temporal coverage comes with the benefit of detailed data on the socio-demographic characteristics and labour market choices of thousands of individuals. Finally, Chapter 1 has the most granular data: tens of thousands of housing sales and rentals, ordered by transaction day and address, for a specific area of London.

Each scale of analysis reveals interesting insights that are hidden at other levels. The housing market is a complex, multi-dimensional object of study, which requires to be analysed from different angles. We are lucky to live in an era when data of all sorts are becoming available. It is the duty of an economist to exploit all these sources to move our understanding of the world forward. I hope that this thesis can be a small step in this direction.

Chapter 1

House Prices and Rents: Micro Evidence from a Matched Dataset in Central London

1.1 Introduction

The value of the entire stock of housing stands at 16 trillion dollars in the U.S. and 3.9 trillion pounds in the U.K.,[1] making housing the biggest item among households' assets.[2] Movements in house prices have considerable impact on economic welfare, as demonstrated by historical crises episodes (Reinhart and Rogoff, 2009) and the recent recession (Mian and Sufi, 2011).

The sale price of a housing unit represents the market value of the flow of current and future housing services that the unit will provide. The existence of a rental market allows the distinction between the forward-looking element of house prices and the current value of housing services. The house price booms and busts that characterise many economic crises are not equally pronounced for rents. In fact, the recent housing boom was characterised by a significant rise in the price-rent ratio (Campbell et al., 2009), and historically rents are less volatile than house prices (Gallin, 2008), as dividends are less volatile than stock prices (Shiller, 1981). Understanding and modeling price-rent ratios is therefore crucial to improve our knowledge of house price movements.

In this chapter I study unit-level data on house prices and rents in Central London. I

[1]U.S. data from the Federal Reserve Board's Flow of Funds Accounts, (table B100, number 49). U.K. data from `http://www.lloydsbankinggroup.com/media/pdfs/halifax/2012/1102_value.pdf`

[2]In 2008 residential real estate constituted 39% of households' assets in the U.K. (Survey of Assets and Wealth) and 29% of households' assets in the U.S. (Flows of Funds).

document the existence of systematic differences in price-rent ratios across property types within the same urban area. Bigger properties and properties located in more expensive neighbourhoods have higher price-rent ratios. These micro-level patterns are useful to assess competing theories on the functioning of housing markets.

According to the standard dividend discount model, properties with higher price-rent ratios should feature higher expected rent growth, higher expected returns, or both. As noted by Sinai and Souleles (2005), the dividend discount model ignores that housing is a necessary consumption good and all households must either rent or own. From this perspective, higher price-rent ratios can also be due to higher rent volatility, which induces people to choose homeownership as a way to lock in future rents. In places with inelastic housing supply, this insurance motive results in higher price-rent ratios rather than higher homeownership. I refer to this model as the "hedging model".

Consistently with the hedging model, I find that in Central London rent growth rates of bigger properties are not different from those of smaller properties, but their volatility is significantly higher. Similarly, rents are not growing faster in more expensive neighbourhoods, but are more volatile.

Both the dividend discount model and the hedging model have been repeatedly tested using aggregate data. For instance, Gallin (2008) uses city-level data to check if price-rent ratios predict future price or rent movements in accordance with the dividend discount model. Sinai and Souleles (2005) show that, across cities, higher rent volatility is associated with higher price-rent ratios—which supports the hedging model. Using data at the individual property level, I am able to expand the study of these models in two directions. First, I take into account the differences that exist across property types. These differences are likely to be substantial: according to the housing-ladder model of Ortalo-Magné and Rady (2006), more expensive properties have more volatile prices. Second, I measure both aggregate and idiosyncratic risk. By their nature, aggregate analyses ignore idiosyncratic risk. However, the balance sheet of most homeowners contains just one property (Flavin and Yamashita, 2002), and most renters are obviously subject to just one rental contract (Genesove, 2003).[3]

Using micro local data to infer general features of housing markets is a common approach in housing research. For instance, Glaeser et al. (2005) use data on Manhattan condominium sales to study the relation between supply regulation and house prices. Guerrieri et al. (2010) analyse house prices at the zip-code level in a group of U.S. cities to propose a model

[3]According to the U.K. Wealth and Assets Survey, only 10% of households own property other than their main residence. Similary, the English Private Landlord Survey of 2010 reveals that 78% of landlords owns just one property for rent.

of neighbourhood gentrification. However, despite the importance of rents, their analysis at the micro local level has been so far limited, due to the lack of reliable data.

My analysis is based on a novel proprietary dataset from a Central London real estate agency. The dataset contains information on achieved prices and rents for tens of thousands of properties, as well as detailed descriptions of property characteristics. The period of analysis, 2005 to 2011, covers the last part of the housing boom, the bust of 2008, and the subsequent recovery.[4] The area under study contains a mix of owner-occupied and private-rented properties, which often lie side by side. The UK private rental market is essentially unregulated,[5] which ensures that observed prices and rents are the result of genuine market mechanisms. Moreover, Central London has one of the most restrictive construction regulations in the world (Cheshire and Hilber, 2008), which implies that higher housing demand translates into higher prices rather than more buildings.

In terms of empirical method, I use hedonic regressions to estimate average prices and rents within cells of observationally equivalent properties. Since hedonic regressions cannot control for unobserved characteristics, and these could differ between sold and rented dwellings, I also run a restricted analysis with properties that are both sold and rented out within 6 months. In this way I am able to measure price-rent ratios exactly and confirm the results of hedonic regressions that use the whole dataset. To measure idiosyncratic risk, I restrict attention to properties that are sold or rented at least twice.

Both the dividend discount model and the hedging model are based on people's expectations of future rents. Hence, I complement my empirical analysis with an expectation survey. This online survey was sent to the members of the mailing list of the real estate agency that provided the price and rent dataset. The survey answers confirm that rent expectations are more uncertain for properties located in more expensive neighbourhoods.

The Central London housing market is a combination of different submarkets. Households looking for small properties face thick markets both in sales and rentals. By contrast, households looking for big properties face a thin rental market and are pushed toward buying. Thin markets are more volatile and, as in Ngai and Tenreyro (2009), are less likely to generate good matches between property characteristics and people's tastes. While Ngai and Tenreyro look at the thick vs thin market distinction over time, I look at it over the cross-section of property types.

[4]Differently from many advanced economies and the rest of the United Kingdom, nominal house prices in Central London are currently higher than in the previous peak (2007).

[5]The most common form of rental contract, the "assured shorthold tenancy", leaves landlords and renters free to renegotiate any rent at the end of the rental period (usually one year). See `http://www.direct.gov.uk/en/HomeAndCommunity/Privaterenting/Tenancies/DG_189101`

The results of this chapter are relevant both for consumers and professional housing investors. Returns to housing are given by the sum of capital gains and rental yields, where rental yields are defined as the inverse of price-rent ratios. The finding of different rental yields across property types is useful for real estate investors' portfolio management (Plazzi et al., 2011). Moreover, the recent crisis has interrupted the upward trend in homeownership (Gabriel and Rosenthal, 2011). The prospect of more concentration in the ownership of the housing stock makes these portfolio considerations all the more relevant.

The rest of the chapter is organised as follows. Section 1.2 discusses the empirical methodology of the paper and Section 1.3 describes the data. In Section 1.4 I present the main results of this chapter: the differences in price-rent ratios across property types, the growth rates and market volatilities of prices and rents, and their idiosyncratic volatilities. Section 1.5 discusses a version of the hedonic regressions with time-varying coefficients and the survey results. Section 1.6 concludes.

1.2 Empirical Method

The log price of a house i at time t can be modeled as the sum of three elements:

$$p_{it} = q_i + \lambda_t + u_{it}, \tag{1.1}$$

where q_i represents the quantity of housing services that the house provides (the "quality" of the house), λ_t is the quality-adjusted price for one unit of housing services at time t, and u_{it} is an idiosyncratic shock centered around zero. The first term varies across properties but is constant over time; the second term is constant across properties but varies over time; and the third term captures property- and time-specific shocks.

Housing is a composite and heterogeneous good and every property represents a different combination of characteristics. Hence q_i can be decomposed as follows:

$$q_i = \beta^* X_i + \gamma^* Z_i, \tag{1.2}$$

where X_i is a vector of observed characteristics and Z_i is a vector of unobserved characteristics. This formulation is at the basis of the hedonic method, which consists in regressing the price of composite goods on their characteristics (Court, 1939; Griliches, 1961). In the context of housing, assuming that the market for properties is competittive and property characteristics enter the utility function, the coefficients β^* and γ^* represent the shadow prices of an additional unit of each characteristic (Rosen, 1974).

By assumption, the prices of characteristics are held fixed over time: all time variation is captured by λ_t. The resulting regression is commonly referred to as the "time-dummy" hedonic regression (Hill, 2012). A more general model would allow the price of each characteristic to change over time, making the aggregate price index λ_t redundant. In the Extensions Section I briefly explore this more general formulation. In the main part of the paper, I stick to the time-dummy approach, which conveniently separates cross-sectional and time variation. Moreover, since the analysed dataset covers only 7 years, from 2005 to 2011, changes in the relative prices of characteristics are likely to be limited.

In empirical work the vector Z_i is unobservable. The estimated model is therefore:

$$p_{it} = \beta X_i + \lambda_t + \varepsilon_{it}. \tag{1.3}$$

The resulting coefficients are affected by the omitted variable bias. For instance, the coefficient β is equal to $\beta^* + \gamma^* \phi_X$, where $\phi_X = (X'X)^{-1}X'Z$.

1.2.1 Comparing price-rent ratios across property types

The dataset used in this paper contains information on both sale prices and rental prices. To distinguish between the two, I use the subscripts s for sales and r for rentals. Equation 1.3 becomes:

$$p_{hit} = \beta_h X_i + \lambda_{ht} + u_{hit}, \tag{1.4}$$

where $h \in \{s, r\}$. This formulation allows for quality, quality-adjusted prices, and errors to differ between prices and rents. Different coefficients in the price and rent hedonic equations imply an effect of the regressors on price-rent ratios, because $\mathrm{E}p_s - \mathrm{E}p_r = \mathrm{E}(p_s - p_r)$.

Using the omitted variable bias formula, the difference between β_s and β_r computed from the hedonic regressions is:

$$\begin{aligned} \beta_s - \beta_r &= \beta_s^* - \beta_r^* + \gamma_s^* \phi_{Xs} - \gamma_r^* \phi_{Xr} \\ &= \beta_s^* - \beta_r^* + (\phi_{Xs} - \phi_{Xr})\gamma_s^* + (\gamma_s^* - \gamma_r^*)\phi_{Xr} \end{aligned}$$

where the final step is obtained by adding $\phi_{Xr}\gamma_s^* - \phi_{Xr}\gamma_s^* = 0$ to the equation. The difference in the estimated coefficients is equal to the true difference in coefficients plus two terms—the first depending on the different types of houses that belong to the sales and rentals datasets $(\phi_{Xs} - \phi_{Xr})$, and the second depending on the different coefficients that regulate the relation between unobserved characteristics and log prices or rents $(\gamma_s^* - \gamma_r^*)$.

The dataset used for the empirical analysis contains properties that were both sold and rented within a short amount of time. For these properties, the price-rent ratio can be directly observed and can serve as dependent variable in the following regression:

$$y_{it} = \beta_m X_i + \lambda_{mt} + \varepsilon_{mit}, \tag{1.5}$$

which mimics the hedonic model and where $y_{it} = p_{sit} - p_{rit}$. For these properties, $\phi_{Xs} = \phi_{Xr}$ so the bias in measuring the effect of property characteristics on price-rent ratios is reduced to $(\gamma_s^* - \gamma_r^*)\phi_{Xr}$.

1.2.2 Measuring growth and aggregate risk by property type

Comparing β_s and β_r highlights differences in price-rent across property types. To test whether these differences are correlated with differences in rent growth rates (consistently with the dividend discount model) or rent volatilities (consistently with the hedging model), I modify the hedonic regression as follows.

Suppose there are two property categories, A and B, where categories are defined as partitions of the set of properties according to the value of one or more elements X_i. The hedonic equation that allows for different price growth across categories is:

$$p_{hct} = \beta_h X_c + \lambda_{hct} + \varepsilon_{hct},$$

where the quality-adjusted price components is now category-dependent: $c \in \{A, B\}$, and the i subscript is omitted to ease notation. In terms of estimation, this method amounts to interacting the time dummies with a dummy corresponding to one of the two categories. One can also interact the category dummy with all other property caracteristics and get:

$$p_{hct} = \beta_{hc} X_{ic} + \lambda_{ct} + \varepsilon_{hct}. \tag{1.6}$$

In this way the coefficients on property characteristics are allowed to be different across property categories. In practice, the two methods give nearly identical results, so in Section 1.4 I only show the output of Equation 1.6. The average growth rate for a given property category c is $\mathrm{E}(\lambda_{hct+1} - \lambda_{hct})$ and the corresponding aggregate risk is $\mathrm{Var}(\lambda_{hct+1} - \lambda_{hct})$.

1.2.3 Measuring idiosyncratic risk by property type

Suppose we observe the price of property i at time T and t. Differencing Equation 1.4 gives the log appreciation of property i:

$$p_{hiT} - p_{hit} = \underbrace{\lambda_{hT} - \lambda_{ht}}_{\substack{\text{aggregate} \\ \text{movement}}} + \underbrace{u_{hiT} - u_{hit}}_{\substack{\text{idiosyncratic} \\ \text{movement}}}. \tag{1.7}$$

Equation 1.7 constitutes the basis of the repeat sales method (Bailey et al., 1963; Case and Shiller, 1989), which allows for the estimation of the term $u_{hiT} - u_{hit}$. Simlarly to aggregate risk,idiosyncratic risk is defined as $\mathrm{Var}(u_{it+1} - u_{it})$. To estimate idiosyncratic risk from the estimates of $u_{hiT} - u_{hit}$ one must make assumptions about the time evolution of u_{it}.

Case and Shiller (1989) assume that $u_{it} = v_{it} + h_{it}$, where v_{it} is a white noise with mean zero and variance σ_v^2, and h_{it} is a random walk with mean zero and variance $t\sigma_h^2$. Under these assumptions, $\mathrm{Var}(u_{iT} - u_{it}) = 2\sigma_v^2 + \sigma_h^2(T-t)$ and $\mathrm{Var}(u_{it+1} - u_{it}) = 2\sigma_v^2 + \sigma_h^2$. Case and Shiller employ these volatility estimates to improve the efficiency of the repeat sales regression. They call their approach the weighted repeat sales estimator (WRS).[6] Idiosyncratic volatilities are however an object of interest per se (Wallace, 2010). The housing crisis has highlighted the importance of estimating the whole distribution of house prices in order to predict the number of mortgage defaults (Korteweg and Sorensen, 2012).

In this chapter I run separate WRS regressions to estimate idiosyncratic volatilities for different property categories. For some categories the number of repeat sales is low. This is less of a problem for rents, because repeat rents are more common than repeat sales.

1.3 Data

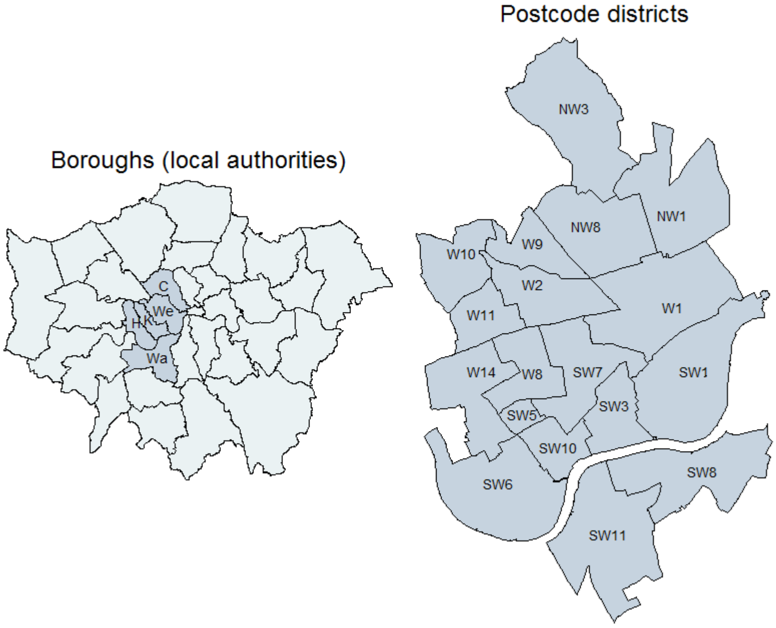
The dataset used in this paper comes from John D Wood & Co., a real estate agency that operates in London and the surrounding countryside.[7] I refer to these data as the JDW Dataset. The empirical analysis sometimes refers to subsets of the JDW Dataset: the Matched Dataset and the Repeat Transactions Dataset.

[6] In practice, the Case and Shiller (1989)'s procedure involves three steps: first, running an OLS regression to estimate Equation 1.7; second, regressing the resulting residuals on a constant (which will provide an estimate $2\sigma_u^2$) and the $(T - T)$ term (which will provide an estimate for σ_h^2); third, estimating Equation 1.7 again running a GLS regression where observations are weighted by the inverse of the square root of the predicted residuals.

[7] `http://www.johndwood.co.uk/`. John D Wood & Co. was established in 1872 and has now 20 offices: 14 in London and 6 in the countryside. UK real estate agencies provide several services ranging from assistance in selling properties to management of rental units. Big agencies have valuation teams whose duties include keeping track of market trends. Agents assemble sale and rental data from their own records as well as from other agencies.

Figure 1.1: Geographical coverage of JDW Datasets

Notes: The local authorities covered by the JDW dataset are Camden (C), Westminster (W), Kensington and Chelsea (K), Hammersmith and Fulham (H), and Wandsworth (W).



1.3.1 The JDW Dataset

The JDW Dataset includes observations from the Central-Western area of London. London is divided in 33 local authorities, which are responsible for running services such as schools, waste collection, and roads. The local authorities covered by the JDW dataset are Camden, Westminster, Kensington and Chelsea, Hammersmith and Fulham, and Wandsworth. These local authorities are shown on the left-hand side of Figure 1.1.

This area is one of the most densely populated in London. Most of the housing stock is made of flats rather than single-family houses. Approximately one fourth of dwellings are privately rented.[8] Appendix Table A.1 shows detailed statistics on the area, gathered from public sources.

A more detailed partition of this area can be obtained using postcode districts. In the U.K. postal code, the postcode district represents the first half of the postcode (one or two letters followed by one or two numbers) and corresponds to 10,000 - 20,000 unique addresses. The right-hand side of Figure 1.1 shows the postcode districts included in the JDW Dataset. In the empirical analysis, postcode district dummies are used in order to capture the effect of location on house prices.

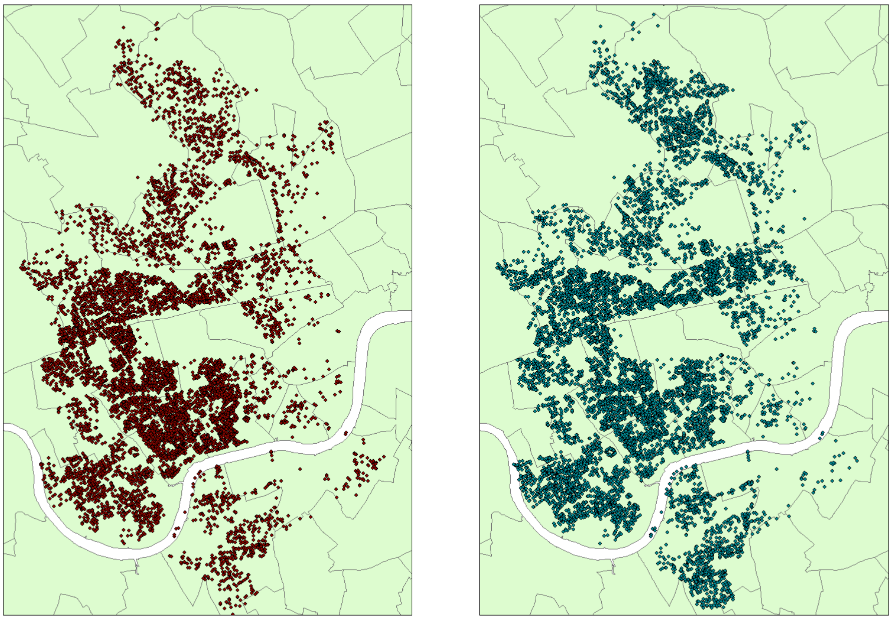
In terms of dataset preparation, to avoid duplicates, every sale or rental contract which

[8] In addition to the private-rented sector, 30% of the housing stock is rented at subsidised prices by local authorities or housing associations. This part of the market is not included in the JDW Dataset.

Figure 1.2: Observations in the JDW Dataset

Notes: Property addresses were geocoded using Google Maps.

(a) Sales (b) Rentals



refers to the same property and occurs within one month is excluded. This operation has the additional advantage of removing short-term rental contracts, which are usually more expensive and targeted to specific markets (e.g. business travellers and tourists). Moreover, since British houses can be sold on a leasehold—an arrangement by which the property goes back to the original landlord after the lease expires—I drop all sales of properties with a leasehold expiring in less than 80 years.[9] Finally, to avoid outliers, I trim properties whose price or rent is below the 1st percentile or above the 99th percentile of the price or rent distribution of their transaction year. Figure 1.2 plots the sale observations on the London map.

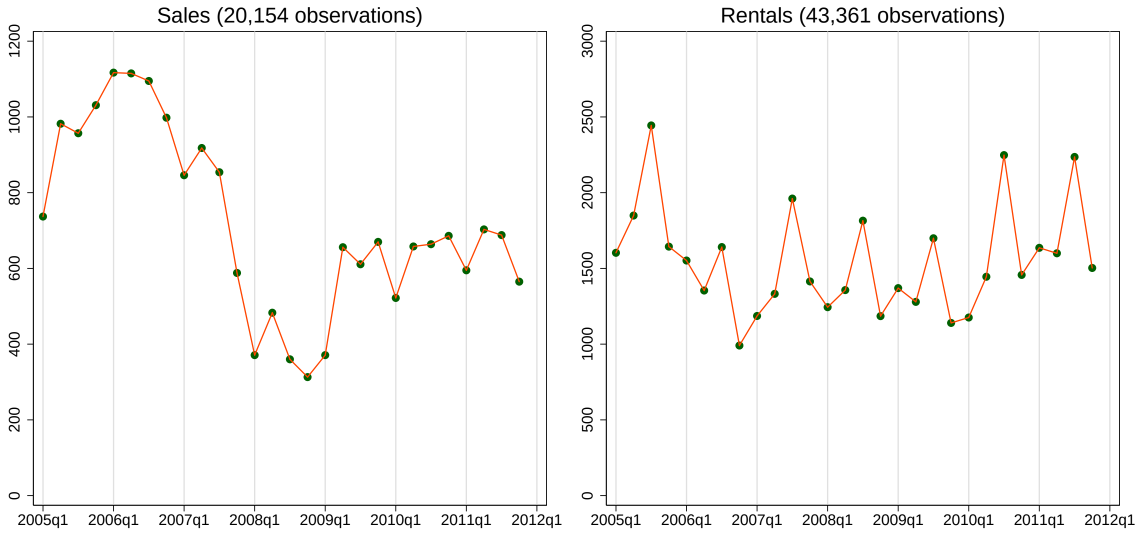
The JDW Dataset contains only a fraction of the housing units present in the whole Central London area. In Appendix A.1.3, I compare some of the features of the JDW Sales Dataset with the Land Registry, the record of all housing transactions in England and Wales, for the 2005-2010 period. Compared to the JDW Sales dataset, the Land Registry does not contain important information on housing characteristics, such as floor area. Moreover, the Land Registry is not a timely description of the market. The JDW Dataset assigns the date of a transaction to the day of the exchange. By contrast, it takes between 4 and 6 weeks for the Land Registry to list a transaction (Thwaites and Wood, 2003, p. 44).

[9]It is commonly believed that the price difference between a freehold (not subject to leasehold) property and a leasehold property is negligible for leaseholds longer than 80 years.

Table 1.1: JDW Datasets: Summary statistics

	Complete dataset		**Matched units**	**Repeat transactions**	
	Sales	Rentals	Sales & Rentals	Sales	Rentals
	(1)	(2)	(3)	(4)	(5)
Observations	20,154	43,361	1,661	1,233	18,710
Median price	694,323		532,746	920,000	.
Median rent		460	524		460
(in 2005 £; rent per week)					
Floor area (sqft)	1059	879	781	1245	863
Property type (%)					
1-bed flat	0.20	0.34	0.33	0.17	0.36
2-bed flat	0.35	0.39	0.41	0.29	0.38
3-bed+ flat	0.21	0.14	0.12	0.20	0.12
House	0.24	0.12	0.14	0.35	0.14
Postocde districts (%)					
NW1	0.03	0.02	0.03	0.03	0.02
NW3	0.03	0.05	0.05	0.02	0.04
NW8	0.05	0.03	0.04	0.04	0.03
SW1	0.15	0.13	0.15	0.15	0.14
SW10	0.07	0.04	0.06	0.09	0.05
SW11	0.03	0.04	0.03	0.01	0.03
SW3	0.09	0.10	0.10	0.14	0.12
SW5	0.03	0.04	0.06	0.04	0.04
SW6	0.06	0.07	0.08	0.03	0.07
SW7	0.09	0.08	0.08	0.12	0.09
SW8	0.02	0.03	0.02	0.01	0.03
W1	0.07	0.12	0.08	0.09	0.13
W10	0.01	0.01	0.01	0.00	0.01
W11	0.04	0.05	0.04	0.03	0.05
W14	0.03	0.03	0.03	0.02	0.03
W2	0.09	0.07	0.08	0.08	0.06
W8	0.08	0.06	0.07	0.08	0.06
W9	0.03	0.02	0.02	0.02	0.01

Figure 1.3: Sales and rental contracts per quarter



The first two columns of Table 1.1 contain the summary statistics for the sold properties (Sales) and rented properties (Rentals). Consistently with the composition of housing stock in this part of London, the majority of housing units in the JDW Dataset are flats. There are more flats in Rentals (88%) than in Sales (76%). Moreover, Sales contain a higher number of large flats (3 or more bedrooms) than Rentals. The median floor area is larger for Sales (1,059 square feet against 879 square feet).

Other authors report similar differences between owner-occupied and rented units. For instance, Glaeser and Gyourko (2007) use the 2005 American Housing Survey to show that "The median owner occupied unit is nearly double the size of the median rented housing unit," and that rental units are more likely to be located near the city centre. These facts are consistent with Linneman (1985)'s production-efficiency argument, according to which smaller units demand less management costs. Therefore both landlords and households prefer them for renting.

Number of observations per quarter Before proceeding to the main analysis, it is useful to measure the evolution of the number of transactions in the sale and rental market. Prices are not the only margin of adjustment in the housing market: volumes and liquidity are also important (Wheaton, 1990; Krainer, 2001; Novy-Marx, 2009; Ngai and Tenreyro, 2009).

Figure 1.3 shows the quarterly number of transactions in the Land Registry and the JDW Rental dataset. The number of sales varies a lot from one period to another. In the 2005-2007 period, when the market was characterized by rising prices, the average number of quarterly transactions was four time as high as the number of transactions during the 2008 bust. The number of rental contracts, by contrast, appears less volatile from one year to another. However, rental contracts display a much clearer seasonal pattern. The third quarter always has 50% more transactions than the first quarter. For sales, the first quarter has usually a lower number of transactions, but seasonality is less pronounced than for rentals.

1.3.2 The Matched Dataset

The Matched Dataset contains properties that appear both in the Sales and Rentals datasets, with the sale taking place between 0 and 6 months before the corresponding rental contract. To increase the number of matched observations, I also add properties that appear both in the JDW Rentals dataset and in the Land Registry—again, with a maximum distance of 6 months between the sale and the subsequent rental.

The goal of the matching procedure is to find, for properties in the JDW Rental Dataset, a sale of the same property either in the JDW Sales Dataset or in the Land Registry. In all these datasets properties are uniquely identified by their address. For houses, the address is made of the the street name and number. For apartments, the address contain all necessary additional information such as floor or unit number.

Since every record comes with a transaction date, I measure the distance in days between sales and rentals. Since there can be multiple sales and multiple rents for each property, for every sale I keep only the closest rental contract. If a rental contract can be imputed to multiple sales, I keep only the closest sale. Properties that were first bought and then rented out are properties bought by "buy-to-let" investors—professional landlords that purchase houses as investments to generate income. Since prices and rents can diverge over time, it is necessary to keep only rental contracts that were signed shortly after the sale of the property. I choose 6 months as the cutoff distance between the sale and the rentals. My window around the sale date is asymmetric in the sense that I do not select rental contract signed a few months before a sale.

Table 1.2: Properties sold and rented out within 6 months

	JDW rent - JDW sales	JDW rent - Land Registry	All
2006	98	165	259
2007	132	347	475
2008	56	214	270
2009	96	109	203
2010	163	224	384

Table 1.2 shows how many matches are retrieved in each year, and the average rent-price

ratios. Most matches come from the Land Registry. Some JDW matches are also found in the Land Registry, so that the sum of the second and third column in the table is in some cases less than the number in the third column. The low number of transactions in 2008 and 2009 causes the number of matches to be low in those years. Moreover, since the available Land Registry data on individual addresses covers only the 2006-2010 period, I concentrate only on those years when analysing matched properties.[10]

1.3.3 The Repeat Transactions Dataset

As shown in Section 1.2, measuring the idiosyncratic volatility of prices and rents requires first to estimate a repeat sales regression. Only properties that appear at least twice in the sample are used to estimate the index. Table 1.3 shows how many repeat observations are contained in the JDW Dataset. Since the turnover in rental contracts is higher than the turnover in owner occupation, repeat observations in Rentals are more common than repeat observations in Sales. Appendix A.1.3 shows that the proportion of repeat sales out of all sales in the JDW Sales Dataset and the Land Registry look similar.

Table 1.3: Repeat transactions in the JDW Dataset

JDW Sales		JDW Rentals	
# Transactions	Properties	# Transactions	Properties
1	17,921	1	24,651
2	1,049	2	5,774
3	45	3	1,594
		4	430
		5	102
		6	18
		7	6

1.4 Results

1.4.1 Price-rent ratios

I start the empirical analysis by estimating Equation 1.4 separately for Sales and Rentals. The vector of characteristics X_{it} contains: a dummy variable to indicate whether the property is a house (as opposed to a flat); three dummy variables indicating the number of bedrooms of the property: 2 bedrooms, 3 bedrooms, and 4 bedrooms or more[11] (1-bedroom properties are the baseline category); floor area measured in square feet; floor area squared, to take

[10]The 2005 file of the Land Registry does not contain individual addresses but only postcodes (corresponding to 10-20 properties).

[11]Only 2.5% of the properties in the sample have more than 4 bedrooms. Properties with more than 10 bedrooms are discarded as outliers

into account the tendency of prices and rents to rise less than proportionally with size; and postcode district dummies to capture the effects of local amenities. I use quarterly dummies to construct a quarter-by-quarter index of log house prices and rents (λ_{st} and λ_{rt}). Ferreira and Gyourko (2011) employ a similar hedonic regression for their recent neighbourhood-level analysis of the start of the US housing boom.

Table 1.4 shows the output of the hedonic regressions on the complete Sales and Rentals dataset in columns 1 and 2. Columns 3 and 4 display the results of the same regressions only using the 2006-2010 period—this is to compare the coefficients with the Matched Dataset, which is available only in those years. Column 5 computes the implied effect on price-rent ratios of the characteristics X. Coefficients are computed as the difference of coefficients in column 3 with coefficients in column 4.[12] Finally, column 6 shows the output from estimating Equation 1.5 on the Matched Dataset.

Table 1.4 shows that, conditional on number of bedrooms and floor area, houses command a positive premium in sales but a small negative premium in rentals. Therefore, on average, houses have higher price-rent ratios than flats. The effect is consistent with the hedonic regression on the matched dataset. Conditional on floor area, the number of bedrooms has a higher effect on Rentals than Sales. Interestingly, the price premium on 4+ bedroom is negative, which indicates that owner occupiers, as opposed to renters, do not like properties divided in too many bedrooms (conditional on floor area). The contribution of floor area is positive, but more for prices than rents. As expected, the coefficient on floor area squared is negative.[13]

In Table 1.4 I sort neighbourhoods from those with the highest price premium (SW3, Chelsea) to those with the lowest one (SW6, Fulham)—the baseline postcode district is W2 (Paddington). In terms of coefficients, both the complete JDW dataset and the Matched Dataset show that more expensive neighbourhoods have higher price-rent ratios. In other words, both prices and rents rise for more expensive neighbourhood, but prices rise more than rents. This fact is well know by housing market practitioners.[14]

The results presented here demonstrate the consistency between the whole JDW dataset and the small subsample of matched properties, which relies on buy-to-let properties owned by investors. Results (non-tabulated) show that the percentages of 1-bed, 2-bed, 3-bed+ and

[12]Standard errors are computed as $s.e.(\text{column 5}) = \sqrt{s.e.(\text{column 3})^2 + s.e.(\text{column 4})^2}$

[13]The data allow me to measure *gross* price-rent ratios, i.e. price-rent ratios which do not take into account maintenance expenses and, for rented properties, vacancies. If these were higher for smaller properties, *net* rent yields (rent-price ratio net of costs) could be more similar than what suggested by their gross counterparts. However, maintenance is commonly thought to be proportionally cheaper for smaller properties (Linneman, 1985). Moreover, anecdotal evidence suggests that more expensive properties stay vacant for longer.

[14]See for instance "London buyers find streets paved with gold", Financial Times, 13 March 2011.

Table 1.4: Hedonic regressions

Notes: Quarterly time dummies used for the complete dataset and half-year dummies for the matched dataset. The baseline property is a 1-bedroom flat in W2.

	$y_{hit} = \beta_h X_{it} + \lambda_{ht} + \varepsilon_{hit}$					
	2005–2011		2006–2010			
	(1)	(2)	(3)	(4)	(5)	(6)
	JDW Sales	JDW Rentals	JDW Sales	JDW Rentals	*Implied*	Matched
	$y = p_s$	$y = p_r$	$y = p_s P$	$y = p_r$	(3) - (4)	$y = p_s - p_r$
House	0.063***	-0.010	0.065***	-0.014*	0.079***	0.147***
	(0.006)	(0.006)	(0.007)	(0.008)	(0.011)	(0.033)
2-bed	0.133***	0.113***	0.118***	0.116***	0.002	0.027
	(0.006)	(0.005)	(0.008)	(0.007)	(0.010)	(0.027)
3-bed	0.122***	0.154***	0.090***	0.158***	-0.068***	-0.033
	(0.009)	(0.008)	(0.010)	(0.011)	(0.015)	(0.045)
4-bed	-0.048***	0.129***	-0.083***	0.124***	-0.206***	-0.150**
	(0.012)	(0.012)	(0.014)	(0.016)	(0.021)	(0.066)
Floor area (sqft*10^{-3})	1.363***	1.155***	1.454***	1.160***	0.293***	0.197***
	(0.009)	(0.011)	(0.012)	(0.013)	(0.018)	(0.067)
Floor area squared	-0.137***	-0.130***	-0.156***	-0.128***	-0.028***	-0.015
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.017)
Postcode:						
SW3	0.317***	0.117***	0.309***	0.122***	0.187***	0.107***
	(0.009)	(0.010)	(0.011)	(0.012)	(0.017)	(0.040)
SW7	0.290***	0.125***	0.288***	0.142***	0.146***	0.158***
	(0.010)	(0.010)	(0.011)	(0.013)	(0.017)	(0.041)
W8	0.249***	0.096***	0.245***	0.104***	0.140***	0.127***
	(0.010)	(0.011)	(0.012)	(0.014)	(0.019)	(0.044)
W1	0.193***	0.117***	0.189***	0.124***	0.065***	0.081*
	(0.010)	(0.010)	(0.012)	(0.013)	(0.018)	(0.044)
W11	0.130***	0.060***	0.123***	0.071***	0.052***	-0.091
	(0.012)	(0.012)	(0.014)	(0.016)	(0.022)	(0.060)
SW1	0.121***	0.113***	0.123***	0.122***	0.000	0.119***
	(0.008)	(0.010)	(0.010)	(0.012)	(0.016)	(0.036)
SW10	0.114***	-0.036***	0.098***	-0.029**	0.126***	0.089**
	(0.010)	(0.011)	(0.012)	(0.014)	(0.019)	(0.042)
SW5	0.085***	-0.020*	0.074***	-0.011	0.086***	0.080*
	(0.013)	(0.012)	(0.015)	(0.015)	(0.021)	(0.047)
NW8	0.006	-0.018	-0.004	-0.023	0.019	0.135**
	(0.012)	(0.015)	(0.015)	(0.020)	(0.025)	(0.064)
SW8	-0.046***	-0.020*	-0.022	-0.004	-0.018	0.059
	(0.016)	(0.011)	(0.019)	(0.014)	(0.023)	(0.058)
NW1	-0.061***	-0.040**	-0.076***	-0.029	-0.047**	-0.054
	(0.014)	(0.016)	(0.016)	(0.020)	(0.025)	(0.060)
NW3	-0.073***	-0.097***	-0.067***	-0.108***	0.041**	0.092
	(0.014)	(0.014)	(0.016)	(0.017)	(0.024)	(0.096)
W14	-0.117***	-0.176***	-0.121***	-0.175***	0.054**	0.054
	(0.013)	(0.014)	(0.015)	(0.018)	(0.024)	(0.062)
W9	-0.151***	-0.150***	-0.162***	-0.160***	-0.002	-0.050
	(0.015)	(0.022)	(0.017)	(0.029)	(0.034)	(0.084)
W10	-0.245***	-0.209***	-0.247***	-0.172***	-0.075*	0.085
	(0.024)	(0.031)	(0.031)	(0.047)	(0.056)	(0.116)
SW11	-0.260***	-0.353***	-0.275***	-0.306***	0.031	0.060
	(0.014)	(0.013)	(0.017)	(0.020)	(0.026)	(0.092)
SW6	-0.275***	-0.258***	-0.285***	-0.226***	-0.059***	-0.061
	(0.011)	(0.011)	(0.014)	(0.014)	(0.020)	(0.061)
Time dummies	✓	✓	✓	✓		✓
N	18,864	15,811	13,052	10,349		494

Figure 1.4: Price and rent indexes, price-rent ratios

houses in the stock of buy-to-let houses are similar to those in the JDW rental dataset.

Evolution of the aggregate price-rent ratio over time The left-hand side part of Figure 1.4 plots the coefficients on time dummies from the hedonic regression in Sales (λ_{st}) and Rentals (λ_{rt}). In the boom period, prices grew at a rate approximately double that of rents. After the peak at the end of 2007, the gap between prices and rents has continued growing, albeit at a lower rate. During the sample period the correlation of the two indexes is very high (90%).

The different growth rates of prices and rents produced increasing price-rent ratios—as shown in the right-hand side of Figure 1.4. The dashed line represents the price-rent ratios implied by the price and rent indexes. The solid line represents an index of actual price-rent ratios computed from the Matched Dataset. The two samples give similar results, although the matched sample is more volatile because of the smaller sample size.

Price-rent ratios and rents The fact that price-rent ratios are higher for bigger and better located properties suggests that price-rent ratios are higher for more expensive properties in general.

It is possible to measure the correlation between price-rent ratios and the value of a house using the Matched Dataset. Either the sale or the rental price can be used as a measure of

Table 1.5: Regression of price-rent ratios on rents (matched dataset)

$y_{it} = p_{sit} - p_{rit}$	(1) $y_{it} = \delta p_{rit}$	(2) $y_{it} = \delta p_{rit} + \lambda_{mt}$	(3) $y_{it} = \delta_t p_{rit} + \lambda_{mt}$
δ	0.084***	0.081***	
	(0.015)	(0.014)	
δ_{2006}			0.060*
			(0.034)
δ_{2007}			0.060**
			(0.026)
δ_{2008}			0.015
			(0.039)
δ_{2009}			0.14***
			(0.039)
δ_{2010}			0.13***
			(0.030)
Year dummies (λ_{mt})		✓	✓
N	1,407	1,407	1,407

the value of the house. It is convenient to use the rental price and estimate:

$$y_{it} = \delta p_{rit} + \nu_{it}, \tag{1.8}$$

where $y_{it} = p_{sit} - p_{rit}$ is the price-rent ratio and δ represents $\frac{\mathrm{d}(p_s - p_r)}{\mathrm{d}p_r} = \frac{\mathrm{d}p_s}{\mathrm{d}p_r} - 1$. Hence, a coefficient significantly greater than zero indicates that the price-rent ratio is positively correlated with rents. With random sale and rental prices δ would be negative because p_{rit} appears on both sides of the equation.

Table 1.5 shows the output of the regression in Equation 1.8. In the first column, the coefficient is positive and significant, meaning that more valuable houses have lower rent-price ratios. The second column displays the regression results when year dummies are added. Without these dummies, one might suspect that years with a lower aggregate rent-price ratio also display higher rents, and drive the results. As the table shows, however, adding year dummies leaves the coefficient on p_{rit} virtually unchanged. Even controlling for year dummies, the regression in column 2 forces the coefficients on p_{rit} to be the same in all years. By interacting year dummies with the log rents, it is possible to separate the different effects of p_{rit} for each year:

$$y_{it} = \delta_t p_{rit} + \lambda_{mt} + \nu_{it},$$

where δ_t is allowed to change from year to year. Results for the whole sample are displayed in the fourth column of Table 1.5. Coefficients are positive and significant in all years except for 2008. This might be due to the exceptional nature of 2008 and the low number of observations. Moreover, coefficients are larger in later years, indicating an increasing divergence of price-rent ratios across properties.

1.4.2 Growth and market volatility by property types

The previous subsection shows that two kinds of characteristics are positively correlated with price-rent ratios: size and expensive neighbourhood. According to the dividend discount model, big and better located properties should display higher rent growth or be associated with lower risk premia. According to the hedging model, big and better located properties should display higher rent volatility.

To measure the extent to which these two theories explain price-rent ratios, I create two housing category classifications. In terms of size, I divide observations into: 1-bedroom flats, 2-bedroom flats, 3-or-more-bedroom flats, and houses. The summary statistics in Table 1.1 show the dimension of these groups with respect to the overall dataset. In terms of location, I divide observations into prime neighbourhoods and other neighbourhoods. Prime neighbourhoods are the first six postcode districts in terms of price premium in the hedonic regression of Table 1.4: SW3 (Chelsea), SW7 (South Kensington), W8 (Holland Park), W1 (Mayfair), W11 (Notting Hill), SW1 (Belgravia and Pimlico). In the JDW Dataset, prime neighbourhoods correspond to 53% of Sales and 54% of Rentals.

Figure 1.5 plots the λ_{hct}'s over the different quarters t, estimated using Equation 1.6. The upper part of the figure shows results according to the first category classification, based on physical characteristics. Consistently with the housing-ladder model of Ortalo-Magné and Rady (2006), the sale prices of bigger houses have grown more in the 2005-2007 boom period. This trend was partially reversed during the brief bust of 2008 but restarted after. In terms of rental prices, the pattern is the same but more pronounced: the rent volatility of bigger properties is clearly higher. A similar impression is given by the prime vs non-prime neighbourhood sale price comparison at the bottom left of Figure 1.5. Sale prices in prime neighbourhood have grown more but are also more volatile. However, rental prices have behaved very similarly in prime and other neighbourhoods, both in terms of growth and volatility.

Table 1.6 lists the average growth and volatilities of the different property categories and confirms the impressions gathered from Figure 1.5. In particular, the standard deviation of houses' rental prices is twice that for 2-bed flats. The first row of the table focus on the whole dataset and shows that, on aggregate, prices are more volatile than rents, consistently with Figure 1.4. This finding is reminiscent of Shiller (1981), who demonstrates that stock prices are more volatile than their dividends. It is also consistent with Gallin (2008), who

Figure 1.5: Growth and Volatility by Housing Categories

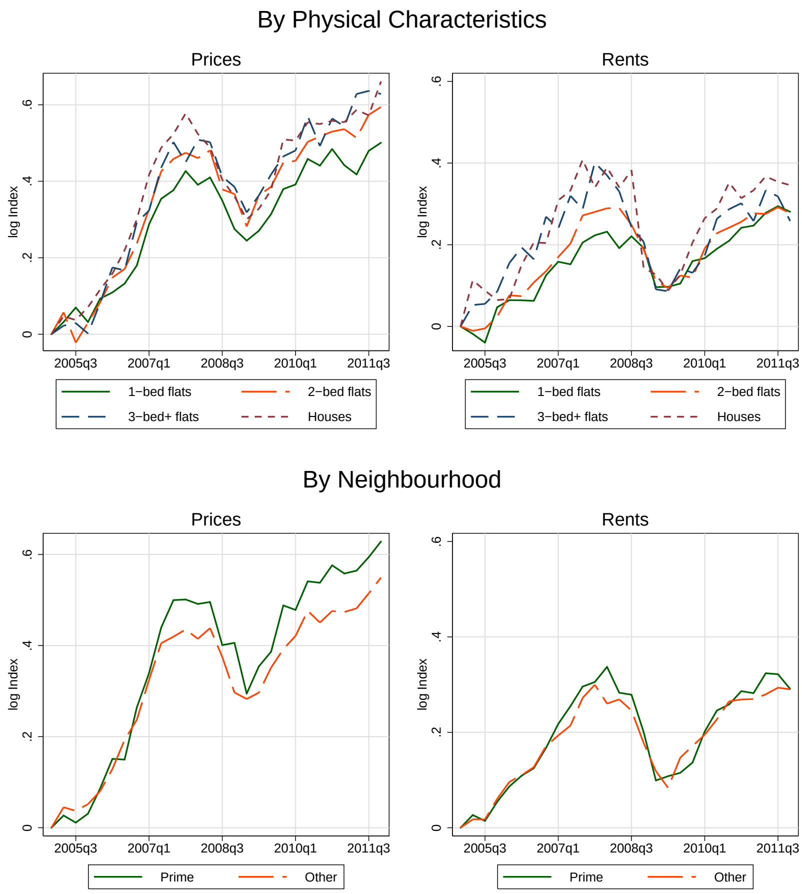
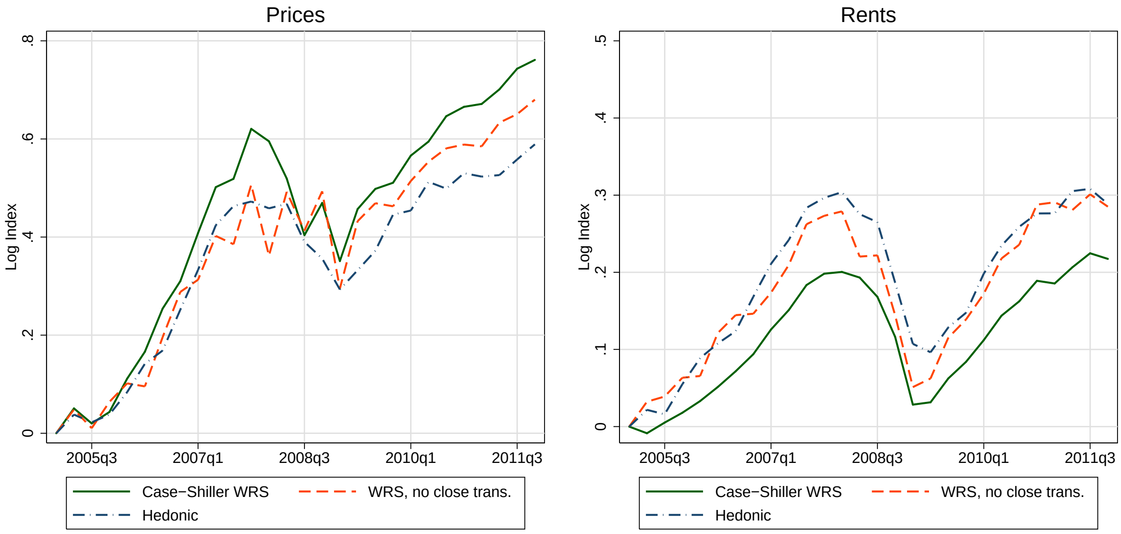


Table 1.6: Prices and rents: Growth and systemic risk

	$p_{ht} = \beta_{hc}X + \lambda_{htc}$			
	JDW Sales Dataset		JDW Rentals Dataset	
	$E(\lambda_{ct+1} - \lambda_{ct})$	$St.Dev.(\lambda_{ct+1} - \lambda_{ct})$	$E(\lambda_{ct+1} - \lambda_{ct})$	$St.Dev.(\lambda_{ct+1} - \lambda_{ct})$
All	*0.022*	*0.042*	*0.011*	*0.033*
1-bed Flats	0.019	0.045	0.010	0.036
2-bed Flats	0.022	0.051	0.010	0.034
3-bed+ Flats	0.023	0.059	0.010	0.059
Houses	0.024	0.053	0.013	0.070
Prime neighbourhood	0.023	0.052	0.011	0.039
Other neighbourhood	0.020	0.039	0.011	0.032

Figure 1.6: Hedonic and repeat sales indexes



also finds, using US city-level data, that price are more volatile than rents.

1.4.3 Idiosyncratic volatility by property types

To measure idiosyncratic volatility as defined in Section 1.2 I restrict my attention to properties that sold or rented at least twice during the sample period.

Repeat transactions indexes Using the subsample of repeat transactions in the JDW Sales and Rental datasets, I compute repeat sales and repeat rentals indexes for the period of interest, 2005-2011, using the weighted repeat sales (WRS) of Case and Shiller (1989). Figure 1.6 shows the result of this exercise, and compare the WRS indexes with the hedonic indexes estimated before.

A substantial literature (e.g. Clapp and Giaccotto, 1992; Goetzmann and Spiegel, 1995) addresses the issue of sample selection bias in repeat sales indexes—an issue that is especially important when these indexes are estimated over short periods of time. The bias is apparent in Figure 1.6: the WRS index for sale prices displays a significantly steeper appreciation than the corresponding hedonic index. To be included in the repeat sales regression, a property must have been sold twice between 2005 and 2011. This is a relatively short period. Property that resell quickly have usually undergone substantial improvements, or belong to a seller who has received a particularly good offer. One way to limit this problem is to exclude properties whose “holding period” (the time between two sales) is below a certain threshold. I choose a threshold of 1000 days (corresponding to approximately 3 years). The result is displayed in the left-hand side of Figure 1.6: the WRS index with no close transitions appreciates less than the unadjusted WRS index, but still more than the original hedonic index.

By contrast, when measuring the index for rental prices, the series computed through repeat transactions is smoother and shows a lower appreciation rate than the one measured through the hedonic method. The different behaviour of repeat rent index is consistent with the findings of Genesove (2003) who, using the American Housing Survey, shows that rents on the same units are sticky, especially when tenants do not change. Moreover, since landlords tend to postpone maintenance works, repeat rents on the same unit suffer from unaccounted depreciation. Again, one can limit the selection bias by excluding from the sample rental contracts that are too close from each other. To be consistent with the procedure adopted for the price index, I choose the same threshold of 1000 days. Again, the resulting WRS index with no close transitions is closer to the hedonic index than the unadjusted WRS index.

To the best of my knowledge, the present exercise represents the first critical evaluation of the repeat sales procedure applied to rental prices. Interestingly, this procedure is likely to generate a downward bias, as opposed to the procedure on sale prices which usually generates an upward bias.

Idiosyncratic volatilities Table 1.7 shows the outcome of running the regression described by Equation 1.7, using both the unadjusted WRS procedure (upper panel) and the WRS with minimum holding period of 1000 days (lower panel).

In terms of the whole dataset (without distinction of property categories), Table 1.7 shows that $\theta_0+\theta_1$ is significantly larger for prices than rents: prices display more idiosyncratic volatility. The coefficient θ_1 is positive in the rent equations but negative in the price equation. While at odds with the Case and Shiller (1989)'s model, it is not uncommon to estimate negative θ_1's in empirical work (Calhoun, 1996). These negative coefficients imply that very close transactions have high idiosyncratic volatility. This pattern is consistent with the sample-selection hypothesis outlined above in relation to the bias in the repeat sales estimator.

The results by property category mirror the results on aggregate volatility. Bigger houses and expensive neighbourhoods have more idiosyncratic volatility. However, as before, the distinction between prime and non-prime neighbourhoods is less evident than the distinction between big and small properties.

The numbers in Table 1.7 show that the WRS with a minimum holding period of 1000 days relies on significantly smaller samples. Nevertheless, coefficients under both approaches give a consistent picture.

Table 1.7: Prices and rents: Idiosyncratic risk

	$\nu^2_{cht} = \theta_{c0} + \theta_{c1}(T-t)$					
	JDW Sales Dataset			JDW Rentals Dataset		
	θ_{c0}	θ_{c1}	Obs.	θ_{c0}	θ_{c1}	Obs.
	Panel A: WRS					
All	0.077***	-0.021***	1,139	0.010***	0.011***	10,786
	(0.008)	(0.007)		(0.001)	(0.001)	
1-bed Flats	0.048***	-0.017**	176	0.010***	0.005*	3,965
	(0.009)	(0.008)		(0.002)	(0.003)	
2-bed Flats	0.077***	-0.033	286	0.008***	0.009***	4,007
	(0.022)	(0.020)		(0.001)	(0.002)	
3-bed+ Flats	0.056***	-0.006	201	0.007***	0.016***	1,147
	(0.011)	(0.010)		(0.002)	(0.002)	
Houses	0.066***	-0.011	395	0.010***	0.017***	1,450
	(0.010)	(0.008)		(0.004)	(0.004)	
Prime neighbourhood	0.084***	-0.018	703	0.009***	0.012***	6,465
	(0.013)	(0.012)		(0.001)	(0.002)	
Other neighbourhood	0.057***	-0.018***	435	0.011***	0.009***	4,321
	(0.006)	(0.005)		(0.002)	(0.003)	
	Panel B: WRS with minimum holding period of 1000 days					
All	0.053***	-0.008	512	0.010***	0.009**	2,390
	(0.018)	(0.012)		(0.006)	(0.004)	
1-bed Flats	0.022	-0.005	77	0.004	0.008**	771
	(0.014)	(0.008)		(0.005)	(0.004)	
2-bed Flats	0.0087	0.008	133	0.014*	0.004	879
	(0.016)	(0.011)		(0.008)	(0.005)	
3-bed+ Flats	0.073*	-0.024	80	0.006	0.015*	288
	(0.038)	(0.024)		(0.013)	(0.009)	
Houses	0.049	-0.004	197	0.024	0.006	381
	(0.033)	(0.021)		(0.021)	(0.014)	
Prime neighbourhood	0.056**	-0.005	296	0.008	0.011**	1,470
	(0.027)	(0.017)		(0.007)	(0.005)	
Other neighbourhood	0.032**	-0.004	214	0.014	0.006	916
	(0.013)	(0.008)		(0.010)	(0.007)	

Figure 1.7: Time-varying hedonic prices

1.5 Extensions

1.5.1 Hedonic regressions with time-varying prices of characteristics

Dropping the assumption of constant characteristic prices β_h, the hedonic Equation 1.4 becomes:

$$p_{hit} = \beta_{ht} X_i + u_{hit}. \tag{1.9}$$

According to this equation house prices are a combination of the time-varying prices of their characteristics. The practical implementation of this approach consists in estimating Equation 1.9 for each period. The JDW Dataset contains 28 quarters. Using the 23 explanatory variables listed in Table 1.4 (6 variables for physical characteristics and 17 dummies for postcode districts), separately for sale and rental prices, produces $28 \times 23 \times 2 = 1288$ coefficients. Figure 1.7 plots the time evolution of some of these coefficients. Some quarters have a limited number of observations and this generates volatile characteristic prices. Despite volatility, however, the main message of these coefficients is consistent over time.

Houses enjoy a positive price premium and a negative rent premium with respect to flats. However, Figure 1.7 shows that this differential has been declining over time. It is possible that, in the aftermath of the housing bust, the demand for house purchases has declined and has been substituted by an increase in demand for housing rentals.

Figure 1.7 also confirms that, conditional on floor area, rentals enjoy a premium for a high number of bedroom (4+). Moreover, the price of a square foot has been rising over time for sales but has stayed constant for rentals. This pattern is consistent with the general price and rent indexes in Figure 1.4, which show a higher appreciation of prices in the 2005-2011 sample period.

The last four charts show the effect of location on prices and rents. It is clearly the case, in all periods, that properties in prime neighbourhoods such as Chelsea (SW3) or Kensington (SW7) command a bigger premium on sales than rentals. When analysing other neighbourhoods, such as Fulham (SW6), the distinction between price and rent coefficients become much less clear or is reversed.

1.5.2 Are house price and rent expectations consistent with the results?

Both the dividend discount model and the hedging model predict that prices today are based on people's expectations of the future. In empirical work, it is common practice to study the historical trend of economic variables and then assume that expectations reflect this trend. Consistently with this approach, in the main part of this chapter I study the historical performances of different categories of properties in Central London to interpret their current valuations.

Another approach would be to directly measure people's expectations. This approach is usually impossible because of lack of data.[15] Fortunately, John D Wood & Co., whose Sales and Rentals Dataset is used in the present analysis, conducts every six months an online survey of the members of its mailing list. The last survey (January 2012) contains a couple of questions on local price and rent expectations:

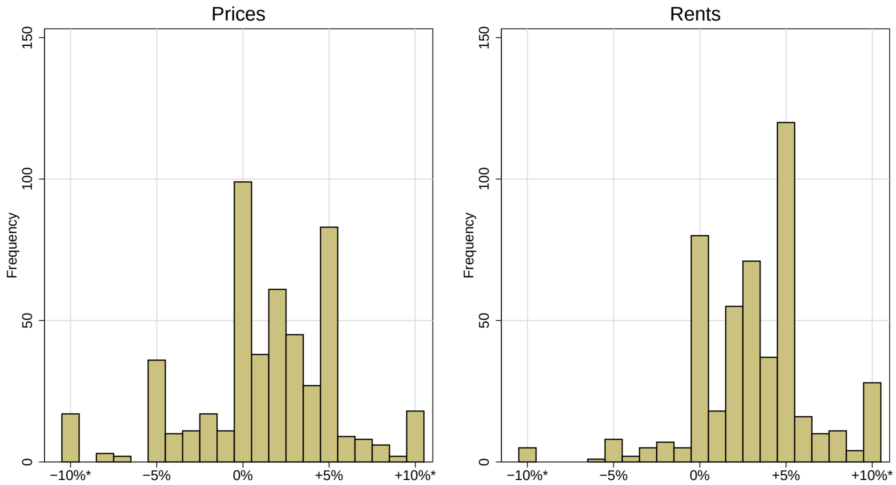
> The next few questions are about nominal house prices in the area where you live.
>
> Please enter the first part of your postcode: ____
>
> - In terms of nominal value, what do you think will happen to *house prices* in your area after 1 year?
>
> - In terms of nominal value, what do you think will happen to *rents* in your area after 1 year?

Both expectation questions are followed by a drop-down menu where the respondents can choose an answer from "-10% or more" to "+10% or more". Figure 1.8 shows the frequency

[15] House price expectations are rarely surveyed. This is contrast with inflation expectations, which are regualrly surveyed by Central Banks and other institutions (Mankiw et al., 2004).

Figure 1.8: Survey expectations

Notes: The questions are "In terms of nominal value, what do you think will happen to *house prices* in your area after 1 year?" and "In terms of nominal value, what do you think will happen to *rents* in your area after 1 year?" The answers are the bottom and top of the range are "-10% or more" and "+10% or more".



of each answer.

The question on postcodes aims at identifying the postcode district of the respondent. With this information, I can compare survey answers with postcode district-level prices and rents from the JDW Dataset. I can check whether the high price-rent ratios of prime neighbourhoods are correlated with high price or rent growth expectations (in accordance with the dividend discount model) or with higher rent uncertainty (in accordance with the hedging model).[16]

Unfortunately, there are no explicit questions on rent risk. Hence, I take the dispersion of rent expectations as a measure of rent uncertainty. This approach is consistent with the empirical literature that looks at disagreement about inflation (Mankiw et al., 2003) or the stock market (Vissing-Jorgensen, 2003). Figure 1.8 shows that disagreement about house prices and rents can be substantial: taken together, respondents fill almost the entire range of possible answers, with round numbers ("-10% or more", "-5%", "0%", "+5%", "+10% or more") being chosen more often.[17]

Using information on the postcode districts, I divide respondents into three groups: those living in the UK outside London, those living in London but not in a prime neighbourhood, and those living in a prime neighbourhood.[18] The definition of prime neighbourhood is the

[16]The survey makes no distinction between properties with different physical characteristics, e.g. flats vs houses. Hence, I can only test the part of results that relates to differences between neighbourhoods, not the one regarding differences between properties of different sizes.

[17]This is a common feature of expectation surveys (Hudomiet et al., 2011).

[18]The question on the postcode appears at the very beginning of the survey and 95% of people that clicked on the survey link filled that question. A few respondents live outside the UK, and are excluded from the present statistics. The last part of the questionnaire contains questions on the socio-demographic characteristics of

Table 1.8: Survey results

Notes: The questions are "In terms of nominal value, what do you think will happen to *house prices* in your area after 1 year?" and "In terms of nominal value, what do you think will happen to *rents* in your area after 1 year?"

	Price Expectations ($E_t p_{st+1}$)			Rent Expectations ($E_t p_{rt+1}$)		
	Mean	(St. Dev.)	Obs.	Coeff.	(St. Dev.)	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
London, prime neighbourhood	2.25	(4.37)	79	2.84	(3.81)	74
London, other neighbourhood	2.10	(4.14)	189	3.78	(3.38)	183
Mean Diff. (T-test)	*0.15*	*(0.58)*		*-0.94**	*(0.51)*	
StDev Ratio (F-test)		*1.12*			*1.28**	
London	2.15	(4.20)	268	3.51	(3.53)	257
Outside London	0.20	(3.58)	200	2.67	(2.97)	191
Mean Diff. (T-test)	*1.95****	*(0.36)*		*0.84****	*(0.31)*	
StDev Ratio (F-test)		*1.37****			*1.41****	

same as the one in the rest of the chapter, namely an address belonging to the following six postcode districts: SW1, SW3, SW7, W1, W8, W11.

The upper half of Table 1.8 shows the differences in price and rent expectations between prime and other neighbourhood. To provide another relevant comparison, the lower half of the table shows the same differences between London and other parts of the UK.

In terms of price expectations, respondents in prime neighbourhoods are slightly more optimistic than other Londoners, but the difference is not significant. By contrast, Londoners are significantly more optimistic than other respondents in the UK, consistently with the different performances of the UK housing market inside and outside London in the last years.[19]

In terms of rent expectations, people living in the non-prime neighbourhoods of London expect slightly higher growth (the difference is significant at the 10% level). Londoners in general expect higher rent growth than non Londoners. The standard deviation of rent expectations is significantly higher for prime London than other parts of London and the same is true for the London vs Outside London comparison.

These results are consistent with the hedging model: rent uncertainty is higher for London's prime neighbourhoods but rent growth is not. Clearly, the evidence presented here is only suggestive. Nevertheless, the respondents to this survey are people on the mailing list of a Central London real estate agency. Their opinions are likely to be representative of the buyers and sellers of this particular housing market.

respondents. A table with summary statistics is shows in Appendix A.1.2.

[19]See "How did London get away with it?", CentrePiece, Winter 2010/2011 (`http://cep.lse.ac.uk/pubs/download/cp333.pdf`).

1.6 Conclusion

This chapter presents novel findings on house prices and rents at the individual-property level. Price-rent ratios are shown to be higher for bigger properties and properties located in more expensive neighbourhoods. In accordance to the hedging model of Sinai and Souleles (2005), these properties display higher rent risk.

Consistently with the finance literature, I measure risk as price volatility, which is also the approach of Sinai and Souleles (2005). However, the hedging model leaves open the possibility that other kinds of risk play a role in the renting vs buying decision. For instance, search costs: a household looking for a 4-bedroom house to rent is not only worried about changes in rental prices, but also about *finding* a 4-bedroom house to rent. Moreover, households might differ in their risk preferences. Workers whose income covaries positively with rents are less sensitive to rent volatility (Ortalo-Magné and Rady, 2002). Families with children are more risk averse (Banks et al., 2010). Future empirical analyses should expand on these different aspects of rent risk and housing market liquidity. One promising way of doing so would be to merge the data on rental transactions with information on the time on the market of properties and their vacancy rates.

Chapter 2

Homeownership and Entrepreneurship

2.1 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 chapter, 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

[0]The work in this chapter was carried out jointly with equal share by Christian Hilber, Olmo Silva, and me.

[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.

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 the 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, 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.

2.1.1 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 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 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.

[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).

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 a small positive effect 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 chapter is structured as follows. In Section 2.2 we describe how we use the BHPS to construct a monthly-spell panel. Section 2.3 discusses our main findings on the link between homeownership and entrepreneurship. Section 2.4 explores different mechanisms and explanations for our key results. Finally, we provide some concluding remarks in Section 2.5.

2.2 Data and descriptive statistics

2.2.1 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

[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.

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 B.2.1.

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

[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.

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. 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 the 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.

2.2.2 Descriptive Statistics

Descriptive statistics for the BHPS monthly-spell dataset are presented in Table 2.1. 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: 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,

[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

Table 2.1: Summary Statistics—BHPS individual level monthly dataset

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

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 jobs since this information is relevant for our fixed-effect identification. Summary statistics are presented in Appendix Table B.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 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.

they focus their analysis on the original members of the survey).

2.3 The negative link between homeownership and entrepreneurship

2.3.1 Main finding

Our first set of results is presented in Table 2.2. 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:

$$Entrep_{ilt} = \alpha_i + \beta \; own_{ilt} + X_{ilt}\gamma + \phi_l + \omega_t + \varepsilon_{ilt} \tag{2.1}$$

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 2.1, 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 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 2.2 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 2.1, 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:

[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.

Table 2.2: OLS and Fixed-Effect Regressions—Various definitions of entrepreneurs

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 2.1. 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		✓		✓	✓	✓	✓

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 2.1. In order to partial out these unobserved factors, we estimate various fixed-effect models, which are presented in Columns (3) to (7) of Table 2.1. 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 local unobservable factors, in Column (4) we add to the fixed-effect models the control variables detailed in Table 2.1, 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.

[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.

To directly explore the relevance of this channel, in Columns (6) and (7) of Table 2.2, 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.

2.3.2 Robustness Checks

Our robustness checks are reported in Appendix Table B.2 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 B.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 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.

2.3.3 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 2.3. 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).

Table 2.3: 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 2.2 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)	(2)	(3)	(4)	(5)
	FE	FE	FE	FE	FE
	No Input	Duration	Duration	Control	Control
	Trans. Date	Linear	Squared	For Lags	For Leads
Homeowner	-0.017	-0.013	-0.018	-0.014	-0.0085
	(0.006)**	(0.006)*	(0.006)**	(0.006)*	(0.0050)$^{+}$
HO Duration		0.003	0.016		
(100)		(0.005)	(0.008)*		
HO Duration Squared			-0.0005		
(1000)			(0.0003)$^{+}$		
P-value, significance of leads/lags	–	–	–	0.6995	0.2120

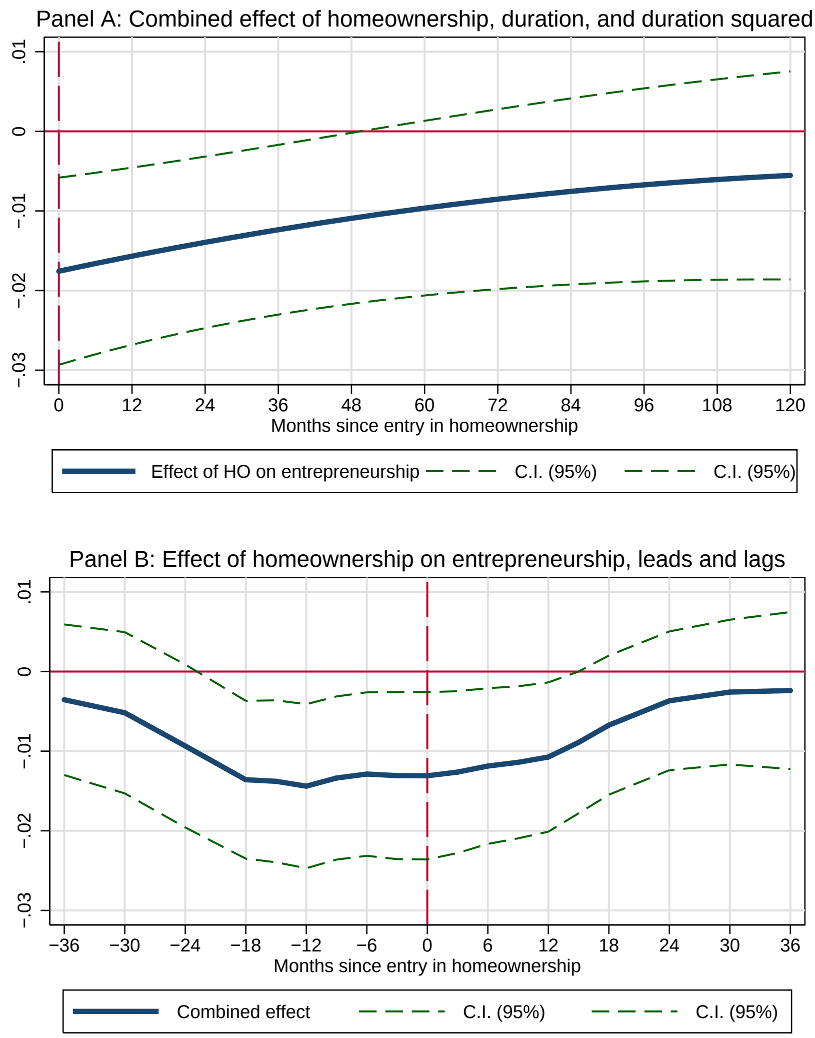
This implies an inverted U-shaped relationship between the time since becoming a homeowner and the probability of becoming an entrepreneur. We present this graphically in the top Panel A of Figure 2.1. The results imply that—on impact—the effect of homeownership on entrepreneurship is as large as -0.018, 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 2.3, 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

Figure 2.1: 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 2.3, 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 2.3 for more details. Dashed lines are confidence intervals at the 95% level obtained from standard errors clustered at the LA level.



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 2.1. 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 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

[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.

been actually bought. This pattern is consistent with the view that homeownership crowds out entrepreneurship because of portfolio risk considerations.

2.4 Exploring the Mechanism: Leverage and Portfolio Considerations

2.4.1 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—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

[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.

Table 2.4: Entrepreneurs with Dependent Workers—The role of leverage

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 2.2 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	-0.002		
		(0.005)**	(0.007)		
Homeowner, outright		-0.002	0.002	0.004	0.006
		(0.009)	(0.009)	(0.007)	(0.007)
Loan-to-value (LTV) of mortgage	-0.015		-0.013	-0.014	-0.0172
	(0.008)*		(0.008)	(0.006)*	(0.0091)*
First-stage:	–	–	–	–	0.829
Coeff. (s.e.) on instrument					(0.013)
T-Stat on instrument	–	–	–	–	63.93

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 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

[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, 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.

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 2.4, 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.

2.4.2 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. 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),

[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.

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 2.5. 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 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

[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.

Table 2.5: Entrepreneurs with Dependent Workers—Risk and cost Sunkness

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)

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 2.4.1. 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.

2.4.3 Credit Constraints as an Alternative Explanation? Some Dispelling Evidence

The results in Table 2.5 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 2.6.

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.

Table 2.6: Entrepreneurs with Dependent Workers—House price dynamics and credit constraints

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 2.2 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	-0.012			
	(0.006)*	(0.006)*			
Homeowner, mortgage			-0.014	-0.015	-0.018
			(0.006)**	(0.005)**	(0.006)**
Homeowner, outright			0.007	0.006	-0.003
			(0.011)	(0.010)	(0.009)
Local HP (logs)	-0.002				
	(0.012)				
Cumulative HP gains		-0.002			
		(0.004)			
Cumulative HP gains			-0.002	0.003	
Home, mortgage			(0.004)	(0.005)	
Cumulative HP gains			-0.008	-0.010	
Home, outright			(0.007)	(0.008)	
Residual Cash flow					0.009
(× 100)					(0.010)
House price measure	Local	Local	Local	Regional	–

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 2.6, 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]

2.5 Conclusion

In this chapter, 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.

[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.

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 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.

Chapter 3

How Long Do Housing Cycles Last? A Duration Analysis for 19 OECD Countries

3.1 Introduction

National house prices went through an unprecedented and synchronized rise across OECD countries in the years preceding the Great Recession (Girouard et al., 2006). Many of those countries are now experiencing a violent decline. In Spain, the U.S., and Ireland, prices are down 20, 32, and 38 percent from their peaks, respectively.[1] While the magnitude of these changes is exceptional, the fact that house prices go through ups and downs is not. Referring to the U.S. housing market, Himmelberg et al. (2005) write that "Over the last quarter century, run-ups in house prices are common, but so are subsequent declines. The national average real house price fell by 7.2 percent from 1980 to 1982; rose by 16.2 percent from 1982 to 1989; fell by 8 percent from 1989 to 1995; and then rose by 40 percent from 1995 to 2004." Available historical records show that this recurring sequence of house price expansions and contractions has been a constant feature of industrial economies at least since the 17th century.[2]

Are these cycles an inherent feature of housing price dynamics? Or are they just an impression caused by the random movements of price indices? Fisher (1925) made the point

[1] Data as of March 2011. Spain: Tinsa index (http://www.tinsa.us/654-imie-spanish-real-estate-market-index.html); U.S.: Case and Shiller index (http://www.standardandpoors.com/indices); Ireland: TSB/ESRI index (http://www.esri.ie/irish_economy/permanent_tsbesri_house_p).

[2] Shiller (2006) documents the ups and downs of U.S. house prices since 1890; Eitrheim and Erlandsen (2004) show Norway house prices since 1819; Eicholtz (1997) examines prices in Amsterdam starting from 1650.

that every statistical series fluctuates above and below its trend, hence the mere presence of expansions and contractions does not imply that cyclical forces are at work.[3] In this chapter, I analyze 40 years of housing expansions and contractions in 19 OECD countries and concentrate on one specific characteristic: duration. I find that longer expansions are more likely to end, or, equivalently, that shorter expansions are less likely to turn into contractions. Moreover, house price contractions that follow long expansions are also longer. These features are defined as duration dependence and lagged duration dependence, and have been studied extensively in the business cycle literature, following the seminal works of Diebold and Rudebusch (1990) and Sichel (1991). Duration dependence points at an inherent cyclicality of housing indices—as opposed to Fisher's random fluctuations—and represents a deviation from a simple random walk model of prices.[4]

It is well known that housing prices do not follow a random walk, but display short-run momentum (Case and Shiller, 1989) and long-run mean reversion (Glaeser and Gyourko, 2006). I focus on duration dependence, rather than these other more commonly studied features, for two reasons. First, policymakers have an interest in knowing how long a housing price expansion (or contraction) is expected to last. I explicitly measure the length of expansions and contractions, and show that, for my sample of OECD countries, upturns have been longer than downturns on average, but the difference almost disappears once the last house price boom is excluded. Second, by studying duration dependence on expansions and contractions separately, I highlight an important difference between these two phases: duration dependence is stronger for expansions, whereas contractions display significant lagged duration dependence. This asymmetry is consistent with boom-bust theories of house price fluctuations. According to these models, housing markets are characterized by rigidities and frictions,[5] which cause prices to periodically overshoot. As expansions get longer, they are increasingly likely to terminate, signaling a progressively unsustainable departure from fundamental price valuations. Contractions often act as adjustment periods after long expansions: the longer an expansion is, the longer the subsequent contraction has to be. Housing cycles are better interpreted as sequences of booms and busts—in this order—rather than

[3] "Of course, if by the business cycle is meant merely the statistical fact that business does fluctuate above and below its average trend, there is no denying the existence of a cycle and not only in business but in any statistical series whatsoever! If we draw any smooth curve to represent the general trend of population, the actual population figures must necessarily rise sometimes above and sometimes below this mean trend line. [...] In the same way weather conditions necessarily fluctuate about their own means; so does the luck at Monte Carlo. Must we then speak of "the population cycle," "the weather cycle" and "the Monte Carlo cycle"?" Fisher (1925)

[4] The likelihood that a random walk index turns up or down is always the same independently of the length of the previous sequence of price increases.

[5] Such imperfections include credit constraint (Ortalo-Magné and Rady, 2006) search frictions (Wheaton, 1990; Novy-Marx, 2009), restricted supply (Glaeser et al., 2008), and market psychology (Shiller, 2007).

successions of crashes and recoveries.

Only a few papers have analyzed the issue of duration dependence in housing cycles. Claessens et al. (2011) describe the characteristics of cycles in credit, stock prices, and house prices ("financial cycles") in advanced and emerging economies. Cunningham and Kolet (2011) study the presence of duration dependence in the house price indices of U.S. and Canada metropolitan areas. My analysis differs from these papers in the way housing cycles are identified and in the test used to detect duration dependence. In this chapter, I highlight these differences in more detail and compare my results with theirs. To be consistent with the terminology of Claessens et al. (2011), I refer to house price expansions as "upturns," and to house price contractions as "downturns."

As previously mentioned, the procedures to identify cycles and study their duration were first explored in the business cycle literature (Diebold and Rudebusch, 1990; Sichel, 1991). The translation of these methods in the housing market context is of great interest but requires special care. Business cycle researchers use the official dates of the start and end of recessions to partition the GDP series into expansions and contractions. In the U.S., for instance, these turning points are announced by the National Bureau of Economic Research (NBER). No official dates exist for housing turning points, and researchers have to identify expansions and contractions by themselves. Following Girouard et al. (2006), I use the Harding and Pagan (2002) BBQ algorithm to divide house price series into upturns and downturns.[6]

In a recent paper, Agnello and Schuknecht (2011) use a similar algorithm to identify upturns and downturns in housing prices for the same sample of OECD countries. Their attention is restricted to the top 25 upturns and downturns in terms of magnitude and persistence. For these phases, which they define booms and busts, Agnello and Schuknecht estimate a probit model in order to identify which macroeconomic variables predict the occurrence of a turning point. The present paper differs from theirs in two fundamental ways. First, similarly to the duration dependence literature on business cycles, I do not restrict my attention to particular phases but consider all expansions and contractions spanning the available time series. Second, the object of my analysis is the statistical regularity of duration dependence in national housing price indices, not the ability of macroeconomic variables to predict turning points. In some regressions, I use macroeconomic variables as controls, in order to show that duration dependence is still significant after their inclusion. Consistently with Agnello and Schuknecht (2011), I show that the recent housing price booms in several OECD countries were exceptional in terms of both magnitude and duration. Moreover, major

[6]The algorithm is denominated BBQ because it is a quarterly (Q) application of the Bry and Boschan (1971) algorithm (BB) designed to detect business cycles in monthly data.

housing price upturns tend to be followed by deep and prolonged downturns.

The rest of the chapter proceeds as follows. Section 3.2 describes the data and the algorithm employed to identify turning points. Section 3.3 analyzes the characteristics of upturns and downturns. Section 3.4 discusses the distribution of phase durations and the duration dependence property. Section 3.5 concludes.

3.2 Data and methodology

3.2.1 Data

I use an OECD dataset containing information on nominal and real house prices, price-income ratios, and price-rent ratios. The data cover 19 countries[7] and are based on official and commonly-used national sources (see André, 2010, p. 52). The dataset has quarterly observations, spanning from the first quarter of 1970 to the first quarter of 2010. Since house prices display a high degree of within-year cyclicality (Ngai and Tenreyro, 2009), series are seasonally-adjusted.

The measurement of house prices poses several challenges. As a consumption good, houses are heterogeneous in terms of physical characteristics (e.g. number of rooms), location (e.g. proximity to amenities or jobs), and state of the building (e.g. repairs and improvements). As an asset, houses are not traded in a centralized market, but through a multitude of bilateral negotiations. In any given year, only a small fraction of the housing stock changes hand.[8] A considerable effort is dedicated to ensure that national indices are comparable across countries, but the interpretation of results should always keep these caveats into account. Reassuringly, the data exploited in this paper have been used in a number of other cross-country studies.[9]

I also collect data on other macroeconomic variables. Real GDP, interest and inflation rates, and working age population are from the OECD Economic Outlook. From the IMF International Financial Statistics (IFS) I gather data on credit to the private sector. Again the choice of sources is consistent with the literature (Claessens et al., 2011).

[7] The countries are Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Korea, Netherlands, Norway, New Zealand, Sweden, Spain, Switzerland, United Kingdom (U.K.), and United States (U.S.).

[8] Piazzesi and Schneider (2009) write that in the U.S., in any given year, "only 6 percent of owner-occupied homes are traded. In contrast, on the New York Stock Exchange, annual volume divided by market capitalization is 120 percent."

[9] See for instance Girouard et al. (2006) and Igan et al. (2011).

3.2.2 Identifying Housing Price Cycles

In order to identify housing price cycles, I use the Harding and Pagan (2002) algorithm to detect turning points in quarterly data. This algorithm belongs to the strand of pattern-recognition methods pioneered by Burns and Mitchell (1946) in their work on business cycles for the National Bureau of Economic Research (NBER), and later formalized by Bry and Boschan (1971). The dating procedure consists in finding a series of local maxima and minima that allow segmenting the series into expansions and contractions. The algorithm requires implementing the following three steps on a quarterly series y_t:[10]

1. Identification of points which are higher or lower than a window of surrounding observations. Using a window of j quarters on each sides, a local maximum y_t^* is defined as an observation of the series such that $(y_{t-j}, \ldots, y_{t-1}) < y_t^+ > (y_{t+1}, \ldots, y_{t+j})$. Symmetrically, a local minimum y_t^- satisfies $(y_{t-j}, \ldots, y_{t-1}) > y_t^- < (y_{t+1}, \ldots, y_{t+j})$.

2. Alternation rule. A local maximum must be followed by a local minimum, and vice versa. In the case of two consecutive maxima (minima), the highest (lowest) y_t is chosen.

3. Censoring rule. The distance between two turning points has to be at least q quarters, where q is chosen by the analyst in order to retrieve only significant series movements and avoid some of the series noise. Harding and Pagan (2002) choose $q = 2$ for U.S. GDP.

The outcome is a binary series where expansion quarters are tagged with "1" and contraction quarters are tagged with "0". This algorithm has initially been confined to the analysis of business cycles. Later its use has expanded to the analysis of asset prices: Pagan and Sossounov (2003) employ it do identify bull and bear markets in stock prices, and check if these phases show any duration dependence. Borio and McGuire (2004) use a similar algorithm both for the stock and the housing market, and see if peaks in equity prices can predict peaks in housing prices. Girouard et al. (2006) apply the Harding and Pagan (2002) method to OECD housing price indices—they compare the last boom with previous expansion episodes to see if the last boom has been exceptional in terms of magnitude and duration. The novelty of my paper is to check whether housing cycles, measured on the same OECD indices and with the same dating algorithm, display duration dependence.

Using the algorithm with series different from GDP requires a decision over the dimension of the rolling window (j) and the minimum phase duration (q). Since house price cycles are

[10] These tasks are usually carried out by computer programs. I wrote a code to detect turning points in Stata, which is available at http://ideas.repec.org/c/boc/bocode/s457288.html.

known to be longer than GDP cycles (Ceron and Suarez, 2006), threshold parameters should be set at a higher level to avoid the identification of spurious phases.[11] Borio and McGuire (2004) suggest a rolling window of 13 quarters, which implies $j = 6$. Girouard et al. (2006) require a minimum phase length (q) of 6 quarters. In this paper I follow these indications.[12]

The method presented here examines the series in level and has been referred to as the "classical cycle". An alternative approach would be to focus on the "growth cycle", which examines a series' deviations from trend (Stock and Watson, 1999). Most growth cycle methods rely on parametric assumptions, and results are sensitive to the chosen detrending method (Canova, 1998).

Since this paper aims at uncovering a relatively new feature of the data, the dating method has to avoid restrictive parametric assumptions. By relying on the "graphical" properties of the series, the Harding and Pagan (2002) algorithm achieves this condition.[13]

3.3 Characteristics of Upturns and Downturns

3.3.1 Descriptive Statistics

Table 3.1 shows the housing price peaks and troughs for all countries. These turning points correspond almost perfectly to the ones identified by André (2010), who discusses recent housing price developments in 18 OECD countries.[14] In Table 3.1, dates are arranged so that the last row corresponds to the peak of the recent housing boom. Countries which are still experiencing an ongoing boom (as of the first quarter of 2010) have their last turning point in the previous row. Figure 3.1 plots peaks and troughs against the corresponding housing price indices, which are normalized to 100 in the second quarter of 2005. During the 1970-2010 period, most indices lie below the 100 level, indicating that in 2005 prices are close to their maximum value in the sample. Germany, Japan, and Korea are exceptions: their housing prices in 2005 were lower than during most of the previous period. National indices

[11] Another reason to impose wider rolling windows is that asset prices are more volatile than underlying fundamentals, potentially giving rise to a high number of spikes (Pagan and Sossounov, 2003).

[12] Additionally, one can impose a minimum cycle duration, so that the distance between two consecutive maxima (minima) is at least k quarters ("Cycle rule"). Harding and Pagan (2002) choose $k = 5$, which means that one cannot have to consecutive phases with minimum duration . I do not impose an additional restriction on the duration of the cycle: an entire cycle already has to last longer than 12 quarters because of the censoring rule.

[13] In Appendix C.3.1 I test that the Harding and Pagan (2002) algorithm does not detect duration dependence in a standard ARIMA process.

[14] His sample does not include Belgium. When differences in turning points exist, turning points happen 1 or 2 quarters earlier/later. These discrepancies are due to the seasonal adjustment algorithm—the dataset changes slightly every time a new update is released. Moreover, André (2010) reports only "major" upturns and downturns, defined as those phases where price changes exceeded 15% in absolute terms. I report all turning points.

FIGURE 3.1: HOUSE PRICE INDEXES AND TURNING POINTS

Notes: Prices are normalized to 100 on 2005q2.

Table 3.1: Peaks and Troughs

Notes: "P" denotes a peak, "T" denotes a trough.

	Australia	Belgium	Canada	Denmark	Finland	France	Germany	Ireland	Italy
P	1974q1								
T	1978q4				1972q2				
P	1981q4				1974q1				
T	1987q1				1979q1				
P	1989q2		1976q4	1973q3	1984q3			1972q2	1971q4
T	1991q1		1985q1	1977q1	1986q2			1976q3	1973q3
P	1994q3		1989q1	1979q2	1989q2	1980q4	1972q2	1979q2	1981q2
T	1996q1	1971q3	1992q1	1982q3	1993q2	1984q4	1976q3	1987q2	1986q2
P	2004q1	1979q3	1994q1	1986q2	1999q4	1991q2	1981q2	1990q2	1992q2
T	2005q3	1985q2	1998q3	1993q2	2001q4	1997q1	1989q2	1994q4	1997q3
P				2007q1	2007q3	2007q4	1994q3	2006q3	2007q4

	Japan	Korea	Netherlands	New Zealand	Norway Norway	Spain Spain	Sweden Sweden	Switzerland	United Kingdom	United States
P										
T				1971q4						
P				1974q3						
T				1980q2						
P				1984q2		1974q3			1973q3	1973q4
T		1987q3		1986q4	1972q4	1976q2	1974q2		1977q3	1975q3
P		1991q2		1988q2	1977q1	1978q2	1979q3	1973q1	1980q3	1979q1
T		2001q1		1992q1	1983q4	1982q2	1985q4	1976q3	1982q1	1982q4
P	1973q4	2003q3	1978q2	1997q2	1987q2	1991q4	1990q1	1989q4	1989q3	1989q4
T	1977q3	2005q1	1985q1	2000q4	1993q1	1996q3	1996q1	2000q1	1996q2	1995q1
P	1991q1	2007q1		2007q3	2007q3	2007q3			2007q4	2006q4

also differ in volatility: some countries display noisy housing prices, which create a sequence of many upturns and downturns. Other countries, such as the Netherlands, have a smoother index and a low number of cyclical phases. These differences motivate the use of country fixed effects in the empirical analysis—a point which I will discuss in the following section.

The two most important characteristics of cyclical phases are amplitude and duration. Amplitudes measure the cumulative increase (decrease) of house prices during an upturn (downturn). Durations are the main object of interest in this paper. For upturns, duration is defined as the distance in quarters between a trough and a peak; for downturns, it is the distance in quarters between a peak and a trough. Table 3.2 shows all the durations and distinguishes between ongoing and completed phases. The last row, corresponding to the ongoing housing price downturn that many countries are experiencing, shows relatively short durations, because the corresponding housing price peaks were reached recently. By contrast, the row just above, corresponding to the last housing price boom, contains relatively long durations, because the recent upturn was of remarkable length.

Table 3.3 shows the descriptive statistics (mean and standard deviation) for durations and amplitudes, distinguishing between upturns and downturns. The structure of the dataset is

Table 3.2: Duration of Phases (Quarters)

Notes: Italics denote ongoing upturns or downturns. “U” indicates upturns and “D” indicates downturns.

	Australia	Belgium	Canada	Denmark	Finland	France	Germany	Ireland	Italy
D	19								
U	12				7				
D	21				20				
U	9				22				
D	7		33	14	7			17	7
U	14		16	9	12			11	31
D	6		12	13	16	16	17	32	20
U	32	32	8	15	26	26	19	12	24
D	6	23	18	28	8	23	32	18	21
U	*18*	*99*	*46*	55	23	43	21	47	41
D				*11*	*10*	*9*	*61*	*13*	*9*

	Japan	Korea	Nether-lands	New Zealand	Norway Norway	Spain Spain	Sweden Sweden	Switzer-land	United Kingdom	United States
D										
U				11						
D				23						
U				16						
D				10		7			16	7
U		15		6	17	8	21		12	14
D	15	39		15	27	16	25	14	6	15
U	54	10		21	14	38	17	53	30	28
D	*76*	6	27	14	23	19	24	41	27	21
U		8	*100*	27	58	44	*56*	*40*	46	47
D		*11*		*9*	*10*	*10*			*9*	*13*

Table 3.3: Descriptive Statistics

Notes: Left-censored phases (those for which the starting date precedes 1970q1 and is unknown) are excluded. The amplitude of upturns is the difference between the peak and its preceding trough, divided by its preceding trough. The amplitude of downturns is computed as the difference between the preceding peak and the trough divided by the trough.

		Duration (quarters)		Amplitude (%)	
	Sample	Mean	StDev	Mean	StDev
Complete upturns	49	24.1	14.8	61.3	56.3
Complete + ongoing upturns	55	28.0	20.6	66.7	60.1
Complete downturns	49	18.2	8.7	30.7	28.4
Complete + ongoing downturns	62	18.4	12.5	28.8	27.5

Table 3.4: Characteristics of the Last Upturn

Notes: A country is classified in an ongoing upturn if, as of 2010q2, no peak has been identified in the national index of housing prices. Amplitude measures the price change from trough to peak or, for ongoing upturns, from trough to 2010q2.

Country	Trough-Peak	Duration (quarters)	Amplitude (%)
	Completed upturns		
Denmark	1993q2-2007q1	55	176.6
Finland	2001q4-2007q3	23	37.6
France	1997q1-2007q4	43	117.8
Ireland	1994q4-2006q3	47	286.4
Italy	1997q3-2007q4	41	59.3
Korea	2005q1-2007q1	8	14.0
New Zealand	2000q4-2007q3	27	98.5
Norway	1993q1-2007q3	58	200.1
Spain	1996q3-2007q3	44	121.6
United Kingdom	1996q2-2007q4	46	160.7
United States	1995q1-2006q4	47	64.3
	Ongoing upturns		
Australia	2005q3-	18	26.3
Belgium	1985q2-	99	186.7
Canada	1998q3-	46	86.9
Netherlands	1985q1-	100	199.8
Sweden	1996q1-	56	140.2
Switzerland	2000q1-	40	22.5

such that every country has an ongoing upturn or downturn at the time of the last observation (2010q1). The descriptive statistics are computed with and without those censored phases. The dataset contains 49 completed upturns, 49 complete downturns, 6 right-censored upturns, and 13 right-censored downturns. On average, upturns last more than downturns, consistently with Claessens et al. (2011). Not surprisingly, the amplitude of upturns is also larger. In terms of standard deviations, downturns display less duration variability than upturns, which hints at the "clustering" of downturn durations discussed in the next section.

3.3.2 The Role of the Last Upturn

The house price boom that OECD countries experienced at the end of the 20th century and the first part of this century was exceptional under many aspects, not least for its duration (Girouard et al., 2006). For each country, Table 4 shows the dates for the last upturn as detected by the BBQ algorithm.[15] Germany and Japan are excluded because they have been experiencing a house price downturn since the nineties. The table distinguishes between countries whose upturn is terminated and countries whose upturn is still ongoing (see Igan

[15]The last datapoint available is 2010q1. By construction, the dating algorithm avoids choosing turning points that are in the last year and a half of data. For those points, it is not possible to construct the window of observations over which local maxima and minima are computed.

and Loungani, 2010, for a discussion of this dichotomy). Most national indices started to rise in the middle of the 1990s; Belgium and Netherlands have been experiencing rising house prices since 1985. It is not surprising that the amplitude of these price movements has been considerable. Ireland's index nearly tripled between 1994 and 2006.

The fact that upturns are longer than downturns is largely due to the exceptional duration of the last upturn experienced by OECD countries. Imagine redrawing Figure 3.1 excluding the last upturn from the country charts. While the original Figure 1 gives the impression that house prices are characterized by an upward trend, the new charts would convey no such message. Regressing the real house price index on country fixed effects (α_c), and a linear time trend (t) yields:

$$INDEX_{ct} = \alpha_c \quad + \underset{(0.042)^{***}}{0.291} \quad t$$

for the complete sample, and:

$$INDEX_{ct} = \alpha_c \quad + \underset{(0.048)^{*}}{0.094} \quad t$$

for the sample excluding the last boom.[16] Once the last boom is excluded, it is not possible to reject at 5% confidence level the null hypothesis of no time trend, and the coefficient on time is much smaller than when the last boom is included. This result is consistent with Eicholtz (1997) and Shiller (2006), who study historical house price data and show that in the long run the upward trend in real house prices is negligible.

3.4 Duration Dependence

3.4.1 The Duration Distribution

Studies of economic cycles often report just the average duration of phases without describing the whole distribution of realized upturn or downturn lengths. This neglects important information. The same mean duration can stand for different distributions: in one of them the probability of ending an upturn or a downturn could be the same in every period, and in another the probability of terminating a cyclical phase could be increasing with time. In other words, the average duration is not informative about duration dependence, the property that describes how the likelihood of exiting an upturn or downturn changes at different durations.

Before explicitly analyzing the issue of duration dependence, I discuss the duration distribution of upturns and downturns found in the data. Table 3.5 shows a breakdown of this

[16] Eicker-White heteroskedasticity-consistent standard errors are shown. *** denotes 1% significance, and * denotes 10% significance.

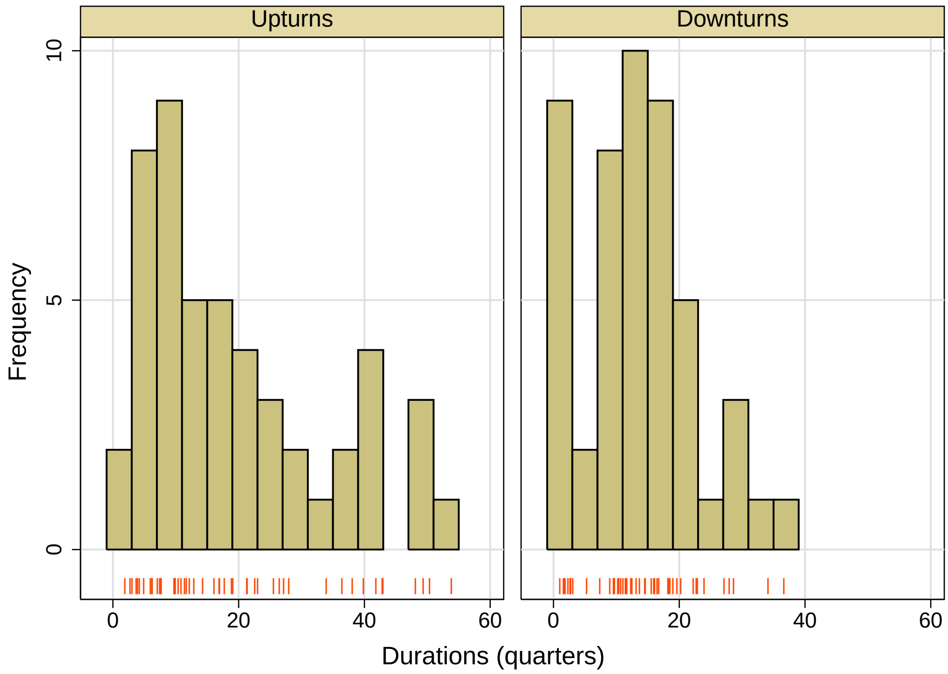
Table 3.5: Distribution of Durations

Notes: The table shows the percentile of the duration distribution for 49 complete upturns and 49 complete downturns.

	Min	Pct10	Pct25	Pct40	Median	Pct60	Pct75	Pct90	Max
Completed upturns	6	8	12	16	21	24	32	47	58
Completed downturns	6	7	13	16	17	20	23	32	41

Figure 3.2: Housing Cycles: Distribution of Durations

Notes: Only completed durations are included. To adjust for left-truncation in the dating algorithm, the first 5 observations of every upturn and downturn are discarded.



distribution by relevant percentiles. The construction of upturns and downturns is such that the minimum duration is 6 for both phases. The 10th, 25th, and 40th percentiles of the two distributions are substantially equal. Upturns are longer than downturn only above the 40th percentile. Figure 3.2 conveys this message graphically. The frequencies of upturn durations decay slowly and the frequencies of downturns cluster in the 10-20 quarter range, with very few downturns lasting more than 20 quarters.

The presence of a one-to-one relation between the distribution of realized durations and the shape of duration dependence would suggest a test for duration dependence based on the distribution in Table 3.5 (Diebold and Rudebusch, 1990). However, international housing cycles are at least partially synchronized (Ceron and Suarez, 2006; Igan et al., 2011; Claessens et al., 2011) and this feature would produce spurious clusters of durations around certain values. To control for synchronization, a regression-based test is more appropriate. In particular, the insertion of annual fixed effects allows me to control for the fact that in some years (such as 2007) peaks in housing prices are more likely to occur because of the synchronization of cycles.

3.4.2 A Nonparametric Test of Duration Dependence

Ohn et al. (2004) suggest a straightforward method to check if the upturns and downturns of a series y_t display duration dependence. Suppose the binary variable S_t takes value 1 if y_t is in an upturn and 0 otherwise. To test for duration dependence in upturns, keep all observations that belong to upturns—i.e. where $S_t = 1$—, plus the first observation of every downturn—i.e. the first observation with $S_t = 0$ after a sequence of quarters where $S_t = 1$. Then use this subsample to estimate the following linear probability model:

$$S_t = \alpha + \beta d_{t-1} \tag{3.1}$$

where d_t is the ongoing duration of the upturn at time t. Importantly, the duration counter d_t must start from the sixth quarter, because the dating algorithm imposes a minimum phase duration. The first 5 quarters of all upturns and downturns must be removed from the sample, because by construction no turning point is associated to them (Ohn et al., 2004). In the regression output, the coefficient β represents the probability that the upturn finishes. A significant and negative β denotes duration dependence.[17] Similarly, to test for duration

[17]In theory, it is possible for the coefficient on duration to be positive. In this case the series would display *negative* duration dependence: longer upturns would be less likely to terminate. While the possibility of negative duration dependence is relevant in labor economics (e.g. long unemployment spells are less likely to end into employment), it is likely to be less relevant here.

dependence in downturns, I keep all observations where $S_t = 0$ plus the first observation with $S_t = 1$ after each sequence of zeros, and I remove the observations below the minimum phase duration. In this case, a significant and positive β denotes duration dependence.

The present test identifies a feature of housing prices that is not captured by simple random walk models. For a random walk, β would be zero both for upturns and downturns. Adding autocorrelation in price changes, as first detected by Case and Shiller (1989), does not induce duration dependence either. In Appendix A, I simulate a series of 10,000 observations from the process $y_t = 0.1 + y_{t-1} + 0.8\Delta y_{t-1} + \epsilon_t$ where ϵ_t is a white noise. I then apply the Harding and Pagan (2002) algorithm to extract the binary series S_t of upturns and downturns, using the same parameters as in the main analysis (i.e. rolling window of 13 quarters and minimum phase duration of 6 quarters). After removing the observations below the minimum phase duration, I estimate regression (1) and find that the β coefficients in upturns and downturns are zero.

In the OECD house price dataset different country indices are pulled together, and, despite the efforts to make the series homogeneous, some countries could display more price volatility just because of a different methodology in constructing the index. A greater volatility would generate more cycles, resulting in lower durations and impacting on the duration dependence test. To control for this possibility, I run regression (1) with country fixed effects (α_c), consistently with Claessens et al. (2011). Moreover, national house prices are partially synchronized, and to control for this concordance I add year fixed effects (γ_t). The equation I estimate is therefore:

$$S_{ct} = \alpha_c + \gamma_t + \beta d_{c,t-1} \tag{3.2}$$

Table 3.6 shows the estimation output. I also allow for a logarithmic and a quadratic specification of $d_{c,t-1}$. Since the last boom has had exceptional characteristics, I estimate the upturn equation with and without it. Both upturn equations indicate a significant and positive effect of duration on the probability that upturns end; the downturn equation, by contrast, displays no such effect.

This result is consistent with what Cunningham and Kolet (2011) find for U.S. and Canadian cities. It seems that house price upturns, especially in their more extreme manifestations (booms), involve a departure of prices from fundamentals. Such departures are increasingly difficult to sustain, leading to the duration dependence we see in the data. Speculation and overbuilding are two real-world mechanisms that make the probability of a house price reversal higher and higher as booms get longer. The same is not true for downturns: their

Table 3.6: Duration Dependence Test

Notes: Results from estimating $S_t = \alpha_c + \gamma_t + \beta x$, where x is one of the variables listed in the first column. For upturns, a negative coefficient indicates a positive effect of duration on the likelihood of terminating the phase. For downturns, the same effect is indicated by a positive coefficient—this is because $S_t = 1$ for upturns and $S_t = 0$ for downturns. The first 5 observations of each upturn and downturn are excluded because the algorithm does not allow any phase to last less than 6 quarters. ***, **, and * denote 0.1%, 1% and 5%, significance respectively, computed using Eicker-White standard errors.

	Upturns			Upturns without last boom			Downturns		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\ln d_{c,t-1}$	-0.0450***			-0.0822***			0.0201		
	(0.0079)			(0.0146)			(0.0141)		
$d_{c,t-1}$		-0.0025***	-0.0050***		-0.0083***	-0.0138**		0.0020	0.0052
		(0.0006)	(0.0012)		(0.0020)	(0.0044)		(0.0013)	(0.0028)
$d^2_{c,t-1}$			0.0000			0.0001			-0.0001
			(0.0000)			(0.0001)			(0.0000)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,273	1,273	1,273	566	566	566	825	825	825

termination probability does not significantly depend on current duration.[18]

3.4.3 Lagged Duration Dependence

The above results suggest a boom-bust view of housing cycles, where deviations from equilibrium housing prices occur during expansions and adjustments occur during contractions. This asymmetric view of housing cycles implies a precise ordering of phases: boom-bust rather than crash-recovery. Hence, the duration of downturns should depend on the duration of previous upturns, but not vice versa. This hypothesis, termed "lagged duration dependence," is testable.

Table 3.7 shows the results from estimating the following regression:

$$S_t = \alpha_c + \gamma_t + \beta_1 d_{c,t-1} + \beta_2 l_{c,t-1}, \tag{3.3}$$

where $l_{c,t-1}$ represents the duration of the previous phase—upturn or downturn. As in Table 3.6, durations and lagged durations are also inserted in logarithmic and quadratic form. The coefficients on the duration term, $d_{c,t-1}$, are very similar to those in Table 3.6: upturns show clear duration dependence across all specifications, whereas downturns do not—except in the quadratic specification, where the linear term is marginally significant. By contrast, the coefficients on lagged duration, $l_{c,t-1}$, are never significant in predicting the end of upturns, but are significant at the 10% level in predicting the end of downturns in the logarithmic and linear specifications.

Downturns that follow long upturns are more likely to last longer, consistently with a boom-bust view of housing price dynamics. Claessens et al. (2011) also include lagged duration dependence in their regressions of housing price downturn and reach the same conclusion. Interestingly, they dont find any lagged duration dependence when studying equity cycles or credit cycles. This particular pattern seems to be a specific feature of housing cycles.

3.4.4 Inspecting the Mechanism: The Role of Fundamentals

If the reason for duration dependence in upturns and lagged duration dependence in downturns lies in a non-fundamental boom-bust component of housing prices, then the inclusion of macroeconomic variables in Equation 3.3 should not alter the main result. To verify this hypothesis, I augment the linear probability model with the following variables:

[18] Claessens et al. (2011) limit their duration dependence analysis to downturns, and find a significant positive effect. The different BBQ algorithm they employ (with minimum upturn and downturn duration of just 2 quarters) and the absence of year dummies are sufficient to explain the discrepancy between their results and the ones presented here.

Table 3.7: Lagged Duration Dependence Test

Notes: Results from estimating $S_t = \alpha_c + \gamma_t + \beta_1 d_{c,t-1} + \beta_2 l_{c,t-1}$, where $d_{c,t-1}$ and $l_{c,t-1}$ are inserted also in logarithmic and quadratic form. For upturns, a negative coefficient indicates a positive effect of duration on the likelihood of terminating the phase. For downturns, the same effect is indicated by a positive coefficient—this is because $S_t = 1$ for upturns and $S_t = 0$ for downturns. The first 5 observations of each upturn and downturn are excluded because the algorithm does not allow any phase to last less than 6 quarters. ***, **, and * denote 0.1%, 1% and 5%, significance respectively, computed using Eicker-White standard errors.

	Upturns			Upturns without last boom			Downturns		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\ln d_{c,t-1}$	-0.0319***			-0.0553***			0.0169		
	(0.0072)			(0.0110)			(0.0118)		
$d_{c,t-1}$		-0.0020***	-0.0043***		-0.0062***	-0.0089*		0.0017	0.0053*
		(0.0004)	(0.0010)		(0.0013)	(0.0040)		(0.0011)	(0.0021)
$d^2_{c,t-1}$			0.0000			0.0001			-0.0001*
			(0.0000)			(0.0001)			(0.0000)
$\ln l_{c,t-1}$	-0.0091			-0.0083			-0.0335*		
	(0.0089)			(0.0234)			(0.0152)		
$l_{c,t-1}$		-0.0012	-0.0036		-0.0013	-0.0038		-0.0025*	-0.0042
		(0.0008)	(0.0029)		(0.0030)	(0.0070)		(0.0010)	(0.0032)
$l^2_{c,t-1}$			-0.0002			0.0002			0.0000
			(0.0001)			(0.0002)			(0.0001)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,260	1,260	1,260	553	553	553	819	819	819

- *the level of short-term nominal interest rate* ($INT_{c,t}$), because it influences the expected returns on housing and affects mortgage financing conditions;
- *the inflation rate* ($INFL_{c,t}$), to separate the effect real interest rates from the effect of inflation, and because recent analyses suggest that inflation might play a role in determining aggregate housing prices (Brunnermeier and Julliard, 2008);
- *the growth rate of real credit to the private sector* ($CREDIT_{c,t}$), because the recent crisis has shown that there are various channels influencing the availability of credit, over and above the level of interest rates (Dell'Ariccia et al., 2008);
- *the growth rate of the real gross domestic product* ($GDP_{c,t}$), because of the obvious link between income and housing demand; and
- *the growth rate of working age population* ($WAP_{c,t}$), because of the relation between demography and housing demand.

The above variables are the same as the ones used by Agnello and Schuknecht (2011), with the exception of the inflation rate, which is an addition of the present paper. These variables capture the demand for housing. The extent to which the demand for housing affects prices depends on the elasticity of supply. Ideally, one would like to have a time-varying measure of housing supply elasticity for all the 19 countries of the sample. In practice this measure does not exist. However, it is reasonable to assume that housing supply elasticities are quite persistent over time. If this is the case, most of the variation in supply responses is between countries, and thus captured by the country fixed effects α_c.[19]

Table 3.8 shows the output of estimating:

$$S_{c,t} = \alpha_c + \gamma_t + \beta_1 d_{c,t-1} + \beta_2 l_{c,t-1} + \delta X_{c,t-1} \tag{3.4}$$

where $X_{c,t-1}$ are the macroeconomic fundamentals. The evidence of duration dependence in upturns is still strong, despite the use of quite a few variables on a dataset that covers just 19 countries with 161 observations each. Downturns, this time, show some duration dependence (the coefficient is significant at the 10% level). More importantly, lagged duration dependence is significant at the 5% level for downturns only. The effect of duration on the likelihood of terminating an upturn is measuring a feature of housing cycles that is independent of macroeconomic fundamentals.

[19]Caldera Sánchez and Johansson (2011) provide a time-invariant measure of housing supply elasticity for the OECD countries studied here. It could be that housing supply elasticities enter equation 4 not only in levels (which are captured by country fixed effects), but also in an interaction with duration. Such specification, however, yields an insignificant interaction term. Results are available from the author upon request.

Table 3.8: Duration Dependence and Fundamentals

Notes: Results from estimating $S_{c,t} = \alpha_c + \gamma_t + \beta_1 d_{c,t-1} + \beta_2 l_{c,t-1} + \delta X_{c,t-1}$, where X is the vector of variables listed in the first column. ***, **, and * denote 0.1%, 1% and 5%, significance respectively, computed using Eicker-White standard errors. I use three-month money-market rates as the nominal interest rate ($INT_{c,t}$), and the national Consumer Price Index (CPI) as the inflation rate ($INFL_{ct}$). $GDP_{c,t-1}$, and $CREDIT_{c,t-1}$ are expressed as deviations from the trend (computed with a Hodrick-Prescott filter). $INT_{c,t-1}$ and $INFL_{c,t-1}$ enter as annualized changes, whereas $WAP_{c,t-1}$ is expressed as annualized growth.

	Upturns	Upturns without last boom	Downturns
	(1)	(2)	(3)
$d_{c,t-1}$	-0.0020**	-0.0059**	0.0034*
	(0.0006)	(0.0013)	(0.0012)
$l_{c,t-1}$	-0.0003*	0.0002	-0.0031**
	(0.0007)	(0.0032)	(0.0010)
$INT_{c,t-1}$	-0.0131	-0.0229*	-0.0105*
	(0.0077)	(0.0106)	(0.0040)
$INFL_{c,t-1}$	0.3044	0.2271	-0.4513
	(0.6616)	(1.0041)	(0.4985)
$CREDIT_{c,t-1}$	0.0244	0.1310	-0.006
	(0.0289)	(0.0773)	(0.1515)
$GDP_{c,t-1}$	0.3754	0.6028	0.1401
	(0.5081)	(0.6302)	(0.3005)
$WAP_{c,t-1}$	1.4333	0.4828	10.2240**
	(2.4625)	(3.487)	(2.9978)
Year Fixed Effects	✓	✓	✓
Country Fixed Effects	✓	✓	✓
Observations	1,223	547	817

In terms of coefficients on $X_{c,t-1}$, positive changes in real interest rates—nominal interest rates controlling for inflation—are associated with an increase in the probability that an upturn ends, and a decrease in the probability that a downturn ends. The only other variable that has a significant effect is the growth rate of the working age population, which affects positively the likelihood of exiting a housing price downturn.

Before concluding, notice that the analysis relies on a simple linear probability model (LPM). Rather than using a LPM, most papers that estimate probabilities associated with housing price upturns and downturns use a probit/logit link function, so that $S_{c,t} = g^{-1}(\alpha_c + \gamma_t + \beta d_{c,t-1})$ is restricted between 0 and 1 (Borio and McGuire, 2004; Agnello and Schuknecht, 2011; Cunningham and Kolet, 2011). Other authors (Castro, 2010; Claessens et al., 2011) use link functions common in survival/duration analysis, such as Weibull (in a continuous-time setting) or log-logistic (in a discrete-time setting). I choose to stick to a LPM as my main specification. The objective of Equation 3.2 is diagnostic (Ohn et al., 2004): determining whether the data provide significant evidence in favor of duration dependence. A simple OLS approach is preferable when the goal is to keep the analysis as nonparametric and transparent as possible, instead of precisely quantifying marginal effects that depend on functional assumptions. As a robustness check, in Appendix C.3.2 I replicate all the regressions of the paper using a logit function. The results on duration dependence, lagged duration dependence, and the effect of macroeconomic variables remain the same.

3.5 Conclusion

In this chapter, I study 40 years of housing cycles in 19 OECD countries and concentrate on one specific characteristic: duration. The descriptive analysis shows that upturns have been longer than downturns on average, but this difference is largely due to the last house price boom, which was particularly long. When I focus on the entire distribution of durations and I test for duration dependence, I show that house price upturns are more likely to end as they get longer, whereas house price downturns are not. This result holds independently of whether the last boom is included or not in the sample. Moreover, the duration of upturns affects the duration of subsequent downturns: long expansions are followed by long contractions.

These results on duration dependence bring forward two insights. First, since duration dependence is not a feature displayed by standard linear stochastic processes, aggregate house price indices behave in a nonlinear way. These nonlinearities can be accounted for using Markov regime-switching models (Ceron and Suarez, 2006) or models with conditional heteroskedasticity (Miles, 2008). Without the need to engage with more complex models, the notion of duration dependence provides an intuitive way to think about these nonstandard features.

The second insight relates to our theoretical understanding of house price cycles. "Hot" housing phases produce fast appreciation, high transaction volumes, and overbuilding; "cold" housing market phases, by contrast, are characterized by low transaction volumes and slow nominal price adjustment (Leamer, 2007). It seems natural to frame cyclical upturns as hot periods in which housing valuations depart from fundamentals, and downturns as periods in which corrections need to take place. In such a framework, the probability of a house price reversal increases as the imbalances of the upturn grow larger, and this generates the statistical regularity of duration-dependent upturns.

From a practical perspective, this paper contributes to the current policy debate on how to deal with real estate booms (Allen and Carletti, 2010; Crowe et al., 2011). In all the countries analyzed here, house prices are cyclical: no nation has ever lived through a perennial house price expansion or contraction. The question is where and when house price reversals are more likely to happen. Despite the usual caveats associated with econometric estimates, it seems fairly safe to conclude that an "overheated" economy—perhaps because of an unusually long house price upturn—is more likely to initiate a housing price downturn. Moreover, the duration of downturns depends on the length of previous expansions: longer booms lead to longer contractions.

A.1 Appendix to Chapter 1

A.1.1 Housing Statistics for Central-Western London

The first two columns of Table A.1 refer to the local authorities covered by the JDW Dataset, the third and the fourth columns refer to the whole London area, and the fifth and sixth colums refer to England.

The upper panel takes data on sales from the 2011 Land Registry. In England as a whole, houses constitute 81% of sales, whereas they are only half of sales in London, and only one quarter of sales in Central-Western London. The median sale price in Central-Western London is more than two times and a half the median English price.

The middle panel takes data on housing tenure from the 2001 Census. Going from England to London and then to Central-Western London, the percentage of owner occupied properties goes down, and the percentage of privately rented properties goes up. A quarter of properties in Central-Western London belong to privately rented market. The percentage of properties rented by a social landlord (either a local authority or a registered housing association) is also higher in London and Central-Western London.

The bottom panel takes data on house building from the U.K. Communities and Local Government Department.[20] (These data are not available at the local authority level). The figures show that, both in England and London, house building tend to focus more on flats than houses, compared to the composition of the existing stock. Within flats, most of the building activity is centered on 2-bedroom flats.

TABLE A.1: GENERAL HOUSING STATISTICS

	Cent.-West London		London		England	
	#	%	#	%	#	%
			Sales (Land Registry 2011)			
Flats	12,318	*0.75*	46,832	*0.51*	121,092	*0.19*
Houses	4,148	*0.25*	44,891	*0.49*	504,909	*0.81*
(Median price)	(£480,000)		(£287,000)		(£185,000)	
			Stock (Census 2001)			
Owner occupied	188,191	*0.44*	1,675,690	*0.58*	13,920,429	*0.71*
Rented from private landlord	108,084	*0.25*	432,482	*0.15*	1,798,864	*0.09*
Rented from social landlord	132,352	*0.31*	790,371	*0.27*	3,940,728	*0.20*
			New supply (Local statistics 2001–2011)			
1-bedroom flats			46,658	*0.24*	137,006	*0.09*
2-bedroom flats			106,506	*0.54*	413,902	*0.29*
3-bedroom+ flats			10,433	*0.05*	14,421	*0.01*
Houses			35,237	*0.18*	879,721	*0.61*

[20] `http://www.communities.gov.uk/housing/housingresearch/housingstatistics/housingstatisticsby/housebuilding/livetables/`.

A.1.2 Summary statistics for expectation survey respondents

The January 2012 John D Wood & Co. online survey of expectations asked respondents for many demographic information, which are summarised below. Respondents are mostly males, married with children, graduated, and homeowners. The sample is not representative of the general UK population, but is reasonably consistent with the expected profile of a home buyer in Central London.

The characteristics in Panel A were asked at the beginning of the online questionnaire, while the characteristics in Panel B were asked at the end. It is common for a percentage of respondents of online questionnaires to drop out of the survey before the end. This explains the lower number of observations for characteristics listed in Panel B.

TABLE A.2: SUMMARY STATISTICS

Variable	%	Obs.	Variable	%	Obs.
		Panel A: Housing			
Residence			*Housing tenure*		
Outside UK	0.07	510	Homeowner (mortgage)	0.50	451
UK outside London	0.47	510	Homeowner (outright)	0.33	451
London, prime neighbourhood	0.16	510	Renting	0.01	451
London, other neighbourhood	0.30	510	Other	0.16	451
		Panel B: Socio-demographic characteristics			
Gender			*Age*		
Male	0.69	293	Less than 31	0.06	294
			31-40	0.32	294
			41-50	0.25	294
Marital status			51-60	0.20	294
Single	0.15	294	61-70	0.15	294
Cohab. (child)	0.03	294	Over 70	0.02	294
Cohab. (no child)	0.06	294			
Married (child)	0.50	294	*Income*		
Married (no child)	0.16	294	Hhold income <£50,000	0.11	278
Separated/divorced	0.07	294	£50,000-100,000	0.29	278
Widowed	0.02	294	£100,000-200,000	0.30	278
			Over £200,000	0.29	278
Education			*Occupation*		
GSCE	0.05	295	Student	0.01	297
A-level / Bacc.	0.09	295	Employed	0.56	297
University degree	0.43	295	Self-employed	0.23	297
Masters	0.32	295	Looking for a job	0.01	297
PhD	0.04	295	Retired	0.12	297
Other	0.06	295	Other	0.08	297

A.1.3 Comparing the JDW Sales Dataset with the U.K. Land Registry

This part of the Appendix studies the subset of observations in the Land Registry that belong to the postcode districts listed in Table 1.1.

Figure A.1: Quarterly registered sales (Land Registry)

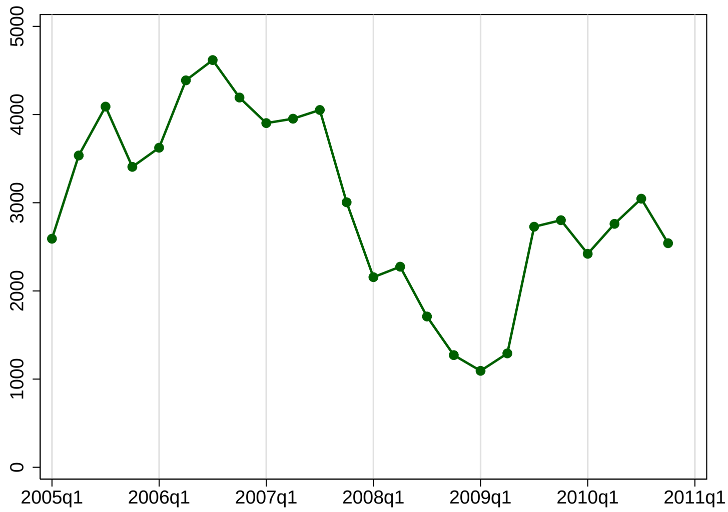


Table A.3: Property characteristics (Land Registry)

	2005-2010
Median price (in 2005 £)	427,948
Flat (%)	0.81
Terraced house (%)	0.16
Semi-detached house (%)	0.02
Detached house (%)	0.01
Newly built (%)	0.06
Total observations	71,459

Table A.4: Repeat sales (Land Registry, 2006–2010)

Dataset	Units that appear … times	
Land Registry sales	1	52,167
	2	2,650
	3	117
	4	4

Figure A.2: Price indexes (Land Registry)

Notes: Indexes are normalised to zero in 2006Q1, start of repeat sales index. The hedonic index is computed using the two variables available in the Land Registry: property type (flat, terraced house, semi-detached house, and detached house) and whether the property is new.

B.2 Appendix to Chapter 2

B.2.1 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.2.2 Additional Tables

Table B.1: 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

Table B.2: Entrepreneurs with Dependent Workers—Robustness and heterogeneity

Notes: All regressions include year dummies; monthly Dummies; SIC92 1-digit sector dummies; and individual controls. See notes to Table 2.2 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)).

	(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	-0.013	-0.01	-0.014	-0.01	-0.015	-0.012	-0.013	-0.003
	$(0.005)^{**}$	$(0.005)^{**}$	$(0.006)^{+}$	$(0.006)^{*}$	$(0.006)^{+}$	(0.013)	$(0.006)^{*}$	$(0.007)^{*}$	(0.004)

C.3 Appendix to Chapter 3

C.3.1 The Distribution of Phase Durations for an ARIMA(1,1,0) Process

Chapter 3 studies features of national housing price cycles, such as duration dependence and lagged duration dependence, which are not captured by simple random walk models. In this Appendix I simulate a process that contains the properties that are usually associated with house price indexes. First, the series must contain a unit root—Igan et al. (2011), using the same house price data as the present paper, show that almost every national series is at least integrated of order 1. Second, I insert a drift in the growth rate of the series to allow for an upward trend. Third, since the seminal paper of Case and Shiller (1989), a large literature has shown that house price growth is in part predictable, i.e. price changes are autocorrelated. Glaeser and Gyourko (2006) show that in the U.S. a 1 dollar increase in real constant quality house prices in one year is associated with a 60-80 cent increase the next year. To match this feature, I impose a first-order autocorrelation of 0.8 on house price growths.

I randomly generate 100,000 observations from the ARIMA(1,1,0) process $y_t = 0.1 + y_{t-1} + 0.8\Delta y_{t-1} + \varepsilon_t$ and apply the same dating algorithm used for national house prices. I count 2248 upturns and 2248 downturns, and plot the distribution of their durations in Figure C.1. Durations are corrected for left-truncation by removing the first 5 observations. The frequencies of both upturn and downturn durations show the decaying pattern typical of a geometric distribution. In general, a geometric distribution $f(x) = (1-\lambda)^{x-1}\lambda$ represents the probability of getting $x-1$ successes before encountering a failure in a sequence of Bernoulli (binary) trials. The crucial feature of such distribution is that failure probability λ is constant. In the context of macroeconomic cycles, this means that the probability of an upturn (downturn) ending is the same no matter how long the upturn (downturn) has lasted. This is equivalent to the absence of duration dependence.

To confirm the result of no duration dependence, I run the regression described by Equation 3.1 on the simulated upturns and downturns. For upturns I get:

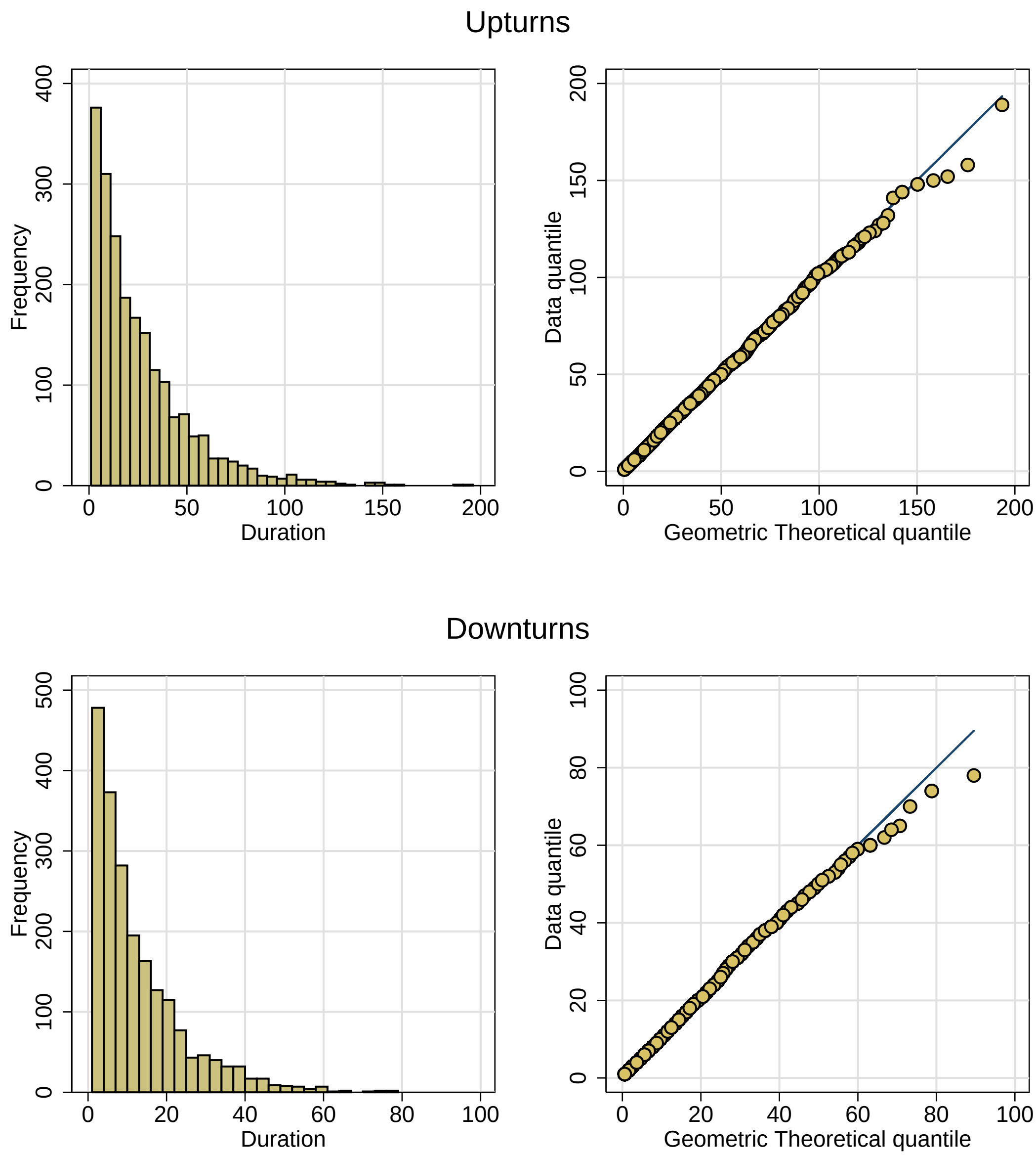
$$S_t = \underset{(0.00133)^{***}}{.96159} \; - \; \underset{(.00003)}{.00001} \; d_{t-1}$$

and for downturns I get:

$$S_t = \underset{(0.00322)^{***}}{.08223} \; - \; \underset{(.00016)}{.00003} \; d_{t-1}$$

Figure C.1: Simulated Phase Durations of an ARIMA (1,1,0) Process

Notes: 100,000 observations from the ARIMA (1,1,0) process $y_t = 0.1 + y_{t-1} + 0.8\Delta y_{t-1} + \varepsilon_t$ were generated. On these observations, the same BBQ dating algorithm used for the national house prices was applied. Durations are adjusted for left-truncation—i.e., the first 5 observations of each upturn and downturn are discarded. The parameter λ of the theoretical geometric distribution corresponds to the unconditional probability of an upturn (downturn) ending. It is computed as the ratio between the number of complete upturns (downturns) and the total amount of quarters spent in upturns (downturns).



C.3.2 Results from Logit Regressions

Table C.1: Logit Duration Dependence Test

Notes: The table displays the results of logit regressions. ***, **, and * denote 0.1%, 1% and 5%, significance respectively.

	Upturns			Upturns without last boom			Downturns		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\ln d_{c,t-1}$	-1.4574**			-1.9206*			0.5353		
	(0.4894)			(0.8280)			(0.3835)		
$d_{c,t-1}$		-0.0923**	-0.2170**		-0.2299*	-0.2088		0.0785	0.2601
		(0.0299)	(0.0774)		(0.0899)	(0.1280)		(0.0447)	(0.1335)
$d^2_{c,t-1}$			0.0018			-0.0007			-0.0053
			(0.0010)			(0.0040)			(0.0036)
$\ln l_{c,t-1}$	-0.1538			-0.3929			-0.6648*		
	(0.3966)			(0.5996)			(0.3328)		
$l_{c,t-1}$		-0.0084	0.1092		-0.0601	0.0623		-0.0781*	-0.0554
		(0.0507)	(0.106)		(0.0778)	(0.2197)		(0.0316)	(0.0904)
$l^2_{c,t-1}$			-0.0049			-0.0042			-0.0004
			(0.0033)			(0.0066)			(0.0023)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cntr. FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Obs.	762	762	762	427	427	427	583	583	583

Table C.2: Logit Duration Dependence and Fundamentals

Notes: The table displays the results of logit regressions. ***, **, and * denote 0.1%, 1% and 5%, significance respectively.

	Upturns	Upturns without last boom	Downturns
	(1)	(2)	(3)
$d_{c,t-1}$	-0.1089***	-0.2396**	0.1053*
	(0.0315)	(0.0886)	(0.0459)
$l_{c,t-1}$	0.0052	-0.0373	-0.0945**
	(0.0529)	(0.1039)	(0.0334)
$INT_{c,t-1}$	-0.3555$*$	-0.3633	-0.2063*
	(0.1613)	(0.2257)	(0.0824)
$INFL_{c,t-1}$	5.8748	-11.0632	-15.5223
	(16.8819)	(27.7996)	(16.2156)
$CREDIT_{c,t-1}$	1.9014	2.4147	-2.3314
	(2.1090)	(3.9344)	(3.2209)
$GDP_{c,t-1}$	3.8564	1.9473	13.5798
	(10.4737)	(12.7093)	(9.1577)
$WAP_{c,t-1}$	75.7709	79.5835	223.5637*
	(85.5395)	(117.215)	(110.374)
Year Fixed Effects	✓	✓	✓
Country Fixed Effects	✓	✓	✓
Observations	747	423	581

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