House Money and Entrepreneurship*

Sari Pekkala Kerr
Wellesley College

William R. Kerr
Harvard Business School

Ramana Nanda
Harvard Business School

September 2014
PRELIMINARY CONFERENCE VERSION

Abstract

We examine the relationship between house prices and entrepreneurship using micro data from the US Census Bureau. Increases in house prices are often thought to drive entrepreneurship through unlocking the collateral channel for bank loans, but this interpretation is challenged by worries regarding omitted variable biases (e.g., rising local demand) or wealth effects (i.e., that wealthier people are more likely to enter entrepreneurship for reasons other than access to collateral). We construct an empirical environment that utilizes very localized price changes, exploits variations in initial home values across residents in the same zip code, and embeds multiple comparisons (e.g., owners vs. renters, homestead exemption laws by state). For the United States during the 2000-2004 period, the link of home prices to the rate of entrepreneurship is relatively small in economic magnitude. This is despite a focus on a time period that experienced the largest concentration of US home price growth over the last two decades. While collateral plays a role in the entry that we observe, wealth effects appear to be more important.

JEL Classification: E44, G21, L26, M13, R12, R31, R32.

Key Words: house prices, mortgages, collateral channel, entrepreneurship, entry.

*Comments are appreciated and can be sent to skerr@wellesley.edu, wkerr@hbs.edu and RNanda@hbs.edu. We are grateful to Manuel Adelino, Nava Ashraf, and Jim Davis and to seminar participants at NYU Stern and L'Université du Québec à Montréal for helpful comments. The research in this paper was conducted while the authors were Special Sworn Status researchers of the US Census Bureau at the Boston Census Research Data Center (BRDC). Support for this research from the NSF grant ITR-0427889 [BRDC] and the Division of Research and Faculty Development at Harvard Business School is gratefully acknowledged. Research results and conclusions expressed are the authors and do not necessarily reflect the views of the Census Bureau or the NSF. This paper has been screened to ensure that no confidential data are revealed.

1 Introduction

To what extent do small businesses face credit constraints in countries with well-developed capital markets? If they do, what role can housing collateral play in alleviating these constraints? These questions have received renewed interest following the sharp decline in home values during the 2008 financial crisis, where academics and policy makers have argued that the reduction in the value of housing collateral may have led to a decline in entrepreneurship.

The potential role of the collateral channel in entrepreneurship is intuitive. Debt financing is important for small and young businesses (e.g., Berger and Udell, 1998; Robb and Robinson, 2014), but the challenges associated with asymmetric information in small business lending are difficult for banks to overcome (e.g., Stiglitz and Weiss, 1981). Pledging personal collateral against business loans aids the lending process, and thus increases in the value of a potential or current entrepreneur's home raises the value of the collateral they can pledge to the bank and may therefore boost the willingness of banks to lend to their business. The degree to which the collateral channel alleviates credit constraints is thus of particular interest to policy makers, because reforms associated with homestead exemptions in bankruptcy or subsidies to mortgage financing can change the relative costs of owning a home or the value of housing collateral to the bank, and hence could directly impact small businesses' access to external finance (e.g., Berkowitz and White, 2000; Cerqueiro and Penas, 2014; Bracke, Hilber, and Silva, 2014).

Studies that exploit regional variation in house price changes to quantify the impact of the collateral channel often judge the effects to be significant. For example, Schmalz, Sraer, and Thesmar (2014), Fairlie and Krashinksky (2012), and Harding and Rosenthal (2013) find large elasticities when examining entry into entrepreneurship. Adelino, Schoar, and Severino (2013) trace the collateral channel to job creation by small firms. Black, de Meza and Jeffreys (1996) provide some of the earliest evidence in this regard. While our paper is related to this work, features of our data allow us to take a new empirical approach and consider detailed variation across home owners within zip codes. This affords greater assurance against omitted variables, such as booming local demand and economic growth, that can bias estimates on house prices and their relationship to new firm formation. The data also allow greater progress at disentangling collateral effects from other channels that could couple house price appreciation and entry—most prominently, wealth effects, where growing house prices increase the wealth levels of entrepreneurs and lead them to start new businesses, *independent of collateral requirements of banks*. As Hurst and Lusardi (2004) demonstrate, the non-linear nature of wealth levels and entrepreneurship make this question especially tricky.

Our setting is a unique laboratory to study these questions—the United States after 2000. We focus on 2000-2004, using data both before and after as described below. Home price appreciation during this period was massive, averaging 45% for our sample and 43% for the

country as a whole. Figure 1 shows how this period holds the most rapid price appreciation since 1990, and provides a special opportunity to look for effects. The advantage of this "event study" goes beyond the size of the treatment, as the decision to buy a house before 2000 was unlikely to be driven by the expectation of rapid increases in the availability of collateral.

Yet, we overall find the impact of this rapid price growth to be quite modest for the likelihood of engaging in entrepreneurship. We have precisely-estimated effects that show home owners in 2000 experiencing rapid price appreciation to be more likely to engage in entrepreneurship by 2004, consistent with the work of others, but the economic size of the effect is modest. To convey this, consider two identical individuals (e.g., age, education, incomes, earnings history) living in the same zip code. The only difference between them is that one individual owns a house at the 25th percentile of value in 2000 (~$95,000) and the other at the 75th percentile (~225,000). If their zip code experienced the national average growth of 45% in prices during this period, we would expect the first individual to unlock about $43,000 of additional collateral (initial value x price growth), while the second would garner about $102,000. Thus, the individual at the 75th percentile would gain a little under $60,000 additional collateral compared to the individual at the 25th percentile. Yet, despite this large differential in gain, we only estimate a 0.3% higher boost for the individual at the 75th percentile becoming a business owner in 2004 (a net effect inclusive of entry into entrepreneurship and exits out of business ownership). This effect is statistically different from zero, but it is quite small relative to the sample average of 6.7% in 2004 for business ownership (i.e., a 4.4% relative effect).

Thus, one central finding of this study is that house price growth during this period had limited consequences for entry decisions in specifications formulated to discern potential roles for the collateral channels and/or wealth effects. While we come to the conclusion using price variation within zip codes, it is worth noting that our findings are in concordance with the macro trends. Figure 2, taken from Decker et al. (2014), shows the long-term trend decline in the share of US business activity in startups and young firms. There appears to be some slight leveling or bump up during 2000-2004, but the massive house price growth is clearly not matched by the trend in entrepreneurship.

The effects that we measure are free of aggregate demand biases, but their interpretation remains challenging. In the above example the $60,000 differential could mean that banks can lend more to the new business, which is often assumed to be the driving role. However, the individual at the 75th percentile is also $60,000 wealthier, and wealth effects alone may be driving the 0.3% differential estimated. The wealth effects can encompass engaging in entrepreneurship as a luxury/consumption good, having a greater nest egg and thus tolerance for the income risk and variability associated with firm ownership, and similar. It is essential to note that in these settings, the house price elasticity for entry is properly identified and causal in nature, but the

interpretation as collateral effects is incorrect.

We are not able to fully decompose these two, but we do provide evidence that suggests wealth effects are likely more important than collateral effects. First, we find that the boost in entrepreneurship towards capital-intensive industries is not very different from industries that are less intensive, whereas we might have affected a differential to exist if collateral was opening up larger bank loans. Second, there is extensive cross-state variation in homestead exemptions associated with personal bankruptcies. These exemptions are designed to protect home owners from losing their home to creditors, and they vary dramatically across states (e.g., being unlimited in Florida to being limited less than $20,000 in some states). Prior work shows these exemptions impact credit access from banks, even for collateralized loans, because banks foresee the limits on their ability to seize collateral in defaults (e.g., Cerqueiro et al., 2014). Thus, states with lower homestead exemptions are believed to have more robust use of homes as collateral for bank loans since banks know that they can collect more back. When we split our sample, states with unlimited homestead exemptions show a stronger elasticity than other states, which is the opposite of what one would expect if collateral played a key role. These findings, along with others noted later, suggest overall that wealth effects are likely more important than collateral effects in our sample. At the very least, our analyses suggest that we are estimating an upper bound for the importance of the collateral channel in entrepreneurship.

Section 2 reviews the literature on the relationship between house prices and entrepreneurship, and Section 3 describes our data. We construct a unique platform that combines the Longitudinal Employer-Household Database (LEHD) and the 2000 Decennial Census of Population. The LEHD provides linked employer-employee records such that we can identify the formation of new firms and the initial employees within these firms. The 2000 Census provides us many important details for respondents: income, employment, demographic characteristics, home ownership status, home values, etc. One key piece is the zip code location of a person's home in 2000. This information, combined with trends in house prices for zip codes, allows us to construct very localized price changes and expected price appreciations. A second key piece of information is the reported value of homes in 2000. Due to our large sample, we can use this variation across initial home values to identify home price appreciation effects even after controlling for zip code level fixed effects. This platform thus offers us a tighter connection than previously possible for linking appreciations in home value with economic behavior while also controlling very closely for aggregate demand effects and other correlated factors. Subject to caveats described later, we also confirm our results are not following from aggregate demand effects by using the local housing supply potential due to city topology quantified by Saiz (2010).

Moreover, we are able to use renters as a "placebo" test for our specifications. As we describe in more detail later, renters are not a true placebo given both positive and negative spillover

effects from local house price appreciation, but they do provide us an important comparison point for assessing whether we have identified effects that are truly consistent with collateral or wealth effects. Our data provide monthly rental payments, from which we can estimate the initial value of the dwellings occupied by renters in 2000. We construct a mirror-image analysis using these initial implied values for renters and local house price growth, and we argue that a necessary, but not sufficient, condition for associating entrepreneurship growth due to collateral mechanisms following house price gains is that a null effect be observed for renters. We establish econometric conditions that achieve this goal in Section 4, and also how weaker conditions allow a renter effect to emerge.

The last section concludes. Our findings are relevant to the literature examining the importance of the collateral channel in driving entrepreneurial outcomes. Several papers document a strong relationship between house price changes and entrepreneurship, as noted above. Although not always directly comparable, our estimates are smaller in economic terms than many of the previous studies. We conclude from this work that housing prices impact entrepreneurship, but that studies need to be very careful in assessing how much of the aggregate demand effect remains embedded in their estimates. We come to a tentative conclusion that, even after the removal of aggregate demand effects, wealth effects are likely more important than collateral channels.

Our findings are relevant also to the extensive literature looking at financing constraints and entrepreneurship. A number of models suggest that individuals are either precluded from entry or that firms enter small and then grow because of the fact that they face initial financing constraints (Evans and Jovanovic, 1989; Holtz-Eakin, Joulfaian, and Rosen, 1994; Rajan and Zingales, 1998; Cooley and Quadrini, 2001; Gentry and Hubbard, 2004; Cabral and Mata, 2004; Cagetti and De Nardi, 2006; Buera, Kaboski, and Shin, 2011). Changes in local banking conditions have been connected with entrepreneurship (e.g., Black and Strahan, 2002; Kerr and Nanda, 2009). On the other hand, studies looking at entry have questioned the extent to which financing constraints are the leading driver behind entry decisions (Hurst and Lusardi, 2004). Our paper is very consistent with Jensen, Leth-Petersen, and Nanda (2014), who find a causal effect of an exogenous increase in home equity on entrepreneurship, but find that the effect is small. Our limited effects also parallel the findings of Bracke, Hilber, and Silva (2014) for the United Kingdom.

2 House Prices and Entrepreneurship

Since new businesses typically require some amount of capital investment before they can generate returns, the expected value of a new venture is an increasing function of the capital invested

in the startup, up to an optimal level. If individuals face credit constraints, then the amount they invest in the business will be less than the optimal level of capital, lowering expected income from entrepreneurship, and hence lowering the probability that the individual will become an entrepreneur. When the amount an individual is able to borrow is not directly observable, their personal wealth, and in particular their housing wealth, is a good proxy for the collateral they can post to access financing for their business. This is because debt financing is the principal form of external finance for most businesses (Robb and Robinson, 2014). Furthermore, banks often use the personal wealth of the owner to assess creditworthiness of new ventures as they have no track record of the firm's performance on which to lend to the business, even if these are young incorporated firms (Berkowitz and White, 2000).

Using data from the 1998 Survey of Small Business Finances, Table 1 shows that personal real estate is used to collateralize loans to businesses about 20% of the time, and about 10% of respondents note that lack of collateral was the official reason their most recent loan was turned down. A more systematic analysis of the importance of collateral in entrepreneurship faces some challenges. First, those who have more housing equity available to collateralize are likely to be wealthier. This correlation may descend from those who want to become entrepreneurs choosing to build up housing assets to collateralize instead of consuming them. Alternatively, higher ability individuals may be able to generate more assets of all varieties, leading to a possible omitted variables bias problem when estimating the correlation between the stock of housing equity and propensity to engage in entrepreneurship. Therefore, recent studies have used house price appreciation to exploit exogenous increases in wealth as a way to identify the impact on entrepreneurship.

While house price appreciation leads to higher collateral values and hence a higher likelihood of receiving bank financing, exploiting house price appreciation faces two challenges. First, areas with high or rising levels of economic activity will be ones where house prices increase and where entrepreneurship is likely to be particularly attractive. Separating the impact of aggregate demand from the supply side drivers of credit will be particularly important. Second, even if one can convincingly show a causal impact of house price increases on entrepreneurship, it still does not fully isolate the mechanism behind the increase in entrepreneurship. While increases in individual wealth reduce credit constraints, they also have the potential to generate wealth effects. For example, wealthy people may have lower absolute risk aversion, making them more likely to become entrepreneurs (e.g., Khilstrom and Laffont, 1979; Evans and Jovanovic, 1989), or they may have a preference for being their own boss that rises with wealth (e.g., Hamilton, 2000; Hurst and Lusardi, 2004; Hurst and Pugsley, 2011). If these mechanisms are important, they can lead to a positive association between wealth and entrepreneurship that is independent of the ability of the potential entrepreneur to access bank loans. Put differently,

an exogenous increase in wealth may affect entrepreneurship through reduced credit constraints, through wealth effects, or both.

We approach these challenges in several ways. First, we isolate the role of house price increases from local aggregate demand by controlling for aggregate factors at the zip code level and by further comparing the response by home owners to that of renters. Our use of very localized price changes is also important for truly grounding the expected appreciation of the property. We also use housing price elasticities across regions to instrument for exogenous increases in house prices. To parse out credit from wealth effects, we decompose our sample in ways that can shed light on the likelihood of a bank loan being important or even possible: the capital intensity of the industry entered, the homestead exemptions present in the state, the role of entrants versus incumbents, and similar. Many of these techniques have been used at least once in the prior literature. Our central contribution is to bring them into the unique laboratory offered by the data depicted in the next section and to structure them under settings where very localized prices and controls are used to guard against aggregate demand confounders.

3 Data Construction and Sample Statistics

3.1 Data Sources

Our study relies on a unique combination of the Longitudinal Employer-Household Database (LEHD) and the 2000 Decennial Census of Population. These datasets are confidential and housed by the U.S. Census Bureau. Built from unemployment insurance records, the LEHD identifies the employees of each firm in the United States and their quarterly compensation. It is longitudinally linked at both the firm and employee levels, allowing one to model how firm employment structures adjust over time, how new entrepreneurial firms form, and how individuals transition into entrepreneurship. This rich data source currently covers 31 states. The initial dates differ across states in terms of inclusion in the LEHD, and we focus on the 16 states that have records that begin in 1995 or earlier. This sample includes major states like California, Florida, Illinois, and Texas. The data extend through 2008. The LEHD directly records some information about individuals, such as age, gender, race, place of birth, and citizenship status. Through employment history files, one can also discern earnings and employment histories. While our sample period runs 2000-2008, our focus on states starting no later than 1995 allows us to measure meaningful income accumulation over the prior decade.

Using the unique person identifiers we match the LEHD to individual-level records contained in the 2000 Decennial Census of Population (Census).[1] The Census has long-form responses for

[1] The Census Bureau creates unique person identifiers (PIKs) that are based on Social Security Numbers (SSN)

1-in-6 of the population, and thus roughly speaking we can match 1-in-6 of our LEHD workers. The long-form is given to a random sample for a nationally representative population. With this match comes a true treasure chest of information about individuals (e.g., level of education, occupation, marital status) and their households (e.g., family composition, household income by source, home ownership and values). Importantly for our purposes, the Census asks whether the unit occupied by the respondent is rented or owned, how long the family has been living in the residence, how much the monthly rent or mortgage payment is, and what the market value of the unit is.[2]

We build a tailored dataset for the analysis of home prices and entry. We start by retaining individuals who have reported positive earnings in our 16 states in each of the three focal years 2000, 2004, and 2008. We require presence at all three points in order to understand the career transitions of these workers. As the LEHD covers only a subset of states, and only businesses paying payroll tax (unemployment insurance records) within these states, we cannot verify whether a person who is not present is unemployed, working in an uncovered state, working in the uncovered public sector, or similar. One potential worry with this approach is that the selection procedure might limit the types of individuals considered (e.g., selecting less-mobile people who are then less inclined to start something new). This is not a material concern given the very large states we consider and the high clustering of included and adjacent states shown in Figure 3). Appendix Table 1 also shows that our sample is not behaving differently with respect to mobility in the 2000 Census compared to the nation as a whole.

We match the LEHD individuals to the Census and retain persons covered by the long form. From the Census, we extract individual-level characteristics from the Person File and household and housing-unit characteristics from the Household File, and geographic location details from the geocode file. We further restrict our sample to individuals aged 25 to 50 in 2000 with non-missing and non-imputed information on all key variables. This age restriction is such that we stay reasonably far away from retirement decisions, as the oldest member of the cohort in 2008

and allow linking individuals across demographic surveys, censuses and administrative record. PIKs are internal Census identifiers that have a one-to-one correspondence with the SSNs.

[2]The exact question in 2000 is "What is the value of this property; that is, how much do you think this house and lot, apartment, or mobile home and lot would sell for if it were for sale?". Respondents selected from 28 ranges of values, with a minimum of "Less than $10,000" to a maximum of "$1,000,000 or more". We convert these to midpoints, excepting the last category that is simply assigned $1,000,000.

For a limited number of individuals we are further able to match them to the 1990 Decennial Census. The creation of individual identifiers in the 1990 Census was based on tax address files from the Internal Revenue Service, and therefore the matching is mostly limited to individuals who file for taxes as household heads. The overall match rate of the 1990 Census to the LEHD is lower and concentrated on white, non-Hispanic males who reside in urban locations and are household heads. We have used these data thus far to confirm the data accuracy of our 2000 information (e.g., we have verified that individuals saying in 2000 that they moved into their home in early 1970s also said this in the 1990 questionnaire). Sampling procedures and limited home price data for the 1990s limit the use of the 1990 match for analytical work.

will be 58. Likewise, the minimum age of 25 in 2000 means that we can compute reasonable pre-period earnings for the sample.

We extract the geographical location of the household at the spatial levels of states, counties, and five-digit zip codes. The county-level information is first used to merge in housing price data collected from Federal Housing Finance Agency (FHFA), following Adelino, Schoar, and Severino (2013). The FHFA data are considered reasonably representative of the overall home price development, although they are based on sales of single-family homes and do not include condos.[3] We merge the data at the Core Based Statistical Area (CBSA) level. A CBSA is one or more adjacent counties that have at least one urban core area of 10,000 or more in population, plus adjacent territory that has a high degree of social and economic integration with the core as measured by commuting ties. There are over 900 CBSAs currently defined, and these include 388 Metropolitan Statistical Areas (MSAs, urban core >50,000) and Micropolitan Statistical Areas. Our data cover 173 CBSAs.

For about 85% of our sample, we are able to collect zip code prices from Zillow. Zillow is an online real estate database that uses information from the Multiple Listing Service (MLS) and public record. Zillow maintains data on average home sale prices and estimates of the average home values for zip codes. The coverage of the Zillow data is in part limited by the fact that the data for small zip codes may be sparse to the extent that few home sales occur.[4] Despite these issues, the Zillow data have several advantages. First, and by no means least, showing our results with two sources of price data are important for robustness and confidence in the patterns observed. Second, the use of zip code information on price changes will allow us more extensive controls for aggregate demand changes and more refined statements about the impact of prices through home collateral versus other channels.

Our sample is quite representative of the US housing market and the opinions of respondents about their home values appear reasonable. To show this, we first take an unweighted average of the respondents' estimated home values by zip code. Our unweighted average across zip codes is $188,000, compared to $186,000 for the United States as a whole in the 2000 Zillow data. Second, for the zip codes in our sample, the correlation in the average estimated value to that in Zillow is 0.91.

Our evaluation of firm entry also utilizes the Longitudinal Business Database (LBD), another

[3]On the FHFA website it states: "The FHFA House Price Index (HPI) is a broad measure of the movement of single-family house prices. The HPI is a weighted, repeat-sales index, meaning that it measures average price changes in repeat sales or refinancings on the same properties. This information is obtained by reviewing repeat mortgage transactions on single-family properties whose mortgages have been purchased or securitized by Fannie Mae or Freddie Mac since January 1975."

[4]Zillow has data on 110 million homes across the United States, and so its value series is not limited to just those homes that were recently sold or currently for sale. While the value estimates of a single home have measurement error, the Zillow price trend data can be quite representative of actual changes in market values for local areas.

Census Bureau dataset that records annual employment at the firm and establishment level. Both the LBD and the LEHD use several levels of establishment and firm identifiers, including the State Employer Identification Number (SEIN) and its federal counterpart (EIN), that are created for tax purposes, and the overall company identifier (ALPHA) that links establishments of multi-unit companies together.[5] Following the procedures described in Haltiwanger, Jarmin, and Miranda (2013), we create for each establishment the first year during which the firm that the establishment belongs to was observed to be in operation within the LBD. We also create for each firm the number of employees that the LBD reports were working for this firm in the initial year. Approaching entrant definition in this way accomplishes several things—it builds off of the national LBD database to avoid issues related to the partial LEHD state coverage, connects SEINs as appropriate into parent firms, and ensures a consistent definition of entry with prior work. Specifically with respect to entry definition, our approach focuses on the formation of employer establishments, whereas the commencement of Schedule C self-employed activity is unmeasured and not considered to be entrepreneurship in this sample.

The LEHD does not designate the founders of a new firm.[6] We use the term "entrepreneur" to describe anyone present in the data who is 1) in an entering firm per the Haltiwanger, Jarmin, and Miranda (2013) definition, 2) present in the LEHD in the first year that the firm entered, and 3) in a firm that entered after 1995. The second condition thus focuses on the initial employees of the firm, and will in some cases include employees other than true business owners. We can think of our work as describing the formation of a founding team and early hires, and we use terms like business ownership and entry in this context. The third condition is imposed by our data. Given the LEHD start dates of 1995 for some states, we are unable to identify the initial workers for older firms. Thus, in 2000, our sample of existing business owners includes young firms only. As we look towards 2004 or 2008, we continue to designate this initial cohort as an entrepreneur if the firm survives (thus, transitions out of entrepreneurship only happen if the business closes). Wage workers are defined as employees hired after the first year.

Our analyses also consider survival and growth of businesses and entrepreneurs present in 2000. There are multiple conceptual definitions feasible. Perhaps the most natural is to model the persistence of an individual in entrepreneurship. In this approach, individuals are considered to have survived in entrepreneurship if they are still entrepreneurs in 2004, even if they have changed companies. Related, individuals are said to have left entrepreneurship if they are now a wage worker, even if the company survived. We find very similar results to those reported below

[5] The data structure of the LEHD and LBD allow for establishments within each firm to have different industries and locations. Where used in this study, we define the main industry and location of a firm through the facility with the largest number of employees.

[6] Our data do not record equity ownership of individuals, but the LEHD does contain bonus pay and similar compensation.

when instead modelling the survival of a business itself. These cases allow the survival of the SEIN without the continued presence of the focal entrepreneur (e.g., the sale of the company), who may now be designated a wage worker in another firm.

3.2 Sample and Key Variables

Table 2 provides descriptive statistics on our sample. Our total sample includes 976,870 workers. (All observation counts in this paper are disguised and "rounded" to end in zero or five according to Census Bureau disclosure restrictions.) In 2000, 66.9% of individuals in this sample own homes and are wage workers, and 27.8% rent homes while also being wage workers. 5.4% of individuals in the sample are entrepreneurs, with this group comprised of 3.8% being home owners and 1.6% being renters. The home ownership rates of entrepreneurs and wage workers are quite similar at around 70%.

Rows 3 and 4 show the home price appreciation at the zip code level for our sample. Our focus is on the 2000-2004 period, where prices rose substantially, between 43%-53% for the groups. Renters tended to live in areas with greater price appreciation. This period provides a strong laboratory for exploring the connection between home prices and entry given the massive adjustments occurred. Our "event study" window is also dictated by the fact that we only observe respondent home values in 2000.

Rows 5-7 show that the rate of business ownership for this sample grows with time, from 5.4% in 2000 to 7.0% in 2008. This is in large part due to our focus on new firms founded 1995, as the size of this group slowly accumulates. It also reflects a fairly natural progression of business ownership as the cohort ages and becomes wealthier. Of the wage workers in 2000 who owned homes, 3.8% start a business by 2004 and 4.9% do so by 2008. These are net effects that do not capture very short-lived entry, and the 2008 figure allows transition back out of entrepreneurship for the 2004 entrants. These four- and eight-year entry rates seem quite reasonable given what is typically reported in the literature. Renters are modestly more likely to transition into entrepreneurship than home owners.

The statistics on business owners in Columns 4 and 5 of these rows can be interpreted as persistence in entrepreneurship of the initial entrepreneurs in 2000. A substantial fraction transition out, especially among renter entrepreneurs. Rows 8 and 9 describe survival of the SEIN. For wage workers, this means survival of the employer firm. For entrepreneurs, this means survival of the SEIN from which the entrepreneur derives the most income in 2000. Not surprisingly, the entrepreneurial firms show higher failure rates, although their businesses are more persistent than the entrepreneurial survival in the prior rows. The average longevity of these entrepreneurs and their businesses is reflective of other estimates in the literature for new firms, with perhaps a modest increase in survival rates due to the 2000 cohort having already

survived the initial "winnowing process" for entrants.

Rows 10-17 provide traits of the groups. Unconditionally, entrepreneurs in the sample are more likely to be male and immigrant than wage workers, while ages, marriage rates, and attainment of bachelors' education are mostly similar. The entrepreneurial sample is also less likely to be Hispanic or African American, compared to Asian or Caucasian. Renters tend be younger, are more likely to be minorities and immigrants, and are less likely to be married of hold a college degree.

Rows 18-23 show our home value data. The Census collects whether the respondent's home is owned, the estimated value of the home, whether there is an outstanding mortgage on the home, and the monthly mortgage payment if it exists. (The Census also collects some traits of the homes, such as the number of rooms, that we do not use here.) 70% of our sample owns a home, compared to a national average of 67% in 2000. Most of our owners have a mortgage outstanding. With the $1,000,000 cap, the average value of a home in our sample is $180,047, with entrepreneurs tending to own higher-valued properties than wage workers. The mean value for the United States as a whole in 2000 was $167,000. We describe in the next section some estimates of home equity that we construct with these variables. For renters, we only know the monthly rental payment. To assign an implied value to rental properties, we simply use 20 times the annual rent.[7] The Census collects the date when a person moved into their home, and owners not surprisingly have a significantly longer average duration in their properties.

Rows 24-27 report earnings estimates used in our study. From the Census long form, we collect total household income in 2000, which includes earned income, business income, and passive income. We will control for this in our estimations and contrast it with home values and price appreciation. From the LEHD, we first collect total earnings in 2000 for the individual (summing across all SEINs associated with the individual). We also calculate for the individual the sum of all LEHD earnings during 1990-2000. This accumulated earnings measure is used as a proxy for the wealth of the individual in 2000, while clearly recognizing the imperfect degree to which it captures all elements of wealth.

4 Empirical Results

4.1 Econometric Strategy

House price changes can link to entrepreneurship through the promotion of new entrants or changes in the behavior of existing entrepreneurs. Likewise, there are multiple strategies running through the literature to separate out local demand-side effects from collateral-based effects,

[7] In 2000, the average multiple was 21.6 using quarterly reports from Case-Shiller and FHFA data.

most notably 1) looking at variations in the degree to which industries require external finance (e.g., Rajan and Zingales, 1998), 2) considering variations in homestead exemptions by state (Berkowitz and White, 2000; Cerqueiro and Penas, 2014), 3) using renters as a control or counterfactual (e.g., Schmalz, Sraer, and Thesmar, 2014), and 4) examining price increases believed to be exogenous from demand size effects due to geography-based housing price elasticities (e.g., Adelino, Schoar, and Severino, 2013) developed from Saiz (2010).

We approach this complicated and multifaceted problem in several steps. We start with estimations that quantify the net consequences of home price changes for the size of the entrepreneurial pool in our sample, considering both entry by wage workers and exits by incumbent entrepreneurs. During these estimations, we also examine the first two strategies of looking at the external finance dependency of industries and homestead exemption levels of states. We also adopt and extend the third strategy by focusing our regressions on home owners and using the entrepreneurial transitions of renters as a comparison group and/or control variable. We jump straight to our preferred specification to quickly establish our key findings. After we have set this groundwork, we then broaden out to robustness checks and extensions, including using the fourth strategy of Saiz (2010) instruments.

Our baseline empirical specifications take the form,

$$y_i^{04} = \beta \left[\ln(VAL_i^{00}) \cdot \ln\left(\frac{HP_z^{04}}{HP_z^{00}}\right) \right] + \gamma \ln(VAL_i^{00}) + \theta \mathbf{X}_i^{00} + \phi_z + \epsilon_i, \tag{1}$$

where y_i^{04} is an indicator variable that takes a value of one if individual i is an entrepreneur in 2004. $\ln(VAL_i^{00})$ is the log value of the individual's home in 2000. The second term in the brackets is the log change in house prices from 2000 to 2004 for the zip code z in which the individual lives in 2000. Our main interest is on the interaction of these two variables and the β coefficient. Since the value of the home in 2000 interacted with the change in house prices is equal to the increased value of home equity, β captures the elasticity of entrepreneurship with respect to an increase in home equity. As already noted, this link could be due to collateral effects, as greater equity unlocks greater lending potential, or due to wealth effects, as greater home equity increases wealth. Home values and prices are demeaned before interaction to restore main effects.

We control for the main effect of home value directly, and the γ coefficient is of interest in its own right. In addition, we control for a vector $\mathbf{X}_i^{00}$ of individual level covariates in 2000. This vector includes log household income and an estimate of initial log equity in the home (for home owners). If no outstanding mortgage exists, we assume home equity is equal to the value of the home. If a mortgage exists, we estimate equity with a straight-line formula using a 20% down payment and the number of years the respondent has been in the home: Share = 20% + 80%

* (years in home / 30). We test variants on this formula later. Further, we include unreported fixed effects for the following traits of individuals, with category counts in parentheses: whether entrepreneur in 2000 (1), age (9), education (6), gender (1), race (4), immigration status (1), marital status (1), LEHD earnings in 2000 (10), accumulated LEHD earnings to 2000 (10), and date of move-in to residency (6). Accumulated earnings are measured relative to the respondent's state due to different durations of states in the LEHD sample.

Finally, but quite important, we include fixed effects ϕ_z for the 5,909 zip codes of our respondents. These fixed effects control for the main effect of house price increases, the general rate of entrepreneurial transitions in the zip code, and related local economic conditions. They require, per our example in the introduction, that the β coefficient be identified through variations in initial home values in 2000 for respondents living in the same zip code. The identifying assumption is that individuals with higher valued homes in 2000 benefit more from subsequent local house price appreciation than those with lower price homes.[8]

4.2 Baseline Estimations

Table 3 documents estimations using home price changes at the zip code level and including zip code fixed effects. Panel A reports estimates for home owners in 2000, and Panel B reports estimates for renters in 2000. We include both wage workers and entrepreneurs in 2000 in our sample, with an indicator variable for their status in 2000 introduced as a control variable. Thus, our β coefficient measures adjustments in net entry—that is, the extent to which the stock of entrepreneurship changes over the period 2000-2004 among those who were either home owners or renters in 2000. Estimates are unweighted, reflecting the random sampling of the 2000 Census from the whole US population. We cluster standard errors by zip code to reflect the price levels used.

The second row of Column 1 shows a strong positive association between home value in 2000 and net increase in entrepreneurship to 2004 for home owners. As the value of the house reflects individual wealth, even in the presence of our fixed effects for accumulated earnings, this relationship is quite intuitive: more wealthy people in 2000 are more likely to become entrepreneurs or remain as entrepreneurs by 2004. A similar relationship is also observed for household income. Both of these effects are also observed in Panel B for renters, which is important for the interpretation of our findings. This comparability remains even though the implied home value for renters is derived through their rental payments and does not reflect

[8] Our specification is considering price growth x initial value. To give a specific example, if an individual owns a $100,000 home and the local price growth is 50%, that home becomes worth $150,000 and adds $50,000 in home equity to the owner. Note that the added equity is orthogonal to the level of initial equity. The gain is $50,000 for an individual regardless of whether their initial equity in the home is $10,000 or $90,000.

ownership of the dwelling. Nevertheless, wealthier people select higher rental properties on average. Reflecting Hurst and Lusardi (2004) and related work, wealthier and higher income individuals are more likely to enter into entrepreneurship over the ensuing four years independent of house price changes in their local area. This could be due to many factors: better resources for starting a business (with or without a bank loan), higher risk tolerance, and so on.

The more interesting contrast is the interaction of initial home values and price growth in the first rows of both panels. The β coefficient for home owners is positive and statistically significant. It is modest in magnitude, as we describe in greater detail shortly, but it is nonetheless present and important. By contrast, we do not find a similar effect for renters on the interaction term. This differential is important for establishing confidence that the impact registered in Panel A is actually due to rising home equity for home owners. We show later that this null effect for renters is not found in more relaxed specifications, suggesting that they are picking up aggregate demand effects. In the approach outlined in this table, the effects in Panels A and B are not statistically different from each other due to the large standard errors for the renter. We later describe formulations under which we discern statistically different effects, in addition to the owner effect being statistically different from zero.

To demonstrate the magnitude of these coefficients, we conduct the following thought experiment that we began in the introduction. Consider two identical individuals (e.g., age, education, incomes, earnings history) living in the same zip code. The only difference between them is that one individual owns a house at the 25th percentile of value in 2000 (~\$95,000, VAL_{25}^{00}) and the other at the 75th percentile (~225,000, VAL_{75}^{00}). For simplicity, we assume here that equity levels in 2000 are the same, but given the very low elasticity in Panel A for equity this assumption is not material for what follows.

Differencing equation (1), we would estimate the following differential in 2004 for these two individuals,

$$y_{75}^{04} - y_{25}^{04} = \beta \left[\ln \left(\frac{VAL_{75}^{00}}{VAL_{25}^{00}} \right) \cdot \ln \left(\frac{HP_z^{04}}{HP_z^{00}} \right) \right] + \gamma \left(\frac{VAL_{75}^{00}}{VAL_{25}^{00}} \right), \tag{2}$$

where $y_{75}^{04} - y_{25}^{04}$ is the greater likelihood of the individual at the 75th percentile of being an entrepreneur in 2004. All other terms in equation (1) are differenced out because these hypothetical individuals only differ in their home values. Starting with the last term, the log ratio of the home values is 0.8622 and γ is equal to 0.0091. This suggest that the individual with the higher valued home is 0.0078 more likely to be entrepreneur in 2004. This compares to 0.0681 mean for the dependent variable in 2004, or a bit over 10% in relative terms. This is roughly comparable to the unconditional initial gap of 0.0075 in entrepreneurship across individuals with these home values in 2000. (For statistics like these, we use a range of +/- \$10,000 in 2000 home values to allow for sufficient observations.)

Turning to the first term, the log ratio of the home values is again 0.8622, and we multiply this by the β coefficient (0.0094) and the log value of the average price growth of 45% (0.3733). This yields the key effect of 0.0030, which retains the statistical significance of the β coefficient. Thus, we anticipate the individual with the higher-valued home, and thus the one to stand the most from house price appreciation, to be 0.3% more likely to be an entrepreneur in 2004. This effect is quite plausible in the context of our prior calculation using the levels effects estimated by the γ coefficient. The 0.3% effect is 39% of the levels difference estimated, and we are contemplating a scenario of a 45% equity gap opening up based upon their price differentials. This is roughly 40% of the initial unconditional gap of 0.0075 in entrepreneurship across individuals with these home values in 2000. As noted in the introduction, ignoring the log transformations, the two individuals would gain about $43,000 and $102,000 of home equity, respectively, for a differential of about $60,000, which is substantial in size.

Thus, while measurable, this induced entry is quite modest compared to the sample average of 6.8% in 2004 for business ownership. The relative effect of 4.4% is reported at the bottom of Panel A. Going forward, we report a similar measure for upcoming specification variants. In doing so, we adjust the interaction term's coefficient and the mean of the dependent variable, but leave the other aspects of the calculation the same. For example, the 25th and 75 percentiles of home value will differ somewhat in states with high vs. low homestead exemptions. To allow comparability, however, we continue to use the same overall home price distribution.

4.3 Collateral vs. Wealth Effects

The modest size of these effects is perhaps our most important finding, and we later test the robustness of this finding in many ways. Before doing so, we continue to highlight our core findings by considering the degree to which we can discern collateral vs. other wealth effects in this result.

Columns 2 and 3 compare the elasticity for industries that are more vs. less capital intensive. Following Hurst and Lusardi (2004), we use the Survey of Small Business Finances to segment businesses based on their starting capital requirement, and code businesses in retail and wholesale trade, as well as manufacturing as capital intensive, and businesses in services and construction as less capital intensive. Most entry occurs in sectors that are less capital intensive, and this prompts the β coefficient to be higher in Column 2 for owners. On the other hand, the relative effect is modestly larger for capital-intensive sectors at 5.3% vs. 4.1%. Thus industry differences do not suggest a substantial role for collateral effects.[9]

[9]While the literature since Rajan and Zingales (1998) typically takes stronger effects in capital-intensive sectors to be evidence for a financing effect, Adelino, Schoar, and Severino (2013) argue the opposite in their analysis of home prices and small businesses. The argument for stronger effects occurring in sectors with less

Columns 4 and 5 instead compare entrepreneurship reactions in states with high personal bankruptcy exemptions to those with low exemptions. Despite home equity loans or lines of credit being collateralized loans, Berger, Cerqueiro and Penas (2009) document that the difficulty in foreclosing such transactions in high exemption states makes banks less willing to lend against personal property. Their work would suggest that we should find a stronger impact in low exemption states, where the value of collateral is more protected and hence banks should be more willing to lend when collateral is unlocked. We split the sample by whether homestead exemptions are unlimited or not, with big states in Column 4 being Florida and Texas. Most states, including California, have low exemption levels in Column 5. These splits suggest, if anything, that the reaction is higher in states with unlimited homestead exemptions. This would not be consistent with the mechanism linking home ownership to entrepreneurship through the willingness of banks to lend more.

Thus, these two exercises do not find support for the role of a collateral channel. This does not rule out a role for collateral, but our evidence thus far leans towards more general wealth effects. Our results seem more consistent with a view that home equity increases may have led wealthier home owners who received larger windfall gains to be more willing to experiment with starting a new business (e.g., Lindh and Ohlsson, 1996; Anderson and Nielsen, 2012).

4.4 Robustness Checks and Extensions

4.4.1 Variations in Price Levels and Geographic Controls

Table 4 tests variations on specification (1) by considering different price levels and geographic controls. Columns 1-4 use home price growth at the zip code level, while Columns 5-8 consider CBSA prices. Within each group, we gradually broaden the fixed effects included from the zip code level to the regional level. Column 1 thus repeats our core specification from Table 3. In specifications where the fixed effect is at a more-aggregated level than the price variable, we include and report a main effect for prices. The second specifications with region fixed effects also introduce a control for the CBSA-level rate at which initial renters transitioned from wage work into entrepreneurship. This control seeks to model differences across cities in how attractive entrepreneurship is during this period.

The most important result from Table 4 is the generally low β coefficients in all variants of Panel A. While we find effects that can be up to 73% larger than our baseline, this still only results in a relative impact of 7.7% using the interquartile comparison.

Second, and of interest from a methodological perspective, is the emergence in Panel B of an

capital intensity would be that the marginal gain in collateral would not be sufficient for tipping the scale on lending decisions for capital-intensive sectors given the large amounts of money involved. Either way, the overall comparability of our estimates in Columns 2 and 3 suggest limited impacts in this regard.

a renter effect on the interaction of implied value in 2000 and local price growth. We selected our baseline specification due to its ability to shut down this effect, offering greater confidence that we could identify the role of home equity. A general theme that emerges from this table is that we need a combination of zip code level price indices and zip code level fixed effects to effectively reduce our "placebo test with renters" to being statistically insignificant, while still preserving an effect for home owners. The price-level element is the more important factor, with owners and renters looking similar to each other when using CBSA-level price growth. This difference, and the general growth in elasticities, is not due to the larger sample, as we obtain very similar results using CBSA price changes within the locations where we also have price changes at the zip code level. Finally, it is worth highlighting that we find in Columns 3 and 7 a direct effect of local price growth on entrepreneurship. This link, however, is even stronger for renters than for home owners. Thus, it is very difficult to interpret these parallel main effects.

More generally, we want to be careful to not push the renter comparison too far. Theoretically, the model of Bracke, Hilber, and Silva (2014) shows the ambiguity of the renter comparison when viewing home ownership and business ownership as part of a portfolio of risky assets. Empirically, even after controlling for aggregate demand, rising home prices in a local area can affect renters and their incentives towards entrepreneurship. Some renters may be discouraged from seeking to establish firms due to the fear of losing savings when the price of homes they want to buy is escalating. They may also suffer from reduced cash flow for entrepreneurship due to higher rent rates if parity to home values is maintained. On the other hand, some renters may be irrationally encouraged to entry if they believe themselves wealthier due to rising home prices around them, even if they do not directly participate. Renters may also benefit from "cheap credit" to the extent that large price appreciation fosters broader adjustments in lending standards (e.g., Glaeser and Nathanson, 2014). The renter comparison helps ground our core specification and the belief that it connects to home equity growth, but the bigger message is the general size of the coefficients present.

4.4.2 Alternatives to Value x Price Growth Interactions

We find similar results when testing variations on the value x price growth interactions. We use this specification as our baseline due to its intuitive and transparent nature, but several modifications are potentially warranted. First, and most important, the format in equation (1) does not make the value x price interaction relative to the initial wealth of individuals. Thus, an expected $50,000 growth in home equity is treated the same for an individual with a fully paid-off home worth $1,000,000 as it is for an individual with just $10,000 equity in a home worth much less. The latter individual, however, has the larger wealth shock in relative terms.

While we do not observe each individual's true wealth, we can take steps to understand

whether these issues are material. Our main approach is to normalize the price growth x value effect by the estimated initial home equity of the respondent. We create a metric that calculates the percentage change in home equity using the formula: (Home equity 2000 + House price change 2000-2004 x Home value 2000) / (Home equity 2000). Using this metric with specification (1) yields a 0.0067 (0.0015) coefficient for home owners; for renters the β coefficient is -0.0024 (0.0026). In this setting, unlike our baseline, the two effects are statistically different from each other. We likewise find similar results when constructing a metric that normalizes the price growth x value effect by household income in 2000 or the accumulated earning of the individual in the LEHD over the 1990s or both together. These results suggest that our technique is robust to making the home equity increases relative to the overall financial positions of respondents.

Additionally, we have used as a control the estimated equity that home owners hold in 2000. Our estimates rarely find this to be very important when also controlling for home value, and thus we have not emphasized its role. We constructed this home equity estimate in Section 3 using a simple straight-line rule based upon number of years in the home, among other things. This formula is easy to understand but not correct. One builds less equity in home loans during the early years of loan repayment due to the overhang of interest repayment. Thus, one worry could be that we underestimate the role of home equity due to measurement error. To provide confidence in our conclusion, we collect from Freddie Mac the average value, interest rate, and number of points on 30-year fixed rate loans for the years in which home owners in our sample moved into their homes. Using a mortgage calculator, we then quantify the expected equity levels by year of move-in for that cohort in 2000. This approach delivers very similar results to our simpler strategy. In general, given that we control directly for value and also fixed effects for move-in dates, remaining details around equity calculations have not been found to be important.

4.4.3 2008 Results and Entry Horizons

Our estimations focus on 2000-2004, but our data continue through 2008. We choose the shorter time period to establish a better event study given the extensive changes that can occur over eight years. Examining net changes in entrepreneurship from 2000-2008, we find almost identical elasticities for home owners. Looking back at Figures 1 and 2, this is not surprising as the majority of the home price adjustments occur during the initial years after 2000. When looking at the longer panel, it is no longer feasible to separate renters from owners. This too is not very surprising given that the long event window allows for confounding factors to emerge, most notably that renters might purchase a home that experiences its own price appreciation.

By looking at entry in 2004, we can also use the later period to study whether the induced entry is short-lived by examining whether the new entrant survives from 2004 until 2008. While we do not find strong differences in this regard, the evidence leans if anything towards a greater

relative growth in longer-term entrants. This suggests that the additional entry associated with home price appreciation is at least as robust as a typical cohort of startups.

4.4.4 Additional Controls

We include many controls into our baseline estimates, but more could be asked. We consider as well specifications that add additional fixed effects for the following traits of individuals in 2000, with category counts in parentheses: occupation (511), industry (211), time period of immigration (8), and number of children (4). All of our results persist with this approach, with the interaction coefficients being slightly diminished. Using specification (1) yields a 0.0074 (0.0034) coefficient for home owners with this larger battery of controls, while renters look the same at 0.0024 (0.0060) to their baseline. Note that this diminishing in coefficient size from the baseline approach would further reinforce our findings that the effects of the home price appreciation for entry are small. Additional estimations also establish that the value x price change interaction is robust to interacting price changes with other variables like household income.

4.4.5 Saiz (2010) Instrument

While we believe that our fixed effect approach does a good job of controlling for aggregate demand, we also follow Adelino, Schoar, and Severino (2013) in using the housing supply elasticities developed by Saiz (2010) to instrument for actual price changes. Saiz (2010) quantifies how cities on coast lines or encircled by nearby mountains face constraints with respect to building new homes compared to cities with level and unconstrained topologies (e.g., San Francisco vs. Houston). The former cities are more likely to observe house price growth due to supply constraints compared to the latter, and these geographic features can be a foothold for isolating price appreciation independent of aggregate demand. While these geographical features are not time varying, and thus it is not immediately clear why they would play an extra role in 2000-2004, it does seem plausible that they would influence the degree to which cities witnessed a sharp price growth.

We build our instrument for initial value x local price growth using the interaction of initial value and the local housing supply elasticity. As the instrument only varies by city, we use CBSA prices and CBSA fixed effects for this exercise. In this setting, we have an OLS elasticity of 0.0163 (0.0031) for home owners and 0.0146 (0.0060) for renters. Empirically, the first stage is very strong with F statistics consistently above 30. We find that the use of this instrument reduces our coefficients. The second stage elasticities are 0.0145 (0.0063) for home owners and 0.0062 (0.0109) for renters. Thus, this approach also confirms our baseline story.[10]

[10] While we note these results, we are also very cautious about this instrument. In more flexible formats that

4.4.6 Decomposing Net Effects

Our core analysis studies net changes in entrepreneurship and home price growth, providing an aggregate view that combines adjustments in the rate that wage workers enter into entrepreneurship with adjustments in the survival of existing entrepreneurs in 2000. When separating these two dimensions, we find most of the impact comes through greater entry rates of wage workers. The relative effect on this dimension is 5.1% compared to the baseline mean for wage transitions, and the effects are precisely estimated (β coefficient is 0.0061 (0.0030)). By contrast, the relative effect for incumbent survival is 0.7% and it is imprecisely estimated. This suggests that most of the increase in net entrepreneurship associated with home price appreciation came from higher rates of inflow into entrepreneurship from wage workers. Looking within this wage worker pool, we find that almost all of the heightened transitions into entrepreneurship by wage workers can be associated with employees of companies founded over the prior five years. Likewise, educated workers and immigrants seem particularly sensitive to opening businesses following appreciation in their home values.

5 Conclusions

The financing conditions of entrepreneurs is a topic of central importance given the link of young firms to economic growth. The massive recent swings in home prices in the United States and other countries have brought renewed interest in the role of adjustments in home equity for decisions to start new firms. Home equity has the potential to play an important role since it is amenable to pledging against bank loans and because its swings can provide substantial windfalls or losses. Yet, looking at the massive price growth during 2000-2004, we find only modest connections between home price changes and rates of entrepreneurship. Moreover, what we do observe seems more consistent with general wealth effects than with the specific channel of collateral effects.

attempt to instrument for both the main effect of housing prices and also the interaction effect, we do not find sufficiently stable results to put much faith in them.

References

[1] Adelino, M., A. Schoar and F. Severino. "House Prices, Collateral and Self-Employment." NBER Working Paper No. 18868 (2013).

[2] Anderson, S. and K. M. Nielsen. "Ability or Finances as Constraints on Entrepreneurship? Evidence from Survival Rates in a Natural Experiment." *Review of Financial Studies* 25, no. 12 (2012): 3684–710.

[3] Berger A. N., G. Cerqueiro and M. F. Penas. "Does Debtor Protection Really Protect Debtors? Evidence from the Small Business Credit Market." *Journal of Banking and Finance* 35, no. 7 (2011): 1843–57.

[4] Berger, A. N. and G. F. Udell. "The Economics of Small Business Finance: The Roles of Private Equity and Debt Markets in the Financial Growth Cycle." *Journal of Banking & Finance* 22, no. 6-8 (1998): 613–73.

[5] Berkowitz, J. and M. J. White. "Bankruptcy and Small Firms' Access to Credit." *RAND Journal of Economics* 35, no. 1 (2004): 69–84.

[6] Black, J., D. de Meza and D. Jeffreys. "House Price, the Supply of Collateral and the Enterprise Economy." *Economic Journal* 106, no. 434 (1996): 60–75.

[7] Black, S. E. and P. E. Strahan. "Entrepreneurship and Bank Credit Availability." *Journal of Finance* 57, no. 6 (2002): 2807–33.

[8] Blanchflower, D. G. and A. Oswald. "What Makes an Entrepreneur?" *Journal of Labor Economics* 16, no. 1 (1998): 26–60.

[9] Bracke, P., C. Hilber and O. Silva. "Homeownership and Entrepreneurship: The Role of Mortgage Debt and Commitment." Working paper (2014).

[10] Buera, F., J. Kaboski and Y. Shin. "Finance and Development: A Tale of Two Sectors." *American Economic Review* 101, no. 5 (2011): 1964-2002.

[11] Cabral, L. M. B. and J. Mata. "On the Evolution of the Firm Size Distribution: Facts and Theory." *American Economic Review* 93, no. 4 (2003): 1075–90.

[12] Cagetti, M. and M. De Nardi. "Entrepreneurship, Frictions, and Wealth." *Journal of Political Economy* 114, no. 5 (2006): 835–70.

[13] Campbell, B., M. Ganco, A. Franco and R. Agarwal. "Who Goes, Where to, and Does It Matter?: Employee Mobility, Entrepreneurship, and the Effects on Source Firm Performance" *Strategic Management Journal* (2014), forthcoming.

[14] Cerqueiro, G., D. Hegde, M. F. Penas and R. Seamans. "Personal Bankruptcy and Innovation." Working paper (2014).

[15] Cerqueiro, G. and M. F. Penas. "How Does Personal Bankruptcy Law Affect Start-Ups?" Working paper (2014).

[16] Cetorelli, N. and P. E. Strahan. "Finance as a Barrier to Entry: Bank Competition and Industry Structure in Local U.S. Markets." *Journal of Finance* 61, no. 1 (2006): 437–61.

[17] Cooley, T. F. and V. Quadrini. "Financial Markets and Firm Dynamics." *American Economic Review* 91, no. 5 (2001): 1286–310.

[18] Decker, R., J. Haltiwanger, R. Jarmin and J. Miranda. "The Role of Entrepreneurship in US Job Creation and Economic Dynamism." *Journal of Economic Perspectives* 28, no. 3 (2014): 3-24.

[19] Evans, D. S. and B. Jovanovic. "An Estimated Model of Entrepreneurial Choice under Liquidity Constraints." *Journal of Political Economy* 97, no. 4 (1989): 808–27.

[20] Fairlie, R. and H. Krashinksky. "Liquidity Constraints, Household Wealth, and Entrepreneurship Revisited." CESIFO Working Paper No. 3784 (2012).

[21] Gentry, W. M. and R. G. Hubbard. "Entrepreneurship and Household Saving." *Advances in Economic Policy and Analysis* 4, no. 1 (2004).

[22] Glaeser, E. L. and C. G. Nathanson. "Housing Bubbles." NBER Working Paper No. 20426 (2014).

[23] Haltiwanger J., R. Jarmin and J. Miranda. "Who Creates Jobs? Small vs. Large vs. Young." *Review of Economics and Statistics* 95, no. 2 (2013): 347–61.

[24] Harding, J. and S. S. Rosenthal. "Homeowner-Entrepreneurs, Housing Capital Gains and Self-Employment." Working paper (2013).

[25] Hamilton, B. H. "Does Entrepreneurship Pay? An Empirical Analysis of the Returns to Self-Employment." *Journal of Political Economy* 108, no. 3 (2000): 604–31.

[26] Holtz-Eakin, D., D. Joulfaian and H. S. Rosen. "Sticking It Out: Entrepreneurial Survival and Liquidity Constraints." *Journal of Political Economy* 102, no. 1 (1994): 53–75.

[27] Hurst, E. and A. Lusardi. "Liquidity Constraints, Household Wealth, and Entrepreneurship." *Journal of Political Economy* 112, no. 2 (2004): 319–47.

[28] Hurst, E. and B. Pugsley. "What Do Small Businesses Do?" *Brookings Papers on Economic Activity* (Fall 2011).

[29] Hurst, E. and F. Stafford. "Home is Where the Equity Is: Liquidity Constraints, Refinancing and Consumption." *Journal of Money, Credit and Banking* 36, no. 6 (2004): 985–1014.

[30] Jensen, T., S. Leth-Petersen and R. Nanda. "Housing Collateral, Credit Constraints and Entrepreneurship: Evidence from a Mortgage Reform." Working paper.

[31] Kerr, W. and R. Nanda. "Democratizing Entry: Banking Deregulation, Financing Constraints and Entrepreneurship." *Journal of Financial Economics* 94, no. 1 (2009): 124–49.

[32] Khilstrom, R. E. and J. J. Laffont. "General Equilibrium Entrepreneurial Theory of Firm Formation Based on Risk Aversion." *Journal of Political Economy* 87, no. 4 (1979): 719–48.

[33] Lindh, T. and H. Ohlsson. "Self-Employment and Windfall Gains: Evidence from the Swedish Lottery." *Economic Journal* 106, no. 439 (1996): 1515–26.

[34] Mian, A. and A. Sufi. "House Prices, Home Equity Based Borrowing and the U.S. Household Leverage Crisis." *American Economic Review* 101, no. 5 (2011): 2132–56.

[35] Moskowitz, T. J. and A. Vissing-Jorgensen. "The Returns to Entrepreneurial Investment: A Private Equity Premium Puzzle?" *American Economic Review* 92, no. 4 (2002): 745–78.

[36] Rajan, R. G. and L. Zingales. "Financial Dependence and Growth." *American Economic Review* 88, no. 3 (1998): 559–86.

[37] Robb, A. M. and D. T. Robinson. "The Capital Structure Decisions of New Firms." *Review of Financial Studies* 27, no. 1 (2014): 695–722.

[38] Saiz, A. "The Geographic Determinants of Housing Supply." *Quarterly Journal of Economics* 125, no. 3 (2010): 1253–96.

[39] Schmalz, M. C., D. A. Sraer and D. Thesmar. "Housing Collateral and Entrepreneurship." Working paper (2014).

[40] Stiglitz, J. E. and A. Weiss. "Credit Rationing in Markets with Imperfect Information." *American Economic Review* 71, no. 3 (1981): 393–410.

Fig. 1: Average US House Prices

Data Source: Federal Housing Finance Agency

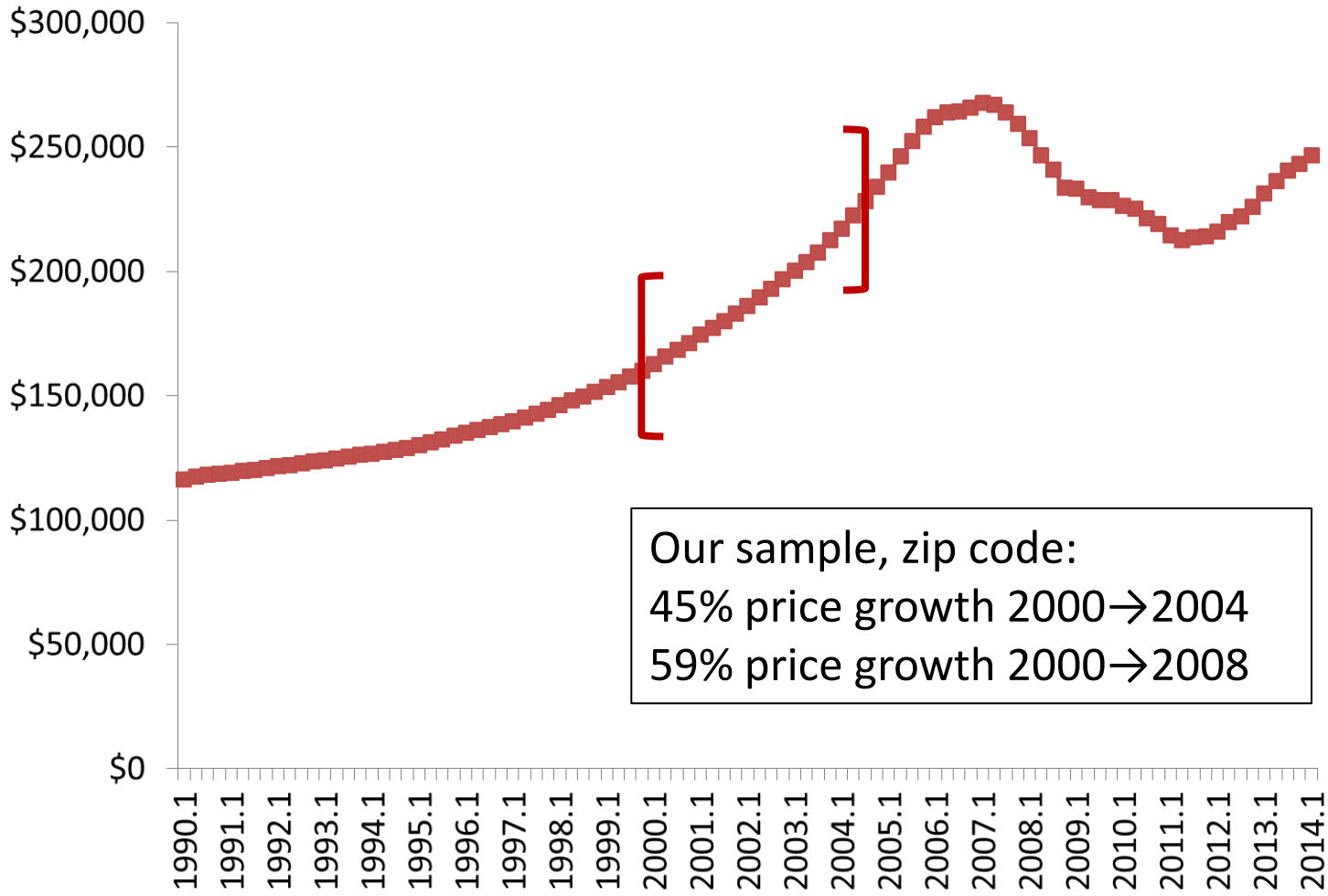
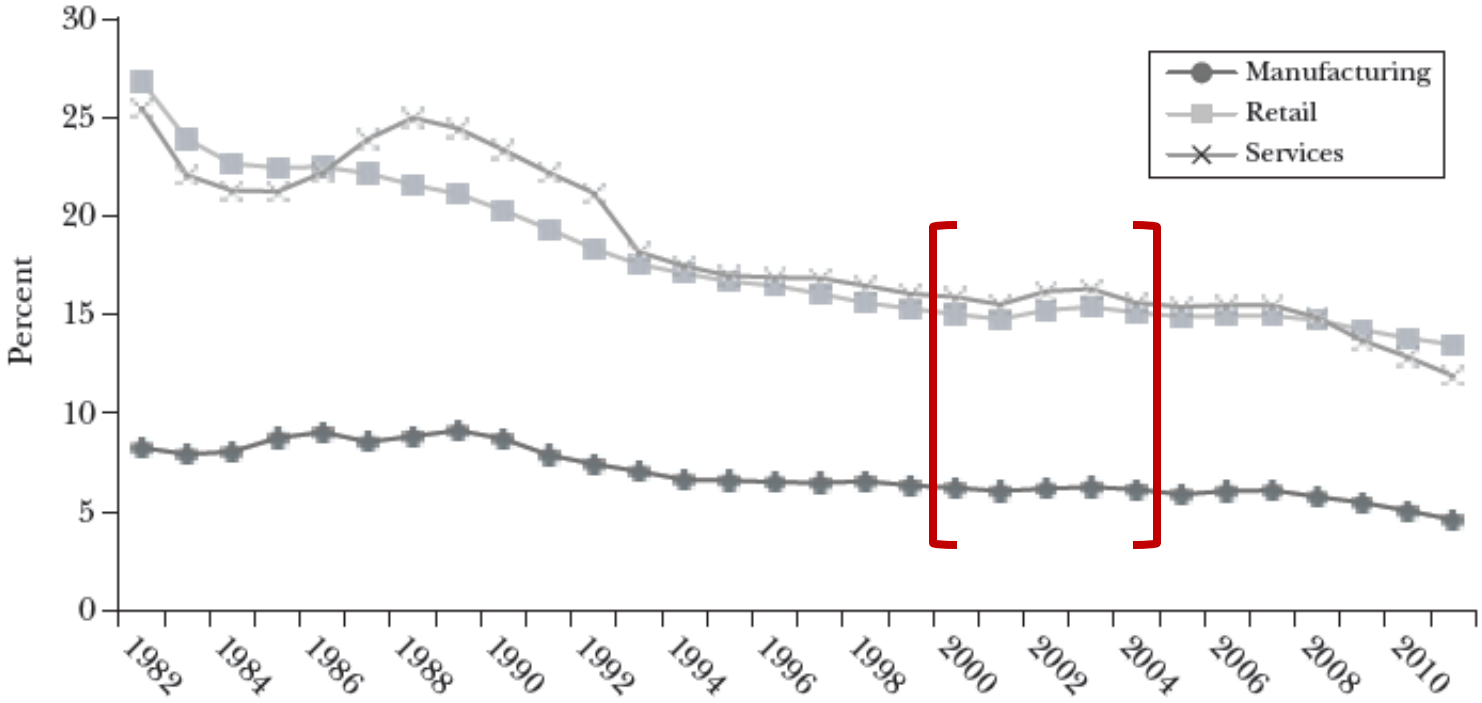


Fig. 2: Share of Young Firm Activity

Data Source: Figure 5 of Decker et al. (2014)

Figure 5

Share of Employment from Young Firms (Firms Age 5 or Less), Selected Sectors



Source: Author calculations from the US Census Bureau's Business Dynamics Statistics.
Notes: Sector definitions are on an Standard Industrial Classification basis. Employment shares in each period are based on the average of employment in period $t-1$ and t (the denominator of the Davis, Haltiwanger, and Schuh (DHS) growth rate).

Figure 3: LEHD State Coverage

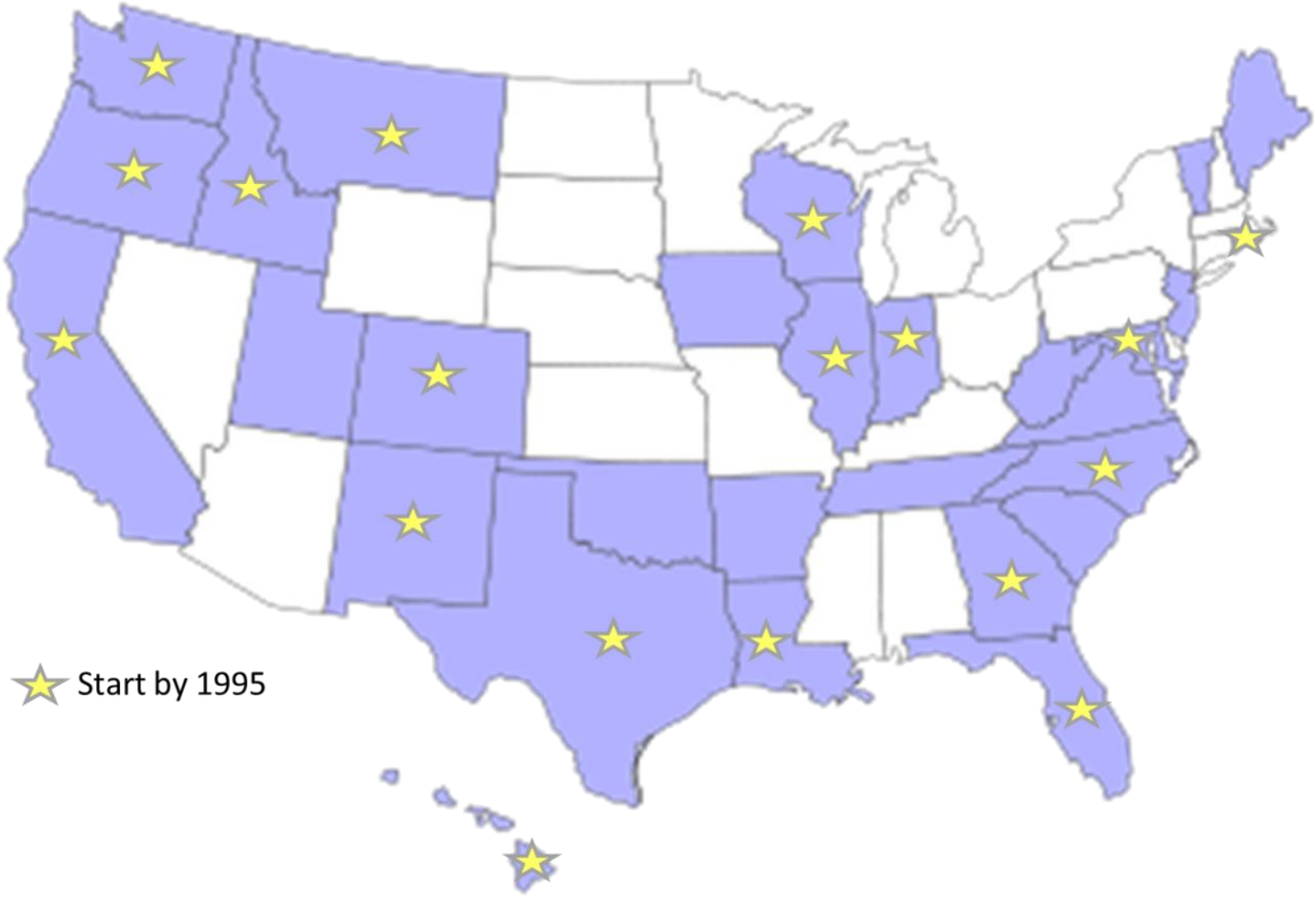


Table 1A

Home equity collateralization of loans

Notes: Based on data from the 1998 Survey of Small Business Finances. The specific results are based on a sub-sample of firms that were less than 10 years old and had under 50 employees at the time of the survey. Percentages are not corrected for sample weights.

	Share secured by collateral	Share secured by personal real estate	Fraction of collateralized loans secured by personal real estate
	(1)	(2)	(3)
Mortgage	88%	55%	63%
Line of credit	44%	12%	28%
Equipment loan	67%	2%	3%
Other loans	37%	9%	25%
Weighted average	56%	19%	34%

Table 1B

Home equity collateralization of loans

Notes: See Table 1A.

	Share of eligible respondents who answered "Yes"
Applied for a loan to the specific institution they did because it required no or less collateral	0%
Personal real estate was used to secure the most recent loan	19%
Lack of collateral was the official reason why the loan was turned down	9%
Did not apply for a loan because feared rejection due to lack of collateral	6%

Table 2

Descriptive statistics on LEHD sample

Notes: This table provides descriptive statistics on our sample. Our sample includes working individuals present in 2000, 2004, and 2008 in a CBSA in one of 16 states included in the LEHD since 1995. Demographic traits are measured in 2000. Per Census Bureau disclosure requirements, listed observation counts are rounded. Zip code price data are available for 85% of the sample. Other variables are available for almost every individual.

	Respondent occupation in 2000	All	Wage	Wage	Entrepreneur	Entrepreneur
	Respondent home ownership in 2000	All	Owner	Renter	Owner	Renter
		(1)	(2)	(3)	(4)	(5)
(1)	N	976,870	653,570	271,070	36,800	15,430
(2)	Share	1.0000	0.6690	0.2775	0.0377	0.0158
(3)	Zip code house price 2004 / price 2000	1.4525	1.4269	1.5104	1.4466	1.5282
(4)	Zip code house price 2008 / price 2000	1.5927	1.5622	1.6626	1.5805	1.6849
(5)	Business owner 2000	0.0535	n.a.	n.a.	1.0000	1.0000
(6)	Business owner 2004	0.0667	0.0383	0.0428	0.5911	0.4381
(7)	Business owner 2008	0.0702	0.0488	0.0520	0.4779	0.3271
(8)	Survival of SEIN in 2000 to 2004	0.8529	0.8679	0.8403	0.7503	0.6846
(9)	Survival of SEIN in 2000 to 2008	0.7534	0.7772	0.7300	0.6015	0.5184
(10)	Age	38.07	39.30	35.17	38.82	35.17
(11)	Male	0.5281	0.5264	0.5215	0.5846	0.5780
(12)	Hispanic	0.1387	0.1167	0.1907	0.1183	0.2043
(13)	African American	0.0771	0.0562	0.1329	0.0340	0.0865
(14)	Asian	0.0538	0.0485	0.0637	0.0627	0.0842
(15)	Immigrant	0.1600	0.1378	0.2068	0.1712	0.2526
(16)	Married	0.7280	0.8254	0.4911	0.8349	0.5099
(17)	Bachelor's education and higher	0.3774	0.4064	0.3115	0.3838	0.2893
(18)	Renter	0.2933	n.a.	1.0000	n.a.	1.0000
(19)	Own with mortgage	0.6597	0.9335	n.a.	0.9332	n.a.
(20)	Own without mortgage	0.0384	0.0665	n.a.	0.0668	n.a.
(21)	Home value (max=$1 million)	180,047	178,750	n.a.	203,117	n.a.
(22)	Implied rental value	163,731	n.a.	163,242	n.a.	172,317
(23)	Move-in date	1993.5	1992.4	1996.1	1992.9	1996.1
(24)	Household income (max=$2.5 million)	76,232	86,479	49,965	97,360	53,252
(25)	LEHD earnings 2000	44,286	49,624	31,235	51,174	30,998
(26)	LEHD earnings 2004	53,901	59,508	40,000	62,357	40,502
(27)	LEHD earnings 2008	64,000	69,745	49,542	74,071	50,659

Table 3

Estimations of net entrepreneurship using home values and prices for respondents in 2000

Notes: This table reports regressions quantifying net entry effects that combine entrepreneurs and wage workers in 2000. Panel A considers home owners, with a specific focus on their home values and local price changes. Panel B considers renters with the implied value of the dwelling. Price changes are specific to a respondent's zip code. Regressions include unreported fixed effects for the following traits of individuals, with category counts in parentheses: whether entrepreneur in 2000 (1), zip code of residence (5909), age (9), education (6), gender (1), race (4), immigration status (1), marital status (1), LEHD earnings in 2000 (10), accumulated LEHD earnings to 2000 (10), and date of move-in to residency (6). Accumulated earnings are measured relative to the respondent's state due to different durations of states in the LEHD sample. Reported regressors are in log values. Regressors are demeaned prior to interactions to restore main effects. Standard errors are clustered at the zip code level.

PANEL A - Home Owners					
Dependent variable	Entrepreneur	Capital intensity 2004		State homestead exemption	
	in 2004	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)
Home value 2000 * Price change 2000-2004	0.0094	0.0031	0.0063	0.0159	0.0071
	(0.0034)	(0.0021)	(0.0031)	(0.0073)	(0.0039)
Home value 2000	0.0091	0.0000	0.0091	0.0114	0.0083
	(0.0019)	(0.0012)	(0.0017)	(0.0034)	(0.0023)
Estimated home equity 2000	0.0021	0.0024	-0.0004	0.0026	0.0019
	(0.0018)	(0.0012)	(0.0016)	(0.0031)	(0.0022)
Household income 2000	0.0060	-0.0021	0.0081	0.0065	0.0058
	(0.0008)	(0.0005)	(0.0007)	(0.0015)	(0.0009)
Number of observations	581,880	581,880	581,880	152,725	429,155
Mean of dependent variable	0.0681	0.0188	0.0492	0.0773	0.0648
75th-25th interaction differential / mean of DV	0.0444	0.0531	0.0412	0.0662	0.0353
PANEL B - Renters					
Home value 2000 * Price change 2000-2004	0.0025	-0.0018	0.0043	0.0270	0.0014
	(0.0060)	(0.0033)	(0.0053)	(0.0166)	(0.0065)
Home value 2000	0.0138	-0.0017	0.0155	0.0205	0.0122
	(0.0060)	(0.0042)	(0.0050)	(0.0179)	(0.0064)
Household income 2000	0.0025	0.0004	0.0022	0.0011	0.0029
	(0.0010)	(0.0005)	(0.0010)	(0.0022)	(0.0016)
Number of observations	244,650	244,650	244,650	61,625	183,025
Mean of dependent variable	0.0641	0.0160	0.0481	0.0689	0.0625
75th-25th interaction differential / mean of DV	0.0390	-0.1125	0.0894	0.3919	0.0224

Table 4

Variations on house price levels and geographic fixed effects

Notes: See Table 3. Estimations repeat Column 1 of Table 3 showing variations on house price levels (zip code versus CBSA) and geographic fixed effects (zip code versus CBSA versus region). Where appropriate, the main effect for house price is introduced and reported. Extensions with region fixed effects test the inclusion of the entry rate for renters between 2000 and 2004 as an additional control variable.

PANEL A - Home Owners								
Price level	Zip				CBSA			
Geographic fixed effect level	Zip	CBSA	Region	Region	Zip	CBSA	Region	Region
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Home value 2000 * Price change 2000-2004	0.0094	0.0127	0.0097	0.0115	0.0119	0.0163	0.0095	0.0119
	(0.0034)	(0.0030)	(0.0029)	(0.0029)	(0.0039)	(0.0035)	(0.0033)	(0.0032)
Price change 2000-2004		0.0080	0.0065	0.0005			0.0092	0.0013
		(0.0057)	(0.0023)	(0.0022)			(0.0029)	(0.0029)
CBSA Rental Entry Rate				0.6541				0.5821
				(0.0480)				(0.0431)
Number of observations	581,880	581,880	581,880	581,880	681,410	681,410	681,410	681,410
Mean of dependent variable	0.0681	0.0681	0.0681	0.0681	0.0678	0.0678	0.0678	0.0678
75th-25th interaction differential / mean of DV	0.0444	0.0600	0.0458	0.0544	0.0565	0.0774	0.0451	0.0565
PANEL B - Renters								
Home value 2000 * Price change 2000-2004	0.0025	0.0049	0.0066	0.0067	0.0109	0.0146	0.0119	0.0127
	(0.0060)	(0.0054)	(0.0050)	(0.0049)	(0.0070)	(0.0063)	(0.0058)	(0.0058)
Price change 2000-2004		-0.0019	0.0102	0.0021			0.0167	0.0048
		(0.0074)	(0.0029)	(0.0028)			(0.0038)	(0.0038)
CBSA Rental Entry Rate				0.9234				0.9056
				(0.0681)				(0.0605)
Number of observations	244,650	244,650	244,650	244,650	286,170	286,170	286,170	286,170
Mean of dependent variable	0.0641	0.0641	0.0641	0.0641	0.0641	0.0641	0.0641	0.0641
75th-25th interaction differential / mean of DV	0.0126	0.0246	0.0331	0.0336	0.0547	0.0733	0.0598	0.0638

Appendix Table 1

Comparison of mobility rates in LEHD sample to full 2000 Census

Notes: This table compares the migration behavior of the respondents included in our sample with everyone present in the 2000 Census. Our sample is restricted to individuals present in 2000, 2004, and 2008 in one of the 18 states included in the LEHD since 1995.

	Move from within the US			Move from		
	Total	Within State	Out-of State	outside US	N	%
	(1)	(2)	(3)	(4)	(5)	(6)
A. Our sample						
Own with mortgage in 2000	47.8	40.8	7.1	0.9	644,732	0.66
Own without mortgage in 2000	21.1	18.3	2.8	0.6	37,708	0.04
Rent in 2000	73.6	58.2	15.4	4.1	296,523	0.30
					978,963	
B. Full sample for the 18 LEHD states						
Own with mortgage in 2000	47.6	39.7	7.9	1.0	1,868,113	0.64
Own without mortgage in 2000	22.1	18.6	3.5	0.5	172,928	0.06
Rent in 2000	75.1	57.4	17.7	4.4	861,486	0.30
					2,902,527	
C. Full sample for all states						
Own with mortgage in 2000	46.0	38.2	7.9	0.8	4,273,307	0.66
Own without mortgage in 2000	20.7	17.3	3.5	0.4	429,096	0.07
Rent in 2000	74.1	55.7	17.7	4.2	1,752,782	0.27
					6,455,185	