Does Homeownership Prolong the Duration of Unemployment?

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January 21, 2016

Abstract

We examine the effects of homeownership on individuals' unemployment durations in the USA, with a particular focus on comparing the pre-recession period (1996 to 2008) to the recession period (2008 to 2013). We take into account that an unemployment spell can terminate with a job or with a non-participation transition. The latter is included to analyze whether the incidence of exiting the labor force (as an alternative to keep searching for a job) has increased for homeowners in the recession. The endogeneity of homeownership is adressed through the estimation of a full maximum likelihood function which jointly models the competing hazards and the probability of being a homeowner. Unobserved factors contributing to the probability of being a homeowner are allowed to be correlated with unobservable heterogeneity in the hazard rates. If we do not control for ownership selection, there is neither a significant difference in the job-finding hazard nor in the non-participation hazard of unemployed owners and renters. If we jointly model the ownership selection, we find that unemployed homeowners are more likely to find a job than renters. The effect is small but statistically significant for some specifications and some subpopulations.

Keywords: Homeownership; Labor mobility; Unemployment duration;

JEL codes: C41, J61, J64, R23.

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1 Introduction

Homeownership and a large proportion of homeowners in the population is in general regarded to be desirable. Subsidies to buying or building homes in many countries testify to the importance that policy-makers usually attach to the ownership society. The United States spends more than $100 billion annually to subsidize homeowners (see the projected tax expenditures by the Congressional Joint Committee on Taxation, 2012).[1] People who own their dwellings, it has been argued, are less mobile, more likely to vote, healthier, and less likely to commit crimes. Dietz and Haurin (2003) give an overview of this literature.

A possibly detrimental effect of homeownership has been brought to attention by Oswald (1996) and again more recently in Blanchflower and Oswald (2013). Since homeowners are less mobile, they argue, they will not be capable or willing to move for a job outside of their current locality. Consequently their job-finding rates will be lower, and their unemployment durations longer. The argument is supported by cross-regional comparisons which indicate that European regions and US states with high homeownership rates exhibit higher unemployment rates. However at this level of aggregation, the effect could be spurious, and it need not be the case that owners and renters have different unemployment risks or durations within a region. Nor can we rule out that residents in regions with high ownership rates are different from residents in regions with low ownership rates. Indeed, mobile individuals with better job-finding prospects might have left distressed regions, leaving behind homeowners and individuals who had low job-finding probabilities in the first place. Finally, one needs to be careful about when exactly being a homeowner becomes a liability. While the probability of becoming unemployed might or might not be influenced by home tenure, we would expect the restricted mobility that comes with owning a home to decrease the probability of finding a new job – once one is unemployed – and hence unemployment durations to increase. If the presence and importance of this effect could be demonstrated, the desirability of the "ownership society" would have to be reconsidered.

[1] Sinai and Gyourko (2004) - defining a homeowner subsidy as the difference in taxes paid by a homeowner and taxes the same person would have paid if he had rented out the property - report a much higher homeowner subsidy cost of $420 billion in 1999. We would also like to highlight that homeownership has been supported or been cited as desirable by the last three American presidents: http://www.usnews.com/news/articles/2015/01/08/president-barack-obama-announces-federal-housing-administration-mortgage-insurance-premium-cut http://www.nytimes.com/2008/12/21/business/worldbusiness/21iht-admin.4.18853088.html?_r=0 http://www.businessweek.com/the_thread/hotproperty/archives/2008/02/clintons_drive.html

In this paper we use individual unemployment spells to address the question whether unemployed owners remain in unemployment longer than unemployed renters. The application is for the United States. We add to the literature in three important ways: First, we revisit the question of homeownership and unemployment duration using the Survey of Income and Program Participation (henceforth SIPP), which offers several advantages to data used in the literature so far and is particularly suited for duration analysis due to a higher number of spells, higher frequency of interviews of sample units, and the availability of a rich set of pre-spell characteristics (see Data section). Second, we analyze whether homeownership has been more of a burden and whether unemployed homeowners were more likely to exit the labor force during the Great Recession. We think this is an important channel to look at as the male labor force participation dropped by 2.8 percentage points between March 2008 and March 2012 (see Blanchflower and Posen (2014)). If housing tenure plays a role in the decision to exit the labor force, omitting the exit to non-participation might lead to biased or misinterpreted results of the effect of homeownership on unemployment duration. We also allow for arbitrary correlations between unobserved heterogeneity in the hazard to employment, the hazard to non-participation, and the probability of being a homeowner. Third, we control for the endogeneity of being a homeowner using a novel instrument: a recently developed panel measure of housing supply regulations. We argue that it is considerably better than the instruments proposed so far.

The literature has addressed the problems associated with making microeconomic inferences from macroeconomic correlations by using household or individual-level data, with few papers attempting to control for the endogeneity of homeownership. By and large the literature does not support Oswald's hypothesis. Goss and Phillips (1997) find a negative effect of ownership on unemployment duration in the PSID which they attribute to higher search intensity to honor mortgage obligations. Also using the PSID, Coulson and Fisher (2002) find a significant negative effect of ownership on unemployment duration, while Green and Hendershott (2001) – using a two-stage estimation procedure to correct for selection bias into ownership – find a positive but negligible effect of ownership on unemployment duration. Valletta (2013) finds no effect of homeownership on unemployment duration in the CPS. Flatau, Forbes, Wood and Hendershott (2003) find with Australian data that mortgage holders exit unemployment quickest, while outright owners are not significantly different from private renters. Munch, Svarer and Rosholm (2006) use non-parametric full information maximum likelihood (FIML) methods (the approach we follow in this paper) to jointly estimate the probability of homeownership and unemployment duration, allowing for unobserved heterogeneity. They use Danish register data to find that homeowners leave unemployment faster than renters.

Munch et al. (2006) decompose the job hazard into transitions to local and to non-local jobs and conclude that owners are more likely to find a local, and less likely to find a non-local job resulting in an overall positive effect of ownership on the job-finding hazard. Several other studies using mostly European data find, even after controlling for endogeneity of being a homeowner, that owners exit unemployment at higher rates than renters.[2] However the fact that labor mobility in Europe is remarkably lower than in the US[3] suggests that housing might not be an important factor for European labor markets. As Munch et al. conclude: *"It is possible that in countries where geographical mobility is a more important element of the functioning of the labor market (such as in the US), homeownership might have an overall detrimental effect on unemployment."* The objective of the present paper is to test this proposition.

The present paper follows the methodology of Munch et al. (2006) – in our opinion the most credible attempt to control for ownership endogeneity. When we do not control for ownership selection the effect of being a homeowner on the job finding probability is positive, but virtually zero. In contrast, the model including ownership selection (FIML) yields a mildly positive relationship. Similarly, exit to non-participation produces insignificant results for most of the specifications. We do not observe any notable differences in the owner-duration relation between the pre-recession and the recession periods.

2 Theoretical Motivation

The main mechanism behind the potential positive relationship between homeownership and unemployment duration is the impact of housing tenure on geographical mobility. Moving is more costly for homeowners. This reduces the residential mobility which in turn translates into lower job mobility for homeowners. This line of thought is supported both theoretically and empirically.[4]

[2] van Vuuren and van Leuvensteijn (2007), van Leuvensteijn and Koning (2004), Battu, Ma and Phimister (2008) reach this conclusion. Brunet and Lesueur (2003) – using French data – is the only study in Europe that concludes the opposite.

[3] Rupert and Wasmer (2009) document average cross-regional mobility rates in Europe of approximately 2 percent. This figure is around 5-6 percent for the US although with a declining trend.

[4] Rabe and Taylor (2010) and Winkler (2011) are the most recent studies that point out the effect of housing on residential and job mobility in the UK and USA.

One implication of this line of argument is that owners are likely to experience longer durations of unemployment than otherwise comparable renters. The existing literature typically distinguishes between the hazard rate into local vs. non-local jobs. A simple model of job search unambiguously suggests that unemployed owners have higher reservation wages for non-local jobs resulting from higher moving costs and therefore they have lower non-local job hazard rates. To offset this, owners increase their effort in the local labor market which usually implies higher hazard rates for local jobs. Although there are two opposing effects (i.e. higher local job hazards and lower nonlocal job hazards) under fairly general assumptions owners are less likely to find a job overall.[5] Since owners are less flexible in some part of the labor market (outside offers), they cannot fully offset this inflexibility by focusing on the rest of the labor market (local offers). Overall, the magnitude of the employment hazard difference between owners and renters depends on the difference in characteristics of local vs. non-local labor markets and the moving cost. In principle as the share of non-local offers increases, the inflexibility of homeowners becomes bigger which implies longer unemployment durations.

The relationship between homeownership and unemployment during the Great Recession deserves a separate analysis. The literature has suggested two additional mechanisms that could aggravate the positive homeownership effect on unemployment duration during a recession: First, Karahan and Rhee (2013) argue that during the Great Recession unemployment dispersion between locations became higher. As argued by Guler and Taskin (2013) for homeowners who live in regions with high unemployment rates, it takes longer to find jobs since the majority of offers are not coming from the local job market. Under such conditions, Head and Lloyd-Ellis (2012) find that the detrimental quantitative impact of homeownership on unemployment is sizable. One should note that this effect is not specific to the 2008 recession. Second, since the onset of the Great Recession a house-lock hypothesis suggested by Ferreira, Gyourko, and Tracy (2010) received notable attention. This hypothesis claims that homeowners suffering from negative equity are less likely to move simply because it is unaffordable to do so. From the perspective of an unemployed person this supposed decline in mobility in turn implies longer unemployment duration. This second effect might be mitigated by the possibility that after certain levels of negative equity it is optimal to default on

[5] Dohmen (2005) and Munch, Rosholm and Svarer (2006), Coulson and Fisher (2008), van Vuuren and van Leuvensteijn (2007), Guler and Taskin (2013).

the loan and move somewhere else (Schulhofer-Wohl (2010)). Overall the effect is ambiguous and evidence coming from quantitative exercises does not suggest a sizable contribution of the housing market on the unemployment spike during the Great Recession (Karahan and Rhee (2013), Nenov (2015)). Having a large number of complete unemployment spells for both the pre-recession and recession periods allows us to analyze the role of the recession on the ownership-unemployment link with individual data.

3 Data

We use the Survey of Income and Program Participation (SIPP). We report results from three different samples. The first consists of the pooled 1996, 2001 and 2004 panels (pre-recession). The second sample is the 2008 panel (recession), and the third is the pooled sample of all panels (pooled). The SIPP follows around 40,000 households and close to 100,000 individuals over three to four years in the pre-recession sample (depending on the panel) and over five years in the recession panel. The SIPP surveys households every four months, creating 9 to 15 *waves*. It thus offers major advantages compared to the annually collected PSID in terms of number of observations and possible recall errors on unemployment durations. On the other hand the SIPP's longitudinal design allows the tracking of individuals and thus – unlike the CPS – provides complete unemployment durations for a majority of unemployment spells and a rich set of pre-spell characteristics. The SIPP provides information on employment status on a weekly basis. We restrict our analysis to males aged between 18 and 65. We do not consider women due to probable complexities in their past and present labor supply and job search decisions, such as fertility and intra-household labor supply decisions. We also apply several restrictions to reduce measurement errors and inconsistencies in our final sample of unemployment spells. The construction of the final data including how we define unemployment spells and the possible exits to employment or non-participation is described in the data description appendix.

The pooled sample consists of 16,527 individuals and 28,124 unemployment spells of which 18,980 end in transitions to work, 5,902 in transitions to non-participation, and 3,242 are right-censored (the panel ends before we can observe the transition out of unemployment). On average we observe an unemployment duration of 12.5 weeks. Table 1 describes the corresponding spell statistics separately for those who are owners and who are renters at the beginning of the spell, and separately for the pre-recession and the recession sample. The effect of the recession is clearly seen in the longer duration of unemployment spells for both owners and renters in the recession sample. Note that this is partly due to a smaller incidence of censored spells for the recession panel (which has the deeper longitudinal dimension). The share of censored spells is significantly different between the two samples. The longer coverage of the recession spell results in fewer censored spells (and longer durations). At the same time, unemployment does indeed last longer in the recession. A regression of the unemployment durations on a recession sample dummy, a dummy for a censored spell, and an interaction term (results not reported) demonstrates that both mechanisms add significantly to the increase in unemployment duration.

In the pre-recession owners remained unemployed 0.4 weeks longer than renters. This difference increased to 0.8 weeks in the recession, resulting in a "difference-in-difference" detrimental effect of the recession on homeowners of an additional 0.4 weeks. This difference is not significant, but is suggestive of a possible negative effect of being a homeowner when the economy is in a recession. Owners are significantly more likely than renters to end their unemployment spell with a job, and this difference seems to have increased in the recession by 0.4 percentage points (insignificant). The probability to exit into non-participation for owners increased relative to renters (p-value 0.11), while the probability to have a censored spell is significantly decreased (p-value 0.011). Given the importance of censoring for the changes in the observed spell durations, it is important to account for right-censoring in the estimation.

Figure 1 and Figure 2 plot the Kaplan-Meier survival functions for owners and renters with exits to employment and non-participation. The hazard rates to employment over the course of the unemployment spell are remarkably similar between owners and renters. Only for unemployment durations for more than a year, which is less than 5% of all spells, do we observe a gap between owners and renters. Moreover, to the naked eye no discernable difference seems to exist in the owner-renter difference between the pre-recession and the recession panel. Equally, no important difference between owners and renters is visible when considering non-participation as an exit (figure 2). The seam-bias – the tendency for individuals to report the same labor force and employment status for the entire reference period of a wave – is clearly more effective for non-participation, but else owners and renters have very similar survival rates both before and during/after the recession. Since exits to job and to non-participation are exhaustive, we deem it important to account for the competing risk of non-participation when estimating the effect of homeownership on the job-finding

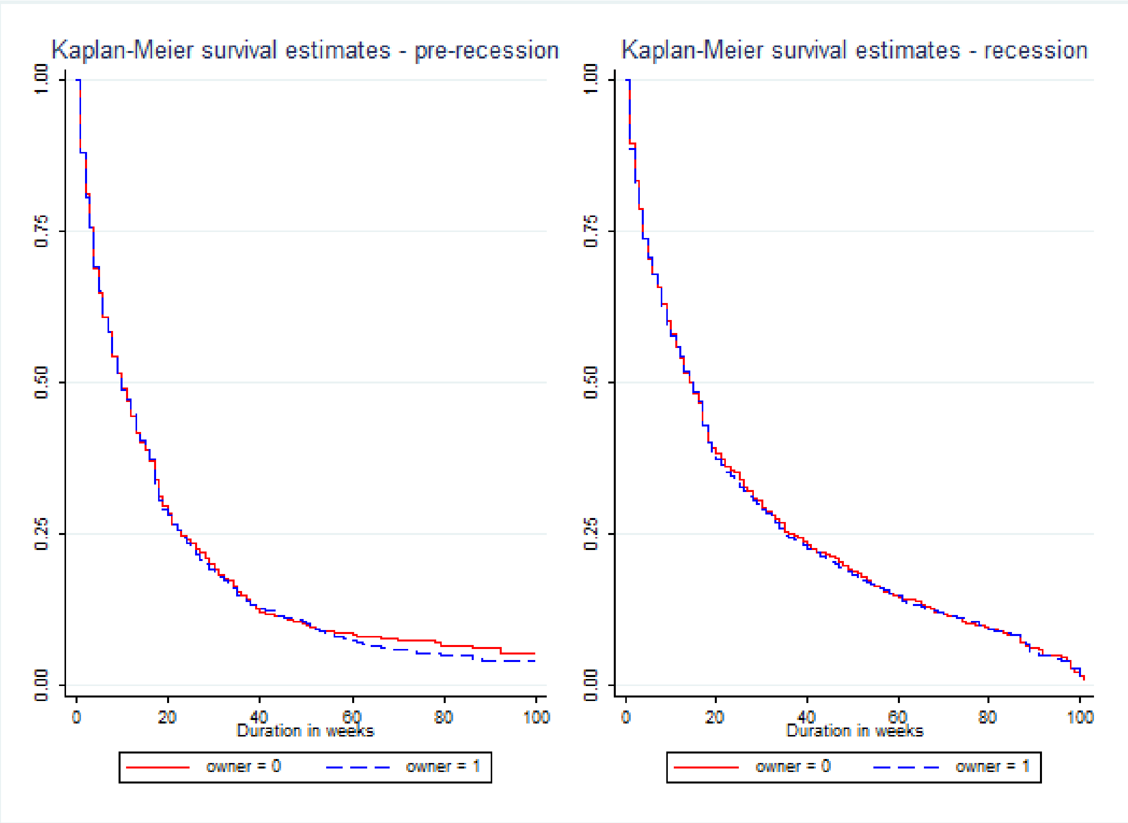

Figure 1: Kaplan-Meier Survival Functions: Exit to employment

hazard.

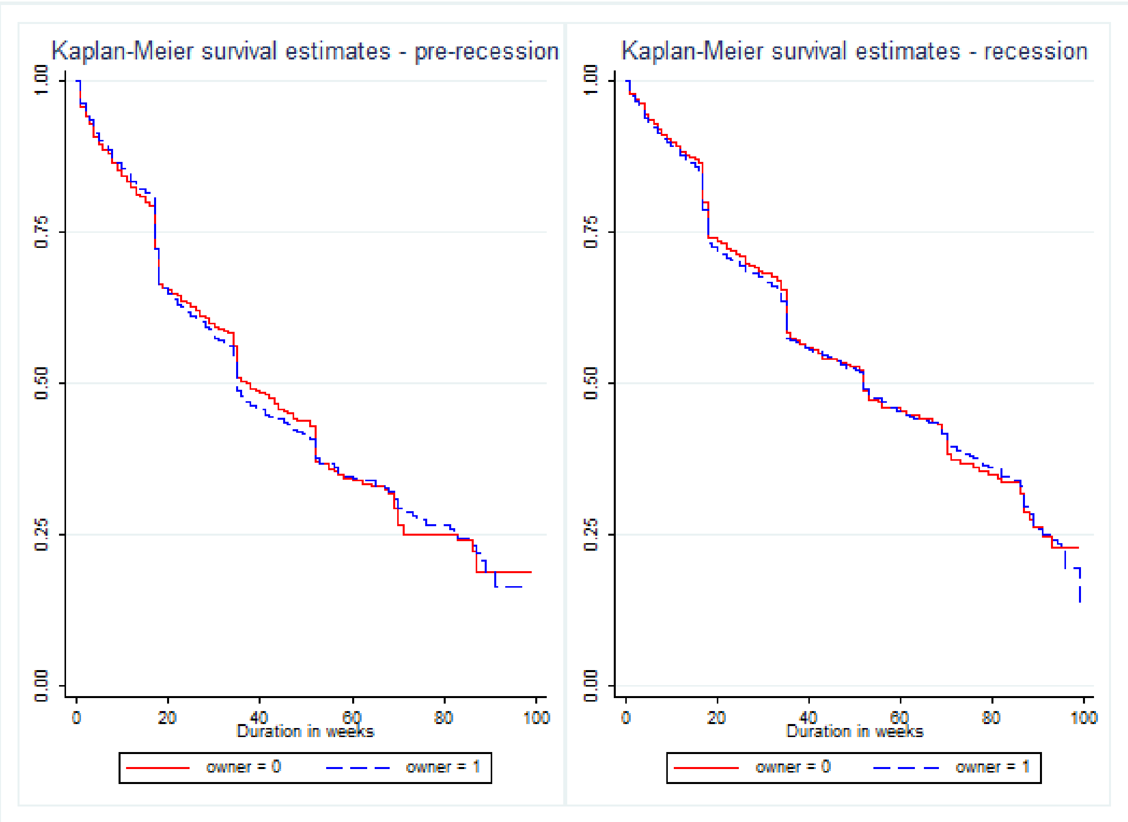

Figure 2: Kaplan-Meier Survival Functions: Exit to non-participation

Table 2 reports descriptive statistics for a subset of the covariates we use in our analysis, separately for spells in the pre-recession and the recession samples. We see that those who were unemployed before the recession differ in important ways from those who were unemployed in the 2008 panel. On average, in the recession an unemployed person was less likely to be black, more likely to be hispanic, more likely to be married, more educated, older, more likely to live in a metropolitan area, and more likely to be homeowners. Finally, table 3 reports descriptive statistics separately for renters and owners for the pooled sample. Owners are more likely to be white, married, with children, better educated, older, and with higher pre-spell incomes. They also are less likely to live in urban areas. All of these differences are highly significant and highlight the importance of controlling for these factors when estimating the effect of homeownership and whether this effect is different during the recession. The absence of a visible change in owner-renter hazards between the pre-recession and the recession in figure 1 could be due to the fact that the spells have very different characteristics in the two time periods. Importantly, they also suggest that the unemployment spells in the recession might be different in important unobservable ways as well.

Mobility The SIPP allows us to characterize 3 types of mobility: 1- Address change, 2- Metropolitan Statistical Area (MSA) change, 3- State change. While MSA is the preferred measure for a labor market, and hence for labor mobility, we don't have MSA information for the 2004 and 2008 panels and for the previous panels the data only lists a subsample of MSAs.[6] Interstate migration, on the other hand, underestimates labor mobility because it excludes inter-county and inter-MSA moves that happened within a state. Therefore we instead use address change as a mobility indicator. According to CPS March Supplements from 1998 to 2012, 30 percent among those who change residence within a year list job related reasons as primary motive for the move. Presumably among the unemployed this share is considerably higher, it is documented that unemployed people move more often compared to otherwise identical employed individuals (Molloy, Smith and Wozniak (2011)).

The SIPP records the address of a household at monthly frequency. Since unemployment spells contain weekly information we consider any address change that happens up to 4 weeks after the termination of the spell as a move within the spell. Although some individuals move more than once during an unemployment

[6]For those panels we end up losing more than 40 percent of the observations.

spell for the reports here and henceforth we always consider the first move. Table 4 describes mobility and unemployment characteristics of individuals separately for owners and renters across the samples. Not surprisingly, renters are more mobile than owners. In the pre-recession sample the difference in the share of spells that exhibit an address change during or shortly after the unemployment spell between renters and owners is 5 percentage points. This difference increased to 5.9 percentage points in the recession, amounting to a significant (for 10% significance level) 1.35 percentage point difference-in-difference effect of the recession on renter-mobility. While probably not large enough to be economically important, this supports the mobility mechanism of unemployment laid out in the Oswald hypothesis. However, a closer look at the spell characteristics in table 5 reveals that the increased mobility of renters has not translated into more favourable outcomes. Movers are a negatively selected group. Their average unemployment duration before the recession was 10.7 weeks longer than those of stayers - more than twice as long. Importantly, this difference has *grown* in the recession, and thus an increase in mobility cannot have alleviated the detrimental effect of the recession. Quite to the contrary, the gap in unemployment duration between stayers and movers has increased by 50% in the recession, and the share of spells ending with a job have decreased for movers and increased for stayers. Rather than being a way out of unemployment, a location change seems to be more of a consequence of an unusually long unemployment spell. The other spell characteristics do not exhibit significant difference-in-difference estimates for movers compared to stayers.

Unlike Munch et al. (2006) we only describe the mobility differences between owners and renters and its trajectory along the recession, and do not proceed to estimate local vs. non-local job finding hazards. This decision is based on the fact we do not have a reliable measure of a geographical labor market in the data. The mobility summary statistics we presented above define an address change as a move. But this definition of a move does not necessarily imply a change in the labor market. Hence using our mobility definition would overestimate the non-local job finding rates. Alternative measures either underestimate the non-local job finding (change in state of residence) or do not exist for most of the estimation period (MSAs). Moreover, unlike European countries, the unemployment spells are considerably shorter in the US. Thus, even with our generous location change measure we have insufficient mobility – especially for homeowners – to have reliable estimates for local vs. non-local hazard rates.

4 Estimation

We have seen that the raw data do not display much difference in unemployment experiences between owners and renters, and that there is no discernible change in the relative job hazard of owners compared to renters over time. However we also know that the characteristics of homeowners and renters are quite different, and that unemployment spells in the pre-recession and the recession had different characteristics. We now proceed to a full econometric analysis to isolate the effect of homeownership from other observable and unobservable determinants of unemployment durations. We first separately estimate proportional hazard models for transitions to employment and transitions to non-participation. The hazard for a given spell and the log-likelihood function for the sample for these models are given by

$$\theta_e(t|x,z) = \lambda_e(t)\exp(\beta_e' x + \gamma z) \tag{1}$$

$$\ln L = \sum_m d_{em} \ln \theta_e(t|x_m, z_m) - \int_0^t \theta_e(s|x_m, z_m)\mathrm{d}s \tag{2}$$

Here e denotes the exit-state of interest (job j, non-participation n), m denotes the spell, and x the vector of covariates, which we restrict to be time invariant (by fixing the variables to the values at the beginning of a spell) given the scale of estimation, d_{em} is an indicator taking the value 1 if spell m ends in exit to state e and 0 otherwise, z is a dummy for homeownership, and $\lambda_e(t)$ is the exit-specific baseline hazard. We specify the baseline hazard as a piecewise-constant function. In particular, the baseline hazard is constant for the intervals of 0 to 4 weeks (most exits falling into this interval), 5 to 10 weeks, 11 to 16 weeks, 17 to 18 weeks (this piece is included to account for the *seam bias* in the SIPP - the observation that reported variables including employment status often change between the end of one and the beginning of the subsequent wave), 19 to 26 weeks (pre-recession unemployment benefits running out after 26 weeks), and longer than 26 weeks. The covariates in our analysis are dummies for black, hispanic, married, three educational categories (less than high school, high school, college), six age categories (18-24, 25-29, 30-39, 40-49, 50-59, and older), the log amount of last observed real earnings before unemployment, a dummy for no positive earnings before unemployment, dummies for the employment status of the partner (if applicable, 0 for non-married individuals), a dummy for the presence of children below the age of 18 in the family, a dummy for no pre-spell family income, the log of pre-spell family income (0 if no pre-spell family income), a dummy for the receipt of unemployment benefits, a dummy for living in a metropolitan area, and dummies for each of the four SIPP panels. For the pooled sample (pre-recession and recession) we also include an interaction term between homeownership status and a dummy for the recession sample. This "difference-in-difference" coefficient estimates the effect of the recession on the hazard for homeowner relative to renters. The estimate can be interpreted causally if we assume unemployment spells conditional on our control variables to be on average the same in unobservable factors in both samples. Given the differences in observable characteristics this is unlikely to hold, but the estimate should be at least suggestive.

After the separate regressions we proceed to the estimation of a full-information maximum likelihood model which introduces unobserved heterogeneity, accounts for the selection into ownership status, and for the possibility of exiting unemployment into employment or non-participation. We follow the specification and notation in Munch et al. (2006). Equation 1 is modified to include an unobserved and exit-specific heterogeneity term v_e

$$\theta_e(t|x,z,v_e) = \lambda_e(t)\exp(\beta_e' x + \gamma z + v_e) \tag{1'}$$

with $e \in \{j,n\}$. The probability of being a homeowner is specified as a logit probability with unobserved heterogeneity v_h

$$P(x_h, v_h) = P(z=1|x_h, v_h) = \frac{\exp(\beta_h' x_h + v_h)}{1+\exp(\beta_h' x_h + v_h)} \tag{3}$$

where x_h consists of covariates described for the unemployment hazards with the addition of an instrumental variable that we describe below. In principle a joint model of unemployment duration and homeownership can be estimated without exclusion restrictions in the ownership selection if multiple spells for a subset of individuals exist and if they switch ownership status between spells (see Honoré, 1993). Since our data only covers 3-5 years per panel, the incidence of ownership switching across spells is very infrequent. We therefore include an instrumental variable that will correct for the selection problem. Our instrument selection is discussed in the next section. We estimate this joint model separately for the pre-recession, the recession, and the pooled samples.

In specifying the distribution of the unobserved heterogeneity components v_j, v_n, v_h we follow the non-parametric specification of Heckman and Singer (1984).[7] The three components are assumed to have a

[7]This specification has been used in more recent papers in estimating duration models with unobserved heterogeneity. See

tri-variate distribution with two points of support in each dimension, thus allowing for eight different types. One point of support for each v_j and v_n needs to be normalized, and so does the constant term in the home-ownership equation (3). We normalize these points to zero. Every type is associated with a probability which corresponds to the share of the sample which is of this type. To find the (unconditional on heterogeneity) probability of an individual spell being a homeowner and being unemployed for t weeks, the heterogeneity terms need to be integrated out, thus giving the following contribution of a spell to the likelihood function:

$$L = \left(\int\int\int P(x_h, v_h)^z \left[1 - P(x_h, v_h)\right]^{1-z} \theta_j(t|x,z,v_j)^{d_j} \theta_n(t|x,z,v_n)^{d_n} \right.$$
$$\left. \exp\left[-\int_0^t \theta_j(s|x,z,v_j)\mathrm{d}s - \int_0^t \theta_n(s|x,z,v_n)\mathrm{d}s \right] \right) \mathrm{d}G(v_j, v_n, v_h) \tag{4}$$

The parameters of the model β, γ, and λ are estimated jointly with the heterogeneity parameters v and the parameters of the distribution G, e.g. the probabilities of each of the eight types.

While the flexibility of the Heckman-Singer model is attractive, it also comes at a cost. The likelihood function is not globally concave, and in estimating it we have frequently encountered near-flat surfaces of it. Estimation thus needs to be carried out by global optimization routines which can take long times to conclude without a guarantee of having found the global maximum. We have estimated the FIML model by simulated annealing (see Goffe et al. (1994), and Li and Smith (2010)). In order to reduce the computing time we don't use the time varying nature of our covariates. All variables, including ownership status, are fixed to their value at the beginning of the spell. Since a big portion of the spells terminate within a month we argue that this does not affect the main conclusion.[8]

Instrument Selection The dominant exclusion restriction used in the literature is the homeownership rate in the state of residence.[9] The exogeneity of this variable with respect to unemployment duration is questionable because there is at least contemporaneous correlation between housing and labor market conditions. Indeed, adding the state homeownership rate to the unemployment duration regression yields a significant relationship.[10] Other instruments that we considered and that have been used in the literature were housing

Munch et al. (2006), Munch et al. (2008), and van Leuvensteijn and Koning (2004). See Heckman and Singer (1984) for a discussion of the problems associated with parametric mixing distributions.

[8]We conduct separate regressions that include time varying covariates and this yields quite similar results for our main variable of interest.

[9]Aaronson (2000), DiPasquale and Glaeser (1999), and van Leuvensteijn and Koning (2004) are few examples.

[10]This is also confirmed by Coulson and Fisher (2009).

price indices, mortgage interest deductions, and measures relating to the difficulty to construct or convert new owner-occupied housing units such as the Wharton Residential Land Use Regulatory Index by Gyourko, Saez and Summers (2007) or the index used by Saks (2008). We ruled out using price indices for the same reason that we ruled out the state homeownership rate: An economic boom might reduce unemployment durations and boost housing demand at the same time. We tried estimating the model using state- and year-variant mortgage interest deduction rates but this instrument turned out to be a poor predictor of homeownership. Indeed, Hilber and Turner (2014) demonstrated that the effect of this rate is sensitive to the housing supply elasticity and household income and is not even monotonic (increasing the incentive for homeownership for some, and decreasing it for other, down-payment constrained households).

This left us with using a proxy for housing supply elasticity. The Wharton index is a popular choice in the literature, but has the important disadvantage not to pre-date our analysis period. The index created by Saks (2008) includes only a number of metropolitan areas, which are not reported in the 2004 and 2008 SIPP panels and which would induce an important sample selection bias by excluding observations not living in metropolitan areas. We thus decided on an exclusion restriction which is pre-determined to labor market conditions but a likely determinant of housing supply and its short-term elasticity, which is determined independently of the recent housing choices of the residents, and which we can - albeit imperfectly - use for all observations, regardless whether they live in a metropolitan area or not. For that we use a state level land use regulation index proposed by Ganong and Shoag (2013) which measures the fraction of cases that involve the word “land use” in the state court records.[11]

Table 6 shows selected values for each state used in our estimation. Notably, our regulation measure is larger in coastal states both in the East and the West regions.[12] This is in line with Saiz (2010) which suggests that local land supply characteristics are strongly related to housing regulation. Ganong and Shoag (2013) show that the index is strongly correlated with widely used previous surveys that are constructed to capture the relative stringency of residential growth controls such as the Wharton index, but has the advantage of exhibiting annual time variation. For the rest of this section we will provide suggestive evidence towards the effectiveness of this variable.

[11] We thank Peter Ganong for sharing their data with us.

[12] One important exception is the state of New York which is highly regulated but has also many legal disputes in general.

The important feature of land use regulation is that it is an exogenous predictor of housing supply: The more regulated a city is the harder it is to build new houses and most shifts in housing demand will be reflected in prices rather than quantities. For that reason we argue that being a homeowner is less probable in states with more regulation. In order to demonstrate the causal linkage between homeownership and land use regulation we characterize the homeownership rate as follows:

$$H_{it} = S_{new_{it}} H_{new_{it}} + (1 - S_{new_{it}}) H_{old_{it}}$$

Here H_{it} denotes homeownership rate in state i at time t, $S_{new_{it}}$ represents the share of newly built houses in that state, and $H_{new_{it}}$ ($H_{old_{it}}$) denotes the homeownership rate in newly built houses (existing stock of houses). In principle all these variables could be affected by land use regulation in that state. To investigate this we show the cross-sectional relationships between these variables and regulation. For illustration we rely on state level data from the 2000 Census since it predates the peak of the housing bubble and the subsequent crisis, however the results for later years are qualitatively similar. We use a one year lag of our regulation measure.

We first show the relationship between regulation and the share of newly built houses[13] for states used in our estimation excluding outliers.[14] Figure 3 shows that highly regulated states have smaller shares of newly built houses. This is in line with the previous studies that have shown that there is strong positive relationship between house prices and land use regulation.

[13]In this particular instance the share refers to the houses built in or after 1999.

[14]Here and henceforth we characterize outliers for each variable of interest by being 2 standard deviations away from the mean. We also exclude NY for the analysis performed in this section.

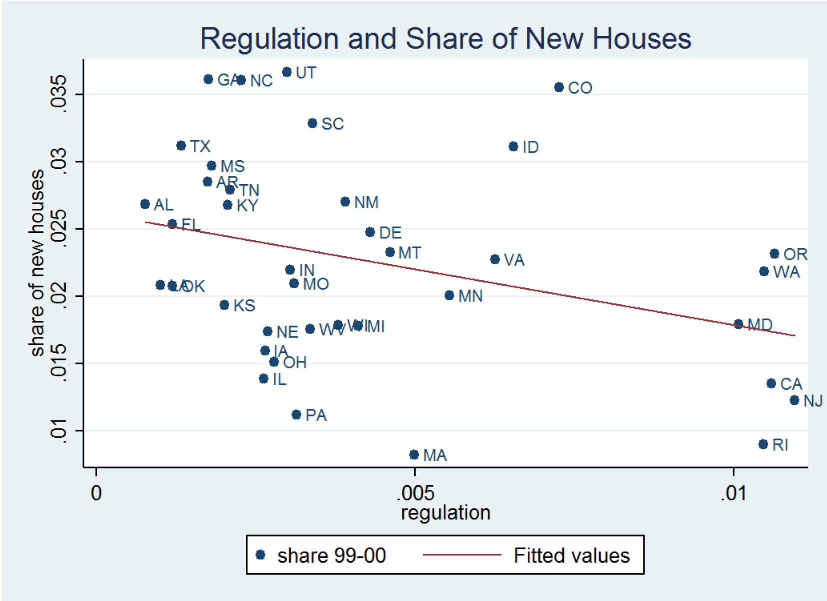

Figure 3: Regulation and Share of New Houses
Source: Ganong and Shoag (2013), Census 2000. Share of new houses includes houses built between 1999 and 2000. Regulation index represents the fraction of cases that involve the word "land use" in the state records. A linear fit of form: $Y = \beta_0 + \beta_1 X$ induces $\beta_1 = -0.824(0.325)$ with standard error in parenthesis.

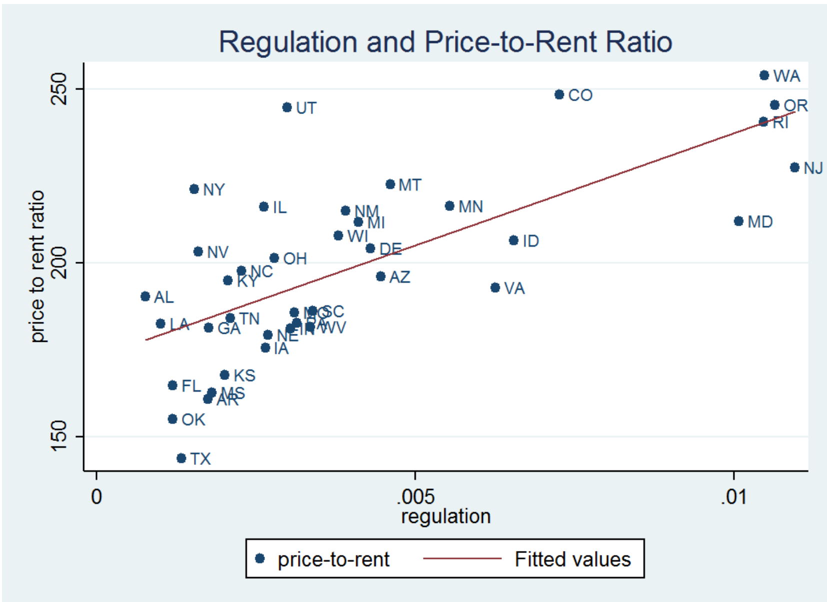

Figure 4: Regulation and Price-to-Rent Ratio
Source: Ganong and Shoag (2013), Census 2000. Price to rent ratio is obtained dividing the median home value by median gross rent in 2000. Regulation index represents the fraction of cases that involve the word "land use" in the state records. A linear fit of form: $Y = \beta_0 + \beta_1 X$ induces $\beta_1 = 6439(1059)$ with standard error in parenthesis.

One might argue that although more regulation yields higher house prices (hence less building activity) this does not necessarily imply higher cost of owning a house vs. renting. In principle, supply restrictions seem just as likely to drive up rents for apartments as prices of homes. For that reason we construct price-to-rent ratios for states using 2000 Census data and demonstrate its relationship with regulation in Figure 4. The relationship is substantially strong and robust to including variables such as per capita income and population density which rules out potential concerns regarding the effect of urbanization on homeownership. Since we showed that regulation is positively correlated with the relative cost of owner-occupied housing this will have direct implications for buying newly built houses. Figure 5 shows that there is a negative relationship between regulation and the homeownership rate among new buildings. Figure 6 also demonstrates a similar effect of regulation on the homeownership rate among old buildings. Finally there is also a mechanical affect coming from the difference between homeownership rates of newly built houses and of old houses. Since our sample period mostly coincides with a housing boom the homeownership rate of newly built houses is substantially greater than the overall homeownership rate. Figure 7 shows this relationship for 2000.[15] This concludes that at the cross-sectional level almost every piece in the homeownership rate decomposition contributes negatively for an increase in regulation.

Although we have shown that our instrument is correlated with the homeownership rate there is also a potential concern regarding its relationship with unemployment duration. One could argue that housing supply restrictions affect commercial development which might in turn affect local job opportunities. In fact, Blanchflower and Oswald (2013) argue that incumbent homeowners in a location could push up further regulations that would result in lower commercial development. In order to see whether this has any effect on our variable of interest, unemployment duration, we compile annual unemployment rates for each state from the BLS and for each year we regress the unemployment rate on our regulation index together with homeownership rate at the state level.[16] If – conditional on homeownership – there is an effect of regulation on unemployment rates, the validity of our instrument would be seriously questioned. We find that from 1995 to 2011 virtually none of the regression coefficients on regulation index turns out to be significant at any meaningful level. Although this does not necessarily rule out the possible relationship between our instrument and unemployment duration, it could suggest that this relationship is at best weak.

[15] We see a reverse figure after the crash of 2008.

[16] We control for the homeownership rate since the instrument, by design, affects the second stage through the endogenous variable.

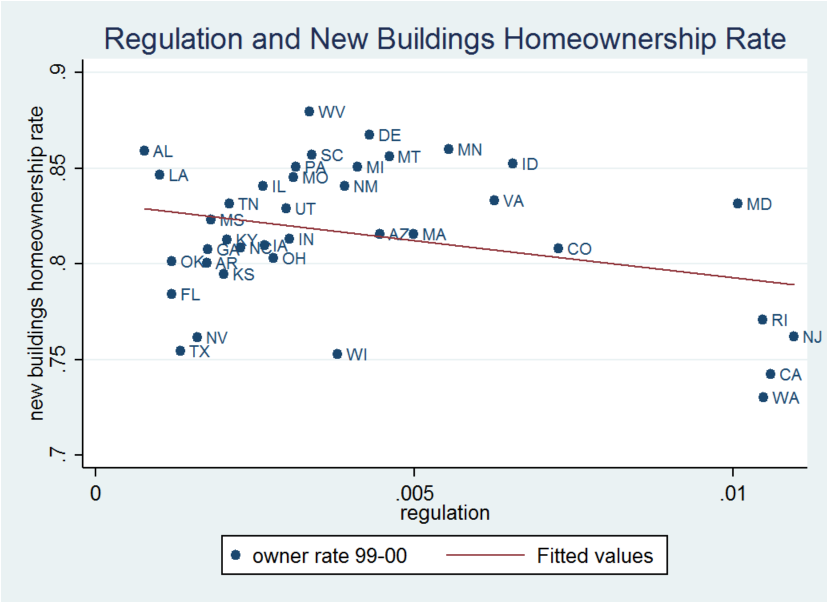

Figure 5: Regulation and New Buildings Homeownership Rate
Source: Ganong and Shoag (2013), Census 2000. New buildings homeownership rate is the share of owner occupied homes within houses built between 1999 and 2000. Regulation index represents the fraction of cases that involve the word ”land use” in the state records. A linear fit of form: $Y = \beta_0 + \beta_1 X$ induces $\beta_1 = -3.892(2.011)$ with standard error in parenthesis.

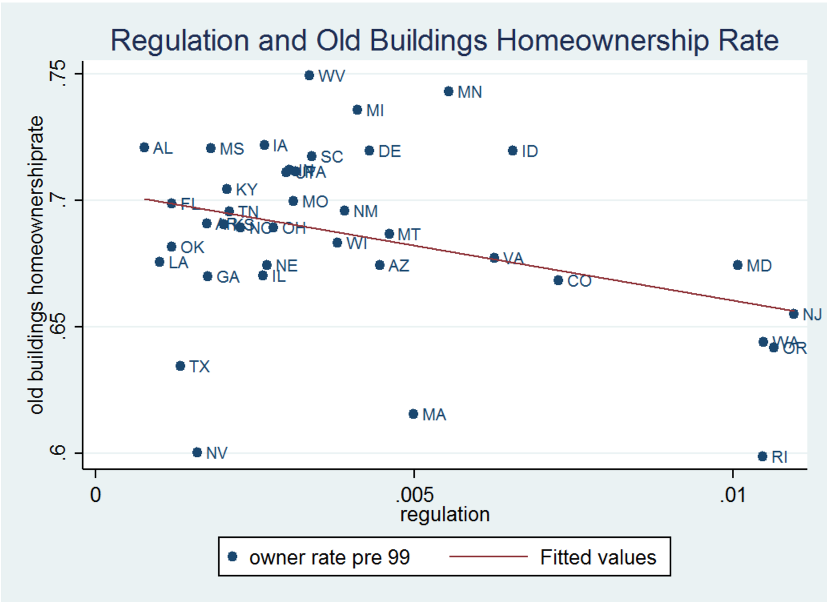

Figure 6: Regulation and Old Buildings Homeownership Rate
Source: Ganong and Shoag (2013), Census 2000. Old buildings homeownership rate is the share of owner occupied homes within houses built before 1999. Regulation index represents the fraction of cases that involve the word ”land use” in the state records. A linear fit of form: $Y = \beta_0 + \beta_1 X$ induces $\beta_1 = -4.341(1.863)$ with standard error in parenthesis.

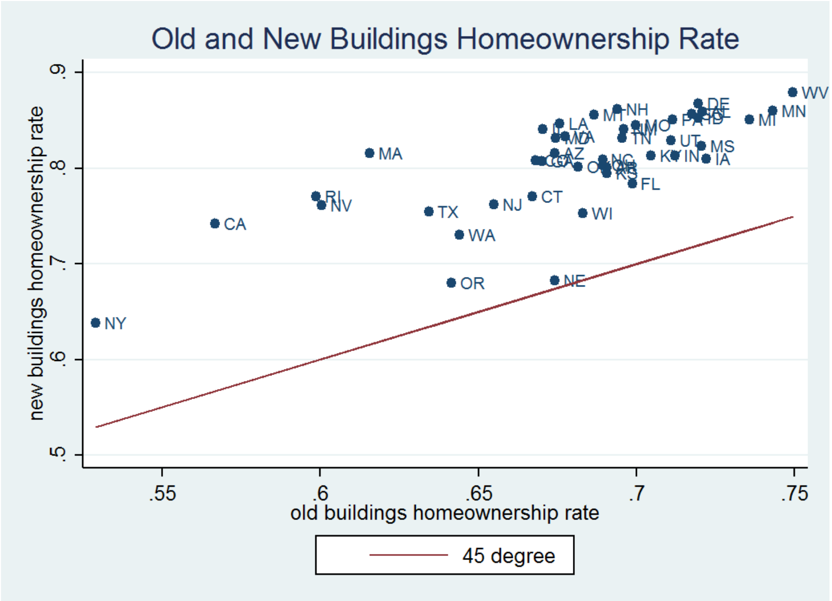

Figure 7: Old and New Buildings Homeownership Rate
Source: Ganong and Shoag (2013), Census 2000. New buildings homeownership rate is the share of owner occupied homes within houses built between 1999 and 2000. Old buildings homeownership rate is the share of owner occupied homes within houses built before 1999.

5 Results

In this section we provide two sets of results: one from separate regressions for the job-finding hazard, non-participation hazard, and the housing choice, and the other from joint estimation of job-finding and non-participation hazards together with selection into homeownership (FIML). For the separate regressions, the competing risk is treated as censored and no heterogeneity assumptions are made. For each estimation we pool three SIPP panels (1996, 2001, 2004, pre-recession from here and afterwards) and report results for the pre-recession, recession and overall samples.[17] For expositional simplicity we provide detailed results for the separate regressions, and for the joint estimation (FIML) of the pre-recession sample; and focus only on the ownership coefficient for the remaining estimations.

Table 7 presents results for the separate regressions for the pre-recession sample. The first two columns report the coefficient and standard error estimates of the housing model. Generally, the estimation results are in line with economic intuition: the probability of being a homeowner increases with age, education and income level. White individuals (compared to black or hispanic individuals) and married individuals are

[17]We repeat the same exercise separately for each panels which mostly yield similar results but with varying magnitude and significance. The results are available upon request.

more likely to be homeowners. Having a child significantly increases the likelihood of being a homeowner. Living in a metropolitan area is negatively associated with the probability of being a homeowner. There is no significant difference between panels in terms of homeownership tenure.

For the job finding hazard we find no significant effect of being a homeowner. The coefficient is 0.001 translating into an economically and statistically insignificant effect. However this calculation omits possible negative externalities of homeownership on other owners or renters of the kind considered in Blanchflower and Oswald (2013), a venue that is not further pursued in this paper.

Our result is in the ballpark of results that have been found for the effect of homeownership on unemployment duration in the United States. Valletta (2013) reports a zero effect and Green and Hendershott (2001) find a zero effect for homeownership if they do not control for selection into homeownership. The job hazard of homeowners becomes significantly positive once they control for selection. However, they use state-fixed effects as exclusion restrictions and we do not see any reason why there should be state-fixed effects in a homeownership regression but not in the duration model. Coulson and Fisher (2002) also report a positive effect of homeownership on the job finding hazard. But their model does not include income variables and uses a sample of only 204 observations. Our result contrasts stronger with results found for Denmark by Munch et al. (2006), who find owners' job hazards to be 40% higher than that of renters, and for Australia by Flatau et al. (2003) who find it to be 15% higher. This might reflect the different institutions governing the labor markets in these countries and in the United States and/or the different set of control variables.

The job finding hazard rates in different panels exhibit negative duration dependence in the sense that as the unemployment spell persists the transition rate into employment decreases. Single, black, and older individuals and individuals with lower pre-earnings have lower rates of finding a job. Education follows a non-monotonic pattern: individuals who have finished high school have the highest job finding rate. For married households, the wife being not in the labor force increases the job hazard, possibly due to the fact that it provides stronger incentives to find a job sooner. Interestingly having children decreases the job hazard. Receiving an unemployment benefit, as expected, also decreases the job hazard.

The non-participation hazard does not exhibit the same stability over time as the job hazard. The piecewise component for the 4th month is strikingly high compared to the other time intervals, most likely reflecting the seam bias in the SIPP. There is no monotonic duration dependence, instead it exhibits a hump shape where the seam bias is the turning point. The effect of ownership on the non-participation hazard is positive but insignificant. Apart from homeownership, we find that married and middle age individuals have significantly lower hazards, and having higher wages before unemployment lowers the hazard. Having an unemployed wife significantly decreases the likelihood of exiting the labor force.

We now turn to the FIML results of the joint model where we control for unobserved heterogeneity as well as selection into homeownership. Owners could be positively or negatively selected into ownership with respect to unobservable characteristics that influence unemployment durations. More productive and skilled individuals might prefer or afford to be a homeowner and at the same time have short potential unemployment durations. This would bias the owner coefficient in a job hazard upward. Indeed, observable characteristics – which are likely to be correlated with these attributes – confirm that more educated individuals with higher incomes are more likely to be homeowners (see the first two columns in Table 7). On the other hand, individuals who value flexibility and wish to remain flexible to be able to take on jobs elsewhere might prefer to rent. This flexibility might also improve their job-finding skills. As a thought experiment, one can imagine a labor market with all citizens being homeowners. Suppose now that the labor market goes into decline. Those citizens who are not willing to accept lower economic standards sell their homes and move to stronger labor markets. Some of them – at least initially – move into rental units. These movers could be positively selected in their job-market skills or ambitions, which made them move in the first place. This effect is not just a theoretical possibility: In the 1996 SIPP 65 percent of individuals who reside in their state of birth are homeowners, whereas only 51% of those who live in a state different from their state of birth are homeowners. Another example could be based on unobserved wealth. Imagine two persons with equal observable variables, but one having more wealth (which is unobserved). The wealthy person might decide to buy a home, but he might also have higher reservation wages in case of unemployment relying on his wealth. In both cases, the unobserved factor that made the person more likely to be renter (e.g. economic ambition or lower wealth) also makes his unemployment duration shorter. The result would be a downward bias in the owner coefficient. In the joint estimation we employ an exclusion restriction that controls for both types of selection.

We argue that land use regulation at the local level is strictly exclusive to the current housing preferences of individuals other than house values since it is not an equilibrium object like the local homeownership rate as implied by a typical housing model. Although it might be correlated with the long run housing demand it is more related to the local housing supply features such as land availability etc. (Saez (2010)). Hence the local land use regulation instrument will allow us to circumvent the endogeneity problems proposed above, on the other hand it is a strong predictor of the housing market. In fact the separate selection regression in Table 7 (first two rows) confirms that land use regulation is negatively correlated with homeownership with a highly significant coefficient.

Results for the joint estimation are reported in Table 8. The parameters in the joint estimation have very similar effects compared to the separate regressions. The zero effect of homeownership turns positive, suggesting that there is a negative correlation between unobservables in the selection equation and the job finding hazard. The coefficient of homeownership on nonparticipation hazard is reduced and still insignificant suggesting a slight positive correlation for the latter hazard with selection. These results suggest that unobserved characteristics that favor owning a house are negatively related to unobserved characteristics that increase the probability of finding a job. The correlations suggested by the Heckman-Singer coefficients and type probabilities testify to this selection: The correlation coefficient for unobserved heterogeneity in owning a home and finding a job is -0.98 whereas the corresponding correlation for the homeownership and non-participation is 0.05. The negative correlation between homeownership and the job findind hazard might seem unintuitive. But we have mentioned above why a priori both positive and negative correlations are conceivable. We would also like to remind the reader that every observation in the sample is already "selected" in the sense of being unemployed. The average unemployed homeowner/renter is likely to be different from the average employed owner/renter. We emphasize that the best solution we found to the likelihood function has converged – even after applying global search methods such as simulated annealing – to the zero-bounds for some of the mass-points. 76% of the probability mass is concentrated on one of the eight possible types, and some others are close to or virtually undistinguishable from zero. This contrasts with the findings of other papers which have used this methodology, but one also has to keep in mind that we are controlling for many more observable characteristics and a relatively flexible specification of duration dependence.

In Table 9 we show estimates of the homeowner coefficient on the job finding hazard for different specifications. In both the pre-recession and the recession samples, the owner coefficient in a duration model without any other control variable yields a zero result, reproducing Figure 1. Adding control variables to the pre-recession sample gives us the result from Table 7, and the last column is the estimate from Table 8. From the third column we see that the zero-result is robust to the inclusion of state-fixed effects. The owner coefficient is slightly higher - but insignificant - in the recession sample once other control variables are included in the regression, and it also turns positive for the FIML estimation.[18] The last panel of the table shows results for the pooled sample. The job hazard is much lower in the recession, but being a homeowner is not more of a liability in a recession than in a non-recession. If anything, homeowners tend to find jobs slightly faster in the recession than renters. With respect to the non-participation hazard we also fail to detect any statistically or economically significant effect of homeownership. Table 10 reports the results for the non-participation hazard. Owners seem a little more likely to exit to non-participation in the separate regressions, but no significant difference between the pre-recession and the recession samples exists. Conversely, for the overall sample, FIML estimation yields a negative and significant effect of homeownership and non-participation.

Although, we could not find any meaningful effect of ownership on job finding and labor force participation overall, this does not rule out the possibility that some subgroups within the population could face this effect. we repeat separate single risk regressions of job finding and non-participation on selected subgroups. Two observations stand out from Tables 11 and 12: First, overall we have opposite signs for the same subgroup between job finding and non-participation hazards. Of those which are significant, we find that the effect of homeownership on job finding is negative for young, single and low income individuals. Similarly, high school graduates, young and low income individuals experience a positive relationship between being

[18] We have also estimated the FIML model with state-fixed effects which produced very similar estimates to the model without state-fixed effects. The results are available upon request. We decided not to include this specification in the text because the variation in the instrument once state-fixed effects are controlled for is rather small: only the time variation remains. This results in a weak partial correlation of the instrument with ownership, that is, a weak "first stage".

a homeowner and exiting the labor force.

One issue left unanswered in these type of studies is the selection into the unemployment pool. Unemployment itself, in general, is an outcome of the individual's decision problem and there could potentially be factors that affect the flow in and out of unemployment jointly. This might be particularly important in our case since homeownership is found to reduce inflow into unemployment (Munch et al. (2008), van Leuvensteijn and Koning (2004)). However, this alone does not tell us the direction of the bias induced by selection into unemployment. For an unbiased estimator, we need to know the counterfactual job finding rates of currently employed homeowners and renters. If the counterfactual job finding rate of employed homeowners and renters are equal to each other and on average higher than the currently unemployed, then we would push the job hazard of owners downward. This also depends on the observation that our data has higher share of homeowners in the unemployed pool compared to the homeownership rate at the aggregate level.[19] However, given the fact that the estimates without accounting for selection into unemployment yield very similar job finding rates for homeowners and renters, we do not have strong evidence implying a bias due to this selection. We leave this issue for further research.

6 Conclusion

This paper investigates the effect of being a homeowner on the duration of unemployment spells of unemployed individuals using micro data covering 18 years in the USA. The question is economically relevant due to the fact that housing is heavily subsidized and makes people less mobile and hence potentially distorts individuals' unemployment durations as well as the relative attractiveness of exiting to employment compared to non-participation. After examining 4 consecutive SIPP panels that cover the period 1996 to 2013 and accounting for unobserved heterogeneity and selection into homeownership, we conclude that 1) ownership did not reduce the job finding hazard or the non-participation hazard in the recession or before the recession, 2) the job-finding hazard and the non-participation hazard of owners compared to renters did not change in the recession. This result is stable over different panels and controlling for possible endogeneity does not change the qualitative conclusion. This result thus rejects Oswald's hypothesis for the US as it has been done in many other case studies.

[19]It is easy to think of this in a simple scenario: Let's assume that all the employed people lose their jobs then find a job within a week. In this case the drop in unemployment duration of renters would be higher than the drop in homeowners' unemployment duration due to higher share of homeowners in the unemployed pool.

In light of the two hypothetical effects of the recession on homeownership we thus reject the hypothesis that homeownership becomes a liability only in economic downturns. However, one should keep in mind the unusual severity and spatial extension of the Great Recession. Jobs might have become scarce everywhere thus possibly attenuating any potential advantage attached to high mobility. A business cycle with more disparity in its regional effects might have produced stronger effects, but we think it unlikely that the major conclusions from this episode would be overturned. Moreover, since the 2008 recession is also associated with a severe housing market downturn with a dramatic increase in foreclosures one would need to incorporate the mortgage market and homeowners' indebtedness into the relationship between homeownership and unemployment. We leave this for future research.

Table 1: Summary statistics of spell characteristics, renters and owners

	1996-2004			2008		
	Owners	Renters	Δ	Owners	Renters	Δ
spells	11,029	7,937		5,597	3,561	
average unemployment duration (weeks)	11.1	10.7	0.4**	16.1	15.3	0.8**
spells ending with job (%)	68.5	65.7	2.8***	69.1	65.9	3.2***
spells ending with nonparticipation (%)	20.9	20.4	0.4	22.4	20.3	2.1**
censored spells (%)	10.7	13.8	-3.1***	8.5	13.8	-5.3***

Source: SIPP 1996-2004 and SIPP 2008. Stars indicate significance levels for tests of mean equality between renters and owners: ***:p<0.01, **:p<0.05.

Table 2: Summary statistics of covariates at the beginning of spell, by period

	1996-2004	2008	Difference
Black (percent)	12.4	10.5	1.9***
Hispanic (percent)	15.3	19.0	-3.7***
Married (percent)	47.3	50.3	-3.0***
Has children (percent)	58.6	58.6	0.0
Less than high school (percent)	20.9	16.9	4.0***
High school (percent)	64.5	65.9	-1.5**
College (percent)	14.7	17.2	-2.5***
Age (mean)	36.9	40.0	-3.0***
Deflated monthly pre- spell earnings in 1996 $ (mean)	1817	1760	57*
Deflated monthly pre-spell family income in 1996 $ (mean)	3468	3422	46
Unemployment benefits (percent)	16.1	20.9	-4.9***
Metro area (percent)	78.0	79.0	-1.0**
Homeowners (percent)	58.2	61.1	-3.0***
Number of spells	18,966	9,158	

Stars indicate significance levels for tests of mean equality between the 1996-2004 panels and the 2008 panel: ***:p<0.01, **:p<0.05, *:p<0.1.

Table 3: Summary statistics of covariates at the beginning of spell, by ownership

	Renters	Owners	Difference
Black (percent)	14.5	9.9	5.7***
Hispanic (percent)	23.0	12.0	11.1***
Married (percent)	39.7	54.2	-14.6**
Has children (percent)	49.4	65.0	-15.6***
Less than high school (percent)	26.8	14.6	12.2***
High school (percent)	62.0	66.9	-4.9***
College (percent)	11.1	18.5	-7.3***
Age (mean)	34.9	40.0	-5.1***
Deflated monthly pre- spell earnings in 1996 $ (mean)	1413	2065	-652***
Deflated monthly pre-spell family income in 1996 $ (mean)	2227	4301	-2074***
Unemployment benefits (percent)	14.3	20.0	-5.7***
Metro area (percent)	81.3	76.2	5.1***
Number of spells	11,498	16,626	

Stars indicate significance levels for tests of mean equality between renters and owners: ***:p<0.01, **:p<0.05, *:p<0.1.

Table 4: Mobility statistics I: Renters and owners

	Movers (%)		
	1996-2004	2008	Δ
owner spells	1.70	1.79	-0.08
renter spells	6.69	7.72	-1.03**
Difference	-4.99***	-5.94***	*0.95**

Source: SIPP 1996-2004 and SIPP 2008. Stars indicate significance levels for tests of mean mobility rates between renters and owners (row Difference), and between the 1996-2004 and the 2008 panels (column Δ): ***:p<0.01, **:p<0.05, *:p<0.1

Table 5: Mobility statistics II: Spell characteristics of renters

	Movers	Stayers	Δ
average spell duration 1996-2004 (weeks)	20.7	10.0	10.7***
average spell duration 2008	29.7	14.1	15.6***
Difference	-9.0***	-4.1***	*-4.9****
spells ending with job 1996-2004 (%)	64.6	65.8	-1.2
spells ending with job 2008	63.6	66.1	-2.5
Difference	1.0	-0.3	*1.3*
spells ending with non-participation 1996-2004 (%)	20.7	20.4	0.3
spells ending with non-participation 2008	21.8	20.1	1.7
Difference	-1.1	0.3	*-1.4*
censored spells 1996-2004 (%)	14.7	13.8	0.9
censored spells 2008	14.5	13.7	0.8
Difference	0.1	0.1	*0.1*

Source: SIPP 1996-2004 and SIPP 2008. Stars indicate significance levels for tests of mean mobility rates between renters and owners (row Difference), and between the 1996-2004 and the 2008 panels (column Δ): ***:p<0.01,**:p<0.05,*:p<0.1

Table 6: Cumulative Fraction of ”Land Use” Cases in 1999

State	Regulation Index
Alabama	0.0008
Louisiana	0.0010
Oklahoma	0.0012
Florida	0.0012
Texas	0.0013
New York	0.0015
Nevada	0.0016
Arkansas	0.0017
Georgia	0.0018
Mississippi	0.0018
Kansas	0.0020
Kentucky	0.0021
Tennessee	0.0021
North Carolina	0.0023
Illinois	0.0026
Iowa	0.0026
Nebraska	0.0027
Ohio	0.0028
Utah	0.0030
Indiana	0.0030
Missouri	0.0031
Pennsylvania	0.0031
West Virginia	0.0033
South Carolina	0.0034
Wisconsin	0.0038
New Mexico	0.0039
Michigan	0.0041
Delaware	0.0043
Arizona	0.0045
Montana	0.0046
Massachusetts	0.0050
Minnesota	0.0055
Virginia	0.0063
Idaho	0.0065
Colorado	0.0073
Maryland	0.0101
Rhode Island	0.0105
Washington	0.0105
California	0.0106
Oregon	0.0106
New Jersey	0.0110
Connecticut	0.0143
New Hampshire	0.0172

Table 7: Estimation results : Separate Regressions, SIPP Panels 1996, 2001 and 2004

	(Selection Equation)		(Job Finding)		(Nonparticipation)	
	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.
Regulation	-0.396***	(0.045)				
Owner			0.001	(0.020)	0.038	(0.035)
Black	-0.402***	(0.050)	-0.303***	(0.031)	0.050	(0.044)
Hispanic	-0.522***	(0.048)	-0.030	(0.027)	0.013	(0.049)
Married	0.128***	(0.042)	0.246***	(0.023)	-0.161***	(0.043)
Pre high school	-0.651***	(0.062)	-0.039	(0.034)	-0.031	(0.059)
High school	-0.200***	(0.051)	0.080***	(0.026)	-0.024	(0.049)
Age 18-24	-1.725***	(0.109)	0.371***	(0.054)	-0.377***	(0.084)
Age 25-29	-2.143***	(0.110)	0.422***	(0.054)	-0.498***	(0.088)
Age 30-39	-1.849***	(0.105)	0.342***	(0.051)	-0.507***	(0.080)
Age 40-49	-1.284***	(0.105)	0.274***	(0.050)	-0.573***	(0.079)
Age 50-59	-0.768***	(0.107)	0.121**	(0.051)	-0.403***	(0.078)
Has children	0.868***	(0.038)	-0.161***	(0.021)	0.052	(0.039)
Spouse unemployed	-0.324***	(0.102)	0.018	(0.054)	-0.318**	(0.131)
Spouse not in labor force	-0.201***	(0.057)	0.091***	(0.029)	-0.042	(0.062)
No unemployment benefits	-0.139***	(0.047)	0.144***	(0.024)	0.369***	(0.053)
No pre-spell earnings	0.302**	(0.143)	-0.514***	(0.079)	0.458***	(0.128)
Log of pre-spell earnings	0.015	(0.019)	0.028***	(0.010)	-0.043**	(0.018)
No pre-spell family income	3.061***	(0.173)	-0.111	(0.084)	0.047	(0.120)
Log of pre-spell family income	0.480***	(0.021)	0.008	(0.009)	-0.021	(0.014)
Urban	-0.280***	(0.041)	-0.011	(0.022)	-0.002	(0.040)
Panel 2001	0.048	(0.041)	-0.176***	(0.022)	0.084**	(0.042)
Panel 2004	0.002	(0.039)	-0.073***	(0.021)	0.246***	(0.038)
d1			-2.984***	(0.104)	-3.343***	(0.182)
d2			-3.444***	(0.104)	-4.071***	(0.184)
d3			-3.648***	(0.106)	-4.264***	(0.187)
d4			-2.920***	(0.109)	-2.061***	(0.183)
d5			-3.694***	(0.109)	-4.182***	(0.194)
d6			-3.927***	(0.108)	-3.758***	(0.186)
constant	-1.591***	(0.209)				
Obs.	18,966		18,966		18,966	

Source: SIPP 1996-2004. Standard errors in parentheses. Stars indicate significance levels: ***:p<0.01, **:p<0.05, *:p<0.1.

Table 8: Estimation results : Joint Regression, SIPP Panels 1996, 2001 and 2004

	(Selection Equation)		(Job Finding)		(Nonparticipation)	
	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.
Regulation	-0.545***	(0.059)				
Homeowner			0.108***	(0.033)	0.018	(0.054)
Black	-0.498***	(0.070)	-0.296***	(0.030)	0.058	(0.046)
Hispanic	-0.647***	(0.070)	-0.019	(0.027)	0.003***	(0.050)
Married	0.081	(0.054)	0.244***	(0.023)	-0.153	(0.044)
Pre high school	-0.638***	(0.082)	-0.029	(0.033)	-0.036	(0.061)
High school	-0.116*	(0.065)	0.082***	(0.026)	-0.025	(0.051)
Age 18-24	-2.014***	(0.138)	0.398***	(0.055)	-0.367***	(0.085)
Age 25-29	-2.505***	(0.142)	0.460***	(0.056)	-0.514***	(0.089)
Age 30-39	-2.060***	(0.134)	0.373***	(0.052)	-0.510***	(0.081)
Age 40-49	-1.384***	(0.133)	0.297***	(0.051)	-0.584***	(0.079)
Age 50-59	-0.818***	(0.134)	0.136***	(0.052)	-0.415***	(0.080)
Has children	0.939***	(0.052)	-0.176***	(0.021)	0.051	(0.040)
Spouse unemployed	-0.191	(0.137)	0.026	(0.054)	-0.337***	(0.123)
Spouse not in labor force	-0.088	(0.075)	0.095***	(0.028)	-0.048	(0.062)
No unemployment benefit	-0.184***	(0.059)	0.149***	(0.022)	0.374***	(0.053)
No pre-spell earnings	-0.593***	(0.207)	-0.509***	(0.077)	0.517***	(0.133)
Log of pre-spell earnings	-0.132***	(0.028)	0.029***	(0.010)	-0.041**	(0.018)
No pre-spell family income	7.864***	(0.396)	-0.208**	(0.088)	0.009	(0.145)
Log of pre-spell family income	1.140***	(0.049)	-0.007	(0.010)	-0.026	(0.018)
Urban	-0.423***	(0.052)	-0.005	(0.022)	0.003	(0.041)
ρ_{hj}	-0.983	[-1.000;-0.457]				
ρ_{hn}	0.050	[-0.695;0.134]				
ρ_{jn}	-0.110	[-0.260;-0.080]				
$P(h_0, j_0, n_0)$	76%	[54.1;78.9]				
$P(h_0, j_1, n_0)$	0%					
$P(h_0, j_0, n_1)$	4%	[2.1;8.5]				
$P(h_0, j_1, n_1)$	0%					
$P(h_1, j_0, n_0)$	0%					
$P(h_1, j_1, n_0)$	19%	[12.4;21.5]				
$P(h_1, j_0, n_1)$	1%	[0;28.5]				
$P(h_1, j_1, n_1)$	0%					
Obs.	18,966		18,966		18,966	

Source: SIPP 1996-2004. Standard errors (95% confidence intervals for the correlation coefficients and probabilities) in parentheses. Stars indicate significance levels: ***:p<0.01, **:p<0.05, *:p<0.1. Confidence intervals are based on the 2.5 and 97.5 percentiles of 1000 simulations with parameters drawn from a normal distribution with mean equal to the estimated coefficient vector and variance equal to the estimated variance-covariance matrix. All hazard functions are piece-wise constant. Dummies for the panels not reported. ρ_{ij} refers to the correlation between unobserved heterogeneity in i and j, where h refers to being a homeowner, j to the job-finding hazard, and n to the non-participation hazard.

Table 9: Estimation results : Effect of homeownership on job hazard

	Panels 1996-2004			
owner	0.010	0.001	-0.005	0.108*
	(0.018)	(0.020)	(0.020)	(0.059)
controls	no	yes	yes	yes
state fixed effects	no	no	yes	no
joint estimation	no	no	no	yes
	Panel 2008			
owner	0.010	0.014	0.008	0.122**
	(0.026)	(0.029)	(0.029)	(0.053)
controls	no	yes	yes	yes
state fixed effects	no	no	yes	no
joint estimation	no	no	no	yes
	Pooled Panels			
owner	0.010	-0.005	-0.011	0.117***
	(0.018)	(0.019)	(0.019)	(0.031)
D_{2008}	-0.257***	-0.258***	-0.262***	-0.259***
	(0.025)	(0.025)	(0.025)	(0.025)
$D_{2008}\times$ owner	-0.001	0.020	0.024	0.020
	(0.032)	(0.032)	(0.032)	(0.032)
controls	no	yes	yes	yes
state fixed effects	no	no	yes	no
joint estimation	no	no	no	yes

Standard errors in parentheses. Stars indicate significance levels: ***:p<0.01, **:p<0.05, *:p<0.1. All hazard functions are piece-wise constant. Controls are the same as in table 7. D_{2008} is a dummy variable for the 2008 panel.

Table 10: Estimation results : Effect of homeownership on non-participation hazard

	Panels 1996-2004			
owner	-0.015	0.038	0.049	0.018
	(0.032)	(0.050)	(0.036)	(0.054)
controls	no	yes	yes	yes
state fixed effects	no	no	yes	no
joint estimation	no	no	no	yes
	Panel 2008			
owner	0.043	0.066	0.045	-0.102
	(0.047)	(0.51)	(0.052)	(0.095)
controls	no	yes	yes	yes
state fixed effects	no	no	yes	no
joint estimation	no	no	no	yes
	Pooled Panels			
owner	-0.016	0.032	0.039	-0.160**
	(0.032)	(0.034)	(0.035)	(0.063)
D_{2008}	-0.355***	-0.314***	-0.324***	-0.315***
	(0.045)	(0.045)	(0.045)	(0.044)
$D_{2008}\times$ owner	0.061	0.037	0.036	0.039
	(0.057)	(0.057)	(0.057)	(0.056)
controls	no	yes	yes	yes
state fixed effects	no	no	yes	no
joint estimation	no	no	no	yes

Standard errors in parentheses. Stars indicate significance levels: ***:p<0.01, **:p<0.05, *:p<0.1. All hazard functions are piece-wise constant. Controls are the same as in table 7. D_{2008} is a dummy variable for the 2008 panel.

Table 11: The effect of ownership on the Job Hazard (single risk) for subsamples

	owner coefficient	standard error
age 18-24	0.020	(0.039)
age 25-29	-0.111**	(0.046)
age 30-39	0.024	(0.032)
age 40-49	-0.006	(0.034)
age 50-59	0.077*	(0.043)
age 60-65	0.137	(0.103)
married	0.100***	(0.024)
not married	-0.057**	(0.023)
less than high school	0.025	(0.036)
high school	0.007	(0.020)
above high school	-0.063	(0.045)
high family income	0.024	(0.025)
low family income	-0.067***	(0.023)
non-participation	0.089	(0.068)

Source: SIPP 1996-2008. Standard errors in parentheses. Stars indicate significance levels: ***:p<0.01, **:p<0.05, *:p<0.1. Regressions include the same regressors as in table 7. High (low) family income corresponds to above (below) the median income. Non-participation is identified as having no pre-unemployment earnings.

Table 12: The effect of ownership on the Non-Participation Hazard (single risk) for subsamples

	owner coefficient	standard error
age 19-24	-0.021	(0.058)
age 25-29	0.157*	(0.090)
age 30-39	0.045	(0.066)
age 40-49	0.101	(0.067)
age 50-59	-0.017	(0.070)
age 60-65	-0.031	(0.126)
married	0.006	(0.050)
not married	0.052	(0.036)
less than high school	-0.050	(0.059)
high school	0.083**	(0.036)
above high school	0.015	(0.086)
high family income	-0.083*	(0.048)
low family income	0.102***	(0.037)
non-participation	0.077	(0.057)

Source: SIPP 1996-2008. Standard errors in parentheses. Stars indicate significance levels: ***:p<0.01, **:p<0.05, *:p<0.1. Regressions include the same regressors as in table 7. High (low) family income corresponds to above (below) the median income. Non-participation is identified as having no pre-unemployment earnings.

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Appendix - Data description

The SIPP records data for three different time periods. The labor force status is recorded weekly. Other variables are recorded for the entire month. For example a person's earned income refers to the month, irrespective of how many weeks in the month the person worked. Finally, some variables are recorded for a survey wave (four months). For example the industry in which a person works is extended to the entire wave, even if the person has been unemployed or not in the labor force for most of that period.

The final sample are (completed or right-censored) unemployment spells of men who 1) are at least 18 and at most 65 years of age, 2) are present at the first wave of the survey (thus excluding individuals who move into a survey household after the first wave), 3) do not have any gaps in the data (for example individuals who temporarily live abroad), and 4) have information for at least three months. We further exclude spells of men who are in the or whose last job was with the armed forces, who live in Alaska or Hawaii, or who live in dormitories, subsidized or public housing, or in mobile or temporary homes. Residents of Vermont, Wyoming, North Dakota, South Dakota, and Maine are also excluded because those states cannot be identified separately in all the SIPP waves and the instrument employed in section 4 requires knowledge of the state of residence. We also exclude unemployment spells which are preceded by school enrollment, which change their education classification during the spell, and observations which are in the data for less than 15 weeks. Finally we drop some observations with (very likely) inconsistent data, e.g. observations for whom gender or race information changes, or who report unemployment for an entire wave but are reported to have a job or a business.

There are two types of entry into unemployment. An unemployment spell from a *job*-to-unemployment transition begins if an observation was employed in the previous week, and reports looking for work this week, or reports not looking for work for up to three weeks after employment followed by reporting to look for work. For example, if somebody has a job in week 1, reports not working and not looking for a job in weeks 2 and 3, and looks for work in week 4, he is coded as an unemployment spell entering from a job and beginning at the start of week 2. A *non-participation*-to-unemployment transition is unemployment preceded by at least four weeks of non-participation. For example, somebody who reports not having a job and not looking for one for weeks 1 to 4, and reports looking for work in week 5, is recorded as starting an unemployment spell at the beginning of week 5. An unemployment spell can terminate in either work or non-participation. We code termination in work only if the employment spell lasts at least four weeks, and termination in non-participation only if the observation does not look for work for four consecutive weeks. For example, if an observation is in an unemployment spell, works for two weeks, then is unemployed again, we treat this as an uninterrupted unemployment spell.

We take the land use regulation index proposed by Ganong and Shoag (2013) as an instrumental variable for homeownership. This is a time varying variable at the state level with an annual frequency. For each SIPP panel we merge the state level index value at the start year of the panel and keep it constant over the course of the panel.

We deflate the nominal variables (eg. unemployment benefits, income etc.) with the CPI reported by the Bureau of Labor Statistics.

Other variables used in the paper are:

- the log of last earned monthly income observed before the beginning of an unemployment spell,
- a dummy indicating no information on previous earnings (an unemployment spelling entering from non-participation and without prior job information in the SIPP),
- the log of family income at the beginning of the spell,
- a dummy indicating no family income at the beginning of the spell,
- dummies for blacks, and hispanics,
- dummies for men without high school, and with high school but no college degrees,
- dummies for age categories 18 to 24, 25 to 29, 30 to 39, 40 to 49, 50 to 59,
- a dummy for married men,
- a dummy indicating whether the spouse is unemployed,
- a dummy indicating whether the spouse is not participating in the labor force,
- a dummy indicating the presence of children in the household,
- a dummy indicating the receipt of unemployment benefits,

- a dummy indicating whether the household lives in an urban location,

- dummies indicating the SIPP panels,