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Abstract

We examine the effects of homeownership on individuals' unemployment durations in the USA. We take into account that an unemployment spell can terminate with a job or with a non-participation transition. 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. We find that unemployed homeowners are less likely to find a job than renters. The effect is small but statistically significant for most specifications. The effect is stronger for outright owners and weaker for mortgage holders. We also find that outright owners have a higher and mortgage holders a lower probability of exiting to non-participation than renters.

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

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[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.

(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, he argues, they will not be capable or willing to move for a job outside of their current locality. He supports this argument 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.

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 four 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 allow for non-participation as an alternative exit out of unemployment considering the possibility that some individuals may choose exiting the labor force over employment. If housing tenure plays a role in that decision, 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. Fourth, as an extension we analyze differences in job-finding and non-participation probabilities between outright owners and owners with a mortgage.

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. 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. Our analysis on mortgage status yields results similar to those suggested by Flatau et al. : that mortgage holders find employment faster than outright owners. 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. 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. – in our opinion the most credible attempt to control for ownership endogeneity – but stands out from the aforementioned papers in that we find a sizeable *positive* effect of being a homeowner on unemployment duration. Being

[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.

a homeowner increases unemployment durations by 10% or, with the average duration being 10 weeks, by one week. The magnitude of the effect is reduced to insignificance in the FIML estimation, but appears to be even stronger in our extended model in which we include the mortgage status of the owner. However we do not attempt to control for endogeneity in this extended model due to the presence of two endogenous variables: ownership and mortgage status. We observe that outright owners exhibit lower probabilities of job finding compared to mortgage holders although both types have lower job finding probabilities compared to renters. Exit to nonparticipation does not produce stable results for homeowners overall, however we find significant differences by mortgage status: mortgage holders are less likely to exit the labor force compared to renters and outright owners are more likely to drop out of the labor force.

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]

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.[5] 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 (see van Vuuren and van Leuvensteijn (2007), Güler and Taşkın (2011), Morescalchi (2011).) 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 unemployment 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

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

[5]Dohmen (2005) and Munch, Rosholm and Svarer (2006), Coulson and Fisher (2008), van Vuuren and van Leuvensteijn (2007), Guler and Taskin (2011), Morescalchi (2011).

implies longer unemployment durations.

Some papers have looked at the quantitative implications of homeownership in structural models calibrated to mobility costs and allowing for multiple locations. Head and Ellis (2008) find that the aggregate effect of ownership on unemployment, under a plausible parametrization of the US economy, is quantitatively small unless the unemployment rate is high. Karahan and Rhee (2012) and Penov (2012) include liquidity constraints for homeowners and quantify the effect of the housing market on the recent unemployment spike. They both find a significant but small contribution of the housing bust to high unemployment levels experienced during the great recession.

3 Data

We use the Survey of Income and Program Participation (SIPP). We report results from the 1996, 2001, 2004, and (incomplete) 2008 panels. The SIPP follows around 40,000 households and close to 100,000 individuals over three to four years (depending on the panel) and surveys them every four months, creating 9 to 12 *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 the following restrictions to obtain our final sample of unemployment spells: 1) We drop observations residing in Alaska, Hawaii (since the house-lock theory does not apply to these states); we also drop residents in Maine, Vermont, South Dakota, North Dakota and Wyoming because the 1996 and 2001 panels do not differentiate those states separately, 2) we drop observations who were not in the initial wave (since those individuals are not followed upon leaving the household), 3) we drop observations who have gaps in the data (temporary drop-outs), 4) we drop observations in the armed forces, 5) we drop observations in dormitories, trailers, motels, and other mobile or temporary living quarters, 6) we drop observations in public and/or subsidized housing, 7) we drop observations who are in the data for less than 15 weeks, 8) we drop spells who enter unemployment from being in school, and 9) we drop spells which change their education classification during a spell.

All these exclusions leave us with 16,969 individuals and 32,027 unemployment spells of which

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

	Full Sample	Owners		Renters	
	Obs.	**Obs.**	**Pct.**	**Obs.**	**Pct.**
individuals	16,969				
spells	32,027	18,763	100	13,264	100
censored spells	3,045	1,561	8	1,484	11
spells ending with job	23,322	13,818	74	9,504	72
spells ending with nonparticipation	5,660	3,384	18	2,276	17
unemployment duration	10.56	10.88		10.12	

Source: SIPP 1996-2008.

23,322 end in transitions to work, 5,660 in transitions to non-participation, and 3,045 are right-censored (the panel ends before we can observe the transition out of unemployment). On average we observe an unemployment duration of 10.56 weeks. Table 1 describes the corresponding spell statistics separately for those who are owners and who are renters at the beginning of the spell. Here we see only slight differences between owners and renters: a slightly bigger share of owners end up finding a job or exiting the labor force, however the average unemployment duration of a homeowner is 0.66 weeks greater than that of renter's. The similarity of the unemployment experiences between the two groups is further illustrated in Figure 1 and Figure 2 which plot the Kaplan-Meier survival functions for owners and renters with exits to employment and non participation. From the survival figures we observe that exit rate of owners and renters are virtually inseparable, a consistent fact regardless of the exit being a job or non-participation.

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 Supplement between years 1998 and 2012, 30 percent among those who change residence within a year list job related reasons as primary motive of the move. Presumably among the unemployed this share is considerably bigger, it is a well known fact

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

Table 2: Mobility statistics, renters and owners

Unemployment Spells	Full Sample		Stayers		Movers	
	Obs.	**Pct.**	**Obs.**	**Pct.**	**Obs.**	**Pct.**
individual spells	**32,027**	**100**	**30,640**	**100**	**1,387**	**100**
censored spells	3,045	9.5	2,872	9.4	173	12.5
spells ending with job	23,322	72.8	22,355	73.0	967	69.7
spells ending with nonparticipation	5,660	17.7	5,413	17.7	247	17.8
unemployment duration	10.56		10.17		9.31	
owner spells	**18,763**	**100**	**18,429**	**100**	**334**	**100**
censored spells	1,561	8.3	1,521	8.3	36	10.8
spells ending with job	13,818	73.6	13,584	73.7	234	70.1
spells ending with nonparticipation	3,384	18.0	3,324	18.0	60	18.0
unemployment duration	10.88		10.70		9.77	
renter spells	**13,264**	**100**	**12,211**	**100**	**1,053**	**100**
censored spells	1,484	11.2	1,351	11.1	133	12.6
spells ending with job	9,504	71.7	8,771	71.8	733	69.6
spells ending with nonparticipation	2,276	17.2	2,089	17.1	187	17.8
unemployment duration	10.12		9.37		9.16	

Source: SIPP 1996-2008.

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 spell for the reports here and henceforth we always consider the first move.[7]

Table 2 describes mobility and unemployment characteristics of individuals separately for owners and renters. Within approximately 10 months weeks of an unemployment spell 4.33% of individuals report an address change. Compared to people that stayed in the same housing unit within the course of a spell, movers have relatively inferior labor market outcomes. 70% of movers find a job after the move, whereas that share is 74% for stayers. Moreover mean unemployment duration after the move is very close to the average unemployment duration of a stayer.[8] That means movers on average spend roughly the same time to find a job after the move compared to the total time spent by stayers.

When we compare mover statistics for owners and renters we have considerable differences in the outcomes. Individuals who are homeowners at the beginning of the spell move 1.78% of the time whereas 7.94% of the renters move within the course of a spell. Thus the nonlocal job finding probability is almost 5 times bigger for renters. Although owners find local jobs quite often, it takes them on average 1.33 weeks longer than renters to enter a job. This relationship is preserved for nonlocal jobs as well. Note that the overall unemployment durations of owners and renters are closer to each other because renters move more often than owners and movers have longer unemployment spells.

Finally Table 3 reports descriptive statistics for a subset of the covariates we use in our analysis, separately for those who are owners and who are renters at the beginning of the spell. Renters are more likely to be black or hispanic, are somewhat less educated, younger, less likely to be married and less likely to have kids. Living in a metropolitan area is more common for renters. Unsurprisingly owners and renters have drastic differences in terms of income: renters had considerably lower paying jobs before unemployment and have lower family income. Furthermore owners are

[7] Out of 1387 spells that experience an address change we have 57 multiple moves of which 55 move twice.

[8] Total unemployment duration of a mover is on average 19.29 weeks, that number is 20.83 weeks for owners and 18.8 week for renters. One may also think of this in the reverse direction: as the unemployment spell is prolonged the likelihood of moving to another place increases.

Table 3: Summary statistics of covariates at the beginning of spell, renters and owners

	Renters	Owners
Black (percent)	14.67	9.77
Hispanic (percent)	23.02	11.50
Married (percent)	39.57	54.90
Kids (percent)	47.90	63.79
Less than high school (percent)	26.73	14.49
High school (percent)	62.52	67.39
College (percent)	10.75	18.12
Age (mean)	34.85	40.27
Deflated monthly pre-spell earnings (mean)	1357	2000
Unemployment benefits (percent)	14.74	20.91
Metro area (percent)	81.35	76.21

Source: SIPP 1996-2008.

more likely to receive unemployment benefits. This income heterogeneity will play an important role in the next section.

4 Estimation

We have seen that the raw data do not display much difference in unemployment experiences between owners and renters. However we also see that the characteristics of homeowners and renters are quite different. 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

$$\theta_e(t|x_t, z_t) = \lambda_e(t)\exp(\beta_e' x_t + \gamma z_t) \quad (1)$$

$$\ln L = \sum_m d_{em} \ln \theta_e(t|x_{tm}, z_{tm}) - \int_0^t \theta_e(s|x_{tm}, z_{tm})\mathrm{d}s \quad (2)$$

Here e denotes the exit-state of interest (job j, non-participation n), m denotes the spell, x_t denote the vector of covariates, which we restrict to be time invariant 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 (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), five age categories (18-24, 25-29, 30-39, 40-49, 50 and older), the log amount of last observed real earnings before unemployment, a dummy for positive earnings before unemployment, dummies for the employment status of the partner (if applicable), a dummy for the presence of children below the age of 18 in the family, a rank value for total family income[9], a dummy for receipt of unemployment benefits, the log of the amount of unemployment benefits, indicators for positive property and transfer income for the family, a dummy that indicates whether the household lives in a metropolitan area, and dummies for each of the four SIPP panels.

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

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

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

[9]We take the real family income and create deciles to be used in the estimation. For computational purposes we treat the decile bins as a continuous variable. This characterization gives us better likelihood value than using just the log income. We do not separately control for the "no family income" case, instead we argue that this could be captured by the two variable characterization of pre-spell earnings.

(see Honoré, 1993). Since our data only covers 3-4 years, the incidence of ownership switching across spells is highly unlikely. We therefore include an instrumental variable that will correct for the selection problem.

The dominant exclusion restriction used in the literature is the homeownership rate in the state of residence.[10] 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.[11] We propose an exclusion restriction which is pre-determined to labor market conditions but a likely determinant of housing supply and its short-term elasticity and which is determined independently of the recent housing choices of the residents. 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.[12] It is strongly correlated with widely used previous surveys that are constructed to capture the relative stringency of residential growth controls such as the Wharton Land Use Regulation Index by Gyourko, Saez and Summers (2007), but has the advantage of exhibiting annual time variation. For our pooled data we take the land use regulation value a year before the inception year of each SIPP panel. The novel 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. We will revisit the success of this variable in the results section.

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).[13] All three components are assumed to have a discrete distribution with two points of support, 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 homeownership 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

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

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

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

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

the following contribution of a spell to the likelihood function:

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

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. In order to reduce the computing time we don't use the time varying nature of our covariates. Since a big portion of the spells terminate within a month we argue that this does not affect the main conclusion.[14]

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 four SIPP panels (1996, 2001, 2004, 2008) and report results for the overall sample.[15] For expositional simplicity we provide detailed results for the separate regressions then proceed to the FIML estimation.

The first two columns in Table 4 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 are more likely to be homeowners than black or hispanic individuals. The coefficient for being married is not significant, however 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.

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

[15] We repeat the same exercise separately for each panels which qualitatively yield similar results but with varying magnitude and significance.

For the job finding hazard we find a significant negative effect of being an owner meaning that it is harder for unemployed homeowners to find a job. The first two rows in Table 4 list the coefficient of homeownership with the corresponding standard error. We find that although we do not observe a different unemployment hazard pattern in nonparametric survival curves, once we control for the observed factors that could potentially affect unemployment duration, we obtain quite different results. For the single risk regression the coefficient on homeownership is -0.102 indicating that unemployed homeowner's job finding hazard is approximately 10 percent lower ($exp(-0.102) - 1$) than that of renters.[16] The main reason behind that result is the heterogeneity of income between owners and renters. When we remove the income controls from regression (i.e. pre-spell earnings, family income, transfer and property income indicators for the family) the coefficient on homeowner becomes virtually zero. We argue that one potential reason behind lack of evidence on so-called Oswald hypothesis could be due to omitted variables along the income margin.[17]

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 low income[18] individuals have lower rates of finding a job, age has a non-monotonic effect on the hazard rate with the youngest and the oldest age groups having the lowest job finding probability. Education also follows a non-monotonic pattern, individuals who do not have a college diploma but finished high school have the highest job finding rate. For married households, the wife being employed reduces the job hazard compared to her not being in the labor force or being unemployed, possibly due to the fact that the wife's income provides sufficient insurance to keep the reservation wage higher than in the absence of other earned income. Interestingly having children decreases the job hazard. Living in a metropolitan area also reduces the job finding. Finally for the 2001 and 2008 panels the job finding probability is significantly lower possibly due to business cycle conditions in those years.

The non-participation hazard does not exhibit the same stability over time as the job hazard. The piecewise component for the 4^{th} month is strikingly high compared to the other time intervals, most likely reflecting the seam bias in the SIPP. There is no monotonic duration de-

[16] Using the approximation $log(duration) \simeq -log(hazard)$ an owner's unemployment duration is 10 percent higher. Given an average unemployment duration of ten weeks this translates into an additional week of unemployment.

[17] We also run separate regressions on selected subgroups and obtain that the negative effect of homeownership on unemployment duration derives mainly from young, single and educated (especially college graduates) individuals with relatively low income. The results are available upon request.

[18] For the individuals who have positive earnings before unemployment the income is negatively correlated with job finding.

pendence, instead it exhibits a hump shape where the seam bias is the turning point. The effect of ownership on the non-participation hazard is virtually zero. Repeating this regression with different subgroups or time periods yields mostly insignificant or unstable results. However, as we will see in the next section, this relationship appears to have significant features when we include mortgage status. Apart from homeownership, we find that married and middle age individuals have significantly lower hazards, and having higher wages before unemployment lowers the hazard. On the other hand having a higher family income increases 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 Table 2 and first two columns in Table 4). On the other hand, individuals who value flexibility and wish to remain so to be able to take on jobs elsewhere might prefer to rent. This flexibility might also improve their job-finding skills. In this case the owner coefficient would be biased downward. This latter 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. This shows that mobility tendencies could also play a role for housing decisions of individuals. 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 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 4 (first two rows) confirms that land use regulation is negatively correlated with homeownership probability with a highly significant coefficient. Moreover, in principle, we could exploit the time varying nature of the land use regulation index proposed by Ganong and Shoag (2013) to capture the state specific changes in the housing market over the course of time. Unfortunately our instrument does not produce consistent results net of fixed state factors because there is too little within-state variation in the index. Although we still use the time varying nature of the land use regulation we don't include state fixed effects in our estimation.

Results for the joint estimation are reported in Table 6. The parameters in the joint estimation have very similar effects compared to the separate regressions. The negative effect of homeownership mildly drops down suggesting that there is a slight negative correlation between unobservables in the selection equation and the job finding hazard. Similarly the coefficient of homeownership on nonparticipation hazard becomes negative (although still insignificant) suggesting a positive correlation for the latter hazard with selection. These results suggest that unobserved characteristics that favor renting are on balance more 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 -0.20. 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. 86% of the probability mass is concentrated on one of the eight possible types, and the 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 thus reducing the variation that would and could be explained by unobserved heterogeneity.

In the joint estimation we don't include state fixed effects for two reasons: First we do not have a time varying instrument that successfully captures the selection nature of homeownership, second even in the existence of a time varying instrument the estimation is practically infeasible because of the high dimensionality. However we argue that this choice does not alter the results as far as the coefficient of interest is concerned. In tables 8 and 9 (columns 1 and 2) we provide estimates of homeownership using the pooled SIPP panels and we repeat separate job hazard and non-participation hazard regressions with and without state fixed effects. The results suggest that adding state fixed effect to the regression virtually leaves the owner coefficient unchanged for both the job finding and the non-participation hazards, giving us reason to believe that the homeowner-effect on unemployment duration is not related to any state-wide effects.

6 The Effect of Mortgage Status

One argument left unanswered is whether the mortgage and equity status on the house plays a role in job finding and exiting the labor force probabilities of the households. Although for homeowners there are certain costs associated with moving to another area, mortgaged households have also

Table 4: Summary statistics of spell characteristics, mortgage holders and outright owners

	Owners	Mortgaged Owners		Outright Owners	
	Obs.	**Obs.**	**Pct.**	**Obs.**	**Pct.**
spells	18,763	13,965	100	4,798	100
censored spells	1,561	1,153	8	408	8
spells ending with job	13,818	10,577	76	3,241	68
spells ending with nonparticipation	3,384	2,235	16	1,149	24
unemployment duration	10.88	10.81		11.08	

Source: SIPP 1996-2008.

incentives to find jobs to keep honoring mortgage debt as argued by Flatau et al (2003). For that reason we obtain households' mortgage status information reported in topical modules of the SIPP panels[19] and identify whether the household is an outright owner or holds a mortgage. Table 4 describes the features of unemployment spells among homeowners by mortgage status.

Among homeowners around 75% of the households report that they hold at least one mortgage on the house that they live in. 76% of unemployment spells with a mortgage end up finding a job whereas this number is only 68% for outright homeowners. We see the mirror image of these numbers for the non-participation hazard by having an 8% censored spell share for each homeowner type. On average unemployment duration of outright owners is slightly greater than that of owners with mortgages.[20]

We repeat the separate regressions for job finding and exiting the labor force hazards by including mortgage holder status as an additional explanatory variable. We do not attempt to correct for endogeneity in this section, since this would require exclusion restrictions for both the ownership and the mortgage status. Columns 3 and 4 in tables 8 and 9 report the estimates of ownership and mortgage status with and without state fixed effects for both hazard types. The regression results directly follow the summary statistics in the sense that mortgage holding increases the likelihood of finding a job and reduces the likelihood of exiting the labor force. Unlike Flatau et al (2003) we find that even a mortgage holder experiences a longer unemployment duration compared to

[19]Individuals report their mortgage status in the 3rd, 6th, 9th and 12th waves. Since we have weekly data for unemployment spells and monthly data for other variables we fill missing mortgage information using a methodology provided in the Appendix.

[20]At the mobility margin we see that outright homeowners and homeowners with mortgage have almost identical address change rates (i.e. 1.79 for mortgage holders and 1.75 for outright owners) within a spell. For that reason we do not pursue any explanation related to mobility differences.

a renter in order to find a job. However for the non-participation hazard the results are even more striking: Mortgage holders are less likely to exit the labor force compared to renters and outright owners are more likely to switch to nonparticipation compared to renters. We argue that the insignificant relationship in the previous section is due to the fact that leverage on the house offsets the effect of ownership on non-participation.

In order to test whether liquidity or indebtedness conditions affect households' job finding and exiting the labor force probabilities we use home equity, mortgage debt and wealth information provided in the topical modules of the SIPP. The non-response rate for these variables are relatively higher, therefore we use a subsample of the households for which we have reliable information.[21] We argue that the difference in the outcomes between outright owners and mortgage holders reflects mainly differences in the share of the outstanding mortgage debt on the property value. For that we characterize a variable, equity share, that reflects the portion of the house owned by the household.[22] Since we are particularly interested in the effect of equity share within homeowners we restrict our attention to homeowners only.

Tables 10 and 11 report the estimates of mortgage status and home equity share with state fixed effects for both hazard types. For job finding we still find that mortgage status increases the likelihood, however the effect is significantly reduced as the household owns a larger portion of the house. To put it in numbers, the job finding probability of a mortgage holder who is close to 100 percent housing equity and an otherwise identical outright owner only differs by around 2 percent ($exp(0.168 - 0.146) - 1$). This difference strengthens as we condition on information on home equity and wealth levels. For exiting the labor force, the results support the aforementioned hypothesis: Although mortgage holders have a lower likelihood of exiting the labor force, this is to a large extent due to the relative indebtedness of the household. When we control for housing equity and wealth levels we have quite similar outcomes in non-participation rates for the mortgage holder who has a high equity share and an otherwise identical outright homeowner. We repeat the same regressions with different sample groups (i.e. different age, education groups etc.) and obtain qualitatively similar results. We conclude that relative outstanding debt on mortgage is a robust predictor for job finding and non-participation outcomes since it creates incentives for finding a job. We caution against a strictly causal interpretation of our results since we do not correct for endogeneity of any of our variables.

[21] As for the mortgage status we describe the merging methodology in the Appendix.

[22] In particular we add housing equity and mortgage debt which roughly reflects the property value, then we divide home equity value on this constructed property value. We force this variable to take values between 0 and 1, for that reason we assign 0 for the negative home equity households. Similarly outright owners also have a value of 0 for this variable.

7 Conclusion

This paper investigates the effect of being a homeowner on the duration of unemployment spells of unemployed individuals using micro data covering 15 years in the USA. The question is economically relevant due to the fact that housing is heavily subsidized and makes people less mobile 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 2011 by FIML estimation accounting for unobserved heterogeneity and selection into homeownership we conclude that ownership does reduce the job finding hazard. This result is stable over different panels and controlling for possible endogeneity does not change the qualitative conclusion. This result confirms the so called Oswald hypothesis for the US which is in contrast to the opposite conclusions obtained in the literature. However we would like to emphasize that the effect is much smaller than the one found by Oswald.

We also include household mortgage status in order to capture the leverage effects on unemployment duration and observe that mortgage holders experience significantly higher job finding rates compared to an outright owner. However renters have the highest likelihood of finding a job. We find a non-trivial effect of ownership on the non-participation hazard: a mortgage holder exits the labor force less often compared to a renter, however an outright owner is more likely to exit the labor force. If we fail to control for mortgage status, the two opposing effects result in an insignificant coefficient for homewownership.

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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. Information on mortgages and home equity is collected only in waves 3, 6, 9, and - in the 1996 and 2004 surveys - wave 12.

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 subsidized or public housing, or in mobile 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. 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.

Since the mortgage and home equity information was collected only in waves 3, 6, 9, and 12, we imputed missing mortgage information in the following way: First, the mortgage information was carried forward to replace missing values if the ownership status did not change. For example, if the household owned their dwelling during waves three, four, and five, and reported to hold a mortgage in wave three, then we assumed that the dwelling is also mortgaged during wave four and five. We then backward filled mortgage data. For example, if holding a mortgage was reported in wave 12, then we assumed that a mortgage was also held in wave 11 and wave 10 if the household owned a home then (note that we could not have forward filled mortgage information if the household only acquired a home in wave 10). This procedure left only very few observations for which we could not determine mortgage status. For example, if a household was a homeowner in waves one and two, but a renter in wave three, we cannot know whether their house in waves one and two was mortgaged.

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 last earned monthly income amount 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),
- a dummy indicating positive property income for the family,
- a dummy indicating positive transfer income for the family,
- the income decile of the family as of first month of unemployment (for the unemployment spells that end within the same month we use the previous month's income),
- 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,
- a dummy for married men,
- a dummy indicating whether the spouse is working,

- 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,
- the amount of unemployment benefits received,
- a dummy indicating whether the household lives in an urban location,
- dummies indicating the SIPP surveys,

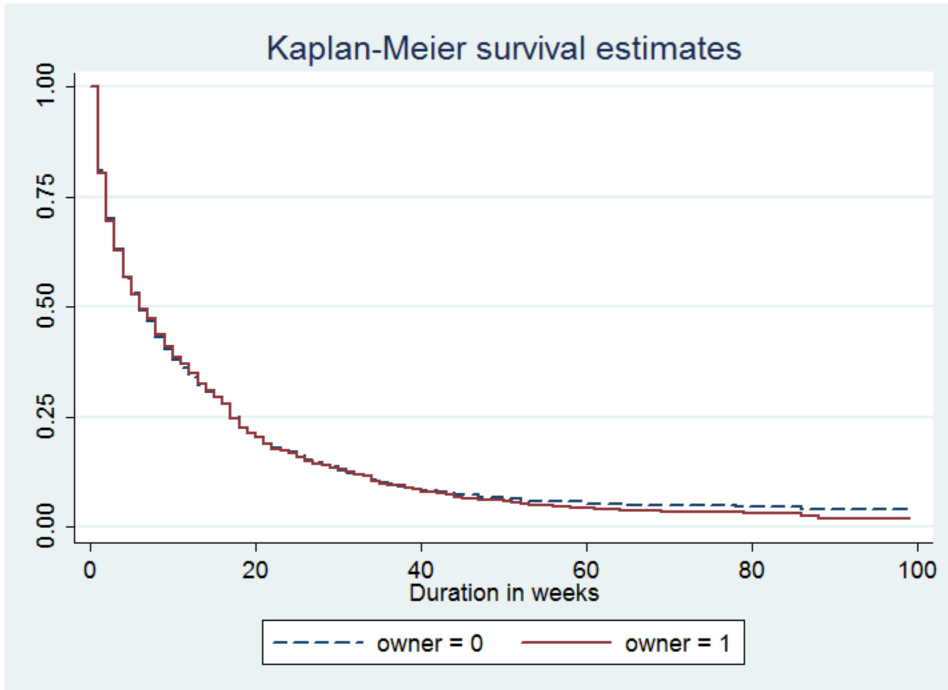

Figure 1: Kaplan-Meier Survival Functions: Exit to employment

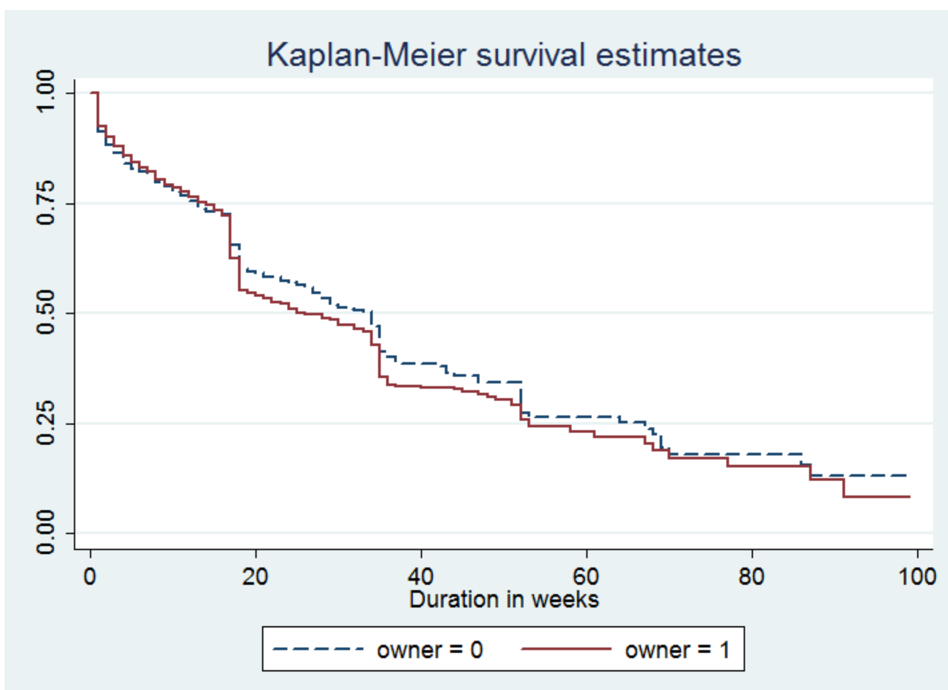

Figure 2: Kaplan-Meier Survival Functions: Exit to non-participation

Table 5: Estimation results : Separate Regressions, SIPP Panels 1996, 2001, 2004 and 2008

	(Selection Equation)		(Job Finding)		(Nonparticipation)	
	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.
regulation	-0.488	(0.035)				
owner			-0.102	(0.015)	0.008	(0.030)
black	-0.274	(0.041)	-0.257	(0.023)	0.051	(0.037)
hispanic	-0.398	(0.038)	0.042	(0.020)	-0.006	(0.041)
married	-0.0048	(0.034)	0.115	(0.017)	-0.183	(0.036)
pre high school	-0.353	(0.050)	0.032	(0.025)	-0.008	(0.051)
high school	-0.061	(0.041)	0.101	(0.020)	-0.024	(0.041)
age 19-24	-1.185	(0.049)	0.025	(0.025)	-0.010	(0.046)
age 25-29	-1.476	(0.050)	0.180	(0.025)	-0.179	(0.052)
age 30-39	-1.204	(0.042)	0.187	(0.021)	-0.194	(0.049)
age 40-49	-0.607	(0.042)	0.141	(0.020)	-0.219	(0.041)
kids	0.707	(0.030)	-0.205	(0.015)	-0.037	(0.032)
spouse unemp.	-0.038	(0.079)	0.118	(0.039)	-0.183	(0.010)
spouse nolab.	-0.045	(0.045)	0.276	(0.021)	0.084	(0.052)
no UI	0.069	(0.263)	-1.294	(0.115)	0.632	(0.319)
UI amount	0.020	(0.041)	-0.240	(0.018)	0.025	(0.049)
no earnings	0.213	(0.103)	-1.363	(0.051)	0.373	(0.103)
earnings	0.0012	(0.014)	-0.092	(0.006)	-0.058	(0.014)
family income	0.222	(0.006)	0.059	(0.003)	0.023	(0.006)
property	0.737	(0.028)	0.060	(0.015)	0.009	(0.031)
transfer	-0.371	(0.050)	-0.273	(0.029)	0.118	(0.044)
metro	-0.338	(0.033)	-0.074	(0.016)	0.005	(0.034)
panel 2001	0.053	(0.040)	-0.187	(0.020)	0.080	(0.041)
panel 2004	-0.050	(0.037)	-0.084	(0.018)	0.266	(0.038)
panel 2008	0.034	(0.035)	-0.281	(0.018)	-0.271	(0.039)
d1			-0.218	(0.126)	-4.100	(0.339)
d2			-1.078	(0.126)	-4.734	(0.340)
d3			-1.236	(0.127)	-4.871	(0.341)
d4			-0.503	(0.129)	-2.723	(0.340)
d5			-1.377	(0.129)	-4.768	(0.343)
d6			-1.653	(0.128)	-4.465	(0.341)
constant	-0.083	(0.283)				
Obs.	32027		32027		32027	

Standard errors in parentheses.

Table 6: Estimation results : The effect of Ownership on Selected Subgroups

	coef.	std. err.
black	-0.103	(0.046)
hispanic	-0.138	(0.036)
white	-0.095	(0.018)
married	-0.010	(0.022)
nokids	-0.142	(0.023)
single	-0.156	(0.021)
pre high school	-0.072	(0.033)
high school	-0.104	(0.019)
college	-0.155	(0.043)
age 18-24	-0.084	(0.037)
age 25-29	-0.122	(0.043)
age 30-39	-0.144	(0.030)
age 40-49	-0.088	(0.031)
age 50-65	0.009	0.037
high income	0.002	(0.032)
low income	-0.132	(0.027)
no property inc.	-0.124	(0.021)
from employment	-0.080	(0.016)
from nonparticipation	-0.091	(0.042)

High (low) income represents spells with top (bottom) 30% of family income.

Table 7: Estimation results : Joint Estimation, SIPP Panels 1996, 2001, 2004 and 2008

	(Selection Equation)		(Job Finding)		(Nonparticipation)	
	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.
regulation	-0.554	(0.047)				
owner			-0.057	(0.063)	-0.123	(0.094)
black	-0.328	(0.053)	-0.262	(0.024)	0.052	(0.039)
hispanic	-0.476	(0.063)	0.059	(0.025)	-0.025	(0.047)
married	0.023	(0.040)	0.112	(0.018)	-0.202	(0.038)
pre high school	-0.398	(0.062)	0.038	(0.027)	-0.036	(0.053)
high school	-0.061	(0.045)	0.108	(0.021)	-0.049	(0.046)
age 19-24	-1.284	(0.063)	0.007	(0.028)	-0.051	(0.049)
age 25-29	-1.659	(0.089)	0.189	(0.030)	-0.222	(0.056)
age 30-39	-1.330	(0.072)	0.197	(0.025)	-0.235	(0.048)
age 40-49	-0.658	(0.053)	0.150	(0.023)	-0.255	(0.048)
kids	0.799	(0.046)	-0.240	(0.024)	0.005	(0.055)
spouse unemp.	0.001	(0.087)	0.113	(0.039)	-0.180	(0.097)
spouse nolab.	-0.053	(0.051)	0.298	(0.024)	0.078	(0.059)
UI amount	0.029	(0.047)	-0.286	(0.032)	0.014	(0.073)
no UI	0.118	(0.298)	-1.559	(0.201)	0.558	(0.465)
earnings	-0.006	(0.016)	-0.107	(0.009)	-0.054	(0.023)
no earnings	0.189	(0.118)	-1.521	(0.090)	0.434	(0.241)
metro	-0.376	(0.041)	-0.066	(0.0171)	-0.003	(0.035)
ρ_{hj}	-0.200					
ρ_{hn}	0.500					
ρ_{jn}	-0.547					
$P(h_0, j_0, n_0)$	0.859	(0.225)				
$P(h_0, j_1, n_0)$	0.022	(0.210)				
$P(h_0, j_0, n_1)$	0.000	(0.235)				
$P(h_0, j_1, n_1)$	0.008	(0.227)				
$P(h_1, j_0, n_0)$	0.073	(0.186)				
$P(h_1, j_1, n_0)$	0.000	(0.192)				
$P(h_1, j_0, n_1)$	0.019	(0.212)				
$P(h_1, j_1, n_1)$	0.019	(0.030)				

Standard errors in parentheses. 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 8: Estimation results : The effect of Ownership on Job Hazard

	(1)	(2)	(3)	(4)
owner	-.102	-.109	-.177	-.186
	(.015)	(.015)	(.022)	(.022)
mortgage			.100	.103
			(.021)	(.021)
state fixed effects	no	yes	no	yes

Table 9: Estimation results : The effect of Ownership on Exit Hazard

	(1)	(2)	(3)	(4)
owner	.008	.014	.185	.198
	(.030)	(.031)	(.039)	(.040)
mortgage			-.266	-.270
			(.037)	(.038)
state fixed effects	no	yes	no	yes

Table 10: Estimation results : The effect of Housing Equity on Job Hazard

	(1)	(2)	(3)	(4)
mortgage	.100	.142	.170	.168
	(.021)	(.026)	(.039)	(.039)
equity share		-.100	-.148	-.146
		(.036)	(.056)	(.056)
equity level[1]	no	no	yes	yes
wealth[2]	no	no	no	yes
state fixed effects	yes	yes	yes	yes
# of spells	18,763	18,632	18,632	18,632

[1]Housing equity variables include indicators for zero and positive home equity and deflated log home equity value

[2]Wealth variables include indicators for zero and positive wealth and deflated log wealth value

Table 11: Estimation results : The effect of Housing Equity on Exit Hazard

	(1)	(2)	(3)	(4)
mortgage	-.269	-.378	-.535	-.520
	(.039)	(.051)	(.074)	(.073)
equity share		.247	.492	.479
		(.077)	(.110)	(.108)
equity level[1]	no	no	yes	yes
wealth[2]	no	no	no	yes
state fixed effects	yes	yes	yes	yes
# of spells	18,763	18,632	18,632	18,632

[1]Housing equity variables include indicators for zero and positive home equity and deflated log home equity value

[2]Wealth variables include indicators for zero and positive wealth and deflated log wealth value