Home Ownership and the Duration of Unemployment:
A Test of the Oswald Hypothesis

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Comments welcome.

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Abstract

The last five years has seen the appearance of a substantial number of papers of the social implications of tenure choice. While the bulk of these papers suggest that owner-occupied housing produces social benefits, Andrew Oswald maintains that it also produces a social cost: unemployment. Oswald showed a correlation between homeownership and unemployment both within and across countries. These results have been confirmed by Nickell and Layard (OECD countries) and Green and Hendershott (US states).

Oswald's method, however, suffers from two potential drawbacks: selectivity bias and aggregation bias. In order to address both these biases, we track through time individual households with people who enter unemployment, and we attempt to control for selectivity bias. We find some evidence that earners of homeowning households who become unemployed find work less quickly than do earners of renter households. However, the impact is only an eighth of that reported by Oswald and others using aggregate data.

Introduction

A.J. Oswald (1996) and (1997) suggests that a major negative externality may be associated with home ownership: higher unemployment seems to accompany/be-caused-by greater homeownership. Oswald's argument is that homeowners are less willing than private renters to move to jobs when they become unemployed because owners have larger costs of moving than do renters – selling costs and longer periods of double house payments until their house is sold. As a result, when owners become unemployed, they stay unemployed longer than renters do. Oswald also emphasizes "indirect" effects on owner mobility. Areas with high home ownership rates have tighter planning laws and restrictions on land development, which discourages business start-ups, and have greater congestion owing to owners commuting further than renters, increasing the cost of having a job. The former suggests lower income growth and thus more difficulty for the unemployed becoming reemployed (fewer available jobs) or lower net benefits (wages) for the employed; the latter implies less desire of those who become unemployed to wish to become employed.

Oswald provides an impressive array of data indicating a strong positive relationship between homeownership and unemployment: a ten-percentage point increase in ownership leads to a two percentage point increase in unemployment. The data supporting this finding are both cross-sectional for countries (in both 1960 and 1990) and for regions within countries (European regions and U.S. states) and time series for countries (changes between 1960 and 1990) and for U.S. states (changes between 1970 and 1990). In effect, Oswald sees the rise in homeownership in Europe since 1960 as explaining the rise in unemployment and current differences in homeownership rates across countries as explaining much of current differences in unemployment rates.

Oswald's results can be criticized on a number of grounds. The first is a lack of co-variates, such as unemployment benefits and the extent of unionization, that might reasonably affect unemployment. Nickell and Layard (1999) address this issue in an analysis of 20 OECD countries (two data points – averages of 1983-88 and 1989-94) by including three variables each for both benefits (replacement ratio, duration of benefits, and a measure of policies to get unemployed reemployed) and unionization (union density, coverage and coordination with employers). All six variables were statistically significant with the anticipated sign. However, owner-occupation was also significant with a ten-point rise in ownership being associated with a one to 1.5 percentage point rise in the unemployment rate.

A second line of criticism relates to the unweighted regressions, where small regions matter as much as large ones, young households who have accumulated little wealth and have had less time to become attached to an area are weighted equally with older households, and household heads upon whose income the family depends are weighted equally with other household members. We have confirmed Oswald's finding for U.S. states for the most plausible middle (35 to 64) age classes (Green and Hendershott, 2001). Moreover, comparison of results for the total population with those for household heads suggests that non-household heads are more mobility constrained and therefore more likely than household heads to be influenced by housing tenure. Our estimated response (for total workers between ages 35 and 65) is close to the Oswald result of ten percentage points of additional ownership leading to a two percentage point higher unemployment rate.

Unfortunately, a long literature shows that using macro-economic modeling to draw inferences about microeconomic behavior is problematic. As Intriligator, Bodkin and Hsiao (1996) note, for aggregated regressions to reflect individual behaviors, "aggregation conditions" must be in place: the Engle or "adding-up" condition and the Cournot condition. Both these conditions place constraints on price and income elasticities of demand that are quite restrictive. Because the unit of observation in Oswald is either state or country, the validity of the Oswald result is dependent on whether the aggregation conditions are met.

Oswald's result is also subject to possible selectivity bias. A long literature shows that tenure choice is a function of the relative user cost of owning and renting (see Rosen, 1979; Rosen, 1986). Generally speaking, tenure choice models take into account the flow costs of housing: the after tax costs of maintenance, depreciation, and financing, as well as the annualized value of the fixed costs of owning vs. renting. Owning requires a series of sunk costs, including mortgage origination fees, title searches, appraisals, and costs of eventual sale. Those who have long expected lengths of stay will tend to have lower user costs of owning than those with short expected lengths of stay because long-term owners can amortize their fixed costs over a longer period than short term owners can.[1]

The implication is that households that plan to be mobile are less likely to choose owning than households that plan to stay put. This being the case, we would expect owners who become unemployed to remain unemployed longer than renters who become unemployed, even if ownership per se has no impact on the length of unemployment spells. Consider two reasons for longer expected lengths of stay, a stable extended household and a good job. If the household loses its job and its reason for a long expected length of stay was a stable extended household, its lower tendency to move to find new employment simply reflects characteristics inherent to the owner, rather than something "caused" by tenure status. On the other hand, if the household loses its job and its reason for a long expected length of stay was the job, a lower tendency to move would reflect ownership causing lengthened unemployment.

One method of disentangling the characteristics of households that own from characteristics that are caused by owning is to use Heckman's (1979) selectivity correction technique with an individual household database to estimate a two-stage model of how tenure

[1] Selectivity could work in the opposite direction. People who self-select into owning may, for example, have better organizational skills and might therefore be better able to find a new job upon encountering unemployment.

might influence the duration of unemployment (see also Barnow, Kain and Goldberger, 1981). The Panel Survey of Income Dynamics, which gives information about tenure, household, and labor mobility characteristics for roughly 9000 households, is such a database. Using this database, we have undertaken this estimation. Such an analysis is especially relevant because the ownership-unemployment relationship is unlikely to be fully convincing to most until it is established using household level data.

The next section is a brief review of the literature on the impact of homeownership on a wide range of social outcomes. The following sections describe our unemployment duration model and the duration hazard estimation, specify the hazard function, and present results. We conclude with a summary.

Homeownership and Social Outcomes

While the working assumption of many policymakers has been that homeownership is a good thing for society, the literature on the influence of tenure on social outcomes was sparse until the late 1990s. We now have a better, if still imperfect, handle on how much tenure matters to society. The effects are on neighborhood conditions, civic participation, children and personal satisfaction, and, possibly, labor markets.

The oldest literature involving tenure and outcomes is in the area of home maintenance. In a theoretical paper, Henderson and Ioannides (1983) present a model in which owners are more likely to maintain their houses at an optimal level than are renters. Theirs is a principal-agent argument: because owners are both principal (the owner of a property) and agent (the manager of a property), the incentives of principal and agent are always aligned. This stands in contrast to rental housing, where the principal and agent are necessarily different people. The incentives of renters and landlords are generally not in alignment, particularly because renters often plan short lengths of tenure while landlords plan on holding buildings for long periods of time. Consequently, it is possible that rental housing units will not be maintained as well as owner housing units.

This might create an undesirable social outcome because poorly maintained buildings can produce negative externalities. O' Sullivan (1993) suggests that neighborhoods that physically deteriorate breed crime. Moreover, as buildings lose value, they pay less in local taxes while presumably consuming no less in local government services. Not only are poorly maintained buildings are aesthetically unpleasant, they are bad for child development (see below).

The empirical literature testing the impact of tenure on maintenance contains at least four papers: Galster (1983), Shilling, Sirmans and Dumbrow (SSD, 1991), Rohe and Stewart (1996) and Gatzlaff, Green and Ling (GGL, 1998). Galster tests the effect of tenure on maintenance effort: using a sample of 559 houses in Wooster, Ohio, he finds that owners are between 58 and 132 percent more likely to perform maintenance, and 84 to 92 percent less likely to have structural problems. SSD estimate hedonic regressions with a sample of 360 houses in Baton Rouge, Louisiana and find that after controlling for a variety of physical and neighborhood characteristics, owner-occupied houses depreciated at an annual rate of 0.6 percentage points less than renter-occupied houses. Rohe and Stewart looked at a random sample of census tracts to investigate the effect of the homeownership rate on the change in property values between 1980 and 1990. Using two-stage techniques, they found that a one percentage point increase in homeownership produced an $800 increase in value in the average priced house. GGL used repeat sales techniques on 47,329 single-family detached houses followed over 24 years in Pinellas County, Florida and found that owner-occupied house prices increased by 0.16 percentage points more per year than rental house prices. Given their large sample, the difference was statistically significant. The four papers are all consistent with the Henderson and Ioannides hypothesis.

Two recent papers have established a positive effect of tenure on civic participation. The link between the two seems straightforward. Owners have a financial stake in their communities: when communities are run efficiently and responsively, property values rise, educational services/outcomes are better, etc. Owners therefore have an incentive to be involved with, or at least monitor, how their communities are run. Rossi and Weber (1996) generate a series of correlations between owning and renting after controlling for socioeconomic and marital status. They find that owners are more likely to be interested in public affairs, read a newspaper, belong to a local improvement group, participate in campaigns, lobby, give money to political candidates, and know the names of their congressman, governor, and school superintendent. DiPasquale and Glaeser (1999) use a more sophisticated modeling strategy testing many of the same outcomes. They find that in the United States and Germany, ownership positively affects civic participation after controlling for a large number of variables and correcting for selectivity bias.

Green and White (1997) and Haurin, Parcel and Haurin (2000) test for the impacts of tenure choice on child outcomes. The former investigates school outcomes for children of homeowners and renters and finds that children of owners are more likely to finish high school than renters, even after controlling for a variety of household and neighborhood characteristics as well as making an attempt at selectivity bias correction. Green and White also find that girls under age 18 from owning households are less likely to become pregnant than such girls from renting households. The Green-White paper features three data sets: the Public Use Micro Sample of the United States Census, the High School and Beyond Survey, and the Panel Survey of Income Dynamics.

Haurin, et al. use the National Longitudinal Survey of Youth. Their data set, augmented by the NLSY-Child Data, consists of more than 1,000 children, ages five to eight in 1988, who also were surveyed in 1990, 1992, and 1994. The child data are matched with extensive social, demographic, and economic data on parents, this information first collected in 1979 and updated annually. They use a random effects model to estimate the impact of home ownership on the quality of the home environment and the impacts of both home environment and home ownership on child outcomes. They find substantial positive effects of home ownership on the home environment. Further, both increased quality of home environment and home ownership itself increase child cognition and reduce child behavior problems. The math and reading achievements of children of owners are up nine and seven percent respectively, and child behavioral problems are down three percent, all of these being statistically different from zero. Moreover, the longer a parent owns a home, the greater is their children's cognition and the lower are behavior problems. This is doubtless the most thorough test so far of the effect of tenure on childhood outcomes, and it continues to confirm that home-ownership produces desirable outcomes for children.

Finally we note a potential dark-side of homeownership. While past work suggests that homeownership could well produce neighborhood stability and civic involvement, these phenomena could attach households more closely to their communities, thereby reducing mobility. While many, particularly those who advocate the creation of "social capital" (e.g., Fine (1999), see such an outcome as an unambiguously good thing, reduced mobility could lead to longer durations of unemployment as Oswald has argued.

A Model of Unemployment Duration

We modify the modeling strategy adapted in Lumdaine, Stock and Wise (1995). Unemployed households are sometimes confronted with the choice of moving, and taking on the attendant moving expenses, in order to take a job immediately or waiting to see if they can find a job locally. When confronted with this choice, households maximize the value function:

$$W_t = \max\left(E_t\left[U_w\left(Y_t + e_{1t} + bW_{t+1}\right)\right] - F, E_t\left[U_w\left(Y_{t+1} + e_{1t+1} + bW_{t+2}\right)\right]\right) \qquad (1)$$

where W_t is the discounted value of lifetime income at time t, Y_t is income at time t, E is the expectations operator, U is a twice differentiable, concave utility function, F is the fixed cost of moving, and b is a discount factor. Equation (1) simply means that households have a choice of taking a job in period t, along with the stream of benefits arising from the job over time and the costs F associated with moving, or waiting to period t+1 and taking a local job whose income stream will be discounted at rate b, which reflects both the opportunity costs and the risks involved with waiting. Waiting, on the other hand, will not involve moving costs. If the moving costs for owners are higher than the moving costs for renters, we should observe, ceteris paribus, that owners are less willing to move than renters.

In effect, the decision to remain unemployed or not depends on the costs involved in regaining employment relative to the gains from becoming reemployed, as well as the available opportunities. There are two general costs to regaining employment. The first, paid by everyone, is the cost of searching for another job (this might vary with the occupation and the local unemployment rate). The second, paid by only those households that move to a new job location, depends on the severity of dislocations caused by the move. The severity will be greater the more other workers are in the household (they will have to give up their current employment and search for another job), the more school age children are in the household (they will need to become accustomed to a new neighborhood/school), and the longer one has been in the existing housing unit (the greater is the disruption in relationships caused by the move). Thus these variables, too, are ideally included in the analysis as control variables.[2]

While Oswald emphasizes indirect factors that cause homeowners to be less mobile than renters, housing economists would refer to direct costs associated with the causes of the unemployment increase, namely rising real interest rates and falling area real incomes, both of which lower house prices. Lower prices in turn prevent owners who had made low downpayments from remaining owners if they were to move (Archer, Ling and McGill, 1996, and Caplin, Freeman and Tracy, 1997), leading to an aversion to default (Deng, Quigley and Van Order, 2000).[3] Rising interest rates also reduce mobility of households with nonassumable mortgages because they are unwilling to give up now below-market interest rates on long-term fixed-rate loans (Hendershott and Hu, 1982, and Quigley, 1987). In fact, this impact was so large, that unemployment was almost certainly increased. We control for the lack of equity by including two loan-to-value dummy variables indicating that the LTV is in excess of 1 and is in excess of 0.9. We abstract from the extraordinary impact of rising interest rates by following households during a period in which interest rates generally fell.

Of course the key variable to Oswald's hypothesis is whether or not the duration of unemployment is greater if the newly unemployed is in a household that owns a home. We test for this impact in two ways. First, we simply include a zero-one dummy for ownership. Alternatively, to correct for selection bias, we estimate a probit tenure choice equation and use predicted ownership, rather than actual ownership, in the length-of-unemployment estimation. For explanatory variables we use determinants of household permanent income (the product of household wage income and series of age dummy variables and education dummy variables for the household head), and a series of demographic variables (race, gender, household size, marital status).

We also control for variation in the user cost of owning versus the cost of renting. Measuring the owner user cost accurately is difficult. The real after-tax interest rate is that expected to exist over the expected household holding period, and even measuring tax rates for known incomes is tricky (Hendershott and Slemrod, 1983). Further, estimating the rent on a

[2] The PSID makes it difficult to determine length of tenure without losing large numbers of observations. We will return to this issue in a later version of this paper.
[3] Engelhardt (1996) argues that employed owners respond to house price declines by increasing their saving, thereby rebuilding their equity.

house that is comparable to the average priced house in a region, much less an individual house, is difficult (Green, Malpezzi and Chun, 1998). To simplify matters, we use state and annual dummy variables to proxy for the user cost ratio in the estimation. This will allow the ratio to vary across states and across years, but not across households within states.

We estimate a tenure choice equation for each year, 1988-92, for all households in the PSID in January of the year (the estimates are reported in Appendix A). The estimated probability of being an owner produced by this equation, rather than the actual choice, is then used as a variable to explain household unemployment duration. This should largely purge the model of the selection problem because the fitted value for tenure will not be correlated with idiosyncratic household characteristics. The state dummy variables are used as instruments: they are included in this first stage equation, but not in the second stage equation.

Duration Hazard Estimation

Our sample is drawn from the PSID, which by 1992 is a panel containing 9792 households, with lost households replaced. All the data needed for our estimations are available beginning with the panel year 1986. For now, however, our data set consists of households that contain individuals who became unemployed between January 1988 and November 1991[4], or 46 months. We exclude 1986 and 1987 because this is a period of rising interest rates and thus could lead to an extraordinarily large impact owing to the mortgage "lock-in" effect. We exclude 1992 because we cannot match these data to the 1993 panel (which contains the 1992 unemployment information) owing to the absence of the 1992 interview number. We follow each household until it drops out of the sample, the unemployed individual becomes re-employed, or December 1991, whichever comes first (the PSID finalized data from which we are confident in our matches ends in 1992, which includes monthly observations through December 1991). This gives us a total of 1753 households. Because tenure status is available annually only, we do not know the tenure status of households during the month they became unemployed if their tenure changed during that year: we therefore drop observations in which tenure changes. We also get data on monthly unemployment for a particular year from the panel interviews of the following year, e.g., we get our 1990 unemployment data from the interviews with the 1991

[4] This obviously means that we have right-censored observations in our data. We treat this issue at some length later.

panel. Needless to way, we must therefore combine information from two panel years to get each observation: we match, for instance, information on marriage from the 1990 interview with information on unemployment from the 1991 interview. Because households enter, exit, and split-off across time, we cannot match every instance of unemployment with a corresponding set of explanatory variables. After eliminating these observations, our sample reduces to 1590 households. Table 1 lists the number of households included in each month of our sample.

We report a total of eight hazard functions. First, we estimate separate equations for unemployed household heads and non-heads. Presumably heads are more willing to move to pursue employment than are non-heads, the head being the higher income earner. We estimate these equations first by employing a zero-one dummy variable for ownership and then using the predicted probability of ownership, giving a total of four hazard functions. That is, for both household heads and "wives," we estimate

$$T = S(X\eta + Ho\gamma_A + \varepsilon_A) \tag{2}$$

and

$$T = S(X\eta + \hat{H}o\gamma_F + \varepsilon_F) \tag{3}$$

where the hat denotes a fitted value, the subscripts "A" and "F" denote the coefficients on actual homeownership and the predicted likelihood of homeownership, respectively, and S( ) is a survival function that depends on tenure (Ho is a dummy variable that equals one when the household owns and zero when it rents) and a vector of other variables (*X*) with its coefficient vector η. In our two stage equations, we identify the γ coefficient both by including state dummies in the first stage equation but not in the second, and via the nonlinearity of the probit function, which returns the predicted probability of homeownership. Maddala (1983) notes the acceptability of using the functional form for identification. Moreover, while the state dummies in the first stage proxy for user cost, we include a variable for unemployment conditions in the county of residence, as well as proxies for seasonal effects and business cycle effects (dummy variables for the month and year unemployment begins) in the second stage equation. We also report an estimate for household heads with the censored observations deleted using the predicted probability of ownership.

Second, we estimate separate equations for unemployed heads who are owners and who are renters. This allows us to determine whether the impacts obtained for households heads are due to the behavior of owners or of renters. Further, only in an equation for owners can we include the loan-to-value dummies. Specifically, we estimate the following model:

$T_o = S(X_o h_o + \partial_o)$ for Ho = 1 and (4)

$T_r = S(X_r h_r + \partial_r)$ for Ho = 0 (5)

with

Ho = 1 if $\Phi(Zd) > 0$ and (6)

Ho = 0 if $\Phi(Zd) \leq 0$ (7)

where Φ is the cumulative distribution for the Normal Distribution evaluated at the household's characteristics, the r and o subscripts denote owners and renters, the Xs are explanatory variables for duration of unemployment, the **Z** is a vector of explanatory variables for tenure choice, and d is a coefficient vector. Equations (4) and (5) are the survival functions for owners and renters, respectively, and (6) and (7) represent the outcomes of the tenure choice equation. Note that we use the probit as the function for determining tenure choice. We are unable to estimate separate regressions for non-heads because the number of observations available for control variables involving age are sparse (there are not many unemployed, married renters above certain age classes).

Because the residuals of the tenure choice equation could be correlated with the residuals of the survival functions, we must correct for selectivity bias. To do this, we estimate the survival functions with the inverse Mills Ratio as a regressor. That is, we add the term f/Φ as a regressor to the survival function for owners, and $f/(1-\Phi)$ as a regressor to the survival function for renters, where f is the probability density function of the Normal Distribution for the tenure choice model again evaluated using the household's characteristics. As we shall discuss below, by using the Weibull distribution for estimating the hazard function, we separate the baseline hazard function from the regression function, and estimate a linear regression function. This allows Heckman's (1979) arguments for determining the coefficient standard errors from his two-stage method for correcting for selectivity to carry through. Censoring presents us with a more challenging problem, which we shall discuss later.

Specification of the Hazard Function

To estimate an equation explaining the period of time between entering and leaving unemployment, we use the Weibull distribution. Specifically, we estimate the hazard function of the form:

$$l(t, x, h, a) = \exp(Xh)t^{a-1} \tag{8}$$

which produces the survivor function

$$S(t) = \exp[-\mathrm{A}_o(t)\exp(Xh)] \tag{9}$$

where $\mathrm{A}_o(t) = \int u^{a-1}du$. We estimate this function for owners and renters together, as well as for owners and renters separately. The Weibull reduces to the exponential distribution when a = 1. In all cases we can reject the null hypothesis that a = 1. The interpretation of the hazard is as follows. The $\exp(Xh)$ component shifts the baseline hazard function. As explanatory variables with positive coefficients get larger, the speed at which the household exits unemployment gets faster. See Kiefer (1988) for a fuller explanation.

We explain length of unemployment as a function of the unemployed person’s age, educational attainment, gender, marital status, race, the number of additional earners in the household, the household income of the unemployed person in the year preceding the year in which the person becomes unemployed, the unemployment rate in the county in which the unemployed person resides, the health status of the household head, the number of children in the household, and, for the equations for homeowners alone, dummy variables based on the loan-to-value ratio. We define race as whether the household is non-hispanic white or not. The marital status categories include married, single, widowed, divorced and separated (the dropped category). Co-habitors are classified as married. We also include dummy variables for the year and month in which the person became unemployed in order to take into account business cycle and seasonal effects. As noted above, we run separate specifications for household heads and household "wives," with a homeownership dummy as an explanatory variable and with a fitted probability of homeownership variable as an explanatory variable. We are, of course, testing for whether homeownership affects the length of unemployment.

We include age because we expect that older people—people closer to retirement—have less incentive to find work quickly, particularly if they have accumulated substantial retirement wealth, and generally have relatively smaller mortgages to carry (the impact should be stronger for owners than renters). Lumsdaine, Stock and Wise (1995) estimate a hazard model that takes into account the retirement "spike" at age 65, and in doing so show the relationship between age and participation in the labor market.

We include marital status because married couples have a number of financial advantages that could work to both increase and decrease the length of time people are unemployed. We also note that if one agrees that homeownership reduces mobility, one could make the same argument about marriage. It is more likely that one of the spouses has roots in a community than does a single person and thus that the costs of moving are greater for a married couple than they are for a single person.

We include additional earners because they can insulate households from the most painful effects of unemployment, and therefore allow unemployed workers to take more time to find the "preferred" job. Further, if the head moves, other earners also have to find new jobs. Similarly, those who had higher incomes in the previous year are better able to cope with the financial pressures of unemployment, assuming they placed some of the higher earnings in savings. On the other hand, those with higher earnings also have a higher opportunity cost to unemployment, and might therefore be motivated to find a job more quickly. We also include education attainment because the opportunity cost of unemployment becomes higher as education rises, although the income variable may capture this effect. Finally, the "available opportunities" might vary by education of the worker.

We include health status because healthy people are better able to take on the rigors of a job search and will be able to offer higher levels of labor productivity to employers. And we include the year dummies and monthly dummies to take into account the business cycle and seasonality, characteristics that Castaneda, et al. (1998) and others have shown to be important for modeling unemployment spells. Finally, among homeowners, we run a model that includes high loan-to-value dummy variables to discern whether negative or near negative equity could prevent people from moving (because they wish to avoid foreclosure or giving up ownership).

Data and Results

As already noted, our data come from the Panel Survey of Income Dynamics. For household heads, we have 1875 observations, of which 371 are right censored for three reasons: the head is still unemployed in our last observation (December 1991), the head drops out of the sample, or the head leaves the labor force. For non-heads or "wives," as that term is defined in the PSID, we have 789 observations, of which 299 are right-censored. To follow heads across time, we matched panel years by interview years. That is, we took the 1988 interview number from the 1989 wave, and matched it to the 1988 wave accordingly. We then checked the split-off indicator to see whether the 1989 household was new: if it was, we knew not to connect its head's information to the 1988 information. Table 2 contains the results.

The coefficients are directly taken from SAS and as such reflect a specification that predicts the time at which the person will exit unemployment.[5] Consequently, a positive sign means that as the explanatory variable increases, the length of time for which the person is unemployed does also.

For household heads, the coefficient on the homeownership dummy is effectively zero. The coefficient using the fitted probabilities, on the other hand, supports Oswald hypothesis that renters leave unemployment faster than owners.[6] The coefficient is significantly positive (one-tail test) at the 95 percent confidence level and different from zero at the 90 percent level. The larger standard errors when the fitted probability of owning is employed may be caused by the use of fitted values in a censored equation (Amemiya, 1979). To address this, we rerun our regressions for household heads dropping all the censored observations. The results from this exercise are reported in column 2. Note that most of the coefficients and standard errors remain quite similar. In particular, the predicted ownership coefficient still has a t-ratio of 1.6.

Somewhat surprisingly, the estimated coefficient on predicted ownership in the hazard for non-heads is smaller and barely exceeds its standard error. Recall that we expect that non-

[5] Coefficients of hazard functions generally indicate the speed at which exit takes place. The SAS output thus reports coefficients somewhat idiosyncratically.
[6] The number of observations is less because some households do not have all the explanatory variables needed to predict homeownership.

heads would be less likely to move to find employment because their moving would also require that the higher earning head also move. We note that the sample here is 60 percent less than for heads, which may contribute to the relatively lower significance level.

There is evidence of cyclical and seasonal effects as in Castraneda et. al. (1998). Heads who became unemployed in 1988 (the deleted year) were reemployed significantly quicker than in later years as the boom died. Regarding seasonal effects, reemployment is significantly faster for those losing their jobs in December (the deleted month) and slower for those losing jobs in January, April and November.

A number of additional effects show up for heads. First, younger (under age 45), and presumably less wealthy households with higher mortgages payments relative to income, have shorter durations of unemployment. This is especially true for those under age 25. A little surprising, given the results of Lumsdaine, Stock and Wise (1998), the reemployment of those over age 65 is quite similar to those ages 45 to 65. Second, also as expected, those in poor health (the deleted category) and with more children remain unemployed longer. Third, unemployed widows and separated marrieds (the deleted category), who presumably have fewer resources, get reemployed more quickly.

The seasonal, age, and health effects largely carry over to non-heads, although the number of children effect does not. Regarding cyclical effects, reemployment was quicker in 1990 as well as 1989. In addition, there is an education effect. High school graduates and those with some college are reemployed faster than high-school dropouts, while those with a college degree are reemployed less rapidly.

We now turn to the separate estimates for owners and renters, employing the inverse-Mills ratio term, which is significant for renters. The above-noted age effect is largely due to owners, suggesting a mortgage payment influence. So also are the number of children and health effects. Further, all the included family status coefficients rise for owners, suggesting that separated (the dropped category) owners get reemployed much more rapidly. Two additional variables show up here. First, for owners, unemployed college graduates are reemployed quicker and those with some college less rapidly. Second, white renters are reemployed faster than are minority renters.

A final point: the log-likelihood statistics show that the separate hazard functions for owners and renters are better specifications than single hazard using predicted housing tenure.

Specifically, the test statistic on the improvement provided by the interaction terms in equations without the selectivity correction is 97. This statistic has a Chi-square distribution with 37 degrees of freedom. The 99th percentile critical value is a little less than 66.77. This is not surprising given the significant differences found in owner and renter behavior.

Our last equation tests the hypothesis that owners with no or little equity seek employment faster than owners with significant equity in their home. The coefficient on the LTV dummy (over unity) is not statistically different from zero, but it is negative as expected; those with no equity are reemployed faster. Further testing is needed.

Quantitative Impacts

The percentage increase in unemployment due to a ten-percentage point decline in homeownership is simply the percentage increase in the duration of the unemployed. We first compute the reduction in the constant term in the ownership probit that is sufficient to shift ten percent of owners to renters (from probability of ownership above 0.5 to below 0.5). The probit predicts the sample mean of 66 percent owners, so we lower the constant by an equal amount for each household until the equations predict 56 percent owners. We then insert the resultant lower predicted probability of ownership for each household in our sample in the survival function to obtain a lower predicted duration of unemployment. To obtain the predicted impact on the unemployment rate, we compute the average resultant unemployment duration, divide this by the original sample average duration, and multiply by the initial sample unemployment rate. The result is a 0.25 percent decline in the duration of unemployment and thus in the unemployment rate. This is "an eighth" of the two percent decline that Oswald and others obtained from the macroeconomic data.

Table 4 illustrates the quantitative impact of other variables on the mean duration of unemployment. These impacts are obtained by setting all variables except those of interest at their mean values and then setting the relevant variables – age, health, etc. in turn – at the appropriate values. Impacts are given for heads and nonheads and for the former. Impacts for number of children and separated households will be added shortly. Owing households with many children have significantly longer durations of unemployment, while separated owners have significantly shorter durations.

Conclusion

The last five years have seen the appearance of a substantial number of papers on the social implications of tenure choice. While the bulk of these papers suggest that owner-occupied housing produces social benefits, Andrew Oswald maintains that ownership also produces a social cost: unemployment. Oswald showed a strong positive correlation between homeownership and unemployment both within and across countries, and Nickell and Layard Green and Hendershott confirmed this result using aggregate data.

These results suffer from two potential drawbacks: selectivity bias and aggregation bias. In order to address both of these issues, we follow individual households with people who enter unemployment and attempt to control for selectivity bias. We find that unemployed individuals in owning households find jobs less rapidly than do unemployed individuals in renting households, just as Oswald hypothesized. However, the quantitative impact is only about a tenth that suggested by Oswald.

We obtain numerous other relationships on the duration of unemployment. Unemployed younger (under age 45 and especially under age 25) homeowning household heads tend to become reemployed faster than older household heads, possibly owing to the pressures of relatively larger mortgage payments. Unemployed homeowning household heads with poor health or with children tend to be reemployed slower than similar heads with good health or without children, while widows and especially separated owing household heads are reemployed more rapidly. Unemployed white renters are reemployed faster than unemployed minority renters, and secondary workers with poor health are particularly slow to be reemployed.

In work in process we are adding the 1986 and 1987 data. Not only will this roughly double the data sample, but it will allow for an estimation of (a modest) mortgage “lock-in” effect on mobility.

Appendix: Tenure Choice Estimation

In estimating tenure choice, we used a probit specification and included as explanatory variables the age, educational attainment, gender, race and marital status of the household head, the number of earners in the household, and state dummy variables. We ran separate regressions for each year from 1988 to 1992. Green (1996) showed that tenure choice coefficients can vary substantially across years.

Our results were much as we expected them: the probability of owning rises with age in a statistically significant manner (for all age coefficients, the t-statistics are in excess of 7), rises somewhat with education (high school graduate is the only level of educational attainment with a t-statistic consistently in excess of 2), rises with income (again, t-statistics are typically greater than 7), is higher for whites (t-statistics in excess of 9) and for married couples (t-statistic in excess of 10). The state dummies were jointly different from zero at the 99 percent level of confidence. A table containing the estimates for each year is available from the authors on request.

The regressions substantially improved on our ability to predict tenure relative to the 66 percent sample mean: the fraction of households correctly placed in each category was 78 percent, 79 percent, 74 percent, 76 percent and 76 percent for 1988, 1989, 1990, 1991 and 1992 respectively.

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Table 1
Number Entering Sample
By Month and Year

	1988	1989	1990	1991
January	--	95	99	86
February	19	18	16	19
March	18	11	22	23
April	28	23	26	27
May	17	21	19	22
June	34	29	35	47
July	36	33	34	33
August	38	26	34	35
September	38	40	55	25
October	41	42	52	30
November	56	59	74	52
December	64	82	91	51

Table 2
Hazard Results for Household Heads and Non-heads

	Heads		Censored Obs. Deleted	Non-heads	
	(1)	(2)	(3)	(4)	(5)
Intercept	1.35 (0.22)	1.11 (0.25)	1.35 (0.22)	1.60 (0.34)	1.57 (0.35)
Age < 25	-0.52 (0.14)	-0.63 (0.17)	-0.31 (0.11)	-0.32 (0.28)	-0.40 (0.29)
Age ≥ 25 and Age < 35	-0.28 (0.13)	-0.38 (0.14)	-0.22 (0.11)	-0.18 (0.28)	-0.24 (0.29)
Age ≥ 35 and Age < 45	-0.27 (0.13)	-0.32 (0.13)	-0.28 (0.11)	0.23 (0.28)	0.27 (0.29)
Age ≥ 45 and Age < 55	-0.06 (0.13)	-0.10 (0.13)	0.11 (0.11)	0.13 (0.29)	-0.17 (0.29)
Age ≥ 55 and Age < 65	-0.04 (0.14)	-0.03 (0.13)	0.03 (0.05)	0.01 (0.30)	-0.01 (0.30)
Owning Household	-0.03 (0.05)			0.06 (0.07)	
Fitted Prob. of Owning		0.35 (0.21)	0.27 (0.17)		0.22 (0.19)
High School Graduate	-0.10 (0.05)	-0.06 (0.05)	-0.03 (0.04)	-0.15 (0.10)	-0.14 (0.10)
Attended College	0.06 (0.06)	0.06 (0.06)	0.03 (0.05)	-0.24 (0.09)	-0.25 (0.09)
College Graduate	-0.07 (0.06)	-0.06 (0.07)	-0.07 (0.06)	0.23 (0.11)	0.23 (0.11)
Number of Extra Earners	0.03 (0.04)	0.04 (0.04)	0.02 (0.03)	-0.04 (0.05)	-0.02 (0.05)
Taxable Inc. in Previous Year (000)	-0.0006 (0.0009)	-0.0001 (0.0010)	-0.005 (0.009)	-0.0022 (0.0014)	-0.0012 (0.0017)
Male Head	-0.10 (0.07)	-0.09 (0.07)	-0.08 (0.06)	-0.04 (0.12)	-0.03 (0.12)
White	-0.10 (0.10)	-0.05 (0.05)	0.03 (0.04)	0.006 (0.06)	0.02 (0.07)
Married	0.10 (0.10)	0.17 (0.11)	0.03 (0.10)		
Single	0.21 (0.09)	0.21 (0.09)	0.02 (0.07)		
Widowed	-0.04 (0.14)	0.02 (0.14)	0.07 (0.12)		
Divorced	0.12 (0.10)	0.15 (0.10)	0.08 (0.09)		
Number of Children	0.12 (0.05)	0.11 (0.05)	0.05 (0.01)	0.04 (0.03)	0.03 (0.03)

Table 2 (cont.)
Hazard Results for Household Heads and Non-heads

	(1)	(2)	(3)	(4)	(5)
Became Unemp. in '89	0.46 (0.07)	0.47 (0.07)	0.11 (0.06)	0.19 (0.09)	0.19 (0.09)
Became Unemp. in '90	0.23 (0.06)	0.25 (0.06)	-0.01 (0.07)	-0.04 (0.10)	-0.03 (0.10)
Became Unemp. in '91	0.37 (0.07)	0.36 (0.07)	-0.42 (0.06)	0.40 (0.10)	0.39 (0.10)
Became Unemp. in Jan	0.72 (0.06)	0.71 (0.06)	0.80 (0.05)	0.86 (0.08)	0.86 (0.08)
Became Unemp. in Feb	0.30 (0.09)	0.30 (0.09)	0.33 (0.07)	0.33 (0.13)	0.31 (0.13)
Became Unemp. in Mar	0.23 (0.08)	0.21 (0.08)	0.34 (0.07)	0.62 (0.12)	0.62 (0.13)
Became Unemp. in Apr	0.57 (0.08)	0.57 (0.08	0.40 (0.07)	0.48 (0.12)	0.48 (0.13)
Became Unemp. in May	0.35 (0.08)	0.36 (0.08)	0.46 (0.07)	0.52 (0.11)	0.53 (0.11)
Became Unemp. in Jun	0.29 (0.07)	0.28 (0.07)	0.22 (0.06)	0.33 (0.09)	0.32 (0.10)
Became Unemp. in Jul	0.36 (0.07)	0.33 (0.07)	0.22 (0.07)	0.56 (0.10)	0.56 (0.10)
Became Unemp. in Aug	0.21 (0.07)	0.20 (0.07)	0.02 (0.06)	0.51 (0.11)	0.50 (0.11)
Became Unemp. in Sep	0.41 (0.07)	0.39 (0.07)	0.23 (0.06)	0.39 (0.10)	0.41 (0.10)
Became Unemp. in Oct	0.44 (0.08)	0.42 (0.08)	0.27 (0.06)	0.36 (0.11)	0.36 (0.11)
Became Unemp. in Nov	0.59 (0.07)	0.60 (0.07)	0.21 (0.06)	0.54 (0.11)	0.54 (0.11)
Health is Excellent	-0.17 (0.14)	-0.19 (0.14)	-0.17 (0.12)	-0.33 (0.19)	-0.36 (0.19)
Health is Good	-0.28 (0.14)	-0.27 (0.15)	-0.27 (0.12)	-0.33 (0.18)	-0.36 (0.18)
Health is Average	-0.17 (0.14)	-0.17 (0.15)	-0.17 (0.12)	-0.37 (0.18)	-0.39 (0.18)
Health is Fair	-0.19 (0.16)	-0.20 (0.15)	-0.30 (0.13)	-0.34 (0.19)	-0.37 (0.19)
County Unemp. Rate	0.02 (0.01)	0.02 (0.01)	0.02 (0.01)	-0.00 (0.01)	-0.00 (0.01)
Scale Parameter	0.82 (0.02)	0.82 (0.02)	0.68 (0.01)	0.71 (0.02)	0.71 (0.02)
N	1875	1826	1461	738	730
Censored N	371	365		126	126

Table 3
Hazard Results for Owning and Renting Heads

	Renter Head	Owner Head	Owner LTV
	(1)	(2)	(3)
Intercept	1.26 (0.30)	0.82 (0.36)	0.81 (0.36)
Age < 25	-0.41 (0.21)	-0.69 (0.25)	-0.69 (0.25)
Age ≥ 25 and Age < 35	-0.09 (0.21)	-0.46 (0.17)	-0.44 (0.17)
Age ≥ 35 and Age < 45	-0.07 (0.21)	-0.38 (0.17)	-0.36 (0.17)
Age ≥ 45 and Age < 55	-0.06 (0.22)	-0.15 (0.17)	-0.14 (0.17)
Age ≥ 55 and Age < 65	0.11 (0.24)	-0.11 (0.17)	-0.10 (0.17)
Mills Ratio	0.0022 (0.0008)	-0.0006 (0.0004)	-0.0006 (0.0004)
High School Graduate	-0.09 (0.07)	-0.05 (0.08)	-0.05 (0.08)
Attended College	-0.00 (0.07	0.20 (0.10)	0.20 (0.10)
College Graduate	0.03 (0.10	-0.22 (0.11)	-0.23 (0.11)
Number of Extra Earners	0.02 (0.05	0.07 (0.05)	0.07 (0.05)
Taxable Inc. in Previous Year (000)	-.002 (0.001)	0.0009 (0.0010)	0.0009 (0.0010)
Male Head	-0.12 (0.08)	-0.03 (0.15)	-0.04 (0.15)
White	-0.14 (0.06)	0.05 (0.08)	0.05 (0.08)
Married	0.18 (0.11)	0.27 (0.23)	0.28 (0.24)
Single	0.16 (0.10)	0.48 (0.25)	0.48 (0.25)
Windowed	-0.04 (0.19)	0.28 (0.26)	0.32 (0.26)
Divorced	0.14 (0.12)	0.40 (0.24)	0.40 (0.24)
Number of Children	0.01 (0.06)	0.26 (0.09)	0.26 (0.09)

Table 3 (cont.)

	(1)	(2)	(3)
Became Unemp. in ‘89	0.41 (0.10)	0.52 (0.10)	0.52 (0.10)
Became Unemp. in ‘90	0.23 (0.08)	0.32 (0.10)	0.32 (0.10)
Became Unemp. in ‘91	0.39 (0.07)	0.39 (0.12)	0.39 (0.12)
Became Unemp. in Jan	0.65 (0.09)	0.83 (0.09)	0.83 (0.09)
Became Unemp. in Feb	0.29 (0.11)	0.39 (0.15)	0.39 (0.15)
Became Unemp. in Mar	0.11 (0.10)	0.40 (0.14)	0.40 (0.14)
Became Unemp. in Apr	0.61 (0.10)	0.59 (0.14)	0.59 (0.14)
Became Unemp. in May	0.39 (0.11)	0.36 (0.13)	0.38 (0.13)
Became Unemp. In Jun	0.21 (0.28)	0.36 (0.13)	0.35 (0.13)
Became Unemp. In Jul	0.29 (0.09)	0.30 (0.12)	0.30 (0.12)
Became Unemp. In Aug	0.25 (0.10)	0.16 (0.12)	0.16 (0.12)
Became Unemp. In Sep	0.45 (0.09)	0.32 (0.14)	0.31 (0.14)
Became Unemp. In Oct	0.42 (0.09)	0.49 (0.13)	0.49 (0.13)
Became Unemp. In Nov	0.62 (0.09)	0.58 (0.11)	0.58 (0.11)
Health is Excellent	-0.09 (0.20)	-0.22 (0.21)	-0.22 (0.21)
Health is Good	-0.32 (0.20)	-0.12 (0.21)	-0.13 (0.21)
Health is Average	-0.14 (0.20)	-0.12 (0.21)	-0.12 (0.21)
Health is Fair	0.06 (0.21)	-0.30 (0.22)	-0.30 (0.22)
County Unemp. Rate	0.02 (0.01)	0.01 (0.02)	0.01 (0.02)
LTV > 1			-0.18 (0.18)
Scale Parameter	0.80 (0.02)	0.81 (0.03)	0.81 (0.02)
N	1073	753	753
Censored N	210	155	155

Table 4
Difference in Time in Months to Re-employment
Evaluated at Means

	Heads		Non-heads	
Actual Homeowning				
Fitted Homeowning (change of 10 percentage points)		0.2		
Age < 25	-1.5	-1.7		
Age > 25 and < 35	-0.9	-1.2		
Age > 35 and < 45	-0.9	-1.0		
Attended College			-0.8	-0.8
High School Grad	-0.35			
			1.0	1.0
Single	0.9	0.9		
Good Health (relative to poor)	-0.9	-0.9	-1.1	-1.1
County Unemployment Rate (one percentage point change)	0.1	0.1		

Note: Blanks indicate coefficients in model we statistically not different from zero at the 90 percent level of confidence.

30-year Mortgage Interest Rates (88-92)