Fernando Ferreira, Joseph Gyourko, and Joseph Tracy

Housing Busts and Household Mobility: An Update

- The relationship between household mobility and financial frictions, especially those associated with negative home equity, has attracted greater attention following the recent volatility in the U.S. housing markets.
- The decline in mortgage rates, along with policy interventions to encourage historically low-rate refinancing, likewise recommend a closer look at mortgage interest rate lock-in effects, which are apt to become important once Federal Reserve interest rate policy normalizes.
- This article updates estimates in a 2010 study by the authors of the impact of three financial frictions—negative equity, mortgage interest rate lock-in, and property tax lock-in—on household mobility. The addition of 2009 American Housing Survey data to their sample allows the authors to incorporate the effect of more recent house price declines.
- The new study's findings corroborate the 2010 results: Negative home equity reduces household mobility by 30 percent, and $1,000 of additional mortgage or property tax costs lowers it by 10 to 16 percent.

1. Introduction

A long literature on housing economics has noted that a rise in mortgage rates could "lock-in" an owner to his or her current house, thereby slowing or preventing a permanent move to a new residence if mortgage interest rates rise sufficiently to make the new debt service payment unaffordable (see, for example, Quigley [1987, 2002]). Other financial frictions—such as the one arising from California's Proposition 13 property tax rules, which essentially imply an often large increase in property taxes after a move—would have similar effects on household mobility (Ferreira 2010). Negative equity, by which we mean the current value of the house is less than the outstanding mortgage balance, could also reduce mobility if the owner lacks sufficient liquidity to pay off the full loan balance, which is required for a permanent move and sale of the property if the borrower is to avoid the cost of a default (Stein 1995; Chan 2001; Engelhardt 2003).

These three potential financial frictions are all associated with the sale of the house, so there is a transfer of economic ownership, not just a change of residence. Thus, the type of household mobility that may be impacted by these frictions involves permanent moves in which both physical location and economic ownership change for the previous owner. The housing literature on financial frictions does not have clear implications for temporary moves in which the owner leaves the house for a period of time—perhaps to rent it out—and

Fernando Ferreira is an associate professor in the Departments of Real Estate, and Business Economics and Public Policy, and Joseph Gyourko is the Martin Bucksbaum Professor of Real Estate at the University of Pennsylvania's Wharton School; Joseph Tracy is an executive vice president and senior advisor to the Bank President at the Federal Reserve Bank of New York. Correspondence: joseph.tracy@ny.frb.org

The authors appreciate the helpful comments of Anthony DeFusco, Andrew Haughwout, and the referees. Fernando Ferreira and Joseph Gyourko also thank the Research Sponsor Program of the Zell-Lurie Real Estate Center at Wharton for financial support. The views expressed are those of the authors and do not necessarily reflect the position of the Federal Reserve Bank of New York or the Federal Reserve System.

returns at a later date. Overall, mobility reflects permanent and temporary moves, but the appropriate mobility measure depends on the question being addressed. Given our focus on the impact of financial frictions on homeownership transitions, our preferred measure in the analytics reported below reflects only permanent moves as best as possible.

Interest in the relationship between homeowner mobility and financial frictions, especially frictions associated with negative home equity, was piqued for researchers and policymakers by the recent extraordinary boom and bust in U.S. housing markets. With house prices falling 30 percent nationally, the prevalence of negative equity greatly expanded across many markets. More recently, the sharp fall in mortgage interest rates and the various policy interventions to encourage refinancing at historically low rates suggest that we also need to update our knowledge of the impact of mortgage interest rate lock-in effects, as they seem likely to become important after Federal Reserve interest rate policy normalizes.

Interest in the relationship between homeowner mobility and financial frictions, especially frictions associated with negative home equity, was piqued for researchers and policymakers by the recent extraordinary boom and bust in U.S. housing markets.

Because the studies cited above were dated or based on samples from specific geographic regions or population subgroups, our first paper (Ferreira, Gyourko, and Tracy 2010) used the U.S. Census Bureau's American Housing Survey (AHS) panel from 1985-2007 to provide new and more general estimates for the nation that include all three forms of financial frictions in the same econometric specification. Our paper's three primary results were: 1) owners with negative equity were one-third less likely to move than otherwise observationally equivalent owners without negative equity; 2) for every additional $1,000 in mortgage debt service costs, mobility was about 12 percent lower; and 3) similar increases in property tax costs from Proposition 13 in California also reduced mobility by about 12 percent.

This article updates our previous work in two important ways. It adds data from the most recent AHS for 2009, providing the first evidence from the beginning of the bust in home prices in many markets. It also addresses Schulhofer-Wohl's (2011) criticism of our sample selection procedures used in Ferreira, Gyourko, and Tracy (2010). We demonstrate that those selection procedures are appropriate for studying the effect of negative equity (and the other financial frictions noted) on permanent moves. This update also documents that our previous findings are robust to the inclusion of new data and new measures of permanent mobility, which we discuss more fully below.

Our research is related to an emerging, and potentially very important, literature on labor economics investigating whether reduced mobility among homeowners is impairing adjustment in the labor market that might prevent the unemployment rate from falling as much as it would otherwise (see, for example, Aaronson and Davis [2011], Bricker and Bucks [2011], Donovan and Schnure [2011], Modestino and Dennet [2012], Molloy, Smith, and Woznak [2011], and Valletta [2010]). Because we focus solely on how mobility impacts homeowners, our results do not directly address potential spillovers into the labor market. However, our finding of a large impact of negative equity on owner mobility is consistent with the preliminary conclusion of the labor literature: There are little or no significant impacts on the unemployment rate. As we discuss, most moves are within a labor market area, so there can be a significant decline in such moves with no effect on access to job opportunities in that area.

Much work is needed to more fully understand the linkages between housing and labor markets on this issue. For example, the likelihood that labor markets deteriorate along with housing markets raises the possibility that owners with negative equity are not moving in part because good job opportunities do not exist. Distinguishing between these two potential causes of reduced mobility requires expanding one's theoretical and empirical horizons to better control for labor market conditions, and that is the direction in which we urge future research on this topic to turn.

Finally, reduced homeowner mobility due to financial frictions has economic and social effects beyond its possible ramifications for labor markets. For example, locked-in owners are more likely to be mismatched relative to their desired housing units and local public service bundle (such as school systems and the like). The utility loss just from this mismatch could be significant. Whether owners with negative equity even act like true owners and provide the positive social externalities alleged for homeownership is unknown. Economically, these owner-occupants are "renters." Moreover, immobility associated with any type of friction could alter the nature of any housing recovery by shrinking the potential trade-up market. All of these issues require further study, because the evidence suggests that negative equity in particular is associated with much lower mobility, and we suspect that mortgage interest

rate lock-in will become more important in a future recovery. The starting point for that conclusion is a set of robust estimates of mobility effects attributable to financial frictions. It is to that analysis that we now turn.

2. Financial Frictions and Homeowner Mobility: A Brief Review

High transaction costs of buying and selling a home provide an incentive for people to extend their stay in the house in order to amortize these costs over a longer holding period. Additional financial frictions can arise that exacerbate this effect. For example, Quigley (1987) examines the financial friction from fixed-rate mortgages in an environment of rising mortgage rates. Ferreira (2010) and Wasi and White (2005) study the impact of financial frictions arising from restrictions on the rate of property tax increases in California under Proposition 13. A third financial friction is created when house prices decline sufficiently to push borrowers into negative equity. Chan (2001) and Engelhardt (2003) study the impact of negative equity on household mobility.

In Ferreira, Gyourko, and Tracy (2010), we estimate the impact of all three of these financial frictions on household mobility using a consistent empirical methodology and data

High transaction costs of buying and selling a home provide an incentive for people to extend their stay in the house in order to amortize these costs over a longer holding period. Additional financial frictions can arise that exacerbate this effect.

that span the 1985-2007 period. It is important to keep in mind that each of these frictions applies to the sale of the house, not just to whether the owner continues to live there. Hence, we were interested in how these financial frictions impact permanent moves that require the house to be sold.

The AHS data are well suited to address this issue. The data follow a panel of residences through time rather than a panel of households. They contain information sufficient for measuring each of the three financial frictions as well as other determinants of mobility. A limitation of these data, however, is that when an owner sells a house and relocates, we do not know where he or she moves to or the primary motive for the move.

Recall that Ferreira, Gyourko, and Tracy (2010) estimate large impacts of financial frictions on the permanent mobility of homeowners using the AHS panel. Subsequently, Schulhofer-Wohl (2011) uses our data and estimation code, but expands the definition of a move beyond clearly permanent ones to include any change in residence between adjacent American Housing Surveys. Schulhofer-Wohl is correct in observing that we underreported overall mobility by censoring these transitions. However, that decision was made by design in order to distinguish between permanent and temporary moves, as the underlying theory from earlier research implies that it is only with respect to permanent moves that these potential financial frictions should lead to lower mobility. A temporary move reflects a situation in which an owner-occupied residence is reported as vacant or rented for one or more surveys, with the original owner subsequently returning to the residence. These moves can occur because a homeowner in fact vacates his or her home temporarily or because vacancy status is misreported in the AHS data. Economic ownership does not change in such cases, so the costs associated with the frictions have not yet been incurred.

Nevertheless, Schulhofer-Wohl's (2011) critique led us to develop an improved measure that better exploits the panel structure of the AHS to distinguish between the two types of moves. This raises our reported mobility rates substantially, by more than 25 percent, but it does not materially affect our findings, as reported in Section 3. We do not adopt Schulhofer-Wohl's strategy of counting all transitions from ownership to rental or vacancy status as permanent moves because it dramatically overstates their number. His finding of a zero or a slight positive correlation between homeowner mobility and negative equity is likely due largely to conflating temporary and permanent moves.[1] We show below that over the 1985-99 period in the AHS data, more than 20 percent of Schulhofer-Wohl's moves are temporary in nature, which makes his measure problematic for use in research on lock-in effects. These temporary moves correspond to approximately 50 percent of the additional moves that Schulhofer-Wohl tallied in excess of our new, preferred mobility measure. There is still uncertainty about the economic ownership of the property for the other 50 percent of additional moves.

[1] Schulhofer-Wohl used data and codes from our 2010 study to generate his mobility measure, and he compared his results with our baseline measure of mobility. He then provided his underlying code, just as we did for him. Our discussion of his mobility measure always applies to the first of four such variables from his 2011 paper.

Schulhofer-Wohl's measure of mobility also can be dynamically inconsistent, with moves in one period recoded at a later date as nonmoves as additional waves of AHS data are included in the estimation sample. These issues are especially worrisome if one is trying to understand the impact of the recent housing bust on household mobility, because the errors from conflating temporary and permanent moves are concentrated near the end of the data, and the AHS does not yet have enough post-crisis surveys to allow researchers to distinguish between these types of moves.[2]

While this update highlights how noisy the data from American Housing Surveys are, we know of no superior source to use to investigate this issue. Given that it takes time to resolve uncertainty about whether some transitions are permanent or temporary in nature, there is no variable that perfectly reflects the mobility relevant to analysis of the impact of financial frictions. That includes our improved measure reported in this article. It still understates true mobility rates to the extent that any of the moves that we censor due to uncertainty about whether a change in economic ownership of the property occurred actually reflects permanent moves. Precisely where to draw the line on this measurement issue requires careful consideration of the costs and benefits of overstating versus understating the number of permanent moves. We continue to advocate for a conservative coding strategy that is dynamically consistent over time, but this clearly is not costless. The next sections detail why we came to that conclusion.

3. Additional Data and New Measures of Mobility

3.1 Changes in the Data and Summary Statistics

There are four changes to the data used in this update of Ferreira, Gyourko, and Tracy (2010). The first is the addition of the 2009 AHS sample, which became available after we had published our previous study. The 2009 AHS data allow researchers to begin to examine the impact of the house price declines between 2005 and 2007 on household mobility from 2007 to 2009. This is straightforward, and we present and compare results with and without the new data. It does not result in any meaningful changes in our findings.[3]

The second change involves the use of First American-Core Logic (FACL) repeat-sales house price indexes in lieu of the Federal Housing Finance Administration (FHFA) series when we create instruments to address measurement error in the creation of negative equity variables. Unlike the FHFA series, which are based only on conforming loans, the FACL series include arm's-length purchases made with conforming and nonconforming loans, including subprime, Alt-A, and jumbo mortgages. We believe this provides a more complete picture of what was occurring in terms of local house prices, especially in recent years, but this change also has no material impact on the results.[4]

The third change involves additional cleaning of the panel structure of the AHS data. The American Housing Survey was designed to be used primarily as a series of cross-sections rather than as a panel. For this reason, a variable that we employ to define the panel structure—the purchase year of the house—was not dependent coded.[5] By that, we mean that the interviewer does not have access to the responses for this variable from prior surveys, so there is no way at the time of the interview to ensure consistent coding across surveys. As a result, the purchase year can vary in the data even for the same household. If left uncorrected, this spurious variation in the reported purchase year will induce false household transitions. Ferreira, Gyourko, and Tracy (2010) developed several rules that were used to identify and clean these false household transitions in the data. For this update, we also include hard-coded edits to the purchase year based on an inspection of the data history for each residence, including information on the household head's demographic characteristics. This additional cleaning of the panel structure significantly improves on our earlier rule-based edits.[6]

The fourth and most important change involves the use of an improved measure of mobility, which is the dependent variable in our analysis. This alteration was motivated by Schulhofer-Wohl's (2011) critique of our sample selection procedures. In Ferreira, Gyourko, and Tracy (2010), we deliberately chose a conservative definition of what constituted a move for the reason noted above—namely, theory suggests that financial frictions involving the likes of negative equity or mortgage lock-in should impact mobility for permanent

[2] The distinction between permanent and temporary moves will also be a data issue for researchers using household panel data sets, such as the Panel Survey on Income Dynamics. Exact property address information will be required to reliably distinguish between these two types of moves.

[3] We caution below that this does not necessarily signal that the estimated relationship between mobility and negative equity during this housing market downturn will not change as additional AHS data become available. See the discussion below for more on this topic.

[4] The FACL data used here include the impact of distressed transactions. We have experimented with a series that does not include the data, and it does not change our results.

[5] Ideally, for a residence that is owner-occupied, changes in the purchase year coincide with changes in ownership of the residence.

[6] In the current work, we also follow Schulhofer-Wohl (2011) in setting tenure to missing whenever tenure was imputed by the AHS. There were 2,183 cases in which the reported imputed tenure was reported as owner-occupied and 458 cases reported as rental.

Table 1
Mobility Measures

	1985-2007		
	Percentage Moved	Noncensored	Percentage Censored
MOVE	7.8	61,801	17.3
MOVE-ALL	16.4	68,206	8.8
MOVE1	10.0	63,700	14.8
MOVE2	11.0	64,450	13.8
	1985-2009		
	Percentage Moved	Noncensored	Percentage Censored
MOVE	7.5	66,280	17.7
MOVE-ALL	16.0	73,096	9.2
MOVE1	9.7	68,371	15.1
MOVE2	10.8	69,181	14.1

Source: U.S. Census Bureau, American Housing Survey.

Notes: Percentage moved is computed conditional on being in our final regression sample, which requires no missing data for all regressors pertaining to household and housing unit characteristics. It is the ratio of moves to the sum of moves and nonmoves. Percentage censored is the ratio of censored moves to the sum of moves, nonmoves, and censored moves.

moves. To ensure that we did not mistakenly include temporary moves (or false transitions attributable to any remaining reporting errors in the survey), we restricted our sample to those observations in which it was immediately clear either that the same household resided in the given housing unit across consecutive surveys (in which case, there was no move) or that a different household lived in *and* owned the unit that had been owned by another household in the previous survey (in which case, there was a permanent move because both physical location and economic ownership had changed).

Summary statistics of our original mobility variable, here called MOVE, are reported in the first row of Table 1. This measure is identical to the one used in our 2010 paper. Focusing initially on the top panel, which reports data for the 1985-2007 period covered in that paper, we see that 7.8 percent of the 61,801 housing transitions used in our regression analysis are moves according to this definition.[7] Those 61,801 transitions represent only 82.7 percent of the total number of observations potentially available to us.[8] That is, we treat 17.3 percent of the potential transitions as censored. In 2.4 percent of the cases, the move is censored because the observation is the last in the panel data for a particular residence. The remaining cases involve transitions of the property from ownership to rental or from ownership to vacancy where it is possible that the original owner may still own the property.

In his first and preferred mobility measure, Schulhofer-Wohl (2011) effectively counted as a move all cases in which a unit that had been owned in a given survey and was now being rented or was vacant in the subsequent survey. Using the code he provided, we created this variable in our data. It is labeled MOVE-ALL in the second row of Table 1 because it captures all transitions, whether permanent or transitory in nature. Note the much higher mobility given this definition—16.4 percent of transitions are moves, versus 7.8 percent given the definition in Ferreira, Gyourko, and Tracy (2010).[9] A much smaller fraction of the data is censored using the MOVE-ALL measure, reflecting only the 2.4 percent of cases noted earlier in which the observation is the final one in the data panel for a particular residence.

3.2 Two New Measures of Mobility that Exploit the AHS Panel Structure

Because the conservative coding approach in our 2010 study could result in dropping some permanent moves in a nonrandom way that might affect our key estimates, we develop an improved measure of mobility that uses the AHS panel structure to help mitigate this potential problem. This new variable is labeled MOVE1 in Table 1. By creating it for all cases in which the next survey indicates that the house is vacant or rented, we now look forward across all available surveys to

[7] The reported mobility rate drops from 11.4 percent in our previous work to 7.8 percent in this new estimation sample. This decline reflects the removal of false moves as a result of the additional data cleaning.

[8] There are 74,774 observations on potential transitions between 1985 and 2007 for which we have complete data on all of the control variables as well as instruments used in our regression specification reported below. The estimation sample of 61,801 is nearly identical to our earlier estimation sample of 61,803. This reflects the fact that the extra observations added to the estimation sample because of the cleaning of previously uncaught false transitions in the panel structure nearly balance the number of observations lost because of the deletion of observations with imputed tenure status.

[9] As we show, the MOVE-ALL measure would reflect even higher mobility if it literally did what Schulhofer-Wohl states in his paper (2011, p. 5): "As I explain in the introduction, FGT [Ferreira, Gyourko, and Tracy] drop from the sample all cases where a house is owner-occupied in year t but is vacant or rented in year t+2. I make only one change to FGT's data: I code those cases as moves." Our study does not actually censor all such cases. For example, if the existing owner were to temporarily leave the unit vacant or rent it out and then come back to the unit in a subsequent survey, our data set would not censor the initial observation in that sequence. Our code would recognize that the initial observation in that sequence was not the last one for the given household, and we only allow moves for the last observation on the household. By using the code from our 2010 paper, Schulhofer-Wohl effectively corrects for some temporary moves like this, so that not every case in which a "house is owner-occupied in year t but is vacant or rented in year t+2" is counted as a move in his data.

see if the house again becomes owner-occupied by another household, not just by the previous owner. If it does, we note the year in which the house was purchased. If the purchase year is between the current survey year and the next survey, we code this as a permanent move.

In the example below, the first row reports the American Housing Survey year, the second indicates tenure status (owned or rented), and the third reports the year the home was purchased by its owner.

Example 1

Survey year	2003	2005	2007	2009
Tenure status	Own	Rent	Rent	Own
Year purchased	1997	NA	NA	2004

In this case, the housing unit was owned as of 2003 by someone who purchased it in 1997. The same housing unit is reported as rented in the next two surveys. Then, the 2009 survey reports the unit as again being owned, with the owner having purchased the home in 2004. This tells us there was a permanent move by our prior owner, with the house being sold to a new owner in 2004 and that owner presumably renting it out for a period of time. In our previous coding, situations like this would have resulted in a censored value for our dependent variable in 2003, with the observation being dropped from the analysis. Our new mobility measure, MOVE1, will code this as a move for the 2003 observation.

We also take advantage of a variable in the AHS that records the vacancy status of a unit (*vacancy*) to help resolve some of the cases censored under the rules creating the MOVE mobility indicator. For example, we code MOVE1 as indicating that a move and sale took place if the vacancy variable indicates that the house has been "sold but not yet occupied" (*vacancy* = 5). We code MOVE1 to indicate that the original owner has not moved if the unit is listed as being held for occasional use, seasonal use, or usual residence elsewhere (*vacancy* = 6-11). Each of these instances suggests the presence of multiple homes for the household, so that one should not interpret a transition as a permanent move and sale of the property. We also code MOVE1 to indicate that the unit has not sold if the unit is listed as noncash rent for one or more surveys followed by owner-occupied status with the purchase year outside of the window between survey years. Finally, we code MOVE1 to indicate that a move and sale have not taken place if the unit is vacant for two consecutive surveys and listed as sold but not occupied in the second survey (*vacancy* (t+2) = 5).

Table 1 shows that resolution of previously censored cases in this manner results in 10 percent of our regression sample transitions now being coded as permanent moves. MOVE1 mobility is much higher than MOVE, by 28 percent, but it remains well below that for MOVE-ALL. We discuss the differences across measures more fully below; but first, we introduce another mobility variable, MOVE2.

For MOVE2, we maintain the requirement that we are certain that the household has permanently moved, but relax the restriction that we know that the house has sold in the interval between the relevant surveys. Naturally, this leads to an even higher percentage of transitions being classified as permanent moves, as indicated in this second example.

Example 2

Survey year	2003	2005	2007	2009
Tenure status	Own	Rent	Rent	Own
Year purchased	1997	NA	NA	2008
MOVE1	Censored	NA	NA	
MOVE2	Yes	NA	NA	

In this case, we cannot tell if the owner in 2003 changed residence and sold the property between 2003 and 2005. It is possible that a move and sale did take place and that the new owner decided to rent out the property until 2008, when the property was resold. That new owner then decides to live in the property and reports a purchase year of 2008 in the 2009 AHS. However, it is also possible that the owner in 2003 decided to move and to rent out the property, becoming an absentee landlord. The house is then sold in 2008. Since both situations are consistent with the reported data, this would result in MOVE1 being censored and recorded as missing. However, in MOVE2 we classify this as a move in 2003 because we know that the original owner moved and did not return to the property. Thus, MOVE2 includes cases in which we know there was a permanent move, but cannot resolve the timing of the sale by the original owner. The last row of the top panel of Table 1 shows that the fraction of MOVE2 transitions is 10 percent higher than for MOVE1 (11.0 percent versus 10.0 percent). Still, this more expansive definition does not generate anything close to the level of mobility indicated by MOVE-ALL.

The bottom panel of Table 1 reports the analogous data for each mobility measure for the full sample that includes the 2009 survey data. Note that mobility is lower for each variable, which indicates that measured mobility declined between the 2007 and 2009 surveys. We exploit this issue in more detail below.

3.3 Trade-Offs across Different Measures of Mobility

Our concern about Schulhofer-Wohl's (2011) empirical strategy for the question we are addressing is that several of the

housing transitions that he considers moves are false positives in the sense that they are temporary or reflect coding errors in the underlying survey. To gauge how serious the potential problem is of conflating these types of moves, we evaluated the likelihood of Type I and Type II coding errors in his mobility measure by coding them in "real time" in the AHS data. That is, we begin by reading in the cleaned panel and selecting observations for 1985 and 1987. We then code MOVE-ALL based on his code for 1985 using data from the 1985 and 1987 surveys. These values for MOVE-ALL are saved and the exercise is repeated using the 1987-89 pair of surveys, the 1989-91 pair, and so on, until 1997-99. We end this exercise in 1999 to ensure that we have enough future surveys to assess whether Schulhofer-Wohl's moves turned out to be permanent or temporary. We call this real-time version of the Schulhofer-Wohl mobility measure MOVE-ALL^R.

It is important to note that the coding of MOVE-ALL^R in this real-time analysis differs from the coding of MOVE-ALL in the estimation sample. Our third example illustrates why.

Example 3:

Survey year	2003	2005	2007
Tenure status	Own	Rent	Own
Year purchased	1997	NA	1997

When the 2003 AHS data are added to the estimation sample, MOVE (and our two other mobility measures), MOVE-ALL, and MOVE-ALL^R for 2003 will all be censored because at that time this is the last observation in the panel for the residence. When the 2005 AHS data are added, MOVE for 2003 will remain censored and MOVE-ALL and MOVE-ALL^R for 2003 will be recoded as a move. However, when the 2007 AHS data are merged into the sample, MOVE for 2003 (as well as MOVE1 and MOVE2) will be recoded from censored to a nonmove, while MOVE-ALL for 2003 will be recoded from a move to a nonmove and MOVE-ALL^R for 2003 will remain coded as a move (since we do not allow the real-time measure to be recoded once it indicates that a move has taken place). The reason for the recoding of MOVE and MOVE-ALL is that when constructing these mobility measures, we sort the data by residence, household (based on a unique household identification number we create), and survey year. Based on the sorted data, a move is only considered for the last observation for that household. As a result, our coding strategy for MOVE (as well as for MOVE1 and MOVE2) only recodes censored observations as either nonmoves or moves and it never recodes noncensored mobility observations. In contrast, the coding for MOVE-ALL can be dynamically inconsistent over time, with moves recoded at a later date as nonmoves. By construction, MOVE-ALL^R maintains dynamic consistency by not recoding a move as a nonmove even when information becomes available indicating that the original owner has returned.

Table 2
Permanent versus Temporary Moves

Cross-Tabulation of MOVE2 with MOVE-ALL^R

		MOVE-ALL^R	
		0	1
MOVE2	0	70,707	3,557
	1	0	8,550
	. (missing)	0	5,050

Percentage of False Positives Resolved over Time

Four years or first subsequent survey	66.0
Six years or second subsequent survey	17.4
Eight years or third subsequent survey	7.7
Ten years or fourth subsequent survey	4.7
Twelve years or fifth subsequent survey	1.9
Fourteen years or sixth subsequent survey	1.2
Sixteen-plus years	1.1

Source: U.S. Census Bureau, American Housing Survey, 1985-2009.

Note: 15.1 percent of false positives are resolved using vacancy status.

The top panel of Table 2 reports cross-tabulations of our MOVE2 indicator, which takes full advantage of the panel to differentiate between permanent and temporary transitions, and MOVE-ALL^R.[10] We use MOVE2 for this analysis since our focus here is whether a move is permanent or not, regardless of when the property was sold. The first column of the table documents that these two mobility variables confirm that there were 70,707 cases in which no move occurred. There are no cases in which our MOVE2 measure considered some transition a move when MOVE-ALL^R did not (that is, there is no evidence of Type II errors); nor is MOVE2 ever censored or missing when MOVE-ALL^R indicates that no move took place.

The table's second column is more interesting because both mobility measures have 8,550 moves, but MOVE-ALL^R has an additional 8,607 moves. Moreover, 41.3 percent (3,557/8,607) of the additional moves in MOVE-ALL^R turn out to be temporary in nature because they reflect Type I errors. That is, using the full panel of surveys up to 2009, we observe the owner return to the unit at some point in the future, or the surveys reflect some other trait that leads us to conclude that there has not been a permanent move.[11]

[10] Here, we use all available transitions from the AHS for owner-occupied residents between twenty-one and fifty-nine years of age over the 1985-99 period and do not restrict the observations to those with nonmissing values for all of the regressors that we use in the final mobility estimation.

Out of all the false positives from MOVE-ALL^R, in two-thirds of the cases the Type I error could be eliminated by looking at only one subsequent American Housing Survey, as shown in the bottom panel of Table 2. To better understand this, presume that we are uncertain about whether a transition in the 1985 data is permanent or temporary. That is, the data clearly show a given owner-occupant in 1985, but a different occupant or a reported vacancy in 1987. In 66 percent of cases, the 1989 survey fully resolves the uncertainty. In these "false positive" cases, we see the same household living and owning the same unit in 1985 and 1989. Another 17.4 percent of the false positives are resolved by the next available survey (that is, after six years have passed), so that more than 83 percent of cases are clarified by 1991 in this example. The remaining cases are clarified by future surveys, with some owners being absent for long periods of time. However, the number of those cases is quite small.[12]

It is also important to note that for 5,050 transitions, MOVE2 is assigned a censored value while MOVE-ALL^R considers them moves. While none of these cases can be definitively identified as permanent moves with the currently available data, some of them undoubtedly are and will be revealed and coded as such over time as additional survey data become available. In practice, this means that MOVE2 still does not include all true permanent moves. This highlights the fact that there is no perfect measure of such mobility as long as the data do not allow for the immediate recognition of whether an economic change in ownership has occurred.

4. Results

4.1 Estimation Methodology

In Ferreira, Gyourko, and Tracy (2010), we showed that each of our financial friction variables, which are based on self-reported values, is subject to substantial measurement error that causes severe attenuation bias in estimated mobility effects.[13] Such measurement error can be mitigated by using an instrumental-variable approach.[14] In the case of house equity variables, we use the purchase price of the house and any house price appreciation implied by the First American-Core Logic repeat-sales house price index for the relevant metropolitan area in order to calculate our instrument for the self-reported measure of negative equity. The instrumental variable for mortgage lock-in is based on the average rate on thirty-year fixed-rate mortgages during the year in which the house was purchased for the self-reported interest rate. The real annual difference in mortgage payments is calculated using the difference between this rate and the prevailing mortgage rate variable. In both cases, our instrument relies on the intuition that aggregate information averages out individual-level measurement error.

The Proposition 13 property tax subsidy variable is constructed from two self-reported variables. To address the likely measurement error, we create an instrument defined as the difference between the growth in the metropolitan area repeat-sales house price index and the maximum allowable growth in the property tax over the same period, all multiplied by the fully assessed property tax on the purchase value of the house. Needless to say, the value of the implied subsidy still is zero for non-California households.

To accommodate our data structure, we use a recursive mixed-process model that expands upon the classic mobility specifications introduced by Hanushek and Quigley (1979) and Venti and Wise (1984), which also served as the foundation for our earlier empirical work. The following four-equation system describes our mobility outcome and our three instrumental variables:

$$
\begin{aligned}
I^*_{mi} &= X_i\beta + \beta_{P13}X_{P13i} + \beta_{FRM}X_{FRMi} + \beta_N I^1_{Ni} + \varepsilon_{1i} \\
X_{P13i} &= X_i\alpha + \alpha_{P13}Z_{P13i} + \alpha_{FRM}Z_{FRMi} + \alpha_N I^2_{Ni} + \varepsilon_{2i} \\
X_{FRMi} &= X_i\gamma + \gamma_{P13}Z_{P13i} + \gamma_{FRM}Z_{FRMi} + \gamma_N I^2_{Ni} + \varepsilon_{3i} \\
I^{*1}_{Ni} &= X_i\delta + \delta_{P13}Z_{P13i} + \delta_{FRM}Z_{FRMi} + \delta_N I^2_{NI} + \varepsilon_{4i}
\end{aligned}
$$

$$
\begin{aligned}
I_{mi} &= 1 \text{ if } I^*_{mi} \geq 0 \\
&\quad\ 0 \text{ otherwise} \\
I^1_{Ni} &= 1 \text{ if } I^{*1}_{Ni} \geq 0 \\
&\quad\ 0 \text{ otherwise}
\end{aligned}
$$

[11] As noted above, the lack of dependent coding for this variable means that some of these cases could be attributable to coding error by the AHS survey taker in the sense that he or she does see or interview the original owner and mistakenly concludes that the unit is not occupied by the same person. The best example of this involves units described as being vacant and held for occasional or seasonal use. This group represents 14 percent of the 3,557 cases. There is a much smaller fraction of units (1.2 percent) for which there is noncash rent and a subsequent sale outside the relevant sample interval. There is an even smaller share of units (0.3 percent) that are vacant across two consecutive surveys, with the second survey listing the housing unit as sold but not yet occupied.

[12] Subsequent to a temporary move, the mean (median) duration of the owner in the residence is 6.1 (5.0) years. In 38 percent of cases, the post-temporary move duration is censored by the end of the data in 2009.

[13] Kain and Quigley (1972) is the seminal work on this issue. More recently, Bayer, Ferreira, and McMillan (2007) observe that self-reported values are less accurate the longer ago the occupant moved in. Hence, wide swings in prices like those seen over our sample period increase the dispersion of self-reported home values. Schwartz (2006) also reports measurement error in interest rates.

[14] See Ashenfelter and Krueger (1994) for a classic reference on how to create an alternative measure of the "treatment" variable of interest, and then to use that measure as the instrumental variable.

$$\begin{bmatrix} \varepsilon_{1i} \\ \varepsilon_{2i} \\ \varepsilon_{3i} \\ \varepsilon_{4i} \end{bmatrix} \sim N(0, \Sigma), \text{ where } \Sigma = \begin{bmatrix} 1 & \sigma_{12} & \sigma_{13} & \sigma_{14} \\ \bullet & \sigma_2^2 & \sigma_{23} & \sigma_{24} \\ \bullet & \bullet & \sigma_3^2 & \sigma_{34} \\ \bullet & \bullet & \bullet & 1 \end{bmatrix},$$

where I_{mi} is our observed mobility indicator, I_{Mi}^{*} a continuous latent index for the propensity to move, I_{Ni}^{1} our negative equity indicator based on the self-reported house value, I_{Ni}^{2} our alternative negative equity indicator based on the metro area house price index, I_{Ni}^{*1} a continuous latent index for whether the borrower is in negative equity, Z_{P13i} our instrument for the annual property tax cost of moving attributable to Proposition 13 for California residents, and Z_{FRMi} our instrument for the annual interest rate cost associated with refinancing for households with a fixed-rate mortgage.

We estimate this system using Roodman's Cmp program in STATA. A description of the program, its implementation, and applications is given in Roodman (2009). For comparison with our earlier findings, we also present results for a single-equation Probit (used in Ferreira, Gyourko, and Tracy [2010]) and a standard linear-probability model.[15]

4.2 Negative Equity

In this section, we first present updated results on the relationship between mobility and negative equity using new data from the 2009 AHS and for the five different mobility variables described above. For the rest of our discussion, we code MOVE-ALL^{R} for the full sample period from 1985 to 2007 or to 2009. Table 3 begins by providing summary statistics on the distribution of self-reported negative equity according to whether there was a move. Table 4 then reports the results of re-estimating the core mobility specification from Ferreira, Gyourko, and Tracy (2010) using the five mobility measures described above as the dependent variable. The top panel of Table 4 reports marginal effects from that specification estimated with the cleaned and edited AHS data from 1985 to 2007. Results for the expanded 1985-2009 AHS data are reported in the bottom panel.

[15] Schulhofer-Wohl (2011) correctly notes that our negative equity indicator was a dichotomous dummy and thus did not have the requisite properties for the IV Probit estimation procedure as carried out in our 2010 study. Consequently, our main results of this update are based on the IV Probit marginal effects from the joint estimation of the four-equation system outlined above. For comparison, we also report estimates from a single-equation IV Probit (used in our previous paper) as well as an IV linear-probability version of the model, with those results reported in the second and third columns of Table 4. Schulhofer-Wohl does not instrument for the measurement error. As our paper showed, there is never any significant correlation between a financial friction and permanent moves unless attenuation bias is dealt with in some fashion.

Table 3
Cross-Tabulations of Negative Equity and Mobility Indicators

	Negative Equity	
Mobility Indicator	No	Yes
MOVE		
No	74.02	2.11
Yes	6.05	0.15
Censored	16.22	1.46
MOVE1		
No	74.51	2.14
Yes	8.04	0.23
Censored	13.74	1.34
MOVE2		
No	74.51	2.14
Yes	8.99	0.28
Censored	12.79	1.29
MOVE-ALL		
No	74.02	2.11
Yes	14.15	0.51
Censored	8.12	1.09
MOVE-ALL^{R}		
No	68.60	1.91
Yes	14.71	0.57
Censored	12.98	1.23

Source: U.S. Census Bureau, American Housing Survey (1985-2009).

Notes: Negative equity is based on self-reported house values. MOVE-ALL^{R} is the real-time calculation of MOVE-ALL over the full sample period in which we do not allow moves to be subsequently recoded as nonmoves. Cell percentages are shown.

Focusing first on the multi-equation Probit marginal effects in column 1, we observe a statistically significant negative relationship between the presence of negative equity and mobility for our original MOVE indicator as well as for our improved MOVE1 indicator. For our earlier sample period from 1985 to 2007, our preferred MOVE1 indicator implies that negative equity is associated with a two-year mobility rate that is 3 percentage points lower, *ceteris paribus*. This is 30 percent of the baseline mobility rate of 10 percent, which is similar to the relative impact reported in Ferreira, Gyourko, and Tracy (2010). The MOVE variable used in our earlier paper generates a slightly larger impact, but it is not statistically or economically different from that for MOVE1. The more expansive definition of permanent mobility reflected in MOVE2 yields a slightly lower marginal effect of 2.8 percentage points, or about one-fourth of the baseline mobility rate. It is different from zero at a 10 percent confidence level for the

Table 4
Empirical Estimates

	1985-2007		
	IV Probit (Multi-Equation)	IV Probit (Single-Equation)	IV Linear Probability
MOVE	-0.043**	-0.050**	-0.062**
N=61,801	(0.012)	(0.014)	(0.017)
MOVE1	-0.030**	-0.047**	-0.056**
N=63,700	(0.014)	(0.016)	(0.019)
MOVE2	-0.028*	-0.047**	-0.043**
N=64,450	(0.015)	(0.020)	(0.020)
MOVE-ALL	0.019	0.029	0.029
N=68,206	(0.021)	(0.024)	(0.024)
MOVE-ALL^R	0.029	0.063**	0.061**
N=64,181	(0.021)	(0.029)	(0.029)
	1985-2009		
MOVE	-0.037**	-0.046**	-0.054**
N=66,280	(0.011)	(0.017)	(0.016)
MOVE1	-0.024*	-0.044**	-0.048**
N=68,371	(0.014)	(0.016)	(0.018)
MOVE2	-0.022	-0.037**	-0.036*
N=69,181	(0.014)	(0.017)	(0.019)
MOVE-ALL	0.027	0.032	0.035
N=73,096	(0.018)	(0.023)	(0.023)
MOVE-ALL^R	0.037*	0.066**	0.066**
N=69,079	(0.020)	(0.027)	(0.027)

Source: U.S. Census Bureau, American Housing Survey.

Notes: Probit marginal effects are average differences. Standard errors are in parentheses. MOVE-ALL^R is the real-time version of MOVE-ALL over the full sample period in which we do not allow moves to be subsequently recoded as nonmoves.

** Statistically significant at the 95 percent confidence level.
* Statistically significant at the 90 percent confidence level.

1985-2007 sample, and we cannot reject the null hypothesis that the effects are the same across all three measures.
A comparison of results across columns in the top panel of Table 4 indicates that implied marginal effects from the multi-equation Probit specification are consistently lower than effects from the single-equation Probit and the linear-probability specifications, although the pattern of findings is quite consistent. In addition, the standard errors are such that we cannot conclude that the levels of the implied effects differ by estimation strategy.

The first column of Table 4's second panel adds in the data from the 2009 survey. We find modestly lower marginal effects here compared with the 1985-2007 results, and negative equity is no longer associated with statistically significant lower mobility for the MOVE2 variable. However, these marginal effects are not significantly different from those of the earlier sample period, so there is no evidence yet that the most recent housing bust has materially changed the relationship between negative equity and owner mobility. That said, one cannot and should not conclude that the relationship will not change over this cycle as more data become available, as cautioned in our original paper. The previous section implies that it takes four to six years for the vast majority of the censored housing transitions to be resolved. Hence, it will be much later in this decade before we can more confidently know how negative equity affected permanent mobility in this latest downturn.

Note that the coefficient on the MOVE-ALL indicator as constructed by Schulhofer-Wohl (2011) suggests a positive correlation between negative equity and mobility. In neither sample period is this statistically different from zero, but the point estimates are positive, not negative. The misclassification of so many temporary moves as permanent ones is likely to be critical here. Recall that theory does not suggest a negative correlation between temporary moves and negative equity. Hence, it should not be surprising to find a weak and imprecise correlation when more than one-fifth of the coded moves may not involve a permanent move and sale of the home.[16]

This intuition that the conflation of temporary and permanent moves is the driving factor behind the difference between our negative equity results and those reported by Schulhofer-Wohl is corroborated by comparing the different estimates associated with MOVE-ALL and MOVE-ALL^R. Recall that the distinction between these two measures is that MOVE-ALL^R retains moves identified by Schulhofer-Wohl that are known ex post to be temporary, whereas MOVE-ALL allows these temporary moves to be recoded as nonmoves. Retaining these temporary moves increases the measured mobility rate from 16.1 percent for MOVE-ALL to 17.8 percent for MOVE-ALL^R. The estimates in Table 4 indicate that the inclusion of these additional temporary moves raises in each case the estimated positive effect of negative equity on mobility.

Of course, the underlying sample used in generating these estimates is the result of censoring all cases in which we cannot tell whether physical location and economic ownership

[16] We also estimated all models with the original FHFA price series used to help determine negative equity. Focusing on the system IV Probit results, we note that MOVE-ALL remains positive but is still statistically insignificant. MOVE continues to be positive and statistically significant. The marginal effects for MOVE1 and MOVE2 decline by around 25 percent for the 1985-2007 sample and around 40 percent for the 1985-2009 sample, and they are no longer statistically significant. This drop in the magnitude of marginal effects likely reflects the inability of the FHFA house price indexes to accurately track the declining prices due to the indexes' narrow focus on houses financed with conforming mortgages.

Table 5

Impact of Other Financial Frictions on Household Mobility

	IV Probit (Multi-Equation)	IV Probit (Single-Equation)	IV Linear Probability
Mobility indicator: MOVE1			
Fixed-rate mortgage lock-in ($1,000)	-0.016**	-0.018*	-0.013
	(0.009)	(0.009)	(0.009)
Proposition 13 property tax lock-in ($1,000)	-0.010**	-0.010**	-0.008**
	(0.005)	(0.004)	(0.004)
Mobility indicator: MOVE2			
Fixed-rate mortgage lock-in ($1,000)	-0.023**	-0.024**	-0.019**
	(0.009)	(0.009)	(0.009)
Proposition 13 property tax lock-in ($1,000)	-0.009*	-0.009*	-0.008**
	(0.005)	(0.005)	(0.004)

Source: U.S. Census Bureau, American Housing Survey, 1985-2009.

Note: Probit marginal effects are average derivatives, with standard errors in parentheses.

** Statistically significant at the 95 percent confidence level.
* Statistically significant at the 90 percent confidence level.

changed. That is roughly half of the excess moves in MOVE-ALL^R relative to MOVE2 based on our real-time analysis of the 1985-99 period. Practically speaking, most of the censored cases in our full data set are from recent waves of the AHS, and Table 2's results suggest that if past patterns persist, the vast majority will be resolved within four to six years. However, it seems likely that at least some of the cases in which the previous owner is coded as no longer living in the unit over multiple surveys, but for which there is still no clear evidence of a sale, actually are permanent moves.[17]

[17] This raises the question of whether we could improve the measure MOVE2 by counting as moves situations in which it seems likely (but not certain) that a permanent move has taken place. Intuition might suggest that the longer the ownership gap observed in which the residence is reported as rental or vacant, the more likely that the previous owner will not return. To check on this possibility, we looked at ownership gaps of different lengths and computed the fraction of cases in which the move turned out to be temporary, conditional on having the information to make this determination. For situations in which the residence was rented or vacant for at least three surveys, the transition turned out to be temporary in 59 percent of the cases in which we could determine the final outcome. If we lengthen the ownership gap to four or more surveys, the percentage of temporary moves actually increases to 62 percent. This pattern continues for ownership gaps of five or more and six or move surveys. Thus, the simple intuition that the longer the current ownership gap, the more likely the move will turn out to be permanent, is not supported in the data. For this reason, we do not think one can improve on MOVE2 by recoding censored transitions as moves given an ownership gap of some specified length. However, it is still useful to understand that the potential fragility of our results (and possibly of previous researchers) arises from the fact that it is difficult to properly measure mobility in a number of cases.

4.3 Fixed-Rate Mortgages and Property Tax Lock-Ins

Updated results on the impact of two additional financial frictions on household mobility are presented in Table 5. The first friction pertains to homeowners with a fixed-rate mortgage. In a rising interest rate environment, if a homeowner with this type of mortgage moves, the monthly cost of an identically sized mortgage can be higher. The second friction pertains to homeowners in California whose property tax increases have been limited over time due to Proposition 13. If the homeowner moves to a similarly valued property, taxes would be set to the fully assessed value of the house. In both cases, we examine the marginal effect of an additional $1,000 annual cost on the likelihood that the household moves. We provide estimates for specifications containing our two improved mobility indicators for the expanded sample period, in which we use the FACL overall house prices to update home values. The data confirm our earlier finding that both frictions give rise to reduced household mobility—10 percent to 16 percent less per $1,000 using our preferred mobility measure MOVE1. In none of the specifications do the data reject the notion that the mobility friction is the same whether it is generated by rising rates for fixed-rate borrowers or higher property taxes for California homeowners.

We suspect that this interest-rate–related lock-in effect will become increasingly important as monetary policy is normalized in the future. To illustrate, we consider the

Table 6
Main Reason for Move: Overall and by Distance of Move

		1985-95				
Reason	1985-2009	All	Same Metropolitan Statistical Area	Same State	Different State	Out of Country
Job-related	12.58	13.23	3.85	21.20	60.53	66.10
Quality-of-life	26.70	23.94	26.67	24.97	8.18	3.39
Personal/family	23.88	20.44	19.73	16.64	10.22	6.78
Financial	21.83	25.55	33.00	20.55	4.25	6.78
Other	11.84	13.18	12.90	13.13	14.94	15.25
All equal	3.17	3.67	3.85	3.51	1.89	1.69

Sources: U.S. Census Bureau, American Housing Survey; authors' calculations.

Note: The sample is restricted to owner-occupied respondents between the ages of twenty-one and fifty-nine.

hypothetical case of a 250 basis point increase in the average thirty-year fixed-rate mortgage interest rate as a result of the normalization of monetary policy. For homeowners in 2009 with a fixed-rate mortgage, this results in a mean (median) annual payment difference of $2,300 ($1,710). According to the Probit marginal effects for MOVE1, this implies a mean (median) reduction in the two-year mobility rate of 3.7 (2.7) percentage points. If we calculate using the estimates for MOVE2, we obtain a reduction in the two-year mobility rate of 5.3 (3.9) percentage points. This suggests that as negative equity (hopefully) diminishes in importance over the coming years, it well may be offset by an increasing fixed-rate-mortgage friction.[18]

5. Spillovers into the Labor Market and Other Implications

Policymakers naturally have been interested in whether reduced mobility among homeowners (from negative equity especially) might be playing a role in what has heretofore been a very sluggish employment recovery. Perhaps being stuck in one's home because of the high costs of curing negative equity prevents a sufficiently large number of people from moving to accept jobs, which affects the measured unemployment rate.

Our analysis is restricted to the housing market because the AHS follows residences rather than households and therefore it is not suited to addressing job mobility. However, the preliminary answer on this question from the initial set of research in labor economics is "no." Since long-distance moves are more likely to be job related, these studies tend to focus on moves across states or counties.[19] The AHS files are also useful for examining the types of moves likely to be impacted by housing market frictions. For example, the AHS asks recent movers (that is, those who moved within the last two years) about the primary reason for their move and, until 1995, the distance of the move. A high percentage of moves—73 percent—are local, while only 13 percent cross a state border. Table 6 provides more detail on the primary reason for moves, both overall and broken down by distance. Most moves are for quality-of-life, personal/family, and financial reasons, and do not appear to be primarily job related. This is especially true for local moves. In contrast, longer-distance moves, particularly across states, tend to be job related. One implication of these data that is consistent with the initial labor market analysis results is that financial frictions affecting household mobility may well be more likely to reduce local moves that need not have significant spillover effects into the labor market. Nevertheless, it is too early to conclude that this is the final word on potential spillovers into the labor market. That conclusion should await a fuller recovery as well as confirming evidence from studies using micro data and modeling individual household behavior.

We emphasize that even if reduced mobility attributable to financial frictions has no spillovers into the labor market, that does not make them economically unimportant. The fewer

[18] This is particularly true for borrowers who received a below-market mortgage rate through a private modification or a Home Affordable Modification Program modification (conditional on the borrower not redefaulting on the modified mortgage). If these low-rate mortgages were either assumable or portable, there would be no associated mobility friction.

[19] Several of these papers (for instance, Aaronson and Davis [2011], Modestino and Dennett [2012], and Molloy, Smith, and Woznak [2011]) also estimate aggregate models of migration rates rather than micro models of whether a household moves. Donovan and Schnure (2011) also pursue an aggregate-level analysis, but theirs is more comprehensive in the sense that it investigates the impact of negative equity within and across counties (and also within and across states).

within-metropolitan-area moves that we see due to negative equity have direct effects on owner economic welfare and potentially important implications for the nature of the housing sector recovery. Being locked into one's current residence because of the high costs of curing negative equity means the household is imperfectly matched in its residence. The welfare losses from being mismatched are not just from having the wrong-sized house (such as not enough bedrooms now that there is an additional child), but also from being in the wrong location. Many families, for instance, may not be able to move to their preferred school district, even if there is no desire to change jobs.

In addition to these welfare consequences are the potential impacts on the scale and intensity of trade-up (and trade-down) purchases. There are vastly more sales of existing homes than new homes in a typical year, so lower transaction levels in the existing stock materially affect the state of the housing market, including the incomes of realtors and others who work in the housing sector and in durable goods sales that coincide with turnover of owned housing, as well as the finances of many state and local governments that rely on transfer taxes.[20]

Finally, it is natural to focus on the potential ramifications of lower mobility due to negative equity, but we should not forget that the mortgage interest rate lock-in effect could become much more important in the future. We find economically meaningful interest rate lock-in effects in past cycles, and the stage is set for them to become empirically relevant. Federal Reserve interest rate policy and other public policies have been successful at encouraging refinancing at historically low rates. When rate policy normalizes, we may find many owners constrained from moving because of the much higher debt service payments they would incur from buying a different home.

[20] Low transaction volumes in housing markets also complicate the appraisal process because of a lack of comparables. This likely leads to conservative appraisals and therefore the need for households to make larger down-payments in order to purchase a home. This creates the possibility of an adverse-feedback effect that can further reduce home sales.

6. Summary and Implications for Future Research

Our inclusion of the most recent American Housing Survey for 2009, which reflects initial data from the recent housing bust, does not materially change previously reported estimates of how negative equity and other financial frictions are correlated with homeowner mobility. Homeowners with negative equity remain about one-third less likely to move than otherwise observationally equivalent owners. However, the uncertainty surrounding changes in economic ownership involving various transitions concentrated in the last few surveys suggests that we cannot really know for sure how the recent housing bust impacted permanent mobility until a few years into the future. Then, the additional survey data will reveal the true nature of many of those transitions.

A critique of the sample selection procedures used in our earlier work (Ferreira, Gyourko, and Tracy 2010), which claims to reverse this result, appears largely due to the incorrect classification of many transitions as moves that are likely to be temporary and not permanent, or simply reflect coding error in the individual surveys. Whether negative equity can be positively associated with temporary moves is a question that we did not attempt to answer then. That said, our improved measure still does not reflect mobility perfectly because of our conservative policy of censoring transitions that cannot be definitively defined as permanent in nature. Hopefully, researchers will develop other data sources or ways to reduce this noise in the AHS panels.

Going forward, it is more important for scholars to tackle the question of whether this correlation is causal in nature. That will require new theoretical and empirical strategies to better control for labor market conditions. As long as labor and housing markets move together (and there is sound reason conceptually and empirically to believe they do), the correlation documented here could be driven predominantly by the lack of good job opportunities to attract potential movers. Until we address this issue, we will not know the true social cost of highly leveraged home purchases that are more likely to lead to negative equity situations.

References

Aaronson, D., and J. Davis. 2011. "How Much Has House Lock Affected Labor Mobility and the Unemployment Rate?" Federal Reserve Bank of Chicago, Chicago Fed Letter no. 290, September.

Ashenfelter, O., and A. B. Krueger. 1994. "Estimates of the Economic Returns to Schooling from a New Sample of Twins." American Economic Review 84, no. 5 (December): 1157-73.

Bayer, P., F. Ferreira, and R. McMillan. 2007. "A Unified Framework for Measuring Preferences for Schools and Neighborhoods." Journal of Political Economy 115, no. 4 (August): 588-638.

Bricker, J., and B. Bucks. 2011. "Negative Home Equity, Economic Insecurity, and Household Mobility: Evidence from the 2007-2009 SCF Panel." Paper presented at the Federal Reserve Bank of St. Louis Applied Micro Economics Conference, St. Louis, Missouri, May 5-6.

Chan, S. 2001. "Spatial Lock-In: Do Falling House Prices Constrain Residential Mobility?" Journal of Urban Economics 49, no. 3 (May): 567-86.

Donovan, C., and C. Schnure. 2011. "Locked in the House: Do Underwater Mortgages Reduce Labor Market Mobility?" Unpublished paper, May 31.

Engelhardt, G. V. 2003. "Nominal Loss Aversion, Housing Equity Constraints, and Household Mobility: Evidence from the United States." Journal of Urban Economics 53, no. 1 (January): 171-95.

Ferreira, F. 2010. "You Can Take It with You: Proposition 13 Tax Benefits, Residential Mobility, and Willingness to Pay for Housing Amenities." Journal of Public Economics 94, nos. 9-10 (October): 661-73.

Ferreira, F., J. Gyourko, and J. Tracy. 2010. "Housing Busts and Household Mobility." Journal of Urban Economics 68, no. 1 (July): 34-45.

Hanushek, E. A., and J. M. Quigley. 1979. "The Dynamics of the Housing Market: A Stock Adjustment Model of Housing Consumption." Journal of Urban Economics 6, no. 1 (January): 90-111.

Kain, J. F., and J. M. Quigley. 1972. "Note on Owner's Estimate of Housing Value." Journal of the American Statistical Association 67, no. 340 (December): 803-6.

Modestino, A. S., and J. Dennett. 2012. "Are American Homeowners Locked into Their Homes? The Impact of Housing Market Conditions on State-to-State Migration." Federal Reserve Bank of Boston Working Paper no. 12-1, February. Available at http://www.bostonfed.org/economic/wp/wp2012/wp1201.pdf.

Molloy, R., C. L. Smith, and A. Woznak. 2011. "Internal Migration in the United States." Journal of Economic Perspectives 25, no. 3 (summer): 173-96.

Quigley, J. M. 1987. "Interest Rate Variations, Mortgage Prepayments, and Household Mobility." Review of Economics and Statistics 69, no. 4 (November): 636-43.

———. 2002. "Homeowner Mobility and Mortgage Interest Rates: New Evidence from the 1990s." Real Estate Economics 30, no. 3 (fall): 345-64.

Roodman, D. 2009. "Estimating Fully Observed Recursive Mixed-Process Models with Cmp." Center for Global Development Working Paper no. 168, April.

Schulhofer-Wohl, S. 2011. "Negative Equity Does Not Reduce Homeowners' Mobility." NBER Working Paper no. 16701, January. Available at http://www.nber.org/papers/w16701.

Schwartz, A. 2006. "Household Refinancing Behavior in Fixed-Rate Mortgages." Unpublished paper, Harvard University, November/December.

Stein, J. C. 1995. "Prices and Trading Volume in the Housing Market: A Model with Down-Payment Effects." Quarterly Journal of Economics 110, no. 2 (May): 379-406.

Valletta, R. G. 2010. "House Lock and Structural Unemployment." Paper presented at the Federal Reserve System Committee on Regional Analysis, New Orleans, Louisiana, November 14-16.

References (Continued)

Venti, S. F., and D. A. Wise. 1984. "Moving and Housing Expenditure: Transaction Costs and Disequilibrium." Journal of Public Economics 23, nos. 1-2 (February-March): 207-43.

Wasi, N., and M. J. White. 2005. "Property Tax Limitations and Mobility: Lock-In Effect of California's Proposition 13." Brookings-Wharton Papers on Urban Affairs 2005, 59-97.