Federal Reserve Bank of New York
Staff Reports

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Staff Report no. 526
November 2011

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JEL classification: R21, R31, R38

Abstract

This paper provides updated estimates of the impact of three financial frictions—negative equity, mortgage lock-in, and property tax lock-in—on household mobility. We add the 2009 wave of the American Housing Survey (AHS) to our sample and also create an improved measure of permanent moves in response to Schulhofer-Wohl's (2011) critique of our earlier work (2010). Our updated estimates corroborate our previous results: Negative equity reduces household mobility by 30 percent, and $1,000 of additional mortgage or property tax costs reduces household mobility by 10 to 16 percent. Schulhofer-Wohl's finding of a slight positive correlation between mobility and negative equity appears due to a large fraction of false positives, as his coding methodology has the propensity to misclassify almost half of the additional moves it identifies relative to our measure of permanent moves. This also makes his mobility measure dynamically inconsistent, as many transitions originally classified as a move are reclassified as a nonmove when additional AHS panels become available. We conclude with directions for future research, including potential improvements to measures of household mobility.

Key words: negative equity, mobility

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I. Introduction

A long literature in housing economics has noted that a rise in mortgage rates could 'lock-in' an owner to her current house, thereby slowing or preventing a permanent move to a new residence if mortgage interest rates had risen sufficiently to make the new debt service payment unaffordable (Quigley (1987, 2002)). Other financial frictions, such as the one arising from California's Proposition 13 property tax rules, 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, also could reduce household mobility if the owner does not have enough 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), and 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 financial frictions involves permanent moves in which both physical location and economic ownership changes for the previous owner. The housing literature on financial frictions does not have clear implications for temporary moves in which the household leaves the house for a period of time – perhaps to rent out the house – and returns at a later date. Overall mobility reflects both permanent and temporary moves. The mobility measure of interest depends on the question being addressed. Given our underlying research question, when we refer to mobility in this paper, we mean mobility arising from permanent moves.

Interest in the relationship between the mobility of homeowners and financial frictions, especially those associated with negative home equity, was piqued both for researchers and

policy makers 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, we used the *American Housing Survey* (*AHS*) panel from 1985-2007 to provide new and more general estimates for the nation (Ferreira, Gyourko and Tracy (2010); FGT (2010) hereafter) that include all three forms of financial frictions in the same econometric specification. FGT's (2010) three primary results were as follows: (a) owners with negative equity were one-third less likely to move than otherwise observationally equivalent owners without negative equity; (b) for every $1,000 in higher mortgage debt service costs, mobility was about 12% lower; c) similar increases in property tax costs from Proposition 13 in California also reduced mobility by about 12%.

This note updates our previous work in two important ways. It adds data from the most recent 2009 *AHS* survey, providing the first evidence from the beginning of the bust in home prices in many markets. Second, we address Schulhofer-Wohl's (2011) criticism of our sample selection procedures, which he claimed underreported mobility and were responsible for our reported negative correlation of mobility with negative equity.

Schulhofer-Wohl (2011) is correct that we underreported overall mobility. However, that was done by design in order to distinguish between permanent and temporary moves. This is crucial for our research question, as the underlying theory from earlier work 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 can occur because of a homeowner really vacates her home temporarily or because vacancy status is misreported in the *AHS* survey data. Economic

ownership does not change in this latter case 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 over 25%), but it does not materially affect our findings from FGT (2010), as reported below in Section III.

In that analysis, we do not adopt Schulhofer-Wohl's (2011) 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 slight positive correlation between homeowner mobility and negative equity is likely due largely to conflating temporary and permanent moves.[2] We show below that over the period from 1985 to 1999 in the *AHS* data, more than 20% of his moves are temporary in nature, which makes his measure problematic for use in research about lock-in effects. These temporary moves correspond to approximately 50% of the additional moves that Schulhofer-Wohl (2011) tallied in excess of our new, preferred mobility measure. There is still uncertainty about the economic ownership of the property for the other 50% of additional moves.

Schulhofer-Wohl's (2011) measure of mobility also can be dynamically inconsistent, with moves in one period recoded at a later date as non-moves 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

[2] Schulhofer-Wohl used FGT (2010) data and codes to generate his mobility measure, and 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.

AHS does not yet have enough post crisis surveys to allow researchers to distinguish between these types of moves.[3]

This update highlights how noisy are the data from *AHS* surveys, but we know of no superior source with which 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 paper. It still understates true mobility rates to the extent any of the moves that we censor due to uncertainty about whether a change in economic ownership of the property occurred actually reflect 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 amount of permanent moves. We continue to advocate for a conservative coding strategy which is dynamically consistent over time, but this clearly is not costless.

Beyond doing everything possible to reduce the noise in the data, future research should address the issue of the extent to which these correlations represent a causal relationship. As we noted in our previous work, the likelihood that labor markets deteriorate along with housing markets raises the possibility that owners with negative equity are not moving in part because there are no good job opportunities elsewhere for them. 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.

[3] 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 (PSID). Exact property address information will be required to reliably distinguish between these two types of moves.

II. Additional Data and New Measures of Mobility

II.A. Changes in the data and summary statistics

There are four changes to the data used in this update of FGT (2010). The first is the addition of the 2009 *AHS* sample, which became available after we had published our previous article. 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. This does not result in any meaningful changes in our findings.[4]

The second change involves the use of First American-Core Logic (FACL) repeat-sales 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 arms-length purchases made with conforming and non-conforming loans, including subprime, Alt-A and jumbo mortgages. We believe this provides a more complete picture of what was going on in terms of local house prices especially in recent years, but this change also has no material impact on the results.[5]

The third change involves additional cleaning of the panel structure of the *AHS* data. The *AHS* survey was designed to be primarily used 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. By that, we mean that the interviewer does not have access to the responses for this variable from prior surveys, so that there is no way to ensure consistent

[4] 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* survey data becomes available. See the discussion below for more on this.

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

coding across surveys at the time of the interview. 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. FGT (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 FGT (2010), we deliberately chose a conservative definition of what constituted a move for the reason noted above—namely, that theory suggests financial frictions involving the likes of negative equity or mortgage lock-in should impact mobility for permanent moves. To ensure we did not mistakenly include temporary moves (or false transitions due to any remaining reporting error in the survey), we restricted our sample to those observations where it was immediately clear either that the same household resided in the given housing unit across consecutive surveys (in which case there was not a 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 FGT (2010). Focusing

[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 where the reported imputed tenure was reported as owner-occupied and 458 cases reported as rental.

initially on the top panel which reports data for the 1985-2007 period covered in FGT (2010) shows that 7.8% of the 61,801 housing transitions used in our regression analysis are moves according to this definition.[7] Those 61,801 transitions are only 82.7% of the total number of observations potentially available to us.[8] That is, we treat 17.3% of the potential transitions as censored. In 2.4% of the cases, the move is censored due to the observation being the last in the panel of 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 still possible that we may observe the original owner return to the property.

In his first and preferred mobility measure, Schulhofer-Wohl (2011) effectively counted 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 as a move. Using the code Schulhofer-Wohl provided, we created this variable in our data. It is labeled MOVE-SW in the second row of Table 1. Note the much higher mobility given this definition—16.4% of transitions are moves, versus 7.8% given FGT (2010)'s definition.[9] A much lower fraction of the data is censored using the MOVE-SW

[7] The reported mobility rate drops from 11.4% in our previous work to 7.8% 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 size of 61,803. This reflects the fact that the additional observations added to the estimation sample due to cleaning of previously uncaught false transitions in the panel structure nearly balance the number of observations lost due to deleting observations with imputed tenure status.

[9] As we will show later, the MOVE-SW 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 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." FGT (2010) 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, the data set for our 2010 paper would not censor the initial observation in that sequence. Our code would recognize that the initial observation in that sequence was not the last 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 (2011) 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. We comment more on this below.

measure, reflecting only the 2.4% of cases noted earlier where the observation is the final one in the data panel for a particular residence.

II.B. Two new measures of mobility that exploit the AHS panel structure

Because the conservative coding approach in FGT (2010) could result in dropping some permanent moves in a non-random 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. In creating it, for all cases where the next survey indicates that the house is vacant or rented, we now look forward across all available surveys to see if the house again becomes owner-occupied by another household, not just 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.

An example is illustrated below. The top row reports the *AHS* survey year; the second row indicates tenure status (owned or rented); and the third row reports the year the home was purchased by its owner.

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 that are 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 non-cash 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 has 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 resolving previously censored cases in this manner results in 10.0% of our regression sample transitions now being coded as permanent moves. MOVE1 mobility is much higher than MOVE (by 28%), but it remains well below that for MOVE-SW. We will discuss the differences across measures more fully just below, but before we do, we introduce another mobility variable called 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 sold in the interval

between the relevant surveys. Naturally, this leads to an even higher fraction of transitions being classified as permanent moves, as indicated in the following example.

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 both 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 re-sold. That new owner then decides to live in the property and reports a purchase year of 2008 in the 2009 *AHS*. On the other hand, it is also possible that the owner in 2003 decides 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 ten percent higher than for MOVE1 (11.0% versus 10.0%). Still, this more expansive definition does not generate anything close to the level of mobility indicated by MOVE-SW.

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.

II.C. Trade-offs across different measures of mobility

Our worry about Schulhofer-Wohl's empirical strategy for the question that we are addressing is that several of the housing transitions that he considers as moves are false positives in the sense they are temporary moves or reflect coding error in the underlying survey. To gauge how serious is the potential problem of conflating these types of moves, we evaluated the likelihood of Type I and Type II coding errors on 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-SW based on his code for 1985 using data from both the 1985 and 1987 surveys. These values for MOVE-SW are saved and the exercise is repeated using the 1987 and 1989 pair of surveys, the 1989-1991 pair, and so on, until 1997-1999. We stop in 1999 for this exercise 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-SW^R.

It is important to note that the coding of MOVE-SW^R in this real-time analysis differs from the coding of MOVE-SW used in the estimation sample. The example below illustrates why.

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

When the 2003 AHS data is added to the estimation sample, MOVE (and our two other mobility measures), MOVE-SW and MOVE-SW^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 is added, MOVE for 2003 will remain censored and both MOVE-SW and MOVE-SW^R for 2003 will be recoded as a move. However, when the 2007 *AHS* data is merged into the sample, MOVE for 2003 (as

well as MOVE1 and MOVE2) will be recoded from censored to a non-move, while MOVE-SW for 2003 will be recoded from a move to a non-move, and MOVE-SW^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-SW is that in constructing these mobility measures the data are sorted by residence, household (based on a unique household identification number we create), and survey year. Using 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 MOVE1 and MOVE2) only recodes censored observations as either non-moves or moves, and never recodes non-censored mobility observations. In contrast, the coding for MOVE-SW can be dynamically inconsistent over time, with moves recoded at a later date as non-moves. By construction, MOVE-SW^R maintains dynamic consistency by not recoding a move as a non-move even when information becomes available indicating that the original owner has returned.

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-SW^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 documents that these two mobility variables agree that there were 70,707 cases in which there was no move. There are no cases in which our MOVE2 measure considered some transition a move when MOVE-SW^R did not (that is, there is no evidence of Type II errors); nor is MOVE2 ever censored or missing when MOVE-SW^R indicates that no move took place.

[10] Here we use all available transitions from the *AHS* for owner-occupied residents between 21 and 59 years of age over the period from 1985 to 1999 and do not restrict the observations to those with non-missing values for all of the regressors that we use in the final mobility estimation.

The second column is more interesting because both mobility measures have 8,550 moves, but MOVE-SW^R has an additional 8,607 moves. Moreover, 41.3% (= 3,557 / 8,607) of the additional moves in MOVE-SW^R turn out to be temporary in nature in that 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 they reflect some other trait that leads us to conclude there has not been a permanent move.[11]

Out of all false positives from MOVE-SW^R, in two-thirds of the cases the Type I error could be eliminated by looking at only one subsequent *AHS* 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 are clear that there was a given owner-occupant in 1985, but a different occupant or reported vacancy in 1987. In 66% of the cases, the 1989 survey fully resolves the uncertainty. In these "false positive" cases, that means we see the same household living and owning the same housing unit in both 1985 and 1989. Another 17.4% of the false positives are resolved by the next available survey (i.e., after six years have passed), so that over 83% of the cases are clarified by 1991 in this example. The remaining cases are all clarified with 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-SW^R considers them moves. While none of these cases can be definitively

[11] As noted above, the lack of dependent coding for this variable means that some of these cases could be due to coding error by the *AHS* survey taker in the sense she does see or interview the original owner and mistakenly concludes the unit is not occupied by the same person. The most important of the latter type of factor involves units that are described as being vacant and held for occasional or seasonal use. This group comprises 14% of the 3,557 cases. There is a much smaller fraction (1.2%) for which there is non-cash rent and a subsequent sale outside the relevant sample interval. There is an even smaller number (0.3%) 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 by the owner in the residence is 6.1 (5.0) years. In 38 percent of the cases, the post-temporary move duration is censored by the end of the data in 2009.

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 becomes available. In practice, this means MOVE2 still does not include all true permanent moves. This highlights 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.

III. Results

III.A. Estimation Methodology

As discussed in FGT (2010), given the likely measurement error in our measures of the three financial frictions, we want to be able to instrument for this measurement error to reduce the attenuation bias. While our alternative measures for these three frictions are also subject to measurement error, if the sources of these errors are uncorrelated, then we can use them as instruments. 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.

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

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

$$
I^{1}_{Ni} = \begin{cases} 1 & \text{if } I^{*1}_{Ni} \geq 0 \\ 0 & \text{otherwise} \end{cases}
$$

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

where I_{mi} is our observed mobility indicator, I^{1}_{Ni} our negative equity indicator based on the self-reported house value, I^{2}_{Ni} our alternative negative equity indicator based on the metro area house price index, Z_{P13i} our instrument for the annual property tax cost of moving due to Prop13 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 robustness, we also present results for a single-equation probit (used in FGT (2010)) and a standard linear probability model.[13]

[13] Schulhofer-Wohl (2011) correctly noted 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 FGT (2010). 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 just 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 being reported in the second and third columns of Table 4 below. Schulholfer-Wohl (2011) does not instrument for the

III.B. 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-SW^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 by whether or not there was a move. Table 4 then reports the results of re-estimating the core mobility specification from FGT (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.

Focusing first on the multi-equation probit marginal effects in column 1 indicates 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%, which is a similar to the relative impact than reported in FGT (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

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measurement error. As FGT (2010) showed, there never is any significant correlation between any financial friction and permanent moves unless attenuation bias is dealt with in some fashion.

mobility rate. It is different from zero at a ten percent confidence level for the 1985 to 2007 sample, and we can never reject the null that the effects are the same across all three of these measures. Comparing results across columns in the top panel indicates that implied marginal effects from the multi-equation Probit specification are consistently lower than for 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 the second panel adds in the data from the 2009 survey. We find modestly lower marginal effects here as compared to the 1985 to 2007 results, and negative equity no longer is associated with statistically significantly lower mobility for the MOVE2 variable. However, these marginal effects are not significantly different from those from 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 becomes available, as cautioned in the original FGT (2010). The previous section implies that it takes 4 to 6 years for the vast majority of the censored housing transitions to be resolved. Hence, it will be much later 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-SW 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.[14]

This intuition that the driving factor behind the difference in our negative equity results and those reported by Schulhofer-Wohl (2011) is the conflation of temporary and permanent moves is corroborated by comparing the different estimates associated with MOVE-SW and MOVE-SW^R. Recall that the distinction between these two measures is that MOVE-SW^R retains moves as identified by Schulhofer-Wohl that are known *ex post* to be temporary, whereas MOVE-SW allows these temporary moves to be recoded as non-moves. Retaining these temporary moves increases the measured mobility rate from 16.1% for MOVE-SW to 17.8% for MOVE-SW^R. The estimates in Table 4 indicate that adding these additional temporary moves raises in each case the estimated positive impact 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 both physical location and economic ownership changed. That is roughly half of the excess moves in MOVE-SW^R relative to MOVE2 based on our real time analysis of the 1985-1999 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 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

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

unit over multiple surveys, but for which there still is no clear evidence of a sale, actually are permanent moves.

This raises the question of whether we could improve on our permanent mobility measure MOVE2 to count as moves situations where it seems likely (but not certain) that a permanent move has taken place. Intuition might suggest that the longer the ownership gap observed where 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 fractions of cases that the move turned out to be temporary conditional on having the information to make this determination. For situations where the residence has been rented or vacant for at least three surveys, the transition turned out to be temporary in 59% of the cases where 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%. This pattern continues for ownership gaps of five or more and of six or move surveys. Thus, conditional on those cases where we can resolve the nature of the transition, 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 that you can improve on MOVE2 by recoding censored transitions as moves given an ownership gap of some specified length. However, it still is useful to understand that the potential fragility of our results (and, possibly, those who came before us) arises from the fact that it is difficult to properly measure mobility in a number of cases.

III.B. Fixed Rate Mortgage 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

(FRM). In a rising interest rate environment, if a homeowner with a FRM 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, then 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 where we use the FACL overall house prices to update home values. The data confirm our earlier findings that both frictions give rise to reduced household mobility – from 10% to 16% lower per $1,000 using our preferred mobility measure MOVE1. In none of the specifications do the data reject that the mobility friction is the same whether it is being generated by rising rates for a fixed rate borrower 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, consider the hypothetical case of a 250 basis point increase in the average 30-year fixed-rate mortgage interest rate as a result of the normalization of monetary policy. For homeowners in 2009 with a FRM, this results in a mean (median) annual payment difference of $2,300 ($1,710). Using the probit marginal effects for MOVE1, this implies a mean (median) reduction in the 2-year mobility rate of 3.7 (2.7) percentage points. If we redo the calculation using the estimates for MOVE2, we get a reduction in the 2-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.[15]

III.C. Housing Frictions and Labor Markets

Finally, there has been increased interest in the degree to which any mobility frictions from the housing market may generate labor market frictions. While we do not have space in this note to discuss in detail the recent literature on this question, the *AHS* data are useful in examining the types of moves that are likely to be impacted by housing market frictions. The *AHS* asks recent movers (moved within the last 2 years) the primary reason for the move and, until 1995, the distance of the move. A high percentage of moves – 73 percent – are local, while only 13 percent of moves cross a state border. Table 6 provides more detail in its tabulation of the primary reason for moves, both overall and broken down by distance of the move. Most moves are for quality-of-life, personal/family and financial reasons, and do not appear to be primarily job-related. This is especially the case for local moves. In contrast, longer distance moves, particularly those that cross a state border tend to be job-related. One potential implication of these data is that financial frictions to household mobility are more likely to reduce local moves such as trade-up purchases that need not have any significant spillover effects for labor markets. This view is consistent with recent work by Donovan and Schnure (2011) using American Community Survey data as well as by Aaronson and Davis (2011) using Survey of Income and Program Participation (SIPP) data.[16]

[15] This is particularly true for borrowers who received a below market mortgage rate through a private modification or a HAMP modification (conditional on the borrower not re-defaulting on the modified mortgage). If these low rate mortgages were assumable, then there would be no associated mobility friction.

[16] Household panel data sets such as SIPP data have the advantage that they can follow a household across different residences. A challenge, though, is identifying permanent from temporary moves since this requires access to specific address information.

IV. Summary and Implications for Future Research

Including the most recent *AHS* survey for 2009, which reflects the 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 we cannot really know for sure how the recent housing bust impacted permanent mobility until a few years into the future when the data will reveal the true nature of those transitions.

A critique of our sample selection procedures which claimed 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 in our earlier work. 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, others can develop better data sources or improved ways to reduce the noise in the *AHS* panels.

For the future, we think it as or 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 both conceptually and empirically to believe they do), the correlation documented here could be driven predominantly by there being a lack of good job

opportunities to which people would want to move. 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.

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Table 1: Mobility Measures

	1985 - 2007		
	% Moved[1]	Non-censored	% Censored
MOVE	7.8	61,801	17.3
MOVE-SW	16.4	68,206	8.8
MOVE1	10.0	63,700	14.8
MOVE2	11.0	64,450	13.8

	1985 - 2009		
	% Moved	Non-censored	% Censored
MOVE	7.5	66,280	17.7
MOVE-SW	16.0	73,096	9.2
MOVE1	9.7	68,371	15.1
MOVE2	10.8	69,181	14.1

[1] The 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. Percent moved is the ratio of moves to the sum of moves and non-moves. Percent censored is the ratio of censored moves to the sum of moves, non-moves and censored moves.

Table 2: Permanent Vs. Temporary Moves

	Cross-tab of MOVE2 with MOVE-SW^R		
		MOVE-SW^R	
		0	1
MOVE2	0	70,707	3,557
	1	0	8,550
	. (missing)	0	5,050

% of False Positives Resolved Over Time	
4 years or 1st subsequent survey	66.0
6 years or 2nd subsequent survey	17.4
8 years or 3rd subsequent survey	7.7
10 years or 4th subsequent survey	4.7
12 years or 5th subsequent survey	1.9
14 years or 6th subsequent survey	1.2
16+ years	1.1

Note: 15.1% of false positives are resolved using vacancy status

Table 3: Cross-Tabs of Negative Equity and Mobility Indicators

Mobility Indicator		Negative Equity	
		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-SW	No	74.02	2.11
	Yes	14.15	0.51
	Censored	8.12	1.09
MOVE-SW^{R}	No	68.60	1.91
	Yes	14.71	0.57
	Censored	12.98	1.23

Note: American Housing Survey, 1985-2009. Negative equity is based on self-reported house values. MOVE-SW^{R} is the real-time calculation of MOVE-SW over the full sample period where we do not allow moves to be subsequently recoded as non-moves. Table shows cell percents.

Table 4: Empirical Estimates

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

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

** denotes statistical significance at the 95% confidence level.

* denotes statistical significance at the 90% confidence level.

Table 5. Impact of Other Financial Frictions on Household Mobility

	IV Probit (multi-equation)	IVProbit (single equation)	IV Linear Probability
Mobility indicator: *MOVE1*			
Fixed-Rate Mortgage Lock-In ($1,000)	–0.016** (0.009)	−0.018* (0.009)	–0.013 (0.009)
Proposition 13 Property Tax Lock-In ($1,000)	–0.010** (0.005)	−0.010** (0.004)	–0.008** (0.004)
Mobility indicator: *MOVE2*			
Fixed-Rate Mortgage Lock-In ($1,000)	–0.023** (0.009)	−0.024** (0.009)	–0.019** (0.009)
Proposition 13 Property Tax Lock-In ($1,000)	–0.009* (0.005)	−0.009* (0.005)	–0.008** (0.004)

Note: Probit marginal effects are average derivatives with standard errors given in parentheses. Sample: 1985-2009

** denotes statistical significance at the 95% confidence level.

* denotes statistical significance at the 90% confidence level.

Table 6. Main Reason For Move: Overall and by distance of move

		1985 - 1995				
Reason	1985 - 2009	All	Same MSA	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

Note: American Housing Survey, authors' calculations – sample restricted to owner-occupied respondents between 21 and 59 years of age.