The (Un)importance of Geographical Mobility in the Great Recession*

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Abstract

Unemployment during and after the Great Recession has been persistently high. One concern is that the housing bust reduced geographical mobility and prevented workers from moving for jobs. We characterize flows out of unemployment that are related to geographical mobility to construct an upper bound on the effect of mobility on unemployment between 2007 and 2012. The effect of geographical mobility is always small: Using pre-recession mobility rates, decreased mobility can account for only an 11 basis points increase in the unemployment rate over the period. Using dynamics of renter geographical mobility in this period to calculate homeowner counterfactual mobility, delivers similar results. Using the highest mobility rate observed in the data, reduced mobility accounts for only a 33 basis points increase in the unemployment rate.

Keywords: Mobility, Unemployment, Great Recession, Geographical Mismatch, Stock-Flow Equations

JEL Codes: E24, E32, J61, J64.

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1. Introduction

The persistently high unemployment rate during and in the aftermath of the Great Recession, along with the fact that the recession was accompanied by a housing bust, has stimulated interest in the role of geographical mobility in labor market outcomes. In this paper we quantify the effect of the housing bust on unemployment through the mobility channel.[1] In particular, we ask: how much lower would unemployment have been in 2012 if the housing bust had not affected mobility between 2007 and 2012? We answer this question using an accounting exercise that focuses on the outflow from unemployment into employment due to mobility, and boosts this flow by assuming a counterfactual mobility rate that is higher than the observed mobility rate. Our counterfactual exercise has two distinct features: First, it accounts for the cumulative effect of lower mobility on unemployment from 2007 and 2012. Second, it is designed such that it gives an upper bound on the effect of mobility on unemployment.

We find that while the percent drop in mobility rate for homeowners between 2007 and 2012, as measured using CPS data, was large, the effect of this on the unemployment rate was negligible, even after compounding the effect of lower mobility over five years. The intuition is easy to explain with a simple example: Between 2006 and 2007, 2.6% of homeowners moved between counties, while between 2009 and 2010 only 1.9% of homeowners moved between counties (a 27% decrease). To bound the effect of mobility, suppose that *all* unemployed homeowners who move between counties find a job immediately. Had mobility of homeowners been at its pre-recession level between 2009 and 2010, an additional 0.7% of homeowners would have moved between 2009 and 2010. As homeowner unemployment rate in March 2010 was 8.3%, and their share in the labor force was 69%, the effect of the additional moves on the unemployment rate would have been 0.007*0.69*0.083, approximately 4 basis points. While the percent drop in mobility rate was large, the levels are so small, that the effect on unemployment is tiny. In our accounting exercise we formalize this example, compound it over five years, measure job-related mobility more carefully, and account for the fact that homeownership and unemployment status are measured post-move in the CPS. We then repeat this exercise using counterfactuals other than pre-recession mobility, including homeowner mobility in previous recessions and renter mobility in the Great Recession. The advantage of using the latter, is that it allows us to capture distinct features of the Great Recession. One can speculate that, had the housing bust not occurred, mobility in the Great Recession would have been higher than pre-recession levels as there are more unemployed who might have a greater incentive to move. If this is indeed the case, and as moving

[1] The term mobility has a broad interpretation in the labor economics literature, including occupational mobility and intergenerational mobility. This paper is focused on geographical mobility only. Throughout, for brevity, we refer to geographical mobility as mobility.

costs for renters were not directly affected by the housing bust, we should have seen renters move more in the Great Recession. Conducting our formal exercise, we find that reverting mobility to pre-recession level would have implied an unemployment rate that is only 11 basis points lower than the observed unemployment rate in March 2012. This implies that mobility can account for less than 3% of the increase in unemployment rate observed between 2007 and 2012. The results when using mobility patterns from previous recessions, or renter mobility in the Great Recession are very similar. Lastly, even in the extreme case of using the highest mobility rate observed in our data as the counterfactual, mobility cannot explain more than 8.6% (or 33 basis points) of the increase in unemployment rate between 2007 and 2012.

Our work is related to two strands of recent literature. First, there is a debate in the literature on whether housing busts decrease mobility in general, and in particular in the Great Recession. Quigley (1987) , Chan (2001), Engelhardt (2003) and Ferreira, Gyourko and Tracy (2010, forthcoming) find that falling house prices, negative equity and loss aversion have an adverse effect on mobility. Schulhofer-Wohl (2012) finds that households with negative equity move with a slightly higher probability than those with positive equity, and Coulson and Grieco (2013) find no evidence for the lock-in effect using PSID data. In the context of the Great Recession, Aaronson and Davis (2011), using SIPP data, find that the pattern of inter-state homeowner mobility is no different to that of renters (and compared to previous recessions with no housing bust). Donovan and Schnure (2011), using data from the American Community Survey (ACS), find that the lock-in effect impacts mostly intra-county moves (which should have a smaller effect on employment outcomes). Molloy, Smith and Wozniak (2011) find that inter-state and inter-county mobility have fallen during the Great Recession, but do not find evidence that homeowner mobility declined by more than renter mobility.[2] In Section 2 we describe trends in homeowner and renter mobility in the CPS, focusing on measures of job-related mobility which are used as inputs into our accounting exercise. We find that the magnitude of fall in mobility for homeowners and renters is sensitive to whether or not we correct for the fact that homeownership is measured post move in the CPS.

A second strand of the literature attempts to measure directly the effect of mobility on unemployment during and after the Great Recession.[3] Batini et al. (2010) run a state-level regression of changes in the unemployment rate on measures of skill mismatch and housing market conditions and find that structural

[2]The magnitude of the fall is different across different surveys, being the largest for the CPS.

[3]A related literature pioneered by Blanchard and Katz (1992) looks at the role of internal migration in determining long run outcomes in the labor market as opposed to business cycle frequencies. Blanchard and Katz (1992) find that in response to local shocks, employment responds in the long run, indicating that labor markets equilibriate through labor mobility. Also related is Oswald (1996) which suggests (and provides evidence using aggregate data) that lower labor mobility among homeowners is a reason for why high homeownership countries have less efficient labor markets and higher unemployment rates. Green and Hendershott (2002) revise Oswald's analysis using PSID micro data and find that while homeowners have lower job-finding rates, the effect is an order of magnitude smaller compared to the one documented by Oswald using aggregate data.

unemployment has gone up by 1.5 percentage points. Sterk (2011) uses a DSGE model without explicitly modeling local labor markets and finds that reduced mobility can explain the flattening of the Beveridge curve before 2010. Nenov (2012) models local labor markets to incorporate directed migration. He finds that the effect of the housing bust on unemployment is at most 0.4 percentage points. Sahin et al. (2012) use a matching model to derive a planner's allocation of workers across sectors and regions. They use data on vacancies and unemployment to construct a mismatch index, and find that geographical mismatch plays no role. In a recent paper, Modestino and Dennett (forthcoming) find using IRS inter-state migration data that while the housing bust affected mobility, this had a negligible effect on unemployment. Finally, Valletta (2012) finds similar effect of drops in house prices on unemployment durations for homeowners and for renters. Our paper complements this literature by introducing a different approach to quantifying the effect of reduced mobility on unemployment. We focus on a stock-flow analysis of unemployment, and design our exercise as an upper bound on the effect that the drop in mobility had on unemployment. Our calculation of counterfactual unemployment is not restricted to a single counterfactual (such as pre-recession mobility rates), but rather covers a wide range of plausible scenarios of counterfactual mobility rates. Conducting our counterfactual exercise over five years, we show that the effect of mobility on unemployment is limited not only in the context of the Great Recession, but also in the context of the jobless recovery that followed.

The rest of the paper is structured as follows: Section 2 summarizes trends in mobility. In Section 3, we describe our methodology, and in Section 4 we discuss how we measure mobility. Section 5 presents the main findings, while Section 6 provides additional results for some extreme counterfactual scenarios. Section 7 introduces some alternative measures for mobility, and Section 8 concludes.

2. Mobility Trends

We start by documenting key mobility trends from the CPS with special emphasis on the Great Recession. A few studies have looked at mobility trends and found only a modest decline in inter-state and inter-county mobility over the recession compared to their long term trend (Kaplan and Schulhofer-Wohl (2012), Molloy, Smith and Wozniak (2011)). Rather than replicating their results, we focus on particular measures of mobility which are the most relevant for our exercise, and will function as inputs to our calculations. First, we focus on homeowners, the group which is most likely to be affected by the housing bust, and compare them to renters, who are not exposed to the lock-in effect.[4]

[4]There might be second order effects of the housing bust on renter mobility. For example, if rent goes down, then renters might find it advantageous to renegotiate their contracts or move to a new rental property. For the rest of the paper, we assume that these second order effects can be neglected and, therefore, the housing bust did not affect renter mobility.

Second, we construct different measures of job-related mobility. A common way to measure mobility that affects labor market outcomes, is to focus on mobility across labor markets. An accurate measure of that type is hard to come by, since it is difficult to define what constitutes a crossing between two labor markets. Inter-state mobility, for example, would most likely understate job-related mobility, while inter-county mobility, would most likely overstate it. We therefore combine measures of total moves, and of inter-county moves with a measure of self-reported reason for moving.

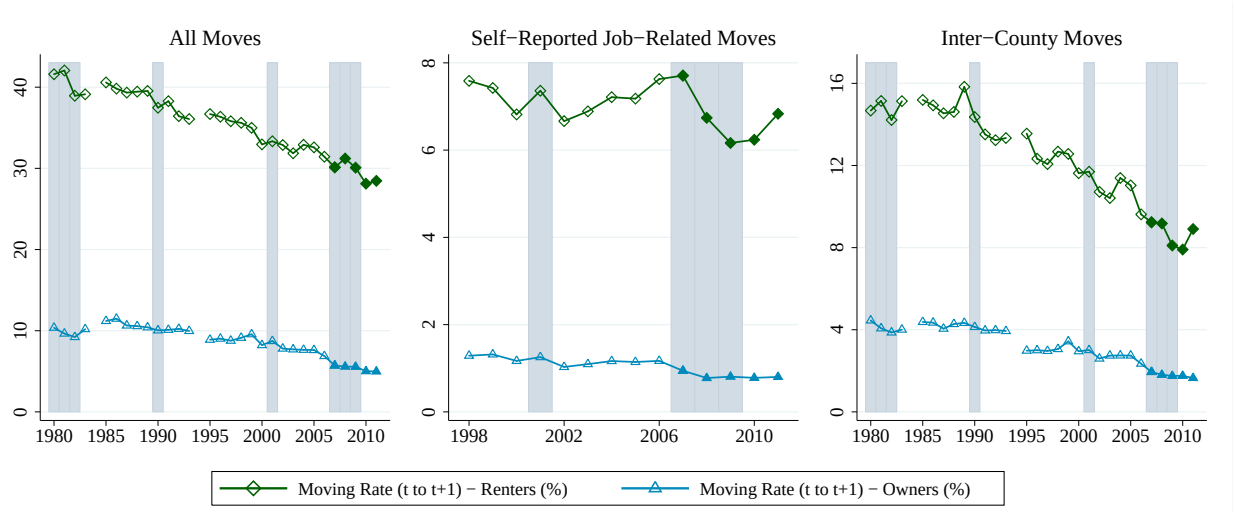

Figure 1: Mobility Rates for Homeowners and Renters

Notes: Shows the share of homeowners and renters in the labor force at t+1 who moved between t and $t+1$ based on the March CPS for three mobility measures: (1) All Moves; (2) Only self-reported job-related moves; (3) Only inter-county moves. The classification of homeowners and renters is post-move. Recession bars correspond to NBER recession dates. Year t is considered to be a recession year if at least 1 month between March of year t and March of year $t+1$ was classified as a recession month by the NBER. Observations with imputed migration data were removed (following Kaplan and Schulhofer-Wohl (2012)). Homeowner mobility is recorded on the right scale and renter mobility on the left scale.

Figure 1 shows mobility rates for homeowners and for renters based on the March supplement of the CPS for three measures: (1) All Moves; (2) All moves, if self-reported as job-related moves; (3) Only inter-county moves. Each point in the graph shows the mobility rate between t and $t+1$. The classification as homeowner or renter is post-move (i.e., at time $t+1$). As is clear from the graphs, renter mobility (left scale) is much higher compared to mobility of homeowners (right scale), and both groups show a secular decrease in mobility. Between 2006 and 2011, total homeowner mobility dropped 1.9 percentage points (27.5%), while renter mobility rates fell 3 percentage points (9.5%) over the same period. Our next two measures are better proxies for job-related mobility. Self-reported job-related mobility, dropped 0.4 percentage points (31.5%) for homeowners. The drop from 2006 to 2011 for renters was 0.8 percentage points (10.4%). Finally, inter-county mobility fell 0.7 percentage points (29.7%) for homeowners and 0.7 percentage points

(7.4%) for renters. However, the similarity between the percentage points drop for homeowners and renters in this case is driven mainly by the sharp increase in inter-county mobility for renters in 2011. For all measures, the percentage points drop is at least as large for renters, while the percent drop is larger for homeowners.[5]

One concern when using March CPS data to compare mobility of homeowners and renters is that the classification of individuals as homeowner or renter is post-move. According to the CPS, during the Great Recession, homeownership dropped by 3 percentage points from 71.6% in 2006 to 67.7% in 2012.[6] Assuming that at least some of this shift is due to homeowners who became renters (whether due to foreclosures or by choice), the CPS based mobility rates are underestimated for homeowners and overestimated for renters over this period. We offer a homeownership correction for the total mobility rate for homeowners and renters (detailed in Section 7). Applying this correction, we find a 1.1 percentage points (14.8%) drop in homeowner mobility from 2006 to 2011, and a 4.2 percentage points (14.2%) drop in renter mobility.

Finally, as is clear from the graphs for all moves and for inter-county moves, homeowner and renter mobility rates show clear downward trends over the last 30 years. We find that accounting for the trend does not qualitatively affect the comparison of the drop in mobility for homeowners and renters. As long as the homeownership correction is not applied, the percent change in mobility rates (after accounting for trends) for all moves and for inter-county moves, is larger for homeowners compared to renters, while the percentage points drop is similar or larger for renters. Applying the homeownership correction, we no longer find that renter mobility is more below trend than homeowner mobility, even for percent change.[7]

In summary, whether the drop in homeowner mobility is larger than the drop in renter mobility during and in the aftermath of the Great Recession is sensitive to the particular mobility measure which is applied. In our accounting exercise, we show that the effect of reduced mobility on unemployment is small even when considering measures which imply the largest drop in homeowners mobility.

3. Methodology

We first set up a stock-flow equation that highlights the flows out of unemployment that are related to mobility of homeowners. The counterfactual exercise that follows involves increasing the flows out of

[5]The finding that percentage points drop in mobility rates over the period is larger for renters, while the percent change in mobility rates is larger for homeowners is consistent with other authors findings using other data. Aaronson and Davis (2011) for example, find that SIPP state-to-state migration rates fell 6 basis points (24%) for homeowners and 13 basis points (13.3%) for renters, when comparing the 2005-2007 period to the December 2008 - July 2010 period.

[6]See Callis and Kresin (2012) for a U.S. Census press release detailing changes in homeownership.

[7]The full set of regressions, which compare the drop of mobility for homeowners and renters accounting for the trend, are reported in the web appendix. To account for trend when studying percent change in mobility, we run the regressions in logs.

unemployment due to mobility in an attempt to undo the effect that the housing bust had on mobility and through that on unemployment. The exercise is designed to yield an upper bound for the effect of mobility on unemployment. The full derivations and upper bound arguments can be found in the web appendix.

Unemployment Stock-Flow Equations: Equation (1) summarizes the mobility-related outflow of homeowners from unemployment:

$$U_t = U_{t-1} - \underbrace{U_{H,t-1} m_{t-1}^{UH} j_{t-1}^{UH} \left(e_{t-1}^{UH} - \bar{e}_{t-1}^{UH}\right)}_{\substack{\text{mobility-related outflow} \\ \text{from unemployment of HO}}} + N_t. \tag{1}$$

Here U_t is unemployment at time t; $U_{H,t}$ is the unemployment of homeowners; m_t^{UH} is moving rate among unemployed homeowners between $t-1$ and t; j_t^{UH} is the job-related moves as a share of all moves among unemployed homeowners; e_t^{UH} is the finding rate for unemployed homeowners who moved for job; and $\bar{e}_{t-1}^{UH}$ is the finding rate for other unemployed homeowners. N_t represents all other net inflows into unemployment.[8] The additional outflow from unemployment for the group of job-related movers is given by the size of this group, $U_{H,t-1} m_{t-1}^{UH} j_{t-1}^{UH}$, times the difference in the job-finding rate between this group and the base group, $e_{t-1}^{UH} - \bar{e}_{t-1}^{UH}$.

Counterfactual Exercise: Our main counterfactual exercise involves increasing moving rate, m_{t-1}^{UH}, to a higher level $m_{t-1}^{UH,C}$. The share of job-related moves for unemployed homeowners (j_{t-1}^{UH}) could respond to changes in incentives to move. If there is greater incentive to move for jobs, then j_{t-1}^{UH} should also increase in the counterfactual. As detailed in the results section, we consider some cases in which j_{t-1}^{UH} is changing in the counterfactual. The effect of reduced mobility is measured through the difference between the counterfactual unemployment and the observed unemployment. Given measures of the shares m_{t-1}^{UH}, j_{t-1}^{UH}, e_{t-1}^{UH}, and $\bar{e}_{t-1}^{UH}$, we start by recovering the residual N_t, using the stock-flow equation (1): $\hat{N}_t = U_t - U_{t-1} + U_{H,t-1} m_{t-1}^{UH} j_{t-1}^{UH} \left(e_{t-1}^{UH} - \bar{e}_{t-1}^{UH}\right)$. We then boost the mobility rate of homeowners, m_{t-1}^{UH}. For the first time period, the counterfactual unemployment level is calculated using the same stock-flow equation but with a higher mobility rate $m_0^{UH,C}$ and the recovered residual $\hat{N}_1$:

$$U_1^C = U_0 - U_{H,0} m_0^{UH,C} j_0^{UH} \left(e_0^{UH} - \bar{e}_0^{UH}\right) + \hat{N}_1, \tag{2}$$

For subsequent periods, the counterfactual unemployment rate is calculated by compounding the counterfac-

[8] N_t therefore includes all inflows to unemployment, minus renter flow out of unemployment and homeowner non-mobility-related flow out of unemployment, plus transitions from out of the labor force to unemployment.

tual from the previous period.[9] The maintained assumption in the above exercises is that the counterfactual has no effect on the residuals $\hat{N}_t$ i.e. as we boost mobility, all other net flows into unemployment remain unchanged (or at least do not decrease). In the web appendix we formalize the conditions under which ignoring the effect of the counterfactual on these residuals exaggerates the fall in unemployment in our counterfactual, thus yielding an upper bound for the effect of mobility on unemployment.[10] The intuition is that once we remove an unemployed worker from the unemployment pool, this worker never transitions back to unemployment in our counterfactual exercise.

4. Measurement

In order to implement our exercise, we need to measure U_t, $U_{H,t}$, m_t^{UH}, j_t^{UH}, and $\left(e_t^{UH} - \bar{e}_t^{UH}\right)$. We briefly survey these measures in this section, leaving the details to the web appendix. We use the Current Population Survey (CPS) from March 2006 to March 2012 to measure total, and homeowner unemployment (U_t and $U_{H,t}$).[11]

We measure mobility for unemployed homeowners who move for job-related reasons ($m_t^{UH} * j_t^{UH}$) in three steps. First, we use the CPS March Supplement (provided by IPUMS-CPS, King et al. (2010)) to measure the number of homeowners who moved.[12] We recover a monthly mobility rate for all homeowners by allocating the annual moves evenly over the year.[13,14]

[9] We implicitly assume that the additional people who move for jobs get the higher job-finding rate e_t^{UH} for only one period. If the additional movers get a higher job-finding rate for multiple periods, then we need to keep track of the stock of people who have moved for jobs in the past but are still unemployed. In all our counterfactual exercises, we set $e^{UH} = 1$. This sidesteps the issue of keeping track of the stock of unemployed people who have moved for jobs in the past, as this set is always empty.

[10] The main assumptions needed are that increased mobility does not (positively) affect finding rates for renters, and non-movers, and does not reduce other net flows into unemployment (from separations or from out of the labor force).

[11] For all tables we use the CPS classification for unemployment. Results are qualitatively similar for unemployment measures that include discouraged workers, and are available from the authors upon request.

[12] We ideally want to measure the number of people who moved and were homeowners before moving. However, the CPS allows us to measure the number of people who moved and were homeowners after moving. We assume that our post-move measure of homeowner mobility is a good approximation for pre-move homeowner mobility. In Section 7 we discuss measures which are robust to this assumption, and apply a correction which allows us to directly measure (at some cost) pre-move mobility for homeowners.

[13] One concern might be that if the correlation between seasonality of mobility and seasonality of unemployment is not zero, then allocating mobility equally over the year could bias the results. We repeated our first specification, reported in column 1 of Panel A of Table 1, allocating mobility over the year using the quarterly seasonality for homeowners in 2009 calculated using the second wave of the 2008 Survey of Income and Program Participation (SIPP) panel as reported in Table 1 of Marlay and Mateyka (2011). Since the results were identical to the results reported without the correction up to the fourth digit (in terms of percentage points), we conducted the rest of the exercises without the correction for seasonality.

[14] We abstract from repeated moves of the same individual within a particular year. Since we assign movers a probability of 1 for becoming employed upon a job related move, the only repeated moves which should be counted as an additional move are those that trigger a transition from unemployment to employment more than one time within a year (for example an unemployed becoming employed due to a move, losing his job, and becoming employed again due to a move). Marlay and Mateyka (2012) show using the SIPP that in 2009 only about 9% of movers moved more than once within a year. Since the relevant share of repeated moves according to the criterion above is expected to be small, the bias on mobility rate which is generated by repeated moves is expected

Second, we correct this measure for the possibility that unemployed homeowners move more than employed homeowners. We use the Panel Study of Income Dynamics (PSID) to construct the ratio $\frac{m_t^{UH}}{m_t^H}$, where m_t^H is mobility rate for all homeowners. Since 1981, this ratio ranges between about 0.8 and 2.6 with an average of 1.5 in the PSID. As this ratio is used to multiply both observed and counterfactual mobility, the larger it is, the larger the effect of mobility on unemployment. For consistency with our upper bound strategy we adopt a value of 2 for this ratio, slightly lower than the average over the highest 5 years of 2.2.[15]

Third, we restrict our mobility measure to job-related moves. We use self-reported reason for move to distinguish job-related moves. Guided by our goal to obtain an upper bound on the effect of mobility on unemployment, we try to take the broadest measure for mobility for job-related reasons, classifying the following answers to the why moved question as moving for a job: "new job or job transfer", "to look for work or job lost", "for easier commute", "other job-related reason". The fraction of self-reported job-related moves out of total moves ranges between 14.5% and 17.7% for homeowners and between 20.3% and 25.5% for renters between 2006 and 2012.[16] We assume that this fraction does not change within the year and then apply this to our counterfactual analysis at a monthly frequency. As in the case of measuring total mobility, homeownership status is measured post-move, and we need to assume that the share of self-reported job-related mobility is similar for unemployed homeowners. How realistic is this last assumption? We find that pooling over all years for which self-reported reason for move is reported in the PSID, the share of unemployed homeowners who move for jobs out of unemployed homeowners who move is 13%, slightly lower than our CPS measure.[17]

We use two measures of mobility which differ based on distance moved: all moves, and inter-county moves only. In all we construct 3 measures for mobility: (1) all self-reported job-related moves; (2) all self-reported job-related inter-county moves;[18] (3) all inter-county moves. The third measure assumes that all inter-county moves are job-related. Since only about a third of the homeowner inter-county moves are

to be much smaller than 9%. Given the small nature of the bias we neglect this issue from most of our mobility measures. We note, that our match-based mobility measure is a truly monthly measure (see section 7).

[15] m_t^{UH} is directly observed in the PSID. An alternative approach would be to use PSID to measure m_t^{UH}. This is not practical for two reasons: First, the PSID is biennial, implying that only a single mobility rate (between 2007 and 2009) can be calculated for the Great Recession. Second, PSID's small sample makes annual mobility measures within the unemployed group noisy. We therefore focus on calculating the measure ($\frac{m_t^{UH}}{m_t^H}$) and looking at averages of this measure over time to reduce noise.

[16] Family reasons account for about a quarter of moves (inter- and intra-county). Renters becoming owners as well as house and neighborhood improvement are two other leading reasons for move. We give the full distributions of reasons for homeowners and renters for both all and inter-county moves in the web appendix.

[17] We cannot make a correction based on the largest observed ratios in the PSID as the sample size in the PSID for unemployed homeowners who move for job-related reasons is very small.

[18] Why not use only inter-county moves? Our main reason, is that there is a fairly large share of intra-county moves which are job-related. According to March CPS, in 2006 6.28% of homeowner intra-county moves were job-related (3.6% if excluding easier commute).

self-reported as job-related (30.3% in 2006), this measure is expected to dramatically overstate job-related mobility. We use this measure as an extreme scenario, which is robust to potential errors in self-reported measures.

Finally, we set the difference in job-finding rates $\left(e_{t-1}^{UH} - \tilde{e}_{t-1}^{UH}\right)$ to 1, implying a monthly finding rate of 1 for movers for jobs ($e_{t-1}^{UH} = 1$) and of 0 for all other homeowners ($\tilde{e}_{t-1}^{UH} = 0$).

5. Results

The base period for the counterfactual is March 2007. We compound the counterfactual for 5 years until March 2012. We consider three comparison groups for counterfactual mobility: (1) Unemployed homeowners before the Great Recession (2006); (2) Unemployed homeowners in other recessions (1991 and 2001), adjusting for trend; and (3) Renters in the Great Recession.

Comparison Group: Homeowners Before the Great Recession. In column 1 of Table 1, we use pre-recession unemployed homeowner mobility as the counterfactual mobility rate. In Panel A we focus on a mobility measure which is based on all moves, where job-related mobility (which is unchanged in the counterfactual) is based on the self-reported measure. The average measured monthly mobility rate for job reasons was 0.14% for unemployed homeowners. The average counterfactual mobility rate for job reasons was 0.18% (based on homeowner mobility between 2006 and 2007), which is equivalent to a boost of about 29% on average. In our counterfactual exercise, the unemployment rate is 8.26%, just 11 basis points less than the real unemployment rate.[19] In comparison, from March 2007 to March 2012 (the period for which we conduct the exercise), the unemployment rate increased 382 basis points, implying that for this specification the decline in mobility can account for less than 3% of the increase in the unemployment rate.[20] Panel B reports similar results for our second mobility measure, inter-county moves that are self-reported as job-related moves.

Comparison Group: Homeowners in Other Recessions. What if homeowners move more during recessions? We consider now homeowners in the 1991 and 2001 recessions as the comparison group. We do

[19] Standard errors for the difference between u_{2012}^{C} and u_{2012} are calculated using an iterative delta method. The web appendix provides details regarding the implementation of the delta method. Essentially, $u_{2012}^{C} - u_{2012}$ is a function of the level of unemployment for homeowners for each month from March 2007 to March 2012, the level of the labor force in March 2012, the shares of job-related moves among unemployed homeowners from 2007 to 2012 ($m_t^{UH} * j_t^{UH}$), and the counterfactual shares of job-related moves among unemployed homeowners ($m_t^{UH,C} * j_t^{UH,C}$). The web appendix describes how we compute the covariance matrix of these variables and also the structure of the Jacobian of $u_{2012}^{C} - u_{2012}$ with respect to these variables.

[20] We repeat the exercise allowing for mobility to also affect transitions from employment to unemployment. Assuming that movers have an employment to unemployment transition probability of zero if they move, and the average observed transition probability in the population if they do not move implies counterfactual unemployment which is 11.1 basis points lower than the observed value.

TABLE 1: RESULTS: COUNTERFACTUAL UNEMPLOYMENT RATE

Comparison group	(1) Homeowners before the recession	(2) Homeowners '91 and '01 recessions	(3) Renter Reweighted: Percent Change '07 to '12	(4) Renter Reweighted: Percentage Points Change '07 to '12
Sample: Labor Force. March 2012 unemployment rate: 8.36				
A. All moves, self-reported job-related mobility				
March. 2012 counterfactual unemp. rate (%)	8.26	8.24	8.25	8.32
u_{2012}-u^C_{2012} (percentage points)	**0.11** (0.013)	**0.12** (0.006)	**0.12** (0.045)	**0.04** (0.102)
$(u_{2012}-u^C_{2012})/(u_{2012}-u_{2007})$ (%)	**2.88**	**3.14**	**3.14**	**1.05**
Avg. job-related mobility (%)	0.14	0.14	0.14	0.14
Avg. counterfactual job-related mobility (%)	0.18	0.19	0.19	0.16
B. Inter-county moves, self-reported job-related mobility				
March. 2012 counterfactual unemp. rate (%)	8.28	8.23	8.30	8.40
u_{2012}-u^C_{2012} (percentage points)	**0.08** (0.013)	**0.13** (0.006)	**0.06** (0.035)	**-0.04** (0.09)
$(u_{2012}-u^C_{2012})/(u_{2012}-u_{2007})$ (%)	**2.09**	**3.40**	**1.57**	**-1.05**
Avg. job-related mobility (%)	0.10	0.10	0.10	0.10
Avg. counterfactual job-related mobility (%)	0.13	0.15	0.12	0.08
Sample: Ages 16-65. March 2012 unemployment rate: 8.49				
C. All moves, self-reported job-related mobility with homeownership correction				
March. 2012 counterfactual unemp. rate (%)	8.42	8.45		
u_{2012}-u^C_{2012} (percentage points)	**0.07** (0.018)	**0.04** (0.008)		
$(u_{2012}-u^C_{2012})/(u_{2012}-u_{2007})$ (%)	**1.80**	**1.03**	N/A	N/A
Avg. job-related mobility (%)	0.16	0.16		
Avg. counterfactual job-related mobility (%)	0.19	0.18		

Notes: The table reports the results for the counterfactual exercise for four cases. Data sources include CPS monthly files and CPS March Supplement. In column 1, counterfactual mobility is based on homeowner mobility between 2006 and 2007 (the last year before the recession). In column 2 it is based on mobility rates of homeowners in the 1991 and the 2001 recessions (from a regression of mobility rates on a time trend and a recession dummy). In column 3, counterfactual mobility is calculated using the percent change in reweighted job-related mobility for renters starting 2007. Column 4 uses the percentage points change in reweighted renter mobility to construct counterfactual mobility rates. Panel A shows the results for a measure of mobility which includes all moves, and uses self-reported job-related moves to measure job-related mobility. Panel B show the results for a measure of inter-county moves, which were also self-reported as job-related moves. Panel C reports the results for a mobility measure which is similar to Panel A, with the homeownership correction applied. The population is all individuals in the labor force for Panels A and B and individuals aged 16-65 for Panel C. Standard Errors are calculated using the delta method detailed in the appendix.

not include the twin recessions in the early 1980s, as these recessions were accompanied by a housing bust that depressed mobility. Our procedure is running a regression of log mobility on a linear time trend and a recession indicator for 2001 and 1991, and project counterfactual mobility rate for the Great Recession. Since the coefficient on the recession indicator is small, this essentially reverts mobility to its long term trend.[21]

The results are reported in column 2 of Table 1. The effect is slightly larger than the case in column 1

[21] We explored different specifications, including quadratic trends, using unemployment rate as the right-hand side variable in the regression instead of a recession indicator, and repeating the regressions at the state-level. In non of these we find evidence for counter-cyclical mobility (and in some we find that it is pro-cyclical). The finding that mobility is not higher in the 1991 and 2001 recessions is consistent with the findings in Molloy, Smith and Wozniak (2011) for inter-state and inter-county mobility.

because the mobility rate was already below the trend for homeowners between 2006 and 2007, implying that reversion to trend generates counterfactual mobility that is higher than pre-recession levels.

Comparison Group: Renters During the Great Recession. The Great Recession was a far deeper recession than the 1991 and 2001 recessions, especially from the point of view of unemployment. It is possible that had the housing bust not occurred, mobility in this recession would have been higher than what was predicted in the previous case. Furthermore, if the housing bust was more severe in exactly those labor markets in which unemployment increased sharply (e.g. Nevada and Florida), then we might have seen a higher mobility rate during the Great Recession than the mobility rate observed before the recession.

To address these cases, we use renters in the Great Recession when calculating the counterfactual. Because moving costs for renters were not directly affected by the housing bust, if incentives to move were higher, then we should have seen renters move more. Moreover, while for the first two counterfactual exercises we left the share of self-reported job-related moves $\left(j_t^{UH}\right)$ as is in the data, in this exercise we use renters self-reported job-related mobility to conduct a counterfactual on j_t^{UH} as well. If movers for job are exactly the ones hit by the housing bust, depressing the shares of job-related moves for homeowners, this might show up as a change in share of job-related moves for renters. While renters are an appealing comparison group in the sense that they are not affected by the housing bust, they are different from homeowners in many aspects, both observable (for example, age and family composition) and unobservable.[22] We first reweight renters so that their demographic characteristics match homeowners (see web appendix for details) and then use the reweighted mobility series to conduct two counterfactual exercises. In column 3 we take the year-on-year percent change in rates of mobility for renters and apply them to homeowner mobility after 2007 while in column 4 we use the percentage points fall in renters reweighted mobility to construct counterfactual mobility rates.[23] When using percent changes, the difference between the observed and the counterfactual unemployment rate is 12 basis points for all moves (panel A) and 6 basis points for inter-county moves only (panel B).[24] In column 4, for the inter-county case, the counterfactual unemploy-

[22] In a recent paper, Winkler (2011) uses a structural model and shows that even when eliminating the transaction costs for homeowners, homeowner mobility is less responsive to labor market shocks. While this suggests that using renter mobility in levels is unsuitable for our exercise, it also suggests that renter mobility is expected to respond to increase in incentives to move, making the use of changes in renter mobility attractive.

[23] Using percent changes in renter mobility to construct counterfactual homeowner mobility is consistent with our upper bound approach as the percent fall in job-related mobility for renter was much smaller than homeowner while the percentage points fall in renter mobility was about the same or even larger for renters than homeowners. Also, the trends for total mobility and inter-county mobility from 1980 to 2006 are very different for homeowners and renters in levels, with renters having a much steeper trend, but are much more similar for percent changes over time. However, this feature is not robust to the homeownership correction described in Section 7. To remain agnostic about whether the correct counterfactual should use percent or percentage points change, we report both.

[24] The results are similar if we do not reweigh renters mobility to reflect homeowners demographic characteristics. If we use growth rates in non-reweighted renters mobility then the difference between the observed and counterfactual unemployment rates are 8 and 6 basis points for the all moves and inter-county moves respectively (compared to 12 and 6 basis points using reweighted

ment rate is actually higher than the observed unemployment rate as the fall in renters mobility in percentage points was larger than the fall in homeowner mobility.

One concern when using patterns in aggregate renter mobility is that this might miss heterogeneity across local labor markets. If, for example, renters are moving more in some states and less in others, the aggregate could remain unchanged. If the local changes in renter mobility are further correlated with local labor market unemployment rate, the use of aggregate renter mobility as a counterfactual might generate misleading results. To address that, we repeat our reweighted renters counterfactual using mobility of homeowners and of reweighted renters from the five states which had the highest share of mortgages with loan-to-value ratio higher than 95% in the fourth quarter of 2009 according to Corelogic (2010). These states were Nevada, Arizona, Florida, Michigan and California. Indeed, inter-county (which includes also inter-state) self-reported job-related mobility of homeowners in these states fell by 43% between 2007 and 2012 while the same measure for reweighted renters fell by only 1%. Our exercise applies these homeowner mobility rates to the entire economy, and calculates counterfactual mobility using reweighted renter job-related mobility growth in these states. It yields counterfactual unemployment of 8.24% for March 2012, just 6 basis points lower than the comparable estimate using aggregate renter mobility (Table 1, column 3, Panel B).

Finally, the Great Recession was characterized by an increase in the number of people who foreclosed on their houses. The impact of foreclosures on unemployment depends on the assumption that we make on the effect of foreclosures on labor market opportunities. Consider two extreme cases: First, suppose that foreclosures are purely forced moves of the type that have no positive effect on labor market outcome. Then, and given that foreclosing households are likely to be classified as renters post-move, our baseline results in Panels A and B of Table 1 are valid as an upper bound, because the foreclosed individuals do not fall in our group of interest (job related moves) whose labor market outcomes improve post-move. If anything, the renter mobility measures are upward biased because of foreclosures, making the estimated effects larger whenever renter mobility is used for calculating the counterfactual. On the other hand, suppose that foreclosed individuals can move to a different labor market and that this might improve their job-finding rate. Since the idea behind our counterfactual experiment is to "undo" the housing bust, it would imply that in a counterfactual economy at least some of the foreclosures would not take place. This would make the counterfactual mobility profile *lower* than it would have been otherwise. Under the assumption that foreclosed individuals are classified as renters, this implies an even smaller effect of mobility on unemployment than the already small effect reported in Table 2 for the homeownership correction which is discussed in section 7.

mobility which is reported in Table 1).

6. Counterfactual Scenarios with Large Effects on Unemployment

So far, we reported what we considered to be more or less realistic scenarios for counterfactual mobility for homeowners, trying to measure those with an upper bound. We now take the opposite approach, trying to characterize a class of counterfactual scenarios that could generate a large impact of mobility on unemployment. What would happen to unemployment if we assume that all inter-county moves are job-related (and results in a 100% finding rate)? If we turn all homeowners to renters in the recession? If we boost mobility to its historical high over the last 30 years? While non-realistic, these exercises are useful when trying to quantify what is the level of mobility rate that is required for generating a significant effect of mobility on unemployment.

In column 1 of Table 2 we assume that *all* inter-county moves are job-related, and boost mobility to pre-recession level. The effect on unemployment is of 25 basis points, about three times larger than the comparable specification in Table 1 (column 1, Panel B), but still fairly modest. In column 2 we show what would happen if instead of using *changes* in renter mobility, we would use the *level* of the reweighted renter job-related mobility measure when calculating counterfactual unemployment.[25] This counterfactual is closest to answering a question of the type: "what would have happened if all unemployed homeowners were renters?". Indeed, assuming that all unemployed homeowners move as much as renters do, and find a job with a probability of 1 upon moving, gives a much larger effect of mobility on unemployment, amounting to up to almost 17% of the increase in unemployment rate between March 2007 and March 2012.

As seen in Figure 1, inter-county mobility for homeowners peaked between 1980 and 1981. Column 3 of Table 1 presents the results for using the historically high inter-county mobility rate as counterfactual mobility *for five full years*. Unemployment rate in the counterfactual is 33 basis points less. In column 4 we reports our most extreme scenario, setting inter-county mobility rates to historically high, and assuming *all* inter-county moves result in a job finding probability of 1. The relatively large drop in counterfactual unemployment of 95 basis points, demonstrates how extreme a scenario one should invoke in order to generate a large impact of mobility on unemployment. Note that the counterfactual exercises reported in columns 2 to 4 of Table 2 require at least doubling homeowner mobility rate for job. Ferreira, Gyourko and Tracy (forthcoming) , whose estimates are in the high range of estimates for the effect of negative equity on mobility, conclude that homeowners with negative equity move 30% less often than homeowners with positive equity. Considering that not all unemployed homeowners have a mortgage and even those that

[25] Recall from Section 4 that we recovered an estimate for $\frac{m_t^{UH}}{m_t^H}$ from the PSID. Similarly, here we recover $\frac{m_t^{UR}}{m_t^R}$, the ratio of unemployed renter mobility to total renter mobility. We use a value of 1.2 which is the average of $\frac{m_t^{UR}}{m_t^R}$ for the 5 highest years.

TABLE 2: LARGE IMPACT OF MOBILITY REQUIRES UNREALISTIC SCENARIOS

	(1)	(2)	(3)	(4)
Comparison group	Homeowners before the recession	Renters, Reweighted: Levels	Historically High	Historically High
Mobility measure	Inter-county, j=1	Inter-county	Inter-county	Inter-county, j=1
Sample: Labor Force. March 2012 unemployment rate: 8.36				
March. 2012 counterfactual u (%)	8.11	7.73	8.03	7.41
u_{2012}-u^{C}_{2012} (percentage points)	**0.25** (0.039)	**0.64** (0.034)	**0.33** (0.029)	**0.95** (0.074)
$(u_{2012}-u^{C}_{2012})/(u_{2012}-u_{2007})$ (%)	**6.54**	**16.75**	**8.64**	**24.86**
Avg. job-related mobility (%)	0.32	0.1	0.1	0.32
Avg. counterfactual job-related mobility (%)	0.43	0.37	0.23	0.76

Notes: The table reports the results for the counterfactual exercise for 4 cases. Data sources include CPS monthly files and CPS March Supplement. In column 1, all inter-county moves are considered as job-related moves, and counterfactual mobility is based on homeowner mobility between 2006 and 2007 (the last year before the recession). In column 2, counterfactual mobility is calculated using levels of inter-county mobility for job-related reasons for renters starting 2007, where renters mobility is adjusted for demographics. In column 3, counterfactual mobility is based on inter-county homeowner mobility between 1980 and 1981, which is the highest observed in our data. Column 4 is similar to column 3, but like column 1, it also assumes that all inter-county moves are job-related. The population is all individuals in the labor force. Standard Errors are calculated using the delta method detailed in the appendix.

do are not necessarily underwater, the estimates from Ferreira, Gyourko and Tracy (forthcoming) imply counterfactual mobility that is much closer to the ones used in Table 1 than to those used in Table 2.

7. Alternative Measures and Specifications

We turn to look at two alternative measures of mobility, which take into account the fact that our measures so far are based on post move homeownership and unemployment status, and on annual rather than monthly mobility rates. A detailed description of the measures is given in the web appendix.

Correcting for Change in Homeownership over Time. Our measures of homeowner mobility so far have been based on homeownership post- rather than pre-move, which is potentially sensitive to changes in homeownership rates. Our correction is based on the observation that transitions from renting to owning status and vice verse would most likely be accompanied by a move. This implies that if homeownership is declining, a homeowner mobility measure based on post-move homeownership reporting, is biased downward, as it is missing all the homeowners who moved and became renters. This can be corrected by adding the change in number of homeowners to the number of homeowners who moved. The drawback of this correction, is that we could mistakenly count changes to the population of interest (the labor force) as changes to homeownership. We therefore focus on the population of individuals aged 16-65, rather than on the labor force. As we know how this population evolves over time, we can eliminate this problem. As shown in Panel C of Table 1, the measured effect of mobility is even smaller when applying the homeownership correction

for all columns.[26] This is not surprising, given that decreasing homeownership was implying a downward (upward) bias in the measure of *observed* mobility for homeowners (renters) in 2007-2012, implying that the difference between observed and counterfactual mobility is smaller.

CPS Match-Rate Mobility. One issue with the mobility measures used so far, is that unemployment status before the move is not observed. To address this we used the PSID to measure the ratio of mobility for unemployed and employed. We now utilize the panel dimension of the CPS to construct an alternative measure for mobility.[27] The CPS is an address based panel. The non-match rate of the CPS from month to month is therefore informative about mobility. The non-match based mobility measure has the advantage that it is monthly, and that homeownership and unemployment status are observed pre-move. The disadvantage is that other than mobility, sources of non-matches include non-response, mortality, and recording errors. Madrian and Lefgren (1999) show that for March-to-March match, mobility can account for up to 56% of non matches in the CPS. To address this concern we take two approaches. In the first, we rescale the match-based mobility rate to equal annual March CPS mobility. As we cannot measure job-related moves directly, we apply the self-reported job-related mobility rates to the matched-based mobility series. The measured effect on unemployment is similar to the results reported in Table 1: reverting mobility to pre-recession levels we find a 8 basis points decrease on March 2012 unemployment.[28] Our second approach is to scale the non-match rate by half to estimate the mobility for homeowners. Considering that Madrian and Lefgren (1999) found that mobility can explain up to 56% of March CPS non-matches for the entire sample (not just homeowners), this is most likely an over estimate of mobility rates for homeowners, again suggesting an upper bound measure. The results are still modest, with a 19 basis points effect of mobility on unemployment for reverting to pre-recession levels.

8. Conclusion

In this paper, we show that the fall in mobility can account for only a very small rise in the unemployment rate between 2007 and 2012. The reason for the unimportance of mobility is that flows out of unemployment

[26] While we cannot use the reweighted renter mobility measures with the homeownership correction, we can conduct the counterfactual for the homeownership correction case by using a non-reweighted measure of renter mobility. The difference between the observed and counterfactual unemployment rate in this case is 4 basis points when using percent change in renter mobility and of -11 basis point when using renter percentage points difference of mobility to calculate counterfactual mobility for homeowners.

[27] We thank an anonymous referee for suggesting this measure.

[28] The non-match monthly rates show the expected decline in unemployed homeowner mobility, from an average of 5.9% in 2007 to an average of 4.9% and 5.1% in 2010 and 2011, respectively. The rates however, have a large standard deviation within a year (ranging from 0.6% to 1.4%) implying that using renter growth rates is sensitive to the choice of the first month for counterfactual within the year. We note that according to this measure, the annual average of non-match for unemployed renters fell from 11.6% in 2007 to 9.3% in 2011, a larger drop in percent compared to homeowners.

that are related to mobility are very small to start with, and therefore boosting mobility by any reasonable amount has a small effect on the stock of unemployed workers. While we show that the lower mobility caused by the housing bust did not play a major role in explaining the increase in unemployment between 2007 and 2012, it is important to note that we cannot rule out that long-term mobility trends play a role in structural unemployment.

Although the accounting technique we adopt in this paper has the appealing feature of being simple and transparent, it also has some limitations. There are some channels through which mobility might affect unemployment which are not taken into account by our framework. One such channel is the effect of mobility on job-to-job transitions. While we briefly address the direct effect of reduced job-to-job transitions on unemployment by looking at employment to unemployment flows, there might be an indirect effect operating through reduced productivity for existing matches. We also do not consider labor market equilibrium effects, or the effect of mobility on demand for services (such house renovation). While our methodology cannot address these caveats directly, given that our results show such a small direct effect of mobility on unemployment, we believe that it is unlikely that any of the indirect channels can amplify the effects to make mobility an important factor in explaining the high unemployment rate during and after the Great Recession.

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