Received: 30 January 2017 | Revised: 28 September 2017 | Accepted: 4 December 2017
DOI: 10.1111/jors.12380

WILEY JOURNAL OF REGIONAL SCIENCE

ORIGINAL ARTICLE

Decreasing house prices and household mobility: An empirical study on loss aversion and negative equity

Joep Steegmans[1] | **Wolter Hassink**[1,2]

[1]Utrecht University School of Economics, Utrecht, the Netherlands

[2]Institute for the Study of Labor (IZA), Bonn, Germany

Correspondence
Joep Steegmans, Utrecht University School of Economics, P.O. Box 80125, 3508 TC Utrecht, The Netherlands.
Email: J.W.A.M.Steegmans@uu.nl

Funding information
Ministry of the Interior and Kingdom Relations

Abstract

This paper examines the effects of loss aversion and negative equity on household mobility. We stress the importance of studying these mechanisms simultaneously. By making use of a unique administrative data set of Statistics Netherlands, covering the period 2006–2011, we estimate the effects of loss aversion and negative equity. The results provide strong evidence for loss aversion, while less evidence is found for a lock-in effect of negative equity. The results indicate that moderately underwater households do have a lower mobility, but heavily underwater households do not. Additional results indicate that the particularly high mobility of heavily underwater households is not default-driven.

KEYWORDS
household mobility, housing market, loss aversion, negative equity

1 | INTRODUCTION

House prices in the Netherlands have been rising from the early 1980s until prices peaked in 2008. The following drop in house prices led to a sharp decrease in transaction numbers. Loss aversion and negative equity can both explain how decreasing house prices affect household mobility. The decrease in house prices and its effects on household mobility have been debated widely, but there seems to be no agreement on the exact mechanisms. The relation between decreasing house prices and household mobility, therefore, deserves further attention.

We will study the effects of decreasing house prices in the owner-occupied market on sales rates and household mobility as it is not clear whether the decrease in transaction numbers is caused by financial constraints or by loss aversion. We will investigate whether households did not want to move or were no longer able to do so after prices started dropping. Studying the difference between the binding and nonbinding constraints will lead to a better understanding on how the housing market functions.

Two main strands of literature exist within the study of reduced household mobility due to decreasing house prices. The first strand focuses on nominal loss aversion. Loss averse households are not willing to sell their home for less than they paid themselves (Engelhardt, 2003; Genesove & Mayer, 2001). Facing a prospective nominal loss thus reduces mobility. Even though these households could move from a financial point of view, they are not willing to do so at a nominal loss. The second strand focuses on reduced mobility due to financial constraints (Chan, 2001; Ferreira, Gyourko, & Tracy, 2010, 2012; Henley, 1998; Schulhofer-Wohl, 2012). Equity constraints may severely limit possibilities of obtaining a mortgage for a new home. Households with negative equity are spatially locked-in as they are not able to move; even though there is no formal downpayment constraint in the Netherlands, the prospective residual debt causes a barrier in obtaining a new mortgage.

Most scholars have studied the effects of loss aversion and equity constraints on household mobility separately. We argue, as did Engelhardt (2003), that loss aversion and negative equity effects should be studied simultaneously. We contribute to the existing literature by making a clear distinction between loss aversion and negative equity effects and estimate the effects simultaneously. Besides, we provide estimates of negative equity effects conditional on household savings and look into voluntary and involuntary mobility. To the best of our knowledge, loss aversion and negative equity have not been investigated this extensively before in connection with housing markets.

Our analysis makes use of a unique administrative data set of Statistics Netherlands that contains the stock of Dutch owner-occupied houses and the traits of the households living in them. The period under investigation, 2006–2011, contains the peak in house prices and the following decline. Differences in housing durations and price decreases provide the variation that we need for estimation and identification. This paper makes use of duration analysis to estimate the hazard rates of moving. The hazard rates are estimated with an extended Cox model.

The results suggest a strong effect of loss aversion. A 1 percent increase in prospective nominal loss decreases household mobility by more than 20 percent. We find limited evidence of negative equity effects. Moderately underwater households seem to have a somewhat reduced mobility but heavily underwater households are the most mobile of all. Furthermore, the positive effect of household savings on mobility for underwater households provides evidence that the mobility is voluntary.

The remainder of this paper is organized as follows. Section 2 provides the institutional setting. Section 3 presents the theoretical background. Section 4 discusses the data set and variables. Section 5 describes the empirical model. Section 6 reports the estimates. Section 7 looks into the robustness of the results by incorporating local labor market conditions. Section 8 summarizes and concludes.

2 | INSTITUTIONAL SETTING

In the Netherlands, during the period under investigation (2006–2011), few restrictions existed on the deductibility of home mortgage interest payments. That is, the interest paid on the home mortgage is fully deductible from taxable income as long as the property is occupied by the owner. The United States is one of the few countries in the world where home mortgage interest payments are fully deductible too (Rouwendal, 2007). Still, apart from the shared interest deductibility important institutional differences exist between the Netherlands and the United States.

The most notable differences are the absence of a formal downpayment requirement and the existence of exclusively recourse mortgages in the Netherlands. The first implies that Dutch households do not have to bring in their own money if they buy a house. As a matter of fact, Dutch homeowners have been generally allowed to borrow more than the value of the house.[1] Equity constraints, therefore, are likely to be smaller in the Netherlands than in markets where formal downpayment requirements exist. The second institutional difference implies that "strategic" defaults do not exist in the Netherlands: defaulting would leave a household with a residual debt. Households are thus not able

[1] A binding code of conduct for mortgage loans (GHF), setting a formal limit to the loan-to-value (LTV) ratios, was introduced in August 2011. The initial LTV limit was set at 106 percent, decreasing to 100 percent in 2018.

to walk away from the negative housing equity if the mortgage is larger than the house value. Consequently, voluntary defaults do not exist in the Netherlands.

The lack of a formal downpayment requirement and the fiscal policy that encourages mortgage debt through the full deductibility of mortgage interest payments have led to high LTV ratios, making the LTV ratios in the Netherlands among the highest in the world (Dröes & Hassink, 2014; Dutch Central Bank & Netherlands Authority for the Financial Markets, 2009). Summary statistics for the LTV ratios are presented in Section 4.

The relationship between mobility and house sales also differs between the Netherlands and the United States. More particularly, house sales are a good measure of mobility of homeowners in the Netherlands as nonsale moves of owner-occupiers are rare. Sale-and-rent-back constructions have been virtually nonexistent in the Netherlands; that is, a very small number of parties have offered either sale and rent backs or sale with lifelong usufruct to the elderly but a market never developed. By 2010, the market had come to a standstill altogether (Reifner, Clerc-Renaud, Pérez-Carrillo, Tiffe, & Knobloch, 2009; Taskforce Verzilveren, 2013).

Moving while renting out the old home is not common in the Netherlands either. The tenant protection is such that with a regular contract even the sale of a property is "*not* a valid reason for the termination of a rental contract" (Huisman, 2016a, p. 97, emphasis hers), making houses with tenants in them not desirable for potential buyers. Consequently, owner-occupiers will opt for temporary rental contracts; otherwise they become permanent landlords. The Law on Vacancies (*Leegstandswet*) allows for temporary contracts but only under strict conditions (Huisman, 2016b).

In order to get a permit for a temporary rent out from the municipality, the property has to be vacant and for sale. The permit is valid for a limited duration only and it comes with a rent ceiling (Haffner, Van der Veen, & Bounjouh, 2014; Huisman, 2016a, 2016b; Scanlon & Elsinga, 2014). Still, based on the political debate it seems that getting permission from the mortgage lender to rent out the house is an even larger hurdle than getting permission from the local government (e.g., De Jager, 2009; Van der Laan, 2009). It was only post-2012 that the strict conditions on temporary tenancies of (previously) owner-occupiers dwellings were lifted (Huisman, 2016b). The Dutch association of owner-occupiers (VEH) claims that during the period 2006–2011 the Law on Vacancies was "almost exclusively" used by nonprivate parties (Vereniging Eigen Huis, 2017, translation ours). Official statistics, however, are not available (Huisman, 2016a).

All in all, even though sales might not perfectly measure household mobility of owner-occupiers, in the case of the Netherlands it does seem plausible to define mobility in terms of house sales. A polemic as the one that developed between Ferreira et al. (2010, 2012) and Schulhofer-Wohl (2012) about whether "temporary moves"—moves by households that do return to their (unsold) original home—may explain the difference between finding a negative effect of negative equity on household mobility or finding a positive effect would therefore not apply to the Netherlands.

3 | THEORETICAL BACKGROUND

3.1 | Loss aversion

Loss aversion is one of the mechanisms that explains how decreasing house prices can deter household mobility. Loss aversion describes the behavior that households are not willing to incur a nominal loss if they sell their house. The nominal price that was originally paid functions as a reference point in the household's selling decision (Genesove & Mayer, 2001). Prospective losses thus deter residential mobility (Engelhardt, 2003). Loss aversion was first introduced in prospect theory to describe the behavior that people give more importance to avoiding losses than to obtaining equivalent gains (see Kahneman & Tversky, 1979; Tversky & Kahneman, 1991).[2]

In their seminal paper, Genesove and Mayer (2001) apply loss aversion to the housing market and study the effect of nominal loss aversion on asking prices, selling prices, and time-on-the-market. They corroborate that sellers use the transaction price that they originally paid as a reference point in their selling decision. Based on data of downtown

[2] It should be noted that in most housing market studies the gain domain is not defined; that is, nominal losses are not compared to equivalent gains but to the more general situation where losses do not occur (e.g., Anenberg, 2011; Genesove & Mayer, 2001).

Boston for the years 1990–1995 they conclude, as hypothesized, that facing a nominal loss leads to a higher selling price. The higher selling price is the result of a higher list price and a lower probability of sale. Genesove and Mayer (2001) do not study household mobility itself, but following their paper mobility studies have started to incorporate loss aversion into their studies.

Engelhardt (2003) studies the effect of equity constraints and loss aversion on household mobility in the United States. The focus is on the identification of these effects as both occur when prices start falling; periods of declining house prices are required for both binding equity constraints and nominal loss aversion. High equity households that are (financially) unconstrained are used for the identification of the nominal loss effect, while households potentially at risk of being constrained are used for the identification of negative equity effects. Engelhardt (2003) concludes that: "Household mobility is significantly influenced by nominal loss aversion. There is little evidence that low equity because of fallen house prices constrains mobility" (p. 171). Anenberg (2011) focuses on the effects of loss aversion and negative equity on house prices. He finds strong evidence that nominal losses and high LTV ratios have a positive effect on the selling price.

Loss aversion in the Dutch housing market has received almost no attention. Eichholtz and Lindenthal (2013) are a notable exception. They study loss aversion through the centuries based on housing transactions of the Herengracht in Amsterdam, spanning 324 years. They conclude that loss aversion has gotten more important over time. Still, a major concern of this paper is that it does not differentiate between loss aversion and equity effects. Financial constraints are even explicitly mentioned as an explanation for the psychological barrier that is loss aversion (Eichholtz & Lindenthal, 2013, p. 13).

3.2 | Equity constraints

Equity constraints are the second mechanism that relate decreasing house prices and household mobility. Stein's downpayment model (1995), which relates the downpayment constraint in the mortgage market to prices and trading volume, is the basis of most of the empirical studies on equity constraints. We will not focus on low (positive) equity households as there is no downpayment requirement in the Netherlands; instead we concentrate on the situation where the mortgage is larger than the contemporaneous house value, that is, negative housing equity. Having negative equity, or being "underwater" as it is also called, can make it impossible to obtain a mortgage for a new home. Like low equity households in a downpayment setting these households are spatially locked-in (Chan, 2001). Nonetheless, negative equity could also increase mobility through defaults and foreclosures.

Henley (1998) is one of the first to study the effects of negative equity on household mobility. He finds strong evidence that negative net housing equity deters residential mobility and labor market flexibility. Chan (2001) studies whether falling house prices reduce mobility of households with little equity (high LTV ratios). If such a household sells its house, it is left with insufficient funds to repay its mortgage and make a downpayment on a new home, leading to a spatial lock-in. The household's contemporaneous LTV ratio is the variable of main interest. The crucial value for the LTV is set at 80 percent, as it is assumed that higher LTV ratios make a downpayment on a new house impossible. Chan (2001) recognizes that loss aversion may affect mobility and incorporates a cumulative house price change variable in the estimated models.[3] She does conclude that there is clear evidence of "severe constraints to mobility as a result of negative housing market shocks" (p. 584).

Ferreira et al. (2010, 2012) and Schulhofer-Wohl (2012) find contradicting results for negative equity based on the same U.S. data. Ferreira et al. (2010, 2012) find a negative effect of negative equity on household mobility while Schulhofer-Wohl (2012) finds the contrary, i.e., that homeowners with negative equity are more mobile. Schulhofer-Wohl (2012) argues that Ferreira et al. (2010) underreport household mobility by excluding temporary moves, moves by households that do return to their original home.

Coulson and Grieco (2013) find that underwater households are more mobile than households with positive equity. That is, moderately underwater households have the same mobility rate as above-water households, while heavily

[3] In this specification, the cumulative house price change measures more than only loss aversion, so no conclusive results of a loss aversion effect are presented.

underwater households are the most mobile category. The results, therefore, go against the predictions of the lock-in mechanism. Coulson and Grieco (2013) give both increased mobility due to defaults and increased mobility in order to prevent an approaching default as possible explanations for the empirical findings. The results found by Coulson and Grieco (2013) indicate that lock-in may not be the only mechanism through which negative equity can affect household mobility.

It is regularly hypothesized that defaults and foreclosures may increase mobility (Chan, 2001; Ferreira et al., 2010; Schulhofer-Wohl, 2012). Andersson and Mayock (2014) explicitly differentiate between voluntary mobility and default-induced mobility (due to strategic behavior or the inability to pay), i.e., they disentangle the lock-in mechanism from the default mechanism. Their results show a U-shaped relationship between equity and household mobility; at moderate debt levels an increase in debt decreases mobility, while at high debt levels an increase in debt increases mobility.[4] In other words, they find that for low levels of negative equity the lock-in effect dominates, while for high levels of negative equity the default mechanism dominates.[5]

3.3 | Simultaneous mechanisms

Both loss aversion and equity constraints are driven by decreasing house prices, resulting in a positive correlation between them. The correlation between the two mechanisms seems to make it impossible to study one without the other. Estimating the effect of negative equity without incorporating loss aversion will overestimate the absolute effect of negative equity, that is, the true effect of negative equity is likely to be less negative than found in studies that do not account for loss aversion.

Strong evidence exists that loss aversion has a negative effect on mobility, whereas the evidence for a negative effect of equity constraints is less conclusive. Prior studies that take loss aversion into account have found little evidence that equity constraints hampers mobility (Engelhardt, 2003). Studies that do find a lock-in effect of equity constraints have generally refrained from distinguishing between loss aversion and negative equity effects (Henley, 1998; Ferreira et al., 2010, 2012; Struyven, 2015).

In our analysis, we will distinguish between loss aversion and negative equity effects. We will look into nonhousing wealth of underwater households as being locked-in is conditional on household savings; it is not evident that negative housing equity hinders mobility if a household has additional sources of wealth. By taking into account nonhousing wealth, we are able to investigate the U-shaped relationship between negative equity and household mobility that is suggested by Andersson and Mayock (2014). To our knowledge, this study is the first to investigate the relationship between decreasing house prices and household mobility in such detail.

4 | DATA

4.1 | Data set

The data set, covering the period 2006–2011, consists of housing spells and characteristics of households living in the stock of owner-occupied existing row houses in the Netherlands.[6] Most of our observations have housing spells that started before our stock sampling date, January 2006. Houses and households are observed annually until 2011, or

---

[4] Andersson and Mayock (2014) lump all LTV ratios between 0 and 0.8 together in a single group (over 53 percent of their sample). Equity effects for above-water households with LTVs under 0.8 can therefore not be distinguished, while Henley (1998) shows that household mobility increases with positive house equity. Coulson and Grieco (2013) also provide estimates that show that household mobility increases with positive house equity for above-water households, up to an LTV of 0.9.

[5] Ghent and Kudlyak (2011) study differences in default between recourse and nonrecourse states in the United States. The results indicate that having a recourse loan affects default through a decrease in the sensitivity to negative equity. In recourse states, defaults are involuntary (due to liquidity constraints), while in nonrecourse states defaults may also be strategic. The lack of nonrecourse loans in the Netherlands suggests that default-induced mobility will be substantially lower than in countries with nonrecourse loans.

[6] The housing stock is divided into existing homes and newly build houses. Newly build houses only enter the analysis after they have been sold, that is, after they have become an existing home.

until the moment that the house is sold. The data set is extended with new housing spells beginning between 2006 and 2011. That is, houses and households can reenter the data set after a sale. These latter observations have spells that started after the stock sampling date. The data set is thus constructed as a stock sample extended with an inflow sample. In total, the data set consists of 2,441,598 observations of 566,900 unique spells.[7]

The data set has been constructed by making use of unique administrative data of Statistics Netherlands (CBS). The data set combines individual data from the Cadastre records (*Bestaande Koopwoningen*), the Housing Stock Register (*Woonruimteregister verrijkt*), the Population Register (*Adresbus*, *Huishoudensbus*, *Persoontab*), the Integrated Capital Data Set (*Integraal Vermogensbestand*), and the Integrated Income Data Set (*Integraal Huishoudens Inkomen*). Job Register information (*Baankenmerkenbus*, *Baansommentab*, *Hoofdbaanbus*) is used for robustness checks.

The Cadastre records are matched with the Housing Stock Register to identify the owner-occupied houses in the Netherlands. The Cadastre records contain information on transactions of existing homes, thereby providing information on mobility and housing duration. The transaction records consist of both voluntary and involuntary sales; forced sales are included in the data but it is not possible to distinguish them from the other sales. The Population Register, based on information from the municipalities, contains information on household composition and demographic characteristics. The Dutch tax authority is the main source of information for both the Integrated Capital Data Set and the Integrated Income Data Set. The former provides information on the assets and liabilities of the households, while the latter contains information on household income and the income composition. The Job Register has been compiled by Statistics Netherlands out of administrative sources from the tax office and the Employee Insurance Agency (UWV). It provides information on all employment relationships in the Netherlands (see Schoonhoven & Bottelberghs, 2014).

The panel data set that we have constructed contains the stock of owner-occupied row houses in the Netherlands and the characteristics of the households living in these homes. Row house are by far the largest category of family homes, making up almost 50 percent of the transactions during the years 1995–2011; the annual percentage ranges between 45.0 and 50.2 percent (CBS StatLine, 2017). As row houses have on average lower transaction prices than corner houses, semidetached houses, and detached houses, the households living in them have on average lower incomes and wealth (Steegmans & Hassink, 2017, pp. 12–13). Households living in row houses tend to have shorter durations than households living in the other types of family homes. This implies that left-censoring, an unobserved spell start, is less of a problem for row houses (see Section 5.2). It is due to computational limitations and the advantages regarding left-censoring that we restrict our analysis to row houses.

4.2 | Spell length and mobility

The Cadastre records (1995–2011) are the main source for our owner-occupied housing duration variable. For the stock-sampled observations, house sales in the period 1995–2005 provide the beginning of the spell if a house is an existing home; the duration start of houses that were newly build in the period 1995–2005 is found in the Housing Stock Register. Durations of houses last sold before 1995 are not observed directly.[8] For the inflow-sampled observations, the spell begins as soon as a house is bought after the stock sampling date. House sales in the period 2006–2011 provide, if a house is sold, the end of a spell for both the stock-sampled and the inflow-sampled observations. A move is thus defined as a house sale after the stock sampling date.

Table 1 shows the distribution of the starting years of the housing spells at the stock sampling date. The table shows that of the spells that started before January 2006 11.50 percent (51,497 observations) did start before 1995. For these observations, the exact spell start is not observed; these observations are said to be left-censored. The way to handle left-censored observations is discussed in Section 5.2 (and in more detail in Appendix C).

Figure 1 shows the regional distribution in median duration in the Netherlands. The economic core, the *Randstad*, has relatively long durations compared to the periphery. However, major differences are observed in the so-called

[7] Including the observations with an unobserved spell start the data set counts 2,576,225 observations of 619,420 unique spells.

[8] The Housing Stock Register provides the date that a (newly build) house is added to the housing stock. These addition dates go back until January 1992. However, as (re)sales between 1992 and 1994 are not observed the spell start of the houses that were newly build between 1992 and 1994 cannot be determined with absolute certainty.

TABLE 1 Year of duration start

	Frequency	Percent	Cum. percent
pre-1995	51,497	11.50	11.50
1995	23,279	5.20	16.69
1996	27,110	6.05	22.75
1997	30,475	6.80	29.55
1998	34,975	7.81	37.36
1999	37,812	8.44	45.80
2000	36,069	8.05	53.85
2001	40,844	9.12	62.97
2002	44,302	9.89	72.86
2003	44,474	9.93	82.79
2004	43,115	9.63	92.42
2005	33,960	7.58	100.00
Total	447,912	100.00	

Note: Statistics of stock-sampled row houses in 2006.

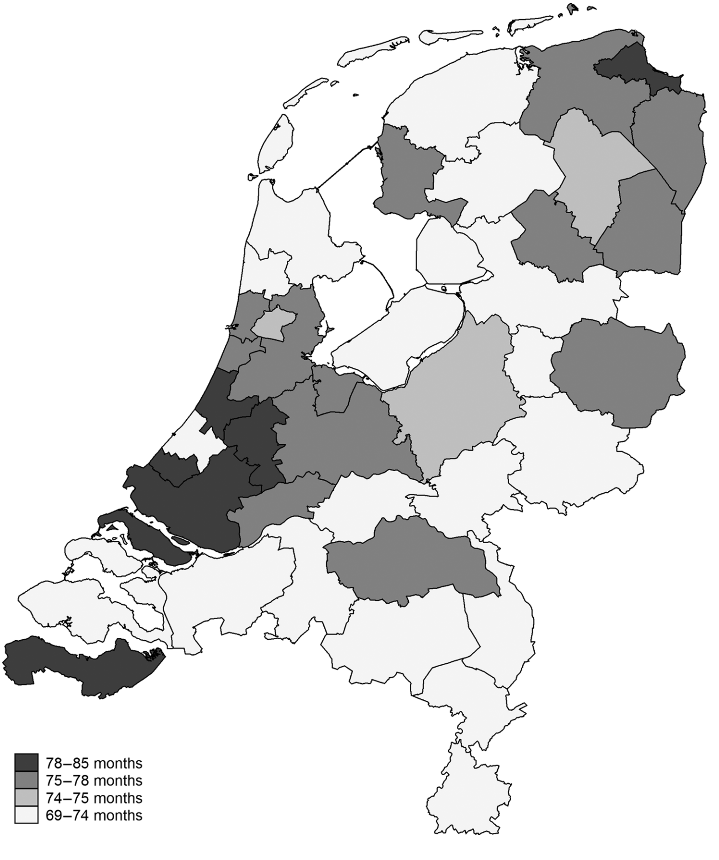

FIGURE 1 Median duration of stock-sampled row houses in the Netherlands in 2006

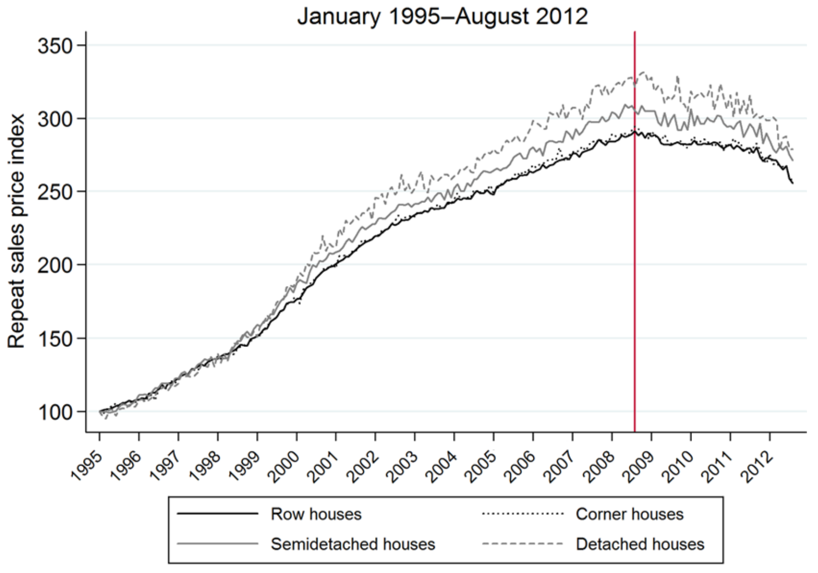

FIGURE 2 Repeat sales price index for family homes in the Netherlands (nominal price development) [Color figure can be viewed at wileyonlinelibrary.com]

shrinking regions: the southwest corner (Zeeuws-Vlaanderen) and the northeast corner of the Netherlands (Groningen) have relatively long durations, whereas the durations in the southernmost province (Limburg) are relatively short. Evidently, the regional differences in duration imply differences in mobility as well.

4.3 | Decreasing prices

The price development of family homes in the Netherlands is presented in Figure 2. The repeat sales price indices that we have estimated show that house prices peaked in 2008.[9] Prices gradually increased up to 2008 and started decreasing afterward; for row houses prices decreased 6.2 percent on average between August 2008 and December 2011. The figure shows that the price development of row houses is very similar to the development of the other types of family homes. The price decreases are important as they are the main driver for both negative equity and loss aversion. In the following subsections we will look into the measures of negative equity and loss aversion. Summary statistics of the remaining household characteristics and statistics per cohort can be found in Tables B1 and B2 in Appendix B.

4.4 | Prospective losses

Observed sale prices cannot be used to identify loss aversion as unsold houses are the likeliest to be affected by loss aversion and their sale prices are by definition not observed. Instead of actual losses we have to resort to prospective losses. After all, whether a nominal gain or loss would occur depends on the price that could be obtained if the house was to be sold, while potential losses could result in transactions not taking place.

In this study, we define the market value of a house as the purchase price adjusted by the cumulative change in the regional repeat sales price index.[10] In other words, the market value of a house is determined by the price at which the

[9] The estimation of the repeat sales price index is discussed in Appendix A.

[10] As we are interested in the (relative) price development only, the smoothed repeat sales price index fits our purpose very well. A comparison between various price indices for the Netherlands is done by Jansen, de Vries, Coolen, Lamain, and Boelhouwer (2008) and De Vries, de Haan, van der Wal, and Mariën (2009).

house was bought (P_0), the price index at the time the house was bought (I_0), and the contemporaneous price index (I_t).

$$P_{it} = P_{i0}\left(1 + \frac{I_{ct} - I_{c0}}{I_{c0}}\right), \tag{1}$$

where subscript c of the price index denotes the region.

A household faces a prospective nominal loss if the contemporaneous value (P_t) is less than the price that was initially paid (P_0). Given that P_t is expressed in terms of P_0 the size of the prospective loss can be expressed in terms of the price index.

$$pros.\ loss = \begin{cases} 0 & \text{if } I_{ct} \geq I_{c0}, \\ \frac{I_{c0} - I_{ct}}{I_{c0}} * 100\% & \text{if } I_{ct} < I_{c0}. \end{cases} \tag{2}$$

We have estimated monthly repeat sales price indices for all 40 COROP regions in the Netherlands.[11] That means that loss aversion is identified through the use of the regional repeat sales price index.[12] The estimation of the repeat sales price indices is discussed in more detail in Appendix A.

Figure 2 implies that only houses that were bought not that long before the stock sampling date are confronted with potential nominal losses. Even though regional differences exist, it is only toward 2011 that prices had decreased until the price level of around 2006. This means that the lion's part of the households facing a prospective loss have spells that started after the stock sampling date. For the households with a spell starting before January 2006, 0.16 percent of the observations (3,411 obs.) have a prospective loss, while for the households with a spell starting after January 2006, 30.5 percent of the observations (107,167 obs.) have a prospective loss.[13]

4.5 | LTV ratios

The effects of negative equity are studied by making use of the household's LTV ratio, i.e., the value of the mortgage relative to the value of the house, which is observed annually. The LTV information originates from the Dutch tax authorities.[14] We observe the overall value of the mortgage; that is, if multiple mortgages on a home exist we observe the total value only. The value of the house finds its origin in the (administrative) municipality valuation, the so-called WOZ value. The municipality bases its valuation on house characteristics, local price developments, and comparable properties. The valuation procedure is legally defined in the Netherlands (Council for Real Estate Assessment, 2010; Dröes & Hassink, 2014). It has to be noted that the LTV ratios are occasionally overestimated as the asset side in endowment mortgages (in Dutch *beleggingshypotheek* and *spaarhypotheek*) is not observed by the tax authorities.[15]

[11] The COROP regions were defined in 1971 by a committee named *Coördinatiecommissie Regionaal Onderzoeksprogramma*, hence the name COROP. A COROP is an administrative region, in size between provinces and municipalities, that joins together regional labor markets based on commuting flows. Most COROPs, therefore, consist of a larger city and its periphery. Estimation at a lower level of aggregation is not possible as observations become too sparse.

[12] The regional indices have been smoothed through (second degree) local polynomial smoothing in order to limit monthly fluctuations from the trend.

[13] For robustness purposes, we have also defined prospective nominal losses in terms of the observed previous transaction price, P_{i0}, and the house value based on the formal municipality valuation, P_{it} (see the next subsection for details). For this smaller sample, the findings are qualitatively the same although the coefficients for prospective losses and negative equity become smaller in absolute terms. We prefer regional price indices to define prospective losses as it provides monthly observations under few assumptions and, therefore, seems likelier to correspond to household decision making. Still, one could argue that municipal valuations correspond closer to self-assessed house values than the house value that we use. However, a detailed discussion of the role self-assessed values is beyond the scope of this study.

[14] As LTV data are available only from 2006 onward, we are not able to differentiate the effect of the initial LTV, that is, the LTV at the moment of origin of the mortgage, from the overall LTV effect, which incorporates the price decreases.

[15] Using Dutch survey data, Schilder and Conijn (2012) exploit information on mortgage expenditures and interest payments to estimate the asset side of endowment mortgages and include endowment mortgage assets in the calculated potential residual debt. They estimate that households with an expected residual debt have, on average, a capital insurance worth 5,950 euros. Nevertheless, as this information is not available in our data set we are not able to follow this approach.

TABLE 2 Percentiles of loan-to-value ratios

	Nonleft-Cens.	Left-Cens.	Total
p1	0.000	0.000	0.000
p5	0.250	0.000	0.117
p10	0.394	0.000	0.283
p25	0.591	0.163	0.523
p50	0.823	0.335	0.777
p75	1.007	0.538	0.991
p90	1.128	0.787	1.117
p95	1.218	0.984	1.208
p99	1.405	1.322	1.401
Observations	396,415	51,497	447,912

Note: Statistics of stock-sampled row houses in 2006. Spells starting before 1995 are left-censored. The respective percentiles are given by p1 until p99.

Table 2 shows the distribution of LTV ratios for left-censored and nonleft-censored observations.[16] The table shows that households with the longest spells, that is the spells that started before 1995, have lower LTV ratios. The median LTV for spells that started before 1995 is 0.335, whereas the median LTV for spells starting after 1995 is 0.823. The table also shows that within the left-censored observations many more households have paid off their mortgages than within the nonleft-censored observations, between 10 and 25 percent and 1 and 5 percent, respectively. These differences suggest that simply discarding the left-censored observations when analyzing equity effects might affect the results.

The LTV ratios have been used to create seven LTV groups, which increase 0.2 (20 percent) per category (see Table 3). The latter two groups, LTV between 1.0 and 1.2 and LTV above 1.2, respectively, are so-called underwater households as their mortgage is larger than their house value. In Table 3, the underwater households have also been subdivided into different groups based upon additional wealth, i.e., wealth excluding housing wealth. The table shows that the great majority of underwater households has additional wealth, but that the additional wealth is smaller than the amount that the household is underwater. This holds for both the moderately ($1.0 < \text{LTV} \leq 1.2$) and the heavily ($\text{LTV} > 1.2$) underwater households.

5 | EMPIRICAL MODEL

5.1 | Specification of the hazard rate

Duration analysis is particularly well-suited to study mobility in the housing market as it easily allows for the inclusion of (right) censored observations and duration dependence; that is, duration analysis does not exclude households that do not move or sell from the analysis, while at the same time duration length itself is allowed to have an impact on the moving or selling probability of a household. Mobility is generally studied by estimating hazard rates, i.e., the probability that a household will move in a given period conditional on not having moved before. In order to analyze housing duration we will be estimating an extended Cox model. We will be applying a continuous time specification as the ratio of the interval length (duration is measured in months) to the typical housing duration is relatively small (Jenkins, 2005, p. 21).

[16] Following the convention in Dutch studies, LTV ratios larger than 1.5 have been excluded from the analysis (e.g., Schilder & Conijn, 2012; Van Veldhuizen, Vogt, & Voogt, 2016). As information on loans and values originates from the same data set, we consider the (remaining) large LTV ratios plausible. The distribution of LTV ratios seems to corroborate this. Furthermore, the conclusions in this paper are robust to lowering or increasing the LTV upper limit.

TABLE 3 Ratios of LTV groups with and without left-censored observations

	Nonleft-Cens.	Left-Cens.	Total
LTV ≤ 0.2	0.040	0.298	0.070
0.2 < LTV ≤ 0.4	0.063	0.295	0.089
0.4 < LTV ≤ 0.6	0.156	0.207	0.162
0.6 < LTV ≤ 0.8	0.216	0.106	0.203
0.8 < LTV ≤ 1.0	0.264	0.048	0.240
1.0 < LTV ≤ 1.2	0.203	0.027	0.183
LTV > 1.2	0.057	0.020	0.053
Moderately underwater 1.0 < LTV ≤ 1.2 (subgroups):			
W < 0	0.004	0.001	0.004
0 ≤ W < U	0.196	0.025	0.177
W ≥ U	0.003	0.001	0.003
Heavily underwater, LTV > 1.2 (subgroups):			
W < 0	0.001	0.001	0.001
0 ≤ W < U	0.055	0.019	0.051
W ≥ U	0.001	0.001	0.001
Observations	396,415	51,497	447,912

Note: Statistics of stock-sampled row houses in 2006. Moderately underwater (1.0 < LTV ≤ 1.2). Heavily underwater (LTV > 1.2). Additional wealth (W). Amount underwater (U).

The Cox proportional hazard model (Cox, 1972, 1975) has empirically been very successful (Cameron & Trivedi, 2005). The Cox proportional hazard is a semiparametric method: nonparametric regarding the baseline hazard, parametric regarding the effects of the set of covariates. The starting point is the standard proportional hazards framework. The hazard rate is given as follows (see Cameron & Trivedi, 2005):

$$\lambda(t|\mathbf{x}, \beta) = \lambda_0(t)\phi(\mathbf{x}, \beta), \tag{3}$$

where t is duration, $\mathbf{x}$ is the set of covariates, and λ_0 is the baseline hazard. The baseline hazard is a function of t alone and $\phi(\mathbf{x}, \beta)$ is a function of $\mathbf{x}$ alone. As $\phi(\mathbf{x}, \beta)$ is generally specified in an exponential form, i.e., $\exp(\mathbf{x}'\boldsymbol{\beta})$, the conditional hazard rate becomes:

$$\lambda(t|\mathbf{x}, \beta) = \lambda_0(t)\exp(\mathbf{x}'\boldsymbol{\beta}). \tag{4}$$

The hazard functions $\lambda(t|\mathbf{x})$ are all proportional to the baseline hazard, hence its name. Differences in characteristics simply imply a scaling of the baseline hazard. The scaling factor is given by $\exp(\mathbf{x}'\boldsymbol{\beta})$. In other words, the hazard ratios depend on the covariates but not on t. Cox (1972, 1975) suggested a partial likelihood approach that allows for estimation of the parameters without estimating the baseline hazard.

The Cox proportional hazard model can easily be extended to include time-varying covariates

$$\lambda(t|\mathbf{x}(t)) = \lambda_0(t)\phi(\mathbf{x}(t), \beta). \tag{5}$$

However, as $\mathbf{x}$ depends on t the proportionality factor now varies with survival time, that is, the proportional hazard assumption is no longer satisfied. Still, as long as the partial likelihood is adjusted accordingly, the model can be estimated (Cameron & Trivedi, 2005; Jenkins, 2005). It is the Cox model with time-varying covariates that is called the extended Cox model. Even though it is not a proportional hazard model in a strict sense, it is often referred to as a proportional hazard with time-varying covariates (Cameron & Trivedi, 2005, p. 991).[17]

5.2 | Left truncation and left-censoring

The above model could directly be estimated if one uses an inflow sample, that is, a sample of all households starting a housing spell in a given time interval. However, a large part of our data set consists of a stock sample. These spells start before our stock sampling date. The problem is that the probability of observing a short duration is smaller than observing a longer duration. This sample selection problem is known as left truncation. It is easy to deal with as long as we observe the starting dates of the spells and have observations of some spells after the sampling date (Cameron & Trivedi, 2005). We can correct for the sample bias by taking into account the time between the start of the spell and the moment of sampling. Put differently, we can analyze the observations conditional on surviving up to the sampling date (Jenkins, 2005, pp. 64–66).

Some of the houses in our stock sample have not been sold between 1995 and 2005. The exact starting dates of these housing spells remain unobserved. These observations are said to be left-censored. Left-censoring could lead to a selection bias as the longest durations are excluded from the analysis (Iceland, 1997). Even though the proportion of left-censored spells is relatively small in our data set, 11.50 percent of the stock sampled observations (see Table 1), we consider it necessary to investigate whether excluding left-censored spells causes selection bias in our results.

In order to examine the effects of excluding the left-censored observations from the analysis, we will provide estimation results with and without the left-censored spells. We employ two different methods to include the left-censored observations. The first method uses a proxy/matching approach. More precisely, the Housing Stock Register provides likely starting dates for "left-censored homes" that have been added to the housing stock between 1992 and 1994. The proxied spell starts of houses newly built in the period 1992–1994 are then matched, based on the age of the head of the household, with the remaining left-censored observations. The second method naively substitutes the left-censoring date as the spell start. The comparison of the results with and without the left-censored spells will show whether omitting the left-censored spells leads to selection bias (Iceland, 1997; Stevens, 1999).[18] For details on left-censoring, the potential bias, and ways of handling left-censoring, we refer to Appendix C.

5.3 | Covariates

The variables of main interest are the loss indicator, indicating whether there is a prospective nominal loss based on the regional house price index, and the LTV indicators. The other covariates that will be used to estimate Equation (5) include a loan-to-income (LTI) indicator (six categories), an age indicator (10 categories), a household type indicator (seven categories), a gender indicator, a divorce indicator, a region indicator (40 COROPs), and a cohort indicator (16 start years). The latter captures, for instance, mortgage particularities that are shared by households that bought at a specific time.

The LTV categories are LTV below 0.2, 0.2–0.4, 0.4–0.6, 0.6–0.8, 0.8–1.0 (reference category), 1.0–1.2, and LTV above 1.2. The LTI categories are LTI below 1.0 (reference category), 1.0–2.0, 2.0–3.0, 3.0–4.0, 4.0–5.0, and LTI above 5.0. The age groups are under 25 (reference category), 25–30, 30–35, 35–40, 40–45, 45–50, 50–55, 55–60, 60–65, and over 65. The household types are single-person household (reference category), unmarried couple without children, married couple without children, unmarried couple with children, married couple with children, one parent household, and other household types.

[17] Estimating a frailty model, a model that incorporates unobserved heterogeneity, is not possible from a theoretical point of view; the frailty term cannot be identified in the stock sample we use (see Van den Berg & Drepper, 2016, for a thorough discussion). Besides, estimation would not have been feasible empirically.

[18] Following Stevens (1999), we have also run a regression with an artificial stock sampling date, that is, we excluded durations that started in 1995 and 1996 from the sample of nonleft-censored observations. The estimates of the standard (left-censored) sample and the artificially left-censored sample are virtually the same, suggesting no effect of a sample selection bias due to left-censoring.

6 | ESTIMATES

The estimation results of Equation (5) can be found in Table 4. The table presents the estimated hazard ratios of the semiparametric extended Cox model with COROP clustered standard errors. A ratio of one indicates that the effect is the same as the baseline hazard. Coefficients below one indicate a probability lower than the baseline, whereas coefficients above one indicate a higher probability.

The first column of Table 4 shows the results where the left-censored observations have been discarded.[19] The coefficient for the prospective loss variable is 0.788, indicating that when the prospective nominal loss increases 1 percent the probability of selling is only 78.8 percent of the situation where there is no such loss. A 1 percent increase in prospective loss thus decreases the probability of selling by 21.2 percent.[20]

Compared to households that have a mortgage between 80 and 100 percent of the house value (the reference category), those with a mortgage between 100 and 120 percent of the house value have a 5.5 percent lower probability of moving (the coefficient is 0.945). These moderately underwater households thus have a lower probability of moving than the group that has a slightly better financial position. Note, however, that the moderately underwater households have a higher probability of moving than the households with LTVs between 0 and 80 percent.[21] The coefficient for households with mortgages over 120 percent of the house value is 1.425, meaning that these heavily underwater households have a 42.5 percent higher probability of moving than the reference category ($0.8 < LTV \leq 1.0$). The heavily underwater households, therefore, have the highest mobility of all LTV categories.[22]

The results in column 1 also show that, overall, mobility decreases with age; people under 25 (the reference category) have by far the highest mobility. There are no significant differences between men and women, while divorced people have a substantially higher probability of selling/moving than nondivorced people. Furthermore, the coefficients of the LTI ratios show a U-pattern; households with a moderate LTI have the lowest mobility.

The second and third column of Table 4 show the estimates when the left-censored observations have been included. Column 2 shows the results for the matching approach, column 3 shows the results for the naive approach (see Section 5.2). A 1 percent increase in prospective loss is estimated to decrease mobility by 21.8 percent in the matching approach and 21.7 percent in the naive approach. Compared to the reference category being moderately underwater reduces mobility by 1.6 and 1.5 percent, respectively. However, these estimates are not significantly different from the reference category. Being heavily underwater increases mobility by 62.1 and 63.0 percent, respectively. The patterns and magnitudes of the estimated effects are similar for all three approaches, that is, the inclusion or exclusion of the left-censored observations does not drive our results.

In the estimates that are presented in the columns 4, 5, and 6 of Table 4, the moderately underwater households ($1.0 < LTV \leq 1.2$) and the heavily underwater households ($LTV > 1.2$) have been divided into three different groups based on wealth excluding net housing wealth (savings, etc.). The first group has negative wealth/savings, that is, the household has additional debt. The second group has positive wealth/savings but the total is smaller than the amount that the household is underwater, while the third group has positive wealth/savings that is larger than the amount that it is underwater.

[19] All results are robust to, for instance, the exclusion of cohort dummies (i.e., year of spell start dummies), the inclusion of year of observation dummies (that could capture exogenous market circumstances), and municipality fixed effects (instead of COROP fixed effects). Although the results are almost identical, we prefer not to present the results with both cohort and year of observation dummies because, strictly speaking, the spell start and the duration imply the year of observation. Estimation with duration in years instead of months does not alter the conclusions either.

[20] An additional regression confirms that dropping the prospective loss variable from the regression leads to smaller coefficients for the negative equity categories (see the discussion in Section 3.3). In other words, not including the measure of loss aversion in the regression model indeed leads to an overestimation (in absolute terms) of the effect of negative equity on mobility.

[21] The results indicate that for above-water households mobility is lowest for the lowest LTV groups. This corresponds to the findings of Henley (1998) and Coulson and Grieco (2013), who find that (positive) house equity decreases mobility. This result is consistent, for instance, with low LTV households taking larger steps on the property ladder, resulting in less moves over a lifetime.

[22] Lowering or increasing the upper limit of the highest LTV category (set at 1.5) leaves the conclusion unaltered. Heavily underwater households remain clearly the most mobile LTV category.

TABLE 4 Hazard ratios of households mobility: Regression results of Equation (5)

	(1) Left-Censored		(2) Matching		(3) Naive		(4) Left-Cens		(5) Matching		(6) Naive	
Prospective loss	0.788***	(0.015)	0.782***	(0.015)	0.783***	(0.015)	0.788***	(0.015)	0.783***	(0.015)	0.783***	(0.015)
LTV ≤ 0.2	0.387***	(0.017)	0.522***	(0.015)	0.520***	(0.015)	0.391***	(0.017)	0.527***	(0.015)	0.526***	(0.015)
0.2 < LTV ≤ 0.4	0.666***	(0.023)	0.690***	(0.017)	0.679***	(0.016)	0.671***	(0.023)	0.696***	(0.017)	0.685***	(0.016)
0.4 < LTV ≤ 0.6	0.845***	(0.027)	0.823***	(0.021)	0.817***	(0.020)	0.849***	(0.027)	0.827***	(0.021)	0.821***	(0.021)
0.6 < LTV ≤ 0.8	0.942**	(0.018)	0.921***	(0.016)	0.920***	(0.017)	0.944**	(0.018)	0.923***	(0.016)	0.922***	(0.017)
1.0 < LTV ≤ 1.2	0.945***	(0.016)	0.984	(0.017)	0.985	(0.017)						
LTV > 1.2	1.425***	(0.066)	1.621***	(0.072)	1.630***	(0.074)						
1.0 < LTI ≤ 2.0	0.603***	(0.013)	0.850***	(0.014)	0.852***	(0.015)	0.606***	(0.013)	0.851***	(0.014)	0.854***	(0.015)
2.0 < LTI ≤ 3.0	0.357***	(0.011)	0.570***	(0.015)	0.572***	(0.015)	0.361***	(0.011)	0.574***	(0.015)	0.575***	(0.015)
3.0 < LTI ≤ 4.0	0.277***	(0.013)	0.472***	(0.020)	0.473***	(0.020)	0.280***	(0.013)	0.476***	(0.020)	0.477***	(0.020)
4.0 < LTI ≤ 5.0	0.304***	(0.019)	0.545***	(0.032)	0.546***	(0.032)	0.308***	(0.019)	0.549***	(0.032)	0.551***	(0.032)
LTI > 5.0	0.532***	(0.033)	0.959	(0.054)	0.963	(0.054)	0.536***	(0.034)	0.963	(0.054)	0.967	(0.055)
Age 25–30	0.775***	(0.041)	0.814***	(0.045)	0.813***	(0.045)	0.775***	(0.041)	0.813***	(0.045)	0.813***	(0.044)
Age 30–35	0.657***	(0.034)	0.706***	(0.038)	0.706***	(0.038)	0.655***	(0.034)	0.704***	(0.038)	0.703***	(0.038)
Age 35–40	0.511***	(0.027)	0.557***	(0.031)	0.556***	(0.031)	0.508***	(0.027)	0.553***	(0.031)	0.553***	(0.031)
Age 40–45	0.361***	(0.020)	0.402***	(0.023)	0.399***	(0.023)	0.359***	(0.020)	0.399***	(0.023)	0.395***	(0.023)
Age 45–50	0.263***	(0.015)	0.317***	(0.018)	0.316***	(0.018)	0.261***	(0.015)	0.315***	(0.018)	0.314***	(0.018)
Age 50–55	0.217***	(0.012)	0.291***	(0.016)	0.291***	(0.016)	0.216***	(0.012)	0.289***	(0.016)	0.289***	(0.016)
Age 55–60	0.208***	(0.012)	0.284***	(0.016)	0.286***	(0.017)	0.206***	(0.012)	0.282***	(0.016)	0.284***	(0.016)
Age 60–65	0.233***	(0.014)	0.297***	(0.018)	0.301***	(0.018)	0.231***	(0.014)	0.293***	(0.018)	0.298***	(0.018)
Age >65	0.234***	(0.014)	0.286***	(0.017)	0.298***	(0.018)	0.232***	(0.014)	0.283***	(0.017)	0.294***	(0.017)
Male	0.960	(0.021)	0.979	(0.019)	0.984	(0.019)	0.960	(0.021)	0.979	(0.019)	0.985	(0.019)
Unmarried w/o children	0.738***	(0.012)	0.829***	(0.011)	0.825***	(0.011)	0.739***	(0.012)	0.829***	(0.011)	0.825***	(0.011)

(Continues)

TABLE 4 (Continued)

	(1) Left-Censored		(2) Matching		(3) Naive		(4) Left-Cens		(5) Matching		(6) Naive	
Married w/o children	0.674***	(0.014)	0.827***	(0.012)	0.822***	(0.012)	0.675***	(0.014)	0.827***	(0.012)	0.822***	(0.012)
Unmarried with children	0.695***	(0.021)	0.759***	(0.019)	0.755***	(0.018)	0.695***	(0.021)	0.759***	(0.018)	0.754***	(0.018)
Married with children	0.592***	(0.014)	0.673***	(0.012)	0.668***	(0.012)	0.593***	(0.014)	0.673***	(0.012)	0.668***	(0.012)
One parent household	1.055	(0.031)	0.996	(0.022)	0.996	(0.022)	1.055	(0.031)	0.996	(0.022)	0.996	(0.022)
Other household type	0.593***	(0.060)	0.673***	(0.064)	0.667***	(0.065)	0.595***	(0.060)	0.674***	(0.064)	0.669***	(0.065)
Divorced	5.223***	(0.157)	3.151***	(0.057)	3.168***	(0.058)	5.233***	(0.158)	3.158***	(0.057)	3.175***	(0.058)
1.0 < LTV ≤ 1.2 & W < 0							0.908*	(0.043)	0.918	(0.047)	0.919	(0.047)
1.0 < LTV ≤ 1.2 & 0 ≤ W < U							0.931***	(0.015)	0.967*	(0.016)	0.968*	(0.016)
1.0 < LTV ≤ 1.2 & W ≥ U							1.784***	(0.073)	1.977***	(0.061)	1.975***	(0.069)
LTV > 1.2 & W < 0							1.493***	(0.099)	1.578***	(0.104)	1.584***	(0.099)
LTV > 1.2 & 0 ≤ W < U							1.396***	(0.065)	1.589***	(0.072)	1.597***	(0.074)
LTV > 1.2 & W ≥ U							3.141***	(0.224)	3.259***	(0.187)	3.265***	(0.193)
COROP fixed effects	Yes		Yes		Yes		Yes		Yes		Yes	
Cohort fixed effects	Yes		Yes		Yes		Yes		Yes		Yes	
Number obs.	2,441,598		2,576,225		2,576,225		2,441,598		2,576,225		2,576,225	
Log-likelihood	−1043758		−1430352		−1444541		−1043631		−1430168		−1444359	

Note: Exponentiated coefficients (hazard ratios) with COROP clustered standard errors in parentheses. W indicates additional wealth, U indicates amount underwater. Reference categories: LTV between 0.8 and 1.0, LTI below 1.0, age below 25, and single person household. For details on the matching and naive approach, see Appendix C. *$P < 0.05$, **$P < 0.01$, ***$P < 0.001$.

The results in the columns 4, 5, and 6 confirm that the mobility of the moderately underwater households is lower than the mobility of the heavily underwater households. The estimates also show that the moderately underwater households with additional debt are the least mobile subgroup. These households are between 9.2 and 8.1 percent less mobile than the group with an LTV between 80 and 100 percent although only the first estimate is significant. Another important observation is that overall the coefficients for the subgroups increase with additional wealth, thereby showing that mobility of households with negative equity rises with additional (nonhousing) wealth. This holds for both the moderately and heavily underwater households. It seems that the high mobility of the heavily underwater households is not associated with involuntary mobility. After all, the (heavily) underwater households without household savings (i.e., with additional debt) are most likely to default on their mortgage payments and to be confronted with forced house sales. We observe exactly the opposite, suggesting that the high mobility of underwater households is not default-driven.[23]

7 | LOCAL LABOR MARKETS

Thus far we have incorporated local labor markets only by distinguishing between COROPs. In other words: local labor market have been taken into account through the inclusion of administrative labor market regions, which are treated as exogenous. We have, arguably, abstracted from interdependencies between the housing market and the labor market. In this section, we will look into the robustness of our results by extending the analysis through the inclusion of local labor market conditions.

We recognize that proxies for local labor market conditions are potentially, or even likely, endogenous.[24] Nevertheless, as suitable instruments for local labor market conditions are lacking we resort to a proxy-based approach. For robustness purposes, we use a wide range of proxies to account for local labor market conditions. The unavailability of regional vacancy numbers for the period under investigation, necessary to create a market thickness indicator such as the ratio of the number of unemployed workers (job searchers) and the number of vacant jobs (see, for example, Broersma & Van Ours, 1999; Petrongolo & Pissarides, 2001), is an additional reason to opt for a multitude of alternative proxies.

We use regional unemployment rate, regional labor market participation (gross and net), an estimate of the regional commuting distance,[25] and regionally aggregated changes in employment to proxy local labor market conditions. All these proxies for local labor market conditions are observed at the COROP level. Unemployment rate, labor market participation, and commuting distances are obtained from CBS StatLine (2013, 2014). The job dynamics proxies consist of regional aggregates, based on the microdata at our disposal, that describe administrative changes in employment status: no change in job or jobs (reference category), loss of a job, getting a (or an extra) job, and losing a job while getting another. Apart from the average commuting distance, which is expressed in kilometers, the proxy variables are expressed in percentage terms.

While none of the proxies is ideal Table 5 does suggest that our findings are not driven by local labor market conditions. After all, the various proxies do not alter any of our main conclusions. The coefficient of the nominal loss measure ranges from 0.783 to 0.793. The coefficients for moderately and heavily underwater households range from 0.937 to

[23] We have looked into potential endogeneity due to the business cycle affecting the value of other assets in a household's portfolio by distinguishing between households that do and do not own stocks. After all, the business cycle would affect the portfolios of households that do own stocks stronger than those that do not own stocks. We have estimated the same regressions for households that owned stocks at least one of the years under observation (41 percent of the observations of the sample), households that owned stocks all years under observation (21 percent), and households that never owned stocks (59 percent). The results are very similar for all groups. All in all, there is no indication that the relation between the business cycle and other assets does affect our results. The estimates are available upon request.

[24] See, for instance, Munch, Rosholm, and Svarer (2006) and Coulson and Fisher (2009) for endogeneity issues related to the housing market and labor market. For studies on the Netherlands, in particular, see Van Leuvensteijn and Koning (2004), Sanz-de Galdeano (2006), and Van Vuuren (2009).

[25] It is an estimate as the job location is approximated and not perfectly observed. We hypothesize that average commuting distance increases during economic downturns. A thorough analysis of the relation between commuting distance and housing lock is beyond the scope of the current study.

TABLE 5 Hazard ratios of households mobility with local labor market proxies (left-censored)

	(1) Unemployment		(2) Gross part.		(3) Net part.		(4) Commuting		(5) Job dynamics	
Prospective loss	0.793***	(0.014)	0.783***	(0.014)	0.786***	(0.015)	0.788***	(0.014)	0.783***	(0.016)
LTV ≤ 0.2	0.384***	(0.017)	0.388***	(0.017)	0.387***	(0.017)	0.386***	(0.017)	0.382***	(0.017)
0.2 < LTV ≤ 0.4	0.660***	(0.023)	0.667***	(0.023)	0.667***	(0.023)	0.666***	(0.023)	0.651***	(0.023)
0.4 < LTV ≤ 0.6	0.841***	(0.027)	0.846***	(0.027)	0.846***	(0.027)	0.845***	(0.027)	0.832***	(0.027)
0.6 < LTV ≤ 0.8	0.940**	(0.018)	0.942**	(0.018)	0.942**	(0.018)	0.942**	(0.018)	0.936***	(0.018)
1.0 < LTV ≤ 1.2	0.959*	(0.017)	0.937***	(0.016)	0.942***	(0.016)	0.945***	(0.016)	0.978	(0.016)
LTV > 1.2	1.459***	(0.070)	1.402***	(0.066)	1.417***	(0.067)	1.425***	(0.067)	1.481***	(0.070)
1.0 < LTI ≤ 2.0	0.601***	(0.013)	0.603***	(0.013)	0.603***	(0.013)	0.603***	(0.013)	0.599***	(0.013)
2.0 < LTI ≤ 3.0	0.356***	(0.011)	0.358***	(0.011)	0.358***	(0.011)	0.357***	(0.011)	0.353***	(0.011)
3.0 < LTI ≤4.0	0.275***	(0.013)	0.278***	(0.013)	0.277***	(0.013)	0.277***	(0.013)	0.273***	(0.013)
4.0 < LTI ≤ 5.0	0.301***	(0.019)	0.305***	(0.019)	0.305***	(0.019)	0.304***	(0.019)	0.299***	(0.019)
LTI > 5.0	0.528***	(0.033)	0.534***	(0.033)	0.532***	(0.033)	0.531***	(0.033)	0.525***	(0.033)
Age 25–30	0.771***	(0.041)	0.777***	(0.041)	0.776***	(0.041)	0.775***	(0.041)	0.783***	(0.042)
Age 30–35	0.654***	(0.034)	0.659***	(0.035)	0.657***	(0.035)	0.657***	(0.034)	0.671***	(0.035)
Age 35–40	0.509***	(0.027)	0.513***	(0.028)	0.511***	(0.028)	0.511***	(0.027)	0.523***	(0.028)
Age 40–45	0.360***	(0.020)	0.363***	(0.021)	0.361***	(0.020)	0.361***	(0.020)	0.372***	(0.021)
Age 45–50	0.262***	(0.015)	0.264***	(0.015)	0.263***	(0.015)	0.263***	(0.015)	0.271***	(0.015)
Age 50–55	0.217***	(0.012)	0.218***	(0.012)	0.218***	(0.012)	0.218***	(0.012)	0.224***	(0.012)
Age 55–60	0.208***	(0.012)	0.209***	(0.012)	0.208***	(0.012)	0.208***	(0.012)	0.215***	(0.013)
Age 60–65	0.233***	(0.014)	0.234***	(0.014)	0.233***	(0.014)	0.233***	(0.014)	0.242***	(0.015)
Age >65	0.234***	(0.014)	0.235***	(0.014)	0.234***	(0.014)	0.234***	(0.014)	0.244***	(0.015)
Male	0.959	(0.021)	0.961	(0.021)	0.960	(0.021)	0.960	(0.021)	0.958	(0.021)
Unmarried w/o children	0.736***	(0.012)	0.739***	(0.012)	0.738***	(0.012)	0.738***	(0.012)	0.733***	(0.012)

(Continues)

TABLE 5 (Continued)

	(1) Unemployment		(2) Gross part.		(3) Net part.		(4) Commuting		(5) Job dynamics	
Married w/o children	0.673***	(0.014)	0.674***	(0.014)	0.674***	(0.014)	0.674***	(0.014)	0.670***	(0.014)
Unmarried with children	0.693***	(0.021)	0.696***	(0.021)	0.695***	(0.021)	0.695***	(0.021)	0.693***	(0.021)
Married with children	0.591***	(0.013)	0.593***	(0.014)	0.592***	(0.014)	0.592***	(0.014)	0.590***	(0.014)
One parent household	1.055	(0.031)	1.056	(0.031)	1.055	(0.031)	1.055	(0.031)	1.059	(0.031)
Other household type	0.587***	(0.060)	0.594***	(0.060)	0.593***	(0.060)	0.593***	(0.060)	0.590***	(0.060)
Divorced	5.219***	(0.159)	5.228***	(0.158)	5.224***	(0.157)	5.223***	(0.157)	5.225***	(0.160)
Regional unemployment rate	0.917***	(0.014)								
Regional labor market part. (gross)			0.943***	(0.011)						
Regional labor market part. (net)					0.987	(0.008)				
Regional commuting distance							1.022	(0.035)		
No job (reg. aggregate)									1.086***	(0.013)
Job plus (reg. aggregate)									1.227***	(0.047)
Job minus (reg. aggregate)									0.925*	(0.031)
Job plus and minus (reg. aggregate)									1.254***	(0.020)
COROP fixed effects	Yes		Yes		Yes		Yes		Yes	
Cohort fixed effects	Yes		Yes		Yes		Yes		Yes	
Number obs.	2,436,244		2,441,598		2,441,598		2,441,598		2,441,598	
Log-likelihood	−1040817		−1043623		−1043748		−1043756		−1042156	

Note: Exponentiated coefficients (hazard ratios) with COROP clustered standard errors in parentheses. W indicates additional wealth, U indicates amount underwater. Reference categories: LTV between 0.8 and 1.0, LTI below 1.0, age below 25, single person household, and no changes in job(s) for job dynamics. Job dynamics variables are aggregated at the regional (COROP) level. Job plus indicates getting a (or an extra) job, job minus indicates losing a job, job plus and minus indicates losing a job while getting another. *$P < 0.05$, **$P<0.01$, ***$P<0.001$.

0.978 and from 1.402 to 1.481, respectively.[26] All in all, these results seem to imply that while labor market conditions undoubtedly matter, they do not drive our results.

8 | CONCLUSIONS

In this study, we make a clear distinction between loss aversion and equity constraints. Using administrative data from the Netherlands (2006–2011), the prospective loss indicator is used to identify nominal loss aversion, while LTV ratios larger than one indicate the existence of negative equity. The analysis has shown that a 1 percent increase in prospective loss decreases mobility in the owner-occupied housing market by more than 20 percent. Being moderately underwater (LTV between 1.0 and 1.2) reduces mobility by about 5 percent compared to the group that has a mortgage that is not larger than its house value (LTV between 0.8 and 1.0) although the difference is not significant in all of the estimations. Nevertheless, the mobility rate of the moderately underwater households remains higher than the households with lower LTVs. The analysis shows that heavily underwater households have the highest mobility: roughly 50 percent higher than those with an LTV between 0.8 and 1.0. The analysis also shows that additional wealth/savings increases mobility for underwater households. The effects are similar for moderately and heavily underwater households, the difference being that mobility is roughly 1.6 times higher for the heavily underwater households.

The conclusions are threefold. First, our results—consistent with the findings of Engelhardt (2003)—indicate the existence of nominal loss aversion as prospective losses decrease mobility substantially. Second, there is much less evidence for negative equity effects; moderately underwater households are slightly less mobile than households with mortgages between 80 and 100 percent of their house values, but moderately underwater households are more mobile than households with lower LTV ratios. This finding is similar to the findings of Schulhofer-Wohl (2012) and Coulson and Grieco (2013). Moderately underwater households might have encountered some negative effects—especially households with additional debt—but heavily underwater households have the highest mobility. Third, nonhousing wealth increases mobility for underwater households, suggesting that the high mobility for heavily underwater households is not default-driven. If the higher mobility for heavily underwater households was default-driven then we would have seen higher mobility rates for the households with negative wealth/savings. After all, households with positive wealth are likelier to be able to make their mortgage payments even if their house is underwater. The high mobility of heavily underwater households is an interesting phenomenon that needs attention in future research. Possibly heavily underwater households use their financial means to move instead of continuing mortgage payments on their underwater home.

This study has presented evidence that decreasing house prices have hampered household mobility through nominal loss aversion. There is less evidence that negative equity limits household mobility even though some particular groups with negative equity are indeed less mobile. All in all, it seems that households did not want to move in a market with decreasing prices, while they generally could have from a financial perspective.

ACKNOWLEDGMENTS

This research has been made possible through financial support of the Ministry of the Interior and Kingdom Relations (the Netherlands). We would also like to thank Marc Schramm, Janneke Plantenga, Jacques Siegers, Adriaan Kalwij, Mark Kattenberg, *JRS* editor Edward Coulson, and two anonymous referees for their valuable comments.

REFERENCES

Allison, P. D. (1984). *Event history analysis: Regression for longitudinal event data*. No. 46 in Quantitative Applications in the Social Sciences. Beverly Hills, CA: Sage Publications, Inc.

Andersson, F., & Mayock, T. (2014). How does home equity affect mobility?*Journal of Urban Economics*, *84*(Nov.), 23–39.

[26] Using lagged proxies does not alter any of the conclusions presented here.

Anenberg, E. (2011). Loss aversion, equity constraints and seller behavior in the real estate market. *Regional Science and Urban Economics*, *41*(1), 67–76.

Bailey, M. J., Muth, R. F., & Nourse, H. O. (1963). A regression method for real estate price index construction. *Journal of the American Statistical Association*, *58*(304), 933–942.

Broersma, L., & Van Ours, J. C. (1999). Job searchers, job matches and the elasticity of matching. *Labour Economics*, *6*(1), 77–93.

Cameron, A. C., & Trivedi, P. K. (2005). *Microeconometrics: Methods and applications*. New York, NY: Cambridge University Press.

CBS StatLine. (2013). *Beroepsbevolking; regio's 1996-2013* [*Labor force; regions 1996-2013*]. Data file. Centraal Bureau voor de Statistiek (Statistics Netherlands), The Hague. Retrieved July 30, 2017 from http://statline.cbs.nl/Statweb/

CBS StatLine (2014). *Banen werknemers en afstand woon-werk; woon- en werkregio's; 2006-2014* [*Jobs of employees and commuting distance; residence and working regions; 2006-2014*]. Data file. Centraal Bureau voor de Statistiek (Statistics Netherlands), The Hague. Retrieved July 30, 2017 from http://statline.cbs.nl/Statweb/

CBS StatLine (2017). *Bestaande koopwoningen; woningtype; verkoopprijzen prijsindex 2010 = 100* [*Existing houses; house type; sales price index 2010 = 100*]. Data file. Centraal Bureau voor de Statistiek (Statistics Netherlands), The Hague. Retrieved August 30, 2017 from http://statline.cbs.nl/Statweb/

Chan, S. (2001). Spatial lock-in: Do falling house prices constrain residential mobility?*Journal of Urban Economics*, *49*(3), 567–586.

Coulson, N. E., & Fisher, L. M. (2009). Housing tenure and labor market impacts: The search goes on. *Journal of Urban Economics*, *65*(3), 252–264.

Coulson, N. E., & Grieco, P. L. (2013). Mobility and mortgages: Evidence from the PSID. *Regional Science and Urban Economics*, *43*(1), 1–7.

Council for Real Estate Assessment (2010). *Notitie modelmatige waardebepaling* [*Memo model property valuation*]. November 2010, Waarderingskamer *[*Council for Real Estate Assessment*]*, The Hague.

Cox, D. R. (1972). Regression models and life tables (with discussion). *Journal of the Royal Statistical Society Series B (Methodological)*, *34*(2), 187–220.

Cox, D. R. (1975). Partial likelihood. *Biometrika*, *62*(2), 269–276.

De Jager, J. (2009). *Kamerbrief inzake de (on)mogelijkheid een met hypotheek bezwaard huis te verhuren* [*Parliamentary Letter regarding the (im)possibility of renting out a house with a mortgage*]. Letter of the Minister of Finance (20 Februari, 2009), No. AFP/2009/104 U.

De Vries, P., de Haan, J., van der Wal, E., & Mariën, G. (2009). A house price index based on the SPAR method. *Journal of Housing Economics*, *18*(3), 214–223.

Dröes, M. I., & Hassink, W. H. J. (2014). Credit constraints and price expectations of homeowners. *Housing Studies*, *22*(2), 92–99.

Dutch Central Bank & Netherlands Authority for the Financial Markets (2009). Risico's op de hypotheekmarkt voor huishoudens en hypotheekverstrekkers [Risks on the mortgage market for households and mortgage lenders]. Report, Amsterdam: DNB.

Eichholtz, P., & Lindenthal, T. (2013). *That's what we paid for it: The spell of the home purchase price through the centuries*. Unpublished manuscript, Maastricht University, Netherlands.

Engelhardt, G. V. (2003). Nominal loss aversion, housing equity constraints, and household mobility: Evidence from the United States. *Journal of Urban Economics*, *53*(1), 171–195.

Ferreira, F., Gyourko, J., & Tracy, J. (2010). Housing busts and household mobility. *Journal of Urban Economics*, *68*(1), 34–45.

Ferreira, F., Gyourko, J., & Tracy, J. (2012). Housing busts and household mobility: An update. *Economic Policy Review*, *18*(3), 1–15.

Genesove, D., & Mayer, C. (2001). Loss aversion and seller behavior: Evidence from the housing market. *Quarterly Journal of Economics*, *116*(4), 1233–1260.

Ghent, A. C., & Kudlyak, M. (2011). Recourse and residential mortgage default: Evidence from US states. *Review of Financial Studies*, *24*(9), 3139–3186.

Gottschalk, P., & Moffitt, R. A. (1994). Welfare dependence: Concepts, measures, and trends. *American Economic Review*, *84*(2), 38–42.

Guo, G. (1993). Event-history analysis for left-truncated data. *Sociological Methodology*, *23*, 217–243.

Haffner, M., Van der Veen, M., & Bounjouh, H. (2014). TENLAW: Tenancy law and housing policy in multi-level Europe. National Report for the Netherlands.

Henley, A. (1998). Residential mobility, housing equity and the labour market. *Economic Journal*, *108*(447), 414–427.

Huisman, C. J. (2016a). A silent shift? The precarisation of the Dutch rental housing market. *Journal of Housing and the Built Environment*, *31*(1), 93–106.

Huisman, C. J. (2016b). Temporary tenancies in the Netherlands: From pragmatic policy instrument to structural housing market reform. *International Journal of Housing Policy*, *16*(3), 409–422.

Iceland, J. (1997). The dynamics of poverty spells and issues of left-censoring. PSC Research Report Series No. 97-378 (January 1997), University of Michigan.

Jansen, S., de Vries, P., Coolen, H., Lamain, C., & Boelhouwer, P. (2008). Developing a house price index for the Netherlands: A practical application of weighted repeat sales. *Journal of Real Estate Finance and Economics*, *37*(2), 163–186.

Jenkins, S. P. (2005). *Survival analysis*. Unpublished manuscript, Institute for Social and Economic Research, University of Essex, Colchester, UK.

Kahneman, D., & Tversky, A. (1979). Prospect theory: An analysis of decision under risk. *Econometrica*, *47*(2), 263–291.

Lawrance, A. B., & Marks, R. E. (2008). Survival of firms and plants in a supply-constrained industry. Working paper, School of Economics, University of NSW, Sydney, Australia. Retrieved July 30, 3017 from http://www.agsm.edu.au/bobm/papers/lawrance_marks_wp0801.pdf

Moffitt, R. A., & Rendall, M. S. (1995). Cohort trends in the lifetime distribution of female family headship in the United States, 1968–1985. *Demography*, *32*(3), 407–424.

Munch, J. R., Rosholm, M., & Svarer, M. (2006). Are homeowners really more unemployed? *Economic Journal*, *116*(514), 991–1013.

Petrongolo, B., & Pissarides, C. A. (2001). Looking into the black box: A survey of the matching function. *Journal of Economic Literature*, *39*(2), 390–431.

Reifner, U., Clerc-Renaud, S., Pérez-Carrillo, E. F., Tiffe, A. & Knobloch, M. (2009). Study on equity release schemes in the EU, Part II: Country reports. Project no. MARKT/2007/23/H, Institut für Finanzdienstleistungen e.V.

Rouwendal, J. (2007). Mortgage interest deductibility and homeownership in the Netherlands. *Journal of Housing and the Built Environment*, *22*(4), 369–382.

Sanz-de Galdeano, A. (2006). Discussion: The effect of home-ownership on labour mobility in the Netherlands. In J. Messina, C. Michelacci, J. Turunen, & G. Zoega (Eds.), *Labour market adjustments in Europe* (pp. 207–210). Cheltenham, UK: Edward Elgar Publishing.

Scanlon, K., & Elsinga, M. (2014). Policy changes affecting housing and mortgage markets: How governments in the UK and the Netherlands responded to the GFC. *Journal of Housing and the Built Environment*, *29*(2), 335–360.

Schilder, F., & Conijn, J. (2012). Restschuld in Nederland: Omvang en consequenties [Residual debt in the Netherlands: Magnitude and consequences]. ASRE Research Paper 2012-01, Amsterdam School of Real Estate.

Schoonhoven, R., & Bottelberghs, I. (2014). *Baangegevens in microdata* [*Job information in microdata*]. Unpublished manuscript, Statistics Netherlands, The Netherlands.

Schulhofer-Wohl, S. (2012). Negative equity does not reduce homeowners' mobility. *Quarterly Review*, *35*(1), 2–14.

Steegmans, J., & Hassink, W. (2017). Financial position and house price determination: An empirical study of income and wealth effects. *Journal of Housing Economics*, *36*, 8–24.

Stein, J. C. (1995). Prices and trading volume in the housing market: A model with down-payment effects. *Quarterly Journal of Economics*, *110*(2), 379–406.

Stevens, A. H. (1999). Climbing out of poverty, falling back in. *Journal of Human Resources*, *34*(3), 557–588.

Struyven, D. (2015). Housing lock: Dutch evidence on the impact of negative home equity on household mobility. Job market paper, Massachusetts Institute of Technology.

Taskforce Verzilveren. (2013). Eigen haard is zilver waard. Report offered to the Ministry of Housing, Taskforce Verzilveren *[*Taskforce Capitalizing Housing Assets*]*.

Tversky, A., & Kahneman, D. (1991). Loss aversion in riskless choice: A reference-dependent model. *Quarterly Journal of Economics*, *106*(4), 1039–1061.

Van den Berg, G. J., & Drepper, B. (2016). Inference for shared-frailty survival models with left-truncated data. *Econometric Reviews*, *35*(6), 1075–1098.

Van der Laan, E. (2009). *Kamerbrief inzake de verhuur van met hypotheek bezwaarde woningen* [*Parliamentary Letter regarding the renting out of houses with a mortgage*]. Letter of the Minister of Housing (30 January, 2009), No. ABC200900 7088.

Van Leuvensteijn, M., & Koning, P. (2004). The effect of home-ownership on labor mobility in the Netherlands. *Journal of Urban Economics*, *55*(3), 580–596.

Van Veldhuizen, S., Vogt, B., & Voogt, B. (2016). Negative home equity and household mobility: Evidence from administrative data. CPB Discussion Paper No. 323, CPB Netherlands Bureau for Economic Policy Analysis.

Van Vuuren, A. (2009). The impact of homeownership on unemployment in the Netherlands. In C. van Ewijk & M. Van Leuvensteijn (Eds.), *Homeownership and the labour market in Europe* (pp. 113–135). Oxford: Oxford University Press.

Vereniging Eigen Huis. (2017). *Tijdelijke verhuur gebaseerd op Leegstandwet* [*Temporary rent outs based on the Law of Vacancies*]. Webpage, Dutch association of owner-occupiers, Retrieved July 30, 2017 from http://www.eigenhuis.nl/besparen/tijdelijke-verhuur/tijdelijke-verhuur-gebaseerd-op-leegstandwet

Wang, F. T., & Zorn, P. M. (1997). Estimating house price growth with repeat sales data: What's the aim of the game? *Journal of Housing Economics*, *6*(2), 93–118.

How to cite this article: Steegmans J, Hassink W. Decreasing house prices and household mobility: An empirical study on loss aversion and negative equity. *J Regional Sci*. 2018;58:611–634. https://doi.org/10.1111/jors.12380

APPENDIX A: REPEAT SALES PRICE INDEX

The repeat sales index makes use of repeated sales of houses or pairs of transactions as Bailey, Muth, and Nourse (1963) call them in their seminal paper. Under the assumption that house quality is constant, house price changes over time can be estimated without house characteristics being observed (e.g., Wang & Zorn, 1997). The starting point for the repeat sales index is a standard hedonic pricing model with a time indicator for the moment of sale.

$$ln(P_{it}) = \beta_0 + \sum_{k=1}^{K} \beta_k z_{ik} + \sum_{t=2}^{T} \gamma_t D_{it} + \mu_{it}, \tag{A1}$$

where P_{it} is the price of property i at time t, z_{ik} is the kth house characteristic, D_{it} is the sale time indicator, and μ_{it} is a random error term.

The price change for a house that is sold twice is easily found by subtracting the price at time t_1 from the price at time t_2 (where $0 \leq t_1 < t_2 \leq T$). It follows that the difference in price between sale and resale is given by

$$\begin{aligned} \ln(P_{it_2}) - \ln(P_{it_1}) &= \ln\left(\frac{P_{it_2}}{P_{it_1}}\right) \\ &= \sum_{t=2}^{T} \gamma_{t_2} D_{it_2} - \sum_{t=2}^{T} \gamma_{t_1} D_{it_1} + (\mu_{it_2} - \mu_{it_1}) \\ &= \sum_{t=2}^{T} \delta_t D^{\star}_{it} + \epsilon_{it}, \end{aligned} \tag{A2}$$

where $D^{\star}_{it}$ is a time indicator that is equal to one in the period of the resale, minus one in the period of the (original) sale, and zero otherwise. The random error term is given by ϵ_{it}. The repeat sales index I_t is found by exponentiating the Ordinary Least Squares regression results of Equation (A2). By multiplying the coefficients with 100 we set the base for I_0 at 100.

$$I_t = 100\, exp\,(\hat{\delta_t}). \tag{A3}$$

We have estimated a separate price index, I_{ct}, per COROP region. Thus, we have estimated a total of 40 regional repeat sales price indices.

APPENDIX B: SUMMARY STATISTICS

TABLE B1 Household summary statistics

	Nonleft-Cens.	Left-Cens. Obs.	Total
Age	41.3	55.2	42.9
	(10.5)	(12.7)	(11.7)
Male	0.916	0.871	0.911
	(0.278)	(0.335)	(0.285)
Single person household	0.129	0.178	0.135
	(0.335)	(0.383)	(0.341)
Unmarried couple w/o children	0.136	0.035	0.124
	(0.342)	(0.183)	(0.330)
Married couple w/o children	0.157	0.325	0.177
	(0.364)	(0.469)	(0.381)
Unmarried couple with children	0.095	0.028	0.087
	(0.293)	(0.164)	(0.282)
Married couple with children	0.446	0.387	0.439
	(0.497)	(0.487)	(0.496)
One parent household	0.036	0.046	0.038
	(0.187)	(0.210)	(0.190)
Other household types	0.001	0.001	0.001
	(0.035)	(0.033)	(0.035)
Divorced	0.009	0.018	0.010
	(0.096)	(0.134)	(0.101)
Loan-to-income	3.1	1.6	2.9
	(40.1)	(409)	(37.7)
Observations	396,415	51,497	447,912

Note: Statistics of stock-sampled row houses in 2006. Standard deviations are shown under the means. Age and loan-to-income have been divided into different groups in the analysis.

TABLE B2 Summary statistics per cohort

	Age	LTV	LTV > 1	LTI	Pros. loss	Obs.
pre-1995	55.2	0.382	0.047	1.621	0.000	51,497
1995	47.4	0.525	0.047	1.883	0.000	23,279
1996	46.7	0.571	0.060	2.070	0.000	27,110
1997	45.3	0.612	0.070	2.337	0.000	30,475
1998	44.4	0.657	0.085	2.542	0.000	34,975
1999	43.2	0.717	0.115	2.698	0.000	37,812
2000	41.4	0.775	0.170	3.147	0.000	36,069

(Continues)

TABLE B2 (Continued)

	Age	LTV	LTV > 1	LTI	Pros. loss	Obs.
2001	40.3	0.840	0.241	3.165	0.000	40,844
2002	39.1	0.892	0.331	3.472	0.000	44,302
2003	38.3	0.931	0.458	4.202	0.000	44,474
2004	37.6	0.935	0.495	3.555	0.000	43,115
2005	36.7	0.950	0.553	3.722	0.000	33,960
2006	43.8	0.696	0.202	2.904	0.000	27,929
2007	42.6	0.712	0.209	3.228	0.001	27,207
2008	41.8	0.721	0.216	2.976	0.007	24,575
2009	40.4	0.804	0.357	3.453	0.366	18,244
2010	40.3	0.805	0.395	3.351	0.457	12,944
2011	43.3	0.718	0.283	2.670	0.164	9,238

Note: Summary statistics at the stock sampling date for cohorts before 2006 and at the year of inflow for cohorts thereafter.

APPENDIX C: LEFT-CENSORING

Left-censoring is most commonly handled by discarding the left-censored data altogether. Although Allison (1984, p. 57) calls this the "safest approach"—claiming that "it should not lead to any biases"—the contemporary view is that discarding the left-censored observations could cause serious selection bias (Gottschalk & Moffitt, 1994; Iceland, 1997; Moffitt & Rendall, 1995; Stevens, 1999). Apart from simply discarding the left-censored data one could also refine the research question to exclude the left-censored observations (Iceland, 1997). In our analysis that would have meant restricting the research question to exclude the longest durations from our analysis.

The simplest way to include the left-censored observations is to substitute the left-censoring moment as the beginning of the spell (Guo, 1993). An empirical application of this approach can be found in Lawrance and Marks (2008). However, this approach is only optimal if the hazard rate is constant, which is generally not the case (Allison, 1984; Guo, 1993; Iceland, 1997). For obvious reasons, we will call this the naive approach. A more elaborate approach is "integrating out" over all possible durations (see Gottschalk & Moffitt, 1994; Moffitt & Rendall, 1995). This approach, however, is not feasible with time-varying covariates as is the case in our analysis (Gottschalk & Moffitt, 1994; Stevens, 1999). The remaining approaches estimate the durations of the left-censored spells through additional assumptions on the distribution of the durations (e.g., Guo, 1993).

Our preferred way of handling the left-censored data makes optimal use of a not yet exploited feature of the left-censored observations in our data set. That is, for a part of our left-censored observations we observe the date that the house has been added to the housing stock. While the transaction records of the Cadastre records do not go back further than 1995, the Housing Stock Register goes back until 1992, providing likely starting dates for houses that have been added to the housing stock between 1992 and 1994. While the spell start is not observed directly, it is not likely that these "left-censored homes" have been sold twice in a very short period. The date (in the period 1992–1994) that the newly build house has been added to the housing stock can serve as a proxy for the beginning of the housing spell.

Furthermore, these observed "left-censored" durations can be matched with the remaining left-censored observations. Given the strong correlation between the age of the owner and the duration of the left-censored observations, age is used to match the proxied observations with the left-censored observations lacking this proxy. Even though the majority of the left-censored observations is likely to have started between 1992 and 1994 (see Table 1), the estimated left-censored durations will be an underestimation of the actual durations as no matched spells start before 1992. The main advantage of this approach, however, is that we do not need any further distributional assumptions while optimally using the available information.