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No pension and no house?

The effect of LTV limits on the housing wealth accumulation of the self-employed

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Abstract

Investing in housing could be an attractive alternative to privately saving for a pension, especially for those who are not obliged to save for an occupational pension (the self-employed for instance). But access to the housing market requires a down payment. Macroprudential measures, such as loan-to-value (LTV) norms could hamper access to the housing market for young buyers and require additional saving for this purpose. We study the effect of introducing and sharpening the LTV limit in the Netherlands on the probability of the self-employed and wage employed becoming homeowners. We construct a treatment and control group using parental wealth as a proxy for being liquidity constrained. We show that, during the period in which the LTV limit was introduced, the self-employed were 47% less likely to purchase their first home, relative to the wage employed and relative to periods without LTV being limited. However, we show that this was not caused by reducing the LTV limit, but by concurrent cofounding factors. Sharpening the LTV limit has not reduced the probability of the self-employed becoming homeowners. We also show some evidence suggesting that their status put self-employed workers at a disadvantage when the policy was enacted, possibly leading to dynamic selection out of self-employment.

Samenvatting

Geen pensioen en geen huis?
Het effect van LTV-limieten op de vermogensopbouw van zelfstandigen

Het kopen van een huis kan een aantrekkelijk alternatief zijn voor mensen die willen sparen voor hun pensioen, zeker voor degenen zonder verplichte deelname aan de tweede pijler, bijvoorbeeld zelfstandigen. Maar toegang tot de woningmarkt vereist nu een aanbetaling met eigen geld. Macroprudentiële maatregelen, zoals de LTV-normen (loan-to-value caps) kunnen de toegang tot de woningmarkt voor jonge kopers belemmeren door de vereiste extra besparingen. We bestuderen het effect van de invoering en aanscherping van de LTV-limiet in Nederland op de kans dat zelfstandigen huiseigenaars worden. We construeren een controlegroep waarbij potentiële kopers met rijkere ouders de nieuwe krediet restricties als minder/niet bindend ervaren, en een treatment groep die geen financiële steun van de familie kan verwachten en is daardoor zeker blootgesteld aan de scherpere leennormen. We laten zien dat zelfstandigen in de periode waarin de LTV-limiet werd ingevoerd, 47% minder vaak hun eerste woning zijn gaan kopen, vergelijken met werknemers en ten opzichte van perioden zonder LTV-limiet. We laten echter zien dat dit niet werd veroorzaakt door het verlagen van de LTV-limiet, maar door gelijktijdige macro effecten. Het aanscherpen van de LTV-limiet heeft de kans dat zelfstandigen eigenaarsbewoners worden niet verkleind. We laten ook bewijs zien dat suggereert dat zelfstandigen benadeelde worden toen (niet vanwege) het beleid werd uitgerold, wat mogelijk een dynamische selectie teweegbracht waarbij zelfstandigen er voor kozen om werknemers te worden vlak voor het kopen van hun huis.

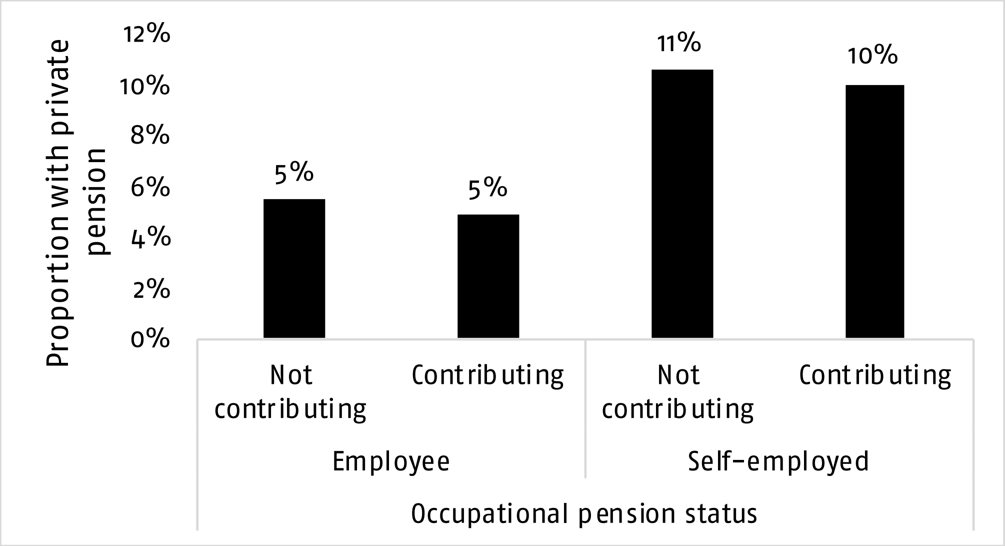
1. Introduction

Self-employed workers in the Netherlands are typically not obliged to save for their pension in addition to the social security benefit (AOW). Only a small group, about 10% (Li et al 2016), are affiliated with a pension fund because of their occupation. On the contrary, about 85-90% of the wage employed must save within a second-pillar/ occupational pension arrangement, while the rest does not.

This does not necessarily mean that the large group of self-employed people who do not contribute to occupational pensions are not preparing at all for retirement. The self-employed might be saving for their retirement by investing in housing, the most important private asset for Dutch households, rather than investing in private pensions.

Looking at a house as a possible source of post-retirement buffer is important because those who are not saving in the second pillar are also not saving using private pensions (which are equally fiscally facilitated). Figure 1 shows the within-group share by occupation of those saving in a private pension fund (i.e. the third pillar of the Dutch pension system, in addition to old-age and occupational pensions). This shows that 10%-11% of the self-employed contribute to a private pension; this is more than for the wage employed, as only 5% of them contributed to a private pension in 2019. This means that the largest majority of all employees did not explicitly save for retirement in 2019 using a third pillar product. Also, their occupational pension status does not seem to matter.

Figure 1: Within-group share of private pension contributors in 2019



Explanatory note: Source, CBS microdata, own computations.

Entering the owner-occupied market is typically only an option if one qualifies according to current borrowing constraints, such as loan-to-income (LTI) or loan-to-value (LTV) caps. Caloia et al (2021) show that increasing LTI limits did not cause self-employed mortgage owners to borrow more relative to the wage employed, except for those who have a lower income (as they might be more credit constrained). This generally positive outlook for the self-employed mortgage owner, at the intensive margin of debt, could depend on a selection occurring at mortgage application. The self-employed are more often denied credit, receive it later, or find themselves less likely to qualify for it. We add to their study by explicitly studying this extensive margin, so we look at home purchase decisions with specific attention to the self-employed.

Access to the housing market requires a down payment, which is at least the difference between the amount that can be borrowed, and the price of the house augmented with several markups (administrative fees and moving costs for instance). Because of sharper LTV limits in recent years, the self-employed might have become less likely to obtain credit or to save enough for a down payment. This could be the case for instance when young self-employed workers need resources to finance their business. Has the reduction in the LTV limit in the Netherlands affected the self-employed more, and are they therefore more likely to end up without a pension and without their own home?

Saving in general, thus also for a down payment, is not an easy task. Recent Netspar research shows that those who are not obliged to save lag behind in terms of personal savings in the Netherlands. This is evident when one compares wealth accumulation to post-retirement basic needs (Knoef et al, 2017). Li et al (2016) review the literature on the displacement effect (how much you save privately of one euro that you are not obliged to save) and find that the self-employed do not fully compensate for the lack of compulsory savings. They estimate that for each euro not invested in occupational pensions, 60 cents are saved privately, while for the wage employed they estimate about 30 cents.

The introduction of a loan-to-value (LTV) limit in the Netherlands resulted in an effective down payment constraint. Regulations imposed a 106% LTV limit in 2013, which was to be progressively reduced to 100% from 2018 on. Whereas a borrowing limit of 100% of the collateral value still constitutes a world record, the constraint is likely to be binding for first-time home buyers, as transaction/search costs, notary/broker fees and taxes amount to approx. 6% of the property value on average (Biesenbeek et al, 2021). These expenses, along with possible home renovation and moving costs, must be paid by the borrowers upfront out of pocket or financed using non-mortgage credit (if macroprudential limits allow).

We study the effect of the introduction of the LTV limit in the Netherlands on the probability of first-time buyers becoming homeowners using a duration model. Our research design is underpinned by a theoretical model based on Brueckner (1986). This model shows that a reduction in the LTV limit results in postponing or deciding against homeownership, but only for liquidity constrained individuals. We use this model to construct a treatment and control group with parents' financial wealth as a proxy for being liquidity constrained. We also disentangle the effects of the LTV limit on the timing of the transition to first-time homeownership from other market developments. The constrained group consists of individuals whose parents have less than a given amount of financial wealth (€50,000 in our baseline specification, a threshold later to be checked for sensitivity). The control group has wealthy parents and could potentially overcome liquidity constraints by receiving transfers or borrowing from parents to make a required down payment. The intuition behind this is that those who were not planning to use the entire LTV space allowed after the reduction in borrowing capacity will be unaffected by the changes in the LTV limit. Notice that the individual level of (pension) wealth is potentially endogenous and would not qualify as an appropriate indicator to define a treatment and control group.

We study the self-employed and wage employed separately in order to gauge the effect of these borrowing limits for those who are not included in the occupational pension system on access to housing as an additional possible form of pension savings. It is difficult to isolate the effect of LTV limits though, as the probability and timing of the transition to homeownership are affected by liquidity constraints and housing market conditions. For example, rising house prices impact the probability of becoming a homeowner, as shown by Boehm (2011) and Boehm (2014). Housing is not only a consumption decision but also an investment decision, an alternative to more traditional pension investments, as shown by Plaut (1987), especially if we think about the pension of the self-employed, who can make their own pension choices.

We use the same exogenous policy changes on a treatment and control group as in Biesenbeek et al. (2021). We show that the effect of the introduction/sharpening of the LTV limit on the transition into home ownership is small in general. When we differentiate between the self-employed and the wage employed, we find that the self-employed are less likely to have to purchase in each period when LTV caps were reduced. We show that during the period in which the LTV limit was introduced, the self-employed were about 40% less likely to purchase their first home, relative to the wage employed and relative to periods when the LTV was not being limited. However, we show that this was not caused by reducing the LTV limit, but by concurrent business-cycle-related factors. Sharpening the LTV limit has not reduced the probability of the self-employed becoming homeowners. We also present some evidence suggesting that their status put them at a disadvantage when the policy was enacted, possibly leading to dynamic selection out of self-employment.

Our results contribute to the policy debate in the Netherlands on two grounds. First, directly linking the results of this study, we discuss the possibility that macro-prudential policy might reduce access to the housing market for specific vulnerable groups. The self-employed tend to be redlined more often when applying for a mortgage because of their volatile income. For the same reason, they might also tend to wait longer before purchasing a home if the overall economic outlook worsens, for instance, because of a financial crisis. We find that when house prices had dropped the most and new measures (such as the new LTV limit) were introduced, the self-employed delayed purchasing their first home. However, this did not happen because of the LTV limit, as also less/no credit constrained self-employed workers experienced a similar delay.

Second, reasoning more in general, the current pension reform in the Netherlands is designed to attract self-employed workers to participate in occupational pension funds. Two main strategies are being used. The first is keeping in the system those who become self-employed after a career as employees (voluntary continuation or *vrijwillige continuereing* in Dutch). This option has hardly been used so far[1]. The second idea revolves around setting out experiments to attract new self-employed workers, mostly younger workers who had no past affiliation to a pension fund. If the self-employed though, when young, have a stronger preference or need to save for a house, they might postpone participation in occupational pensions, and after that, because of inertia, never participate at all. So, the planned experiments, which have been postponed because of a lack of political will and fear of a delay in the introduction of the new pension contract, might also end up with the self-employed showing limited interest in pension savings. This would not necessarily mean that people are not interested in saving for retirement per se, but that other saving motives are more relevant/pressing at a young age. This makes it interesting to look at the difference in home-purchasing behavior between the wage employed and the self-employed at young ages, as this could signal the importance of an alternative saving motive and help us understand the future results of the experiments currently being set up.

The study is organized as follows. In Section 2, we describe our data. In Section 3, we present our model and estimation results. Section 4 contains our conclusions.

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[1] *https://zoek.officielebekendmakingen.nl/blg-777815.pdf*

2. Data and descriptive evidence

We use a similar, highly granular, data set as that used by Biesenbeek et al. (2021) which contains the exact date of moving to a different address at the individual level. This dataset consists of more than 3.6 million individuals and their partners and parents, who are either wage employed or self-employed. We exploit mortgage data at the loan-level, which allows us to estimate the required down payment for every individual in the data.

CBS data

Our unit of analysis is the individual housing spell, that is the period elapsing between two subsequent moves to a different address, from CBS. By this, we mean the period elapsing between two subsequent moves to a different address. Every spell includes an anonymized person ID, an address ID and the start and end date of the individual living at this address. Our dataset contains every inhabitant in the Netherlands since 1995 (23 million inhabitants). There are on average three different address spells available for every individual in the dataset, or 69.5 million spells in total (see Table 1). Our sample is a subset of this large dataset. First, we are interested in individuals who are first-time home buyers and who could potentially be working and saving for a pension, so we restrict the sample to individuals aged 18 to 40 (15.6 million individuals). Moreover, we take only spells starting after 1984 into consideration (most of the required covariates are available from 2006 on; individuals who turn 18 in 1984 turn 40 in 2006). We treat homeownership as an absorbing state: individuals are removed from the sample after their first transition to homeownership. We leave all individuals who are homeowners at the first observation out of the sample as well because we cannot observe the transition to homeownership. Finally, we drop all individuals with one or more spells with unknown housing types, because we might not observe the transition to homeownership. There are 8.1 million individuals left (see Table 1), further reduced to about 3.6 million when selecting for parental wealth and some background characteristics, including employment status.

We obtain additional information about individual characteristics by merging several other data files from CBS, including gender, age, migration background, parents' ID, highest level and field of education achieved, annual income, wealth, gifts, and inheritances received, main social-economic category and being part of a couple or not. We observe most of the characteristics annually so that we can allow for time-varying control variables (exceptions are gender, date of birth and ethnicity). The dataset contains a time-varying dummy variable on the presence/sharpening of

Table 1: Sample size after selections (millions)

	Individuals	Spells
Total sample	23	69.5
Within the age group 18-40	15.6	41.7
Only spells after 1984	12.7	37.2
Dropped after transition	12.7	26.7
Without homeowners after first observation	9.3	23.3
Without unknown residence type	8.1	19.1
Parental wealth available	4.6	8.3
Control variables available	3.9	7.3
Keep only self/wage employed	3.76	

Explanatory note: This selection table shows how many individuals are left after each data preparation step. Source: CBS microdata, own computations.

an LTV limit. An LTV limit was introduced in 2012 in the Netherlands. The dummy in our dataset equals 0 for every individual until the end of 2011, and 1 thereafter. An alternative specification, with the level of the LTV limit rather than a dummy, is left for the robustness test.

In our baseline model, we assume that the introduction of an LTV limit has a larger effect on the probability of becoming a homeowner than the slight annual reduction of the LTV limit. As the LTV limit was introduced to all potential buyers simultaneously, the identification of a control and treatment group that allows empirical estimation of the effect of the LTV limit introduction discussed above is not obvious. However, like all credit constraints, these are only relevant for those who are credit restricted. If someone was able to make a substantial down payment, the introduction of the LTV limit should have, other things being equal, no effect. Although data on personal wealth and pension wealth is available, it would not qualify to identify the lack of restrictions, as it is endogenous to the purchase decision. Instead, we define the treatment and control groups using parental financial wealth.

Financial support from parents or others can help constrained individuals to overcome liquidity constraints. Constrained individuals can buy a smaller house or need to save longer than is consistent with their permanent income. Financial support from parents overcomes this distortion in inter-temporal consumption (Cox, 1972; Engelhardt and Mayer, 1998; Guiso and Jappelli, 2002). We also perform sensitivity analysis by using other indicators for a lack of constraint, namely other types of parents' wealth and inheritances and gifts received.

Parental financial wealth is calculated in two steps. First, we merge the (anonymized) parents' ID by a parent-child table. The mother's ID is available for 82.1% of our population and the father's ID for 77.8%. Second, we find the annual wealth levels of those mothers and fathers. Wealth is registered at the household level. If the parents do not live in the same household, total parents' wealth is calculated as the sum of the mother's and father's household wealth. Parents' wealth is available for 56.4% of the sample. We assume individuals are unconstrained if their parents have at least €50,000 of financial wealth. Financial wealth is defined as the total value of financial assets, including bank balances, savings, and securities.

As we are interested in transitions to the first owner-occupied house, we need to identify those who are tenants, owner-occupants or living at their parents' house. We retrieve residential type from the registered property type of the address. This is available for almost all properties from 2006 on. If not, we use the first available property type of the address. If an individual is registered at the same address as one or both parents, we set property type as living with parents rather than rental or owned.

We enrich the empirical analysis using a set of background variables. Age, gender, and migration background are available for all individuals in the sample through the Municipal Records Database (Gemeentelijke Basisadministratie, GBA). The highest level of education attained – and the corresponding field of education – are derived at a reference date from registers and the Labor Force Survey and grouped into ISCED and ISCO classifications. Attained education is available for 85.5% of the sample. Income is registered on the individual level and derived at a reference date from multiple sources, including the Dutch Tax Authority. The income variable in our database refers to gross income from labor, business and social benefits and it is normalized. The socio-economic category, which is provided by CBS, is based on the largest income category. Table 2 contains a summary of the key variables for the treatment and control groups. Most characteristics do not significantly differ across groups, yet it will be relevant to control for these variables in the empirical analysis. The table shows that some specific groups (the less educated for instance) are underrepresented in the control group. This may be because these groups are identified by higher parental wealth, and one could speculate that richer parents also transfer higher human capital to their children.

DNB loan-level data

We can merge the DNB loan-level data with the CBS data described above. In that dataset, we also have an indicator for people who are self-employed or wage employed at the time of mortgage inception. For banks, this is an important distinction, as financial institutions do not favor loans with high LTVs, in particular to customers who are seen as being riskier. Among those, the strongly increasing group of self-employed workers is a candidate for being redlined, as they face high

Table 2: Descriptive statistics on the treatment (with limited parental financial wealth) and control group in 2019.

	Treatment	Control
Residential type		
Rental	31%	30%
With parents	69%	70%
Gender		
Male	52%	54%
Female	48%	46%
Level of education		
Low	37%	20%
Medium	52%	59%
Bachelors	8.4%	15%
Masters	2.5%	6.4%
Migration background		
Dutch	73%	90%
Western	7.3%	5.5%
Non-western	20%	4.2%

	Treatment	Control
Socio-economic category		
Employee	74%	75%
Self-employed	20%	22%
Other	5.8%	2.4%
Median values		
Age	23.3	23
Income	13.5	14.8
Household income	17.7	18.9
Household wealth	12.7	141.1
Parents' household wealth	15	212.8
Received gifts	0.1	0.8
Received inheritances	0.1	0.4

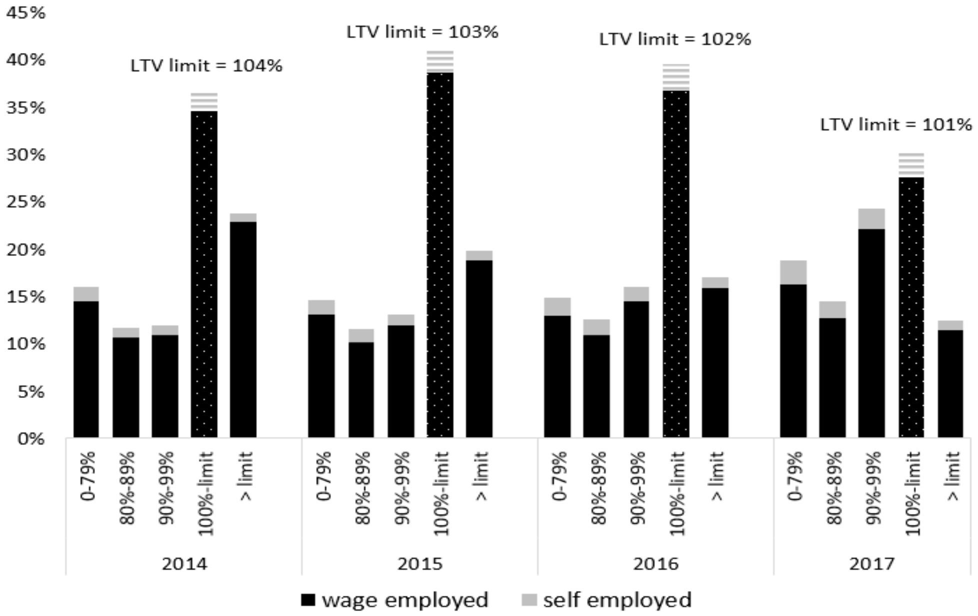
Explanatory note: Source: CBS microdata, own computations.

income volatility. For the same reasons those switching occupation often might also be redlined. What's more, their post-retirement income drop could be larger relative to their lower participation in the second pillar (Mastrogiacomo, 2016b) and make re-mortgaging potentially more unlikely.

Figures 2 and 3 show the distribution of the LTV of starters buying a house in the period 2014-2017. We slice the data in bins based on the distribution of LTV ratios. On the left, the most chosen LTV category is the one between 100% and the applicable LTV cap. On the right, we focus on each LTV bin separately and show two interesting trends.

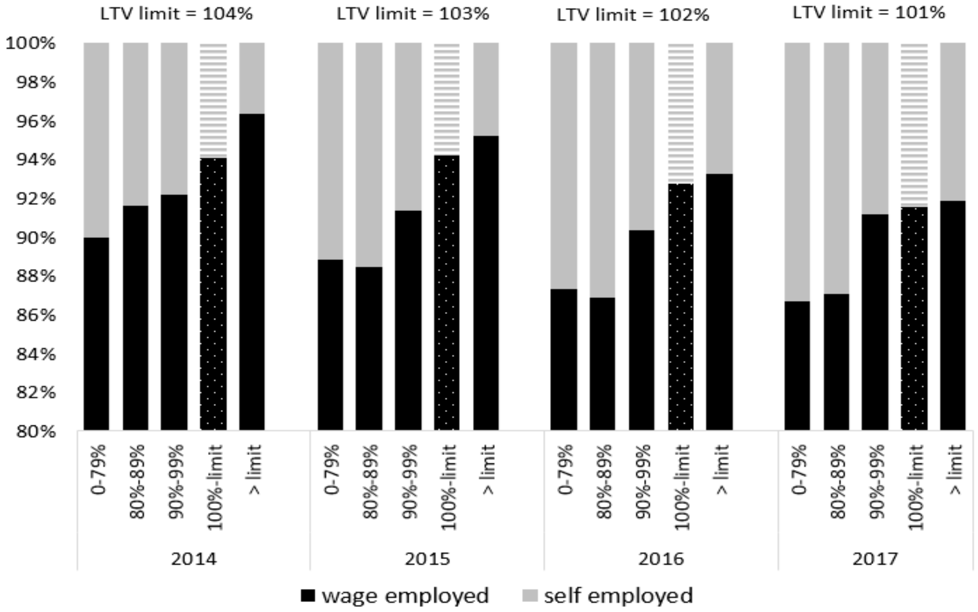
First, the proportion of the self-employed among starters has increased over time at all levels of the LTV ratio. This could be due to the increase in the number of self-employed in general. But it could also result from a rebalancing effect whereby self-employed people caught up as the market recovered. They were possibly less likely to buy a house in a period of falling house prices because of demand (e.g.: they do not have enough money) and supply side factors (e.g., banks find them too risky as new customers). Second, the increase in the share of self-employed is larger for lower LTV bins. While 10% of those obtaining a loan with an LTV ratio below 70% in 2014 were self-employed, this percentage had increased to 15% in 2017, and the same is true for all LTVs below the cap.

Figure 2: Distribution of LTV of starters buying a house, by type of employment. In 2017 the self-employed are more often present among low-LTV borrowers.



Explanatory note: LTV = loan-to-value ratio. Source: DNB loan level-data, own computations.

Figure 3: Distribution of LTV of starters buying a house, by type of employment, by LTV bin. In 2017 the self-employed are more often present among low-LTV borrowers.

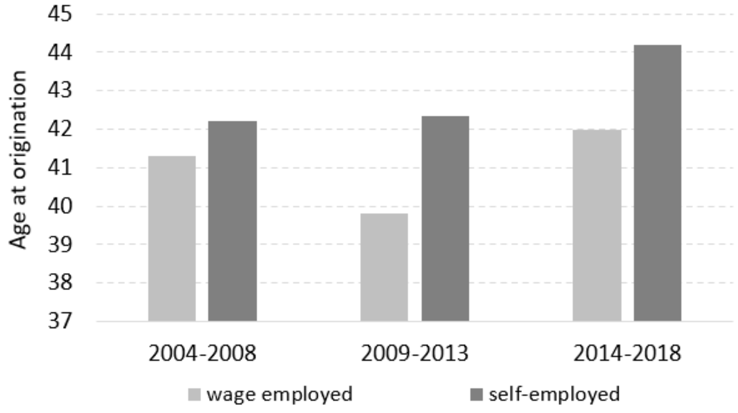


Explanatory note: LTV = loan-to-value ratio. Source: DNB loan level-data, own computations

This seems to be a prima-facie indication that the self-employed are among the safest customers that a bank could wish for, but it could also result from stricter screening that the self-employed face when they apply for a loan, as we mentioned above. The strictness of screening processes is an important determinant of access to credit (Mian and Sufi, 2017, Caloia et al, 2021). While the wage employed often only need to report their last salary, the self-employed (including own-account workers) need to produce their company results for the last three years and discuss with the bank what their 'normal' income is. This is more difficult in a period of pronounced economic fluctuations. As a result, loan negotiations could last longer or lead to the self-employed making higher down payments to obtain a loan (and thus possibly access to cheaper credit if needed, see Adelino et al, 2015). So, what appears to be the result of an improving financial position of the self-employed relative to the wage employed, could be the result of a selection mechanism where the self-employed qualify for a loan only in the presence of a larger down payment. It is therefore relevant to research whether sharpening credit restrictions disproportionally affects the self-employed, as this could be an impediment not only to purchasing a home but also to increasing their pension savings.

Figure 4 shows the age at (latest) mortgage origination, including both starters and re-negotiators together. The data used, which are retrospective, show that in each period the self-employed tend to be older than the wage employed when they apply for a mortgage. The difference became larger during the financial crisis (2009-2013) when it peaked at two and a half years. This evidence suggests that the self-employed needed longer to receive credit, which might suggest a higher level

Figure 4: Age at current mortgage origination by labor market status.



Explanatory note: Retrospective evidence based on 2018 Q4 data. Source: DNB loan level-data, own computations.

of scrutiny from banks, or a longer period to achieve the required down payment. Referring to the fact that their population increased in number, in the first period the share of self-employed among borrowers was 8%; this share increased to 14% in the most recent period. This could in turn point to a selection effect whereby older individuals become self-employed and take on a (new) mortgage at the same time.

3. Model and estimation results

Theoretical considerations

We employ the same model as that used by Biesenbeek et al. (2021), which is underpinned by a theoretical model devised by Brueckner (1986), who shows that an increase in the LTV limit reduces the probability of becoming a homeowner if the down payment constraint is binding. We test whether the results differ between the wage employed relative to the self-employed. We also study the possible dynamic selection of those shifting between labor market states prior to home purchases.

The effect of the introduction of the LTV limit, which was common to all households each year, can be separately identified relative to other year-specific effects (such as housing market conditions) by comparing a treatment group ("the constrained") against a control group for whom the 6% down payment constraint is not/less binding ("the unconstrained"). The parameters of interest are the interactions of the treatment group dummy with the required down payment, with the dummy that indicates the years since 2012, when the LTV limit was introduced, and with the self-employment indicators.

The probability of becoming a (first-time) homeowner in a certain period is the conditional likelihood of buying a house given the probability that one did not ever buy a house before. We use a duration model to estimate the probability for a first-time homebuyer to buy a house, which is conditional on covariates, including the exogenous indicators for the presence of an LTV limit, the required down payment and for being credit constrained.

Duration models are dynamic models with a hazard rate as a key parameter. In economics, duration models are mostly known for their applications in unemployment studies, where exit out of the sample (success) means finding a job. There are also some applications in housing economics, where the hazard rate is defined as the transition from one type of housing to another. Andrew, Hauren, and Munasib (2016), Boehm and Schlottmann (2008) estimate the hazard rate from renting to ownership; Bahchieva and Hosier (2001) examine the hazard rate of leaving a public house; Guiso and Jappelli (2002) estimate the effect of private transfers on the hazard rate of becoming a homeowner; Deutsch, Tiwari, and Moriizumi (2006) evaluate the spells starting at adulthood age to first-time homeownership; di Salvo and Ermisch (1997) use a competing risk proportional hazards model to estimate the hazard of either home ownership or social housing. The most common models used to estimate the effect of borrowing constraints on the probability of home ownership in the literature are binary choice models such as the logit and probit models (Acolin, Bricker, Calem,

& Wachter, 2016; Bourassa, 1995; Linneman & Wachter, 1989; Quercia, McCarthy, & Wachter, 2003). Duration models have several advantages. First, they control for right censoring. Some individuals do not buy a house within the observation period, and those individuals are not random. Leaving them out would bias the estimated effect of the LTV limit on the probability of transition to homeownership. Furthermore, a duration model estimates the probability of homeownership over the full length of the spell before purchasing and thus employs more information than a simple model that estimates the probability at a specific point in time. Finally, duration models allow for time-varying coefficients and take life events including a new job with a higher wage or becoming a couple into account.

Estimating model

We use a proportional hazard model to estimate hazard rates after controlling for censoring. This approach is similar to that used by Biesenbeek et al. (2021), Guiso and Jappelli (2002) and Deutsch et al. (2006). We assume individuals would start considering homeownership from adulthood at the age of 18. The proportional hazard model can be written as a combination of a baseline hazard and an individual hazard:

$$h(t) = h_0(t)\exp\left(\beta_x X'_{i,t}\right) \tag{1}$$

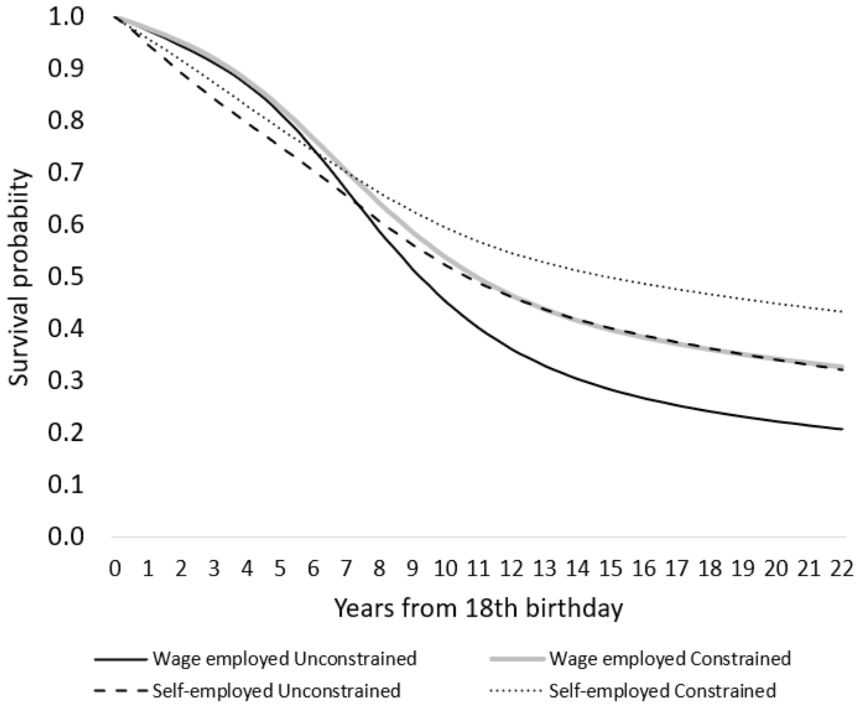
where $h_0(t)$ is the baseline hazard that is the same for everyone, t time of house purchase, X_i a vector of individual (time-varying) characteristics and $ß_x$ the effect of these characteristics on the hazard rate.

The hazard rate $h(t)$ is the probability of becoming a homeowner at t, given that one has not been a homeowner before, and is equal to the ratio between the cumulative probability (or the probability of becoming a homeowner before t) and its inverse. Cox proportional hazard models do not impose any restrictions on the baseline hazard and are less prone to misspecification.

Inspection of the data shows that the median time before purchasing a house is approximately 9 years for unconstrained individuals and 10.5 years for constrained individuals. This implies that, after 9 (10.5) years, approximately half of the constrained (or unconstrained) sample has made the transition to homeownership, while the other half of the sample still rents or lives at their parents' house. Approximately 30% of the constrained sample versus 16% of the unconstrained sample has not made the transition to homeownership before being censored at the age of 40.

Figure 5 shows the smoothed empirical survivor function for the four groups of interest, namely the wage employed and the self-employed when they are either constrained (treated) or unconstrained (control).

Figure 5: KM survivor functions for first-time homebuyers



Explanatory note: Unconstrained individuals are those with higher parental wealth. Source: CBS microdata, own computations.

This is plotted on the 22 years between the age of 18 and 40. All observations are censored thereafter. The figure shows that the survival probability first decreases at an increasing rate and later at a decreasing rate. Adding the employment dimension, we can now look at the general difference between constrained and unconstrained mentioned above for the specific groups of self-employed and wage employed people. The figure shows that this difference is approximately the same for the self-employed and the wage employed. The self-employed are more likely than the wage employed to purchase at younger ages, less when older when most tenants buy. These figures do not correct for business cycle nor background characteristics, which we deal with next.

We have no a priori knowledge about the shape of the baseline hazard rate and assume that elapsed time since the 18th birthday plays no substantial role in the probability of buying a first house. We estimate the following hazard function:

$$h(t) = h_0(t) \cdot exp\left(\alpha C'_{i,t} \cdot \delta L'_t \cdot \gamma_1 (L * C)'_{i,t} \cdot \gamma_2 (C * S)'_{i,t} \cdot \gamma_3 (L * S)'_{i,t} \cdot \gamma_4 (L * C * S)'_{i,t} \cdot \beta_x X'_{i,t}\right) \tag{2}$$

where L is a dummy for the presence of an LTV limit, with value 0 before 2012 and 1 in or after 2012; C a dummy for being financially constrained, $X_{i,t}$ a vector of control variables including a self-employment dummy (S). The interaction terms allow us to estimate the γ's that are the key coefficients here. These allow us to test whether the LTV limit caused constrained individuals to postpone purchase (γ_1), whether the constrained self-employed are always less likely to buy (γ_2), and whether the self-employed were less likely to buy when the policy was enacted (γ_3) and whether this was actually due to the policy itself (γ_4) rather than to other concurrent confounding factors.

Estimation results, different specifications

Table 3 shows the estimation results using a Weibull hazard function, where we present four models. The different specifications refer to a model with no specific consideration for self-employment (1), a lagged definition of self-employment (2), a current one (3) and finally one with a triple interaction term (4).

Table 3: Parametric duration model with Weibull distribution, hazard ratios

	Model 1	Model 2	Model 3	Model 4
Constrained	0,828***	0,913***	0,932***	0,916***
LTV limit applies	0,857***	1,076***	1,233***	1,188***
Constrained * LTV limit applies (γ_1)	0,937***	0,930***	0,874***	0,920***
Constrained * self-employed (γ_2)				1,002
LTV limit applies * self-employed (γ_3)				0,532***
Constrained * LTV limit applies * self-empl. (γ_4)				1,047***
Self-employed in t-1		1,141***		1,351***
Self-employed in t			0,771***	
Currently living with parents	0,820***	1,242***	1,375***	1,250***
Female	1,408***	1,488***	1,350***	1,477***
West migrant	0,946***	0,895***	0,854***	0,895***
Other migrant	0,873***	0,730***	0,688***	0,731***
Cohabiting	0,362***	0,306***	0,306***	0,306***
Living alone	1,294***	1,198***	1,043***	1,206***
Education: secondary	1,272***	1,276***	1,287***	1,256***
Education: Bachelors	1,110***	1,353***	1,223***	1,315***
Education: Masters	0,949***	1,052***	0,994	1,006
Education: secondary * female	1,055***	0,987**	0,976**	0,984**
Education: Bachelors * female	0,880***	0,788***	0,819***	0,789***
Education: Masters * female	0,823***	0,780***	0,833***	0,783***
Household income (normalized)	2,599***	4,650***	4,527***	4,743***
Household income (normalized) squared	0,934***	0,844***	0,855***	0,842***
Value of purchasable house (prediction)	0,968***	0,955***	0,945***	0,955***
Constant	0,130***	0,115***	0,142***	0,105***
Observations (millions)	3.76	0.9	3.76	3.76

Explanatory note: Dependent variable: analysis time in years, starting at the 18th birthday, ending at censoring or transition to homeownership. Robust standard errors, clustered by individual (N). Source: CBS microdata, own computations.

The coefficients have already been exponentiated. The estimate for appears in all models and always shows that constrained individuals are less likely (<1) to buy a house at each point in time. So in Model 1 one could say that constrained individuals are 6.3% (1–0.937) less likely to buy a house in each period. Across the different models, this figure changes slightly and always stays significant.

The separate effect of the self-employment indicator varies depending on whether we measure the self-employment lagged (Model 2) or at current time (Model 3). We show this in order to highlight the fact that employment choices are a dynamic decision margin, thus possibly endogenous to moving choice in each t. Think of the following example. The self-employed experience a higher credit rejection rate (Caloia et al 2021). They could choose (temporary) wage employment to reduce their income volatility and receive credit. If this happens, employment choices and home purchases are endogenously related. If the self-employed who experience credit rejection in $t\text{-}1$ become wage employed in t in order to receive credit, this might select the sample and leave in t only the most creditworthy individuals in self-employment. This could for instance explain why in model (3) the self-employed at the moment of purchase are 23% (1–0.771) less likely to buy, and in model (2) if they are self-employed, they are actually only in t–1 14% (1.141 – 1) more likely to purchase a home. At the same time though, switches in employment status could signal income volatility, often delaying credit.

The specification in Model (4) shows all the interaction terms. Most notably γ_3 is below 1, indicating that after 2012 (when the LTV limit was introduced and then lowered) – thus in the years after the financial crisis had reached a peak – the self-employed were 47% (1–0.532) less likely to buy a house relative to the wage employed and relative to previous periods. This is, however, true for the self-employed both in the treatment and in the control group. The estimate of γ_4 is so close to 1 (namely 1.047) that there is hardly any additional difference caused by the constraint on the self-employed, and if any, it would even indicate a modest positive effect. We consider this the main result of this study, as the very different saving incentives across occupations could be expected to cause a differential effect of sharpening credit constraints. After the financial crisis the self-employed were less likely to buy, but not because of the sharpening of the LTV limit. In Model 4, we have chosen to apply the definition of the self-employed at time t, as in Model (3) this seemed to deliver a negative effect on purchases. So the absence of a differential impact for constraints on the self-employed presented here delivers a more convincing result. Specification checks with the definition of self-employed in t–1 delivered a similar γ_4 (not shown). All other estimated parameters are qualitatively similar across the different specifications. Females, higher educated, and those living with parents are more likely to buy in each period. Migrants, cohabiting individuals and those likely to buy a more expensive property are less likely to do so. Our approach could be deployed on these specific groups as well, for instance, to test whether the less educated are more affected by the new LTV limit. This is however beyond the aim of this study, which is concerned with the group of those without compulsory occupational pensions.

Finally, we also carried out a specification of Model 2 where different types of self-employed people (entrepreneurs, those working in family business, freelancers) are considered separately, in order to pick up part of the heterogeneity across the self-employed discussed above. This specification (not shown) did not return any noteworthy difference among subgroups of the self-employed.

To sum up, the estimation results suggest that in general LTV limits delay the purchase of a home very little, the crisis impacted the self-employed a lot, but additional credit constraints did not cause the self-employed to further delay home purchases. Yet some individuals might have abandoned self-employment just prior to purchasing a home.

Table 4: Robustness checks

	Constrained * LTV limit applies (γ_1)	Constrained * self-employed (γ_2)	LTV limit applies * self-empl. (γ_3)	Constrained * LTV limit * self-empl. (γ_4)	N
Baseline model	*0,920****	*1,002*	*0,532****	*1,047****	*3 765 095*
Different specification of parental wealth:					
Parental wealth threshold = 100k	0,897***	0,943***	0,497***	1,130***	3 765 092
Parental wealth threshold = 30k	0,943***	1,050***	0,552***	0,987	3 765 085
Parental wealth threshold divided by number of siblings = 20k	0,922***	1,011	0,546***	1,008	3 765 088
Parental wealth threshold divided by number of siblings = 35k	0,898***	0,965***	0,522***	1,067***	3 765 090
Parental wealth threshold: housing wealth = 100k	0,977***	0,999	0,553***	0,986	3 765 095
Different inclusion in the sample:					
Sample inclusion starts at age 21	0,914***	1,070***	0,632***	1,022	2 858 591
Sample inclusion starts with first employment	0,908***	1,021**	0,493***	1,076***	2 693 617
Different hazard model:					
Cox proportional hazard	0,926***	1,000	0,517***	1,055***	3 765 095

Explanatory note: The baseline model is Model 4 in Table 3. Robust standard errors, clustered by individuals (N). Source: CBS microdata, own computations.

Robustness checks

The results above rely on a number of assumptions related to the attribution to a specific treatment and control group (parental wealth above €50,000), the age selection of the sample, and also to the hazard model. In Table 4, we carry out some robustness checks with which we modify our assumption and the hazard model.

We have estimated the specification in Model 4 above where we assume that those that are less credit constrained are those with parental wealth above 100k or 30k; with parental wealth divided by the number of siblings above 20k or 35k; or with housing wealth above 100k. The upper panel of the table shows that only in the first case do the results depart somewhat, though not much, from the baseline specification, whereby the constrained self-employed seem to buy more easily relative to the wage employed. In the middle panel instead, we drop the sample aged between 18 and 20, as they are very likely to be students, or we choose the age of first employment as a starting date. In either case, the baseline result is confirmed. Notice that we do not increase the age after 40, because hardly anyone purchases a first home in the Netherlands after that age. Finally, the Cox proportional hazard model also returned similar results to the Weibull model.

4. Summary and conclusions

Those who are not obliged to save for a pension – for instance the self-employed in many countries – might be investing in housing instead. Data shows that in the Netherlands relatively few self-employed people invest in private pensions, so investing in housing could be a plausible alternative. However, sharpening macroprudential measures, a common response after the financial crisis, in the form of tighter borrowing constraints, might delay or even prevent access to the housing market, thus conflicting with the housing channel as a possible future source of retirement income. We study the effect of the introduction and sharpening of the LTV limit in the Netherlands on the probability of the self-employed versus wage-employed first-time buyers becoming homeowners, thus saving at least the required down payment. We construct a treatment and control group using parental wealth as a proxy for being liquidity constrained. We find a larger delay for the credit constrained self-employed in the aftermath of the crisis, but this was not caused by the introduction of the LTV limit. The introduction and sharpening of the LTV limits have no differential impact on the credit constrained self-employed relative to the constrained wage employed. We suggest that some individuals might have abandoned self-employment just prior to purchasing a home, though this dynamic selection would need additional investigation.

These findings link to several other policy discussions not directly researched here, such as the aim to increase the participation in occupational pension plans of younger self-employed people using experiments. Here we show that sharper LTV limits, thus a greater need to save for a down payment, did not impact the self-employed specifically. So, we should not expect these limits to impact their retirement saving decisions, nor interfere with the results of such experiments.

We speculate that policies that could affect the self-employed more directly are those designed to monetize home equity after retirement. Products such as reverse mortgages and cash-out loans have a very different appeal at the moment and might need further development. Future research could address this. Also, the dataset we built and the method that we applied could be used to estimate the causal effect of policies that apply to liquidity constrained individuals, even in the absence of differential treatment in the policy. For instance, one could study different decision margins that could be affected by sharpening credit constraints. Think of saving behavior or labor supply, which could both increase and prevent a delay in the purchase of a house. In so far as a house is a substitute for an occupational pension, alternative pension savings could also be affected by such policies. We leave further exploration of these issues to future research.

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