Limiting debt to income: Household adaptation and avoidance*

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Abstract

This paper investigates the effect of the debt-to-income limit for residential mortgages imposed in Norway in 2017. Information from the tax register merged with house transaction data shows that debt to income remained high with a share of about 18 per cent of home buyers above the limit of five also after the regulation was implemented. The findings from a difference-in-differences analysis indicate that there was no significant effect of the limit on affected households' house value, household debt or financial wealth. However, interest rates for exposed home buyers increased by 0.12-0.14 percentage points after the regulation was implemented.

JEL-codes: *E21; E58; G21; G28; G51*

Keywords: *Household leverage, Financial regulation, Macroprudential policy, Mortgage markets*

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1 Introduction

Excessive growth in house prices and household credit in a boom, increases the likelihood of an economic crisis as well as the duration and depth of the crisis.[1] For this reason, many countries have adopted macroprudential policies in the wake of the global financial crisis. Policies include capital requirements for banks and limits on leverage for residential mortgages, to counter the build ups of financial imbalances and to make banks more resilient. Residential mortgage regulations might also have unintended consequences, such as reducing opportunities in the housing market for first-time buyers and low-income households and reducing households' liquid assets.[2]

This paper investigates how the debt-to-income (DTI) limit imposed in Norway in 2017 affected house values, household debt and financial wealth for Norwegian households. Tax records at the household level in 20 Norwegian municipalities reveal that the share of home buyers with DTI above the limit of five remained stable at around 18 per cent after the regulation was implemented. The results from a difference-in-differences analysis indicate that there was no significant effect on house values or household debt from the regulation. However, interest rates for exposed house buyers increased by 0.12-0.14 percentage points after the regulation.

The analysis is based on information from the tax-register merged with housing transaction data from Eiendomsverdi. The empirical framework is

[1] See for example Kiyotaki and Moore (1997), Reinhart and Rogoff (2009), Mian and Sufi (2009), Jorda et al. (2013), Leamer (2015), Anundsen et al. (2016) and Mian et al. (2017).

[2] See Acharya et al. (2022) and Aastveit et al. (2020).

a difference-in-differences approach in which the treated group consists of households predicted to have DTI above the limit in absence of the regulation. This prediction is made on the basis of a household model for DTI with data from the year prior to the regulation (2016). DTI is regressed, using ordinary least squares, on a set of household characteristics including age, education, zip-code and household balance sheet data. Further, a dynamic difference-in-differences approach is implemented to inspect whether house values, household debt and financial wealth are reduced for treated households compared to households that are not treated after the DTI-limit was introduced in 2017.

The results show that there was no significant effect on house values or household debt for affected households. Neither was there any effect on financial wealth (as a proxy for liquid assets), which could have been a potential negative side-effect of the regulation, see for example Aastveit et al. (2020). Regarding the more exposed groups; low-income households and first-time-buyers, results are similar to those for the whole sample. The findings are robust to several sensitivity checks within the difference-in-differences framework. The difference-in-differences analysis is also supplemented with a regression discontinuity exercise to demonstrate that the pattern of no or little effect is valid also when using an alternative empirical framework.

Implicit interest rates, calculated as interest expenses as a share of total debt, increased by 0.12-0.14 percentage points for house buyers with DTI above five relative to other households, after the regulation was implemented. The increase in implicit interest rates was more than 0.20 percentage points

for low-income households and households owning a secondary home (referred to as investors in the following). Loans have thus become more expensive for exposed households. This could be caused by banks requiring fixed-rate mortgages or a higher floating mortgage rate for households in violation of the requirements, but it could also mean that households increased consumer loan borrowing, which was not a part of the regulation before mid-2019. If consumer loans partly replaced residential mortgages after the regulation on residential mortgages was implemented, this could also be an explanation for the debt-to-income level remaining high. More thorough investigation of this hypothesis would require data on consumer loans which is not available in the data set used here.

Debt to income stabilizes at a high level after the regulation. The share of home buyers with debt to income above the limit of five is stable at around 18 per cent from 2016 to 2018. The lack of effect on house values and household debt might be a natural consequence of this finding; the limit does not bind for a large share of households. The flexibility quota of 10 per cent of the lending volume allowed to deviate from the regulation each quarter might be large enough for the limit not to be binding. The banks might choose to use a larger share of the flexibility quota on new home buyers and a lower share on loans to existing home owners. Another possible explanation for the high share of home buyers in voilation with the limit could be increased use of co-borrowing with parents outside the household. Young households desiring a high DTI can share some of the risk with a parent with low DTI, and use the parent income as part of the denominator in the DTI-calculation. More data is needed to conclude on this.

The main contribution of this paper is first to be one of very few papers to present an empirical investigation of a DTI-limit, which has become a common supplement to LTV-limits that are more often analysed in the empirical literature. Second, this is the first paper, to the best of my knowledge, to report that the limit does not seem to bind and to suggest a few possible explanations for why a large share of home buyers continue to violate the requirement. Although more work is needed to conclude on this, I present evidence that can help inform and guide policy makers that are responsible for the design of such policies. The findings suggest that the Norwegian DTI-limit has not been successful in reducing house prices and household debt and thereby vulnerabilities in the household sector. Future regulation designs should therefore focus at other measures than the DTI-limit or make changes in the regulation such that it becomes more binding.

Literature review

This paper relates to the empirical literature on macroprudential policies. The last years, several studies have documented that such policies do have the intended dampening effect on house prices and credit. Jimenez et al. (2017) find that countercyclical capital buffers had a smoothing effect on the credit cycle in Spain. A set of cross-country studies find that macroprudential policies have a negative effect on house prices and credit, but there is less consensus about the size and persistence of the effects and which measures are relatively more effective, see for example Kuttner and Shim (2016), Claessens et al. (2013), Carreras et al. (2018), Alam et al. (2019) Davis et al. (2019), Nymoen et al. (2019) and Akinci and Olmstead-Rumsey (2018).

There are several papers investigating whether loan to value- (LTV) lim-

its have been effective in dampening growth in household credit and house prices in individual countries. Armstrong et al. (2019) analyze the LTV-limits in New Zealand and Laufer and Tzur-Ilan (2021) and Tzur-Ilan (2023) look at the Isralian LTV-policies. Aastveit et al. (2020), Van Bekkum et al. (2019) and DeFusco et al. (2020) analyze LTV-limits using a methodological framework similar to this paper. They all find the expected dampening effect on credit and house prices of the LTV-limits, but some also document non-intended side-effects, such as a reduction in liquid assets after the implementation of the regulations. Balke et al. (2023) show analytically how LTV-policies delay saving for housing and postpone home ownership.

This paper contributes to this literature by digging deeper into the effect of DTI-limits, which have become a common supplement to LTV-policies the last years.

There are also a few other studies investigating the effect of DTI-limits. Acharya et al. (2022) investigate the effect of DTI- and LTV-limits on different income quintiles in Ireland and Peydró et al. (2020) look at the effect of a loan to income- (LTI) limit in the UK on low-income households. Both studies find that low-income households are harder-hit by the limits. Haug (2022) does not find any significant effect of the DTI-limit in Norway on credit or house prices, but detects a significant positive effect on the price of credit for affected households.

The results in this paper complements these studies by bringing forward an example from Norway in which the DTI-limit does not seem to bind and seek to bring forward some explanations for why. This should act as valuable input for policy makers in Norway, as well as in other countries that have

adopted or plan to adopt such policies.

The paper proceeds as follows. In Section 2 the institutional background, data and empirical approach are presented. In Section 3 and 4 the main results are introduced and discussed. Robustness checks are shown in Section 5 and the final section concludes.

2 Institutional background, data and empirical approach

2.1 Residential mortgage regulations in Norway

Macroprudential policies directed towards residential mortgages, for example upper limits on loan to value (LTV), debt to income (DTI) and amortization requirements, had been implemented in 26 European countries by 2022 according to the Financial Supervisory Authority of Norway, Finanstilsynet (2022b).

In Norway, residential mortgage loan guidelines, including a LTV-limit of 90 per cent and a DTI-limit of 3, were implemented in March 2010. The LTV-limit was tightened to 85 per cent in December 2011, while the DTI-limit was removed. In July 2015, the guidelines were formalized as regulation. A flexibility quota of 10 per cent of the lending volume could deviate from the requirements each quarter. In January 2017, the regulation was tightened along several dimensions and a DTI-limit of 5 was introduced. In 2020, the flexibility quota was temporarily increased (due to the covid-19 crisis) from 8 per cent in Oslo, and 10 per cent in the rest of the country, to 20 per cent in

Table (1) Residential mortgage loan regulations in Norway

	Guidelines			Regulation			
	2010 Mar.	2011 Dec.	2015 Jul.	2017 Jan.	2020 Apr.	2020 Oct.	2023 Jan.
LTV-limit	0.90	0.85	0.85	0.85	0.85	0.85	0.85
DTI-limit	3			5	5	5	5
LTV line of credit	75	70	70	60	60	60	60
Interest rate stress		5pp	5pp	5pp	5pp	5pp	3pp*
LTV sec. homes				(0.60)	(0.60)	(0.60)	
Flexibility quota			0.10	0.10	0.20	0.10	0.10
				(0.08)		(0.08)	(0.08

Notes: This table gives an overview of residential mortgage guidelines and regulations in Norway. Requirements only applicable to the capital Oslo are in parenthesis. DTI is short for debt to income, while LTV is short for loan to value. *A required minimum stress interest rate of 7 per cent.

the whole country. In 2021, the regulation was extended to include unsecured loans like consumer loans. In 2023, the interest rate stress test was eased to 3 percentage points, but with a requirement that the total stress rate is 7 per cent or higher. Table 1 summarizes the guidelines and regulations.

2.2 Data and summary statistics

The data set is from the Norwegian tax registry, and include annual observations on households' balance sheets from the 20 most populated municipalities in Norway for the period 2004-2019. The data consist of balance sheet information like income, debt, wealth, interest expenses, market value of primary and secondary homes, as well as education, profession, age, sex and household type. The household balance sheet data are linked, through a personal identification number to a data set from Eiendomsverdi, contain-

Table (2) Restrictions on the data set

Restrictions	Obs. 2016	Obs. 2018
No restrictions	23,096	24,083
Exclude self-employed	20,553	21,613
Include only positive debt	20,115	21,148
Exclude incomes below poverty line	18,154	19,340
Trim 1st and 99th percentile	16,412	17,497

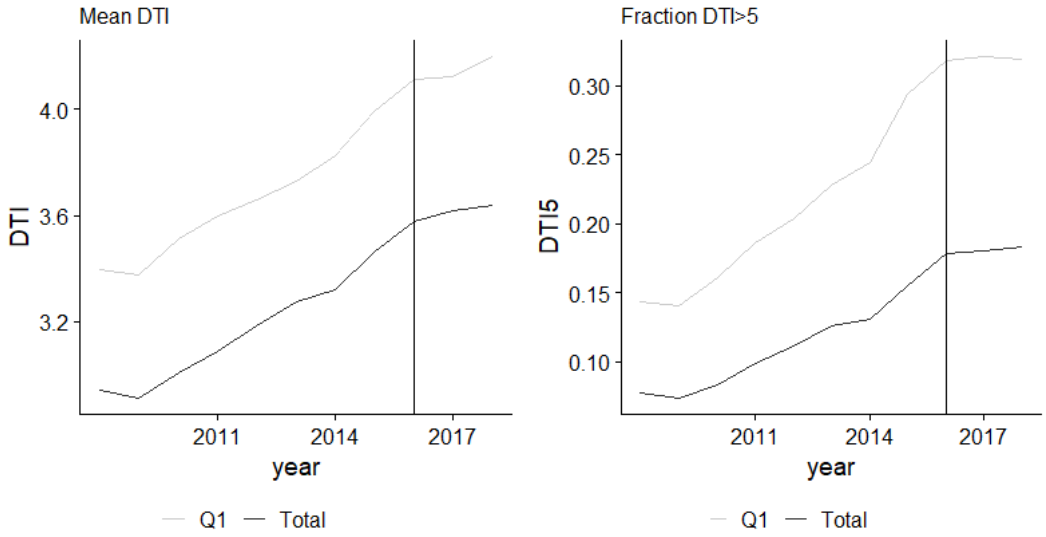
Notes: This table shows the restrictions made on the data for housing transactions in 2016 and 2018 and how each restriction affects the number of observations. Trimming on the 1^{st} and 99^{th} percentile is done for the variables income, debt, wealth, debt to income, house prices and living area.

ing information about Norwegian housing transactions from 2000-2020. This data set includes information about the sales date, ask and sell price and house- and location characteristics.

The data is trimmed by removing observations with negative incomes, while negative capital incomes are set to zero. An implicit assumption is therefore that banks do not count in negative incomes in their calculations of debt to income. The rationale for making this assumption is that banks are interested in expected income, and negative incomes are probably not expected to be repeated in the following year. For households with one, or several, household members experiencing income growth of more than 10 per cent, it is assumed that this individual has changed occupation, from for example student to employed, or from unemployed to employed. Since banks are assumed to use current and expected income in the period ahead when granting loans, income for the succeeding year is used for these individuals.

Self-employed are excluded from the analysis to avoid counting in corporate debt and because incomes of self-employed are volatile and might

Figure (1) Debt to income house buyers. 2008-2018

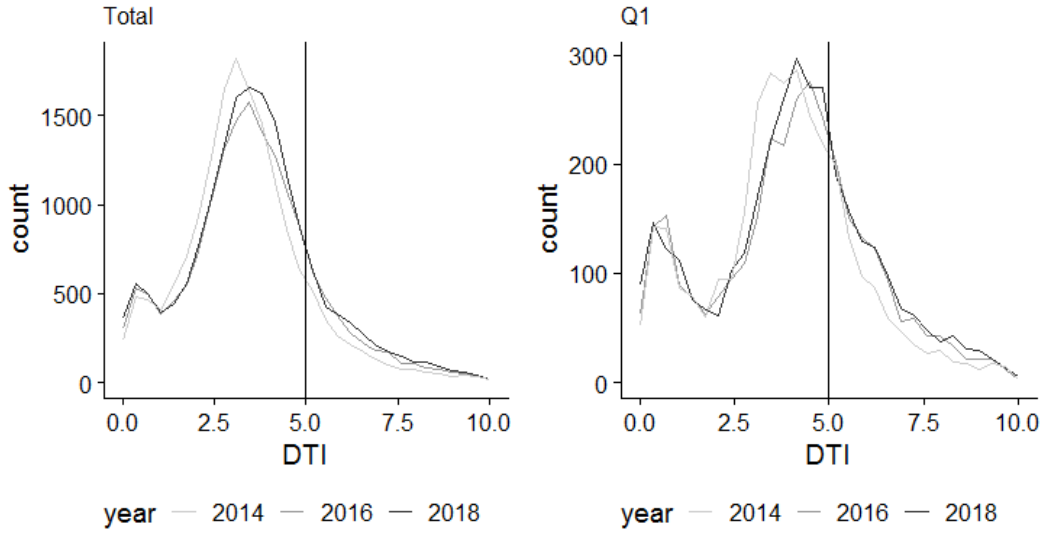


Notes: DTI is defined as total debt divided by total income. $DTI > 5$ is the fraction of households with a DTI above 5. Q1 is the 20 per cent of households with the lowest income (between NOK 300K and NOK 500K).

make the debt-to-income measure misleading. Only households with positive debt are included for it to be possible to calculate the debt-to-income measure. Households with income below the Norwegian poverty line are not part of the analysis, since a non-trivial share of these households are likely to have obtained mortgages from The Norwegian State Housing Bank, the main agency implementing Norwegian social housing policy at the national level, which is not covered by the regulation. Subsequently, income, debt, wealth, debt-to-income, house values and living area are trimmed on the 1st and 99th percentiles. The restrictions made on the data set and the effect on the number of observations for house buyers in 2016 and 2018 are summarized in Table 2.

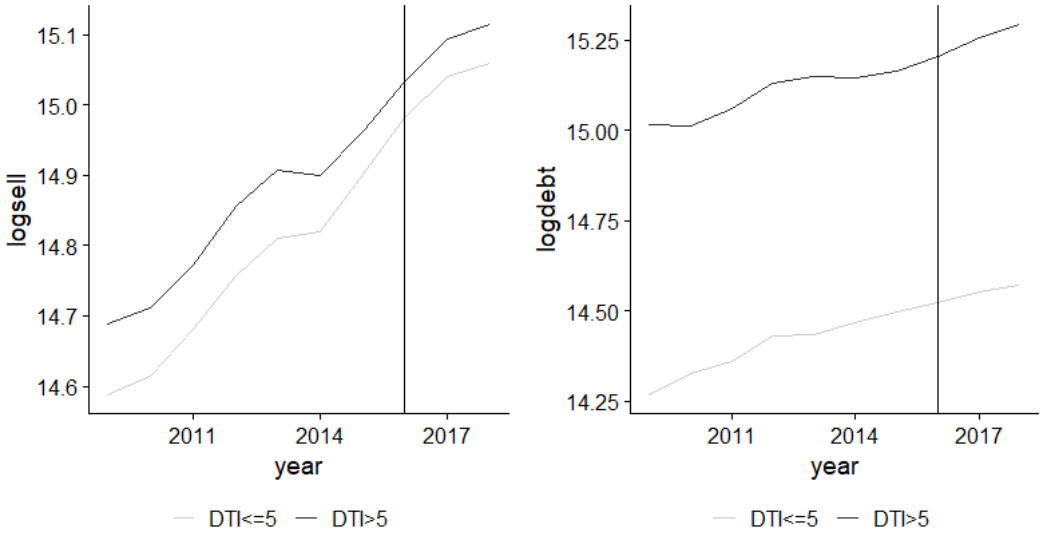
Figure 1 gives us an overview of developments in average DTI and the

Figure (2) Distribution debt to income house buyers. 2014, 2016 and 2018



Notes: DTI is defined as total debt divided by total income. The vertical line shows the DTI-limit of 5. Q1 is the 20 per cent of households with the lowest income (between NOK 300K and NOK 500K).

Figure (3) House values and household debt for house buyers. 2009-2018



Notes: The right hand side chart shows the trends in household debt, while the left hand chart shows trends in house values for households with debt to income (DTI) above (black line) and below (grey line) five, respectively. All variables are in log of 1000 NOK.

fraction of households with DTI above 5 for house buyers from 2008-2018. We can see that there has been an increasing trend in these variables over the period, which seems to flatten somewhat in 2017, when the regulation was implemented. The flattening of the trend suggests that the regulation has been binding to some extent. On the other hand, the fact that the trend is not decreasing after the DTI-limit was implemented, suggests a modest effect of the regulation. We can also see developments in the same variables for the first income quintile (Q1), defined as the 20 per cent of households with the lowest household income. The trends look similar, while the levels are substantially higher for low-income households.

In Figure 2, we can see the complete distribution of DTI for house buyers in 2016, the year before the DTI-limit, and in 2018, the year after the DTI-limit was implemented. The DTI-distribution in 2014 is also included to illustrate how the distribution has shifted to the right over time, also before the DTI-limit was implemented. The distribution of DTI is quite similar in 2016 and 2018, which once again suggests a modest effect of the regulation. Again, it is evident that the increasing trend from 2014 to 2016 is not repeated from 2016 to 2018. Hence, the regulation might have had some effect, but there is still a fat tale on the right side of the distribution in 2018, indicating that many households are not compliant with the limit. Again, we see that low-income households in general have a higher DTI-level than other households.

In Figure 3 we see the development in the average house value and average household debt (in logs) for house buyers from 2009 to 2018 for those with

Table (3) Summary statistics households purchasing a home in 2016 and 2018

	2016				2018			
	1. qu.	Med.	Mean	3. qu.	1. qu.	Med.	Mean	3. qu.
Income (ths.)	534	785	905	1120	563	812	946	1178
Debt(ths.)	1846	2672	3005	3818	1961	2816	3217	4063
House value(ths.)	2415	3162	3583	4300	2570	3350	3813	4550
Liv.area (sqm)	62	83	102	128	62	83	102	129
H.value/sqm(ths.)	26	36	41	53	28	39	44	58
Interest exp.(ths.)	26	48	58	77	28	50	61	82
Value.pr.home(ths.)	2171	3078	3171	4054	2353	3282	3415	4347
Fin.cap.(ths.)	80	197	409	452	87	211	426	473
Age	28	34	38	45	28	34	38	46
DTV	0.619	0.894	0.912	1.111	0.605	0.889	0.895	1.098
DTI	2.47	3.48	3.58	4.55	2.52	3.53	3.64	4.55
$DTI > 5$			17.9				18.3	
No.obs	16,412				17,497			

Notes: Table 3 shows summary statistics for house buyers in 2016 and 2018. DTV is defined as total debt divided by the price of your house. DTI is defined as total debt divided by total income. $DTI > 5$ is the fraction of households with a DTI above 5. Age is measured in years. No.obs is the number of observations. The remaining variables are measured in 1000 NOK.

DTI above 5 versus those with DTI equal to or below 5. Here, we can inspect whether there are indications that the trend in these variables, prior to implementation of the DTI-limit, differs between the groups. The main impression from Figure 3 is that trends in house values and household debt look relatively similar for households below and above the limit prior to regulation.

Table 3 gives an overview of summary statistics for key variables for house buyers in 2016 and 2018. We can see that household income, debt and house values increased from 2016 to 2018. Since the living area remained quite

constant, also the square meter price of houses increased somewhat. Interest expenses, the value of primary homes, as calculated in the tax registry, and financial wealth increased from 2016 to 2018, while the age distribution of house buyers is similar across the two years. Household debt to house value (DTV) decreased slightly, while debt to income (DTI) actually increased from an average of 3.58 2016 to 3.64 in 2018, despite the implementation of the DTI-limit of 5 in 2017. Also the fraction of households with DTI above the limit increased slightly in this period, from 17.9 per cent to 18.3 per cent. This resembles the picture in Figure 1, but is in stark contrast to the figures reported by banks to the Financial Supervisory Authority of Norway, Finanstilsynet (2022a), in which there is a sharp drop in mortgage volumes with a DTI above 5 after regulation. The figures are not directly comparable, but it is still worth noting that there are no indications of bunching of loans close to, or below the limit of 5 in the tax statistics, while there are clear indications of such bunching in Finanstilsynet (2022a).

2.3 Empirical approach

To estimate the causal effect of the debt-to-income limit to house prices, household debt and financial wealth for Norwegian households, I use an empirical approach which is similar to that of Van Bekkum et al. (2019) and Aastveit et al. (2020). The framework is a difference-in-differences approach, in which the treated group are households who in absence of regulation are predicted to have DTI above the limit of 5 after the limit is implemented. A central assumption is common time trends among those below and above the limit in absence of the limit. The identification is made in two steps.

$$DTI_i = \alpha_k + \beta_1 age_i + \beta_2 edu_i + \eta_1 I_i + \eta_2 S_i + \sum_{j=2}^{5} \eta_{3,j} qi_{i,j} + \eta_4 U_i + \mathbf{b'X_i} + \epsilon_i \quad (1)$$

First, DTI is regressed on a set of household characteristics in the year prior to implementation of the DTI-limit (2016). The left-hand side variable in equation (1) is debt-to-income (DTI) with subscript i indicating household i. On the right hand side, α_k represents zip-code fixed effects, while β_1 and β_2 are coefficients on household age and education, respectively. η_1 is the coefficient on a dummy-variable equal to 1 if the household owns a secondary home, while η_2 and $\eta_{3,j}$ are coefficients for dummy variables for one-member households and each income quintile, j. U_i is equal to one if one or more household members are unemployed. **b'** is a coefficient vector on household balance sheet variables $\mathbf{X_i}$. These variables are current and lagged values of interest expenses, the value of primary home and financial wealth, in addition to lagged values of income and debt.

Based on the results from the estimation of Equation 1, debt-to-income is predicted; $\widehat{DTI}_{i,t}$. A dummy variable equal to one if $\widehat{DTI}_{i,t}$ is above the DTI-limit of 5 is defined as the treatment variable. This dummy-variable is called $\widehat{DTI}_{i,t}^{treated}$. The actual and predicted distribution of DTI from the model are shown in Appendix A.

$$y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI_{i,t}^{treated}} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI_{i,t}^{treated}} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI_{i,t}^{treated}} + \epsilon_{i,t} \tag{2}$$

The second step is showed in Equation 2. Here $\widehat{DTI_{i,t}^{treated}}$ is the treatment group in a dynamic difference-in-differences framework. Also here, zip-code fixed effects are included in addition to current and lagged house price growth at the municipality level. The β_t's show the effect of the interaction between treatment and years. For the results to indicate an effect of the DTI-limit, we should see statistically significant coefficients for the β_t's after implementation of the limit (2017 and 2018), while they should be insignificant in the years prior to implementation. Household controls are all included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners and single households. In addition, year-fixed effects, δ_t, and the treatment variable are included. In the main estimation, all standard errors are clustered by zip-code. I have also experimented with using bootstrap wild standard errors, but these are very similar to the normal cluster robust standard errors.

The dependent variables, $y_{i,t}$, in Equation 2 are house values, household debt and financial wealth (as a proxy for liquid assets). The logarithm of these variables are considered in all estimations.

3 Results

3.1 Baseline results

Table 4 shows the results of the baseline estimation. All dependent variables are in logs. The first column shows the effect on log of house values. We see that the effect of the interaction between the treatment variable (households predicted to have a DTI above 5) and the post variable (year 2018) is not statistically significant and the sign is actually positive, in contrary to an ex ante expectation that the tighter mortgage regulation should have a negative effect on house values and household debt at the household level. The first two control variables show that higher education is associated with higher house values. This is also true for household age. Municipality house price growth in the previous and current period is also associated with higher house values, while dummy variables for households with secondary homes, one-person households and households with at least one unemployed adult are all negatively associated with house values.

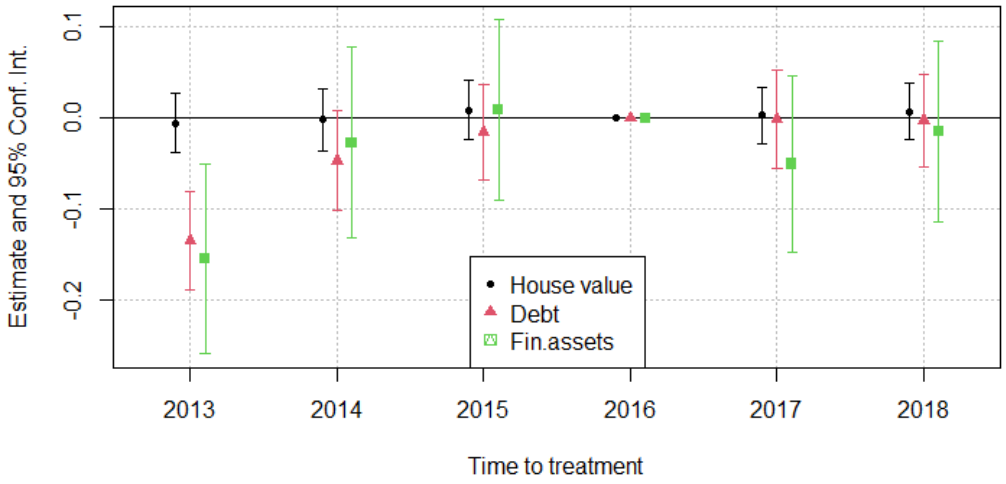
In the second column, the effect on log of household debt is presented. The coefficient on the interaction between treatment and post is negative, as expected, but still the coefficient is not statistically significant. In the third column, we see the effect on financial wealth, as a proxy for liquid assets. Also here, the coefficient is negative, but not statistically significant. In summary, there are no indications from the difference-in-differences analysis of a negative effect on house values, household debt or financial assets as a consequence of the DTI-limit.

Table (4) Baseline results dynamic difference-in-differences

	log(House values)	log(Household debt)	log(Fin. assets)
Treat x I(yr=2018)	0.007	-0.003	-0.016
	(0.016)	(0.026)	(0.051)
Edu_1	0.239***	0.457***	0.495***
	(0.005)	(0.007)	(0.012)
Edu_2	0.0356***	0.605***	0.763***
	(0.015)	(0.008)	(0.014)
Age	0.005***	-0.012***	0.022***
	(0.000)	(0.000)	(0.000)
HPgrowth	0.006***	0.002***	0.007***
	(0.001)	(0.001)	(0.001)
LagHPgrowth	0.013***	0.005***	0.007***
	(0.001)	(0.001)	(0.001)
Investor	-0.044***	0.283***	0.340***
	(0.004)	(0.008)	(0.011)
Single	-0.062***	-0.153***	-0.089***
	(0.004)	(0.007)	(0.011)
Unemployed	-0.022***	-0.016***	-0.084***
	(0.006)	(0.011)	(0.021)
Zip-code FE	✓	✓	✓
Year FE	✓	✓	✓
No.obs	96,100	96,100	96,100

Notes: This table shows the baseline results from the main difference-in-difference estimation of equation (2):
$y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI_{i,t}^{treated}} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI_{i,t}^{treated}} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI_{i,t}^{treated}} + \epsilon_{i,t}$.
$\widehat{DTI_{i,t}^{treated}}$ are households predicted to have DTI above 5 in absence of the DTI-limit. Household controls are included in $\mathbf{X_{i,t}}$; age, education (Edu_1 and Edu_2 are variables representing higher education) and the dummy variables for unemployed, secondary home owners and single households. Separate terms for year-fixed effects, δ_t, and the treatment variable are also included. *p<0.1; **p<0.05; ***p<0.01. All standard errors are clustered at the zip-code level.

Figure (4) Dynamic difference-in-differences. Baseline estimation



Notes: Estimates of β_t's in equation (2):
$y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI_{i,t}^{treated}} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI_{i,t}^{treated}} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI_{i,t}^{treated}} + \epsilon_{i,t}$.
$\widehat{DTI_{i,t}^{treated}}$ are households predicted to have DTI above 5 in absence of the DTI-limit. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners and single households. Year-fixed effects, δ_t, and the treatment variable are also included. All standard errors are clustered at the zip-code level.

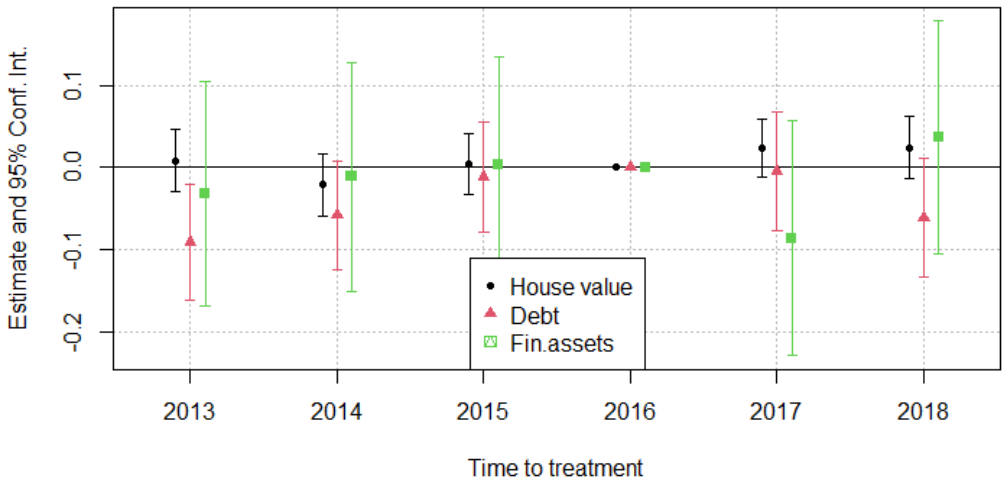
In Figure 4, we see the coefficients on all interaction terms between year and the predicted treated, from 2013 and up until 2018. We can recognise the coefficients for 2018 from Table 4, while we also see that there is no evidence of a short-term effect in 2017. The years prior to 2016 are included to support a common trend assumption and to make sure that if we had found negative, significant effects on the treatment group post regulation, this should not be present pre-regulation.

3.2 Low income households

Although there is no statistically significant effect in an estimation including the total sample of households, there could be some groups that are harder hit from the regulation than others. Groups that are often argued to be harder hit by mortgage regulations are low-income households and first-time buyers, see for example Acharya et al. (2022). Therefore, I have tested whether there could be effects of the DTI-limit on these groups that are not captured by the average effect reported in the previous subsection.

Low-income households are here defined as the 20 per cent of households with the lowest income (with a household income of about NOK 300K-500K per year). Figure 5 is equivalent to Figure 4, but including coefficients for low-income households only. We see that even for low-income households there are no clear indications of a negative effect of the regulation on house values. The effect on household debt is negative with a p-value of 10, but not significant at the 5 per cent level. While results indicate some effect on household debt, one should be careful making causal conclusions since a

Figure (5) Dynamic difference-in-differences. Low-income households



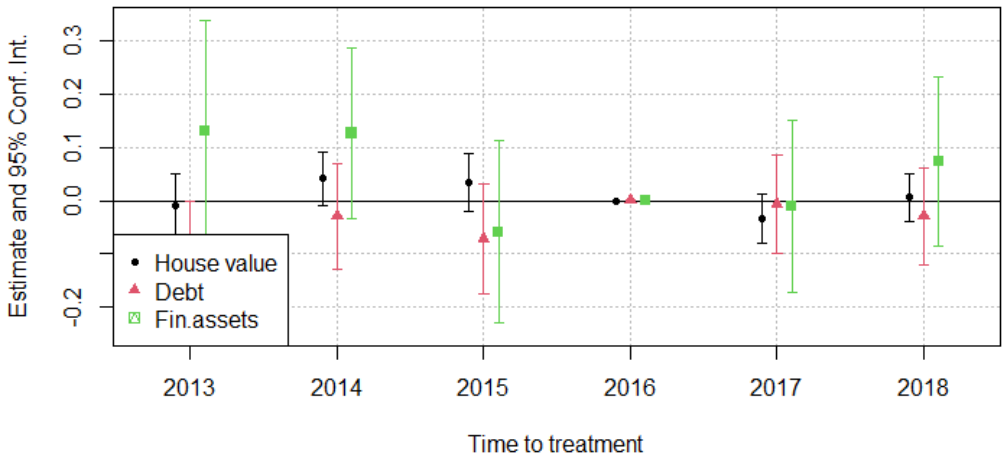
Notes: Estimates of β_t's in equation (2):
$y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI_{i,t}^{treated}} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI_{i,t}^{treated}} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI_{i,t}^{treated}} + \epsilon_{i,t}$.
$\widehat{DTI_{i,t}^{treated}}$ are households predicted to have DTI above 5 in absence of the DTI-limit. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners and single households. Year-fixed effects, δ_t, and the treatment variable are also included. All standard errors are clustered at the zip-code level.

similar effect is observed in 2013 and 2014, long before the regulation was implemented.

3.3 First time buyers

The DTI-limit might also have affected first time buyers harder than existing home owners. First time buyers are here defined as households with no housing wealth in the year prior to purchasing a house and that are below 40 years of age. There might be some households that are categorized as

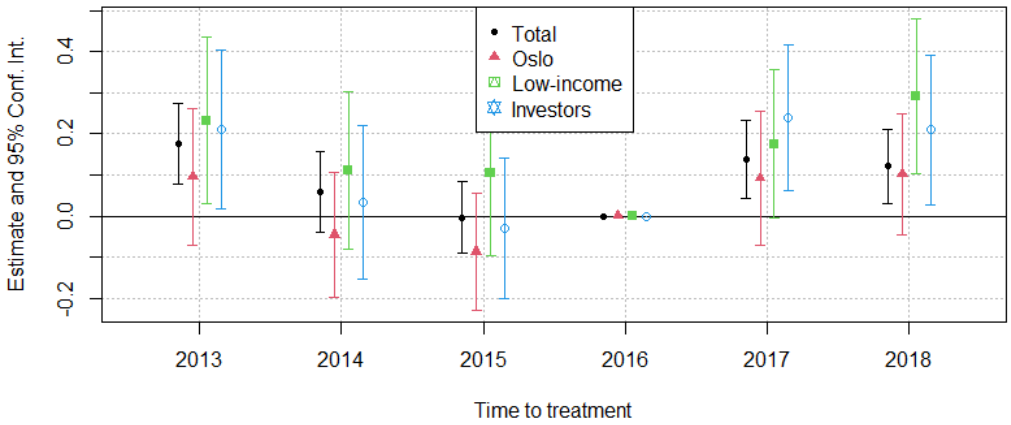
Figure (6) Dynamic difference-in-differences. First time buyers



Notes: Estimates of β_t's in equation (2):
$y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI_{i,t}^{treated}} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI_{i,t}^{treated}} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI_{i,t}^{treated}} + \epsilon_{i,t}$.
$\widehat{DTI_{i,t}^{treated}}$ are households predicted to have DTI above 5 in absence of the DTI-limit. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners and single households. Year-fixed effects, δ_t, and the treatment variable are also included. All standard errors are clustered at the zip-code level.

first time buyers here, but that in reality have been in the housing market previously and sold out due to for example moving temporarily abroad or divorcing, but the majority of households within this definition are likely to be first time buyers. For first time buyers, as for the complete sample, there is no evidence of a negative effect on house values, household debt or financial assets following the introduction of the DTI-limit, see Figure 6.

Figure (7) Dynamic DiD. Implicit interest rate



Notes: Estimates of β_t with 95 per cent CI in equation (2): $y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t DTI_{i,t}^{\widehat{treated}} + \sum_{t=2017}^{2018} \beta_t DTI_{i,t}^{\widehat{treated}} + \mathbf{b'X_{i,t}} + \delta_t + DTI_{i,t}^{\widehat{treated}} + \epsilon_{i,t}$. The treatment group is households with DTI above 5. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners and single households. Year-fixed effects, δ_t, and the treatment variable are also included. All errors are clustered at the zip-code level.

3.4 Effect on mortgage rates

Although a large share of house buyers is not compliant with the DTI-limit, one could imagine banks adapting to the limit in other ways, for example by requiring a higher price for mortgages not compliant with the regulation. In Figure 7, we see the results of a dynamic difference-in-differences estimation, in which the treated are households that actually have a DTI above five and the dependent variable is the implicit interest rate one year ahead. For all households, we see an increase in interest rates of about 0.10-0.15 percentage points and a somewhat larger increase for low-income households and households who own secondary homes. The effect is statistically significant for all groups, except in the capital, Oslo.

This could indicate that banks require a higher price for mortgages with DTI above 5 after the regulation either by requiring a higher floating rate or by requiring that households have a fixed rate on their mortgage, which at the time was higher than the floating rate. Also, an increase in consumer loan mortgages, in which interest rates are higher than mortgage rates and that was not covered by the regulation at the time, could explain parts of the increase in the average implicit interest rates.

The fact that the coefficients are positive and significant also for 2013 should make us careful concluding causally about the effect. Especially since 2013 was a period with a weak trend in house prices (as in 2017 and 2018) and one could imagine that banks require a higher price for more risky borrowers (with high DTI) in these periods. The key take-away is that we observe an increase in interest rates for households with DTI above 5 after the DTI-

limit was implemented, but it is hard to conclude that this is caused by the regulation.

3.5 Extensive margin

In addition to investigating the effect on outcome variables for house buyers- the intensive margin, there could also be extensive margin effects, meaning that some households who would have bought a house without the DTI-limit choose not to buy or are unable to.

Since this data set only covers house buyers, it is challenging to make causal conclusions about the extensive margin effects. However, a simple comparison of the number of house buyers before and after the regulation and with DTI below and above the limit of 5 does not indicate any strong extensive margin effects. The total number of house buyers is increasing, as is the number of house buyers with DTI above the limit, see Table 5. For first-time-buyers, however, there is a slight decrease in the number of house buyers with DTI above the limit. This could be an extensive margin effect, but it could also be an intensive margin effect, as some house buyers might just have reduced their DTI. Since the total number of house buyers is increasing quite a bit, the extensive margin effect is most likely not substantial. It is not possible to rule out an extensive margin effect based on this illustration of course, but it least it gives an indication that the effect is most likely modest.

Table (5) Number of house buyers below and above the limit

	2016		2017		2018	
	$DTI < 5$	$DTI > 5$	$DTI < 5$	$DTI > 5$	$DTI < 5$	$DTI > 5$
Total sample	13,480	2,932	13,592	2,994	14,297	3,200
Low-income	2,238	1,044	2,252	1,065	2,383	1,117
First-time-buyers	5,473	1,341	5,489	1,226	5,818	1,297

Notes: This table shows the number of house buyers before the implementation of the DTI-limit (2016) and after (2017 and 2018).

3.6 Discussion: Adaptation and avoidance

The most striking finding so far is that the debt-to-income limit in Norway does not seem to be binding, which may also explain the lack of effect on house values and houshold debt. There are several reasons to be surprised. First, studies from other countries, such as Acharya et al. (2022) and Peydró et al. (2020) find more clear effects of DTI-limits on house prices and household debt, especially for low-income households. Second, results from banks reported to the Norwegian Financial Supervisory Authority, Finanstilsynet (2022a), indicate a very clear bunching of DTI around five, while the tax statistics used in this article show a fat tail to the right of the DTI-distribution, also after the limit was implemented (see Figure 2).

The findings in this paper do not necessarily mean that banks are not compliant with the limit. The data used in this analysis differs from the data banks report to the FSA in several ways. This data include intermediate financing (temporary mortgages given to movers who buy before selling

in the housing market), which is taken out of the DTI-measure for banks. Households may also have some tax-free income, for example if they rent out less than 40 per cent of their own house. Besides, the data used here cover only 20 Norwegian municipalities, albeit the largest cities, not the whole country. Moreover, FSA's survey only covers a few weeks, while this data cover the whole year. Nevertheless, it is puzzling that the distribution of DTI is close to identical before and after the DTI-limit, according to the tax statistics.

Banks have a flexibility quota of 10 (8 in Oslo) per cent of the total mortgage volume that each quarter can deviate from the requirements. This quota might be large enough for the DTI-limit not to be binding. Since also refinancing of existing mortgages is included in the total volume, banks can choose to use more of the flexibility on new mortgages. Results from Finanstilsynet (2022a), however, show that a substantial fraction of the flexibility quota is also used on refinancing of loans.

One final possible explanation for the modest change in the DTI-distribution following the DTI-limit could also be that households with high DTI's have a co-borrower outside the household, for example a parent. A co-borrower will be responsible for repayment of the mortgage, together with the main-borrower, and therefore the co-borrower's income can be used in the calculation of debt to income. This would, however, not show up in the tax statistics, because the debt will not be registered for the co-borrower even though their income can be used in the calculation of DTI. According to the FSA (2021) only four per cent of house buyers had a co-borrower in

the residential mortgage survey for the fall 2021, but more than 18 per cent of first time buyers.

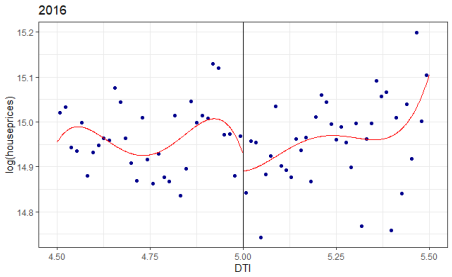
4 Robustness Checks

The difference-in-differences framework used in Section 3 relies heavily on the household model, see Appendix A, which is used to create the treatment group. In the first subsection a regression discontinuity design is used as an alternative empirical framework. In the second subsection, sensitivity checks within the difference-in-differences framework is briefly discussed.

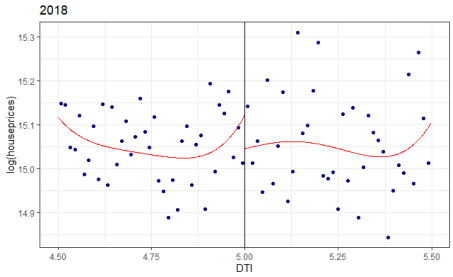
4.1 Regression discontinuity design

To inspect how households with DTI very close to the limit have reacted to the regulation, regression discontinuity plots with DTI as running variable and log of house values and household debt as dependent variables are presented in this section. The cut-off is set to 5 and the sample is limited to households with DTI between 4.5 and 5.5. The same exercise has been performed with the total sample and for wider windows of DTI such as 3-7 and 4-6 for robustness, but that does not change the main take-aways from the exercise. First, actual DTI is chosen as running variable. The advantage with using actual DTI is that it does not rely on the household model, while the limitation is of course that households will change their DTI as a consequence of the regulation. Therefore, we should be careful making causal conclusions based on this exercise. Rather, this is an attempt at inspecting whether we can see changes in behaviour close to the limit, even though we do not find

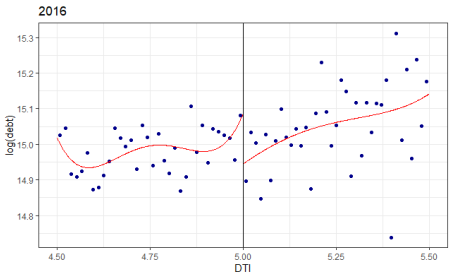
Figure (8) Regression discontinuity plots. DTI running variable



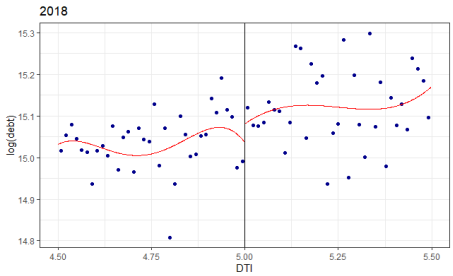
(a) House prices 2016



(b) House prices 2018



(c) Household debt 2016



(d) Household debt 2018

statistically significant effects in the difference-in-differences set-up. It is also relevant to inspect the differences-in-discontinuities, see for example Grembi et al. (2016), meaning that we can take a look at the discontinuity at DTI equal to 5 before the reform compared to after. This is what is shown in Figure 8.

The same exercise is repeated with predicted DTI as a running variable, see Figure 9. This is closer to the difference-in-differences framework, while now investigating the effect for households with DTI close to the limit only. The regression discontinuity plots corroborates the previous findings, namely that there is no significant decline in house values or household debt around the cut-off of five in 2018 after the regulation compared to 2016 before the

Figure (9) Regression discontinuity plots. Predicted DTI running variable

(a) House prices 2016

(b) House prices 2018

(c) Household debt 2016

(d) Household debt 2018

regulation. In Figure 9, Panel a and Panel b, we do see that there is a positive break around the cut-off in 2016 which is not present in 2018. This in isolation might support a negative effect on house values from the regulation, but the variation is quite large and this result is sensitive to whether we include a slightly wider window of predicted DTI. The conclusion is still that the DTI-limit seems to have had a limited effect on house values and household debt in Norway.

4.2 Other sensitivity analysis

In this section, alternative assumptions within the difference-in-differences framework are discussed. A first test is whether the results are driven by

the tails of the DTI-distribution. A way to check this is to perform the base estimation restricting the sample to households with a DTI between 3 and 7. These results are shown in Appendix B and are in line with what is found in the main analysis. A second test is if results change when using an alternative treatment group. The results with treatment defined as households with a predicted DTI between 4 and 5 are shown in Appendix C. The significant and positive effect on interest rates vanishes with the alternative treatment, while the other effects are still not statistically significant.

The regulation was announced before it was implemented, which might have changed household behavior before the limit. In Appendix D, the last quarter of the year is excluded from the sample for all years, to be certain such effects are not driving the results. These results are very similar to those in the base estimation, indicating that announcement effects are not important for the conclusions.

5 Conclusion

Residential mortgage requirements aim at reducing build-ups of financial imbalances that can arise when growth in house prices and household debt is unsustainably high. This, in turn, can reduce the risk of a severe economic crisis. In this paper, the debt-to-income (DTI) limit introduced in Norway in 2017 is analysed, applying a difference-in differences framework. The results show that there was no effect of the limit on purchase probabilities, house values, household debt or financial assets. In fact, the DTI-limit seems not to bind for many households, as a share of 18 percent of house buyers have

a DTI-level above five, also after the limit was implemented.

Implicit interest rates, however, increased for exposed house buyers after the limit, by about 0.12-0.14 percentage points on average and by more than 0.20 percentage points for low-income households and investors.

Norway was one among 26 European countries to adopt residential mortgage regulations in 2022, Finanstilsynet (2022b). For policymakers designing such tools it is of crucial importance to understand how these measures work, also if they do not work as intended. The results in this paper suggest that the Norwegian DTI-limit was not successful in reducing vulnerabilities in the household sector as it did not reduce growth in house values and debt for house buyers. One should therefore consider other measures than the DTI-limit in future design of policies, or make the DTI-limit more binding.

In future work, more data on refinancing of existing mortgages, children-parent links in the housing market and consumer loans, could contribute to increase our understanding of why the debt-to-income limit does not bind for a high share of house buyers and thereby go one step further in suggesting how design of future policies should look like.

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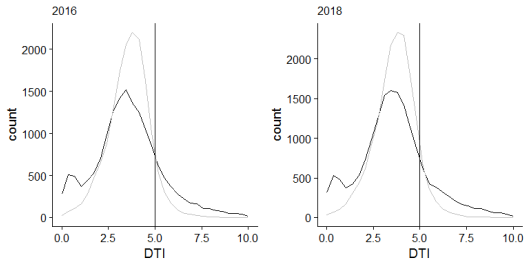
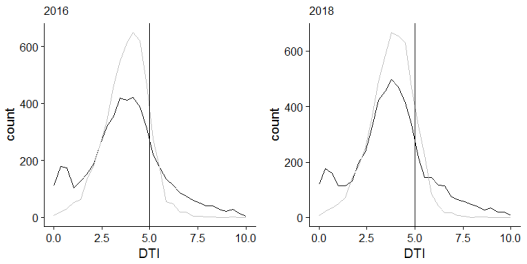
Appendix A: Household OLS-model

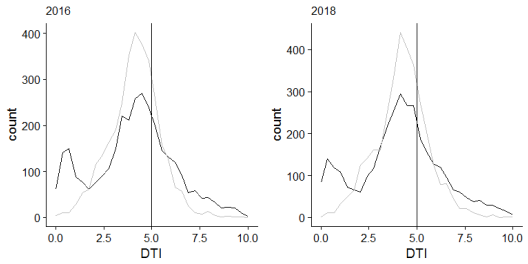
	Baseline	Extended	Oslo	Low-income	FTB	Investors
Adj. R^2	0.361	0.363	0.400	0.352	0.249	0.373
RMSE	1.45	1.45	1.97	1.76	1.56	1.85
+/- 10 percent	26	26	27	24	24	19
+/- 20 percent	47	47	47	44	45	36
+/- 30 percent	63	63	62	56	61	51



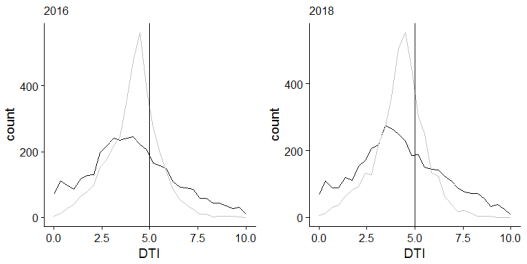
(a) Total



(b) Oslo



(c) Low-income



(d) Investors

To create a treatment group for the difference-in-differences analysis, a household model is applied, in which debt-to-income is regressed on a set of household characteristics in the year prior to implementation of the DTI-limit (2016).

The model is shown in Equation 1. Debt to income (DTI) at the house-

hold level is regressed on household age and education, zip-code, dummy variables for households with a secondary home, single households, each income quintile and households with one or more unemployed members. Also, several household balance sheet variables are included: current and lagged values of interest expenses, the value of primary home and financial wealth in addition to lagged values of income and debt.

In the table above, a few measures of goodness of fit of the household model are presented. Half of the data in 2016 is randomly chosen to make the model and the other half to test it. We can see from the adjusted R^2 that the model can not explain all variation in DTI and that the root mean squared error is relatively high for all models. For the baseline model a little less than 50 per cent of the observations have predicted DTI's that deviate no more than 20 per cent from the actual value. For the model for Oslo, low-income and investors, the goodness of fit is a little less accurate than for the baseline model.

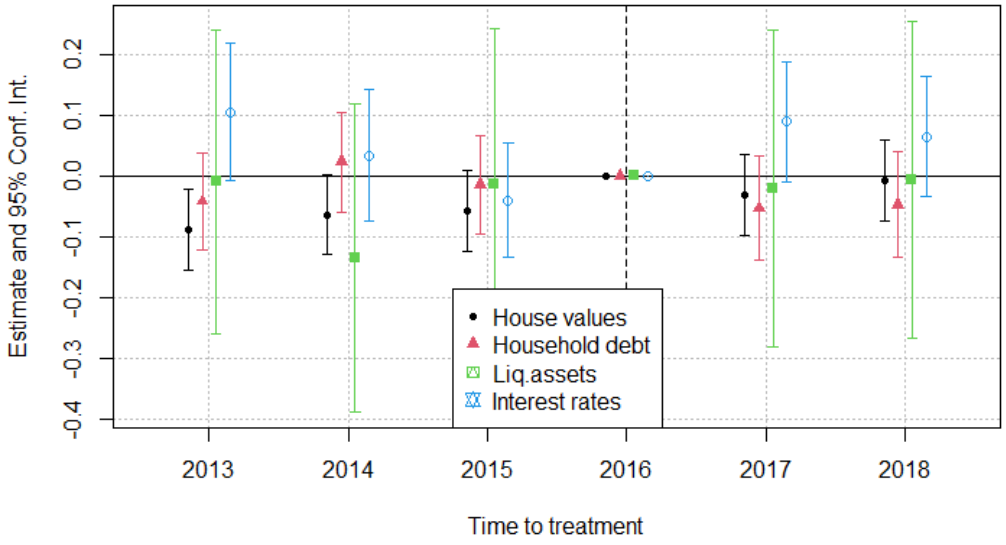
Machine learning methods such as Lasso, Random Forest and Boosting, have similar or poorer goodness of fit than the OLS-model. Neither does including more variables in the model or including squared terms of the X-variables and interaction terms of age and lagged income, debt and financial wealth (see column two) improve goodness of fit.

In the figure, the actual distribution of DTI is shown in black and the predicted distribution of DTI from the OLS-model in grey in 2016 (in-sample) to the left and in 2018 (out-of-sample) to the right to give a visual image of how well the model fits. The main limitation of the model is that it is

incapable of predicting the fat tails in the actual distribution. This is a limitation of the framework because the treatment group is in the right tail and the model underestimates the fraction of households in this group.

Appendix B: DTI between 3 and 7

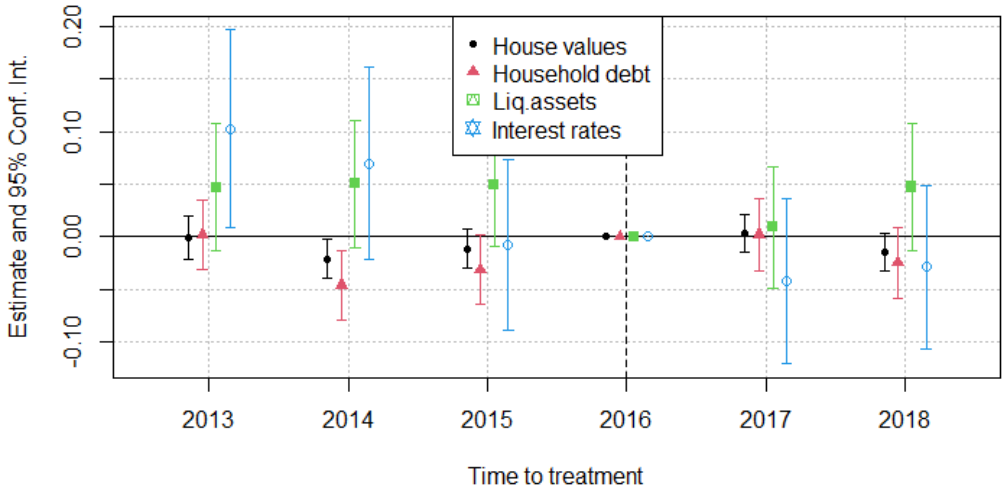
In the figure below are the results for all households with DTI between 3 and 7. The results for house values, household debt and financial assets are in line with the findings in the baseline estimation, while the effect on interest rates is no longer significant.



Notes: Estimates of β_t with 95 per cent CI in equation (2):
$y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI_{i,t}^{treated}} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI_{i,t}^{treated}} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI_{i,t}^{treated}} + \epsilon_{i,t}$.
The treatments group $\widehat{DTI_{i,t}^{treated}}$ are households predicted to have DTI above 5 in absence of the DTI-limit. I control for age, education level and zip-code. All standard errors are clustered at the zip-code level.

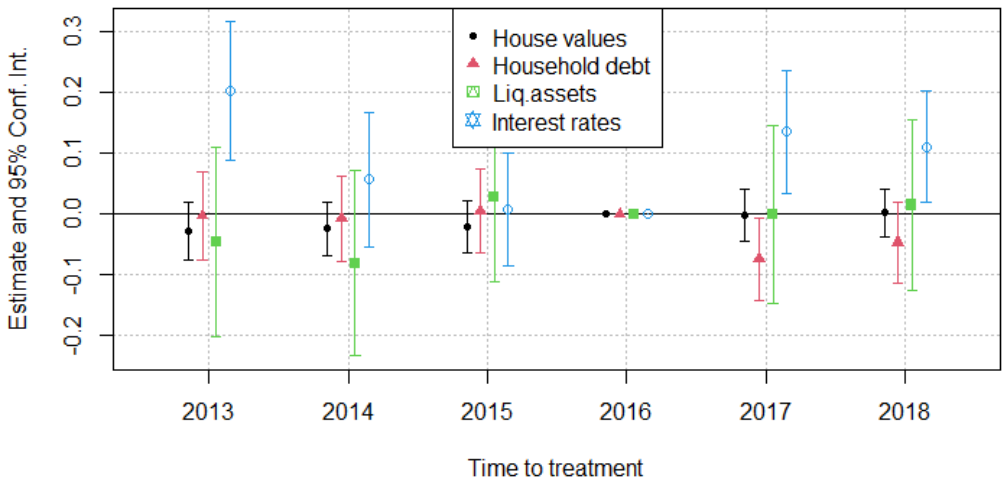
Appendix C: Alternative treatment

In the figure below are the results with the treated defined as households with predicted DTI between 4 and 5. The effects on house values, household debt and financial assets remain insignificant and the effect on interest rates vanishes.



Notes: Estimates of β_t with 95 per cent CI in equation (2): $y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI_{i,t}^{treated}} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI_{i,t}^{treated}} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI_{i,t}^{treated}} + \epsilon_{i,t}$. The treatments group $\widehat{DTI_{i,t}^{treated}}$ are households predicted to have DTI above 5 in absence of the DTI-limit. I control for age, education level and zip-code. All standard errors are clustered at the zip-code level.

Appendix D: Announcement effects



Notes: Estimates of β_t with 95 per cent CI in equation (2): $y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI_{i,t}^{treated}} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI_{i,t}^{treated}} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI_{i,t}^{treated}} + \epsilon_{i,t}$. The treatments group $\widehat{DTI_{i,t}^{treated}}$ are households predicted to have DTI above 5 in absence of the DTI-limit. I control for age, education level and zip-code. All standard errors are clustered at the zip-code level.

The DTI-limit of 5 was announced by the Ministry of Finance (MF) 14 December 2016 and implemented 1 January 2017. Hence, the period from announcement to implementation was very short and since housing transactions involve some planning, this is likely too short for households to be able to adapt prior to the implementation. However, the FSA published an advise to implement such a limit 8 September 2016 and this suggestion was debated during the fall of 2016. Therefore, one could imagine that some households aiming for a high DTI responded to this by for example buying a house before year end to be able to borrow more. To investigate this, I

have left out the last quarter of the year in each year and done a separate estimation. All results are similar to the main estimation, suggesting that announcement effects are not driving the results.