Mortgage Borrowing Limits and House Prices: Evidence from a Policy Change in Ireland

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Abstract

This paper studies how mortgage borrowers and house prices react to a tightening of mortgage limits following a policy change in Ireland in 2015. The policy introduced limits to the loan-to-income and loan-to-value ratios of new mortgages issued. In response to a tightening borrowing constraint, borrowers can choose to purchase a cheaper house or to reduce the leverage (LTV) of the mortgage. Using a difference-in-difference methodology, I find that groups of (poorer) borrowers, who were more likely to be above the loan-to-income threshold before the policy, responded primarily by buying cheaper houses after the policy change. On the other hand, groups of (richer) borrowers, who were more likely to be above the loan-to-value threshold, responded primarily by reducing the LTV of the mortgage. Borrowers who purchase cheaper houses could be buying smaller houses or the same size houses at a lower equilibrium price. To test for changes in equilibrium prices, I compare prices across postcodes and find that houses prices fell after the policy change in postcodes where a higher fraction of borrowers were above the loan-to-income threshold before the policy.

JEL codes— G21, E21, E44, E58, R21, R30

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1 Introduction

This paper studies how mortgage borrowing limits, specifically loan-to-value (LTV) and loan-to-income (LTI) limits, affect housing prices. This is a challenging question to answer because, despite observing that high house prices are associated with looser borrowing constraints, the endogenous setting of borrowing limits by banks (and choices made by households) makes it hard to disentangle the effect of borrowing limits from the impact of other shocks occurring over the business cycle. Indeed, it is plausible that prices are high *because* lending is large, or that borrowing is large *because* prices are high. This paper seeks to address this issue by examining changes in borrowing and house prices around a plausibly exogenous policy change.

The policy change occurred in 2015 in Ireland, when the Central Bank of Ireland introduced loan-to-income and loan-to-value caps on new mortgage lending. At this time, Ireland was beginning to recover from financial crises of 2008, when a house price collapse was followed by banking and sovereign debt crises. The Central Bank's stated aim was to reduce the likelihood of future house price collapses by limiting credit related house price growth. Patrick Honohan, who was Governor of the Central Bank at the time, stated that they implemented "relatively tight macro-prudential rules relating to loan-to-value and loan-to-income thresholds for mortgage lending, designed to prevent the re-emergence of bubble-like developments in the property market"(Honohan, 2019, pg.172). This type of policy has broader implications outside of Ireland as well, as the European Systemic Risk Board has encouraged other euro area countries to implement similar policies (Financial Times, 2019).

Before investigating the response to the policy, I show that the policy was both unexpected and binding. To test for expectations, I use Google Trends to show that there was no searching for the policy before the announcement, and that search increased immediately and persistently after the announcement. To test whether the policy was binding, I track the evolution of the distributions of LTI and LTV over time and compare these to the specific rules introduced: a loan-to-income cap of 3.5 for all new borrowers, and loan-to-value caps of 90% for first-time buyers, 80% for subsequent time buyers and 70% for buy-to-rent investors[1]. First, at the peak of the previous house price boom 2006, the majority of new mortgages were in excess of the borrowing limits that were not in place at that time, implying that the limits could be binding in a boom. By 2014, the year before the policy change,

[1]These numbers reflect the caps from 2017 to 2020. In the first two years of the policy 2015-2016, first-time buyers had a LTV cap of 80-90% depending on the price of the house, the details of which are outlined in Section 3 and Table 1.

39% of mortgages would not have conformed to both of the limits. In the years following, as house prices have increased, the distributions have shifted to the right and bunched at the limits: in 2018 over 20% of the mass is at the LTI limit and between 25-45% are at the respective LTV limits. The bunching at the caps, and past evidence of many high LTI and LTV mortgages, suggests that the constraints are indeed binding after the policy.

When either mortgage limit binds, I show theoretically that borrowers can adjust by either buying a cheaper house or by reducing the LTV of the mortgage. In the second case, reducing leverage requires putting a larger downpayment to replace the lower mortgage. This motivates my first set of empirical specifications, which will compare how borrowers change their purchase price and LTV in response to the policy change. When borrowers buy cheaper houses, this can be further decomposed into borrowers buying smaller houses or equilibrium prices adjusting so that the same houses are cheaper. This motivates my second set of empirical specifications, which compare how equilibrium prices and rents adjust in areas that are more or less affected by the policy.

I test the response of borrowers purchase price and LTV to the change in the limits by comparing groups of borrowers — grouped into cells based on income, age and whether they previously ever had a mortgage — that had a higher or lower proportion of the group above the new 2015 LTI or LTV limits in the year prior to the policy change in 2014. I find that cell groups that had a higher proportion of the group above the LTI threshold react by mostly reducing the purchase price of the house, and cell groups that has a higher proportion of the group above the LTV threshold react by mostly reducing the LTV of the mortgage. Estimating these changes in a difference-in-difference event study regression shows that the changes occur in the year after the policy change and are persistent through to 2018. The estimates are quantitatively large when comparing groups of borrowers near the extremes of the LTV and LTI distribution: the estimates imply a 30% lower purchase price for groups with 30% above the LTI threshold versus 0% above; and a 20% lower LTV for groups with 50% above the LTV threshold versus 0% above.

To explain these differences in responses I highlight two potential reasons. First, different types of borrowers were above the LTI and LTV limits in the pre-period. Younger borrowers with lower income and lower wealth tended to be above the LTI threshold, whereas borrowers with higher income and higher wealth tended to be above the LTV threshold. This suggests borrowers who were above the LTV threshold had the resources to lower their LTV (by placing a higher downpayment) when the policy required them to do so. On the other hand, borrowers who were above the LTI threshold may not have had the resources to substitute lower mortgage financing to a higher downpayment. Secondly, the LTI and LTV limits have

different incentives to save away from the constraint. In the case of someone constrained by the LTI limit, an additional euro of downpayment allows the borrower to purchase a house that is one euro more expensive, however in the case of someone constrained by the LTV limit, an additional euro of downpayment allows the borrower to lever up and to purchase a house that is $\frac{1}{1-LTV}$ euro higher.

To test whether borrowers buying cheaper houses is composed of borrowers choosing smaller houses or a fall in equilibrium prices, I compare equilibrium prices in geographic areas (postcodes) that had a higher or lower proportion of borrowers above the new 2015 LTI or LTV limits in the year prior to the policy change in 2014. Using a difference-in-difference specification, I find that the price-to-rent ratio fell in areas that had a higher proportion of new borrowers above the LTI threshold in the year prior to the policy, while it was largely unchanged in areas with a higher proportion above the LTV threshold, which is consistent with the results on individual borrowing. These estimates are also quantitatively large when comparing areas near the extremes of the LTV and LTI distribution: the estimates imply a 12% lower price-to-rent in areas with 30% above the LTI threshold versus 0% above; and an 8% higher price-to-rent in areas with 50% above the LTV threshold versus 0% above. Using the price-to-rent ratio partially controls for changes in prices that may come from the capitalization of rents which were rising through out this period. I also show that prices fell most in areas with a higher proportion above the LTI threshold, as well as falling to a lesser extent in areas with a higher proportion above the LTV threshold. These results suggest that borrowers purchasing cheaper houses was in part due to falls in equilibrium prices.

1.1 Contribution to the literature

This paper contributes a broad literature on housing in macroeconomics, as surveyed by Piazzesi and Schneider (2016), Guerrieri and Uhlig (2016), and Mian and Sufi (2016). Much of this literature is concerned with answering the question of how changes in credit affect house prices and how such changes interact with the macro economy. On the one hand, a number of papers provide evidence that credit played a major role in the house price boom in the United States in the early 2000s, including Landvoigt, Piazzesi and Schneider (2015), Favilukis, Ludvigson and Nieuwerburgh (2017), Greenwald (2018), and Greenwald and Guren (2019). On the other hand Kaplan, Mitman and Violante (2019) argue that expectations — and not changing credit constraints — were the primary driver of this boom. Much of the disagreement arises from the modeling choices made in each paper, such the degree to which markets are segmented into rental and owner markets or into quality tiers. This paper thus contributes to this literature by bringing new empirical evidence through which model free

inferences can be made. To do so, I use a plausibly exogenous policy change to identify the role of credit constraints and highlight important differences in how borrowers and house prices respond to different types of constraints.[2].

Second, I contribute to a growing literature that uses quasi-experimental methods to study credit in housing markets (Favara and Imbs, 2015; DeFusco and Paciorek, 2017; DeFusco, Johnson and Mondragon, 2020; Laufer and Tzur-Ilan, 2021; Chi, LaPoint and Lin, 2023). The closest paper is Acharya, Bergant, Crosignani, Eisert and McCann (2022) who study the same policy change in Ireland. They find (i) that credit is reallocated from low to high-income borrowers and from high price areas to low price areas, (ii) that house price growth slows in areas which credit is reallocated away from, and (iii) that banks take more risk in other securities and corporate credit. My paper complements this prior work by studying the differential impacts of the LTV and LTI requirements. In contrast to Acharya et al. (2022), who estimate the impact of both constraints together, I show that it is primarily the loan-to-income constraint that causes households to reduce the purchase price of their homes.[3] Likewise, I show that it is in areas that were constrained by LTI, and not by LTV, that the house price-rent ratio falls. These results are consistent with Acharya et al. (2022)'s findings that house prices growth falls in constrained areas, yet they provide evidence that it is the LTI constraint rather than the LTV constraint that drives this result. Furthermore, my results suggest that not only did house price growth slow, but the level of house price fell once they are appropriately normalized by rents, which may be increasing for other factors such as housing supply and population growth. Thus my results suggest that the policy made owning more affordable relative to renting. Overall, the main contribution of this paper is to study the differential impact of LTI and LTV constraints, which is novel relative to both Acharya et al. (2022) as well as the broader literature studying other countries.

Third, this paper relates to a broader literature on the role and impact of macroprudential policy. Several papers show circumstances under which externalities justify macroprudential policies that constrain financial markets. Lorenzoni (2007) highlights the role of macroprudential policies in the presence of credit constraints in financial contracts, Farhi and Werning (2016) show it in the presence of nominal rigidities and aggregate demand externalities. Guren and McQuade (2019) highlight that imperfections in housing market, that lead

[2]By focusing on a policy change, I also contribute to a burgeoning literature using natural experiments and causal inference methods in macro and finance (Fuchs-Schundeln and Hassan, 2016; Huber, 2018; Nakamura and Steinsson, 2018; Verner and Gyongyosi, forthcoming)

[3]Acharya et al. (2022) show that credit is allocated away from households who are "close" to the credit limit in the pre-period. They compute a normalized distance to both the LTI and LTV constraint, and use the minimum distance to either constraint.

to foreclosure externalities, can justify government action. Diamond and Kashyap (2016) summarize the literature on macroprudential polices in the banking sector.

Lastly, I contribute to a growing literature using Irish microdata to study a variety of mortgages and household finance topics (Byrne, Kelly and O'Toole, 2021; O'Malley, 2021; Acharya, Bergant, Crosignani, Eisert and McCann, 2022; Palmer, Byrne, Devine, King and McCarthy, 2022; Higgins, O'Malley and Yao, 2023).[4] Ireland is an especially interesting setting considering that it had one of the most severe economic and banking crises in response to the global finance crisis (e.g. see Lane (2012) and Honohan (2019)).

1.2 Roadmap

This paper proceeds as follows. Section 2 provides a theoretical framework for analysing the LTI and LTV limits, as well as the differences between them, and motivates the empirical analyses. Section 3 provides background to the policy change. Section 4 describes the data used. Section 5 considers whether the limits were binding by examining the evolution of the distributions of LTI and LTV before and after the policy change. Section 6 examines the impact of the policy on individual borrowing and Section 7 examines the impact of the policy on equilibrium prices in the cross section. Section 8 examines aggregate trends in house prices.

2 Theoretical framework

In this section, I use a two period model with housing, to motivate my empirical analysis and to highlight a number of differences between the LTI and LTV constraints.

Consider the problem of a household who choices to buy a house. The owner get utility from consumption of the numeraire c and housing h with first period felicity $u(c, h)$ and second period utility $v(c')$. The household maximizes utility subject to budget constraints

$$\max_{c,m,h} \quad u(c,h) + v(c') \tag{1}$$

$$s.t. \quad c + ph = w + y + m \tag{2}$$

$$c' = p'h + y' - Rm \tag{3}$$

[4] O'Malley (2021) shows that due to a legal ruling (the "Dunne judgment"), properties mortgaged before December 2009 could not be repossessed. They use this legal ruling to estimate the effect of repossession risk on default, which provides an estimate of the amount of strategic default that occurs when properties are not repossessed. Importantly, the ruling does not apply to mortgages that were issued during my sample (post-2012) and such mortgages could face repossession in the case of default. Therefore, the specific dynamics in O'Malley do not affect my estimates.

where w is wealth in the first period, y is income in a given period and R is the interest rate on the mortgage. We also impose a loan-to-income and and a loan-to-value constraint

$$\text{LTI:} \quad m \leq \psi^{lti} y \tag{4}$$

$$\text{LTV:} \quad m \leq \psi^{ltv} ph. \tag{5}$$

Rearranging the budget constraint (2) we can immediately see the relationship between the mortgage choice, the purchase price and the downpayment

$$\underbrace{m}_{\text{mortgage}} = \underbrace{ph}_{\text{purchase price}} - \underbrace{(w + y - c)}_{\text{downpayment}}. \tag{6}$$

While this identity may appear obvious, it is useful because it holds for every set of choices and so any change in the mortgage choice must be associated with a reduction in the purchase price or an increase in the downpament. Consider any change to ψ^{lti} or ψ^{ltv} in equations (4) or (5) that lowers the optimal mortgage choice. If the optimal purchase price falls proportionate to the change in the mortgage, this is associated with the same LTV (as the downpayment will also to fall for equation (6) to hold). On the other hand if the change in purchase price is less than proportionate to the change in the mortgage then the LTV will fall (as the downpayment rises). In other words, in response to a lower mortgage, consumers can respond by reducing the purchase price or the LTV. This connection motivates the empirical specifications in Section 6 on individuals response to the policy, which will estimate the change in purchase price and change in LTV for consumers following the policy change. These can be seen as estimating the impact on the purchase price choice and change in the downpayment choice relative to the purchase price choice.

From equation (6) we can also see that any change in the purchase price ph can be decomposed into changes in p and h. If consumers buy cheaper houses ph that could be because they have bought smaller houses h or because prices in equilibrium p have fallen. This motivates my analysis of equilibrium prices in Section 7, which I will compare equilibrium prices p across regions that are more or less affected by the policy,

When the LTI and LTV limits bind, they both reduce the maximum loan size, however they differ in when they bind and the incentives to save away from them the constraint. The LTI limit binds when the optimal house size is high relative to the consumers current income y, whereas the LTV limit binds when the consumers optimal house size is high relative to current cash on hand $w + y - c$.

To see the differences in incentives to save, we examine the constraints when each binds on it own. In the case where the LTI constraint is binding (so equation (4) holds with equality) and the other constraint does not bind, subbing into the budget constraint and taking the derivative of the house price with respect to savings yields

$$\frac{\partial ph}{\partial(w-c)} = 1. \tag{7}$$

This says that an extra euro of savings allows the house purchase price to increase by one euro. In the other case where the LTV constraint is binding (so equation (5) holds with equality) and the other constraint does not bind, subbing into the budget constraint and taking the derivative of the house price with respect to savings yields

$$\frac{\partial ph}{\partial(w-c)} = \frac{1}{(1-\psi^{ltv})}. \tag{8}$$

This says that an extra euro of savings allows the house purchase price to increase by $\frac{1}{(1-\psi^{ltv})}$ which is greater than one for any LTV between 0 and 1. Both LTI and LTV constrained borrowers can save to increase the house purchase, however an extra euro of savings allows the LTV constrained borrower to borrow more, whereas in the LTI constrained borrow will still not be able to borrow more. This is an important difference between the way both limits work, which may explain the different response we see in the empirical section.

This simple model does not allow for a labor supply choice, which would allow the consumer to could choose to relax the LTI constraint by working more, nor do not consider the labor supply margin in the rest of this paper for two reasons. Firstly, my data do not allow credible identification of a labor supply response. Secondly, micro estimates of labor supply elasticities (with respect to wages) are small (Chetty et al., 2011a). This is at least in part due to adjustment frictions (such as indivisibility of labor) (Chetty et al., 2011b; Chetty, 2012) and since these same frictions apply for adjustments in response borrowing constraints then it may be reasonable also expect the labor supply response to be small. This is ultimately an empirical question and it would be worthwhile for future work to investigate the labor supply response with respect to borrowing constraints.

This section used a two period model to motivate the empirical specifications used in the rest of the paper. I also highlighted the different ways the LTI and LTV limits change incentives, which may explain different responses to the policy change.

3 Background to the policy change

3.1 Historical context of the Irish housing market

Ireland experienced rapid economic growth beginning in the mid 1990s. A period known as the "Celtic Tiger" resulted in GDP growth of 50% and house price growth 220% between 2000 and the peak of the boom in 2007. Following the peak, GDP fell to a trough in 2009 that was 11.3% lower, house prices fell 54% before bottoming out in 2013, and unemployment reached a peak of 15.8% in 2012. The fall in output was approximately two and a half times as large as that in the US during the Financial Crisis and the fall in house prices 10 percentage points larger[5]. The decline in economic activity resulted in the six main banks being effectively nationalized and the government receiving financial assistance from the European Commission, European Central Bank and the International Monetary Fund. Ireland's GDP grew in every quarter except one since mid-2013 to 2018 and house prices also grew through 2013-18. This is context in which the mortgage borrowing limits were introduced in early 2015, less than two years into the recovery of house prices.

3.2 Details of mortgage borrowing limits introduced in Ireland

The Central Bank of Ireland (2015) implemented new limits on lending to Irish households, effective immediately, on February 9th 2015. The rules were announced four months prior, on October 7th 2014, in Consultation Paper No.87. I refer to these as the "implementation" and "announcement" dates respectively.

The stated aim of the Central Banks macroprudential policy is to "increase the resilience of the banking and household sectors to the property market and to reduce the risk of bank credit and housing price spirals from developing in the future"[6].

The new rules placed maximum limits on loan-to-income and loan-to-value[7] (i.e. mini-

[5]Figures are taken from FRED (2019) and the OECD (2019).

[6]The Central Bank's macroprudential framework elaborates the aims of macroprudential policy, which are to: (i) strengthen the resilience of the financial system so that it can withstand adverse movements in credit and property cycles or the impact of other economic shocks; and (ii) reduce the potential for vulnerabilities that could lead to the accumulation of financial distress. Many of the vulnerabilities arise through the pro-cyclicality of the credit cycle (Central Bank of Ireland (2016).

[7]Central Bank of Ireland (2015) defined the loan-to-income as housing loan advanced by a lender in respect of a residential property under which the total amount advanced is a multiple of the borrower's income, where income means the total gross annual income, before tax or other deductions, of the borrower. Loan-to-value ratio means the ratio of the total amounts advanced under a housing loan to the value of the residential property.

mum downpayment) ratios for all buyers, the details of which differed across buyer groups: first-time buyers, subsequent-time buyers and buy-to-rent investors[8]. For each rule banks were given allowances to exceed the cap for a fixed proportion of new lending. The following paragraphs describe the implemented rules which are also summarized in Table 1. Further details are provided in Central Bank of Ireland (2015, 2016, 2017, 2018) and Cassidy and Hallissey (2016).

Loan-to-income. For owners of primary dwellings the loan-to-income cap was set at 3.5 times the borrower's current annual before tax income. Thus, borrowers with an annual income of €100,000 could borrow up to €350,000 in a mortgage. In the case of a joint mortgage, the combined income of those taking out the mortgage is used to calculate current income.

There was no loan-to-income cap for buy-to-rent investors because these investors use rental income, rather than labor income, to repay the mortgage.

Loan-to-value. The loan-to-value caps differed for first and subsequent-time buyers. First time buyers required a 10% downpayment on the first €220,000 of the property and 20% on the remaining value of the property[9]. Effective January 1st 2017, the threshold of €220,000 was removed, such that a 90% LTV limit applied to all first time buyers. The Central Bank stated that the nominal threshold represented an effective tightening of the rules as prices increased. The Central Bank choose to remove this nominal threshold to prevent frequent re-calibration of the rules.

Exemptions. The regulation applied to new lending, however there were exemptions (i) for certain borrower types and (ii) for a fraction of each bank's lending.

The regulation exempted certain borrower types to prevent unintended side effects on those who received mortgages before the regulation. In particular, the regulation exempted borrowers who were refinancing a loan, up to the full amount outstanding, so long as the loan is secured on the same residential property. Borrowers were exempted even if the refinancing occurred at a new lender. The regulation also exempted borrowers receiving a housing loan with the purpose of addressing payment arrears by agreeing alternative repayment arrangements. These borrower type exemptions were unchanged in subsequent reviews of

[8]Subsequent-time borrowers are defined "Where the borrower under a housing loan is more than one person and a housing loan has previously been advanced to any one of those persons, none of those persons is a first-time buyer" Central Bank of Ireland (2015). Buy-to-rent investors are those who purchase a home with the intention of renting it instead of using it as their primary dwelling.

[9]€220,000 was chosen as the threshold because it was the median house price in Dublin at the time (Central Bank of Ireland, 2016)

the regulation.

In addition to exempting specific types of borrowers, the regulation allowed banks to exceed the caps for a proportion - by value - of their lending. Initially banks were allowed to exceed the loan-to-value cap in 15% and loan-to-income cap in 20% of their combined lending to first and subsequent time buyers. They were allowed to exceed the loan-to-value cap in 10% of lending to buy-to-rent investors. These bank exemptions were adapted in subsequent reviews, the details of which are presented in Table 1.

3.3 Anticipation of the policy change

This section examines the extent to which the policy change was anticipated. One could worry that the policy had been discussed in the public domain beforehand and that consumers had already reacted in anticipation of the policy. Figure 1 illustrates that this is unlikely to be the case: Google searches for terms related to the policy increased sharply upon announcement and remain persistently higher than before. The figure includes variations on the term "mortgage rules" because this is the term commonly used in the Irish media (Irish Times, 2016), and also "mortgage caps" to ensure that other variations were not used before the policy was announced.

Given that the timing of the policy change was clearly not random, one could worry that the endogenous timing of the policy could influence the results of this paper. In particular, if the policy was introduced at a time when the price-to-rent ratio would already have been expected to fall (for instance due to mean reversion when prices are high), then it would be hard to disentangle the impact of the policy change from the expected mean reversion of house prices. While this is an important caveat to this paper, this specific concern mitigated because the Central Bank explicitly timed the introduction of the rules early in the business cycle. In his book, Patrick Honohan, Governor of the Central Bank at the time, states that "The speed at which Irish housing prices bounced back from their trough in 2013 risked triggering the return of a bubble psychology in the market. If enough people believed that prices would spiral up, a price-credit-price spiral could get under way. To be sure, the total volume of lending was small - most of house purchases were now cash sales - but this was the time to put in protections." (Honohan, 2019, pp.172). This quote indicated that the Central Bank did not think that prices or credit were too high at the time of implementation, but rather the policy was implemented before prices or credit growth became too high. In other words, the policy was implemented early in a business or financial cycle, which mitigates the little worry that the policy was implemented when aggregate prices were expected to fall.

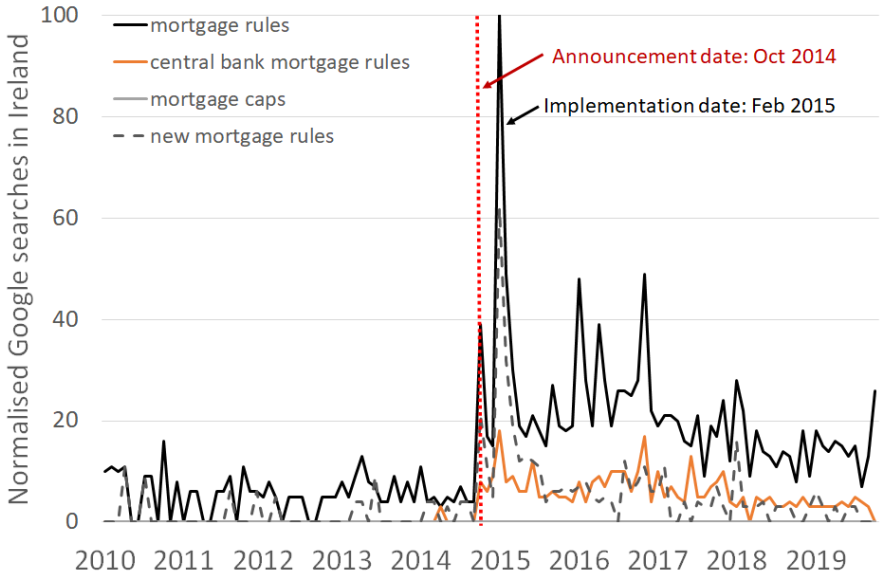
Table 1: Description of Borrowing Limits

February 2015-16	**Loan-to-value**	**Loan-to-income**
First time buyer (FTB)	Initial 200k: 90% Remaining value: 80%	3.5
Subsequent time buyer (STB)	80%	3.5
Buy-to-rent investor	70%	
Exemptions: Lending (by value) that each bank is allowed exceed limits		
First time buyer (FTB) Subsequent time buyer (STB)	15% combined for FTB and SSB	20% combined for FTB and SSB
Buy-to-rent investor	10%	
Exemptions: Borrower types exempted from limits		
	Borrowers refinancing mortgages Restructuring of mortgages in arrears	Borrowers refinancing mortgages Restructuring of mortgages in arrears

2017-18	**Loan-to-value**	**Loan-to-income**
First time buyer	90%	3.5
Subsequent time buyer	80%	3.5
Buy-to-rent investor	70%	
Exemptions: Lending (by value) that each bank is allowed exceed limits		
First time buyer (FTB)	5%	20% combined
Subsequent time buyer (STB)	20%	for FTB and SSB
Buy-to-rent investor	10%	
Exemptions: Borrower types exempted from limits		
Unchanged		

2018-19	**Loan-to-value**	**Loan-to-income**
First time buyer	90%	3.5
Subsequent time buyer	80%	3.5
Buy-to-rent investor	70%	
Exemptions: Lending (by value) that each bank is allowed		
First time buyer (FTB)	5%	20%
Subsequent time buyer (STB)	20%	10%
Buy-to-rent investor	10%	
Exemptions: Borrower types exempted from limits		
Unchanged		

Figure 1: Google searches in Ireland for terms related to the policy



Notes: Figure shows the number of (normalised) Google searches in Ireland for "mortgage rules", "Central bank mortgage rules", "mortgage caps", and "new mortgage rules". Vertical dashed lines show the announcement and implementation dates respectively. It shows that the policy introduction was unexpected, with significant public attention only occurring after the policy was announced in a consultation document was published in October 2014. Google Trends normalise the series to 100 in the month with most searches. Source: Google Trends, 2019.

4 Description of data

4.1 Price and rent data

Property data for this project come from *daft*, Ireland's largest property advertising portal. *daft* produce price and rental indices using a hedonic regression approach (daft, 2019). I use their national, county-level and postcode-level price indices[10].

4.2 Mortgage data

Mortgage data come from a loan-level mortgage dataset held at the Central Bank of Ireland. This datasets include information on: the loan (current and at origination) including origi-

[10]Ireland does not have a unique postcode definition. I define postcodes as the union of counties and Dublin postcodes. This is consistent with the definition using by An Post, the national postal service, before the introduction of the *Eircode* system in 2015. I do not use *Eircodes*, which are a finer level of aggregation, because they are not frequently recorded in my available datasets.

nation date, amount outstanding, and interest rate and contract type (e.g. fixed, variable, indexed); the borrower (at origination) including age, marital status, household income, and borrower type (i.e. first-time buyer, subsequent-time buyer or buy-to-rent investor); and the collateral (at origination) including price and geographic location. Loan level data was first collected by the Central Bank in 2012. It contains information on every mortgage on the books of all significant banks at that date and is updated quarterly ever since. It includes approximately 90% of mortgages in the Irish mortgage market and covered the five largest banks (Acharya et al., 2020).[11]

I use this loan-level dataset to construct statistics at the county and postcode level[12]. These statistics include moments of the loan-to-income and loan-to-value distribution (including mean, median and various percentiles) and the proportion of mortgages constrained by the rules.

There are two potential selection issues with these data. While most of the analysis in the paper is based on data post-2012, it is worth noting that since loan-level data was first collected in 2012, it is missing any mortgage that was issued before 2012 and no longer remained on the books of the banks in 2012. This would include mortgages issued and then repaid or removed from the banks balance sheet before 2012[13]. As a result, any statistics computed before 2012 may suffer from selection bias if a non-representative portion of the mortgages no longer remain on the banks books in 2012. As an example, for mortgages issued in 2006 we could imagine two selection biases: the best performing mortgages could have been repaid or refinanced before 2012 and thus no longer be on the books; and the worst performing mortgages could have been sold externally (to NAMA or a private institution) and thus no longer remaining on the books. Given both examples, it is hard to sign the direction of the selection bias.

The second source of selection arises because only two banks provide geographical data at the postcode level (all banks provide county level geographical data). Using only county

[11]With only five main banks, the Irish banking sector is relatively concentrated. (Acharya et al., 2020) leverage cross bank exposure to the rules to show that banks that were more exposed to high-leverage mortgages reallocate credit to the conforming sector. In contrast, this paper focuses on the response of households and thus tracks households over time based on their pre-policy exposure to the limits. This approach is consistent with the simple model in section 2. In equilibrium it will also be consistent with the actions of banks and thus the results of (Acharya et al., 2020).

[12]As noted previously, I define postcodes as the union of counties and Dublin postcode.

[13]Unlike in the US, Irish mortgages typically remain on the balance sheet of banks. However, during the period post-2008 there were some mortgages sold to other entities, including the National Asset Management Agency (NAMA), a government financed agency responsible for purchasing poorly performing assets from the Irish banks.

data is problematic because, Dublin is classified as a single county but represents a significant fraction of mortgage issuance. I therefore use postcode data, even though it is subject to selection issues, to incorporate additional within-Dublin variation. Future work will test the consistency of results at the county and postcode level.

4.3 Household data

Household data comes from the European Central Bank's Household Finance and Consumption dataset. I use the Ireland sample, which contains 5,419 observations surveyed between March and September 2013. This dataset includes information on individuals income, assets and consumption, which I use to construct a measure of income, housing wealth (for the primary home), mortgage outstanding and other assets for each individual.

5 Distributions of LTI and LTV before and after the policy

This section looks at how the aggregate distribution changed around the time of the policy change, with the aim of answering whether the new caps introduced we're binding. Prior to introduction of loan-to-income and loan-to-value caps, there were no legal limits of this kind on lending for banks. Banks typically placed their own limits on individual borrowers, but these varied across banks and over the business cycle, so this section investigates whether the policy was binding on these banks.

5.1 Data construction

This section uses the mortgagee data described in Section 4. The histograms are weighted by the euro value of the loan at origination.

5.2 Results

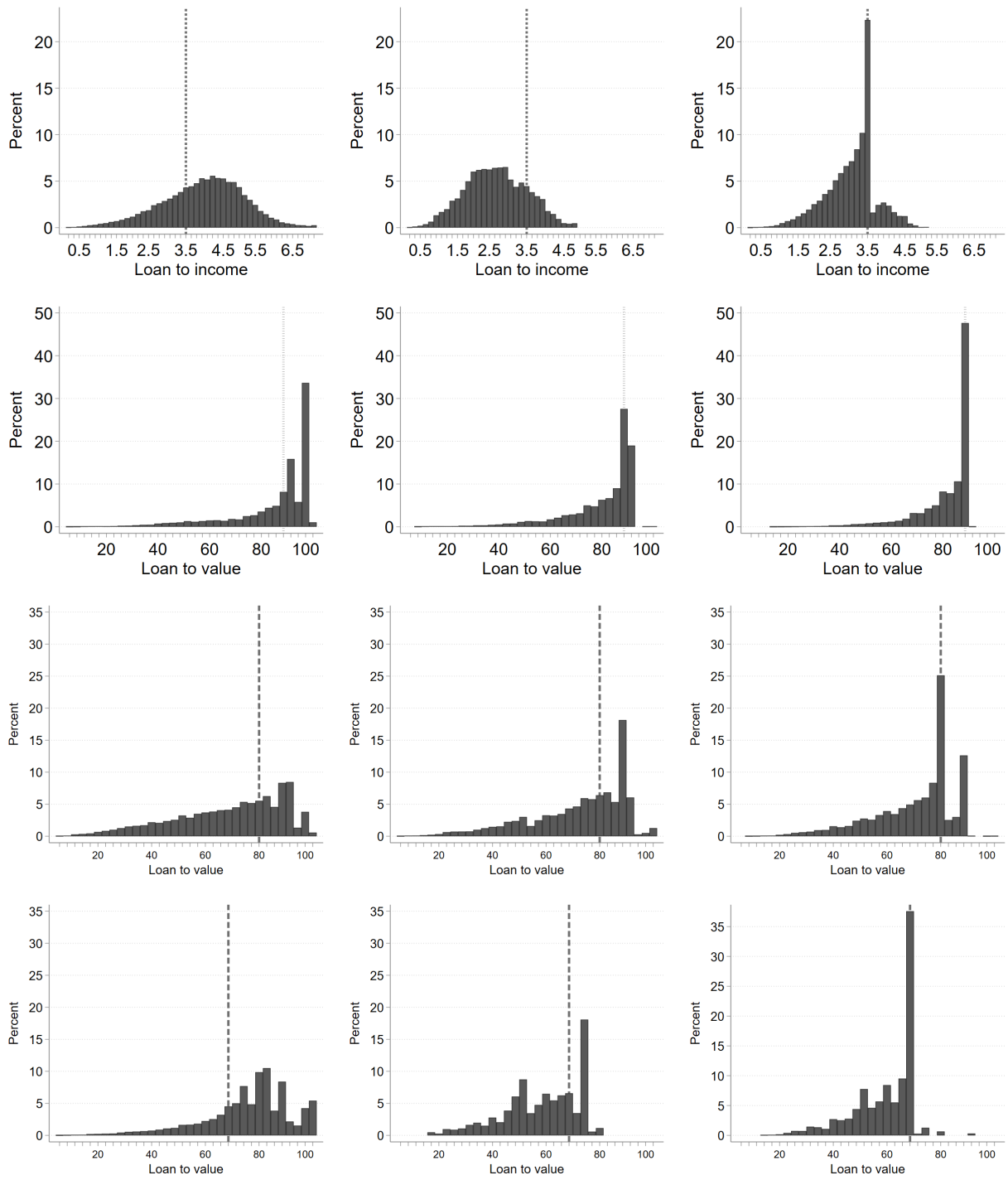
Figure 2 presents the distribution of mortgages by loan-to-income and loan-to-value for three dates: the peak of the previous boom (2006); the year prior to the introduction of the mortgage caps (2014); and the most recent year of data (2018). These charts illustrate that many of the loans issued in the previous boom would have breached the recent caps, and that the distribution is now bunched up against the cap. The majority of loans in the early stage of the past boom 2000-03 did not breach the LTI cap - see appendix **??** for each of these distributions for 2000-2018 - however by 2006 the majority of new mortgages were in

excess of the 3.5 level. The new loans did not appear to be binding at their introduction in 2014, however by 2018 they appear to be binding for a considerable fraction — with 22% sitting at exactly 3.5 and another 25% above 3.

Figure 11 (in Appendix **??**) plots the value of mortgages issued from 2000 to 2018 categorized by whether it would have conformed to the rules imposed in 2015. It is clear that the constraint would have been binding. In the two years prior to the announcement 39% of mortgages would not have conformed to the rules imposed. This proportion decreased in the years after implementation, albeit remaining above zero due to the exemptions given to the banks.

In addition to binding on impact, the constraints would have been binding had they been imposed at the peak of the previous boom: 71% of mortgage issued in 2006 and 2007 would not have conformed to the limit. There is one important caveat to looking at historical mortgages: the mortgage database only includes mortgages that remained on the books of the banks in 2012, the time at which the data was first collected. Thus mortgages issued and repaid before 2012 do not appear in the data. This was discussed in further detail in Section 4. With this caveat in mind, there is still a substantial volume of mortgages issued in 2006 and 2007 that were in excess of the later 2015 caps.

Figure 2: Distribution of mortgages originated: LTI for FTB and SSB combined (top panel); LTV for FTB (upper middle), SSB (lower middle) and BTR (bottom)



(a) 2006 (b) 2014 (c) 2018

Notes: Figure shows distribution mortgages originated based on loan-to-value (LTV) and loan-to-income (LTI) at the peak of the previous boom (2006), in the year before the mortgage rules were introduced (2014), and in the most recent year (2018). The top panel shows the loan-to-income distribution for first time buyers (FTB) and second or subsequent time buyers (SSB) combined because the 3.5 cap applied to both groups — as illustrated by vertical line. The upper middle, lower middle and bottom panels show the loan-to-value distribution for FTB, SSB and buy-to-rent (BTR) buyers respectively. The vertical lines represent the caps of 90%, 80% and 70% LTV for each of these groups respectively. Histograms are weighted by the euro value of the loan at origination.

6 Impact of the policy on individual borrowing

The objective of this section is to compare the borrowing of individuals before and after the policy. However, I do not have a panel dataset, both because the data on individual mortgages do not contain individual borrower id's and because borrowers typically get new mortgage infrequently. To overcome this constraint, I create a panel of groups of similar borrowers. I use this constructed panel to answer (i) what groups were above the threshold in the pre-period, and (ii) how did the choices — of purchase price and leverage — of these groups change following the policy.

6.1 Data construction

I construct the panel of cells using the mortgage data described in section 4.2. I create cells based 7 age bins (<30, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59, $\geq$ 60), the 10 income deciles of the mortgage holders in 2014, 2 buyer types (first time buyers and second time buyers) and 7 years of data (3 pre and 4 post-policy). Not every cell is populated in every year, resulting in 927 cell-year observations. I exclude buy-to-rent mortgages in this section because the loan-to-income constraint was not applicable to this group. I exclude one large bank (X% of the sample) because this bank did not report income data before the policy change.

I compute the mean purchase price, mortgage balance, mortgage deposit, loan-to-income ratio, loan-to-value ratio, and borrowers income of new mortgages issued to borrowers within each cell in each year. Ideally, we would could define a *treated* cell if it was above either the LTI or LTV threshold in the pre-period, however when we compute the average LTI or LTV of the cells we rarely observe cells in which the mean of the cell is above the threshold. Instead, I also compute the percentage of borrowers in each cell in 2014 (the year prior to the policy) who are above the LTI and LTV thresholds that were implemented in 2015. This is a measure of how many individuals within the cell in 2014 would have been constrained if the policy had been implemented one year prior. It is an attractive measure of the *treatment intensity* for each cell because it is a continuous measure of treatment, and is positively correlated with alternative measures such as the mean LTI/LTV of the cell or the average distance of the cell to the threshold. Relative to these other measures of treatment intensity, the percentage above the threshold has a clear interpretation as the impact from increasing the proportion of potentially constrained individuals by 1 percentage point.

The size of each cell varies so I weight all results in the following section by the cell size

in 2014. This is in line with the grouping estimators that were similarly used for estimating the impact of tax policy changes in datasets with repeated crosssectional data Blundell et al. (1998). I hold the size of the cell fixed to its pre-policy value so that I estimate the change in borrowing conditional on the relative size of the group not changing.

The mortgage data includes information on age and income however it does not include information on assets. To investigate differences in assets, I merge the average net wealth of each cell group using the HFCS data described in section 4.3. There is a one year difference between the HFCS data (2013) and the pre-policy mortgage data (2014). This is the only wave of the HFCS data collected in Ireland before the policy change in 2015. These data are merged using the same cell definitions as the mortgage data.

In the rest of this section, I use this panel to ask (i) what groups had the most people above the threshold in the pre-period, and (ii) how did the choices — of purchase price and leverage — change after the policy change.

6.2 Relationship between cell characteristics and the percentage of the cell above the LTI and LTV thresholds before the policy

Figure 3 plots the relationship between the characteristics of each cell — income, age, wealth — and the percentage of people in each cell who are above the loan-to-income (panel 3a) threshold and loan-to-value threshold (panel 3b). Each cicle represents a cell and the size of the circle represents the number of people in the cell. The line is predicted from a weighted linear regression.

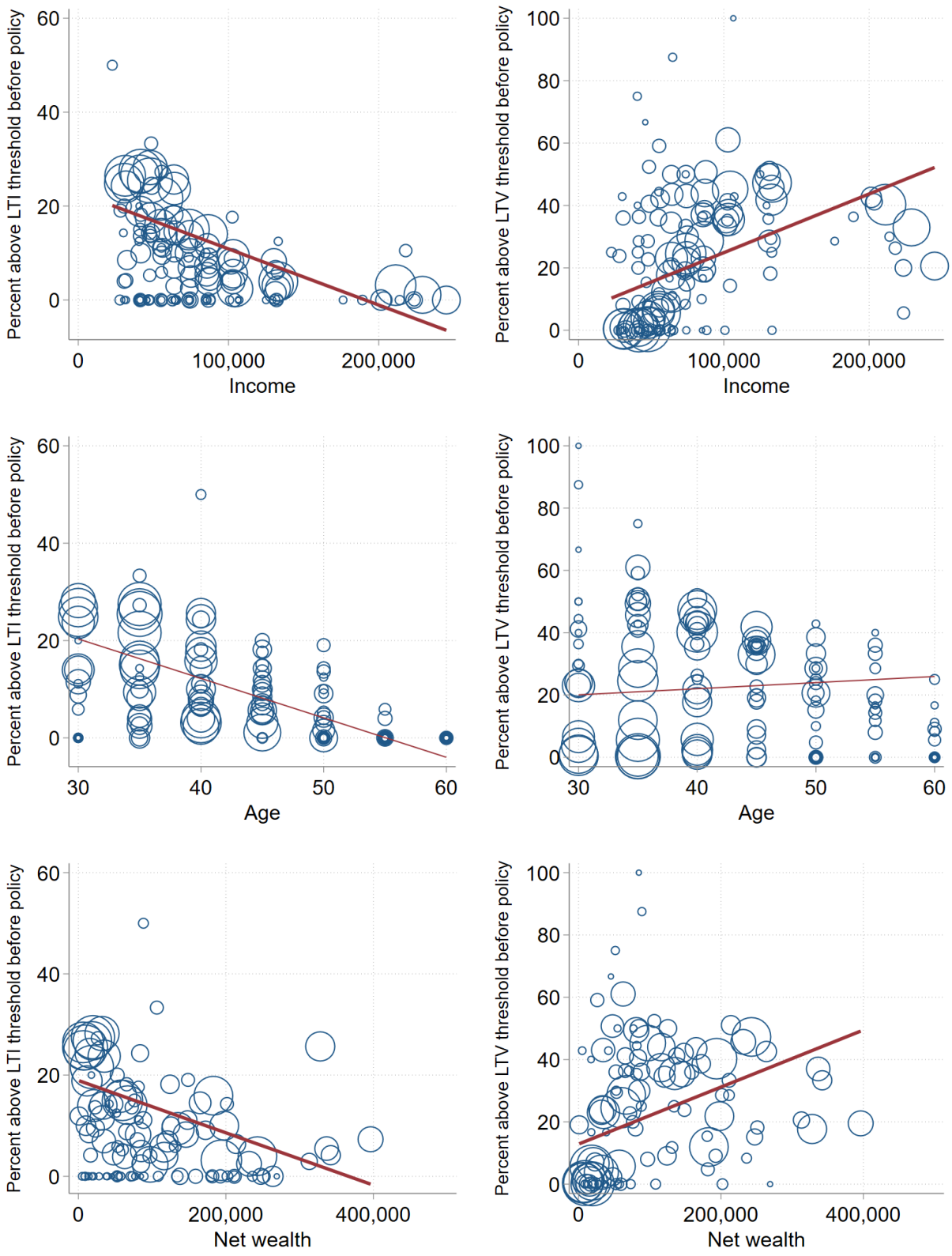
Panel 3a shows cell groups which had a larger fraction of people above the LTI threshold in 2014 were younger, lower income and less wealthy. Conversely, cell groups which had a larger fraction of people above the LTV threshold in 2014 were higher income and wealthier. There is little variation in age among the high and low LTV cell groups. This shows that the each constraint was likely to be binding on different groups of individuals and that these groups have different resources available to reach to the constraints. These differences may explain the different responses to the policy changes, which will be shown in section 6.3.

It is worth noting the spread across cell groups of the percent above the LTI threshold and percent above the LTV threshold variables. For the LTI variable, most groups vary between 0% and 30% whereas the spread is between 0% and 50% for the LTV variable. When interpreting the later estimates in this section, I will compare the estimated change between groups at these bounds on the LTI and LTV variables.

Figure 3: Relationship between cell characteristics and the percentage of the cell above the LTI and LTV thresholds before the policy

(a) By percent above LTI threshold

(b) By percent above LTV threshold



Note: Figure plots the relationship between the proportion of borrowers in cell groups that are above the LTI and LTV thresholds and the cell characteristics before the policy (income, age, net wealth). The size of the circle represents the number of borrowers in each cell group. The fitted line is from a weighted linear regression. The x-axis on income and net wealth are truncated to at €250,000 and €400,000 to make the visualization clearer.

6.3 How did choices change after the policy

This section examines how choices of purchase prices and leverage changed after the policy change. I compare these changes in choices across cell groups based on the whether they are high on the LTI and LTV threshold. I will first show scatter plots of changes in the year before and after the policy, where we can see the changes of every cell group. Then, I will formally estimate the change in slope over several years using a difference in difference event study regression.

6.3.1 Non-parametric results

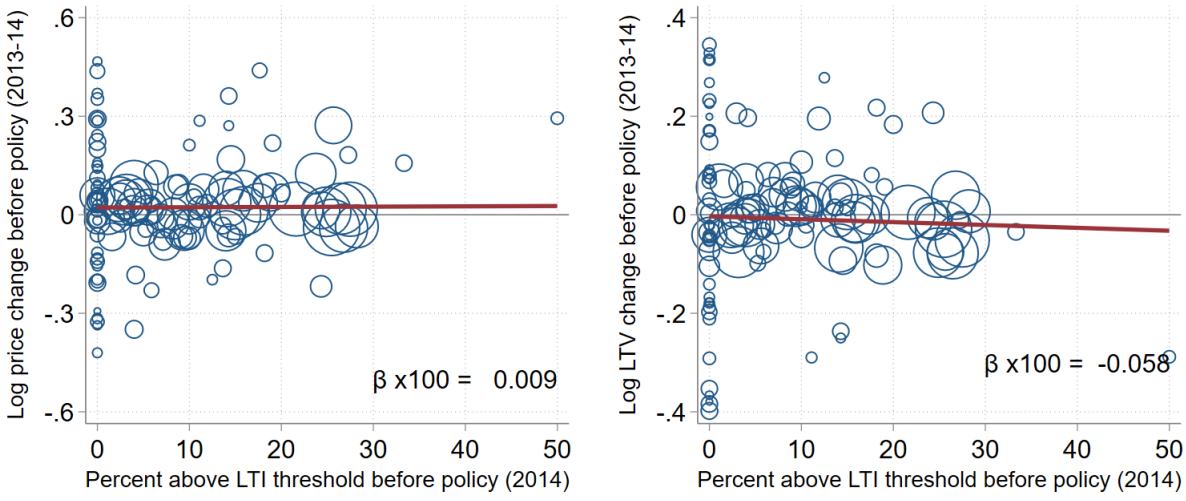
Figures 4 and 5 show the change in purchase price (left-hand panels) and leverage (right hand panels) by LTI and LTV respectively. The change in purchase price or leverage in the year prior to the policy did not vary by cell groups with many or few above the LTI threshold (panel 4a). This pattern changes in the year after the policy change (panel 4b) — the low LTI groups increase their purchase price while high groups do not. The largest cells range between 0% and 30% above the LTI threshold in 2014, so I compare the changes between the endpoints of this range. The slope of the regression estimate (-0.0057) implies that the cell groups with 0% above the LTI threshold increased their price by 17 percentage points more than cell groups with 30% above the LTI threshold. The pattern for leverage also changed — the low LTI groups decrease their leverage in the year following the policy while the high LTI groups leverage increases slightly. The slope of the regression estimate (0.00365) implies that the cell groups with 0% above the LTI threshold decreased their leverage by 11 percentage points more than cell groups with 30% above the LTI threshold.

Figure 5 shows the same change in purchase price (left-hand panels) and leverage (right hand panels), however in this case I plot the changes against LTV in the pre-period. Again, the change in purchase price or leverage in the year prior to the policy did not vary by cell groups with many or few above the LTV threshold (panel 5a). This pattern also changes, in the opposite directions to LTI, in the year after the policy change (panel 5b) — the high LTV groups increase their purchase price while low groups do not. The largest cells range approximately between 0% and 50% above the LTV threshold in 2014, so I compare the changes between the endpoints of this range. The slope of the regression estimate (0.0027) implies that cell groups with 50% above the LTV threshold increased their price by 14 percentage points more than cell groups with 0% above the LTV threshold. The pattern for leverage also changed in the opposite direction than LTI groups — the high LTV groups decrease their leverage in the year following the policy while the low LTV groups leverage

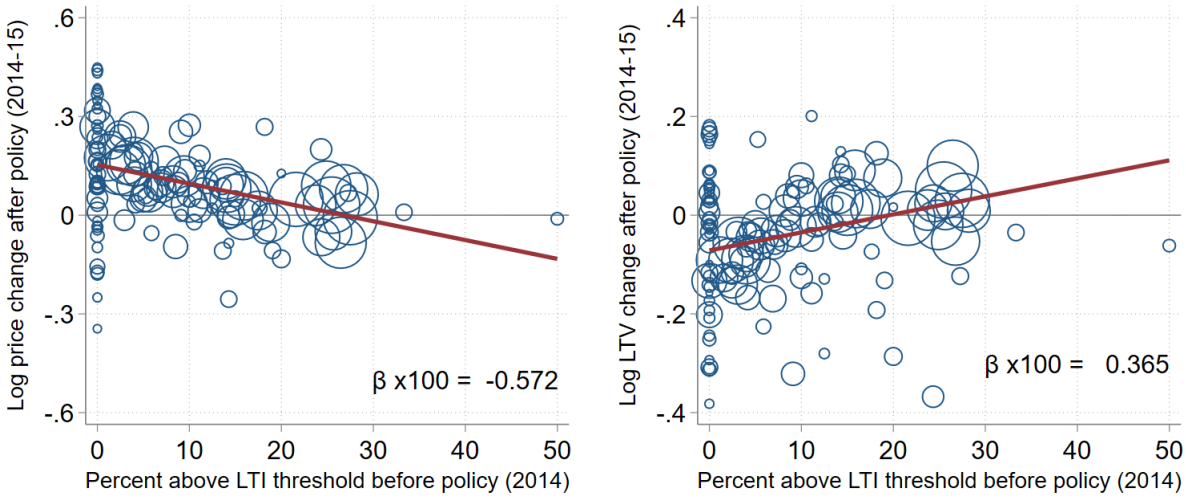
increases slightly. The slope of the regression estimate (0.00365) implies that the cell groups with 50% above the LTV threshold decreased their leverage by 18 percentage points more than cell groups with 0% above the LTV threshold.

These plots show that in the year prior to the policy, changes in purchase price and leverage are uncorrelated with the proportion of cells that are above the LTI or LTV thresholds. In the year after the policy, this pattern changes in different directions — high LTI groups adjust by decreasing purchase price and increasing leverage while high LTV groups adjust by decreasing leverage and increasing purchase price. These scatter plots are a useful way of showing how choices change because we can observe the full distribution of changes in the year before and after. However, since there are two treatments — high LTI and high LTV groups — I want to test the impact of each policy while controlling for the other. I use a difference-in-difference event study regression to do so. This regression will also allow me to test whether the change in slope is statistically significant, whether there were pre-trends in years other than the year before, and whether the change in slope persisted at different horizons.

Figure 4: Change in purchase price (left) and leverage (right) by loan-to-income



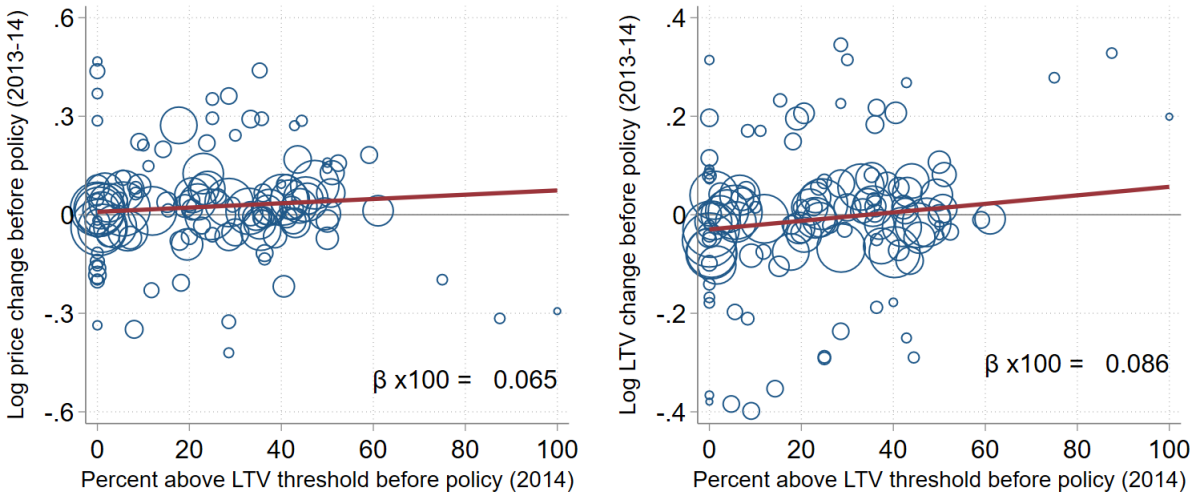
(a) Change in year before policy (2013-14)



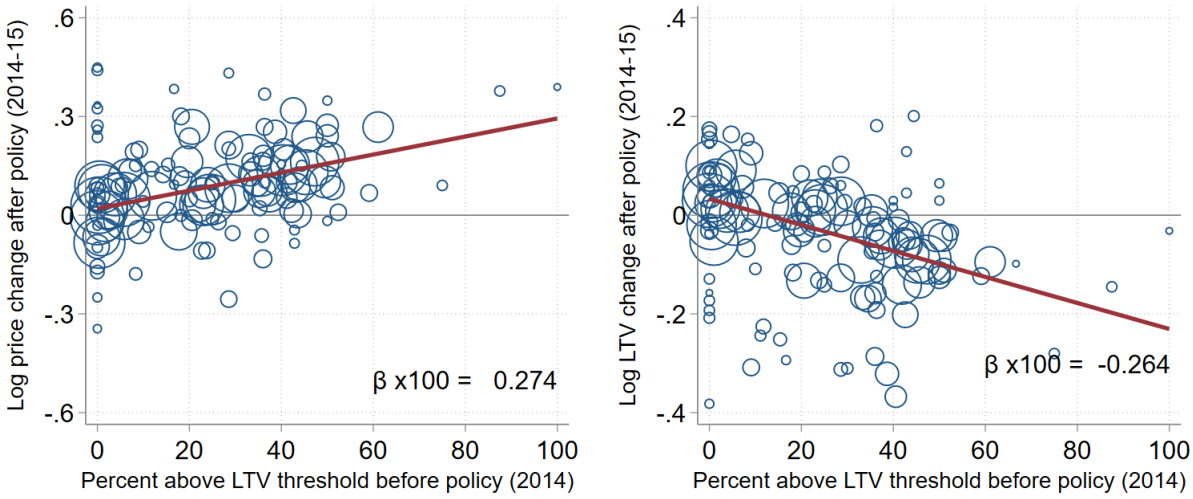
(b) Change in year after policy (2014-15)

Note: Figure plots the relationship between the proportion of borrowers in cell groups that are above the LTI thresholds and changes in log purchase price and log LTV in the year before and after the policy change. The size of the circle represents the number of borrowers in each cell group in 2014. The fitted line is from a weighted linear regression.

Figure 5: Change in purchase price (left) and leverage (right) by loan-to-value



(a) Change in year before policy (2013-14)



(b) Change in year after policy (2014-15)

Note: Figure plots the relationship between the proportion of borrowers in cell groups that are above the LTV thresholds and changes in log purchase price and log LTV in the year before and after the policy change. The size of the circle represents the number of borrowers in each cell group in 2014. The fitted line is from a weighted linear regression.

6.3.2 Difference-in-difference results

In this section, I parametrically test the size and persistence of the change in purchase price and leverage shown in Figures 4 and 5 using a difference-in-difference event study regression. For a given outcome variable Y_{it}, the difference in difference regression takes the following

form

$$
\begin{aligned}
Y_{it} \quad = \quad & \sum_{k \neq 2014} \delta_k^{LTI} [\%\text{ above LTI threshold}_i^{2014} \times \mathbb{1}_{t=k}] + \\
& \sum_{k \neq 2014} \delta_k^{LTV} [\%\text{ above LTV threshold}_i^{2014} \times \mathbb{1}_{t=k}] + \boldsymbol{\tau_t} + \boldsymbol{\gamma_i} + \epsilon_{it} \qquad (9)
\end{aligned}
$$

where $\boldsymbol{\tau_t}$ are year fixed effects, $\boldsymbol{\gamma_i}$ are cell fixed effects, and ϵ_{it} is the error term. The variables % above LTI threshold$_i^{2014}$ and % above LTV threshold$_i^{2014}$ are the proportion of individuals in cell i in 2014, the year before the policy, that are above the LTI and LTV threshold. The impact of having one percentage point more of the cell above the LTI threshold δ_k^{LTI} and LTV threshold δ_k^{LTV} are both allowed to be time varying with δ_k^{LTI} δ_k^{LTV} denoting the year-by-year impact of the being relatively constrained.

In this specification the δ_k^{LTV} and δ_k^{LTV}, can be interpreted as the relative impact of having one percentage point higher proportion of the cell above the relative threshold, conditional proportion of the cell above the other threshold, in each year.

I omit δ_{2014}^{LTI} and δ_{2014}^{LTV} so the other δ_k^{LTI}'s and δ_k^{LTV}'s can be interpreted as relative to the pre-policy baseline period. It is worth noting that the difference-in-difference estimates do not necessarily measure the aggregate impact of the policy change as they difference out general equilibrium effects (Auclert et al., 2019; Chodorow-Reich, 2020).

Regression results are weighted by the cell size in 2014. Standard errors are clustered at the cell level to account for within cell group correlation between the treatment exposure variable and unobservable shocks.

The identifying assumption for my difference-in-difference specification is that in the absence of the policy change, the choices for cell-groups with more or less people above the LTV or LTI threshold would have evolved in parallel. This parallel trends assumption implies there are no other reasons why the cell groups would act differently in the absence of the policy change.

The main regression specification includes both % above LTI threshold$_i^{2014}$ and % above LTV threshold$_i^{2}$ in the same regression. This means the estimates for the impact of one policy are conditional on the impact of the other policy. Since both policies came into effect at the same time, failure to include both the LTV and LTI measures could lead to omitted variable bias due to the correlation of both measures. Figure 3 shows that high income groups tend to have a larger proportion above the LTV threshold and a smaller proportion abover the LTI threshold.

If the LTI policy change induced groups with a higher proportion to reduce their purchase price, this could show up as a negative relationship with LTI and a positive relationship with LTV after the policy change (as seen in Figures 4 and 5). Thus, estimating conditional impacts is an advantage of the difference-in-difference framework relative to the scatter plots in Figures 4 and 5 where we only observe unconditional changes. For robustness, I also estimate each impact separately as in the following specifications and I report the results in Appendix C.[14]

Figure 6 plots the estimated δ_k^{LTI} and δ_k^{LTV} coefficients from the main difference-in-difference specification in 9. The outcome variables are log purchase price and log LTV because, as discussed in section 2, constrained borrowers can adjust on either of these dimensions.

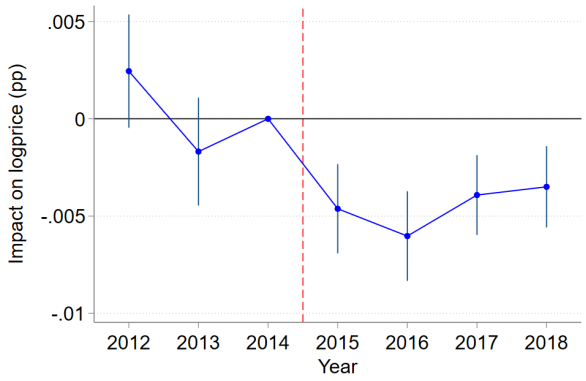
[14]The regression equations for this robustness are

$$Y_{it} = \sum_{k \neq 2014} \delta_k^{LTI} [\% \text{ above LTI threshold}_i^{2014} \times \mathbb{1}_{t=k}] + \boldsymbol{\tau_t} + \boldsymbol{\gamma_i} + \epsilon_{it} \quad (10)$$

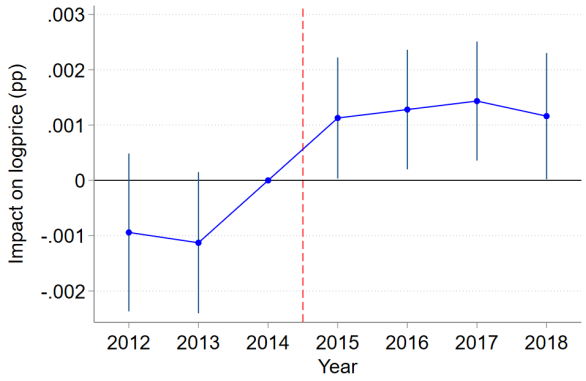
$$Y_{it} = \sum_{k \neq 2014} \delta_k^{LTV} [\% \text{ above LTV threshold}_i^{2014} \times \mathbb{1}_{t=k}] + \boldsymbol{\tau_t} + \boldsymbol{\gamma_i} + \epsilon_{it} \quad (11)$$

Figure 6: Difference-in-difference estimates

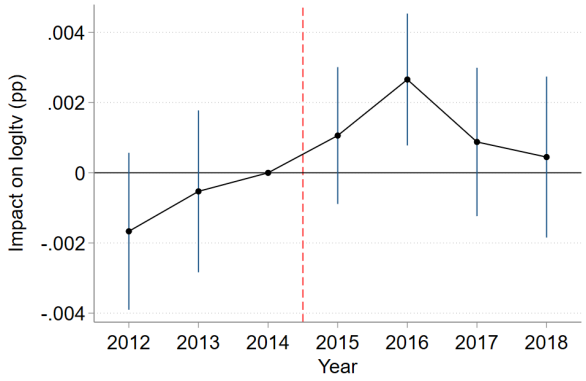
(a) Impact of LTI treatment on log purchase price, δ_k^{LTI}



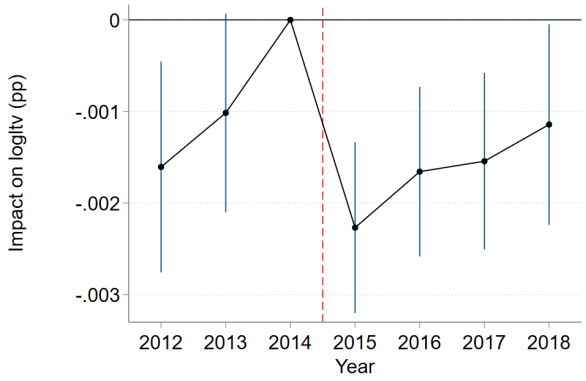
(b) Impact of LTV treatment on log purchase price, δ_k^{LTV}



(c) Impact of LTI treatment on log ltv, δ_k^{LTI}



(d) Impact of LTV treatment on log ltv, δ_k^{LTV}



Note: Regressions are weighted by cell size in the pre-treatment year (2014). Standard errors are clustered at the cell level.

The top panels of Figure 6 plot the year-by-year impacts on log purchase price of LTI, δ_k^{LTI}, (panel 6a) and of LTV, δ_k^{LTV}, (panel 6b). The bottom panels of Figure 6 plot the year-by-year impacts on log ltv of LTI, δ_k^{LTI}, (panel 6c) and of LTV, δ_k^{LTV}, (panel 6d). The numbers underlying Figure 6 are reported in Table 2 in Appendix C.

Panel 6a plots the year-by-year impacts on log purchase price of the LTI treatment intensity, δ_k^{LTV}. The coefficient on the pre-policy years 2012 and 2013 are not statistically significant from zero (at the 95% confidence level) and present no clear trend. The coefficient in the year after the policy is 0.005, implying cell groups with one percentage point higher share above the LTI threshold decreased their purchase price by 0.5%. The coefficients are persistently negative in all years after the policy change.

Panel 6b plots the year-by-year impacts on log purchase price of the LTV treatment intensity, δ_k^{LTI}. The coefficient on the pre-policy years 2012 and 2013 are not statistically significant from zero (at the 95% confidence level) however there is an upward trend starting in 2013 and continuing to 2015. The coefficient in the year after the policy is 0.001, implying cell groups with one percentage point higher share above the LTV threshold increased their purchase price by 0.1%. The coefficients are persistently positive in all years after the policy change.

Panel 6c plots the year-by-year impacts on log LTV of the LTI treatment intensity, δ_k^{LTV}. The coefficient on the pre-policy years 2012 and 2013 are not statistically significant from zero (at the 95% confidence level) however there is an upward trend starting in 2012 and continuing until 2016. The coefficient in the year after the policy is 0.001, implying cell groups with one percentage point higher share above the LTI threshold increased their LTV by 0.1%. The coefficients are only significantly different from zero in one post-policy year, 2016.

Panel 6d plots the year-by-year impacts on log LTV of the LTV treatment intensity, δ_k^{LTI}. The coefficient on the pre-policy years 2012 and 2013 are not statistically significant from zero (at the 95% confidence level) and upward trend starting in 2013 and continuing to 2015. The coefficient in the year after the policy is -0.002, implying cell groups with one percentage point higher share above the LTI threshold increased their purchase price by 0.2%. The coefficients are negative in all years after the policy change.

Interpreting the results of Figure 6 together, it appears that cell groups constrained by the LTI cap — those with a larger fraction of above the LTI threshold in the pre-period — adjusted their borrowing by primarily reducing their purchase price. This is seen by the immediate and persistent negative impact on log purchase price and no significant change in the trend for log LTV. On the other hand cell groups constrained by the LTV cap — those with a larger fraction of above the LTV threshold in the pre-period — adjusted their borrowing reducing their leverage. This is seen by the immediate and persistent negative impact on log LTV and no clear change in trend in the impact on log price.

Magnitude of these changes are economically large. As noted in Figure 3, the percent above LTI threshold variable primarily varies between 0% and 30% while the percent above the LTV threshold variable primarily varies between 0% and 50%. Comparing the endpoints of these ranges, I find that cell groups with 30% above the LTI threshold reduce their purchase price by 15% percent relative to cell groups with 0% above the same threshold. Cell groups with 50% above the LTV threshold reduce their LTV by 10% percent relative to cell groups

with 0% above the same threshold.

7 Impact of the policy on equilibrium prices

The objective of this section is to compare the trajectories of the equilibrium price-to-rent ratio before and after the policy in areas that were more or less exposed to the policy change. In the previous section we compared the choices of similar groups of individuals across time and compared changes between groups that more or less above the constraints in the pre-period. As noted in section 2, a decrease in the observed purchase price could come from a change in the size of the house purchased or a change in the equilibrium price of the house. In this section I focus on the change in equilibrium prices by comparing equilibrium prices across regions. This will include transacted prices and rents for homes, which differs from the previous section which covered the purchase price only for those who took out mortgages.

In the text, I report results for the change in the price-to-rent ratio. Using the price-to-rent ratio partially accounts for differences in prices that might come from changes in income or rents across areas. Appendix D reports the results for prices and rents separately, and I comment on the differences where relevant.

7.1 Data construction

This section uses a regional dataset at the postcode level[15] combining information from the mortgage, and price and rent datasets described in Section 4.

I construct quarterly panel of prices, rents and the price-to-rent ratio for each county. I use the mortgage dataset to compute the percentage of borrowers above the LTI threshold and the percentage of borrowers above the LTV threshold in each county in the year prior to the policy change. This is consistent with the metric used in the previous section (6) for individual borrowing. As noted, it is an attractive measure of the *treatment intensity* for each postcode because it is a continuous measure of treatment intensity, and is positively correlated with alternative measures such as the mean LTI/LTV of the cell or the average distance of the cell to the threshold. Relative to these other measures of treatment intensity, the percentage above the threshold has a clear interpretation as the impact from increasing the proportion of potentially constrained individuals by 1 percentage point.

I use this constructed panel to answer (i) what groups were above the threshold in the pre-period, and (ii) how did the equilibrium price-to-rent ratio of these areas change following

[15]As noted in section 4, I define postcodes to be the union of counties and *An Post* postcodes within County Dublin.

the policy.

7.2 Relationship between postcode characteristics and the percentage of the new mortgages above the LTI and LTV thresholds before the policy

Before examining the change in prices after the policy change, Figure 7 plots the relationship between postcode characteristics — price and price-to-rent ratio — and the percentage of new mortgages that were above the LTV or LTI thresholds.

The left-hand panel 7a shows the relationship by the percentage of new mortgages in the postcode above the LTI threshold and the right-hand panel 7b shows the relationship by the percentage above the LTV threshold. In both panels the top figure includes the price-to-rent ratio while the bottom figure plots the price. The relationship with rents can be deduced from the difference between prices and price-to-rent, and for completeness is included in Figure 19 in Appendix D. In this figure, each dot is a postcode in 2014 and the line is the fit from a linear regression.

Panel 7a shows that percentage above the LTI threshold is largely uncorrelated with the price-to-rent ratio even though it is positively correlated with the price. This implies that rents are also positively correlated with the percentage above the LTI threshold, which is confirmed in Appendix D. Panel 7b shows that percentage of borrowers in a county above the LTV threshold is positively correlated with both prices and the price-to-rent ratio. Appendix D also shows that rents are also positively correlated with the percentage of borrowers in a county above the LTV threshold, however the positive correlation with price-to-rent implies that the correlation is stronger among prices and rents.

Figure 7 tells us that both the policies are more likely to bind in more expensive areas both in terms of prices and rents. At least in the case of the LTI policy, it is not likely to be more or less binding in areas in terms of valuation from an asset pricing perspective, as measured by the price-to-rent ratio.

It is worth pointing out the spread on the percentage above the LTV and LTI thresholds in Figure 7. In the LTI case, the percentage ranges approximately between 5% and 35% while in the LTV case it ranges between 5% and 45%. This is similar to the range in the cell groups, however, there are two differences to the ranges among with the cell groups of individuals: no postcode has 0% of borrowers above the LTI or LTV thresholds; and the ranges are closer than for cell groups (where they spanned 0-30% and 0-50% respectively.

. As before, in the regressions I will compare the impacts over a range of LTV and LTI percentages, and since the respective ranges are closer to each other I will compare across the same range of 0-30%.

7.3 How did choices change after the policy

This section examines how equilibrium prices changed after the policy change. I use a difference in difference framework to compare these changes across areas based on the whether they were high or low on the percentage of borrowers above the LTI or LTV threshold.

7.3.1 Difference-in-difference results

For a given outcome variable Y_{it}, the difference-in-difference regression takes the following form

$$\begin{aligned} Y_{it} \quad = \quad & \sum_{k \neq 2014} \delta_k^{LTI} [\%\text{ above LTI threshold}_i^{2014} \times \mathbb{1}_{t=k}] + \\ & \sum_{k \neq 2014} \delta_k^{LTV} [\%\text{ above LTV threshold}_i^{2014} \times \mathbb{1}_{t=k}] + \boldsymbol{\tau_t} + \boldsymbol{\gamma_i} + \boldsymbol{\mu_i} t + \epsilon_{it} \end{aligned} \tag{12}$$

where $\boldsymbol{\tau_t}$ are year fixed effects, $\boldsymbol{\gamma_i}$ are postcode fixed effects, $\boldsymbol{\mu_i}$ are postcode specific time trends and ϵ_{it} is the error term. Standard errors are clustered at the postcode level. The variables % above LTI threshold$_i^{2014}$ and % above LTV threshold$_i^{2014}$ are the proportion of individuals in cell i in 2014, the year before the policy, that are above the LTI and LTV threshold. The impact of having one percentage point higher proportion of borrowers in the postcode above the LTI threshold δ_k^{LTI} and LTV threshold δ_k^{LTV} are both allowed to be time varying with δ_k^{LTI} δ_k^{LTV} denoting the year-by-year impact of the being relatively constrained.

Standard errors are clustered at the postcode level, which accounts for within postcode correlation between the treatment exposure variable and unobservable shocks. All regressions are unweighted.

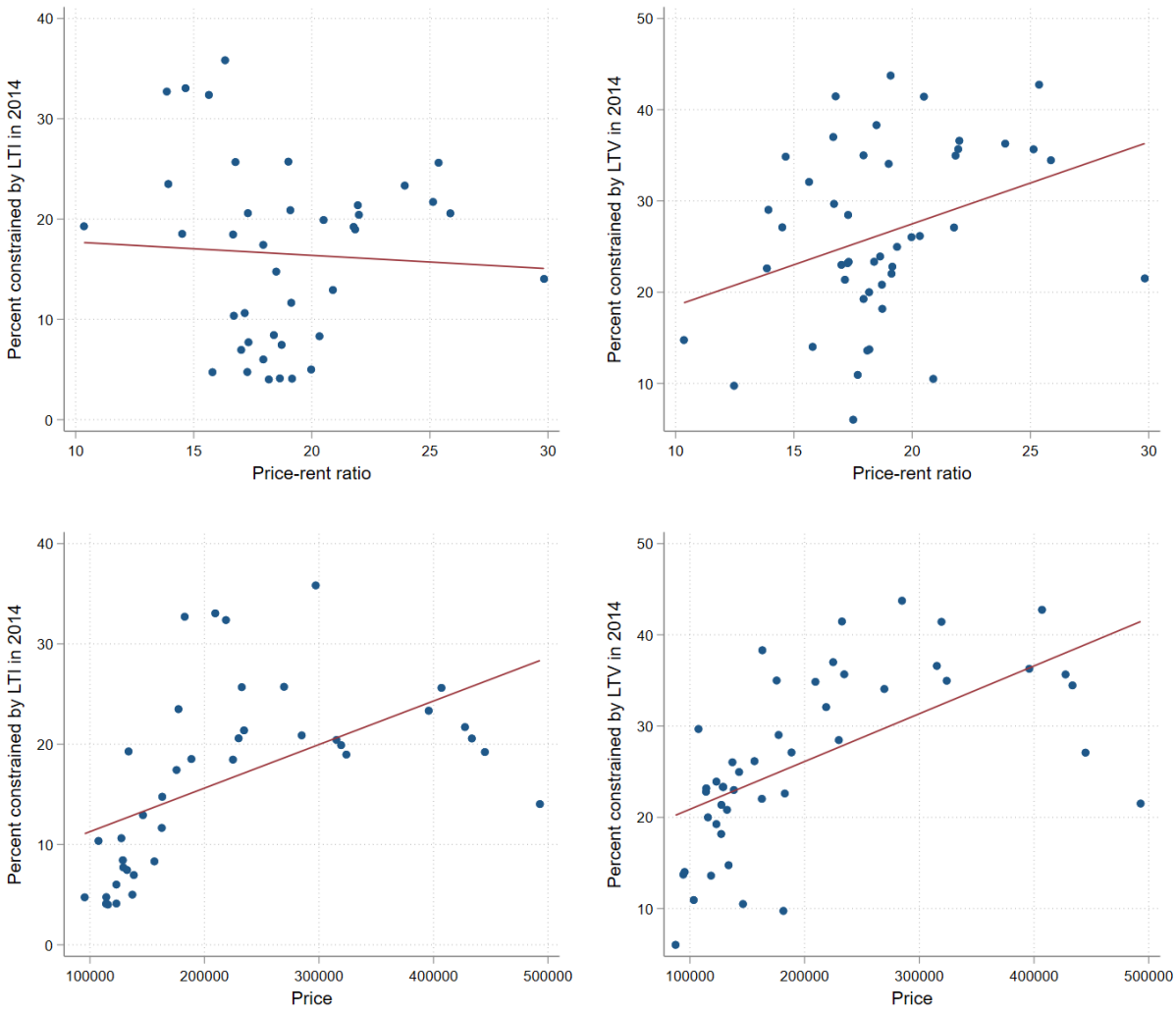
In this specification the δ_k^{LTV} and δ_k^{LTV}, can be interpreted as the relative impact of having one percentage point higher proportion of borrowers in the postcode above the relative threshold, conditional proportion of the cell above the other threshold, in each year.

I omit δ_{2014}^{LTI} and δ_{2014}^{LTV} so the other δ_k^{LTI}'s and δ_k^{LTV}'s can be interpreted as relative to the pre-policy baseline period. Particularly in the case equilibrium prices, it is worth noting again that the difference-in-difference estimates do not necessarily measure the aggregate impact of the policy change as they difference out general equilibrium effects (Auclert et al.,

Figure 7: Relationship between postcode characteristics and the percentage of the mortgages in the postcode above the LTI and LTV thresholds before the policy

(a) By percentage above the LTI threshold

(b) By percentage above the LTV threshold



2019; Chodorow-Reich, 2020).

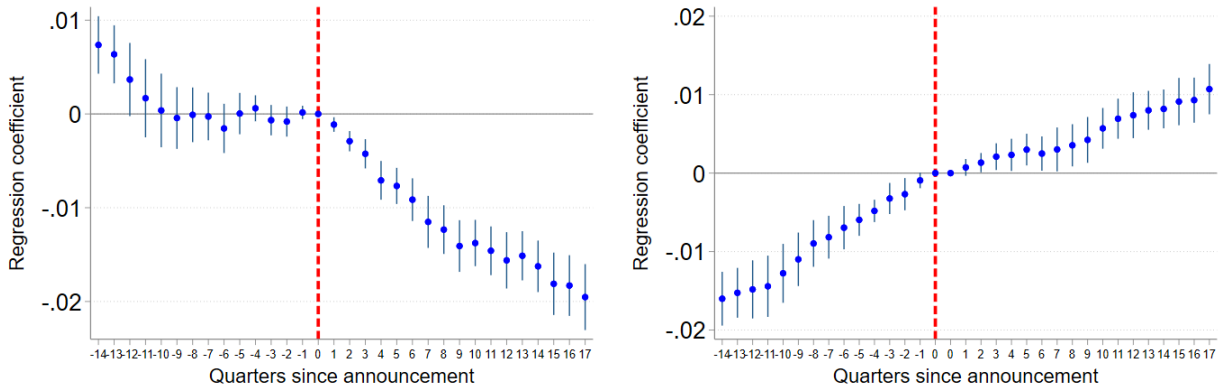
This regression takes the same form as in section 6 apart from two differences. Firstly, I include postcode specific time trends to account for some time trends in prices and rents throughout this time. This means the treatment effect is now relative to any postcode specific time trend in the pre-period. Table 3 in Appendix D reports robustness without time trends. Secondly, I do not weight the results by some measure of unit size. In the case of individual borrowing, the constructed cells varied by orders of magnitude in terms of size with specific groups such as younger, high income cell groups approaching zero. In that case, I weighted the results to prevent these tiny groups from having a large impact on the results. In the case of postcodes, the size of each unit is more comparable, especially since I have broken the largest county Dublin into it's constituent postcodes (as discussed in Section 4).

The identifying assumption is the same as before, that is that in the absence of the policy change, the equilibrium prices for post codes with more or less people above the LTV or LTI threshold would have evolved in parallel. This parallel trends assumption implies there are no other reasons why the cell groups would act differently in the absence of the policy change.

As before, the main regression specification includes both % above LTI threshold$_i^{2014}$ and % above LTV threshold$_i^{2014}$ in the same regression. This means the estimates for the impact of one policy are conditional on the impact of the other policy. Since both policies came into effect at the same time, failure to include both the LTV and LTI measures could lead to omitted variable bias due to the correlation of both measures. For robustness, I estimate each impact separately and I report the results in Appendix C.

Figure 8: Difference-in-difference estimates on log price-to-rent

(a) Impact of LTI treatment, δ_k^{LTI} (b) Impact of LTV treatment, δ_k^{LTV}



Notes: Figure presents results of difference-in-difference regression in Equation (12). The unit of observation is a postcode. The announcement date was 2014Q3.

The top panels of Figure 8 plot the year-by-year impacts on log price-to-rent ratio of LTI, δ_k^{LTI}, (panel 8a) and of LTV, δ_k^{LTV}, (panel 8b). The numbers underlying Figure 8 are reported in Table 3 in Appendix D.

Panel 8a plots the quarter-by-quarter impacts of the LTI treatment intensity, δ_k^{LTV}, on the log price-to-rent ratio. The coefficient on the twelve quarters prior to the policy change not statistically significant from zero (at the 95% confidence level), albeit there was a downward trend before this time. The coefficient fourth quarter after the policy is 0.007, implying that the price-to-rent ratio fell by 0.7% in postcodes with one percentage point higher share above the LTI threshold. The coefficients are persistently negative in all quarters after the policy change.

Panel 8b plots the quarter-by-quarter impacts of the LTV treatment intensity, δ_k^{LTI}, on the log price-to-rent ratio. The coefficient on the twelve quarters prior to the policy change are negative and trending upwards implying the parallel trends assumption does not hold. The coefficient in the year after the policy is 0.002, implying cell groups with one percentage point higher share above the LTV threshold increased their purchase price by 0.02%. The trend is slightly lower in the post-policy period.

Interpreting the results of Figure 8 together, it appears the price-to-rent ratio fell in that postcodes with many borrowers constrained by the LTI cap but less so in areas with many borrowers constrained by the LTV cap. This is seen by the by the persistent negative impact of δ^{LTI} whereas δ^{LTV} continues to trend upwards, albeit at a lower slope. This is consistent with the results in section 6 that showed the purchase price primarily fell for cell groups who were constrained by the LTI cap.

Magnitude of these changes are economically large. As noted in Figure 7, the percent above LTI threshold variable primarily varies between 5% and 35%. Comparing the end-points of this ranges, I find that postcodes with 35% above the LTI threshold experienced a 21% percent fall in the price-to-rent ratio relative to postcodes with with 5% above the same threshold.

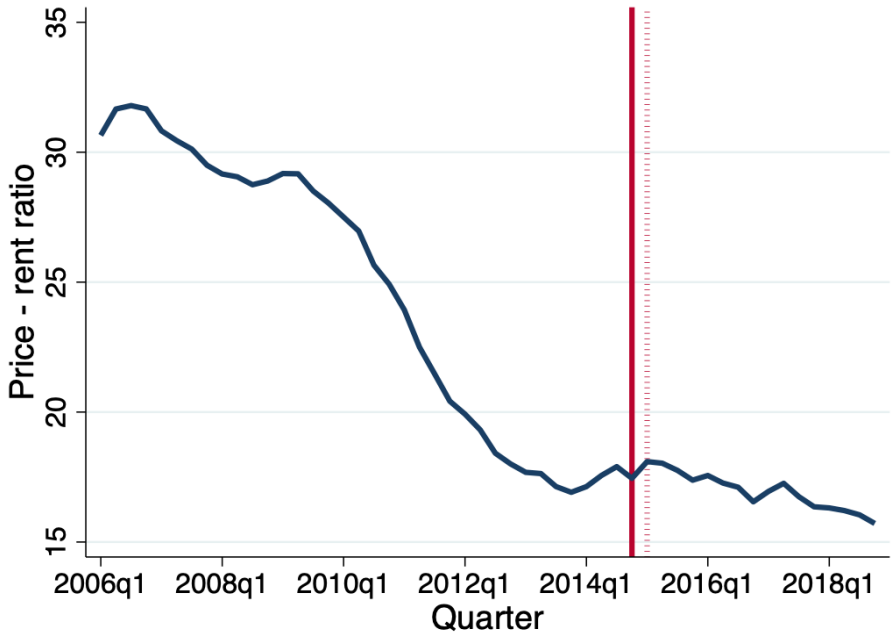
8 Aggregate variables before and after the policy

The difference-in-difference results inherently cancel out any aggregate or general equilibrium impact of the policy change. This section presents evidence that the policy also impacted price growth and the price-to-rent ration in the aggregate.

Figure 9 plots the evolution of price and rents before and after the policy change. Here

we see that the price-rent ratio fell by over one third from its peak in 2007. Following the trough in 2013, the price-to-rent ratio grew for three quarters before the introduction of the borrowing constraints in February 2015. The price-rent ratio declined by 11 percent after the policy change, reaching a lower level than the initial trough in 2013.

Figure 9: Price-to-rent ratio 2006-18



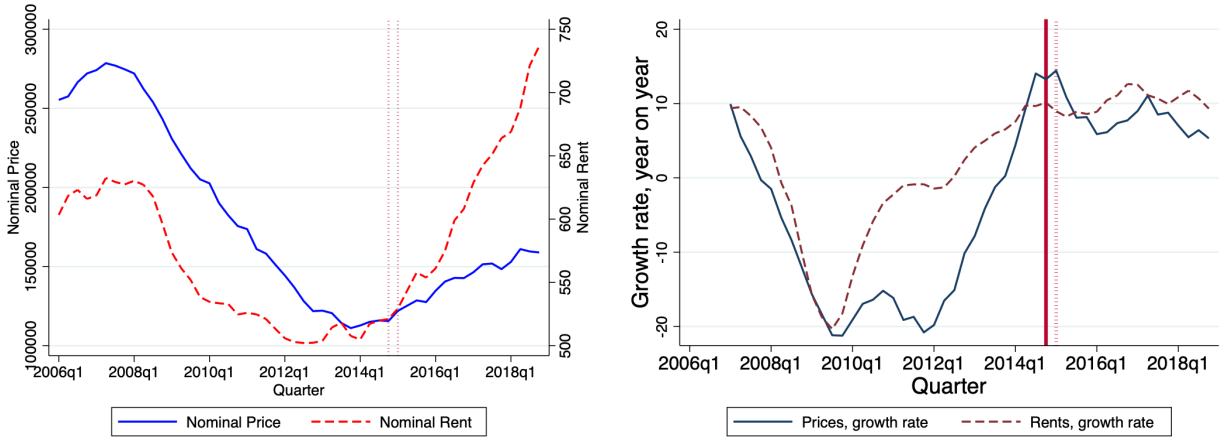
Notes: Figure plots the price-to-rent ratio nationally, 2006q1 to 2018q4. The two vertical dashed lines show the announcement and implementation dates respectively. Source: daft.

The price-rent ratio is broken into its individual parts in Figure 10. In Panel 10a we see that both prices and rents are rising after the policy change. This implies that the price-rent ratio is declining due to rents rising faster than prices. Indeed this can be seen directly in the growth rates in Panel 10b. The price-rent ratio was increasing when the growth rate in prices (blue solid line) exceeded the growth rate in rents (rend dashed line). After the policy change, the growth rate in rents is higher, which resulted in the falling price-rent ratio.

The declining price-rent ratio coupled with rising rents and prices is surprising for two reasons. Firstly, we might expect the price-rent ratio to recover more, or for a longer period time, after a deep recession. For instance the price-rent ratio in the US has increased every quarter over 2012-17 (OECD, 2019). Secondly, it is unusual to see the price-rent ratio decline due to fast growing rents. We might expect these rising rents to be capitalized into the price, especially when the price-rent ratios is relatively low. Combined with the cross

sectional evidence, this suggests that the rules change may have reduced the price-to-rent ratio by constraining price growth.

Figure 10: House prices and rents 2006-18



(a) Levels

(b) Growth rates

Notes: Left figure shows the price and rent indices nationally, 2007q1 to 2018q4. Right figure shows the growth rate (log change) in the same indices. The two vertical dashed lines show the announcement and implementation dates respectively. Source: daft.

9 Conclusion

In conclusion, this paper has made significant contributions to understanding the impact of mortgage borrowing limits on housing markets, particularly in the context of the 2015 policy change in Ireland. The analysis reveals that different borrower types responded distinctively to loan-to-value (LTV) and loan-to-income (LTI) constraints, highlighting the nuanced nature of borrowing behavior and housing market dynamics.

The paper finds that borrowers constrained by the LTI limits primarily adjusted by reducing the purchase prices of houses. This response is logical given that these borrowers, typically younger with lower incomes and wealth, had limited resources to increase down payments. On the other hand, borrowers affected by the LTV limits, who generally had higher incomes and wealth, responded by reducing the loan-to-value ratios of their mortgages. This finding is insightful as it demonstrates that borrowers with greater financial resources are more capable of adapting to borrowing constraints by adjusting their leverage rather than the value of the property they purchase.

Furthermore, the paper's analysis on the impact of borrowing limits on equilibrium prices

in the housing market adds another layer of depth. The study shows that the price-to-rent ratio declined post-policy in areas with a higher proportion of borrowers above the LTI threshold, while areas with more borrowers above the LTV threshold saw less of a decline. This suggests that LTI constraints had a more pronounced effect on cooling down the housing market than LTV constraints.

These findings have broad implications for housing market policy and financial stability. They indicate that macroprudential policies like borrowing limits can be effective tools for managing housing market dynamics and preventing overheating. However, the differential impacts based on borrower characteristics and regional variations suggest that policymakers need to consider these factors to fine-tune such policies for desired outcomes.

In terms of future research, it would be beneficial to investigate further into the long-term effects of such borrowing limits on housing markets, particularly in terms of housing affordability and accessibility for different demographic groups. Additionally, exploring the labor supply response to borrowing constraints could provide a more comprehensive understanding of the broader economic implications of such policies.

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A Appendix to Section 2: Theoretical Framework

In this section, I provide the full derivation of equations included in Section 2.

In the case where the LTI constraint is binding (so equation (4) holds with equality) and the other constraint does not bind, equation (7) follows from subbing the LTI constraint into the budget constraint and rearranging

$$\begin{aligned} c + ph &= w + y + m \\ c + ph &= w + y + \psi^{lti} y \\ ph &= (w - c) + (1 + \psi^{lti}) y. \end{aligned}$$

Taking the derivative with respect to savings (conditional on income) yields

$$\frac{\partial ph}{\partial (w - c)} = 1.$$

In the case where the LTV constraint is binding (so equation (5) holds with equality) and the other constraint does not bind, equation (8) follows from subbing the LTI constraint into the budget constraint and rearranging

$$\begin{aligned} c + ph &= w + y + m \\ c + ph &= w + y + \psi^{ltv} ph \\ (1 - \psi^{ltv}) ph &= (w - c) + y \\ ph &= \frac{1}{(1 - \psi^{ltv})} (w - c) + y \end{aligned}$$

Taking the derivative with respect to savings (conditional on income) yields

$$\frac{\partial ph}{\partial (w - c)} = \frac{1}{(1 - \psi^{ltv})}.$$

B Appendix to Section 5: Distributions of LTI and LTV before and after the policy

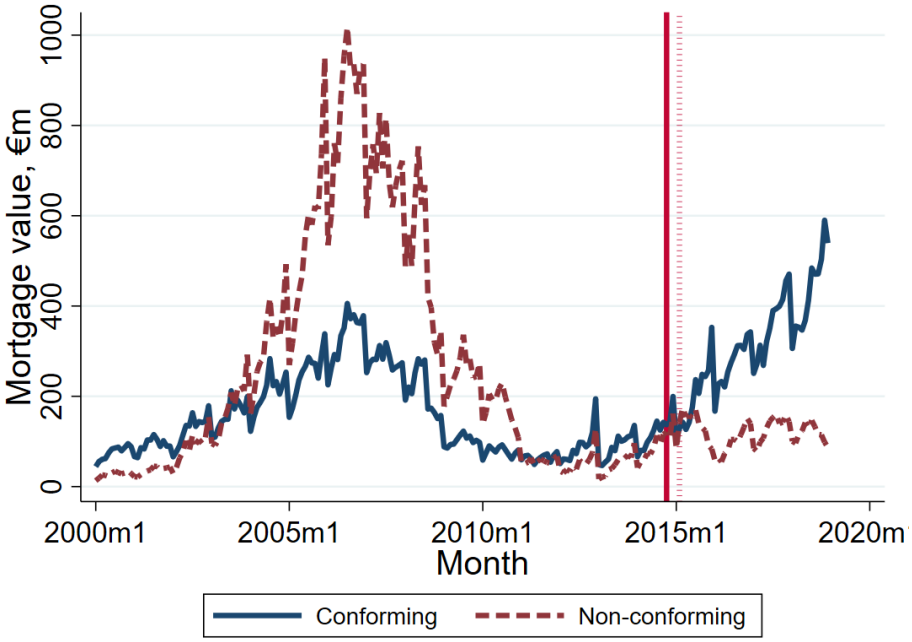


Figure 11: Mortgages conforming to 2015 policy

Notes: Figure shows the value of loans conforming to the 2015 LTI and LTV mortgage rules before and after the introduction of the rules. Vertical dashed lines show the announcement and implementation dates respectively. Source: Central Bank of Ireland's mortgage datasets.

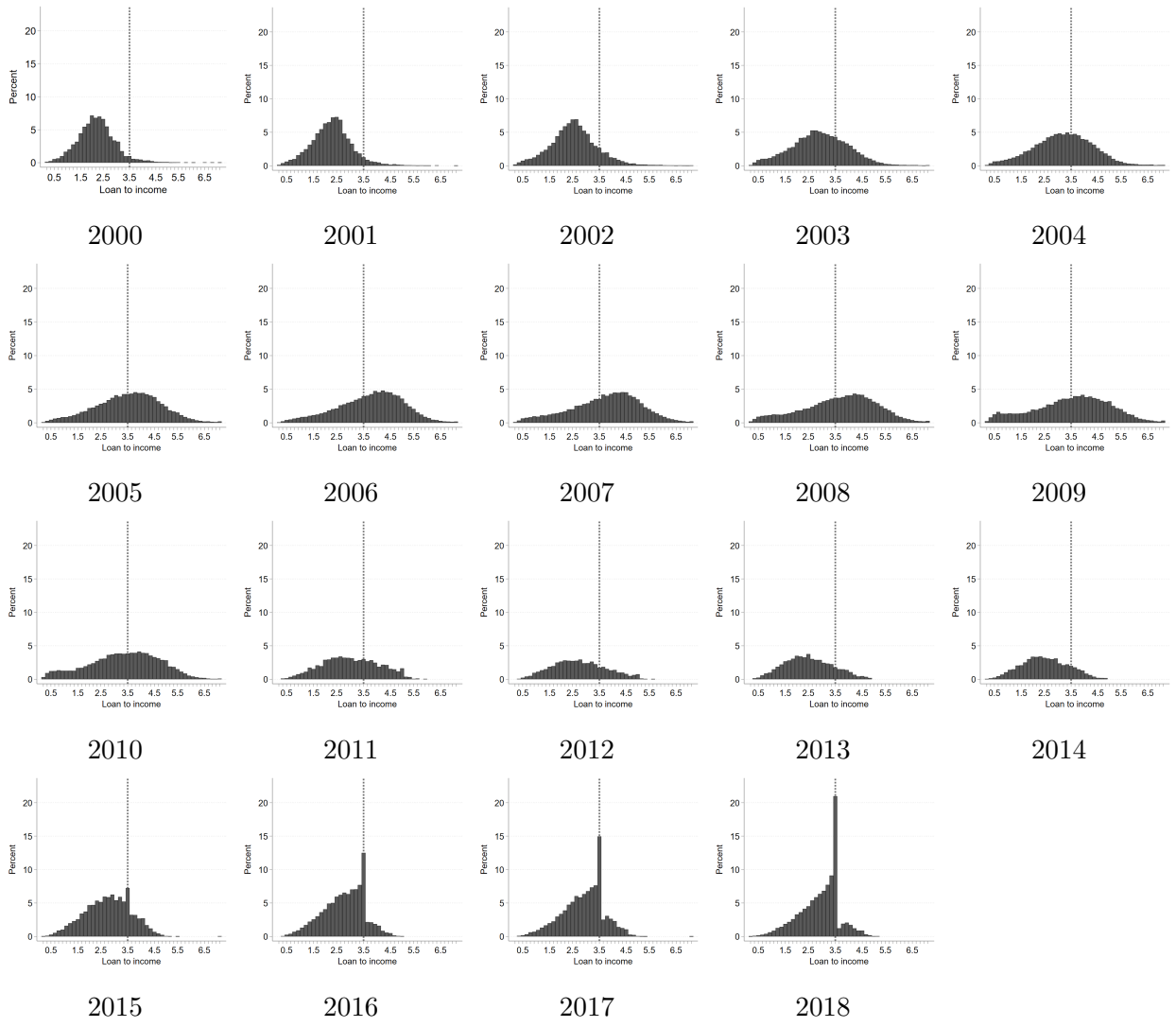


Figure 12: Distribution of mortgages originated: LTI for FTB and SSB combined

Notes: Figure shows distribution mortgages originated based on loan-to-income (LTI) 2000-2018. Vertical line shows the cap of 3.5 that was introduced after 2014.

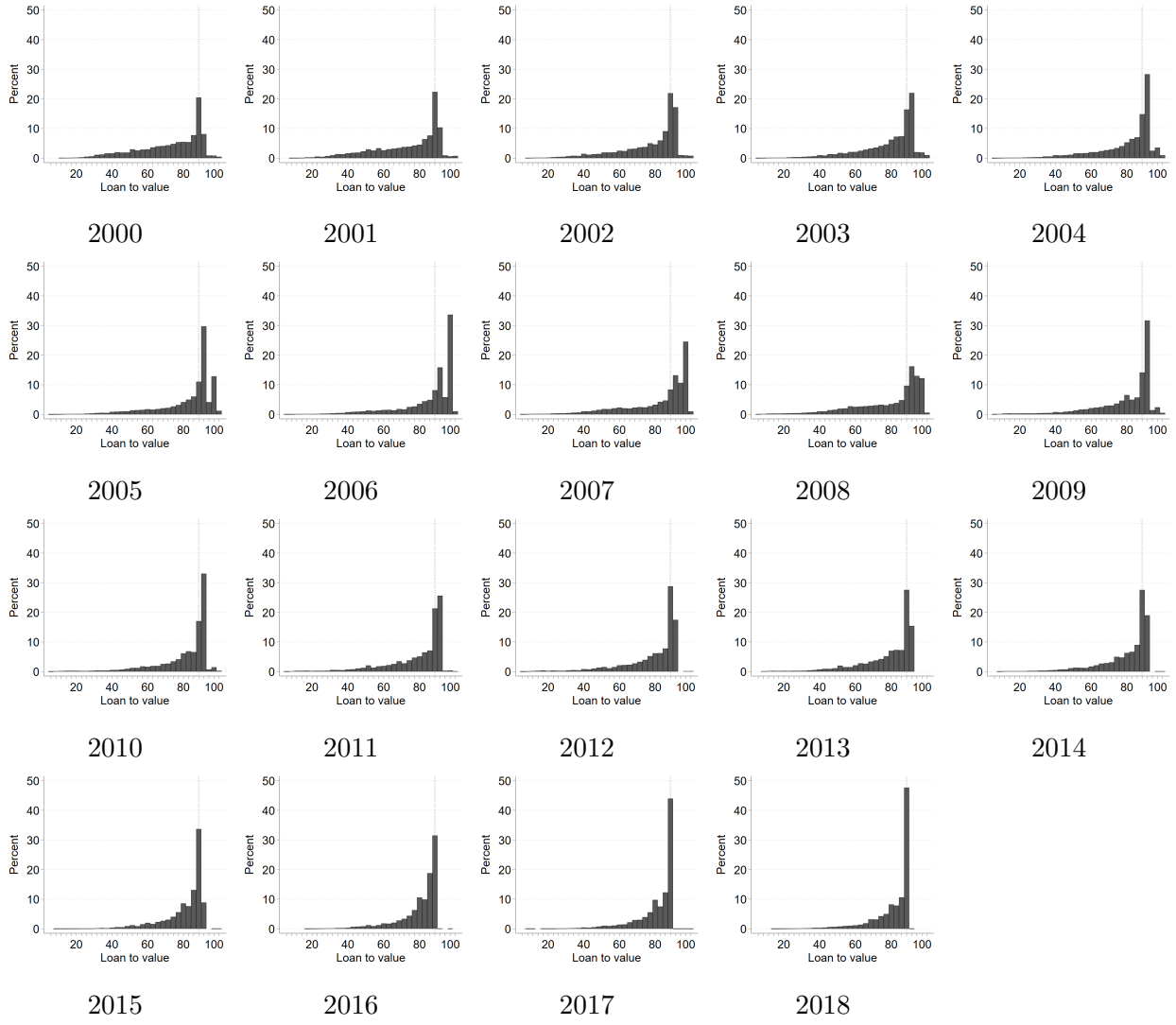


Figure 13: Distribution of mortgages originated: LTV for FTB

Notes: Figure shows distribution mortgages originated based on loan-to-value (LTV) 2000-2018. Vertical line shows the cap of 90% that was introduced after 2014. For these borrowers in 2015 and 2016 there was a cap of 90% on mortgage balances up to €220,000 and a tighter 80% cap the remainder of the mortgage balance that was above €220,000.

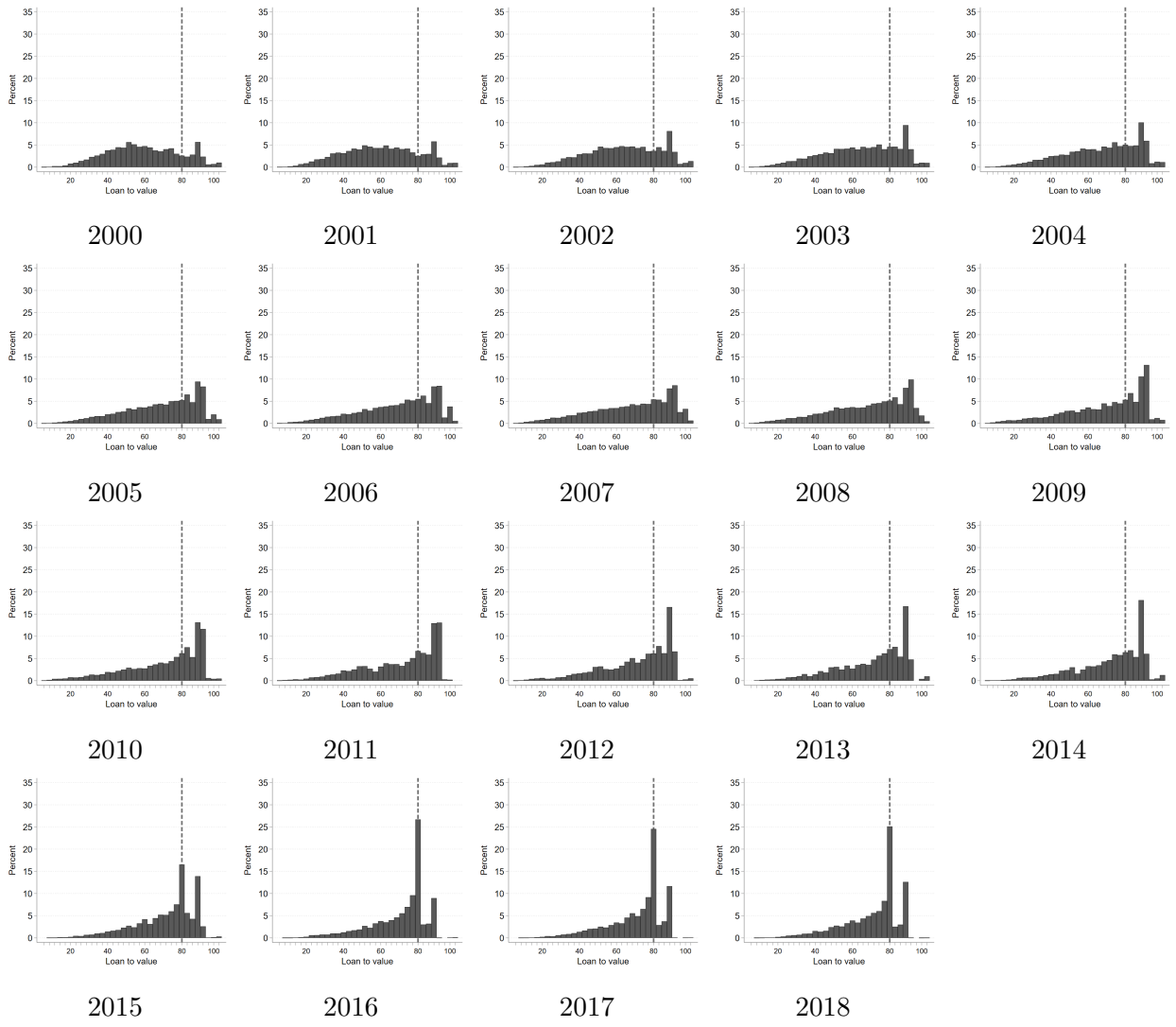


Figure 14: Distribution of mortgages originated: LTV for second or subsequent buyers (SSB)
Notes: Figure shows distribution mortgages originated based on loan-to-value (LTV) 2000-2018. Vertical line shows the cap of 80% that was introduced after 2014.

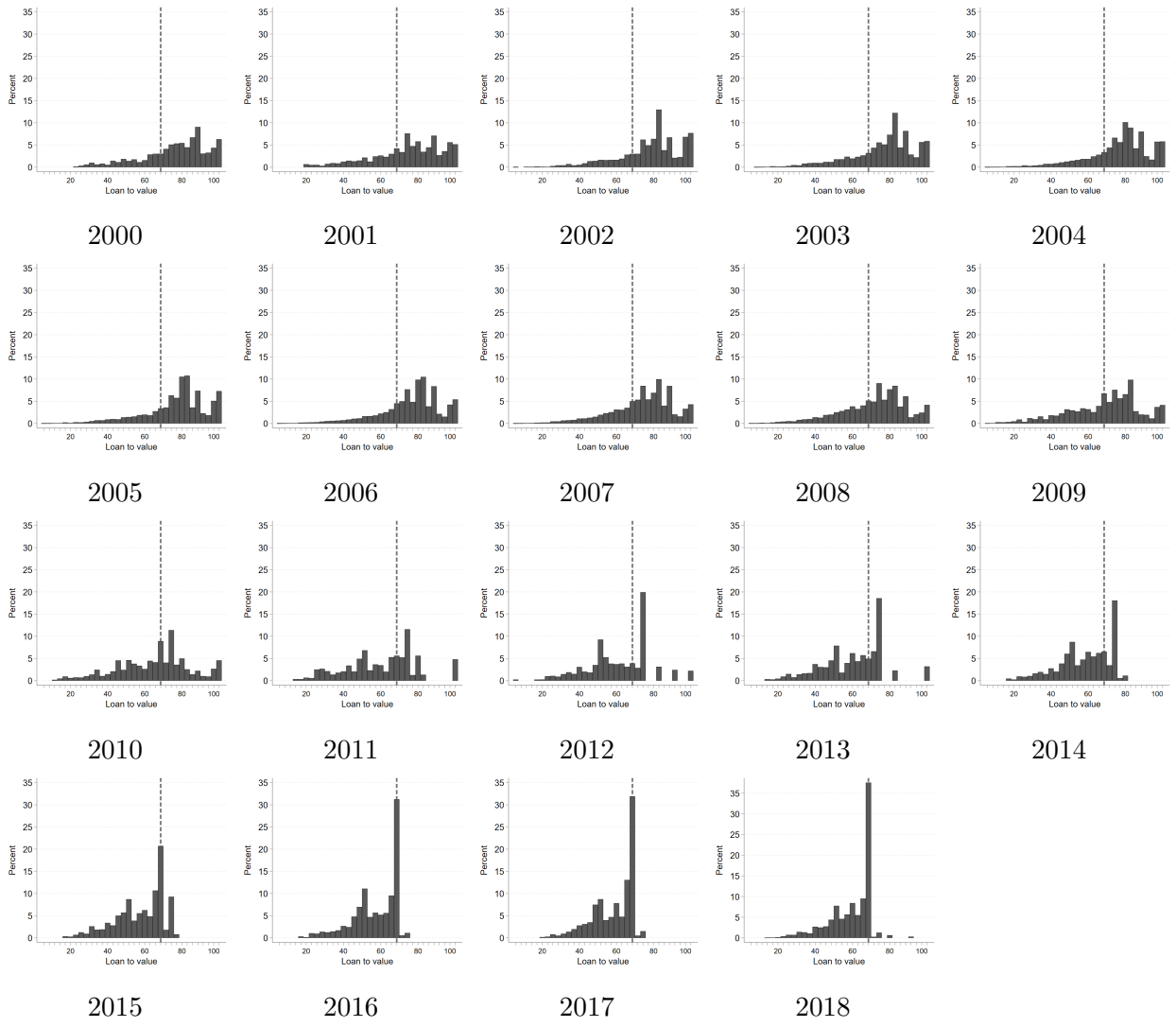


Figure 15: Distribution of mortgages originated: LTV for buy-to-rent

Notes: Figure shows distribution mortgages originated based on loan-to-value (LTV) 2000-2018. Vertical line shows the cap of 70% that was introduced after 2014.

C Appendix to Section 6: Impact of the policy on individual borrowing

This appendix includes additional results for Section 6. Table 2 presents the estimates used to construct Figure 6.

Figure 16 reports results from the main difference-in-difference regression in Equation 9 on additional outcome variables log purchase price, log downpayment and log mortgage.

In the main text, the difference-in-difference regression (Equation 9 and Figure 6) included both the impact the LTI and LTV thresholds in the same equation. Figures 17 and 18 reports the results when either but not both treatment intensity variables are included, specifically Figure 17 includes the percent above the loan-to-income threshold

$$y_{it} = \sum_{k \neq 2014} \delta_k^{LTI} [\% \text{ above LTI threshold}_i^{2014} \times \mathbb{1}_{t=k}] + \boldsymbol{\tau_t} + \boldsymbol{\gamma_i} + \epsilon_{it}$$

and Figure 18 includes the percent above the loan-to-value threshold

$$y_{it} = \sum_{k \neq 2014} \delta_k^{LTI} [\% \text{ above LTV threshold}_i^{2014} \times \mathbb{1}_{t=k}] + \boldsymbol{\tau_t} + \boldsymbol{\gamma_i} + \epsilon_{it}.$$

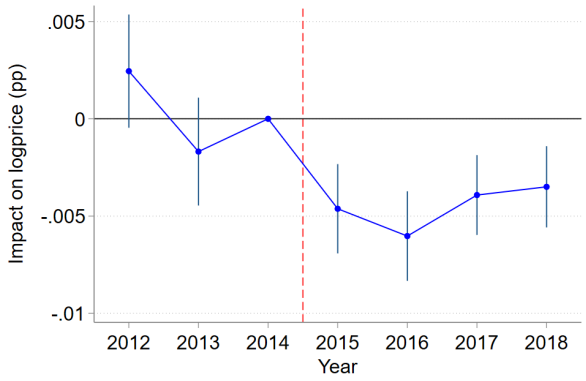
Table 2: Difference-in-difference estimates for Figure 6

	(1) Log price		(2) Log LTV	
Percent above LTI threshold × 2012	0.002*	(0.0015)	-0.002	(0.0011)
Percent above LTI threshold × 2013	-0.002	(0.0014)	-0.001	(0.0012)
Percent above LTI threshold × 2014	0.000	(.)	0.000	(.)
Percent above LTI threshold × 2015	-0.005***	(0.0012)	0.001	(0.0010)
Percent above LTI threshold × 2016	-0.006***	(0.0012)	0.003***	(0.0009)
Percent above LTI threshold × 2017	-0.004***	(0.0010)	0.001	(0.0011)
Percent above LTI threshold × 2018	-0.003***	(0.0011)	0.000	(0.0012)
Percent above LTI threshold	-0.020***	(0.0044)	0.010***	(0.0018)
Percent above LTV threshold × 2012	-0.001	(0.0007)	-0.002***	(0.0006)
Percent above LTV threshold × 2013	-0.001*	(0.0006)	-0.001*	(0.0005)
Percent above LTV threshold × 2014	0.000	(.)	0.000	(.)
Percent above LTV threshold × 2015	0.001**	(0.0006)	-0.002***	(0.0005)
Percent above LTV threshold × 2016	0.001**	(0.0005)	-0.002***	(0.0005)
Percent above LTV threshold × 2017	0.001***	(0.0005)	-0.002***	(0.0005)
Percent above LTV threshold × 2018	0.001**	(0.0006)	-0.001**	(0.0006)
Percent above LTV threshold	0.009***	(0.0019)	0.004***	(0.0009)
2012	0.002	(0.0319)	0.067***	(0.0254)
2013	0.024	(0.0300)	0.039	(0.0272)
2014	0.000	(.)	0.000	(.)
2015	0.116***	(0.0262)	0.011	(0.0215)
2016	0.158***	(0.0278)	-0.027	(0.0231)
2017	0.189***	(0.0225)	-0.005	(0.0222)
2018	0.200***	(0.0225)	-0.004	(0.0258)
Constant	12.470***	(0.1024)	4.027***	(0.0416)
Observations	898		898	
Adjusted R^2	0.620		0.220	

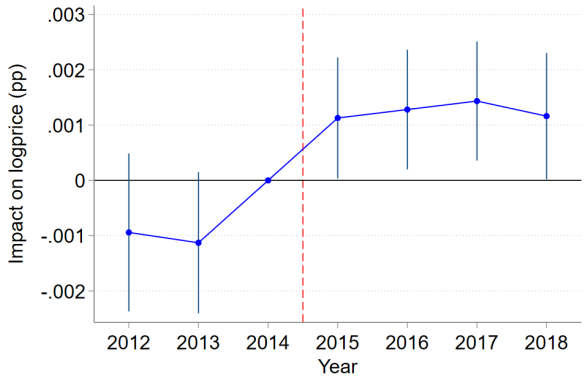
Note: Regressions are weighted by cell size in the pre-treatment year (2014). Standard errors are clustered at the cell level, and reported in parenthesis. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

Figure 16: Difference-in-difference estimates

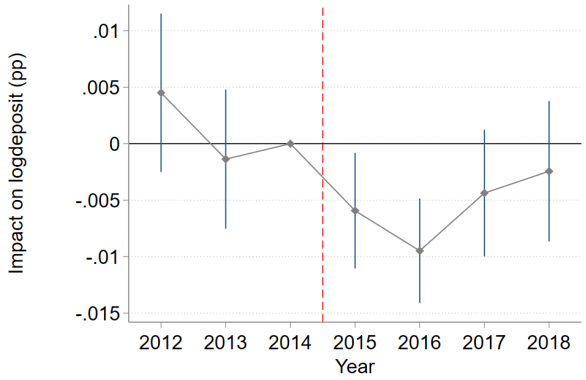
(a) Impact of LTI treatment on log purchase price, δ_k^{LTI}



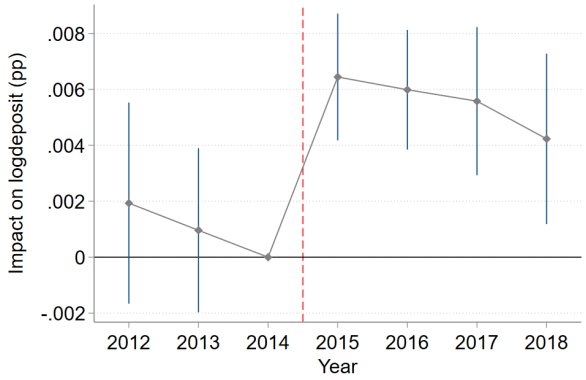
(b) Impact of LTV treatment on log purchase price, δ_k^{LTV}



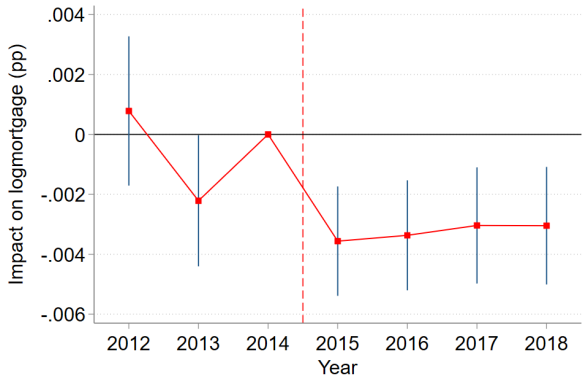
(c) Impact of LTI treatment on log downpayment, δ_k^{LTI}



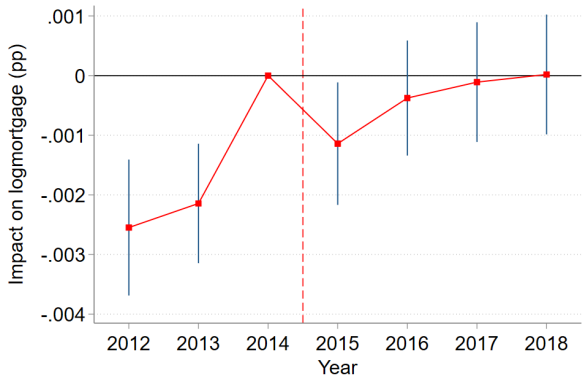
(d) Impact of LTV treatment on log downpayment, δ_k^{LTV}



(e) Impact of LTI treatment on log mortgage, δ_k^{LTI}

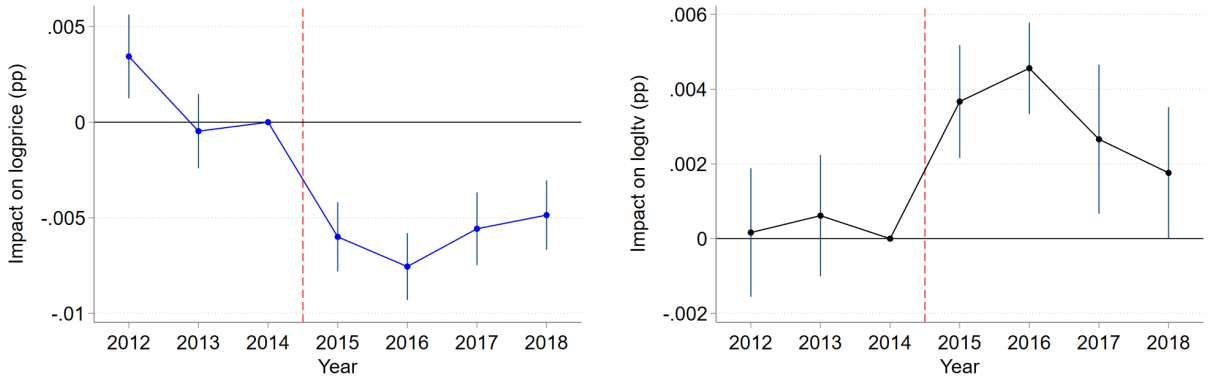


(f) Impact of LTV treatment on log mortgage, δ_k^{LTV}



Note: Regressions are weighted by cell size in the pre-treatment year (2014). Standard errors are clustered at the cell level.

Figure 17: Difference-in-difference estimates, impact of LTI

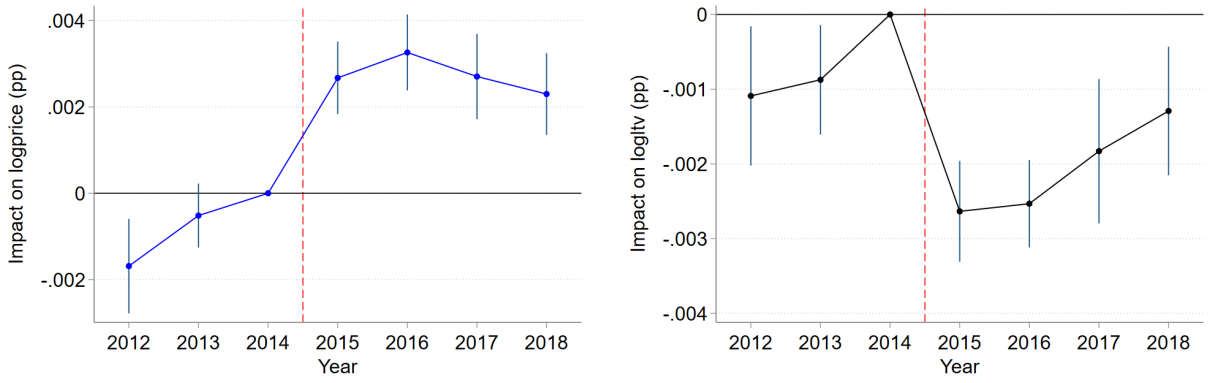


(a) Price paid (log)

(b) Leverage (log ltv)

Note: Regressions are weighted by cell size in the pre-treatment year (2014). Standard errors are clustered at the cell level.

Figure 18: Difference-in-difference estimates, impact of LTV



(a) Price paid (log)

(b) Leverage (log ltv)

Note: Regressions are weighted by cell size in the pre-treatment year (2014). Standard errors are clustered at the cell level.

D Appendix to Section 7: Impact of the policy on equilibrium prices

This appendix includes additional results for section 7. Figure 19 reports the relationship between postcode characteristics (price, rent, price-to-rent) and the percentage of mortgages in the postcode above the LTI and LTV thresholds in the year before the policy change.

In the main text, Figure 20 difference-in-difference estimates for the log price-to-rent ratio. Figure 20 extends this analysis, reporting the results for additional outcome variables log price log rent, as well as the log price-to-rent, which was reported in the main body. Table 3 reports the numbers underlying Figures **??** and 20. Table **??** reports compares the baseline specification (with county specific time trends) to estimates without such time trends as well as one with Dublin specific year fixed effects.

Figure 19: Relationship between postcode characteristics and the percentage of the mortgages in the postcode above the LTI and LTV thresholds before the policy

(a) By percentage above LTI threshold

(b) By percentage above LTV threshold

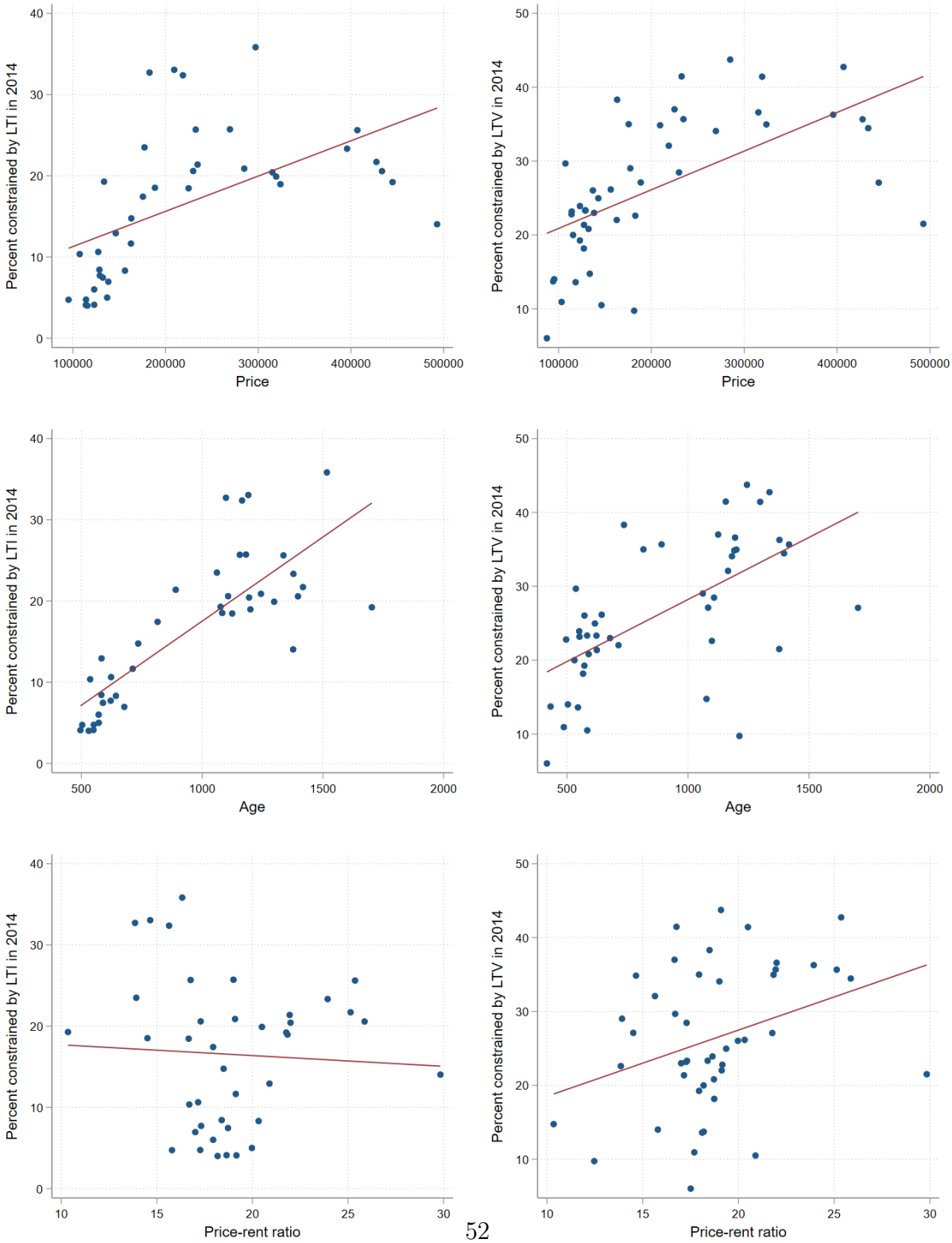
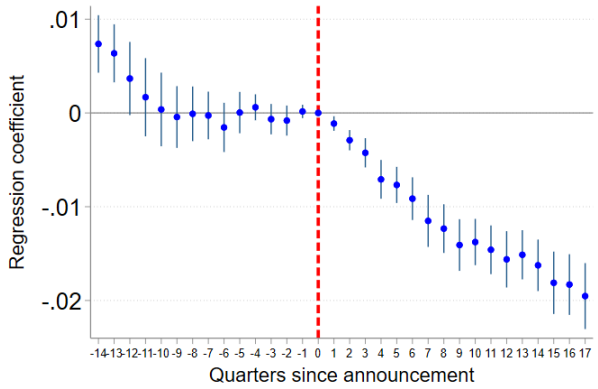
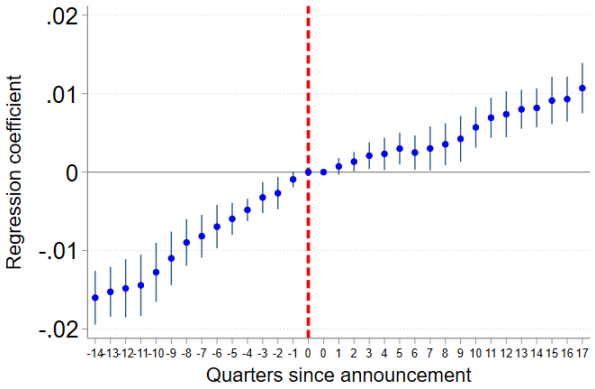


Figure 20: Difference-in-difference estimates: log price-to-rent, log price, and log rent

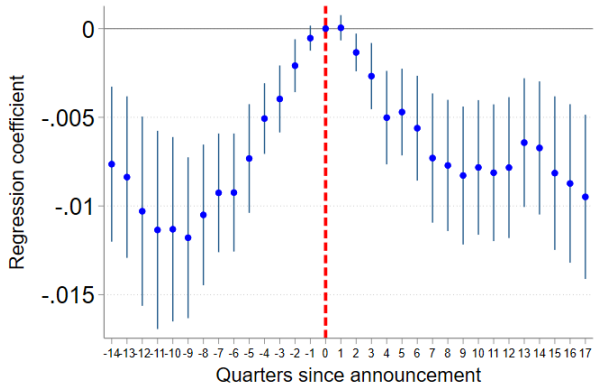
(a) Impact of LTI treatment on log price-to-rent, δ_k^{LTI}



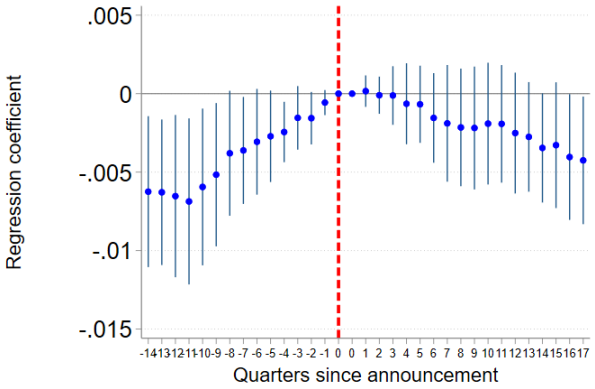
(b) Impact of LTV treatment on log price-to-rent, δ_k^{LTV}



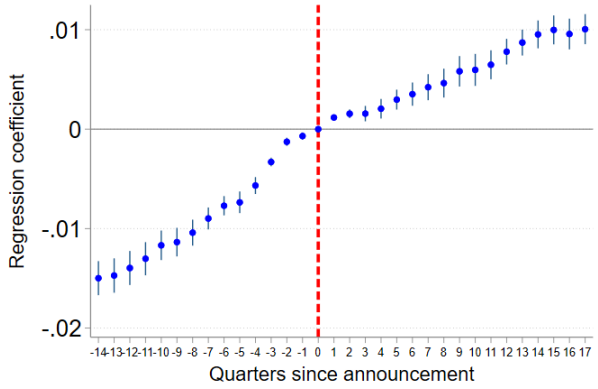
(c) Impact of LTI treatment on log price, δ_k^{LTI}



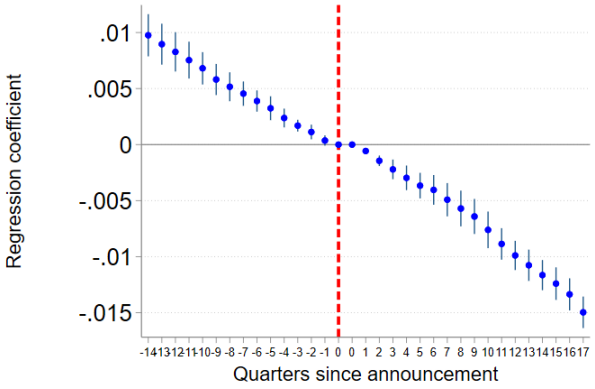
(d) Impact of LTV treatment on log price, δ_k^{LTV}



(e) Impact of LTI treatment on log rent, δ_k^{LTI}



(f) Impact of LTV treatment on log rent, δ_k^{LTV}



Note: Standard errors are clustered at the postcode level.

Table 3: Difference-in-difference estimates for Figures 8 and 20

	(1) Log price-to-rent		(2) Log price		(3) Log rent	
Percent above LTI threshold × 2011 Q1	0.007***	(0.0015)	-0.008***	(0.0022)	-0.015***	(0.0008)
Percent above LTI threshold × 2011 Q2	0.006***	(0.0015)	-0.008***	(0.0022)	-0.015***	(0.0009)
Percent above LTI threshold × 2011 Q3	0.004*	(0.0019)	-0.010***	(0.0026)	-0.014***	(0.0008)
Percent above LTI threshold × 2011 Q4	0.002	(0.0021)	-0.011***	(0.0028)	-0.013***	(0.0008)
Percent above LTI threshold × 2012 Q1	0.000	(0.0019)	-0.011***	(0.0026)	-0.012***	(0.0007)
Percent above LTI threshold × 2012 Q2	-0.000	(0.0016)	-0.012***	(0.0022)	-0.011***	(0.0007)
Percent above LTI threshold × 2012 Q3	-0.000	(0.0014)	-0.011***	(0.0020)	-0.010***	(0.0006)
Percent above LTI threshold × 2012 Q4	-0.000	(0.0013)	-0.009***	(0.0017)	-0.009***	(0.0005)
Percent above LTI threshold × 2013 Q1	-0.002	(0.0013)	-0.009***	(0.0016)	-0.008***	(0.0005)
Percent above LTI threshold × 2013 Q2	0.000	(0.0011)	-0.007***	(0.0015)	-0.007***	(0.0005)
Percent above LTI threshold × 2013 Q3	0.001	(0.0007)	-0.005***	(0.0010)	-0.006***	(0.0004)
Percent above LTI threshold × 2013 Q4	-0.001	(0.0008)	-0.004***	(0.0009)	-0.003***	(0.0002)
Percent above LTI threshold × 2014 Q1	-0.001	(0.0008)	-0.002***	(0.0007)	-0.001***	(0.0002)
Percent above LTI threshold × 2014 Q2	0.000	(0.0004)	-0.001	(0.0003)	-0.001***	(0.0002)
Percent above LTI threshold × 2014 Q3	0.000	(.)	0.000	(.)	0.000	(.)
Percent above LTI threshold × 2014 Q4	-0.001***	(0.0004)	0.000	(0.0004)	0.001***	(0.0001)
Percent above LTI threshold × 2015 Q1	-0.003***	(0.0005)	-0.001**	(0.0005)	0.002***	(0.0002)
Percent above LTI threshold × 2015 Q2	-0.004***	(0.0008)	-0.003***	(0.0009)	0.002***	(0.0004)
Percent above LTI threshold × 2015 Q3	-0.007***	(0.0010)	-0.005***	(0.0013)	0.002***	(0.0005)
Percent above LTI threshold × 2015 Q4	-0.008***	(0.0010)	-0.005***	(0.0012)	0.003***	(0.0005)
Percent above LTI threshold × 2016 Q1	-0.009***	(0.0011)	-0.006***	(0.0015)	0.004***	(0.0006)
Percent above LTI threshold × 2016 Q2	-0.012***	(0.0014)	-0.007***	(0.0018)	0.004***	(0.0006)
Percent above LTI threshold × 2016 Q3	-0.012***	(0.0013)	-0.008***	(0.0018)	0.005***	(0.0007)
Percent above LTI threshold × 2016 Q4	-0.014***	(0.0014)	-0.008***	(0.0019)	0.006***	(0.0008)
Percent above LTI threshold × 2017 Q1	-0.014***	(0.0012)	-0.008***	(0.0019)	0.006***	(0.0008)
Percent above LTI threshold × 2017 Q2	-0.015***	(0.0013)	-0.008***	(0.0019)	0.006***	(0.0007)
Percent above LTI threshold × 2017 Q3	-0.016***	(0.0015)	-0.008***	(0.0020)	0.008***	(0.0006)
Percent above LTI threshold × 2017 Q4	-0.015***	(0.0013)	-0.006***	(0.0018)	0.009***	(0.0006)
Percent above LTI threshold × 2018 Q1	-0.016***	(0.0014)	-0.007***	(0.0019)	0.010***	(0.0007)
Percent above LTI threshold × 2018 Q2	-0.018***	(0.0016)	-0.008***	(0.0021)	0.010***	(0.0007)
Percent above LTI threshold × 2018 Q3	-0.018***	(0.0016)	-0.009***	(0.0022)	0.010***	(0.0008)
Percent above LTI threshold × 2018 Q4	-0.020***	(0.0017)	-0.009***	(0.0023)	0.010***	(0.0007)
Percent above LTV threshold × 2011 Q1	-0.016***	(0.0017)	-0.006**	(0.0024)	0.010***	(0.0009)
Percent above LTV threshold × 2011 Q2	-0.015***	(0.0016)	-0.006***	(0.0023)	0.009***	(0.0009)
Percent above LTV threshold × 2011 Q3	-0.015***	(0.0018)	-0.007**	(0.0026)	0.008***	(0.0009)
Percent above LTV threshold × 2011 Q4	-0.014***	(0.0019)	-0.007**	(0.0026)	0.008***	(0.0008)
Percent above LTV threshold × 2012 Q1	-0.013***	(0.0019)	-0.006**	(0.0025)	0.007***	(0.0007)
Percent above LTV threshold × 2012 Q2	-0.011***	(0.0017)	-0.005**	(0.0023)	0.006***	(0.0007)
Percent above LTV threshold × 2012 Q3	-0.009***	(0.0015)	-0.004*	(0.0020)	0.005***	(0.0006)
Percent above LTV threshold × 2012 Q4	-0.008***	(0.0013)	-0.004**	(0.0017)	0.005***	(0.0005)
Percent above LTV threshold × 2013 Q1	-0.007***	(0.0014)	-0.003*	(0.0017)	0.004***	(0.0005)
Percent above LTV threshold × 2013 Q2	-0.006***	(0.0010)	-0.003*	(0.0014)	0.003***	(0.0005)
Percent above LTV threshold × 2013 Q3	-0.005***	(0.0007)	-0.002**	(0.0009)	0.002***	(0.0004)
Percent above LTV threshold × 2013 Q4	-0.003***	(0.0010)	-0.002	(0.0010)	0.002***	(0.0003)
Percent above LTV threshold × 2014 Q1	-0.003**	(0.0010)	-0.002*	(0.0008)	0.001***	(0.0003)
Percent above LTV threshold × 2014 Q2	-0.001*	(0.0005)	-0.001	(0.0004)	0.000	(0.0002)
Percent above LTV threshold × 2014 Q3	0.000	(.)	0.000	(.)	0.000	(.)
Percent above LTV threshold × 2014 Q4	0.001	(0.0005)	0.000	(0.0005)	-0.001***	(0.0001)
Percent above LTV threshold × 2015 Q1	0.001**	(0.0006)	-0.000	(0.0006)	-0.001***	(0.0002)
Percent above LTV threshold × 2015 Q2	0.002**	(0.0008)	-0.000	(0.0009)	-0.002***	(0.0004)
Percent above LTV threshold × 2015 Q3	0.002**	(0.0010)	-0.001	(0.0013)	-0.003***	(0.0005)
Percent above LTV threshold × 2015 Q4	0.003***	(0.0010)	-0.001	(0.0012)	-0.004***	(0.0006)
Percent above LTV threshold × 2016 Q1	0.002**	(0.0011)	-0.002	(0.0014)	-0.004***	(0.0007)
Percent above LTV threshold × 2016 Q2	0.003**	(0.0014)	-0.002	(0.0018)	-0.005***	(0.0007)
Percent above LTV threshold × 2016 Q3	0.004**	(0.0013)	-0.002	(0.0019)	-0.006***	(0.0008)
Percent above LTV threshold × 2016 Q4	0.004***	(0.0014)	-0.002	(0.0019)	-0.006***	(0.0008)
Percent above LTV threshold × 2017 Q1	0.006***	(0.0013)	-0.002	(0.0019)	-0.008***	(0.0008)
Percent above LTV threshold × 2017 Q2	0.007***	(0.0013)	-0.002	(0.0018)	-0.009***	(0.0007)
Percent above LTV threshold × 2017 Q3	0.007***	(0.0014)	-0.003	(0.0019)	-0.010***	(0.0006)
Percent above LTV threshold × 2017 Q4	0.008***	(0.0012)	-0.003	(0.0017)	-0.011***	(0.0007)
Percent above LTV threshold × 2018 Q1	0.008***	(0.0012)	-0.003*	(0.0017)	-0.012***	(0.0007)
Percent above LTV threshold × 2018 Q2	0.009***	(0.0015)	-0.003	(0.0020)	-0.012***	(0.0007)
Percent above LTV threshold × 2018 Q3	0.009***	(0.0014)	-0.004**	(0.0020)	-0.013***	(0.0007)
Percent above LTV threshold × 2018 Q4	0.011***	(0.0016)	-0.004**	(0.0020)	-0.015***	(0.0007)
Constant	3.587***	(0.0383)	13.213***	(0.0512)	7.140***	(0.0195)
Observations	1248		1248		1248	
Adjusted R^2	0.978		0.992		0.999	
Time FE	Yes		Yes		Yes	
Postcode FE	Yes		Yes		Yes	
Postcode × time trend	Yes		Yes		Yes	

Note: Standard errors are clustered at the postcode level, and reported in parenthesis. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.